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Three Essays on the Impacts of Air Pollution and Environmental Policy

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ABSTRACT

This dissertation analyzes the effects of United States environmental policy - specifically that which regards air pollution - on health, labor market, and environmental outcomes. The first chapter examines the potential long-term effects of childhood exposure to atmospheric lead. The outcome of interest is crime, and the policy analyzed is the leaded gasoline phaseout. The second chapter seeks to investigate the effects of environmental regulation on labor markets. Nonattainment status designation creates variation in regulatory levels across counties based on a county's air quality for a given pollutant, in this case ozone. The third chapter provides analysis of the design ramifications of the Acid Rain Program's tradable permit market for sulfur dioxide established by Title IV of the Clean Air Act Amendments of 1990. The study examines how the two-phase approach as well as the initial permit allocation rule affected emissions. These studies all show evidence of the wide range of effects environmental policy can have.

THREE ESSAYS ON THE IMPACTS OF AIR POLLUTION AND ENVIRONMENTAL POLICY

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Table of Contents

List of Figures

List of Tables

1. Three Essays on the Impacts of Air Pollution and Environmental Policy

Environmental policy can have a broad range of ramifications in society. The present work seeks to analyze multiple air pollution policy measures in the United States and the assorted impacts of these initiatives. The topics investigated in these three essays cover different stages of environmental policy from the initial design to the aftermath. Several econometric techniques are used to analyze the various effects of air pollution policy.

The first chapter investigates the public health benefits that can result from reducing the levels of a harmful pollutant in the atmosphere. The phaseout of leaded gasoline in the United States eliminated the vast majority of atmospheric lead pollution from the environment. Medical studies have linked lead exposure to an array of health concerns, and exposure to lead as a child can have long-term effects that may relate to criminal behavior. This chapter analyzes the impact of changes in atmospheric lead on trends in crime over time and across areas in the United States. To address endogeneity concerns, an air stagnation index is employed as an instrumental variable. Air stagnation accounts for meteorological conditions that contribute to the residence time of air pollution. Less-stagnant areas disperse emissions more readily; so, as the primary source of atmospheric lead was removed over the course of the phaseout, more-stagnant areas on average saw greater reductions in atmospheric lead than less-stagnant areas. The regression analysis produces evidence that decreased atmospheric lead later reduced adult incarceration likelihoods and arrests. An average drop in atmospheric lead over the course of the phaseout is estimated to reduce incarceration probability by about 0.4 percentage points between cohorts. Based on the data, this represents a roughly 16 percent decline in the probability of incarceration between birth cohorts. Baseline regressions estimate elasticities of roughly 0.32 between instrumented atmospheric lead and violent crime arrests and 0.51 between instrumented atmospheric lead and property crime arrests.

The second chapter analyzes how the implementation of environmental regulations can affect labor markets. In 2004, a change in the United States Environmental Protection Agency's air quality standards led to counties across the country being reclassified as *nonattainment* or *attainment*. Counties considered nonattainment are subject to stricter regulations and penalties if they do not clean up their air. This study employs a regression discontinuity (RD) design to test the potential effects of nonattainment status and the accompanying regulations on local economic conditions. The sample analyzed consists of counties monitored for ozone levels by the EPA from 2004-2011. When focusing on highly polluting industries – those most likely affected by the regulations – the RD results show a negative, statistically significant effect of nonattainment status on employment in such industries. The estimated magnitude is roughly 24 percent lower employment in polluting industries for nonattainment counties. The results for establishments also show a negative coefficient but are not statistically significant. Additional analysis finds no statistically significant impact of nonattainment status on total county economic conditions, implying that any economic effects of the regulations are limited to polluting industries.

The third and final chapter examines the design consequences of the United States Acid Rain Program (ARP). As reducing emissions of harmful pollutants remains a primary concern in environmental policy, it is beneficial to examine a successful policy initiative – the United States Acid Rain Program. This portion of the Clean Air Act Amendments of 1990 created a credit-trading program for sulfur-dioxide emissions. This study performs difference-in-differences regression analysis exploiting the two-phase execution to estimate how the design of the program affected emissions of sulfur dioxide. The results indicate that the ARP indeed reduced emissions for Phase 1-only plants relative to Phase 2-only plants. Further, this chapter also strives to examine the ex-post efficiency of the program's initial permit allocations. Theoretically, initial allocations should not affect efficiency as firms reach the efficient level of emissions through abatement or trading credits regardless of their initial permit endowment. Put another way, the effect of initial allocations on emissions should be zero if standard assumptions hold. The results of this analysis imply that initial allocations have a statistically significant non-zero impact on emissions. For Phase 2 allocations, the results imply a nearly one-to-one relationship between emissions and allocations, while one allocation in Phase 1 implies around 0.6 emissions.

Overall, these analyses show further evidence of the potential effects of environmental policy. The design and implementation mechanisms of policy are important matters to consider in future policy decisions. Reducing air pollution can have major environmental and public health benefits while also having assorted economic impacts.

2. The Effects of Childhood Atmospheric Lead Exposure on Crime

2.1 Introduction

The phaseout of leaded gasoline, which began in the United States in the mid-1970s and officially ended in the mid-1990s, was one of the largest environmental policy endeavors in history. Anti-lead initiatives succeeded in greatly reducing the amount of lead in the environment - atmospheric lead in the United States was almost completely eliminated over the course of the leaded gasoline phaseout. The removal of lead from the environment has been a chief concern in the United States since the 1970s, but worry over the harmful effects of lead exposure existed long before then. Lead can enter the body through various sources, and greater frequency and intensity of exposure increase the risk of lead negatively impacting health. Even low levels of exposure can have adverse effects on health, especially for children, and past lead exposure may continue to affect the population. The same negative developmental outcomes associated with lead exposure (impaired cognitive development, aggression, hyperactivity, etc.) can be related to various life outcomes such as academic performance and social behavior. Case and cohort studies in the scientific literature have analyzed the effect of childhood lead exposure on cognitive test scores, while the behavioral effects of lead exposure have led some researchers to hypothesize a relationship between childhood lead exposure and criminal activity.

The major crime declines seen in the United States during the 1990s resulted in a wave of research investigating the potential causes of such a dramatic decrease (see Levitt (2004), among others). Some analysis has postulated that the large reduction of lead exposure beginning in the late 1970s and early 1980s could be related to the sizable crime declines of the late 1990s. Past research has produced evidence of a correlation between lead exposure and crime, and stronger evidence of a causal effect of lead exposure on crime would imply additions to the already large estimated benefits of the leaded gasoline phaseout.

This study uses the U.S. leaded gasoline phaseout to analyze the potential effects of lead exposure over time across urban areas. While the phaseout affected atmospheric lead in all areas in the United States, the environmental impacts varied across areas in both magnitude and timing. Much of this cross-sectional variation could be endogenously determined and produce estimation bias when considering outcomes such as crime. For example, socioeconomic status can be associated with criminal behavior as well as lead exposure. Low socioeconomic status individuals could be more likely to later engage in criminal activity while also having had greater exposure to lead as a child if they resided in an area or home with greater automobile traffic, less fresh air, more lead paint, etc. Poorer areas could have had greater crime and may have also had a greater concentration of older cars that still required leaded gasoline. Such areas would then have had greater atmospheric lead emissions and exposure risk as well. Political endogeneity could exist as well. Active local or state governments could have instituted stricter lead laws while also being tougher on crime.

To overcome such endogeneity concerns, the empirical strategy in this study relies on the impact of air stagnation on lead pollution levels over time. The Air Stagnation Index (ASI) is used as an instrumental variable in this analysis and can be seen as a proxy for climate – generally speaking, ASI is a meteorological index that provides an estimate for atmospheric circulation based on wind, precipitation, and atmospheric temperature. More-stagnant air increases air pollution residence time – how long particles stay in the atmosphere for a given location. In the context of the leaded gasoline phaseout, more-stagnant areas should have, on average, seen a greater reduction in environmental lead levels than less-stagnant areas following the gradual removal of leaded gasoline – the primary source of atmospheric lead. Less-stagnant areas had climatological mechanisms that reduced residence time of atmospheric lead, so removing the source had less of an impact on exposure risk. Residence time would have been longer in stagnant areas. As a greater proportion of lead emissions would linger for a longer amount of time in stagnant areas, removing the source of those emissions was presumably more beneficial over time for such locations. Results of the regression analysis in this study validate this expectation and are strongly significant.

Air stagnation is also assumed to be unrelated to other trends that could have impacted the change in crime over time – environmental lead exposure is assumed to be the only channel through which air stagnation affects trends in criminal activity over time. Air stagnation is regionally correlated, so regional and regional-time control variables are included in the main specifications. The relationship between atmospheric lead and air stagnation is also seen with other air pollutants, but this is less of a concern in the present study. In the years analyzed, the drop in atmospheric lead levels outpaces those in other pollutants (see Table 2.1). Further, the primary health consequences associated with other air pollutants (ozone, particulate matter, etc.) are mostly respiratory and cardiovascular issues.[1] Such effects are less likely to directly relate to crime than the potential cognitive and behavioral developmental impacts linked to lead exposure; however, there could be effects of general air pollution on childhood health outcomes that affect education that in turn could impact crime. Such matters are discussed in greater detail later in the chapter.

The main analysis of this study examines the potential impact of environmental lead exposure on crime-related outcomes. This study produces evidence that the massive reduction in childhood environmental lead exposure impacted trends in crime. The estimates imply that an average decline in atmospheric lead over the course of the phaseout would have reduced the probability of adult incarceration by roughly 0.4 percentage points. About 2.5 percent of the sample was incarcerated, so the estimated reduction is about 16 percent of the average probability of incarceration. The results from additional analysis of arrest trends at the Metropolitan Statistical Area (MSA) level support these findings. Baseline regressions estimate an elasticity of roughly 0.32 between instrumented atmospheric lead and violent crime and an

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[1] "Health Effects of Air Pollution". United States Environmental Protection Agency. http://www.epa.gov/region07/air/quality/health.htm

estimated elasticity of 0.51 for instrumented atmospheric lead and property crime. The overall analysis provides additional evidence of broad societal benefits resulting from major environmental policy. The results support past scientific findings regarding the developmental consequences of childhood lead exposure as well as the possibility that such consequences could manifest themselves in criminal activity. From a policy perspective, the key decision is whether to take measures to prevent lead exposure in the first place, or address the potential consequences later.

Before describing the empirical analysis in greater detail, it is useful to first provide more information on the phaseout of leaded gasoline in the United States, the health effects of lead exposure, and past studies of lead and leaded gasoline phaseouts.

2.2 Background Information

2.2.1 The U.S. Leaded Gasoline Phaseout[2]

The phaseout of leaded gasoline is one of the largest and most impactful environmental policy measures in U.S. history. The amount of lead in the atmosphere was greatly reduced, and the decrease in lead was significant even relative to the declines in other pollutants (see Table 2.1). The potential hazards of lead had been known for centuries; however, these were typically considered to be direct occupational risks. Oil refineries began adding lead to gasoline in the United States starting in the 1920s in order to improve engine performance. Clair Patterson and other advocates for reducing lead pollution were largely ignored by industry and government for several decades until public concern over the harmful effects of lead exposure intensified in the early 1970s.[3] In 1975, the U.S. Environmental Protection Agency (EPA) initiated the gradual phaseout of leaded gasoline. At the same time, the U.S. Congress passed a requirement that all new cars be equipped with catalytic converters. Catalytic converters make car emissions less

[2] Unless otherwise noted, sources for this background information consist of Newell and Rogers (2003) and an assortment of EPA documents included in the References section.
[3] *Cosmos*, Episode: "The Clean Room".

toxic and do not work with leaded gasoline. The EPA mandated that all gasoline stations carry unleaded gasoline, and gas tanks on new cars were designed to only work with unleaded fuel nozzles.

Studies released in the late 1970s on the harmful effects of lead exposure further heightened public awareness and support for lead reduction. The EPA banned lead in household paint in 1978 and set standards for lead content in gasoline in 1979. In the 1980s, the EPA further reduced lead content standards, and created a lead credit-trading program to help refineries meet the stricter limits. Leaded gasoline was officially banned in the United States in 1996, but had been almost entirely phased-out by the early 1990s.

The reduction of atmospheric lead occurred through two main mechanisms – fleet turnover and lead content standards. As post-1975 automobile models were purchased, consumers began to need unleaded gasoline, so the share of unleaded gasoline sharply rose. Leaded gasoline was still an option for older cars as leaded gasoline helped engine performance and was initially cheaper than unleaded fuel.[4] However, lead content in leaded gasoline rapidly declined as the EPA implemented new and gradually stricter standards.

2.2.2 The Adverse Effects of Lead Exposure

Lead has been a useful substance for thousands of years; however, it is toxic to humans – even in small doses.[5] Physiologically, lead can affect biochemical processes by interacting with proteins, masking itself as calcium, or stymying calcium-related processes.[6] Lead's greatest impact comes in the brain, where calcium is crucial for healthy functioning and development. A great amount was accomplished in the U.S. through the leaded gasoline phaseout, lead paint ban, and other measures. The U.S. Center for Disease Control regards 5 micrograms of lead per

[4] Borenstein (2003 Working Paper).
[5] Institute of Medicine. *Lead in the Americas: A Call for Action*.
[6] Wolf, Lauren K. "The Crimes of Lead". *Chemical and Engineering News*. February 3, 2014.

deciliter of blood (μg/dL) to be “abnormal”.[7] In 1976, the U.S. average blood lead level was 16 μg/dL (18 μg/dL for children under 6 years old), while the average in 1991 had decreased to 3 μg/dL (2.8 μg/dL for children).[8]

Numerous studies have linked lead exposure – typically measured by blood-lead level (BLL for short) – to adverse health effects. Lead exposure can be associated with many different health concerns: higher blood pressure, anemia, low sperm count and mobility, gastrointestinal problems, renal difficulties, memory and concentration issues, premature births and low birth weight, and stunted cognitive development in children.[9] The cognitive developmental impacts of lead exposure on children may take the form of hyperactivity, irritability, impulsivity, and lower IQ.[10] Such consequences could be actualized in lower test scores, riskier behavior, and criminal activity.[11] For example, if lead exposure increases aggressive behavior and impulsivity, an individual could then be more likely to commit a crime. There may be no threshold for “safe” lead levels for children, and adverse effects of lead exposure can persist into adulthood.[12] Low-level lead exposure is not typically treated medically; prevention is the best strategy in reducing the adverse effects of lead exposure.[13]

Before the phaseout of leaded gasoline, the most pervasive source of lead exposure was air pollution from automobile exhaust.[14] Inhalation of lead from the air represents one of several channels for lead to enter the body. Once lead enters the body, it is distributed by the blood to organs such as the brain, kidneys, and liver; lead that is not processed out of the body is typically stored in bones and/or teeth where it can potentially be remobilized into the blood later

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[7] Ibid.
[8] Reyes (2007).
[9] "Lead and Its Human Effects”. Public Health - Seattle & King County.
http://www.kingcounty.gov/healthservices/health/ehs/toxic/LeadGeneral.aspx
[10] Mt. Washington Pediatric Hospital
http://www.mwph.org/services/effects_lead_poisoning.htm
[11] Reyes (2014 Working).
[12] Center for Disease Control
http://www.atsdr.cdc.gov/csem/csem.asp?csem=7&po=10
[13] Needleman (2007).
[14] Institute of Medicine. *Lead in the Americas: A Call for Action*.

in life.[15] As increased frequency and/or magnitude of exposure to lead increases the chance it is deposited in the body, greater exposure means greater health risk.[16] While it is difficult to estimate the direct effect of air lead on blood lead levels, past research has linked atmospheric lead levels to BLLs. For example, a case study of children in Detroit performed in Zahran et al. (2013) finds that a positive change in atmospheric lead of 0.0069 $\mu g/m^3$ increases BLLs in 1 year olds by 10 percent, controlling for other potential factors. Past research also concludes that atmospheric lead levels relate to lead levels in the soil – another important environmental lead exposure channel (see Sheets et al. (2001), and Schmidt (2010), among others). Now, past studies more relevant to the present analysis will be discussed.

2.3 Literature Review: Developmental Impacts of Childhood Lead Exposure

An extensive literature exists examining the health effects of lead, but most are case or cohort studies. The general consensus is that lead exposure has an adverse effect on a number of health outcomes, most notably cognitive development in children. Many studies have found significant, negative relationships between childhood lead exposure and cognitive development. Examples of such studies include Chen, Dietrich, Ware, Radcliffe, and Rogan (2005), Chandramouli, Steer, Ellis, and Emond (2009), Strayhorn and Strayhorn (2012), and Brink et al. (2013). Nillson (2009 Working) shows a negative relationship between lead exposure as a child and life outcomes such as education and wage.

More relevant to this study, several past analyses address the hypothesized relationship between lead exposure and crime. Nevin (2007) employs lags between 18 and 23 years to examine the impact of preschool BLLs on future crime. The study asserts that childhood BLLs over 10 $\mu g/dL$ are harmful to learning and behavior. Reyes (2007) uses lead content of gasoline to examine the effect of atmospheric lead on crime at the state level, estimating that the leaded gasoline phaseout accounted for a 56 percent decline in violent crime in the 1990s. Mielke and

[15] http://www.who.int/mediacentre/factsheets/fs379/en/
[16] https://www.health.ny.gov/publications/2584/

Zahran (2012) analyzes lead emissions and aggravated assault in six U.S. cities using a 22-year lag in exposure. The study determines that a 1 percent rise in air lead 22 years prior would raise current-year aggravated assault by 0.46 percent. Stretesky and Lynch (2004) finds that 1990 lead levels affected mid-1990s crime, and the effect of lead on property crime was larger than that on violent crime. Lersch and Hart (2014) provides a case study of Hillsborough County, Florida, and investigates the impact of the spatial distribution of lead-emitting facilities on crime. The spatial distribution of lead-emitting facilities improves the prediction of property crime but not that of violent crime. Farrell (2013) tests assorted hypotheses for the large crime drop in the 1990s, finding the childhood lead exposure hypothesis to have strengths and weaknesses. Of the study's five tests, the childhood lead exposure hypothesis passed three (cross-country relevance, explanation of crime increase, and past empirical evidence) while failing two (phone theft and cybercrime effect and similar effects across crime types). Only one hypothesis – improved security – performed better in the study than the childhood lead exposure hypothesis. Other studies examining the relationship between lead and crime (or associated behavioral problems) include Needleman, Riess, Tobin, Biesecker, and Greenhouse (1996), Nevin (2000), Dietrich, Douglas, Succop, Berger, and Borenstein (2001), Stretesky and Lynch (2001), Marcus, Fulton, and Clarke (2010), Haynes et al. (2011), Reyes (2015), and Feigenbaum and Muller (2014 Working).

A number of studies utilize leaded gasoline phaseouts in various countries as shocks to lead levels. Studies that examine the effect of phasing out leaded gasoline on atmospheric lead levels include Romero (1996), Kondo et al. (2007), and Mielke, Laidlaw, and Gonzales (2011). The general conclusion is that phasing out leaded gasoline, as expected, reduces lead levels in the atmosphere; however, as Mielke et al. (2011) notes, lead that is deposited in the soil, water, or plants can linger for much longer. Nichani (2006), Graber et al. (2011), Huang et al. (2012), and others look into the effect of leaded gasoline phaseouts on blood lead levels. These studies all find the anticipated positive relationship between BLLs and leaded gasoline availability. Several analyses have attempted to estimate the costs and benefits of the policy endeavor in the United States. The EPA performed a Regulation Impact Analysis (RIA) in 1985, finding that benefits greatly exceed costs. Studies such as Schwartz (1994b) and Salkever (1995) provide updated estimates to the EPA's efforts by considering lower levels of lead exposure as well as less direct effects of childhood lead exposure. The estimated benefits of the major reduction in lead exposure given in such studies are typically large – Schwartz (1994b) asserts a roughly $7 billion benefit while Salkever (1995) argues an additional $2.5 billion dollars in total benefits.

The leaded gasoline phaseout directly affected atmospheric lead levels while implicitly affecting BLLs. The scientific literature has established an adverse relationship between early-life lead exposure and childhood development. These findings are mostly derived from cohort or case studies, and the strong results have been applied to cost-benefit analyses of leaded gasoline phaseouts. The social science literature has found evidence of an impact of lead exposure on outcomes such as IQ or other cognitive measures and crime. Observational studies typically fail to properly account for potential endogeneity arising from unobserved variables that may affect both BLLs (or the selected instrumental variable) and the outcome being analyzed.

Dynamic and cross-sectional variation is also lacking within the literature. Few studies look at the effect of changes in lead exposure on trends over time. Some studies are too geographically broad – opting to focus on national or state-level trends. Others focus on one or a select few urban areas, limiting the generalizability of their findings. Proper identification strategies and broader analysis across urban areas and over time can provide better estimates of the individual and potentially societal costs of early-life lead exposure as well as the benefits of major environmental policy like the US leaded gasoline phaseout. The framework and results of the present analysis will now be described.

2.4 Empirical Strategy and Data

2.4.1 Empirical Strategy

This study strives to fill gaps in the literature through several methodological advances. Like some studies in the past, the leaded gasoline phaseout will be utilized as a long-term shock to atmospheric lead levels. This shock greatly reduced lead pollution across the country between the 1970s and the 1990s and provides large variation to analyze the effects of changes in lead levels. Despite this, the broad societal impacts of the phaseout have been relatively understudied in the literature. One complication arises from the nature of the phaseout – it was not an immediate change, and the national initiative had intricacies that varied across local areas. The phaseout's official start year, 1975, does not represent an instant, dramatic change in lead levels.

While the national regulation started the phaseout, localities could make their own decisions above what was required by the EPA. For example, the city of Chicago banned the sale of leaded gasoline in 1984, a full twelve years before the national ban.[17] Studies of the phaseout also often fail to separate out direct effects of the phaseout from other anti-lead initiatives such as the lead paint ban in 1978. BLLs can be affected by various sources of lead exposure, many of which can be related to crime through other channels such as socioeconomic status.

By focusing on the outdoor environment, this study can more properly isolate the impact of the leaded gasoline phaseout while examining a lead exposure channel over which individuals have less control. BLLs simply represent the amount of lead in the blood, meaning the source of changes in lead exposure is not known. The declines in BLLs could come from reduced lead in the environment, lead paint removal, and/or behavioral changes resulting from increased awareness of the dangers of lead exposure. Of these, lead in the outdoor environment would be most directly affected by the leaded gasoline phaseout. Employing atmospheric lead as a proxy for lead exposure also presents an opportunity to exploit exogenous variation across areas

[17] Kovarik (2005).

regarding climate and geography.

This study uses air stagnation as an instrumental variable for atmospheric lead levels. The Air Stagnation Index factors in meteorological factors such as temperature inversions, precipitation, and wind. The calculation of the ASI will be discussed in more detail shortly. To the knowledge of the author, the ASI used in this study has not been employed as an instrumental variable in any previous studies in the literature, nor have air stagnation measures been utilized in analyzing the effects of atmospheric lead. Atmospheric stagnation and components of the ASI were previously employed in air pollution studies such as Bharadwaj and Eberhard (2008 Working), Arceo-Gomez (2012), Ransom and Pope (2013 Working), and Herrnstadt and Muehlegger (2015 Working).

As a circulating atmosphere can disperse air pollution, one would expect ASI, holding pollution constant, to have a positive relationship with air pollution levels – more stagnant places have higher ASI and typically higher air pollution levels. Of interest to the present study is the dynamic effect of air stagnation on lead pollution. The expectation is that the leaded gasoline phaseout affected stagnant areas more than it affected less-stagnant areas, meaning that ASI is negatively associated with the change in atmospheric lead. More-stagnant atmospheres meant lead in the environment was more likely to linger in high-ASI areas. So, by removing the main source of lead pollution (via the phaseout of leaded gasoline), more-stagnant areas should have seen a larger reduction in atmospheric lead.

2.4.2 Data

The data for atmospheric lead are from monitor data from the EPA, which were acquired for use in this study through a Freedom of Information Act (FOIA) request. The data cover all available atmospheric lead monitors in the continental United States. The years in the sample are 1960 to 2000, though the number of monitor observations before the mid-1960s is quite low. The units of measurement are micrograms per cubic meter ($\mu g/m^3$), and the unit of observation is at the county level. The time trend in the lead data from 1965 to 2000 can be seen in Figure 2.1. The average atmospheric lead decreases over time, as expected, and the major declines occur during the years of the leaded gasoline phaseout. Of note, the National Ambient Air Quality Standard (NAAQS), which the EPA sets for various pollutants at the respective levels deemed permissible for public health, has been 0.15 $\mu g/m^3$ for lead since 2008. This standard is represented by the horizontal line in Figure 2.1. In the data, average lead levels in the early 1970s were eight times higher at their peak than the current lead NAAQS. The average county in the monitor sample failed to meet the current NAAQS for lead until the late 1980s. The variation in lead levels among areas initially is large, but declines over time. A data concern relevant to this and most other air pollution studies is the placement and availability of monitors. Monitor placement is assumed to be random among and within geographic areas, though this may not be the case.

The ASI data come from the National Oceanic and Atmospheric Administration (NOAA), with the specific index data taken from Wang and Angell (1999). The Air Stagnation Index is calculated as the monthly number of air stagnation periods for a given latitude and longitude. An air stagnation period consists of four consecutive air stagnation days. In simple terms, an air stagnation day consists of low or no wind and no precipitation, and may include temperature inversion – an atmospheric phenomenon that is conducive to air being trapped over an area, possibly keeping pollution close to the ground.[18] More specific meteorological definitions for air stagnation days can be found in Wang and Angell (1999).

For the purposes of this study, the monthly ASI for a given latitude-longitude pair is averaged from 1973 to 1997. These years serve to cover the entire timeframe associated with the leaded gasoline phaseout. As ASI can be quite volatile month to month, the averaging process provides a general measure of how stagnant the atmosphere is for a given area. Over the time

[18] http://www.wrh.noaa.gov/slc/climate/TemperatureInversions.php.

period analyzed, there is not great variation in the average ASI for a given location year-to-year. The ASI measures cover the contiguous United States. County centroids are matched to the nearest latitude-longitude pair in the ASI data to provide each county with an ASI value. Then, ASI is averaged by MSA or state. There is little variation in ASI within small geographic areas like counties and MSAs, while variation increases as the geographic unit of observation broadens. Figure 2.2 is a map showing variation in state averages for ASI by quartile. Clearly, there are some regional elements to ASI. This is addressed in the main regression analysis through the inclusion of regional and regional-time controls.

For the crime outcome analysis, the individual-level data are from the American Community Survey (ACS) via the Integrated Public Use Microdata Series (IPUMS). The survey years are 2001 to 2012, and the sample is limited to adults. Two birth cohorts are examined – individuals born between 1965 and 1969 and individuals born between 1985 and 1989. These years were selected based on data availability and the timing of the phaseout. The earlier birth cohort will be defined as the "high exposure" cohort as it represents a group clearly born before the phaseout. The later birth cohort is defined as the "low exposure" cohort since it represents individuals born after most of the reduction in atmospheric lead had occurred. Later birth years were not used in order to provide enough adults in the "low exposure" sample.

The incarceration variable is binary in the present analysis – it is a "1" if the individual was living in a prison, mental hospital, or assisted-living community in the given survey year. This information comes from a Census question regarding group quarters. A precise "prison" indicator is not available; however, past studies have treated this group quarters category as representing incarcerated individuals.[19] While this measure does not provide complete certainty regarding the means by which one is institutionalized, it should be a reasonably close representation of incarceration. The age of the sample implies few individuals would be living in

[19] Examples of such studies include Caceres-Delpiano and Giolito (2012), Charles and Luoh (2010), Borjas, Grogger, and Hanson (2006 Working), and Lochner and Moretti (2004).

assisted-living communities. Further, when defining "institutionalized population" as those in prisons or mental hospitals, roughly 97 percent of institutionalized adults were in prison and just 3 percent in mental hospitals by the year 2000.[20] Another limitation of this measure is that to be in jail, one must be caught committing a crime and found guilty. For this study, it is assumed that any differential trends in propensity to be jailed are not correlated with air stagnation after controlling for other factors such as region and demographics.

Summary statistics for the state atmospheric lead, state ASI, and incarceration variables are included in Table 2.2. The lead and ASI averages are weighted by county population by cohort to better approximate population exposure to atmospheric lead. Roughly 2.5 percent of the sample is institutionalized. One may note that the lead data has a minimum of zero – a value that corresponds to monitors in Wyoming. Excluding these observations does not alter the results, so they are kept in the sample. Comparisons of atmospheric lead and incarceration summary statistics between cohorts are also included in Table 2.2. Average atmospheric lead levels decline by roughly 0.9 μg/m3 between the cohort years, while more of the post cohort is institutionalized compared to the pre cohort (See Table 2.2). The difference in average incarceration for the birth cohorts as a whole is likely due to age – the "pre" cohort individuals are in their 40s while individuals in the "post" cohort are in their 20s and more likely to be presently committing crimes.

Regional and individual-level demographic controls come from the ACS data through IPUMS. A limitation of this analysis is the inability to perfectly determine childhood locations. Even when birth state is known, it is not certain if the individual stayed in their birth location for their formative years. Still, birth state is assumed to be a strong indicator of early life location. In 2000, almost 90 percent of households resided in the same state as they had in 1995.[21] These

[20] Harcourt (2006).
[21] United States Bureau of the Census, 2003. "Migration and Geographic Mobility in Metropolitan and Nonmetropolitan America: 1995 to 2000".

years are not part of this study's sample years; however, the statistic supports the notion of limited interstate mobility over a small timeframe. Looking at the ACS data, a majority of individuals reside as adults in the state in which they were born – roughly two-thirds of the respondents were current residents of their birth state. It is reasonable to assume that an even greater percentage of individuals reside in their birth state for the first few years of life.

2.5 Methodology and Primary Regression Results

The anticipated effect of ASI on changes in atmospheric lead levels is negative. The expectation is that over time, the positive relationship between ASI and atmospheric lead will diminish as more-stagnant areas see larger reductions in their lead levels than less-stagnant areas over the course of the phaseout. Put another way, the reduction in lead over time is expected to have been larger in stagnant areas. This relationship is seen in the data and shown graphically in Figures 2.3 and 2.4. One can see that the average decline in lead over the course of the phaseout is larger in magnitude for high-ASI areas. Further, this relationship is similar when comparing state averages to MSA averages.

The general methodology is an instrumental variable approach with the instrumental variable being the interaction of ASI and the relevant time dummy variable. This strategy relies on two major assertions. First, air stagnation environment, conditional on the other explanatory variables, is related to changes in atmospheric lead levels. This expected relationship can be tested, and results for changes in atmospheric lead regressed on ASI are included and discussed in the next section. Second, it is assumed that ASI is not correlated with the error term, meaning ASI is not related to crime trends over time except through lead pollution. There is no danger of reverse causality since ASI itself is not affected by crime trends; however, the potential exists for correlation between ASI and other trends that can be related to trends in crime. As air stagnation is highly related to geography, perhaps there are regional or state trends that are thus related to ASI and also impacted crime. Say that, over the years of the phaseout, an area attracted moreeducated residents who were less prone to committing crimes. Further, consider that this area has a high ASI – an observed relationship between ASI and crime may be overstated. Checks are performed to assess the validity of the second assumption and will be discussed later.

All regressions follow the general form described below.[22] Regional indicators are used to account for geographic correlation in ASI.[23] As demographic factors could affect the assorted outcomes analyzed, relevant controls are included in some specifications.[24] The variable of interest in the reduced-form regressions is the interaction between ASI and the "Low Exposure Cohort" variable. This interaction variable provides a coefficient estimate representing the effect of air stagnation over time. For the IV specifications, the variable of interest is "Atmospheric Lead", which is the cohort period average atmospheric lead level for the birth state weighted by county population.[25] It is instrumented by the ASI-cohort interaction term. The first-stage analysis will now be discussed followed by the description of the second-stage analysis.

2.5.1 First-Stage Analysis

For the first stage, average atmospheric lead is a function of ASI, a dummy variable for birth cohort, and the interaction of the ASI and cohort variables. In the primary analysis, the environmental factors are averaged at the state-level as birth state is the most refined geographic indicator of an individual's birthplace. The first-stage regression equation for individual *i* is

$$[1] \qquad AtmosphericLead_{STit} = \alpha_0 + \gamma_1 LowExposureCohort_t + \gamma_2 AirStagnationIndex_{it} + \gamma_3 (AirStagnationIndex \times LowExposureCohort)_{it} + \gamma V_{it} + \eta_{it}$$

"$AtmosphericLead_{ST}$" represents atmospheric lead averaged by state weighted by county population in cohort *t*; "*AirStagnationIndex*" is air stagnation averaged at the state level and weighted by county population in cohort t; the "*LowExposureCohort*" variable is a "0" for

[22] Standard errors are clustered by birth state to account for serial correlation by geographic area.
[23] Regions are defined using U.S. Census definitions.
[24] The analysis was also performed using sample weights, yielding comparable results.
[25] Regressions using unweighted state averages for atmospheric lead and ASI were also performed. The coefficient estimates are comparable to those using weighted averages – the statistical significance is similar and the estimated magnitudes are slightly larger.

individuals born between 1965 and 1969 and "1" for those born between 1985 and 1989; V is a vector of control variables; α_0 is a constant; and η is the error term. Due to some missing values in the data, lead is averaged for 1965 to 1974 for the "pre" cohort and 1980 to 1989 for the "post" cohort. Most monitoring was performed during the prime phaseout years, so many monitors were not available in the early and later years of the sample.

Table 2.3 shows the first-stage results. The coefficient signs are all as anticipated. The relationship between atmospheric lead levels and the ASI interaction term is negative as expected and strongly statistically significant. The estimated effect after adding in assorted controls for birth region and demographic characteristics does not greatly differ from the baseline results – in fact, the coefficient becomes slightly larger in magnitude. The main result holds across specifications – as lead declined over time, there were bigger declines in more-stagnant states. Put another way – the variation in lead levels between highly stagnant air environments (generally high lead) and less-stagnant air environments (typically lower lead levels) decreased over time. The results imply that an additional air stagnation period per month would on average lead to a 0.14 μg/m^3 greater reduction in atmospheric lead over the course of the phaseout. The current NAAQS have the maximum atmospheric lead concentration to be 0.15 μg/m^3, so such a decline is fairly substantial. The instrument is statistically significant at the 1 percent level in all specifications, and the explanatory power of the model is quite high.

2.5.2 Second-Stage Analysis

The strategy in the second stage accounts for the delayed impact of childhood lead exposure as children age into committing crimes. The second-stage analysis examines how changes in atmospheric lead exposure over time and variation across geographic areas affected the assorted outcome variables:

$$[2] \qquad Incarceration_i = \alpha + \beta_1 \widehat{AtmosphericLead}_{STit} + \beta_2 LowExposureCohort_t + \beta_3 AirStagnationIndex_{it} + \beta X_{it} + \varepsilon_{it}$$

"$\widehat{AtmosphericLead}$" is the predicted change in lead levels as instrumented by the "*ASI×LowExposureCohort*" variable; "*AirStagnationIndex*" and "*LowExposureCohort*" represent the birth state air stagnation average and cohort dummy variable respectively; and X is the appropriate vector of control variables. The outcome analyzed is individual incarceration at the time of the survey.

From the IPUMS data, one will notice that changes in atmospheric lead are associated with trends in incarceration (see Figures 2.5 through 2.7). Decline in atmospheric lead between the two cohorts is negatively related to changes in incarceration – the rise in incarceration between the cohorts is smaller for higher declines in lead (see Figure 2.5). For the "pre" cohort, Figure 2.6 shows a positive relationship between atmospheric lead in birth state and incarceration as an adult for the "pre" cohort, and as state lead levels converge in the "post" cohort, the relationship dissipates (see Figure 2.6). The change in trends between the two cohorts is also compared by birth state ASI in Figure 2.7. The incarceration trend is negatively related to ASI as anticipated (see Figure 2.7).

The reduced-form and IV results are included in Tables 2.4 and 2.5. The estimated effect of ASI on incarceration trends shows the expected negative sign and strong statistical significance (see Table 2.4). Including regional controls reducing the magnitude of the effect; however, the coefficient remains negative and statistically significant across specifications. Including the relevant control variables, an additional air stagnation period per month implies between a 0.05 and 0.06 percentage point reduction in the probability of incarceration between birth cohorts. If comparing an average ASI location (ASI around 5) with a high ASI location (ASI around 10), the highly stagnant area, based on these results, would have had a roughly 0.25 percentage point drop in incarceration roughly. This represents roughly 10 percent of the average incarceration in the data sample.

The IV results (see Table 2.5) imply a positive, statistically significant effect of instrumented atmospheric lead on incarceration trends. Including regional controls lowers the magnitude of the estimated lead effect, but the effect remains positive and statistically significant. Reducing lead in birth state by 1 $\mu g/m^3$ during the phaseout years would imply an average decrease in incarceration probability of roughly 0.4 percentage points when controlling for time and ASI as well as birth region, individual demographic characteristics, and the associated time trends. The average drop in atmospheric lead in the sample was around 0.9 $\mu g/m^3$, so 1 $\mu g/m^3$ is a reasonable decline. Roughly 2.5 percent of the full sample was institutionalized, so the estimated incarceration probability change is slightly under one-sixth of the mean in the full sample. So, all else equal, a typical drop in atmospheric lead during the phaseout implies a probability of incarceration in the "low exposure" cohort that is roughly 16 percent smaller than that for the "high exposure" cohort.

These results can be compared to other factors that have been linked to incarceration rates in past studies. The focus will be on studies that used the same incarceration measure as was employed in this analysis. Lochner and Moretti (2004) examines the impact of years of schooling on incarceration separately analyzing by race. The study finds that one additional year of schooling results in a 0.1 percent reduction in the probability of incarceration for whites and a 0.37 percent reduction for blacks. From the present analysis, an average drop in atmospheric lead over the course of the phaseout would, controlling for other factors, have had an effect on likelihood of incarceration comparable to four years of schooling for whites and one year of schooling for blacks. Lochner and Moretti (2004) find even larger impacts from a binary indicator of high school graduation – high school graduation is estimated to reduce incarceration probability by roughly 0.9 percentage points for whites and roughly 8 percentage points for blacks. Caceres-Delpiano and Giolito (2012) investigates the effect of unilateral divorce reform on the likelihood of children being incarcerated as adults. Increased divorce rates were determined to raise the likelihood of incarceration by about 60 percent. These results and the results from the present study together imply that the estimated impact, controlling for other factors, of the leaded gasoline phaseout on incarceration likelihood is roughly one-fourth that of divorce law reform. In sum, the estimated effect of atmospheric lead exposure on probability of incarceration is not outrageous compared to other studies that analyze incarceration trends. Still, for reasons to be discussed shortly, it is useful to check the strength and validity of the estimates of the primary analysis.

2.6 Supporting Analysis

Several concerns arise from the primary regression analysis. First is the aforementioned assumption in the IV methodology – ASI is assumed to only relate to incarceration trends through atmospheric lead. Second are the limitations of the incarceration variable and state-level analysis. The incarceration variable covers two birth cohorts who are at very different ages in the years of observation. The incarceration variable also is not a perfect proxy for crime, nor can it be absolutely certain that it only includes incarcerated individuals as opposed to other institutionalized persons. Birth state is the most refined geographic indicator of an individual's birth location; however, MSA-level arrest data are also available for years appropriate for leaded gasoline phaseout analysis. Thus, MSA-level analysis can be used to test the validity of the primary results. A third concern relates to the previous issue but is more specific - excluding California from the sample of states dramatically changes the results while removing any other state (so long as California is still included) does not. California has the most individuals by state in the sample and contains nearly all of the highest ASI areas as well as many of the largest declines in atmospheric lead. So, a major alteration of the first-stage results is not too surprising. More troubling is if California had drastically different time trends aside from atmospheric lead declines that relate to incarceration, arrests, or criminal behavior in general. Such differential trends could then be driving the estimated effect of ASI on crime. Graphical analysis is undertaken regarding arrest trends in California to provide additional testing of the primary estimations.

2.6.1 MSA-level Arrests

Examining crime trends at the MSA level can provide additional support for the main findings. The data specifically measure property and violent crime arrests. As with the incarceration variable, the arrest data only count individuals caught committing a crime. Though not perfect, number of arrests does have a very strong correlation with criminal activity (see Lochner and Moretti (2004)). The use of this data avoids the potential issues of the "incarceration" variable not representing prisons or possibly representing non-violent criminals such as drug offenders. Further, measuring crime in the 1980s more accurately accounts for criminal activity of individuals born before the phaseout in years when they became prime crime-committing age. A drawback of this MSA-level analysis is that crime observations are aggregated by MSA and birthplace of individuals is unknown. It is assumed that many individuals who commit crimes do so in their area of birth because factors such as poverty could relate to both criminal activity and non-migration.

MSA crime data come from the National Archive for Criminal Justice Data (NACJD) through the Interuniversity Consortium for Political and Social Research (ICPSR). These data originate from the Federal Bureau of Investigation's Uniform Crime Reports (UCR). The UCR offers good coverage of the United States – 97.4 percent of the U.S. population was represented by counties in the 2010 data set.[26] The unit of observation is the county, and the measurement is number of arrests. Two types of crime are analyzed in this study: property crime and violent crime. Violent crime consists of murders, rapes, robberies, and aggravated assault. Property crime consists of burglaries, larceny, and arson. Number of arrests has been found to have a very high correlation with the number of crimes committed, and is often employed in the crime literature (see Lochner and Moretti (2004), among others). The data are aggregated to the MSA

[26] (Ranson 2014).

for the purposes of this analysis. Summary statistics for the crime per capita variables are included in Table 2.6.[27]

Control variables for the MSA-level analysis include geographic controls as well as a few demographic controls. Population estimates are used to put the crime data into per capita terms. The data also come from the NACJD data set. Specifications were also run which include variables accounting for population characteristics. These data came from the U.S. Census and were acquired through ICPSR.

2.6.2 Regression Analysis of MSA Arrests

For the MSA-level analysis, the first-stage equation is altered accordingly:

$$[3] \quad AtmosphericLead_{MSAit} = \alpha_1 + \delta_1 PostPeriod_t + \delta_2 AirStagnationIndex_i + \delta_3 (AirStagnationIndex \times PostPeriod)_{it} + \gamma W_{it} + \eta_{0it}$$

Where "$AtmosphericLead_{MSA}$" is atmospheric lead at the MSA level, "*PostPeriod*" is a dummy variable taking on "0" for the "pre" period and "1" for the "post" period, "*AirStagnationIndex*" is MSA Air Stagnation Index, "*AirStagnationIndex×PostPeriod*" is the interaction term, *W* represents any MSA control variables, α_1 is a constant, and η_0 is the error term. The dividing year (year t) used for this portion of the analysis is 1980 – atmospheric lead values are averaged for pre (1960 to 1979) and post (1980 to 2000) periods. Since age of individuals and thus years of childhood exposure are unknown, the ranges of lead data used are expanded. The "pre" and "post" periods for lead are 1960 to 1979 and 1980 to 2000. The average lead for years before 1980 provides a general estimate of atmospheric lead exposure for children born before or at the beginning of the phaseout. Averaging lead values from 1980 to 2000 provides an estimate of atmospheric lead exposure for children born after lead levels had been greatly reduced.

[27] One note about the data collection regards the handling of missing crime reports – missing data are accounted for using a different algorithm beginning in 1994 compared to earlier years. Before 1994, annual data for jurisdictions not reporting at least six months of crime statistics were excluded from county totals; however, from 1994 onward, any jurisdiction with some available data could be included in the county totals through weighting or substitution. Further, MSA population for several years in the "pre" period is total population, while in most years MSA population only includes those counties that have associated crime data. Additional details on how the NACJD data sets were created can be found at http://www.icpsr.umich.edu/icpsrweb/content/NACJD/guides/ucr.html#desc_cl

The second-stage equation is

$$[4] \qquad Arrests_{it2} = \alpha_2 + \beta_1 \widehat{AtmopshericLead}_{it1} + \beta_2 PostPeriod_t + \beta_3 ASI_i + \beta Z_{it} + \varepsilon_{0it}$$

"$\widehat{AtmopshericLead}$" is the predicted change in lead levels as instrumented by the "*AirStagnationIndex×Post*" variable, "*AirStagnationIndex*" and "*PostPeriod*" represent the MSA air stagnation average and cohort dummy variable respectively, and Z is the appropriate vector of control variables. The constant here is α_2 and the error term is ε_0. The outcome analyzed is "*Arrests*", measured as the period average of arrests (property crime or violent crime) per one-thousand residents. In some specifications, the logarithm of arrests is used; in such specifications, the logarithm of period average MSA population is included as a control variable.

For the arrest variable, the "pre" and "post" periods are 1980 to 1990 and 2000 to 2010. These periods are matched to their respective counterparts in the first-stage regression. Figures 2.8 and 2.9 show the relationship between atmospheric lead and changes in MSA crime over time. The "pre" and "post" periods for lead are 1960 to 1979 and 1980 to 2000, while the "pre" and "post" periods are 1980 to 1990 and 2000 to 2010 for crime. The period selections account for childhood exposure to lead and the mechanical element of aging into committing crime years after the most-impactful lead exposure occurs. A greater decrease in lead is associated with a larger decline in crime over time (see Figure 2.8). Figure 2.9 shows the relationships between ASI and crime trends that are seen in the data. For property crime, a higher ASI (more air stagnation) is associated with a greater decline in crimes per capita (see Figure 2.9, Left Panel). Based on the graph, ASI and the change in violent crime per capita have little to no relationship (see Figure 2.9, Right Panel).

Regressions were run both using observations for all years of available data and using "pre" and "post" period averages by MSA. Due to the similarity of the results and the greater simplicity of the latter set, only the period-based regressions are included and discussed here. For all of these regressions, standard errors are clustered by MSA. Table 2.7 shows the first-stage results. The dependent variable is MSA-averaged atmospheric lead in $\mu g/m^3$ averaged by period. The relationship between atmospheric lead levels and the ASI interaction term is once again negative and strongly statistically significant.

The reduced-form regressions directly show the relationship between ASI and crime trends, while the IV regressions show the lead-crime trend relationship when lead is instrumented by ASI. The sample analyzed is all MSAs meeting the data requirements, and the years are 1980 through 1990 and 2000 through 2010. The dependent variable is number of arrests (or arrests per capita) for property or violent crime. For most of the specifications, the crime data are in per capita form by thousand residents – meaning a "1" represents one crime per thousand people. To better gauge the direct impact of ASI and atmospheric lead on crime trends, the logged specification uses the logarithm of crime as the dependent variable and employs the logarithm of MSA population as a control variable.

Results for the reduced-form regressions are included in Tables 2.8a and 2.8b. In the property crime regressions (see Table 2.8a), the coefficient estimates are statistically significant at the 1 percent significance level in the basic specification. Based on the results, being in the 95th percentile in ASI implies an additional decrease in property crime of roughly two property crimes per thousand residents compared to an MSA in the 50th percentile of ASI. The estimated negative effect is slightly weaker in magnitude and statistical significance when adding in regional controls (see Column (2)). Specifications using the logarithm of crime as the dependent variable yield intuitively comparable results (see Columns (3) and (4) of Table 2.8a). The violent crime results are generally not statistically significant (see Table 2.8b). The estimated coefficients are negative when using the logarithm of crime as the dependent variable and including population as a control; the coefficient on the ASI-time interaction terms is statistically significant at the 5 percent level in the most basic specification (see Column (3) of Table 2.8b). This estimate suggests that an additional air stagnation period per month would imply a 2.5

percent reduction in violent crime.

Turning to the IV regression results in Tables 2.9a and 2.9b, the impact of atmospheric lead on crime is positive as expected. In the level regressions, a 1 $\mu g/m^3$ decrease in lead implies a reduction in property crime ranging from around 5 crimes per thousand to around 9 crimes per thousand (see Table 2.9a). The estimated coefficients are statistically significant at the 5 percent level in the most basic regression. The violent crime regressions in levels do not yield statistically significant results for the estimated effect of lead on crime (see Table 2.9b). Columns (3) and (4) of Tables 2.9a and 2.9b present regression results for the period-based sample when using logged crime as the dependent variable and including logged MSA population as a control variable. The results for property crime are intuitively comparable to the level results, but violent crime results differ from those for the level regressions. In the logged specification, the effect of lead is positive and statistically significant in the most basic specification (see Columns (3) and (4) of Table 2.9b).

From the most basic results, the elasticity of property crime with respect to atmospheric lead is 0.51, while the elasticity of violent crime with respect to atmospheric lead is roughly 0.32. These estimates suggest that a 100 percent decline in atmospheric lead implies a reduction in property crime of 51 percent and a decline in violent of 32 percent. When adding in regional controls, the elasticity becomes 1.29 for property crime and 0.24 for violent crime. Factoring in the drop in average atmospheric lead from 1975 to 1985, my estimated elasticities would predict a 14 to 26 percent decline in violent crime from 1995 to 2005. Numerous factors can impact crime rates, and the results of this analysis support the hypothesis that childhood lead exposure may be an important factor to consider.

There is support in the literature for the results of the MSA-level analysis in this study. Though different in their analytical framework, studies such as Stretesky and Lynch (2004) and Lersch and Hart (2014) also find the effects of atmospheric lead on property crime to be larger than such effects on violent crime. Several studies in the literature have identification frameworks similar to that in the present study. No previous study uses ASI, but the atmospheric lead results can be compared to some past work. Reyes (2007) performs a panel data state-level analysis using gasoline lead content as an instrument for atmospheric lead. The results show no statistical effect of atmospheric lead on property crime, but the elasticity of violent crime with respect to atmospheric lead is estimated to be 0.8. Mielke and Zahran (2012) investigate the effect of lead emissions in six U.S. cities (San Diego, Indianapolis, Chicago, New Orleans, Minneapolis, and Atlanta) on aggravated assault rates 22 years later. The study finds that a 1 percent decline in air lead implies a 0.46 percent drop in aggravated assault rates 22 years later. As they relate more closely to the literature estimates, the violent crime elasticity estimates are more relevant to this discussion. The estimates in this study imply violent crime elasticity to be around 0.32 in the most basic specification and about 0.24 when controlling for region and region-time effects.

The elasticity estimates in these past studies are higher than those in the present analysis. Some variation is to be expected given the nature of this type of environmental exposure analysis (reliance on monitor data, aggregation over a geographic area, etc.). The difference in results could also be driven by improper identification that causes positive bias in past estimates. The instrument of state lead content of gasoline used in Reyes (2007) is not naturally occurring like the ASI instrumental variable used in the present analysis. Lead content reductions were indirectly due to EPA regulation and directly determined by petroleum companies, but local preferences and conditions would still affect demand for different types of gasoline. Drivers of demand (e.g. socioeconomic status) could also affect or be related to crime. It is also possible that local or state conditions contributed to the practices of the corresponding petroleum companies. If petroleum company behavior before or during regulation was influenced by state or local conditions that relate to crime, this could also bias the results.

The estimated elasticity in Mielke and Zahran (2012) is more comparable to the estimate in the present analysis, but still higher. This could simply be due to their focus on aggravated assault – lead exposure may have a greater impact on assault than on violent crime as a whole. The study is also limited in its sample size of six cities, so results could be higher simply due to the nature of these select urban centers. Further, the identification strategy is to use relevant control variables such as city income and youth population. The lack of an instrumental variable strategy presents greater risk of unobserved variable bias. Maybe an active local government pushed for anti-lead initiatives as well as other measures that improved local living and rendered people less likely to assault each other.

2.6.3 Graphical Analysis: The Case of California

To further assess the strength of the primary results, I graphically analyze these MSA-level data within California. California is crucial to the strength of the instrumental variable strategy in this analysis. This is not too surprising as California MSAs represents much of the upper tail for both air stagnation and atmospheric lead. It is concerning if there are confounding state-level differences for California that are driving the crime results. California has variation in air stagnation within the state, and any potentially confounding state-level difference (e.g. crime policy) should affect all California MSAs– presumably independently of air stagnation. Within California, the lead-ASI relationship is as expected and seen in the full sample – bigger declines in atmospheric lead were seen in higher ASI MSAs (see Figure 2.10).

The graphical relationships between crime trends and the two environmental measures (atmospheric lead and ASI) are generally as expected. These relationships are depicted in Figures 2.11 and 2.12. The decline in atmospheric lead is negatively associated with changes in violent crime per capita for MSAs within California (see Figure 2.11, Right Panel). The decline in atmospheric lead is also negatively associated with changes in property crime per capita in California (Figure 2.11, Left Panel). In California, property crime per capita shows a positive relationship with ASI at the MSA level (see Figure 2.12, Left Panel); however, the expected negative relationship is estimated for changes in violent crime per capita (Figure 2.12, Right Panel).

The regression analysis of the full MSA sample and the graphical analysis of the California MSAs provide additional support for the results in the main analysis. The regression analysis generally estimates the expected relationships between atmospheric lead and crime trends and ASI and crime trends. Often the estimated effects are statistically significant. Graphical analysis of MSAs in California and all MSAs not in California show relationships similar to those seen in the full sample analysis. Further, the inclusion of California appears most important in the first-stage analysis of the relationship between atmospheric lead changes and ASI. This makes sense as California contains most of the highest ASI areas as well as many of the areas with the largest declines in atmospheric lead. The results of the supporting analysis of arrests at the MSA level do not eliminate all potential concerns with the primary analysis; however, they provide strong evidence that supports the main findings.

2.7 Discussion

As touched upon earlier, two elements are critical in the identification strategy of this analysis. The results show that ASI does affect changes in atmospheric lead, validating the first condition needed for this methodology to be appropriate. As for the second condition, the analysis does produce evidence that ASI relates to trends in crime, assumedly through its effect on changes in atmospheric lead levels. This assertion rests on the assumption that ASI is not related to crime trends through other channels.

The supporting analysis discussed in the prior section is in accord with the estimated effects from the main analysis; however, a primary concern with the instrument is that ASI will likely have similar impacts on pollutants other than lead. Indeed, Table 2.10 shows first-stage results using 1980 through 2010 ozone instead of lead. These data are from the U.S. EPA.

Ozone is the primary component of smog and is used here to represent “clean air”. As these data do not extend far enough back to perform the main analysis using ozone instead of atmospheric lead, running such a robustness check is regrettably outside the scope of this analysis. However, the estimated relationship between ozone trends and ASI seen in the data is not nearly as strong as that between atmospheric lead changes and ASI. This could be due to ozone being more impacted by industrial sources of pollution as well as more recent environmental policies specific to ozone.

From a scientific perspective, the health and developmental consequences of lead exposure are more directly relevant to criminal behavior compared to those of other air pollutants (e.g. respiratory issues). Other atmospheric lead studies have found no effect of general air pollution on crime (for example, Stretesky and Lynch (2004)). However, past work outside the lead literature has shown a link between general air pollution exposure and childhood health outcomes such as asthma or adverse birth effects (see Currie, Neidell, and Schmieder (2009), and Sanders (2012), among others) that could relate to later life outcomes (e.g. education). Since educational attainment can relate to crime (see Lochner and Moretti (2004), among others), an indirect relationship between childhood air pollution exposure and criminal activity could exist.

Bounding can be performed to provide a rough estimate of the potential effect of general air pollution on crime. I employ the results of several past analyses to link air pollution to birth weight to educational attainment. In light of its presence in car exhaust, a reduction in emissions over time (though not nearly as great as that for lead), and past usage in the literature, I select carbon monoxide as a representative pollutant for this exercise. Currie et al. (2009) estimates the impact of carbon monoxide pollution on birth outcomes. The study also performs a bounding exercise using results from Black, Devereux, and Salvanes (2007). Black et al. (2007) analyzes the effects of birth weight on an array of outcomes using twin data, and the high school attainment impacts estimated in the study can then be linked to incarceration results in Lochner and Moretti (2004).

An upper bound estimate for an effect of general air pollution (here represented by carbon monoxide) on education will now be derived. Given data availability, the change in carbon monoxide during the 1980s will be used for comparison to the drop in lead during the primary leaded gasoline phaseout years. Currie et al. (2009) estimates that a one part per million increase in carbon monoxide would decrease birth weight by roughly 0.005 percent. From available EPA data, the average carbon monoxide level in the United States decreased roughly three parts per million between 1980 and 1990 with most of the drop coming after 1982. An accompanying change in birth weight based on the Currie et al. (2009) results would then be roughly 0.015 percent. From Black et al., a 10 percent increase in birthweight increases the probability of high school completion by roughly 1 percentage point. So, the national average change in carbon monoxide in the 1980s would imply a 0.0015 percentage point increase in the likelihood of high school graduation. Graduating from high school (a binary variable) is estimated in Lochner and Moretti (2004) to reduce incarceration risk by roughly 0.9 percentage points for whites and roughly 8 percentage points for blacks. This implies that the carbon monoxide drop in the 1980s would increase high school graduation probability by 0.00135 and 0.012 percentage points for whites and blacks respectively.

Recall that the present analysis estimated that a 1 $\mu g/m^3$ drop in atmospheric lead reduced incarceration likelihood by roughly 0.4 percentage points. For this exercise, I will take the 0.012 percentage point estimate as an upper bound for a carbon monoxide effect on incarceration and assume that a general air pollution effect roughly this size was part of the estimated impact of atmospheric lead on incarceration likelihood. So, a 1 $\mu g/m^3$ drop in atmospheric lead during the leaded gasoline phaseout would still reduce incarceration likelihood by about 0.388 percentage points. Clearly, the estimated "general air pollution" effect is a rough estimate using one representative pollutant and also assumes adequate generalizability and correct effect estimation in these past analyses; however, this exercise implies that such an impact of other air pollution on incarceration would be quite small.

2.8 Conclusion

Past studies have linked lead exposure to an array of health concerns and adverse outcomes. In the United States, lead levels in the environment declined substantially during the leaded gasoline phaseout; however, the negative intellectual and behavioral outcomes that may be associated with lead exposure as a child could manifest themselves later in life. Exposure to atmospheric lead could vary greatly based on when and where an individual was born. The literature has found some evidence of an effect of lead exposure on outcomes such as crime; however, endogeneity concerns persist.

The present study addresses the issue of endogeneity through use of an air stagnation instrumental variable. Air stagnation is a natural characteristic of a geographic area that affects atmospheric lead but is not impacted by trends that may be related over time to both atmospheric lead changes and changes in outcomes such as crime. Two major assertions are made in order to employ this methodology – air stagnation should be related to atmospheric lead trends, but air stagnation should not be directly related to crime trends. The first assumption is met as air stagnation and changes in atmospheric lead have the expected relationship, and the relationship is highly statistically significant. Some potential confounders of the second assumption are addressed in several ways. Control variables for region, demographics, and the associated time trends are included in most specifications. Additional analysis produces intuitively comparable estimates when examining crime trends at the MSA level. These estimated relationships are also seen graphically for MSAs within the state of California.

The results of this analysis support past findings that atmospheric lead impacts crime trends while also showing that air stagnation relates to crime trends through its effect on the residence time of atmospheric lead in the environment. ASI is found to have negative effects on incarceration trends. ASI is also shown to have a statistically significant effect on atmospheric lead changes. The results imply that the effect of childhood atmospheric lead exposure on incarceration is positive as expected and statistically significant. The estimated effect is around a 0.4 percentage point decrease in probability of incarceration between cohorts for a 1 $\mu g/m^3$ drop in atmospheric lead. A 1 $\mu g/m^3$ decrease is around the average decline in lead between the two birth cohorts, and roughly 2.5 percent of the sample is institutionalized. So, an average decline in atmospheric lead implies a roughly 16 percent smaller probability of being institutionalized for the "post" cohort compared to the "pre" cohort.

The study finds evidence supporting past scientific findings regarding the health consequences of childhood lead exposure as well as the notion that such developmental impacts could manifest themselves in criminal activity. The results imply both individual and societal ramifications from the immense drop in atmospheric lead caused by the leaded gasoline phaseout. From a policy perspective, the alternative to proactive policy to prevent exposure to harmful substances is to treat exposure consequences ex-post. The results of this and other analyses show major positive effects from reducing lead exposure, and past cost-benefit analysis has shown the leaded gasoline phaseout was highly successful proactive policy. As reducing air pollution remains a top priority in the U.S. and abroad, major environmental policy decisions will continue to impact society. Further, while atmospheric lead exposure is less of a concern today than it had been in the past, troubling lead exposure situations persist (e.g. the water crisis in Flint, Michigan, lead paint in older housing, and more-recent leaded gasoline phaseouts in other nations within the past twenty years). Addressing lead exposure concerns (particularly childhood exposure sources) through policy interventions is becoming an increasingly relevant issue. Additional research into such policy measures and the benefits of reduced lead exposure could be quite useful.

3. Labor Market Impacts from Ozone Nonattainment Status: A Regression Discontinuity Analysis

3.1 Introduction

In 1963, the United States passed the Clean Air Act (CAA) in order to protect its citizens from hazardous air contaminants. In the 1970s, amendments to the CAA were passed to give the legislation more force. Among the more prominent tools created were the National Ambient Air Quality Standards (NAAQS) and the accompanying attainment/nonattainment designations. The NAAQS created air quality standards to which counties were supposed to adhere. Counties in violation of the air quality standards were labeled "nonattainment", while those who met the standard were labeled "attainment". Nonattainment counties are subject to stricter environmental regulation and potential penalties for remaining in violation of the NAAQS, such as mandated technology improvements, penalty fees, or diminished funding from the federal government. The regulations and penalties were heightened in the 1990 CAA amendments, and the NAAQS have been modified for several pollutants since their first inception. A more-detailed account of the NAAQS and nonattainment status is given in the next section.

Nonattainment status and the accompanying regulations are important to analyze for an array of reasons. While there are clearly environmental benefits to cleaning up the air, a stated purpose of the CAA is to protect public health. While societal benefits of reduced air pollution can help validate such policy, the potential effects of air quality regulation on local industry are cause for investigation. The exact nature of these effects (if they indeed exist) is theoretically ambiguous. Households may opt to live away from polluted areas, or they may need to live in a highly polluted area because of job availability. Firms may desire to locate in polluted areas where other firms are located in order to capitalize on agglomeration economies, or they may choose a laxer regulatory environment. Air pollution could be reduced if high abatement costs induce polluting firms to not locate in a nonattainment area; however, reductions in air pollution could also come about as firms clean up their present establishments, or new firms start out with greener technology. With stricter regulations or pressure from local governments, firm costs may increase as they are mandated to adopt new technologies or reform their current production means to be more environmentally friendly. Such changes could result in cutting workers to save on cost.

Reducing the size or concentration of polluting firms would likely have air quality benefits but could hypothetically harm local economies dependent on such industries. On the other hand, while individual polluting firms may be affected, local economies overall may be stable as displaced workers shift industries, or economies simply move away from being manufacturing-based. Additionally, nonattainment status could alter how local governments behave with regard to industry.

This study focuses on ozone regulations and standards because of the emphasis of past literature on ozone effects, the prevalence of counties in nonattainment for ozone, and developments in the realm of regulations and standards for ozone. The sample years range from 2004 to 2011. The main question in this study regards how, if at all, local economic activity is affected by a county being designated nonattainment status during this period. The focus is on employment and establishments in highly polluting industries, but total county employment and establishments are also analyzed for comparative purposes. In answering the main research question, this study employs a regression discontinuity (RD) design. RD design is a technique that can be utilized to determine the effect of a policy or treatment by looking at those just above and just below a threshold that determines treatment. Assuming these entities around the threshold are similar in most regards aside from the treatment (here, nonattainment status), an RD regression will yield an estimate of the local average treatment effect. RD design can improve upon other econometric methods (e.g. ordinary least squares) by better controlling for unobserved differences and reducing omitted variable bias.

This study finds nonattainment status to have a negative effect on employment and establishments in affected industries – specifically highly polluting industries with regard to ozone; however, the analysis finds no statistically significant effects for countywide conditions across all industries. For polluting industries, the estimated impacts of nonattainment status are roughly 24 percent lower employment and about 9 percent lower number of establishments. Several supplemental regressions are performed to investigate the nature of the impact. In order to provide context for the present analysis, it is useful to now describe nonattainment status in greater detail and discuss the past literature.

3.2 Background Information: Nonattainment Status Designation

The NAAQS create a threshold which, in most cases, determines whether a county is attainment or nonattainment for a given air pollutant. A county will be considered *nonattainment* if it fails to meet the NAAQS or is considered a "contributor" to a nearby nonattainment county.[28] A nonattainment county is subject to any accompanying regulations or penalties until it sufficiently cleans up, or a new standard is put in place.

Based on the CAA Amendments of 1990, all states submit a state implementation plan (SIP) when new NAAQS are introduced. SIPs show how a state will attain and/or maintain the air quality standards for different pollutants. The SIP outlines what relevant regulatory actions will be taken (emission control requirements, air quality management programs, etc.). Nonattainment areas require more intensive SIPs than attainment areas, and these SIPs are supposed to be submitted by a certain due date (usually within one to three years of designation). These areas must adopt additional programs in order to demonstrate a commitment to meeting and maintaining the NAAQS. If a state does not submit the necessary SIP for its nonattainment area(s), the U.S. Environmental Protection Agency (EPA) will step in and develop a federal plan that may not be in the best interest of the county and/or state. Further, states/counties could be

[28] https://www.epa.gov/ozone-pollution

subject to losing federal highway funds.

To acquire attainment status, a nonattainment area must meet the air quality standard and develop the proper SIP. Required SIP components for the 1997 NAAQS for ozone include ozone attainment demonstration, emissions inventory, emissions monitoring, transportation control measures, and "reasonably available" control measures and techniques at the county level as well as at the firm level for polluting industries. Reasonably available control measures/techniques refer to "the lowest emission limitation that a particular source is capable of meeting by the application of control technology that is reasonably available considering technological and economic feasibility" and are applicable to existing firms looking to expand operations.[29] New firms are typically subject to meeting the lowest achievable emission rate guideline set forth by the EPA on a case-by-case basis. Failure to meet the NAAQS by the EPA's due date can also lead to penalty fees. For example, several counties in New Jersey were subject to such fees in 2009 for failing to meet the NAAQS by 2007. Fees were assessed on facilities with emissions over the EPA's calculated baseline emissions – the estimated fee was $7,951 per ton of emissions over the baseline.[30]

The initial amendments to the Clean Air Act were passed in 1970 and 1977, with nonattainment designations stemming from the latter set of amendments. In 1990, amendments were passed to the CAA that raised air quality regulations and made penalties for nonattainment harsher for all pollutants. In 1997, the NAAQS for ozone were changed and an 8-hour standard was chosen to replace the previous 1-hour standard. After legal battles, the new NAAQS for ozone were finally officially adopted in 2004. In 2008, the standards for ozone were tightened from 0.08 parts per million to 0.075 parts per million. Again, there were legal troubles, and the new ozone NAAQS began in 2012.

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[29] "NOx RACT Summary". https://www3.epa.gov/region1/airquality/noxract.html
[30] All4 Inc. http://www.all4inc.com/northeastern-new-jersey-nonattainment-penalty-fees-for-ozone

3.3 Literature Review

Past research has investigated such questions of how air pollution regulations, specifically those attached to nonattainment status, affect industrial activity. In line with the theoretical perspectives, the overall results of these analyses have been ambiguous; however, studies have found significant negative effects of nonattainment status on industrial activity aspects such as manufacturing employment, firm location decisions, and firm investment. These effects can differ across industries as well as across types of pollutants.

Environmental regulations, particularly nonattainment status, have been found to affect several key areas.[31] Studies on the potential impact of nonattainment on the economic outcomes discussed in the present study include McConnell and Schwab (1990); Henderson (1996); Kahn (1997); Becker and Henderson (2000); Berman and Bui (2001); Greenstone (2002); List, McHone, and Millimet (2004); Condliffe and Morgan (2009); Lowe and Islam (2009); Cole, Elliot, and Lindley (2009); Walker (2011); Walker (2013); and Kahn and Mansur (2013).

Based on the existing literature, air pollution and environmental regulations could have effects on economic outcomes in various ways. While there is no general consensus in the literature, past research has found negative, statistically significant effects of nonattainment status on manufacturing industries – particularly for firms that are the largest polluters. The literature employs a number of proxies for industrial activity, most notably employment and firm location decisions. Studies such as Greenstone (2002) find effects on employment, while studies like Henderson (1996) see negative effects on industrial firm numbers through the impact of nonattainment status on firm locational decisions. Past research has analyzed variation across industries based on pollution intensity, and ozone is often the pollutant of interest. Studies have examined effects at the plant level as well as at larger geographic levels.

[31] Studies of the impact of nonattainment status on outcomes not researched in this present study include Jorgenson and Wilcoxen (1993); Greenstone (2003); Becker (2005); Altman (2001); Chay and Greenstone (2005); Auffhammer, Bento, and Lowe (2011); Carr (2011); and Greenstone, List, and Syverson (2011).

This study strives to fill several gaps in the literature. First, RD can improve on past estimates of the effects of nonattainment status on labor markets by better controlling for unobservable factors (see Lee and Lemieux (2010), among others). The NAAQS program design fits well with the RD methodology; however, the literature is lacking in studies employing this technique. RD analysis is used in Chay and Greenstone (2005), but it is used to study the effect of county nonattainment status on local housing prices, not industrial activity. Kahn and Mansur (2013 Working) analyzes the potential effect of nonattainment status on industry using RD analysis, but the focus is on geographically adjacent counties rather than those with comparable designation values. Focusing on counties around the designation value threshold helps to isolate the policy impact of the NAAQS comparing counties that are nearly the same in air quality but are regulated differently. Further, RD design also allows for the estimation the effect of distance from the threshold. Firms in nonattainment counties (or the counties themselves) that are very close to attainment could behave differently than firms in counties (or counties as a whole) that have worse pollution and little chance of meeting the NAAQS.

The literature is also limited in its use of recent NAAQS; many of the past studies are performed using data from the 1970s, 1980s, or 1990s. This study analyzes the period of the second-most recent update of the NAAQS and better reflects modern economic conditions. Finally, nonattainment status is a county-level regulation; however, the literature has rarely analyzed the total effect of the regulations on local economies across the United States. This chapter focuses on labor market impacts to examine whether such potential regulatory effects have broader implications than just affecting individual firms. If there are ramifications of nonattainment status for local economies, not just individual plants or specific industries, comparing costs and benefits of air quality regulation becomes a more interesting and necessary endeavor.

3.4 Data

The geographic area of analysis for this study is the contiguous United States, with observations at the county level. The environmental data come from the U.S. Environmental Protection Agency (EPA). The EPA provides nonattainment status designations dating back to 1978. It also provides air quality data for counties with monitors for a given pollutant (ozone, sulfur dioxide, etc.) from 1980 to the present. The EPA uses this monitor data to calculate designation values for all counties. The designation values come via the EPA Green Book. Designation values are calculated differently for different years and pollutants. For ozone, the designation values in 2004 were, except for several unique situations, determined by averaging the annual fourth-highest maximum 8-hour concentration of ozone from 2001, 2002, and 2003. Official designation values were released in 2004 by the EPA. The 1997 threshold (in place from 2004 until 2012) is technically set at .08 parts per million (ppm); however, due to rounding practices, the threshold in practice is .085 ppm.[32] For years prior to 2004 (before the 1997 standard went into effect), ozone attainment was defined as having a maximum hourly concentration above the one-hour ozone threshold of 0.12 ppm for no more than one day per year (see Table 3.1).[33]

If a county was in violation of the standard for a given pollutant or deemed a contributor to a nearby county's violation, that county received nonattainment status. Therefore, some counties that were not directly in violation of the NAAQS for ozone still received nonattainment status. Alternatively, some counties with 2004 designation values above the threshold did not receive nonattainment designation. These counties were in areas that received Early Action Compact (EAC) status. If an area makes a valid case for having its air quality measuring redone, nonattainment designation is deferred to a later date. Counties in the EAC areas then had new

[32] Designation values are typically listed as an integer, meaning the parts per million measurement is multiplied by 1,000 (i.e. the ozone threshold is a designation value of 85).

[33] The original standard was not officially revoked until 2005; however, no counties in my primary data sample were simultaneously in attainment for the 8-hour standard and nonattainment under the 1-hour standard in 2004.

designation values determined based on 2005, 2006, and 2007 ozone readings. If the new designation value met the NAAQS, that area was designated attainment.[34]

For the period analyzed, fourteen geographic areas were EAC.[35] Of these, thirteen met attainment by the required date and were never designated as nonattainment areas.[36] Effectively, their initial designation values were ignored, and they were considered to have met the threshold (i.e. in attainment) conditional on being reassessed a few years later. The only EAC area that was later designated nonattainment was the greater Denver area in Colorado. This study's sample of counties includes nineteen EAC counties that were initially above the threshold but successful deferred and avoided nonattainment status under the 8-hour standard. To determine if such counties are affecting the regression results, robustness checks are run excluding the successful EAC counties from the regressions.

This study utilizes the designation values as well as the nonattainment statuses for counties which had ozone monitors during the period analyzed. 651 such counties meet the air quality data needs of this study. Some counties that have monitors for ozone did not have the needed economic or population data for the purposes of this analysis and were excluded from the sample. The counties in the full sample have an average 2004 designation value of 82.75, so the average county would be just below the threshold.

The economic data at the county level come from the U.S. Census County Business Patterns. U.S. Census data, including but not limited to the County Business Patterns, are the most common source of employment and establishment measures in this particular literature (see Henderson (1996), Kahn and Mansur (2013), among others). The population data come from the 2000 U.S. Census. The economic data are taken as annual averages by county by industry. The outcome variables of interest are employment and number of establishments. For the County

[34] *EPA Greenbook.*
[35] "Geographic area" may represent one or multiple counties.
[36] *EPA Greenbook.*

Business Patterns, most of the industry-level employment data are confidential; however, the numbers of firms per county with given ranges of employment (e.g. five firms with 50 to 99 employees) are available for all counties. In order to estimate industry-specific county-level employment, the midpoints for each range are utilized with the number of establishments in each employment range. The general formula is the countywide sum of the number of firms in polluting industries in each given employment range multiplied by the midpoint for that given range.[37] For example, if county X has two firms with 1 to 4 employees and one firm with 5 to 9 employees, the midpoint estimate would yield 12 employees.[38]

The primary sample of interest contains industrial employment and establishments in highly polluting industries. The standard for "highly polluting" used in this study comes from Greenstone (2002), which considers polluting industries to be those that contribute 7 percent or more of nationwide emissions for a given pollutant.[39] The present study takes the Standard Industrial Classification (SIC) codes provided in Greenstone (2002) and matches them to related North American Industry Classification System (NAICS) codes. The following are broad categories of highly polluting industries for ozone: Printing; Organic Chemicals; Rubber and Plastic; Fabricated Metals; Motor Vehicles; Petroleum refining; Stone, Clay, Glass, and Concrete; Pulp and Paper; and Iron and Steel. All of these industries are classified under NAICS two-digit codes 32 and 33. Summary statistics for the outcome variables for the polluting industry sample are included in Table 3.2.

3.5 Methodology

The main methodology for this study is a regression discontinuity approach. Lee and Lemieux (2010) and Imbens and Lemieux (2008) provide excellent overviews of the technique

[37] The author acknowledges that this process is imperfect. It may miss smaller changes that occur within a range of employment, misestimate the size of employment changes when moving from one range to another, etc.
[38] Measures were also developed using range minima and maxima in place of midpoints. These are of course correlated with the midpoint-calculated estimates, and using such estimates in the regression analysis yields intuitively comparable results. They are thus excluded from the dissertation, but are available upon request.
[39] For ozone, the contributing emissions are either nitrogen dioxide or volatile organic compounds

for readers interested in a more technical discussion. Of the two main variations of RD – sharp and fuzzy – the fuzzy is utilized in this study. The fuzzy RD can be viewed as the two-stage version of the sharp RD. Sharp RD is used when the probability of treatment is 0 or 1, meaning all participants to one side of the threshold are treated while none are treated on the other side. The outcome variable is a function of a binary treatment (here, it would be nonattainment status), the distance of the assignment variable from the threshold, the interaction of those two terms, and any control variables. For example:

$$[5] \qquad Outcome_{it} = \beta_0 + \beta_1 T_{it} + \beta_2 DVD_i + \beta_3 T \times DVD_{it} + \beta_4 Pop_i + \varepsilon_{it}$$

"*T*" would be 1 if county *i* is nonattainment in year *t* and 0 otherwise, "*DVD*" is the 2004 designation value of county *i* minus 85, *T*×*DVD* is the interaction of *T* and *DVD*, and *Pop* is the 2000 census population of county *i*. The outcomes on the left-hand side would be county *i* polluting industry employment or establishments in year *t*. The main coefficient of interest is β_1, which represents the local average treatment effect of nonattainment status. The coefficients for *DVD* and *T*×*DVD* respectively represent the effect of closeness to the threshold for attainment counties (β_2) and the difference between those effects for attainment and nonattainment counties (β_3).

The fuzzy RD is utilized in this study and can be run as a two-stage least squares (2SLS) regression. Fuzzy RD should be used if the probability of treatment is not binary. For example, in this study, some counties below the threshold received nonattainment status, while some EAC counties technically above the threshold did not receive nonattainment status. Further, counties could change designation status over the course of the sample. For a sharp RD to be preferred, all counties below the threshold would initially be attainment and all above the threshold would be (and consistently remain) nonattainment. The instrument for the fuzzy RD is a binary indicator of position around the threshold. Here, the nonattainment status treatment variable "T" would be instrumented by a dummy variable, "D", indicating whether designation value is above

85 (1 if 85 or higher, 0 otherwise).[40] The second stage would then be equation (5) using the instrumented "T" variable from this first stage.

Several important assumptions are tied to regression discontinuity design. The major element of RD is the analysis of a discontinuity in the outcome variable around the assignment variable threshold. The focus is then on observations close to the threshold. As an example, Figure 3.1a shows average employment at the initial designation values. Clearly, there is a positive jump in average polluting industry employment just after the threshold. Since the employment numbers are raw and thus related to population, the jump may simply reflect more polluted areas being more populated. Indeed, when controlling for population and using an employment per capita outcome measure, one sees a drop in average polluting industry employment soon after crossing the threshold (see Figure 3.1b). After this drop, average employment in polluting industries is generally higher in counties with larger designation values. So, there is a discontinuity around the threshold in both graphs, but the direction of the discontinuity appears to be driven by population. The graphs are similar for the establishment data, though the population-controlled discontinuity in the establishment data is not as clean as that for the employment data (Figures 3.2a and 3.2b).

Population will have a direct effect on the variable for employment and possibly a direct or indirect effect on the establishment variable because these variables are in raw numeric form, i.e. a highly populated area is likely to have larger total employment and thus will probably have larger employment within sectors of the economy. Population could be associated with the assignment variable (designation value) and thus could relate to the treatment variable (nonattainment status). RD requires that no omitted variables be correlated with the treatment dummy variable. Highly populated counties are more likely to be polluted and thus are more likely to have nonattainment status. Further, the outcome variables are likely to be higher in

[40] Note that counties that were initially nonattainment but later gained attainment status are given D = 0 for years after attainment status is acquired.

more heavily populated areas as manufacturing is likely to be more prevalent in polluted areas. These issues should become less of a concern as the range of designation values is narrowed so that only those close to the threshold are included. Areas near the threshold should be more similar in terms of local economic environments as well as population. Still, population is used as a control variable in all regressions.

Selection is also a potential issue in RD analysis. It should be difficult for counties to specifically "select" their ozone reading, but the possibility for strategically emitting pollution to garner low ozone readings does exist.[41] While this notion more likely impacts individual plant readings, counties are aware of the threshold, so, in theory, they could make scheduling adjustments to try to get their emissions just under the threshold. Selection issues can be examined by looking at the frequency of counties around the threshold (See Figure 3.3). Counties would prefer to be attainment rather than nonattainment, so selection would be a concern in this study if there were a large concentration just under the designation value threshold. Based on the density of counties around the threshold, this does not appear to be a problem. The peak in the kernel density graph appears to the right of the threshold with a sort of valley occurring just below the threshold.

The baseline panel regression is done with the outcome variables in levels, nonattainment status as the treatment variable, year and region (as defined by the U.S. Census) dummies, and population as a control variable. The optimal bandwidth was found to be 1.6868, which has been rounded up to 2 due to the discrete nature of the assignment variable. With discrete assignment variables, some data away from the threshold is needed.[42] Based on this information, the preferred bandwidth is a designation value range from 82 to 88. These represent counties that

[41] Henderson (1996).
[42] Lee and Card (2008).

are close to the threshold but numerous enough to provide adequate power for the RD analysis.[43] For reference, regressions are also run using a designation value range from 75 to 95 that covers most of the full sample of counties. These results will help show the value (if any) of narrowing the sample of counties to those close to the threshold.

3.6 Regression Discontinuity Results

Nonattainment designation is not perfectly correlated with a designation value of 85 or greater; thus, a fuzzy RD is the preferred approach for this analysis. I include baseline regressions using ordinary least squares (see Table 3.3) and two-stage least squares (Table 3.4). Table 3.5 contains the fuzzy regression discontinuity results for outcomes of polluting industry employment and establishments. The results included are those utilizing the logarithmic form of the assorted dependent variables.[44] Standard errors are clustered based on county. As in the baseline regressions, the RD specifications contain a population control as well as year and region dummy variables. As anticipated, the population effect is positive and statistically significant, while the year effects are all negative and statistically significant. The variable of interest – nonattainment status – has a negative coefficient for both ranges of designation values. The results are stronger in magnitude when the sample is limited to counties closest to the threshold. For nonattainment counties near the threshold (DV range 82 to 88), the estimated treatment effect is roughly 24 percent lower employment in polluting industries for nonattainment counties compared to similar attainment counties. The establishment results are also negative but statistically insignificant.

The treatment effect estimates for employment in the fuzzy regression discontinuity design are generally stronger in statistical significance and magnitude than those for both the OLS and 2SLS baseline regressions. The 2SLS estimated treatment effect is nearly identical to

[43] Samples using similarly narrow designation value ranges around the threshold yield comparable results to the preferred sample.
[44] Regressions using levels as the functional form of the dependent variables yield intuitively comparable results. These are available upon request.

the wider sample RD estimate; however, the RD estimated treatment effect for the narrow range is stronger in magnitude and significance than its 2SLS counterpoint. The establishment results for the fuzzy RD are also larger in magnitude than those in the baseline regressions but remain statistically insignificant, though the narrow sample yields a higher t-statistic than that in the OLS and 2SLS regressions. The larger results in the RD design would imply that omitted variables could be biasing the OLS and 2SLS upwards - the estimated negative impact is smaller. One possible explanation is that counties narrowly in attainment are also shifting away from polluting industries, and, without controlling for exact position around the threshold, the effect of nonattainment status on local industry is not as large when the comparison group is such attainment counties.

The other RD-specific variables are weaker in statistical significance but show the additional effects of designation value. In the broad sample, the estimated coefficient for the distance from the threshold variable is positive, meaning that counties that are in attainment would have higher polluting industry employment (or establishments) approaching the threshold from the left ("Distance Over Threshold" becomes less negative) or moving further from the threshold to the right ("Distance Over Threshold" becomes more positive). This is logical – more pollution (higher designation value) implies greater polluting industry presence. With a narrower sample, the estimated effect of distance from threshold is negative – implying that a higher designation value above the threshold leads to lower industrial employment. This could be driven by counties that were initially nonattainment but became attainment within the sample period. A reduced designation value over time presumably means lowering pollution and could imply that polluting industry presence has gone down as well.

The interaction term coefficient represents the difference between the effect of designation value distance from threshold for nonattainment counties compared to that for attainment counties. The sign is negative and statistically significant for the wider designation value range and becomes positive but statistically insignificant in the narrow sample. In essence, nonattainment status reverses the impact of the distance variable. For the wider designation value range, this implies that, for counties above the threshold, being nonattainment means distance from threshold reduces polluting industry employment and establishments. Areas with greater ozone levels (or polluting firms in those areas) could be subject to stricter environmental regulations, or perhaps these areas simply feel a stronger need to reduce industrial pollution than areas closer to attainment.

I also examine some potential spillover effects. It is plausible that the estimated effects on local industrial employment and establishments could affect the entire local economy. Further, the estimated negative effects of nonattainment status on local industrial employment do not shed light on where potentially displaced workers go. They could remain unemployed or retire, find jobs in different sectors, or migrate seeking employment. Past research provides some insight into these questions. Walker (2013) examines sectoral reallocation following regulation changes due to the Clean Air Act Amendments in 1990. The study finds a reduction in sectoral employment of industries affected by the regulations; however, the study also concludes that transitioning workers most likely exited the affected industry and joined an entirely different industry, typically in the same county as they had previously worked.

To see if nonattainment status affects the local economy across industries, I run regressions using total employment and establishment data for the entire county as the dependent variable. These results are included in Table 3.6. The estimated effects of nonattainment status on county employment are negative but statistically insignificant. Further, subtracting out polluting industry employment and establishments from total county employment and establishments weakens the statistical significance even further (see Table 3.7). For the broad sample, the estimated treatment effects on county employment and establishments in non-polluting industries are actually positive (See Columns (1) and (3)). Based on these results, it does not appear that nonattainment status has a significant effect on total county employment. Instead, it seems that any consequences on the local economy stemming from nonattainment status and the accompanying regulations are primarily limited to polluting industries. Even when focusing around the threshold, there is no statistically significant evidence of countywide effects.

In sum, the results of this analysis imply that any effects of nonattainment status are primarily limited to highly polluting industries. For these highly polluting industries, the RD results show a negative, statistically significant impact of nonattainment status designation on county employment in polluting industries and a negative, statistically insignificant effect on polluting establishment numbers. So far, it is not clear what drives this result. This could be individuals seeking employment elsewhere or in other industries, firms reacting to the cost of regulation by cutting workers, local industry declining in general, or a broad county-level shift away from polluting industries. The impacts of nonattainment status could also differ between initial designation status and persistent designation status. Such questions are analyzed through a series of supplemental regressions.

3.7 Supplemental Regressions

Since counties may change designation status over the course of the sample years, I run several robustness checks. First, it is useful to see if the impact of nonattainment status stems from persistence of nonattainment status, or if initial designation is all that matters. Counties that are on the margin may react differently to initial nonattainment status because it is easier for them to improve and meet the NAAQS. If regulation is driving the results, one may find that persistence of nonattainment status has a more important impact on industry than the initial designation.

To analyze the role of persistence of nonattainment and any differential effects between counties that stay nonattainment and those that eventually have their nonattainment designation removed, I add a “switcher” indicator and interaction term to the main regression analysis. The "switcher" variable is a binary indicator regarding whether a county changed designation over the period analyzed. To clarify, a "0" would indicate that a county was either attainment the entire time or nonattainment the entire time, while a "1" means that the county went from nonattainment to attainment or, for a few areas, from attainment to nonattainment. Roughly 40 percent of the narrow sample switched designation status at some point in the sample years. The interaction term is for the "switcher" indicator and current nonattainment status.[45] Therefore, the switcher/nonattainment interaction term can vary year to year.

For these regressions, the estimated treatment effect is much stronger in both statistical significance and magnitude (see Table 3.8).[46] Of note, the establishment results are now statistically significant at the 10 percent level (see columns (3) and (4)). The interaction term coefficient estimates are positive and statistically significant. Those counties that switched from nonattainment to attainment had much higher polluting industry employment and establishments in years when they were nonattainment compared to nonattainment areas that did not switch.

These results provide some support for the findings in the original RD regressions while shedding light on the nature of the estimated impact. It appears that the persistence of nonattainment status and the accompanying regulations is the driving force behind the estimated effects on industrial employment. The estimated reduction in employment and establishments in polluting industries seen in nonattainment counties is primarily from counties that remained nonattainment throughout the sample. Perhaps lowered employment and establishments in polluting industries is not an attempt to clean up and meet the NAAQS but is instead a mechanical reaction to regulation. Consistently nonattainment areas could also have sources of ozone pollution other than industry (e.g. automobiles) that are more detrimental to air quality than such sources in switcher counties.

[45] Using the "switcher" variable and an interaction with initial nonattainment status yields results quite similar to the main results. Results are available upon request.

[46] Not surprisingly, results are nearly identical when running the original regressions and simply excluding switchers from the sample.

A check of the impact of EAC counties was also performed (see Table 3.9). Recall that EAC counties were a unique case – their designation was delayed several years, so they initially counted as “attainment” under the new standard despite technically having designation values over 85. Excluding these counties from the sample reduces the magnitudes and statistical significances of the results; however, the estimated effects are still comparable. The lower magnitude implies that EAC counties have high employment and establishments in polluting industries. They counted as attainment counties in the main regressions, and attainment counties in the sample were seen to have higher employment and establishments than nonattainment counties in the sample. Thus, the estimated adverse impact on the treated group was larger and more statistically significant. EAC counties had initial ozone pollution comparable to nonattainment counties but were able to avoid nonattainment designation and the associated regulation. The results of this check further imply that environmental regulations impacted polluting industries in nonattainment counties when compared to such industries in similar attainment counties.

3.8 Conclusions

This chapter sets out to test the effects of nonattainment status on local economic conditions, specifically in highly polluting industries. It contributes analysis focusing on the impacts of regulation differences between areas comparable in air quality as well as the role of distance from the designation value threshold. The analysis examines a time period understudied in the literature and employs a regression discontinuity design not previously implemented for this data in this context. The general results align with past research in this area, finding negative effects of environmental regulations on industrial employment and establishments. Supplemental regressions that show a lack of significant countywide spillovers also support past findings in the literature.

Near the designation value threshold, the estimated impacts of the nonattainment treatment variable on polluting-industry employment are statistically significant, and the estimated negative effect of nonattainment status on polluting-industry employment and establishments holds across different sample sizes and regression forms. The RD results are stronger in magnitude and statistical significance than baseline results using ordinary least squares and two-stage least squares. In the main fuzzy RD specification for samples close around the threshold, results indicate that, all else equal, nonattainment status would, on average, lower employment in polluting industries by about 24 percent. Additional analysis yields estimates that imply that the persistence of nonattainment status is more impactful than initial designation. A larger negative effect is seen in counties that remained nonattainment compared to those who started as nonattainment but later became attainment.

Regressions were also performed to investigate potential spillover effects. The results show no statistically significant evidence of any effects of nonattainment designation affecting non-polluting industries nor the county economy as a whole – local economic effects of ozone nonattainment status appear to be strongest in and mostly limited to highly polluting industries.

This study does not analyze or assert whether any of the estimated effects are “good” or “bad”. Future research in this field could focus more on cost-benefit analysis of the NAAQS. Particular attention should be paid to any variance in effects as standards are tightened and regulations and penalties are increased over time. Research could also strive to determine the exact cause(s) of any adverse effect of nonattainment status on polluting industries beyond what was analyzed in the present analysis. For example, firms could be reacting to being regulated, or counties may be pressuring local industry to clean up. In general, as the NAAQS continue to be revised and debated, further research into other potential costs (e.g. wage impacts) and benefits (e.g. health improvements) of nonattainment status regulations would be useful.

4. The Impact of Tradable Permit Program Design on Emissions: Evidence from the United States Acid Rain Program

4.1 Introduction

Over the past few decades, tradable permit markets for emissions have been of particular interest to policy-makers and researchers. In the United States, the most prominent example has been the sulfur-dioxide emissions permit market (also referred to as the Acid Rain Program, henceforth ARP) created by Title IV of the 1990 Clean Air Act Amendments. The goal of Title IV was to roughly halve the 1980 level of sulfur-dioxide emissions by the year 2010. The ARP was implemented in two phases – Phase 1 lasted from 1995 to 2000, while Phase 2 began in 2000. Phase 1 provided allowance allocations to 110 of the dirtiest sulfur dioxide emitting electricity-generating plants, while Phase 2 added in remaining plants with capacities at or above 25 megawatts (MW). The consensus is that the program succeeded in reducing both sulfur-dioxide emissions and acid rain; however, it is difficult to determine how things would have been in the absence of the program or under a different policy. Past research on the matter has been largely observational or theoretical. Tradable permit markets are a potential solution to pollution problems, so further investigating the design, implementation, and outcomes of a real-world example can be beneficial to future policy development.

This study exploits the unique two-phase implementation of the ARP and provides regression analysis of the effect of the ARP on plant behavior. I use a difference-in-differences approach to isolate the effects of being part of the program by comparing Phase 1 facilities (also referred to as "Table A" plants) to Phase 2 facilities. The results of this study indicate that starting out as part of the program mattered - Phase 1 plants more-sharply reduced emissions relative to Phase 2 plants during the Phase 1 period. Further, the timing of phase-in seems to affect how the program ran during Phase 2. Plants only included in Phase 2 show an increase (or smaller decrease) in emissions compared to their Phase 1 counterparts from 2000 on.

This study also investigates the role of initial allocations. Theoretically, initial allocations should not affect efficiency as firms act in self-interest and, depending on cost, meet the efficient abatement outcome by either abating their pollution or trading for permits.[47] This notion relies on several key assumptions (e.g. no transaction costs, no barriers to trade, etc.); therefore, unsurprisingly, this is often not thought to be the case in reality. Indeed, the results of this study show a statistically significant non-zero effect of initial permit allocations on emissions. The nature of this effect appears to differ between phases. In Phase 1, one permit allocation is estimated to reduce emissions by about 0.6, while in Phase 2 this estimate is roughly a one-to-one relationship. Put another way, plants in Phase 1 are estimated to have utilized roughly 60 percent of their permit allocations, while plants in Phase 2 on average utilized nearly all of their allocations. Over-compliance in the early years of Phase 1 is seen in the data and past work, and several explanations have been discussed in the literature.

The discussion will return to the original research and results of the present analysis following an in-depth summary of the design, implementation, and past analysis of the ARP.

4.2 Background Information: Sulfur-Dioxide Regulation in the United States

Sulfur dioxide is emitted during fossil fuel combustion, mostly at power plants and other industrial facilities. Sulfur dioxide is a major environmental concern as it is a precursor to acid rain. The gas can also have adverse effects on human health; it has been linked to assorted respiratory issues such as asthma and bronchoconstriction. Sulfur dioxide is the most troubling of the sulfur oxides – a group of gases that, after combining with other atmospheric particles, can lead to or exacerbate respiratory conditions and/or worsen pre-existing heart disease.[48]
Prior to the Clean Air Act (CAA) of 1963, air pollution regulation was performed exclusively at the state level. After the passage of the CAA, air pollution regulation was radically altered in the 1970s and beyond. The Environmental Protection Agency (EPA) has been at the forefront of all

[47] Callen and Thomas, 112.
[48] "Sulfur Dioxide". U.S. Environmental Protection Agency http://www.epa.gov/airquality/sulfurdioxide/index.html

environmental policy since its formation in 1970. The CCA and its subsequent amendments in 1970, 1977, and 1990 increased the powers of the federal government and tightened air quality requirements. Older plants were not subject to these new regulations, but plants constructed or significantly modified after 1970 were. A sulfur-dioxide standard was first established in 1971. The CAA Amendments created the National Ambient Air Quality Standards (NAAQS) for sulfur dioxide and other criteria pollutants. The NAAQS established air quality standards to which counties were supposed to adhere. Counties in violation of the air quality standards were labeled "nonattainment" and were subject to stricter environmental regulation and potential punishments for remaining in violation of the NAAQS. The NAAQS for all pollutants have become stricter over time, recently being revised in 2008 (with the revisions implemented in 2012).[49]

The 1990 Clean Air Act Amendments again tightened the air pollution regulation for all pollutants. With regard to sulfur dioxide, Title IV of the legislation created a market for sulfur-dioxide allowances with the explicit goal of reducing acid rain. One allowance permitted a plant to emit one ton of sulfur dioxide. Exactly when this emission could occur depended on the permit's "vintage year". The vintage year indicated the first year during which the allowance would cover an emitted ton. For example, a vintage year of 1995 meant a plant could use that allowance to emit one ton of sulfur dioxide in any year 1995 or later. At the end of every year, all plants subject to the ARP needed to have enough valid allowances to cover their amount of emissions for that year. So, to be in compliance, a plant essentially needed to reduce emissions enough to meet its allowance allocation or else purchase allowances from another plant or at auction. Failure to be in compliance resulted in an automatic penalty of $2,000 per ton (in 1990 U.S. dollars).[50]

Plants in the program were initially allocated permits based on a heat input baseline. This

[49] "Air and Radiation". U.S. Environmental Protection Agency. http://epa.gov/air
[50] "Acid Rain Program". U.S. Environmental Protection Agency http://www.epa.gov/airmarkets/progsregs/arp/basic.html

effectively made permit allocations a function of plant size. For Phase 1, a plant's average heat input in millions of British Thermal Units (mmBtu) from 1985-1987 was multiplied by 2.5 pounds of sulfur dioxide per mmBtu of heat input. The multiplier was reduced to 1.2 pounds of sulfur dioxide per mmBtu of heat input for Phase 2. Firms (either electric-generating entities or anyone with an interest in owning permits) could also purchase allowances at auction. For example, allowances could be purchased by a plant that needs to cover its emissions, or by an environmentalist group who planned to simply retire the allowances to reduce pollution.[51] Plants could also earn additional allowances through certain activities such as adopting cleaner energy sources, earlier-than-required emissions reduction, or high expected production growth.[52] These "bonus allowances" were more prevalent in Phase 1, representing roughly 20 percent of total allocations in 1995; however, they were less utilized in Phase 2.[53]

The market was expected to reduce abatement costs. Plants with lower abatement costs would be able to reduce their emissions and need fewer allowances. Plants with higher abatement costs could then purchase allowances as needed. As the program was structured, while an individual plant's emissions could go up, the overall level of emissions would decline. Trade was not restricted by geography or time. Plants could bank allowances to use or trade later, swap allowances with different vintage years, and trade with other plants regardless of geographic region.

The program was executed in two phases. Phase 1 lasted from 1995 through 1999. Plants mandated to participate in Phase 1 were the 110 dirtiest, large electricity-generating plants. Other plants or units had the opportunity to opt-in, receiving allowances but also being subject to the requirements of the program. With compliance being required in 1995, Phase 1 firms needed to make investment decisions in the early 1990s (install scrubbers, switch fuel type,

[51] Israel (2007).
[52] Schwarze and Zapfel (2000).

etc.). Phase 2 began in 2000 as the remaining hundreds of electricity-generating plants with capacity above 25 megawatts (MW) were added. By the end of the decade, the program was irrelevant. Additional regulations greatly interfered with the market, and the success of the program reduced the abatement cost heterogeneity among plants, which made trade valuable in earlier years.[54]

4.3 Successes and Shortcomings of the Acid Rain Program

The literature provides a variety of analyses on the Acid Rain Program and its aftermath, and generally finds that the ARP actually performed better than expected. In addition to EPA research and other non-academic reports (see Burns, Lynch, Cosby, Fenn, and Baron (2011), among others), studies in the academic literature such as Farrell and Lave (2004) and Chestnut and Mills (2005) determine that benefits greatly exceeded costs. The benefits of the ARP were not limited to accomplishing its explicit goal of reducing acid rain. Additional positive consequences include an array of public health and environmental benefits as well an ancillary benefit of reduced mercury levels (see, among others, Chestnut and Mills (2005)). The program also provided better incentive for plants to develop more efficient abatement technology.[55]

Benefits were not only greater than anticipated ex-ante, costs were much lower than initially projected. While lower costs were directly or indirectly due to the program itself, external trends also contributed. Schmalensee and Stavins (2013) attributes much of the cost-reduction to railroad deregulation of previous decades and the increased prevalence of low-sulfur coal. Switching to low-sulfur coal was becoming economically advantageous years before the ARP when the railroads were deregulated in the late 1970s and early 1980s. As rail prices fell, eastern plants could more easily afford low-sulfur coal from the Powder River Basin in Wyoming. Chestnut and Mills (2005) further support this notion – finding that coal switching in the early 1990s was due to economic reasons rather than the ARP. Kumar (2010) concludes that

[54] Schmalensee and Stavins (2013).
[55] Popp (2003).

the ARP made a difference in technological innovation, but the endogenous effect was small relative to exogenous technological change. Regardless of the cause, abatement technology improvements did aid in lowering costs.[56]

Several studies note that the program worked well but still did not minimize costs. Carlson, Burtraw, Cooper, and Palmer (2000) notes that cost-minimization as well as the full potential gains from trade were not realized in the early years of the program. Transaction costs and uncertainty may have contributed to this as well as to the high propensity to bank. Bohi and Burtraw (1997) asserts that transactions were low in number early on and often within firm. Hahn and Stavins (2010 Working) discusses what conditions affect the efficiency of cap-and-trade programs, determining that the ARP was initially hindered by transaction costs.

Other regulations – particularly state regulation of utilities – may also have adversely affected the ARP. Stavins (1998) concludes that both local environmental regulations and state utility regulations affected the performance of the ARP. Fullerton, McDermott, and Caulkins (1997) finds that Public Utility Commissions (PUCs) can affect utility abatement decisions. Hahn and Stavins (2010 Working) asserts that regulated plants were more likely to switch to low-sulfur coal, which was typically more expensive than buying allowances. On the other hand, the study also shows that about half of the plants in the ARP still did not switch to low-sulfur coal even when it was economical to do so. The National Ambient Air Quality Standards (NAAQS) had effects on trade in the ARP as the EPA tried to discourage allowances trades to non-attainment areas.[57]

A few unexpected developments during the implementation of the ARP are investigated in the literature. One important outcome from Phase 1 was over-compliance in the early years of the program as plants greatly reduced emissions. Several reasons for this phenomenon are discussed in the literature. Banking allowances was a crucial component of Phase 1 of the ARP.

[56] Chestnut and Mills (2005).
[57] Henry, Muller, and Mendelsohn (2011).

Plants reduced emissions to comply with the ARP and saved their allowances to ease their transition into Phase 2.[58] The pros and cons of banking in the ARP are debated in the literature. Some studies determine that banking was indeed efficient (see Ellerman and Montero (2002 working)), while others additionally note disproportionate temporal impacts (see Burtraw and Mansur (1999)). While banking implies lower emissions in the present, it also likely means more emissions at some point in the future. Schmalensee, Joskow, Ellerman, Montero, and Bailey (1998) reasons that in addition to banking, investment in scrubbers and commitments to high quantities of low-sulfur coal also contributed to over-compliance early in the ARP. As compliance requirements began in 1995 for Phase 1 plants, investment in meeting such requirements needed to start years earlier. Large investment in abatement technology drove costs down in the short-run, so it was more sensible to reduce emissions early and hold onto allowances for later.

Another interesting facet of the ARP was price dynamics in the allowance market. Allowance prices were far lower than expected, especially in Phase 1. Schmalensee et al. (1998) notes that prices were lower than anticipated because substitute means of compliance were cheaper than expected. Indeed, from a theoretical perspective, prices in the market should equal marginal costs of abatement in equilibrium.[59] So, if abatement costs are lower, prices will be lower. Prices increased and became more volatile in Phase 2 as other regulations and proposed policies interfered with the market adjustment process.[60]

Despite the perceived success of the ARP, several concerns exist that have been addressed in the literature. One question regards the mechanism for the decline in emissions. In theory, assuming total generation on the grid continued to meet demand, plants could have reduced their generation in order to lower emissions. Several studies rebut such a notion.

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[58] Ellerman, Schmalensee, Bailey, Joskow, and Montero (2000).
[59] Callan and Thomas, 269-277.
[60] Schmalensee and Stavins (2012 Working).

Regarding the program in practice, Schwarze and Zapfel (2000) asserts that generation shifting was not a major issue. Further, Schmalensee and Stavins (2013) shows that electric generation went up 25 percent even though emissions went down 38 percent. The potential negative impact from opt-ins in Phase 1 was another concern that has been generally dispatched in ex-post research. Carlson et al. (2000) notes that volunteers could join Phase 1, but these plants could not have average emission rates increase. Ellerman, Joskow, and Harrison (2003) sees that Phase 2 units that opted into Phase 1 were mostly part of a Phase 1 utility. Further, the study concludes that the potential impact on emissions due to opt-ins was negligible.

A few possible negative consequences of the ARP have been analyzed. Concerns over hotspots are often associated with tradable permit programs. The potential exists for tradable permit programs to lead to a concentration of pollution in certain areas, which could disproportionately harm particular communities or demographic groups. Chestnut and Mills (2005) finds that hotspots were not an issue in the ARP; in fact, low-emission areas stayed low while high-emission areas saw the biggest reductions. Ringquist (2011) looks into if and how the ARP transferred pollution to poor areas and/or areas with greatest concentrations of minorities. The study finds no negative effect of the ARP on these types of areas and determines that allowance trading may actually have helped minorities.

Of relevance to the present study, the literature has discussed the potential role of initial allocations in tradable permit programs and lacks consensus as to their possible effects. An implication of the Coase Theorem is that market equilibrium in cap-and-trade is efficient independent of initial allocations; however, conditions such as transaction costs, imperfect information, and extraneous regulation can prevent this result from happening in reality.[61] Grimm and Illieva (2013) produces experimental evidence that initial allocation affects final allocation. Bohi and Burtraw (1997) and Fullerton et al. (1997) attest that initial allocations can

[61] Hahn and Stavins (2010 Working).

affect plant decisions; however, Ellerman et al. (2003) finds no evidence that initial allocations mattered in the ARP.

In sum, this analysis contributes regression analysis regarding both the phase-based implementation of the ARP and the potential role of initial allocations. Few tradable permit programs have been put into practice; so, much of past analysis has been theoretical. The ARP has often been utilized for observational data analysis, but regression analysis can provide further insight. This study adds empirical evidence supporting or questioning previous assertions. Better understanding the potential effects of the design and implementation of the ARP would be beneficial to both academic research and policy design.

4.4 Data and General Methodology

The data for this project come from several sources. The emissions and allowance history data for the present analysis are from the EPA's Air Market Data for the ARP. The emissions data are available every five years from 1980 until 1995, and annually after that. The EPA also provides data on initial allowances as well as allowance transactions. The sample used in this study only includes facilities that were in Phase 1 and/or Phase 2 of the ARP. The data originally represent generating units; however, the present analysis focuses on emissions and allowances aggregated by facility. While a unit-based analysis is possible, the focus of the present analysis is on facilities for several reasons. Effects of the ARP on emissions at the unit-level may represent strategic decisions regarding generation activity, allowance transactions, and/or abatement investment. For example, plants could have chosen to distribute generation activity among their units depending on the characteristics of different units (e.g. fuel type). These are potentially interesting extensions to investigate, but the analysis in this present analysis will only be concerned with how facilities respond to when and to what extent they are included in the ARP.

The emissions data are measured in tons of sulfur dioxide while the heat input is in mmBtu as described earlier. The allocations data are in number of allowances, with one allowance being equivalent to one permitted ton of emissions. Summary statistics by period for sulfur-dioxide emissions (in tons) and heat input (in mmBtu) are included in Table 4.1. Table 4.2 includes summary statistics for the main sample regarding allocations: “Phase 1 Allocations” is the total number of allowances initially allocated to a facility in Phase 1 and “Phase 2 Allocations” is the total number of allowances initially allocated to a facility in Phase 2. The summary statistics are based on plants that actually received allowances (i.e. Phase 2-only plants are not included in the Phase 1 summary statistics). Roughly one-sixth of the primary sample is plants that were included in Phase 1.

For the central analysis, data are aggregated to the facility level to include all units in the data set for that facility. Some robustness checks were run by aggregating heat input and sulfur-dioxide emissions when excluding units that should not be greatly affected by the ARP – those that do not emit sulfur dioxide, and those which use combustion turbines and/or use natural gas as their primary fuel source. It is possible these units received allowance allocations that would have been used at the facility level; so, the allocation aggregations for facilities are maintained across samples.

The establishment of counterfactuals representing plant emissions in the absence of the ARP has been a contentious point in the literature. Previous studies have estimated emissions in the absence of the program using counterfactuals based on heat input changes (Schmalensee et al. (1998), Ellerman and Montero (2002 working)), initial allocations (Henry, Muller, and Mendelsohn (2011)), or performance standards (Henry et al. (2011)). Rather than attempt to compare a tradable permit policy to the absolute absence of any program or an alternative policy such as direct regulation or emissions tax, this study focuses instead on elements of the design and timing of the ARP. Counterfactuals are formed using plants in the other phase of the program as the comparison group.

The basic methodology for the present analysis is a difference-in-differences regression exploiting the two-phase approach of the ARP. A few studies in the tradable permits literature have utilized difference-in-differences estimation. Most similar to the present analysis, Fowlie, Holland, and Mansur (2009) focuses on the Regional Clean Air Incentives Market (RECLAIM) in California – a tradable permit program used to address nitrogen oxide emissions. The study employs both difference-in-differences and propensity-score matching, finding that emissions decreased by 20 percent on average at RECLAIM plants relative to similar non-RECLAIM plants. Busse and Keohane (2008) analyze potential price discrimination by low-sulfur coal shippers resulting from the ARP. Like the present analysis, the study uses Phase 1 and Phase 2 plants as the treatment and comparison groups, determining that Phase 1 plants paid more for low-sulfur coal in the early years of the program.

One facet of the present analysis in this chapter is a simple comparison of plant emissions based on inclusion or exclusion from Phase 1. Phase 1 plants were typically dirtier and larger than those only in Phase 2 were; however, the trends in sulfur-dioxide emissions before the ARP was announced were comparable when controlling for plant size (see Figure 4.1). The trends are not similar when looking only at raw emissions data (see Figure 4.2). Figure 4.1 shows that, when controlling for plant size, Phase 1 and Phase 2-only plants on average display similar trends in emission reduction prior to the start of Phase 1. There is a large drop in emissions for Phase 1 plants in 1995, but then the emissions trends are again fairly similar to those of Phase 2-only plants. For the pre-ARP period, the same abatement technologies would have been available to all plants, and, since the baseline for allocations in both phases is based on 1985-1987 heat input, plants were unable to strategize in this regard. The similarity in emissions trends between plants from different phases presents the opportunity to also analyze if delayed inclusion impacted Phase 2-only plants relative to their Phase 1 counterparts that were already under compliance.

4.5 Empirical Strategy and Results

4.5.1 Acid Rain Program Phase 1 Analysis

Starting in 1995, only plants in Phase 1 – whether volunteers or mandatory participants – were required to comply with the ARP rules regarding allowances matching or exceeding emissions. From 2000 on, all ARP plants were then subject to the ARP rules. Employing different treatment groups can show how the timing of the phases impacted emissions. Plants in the other phase constitute the comparison group. The basic structure of a regression for this portion of the analysis is the following:

$$[6] \qquad Emissions_{it} = \alpha_0 + \beta_1 Phase1_i + \beta_2 Time_t + \beta_3 (Time \times Phase1)_{it} + \beta_4 X_{it} + \varepsilon$$

Equation (6) represents the regression for the Phase 1 analysis. "*Emissions*" represents the sulfur-dioxide emissions in tons at plant *i* in year *t*. For Equation (6), the "*Phase1*" variable is a dummy variable for being in Phase 1 (1 if facility was in Phase 1, 0 otherwise); "*Time*" is a dummy variable for time (1 if year *t* is 1995 or later, 0 otherwise); "*Time×Phase1*" is the interaction of the previous two variables; *X* represents control variables (e.g. heat input); α_0 is a constant term; and ε is the error term. The time period for the Phase 1 regressions is 1980 through 1999 with observations every five years. So, the "pre" period is 1980, 1985, and 1990, while 1995 and 1999 represent the post period.[62] It is expected that being in Phase 1 means higher emissions, emissions are generally declining over time, and emissions are decreasing more rapidly at Phase 1 plants relative to Phase 2 plants. So, β_1 is expected to positive, while β_2 and β_3 should be negative.

Analysis is also performed regarding the role of initial allocations. The general format of the difference-in-differences remains the same but with allocation variables included:

$$[7] \qquad Emissions_{it} = \alpha_1 + \delta_1 Phase1_i + \delta_2 Time_t + \delta_3 (Time \times Phase1)_{it} + \delta_4 (Phase\ 1\ Allocations) + \delta_5 (Phase\ 1\ Allocations \times Time) + \eta$$

[62] Regressions run using 1996 data in place of the 1995 observations yield similar results.

"*Phase 1 Allocations*" is the initial allocations in Phase 1 for a given plant, while "*Phase 1 Allocations×Time*" is the interaction of allocations and the time dummy. Specifications are run including each allocation variable separately and together. As discussed earlier, initial allocations theoretically should not affect emissions as firms trade to reach their efficient amount. However, in reality conditions such as transaction costs and uncertainty will likely lead to a non-zero effect. If one expects a non-zero impact of allocations on emissions trends, the overall effect of allocations should to be positive since one permit gives the right to pollute one ton of sulfur dioxide. Heat input is no longer included as a control since it is directly correlated with allowance allocations. An endogeneity concern exists since most but not all of initial allocations were determined exogenously by historical heat input. Initial allocations are based on the plant's heat input baseline; however, plants may have earned "bonus allowances" that contributed to the initial total. The baseline is calculated using heat input average from several years before the announcement of the ARP, so firms could not have strategized in this regard. However, bonus allowances or auction purchases are based on firm decisions after the announcement of the ARP.

In light of this, a robustness check was performed using the estimated allocations (i.e. the 1985-1987 heat input average multiplied by the phase-appropriate multiplier) as an instrument for actual allocations. The reduced-form results using estimated allocations were nearly identical to the primary results using actual allocations. This implies that the estimated effect is not being driven by such allocations. Still, initial allocations are based on historic heat input, and historic heat input is likely correlated with historic emissions. Effectively, treatment is not entirely random as the Phase 1 plants were on average much larger and dirtier than Phase 2-only plants. The difference-in-differences strategy helps control for such group effects; however, it is still possible that the observed differential trends in emissions are not solely driven by the policy.

Another concern is that the distribution of sulfur-dioxide emissions may not be continuous. For example, emission reduction could be fast and permanent rather than gradual. Facilities may alter banking strategies if there is no margin for substitution – once fuel switching is complete, there is no need for further abatement and thus no decision between abatement and using permits. There could be concentrations of high-emitters and low-emitters rather than a smooth distribution. Figure 4.3 shows the distribution for emissions per heat input by facility. As one can see, the distribution is relatively smooth with a large concentration near zero and a gradual decline in number of plants as emissions per heat input increase.

Finally, other environmental regulations – most notably those accompanying nonattainment status designations – could have been simultaneously affecting these firms. While new air quality standards for sulfur dioxide were not implemented during the time period analyzed, new standards for particulate matter and ozone were.[63] A small percentage of plants were located in counties that were designated nonattainment in particulate matter during the years of the ARP; however, roughly 20 percent of the sample were in ozone nonattainment counties. Looking at plant emissions trends by county attainment status, it does not appear that plants in ozone attainment counties had differential declines in sulfur-dioxide emissions compared to those in ozone nonattainment counties. Therefore, nonattainment status is not addressed in the regression analysis for this chapter.

The present analysis focuses on the difference-in-differences regressions described in the previous section. In all regressions, standard errors are clustered by facility. The results are included in Tables 4.3 and 4.4. Excluding units that were unaffected or hardly affected by the ARP from facility-level aggregation (those discussed in the previous section) does not have a major effect on the results.[64] The following discussion focuses on the primary results that

[63] EPA Greenbook.
[64] These results are excluded from the present analysis but are available upon request.

include all units in the sample.[65]

Using plants in the opposite program phase as the counterfactual, these regressions analyze the potential effects of both inclusion/exclusion from Phase 1 as well as the extent to which a plant is included in the ARP (i.e. the initial allocations). The regression results generally support what has been hypothesized about the ARP and previously observed in the data. For the Phase 1 analysis, the period of interest is 1980 through 1999 with observations every five years (1999 is used instead of 2000 due to the start time of Phase 2). The dependent variable in all specifications is "SO_2 Emissions", which represents sulfur-dioxide emissions in tons by facility. Column (1) of Table 4.3 shows the results from a simple difference-in-differences regression with heat input to control for plant size. "Heat Input" is heat input in mmBtu, "Phase 1" represents the dummy variable for being included or excluded from Phase 1, "PostDummy1" is the time dummy variable, and "Phase1×PostDummy1" is the interaction term. The signs are all as expected, and the coefficient estimates are statistically significant at the 1 percent level. On average, Phase 1 plants emit more sulfur-dioxide than Phase 2 only plants, emissions are lower in the post period (1995 and 1999), and sulfur-dioxide emissions decline after compliance begins at Phase 1 plants relative to Phase 2 plants.

The additional specifications in columns (2), (3), and (4) of Table 4.3 add in variables for actual allocations received and remove "Heat Input" because heat input directly relates to allocations. The variable "Phase 1 Allocations" represents the number of sulfur-dioxide allowances given to a facility in Phase 1 and "Phase 1 Allocations×PostDummy1" is the interaction of "Phase 1 Allocations" and the time dummy, "PostDummy1". Not surprisingly, the number of allocations received relates positively and strongly to emissions. Large plants on average emit large quantities and received large quantities of allowances. The results are most

[65] Another complication in aggregating units to the facility level, including age of facility as a control variable is complicated by units being added years after a facility's inception. Regardless, including an indicator for facility age does not greatly alter the main results. Such results are available upon request.

interesting when including the interaction term. Theory suggests that the effect of allocations on emissions should be zero under ideal conditions; however, the coefficients on "Phase 1 Allocations×PostDummy1" are statistically significantly different from zero. This implies that other factors (e.g. uncertainty) affected firm behavior. When including both allocation terms (see Column (4) of Table 4.3), the results imply that the effects of the ARP on emissions in Phase 1 were primarily driven by allocations and not simply by being in the program. As a check, regressions were run including state fixed effects as well as state-year fixed effects. The results from these regressions are included in Table 4.4. While the magnitudes change slightly, the results are quite comparable across specifications.

If one were to assume that the effect of allocations would not be zero due to various conditions (transaction costs, imperfect information, etc.), one would still anticipate an effect close to one-to-one given that each allocation lets the holder release one ton of emissions without penalty. This is not seen in the Phase 1 results – the average effect of allocations is an increase of 0.611 tons of sulfur dioxide per additional allowance (see Column (3) of Table 4.3). By looking at Column (4), one can see the breakdown of this total effect – the estimated coefficient for "Phase 1 Allocations" is positive as expected while "Phase 1 Allocations×PostDummy1" has an estimated negative impact of about 0.9 emissions per allowance. Once again, the estimated effects are similar when adding in the state and state-year fixed effects (see Table 4.4). This result supports the high amount of banking in Phase 1 – plants held onto allowances while reducing their emissions in order to comply with the ARP.

4.5.2 Acid Rain Program Phase 2 Analysis

The empirical analysis for the Phase 2 sample is analogous to that for Phase 1 except for a few necessary alterations:

$$[8] \qquad Emissions_{it} = \lambda_0 + \gamma_1 Phase2_i + \gamma_2 Time_t + \gamma_3 (Time\times Phase2)_{it} + \gamma_4 Z_i + \mu$$

Equation (8) logically follows the description of Equation (6): the "*Phase 2*" variable is a dummy variable for being only in Phase 2 (1 if plant is in only Phase 2, 0 if in both phases); "*Time*" is a dummy variable for time (1 if year t is 2000 or later, 0 otherwise); "$Time \times Phase2$" is the interaction of the previous two variables; Z represents control variables; λ_0 is a constant term; and μ is the error term. The control variable in the simplest specifications is the 1985 heat input (in mmBtu) used as a proxy for plant size. As in the Phase 1 analysis, other specifications remove the heat input variable and add in allocation-related variables. These additional specifications are analogous to Equation (7) and include a variable for number of allocations and/or an interaction term between number of allocations and the time dummy.

Regressions for the Phase 2 period use annual observations from 1995 to 2009. The end year is chosen for several reasons. Allowance trading was essentially finished by the end of the decade as other regulations supplanted the ARP.[66] Further, the Clean Air Act stipulated a hard cap on allowances starting in 2010, thus limiting total annual emissions.[67] This broke up Phase 2 into two parts with different initial allocations, so 2010 could be seen as the start of a different phase. Thus, 2010 to the present is excluded from this analysis.

Tables 4.5 and 4.6 show the Phase 2 regression results. The column layout is the same as in the Phase 1 regression results table (see Table 4.3). The variable names also follow the same pattern. Plants that were only in Phase 2 are now the treatment group while Phase 1 plants are the comparison group. After compliance began in 1995, Phase 1 plants show similar emissions trends compared to Phase 2 plants. These regressions essentially test the impact of delayed phase-in, and the results appropriately vary slightly from the Phase 1 regressions that tested early phase-in. Across specifications, Phase 2 plants are on average lower sulfur-dioxide emitters, and emissions for all plants decline over time. Compared to Phase 1 plants, Phase 2 plants on average abated less following the commencement of Phase 2. The positive, statistically

[66] Schmalensee and Stavins (2013).
[67] "Acid Rain Program SO_2 Allowances Fact Sheet". U.S. Environmental Protection Agency. http://www.epa.gov/airmarkets/trading/factsheet.html

significant coefficients on the interaction term (Phase2×PostDummy2) implies that emissions at Phase 2 plants were declining over time less than they were at Phase 1 plants. This makes sense intuitively. Phase 1 plants had been under compliance restraints for several years prior to Phase 2 and had already invested in abatement technology. Lower abatement costs meant lower permit prices, and Phase 1 plants had both their Phase 2 allocations and those permits banked from Phase 1. Phase 2 plants capitalized on high supply and low prices in the permit market and could delay abatement investment. Once again, including fixed effects does not drastically alter the results (See Table 4.6).

The main takeaway from the allocation specifications (Columns (3) and (4) of Table 4.5) is that the overall effect of allocations is much closer to a one-to-one tradeoff than the estimates in the Phase 1 analysis were. As in the Phase 1 regression, the allocation variables are statistically significant at the 1 percent level across specifications. One can see in Column (3) of Table 4.5 that the estimated effect of an additional allowance is 0.997 – essentially a one-ton increase in sulfur-dioxide emissions for each permit allocated. This implies that, on average; an additional permit allocation in Phase 2 was utilized for polluting rather than banked. Indeed, Column (4) shows a much smaller negative effect (about -0.3 emissions per allowance) of the allocations-time interaction term than the -0.9 coefficient in the Phase 1 estimation.

4.6 Discussion and Conclusion

The regression results in this study further validate much of what has been advocated in the literature. They provide evidence regarding the impact of certain design elements of the ARP. The results show that the timing of phase-in made a difference, which is not too surprising. More interesting is the implication that initial allocations had a significant effect on emissions. This suggests that the determination of total market permits and the initial allocation of permits are crucial factors in future design of tradeable permit markets.

Permit allocations are seen to affect sulfur dioxide emissions in both phases of the ARP;

however, allocation utilization differs between phases. Under proper conditions (e.g. no transaction costs), permit allocations should have no effect on emissions. In both phases, the results imply a statistically significant non-zero impact. In Phase 1, this impact is roughly 0.6 tons of sulfur dioxide for each permit allocated. Since one permit provides the right to emit one ton of sulfur dioxide, allocations were seemingly under-utilized in Phase 1. Phase 1 saw higher rates of banking and early over-compliance – emission reductions were greater than anticipated given the requirements provided by the program design. Abatement was not as costly as expected, so plants on average were able to reduce emissions without acquiring more permits or utilizing their full initial allocation.

Banking is seen in the data but the reasons for banking cannot be determined with certainty - several mechanisms could be involved in this scenario. Plants are making decisions ex-ante amid uncertainty regarding the permit market as well as their own plant operations. If plants expected permit prices to rise, abating in the present and saving excess permits for later would be a rational decision. Uncertainty over prices could also have led to banking if plants were interested in arbitrage – saving cheap permits in the present to sell later when permits increase in value. With abatement relatively inexpensive, plants had little problem reducing emissions. With regard to the market, this meant there was low demand for additional permits and thus few trade partners available. Plants in Phase 1 knew that Phase 2 was on the horizon, and that allocations per plant would be reduced. Given uncertainty over future output and abatement costs, saving presently unnecessary permits for later would be a good safety net to have.

In Phase 2, the results show a nearly one-to-one relationship between allocated permits and sulfur dioxide emissions. With a tightened allocation rule, plants received fewer allocations than under Phase 1 rules; however, plants included in Phase 1 still had previously banked permits. Plants in Phase 2 did not over-comply as plants had done early in Phase 1. Indeed, little banking was done early in Phase 2 as plants fully utilized their permit allocations. Additional pollution was permissible given the banked permits from Phase 1 available to be used by the holding plant or purchased by a different plant. Indeed, the emissions trends show a leveling off and even some slight increases in emissions in the early 2000s (see Figure 4.1). This calls to mind the potential for banking in permit programs to lead to temporal inequality (see Burtraw and Mansur (1999)). While the levels of emissions were still significantly lower than years past, permit banking affected the trend in emissions. From 2000 to 2005, emissions were greater than annual allocations; however, there was perfect compliance from 2005 onward.[68]

The general goal of the ARP was to reduce sulfur dioxide emissions (and by extension, acid rain), and this goal was nominally accomplished. With an ever-increasing need for air pollution policy, it is helpful to dissect the ARP to determine what worked and what could be improved. One criticism of the program relates to a lack of built-in flexibility regarding the emissions cap (see Siikamäki et. al (2012)). As plants initially over-complied, the emission reduction targets were exceeded; however, the design of the program did not allow for an update of the standards. The rate of emission reduction slowed and banked permits from Phase 1 had additional ramifications in Phase 2 of the program. It is easy to pick apart the program ex-post; however, ex-ante it was not known that abatement would inexpensive and that there would be such an excess of permits. There are tradeoffs at work – banked permits may have influenced emission reductions in Phase 2, but the reductions in Phase 1 may not have been as large without the option to bank unused permits. Giving regulators the ability to update permit allocations based on program performance creates additional uncertainty for the regulated firms to consider. One compromise could be gradual tightening of allowed emissions of which regulated firms receive some advanced notice. Perhaps the emissions limit will be reduced each year; however, the exact amount of the reduction is announced at least one year in advance. That way,

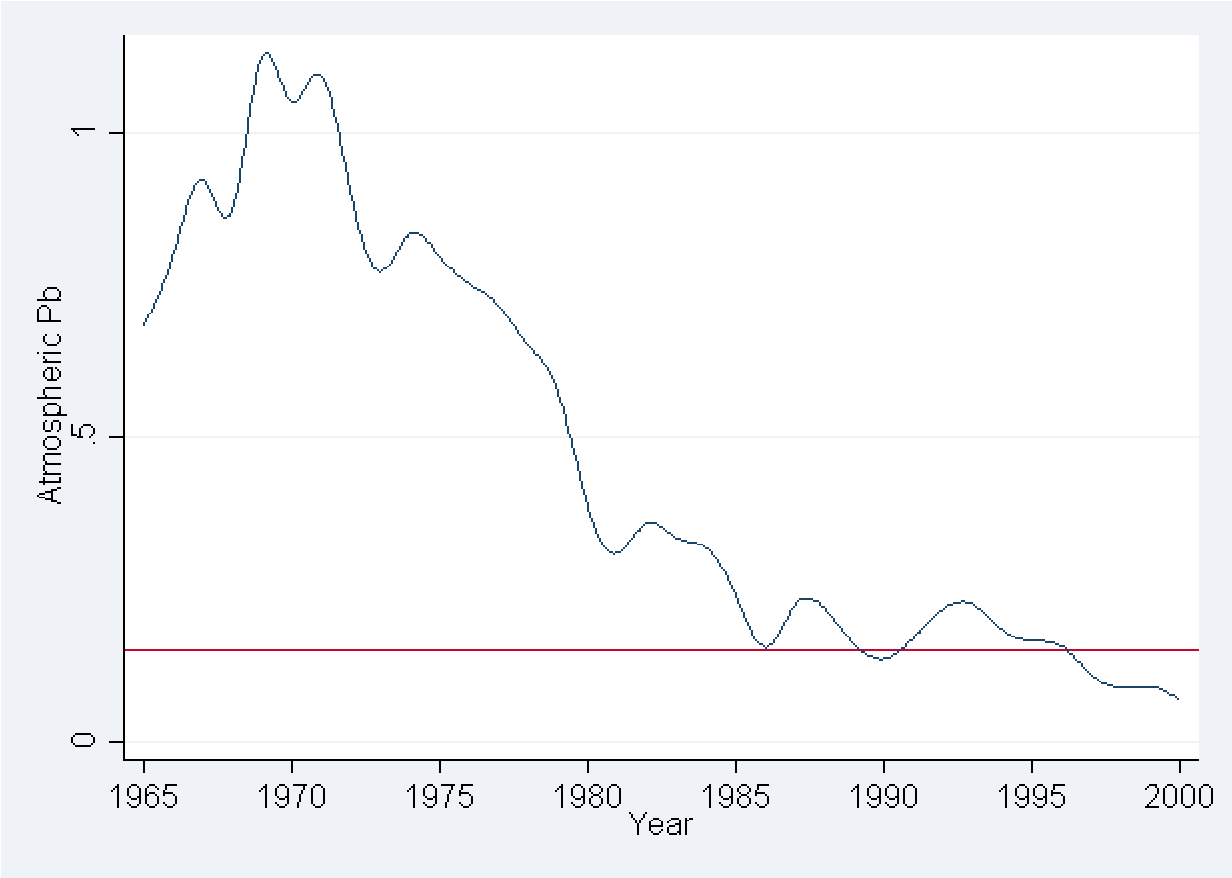
[68] Burns et. al (2011)

regulators can further reduce the threshold if it seems feasible and firms can more efficiently plan regarding abatement, trading, and banking decisions.

The basic results in this present analysis provide interesting evidence regarding the effects of the ARP's design and implementation. The generally positive view of the ARP has led to it often being an example in proposing new tradable permit market policy. The results of the present analysis have some tradeable permit policy implications. It appears that, as acknowledged in past work, uncertainty had an effect on Phase 1 facilities. This uncertainty may have regarded future abatement costs, permit prices, the lifespan of the ARP, etc. The prevalence of banking may be a manifestation of such concerns. With specific regard to the ARP, the two-phase approach has several ramifications. Plants in Phase 1 invested in abatement more and utilized permits less than plants added in Phase 2. The design of the market (the option to bank permits, the number of allocations, price determination, etc.) will have major consequences on abatement as well as permit trading. Tradeable permit markets can be an efficient policy solution to pollution problems; however, design elements must be carefully considered.

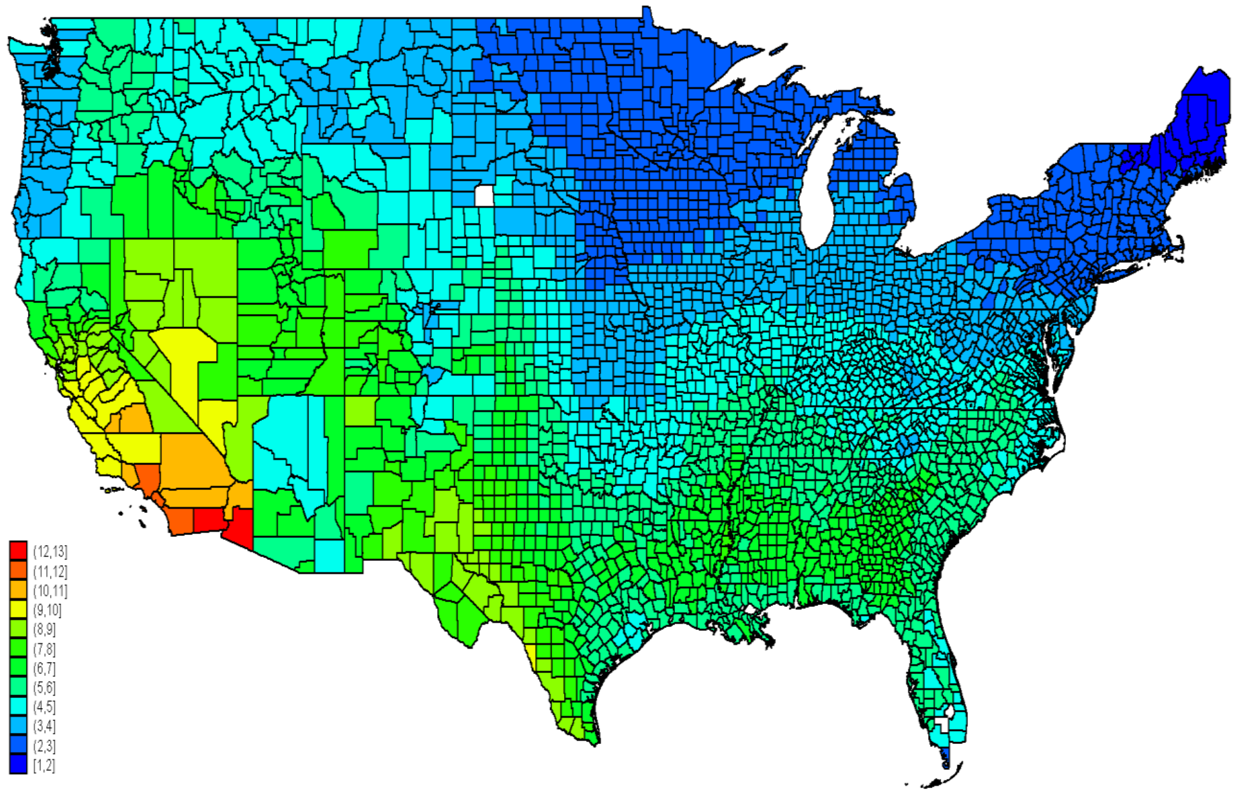
Figures and Tables

Figure 2.1: Avg. Atmospheric Lead Level in the United States for Years 1965-2000



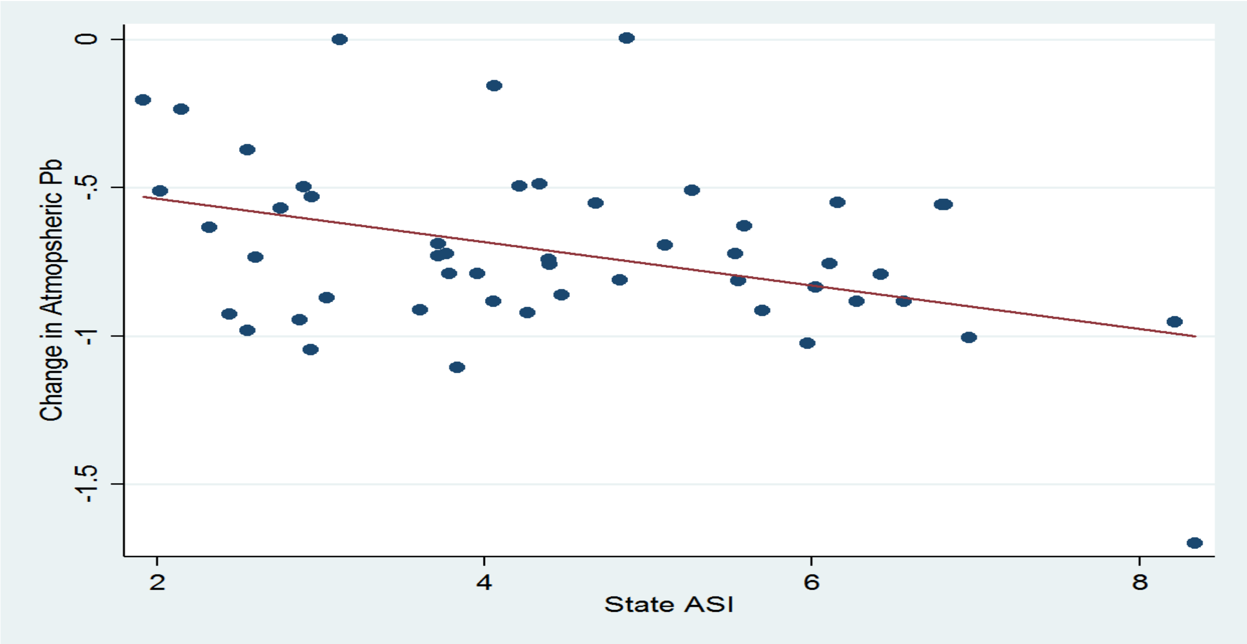
Notes: "Atmospheric Pb" is county monitor readings for lead averaged by year. The horizontal line represents the current National Ambient Air Quality Standard for Lead of 0.15 μg/m^3.

Figure 2.2: Air Stagnation Index by County



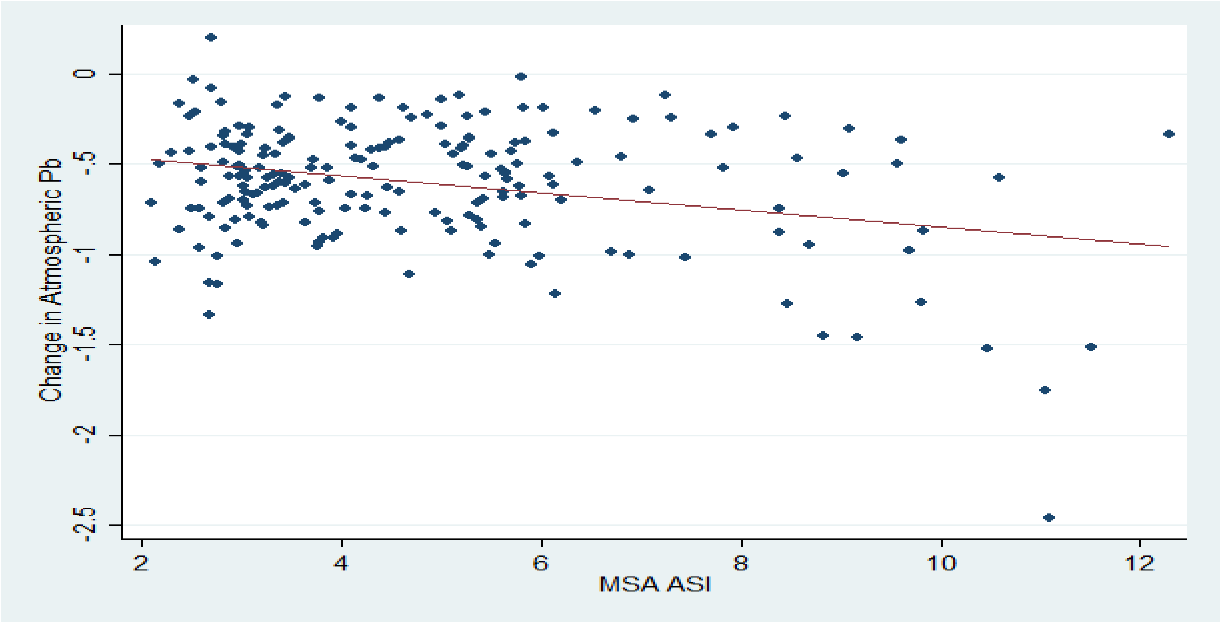
Notes: ASI is average air stagnation periods per month. The map is divided into discrete bins covering the full range of ASI values at the county level. Red represents the most-stagnant areas while blue represents the least. Mapping shape files courtesy of NOAA.

Figure 2.3: Change in State Atmospheric Lead by Air Stagnation



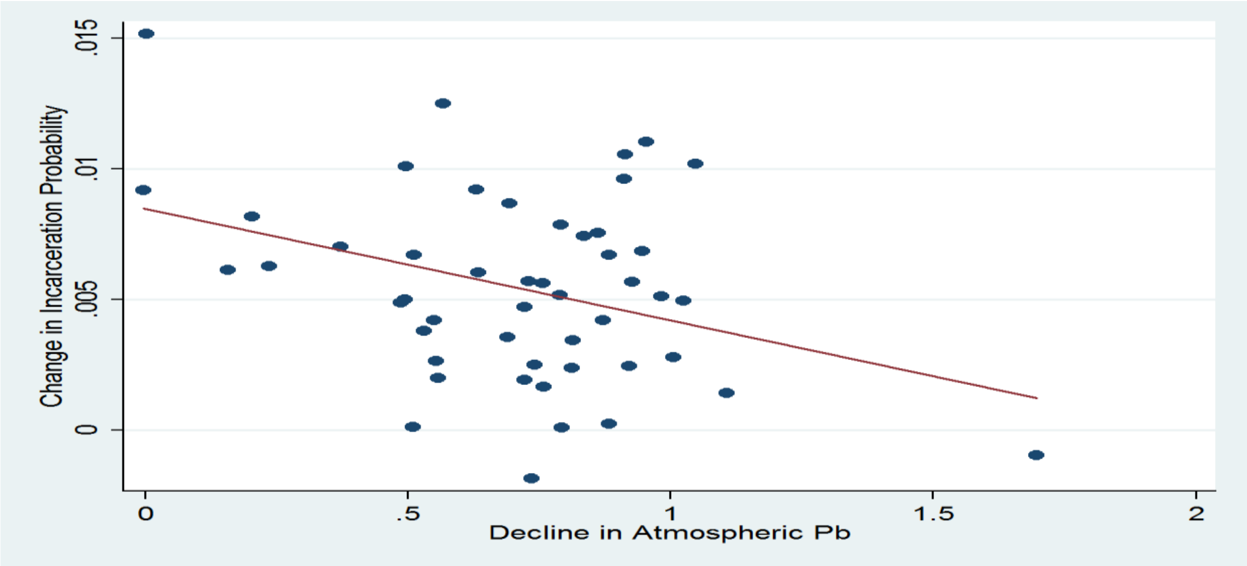
Notes: Unit of observation is the state. The X-axis (State ASI) is the Air Stagnation Index, specifically the average number of stagnation periods per month from 1973 to 1997. Lead and ASI are both averaged weighted by county population. The Y-axis (Change in Atmospheric Pb) represents the change in the average atmospheric lead 1965 to 1974 and the average for 1980 to 1989.

Figure 2.4: Change in MSA Atmospheric Lead by Air Stagnation Index



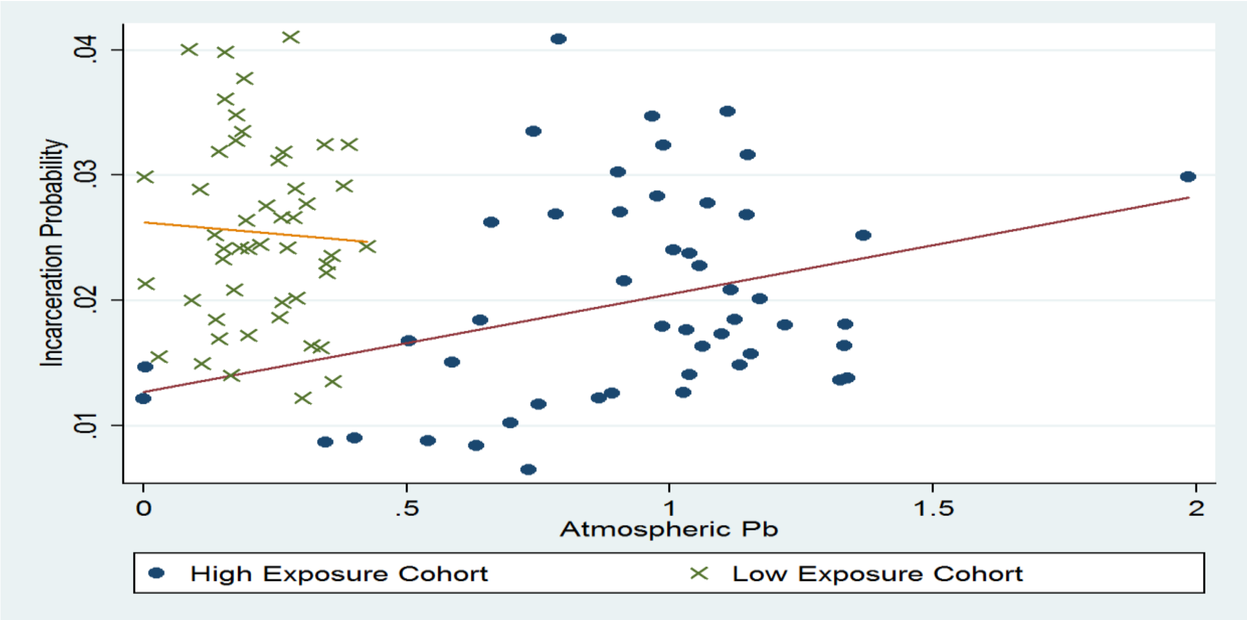
Notes: Unit of observation is MSA. The Y-axis (Change in Atmospheric Pb) compares the average atmospheric lead by MSA for 1960 to 1979 with the MSA average for 1980 to 2000. MSA ASI is the Air Stagnation Index for a given Metropolitan Statistical Area, specifically the average number of stagnation periods per month from 1973 to 1997.

Figure 2.5: Change in State Incarceration by State Decline in Atmospheric Lead



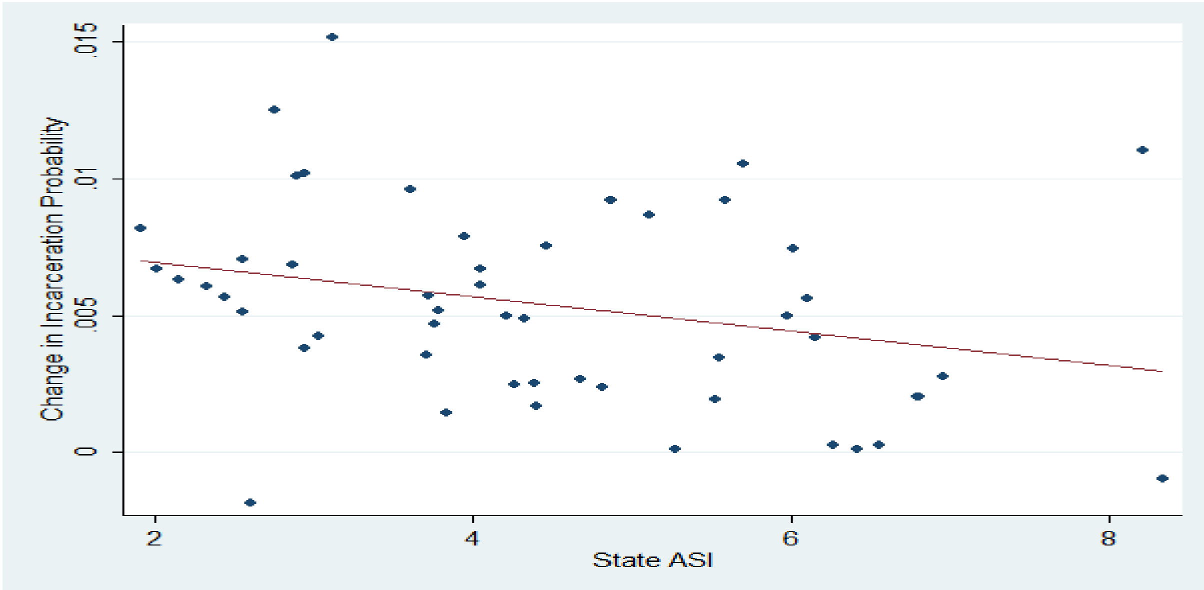
Notes: Incarceration is "0" if not living in an institution in the current survey, and "1" if institutionalized. The Y-axis (Change in Incarceration Probability) is the difference between the cohort averages in the sample for "Incarceration" by state. The decrease in lead compares the average atmospheric lead 1965 to 1974 with the average for 1980 to 1989, where the state averages are weighted by county population. Lead is measured in $\mu g/m^3$.

Figure 2.6: Incarceration by Birth State and Cohort Atmospheric Lead



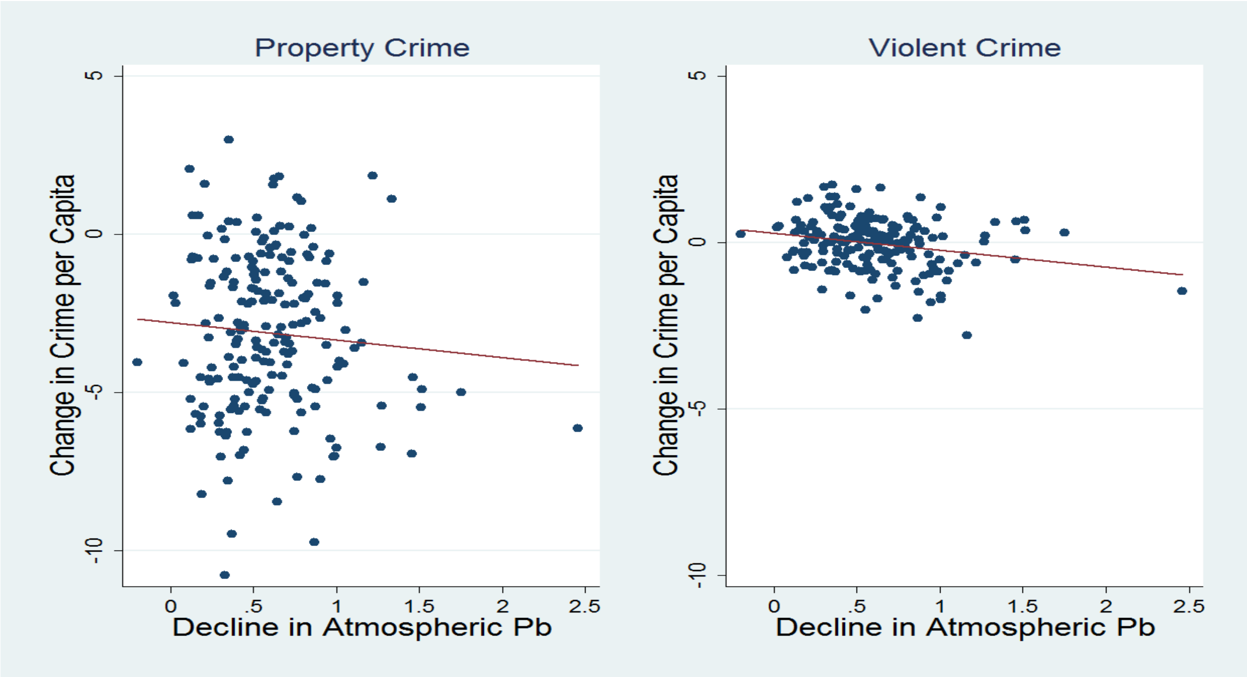
Notes: The Y-axis (Incarceration Probability) is the sample average for "Incarceration" by state by cohort. Incarceration is "0" if not living in an institution in the current survey, and "1" if institutionalized. Atmospheric Pb is averaged by cohort and state weighted by county population. "High Exposure Cohort" consists of individuals born between 1965 and 1969; "Low Exposure Cohort" consists of individuals born between 1985 and 1989. Lead is measured in $\mu g/m^3$.

Figure 2.7: Change in State Incarceration by State Air Stagnation



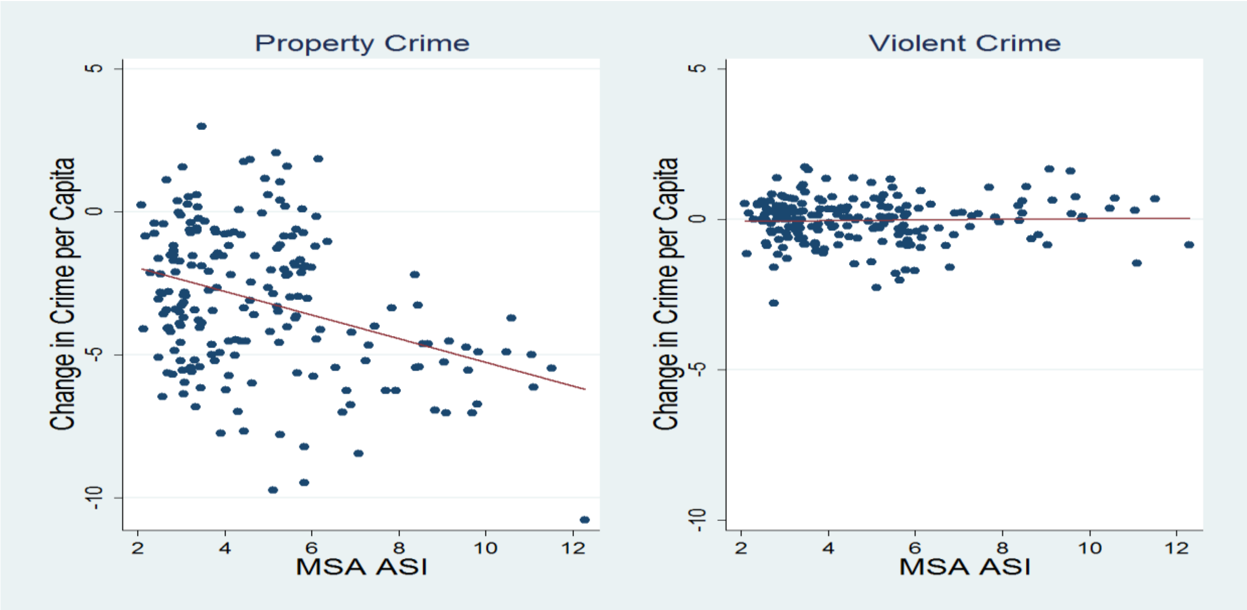
Notes: "Change in Incarceration Probability" is the difference between cohorts in the sample average for "Incarceration" by state. The incarceration variable is "0" if not living in an institution in the current survey, and "1" if institutionalized. ASI is the Air Stagnation Index, specifically the average number of stagnation periods per month from 1973 to 1997. The state average is weighted by county population.

Figure 2.8: Change in MSA Crime 1980s to 2000s by Change in Atmospheric Lead



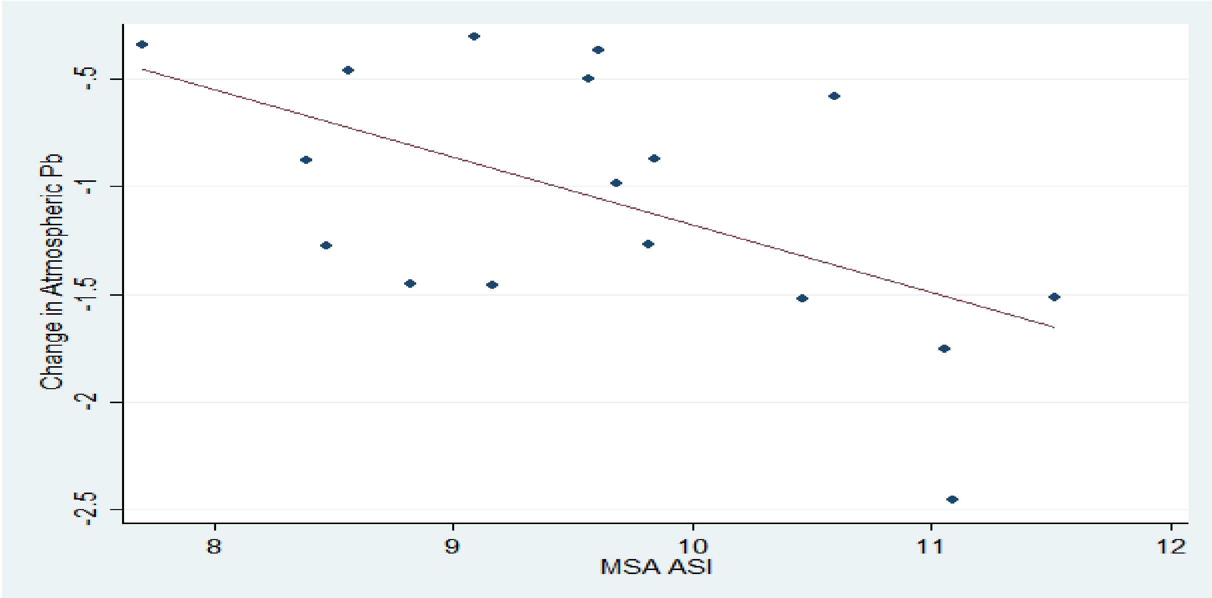
Notes: The unit of observation is MSA. The variable on the Y-axis is the change in the number of crimes per 1,000 residents comparing the 1980-1990 average and the 2000-2010 average. Change in lead compares the average atmospheric lead 1960 to 1979 with the average for 1980 to 2000. Lead is measured in μg/m^3.

Figure 2.9: Change in MSA Crime 1980s to 2000s by Air Stagnation Index



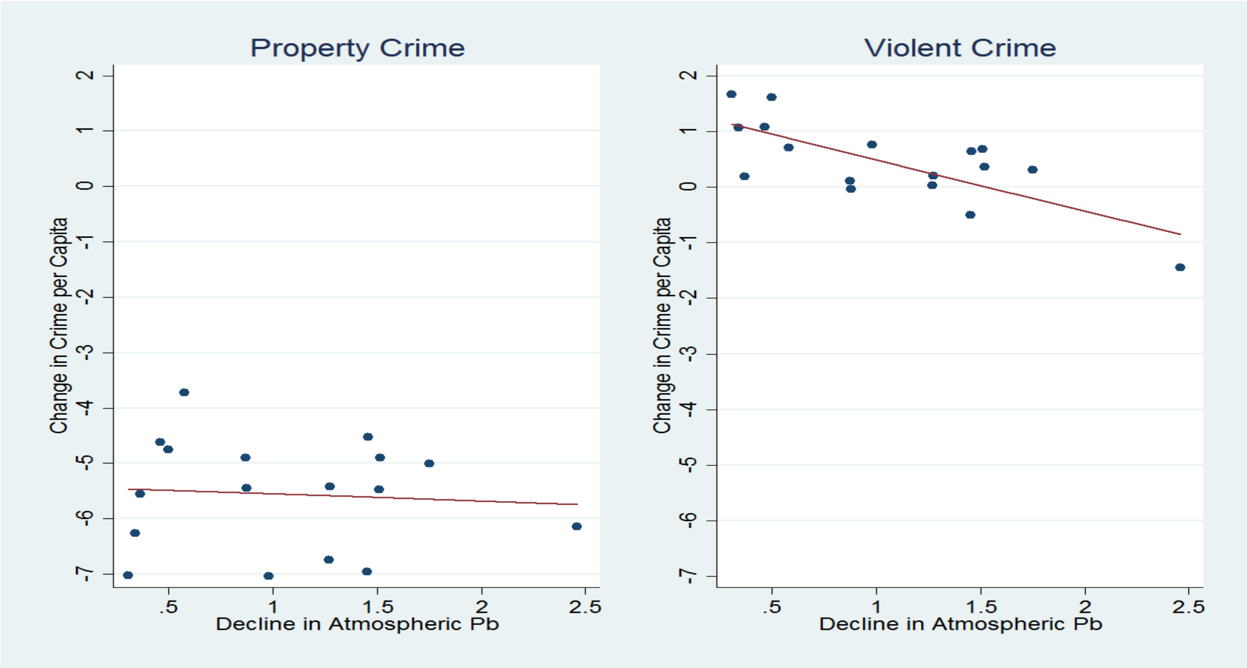
Notes: The unit of observation is MSA. The variable on the Y-axis is the change in the number of crimes per 1,000 residents comparing the 1980-1990 average and the 2000-2010 average. The left panel is property crime while the right panel is violent crime. ASI is the Air Stagnation Index, specifically the average number of stagnation periods per month from 1973 to 1997.

Figure 2.10: Change in California MSA Atmospheric Lead by Air Stagnation Index



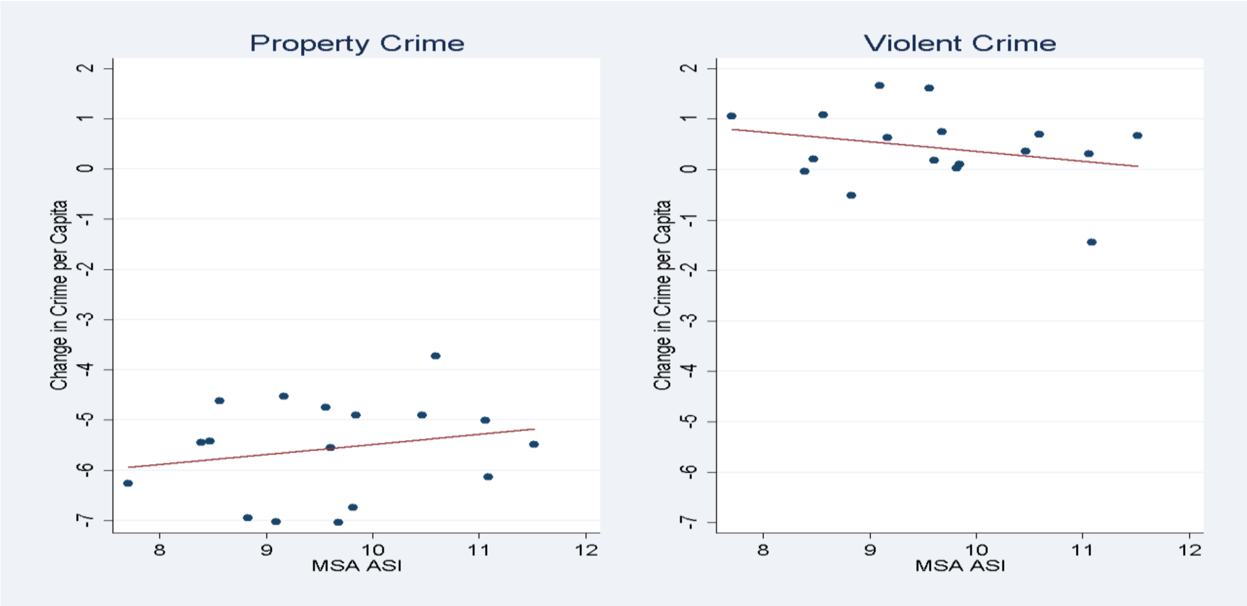
Notes: Unit of observation is MSA. “Change in Atmospheric Pb” compares the average atmospheric lead 1960 to 1979 with the average for 1980 to 2000. “MSA ASI” is the Air Stagnation Index, specifically the average number of stagnation periods per month from 1973 to 1997.

Figure 2.11: Change in California MSA Crime by Decline in Atmospheric Lead



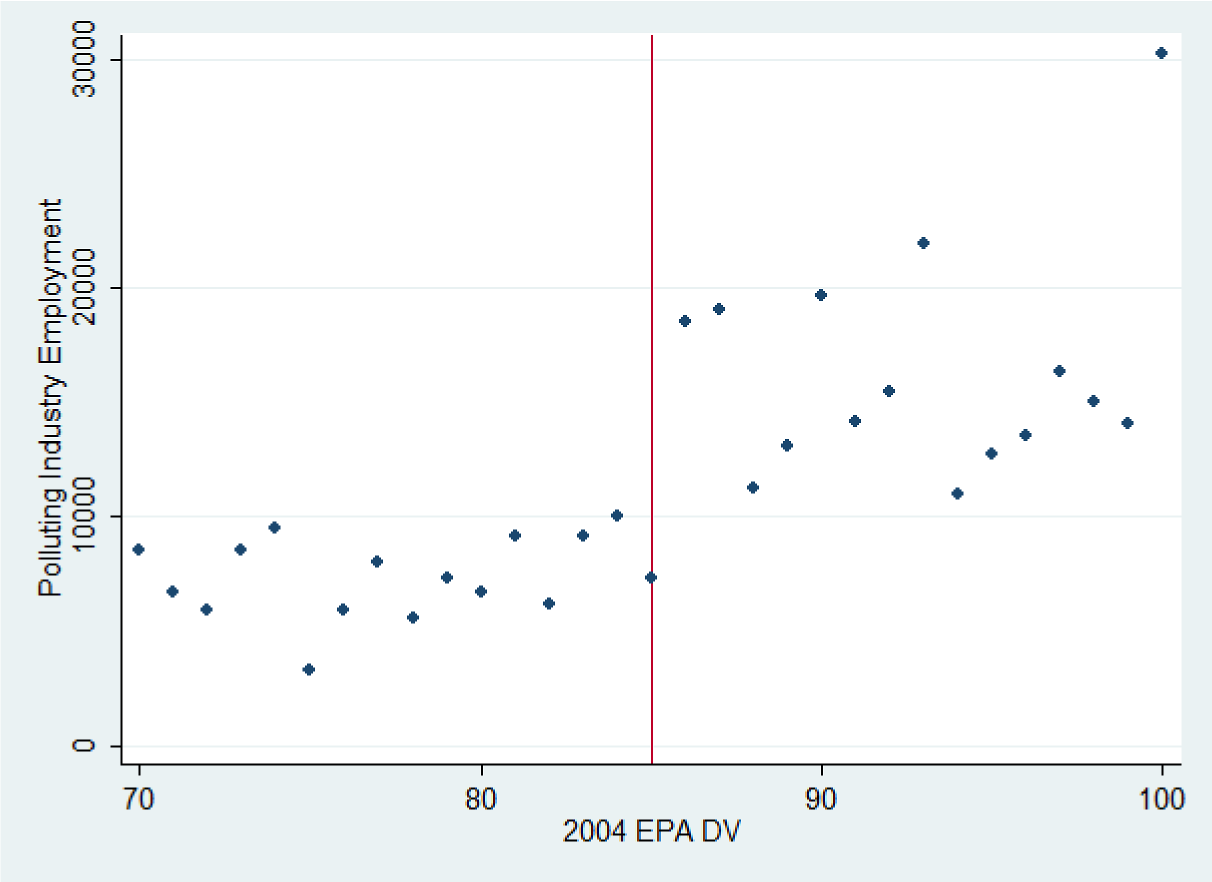
Notes: The unit of observation is MSA. The Y-axis is the change in the number of crimes per 1,000 residents comparing the 1980-1990 average and the 2000-2010 average. The left panel is property crime while the right panel is violent crime. "Decline in Atmospheric Pb" compares the average atmospheric lead 1960 to 1979 with the average for 1980 to 2000. Lead is measured in µg/m^3.

Figure 2.12: Change in California MSA Crime by Air Stagnation Index



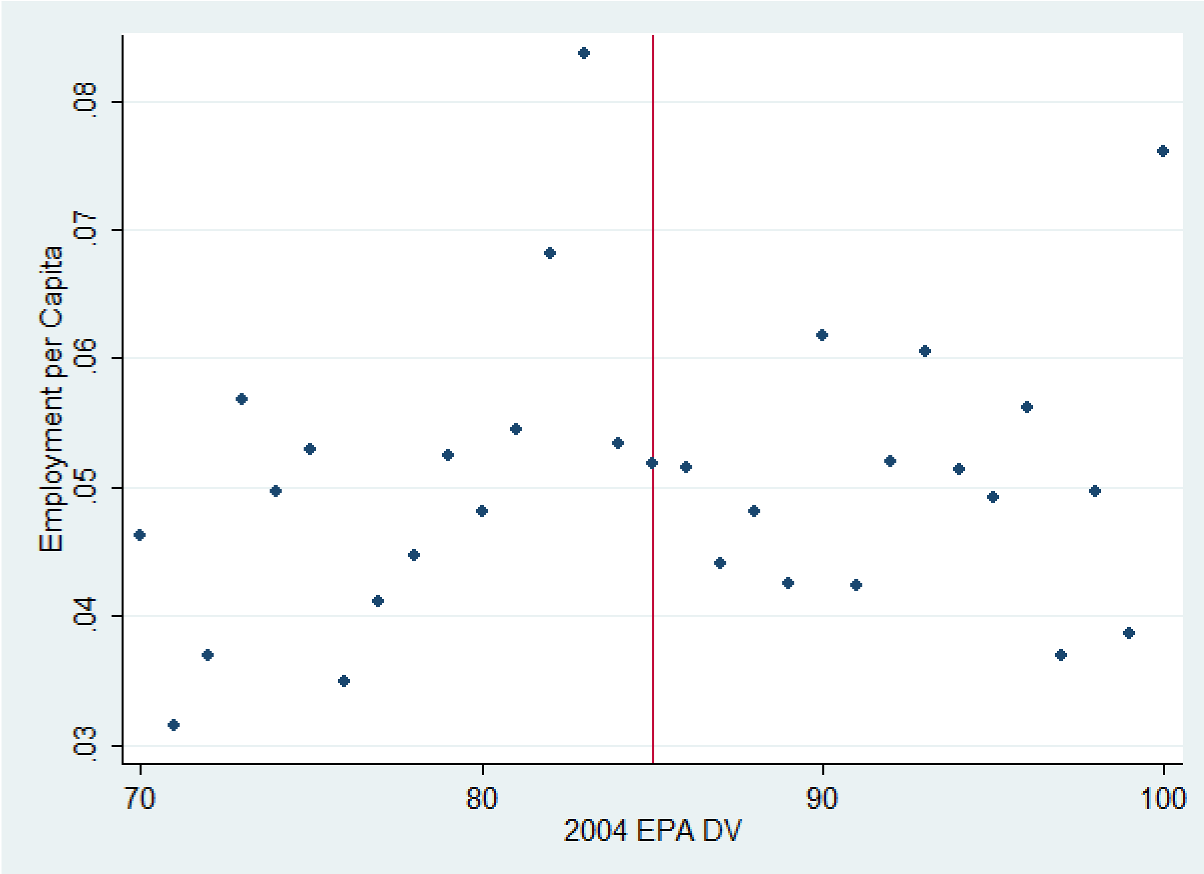
Notes: The unit of observation is MSA. The Y-axis is the change in the number of property crimes per 1,000 residents comparing the 1980-1990 average and the 2000-2010 average. "MSA ASI" is the Air Stagnation Index for an MSA, specifically the average number of stagnation periods per month from 1973 to 1997.

Figure 3.1a: Average Polluting-Industry Employment by EPA Designation Value



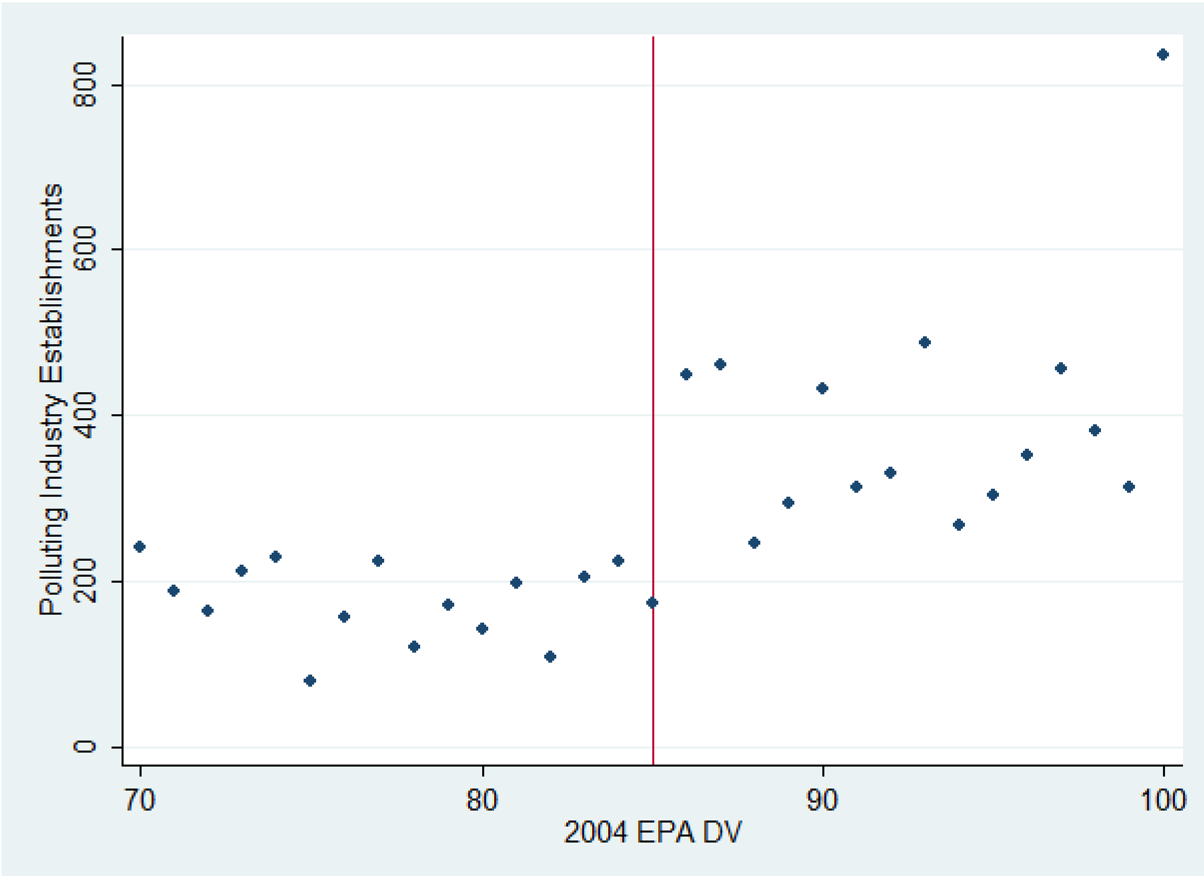
Notes: "EPA DV" represents 2004 nonattainment designation value for ozone. The sample was narrowed to provide a better view around the threshold. The attainment threshold is at 85 and represented by the vertical line. "Polluting Industry Employment" is average county employment in highly polluting industries from 2004 to 2011.

Figure 3.1b: Average Polluting-Industry Employment per Capita



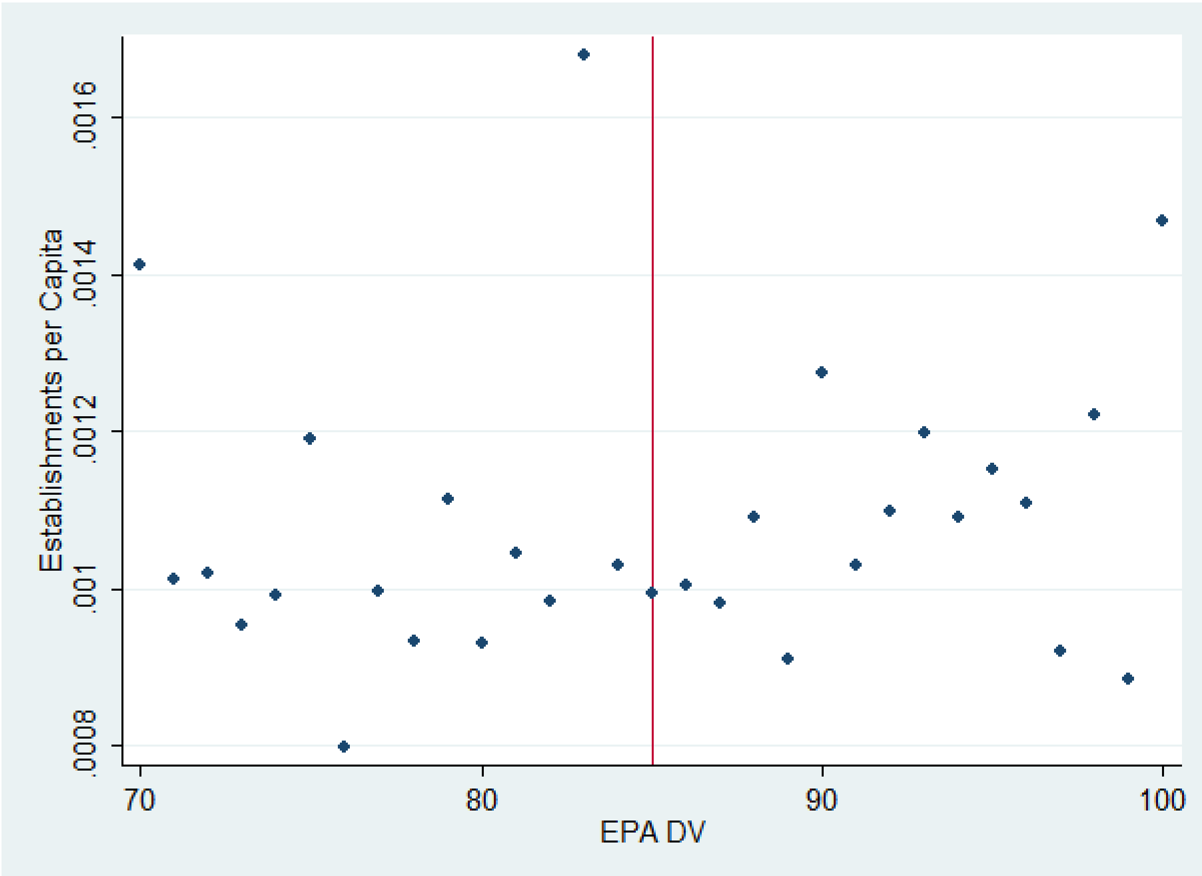
Notes: "EPA DV" represents 2004 nonattainment designation value for ozone. The sample was narrowed to provide a better view around the threshold. The attainment threshold is at 85 and represented by the vertical line. "Employment per Capita" regards average employment in highly polluting industries from 2004 to 2011 divided by county population.

Figure 3.2a: Average Polluting-Industry Establishments by EPA Designation Value



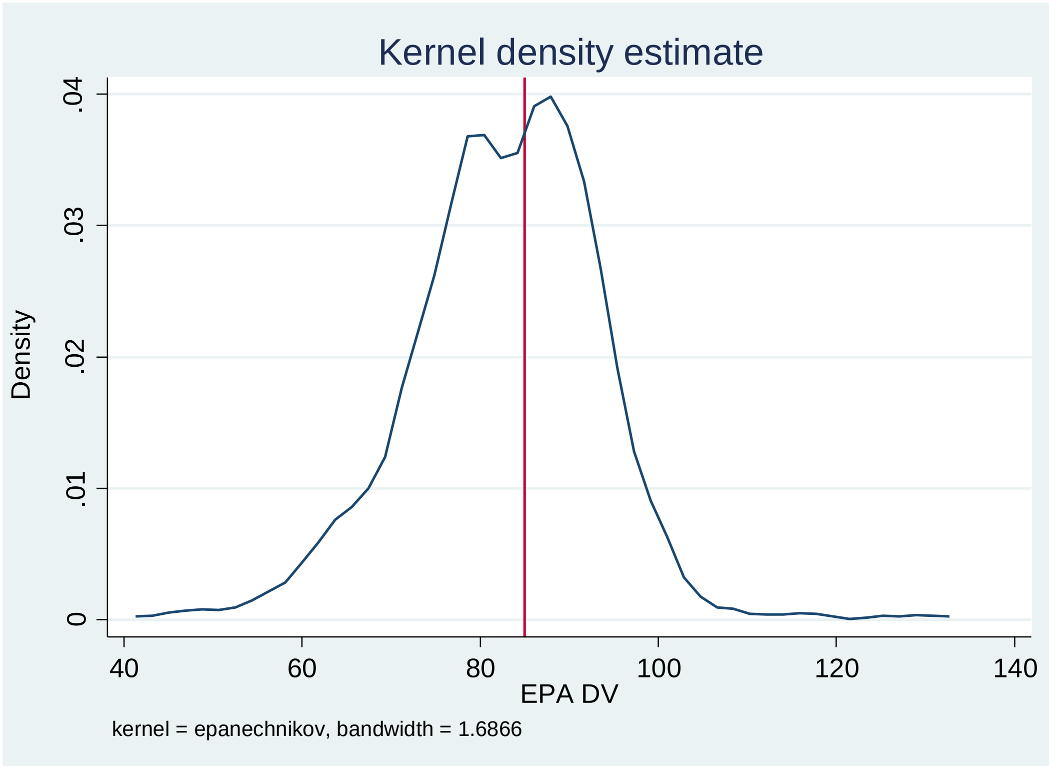
Notes: "EPA DV" represents 2004 nonattainment designation value for ozone. The sample was narrowed to provide a better view around the threshold. The attainment threshold is at 85 and represented by the vertical line. "Polluting Industry Establishments" is average county establishments in highly polluting industries from 2004 to 2011.

Figure 3.2b: Average Polluting-Industry Establishments per Capita



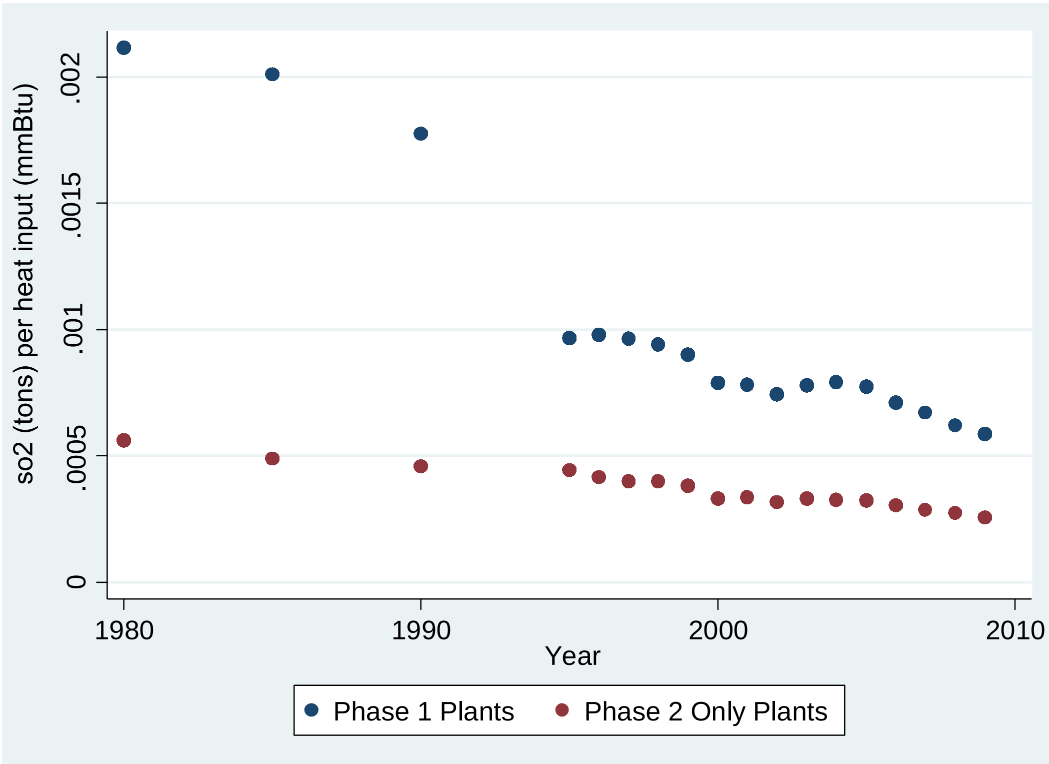
Notes: "EPA DV" represents 2004 nonattainment designation value for ozone. The sample was narrowed to provide a better view around the threshold. The attainment threshold is at 85 and represented by the vertical line. "Establishments per Capita" is average county establishments in highly polluting industries from 2004 to 2011 divided by county population.

Figure 3.3: Density of Counties by Designation Value



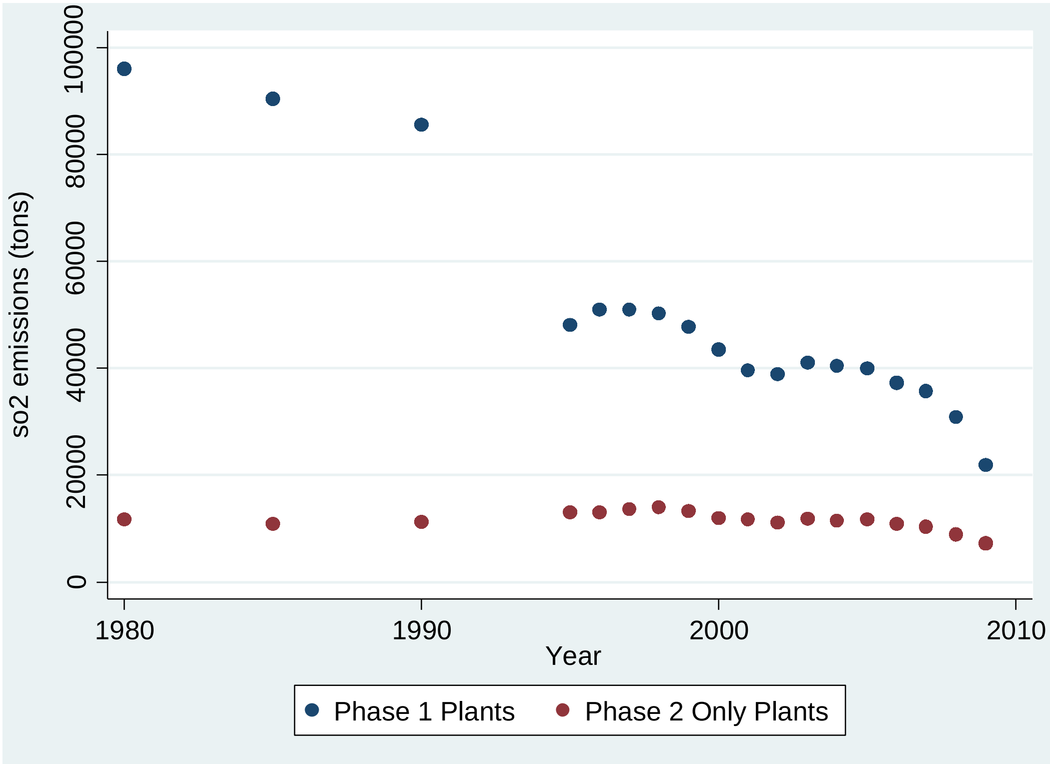
Note: "EPA DV" represents nonattainment designation value. The NAAQS threshold is at 85.

Figure 4.1: Average Sulfur-Dioxide Emissions in Tons per Heat Input (mmBtu)



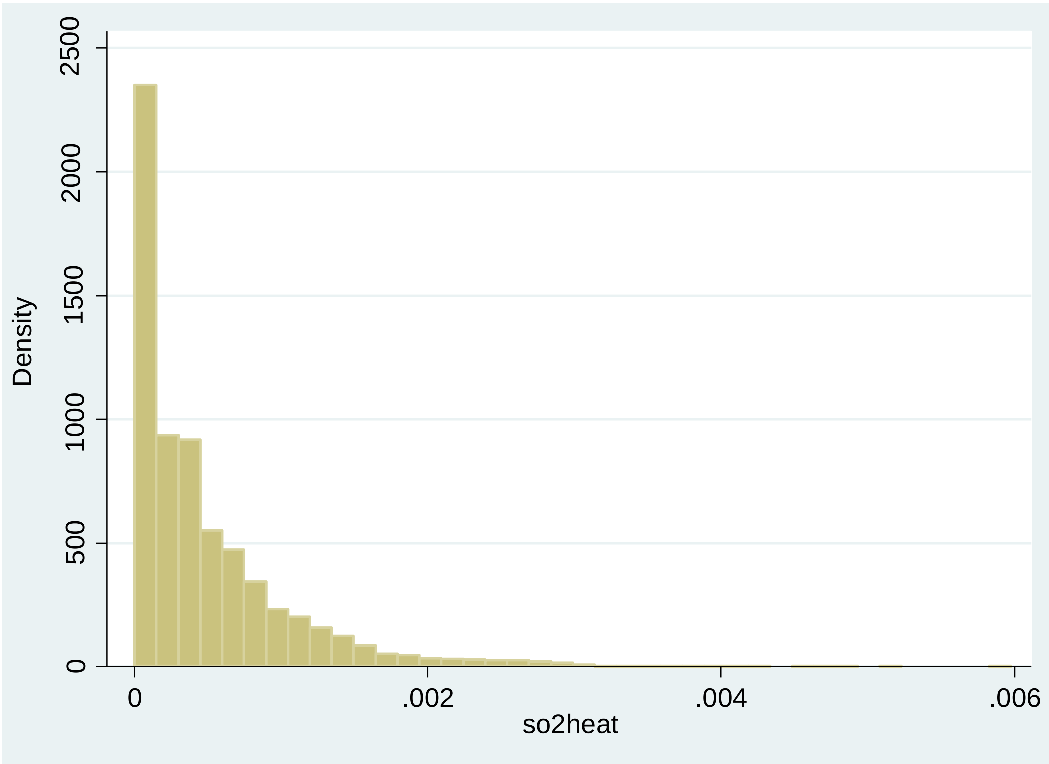
Notes: Phase 1 Plants are those given allocations in Phase 1 of the ARP, while Phase 2 Only Plants are those only given allocations in Phase 2. The Y-axis represents sulfur dioxide emissions in tons per heat input mmBtu.

Figure 4.2: Average Sulfur-Dioxide Emissions in Tons



Notes: Phase 1 Plants are those given allocations in Phase 1 of the ARP, while Phase 2 Only Plants are those only given allocations in Phase 2. The Y-axis is raw sulfur-dioxide emissions in tons.

Figure 4.3: Distribution of Emissions per Heat Input by Facility



Note: "so2heat" represents sulfur dioxide emissions per mmBtu of heat input.

Table 2.1: U.S. National Emissions Decline Estimates 1980 to 1990

Pollutant	Decline in Emissions
Lead	93%
Particulate Matter	50%
Volatile Organic Compounds (VOC's)	23%
Carbon Monoxide	19%
Sulfur Dioxide	12%
Nitrogen Oxide	7%

Source: "Air Quality Trends." *EPA*

Notes**:** This table depicts that percentage declines in emissions for different air pollutants between 1980 and 1990.

Table 2.2: Summary Statistics by Individual

Variable	Cohort	Observations	Mean	Std. Dev.	Min	Max
Institutionalized	All	2,189,417	0.0247	0.1551	0	1
	Pre	1,136,055	0.0219	0.1465	0	1
	Post	1,053,362	0.0276	0.1639	0	1
State Atmospheric Lead (μg/m^3)	All	2,189,417	0.6876	0.5058	0	1.9859
	Pre	1,136,055	1.1096	0.3431	0	1.9859
	Post	1,053,362	0.2324	0.0751	0.0031	0.4242
State Air Stagnation Index (stagnation periods per month)	All	2,189,417	4.7250	2.3247	2.0432	10.3468
	Pre	1,136,055	4.5290	2.2070	2.0432	10.3284
	Post	1,053,362	4.9368	2.4275	2.0438	10.3468

Notes: The data for this table are from the American Community Survey through IPUMS. The survey years are 2001 to 2012. The sample consists of adults born in one of two cohort periods. The "pre" cohort consists of individuals born between 1965 and 1969, while the "post" cohort has individuals born between 1985 and 1989. "State Pb" is the county population-weighted average of atmospheric lead in a state by cohort. Due to limitations in the monitor data, the "pre" period for lead is 1965 to 1974, and the "post" period is 1980 through 1989. "State ASI" is the county-population weighted average of Air Stagnation Index by state and cohort. ASI is the monthly average of stagnation periods from 1973 to 1997. "Institutionalized" is a binary variable that is "1" if the individual was institutionalized (either in prison or in a mental hospital) in the year of survey and "0" otherwise.

Table 2.3: First-Stage Results for Individuals by Birth State

VARIABLES	Atmospheric Lead	Atmospheric Lead	Atmospheric Lead
ASI × Low Exposure Cohort	-0.0960***	-0.1357***	-0.1358***
	(0.0314)	(0.0175)	(0.0174)
ASI	0.1038***	0.1425***	0.1427***
	(0.0331)	(0.0202)	(0.0201)
Low Exposure Cohort	-0.4458***	-0.2637	-1.6978
	(0.1355)	(0.1678)	(18.214)
Birth Region	No	Yes	Yes
Birth Region × Low Exposure Cohort	No	Yes	Yes
Age	No	No	Yes
Age × Low Exposure Cohort	No	No	Yes
Female	No	No	Yes
Female × Low Exposure Cohort	No	No	Yes
Race	No	No	Yes
Race × Low Exposure Cohort	No	No	Yes
Observations	2,189,417	2,189,417	2,189,417
R-squared	0.8578	0.9393	0.9394

Notes: The dependent variable is atmospheric lead in $\mu g/m^3$ averaged by birth state and cohort. "Low Exposure Cohort" is "0" if individual born between 1965 and 1969, and "1" if individual born between 1985 and 1989. Regional indicators are based on U.S. Census definitions. ASI and Lead are both state averages weighted by county population by cohort. Results are comparable with or without the Age and Age×Low Exposure Cohort variables. "Female" is "1" if the observation is female; "Race" is a set of indicators for each coded race in the sample. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level, and * indicates significance at the 10 percent level. Standard errors are included in parentheses and are clustered by birth state.

Table 2.4: Incarceration Reduced-Form Regression Results

VARIABLES	Institutionalized	Institutionalized	Institutionalized
ASI × Low Exposure Cohort	-0.0013***	-0.0006***	-0.0005**
	(0.0002)	(0.0002)	(0.0002)
ASI	0.0025***	0.0024***	0.0012***
	(0.0007)	(0.0004)	(0.0003)
Low Exposure Cohort	0.0113***	0.0054***	0.0520
	(0.0010)	(0.0016)	(1.5428)
Birth Region	No	Yes	Yes
Birth Region × Low Exposure Cohort	No	Yes	Yes
Age	No	No	Yes
Age × Low Exposure Cohort	No	No	Yes
Female	No	No	Yes
Female × Low Exposure Cohort	No	No	Yes
Race	No	No	Yes
Race × Low Exposure Cohort	No	No	Yes
Observations	2,189,417	2,189,417	2,189,417
R-squared	0.0011	0.0021	0.0411

Notes: The dependent variable, "Institutionalized", is a binary variable that is "1" if the individual was incarcerated in the year of survey and "0" otherwise. "ASI" is the county-population weighted average of Air Stagnation Index by state and cohort. "Low Exposure Cohort" takes on 0 for the "pre" cohort and 1 for the "post" cohort. "ASI×Low Exposure Cohort" is the interaction of "ASI" and "Low Exposure Cohort". Column (1) has controls for birth region (U.S. Census definition) and interactions between cohort and birth region, and column (2) adds demographic controls and the associated time interactions to the initial specification. Results are comparable with or without the Age and Age×Post variables. "Female" is "1" if the observation is female; "Race" is a set of indicators for each coded race in the sample. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level, and * indicates significance at the 10 percent level. Standard errors are in parentheses and are clustered by birth state.

Table 2.5: Incarceration Instrumental Variable Regression Results

VARIABLES	Institutionalized	Institutionalized	Institutionalized
Atmospheric Lead	0.0132**	0.0046***	0.0039***
	(0.0054)	(0.0011)	(0.0012)
ASI	0.0010*	0.0017***	0.0006*
	(0.0006)	(0.0004)	(0.0003)
Low Exposure Cohort	0.0168***	0.0073***	-0.0502***
	(0.0045)	(0.0013)	(0.0068)
Birth Region	No	Yes	Yes
Birth Region × Low Exposure Cohort	No	Yes	Yes
Age	No	No	Yes
Age × Low Exposure Cohort	No	No	Yes
Female	No	No	Yes
Female × Low Exposure Cohort	No	No	Yes
Race	No	No	Yes
Race × Low Exposure Cohort	No	No	Yes
Observations	2,189,417	2,189,417	2,189,417

Notes: The dependent variable, "Institutionalized", is a binary variable that is "1" if the individual was institutionalized (either in prison or a mental hospital) in the year of survey and "0" otherwise. "Atmospheric Pb" is the county population-weighted average of atmospheric lead (in micrograms per cubic meter) in a state by cohort and is instrumented by "ASI×Low Exposure Cohort". "ASI×Low Exposure Cohort" is the interaction of "ASI" and "Low Exposure Cohort". "ASI" is the county-population weighted average of Air Stagnation Index by state and cohort. "Low Exposure Cohort" takes on 0 for the "pre" cohort and 1 for the "post" cohort. Column (1) has controls for birth region (U.S. Census definition) and interactions between cohort and birth region, and column (2) adds demographic controls and the associated time interactions to the initial specification. Results are comparable with or without the Age and Age×Post variables. "Female" is "1" if the observation is female; "Race" is a set of indicators for each coded race in the sample. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level, and * indicates significance at the 10 percent level. Standard errors are in parentheses and are clustered by birth state.

Table 2.6: Period-Based Summary Statistics by MSA

Variable	Observations	Mean	Std. Dev.	25th Percentile	50th Percentile	75th Percentile	95th Percentile
Atmospheric Lead (μg/m^3)	394	0.4566	0.3976	0.1363	0.3196	0.7233	1.2206
Air Stagnation Index (stagnation periods per month)	394	4.8153	2.1556	3.0833	4.2483	5.7517	9.6067
Property Crime (crimes per 1,000 pop)	394	7.6101	2.8601	5.5023	7.0250	9.5630	13.096
Violent Crime (crimes per 1,000 pop)	394	1.9040	0.9531	1.2420	1.6694	2.3541	3.6845

Notes: These summary statistics pertain to the MSA-level sample. The crime data are from National Archive for Criminal Justice Data (NACJD) and were attained through the Interuniversity Consortium for Political and Social Research (ICPSR). These data originate from the Federal Bureau of Investigation's Uniform Crime Reports (UCR). The lead data come from the U.S. Environmental Protection Agency and were acquired through a Freedom of Information Act (FOIA) request. The ASI data are from the National Oceanic and Atmospheric Administration (NOAA). Atmospheric lead is in micrograms per cubic meter, and the ASI is the monthly average of stagnation periods from 1973 to 1997. The crime variables are measured in number of arrests in the respective categories. The various percentiles refer to the percentiles of the respective variable.

Table 2.7: First-Stage Results by MSA

VARIABLES	Atmospheric Lead	Atmospheric Lead
ASI×Post2000	-0.0474***	-0.0644***
	(0.0178)	(0.0229)
ASI	0.0414**	0.0682***
	(0.0186)	(0.0257)
Post2000	-0.373***	-0.592***
	(0.0784)	(0.1090)
Region	No	Yes
Region×Post2000	No	Yes
Observations	394	394
R-squared	0.599	0.674

Notes: The dependent variable is atmospheric lead in $\mu g/m^3$ averaged by MSA and period. The "pre" period is 1960 through 1979 and the "post" period is 1980 to 2000. ASI is the average number of stagnation periods per month from 1973 to 1997. Post2000 is a dummy variable taking on 0 for the "pre" period and 1 for the "post" period. "Region" is based on U.S. Census definitions. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level, and * indicates significance at the 10 percent level. Standard errors are included in parentheses and are clustered by MSA.

Table 2.8a: Period-Based Reduced-Form Property Crime Regression Results

VARIABLES	Property Crime per Capita	Property Crime per Capita	Ln(Property Crime)	Ln(Property Crime)
ASI×Post2000	-0.415***	-0.237**	-0.0404***	-0.0356***
	(0.0660)	(0.106)	(0.0081)	(0.0115)
ASI	0.495***	0.0043	0.0595***	0.0029
	(0.0843)	(0.124)	(0.0083)	(0.0105)
Post2000	-1.109***	-3.914***	-0.195***	-0.381***
	(0.367)	(0.644)	(0.0495)	(0.109)
Ln (MSA Population)			0.952***	0.976***
			(0.0166)	(0.0159)
Region	No	Yes	No	Yes
Region×Post2000	No	Yes	No	Yes
Observations	394	394	394	394
R-squared	0.367	0.588	0.925	0.946

Notes: The dependent variable is property crimes per 1,000 residents or the logarithm of property crime. These are averages (or the logarithm of averages) by MSA and period. Crime is based on number of arrests. The “pre” period is 1980 through 1990 and the “post” period is 2000 to 2010. “Region” and “Region×Post2000” are geographic control variables based on U.S. Census definitions. ASI is the Air Stagnation Index average by MSA and Post2000 is a dummy variable taking on 0 for the “pre” period and 1 for the “post” period. The logarithm of MSA population is the logarithm of the average MSA population by period. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level, and * indicates significance at the 10 percent level. Standard errors are included in parentheses and are clustered by MSA.

Table 2.8b: Period-Based Reduced-Form Violent Crime Regression Results

VARIABLES	Violent Crime per Capita	Violent Crime per Capita	Ln(Violent Crime)	Ln(Violent Crime)
ASI×Post2000	0.011	0.0077	-0.0249**	-0.0066
	(0.0255)	(0.0401)	(0.0116)	(0.0212)
ASI	0.208***	0.197***	0.113***	0.0886***
	(0.0342)	(0.0449)	(0.0141)	(0.0213)
Post2000	-0.074	-0.514**	0.110	0.0836
	(0.130)	(0.215)	(0.0696)	(0.196)
Ln (MSA Population)			1.145***	1.126***
			(0.0262)	(0.0269)
Region	No	Yes	No	Yes
Region×Post2000	No	Yes	No	Yes
Observations	394	394	394	394
R-squared	0.258	0.232	0.911	0.92

Notes: The dependent variable is violent crimes per 1,000 residents or the logarithm of violent crime. These are averages (or the logarithm of averages) by MSA and period. Crime is based on number of arrests. The "pre" period is 1980 through 1990 and the "post" period is 2000 to 2010. "Region" and "Region×Post2000" are geographic control variables based on U.S. Census definitions. ASI is the Air Stagnation Index average by MSA and Post2000 is a dummy variable taking on 0 for the "pre" period and 1 for the "post" period. The logarithm of MSA population is the logarithm of the average MSA population by period. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level, and * indicates significance at the 10 percent level. Standard errors are included in parentheses and are clustered by MSA.

Table 2.9a: Period-Based Instrumental Variable Property Crime Regression Results

VARIABLES	Property Crime per Capita	Property Crime per Capita	Ln(Property Crime)	Ln(Property Crime)
Atmospheric Lead	8.755**	3.681		
	(3.764)	(2.422)		
Ln(Atmospheric Lead)			0.514***	1.291
			(0.160)	(1.662)
ASI	0.132*	-0.247**	0.0457***	-0.0637
	(0.0754)	(0.0956)	(0.0111)	(0.0850)
Post2000	2.157	-1.737	0.490*	2.221
	(2.194)	(1.871)	(0.277)	(3.146)
Ln (MSA Population)			0.859***	0.749**
			(0.0348)	(0.296)
Region	No	Yes	No	Yes
Region×Post2000	No	Yes	No	Yes
Observations	394	394	394	394

Notes: The dependent variable is property crimes per 1,000 residents or the logarithm of property crime. These are averages (or the logarithm of the average) by MSA and period. Crime is based on number of arrests. The "pre" period is 1980 through 1990 and the "post" period is 2000 to 2010. Atmospheric lead is the average atmospheric lead in micrograms per cubic meter by MSA and period. "Region" and "Region×Post2000" are geographic control variables based on U.S. Census definitions. ASI is the Air Stagnation Index average by MSA and Post2000 is a dummy variable taking on 0 for the "pre" period and 1 for the "post" period. The logarithm of MSA population is the logarithm of the average MSA population by period. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level, and * indicates significance at the 10 percent level. Standard errors are included in parentheses and are clustered by MSA.

Table 2.9b: Period-Based Instrumental Variable Violent Crime Regression Results

VARIABLES	Violent Crime per Capita	Violent Crime per Capita	Ln(Violent Crime)	Ln(Violent Crime)
Atmospheric Lead	-0.221	-0.120		
	(0.564)	(0.630)		
Ln(Atmospheric Lead)			0.316*	0.239
			(0.168)	(0.748)
ASI	0.217***	0.205***	0.105***	0.0763**
	(0.0327)	(0.0439)	(0.0134)	(0.0327)
Post2000	-0.157	-0.586	0.531*	0.449
	(0.338)	(0.514)	(0.292)	(1.450)
Ln (MSA Population)			1.087***	1.084***
			(0.0357)	(0.132)
Region	No	Yes	No	Yes
Region×Post2000	No	Yes	No	Yes
Observations	394	394	394	394

Notes: The dependent variable is violent crimes per 1,000 residents or the logarithm of violent crime. These are averages (or the logarithm of the average) by MSA and period. Crime is based on number of arrests. The "pre" period is 1980 through 1990 and the "post" period is 2000 to 2010. Atmospheric lead is the average atmospheric lead in micrograms per cubic meter by MSA and period. "Region" and "Region×Post2000" are geographic control variables based on U.S. Census definitions. ASI is the Air Stagnation Index average by MSA and Post2000 is a dummy variable taking on 0 for the "pre" period and 1 for the "post" period. The logarithm of MSA population is the logarithm of the average MSA population by period. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level, and * indicates significance at the 10 percent level. Standard errors are included in parentheses and are clustered by MSA.

Table 2.10: Results for Ozone Regressed on ASI, 1980 to 2010

VARIABLES	Ozone	Ozone
ASI×Post1990	0.0004	-0.0015**
	(0.0005)	(0.0007)
ASI	-0.0006	0.0035***
	(0.0006)	(0.0008)
Post1990	-0.0091***	-0.0126***
	(0.0023)	(0.0031)
Observations	1,497	1,497
R-squared	0.051	0.253

Notes: These regressions test the relationship between air stagnation and trends in ozone. The dependent variable is ozone in parts per million (ppm). Post1990 takes on "0" if year is before 1990 and "1" if year is 1990 or later. Results are comparable when using 1995 as starting "post" year. Column (1) includes no geographic controls, Column (2) controls for regional and regional-time variables (based on U.S. Census). Standard errors are included in parentheses and are clustered by MSA. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level, and * indicates significance at the 10 percent level.

Table 3.1: Historic NAAQS for Ozone 1979 to 2008

Year Rule Established	Averaging time	Level	Form
1979	1 hour	0.12ppm	Not to be exceeded for more than 1 hour per year
1997	8 hour	0.08ppm	Annual fourth-highest daily maximum 8 hour concentration, averaged over 3 years
2008	8 hour	0.075ppm	Annual fourth-highest daily maximum 8 hour concentration, averaged over 3 years

Source: United States Environmental Protection Agency

Note: “ppm” stands for “parts per million”.

Table 3.2: Summary Statistics for Polluting-Industry Outcome Variables

Variable	Observations	Mean	Std. Dev.	Min	Max
Employment (Full Sample)	5,200	11,781	22,720	3	390,304
Employment (EPADV 82-88)	1,336	12,168	24,058	66	229,673
Establishments (Full Sample)	5,200	288	595	1	10,817
Establishments (EPADV 82-88)	1,336	280	609	4	5,690

Note: "EPADV" refers to the designation value range. Data come from the U.S. Census County Business Patterns.

Table 3.3: Baseline OLS Results for Polluting-Industries

VARIABLES	DV 75-95 Ln(Employment)	DV 82-88 Ln(Employment)	DV 75-95 Ln(Estbs.)	DV 82-88 Ln(Estbs.)
Nonattainment Status	-0.123*	-0.180*	-0.00300	-0.0447
	(0.0624)	(0.0972)	(0.0367)	(0.0657)
Ln(County Population)	1.039***	1.006***	0.969***	0.963***
	(0.0371)	(0.0615)	(0.0200)	(0.0368)
Constant	-2.790***	-2.141***	-6.148***	-5.866***
	(0.454)	(0.756)	(0.248)	(0.469)
Year	Yes	Yes	Yes	Yes
Census Region	Yes	Yes	Yes	Yes
Observations	3,728	1,328	3,728	1,328
R-squared	0.740	0.718	0.874	0.865

Note**:** This table shows the results from a baseline regression using Ordinary least squares. Standard errors are in parentheses and clustered by county. *** indicate statistical significance at the 1 percent level, ** indicate statistical significance at the 5 percent level, and * indicates statistical significance at the 10 percent level. Year and U.S. Census region dummy variables are included. Nonattainment status is a binary variable indicating whether a county was in attainment of the ozone NAAQS for a given year (0 for in attainment, 1 for nonattainment). "Ln(Employment)" represents the logarithm of the number of workers in highly polluting industries. "Ln(Estbs.)" represents the logarithm of the number of highly polluting industry establishments. "DV" stands for "designation value" with the threshold for attainment being 85 ppm. The 75-95 sample contains most counties in the full sample, while the 82-88 sample is the preferred selection of counties close to the threshold.

Table 3.4: Baseline 2SLS Results for Polluting Industries

VARIABLES	DV 75-95 Ln(Employment)	DV 82-88 Ln(Employment)	DV 75-95 Ln(Estbs.)	DV 82-88 Ln(Estbs.)
Nonattainment Status	-0.179**	-0.201	-0.0302	-0.0333
	(0.0765)	(0.125)	(0.0467)	(0.0871)
Ln(County Population)	1.046***	1.009***	0.972***	0.961***
	(0.0380)	(0.0633)	(0.0210)	(0.0396)
Constant	-2.637***	-2.282***	-6.053***	-5.937***
	(0.477)	(0.791)	(0.264)	(0.504)
Year	Yes	Yes	Yes	Yes
Census Region	Yes	Yes	Yes	Yes
Observations	3,728	1,328	3,728	1,328
R-squared	0.740	0.718	0.874	0.865

Note**:** This table shows the results from a baseline regression using Two-Stage Least Squares. The instrument is the 2004 ozone designation value for a county. Standard errors are in parentheses and clustered by county. *** indicate statistical significance at the 1 percent level, ** indicate statistical significance at the 5 percent level, and * indicates statistical significance at the 10 percent level. Year and U.S. Census region dummy variables are included. Nonattainment status is a binary variable indicating whether a county was in attainment of the ozone NAAQS for a given year (0 for in attainment, 1 for nonattainment). “Ln(Employment)” represents the logarithm of the number of workers in highly polluting industries. “Ln(Estbs.)” represents the logarithm of the number of highly polluting industry establishments. “DV” stands for “designation value” with the threshold for attainment being 85 ppm. The 75-95 sample contains most counties in the full sample, while the 82-88 sample is the preferred selection of counties close to the threshold.

Table 3.5: Fuzzy RD Results for Polluting Industries

VARIABLES	DV 75-95 Ln(Employment)	DV 82-88 Ln(Employment)	DV 75-95 Ln(Estbs.)	DV 82-88 Ln(Estbs.)
Nonattainment Status	-0.179**	-0.241**	-0.0346	-0.0911
	(0.0828)	(0.113)	(0.0521)	(0.0787)
Ln(County Population)	1.035***	1.022***	0.966***	0.968***
	(0.0374)	(0.0609)	(0.0207)	(0.0366)
Distance Over Threshold	0.0210***	-0.0693*	0.0140***	-0.0164
	(0.0074)	(0.0407)	(0.0048)	(0.0240)
Distance×Nonattainment Status	-0.0275**	0.0937	-0.0176**	0.0413
	(0.0120)	(0.0576)	(0.00762)	(0.0425)
Constant	-2.503***	-2.421***	-5.966***	-6.003***
	(0.472)	(0.768)	(0.262)	(0.481)
Year	Yes	Yes	Yes	Yes
Census Region	Yes	Yes	Yes	Yes
Observations	3,728	1,328	3,728	1,328
R-squared	0.743	0.723	0.876	0.866

Notes**:** This table contains the primary regression discontinuity regression results. Standard errors are in parentheses and clustered by county. *** indicate statistical significance at the 1 percent level, ** indicate statistical significance at the 5 percent level, and * indicates statistical significance at the 10 percent level. Year and U.S. Census region dummy variables are included. "Nonattainment status" is a binary variable indicating whether a county was in attainment of the ozone NAAQS for a given year (0 for in attainment, 1 for nonattainment). The "distance" variable takes the 2004 designation value and subtracts 85. "Ln(Employment)" represents the logarithm of the number of workers in highly polluting industries. "Ln(Estbs.)" represents the logarithm of the number of highly polluting industry establishments. "DV" stands for "designation value" with the threshold for attainment being 85 ppm. The 75-95 sample contains most counties in the full sample, while the 82-88 sample is the preferred selection of counties close to the threshold.

Table 3.6: Fuzzy RD Results for Total County

VARIABLES	DV 75-95 Ln(Employment)	DV 82-88 Ln(Employment)	DV 75-95 Ln(Estbs.)	DV 82-88 Ln(Estbs.)
Nonattainment Status	-0.0078	-0.0739	0.0345	-0.0282
	(0.0394)	(0.0894)	(0.0314)	(0.0742)
Ln(County Population)	1.179***	1.157***	1.052***	1.046***
	(0.0189)	(0.0348)	(0.0162)	(0.0283)
Distance Over Threshold	-0.0030	-0.0266	-0.0068**	-0.0168
	(0.0039)	(0.0238)	(0.0033)	(0.0192)
Distance×Nonattainment Status	0.00151	0.0712	0.0076	0.0661
	(0.0064)	(0.0493)	(0.0056)	(0.0425)
Constant	-2.839***	-2.537***	-4.309***	-4.211***
	(0.239)	(0.459)	(0.205)	(0.381)
Year	Yes	Yes	Yes	Yes
Census Region	Yes	Yes	Yes	Yes
Observations	3,724	1,328	3,724	1,328
R-squared	0.934	0.921	0.941	0.938

Notes**:** This table contains the regression discontinuity regression results for total county outcomes. Standard errors are in parentheses and clustered by county. *** indicate statistical significance at the 1 percent level, ** indicate statistical significance at the 5 percent level, and * indicates statistical significance at the 10 percent level. Year and U.S. Census region dummy variables are included. "Nonattainment status" is a binary variable indicating whether a county was in attainment of the ozone NAAQS for a given year (0 for in attainment, 1 for nonattainment). The "distance" variable takes the 2004 designation value and subtracts 85. "Ln(Employment)" represents the logarithm of the number of workers in highly polluting industries. "Ln(Estbs.)" represents the logarithm of the total county establishments. "DV" stands for "designation value" with the threshold for attainment being 85 ppm. The 75-95 sample contains most counties in the full sample, while the 82-88 sample is the preferred selection of counties close to the threshold.

Table 3.7: Fuzzy RD Results for Total County Less Polluting Industries

VARIABLES	DV 75-95 Ln(Employment)	DV 82-88 Ln(Employment)	DV 75-95 Ln(Estbs.)	DV 82-88 Ln(Estbs.)
Nonattainment Status	0.0133	-0.0658	0.0379	-0.0260
	(0.0405)	(0.0969)	(0.0316)	(0.0754)
Ln(County Population)	1.209***	1.191***	1.056***	1.050***
	(0.0192)	(0.0355)	(0.0165)	(0.0287)
Distance Over Threshold	-0.0062	-0.0197	-0.0077**	-0.0174
	(0.0041)	(0.0242)	(0.0033)	(0.0196)
Distance×Nonattainment Status	0.0038	0.0682	0.0084	0.0674
	(0.0067)	(0.0533)	(0.0056)	(0.0431)
Constant	-3.458***	-3.205***	-4.430***	-4.327***
	(0.241)	(0.465)	(0.208)	(0.384)
Year	Yes	Yes	Yes	Yes
Census Region	Yes	Yes	Yes	Yes
Observations	3,723	1,328	3,724	1,328
R-squared	0.933	0.921	0.940	0.937

Notes**:** This table contains the regression discontinuity regression results for total county employment less polluting industries. Standard errors are in parentheses and clustered by county. *** indicate statistical significance at the 1 percent level, ** indicate statistical significance at the 5 percent level, and * indicates statistical significance at the 10 percent level. Year and U.S. Census region dummy variables are included. “Nonattainment status” is a binary variable indicating whether a county was in attainment of the ozone NAAQS for a given year (0 for in attainment, 1 for nonattainment). The “distance” variable takes the 2004 designation value and subtracts 85. “Ln(Employment)” represents the logarithm of the number of workers in highly polluting industries. “Ln(Estbs.)” represents the logarithm of total county establishments excluding those in highly polluting industries. “DV” stands for “designation value” with the threshold for attainment being 85 ppm. The 75-95 sample contains most counties in the full sample, while the 82-88 sample is the preferred selection of counties close to the threshold.

Table 3.8: Fuzzy RD Results for Polluting Industries, Controlling for Status Switching

VARIABLES	DV 75-95 Ln(Employment)	DV 82-88 Ln(Employment)	DV 75-95 Ln(Estbs.)	DV 82-88 Ln(Estbs.)
Nonattainment Status	-0.483***	-0.636***	-0.158*	-0.231*
	(0.144)	(0.187)	(0.0923)	(0.140)
Ln(County Population)	1.059***	1.079***	0.974***	0.984***
	(0.0392)	(0.0658)	(0.0216)	(0.0413)
Distance Over Threshold	0.0356***	-0.0462	0.0213***	-0.0003
	(0.0115)	(0.0462)	(0.00759)	(0.0297)
Distance×Nonattainment Status	-0.0408***	0.0688	-0.0246***	0.0242
	(0.0137)	(0.0617)	(0.00932)	(0.0461)
Switcher Status	-0.282**	-0.246	-0.139	-0.149
	(0.139)	(0.174)	(0.0890)	(0.118)
Switcher×Nonattainment Status	0.664***	0.788***	0.276**	0.304*
	(0.174)	(0.240)	(0.109)	(0.161)
Constant	-2.776***	-3.118***	-6.046***	-6.166***
	(0.518)	(0.873)	(0.282)	(0.559)
Year	Yes	Yes	Yes	Yes
Census Region	Yes	Yes	Yes	Yes
Observations	3,728	1,328	3,728	1,328
R-squared	0.748	0.728	0.877	0.866

Notes**:** This table contains the regression discontinuity regression results for polluting industries when controlling for counties that changed designation over the course of the sample (e.g. went from nonattainment to attainment). Standard errors are in parentheses and clustered by county. *** indicate statistical significance at the 1 percent level, ** indicate statistical significance at the 5 percent level, and * indicates statistical significance at the 10 percent level. Year and U.S. Census region dummy variables are included. “Nonattainment status” is a binary variable indicating whether a county was in attainment of the ozone NAAQS for a given year (0 for in attainment, 1 for nonattainment). The “distance” variable takes the 2004 designation value and subtracts 85. “Switcher Status” is a binary variable indicating whether a county changed designation status during the sample years. “Ln(Employment)” represents the logarithm of the number of workers in highly polluting industries. “Ln(Estbs.)” represents the logarithm of the number of highly polluting industry establishments. “DV” stands for “designation value” with the threshold for attainment being 85 ppm. The 75-95 sample contains most counties in the full sample, while the 82-88 sample is the preferred selection of counties close to the threshold.

Table 3.9: Fuzzy RD Results for Polluting Industries Excluding EAC Counties

VARIABLES	DV 75-95 Ln(Employment)	DV 82-88 Ln(Employment)	DV 75-95 Ln(Estbs.)	DV 82-88 Ln(Estbs.)
Nonattainment Status	-0.161*	-0.165	-0.0254	-0.0623
	(0.0855)	(0.118)	(0.0531)	(0.0767)
Ln(County Population)	1.029***	0.994***	0.963***	0.958***
	(0.0384)	(0.0623)	(0.0211)	(0.0371)
Distance Over Threshold	0.0198**	-0.0882**	0.0129***	-0.0243
	(0.00785)	(0.0436)	(0.00502)	(0.0239)
Distance×Nonattainment Status	-0.0260**	0.110*	-0.0165**	0.0482
	(0.0120)	(0.0566)	(0.00760)	(0.0416)
Constant	-2.433***	-2.128***	-5.941***	-5.897***
	(0.483)	(0.785)	(0.267)	(0.488)
Year	Yes	Yes	Yes	Yes
Census Region	Yes	Yes	Yes	Yes
Observations	3,576	1,224	3,576	1,224
R-squared	0.743	0.726	0.878	0.874

Notes**:** This table contains regression results when excluding Early Action Compact counties from the sample. Standard errors are in parentheses and clustered by county. *** indicate statistical significance at the 1 percent level, ** indicate statistical significance at the 5 percent level, and * indicates statistical significance at the 10 percent level. Year and U.S. Census region dummy variables are included. “Nonattainment status” is a binary variable indicating whether a county was in attainment of the ozone NAAQS for a given year (0 for in attainment, 1 for nonattainment). The “distance” variable takes the 2004 designation value and subtracts 85. “Ln(Employment)” represents the logarithm of the number of workers in highly polluting industries. “Ln(Estbs.)” represents the logarithm of the number of highly polluting industry establishments. “DV” stands for “designation value” with the threshold for attainment being 85 ppm. The 75-95 sample contains most counties in the full sample, while the 82-88 sample is the preferred selection of counties close to the threshold.

Table 4.1: Summary Statistics for Full Sample

	Variable	Observations	Mean	Standard Deviation	Minimum	Maximum
All Years	SO_2 Emissions (tons)	11,503	17,781	32,148	0	374,920
	Heat Input (mmBtu)	11,503	34,600,000	41,700,000	0	265,000,000
Pre-ARP Period	SO_2 Emissions (tons)	2,034	23,797	47,451	0	374,920
	Heat Input (mmBtu)	2,034	27,300,000	33,100,000	0	208,000,000
Phase 1 Period	SO_2 Emissions (tons)	3,244	19,193	31,706	0	284,616
	Heat Input (mmBtu)	3244	35,700,000	42,100,000	0	245,000,000
Phase 2 Period	SO_2 Emissions (tons)	6,225	15,079	25,108	0	206,442
	Heat Input (mmBtu)	6,225	36,500,000	43,700,000	0	265,000,000

Notes: Data are from the EPA's Air Market Data for the Acid Rain Program. "Pre-ARP Period" is 1980, 1985, and 1990; "Phase 1 Period" is 1995 to 1999; "Phase 2 Period" is 2000 to 2009.

Table 4.2: Initial Allocation Summary Statistics by Facility

Variable	Observations	Mean	Standard Deviation	Minimum	Maximum
Phase 1 Plant Allocations	104	52,110	46,929	2,571	247,881
Estimated Phase 1 Plant Allocations	104	51,496	46,808	883.74	252,625
Phase 2 Plant Allocations	654	13,772	17,862	1	109,781
Estimated Phase 2 Plant Allocations	654	16,477	20,010	0	124,748

Notes: Data are from the EPA's Air Market Data for the Acid Rain Program. The "Estimated" Allocations are calculated based on the 1985-1987 heat input baseline multiplied by the respective multiplier for each phase.

Table 4.3: Results for Full Sample ARP Phase 1 Analysis

VARIABLES	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions
Phase 1 × PostDummy1	-45,764***	-45,712***	-76,481***	1,849
	(6,457)	(6,096)	(6,968)	(6,771)
Phase 1	64,539***	19,703***	79,414***	1,084
	(5,565)	(4,725)	(7,633)	(5,302)
PostDummy1	-3,141***	1,859***	1,859***	1,859***
	(520.7)	(483.1)	(483.1)	(483.2)
Heat Input	0.624***			
	(0.0416)			
Phase 1 Allocations		1.167***		1.530***
		(0.0835)		(0.128)
Ph1 Alloc. × PostDummy1			0.611***	-0.919***
			(0.116)	(0.187)
Constant	-3,395***	11,264***	11,264***	11,264***
	(789.1)	(860.3)	(860.3)	(860.5)
Observations	3,318	3,318	3,318	3,318
R-squared	0.615	0.602	0.362	0.641

Notes: Standard errors listed in parentheses are clustered by facility. *** indicates significance at the 1 percent level; ** indicates significance at the 5 percent level; * indicates significance at the 10 percent level". "PostDummy1" is a binary indicator that represents "0" if the year is before 1995 and "1" if 1995 or later. "Phase 1" is a binary indicator for whether a facility was included in Phase 1, while "Phase 1 Allocations" is the number of yearly tradable permit allocations for a given facility. The unit for "Heat Input" is 1000 mmBtu.

Table 4.4: Results for ARP Phase 1 Analysis with Fixed Effects

VARIABLES	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions
Phase 1 × PostDummy1	-45,764***	-45,688***	-41,263***	-76,481***	-69,067***	-69,433***
	(6,457)	(6,472)	(5,688)	(6,968)	(7,401)	(7,316)
Phase 1	64,539***	55,978***	53,989***	79,414***	69,251***	67,813***
	(5,565)	(5,261)	(5,112)	(7,633)	(7,119)	(7,089)
PostDummy1	-3,141***	-3,265***		1,859***	1,448***	
	(520.7)	(530.1)		(483.1)	(499.3)	
Heat Input	0.624***	0.601***	0.609***			
	(0.0416)	(0.04)	(0.0416)			
Ph1 Alloc. × PostDummy1				0.611***	0.468***	0.548***
				(0.116)	(0.143)	(0.130)
Constant	-3,395***	-1,272	-2,765***	11,264***	13,046***	13,599***
	(789.1)	(882.5)	(986.6)	(860.3)	(975.4)	(989.9)
State FE	No	Yes	No	No	Yes	No
State-Year FE	No	No	Yes	No	No	Yes
Observations	3,318	3,318	3,318	3,318	3,318	3,318
R-squared	0.615	0.658	0.670	0.362	0.439	0.453

Notes: Standard errors listed in parentheses are clustered by facility. *** indicates significance at the 1 percent level; ** indicates significance at the 5 percent level; * indicates significance at the 10 percent level". Columns 2 and 5 include state fixed effects, while Columns 3 and 6 include state-year fixed effects. "PostDummy1" is a binary indicator that represents "0" if the year is before 1995 and "1" if 1995 or later. "Phase 1" is a binary indicator for whether a facility was included in Phase 1, while "Phase 1 Allocations" is the number of yearly tradable permit allocations for a given facility. The unit for "Heat Input" is 1000 mmBtu.

Table 4.5: Results for Full Sample ARP Phase 2 Analysis

VARIABLES	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions
Phase 2 Only×PostDummy2	10,257***	9,713***	25,325***	5,100**
	(3,155)	(3,134)	(4,323)	(2,524)
Phase 2 Only	-25,987***	-19,074***	-36,220***	-15,995***
	(3,864)	(3,557)	(4,749)	(3,273)
Post Dummy2	-13,132***	-12,654***	-39,574***	-4,640**
	(3,136)	(3,117)	(4,398)	(2,206)
Heat Input	0.389***			
	(0.294)			
Phase 2 Allocations		1.097***		1.294***
		(0.0624)		(0.0721)
Phase 2 Alloc.× Post Dummy 2			0.997***	-0.297***
			(0.0647)	(0.0531)
Constant	27,133***	19,991***	49,607***	14,673***
	(3,757)	(3,477)	(4,646)	(3,051)
Observations	9,469	9,469	9,469	9,469
R-squared	0.517	0.627	0.419	0.634

Notes: Standard errors listed in parentheses are clustered by facility. *** indicates significance at the 1 percent level; ** indicates significance at the 5 percent level; * indicates significance at the 10 percent level". "Post Dummy2" is a binary indicator that represents "0" if the year is before 2000 and "1" if later. "Phase 2" is a binary indicator for whether a facility was only included in Phase 2, while "Phase 2 Allocations" is the number of yearly tradable permit allocations for a given facility. The unit for "Heat Input" is 1000 mmBtu.

Table 4.6: Results for ARP Phase 2 Analysis with Fixed Effects

VARIABLES	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions	SO_2 Emissions
Phase 2 × PostDummy2	10,257***	10,367***	8,980***	25,325***	23,578***	19,722***
	(3,155)	(3,169)	(3,748)	(4,323)	(4,186)	(4,807)
Phase 2	-25,987***	-19,620***	-18,713***	-36,220***	-29,195***	-26,933***
	(3,864)	(3,812)	(4,145)	(4,749)	(4,641)	(5,105)
PostDummy2	-13,132***	-13,032***		-39,574***	-36,299***	
	(3,136)	(3,150)		(4,398)	(4,186)	
Heat Input	0.389***	0.374***	0.373***			
	(0.0294)	(0.0274)	(0.0285)			
Ph2 Alloc. × PostDummy2				0.997***	0.878***	0.952***
				(0.0647)	(0.0589)	(0.0620)
Constant	27,133***	22,251***	13,690***	49,607***	44,245***	19,321***
	(3,757)	(3,631)	(2,608)	(4,646)	(4,428)	(2,567)
State FE	No	Yes	No	No	Yes	No
State-Year FE	No	No	Yes	No	No	Yes
Observations	9,469	9,469	9,469	9,469	9,469	9,469
R-squared	0.517	0.585	0.605	0.419	0.478	0.519

Notes: Standard errors listed in parentheses are clustered by facility. *** indicates significance at the 1 percent level; ** indicates significance at the 5 percent level; * indicates significance at the 10 percent level". Columns 2 and 5 include state fixed effects, while Columns 3 and 6 include state-year fixed effects. "PostDummy2" is a binary indicator that represents "0" if the year is before 2000 and "1" if later. "Phase 2" is a binary indicator for whether a facility was only included in Phase 2, while "Phase 2 Allocations" is the number of yearly tradable permit allocations for a given facility. The unit for "Heat Input" is 1000 mmBtu.

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