EVALUATING THE IMPACT OF MONITORING AND ENFORCEMENT IN THE CLEAN AIR ACT

By

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To my family

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LIST OF ABBREVIATIONS

AFS: Air Facility System

BEA: Bureau of Economic Analysis

BLS: Bureau of Labor Statistics

CAA: Clean Air Act

CARB: California Air Resources Board

EPA: U.S. Environmental Protection Agency

FCE: full compliance evaluation

NAAQS: National Ambient Air Quality Standards

NCDC: U.S. National Climatic Data Center

NOV: notice of violation

NO_x: nitrogen oxides

PCE: partial compliance evaluation

ppb: parts per billion

ppm: parts per million

PM_{10}: Particulate matter with a diameter of 10 micrometers or less

$PM_{2.5}$: Particulate matter with a diameter of 2.5 micrometers or less

RECLAIM: Regional Clean Air Incentives Market

TRI: Toxics Release Inventory

INTRODUCTION

Congress passed the Clean Air Act in 1963 to protect air resources and promote public health and welfare. In 2010 alone, state and federal regulators conducted over 70,000 inspections and assessed a total of $115 million in penalties under the Clean Air Act. Surprisingly, there has been relatively little research into the effectiveness of monitoring and enforcement actions. In this dissertation, I investigate the impact of monitoring and enforcement actions on air pollution emissions and air quality and examine escalating penalties for repeat violations of environmental regulations.

In Chapter I, I investigate the impact of monitoring and enforcement actions on emissions of criteria pollutants and find that penalties decrease emissions. Criteria pollutants are commonly found air pollutants that harm human health and the environment; yet, due to data limitations, few researchers have studied the impact of monitoring and enforcement on criteria pollutant emissions. Using a large dataset of nitrogen oxides emissions in California, I find that penalties reduce emissions. Increasing the penalty from the 25th percentile to the 75th percentile of the distribution of positive penalties reduces emissions by 1.46 tons and facilities that were assessed a penalty in the previous year reduce emissions by 5.60 tons on average. This reduction is approximately 2.0% to 7.7% of mean facility emissions. Interestingly, inspections have no significant impact, likely because inspection rates in California are consistently high.

In Chapter II, I examine whether monitoring and enforcement actions improve air quality. Focusing again on California, I find that penalties are successful at reducing ambient ozone concentrations. Previous studies have found that monitoring and enforcement improve a facility's compliance with its pollution permit and reduce its emissions, but this alone does not measure the efficacy of the regulatory regime. While compliance and emissions are important measures, noncompliance and high emissions do not directly harm health and the environment; they do harm by increasing ambient pollution. Most of the previous research examining air quality has found that nonattainment of federal air quality standards—which implies a more stringent regulatory regime—improves air quality. No research has directly examined the impact of monitoring and enforcement on air quality.

I fill that gap in the literature by investigating how monitoring and enforcement actions at facilities located near an air quality monitor affect ambient ozone concentrations measured at the air quality monitor. I find that increasing penalties improves air quality; increasing total penalties assessed from the 25th percentile to the 75th percentile of the penalty distribution reduces ambient ozone concentrations by 0.348 parts per billion, a 0.4% reduction in ambient ozone concentrations.

In Chapter III, I examine the theoretical and empirical support for escalating penalties for repeat environmental violations and find that facilities with a history of violations are not necessarily the worst polluters as their violations seem to be less severe on average. Although escalating penalties for repeat offenders are common, the law and economics literature has not fully explored this concept. I argue that existing theoretical models could better justify escalating penalties by incorporating fairness and the social norm of law compliance.

Next, I perform empirical analysis of repeat violations in California and find limited evidence of escalating penalties. I find that most facilities are compliant; 71.3% were compliant in at least eight out of the nine years I study. I also find no evidence that penalties escalate. Somewhat surprisingly, facilities that were in violation for longer periods of time were assessed smaller penalties on average per year of violation. I argue that this is because repeat violations are less severe, and thus do not merit large penalties. Therefore, even though penalties do not escalate, persistent violators might not be "bad apples" as their violations are less serious.

Thus, my dissertation examines the impact of monitoring and enforcement policy and shows that penalties are effective at reducing emissions and improving air quality. This is a relatively new finding; very little of the previous literature on monitoring and enforcement has found that penalties are effective at improving environmental performance. My dissertation shows that penalties are effective as a part of an aggressive air pollution regulation regime like California's.

CHAPTER I

EXAMINING THE IMPACT OF MONITORING AND ENFORCEMENT ON STATIONARY SOURCE EMISSIONS

Introduction

Public monitoring and enforcement are an important part of any regulatory regime. This is especially true for environmental regulations as environmental harms can be complex and widespread and private parties are often ill equipped to perform monitoring and enforcement. At the same time, monitoring and enforcement actions are costly, and, in order to justify the cost, they should produce environmental benefit. This chapter investigates how monitoring and enforcement actions affect stationary source emissions, focusing on emissions of nitrogen oxides (NO_x), an important air pollutant that is responsible for major air quality problems but has not been studied. This chapter is the first research that examines how monitoring and enforcement affect criteria pollutant emissions, and I find that penalties decrease emissions.

The U.S. Environmental Protection Agency (EPA) has identified ozone and fine particles as the pollutants that cause the most significant human health effects (EPA 2011a). Ozone and fine particles are two of six “criteria pollutants,” commonly found air pollutants that affect human health and the environment, for which the EPA has set national air quality standards. Ozone is not emitted directly, but is created by chemical reactions between NO_x and volatile organic compounds. There are two types of particulate matter: particulate matter with a diameter of 10 micrometers or less (PM_{10}) and particulate matter with a diameter of 2.5 micrometers or less ($PM_{2.5}$). $PM_{2.5}$, also known as fine particles, may be emitted directly but is mostly formed by reactions between other air pollutants such as sulfur dioxide and NO_x. Because NO_x is a precursor of both ozone and fine particles, it is an important pollutant on which regulators focus.

As criteria pollutants are ubiquitous, it is crucial to establish the impact that monitoring and enforcement have on criteria pollutant emissions. Unfortunately, due to the limited availability of data,[1] the literature has not done so. Articles that studied the effect of regulatory actions on air pollution have focused on compliance and not emissions (e.g., Gray and Deily 1996; Nadeau 1997; Deily and Gray 2007); those that studied emissions examined toxic chemicals covered by the Toxics Release Inventory[2] (TRI) (Hanna and Oliva 2010).[3] However, the TRI does not cover NO_x and other criteria pollutants, and researchers have not examined emissions of criteria pollutants. This chapter is the first to study the impact of monitoring and enforcement on emissions of criteria pollutants.

Some of the earliest work on monitoring and enforcement focused on the impact of monitoring and enforcement on oil spills. For instance, Epple and Visscher (1984) found that increasing monitoring activity reduced the amount of oil spilled, and Cohen (1987) found that certain types of monitoring activity were more effective than others. Researchers also found mixed evidence on the effectiveness of penalties. For example, Viladrich Grau and Groves (1997) found that enforcement reduced the probability of oil spills as well as the spill volume, but found that the size of the penalty had no significant

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[1] Gray and Shimshack (2011) point out that while air pollution compliance is observable to researchers, emissions are not.
[2] The Emergency Planning and Community Right-to-Know Act requires manufacturing plants to report releases of certain toxic chemicals. These releases are reported to a database known as the Toxics Release Inventory.
[3] On the other hand, researchers studying the impact of monitoring and enforcement on water pollution have examined both compliance and effluent (e.g., Magat and Viscusi 1990).

impact. On the other hand, Weber and Crew (2000) found that the size and swiftness of the penalty reduced the size of oil spills, but the certainty of the penalty did not.

Other studies have explored the relationship between regulatory actions and a facility's compliance and pollution releases for both air and water pollution.[4] Water pollution studies generally investigated the relationship between regulatory action and effluent and compliance. For example, Magat and Viscusi (1990) measured the effect of inspections of pulp and paper plants on the plant's compliance with the Clean Water Act and found that inspections in the previous quarter increased compliance and decreased effluent. Laplante and Rilstone (1996) performed a similar study on water pollution at pulp and paper plants in Quebec and found that current and previous inspections lowered absolute discharges and discharges relative to the norm, and that the predicted probability of inspection produced larger reductions than actual inspections. On the other hand, Earnhart (2004a, b) studied municipal wastewater plants in Kansas and found that actual inspections and enforcement actions reduced effluent but predicted inspections and enforcement actions had no significant impact.

Articles that investigated air pollution have studied the impact of monitoring and enforcement on whether a facility complies with its permit limits, the duration a facility remains noncompliant, and emissions.[5] Gray and Deily (1996) used both lagged and predicted inspections and enforcement actions in their study of Clean Air Act (CAA) compliance at steel-making plants. They found that the predicted number of inspections and enforcement actions had no effect on compliance, but actual inspections and

[4] For the purposes of this chapter I refer to water pollution releases as effluent and air pollution releases as emissions.
[5] Keohane, Mansur, and Voynov (2009) studied the impact of impending litigation on emissions at power plants and found that litigation reduced emissions. As this chapter focuses on aspects of regulation other than litigation, I do not discuss this study in depth.

enforcement actions increased compliance. In a later paper (Deily and Gray 2007), their investigation of the joint effect of environmental regulations and health and safety regulations yielded similar results.

Nadeau (1997) studied how the number of inspections and enforcement actions affected the duration a facility remained out of compliance. He found that predicted monitoring and enforcement actions, when predictions were based on the noncompliant sample, significantly reduced the duration a facility remained noncompliant.

Lastly, Hanna and Oliva (2010) studied emissions of toxic chemicals covered by the TRI at manufacturing plants between 1985 and 2001. They found that actual CAA inspections decreased emissions of toxic chemicals but the probability of inspection had no significant effect. They also found that fines seemingly increased emissions; however, they argued that industries with high abatement costs preferred fines to abatement, thus producing this counterintuitive result.

In this chapter, I use a dataset of stationary source emissions in California to examine the impact of monitoring and enforcement on NO_x emissions. I find that penalties produced significant reductions in NO_x emissions. A majority of the surveyed literature studied water pollution; of the articles that studied the effect of regulatory actions on air pollution, only one examined emissions—Hanna and Oliva (2010) studied toxic emissions at manufacturing facilities in the United States. This chapter extends the literature by examining NO_x, an important pollutant that has not been studied because of the lack of data. Furthermore, limiting the sample to one jurisdiction, California, reduces the heterogeneity in regulatory policy and allows me to better understand state and local regulatory policy. This is especially important because state and local regulators play a major role in the implementation of the CAA.

Regulatory Background

The CAA is a comprehensive national air pollution control program that regulates nation-wide air quality. Under § 109 of the CAA, the EPA establishes national ambient air quality standards (NAAQS) for six commonly found pollutants that harm health and the environment. These pollutants, also called criteria pollutants, are ozone, particulate matter, carbon monoxide, nitrogen oxides, sulfur dioxide, and lead. Although the NAAQS are federal standards, states have primary responsibility for achieving and maintaining these standards. Sources located in areas that are in NAAQS nonattainment face more stringent regulations, such as lower emissions limits, that aim to bring the area into attainment.

In California, stationary source monitoring and enforcement are handled primarily by thirty-five local air districts, not by the EPA.[6] Monitoring and enforcement practices depend on the type of source: major sources are sources that emit (or have the potential to emit) more than 100 tons per year of any pollutant and synthetic minor sources are sources that emit (or have the potential to emit) above 80% of the major source threshold (EPA 2001). Minor sources are sources whose potential uncontrolled emissions are below 100 tons per year. In my analysis, I focus on inspections, enforcement actions, and penalties. There are two types of inspections, full compliance evaluations (FCEs) and

[6] Local air districts are responsible for almost 99% of the regulatory actions in my data, so my discussion focuses on air district policy and practice, not EPA policy and practice. I base this description of air district policy on discussions with staff at the four largest air districts, which constitute 80% of my sample.

partial compliance evaluations (PCEs), and three types of enforcement actions, notices of violation (NOVs), administrative orders, and consent decrees. Administrative orders are usually accompanied by penalties.

An FCE is a comprehensive evaluation of the facility, addressing all regulated pollutants and emission units, and a PCE focuses on a subset of pollutants, requirements, or emission units and can be used to address particular areas of concern at a facility (EPA 2001). Air districts typically perform an FCE once every two years for major sources and once every five years for synthetic minor sources. Districts typically perform other smaller inspections, PCEs, when there are complaints, ongoing violations, or reports of equipment breakdown. All districts report FCEs to the EPA's data system.[7] Most districts do not report PCEs, but some districts report some or all of their PCEs.[8] For districts that do not report PCEs, I enter zero PCEs. In my analysis, I consider both FCEs and PCEs as inspections.

There are three types of enforcement actions: NOVs, administrative orders, and consent decrees.[9] The enforcement process begins with the discovery of a violation; inspectors can discover a violation through self-reporting, record review, or inspections. Upon discovering a violation, inspectors typically issue an NOV. A facility with multiple violations might receive one NOV for all the violations or one NOV for each violation; there is no fixed practice regarding the number of NOVs. The time between detecting a violation and issuing an NOV can vary. An NOV can be issued the same day, but it may take several weeks for more involved violations, such as violations that require review of

[7] Although air districts may perform FCEs via a series of PCEs, they report the underlying FCEs, not the individual PCEs.
[8] The San Joaquin Valley Air Pollution Control District reports some of its PCEs, and the South Coast Air Quality Management District reports all of its PCEs.
[9] I based this classification on the EPA's classification (EPA 2011b, 2012b).

the facility's records. District regulators place an emphasis on correcting violations; once detected, violations are usually remedied quickly, sometimes the same day. NOVs usually expose the facility to penalties.

After receiving an NOV, facilities typically resolve the matter administratively, and the district assesses a penalty and issues an administrative order. An administrative order might deal with multiple violations or multiple NOVs. The time between an NOV and an administrative order varies, but it mostly takes less than nine months.

It is worth noting that, in California, NOVs typically expose the facility to penalties and almost all NOVs end up as administrative orders.[10] An NOV notes a violation that is later addressed by an administrative order. However, because an administrative order can address multiple violations or multiple NOVs, there might not be a one-to-one relationship. Furthermore, most violations are corrected early in the process, before the penalty is assessed. Facilities rarely, if ever, refuse to correct a violation.

Administrative orders may include other requirements. For instance, an administrative order could include a shutdown order, which requires the facility to stop operating the particular piece of equipment, or it could include a variance, which loosens the facility's permit restrictions. Obviously, the shutdown order is far more costly than the variance, and this aspect of the enforcement process is important. However, I do not have any of these details about the administrative orders. Figure 1 shows the timeline of a typical violation.

Some cases might go through the judicial process instead of the administrative process, which can take three to five years and usually ends in a consent decree.

[10] This is different from other jurisdictions where only some of the NOVs expose the facility to penalties.

However, this is rare; almost all cases go through the administrative process.[11] Districts also vary in their reporting practices; all districts report violations that are considered high-priority violations,[12] but some districts report some or all violations that are not considered high-priority violations.[13] In my analysis, I collapse NOVs, administrative orders, and consent decrees into one enforcement actions variable.

Theoretical Model

I assume that the facility is run by a profit-maximizing, price-taking firm, and the only violation is a breakdown of abatement equipment. The firm chooses the quantity produced, q, and the amount spent maintaining abatement equipment, a, to maximize expected profits. The firm's variable costs are determined by the cost function $c(q)$. As quantity increases, cost increases at an increasing rate; that is, $c_1(q) > 0$ and $c_2(q) > 0$, where c_1 and c_2 are the first and second derivatives of c respectively. The probability of breakdown of abatement equipment is determined by the function $b(a)$, where the probability is between zero and one, $0 < b(a) \leq 1$. As abatement expense increases, the probability of breakdown decreases at a decreasing rate; that is, $b_1(a) < 0$ and $b_2(a) > 0$. If there is a breakdown, the violation is detected with probability d, and the firm faces a fine f if detected. Thus, the firm's expected profit is given by

[11] Only 1.1% of the enforcement actions in my data are consent decrees.
[12] High-priority violations are violations that the EPA believes should receive the "highest scrutiny and oversight" (EPA 1998, p. 3). These include more serious permit, emissions, and testing violations, and chronic violations. All districts report high-priority violations; the San Joaquin Valley Air Pollution Control District reports some of its violations that are not considered high-priority violations, and the South Coast Air Quality Management District reports all of its violations regardless of whether they are considered high-priority violations.
[13] Regulatory authorities also have other types of enforcement actions at their disposal, such as notices to comply. However, as these are not reported to the EPA database, I do not discuss them.

$$\pi = p \times q - c(q) - a - b(a) \times d \times f, \tag{1}$$

where π is the expected profit and p is the price of the firm's output. Taking the derivative with respect to quantity gives equilibrium quantity

$$q^* = c_1^{-1}(p), \tag{2}$$

where c_1^{-1} is the inverse of c_1. Because $c(q)$ increases at an increasing rate ($c_2 > 0$), c_1 is a monotone increasing function and so c_1^{-1} is also a monotone increasing function. Therefore, as p increases, q^* also increases; quantity produced increases as price increases. Similarly, taking the derivative of π with respect to a gives equilibrium abatement expense

$$a^* = b_1^{-1}(-\tfrac{1}{d \times f}). \tag{3}$$

Because $b(a)$ decreases at a decreasing rate ($b_2 > 0$), b_1 is a monotone increasing function and so b_1^{-1} is also a monotone increasing function. Therefore, as d and f increase, a^* also increases; abatement expense increases as the probability of detection increases and the size of the fine increases. For notational convenience let $q^* = c_1^{-1}(p) = q^*(p)$ and $a^* = b_1^{-1}\left(-\frac{1}{d \times f}\right) = a^*(d, f)$. Thus far, I have established that $q_1^*(p) > 0$, $a_d^*(d, f) > 0$, and $a_f^*(d, f) > 0$, where a_d^* and a_f^* are the partial derivatives of $a^*(d, f)$ with respect to d and f, respectively.

Next, assume that if abatement equipment breaks down, emissions are equal to the quantity produced, $e = q$. If abatement equipment is working, emissions are reduced to a fraction of uncontrolled emissions, $e = r \times q$, where $0 \leq r < 1$. Thus, expected emissions are $e = q \times \big(b + r \times (1 - b)\big)$. Substituting q^* and a^* in, expected emissions are

$$e = q^*(p) \times (b(a^*(d, f)) \times (1 - r) + r). \quad (4)$$

The chain rule implies that $e_p = e_q \times q_1^*$. Thus, the partial derivative of e with respect to p is

$$e_p = q_1^* \times (b \times (1 - r) + r). \quad (5)$$

As previously established, $b > 0$, $1 - r > 0$, $r \geq 0$, and $q_1^* > 0$; thus $e_p > 0$. This means that as prices increase, emissions increase. Similarly, $e_d = e_b \times b_1 \times a_d^*$ and $e_f = e_b \times b_1 \times a_f^*$. Thus,

$$e_d = q^* \times (1 - r) \times b_1 \times a_d^* \text{ and} \quad (6)$$

$$e_f = q^* \times (1 - r) \times b_1 \times a_f^*. \quad (7)$$

As previously established, $q^* > 0$, $1 - r > 0$, $b_1 < 0$, $a_d > 0$, and $a_f > 0$; therefore, $e_d < 0$ and $e_f < 0$. This means that emissions decrease as the probability of detection increases, and emissions decrease as the size of the fine increases.

Thus, more inspections, which increase the probability of detection, d, should decrease emissions. An increase in penalties, which increases f, should also decrease emissions. Enforcement actions can affect both probability of detection d and fines f. An increase in the number of enforcement actions might reflect an increase in detection of violations, d; at the same time, repeat offenders might also face larger fines, thus an enforcement action might reflect an increase in f. Therefore, enforcement actions should decrease emissions.

Data Description

I obtain emissions data from the California Air Resources Board's (CARB) emissions inventory and monitoring and enforcement data from the EPA's Air Facility System (AFS) database. While I do not restrict my sample to specific industries like other articles do, potentially introducing heterogeneity among the sources, studying only California limits heterogeneity in monitoring and enforcement policy. This also allows me to get a better understanding of state and local regulatory policy.

CARB's emissions inventory contains information about facilities regulated by California, including facility name, address, city, county, and air district responsible for its regulation, as well as amount of emissions the facility produced in that year. Every year, each facility's emissions are estimated by the air district based on information submitted by the facility, such as fuel usage, and other information about the facility, such as equipment and abatement technology. Air districts compile this information for all facilities that emit over ten tons of pollutants per year.

Emissions data are available between 1995 and 2008. However, I limit my period of study to 2002-2008 because compliance monitoring policy and data reporting practices changed significantly in 2001.[14] I also dropped the facilities that produced no NO_x over the entire period—all the facilities in my sample had positive emissions in at least one year. The mean facility NO_x emissions, by year, are presented in Table 1. NO_x decreases steadily from a mean of 79.7 tons per year in 2003 to 64.0 tons per year in 2008. The overall mean NO_x emissions per facility per year, across the entire time period, is 73.0

[14] The EPA adopted a new Compliance Monitoring Strategy in October of 2001 (EPA 2001). This introduced new types of regulatory actions and changed the recommended inspection frequency.

tons. I also calculated each facility's mean over the entire time period. There are 857 facilities; the facility with the lowest NO_x emissions over the entire period emitted just 0.0002 tons per year, while the facility with the highest NO_x emissions over the period emitted 4,853 tons per year.

I restricted the sample to facilities that existed during the entire study period. Despite that, there are missing values. Even though there are 857 facilities, which should generate 5,142 observations, there are only 4,702 observations because of missing values. The number of missing values decreases steadily between 2003 and 2008.

Different factors could have caused the missing values, and each reason implies a different method of overcoming the problem. First, the facility might have shut down permanently or new facilities might have begun operation; I control that by limiting the sample to facilities that existed through the entire period. Second, the facility might have failed to submit information due to reasons unrelated to emissions or regulatory actions. In this case, missing values will not bias the results. Third, the facility might have shut down temporarily due to factors such as a short-term decline in demand; in this case, these missing values should be treated as zero tons of emissions. Air districts usually fill in zero if this is the case, but it is still possible that such observations slip through as missing values.

Fourth, the facility might have failed to submit emissions information due to reasons related to emissions or monitoring and enforcement actions. For example, facilities might choose not to submit information if emissions are unusually high, or a high number of regulatory actions at the facility might burden the facility's environmental staff with other responsibilities and cause them to fail to submit the information. Regulators indicated that they did not think that facilities were trying to hide high emissions levels by not submitting the required information. Because I am unable to discern the reason for the missing values, I treat the missing values as randomly missing values. Thus, I do not perform any corrections to compensate for missing values. As a precaution, I also run additional regression equations on the subsample of facilities that have no missing NO_x values.

The AFS is the EPA's database for CAA-regulated sources. The database contains details of each polluting facility, such as its geographic coordinates, address, program identification number, and permit type. Additionally, it has details of regulatory actions since 2002: the date and type of regulatory action as well as the penalty. I classify all state and federal FCEs and PCEs as inspections, and all state and federal NOVs, administrative orders, and consent decrees as enforcement actions. Table 2 shows a summary of the number of inspections and enforcement actions and total penalty per facility-year.

There is a mean of 1.93 inspections per facility-year, and 73.6% of the facilities-years receive at least one inspection. The mean number of enforcement actions is 0.76 and 23.6% of the facility-years receive at least one enforcement action. The mean penalty per facility-year is $12,994. However, only 17.2% of the facility-years have any penalties. Conditional upon a penalty, the mean is $75,564.[15] As most penalties are small but there are several very large penalties, my regressions use the natural logarithm of

[15] The AFS has information on the compliance status of the facility; many studies have used compliance status as the dependent variable. However, the EPA cautions that compliance status is subject to errors of omission (EPA 2012a). Facility noncompliance in the AFS is a flag that regulators have to flip on or off when a facility becomes noncompliant or comes back into compliance. As it is located in a different part of the data system, regulators often overlook entering this data. In my sample, the average compliance rate is 97.5%. Facilities that have received any sort of enforcement action in the current or previous year—and thus should be considered noncompliant—have a compliance rate of 94.3%. This indicates a high level of inaccuracy. As this chapter focuses on emissions, I do not investigate compliance further.

penalties. I add one to all penalties before taking the natural logarithm as there are many zero values. The mean of the logarithm of penalties is 1.47.

Note that the data only show whether the facility received an enforcement action; they do not specify which pollutant the enforcement action concerns. Thus, I cannot tell if the facility violated a NO_x emissions limit or some other permit condition. Additionally, while some of the water pollution literature used information on individual effluent pipes within the facility, my data treat the facility as the unit of observation, and I do not have information on individual smokestacks within a facility. Lastly, while one firm may own many facilities, the data are at the facility level, so I study the facility as the unit of observation, not the firm.

Table 3 shows the correlations between current- and previous-year regulatory actions. Part A of the table shows the correlations between the number of actions and logarithm of the penalty, while part B shows the correlations between the dummy variables for whether the facility received the specific regulatory action or penalty. The number of inspections is weakly correlated with other regulatory actions and their lagged values, probably because most inspections are performed at a predetermined frequency. It is also worth noting that enforcement actions are highly correlated with penalties because most violations expose the facility to penalties.

I merge AFS inspection and enforcement information with CARB emissions information based on facility name and address. I manage to match about two-thirds of the AFS facilities to their emissions. As AFS removes facilities that have shut down, I restrict the study to facilities that have been in the sample for the entire period, excluding those that were in existence during the start of the period but shut down during the period and those facilities that began operating during the period.[16] Whether this biases the results depends on why the facilities shut down or begin operating during the period. If the facilities begin operation and shut down for reasons unrelated to regulatory actions and are affected by regulatory action in the same way that existing firms are, then this limitation of the sample should not bias results. If facilities shut down due to regulatory pressure, for instance, if large penalties render the facilities unprofitable, then the regulatory actions would have reduced emissions to zero; thus, in this case, my results would underestimate the impact of regulatory actions. New facilities that began operation during the study period might have better pollution abatement equipment. Thus, regulatory actions against these facilities might be less effective because they already have relatively advanced abatement equipment. In this case, my regression results would overestimate the impact of regulatory actions.[17]

I also control for air quality, including variables for NAAQS attainment status, and demographic factors, including variables for per capita income, unemployment, and percent white at the county-year level. Being in a nonattainment area might increase regulatory activity at a facility but might also cause a facility to face other pressure from the community to control emissions. Thus, I include NAAQS attainment status, from the EPA's Green Book, to avoid omitted variables bias. I control for attainment status for

[16] I use compliance information to decide whether a facility was operating; I took its earliest compliance entry as the earliest date it was in operation. Additionally, through a Freedom of Information Act request, I obtained information regarding facilities that had shut down and were thus removed from the AFS. However these data were very difficult to match with the emissions inventory data. Thus, I did not pursue this.

[17] Future iterations of this project can add facilities that have shut down (from the data I received through a Freedom of Information Act request) and facilities that began operation during the time period (which are already in the EPA database).

ozone, PM_{10}, $PM_{2.5}$, and carbon monoxide.[18] I do not control for attainment status for nitrogen oxides, sulfur dioxide, and lead because all areas are in attainment of those standards. I get information about per capita income from the Bureau of Economic Analysis and unemployment rate from the Bureau of Labor Statistics and include them in my regressions to control for price and cost of a facility's output. I also obtain information about the percentage of white people in the county from the Census; counties with a large minority population might have less political power and less regulatory pressure to lower emissions, and omitting this might cause omitted variables bias.

Table 4 presents a brief description and summary statistics for each variable. As the table shows, many facilities are situated in nonattainment counties. Sixty-seven percent of the facility-years are in PM_{10} nonattainment counties while 45.4% of the facilities-years are in $PM_{2.5}$ nonattainment counties. Ninety-one percent of the facility-years are in ozone nonattainment counties, while 31.3% of the facility-years are in carbon monoxide nonattainment counties. The average unemployment rate is 6.5%, average income is \$37,191, and average percent of white population is 78.6%.

Econometric Methods

When investigating the impact of monitoring and enforcement on pollution, researchers have to overcome reverse causality. For instance, high emissions may attract regulatory action, leading to the mistaken conclusion that regulatory action causes noncompliance. Researchers have developed several methods of overcoming reverse

[18] Nonattainment classification for $PM_{2.5}$ started only in 2005. All areas were classified as in attainment prior to 2005. In my regressions, year dummy variables account for this.

causality: including a wide range of control variables; limiting the analysis to facilities in one industry; and adjustments to the regulatory action explanatory variable.

Most studies combat this problem by including a wide range of control variables, such as the facilities' output and production process (e.g., Magat and Viscusi 1990). For example, if a facility's output increases, it might increase emissions but also draw regulators' attention. Including an extensive set of controls can alleviate such problems. I do not have detailed facility information, but I use facility fixed effects, control for income, unemployment rate, percent white, and NAAQS attainment status on the county-year level, and control for time trends.

Furthermore, most articles constrain analysis to facilities in one industry or several similar industries, such as pulp and paper mills, to ensure that the facilities studied are approximately similar and thus would react similarly to regulatory action. Not limiting the sample in this way can exacerbate causality problems. For example, Hanna and Oliva (2010) studied all manufacturing facilities and found that facilities that were fined did not reduce their emissions while facilities that were not fined reduced their emissions. They argued that this was due to the differences across industries; industries with high abatement costs preferred fines to emissions reduction. Thus, these facilities polluted more and also faced higher fines, causing the positive relationship between emissions and fines. Although I do not limit my sample to facilities in specific industries, I use fixed effects regression analysis. Fixed effects regression measures variation at a facility over time, so time-invariant characteristics, like the industry of the facility, will not cause the problem that Hanna and Oliva faced. I also ran additional regressions limiting my sample to sources considered to be major sources and manufacturing facilities.

Articles addressed reverse causality by making three different adjustments to the explanatory variables: using lagged regulatory variables, using the predicted probability of regulatory action, and using regulatory action directed at other facilities as an instrumental variable. Researchers most commonly compensate for reverse causality by regressing compliance or pollution releases in the current time period against monitoring and enforcement actions in previous time periods. Lagged variables avoid reverse causality because current noncompliance or pollution releases cannot cause inspections or enforcement actions in a preceding time period. For example, Magat and Viscusi (1990) found that previous-quarter inspections increased compliance and decreased effluent at pulp and paper plants.

Other studies addressed the reverse causality problem by using the predicted probability of an inspection or enforcement action as explanatory variables, in addition to using lagged regulatory variables. Laplante and Rilstone (1996) studied water pollution at pulp and paper plants in Quebec and found that current and previous inspections lowered discharges and the fitted probability of inspections produced larger reductions than actual inspections. However, Earnhart (2004a, b) performed similar studies on publicly owned municipal wastewater plants in Kansas and found that actual inspections and enforcement actions reduced effluent but predicted inspections and enforcement actions did not reduce effluent, and even increased effluent in some cases.

For air pollution studies, researchers have generally found that actual inspections and enforcement actions increased compliance or lowered emissions, but the predicted probability of inspection and enforcement actions had no significant impact. Gray and Deily's (1996) study of CAA compliance at steel-making plants found that the predicted number of inspections and enforcement actions had no effect on compliance, but if the plant had been inspected or faced an enforcement action in the past two years, it was more likely to be in compliance. Similarly, Hanna and Oliva (2010) studied emissions of toxic chemicals at manufacturing plants and found that actual CAA inspections decreased emissions of toxic chemicals but the probability of inspection had no significant effect.

Lastly, researchers used instrumental variables to compensate for reverse causality. In their study of pulp and paper mills, Shimshack and Ward (2005) used the rate of inspection at other regulated plants in the same jurisdiction as an instrument for inspections at the facility. They reasoned that, as pulp and paper plants formed a very small portion of the regulator's responsibilities, inspections at other regulated plants were unrelated to any idiosyncratic plant-specific effects. Thus, they argued, the variable reflected an overall rate of inspection and met the exclusion restriction. They found that a fine (both the presence and magnitude of the fine) in the previous year on any plant in the same jurisdiction increased compliance at that plant, as did a previous-year inspection on the plant. Current inspections, for which the authors used instrumental variables, had no significant impact. On the other hand, Earnhart (2004b), in his study of municipal wastewater treatment plants, used enforcement at all regulated plants in the jurisdiction as an explanatory variable (not an instrumental variable) and found that the general enforcement rate reduced effluent.

The studies mentioned above generally found that actual inspections and enforcement actions improved compliance and decreased pollution released. However, predicted inspections and enforcement actions seem to be ineffective: only one of the studies found that predicted inspections reduced effluent (Laplante and Rilstone 1996), and there is little evidence that predicted enforcement had any impact. This is, perhaps, because the predicted probability of regulatory action is a poor measure of the polluter's perceived probability of regulatory action.

In this chapter, I use the fixed effects regression model, which examines the changes in NO_x at the same facility across time. This can reduce problems presented by differences in facilities across industries. I also use lagged regulatory variables to control for reverse causality. I describe my regression models in more detail in the next section.

Regression Analysis

In order to analyze the impact of monitoring and enforcement on emissions, I use a fixed effects regression model. The econometric model is represented by

$$y_{it} = RegulatoryAction_{it}'\beta + X_{it}'\gamma + a_i + Year_t + \varepsilon_{it}. \quad (8)$$ [19]

Emissions for facility i at time t are represented by y_{it}. In this case, it measures the tons of NO_x released in that year. Time-invariant facility characteristics, such as technology, industry, or location of the facility, are captured by a_i. Year dummy variables are represented by $Year_t$, and capture general time trends in emissions. Other time-varying characteristics are captured by X_{it}; this controls for demographic characteristics (income, unemployment rate, and percent white) and attainment status (for particulate matter, ozone, and carbon monoxide) of the county in which the facility is located.

[19] While some articles control for the lagged dependent variable $y_{i,t-1}$, controlling for it in this case would cause estimators to be inconsistent because fixed effects regressions are estimated by mean-differencing (Cameron and Trivedi 2010).

The variable $RegulatoryAction_{it}$ represents regulatory action, such as inspections and enforcement actions taken or penalties assessed, which can be measured in two different ways. One option is to use the number of regulatory actions, such as the number of inspections or number of enforcement actions, taken in any given year, directly. This has the advantage of providing a more complete picture: if a facility is subject to more inspections, then it should show a larger effect on emissions than a facility that is subject to fewer inspections.

The second option is to use dummy variables to indicate whether the facility has received a particular regulatory action. This would make sense if the presence of regulatory action, not the number of regulatory actions, is what drives any changes in emissions. Additionally, in my data, the number of regulatory actions might be driven primarily by the size of the facility, not the degree of regulatory scrutiny the facility is facing. Because I do not limit my data to facilities in specific industries, there is great variation in the size of the facilities and therefore great variation in the number of regulatory actions. For example, a large facility can receive multiple enforcement actions for multiple smokestacks but a small facility with only one smokestack is unlikely to receive multiple actions. Thus, the number of regulatory actions is perhaps more representative of the size of the facility than the degree of regulatory scrutiny the facility is facing; this would make dummy variables more appropriate.

Furthermore, dummy variables can minimize problems in my data. For instance, the data show that many large facilities received multiple FCEs per year. This is likely a reporting error as inspectors state that it is highly unlikely that they could complete multiple FCEs on a large facility in a year because FCEs at large facilities are time consuming.[20] The literature has taken both approaches, so I run the regression using both the number of actions and dummy variables.

Additionally, the measure of regulatory action should account for reverse causality. As previously mentioned, using current regulatory action as the explanatory variable creates the risk of reverse causality. One solution is to use regulatory actions in the previous year instead of the current year, $RegulatoryAction_{i,t-1}$ instead of $RegulatoryAction_{it}$. This avoids reverse causality because emissions this year cannot cause regulatory actions in the previous year. Thus, my regression equation is

$$y_{it} = RegulatoryAction'_{i,t-1}\beta + X'_{it}\gamma + a_i + Year_t + \varepsilon_{it}. \tag{9}$$

In the equation, $RegulatoryAction_{i,t-1}$ is a vector of variables representing the number of regulatory actions in the previous year: inspections, enforcement actions, and penalties in the previous year. As the distribution of penalties is very skewed, I use the natural logarithm of penalties instead of the amount of penalties. As discussed in the theoretical model, increasing the probability of detecting a violation and increasing the size of penalties should decrease emissions. Thus, I expect the coefficients of inspections, enforcement actions, and penalties to be negative.

In regression (1) of Table 5, I present the effect of having at least one previous-year inspection, at least one previous-year enforcement action, and a positive previous-year penalty on emissions. The coefficient of penalties is negative and significant at the 10% level. If a facility received a penalty in the previous year, its NO_x emissions decrease by 5.60 tons on average, which is about 7.7% of the mean emissions of 72.98

[20] Such recording mistakes could happen if, for instance, a facility is inspected once for two different air programs, hazardous air pollutants and criteria pollutants. Although it is only one action, it might be recorded twice because it involved two air programs. The AFS tries to minimize such mistakes but they still occur.

tons. Surprisingly, enforcement actions have no significant impact on emissions. This is probably because enforcement actions are often accompanied by penalties; thus, enforcement actions alone have no significant impact as penalties capture the deterrent effect of the enforcement action.

In the regression, inspections do not affect emissions. This is probably because there is not much variation within the facility. Fixed effects regression tracks each facility over time; for an inspection to produce a significant impact on pollution, the inspection rate at a facility must change over time. Most air districts perform inspections at the same frequency through the study period, once every two years for major sources and once every five years for synthetic minor sources, so there is not much within-facility variation. Furthermore, the inspection rate is high, with 73.6% of facility-years receiving at least one inspection, and a marginal increase in inspections might have no impact. Thus, these two factors might be responsible for the lack of significance of the coefficient of inspections. It is worth noting that other studies have found that inspections improved compliance and decreased pollution releases. For example, Hanna and Oliva (2010) found that inspections significantly reduced toxic emissions, but they had dropped facilities that face yearly inspections from their sample. Other studies (e.g., Gray and Deily 1996) also found that inspections improved compliance. My sample is more recent and focuses on California, which has an aggressive inspection regime; thus, it likely has less variation in inspection rates, causing the insignificant coefficients.

In regressions (2), (3), and (4), of Table 5, I run the same regression with different samples. Regression (2) displays the regression results when I use only facilities that are considered major sources. The results are similar, and a penalty reduces emissions by

6.24 tons. Regression (3) displays the regression results when I use a balanced panel of facilities that have no missing NO_x values. The coefficient of penalties remains negative and significant; receiving a penalty reduces NO_x emissions by 6.68 tons. Lastly, regression (4) displays the regression results for facilities that are in the manufacturing industry (Standard Industrial Classification code 20-39). The coefficient of penalties remains negative and significant; receiving a penalty reduces emissions by 10.73 tons for manufacturing facilities.

In Table 6, I present the results of regressions when I use the number of regulatory actions (the number of inspections, the number of enforcement actions, and the natural logarithm of total penalties) as the explanatory variable instead of dummy variables that indicate the presence of a regulatory action. The coefficient of penalties remains negative and statistically significant in most specifications. Regression (1) displays the effect of previous-year inspections, enforcement actions, and penalties on emissions for all facilities. The coefficient of penalties is negative and significant at the 10% level; increasing the penalty by 1% decreases NO_x emissions by 0.007 tons. Although this number is small, penalties are relatively large and can cause large reductions. For instance, increasing the penalty from $1 to the average of $12,994 decreases emissions by 6.41 tons; increasing the penalty from the 25th percentile to the 75th percentile of the distribution of positive penalties (an increase from $1,500 to $12,896), decreases emissions by 1.46 tons. A 1.46 ton reduction is approximately 2.0% of the mean emissions of 72.98 tons.

Regressions (2) to (4) display the regression results when I use different samples. The results are similar; the coefficient of penalties remains negative and significant for all the samples except the balanced panel.

Robustness Tests

Next, I examine whether the fixed effects results in Table 5 and Table 6 are robust. First, I examine whether omitting year dummy variables and fixed effects affects the results. Table 7 shows the coefficients of the regulatory action variables when I omit year dummy variables. Regressions (1) to (4) are very similar to the analogous regressions in Table 5 and regressions (5) to (8) are very similar to the analogous regressions in Table 6. Thus, the results are robust to omitting time trends.

Additionally, I examine how using pooled ordinary least squares, which omits facility fixed effects, affects the result. I run the regression

$$y_{it} = RegulatoryAction'_{i,t-1}\beta + X'_{it}\gamma + \alpha + Year_t + \varepsilon_{it}. \tag{10}$$

where, instead of facility fixed effects, α_i in equation (9), there is a common intercept term, α. Table 8 shows the results: inspections and enforcement actions seem to increase emissions. This is likely due to the fact that regulators focus their regulatory actions on facilities that produce more emissions. Using fixed effects mitigates this reverse causality problem. Table 9 shows that the results are similar if I omit both fixed effects and time trends.

Next, I examine whether omitting various control variables affects the results. Table 10 displays the coefficients of regulatory actions when I run a fixed effects regression on all facilities but omit various control variables. Regressions (1) to (3) are similar to regression (1) in Table 5, examining how the presence of regulatory actions affects NO_x emissions, and regressions (4) to (6) are similar to regression (1) in Table 6, examining how the number of regulatory actions affects NO_x emissions. Omitting various control variables does not affect the significance and magnitude of the results. For example, as regression (1) of Table 10 shows, if I omit the NAAQS nonattainment variables from the regression, the coefficient of penalties is still significant and similar in magnitude, -5.70. As the table shows, the results are robust to omitting various control variables. In Appendix A, I present the regression results for the other subsamples, major facilities, the balanced panel, and manufacturing facilities, as well as further robustness tests.

Conclusion

This chapter is one of the few studies to directly examine air pollution emissions, and the first to examine the effect of monitoring and enforcement on NO_x and criteria pollutant emissions. Furthermore, by limiting my study to one jurisdiction, I am able to better understand the monitoring and enforcement policies of the regulator; this is important because state and local regulators play a big role in the CAA.

Penalties have the most robust effect; having a positive penalty reduces emissions by an average of 5.60 tons and increasing the penalty from the 25th percentile to the 75th percentile of the penalty distribution reduces emissions by 1.46 tons. The coefficients of inspections and enforcement actions are not significant, perhaps indicating that the deterrent impact of the regulatory regime lies in the penalties. Previous work has found that inspections and enforcement actions improved pollution outcomes. On the other hand, little work has found penalties to be effective, with the exception of Shimshack and Ward's (2005) finding that penalties on other facilities in the same jurisdiction improve compliance and Weber and Crew's (2000) finding that larger penalties reduce the size of oil spills.

My result, that penalties are effective, might be due to California's uniquely aggressive pollution control regime and the certainty with which a violation results in a penalty. First, the inspection rate in California is very high; the inspection rate for all states other than California was 24.5%, far less than the inspection rate of 73.6% in my sample. Thus, it is more likely that violations will be detected and result in a penalty. Second, in California, almost every violation exposes the facility to a penalty, even if the violation is fixed quickly. Thus, given the relative certainty of penalties, it is unsurprising that penalties are effective at improving facility behavior.

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Tables

Table 1. Mean Facility NO_x Emissions in Tons

Year	Obs.	Mean	(Std. Dev.)	Min.	Max.
2003	756	79.650	(336.332)	0	4,813.100
2004	762	76.830	(315.653)	0	4,483.260
2005	770	77.500	(321.292)	0	4,753.500
2006	808	71.932	(309.097)	0	4,753.500
2007	807	68.753	(308.542)	0	5,265.266
2008	799	63.990	(280.459)	0	5,108.270
Overall	4,702	72.984	(311.910)	0	5,265.266
Facility mean	857	68.477	(295.051)	0.0002	4,852.883

Source: Author's calculations and California Air Resources Board Emissions Inventory, 2003-2008.

Table 2. Means for Inspections, Enforcement Actions, and Penalty Amount per Facility-Year

	Mean	(Std. dev.)	Proportion positive
Inspections	1.932	(3.003)	0.736
Enforcement actions	0.755	(2.853)	0.236
Penalty	12,993.61	(190,945.97)	0.172
Penalty (log)	1.468	(3.309)	0.172
Given penalty > 0			
Penalty	75,563.84	(455,542.62)	
Penalty (log)	8.536	(1.820)	

Source: Author’s calculations and EPA Air Facility System, 2002-2008.

Table 3. Correlations Between Current- and Previous-Year Regulatory Actions at a Facility

	Inspections	Enforcement actions	Penalty (log)
A. Number of regulatory actions			
Current year actions:			
Inspections	1.000		
Enforcement actions	0.200	1.000	
Penalty (log)	0.171	0.517	1.000
Previous year actions:			
Inspections	0.714	0.132	0.140
Enforcement actions	0.245	0.516	0.311
Penalty (log)	0.146	0.212	0.230
B. Dummy variable for regulatory actions			
Current year actions:			
Inspections	1.000		
Enforcement actions	0.111	1.000	
Positive penalty	0.080	0.819	1.000
Previous year actions			
Inspections	0.208	0.058	0.051
Enforcement actions	0.076	0.380	0.380
Positive penalty	0.081	0.199	0.200

Source: Author’s calculations and EPA Air Facility System, 2002-2008.

Table 4. Description and Mean (per Facility-Year) of Variables

Variable	Description	Mean	(Std. dev.)
Dependent variable			
NO_x	NO_x emissions in tons. Source: CARB.	72.984	(311.910)
Explanatory variables			
Inspections	Number of full and partial compliance evaluations performed. FCEs address all pollutants and emission units and are performed once every two years for major facilities and once every five years for synthetic minor facilities. PCEs focus on a subset of pollutants or emission units, and are usually performed in response to ongoing violations and breakdown reports. Source: EPA.	1.932	(3.003)
Enforcement actions	Number of notices of violation, administrative orders, and consent decrees issued. NOVs are issued within a month of discovering the violation and administrative orders address a violation and usually take place within nine months of discovery. Source: EPA.	0.755	(2.853)
Penalty	Total penalty the facility received in the year, in thousands of dollars. Source: EPA.	12.994	(190.946)
PM_{10} nonattainment	Indicator for whether the county was in attainment of PM_{10} NAAQS (1 = nonattainment). Source: EPA.	0.674	(0.469)
$PM_{2.5}$ nonattainment	Indicator for whether the county was in attainment of $PM_{2.5}$ NAAQS (1 = nonattainment). Source: EPA.	0.454	(0.498)
Ozone nonattainment	Indicator for whether the county was in attainment of ozone NAAQS (1 = nonattainment). Source: EPA.	0.910	(0.286)
Carbon monoxide nonattainment	Indicator for whether the county was in attainment of carbon monoxide NAAQS (1 = nonattainment). Source: EPA.	0.313	(0.464)
Unemploy-ment rate	Percentage unemployment rate in the county. Source: BLS.	6.521	(2.105)
Income	Per capita income in the county, in thousands of dollars. Source: BEA.	37.191	(9.936)
Percent white	Percent white population in the county. Source: Census.	78.581	(7.660)

Source: Author's calculations, California Air Resources Board (CARB) emissions inventory, EPA Air Facility System, Bureau of Labor Statistics (BLS), Bureau of Economic Analysis (BEA), and U.S. Census.

Table 5. Fixed Effects Regressions of the Impact of the Presence of Regulatory Actions in the Previous Year on NO_x Emissions

Sample[a]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)
Presence of any inspections	-0.497	0.735	-0.843	0.827
in the previous year	(1.769)	(2.770)	(2.036)	(3.612)
Presence of any enforcement	2.289	2.451	3.030	5.208
actions in the previous year	(2.572)	(2.969)	(2.790)	(4.237)
Presence of a penalty in the	-5.596^{+}	-6.236^{+}	-6.684^{+}	-10.732*
previous year	(3.215)	(3.690)	(3.507)	(5.246)
PM_{10} nonattainment	-2.469	-2.896	-2.836	-6.267
	(3.504)	(4.347)	(3.778)	(7.367)
$PM_{2.5}$ nonattainment	0.220	0.342	0.169	1.459
	(1.057)	(1.512)	(1.160)	(2.418)
Ozone nonattainment	-6.080^{+}	-7.464	-5.950	-7.035
	(3.589)	(5.091)	(3.897)	(6.762)
Carbon monoxide	-3.152	2.263	-0.750	-8.443
nonattainment	(7.480)	(11.800)	(8.904)	(15.680)
Unemployment rate	-2.224	-3.046	-2.139	2.498
	(4.219)	(5.603)	(4.531)	(6.213)
Income (/$1000)	7.794^{+}	10.758*	8.525*	16.957*
	(4.043)	(5.444)	(4.276)	(6.709)
Percent white	-5.117	-7.980	-4.097	-7.025
	(4.137)	(5.450)	(4.630)	(7.315)
Observations	4,702	3,459	4,032	2,316
Facilities	857	613	672	423
Adjusted R-squared	0.961	0.960	0.962	0.966

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; year dummy variables included but not shown.

[a] Column titles ("all," "major," "balanced," and "manufacturing") refer to the sample used in the regression.

Table 6. Fixed Effects Regressions of the Impact of the Number of Regulatory Actions in the Previous Year on NO_x Emissions

Sample[a]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)
Number of inspections in	-1.115	-1.080	-0.869	0.042
the previous year	(0.758)	(0.892)	(1.007)	(1.109)
Number of enforcement	0.960	0.974	0.320	1.208
actions in the previous year	(1.083)	(1.097)	(1.235)	(1.535)
Amount of penalty (log) in	-0.677^{+}	-0.746^{+}	-0.579	-1.046^{+}
the previous year	(0.390)	(0.434)	(0.450)	(0.570)
PM_{10} nonattainment	-2.671	-3.325	-3.019	-6.003
	(3.410)	(4.279)	(3.685)	(7.048)
$PM_{2.5}$ nonattainment	-0.020	-0.018	0.011	1.462
	(1.083)	(1.575)	(1.198)	(2.535)
Ozone nonattainment	-5.603	-6.922	-5.678	-6.573
	(3.605)	(5.121)	(3.959)	(6.821)
Carbon monoxide	-3.319	1.820	-0.740	-8.178
nonattainment	(7.329)	(11.707)	(8.785)	(15.281)
Unemployment rate	-2.290	-2.936	-2.327	2.752
	(4.175)	(5.501)	(4.511)	(6.018)
Income (/$1000)	7.527^{+}	10.885^{+}	8.011^{+}	17.680**
	(4.183)	(5.594)	(4.463)	(6.673)
Percent white	-4.189	-6.839	-3.441	-7.083
	(3.991)	(5.196)	(4.495)	(6.886)
Observations	4,702	3,459	4,032	2,316
Facilities	857	613	672	423
Adjusted R-squared	0.961	0.961	0.962	0.966

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; year dummy variables included but not shown.

[a] Column titles ("all," "major," "balanced," and "manufacturing") refer to the sample used in the regression.

Table 7. Fixed Effects Regressions of the Impact of Regulatory Actions in the Previous Year on NO_x Emissions, Without Time Trends

Regulatory actions[a]	Presence of Regulatory Actions				Number of Regulatory Action			
Sample[b]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)	All (5)	Major (6)	Balanced (7)	Manufacturing (8)
Inspections in the previous year	-0.593	0.332	-1.131	0.258	-1.298*	-1.360*	-1.100	-0.372
	(1.622)	(2.521)	(1.857)	(3.270)	(0.705)	(0.795)	(0.911)	(0.987)
Enforcement actions in the previous year	2.576	2.845	3.288	5.970	0.979	1.005	0.340	1.263
	(2.629)	(3.044)	(2.849)	(4.459)	(1.086)	(1.100)	(1.235)	(1.534)
Penalty in the previous year (log)	-5.808*	-6.466*	-6.942*	-11.300**	-0.681*	-0.748*	-0.590	-1.059*
	(3.341)	(3.808)	(3.635)	(5.552)	(0.396)	(0.438)	(0.455)	(0.571)

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; NAAQS nonattainment and demographic variables included but not shown.

[a] Headings refer to the type of explanatory variable used in the regression.

[b] Column titles (“all,” “major,” “balanced,” and “manufacturing”) refer to the sample used in the regression.

Table 8. Pooled Ordinary Least Squares Regressions of the Impact of Regulatory Actions in the Previous Year on NO_x Emissions, Without Fixed Effects

Regulatory actions[a]	Presence of Regulatory Actions				Number of Regulatory Action			
Sample[b]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)	All (5)	Major (6)	Balanced (7)	Manufacturing (8)
Inspections in the previous year	29.534* (14.190)	20.601 (22.356)	29.890$^+$ (15.629)	50.502 (32.419)	4.920 (3.288)	2.657 (3.407)	8.383$^+$ (4.489)	6.389 (7.039)
Enforcement actions in the previous year	55.209* (24.893)	49.225 (30.227)	60.055* (27.583)	83.371$^+$ (42.734)	16.327** (6.237)	15.935* (6.303)	25.297** (7.669)	21.610** (8.111)
Penalty in the previous year (log)	25.583 (19.634)	28.591 (22.309)	25.997 (21.851)	60.643$^+$ (33.397)	3.973 (3.648)	4.036 (4.213)	1.539 (3.868)	9.157 (5.811)

** $p < 0.01$, * $p < 0.05$, $^+$ $p < 0.1$; cluster-robust standard errors in parentheses; NAAQS nonattainment, demographic variables, and year dummy variables included but not shown.

[a] Headings refer to the type of explanatory variable used in the regression.

[b] Column titles ("all," "major," "balanced," and "manufacturing") refer to the sample used in the regression.

Table 9. Ordinary Least Squares Regressions of the Impact of Regulatory Actions in the Previous Year on NO_x Emissions, Without Fixed Effects and Time Trends

Regulatory actions[a]	Presence of Regulatory Actions				Number of Regulatory Action			
Sample[b]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)	All (5)	Major (6)	Balanced (7)	Manufacturing (8)
Inspections in the previous year	28.683* (13.767)	19.331 (20.740)	28.977^{+} (15.078)	48.968 (31.538)	5.135 (3.377)	2.848 (3.474)	8.611^{+} (4.568)	6.772 (7.251)
Enforcement actions in the previous year	54.534* (24.696)	48.411 (29.507)	59.116* (27.310)	82.125^{+} (42.111)	16.169** (6.231)	15.738* (6.286)	25.125** (7.645)	21.301** (8.073)
Penalty in the previous year (log)	25.148 (19.527)	28.380 (22.179)	25.683 (21.763)	60.835^{+} (33.158)	3.875 (3.605)	3.944 (4.130)	1.431 (3.822)	9.108 (5.773)

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; NAAQS nonattainment and demographic variables included but not shown.

[a] Headings refer to the type of explanatory variable used in the regression.

[b] Column titles (“all,” “major,” “balanced,” and “manufacturing”) refer to the sample used in the regression.

Table 10. Fixed Effects Regression of the Impact of Regulatory Actions in the Previous Year on NO_x Emissions at All Facilities, Omitting Control Variables

Regulatory actions[a]	Presence of Regulatory Actions			Number of Regulatory Action		
	(1)	(2)	(3)	(4)	(5)	(6)
Inspections in the previous year	-0.450	-0.549	-0.474	-1.148	-1.135	-1.159
	(1.644)	(1.716)	(1.564)	(0.764)	(0.739)	(0.754)
Enforcement actions in the previous year	2.260	2.415	2.439	0.961	0.988	0.993
	(2.582)	(2.625)	(2.660)	(1.083)	(1.080)	(1.079)
Penalty in the previous year (log)	-5.696$^+$	-5.586$^+$	-5.661$^+$	-0.687$^+$	-0.676$^+$	-0.679$^+$
	(3.186)	(3.249)	(3.205)	(0.389)	(0.392)	(0.390)
NAAQS nonattainment (PM_{10}, $PM_{2.5}$, ozone, and CO)	✗	✓	✗	✗	✓	✗
Demographic variables (income, unemployment, and percent white)	✓	✗	✗	✓	✗	✗

** $p < 0.01$, * $p < 0.05$, $^+$ $p < 0.1$; cluster-robust standard errors in parentheses; NAAQS nonattainment and demographic variables are included in some specifications but not shown; year dummy variables included but not shown.

[a] Headings refer to the type of explanatory variable used in the regression.

Figures

Day 0		Day 0 to 30		Day 0 to 300
Discovery of violation	→	Notice of violation	→	Administrative order and penalty

Figure 1. Timeline of a Typical Violation

CHAPTER II

EXAMINING THE IMPACT OF MONITORING AND ENFORCEMENT ON AIR QUALITY

Introduction

The Clean Air Act (CAA) is a comprehensive air pollution control scheme, which includes nation-wide air quality standards, emissions limitations for polluting facilities, and monitoring and enforcement of permit limits, with the central purpose of promoting public health and welfare by enhancing air quality. Thus, any assessment of the monitoring and enforcement regime must examine its impact on air quality. In this chapter, I use a dataset of air quality measures in California to examine the impact of monitoring and enforcement on ambient ozone concentrations. My study focuses on California because its aggressive pollution control policies allow me to test the effectiveness of air pollution regulation. Additionally, focusing on one jurisdiction allows me to better understand its regulatory regime. I find that penalties improve air quality. Inspections and enforcement actions also improve air quality, but the effect is less robust.

Under the CAA, the U.S. Environmental Protection Agency (EPA) sets national ambient air quality standards (NAAQS) for “criteria pollutants,” which are six commonly found air pollutants that affect human health and the environment. State and local regulators are responsible for attaining the NAAQS by monitoring and enforcing stationary source emissions permits. As air quality is the main focus of federal regulation, the benefits of monitoring and enforcement should be measured in terms of improvements to air quality. Unfortunately, previous research on the impact of monitoring and enforcement has not examined that question. Instead, it has focused on the impact of monitoring and enforcement actions on compliance and emissions. Other research that examined air quality used nonattainment of NAAQS as a proxy for increased regulatory stringency and investigated the impact of nonattainment on air quality, instead of directly studying the impact of monitoring and enforcement actions on air quality.

Many studies have explored the relationship between regulatory actions and a facility's compliance and pollution releases for both air and water pollution. These studies usually use the lagged and predicted regulatory action variables to avoid reverse causality problems. As regulators are likely to perform more inspections and enforcement actions at facilities that release more pollution or commit more violations, not correcting for reverse causality can lead to the erroneous conclusion that regulatory actions cause noncompliance or higher emissions. Using lagged and predicted regulatory actions can solve this problem.

Water pollution studies have investigated the relationship between regulatory action and effluent and compliance. For example, Magat and Viscusi (1990) measured the effect of inspections of pulp and paper plants on the plant's compliance with the Clean Water Act and found that inspections in the previous quarter increased compliance and decreased effluent. Laplante and Rilstone (1996) found that the predicted probability of inspection produced larger reductions in effluent than actual inspections at pulp and paper plants in Quebec but Earnhart (2004a, b) found that actual inspections and enforcement actions reduced effluent but predicted inspections and enforcement actions had no significant impact at municipal wastewater plants in Kansas.

Articles that investigated air pollution have studied the impact of monitoring and enforcement on whether a facility complies with its permit limits, the duration a facility remains noncompliant, and emissions. For example, Gray and Deily (1996) found that predicted regulatory actions had no impact on CAA compliance at steel-making plants, but the actual number of inspections and enforcement actions improved compliance. In a later paper (Deily and Gray 2007), their investigation of the joint effect of environmental regulations and health and safety regulations yielded similar results.

Nadeau (1997) studied how the number of inspections and enforcement actions affected the duration a facility remained out of compliance with its permit. He found that predicted monitoring and enforcement actions, when predictions were based on the noncompliant sample, significantly reduced the duration a facility remained noncompliant. Hanna and Oliva (2010) found that actual CAA inspections decreased emissions of toxic chemicals at manufacturing plants, but the probability of inspection had no significant effect.

Even if the literature shows that monitoring and enforcement improve compliance and decrease emissions, low compliance and high emissions levels alone do not cause harm; rather, they cause harm through high ambient concentrations of pollution. Ambient air quality is an important measure of the success of regulation that has not been adequately studied. Although it seems natural that increased compliance and reduced emissions would result in better air quality, that might not be true due to other factors such as weather conditions or characteristics of the air basin. For example, if the weather conditions are not conducive to ozone formation, a decrease in emissions of ozone precursors might not improve air quality; alternatively, if the air basin's geography permits winds to transport pollutants away, then increased enforcement might lead to decreased emissions, but might not improve air quality.

Literature that examined air quality does not examine the impact of regulatory actions on air quality. Instead, authors used NAAQS nonattainment as a proxy for more stringent regulatory requirements as facilities located in areas that are in nonattainment of the NAAQS face tougher regulatory requirements, such as lower emissions limits, that aim to bring the area into attainment of the NAAQS. Most of the literature found that nonattainment—and thus more stringent regulatory standards—improved air quality.

There is ample evidence that the NAAQS improves air quality; this evidence comes primarily from studies comparing attainment and nonattainment areas. For example, Kahn (1997) examined trends in particulate matter between 1969 and 1992 and found that pollution per unit of manufacturing increased more in the less-regulated attainment counties. Examining the 1987 change in the particulate matter NAAQS from regulating total suspended particles (particles with a diameter of 40 micrometers or less) to regulating PM_{10} (particles with a diameter of 10 micrometers or less), he found that counties that transitioned from nonattainment of the total suspended particles standard to attainment of the PM_{10} standard produced double the amount of pollution per unit of manufacturing than before, implying that easing of regulations increased pollution. Henderson (1996) focused on ozone levels during the worst ozone month, July, and annual ozone levels, and found that ozone nonattainment significantly reduced July ozone levels, but not annual levels. Even though he found no effects on the annual ozone levels, the NAAQS proved effective at improving air quality at the time of year when ozone levels are usually the worst.

On the other hand, Greenstone (2004) analyzed the effect of attainment status on county sulfur dioxide levels between 1969 and 1997 but did not find consistently significant results. He suggested that this might be due to problems with the data: his data had many counties that had ambient levels below the national standard but were still classified as nonattainment areas. States are responsible for petitioning the EPA to get a county redesignated from nonattainment to attainment, a process which requires states to develop expensive models. Greenstone argued that, instead of going through this costly process, states might have informally requested, and the EPA agreed to, reduced regulatory oversight in such counties. Thus, in his view, nonattainment was not an accurate proxy for regulatory stringency and the finding of no significant effect was not surprising.

Auffhammer, Bento, and Lowe (2009) suggested that Greenstone's (2004) lack of significant results could be due to "averaging out": the nonattainment dummy captures the average effect of regulation on all monitors in nonattainment counties. If regulators focused on the most polluted parts of a nonattainment county, this averaging out effect might wrongly suggest that regulations are not effective. They examined PM_{10} levels and found that being in a nonattainment county did not have an effect on the change in ambient PM_{10} levels—similar to Greenstone's findings. However, monitors that had readings exceeding the national standard in the previous year showed marked declines in PM_{10} levels regardless of whether they were in attainment or nonattainment counties. This suggested that regulators focused on such "noncompliant" monitors.

Thus, the literature has established that monitoring and enforcement improve compliance and decrease emissions, and being in nonattainment of the NAAQS can cause improvements in air quality. However, researchers have not studied the direct link between regulatory actions and air quality. This chapter will contribute to the literature by exploring how monitoring and enforcement affect air quality.

In the next section, I describe the regulatory framework of the CAA in California. The following sections describe the theoretical model, the data, and regression analysis. I provide concluding remarks in the final section.

Regulatory Background

Under § 109 of the CAA, the EPA establishes the NAAQS for six commonly found pollutants that harm health and the environment. These pollutants, also called criteria pollutants, are ozone, particulate matter, carbon monoxide, nitrogen oxides, sulfur dioxide, and lead. Although the NAAQS are federal standards, states have primary responsibility for achieving or maintaining these standards. For instance, states are responsible for issuing stationary source permits and monitoring each source's compliance with the permits. Of the criteria pollutants, ozone and $PM_{2.5}$ (particulates with a diameter of 2.5 micrometers or less) cause the most significant human health effects (EPA 2011). Ozone is not emitted directly but is formed through reactions between nitrogen oxides and volatile organic compounds in the presence of sunlight. Ground-level ozone can cause respiratory problems to humans and damage vegetation. Under the ozone NAAQS, the annual fourth-highest daily maximum eight-hour concentration, averaged over three years, should not exceed 0.075 parts per million (ppm).[21] This standard was lowered from 0.08 ppm in 2008.[22]

In California, stationary source monitoring and enforcement are handled primarily by thirty-five local air districts.[23] Monitoring and enforcement practices depend on the type of source: major sources are sources that emit (or have the potential to emit) more than 100 tons per year of any pollutant and synthetic minor sources are sources that emit (or have the potential to emit) above 80% of the major source threshold (EPA 2001). In my analysis, I focus on inspections, enforcement actions, and penalties. There are two types of inspections, full compliance evaluations (FCEs) and partial compliance evaluations (PCEs), and three types of enforcement actions, notices of violation (NOVs), administrative orders, and consent decrees. Administrative orders and consent decrees are usually accompanied by penalties.

An FCE is a comprehensive evaluation of the facility that addresses all regulated pollutants and emission units, and a PCE is an inspection that focuses on a subset of pollutants, requirements, or emission units (EPA 2001). Air districts typically perform an FCE once every two years for major sources and once every five years for synthetic minor sources. Districts typically perform PCEs when there are complaints, ongoing violations, or reports of equipment breakdown. All districts report FCEs to the EPA's data system, but most districts do not report PCEs.

[21] Each day there are sixteen eight-hour averages and, naturally, one maximum of those sixteen eight-hour averages. Over the year, there are 365 of these maxima and one fourth-highest maximum. This fourth-highest value is averaged over three years and compared to the air quality standard to determine the attainment status of the area.

[22] Although 0.08 parts per million translates to 80 parts per billion, because of rounding, the limit was effectively 84 parts per billion.

[23] Much of the regulatory background is covered in chapter 1; I provide a brief description here.

If regulators find a violation, they pursue enforcement action. There are three types of enforcement actions: NOVs, administrative orders, and consent decrees. The enforcement process begins with the discovery of a violation; inspectors can discover a violation through self-reporting, record review, or inspections. Upon discovering a violation, inspectors typically issue an NOV within a few weeks. District regulators place an emphasis on correcting violations; once detected, violations are usually remedied quickly, sometimes the same day.

Districts typically handle violations administratively; after issuing an NOV, the district assesses a penalty and issues an administrative order. Additionally, an administrative order might have other components. For instance, it could contain a shutdown order, which orders the facility to shut down the piece of violating equipment. A shutdown order could cost the facility more than the penalty assessed, and this aspect of the enforcement process is important. However, I do not have any of these details about the administrative orders.

The time between an NOV and an administrative order varies, but it typically takes less than nine months. It is worth noting that in California, unlike other jurisdictions, almost all NOVs end up as administrative orders and thus entail penalties.[24] However, because an administrative order can address multiple violations or multiple NOVs, there might not be a one-to-one relationship between NOVs and administrative orders.

Some cases might go through the judicial process instead of the administrative process, which can take three to five years and usually ends in another type of enforcement action, a consent decree. However, this is rare and an overwhelming

[24] This is different from other jurisdictions where only some of the NOVs expose the facility to penalties.

majority of enforcement actions are administrative.[25] Districts also vary in their reporting practices; all districts report violations that are considered high-priority violations,[26] but some districts report some or all violations that are not considered high-priority violations.[27] Penalties are part of the enforcement process but are not considered an individual enforcement action. Instead, penalties usually accompany administrative orders and consent decrees.

In my analysis, I collapse both FCEs and PCEs into one inspections variable as this is more consistent with the literature and PCEs are not consistently reported by every air district. I also collapse NOVs, administrative orders, and consent decrees into one enforcement actions variable as administrative orders invariable follow NOVs and this is more consistent with the literature.

Theoretical Model

I extend the model from my previous chapter. In the previous chapter, I assume that each facility is run by a profit-maximizing, price-taking firm. Emissions of firm i are determined by the equation

$$e^i = e(p^i, d^i, f^i), \tag{11}$$

[25] Only 1.1% of enforcement actions in my data—EPA data for CAA-regulated facilities—are consent decrees.
[26] High-priority violations are violations that the EPA believes should receive the "highest scrutiny and oversight" (EPA 1998, p. 3). These include more serious permit, emissions, and testing violations, and chronic violations. All districts report high-priority violations; the San Joaquin Valley Air Pollution Control District reports some of its violations that are not considered high-priority violations and the South Coast Air Quality Management District reports all of its violations regardless of whether they are considered high-priority violations.
[27] Regulatory authorities also have other types of enforcement actions at their disposal, such as notices to comply. However, as these are not reported to the EPA database, I do not discuss them.

where e^i is the emissions for facility i, p^i is the price of the facility's output, d^i is the probability of detecting a violation, and f^i is the fine if the violation is discovered. I previously established that emissions are increasing in price and decreasing in probability of detection and the size of a fine ($e_p > 0$, $e_d < 0$, and $e_f < 0$).

Next, assume that there are n facilities around an air quality monitor and ambient concentration at the monitor is a function of the emissions of surrounding sources and background air quality,

$$y = c(e^1, e^2, \dots, e^n) + B = c(E) + B, \quad (12)$$

where y is the ambient concentration at the monitor, E is a of vector emissions from surrounding sources, and c is increasing in emissions ($c_{e^i} > 0, \forall i$). I assume the relationship between ambient concentration and emissions is linear, so ambient concentration is determined by

$$y = \sum_{i=1}^{n} c^i e^i + B, \quad (13)$$

where c^i is the transfer coefficient and $c^i > 0$ such that an increase in emissions at source i by Δe^i will increase ambient concentrations by $c^i \Delta e^i$.

For simplicity, I assume that all the facilities are similar and have the same output price, probability of detection, fine, and transfer coefficient. Thus, the ambient concentration, y, is determined by

$$y = n \times c \times e(p, d, f), \quad (14)$$

where e is the emissions of the representative firm, p is the price of the output, d is the probability of detection, f is the fine, and c is the transfer coefficient. Thus, as emissions increase, ambient concentration increases ($y_e > 0$).

The partial derivative of y with respect to d and f are:

$$y_d = n \times c \times y_e \times e_d \text{ and} \quad (15)$$

$$y_f = n \times c \times y_e \times e_f. \quad (16)$$

As previously discussed, $n > 0$, $c > 0$, $y_e > 0$, $e_d < 0$, and $e_f < 0$; therefore, $y_d < 0$ and $y_f < 0$. This means that as the probability of detection and the size of fines increase, ambient concentration decreases. Equation (14) also leads to the intuitive result that ambient concentration increases with the number of nearby facilities ($y_n > 0$) and increases with output price ($y_p > 0$).

Inspections increase the probability of detection, d, and thus should decrease ambient concentration. Higher penalties increase f and thus decrease ambient concentration. Enforcement actions can affect both probability of detection d and fines f. An increase in the number of enforcement actions might reflect an increase in detection of violations, d; at the same time, repeat offenders might also face larger fines, thus an enforcement action might reflect an increase in f. Therefore, enforcement actions should decrease ambient concentration.

Data Description

I obtain air quality data between 2003 and 2010 from the EPA's Air Quality System database. The EPA tracks hourly ozone concentrations at air quality monitors in California. The database has information about the location of the air quality monitor as well as information on when the monitor was established and terminated. I focus on ozone because ozone has been identified as one of the criteria pollutants that cause the most significant human health effects (EPA 2011) and I have more observations for ozone than for other pollutants.

As the NAAQS measures the fourth-highest daily maximum eight-hour average against the federal standard, the effects of regulatory actions should show up in that measure. Using the EPA air quality data, I calculate the fourth-highest daily maximum eight-hour average ozone concentration at each air quality monitor for each year. I exclude all readings for which the state requested an exclusion due to special circumstances, such as wildfires. I also restrict the sample to air quality monitors that were operating through the entire time period. Whether this restriction biases my coefficients depends on the reasons that air quality monitors might be added or removed. If air quality monitors are added or removed for reasons unrelated to regulatory actions and air quality, then restricting the sample will not bias my coefficients.

However, regulators are likely to add monitors in areas with poor air quality, and remove monitors in areas that have good air quality. Ambient ozone concentrations in areas with good air quality are likely to be less responsive to regulatory action; thus, this could cause my regressions to overestimate the effectiveness of regulatory actions.[28] Nonetheless, my regression coefficients would still be valid for areas with poor air quality.

Table 11 presents summary statistics of ozone concentrations in parts per billion (ppb). The mean ozone level is 78.6 ppb and the standard deviation is 16.9. There is a general downward trend, with ozone concentrations decreasing from 83.8 ppb in 2003 to 74.6 ppb in 2010. Figure 2 shows the decreasing trend of mean ozone concentrations over

[28] Future work can account for this problem by including air quality monitors that stop operating or begin operation during the study period.

time. Figure 3 shows the distribution of ozone concentrations, along with the 2008 ozone standard and 1997 ozone standard; there are no extremely large outliers and the peak of the distribution is slightly less than the 2008 standard of 75 ppb.

The Air Facility System is the EPA's database for CAA-regulated sources. The database contains details of each facility, such as its geographic coordinates, address, program identification number, and permit type. Additionally, it has details of regulatory actions since 2002: the date and type of regulatory action as well as the associated penalty. I limit my period of study to 2002-2010 because compliance monitoring policy and data reporting practices changed significantly in 2001 (EPA 2001). I classify all state and federal FCEs and PCEs as inspections, and all state and federal NOVs, administrative orders, and consent decrees as enforcement actions. To measure the intensity of regulatory activity around each monitor, I use regulatory activity at facilities within 20 miles of the monitor.[29] I drop all monitors that have no facilities located within twenty miles. As a robustness check, I run the regressions using different radii; these results are shown in Appendix B.

Facilities that have shut down are removed from the EPA database, so it only has information on facilities that were still operating at the time the database was accessed. Thus, facilities that were in operation at the start of my study period, 2002, but shut down before the end of the period, 2010, are not in my data. Such an exclusion is likely to overstate the significance of my regression coefficients. Facilities shut down and reduce emissions, resulting in an improvement in air quality. Such an improvement is attributed

[29] This radius is somewhat arbitrary as factors affecting ambient ozone concentration at an air quality monitor can be quite specific to the monitor, such as surrounding geography and upwind emissions. While it might be more logical to examine how regulatory actions within an air basin affects the air quality in the air basin, this method would reduce the number of observations as there are only 15 air basins in California.

to fewer regulatory actions (those on existing facilities) than the true value (those on existing and shut-down facilities), thus overstating my regression coefficients.[30] Facilities that began operations during my study period are in the data.

Table 12 presents a summary of the inspections, enforcement actions, and penalties at facilities within 20 miles of each monitor. There is an average of 55.2 facilities within 20 miles of the air quality monitor. I computed the total number of regulatory actions and the average number of regulatory actions within 20 miles of a monitor. There is an average of 72.8 inspections at all facilities around a monitor and an average of 1.7 inspections per facility.[31] There is an average of 28.2 enforcement actions at facilities surrounding monitors and an average of 0.5 enforcement actions per facility. Lastly, there is an average penalty of $323,925 dollars and facilities faced an average of $4,682 in penalties. As most penalties are small but there are several very large penalties, in my regressions, I use the natural logarithm of penalties as an explanatory variable. Based on the theoretical model, I expect increased inspections, enforcement actions, and penalties to decrease ambient concentrations of ozone and an increase in the number of facilities to increase ambient concentrations of ozone.

I control for NAAQS attainment status for other pollutants (carbon monoxide, PM_{10}, and $PM_{2.5}$) and demographic factors (per capita income, unemployment, and percent white) at the county-year level. Being in a nonattainment county might cause a facility to face pressure from the community to improve air quality, which is not reflected

[30] Future research can include those facilities, based on information obtained through a Freedom of Information Act request.

[31] These numbers are somewhat different from those in chapter 1. For instance, the average number of inspections in chapter 1 is 1.9 but the analogous number in chapter 2 is 1.7. These are measuring different values. In chapter 1, the 1.9 average is the average number of inspections at the facilities that I could match to the California emissions data. In chapter 2, I first average the number of inspections at facilities around each air quality monitor; then I take the average of that number of all the monitors to get 1.7.

in regulatory activity. Thus, I expect nonattainment of the NAAQS to result in lower ozone concentrations. I get county NAAQS attainment status from the EPA's Green Book and focus on attainment status for carbon monoxide, PM_{10}, and $PM_{2.5}$, and omit lead, nitrogen oxides, and sulfur dioxide because all California counties are in attainment of those standards.

I get county per capita income information from the Bureau of Economic Analysis and unemployment rate from the Bureau of Labor Statistics and include them in my regressions to control for the strength of the economy, the price of output and cost of input. I also obtain information about the percentage of white people in the county from the Census; counties with a high percentage of minority population might wield less political power and have less regulatory pressure to improve air quality, and omitting this might cause omitted variables bias.

Additionally, I control for weather conditions as they affect ozone formation. As the worst ozone concentrations tend to occur during July, I control for July temperature, precipitation, and wind speed. I use the National Climatic Data Center's Global Summary of the Day and match each air quality monitor to the nearest weather station. If there are no weather stations within 20 miles, I use a dummy variable to indicate missing weather data. Lastly, I use monitor fixed effects to control for time-invariant monitor characteristics and year dummy variables to control for time trends in air quality.[32]

[32] Mobile sources are a major source of ozone precursors, and future iterations of this chapter can account for traffic patterns around the air quality monitor to control for mobile source emissions.

Regression Analysis

Regulators are more likely to target monitoring and enforcement actions at facilities around monitors that register poor air quality, so studying the impact of current-year regulatory actions on air quality can cause the erroneous conclusion that regulatory action causes poor air quality. I use previous-year regulatory actions as the explanatory variable as current-year air quality cannot cause previous-year regulatory actions, thus avoiding reverse causality.

Additionally, I use the fixed effects regression model, which examines the changes in air quality at the same air quality monitor over time. This method regresses the mean-differenced dependent variable on mean-differenced explanatory variables. It factors out any time-invariant characteristics of the air quality monitors such as location and surrounding geography, and can reduce problems presented by differences in the monitors. Furthermore, it is quite likely that regulators in areas that have persistently poor air quality are more stringent. Fixed effects will account for this, as long as the level of stringency does not change over time.

Air quality is a function of regulatory action and other control variables. I assemble the data into a panel of monitor-years, and used monitor fixed effects to account for time-invariant individual characteristics. The regression model is represented by the equation

$$y_{it} = RegulatoryAction'_{i,t-1}\beta + X'_{it}\gamma + a_i + Year_t + \varepsilon_{it}, \tag{17}$$

where y_{it} is the ambient pollution concentration at monitor i in year t: the fourth-highest daily maximum eight-hour average for ozone (in ppb). Time-invariant monitor characteristics, such as the location of the monitor, are captured by a_i. Year dummy variables, $Year_t$, capture general time trends in ambient ozone concentrations.

The variable $RegulatoryAction_{i,t-1}$ represents regulatory action taken at nearby facilities in the previous year, such as the number of inspections and enforcement actions taken and penalties assessed. I use the total number of regulatory actions, such as the number of inspections or number of enforcement actions, taken in any given year as it provides a more complete picture: if facilities around a monitor are subject to more inspections, then it should show a larger effect on air quality than an air quality monitor that has fewer inspections. As regulatory action should improve air quality and decrease ambient concentrations of ozone, I expect the coefficients of regulatory action to be negative.

However, the number of regulatory actions might be a reflection of the number of facilities surrounding a monitor, not necessarily the intensity of regulatory action.[33] Thus, I also examine the average number of inspections, enforcement actions, or penalties at the surrounding facilities. As the impact of a regulatory action may depend on the size of the facility or its proximity to the monitor, it might make sense to weight regulatory actions based on those factors. However, I do not have any algorithm to weight these factors. Instead, I run robustness tests by considering all regulatory actions within 10, 15, and 30 miles. These robustness tests are presented in Appendix B.

[33] This is mitigated to some extent by the fixed effects model which uses mean-differencing. For each variable, the regression model deducts each monitor's mean, across time. This forms the model: $y_{it} - \bar{y}_i = \left(RegulatoryAction_{i,t-1} - \overline{RegulatoryAction_i}\right)'\beta + (X_{it} - \bar{X}_i)'\gamma + Year_t + (\varepsilon_{it} - \bar{\varepsilon}_i)$, where $\bar{y}_i = \frac{1}{T}\sum y_{it}$. Thus, the model only uses differences from the mean of the number of regulatory actions.

Other time-varying characteristics are captured by X_{it}. This includes the number of facilities around a monitor, controls for county demographic characteristics (income, unemployment rate, and percent white), county attainment status (particulate matter and carbon monoxide attainment status), and weather characteristics (July mean temperature, total rainfall, and mean wind speed). I expect that an increase in the number of facilities around a monitor will increase ozone concentrations. Income and unemployment reflect the strength of the economy: an increase in economic activity is likely to result in increased emissions and thus increases in ozone concentrations. However, an increase in income also results in an increase in the value of clean air, which might decrease ozone levels. As minorities might wield less political power, a higher proportion of white population may result in lower ozone concentrations. Lastly, as sunlight is needed for ozone formation, I expect mean temperature to be positively related with ozone concentrations. Precipitation and wind remove pollutants from the air, so I expect total precipitation and wind speed to be negatively related with ozone concentration.

Regression (1) of Table 13 displays the effect of the total previous-year inspections, enforcement actions, and penalties on ozone concentrations. The number of enforcement actions has no significant impact on the ozone concentration, and, as expected, an increase in the number of facilities increases ozone concentrations. An additional inspection decreases ozone concentration by 0.008 ppb. This effect is significant at the 10% level. Although the coefficient seems small, the relatively large number of inspections can still generate a large impact. For instance, the average number of inspections is 72.8, which reduces ozone concentrations by 0.583 ppb, compared to no inspections; increasing the number of inspections from the 25th percentile (13 inspections) to the 75th percentile (80 inspections) of the distribution of inspections reduces ozone concentrations by 0.536 ppb.

Additionally, penalties have a negative and significant impact on ozone. As the dependent variable is the level of ozone concentration and the explanatory variable is the natural logarithm of the penalty, the coefficient can be interpreted as the effect of a percentage increase in penalty on the level of ozone concentration: increasing the penalty by 1% would reduce ozone concentrations by approximately 0.002 ppb. The average penalty is $323,925, which reduces ozone concentrations by 2.017 ppb compared to a penalty of $1; increasing the penalty from the 25th percentile ($14,000) to the 75th percentile ($125,000) of the penalty distribution reduces ambient ozone concentration by 0.348 ppb.

Monitors in $PM_{2.5}$ nonattainment counties have ozone concentrations 3.499 ppb lower than attainment counties. This is likely because particulate matter nonattainment is a serious problem in California, and there are other unobserved factors that put pressure on these nonattainment counties to improve air quality. Additionally, increased unemployment reduces ambient concentrations of ozone. This is not surprising as unemployment is a proxy for economic activity, and poor economic performance should decrease ambient pollution. On the other hand, increased income reduces ambient ozone concentrations. This is somewhat surprising as income is a proxy for economic activity, and increased economic activity should worsen air quality. Perhaps higher income indicates better paying, less polluting jobs or an increase in the value of clean air, resulting in better air quality. Lastly, as expected, mean temperature increases ozone concentrations, but total precipitation and mean wind speed have no impact on ozone concentration.

Regression (2) of Table 13 displays the impact of the average number of inspections and enforcement actions and the natural logarithm of the average penalty at the facilities around the monitor. The penalty variable remains significant; increasing the average penalty by 1% reduces the ambient ozone concentration by 0.002 ppb.[34]

Additionally, as a test for robustness, I run the same regressions using the natural logarithm of ozone as the dependent variable.[35] The literature uses both levels and natural logarithms: for instance, Henderson's (1996) study used the natural logarithm of ozone concentrations, while Auffhammer, Bento, and Lowe's (2009) study used the level of PM_{10} concentrations. The results are presented in Table 14. In regression (1), the coefficient for inspections is negative and significant: increasing the number of inspections by one decreases ozone concentrations by 0.011%. The coefficient of penalties is also negative and significant. As both the dependent and explanatory variables are logarithms, the coefficient can be interpreted as the elasticity of ozone concentrations with respect to penalties: increasing the penalty by 1% decreases the ozone concentration by 0.002%. The interpretation for the average regulatory actions in regression (2) follows similarly. Notably, an increase in the average penalty by 1% decreases ozone concentrations by 0.002%. Although these coefficients seem small,

[34] Admittedly, the mean penalty of $4,682 is probably small compared to the facility's operating costs. It is possible that the facilities are not deterred by the average penalty, but are concerned about the potential for far larger penalties, and the average penalty merely reflects this possibility. In Appendix B, I examine whether the maximum penalty improves air quality.

[35] As shown in Figure 3, my data do not have large outliers. Thus, using natural logarithms might not be necessary.

given the large number of inspections and the large total penalty, the regulatory actions still make a relatively large impact on ozone concentrations.

Next, as another robustness test, I perform the first differences regression, which regresses the change in ozone concentrations from the previous year on the change in the explanatory variables from the previous year. When using the first-differences model, I lose one year of data (168 observations). The regression equation is:

$$\Delta y_{it} = \Delta RegulatoryAction'_{i,t-1}\beta + \Delta X'_{it}\gamma + Year_t + \Delta\varepsilon_{it}, \quad (18)$$

where $\Delta x_{it} = x_{it} - x_{i,t-1}$. The results are shown in Table 15. Across the two regressions, the coefficients of enforcement actions are negative and statistically significant. In regression (1), increasing the number of enforcement actions by one is associated with a decrease in ozone concentrations by 0.016 ppb. In regression (2), increasing the average number of enforcement actions by one is associated with a decrease in ozone concentrations by 0.849 ppb. The coefficients of penalties are no longer significant.

Oddly, an increase in the number of facilities is associated with a decrease in the ambient concentrations of ozone. Perhaps this is due to natural progression over time as the number of facilities increases over time and ozone concentrations also decrease over time. Other than these differences, the first differences regressions are similar to the fixed effects regressions.

Robustness Tests

In this section, I examine whether the results in the fixed effects regressions are sensitive to varying specifications. In Table 16 and Table 17, I show that the results are robust to omitting various control variables. Table 16 shows the coefficients of the impact of the total number of regulatory actions in regressions that omit various control variables. Regression (1) shows the coefficients of the regulatory variables when I run the fixed effects regression in equation (17) while omitting the total number of facilities surrounding the air quality monitor. The coefficient of penalties remains statistically significant: increasing the penalty by 1% reduces ozone concentrations by approximately 0.002 ppb. Regressions (2) to (5) show that the size and significance of the coefficient of penalties are robust to omitting various control variables. The coefficient of inspections is not as robust; it is significant in only three of the five specifications. Similarly, Table 17 shows the impact of the average number of enforcement actions and the size of the penalty. The size and significance of the penalty coefficient is robust across the specifications.

Next, I examine whether omitting year dummy variables changes the results. Regressions (1) and (2) of Table 18 show that the effect of the penalty is robust to omitting time trends: the coefficient of penalties is negative and statistically significant.

Additionally, I examine how using pooled ordinary least squares, instead of fixed effects, affects the results. I run this regression

$$y_{it} = RegulatoryAction'_{i,t-1}\beta + X'_{it}\gamma + \alpha + Year_t + \varepsilon_{it}, \tag{19}$$

where, instead of having an intercept for each air quality monitor, α_i, there is a common intercept term, α. As shown in regressions (3) to (6) of Table 18, the coefficient of the penalty variable is positive, quite large, and significant. Although the usual interpretation of such a coefficient is that penalties cause an increase in ozone concentrations, it is far more likely that regulators impose higher penalties around monitors that report persistent air quality problems. Using fixed effects accounts for such causality problems.

Lastly, I examine the robustness of the first differences regressions by comparing them to the differences-in-differences regressions. The differences-in-differences regression specification can account for general trends in ozone concentrations, regulatory actions, and other explanatory variables. The regression equation is

$$\Delta^2 y_{it} = \Delta^2 RegulatoryAction'_{i,t-1}\beta + \Delta^2 X'_{it}\gamma + Year_t + \Delta^2 \varepsilon_{it}, \quad (20)$$

where $\Delta^2_{it} = \Delta_{it} - \Delta_{i,t-1}$. Table 19 shows the regression results. Compared to the first differences results in Table 15, the size and significance of the coefficient of the enforcement variable is similar; enforcement actions still decrease ambient ozone concentrations. However, in regression (1), an increase in the number of inspections seems to be associated with an increase in ozone concentrations.

In Appendix B, I show that using different radii—for example, counting the total number of regulatory actions within 30 miles of the air quality monitor, instead of 20 miles—does not affect the results, and I also investigate possible instrumental variables.

Conclusion

This chapter is the first article that examines the impact of inspections and enforcement actions on ambient air quality. Previous research has found that nonattainment of federal air quality standards and the more stringent regulatory regime that follows nonattainment improve air quality. However, the literature has not examined the impact of monitoring and enforcement actions on air quality. In this chapter, I find that penalties significantly improve air quality: increasing the total penalty at facilities around an air quality monitor by 1% improves ozone concentrations by 0.002 ppb. At the average penalty of $323,925, this translates into a 2.017 ppb improvement. This result is fairly robust. Additionally, there is some evidence that enforcement actions improve air quality. This complements the results in my previous chapter, which found that penalties decrease emissions of nitrogen oxides.

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Table 11. Mean Ozone Ambient Concentration in ppb

Year	Mean	Std. dev.	Min.	Max.
2003	83.770	(19.689)	41.500	137.625
2004	78.721	(15.209)	41.125	122.750
2005	76.736	(19.085)	34.250	130.125
2006	81.318	(18.184)	43.500	125.500
2007	76.362	(15.941)	45.857	126.250
2008	79.517	(15.825)	38.663	120.875
2009	77.014	(14.285)	38.000	108.500
2010	74.593	(14.191)	25.000	109.761
Overall	78.575	(16.913)	25.000	137.625

Source: Author’s calculations and EPA Air Quality System.

Table 12. Descriptions and Means of Explanatory Variables

Variable	Description	Mean	(Std. dev.)
Number of facilities	Number of facilities within twenty miles of the air quality monitor. Source: EPA.	55.221	(77.351)
Inspections	Previous-year full and partial compliance evaluations performed at surrounding facilities. Source: EPA.		
Total	Total number of inspections performed.	72.826	(105.658)
Average	Average number of inspections performed.	1.666	(1.319)
Enforcement actions	Previous-year notices of violation, administrative orders, and consent decrees issued at surrounding facilities. Source: EPA.		
Total	Total number enforcement actions issued.	28.189	(50.996)
Average	Average number of enforcement actions issued.	0.463	(0.714)
Penalty	Previous-year penalties assessed at surrounding facilities, in thousands of dollars. Source: EPA.		
Total	Total penalty at the surrounding facilities.	323.925	(1,104.261)
Average	Average penalty at surrounding facilities.	4.682	(16.892)
NAAQS non-attainment	Indicator for whether the county, in which the monitor is located, was out of attainment of NAAQS (1 = nonattainment). Source: EPA.		
CO non-attainment	Indicator for whether the county was out of attainment of the carbon monoxide standard.	0.116	(0.320)
PM_{10} non-attainment	Indicator for whether the county was out of attainment of the PM_{10} standard.	0.415	(0.493)
$PM_{2.5}$ non-attainment	Indicator for whether the county was out of attainment of $PM_{2.5}$ standard.	0.277	(0.448)
Unemployment rate	Unemployment rate in the county, in percent. Source: BLS.	8.136	(3.957)
Income	Per capita income in the county, in thousands of dollars. Source: BEA.	37.489	(9.943)
Percent white	Percent white population in the county. Source: Census.	81.775	(9.130)
July weather	July weather at the nearest weather station within twenty miles. Source: NCDC.		
Temperature	Mean temperature in July, in degrees Fahrenheit.	74.831	(9.914)
Precipitation	Total precipitation in July, in inches.	0.027	(0.154)
Wind speed	Mean wind speed in July, in knots.	5.850	(2.091)

Source: Author's calculations, EPA, Bureau of Labor Statistics (BLS), Bureau of Economic Analysis (BEA), U.S. Census, and National Climatic Data Center (NCDC).

Table 13. Fixed Effects Regressions of the Impact of Regulatory Actions in the Previous Year on Ozone Concentrations

Regulatory Actions[a]	Total (1)	Average (2)
Inspections in the previous year	-0.008^{+} (0.005)	-0.228 (0.174)
Enforcement actions in the previous year	-0.002 (0.005)	-0.253 (0.373)
Penalty in the previous year (log)	-0.159* (0.061)	-0.204* (0.083)
Number of surrounding facilities	0.043* (0.021)	0.031 (0.021)
Carbon monoxide nonattainment	1.584 (1.226)	1.410 (1.202)
PM_{10} nonattainment	-1.472 (1.377)	-1.502 (1.390)
$PM_{2.5}$ nonattainment	-3.499** (0.965)	-3.841** (0.944)
Percent white population of the county	-0.450 (0.464)	-0.449 (0.465)
Mean income of the county (/$1000)	-0.280* (0.140)	-0.275* (0.139)
Unemployment rate of the county	-0.580^{+} (0.327)	-0.596^{+} (0.326)
July mean temperature	0.229** (0.086)	0.231** (0.087)
July total precipitation in inches	-0.131 (1.090)	-0.133 (1.083)
July mean wind speed	0.059 (0.148)	0.057 (0.149)
Observations	1,224	1,224
Number of monitors	167	167
Adjusted R-squared	0.289	0.288

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; missing weather and year dummy variables included but not shown.

[a] Column titles (“total” and “average”) refer to the explanatory regulatory actions variable.

Table 14. Fixed Effects Regressions of the Impact of Regulatory Actions in the Previous Year on the 100 * Natural Log of Ozone Concentrations

Regulatory Actions[a]	Total (1)	Average (2)
Inspections in the previous year	-0.011^{+}	-0.341
	(0.006)	(0.224)
Enforcement actions in the previous year	-0.009	-0.826^{+}
	(0.007)	(0.485)
Penalty in the previous year (log)	-0.192*	-0.220^{+}
	(0.083)	(0.112)
Number of surrounding facilities	0.044	0.026
	(0.028)	(0.027)
Carbon monoxide nonattainment	1.693	1.373
	(1.420)	(1.385)
PM_{10} nonattainment	-0.932	-1.006
	(1.647)	(1.666)
$PM_{2.5}$ nonattainment	-2.265^{+}	-2.774*
	(1.291)	(1.265)
Percent white population of the county	-0.745	-0.720
	(0.616)	(0.617)
Mean income of the county (/$1000)	-0.472*	-0.452*
	(0.197)	(0.195)
Unemployment rate of the county	-0.527	-0.557
	(0.420)	(0.419)
July mean temperature	0.255*	0.260*
	(0.101)	(0.102)
July total precipitation in inches	0.020	-0.038
	(1.584)	(1.559)
July mean wind speed	0.041	0.039
	(0.220)	(0.220)
Observations	1,224	1,224
Number of monitors	167	167
Adjusted R-squared	0.241	0.241

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; missing weather and year dummy variables included but not shown.

[a] Column titles ("total" and "average") refer to the explanatory regulatory actions variable.

Table 15. First Differences Regressions of the Impact of Regulatory Actions in the Previous Year on Ozone Concentrations

Regulatory Actions[a]	Total (1)	Average (2)
Inspections in the previous year	-0.002	-0.190
	(0.004)	(0.177)
Enforcement actions in the previous year	-0.016**	-0.849*
	(0.006)	(0.398)
Penalty in the previous year (log)	-0.084	-0.088
	(0.067)	(0.094)
Number of surrounding facilities	-0.091**	-0.092*
	(0.035)	(0.035)
Carbon monoxide nonattainment	-1.633	-1.736
	(1.498)	(1.477)
PM_{10} nonattainment	0.060	0.141
	(1.912)	(1.911)
$PM_{2.5}$ nonattainment	1.364	1.180
	(1.442)	(1.477)
Percent white population of the county	0.261	0.300
	(0.536)	(0.535)
Mean income of the county (/$1000)	-0.682**	-0.685**
	(0.144)	(0.146)
Unemployment rate of the county	0.452	0.453
	(0.396)	(0.398)
July mean temperature	0.443**	0.444**
	(0.108)	(0.108)
July total precipitation in inches	0.935	0.792
	(1.244)	(1.219)
July mean wind speed	0.088	0.080
	(0.194)	(0.192)
Observations	1,056	1,056
Number of monitors	166	166
Adjusted R-squared	0.186	0.185

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; missing weather and year dummy variables included but not shown.

[a] Column titles (“total” and “average”) refer to the explanatory regulatory actions variable.

Table 16. Coefficients of Total Regulatory Actions in Fixed Effects Regression of the Impact of Regulatory Actions on Ozone Concentration, Omitting Various Control Variables

	(1)	(2)	(3)	(4)	(5)
Total inspections in the previous year	-0.005	-0.012*	-0.008	-0.010*	-0.013*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Total enforcement actions in the previous year	-0.003	-0.002	-0.004	-0.003	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Total penalty in the previous year (log)	-0.152*	-0.154*	-0.141*	-0.160**	-0.141*
	(0.061)	(0.063)	(0.062)	(0.061)	(0.063)
Number of surrounding facilities	✗	✓	✓	✓	✗
NAAQS nonattainment (CO, PM_{10}, and $PM_{2.5}$)	✓	✗	✓	✓	✗
Demographic variables (income, unemployment, and percent white)	✓	✓	✗	✓	✗
Weather variables (mean temperature, total precipitation, and mean wind speed)	✓	✓	✓	✗	✗

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; number of surrounding facilities, NAAQS nonattainment, demographic variables and weather variables included in some specifications but not shown; year dummy variables included but not shown.

Table 17. Coefficients of Average Regulatory Actions in Fixed Effects Regression of the Impact of Regulatory Actions on Ozone Concentration, Omitting Various Control Variables

	(1)	(2)	(3)	(4)	(5)
Average inspections in the previous year	-0.238	-0.286	-0.223	-0.334^{+}	-0.369*
	(0.175)	(0.177)	(0.177)	(0.170)	(0.171)
Average enforcement actions in the previous year	-0.299	-0.291	-0.354	-0.242	-0.293
	(0.375)	(0.383)	(0.373)	(0.380)	(0.380)
Average penalty in the previous year (log)	-0.198*	-0.190*	-0.175*	-0.206*	-0.181*
	(0.083)	(0.085)	(0.083)	(0.083)	(0.084)
Number of surrounding facilities	✗	✓	✓	✓	✗
NAAQS nonattainment (CO, PM_{10}, and $PM_{2.5}$)	✓	✗	✓	✓	✗
Demographic variables (income, unemployment, and percent white)	✓	✓	✗	✓	✗
Weather variables (mean temperature, total precipitation, and mean wind speed)	✓	✓	✓	✗	✗

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; number of surrounding facilities, NAAQS nonattainment, demographic variables and weather variables included in some specifications but not shown; year dummy variables included but not shown.

Table 18. Regression Coefficients of Regulatory Actions in Fixed Effects Regressions of the Impact of Regulatory Actions in the Previous Year on Ozone Concentrations

	Fixed Effects without Year Dummies		Ordinary Least Squares without Fixed Effects with Year Dummies		Ordinary Least Squares without Fixed Effects and Year Dummies	
Regulatory Actions[a]	Total (1)	Average (2)	Total (3)	Average (4)	Total (5)	Average (6)
Inspections in the previous year	-0.008 (0.006)	-0.305 (0.196)	-1.854^{+} (1.097)	-1.955** (0.506)	-2.136^{+} (1.094)	-2.093** (0.456)
Enforcement actions in the previous year	-0.008 (0.005)	-0.465 (0.401)	-0.816 (1.368)	0.015 (0.810)	-0.958 (1.407)	-0.050 (0.833)
Penalty in the previous year (log)	-0.172** (0.058)	-0.222** (0.079)	52.635** (16.140)	0.599** (0.209)	51.241** (15.995)	0.579** (0.207)

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; number of surrounding facilities, NAAQS nonattainment, demographic variables, and weather variables included but not shown.

[a] Column titles ("total" and "average") refer to the explanatory regulatory actions variable.

Table 19. Differences-in-Differences Regressions of the Impact of Regulatory Actions in the Previous Year on Ozone Concentrations

Regulatory Actions[a]	Total (1)	Average (2)
Inspections in the previous year	0.012**	-0.363^{+}
	(0.003)	(0.214)
Enforcement actions in the previous year	-0.012*	-1.212**
	(0.005)	(0.448)
Penalty in the previous year (log)	0.049	0.056
	(0.041)	(0.097)
Number of surrounding facilities	-0.026	-0.018
	(0.063)	(0.063)
Carbon monoxide nonattainment	-1.014	-1.177
	(1.060)	(1.053)
PM_{10} nonattainment	-0.176	0.399
	(2.152)	(2.124)
$PM_{2.5}$ nonattainment	-0.331	-1.089
	(1.317)	(1.302)
Percent white population of the county	0.524	0.586
	(0.687)	(0.681)
Mean income of the county (/$1000)	-0.035	-0.102
	(0.177)	(0.169)
Unemployment rate of the county	-0.762	-0.735
	(0.553)	(0.538)
July mean temperature	0.120*	0.125*
	(0.058)	(0.058)
July total precipitation in inches	0.922	1.002
	(0.860)	(0.836)
July mean wind speed	-0.028	-0.006
	(0.127)	(0.135)
Observations	895	895
Number of monitors	162	162
Adjusted R-squared	0.173	0.165

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; missing weather and year dummy variables included but not shown.

[a] Column titles ("total" and "average") refer to the explanatory regulatory actions variable.

Figures

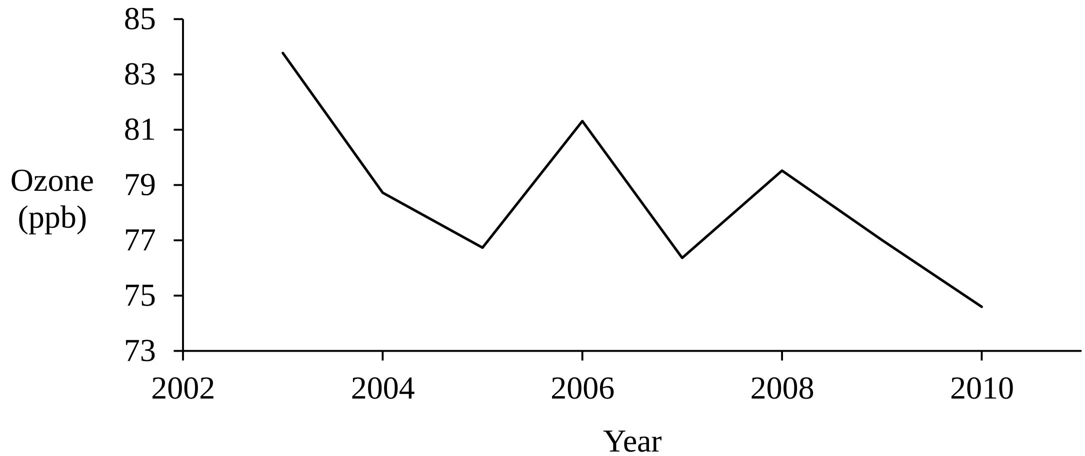

Figure 2. Graph of Mean Ozone Concentrations Over Time

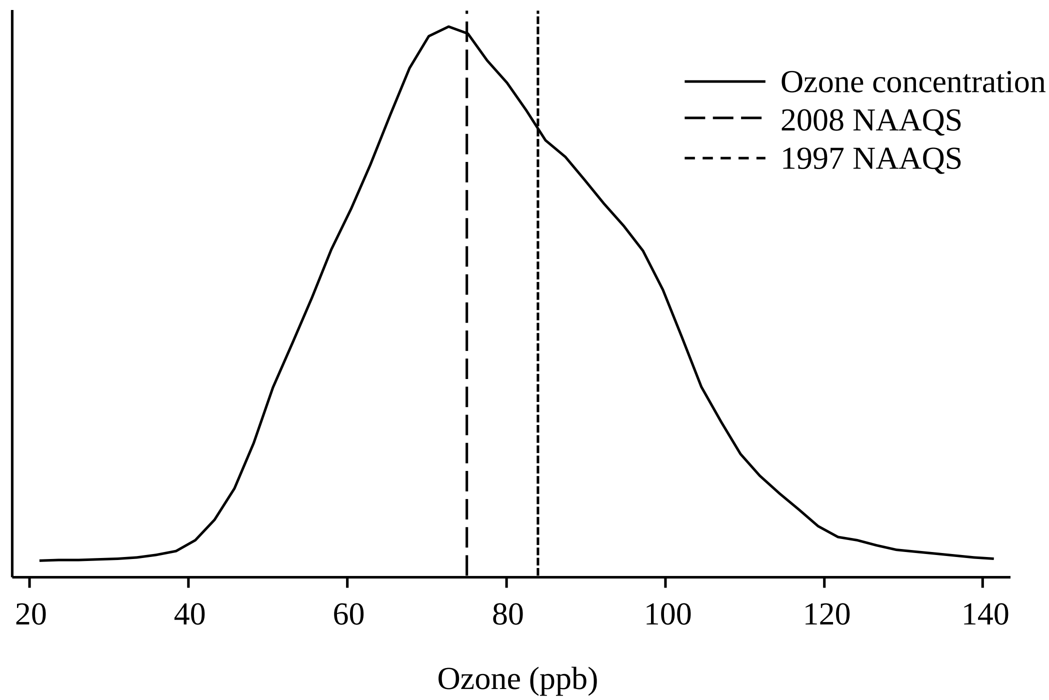

Figure 3. Distribution of Ozone Concentration

CHAPTER III

INVESTIGATING ESCALATING PENALTIES FOR REPEAT ENVIRONMENTAL VIOLATIONS

Introduction

Escalating penalties for repeat offenders are common in the law. For example, under the U.S. Environmental Protection Agency's (EPA) civil penalty guidelines, a violator's history of noncompliance can increase the size of the penalty assessed (EPA 1984). Indeed, it seems like common sense that those that have repeatedly shown disregard for the law should be subject to increasing punishment for their actions. However, existing theoretical research in law and economics has not been able to justify the ubiquity of escalating penalties, and there is little existing empirical research that investigates the existence and impact of escalating penalties.

In this chapter, I examine the theoretical and empirical literature on escalating penalties, as applied to environmental regulation, and analyze possible implications for repeat offender policy. First, I analyze the law and economics theory regarding escalating penalties for repeat offenders and suggest some possible extensions to the theory that can account for the impact of fairness and the social norm of law compliance. Second, I discuss the empirical literature that examines repeat offenders in environmental regulation.

Third, I present my empirical analysis of repeat offenders in air pollution regulation in California. I find that most facilities comply: over half the facilities did not commit any violations over the entire nine-year study period. Even repeat offenders are not assessed very large penalties, implying that their violations are not very serious. I also find no evidence of increasing penalties. Additionally, I find that facilities with long spells of noncompliance face smaller penalties on average, indicating that the "worst actors," with the longest spells of noncompliance, are committing less severe violations on average than the better actors. Thus, while my data do not show escalating penalties, it is likely because repeat violations are less severe and any escalation is mitigated by the reduced severity of the subsequent violation.

The Theory of Escalating Penalties

The theoretical law and economics literature has focused on the optimality of the enforcement regime, explaining how a regime of escalating penalties for repeat offenders can improve social welfare. Generally, these models use mathematical equations to represent each individual's utility and decisions, and they assume that regulators aim to maximize social welfare—the sum of all individuals' utilities.[36]

In this section, I describe the model of efficient public law enforcement, and then discuss various models that examine the optimality of escalating penalties. I explore several models that argue that decreasing penalties are optimal, then analyze models that rely on various assumptions to justify escalating penalties.[37] Lastly, I discuss which assumptions are the most realistic. I believe a model in which the gains to the violator are not counted in social welfare and different types of violators have different benefit from

[36] In this chapter, I use environmental regulation as an example, although a lot of this literature is applicable to other types of public law enforcement, such as the criminal justice system.
[37] Although my analysis divides the models by their assumptions, it is important to note that many models rely on more than one of these assumptions.

each violation is the most realistic. I also describe potential additions to the models that can better account for fairness and the social norm of law compliance.

Optimal Deterrence Theory

In order to maximize social welfare, regulators should aim for optimal deterrence, not complete deterrence (Polinsky and Shavell 2000a).[38] As some individuals might stand to gain more than society is harmed by a violation, complete deterrence, which deters all individuals from committing the violation, might not be socially efficient. Instead, regulators should deter only violations that would harm society more than they benefit the violators, and imposing an expected penalty that equals to the harm provides optimal deterrence.

Based on this model of optimal deterrence,[39] it is difficult to justify escalating penalties for repeat offenders. If the penalty scheme induces optimal behavior and only socially beneficial violations are committed, then deviating from the optimal penalty, by making the penalty depend on previous violations, will incentivize inefficient behavior. Nonetheless, some models show that escalating penalties can be efficient because they allow regulators to focus costly regulatory effort on a smaller group of repeat violators.

Models that Support Decreasing Penalties

Several theoretical models advocate decreasing penalties for repeat violations. The underlying intuition is relatively simple: in order to detect a repeat violation, regulators must detect a first violation. As regulators are more likely to impose a penalty

[38] Even though the EPA cannot consider costs when setting the National Ambient Air Quality Standards, regulators are still permitted to consider costs in making enforcement decisions.
[39] There have been other models that consider non-economic factors, for instance, Polinsky and Shavell (2002b) describe a model that accounts for fairness.

for the first violation than for a repeat violation, it makes sense to maximally deter the first violation.

For instance, Burnovski and Safra (1994) described a model in which individuals decide how many violations to commit, and found that the optimal policy involved decreasing penalties. Emons's (2003) two-period model allowed individuals to choose to violate in each time period, and he found that decreasing penalties are optimal. In a later paper, he (Emons 2004) found that decreasing penalties are subgame perfect.

Dana (2001) examined escalating penalties in the context of behavioral biases. He argued that first-time offenders underestimate detection because of optimism bias and repeat offenders overestimate detection because of salience. Therefore, he asserted, decreasing penalties make more sense.

Thus, even though escalating penalties create additional deterrence for repeat offenders, some models argued that decreasing penalties are optimal because they deter the first violation, thus deterring repeat violations.

Minimizing Enforcement Costs

Enforcement is costly, and many researchers have found that escalating penalties can reduce enforcement costs and thus improve social welfare. Harrington's (1988) model divided firms into two groups: a compliant group and a noncompliant group. Firms in the compliant group face a lower probability of detection and a lower penalty if they are found in violation; if found in violation, they are moved to the noncompliant group, where they face a higher probability of detection and a higher penalty if they are found in violation again. Firms in the noncompliant group may move back into the compliant group if they are found to be in compliance.

Harrington found that, because detection is costly, the regulator can save on detection costs by concentrating its enforcement effort on the small noncompliant group and imposing higher penalties on that group if a violation is found. However, this model does not fully explain why escalating penalties are optimal because Harrington assumed that penalties are higher in the noncompliant group. In a later model that had less restrictive assumptions, Harford (1991) found that the optimal penalty in both groups is the maximum penalty and the optimal solution does not involve escalating penalties.

Polinsky and Shavell (1998) developed a two-period model in which it is efficient to treat repeat offenders more harshly. In their scheme, the optimal policy is to impose the maximum possible penalty for any offense in the first period; in the second period, regulators impose the maximum possible penalty on those that are considered repeat offenders and impose a smaller penalty on those who are considered first-time offenders. This penalty scheme is somewhat unusual because first-time offenders in the second period are treated differently than first-time offenders in the first period and the penalties for the first and second violations are the same. Thus, this model does not describe escalating penalties.

Although these models do not fully justify escalating penalties, they provide some insight as to how escalating penalties might improve social welfare by reducing detection and enforcement costs.

Changes in Benefits from Violation and Detection Probabilities

Some authors justify escalating penalties using changes in the violator or regulator between the first violation and repeat violations. For example, a firm with a history of environmental violations might lose customers, thus reducing its profits. Miceli and Bucci (2005) found that, if opportunity costs for repeat violations are smaller, escalating penalties are needed to maintain deterrence.[40]

Several articles examined the role of learning. For instance, Friehe (2009) showed that if violators are unsure if their acts are violations but overestimate the probability that they are violations, escalating penalties are optimal. In Mungan's (2010) model, violators could learn to evade detection, and regulators could learn to detect violations. He found that if violators learned more than regulators did, escalating penalties are optimal.

Repeated Violations Reveal Information About the Violator

Many models divided potential violators into types and used repeated violations as a mechanism to reveal information about the violator. For instance, Emons's (2007) two-period model assumed that individuals choose whether to be law abiding; he found that, if the benefit from the violation is high relative to the individual's wealth, the regulator imposes increasing penalties to induce individuals to be law abiding.

McCannon (2009) divided violators into occasional violators and habitual violators. He made three assumptions: (1) violations by occasional violators are socially beneficial; (2) violations by habitual violators are socially undesirable; and (3) habitual violators gain more from a violation than occasional violators.[41] He found that, under these assumptions, increasing penalties are optimal. A central assumption of the model is that violations by different types of violators cause different amounts of harm, thus justifying escalating penalties. However, it is not clear why the same violation by

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[40] The authors assumed that all gains from violation are illicit and are not counted in social welfare. However, they admitted that, if the benefit from violation is counted in social welfare, then optimal penalties would not escalate as individuals would fully account for future lost income when deciding whether to violate.

[41] In other words, he assumed that $harm_{occassional} < benefit_{occassional} < benefit_{habitual} < harm_{habitual}$.

different types of violators should cause different amounts of harm, especially in the context of environmental regulation.

Miceli's (2012) model used three types of potential violators: those with a low gain, those with a high gain, and undeterrable, irrational violators.[42] The author found that escalating penalties are desirable as the fraction of undeterrable offenders in the population becomes high. While this model justifies escalating penalties, it is not very applicable to environmental regulation as it requires irrational firms to be a high proportion of the regulated community.

These models rely on repeat violations to reveal information about the violator and reduce enforcement costs by allowing the regulator to focus enforcement resources on those who are more likely to violate. This structure is applicable to environmental regulation as facilities with high abatement costs likely gain more from violation (by avoiding the abatement costs) than those with low abatement costs; thus, escalating penalties allow regulators to deter these facilities more strongly.

Illicit Gains from Violation

Other models consider some or all of the violator's gain from the violation as "illicit gains," which are not considered in the social welfare function but still drive the violator's decision to violate. While this assumption is not traditional, it is perhaps more realistic. The EPA (1984, 1991) guidelines advise that the penalty should, at a minimum, remove all the benefit derived from a violation.[43] This implies that regulators consider this gain at least somewhat illegitimate.

[42] The author treats gains for violation as illicit and does not include gains from violation in the social welfare function.

[43] The penalty guidelines also consider the harm caused by the violation. More specifically, the penalty has a benefit component, which accounts for the benefit derived from the violation, and a gravity component,

Polinsky and Rubinfeld (1991) developed a model in which individual actors have an unobservable illicit gain from committing a violation. The illicit gain could, for instance, represent the individual's propensity to violate. Individuals with higher illicit gains are more likely to violate repeatedly, and, because the illicit gains are not counted in the social welfare function, violators with higher illicit gains must be deterred more strongly. They found that, for certain parameter values, higher penalties on repeat offenders are optimal.

Baik and Kim (2001) extended the Polinsky and Rubinfeld model to include sociological characteristics by allowing illicit gains to change over time. They found that, if the change in illicit gains is large compared to the initial illicit gains, it is desirable to punish repeat offenders as seriously as first-time offenders. This is because individuals anticipate the future increase in illicit gains, and first-time offenders must be punished more severely in order to offset that future increase in illicit gain. They also found that, for certain parameter values, escalating penalties are optimal.

Implications of Theory and Possible Extensions

Although illicit gains are a somewhat unconventional assumption, they match up with the moral intuition that individuals should not benefit from their wrongdoing and the EPA (1984, 1991) penalty guidelines that encourage regulators to, at minimum, assess the full benefit of a violation. In my opinion, the strongest case for escalating penalties is that regulators consider some or all of the gains from a violation as illicit.

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which considers other factors, including the harm caused by the violation, the importance of the violation to the regulatory scheme, and the size of the violator. The gravity component can be further adjusted to reflect the degree of willfulness or negligence in the violation, the violator's degree of cooperation, the violator's history of noncompliance, and resulting environmental damage.

Each firm's illicit benefit of violation is not necessarily unobservable to the regulator; for instance, the EPA uses models to estimate the economic benefit of violation. Nonetheless, repeat violations reveal that the previous penalties have been insufficient to deter and the facility has an otherwise unobservable higher propensity to violate. Thus repeat violations can reveal that the violator's gains are high (or higher than the regulator previously thought), so escalating penalties are warranted. Focusing on these facilities could be the best way to allocate a regulator's limited budget. Thus, a combination of illicit gains from violation, facility type, and cost minimization can justify escalating penalties. Nonetheless, I believe models should incorporate the social norm of law compliance and a sense of fairness to better explain escalating penalties.

Social norms likely drive managers' decisions on whether to comply with environmental regulations. While there are many norms at play (Vandenbergh 2003), I focus on the norm of law compliance as it can produce positive externalities; a strong norm of law compliance can allow regulators to achieve a high compliance rate with relatively little enforcement resources.

Many models accommodate concerns other than the monetary gain from violation; for instance, Polinsky and Rubinfeld (1991) suggested that the illicit gains in their model might represent the propensity to commit an offense. Similarly, for environmental regulations, one could model the social norm of law compliance as the illicit gain portion of the facility's decision to violate. Within a facility, individual managers make compliance decisions; these decisions are, in turn, influenced by the facility's culture and attitude towards law compliance (Simpson et al. 2013). Those facilities with a strong desire to comply with the law would benefit less from a violation than those with less desire to comply, even though the monetary benefit might be the same. Thus, to the extent that these models allow for individuals to benefit differently from a violation, they are, implicitly, accounting for this social norm.

However, norms can change in response to regulators' decisions, and these models have not accounted for that.[44] In the case of repeat violations, escalating penalties could be part of the norm of law compliance. If penalties do not escalate, then penalties might be seen as the price of a violation (Gneezy and Rustichini 2000) that can be bought freely in the market for environmental compliance. This, in turn, could weaken the norm of law compliance. Thus, the strength of the norm, modeled as an illicit benefit of violation, could depend on how much penalties escalate.

Lastly, regulators are likely concerned about fairness to the regulated entities and the community affected by pollution. The concern regarding fairness towards the regulated entities likely imposes some limitations on the maximum penalty.[45] Although many models described the maximum penalty as limited by wealth, Harrington (1988) suggested that large penalties could be viewed as unfair, placing practical limits on penalties. Thus, many of these models that assumed a maximum penalty implicitly accounted for this; however, they do not allow the maximum penalty to vary based on violation history.

A sense of fairness could limit the maximum penalty differently for first and repeat violations. For example, a large penalty for the first violation might seem unfair, especially if the facility had made some effort to comply. Thus, fairness could limit the

[44] At the most basic, if there is a very low enforcement rate, then the norm of law compliance might get much weaker. In an economic model, this could be modeled as a change in benefit from a violation that depends on the probability of detection.
[45] Although there are statutory limits on penalties as well, these limits are generally very high and not binding.

maximum penalty for first-time violations. On the other hand, repeat violations might show that the facility is not making any effort to comply, and a large penalty for a repeat violation might not seem unfair. Thus, fairness might restrict maximum penalties for the first violation but allow larger maximum penalties for repeat violations.

Alternatively, fairness could be part of the social welfare function. For instance, Polinsky and Shavell (2000b) introduced a model in which the fairness of a penalty, which depends on the size of the penalty, is part of the objective function that the regulator maximizes. A modification to their model, which allows the fairness of the penalty to depend on the size of the penalty and the facility's violation history, would allow researchers to investigate the role fairness plays in escalating penalties.

The regulator might also be concerned about fairness to the surrounding community. While it might be willing to tolerate occasional violations, if a facility violates repeatedly, it repeatedly releases pollution in the same affected community. Society might believe that some environmental damage is inevitable, but it might believe that this damage should be spread equally throughout the population in general, instead of being focused on the same community surrounding the repeat violator. Thus, out of a sense of fairness, regulators might impose increasing penalties so that a surrounding community does not have to put up with persistently poor environmental quality.

Thus, these law and economics models that analyzed illicit gains, different types of violators, and enforcement costs provide the most likely justification for escalating penalties. Nonetheless, none of them explains the phenomenon fully, and accounting for social norms and fairness could improve the models.

Existing Empirical Evidence Regarding Escalating Penalties

The empirical literature that specifically examines escalating penalties for repeat violations in environmental regulation is somewhat sparse. There is little information on how prevalent repeat offenders are and how regulatory policy treats them. In this section, I will describe some of the evidence regarding repeat offenders. First, I briefly discuss the different types of deterrence and how they relate to repeat violations. Second, I describe various studies that investigated whether poor compliance history increases the size of penalties. Third, I discuss studies that examined the impacts of regulatory actions on repeat violations.

Penalties provide two types of deterrence, specific deterrence and general deterrence. Specific deterrence discourages the same violator from violating again in the future, while general deterrence discourages other entities from violating. Thus, research that examines the effectiveness of specific deterrence can be said to examine repeat violations, even if it does not focus on escalating penalties. For instance, Weber and Crew (2000), in their analysis of oil spills, found that penalties were effective at reducing amount of oil spilled, but Viladrich Grau and Groves (1997) found that penalties had no impact on the frequency of oil spills and amount of oil spilled during oil transfer. Helland (1998) investigated inspection targeting, not escalating penalties, and found evidence of inspection targeting based on the facility’s compliance history, but no evidence that such targeting improved compliance outcomes.

Most of the articles that examined repeat offenders examined the characteristics of repeat offenders and whether the size of penalties depends on violation history. While it is EPA policy to assess higher penalties on those with a violation history (e.g., EPA

1984, 1991), actual practice might differ from policy. For instance, the EPA guidelines allow regulators to consider the firm's ability to pay when it is assessing penalties (EPA 1991), and regulators may be reluctant to assess large penalties as that might cause facilities to shut down and employees to lose their jobs (Gray and Deily 1996); thus, penalties might not escalate.

Denning and Shastri (2000) examined the characteristics of companies against whom the EPA brought civil suits that were not settled.[46] Of the firms in their sample, they found that 34.6% of the public companies were repeat offenders, and 12.6% of non-profit organizations were repeat offenders. Additionally, 8.4% of closely held firms were repeat offenders.

Oljaca, Keeler, and Dorfman (1998) examined the determinants of penalties for water pollution violations in Georgia. They found that firms with a history of violations received a penalty that was, on average, $5,616 larger. This is quite large compared to the average penalty of their sample, $12,786. Kleit, Pierce, and Hill (1998) investigated the determinants of penalties on water pollution violations in Louisiana. In their analysis, an enforcement action could result in a compliance order or a penalty, and they investigated how the number of previous violations affected whether the enforcement action resulted in a penalty and the size of the penalty. They found that increasing the number of previous violations increased the probability that the facility was issued a penalty and finally agreed to pay a penalty. Additionally, they found that an increase in the number of previous violations by one increased the penalty assessed for a violation by $4,580 and increased the final agreed settlement by $2,180.

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[46] Note that this sample is not typical of environmental enforcement actions; under many federal environmental statutes, states are responsible for a majority of the enforcement actions and they usually resolve the cases administratively.

Simpson, Garner, and Gibbs (2007) examined repeat violations at the firm level. This study is different as most enforcement studies focus on the facility level, not the firm level, and substantial research is needed to understand the corporate ownership of the facilities. They found that enforcement actions and inspections had no impact on their measure of recidivism, the number of violations committed by the firm in that quarter. Miller (2005) examined all federal regulatory actions against U.S. companies for environmental violations between 1970 and 1997 and found that civil judicial law suits were not more effective than administrative actions, which carry lower penalties, at reducing repeat violations. However, he found that criminal suits significantly reduced repeat violations.

Generally, there has been very little empirical research on escalating penalties and repeat offenders. A few studies found that penalties increased as violation history increased (Denning and Shastri 2000; Oljaca, Keeler, and Dorfman 1998). Others found mixed evidence on the effectiveness of monitoring and enforcement actions on repeat violations at the firm level: Simpson, Garner, and Gibbs (2007) found that enforcement actions had no impact on repeat violations at the firm level, while Miller (2005) found that civil enforcement actions were not effective but criminal enforcement actions were.

New Empirical Research on Repeat Violators and Escalating Penalties

In this section, I present my research into the nature of escalating penalties for repeat offenders in California air pollution regulation and show that, generally, repeat offenders are not a serious problem. I show that facilities are quite compliant, even though there is no evidence of escalating penalties. Furthermore, facilities that have long spells of noncompliance receive smaller penalties, on average, than those that have short spells of noncompliance, implying that repeat violators commit less severe violations on average.

Data Description and Summary Statistics

My study focuses on California because its persistent air quality problems and aggressive monitoring and enforcement policy provide an interesting case study. I obtain monitoring and enforcement data from the EPA's Air Facility System (AFS) database. Regulatory data are available between 2002 and 2010; I limit my period of study to the period after 2001 because compliance monitoring policy and data reporting practices changed significantly in late 2001 (EPA 2001).

The AFS is the EPA's database for sources regulated by the Clean Air Act. The database contains details of each polluting facility and regulatory actions carried out against it.[47] I restrict the sample to facilities that existed during the entire study period because the EPA removes facilities that have shut down from the AFS. This restriction could cause selection problems if repeat violators are forced to shut down by escalating penalties. This would weaken my finding that penalties do not escalate; it would also imply that escalating penalties are effective at reducing future violations. Additionally, if the worst violators are the ones that are forced to shut down, then my analysis of the repeat violator policy will be missing a crucial piece of the puzzle—the impact of the policy on the worst violators.[48] However, this might not be an issue because regulators

[47] For enforcement actions, I only have information on the size of the penalty, not the nature of the enforcement action. Escalating penalties could manifest as a more stringent enforcement action, such as a shutdown order instead of a compliance order. Unfortunately, I cannot study this because I lack the necessary data.

[48] Future research can overcome this problem by examining facilities that have shut down. For instance, I could examine whether those facilities tend to be repeat violators, and whether they were assessed escalating penalties. I have obtained this data through a Freedom of Information Act request.

generally try to avoid causing facilities to shut down as it entails job loss. Furthermore, both EPA (1991) guidelines and the California Health and Safety Code allow regulators to consider the violator's ability to pay when deciding the penalty.

There are 1,355 facilities over nine years, which makes 12,195 observations. Table 20 shows a summary of the number of inspections and enforcement actions and total penalty per facility-year. There is a mean of 1.79 inspections per facility-year, and 63.4% of facilities-years received at least one inspection. The mean number of enforcement actions is 0.60 and 17.1% of the facility-years had at least one enforcement action. The mean penalty per facility-year is $6,993. However, only 12.7% of the facility-years had any penalties. Conditional upon a penalty, the mean is $54,910.

Evidence of Escalating Penalties

I treat receiving a penalty as an indication of noncompliance as it represents the termination point of the violation and the regulator's decision that the facility has committed a violation severe enough to warrant a penalty. If a facility was assessed a penalty in the year, I treat the facility as noncompliant for the year. As shown in Table 20, the average noncompliance rate is 12.7%. Table 21 shows the number of years the facilities were noncompliant over the entire period. Over half of the facilities in the sample had no violations for the entire period; 19.7% spent one year in violation and 12.3% spent two years in violation. A majority of the facilities were relatively compliant; 83.6% of the facilities spent two or fewer of the nine years in violation. On the other end of the spectrum, three facilities (0.2%) were in violation for all nine years and nine facilities (0.7%) were in violation for eight of the nine years.

Next, I use regression analysis to examine escalating penalties. Table 22 shows the impact of the natural logarithm of the size of previous-year penalties on the natural logarithm of the size of current-year penalties for facilities with both previous- and current-year violations. I control for other variables on the county-year level: unemployment rate from the Bureau of Labor Statistics, income from the Bureau of Economic Analysis, percent white from the Census Bureau, and National Ambient Air Quality Standards nonattainment from the EPA's Green Book.

The regression shows no evidence of increasing penalties. Instead, the regression implies decreasing penalties: an increase in the previous-year penalty by 1% is associated with a 0.21% decrease in the current-year penalty. This shows that, for facilities that are in violation for two consecutive years, penalties do not escalate; instead, penalties are decreasing. Note that previous-year enforcement actions are associated with an increase in penalties, showing that violation history can increase penalties.[49] Naturally, this analysis applies only to instances in which facilities were found to be in violation for two consecutive years.

Penalties over the Spells of Noncompliance

Lastly, I examine the length of the spells of noncompliance. I consider a noncompliance spell as a number of consecutive years that a facility is noncompliant. Thus, if a facility was noncompliant in years 2004 and 2005 but compliant in all other years, its maximum noncompliance spell is two years. If a facility was noncompliant in 2004 and 2006 only, then its maximum noncompliance spell is one year. I consider

[49] Future iterations of this study can investigate the different impacts of enforcement actions that result in penalties and enforcement actions that do not. This will allow me to disentangle the different impacts of the penalty and the enforcement action.

facilities that have a noncompliance spell of at least two years as repeat offenders.[50] Table 23 shows a summary of the lengths of the longest noncompliance spells of each facility. Most facilities have short noncompliance spells; 31.7% of the facilities have noncompliance spells of one year. Other facilities can be considered repeat offenders: 9.8% have noncompliance spells of two years, and 6.9% have noncompliance spells of three years or longer. A total of 16.7% of the facilities can be considered as repeat offenders at some point during the time period.

Next, I examine the nature of repeat violations. Table 24 shows the trend of the penalty as the noncompliance spell progresses for facilities that have completed spells of noncompliance.[51] I omit from this table facilities with incomplete spells of noncompliance—facilities whose longest spell of noncompliance was still ongoing in 2010, the last year of available data. Focusing on the second column of the table, there were 116 facilities whose longest spell of noncompliance was two years long. The mean penalty during the first year of the noncompliance spell was $61,307. The standard deviation was $303,926, and the median was $3,779. During the second year of noncompliance, the mean penalty was $67,640 and the median penalty was $3,000. The difference in means is not statistically significant ($p = 0.92$).[52] I also calculated each facility's total penalty over the entire noncompliance spell and the per-year penalty over

[50] In this case I consider another violation of the Clean Air Act by the same facility in the next year as a repeat violation. Naturally, the definition of repeat violations can vary. For example, one could define a repeat violation as another violation of any environmental statute by the same firm in the next five years (EPA 2008).

[51] If there were more than one noncompliant spells of the same maximum length, I use the first noncompliance spell.

[52] These figures, with the mean much larger than the median and a large standard deviation, indicate that there are several very large values. For instance, the maximum penalty during the first year of noncompliance was $2.44 million, while the maximum for the second year of noncompliance was $6.50 million. (The two penalties were at different facilities.) Thus, I also included the medians in the table.

the entire noncompliance spell. The average total penalty was $128,947, while the median total penalty was $8,525. The median per-year penalty was $4,263.

If penalties escalate for repeat violations, then penalties for the second year of noncompliance should be higher than those for the first year of noncompliance, penalties for the third year of noncompliance should be higher than those for the second year of noncompliance, and so on. However, looking at Table 24, this is not entirely the case.[53] While penalties generally increase in the first two years of noncompliance, they do not keep increasing after that. For instance, for those with spells of noncompliance of four or more years, the first-year mean penalty is $28,761 and the median penalty is $6,635. For the second year, the mean increases to $29,171 and the median increases to $8,040. But, for the third year, although the mean penalty increases to $75,840, the median decreases to $6,220. Based on this table and on Table 22, it seems that, if penalties escalate, they do not escalate very much.

Regulators likely focus on the severity of the violation in determining the size of the penalty; less severe violations draw smaller penalties. Thus, decreasing penalties likely show that subsequent violations are less serious than previous violations, not necessarily that the size of penalties are not affected by violation history. If there is penalty escalation for repeat violations, it is not obvious and might be obscured by a decrease in penalty due to a decrease in the severity of the violation. Nonetheless, there is no evidence that the regulators increase penalties sharply to bring the facilities into full compliance.

[53] Even if the penalties decline, it might still be considered escalating penalties if the repeat violations are less severe. I discuss this later in the chapter.

Moreover, penalties seem to be lower, on average, for those with longer noncompliance spells. Focusing on the first two years of noncompliance, those with four or more years of noncompliance were assessed lower penalties ($28,761 and $29,171) than those with three years of noncompliance ($44,553 and $40,137), which, in turn, were assessed lower penalties than those with two years of noncompliance ($61,307 and $67,640). Additionally, per-year penalty seems to be lower for facilities with longer noncompliance spells. For instance, the average penalty for those with noncompliance spells of four or more years is $41,029, which is lower than the penalty for those with three-year spells, $49,129. In turn, this is lower than the average for those with two-year spells. Again, this implies that those with longer noncompliance spells, on average, commit less severe violations.

However, the median per-year penalty increases for those with longer noncompliance spells, perhaps an indication of escalating penalties for at least some of the facilities. It is possible that penalties escalate slightly for most facilities, but fluctuations in the severity of the violations tend to dominate these small escalations in penalties.

Table 25 shows the statistics for facilities whose longest noncompliance spells continued through to the last year of the available data. There are far fewer observations, with 51 in total. These facilities seem quite different from those with completed spells of noncompliance. The 18 facilities that had one-year noncompliance spells that ended the year my data ended had been compliant for the entire period, 2002-2009, but violated in the last year, 2010. For these facilities, the mean penalty is rather high, $241,076, likely driven by an outlier.[54] The median penalty is $2,000, which is slightly smaller than that at facilities that had completed spells of noncompliance. Based on this column, even first-time offenders can be assessed large penalties, likely because of the severity of the violation.

Generally, those that were in violation for a relatively long period of time and were still in violation by the end of the period could be considered to be bad actors. Even then, they were not assessed very high penalties. The mean penalty is generally quite low; for example, those in years six and seven of their violation spell were assessed a mean of $15,849 and $21,371 (and a median of $10,200 and $16,325).[55] For those that were in violation for all nine years of the data, the mean penalty in the ninth year of their violation was $5,597 and the median was $5,000. That is one of the lowest mean and median penalties. Thus, even those that are persistently noncompliant might not receive high penalties.

Thus, Table 24 and Table 25 show that there are some persistent repeat offenders. However, taking the size of the penalty as an indication of the severity of the offense, repeat offenders might not be the worst actors.[56] Although the median penalties for the repeat offenders tend to be higher, the means are lower in many instances.

These tables also imply at least mixed evidence regarding escalating penalties. While these tables look at the trends in penalties over time, they do not account for other

[54] The largest value is $3,826,000 and the second largest value is $458,278.
[55] However, there are, occasionally, very large penalties among these bad actors. For instance, the maximum penalty for the eighth year of noncompliance was $456,500.
[56] Ideally, I would have some information on the severity of violations and would use that information to determine which facilities are the worst actors. Unfortunately, my data do not contain information on the severity of the violations. While certain types of violations, high-priority violations, are more severe than other types of violation, my data do not have information on the type of violation. Moreover, many California air districts report only high-priority violations to the EPA database. Thus, to the best of my knowledge, the only proxy for the severity of the violation is the size of the penalty.

factors and the analysis is somewhat limited. Unfortunately, I have no information on the severity of the violation. Assuming that larger penalties are assessed for more serious violations, it seems likely that some firms with shorter noncompliance spells are, on average, committing more serious violations and thus receiving larger penalties.

Summary of Empirical Results

These statistics suggest certain patterns. First, a vast majority of the facilities are very compliant; 71.3% of the facilities spent one or fewer years in violation over the nine-year period (Table 21). Second, there seems to be no clear pattern of escalating penalties. For facilities that spend two consecutive years in violation, there is no evidence of increasing penalties (Table 22). Over longer noncompliance spells, there is some evidence of escalating penalties: the median per-year penalty increases as the noncompliance spell progresses (Table 24). However, this escalation effect seems to be dominated by the severity of the violation as the mean penalty can be lower in later years of the noncompliance spells. If there are escalating penalties, it is not the main factor in consideration; the regulator does not assess huge penalties for the mere fact of a previous violation.

Third, turning to each facility's longest spell of noncompliance, 83.3% had noncompliance spells of one year or less. Thus, based on the definition of repeat offenders being those that had at least two consecutive years of noncompliance, only 16.7% of the facilities were, at some point in the nine-year period, repeat offenders (Table 23). Finally, at least some facilities with longer noncompliance spells are, on average, assessed smaller penalties than those with shorter noncompliance spells (Table 24). This implies that facilities with longer spells of noncompliance are committing less serious violations. Thus, even the persistently noncompliant facilities might not be the worst actors.

Conclusion and Policy Implications

Law and economics theory has had some success in explaining the prevalence of escalating penalties. Some models rely on the concept of illicit gains, that some or all of a facility's benefit from a violation should not be counted in social welfare, to justify escalating penalties as optimal. While that is a somewhat unconventional assumption to economics, judging by the EPA's penalty policies, I believe it is more realistic. Additionally, some models assume some types of violators benefit more than other types, and escalating penalties are required to deter those that benefit more from violation. When applied to environmental regulation, this seems likely as different facilities have different abatement costs; those that have high abatement costs can be thought of as the type of violator that stands to benefit the most from violating. Thus, models that explain escalating penalties based on illicit gains and different types of violators are the most realistic representation of current policy.

Nonetheless, these models can be further improved by accounting for the social norm of law compliance and fairness. The social norm of law compliance can be introduced as part of the illicit gain, and is thus included in many existing models. Furthermore, social norms may change depending on the regulator's actions; escalating penalties might be necessary to reinforce the social norm of law compliance. For example, if regulators do not impose escalating penalties, penalties might start to seem like the price of a violation that can be purchased from the regulator, thus weakening the norm of law compliance. In theoretical models, illicit gains from the norm of law compliance might be a function of how steeply the penalty escalates for repeat offenders.

Additionally, regulators are concerned with fairness to the regulated entity and the affected community. Specifically, penalties might escalate because a sense of fairness limits the penalty for a first-time offender but not for a repeat offender. Regulators might also escalate penalties because fairness dictates that the same community situated around the repeat violators should not have to bear the cumulative cost of the facility's repeated violation. Some environmental damage is inevitable, but regulators might believe that environmental damage should be spread throughout the population, instead of being concentrated on the community surrounding repeat violators. Thus, repeat violations cause more harm than first-time violations by offending this sense of fairness. This increased harm, in turn, can justify increasing penalties.

Empirically, there is existing evidence that shows that repeat offenders receive higher penalties, but the evidence is somewhat mixed on whether enforcement actions affect repeat violations. My empirical analysis shows that most facilities are compliant. Repeat violations do occur, but there is limited evidence that penalties escalate. Moreover, facilities with the longest spells of noncompliance have lower per-year average penalties than facilities with shorter spells of noncompliance. This indicates that facilities with longer spells of noncompliance are committing less serious violations than facilities with shorter spells of noncompliance. These repeat offenders are not necessarily the worst actors as their violations are less severe.

This has two implications. First, it shows that repeat violations can be complex. Persistent repeat violators are not necessarily the worst actors as they might be committing less severe violations. Any future research into repeat violations should examine the severity of violations as well. Second, the current repeat offender policy seems to be working well, even if my data show that penalties do not escalate. Very few of the facilities are persistent violators, and the persistent violators seem to commit less severe violations on average.

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Table 20. Means for Inspections or Enforcement Actions and Penalty Amount per Facility-Year

	Mean	(Std. dev.)	Proportion positive
Inspections	1.785	(3.333)	0.634
Enforcement actions	0.603	(2.972)	0.171
Penalty (/$1,000)	6.993	(126.288)	0.127
Given penalty > 0			
Penalty (/$1,000)	54.910	(350.251)	

Source: Author’s calculations and EPA Air Facility System, 2002-2010.

Table 21. Statistics for the Total Number of Years a Facility Was in Violation

Years in violation	Number	Percentage
0	699	51.6
1	267	19.7
2	167	12.3
3	96	7.1
4	49	3.6
5	34	2.5
6	18	1.3
7	13	1.0
8	9	0.7
9	3	0.2
Total	1,355	100.0

Source: Author's calculations and EPA Air Facility System, 2002-2010.

Table 22. Fixed Effects Regression of the Impact of the Size of Previous-Year Penalties on the Size of Current-Year Penalties for Facilities with Previous- and Current-Year Violations

	(1)
Previous-year penalty (log)	-0.210**
	(0.079)
Previous-year inspections	-0.015
	(0.009)
Previous-year enforcement actions	0.036**
	(0.011)
Unemployment rate	0.276**
	(0.089)
Income (/$1000)	-0.087
	(0.111)
Percent white	-0.355
	(0.221)
PM_{10} nonattainment	0.358
	(0.408)
$PM_{2.5}$ nonattainment	-0.535
	(0.602)
Carbon monoxide nonattainment	0.471
	(0.563)
Observations	486
Facilities	226
Adjusted R-squared	0.123

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; year dummy variables and fixed effects included but not shown.

Table 23. Summary of Length of Longest Noncompliance Spell

Length of longest noncompliance spell	Number	Percentage
0	699	51.6
1	430	31.7
2	133	9.8
3	51	3.8
4	17	1.3
5	5	0.4
6	5	0.4
7	3	0.2
8	9	0.7
9	3	0.2
Total	1,355	100.0

Source: Author’s calculation and EPA Air Facility System, 2002-2010.

Table 24. Mean, (Standard Deviation), and [Median] of Penalty/1,000 During a Facility's Longest Completed Noncompliance Spell

	Length of Noncompliance Spell in Years				
	1	2	3	≥ 4	All
Year 1 penalty (/$1,000)	54.058 (439.454) [2.500]	61.307 (303.926) [3.779]	44.553 (141.898) [6.250]	28.761 (52.685) [6.635]	53.429 (388.182) [3.000]
Year 2 penalty (/$1,000)		67.640 (603.176) [3.000]	40.137 (89.322) [6.750]	29.171 (44.959) [8.040]	54.906 (469.421) [4.500]
Year 3 Penalty (/$1,000)			62.699 (161.210) [6.200]	75.840 (291.541) [6.220]	67.990 (221.317) [6.220]
Year 4 penalty (/$1,000)				22.980 (54.760) [6.500]	22.980 (54.760) [6.500]
Year 5 penalty (/$1,000)				185.003 (655.194) [6.000]	185.003 (655.194) [6.000]
Year 6 penalty (/$1,000)				31.185 (42.541) [15.000]	31.185 (42.541) [15.000]
Year 7 penalty (/$1,000)				42.676 (35.352) [23.750]	42.676 (35.352) [23.750]
Year 8 penalty (/$1,000)				61.855 (41.107) [77.900]	61.855 (41.107) [77.900]
Total penalty (/$1,000)	54.058 (439.454) [2.500]	128.947 (671.928) [8.525]	147.388 (277.084) [21.300]	283.692 (638.881) [57.250]	87.280 (496.697) [4.500]
Per-Year penalty (/$1,000)	54.058 (439.454) [2.500]	64.474 (335.964) [4.263]	49.129 (92.361) [7.100]	41.029 (87.327) [12.756]	55.013 (392.360) [3.090]
Observations	412	116	46	31	605

Source: Author's calculation and EPA Air Facility System, 2002-2010.

Table 25. Mean, (Standard Deviation), and [Median] of Penalty/1,000 During a Facility's Longest Incomplete Noncompliance Spell

	Length of Noncompliance Spell in Years				
	1	2	3	≥ 4	All
Year 1 penalty (/$1,000)	241.076 (901.073) [2.000]	8.550 (12.852) [2.700]	11.465 (16.251) [5.075]	104.705 (249.564) [16.500]	111.643 (547.034) [2.970]
Year 2 penalty (/$1,000)		4.998 (6.893) [2.250]	4.790 (6.626) [2.400]	24.276 (42.855) [10.800]	11.392 (26.244) [3.500]
Year 3 Penalty (/$1,000)			2.900 (2.488) [2.000]	286.099 (811.373) [15.000]	197.599 (676.214) [7.300]
Year 4 penalty (/$1,000)				41.267 (62.449) [15.000]	41.267 (62.449) [15.000]
Year 5 penalty (/$1,000)				119.590 (267.661) [22.910]	119.590 (267.661) [22.910]
Year 6 penalty (/$1,000)				15.849 (12.783) [10.200]	15.849 (12.783) [10.200]
Year 7 penalty (/$1,000)				21.371 (23.275) [16.325]	21.371 (23.275) [16.325]
Year 8 penalty (/$1,000)				110.155 (194.422) [34.900]	110.155 (194.422) [34.900]
Year 9 penalty (/$1,000)				5.597 (1.488) [5.000]	5.597 (1.488) [5.000]
Total penalty (/$1,000)	241.076 (901.073) [2.000]	13.548 (14.197) [7.000]	19.155 (22.484) [11.800]	639.759 (1172.963) [140.000]	229.467 (780.185) [8.000]
Per-Year penalty (/$1,000)	241.076 (901.073) [2.000]	6.774 (7.098) [3.500]	6.385 (7.495) [3.933]	102.474 (197.489) [22.574]	110.072 (542.929) [4.000]
Observations	18	17	5	11	51

Source: Author's calculation and EPA Air Facility System, 2002-2010.

APPENDIX A

ADDITIONAL ROBUSTNESS TESTS FOR CHAPTER I

In this appendix, I further examine the robustness of my Chapter I empirical results. First, I examine whether excluding facilities that participate in the Regional Clean Air Incentives Market (RECLAIM) program affects my results. The RECLAIM program is a cap-and-trade program for the largest emitters of NO_x and sulfur oxides in the South Coast Air Quality Management District. Table 26 and Table 27 show the regression results when I exclude facilities that participate in the RECLAIM program. These tables show that the results in Table 5 and Table 6 are robust to dropping RECLAIM facilities; the coefficients maintain their significance and are larger in magnitude. This is not surprising as RECLAIM facilities' NO_x emissions are limited by their emissions permits, and less so by regulatory action.

Next, I examine whether my results in Table 5 and Table 6 are robust to omitting various control variables. Table 28 shows the impact of regulatory actions on major sources. The coefficients are very similar in significance and magnitude to regression (2) of Table 5 and regression (2) of Table 6, regardless of which control variables are omitted. Table 29 displays analogous regressions for the balanced panel, and Table 30 displays analogous regressions for manufacturing facilities. The coefficients are robust to omitting various control variables.

Next, I examine whether my results are affected by using first differences instead of fixed effects. First differences regression is another method of accounting for time-invariant facility characteristics. The first differences regression method regresses the change in the dependent variable on the changes in the explanatory variable. The regression equation is

$$\Delta y_{it} = \Delta RegulatoryAction'_{i,t-1}\beta + \Delta X'_{it}\gamma + Year_t + \Delta\varepsilon_{it}, \tag{21}$$

where $\Delta x_{it} = x_{it} - x_{i,t-1}$. The results are shown in Table 31 and Table 32. Unfortunately, the coefficients are not significant. Perhaps this is due to the reduced sample size: using first differences instead of fixed effects reduces the sample size because I lose one year of data.

Additionally, I also run a differences-in-differences regression in order to account for possible trends in emissions and regulatory actions. The regression equation is

$$\Delta^2 y_{it} = \Delta^2 RegulatoryAction'_{i,t-1}\beta + \Delta^2 X'_{it}\gamma + Year_t + \Delta^2\varepsilon_{it}, \tag{22}$$

where $\Delta^2 x_{it} = \Delta x_{it} - \Delta x_{i,t-1}$. The results are shown in Table 33 and Table 34. Much like the first-differences regressions, the coefficients for regulatory action are not significant, again perhaps due to a smaller sample size.

Lastly, I explore instrumental variables, another method of accounting for reverse causality. Shimshack and Ward (2005) used inspection rate at other facilities in the jurisdiction as an instrument for inspections in the current time period. They argued that inspections at other facilities in the same jurisdiction were uncorrelated with the individual facility's compliance or emissions, but were correlated with the probability of inspection, thus creating a valid instrument. For my data, the same logic as Shimshack and Ward's applies, but the link might be attenuated. For example, inspection frequencies are relatively fixed, once every two years for major facilities, so the inspection rate at other facilities in the same jurisdiction might not reflect the probability of inspection very accurately. Additionally, while the enforcement rate at other facilities might be correlated with a regulator's strictness, enforcements actions are probably determined by whether there was a violation more than they are determined by the regulator's strictness.

I employ a similar set of instruments.[57] For the number of regulatory actions directed at a facility in the current year, I use the average number of the same regulatory action directed at other facilities in the same air district in the current and previous years. For the dummy variable of whether the facility was subjected to a specific regulatory action in the current year, I use the proportion of other facilities in the same air district that were subject to the same regulatory action in the current and previous years. As instrumenting for all three regulatory action variables at the same time creates very weak instruments, I instead instrument for only one variable at the time. The regression equation is

$$y_{it} = \delta InstrAction_{it} + OtherAction'_{i,t-1}\beta + X'_{it}\gamma + a_i + Year_t + \varepsilon_{it}, \qquad (23)$$

where $InstrAction_{it}$ is the instrumented regulatory action for facility i in period t, and $OtherAction_{i,t-1}$ is a vector of the other regulatory variables, which are not instrumented, for facility i in the previous period $t-1$. Thus, if I am instrumenting for inspections, $InstrAction_{it}$ will consist of instrumented current-year inspections and $OtherAction_{i,t-1}$ will consist of previous-year enforcement actions and penalties. I run this regression equation three times, instrumenting for each regulatory action (while using the lags for the other regulatory actions).

Table 35 shows the coefficients and standard errors for the instrumented activity, as well as the p values for the endogeneity test, the F-statistics for instrument strength,

[57] I also looked into the CARB budget as a possible instrument. However, while there is information on the general budget for CARB's stationary source enforcement, information on the budget for each district was not publicly available.

and the p values for the overidentification test. When I run a fixed effects regression equation (23) instrumenting for the presence of current-year inspections and using lagged regulatory variables for the presence of enforcement actions and penalties, the results are shown in the rows for inspections in column (1) of Table 35. Thus, the coefficient for current-year inspections is -4.07 and the cluster-robust standard error is 7.37, which indicates that inspections do not have a significant impact on emissions. I do not show the coefficients for the other variables as they are not very different from previous regressions shown. I also test whether the endogenous regressor, current-year inspections in this case, can be treated as exogenous; the p value is 0.73, and I cannot reject the null hypothesis that current-year inspections can be treated as exogenous. Next, I test the strength of the instruments (the current- and previous-year proportion of other facilities in the same air district that were inspected). The instrument is strong, with an F-statistic of 97.20, above the rule-of-thumb threshold of 10.[58] I also perform the Sargan-Hansen test of overidentifying restrictions, which tests whether the instruments are exogenous. The p value is 0.52, and I cannot reject the hypothesis that the instruments are valid. Column (2) presents the coefficient and related statistics when the explanatory variable is the number of inspections at the facility.

None of the coefficients for the instrumented variables are statistically significant. Interestingly, the test for endogeneity never rejects the hypothesis that the regulatory variable is exogenous, perhaps implying that endogeneity is not a concern for the sample. This might be the case if inspections and enforcement actions are determined in advance with little room for discretion.

[58] For errors that are not independently and identically distributed, Baum, Schaffer, and Stillman (2007) recommend comparing the Kleibergen-Paap rk Wald F-statistic against the rule-of-thumb threshold of ten.

Table 26. Fixed Effects Regressions of the Impact of the Presence of Regulatory Actions in the Previous Year on NO_x Emissions, without RECLAIM Facilities

Sample[a]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)
Presence of any inspections in the previous year	0.161	2.248	0.167	1.982
	(1.998)	(3.493)	(2.354)	(3.942)
Presence of any enforcement actions in the previous year	2.837	3.647	3.753	6.972
	(3.227)	(3.927)	(3.558)	(5.603)
Presence of a penalty in the previous year	-7.500^{+}	-9.079^{+}	-8.827*	-14.500*
	(4.044)	(4.783)	(4.481)	(6.846)
PM_{10} nonattainment	-3.661	1.861	-1.337	-11.060
	(8.522)	(12.889)	(10.041)	(19.662)
$PM_{2.5}$ nonattainment	-2.200	-3.039	-2.341	3.508
	(4.725)	(6.515)	(5.162)	(6.473)
Ozone nonattainment	7.911^{+}	10.839^{+}	8.454^{+}	18.755*
	(4.425)	(5.859)	(4.691)	(7.687)
Carbon monoxide nonattainment	-4.728	-7.404	-3.544	-4.403
	(5.971)	(7.842)	(6.858)	(10.825)
Unemployment rate	-2.531	-2.829	-2.893	-6.852
	(4.052)	(5.082)	(4.355)	(8.973)
Income (/$1000)	0.072	-0.027	-0.022	1.532
	(1.158)	(1.702)	(1.265)	(3.087)
Percent white	-6.696^{+}	-8.648	-6.610	-8.375
	(4.028)	(5.949)	(4.392)	(7.683)
Observations	3,931	2,757	3,331	1,805
Facilities	722	494	556	334
Adjusted R-squared	0.960	0.959	0.961	0.965

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; year dummy variables included but not shown.

[a] Column titles (“all,” “major,” “balanced,” and “manufacturing”) refer to the sample used in the regression.

Table 27. Fixed Effects Regressions of the Impact of the Number of Regulatory Actions in the Previous Year on NO_x Emissions, without RECLAIM Facilities

Sample[a]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)
Number of inspections in	-1.127	-1.120	-0.855	0.073
the previous year	(0.787)	(0.930)	(1.058)	(1.170)
Number of enforcement	1.086	1.129	0.429	1.406
actions in the previous year	(1.102)	(1.117)	(1.268)	(1.564)
Amount of penalty (log) in	-0.911^{+}	-1.044^{+}	-0.808	-1.391^{+}
the previous year	(0.479)	(0.554)	(0.558)	(0.741)
PM_{10} nonattainment	-3.608	1.596	-0.994	-10.371
	(8.366)	(12.778)	(9.913)	(19.436)
$PM_{2.5}$ nonattainment	-2.110	-2.566	-2.369	3.790
	(4.642)	(6.291)	(5.090)	(6.251)
Ozone nonattainment	7.742^{+}	11.340^{+}	8.036	19.872*
	(4.604)	(6.061)	(4.936)	(7.708)
Carbon monoxide	-3.771	-6.228	-2.921	-4.433
nonattainment	(5.793)	(7.490)	(6.663)	(10.288)
Unemployment rate	-2.670	-3.237	-2.995	-6.432
	(3.954)	(5.002)	(4.254)	(8.670)
Income (/$1000)	-0.148	-0.365	-0.152	1.569
	(1.187)	(1.780)	(1.307)	(3.213)
Percent white	-6.103	-7.906	-6.238	-7.918
	(4.039)	(5.942)	(4.457)	(7.745)
Observations	3,931	2,757	3,331	1,805
Facilities	722	494	556	334
Adjusted R-squared	0.960	0.959	0.961	0.965

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; year dummy variables included but not shown.

[a] Column titles (“all,” “major,” “balanced,” and “manufacturing”) refer to the sample used in the regression.

Table 28. Fixed Effects Regression of the Impact of Regulatory Actions in the Previous Year on NO_x Emissions at Major Sources, Omitting Control Variables

Regulatory actions[a]	Presence of Regulatory Actions			Number of Regulatory Action		
	(1)	(2)	(3)	(4)	(5)	(6)
Inspections in the previous year	1.158	0.829	1.114	-1.162	-1.086	-1.176
	(2.620)	(2.898)	(2.593)	(0.901)	(0.854)	(0.872)
Enforcement actions in the previous year	2.378	2.728	2.727	0.982	1.013	1.026
	(2.957)	(3.058)	(3.095)	(1.098)	(1.095)	(1.096)
Penalty in the previous year (log)	-6.407^{+}	-6.273^{+}	-6.442^{+}	-0.765^{+}	-0.742^{+}	-0.753^{+}
	(3.625)	(3.740)	(3.668)	(0.432)	(0.436)	(0.434)
NAAQS nonattainment (PM_{10}, $PM_{2.5}$, ozone, and CO)	✗	✓	✗	✗	✓	✗
Demographic variables (income, unemployment, and percent white)	✓	✗	✗	✓	✗	✗

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; NAAQS nonattainment and demographic variables are included in some specifications but not shown; year dummy variables included but not shown.

[a] Headings refer to the type of dependent variable used in the regression.

Table 29. Fixed Effects Regression of the Impact of Regulatory Actions in the Previous Year on NO_x Emissions Using a Balanced Panel, Omitting Control Variables

Regulatory actions[a]	Presence of Regulatory Actions			Number of Regulatory Action		
	(1)	(2)	(3)	(4)	(5)	(6)
Inspections in the previous year	-0.713	-0.923	-0.812	-0.900	-0.900	-0.921
	(1.891)	(1.998)	(1.793)	(1.005)	(0.976)	(0.989)
Enforcement actions in the previous year	2.963	3.137	3.157	0.320	0.357	0.367
	(2.801)	(2.844)	(2.889)	(1.234)	(1.228)	(1.229)
Penalty in the previous year (log)	-6.743^{+}	-6.671^{+}	-6.713^{+}	-0.588	-0.582	-0.584
	(3.476)	(3.530)	(3.490)	(0.448)	(0.450)	(0.448)
NAAQS nonattainment (PM_{10}, $PM_{2.5}$, ozone, and CO)	✗	✓	✗	✗	✓	✗
Demographic variables (income, unemployment, and percent white)	✓	✗	✗	✓	✗	✗

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; NAAQS nonattainment and demographic variables are included in some specifications but not shown; year dummy variables included but not shown.

[a] Headings refer to the type of dependent variable used in the regression.

Table 30. Fixed Effects Regression of the Impact of Regulatory Actions in the Previous Year on NO_x Emissions at Manufacturing Facilities, Omitting Control Variables

Regulatory actions[a]	Presence of Regulatory Actions			Number of Regulatory Action		
	(1)	(2)	(3)	(4)	(5)	(6)
Inspections in the previous year	0.785	0.752	0.796	-0.041	-0.051	-0.109
	(3.150)	(3.489)	(3.023)	(1.153)	(1.031)	(1.096)
Enforcement actions in the previous year	5.220	5.526	5.534	1.194	1.288	1.271
	(4.251)	(4.411)	(4.415)	(1.541)	(1.526)	(1.533)
Penalty in the previous year (log)	-10.920*	-10.906*	-11.058*	-1.065^{+}	-1.060^{+}	-1.072^{+}
	(5.230)	(5.308)	(5.236)	(0.572)	(0.568)	(0.572)
NAAQS nonattainment (PM_{10}, $PM_{2.5}$, ozone, and CO)	✗	✓	✗	✗	✓	✗
Demographic variables (income, unemployment, and percent white)	✓	✗	✗	✓	✗	✗

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; NAAQS nonattainment and demographic variables are included in some specifications but not shown; year dummy variables included but not shown.

[a] Headings refer to the type of dependent variable used in the regression.

Table 31. First Differences Regressions of the Impact of the Presence of Regulatory Actions in the Previous Year on NO_x Emissions

Sample[a]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)
Presence of any inspections in the previous year	0.595	1.418	1.414	2.459
	(1.931)	(2.972)	(2.305)	(3.753)
Presence of any enforcement actions in the previous year	0.028	0.123	1.297	1.474
	(2.849)	(3.292)	(3.159)	(5.006)
Presence of a penalty in the previous year	-2.741	-3.321	-3.350	-5.746
	(3.093)	(3.519)	(3.004)	(5.364)
PM_{10} nonattainment	0.069	6.772	1.493	-0.474
	(7.266)	(11.082)	(8.364)	(15.163)
$PM_{2.5}$ nonattainment	-2.737	-2.395	-4.251	-1.172
	(5.284)	(6.911)	(5.839)	(6.727)
Ozone nonattainment	5.462	7.777	-0.423	10.321^{+}
	(3.665)	(5.709)	(3.500)	(5.519)
Carbon monoxide nonattainment	-7.243*	-10.295*	-6.960^{+}	-13.902*
	(3.191)	(4.405)	(3.674)	(6.245)
Unemployment rate	-4.467	-5.285	-4.856	-10.604
	(3.868)	(4.670)	(3.922)	(7.589)
Income	-0.485	-0.581	0.346	-1.022
	(1.371)	(1.952)	(1.308)	(3.476)
Percent white	-5.040	-6.064	6.621	-5.716
	(4.501)	(6.483)	(4.580)	(9.043)
Observations	3,810	2,825	4,017	1,872
Facilities	839	604	672	411

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; year dummy variables included but not shown.

[a] Column titles ("all," "major," "balanced," and "manufacturing") refer to the sample used in the regression.

Table 32. First Differences Regressions of the Impact of the Number of Regulatory Actions in the Previous Year on NO_x Emissions

Sample[a]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)
Number of inspections in the previous year	-0.140	-0.132	-0.127	1.663
	(0.877)	(1.025)	(1.189)	(1.651)
Number of enforcement actions in the previous year	1.342	1.397	0.282	1.727
	(1.257)	(1.277)	(1.222)	(1.708)
Amount of penalty (log) in the previous year	-0.538	-0.598	-0.306	-0.792
	(0.362)	(0.399)	(0.342)	(0.528)
PM_{10} nonattainment	0.427	7.233	2.233	0.297
	(7.405)	(11.365)	(9.010)	(15.472)
$PM_{2.5}$ nonattainment	-2.871	-2.528	-3.053	-0.867
	(5.120)	(6.773)	(5.549)	(6.232)
Ozone nonattainment	6.013$^+$	8.913	6.314$^+$	10.413*
	(3.561)	(5.598)	(3.799)	(4.922)
Carbon monoxide nonattainment	-7.404*	-10.698*	-7.750*	-14.564*
	(3.284)	(4.619)	(3.823)	(6.331)
Unemployment rate	-4.217	-4.981	-5.004	-9.222
	(3.788)	(4.590)	(4.053)	(7.362)
Income	-0.599	-0.768	-0.551	-0.912
	(1.353)	(1.967)	(1.502)	(3.414)
Percent white	-4.487	-5.245	-5.387	-4.822
	(4.818)	(7.015)	(5.396)	(9.737)
Observations	3,810	2,825	3,360	1,872
Facilities	839	604	672	411

** $p < 0.01$, * $p < 0.05$, $^+$ $p < 0.1$; cluster-robust standard errors in parentheses; year dummy variables included but not shown.

[a] Column titles (“all,” “major,” “balanced,” and “manufacturing”) refer to the sample used in the regression.

Table 33. Differences-in-Differences Regressions of the Impact of the Presence of Regulatory Actions in the Previous Year on NO_x Emissions

Sample[a]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)
Presence of any inspections in the previous year	-0.169	0.423	0.222	1.876
	(3.266)	(4.977)	(3.544)	(6.793)
Presence of any enforcement actions in the previous year	-0.339	0.348	0.743	0.108
	(3.445)	(3.945)	(4.169)	(5.838)
Presence of a penalty in the previous year	0.331	-0.620	-1.830	-0.400
	(3.424)	(3.872)	(4.101)	(5.983)
PM_{10} nonattainment	10.166	16.923	8.290	31.283
	(8.353)	(12.937)	(8.603)	(19.563)
$PM_{2.5}$ nonattainment	-0.440	0.774	-3.944	4.282
	(7.369)	(9.693)	(7.514)	(8.141)
Ozone nonattainment	-13.229^{+}	-16.367^{+}	-14.224^{+}	-29.434*
	(7.235)	(9.166)	(7.838)	(14.948)
Carbon monoxide nonattainment	5.551	6.272	1.091	15.803
	(8.571)	(12.793)	(3.907)	(14.596)
Unemployment rate	1.066	1.421	-5.214	4.747
	(4.887)	(6.590)	(4.445)	(8.691)
Income	-1.783	-2.890	1.307	-5.238
	(3.723)	(5.638)	(2.831)	(8.915)
Percent white	18.688	26.099	-2.656	72.496
	(29.249)	(37.649)	(33.118)	(83.018)
Observations	2,968	2,218	3,345	1,459
Facilities	812	597	672	393

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; year dummy variables included but not shown.
[a] Column titles (“all,” “major,” “balanced,” and “manufacturing”) refer to the sample used in the regression.

Table 34. Differences-in-Differences Regressions of the Impact of the Number of Regulatory Actions in the Previous Year on NO_x Emissions

Sample[a]	All (1)	Major (2)	Balanced (3)	Manufacturing (4)
Number of inspections in the previous year	-0.785	-0.896	-0.937	0.586
	(0.799)	(0.904)	(1.038)	(1.308)
Number of enforcement actions in the previous year	1.655	1.731	0.556	2.239
	(1.421)	(1.438)	(1.445)	(1.913)
Amount of penalty (log) in the previous year	-0.252	-0.280	-0.003	-0.342
	(0.463)	(0.509)	(0.462)	(0.705)
PM_{10} nonattainment	12.242	20.698	14.629	31.955
	(9.024)	(13.289)	(10.161)	(21.709)
$PM_{2.5}$ nonattainment	-0.633	0.454	-0.032	3.957
	(7.288)	(9.838)	(7.745)	(8.313)
Ozone nonattainment	-14.498*	-18.361*	-15.346*	-31.051*
	(7.040)	(8.836)	(7.501)	(13.776)
Carbon monoxide nonattainment	6.923	8.559	6.682	17.999
	(7.797)	(11.910)	(8.220)	(11.683)
Unemployment rate	1.569	2.248	1.194	7.462
	(4.770)	(6.330)	(5.364)	(8.219)
Income	-1.928	-3.255	-2.181	-5.193
	(3.655)	(5.556)	(4.065)	(8.444)
Percent white	16.166	22.972	15.240	55.048
	(28.083)	(35.871)	(29.935)	(78.467)
Observations	2,968	2,218	2,688	1,459
Facilities	812	597	672	393

** $p < 0.01$, * $p < 0.05$, + $p < 0.1$; cluster-robust standard errors in parentheses; year dummy variables included but not shown.

[a] Column titles ("all," "major," "balanced," and "manufacturing") refer to the sample used in the regression.

Table 35. Coefficients of Instrumented Variables in Regressions of the Impact of Regulatory Actions on NO_x Emissions

Regulatory actions[a]	Presence (1)	Number (2)
Inspections		
Coefficient	-4.067	3.523
(Cluster-robust standard error)	(7.374)	(2.801)
Endogeneity test p value	0.725	0.166
Kleibergen-Paap rk Wald F-statistic	97.197	14.169
Sargan overidentification p value	0.521	0.920
Enforcement actions		
Coefficient	6.406	0.223
(Cluster robust standard error)	(39.001)	(2.492)
Endogeneity test p value	0.844	0.492
Kleibergen-Paap rk Wald F-statistic	24.953	15.522
Sargan overidentification p value	0.834	0.954
Penalties (log or dummy variable)		
Coefficient	-18.043	-4.247
(Cluster robust standard error)	(45.152)	(8.848)
Endogeneity test p value	0.711	0.383
Kleibergen-Paap rk Wald F-statistic	18.789	24.953
Sargan overidentification p value	0.970	0.239

Note: none of the coefficients are significant. Coefficients for other lagged regulatory variables and control variables are not shown.

[a] Column titles (“presence” and “number”) refer to the explanatory variables used in the regression.

APPENDIX B

ADDITIONAL ROBUSTNESS TESTS FOR CHAPTER II

In this appendix, I perform several robustness tests for Chapter II. I first examine the impact of the maximum penalty around an air quality monitor. As previously discussed, the average penalty is $4,682, which is probably a small amount compared to a facility's operating costs. Thus, it seems unlikely that this average penalty is much of a deterrent to facilities. It is possible that firms are not concerned with the average penalty; they might be more worried about being assessed a large penalty, and the average penalty is correlated with that. The facilities might be deterred by the total penalty or maximum penalty assessed, rather than the average penalty. Thus, for each air quality monitor, I find the maximum penalty assessed at surrounding facilities during that year. The mean of the maximum penalty is $236,291 and the standard deviation is $939,721.

In Table 36, I examine the impact that the maximum penalty has on air quality. In this table, the inspection and enforcement action variables are totals or averages, and the penalty is the maximum penalty at all the facilities around the air quality monitor. For example, in regression (1), a one-unit increase in the total number of inspections reduces ozone concentrations by 0.007 ppb, but the coefficient is statistically insignificant. The coefficient of the natural logarithm of the maximum penalty in the previous year is -0.162. This means that increasing the maximum penalty by 1% decreases ambient ozone concentrations by approximately 0.002 ppb. Thus, increasing the maximum penalty from $1 to the average of $236,291 reduces ambient ozone concentrations by 2.01 ppb. Increasing the maximum penalty from the 25th percentile ($0) to the 75th percentile

($71,000) of the penalty distribution reduces ambient ozone concentration by 1.810 ppb. Regression (2) shows a similar result. Thus, it is possible that facilities are deterred by the largest penalties assessed around the air quality monitor.

Next, I examine whether the results are robust to using different radii around the monitor to compute regulatory actions and explore possible instrumental variables. In the chapter, I examine monitoring and enforcement actions within a twenty-mile radius of the air quality monitor. To test whether the results are sensitive to the chosen radius, instead of counting all regulatory actions within 20 miles, I examine all regulatory actions within 10, 15, and 30 miles. In Table 37, I present a summary of the coefficients of regulatory actions when I use different radii.

The effect is fairly robust, and holds for the fifteen- and thirty-mile radius specifications. As shown in regressions (1) and (4), the significant effect of penalties does not hold in the ten-mile radius cases. This is likely due to a smaller sample size. Reducing the radius reduces the number of facilities surrounding each monitor and, because I drop all monitors that have no facilities within the radius, it also reduces the number of air quality monitors in the data. Nonetheless, using fifteen- and thirty-mile radii produces similar results as the twenty-mile radius regressions and the coefficient of penalty variable is negative and statistically significant.

In the next four tables, I show the summary statistics and more detailed regression results for the different radii. In Table 38, I present summary statistics. It is worth noting that the average number of inspections and enforcement actions do not change much as radius increases. For instance, the average number of inspections within 10 miles is 1.89, the average within 15 miles is 1.79, and the average number of inspections within 30 miles is 1.70. If regulators focused on facilities near the monitors, then the average numbers should decrease as radius increases. On the other hand, the average penalty increases as radius increases, perhaps due to a few extraordinarily large penalties.

Table 39 to Table 41 show the full fixed effects regressions for the different radii. The with the exception of regulatory variables, the coefficients are fairly similar regardless of radius. The magnitudes of the penalty coefficient get larger and more statistically significant as the radius increases: for instance, the coefficient of penalties is -0.114, significant at 5%, for the fifteen-mile radius; it is -0.237, significant at 1%, for the twenty-mile radius. Perhaps this is because regulatory actions are effective over a long distance, and a large penalty can improve air quality in places up to 30 miles away.

Additionally, I explore using an instrumental variables approach. Regulatory actions are not random. It is likely that regulators focus their efforts on areas with poorer air quality. Thus, instrumental variables are appropriate. The instrumental variable has to be correlated with regulatory actions and otherwise uncorrelated with ozone concentrations. It is difficult to come up with viable instruments. Facilities in nonattainment areas likely face more regulatory actions. Thus, I use previous-year nonattainment for particulate matter and carbon monoxide as instruments. If current-year ozone concentrations are otherwise unrelated to previous-year nonattainment for other pollutants, then the exclusion restriction is fulfilled and this is a valid instrument.

I instrument for current-year regulatory actions using previous-year nonattainment for carbon monoxide, PM_{10}, and $PM_{2.5}$. I instrument for the current year regulatory actions one at the time, using the lagged regulatory variables for the other actions. The regression equation is:

$$y_{it} = \delta InstrAction_{it} + OtherAction'_{i,t-1}\beta + X'_{it}\gamma + a_i + Year_t + \varepsilon_{it}, \qquad (24)$$

where $InstrAction_{it}$ is the instrumented regulatory action for monitor i in period t, and $OtherAction_{i,t-1}$ is a vector of the other regulatory variables, which are not instrumented, for monitor i in the previous period $t-1$.

The results are presented in Table 42. When I run the fixed effects regression equation (24) instrumenting for the total number of inspections and using lagged regulatory variables for total enforcement actions and the size of penalties, the results are shown in the rows for inspections in column (1). The coefficient for current-year total inspections is -0.034, and the cluster-robust standard error is 0.121, which indicates that inspections do not have a significant impact on ozone concentrations. I also test whether the endogenous variable, current-year inspections, can be treated as exogenous. The p value is 0.661, and I cannot reject the null hypothesis that current-year inspections can be treated as exogenous. Next, I test the strength of the instruments (previous-year nonattainment for carbon monoxide, PM_{10}, and $PM_{2.5}$). The instrument is weak, with an F-statistic of 6.393, which is lower than the rule-of-thumb threshold of ten.

I also perform the Sargan-Hansen test of overidentifying restrictions, which tests whether the instruments are exogenous. The p value is 0.025, and I can reject the null hypothesis that the instruments are valid. This is likely because the same pollution sources that cause carbon monoxide and particulate matter nonattainment also cause ozone pollution. Unfortunately, overall, the instruments perform quite badly. The instruments are weak and cannot be considered to be exogenous. I have not been able to find a better instrument.

Table 36. Fixed Effects Regressions of the Impact of Regulatory Actions and Maximum Penalty in the Previous Year on Ozone Concentrations

Regulatory Actions[a]	Total (1)	Average (2)
Inspections in the previous year	-0.007	-0.230
	(0.005)	(0.176)
Enforcement actions in the previous year	-0.003	-0.301
	(0.005)	(0.366)
Maximum penalty in the previous year (log)	-0.162*	-0.152*
	(0.062)	(0.065)
Number of surrounding facilities	0.043*	0.033
	(0.021)	(0.021)
Carbon monoxide nonattainment	1.562	1.459
	(1.220)	(1.199)
PM_{10} nonattainment	-1.469	-1.518
	(1.376)	(1.385)
$PM_{2.5}$ nonattainment	-3.537**	-3.813**
	(0.969)	(0.946)
Percent white population of the county	-0.463	-0.458
	(0.463)	(0.466)
Mean income of the county (/$1000)	-0.278*	-0.266^{+}
	(0.140)	(0.138)
Unemployment rate of the county	-0.582^{+}	-0.596^{+}
	(0.327)	(0.327)
July mean temperature	0.230**	0.231**
	(0.086)	(0.087)
July total precipitation in inches	-0.127	-0.097
	(1.091)	(1.081)
July mean wind speed	0.054	0.050
	(0.147)	(0.147)
Observations	1,224	1,224
Number of monitors	167	167
Adjusted R-squared	0.283	0.283

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; missing weather and year dummy variables included but not shown.

[a] Column titles ("total" and "average") refer to the explanatory regulatory actions variable.

Table 37. Regression Coefficients of Regulatory Actions Using Different Air Quality Monitor Radii

Regulatory Actions[a]	Total			Average		
Radius	10 miles (1)	15 miles (2)	30 miles (3)	10 miles (4)	15 miles (5)	30 miles (6)
Inspections in the previous year	-0.014^{+}	-0.008	-0.006^{+}	-0.278*	-0.200	-0.365^{+}
	(0.008)	(0.006)	(0.003)	(0.122)	(0.160)	(0.200)
Enforcement actions in the previous year	-0.005	-0.002	0.001	0.038	-0.126	0.012
	(0.010)	(0.006)	(0.004)	(0.253)	(0.320)	(0.477)
Penalty in the previous year (log)	-0.074	-0.114*	-0.237**	-0.102	-0.147^{+}	-0.295**
	(0.057)	(0.056)	(0.060)	(0.073)	(0.076)	(0.085)
Observations	1,115	1,200	1,258	1,115	1,200	1,258
Number of monitors	152	164	172	152	164	172
Adjusted R-squared	0.274	0.276	0.284	0.275	0.277	0.283

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; number of surrounding facilities, NAAQS nonattainment, demographic variables, weather variables, and year dummy variables included but not shown.

[a] Column titles ("total" and "average") refer to the explanatory regulatory actions variable.

Table 38. Means and Standard Deviations of Explanatory Variables Using Different Radii

	Ten-mile radius		Fifteen-mile radius		Thirty-mile radius	
Variable	Mean	(Std. dev.)	Mean	(Std. dev.)	Mean	(Std. dev.)
Number of facilities	20.169	(28.833)	35.523	(49.471)	95.904	(123.244)
Inspections						
Total	30.319	(48.917)	51.533	(75.29)	133.673	(170.669)
Average	1.888	(2.143)	1.792	(1.544)	1.698	(1.239)
Enforcement actions						
Total	10.408	(26.248)	18.108	(38.595)	45.386	(75.053)
Average	0.446	(0.912)	0.446	(0.799)	0.428	(0.614)
Penalty						
Total	124.755	(648.595)	221.744	(909.691)	587.357	(1,501.666)
Average	4.393	(22.085)	4.494	(17.320)	5.523	(20.396)
NAAQS nonattainment						
CO	0.124	(0.329)	0.118	(0.323)	0.115	(0.319)
PM_{10}	0.438	(0.496)	0.419	(0.494)	0.406	(0.491)
$PM_{2.5}$	0.288	(0.453)	0.278	(0.448)	0.270	(0.444)
Unemployment rate	8.039	(3.976)	8.079	(3.949)	8.142	(3.938)
Income	37.778	(10.074)	37.639	(9.971)	37.357	(9.878)
Percent white	81.086	(9.161)	81.602	(9.138)	82.009	(9.135)
July weather						
July temperature	74.264	(9.659)	74.773	(9.826)	75.005	(10.098)
July precipitation	0.027	(0.159)	0.027	(0.159)	0.032	(0.225)
July wind speed	6.065	(2.365)	5.876	(2.161)	5.898	(2.149)
Observations	1,115		1,200		1,258	
Monitors	152		164		172	

Source: Author’s calculations, EPA, Bureau of Labor Statistics, Bureau of Economic Analysis, U.S. Census, and National Climatic Data Center.

Table 39. Fixed Effects Regressions of the Impact of Regulatory Actions in the Previous Year on Ozone Concentrations (Radius = 10 Miles)

Regulatory Actions[a]	Total (1)	Average (2)
Inspections in the previous year	-0.014[+]	-0.278*
	(0.008)	(0.122)
Enforcement actions in the previous year	-0.005	0.038
	(0.010)	(0.253)
Penalty in the previous year (log)	-0.074	-0.102
	(0.057)	(0.073)
Number of surrounding facilities	0.055	0.037
	(0.045)	(0.043)
Carbon monoxide nonattainment	1.888	1.826
	(1.235)	(1.210)
PM_{10} nonattainment	-1.642	-1.680
	(1.489)	(1.503)
$PM_{2.5}$ nonattainment	-3.215**	-3.449**
	(1.008)	(0.989)
Percent white population of the county	-0.156	-0.171
	(0.463)	(0.458)
Mean income of the county (/$1000)	-0.273[+]	-0.279+
	(0.144)	(0.143)
Unemployment rate of the county	-0.462	-0.446
	(0.336)	(0.335)
July mean temperature	0.023	0.024
	(0.085)	(0.085)
July total precipitation in inches	-0.510	-0.270
	(1.435)	(1.319)
July mean wind speed	0.070	0.076
	(0.206)	(0.205)
Observations	1,115	1,115
Number of monitors	152	152
Adjusted R-squared	0.274	0.275

** $p < 0.01$, * $p < 0.05$, [+] $p < 0.1$; cluster-robust standard errors in parentheses; missing weather and year dummy variables included but not shown.

[a] Column titles (“total” and “average”) refer to the explanatory regulatory actions variable.

Table 40. Fixed Effects Regressions of the Impact of Regulatory Actions in the Previous Year on Ozone Concentrations (Radius = 15 Miles)

Regulatory Actions[a]	Total (1)	Average (2)
Inspections in the previous year	-0.008	-0.200
	(0.006)	(0.160)
Enforcement actions in the previous year	-0.002	-0.126
	(0.006)	(0.320)
Penalty in the previous year (log)	-0.114*	-0.147^{+}
	(0.056)	(0.076)
Number of surrounding facilities	0.053*	0.043^{+}
	(0.026)	(0.026)
Carbon monoxide nonattainment	1.480	1.359
	(1.229)	(1.204)
PM_{10} nonattainment	-2.320^{+}	-2.390^{+}
	(1.367)	(1.375)
$PM_{2.5}$ nonattainment	-3.544**	-3.787**
	(0.976)	(0.959)
Percent white population of the county	-0.325	-0.324
	(0.458)	(0.458)
Mean income of the county (/$1000)	-0.259^{+}	-0.257^{+}
	(0.141)	(0.140)
Unemployment rate of the county	-0.612^{+}	-0.613^{+}
	(0.345)	(0.345)
July mean temperature	0.180*	0.181*
	(0.085)	(0.086)
July total precipitation in inches	-0.332	-0.318
	(1.132)	(1.126)
July mean wind speed	-0.002	-0.004
	(0.161)	(0.161)
Observations	1,200	1,200
Number of monitors	164	164
Adjusted R-squared	0.276	0.277

** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$; cluster-robust standard errors in parentheses; missing weather and year dummy variables included but not shown.

[a] Column titles (“total” and “average”) refer to the explanatory regulatory actions variable.

Table 41. Fixed Effects Regressions of the Impact of Regulatory Actions in the Previous Year on Ozone Concentrations (Radius = 30 Miles)

Regulatory Actions[a]	Total (1)	Average (3)
Inspections in the previous year	-0.006$^+$	-0.365$^+$
	(0.003)	(0.200)
Enforcement actions in the previous year	0.001	0.012
	(0.004)	(0.477)
Penalty in the previous year (log)	-0.237**	-0.295**
	(0.060)	(0.085)
Number of surrounding facilities	0.020	0.010
	(0.016)	(0.015)
Carbon monoxide nonattainment	1.426	1.361
	(1.233)	(1.217)
PM_{10} nonattainment	-1.382	-1.330
	(1.358)	(1.369)
$PM_{2.5}$ nonattainment	-3.486**	-3.804**
	(0.964)	(0.940)
Percent white population of the county	-0.520	-0.550
	(0.463)	(0.461)
Mean income of the county (/$1000)	-0.264$^+$	-0.272*
	(0.138)	(0.137)
Unemployment rate of the county	-0.506	-0.519
	(0.317)	(0.314)
July mean temperature	0.198*	0.199*
	(0.080)	(0.080)
July total precipitation in inches	0.686	0.650
	(0.590)	(0.601)
July mean wind speed	0.064	0.050
	(0.141)	(0.144)
Observations	1,258	1,258
Number of monitors	172	172
Adjusted R-squared	0.284	0.283

** $p < 0.01$, * $p < 0.05$, $^+$ $p < 0.1$; cluster-robust standard errors in parentheses; missing weather and year dummy variables included but not shown.

[a] Column titles (“total” and “average”) refer to the explanatory regulatory actions variable.

Table 42. Coefficients of Instrumented Variables in Regressions of the Impact of Regulatory Actions on Ozone Concentrations

Regulatory Actions[a]	Total (1)	Average (2)
Inspections		
Coefficient	-0.034	-9.673
(Cluster-robust standard error)	(0.121)	(5.881)
Endogeneity test p value	0.661	0.030
Kleibergen-Paap rk Wald F-statistic	6.393	5.460
Sargan overidentification p value	0.025	0.150
Enforcement actions		
Coefficient	-0.021	4.192
(Cluster-robust standard error)	(0.056)	(4.260)
Endogeneity test p value	0.717	0.320
Kleibergen-Paap rk Wald F-statistic	9.455	6.502
Sargan overidentification p value	0.032	0.046
Penalties (log or dummy variable)		
Coefficient	-0.946[+]	-1.302*
(Cluster-robust standard error)	(0.529)	(0.659)
Endogeneity test p value	0.121	0.086
Kleibergen-Paap rk Wald F-statistic	5.210	5.524
Sargan overidentification p value	0.085	0.102

** $p < 0.01$, * $p < 0.05$, [+] $p < 0.1$; cluster-robust standard errors in parentheses. Coefficients for other lagged regulatory variables and control variables are not shown.

[a] Column titles ("total" and "average") refer to the explanatory regulatory actions variable.