CEO compensation and adverse shocks: Evidence from changes in environmental regulations*

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Abstract

How do adverse shocks to firms influence CEO incentive compensation? By constructing firm-year measures of environmental regulatory stringency, we find that adverse shocks typically prompt corporate boards to reduce the risk-taking incentives of CEO compensation. However, this pattern is not uniform. Financially distressed firms exhibit milder reductions in compensation convexity, with some even increasing it, suggesting a "gambling for resurrection" strategy. Moreover, the strength of corporate governance influences shareholders' capacity to align executive incentives with shareholder risk preferences following unexpected changes in the stringency of environmental regulations.

JEL Classification: G34; G38; M52; Q53
Keywords: CEO compensation, risk-taking incentives, environmental regulation, pollution

*We greatly appreciate the comments of James Brown, Sudipto Dasgupta, Chang-Mo Kang, Jungmin Kim, Pureum Kim, Ryoonhee Kim, Yu Kyung Lee, Holger Mueller, and seminar participants at UC Berkeley Haas School of Business, Hanyang University, Financial Information Society of Korea 2023, Boca Corporate Finance and Governance Conference 2023, CAFM 2023, BAR Annual Conference (Harvard) 2023, Haskell & White Academic Conference 2023, SWFA 2024, SFS Cavalcade North America 2024, and Joint Conference with the Allied Korea Finance Associations 2024. We also thank the EPA for helpful discussions on data. All remaining errors are our own.

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1. Introduction

Foundational theories of the firm suggest that adverse shocks can influence shareholder preferences toward corporate risk-taking and the structure of executive compensation (e.g., Jensen & Meckling, 1976; Myers, 1977; Myers & Majluf, 1984; Pindyck, 1988). For example, adverse shocks might lead shareholders to prefer safer corporate investments to mitigate bankruptcy risk, leading them to offer their executives compensation packages with reduced risk-taking incentives, i.e., less convex compensation. However, shareholders of financially distressed firms might react oppositely, preferring their firms to shift toward higher-risk strategies to generate the returns necessary to avert bankruptcy. The effectiveness of corporate governance further complicates the relationship between adverse shocks and executive compensation, as the ability of shareholders to alter executive compensation depends on each firm's governance structure (e.g., Chhaochharia & Grinstein, 2009; Dai, Rau, Stouraitis, & Tan, 2020; Hoi, Wu, & Zhang, 2019; Humphery-Jenner, Lisic, Nanda, & Silveri, 2016). Thus, the impact of adverse shocks on executive compensation is an empirical question that may depend on firms' financial conditions and governance structures.

Nevertheless, few researchers have explored how firms adjust the convexity of executive compensation in response to various shocks. Gormley, Matsa, and Milbourn (2013) provide a notable exception. They examine the link between shocks to corporate liability risk and the convexity of new equity grants. However, they and others do not evaluate how different financial conditions and governance structures modulate the impact of adverse shocks on executive compensation, which theory highlights as first-order considerations.

To address this gap, we examine the impact of one shock—unexpected changes in environmental regulatory stringency—on executive compensation while differentiating firms by financial conditions and governance structures. Research demonstrates that environmental regulations significantly affect production costs (e.g., Choi, Levine, Park, & Xu, 2023; Dang, Wang, & Wang, 2022; Hsu, Li, & Tsou, 2023; Karpoff, Lott, & Wehrly, 2005; Krueger, Sautner, & Starks, 2020; Seltzer, Starks, & Zhu, 2022; Xu & Kim, 2022). However, researchers have not explored how these regulations influence shareholder preferences toward their firm's risk-taking or the compensation packages offered to executives. Thus, besides exploiting unexpected changes in environmental regulations to provide new evidence on how shocks influence executive compensation, we provide novel information on a potentially significant consequence of environmental regulations: regulations may alter the risk-taking incentives of executive compensation packages.

We leverage a unique feature of the U.S. Clean Air Act (CAA) to evaluate the effect of environmental regulations on executive compensation. The CAA requires the annual designation of counties as either in attainment or nonattainment with National Ambient Air Quality Standards (NAAQS) for ground-level ozone. These designations can change over time. For example, when a county fails to comply with NAAQS emission standards, the Environmental Protection Agency (EPA) switches its designation from attainment to nonattainment. Nonattainment designations lead to stricter environmental regulations that increase production costs on ozone-emitting facilities in such "treated" counties (Becker, 2005; Becker & Henderson, 2000, 2001; Greenstone, 2002).

This regulatory policy yields considerable cross-time, cross-firm heterogeneity in the stringency of environmental regulations. First, changes in attainment/nonattainment status trigger cross-county, cross-time variation in environmental regulations. Second, the impact of these regulatory changes on otherwise identical firms depends on the cross-county distribution of firms' plants. Third, the effect of attainment/nonattainment designations on firms, even firms with the same cross-county distribution of plants, depends on ozone emissions. Specifically, nonattainment designations only intensify the regulation of ozone-emitting plants, and the impact of those regulations on plants' production costs is positively related to each plant's ozone emissions. We exploit this setting to explore the effects of time-varying, firm-specific changes in environmental regulatory stringency on executive compensation.

We construct a firm-year measure of environmental regulatory stringency, ($A \to NA$) *exposure*, that combines information on (a) whether a firm's polluting facilities are in counties that switch from attainment to nonattainment status and (b) the ozone emissions from those plants. While counties can and do switch from nonattainment to attainment, these $NA \to A$ switches are infrequent, as the average duration of a county that receives a nonattainment designation is 16 years. Thus, we focus on $A \to NA$ switches.

We use *Vega*, the sensitivity of CEO wealth to stock return volatility, to measure the risk-taking incentives of CEOs' compensation packages. *Vega* is commonly employed to gauge the convexity of executive compensation. Research suggests that *Vega* is positively associated with increases in corporate risk-taking (e.g., Armstrong & Vashishtha, 2012; Bakke, Mahmudi, Fernando, & Salas, 2016; Chava & Purnanandam, 2010; Coles, Daniel, & Naveen, 2006; Edmans, Gabaix, & Jenter, 2017; Guay, 1999; Hayes, Lemmon, & Qiu, 2012; Liu & Mauer, 2011; Low, 2009; Mao & Zhang, 2018; Rajgopal & Shevlin, 2002; Shue & Townsend, 2017). In contrast to this literature, we explore how environmental regulatory shocks reshape

Vega.

We employ staggered difference-in-differences (DiD) analyses with continuous treatment to evaluate the impact of $(A \to NA)$ *exposure* on *Vega*. Our sample covers the 1993-2019 period and comprises over 2,700 publicly listed U.S. firms with over 30,000 firm-year observations. Besides conditioning on firm- and year-fixed effects, the analyses control for an array of time-varying firm and CEO traits that past research relates to compensation convexity.

One concern with using $(A \to NA)$ *exposure* to identify the impact of environmental regulations on executive compensation is that omitted variables correlated with the attainment-to-nonattainment switch might drive changes in executive compensation, not changes in environmental regulatory stringency per se. To address this concern, we employ a regression discontinuity design (RDD) to decompose nonattainment designations into an exogenous ("unexpected") and endogenous ("expected") component. The assumption underlying our RDD is that nonattainment designations are close to random—and therefore unexpected—when counties' ozone concentration levels are "close" to the NAAQS threshold level. To create an RDD satisfying this condition, we first note that when ozone concentrations are far above (below) NAAQS threshold levels, the county's probability of receiving a nonattainment designation next year is close to one (zero)—and therefore expected. We then focus on counties close to NAAQS threshold levels. Using the Calonico, Cattaneo, and Titiunik (2014) method for deriving asymptotically optimal definitions of "close" to the NAAQS threshold, we construct firm-year measures of the expected and unexpected components of $(A \to NA)$ *exposure* and include both in the DiD analyses. Our identification assumption is that the unexpected component of $(A \to NA)$ *exposure* captures the quasi-exogenous variation in switches to nonattainment status orthogonal to omitted confounding variables.

A related concern with identifying the impact of regulatory shocks on executive compensations is that firms might alter ozone emissions to influence attainment/nonattainment designations. While the RDD addresses this concern, we also note the following. First, even though the nonattainment designation intensifies environmental regulatory restrictions on plants, plants account for only about 12% of ozone emissions, with vehicles, residential heating, commercial and consumer solvents, etc., producing the bulk of ozone-generating chemicals. This observation does not eliminate the possibility that large plants could influence environmental regulatory designations in a county; however, the data suggest that such situations are unlikely to be a pervasive source of bias in our analyses. Moreover, our results are robust, with hardly any change in estimated coefficients, to eliminating plants that account for a relatively large proportion of total ozone emissions in a given county. Second, we only allow nonattainment designations to influence executive compensation in the first year of the $A \rightarrow NA$ switch. That is, we only consider non-zero values of $(A \rightarrow NA)$ *exposure* in the year that a county switches from attainment to nonattainment. In this way, we abstract from the possibility that firms shape the duration of the nonattainment designation through pollution abatement. While this likely leads to an underestimate of the impact on *Vega* because firms might adjust executive compensation in subsequent years, it reduces identification concerns.

We discover that more stringent environmental regulations reduce *Vega*. Moreover, only the unexpected component of nonattainment designation triggers reductions in *Vega*. This finding suggests that adverse regulatory shocks induce boards to reduce the convexity of CEO compensation. To assess the estimated economic effect, consider an average firm in our sample that initially has no exposure to nonattainment shocks and then experiences a one standard deviation increase in its nonattainment exposure. This additional exposure to environmental regulations implies that a 0.01 change in the firm's annual stock return volatility will change CEO wealth by \$121,000 instead of \$134,300, corresponding to a decrease in *Vega* of \$13,300.

Four methodological tests support this conclusion. First, we confirm the parallel trends assumption and show that *Vega* falls only after firms are treated with unexpected nonattainment designations. Second, the results hold when using propensity score matching to address the possible nonrandom assignment of firms into treated and control groups. Third, when implementing the de Chaisemartin and D'Haultfoeuille (2020, 2022) procedure for addressing potential biases from heterogeneous treatments in staggered DiD settings, we continue to find that unexpected increases in nonattainment exposure trigger reductions in *Vega*. Fourth, our results are robust to various alternative measures of nonattainment exposure. In particular, the results hold when accounting for the ability of multi-plant firms to reallocate emissions from nonattainment to attainment counties.

We also address two questions about CEO compensation. First, what accounts for the fall in *Vega*? *Vega* can fall because corporate boards change executive compensation packages or CEOs change corporate securities holding, e.g., by exercising options. We find that unexpected increases in $(A \rightarrow NA)$ *exposure* reduce the convexity of CEOs' compensation, but those increases do not affect CEOs' decisions to exercise options. Second, do environmental regulations change other features of CEO compensation? We find that unexpected nonattainment designations reduce new option grants and increase bonuses but have no significant effect on overall compensation. This is consistent with the view that more stringent environmental regulations induce corporate boards to restructure executive compensation to incentivize lower-risk investments.

We next exploit institutional details in how the EPA applies regulations across facilities within the same county to assess further the impact of unexpected increases in environmental regulatory stringency on *Vega*. Specifically, when counties receive nonattainment designations, the EPA intensifies its regulatory scrutiny more on (1) facilities geographically closer to ozone monitors (Auffhammer, Bento, & Lowe, 2009; Bento, Freedman, & Lang, 2015; Gibson, 2019), (2) newer facilities, as older facilities are often exempted from fully satisfying the more stringent regulatory requirements until they expand operations, and (3) facilities with histories of regulatory noncompliance such as those designated as high priority violators (HPV) by the EPA or those with EPA enforcement cases. From this perspective, the same nonattainment shock will boost regulatory stringency more among facilities closer to ozone monitors, younger facilities, and those with a history of regulatory noncompliance. To assess this prediction, we include the interaction between unexpected $(A \rightarrow NA)$ *exposure* and these three regulatory intensity measures. Consistent with the view that adverse shocks induce boards to reduce the convexity of executive compensation, unexpected $(A \rightarrow NA)$ *exposure* and its interaction with the regulatory intensity measures enter negatively and significantly in the *Vega* regressions.

We evaluate a crucial prediction from theory: the impact of adverse shocks on the convexity of executive compensation packages depends on firms' pre-shock financial conditions (Jensen & Meckling, 1976). For example, adverse shocks are more likely to push financially distressed firms into a negative equity position. Such a position could incentivize shareholders to favor higher-risk projects that could push the value of shares above zero. Thus, the relationship between nonattainment exposure and *Vega* could become less negative, or even positive, among sufficiently financially distressed firms.

To assess this hypothesis, we examine the interaction between unexpected $(A \rightarrow NA)$ *exposure* and measures of the pre-treatment financial conditions of firms. We test the joint hypothesis that (1) the linear unexpected $(A \rightarrow NA)$ *exposure* term enters negatively and (2) the interaction between unexpected $(A \rightarrow NA)$ *exposure* and financial distress enters positively, indicating that the negative impact of environmental stringency on the convexity of CEO compensation is dampened among financially distressed firms.

We find that the impact of nonattainment exposure on *Vega* becomes less negative, and sometimes positive, among more financially distressed firms. Unexpected $(A \rightarrow NA)$ *exposure* enters negatively and significantly, and the interaction term—unexpected $(A \rightarrow NA)$ *exposure* times the firm's pre-treatment level of financial distress—enters positively and significantly. The results hold across several measures of financial distress. Thus, consistent with foundational theories of the firm, pre-treatment financial distress mitigates the negative impact of adverse shocks on the convexity of executive compensation.

Finally, we examine the prediction that the impact of shocks on executive compensation depends on corporate governance. While past research shows that corporate governance influences executive compensation (Coles, Daniel, & Naveen, 2014; Morse, Nanda, & Seru, 2011), we evaluate whether shocks to environmental regulatory stringency reduce *Vega* more among firms with more effective corporate governance systems using nine governance indicators. We discover that unexpected $(A \rightarrow NA)$ *exposure* reduces the convexity of executive compensation more among firms with more effective corporate governance. The results are consistent with the view that (1) environmental regulatory shocks diminish shareholder preferences for corporate risk-taking, and (2) those shareholders can more readily reduce the convexity of executive compensation packages to align CEO-shareholder risk-taking incentives in firms with more effective governance systems.

Our study contributes to research on how environmental regulations impact CEO compensation. Deng and Gao (2013) and Banerjee, Humphery-Jenner, Nanda, and Zhang (2022) show that companies in polluted areas compensate their CEOs more. We focus on the impact of environmental regulations, not the pollution level, on executive compensation. Specifically, besides examining the effect of environmental regulations on overall compensation and its components, we dissect how environmental regulations alter the risk-taking incentives reflected in the convexity of CEO compensation packages.

Our study also relates to other work on the determinants of *Vega*. Hayes et al. (2012) find that firms reduce their usage of stock options in response to increased accounting costs. Chen, Jung, Peng, and Zhang (2022) demonstrate that firms convexify compensation payoffs when CEOs face restricted outside job opportunities. De Angelis, Grullon, and Michenaud (2017) find that removing short-selling constraints causes firms to increase the convexity of compensation payoffs. We show that (1) unexpected changes in the environmental regulatory stringency impacting firms alter the design of their managerial incentive contracts and (2) the response of executive compensation to those shocks depends on firms' financial conditions and governance effectiveness.

2. Institutional background

The CAA requires the EPA to set NAAQS for six pollutants: carbon monoxide, nitrogen dioxide, ozone, sulfur dioxide, particulate matter, and lead. We focus on ozone because (1) counties most often fail to comply with NAAQS by exceeding ozone limits (Curtis, 2020), and (2) counties failing to comply with NAAQS ozone limits trigger regulatory actions that boost firms' costs more than when counties fail to satisfy NAAQS limits on other pollutants (US EPA, 2015).[1] Thus, focusing on ozone offers a comparatively large and economically relevant treatment group, while reducing measurement error.

Each year, the EPA designates each county as being in attainment or out of attainment (nonattainment) with the NAAQS threshold based on ozone monitoring stations across the United States. Specifically, the EPA calculates an annual county-level summary statistic using high-frequency monitor readings, known as the "design value" (DV). The EPA designates counties with DVs above the NAAQS threshold as "nonattainment" counties and counties with DVs below the threshold as "attainment" counties. During our sample period from 1993 to 2019, the EPA used four different NAAQS thresholds as detailed in Internet Appendix Table IA.1.[2]

When a county is designated nonattainment, the EPA requires the state to submit and adopt a state implementation plan (SIP) that outlines how the state will bring nonattainment counties back into compliance with the NAAQS. While SIPs vary across states, they must follow EPA's guidelines and be approved by the EPA. Failure to submit and execute an acceptable SIP can result in federal sanctions, including withholding federal grants, penalties, and construction bans on new polluting establishments. The SIP is federally-enforced and legally binding for all firms that operate polluting plants in the nonattainment county.

In nonattainment counties, ozone-emitting plants are required to satisfy the standard of "lowest achievable emission rate" (LAER), which involves the installation of the cleanest available technology, regardless of economic cost. Plants in attainment counties face significantly less stringent environmental standards than those in nonattainment counties. These plants are

[1]The EPA's regulatory impact analysis estimates that achieving nationwide compliance with the current ozone NAAQS would cost \$2.2 billion across all counties. In comparison, compliance with particulate matter NAAQS is estimated to cost between \$53 million and \$350 million.

[2]Furthermore, there is a key identification advantage to focusing on one pollutant rather than using the common approach of pooling together the nonattainment designations of all six pollutants and classifying a firm as treated if it operates a polluting plant in a "nonattainment" county (e.g., Dang et al., 2022; Xu & Kim, 2022). The problem with such a pooling approach is that environmental regulations are pollutant-specific, so not all polluting plants emit the regulated pollutant. As a result, using the pooling approach will introduce measurement error into the treatment variable. Defining nonattainment only in terms of whether counties fail to meet ozone NAAQS reduces such measurement errors in the treatment variable.

subject to the installation of the "best available control technology" (BACT), whereby the EPA considers the technology's economic burden on the plant as the foremost priority in determining an acceptable emissions technology. As a result, compliance costs are significantly higher in nonattainment counties (e.g., Becker, 2005).

Beyond the LAER standard for capital investments, SIPs also require states to develop plant-specific regulations to reduce emissions from existing facilities, e.g., by altering operating and maintenance procedures and materials (Becker & Henderson, 2000), and thus increase the costs of operating plants in nonattainment counties. Becker and Henderson (2001) find that total operating costs are, on average, 17% higher in polluting plants from nonattainment areas relative to similar plants in attainment areas. Moreover, any additional emissions from one pollution source must be offset by paying another source in the same county to reduce its emissions (Nelson, Tietenberg, & Donihue, 1993). Shapiro and Walker (2020) show that expenditures on these emission offsets are among the largest environmental expenditures for polluting plants in nonattainment areas. In addition to abatement compliance costs, plants face more persistent inspections and oversight in nonattainment counties.

3. Data and variables

We examine the relationship between the stringency of environmental regulations facing firms and CEO incentive compensation from 1993 to 2019. We obtain compensation data from ExecuComp and merge these data with the Center for Research in Securities Prices (CRSP) and Compustat datasets, which provide the financial and accounting data. Following the literature (Coles et al., 2006), we exclude financial firms [standard industrial classification (SIC) codes between 6000 and 6999] and utility firms (SIC codes between 4900 and 4999). We require all firms to have non-negative, non-missing data on sales, total assets equity compensation. Following Chen et al. (2022), we exclude firm-years with stock prices less than $5.

Firms' plant-level ozone pollution data comes from the EPA's TRI database. The TRI data file contains information on the disposal and release of over 650 toxic chemicals from more than 50,000 plants in the U.S. since 1987. Firms with plants within specific industries (e.g., manufacturing, waste management, mining, etc.), have ten or more full-time employees, and handle amounts of toxic chemicals above specified thresholds must submit detailed annual reports on toxic emissions at the plant level and identifying information about the facility, such as the plant's name, county of location, industry, and parent company's name. Firms self-report their emissions to the TRI. The EPA regularly assesses these filings, and misreporting these emissions data can lead to criminal or civil penalties (Xu & Kim, 2022). Ozone is not directly emitted by plants. Rather, ozone is formed when ozone precursors trigger chemical reactions in the atmosphere. We designate a plant as "ozone-emitting" if it emits ozone precursors.[3] Internet Appendix Figure IA.1 shows the fraction of TRI plants labeled as ozone emitters across industries in nonattainment counties. Even within two-digit industry NAICS codes, there is considerable variation in the fraction of plants classified as ozone polluters. Although the TRI data provide information on chemical emissions through the ground, air, and water, we only consider air emissions (measured in pounds) because the NAAQS only regulates air emissions.

Each county's designation status is manually collected from the Federal Register. Furthermore, we obtain monitor-level ozone concentrations from the Air Quality System (AQS) database. For each ozone monitor, the database includes ozone concentration readings and the county location of the monitor. We use these ozone concentrations to calculate DVs, the statistics the EPA uses to determine whether a county complies with the NAAQS. Table IA.1 of the Internet Appendix provides the rules we use to calculate the DVs for different ozone standards and the relevant thresholds.

After merging these data, our final sample consists of 2,765 unique US publicly listed firms containing 31,202 firm-year observations. However, the sample decreases when using additional data in the analyses.

3.1. Compensation variables

To measure the convexity of compensation payoffs, we follow the existing literature (Coles et al., 2006; Guay, 1999) and compute the sensitivity of CEO wealth to stock return volatility. *Vega* equals the dollar change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns (Core & Guay, 2002). Following Coles et al. (2006), we assume that vega of stock holdings is zero. Since managers can adjust their accumulated option holdings, we also compute the vega of managers' current year compensation of option grants (*Flow vega*). *Vega* and *Flow vega* are stated in thousands of dollars and are winsorized at the 99th percentile.

We construct variables to measure changes in the composition of a CEO's portfolio of option holdings. *Number of options granted* is the number of options granted to the CEO

[3]We use the mapping from TRI chemicals to CAA criteria pollutants from Greenstone (2003). However, additional chemicals have been introduced into the TRI since the creation of the mapping. Thus, we contacted the EPA and hired a Ph.D. chemist in atmospheric science to classify the remaining chemicals.

in the current year multiplied by one thousand (for ease of interpretation) divided by the number of the firm's outstanding shares (Hayes et al., 2012). *Value of options exercised* is the dollar (in thousands) value of options exercised by the CEO in the current year (Gormley et al., 2013). *Number of options exercised* is the number of options exercised by the CEO in the current year multiplied by one thousand (for ease of interpretation) divided by shares outstanding (Chen et al., 2022).

We use several measures of the structure of CEO compensation (Humphery-Jenner et al., 2016). *Total pay* is the logarithm of one plus the CEO's total compensation (in thousands), represented by the data item TDC1 in ExecuComp. It consists of salary, bonuses, the value of restricted stocks granted, the value of options granted, long-term incentive awards, and other types of compensation. *Option intensity*, *Salary intensity*, *Bonus intensity*, and *Cash intensity* are the amount of compensation from option grants, salary, bonuses, and the sum of salary and bonuses, respectively, divided by total compensation (i.e., TDC1).

3.2. Measure of environmental regulatory stringency: Nonattainment exposure

We construct a time-varying, firm-specific measure of environmental regulatory stringency that exploits (1) cross-county, cross-time variation in nonattainment designations, (2) cross-firm, cross-county variation in the location of firms' plants, and (3) information on each plant's ozone emissions. While some counties switch back from nonattainment to attainment, this happens infrequently, as the average duration in nonattainment is 16 years. Thus, we study attainment-to-nonattainment switches.

Formally, we define the environmental regulatory stringency facing firm i in year t, i.e., its nonattainment exposure, as

$$(A \rightarrow NA)\ exposure_{i,t} = \ln\left(1 + \sum_{j} ozone_{j,i,t-1} \cdot (A \rightarrow NA)_{j,i,t}\right), \tag{1}$$

where j denotes plant, i denotes firm, and t denotes year. $ozone_{j,i,t-1}$ equals total ozone air emissions for plant j of firm i in year $t-1$. We use total emissions because that is how SIPs define their criteria. $(A \rightarrow NA)_{j,i,t}$ is a dummy variable that equals one if plant j of firm i is located in a county that switches from attainment to nonattainment in year t, and zero otherwise. $(A \rightarrow NA)$ *exposure* measures a firm's time-varying exposure to counties that switch from attainment to nonattainment. For example, a multi-plant firm that operates many heavy ozone-emitting plants in counties that switch from attainment to nonattainment will have a higher exposure to the more intense environmental regulations associated with nonattainment than a similar firm with most of its plants located in attainment counties. That is, the firm experiences greater environmental regulatory stringency. We also examine the robustness of the results to several alternative definitions of $(A \rightarrow NA)$ *exposure*, as discussed in Section 5.3.3 below.

We highlight three features of the above definition. First, we lag plant ozone emissions by one year because the specific timing of the release of the TRI data implies that emissions data for a given year only become available and influence regulatory oversight the following year (Hsu et al., 2023).

Second, the switch-to-nonattainment dummy variable, $A \rightarrow NA$, equals one only in the year that a county first switches from attainment to nonattainment. It equals zero in subsequent years, even when the county remains in nonattainment. Specifically, suppose county j switches from attainment to nonattainment in year 2000 and remains in nonattainment thereafter. If firm i operates ozone-emitting plants in county j in 2000, then county j would only alter the intensity of firm i's $(A \rightarrow NA)$ *exposure* in 2000, and not in subsequent years, as $A \rightarrow NA$ would be zero from 2001 onward. This feature of $(A \rightarrow NA)$ *exposure* is likely to lead to an underestimate of the strength of the relationship between environmental regulatory stringency and *Vega* because (1) environmental regulatory stringency will remain high during the years after the initial switch to nonattainment when counties retain the nonattainment designation (which typically lasts for 16 years), and (2) firms might adjust executive compensation during the years following the switch. Our measure, $(A \rightarrow NA)$ *exposure*, will not capture any of these potential connections between changes in *Vega* and intensified environmental regulatory stringency in the years after the initial switch. As a result, $(A \rightarrow NA)$ *exposure* will likely yield an underestimate of the relationship between environmental regulatory stringency and executive compensation. However, this feature of $(A \rightarrow NA)$ *exposure* has an advantage: it reduces identification concerns by abstracting from the possibility that firms shape the duration of the nonattainment designation.

Third, by weighting the switch-to-nonattainment dummy by a plant's total amount of ozone emissions, this measure captures the fact that regulatory stringency is increasing in a plant's ozone emissions. For example, a plant that does not emit ozone in a nonattainment county is unaffected by the regulation, and a plant that emits very little ozone likely faces smaller additional costs than a heavy ozone emitter.

4. Research design

4.1. Baseline DiD specification

We examine the impact of nonattainment exposure on CEO incentive compensation using a staggered DiD specification with continuous treatment (Acemoglu, Autor, & Lyle, 2004; Bertrand & Mullainathan, 2003). Specifically, we estimate the following firm-year panel regression:

$$Vega_{i,t} = \beta_0 + \beta_1 (A \rightarrow NA)\ \textit{exposure}_{i,t} + \beta_2 X_{i,t-1} + \tau_i + \rho_t + \varepsilon_{i,t} \tag{2}$$

where i denotes firm and t denotes year. The dependent variable, *Vega*, captures the risk-inducing incentives provided by CEOs' compensation. Treatment in this setting is measured by the continuous variable $(A \rightarrow NA)$ *exposure*. For example, in the years where a firm operates ozone-emitting plants in only attainment counties or only non-ozone-emitting plants in nonattainment counties, this variable takes on a value of 0. However, for the firm-years where the firm operates an ozone-emitting plant in a county that switches from attainment to nonattainment, this variable will change from 0 to a positive value that captures treatment intensity. Firms that do not own any polluting plants will, by definition, have a nonattainment exposure of 0; these observations serve as the never-treated units.

Our baseline specification uses standard two-way fixed effects based on firm and year. Firm-fixed effects (τ_i) control for all time-invariant firm-specific factors. Year-fixed effects (ρ_t) control for time-specific factors, including trends, that are common across firms. Since our sample period covers four different ozone-standard cohorts, we also estimate a more stringent specification that allows firm and year-fixed effects to vary by ozone-standard cohort by using firm $\times$ cohort and year $\times$ cohort fixed effects. Gormley and Matsa (2014) show that this approach is more conservative than including two-way fixed effects, reducing omitted variable concerns. Standard errors are clustered at the firm level. The main coefficient of interest is β_1, the DiD estimate of the effect of nonattainment exposure on CEOs' *Vega*.

We include a vector of time-varying firm traits, $X_{i,t-1}$, to control for factors that prior research shows affect the convexity of compensation packages that boards grant to CEOs (Core & Guay, 1999; Guay, 1999). We control for the CEO's age (in years), tenure (log of one plus the number of years as CEO), and ownership (fraction of the firm's outstanding shares owned by the CEO). At the firm level, we follow Core and Guay (1999) and control for investment opportunities using firm size, book-to-market ratio, and leverage. Following Hoi et al. (2019), we include the return on assets and stock returns to control for the influences of managerial ability and luck on CEO incentive pay. We also control for cash, sales growth, and stock return volatility. Table A.1 in Appendix A describes the control variables in detail.

4.2. Decomposition of nonattainment exposure

Despite including extensive controls, there might be concerns that the analyses fail to account for factors that simultaneously lead firms to expect nonattainment designations and adjust executive compensation. If we omit such factors from the analyses, we might draw inappropriate inferences about the impact of environmental regulatory stringency on CEO vega. One strategy for reducing potential omitted variable bias is to identify unexpected shocks to nonattainment designations and assess the relationship between those adverse shocks and executive compensation.

To address this concern, we employ a RDD that decomposes attainment-to-nonattainment switches into exogenous ("unexpected") and endogenous ("expected") components. We then evaluate the relationships between executive compensation and both unexpected and expected changes in environmental regulatory stringency. Our strategy is based on the following assumptions: (1) counties with ozone concentration levels far above the NAAQS threshold will receive a nonattainment designation with probability close to one, (2) counties with ozone concentration levels far below the threshold will receive a nonattainment designation with probability close to zero, and (3) counties will have nonattainment designations that are close to random—and therefore unexpected—when their ozone concentration levels are sufficiently "close" to the NAAQS threshold level. Thus, we use an RDD design to identify counties that are sufficiently close to the NAAQS threshold, such that we can consider nonattainment designations as unexpected, random outcomes that are orthogonal to potential confounding influences.

To operationalize this strategy for decomposing attainment-to-nonattainment switches into expected and unexpected components, we use the Calonico et al. (2014) method for deriving asymptotically optimal definitions of "close", i.e., we estimate an optimal "bandwidth" of the region where ozone concentrations are as good as randomly assigned and, hence, unexpected. For brevity, we provide full details of the RDD specification and specification tests in Section IA of the Internet Appendix.[4]

[4]As noted, some counties switch from nonattainment to attainment. However, the criteria for switching from nonattainment to attainment are not symmetrical with those for switching from attainment to nonattainment. As a result, we cannot employ this RDD strategy to extract the exogenous component of nonattainment-to-attainment switches. Specifically, any random event that pushes a county above the threshold pollution

We summarize the decomposition procedure in Figure 1, which plots a county's probability of nonattainment conditional on the distance of its DV (ozone concentrations) from the threshold. As expected, the probability of nonattainment appears to be a continuous and smooth function of the centered DVs everywhere except at the NAAQS threshold, where there is a discontinuous jump upwards. The two dashed vertical lines on either side of the discontinuity represent the optimal bandwidth estimate. Counties with a DV falling within the predicted attainment region are almost certain to be designated attainment, while those within the predicted nonattainment region are almost certain to be designated nonattainment. The area within the bounds of the optimal bandwidth is the unpredictable region; changes in the probability of nonattainment are attributable to random fluctuations in the underlying DVs and, hence, unpredictable. Thus, within the optimal bandwidth, we treat nonattainment designations as quasi-exogenous.

We define an attainment county that has a DV falling in the unpredictable region and subsequently switches to nonattainment as an "unexpected" nonattainment. Conversely, an attainment county that has a DV falling in the predicted nonattainment region and subsequently switches to nonattainment is defined as an "expected" nonattainment. This decomposition allows us to measure a firm's exposure to unexpected and expected nonattainment designations, respectively, as follows:

$$Unexp.\ (A \rightarrow NA)\ exposure_{i,t} = \ln\left(1 + \sum_j ozone_{j,i,t-1} \cdot Unexp.\ (A \rightarrow NA)_{j,i,t}\right), \tag{3}$$

$$Exp.\ (A \rightarrow NA)\ exposure_{i,t} = \ln\left(1 + \sum_j ozone_{j,i,t-1} \cdot Exp.\ (A \rightarrow NA)_{j,i,t}\right), \tag{4}$$

where $Unexp.\ (A \rightarrow NA)_{j,i,t}$ ($Exp.\ (A \rightarrow NA)_{j,i,t}$) is a dummy variable equal to one if plant j of firm i is located in a county that unexpectedly (expectedly) switches from attainment to nonattainment in year t, and zero otherwise. A higher value of $Unexp.\ (A \rightarrow NA)\ exposure$ ($Exp.\ (A \rightarrow NA)\ exposure$) indicates that the firm has a greater exposure to unexpected (expected) nonattainment designations. We also estimate a similar staggered DiD as Equation (2), except we decompose $(A \rightarrow NA)\ exposure$ into its unexpected and expected components as

level triggers a nonattainment designation. This is not the case for nonattainment-to-attainment switches. Switching from a nonattainment to an attainment designation requires the county to demonstrate a sustained period of ozone concentrations below the threshold level. Usually, counties must prove to the EPA (through projected emissions modeling and other "maintenance plans") that they can maintain concentrations below the threshold for at least 10 years. Thus, random events, such as changes in wind patterns, that push ozone concentrations below the threshold for one particular year will not trigger a redesignation. This institutional feature helps explain why redesignations are relatively infrequent and why the RDD strategy for identifying unexpected shifts to nonattainment status cannot be applied to nonattainment-to-attainment switches.

follows:

$$Vega_{i,t} = \beta_0 + \beta_1 Unexp.\ (A \rightarrow NA)\ exposure_{i,t} + \beta_2 Exp.\ (A \rightarrow NA)\ exposure_{i,t} + \beta_3 X_{i,t-1} + \tau_i + \rho_t + \varepsilon_{i,t}. \tag{5}$$

The main coefficient of interest is β_1, the estimated relationship between CEO vega and the unexpected component of environmental regulatory stringency measured by *Unexp.* $(A \rightarrow NA)$ *exposure.*

Before describing the results, we highlight an additional feature of the research setting that reduces concerns that firms manipulate ozone emissions to influence attainment/nonattainment designations, impeding identification. As just described, decomposing nonattainment designations into expected and unexpected components reduces such concerns and strengthens our ability to draw causal inferences from the analyses. We now highlight firms' plants account for only about 12% of ozone emissions as shown in Figure 2. Vehicles, residential heating, commercial and consumer solvents, etc., produce the bulk of ozone-generating chemicals. This observation does not entirely eliminate the possibility that large plants could influence environmental regulatory designations in a county; however, the data suggest that such situations are unlikely to introduce significant bias into our analyses. Furthermore, the results hold when eliminating firms operating plants in counties where their emissions are "large" relative to other ozone emission sources in the county, as shown in Section 5.3.4. These observations enhance the identification strategy based on differentiating between expected and unexpected nonattainment designations and including extensive controls.

5. Main analyses

5.1. Descriptive statistics

Table 1 presents summary statistics for key variables. The mean value of *Vega* is \$126.69 thousand, while the mean value of *Flow vega* is \$22.30 thousand. On average, total CEO compensation is \$2.83 million, *Option intensity* is 25.7%, and *Cash intensity* is 40.4%. CEOs, on average, are 55.6 years old, have 4.9 years of tenure at their current job, and hold 2.4% of the firm's equity. These sample statistics align with prior studies (Hayes et al., 2012; Humphery-Jenner et al., 2016).

For firm-year observations with non-zero $(A \rightarrow NA)$ *exposure* values, the average $(A \rightarrow NA)$ *exposure* is 8.2, with a standard deviation of 3.6, indicating substantial variation in firms' exposure to nonattainment designations. Comparing the mean and median of *Unexp.* $(A \rightarrow$

NA) *exposure (non-zero)* and *Exp.* ($A \rightarrow NA$) *exposure (non-zero)*, the average treated firm has a higher exposure to unexpected nonattainment designations than expected nonattainment designations.

5.2. Effect of nonattainment exposure on CEO incentive compensation

Table 2 presents estimates of the relationship between CEO vega and nonattainment exposure. We first provide results for ($A \rightarrow NA$) *exposure*, in which we do not distinguish between the expected and unexpected component of changes in nonattainment exposure. We then provide the estimates of *Exp.* ($A \rightarrow NA$) *exposure* and *Unexp.* ($A \rightarrow NA$) *exposure*, in which we distinguish between the expected and unexpected components of nonattainment exposure.

The results suggest that intensifying environmental regulatory stringency is associated with sharp declines in CEO vega. For example, column (1) of Table 2 includes ($A \rightarrow NA$) *exposure* as the only independent variable. The estimated coefficient on ($A \rightarrow NA$) *exposure* is -3.326 and is significant at the 1% level, indicating a decrease in the convexity of compensation payoffs following an increase in firms' nonattainment exposure. The decline in vega remains robust after controlling for CEO and firm characteristics (column (3)) and including firm-cohort and year-cohort fixed effects (column (5)).[5] To interpret the economic magnitude, consider an average firm that initially has no exposure to nonattainment shocks and then experiences a one standard deviation increase in its nonattainment exposure (2.376). The estimate in column (3) of Table 2 on ($A \rightarrow NA$) *exposure* (-5.596) suggests that this additional exposure to environmental regulations will reduce *Vega* by \$13,300 ($\approx -5.596 \times 2.376$ thousand), which implies that a 0.01 change in the firm's annual stock return volatility will change CEO wealth by \$121,000 instead of \$134,300, i.e., a decrease of \$13,300. This decrease of \$13,300 is equivalent to approximately 10% of the sample mean of *Vega* and 4% of its sample standard deviation.

We also discover that it is the unexpected component of nonattainment exposure that drives this result. As shown in columns (2), (4), and (6) of Table 2, the estimated coefficient on *Unexp.* ($A \rightarrow NA$) *exposure* enters negatively and statistically significantly in all specification, indicating that adverse environmental regulatory shocks decrease the convexity of compensation payoffs. However, *Exp.* ($A \rightarrow NA$) *exposure* enters insignificantly in all specifications. These results suggest that the observed decreases in the convexity of compensation payoffs following increases in environmental regulatory stringency are primarily driven by the exogenous

[5] The signs of the estimated coefficients on the control variables are largely consistent with the existing literature (Core & Guay, 1999; Guay, 1999; Hayes et al., 2012).

component of nonattainment designations, rather than the expected component.

5.2.1. Dynamic effects

Our identification strategy is based on the parallel trends assumption that treated and control firms exhibit similar trends in *Vega* prior to nonattainment exposure. Identification requires that the impact of $(A \to NA)$ *exposure* on *Vega* manifests only after the switch to nonattainment. To test for pre-trends, we estimate a dynamic version of Equation (5), focusing on the four years preceding and following nonattainment exposure. As our treatment variable is continuous, we follow the approach employed in previous studies (Fuest, Peichl, & Siegloch, 2018; Smith, Yagan, Zidar, & Zwick, 2019) to estimate the dynamic treatment effects based on the intensity of treatment as follows:

$$Vega_{i,t} = \sum_{\substack{\ell=-4 \\ \ell\neq-1}}^{\ell=+4} \gamma_\ell \mathit{Unexp.}\ (A \to NA)\ \mathit{intensity}^{\ell}_{i,t} + \sum_{\substack{\ell=-4 \\ \ell\neq-1}}^{\ell=+4} \lambda_\ell \mathit{Exp.}\ (A \to NA)\ \mathit{intensity}^{\ell}_{i,t} + \beta X_{i,t-1} + \tau_i + \rho_t + \varepsilon_{i,t} \tag{6}$$

where

$$\mathit{Unexp.}\ (A \to NA)\ \mathit{intensity}^{\ell}_{i,t} = \begin{cases} \sum_{s=-\infty}^{\ell} \Delta \mathit{Unexp.}\ (A \to NA)\ \mathit{exposure}_{i,t-s}, & \text{if } \ell = -4 \\ \Delta \mathit{Unexp.}\ (A \to NA)\ \mathit{exposure}_{i,t-\ell}, & \text{if } -4 < \ell < +4 \\ \sum_{s=\ell}^{\infty} \Delta \mathit{Unexp.}\ (A \to NA)\ \mathit{exposure}_{i,t-s}, & \text{if } \ell = +4 \end{cases} \tag{7}$$

and *Exp.* $(A \to NA)$ $\mathit{intensity}^{\ell}_{i,t}$ is defined similarly.[6] All other variables are defined as in Equation (5).

Equation (6) is a generalization of Equation (5) that allows for the effects of *Unexp.* $(A \to NA)$ *exposure* and *Exp.* $(A \to NA)$ *exposure* to evolve over the four years before and after the switch to nonattainment. The dynamic effects, denoted as γ_ℓ and λ_ℓ, provide event-study style regression estimates that capture the varying trend of *Vega* for firms exposed to unexpected and expected nonattainment designations, respectively. We define the year before the switch to nonattainment as the reference period, denoted by year $\ell = -1$. This choice allows us to express all dynamic effects relative to this reference year. To identify the dynamic effects during the event window, we bin the endpoints ($\ell = -4, +4$) according to Equation (7).

Our results do not reject the parallel trends assumption, as shown in Panel A of Figure 3.

[6] Here, $\Delta Y_{i,t} = Y_{i,t} - Y_{i,t-1}$.

The dynamic effects from Equation (6) provide no indication that the impact of $(A \rightarrow NA)$ *exposure* on *Vega* materializes before the attainment-to-nonattainment treatment. In the periods following the nonattainment designation, CEOs' vega decreases for firms exposed to unexpected nonattainment designations. This decrease begins in the year of the designation and continues to remain lower thereafter. In contrast, CEOs' vega for firms with expected nonattainment exposure remains unchanged throughout the post-treatment periods, with none of the coefficients significantly differing from zero. Overall, the results indicate that firms' exposure to unexpected nonattainment designations leads to a decrease in the convexity of compensation payoffs.

5.3. Robustness of main analyses

5.3.1. Propensity score matching

One possible concern is that firms with non-zero nonattainment exposure ("treated") may not be directly comparable to those with no exposure ("control") because they differ on other key dimensions omitted from the regressions. This could lead to biased estimates of the impact of environmental regulatory stringency on the convexity of CEO compensation.

To address this concern, we use propensity score matching (PSM) to account for systematic differences between treated and control observations. The propensity score, $\widehat{p}$, is generated by estimating a logistic regression model, where the dependent variable is a dummy variable equal to one if the firm-year observation belongs to the treated group and zero otherwise. The independent variables include all variables specified in the baseline model described in Equation (2). Using the propensity scores, each treated observation is matched with a control observation using one-to-one nearest neighbor matching with replacement (Roberts & Whited, 2013). This matching procedure ensures that treated and control observations have similar propensity scores, accounting for systematic differences between the two groups. To assess the effectiveness of the matching procedure, Internet Appendix Table IA.4 shows that there are no observable differences between treated and control observations after the matching.

Using the matched sample, we re-estimate Equations (2) and (5), and the results are reported in columns (1) and (2) of Table 3, respectively. In these columns, we examine the effect of nonattainment exposure on *Vega* by comparing firms with non-zero nonattainment exposure to those with comparable propensity scores but without actual exposure. The PSM results confirm our core finding that increases in firms' nonattainment exposure lead to decreases in the convexity of compensation payoffs, reducing concerns that systematic differences between the treated and control groups drive our results.

Instead of discarding non-matched observations, an alternative approach is to incorporate all observations using a weighted least squares procedure. This method assigns weights that are inversely proportional to the probability of an observation being a treated or control unit. Specifically, we follow the procedure in Caliendo and Kopeinig (2008), whereby firm-year observations in the treated group receive a weight of $1/\widehat{p}$, while those in the control group receive a weight of $1/(1-\widehat{p})$. Intuitively, propensity score weighting assigns a lower weight to treated observations that are "very different" (in terms of CEO and firm characteristics) from control observations and control observations that are "very different" from treated observations. The results are presented in columns (3) and (4) of Table 3. As before, the analysis demonstrates that nonattainment exposure reduces *Vega*. Overall, the results in this section suggest that the relationship between nonattainment exposure and *Vega* is unlikely to be driven by selection bias.

5.3.2. Heterogeneous treatment effects

There are also concerns that negative weights in two-way fixed effects regressions and heterogeneous treatment effects in staggered DiD designs could yield bias estimates. To address these concerns, we follow the approaches proposed by de Chaisemartin and D'Haultfoeuille (2020, 2022). First, we estimate the weights attached to the two-way fixed effects regressions in our analysis and find that only a small percentage (4%) of the weights are negative, with the sum of these weights being -0.001. This indicates that negative weights are not a significant concern in our study.

To address treatment effect heterogeneity, we employ the DiD estimator developed by de Chaisemartin and D'Haultfoeuille (2020, 2022). Since their estimator is most suitable for binary treatments, we dichotomize our continuous treatment variable.[7] Specifically, we define the variable $(A \rightarrow NA)$ *exposed* as a dummy variable equal to one for firm-year observations with non-zero nonattainment exposure, and zero otherwise. Similarly, we define the variables *Unexp.* $(A \rightarrow NA)$ *exposed* and *Exp.* $(A \rightarrow NA)$ *exposed* as dummy variables equal to one for firm-year observations with non-zero exposure to unexpected and expected nonattainment designations, respectively, and zero otherwise.[8]

The results, presented in Table 4, show that our inferences continue to hold after controlling

[7] Although the estimator developed by de Chaisemartin and D'Haultfoeuille (2020, 2022) can handle treatment effect heterogeneity for continuous treatments, it is not well-suited when the continuous treatment variable, such as $(A \rightarrow NA)$ *exposure*, can take on a large number of values.

[8] Note that we can only include the unexpected or expected nonattainment treatment one at a time due to the setup of the de Chaisemartin and D'Haultfoeuille (2020, 2022) estimator.

for treatment effect heterogeneity.[9] Furthermore, none of the individual pre-trend estimators enter with statistically significant coefficients, and we fail to reject the null hypothesis that all pre-trend estimators equal zero. These analyses do not detect pre-trends in the four years before nonattainment exposure.

5.3.3. Alternative measures of nonattainment exposure

We next address concerns with our measure of firms' exposure to nonattainment designations by constructing and analyzing alternative measures. When constructing these alternatives, we use different measures of $ozone_{j,i,t-1}$ in Equations (3) and (4).

First, there might be concerns that multi-plant firms have greater ability to reallocate production (and hence, emissions) from nonattainment to attainment counties, reducing the adverse effects of the treatment and the incentives to reduce CEO vega.[10] To address this concern, we construct and analyze two additional exposure measures. In the first measure, we set $ozone_{j,i,t-1}$ in Equations (3) and (4) equal to $ozone\ (\#Plant)_{j,i,t-1} = (1/\#Plant_{i,t}) \cdot ozone_{j,i,t-1}$, where $\#Plant_{i,t}$ is the total number of polluting plants owned by firm i in year t. Dividing ozone emissions by the total number of polluting plants owned by the firm recognizes that a multi-plant firm with the same ozone emissions in nonattainment counties as a single-plant firm may have lower nonattainment exposure due to its ability to redistribute emissions. In the second measure, we set $ozone_{j,i,t-1}$ in Equations (3) and (4) equal to $ozone\ (Prod.\ ratio)_{j,i,t-1} = Production\ ratio_{j,t} \cdot ozone_{j,i,t-1}$, where $Production\ ratio_{j,t}$ is the ratio of plant j's production in year t relative to its production in year $t-1$. By weighting plant j's ozone emissions by its production ratio, this measure recognizes that a multi-plant firm that reallocates production from plants in nonattainment counties to those in attainment counties will have lower nonattainment exposure.

Second, $(A \rightarrow NA)$ *exposure* may not reflect the relative importance of firms' polluting plants across counties. For example, it may be more costly if polluting plants that generate the majority of sales for a given firm are located in nonattainment counties. To address this concern, we construct and analyze two additional exposure measures. Specifically, $ozone\ (Sales\ share)_{j,i,t-1} = Sales\ share_{j,i,t} \cdot ozone_{j,i,t-1}$ and $ozone\ (Employees\ share)_{j,i,t-1} =$

[9] The economic magnitude of the estimated effects are larger in this analysis compared to the baseline model, as we are now examining a discrete change in nonattainment exposure rather than a change in the intensity of the continuous treatment variable.

[10] In practice, given that firms need time to make the necessary investments to shift production, it may be difficult for firms to strategically time their investments to expand into attainment counties. Additionally, the benefits from the less stringent regulations in attainment counties may be offset by the costs of sacrificing local supply chains and local customers in nonattainment counties, which may make reallocation less appealing.

Employees share$_{j,i,t} \cdot$ *ozone*$_{j,i,t-1}$, where *Sales share*$_{j,i,t}$ (*Employees share*$_{j,i,t}$) is plant j's dollar amount of sales (number of employees) in year t divided by the total sales (employees) of all polluting plants of firm i in the same year. By weighting ozone emissions by sales and employees, respectively, these measures recognize the cross-plant differences in importance to the firm.

Third, $(A \rightarrow NA)$ *exposure* does not account for the varying toxicity levels of the different chemicals composing the aggregate measure of ozone emissions. That is, the same quantity of the different chemicals composing the aggregate ozone measure have different effects on human health. Although nonattainment regulations are based on aggregate ozone emissions and do not account for cross-chemical differences in toxicity, local regulators may consider the toxicity of different chemicals when supervising facilities. In particular, they may target facilities with highly toxic emissions when conducting investigations. Thus, firms with more toxic emissions may experience more adverse shocks than otherwise similar firms with the same aggregate ozone emission levels due to additional regulatory oversight by local authorities. To address this concern, we set $\textit{ozone (TW)}_{j,i,t-1} = \sum_c TW_c \cdot \textit{ozone}_{c,j,i,t-1}$, where TW_c is the toxicity weight of chemical c derived from the EPA's Risk-Screening Environmental Indicator model. Given our focus on air emissions, we follow the approach of Gamper-Rabindran (2006) and utilize inhalation toxicity weights.

Fourth, we investigate the sensitivity of our findings to changes in the reporting requirements of chemicals in the TRI data, such as the removal or addition of specific chemicals. We restrict our focus to "core chemicals", which are chemical groups defined by the EPA and comprise chemicals subject to consistent reporting requirements throughout all reporting years to the TRI. Specifically, core chemicals exclude any chemicals added or removed from the TRI reporting list during our sample period. Additionally, core chemical groups undergo regular inspections and audits by the EPA to ensure accurate reporting (Kim, Wan, Wang, & Yang, 2019). We set $\textit{ozone (Core chemical)}_{j,i,t-1} = \sum_c \textit{Core chemical}_c \cdot \textit{ozone}_{c,j,i,t-1}$, where *Core chemical*$_c$ is a dummy variable equal to one if chemical c is a core chemical, and zero otherwise. By weighting ozone emissions using the core chemical dummy, the measure considers only emissions from a subset of chemicals that consistently require reporting to the TRI, reducing the likelihood that changes in reporting requirements over time are driving our results.

Fifth, the EPA requires plants in nonattainment counties that have the potential to be "major source" emitters to obtain a "Title V permit" to continue their operations. These permits are expensive and may also impose facility-specific requirements, such as restrictions on construction, specified air emission limits, and operational guidelines. Consequently, plants required to obtain Title V permits likely experience more adverse shocks than other plants in nonattainment counties. We define $\textit{ozone (Permit)}_{j,i,t-1} = \sum_c \textit{Permit}_{c,j,t} \cdot \textit{ozone}_{c,j,i,t-1}$, where $\textit{Permit}_{c,j,t}$ is a dummy variable equal to one if plant j holds operating permits to emit chemical c in year t, and zero otherwise. By weighting ozone emissions using the permit dummy, the measure considers only the subset of emissions originating from facilities with permits in nonattainment counties.

Our main results remain robust when employing all of the alternative measures of nonattainment exposure described above, as demonstrated in Internet Appendix Table IA.5.

5.3.4. Removing firms with large local emissions

Figure 2 indicates that while ozone emissions from TRI facilities, aggregated across all counties, constitute only a small fraction of total ozone emissions in any given year, certain counties may harbor TRI plants whose emissions represent a significant share of local ozone emissions. Consequently, the designation of nonattainment status for these counties may not be fully exogenous to the plant's emissions, as the designation could be influenced by the plant's own emissions.

To address this issue, we begin by computing total ozone emissions from point sources and all sources (the sum of point, non-point, on-road mobile, and non-road mobile sources) within a county for a given year, utilizing EPA's National Emissions Inventory (NEI) data.[11] For each year, we aggregate a firm's ozone emissions from all its plants within a county. We calculate two ratios: one representing the firm's ozone emissions relative to point source emissions within the county, and the other representing the firm's ozone emissions relative to all source emissions within the county. Then, for each year, we identify the 95th and 99th percentiles of these two ratios across all counties. We exclude firm-year observations from our DiD regression if their ratios exceed the 95th or 99th percentiles. This approach effectively filters out firms operating plants in counties where their emissions are "large" relative to either point source or all source emissions.

The results are presented in Internet Appendix Table IA.6. Columns (1) and (3) consider the ratio of the firm's ozone emissions to point source emissions, while columns (2) and (4) examine the ratio of the firm's ozone emissions to all source emissions. Across all columns, we observe minimal changes to the estimated coefficients. Specifically, the coefficient on

[11]We employ linear interpolation to estimate emission values between NEI assessment years.

Unexp. $(A \rightarrow NA)$ *exposure* remains negative and statistically significant, while the coefficient on *Exp.* $(A \rightarrow NA)$ *exposure* remains insignificant. Therefore, our results are unlikely to be influenced by individual plants whose emissions are large relative to those in the local county.

5.3.5. Placebo tests

We conduct placebo tests to assess whether nonattainment designations per se drive the results. Ozone nonattainment designations only regulate onsite ozone emissions. Therefore, firms that produce offsite ozone emissions or non-ozone chemicals such as particulate matter should not be affected by nonattainment regulation. Consequently, we can define placebo treatment variables by replacing ozone emissions with offsite ozone emissions or particulate matter emissions in the definition of $(A \rightarrow NA)$ *exposure*. If the board of directors reduces risk-taking incentives in response to actual regulatory exposure, the placebo treatment should have no relationship with *Vega*. The findings in the data are consistent with this view (Internet Appendix Table IA.7), as the placebo treatment variables enter insignificantly in the *Vega* regressions.

5.3.6. Alternative measures of CEO incentive compensation

We also consider three additional methods for measuring the risk-taking incentives of CEO compensation. First, to account for potential skewness in vega, we use the natural log transformation of one plus vega as the dependent variable. The results hold, as shown in columns (1) and (2) of Internet Appendix Table IA.8. Second, to mitigate potential biases arising from using this log transformation, we employ the fixed-effects Poisson model, as suggested by recent literature (Cohn, Liu, & Wardlaw, 2022). The results hold, as shown in columns (3) and (4). Lastly, following De Angelis et al. (2017), we utilize the ratio of vega to delta as the dependent variable.[12] This measure captures the trade-off between risk and return that managers face when considering project decisions. Specifically, high vega compensation may encourage a manager to accept a risky negative NPV project, while high delta compensation could counterbalance this effect by motivating the manager to reject such a project. The scaling of vega by delta captures this offsetting relationship. Our results, presented in columns (5) and (6) of Internet Appendix Table IA.8, confirm the robustness of our findings when using this scaling.

[12]Delta measures the dollar (in thousands) change in the value of the CEO's portfolio of current option and stock grants and accumulated option and stock holdings for a 1% change in the stock price.

5.3.7. *Treatment sample only*

To test whether our results are driven by changes in CEOs' incentive compensation for firms in the control sample, we conduct the analysis using firms that have been treated at least once during the sample period. In this setting, firm-year observations that experience a change in the intensity of nonattainment exposure later in the sample period are considered as "controls" for those that experience a change earlier in the sample period. Results presented in Internet Appendix Table IA.9 show that nonattainment exposure leads to a decrease in vega even among treated firms. This finding indicates that the baseline effect we document is not reliant on the control group and that the treatment effect arises from the exposed firms.

5.3.8. *Expected investment effects*

There might be concerns that nonattainment designations directly influence firms' investments, which in turn alter CEO compensation. For example, Dang et al. (2022) demonstrate that nonattainment designations influence R&D and capital investments over subsequent years, raising the possibility that these investments drive our findings.

However, several unique features of our identification strategy ameliorate such concerns. First, we examine unexpected switches from attainment to nonattainment, while past researchers do not make this distinction. As emphasized above, narrowing the focus to unexpected regulatory changes enhances our ability to identify the impact of intensifying environmental regulatory stringency. Second, our estimation strategy only allows nonattainment designations to influence CEO vega in the first year of the $A \rightarrow NA$ switch. This approach abstracts from the possibility that mandatory pollution actions triggered by nonattainment influence CEO vega through their effects on firm investments during the many years that typically follow $A \rightarrow NA$ switches.

Moreover, we find no evidence that unexpected changes in environmental regulatory stringency influence R&D and capital investments. In particular, we repeat the analyses of Dang et al. (2022) using our identification strategy and report the results in Internet Appendix Table IA.10. We discover that unexpected nonattainment designations do not affect firms' investments, reducing the likelihood that such effects drive our results.

6. Compensation metrics and regulatory intensity

This section extends the main analyses by assessing the relationships between (1) nonattainment exposure and more granular metrics of CEO compensation and (2) cross-firm differences in the regulatory intensity of nonattainment shocks and *Vega*.

6.1. Compensation metrics

In this subsection, we investigate the source of the decrease in the convexity of CEO compensation payoffs in response to nonattainment exposure by studying more granular measures of CEOs' compensation.

6.1.1. Effect of nonattainment exposure on the structure of new option grants

There are two primary ways CEOs' vegas change: boards change CEO compensation and CEOs change their holdings of their firm's securities, e.g., by exercising vested options.

To assess whether boards of directors change CEO compensation in response to nonattainment exposure, we examine *Flow vega*, which equals the vega of managers' *current* year compensation and ignores the past accumulated stock of options and other securities. More specifically, we replace the dependent variable in Equation (2) with *Flow vega* and present the results in Table 5.

We find that nonattainment exposure leads to a decrease in *Flow vega*, as shown in column (1) of Table 5; that is, boards reduce their granting of new options to CEOs in treated firms. Column (2) demonstrates that this decrease is driven by exposure to unexpected nonattainment designations rather than expected ones. Economically, a one standard deviation increase in *Unexp.* $(A \rightarrow NA)$ *exposure* reduces *Flow vega* by 4.27% relative to the sample mean, while a one standard deviation increase in *Exp.* $(A \rightarrow NA)$ *exposure* only reduces *Flow vega* by 2.84%. Panel B of Figure 3 presents the dynamic effects of unexpected and expected nonattainment exposure on *Flow vega* by estimating Equation (6) with *Flow vega* as the outcome variable. Consistent with the findings for *Vega*, we observe no significant changes in managers' current year compensation prior to firms' exposure to unexpected or expected nonattainment designations. However, following exposure to unexpected nonattainment designations, *Flow vega* decreases, while it remains unchanged after exposure to expected nonattainment designations.

We find consistent results when examining the *Number of options granted.* The number of options granted to the CEO relative to shares outstanding decreases significantly in response to nonattainment exposure, as shown in columns (3) and (4) of Table 5. The economic impact of *Unexp.* $(A \rightarrow NA)$ *exposure* on the *Number of options granted* is larger than that of *Exp.* $(A \rightarrow NA)$ *exposure*, as we reject the null hypothesis of equality between their coefficients (F-statistic = 9.87, p-value = 0.002).

To assess whether CEOs change their securities holdings in response to nonattainment exposure, we examine the extent to which treated CEOs exercise options in their firms. Thus, we use the same regression framework to evaluate the impact of $(A \rightarrow NA)$ *exposure*, *Unexp.* $(A \rightarrow NA)$ *exposure*, and *Exp.* $(A \rightarrow NA)$ *exposure* on *Value of options exercised* and *Number of options exercised.* The data do not reject the hypothesis that nonattainment exposure has no effect on CEOs exercising options, whether using the *Value of options exercised* or the *Number of options exercised.* Overall, the results are consistent with the view that boards adjust CEO compensation to reduce vega in response to nonattainment exposure, but CEOs do not change their option exercising decisions in response to changes in nonattainment exposure.

6.1.2. Effect of nonattainment exposure on CEO compensation structure

We next examine the effect of nonattainment exposure on CEO compensation beyond vega. Our findings thus far demonstrate that nonattainment exposure is associated with a sharp drop in vega as boards grant fewer and less valuable options to CEOs. However, they leave open the question of what happens to overall CEO compensation and the structure of that compensation. In Table 6, we provide results assessing unexpected changes in nonattainment exposure on total compensation, new option grants, base salary, bonuses, and the sum of salary and bonuses. We find that unexpected increases in nonattainment exposure trigger decreases in new option grants, increases in bonuses, and no changes in base salary, the sum of salary and bonuses, or total compensation. These results are consistent with the view that increases in nonattainment exposure induce a restructuring of CEO compensation toward a package with weaker risk-taking incentives.

6.2. Regulatory intensity of nonattainment shocks

In this section, we exploit three features of EPA regulations to further differentiate the impact of nonattainment exposure on firms. In particular, the same nonattainment exposure can interact with firm-specific traits and EPA regulatory guidelines to yield different cross-firm regulatory intensities. Thus, in response to the same nonattainment exposure shocks, we expect that the boards of firms that experience sharper increases in regulatory intensity will reduce risk-taking incentives more than otherwise similar firms. Our empirical specification expands Equation (5) by introducing interaction terms between *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* with a variable Z, which captures regulatory intensity.

We use three proxies to capture cross-firm variations in regulatory intensity. First, firms operating ozone-emitting plants located closer to monitors face more intense regulatory oversight than those located farther away, as regulatory efforts are concentrated in the vicinity of the monitors (Auffhammer et al., 2009; Bento et al., 2015; Gibson, 2019). Given the higher regulatory costs incurred by such firms, we anticipate that their boards would reduce risk-taking incentives in response to nonattainment exposure more than similar firms with plants farther away from monitors. Following the existing literature, we define a dummy variable, *Close monitor*, equal to one if a firm operates ozone-emitting plants within one mile of an ozone air quality monitor in a nonattainment county, and zero otherwise.

Second, Becker and Henderson (2000) observe that newer plants bear the brunt of nonattainment regulations due to expensive LAER requirements, while older plants are grandfathered and escape regulation until they expand operations.[13] Specifically, Becker and Henderson (2001) estimate that compliance costs are higher for young ozone-emitting plants between zero and five years of age in nonattainment counties compared to similar plants in attainment counties. Following their definition, we define *Young plant* as a dummy variable equal to one if a firm operates ozone-emitting plants that are between zero and five years of age in nonattainment counties, and zero otherwise.[14]

Finally, we consider two measures that capture a facility's history of regulatory noncompliance based on regulatory violations. The first measure gauges whether the facility is a high priority violator (HPV), as designated in the EPA's ICIS-Air database. When a facility is classified as HPV, it signifies serious or repeated violations that result in intense oversight by the EPA.[15] This heightened regulatory intensity can lead to higher fines and additional reporting requirements, thereby increasing the operating costs of the facility (Blundell, Gowrisankaran, & Langer, 2020). The second measure is the facility's enforcement cases, obtained from the EPA's FE&C database. Enforcement cases encompass judicial and administrative actions initiated by the EPA against facilities violating environmental statutes. Facilities that have enforcement cases are subject to greater regulatory intensity due to additional inspections and compliance evaluations and can be financially burdensome due to potential legal penalties

[13]While younger plants may benefit from specific cost savings in terms of NPV due to a slower equipment renewal rate than older plants, they also face immediate costs associated with nonattainment designations. Older plants may already have implemented LAER measures, thus avoiding additional capital expenditures. In contrast, younger plants may need to invest in implementing these control measures. Similarly, older plants may have established maintenance procedures to reduce emissions, while younger plants may not have implemented such procedures yet. These factors contribute to the higher immediate compliance costs faced by younger plants when subjected to nonattainment regulations.

[14]The first year a plant appears in the TRI database is not necessarily its first year of operation, since a plant only reports to TRI if it meets the reporting requirements. Thus, to compute the age of a given plant, we use the first year of operation of a given facility in the National Establishment Time-Series (NETS) database.

[15]HPVs cover a broad range of issues related to regulatory noncompliance, including excess emissions, failure to install required plant modifications and violations of operating parameters, among others.

(Shive & Forster, 2020; Xu & Kim, 2022). We define *HPV* and *Enforcement* to be dummy variables equal to one if a firm has experienced HPV status or an enforcement case, respectively, within the past three years among their ozone-emitting plants in nonattainment counties, and zero otherwise.

We find that boards of firms experiencing sharper increases in regulatory intensity following a given nonattainment exposure reduce the vega of their CEOs more than otherwise similar firms, as shown in Table 7. These results hold for each of our measures of regulatory intensity. The results in column (1) indicate that the negative effect of unexpected nonattainment exposure on *Vega* is more pronounced for firms with ozone-emitting plants closer to air monitors. The findings in column (2) show that the interaction term *Unexp.* $(A \rightarrow NA)$ *exposure* $\times$ *Young plant* enters negatively and significantly, indicating that firms operating young ozone-emitting plants exhibit a greater reduction in *Vega* when faced with the same unexpected nonattainment exposure as older plants experiencing the same shock. Columns (3) and (4) indicate that firms with a history of regulatory noncompliance experience larger decreases in vega in response to unexpected nonattainment designations.

7. Corporate financial conditions

The results thus far indicate that nonattainment exposure—and the firm-specific intensity of that treatment—is negatively associated with *Vega*. These findings are consistent with the view that adverse environmental regulatory shocks induce firms to alter CEO compensation in ways that reduce risk-taking incentives. In this section, we examine an additional testable prediction concerning the impact of adverse shocks on executive compensation: The response of shareholders to environmental regulatory shocks, including the convexity of compensation packages offered to executives, depends on the pre-existing financial conditions of the firm.

An extensive body of research suggests that the impact of adverse shocks on the risk-taking incentives of shareholders depends on firms' pre-shock financial conditions (e.g., Anantharaman & Lee, 2014; Brander & Poitevin, 1992; Eisdorfer, 2008; John & John, 1993). Specifically, consider the shareholders of financially distressed firms receiving adverse shocks from nonattainment exposure. Such shocks could drive the expected value of firms' equity below zero. Under these conditions, shareholders with limited liability might decide to have their firms pursue riskier projects, as successful outcomes could lead to a recovery in the value of their shares while pursuing lower-risk, lower-expected-return projects would likely leave the value of their shares below zero (Jensen & Meckling, 1976). Thus, these "gambling for resurrection" incentives could induce the boards of directors of sufficiently financially distressed firms to increase *Vega* in response to nonattainment exposure shock. More generally, there might be a nonlinear relationship between nonattainment exposure and *Vega* that becomes less negative and potentially even positive among firms facing more stringent financial constraints.

To assess whether shareholders in financially distressed firms reduce the extent to which they lower CEO vega in response to nonattainment exposure or engage in "gamble for resurrection" behavior by boosting *Vega*, we employ four measures of financial distress. First, we utilize two accounting-based measures of financial constraints commonly used in the literature: the Kaplan-Zingales index (Baker, Stein, & Wurgler, 2003; Kaplan & Zingales, 1997) and the Whited-Wu index (Whited & Wu, 2006).[16] Research has shown that financial constraints can hamper investment in valuable projects because the inability to borrow externally can force firms to bypass attractive investment opportunities (Campello, Graham, & Harvey, 2010). Thus, we adopt the perspective that "financial distress is a form of being financially constrained" (Kaplan & Zingales, 2000, p. 710). The two indices we employ are based on linear combinations of observable firm characteristics to proxy for firms' ability to access external financing. A higher value of these indices suggests a firm is more constrained.

However, in our setting, relying solely on accounting-based measures of financial constraints might be problematic since they tend to be correlated with production levels, which is a determinant of ozone emissions. To address this issue, we complement our analysis by incorporating a text-based financial constraint measure proposed by Hoberg and Maksimovic (2015). Their method relies on qualitative data extracted from corporate disclosures to quantify instances where firms faced constraints in raising capital. Following Xu and Kim (2022), our analysis uses the debt-market constraint index. Firms with a higher index value are prone to delaying investments due to liquidity issues and plan to mitigate these problems by issuing debt.

Lastly, we use the Campbell-Hilscher-Szilagyi index (Campbell, Hilscher, & Szilagyi, 2008) as a more direct measure of financial distress. The index is based on a logit model forecasting a firm's probability of failure over the subsequent 12 months, consisting of accounting and stock return data. Higher values of this index positively correlate with a firm's forecasted probability of failure.

Using each of the four financial distress measures (*FC index*), we find a nonlinear relationship between nonattainment exposure and CEO vega, such that the impact of nonattainment exposure on *Vega* becomes less negative, and sometimes even positive, among more financially dis-

[16]Variable definitions and details of their construction can be found in Table A.1 in Appendix A.

tressed firms. As shown in Table 8, we include interactions between *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* and the four financial constraint measures (*FC index*). To facilitate interpretation, the financial constraint measures are normalized to start from zero. This allows us to interpret the coefficients on *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* as the effects of unexpected and expected nonattainment exposure, respectively, on *Vega* for firms with no financial constraints. Across all specifications, we observe a significant negative coefficient on *Unexp.* $(A \rightarrow NA)$ *exposure*, indicating that firms without financial constraints reduce risk-taking incentives in response to nonattainment exposure. Moreover, the coefficient on the interaction term *Unexp.* $(A \rightarrow NA)$ *exposure* $\times$ *FC index* is positive and statistically significant. This implies that financially constrained firms exhibit a relatively smaller reduction in *Vega* in response to unexpected nonattainment shocks compared to their less financially distressed counterparts.

To illustrate how risk-taking incentives vary in response to nonattainment exposure based on financial constraints, we plot the marginal effects of *Unexp.* $(A \rightarrow NA)$ *exposure* on *Vega* conditional on the level of financial constraints in Figure 4. The solid line represents the point estimates, while the dashed lines indicate the 95% confidence intervals. We divide the sample into quartiles based on each financial constraint index, denoted as Q1, Q2, and Q3. Across all panels, we find that the marginal effect of unexpected nonattainment exposure on risk-taking incentives increases with a firm's financial constraints. However, among firms in the top quartile, we observe some evidence of a reversal in the sign of the marginal effect from negative to positive. This suggests that when exposed to nonattainment events, the most financially distressed firms increase risk-taking incentives—consistent with gambling for resurrection behavior.

8. Corporate governance

Research also suggests that when shocks alter the risk-taking incentives of shareholders, the ability of shareholders to alter the behavior of executives depends on the effectiveness of corporate governance (e.g., Coles et al., 2014; Morse et al., 2011). In particular, when firms' corporate governance mechanisms more effectively ameliorate principal-agent frictions, this research predicts that nonattainment exposure shocks that reduce shareholders' risk-taking incentives will induce large decreases in CEO vega than in firms with less effective governance systems. We use four categories of corporate governance measures to assess how the relationship between nonattainment exposure and *Vega* varies across firms with different governance structures.

8.1. CEO entrenchment

Firms with entrenched CEOs are more likely to experience a misalignment of risk preferences between managers and shareholders due to agency problems (Core, Holthausen, & Larcker, 1999). As adjustments to decrease vega can be influenced by negotiations between the CEO and the board, firms with more entrenched CEOs may hinder the board's ability to effectively reduce risk-taking incentives in response to nonattainment exposure.

We employ three measures to gauge CEO entrenchment. First, we utilize the *E-index* (Bebchuk, Cohen, & Ferrell, 2009), an index comprising six key anti-takeover provisions that indicates the degree of entrenchment, with higher values suggesting greater entrenchment. Second, following the approach of Adams, Almeida, and Ferreira (2005), we use a dummy variable equal to one if a firm's CEO also serves as the chairperson of the board in a given year, and zero otherwise (*CEO duality*). Lastly, we adopt the measure proposed by Coles et al. (2014), which is defined as the number of CEO appointed directors divided by the total number of board members for a firm in a given year (*Co-option*). Both *CEO duality* and *Co-option* capture the CEO's personal influence over the board. In columns (1) to (3) of Table 9, we include interactions between *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* with the CEO entrenchment measures.

The results indicate that firms with higher CEO entrenchment exhibit a smaller decrease in risk-taking incentives in response to nonattainment exposure. This finding suggests that when the board's monitoring effectiveness is compromised by entrenched managers, the ability to adjust vega is more limited.

8.2. Institutional investors

Research suggests that long-term institutional investors typically play a significant role in corporate governance due to their substantial ownership stakes and longer investment horizons (Derrien, Kecskés, & Thesmar, 2013; Harford, Kecskés, & Mansi, 2018). As a result, we anticipate that firms with higher proportions of long-term institutional investors will have stronger corporate governance, enabling the board to make more substantial downward adjustments to vega in response to nonattainment exposure. To classify institutional investors, we adopt the framework proposed by Bushee and Noe (2000), which considers portfolio turnover rates and portfolio diversification, resulting in three categories of institutional investors: dedicated investors, transient investors, and quasi-indexers. To assess the influence of long-term and short-term investors, we measure the fraction of a firm's shares held by dedicated (*IO DED*) and transient (*IO TRA*) institutional investors, respectively.

The findings presented in columns (4) and (5) of Table 9 reveal that the coefficient on the interaction term *Unexp.* ($A \rightarrow NA$) *exposure* $\times$ *IO DED* is negative, indicating that the presence of long-term investors corresponds to a more pronounced decrease in vega. Conversely, the coefficient on the interaction term *Unexp.* ($A \rightarrow NA$) *exposure* $\times$ *IO TRA* is positive, suggesting that firms with a higher proportion of short-term investors experience a less significant reduction in vega.

8.3. CEO bargaining power

Previous studies highlight that CEOs with greater bargaining power often have more influence over corporate policies, including the design of compensation packages (Bebchuk, Cremers, & Peyer, 2011). If CEOs with higher bargaining power make it more difficult for the board to modify incentive compensation, we would expect to observe a less pronounced decrease in risk-taking incentives in response to nonattainment exposure for such firms.

To capture CEOs' bargaining power, we employ two commonly used measures following Bebchuk et al. (2011): the ratio of total CEO compensation to the highest compensation earned by any other executive in the firm (*Pay slice 1*), and the CEO's total compensation scaled by the sum of the total compensation of the top-three highest remunerated non-CEO executives (*Pay slice 3*). Consistent with our expectations, the results presented in columns (6) and (7) of Table 9 indicate that an increase in CEO bargaining power is associated with a significantly less pronounced decrease in vega in response to nonattainment exposure.

8.4. CEO overconfidence

Research suggests that overconfident CEOs tend to overestimate investment returns and underestimate risks (Malmendier & Tate, 2005, 2008). In the presence of overconfident CEOs, firms may opt to further constrain risk-taking incentives to mitigate the adverse shock of nonattainment exposure and to curb excessive risk-taking behavior.

To measure CEO overconfidence, we employ two commonly used measures based on existing literature: a continuous measure called *Confidence*, which captures the extent to which the CEO's vested stock options are in-the-money (Banerjee, Humphery-Jenner, & Nanda, 2015), and a binary measure called *Holder67*, which equals one if the CEO fails to exercise options with five years remaining duration despite a 67% or higher increase in stock price since the grant date, and zero otherwise (Malmendier, Tate, & Yan, 2011). Consistent with our expectations, columns (8) and (9) of Table 9 demonstrate that the board adjusts risk-taking incentives downward even more when faced with overconfident CEOs.

9. Conclusion

Our study examines how unexpected adverse environmental regulatory shocks that intensify environmental regulatory stringency affect the risk-taking incentives provided to CEOs through the structure of their incentive compensation. Using a staggered DiD approach, we exploit switching to nonattainment status under the NAAQS as exogenous sources of variation in environmental regulatory stringency. We discover that firms exposed to nonattainment designations decrease the convexity of CEOs' compensation payoffs. Moreover, CEO vega falls because corporate boards change the structure of CEO compensation, not because CEOs alter their option-exercising behavior. The evidence is consistent with corporate boards actively adjusting CEOs' compensation to align executive incentives with declines in shareholders' preferences for corporate risk-taking following shocks to environmental regulations.

Firms' pre-existing financial conditions and governance effectiveness shape how corporate boards adjust the convexity of compensation contracts offered to executives in response to changes in environmental regulatory stringency. Following unexpected, adverse regulatory shocks, financially distressed firms exhibit more muted reductions in compensation convexity than financially robust firms. Indeed, those firms that are sufficiently financially distressed boost CEO pay convexity in response to adverse environmental regulatory shocks. This finding is consistent with the view that adverse shocks can induce the shareholders of financially distressed firms to seek higher risk-return strategies to avoid bankruptcy. Finally, we investigate the impact of various aspects of a firm's existing corporate governance structure on CEO incentive compensation dynamics. When corporate governance structures reduce principal-agent frictions, CEO pay convexity responds more elastically to unexpected changes in environmental regulatory stringency. Our findings provide strong evidence that environmental regulations shape CEOs' incentive compensation and highlight the role of corporate boards in adjusting executive incentives to correspond with shareholders' risk preferences.

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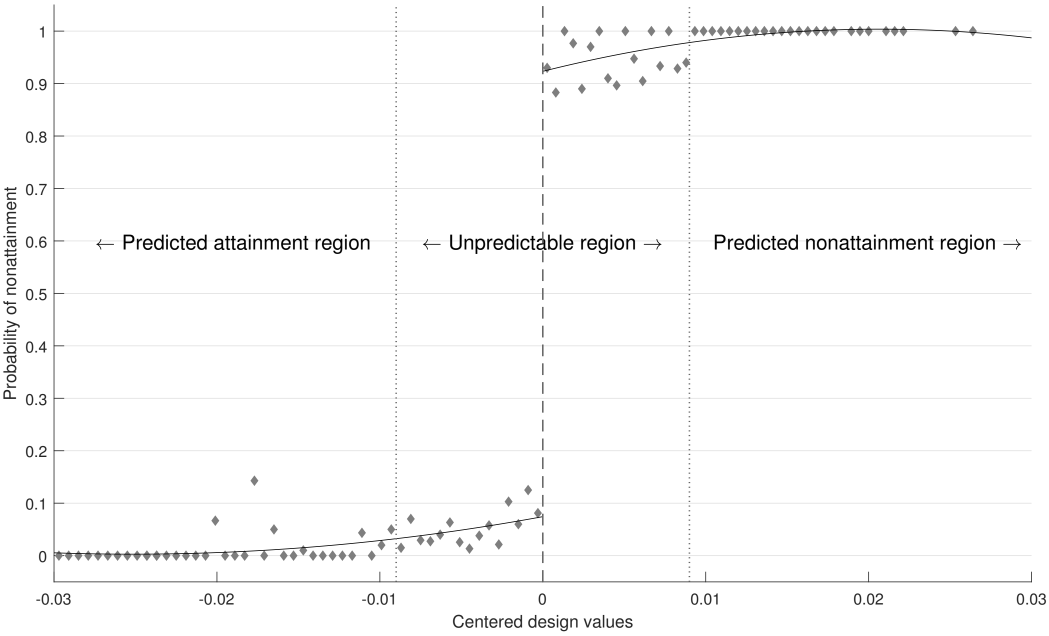
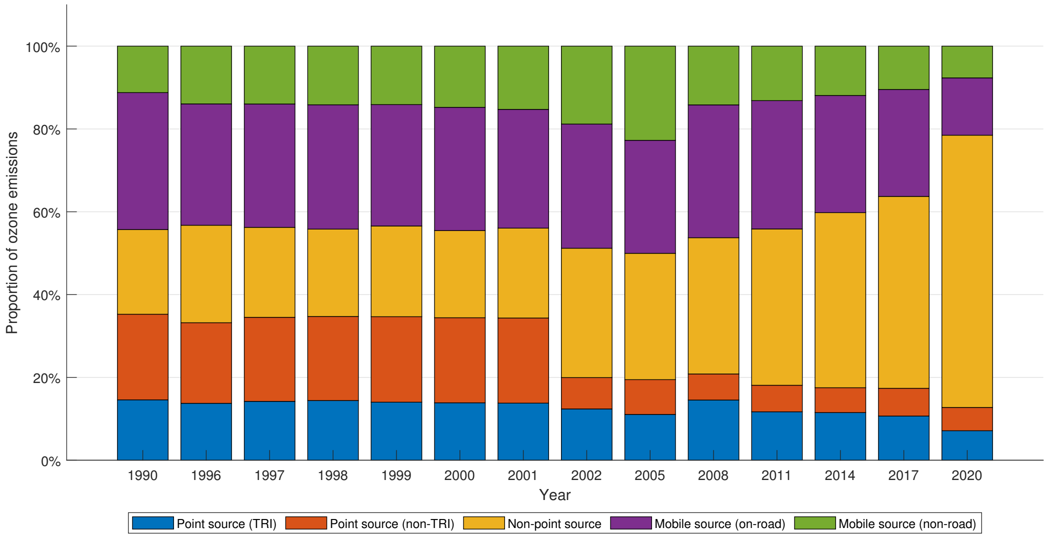
Figure 1
Probability of nonattainment around ozone NAAQS thresholds.



This figure presents the regression discontinuity relating centered DVs to the probability of nonattainment. The regression discontinuity is estimated from a local linear regression specification using the mean squared error optimal bandwidth with rectangular kernels following Calonico et al. (2014). Further details are provided in Section IA of the Internet Appendix. The vertical axis shows the probability of nonattainment. The horizontal axis shows the centered DVs around zero by subtracting the NAAQS threshold from the DVs. The dashed vertical line at zero represents the NAAQS threshold for ozone nonattainment status. Observations on the right (left) of the line indicate that the county is in violation of (compliance with) the NAAQS threshold. Each dot in the figure represents the average of $NA_{c,t+1}$, defined as a dummy variable equal to one if county c is designated nonattainment in year $t + 1$, using integrated mean squared error optimal bins following Calonico et al. (2014). The solid lines on either side of the NAAQS threshold is based on two separate regressions of $NA_{c,t+1}$ on local quartic polynomials in centered DVs. The unpredictable region refers to the narrow region surrounding the NAAQS threshold, which is bounded by the mean squared error optimal bandwidth. The predicted nonattainment region refers to the region to the right of the optimal bandwidth. The predicted attainment region refers to the region to the left of the optimal bandwidth.

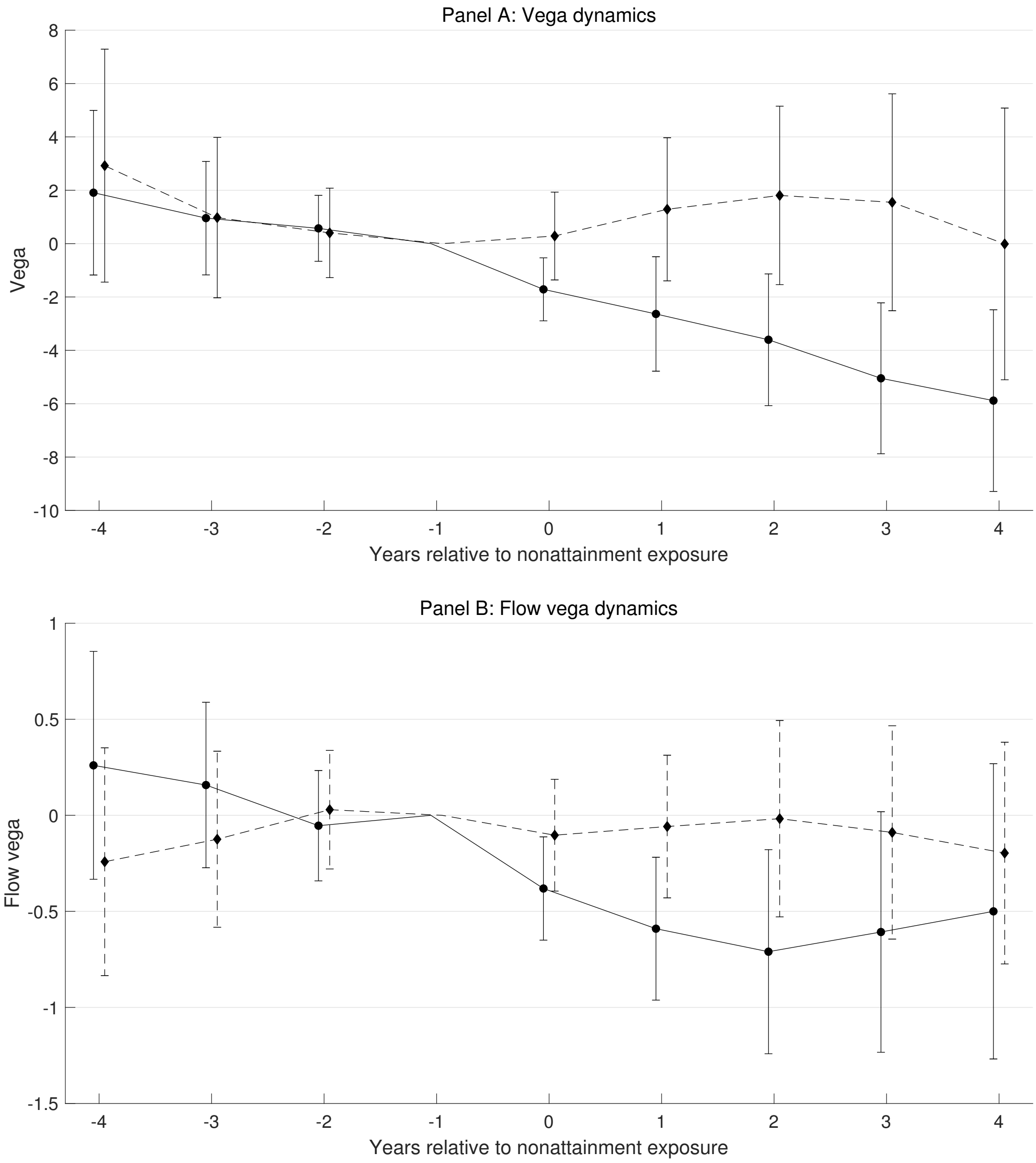
Figure 2

Proportion of ozone emissions by source.



This figure presents the proportion of ozone emissions from different sources across all counties based on EPA's National Emissions Inventory (NEI) from 1990 to 2020. The NEI provides a detailed assessment of air emissions originating from various sources. The inaugural assessment was conducted in 1990, followed by annual releases from 1996 to 2002, and subsequent releases every three years thereafter. Pollution sources are categorized into four types: point sources (including TRI facilities and non-TRI sources), non-point sources, on-road mobile sources, and non-road mobile sources. Point sources refer to emissions originating from fixed and stationary locations. These encompass the TRI plants (labeled Point source (TRI)) examined in this study, along with other stationary pollution sources (labeled Point source (non-TRI)) such as airports, wastewater treatment plants, and pipelines, etc. Non-point sources, on the other hand, encompass emissions from various sources that individually do not emit enough to be categorized as point sources. Examples include emissions from residential heating, commercial combustion, asphalt paving, and the use of commercial and consumer solvents. On-road mobile sources pertain to emissions from vehicles operating on roads, utilizing gasoline, diesel, or other fuels, including cars, trucks, and motorcycles. Non-road mobile sources consist of emissions from construction equipment, lawn and garden machinery, aircraft ground support equipment, locomotives, and commercial marine vessels. As depicted in the figure, ozone emissions from TRI facilities constitute only a minor portion of the total ozone emissions, averaging 12% across all NEI years. In contrast, non-point sources contribute approximately 31%, while emissions from mobile sources represent about 42%.

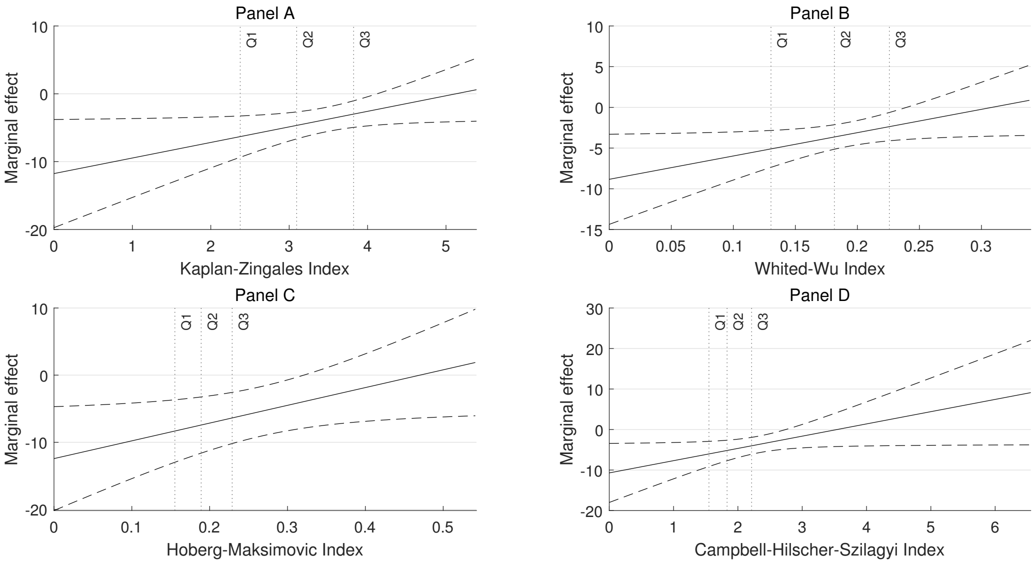
Figure 3
Dynamic effects of nonattainment exposure on CEO incentive compensation.



This figure plots the event study estimates and corresponding 95% confidence intervals according to the specification in Equation (6). The sample period is fiscal year 1993 to 2019. We focus on a window of four years before to four years after the nonattainment exposure. Event year $t = -1$ is the omitted category, implying that all coefficient estimates are relative to this year. The dependent variable in Panel A is *Vega*, which measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. The dependent variable in Panel B is *Flow vega*, which measures the dollar (in thousands) change in the value of the CEO's current option grants for a 0.01 increase in the annualized standard deviation of a firm's stock returns. The solid and dashed lines represent the dynamic effects of unexpected and expected nonattainment exposure on the dependent variables, respectively.

Figure 4

Marginal effects of nonattainment exposure on CEO incentive compensation conditional on financial constraints.



This figure plots the marginal effects of unexpected nonattainment exposure on CEO portfolio vega conditional on financial constraints. Panels A, B, C, and D plot the estimates of the marginal effects and corresponding 95% confidence intervals for the Kaplan-Zingales, Whited-Wu, Hoberg-Maksimovic, and Campbell-Hilscher-Szilagyi indices, respectively, based on the regression results in Table 8. Note that each index is normalized so that it begins from zero. The dashed vertical lines split the sample into quartiles based on the financial constraints index.

Table 1
Summary statistics.

Variables	N	Mean	Median	Std. dev.	P25	P75
CEO compensation						
Vega	31,202	126.688	42.018	296.057	10.577	125.194
Flow vega	31,195	22.303	5.447	36.843	0.000	26.414
Number of options granted	30,331	1.648	0.664	2.670	0.000	2.140
Value of options exercised	30,329	1498.270	0.000	4059.690	0.000	797.424
Number of options exercised	30,329	0.864	0.000	2.012	0.000	0.657
Total pay	31,202	7.948	7.983	1.104	7.195	8.711
Option intensity	30,975	0.257	0.190	0.275	0.000	0.446
Salary intensity	31,202	0.289	0.218	0.225	0.130	0.379
Bonus intensity	31,202	0.115	0.008	0.162	0.000	0.198
Cash intensity	31,202	0.404	0.319	0.288	0.168	0.587
CEO characteristics						
CEO age	30,985	55.625	56.000	7.446	51.000	60.000
CEO tenure	29,143	1.769	1.792	0.877	1.099	2.398
CEO ownership	30,427	0.024	0.004	0.056	0.001	0.015
Firm characteristics						
$(A \rightarrow NA)$ *exposure*	31,202	0.605	0.000	2.376	0.000	0.000
Unexp. $(A \rightarrow NA)$ *exposure*	31,202	0.547	0.000	2.253	0.000	0.000
Exp. $(A \rightarrow NA)$ *exposure*	31,202	0.512	0.000	2.211	0.000	0.000
$(A \rightarrow NA)$ *exposure (non-zero)*	2,307	8.222	9.146	3.619	6.096	10.808
Unexp. $(A \rightarrow NA)$ *exposure (non-zero)*	2,307	7.440	8.653	4.109	4.635	10.513
Exp. $(A \rightarrow NA)$ *exposure (non-zero)*	2,307	3.645	0.000	5.005	0.000	9.074
Firm size	31,202	7.278	7.133	1.591	6.132	8.285
Book-to-market	31,202	0.606	0.595	0.257	0.414	0.783
ROA	31,154	0.144	0.140	0.101	0.097	0.191
Leverage	31,195	0.211	0.201	0.171	0.051	0.327
Cash	31,197	0.153	0.086	0.171	0.027	0.221
Sales growth	31,179	0.168	0.086	2.085	0.008	0.199
Stock return	31,120	0.212	0.118	0.695	-0.115	0.380
Stock volatility	31,111	0.111	0.097	0.062	0.070	0.135

This table reports summary statistics over the sample period from fiscal year 1993 to 2019. Std. dev. displays the standard deviation, P25 the first and P75 the third quartile of the respective variable. Variable definitions are presented in Table A.1 in Appendix A.

Table 2
The effect of nonattainment exposure on CEO incentive compensation.

Dep. variable: *Vega*	(1)	(2)	(3)	(4)	(5)	(6)
($A \rightarrow NA$) exposure	-3.326***		-5.596***		-3.598***	
	(-2.63)		(-3.83)		(-2.73)	
Unexp. ($A \rightarrow NA$) exposure		-3.237***		-4.199***		-3.130***
		(-2.58)		(-3.56)		(-2.84)
Exp. ($A \rightarrow NA$) exposure		-1.901		-3.370		-1.370
		(-1.22)		(-1.42)		(-0.68)
CEO age			-0.901	-0.902	-1.255**	-1.269**
			(-1.18)	(-1.18)	(-1.96)	(-1.98)
CEO tenure			30.004***	30.021***	33.924***	33.978***
			(6.57)	(6.57)	(9.42)	(9.43)
CEO ownership			-31.619	-31.821	94.795	94.749
			(-0.26)	(-0.26)	(0.76)	(0.76)
Firm size			78.189***	77.997***	64.690***	64.665***
			(7.07)	(7.05)	(8.43)	(8.44)
Book-to-market			-91.899***	-91.142***	-103.629***	-103.226***
			(-5.62)	(-5.59)	(-8.56)	(-8.55)
ROA			19.208	19.992	22.841	23.427
			(0.55)	(0.58)	(1.14)	(1.17)
Leverage			-56.405*	-57.015*	-79.683***	-79.943***
			(-1.86)	(-1.88)	(-3.78)	(-3.78)
Cash			-0.984	-0.642	5.688	5.580
			(-0.02)	(-0.02)	(0.20)	(0.19)
Sales growth			-0.131	-0.131	-0.095	-0.095
			(-1.03)	(-1.04)	(-0.86)	(-0.87)
Stock return			-9.228***	-9.256***	-10.025***	-10.039***
			(-3.39)	(-3.40)	(-3.98)	(-3.98)
Stock volatility			-62.960	-63.058	-18.510	-18.728
			(-1.59)	(-1.59)	(-0.68)	(-0.69)
Firm F.E.	Yes	Yes	Yes	Yes	No	No
Year F.E.	Yes	Yes	Yes	Yes	No	No
Firm × Cohort F.E.	No	No	No	No	Yes	Yes
Year × Cohort F.E.	No	No	No	No	Yes	Yes
Observations	31,089	31,089	28,054	28,054	26,888	26,888
Adj R^2	0.55	0.55	0.50	0.50	0.65	0.65

This table reports coefficients from fixed effects panel regressions of CEO portfolio vega on nonattainment exposure. The sample period is fiscal year 1993 to 2019. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. ($A \rightarrow NA$) *exposure* measures a firm's time-varying exposure to nonattainment designations based on the geographic distribution of its plants across counties that switch from attainment to nonattainment and the amount of ozone emissions at each plant. *Unexp.* ($A \rightarrow NA$) *exposure* and *Exp.* ($A \rightarrow NA$) *exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for ($A \rightarrow NA$) *exposure*, *Unexp.* ($A \rightarrow NA$) *exposure*, and *Exp.* ($A \rightarrow NA$) *exposure* are given in Equations (1), (3), and (4), respectively. For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table 3
Propensity score matching and weighting models.

	Matched sample		Weighted least squares	
Dep. variable: *Vega*	(1)	(2)	(3)	(4)
$(A \rightarrow NA)$ *exposure*	-4.347***		-4.571***	
	(-2.95)		(-3.65)	
Unexp. $(A \rightarrow NA)$ *exposure*		-3.152***		-3.535***
		(-2.69)		(-3.39)
Exp. $(A \rightarrow NA)$ *exposure*		-2.024		-2.770
		(-0.85)		(-1.33)
Controls	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Observations	15,386	15,386	28,054	28,054
Adj R^2	0.55	0.55	0.51	0.51

This table reports coefficients from firm and year fixed effects panel regressions of CEO portfolio vega on nonattainment exposure using propensity score matching and weighting techniques. The sample period is fiscal year 1993 to 2019. In columns (1) and (2), we match firms with non-zero nonattainment exposure ("treated") to those with no exposure ("control") using one-to-one nearest neighbor propensity score matching with replacement (Roberts & Whited, 2013). In columns (3) and (4), we use weighted least squares regression with propensity score-derived weights, as in Caliendo and Kopeinig (2008). To generate the propensity score, $\widehat{p}$, we estimate a logistic regression where the dependent variable is one if the firm-year belongs to the treated group, and zero otherwise, and the independent variables are the control variables in Table 2. Firm-year observations in the treated group receive a weight of $1/\widehat{p}$, while those in the control group receive a weight of $1/(1-\widehat{p})$. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. $(A \rightarrow NA)$ *exposure* measures a firm's time-varying exposure to nonattainment designations based on the geographic distribution of its plants across counties that switch from attainment to nonattainment and the amount of ozone emissions at each plant. *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for $(A \rightarrow NA)$ *exposure*, *Unexp.* $(A \rightarrow NA)$ *exposure*, and *Exp.* $(A \rightarrow NA)$ *exposure* are given in Equations (1), (3), and (4), respectively. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility*. For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table 4
Alternative difference-in-differences estimator with heterogeneous treatment effects.

Dep. variable: *Vega*	(1)	(2)	(3)
$(A \rightarrow NA)$ *exposed*	-13.893**		
	(-1.98)		
Unexp. $(A \rightarrow NA)$ *exposed*		-11.757**	
		(-1.99)	
Exp. $(A \rightarrow NA)$ *exposed*			20.831
			(1.62)
Pretrend(-2)	-12.669	-3.295	12.416
	(-1.08)	(-0.42)	(0.80)
Pretrend(-3)	0.443	-4.053	-11.648
	(0.05)	(-0.47)	(-0.50)
Pretrend(-4)	10.818	8.356	21.217
	(1.28)	(1.27)	(1.11)
Controls	Yes	Yes	Yes
Observations	23,215	23,215	20,881
p-value: All pretrends are zero	0.183	0.176	0.211

This table reports the results using the difference-in-differences estimator developed by de Chaisemartin and D'Haultfoeuille (2020, 2022), which addresses the issues of treatment effect heterogeneity and negative weights that may bias the standard two-way fixed effects estimator. The sample period is fiscal year 1993 to 2019. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. $(A \rightarrow NA)$ *exposed* is a dummy variable equal to one for firm-year observations with non-zero nonattainment exposure, and zero otherwise. *Unexp.* $(A \rightarrow NA)$ *exposed* and *Exp.* $(A \rightarrow NA)$ *exposed* are dummy variables equal to one for firm-year observations with non-zero exposure to unexpected and expected nonattainment designations, respectively, and zero otherwise. *Pretrend(-k)* is the placebo estimator of de Chaisemartin and D'Haultfoeuille (2020) that estimates the pre-trends k years before exposure to nonattainment designations. The omitted category is $k = -1$. We also provide the p-value of the joint test that all pre-trend estimators are equal to zero. Control variables include *CEO age*, *CEO tenure*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Sales growth*, and *Stock return*. For all specifications, standard errors are clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table 5
The effect of nonattainment exposure on flow vega, options granted, and options exercised.

Dep. variable:	*Flow vega*		*Number of options granted*		*Value of options exercised*		*Number of options exercised*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$(A \rightarrow NA)$ exposure	-0.393**		-0.027***		-9.444		-0.001	
	(-2.27)		(-3.02)		(-0.46)		(-0.11)	
Unexp. $(A \rightarrow NA)$ exposure		-0.423**		-0.051***		-0.790		0.002
		(-2.34)		(-4.79)		(-0.04)		(0.33)
Exp. $(A \rightarrow NA)$ exposure		-0.286		-0.009		-2.811		-0.005
		(-1.10)		(-1.04)		(-0.10)		(-0.78)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,049	28,049	27,768	27,768	27,764	27,764	27,779	27,779
Adj R^2	0.56	0.56	0.29	0.29	0.23	0.23	0.19	0.19

This table reports results from firm and year fixed effects panel regressions describing changes in a CEO's portfolio of option holdings driven by nonattainment exposure. The sample period is fiscal year 1993 to 2019. The dependent variables are the dollar (in thousands) change in the value of the CEO's current option grants for a 0.01 increase in the annualized standard deviation of a firm's stock returns (*Flow vega*), the number of options granted to the CEO in the current year multiplied by one thousand divided by shares outstanding (*Number of options granted*), the dollar (in thousands) value of options exercised by the CEO in the current year (*Value of options exercised*), and the number of options exercised by the CEO in the current year multiplied by one thousand divided by shares outstanding (*Number of options exercised*). *$(A \rightarrow NA)$ exposure* measures a firm's time-varying exposure to nonattainment designations based on the geographic distribution of its plants across counties that switch from attainment to nonattainment and the amount of ozone emissions at each plant. *Unexp. $(A \rightarrow NA)$ exposure* and *Exp. $(A \rightarrow NA)$ exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for *$(A \rightarrow NA)$ exposure*, *Unexp. $(A \rightarrow NA)$ exposure*, and *Exp. $(A \rightarrow NA)$ exposure* are given in Equations (1), (3), and (4), respectively. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility.* For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table 6

The effect of nonattainment exposure on CEO compensation structure.

Dep. variable:	*Total pay*		*Option intensity*		*Salary intensity*		*Bonus intensity*		*Cash intensity*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$(A \to NA)$ *exposure*	0.001		-0.002**		-0.001		0.001***		0.000	
	(0.29)		(-2.06)		(-1.37)		(2.71)		(0.41)	
Unexp. $(A \to NA)$ *exposure*		0.001		-0.003**		-0.001		0.002***		0.000
		(0.36)		(-2.35)		(-1.37)		(3.19)		(0.52)
Exp. $(A \to NA)$ *exposure*		0.000		-0.001		-0.000		-0.000		0.000
		(0.14)		(-0.73)		(-0.30)		(-0.19)		(0.11)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,054	28,054	27,841	27,841	28,054	28,054	28,054	28,054	28,054	28,054
Adj R^2	0.68	0.68	0.41	0.41	0.48	0.48	0.46	0.46	0.50	0.50

This table reports coefficients from firm and year fixed effects panel regressions of the structure of CEO compensation on nonattainment exposure. The sample period is fiscal year 1993 to 2019. The dependent variables are the logarithm of one plus the CEO's total compensation (in thousands) (*Total pay*), the proportion of total annual CEO compensation that comes from option grants (*Option intensity*), and the proportion of total annual CEO compensation that comes from salary (*Salary intensity*), bonuses (*Bonus intensity*), and the sum of salary and bonuses (*Cash intensity*). $(A \to NA)$ *exposure* measures a firm's time-varying exposure to nonattainment designations based on the geographic distribution of its plants across counties that switch from attainment to nonattainment and the amount of ozone emissions at each plant. *Unexp.* $(A \to NA)$ *exposure* and *Exp.* $(A \to NA)$ *exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for $(A \to NA)$ *exposure*, *Unexp.* $(A \to NA)$ *exposure*, and *Exp.* $(A \to NA)$ *exposure* are given in Equations (1), (3), and (4), respectively. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility.* For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table 7
Impact of regulation intensity on the relation between nonattainment exposure and CEO incentive compensation.

Dep. variable: *Vega*	(1)	(2)	(3)	(4)
Z =	*Close monitor*	*Young plant*	*HPV*	*Enforcement*
Unexp. $(A \rightarrow NA)$ *exposure*	-4.113***	-4.063***	-3.339**	-2.360**
	(-3.28)	(-3.99)	(-2.41)	(-2.03)
Exp. $(A \rightarrow NA)$ *exposure*	-0.902	-1.143	-1.410	-1.609
	(-0.35)	(-0.93)	(-1.50)	(-1.25)
Z	45.274*	-1.899	14.655	23.369
	(1.69)	(-0.18)	(0.71)	(0.94)
Unexp. $(A \rightarrow NA)$ *exposure* $\times Z$	-10.758***	-3.551**	-5.282***	-5.622**
	(-2.67)	(-2.01)	(-2.68)	(-2.00)
Exp. $(A \rightarrow NA)$ *exposure* $\times Z$	-0.741	2.177	1.823	0.771
	(-0.28)	(1.50)	(0.55)	(0.22)
Controls	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Observations	28,054	28,054	28,054	28,054
Adj R^2	0.50	0.56	0.56	0.55

This table contains models that analyze the impact of regulation intensity on the relation between nonattainment exposure and CEO portfolio vega. The sample period is fiscal year 1993 to 2019. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. The measures of regulation intensity are a dummy variable equal to one if a firm operates ozone-emitting plants located within one mile of an ozone air quality monitor in a nonattainment county, and zero otherwise (*Close monitor*), a dummy variable equal to one if a firm operates ozone-emitting plants that are between zero and five years of age in nonattainment counties, and zero otherwise (*Young plant*), a dummy variable equal to one if a firm experiences a high priority violation in the past three years among ozone-emitting plants in nonattainment counties, and zero otherwise (*HPV*), and a dummy variable equal to one if a firm experiences a judicial or administrative enforcement case in the past three years among ozone-emitting plants in nonattainment counties, and zero otherwise (*Enforcement*). *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* are given in Equations (3) and (4), respectively. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility*. For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table 8
Impact of financial constraints on the relation between nonattainment exposure and CEO incentive compensation.

Dep. variable: *Vega*	(1)	(2)	(3)	(4)
FC index =	*KZ index*	*WW index*	*HM index*	*CHS index*
Unexp. $(A \rightarrow NA)$ *exposure*	-11.759***	-8.836***	-12.430***	-10.709***
	(-2.89)	(-3.13)	(-3.15)	(-2.88)
Exp. $(A \rightarrow NA)$ *exposure*	-1.172	0.356	0.573	-2.033
	(-0.28)	(0.10)	(0.12)	(-0.38)
FC index	2.350	6.151	-25.929	0.642
	(0.74)	(0.15)	(-0.72)	(0.10)
Unexp. $(A \rightarrow NA)$ *exposure* × *FC index*	2.294**	28.649**	26.443**	3.024**
	(2.01)	(2.03)	(2.03)	(1.97)
Exp. $(A \rightarrow NA)$ *exposure* × *FC index*	-0.463	-22.764	-4.382	-0.500
	(-0.40)	(-1.24)	(-0.24)	(-0.22)
Controls	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Observations	28,019	27,663	16,357	26,301
Adj R^2	0.59	0.59	0.56	0.50

This table contains models that analyze the impact of firms' financial constraints on the relation between nonattainment exposure and CEO portfolio vega. The sample period is fiscal year 1993 to 2019, except for column (3) where the sample period is fiscal year 1997 to 2015. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. The measures of financial constraints are the Kaplan-Zingales (*KZ index*), Whited-Wu (*WW index*), Hoberg-Maksimovic (*HM index*), and Campbell-Hilscher-Szilagyi (*CHS index*) indices. We normalize each index so that it begins from zero. *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* are given in Equations (3) and (4), respectively. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility.* For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table 9

Impact of CEO entrenchment, institutional investors, CEO bargaining power, and CEO type on the relation between nonattainment exposure and CEO incentive compensation.

	CEO entrenchment			Institutional investors		CEO bargaining power		CEO type	
Dep. variable: *Vega*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Z =$	*E-index*	*CEO duality*	*Co-option*	*IO DED*	*IO TRA*	*Pay slice 1*	*Pay slice 3*	*Confidence*	*Holder67*
Unexp. $(A \rightarrow NA)$ *exposure*	-9.389***	-5.823***	-7.011***	-2.121*	-4.343***	-9.314***	-10.515***	-2.971*	-2.450***
	(-3.13)	(-3.92)	(-2.95)	(-1.72)	(-3.08)	(-4.10)	(-4.61)	(-1.69)	(-3.05)
Exp. $(A \rightarrow NA)$ *exposure*	-2.294	0.206	0.262	-0.839	-5.786***	-2.337	-2.633	-2.287	-1.647
	(-0.55)	(0.11)	(0.06)	(-0.40)	(-2.62)	(-0.64)	(-0.72)	(-0.85)	(-1.62)
Z	-0.011	-19.892***	-17.439	45.177**	-85.700***	5.555***	11.119***	-143.076***	-10.245***
	(-0.00)	(-3.32)	(-1.64)	(2.16)	(-6.35)	(3.68)	(3.47)	(-8.53)	(-2.75)
Unexp. $(A \rightarrow NA)$ *exposure* $\times$ Z	1.543**	4.100**	7.330*	-16.305*	24.565***	2.617***	7.209***	-9.918*	-3.508***
	(2.02)	(2.16)	(1.82)	(-1.81)	(2.87)	(3.18)	(3.60)	(-1.80)	(-2.62)
Exp. $(A \rightarrow NA)$ *exposure* $\times$ Z	-0.139	-2.525	0.684	-5.552	12.307*	-0.522	-0.859	-4.927	0.680
	(-0.11)	(-1.08)	(0.10)	(-0.66)	(1.88)	(-0.53)	(-0.35)	(-0.99)	(0.53)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,640	22,158	17,139	27,758	27,758	27,758	27,797	23,863	27,797
Adj R^2	0.50	0.69	0.69	0.56	0.57	0.50	0.50	0.53	0.56

This table contains models that analyze the impact of firms' corporate governance, CEO bargaining power, and CEO type on the relation between nonattainment exposure and CEO portfolio vega. The sample period is fiscal year 1993 to 2019. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. The measures of corporate governance are the total number of anti-takeover provisions a firm has in a given year, including staggered boards, limits to shareholder bylaw amendments, poison pills, golden parachutes, and supermajority requirements for mergers and charter amendments (*E-index*) (Bebchuk et al., 2009), a dummy variable equal to one if a firm's CEO also serves as the chairperson of the board in a given year, and zero otherwise (*CEO duality*) (Adams et al., 2005), the number of CEO appointed directors divided by the total number of board members for a firm in a given year (*Co-option*) (Coles et al., 2014), and the fraction of a firm's shares held by dedicated (*IO DED*) and transient (*IO TRA*) institutional investors following Bushee and Noe's (2000) classification. The measures of CEO bargaining power are the ratio of total CEO compensation to the highest compensation earned by any other executive in the firm (*Pay slice 1*) (Bebchuk et al., 2011) and the CEO's total compensation scaled by the sum of the total compensation of the top-three highest remunerated non-CEO executives (*Pay slice 3*) (Bebchuk et al., 2011). The measures of CEO type are a measure of how in-the-money the CEO's vested stock options are (*Confidence*) (Banerjee et al., 2015) and a measure of CEO overconfidence (*Holder67*) (Humphery-Jenner et al., 2016). *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* are given in Equations (3) and (4), respectively. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility.* For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Appendix A: Variable definitions

Table A.1
Variable definitions.

Variable	Definitions	Data source
Vega	The dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns (Core & Guay, 2002).	ExecuComp
Flow vega	The dollar (in thousands) change in the value of the CEO's current option grants for a 0.01 increase in the annualized standard deviation of a firm's stock returns.	ExecuComp
Number of options granted	The number of options granted to the CEO in the current year multiplied by one thousand divided by shares outstanding.	ExecuComp
Value of options exercised	The dollar (in thousands) value of options exercised by the CEO in the current year.	ExecuComp
Number of options exercised	The number of options exercised by the CEO in the current year multiplied by one thousand divided by shares outstanding.	ExecuComp
Total pay	The logarithm of one plus the CEO's total compensation (in thousands), consisting of salary, bonuses, value of restricted stocks granted, value of options granted, long-term incentive awards, and other types of compensation.	ExecuComp
Option intensity	The proportion of total annual CEO compensation that comes from option grants.	ExecuComp
Salary intensity	The proportion of total annual CEO compensation that comes from salary.	ExecuComp
Bonus intensity	The proportion of total annual CEO compensation that comes from bonuses.	ExecuComp
Cash intensity	The proportion of total annual CEO compensation that comes from salary and bonuses.	ExecuComp
$(A \rightarrow NA)$ *exposure*	For a given firm i, we measure its exposure to nonattainment designations in year t as $$\ln\left(1+\sum_j ozone_{j,i,t-1} \cdot (A \rightarrow NA)_{j,i,t}\right),$$ where $ozone_{j,i,t-1}$ is the total amount of ozone air emissions for plant j of firm i in year $t-1$ and $(A \rightarrow NA)_{j,i,t}$ is a dummy variable equal to one if plant j of firm i is located in a county that switches from attainment to nonattainment in year t, and zero otherwise.	TRI; Federal Register
Unexp. $(A \rightarrow NA)$ *exposure*	The same expression as $(A \rightarrow NA)$ *exposure* except $(A \rightarrow NA)_{j,i,t}$ is replaced with *Unexp.* $(A \rightarrow NA)_{j,i,t}$, which is a dummy variable equal to one if plant j of firm i is located in a county that unexpectedly switches from attainment to nonattainment in year t, and zero otherwise.	TRI; Federal Register; AQS
Exp. $(A \rightarrow NA)$ *exposure*	The same expression as $(A \rightarrow NA)$ *exposure* except $(A \rightarrow NA)_{j,i,t}$ is replaced with *Exp.* $(A \rightarrow NA)_{j,i,t}$, which is a dummy variable equal to one if plant j of firm i is located in a county that expectedly switches from attainment to nonattainment in year t, and zero otherwise.	TRI; Federal Register; AQS
CEO age	The CEO's age (in years).	ExecuComp
CEO tenure	The logarithm of one plus the number of years the CEO has been in office.	ExecuComp
CEO ownership	The CEO's ownership in the firm. This is derived by dividing the CEO's stock ownership by shares outstanding.	ExecuComp
Firm size	The logarithm of one plus the book value of assets (at).	Compustat
Book-to-market	Book-to-market ratio ($at/(at - ceq + prcc\_f \times csho)$).	Compustat
ROA	Net income divided by total assets (ni/at).	Compustat
Leverage	Total liabilities divided by total assets ($(dltt + dlc)/at$).	Compustat
Cash	Cash divided by total assets (che/at).	Compustat
Sales growth	The logarithm of current year sales divided by previous year sales ($\log(sale_t/sale_{t-1})$).	Compustat
Stock return	The annual stock return of the firm.	CRSP
Stock volatility	The standard deviation of stock returns over the past 12 months.	CRSP

Table A.1 continued

Variable	Definitions	Data source
Close monitor	A dummy variable equal to one if a firm operates ozone-emitting plants located within one mile of an ozone air quality monitor in a nonattainment county, and zero otherwise.	TRI; AQS
Young plant	A dummy variable equal to one if a firm operates ozone-emitting plants that are between zero and five years of age in nonattainment counties, and zero otherwise.	NETS; TRI
HPV	A dummy variable equal to one if a firm experiences a high priority violation in the past three years among ozone-emitting plants in nonattainment counties, and zero otherwise.	ICIS-Air; TRI; Federal Register
Enforcement	A dummy variable equal to one if a firm experiences a judicial or administrative enforcement case in the past three years among ozone-emitting plants in nonattainment counties, and zero otherwise.	FE&C; TRI; Federal Register
KZ index	Kaplan-Zingales index defined as $-1.002[(dp+ib)/at] - 39.368[(dvc+dvp)/at] - 1.315[che/at] + 3.139[(dltt+dlc)/(dltt+dlc+teq)]$. Normalized to begin from zero.	Compustat
WW index	Whited-Wu index defined as $-0.091[(ib+dp)/at] - 0.062$dividend indicator $+ 0.021[dltt/at] - 0.044\log(at) + 0.102$three-digit SIC industry sales growth $- 0.035$sales growth. Normalized to begin from zero.	Compustat
HM index	Hoberg-Maksimovic index normalized to begin from zero.	Hoberg-Maksimovic Financial Constraints Repository
CHS index	Campbell-Hilscher-Szilagyi index defined as $-20.26NIMTAAVG + 1.42TLMTA - 7.13EXRETAVG + 1.41SIGMA - 0.045RSIZE - 2.13CASHMTA + 0.075MB - 0.058PRICE - 9.16$, where $NIMTAAVG$, $TLMTA$, and $CASHMTA$ are the geometrically decreasing average of quarterly net income, total liabilities, and cash plus short-term investments, respectively, all divided by the sum of the market value of equity and total liabilities; $EXRETAVG$ is the difference between a firm's 1-year average monthly raw return and the S&P 500 monthly return; $SIGMA$ is the annualized 3-month return standard deviation; $RSIZE$ is the ratio of a firm's market value of equity to the total S&P 500 market value; MB is the ratio of the market value of equity to the book value of equity; and $PRICE$ is the stock price winsorized at \$15. Normalized to begin from zero.	Compustat
E-index	The total number of anti-takeover provisions a firm has in a given year, including staggered boards, limits to shareholder bylaw amendments, poison pills, golden parachutes, and supermajority requirements for mergers and charter amendments.	Bebchuk et al.'s (2009) website
CEO duality	A dummy variable equal to one if a firm's CEO also serves as the chairperson of the board in a given year, and zero otherwise (Adams et al., 2005).	ExecuComp
Co-option	The number of CEO appointed directors divided by the total number of board members for a firm in a given year (Coles et al., 2014).	RiskMetrics
IO DED	The fraction of a firm's shares held by dedicated institutional investors following Bushee and Noe's (2000) classification.	Bushee and Noe's (2000) website; Thomson Reuters s34
IO TRA	The fraction of a firm's shares held by transient institutional investors following Bushee and Noe's (2000) classification.	Bushee and Noe's (2000) website; Thomson Reuters s34
Pay slice 1	The ratio of total CEO compensation to the highest compensation earned by any other executive in the firm (Bebchuk et al., 2011).	ExecuComp
Pay slice 3	The CEO's total compensation scaled by the sum of the total compensation of the top-three highest remunerated non-CEO executives (Bebchuk et al., 2011).	ExecuComp
Confidence	A measure of how in-the-money the CEO's vested stock options are following Banerjee et al. (2015).	ExecuComp
Holder67	A dummy variable equal one if the CEO fails to exercise options with five years remaining duration despite a 67% or higher increase in stock price since the grant date, and zero otherwise (Malmendier et al., 2011).	ExecuComp

Internet Appendix For Online Publication Only

IA. Regression discontinuity design

Formally, we perform the RDD by using a nonparametric, local linear estimation. Small neighborhoods on the left- and right-hand sides of the NAAQS threshold are used to estimate discontinuities in nonattainment probability. We follow Calonico et al. (2014) to derive the asymptotically optimal bandwidth under a squared-error loss. The choices of the neighborhood (bandwidth) are data-driven (determined by the data structure) and different across samples and variables. By choosing the optimal bandwidth to the left and right of the threshold, we only include observations in the estimation if the absolute difference between the DV for that observation and the threshold is less than the bandwidth. The local linear regression model can therefore be specified as

$$NA_{c,t+1} = \alpha + \beta Noncompliance_{c,t} + \phi f(R_{c,t}) + \varepsilon_{c,t+1} \tag{IA.1}$$

for county c and year t. $NA_{c,t+1}$ is a dummy variable equal to one if county c is designated nonattainment in year $t+1$, and zero otherwise. $Noncompliance_{c,t}$ is a dummy variable equal to one if county c's DV is in violation of the NAAQS threshold in year t, and zero otherwise. $R_{c,t}$ is the centered DV (i.e., the running variable in RDD parlance), defined as the difference between the DV of county c in year t and the NAAQS threshold. Negative (positive) values indicate that the county is in compliance with (violation of) the NAAQS threshold. We use local linear functions in the running variable with rectangular kernels as represented by $f(R_{c,t})$. Since treatment assignment is at the county-level, standard errors are clustered by county and bias-corrected as discussed in Calonico et al. (2014).

The identifying assumption of the RDD is that, around the NAAQS threshold, a county's designation status is as good as randomly assigned. In the following sections, we perform two standard tests for the RDD validity that counties cannot precisely manipulate the running variable so that their DVs are right below the NAAQS threshold (Lee & Lemieux, 2010). If this assumption is satisfied, then the variation in a county's designation status around the NAAQS threshold should be as good as that from a randomized experiment.

IA.1. Continuity in the distribution of design values

Since being classified as nonattainment imposes costly regulatory actions to curb emissions, counties have a strong incentive to keep pollution levels below the threshold. Thus, one potential concern is that counties just above the threshold might try to manipulate their monitored ozone concentrations in order to be right below the threshold to avoid noncompliance. The first test that we conduct evaluates whether the distribution of DVs is continuous around the NAAQS threshold. Any discontinuity would suggest a nonrandom assignment of attainment versus nonattainment status around the threshold.

In practice, however, it is unlikely that counties could strategically manipulate their DVs. Since all counties are evaluated on the same standards, the EPA's federal enforcement power limits the states' ability to overlook non-compliers. Additionally, studies show that nonattainment designations often depend on weather patterns (Cleveland & Graedel, 1979; Cleveland, Kleiner, McRae, & Warner, 1976). Combined with the fact that ozone emissions are a result of complex chemical reactions in the atmosphere between pollutants such as volatile organic compounds and nitrogen oxides, it is extremely difficult for counties to manipulate their ozone concentration levels precisely around the NAAQS threshold. Lastly, ozone emissions that contribute to a county's DV not only originate from stationary sources such as the facilities examined in this paper, but also from mobile pollution sources (such as those from vehicles). For example, using the 2017 National Emissions Inventory Data provided by the EPA, we estimate that 83% of national non-biogenic ozone emissions come from non-point sources. Thus, even if there were a coordinated effort to manipulate ozone emissions by a group of facilities, it would still be unlikely to influence the DV of the entire county given other non-stationary emission sources.

Internet Appendix Figure IA.2 plots the local density of centered DVs, estimated separately on either side of the NAAQS threshold with the corresponding 95% confidence interval bounds, calculated using the plug-in estimator proposed by Cattaneo, Jansson, and Ma (2020). Observations on the left (right) of the vertical dashed line indicate that the county is in compliance with (violation of) the NAAQS threshold. If counties were manipulating their DVs to strategically avoid nonattainment designations, one would expect to see a bunching of counties just below the NAAQS thresholds. As shown in the figure, there is no evidence for a discontinuous jump around the threshold. Using the density break test following Cattaneo et al. (2020),[17] we fail to reject the null hypothesis that counties are unable to manipulate their pollution levels in order to be right below the NAAQS threshold (p-value = 0.943).

IA.2. Preexisting differences

The second testable implication of the randomness assumption is that the polluting facilities in counties whose DVs are immediately below or above the threshold should be very similar on the basis of ex ante characteristics. In other words, if a county's designation status is as good as randomized, it should be orthogonal to facility characteristics prior to the designation. In Internet Appendix Table IA.2, we examine whether there are any preexisting differences between plants operating in counties that violate and comply with the thresholds. The variables that we examine include a dummy variable equal to one if a plant emits ozone core chemicals as defined by TRI, and zero otherwise (*Core chemical*);[18] a dummy variable equal to one if a plant holds operating permits for ozone emissions, and zero otherwise (*Permit*); the logarithm of one plus the total amount (in pounds) of ozone source reduction activities that a plant engages in (*ln(Source reduction)*); the plant's ozone production ratio (*Production ratio*); the logarithm of one plus the number of employees at the plant (*ln(Employees)*); the logarithm of one plus the dollar amount of sales at the plant (*ln(Sales)*); the plant's minimum paydex score in a given year (*Paydex*);[19] a dummy variable equal to one if a plant experiences a high priority violation in the past three years, and zero otherwise (*HPV*); and a dummy variable equal to one if a firm experiences an enforcement case in the past three years, and zero otherwise (*Enforcement*).

In column (1) of Internet Appendix Table IA.2, we examine these characteristics in the year preceding the designation ($t-1$). In column (2), we examine the change in these characteristics between years $t-2$ and $t-1$. Both columns report the differences using a narrow window around the NAAQS threshold by computing the mean squared error optimal bandwidth following Calonico et al. (2014). As can be seen in both columns, there are no systematic or statistically significant differences in facility characteristics in the optimal neighborhood around the threshold, which lends support to our identification strategy.

IA.3. Estimation results

We present the estimation results of Equation (IA.1) in Table IA.3 of the Internet Appendix. The coefficient estimate of β captures the discontinuity at the NAAQS threshold and is equal

[17] The density break test builds upon the more standard density manipulation test by McCrary (2008).

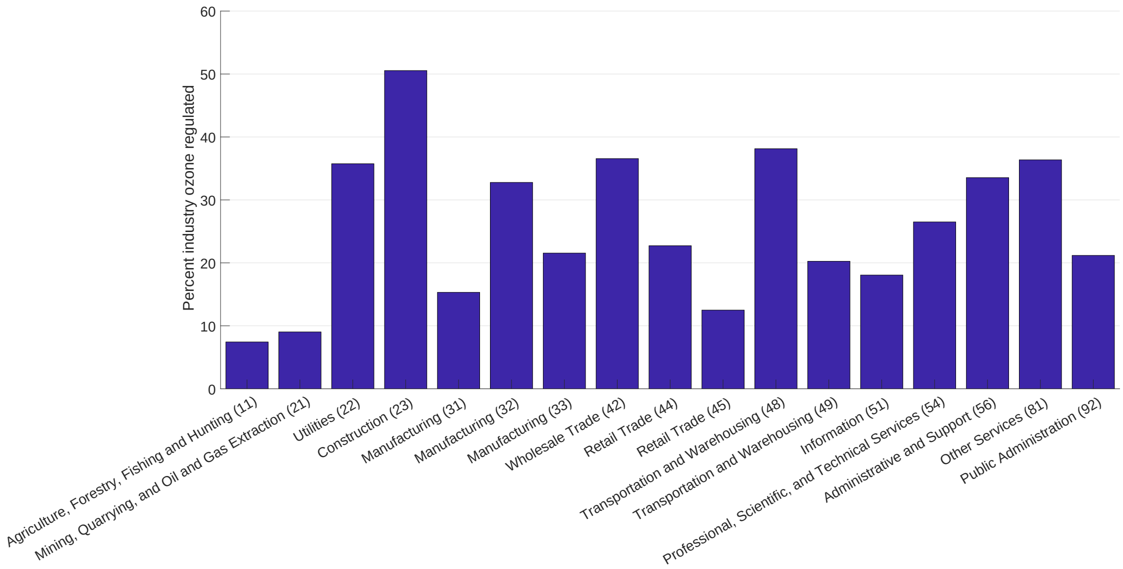
[18] Core chemicals are those that have consistent reporting requirements in TRI.

[19] This variable is obtained from NETS, which represents the facility's trade credit performance on a scale of 0 to 100. Higher paydex scores indicate greater ability to meet contractual repayment obligations.

to the difference in the probability of nonattainment between counties that marginally violate the NAAQS threshold and those that marginally comply with the threshold. In column (1), we estimate the baseline specification without any covariate adjustments. Noncompliance based on DVs leads to an increase in the probability of nonattainment by roughly 74%. In column (2), following Curtis (2020), the point estimates on β and optimal bandwidth selection are covariate-adjusted by including additional county-level covariates such as the natural logarithm of one plus the employment levels in a given county, a given county's NOx emissions to employment ratio, the change in a given county's employment levels, and a dummy variable equal to one if the county is located in a MSA. We obtain qualitatively similar results.

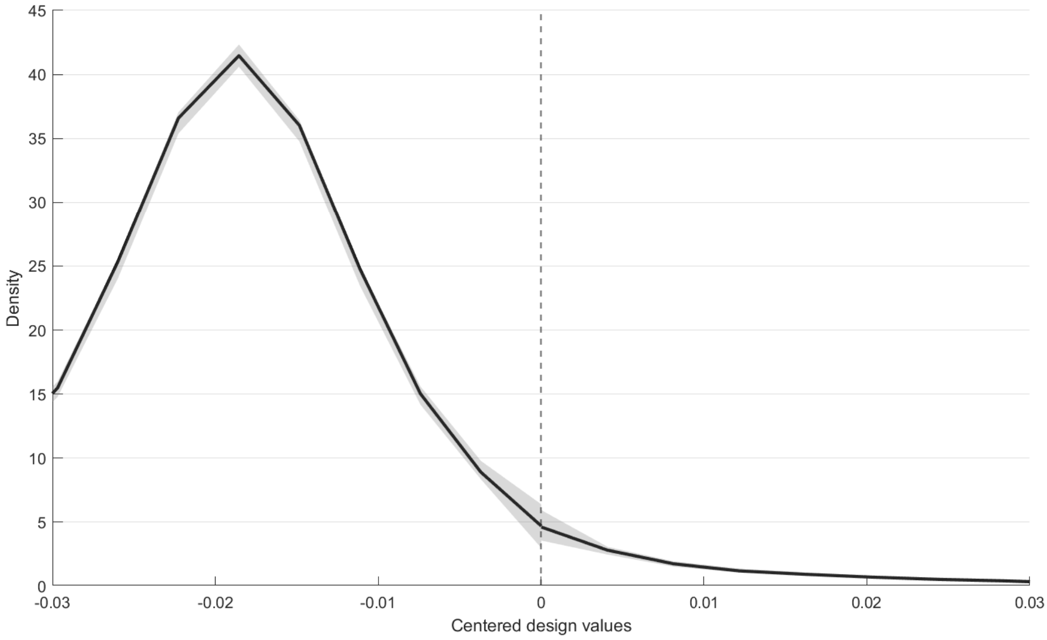
Internet Appendix Table IA.3 also provides the estimates of the optimal bandwidth. The bandwidth estimate of 0.009 in both columns implies that counties with DVs that are within 0.009 ppm of the NAAQS threshold have ozone concentration levels that are as good as randomized. Counties with DVs that exceed the threshold by more than 0.009 ppm are considered to be far "enough" *above* the threshold that they will most likely be designated nonattainment in the following year. Similarly, counties with DVs that are below the threshold by more than 0.009 ppm are considered to be far "enough" *below* the threshold that they will most likely remain in attainment in the following year.

Figure IA.1
Fraction of ozone plants by industry in nonattainment counties.



This figure shows the fraction of ozone-emitting plants by major industry (categorized using two-digit industry NAICS codes) in nonattainment counties.

Figure IA.2
Density break test around NAAQS thresholds.



This figure presents the density of observations by the distance to the ozone NAAQS threshold. The horizontal axis shows the centered DVs around zero by subtracting the NAAQS threshold from the DVs. The dashed vertical line at zero represents the NAAQS threshold for ozone nonattainment status. Observations on the right (left) of the line indicate that the county is in violation of (compliance with) the NAAQS threshold. The solid black lines represent the local density on either side of the NAAQS threshold and the shaded gray area corresponds to the 95% confidence interval bounds, calculated using the plug-in estimator proposed by Cattaneo et al. (2020). We fail to reject the null hypothesis that there is no break in density around the threshold, with a p-value of 0.943.

Table IA.1
Ozone NAAQS.

Standard	Effective date	Averaging time	Threshold (ppm)	Form
1-Hour Ozone (1979)	January 6, 1992	1 hour	0.12	Attainment is defined when the expected number of days per calendar year, with maximum hourly average concentration greater than 0.12 ppm, is equal to or less than 1
8-Hour Ozone (1997)	June 15, 2004	8 hours	0.08	Annual fourth-highest daily maximum 8-hr concentration, averaged over 3 years
8-Hour Ozone (2008)	July 20, 2012	8 hours	0.075	Annual fourth-highest daily maximum 8-hr concentration, averaged over 3 years
8-Hour Ozone (2015)	August 3, 2018	8 hours	0.070	Annual fourth-highest daily maximum 8-hr concentration, averaged over 3 years

This table provides basic descriptions of the ozone NAAQS used in our study. Standard refers to the name of the ozone NAAQS. Effective date is the date on which the standard is effectively implemented as stated in the Federal Register. Averaging time is the sampling frequency of the ozone concentration used to calculate DVs. Threshold refers to the DV value which if exceeded, then the county is considered to be in nonattainment. This value is measured in parts per million (ppm). Form is the rule used to compute the DVs for the relevant ozone standard. Our sample period is from 1993–2019. From 1993 to 2003, we use the 1-Hour Ozone (1979) standard. From 2004 to 2011, we use the 8-Hour Ozone (1997) standard. From 2012 to 2017, we use the 8-Hour Ozone (2008) standard. From 2018 onwards, we use the 8-Hour Ozone (2015) standard. This table is adapted from `https://www.epa.gov/ground-level-ozone-pollution/timeline-ozone-national-ambient-air-quality-standards-naaqs`.

Table IA.2
Preexisting differences in facility characteristics.

	Year $(t-1)$	Δ from year $(t-2)$ to $(t-1)$
	(1)	(2)
Core chemical	-0.022	-0.005
	(0.023)	(0.007)
Permit	-0.004	-0.003
	(0.034)	(0.004)
ln(Source reduction)	-0.100	0.014
	(0.317)	(0.065)
Production ratio	0.001	-0.005
	(0.023)	(0.013)
ln(Employees)	-0.045	-0.014
	(0.067)	(0.030)
ln(Sales)	0.010	-0.091
	(0.070)	(0.105)
Paydex	0.031	-0.273
	(0.355)	(0.204)
HPV	0.004	-0.002
	(0.010)	(0.004)
Enforcement	-0.002	-0.004
	(0.006)	(0.004)
Sample:	Optimal	Optimal

This table examines the differences in observable facility characteristics between those that operate in counties that are in violation of the NAAQS thresholds and those operating in counties that are in compliance. In column (1), these characteristics are measured in the year preceding the designation $(t-1)$. Column (2) considers the change in these characteristics between years $t-2$ and $t-1$. We focus on a narrow window around the NAAQS threshold by computing the mean squared error optimal bandwidth following Calonico et al. (2014). For all specifications, standard errors are clustered by county, bias-corrected following Calonico et al. (2014), and reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table IA.3
Noncompliant design values and probability of nonattainment.

Dep. variable: $NA_{c,t+1}$	(1)	(2)
$Noncompliance_{c,t}$	0.743***	0.721***
	(32.23)	(31.75)
Kernel	Rectangular	Rectangular
Bandwidth type	Optimal	Optimal
Bandwidth estimate	0.009	0.009
Covariates	No	Yes
Observations	7,409	6,723

This table presents the probability of nonattainment designation when a given county's DV is in violation of the NAAQS threshold. We estimate the local linear regression specification given in Equation (IA.1) using the mean squared error optimal bandwidth with rectangular kernels following Calonico et al. (2014). $NA_{c,t+1}$ is a dummy variable equal to one if county c is designated nonattainment in year $t+1$, and zero otherwise. $Noncompliance_{c,t}$ is a dummy variable equal to one if county c's DV is in violation of the NAAQS threshold in year t, and zero otherwise. County-level covariates include the natural logarithm of one plus the employment levels in a given county, a given county's NO_x emissions to employment ratio, the change in a given county's employment levels, and a dummy variable equal to one if the county is located in a MSA. For all specifications, standard errors are clustered by county and bias-corrected following Calonico et al. (2014); t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table IA.4
Differences between firm characteristics using propensity score matching.

	Treatment ($N = 7,873$)	Control ($N = 7,873$)	Difference	
Variables	Mean	Mean	Estimate	p-value
CEO age	56.971	57.065	-0.094	0.664
CEO tenure	1.681	1.690	-0.009	0.722
CEO ownership	0.013	0.013	0.000	0.832
Firm size	7.999	8.010	-0.011	0.890
Book-to-market	0.668	0.676	-0.008	0.386
ROA	0.140	0.136	0.003	0.251
Leverage	0.264	0.265	-0.001	0.840
Cash	0.095	0.091	0.004	0.388
Sales growth	0.085	0.091	-0.006	0.225
Stock return	0.150	0.147	0.003	0.688
Stock volatility	0.097	0.097	0.000	0.957

This table presents the mean firm characteristics across two subsamples based on propensity score matching. We match firm-year observations with non-zero nonattainment exposure ("treated") to those with no exposure ("control") using one-to-one nearest neighbor propensity score matching with replacement (Roberts & Whited, 2013). We test for differences in the means between the two subsamples and provide the p-values. Standard errors are clustered at the firm-level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table IA.5

Alternative measures of nonattainment exposure.

Dep. variable: *Vega*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unexp. ($A \rightarrow NA$) exposure (#Plant)	-5.848***						
	(-3.72)						
Exp. ($A \rightarrow NA$) exposure (#Plant)	-3.481						
	(-1.11)						
Unexp. ($A \rightarrow NA$) exposure (Prod. ratio)		-4.068***					
		(-3.39)					
Exp. ($A \rightarrow NA$) exposure (Prod. ratio)		-3.494					
		(-1.44)					
Unexp. ($A \rightarrow NA$) exposure (Sales)			-6.052***				
			(-3.82)				
Exp. ($A \rightarrow NA$) exposure (Sales)			-4.764				
			(-1.43)				
Unexp. ($A \rightarrow NA$) exposure (Employee)				-6.017***			
				(-3.82)			
Exp. ($A \rightarrow NA$) exposure (Employee)				-4.796			
				(-1.45)			
Unexp. ($A \rightarrow NA$) exposure (TW)					-1.179**		
					(-2.57)		
Exp. ($A \rightarrow NA$) exposure (TW)					-0.878		
					(-1.21)		
Unexp. ($A \rightarrow NA$) exposure (Core)						-4.287***	
						(-3.46)	
Exp. ($A \rightarrow NA$) exposure (Core)						-3.143	
						(-1.31)	
Unexp. ($A \rightarrow NA$) exposure (Permit)							-5.127***
							(-3.49)
Exp. ($A \rightarrow NA$) exposure (Permit)							-2.596
							(-0.71)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,054	28,054	27,554	27,554	28,054	28,054	28,054
Adj R^2	0.50	0.50	0.50	0.50	0.72	0.50	0.50

This table reports coefficients from firm and year fixed effects panel regressions of CEO portfolio vega on alternative measures of nonattainment exposure. The sample period is fiscal year 1993 to 2019. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. *Unexp.* ($A \rightarrow NA$) *exposure (#Plant)* and *Exp.* ($A \rightarrow NA$) *exposure (#Plant)* are constructed by replacing $ozone_{j,i,t-1}$ with $ozone\ (\#Plant)_{j,i,t-1} = (1/\#Plant_{i,t}) \cdot ozone_{j,i,t-1}$, where $\#Plant_{i,t}$ is the total number of polluting plants owned by firm i in year t. *Unexp.* ($A \rightarrow NA$) *exposure (Prod. ratio)* and *Exp.* ($A \rightarrow NA$) *exposure (Prod. ratio)* are constructed by replacing $ozone_{j,i,t-1}$ with $ozone\ (Prod.\ ratio)_{j,i,t-1} = Production\ ratio_{j,t} \cdot ozone_{j,i,t-1}$, where $Production\ ratio_{j,t}$ is plant j's ozone production ratio in year t. *Unexp.* ($A \rightarrow NA$) *exposure (Sales)* and *Exp.* ($A \rightarrow NA$) *exposure (Sales)* are constructed by replacing $ozone_{j,i,t-1}$ with $ozone\ (Sales\ share)_{j,i,t-1} = Sales\ share_{j,i,t} \cdot ozone_{j,i,t-1}$, where $Sales\ share_{j,i,t}$ is plant j's dollar amount of sales in year t divided by the total sales of all polluting plants of firm i in the same year. *Unexp.* ($A \rightarrow NA$) *exposure (Employee)* and *Exp.* ($A \rightarrow NA$) *exposure (Employee)* are constructed by replacing $ozone_{j,i,t-1}$ with $ozone\ (Employees\ share)_{j,i,t-1} = Employees\ share_{j,i,t} \cdot ozone_{j,i,t-1}$, where $Employees\ share_{j,i,t}$ is plant j's number of employees in year t divided by the total employees of all polluting plants of firm i in the same year. *Unexp.* ($A \rightarrow NA$) *exposure (TW)* and *Exp.* ($A \rightarrow NA$) *exposure (TW)* are constructed by replacing $ozone_{j,i,t-1}$ with $ozone\ (TW)_{j,i,t-1} = \sum_c TW_c \cdot ozone_{c,j,i,t-1}$, where TW_c is the inhalation toxicity weight of chemical c derived from the EPA's Risk-Screening Environmental Indicator model. *Unexp.* ($A \rightarrow NA$) *exposure (Core)* and *Exp.* ($A \rightarrow NA$) *exposure (Core)* are constructed by replacing $ozone_{j,i,t-1}$ with $ozone\ (Core\ chemical)_{j,i,t-1} = \sum_c Core\ chemical_c \cdot ozone_{c,j,i,t-1}$, where $Core\ chemical_c$ is a dummy variable equal to one if chemical c is a core chemical, and zero otherwise. *Unexp.* ($A \rightarrow NA$) *exposure (Permit)* and *Exp.* ($A \rightarrow NA$) *exposure (Permit)* are constructed by replacing $ozone_{j,i,t-1}$ with $ozone\ (Permit)_{j,i,t-1} = \sum_c Permit_{c,j,t} \cdot ozone_{c,j,i,t-1}$, where $Permit_{c,j,t}$ is a dummy variable equal to one if plant j holds operating permits to emit chemical c in year t, and zero otherwise. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility*. For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table IA.6
Removing firms with large local emissions.

Cutoff:	99th percentile		95th percentile	
	Point sources	All sources	Point sources	All sources
Dep. variable: *Vega*	(1)	(2)	(3)	(4)
Unexp. $(A \rightarrow NA)$ *exposure*	-4.151***	-4.506***	-3.947***	-4.898***
	(-3.39)	(-3.61)	(-2.95)	(-3.48)
Exp. $(A \rightarrow NA)$ *exposure*	-2.213	-2.932	-2.935	-2.800
	(-0.87)	(-1.11)	(-1.07)	(-1.12)
Controls	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Observations	27,584	27,782	26,550	26,780
Adj R^2	0.50	0.50	0.49	0.50

This table reports coefficients from fixed effects panel regressions of CEO portfolio vega on nonattainment exposure after filtering out firms with large local emissions. The sample period is fiscal year 1993 to 2019. Columns (1) and (3) exclude firm-year observations if the ratio of the firm's ozone emissions to point source emissions in a given county exceeds the 99th or 95th percentiles, respectively. Columns (2) and (4) exclude firm-year observations if the ratio of the firm's ozone emissions to all source emissions in a given county exceeds the 99th or 95th percentiles, respectively. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* are given in Equations (3) and (4), respectively. For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table IA.7
Placebo nonattainment exposure.

Dep. variable: *Vega*	(1)	(2)	(3)	(4)
$(A \rightarrow NA)$ *exposure (offsite)*	-1.740			
	(-1.11)			
Unexp. $(A \rightarrow NA)$ *exposure (offsite)*		-1.518		
		(-0.93)		
Exp. $(A \rightarrow NA)$ *exposure (offsite)*		-2.058		
		(-0.56)		
$(A \rightarrow NA)$ *exposure (PM)*			-2.750	
			(-1.04)	
Unexp. $(A \rightarrow NA)$ *exposure (PM)*				-0.430
				(-0.19)
Exp. $(A \rightarrow NA)$ *exposure (PM)*				-3.414
				(-0.44)
Controls	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Observations	28,054	28,054	28,054	28,054
Adj R^2	0.50	0.50	0.50	0.50

This table reports coefficients from firm and year fixed effects panel regressions of CEO portfolio vega on placebo measures of nonattainment exposure. The sample period is fiscal year 1993 to 2019. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. $(A \rightarrow NA)$ *exposure (offsite)* measures a firm's exposure to nonattainment designations based on offsite ozone emissions. *Unexp.* $(A \rightarrow NA)$ *exposure (offsite)* and *Exp.* $(A \rightarrow NA)$ *exposure (offsite)* are measures of unexpected and expected nonattainment exposure based on offsite ozone emissions. $(A \rightarrow NA)$ *exposure (PM)* measures a firm's exposure to nonattainment designations based on onsite particulate matter emissions. *Unexp.* $(A \rightarrow NA)$ *exposure (PM)* and *Exp.* $(A \rightarrow NA)$ *exposure (PM)* are measures of unexpected and expected nonattainment exposure based on onsite particulate matter emissions. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility*. For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table IA.8
Alternative measures of CEO incentive compensation.

Dep. variable:	$ln(1+Vega)$		$Poisson\ Vega$		$Vega/Delta$	
	(1)	(2)	(3)	(4)	(5)	(6)
$(A \rightarrow NA)$ exposure	-0.016**		-0.023***		-0.004**	
	(-2.13)		(-3.97)		(-2.20)	
Unexp. $(A \rightarrow NA)$ exposure		-0.016**		-0.018***		-0.004***
		(-2.15)		(-3.48)		(-2.61)
Exp. $(A \rightarrow NA)$ exposure		-0.003		-0.001		-0.001
		(-0.36)		(-0.11)		(-0.76)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,054	28,054	27,498	27,498	27,960	27,960
Adj R^2	0.61	0.61	0.76	0.76	0.50	0.50

This table reports coefficients from firm and year fixed effects panel regressions of alternative measures of CEO incentive compensation on nonattainment exposure. The sample period is fiscal year 1993 to 2019. *Vega* measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. *Delta* measures the dollar (in thousands) change in the value of the CEO's portfolio of current option and stock grants and accumulated option and stock holdings for a 1% change in the stock price. Columns (1), (2), (5), and (6) use ordinary least squares regression while columns (3) and (4) use Poisson regression. *$(A \rightarrow NA)$ exposure* measures a firm's time-varying exposure to nonattainment designations based on the geographic distribution of its plants across counties that switch from attainment to nonattainment and the amount of ozone emissions at each plant. *Unexp. $(A \rightarrow NA)$ exposure* and *Exp. $(A \rightarrow NA)$ exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for *$(A \rightarrow NA)$ exposure*, *Unexp. $(A \rightarrow NA)$ exposure*, and *Exp. $(A \rightarrow NA)$ exposure* are given in Equations (1), (3), and (4), respectively. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility.* For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table IA.9
The effect of nonattainment exposure on CEO incentive compensation using only the treatment sample.

Dep. variable: *Vega*	(1)	(2)	(3)	(4)
$(A \rightarrow NA)$ *exposure*	-4.049***		-2.725**	
	(-2.66)		(-2.07)	
Unexp. $(A \rightarrow NA)$ *exposure*		-3.221***		-2.436**
		(-2.61)		(-2.20)
Exp. $(A \rightarrow NA)$ *exposure*		-0.726		-0.083
		(-0.29)		(-0.04)
Controls	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	No	No
Year F.E.	Yes	Yes	No	No
Firm × Cohort F.E.	No	No	Yes	Yes
Year × Cohort F.E.	No	No	Yes	Yes
Observations	7,823	7,823	7,509	7,509
Adj R^2	0.48	0.48	0.61	0.61

This table reports coefficients from fixed effects panel regressions of CEO portfolio vega on nonattainment exposure using only the treatment sample. The sample period is fiscal year 1993 to 2019. The dependent variable, *Vega*, measures the dollar (in thousands) change in the value of the CEO's portfolio of current option grants and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns. $(A \rightarrow NA)$ *exposure* measures a firm's time-varying exposure to nonattainment designations based on the geographic distribution of its plants across counties that switch from attainment to nonattainment and the amount of ozone emissions at each plant. *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for $(A \rightarrow NA)$ *exposure*, *Unexp.* $(A \rightarrow NA)$ *exposure*, and *Exp.* $(A \rightarrow NA)$ *exposure* are given in Equations (1), (3), and (4), respectively. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility.* For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.

Table IA.10

Impact of nonattainment exposure and financial constraints on capital expenditure and R&D investment.

FC index =	*KZ index*		*WW index*		*HM index*		*CHS index*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. variable:	*Capex*	*R&D*	*Capex*	*R&D*	*Capex*	*R&D*	*Capex*	*R&D*
Unexp. $(A \rightarrow NA)$ *exposure*	0.0022	0.0035	0.0001	0.0002	-0.0003	-0.0002	0.0001	0.0002
	(0.30)	(0.90)	(0.47)	(1.45)	(-0.53)	(-0.88)	(0.39)	(1.62)
Exp. $(A \rightarrow NA)$ *exposure*	0.0107	0.0069**	-0.0004	0.0000	0.0002	0.0007*	-0.0002	0.0005***
	(1.04)	(2.01)	(-1.61)	(0.31)	(0.45)	(1.79)	(-0.80)	(3.33)
FC index	0.0006**	0.0003*	-0.0150	-0.0194	0.0011	0.0018	-0.0025**	0.0044***
	(2.37)	(1.85)	(-0.99)	(-1.23)	(0.16)	(0.33)	(-2.39)	(5.84)
Unexp. $(A \rightarrow NA)$ *exposure*	-0.0000	-0.0000	-0.0022	-0.0011	0.0001	0.0006	-0.0004	-0.0001
× *FC index*	(-0.33)	(-0.88)	(-1.14)	(-1.11)	(0.07)	(0.53)	(-1.53)	(-0.87)
Exp. $(A \rightarrow NA)$ *exposure*	-0.0001	-0.0001**	0.0030	0.0010	-0.0056**	-0.0015	-0.0002	-0.0003***
× *FC index*	(-1.07)	(-1.96)	(1.25)	(1.00)	(-2.18)	(-0.93)	(-0.64)	(-2.79)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,019	28,019	27,663	27,663	16,357	16,357	26,301	26,301
Adj R^2	0.71	0.87	0.75	0.87	0.74	0.84	0.71	0.87

This table contains models that analyze the impact of firms' nonattainment exposure and financial constraints on firm investment. The sample period is fiscal year 1993 to 2019, except for columns (5) and (6) where the sample period is fiscal year 1997 to 2015. The dependent variables are firms' capital expenditures (*Capex*) and R&D investment (*R&D*). The measures of financial constraints are the Kaplan-Zingales (*KZ index*), Whited-Wu (*WW index*), Hoberg-Maksimovic (*HM index*), and Campbell-Hilscher-Szilagyi (*CHS index*) indices. We normalize each index so that it begins from zero. *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* decompose a firm's exposure to nonattainment designations into an unexpected and expected component, respectively. The detailed definitions for *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *exposure* are given in Equations (3) and (4), respectively. Control variables include *CEO age*, *CEO tenure*, *CEO ownership*, *Firm size*, *Book-to-market*, *ROA*, *Leverage*, *Cash*, *Sales growth*, *Stock return*, and *Stock volatility*. For all specifications, standard errors are robust to heteroskedasticity and clustered at the firm-level; t-statistics are reported in the parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variable definitions are presented in Table A.1 in Appendix A.