CEO compensation and cash-flow shocks: Evidence from changes in environmental regulations*

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Abstract

This paper investigates how shocks to expected cash flows influence CEO incentive compensation. Exploiting changes in compliance with environmental regulations as shocks to expected future cash flows, we find that adverse shocks typically prompt corporate boards to recalibrate CEO compensation to reduce risk-taking incentives. 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.

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1. Introduction

Foundational theories of the firm suggest that shocks to expected cash flows 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 less risky corporate investments to mitigate bankruptcy risk and, therefore, offer their executives convex compensation packages with reduced risk-taking incentives, i.e., less convex compensation packages. 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 relationship between cash-flow shocks and executive compensation is further complicated by the effectiveness of corporate governance, as the effect of shareholder preferences on 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 expected cash flows on executive compensation is an empirical question that may depend on firms' financial conditions and governance structures.

Despite the importance of understanding how firms respond to shocks, few researchers have empirically examined the impact of expected cash flows on the convexity of executive compensation. Gormley, Matsa, and Milbourn (2013) address critical aspects of this question by examining the link between increased corporate liability risk and the convexity of new equity grants. Yet, there is a lack of evidence on how different financial conditions and governance structures modulate the response of executive compensation to expected cash-flow shocks, which theory highlights as crucial to understanding this relationship.

To address this gap, we examine the impact of changes in environmental regulations that alter expected net cash flows on executive compensation while differentiating by pre-existing corporate financial conditions and governance effectiveness. Research demonstrates that environmental regulations significantly affect firms' expected cash flows, forming the basis of this study (e.g., Choi, Levine, Park, & Xu, 2023; 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 they offer executives. Thus, besides exploiting changes in environmental regulation to provide new evidence on how (1) shocks to expected cash flows influence executive compensation and

(2) firms' pre-existing financial conditions and governance structures shape the response of executive compensation to those shocks, we provide novel information on potentially significant consequences of environmental regulations: shareholders' preferences toward risk-taking and executive compensation.

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. Nonattainment designations lead to more stringent environmental regulations, increasing production costs and reducing expected cash flows on ozone-emitting facilities in "treated" counties (Becker, 2005; Becker & Henderson, 2000, 2001; Greenstone, 2002). In particular, nonattainment designations constitute exogenous shocks to these polluting plants, as their ozone emissions contribute only a small fraction to the total ozone emissions from all pollution sources that determine a county's nonattainment status. Consequently, nonattainment designations are unlikely to be influenced by the emissions from such plants themselves. This regulatory policy yields considerable heterogeneity in the environmental regulations affecting firms, as the attainment status of counties changes over time, only ozone-emitting plants within treated counties experience shocks to production costs, and otherwise identical firms may experience different environmental regulations depending on the cross-county locations of their ozone-emitting plants. We exploit this regulatory setting to explore the effects of adverse cash flow shocks on the design of managerial incentive contracts.

We construct annual, firm-specific measures of environmental regulations and executive compensation. To gauge firm-year exposure to environmental regulations, we construct ($A \rightarrow NA$) *exposure*, which is based on (1) whether a firm's polluting facilities operate in counties that *switch* from attainment to nonattainment status for the first time, as nonattainment designations are fairly persistent; and (2) the ozone emissions from these facilities, as only those emitting ozone are subject to regulation. We focus on ozone emissions because violating ozone NAAQS is the principal reason counties receive a nonattainment designation and complying with ozone NAAQS entails the highest economic costs. To measure the risk-taking incentives of CEO's compensation packages, we primarily use *Vega*—the sensitivity of CEO wealth to stock return volatility. *Vega* is commonly employed to gauge the convexity of executive compensation. A large body of 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 $(A \rightarrow NA)$ *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 the convexity of executive compensation.

One concern with using $(A \rightarrow NA)$ *exposure* to identify the impact of environmental regulations on executive compensation is the potential influence of unobservable confounding variables that affect both risk-taking incentive compensation and coincide with counties switching from attainment to nonattainment. 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. If 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. 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 $(A \rightarrow NA)$ *exposure* and include both in the DiD analyses. The unexpected component captures the quasi-exogenous variation in switches to nonattainment status that are orthogonal to potential confounding variables.

We discover that more stringent environmental regulations reduce *Vega*. Moreover, only the unexpected component of nonattainment designation triggers reductions in *Vega*. This finding suggests regulatory shocks that boost production costs induce boards to reduce the convexity of CEO compensation packages. The estimates imply an economically significant effect. For example, 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, particularly those that account for the ability of multi-plant firms to reallocate emissions from nonattainment to attainment counties.

We also address two more granular questions about CEO compensation. First, what accounts for the fall in *Vega*? *Vega* can fall because corporate boards change executive compensation packages or because CEOs change their corporate securities holding, e.g., by exercising options. We find that unexpected increases in $(A \rightarrow NA)$ *exposure* reduce the convexity of CEOs' compensation packages. However, we find no evidence that $(A \rightarrow NA)$ *exposure* shocks affect CEOs' decisions to exercise options. Second, do environmental regulations change other features of CEO compensation, including overall 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 Environmental Protection Agency (EPA) applies regulations across facilities within the same county to assess further the view that adverse cash-flow shocks, on average, reduce *Vega.* Specifically, when counties receive nonattainment designations, the EPA imposes more regulatory intensity on (1) facilities geographically closer to ozone monitors, where research shows regulators focus their efforts (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 reduce expected corporate cash flows more when treated facilities are closer to ozone monitors, are younger, and have a history of regulatory noncompliance. We include the interaction between unexpected

$(A \rightarrow NA)$ *exposure* and these three regulatory intensity measures to assess this prediction. Consistent with the view that adverse cash flow 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 next evaluate a crucial prediction from theory: the impact of adverse expected cash-flow 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 net equity position. Such a position could incentivize shareholders with limited liability 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 four measures of the pre-treatment financial conditions of firms. Two of the measures use accounting data to assess financial constraints (e.g., Baker, Stein, & Wurgler, 2003; Kaplan & Zingales, 1997; Whited & Wu, 2006); one uses text-based analyses of corporate disclosure statements to gauge firms' financial conditions (Hoberg & Maksimovic, 2015); and one uses stock return data to forecast the firm's probability of failure (Campbell, Hilscher, & Szilagyi, 2008). We include these financial distress measures and their interactions with unexpected $(A \rightarrow NA)$ *exposure* in the DiD *Vega* regressions. 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 each financial distress measure enters positively, indicating that the negative impact of environmental stringency on the convexity of CEO compensation is dampened among financially distressed firms.

Consistent with this joint hypothesis, the impact of nonattainment exposure on *Vega* becomes less negative, and sometimes positive, among more financially distressed firms. With *Vega* as the dependent variable, 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 each of the four measures of financial distress. Thus, consistent with foundational theories of the firm, pre-treatment financial distress mitigates the negative impact of adverse cash flow shocks on the convexity of executive compensation.

Finally, we examine the prediction that the impact of expected cash-flow shocks on executive compensation will depend on the effectiveness of firms' corporate governance structure.

Research shows corporate governance influences executive compensation (Coles, Daniel, & Naveen, 2014; Morse, Nanda, & Seru, 2011). We evaluate whether adverse cash flow shocks triggered by nonattainment designations reduce *Vega* more among firms with more effective corporate governance systems.

We use nine indicators of corporate governance effectiveness to assess this hypothesis. Specifically, we consider (a) three measures of CEO entrenchment (Adams, Almeida, & Ferreira, 2005; Bebchuk, Cohen, & Ferrell, 2009; Coles et al., 2014), (b) two measures of the crucial role of long-term institutional investors in monitoring firms (Bushee & Noe, 2000; Derrien, Kecskés, & Thesmar, 2013; Harford, Kecskés, & Mansi, 2018), (c) two measures of the bargaining power of corporate executives based on compensation (Bebchuk, Cremers, & Peyer, 2011), and (d) two measures of CEO overconfidence, which can lead to excessive risk-taking, where the measures are based on the CEOs' history of exercising options (Banerjee, Humphery-Jenner, & Nanda, 2015; Malmendier, Tate, & Yan, 2011).

We discover that unexpected ($A \rightarrow NA$) *exposure* reduces the convexity of executive compensation more among firms with more effective corporate governance. This result holds across the nine corporate governance measures. The results are consistent with the view that (1) environmental regulatory shocks that shrink expected net cash flows diminish shareholder preferences for corporate risk-taking, and (2) with more effective corporate governance, those shareholders can more readily reduce the convexity of executive compensation packages to align CEO risk-taking incentives with their preferences for lower corporate risk-taking.

In additional tests, we consider alternative interpretations for our findings and conclude that the evidence for them is not compelling. First, we investigate whether the mandatory pollution abatement required by nonattainment designations could influence firms' investment decisions through financial constraints, which could in turn change executives' risk-taking incentive compensation (Dang, Wang, & Wang, 2022). We find that only *expected* nonattainment designations impact on firms' R&D investments and capital expenditures conditional on financial constraints, while *unexpected* nonattainment designations do not significantly affect these investments. Since changes in executives' incentive compensation occur only with exposure to unexpected nonattainment designations, it is unlikely that changes in firm investments are driving our results. Second, we address the potential concern that multi-plant firms might shift production from nonattainment to attainment counties to evade compliance costs. Using facility-level regressions, we find no evidence of such reallocation in our sample, alleviating concerns that such behavior may diminish the impact of negative cash flow shocks for these firms.

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 level of pollution, on executive compensation. In addition, 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. By leveraging the NAAQS as a natural experiment, we explore the effects of adverse cash flow shocks on the design of managerial incentive contracts and show the response of executive compensation to those shocks depends on firms' financial conditions and governance effectiveness.

2. Institutional background and identification strategy

In this section, we discuss the regulatory framework that forms the basis of our identification strategy. 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 counties most often fail to meet the NAAQS by exceeding ozone limits (Curtis, 2020). Moreover, the economic costs associated with adhering to ozone NAAQS are the largest among the six pollutants (US EPA, 2015).[1] As a result, ozone offers a much larger treatment group of counties for our analyses,[2] as well as a setting where the economic implications are significant enough to adversely impact firms' future expected cash flows.

Each year, the EPA designates each county as being in attainment or out of attainment (nonattainment) with the NAAQS threshold. These designations are federally mandated and rely on ozone monitoring stations across the United States. Specifically, the EPA calculates

[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] Another advantage of focusing only on ozone is that the NAAQS specifies only one primary standard for ozone but specifies a primary and secondary standard for other pollutants. The existence of only one standard for ozone allows us identify treatment and control groups precisely.

an annual county-level summary statistic using high-frequency monitor readings across the county, known as a "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.[3] During our sample period from 1993 to 2019, the EPA used four different ozone standards, each characterized by a different NAAQS threshold. Detailed information about these standards can be found in Internet Appendix Table IA.1.

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 may vary from state to state, 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 regardless of, for example, whether the firm has a record of good environmental performance before the designation (Greenstone, 2002).

Environmental regulations under the SIP in nonattainment counties are intended to curb emissions from all pollution sources. 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 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 of these differing standards, compliance costs are significantly higher in nonattainment counties than in attainment counties. For example, using plant-level survey data, Becker (2005) finds that BACT is significantly less costly to plants than following the standard of LAER.

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

[3]Emissions from all types of pollution sources, both stationary and non-stationary, contribute to a county's DV. Importantly, the plants considered in this paper represent a very small fraction of the total ozone emissions relative to other sources, implying that nonattainment designations are unlikely to be influenced by the ozone emissions from these plants. We discuss this in further detail in Section 2.1.

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.

In summary, nonattainment designations entail substantial economic costs for polluting plants, adversely affecting the expected future cash flows of firms operating in these counties.

2.1. Nonattainment designations as an identification strategy

We exploit three sources of variation from county-level nonattainment designations to assess the impact of environmental regulatory stringency on executive compensation. First, only counties with DVs that violate the NAAQS threshold are designated nonattainment. Thus, there is cross-sectional variation in regulatory stringency across counties at each point in time, allowing us to condition out all time-invariant county traits.

A potential concern is that nonattainment designations at the county level may not be an exogenous shock to polluting plants, as these designations could be influenced by the emissions from such plants themselves. However, empirical evidence reveals that the majority of non-biogenic ozone emissions contributing to a county's DV originate from non-stationary pollution sources rather than stationary sources such as the facilities examined in this study. To illustrate, Figure 1 displays the proportion of non-biogenic ozone emissions from different pollution sources aggregated across all counties based on EPA's National Emissions Inventory (NEI) spanning from 1990 to 2020.[4] 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.[5] As depicted in the figure, ozone emissions from TRI

[4]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. The NEI draws primarily from data supplied by state and local air agencies regarding pollution sources within their respective jurisdictions, supplemented by data developed by the EPA. The data is obtained from `https://www.epa.gov/air-emissions-inventories/national-emissions-inventory-nei`.

[5]Point sources refer to emissions originating from fixed and stationary locations. These encompass the TRI facilities examined in this study, along with other stationary pollution sources 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,

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%. Therefore, considering that the majority of ozone emissions originate from sources beyond the control of the TRI facilities examined in this paper, it is likely that nonattainment designations are exogenous events to such plants.

Second, over time, counties can both become nonattainment and later regain their attainment status by reducing their emissions or the EPA changing NAAQS thresholds.[6] Thus, for any plant, there can be intertemporal variation in regulatory stringency, allowing us to condition out all time-invariant firm characteristics. In particular, by including firm fixed effects, the estimated coefficients reflect only the experiences of firms treated with a change in environmental regulations, allowing for pre and post-treatment comparisons of the outcome variable.

However, nonattainment designations are fairly persistent; the mean duration of nonattainment for the sample of counties we study is around 16 years. A potential concern is that plants' ozone emissions may be endogenous with respect to the treatment in counties that are chronic violators and remain in nonattainment for an extended period of time.[7] To mitigate this issue, our analysis focuses exclusively on "switchers"—counties that switch from attainment to nonattainment for the first time. Internet Appendix Figure IA.1 displays the variation in each county's designation status in 2004. In a given year, a county designated as nonattainment could either have maintained its nonattainment status from the previous year or switched from attainment to nonattainment for the first time. In the construction of our treatment variable, we only consider the variation introduced by the newly designated nonattainment counties as detailed in Section 3.2.

Third, within any nonattainment county, a polluting plant is regulated only if it emits ozone. This intracounty variation implies that the stringency of environmental regulations differs across plants within nonattainment counties because not all plants emit ozone. Previous studies that utilize nonattainment designations identify affected firms based on their operation of a TRI plant within the nonattainment county (Dang et al., 2022; Xu & Kim, 2022). However, not all TRI plants emit ozone. For instance, a heavy polluting plant that emits only particulate matter in a nonattainment county remains unaffected by additional compliance

and commercial marine vessels.

[6]It is rare for a county to be designated as nonattainment for a second time once it has been redesignated to attainment.

[7]For example, some of these plants might already possess the necessary technology to lower emissions during prolonged periods of nonattainment, thereby reducing the effort needed to comply with regulations.

costs. Therefore, to accurately identify the firms impacted by adverse cash flow shocks arising from nonattainment designations, we further classify all chemicals emitted by TRI plants into ozone precursors or not, enabling us to precisely identify ozone-emitting plants. Internet Appendix Figure IA.2 shows the fraction of TRI plants labeled as ozone emitters across major industries in nonattainment counties. Even within two-digit industry NAICS codes, there is considerable variation in the fraction of plants classified as ozone polluters.

Taken together, these sources of variation in the stringency of environmental regulations facing firms allow us to construct a time-varying continuous treatment variable that measures a firm's "nonattainment exposure" in any given year. We employ a staggered DiD methodology in a panel regression setting to gauge the effect of the continuous treatment on CEO compensation packages. The construction of the treatment variable and the DiD regression estimator will be formally described in Sections 3.2 and 4, respectively.

Although nonattainment designations are typically treated as exogenous events because of their federally mandated nature (Greenstone, 2002; Walker, 2013), a potential concern is that local pollution levels may not be randomly assigned, implying that time-series variation in nonattainment designations may be correlated with local economic activity. For example, counties that switch to nonattainment may also have more economic activity. This potential endogeneity implies that some nonattainment designations may be anticipated. Borochin, Celik, Tian, and Whited (2022) show that estimated market reactions in event studies may be biased downwards due to event anticipation. To address this issue, we exploit the regulatory design of DVs in an RDD to generate quasi-exogenous variation to decompose nonattainment designations into an exogenous ("unexpected") and endogenous ("expected") component. This procedure allows us to control for unobservable confounding variables that affect both risk-taking incentive compensation and coincide with shifts from attainment to nonattainment. We discuss this procedure in more detail in Section 4.2.

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 sales and total assets, and non-missing equity compensation data. We also 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. Industrial facilities that fall within a specific industry (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 their releases of toxins to the TRI. The TRI provides self-reported 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.[8] We use the emissions data in TRI to classify whether a facility is a polluter of ozone.[9] In any given year, a facility is labeled as an ozone-emitting plant if it emits chemicals classified as volatile organic compounds or nitrogen oxides, both precursors to ozone formation.[10] 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

[8]While the TRI data are self-reported, the EPA regularly conducts quality analyses to identify potential errors and purposefully misreporting emissions can lead to criminal or civil penalties (Xu & Kim, 2022).

[9]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.

[10]Ozone is not directly emitted by plants. It is formed through chemical reactions in the atmosphere. Henceforth, we refer to emitters of ozone precursors as ozone emitters.

and accumulated option holdings for a 0.01 increase in the annualized standard deviation of a firm's stock returns (Core & Guay, 2002).[11] 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 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 nonattainment exposure

Combining the three sources of variation described earlier in Section 2.1, we construct a firm-level measure of nonattainment exposure based on the cross-county location of a firm's plants, whether these counties newly switch to nonattainment or not, and the amount of ozone emissions at each plant in the year prior to the switch. Formally, we define firm i's nonattainment exposure in year t as

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

where j denotes plant, i denotes firm, and t denotes year. $ozone_{j,i,t-1}$ is the total amount of ozone air emissions for plant j of firm i in year $t-1$.[12] $(A \to NA)_{j,i,t}$ is a dummy variable

[11] Following Coles et al. (2006), we assume that vega of stock holdings is zero.

[12] We use the total amount of a plant's ozone emissions, not ozone emissions per unit of production output, because emission limits applied to plants under the SIP are based on the plant's level of ozone emissions rather

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. $(A \rightarrow NA)$ *exposure* is a measure of 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 than a similar firm with most of its plants located in attainment counties. That is, the firm has more nonattainment exposure. 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 becomes available the following year (Hsu et al., 2023). Second, the switch-to-nonattainment dummy $(A \rightarrow NA)$ ensures that we capture only a firm's exposure to newly designated nonattainment counties, rather than those that persistently remain in nonattainment. For instance, suppose a county j switches from attainment to nonattainment in year 2000 and remains in nonattainment thereafter. If firm i operates ozone-emitting plants in this county in 2000, then this county 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. Third, by weighting the switch-to-nonattainment dummy by a plant's total amount of ozone emissions, this measure captures the notion that the cross-plant impact of a county's nonattainment designation 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:

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

than its emissions relative to its output. For example, plants are subject to LAER if they have the potential to emit over 100 tons of ozone per year, regardless of their production levels.

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) ensure that we estimate the impact of nonattainment exposure after controlling for any time-invariant firm-specific factors. Year-fixed effects (ρ_t) control for any time effects, including trends. 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, which helps to strengthen identification further. The standard errors are clustered at the firm level. The main coefficient of interest is β_1, which is the coefficient of the DiD estimate for the causal effect of nonattainment exposure on CEOs' risk-taking incentives through their compensation package.

We include a vector of variables, $X_{i,t-1}$, 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 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

Unobservable confounding variables may exist that correlate with both the switch to nonattainment status and corporate variables affecting incentive compensation. For example, counties switching to nonattainment may coincide with local economic expansions that increase local pollution. Simultaneously, improved economic prospects could lead to changes in firm investments, affecting risk-taking incentives.[13] Consequently, our continuous treatment variable, $(A \rightarrow NA)$ *exposure*, may not be an entirely exogenous measure of a firm's nonattainment exposure (Borochin et al., 2022). Since county-level DVs are observable, we can decompose nonattainment designations into an exogenous (i.e., unexpected) and endogenous (i.e., expected) component. The intuition is that counties with a DV far above the NAAQS threshold will almost for certain be designated nonattainment, while those with a DV far below the threshold will almost for certain be designated attainment. The question then becomes how far above or below the NAAQS threshold can one reasonably predict a county's designation status.

The idea underlying our approach is that nonattainment designations are essentially a random outcome for counties with DVs in an arbitrarily small interval around the NAAQS threshold. To operationalize this, we use an RDD to exploit the sharp increase in nonattainment probability when a county's DV violates the threshold to estimate an optimal "bandwidth" that determines the region where ozone concentrations are as good as randomly assigned and, hence, unpredictable.[14] For brevity, we provide the full details of the RDD specification along with tests that support the identifying assumptions in Section IA of the Internet Appendix.

We summarize the decomposition procedure in Figure 2, which plots a county's probability of nonattainment conditional on the distance of its DV 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, nonattainment designations are quasi-exogenous and are orthogonal to confounding variables such as local economic conditions by construction of the RDD.

We define an attainment county that has a DV falling in the unpredictable region and

[13] We specifically examine whether changes in firm investments drive our results in Section 9.1.

[14] For example, fluctuations in DVs around the threshold generally depend on weather patterns (Cleveland & Graedel, 1979; Cleveland, Kleiner, McRae, & Warner, 1976)—a factor that is unlikely to be related to local economic activity.

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.[15] This decomposition allows us to measure a firm's exposure to unexpected and expected nonattainment designations, respectively, as follows:

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

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

where $\textit{Unexp. } (A \rightarrow NA)_{j,i,t}$ ($\textit{Exp. } (A \rightarrow NA)_{j,i,t}$) 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 $\textit{Unexp. } (A \rightarrow NA) \textit{ exposure}$ ($\textit{Exp. } (A \rightarrow NA) \textit{ exposure}$) 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 $(A \rightarrow NA) \textit{ exposure}$ into its unexpected and expected components as follows:

$$\begin{aligned} Vega_{i,t} = {} & \beta_0 + \beta_1 \textit{Unexp. } (A \rightarrow NA) \textit{ exposure}_{i,t} + \beta_2 \textit{Exp. } (A \rightarrow NA) \textit{ exposure}_{i,t} \\ & + \beta_3 X_{i,t-1} + \tau_i + \rho_t + \varepsilon_{i,t}. \end{aligned} \quad (5)$$

The main coefficient of interest is β_1, which is the coefficient of the DiD estimate of the causal effects driven by the exogenous component of nonattainment exposure.

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%

[15] The rationale behind these definitions is as follows. For a county experiencing an unexpected switch to nonattainment, even if the DVs are slightly above the threshold, they are not significantly beyond it to confidently predict the county's nonattainment designation for the next year since random fluctuations within the unpredictable region may cause the DVs to drop below the threshold before next year's designations. In contrast, if the DVs fall within the predicted nonattainment region, the attainment county is highly likely to switch to nonattainment the following year because it is improbable for the DVs to suddenly decrease below the threshold before next year's designations. Hence, these switches are termed expected nonattainment designations.

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 that have a non-zero value of $(A \rightarrow NA)$ *exposure*, 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)*, we find that 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 first presents the results from estimating the relationship between nonattainment exposure and CEO vega using Equation (2). Column (1) 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)).[16]

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.

To account for the potentially predictable component of nonattainment designations, we next present the results from estimating Equation (5) in columns (2), (4), and (6) of Table 2. Across all specifications, the coefficients on *Unexp.* $(A \rightarrow NA)$ *exposure* are negative and statistically significant, indicating a decrease in the convexity of compensation payoffs. However, the coefficients on *Exp.* $(A \rightarrow NA)$ *exposure* are statistically insignificant and smaller in magnitude. These results suggest that the observed decrease in the convexity of compensation payoffs is primarily driven by the exogenous component of nonattainment

[16]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).

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 $(A \rightarrow NA)$ *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:

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

where

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

and *Exp.* $(A \rightarrow NA)$ *intensity*$_{i,t}^{\ell}$ is defined similarly.[17] All other variables are defined as in Equation (5).

Equation (6) is a generalization of Equation (5) that allows for the effects of *Unexp.* $(A \rightarrow NA)$ *exposure* and *Exp.* $(A \rightarrow NA)$ *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).

Panel A of Figure 3 shows the dynamic effects of estimating Equation (6). There is no

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

indication of any significant changes in vega before firms' exposure to unexpected or expected nonattainment designations. This finding supports our assumption that there are no differential responses before switches to nonattainment. In the periods following the nonattainment designation, CEOs' vega shows a decrease 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. 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.[18] Specifically, we define the variable $(A \to NA)$ *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.* $(A \to NA)$ *exposed* and *Exp.* $(A \to NA)$ *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.[19]

The results, presented in Table 4, show that our inferences continue to hold after controlling for treatment effect heterogeneity.[20] Furthermore, none of the individual pre-trend estimators

[18] 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 $(A \to NA)$ *exposure*, can take on a large number of values.

[19] 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.

[20] The economic magnitude of the estimated effects are larger in this analysis compared to the baseline

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 reallocate production (and hence, emissions) from nonattainment to attainment counties.[21] To address this concern, we construct and analyze two additional exposure measures. In the first measure, we set $ozone\ (\#Plant)_{j,i,t-1} = (1/\#Plant_{i,t}) \cdot ozone_{j,i,t-1}$, 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 $ozone\ (Prod.\ ratio)_{j,i,t-1} = Production\ ratio_{j,t} \cdot ozone_{j,i,t-1}$, where $Production\ ratio_{j,t}$ is plant j's ozone production ratio in year t.[22] By weighting ozone emissions with the ozone production ratio, this measure assigns more weight to plants with higher production levels.

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 $ozone\ (Employees\ share)_{j,i,t-1} = Employees\ share_{j,i,t} \cdot ozone_{j,i,t-1}$, 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

model, as we are now examining a discrete change in nonattainment exposure rather than a change in the intensity of the continuous treatment variable.

[21] 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. In Section 9.2, we present empirical evidence demonstrating that intrafirm reallocation of production from nonattainment to attainment counties does not occur in our sample.

[22] For example, if a chemical is used in the manufacturing of refrigerators, the production ratio for year t is given by $\frac{\#\text{Refrigerators produced}_t}{\#\text{Refrigerators produced}_{t-1}}$. If the chemical is used as part of an activity and not directly in the production of goods, then the production ratio represents a change in the activity. For instance, if a chemical is used to clean molds, then the production ratio for year t is given by $\frac{\#\text{Molds cleaned}_t}{\#\text{Molds cleaned}_{t-1}}$.

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 negative shocks to cash flows 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 $\textit{Core chemical}_c$ 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 cash-flow shocks than other plants in nonattainment counties. We define $ozone\ (Permit)_{j,i,t-1} = \sum_c Permit_{c,j,t} \cdot ozone_{c,j,i,t-1}$, 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. 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. 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 $(A \to NA)$ *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.6), as the placebo treatment variables enter insignificantly in the *Vega* regressions.

5.3.5. Alternative measures of CEO incentive compensation

We also consider three additional methods for constructing the measure of the risk-taking incentives of CEO compensation. 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.7. To mitigate potential biases arising from using this log transformation, we employ the fixed-effects Poisson models, 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.[23] 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

[23]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.

scaling of vega by delta captures this offsetting relationship. Our results, presented in columns (5) and (6) of Internet Appendix Table IA.7, confirm the robustness of our findings when using this scaling.

5.3.6. 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.8 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.

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.* $(A \to NA)$ *exposure* reduces *Flow vega* by 4.27% relative to the sample mean, while a one standard deviation increase in *Exp.* $(A \to NA)$ *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.* $(A \to NA)$ *exposure* on the *Number of options granted* is larger than that of *Exp.* $(A \to NA)$ *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 $(A \to NA)$ *exposure*, *Unexp.* $(A \to NA)$ *exposure*, and *Exp.* $(A \to NA)$ *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 four 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 four 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.[24] 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

[24]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.

one if a firm operates ozone-emitting plants that are between zero and five years of age in nonattainment counties, and zero otherwise.[25]

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.[26] 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 (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* × *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.

[25]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.

[26]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.

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 cash flow 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 cash-flow 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 cash flow 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 et al., 2003; Kaplan & Zingales, 1997) and the Whited-Wu index (Whited & Wu, 2006).[27] 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

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

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 et al., 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 distressed 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 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 events 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 et al., 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 et al. (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 et al., 2013; Harford et al., 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 et al., 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 negative impact of nonattainment exposure on cash flows 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 et al., 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 et al., 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. Additional analyses

9.1. Firm investments

In this section, we address the potential concern that nonattainment designations may influence a firm's investments, which could in turn alter executives' risk-taking incentive compensation. Specifically, Dang et al. (2022) demonstrate that when faced with a nonattainment status designation, financially unconstrained firms typically increase investments in R&D and capital expenditure, as mandatory pollution abatement crowds in other investments. Conversely, financially constrained firms tend to decrease both types of investments due to the diversion of resources towards pollution abatement.

Our instrument, based on a firm's *unexpected* exposure to counties *switching* to nonattainment, differs from Dang et al.'s (2022) study in three key aspects that mitigate this concern. First, our focus is solely on attainment counties that switch to nonattainment status for the first time, whereas Dang et al. (2022) include all nonattainment designations, irrespective of whether a county is switching to nonattainment or has been in nonattainment for a prolonged period. Given the persistent nature of nonattainment designations, they could be correlated with unobserved factors such as local economic conditions, which could also influence a firm's investment decisions, leading to a spurious relation between a firm's investment and its nonattainment exposure.

Second, we use RDD to estimate an optimal bandwidth to examine unexpected nonattainment designations, while Dang et al. (2022) do not take into account the distance from the threshold of the nonattainment county. Since nonattainment counties with DVs significantly above the threshold are highly likely to remain in nonattainment, firms' investments may change in anticipation of ongoing nonattainment. Thus, for these expected nonattainment designations, any observed correlation between nonattainment exposure and firm investments may reflect pre-existing adjustments in investments.

Third, Dang et al. (2022) pool together the nonattainment designations of all six pollutants and classify a firm as treated if it operates a TRI plant in a nonattainment county. However, this approach may introduce measurement errors into the treatment variable because not all TRI plants emit the regulated pollutants due to significant heterogeneity in the types of chemicals that TRI plants emit. To address this issue, we classify TRI plants based on whether they emit ozone, allowing us to identify treatment status at the plant level.

To show that our main results are not driven by firm investments, we follow Dang et al. (2022) and investigate the impact of mandatory pollution abatement under nonattainment designations on firm investments, conditional on financial constraints. First, as shown in Internet Appendix Table IA.9, firms exposed to both unexpected and expected nonattainment designations increase their pollution abatement efforts, consistent with the additional costs that nonattainment designations impose on these firms.[28]

Next, Internet Appendix Table IA.10 examines the impact of nonattainment exposure on firms' R&D expenditure to total assets (*R&D*) and the ratio of capital expenditure to total

[28]Plants reporting to the TRI database document the extent of source reduction activities at the chemical level aimed at limiting the release of hazardous substances. Ozone emissions can undergo treatment, recovery, or recycling before discharge into the environment, with treatment being the primary mode of abatement. Facilities must also detail the type of source reduction activities they undertake. Given that there is no available data specifically on plant-level pollution abatement costs for ozone emissions, we use observable source reduction efforts as proxies for pollution abatement costs (Xu & Kim, 2022). Our variables of interest are the natural logarithm of one plus the amount of onsite ozone air emissions that are treated (*Onsite treated*), undergo recovery (*Onsite recovery*), or are recycled (*Onsite recycle*) for a given firm in a given year. Additionally, we create a dummy variable that takes a value of one if a firm undertakes source reduction activities related to ozone in a given year, and zero otherwise (*SR activity*).

assets (*Capex*), conditional on the four financial constraint variables utilized in Section 7. We find some results consistent with those of Dang et al. (2022); however, all of these results manifest through firms' exposure to expected nonattainment designations, while there are no effects on firms' investments through unexpected nonattainment exposure. For instance, in columns (2) and (8), we find that financially unconstrained firms increase their R&D investments when their exposure to expected nonattainment designations increases (positive and significant coefficient on *Exp.* $(A \rightarrow NA)$ *exposure*). Conversely, financially constrained firms decrease their R&D investments under the same conditions (negative and significant coefficient on *Exp.* $(A \rightarrow NA)$ *exposure* $\times$ *FC index*). Using the *HM index*, when there is an increase in expected nonattainment exposure, only financially constrained firms decrease their capital expenditures, as shown in column (5) (negative and significant coefficient on *Exp.* $(A \rightarrow NA)$ *exposure* $\times$ *FC index*), while only financially unconstrained firms increase R&D investments in column (6) (positive and significant coefficient on *Exp.* $(A \rightarrow NA)$ *exposure*).

Therefore, the results of Dang et al. (2022) are only observed through expected nonattainment designations, with no significant effects observed on firm investments due to unexpected nonattainment designations. Given that the changes in executives' incentive compensation that we document only occur through firms' exposure to unexpected nonattainment designations, it is unlikely that changes in firm investments are driving our results.

9.2. Intrafirm reallocation

In this section, we address the concern that multi-plant firms might shift production from nonattainment to attainment counties to avoid compliance costs. If this behavior is widespread in our sample, it could lessen the impact of negative cash flow shocks for such firms. Although our main findings remain robust when considering alternative measures of nonattainment exposure that incorporate multi-plant firms' capacity to redistribute emissions (see Section 5.3.3), we offer empirical evidence indicating that multi-plant firms in our sample do not reallocate ozone emissions from nonattainment to attainment counties.

We conduct facility-level regressions by restricting the sample to only ozone-emitting plants in attainment counties. We consider four outcome variables of interest: *ln(Ozone)* is the natural logarithm of one plus the total amount of ozone air emissions for a given plant, *Production ratio* is a given plant's ozone production ratio, *ln(Sales)* is the natural logarithm of one plus the dollar amount of sales for a given plant, and *ln(Employees)* is the natural logarithm of one plus the number of employees for a given plant. We then regress these outcome variables on two dummy variables, *Other Unexp.* $(A \rightarrow NA)$ *exposure* and

Other Exp. ($A \rightarrow NA$) *exposure*, defined to be equal to one if a given plant in a given year belongs to a multi-plant firm that has non-zero unexpected or anticipated nonattainment exposure, respectively, and zero otherwise.

Internet Appendix Table IA.11 displays the results. All specifications include fixed effects for plant, industry (three-digit NAICS code) × year, and county × year. Standard errors are robust to heteroskedasticity and clustered at the county level. Across all columns, none of the coefficients on the two dummy variables are significant, indicating that multi-plant firms exposed to nonattainment designations do not reallocate production to plants located in attainment counties. Our findings are in line with those of Cui and Ji (2016), who also find no evidence of intrafirm ozone emissions relocation among multi-plant firms operating in both nonattainment and attainment counties.

10. Conclusion

Our study examines how shocks to cash flows triggered by unexpected changes in 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 regulatory stringency that negatively affects firms' cash flows. 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. In response to adverse cash flow shocks triggered by an intensification of environmental regulations, 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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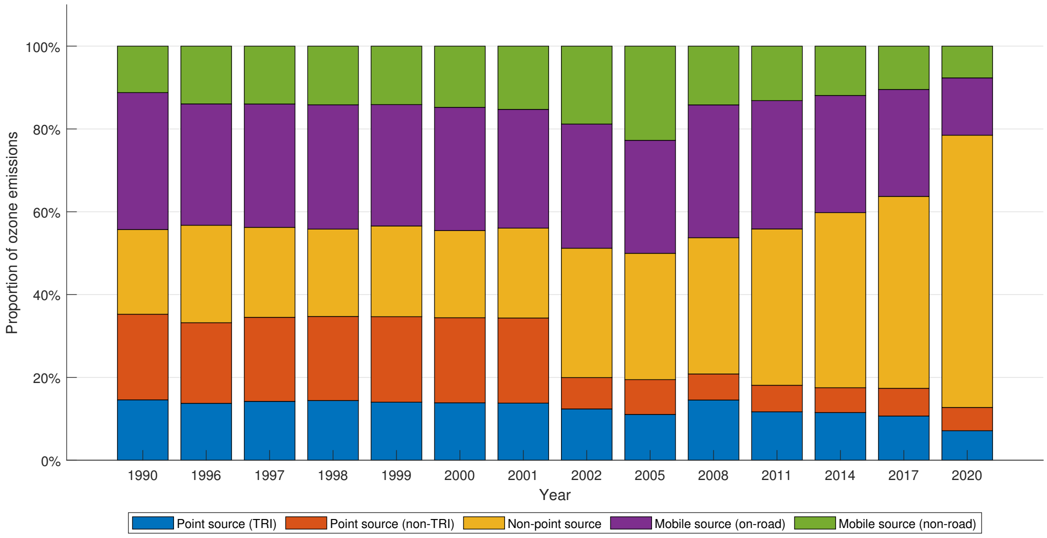
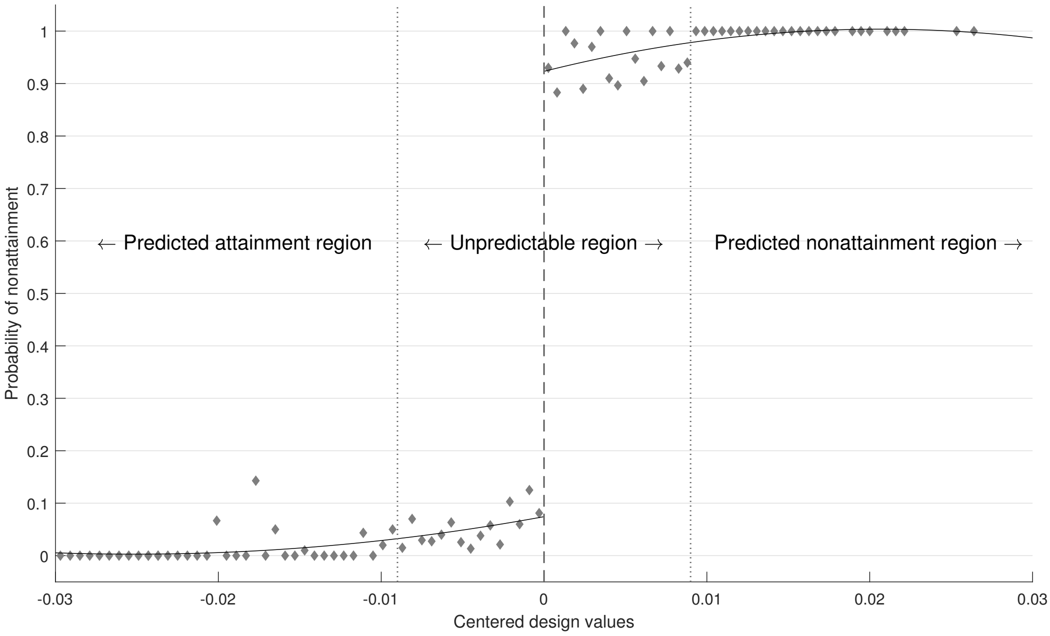
Figure 1
Proportion of ozone emissions by source.



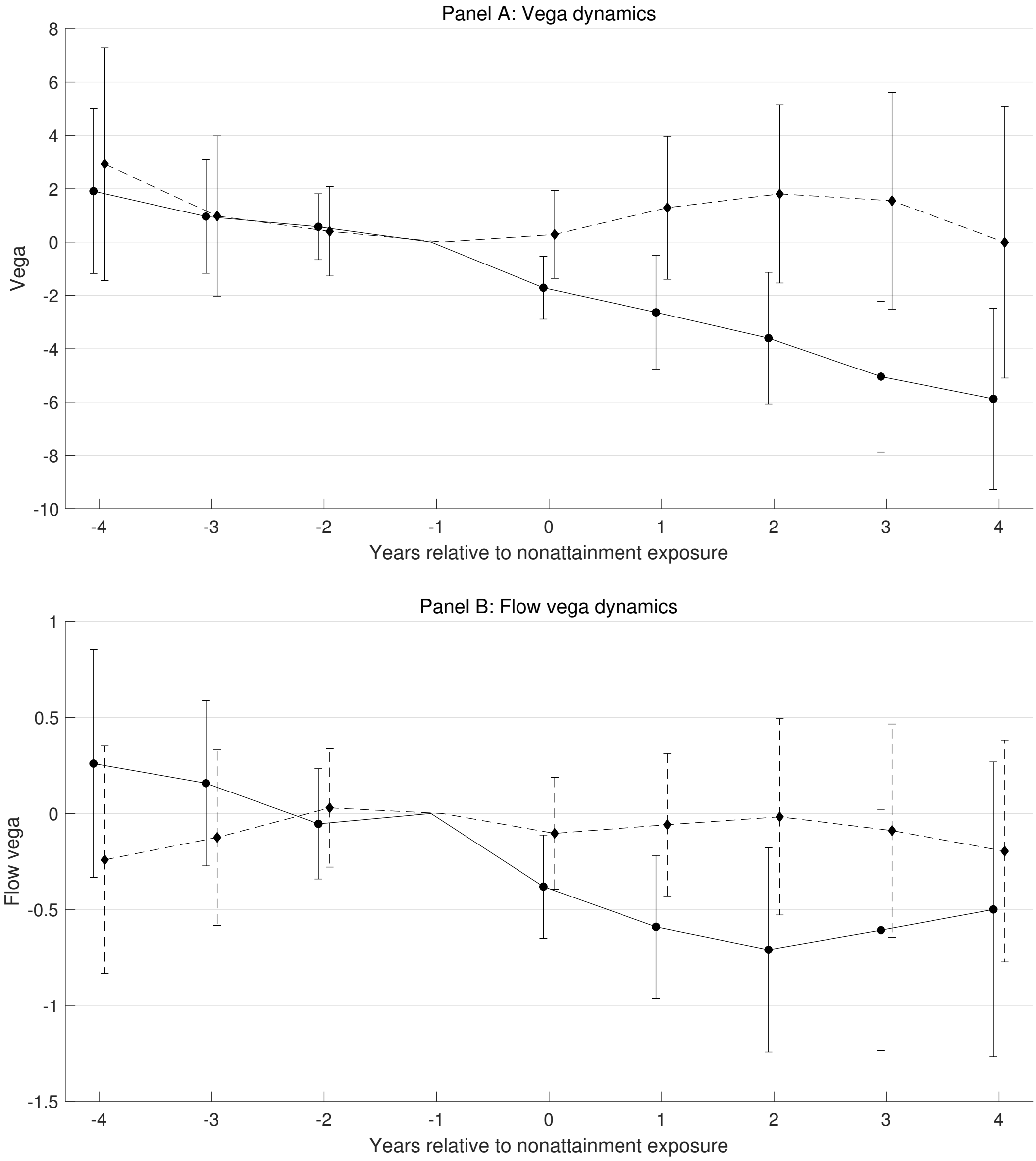
This figure presents the proportion of ozone emissions from different sources across all counties based on EPA's National Emissions Inventory from 1990 to 2020.

Figure 2
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.

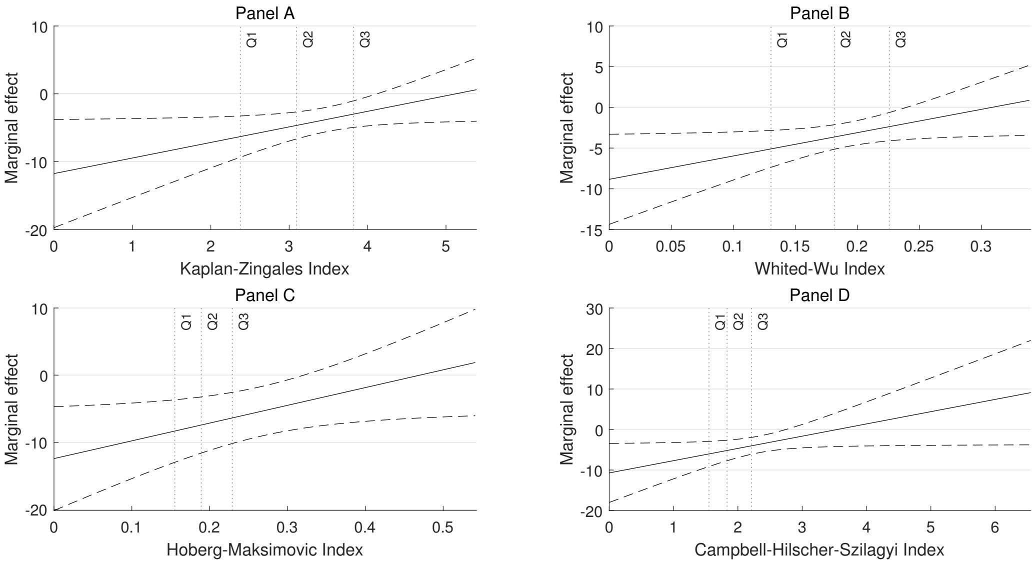
Figure 3
Dynamic effects of nonattainment exposure on CEO incentive compensation.



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)
$(A \rightarrow NA)$ *exposure*	0.001		-0.002**		-0.001		0.001***		0.000	
	(0.29)		(-2.06)		(-1.37)		(2.71)		(0.41)	
Unexp. $(A \rightarrow NA)$ *exposure*		0.001		-0.003**		-0.001		0.002***		0.000
		(0.36)		(-2.35)		(-1.37)		(3.19)		(0.52)
Exp. $(A \rightarrow NA)$ *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*). $(A \rightarrow NA)$ *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 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. $(A \to NA)$ *exposure*	-4.113***	-4.063***	-3.339**	-2.360**
	(-3.28)	(-3.99)	(-2.41)	(-2.03)
Exp. $(A \to NA)$ *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. $(A \to NA)$ *exposure* $\times$ Z	-10.758***	-3.551**	-5.282***	-5.622**
	(-2.67)	(-2.01)	(-2.68)	(-2.00)
Exp. $(A \to NA)$ *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.* $(A \to NA)$ *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 *Unexp.* $(A \to NA)$ *exposure* and *Exp.* $(A \to NA)$ *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
$(A \to NA)$ *exposure*	For a given firm i, we measure its exposure to nonattainment designations in year t as $$\ln\left(1+\sum_j ozone_{j,i,t-1} \cdot (A \to NA)_{j,i,t}\right),$$ 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 $(A \to NA)_{j,i,t}$ 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. $(A \to NA)$ *exposure*	The same expression as $(A \to NA)$ *exposure* except $(A \to NA)_{j,i,t}$ is replaced with *Unexp.* $(A \to NA)_{j,i,t}$, 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. $(A \to NA)$ *exposure*	The same expression as $(A \to NA)$ *exposure* except $(A \to NA)_{j,i,t}$ is replaced with *Exp.* $(A \to NA)_{j,i,t}$, 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

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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 et al., 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.3 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),[29] 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.3, 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*);[30] 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*);[31] 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.3, 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.2 of the Internet Appendix. The coefficient estimate of β captures the discontinuity at the NAAQS threshold and is equal

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

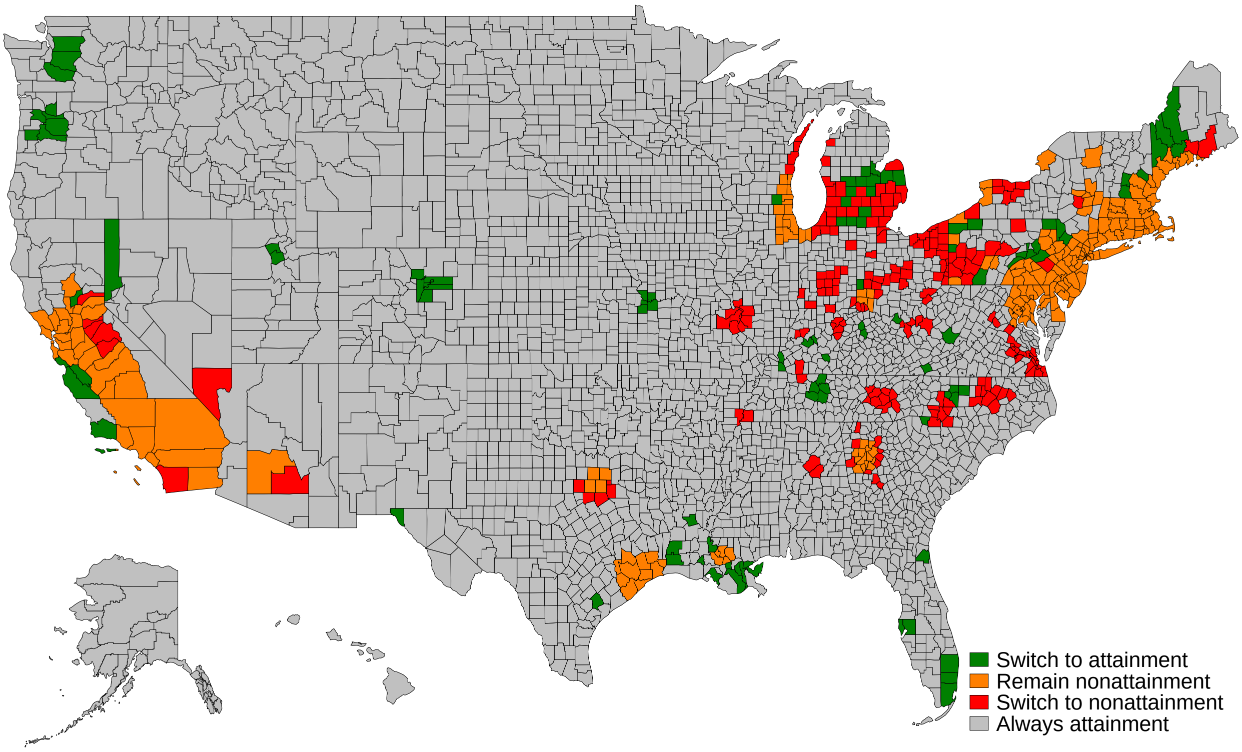
[30] Core chemicals are those that have consistent reporting requirements in TRI.

[31] 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.

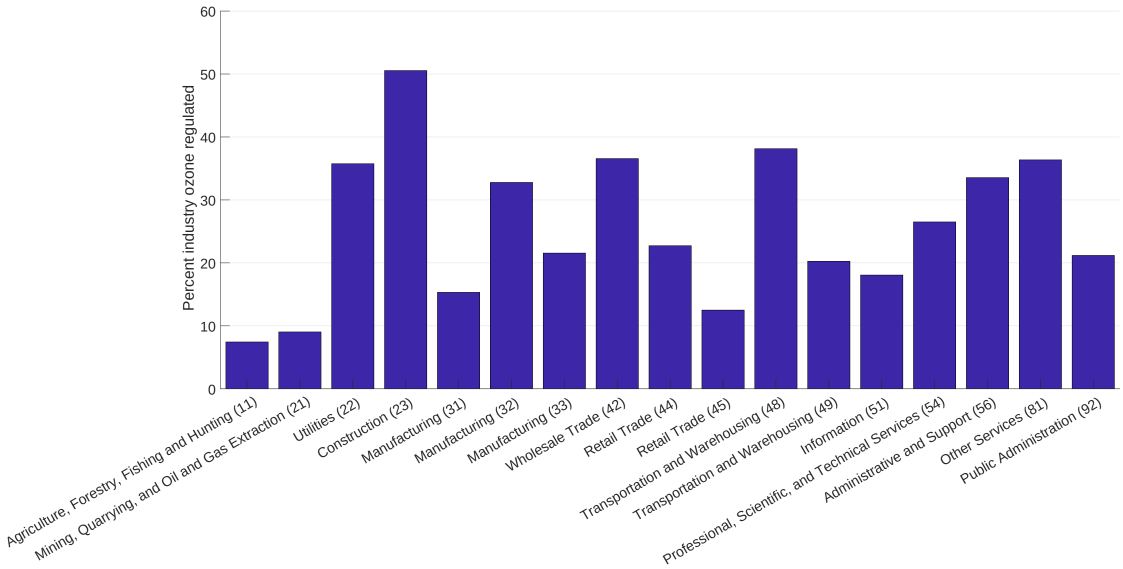
Internet Appendix Table IA.2 also provides the estimates of the optimal bandwidth. The bandwidth estimate of 0.009 in both columns implies that counties with DVs that are within 0.009 ppm of the NAAQS threshold have ozone concentration levels that are as good as randomized. Counties with DVs that exceed 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
Nonattainment designations in 2004.



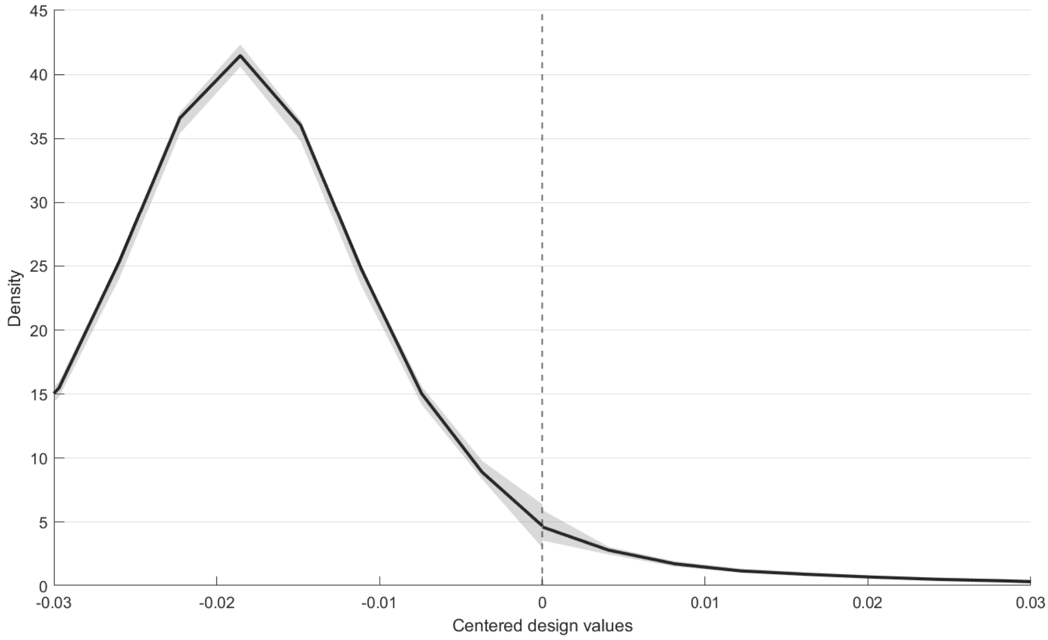
This figure compares the nonattainment/attainment status for each county for the 1997 ozone standard on the effective date, June 15, 2004 with that of the previous 1979 ozone standard.

Figure IA.2
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.3
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

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.3
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.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
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.7
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.8

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.9

Impact of nonattainment exposure on pollution abatement efforts.

Dep. variable:	*Onsite treated*	*Onsite recovery*	*Onsite recycle*	*Onsite total*	*SR activity*
	(1)	(2)	(3)	(4)	(5)
Unexp. $(A \rightarrow NA)$ *exposure*	0.125***	0.151***	0.065***	0.226***	0.014***
	(4.64)	(5.51)	(3.17)	(8.55)	(6.91)
Exp. $(A \rightarrow NA)$ *exposure*	0.138***	0.225***	0.050**	0.200***	0.024***
	(4.30)	(5.45)	(2.08)	(6.53)	(7.99)
Controls	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
Observations	28,054	28,054	28,054	28,054	28,054
Adj R^2	0.82	0.68	0.73	0.85	0.58

This table contains models that analyze the impact of firms' nonattainment exposure on their pollution abatement efforts for ozone emissions. The sample period is fiscal year 1993 to 2019. The dependent variables consist of the natural logarithm of one plus the amount of onsite ozone air emissions that are treated (*Onsite treated*), undergo recovery (*Onsite recovery*), or are recycled (*Onsite recycle*) for a given firm in a given year. Additionally, there is a dummy variable that takes a value of one if a firm undertakes source reduction activities related to ozone in a given year, and zero otherwise (*SR activity*). *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 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.

Table IA.11
The effect of nonattainment exposure on the intrafirm reallocation of production.

Dep. variable:	*ln(Ozone)*	*Production ratio*	*ln(Sales)*	*ln(Employees)*
	(1)	(2)	(3)	(4)
Other Unexp. $(A \rightarrow NA)$ *exposure*	0.022	-0.002	-0.002	-0.013
	(0.34)	(-0.21)	(-0.08)	(-0.69)
Other Exp. $(A \rightarrow NA)$ *exposure*	0.133	-0.003	-0.007	-0.001
	(1.31)	(-0.19)	(-0.27)	(-0.03)
Plant F.E.	Yes	Yes	Yes	Yes
Industry × Year F.E.	Yes	Yes	Yes	Yes
County × Year F.E.	Yes	Yes	Yes	Yes
Observations	34,478	34,478	25,490	25,490
Adj R^2	0.82	0.47	0.87	0.88

This table reports results from facility-level regressions examining the effect of nonattainment designations on the intrafirm reallocation of production from facilities in nonattainment counties to those in attainment counties. The sample period is fiscal year 1993 to 2019. The sample consists only of ozone-emitting plants in attainment counties. *ln(Ozone)* is the natural logarithm of one plus the total amount of ozone air emissions for a given plant. *Production ratio* is a given plant's ozone production ratio. *ln(Sales)* is the natural logarithm of one plus the dollar amount of sales for a given plant. *ln(Employees)* is the natural logarithm of one plus the number of employees for a given plant. *Other Unexp.* $(A \rightarrow NA)$ *exposure* and *Other Exp.* $(A \rightarrow NA)$ *exposure* are dummy variables equal to one if a given plant belongs to a multi-plant firm that has non-zero unexpected or anticipated nonattainment exposure, respectively, and zero otherwise. For all specifications, standard errors are robust to heteroskedasticity and clustered at the county-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.