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Analyst Coverage and Corporate Environmental Policies

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Abstract

Exploiting two quasi-natural experiments, we find that firms increase emissions of toxic pollution following decreases in analyst coverage. The effects are stronger for firms with low initial analyst coverage, poor corporate governance and firms subject to less stringent monitoring by environmental regulators. Decreases in environmental-related questions raised in conference calls, an increased cost of monitoring to institutional shareholders, reductions in pollution abatement investment and the weakening of internal governance related to environmental performance are channels through which reduced analyst coverage contributes to increases in firm pollution. Our study highlights the monitoring role analysts play in shaping corporate environmental policies.

Keywords: financial analysts, corporate environmental policies, toxic pollution, external monitoring

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I. Introduction

Over the last decade, approximately 3.8 billion pounds of toxic chemicals were released into the environment each year on average by U.S. registered plants (EPA (2019)). When exposed to the human body, toxic pollution can lead to serious health consequences such as birth defects, neurodevelopment disorders, illnesses, and even death.[1] In addition to risks to human health, economic activities are also significantly influenced by toxic pollution. In particular, literature has documented the negative externalities of toxic pollution such as decreased worker productivity (Graff Zivin and Neidell (2012)), deterioration of labor supply (Hanna and Oliva (2015)), and lower home prices (Currie, Davis, Greenstone, and Walker (2015)). Given the severe consequences of toxic pollution, increasing effort has been devoted to study the determinants of corporate pollution. In this paper, we focus on the role of financial analysts in influencing corporate environmental policies, in particular, toxic emissions.

As important information intermediaries in capital markets, financial analysts serve as external monitors that contribute to the detection and discipline of corporate misbehaviors such as corporate fraud, earnings management, and workplace safety issues (e.g. Dyck, Morse, and Zingales (2010)). Building upon this literature on the external governance role of analysts, we propose our central hypothesis on the effect of analyst coverage on corporate environmental policies, namely, the *external monitoring hypothesis*. This hypothesis is based on the premise that analysts can play both direct and indirect monitoring roles (e.g. Chen, Harford, and Lin (2015), Kim, Lu, and Yu (2019b)) that influence corporate environmental behavior such as toxic emissions.

Firms in the U.S. are subject to environmental protection laws and face penalties and enforcement actions upon violations of these laws (Xu and Kim (2022)). In the absence of external

[1] In 2015, approximately 9 million premature deaths worldwide were caused by pollution-related diseases (Landrigan et al. (2018)).

monitoring, the probability of detecting environmental misbehaviors decreases (Hart and Zingales (2016)). Consequently, managers may lack incentives to invest in costly abatement processes and technologies, leading to higher pollution. From this perspective, analysts can directly monitor firms' environmental behavior by collecting information through public and private channels (e.g. corporate disclosures and site visits). In addition, analysts can also play an indirect monitoring role by disseminating information regarding firms' environmental policies, thereby reducing monitoring costs for other stakeholders (Chen, Chiu, and Shevlin (2018)). Crucially, the monitoring roles of analysts not only facilitate the detection of environmental misbehaviors, but also increase the consequences of these misbehaviors. Given the high cost of environmental misbehaviors (e.g. negative stock market reactions (Karpoff, Lott, and Wehrly (2005)) and the ex ante increases in these costs, the external monitoring hypothesis predicts that greater analyst coverage should result in less toxic emissions by firms.

The main empirical challenge is that analyst coverage and corporate environmental policies could be endogenous. For instance, there might be concerns related to reverse causality if sell-side analysts prefer covering firms with good environmental performance (Luo, Wang, Raithel, and Zheng (2015)). To circumvent these concerns, we exploit two quasi-natural experiments involving brokerage exits (i.e. brokerage closures and mergers) that create plausibly exogenous *decreases* in analyst coverage. As these decreases in analyst coverage are not related to individual firms' environmental policies and their characteristics, they are helpful in establishing causality.

To analyze firms' pollution output, we rely on the Toxic Release Inventory (TRI) database maintained by the Environmental Protection Agency (EPA). We utilize a propensity-score matched difference-in-differences approach where treated firms are firms that were affected by brokerage exits. Exploiting 35 brokerage exits from 1999 to 2011, we compare pollution outcomes for 303 treatment firms to a group of matched firms one year prior to these exits to one year after.

Our main findings show that decreases in analyst coverage lead to higher corporate toxic emissions. Specifically, the total log nominal (output scaled) toxic emissions of treated firms increase by approximately 13% (12.6%) of the standard deviation as compared to our matched group of control firms. This baseline result is robust to the inclusion of firm and industry-year fixed effects, a battery of firm control variables, different estimation windows, alternative matching criteria, and various subsamples. In addition, we also observe that the number of enforcement actions received by treated firms for EPA violations increases by 7.3% after declines in analyst coverage. Taken together, these findings support the external monitoring hypothesis that analysts fulfill important monitoring roles with respect to the emission of firms' toxic pollution.

We perform several cross-sectional tests on the effects of analyst coverage on corporate pollution to deepen our understanding of the external monitoring hypothesis. We find that the effect of decreases in analyst coverage on toxic emissions is more pronounced in the subsample of treated firms with low initial analyst coverage, poor corporate governance, and firms subject to less regulatory scrutiny. Consistent with the monitoring role played by analysts, analyst coverage appears to be most impactful when the firm is operating in an environment where the overall monitory oversight is weak and where monitoring by analysts substitutes for traditional monitoring mechanisms such as a firm's corporate governance and regulatory oversight.

Next, we explore four non-mutually exclusive channels through which decreases in analyst coverage can lead to higher firm pollution. The first channel—*earnings conference calls*—posits that analysts can play a direct monitoring role by raising questions during earnings conference calls to uncover firm-specific environmental information which facilitates the detection and discipline of any misbehaviors (Chen et al. (2015)). Using textual analysis of Q&A sessions in conference calls to identify environmental-related questions raised by analysts, we find that decreases in analyst coverage significantly reduce the likelihood and the total number of environmental-related questions put forth.

The second channel—*monitoring costs for institutional investors*—postulates that decreases in analyst coverage lead to increases in the cost of monitoring for institutional investors. Institutional investors increasingly incorporate environmental issues into their investment decisions and exert pressure on managers to enhance environmental performance (Dyck, Lins, Roth, and Wagner (2019), Krueger, Sautner, and Starks (2020)). However, the cost of monitoring to institutional investors is dependent on the information sets available to them. Therefore, institutional investors should decrease their holdings following declines in analyst coverage as the cost of monitoring increases (Kim, Wan, Wang, and Yang (2019a), Chen, Dong, and Lin (2020)). Following their exit, pressures to maintain costly environmental policies are alleviated, leading to increases in firms' toxic emissions. Consistent with this explanation, we show that the ownership of treated firms by quasi-indexers and public pension funds (two institutional investor groups that are more long-term oriented and environmentally conscious) declines following decreases in analyst coverage.

The third channel—*investments in pollution abatement*—states that decreases in analyst coverage lead to increases in corporate pollution through underinvestment in pollution abatement technologies. When firms are not closely monitored and the probability of being detected for environmental misbehaviors is low (Hart and Zingales (2016)), they may lack incentives to invest in costly abatement technologies. In support of this, we find that total environmental expenditure and the number of green patents filed decrease, suggesting that less resources are allocated to pollution abatement in treated firms after declines in analyst coverage.

The last channel—*environmental internal governance*—examines if analyst coverage can affect corporate pollution by influencing the design of internal governance mechanisms that promote firms' pro-environmental policies. To the extent that lower analyst coverage decreases the consequences of environmental misbehaviors, firms would respond by relaxing internal governance mechanisms connected to environmental performance. Consistent with this, we find that decreases in analyst coverage reduce the probability of linking executives' pay to environmental goals and the probability of having a sustainability committee.

Our paper makes two main contributions. First, it contributes to the fast-growing literature on the determinants of corporate environmental policy. Akey and Appel (2021) show that moral hazard issues associated with limited liability lead to higher toxic emissions while Shive and Forster (2020) find that public firms pollute more than private firms. Kim et al. (2019a) provide evidence that firms with a higher proportion of local institutional investors pollute less, while Akey and Appel (2019) and Chu and Zhao (2019) document that hedge fund activism is effective in reducing pollution at target firms. Besides firm ownership and organizational form, Xu and Kim (2022) and Goetz (2019) show that financially constrained firms emit more toxic pollution due to reductions in abatement investments. Our study complements this line of literature by highlighting the external monitoring role of financial analysts in reducing firms' environmental pollution.[2]

[2] Our paper is also related to studies on how financial analysts influence a firms' CSR performance. Extant evidence is mixed. Qian, Lu and Yu (2019) find a negative relation between analyst coverage and firm CSR performance, while Dong, Lin, and Zhan (2017) documents the opposite. However, unlike these studies focusing on binary measures of aggregate CSR performance from the KLD database, our study takes advantage of the continuous measures of firm environmental performance from the TRI database which provides us with detailed information about corporate pollution. Indeed, Kim et al. (2019a) point out that the correlation between firm-level TRI toxic pollution and the KLD environmental score is small (-0.17) and capture very different elements of a firm's CSR. As such, our analysis allows us to more cleanly investigate the effect of analyst coverage on an important aspect of CSR, corporate pollution.

Second, we contribute to the debate on the positive and negative effects of financial analysts. On the positive side, analysts reduce information asymmetries among investors (Kelly and Ljungqvist (2012)), improve the quality of corporate disclosures (Irani and Oesch (2013)), increase firms' investment efficiency (Derrien and Kecskes (2013), Guo, Pérez-Castrillo, and Toldrà-Simats (2019)), enhance stock liquidity (Balakrishnan, Billings, Kelly, and Ljungqvist (2014)) and curb agency issues and misbehaviors (Chen et al. (2015), Irani and Oesch (2016), Yu (2008)). On the negative side, analysts are often overly optimistic in their earnings forecasts (Hong and Kubik (2003)), which may distort corporate financing activities (Bradshaw, Richardson, and Sloan (2006)) and impose excessive pressure on managers to focus on short-term goals (Graham, Harvey, and Rajgopal (2005), He and Tian (2013)). Adding to this debate, our study provides strong evidence of the positive benefits financial analysts bring and their role in improving corporate environmental policies. Our findings support the view that financial analysts are key external monitors in restricting corporate misbehaviors, and highlight that the monitoring function of analysts works as a substitute to both traditional corporate governance mechanisms and regulatory monitoring in restraining firms' environmental misbehaviors.

II. Related Literature and Hypothesis Development

Financial analysts, as important information intermediaries and gatekeepers in capital markets, have real effects on a wide range of corporate policies (Bradshaw, Ertimur, and O'Brien (2017)). A core part of the responsibilities that analysts undertake involves the gathering, processing, and dissemination of public and private information regarding corporate performance and policies. In doing so, analysts are able to decrease the information opacity of firms. Their duties also afford them unique opportunities and comparative advantages in monitoring firms through interacting with management during earnings conference calls and acting as "whistle blowers" by expressing their concerns through research reports, analyst forecasts and recommendations (Chung and Jo (1996), Jensen and Meckling (1976)). Analysts are, therefore, able to facilitate the detection and discipline of corporate misbehaviors, thereby fulfilling important roles in external governance.

Extant studies provide strong evidence for this. Dyck et al. (2010) find that analysts can detect corporate fraud. Yu (2008) and Irani and Oesch (2016) document that analyst coverage leads to less financial and real earnings management. Chen et al. (2015) provide comprehensive evidence on the external monitoring role of analysts by showing that a decrease in analyst coverage reduces the value of cash holdings and leads to higher excess CEO pay and more value-destroying acquisitions, while Bradley, Mao and Zhang (2022) find that analyst monitoring enhances workplace safety.

Given the above, we formulate an *external monitoring hypothesis* in which analysts play a monitoring role in restraining firms' environmental misbehavior, leading to decreases in toxic emissions. Firms in the U.S. are required to partially internalize environmental costs by allocating resources for environmental protection by investing in environmental abatement processes and technologies. Compliance with these regulations is overseen by the EPA which issues firms with monetary penalties and enforcement actions for environmental violations. However, investments in environmental abatement processes and technologies are costly (Clarkson, Li, and Richardson (2004)).[3] Therefore, firms trade off the costs of abatement against legal and regulatory liabilities (Xu and Kim (2022)). In the absence of external monitoring, wherein the probability of detecting firm environmental misbehavior is low, managers may lack incentives to invest in costly abatement technologies to maximize short-term profit (Hart and Zingales (2016)).

[3] For instance, Clarkson et al. (2004) find that environmental capital expenditures of pulp and paper companies account for 9.77% of total capital expenditures.

From this perspective, financial analysts, who are typically well-trained professionals with industry-specific knowledge, have strong incentives and expertise to monitor and report on firms' environmental policies. This is because corporate environmental performance is a positive driver of firm value and performance (Karpoff et al. (2005), Konar and Cohen (2001), Matsumura, Prakash, and Vera-Munoz (2014)). As a result, analysts increasingly incorporate value-relevant environmental information in their reports to guide forecasts (Jemel-Fornetty, Louche, and Bourghelle (2011)). For instance, in 2013, approximately 27,000 analyst reports include an analysis of a firm's environmental performance (Dong et al. (2017)). Empirical evidence also shows that corporate environmental performance is a significant contributor to analyst recommendations, suggesting that analysts pay close attention to environmental issues (Eccles, Serafeim, and Krzus (2011), Ioannou and Serafeim (2015)).

Analysts can, therefore, play a *direct monitoring* role and contribute to the detection of corporate misbehaviors as "whistle blowers" (Dyck et al. (2010)). In particular, analysts are able to collect information through both public and private channels (e.g. tracking corporate disclosures and corporate site visits) and raise their concerns in corporate conference calls (Chen et al. (2015)). During conference calls, analysts have the opportunity to ask a broad spectrum of questions not only about financial but also non-financial issues such as environmental performance (Henry, Jiang, and Rozario (2021)). In doing so, analysts may uncover new (environmental related) information that was previously unavailable to outsiders. Consistent with this, Huang, Lehavy, Zang, and Zheng (2018) show that analysts provide new exclusive topics of discussion in their reports beyond what was discussed during conference calls. Through these various monitoring activities, corporate environmental performance and policies are likely to be actively and continuously scrutinized by analysts.

Analysts can also play an *indirect monitoring* role by disseminating information to capital markets through media and research reports (Miller (2006)). This reduces the monitoring costs for other stakeholders (e.g. institutional investors) when monitoring firm managers (Chen et al. (2018)). More specifically, analysts can provide and contextualize abstract environmental information that makes it easier for institutional investors to monitor these issues, thereby facilitating and complementing monitoring by institutional investors.

Importantly, the effects of direct and indirect monitoring by analysts not only increase the probability of detecting corporate environmental misbehaviors, but also the consequences of these misbehaviors. These consequences can be severe. Anecdotal evidence suggests that firms' environmental misbehaviors can lead to analysts issuing unfavorable stock recommendations and downgrades.[4] Environmentally harmful behaviors (e.g. toxic pollution and EPA violations) can also damage market value and performance of the firm through higher litigation risk and penalties imposed by regulatory agencies (Karpoff et al. (2005)), reputational loss (Porter and Van der Linde (1995)), difficulties in retaining executives (Levine, Lin, and Wang (2018)), and higher financing cost (Chava, (2014), Sharfman and Fernando (2008)). This suggests that capital markets, where analysts contribute to the dissemination of environmental-related bad news, punish firms with poor environmental performance. Overall, the external monitoring hypothesis predicts that greater analyst coverage leads to reductions in toxic emissions by increasing the ex-ante expected cost of a firm's environmentally harmful behaviors.[5]

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[4] For example, on January 27, 2020, an analyst at Zacks downgraded the recommendation of American Electric (NYSE: AEP) from "outperform" to "neutral". The primary reason for the downgrade was AEP's exposure to substantial environmental risks. Annually, 77 million tons of coal are burned by their plants, releasing large amounts of nitrogen, sulfur, mercury, and carbon dioxide into the air.

[5] Alternatively, it might also be argued that financial analysts could exacerbate managerial myopia by imposing excessive short-term pressure on managers to meet earnings forecasts (Dechow, Richardson, and Tuna (2003)), leading to increases in toxic emissions. As part of their responsibilities, analysts often issue earnings forecasts on the short-term future performance (e.g. 1-year EPS forecast) of firms. Accordingly, failure to meet earnings forecasts would lead to negative stock market reactions (Kasznik and McNichols (2002), lower managerial compensation

III. Sample Construction and Identification

A. Pollution Data

The pollution data employed in our analysis comes from the TRI program that was established by the EPA.[6] Since its release, the TRI data has been the primary measure of a plant's environmental performance and is used extensively in various studies (e.g. Akey and Appel (2021)). Beginning in 1986, the TRI program mandates that U.S. plants belonging to public and private firms that: (1) manufacture, process or emit a list of specific hazardous pollutants in an amount greater than the specified threshold; (2) have more than 10 full-time employees and; (3) operate in one of the approximately 400 industries (e.g. manufacturing, mining, and merchant wholesalers) identified at the six-digit North American Industry Classification System (NAICS) level, are required to report the quantity of toxic pollution released into the environment. Currently, the TRI dataset contains information for over 700 individually listed chemicals (33 chemical categories) that are specified as hazardous from around 60,000 plants.[7,8]

(Matsunaga and Park (2001), and forced managerial turnovers (Hazarika, Karpoff, and Nahata (2012)). To meet these forecasts, managers may focus on short-term profit maximization and underinvest in long-term projects (He and Tian (2013)). In particular, myopic managers are likely to decrease investments in pollution abatement technologies and processes in order to increase short-term profit. However, the results of our analysis do not support this view.

[6] More details can be found at https://www.epa.gov/toxics-release-inventory-tri-program

[7] Some reporting requirements (e.g. the list of toxic chemicals and the industries subject to reporting) are changed over time. However, it is not obvious how this could systemically bias our results in any particular direction. For instance, we use a number of pollution outcomes including total pollution. This reduces the effect of any one specific chemical driving our results. Further we also include various fixed effects such as year and industry fixed effects to control for these systemic differences. Refer to https://www.epa.gov/toxics-release-inventory-tri-program/basics-tri-reporting for reporting requirements.

[8] As TRI data are self-reported by individual plants, there could be concerns of misreporting. However, this is unlikely as the EPA provides stringent reporting and monitoring guidelines to ensure accuracy. Further, independent senior officials are required to certify the accuracy and completeness of reported information. Additionally, the EPA frequently initiates civil and administrative penalties for deliberate misreporting, not for reporting high emissions (Greenstone (2003)). For instance, in 2019, the EPA reports issuing a $60,000 fine to a plant owned by Hexion Inc. as the plant "*failed to comply with reporting requirements*".[8] As a result, there is little evidence to suggest systemic misreporting of emissions data (Bui and Mayer (2003)).

Next, we merge plant-level TRI data with the Compustat and the Institutional Brokers' Estimate System (I/B/E/S) to retrieve financial and analyst information for our sample of public U.S. firms. As there is no consistent and common identifier in the TRI, Compustat, and I/B/E/S databases, we use a fuzzy string matching algorithm to match the unique parent company name of each plant with the company name of public firms in Compustat and I/B/E/S. To ensure the accuracy of the match, we manually check our sample firms on several identifiers such as headquarter location, company website, and their DUNS numbers.[9] Next, similar to Akey and Appel (2019; 2021), we drop plants with zero total emissions. We also exclude firms from the financial (SIC codes 6000-6999) and utility industries (SIC codes 4900-4999). Our initial sample (prior to matching and criteria imposed for our identification strategy) consists of 764 unique firms with 5,868 plants for the years 1999-2011.

As the TRI data are provided at the chemical-plant-year level, we aggregate chemical-plant level emissions of all toxic chemicals to the firm-year level to construct firm-level measures of total toxic pollution. More specifically, we follow Berrone and Gomez-Mejia (2009) and Delmas and Toffel (2008) and define a firm's total toxic pollution as the sum of total on and off-site pollution. On-site pollution is the amount of toxic pollution released on-site into the air, water, and ground, while off-site pollution consists of the quantity of toxic pollution transferred to an off-site location for further release or disposal.[10] The two main measures of firm-level pollution we use are: (1) log(TOT_POL), the logarithm of total pollution and; (2) log(TOT_POL_TO_SALES), the logarithm of total pollution scaled by total sales. In additional tests, we also run regressions for the

[9] The DUNS number, issued by Dun & Bradstreet (D&B), is a unique nine-digit business identifier. The DUNS number of public firms is available at https://www.dnb.com/duns-number/lookup.html.

[10] Air pollution is composed of stack emissions and fugitive emissions. Stack emission refers to toxic chemical emissions to the air through confined air streams (such as stacks, ducts or pipes). Fugitive emissions are toxic air emissions that are not released through confined air streams (such as equipment leaks and evaporative losses). Water pollution is the total quantity of toxic pollution released on-site as surface water discharges. Ground pollution is the total quantity of toxic pollution released to the on-site ground.

individual components in total pollution (on-site, off-site, air, water, and ground pollution). We describe this further in Section IV.B.

B. Identification Strategy

1. Two Quasi-experiments: Brokerage Closures and Brokerage Mergers

The most straightforward way to investigate if analyst coverage affects corporate pollution is to regress a firm's toxic emissions on the number of analysts following. However, estimates from this regression are likely to be biased due to endogeneity. Reverse causality is likely to be an important concern as previous studies show that analysts are more likely to cover environmentally-friendly firms (Ioannou and Serafeim (2015), Luo et al. (2015)). Further, unobservable firm heterogeneity (e.g. corporate culture) correlated with both analyst coverage and a firm's environmental policies could also confound estimation results. To address these concerns, we exploit two quasi-natural experiments that create exogenous variations in analyst coverage.

The first quasi-natural experiment is brokerage closures. Kelly and Ljungqvist (2012) show that closures of brokerage firms are largely due to business considerations (such as increased market competition or government regulation) rather than the characteristics of the firms they cover. The second quasi-natural experiment is brokerage mergers. Hong and Kacperczyk (2010) explain that when two brokers merge, analysts are often made redundant. More specifically, if both the acquiring and target brokers have analysts covering the same firm before the merger, the acquiring broker often dismisses at least one analyst from the target broker due to culture clashes and for reasons of redundancy (Wu and Zang (2009)). Therefore, brokerage closures and mergers provide an exogenous decrease in the number of analysts covering a firm that is unrelated to firm-specific characteristics such as environmental policies.[11]

2. Identifying Treatment and Control Firms

To investigate the effect of analyst coverage on corporate pollution, we rely on a standard difference-in-differences (DiD) methodology. To enable us to identify our group of treated firms, we begin by constructing a list of brokerage exits, pooling together both closures and mergers. To identify brokerage closures, we follow the procedure set out in Chen et al. (2015). First, using the I/B/E/S database, we identify a list of brokers that disappeared from the database between 2000 and 2010. Next, we use BrokerCheck to verify the status of disappeared brokers and their closure dates and manually check press releases in Bloomberg, LexisNexis and Google to ascertain its accuracy. We supplement our brokerage closures list with that from Kelly and Ljungqvist (2012) to obtain a sample of 30 closure events from 2000 to 2010.

To identify brokerage mergers and their dates, we follow Hong and Kacperczyk (2010) and Chen et al. (2015) and rely on the Thomson Reuters SDC Mergers and Acquisition database. First, we restrict the primary SIC code of the target and acquirer to be 6211 or 6282 as firms in these industries are more likely to hire sell-side analysts.[12] We then keep only completed mergers and mergers in which 100% of the target broker is acquired. We manually match these mergers to the broker house in the I/B/E/S database and retain only mergers where both the target and acquirer have overlapping stocks. This results in a list of 24 brokerage mergers. In total, similar to prior

[11] The internal validity of the two quasi-natural experiments has been extensively assessed by prior studies that utilize this setting (e.g. Derrien and Kecskes (2013), He and Tian (2013), Hong and Kacperczyk (2010), Kelly and Ljungqvist (2012)).
[12] SIC code 6211 contains Security Brokers, Dealers, and Flotation Companies. Investment Advice firms have SIC code 6282.

studies that utilize brokerage closures (e.g. Chen et al. (2015), Hong and Kacperczyk (2010), Kelly and Ljungqvist (2012)), we have 54 brokerage exits (30 closures and 24 mergers).

Next, we merge our list of 54 brokerage exits with the I/B/E/S unadjusted historical detailed dataset to obtain a panel dataset that includes firms that are covered by brokers that exit (as well as firms that are unaffected by these exits). From this, we construct our estimation window required for the DiD analysis. Event dates are supposedly the dates of brokerage exits. However, it is important to note that the dates (month) of brokerage closures or mergers in our list (from BrokerCheck or the Thomson Reuters SDC Mergers and Acquisition database) do not always correspond with the disappearance date (month) in the I/B/E/S stop file as the completion of a broker closure or merger takes several months. Since there is no way of reconciling the event dates when they differ, we follow prior studies (e.g. Derrien and Kecskes (2013), He and Tian (2013)) and define a 6-month "event period" (denoted t) symmetrically around the disappearance date; three months before (after) the event month.[13]

We then use an estimation window of one year before (t-1) and one year after (t+1) the event period.[14] By exploiting exogenous short-term reductions in analyst coverage and the subsequent effects on firms' pollution emissions, we are able to obtain cleaner estimates, as other brokers or new entries are likely to fill the gap of affected stocks in the long-run (Chen et al. (2015)). Since our event period t spans 6 months, year t-1 is defined as the last fiscal year before the event and t+1 is defined as the first *complete* fiscal year after the event. For example, if a firm

[13] For example, Robertson Stephens was closed in July 2002. Therefore, the event closure period is defined as April 2002 to October 2002. In sensitivity tests, we employ 8-, 4-, and 0-month event periods and find that our main results remain unchanged.
[14] In robustness tests described in Appendix A.3, we also show that our results are robust when we compare outcomes 2-year (t-2 to t+2) and 3-year (t-3 to t+3) prior to and after brokerage exits.

has a December fiscal year-end and the event date is May 31, 2001, year *t*-1 (*t*+1) would be December 31, 2000 (2002), respectively.

We then merge this list of covered stocks from the I/B/E/S dataset with firms in our pollution sample and require firms to have Compustat financial information and TRI pollution data for all years from *t*-1 to *t*+1. From this, we identify treated and control firms. Treatment firms are firms that have reduced analyst coverage as a result of brokerage closures or mergers. For brokerage closures, treated firms are defined as firms covered by the closed broker in the year before the event (*t*-1), which continue to exist in the I/B/E/S database to the year after closure (*t*+1). Similarly, for brokerage mergers, treated firms are firms covered by both the target and acquirer in year *t*-1 and continue to be covered by the acquirer in year *t*+1.[15] The remaining firms which are unaffected by brokerage exits are control firms. This constitutes our unmatched sample that consists of 326 (764) unique treatment (control) firms with 35 brokerage exits between 1999 and 2011.[16]

3. Matched Treatment and Control Firms

From the unmatched sample, we use a propensity score matching (PSM) method to construct matched treated and control firms. We use a matched sample for our analyses as treated and control firms could differ across various firm characteristics. We perform a one-to-one nearest-neighbor matching with replacement on several firm characteristics (FIRM_SIZE, BOOK_TO_MARKET, ROA, TANGIBILITY, and two-digit SIC code) that are likely to predict treatment prior to

[15] We impose this criterion to ensure that the treated firm is not “stopped” in the I/B/E/S database; this alleviates concerns that the analyst terminates coverage of the treated firm in anticipation of specific corporate policies such as pollution (Derrien and Kecskes (2013)).
[16] It is worth noting that a firm could be treated multiple times; i.e. affected by more than one brokerage exits during our sample period.

brokerage exits (in t-1).[17] Our final matched sample consists of 254 (116) unique treated (control) firms with 1,212 firm-year observations (606 firm-year observations per treated and control group).

To ascertain the validity of our matching process, we conduct t-tests for differences (displayed in Panel B of Table 1) in the means of firm characteristics between matched treated and control firms in the year prior to brokerage exits (t-1). The means of firm characteristics are largely indistinguishable after matching, suggesting that our matching process is successful in balancing ex-ante differences in firm characteristics between treatment and control firms.

C. Empirical Model

To investigate the effect of an exogenous decrease in analyst coverage on corporate pollution, we employ a difference-in-differences estimator to compare the change in corporate pollution the year before and after brokerage exits for treatment and control groups. We estimate the following empirical model:

$$(1) \qquad y_{i,t} = \alpha + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$$

where i and t index firm and year. y is one of two measures for a firm's total toxic emissions, namely log(TOT_POL)$_{i,t}$ and log(TOT_POL_TO_SALES)$_{i,t}$. TREATMENT$_{i,t}$ is an indicator variable which equals one if the firm has experienced an exogenous decrease in analyst coverage due to brokerage closures or mergers and zero otherwise. AFTER$_{i,t}$ is an indicator variable equal to one in the year after brokerage exits (t+1) and zero in the year before (t−1). Our variable of interest in equation (1) is the coefficient β_1 on the interaction item. It captures changes in corporate pollution for firms after exogenous decreases in analyst coverage relative to before, and relative to

[17] Appendix A.2 describes in more detail the matching process. The matching strategy does not affect our main findings. We obtain similar results when conducting our analysis using an unmatched sample as well as when we apply different matching strategies (See Appendix A.3).

a group of matched control firms. The vector $X_{i,t}$ contains firm-specific control variables. In our main regression, we have two sets of fixed effects: (1) Firm FE and Year FE and; (2) Firm FE and Industry-Year FE. We describe this further in Section IV.A.

[Insert Table 1 here]

To mitigate the effect of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. Summary statistics are presented in Panel A of Table 1. Appendix Table A.1 shows definitions of all variables that we use. On average, firms in our sample release 2.26 million pounds of toxic pollution into the environment in a year; 2.08 million pounds are released on-site while 0.18 million pounds are released off-site.

D. Verification and Diagnostics Tests

Our identification strategy relies on the idea that brokerage exits (closures and mergers) lead to exogenous decreases in the analyst coverage of treated firms. We verify this in Figure 1 by plotting the mean difference in analyst coverage between the treatment and control groups (treatment - control) around a 3-year window before (t-3) and after (t+3) brokerage exits. As observed, the mean difference is approximately constant prior to brokerage exits (from years $t-3$ to $t-1$). Crucially, mean analyst coverage for treated firms decreases by approximately one analyst between year t-1 and year t+1. The magnitude of this decrease is consistent with prior studies (e.g. Chen and Lin (2017), Derrien and Kecskes (2013)).[18] This provides supporting evidence that brokerage exits lead to a reduction in analyst coverage for treated firms.

[18] In unreported results, we further confirm that analyst coverage decreases for treated firms after brokerage exits. Specifically, we conduct a DiD estimation with analyst coverage as the dependent variable. Reassuringly, the coefficient of the interaction term (TREATMENT × AFTER) is negative and statistically significant with a t-value of -4.58. This is consistent with Figure 1 that shows treated firms lose about one financial analyst after brokerage exits as compared to control firms.

[Insert Figures 1 and 2 here]

A key identifying assumption in DiD analysis is the parallel trends assumption. It states that absent treatment, changes in the outcome variable would have evolved similarly for both treatment and control groups. As this assumption cannot be directly tested, we rely on the conventional approach of showing similarity in the pre-event period to provide some support for it. We follow prior studies (e.g. He and Tian (2013)) and plot in Figure 2 the mean difference in total pollution between treatment and control firms for a 3-year window around brokerage exits. Notably, the figure shows that the net difference in total pollution between treated and control firms remains stable (similar trends) prior to brokerage exits (*t*-3 to *t*-1). We also observe that the net difference in pollution between the two groups increases from year *t*-1 to year *t*+1. This suggests that brokerage exits have a significant impact on pollution outcomes. Overall, the results of our two diagnostic tests lend confidence to the validity of our empirical strategy.

IV. Main Results

A. Baseline Results: Analyst Coverage and Toxic Pollution

Table 2 shows the baseline estimation results of decreases in analyst coverage on toxic pollution for our matched sample of treated and control firms following equation (1). In columns 1-3, the dependent variable is the firm-year nominal measure of total toxic pollution (log(TOT_POL)), while columns 4-6 display firm-year sales-adjusted toxic pollution (log(TOT_POL_TO_SALES)).

We start the estimation without control variables in column 1 and then include control variables in column 2. Firm and year fixed effects are included in columns 1-2 to control for time-invariant firm and year characteristics. This implies that any time-invariant firm-specific omitted variables such as a firm's environmental culture or propensity for pollution is unlikely to drive our results. Further, since we also include year dummies, any systemic changes in pollution (e.g. environmental awareness) are controlled for. In column 3, we include firm and industry-year fixed effects (industry is defined at the two-digit SIC code level). The inclusion of industry-year interacted fixed effects means that our analysis is comparing treated and control firms in the same industry in the same year. This rules out any alternative explanations such as time-varying regulatory changes or industry-technological shifts.

[Insert Table 2 here]

Throughout all specifications in columns 1-3, we observe that the coefficient on the variable of interest TREATMENT × AFTER is positive and statistically significant at the 1% level. This is in line with the external monitoring hypothesis that treatment firms significantly increase their nominal emissions of toxic pollution in response to decreases in analyst coverage. In terms of economic magnitude (e.g. column 3), we observe that treatment firms release 0.361 higher log toxic chemicals into the environment after decreases in analyst coverage (which is approximately 13% of the dependent variable's standard deviation). This translates into an increase of 36.1% in log total emissions. The size of the magnitude is comparable to other studies that analyze firm-level emissions. For instance, Shive and Forster (2020) find that independent private firms release 33% less greenhouse gas as compared to public firms.[19]

In columns 4-6, we obtain similar results when we use a scaled measure of pollution. The scaled measure captures the firms' eco-efficiency and mitigates the concern that the increase in pollution is driven by increases in production (Konar and Cohen (2001)). When using this measure,

[19] In Appendix Table A.4, we show that the increase is mainly concentrated amongst small polluters; that is, firms with ex-ante lower levels of emissions. This suggests that the percentage increase we observe might not necessarily translate into large nominal increases in emissions.

the coefficient on TREATMENT × AFTER continues to remain robust and has economic magnitudes similar to the unscaled measure in columns 1-3.

Our baseline results survive a battery of robustness tests. As discussed in detail in Appendix A.3, our results are robust to the use of alternative estimation windows, different matching strategies, the exclusion of the financial crisis period, and the exclusion of multiple treatment events. Overall, we find strong evidence to support our hypothesis that analysts play an active monitoring role in restraining firms' emissions of toxic pollution.

B. Additional Tests

So far, our baseline results show that a decrease in analyst coverage leads to increases in total toxic pollution by firms. As total pollution is made up of on-site air, water, and ground pollution and off-site pollution, we conduct further analysis to investigate which components of pollution are likely to matter. As observed, the increase in total pollution is driven by on-site and air pollution (Panels A and B of Appendix Table A.5 respectively). When firms are faced with weaker external monitoring through a reduction in analyst coverage, these firms are more likely to increase on-site pollution rather than transfer the pollution to costlier off-site locations for further release or disposal at specialized waste management facilities (Kim et al. (2019a)).

Appendix A.6 describes additional tests where we use EPA enforcement actions as an alternative measure of firms' environmental misbehavior. In particular, it measures non-compliance with EPA's regulations and links firms' pollution outputs to regulatory and litigation risks. Consistent with the external monitoring hypothesis, we find that treated firms receive more EPA enforcement actions, particularly non-judicial enforcement actions, after decreases in analyst monitoring.

V. Cross-sectional Analysis on the Effects of Analyst Coverage

To the extent that analysts reduce corporate pollution by performing an external monitoring role, which substitutes for alternate monitoring mechanisms, we expect the effect of analyst coverage to be stronger when alternate monitoring forces are weak. Specifically, we test whether the effects of analyst monitoring vary in predictable ways with initial analyst coverage, corporate governance and regulatory scrutiny.

A. Analyst Coverage and Initial Analyst Coverage

We first investigate the effect of initial analyst coverage (the coverage before brokerage exits) on the relation between analyst coverage and corporate pollution. If fewer analysts are covering a stock, there should be a larger effect on subsequent firm policies (higher levels of toxic emissions) following a reduction in analyst coverage (Hong and Kacperczyk (2010)). This reflects the idea that the marginal benefits brought upon by an additional monitor should matter most when monitory oversight is weak; i.e. when initial coverage is low (Irani and Oesch (2013)).

To test if the treatment effect differs for firms with low or high initial analyst coverage, we follow Irani and Oesch (2013, 2016) and Qian et al. (2019) and estimate a variant of equation (1) where we interact our TREATMENT × AFTER variable with dummies indicating if a treatment firm has high or low initial analyst coverage:

$$(2) \quad y_{it} = \alpha + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{LOW\_INITIAL\_COVERAGE}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{HIGH\_INITIAL\_COVERAGE}_{i,t} + \beta_3 \text{TREATMENT}_{i.t} + \beta_4 \text{AFTER}_{i,t} + \delta \text{X}_{i,t} + \varepsilon_{i,t}$$

where LOW (HIGH)_INITIAL_COVERAGE$_{i,t}$ is a dummy variable that equals one for treatment firms in the bottom (top) tercile of analyst coverage prior to brokerage exits.[20] Our coefficients of interest in equation (2) are β_1 and β_2 on the triple interaction terms. The coefficients of these variables measure the differential treatment effect that low (high) initial analyst coverage has on toxic pollution for firms affected by brokerage exits.[21]

[Insert Table 3 here]

The results are shown in Table 3. We find that the effect of analyst coverage on pollution is stronger for treated firms with low initial analyst coverage. As observed, the coefficients on TREATMENT × AFTER × LOW_INITIAL_COVERAGE are positive and statistically significant at the 1% level for firms with low initial analyst coverage. By contrast, the coefficient on the triple interaction term TREATMENT × AFTER × HIGH_INITIAL_COVERAGE is statistically weak. The results suggest that the effect of a decrease in analyst coverage on increasing corporate pollution is stronger in treated firms that experience a relatively larger marginal decline in analyst monitoring intensity. This interpretation is also supported when we conduct *t*-tests for statistical differences in coefficients for the two triple interaction terms (*p*-values for differences are reported in Table 3), indicating that the effects of initial analyst coverage that we find for both sets of firms are statistically different. Overall, this is consistent with findings in previous studies (e.g. Irani and Oesch (2013)) that the effects of decreases in analyst coverage on firms are mainly driven by the subsample with low initial analyst coverage.

[20] The average analyst coverage in the bottom (top) tercile group is 4.5 (17.4). We select our cut-off to be terciles to follow He and Tian (2013) and Chen et al. (2015). The results are similar when we compare the top and bottom quartiles.

[21] It is worth noting that equation (2) can also be modified to test for other differential treatment effects by replacing LOW (HIGH)_INITIAL_COVERAGE with a dummy variable that equals one if the treated firm has a value of this particular variable lower (higher) than a threshold that is specified.

B. Analyst Coverage and a Firm's Corporate Governance

Next, we examine the effect of corporate governance on the relation between analyst coverage and corporate pollution. In this regard, financial analysts play an important external governance role in mitigating managerial agency problems and may serve as substitutes for traditional governance mechanisms (e.g. Chen et al. (2015)). To the extent that the monitoring role of financial analysts matters, we would expect the effect of analyst coverage on corporate pollution to be more pronounced for firms with weaker corporate governance.

We use two proxies for the quality of a firm's corporate governance. The first proxy is product market competition. Prior research shows that highly competitive product markets are effective in restraining rent-seeking activities by managers and motivating them to improve corporate social and environmental performance (Flammer (2015)). As such, we expect firms operating in highly competitive product markets to be better governed and have more environmentally conscious policies. Consequently, analyst coverage should matter most (least) for firms that operate in uncompetitive (competitive) product markets. To that end, we rely on the total product similarity measure developed by Hoberg and Phillips (2016) to quantify competitive threats faced by a firm.[22] We define LOW (HIGH)_COMPETITION as a dummy that equals one for treated firms facing low (high) competitive product market threats as measured by the median product similarity the year prior to brokerage exits.

Our second proxy for corporate governance is the E-index of Bebchuk, Cohen, and Ferrell (2009). It measures how much rights a firm gives to shareholders as well as the ease of being

[22] The total product similarity is the sum of the pairwise similarity scores between a given firm and all other firms in a given year. The pairwise similarity score is constructed using textual analysis of each firm's product descriptions obtained from their 10-K files. The pairwise similarity score is high between a firm and its competitors if the words used to describe their products are similar. Therefore, total product similarity can be used as a measure of the competitive threats faced by a firm. The product similarity measure can be downloaded from: https://hobergphillips.tuck.dartmouth.edu/

acquired.[23] Empowering shareholders and having provisions that make it easier for a firm to be taken over may serve as effective governance mechanisms that incentivize managers to avoid stock price declines caused by poor environmental performance (Kock, Santalo, and Diestre (2012)). This perspective predicts that better corporate governance (lower E-index) would restrain managers' incentives to harm the environment. As before, we classify treated firms as having good (LOW_E-INDEX) or poor (HIGH_E-INDEX) corporate governance based on the median E-index the year before brokerage exits. Thus, we expect that analyst coverage should matter most for firms that are less well governed (have a higher E-index).

Table 4, estimated using a similar model as equation (2), shows the results. As observed in columns 1-2 we find increases in toxic emissions for treated firms operating in non-competitive product markets; the coefficient on TREATMENT × AFTER × LOW_COMPETITION is positive and statistically significant at the 1% level. While we still find some evidence that treated firms in competitive markets increase their toxic emissions (10% statistical significance on TREATMENT × AFTER × HIGH_COMPETITION), *t*-tests for differences in coefficients for the two triple interaction terms reveal that the effect of decreased analyst coverage on corporate pollution is more statistically pronounced for treated firms facing lower levels of product market competition (which are less likely to be better governed) after brokerage exits.

[Insert Table 4 here]

The findings are similar when we use the E-index to proxy for corporate governance. In columns 3-4, we continue to find that the effect of a decrease in analyst coverage on corporate pollution is more pronounced for firms with weaker corporate governance (i.e. firms with higher

[23] The E-index aggregates six antitakeover provisions: staggered boards, limits to shareholder bylaw amendments, poison pills, golden parachutes, and supermajority requirements for mergers and charter amendments.

E-index) as compared to well governed firms.[24] Overall, the results are consistent with the notion that the monitoring role of financial analysts serves as a substitute for traditional corporate governance mechanisms. This is also consistent with evidence documented by Shive and Forster (2020) that corporate governance matters in restraining the emission of greenhouse gases.

C. Analyst Coverage and the Intensity of Regulatory Scrutiny

In the last of our cross-sectional tests, we investigate the moderating effect of regulatory monitoring on the relation between analyst coverage and corporate pollution. Regulators can be influential in shaping and enforcing corporate environmental policies (Delmas and Toffel (2008)). In particular, firms that are monitored more closely by regulators are more likely to comply with environmental regulations (Cohen (1998)) and voluntarily participate in environmental programs (King and Lenox (2000)), leading to better environmental performance (Earnhart (2004)). To the extent that the monitoring role of analysts serves as a substitute for regulatory monitoring, we expect the effect of analyst coverage on corporate pollution to be more pronounced for firms that are monitored less intensely by regulators.

To proxy for the intensity of regulatory monitoring, we rely on the geographical distance between plants of the firm and EPA offices (Kedia and Rajgopal (2011)). A greater distance from a plant to an EPA regulatory office increases the cost of regulatory monitoring and enforcement (e.g. collection of information and site inspections). Therefore, we expect the EPA to be able to monitor and detect environmental misbehaviors more effectively for proximate plants.

[24] As robustness checks, we also show in Appendix Table A.7 that our findings continue to hold when we use other proxies for corporate governance, namely the G-index by Gompers, Ishii, and Metrick (2003), the co-opted board independence measure by Coles, Daniel, and Naveen (2014) and a measure of industry concentration based on the sales market share of the top four firms in each industry (Eckbo (1985)).

We begin by identifying the regional offices of the EPA.[25] Figure 3 shows the geographical distribution of the ten regional offices and the specific states that fall under the purview of these offices. As pollution and enforcement occur at the plant level, we first calculate the geographical distance (DISTANCE) from each plant to the EPA office that supervises it.[26] We then construct a firm-year distance measure of regulatory intensity by taking the average distance of each plant owned by the firm to its relevant EPA office (AVG_DISTANCE). Therefore, a larger (smaller) AVG_DISTANCE represents weaker (stronger) regulatory scrutiny by the EPA for a particular firm. We proceed to divide our sample into low and high average distance groups the year before brokerage exits; LONG (SHORT)_DISTANCE is a dummy variable that equals one for treated firms above (below) the median average distance (which is 100.42 miles).

[Insert Table 5 here]

The results in Table 5 are consistent with our expectations. Firms located farther away from EPA regional offices increase their toxic pollution more than proximate firms after brokerage exits; the coefficient on TREATMENT × AFTER × LONG_DISTANCE is positive and statistically significant at the 1% level. Overall, the findings suggest that analysts play an important role in reducing corporate pollution in the absence of strong regulatory scrutiny, consistent with a substitution effect between analyst and regulatory monitoring.

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[25] Each office is responsible for the supervision of plants in several neighboring states. For example, regional office 1 is located in Boston, MA, and is responsible for the states of CT, MA, ME, NH, RI, and VT.

[26] To calculate the geographical distance between each plant and its relevant EPA regional office, we follow Coval and Moskowitz (1999) and define the distance between locations 1 and 2 as follows: $Distance_{12} = arccos\{cos(lat1)\,cos(lon1)\,cos(lat2)\,cos(lon2) + cos(lat1)\,sin(lon1)\,cos(lat2)\,sin(lon2) + sin(lat1)\,sin(lat2)\}\,2\pi r/360$ r $\approx$3963 statute miles (the radius of the earth) while lat and lon are latitude and longitude respectively. The TRI database provides the longitude and latitude of each plant, while addresses of the ten EPA regional offices can be found on the EPA's website (https://www.epa.gov/aboutepa/visiting-regional-office).

VI. Potential Channels

Our analyses thus far point to a causal relationship between analyst coverage and corporate pollution. Building on this finding, this section explores four non-mutually exclusive channels through which decreases in analyst coverage might lead to higher corporate pollution.

A. Direct Monitoring: Earnings Conference Calls

The first channel—earnings conference calls—suggests that decreases in analyst coverage lead to higher levels of corporate pollution by reducing direct monitoring by analysts during conference calls. Earnings conference calls are an important platform for firms to disclose information to capital markets and are informative to stakeholders of the firm (Heinrichs, Park, and Soltes (2019)). As important "whistle blowers" analysts can directly monitor firms during the Q&A (question-&-answer) sessions of conference calls by raising their concerns to senior management and scrutinizing firm policies (Matsumoto, Pronk, and Roelofsen (2011), Mayew and Venkatachalam (2012)).

To that end, analysts can play a direct monitoring role by raising environmental-related questions (e.g. regarding toxic emissions and pollution abatement expenses) during Q&A sessions.[27] Accordingly, information uncovered from these sessions is also contextualized and incorporated in future analyst reports (Huang et al. (2018)). In the presence of such direct monitoring activities, managers are strongly incentivized to improve corporate environmental performance. Consequently, an exogenous decrease in analysts may lead to reduced analyst

[27] Appendix Table A.8 presents some examples of environmental-related questions raised by analysts. Even if these questions garner a non-response (in the form of refusals or reliance on prepared scripts), this might still be informative to investors who may well then interpret this as an adverse signal and react negatively (Gow, Larcker, and Zakolyukina (2021), Hollander, Pronk, and Roelofsen (2010), Lee (2016)). Therefore, regardless of the informativeness of answers given by managers, analysts can perform a monitoring role just by raising questions during conference calls.

involvement in conference calls. This, in turn, reduces the scrutiny faced by managers regarding environmental performance improvements, leading to higher toxic emissions.

To test this direct monitoring channel, we manually collecte 1,995 quarterly earnings conference call transcripts for firms in our sample from LexisNexis and Capital IQ. Notably, we find that 74% of the analysts that were lost as a result of brokerage exits participated in these earnings calls the year prior to these exits. This is comforting as it provides cursory evidence that the vast majority of analysts are actively involved in monitoring activities. To ensure we are capturing effects from analysts that were lost, we retain only earnings calls in which these lost analysts participated in the year prior to their brokerage exits.

Using a list of keywords related to corporate environmental performance, we then perform textual analysis of the Q&A sections of the earnings calls to identify environmental-related questions put forth by analysts.[28] In total, we identify 134 conference call transcripts (6.77% of the total number of quarterly transcripts) where environmental-related questions were raised by analysts. Using this, we construct a dependent variable, ENVIRON_QUESTIONS, that equals one if at least one environmental-related question was raised in a particular firm-year, and zero otherwise. About 14.34% of firm-year observations in our sample saw at least one such question.

[Insert Table 6 here]

We investigate in Table 6 whether decreases in analyst coverage reduce the probability of environmental-related questions being raised by analysts during conference calls. Using a probit model in columns 1-2, we find that treated firms, which had decreased analyst coverage, are significantly less likely to receive environmental-related questions during conference calls. In

[28] The keywords include “environmental”, “environmentally”, “environmental protection agency”, “clean air act”, “pollut*”, “emission”, “climate change”, “global warming”, “coal cleaning”, “green energy”, “renewable”, and “waste”. We manually check the results of our textual analysis to ensure that the identifying keywords are indeed used in a context related to environmental performance.

addition, we observe that the total number of environmental-related questions posed by analysts, #_ENVIRON_QUESTIONS, decreases (columns 3-4). Overall, our results suggest that raising environmental-related questions during conference calls, as a form of direct monitoring by analysts, is an important channel that affects firms' toxic emissions.

B. Indirect Monitoring: Institutional Investors

The second channel—monitoring costs for institutional investors—posits that decreases in analyst coverage lead to increases in corporate pollution by reducing the role and influence of institutional shareholders in shaping corporate environmental policies. Indeed, institutional investors are increasingly incorporating environmental issues into their investment decisions and exert pressure on managers to enhance environmental performance (Dyck et al. (2019), Krueger et al. (2020)). For instance, Kim et al. (2019a) document that local institutional ownership reduces corporate pollution. Consequently, institutional investor monitoring can lead to improvements in firms' corporate environmental ratings (Chen et al. (2020)). However, the cost of monitoring to institutional investors is dependent on the firm's information environment. In the course of their duties, analysts disseminate information on a firm's environmental policies to capital markets (Miller (2006)). This "indirect monitoring" role undertaken by analysts reduces the monitoring cost for other stakeholders, in particular institutional investors (Chen et al. (2015)). In support of this, prior studies find that institutional shareholders are more likely to shy away from firms after analyst coverage decreases as they anticipate these firms becoming harder to monitor (e.g. Bushee and Noe (2000), O'Brien and Bhushan (1990)). Consequently, this reduces the role and influence of institutional shareholders in shaping a firm's environmental policies.[29]

[29] It is also possible that changes in the information environment as a result of the direct and indirect monitoring roles played by analysts could lead to higher levels of corporate pollution, particularly through its impact on firms' ability

To test this channel, we first use the total equity ownership of all institutional investors as a dependent variable. The results are reported in columns 1-2 of Panel A in Table 7. Column 2 shows that relative to control firms, the institutional ownership of treated firms decreases by 3.8% after decreases in analyst coverage.[30] This suggests that increases in monitoring cost pertaining to environmental policies of the firm cause institutional investors to shy away from treated firms.

[Insert Table 7 here]

To sharpen our analysis, we focus on groups of institutional investors that are more long-term oriented and environmentally conscious, as different institutional investors have heterogeneous preferences and investment strategies (Hong and Kacperczyk (2009), Hong and Kostovetsky (2012)). We identify two such groups of institutional investors that might care more about a firm's long-term environmental performance; namely, quasi-indexers and public pension funds. Quasi-indexers are long-term institutional investors with the ability to monitor managers and influence corporate decisions through large voting blocs (Appel, Gormley, and Keim (2016), Bushee (2001)). As quasi-indexers have relatively long investment horizons, they are more likely to impose pressure on managers to improve environmental performance (Chen et al. (2020), Kim et al. (2019a)). In columns 3-4 of Panel A, using Bushee's (2001) classification of institutional investors, we find that the ownership of quasi-indexers in treatment firms decreases by 4.6%-4.8% after decreases in analyst coverage relative to control firms.

Next, we focus on the equity ownership of public pension funds. These funds have a relatively long investment horizon and are often under pressure to invest in a socially acceptable

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to raise funds. Specifically, informationally opaque firms might find it harder to raise external funds, become more financially constrained and, consequently, underinvest in pollution abatement technologies and processes (Xu and Kim (2022)). Using text-based measures by Hoberg and Maksimovic (2015), we do not find evidence of this interpretation; treated firms do not become more financially constrained after brokerage exits.

[30] This finding is in line with evidence from Ellul and Panayides (2018) who examine analyst coverage terminations on the quarterly holdings of institutional investors.

manner. For instance, pension funds are often reluctant to invest in "sin" stocks (Hong and Kacperczyk (2009)) and are more likely to initiate social and environmental shareholder proposals (Chidambaran and Woidtke (1999)). In addition, public pension funds are "independent" in that they usually do not have business relationships with the firms they invest in, and are thus more willing to monitor and influence management (Chen, Harford, and Li (2007)). Again, following Bushee's (2001) classification, in columns 5-6 of Panel A, we find that the ownership of public pension funds in treatment firms decreases by 0.2 % after decreases in analyst coverage relative to control firms.

Having established that decreases in analyst coverage are associated with institutional investor exits that weaken institutional investor monitoring, we further explore the indirect monitoring role of analysts by examining whether analyst monitoring serves as a complement to institutional investor monitoring. If analysts play a complementary role in facilitating monitoring by institutional investors, we should expect to see larger (smaller) decreases in pollution for treated firms with high (low) levels of institutional ownership. This follows the idea that monitoring by institutional investors is most effective when institutional holders hold a higher stake in the firm as compared to when they hold a smaller stake.

Using an empirical design similar to equation 2, we compare the effects of decreases in analyst coverage on corporate pollution for treated firms with high (above-median) versus low (below-median) institutional ownership in the year prior to brokerage exits. Consistent with the notion that analysts facilitate institutional investor monitoring, we find in Panel B of Table 7 that the effect of analyst coverage on pollution is more pronounced when institutional investors, quasi-indexers and public pension funds hold high equity ownership stakes as compared to when they hold low equity stakes. In summary, this section shows how analysts can play an indirect monitoring role by influencing the presence and efficacy of monitoring by institutional investors. Together with the direct monitoring role documented in Section VI.A, our findings provide evidence that analysts undertake important external governance roles with regard to firms' environmental performance.

C. Investments in Pollution Abatement

The third channel—investments in pollution abatement—states that decreases in analyst coverage lead to more corporate pollution through underinvestment in pollution abatement technologies. To mitigate toxic pollution, firms can invest in pollution abatement activities such as developing green technologies (Akey and Appel (2021)). However, investments in abatement are costly. When not properly monitored, firms are likely to have reduced incentives to invest in abatement technologies if the probability of being detected and punished for poor environmental performance is low (Hart and Zingales (2016)). Further, firms are more likely to reduce investments in pollution abatement if they are not rewarded for it by market participants. From this perspective, analysts play an important role in reducing the information asymmetry of a firm's environmental policies to capital markets (Derrien and Kecskes (2013), Kelly and Ljungqvist (2012)).

We employ two proxies for investments in pollution abatement to test if firms reduce this type of investment after decreases in analyst coverage. The first is firm-year expenditure on environmental activities (log(ENVIRON_EXPEND)).[31] Columns 1-2 of Table 8 report the results. Consistent with our expectation, we find that treated firms decrease their log environmental

[31] We manually collect environmental expenditures data from the 10-K files under the sections outlined "environmental matters" or "environment". Approximately 20.3% firm-year observations in our sample report positive environmental expenditures. Following Fernando, Sharfman, and Uysal (2017), if firms do not disclose their environmental expenditures, we set the value as zero. Average environmental expenditures in our sample as a percentage of total capital expenditures is 9.83% and is comparable to the 9.77% reported in Clarkson et al. (2004).

expenditure by approximately 34.7% (column 2) after decreases in analyst coverage. This suggests that increases in pollution at treated firms can be partly attributed to lower capital expenditure on abatement activities and processes related to the environment.

[Insert Table 8 here]

Our second proxy for investments in pollution abatement is the number of green patents filed in a firm-year. Green patents arise as a result of a firm's investments in environmental innovation and green technologies and, therefore, proxy for the firm's expenditure in this area (Chu and Zhao, 2019).[32] We use the number of green patents (GREEN_PATENTS) as our dependent variable and treat this as zero if no patents are filed. We also include an additional indicator variable as a control for zero-patent (ZERO_PATENT) firms as some firms may forgo patent protection to avoid disclosing proprietary information (Lerner (2002)). As there is a time lag between initial investments in green innovation and its subsequent innovation outputs, we employ a longer time window for this test. Specifically, we compare the number of green patents in the two years before and after decreases in analyst coverage and show the results in columns 3-4 of Table 8.[33] The negative coefficient on TREATMENT × AFTER indicates that the number of green patents declines significantly after decreases in analyst coverage. Overall, our evidence suggests that decreases in pollution abatement investments is a channel through which reduced analyst coverage increases corporate pollution.

[32] We obtain patent data from a database compiled by Kogan, Papanikolaou, Seru, and Stoffman (2017) that includes detailed patent information from 1926 to 2010. We identify innovations in green technologies and processes based on the classification in Carrión-Flores and Innes (2010) and then calculate the number of green patents filed in each firm-year. Green innovation includes patents related to wind energy, solid waste prevention, water pollution, recycling, alternative energy, alternative energy sources, geothermal energy, air pollution control, solid waste disposal, and solid waste control.

[33] As a robustness test, we follow He and Tian (2013) and utilize a longer time horizon of year *t*-3 to year *t*+3 and find qualitatively similar results.

D. Environmental Internal Governance

The final channel we investigate—environmental internal governance—examines if analyst coverage can affect corporate pollution by influencing the design of internal governance mechanisms that promote firms' pro-environmental behavior. To the extent that analyst coverage increases the consequences of environmental misbehaviors (e.g. issuing unfavorable stock recommendations), firms (the board of directors in particular) would respond by establishing internal governance mechanisms tailored to improve environmental performance. Conversely, when analyst coverage decreases, the incentives to maintain internal governance mechanisms that promote pro-environmental policies may also be scaled back. Specifically, we focus on two such mechanisms related to executives' compensation contracts and sustainability committees.

Compensation contracts are effective mechanisms to align the interest of managers to various objectives set by the firm (Frydman and Jenter (2010)). Incentive contracts that take into account environmental performance can thus be an effective governance tool to incentivize managers to increase green innovations and reduce toxic emissions (Flammer, Hong, and Minor (2019)).[34] We search for environmental-related keywords in compensation contracts of named-executives of the firm and construct a firm-year dummy variable that equals one if there is at least one executive that has their compensation contract linked to environmental performance, and zero otherwise (ENVIRON_COMP).[35] Results from a probit model in column 1 of Table 9 show that

[34] In recent years, there has been an increasing number of compensation contracts linking executive pay to social and environmental performance. For instance, the proportion of S&P 500 firms offering social and environmental performance-based compensation increased from 12% in 2004 to 37% in 2013 (Flammer et al., 2019).

[35] Following previous studies on performance-based compensation (e.g. Bennett, Bettis, Gopalan, and Milbourne (2017), Bettis, Bizjak, Coles, and Kalpathy (2018)), we rely on the information provided by ISS Incentive Lab database for the largest 750 public firms. We define executives' compensation contracts as containing environmental targets if compensation contracts mention keywords "environment", "emission", "waste", "toxic", or "release", and zero otherwise. In our sample, about 5% of firm-year observations have environmental-related incentives in their executives' contracts.

firms that experience decreases in analyst coverage are significantly less likely to link executives' pay to environmental performance.

[Insert Table 9 here]

Second, we examine the presence of sustainability committees as another environmental governance mechanism. Firms assemble board committees for different strategic goals and may set up a sustainability committee to monitor and advise managers on issues of sustainability awareness and goals (Fu, Tang, and Chen (2020)). Indeed, previous studies find that the presence of such committees enhances corporate environmental sustainability (Dixon-Fowler, Ellstrand, and Johnson (2017)). However, the creation and subsequent participation in sustainability committees require considerable time and effort from directors and managers. Therefore, when external monitoring is decreased as a result of decreases in analyst coverage, we expect that these committees are less likely to be formed.

We again use a probit model to examine the probability of having a sustainability committee in treatment firms as compared to control firms.[36] SUSTAIN_COMM is a firm-year dummy variable which equals one if a firm has a sustainability committee, and zero otherwise. Our test focuses on the two years before and after decreases in analyst coverage, as setting up a new board committee may require more time than other firm policy responses. As observed in column 2 of Table 9, treated firms are less likely to establish a sustainability committee after decreases in analyst coverage. Overall, we find evidence that decreases in analyst coverage can lead to increases in firms' toxic emissions by curtailing internal governance mechanisms that promote pro-environmental policies

[36] We first obtain the names of board committees from the BoardEx database. Following Fu et al. (2020), committees with the word "sustainability", "sustainable", "responsibility", "ethics" or "environment" in their names are coded as sustainability committees.

VII. Conclusions

This paper exploits two quasi-natural experiments (brokerage closures and mergers) to investigate the monitoring role of financial analysts in influencing corporate environmental policies. Difference-in-differences estimates show that firms experiencing exogenous decreases in analyst coverage significantly increase their toxic pollution relative to a matched group of control firms.

In cross-sectional tests, we find the effect is more pronounced in treated firms with low initial analyst coverage, poor corporate governance, and firms that are monitored less intensely by environmental regulators. We then provide evidence on four non-mutually exclusive channels through which decreases in analyst coverage lead to higher corporate pollution: fewer environmental questions raised during conference calls, higher cost of monitoring for institutional investors, reductions in firm investments in pollution abatement technologies and processes, and deteriorating internal governance related to environmental goals.

Overall, our evidence is consistent with an external monitoring hypothesis, which suggests that analysts play a key role in the monitoring of firms' environmentally harmful behaviors. Given the negative externalities of toxic emissions, our findings suggest that increased oversight of firms' environmental policies can generate welfare gains for society.

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Figure 1. Differences in Analyst Coverage between Treated and Control Firms

This figure shows the mean difference in analyst coverage (the number of analysts covering a firm) between treatment and control firms (treatment-control) three years before (*t*-3) to 3 years after (*t*+3) brokerage exits. Control firms are matched by total assets, the book-to-market ratio, return on assets (ROA), tangibility, and two-digit SIC code.

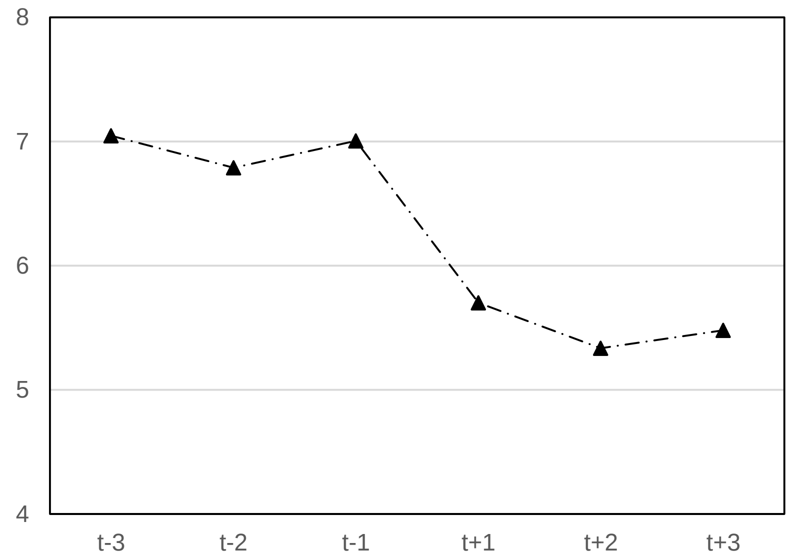


Figure 2. Differences in Total Pollution between Treatment and Control Firms

This figure shows the mean difference in total pollution (the natural logarithm of one plus the total pollution) between treatment and control firms (treatment-control) three years before (*t*-3) to 3 years after (*t*+3) brokerage exits. Control firms are matched by total assets, the book-to-market ratio, return on assets (ROA), tangibility, and two-digit SIC code.

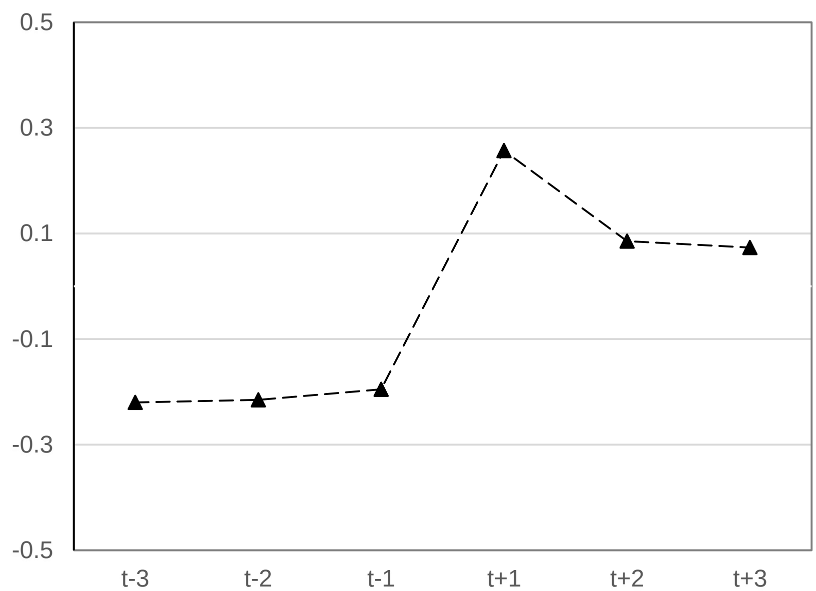
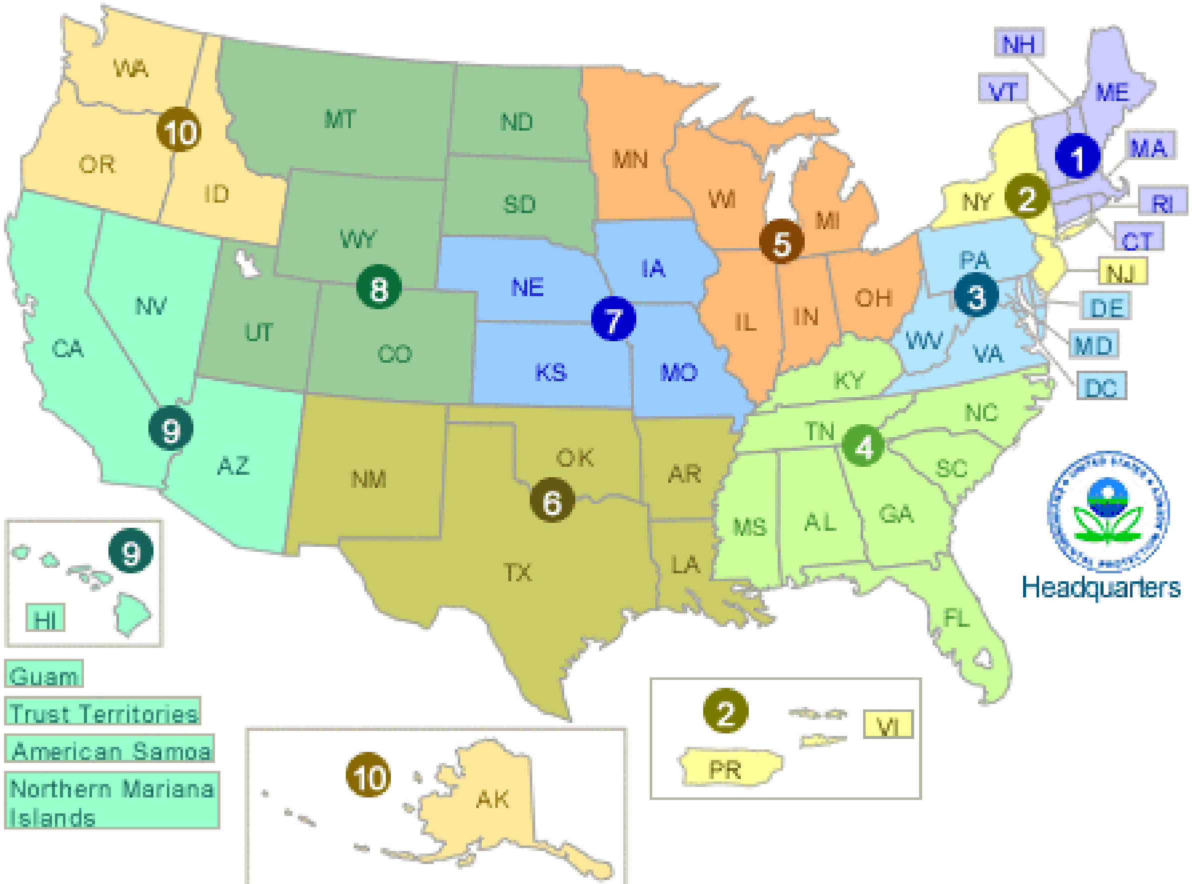


Figure 3. Distribution of EPA Regional Offices

This figure shows the geographical distribution of EPA regional offices across the U.S. There are 10 regional offices (EPA regions 1 to 10). Regional offices are given responsibility for monitoring the operation of plants in neighboring states.



Source: https://www.epa.gov/aboutepa/visiting-regional-office

Table 1. Descriptive Statistics

This table reports descriptive statistics for treated and control firms. The sample consists of 1,212 firm-year observations for 370 unique U.S. public firms between 1999 and 2011. Panel A presents summary statistics of the matched sample. Panel B reports means and *t*-tests for differences between treated and control firms in the pre-event period (*t*-1). Refer to Appendix Table A.1 for the definition and construction of variables.

Panel A. Summary Statistics

Variables	Obs	Mean	Median	Std. Dev.	25th	75th
Pollution Variables						
TOT_POL (1000s)	1,212	2262.77	64.03	22538.13	8.45	464.55
ON-SITE_POL (1000s)	1,212	2086.88	41.87	22444.55	2.55	314.09
OFF-SITE_POL (1000s)	1,212	175.89	1.93	1152.64	0.00	41.48
AIR_POL (1000s)	1,212	738.78	31.68	2235.94	2.26	244.26
WATER_POL (1000s)	1,212	174.22	0.00	1409.63	0.00	0.68
GROUND_POL (1000s)	1,212	1173.88	0.00	22103.92	0.00	0.02
log(TOT_POL)	1,212	10.71	11.07	3.49	9.04	13.05
log(ON-SITE_POL)	1,212	9.92	10.64	4.10	7.85	12.66
log(OFF-SITE_POL)	1,212	6.48	7.57	4.89	0.00	10.63
log(AIR_POL)	1,212	9.61	10.36	4.08	7.72	12.41
log(WATER_POL)	1,212	3.40	0.00	4.63	0.00	6.52
log(GROUND_POL)	1,212	2.44	0.00	4.50	0.00	3.09
log(TOT_POL_TO_SALES)	1,212	-10.87	-10.36	3.16	-12.41	-8.78
log(ON-SITE_POL_TO_SALES)	1,212	-11.66	-10.94	3.73	-13.45	-9.16
log(OFF-SITE_POL_TO_SALES)	1,212	-15.09	-14.11	4.48	-19.89	-11.41
log(AIR_POL_TO_SALES)	1,212	-11.97	-11.11	3.71	-13.64	-9.35
log(WATER_POL_TO_SALES)	1,212	-18.17	-19.79	4.13	-21.10	-16.18
log(GROUND_POL_TO_SALES)	1,212	-19.14	-20.76	4.42	-21.64	-18.23
Firm Variables						
FIRM_SIZE	1,212	7.784	7.621	1.482	6.763	8.537
ROA	1,212	0.036	0.045	0.082	0.009	0.074
BOOK_TO_MARKET	1,212	0.492	0.456	0.528	0.279	0.693
TANGIBILITY	1,212	0.281	0.249	0.152	0.164	0.366
BOOK_LEVERAGE	1,212	0.277	0.265	0.171	0.165	0.373
R&D_TO_ASSETS	1,212	0.024	0.016	0.032	0.000	0.030
DIVIDEND_TO_ASSETS	1,212	0.013	0.008	0.017	0.000	0.019
CASH_TO_ASSETS	1,212	0.087	0.052	0.099	0.020	0.115
ANALYST_COVERAGE	1,212	6.868	5.250	6.410	2.083	9.458

Panel B. Difference in Means in Pre-Brokerage Exits (*t*-1) between Treated and Control Firms

Variable	Mean (Treated)	Mean (Control)	Diff.	*P*-value
Firm Characteristics				
FIRM_SIZE	7.700	7.761	-0.061	0.615
ROA	0.050	0.049	0.000	0.956
BOOK_TO_MARKET	0.469	0.454	0.015	0.667
TANGIBILITY	0.291	0.273	0.017	0.154
BOOK_LEVERAGE	0.278	0.279	0.000	0.988
R&D_TO_ASSETS	0.026	0.022	0.004	0.160
DIVIDEND_TO_ASSETS	0.013	0.014	-0.001	0.519
CASH_TO_ASSETS	0.083	0.078	0.005	0.472

Table 2. Decreases in Analyst Coverage and Corporate Pollution

This table reports firm-year results of the DiD regression on the effects of decreases in analyst coverage on corporate pollution. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts *i* and *t* indicates firm *i* and year *t* respectively while $X_{i,t}$ is a vector of control variables. Our sample consists of 1,212 firm-year observations (606 treatment and control firm-year observations) from 1999 to 2011. The dependent variable is $\log(\text{TOT\_POL})_{i,t}$ in columns 1-3 and $\log(\text{TOT\_POL\_TO\_SALES})_{i,t}$ in columns 4-6. $\log(\text{TOT\_POL})_{i,t}$ is the natural logarithm of one plus the amount of total pollution. $\log(\text{TOT\_POL\_TO\_SALES})_{i,t}$ is the natural logarithm of one plus the amount of sales-adjusted total pollution. $\text{TREATMENT}_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. $\text{AFTER}_{i,t}$ is a dummy variable that equals 1 for the year after (*t*+1) brokerage exits and 0 for the year before (*t*-1). Refer to Appendix Table A.1 for the definition and construction of variables. Standard errors are clustered at the firm level. *t*-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

	log(TOT_POL)			log(TOT_POL_TO_SALES)		
	1	2	3	4	5	6
TREATMENT×AFTER	0.452***	0.443***	0.361***	0.458***	0.462***	0.397***
	(2.86)	(2.79)	(2.60)	(2.91)	(2.92)	(2.82)
AFTER	-0.290	-0.295	-0.125	-0.311*	-0.305	-0.143
	(-1.58)	(-1.59)	(-0.73)	(-1.67)	(-1.63)	(-0.81)
FIRM_SIZE		0.479**	0.581**		-0.175	-0.161
		(2.32)	(2.41)		(-0.84)	(-0.64)
ROA		0.364	0.344		-0.272	-0.140
		(0.39)	(0.41)		(-0.30)	(-0.17)
BOOK_TO_MARKET		-0.002	-0.050		0.046	-0.024
		(-0.02)	(-0.33)		(0.42)	(-0.16)
TANGIBILITY		0.914	0.906		0.547	-0.057
		(0.76)	(0.63)		(0.46)	(-0.04)
BOOK_LEVERAGE		0.740	1.612*		0.813	1.771**
		(1.02)	(1.84)		(1.11)	(1.99)
R&D_TO_ASSETS		2.969	2.787		1.030	0.691
		(0.41)	(0.34)		(0.14)	(0.08)
DIVIDEND_TO_ASSETS		7.260	4.063		4.929	1.770
		(1.10)	(0.66)		(0.74)	(0.28)
CASH_TO_ASSETS		0.060	-0.273		0.347	-0.082
		(0.06)	(-0.20)		(0.35)	(-0.06)
Industry-Year FE	No	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes	No
N	1,212	1,212	1,212	1,212	1,212	1,212
R-sq	0.119	0.137	0.448	0.195	0.201	0.481

Table 3. Cross-sectional Analysis: Initial Analyst Coverage

This table reports firm-year results of the DiDiD regression on the effects of decreases in analyst coverage on corporate pollution conditional on initial analyst coverage. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{LOW\_INITIAL\_COVERAGE}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{HIGH\_INITIAL\_COVERAGE}_{i,t} + \beta_3 \text{TREATMENT}_{i.t} + \beta_4 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts *i* and *t* indicates firm *i* and year *t* respectively while $X_{i,t}$ is a vector of control variables. $\text{LOW\_INITIAL\_COVERAGE}_{i,t}$ is an indicator variable which equals 1 if initial analyst coverage is in the bottom tercile for treated firms in the year prior to brokerage exits (*t*-1) and 0 otherwise. $\textit{HIGH\_INITIAL\_COVERAGE}_{i,t}$ is an indicator variable which equals 1 if initial analyst coverage is in the top tercile for treated firms in the year prior to brokerage exits (*t*-1) and 0 otherwise. The dependent variable is $\log(\text{TOT\_POL})_{i,t}$ in column 1 and $\log(\text{TOT\_POL\_TO\_SALES})_{i,t}$ in column 2. $\log(\text{TOT\_POL})_{i,t}$ is the natural logarithm of one plus the amount of total pollution. $\log(\text{TOT\_POL\_TO\_SALES})_{i,t}$ is the natural logarithm of one plus the amount of sales-adjusted total pollution. $\text{TREATMENT}_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. $\text{AFTER}_{i,t}$ is a dummy variable that equals 1 for the year after (*t*+1) brokerage exits and 0 for the year before (*t*-1). Refer to Appendix Table A.1 for the definition and construction of variables. *P*-values are reported for the tests of coefficient differences in triple interaction terms. Standard errors are clustered at the firm level. *t*-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

	log(TOT_POL)	log(TOT_POL_TO_SALES)
	1	2
TREATMENT×AFTER×LOW_INITIAL_COVERAGE	0.527***	0.548***
	(3.07)	(3.19)
TREATMENT×AFTER×HIGH_INITIAL_COVERAGE	0.285	0.300*
	(1.62)	(1.71)
AFTER	-0.300	-0.310*
	(-1.61)	(-1.65)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
Tests of coefficient differences in triple interaction terms (*p*-value)	0.049**	0.046**
N	1,212	1,212
R-sq	0.139	0.203

Table 4. Cross-sectional Analysis: Corporate Governance

This table reports firm-year results of the DiDiD regression on the effects of decreases in analyst coverage on corporate pollution conditional on corporate governance. Product market similarity by Hoberg and Phillips (2016) and the E-index by Bebchuk et al. (2009) are used as proxies for corporate governance. The specification in columns 1-2 is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{LOW\_COMPETITION}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{HIGH\_COMPETITION}_{i,t} + \beta_3 \text{TREATMENT}_{i,t} + \beta_4 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ while the specification in columns 3-4 is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{HIGH\_E-INDEX}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{LOW\_E-INDEX}_{i,t} + \beta_3 \text{TREATMENT}_{i,t} + \beta_4 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts *i* and *t* indicates firm i and year *t* respectively while $X_{i,t}$ is a vector of control variables. $\text{LOW\_COMPETITION}_{i,t}$ is an indicator variable which equals 1 if product similarity is lower than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. $\text{HIGH\_COMPETITION}_{i,t}$ is an indicator variable which equals 1 if product similarity is higher than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. $\text{HIGH\_E-INDEX}_{i,t}$ is an indicator variable which equals 1 if E-index is higher than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. $\text{LOW\_E-INDEX}_{i,t}$ is an indicator variable which equals 1 if E-index is lower than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. The dependent variable is $\log(\text{TOT\_POL})_{i,t}$ in columns 1 and 3 and $\log(\text{TOT\_POL\_TO\_SALES})_{i,t}$ in columns 2 and 4. $\log(\text{TOT\_POL})_{i,t}$ is the natural logarithm of one plus the amount of total pollution. $\log(\text{TOT\_POL\_TO\_SALES})_{i,t}$ is the natural logarithm of one plus the amount of sales-adjusted total pollution. $\text{TREATMENT}_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. $\text{AFTER}_{i,t}$ is a dummy variable that equals 1 for the year after (*t*+1) brokerage exits and 0 for the year before (*t*-1). Refer to Appendix Table A.1 for the definition and construction of variables. *P*-values are reported for the tests of coefficient differences in triple interaction terms. Standard errors are clustered at the firm level. *t*-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

	log(TOT_POL)	log(TOT_POL_TO_SALES)	log(TOT_POL)	log(TOT_POL_TO_SALES)
	1	2	3	4
TREATMENT×AFTER×LOW_COMPETITION	0.594***	0.631***		
	(3.30)	(3.51)		
TREATMENT×AFTER×HIGH_COMPETITION	0.308*	0.308*		
	(1.76)	(1.76)		
TREATMENT×AFTER×HIGH_E-INDEX			0.480***	0.499***
			(2.71)	(2.82)
TREATMENT×AFTER×LOW_E-INDEX			0.174	0.192
			(1.07)	(1.17)
AFTER	-0.292	-0.303	-0.105	-0.117
	(-1.55)	(-1.60)	(-0.65)	(-0.71)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Tests of coefficient differences in triple interaction terms (*p*-value)	0.029**	0.016**	0.032**	0.033**
N	1,188	1,188	872	872
R-sq	0.141	0.206	0.181	0.239

Table 5. Cross-sectional Analysis: Regulatory Monitoring

This table reports firm-year results of the DiDiD regression on the effects of decreases in analyst coverage on corporate pollution conditional on the intensity of regulatory monitoring. The average physical distance from a firm's plants to the regional EPA office that supervises it is used as a proxy for regulatory scrutiny. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{LONG\_DISTANCE}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{SHORT\_DISTANCE}_{i,t} + \beta_3 \text{TREATMENT}_{i,t} + \beta_4 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts i and t indicates firm i and year t respectively while $X_{i,t}$ is a vector of control variables. $\text{LONG\_DISTANCE}_{i,t}$ is an indicator variable which equals 1 if the average firm level distance of plant-EPA pairs is higher than the median value for treated firms in the year prior to brokerage exits (t-1) and zero otherwise. $\text{SHORT\_DISTANCE}_{i,t}$ is an indicator variable which equals 1 if the average firm level distance of plant-EPA pairs is lower than the median value for treated firms in the year prior to brokerage exits (t-1) and zero otherwise. The dependent variable is $\log(\text{TOT\_POL})_{i,t}$ in column 1 and $\log(\text{TOT\_POL\_TO\_SALES})_{i,t}$ in column 2. $\log(\text{TOT\_POL})_{i,t}$ is the natural logarithm of one plus the amount of total pollution. $\log(\text{TOT\_POL\_TO\_SALES})_{i,t}$ is the natural logarithm of one plus the amount of sales-adjusted total pollution. $\text{TREATMENT}_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. $\text{AFTER}_{i,t}$ is a dummy variable that equals 1 for the year after (t+1) brokerage exits and 0 for the year before (t-1). Refer to Appendix Table A.1 for the definition and construction of variables. *P*-values are reported for the tests of coefficient differences in triple interaction terms. Standard errors are clustered at the firm level. *t*-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

	log(TOT_POL)	log(TOT_POL_TO_SALES)
	1	2
TREATMENT×AFTER×LONG_DISTANCE	0.575***	0.596***
	(3.11)	(3.22)
TREATMENT×AFTER×SHORT_DISTANCE	0.310*	0.327*
	(1.85)	(1.95)
AFTER	-0.293	-0.302
	(-1.58)	(-1.62)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
Test of coefficient differences in triple interaction terms (*p*-value)	0.045**	0.044**
N	1,212	1,212
R-sq	0.139	0.204

Table 6. Channels: Environmental-related Questions During Earnings Conference Calls

This table reports firm-year results of the DiD regression on the effects of decreases in analyst coverage on environmental-related questions raised in conference calls. Probit models are used in columns 1-2. Tobit models are used in columns 3-4. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts *i* and *t* indicates firm *i* and year *t* respectively while $X_{i,t}$ is a vector of control variables. The dependent variable is $\text{ENVIRON\_QUESTIONS}_{i,t}$ in columns 1-2, which is an indicator variable that equals 1 if at least one financial analyst raises environmental-related questions in the Q&A session during earnings conference calls, and zero otherwise. The dependent variable is $\text{\#\_ENVIRON\_QUESTIONS}_{i,t}$ in columns 3-4, which is the number of environmental-related questions raised by analysts in the Q&A session. $\text{TREATMENT}_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. $\text{AFTER}_{i,t}$ is a dummy variable that equals 1 for the year after (t+1) brokerage exits and 0 for the year before (t-1). Refer to Appendix Table A.1 for the definition and construction of variables. Standard errors are clustered at the firm level. *t*-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

	ENVIRON_QUESTIONS		#_ENVIRON_QUESTIONS	
	1	2	3	4
TREATMENT×AFTER	-0.601**	-0.545*	-1.558***	-1.331***
	(-2.05)	(-1.74)	(-5.90)	(-5.05)
AFTER	0.310	0.296	1.207***	1.045***
	(1.28)	(1.19)	(5.13)	(4.26)
TREATMENT	-0.184	-0.175	-0.896***	-0.862***
	(-0.69)	(-0.63)	(-3.80)	(-3.57)
Controls	No	Yes	No	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	371	371	516	516
pseudo R-sq	0.089	0.152	0.136	0.179

Table 7. Channels: Institutional Investors

This table reports firm-year results of the DiD regression on the effects of decreases in analyst coverage on institutional ownership in Panel A and DiDiD regressions on the effects of decreases in analyst coverage on corporate pollution conditional on institutional ownership in Panel B. The specification in Panel A is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ while the specification in Panel B is $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{HIGH\_OWNERSHIP}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{LOW\_OWNERSHIP}_{i,t} + \beta_3 \text{TREATMENT}_{i,t} + \beta_4 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts i and t indicates firm i and year t respectively while $X_{i,t}$ is a vector of control variables. In Panel A, the dependent variable is the percentage of equity of the firm owned by: institutional investors ($IO_{i,t}$) in columns 1-2; quasi-indexers (QUASI-INDEXERS$_{i,t}$) in columns 3-4, and; public pension funds (PUBLIC_PENSION_FUNDS$_{i,t}$) in columns 5-6. IO$_{i,t}$ is the percentage of shares held by intuitional investors. QUASI-INDEXERS$_{i,t}$ is defined following Bushee (2001) and is calculated as the percentage of shares held by quasi-indexers. PUBLIC_PENSION_FUNDS$_{i,t}$ is defined following Bushee (2001) and is calculated as the percentage of shares held by public pension funds. In Panel B, the dependent variable is log(TOT_POL)$_{i,t}$ in odd columns and log(TOT_POL_TO_SALES)$_{i,t}$ in even columns. log(TOT_POL)$_{i,t}$ is the natural logarithm of one plus the amount of total pollution. log(TOT_POL_TO_SALES)$_{i,t}$ is the natural logarithm of one plus the amount of sales-adjusted total pollution. HIGH (LOW_IO)$_{i,t}$ is an indicator variable which equals 1 if the equity % owned by institutional investors for treated firms is higher (lower) than the median in the year prior to brokerage exits (t-1) and zero otherwise. HIGH (LOW)_QUASI-INDEXERS$_{i,t}$ is an indicator variable which equals 1 if the equity % owned by quasi-indexers for treated firms is higher (lower) than the median in the year prior to brokerage exits (t-1) and zero otherwise. HIGH (LOW)_PENSION_FUNDS$_{i,t}$ is an indicator variable which equals 1 if the equity % owned by public pension funds for treated firms is higher (lower) than the median in the year prior to brokerage exits (t-1) and zero otherwise. TREATMENT$_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. AFTER$_{i,t}$ is a dummy variable that equals 1 for the year after (t+1) brokerage exits and 0 for the year before (t-1). Refer to Appendix Table A.1 for the definition and construction of variables. P-values are reported for the tests of coefficient differences in triple interaction terms. Standard errors are clustered at the firm level. t-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

Panel A. Decreases in Analyst Coverage and Institutional Holdings

	IO		QUASI-INDEXERS		PUBLIC_PENSION_FUNDS	
	1	2	3	4	5	6
TREATMENT×AFTER	-0.042**	-0.038**	-0.048***	-0.046***	-0.003**	-0.002*
	(-2.09)	(-1.97)	(-2.95)	(-2.81)	(-1.99)	(-1.66)
AFTER	0.028	0.028	0.025	0.026*	0.000	0.000
	(1.44)	(1.53)	(1.63)	(1.82)	(0.26)	(0.14)
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	624	624	624	624	624	624
R-sq	0.526	0.587	0.682	0.700	0.142	0.179

Panel B. Cross-sectional Analysis (Institutional Investors)

Dep. Variable in columns 1, 3, 5 = log(TOT_POL)						
Dep. Variable in columns 2, 4, 6 = log(TOT_POL_TO_SALES)						
	1	2	3	4	5	6
TREATMENT×AFTER×HIGH_IO	0.731***	0.725***				
	(2.67)	(2.63)				
TREATMENT×AFTER×LOW_IO	0.369	0.376				
	(1.44)	(1.45)				
TREATMENT×AFTER×HIGH_QUASI-INDEXERS			0.679**	0.656**		
			(2.58)	(2.48)		
TREATMENT×AFTER×LOW_QUASI-INDEXERS			0.418	0.442		
			(1.56)	(1.63)		
TREATMENT×AFTER×HIGH_PUBLIC_PENSION_FUNDS					0.693**	0.703***
					(2.58)	(2.63)
TREATMENT×AFTER×LOW_PUBLIC_PENSION_FUNDS					0.398	0.388
					(1.51)	(1.44)
AFTER	-0.188	-0.210	-0.185	-0.207	-0.179	-0.202
	(-0.76)	(-0.84)	(-0.75)	(-0.83)	(-0.73)	(-0.81)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Tests of coefficient differences in triple interaction terms (*p*-value)	0.037**	0.047**	0.099*	0.152	0.072*	0.065*
N	624	624	624	624	624	624
R-sq	0.154	0.189	0.152	0.187	0.153	0.188

Table 8. Channels: Investments in Pollution Abatement

This table reports firm-year results of the DiD regression on the effects of decreases in analyst coverage on investments in pollution abatement technologies and green innovations. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts *i* and *t* indicates firm *i* and year *t* respectively while $X_{i,t}$ is a vector of control variables. The dependent variable is log(ENVIRON_EXPEND)$_{i,t}$ in columns 1-2 and GREEN_PATENTS$_{-2,+2i,t}$ in columns 3-4. log(ENVIRON_EXPEND)$_{i.t}$ is the natural logarithm of one plus the amount of environmental expenditure on pollution abatement obtained from a firm's 10-K files. GREEN_PATENTS$_{-2,+2i.t}$ is the number of green patents for two years before (*t*-2) and after (*t*+2) brokerage exits. ZERO_PATENT$_{i,t}$ is a dummy variable that takes the value one if a firm has zero patents, and zero otherwise. TREATMENT$_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. AFTER$_{i,t}$ is a dummy variable that equals 1 for the year after (*t*+1) brokerage exits and 0 for the year before (*t*-1). Refer to Appendix Table A.1 for the definition and construction of variables. Standard errors are clustered at the firm level. *t*-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

	log(ENVIRON_EXPEND)		GREEN_PATENTS$_{2,+2}$	
	1	2	3	4
TREATMENT×AFTER	-0.373*	-0.347*	-1.333*	-1.325*
	(-1.80)	(-1.69)	(-1.75)	(-1.70)
AFTER	0.238	0.224	0.447	0.149
	(1.06)	(1.03)	(0.46)	(0.16)
ZERO_PATENT			-2.553***	-2.551***
			(-3.18)	(-3.28)
Controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	1,212	1,212	1,112	1,112
R-sq	0.040	0.061	0.081	0.094

Table 9. Channels: Compensation Contracts and Sustainability Committees

This table reports firm-year results of the DiD regression on the effects of decreases in analyst coverage on managerial compensation contracts and the presence of a sustainability committee. Probit models are used. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts i and t indicates firm i and year t respectively while $X_{i,t}$ is a vector of control variables. The dependent variable is $\text{ENVIRON\_COMP}_{i,t}$ in column 1 and $\text{SUSTAIN\_COMM}_{-2,+2i,t}$ in column 2. $\text{ENVIRON\_COMP}_{i,t}$ is an indicator variable that equals 1 if firms set environmental targets in the executives' performance-based compensation and 0 otherwise. $\text{SUSAIN\_COMM}_{-2,+2i,t}$ is an indicator variable which equals 1 if firms have a specialized sustainability committee and 0 otherwise for the 2 years before (t-2) and after (t+2) brokerage exits. $\text{TREATMENT}_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. $\text{AFTER}_{i,t}$ is a dummy variable that equals 1 for the year after (t+1) brokerage exits and 0 for the year before (t-1). Refer to Appendix Table A.1 for the definition and construction of variables. Standard errors are clustered at the firm level. t-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

	ENVIRON_COMP	$\text{SUSTAIN\_COMM}_{-2,+2}$
	1	2
TREATMENT×AFTER	-0.576*	-0.778**
	(-1.87)	(-2.45)
AFTER	1.063*	0.955***
	(1.90)	(2.70)
TREATMENT	2.117***	1.398***
	(5.14)	(3.40)
Controls	Yes	Yes
Industry FE	Yes	Yes
Year FE	Yes	Yes
N	213	406
pseudo R-sq	0.357	0.504

Internet Appendix for

"Analyst Coverage and Corporate Environmental Policies"

(Not to be published)

Table A.1. Variable Definitions

Variable	Definition	Data Source
Pollution Variables		
TOL_POL	Total quantity of on- and off-site toxic emission at the firm-year level	TRI
ON-SITE_POL	Total quantity of toxic pollution released onsite into the air, water, and ground at the firm-year level	TRI
OFF-SITE_POL	Total quantity of toxic pollution transferred to off-site locations for further release or disposal at the firm-year level	TRI
AIR_POL	Total quantity of onsite stack emissions and on-site fugitive emissions at the firm-year level	TRI
WATER_POL	Total quantity of toxic pollution released on-site as surface water discharges at the firm-year level	TRI
GROUND_POL	Total quantity of toxic pollution released to on-site grounds at the firm-year level	TRI
log(TOTAL_POL)	Natural logarithm of (one plus) the total pollution	TRI
log(ON-SITE_POL)	Natural logarithm of (one plus) the on-site pollution	TRI
log(OFF-SITE_POL)	Natural logarithm of (one plus) the off-site pollution	TRI
log(AIR_POL)	Natural logarithm of (one plus) the air pollution	TRI
log(WATER_POL)	Natural logarithm of (one plus) the water pollution	TRI
log(GROUND_POL)	Natural logarithm of (one plus) the ground pollution	TRI
log(TOTAL_POL_TO_SALES)	Natural logarithm of (one plus) the sales adjusted total pollution (total pollution/sales)	TRI
log(ON-SITE_POL_TO_SALES)	Natural logarithm of (one plus) on-site pollution scaled by sales	TRI
log(OFF-SITE_POL_TO_SALES)	Natural logarithm of (one plus) off-site pollution scaled by sales	TRI
log(AIR_POL_TO_SALES)	Natural logarithm of (one plus) air pollution scaled by sales	TRI
log(WATER_POL_TO_SALES)	Natural logarithm of (one plus) water pollution scaled by sales	TRI
log(GROUND_POL_TO_SALES)	Natural logarithm of (one plus) ground pollution scaled by sales	TRI
log(TOL_ENFORCE)	Natural logarithm of (one plus) the number of EPA enforcement cases) at the firm-year level	ICIS FE&C
log(NON-JDC)	Natural logarithm of (one plus) the number of non-judicial cases at the firm-year level	ICIS FE&C
log(JDC)	Natural logarithm of (one plus) the number of judicial cases at the firm-year level	ICIS FE&C
Firm Characteristics		
FIRM_SIZE	Natural logarithm of (one plus) total assets	Compustat
ROA	Operating income divided by total assets	Compustat
BOOK_TO_MARKET	Book value of equity divided by the market value of equity	Compustat

TANGIBILITY	Net property, plant, and equipment divided by total assets	Compustat
BOOK_LEVERAGE	The sum of current liabilities and long-term debt divided by the total assets	Compustat
R&D_TO_ASSETS	Research and development expenses divided by total assets	Compustat
DIVIDEND_TO_ASSETS	The sum of common dividends and preferred dividends divided by total assets	Compustat
CASH_TO_ASSETS	Cash and short-term investments divided by total assets	Compustat
Cross-sectional Analysis		
ANALYST_COVERAGE	Arithmetic mean of the 12 monthly numbers of earnings forecasts over the fiscal year measured at the firm-year level	I/B/E/S
INITIAL_ANALYST_COVERAGE	Analyst coverage prior to the year before brokerage exits measured at the firm-year level	I/B/E/S
LOW (HIGH)_INITIAL_ANALYST_COVERAGE	Low (High) initial coverage is an indicator variable which equals 1 if initial analyst coverage is in the bottom (top) tercile for treated firms in the year prior to brokerage exits and 0 otherwise	I/B/E/S
PRODUCT_SIMILARITY	The total product similarity is the sum of the pairwise similarities between a given firm and all other firms in a given year	Hoberg and Phillips (2016)
LOW (HIGH)_COMPETITION	Low (High) Competition is an indicator variable which equals 1 if product similarity is lower (higher) than the median value for treated firms in the year prior to brokerage exits and zero otherwise	Hoberg and Phillips (2016)
E-INDEX	The sum of six anti-takeover provisions introduced by Bebchuk et al. (2009) measured at the firm-year level	IRRC
LOW (HIGH)_E-INDEX	Low (High) E-index is an indicator variable which equals 1 if E-index is lower (higher) than the median value for treated firms in the year prior to brokerage exits and zero otherwise	IRRC
AVERAGE_DISTANCE	Average firm-year geographic distance between plants owned by a firm and its supervising EPA regional office	TRI
LONG (SHORT)_DISTANCE	Long (Short) distance is an indicator variable which equals 1 if the average firm level distance of plant-EPA pairs is higher (lower) than the median value for treated firms in the year prior to brokerage exits and zero otherwise	TRI
Channels Analysis		
ENVIRON_QUESTIONS	Indicator variable that equals one if at least one financial analyst asks environmental-related questions in the Q&A session during earnings conference calls and zero otherwise measured at the firm-year level	LexisNexis; Capital IQ

#_ENVIRON_QUESTIONS	The number of environmental-related analyst questions in the Q&A session measured at the firm-year level	LexisNexis; Capital IQ
IO	Fraction of a firm's shares held by institutional investors measured at the firm-year level	Thomson Reuters 13F
HIGH (LOW) IO	High (Low) IO is an indicator variable which equals 1 if the percent of equity owned by institutional investors for treated firms is higher (lower) than the median in the year prior to brokerage exits and zero otherwise	Thomson Reuters 13F
QUASI-INDEXERS	Fraction of a firm's shares held by quasi-indexers (defined following Bushee (2001)) measured at the firm-year level	Thomson Reuters 13F
HIGH (LOW)_QUASI-INDEXERS	High (Low) Quasi-indexers is an indicator variable which equals 1 if the percent of equity owned by quasi-indexers for treated firms is higher (lower) than the median in the year prior to brokerage exits and zero otherwise	Thomson Reuters 13F
PUBLIC_PENSION_FUNDS	Fraction of a firm's shares held by public pension funds (defined following Bushee (2001)) measured at the firm-year level	Thomson Reuters 13F
HIGH (LOW)_PUBLIC_PENSION_FUNDS	High (Low) Public pension funds is an indicator variable which equals 1 if the percent of equity owned by public pension funds for treated firms is higher (lower) than the median in the year prior to brokerage exits and zero otherwise	Thomson Reuters 13F
log(ENVIRON EXPEND)	Natural logarithm of (one plus the firm-year amount of a firm's environmental expenditure on pollution abatement)	10-K
GREEN_PATENTS	The number of green patents measured at the firm-year level	Kogan et al. (2017)
ENVIRON_COMP	Firm-year indicator variable that equals one if firms set environmental performance-based compensation contracts for any named-executive and zero otherwise	DEF 14A
SUSTAIN_COMM	Firm-year indicator variable equals one if firms have a sustainability committee and zero otherwise	BoardEx

A.2. Matched Treatment and Control Firms

From the unmatched sample, we use a propensity score matching (PSM) method to construct matched treated and control firms. We use a matched sample for our analysis as treated and control firms could differ across various firm characteristics. For instance, if larger firms tend to be covered more by analysts (and thus more likely to be treated), these large firms, by virtue of their size, could also find it more efficient to invest in pollution abatement technologies. Further, having firms that are similar in observable characteristics reduces concerns that these firms differ along *unobservable* dimensions (Roberts and Whited (2013)).

To construct our sample of matched treated and control firms, we follow previous studies (e.g. Derrien and Kecskes (2013), He and Tian (2013), Hong and Kacperczyk (2010), Irani and Oesch (2013), Kelly and Ljungqvist (2012)) and match on several firm characteristics that are likely to predict treatment prior (*t*-1) to brokerage exits; namely, total assets (FIRM_SIZE), the book-to-market ratio (BOOK_TO_MARKET), return on assets (ROA), tangibility (TANGIBILITY), and the two-digit SIC. We match on firm size, performance and the book-to-market ratio as larger and better performing firms tend to attract more analyst coverage, which increases the probability of being affected by brokerage exits (Hong and Kacperczyk (2010)). We also match on tangibility as firms with a higher proportion of tangible assets may have different environmental strategies that could influence an analyst's decision to cover the firm (Akey and Appel (2021), Ioannou and Serafeim (2015), Luo, Wang, Raithel, and Zheng (2015)).

In the first step, we run a logit regression where the dependent variable equals one if a firm is considered as treated in a particular firm-year, and zero otherwise.[37] This regression is estimated using our unmatched DiD sample as described in Section III.B.2 of the main manuscript. The estimated coefficients are used to predict the probability of treatment (propensity scores). Using these scores, we perform a one-to-one nearest-neighbor match with replacement. Our final matched sample consists of 254 (116) unique treated (control) firms with 1,212 firm-year observations (606 firm-year observations per treated and control group).[38] There are 303 pairs of treated and control firms affected by brokerage exits (2 firm-year observations (*t*-1 and *t*+1) each).

[37] Note that because of the staggered nature of brokerage exits, there is a possibility that treated firms could enter into our control group of firms after the difference-in-difference window. We do not allow treated firms to enter our control group for the matching process to ensure a cleaner match; i.e. treated firms are always considered as treated regardless of the DiD window.

[38] As the number of firms with pollution data is relatively limited (765 unique firms) and our matching requires firms to be in a similar industry (SIC two-digit code), we lose about 100 treated firm-year observations.

As described in Section III.B.3. in the main manuscript, we show that the means of firm characteristics are largely indistinguishable after matching, suggesting that our matching process is successful in balancing ex-ante differences in financial characteristics between treatment and control firms.

A.3. Robustness Tests

In this section, we conduct a range of robustness tests on our baseline findings. Table A.3 presents the results. As with the baseline model, the dependent variable is log total pollution in columns 1-3 and log sales-adjusted total pollution in columns 4-6. For brevity, we only report the coefficient and t-value of the interaction (TREATMENT × AFTER). Results are displayed in Table A.3.

We start off by using different estimation windows. In our baseline analysis, we use a 1-year pre and post estimation window around brokerage exits (t-1 and t+1). This is our preferred specification as it allows us to more cleanly capture the effects of exogenous decreases in analyst coverage on corporate pollution and reduces the possibility that our results might be biased by new analysts stepping in to cover our group of treated firms. Further, since we also rely on a longer estimation window for some other tests in our paper, we also want to ensure that our findings of increases in pollution are also robust to these estimation horizons. In Rows (1) and (2), we show that our results continue to hold when we use a 2-year (t-2 and t+2) and 3-year (t-3 and t+3) window around brokerage exits.

In the second series of robustness tests, we investigate if our results are sensitive to the choice of matching variables in creating our matched sample used in the baseline model. In Row (3), we start with the unmatched sample. While estimations of the unmatched sample are likely biased due to differing characteristics for treated and control firms, we nonetheless show that our findings are robust even when we use the unmatched sample. Similar to our baseline results, we find evidence consistent with the monitoring hypothesis that analyst coverage reduces corporate pollution. While comforting, our preference in specification is still the matched sample DiD to ensure covariate balance.

Rows (4) to (7) employ different combinations of matching variables to create our matched sample. Row (4) creates a simple matched sample based only on FIRM_SIZE. Row (5) is our main matched sample used in Table 2 (reproduced for comparability) and matches on FIRM_SIZE, ROA, BOOK_TO_MARKET, TANGIBILITY and 2-digit SIC code. Row (6) adds R&D_TO_ASSETS to the matching variables used in the main specification as investments in research and development could be related to a firm's use of green technologies and pollution abatement (Chu and Zhao (2019)). Lastly, Row (7) adds monthly stock returns (RETURN) and stock return volatility (VOLATILITY) to the matching variables from the previous row following Hong and Kacperczyk (2010) as the authors find that stocks experiencing brokerage closures are more volatile. Regardless of the choice of matching variables in creating a matched sample, our results continue to remain robust.

Third, we address the concern that financial crises could simultaneously lead to brokerage exits and increases in corporate pollution due to financial constraints (Xu and Kim (2022)). In Row (8), we drop all brokerage closure and merger events that occurred from 2008-2010. In Row (9), we follow He and Tian (2013) and drop brokerage exits that occurred during the internet bubble of 2001-2002. Our results remain largely unchanged, alleviating concerns that financial crises are driving our results.

Fourth, prior studies (e.g. Shapiro and Walker (2018)) document a persistent and significant decrease in toxic pollution in the U.S. from the 1990s to the early 2000s due to changes in environmental regulation (e.g. implicit pollution tax). Further, in our sample, approximately one-third of brokerage exits occurred during 2000-2001. To ameliorate concerns that the decrease in pollution and a large number of brokerage exits during this period could be influencing our results, we drop brokerage closures and mergers that occurred in 2000-2001. Our estimations in Row (10) continue to remain robust.

Lastly, in our baseline analysis, we note that approximately one-third of treated firm-year observations are treated more than once (stocks covered by brokers that are closed or merged). As multiple treatment events could confound estimations (Kim, Lu, and Yu (2019b)), we retain only observations affected by the first treatment event (if they are treated more than once) and re-run our analysis in Row (11). We continue to obtain similar results.[39]

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[39] We also follow Chen, Chiu, and Shevlin (2018) and randomly retain treatment events (instead of retaining the first treatment event) for firms that are treated more than once. Our results remain materially unchanged.

Table A.3. Robustness Tests

This table reports various robustness tests for our baseline DiD regression. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 TREATMENT_{i,t} \times AFTER_{i,t} + \beta_2 TREATMENT_{i,t} + \beta_3 AFTER_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts i and t indicates firm i and year t respectively while $X_{i,t}$ is a vector of control variables. Panel A uses different estimation windows. Panel B shows results with alternative matching criteria. Panel C excludes brokerage exits that occurred during the financial crisis or the internet bubble. Panel D excludes years 2001-2002 due to large decreases in pollution. Panel E retains observations only for their first treatment (if treated more than once). The dependent variable is log(TOT_POL)$_{i,t}$ in columns 1-3 and log(TOT_POL_TO_SALES)$_{i,t}$ in columns 4-6. log(TOT_POL)$_{i,t}$ is the natural logarithm of one plus the amount of total pollution. log(TOT_POL)$_{i,t}$ is the natural logarithm of one plus the amount of sales-adjusted total pollution. TREATMENT$_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. AFTER$_{i,t}$ is a dummy variable that equals 1 for the year after (t+1) brokerage exits and 0 for the year before (t-1). For brevity, only the coefficients of interaction item β_1TREATMENT$_{i,t} \times$ AFTER$_{i,t}$ are reported. Refer to Appendix Table A.1 for the definition and construction of variables. Standard errors are clustered at the firm level. t-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

	log(TOT_POL)			log(TOT_POL_TO_SALES)		
	1	2	3	4	5	6
Panel A. Different DiD Estimation Windows						
(1) t-2 to t+2 years	0.377**	0.371**	0.294**	0.356**	0.368**	0.308**
	(2.25)	(2.27)	(2.09)	(2.16)	(2.26)	(2.17)
(2) t-3 to t+3 years	0.353**	0.336*	0.271*	0.300*	0.325*	0.280*
	(2.01)	(1.94)	(1.75)	(1.74)	(1.89)	(1.81)
Panel B: Alternate PSM-matched Control Firms						
(3) Unmatched Sample	0.248***	0.242***	0.295***	0.238***	0.250***	0.303***
	(3.70)	(3.65)	(4.36)	(3.60)	(3.84)	(4.51)
(4) Matched on: FIRM_SIZE	0.337**	0.321**	0.420***	0.344**	0.371**	0.466***
	(2.01)	(2.02)	(3.04)	(2.05)	(2.30)	(3.34)
(5) Matched on: FIRM_SIZE/ROA/BOOK_TO_MARKET/TANGIBILITY	0.452***	0.443***	0.361***	0.458***	0.462***	0.397***
	(2.86)	(2.79)	(2.60)	(2.91)	(2.92)	(2.82)
(6) Matched on: Row (5) + R&D_TO_ASSETS	0.383**	0.397**	0.436**	0.341**	0.401***	0.456***
	(2.31)	(2.52)	(2.45)	(2.18)	(2.62)	(2.61)
(7) Matched on: Row (6) + RETURN and VOLATILITY	0.409**	0.441***	0.560***	0.428***	0.478***	0.594***
	(2.44)	(2.71)	(3.20)	(2.66)	(2.95)	(3.38)
Panel C. Excluding Brokerage Exits in Financial Crises						
(8) Exclude exits after 2008	0.459***	0.445***	0.371**	0.448***	0.458***	0.402***
	(2.89)	(2.70)	(2.51)	(2.83)	(2.76)	(2.66)
(9) Exclude exits in 2001 and 2002	0.391**	0.355**	0.369**	0.387**	0.374**	0.408**
	(2.18)	(2.10)	(2.28)	(2.19)	(2.21)	(2.49)
Panel D. Excluding Brokerage Exits due to Environmental Changes						
(10) Exclude exits in 2000 and 2001	0.601***	0.613***	0.394**	0.640***	0.647***	0.436**
	(2.90)	(2.87)	(2.30)	(3.12)	(3.04)	(2.52)
Panel E. Retaining First Treatment						
(11) Retain only firm-year obs. impacted by first exit	0.374**	0.369**	0.335**	0.382**	0.389**	0.370**
	(2.34)	(2.28)	(2.10)	(2.39)	(2.39)	(2.31)
Controls	No	Yes	Yes	No	Yes	Yes
Industry-Year FE	No	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes	No

Table A.4. Decreases in Analyst Coverage and Small Polluters

This table reports firm-year results of the DiDiD regression on the effects of decreases in analyst coverage on corporate pollution conditional on the size of pollution. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{SMALL\_POLLUTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{LARGE\_POLLUTER}_{i,t} + \beta_3 \text{TREATMENT}_{i,t} + \beta_4 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts *i* and *t* indicates firm *i* and year *t* respectively while $X_{i,t}$ is a vector of control variables. SMALL_POLLUTER$_{i,t}$ is an indicator variable which equals 1 if the total corporate pollution that firms emit is lower than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. LARGE_POLLUTER$_{i,t}$ is an indicator variable which equals 1 if the total corporate pollution that firms emit is higher than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. TREATMENT$_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. AFTER$_{i,t}$ is a dummy variable that equals 1 for the year after (*t*+1) brokerage exits and 0 for the year before (*t*-1). Refer to Appendix Table A.1 for the definition and construction of variables. *P*-values are reported for the tests of coefficient differences in triple interaction terms. Standard errors are clustered at the firm level. *t*-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

	log(TOT_POL)	log(TOT_POL_TO_SALES)
	1	2
TREATMENT×AFTER×SMALL_POLLUTER	0.575***	0.597***
	(2.99)	(3.11)
TREATMENT×AFTER×LARGE_POLLUTER	0.303**	0.318**
	(1.97)	(2.06)
AFTER	-0.296	-0.305
	(-1.59)	(-1.63)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
Tests of coefficient differences in triple interaction terms (p-value)	0.034**	0.033**
N	1,212	1,212
R-sq	0.139	0.204

Table A.5. Decreases in Analyst Coverage and Sub-categories of Corporate Pollution

This table reports firm-year results of the DiD regression on the effects of decreases in analyst coverage on sub-categories of pollution. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts i and t indicates firm i and year t respectively while $X_{i,t}$ is a vector of control variables. Our sample consists of 1,212 firm-year observations (606 treatment and control firm-year observations) from 1999 to 2011. Panel A investigates the decreases in analyst coverage on firms' on-site and off-site pollution. On-site pollution is the quantity of toxic chemicals released into the air, water, and ground on-site at the plant. Off-site pollution is the quantity of toxic release transferred to off-site locations for further release or disposal at specialized waste management facilities. log(ON-SITE_POL)$_{i,t}$ is the natural logarithm of one plus the amount of on-site pollution. log(ON-SITE_POL_TO_SALES)$_{i,t}$ is the natural logarithm of one plus the amount of sales adjusted on-site pollution. log(OFF-SITE_POL)$_{i,t}$ is the natural logarithm of one plus the amount of off-site pollution. log(OFF-SITE_POL_TO_SALES)$_{i,t}$ is the natural logarithm of one plus the amount of sales adjusted off-site pollution. Panel B splits on-site pollution into air, water, and ground pollution to investigate decreases in analyst coverage on firms' on-site and off-site pollution. Air pollution is the total quantity of on-site stack emissions and on-site fugitive emissions. Water pollution is the total quantity of toxic pollutions released on-site as surface water discharges. Ground pollution is the total quantity of toxic pollution released on-site on grounds. log(AIR_POL)$_{i,t}$ is the natural logarithm of one plus the amount of air pollution. log(AIR_POL_TO_SALES)$_{i,t}$ is the natural logarithm of one plus the amount of sales adjusted air pollution. log(WATER_POL)$_{i,t}$ is the natural logarithm of one plus the amount of water pollution. log(WATER_POL_TO_SALES)$_{i,t}$ is the natural logarithm of one plus the amount of sales adjusted water pollution. log(GROUND_POL)$_{i,t}$ is the natural logarithm of one plus the amount of ground pollution. log(GROUND_POL_TO_SALES)$_{i,t}$ is the natural logarithm of one plus the amount of sales adjusted ground pollution. TREATMENT$_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. AFTER$_{i,t}$ is a dummy variable that equals 1 for the year after (t+1) brokerage exits and 0 for the year before (t-1). Refer to Appendix Table A.1 for the definition and construction of variables. Standard errors are clustered at the firm level. t-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

Panel A. Impact of an Exogenous Decrease in Analyst Coverage on On-site and Off-site Pollution

	On-site Pollution		Off-site Pollution	
	log(ON-SITE_POL)	log(ON-SITE_POL_TO_SALES)	log(OFF-SITE_POL)	log(OFF-SITE_POL_TO_SALES)
	1	2	3	4
TREATMENT×AFTER	0.470***	0.489***	0.278	0.297
	(2.67)	(2.79)	(1.31)	(1.39)
AFTER	-0.243	-0.252	-0.259	-0.268
	(-1.23)	(-1.27)	(-1.24)	(-1.28)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	1,212	1,212	1,212	1,212
R-sq	0.191	0.271	0.080	0.068

Panel B. Impact of an Exogenous Decrease in Analyst Coverage on Air, Water and Ground Pollution

	Air Pollution		Water Pollution		Ground Pollution	
	log (AIR_POL)	log (AIR_POL_TO_SALE)	log (WATER_POL)	log (WATER_POL_TO_SALES)	log (GROUND_POL)	log (GROUND_POL_TO_SALES)
	1	2	3	4	5	6
TREATMENT×AFTER	0.402**	0.421***	-0.076	-0.057	-0.021	-0.002
	(2.52)	(2.65)	(-0.54)	(-0.40)	(-0.10)	(-0.01)
AFTER	-0.189	-0.199	0.030	0.020	-0.389	-0.399
	(-1.05)	(-1.10)	(0.31)	(0.20)	(-1.57)	(-1.62)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1,212	1,212	1,212	1,212	1,212	1,212
R-sq	0.200	0.284	0.070	0.161	0.054	0.088

A.6. EPA Enforcement Actions

Our baseline results in Section IV.A. suggest that a reduction in monitoring from analysts leads to firms behaving in environmentally harmful ways, consistent with the monitoring hypothesis of analysts on a firm's emissions of toxic pollution. In this section, we use EPA enforcement data as an alternative measure of non-compliance to EPA's regulations and examine whether treated firms are more likely to violate EPA regulations after decreases in analyst coverage. In discharging enforcement actions, the EPA investigates cases of non-compliance, issues civil penalties, and monitors the correction of the violation at the plant level. Although EPA violations are not a direct measure of pollution, the measure has the advantage of linking toxic pollution to regulatory and litigation risks that should be pertinent to a firm's choice to pollute (Xu and Kim (2022)).

EPA enforcement data are extracted from the Integrated Compliance Information System for Federal Civil Enforcement Case Data (ICIS FE&C). ICIS FE&C provides plant-year level information about individual enforcement cases such as the primary law violated, settlement date, and case number. The dataset also allows for the distinction between judicial and non-judicial violations. Judicial cases are formal lawsuits that take place in court and include breaches of contract or other civil actions, while non-judicial cases are administrative cases that take place under the EPA's jurisdiction. Distinguishing between judicial and non-judicial violations could be important as managers are likely to weigh the costs and benefits of corporate pollution. If the costs (e.g. administrative corrections) are not sufficiently high as compared to judicial litigations that could lead to concerns of personal reputational damage, loss of board seats, and increased turnover (Aharony, Liu, and Yawson (2015), Fahlenbrach, Low, and Stulz (2017)), firm managers might be more willing to engage in "milder" forms of environmental misconduct.

As enforcement cases are at the plant-year level, we sum up cases to construct a firm-year count of enforcement cases and treat observations without non-compliance records as zero. As the investigation, detection, and settlement of non-compliance cases require time, we compare non-compliances in the two years before the event (t-2) and two years after (t+2). The mean total number of EPA enforcement cases per firm-year is 0.21, of which a majority of the cases (0.18 out of 0.21) are non-judicial; judicial cases make up the remainder.

In Table A.6, we show the results of the effect of a decrease in analyst coverage on EPA enforcement actions. In our specifications, firm and industry-year fixed effects are included as the enforcement data significantly vary across industries (Shive and Forster (2020)). We first look at total enforcement actions in columns 1-2, defined as the natural logarithm of one plus the number of EPA enforcements in a firm-year (TOTAL_ENFORCE). As observed, the coefficients on TREATMENT×AFTER are positive and statistically significant. Interpreting the economic magnitude in column 2, the number of enforcement cases in treated firms increases by 7.3% after experiencing reductions in analyst coverage. This evidence is consistent with the monitoring role of analysts on environmental misconduct.

Next, we split total EPA enforcements into non-judicial (log(NON-JDC)) and judicial enforcement (log(JDC)) in columns 3-4 and columns 5-6, respectively. As observed in columns 3 and 4, we find that a decrease in analyst coverage leads to an increase in non-judicial cases by 9.4% and 8.9%, respectively, depending on specification. However, we do not observe any significant changes in judicial enforcement cases. This is unsurprising as judicial cases tend to lead to greater reputational damage. As such, a firm's managers would be more cautious in engaging in more severe forms of environmental misconduct. Overall, we find that firms facing a reduction in analyst monitoring increase their instances of environmental misconduct, suggesting that managers weigh the costs and benefits of environmental misconduct. Specifically, they only increase environmental misconduct when the potential consequences for their career prospects and reputation are not overly severe.

Table A.6. Decreases in Analyst Coverage and EPA Enforcement

This table reports firm-year results of the DiD regression on the effects of decreases in analyst coverage on EPA enforcement. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta X_{i,t} + \varepsilon_{i,t}$ where subscripts i and t indicates firm i and year t respectively while $X_{i,t}$ is a vector of control variables. The dependent variable log(TOT_ENFORCE)$_{i,t}$ is the natural logarithm of one plus the total number of EPA enforcements (non-judicial + judicial)$_{i,t}$ in columns 1-2. Log(NON-JDC)$_{i,t}$ is the natural logarithm of one plus the number of non-judicial cases in columns 3-4, while log(JDC)$_{i,t}$ is the natural logarithm of one plus the number of judicial cases in columns 5-6. We use EPA cases for two years before (t-2) and after (t+2) brokerage exits as the investigation and settlements of EPA enforcements require time. TREATMENT$_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. AFTER$_{i,t}$ is a dummy variable that equals 1 for the year after (t+1) brokerage exits and 0 for the year before (t-1). For brevity, control variables are not reported. Refer to Appendix Table A.1 for the definition and construction of variables. Standard errors are clustered at the firm level. t-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

	log(TOL_ENFORCE)		log(NON-JDC)		log(JDC)	
	1	2	3	4	5	6
TREATMMENT×AFTER	0.077**	0.073*	0.094**	0.089**	-0.014	-0.014
	(2.02)	(1.96)	(2.54)	(2.48)	(-0.90)	(-0.93)
AFTER	-0.052	-0.051	-0.052	-0.051	-0.010	-0.009
	(-1.07)	(-1.07)	(-1.23)	(-1.23)	(-0.44)	(-0.38)
Controls	No	Yes	No	Yes	No	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1,112	1,112	1,112	1,112	1,112	1,112
R-sq	0.393	0.408	0.366	0.381	0.464	0.473

Table A.7. Cross-sectional Analysis: Other Corporate Governance Measures

This table reports firm-year results of the DiDiD regression on the effects of decreases in analyst coverage on corporate pollution conditional on corporate governance. The G-index, four-firm concentration ratio and co-opted boards are used as proxies for corporate governance. The specification in columns 1-2 is: $Y_{i,t} = \alpha_{i,t} + \beta_1$TREATMENT$_{i,t}$ × AFTER$_{i,t}$ × HIGH_G-INDEX$_{i,t}$ + β_2TREATMENT$_{i,t}$ × AFTER$_{i,t}$ × LOW_G-INDEX$_{i,t}$ + β3TREATMENT$_{i,t}$ + β_4AFTER$_{i,t}$ + $\delta X_{i,t}$ + $\varepsilon_{i,t}$ while the specification in columns 3-4 is: $Y_{i,t} = \alpha_{i,t} + \beta_1$TREATMENT$_{i,t}$ × AFTER$_{i,t}$ × HIGH_4FIRMCONC$_{i,t}$ + β_2TREATMENT$_{i,t}$ × AFTER$_{i,t}$ × LOW_4firmCONC$_{i,t}$ + β3TREATMENT$_{i,t}$ + β_4AFTER$_{i,t}$ + $\delta X_{i,t}$ + $\varepsilon_{i,t}$ and $Y_{i,t} = \alpha_{i,t} + \beta_1$TREATMENT$_{i,t}$ × AFTER$_{i,t}$ × HIGH_CO-OPTED$_{i,t}$ + β_2TREATMENT$_{i,t}$ × AFTER$_{i,t}$ × LOW_CO-OPTED$_{i,t}$ + β3TREATMENT$_{i,t}$ + β_4AFTER$_{i,t}$ + $\delta X_{i,t}$ + $\varepsilon_{i,t}$ in columns 5-6 where subscripts *i* and *t* indicates firm i and year *t* respectively while $X_{i,t}$ is a vector of control variables. HIGH_G-INDEX$_{i,t}$ is an indicator variable which equals 1 if G-index as constructed by Gompers, Ishii, and Metrick (2003) is higher than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. LOW_G-INDEX$_{i,t}$ is an indicator variable which equals 1 if G-index as constructed by Gompers et al. (2003) is lower than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. HIGH_4FIRMCONC$_{i,t}$ is an indicator variable which equals 1 if the industry concentration based on the sales market share of top four firms (Eckbo (1985)) is higher than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. LOW_4FIRMCONC$_{i,t}$ is an indicator variable which equals 1 if the industry concentration ratio is lower than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. HIGH_CO-OPTED$_{i,t}$ is an indicator variable which equals 1 if the co-opted boards measure as described in Coles, Daniel, and Naveen (2014) is higher than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. LOW_CO-OPTED$_{i,t}$ is an indicator variable which equals 1 if the co-opted boards measure as described in Coles et al. (2014) is lower than the median value for treated firms in the year prior to brokerage exits (*t*-1) and zero otherwise. The dependent variable is log(TOT_POL)$_{i,t}$ in columns 1, 3, 5 and log(TOT_POL_TO_SALES)$_{i,t}$ in columns 2, 4, and 6. log(TOT_POL)$_{i,t}$ is the natural logarithm of one plus the amount of total pollution. log(TOT_POL_TO_SALES)$_{i,t}$ is the natural logarithm of one plus the amount of sales-adjusted total pollution. TREATMENT$_{i,t}$ is a dummy variable that equals 1 if the firm has experienced an exogenous decrease in analyst coverage as a result of brokerage exits and 0 otherwise. AFTER$_{i,t}$ is a dummy variable that equals 1 for the year after (*t*+1) brokerage exits and 0 for the year before (*t*-1). Refer to Appendix Table A.1 for the definition and construction of variables. *P*-values are reported for the tests of coefficient differences in triple interaction terms. Standard errors are clustered at the firm level. *t*-values are reported in parentheses. ∗∗∗, ∗∗, and ∗ indicate significance levels at 1%, 5%, and 10%, respectively.

Dependent Variable = log(TOT_POL) in columns 1, 3, 5 Dependent Variable = log(TOT_POL_TO_SALES) in columns 2, 4, 6						
	1	2	3	4	5	6
TREATMENT×AFTER×HIGH_G-INDEX	0.545***	0.584***				
	(2.74)	(2.93)				
TREATMENT×AFTER×LOW_G-INDEX	0.282	0.286				
	(1.37)	(1.37)				
TREATMENT×AFTER×HIGH_4FIRMCONC			0.552***	0.575***		
			(2.97)	(3.08)		
TREATMENT×AFTER×LOW_4FIRMCONC			0.273*	0.292*		
			(1.72)	(1.85)		
TREATMENT×AFTER×HIGH_CO-OPTED					0.408**	0.407**
					(2.14)	(2.12)
TREATMENT×AFTER×LOW_CO-OPTED					0.139	0.167
					(0.79)	(0.95)
After	-0.299	-0.316	-0.316*	-0.326*	-0.015	-0.022
	(-1.44)	(-1.52)	(-1.66)	(-1.71)	(-0.10)	(-0.15)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Tests of coefficient differences in triple interaction terms (p-value)	0.097*	0.071*	0.041**	0.039**	0.070*	0.095*
N	876	876	1,156	1,156	876	876
R-sq	0.139	0.191	0.134	0.198	0.193	0.257

A.8. Environmental-related Questions in Earnings Conference Calls

This appendix presents some examples of environmental-related questions raised by analysts during Q&A sessions in earnings conference calls. The environmental-related keywords are highlighted in bold.

<u>CONSOL Energy and CNX Gas 2009 Q3:</u>
Analyst question: "I guess just first off on the **EPA**, some new roles coming down the pipeline as far as sulfur **emissions**. I just want to get your take on how you see that playing out as **emission** caps come down and how that could be certainly a positive for Northern App."

<u>Headwaters Inc 2009 Q4:</u>
"Analyst question: And then just one final question, which you may have already covered, and I apologize if I missed it, but the actual number of **coal cleaning** facilities that are operating right now? And then the expectation for the full year, fiscal year 2010, the number of **coal cleaning** facilities that will be operating?"

<u>Briggs Stratton Corporation 2008 Q4:</u>
Analyst question: "Just a little more on the **emissions** side. I think the **EPA** just passed a law and it's [phased] through regulations for further reduction in exhaust **emissions**. Do the Briggs products comply with those standards, and what about the competitive front? Do Chinese engines comply with those sorts of standards and does that affect the competitive environment at this point?"

<u>Thomas Betts 2007 Q4:</u>
"Analyst question: My last question, and then I'll hop for the queue after this. Could you just give me a little bit more details on the **environmental charge** and if you expect follow through or charges in the next few quarters?"

<u>Briggs Stratton Corporation 2008 Q1:</u>
Analyst question: "Sure. Next. I realize you may have nothing to say, but wondering if you look out over the next two years, if you've got any thoughts on where we're going **environmentally** and how that is playing out in your thinking in terms of what you're trying to prepare for."

<u>Thomas Betts 2007 Q2:</u>
Analyst question: "One last question with regards to the **environmental remediation expense**. Can you explain exactly what that was and whether or not you anticipate any of that in your guidance going forward?"

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