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# Analyst Coverage and Corporate Environmental Policies

Chenxing Jing
*University of International Business and Economics*
chenxing.jing95@gmail.com

Kevin Keasey
*Leeds University Business School*
k.keasey@lubs.leeds.ac.uk (corresponding author)

Ivan Lim
*Durham University Business School*
ivan.lim@durham.ac.uk

Bin Xu
*Leeds University Business School*
b.xu@leeds.ac.uk

## 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.

## 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)).

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[1]In 2015, approximately 9 million premature deaths worldwide were caused by pollution-related diseases (Landrigan, Fuller, Acosta, Adeyi, Arnold, Baldé, Bertollini, Bose-O'Reilly, Boufford, Breysse, and Chiles (2018)).

Given the severe consequences of toxic pollution, increasing effort has been devoted to study the determinants of corporate pollution. In this article, 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 (2019a)) 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 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 1 year prior to these exits to 1 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 nonmutually 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 (2019b), 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 article 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. (2019b) 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, Goetz (2019) and Xu and Kim (2022) 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]

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 (Yu (2008), Chen et al. (2015), and Irani and Oesch (2016)). 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

[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) document 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.

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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 (Jensen and Meckling (1976), Chung and Jo (1996)). 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)).

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

[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.

environmental performance is a positive driver of firm value and performance (Konar and Cohen (2001), Karpoff et al. (2005), and 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 nonfinancial 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 costs (Sharfman and Fernando (2008), Chava, (2014)). This suggests that capital markets, where analysts contribute to the

[4]For example, on Jan. 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.

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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]

# 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: i) manufacture, process or emit a list of specific hazardous pollutants in an amount greater than the specified threshold; ii) have more than 10 full-time employees; and iii) operate in one of the approximately 400 industries (e.g., manufacturing, mining, and merchant wholesalers) identified at the 6-digit North American Industry Classification System (NAICS) level, are required to report the quantity of toxic pollution released into the environment. Currently, the TRI data set contains information for over 700 individually listed chemicals (33 chemical categories) that are specified as hazardous from around 60,000 plants.[7,8]

Next, we merge plant-level TRI data with the Compustat and the Institutional Brokers' Estimate System (IBES) to retrieve financial and analyst information for

[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 (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.*" As a result, there is little evidence to suggest systemic misreporting of emissions data (Bui and Mayer (2003)).

our sample of public U.S. firms. As there is no consistent and common identifier in the TRI, Compustat, and IBES 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 IBES. 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 Delmas and Toffel (2008) and Berrone and Gomez-Mejia (2009) 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 i) log(TOT_POL), the logarithm of total pollution and ii) log(TOT_POL_TO_SALES), the logarithm of total pollution scaled by total sales. In additional tests, we also run regressions for the 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.

[9]The DUNS number, issued by Dun & Bradstreet (D&B), is a unique 9-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.

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

Next, we merge our list of 54 brokerage exits with the IBES unadjusted historical detailed data set to obtain a panel data set 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

[11]The internal validity of the two quasi-natural experiments has been extensively assessed by prior studies that utilize this setting (e.g., Hong and Kacperczyk (2010), Kelly and Ljungqvist (2012), Derrien and Kecskes (2013), and He and Tian (2013)).

[12]SIC code 6211 contains Security Brokers, Dealers, and Flotation Companies. Investment Advice firms have SIC code 6282.

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 IBES 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; 3 months before (after) the event month.[13]

We then use an estimation window of 1 year before ($t-1$) and 1 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 has a Dec. fiscal year-end and the event date is May 31, 2001, year $t-1$ ($t+1$) would be Dec. 31, 2000 (2002), respectively.

We then merge this list of covered stocks from the IBES data set 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 IBES 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,

[13]For example, Robertson Stephens was closed in July 2002. Therefore, the event closure period is defined as Apr. 2002 to Oct. 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 of the Supplementary Material, we also show that our results are robust when we compare outcomes 2 years ($t-2$ to $t+2$) and 3 years ($t-3$ to $t+3$) prior to and after brokerage exits.

[15]We impose this criterion to ensure that the treated firm is not "stopped" in the IBES 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; that is, affected by more than one brokerage exits during our sample period.

TABLE 1

Descriptive Statistics

Table 1 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 preevent period ($t-1$). Refer to Table A.1 in the Supplementary Material 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 | 2,262.77 | 64.03 | 22,538.13 | 8.45 | 464.55 |
| ON-SITE_POL (1000s) | 1,212 | 2,086.88 | 41.87 | 22,444.55 | 2.55 | 314.09 |
| OFF-SITE_POL (1000s) | 1,212 | 175.89 | 1.93 | 1,152.64 | 0.00 | 41.48 |
| AIR_POL (1000s) | 1,212 | 738.78 | 31.68 | 2,235.94 | 2.26 | 244.26 |
| WATER_POL (1000s) | 1,212 | 174.22 | 0.00 | 1,409.63 | 0.00 | 0.68 |
| GROUND_POL (1000s) | 1,212 | 1,173.88 | 0.00 | 22,103.92 | 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 Prebrokerage 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 |

TANGIBILITY, and 2-digit SIC code) that are likely to predict treatment prior to 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,

[17]Appendix A.2 of the Supplementary Material 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 of the Supplementary Material).

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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:

$$y_{i,t} = \alpha + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta \mathbf{X}_{i,t} + \varepsilon_{i,t}, \tag{1}$$

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 1 if the firm has experienced an exogenous decrease in analyst coverage due to brokerage closures or mergers, and 0 otherwise. AFTER$_{i,t}$ is an indicator variable equal to 1 in the year after brokerage exits ($t+1$) and 0 in the year before ($t-1$). Our variable of interest in equation (1) is the coefficient $\beta_1$ on the interaction item. It captures changes in corporate pollution for firms after exogenous decreases in analyst coverage relative to before, and relative to a group of matched control firms. The vector $\mathbf{X}_{i,t}$ contains firm-specific control variables. In our main regression, we have two sets of fixed effects: i) firm FE and year FE and ii) firm FE and industry-year FE. We describe this further in Section IV.A.

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. Table A.1 in the Supplementary Material 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., Derrien and Kecskes (2013), Chen and Lin (2017)).[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

FIGURE 1

Differences in Analyst Coverage Between Treated and Control Firms

Figure 1 shows the mean difference in analyst coverage (the number of analysts covering a firm) between treatment and control firms (treatment − control) 3 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 2-digit SIC code.

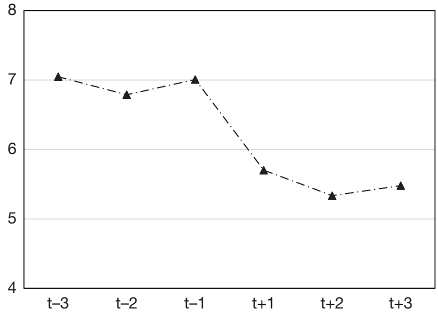
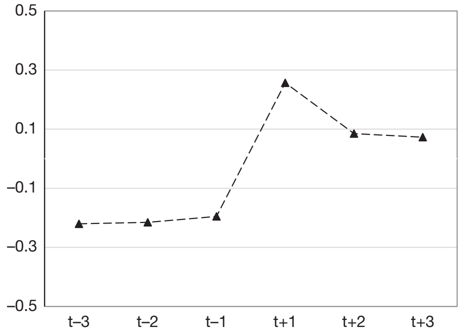



FIGURE 2

Differences in Total Pollution Between Treatment and Control Firms

Figure 2 shows the mean difference in total pollution (the natural logarithm of one plus the total pollution) between treatment and control firms (treatment − control) 3 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 2-digit SIC code.




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 preevent 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

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.

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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)).

TABLE 2

Decreases in Analyst Coverage and Corporate Pollution

Table 2 reports firm-year results of the DiD regression on the effects of decreases in analyst coverage on corporate pollution. The specification is: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta \mathbf{X}_{i,t} + \varepsilon_{i,t}$ where subscripts $i$ and $t$ indicates firm $i$ and year $t$ respectively while $\mathbf{X}_{i,t}$ 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(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_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 Table A.1 in the Supplementary Material for the definition and construction of variables. Standard errors are clustered at the firm level. $t$-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, 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 |
| No. of obs. | 1,212 | 1,212 | 1,212 | 1,212 | 1,212 | 1,212 |
| $R^2$ | 0.119 | 0.137 | 0.448 | 0.195 | 0.201 | 0.481 |

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 2-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.

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, 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 of the Supplementary Material, 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

[19]In Table A.4 in the Supplementary Material, 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.

Table A.5 in the Supplementary Material, 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. (2019b)).

Appendix A.6 of the Supplementary Material describes additional tests where we use EPA enforcement actions as an alternative measure of firms' environmental misbehavior. In particular, it measures noncompliance 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 nonjudicial 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:

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

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

TABLE 3

Cross-Sectional Analysis: Initial Analyst Coverage

Table 3 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: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{LOW\_INITIAL\_COVERAGE}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{HIGH\_INITIAL\_COVERAGE}_{i,t} + \beta_3 \text{TREATMENT}_{i,t} + \beta_4 \text{AFTER}_{i,t} + \delta \mathbf{X}_{i,t} + \varepsilon_{i,t}$ where subscripts $i$ and $t$ indicates firm $i$ and year $t$ respectively, while $\mathbf{X}_{i,t}$ 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. $\text{HIGH\_INITIAL\_COVERAGE}_{i,t}$ 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 Table A.1 in the Supplementary Material 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 at the 10%, 5%, and 1% levels, 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** |
| No. of obs. | 1,212 | 1,212 |
| $R^2$ | 0.139 | 0.203 |

brokerage exits.[20] Our coefficients of interest in equation (2) are $\beta_1$ and $\beta_2$ 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]

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

[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 1 if the treated firm has a value of this particular variable lower (higher) than a threshold that is specified.

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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.

## 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 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).

[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 competitor 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/.

[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.

TABLE 4

**Cross-Sectional Analysis: Corporate Governance**

Table 4 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: $Y_{i,t} = \alpha_{i,t} + \beta_1$TREATMENT$_{i,t}$ × AFTER$_{i,t}$ × LOW_COMPETITION$_{i,t}$ + $\beta_2$TREATMENT$_{i,t}$ × AFTER$_{i,t}$ × HIGH_COMPETITION$_{i,t}$ + $\beta_3$TREATMENT$_{i,t}$ + $\beta_4$AFTER$_{i,t}$ + $\delta\mathbf{X}_{i,t} + \varepsilon_{i,t}$ while the specification in columns 3–4 is: $Y_{i,t} = \alpha_{i,t} + \beta_1$TREATMENT$_{i,t}$ × AFTER$_{i,t}$ × HIGH_E_INDEX$_{i,t}$ + $\beta_2$TREATMENT$_{i,t}$ × AFTER$_{i,t}$ × LOW_E_INDEX$_{i,t}$ + $\beta_3$TREATMENT$_{i,t}$ + $\beta_4$AFTER$_{i,t}$ + $\delta\mathbf{X}_{i,t} + \varepsilon_{i,t}$ where subscripts $i$ and $t$ indicates firm $i$ and year $t$ respectively, while $\mathbf{X}_{i,t}$ is a vector of control variables. 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 0 otherwise. 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 0 otherwise. 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 0 otherwise. 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 0 otherwise. The dependent variable is log (TOT_POL)$_{i,t}$ in columns 1 and 3 and log(TOT_POL_TO_SALES)$_{i,t}$ in columns 2 and 4. 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 Table A.1 in the Supplementary Material 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 at the 10%, 5%, and 1% levels, 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** |
| No. of obs. | 1,188 | 1,188 | 872 | 872 |
| $R^2$ | 0.141 | 0.206 | 0.181 | 0.239 |

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 noncompetitive 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.

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

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corporate governance (i.e., firms with higher 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.

We begin by identifying the regional offices of the EPA.[25] Figure 3 shows the geographical distribution of the 10 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)

[24]As robustness checks, we also show in Table A.7 in the Supplementary Material 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 coopted 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)).

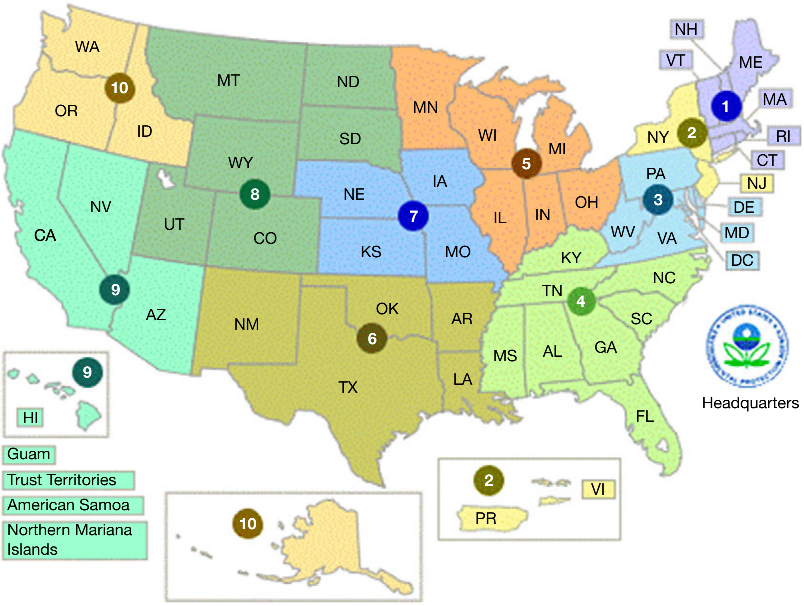
[25]Each office is responsible for the supervision of plants in several neighboring states. For example, regional office 1 is located in Boston, MA, and is responsible for the states of CT, MA, ME, NH, RI, and VT.

[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: $\text{DISTANCE}_{12} = \arccos\{\cos(\text{lat1})\cos(\text{lon1})\cos(\text{lat2})\cos(\text{lon2}) + \cos(\text{lat1})\sin(\text{lon1})\cos(\text{lat2})\sin(\text{lon2}) + \sin(\text{lat1})\sin(\text{lat2})\}2\pi r/360$ $r \approx 3963$ 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 10 EPA regional offices can be found on the EPA's website (https://www.epa.gov/aboutepa/visiting-regional-office).

FIGURE 3

Distribution of EPA Regional Offices

Figure 3 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.




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 1 for treated firms above (below) the median average distance (which is 100.42 miles).

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.

## 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 nonmutually exclusive channels through which decreases in analyst coverage might lead to higher corporate pollution.

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TABLE 5

Cross-Sectional Analysis: Regulatory Monitoring

Table 5 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: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{LONG\_DISTANCE}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{SHORT\_DISTANCE}_{i,t} + \beta_3 \text{TREATMENT}_{i,t} + \beta_4 \text{AFTER}_{i,t} + \delta \mathbf{X}_{i,t} + \varepsilon_{i,t}$ where subscripts *i* and *t* indicates firm *i* and year *t* respectively, while $\mathbf{X}_{i,t}$ 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 0 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 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 Table A.1 in the Supplementary Material 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 at the 10%, 5%, and 1% levels, 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** |
| No. of obs. | 1,212 | 1,212 |
| $R^2$ | 0.139 | 0.204 |

## 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.

[27]Appendix A.8 of the Supplementary Material presents some examples of environmental-related questions raised by analysts. Even if these questions garner a nonresponse (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 (Hollander, Pronk, and Roelofsen (2010), Lee (2016), and Gow, Larcker, and Zakolyukina (2021)). Therefore, regardless of the informativeness of answers given by managers, analysts can perform a monitoring role just by raising questions during conference calls.

(2018)). In the presence of such direct monitoring activities, managers are strongly incentivized to improve corporate environmental performance. Consequently, an exogenous decrease in analyst coverage for treated firms may lead to reduced analyst 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 collect 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 these 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 1 if at least one environmental-related question was raised in a particular firm-year, and 0 otherwise. About 14.34% of firm-year observations in our sample saw at least one such question.

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 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. (2019b) document that local institutional ownership reduces

[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.

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TABLE 6

Channels: Environmental-Related Questions During Earnings Conference Calls

Table 6 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: $Y_{i,t} = \alpha_{i,t} + \beta_1\text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2\text{TREATMENT}_{i,t} + \beta_3\text{AFTER}_{i,t} + \delta\mathbf{X}_{i,t} + \varepsilon_{i,t}$ where subscripts $i$ and $t$ indicates firm $i$ and year $t$ respectively, while $\mathbf{X}_{i,t}$ 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 0 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 Table A.1 in the Supplementary Material for the definition and construction of variables. Standard errors are clustered at the firm level. $t$-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, 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 |
| No. of obs. | 371 | 371 | 516 | 516 |
| Pseudo $R^2$ | 0.089 | 0.152 | 0.136 | 0.179 |

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., O'Brien and Bhushan (1990), Bushee and Noe (2000)). Consequently, this reduces the role and influence of institutional shareholders in shaping a firm's environmental policies.[29]

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 in Panel A of 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.

[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 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.

## TABLE 7
### Channels: Institutional Investors

Table 7 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: $Y_{i,t} = \alpha_{i,t} + \beta_1\text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2\text{TREATMENT}_{i,t} + \beta_3\text{AFTER}_{i,t} + \delta\mathbf{X}_{i,t} + \varepsilon_{i,t}$ while the specification in Panel B is $Y_{i,t} = \alpha_{i,t} + \beta_1\text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{HIGH\_OWNERSHIP}_{i,t} + \beta_2\text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} \times \text{LOW\_OWNERSHIP}_{i,t} + \beta_3\text{TREATMENT}_{i,t} + \beta_4\text{AFTER}_{i,t} + \delta\mathbf{X}_{i,t} + \varepsilon_{i,t}$ where subscripts $i$ and $t$ indicates firm $i$ and year $t$ respectively, while $\mathbf{X}_{i,t}$ is a vector of control variables. In Panel A, the dependent variable is the percentage of equity of the firm owned by: institutional investors ($\text{IO}_{i,t}$) in columns 1–2; quasi-indexers ($\text{QUASI\_INDEXERS}_{i,t}$) in columns 3–4, and; public pension funds ($\text{PUBLIC\_PENSION\_FUNDS}_{i,t}$) in columns 5–6. $\text{IO}_{i,t}$ is the percentage of shares held by intuitional investors. $\text{QUASI\_INDEXERS}_{i,t}$ is defined following Bushee (2001) and is calculated as the percentage of shares held by quasi-indexers. $\text{PUBLIC\_PENSION\_FUNDS}_{i,t}$ 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 $\log(\text{TOT\_POL})_{i,t}$ in odd columns and $\log(\text{TOT\_POL\_TO\_SALES})_{i,t}$ in even columns. $\log(\text{TOT\_POL})_{i,t}$ 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. HIGH $(\text{LOW\_IO})_{i,t}$ 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 0 otherwise. HIGH (LOW) $\text{\_QUASI\_INDEXERS}_{i,t}$ 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 0 otherwise. HIGH $(\text{LOW})\text{\_PENSION\_FUNDS}_{i,t}$ 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 0 otherwise. $\text{TREATMENT}_{i,t}$ 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 Table A.1 in the Supplementary Material 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 at the 10%, 5%, and 1% levels, 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 |
| No. of obs. | 624 | 624 | 624 | 624 | 624 | 624 |
| $R^2$ | 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, and 5 = log(TOT_POL) | | | | | |
|---|---|---|---|---|---|---|
| | Dep. Variable in Columns 2, 4, and 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* |
| No. of obs. | 624 | 624 | 624 | 624 | 624 | 624 |
| $R^2$ | 0.154 | 0.189 | 0.152 | 0.187 | 0.153 | 0.188 |

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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 (Bushee (2001), Appel, Gormley, and Keim (2016)). As quasi-indexers have relatively long investment horizons, they are more likely to impose pressure on managers to improve environmental performance (Kim et al. (2019b), Chen et al. (2020)). In columns 3–4 in Panel A of Table 7, 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 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 in Panel A of Table 7, 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 (Kelly and Ljungqvist (2012), Derrien and Kecskes (2013)).

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

TABLE 8

Channels: Investments in Pollution Abatement

Table 8 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: $Y_{i,t} = \alpha_{i,t} + \beta_1 \text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2 \text{TREATMENT}_{i,t} + \beta_3 \text{AFTER}_{i,t} + \delta \mathbf{X}_{i,t} + \varepsilon_{i,t}$ where subscripts $i$ and $t$ indicates firm $i$ and year $t$ respectively, while $\mathbf{X}_{i,t}$ 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 2 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 0 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 Table A.1 in the Supplementary Material for the definition and construction of variables. Standard errors are clustered at the firm level. $t$-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, 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 |
| No. of obs. | 1,212 | 1,212 | 1,112 | 1,112 |
| $R^2$ | 0.040 | 0.061 | 0.081 | 0.094 |

[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 0. Average environmental expenditures in our sample as a percentage of total capital expenditures is 9.83% and is comparable to the 9.77% reported by Clarkson et al. (2004).

that treated firms decrease their log environmental 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.

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 0 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 2 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 $\times$ 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 are a channel through which reduced analyst coverage increases corporate pollution.

## 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

[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.

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TABLE 9

Channels: Compensation Contracts and Sustainability Committees

Table 9 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: $Y_{i,t} = \alpha_{i,t} + \beta_1\text{TREATMENT}_{i,t} \times \text{AFTER}_{i,t} + \beta_2\text{TREATMENT}_{i,t} + \beta_3\text{AFTER}_{i,t} + \delta\mathbf{X}_{i,t} + \varepsilon_{i,t}$ where subscripts $i$ and $t$ indicates firm $i$ and year $t$ respectively, while $\mathbf{X}_{i,t}$ is a vector of control variables. The dependent variable is ENVIRON_COMP$_{i,t}$ in column 1 and SUSTAIN_COMM$_{-2,+2i,t}$ in column 2. 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. 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. 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 Table A.1 in the Supplementary Material for the definition and construction of variables. Standard errors are clustered at the firm level. $t$-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | ENVIRON_COMP | 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 |
| No. of obs. | 213 | 406 |
| Pseudo $R^2$ | 0.357 | 0.504 |

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 1 if there is at least one executive that has their compensation contract linked to environmental performance, and 0 otherwise (ENVIRON_COMP).[35] Results from a probit model in column 1 of Table 9 show that firms that experience decreases in analyst coverage are significantly less likely to link executives' pay to environmental performance.

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.

[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 0 otherwise. In our sample, about 5% of firm-year observations have environmental-related incentives in their executives' contracts.

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 1 if a firm has a sustainability committee, and 0 otherwise. Our test focuses on the 2 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.

## VII. Conclusions

This article 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 nonmutually 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.

## Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109023000340.`

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