The Real Effects of Environmental Activist Investing

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July 2026

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Abstract

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Keywords: Activism, Environmental Social and Governance, Socially Responsible Investment, Corporate Social Responsibility, Pensions

JEL Classifications: G23, G34, M14, Q50

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Abstract

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Environmental activism, which involves shareholders engaging management to improve a firm's environmental impact, has gained popularity among investors (Dimson et al. 2015; Hong et al. 2020; Krueger et al. 2020; Bauer et al. 2021). With investors further internalizing the negative externalities created by firms, it is essential to understand the real effects that environmental activism has on targeted firms, local economies, and shareholders.

There is limited research studying the real effects of environmental activism on pollution and the environment. On the one hand, there is a mature literature studying how activist shareholders can affect a firm's governance, as well as its financial and operational performance, with an aim of increasing their wealth.[1] On the other hand, there is a growing literature studying investor preferences for socially responsible investments, often reflected by active divestment campaigns.[2] However, between these two literatures, there remains an important gap in our understanding of the willingness and ability of activist long-term shareholders to influence corporate environmental behaviors, the impact that these efforts have on local economies, and concomitant implications for firms' financial performance.

Can environmental activism bring about changes to corporate environmental behaviors? The answer to this question is unclear. Improvements in sustainability can have benefits such as valuable product differentiation, insurance against event risk, lower cost of capital, and lower regulatory risk (Servaes and Tamayo 2013; Chava 2014; Hong and Liskovich 2015; Albuquerque et al. 2019). At the same time, changes through capital investments may be costly, symptomatic of agency conflicts, and potentially harmful to a firm's business operations (Lenox and Eesley 2009; Fowlie 2010; Cheng et al. 2023). Further, if any changes do come about, it is necessary to understand the steps undertaken to achieve these changes, to evaluate the costs and benefits of such actions, and to study their impact on the local economy.

We answer these questions by studying the activist campaign brought forward by the Boardroom Accountability Project (BAP), initiated by the New York City pension system (NYCPS).[3]

[1] Papers studying activism include Nesbitt (1994), Smith (1996), Wahal (1996), Huson (1997), Carleton et al. (1998), Del Guercio and Hawkins (1999), Brav et al. (2008), Aghion et al. (2013), Brav et al. (2015), Appel et al. (2016), and Brav et al. (2018). For surveys of shareholder activism, see Karpoff (2001) and Gillan and Starks (2007).

[2] Empirical research on divestment and negative selection includes Teoh et al. (1999) and Becht et al. (2019). Theoretical studies of impact investing include those by Heinkel et al. (2001), Hart and Zingales (2017), Chowdhry et al. (2018), Morgan and Tumlinson (2019), Broccardo et al. (2022), Green and Roth (2025), and Oehmke and Opp (2025).

[3] Initiated by the NYCPS, the BAP brought together influential investors, including the California Public Employees' Retirement System (CalPERS) and the California Teachers' Retirement System (CalSTRS).

We focus on the BAP campaign because it provides a unique setting to measure the efficacy of environmental activism. First, we observe the target selection procedure, typically unobserved in similar activism settings, that aids in determining the appropriate counterfactual firm in our empirical analysis. Second, we observe the interactions between the BAP campaign, investors, and the management, allowing us to shed light on potential mechanisms for our findings. Third, the setting of the BAP campaign is typical in terms of assets and ownership, likely making our results extendable to other activism settings.

We study the efficacy of the BAP campaign using a stacked cohort difference-in-differences design to identify the real effects of environmental activism. Guided by the target selection model, we construct a propensity score–matched sample of firms in the same industry and with similar financial and sustainability characteristics. As the BAP campaign targeted firms in a staggered manner, we resort to using a stacked-cohort approach (Baker et al. 2022; Roth et al. 2023), whereby we pair each treated firm with the closest propensity score–matched control firm within its cohort. The empirical strategy estimates average treatment effects by exploiting variation in timing within each treated–control pair, conditional on the set of fixed effects. In addition to the high-dimensional fixed effects included in our empirical specifications, this design further mitigates concerns about time-varying shocks that coincide with the staggered targeting of firms.

We use a diverse set of granular databases to provide a comprehensive view of the real effects of environmental activism and to circumvent measurement errors that arise when relying on environmental, social, and governance (ESG) ratings to capture environmental performance (Berg et al. 2022). From the Environmental Protection Agency (EPA), we obtain plant-chemical-level pollutive activity reported under the Toxics Release Inventory (TRI) program. At the community level, we measure the impact of pollution using the Air Quality System (AQS) program and the agency's Risk-Screening Environmental Indicators (RSEI) model. To quantify abatement-related initiatives at the plant level, we use the Pollution Prevention (P2) database from the EPA and data from the Energy Information Administration (EIA). We merge these sources at the plant level with the National Establishment Time Series (NETS). Finally, we complement our analysis with standard firm-level databases, including Compustat, the Center for Research in Security Prices (CRSP), Institutional Shareholder Services (ISS), and ASSET4.

First, we provide evidence that environmental activist investing improves the environmental performance of targeted firms. On average, plants of targeted firms reduce their total toxic chemical releases by 15%. Examining time patterns of pollutive activity, we find no differential trends in the preperiod. Following targeting, however, we observe a gradual and persistent decline in releases relative to matched control firms. Decomposing the source of these reductions, we find that active on-site reductions in releases are the primary driver of overall improvement.

Second, we find that these engagements have positive externalities on the local economies beyond the targeted firms themselves. In a stacked difference-in-differences design, we use air quality monitors within two miles of targeted plants and find that emissions harmful to public health drop significantly compared to plants of matched control firms. Further, using the EPA's modeled estimates to capture the chronic human health risk at the local census block group level, we find that plants of targeted firms reduce their pollution intensity. We confirm that these results at the local economy level are indeed driven by reductions in the intensity of toxic chemical releases using data on modeled estimates at the plant-chemical level. Together, these results provide additional evidence on the positive externalities arising from firms managing the impact of their pollution on stakeholders.

Third, we show that the decline in pollutive activity is driven by a combination of operational improvements and upfront abatement-related expenditures. Plants of targeted firms implement new on-site source reduction initiatives, including process and equipment modifications, improved operating practices, and the installation of spill and leak prevention systems. We also find a significant increase in abatement-related capital expenditures at targeted plants. Consistent with these findings, disclosures in the sustainability reports of targeted firms confirm such investments.

To better understand the costs and benefits of environmental activism, we study its relationship with target firms' financial performance and find a trade-off: although environmental activism leads to real improvements, it comes at the cost of a decline in their profitability and operating incomes. This decline stems from increased operating expenses and upfront abatement expenditures. Further, event-study estimates around the exempt-solicitation date—when most investors learned of the BAP proxy-access proposals—show negative abnormal returns. We interpret the negative equity response as evidence that, on average, investors expected the nearterm costs of these actions to outweigh their longer run benefits.[4] As we discuss later, however, managers viewed the BAP campaign as a credible threat and responded to its specific mandates, reflecting that they perceive costs of inaction to be substantial even if the marginal investor did not view these actions as value-enhancing.

We perform a battery of robustness tests that further reinforce our findings, confirming the drop in toxic chemical releases at the plant level. First, we mitigate concerns that the decline is driven by lower production by showing that production remains unchanged—both at the plant level and at the plant-chemical level. Second, we rule out concerns regarding the sensitivity of estimates to propensity score–matched inputs by including additional covariates such as (i) pretarget level of toxic releases, (ii) pretarget level of plant output, and (iii) pretarget plant-level financial constraints. Third, we assuage concerns that the drop in emissions results from an increase in firms' offshoring production activities by focusing on the subsample of firms (i) in the utility sector where the likelihood of offshore production is relatively low and (ii) with low levels of offshoring identified using the textual measure of Hoberg and Moon (2017). Lastly, the fourth set of tests mitigate concerns that plant closures drive the drop in emissions by focusing on the subset (i) of firms where plants have continuously reported emissions throughout the sample period, (ii) excluding plants that do not report any emissions, and (iii) of firms that did not file for Chapter 11 bankruptcy.

Lastly, we show that our results are most consistent with boards responding to the specific mandates of the activist campaign rather than to the general attention it generated. We establish this with three sets of evidence. First, we hand collect regulatory filings and management communications showing that targeted firms explicitly viewed the campaign as a credible threat. Second, we find that firms targeted for nonenvironmental reasons do not improve their environmental behavior around targeting. Third, we step outside our baseline sample and consider other prosocial campaigns of the BAP, as well as other environmental campaigns, and show that targeted firms respond to specific mandates of investors. Taken together with our baseline estimates, these results suggest that shareholders can impose their prosocial preferences on firms through environmental activist campaigns.

[4] An alternative, not mutually exclusive, interpretation is that investors assign substantial uncertainty to the magnitude and timing of long-term benefits, thereby lowering valuations even if the true expected benefits are positive.

Our results shed light on the efficacy of climate-focused engagements; however, they come with important caveats and must be interpreted cautiously. First, the setting and the data are not well suited to evaluate the welfare consequences of engagements like the BAP. Such an analysis will need to evaluate the nonpecuniary benefits to stakeholders and investors in their entirety. Moreover, the costs are likely difficult to measure and may take a longer time to materialize. Second, our study cautions against policies recommending random targeting to achieve environmental change. Instead, the results should be interpreted as: would the same changes have occurred if the BAP targeted these firms but remained passive in its demands?

This paper contributes to the literature on environmental campaigns by documenting the real effects of such campaigns using granular measures. We provide novel evidence of granular improvements, at the plant and community levels, in responding to such activism and show that these do not stem from changes to production. Importantly, we show that such campaigns incentivize firms to invest in abatement technology.[5] We fill an important gap between two strands of literature. On the one hand, there exists evidence that institutional investors—including pension funds, hedge funds, and index funds—can influence various aspects of firms' governance structures and operations (Carleton et al. 1998; Del Guercio and Hawkins 1999; Brav et al. 2008; Aghion et al. 2013; Brav et al. 2015; Appel et al. 2016; Brav et al. 2018). On the other hand, papers on institutional investors and their campaigns typically rely on either ESG scores or estimated emissions, but the interpretation of such findings is often unclear. In this context, prior work has focused mostly on governance outcomes rather than real effects (Dimson et al. 2015; Gillan et al. 2021). Between these two literatures, however, there is limited evidence that environmental activism has real effects.

Using granular measures advances the literature on the real impact of investor campaigns. First, as noted by Ferrell et al. (2016), a key limitation of prior work is its reliance on static and narrowly defined measures of corporate social responsibility. Moreover, there is well-documented disagreement across ESG data providers (Gibson Brandon et al. 2021; Berg et al. 2022), making it difficult to offer a consistent economic interpretation of results in studies that rely on a single

[5]Different from these studies, regulation is shown to affect abatement investment (De Simone et al. 2026) and, more generally, productivity. Proposed mechanisms for this include R&D spillovers, first-mover advantages (Porter and Linde 1995; Lanjouw and Mody 1996; Jaffe and Palmer 1997), and encouraging firms to optimize energy use and adopt green technologies (Newell et al. 1999; Fan et al. 2019; Wu et al. 2024).

rating provider (Dyck et al. 2019; Avramov et al. 2022; Lindsey et al. 2025). Second, recent studies also raise concerns about data quality of third-party vendors. For example, Aswani et al. (2024) show that systematic differences between vendor-estimated and firm-disclosed emissions introduces significant measurement error.[6] This makes it difficult to disentangle whether observed changes in emissions reflect real changes or differences in vendor methodology over time (Azar et al. 2021; Bolton and Kacperczyk 2021a; Bolton and Kacperczyk 2021b; Zhang 2025). By contrast, we use firm-reported, plant–chemical-level emissions data that allow us to trace granular real effects of environmental activist campaigns, providing sharper inference than studies relying on ESG scores.

Relatedly, this paper documents the direct channel of environmental activist investing—how firms improve their environmental behaviors in response to activist investors that target firms for environmental concerns. We show that long-term investors engage management by creating a credible threat, leading to improvements at targeted firms. Consistently, our results stem from firms undertaking new abatement initiatives instead of reducing their production. In doing so, this study complements two concurrent working papers by Akey and Appel (2020) and Chu and Zhao (2019) that study the externalities of hedge fund activism through an indirect channel whereby changes to a firm's governance structure and operations affect its environmental behaviors. These campaigns involve investors focused on improving the target firm's financial performance in the short-run, and thus improvements tend to stem from changes in operational efficiency instead of firms undertaking new abatement initiatives.

Our study also contributes to the literature on the efficacy of engagement by pension funds in the context of environmental activism. Broccardo et al. (2022) and Berk and Van Binsbergen (2025) argue that engagement is more effective than divestment in pushing firms to act in a socially responsible manner, especially in the context of pension funds who are universal owners with fiduciary duties to their beneficiaries Krueger et al. (2020). Our findings—that portfolio firms reduce pollution through increased investment in abatement technologies—highlights the effectiveness of long-term investors using shareholder engagement to achieve nonfinancial goals. In doing so, our study complements prior research that documents the importance of monitoring and engagement to investors when pursuing financial goals (Black 1990; Appel et al. 2016) and

[6]See other related work on this issue Cohen et al. (2026).

to a burgeoning literature on the impact of long-term investors using shareholder engagement to achieve nonfinancial goals (Gormley et al. 2023; Starks 2023). Moreover, our findings on the trade-off between environmental performance and financial returns highlights that investor engagement successfully prompts costly managerial action and is not perceived as value-enhancing by the marginal investor. These findings bring nuance to the basic tenet of economic theory that a firm should maximize shareholder value and have implications for the debates on stakeholder primacy models (Friedman 1970; Hart and Zingales 2017; Bebchuk and Tallarita 2020; Bhagat and Hubbard 2020).

More broadly, our study fits into the literature examining the determinants of corporate environmental behaviors. Prior research has focused on the roles of financial constraints (Cohn and Deryugina 2018; Götz 2018; Bartram et al. 2022; Xu and Kim 2022) listing status (Shive and Forster 2020), supply chains (Schiller 2018), limited liability (Akey and Appel 2021), and political affiliation (Kahn et al. 2023). Prior work has also documented that firms' environmental profiles are affected by regulation and shaped by market forces (Ben-David et al. 2021; Schlenker and Taylor 2021; Bartram et al. 2022; Duchin et al. 2025). This study contributes by showing that environmental activism has a significant real impact on corporate environmental behaviors. Moreover, our evidence aligns with theoretical research and provides new and important evidence for U.S. pension investments in public equities (Ramadorai and Zeni 2024).

1 Boardroom Accountability Project

The NYCPS publicly announced the BAP on November 4th, 2014.[7,8] The goal of this campaign was to hold boards of the portfolio companies accountable to long-term shareholders and give pensioners a voice in oversight concerning climate change risks, board diversity, and excess CEO pay.[9] The BAP campaign aimed to increase the accountability of board members to sharehold-

[7]Throughout the paper, we align the date of targeting to the corresponding year of the annual shareholder meeting. Therefore, the first wave of targeting corresponds to the 2015 shareholder meeting season.

[8]The NYCPS is a combination of five funds, namely, the New York City Employees' Retirement System, the Teachers' Retirement System of the City of New York, the New York City Police Pension Fund, the New York City Fire Pension Fund, and the New York City Board of Education Retirement System. NYCPS manages approximately \$199 billion in assets under management. As of May 2019, the fund had around 29% of its assets allocated to U.S. public equities and was a long-term shareholder in more than 3,000 U.S. public companies. For more detail, please see the New York City Comptroller's website. Please see Appendix B for examples of communications by the BAP campaign.

[9]Note that the inclusion of climate risk consideration by the NYCPS is increasing among other institutional investors in general, as documented in an extensive survey conducted by Krueger et al. (2020). As they document,

ers by simultaneously filing proxy-access proposals at portfolio firms (see Appendix A). These proposals gave long-term shareholders the right to nominate directors on boards.

At the launch of the campaign, the comptroller emphasized this, as follows:

> Resolutions were filed at companies where we see risks associated with climate change, board diversity, and excessive CEO pay. Especially when it comes to the environment, business as usual is no longer an option. To effect true change, you need the ability to hold entrenched and unresponsive boards accountable, and that is what we are seeking to do. NYC Comptroller (2014)

While the BAP targeted firms for multiple mandates, this study focuses on firms targeted for their approach to climate change risks, which was one of their most significant mandates. Accordingly, the BAP campaign targeted carbon-intensive firms because they failed to address climate change concerns adequately.

Our discussions with the New York City comptroller's office confirmed that the NYCPS did not announce in advance the BAP campaign or the identities of the targeted corporations. The campaign targeted firms based on their underlying carbon reserves, a proxy for their potential to contribute to climate change. We also confirm these aspects through Freedom of Information Act (FOIA) requests and empirical tests.

The BAP campaign allows us to cleanly evaluate the efficacy of environmental activism for several reasons. First, we observe the underlying target selection criteria, unobserved in traditional investor activism settings, which aids in determining the appropriate counterfactual.[10] Second, the management did not anticipate the BAP campaign, and the governance-specific approach of using proxy-access proposals makes it different from targeting by shareholder proposals that are typically anticipated and impede managerial flexibility. Third, conversations with the Comptroller's office, FOIA requests, and evidence from regulatory filings help us better understand the BAP campaign's organization as well as the interaction between the firm and the activists, which ultimately helps us to shed light on the mechanisms driving our findings. Lastly,

investors' motivations for incorporating climate risks into their decision making can be financial, nonfinancial, or both. Further, environmental activism campaigns such as the BAP differ from traditional shareholder activism, which solely focuses on improving the financial and operational performance of the targeted firm (Nesbitt (1994)).

[10] Our ability to understand the target selection process and verify it through empirical tests confers an advantage in constructing appropriate counterfactual firms. This differs from the traditional shareholder activism literature which has relied solely on the target selection model to construct the counterfactual (Nesbitt 1994; Brav et al. 2008).

the NYCPS was not unique in terms of assets, ownership, or ability, potentially making the results extendable to other activism settings.

For the primary analysis, we focus on firms targeted by the BAP campaign instead of conditioning on campaign success. Our choice is motivated by the fact that the BAP campaign was very successful, characterized by high settlement rates with the management and immediate adoption of proxy-access provisions by firms in their bylaws. Moreover, the few proposals that went to the ballot garnered strong support from shareholders with high passage rates, with firms eventual adopting a similar proxy-access provisions that the BAP and its partners put forward. The success was driven by the credible threat posed by the NYCPS and its partners as they coordinated their engagements, communicated with other shareholders, and attended the targeted firms' annual meetings.

2 Data and Empirical Strategy

2.1 Summary of data sets

Our study combines data from the BAP campaign with publicly available data sets and other commercial databases. We hand collect data on which firms were targeted and for what mandate from publicly available shareowner annual reports, provided on the NYCPS's website.[11] Further, we obtain the NYCPS portfolio composition from the Comptroller's office. Table 1 provides the summary of activism events by the year of the shareholder annual meeting (panel A) and industry (panel B). Out of 181 BAP firm mandates in our sample, 62 relate to environmental concerns. We align the targeting year in our study to coincide with the year of the annual shareholder meeting. Sectors are mutually exclusive and are defined using the Fama-French 12 industry classification.

The composition of industries and the broad sample of firms targeted by the BAP are important for studying the real effects of environmental activism. The environmental footprint of these firms are large, with the plants of targeted firms contributing roughly a quarter of the total emissions by all firms in the United States before the BAP campaign. Moreover, the energy and utility sectors are among the heaviest polluters across all industries (Hockstad and Hanel 2018;

[11] The shareowner annual reports can be found here. Last accessed on June 29, 2019.

Shive and Forster 2020). Therefore, the collection of firms, related plants, and emissions makes it an ideal sample to study the effectiveness of environmental activist campaigns in addressing climate change risks at portfolio firms.

We combine data from the BAP campaign with a diverse set of microdata compiled from several sources. Our primary data come from the EPA, which provides detailed plant-chemical-level data on toxic releases reported under the TRI program. The TRI program captures how much of each toxic chemical is released to the environment or managed and includes information about facilities that manufacture, process, or otherwise use these chemicals. We measure abatement efforts through the data reported under the P2 program and expenditures from the EIA. Lastly, we capture the potential externalities around plants using outdoor air monitoring reporting to the AQS database and EPA's RSEI modeled estimates that tracks changes in human health and environmental impacts by integrating hazard, exposure, and population considerations into the evaluation of environmental releases of toxic chemicals.

Further, we merge our sample with other standard commercial databases, including BoardEx, Compustat, Execucomp, CRSP, ISS, Audit Analytics, RavenPack, and ASSET4. In Appendix C, we describe in detail the data construction and matching. Note that since the estimations involve matching across multiple data sets, the sample size and number of firms vary across our tests.

2.2 How did the BAP campaign select the targeted firms?

We begin by characterizing the campaign's targeting criteria. To do so, we engaged in multiple phone calls with the NYC Comptroller's office, submitted FOIA requests, and collected data on the campaign. Guided by these interactions, we observed that the BAP selected firms, not plants, for targeting. As a result, we perform matching at the firm-level to mirror the BAP campaign.

Our conversations with the Assistant Comptroller of the NYCPS indicated that they used the Fossil Free Index (FFI) to target firms for inadequate redressal of climate change risk. This list, which is compiled and maintained by the Carbon Underground 200, identifies the top 100 coal and the top 100 oil and gas publicly traded reserve holders globally, ranked by total carbon reserves.[12]

Using a panel of Russell 3000 firms, the benchmark portfolio for the NYCPS as a universal

[12]More information about this index can be found here.

owner, we verify the BAP's selection criteria by estimating a multivariate logistical regression that correlates pretargeting firm characteristics with the likelihood of being targeted for environmental reasons. Figure 1 plots the coefficient estimates from the target selection model, while Table IA.D1 presents estimates from additional specifications to show robustness.

Our estimates suggest that targeted firms were larger, on average, and more likely to be included in the FFI list. These results are consistent with our discussions and material received from the NYCPS, which indicates that a firm's rank on the FFI list was a driver for targeting. Notably, we do not find a statistically significant relationship between targeting and other financial and governance characteristics.

We also consider and rule out the possibility that the targeting by the BAP may be influenced by the attention brought by environment-related litigation, media coverage, or other activist investors like hedge funds. Using data on environment-related fines from Audit Analytics, we do not find that these fines predict targeting by the BAP. Second, we find no association between media coverage, defined as the natural logarithm of news articles extracted from RavenPack, and BAP targeting. Finally, we consider the possibility that other activist investors such as hedge funds drew the attention of the BAP toward targeted firms. To test this, we include an indicator variable if the firm was also targeted by activist hedge funds. In none of the tests do we find support for the attention channel driving the BAP's targeting choice for environmental reasons.

We note that the primary identification challenge in studying the real effects of the BAP campaign lies in ruling out other confounding events coinciding with the staggered nature of the campaign. In the subsequent section, we highlight how our empirical strategy addresses this issue.

2.3 Main specification

Guided by the target selection model, we use a propensity score–matching approach to identify counterfactual firms. This approach is consistent with the activism literature (Brav et al. 2015; Brav et al. 2018). To construct the candidate counterfactual firms, we use the constituents of the Russell 3000 index, which closely match the holdings of the NYCPS portfolio. We exclude firms that the BAP targeted for nonenvironmental reasons from our main sample. Thus, our baseline specification compares firms targeted for environmental reasons by the BAP to a counterfactual firm not targeted by the BAP but in the NYCPS's portfolio, in the same Fama-French 12 industry, and with similar financial and sustainability characteristics.[13] The propensity score estimation uses the following variables as matching covariates: firm size, operating income before depreciation divided by lagged total assets, market-to-book ratio, and ASSET4 score.[14]

We are careful in defining our dependent variable and in the choice of the estimation methodology. Recent research on the pollution intensity of firms argues for the importance of scaling in the context of equity returns (Shive and Forster 2020; Aswani et al. 2024; Zhang 2025). Moreover, research in regulatory economics suggests that a higher per-unit output of pollutant releases prompts a greater degree of regulatory scrutiny (Shimshack 2014; Earnhart and Friesen 2021). Accordingly, we scale pollution measures by contemporaneous plant-level sales from NETS.[15]

Further, several data sets including the TRI contain highly right-skewed and count-like distributions with masses of values at zero, posing challenges for ordinary least squares (OLS) estimation and interpretation (Cohn et al. 2022). In our own case, as evident from Table 2, some of the pollution measures exhibit similar patterns. There is an active debate among applied econometricians about the best way of dealing with count-like outcomes with similar distributions. Prior research has resorted to using a logarithmic transformation; however, Silva and Tenreyro (2006) and Cohn et al. (2022) have noted that linear estimations of such distributions can result in biases.[16] In our study, we believe these concerns about skewness and count-like data distributions are relevant for several dependent variables, and hence we estimate a fixed-effects Poisson model that produces consistent and reasonably efficient estimates under general conditions.[17]

[13] An alternative approach is to compare firms targeted for environmental reasons by the BAP campaign with those targeted for nonenvironmental reasons within the same campaign. While helpful, these comparisons are less informative for two reasons. First, they cannot control for industry shocks that differentially affect treated and control firms in this case. Second, these comparisons cannot cleanly disentangle whether targeted firms respond to NYCPS's environmental mandate rather than being part of the BAP campaign or being targeted for proxy access. For these reasons, we separately analyze firms targeted for nonenvironmental reasons in Appendix E to rule out concerns about general attention while establishing the importance of the specificity of the demands made by the BAP campaign.

[14] Studying the characteristics of our matching in Appendix C, we find that the matched pair is statistically indistinguishable along important financial characteristics except for firm size.

[15] Several recent studies use NETS merged with TRI plant-level data (see Xu and Kim (2022) and Dasgupta et al. (2023); and citations therein). Appendix C provides matching details.

[16] Ryan et al. (2021) and Bellégo et al. (2022) show that estimating a Poisson model on data that does not follow such a distribution has its own set of potential biases.

[17] A few of our analyses examine the extensive margin response. As such, the dependent variable of interest does not exhibit a count-like distribution, and therefore, we estimate a linear probability model in such cases.

We estimate the following empirical specification:

$$\mathbb{E}[Y_{icst(p)}] = \exp\left(\beta \cdot I\left(Post_{it(p)}\right) \times I\left(Environment_{i(p)}\right) + \kappa_{ic(p)} + \delta_{ct(p)} + \omega_{st(p)} + \varepsilon_{icst(p)}\right), \quad (1)$$

where c indexes a toxic chemical released by a plant i at time t. $I\left(Post_{it(p)}\right)$ is an indicator variable equal to one for all years after the activism event (pseudoevent) year and zero otherwise. To assign pseudoevent years to the control group firms, we use a stacked cohort approach as targeting by the BAP was staggered. This stacked-cohort approach helps address concerns about heterogeneous treatment effects and avoids biases associated with two-way fixed effects estimators (Baker et al. 2022; Roth et al. 2023).

We proceed as follows. For each targeting year t (cohort), we match target and control firms using firm-year observations from the 3 years before to the 3 years after the event, restricting the sample period to 2012–2020. We then select control firms from the same Fama-French 12 industry classification as each treated firm.[18] Next, within each cohort, we match each treated firm with a control firm that has the closest propensity score, forming matched cohort-pairs (p). A key advantage of matching within each cohort-pair is that it allows us to difference out industry trends.[19] Therefore, our empirical specification estimates the average treatment effects (β) by relying on variation in treatment timing within each matched pair of treated and control firms.

$I\left(Environment_{i(p)}\right)$ is an indicator variable equal to one if the BAP targets the firm for environmental reasons and zero otherwise. The primary coefficient of interest is β, which captures the within plant-chemical changes in behaviors following the targeting by the BAP. We take a sampling design approach and cluster standard errors at the firm level (Abadie et al. 2023).

Our empirical specification includes a stringent set of high-dimensional fixed effects. We include plant $\times$ chemical fixed effects ($\kappa_{ic(p)}$) to control for time-invariant heterogeneity at the plant-chemical level. Chatterji et al. (2009), Di Giuli (2013), and Akey and Appel (2021) note that it is unclear how to aggregate toxic releases across chemicals and, importantly, compare

[18]We adopt matching with replacement to minimize bias as highlighted by Abadie and Spiess (2022). This enables a larger sample size by allowing multiple treated firms to be matched to the same control firm that best meets the matching criteria. Alternatively, one can adopt matching without replacement; however, such an approach can introduce bias in the asymptotic distribution of the postmatching regression estimator, particularly when the pool of suitable controls is limited.

[19]Note that since the estimations involve matching this sample with additional data sets, as described in Appendix C, there is variation in the number of firms across our tests.

their environmental impact. Given this, we retain the unit of analysis at plant-chemical-year level, exploit within chemical-year variation by including chemical $\times$ year fixed effects ($\delta_{ct(p)}$). We further include state $\times$ year fixed effects ($\omega_{st(p)}$) to control for time-varying heterogeneity in location-specific differences in environmental regulatory enforcement (Shapiro 2022).[20] In keeping with the literature (e.g., Gormley and Matsa (2011)), we allow these fixed effects to vary by cohort-pair (p), as this approach is more conservative than including simple fixed effects. Note that as we pair every treated firm to the closest propensity score matched control firm, our estimations subsume the coefficient on *Post* and industry trends.[21]

Under our empirical specification, the primary identification assumption is that within each matched treated-control firm pair, after controlling for the stringent set of high-dimensional fixed effects, targeting by the BAP campaign is as good as random assignment. We acknowledge that under such a specification, it seems unlikely that other explanations could drive our findings; for that to happen, systematic differences between each pair of treated and control firm would need to align perfectly with the staggered targeting by the BAP campaign within each cohort and persist even after accounting for the relevant set of high-dimensional fixed effects.

3 The Real Effects of Environmental Activism

3.1 Improvements to corporate environmental behaviors

We begin by studying changes in toxic chemical releases around targeting by the BAP. Specifically, we estimate event-time dummies with plant and year fixed effects using a Poisson empirical specification. We interact these with cohort-pairs to ensure that we compare plants of treated and matched control firms, allowing us to control for industry differences. This specification is the most saturated specification that still preserves sufficient variation while accounting for time-varying differences across industries. Complementing these analyses, subsequent tests in the tables present results from the stringent specification, which additionally addresses residual

[20] The EPA has 10 regional offices, each of which is responsible for several states and territories; for more information, see EPA. Our inclusion of state $\times$ year subsumes EPA enforcement regions, allowing us to better control for time-varying differences in regulatory actions.

[21] We note that including state $\times$ industry $\times$ year in our empirical specification leaves us with little variation to estimate treatment effects for the main tables in the paper.

concerns about differences in location and chemical use.[22]

Plotting the coefficients relative to the year of targeting by the BAP, Figure 2 presents four pieces of evidence: First, Figures 2a and 2b show that there are no differential pretrends in the pollutive activity between treated and control plants. This absence of pretrends suggests that the BAP targeting did not, on average, coincide with the differential decline in chemical releases in the preperiod. Second, as can been from Figure 2b, firms actively reduce their on-site chemical releases—how much of the releases happen on the facility's physical location and are emitted through air, water, and land. Third, across both panels, after the targeting, there is a gradual and significant decrease in chemical releases of plants between treated and control firms. Lastly, this improvement is persistent. As we show later in Section 3.3, a combination of operational improvements in the short-term and investments in abatement in the long-term best explain these time patterns.

Table 3 presents estimates in a regression framework, controlling for an additional stringent set of high-dimensional fixed effects. Estimates from column 1 suggests that plants of targeted firms, on average, reduced their total toxic chemical releases. The point estimate in column 1 is -0.162, suggesting a decline of 14.9% (computed as 1-$exp(-0.162)$) in the growth rate of chemical releases, with the estimate being statistically significant at the 1% level. In column 2, consistent with the graphical evidence, we see that the plants reduce their chemical releases on-site, although by a smaller magnitude, but again with a statistically significant result at the 1% level.[23]

Chemicals vary significantly in toxicity, making it difficult to interpret the economic magnitude of reduction. For instance, a pound of mercury has a drastically different impact than a pound of asbestos.[24] Therefore, we compare our estimates to EPA's assessment of changes in chemical releases. Economically, our estimate of the percent reduction in chemical releases is

[22]When implementing the stringent specification for figures, the lack of independent variation in estimating event-time dummies can yield unstable estimates and imprecise standard errors, as the cluster-robust variance–covariance matrix becomes ill-conditioned and results become sensitive to arbitrary choices of baseline parameterization (Miller 2023; Schmidheiny and Siegloch 2023; Borusyak et al. 2024).

[23] In Table IA.D2, we show that around the BAP campaign there are no changes in plant-level output and employment. Further, as we discuss in Section 5, we find no changes in production activity at the plant-chemical level. Together, these suggest that targeted plants improve their emissions efficiency by lowering pollution but not by changing their production.

[24]Mercury is highly toxic, even in small amounts, and poses a threat to child development, impacting the nervous, digestive, immune, pulmonary, renal, dermatological, and ocular systems (WHO 2021). Asbestos exposure is linked to cancers of the larynx, ovary, and lung as well as mesothelioma (Schlünssen et al. 2023).

comparable to estimates from the EPA's evaluation that finds a reduction of 15% in the amount of toxic chemicals released into the environment after the implementation of source reduction initiatives at plants (Ranson et al. 2015). These findings provide initial evidence of the significant impact of environmental activism.[25]

3.1.1 Extensive margin: Likelihood of starting and stopping the usage of chemicals

The previous analysis combines both the extensive and the intensive margins, ignoring the possibility of firms substituting across chemicals. Therefore, we next decompose our results by considering if firms change their likelihood of starting or stopping the usage of toxic chemicals in response to the activism campaign. Table IA.D4 presents the results from this exercise.

Focusing on the extensive margin of chemical usage, in column (1), we find that plants of targeted firms, on average, have a lower likelihood of starting to use a toxic chemical—if the plant reports releases from a chemical that it had not previously reported—posttargeting relative to the control set of plants. In column (2), we consider the likelihood of a plant stopping its use of a chemical. That is, for each plant, we look at previously reported releases of a given chemical that it no longer emits and does not do so at a future point in our sample. Estimates suggest that plants of targeted firms do not stop their use of chemicals differentially from control plants. Together, these results suggest that the improvements at targeted plants likely stem from changes both at the extensive and the intensive margins.

3.1.2 Robustness tests

Propensity score matching. We first consider robustness to additional plant-level matching inputs. Panel A of Table IA.D5 presents results from this exercise. We first reestimate our baseline specification on a propensity score–matched sample that uses the pretarget level of total toxic releases at plants as an additional matching covariate. Estimates from row 1 suggest that these tests are qualitatively similar to our baseline estimates. As prior work argues for controlling

[25] In Table IA.D3, we match the GHG emissions data reported under the EPA's Greenhouse Gas Reporting Program to the set of targeted and matched control firms to show that targeted firms, on average, reduce their CO_2e emissions. However, the sample coverage skews towards larger plants—those that emit at least 25,000 metric tons of CO_2e per year. This substantially reduces the sample size and induces selection in reporting among plants of targeted firms, and therefore, we relegate these results to the Internet Appendix. Reassuringly, these results provide additional evidence on the efficacy of the BAP campaign.

for the importance of financial constraints (Bartram et al. 2022; Xu and Kim 2022), in row 2, we additionally match on a pretarget plant-level measure of financial constraints from NETS.[26] In row 3, we additionally combine plant-level output as a matching covariate.

We note that the progressive addition of matching covariates leads to economically larger declines in toxic chemical releases. However, we cannot distinguish if these larger declines can entirely be explained by characteristics such as size, which might trend differently around 2015. At the same time, we also see a significant drop in the number of observations and an improvement in the precision of the estimates. Overall, these results suggest that input choices to the matching approach do not affect our conclusions; if anything, they suggest that our baseline estimates present a conservative bound on the true effect of environmental activism.

Ruling out alternative explanations through sample restrictions. Next, we rule out several alternative explanations for our findings. First, plants could reduce their toxic chemicals by offshoring their production. In panel B, we alleviate such concerns by focusing on utility firms where electricity generation and transmission is harder to outsource (row 4) and exclude firms that have a high propensity for offshoring using the textual measure introduced by Hoberg and Moon (2017) (row 5).[27] Across these tests, the estimates are similar to our baseline estimates for on-site reductions.

Second, we rule out the possibility that plant closures could drive the changes. We reestimate our baseline specification by excluding firms filing for Chapter 11 bankruptcy during our sample period in row 6, exclude plants reporting zero emissions to alleviate concerns that minimum reporting thresholds drive our results in row 7, and focus on plants that consistently report a positive amount of toxic release to the EPA throughout our sample in row 8. In these subsamples, we again find quantitatively similar results as our baseline estimates. These tests mitigate additional concerns about the quality of reporting being affected by the expansion of coverage over time, the inclusion of new industries and chemicals, and the potential changing minimum thresholds for reporting plants.

[26] We use the PAYDEX Score provided by NETS, which is a business credit score based on establishments' payment histories with suppliers and vendors, where a lower score indicates higher payment risk and hence a greater degree of financial constraint (Akey and Appel 2021; Chava et al. 2023).

[27] Note that since the focus is on the subsample of utility plants, we have significantly fewer number of observations. Hence, we are unable to include state $\times$ year fixed effects for this analysis.

Our empirical specifications control for time-varying regulatory concerns by way of inclusion of state $\times$ year fixed effects interacted with cohort-pairs. Nonetheless, given the significant differences in regulation in California compared to other states, we examine if targeted plants in California drive our results. In row 9, we reestimate our baseline specification by excluding plants located in California. Further, as Bartram et al. (2022) show the importance of financial constraints, in row 10, we interact pretarget plant-level financial constraints with *Post*, and include it as an additional control in our baseline specification. Across these tests, we find that the estimates are quantitatively similar to our baseline.

Overall, our analysis suggests that targeted firms reduce their total emissions, particularly on-site. This evidence bolsters the argument that environmental activism, on average, can bring about real changes in corporate environmental behaviors, rather than changing the incentives of firms to substitute pollution either within-firm or across geographies (Ben-David et al. 2021; Bartram et al. 2022).

3.2 Real effects on the local economy

3.2.1 Improvement in air quality near plants

We examine the effects of the BAP campaign on the local economy beyond targeted firms. We measure changes in air quality by matching air quality monitors to the plant's geolocation. We rely on the EPA's AQS database, which records outdoor air quality at more than 80,000 monitoring stations across the United States (Heitz et al. 2021). The EPA relies on these readings and sets National Ambient Air Quality Standards for pollutants considered harmful to public health and the environment, referred to as criteria pollutants. The key advantage of the AQS data is that it is not self-reported.

We use daily measurements of criteria gases and estimate separate regression models for each of the gases. Our empirical specification includes cohort-pair (p) $\times$ monitor that ensures we compare treated firms against matched controls from the same industry and monitored by the same air quality monitor. Moreover, we include state $\times$ year to control for differences in enforcement (Shapiro 2022). Table 4 presents estimates from this exercise.[28] Column 1 shows a

[28]Prior work documents heightened health risks in the immediate vicinity of a plant, with these risks declining as distance increases (Davis 2004; Currie et al. 2015; van Binsbergen et al. 2026).

statistically significant reduction in $PM_{2.5}$, with the magnitude corresponding to a drop of 29.5% relative to the preperiod sample average. Column 2 shows a drop in carbon monoxide and is of larger magnitude. We do not find a statistically significant change in other criteria pollutants. Together, these results suggest improvement in air quality in the immediate vicinity of targeted firms' plants.

3.2.2 Reduction in intensity of pollution

A key observation is that not all changes to pollution have the same impact on local economies. Plants of the targeted firms have choices about the steps they can take to reduce their releases, and these decisions have important implications for stakeholders. Among them, one possibility is that firms may focus on reducing their pollution intensity, accounting for the local population, toxicity, and dosage. Alternatively, they may focus on reducing a headline number without considering the local economy.

Therefore, we examine how the intensity of pollution changes around the local economies of targeted plants.[29] We rely on the EPA's RSEI model (EPA 2019a), which calculates a unitless value that accounts for the size of the chemical release, the fate and transport of the chemical through the environment, the size and location of the exposed population, and the chemical's toxicity (see Figure IA.D1). We use the RSEI scores aggregated to the census block group level from the RSEI Geographic Microdata and match it to the location of plants from the TRI database (see Appendix C for details on matching).

Estimates from Table 5 suggest a decrease in pollution intensity at the census block group level. Specifically, consistent with the analysis in Section 3.1, in column 1 of Table 5, we find a significant change in the number of individual toxic chemicals. Moreover, in column 2, we find a differential reduction in the number of chemicals with nonzero concentrations in census block groups in which plants of targeted firms operate. These reductions are economically significant and meaningful, with the estimates implying decreases of 2.4% and 6.8%. In columns 3 and 4, we find a reduction in both the toxic concentration and aggregated toxicity-weighted concentration using only inhalation toxicity weight for cancer effects. However, these estimates are imprecise,

[29]The intensity of pollution depends on the interaction of three factors: (i) the size of the affected population, (ii) the toxicity of the pollution, and (iii) the quantity of the emissions.

making them statistically indistinguishable from zero, perhaps reflecting the noise from aggregation at the census block group level. Therefore, in Appendix Table IA.D6, we present results using plant-level RSEI scores that capture the plant-specific impact on the local area. In these tests, we find that the local-economy reductions are driven by decreases in both the quantity and the intensity of toxic chemical releases from plants of targeted firms relative to matched control plants. Together, these results provide additional evidence on the positive externalities arising from firms managing the impact of their pollution on stakeholders.

3.3 How are firms achieving this reduction?

We consider the steps taken by plants of targeted firms to reduce their chemical releases. First, we focus on source reduction and preventive efforts undertaken in the plants. Second, we consider new abatement initiatives undertaken to reduce waste production. Importantly, in the last section, we show that a decline in production does not drive the reduction in chemical releases.

3.3.1 Source reduction and preventative efforts

Source reduction. We begin our analysis with a focus on the initiatives reported to the EPA's P2 program. This program collects information to track progress in reducing waste generation. Given that we can only examine firms' responses in a short window of 3 years after targeting by the BAP, we expect efforts to be visible among new and immediate initiatives that the plants undertake to reduce pollution.

Panel A of Table 6 focuses on two of the on-site abatement initiatives used to control emissions and comply with environmental regulations. Estimates from column 1 suggest that plants of targeted firms implemented new process and equipment modification initiatives. Examples of these initiatives include capturing leaking vapors from equipment like pumps, compressors, or pressure relief devices or improving the quality of fuel. Column 2 suggests that firms also improved their operating practices including training to reduce waste. For example, plants institute procedures to manage their production schedules efficiently.

Preventative efforts. To better understand the preventative efforts undertaken by firms, we adopt the lens of the waste management hierarchy developed by the EPA. This hierarchy ranks environmentally preferred waste-management strategies (panel B of Figure IA.D2) and acts as a pecking order. As before, we begin by analyzing the time dynamics of the likelihood to initiate on-site recycling in Figure 3. Plotting the coefficients relative to the year of targeting by the BAP, it presents four pieces of evidence: First, we do not see differential pretrends in the initiating on-site preventative efforts between treated and control plants. Second, there is greater propensity among plants of targeted firms to initiate on-site preventative efforts. Third, after the targeting, there is a gradual and significant increase in on-site preventative efforts between plants of targeted and matched control firms. Lastly, this improvement is persistent and as we show next, these time patterns are consistent with investments in new abatement initiatives at plants in the long-term.

Estimates in panel B of Table 6 suggest that targeted firms focus their improvements on reducing the total amount of on-site waste produced. Specifically, across the columns, we see that the plants of targeted firms relative to the plants of matched control firms, are more likely to initiate on-site waste management that includes recycling, combustion for energy recovery, and treatment and disposal (EPA 2019b). In terms of economic magnitude, we note that the strongest response is observed for treatment and disposal, where the likelihood of initiating is higher by 7.1% for plants of targeted firms. Treatment and disposal involves safe and efficient processes to minimize environmental impact, including composting, incineration, and other technologies. Taken together, our results suggest that firms focus their effort toward high-impact on-site waste management strategies in responding to environmental activism.

3.3.2 Abatement initiatives: Operation-related and capital expenditures

We next focus on plant-level capital expenditures related to abatement efforts such as emission control systems for nitrous oxide and sulfur dioxide. We rely on the subset of utility plants where reporting requirements provide detailed information on abatement investments. Specifically, the EIA captures total plant-level expenditures over time broken down into capital expenditures and expenditures on operation and maintenance. We merge this data from the EIA with our sample to pin down the exact capital expenditures plants are undertaking in response to environmental activism. As before, we scale plant level expenditures by plant-level sales for meaningful interpretation. To control for differences in EPA regulation by location, we include EPA region-by-year fixed effects. In addition, we include the following plant-level controls that correlate with expenditures: plant age and installed capacity.

Estimates from column 1 of Table 7 suggest that plants increase their total capital expenditures on abatement initiatives. Estimates from column 2 suggest that this increase primarily comes from new capital expenditures instead of maintenance. The estimate in column 2 is about three times larger relative to the sample standard deviation. In Appendix F, we show that, consistent with our estimates, firms communicate operation-related changes and investments in abatement technology to stakeholders in their annual sustainability reports. Overall our evidence suggests that the improvements to corporate environmental behaviors stem from both operation-related abatement initiatives and new capital expenditures.

3.3.3 No changes in production

A primary alternative explanation is that a reduction in economic activity drives the decline in releases—firms that produce less mechanically release fewer chemicals. While we do not observe actual production in the data, we consider the production ratio from the TRI database (Akey and Appel 2020) as our key proxy for understanding changes to production. The production ratio, defined for each plant's chemical usage, is the ratio of the quantity of output in any given year relative to the quantity of output in the previous year. An advantage of this measure is that it is output-based and available at the plant-chemical level. As a second approach, following Akey and Appel (2020), we construct a proxy for total production by normalizing production to one in the first reporting year.[30] Specifically, we compute normalized production as

$$CumulativeProduction_{i,c,t} = \prod_{t \neq 1}^{t} 1 \times \frac{QuantityProduced_{i,c,t}}{QuantityProduced_{i,c,t-1}} = \prod_{t \neq 1}^{t} 1 \times Production_{i,c,t}. \quad (2)$$

Figure 4 plots changes in production around the BAP campaign. Specifically, Figure 4a plots the coefficients and their 95% confidence intervals for the production ratio, while Figure 4b plots the same for normalized production. As before, we use a Poisson empirical specification and estimate event-time dummies with plant and year fixed effects. We interact these with cohort-pairs to ensure that we compare plants of treated and matched control firms, allowing us to

[30] We replace the missing observations with one when defining this measure.

control for industry differences. Plotting the coefficients relative to the year of targeting by the BAP, we do not find any change in production across either definitions.

We then reestimate Equation (1) for the two dependent variables, controlling for fixed effects similar to our baseline estimations (Table 3). Table 8 presents the results from this exercise. Estimates from column 1 suggest that the growth rate in production did not change for targeted firms' plants relative to plants of control firms. Using the cumulative production as our main dependent variable, column 2 consistently finds that plants of targeted firms do not change their production around the targeting by the BAP.

Together with the results in Table IA.D2 on no changes in plant-level sales, our findings suggest that targeted plants improve their emissions efficiency —by lowering pollution but not by changing their production. Moreover, targeted firms' plants increase their abatement activities rather than decrease production in response to environmental activist campaigns. These results contrast with and complement two concurrent papers (Akey and Appel 2020; Chu and Zhao 2019) that study the externalities of hedge fund activism. These papers document that improvements to environmental behaviors are a byproduct of changes to the firm's governance structure and operations. Since these campaigns involve investors focused on improving the target firm's financial performance in the short-run, improvements in their context stem from changes to operational efficiency instead of firms undertaking new abatement initiatives. In contrast, we focus on environmental activist campaigns, where long-run investors engage management by posing a credible threat to improve a target's environmental performance.

4 Firm Performance

To better understand the costs and benefits of environmental activism, we next examine the effect of the BAP campaign on firms' financial performance. We note that several competing forces make the impact of environmental activism on financial performance unclear. Investor activism can improve the financial, governance, and operational performance of targeted firms (Nesbitt 1994; Brav et al. 2008). At the same time, pursuing socially responsible investments can be costly (David Diltz 1995; Geczy et al. 2021; Fowlie 2010). Moreover, the margins of adjustment firms undertake may require significant capital and operating expenditures upfront, but the benefits may accrue several years in the future (Starks et al. 2026). Thus, it is unclear whether environmental activism is costly or beneficial to targeted firms and their investors.

Financial performance. First, we study the association between environmental improvements and financial performance of targeted firms. Table 9 presents the results. Estimates from column 1 suggest that targeted firms relative to matched-control firms experience a reduction of 3.3 percentage points in their profitability around the BAP campaign. This effect is economically large and implies that profitability on average reduces to 1.8% relative to an average of 5.1% in the preperiod for control group firms. In column 2, we then study changes in the operating income of targeted firms. Our estimates suggest that the operating income decreases by 12%, on average; however, this estimate is statistically insignificant. Lastly, in column 3, we show that targeted firms experienced a 9-percentage-point relative increase in their operating expenses. Relative to the control firm's preperiod operating expenses, this estimate represents an economically meaningful increase of approximately 21%. Overall, these results suggest a negative relationship between environmental activism and measures of firm performance driven by increases in operating expenses.

Equity response. Next, we aim to understand changes in investor expectations. To do so, we examine the market reaction to proxy-access proposals introduced by the NYCPS, allowing us to trace investor expectations about environmental activism through such interventions.

We measure the abnormal returns around the notice of exempt solicitation, the date most investors were reasonably aware of the proxy-access proposal introduced as part of the activist campaign. Specifically, we test whether there are abnormal returns to treated firms around this event date: the standard asset pricing null, $H_0 : E[\alpha_T] = 0$, where α_T is the intercept from projecting the treated firm's return on factors that span priced risk. In this setting, a factor-adjusted alpha is the sufficient statistic for abnormal performance relative to risk-adjusted benchmarks.

In our tests, we focus on the first wave of targeted firms to cleanly isolate investor expectations. We hand collect key dates from form SEC PX14AG.[31,32] This approach confers two

[31]Figure IA.B1 presents an example of the contents in form PX14AG for the firm Arch Coal Inc., which was targeted by the BAP campaign for environmental reasons.

[32]As a matter of deliberate choice, we do not use the announcement of the BAP campaign as our event to understand investor perception of environmental activism. This is because the campaign announcement included firms

advantages. First, using form PX14AG allows us to determine the date on which investors are likely to learn about proxy-access proposals introduced by the NYCPS and their partners.[33] Second, focusing on the dates extracted from exempt solicitation allows us to precisely evaluate and attribute the investors' response to environmental activism through proxy-access proposals.

We estimate cumulative abnormal returns (CARs) around firm-specific targeting. We calculate returns using three benchmarks: market-adjusted returns, risk-adjusted returns using the Capital Asset Pricing Model (CAPM), and risk-adjusted returns using the Fama-French three-factor model (FF3). We aggregate these daily abnormal returns over the event window to compute CARs, plotting them from 10 days before the event ($t = -10$) through 20 days after ($t = 20$), with the day before the filing ($t = -1$) as the omitted date. We use a large pre- and postwindow to investigate the potential anticipation and persistence of estimates.

Figure 5 presents the returns generated by each model. We note that, prior to the event, all three series remained stable, indicating no notable preannouncement effects. Following the event, however, each model exhibits a gradual and persistent decline in returns over the subsequent 20 days. Table 10 presents the daily abnormal returns around the exempt solicitation filings. Under the market-adjusted model (column 1), the postfiling difference in returns is -0.474%. Adjusting for risk, the CAPM (column 2) and FF3 (column 3) specifications produce similarly negative estimates of -0.485% and -0.431%, respectively. Each of these results is statistically significant at the 1% level.[34]

Collectively, our results suggest a substitution between sustainability improvements and financial performance. Specifically, improvements in corporate environmental behaviors are associated with a reduction in profitability and operating incomes for targeted firms. This decline stems from an increase in operating expenses—including selling, general, and administrative expenses—and the upfront abatement expenditures that targeted firms make in responding to activism. Moreover, consistent with this interpretation, the event-study estimates suggest that

targeted for environmental and nonenvironmental reasons. Additionally, the announcement contained value-relevant information regarding future commitment to issues by other institutional investors. These reasons make it difficult to cleanly isolate investor perceptions about environmental activism from other information in the announcement.

[33]Previous research uses the mailing date or the filing date of the proxy statement (Karpoff et al. 1996; Gillan and Starks 2000). However, these statements may contain other value-relevant information, making it hard to disentangle the effect of activism from other information that the investors become aware of.

[34]In Table IA.D7, we test a related but distinct hypothesis—is there a difference in the abnormal returns between treated and matched control firms around the event date—and find that our conclusions are robust to using this alternative benchmark.

the average investor expects the long-run costs of responding to environmental activism outweigh the benefits. As we discuss next, however, managers viewed the BAP campaign as a credible threat and responded to its specific mandates, reflecting that they perceived costs of inaction to be substantial even though the marginal investor did not perceive these actions as value-enhancing.

5 Mechanisms

This section presents evidence that boards of firms targeted by the BAP campaign improved their corporate environmental behaviors in direct response to the specific demands of the activist investors, a direct channel, rather than responding to externalities of investing or nonenvironmental activist campaigns, an indirect channel.

5.1 Evidence of a direct channel

In this section, we present evidence to support the direct channel view. First, we establish that firms saw the BAP campaign as a credible threat. Second, we show that firms targeted for nonenvironmental reasons do not improve their environmental behaviors, thus alleviating concerns about general attention and, more broadly, that the indirect channel is driving our findings. Lastly, we step outside our baseline sample and consider other prosocial campaigns of the BAP, as well as other environmental campaigns, to show that the firms respond to specific demands of activist investors.

The BAP campaign presented a credible threat. We held discussions with the Comptroller's office to understand the campaign and their interactions with targeted firms. Our conversations reveal that the size, coordination, and the low propensity to divest from targeted firms presented a credible threat that was difficult for boards to ignore. Specifically, the BAP was supported by three of the four largest pension funds in the United States (CalPERS, CalSTRS, and the NYCPS). With over $650 billion in assets under management, this group of investors was pivotal when voting on resolutions.

Importantly, the BAP campaign was well organized. First, the NYCPS and its partners coordinated their engagements and attended the targeted firms' annual general meetings. Second, the NYCPS and CalPERS collaborated on exempt solicitations, in which they sent letters to shareholders asking for their support on proxy-access proposals. Third, the BAP could, in principle, exert pressure through their voting on other proposals unrelated to proxy access. Fourth, the NYCPS, in its fiduciary role, has a low propensity to divest from its portfolio firms, which makes the NYCPS more likely to engage, consistent with Krueger et al. (2020) and Starks and Sautner (2021).[35] This evidence is consistent with research pointing to the ability of public pension funds to coordinate with other shareholders and garner broad support for their proposals (Gillan and Starks (2000); Levit and Malenko (2011)).

We examine empirically, where possible, the success rates of the proxy-access proposals put forward by the BAP. We study the average support level of proxy-access proposals by shareholders and whether proxy access was ultimately implemented at targeted firms. To do so, we combine information from the New York City retirement systems' Shareholder Initiatives Postseason Report and the ISS voting analytics database with hand-collected information from firm disclosures. Examining these outcomes allows us to gauge the credibility of these proposals to pose a threat, especially given their nonbinding nature. As a benchmark, we compare the support to other shareholder proposals at the same firm but not put forward by the NYCPS.

Studying support rates for proxy-access proposals from ISS voting analytics database, we find that the BAP campaign was highly influential in garnering support. As shown in rows 1 and 2 of Table IA.E1, we find that support rates for proxy-access proposals introduced by the BAP campaign are nearly twice as large, and the passage rates are nearly 10 times larger. When comparing shareholder support on proxy-access proposals introduced at firms targeted at environmental reasons (row 3) and those made by other shareholders at these same firms (row 4), we again find similarly high support among shareholders for the proposals introduced by the BAP campaign. Lastly, rows 5 and 6 suggest that the more significant support for the BAP proposals holds even when we compare the outcomes to proxy-access proposals introduced at other firms by other investors.

We also examine whether firms preemptively settled and ultimately adopted proxy access

[35]One exception to this is gun manufacturers. The Assistant Comptroller also highlighted, through discussions, that divestment was difficult due to the fiduciary obligations of the funds. The system hired an external consultant to establish a basis for divesting from this industry to divest from gun manufacturers. Our discussions with the New York City Assistant Comptroller confirm that the NYCPS needs to establish a reasonable basis for divestment in its fiduciary role.

after the BAP campaign. Using data from Shareowner Initiatives Postseason Annual Report, we find that the firms targeted by the BAP exhibited relatively high settlement rates and ultimate adoption rates, suggesting that firms were highly responsive to the demands made. Table IA.E2 row 1 studies all firms targeted by the BAP and finds that a significant fraction of proxy-access proposals settled without a shareholder vote. Further, nearly 90% of the proposals put forward by the BAP were eventually adopted in the company bylaws by the targeted firm. Similarly, in row 2, we find that firms targeted for their inadequate redressal of climate change risk had an even higher propensity to settle immediately and adopt these proposals without much change.

5.2 Ruling out the indirect channels

We further rule out the alternative explanation that firms were responding because of the general attention drawn to them through the BAP campaign, instead of responding to the specific demands put forward by investors. In Appendix E, two tests rule out this indirect channel as the dominant force driving the improvements in environmental behaviors in our setting. First, we show that firms targeted through the BAP for other prosocial mandates—board diversity or excess CEO compensation—specifically respond along these dimensions, but do not change their environmental behaviors. Moreover, we document that targeting for proxy access and governance changes through shareholder proposals is unrelated to changes in corporate environmental behaviors. Second, we step outside of the BAP to test other environmental campaigns and show that firms are improving their corporate environmental behaviors around such campaigns. Taken together, these tests provide robust evidence that firms respond to the specific demands of the activist instead of responding to the attention drawn to them by the campaign.

6 Discussion and Conclusion

This paper documents that environmental activist investing positively affects the sustainability performance of targeted firms. We measure changes using highly detailed plant-chemical-level data and provide robust evidence that firms improve their environmental performance in response to climate-focused engagements. These result in positive externalities for the local economies where plants of targeted firms operate. Ruling out alternative explanations, we show that the results are most consistent with the campaigns posing a credible threat to boards, with investors benefiting from monitoring and the threat of discipline. We document that improvements in sustainability performance are associated with lower financial performance, driven by an increase in new abatement and operational expenditures. Overall, our findings have implications for the current debate regarding stakeholder governance and highlight the role of engagement in addressing climate change risks at firms.

Our results come with important caveats and must be carefully interpreted in the context of our setting. Because the BAP did not target firms randomly, any policy prescription suggesting a random targeting of firms to improve their environmental impact is misguided. Instead, the results should be interpreted as the treatment effect on the treated: would the same changes have occurred if the BAP had selected identical target firms but remained passive in its demands? Further, this study does not lend itself to evaluating the welfare consequences of engagements like the BAP. Such an analysis would need to fully evaluate the nonpecuniary benefits for stakeholders, investors, and broader society. Instead, our study provides robust evidence that environmental activism leads to improvements in corporate environmental behaviors.

In light of these caveats, our results provide novel evidence on engagements for sustainability issues that may help overcome collective action problems, whereby engagements can only succeed if there is enough pressure on management. Our evidence suggests that a lead activist may be a necessary catalyst for a successful climate-focused campaign, and is consistent with a direct channel. Moreover, institutional investors' engagements provide a countervailing force to parts of the market that are difficult to monitor and regulate.

In our setting, environmental activists effectively impose their prosocial preferences on firms (Bénabou and Tirole 2010). In the case of the NYCPS and the BAP, the pension funds' beneficiaries vote for trustees and thus represent their interests and preferences. While serving in a fiduciary role, these funds may also act as preference aggregators for the plans' beneficiaries.

Further, our results directly speak to the research evaluating the importance of proxy-access rules as a means to address climate change risks. Prior studies have investigated the market's reaction to proxy-access rules (Becker et al. 2013; Cohn et al. 2016). More specific to the BAP, studies have evaluated changes in board diversity (Barzuza 2019) and stock price reactions to the announcement of the BAP campaign (Bhandari et al. 2021). Building on this research, our study presents new empirical evidence that proxy-access rules, or the threat thereof, may not be sufficient to affect firms along multiple dimensions. Instead, proxy access gives long-term shareholders the ability to effectively monitor and discipline managers.

Code and Data Availability: The replication code and data are available in the Harvard Dataverse at https://doi.org/10.7910/DVN/IRDH3L

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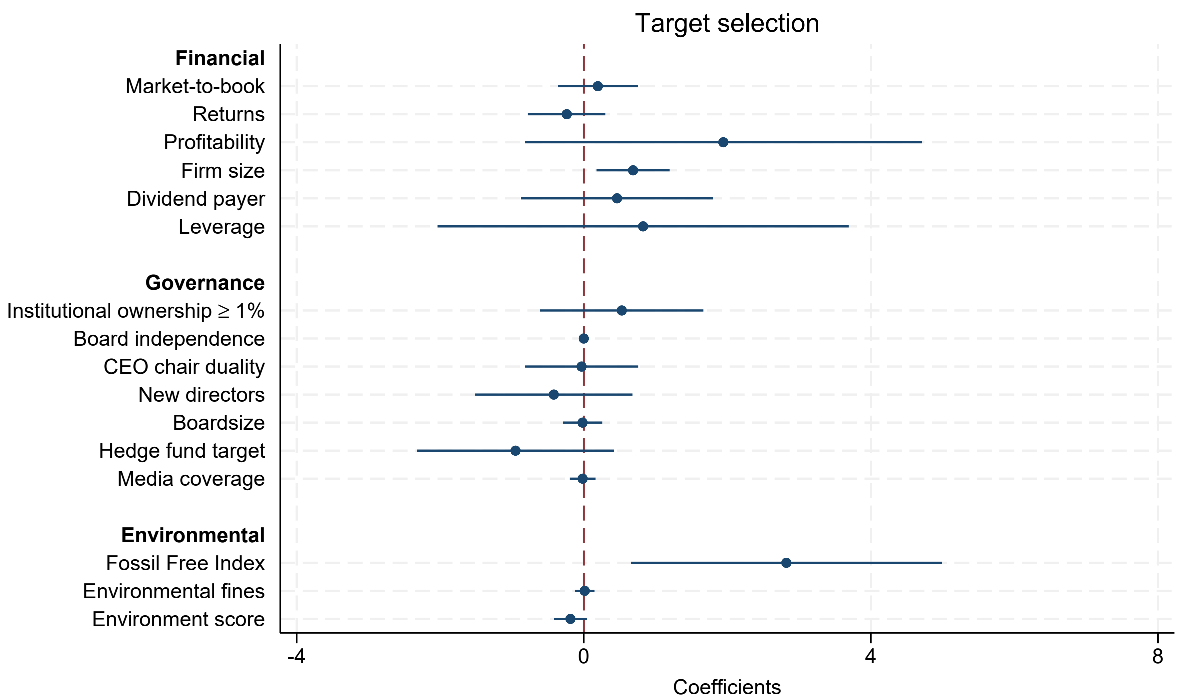
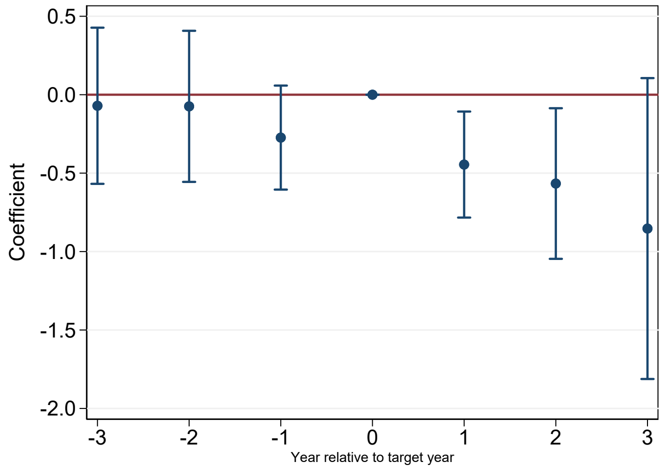
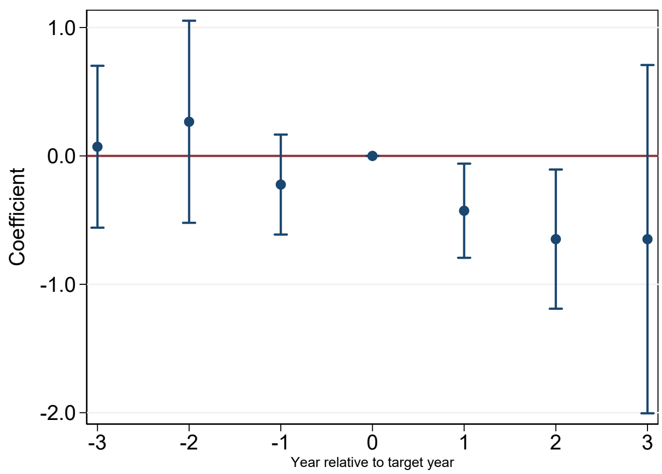


FIGURE 1: SELECTION INTO THE BAP

This figure plots the coefficient estimates and corresponding 95% confidence intervals from a multivariate logistical regression relating firm characteristics that correlate with the likelihood of being targeted by the BAP for environmental reasons. All variables are defined in Table IA.E7. *Sources: Audit Analytics, BoardEx, Compustat, CRSP, RavenPack, and ASSET4.*



(a) Total releases



(b) On-site releases

FIGURE 2: TOXIC CHEMICAL RELEASES AROUND ENVIRONMENTAL ACTIVISM

This figure plots coefficients from a propensity score–matched stacked difference-in-differences specification. In panel A, the dependent variable is the total toxic chemical releases at the plant-chemical-year level scaled by contemporaneous plant-level sales, while in panel B the dependent variable is the on-site toxic chemical releases at the plant-chemical-year level scaled by contemporaneous plant-level sales. The coefficients are estimated using a Poisson pseudolikelihood regression. The horizontal axis is in event time relative to the year of targeting by the BAP campaign. The estimated coefficients and their corresponding 95% confidence intervals, normalized to the year of targeting, correspond to the difference in the toxic chemical releases at plants of targeted firms to the toxic chemical releases at plants of propensity score matched control firms. *Sources: EPA TRI and NETS*.

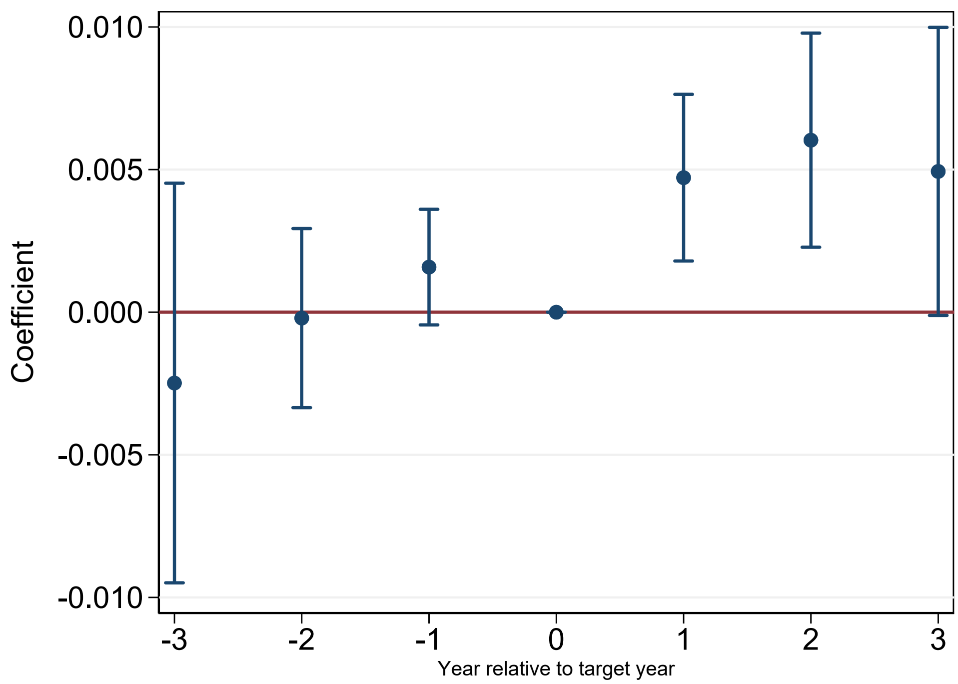
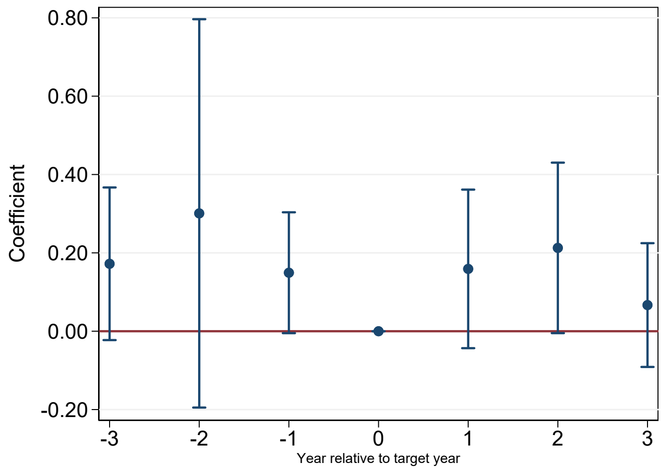
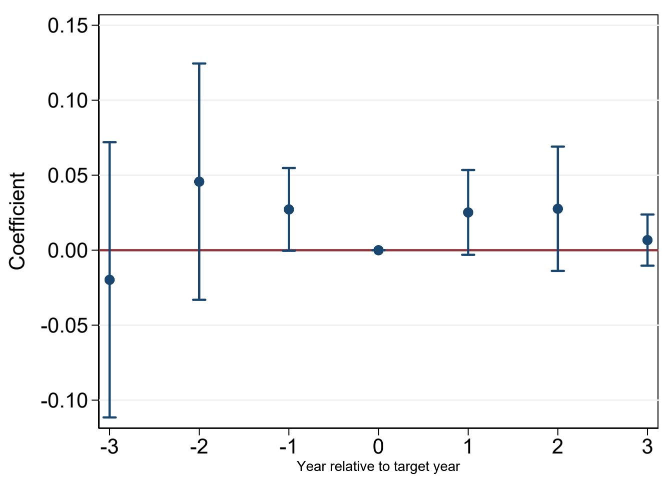


FIGURE 3: LIKELIHOOD OF PREVENTATIVE EFFORTS: ON-SITE RECYCLING

This figure plots coefficients from a propensity score–matched stacked difference-in-differences specification. The dependent variable is an indicator for on-site preventative efforts for recycling at the plant-chemical level. The coefficients are estimated using OLS regression. The horizontal axis is in event time relative to the year of targeting by the BAP campaign. The estimated coefficients and their corresponding 95% confidence intervals, normalized to the year of targeting, correspond to the difference in the toxic chemical releases at plants of targeted firms to the toxic chemical releases at plants of propensity score–matched control firms. *Sources: EPA TRI, P2, and NETS.*



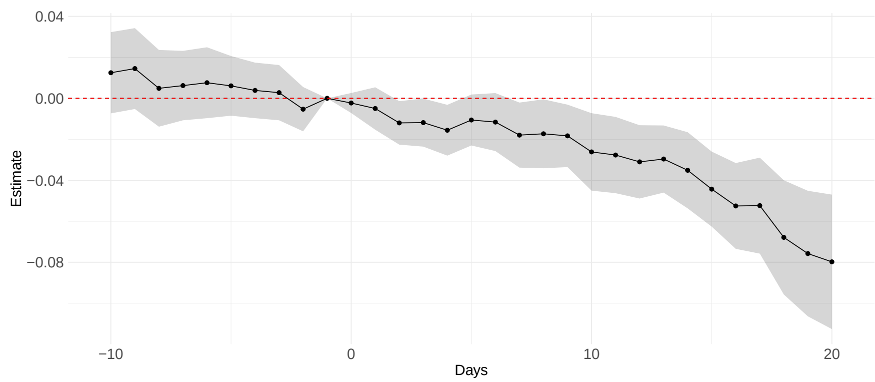
(a) No change in production ratio



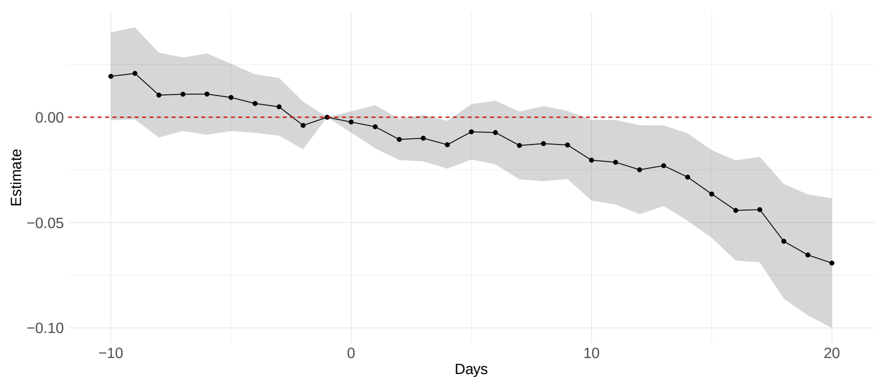
(b) No change in normalized production

FIGURE 4: RULING OUT DECLINE IN PRODUCTION

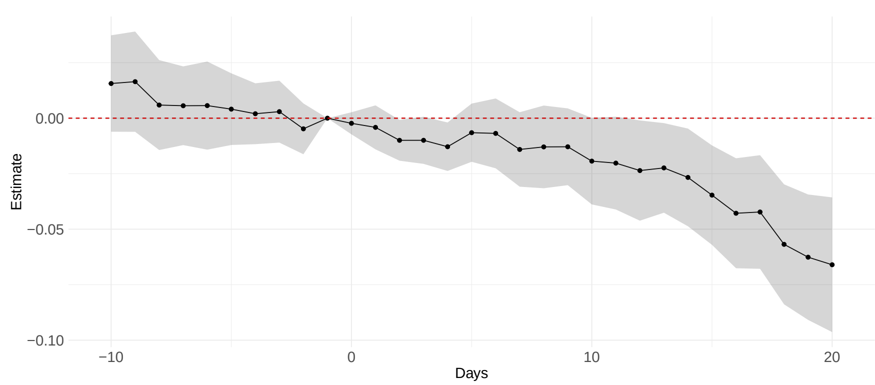
This figure plots coefficients from a propensity score–matched stacked difference-in-differences specification. The dependent variable is the production ratio (panel A) and normalized production (panel B). Production ratio is defined as the quantity of output in any given year relative to the quantity of output in the previous year. Normalized production is a proxy for total production and is defined as the quantity of output in any given year relative to the quantity of output in the first year in our sample. The coefficients are estimated using a Poisson pseudolikelihood regression. The horizontal axis is in event time relative to the year of targeting by the BAP campaign. The estimated coefficients and their corresponding 95% confidence intervals, normalized to the year of targeting, correspond to the difference in the toxic chemical releases at plants of targeted firms to the toxic chemical releases at plants of propensity score–matched control firms. *Sources: EPA TRI and NETS.*



(a) Market-adjusted returns



(b) CAPM



(c) FF3

FIGURE 5: CARS

This figure plots the CARs around the date of exempt solicitation—the date at which investors likely learn about proxy-access proposals introduced by the NYCPS and their partners. CARs are computed using market-adjusted returns (panel A), the CAPM (panel B), and the FF3 (panel C). Error bars represent 95% confidence intervals. *Source: CRSP*.

TABLE 1: SUMMARY OF ACTIVISM EVENTS: FIRMS TARGETED BY THE BAP

This table provides a summary of activism events undertaken by the BAP initiated by the NYCPS. Throughout the paper, we align the date of targeting to the corresponding year of the annual shareholder meeting. Panel A reports the events by shareholder meeting year, while panel B reports the events by industry using the Fama-French 12 industrial classification. We identify activism events through publicly available data from the BAP. Specifically, we identify whether firms are targeted for their inadequate redressal of climate change risk (*Environment*) or if they are targeted for other reasons (*Others*), which include excessive CEO pay, lack of board diversity, inadequate political disclosure, and adherence to labor standards. We use the meeting year a firm is first targeted by the BAP as our event year and adjust for double-counting across categories. Sectors are mutually exclusive and defined using the Fama-French 12 industry classification from SIC codes.

	A. By shareholder meeting year		
First year of event	# of Firms	Environment	Others
2015	75	33	42
2016	26	7	19
2017	57	20	37
2018	23	2	21
Full sample	181	62	119

	B. By industry		
	# of Firms	Environment	Others
Consumer Nondurables	3	0	3
Consumer Durables	5	1	4
Manufacturing	7	0	7
Energy	30	27	3
Chemicals and Allied Products	6	0	6
High Tech	26	2	24
Tele and Communications	3	0	3
Utilities	25	22	3
Wholesale and Retail	19	5	14
Healthcare, Medical Equipment, and Drugs	10	0	10
Finance	32	1	31
Other	15	4	11
Full Sample	181	62	119

TABLE 2: SUMMARY STATISTICS

The table presents the descriptive statistics for the sample. Panel A reports the key statistics at the plant-chemical level, while panel B reports the distribution of variables at the plant level. Panel C reports the distribution of the variables from EPA's RSEI Geographic Microdata aggregated at the census block group level, and panel D presents the distribution of variables at the firm level. The sample spans 2012 to 2020, and the distributions are reported for the final sample on which estimations were performed. All variables are defined in Internet Appendix Table IA.E7.

	N	Mean	SD	P10	P25	P50	P75
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				A. Plant-chemical level			
s (in pounds)	27,951	40,603	145,102	0.10	40	1345	21,066
ses (in pounds)	27,951	39,281	202,687	0.00	16.8	634	13,543
s US$)	27951	5810.84	42728.79	0.00	0.24	10.56	237.19
es US$)	27,951	3856.03	28882.15	0.00	0.10	5.45	156.00
100)	27,951	3.0	17.1	0.00	0.00	0.00	0.00
$_y$ (× 100)	27,951	2.36	15.17	0.00	0.00	0.00	0.00
$_{sposal}$ (× 100)	27,951	36.8	48.25	0.00	0.00	0.00	100
$_{cations}$	27,951	0.008	0.152	0.00	0.00	0.00	0.00
$_{tices\ \&\ training}$	27,951	0.030	0.304	0.00	0.00	0.00	0.00
atio	25,546	1.39	45.97	0.77	0.91	1.00	1.08
production	26,780	6.85	109.53	1.71	3.00	4.97	6.96
				B. Plant-level			
lion US$)	1,889	315.85	938.55	3.36	14.87	52.44	178.78
xpenditures (in million US$)	571	25.37	59.44	0.15	1.24	8.92	28.22
nditures (in million US$)	571	16.21	56.03	0.00	0.00	1.69	10.04
nd maintenance (in million US$)	571	9.15	13.51	0.07	0.80	4.02	13.59
				C. Regional level			
$/m^3$)	30,621	9.36	6.28	3.50	5.32	8.00	11.79
oxide (in parts per million)	15,169	0.35	0.21	0.15	0.20	0.30	0.43
$_{micals}$	2,231	72.57	56.54	22.00	36.00	57.00	90.00
$_{ases}$	2,231	585.66	885.42	62.00	114.00	271.00	634.00
tration	2,231	17518.66	57181.28	28.39	339.87	1793.61	9277.89
	2,231	5265.60	19510.50	5.64	79.15	481.88	2053.57
				D. Firm level			
	572	0.02	0.10	-0.04	0.01	0.03	0.06
come (in million US$)	572	3287	6272	425	709	1573	3730
xpenses	572	0.42	0.49	0.10	0.15	0.21	0.51

TABLE 3: REAL EFFECTS OF ENVIRONMENTAL ACTIVISM

This table reports the impact of environmental activism on toxic chemical releases. The regressions compare toxic chemical releases at plants of targeted firms to toxic chemical releases at plants of Russell 3000 Index firms in a propensity score–matched stacked difference-in-differences empirical setup. The dependent variable is the total toxic chemical releases at the plant-chemical level scaled by contemporaneous plant-level sales reported in NETS. *Post* is an indicator variable equal to one for all years after the activism event year (pseudoevent year) and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons and zero otherwise. All regressions include *Plant* × *chemical*, *chemical* × *time*, and *state* × *time* fixed effects. We allow these fixed effects to vary by cohort and within each pair of treated and control firm. Given the count-like nature of the dependent variable, we estimate a fixed effects Poisson model. The sample includes 473 plants of 45 firms. Standard errors are clustered at the firm level and are robust to heteroscedasticity. **p* <.1; ***p* <.05; ****p* <.01. All variables are defined in Internet Appendix Table IA.E7. *Sources: EPA TRI and NETS*.

Dependent variable:	$\frac{\text{Releases}}{\text{Plant sales}}$	
	Total	On-site
	(1)	(2)
Post × environment	-0.162***	-0.078***
	(0.054)	(0.010)
Fixed effects:		
Plant × chemical × cohort-pair	Yes	Yes
Chemical × time × cohort-pair	Yes	Yes
State × time × cohort-pair	Yes	Yes
Pseudo-R^2	0.59	0.55
Observations	24,248	23,583

TABLE 4: AIR QUALITY MEASURED BY MONITORING STATIONS

This table reports the impact of environmental activism on air quality within a 2-mile radius of the plants of the targeted firms as measured by the EPA's air monitoring stations in a stacked difference-in-differences specification. The dependent variable is the daily average measurement of criteria gases from the monitors closest to each plant: particulate matter (column 1) and carbon monoxide (column 2). *Environment* is a dummy variable indicating whether the plant belongs to the firm that was targeted by the BAP for environmental reasons. *Post* is an indicator variable equal to one for all years after the activism event year (pseudoevent year) and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons and zero otherwise. All specifications include *Monitor* and *State* $\times$ *time* fixed effects. All models are estimated using OLS. The sample includes 93 air quality monitors. Standard errors are clustered at the firm level and are robust to heteroscedasticity. **p <.1; **p <.05; ***p <.01. Source: EPA AQS Database*.

Dependent variable:	Daily average measurement	
Emissions of	Particulate matter $< 2.5\ \mu$ m	Carbon monoxide
	(1)	(2)
Post $\times$ environment	-2.911***	-0.172***
	(0.021)	(0.029)
Measurement units	$\mu g/m^3$	Parts per million
Fixed effects:		
Monitor $\times$ cohort-pair	Yes	Yes
State $\times$ time	Yes	Yes
Adjusted-R^2	0.17	0.27
Observations	30,621	15,169

TABLE 5: REDUCTION IN LOCAL POPULATIONS' EXPOSURE TO TOXICITY

This table reports the impact of environmental activism on the local populations' exposure to the toxicity of the chemical releases at census blocks of plants of the targeted firms to the census blocks of Russell 3000 Index firms in a stacked difference-in-differences specification. The dependent variables are the number of chemicals (column 1), number of releases (column 2), the natural logarithm of toxic concentration (column 3), and the natural logarithm of the RSEI-modeled cancer score (column 4). We scale the dependent variables by the population of the respective census block groups, except for column 4, which already accounts for population exposure in the computation of the score. The RSEI score is a model-implied risk-weighted measure of all chemicals and emissions released, as provided by the EPA. *Post* is an indicator variable equal to one for all years including and after the activism event year (pseudoevent year) and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons and zero otherwise. All regressions include *Census block* and *State* $\times$ *time*. Specifications (1) and (2) are estimated using a Poisson pseudolikelihood regression, while other columns are estimated using OLS. The sample includes 433 census block groups. Standard errors are clustered at the firm level and are robust to heteroscedasticity. **p <.1; **p <.05; ***p <.01. Source: EPA RSEI Model.*

Dependent variable:	No. of chemicals	No. of releases	Toxic concentration	Cancer score
	(1)	(2)	(3)	(4)
Post $\times$ environment	-0.024***	-0.068***	-0.121	-0.126
	(0.003)	(0.005)	(0.126)	(0.141)
Fixed effects:				
Census block $\times$ cohort-pair	Yes	Yes	Yes	Yes
State $\times$ time	Yes	Yes	Yes	No
Pseudo-R^2	0.83	0.90		
Adjusted-R^2			0.95	0.94
Observations	2,164	2,164	2,164	2,148

TABLE 6: SOURCE REDUCTION AND PREVENTATIVE EFFORTS

This table reports the source reduction and preventative efforts undertaken by firms in responding to environmental activism. The regressions compare outcomes at plants of targeted firms to outcomes at plants of Russell 3000 Index firms in a propensity score–matched stacked difference-in-differences empirical setup. In panel A, the dependent variable is the cumulative number of new source reduction activities implemented in whole or in part during the year at the plant-chemical level, while in panel B it is an indicator for other on-site preventative efforts—recycling, energy recovery, and treatment at the plant-chemical level. *Post* is an indicator variable equal to one for all years after the activism event year (pseudoevent year) and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons and zero otherwise. All regressions include *Plant* $\times$ *chemical*, *chemical* $\times$ *time* fixed effects, and *state* $\times$ *time* fixed effects. We allow these fixed effects to vary by cohort and within each pair of treated and control firms. We estimate a fixed effects Poisson model given the count-like nature of the dependent variable in panel A, while in panel B, given the indicator variable, we estimate an OLS model. We multiply the coefficients by 100 in panel B for ease of interpretation. The sample includes 473 plants of 45 firms. Standard errors are clustered at the firm level and are robust to heteroscedasticity. **p <.1; **p <.05; ***p <.01*. All variables are defined in Internet Appendix Table IA.E7. *Sources: EPA TRI, P2, and NETS*.

	A. Source reduction	
Dependent variable:	$\#_{Initiatives}$	
Initiative:	Process & equipment modification	Operating practices & training
	(1)	(2)
Post $\times$ environment	0.001***	0.005***
	(0.000)	(0.000)
Fixed effects:		
Plant $\times$ chemical $\times$ cohort-pair	Yes	Yes
Chemical $\times$ time $\times$ cohort-pair	Yes	Yes
State $\times$ time $\times$ cohort-pair	Yes	Yes
Pseudo-R^2	0.90	0.90
Observations	25,642	25,642

	B. Preventative efforts		
Dependent variable:	$\mathbb{1}_{Initiative}$ ($\times$ 100)		
Initiative:	Recycling	Energy recovery	Treatment & disposal
	(1)	(2)	(3)
Post $\times$ environment	0.186***	0.133**	7.068***
	(0.055)	(0.051)	(0.726)
Fixed effects:			
Plant $\times$ chemical $\times$ cohort-pair	Yes	Yes	Yes
Chemical $\times$ time $\times$ cohort-pair	Yes	Yes	Yes
State $\times$ time $\times$ cohort-pair	Yes	Yes	Yes
Adjusted-R^2	0.90	0.88	0.90
Observations	25,638	25,638	25,638

TABLE 7: ABATEMENT EXPENDITURES

This table reports abatement expenditures for plants of targeted and matched control firms around the environmental activism campaign. The regressions compare expenditures as a fraction of plant sales as reported in the NETS of targeted firms to toxic chemical releases at plants of Russell 3000 Index firms in a propensity score–matched stacked difference-in-differences empirical setup. *Post* is an indicator variable equal to one for all years after the activism event year (pseudoevent year) and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons and zero otherwise. All regressions include *Plant* and *EPA region* × *time* fixed effects. We also include plant-level controls that correlate with expenditures, such as *Plant age* and *Installed capacity*. We allow these fixed effects to vary by cohort and within each pair of treated and control firms. Given the count-like nature of the dependent variable, we estimate a fixed effects Poisson model. Standard errors are clustered at the firm level and are robust to heteroscedasticity. **p* <.1; ***p* <.05; ****p* <.01. All variables are defined in Internet Appendix Table IA.E7. *Sources: EIA and NETS*.

Dependent variable:	$\frac{\text{Expenditures}}{\text{Plant sales}}$		
Expenditure:	Total	Capital	Operations & maintenance
	(1)	(2)	(3)
Post × environment	0.825***	0.775***	-0.366***
	(0.022)	(0.059)	(0.038)
Fixed effects:			
Plant-level controls	Yes	Yes	Yes
Plant × cohort-pair	Yes	Yes	Yes
EPA region × time × cohort-pair	Yes	Yes	Yes
Pseudo-R^2	0.96	0.95	0.93
Observations	571	449	571

TABLE 8: RULING OUT DECLINE IN PRODUCTION

This table examines changes in production around the targeting by the BAP. The regressions compare the growth (level) of production at plants of targeted firms to the growth (level) of production at plants of Russell 3000 Index firms in a propensity score–matched stacked difference-in-differences empirical setup. The dependent variables are the production ratio (column 1) and normalized production (column 2). Production ratio is defined as the quantity of output in any given year relative to the quantity of output in the previous year. Normalized production is a proxy for total production and is defined as the quantity of output in any given year relative to the quantity of output in the first year in our sample. *Post* is an indicator variable equal to one for all years after the activism event year (pseudoevent year) and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons and zero otherwise. All regressions include *Plant* $\times$ *Chemical*, *Chemical* $\times$ *time* fixed effects, and *state* $\times$ *time* fixed effects. We allow these fixed effects to vary by cohort and within each pair of treated and control firms. Given the count-like nature of the dependent variable, we estimate a fixed effects Poisson model. The sample includes 473 plants of 45 firms. Standard errors are clustered at the firm level and are robust to heteroscedasticity. **p <.1; **p <.05; ***p <.01.* All variables are defined in Internet Appendix Table IA.E7. *Sources: EPA TRI and NETS*.

Dependent variable:	Production ratio	Normalized production
	(1)	(2)
Post $\times$ environment	0.103	0.011
	(0.082)	(0.038)
Fixed effects:		
Plant $\times$ chemical $\times$ cohort-pair	Yes	Yes
Chemical $\times$ time $\times$ cohort-pair	Yes	Yes
State $\times$ time $\times$ cohort-pair	Yes	Yes
Pseudo-R^2	0.72	0.88
Observations	23,383	24,192

TABLE 9: ENVIRONMENTAL ACTIVISM AND FIRM PERFORMANCE

This table reports the impact of environmental activism on firms' financial performance. The regressions report the change in firm performance of targeted firms around environmental activism by the BAP. The dependent variable in column 1 is *Profitability*, defined as net income scaled by lagged total assets (AT), while in column 2 the dependent variable is *log(operating) income)*, defined as the natural logarithm of earnings before interest, taxes, depreciation, and amortization (OIBDP). To capture costs, column 3 reports changes to *Operating expenses*, defined as operating expenses, which is sum of cost of goods sold and selling, general, and administrative expenses scaled by lagged total assets (AT). *Post* is an indicator variable equal to one for all years after the activism event year (pseudoevent year) and zero otherwise. All regressions include firm and time fixed effects. We allow these fixed effects to vary by cohort and within each pair of treated and control firm. The regressions are estimated using OLS using standard errors clustered at the firm level. **p <.1; **p <.05; ***p <.01.* All variables are defined in Internet Appendix Table IA.E7. *Sources: Compustat and CRSP*.

Dependent variable:	Profitability	log(operating income)	Operating expenses
	(1)	(2)	(3)
Post $\times$ environment	-0.033**	-0.120	0.091*
	(0.014)	(0.105)	(0.049)
Fixed effects:			
Firm$\times$ cohort-pair	Yes	Yes	Yes
Cohort-pair $\times$ time	Yes	Yes	Yes
Adjusted-R^2	0.14	0.83	0.86
Observations	572	572	572

TABLE 10: STOCK RETURNS OF TARGETED FIRMS AROUND THE BAP CAMPAIGN

This table examines the impact of environmental activism on firms' equity returns. We report results comparing the daily market-adjusted and abnormal equity returns (in percent) of targeted firms 10 days before to 20 days after the NYCPS filed exempt solicitation at target firms. We use market-adjusted returns (column 1), the CAPM (column 2), and the FF3 (column 3). *Post* is an indicator variable for the period of 20 days after the investor filing. All regressions are estimated using OLS. The sample includes 30 firms that were targeted by the BAP in the first year. Standard errors are corrected for heteroscedasticity and dependence across firms. **p <.1; **p <.05; ***p <.01. Sources: CRSP and SEC Filings.*

Dependent variable	Market-adj. return	CAPM	FF3
	(1)	(2)	(3)
Post	−0.480***	−0.490***	−0.440***
	(0.110)	(0.114)	(0.132)
Fixed effects:			
Firm	Yes	Yes	Yes
Adjusted-R^2	0.017	0.016	0.012
Observations	930	930	930

The Real Effects of Environmental Activist Investing

APPENDIX

FOR ONLINE PUBLICATION

A BAP and Proxy Access

Institutional investors have long called for the ability to nominate directors to the corporate ballot—proxy access—but have historically faced opposition (Funds (2015)). The Securities and Exchange Commission (SEC) approved a universal proxy access rule (Exchange Act Rule 14a-11) in August 2010. This rule was to allow shareholders holding greater than 3% of a firm's outstanding shares for more than three years, collectively, to nominate board candidates via the firm's proxy materials.

In 2011, the US Court of Appeals overturned this rule, saying that the SEC had acted "arbitrarily and capriciously." Although the Court of Appeals vacated this new rule on procedural grounds, it did not vacate a provision within the Dodd-Frank Financial Reform Act, enabling shareholders to start requesting proxy access mechanisms through shareholder resolutions (Holly Gregory and Holland (2019)).

Against this backdrop, many institutional investors did not resort to proxy access as a means of external governance and instead preferred voting to raise their concerns. Before 2015, only 15 US companies had adopted proxy access. This number saw a significant increase after 2015, when the NYC Comptroller and the NYCPS launched their Boardroom Accountability Project (BAP), which targeted a set of firms in their portfolio. By the end of 2018, more than 150 targeted firms had adopted a proxy access bylaw. As of 2019, most S&P 500 companies had adopted them too, some voluntarily, and others as a result of shareholder resolutions.

Starting in 2014, the Boardroom Accountability Project was led by the NYCPS. The BAP brought together influential investors, including California Public Employees' Retirement System (CalPERS) and California Teachers' Retirement System (CalSTRS), with total assets under management exceeding $650 billion. The proxy access proposals submitted by the Comptroller closely mirrored the rules previously enacted by the SEC.

The BAP submitted proxy access proposals requesting new bylaws that permitted shareholders who collectively held 3 percent of the company for at least three years to nominate up to 25 percent of the board using the company's proxy materials. Using these proxy access proposals over several years, the BAP pursued very specific mandates, and we focus on firms that were targeted by the BAP to reduce their environmental impact.

B Exempt Solicitation and SEC Rule 14a-8 Decision Letters

CalPERS

SCOTT M. STRINGER
COMPTROLLER
THE CITY OF NEW YORK

April 17, 2015

Dear Fellow Arch Coal Inc. Shareowner:

Support Shareowner Proposal #5 Requesting Proxy Access

The California Public Employees Retirement System (CalPERS) and the New York City Pension Funds urge shareowners to vote "FOR" proposal #5 – a non-binding proposal to give shareowners effective access to the director nomination process – at the Arch Coal Inc. annual meeting on April 23, 2015. Collectively, our funds have $468 billion in assets and are substantial long-term Arch Coal Inc. shareowners, with approximately 2,860,512 shares.

Proxy Access a Fundamental Shareowner Right

Providing access to a company's proxy to permit shareowners the ability to nominate directors to the board is one of the most important rights given to the owners of a company. Without effective proxy access, the director election process simply becomes a ratification of corporate management's slate of nominees. Therefore, we believe long-term shareowners should have meaningful access to this process on the terms specified in the proposal #5. The proposed thresholds include:

- Beneficial ownership of at least 3 percent of the outstanding stock;
- Three years of continuous ownership; and
- Ability to nominate up to 25 percent of the board.

Proposed Terms Based on Thoughtful SEC Proxy Access Rulemaking in 2010

Before adopting its now vacated proxy access rule in 2010, the SEC undertook careful analysis, both internally and through the public comment process. It was the SEC that determined that a 3 percent ownership threshold was appropriate, stating, "The ownership threshold we are establishing should not expose issuers to excessively frequent and costly election contests conducted through use of Rule 14a11, but it is also not so high as to make use of the rule unduly inaccessible as a practical matter." The proposed holding period and slate size limitation are also consistent with the SEC rule.

CFA Institute Concludes Proxy Access Would Benefit the Markets and Boardroom

A 2014 report published by the CFA Institute found that giving investors access to the proxy to nominate directors would benefit shareowners and the capital markets.[1] The specific report findings include:

- Proxy access has the potential to enhance board performance and raise US market capitalization by between $3.5 billion and $140.3 billion.
- Proxy access reform will not hinder board performance.

[1] http://www.cfapubs.org/doi/pdf/10.2469/ccb.v2014.n9.1

FIGURE IA.B1: EXEMPT SOLICITATION LETTER

This figure presents the exempt solicitation letter sent by the New York City Pension System (NYPCS) in collaboration with the California Public Employees Retirement System (CalPERS) to the shareholders of Arch Coal Inc., a firm targeted by the BAP campaign for environmental reasons. These letters were sent to garner support for the proxy-access proposal submitted as part of the BAP campaign. Source: SEC.

ArchCoal

Robert G. Jones
Senior Vice President
Law & General Counsel

Bjones@archcoal.com

December 23, 2014

Office of Chief Counsel
Division of Corporation Finance
Securities and Exchange Commission
100 F Street, N.E.
Washington, D.C. 20549

Re: Securities Exchange Act of 1934 – Section 14(a), Rule 14a-8; Omission of Stockholder Proposal

Ladies and Gentlemen:

I am writing on behalf of Arch Coal, Inc. ("Arch") to inform you, pursuant to Rule 14a-8(j) under the Securities Exchange Act of 1934, as amended (the "Exchange Act"), that Arch intends to omit from its proxy solicitation materials for its 2015 annual meeting of stockholders a stockholder proposal (the "Proponent's Proposal") submitted by the Comptroller of the City of New York, The Honorable Scott M. Stringer, as the custodian and a trustee of the New York City Employee's Retirement System, the New York City Teachers' Retirement System and the New York City Police Pension Fund and custodian of the New York City Board of Education Retirement System (the "Proponent"). In accordance with Rule 14a-8(j), Arch hereby respectfully requests that the staff (the "Staff") of the Division of Corporation Finance of the Securities and Exchange Commission (the "Commission") confirm that it will not recommend enforcement action against Arch if the Proponent's Proposal is omitted from Arch's proxy solicitation materials for its 2015 annual meeting of stockholders (the "2015 Annual Meeting") in reliance on Rule 14a-8(i)(9) under the Exchange Act. Copies of the Proponent's Proposal and accompanying materials are attached as Exhibit A.

Arch expects to file its proxy solicitation materials for the 2015 Annual Meeting on or about March 16, 2015. Accordingly, as contemplated by Rule 14a-8(j), this letter is being filed with the Commission no later than eighty (80) calendar days before the date upon which Arch expects to file the definitive 2015 proxy solicitation materials.

Pursuant to Staff Legal Bulletin No. 14D ("SLB 14D"), I am submitting this request for no-action relief to the Commission under Rule 14a-8 by use of the Commission's email address, stockholderproposals@sec.gov, and have included my name and telephone number both in this letter and the cover email accompanying this letter. In accordance with the Staff's instruction in Section E of SLB 14D, I simultaneously am forwarding by email or facsimile a copy of this letter to the Proponent. The Proponent is requested to copy the undersigned on any response it may choose to make to the Staff.

Arch Coal, Inc.
One CityPlace Drive, Suite 300
St. Louis, Missouri 63141

direct: 314.994.2716
fax: 314.994.2734

archcoal.com

FIGURE IA.B2: MANAGEMENT OPPOSITION TO PROXY-ACCESS PROPOSAL

This figure presents a letter written by the management of Arch Coal Inc. to the SEC requesting the omission of proxy-access proposal from the proxy material for the 2015 annual meeting. This letter was extracted based on the request submitted by Arch Coal Inc. to SEC under rule 14a-8(i)(9). Source: SEC.

UNITED STATES
SECURITIES AND EXCHANGE COMMISSION
WASHINGTON, D.C. 20549

DIVISION OF
CORPORATION FINANCE

February 2, 2015

Robert G. Jones
Arch Coal, Inc.
bjones@archcoal.com

Re: Arch Coal, Inc.
Incoming letter dated December 23, 2014

Dear Mr. Jones:

This is in response to your letter dated December 23, 2014 concerning the shareholder proposal submitted to Arch by the New York City Employees' Retirement System, the New York City Fire Department Pension Fund, the New York City Teachers' Retirement System, the New York City Police Pension Fund and the New York City Board of Education Retirement System. Pursuant to rule 14a-8(j) under the Securities Exchange Act of 1934, your letter indicated Arch's intention to exclude the proposal from Arch's proxy materials solely under rule 14a-8(i)(9).

On January 16, 2015, Chair White directed the Division to review the rule 14a-8(i)(9) basis for exclusion. The Division subsequently announced, on January 16, 2015, that in light of this direction the Division would not express any views under rule 14a-8(i)(9) for the current proxy season. Accordingly, we express no view on whether Arch may exclude the proposal under rule 14a-8(i)(9).

Copies of all of the correspondence related to this matter will be made available on our website at http://www.sec.gov/divisions/corpfin/cf-noaction/14a-8.shtml. For your reference, a brief discussion of the Division's informal procedures regarding shareholder proposals is also available at the same website address.

Sincerely,

Matt S. McNair
Special Counsel

cc: Michael Garland
The City of New York
Office of the Comptroller
mgarlan@comptroller.nyc.gov

FIGURE IA.B3: SEC RULE 14A-8 DECISION LETTER

This figure presents the SEC decision letter in response to Arch Coal Inc. request to review the exclusion of proxy-access proposal submitted as part of the BAP campaign from the proxy material for the 2015 annual meeting under rule 14a-8(i)(9). Source: SEC.

C Data Appendix

Matching. The starting point for our analysis is to manually hand-match 181 firms targeted by the BAP campaign (see Table 1) to firms in Compustat. For our baseline tests, we focus on 62 firms that were targeted for environmental reasons. We use the RUSSELL 3000 firms as our candidate counterfactual firms, based on our conversations with the NYCPS. Note that we eliminate the firms that were targeted by the BAP campaign for non-environmental reasons. We subsequently merge ASSET4 scores for all firms using CUSIPs provided by the data vendor. Together, this allows us to observe the financial and sustainability characteristics of these firms. Using these characteristics, we construct a propensity score matched sample where we match each targeted firm to the closest control firm. Below, we report the table showing the differences between firms that were targeted for environmental reasons and matched control firms. We find that the matched pair is statistically indistinguishable along important financial characteristics except for firm size.

	Control firms	Targeted firms	Difference (means)	*p*-value
	(1)	(2)	(2) - (1)	(4)
Log (assets)	9.317	10.009	0.693	<0.01
$\frac{\text{Operating income}}{\text{Assets}}$	0.054	0.062	0.009	0.71
Market-to-book	0.892	1.005	0.113	0.26
Environment score	-0.000	-0.000	0.000	0.27

National Establishment Time-Series. Next, we match the sample of propensity score matched firms to NETS-TRI data using a multi-step procedure to obtain plant-level sales that we use to scale our pollution measures. Using company names reported in Compustat, we use the key identifying information to match—address and the five-digit ZIP code. We retain the headquarters firm identifier in the NETS database and ensure an exact match on both address and ZIP code to correctly identify the parent firm. Finally, we map changes in ownership for plants in the sample to track changes in corporate structure over time. We retain plant-year observations with complete sales information for the combined data.

Toxics Release Inventory. Once we have the link between firm identifier in Compustat and plant identifiers in the NETS-TRI database, we can merge the resulting sample at the plant-level to EPA's TRI database. For each establishment, NETS provides a crosswalk. We use the TRI Basic Plus Data files, which collectively contain all of the data reported on the TRI Reporting Form R and Form A Certification Statement. The resulting sample is at the plant-chemical-year level. The TRI monitors how specific toxic chemicals, which poses a threat to human health and the environment. Facilities across various US industries are required to report annually on the quantity of each chemical released into the environment and managed through recycling, energy recovery, and treatment. They must also disclose any measures taken to prevent or minimize chemical waste generation, which we use in tests to understand source reduction and preventative efforts undertaken by plants.

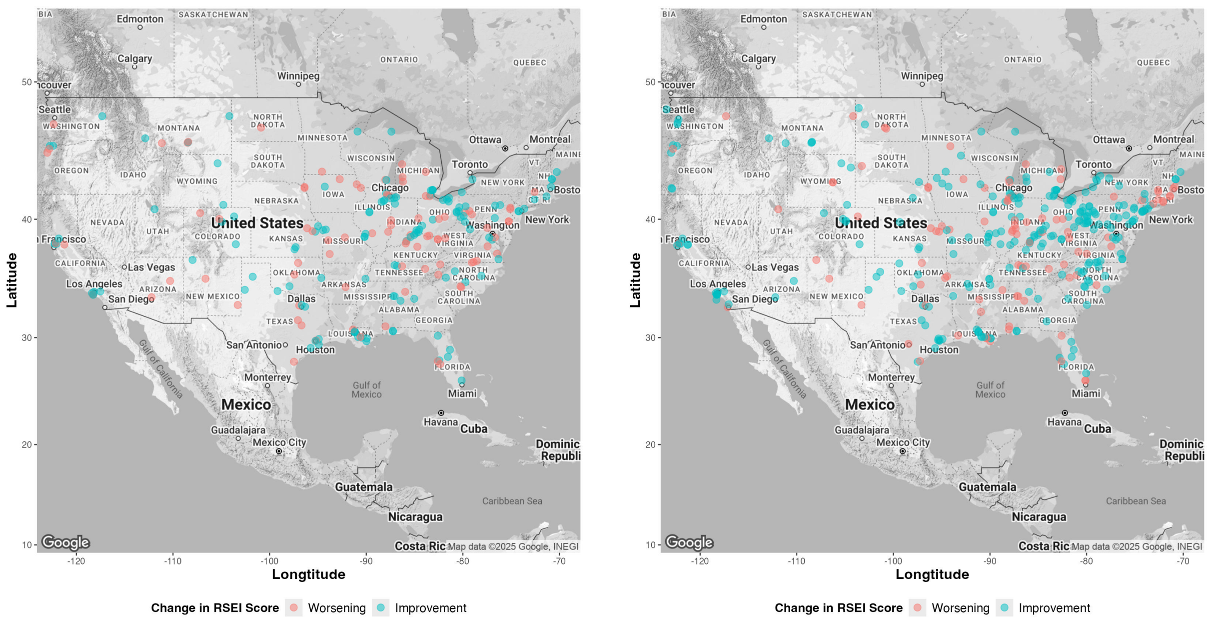
Air Quality Monitoring System. We merge datasets from the EPA's Air Quality System (AQS) and the IMPROVE (Interagency Monitoring of Protected Visual Environments) monitor networks with geolocation data for plants in our final merged sample. For each plant, we identify the air quality monitors located within five miles of each plant in the following way. First, we clean and standardize site information by removing non-continental entries, fixing coordinate systems (e.g., NAD83, WGS84), and generating unique site codes for each monitoring station. We then compute pairwise distances between each plant and each monitor, record the minimum distance for every plant, and filter the resulting dataset to include only plant-monitor pairs within a five-mile radius.

Risk-Screening Environmental Indicators. We use the geolocation information of plants in our sample, which includes latitude and longitude information from TRI data. Using this data, we identify the corresponding FIPS code for each plant to determine its location within specific geographic boundaries. We then incorporate aggregated block-level RSEI microdata from the repository available at RSEI-GM, covering the years 2009 through 2020.

Energy Information Administration. We rely of data collected by EIA on utility plants. The Form EIA-860 collects generator-level specific information about existing and planned generators and associated environmental equipment at electric power plants with 1 megawatt or greater of combined nameplate capacity. The Form EIA-923 collects detailed electric power data—monthly and annually—on electricity generation, fuel consumption, fossil fuel stocks, and receipts at the power plant and prime mover level. In the reported forms, we also observe abatement expenditures; we observe i) all pollution abatement capital expenditures are for new structures and/or equipment and ii) all operation and maintenance expenditures, excluding depreciation expense, cost of electricity consumed, and fuel differential expenses.

Pollution Prevention. The EPA's Pollution Prevention (P2) database serves as a comprehensive repository documenting pollution prevention activities undertaken by plants across various industries. It compiles data on strategies implemented by companies to minimize waste generation and reduce environmental impact. These efforts include process modifications, substitution of hazardous materials with safer alternatives, and the implementation of recycling programs. During our sample period, the EPA categorized the steps into: i) Process and Equipment Modifications, ii) Operating Practices and Training, iii) Inventory and Material Management, iv) Material Substitutions and Modifications, and v) Product Modifications.

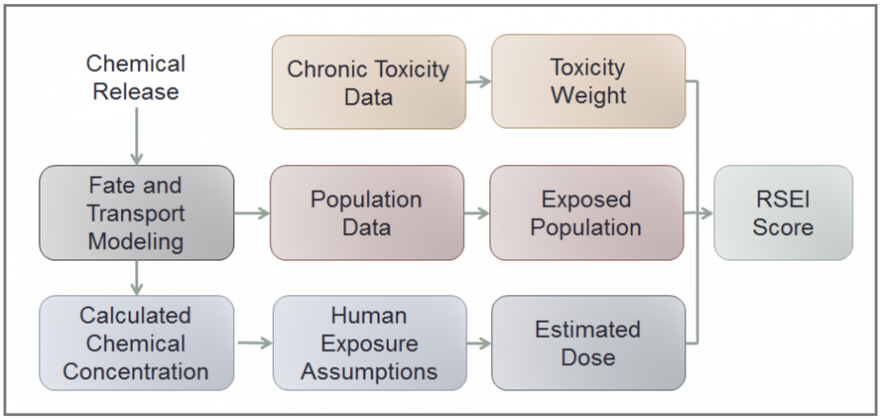
D Supplemental Figures and Tables



(a) Pre-period (b) Targeting-period

FIGURE IA.D1: CHANGES IN RISK-SCREENING ENVIRONMENTAL INDICATORS (RSEI) SCORES

This figure plots the percentage change in RSEI scores at the local census block group level that contain at least one plant of the (i) target firm, or (ii) propensity score matched control firms in the sample. Panel (a) shows changes through the pre-period (2012–2014), and panel (b) shows changes through the targeting-period (2017–2020). The blue circle represents a reduction (improvement) in the RSEI score, whereas the red circle represents an increase (worsening) in the RSEI score. Data Source: EPA Risk-Screening Environmental Indicators (RSEI) Model.



(a) Risk-Screening Environmental Indicators (RSEI) Model

(b) Waste Management Hierarchy

FIGURE IA.D2: ENVIRONMENTAL CONCEPTS FROM THE EPA

This figure presents two key environmental concepts used in this paper. Panel A describes the Risk-Screening Environmental Indicators (RSEI) Model computation methodology, as described by the EPA (2019a). A unitless value, the RSEI score accounts for (i) the size of the chemical release, (ii) the fate and transport of the chemical through the environment, and (iii) the size and location of the exposed population, and the chemical's toxicity. Panel B describes the waste management hierarchy, as described by the EPA (2019b), and ranks the management strategies from the most to the least environmentally preferred. Source reduction and reuse refers to reducing waste at the source, and is the most preferred strategy. Recycling/composition refers to activities related to collecting, sorting, and reprocessing items that would otherwise be considered waste. Energy recovery refers to the conversion of non-recyclable waste materials into useable energy. Treatment and disposal refers to the reduction of the volume and toxicity of waste through processing. Source: EPA.

TABLE IA.D1: TARGET SELECTION

This table reports the coefficients from a multivariate logistic regression relating firm characteristics that correlate the likelihood of being targeted by the BAP for environmental reasons. The sample consists of all Russell 3000 Index firms. The dependent variable is an indicator variable equal to one if the firm was targeted by BAP for environmental reasons. We consider the following financial, governance, and environmental characteristics. All regressions include industry (Fama-French 12 classification) fixed effects and standard errors are clustered at the firm-level. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. All variables are defined in Online Appendix Table IA.E7. Data Source: BoardEx, Compustat, Center for Research in Security Prices (CRSP), and Thomson Reuters' ASSET4 database.

Dependent variable	$\mathbb{1}_{Environment}$			
	Coefficient	Odds ratio	Coefficient	Odds ratio
	(1)	(2)	(3)	(4)
Market-to-Book	0.198	1.220	0.196	1.216
	(0.283)	(0.345)	(0.284)	(0.346)
Returns	-0.235	0.791	-0.237	0.789
	(0.255)	(0.201)	(0.274)	(0.216)
Profitability	1.934	6.920	1.944	6.986
	(1.404)	(9.714)	(1.411)	(9.856)
Firm size	0.689***	1.992***	0.687***	1.987***
	(0.259)	(0.517)	(0.260)	(0.516)
Dividend payer	0.460	1.583	0.463	1.589
	(0.683)	(1.081)	(0.682)	(1.084)
Leverage	0.823	2.276	0.826	2.285
	(1.457)	(3.317)	(1.461)	(3.339)
Cash holdings	-2.885	0.056	-2.896	0.055
	(2.821)	(0.158)	(2.813)	(0.155)
Institutional ownership $\geq$ 1%	0.504	1.656	0.529	1.698
	(0.569)	(0.942)	(0.580)	(0.985)
Board independence	-0.001	0.999	-0.001	0.999
	(0.019)	(0.019)	(0.019)	(0.019)
CEO chair duality	-0.024	0.976	-0.031	0.969
	(0.401)	(0.391)	(0.403)	(0.391)
New directors	-0.428	0.652	-0.418	0.659
	(0.555)	(0.362)	(0.559)	(0.368)
Boardsize	-0.021	0.979	-0.017	0.983
	(0.139)	(0.137)	(0.140)	(0.138)
Hedge fund target	-0.958	0.384	-0.951	0.386
	(0.703)	(0.270)	(0.701)	(0.271)
Media coverage	-0.021	0.979	-0.016	0.984
	(0.090)	(0.089)	(0.092)	(0.090)
Fossil Free Index	2.814**	16.680**	2.822**	16.812**
	(1.113)	(18.561)	(1.105)	(18.577)
Environmental fines	0.018	1.018	0.013	1.013
	(0.067)	(0.069)	(0.069)	(0.070)
Environment score	-0.179	0.837	-0.186	0.830
	(0.113)	(0.095)	(0.118)	(0.098)
Year fixed effects	No	No	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	969	969	969	969

TABLE IA.D2: RULING OUT DECLINES IN PLANT-LEVEL PRODUCTION

This table reports the impact of environmental activism on plant-level sales and employment as reported in National Establishment Time Series (NETS). The regressions compare outcomes at plants of targeted firms to toxic chemical releases at plants of Russell 3000 Index firms in a propensity score matched stacked difference-in-differences empirical setup. The dependent variable in column (1) is the natural logarithm of plant-level sales while in column (2) is the natural logarithm of plant-level employment. *Post* is an indicator variable equal to one for all years after the activism event year (pseudo-event year), and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons, and zero otherwise. All regressions include *Plant*, and *state* × *time* fixed effects. We allow these fixed effects to vary by cohort and within each pair of treated and control firm. We est. The sample includes 473 plants of 45 firms. Standard errors are clustered at the firm level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. All variables are defined in Online Appendix Table IA.E7. Data source: EPA Toxic Release Inventory (TRI) and National Establishment Time Series (NETS).

Dependent variable:	Log(sales)	Log(employment)
	(1)	(2)
Post × Environment	0.157	-0.023
	(0.214)	(0.020)
Fixed effects:		
Plant × cohort-pair	Yes	Yes
State × time × cohort-pair	Yes	Yes
Adjusted-R^2	0.78	0.78
Observations	1,889	1,889

TABLE IA.D3: ENVIRONMENTAL ACTIVISM AND GREENHOUSE GAS EMISSIONS

This table reports the impact of environmental activism on greenhouse gas emissions. The regressions compare greenhouse gas emissions at plants of targeted firms to greenhouse gas emissions at plants of Russell 3000 Index firms in a propensity score matched stacked difference-in-differences empirical setup. The dependent variable is the emission scaled by contemporaneous plant-level sales. The emissions are broken down by greenhouse gas type: *Carbon dioxide* (column 1), *Methane* (column 2), and *Nitrous oxide* (column 3). *Post* is an indicator variable equal to one for all years after the activism event year (pseudo-event year), and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons, and zero otherwise. All regressions include *Plant* and *time* fixed effects. We allow these fixed effects to vary by cohort and within each pair of treated and control firm. Given the count-like nature of the dependent variable, we estimate a fixed effects poisson model. The sample includes 237 plants of 38 firms. Standard errors are clustered at the firm level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. All variables are defined in Online Appendix Table IA.E7. Data source: EPA Greenhouse Gas Reporting Program and National Establishment Time Series (NETS).

Dependent variable:	$\frac{\text{Carbon dioxide}}{\text{Plant sales}}$	$\frac{\text{Methane}}{\text{Plant sales}}$	$\frac{\text{Nitrous oxide}}{\text{Plant sales}}$
	(1)	(2)	(3)
Post × Environment	-0.159**	-0.074	-0.082
	(0.066)	(0.135)	(0.130)
Fixed effects:			
Plant × cohort-pair	Yes	Yes	Yes
Cohort-pair × time	Yes	Yes	Yes
Pseudo-R^2	0.91	0.39	0.50
Observations	1,164	1,164	1,164

TABLE IA.D4: ENVIRONMENTAL ACTIVISM AND THE LIKELIHOOD OF CHEMICAL USE

This table examines changes in the extensive margin of emissions. The regressions compare the likelihood of chemical use at plants of targeted firms to the likelihood of chemical use at plants of Russell 3000 Index firms in a propensity score matched stacked difference-in-differences empirical setup. The dependent variable in column 1, $\mathbb{1}_{Start}$, takes the value of one if a plant reports emissions from a chemical that it had not previously reported and is zero otherwise. The dependent variable in column 2, $\mathbb{1}_{Stop}$, takes the value of one in the first year where a plant that had previously reported emissions of a given chemical no longer does (and will not do so at a future point in our sample) and is zero otherwise. *Post* is an indicator variable equal to one for all years after the activism event year (pseudo-event year), and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons, and zero otherwise. All regressions include *Plant* $\times$ *chemical*, *chemical* $\times$ *time*, and *state* $\times$ *time* fixed effects. We allow these fixed effects to vary by cohort and within each pair of treated and control firm. We estimate the coefficients using ordinary least squares (OLS). For ease of interpretation, we multiply the coefficient by 100. The sample includes 473 plants of 45 firms. Standard errors are clustered at the firm level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. All variables are defined in Online Appendix Table IA.E7. Data source: EPA Toxic Release Inventory (TRI) and National Establishment Time Series (NETS).

Dependent variable:	$\mathbb{1}_{Start}$ ($\times$ 100)	$\mathbb{1}_{Stop}$ ($\times$ 100)
	(1)	(2)
Post $\times$ Environment	-0.313*	0.000
	(0.157)	(0.000)
Fixed effects:		
Plant $\times$ chemical $\times$ cohort-pair	Yes	Yes
Chemical $\times$ time $\times$ cohort-pair	Yes	Yes
State $\times$ time $\times$ cohort-pair	Yes	Yes
Adjusted-R^2	0.54	0.79
Observations	25,642	25,642

TABLE IA.D5: ROBUSTNESS CHECKS

This table provides robustness around our main specification (Table 2). The test are organized around four categories: alternative inputs to propensity score matching (Panel A) and ruling out alternative explanations (Panel B). Row 1 reports the results with matching on total toxic chemical releases scaled by contemporaneous plant-level sales. Row 2 reports the results with matching on total releases and plant-level financial constraints measured using paydex score reported in NETS. Row 3 reports the results with matching on total toxic chemical releases, plant sales, and plant-level financial constraints measured using paydex score reported in NETS. Row 4 restricts the sample to utility firms. Row 7 reports the results where we exclude firms that are in top quartile in terms of their offshoring propensity using the text-based measure developed by Hoberg and Moon (2017). Row 6 reports the results where we exclude firms that filed for Chapter 11 bankruptcy. Row 7 reports the results where we exclude plants that closed during our sample period based on information reported in the NETS-TRI database. Row 8 restricts the sample to plants that report to the EPA's TRI program consistently since 2010. Row 9 reports the results where we exclude firms and plants that are based in the state of California. Rows 10 report results where we control for pre-event plant financial constraints. We follow Leibovici and Wiczer (2024) and identify firms as in financial distress in a given year if they were delinquent at least 30 days late once that year—that is, if their minimum Paydex score is 50 or lower. The remaining firms with a valid Paydex score are classified as in good financial standing. All regressions include *Plant* × *chemical*, *chemical* × *time*, and *state* × *time* fixed effects, except for row (4) where we are unable to include time-varying location-specification differences. We allow these fixed effects to vary by cohort and within each pair of treated and control firm. Given the count-like nature of the dependent variable, we estimate a fixed effects Poisson model. Standard errors are clustered at the firm level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. All variables are defined in Online Appendix Table IA.E7. Data source: EPA Toxic Release Inventory (TRI) and National Establishment Time Series (NETS).

	Toxic chemical release			
Dependent variable:	$\frac{\text{Total}}{\text{Plant sales}}$		$\frac{\text{On-site}}{\text{Plant sales}}$	
	$\beta_{\text{Post}\times\text{Environment}}$	Observations	$\beta_{\text{Post}\times\text{Environment}}$	Observations
	(1)	(2)	(3)	(4)
Panel A: Inputs to Propensity Score Matching				
(1) Matching on total releases	-0.186***	22,026	0.005	21,428
	(0.004)		(0.006)	
(2) Matching on total releases and plant-level financial constraints	-0.196***	19,725	-0.037***	19,172
	(0.003)		(0.005)	
(3) Matching on total releases, plant-level sales and financial constraints	-0.209***	16,909	-0.095***	16,342
	(0.003)		(0.003)	

Continued...

Dependent variable:	Toxic chemical release			
	$\frac{\text{Total}}{\text{Plant sales}}$		$\frac{\text{On-site}}{\text{Plant sales}}$	
	$\beta_{\text{Post}\times\text{Environment}}$	Observations	$\beta_{\text{Post}\times\text{Environment}}$	Observations
	(1)	(2)	(3)	(4)
Panel B: Ruling out alternative explanations				
Offshoring concerns				
(4) Utility firms	-0.334***	9,269	-0.229	9,262
	(0.124)		(0.245)	
(5) Exclude high offshoring firm	-0.209***	18,836	-0.084***	18,376
	(0.003)		(0.003)	
Plant closures				
(6) Exclude firms that filed for Chapter 11	-0.162***	23,729	-0.078***	23,064
	(0.054)		(0.010)	
(7) Drop plants that closed	-0.162***	24,131	-0.078***	23,471
	(0.054)		(0.010)	
(8) Plants continuously reporting	-0.162***	21,150	-0.078***	22,352
	(0.055)		(0.010)	
Regulatory concerns				
(9) Excluding California	-0.167***	22,433	-0.102***	21,776
	(0.055)		(0.011)	
Financial constraints				
(10) Control for pre-event plant-level financial constraints	-0.162***	23,627	-0.078***	22,956
	(0.054)		(0.010)	

TABLE IA.D6: ENVIRONMENTAL ACTIVISM AND PLANT-LEVEL RSEI SCORES

This table examines changes in the plant-level RSEI scores around targeting by the BAP campaign. This table reports the impact of environmental activism on toxic chemical releases. The regressions compare toxic chemical releases at plants of targeted firms to toxic chemical releases at plants of Russell 3000 Index firms in a propensity score matched stacked difference-in-differences empirical setup. The dependent variables are the RSEI risk-related scores that combine chemical quantity, toxicity, and population exposure into a comparative score that reflects relative human health risk (column 1), RSEI hazard-based results that incorporate chemical-specific toxicity (column 2), and total quantity of chemical released without accounting for toxicity or population exposure (column 3). The RSEI Score is a model implied risk-weighted measure of all chemicals and emissions released, as provided by the EPA. *Post* is an indicator variable equal to one for all years including and after the activism event year (pseudo-event year), and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons, and zero otherwise. All regressions include *Plant* × *chemical*, *chemical* × *time*, and *state* × *time* fixed effects. We allow these fixed effects to vary by cohort and within each pair of treated and control firm. Given the count-like nature of the dependent variable, we estimate a fixed effects poisson model. The sample includes 443 plants of 43 firms. Standard errors are clustered at the firm level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data source: EPA Toxic Release Inventory (TRI).

Dependent variable	Score	Hazard	Pounds
	(1)	(2)	(3)
Post × Environment	-0.183**	-0.666***	-0.844**
	(0.080)	(0.040)	(0.329)
Fixed effects:			
Plant × chemical × cohort-pair	Yes	Yes	Yes
Chemical × time × cohort-pair	Yes	Yes	Yes
State × time × cohort-pair	Yes	Yes	Yes
Pseudo-R^2	0.88	0.88	0.67
Observations	5,371	5,552	5,634

TABLE IA.D7: STOCK RETURNS OF TARGETED FIRMS RELATIVE TO MATCHED CONTROL FIRMS AROUND THE BAP CAMPAIGN

This table examines the equity response of firms targeted by the BAP campaign relative to matched control firms. We report results comparing the daily market-adjusted and abnormal equity returns (in percent) of targeted firms ten days before to twenty days after New York City Pension Fund System (NYCPS) filed exempt solicitation at target firms. We use market-adjusted returns (column 1), the Capital Asset Pricing Model (CAPM) (column 2), and Fama-French three-factor model (FF3) (column 3). *Post* is an indicator variable for the period of three days after the investor filling. All regressions are estimated using ordinary least squares (OLS). The sample includes 30 firms targeted in the first wave by the BAP campaign and matched set of 30 control firms. Standard errors are corrected for heteroskedasticity and auto-correlation across firms.***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data source: Center for Research in Security Prices (CRSP) and SEC Filings.

Dependent variable	Market-Adj. Return	CAPM	FF3
	(1)	(2)	(3)
Post × Treated	−0.261***	−0.256***	−0.208**
	(0.077)	(0.079)	(0.090)
Fixed effects:			
Firm	Yes	Yes	Yes
Cohort-pair × time	Yes	Yes	Yes
Adjusted-R^2	0.242	0.247	0.239
Observations	1,860	1,860	1,860

E Additional Tests for Mechanisms

E.1 Management's Opposition to the BAP Campaign

We use regulatory filings of targeted firms to better understand management's opposition to proxy-access proposals. To this point, we hand-collect information from all definitive proxy materials filed on Schedule 14A in the SEC's EDGAR database (see, Figure IA.B1 and Figure IA.B2, for examples). The 14A filings, a mandatory filing when a shareholder vote is required, list all proposals up for voting by shareholders at the annual meeting. Moreover, it presents management's view on proposals .

Studying the BAP campaign, we find that 55% of the targeted firms' management explain their opposition to proxy access in these filings.[36] We also find targeted letters by the management urging shareholders to vote. Further, the management settled immediately in the remaining 45% of the targeted firms and preemptively introduced a similar version of proxy-access as the initial proposal. Further, we collected all shareholder proposal no-action requests, posted online by the SEC's Division of Corporation Finance.[37] They provide us with a rare opportunity to study management's resistance in the process (Bhandari, Iliev, and Kalodimos, 2021; Matsusaka, Ozbas, and Yi, 2021). Interestingly, targeted firms in their first year are most likely to use this defense tactic than others.

Both, the actions of investors and management provide evidence of a direct channel. That is, management's active opposition to the BAP campaign obtained from regulatory filings and interactions with the comptroller's office suggests that the BAP presented a credible threat that lead to a successful engagement which led to firms responding to specific mandates of the investor.

E.2 Do Targeted Firms Respond to the Activist's Specific Mandate?

We present two additional tests using the stacked-cohort approach as in our baseline to show that firms are responding to the activist's specific mandates. First, we show that firms targeted for other pro-social mandates through the BAP (board diversity, excess CEO compensation), specifically respond along these dimensions, but do not change their environmental behaviors. Second, we step outside of the BAP to test other environmental campaigns and show that that firms are improving their corporate environmental behaviors around such campaigns. Taken together, these tests provide the robust evidence that firms respond to the specific demands of the activist instead of responding to the attention drawn to them by the campaign.

E.2.1 BAP Campaign: Board Diversity and Excess Pay Mandates

We first consider how targeted firms change their board composition in response to demands made under the board diversity mandate of the BAP. Estimates presented in Table IA.E5 establishes a consistent narrative to our baseline results, whereby targeted firms add new board members, that tend to be females, and from a minority background. Consistent with Barzuza (2019), column (1) suggests that firms are, on average, adding new individuals. Moreover, estimates from column (2) suggest that the female ratio, the fraction of females on board is increasing by one-third. When studying diversity, column (3) suggests that the HHI of nationality diversity is increasing. Similarly, when using the board member's country of origin, column (4) suggests the diversity of the board is increasing.

We next consider response of boards to the BAP's mandate to curtail excess CEO pay at targeted firms. Table IA.E4 shows that total CEO compensation and percent of excess compensation decreased for firms that were targeted by the BAP. Moreover, the estimates suggest that the growth in compensation also slowed down, with the estimates in column (1) suggesting that targeting by the BAP leads to a decrease in total compensation. We also document a curtailment in excess CEO pay, as reported in columns (3) and (4). Lastly, we do not find statistically significant changes in measures of firm pay-for-performance.[38] Given that firms targeted for non-environmental reasons did not change their environmental behaviors, but only responded to the activist specific mandates provides strong support that our baseline results operate through a direct channel.

[36]Unfortunately, in our setting, we do not observe other forms of outreach such as in-person discussions or phone calls by management or their proxy solicitors.

[37]The division receives requests from companies to state its informal, non-binding views on whether it concurs that there is a legal basis to exclude shareholder proposals under Exchange Act Rule 14a-8 ("Rule 14a-8"). The staff responses to the no-action requests contain correspondence relating to the staff's response and the company's initial submission and any subsequent correspondence. In each case, the hyperlink will be available when the materials are posted on the SEC website, generally within a few business days of the staff's response. See Figure IA.B2 and Figure IA.B3 for examples of letters to the SEC and their decision.

[38]For delta and vega, we extend the methodology from Coles, Daniel, and Naveen (2006, 2014); Core and Guay (2002).

TABLE IA.E1: VOTING OUTCOMES FOR SHAREHOLDER PROPOSALS

This table provides a summary of outcomes for the activism events undertaken by the Boardroom Accountability Project (BAP) initiated by the New York City Pension System (NYCPS). Specifically, it reports the average shareholder support and passage rate of the proposals put forward under the BAP campaign and proxy access proposals at other firms. For the BAP campaign, we identify outcomes through publicly available data from the Boardroom Accountability Project. Specifically, we identify whether firms are targeted for their inadequate redressal of climate change risk (*Environment*) or if they are targeted for other reasons (*Others*) which include excessive CEO pay, lack of board diversity, inadequate political disclosure, and adherence to labor standards. Data Source: Shareowners Initiative Postseason Annual Reports and Institutional Shareholder Services (ISS) Voting Analytics Database.

	Vote for (%)	Passed (%)
	(1)	(2)
(1) All firms targeted by the BAP campaign	57.79	38.06
(2) Other shareholder proposals in the same firm not by the NYCPS	27.47	3.85
(3) Firms targeted for environment reasons by the BAP campaign	58.59	51.02
(4) Other shareholder proposals in the same firm not by the NYCPS	29.10	3.94
(5) All firms targeted by the BAP campaign	57.79	38.06
(6) Proxy access proposals at other firms not by the NYCPS	46.83	27.50

E.2.2 Are These Effects Specific to the BAP campaign?

How generalizable are the results in the broader context of other environmental campaigns? It is essential to understand whether and how investors can target firms for sustainability mandates to catalyze specific changes to address their vulnerability to climate change.

We collect all shareholder proposals from the Institutional Shareholder Services (ISS) database submitted to firms over the same period. From this sample, proposals were categorized by hand on whether they related to an environmental mandate. To ensure the veracity of the matches, we relied on two independent research assistants. Using the resulting list of sustainability campaigns, we re-estimate our baseline specification and examine the effectiveness of these campaigns. Consistent with our baseline specification, we combine a propensity score matched sample with a stacked cohort empirical design.

Estimates from these tests suggest that targeted firms respond to an activist investor's firm-specific sustainability mandate even outside of the BAP campaign. We re-run our baseline specification (Panel A of Table IA.E6) and find similar results: plants of targeted firms, on average, reduced their total toxic chemical releases. Column (1) of Table IA.E6 suggests that targeted firms' plants respond by reducing their total toxic chemical releases. Estimates in column (2) suggests that the active on-site reductions in chemical releases drive the baseline reductions in the total toxic chemical releases.

We are careful in interpreting this evidence. These campaigns are not a relatively clean setting, making it difficult to disentangle treatment effects from selection. Prior work finds that institutional investors file shareholder proposals after failed private negotiations with the management, and hence managers of the firm anticipate such proposals (McCahery, Sautner, and Starks, 2016). Moreover, these proposals are specific (e.g., disclosure), potentially reducing managerial flexibility in responding to them. In light of this, we take these findings as complementary to our baseline results, establishing that investors, beyond the BAP, can discipline firms' non-financial characteristics through firm-specific sustainability mandates.

TABLE IA.E2: BAP CAMPAIGN AND PROXY ACCESS OUTCOMES

This table provides a summary of outcomes for the activism events undertaken by the Boardroom Accountability Project (BAP) initiated by the New York City Pension System (NYCPS). The table reports the average fraction of proxy access proposals that are settled without going for a shareholder vote and the average fraction of proposals that are eventually adopted. For the BAP campaign, we identify outcomes through publicly available data from the Boardroom Accountability Project. Specifically, we identify whether firms are targeted for their inadequate redressal of climate change risk (*Environment*) or if they are targeted for other reasons (*Others*) which include excessive CEO pay, lack of board diversity, inadequate political disclosure, and adherence to labor standards. All variables are defined in Online Appendix Table IA.E7. Data Source: Shareowners Initiative Postseason Annual Reports.

	Settled immediately (%)	Adopted eventually (%)
	(1)	(2)
(1) All firms targeted by the BAP campaign	59.64	88.89
(2) Firms targeted for environment reasons by the BAP campaign	66.18	89.55

TABLE IA.E3: DOES TARGETING FOR OTHER REASONS DRIVE THE REAL EFFECTS?

This table reports results examining the impact of targeting firms for non-environmental reasons (panel a) and proxy access and other governance reasons by other investors (panel b) on their release of toxic chemicals. The regressions compare toxic chemical releases at plants of targeted firms to toxic chemical releases at plants of Russell 3000 Index firms in a propensity score matched stacked difference-in-differences empirical setup. We exclude the firms targeted for environmental reasons by the BAP from this sample. The dependent variable is the toxic chemical release scaled by contemporaneous plant-level sales reported in the National Establishment Time Series. *Post* is an indicator variable equal to one for all years after the activism event year (pseudo-event year), and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons, and zero otherwise. All regressions include *Plant* $\times$ *chemical*, *chemical* $\times$ *time* fixed effects and *state* $\times$ *time* fixed effects. We allow these fixed effects to vary by cohort of treated and control firm. Given the count-like nature of the dependent variable, we estimate a fixed effects Poisson model. The sample in panel A includes 263 plants of 24 firms, and panel B includes 1018 plants of 110 firms. Standard errors are clustered at the firm level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. All variables are defined in Online Appendix Table IA.E7. Data source: EPA Toxic Release Inventory (TRI), ISS Shareholder Proposals database, and National Establishment Time Series (NETS).

	Panel A: Non-environmental targeting by the BAP	
Dependent variable:	$\frac{\text{Releases}}{\text{Plant sales}}$	
	Total	On-site
	(1)	(2)
Post $\times$ Other Reasons	-0.109	0.896
	(0.164)	(0.772)
Fixed effects:		
Plant $\times$ chemical $\times$ cohort-pair	Yes	Yes
Chemical $\times$ time $\times$ cohort-pair	Yes	Yes
State $\times$ time $\times$ cohort-pair	Yes	Yes
Pseudo-R^2	0.37	0.39
Observations	15,129	15,129

	Panel B: Proxy access & Governance	
Dependent variable:	$\frac{\text{Releases}}{\text{Plant sales}}$	
	Total	On-site
	(1)	(2)
Post $\times$ Proxy Access & Governance	-0.319	-0.190
	(0.372)	(0.402)
Fixed effects:		
Plant $\times$ chemical $\times$ cohort-pair	Yes	Yes
Chemical $\times$ time $\times$ cohort-pair	Yes	Yes
State $\times$ time $\times$ cohort-pair	Yes	Yes
Pseudo-R^2	0.33	0.37
Observations	26,022	26,022

TABLE IA.E4: BAP CAMPAIGN AND CEO COMPENSATION

This table reports the impact of targeting by the BAP campaign on companies that failed to align executive compensation with business performance. The regressions compare measures of executive compensation of targeted firms to measures of executive compensation at other firms. The dependent variables are the natural logarithm of total compensation (column 1), growth in total compensation (column 2), excess total compensation (column 3), growth in excess compensation (column 4), and pay-for-performance measure of compensation delta (column 5) and vega (column 6). *Pay* is a dummy variable indicating whether the firm is a target of BAP campaign. *Post* is an indicator variable equal to one for all years after the activism event year (pseudo-event year), and zero otherwise. All regressions include year and firm fixed effects. We allow these fixed effects to vary by cohort of treated and control firm. We estimate ordinary least squares (OLS). The sample includes 69 firms. Standard errors clustered at the firm level. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. All variables are defined in Online Appendix Table IA.E7. Data source: Execucomp.

	CEO compensation		Excess compensation		Pay-for-performance	
Dependent variable	Log (total)	% (total)	Total	% (total)	Delta	Vega
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Pay	-0.164*	-0.246***	-29.838**	-0.209**	1013.120	-157.911
	(0.097)	(0.071)	(11.620)	(0.091)	(932.062)	(119.799)
Fixed effects:						
Firm × cohort-pair	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-pair × time	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.78	-0.43	0.51	0.72	0.85	0.53
Observations	420	412	396	396	316	316

TABLE IA.E5: BAP CAMPAIGN AND DIVERSITY ON BOARDS

This table reports the impact on firms that were targeted by the BAP campaign due to lack of boardroom diversity. The regressions compare measures of boardroom diversity of targeted firms to measures of boardroom diversity at other firms. The dependent variable is the firms' boardsize (column 1), male ratio (column 2), and HHI index for board members' ethnicity (column 3) and nationality (column 4). *Diversity* is a dummy variable indicating whether the firm is a target of BAP campaign. *Post* is an indicator variable equal to one for all years after the activism event year (pseudo-event year), and zero otherwise. All regressions include year and firm fixed effects and are estimated using ordinary least squares (OLS). Standard errors clustered at the firm level. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data source: BoardEx.

Dependent variable	Boardsize	Male Ratio	HHI (Ethinicity)	HHI (Nationality)
	(1)	(2)	(3)	(4)
Post × diversity	0.799**	-0.039***	-0.005*	-0.005*
	(0.319)	(0.013)	(0.003)	(0.003)
Fixed effects:				
Firm × cohort-pair	Yes	Yes	Yes	Yes
Cohort-pair × time	Yes	Yes	Yes	Yes
Adjusted-R^2	0.79	0.71	0.88	0.88
Observations	540	540	538	538

TABLE IA.E6: EXTERNAL VALIDITY

This table reports the impact of environmental activism on toxic chemical releases. The regressions compare toxic chemical releases at plants of targeted firms to toxic chemical releases at plants of Russell 3000 Index firms in a propensity score matched stacked difference-in-differences empirical setup. We exclude the firms targeted by the BAP from this sample. The dependent variable is the toxic chemical release scaled by plant-level sales reported in the National Establishment Time Series. *Post* is an indicator variable equal to one for all years after the activism event year (pseudo-event year), and zero otherwise. *Environment* is an indicator variable equal to one if the BAP targets the firm for environmental reasons, and zero otherwise. All regressions include *Plant* $\times$ *chemical*, *chemical* $\times$ *time* fixed effects and *state* $\times$ *time* fixed effects. We allow these fixed effects to vary by cohort of treated and control firm. Given the count-like nature of the dependent variable, we estimate a fixed effects Poisson model. Standard errors are clustered at the firm level and are robust to heteroscedasticity. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. All variables are defined in Online Appendix Table IA.E7.Data source: EPA Toxic Release Inventory (TRI), ISS Shareholder Proposals database, and National Establishment Time Series (NETS).

Dependent variable:	$\frac{\text{Releases}}{\text{Plant sales}}$	
	Total	On-site
	(1)	(2)
Post $\times$ Environment	-0.651*	-0.678***
	(0.333)	(0.201)
Fixed effects:		
Plant $\times$ chemical $\times$ cohort-pair	Yes	Yes
Chemical $\times$ time $\times$ cohort-pair	Yes	Yes
State $\times$ time $\times$ cohort-pair	Yes	Yes
Pseudo-R^2	0.55	0.49
Observations	133,245	133,245

TABLE IA.E7: VARIABLE DEFINITIONS

This table presents definitions to variables used in the paper. All scaled variables are winsorized at the 0.5% tails.

Variable	Description	Data Source
Panel A: Plant-level		
Total releases	Total quantity of toxic chemical released	EPA Toxics Release Inventory Program
On-site releases	Total quantity of on-site toxic chemical released	EPA Toxics Release Inventory Program
Production ratio	Quantity of output in any given year relative to the quantity of output in the previous year.	EPA Toxics Release Inventory Program
Normalized production	Quantity of output in any given year relative to the quantity of output in the first year in our sample.	EPA Toxics Release Inventory Program
$\mathbb{1}_{Start}$	Indicator variable that takes the value of one if a plant reports emissions from a chemical that it had not previously reported and is zero otherwise.	EPA Toxics Release Inventory Program
$\mathbb{1}_{Stop}$	Indicator variable that takes the value of one in the first year where a plant that had previously reported emissions of a given chemical no longer does (and will not do so at a future point in our sample) and is zero otherwise.	EPA Toxics Release Inventory Program
Process & equipment modification	Cumulative number of initiatives including equipment or technology modifications, process or procedure modifications, undertaken to reduce pollution	EPA Pollution Prevention
Operating practices & training	Cumulative number of procedures and initiatives related to operating practices, including trainings, to reduce waste and manage production schedules efficiently.	EPA Pollution Prevention
Recycling	Indicator variable for on-site recycling initiatives	EPA Pollution Prevention
Energy recovery	Indicator variable for on-site energy recovery initiatives	EPA Pollution Prevention
Treatment & disposal	Indicator variable for on-site treatment & disposal initiatives	EPA Pollution Prevention
Carbon dioxide emissions	Quantity of carbon dioxide emissions	EPA Greenhouse Gas Reporting Program
Methane emissions	Quantity of methane emissions	EPA Greenhouse Gas Reporting Program
Nitrous oxide emissions	Quantity of nitrous oxide emissions	EPA Greenhouse Gas Reporting Program
Total expenditures	Pollution abatement expenditures, sum of capital and operation and maintenance expenditures	Energy Information Administration
Capital expenditures	Expenditures for new structures and equipment during the year, excluding land and interest expense	Energy Information Administration
Operation & maintenance	Operation and maintenance expenditures that includes all contract and self-service pollution abatement operation and maintenance expenditures for each line item.	Energy Information Administration
Plant-level sales	Establishment sales in US$	National Establishment Time Series
Plant-level employment	Establishment employment reported in NETS	National Establishment Time Series
Panel B: Firm-level		
Returns	Stock return in the past 12 months	CRSP
Market-to-book	Ratio of market value of equity plus book value of debt over book value of assets	Compustat
Firm size	Natural logarithm of the book value of assets	Compustat
Leverage	Ratio of book value of debt divided by market value of equity plus book value of debt	Compustat
Dividend payer	Indicator variable for the dividend paying firm.	Compustat
Profitability	Ratio of net income scaled by total assets	Compustat
Market-Adjusted Returns	Daily returns net of the CRSP value-weighted market return, including dividends	CRSP
CAPM Returns	Daily returns net of expected returns from the Capital Asset Pricing Model (CAPM)	CRSP, Ken French's Website
Fama–French Returns	Daily returns net of expected returns from the Fama–French Three-Factor (FF3F) model	CRSP, Ken French's Website
Cash holdings	Ratio of cash to book value of assets	Compustat
Operating income	Earnings before interest, taxes, depreciation, and amortization	Compustat
Settled immediately	Fraction of proxy access proposals that were adopted without a shareholder vote in the company bylaws	NYCPS Annual Report
Adopted eventually	Fraction of proxy access proposals adopted in the company bylaws	NYCPS Annual Report
Vote for	Support rates for shareholder proposals	ISS voting analytics
Passed	Passage rates for shareholder proposals	ISS voting analytics
Board independence	Fraction of independent directors on the board	BoardEx
CEO-chair duality	Indicator variable for firms where the chairman and the CEO positions are held by the same person	BoardEx
New directors	Fraction of new directors added to the board each year	BoardEx
Boardsize	Number of directors sitting on the board each year	BoardEx
CEO Compensation	Total compensation of CEO	Execucomp
Excess Compensation	Residual of the regression model with natural logarithm (Log) of compensation as dependent variable and proxies for economic determinants of CEO compensation, such as firm size, growth opportunities, stock return, accounting return, and industry controls as independent variable	Execucomp
Pay-for-performance, delta	Dollar change in wealth associated with a 1% change in the firm's stock price	Coles et al. (2006); Core and Guay (2002)
Pay-for-performance, vega	Dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's returns	Coles et al. (2006); Core and Guay (2002)
Hedge fund target	Indicator variable for the firms that were targeted by the hedge funds	Brav et al. (2008)
Institutional ownership	Indicator variable for firms with one percent or more of outstanding shares held by institutional investors	Thomson Reuters 13F
Media coverage	Natural logarithm of the number of articles about a firm in the media.	RavenPack
Fossil Free Index	Indicator of whether the firm is listed on the Fossil Free Index	Carbon Underground
Environmental fines	Natural logarithm of the annual environment related fines	EPA
Environment score	Standardized environmental score	Thomson-Reuters ASSET4
Panel C: Regional-level		
Number of chemicals	Number of chemicals with nonzero concentrations for geographic unit.	EPA Risk-Screening Environmental Indicators
Number of releases	Number of individual releases affecting geographic unit.	EPA Risk-Screening Environmental Indicators
Toxic concentration	Concentration multiplied by inhalation toxicity weight, summed over all chemicals impacting geographic unit.	EPA Risk-Screening Environmental Indicators
Cancer score	Average aggregated toxicity-weighted concentration of the cells in the geographic unit, using only inhalation toxicity weight for cancer effects.	EPA Risk-Screening Environmental Indicators

F Excerpts From Sustainability Reports

This table summarizes various ways firms report that they are improving their environmental impact. These are collected from each firm's sustainability reports.

Company	Action
AES Corporation	• The primary reason for a decrease in NOx emissions since 2015 is fuel conversion projects, decommissioning and the sell-down of several fossil fuel-fired units. • Diverse emission reduction projects or low-carbon energy installations were implemented or started implementation for over 1.5 million metric tons of estimated annual CO2e reductions.
Chevron Corporation	• We have developed internal country-specific plans to minimize gas flaring, and we are a member of the World Bank–led Global Gas Flaring Reduction Partnership. • Chevron flares and vents natural gas for safety and operational purposes and in areas where pipelines or other gas transportation infrastructure and utilization alternatives do not exist. We are working to reduce natural gas flaring and venting and the resulting GHG emissions.
ConocoPhillips	• NCG co-injection technology to reduce GHG emissions while reducing operating cost goal to validate technology on commercial scale on full lifecycle reduce GHG 15% in reservoirs affected by thief zones • In 2018, our total gross operated GHG emissions, in CO2 equivalent terms, were approximately 20.3 million tonnes, a decrease of about 1.4%, or 0.3 million tonnes, from 2017
CONSOL Energy Inc.	• Scope 1 emissions related to our current and our former parent company's coal assets have decreased by approximately 50%. • Our scope 2 emissions decreased by 12% compared to 2016 levels, and by 26% compared to 2015 levels
Devon Energy Corporation	• We also use green completions to capture produced gas during completions and well workovers following hydraulic fracturing. • Where flaring is unavoidable, we install monitoring equipment to help ensure the gas is properly destroyed rather than vented. • In recent years, Devon has implemented new technologies and upgraded our existing operations to reduce methane emissions from production sites. Since 2011, we've replaced high-bleed natural gas pneumatic controllers on hundreds of wells in Wyoming, Oklahoma, New Mexico, and Texas, and we no longer use them on new wells. • Spill prevention procedures at our facilities with storage tanks include secondary containment, "nearly full" tank alarms, and offsite monitoring equipment with the ability to shut in facilities remotely.

Company	Action
Duke Energy Corporation	• Since 2005, decreased carbon dioxide (CO2) emissions by 31 percent, sulfur dioxide emissions by 96 percent and nitrogen oxides emissions by 74 percent.
EOG Resources	• Reduced our methane intensity rate and our methane emissions as a percentage of our natural gas produced in 2018 by 46 percent and 45 percent • In 2018, our GHG intensity rate slightly increased by 1 percent, primarily due to increased operational activity and an increase in our emissions intensity associated with combustion sources.
EQT Corporation	• Our total energy consumption across facilities was 49,277 GJ, a 5% decrease from 2016. Our direct energy usage in 2017 totaled 18,905 GJ and was primarily generated by natural gas • In 2017, 100% of EQT's completions operations were flare-less and we used green completions technology for 100% of completed wells.
Freeport-McMoRan Copper & Gold Inc.	• FMC's total GHG emissions in 2018 were 4.9 million metric tons compared to 5 million metric tons the year prior.
Hess Corporation	• We have reduced our absolute Scope 1 and 2 equity emissions from 10.8 million tonnes of CO2e to 3.9 million tonnes, or 64 percent • On an intensity basis, we have reduced our cumulative flaring intensity by 41 percent through 2018, compared with our 2014 baseline. While our flaring intensity remained essentially flat between 2017 and 2018, we are still on track to achieve our 50 percent reduction target by 2020. • The number of non-hydrocarbon spills decreased by 9 percent in 2018 compared with 2017, and the volume of spills decreased by 80 percent.
Marathon Oil Corporation	• Over the last five years, our methane emissions intensity has declined by 31%, largely due to our internal goal of eliminating the routine use of high-bleed pneumatic controllers, which we met ahead of schedule. • Recorded a global spill volume reduction of 8%.

Company	Action
Noble Energy, Inc.	• Our global direct greenhouse gas emissions decreased by 6.4 percent year over year in 2018. • Methane intensity (total methane emitted expressed as a percentage of natural gas production) decreased in 2018 as a result of divesting Gulf of Mexico and Southwest Royalties assets as well as operational improvements. Intensity was 0.09 percent compared to 0.13 percent in 2017.
Occidental Petroleum Corporation	• Using the most recent data, from 2016 to 2017, Occidental's sum of direct and indirect GHG emissions (Scope 1 + Scope 2) decreased more than 3 percent year-over-year (16.0 to 15.5 million tonnes CO2e).The decrease in combined Scope 1 and 2 emissions was primarily due to decreases in CO2e emissions associated with our chemical segment's power consumption. • From 2016 to 2018, Occidental reduced gas flaring emissions intensity by approximately 17 percent at its EOR plants in the Permian Basin.
Pioneer Natural Resources Co.	• Our vapor recovery strategy allows Pioneer to capture potential facility emissions and recover these high-value gases as part of our product offerings. • Pioneer is testing two innovative approaches to monitoring methane emissions in its operations: 1) aerial methane monitoring, which provides field-wide survey capabilities, and 2) continuous methane monitoring, which detects leaks at the facility level.
PPL Corporation	• As part of LG&E and KU's ongoing emissions reduction efforts, the companies use GPS and fleet monitoring software to control idling and optimize routes for their vehicle fleet. The utilities continue to seek out opportunities to use plugin hybrid electric vehicles when costs are favorable. Since 2015, PPL Electric has been working to reduce Sulfur Hexafluoride (SF6) gas emission through predictive analytics.
Southern Company	• Without any regulatory mandates, our system's total annual GHG emissions in 2018 of 102 million metric tons of CO2 equivalent (CO2e), were approximately 35 percent (54 million metric tons) lower than 2007 levels • We have mitigated more than 3.3 million metric tons of CO2e from the atmosphere. These reductions are the result of aggressive investment in programs like those targeting pipeline replacement to improve the safety and performance of our natural gas system.

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