Catalytic Divestment: The World's Largest Investor's Exit from Coal and Global Emissions*

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Abstract

We show that individual bellwether investors can trigger large capital market shifts and sharply reduce a socially harmful activity by publicly blacklisting implicated firms. In 2016, Norway's massive Oil Fund announced that it had divested or begun monitoring coal-intensive companies in its broad, global portfolio. We estimate a roughly 18% relative decrease in targeted firms' greenhouse gas emissions—comparable to that of the EU Emissions Trading System—alongside decreased coal activity, emissions intensity, market valuations, and satellite-measured air pollution around their facilities. The coal exit swayed other large and scrutinized investors to reallocate away from targeted firms.

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1 Introduction

Climate change is modern economic history's most complicated coordination problem. Despite decades of multilateral agreements, global greenhouse gas emissions remain far above levels consistent with even moderate climate targets (IPCC, 2023; UNEP, 2025). Decarbonization requires simultaneous adjustments among firms, consumers, and governments, but individual actors often benefit from continuing their carbon-intensive activities if the expectation is that others will do the same. Such strategic complementarities can generate persistent high-emissions equilibria, even if a green transition is welfare-superior and technologically feasible (Besley & Persson, 2023).

In capital markets—a key part of the challenge—investors increasingly incorporate environmental concerns into portfolio choice guidelines (Financial Times, 2024). To many economists, this is puzzling and unlikely to help: in a frictionless market, divestment by diversified investors should be offset by others, implying little more than ownership reshuffling (Hart, 2020; Broccardo *et al.*, 2022). However, if markets are shaped by social dynamics and perceived norms, portfolio decisions may convey information beyond the capital directly withdrawn. In that case, prominent investors' actions can in theory shift market-expectations about the future valuation of carbon-intensive firms and trigger collective responses. They—possibly even a single especially prominent investor—would then act as a focal point (Schelling, 1960; Brock & Durlauf, 2001), and be able to facilitate the beginning of a shift to a lower-emissions equilibrium.

In this paper, we study a possible such case: the sudden exit from coal-intensive companies in 2016 of Norway's "Oil Fund". The Oil Fund holds only small stakes in individual firms, but it is in aggregate the world's largest single investor, owning about 1.5% of global listed equity value spread across more than 9,000 companies (NBIM, 2024a). It has historically been a particularly prominent actor in responsible investment, owing to its overall scale, public mandate, and relatively transparent governance. The fund's portfolio decisions are much more salient to the market than those of other large institutional investors (Chambers *et al.*, 2012; Greenpeace, 2018; Rudolf & Yuan, 2025).

The ethical framework of the Oil Fund (formally, Government Pension Fund Global) specifies criteria under which portfolio companies may be divested or monitored—based on product involvement (e.g., nuclear weapons) or conduct (e.g., corruption). In 2015, it introduced a new product-based rule targeting coal. Mining companies and power producers deriving a substantial share of revenues from thermal coal could now be divested or placed under monitoring. The criterion was first implemented in 2016, when the fund abruptly announced the sale of its holdings in 59 large firms and placed 11 under monitoring.

We focus on these initial targets and examine whether the fund's coal "exit" affected firms' environmental behavior and demand for their stocks. The coal criterion is particularly informative because it was introduced when responsible investing already had traction. By 2015, signatories to the UN-backed Principles for Responsible Investment (PRI), for example, represented more than half of global institutional assets under management (Brown, 2025).[1] If expectations were poised to shift, a signal could plausibly translate diffuse concerns into market behavior and catalyze coordination. In 2015, the relevant counterfactual is thus not a world without salient climate concern, but one in which adjustment would likely remain delayed absent a focal event.

Empirically, we first compare treated firms—those divested or placed under monitoring in 2016 under the coal criterion—with other large firms in the same sectors that satisfied the fund's other investment criteria and were thus in its 2015 portfolio. We estimate a firm-level total GHG emissions decrease of about 18% in corporate emissions data. To put this in perspective, we compare both to experts' predictions—academic researchers in economics and adjacent fields on average expect an effect size about one-sixth to one-quarter of this magnitude (see Online Appendix G)—and to Europe's cornerstone environmental policy. Our estimates indicate that the Oil Fund's coal exit triggered a relative decrease in GHG emissions about 1.3-1.5 times that implied by a Europe-wide scaling of Colmer *et al.* (2025)'s estimate for the EU Emissions Trading System (see Online Appendix C). We corroborate the estimated decrease in emissions using satellite-based measures of local air pollution around coal-fired power plants. They also show a large, statistically significant induced decline. The results remain stable across alternative control group definitions and a range of sensitivity checks.

We then examine how these firm-wide reductions are achieved. Overall downsizing appears to play a limited role: we find no effect on total revenues. In contrast, treated firms reduce emissions intensity by about 17% and shift their revenue mix away from coal mining and coal-fired power generation, consistent with real operational changes rather than accounting relabeling.

Finally, we more directly examine whether the Oil Fund's coal exit appears to have been a focal signal in capital markets. Our results are difficult to reconcile with explanations based on contemporaneous policy shifts or an ongoing decline of the coal industry. Spin-off or other firm-boundaries explanations are also unlikely: the coal criterion applies at the business-group level; our plant-level evidence does not attribute emissions reduc-

[1] While initiatives such as PRI helped establish a common language for Environmental, Social, and Governance (ESG) stewardship, they do not specify concrete portfolio actions or exposure thresholds.

tions from coal plants sold to continuing operators to the original owner; and so-called Scope 2&3 emissions (from purchased energy and firms' value chains) also decline. Instead we find that other investors—particularly more scrutinized ones—were induced to reallocate their holdings away from targeted firms by the fund's announcement; these ownership shifts are accompanied by lower valuations and higher realized returns (see also Atta-Darkua, 2022; Berle *et al.*, 2025), suggesting a coal premium. A role for "catalytic divestment" in explaining our findings is consistent also with the patterns Rudolf & Yuan (2025) find in exclusion announcement data from 86 major institutional investors and with Greenpeace's assessment that the fund's coal criterion "set a template, which was replicated by many other investors" (Greenpeace, 2018, p.1).[2]

This paper's findings are a form of empirical proof-of-concept: to our knowledge, the first direct evidence that changes in individual bellwether investors' signaled priorities can, at least in some circumstances, trigger broader shifts in capital markets and sharp decreases in blacklisted activities on the ground. We build, first, on work studying how investor preferences and constraints affect prices, ownership, and corporate behavior (see among others Dimson *et al.*, 2015; Hart & Zingales, 2017; Pástor *et al.*, 2021; Oehmke & Opp, 2022; Pástor *et al.*, 2022). A canonical example of investor mandates raising "sin" firms' cost of capital is Hong & Kacperczyk (2009). Overall, economists still expect ownership reshuffling but modest real effects of divestment (see e.g. Heinkel *et al.*, 2001; Berk & van Binsbergen, 2025), but the possibility of abrupt, broad shifts in expectations following a focal announcement or event appears to be more realistic than previously thought. Recent empirical research finds real environmental effects of pressure from financial actors (Azar *et al.*, 2021; Kahn *et al.*, 2023; Sachdeva *et al.*, 2024; Gianinazzi *et al.*, 2025; Green & Vallee, 2025; Berg *et al.*, 2026), and theories of focal points', social norms', and belief co-movement's potential to shift behavior from one equilibrium towards another have begun to find empirical support in cultural and interpersonal settings (Bursztyn *et al.*, 2020; Bursztyn & Yang, 2022; Gelfand *et al.*, 2024). Their potential role in a green transition is widely recognized (Bénabou & Tirole, 2010; Besley & Persson, 2023; van der Ploeg & Venables, 2025), but we are not aware of corresponding empirical evidence.

We also quantify the effectiveness of a form of supply-side environmental policy. Economists have historically favored demand-side policies, such as carbon taxes, cap-and-trade, and offset markets. Their limited adoption and the scale and trend of global emissions have

[2] Rudolf & Yuan (2025) find that "Firms excluded by NBIM [the Oil Fund] face five times more institutional investor exclusions than *other excluded* firms" (emphasis added). Fahlenbrach *et al.* (2026) show evidence that NBIM's public pre-disclosures of its voting intentions on general proposals at companies' annual shareholder meetings affect other shareholders' votes, with downstream governance effects such as director turnover.

increased interest in policy tools that instead attempt to limit the supply of fossil fuels (Bohm, 1993; Hoel, 1994; Harstad, 2012; Collier & Venables, 2014; Asheim *et al.*, 2019; Prest, 2022).[3] With the exception of Ahlvik *et al.* (2026)'s estimates of oil production tax elasticities, there is to our knowledge no existing direct evidence on the effectiveness of such policies.

2 The Oil Fund

Three characteristics of Norway's Oil Fund stand out for this paper's purposes: its size, credibility, and visibility. First, the fund's exceptional scale makes for salient portfolio choices: the press widely covers its decisions (Hoepner & Schopohl, 2018), and the fund itself acknowledges that its trading can move prices.[4] Second, its mandate-based governance links portfolio decisions to publicly defined criteria, enabling observers to interpret them as applications of established rules rather than discretionary trading. Third, the mandate codifies transparency (Government of Norway, 2023); relatively extensive reporting allows others to track the fund's actions over time.

2.1 Background

The fund was established in 1990 to invest government revenues from oil and gas production in international markets. Today, it is valued at a staggering USD 2 trillion, with around 70% in listed equities. The fund holds small individual stakes—roughly 0.9% of a median portfolio firm's shares—and caps ownership at 10%, a ceiling it rarely approaches.

Governance is three-tiered and designed to "put an arm's length between the politics and the savings" (NBIM, 2020b, p.18). Parliament provides democratic oversight and deliberates major policy decisions. The Ministry of Finance sets the fund's management mandate—defining the benchmark index, risk limits, and responsible investment requirements—and reports to Parliament. Norges Bank Investment Management (NBIM), a unit of the central bank, manages the portfolio within this mandate and seeks to maximize long-term returns subject to responsible-investment principles (NBIM, 2024b).

[3] Initiatives such as the Beyond Oil and Gas Alliance and the First Conference on Transitioning Away from Fossil Fuels (in April 2026) have considerable buy-in. IPCC (2022) notes that (even descriptive) evidence on international agreements to restrict fossil-fuel supply remains limited.

[4] Announcement of divestment is "postponed...until securities had actually been sold, so as not to create price pressure" NBIM (2020a, p.19).

2.2 Ethical Guidelines

The fund's responsible investment approach includes publishing governance position papers, expectation documents for portfolio companies, and advance voting intentions and voting records.[5] It also follows *Ethical Guidelines*, introduced in 2004. Issued by the Ministry of Finance and endorsed by Parliament, the Guidelines define product- or conduct-based criteria under which portfolio companies may be divested or monitored. NBIM publicly discloses the lists of affected companies as well as the fund's full year-end holdings. In 2015, the Guidelines expanded with the introduction of a coal criterion, effective February 2016. This rule targets "mining companies and energy producers that derive 30% or more of their revenues from thermal coal or base 30% or more of their operations on thermal coal" (Ministry of Finance, 2017, p.15).

Around the time of the coal criterion, researchers and policy observers frequently described the Oil Fund as an important reference point in responsible investment: a model for institutional investors according to Halvorssen (2023) and Velculescu (2008) and "an exemplar for investors around the world" according to Chambers *et al.* (2012, p. 67), for example. The coal criterion itself was later described as a "gold standard" and "major breakthrough" by Greenpeace (2018, p.1).

3 Data

We merge data on firm-level environmental, financial, and ownership, as well as plant-level coal-fired power generation (see Appendix B for further details).

NBIM. We collect data from NBIM press releases identifying divested or monitored firms (including the announcement date), and year-end lists of the fund's equity holdings. These are matched to the datasets below by name and headquarters country.

Trucost. We use firm-level environmental data from Trucost, which provides the widest available coverage of corporate greenhouse gas (GHG) emissions (Busch *et al.*, 2022). Trucost compiles emissions data from publicly disclosed sources, including company annual reports, regulatory filings, and sustainability reports, and standardizes them following the GHG Protocol. When firms do not voluntarily disclose emissions, Trucost estimates them using an environmentally extended input–output (EEIO) model.[6] Our main variables are

[5] See Government of Norway (2023) and NBIM (2024b).

[6] Trucost's procedure maps firms' business segments to detailed industries; estimates emissions using sectoral environmental intensities and input–output linkages; incorporates disclosed firm-level information

total GHG emissions (CO_2 equivalents, CO_2e), emissions intensity (emissions per unit of revenue), and revenues from coal mining and coal-fired power generation.

Orbis. We obtain financial and accounting data from Bureau van Dijk's Orbis database, including revenues, assets, equity, industry classification, and firm identifiers (ISIN, LEI).

Global Energy Monitor (GEM). We use the 2024 Global Coal Plant Tracker to identify coal-fired power plants, their operating status, capacity, ownership (current or as of the last year before retirement), and GPS coordinates (Global Energy Monitor, 2024a). Plants are linked to firms using firm identifiers and name matching. To capture indirect ownership, we use GEM's Global Ownership Tracker (Global Energy Monitor, 2024b).

Satellite data. We use satellite-derived measures of local air pollution—$PM_{2.5}$ (2012–2022) and NO_2 (2012–2019)—measured at annual, $0.01° \times 0.01°$ resolution by van Donkelaar *et al.* (2016) and Cooper *et al.* (2020), respectively. Pollution is assigned to plants by computing an area-weighted average of all grid cells that intersect with a one and five kilometer buffer around each facility, respectively. As control variables in plant-level regressions, we further use satellite-based precipitation data from the reanalysis Essential Climate Variables (ECVs) by Hersbach *et al.* (2018).

Investor ownership. We obtain information on the number and identity of investors holding each firm's equity over time from LSEG Workspace, which compiles regulatory filings and investor disclosures.

4 Empirical Approach

4.1 Sample Construction and Defining Treatment

Our study sample is an unbalanced panel of 1,908 firms covering 2012–2023, restricted to those with environmental data in the pre-treatment year. All sample firms belonged to the Oil Fund's investment universe and portfolio, but only those with sufficiently high coal exposure were targeted for divestment or monitoring. The setting thus lends itself to a difference-in-differences approach.

The treatment group ($N = 58$) comprises firms divested or monitored in 2016 under the coal criterion. We focus on this initial cohort because it corresponds most closely to

where available; and standardizes reported data.

our broader objective: assessing whether a high-profile investor can signal a new standard of corporate environmental conduct and contribute to shifting perceived market norms. We exclude firms targeted in 2015 under *non-coal* criteria or after 2016 under *any* criterion.

Whether firms are targeted is determined primarily by the announced 30% threshold for revenues derived from thermal coal, but the Oil Fund does not interpret the threshold as mechanically binding. For instance, it may exempt firms that take credible steps to reduce coal exposure, or firms that had "excess" coal shares in one year but not in prior years.[7] In our sample, nearly all targeted companies exceeded 30%, typically by a considerable margin.

We define treated firms as those that were themselves divested or monitored in 2016, rather than the alternative of *all* firms above the 30% cutoff. The alternative definition would, on the one hand, capture possible indirect treatment: above-threshold firms may have been impacted even without the fund's explicit "stamp," if the coal criterion changed market expectations for coal-intensive companies more generally. On the other hand, some above-cutoff firms may not have been targeted due to ongoing decarbonization efforts; considering them as treated could confound estimated treatment effects. These considerations—a threshold used to guide but not fully determine treatment; few firms with coal shares around that threshold; and by design no "high coal but untreated" firms—also illustrate why alternative empirical approaches such as a regression discontinuity design or matching are less suitable in this context.

4.2 Control Group and Estimation

The baseline control group consists of Oil Fund portfolio firms as of year-end 2015, operating in sectors where the coal criterion had "bite": Mining, Utilities, Manufacturing, Construction, Wholesale and Retail Trade, Information, and Management of Companies and Enterprises. These sectors have both firms directly engaged in coal extraction or coal-fired power generation, and diversified firms with coal exposure through subsidiaries or secondary business lines.[8]

This is a natural benchmark comparison group because of its pre-defined "width", and because these firms were, like the treatment group, held by the fund and hence satisfied its pre-2016 investment criteria. Reflecting the coal criterion's design, treated firms are predominantly in Mining and Utilities and are, on average, larger, more emissions-intensive, and more coal-dependent than control firms (see Appendix B for details). Most

[7] Hence, estimated treatment effects are unlikely to reflect temporary fluctuations or mean reversion.

[8] For example, a firm classified in "Management of Companies and Enterprises" may control subsidiaries operating coal-fired power plants.

control group companies were well below 30% (or had no coal revenues) in 2015, but some ($N = 21$) exceeded the threshold. Among all companies covered by Trucost globally in 2015, our sample firms account for 62% of total coal revenues; treated firms account for 45%.

If the introduction of the coal criterion induces *control* companies to reduce coal, such spillovers would attenuate estimated treatment effects. Our results are stable across narrower and wider control group definitions, including specifications restricting the analysis to only firms owning coal-fired power plants. We return to this in Sub-section 5.3.

Our baseline estimating equation is:

$$Y_{ijlt} = \alpha + \beta \cdot Post_t \cdot Treated_i + \gamma_i + \delta_{jt} + \zeta_{lt} + \epsilon_{ijlt}, \tag{1}$$

where Y_{ijlt} denotes the outcome for firm i in sector j and headquarters country l at time t. γ_i, δ_{jt}, and ζ_{lt} are firm, 2-digit industry-year, and country–year fixed effects, respectively. They absorb time-invariant firm characteristics, and time-varying sector-level and headquarters-country shocks, including national regulatory changes for climate policy, disclosures, and the energy sector. Standard errors are clustered at the firm level.

For β to have a causal interpretation, treated and control firms' outcomes (such as emissions) should have evolved in parallel if the coal criterion had not been introduced. The event-study in Figure 1 shows that emissions evolve similarly for treated and control firms prior to 2016, with pre-treatment coefficients close to zero. This also suggests that firms did not adjust behavior in expectation of treatment. This is unsurprising because the fund only announces divestment after positions are "unwound" and because we focus on the initially targeted firms. We implement the "honest" difference-in-differences approach of Rambachan & Roth (2023) to assess robustness to potential violations of parallel trends in Appendix D.

[Figure 1]

5 Effects on Emissions

5.1 Emissions Decline After Oil Fund Divestment and Monitoring

We first investigate how the Oil Fund's coal "exit" affects firms' emissions. Trucost's firm-level CO_2e captures emissions from all sources: direct (Scope 1), purchased-energy (Scope 2), and value-chain (Scope 3). We aggregate Scopes 1–3 since investors assess firms in all scopes (Cenedese *et al.*, 2024), and firms may adjust both direct and indirect emissions.

Oil Fund divestment and monitoring is found to decrease firms' CO_2e emissions by roughly 18% on average, as seen in Column 1 of Table 1, Panel A ($p = 0.055$). One way to benchmark this estimate's large magnitude is to compare to Colmer *et al.* (2025)'s study of the EU Emissions Trading System. If their estimated emissions decrease in France scales Europe-wide, the fund's coal exit appears to have caused a relative decrease in GHG emissions of about 1.3-1.5 times that of EU ETS (see Appendix C), accounting for the much bigger emissions base of the Oil Fund's portfolio firms.[9]

Although Trucost is the standard corporate emissions data source in research, its hybrid construction has limitations. For modeled observations or components, sector-level emissions intensities and input–output linkages may partially overlook firm-specific heterogeneity and adjustments. For observations based on company disclosures, measurement may instead be affected by self-reporting bias or greenwashing. These limitations motivate an additional, independent check for changes in real activity: satellite-based pollution measures.

We consider two satellite-derived measures of local air pollution: $PM_{2.5}$ and NO_2 concentrations around coal-fired power plants. We restrict the sample to firms owning such plants ($N = 115$) because plant-level GPS coordinates allow us to link facilities to gridded observations.[10] While $PM_{2.5}$ and NO_2 are not GHGs, they are, like CO_2, direct by-products of coal combustion, and thus informative proxies for plant-level activity (Zhang, 2016). If firm-level CO_2e reductions reflect real changes, we should arguably observe notable declines in $PM_{2.5}$ and NO_2 too.[11]

The satellite-based evidence corroborates the results from corporate data. At the firm level, $PM_{2.5}$ declines by roughly 14% (column 2). NO_2 falls by 11%, though this estimate is imprecise (column 3, $p = 0.124$). At the plant level, where statistical power is higher ($N = 654$), we estimate 7–12% reductions in both pollutants in treated coal-fired power plants (Columns 4–5).[12] These findings point to real and large changes in production activity.[13]

[9] The estimate we use in this calculation is for Scope 1–2 emissions only to avoid the broader value-chain double counting inherent in Scope 3 (value chain emissions). We find very similar results when restricting the aggregate to Scope 1–2 (Table A1, Column 1).

[10] We find similar effects on Trucost-measured emissions in this subsample of firms (Table A1, Column 3).

[11] Reductions in CO_2 emissions are often accompanied by declines in local air pollutants such as $PM_{2.5}$ and NO_2 (Karlsson *et al.*, 2020). In our data, these pollutants are strongly correlated with CO_2e emissions: 0.81 and 0.70 in levels, and 0.54 and 0.55 in logs, for PM2.5 and NO_2, respectively.

[12] NO_2 data have shorter temporal coverage (ends in 2019) and less within-location variation. The plant-level results are shown as a measurement robustness check, since the coal criterion applies to firms. We thus keep the specification aligned with our firm-level specification, Equation 1.

[13] Table A2 estimates separate effects for divestment and monitoring. We pool them in our baseline specification because both interventions publicly identify firms as coal-exposed under the Oil Fund's guidelines. In our treatment group, 50 and 8 firms were targeted by divestment and monitoring, respectively.

5.2 How Do Firms Achieve Emissions Reductions?

We next examine how firms reduce emissions. We distinguish between changes in scale and changes in production structure: emissions may fall because firms produce less, or because they produce more cleanly and shift away from emissions-intensive activities (Levinson, 2009; Shapiro & Walker, 2018; Martinsson *et al.*, 2024). We study these margins using total revenues, emissions intensity (emissions divided by revenues), and coal share of revenues.

Total revenues are essentially unchanged, with a small and statistically insignificant estimate of -2% (Table 1, Panel B). This implies that overall production scale remained relatively stable, or that reductions in quantities produced were small enough to be offset by higher prices. It also suggests that the emissions effects are less likely to be driven by output reallocation toward control firms in product markets (Barrows *et al.*, 2025).

In contrast, emissions intensity decreases by 17%, nearly matching the 18% drop in total CO_2e emissions. We also observe a shift toward cleaner activities. The share of coal-related revenues falls by seven percentage points—14% relative to the 2015 baseline.[14]

Together, these results suggest that the fund's exit and scrutiny induce fundamental changes in firm-wide production structure: firms achieve lower emissions per dollar of output—consistent with clean technology adoption or operational improvements—and move out of carbon-intensive business lines.

[Table 1]

5.3 Additional Analyses

Several patterns suggest that the observed emissions reductions are attributable to the Oil Fund's coal criterion (see Appendix E). To begin, the effects do not appear to be driven by contemporaneous environmental policies or post-2015 Paris Agreement decarbonization.[15] Table E1 compares treated companies with *dirty non-coal* counterparts: portfolio firms with no coal revenues but similar baseline CO_2e emissions. Although these firms also reduce emissions after 2015, the decline is substantially smaller than for treated firms. We also examine treatment effects by countries' baseline environmental policy stringency,

[14] A potential concern is that control firms have low baseline coal exposure, limiting their ability to capture counterfactual adjustments. To address this, Table A3 re-estimates the specification using alternative control samples with positive or higher baseline coal shares, as well as restricting the sample to coal power plant owners. The estimated effects remain negative and economically large across all specifications.

[15] Pástor *et al.* (2022) show evidence of a green/brown repricing around this time. It is possible that the Oil Fund's coal exit contributed to this "greenium." Whether it did is beyond this paper.

measured by the OECD Environmental Policy Stringency index. We do not find stronger effects in high-stringency countries.

Table E2 and the corresponding event study in Figure E1 demonstrate that the estimates are similar both for narrower control group choices (the most polluting sectors or publicly-listed firms owning coal-fired power plants) and broader ones (all 2015 portfolio firms), as well as with alternative satellite-based emissions measures.[16] Table A1 reports results using the alternative treatment definition that classifies all above-cutoff firms as treated. The point estimates are nearly identical but somewhat more precisely estimated.

Finally, we show further evidence that the estimated emissions decrease is unlikely to be driven by differential pre-treatment trends. Appendix F implements synthetic difference-in-differences (Arkhangelsky *et al.*, 2021), which reweights control firms and pre-treatment periods to better match treated firms' pre-treatment emissions paths. Since this method requires a balanced panel, thus reducing and selecting the sample, we use it only to probe sensitivity. The estimated effects are similar to—and if anything somewhat larger than—the baseline estimates.

6 The Oil Fund as a Focal Point

We next ask if the Oil Fund's coal criterion appears to have been seen as a focal signal in capital markets. It publicly identified firms whose coal exposure was inconsistent with responsible investing, potentially making them salient targets for divestment by others. Such a channel is not necessary to explain emissions reductions: targeted firms may for example also have adjusted directly in response to the Oil Fund's exit itself. But if the intervention more broadly helped shift and coordinate investor beliefs about coal, we should observe responses among other investors and in market valuations.

This prediction is closely related to the logic of "sin" stocks—stocks of companies whose activities are widely recognized as controversial, and that are avoided for reputation, institutional, or mandate-based reasons. If the Oil Fund helped classify coal-exposed firms as "sin," treated firms' stocks should experience reduced demand beyond the fund's own divestment, especially among investors more exposed to public scrutiny.

[16] Emissions effects also remain similar when including firms targeted post-2016 under the coal criterion in the control group (Table A1, Column 5.

6.1 The Oil Fund Triggers Other Investors' Exit

The focal-point interpretation suggests that other investors adjust their holdings in the same direction as the Oil Fund. We group investors according to the degree of public scrutiny they face (see also Hong & Kacperczyk, 2009). At one end are constrained investors—pension funds, endowments, foundations, government agencies, and sovereign wealth funds. They are more likely to face reputational or mandate-based pressure to avoid firms publicly associated with coal. At the other end are unconstrained investors, such as individuals, family offices, corporations, and insiders. Between them lies a heterogeneous set of investors whose incentives are difficult to characterize and which we therefore do not analyze separately.

After the Oil Fund's "stamp," treated firms' investor base shifts away from more publicly scrutinized owners. The number of shareholders in targeted firms declines,[17] and this drop is concentrated among constrained investors; their number falls by about 12%, while that for unconstrained investors is essentially unchanged (Table 2, Panel A). The underlying gross-flow rates point in the same direction (Panel B): exit rates rise among constrained investors, while entry rates rise among unconstrained investors.

The changes in investor counts are not accompanied by large changes in aggregate ownership shares (Panel C). Constrained investors' total ownership share falls slightly, while unconstrained investors' total share rises slightly, but neither estimate is statistically significant. Ownership concentration also does not increase. If anything, the ownership share among the top-ten largest constrained investors falls—by 12%—suggesting that the exit within this group is led disproportionately by its major investors. Overall, these patterns suggest that the Oil Fund's intervention catalyzed a resorting of investors, consistent with targeted coal firms becoming sin stocks.

[Table 2]

6.2 Financial Consequences of the Oil Fund's Focal Role

If the investor exit documented above reflects a shift in how markets view targeted firms, it should also appear in prices. As some investors become less willing or able to hold these stocks, treated firms may need to attract capital from a narrower set of potential investors. This can lower the price investors are willing to pay for a given stream of earnings, and may require higher returns for those who remain willing to hold the stock.

Monthly stock returns increase by 0.4 percentage points following NBIM's announcement (Table 3, Column 1), consistent with treated firms becoming less attractive to some

[17] The Oil Fund is excluded from all outcomes in Table 2 to avoid mechanical effects.

investors.[18] The price-to-EBITDA ratio—how much investors are willing to pay per dollar of a firm's operating earnings—declines by 13% following the announcement (Column 2). This is consistent with reduced investor demand, although it may also—and perhaps relatedly—reflect weaker expectations about future cash flows from coal-related activities. These consequences also extend to other firm valuation measures (Columns 3–4): market capitalization and net assets fall by 15% and 17%, respectively.

[Table 3]

Taken together, the evidence in this section points to a form of market influence related to, but distinct from, ownership-based market power. Large holdings in individual firms may help make an investor focal. Yet, the Oil Fund held small stakes in treated firms in 2015—between 0.05% and 1.78% of shares, with a median of 0.55%. Such holdings are unlikely, by themselves, to explain the wider changes in investor ownership and market valuations documented here. Instead, our results are consistent with the fund's aggregate scale, visibility, and credibility allowing its public coal criterion—in ripe mid-2010s conditions—to serve as a focal signal that shaped broader investor behavior.

7 Alternative Interpretations

(At least) three *a priori* plausible alternative explanations would imply different overall consequences. First, the coal criterion could have induced a reconfiguration of firm boundaries if firms respond by divesting "dirty" plants to entities that are less exposed to international capital markets (Zhou, 2022; Duchin *et al.*, 2025; Berg *et al.*, 2026). This is unlikely to explain our findings. The coal criterion explicitly consolidates activities at the business-group level: it applies also to entities a firm controls. As a result, internal transfers within the group—e.g., sales to subsidiaries—do not allow circumvention. In addition, the satellite-based evidence in Section 5 captures changes in emissions around coal *locations*, and ownership of coal plants is assigned such that we do not attribute reductions in firm emissions achieved by divesting plants to other firms that continue operating them to the original owner. Finally, column 2 of Table A1 is further evidence against this explanation. The estimated effect on Scope 2 and 3 emissions—those due to purchased energy and the firm's value chain—is not nil or positive, but instead negative and three-quarters the magnitude of our baseline estimate.

[18] The estimate corresponds to roughly two-thirds of the average monthly return of the S&P500, 0.61%.

Second, concurrent international policy developments could have affected portfolio firms in ways that are observationally similar to the patterns discussed in Section 5. However, as noted in Sub-section 5.3 and explored further in Appendix Section E, the evidence does not support this interpretation. Estimated effects are similar across countries with low and high baseline environmental policy stringency.

Finally, and perhaps most plausibly from today's vantage point, it could be that the estimated adjustments simply reflect a "structural," global decline of the coal industry and pull-out by firms and investors. Broad declines already being underway in 2015 is unlikely, however. Several OECD countries have adopted increasingly stringent policies to reduce coal use, but coal production and coal-fired electricity generation have continued to expand in several large non-OECD economies, particularly China and India, where regulatory pressure against coal remains weaker.

We compare estimates for OECD countries (blue coefficients in Figure 2) versus China+India (red). Treated firms in China and India if anything reduce emissions more than those in OECD countries, and investor responses also differ sharply: constrained investors exit primarily from China+India firms, while unconstrained investors tend to remain more invested in China+India firms. These patterns are difficult to reconcile with a simple declining-coal explanation.

[Figure 2]

8 Discussion and Conclusion

This paper asks if a large investor with focal point potential can trigger substantial decreases in blacklisted firm activities. We show evidence that the Norwegian Oil Fund's 2016 exit from coal led to massive reductions in firms' emissions, alongside changes in ownership, valuations, returns, and balance sheets consistent with tangible financial consequences for targeted firms.

Our results are consistent with a setting in which expectations about coal investments were already evolving, but remained uncoordinated. In this environment, the Oil Fund's coal exit may have provided a salient signal that aligned beliefs about future financing conditions. Other investors, especially those more exposed to public scrutiny, adjusted their holdings in the same direction. The intervention therefore appears not only to have removed the fund's own capital from targeted firms, but also to have helped make coal exposure a salient flag for exclusion by others.

The resulting emissions reductions among targeted firms are large compared to flagship demand-side environmental policies. Leaving out Scope 3 emissions to minimize any risk of double-counting when aggregating as in the Sub-section 5.1 comparison to Colmer *et al.* (2025)'s estimates for the EU Emissions Trading System,[19] we are left with an annual decline of approximately 428 million tonnes of Scope 1–2 CO_2e, or about 3.4 billion tonnes over the post-treatment period.[20] This is about 13% of cumulative EU GHG emissions over our study period (EEA, 2026a,b) (see Appendix C). While such comparisons should be interpreted with caution, they underscore the economic significance of this paper's results and the potential promise of supply-side environmental policy.

The response of other investors to a prominent investor's exit, alongside the decline in targeted firms' valuations, suggest a role for focal points even in large, liquid capital markets. Their influence need not operate only through divestment. This may be what activists have in mind when criticizing the Oil Fund's current leadership on issues such as children's online safety and human rights, for example for consistently voting against shareholder proposals in technology companies (NRK, 2026).

[19]As discussed in Sub-section 5.1, we find very similar results when restricting the aggregate to Scope 1–2 rather than 1-2-3.

[20]This back-of-the-envelope calculation scales the estimated treatment effect only within sample treated firms, rather than extrapolating it to all coal producers or to overall global emissions.

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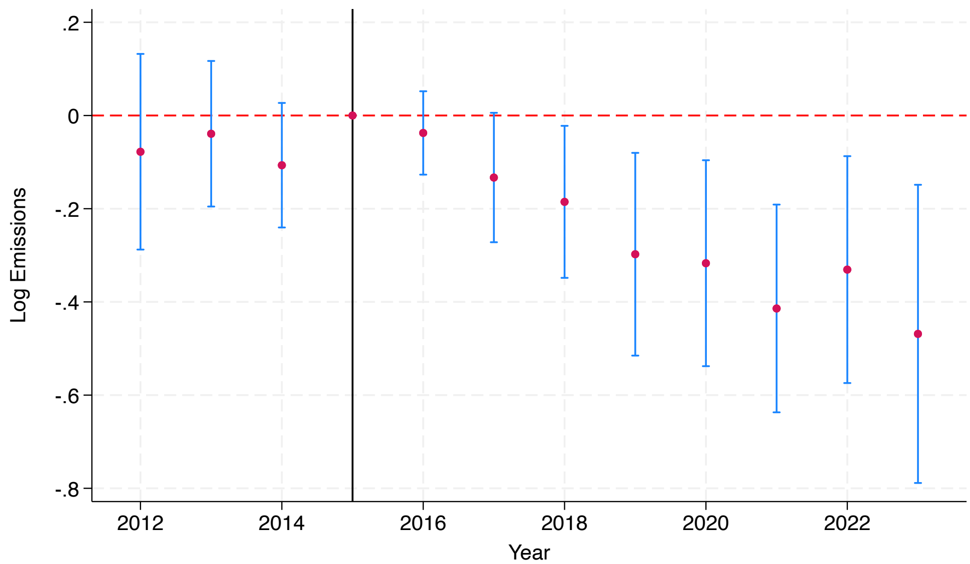
Figure 1: Trends in Log Emissions



Notes: The figure reports event-study estimates for firm-level log greenhouse gas emissions. Emissions are measured in tonnes of CO_2 equivalents and aggregate Scope 1, Scope 2, and Scope 3 emissions from Trucost. The omitted year is 2015, the year immediately before the coal criterion was first implemented. The sample includes firms divested or placed under monitoring by the Oil Fund in 2016 and control firms from the Oil Fund's 2015 portfolio operating in sectors where the coal criterion had bite. The regression includes firm fixed effects, NAICS 2-digit industry-year fixed effects, and headquarters-country-year fixed effects. Vertical bars report 95% confidence intervals based on standard errors clustered at the firm level.

Table 1: Main Results

Panel A: Effects on Emissions

	Corporate Emissions *Full sample*	Satellite Measures *Coal power plant owners*			
	(1) Log Emissions	(2) Log $PM_{2.5}$	(3) Log NO_2	(4) Log $PM_{2.5}$	(5) Log NO_2
Post $*$ Treated	-0.198*	-0.151*	-0.114	-0.131***	-0.074***
	(0.103)	(0.085)	(0.074)	(0.027)	(0.017)
Level of analysis	Firm	Firm	Firm	Coal Plant	Coal Plant
N obs	20707	1290	878	6090	4431
N Firms	1908	115	114	115	114
Firm FE	Yes	Yes	Yes		
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes	Yes	Yes
Plant FE				Yes	Yes

Panel B: Firm-Level Operational Adjustments

	Scale	Production Structure	
	(1) Log Turnover	(2) Log Emissions Intensity	(3) Share Coal Revenues
Post $*$ Treated	-0.019	-0.191**	-0.070***
	(0.062)	(0.083)	(0.014)
N obs	21412	20157	20707
N Firms	1881	1875	1908
Firm FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes

Notes: The table reports difference-in-differences estimates of the effect of being divested or placed under monitoring by the Oil Fund in 2016 under the coal criterion. *Treated* is an indicator equal to one for firms divested or placed under monitoring in 2016; *Post* is an indicator for years after 2015. Panel A reports effects on emissions and local air pollution. Column (1) uses firm-level log greenhouse gas emissions from Trucost, measured in tonnes of CO_2 equivalents and aggregating Scope 1, Scope 2, and Scope 3 emissions. Columns (2)–(5) use satellite-based pollution measures around coal-fired power plants owned by sample firms. $PM_{2.5}$ and NO_2 are assigned to plants using annual gridded pollution data and GPS coordinates from the Global Coal Plant Tracker; pollution is computed as an area-weighted average of grid cells overlapping a one and five km buffer around each facility, respectively. Columns (2)–(3) aggregate these plant-level measures to the firm level among coal power plant owners, while Columns (4)–(5) use plant-level observations. Panel B reports firm-level operational outcomes. Turnover is total firm revenue. Emissions intensity is total Trucost greenhouse gas emissions divided by turnover. Share coal revenues is the share of firm revenues derived from coal mining and coal-fired power generation. Industry-year fixed effects are defined at the NAICS 2-digit level. All specifications include firm fixed effects, industry-year fixed effects, and headquarters-country-year fixed effects; plant-level specifications additionally include plant fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively. Log emissions and Panel B outcomes cover 2012–2023; $PM_{2.5}$ and NO_2 are available only until 2022 and 2019, respectively.

Table 2: Investors

Panel A: Counts

	(1) Investors	(2) Constrained Investors	(3) Unconstrained Investors
Post ∗ Treated	-0.072*	-0.123***	0.015
	(0.043)	(0.039)	(0.053)
N obs	18898	18792	18813
N Firms	1718	1710	1711
Firm FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes

Panel B: Flows

	Log Exit Rates			Log Entry Rates		
	(1) Investors	(2) Constrained Investors	(3) Unconstrained Investors	(4) Investors	(5) Constrained Investors	(6) Unconstrained Investors
Post ∗ Treated	0.001	0.033**	-0.027*	0.013	0.022	0.029**
	(0.007)	(0.015)	(0.016)	(0.009)	(0.018)	(0.014)
N obs	18585	17368	17992	18643	17505	18059
N Firms	1717	1707	1705	1717	1706	1706
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Concentration

	Log Group Shares		Log Top 10		
	(1) Constrained Investors	(2) Unconstrained Investors	(3) Investors	(4) Constrained Investors	(5) Unconstrained Investors
Post ∗ Treated	-0.009	0.013	-0.033	-0.124*	-0.039
	(0.010)	(0.014)	(0.037)	(0.073)	(0.111)
N obs	18648	18645	18451	6311	10315
N Firms	1717	1717	1714	840	1148
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes	Yes	Yes

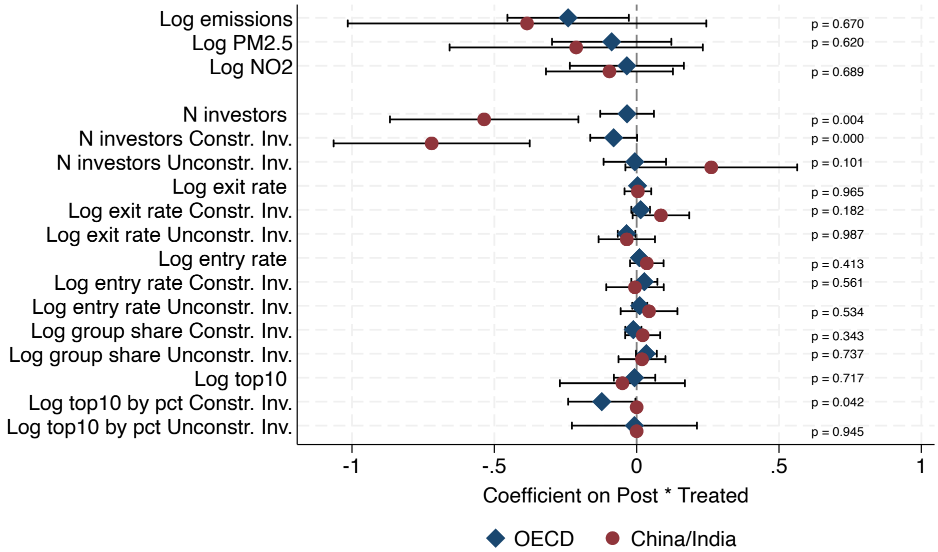
Notes: The table reports difference-in-differences estimates for investor ownership outcomes constructed from LSEG Workspace ownership data. *Treated* is an indicator equal to one for firms divested or placed under monitoring by the Oil Fund in 2016; *Post* is an indicator for years after 2015. The Oil Fund itself is excluded from all investor-ownership outcomes. Panel A reports investor-count outcomes: the total number of investors holding a firm's equity in a given year, the number of constrained investors, and the number of unconstrained investors. Constrained investors are institutional owners more exposed to public or mandate-based scrutiny, including pension funds, endowments, foundations, government agencies, and sovereign wealth funds. Unconstrained investors include individuals, family offices, corporations, and insiders. Panel B reports investor-flow outcomes: the exit rate is the number of previous-year shareholders that no longer hold the firm's equity in the current year divided by the number of previous-year shareholders, and the entry rate is the number of new shareholders in the current year divided by the number of current-year shareholders. Panel C reports ownership concentration outcomes: group shares measure the ownership share held by the relevant investor group, while top-10 outcomes measure the ownership share held by the firm's ten largest investors, overall or within the relevant group. Investor-count outcomes in Panel A are estimated using Poisson pseudo-maximum likelihood regressions, which are suited to non-negative count variables (Correia *et al.*, 2020). All outcomes are measured annually. Industry-year fixed effects are defined at the NAICS 2-digit level. All specifications include firm fixed effects, industry-year fixed effects, and headquarters-country-year fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Table 3: Financial Indicators and Valuation Ratios

	(1) Monthly Returns	(2) Log P-to-EBITDA	(3) Log Market Capitalization	(4) Log Net Assets
Post ∗ Treated	0.004*	-0.139**	-0.166**	-0.184***
	(0.002)	(0.067)	(0.084)	(0.057)
N obs	238937	17723	202938	20144
N Firms	1714	1733	1582	1779
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE		Yes		Yes
Country HQ-Year FE		Yes		Yes
Industry-Month FE	Yes		Yes	
Country HQ-Month FE	Yes		Yes	

Notes: The table reports difference-in-differences estimates for financial outcomes. *Treated* is an indicator equal to one for firms divested or placed under monitoring by the Oil Fund in 2016; *Post* is an indicator for periods after 2015. Column (1) reports monthly stock returns, measured as the monthly percentage change in the firm's stock price. Column (2) reports the price-to-EBITDA ratio, defined as the firm's market price relative to earnings before interest, taxes, depreciation, and amortization. Column (3) reports log market capitalization, measured monthly. Column (4) reports log net assets, defined as total assets minus liabilities and measured annually. Columns (1) and (3) use monthly data and include firm fixed effects, NAICS 2-digit industry-month fixed effects, and headquarters-country-month fixed effects. Columns (2) and (4) use annual data and include firm fixed effects, NAICS 2-digit industry-year fixed effects, and headquarters-country-year fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Figure 2: Declining Coal Industry?



Notes: The figure reports coefficients on *Post* × *Treated* from separate difference-in-differences regressions estimated for firms headquartered in OECD countries and for firms headquartered in China or India. *Treated* is an indicator equal to one for firms divested or placed under monitoring by the Oil Fund in 2016; *Post* is an indicator for periods after 2015. Log emissions are firm-level Trucost greenhouse gas emissions, measured in tonnes of CO_2 equivalents and aggregating Scope 1, Scope 2, and Scope 3 emissions. Log $PM_{2.5}$ and log NO_2 are satellite-based pollution measures around coal-fired power plants owned by sample firms. Investor-count outcomes measure the number of investors holding a firm's equity in a given year, excluding the Oil Fund itself. Constrained investors include pension funds, endowments, foundations, government agencies, and sovereign wealth funds; unconstrained investors include individuals, family offices, corporations, and insiders. Exit rates are computed as the number of previous-year shareholders that no longer hold the firm's equity in the current year divided by the number of previous-year shareholders; entry rates are computed as the number of new shareholders in the current year divided by the number of current-year shareholders. Group-share outcomes measure the ownership share held by the relevant investor group, and top-10 outcomes measure ownership concentration among the firm's ten largest investors overall or within the relevant group. Horizontal bars report 95% confidence intervals. For each outcome, the reported p-value tests equality between the OECD and China/India coefficients. Investor-count outcomes are estimated using Poisson pseudo-maximum likelihood regressions (Correia *et al.*, 2020). All specifications include the same fixed effects as the corresponding baseline regressions, with standard errors clustered at the firm level.

Online Appendix

A Additional Figures and Tables

This appendix reports supporting analyses for Section 5. Table A1 examines whether our main finding on emissions—particularly, Table 1, Panel A Column 1—is sensitive to measurement, sample restrictions, or treatment coding.

Column 1 restricts the emissions outcome to Scope 1–2 only. This addresses concerns that Scope 3 is noisier and more dependent on the imputation method (Busch *et al.*, 2022); it also avoids the value-chain double counting that matters for aggregate benchmarking.

Column 2 similarly restricts the outcome to Scope 2–3 emissions, to address firm boundary concerns: if reported emissions fell mainly because dirty activities were outsourced or shifted outside the firm, purchased-energy and value-chain emissions should not fall.

Column 3 restricts the analysis in Table 1–Panel A–Column 1 to only coal-fired power plant owners. We find similar treatment effects on emissions in this subsample.

Column 4 implements the same analysis as Table 1–Panel A–Column 2, but restricting the temporal coverage of $PM_{2.5}$ to until 2019 only, to mimic the data availability of NO_2.

Columns 5–6 re-estimates our main findings with different specifications. Column 5 adds "not yet treated" firms ($N = 11$) to the control group. Column 6 uses the "mechanical" definition of treatment, where all firms above the 30% threshold are classified as treated. The estimates remain negative and close to baseline across these variants.

Table A2 estimates the impact of the Oil Fund's divestment and monitoring separately. While these two interventions differ in whether the Oil Fund sold its stake, they both publicly identify firms as coal-exposed under the fund's ethical guidelines. In addition, the estimated effects are negative for both interventions and statistically indistinguishable. Thus, we pool divestment and monitoring throughout the study.

Table A3 re-estimates the results on coal revenues from Table 1–Panel B–Column 3, in subsamples with greater baseline coal exposure. This addresses the concern that our main estimate reflects the limited scope for many control firms (which have little or no coal revenue), to reduce coal-related activity. The estimates remain negative, statistically significant, and economically large, supporting the interpretation that treated firms shift away from coal-related activities after the Oil Fund's coal exit.

Table A1: Alternative Specifications for Emissions Analysis

	Full sample		Coal Plant Owners		Controls incl. not-yet-treated firms	Mechanical Treatment Def.
	(1) Log Scope 1+2	(2) Log Scope 2+3	(3) Log Emissions	(4) Log $PM_{2.5}$	(5) Log Emissions	(6) Log Emissions
Post ∗ Treated	-0.205*	-0.151*	-0.191	-0.092	-0.198*	
	(0.120)	(0.081)	(0.163)	(0.074)	(0.102)	
Post ∗ Above 30%						-0.201**
						(0.097)
N obs	20707	20707	1150	878	20782	20707
N Firms	1908	1908	101	114	1919	1908
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports alternative difference-in-differences specifications for our main emissions outcomes (see Table 1–Panel A). Here, Columns 1–2 use alternative ways of aggregating Trucost GHG emissions outcomes. Column 1 uses log(Scope 1+Scope 2), where Scopes 1 and 2 capture direct and purchased-energy emissions, respectively. Column 2 uses log(Scope 2+Scope 3), where Scope 3 captures value-chain emissions. Columns 3–4 restrict the sample to firms owning coal-fired power plants. Column 3 estimates effects on firm-level Trucost-based emissions in this subsample. Column 4 uses satellite-derived $PM_{2.5}$ around those firms' coal-fired power plants, and restricts the data to years before 2020 to mimic the NO_2 data availability. Column 5 includes "not-yet-treated" units ($N = 11$)—i.e., firms divested or monitored after 2016 under the coal criterion—as part of the control group. Column 6 defines treatment mechanically, classifying all firms above the coal criterion's 30% coal-exposure threshold as treated, rather than the Oil Fund's actual 2016 divestment or monitoring decisions. All specifications include firm fixed effects, NAICS 2-digit industry-year fixed effects, and headquarters-country-year fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Table A2: Divestment vs. Monitoring Effects on Emissions

	Corporate Emissions *Full sample*	Satellite Measures *Coal power plant owners*			
	(1) Log Emissions	(2) Log $PM_{2.5}$	(3) Log NO_2	(4) Log $PM_{2.5}$	(5) Log NO_2
Post * Divestment	-0.187*	-0.137	-0.114	-0.115***	-0.075***
	(0.111)	(0.087)	(0.075)	(0.022)	(0.017)
Post * Monitoring	-0.268**	-0.236	-0.114	-0.033	-0.036
	(0.107)	(0.153)	(0.116)	(0.036)	(0.029)
Level of analysis	Firm	Firm	Firm	Coal Plant	Coal Plant
N obs	20707	1290	878	6090	4431
N Firms	1908	115	114	115	114
p-val: Divestment = Monitoring	0.496	0.506	1.000	0.038	0.190
Firm FE	Yes	Yes	Yes		
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes	Yes	Yes
Plant FE				Yes	Yes

Notes: The table reports difference-in-differences estimates separately for firms divested by the Oil Fund in 2016 and firms placed under monitoring in 2016 under the coal criterion. Specifications correspond to Table 1–Panel A. Column 1 uses firm-level log GHG emissions from Trucost, measured in tonnes of CO_2e and aggregating Scope 1–3 emissions. Columns 2–5 use satellite-derived $PM_{2.5}$ and NO_2 concentrations around coal-fired power plants owned by sample firms. Columns 2–3 aggregate plant-level pollution to the firm level among coal power plant owners, while Columns 4–5 use plant-level observations. The reported p-value tests equality between the divestment and monitoring coefficients. All specifications include the same fixed effects as in Table 1, Panel A. Robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Table A3: Share Coal Revenue Effects Across Alternative Samples

	Baseline	$Share_{2015}$>0	$Share_{2015}$>10%	Coal power plant owners
	(1) Share Coal Revenues	(2) Share Coal Revenues	(3) Share Coal Revenues	(4) Share Coal Revenues
Post $*$ Treated	-0.070***	-0.064***	-0.050**	-0.094***
	(0.014)	(0.017)	(0.024)	(0.022)
N obs	20707	1272	767	1150
N Firms	1908	115	70	101
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes	Yes

Notes: The dependent variable is the share of firm revenues derived from coal mining and coal-fired power generation, constructed from Trucost revenue-segment data. Column 1 reports the baseline specification (from Table 1–Panel B–Column 3). Column 2 restricts the sample to firms with positive coal revenues in 2015. Column 3 restricts the sample to firms with coal revenues above 10% in 2015. Column 4 restricts the sample to firms owning coal-fired power plants. All specifications include firm fixed effects, NAICS 2-digit industry-year fixed effects, and headquarters-country-year fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

B Data Sources and Sample Statistics

This appendix provides further details on data sources.

NBIM. From NBIM's website, we obtain two key pieces of information that we use to construct our treatment and control groups. First, we collect data from NBIM press releases, which identify firms that were either divested from or placed under monitoring by the Oil Fund, along with the dates these decisions were publicly announced. Note that the public press release date differs from the actual sell date of the securities (which are sold before the press release). Thus, "treatment" in our study refers to the public announcement. Second, we collect the lists of the Oil Fund's holdings as of December 31 for each year from 2011 onwards. These lists report the market value of the fund's investment in each company, as well as its equity and voting shares.

Trucost. We obtain firm-level data on environmental outcomes from Trucost, the provider with broadest coverage of CO2 emissions data (Busch *et al.*, 2022). In line with the GHG Protocol, Trucost estimates corporate emissions by integrating data from company financial reports, environmental disclosures, and publicly available sources. Trucost's coverage of firms has expanded over time. While coverage in 2015, the year prior to the introduction of the coal criterion, is not complete, it remains high in the sectors most relevant to our analysis (e.g., utilities). Our primary variables of interest are total GHG emissions (expressed in tonnes of CO_2 equivalents); emission intensity, computed as the ratio of emissions and turnover (in millions of USD); and revenues deriving from coal mining and coal power generation. We use ISINs to merge Trucost data to Orbis data.

Orbis. We collect financial data from Bureau van Dijk's (BvD) Orbis database. For each company, we extract yearly information on a wide range of variables, including sector, age, equity, and assets. We also extract 'earnings before interest, taxes, amortization and depreciation' (EBITDA) and international financial identifiers, such as ISIN and LEI.[21] To collect such data, we rely on the BvD's Batch Search proprietary algorithm. To this algorithm, we provide firms' name and country of headquarters as specified in NBIM data.

Global Energy Monitor (GEM). We use GEM's 2024 release of Global Coal Plant Tracker (GCPT), which provides global data on coal-fired power units of 30 megawatts or more,

[21] The ISIN uniquely identifies a specific security, such as a stock or bond. It is used globally to facilitate cross-border trading and settlement. The LEI is a code that uniquely identifies legal entities participating in financial transactions.

including very unit currently operating, every proposed unit since 2010, and every unit retired since 2000. This plant-level dataset includes information on operating status over time, generation capacity, facility characteristics, GPS coordinates, and ownership (current or as of the last year before retirement) (Global Energy Monitor, 2024a). We also use GEM's Global Ownership Tracker to map the full ownership structure of each plant (Global Energy Monitor, 2024b).

We link power plants to companies in our dataset using international financial identifiers—ISIN and LEI—where available, or via name matching. To identify the "owners" of a given plant, we consider both the direct owners (i.e., those at the first level of the ownership structure) as well as every company that indirectly controls more than 10% of a plant.

Satellite data. We use the $0.01° \times 0.01°$-resolution data for PM2.5 and NO_2 from van Donkelaar *et al.* (2016) and Cooper *et al.* (2020), respectively. For each plant, we assign pollution as the area-weighted average of grid cells intersecting a 1 km buffer for $PM_{2.5}$ and a 5 km buffer for NO_2. We also use $PM_{2.5}$ data at a $0.1° \times 0.1°$ resolution from the Joint Research Centre of the European Commission's Emissions Database for Global Atmospheric Research—EDGAR (Muntean *et al.*, 2018)).

Investor Ownership. LSEG (formerly Refinitiv) provides shareholder information via its Ownership database. These data are compiled from regulatory filings, such as SEC filings in the United States, as well as institutional, company, and fund reports. We use these data to construct annual measures of investor ownership for each firm—excluding the Oil Fund itself from all such outcomes.

First, we count the number of investors holding a firm's equity in a given year. We also distinguish between constrained and unconstrained investors. Constrained investors are institutional owners that are plausibly more exposed to public or mandate-based scrutiny, consisting of pension funds, endowments, foundations, government agencies, and sovereign wealth funds. Unconstrained investors consist of individuals, family offices, corporations, and insiders.

Second, we construct investor-flow outcomes: exit rate and entry rate. The exit rate is defined as the number of previous-year shareholders that no longer hold the firm's equity in the current year, divided by the number of previous-year shareholders. The entry rate is defined as the number of new shareholders in the current year, divided by the number of current-year shareholders.

Finally, we construct measures of ownership concentration. "Group share" is the fraction of firm's equity held by the relevant investor group (constrained or unconstrained investors). Top-10 ownership measures are defined as the ownership share held by the firm's ten largest investors, either overall or within the relevant investor group.

A caveat with LSEG data is that the thresholds at which regulatory filing requirements become binding differ across jurisdictions. Further, the timing of ownership filings and reports vary: some are annual, while others are quarterly or event-driven. To harmonize these data into an annual outcomes, we retain the latest observation for each investor-firm pair within a given year. This observation may occur before year-end (e.g., in June, if the investor does not have regulatory filings again later that year). As a result, our outcomes should be interpreted as measures based on a set of unique investors observed in LSEG during the year, rather than from a complete census of shareholders at year-end.

Our study sample is an unbalanced panel of 1,908 firms covered in 2012–2023. Sample firms operate in sectors where the coal criterion had "bite"—meaning that in each of these sectors, at least one firm has been divested or monitored under the criterion since 2016.

Not all outcome variables are available for every firm. This reflects several limitations. First, not all firms are covered by Trucost or have available environmental variables in all years. Firms in energy-intensive sectors (Mining, Utilities, and Manufacturing) tend to report more frequently, and are therefore overrepresented in the final sample. Second, not all firms operate coal-fired power plants. Consequently, satellite-derived emissions are available only for a subset of firms, again skewed toward energy-intensive industries. To preserve statistical power, we allow the estimation sample to vary across specifications.

Of the firms in our sample, 1,850 belong to the control group and 58 to the treated group. Among the treated group, 50 were divested and 8 were monitored by the Oil Fund. Treated firms are concentrated in the Mining and Utilities sectors, which account for 21% and 76% of treated firms, respectively. To understand how our study sample compares to the broader Oil Fund portfolio, Table B1 reports sector-level coverage in the fund's 2015 portfolio. The named sectors are those we use in our study, while the "Other" sector collects portfolio firms outside these sectors and is shown for completeness. Firm-level environmental and financial data are available for roughly two thirds of the Mining and Utilities firms in the portfolio, covering 70% and 87% of the dollars invested by the fund in firms in those sectors. By contrast, satellite-based environmental data are available for only 2% of Mining firms and 21% of Utilities firms, but still cover 4% and 43% of the fund's invested holdings in those sectors, respectively.

Geographically, the sample is concentrated in a small number of countries: the United States (21%), Japan (15%), the United Kingdom (7%), Taiwan (6%), and Canada (6%) together account for 57% of all firms. Treated firms are similarly concentrated, with the largest shares located in the United States (33%), China (12%), Japan (12%), India (9%), Canada (5%) and Hong Kong (5%).

Table B1: Study Sample Coverage of the Oil Fund's 2015 Portfolio

	N_{2015}					Market $Value_{2015}$		
Industry	N	Firm-level (%)	Treated (%)	Plant-level (%)	Treated Plant-level (%)	Mkt Value (mln)	Firm-level (%)	Plant-level (%)
21	337	63.2	3.6	2.1	.6	33.3	69.8	3.7
22	268	66.8	16.4	21.3	10.8	57.5	86.8	42.5
23	265	33.6	.4	0	0	26.6	62.1	0
32	971	34.8	0	2.5	0	84	43.7	2.4
33	1827	40.1	0	.9	0	52.5	72.9	1.9
42	292	36.3	.3	1	0	29.9	58.4	6.5
51	380	49.5	0	0	0	89.2	88.3	0
55	454	13.9	0	.7	0	93.1	5.2	5.6
Other	3422	0	0	0	0	52.1	0	0

Notes: The table describes, by sector, how much of the Oil Fund's 2015 portfolio is covered in our study. The left (N_{2015}) and right (Market $Value_{2015}$) panels describe the number of firms in the Oil Fund's 2015 portfolio and their market value, respectively. The column N reports the number of portfolio firms with available data in each sector. Industry codes are NAICS 2-digit sectors: 21 = Mining; 22 = Utilities; 23 = Construction; 32 = Manufacturing; 33 = Manufacturing; 42 = Wholesale Trade; 51 = Information; 55 = Management of Companies and Enterprises. The named sectors are those in which the coal criterion had bite, defined as sectors with at least one firm divested from or placed under monitoring under the coal criterion; these sectors form the basis of the baseline analysis sample. The row "Other" collects portfolio firms outside these sectors and is shown for completeness; these firms are not included in the baseline analysis sample. In the left panel, firm-level coverage refers to the share of firms with environmental and financial data used in the firm-level analysis, primarily from Trucost and Orbis; plant-level coverage refers to the share of firms matched to coal-fired power plants in the Global Coal Plant Tracker and therefore observed in the satellite-based pollution analysis. Treated indicates firms divested or placed under monitoring by the Oil Fund in 2016 under the coal criterion. In the right panel, market value, reported in million USD, is the Oil Fund's 2015 holdings value in each sector. Firm-level coverage refers to the share of market value invested in each sector that is covered by environmental and financial data and used in the firm-level analysis. Plant-level coverage refers to the share of market value invested in each sector that is matched to coal-fired power plants in the Global Coal Plant Tracker and therefore observed in the satellite-based pollution analysis.

Table B2 reports the main environmental and financial characteristics of treated and control firms in 2015. Column (3) presents differences between the two groups after controlling for industry and country fixed effects. Relative to control firms, treated firms are on average more polluting, more emissions-intensive, and more dependent on coal-related activities. Ownership structures are broadly similar across the two groups, with the only statistically significant difference concerning the number of unconstrained investors.

Table B2: Descriptive Statistics

Variable	(1) Control	(2) Treated	(3) Δ
Log Emissions	13.643	16.785	2.390***
	(1.904)	(1.363)	(0.224)
Log $PM_{2.5}$	3.507	4.469	0.444
	(0.913)	(1.640)	(0.276)
Log NO_2	2.394	3.254	0.459*
	(0.887)	(1.363)	(0.232)
Log Turnover	7.869	8.236	0.262
	(1.516)	(1.139)	(0.167)
Log Emissions Intensity	5.815	8.550	2.096***
	(1.146)	(0.760)	(0.137)
Share Coal Revenues	0.003	0.518	0.505***
	(0.025)	(0.286)	(0.037)
Investors	299.919	306.833	-44.680
	(352.626)	(276.873)	(30.697)
Constrained Investors	11.329	14.778	-0.219
	(14.546)	(16.589)	(1.431)
Unconstrained Investors	21.945	20.759	-3.761**
	(23.584)	(23.400)	(1.796)

Notes: The table reports descriptive statistics for treated and control firms in 2015, the year before the coal criterion was first implemented. Columns (1) and (2) report mean values for control and treated firms, respectively. Column (3) reports the adjusted difference between treated and control firms, estimated controlling for NAICS 2-digit industry fixed effects and headquarters-country fixed effects. Log emissions are firm-level greenhouse gas emissions from Trucost, measured in tonnes of CO_2 equivalents and aggregating Scope 1, Scope 2, and Scope 3 emissions. Log $PM_{2.5}$ and log NO_2 are satellite-based pollution measures around coal-fired power plants owned by sample firms. Turnover is total firm revenue. Emissions intensity is total Trucost greenhouse gas emissions divided by turnover. Share coal revenues is the share of firm revenues derived from coal mining and coal-fired power generation. Investor-count variables are constructed from LSEG Workspace ownership data and exclude the Oil Fund itself. Constrained investors are pension funds, endowments, foundations, government agencies, and sovereign wealth funds; unconstrained investors are individuals, family offices, corporations, and insiders. Robust standard errors are reported in parentheses. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

C Benchmarking Aggregate Emissions Reductions

This appendix describes how we benchmark the magnitude of the estimated emissions reductions against policy-relevant emissions aggregates. The main text focuses on the proportional treatment effect on firms' corporate GHG emissions. Here we clarify how we translate this estimate into aggregate emissions reductions, why we use Scope 1–2 emissions for this calculation, and how the resulting number compares to the EU Emissions Trading System (EU ETS) and total EU GHG emissions.

C.1 Firm-Level Effects and Aggregate Benchmarking

Our baseline emissions outcome is total corporate CO_2e, combining Scope 1, Scope 2, and Scope 3 emissions. We use this measure in the main analysis because it corresponds most closely to the broad emissions footprint relevant to investors (Cenedese *et al.*, 2024). Investors assessing climate exposure may care not only about a firm's direct emissions, but also about emissions embedded in purchased energy and value-chain activities.

However, Scope 1–3 emissions are not generally additive across firms. One firm's Scope 3 emissions may correspond to another firm's Scope 1 or Scope 2 emissions. Aggregating Scope 1–3 reductions across firms can therefore double-count the same physical emissions reductions. The GHG Protocol explicitly cautions that Scope 3 emissions should not be aggregated across companies to determine total emissions in a region (GHG Protocol, 2024). For this reason, we use Scope 1–2 emissions, rather than Scope 1–3 emissions, when constructing aggregate benchmarks.

This choice does not eliminate all possible double counting. Scope 2 emissions capture purchased electricity, steam, heat, and cooling, and may therefore correspond to the Scope 1 emissions of electricity or heat producers. In our setting, this residual concern is likely limited because treated firms are overwhelmingly concentrated in sectors where direct emissions are central: utilities and mining account for 96% of treated firms in the emissions regression sample. Scope 1–2 emissions therefore provide a more appropriate benchmark for aggregate magnitudes than Scope 1–3 emissions, while remaining closely related to the corporate emissions footprint studied in the main analysis.

C.2 Back-of-the-Envelope Calculation

We estimate the treatment effect on log Scope 1–2 emissions using the same specification as in the main analysis. The estimated coefficient is

$$\hat{\beta}_{S1+S2} = -0.205,$$

which corresponds to a proportional reduction of

$$1 - \exp(\hat{\beta}_{S1+S2}) = 1 - \exp(-0.205) = 0.185.$$

Thus, the implied reduction in Scope 1–2 emissions is approximately 18.5%.

Among the 58 treated firms in the regression sample, mean Scope 1–2 emissions in 2015 are 39.8 million tonnes of CO_2e. Total treated Scope 1–2 emissions in 2015 are therefore:

$$58 \times 39.8 \text{ MtCO}_2\text{e} = 2.31 \text{ GtCO}_2\text{e}.$$

Applying the estimated proportional reduction gives an annual avoided-emissions benchmark of:

$$2.31 \text{ GtCO}_2\text{e} \times 0.185 = 0.428 \text{ GtCO}_2\text{e},$$

or approximately 428 million tonnes of CO_2e per year. Over the eight post-treatment years from 2016 to 2023, this corresponds to:

$$0.428 \times 8 = 3.42 \text{ GtCO}_2\text{e}.$$

This calculation scales the estimated treatment effect only within the set of firms targeted by the Oil Fund's 2016 coal criterion. It is not an extrapolation to all coal producers or to global emissions.

C.3 Comparison to Colmer *et al.* (2025)

A natural benchmark is Colmer *et al.* (2025), who estimate the effect of the EU ETS on regulated French manufacturing firms. Their setting is substantially narrower than ours: their main analysis concerns roughly 150 regulated French manufacturing firms, and their emissions outcome is direct CO_2 emissions from energy use. They estimate a proportional reduction of 14–16%, and an aggregate annual reduction of 5.4 million tonnes of CO_2 among regulated French manufacturing firms.

Our proportional Scope 1–2 estimate of 18.5% is therefore close to their 14–16% estimate. The aggregate level effect is much larger because the treated firms in our setting are much larger emitters. Directly comparing aggregate estimates, our implied annual Scope 1–2 reduction of 428 $MtCO_2e$ is about 79 times larger than the 5.4 $MtCO_2$ annual reduction estimated by Colmer *et al.* (2025). This direct comparison is informative about scale, but not one-for-one: their estimate applies to French manufacturing firms, while ours applies to large, coal-exposed global firms targeted by the Oil Fund.

We therefore also consider a mechanical EU ETS-wide benchmark. EU ETS stationary emissions in 2005 were approximately 2,093 $MtCO_2e$. Applying Colmer *et al.* (2025)'s 14–16% proportional effect to this EU ETS-wide emissions base implies:

$$2{,}093 \times 0.14 = 293 \text{ MtCO}_2\text{e}$$

to

$$2{,}093 \times 0.16 = 335 \text{ MtCO}_2\text{e}.$$

Our implied annual Scope 1–2 reduction of 428 $MtCO_2e$ is therefore about 1.3–1.5 times this scaled EU ETS benchmark.

C.4 Comparison to EU ETS and EU-Wide Emissions

We also compare the implied cumulative Scope 1–2 reduction to cumulative EU ETS and EU-wide GHG emissions over the same period as our post-treatment window, 2016–2023. This comparison is useful because our back-of-the-envelope calculation is cumulative over those years.

According to the EEA EU ETS Union Registry data, cumulative EU ETS verified emissions over 2016–2023 were approximately:

$$11.82 \text{ GtCO}_2\text{e}.$$

Our cumulative Scope 1–2 benchmark of 3.42 $GtCO_2e$ therefore corresponds to:

$$3.42/11.82 = 0.29,$$

or about 29% of cumulative EU ETS verified emissions over 2016–2023 (EEA, 2026b).

For total EU GHG emissions, the EEA greenhouse gases data viewer reports cumulative EU-27 net GHG emissions of approximately:

$$26.13 \text{ GtCO}_2\text{e}$$

over 2016–2023. Our cumulative Scope 1–2 benchmark therefore corresponds to:

$$3.42/26.13 = 0.131,$$

or about 13% of cumulative total EU GHG emissions over the same period (EEA, 2026a).

These comparisons should be interpreted as scale benchmarks rather than exact economy-wide accounting exercises. They indicate that the estimated reductions among targeted firms are economically large relative to major climate-policy aggregates, even when using the more conservative Scope 1–2 emissions measure for aggregation.

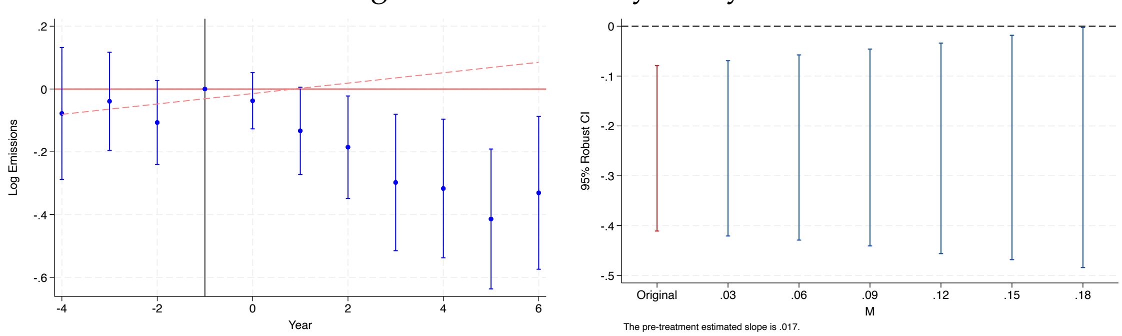
D Assessing the Parallel Trend Assumption through a "More Honest" Approach

We implement the "honest" approach to parallel trends proposed by Rambachan & Roth (2023), which does not require the absence of pre-treatment trends. Instead, it tests whether post-treatment outcomes significantly deviate from a continuation of pre-treatment trends. The framework also allows for bounded violations of parallel trends, quantified by a parameter M, which captures the degree to which post-treatment trends are allowed to depart non-linearly from the pre-treatment path.

Figure D1 presents these diagnostics. The left panel replicates the event study plot from Figure 1, overlaid with a dashed line representing a linear extrapolation of the pre-treatment trend into the post-treatment period (Dobkin *et al.*, 2018). Clearly, post-treatment point estimates lie outside the extrapolated trend, supporting the validity of the treatment effect.

The right panel reports the estimated pre-treatment slope and assesses whether post-treatment effects remain significant under various values of M. The slope estimate for emissions is 0.017. A value of $M = 0.18$ implies that the result remains significant even if the parallel trends assumption is violated by up to roughly $\pm 1000\%$ of the average pre-treatment deviation.

Figure D1: Sensitivity Analysis



Notes: The figure reports sensitivity diagnostics for the parallel-trends assumption using the "honest" difference-in-differences approach of Rambachan & Roth (2023). The outcome is firm-level log greenhouse gas emissions from Trucost, measured in tonnes of CO_2 equivalents and aggregating Scope 1, Scope 2, and Scope 3 emissions. The left panel reports event-study estimates for treated and control firms, with 95% confidence intervals; the red dashed line extrapolates the linear pre-treatment trend into the post-treatment period following Dobkin *et al.* (2018). The right panel reports robust confidence intervals under alternative values of M, where M bounds the extent to which post-treatment violations of parallel trends can deviate non-linearly from the pre-treatment path. The pre-treatment estimated slope is 0.017. All specifications include firm fixed effects, NAICS 2-digit industry-year fixed effects, and headquarters-country-year fixed effects. Standard errors are clustered at the firm level.

E Robustness Checks

We first examine whether our findings may be confounded by broader changes in the economic and policy environment around 2015. This concern is natural because the Oil Fund's coal criterion was introduced at a time of growing climate-policy attention, including the 2015 Paris Agreement, and rising investor concern about carbon-intensive firms. Evidence of green/brown repricing around this period reinforces this concern (Pástor *et al.*, 2022). If such contemporaneous forces independently induced high-emissions firms to reduce emissions, they could generate patterns similar to those we attribute to the coal criterion. At the same time, this distinction is not entirely mechanical: if the market was ripe for a shift in expectations about coal and the Oil Fund's intervention helped coordinate that shift, such a mechanism is part of our focal-point interpretation rather than a separate confound.

Table E1 reports two checks, moving from the broader concern to a more specific policy-based version of it. First, we test whether the results reflect a general post-2015 decline among dirty firms, rather than an adjustment specific to firms targeted under the coal criterion. We construct placebo groups of dirty non-coal controls: untreated firms with zero coal revenue share in 2015 but with baseline emissions above the 25th, 50th, or 75th percentile of treated firms' 2015 emissions distribution. Columns (1)–(3) include both the baseline treatment indicator and the corresponding dirty non-coal post indicator. Dirty non-coal firms also reduce emissions after 2015, consistent with broader pressure on high-emissions firms. However, the treated-firm coefficients remain close to the baseline estimate and are larger in magnitude than the dirty non-coal coefficients across all three definitions. The equality tests are not statistically decisive, but the point estimates suggest that broader brown-firm adjustment explains only part of the treated firms' emissions decline.

Second, we test the most direct policy-based version of the concern by asking whether the estimated emissions effect differs across countries with more or less stringent preexisting environmental policy. We use the OECD Environmental Policy Stringency (EPS) index, a widely used measure in policy analysis (Botta & Kozluk, 2014; Kruse *et al.*, 2022). The EPS is a country-level composite index covering 34 OECD members and 13 policy instruments related primarily to climate change and air pollution. It aggregates three equally weighted sub-indices capturing (i) market-based instruments (e.g. taxes, permits, certificates), (ii) non-market instruments (e.g. performance standards), and (iii) technology-support policies. We classify countries according to their 2015 EPS level and estimate separate treatment effects for firms headquartered in below- and above-median

EPS countries.[22] If our results were driven primarily by contemporaneous climate-policy changes, we would expect stronger responses in high-EPS countries.

Column (4) shows little evidence of such heterogeneity. The point estimate is if anything larger in below-median EPS countries, and the equality test does not reject equal effects across the two groups. This suggests that the emissions decline is unlikely to be driven primarily by contemporaneous policy shifts in countries with more stringent environmental regulation.

Taken together, the evidence in Table E1 suggests that contemporaneous transition pressures may have contributed to emissions reductions among dirty firms, but do not fully account for the patterns we document among treated coal firms.

We also examine whether the results are stable across three dimensions: (i) alternative specifications of the original 2015 portfolio control group, (ii) a control group based on global coal plant ownership from GEM, and (iii) alternative satellite-based emissions measures.

Table E2 presents these results, with corresponding event study figures in Figure E1. First, one concern is that restricting the sample to industries where the coal criterion had bite may be either so narrow that it does not take into account general equilibrium effects or so broad that it takes into account firms whose emissions are not really relevant. Thus, column (1) expands the control group to include all firms in the 2015 portfolio with environmental data, while column (2) restricts the sample to highly polluting industries (Mining, Utilities, and Manufacturing). In both cases, treated firms reduce emissions by about 18%, consistent with our baseline estimates. Second, in columns (3) and (4) we depart from the portfolio-based controls and construct an alternative control group using publicly listed firms with direct or indirect ownership of coal plants in the GEM dataset. The results continue to show large and statistically significant declines in pollution around treated firms' plants. Finally, in columns (5) and (6) we test sensitivity to different emissions measures. When computing emissions using a broader (five kilometer) buffer around coal plants, the estimated effect becomes slightly smaller than in our baseline: pollution captured within the larger buffer likely reflects not only the focal coal plant but also other nearby sources. Still, the coefficient in column (5) remains economically meaningful, although estimated with wider confidence intervals (p-value = 0.199). Using $PM_{2.5}$ estimates from the EDGAR dataset (Muntean *et al.*, 2018) instead shows an even larger treatment effect (column (6)).

[22] Countries with 2015 EPS below the median include Australia, Canada, Spain, and the United States; countries above the median include France, Germany, Japan, and the United Kingdom.

Table E1: Concurrent Policy Shifts and Broader Brown-Firm Trends

	Broader Brown-Firm Adjustment			Concurrent Policy Shifts
	(1)	(2)	(3)	(4)
	Log Emissions	Log Emissions	Log Emissions	Log Emissions
Post ∗ Treated	-0.204**	-0.200*	-0.198*	
	(0.104)	(0.103)	(0.103)	
Post ∗ Dirty non-coal Control [L]	-0.098***			
	(0.032)			
Post ∗ Dirty non-coal Control [M]		-0.118**		
		(0.052)		
Post ∗ Dirty non-coal Control [H]			-0.072	
			(0.051)	
Post ∗ Treated ∗ Below median EPS				-0.277**
				(0.123)
Post ∗ Treated ∗ Above median EPS				-0.127
				(0.238)
N obs	20,707	20,707	20,707	14,626
N Firms	1908	1908	1908	1359
p: Equality test	0.306	0.471	0.264	0.576
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	
Country HQ-Year FE	Yes	Yes	Yes	Yes
EPS-Industry-Year FE				Yes

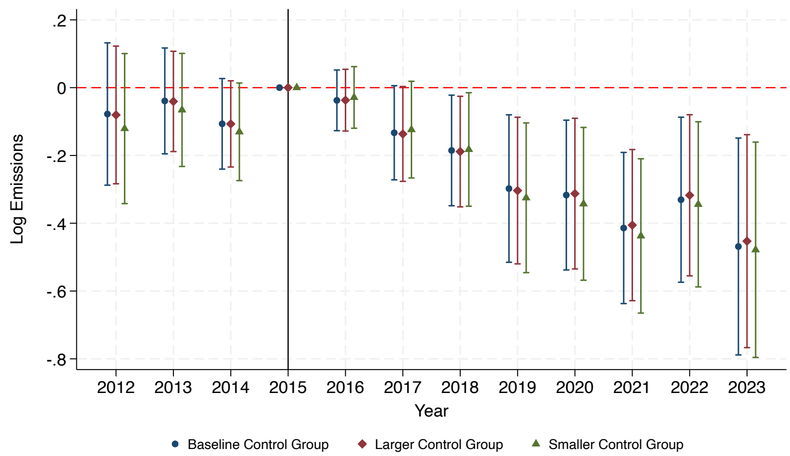
Notes: The table reports difference-in-differences estimates for firm-level log greenhouse gas emissions from Trucost. Emissions are measured in tonnes of CO_2 equivalents and aggregate Scope 1, Scope 2, and Scope 3 emissions. Columns (1)–(3) test whether the baseline emissions decline reflects a broader post-2015 decline among high-emissions firms rather than an effect specific to firms targeted under the coal criterion. These columns include separate post-treatment indicators for treated firms and for dirty non-coal placebo groups, leaving all remaining control firms as the omitted group. Dirty non-coal firms are control firms with zero coal revenue share in 2015 and 2015 emissions above the 25th, 50th, or 75th percentile of treated firms' 2015 emissions distribution, respectively. Column (4) tests whether treatment effects differ by baseline environmental policy stringency, measured using the OECD Environmental Policy Stringency index in 2015. It estimates separate treatment effects for firms headquartered in below- and above-median EPS countries. The p-values in Columns (1)–(3) test equality between the treated-firm and dirty non-coal coefficients. The p-value in Column (4) tests equality between the below- and above-median EPS treatment effects. All specifications include firm fixed effects and headquarters-country-year fixed effects. Columns (1)–(3) include NAICS 2-digit industry-year fixed effects; Column (4) includes EPS-group-by-industry-year fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Table E2: Robustness

	Larger Control Group	Smaller Control Group	All Global Plant Owners		Alternative Emissions	
	(1) Log Emissions	(2) Log Emissions	(3) Log $PM_{2.5}$	(4) Log NO_2	(5) Log $PM_{2.5}$ (5Km Radius)	(6) Log $PM_{2.5}$ EDGAR
Post $*$ Treated	-0.194*	-0.185*	-0.139***	-0.115***	-0.104	-0.212*
	(0.101)	(0.110)	(0.050)	(0.040)	(0.080)	(0.122)
N obs	30694	16960	1809	1320	1140	1140
N Firms	2833	1550	184	181	101	101
Median						
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes			Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports robustness checks using alternative control groups and alternative emissions measures. Columns (1)–(2) use firm-level log greenhouse gas emissions from Trucost, measured in tonnes of CO_2 equivalents and aggregating Scope 1, Scope 2, and Scope 3 emissions. Column (1) expands the control group to include all firms in the Oil Fund's 2015 portfolio with environmental data. Column (2) restricts the sample to highly polluting industries: Mining, Utilities, and Manufacturing. Columns (3)–(4) use an alternative control group of publicly listed firms with direct or indirect ownership of coal-fired power plants in the Global Energy Monitor data, and estimate effects on satellite-based log $PM_{2.5}$ and log NO_2 around those plants. Column (5) recomputes satellite-based $PM_{2.5}$ using a broader 5 km buffer around each coal plant rather than the baseline 1 km buffer. Column (6) uses $PM_{2.5}$ estimates from EDGAR rather than the baseline $PM_{2.5}$ data (Muntean *et al.*, 2018). All specifications include firm fixed effects, NAICS 2-digit industry-year fixed effects, and headquarters-country-year fixed effects. Robust standard errors clustered at the firm level are reported in parentheses. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Figure E1: Event Study Estimates Using Alternative Control Groups



Notes: The figure reports event-study estimates for firm-level log GHG emissions under alternative control-group definitions. Emissions are measured from Trucost and aggregate Scopes 1–3. The baseline control group consists of Oil Fund 2015 portfolio firms operating in sectors where the coal criterion had bite (corresponding to Table 1, Column 1). The larger control group includes all firms in the Oil Fund's 2015 portfolio with environmental data (corresponding to Table E2, Column 1). The smaller control group restricts the sample to highly polluting industries: Mining, Utilities, and Manufacturing (corresponding to Table E2, Column 2). The omitted year is 2015, immediately before treatment. All specifications include firm fixed effects, NAICS 2-digit industry-year fixed effects, and headquarters-country-year fixed effects. Vertical bars show 95% confidence intervals based on standard errors clustered at the firm level.

F Synthetic Difference-in-Differences

Given the presence of a panel structure and the relatively small number of treated firms, we implement a synthetic difference-in-differences (SDID) approach, following Arkhangelsky *et al.* (2021). This exercise is designed to address the concern that treated firms may differ systematically from control firms in size, coal exposure, or pre-treatment outcome trajectories. Rather than matching only on observables such as industry or firm size, SDID reweights both control firms and pre-treatment periods so that the weighted control group better reproduces treated firms' pre-treatment outcome paths. It therefore provides a useful way to assess whether our estimates are driven by comparisons between targeted coal firms and smaller or otherwise different control firms.

While SDID is well suited to settings with few treated units and rich panel data, several considerations lead us not to adopt it as our main empirical approach. First, the method requires a balanced panel, which reduces the number of firms in our main sample by roughly half. Second, the construction of a balanced panel interacts with the coverage patterns of our data providers—most notably Trucost, which tended to begin covering highly polluting firms earlier than less polluting ones. As a result, restricting attention to firms observed throughout the full sample period introduces an additional source of selection, disproportionately retaining larger and more environmentally intensive firms.

Table F1 documents these differences by comparing environmental and financial characteristics of firms in the main (unbalanced) sample used in the core analysis with those in the balanced sample used for the SDID estimation. Columns (1) and (2) report mean characteristics for each group in 2015, while Column (3) presents differences after controlling for industry (NAICS 2-digit) and country fixed effects. Firms in the balanced sample are, on average, significantly more polluting and larger. They also have a higher number of investors overall, including both constrained and unconstrained investors.

These differences highlight that the SDID analysis pertains to a selected subsample of firms, which is systematically larger, more visible to investors, and more environmentally intensive than the firms in the main sample. Consequently, while SDID is useful for addressing concerns that treated and control firms differ in their underlying pre-treatment trajectories, the resulting estimates should be interpreted as applying to this balanced subsample rather than to the full population analyzed in the main body of the paper.

Table F2 reports the SDID estimates and shows that the results are broadly consistent with those presented in the main body of the paper. Panel A indicates that treated firms experience a statistically significant reduction in total emissions following treatment. Panel B further shows that these reductions are primarily driven by technology and com-

Table F1: Balance Table

	All	Balanced	Δ
Log Emissions	13.739	14.151	0.825***
	(1.965)	(1.846)	(0.081)
Log $PM_{2.5}$	3.838	3.689	-0.047
	(1.247)	(1.005)	(0.232)
Log NO_2	2.652	2.566	-0.068
	(1.118)	(1.002)	(0.239)
Log Turnover	7.880	8.339	0.878***
	(1.507)	(1.409)	(0.063)
Log Emissions Intensity	5.901	5.830	-0.057
	(1.232)	(1.107)	(0.048)
Share Coal Revenues	0.019	0.014	-0.007
	(0.104)	(0.085)	(0.004)
Investors	300.136	381.612	148.655***
	(350.431)	(414.571)	(10.820)
Constrained Investors	11.437	14.535	5.268***
	(14.622)	(16.577)	(0.400)
Unconstrained Investors	21.907	25.018	3.856***
	(23.573)	(24.724)	(0.801)

Notes: Columns (1) and (2) report mean values for firms in the main sample and firms in the balanced sample in 2015, respectively. Column (3) shows the adjusted difference in means between (2) and (1) using industry (at NAICS 2 digit level) and country fixed effects. Robust standard errors in parentheses. ***, **, and * indicate significance at 1, 5, and 10% levels, respectively.

position effects—namely, declines in emissions intensity and a reallocation away from coal—rather than by scale effects, such as a contraction in firm size.

The estimated effects on emissions and emissions intensity are approximately twice as large as those reported in Section 5. This difference in magnitude is likely attributable to the composition of the balanced sample used for the SDID analysis, which consists of firms that are, on average, larger and more environmentally intensive. These firms plausibly have greater scope for emissions reductions. The key point for our purposes, however, is that reweighting the control group to match treated firms' pre-treatment outcome paths does not weaken the main result: if anything, the estimated environmental effects become larger.

Table F2: Main Results using SDID

Panel A: Effects on Emissions

	Corporate Emissions *Full sample*	Satellite Measures *Coal power plant owners*			
	(1)	(2)	(3)	(4)	(5)
	Log Emissions	Log $PM_{2.5}$	Log NO_2	Log $PM_{2.5}$	Log NO_2
Post ∗ Treated	-0.421***	-0.140**	-0.079	-0.111***	-0.003
	(0.122)	(0.055)	(0.050)	(0.013)	(0.008)
Level of analysis	Firm	Firm	Firm	Coal Plant	Coal Plant
N obs	11316	1067	824	4774	3768
Firm FE	Yes	Yes	Yes		
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes	Yes	Yes
Plant FE				Yes	Yes

Panel B: Firm-Level Operational Adjustments

	Scale	Production Structure	
	(1)	(2)	(3)
	Log Turnover	Log Emissions Intensity	Share Coal Revenues
Post ∗ Treated	0.032	-0.464***	-0.075***
	(0.088)	(0.108)	(0.029)
N obs	11316	11316	11316
Firm FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Country HQ-Year FE	Yes	Yes	Yes

Notes: The table reports estimates from synthetic difference-in-differences regressions, following Arkhangelsky et al. (2021). The method reweights control firms and pre-treatment periods so that the weighted control group better matches treated firms' pre-treatment outcome paths. Panel A reports effects on emissions and satellite-based local air pollution. Column (1) uses firm-level log greenhouse gas emissions from Trucost, measured in tonnes of CO_2 equivalents and aggregating Scope 1, Scope 2, and Scope 3 emissions. Columns (2)–(5) use satellite-based $PM_{2.5}$ and NO_2 concentrations around coal-fired power plants owned by sample firms. Columns (2)–(3) aggregate plant-level pollution to the firm level among coal power plant owners, while Columns (4)–(5) use plant-level observations. Panel B reports firm-level operational outcomes. Turnover is total firm revenue. Emissions intensity is total Trucost greenhouse gas emissions divided by turnover. Share coal revenues is the share of firm revenues derived from coal mining and coal-fired power generation. The SDID analysis requires a balanced panel, so the sample differs from the baseline sample. Standard errors in parentheses are obtained using a bootstrap procedure with 50 repetitions. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

G Expert Prediction Survey

In February 2026, we launched a public expert survey on the Social Science Prediction Platform (SSPP), targeting students, researchers, and academics active in economics and adjacent social sciences. Members of the platform can voluntarily respond to open surveys posted on the platform. We obtained 73 complete responses over four months, which we use to benchmark our findings against informed prior beliefs. Roughly 60% of respondents are PhD students, with the remainder split between researchers (including postdocs and other research positions) and professors (assistant, associate, or full). The modal field is development economics, followed by a pooled category that combines respondents who selected agricultural, energy, or environmental economics, and then by labor economics, and behavioral and experimental economics.

We use these responses as a descriptive benchmark rather than as the basis for formal statistical testing. Figure G1 compares the distribution of respondents' predictions with our baseline estimates for four outcomes: emissions, input–output-based emissions, local air pollution, and monthly stock returns. The vertical lines in each panel mark our corresponding baseline estimates.

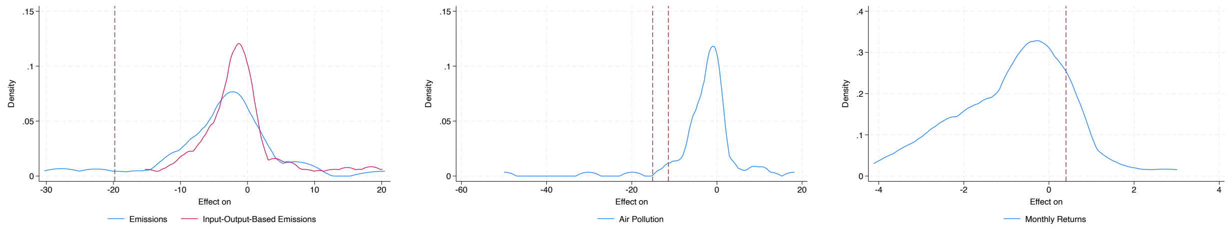
Three patterns emerge from Figure G1. First, respondents expect substantially smaller effects on firms' environmental outcomes than the ones we estimate. On average, they predict a decline in emissions of 4%, well below our baseline estimate. This gap becomes even more pronounced when respondents are asked about input–output-based emissions. For that outcome, the average predicted effect is only -0.9%, nearly an order of magnitude smaller than the prediction for emissions more broadly defined. This sharp drop is informative because input–output-based emissions are more tightly linked to underlying economic activity. In other words, respondents appear to regard a treatment like ours as potentially capable of affecting reported emissions measures to some extent, but as unlikely to induce meaningful changes in emissions measures that more directly track real production decisions.

Second, respondents also expect only limited effects on local air pollution. The average predicted decline is 2.8%, whereas our satellite-based estimates point to materially larger reductions in $PM_{2.5}$ and NO_2 around affected plants.

Third, respondents predict that the treatment should worsen firms' stock-market performance: the average expected effect on monthly stock returns is -0.8 percentage points. By contrast, our estimates imply an increase in returns following treatment.

Taken together, these responses indicate that informed prior beliefs about the consequences of a salient investor's divestment decision were considerably more skeptical than

Figure G1: Expert Predictions vs. Estimated Effects



Notes: The figure reports the distribution of expert predictions from a survey conducted on the Social Science Prediction Platform in February–April 2026. Respondents were asked to predict the difference-in-differences effect of a divestment intervention similar to the Oil Fund's 2016 coal divestment. The left panel reports predictions for greenhouse gas emissions and input-output-based emissions, the middle panel reports predictions for local air pollution, and the right panel reports predictions for monthly stock returns. Greenhouse gas emissions refer to firm-level CO_2 emissions generated by the firm's whole organization, including energy, transportation, and intermediate inputs used in production, and constructed by a third-party data provider using firm disclosures combined with national, international, and sector-level data. Input-output-based emissions use a similar firm-level emissions concept but rely more directly on an input-output model. Local air pollution is $PM_{2.5}$, a satellite-derived measure of fine particulate matter at the GPS locations of coal-fired power plants owned by sample firms. Monthly stock returns are measured as the monthly percentage change in the firm's stock price. Vertical lines indicate the corresponding baseline estimates from the paper. Predictions are expressed as percentage changes for environmental outcomes and in percentage points for monthly stock returns.

the effects we document in the data. In this sense, the survey provides a useful benchmark for how surprising our estimated real and financial effects are relative to the expectations of researchers in economics and related fields.

We report the survey instrument below.

Block_Intro

Study

Divestment and Target Firms' Environmental Performance

Background

Institutional investors—such as government pension funds (e.g., CalPERS in California), asset managers (e.g., BlackRock, Vanguard), and sovereign wealth funds—direct trillions of dollars across global markets. This project examines one such institutional investor, referred to here as the "Investor."

The Investor is among the largest in the world. It was also one of the earliest major investors to establish ethical guidelines for managing its portfolio.

Our central question is: what happens when the Investor divests for ethical reasons? On the one hand, because the Investor is large, its decision may spark change in targeted firms. On the other hand, the Investor's actions may have little effect, as it may be offset by other investors who step in to buy the divested shares.

Policy Change: Coal Divestment

In the 2010s, the Investor expanded its ethical guidelines for divestment by including a coal criterion. Specifically, companies whose revenues or operations were significantly tied to coal were identified as candidates for divestment.

Using a difference-in-differences design, we investigate how the implementation of this policy affected: 1) the emissions of targeted firms; and 2) their stock return.

Sample Construction
Treatment group: companies that the Investor announced as targeted for divestment in the first year it implemented the coal criterion.

Control group: companies that: a) were in the Investor's portfolio in the year before the coal criterion took effect, b) but were never targeted under the coal criterion or any other ethical guideline in subsequent years, and c) operate in sectors in which at least one firm was ever targeted under the coal criterion (Mining, Utilities, Construction, Manufacturing, Wholesale and Retail Trade, Information, and Management of Companies).

Treated firms are primarily located in the United States, followed by Japan and China, India, and Canada and Hong Kong.

Control firms are mainly based in the United States, Japan, Great Britain, Taiwan, Korea, and Canada.

Empirical Estimation
We implement a standard difference-in-differences (DD) estimation strategy (we have only one treatment year). Our specifications include firm fixed effects, sector-by-year fixed effects, and country-by-year fixed effects.

The estimation covers 4 pre-treatment years and 8 post-treatment years.

We are interested in what you think the impact of the coal divestment will be on the target firm's **emissions** and **stock return**.

For each outcome, please provide your prediction.

Even if you do not have strong beliefs about the effects of the interventions, we are still interested in your best guess.

Block_Q1_Randomized

Please predict the DD estimate for *percentage change* in the outcome: **CO2 emissions.**
• This refers to CO2 emissions generated by the firms' whole organization—including energy, transportation, and intermediate inputs used in production.
• It is constructed by a widely used third-party data provider, using firm disclosures combined with national, international, and sector-level data.

For reference:
• In the year prior to treatment, the average level of CO2 emissions among the treated group is 43 million tons of CO2.
• 7.5 tons of CO2 ≈ emissions produced by a year of electricity use by an average U.S. home (estimates from US Environmental Protection Agency).

DD Estimate (percent):

Less CO2 emissions				No impact				More CO2 emissions		
-50	-40	-30	-20	-10	0	10	20	30	40	50

Please predict the DD estimate for *percentage change* in the outcome: **input-output-based CO_2 emissions.**

• This refers to CO2 emissions generated by the firms' whole organization—including energy, transportation, and intermediate inputs used in production.

• It is constructed by a widely used third-party data provider, using an input-output model, combined with firm disclosures and national, international, and sector-level data.

For reference:

• In the year prior to treatment, the average level of CO2 emissions among the treated group is 43 million tons of CO2.

• 7.5 tons of CO2 ≈ emissions produced by a year of electricity use by an average U.S. home (estimates from US Environmental Protection Agency).

DD Estimate (percent):

Less CO2 emissions					No impact				More CO2 emissions	
-50	-40	-30	-20	-10	0	10	20	30	40	50

How confident are you in this prediction? If you are confident it means that you believe your predictions are very accurate.

- ○ Not at all confident
- ○ Slightly confident

- Somewhat confident
- Very confident
- Extremely confident

Block_Q2

Please predict the DD estimate for *percentage change* in the outcome: **air pollution**
• The measure is PM2.5, a type of fine particulate matter that reflects how “dirty” the local air is.
• This outcome is defined for the subsample of firms (primarily in the Utilities sector) that own coal-fired power plants.
• PM2.5 is satellite-derived measure at the GPS locations of coal power plants, summed across all plants owned by the firm.
• You can think of PM2.5 as a proxy for CO_2 emissions (the first outcome you predicted). They have a correlation of 0.76.

For reference:
• In the year prior to treatment, the average value of PM2.5 among the treated group is 397 micrograms per cubic meter.
• The World Health Organization’s guideline for annual average PM2.5 exposure is 5 micrograms per cubic meter.
• The annual mean PM2.5 in New Delhi, India is around 100 micrograms per cubic meter.

DD Estimate (percent):

Less air pollution					No impact					More air pollution
-50	-40	-30	-20	-10	0	10	20	30	40	50

How confident are you in this prediction? If you are confident it means that you believe your predictions are very accurate.

- Not at all confident
- Slightly confident
- Somewhat confident
- Very confident
- Extremely confident

Block_Q3

Please predict the DD estimate for the *change* in the outcome:
monthly stock returns.
• Defined as the percentage change in the firm's stock price from the previous month.
• The post-treatment period for this variable contains 93 months.

For reference:
• The long-term average monthly return of the S&P500 (an index of the largest companies in the US) is 0.61%

DD Estimate (percentage points):

Lower stock return					No impact					Higher stock return
-5	-4	-3	-2	-1	0	1	2	3	4	5

How confident are you in this prediction? If you are confident it means that you believe your predictions are very accurate.

- Not at all confident
- Slightly confident
- Somewhat confident
- Very confident
- Extremely confident

Block_Academic

What describes you best? Please select "yes" if you are currently one of the following:
Graduate student (Master level or PhD)
Faculty
Post-doc
Non-academic researcher

- Yes
- No

Block_YAcademic_FollowupQs

Which institution are you affiliated with?

What is your academic discipline?

- Economics
- Finance
- Political Science
- Psychology
- Other

Please specify your primary field of research.

What is your job title?

- Full Professor
- Associate Professor
- Assistant Professor

- Post-doctoral Researcher
- Researcher
- PhD Student
- Master Student
- Other

What is the highest level of education you have attained?

- Bachelor degree
- Master degree
- PhD
- Other graduate/professional degree

Block_NAcademic_FollowupQs

What type of organization do you currently work for?

- Academia/Research Institute
- Government
- International Organization
- NGO
- Private Sector
- Other

What is the highest level of education you have attained?

- ○ Bachelor degree
- ○ Master degree
- ○ PhD
- ○ [ ] Other graduate/professional degree

Block_End

Have you ever conducted research on topics in finance and/or environmental economics?

- ○ Yes
- ○ No

If you have comments, please enter them below. We would love to hear your feedback.