Modest and politically contingent stock market value for corporate climate pledges

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Whether financial markets reward or penalize voluntary corporate decarbonization pledges shapes firms' incentives to make and publicize them. We assemble a global dataset of 561 precisely dated climate-commitment press releases by listed firms (2017–2023), sampled from a financial newswire to maximize detectability of market reactions. Announcements raise stock prices by 0.23% on average (95% confidence interval 0.03–0.43%) over the first three trading days; the gain then declines and becomes indistinguishable. We detect no difference across third-party certification labels; reactions do not increase with carbon-price stringency and are detectable only for US-incorporated firms. Within the US, announcement returns polarized along state ESG politics over 2021–22, with a two-percentage-point reversal of the between-group gap, alongside a sharp drop in the US share of global announcements. Rewards for publicizing climate commitments are modest and short-lived, and appear politically contingent.

The intensification of climate policy [1] and the associated "transition risks" [2] have catalyzed a global surge in voluntary corporate greenhouse-gas (GHG) mitigation targets. Investors increasingly demand disclosure of firms' climate exposure [3, 4], and because equity prices are forward-looking, the market response to a pledge aggregates investors' beliefs about the costs, benefits and credibility of the announced transition path, and may therefore affect firms' incentives to commit, and to commit publicly, in the first place [5]. Whether financial markets value these pledges when they are announced shapes firms' incentives to make and publicize voluntary commitments.

The sign of this response is theoretically ambiguous. A credible commitment can raise

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firm value by reducing exposure to transition risk, lowering the cost of capital, attracting demand for green assets [6] or reducing the cost of credit through the bank-lending channel [7]. But credible commitments require costly capital expenditure, and furthermore corporate pledges often outrun action [8–10], while pledges perceived as cheap talk may be discounted or even penalized [11].

Existing work studies how carbon risk is priced in financial markets [12–17], whether climate commitments change environmental performance [18–20] and whether they are credible [21–23]; event studies of adjacent green actions find positive reactions [24, 25]. Much less is known about how investors value the commitment announcements themselves, in large part because commitment databases record adoption dates imprecisely, blurring the moment at which the information reaches investors.

We address three open questions. Do markets place a value on the public act of commitment at the moment it is disclosed? Does any such value reflect the credibility conferred by third-party certification, which would be a natural conjecture if validation separates genuine pledges from cheap talk [26]? And is it contingent on the policy and political environment in which the firm operates [27, 28]?

To answer them, we assemble a global dataset of time-stamped climate-commitment press releases from the LSEG financial wire, a feed closely monitored by investors, so that each pledge demonstrably reached the market as salient, precisely dated news. The design minimizes attenuation from limited salience or imprecise timing, though not from anticipation; and because firms choose what to broadcast, the measured reaction may overstate the average pledge. Our estimand is the announcement-window disclosure premium for publicized pledges (Methods).

After the data-preparation pipeline (Methods), the final sample contains 561 announcements by listed firms over 2017–2023; firms incorporated in the United States account for 43% of complete-case events (Supplementary Table 1). For each announcement, we estimate daily abnormal returns, defined as the gap between the firm's realized return and its usual co-movement with its home market, and average them across events. We then examine how reactions vary with commitment characteristics, certification labels, sectors, and the policy and political environment.

Three findings emerge. First, the average market reaction is positive but short-lived: the three-day cumulative average abnormal return is 0.23% (95% confidence interval 0.03–0.43%), driven almost entirely by days 0 and 1; and the cumulative effect is no longer statistically distinguishable from zero when the horizon is extended beyond three days. Second, we detect no evidence that certification status changes the announcement-stage valuation response: reactions are statistically indistinguishable across SBTi-validated, SBTi-committed and unlabeled pledges. Third, the policy environment matters: returns are statistically significant only for US-incorporated firms, and do not rise with exposure to more stringent carbon pricing. Within the United States, announcement returns polarized along state ESG politics as climate finance became partisan on both sides over 2021–22: a gap that initially favoured firms headquartered in anti-ESG states reverses by about two percentage points, alongside a marked decline in the US share of global commitment announcements.

Our contribution is threefold. To our knowledge, it is the broadest announcement-level event study of corporate climate pledges: press-release time stamps allow daily identification across a global, multi-sector sample, where existing evidence relies on US news text [29], single industries [30] or annually dated registries [31] (Supplementary Note 1 reconciles the estimates). We show that the announcement premium is detected only among US-incorporated firms and that within the United States it polarized as climate finance became partisan, connecting pledge pricing to work on climate policy, asset prices and the ESG backlash, [27, 28, 32–35]. And we provide, to our knowledge, the first direct test of whether third-party certification commands an announcement premium (prior work relates certification to portfolio holdings or longer-run outcomes, not to the announcement-window return).

Results

Markets react positively but modestly. Figure 1 shows the cumulative average abnormal return around the mitigation pledge announcement day for the 561 retained announcements, where abnormal returns are computed from a market model estimated against the firm's country benchmark index over the 75 trading days preceding the event window (Methods). We detect no systematic pre-announcement abnormal returns (Methods). At the announcement, prices react immediately: the average abnormal return is 0.10% on day 0 and 0.14% on day 1, and essentially zero thereafter. The three-day cumulative effect, CAAR(0,2), is 0.23% (95% CI 0.03–0.43%) under our primary aggregation rule, which averages the available abnormal returns across events day by day and then cumulates the daily averages. An alternative rule that retains only events observed on every day of the window gives 0.26% (95% CI 0.05–0.47%, $N = 519$); the two series are nearly indistinguishable at every horizon (Fig. 1). After day 2 the point estimate declines: the cumulative abnormal return over days 3–5 is -0.14% (95% CI -0.34 to 0.05%), not significantly negative, and by day +5 the cumulative effect is roughly 0.09% and statistically indistinguishable from zero. Applying the average 0.23% return to the median sample market capitalization of US$12 billion (Supplementary Table 1) implies an illustrative valuation change of roughly US$28 million. The average reaction is thus statistically detectable but small relative to ordinary firm-level return variation (the cross-sectional dispersion of three-day CARs is 2.4%).

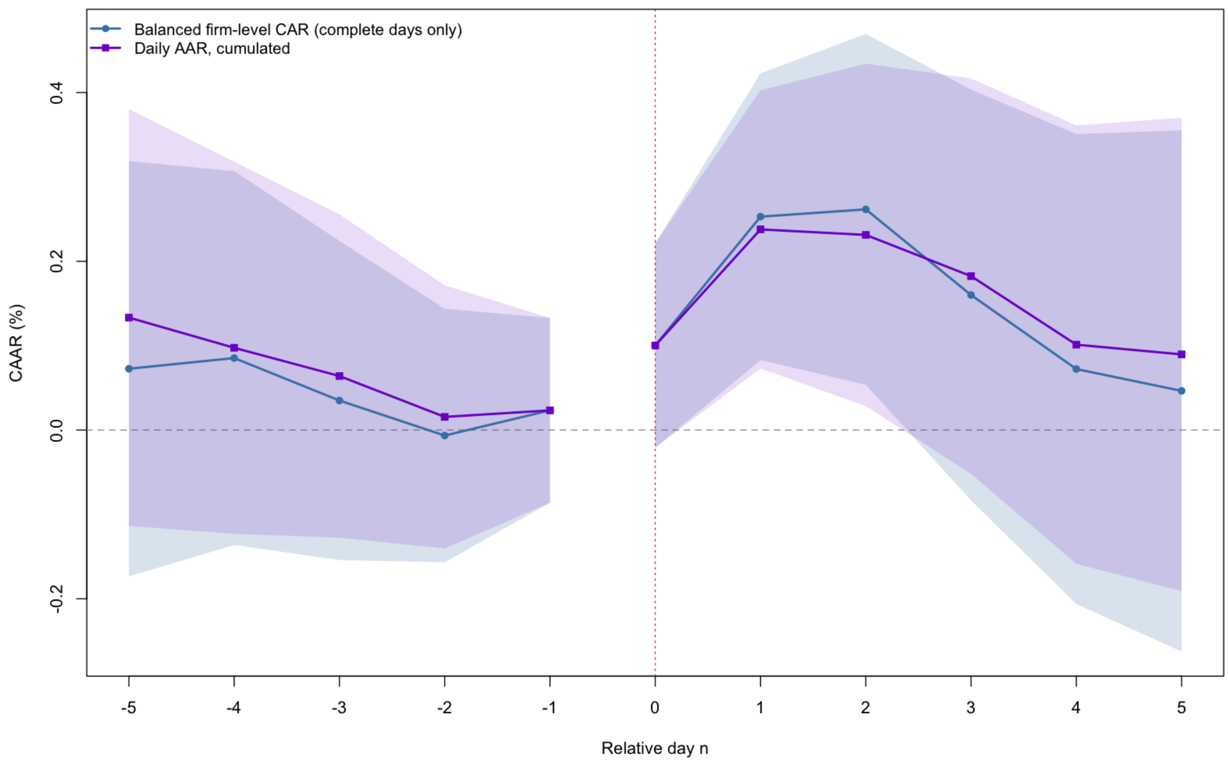

Figure 1: Cumulative average abnormal returns around commitment releases

Notes: Two aggregation rules are shown. Blue, balanced firm-level CAR: abnormal returns are cumulated within firm over the event window, retaining only events with no missing trading day; CAAR is the cross-firm mean and confidence intervals use Brown–Warner cross-sectional standard errors. Purple, AAR-cumulate: daily average abnormal returns are computed first and then cumulated over event time, with cross-period-independence standard errors. Pre-event values cumulate abnormal returns backward from day -1. Shaded bands denote 95% confidence intervals.

Robustness. A central concern is that reactions reflect concomitant news. Excluding announcements around earnings releases, COP meetings (± 7 days) and bundled corporate news leaves the point estimate at 0.19–0.24%, with precision falling in the most restrictive cells ($N = 472$ and 395), whose 95% confidence intervals marginally include zero (Fig. 2). Estimates are similarly stable across geographic, sector and label restrictions (0.18–0.37%), the exceptions being the ex-US and ex-SBTi subsamples. Varying the benchmark index, the abnormal-return model, the estimation, event and buffer windows, the censoring rate, the missingness gate and the aggregation rule gives 0.15–0.40% across 19 of the 20 alternative specifications; 15 remain significant at the 5% or 10% level, and only the five-day horizon is indistinguishable from zero (Extended Data Fig. 2). Balanced-rule companions (Supplementary Figs. 4 and 5) and firm-, date- and two-way-clustered standard errors leave the result unchanged ($t \geq 2.4$; Methods).

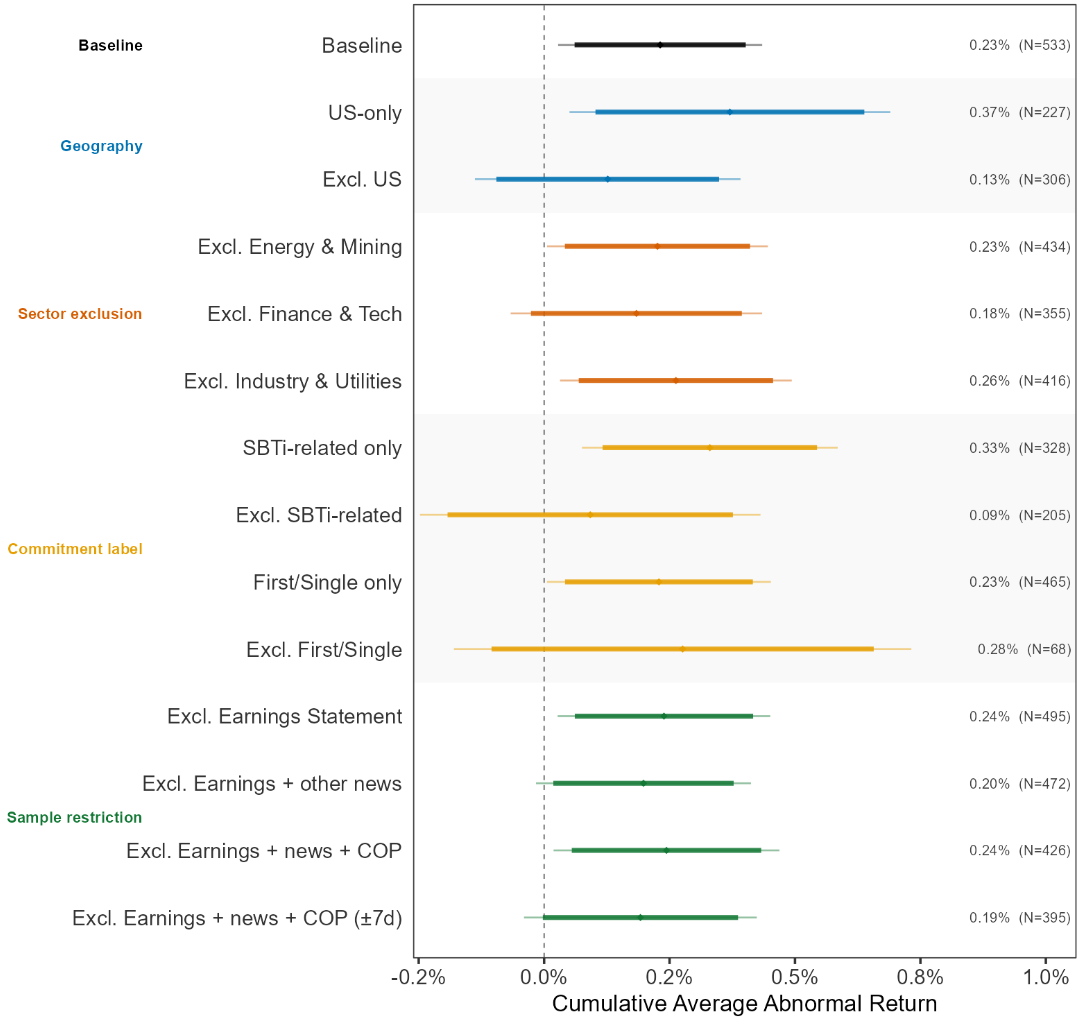

Figure 2: Robustness checks: geography, sectors, labels and sample restrictions

Notes: CAAR(0,2) (AAR-cumulate rule) across additional specifications; thick and thin bars are 90% and 95% confidence intervals (cross-period-independence standard errors, $df = N_{\min} - 1$). The label refers to: commitments labelled by the Science-Based Target initiative (SBTi) and, when several within-sample commitments from the same firm, first vs. subsequent. Sample restrictions exclude, cumulatively, announcements within earnings windows (±15 calendar days of the firm's recorded earnings release date; events with no recorded date are retained), announcements bundled with other material news, and announcements during COP summits (exact days, then ±7 days). Unlike the unique winsorization procedure applied throughout the main analysis, the robustness forests subsample rows re-estimate abnormal returns and re-censor within each subsample. Hence, the US three-day estimate of 0.37% in the forest against 0.40% in the baseline.

No detectable certification premium. If markets attended to the credibility conferred by third-party certification [36, 37], reactions should differ across certification labels. The sign of any such difference is ambiguous ex ante, since credibility raises the expected benefits of a pledge but also the expected compliance and capital-expenditure costs [38], while validation may itself be anticipated; but a differential in either direction would indicate that certification changes perceived net value. In Extended Data Fig. 3, we find none: three-day CAARs are 0.38% for SBTi-validated targets (N = 197), 0.24% for SBTi targets not yet validated (N = 131) and 0.20% for unlabeled new targets (N = 85), and the cross-label differences are not statistically significant at the 5% level. Running an additional equivalence test rules out premia larger than about 0.7 percentage points (Supplementary Note 2); the multivariate regression confirms the picture (Fig. 3; Extended Data Table 1). Target modifications (reference category) elicit essentially no reaction (0.1%, N = 101), consistent with the market pricing the original commitment rather than its restatement; the one negative estimate, for the small GFANZ cell (18 events), is discussed in Supplementary Note 1.

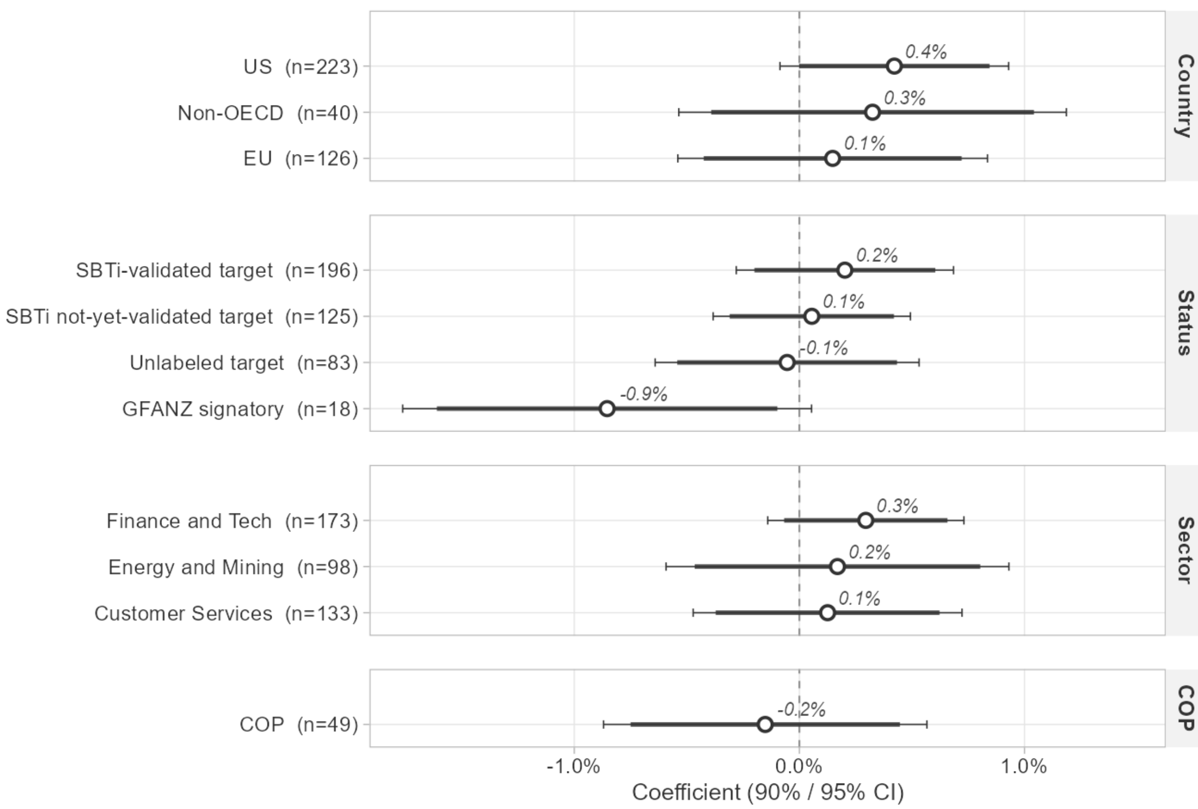

Figure 3: Multivariate analysis of three-day CARs

Notes: OLS coefficients from a single regression of event-level CAR(0,2) on region, commitment-label and sector indicators and a COP dummy (N = 489 complete cases). Reference categories: Other OECD (region), target modifications (label), Industry and Utilities (sector). Thick and thin bars are 90% and 95% confidence intervals from CR2 cluster-robust standard errors, clustered by country of incorporation (G = 39), with classical cluster-robust t critical values (df = G−1) matching the significance coloring. Standard errors do not propagate market-model estimation variance.

No carbon-price stringency premium. A natural hypothesis is that pledges are worth more where carbon pricing is stringent, because the pledge then hedges a larger regulatory cost [39]. Our data do not support it. Announcement returns do not increase with the assigned OECD Effective Carbon Rate (an exposure proxy matched by country, sector and year; Methods): the binned pattern is non-monotonic and its positive low-ECR reactions rest mainly on US events (Fig. 4; Supplementary Fig. 7), and regressions on the continuous rate yield coefficients close to zero (N = 425; Supplementary Table 6). One candidate interpretation, developed in the Discussion, is anticipation: where regulation makes commitments predictable, announcements carry less surprise. Given that the global response is driven largely by US firms, we examine the United States more closely in what follows.

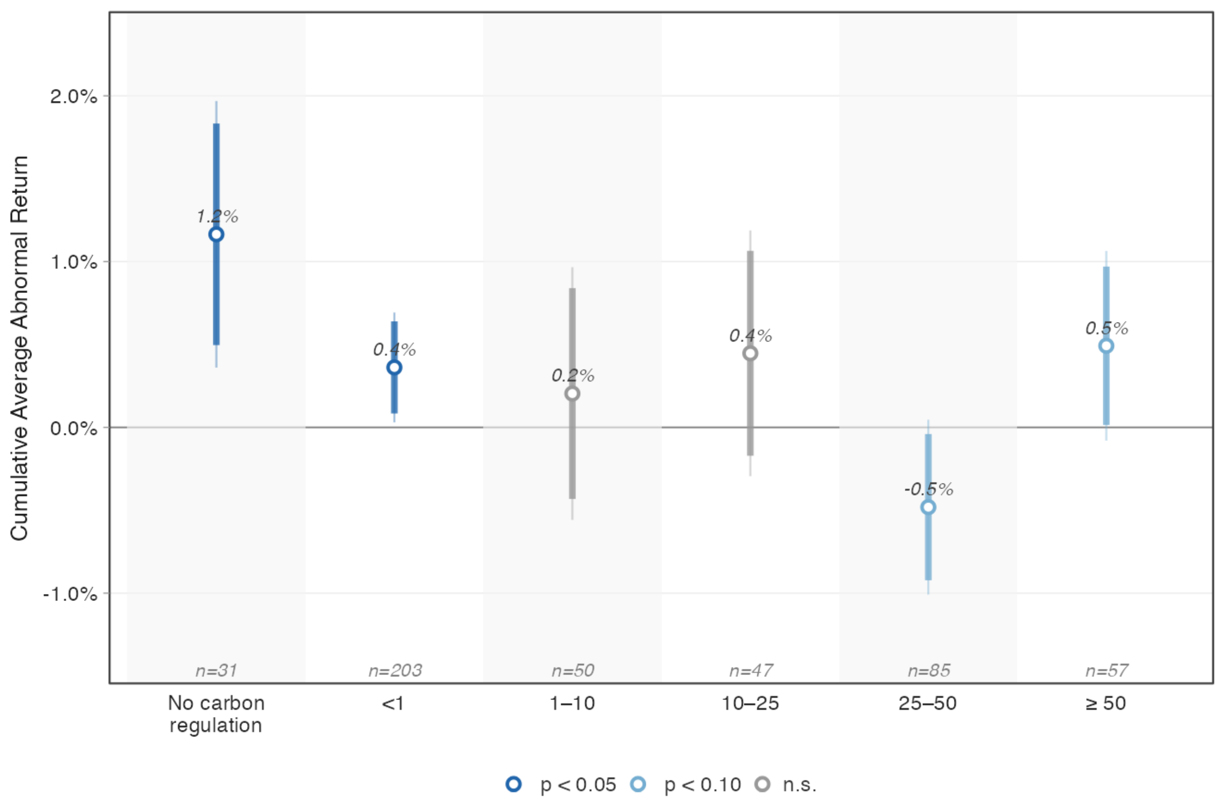

Figure 4: CAAR(0,2) by effective carbon rate

Notes: CAAR(0,2) by bin of the OECD Effective Carbon Rate including free allocations (€/tCO_2), under the AAR-cumulate rule (daily abnormal returns averaged across the available firms each day, then cumulated over the CAR(0,2) window), for the 495 of 561 events with a matched ECR. Per-bin CAAR uses cross-period-independence standard errors ($df = N_{\min} - 1$). Thick and thin bars are 90% and 95% confidence intervals. Bin sample sizes range from 31 (no carbon regulation) to 203 (<1 €). The balanced firm-level CAR companion is reported in Supplementary Fig. 6. Confidence intervals are not adjusted for multiple comparisons across bins.

A US premium, eroded by the anti-ESG backlash. In the robustness panel (Fig. 2), we document a geographic split between significantly positive abnormal returns for US-incorporated firms (0.37%, N = 227, p = 0.023) against a small, insignificant rest of the world (ROW) estimate (0.13%, N = 306, p = 0.347). Extended Data Fig. 4 traces the US-only counterpart of Fig. 1: under the primary aggregation rule the US reaction builds over days 0–2 to 0.40% (N = 227), and we detect no systematic pre-announcement abnormal returns in the US subsample. In the multivariate specification (Fig. 3), the US indicator carries the largest geographic coefficient: 0.42%, but only marginally significant ($p = 0.10$; Extended Data Table 1). The premium is not sector-driven: excluding any of the three largest sector groups leaves the whole-sample estimate at 0.18–0.26% (Fig. 2), while the largest sector contrast in the regression is +0.3 percentage points.

In complement, we test the US-minus-ROW difference directly (Extended Data Table 2): recoding the multivariate specification's country dummies into a single US indicator gives +0.32 percentage points on the pooled sample (N = 519), $p = 0.026$ under a one-sided test of H_1: US > ROW (df $= G - 1$; two-sided $p = 0.053$; Methods). Excluding the commitments exposed to the anti-ESG backlash raises the estimate to +0.46 percentage points and tightens two-sided inference to the 1% level (N = 489; $p = 0.006$; Methods). The results are almost unaffected by adding controls and using Satterthwaite p-values. A US premium is therefore present in the data, and it is the anti-ESG backlash, not statistical noise, that erodes it in the pooled estimate: excluding the commitments exposed to it restores full precision ($N = 489$, $p = 0.006$), and the within-US evidence below shows why.

Announcement returns polarized along state ESG politics. Climate finance became a partisan issue in the United States over 2021–22. The 2020 election shifted expectations on federal climate policy sharply upward, and equity markets repriced climate-responsible firms accordingly [28]; the incoming administration made climate-related financial risk an explicit supervisory objective (Executive Order 14030, 20 May 2021). Republican state officials moved in the opposite direction, restricting public funds from financial institutions that incorporate climate risk [35]. BlackRock's January 2022 letter to CEOs by defending stakeholder capitalism made the largest asset manager worldwide a focal target of that campaign. The federal shift was common, but political environments are geographically sorted [40, 41]. The two movements had opposite signs, different instruments and different dates.

Before 2021, returns were higher in anti-ESG states than elsewhere (a gap of −2.1pp, significant at 5%); the gap reverses monotonically to +1.4pp in 2022 (significant at 10%) before narrowing in 2023 (Extended Data Fig. 5; cell estimates in Extended Data Table 3).

In a cross-sectional regression on the 228 US-headquartered announcements, splitting at 1 January 2022 and coding the pro-ESG-or-neutral group as treated, the interaction coefficient is +2.0 percentage points, rising to +2.2 with size, emissions, SBTi and sector controls (both significant at the 1% level with CR2 standard errors clustered by state of headquarters; G = 34, falling to G = 33 with controls; Extended Data Table 4). It remains significant at the 1% level under heteroskedasticity-robust standard errors (Extended Data Table 5), survives restricting the pre-period to 2021 (+1.7), adding group-specific linear trends (+2.1) and a large range of alternative classifications for anti-ESG (1.6 to 2.2; Methods).

Because the two halves of the reversal are separately dated, the estimate is sensitive to where the sample is split, as a two-stage process should be. While re-dating the post period to June 2021, when Texas signed the first anti-ESG statutes, halves the interaction to +1.1 percentage points and renders it insignificant; by contrast, a placebo cutoff at the January 2021 inauguration, within the pre-2022 sample, yields a significant interaction of +1.6 percentage points (Extended Data Table 6; Methods). Part of the divergence therefore predates the anti-ESG campaign and tracks the change in federal administration.

A post-Fink's-letter divergence also appears in announcement counts, which fall roughly equally across both state groups as the US share of global announcements drops from 54% to 30% (Fig. 5, bottom panel), while announcements by the ROW over the same period rise, so the US share of global announcements falls from 54% to 41% and then 30% (Fig. 5, bottom panel), consistent with 'greenhushing' [42], though composition/coverage changes cannot be ruled out.

Discussion

Taken together, our results document four features of the premium for a corporate net-zero commitment disclosure.

First, the small and short-lived response indicates that the marginal information content of the average publicized pledge is limited, since, by construction, the news reached investors on a known day through a salient channel. Nevertheless, our set-up cannot disentangle whether it is low valuation or full anticipation that drives this result. Indeed, under market efficiency, only the unanticipated component of a pledge moves prices, as in the monetary-policy surprise literature [43, 44], so a commitment the market already expected does not move prices. For the question that motivates the paper, however, the two readings carry the same implication: going public with a climate target adds little to firm value at the margin, whether because investors discount the content or because they had already priced it. Either way, the market-based reward for making and publicizing a voluntary commitment is smaller

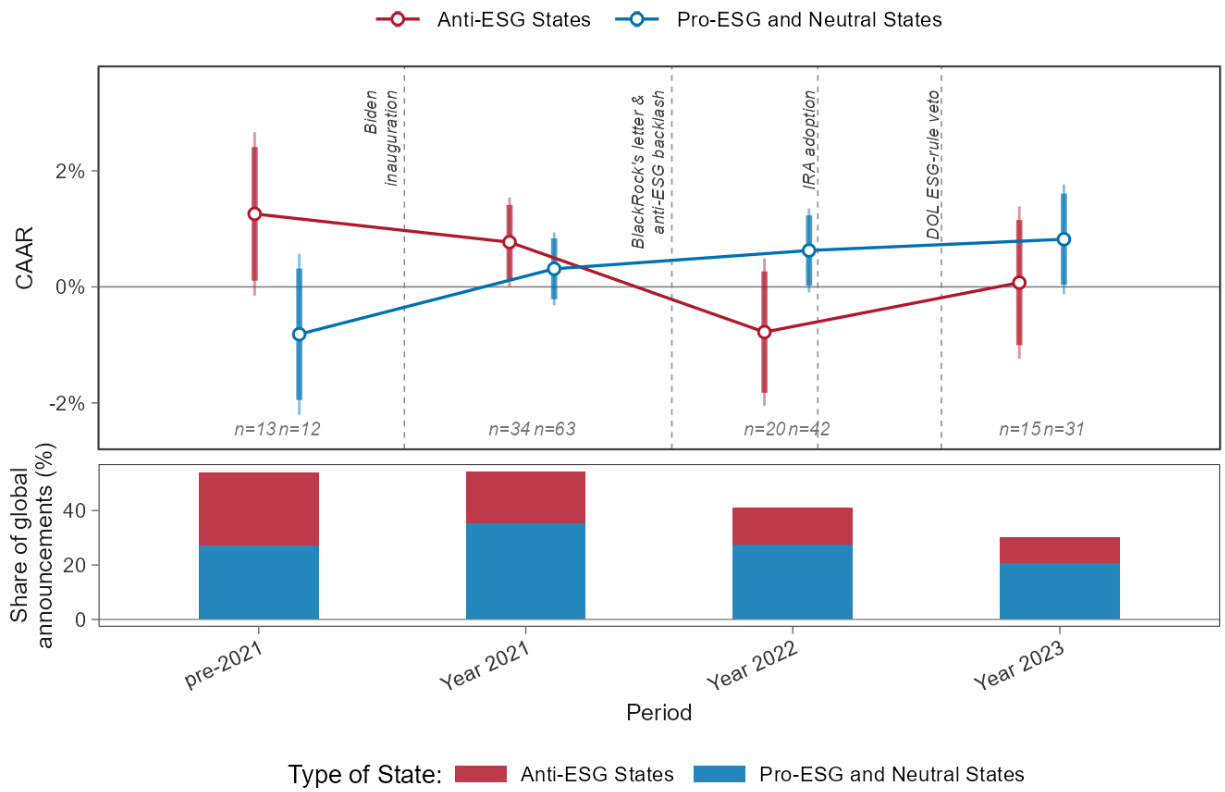

Figure 5: The US anti-ESG backlash and the market response to commitment announcements

Notes: Top: CAAR(0,2) for announcements by firms headquartered in anti-ESG states (red) versus pro-ESG and neutral states (blue), by period (pre-2021, 2021, 2022, 2023); cell sizes shown at the bottom of the panel. Thick and thin bars are 90% and 95% confidence intervals (cross-period-independence standard errors). Vertical lines mark the Biden inauguration, the January 2022 BlackRock letter and onset of the anti-ESG backlash, the adoption of the Inflation Reduction Act (IRA, 16 August 2022), and the March 2023 presidential veto preserving the Department of Labor ESG rule. Bottom: share of global announcements accounted for by US firms, split by state group. State classification is described in the Methods.

than the "it pays to be green" narrative typically presumes.

Second, we detect no certification premium at the announcement stage. Reactions are statistically indistinguishable across SBTi-validated, SBTi-committed and unlabeled pledges, and restatements of existing targets elicit no reaction. Certification may still matter elsewhere: certified targets have been shown to affect emissions outcomes and pledge fulfilment [19, 45], lending relationships and credit risk [7, 46], and net-zero-aligned investing [47], and validation itself can fail. But it does mean the announcement-day premium cannot be read as a price on third-party verification: investors reward the public act of commitment at a moment when its fulfillment is largely unverifiable, and ex-post accountability for pledge outcomes is weak: nearly a third of the corporate targets that came due in 2020 quietly disappeared, and even outright failures drew no detectable market reaction [48] (Supplementary Note 1 discusses corroborating evidence from the banking sector).

Third, the premium is geographically and politically contingent. The premium does not rise with carbon-price stringency and it is concentrated in the United States, where it is precisely estimated; elsewhere the point estimates are smaller and the effect becomes indistinguishable. Within the United States the estimated premium is absent among announcements by firms headquartered in anti-ESG states from 2022 onward. The carbon-price pattern, with the largest reactions where carbon pricing is absent (a contrast resting mainly on US announcements; Supplementary Fig. 7), is consistent with an anticipation mechanism, just as evidence showed that mandatory sustainability reporting reduces the signalling value of voluntary disclosure, as surveyed in [49]. We resist the stronger reading that pledges substitute for regulation where none applies: that contrast rests almost entirely on US events and cannot be separated from the US-specific reaction. This reading fits the evidence but is not established: effective carbon rates co-move with many other features of the institutional environment.

Fourth, the within-US evidence points to a political-economy channel that fragments the value of climate disclosure across jurisdictions. The between-group gap reverses in two stages: it favours anti-ESG states before 2021, closes over 2021 as the federal environment turns supportive, and reaches a roughly two-percentage-point advantage for other states in 2022 before narrowing in 2023. Over the same period US announcements fall by half in both state groups while global announcements continue to rise, so the US share of the sample drops from 54% to 30%. These patterns are consistent with the greenhushing hypothesis (firms facing political cost muting or delaying disclosure rather than announcing), although our data cannot separate fewer commitments from fewer public releases. For firms, the disclosure decision becomes a political calculation; for policymakers, the polarization of ESG finance reduces the public visibility and readability of transition plans. We read these estimates as a quantification of how politically contingent the value of climate disclosure has become, not as the causal effect of anti-ESG legislation (see the caveats that close this section).

These results carry a governance implication: equity-market reactions at the announcement stage appear to provide only a weak mechanism for distinguishing among corporate climate commitments. On average, equity prices respond modestly to pledging disclosure, and attach detectable value neither to carbon pricing risk exposure nor to third-party verification at the announcement stage — precisely the moment when fulfillment is the most unverifiable, and when certification should be most valued. The credibility architecture built around corporate net-zero (SBTi certification, alliance membership, disclosure mandates [50]) therefore cannot look to announcement-stage equity prices to discipline the content of pledges.

Our study has three main limitations. First, the estimand conditions on the choice to broadcast: firms plausibly publicize the pledges they expect to be received favorably, and the

LSEG wire over-represents large firms and advanced economies, so the measured premium plausibly overstates the average across all pledges; our headline claim concerns marginal information content on this channel, not investors' ultimate valuation of decarbonization. Second, the anti-ESG comparison identifies an association rather than a causal effect. Headquarters location is an imperfect proxy for exposure, and the composition of announcing firms changed after 2022, so the estimate mixes repricing of given announcements with selection into announcing. The interaction survives sector-by-period fixed effects and the exclusion of energy firms (Supplementary Table 12), but announcing itself may respond to the backlash, mixing repricing with selection. Third, we measure market valuation, not abatement: nothing here speaks to whether pledges are kept [19, 45], and the analysis is confined to equity markets, whereas transition risk is also priced in credit [46].

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Methods

Commitment announcements from press releases

We extract press releases from the LSEG (ex-Refinitiv) press aggregator up to 31 December 2023, the first climate commitments being recorded in 2017. The aggregator indexes more than 5,000 global press sources, so that a release reaches investors as salient, sharply timed news rather than as a disclosure buried in a sustainability report, and each release carries the Refinitiv Identifier Code (RIC) of the company's main equity, which limits matching error. We retain releases whose headline contains one of thirteen commitment-related keywords or that LSEG categorises under "carbon neutrality" (Supplementary Note 3), and drop commitments announced jointly by a coalition of peers, which yields 947 releases over 2017–2023.

Because the aggregator can record a statement several days after its official release — a lag that would undermine identification in a short-window event study — we verify each date against the release posted on the company website or against financial media reporting the exact disclosure date, and keep only releases we can date unambiguously; Supplementary Fig. 3 shows the distribution of gaps. Whenever this search uncovers further relevant releases from the same company, we add them to the sample.

We also record the characteristics of the target: the reduction percentage, the net-zero target year and the interim target year, if any. A release is coded as a "New" commitment when it announces a new net-zero target, an interim target (target year, scope 1&2 objective and base year), the year in which the firm planned to reach net zero on its scope 1&2 emissions, or a step in the SBTi process (commitment, submission or validation) [51]. When several interim targets appear, we keep the most stringent one, discounting time linearly. Linear discounting is imperfect (target completion varies systematically with target difficulty and horizon [52]), but no alternative is clearly better supported.

We further record the label validating the target, focusing on the SBTi and on GFANZ, which groups the Net-Zero Banking Alliance, the Net-Zero Insurance Alliance and the Net-Zero Asset Managers. A company is treated as engaged in the SBTi process only when it states so explicitly, since some releases mention the SBTi without claiming membership; we classify its progress as a letter of commitment, goals consistent with SBTi standards, or validation by the SBTi (Supplementary Fig. 2 shows a validation announcement). Finally, we record whether a release contains information unrelated to the commitment (Fig. 2).

Commitment-label taxonomy. For the label heterogeneity analyses, each announcement is assigned to exactly one category by applying the following hierarchy to the status communicated in the release itself (not the firm's later status): announcements that restate or revise an existing target are classified as *target modifications*; among new targets, GFANZ-affiliated announcements form the *GFANZ signatory* category; the remaining new targets that engage the SBTi are split into *SBTi-validated* targets and *SBTi not-yet-validated* targets (the latter pooling the commitment-letter and submission steps); new targets carrying none of these labels are *unlabeled new targets*. The categories are therefore mutually exclusive by construction, and a target validated after its initial pledge appears under its announcement-day category (GFANZ affiliation is an alliance membership rather than a target certification, which is why it forms its own category). Target modifications serve as the reference category in the multivariate regression, so the label coefficients measure differences relative to announcements that restate existing targets.

Day 0 convention. If an announcement occurs on a non-trading day, Day 0 is defined as the first subsequent trading day. Specifically, the algorithm searches for the next available trading date within a maximum window of five calendar days (day 0 to day +5). This convention follows standard practice in event study methodology. Daily average abnormal returns are slightly larger on day 1 than on day 0, consistent with a share of releases published after the market close of their calendar day; the CAR(0,2) cumulation window is designed to absorb this timing.

Analysis samples

Of the 947 candidate releases, 654 are press releases by listed firms with a recorded release date. The main event-study sample comprises the 561 press releases retained after the standard event-study data-preparation pipeline (verification of release dates, exclusion of firms whose country of exchange falls outside the LSEG FR Global Equity universe, removal of events for which the local equity series is flat over the post-event horizon, and removal of events for which insufficient pre-event return data are available). Sample composition by country, sector, year and commitment label is reported in Supplementary Table 1. Index returns are complete over every event's estimation window. Stock returns are not: 90% of retained events have at least one missing stock return in the 75-trading-day estimation window (maximum missing share 14.7%, below the 15% baseline gate), and the stricter 5% and 10% missingness gates used as robustness checks are binding: they retain 166 and 468 of the 561 events; the corresponding robustness-forest rows report minimum daily counts of 162 and 444 over the CAR(0,2) window.

Some robustness checks and the heterogeneity analysis involve additional sample restrictions due to missing values in the covariates: the multivariate analysis is run on 519 events with a non-missing complete-case CAR(0,2) and label classification; 489 events in Fig. 5

where we exclude US announcements exposed to the anti-ESG backlash; 495 events have a matched OECD Effective Carbon Rate (the binned carbon-price figure uses these), and the carbon-pricing regressions are run on the 425 events that in addition have non-missing emissions and peer-share covariates (shared complete-case mask across all specifications; Supplementary Table 6). Sample sizes therefore differ across displays by construction: 561 events enter the event study; the daily AAR panels use the events observed on each day (533–555 over days 0–2); the balanced firm-level CAR rule and the multivariate regression use the 519 events with a complete-case CAR(0,2); and the label-level cells use the per-label $N_{\min}$ of the AAR-cumulate rule.

Firm-level GHG emissions are not observed for every event, and their coverage is plausibly non-random: for instance, larger, more scrutinized firms report more consistently. So the specifications that condition on emissions (the carbon-price regressions on the 425 complete cases, and the emissions-controlled anti-ESG columns, which drop to $G = 33$ clusters) may over-represent such firms; we therefore read them as robustness checks rather than as population estimates.

Stock and Market Price Data

Corporate Equity. Event and financial data are matched on the Refinitiv Identifier Code (RIC) carried by the news release, completed manually where the release carries no RIC or a RIC other than that of the firm's ordinary share. Because the LSEG Screener App assigns each listed firm a unique reference RIC (its primary security, excluding dual listings) this rules out double counting and cross-listing mismatches.

We extract from LSEG Datastream the daily close prices (TR.ClosePrice), as well as all required companies' characteristics for the heterogeneity analysis (Supplementary Note 4), for all firms within our sample between 1 January 2014 and 31 January 2024. The close price is the last trade price of the company's main equity on a given day.

For each equity, LSEG publishes two indices: a Price Return Index, which is based on the overall free float market capitalization of the constituents, and a Total Return Index, which accounts for gross dividend payments. We use price return indices for both firm and benchmark series, for consistency with the Refinitiv market indices. Over the three-day windows considered here, the choice is immaterial: dividend distributions are unrelated to announcement timing and negligible relative to abnormal returns at this horizon.

Market benchmark. Benchmarks are Datastream Global Equity Indices, a family of free-float-adjusted, market-capitalisation-weighted indices built under a common methodology across 53 developed and emerging markets, which makes them mutually comparable and, unlike blue-chip indices, extends coverage beyond large caps [53]. Eighteen otherwise-eligible events are excluded because our index extraction contains no series for the firm exchange country (Thailand (5), Malaysia (4), the Philippines (3), and one each from Colombia, Iceland, Indonesia, Romania, Slovenia and Vietnam) and one further event is excluded because the data extraction did not resolve its exchange country. All series are in local currency. The baseline reference is the country total market index; the S&P 500 and country–sector indices enter only as robustness checks, since in most countries a sector index rests on too few constituents for the announcing firm to be negligible within its own benchmark, an overlap that attenuates estimated abnormal returns.

The precise methodology used to compute these indices is provided in [53].

Carbon pricing data

Our measure of carbon-price exposure comes from the OECD Carbon Pricing and Energy Taxation database underlying the Effective Carbon Rates (ECR) series [54], 2023 edition, whose modelled years are 2018 and 2021. The database reports, for each country, sector and year, the price signal arising from emissions trading systems, carbon taxes and fuel excise taxes, in euros per tonne of CO_2e. The OECD provided us with a more granular version (7 sectors, 30 subsectors) together with country $\times$ sector free-allocation shares, which are large and unevenly distributed (these shares range from 0% to 100%, with an unweighted average of 12.7% for all sectors with a non-zero ECR.). Our key regressor is therefore the Effective Carbon Rate including free allocations (`ecr_incl_fa`), which adjusts the gross rate downward for the implicit subsidy granted under cap-and-trade schemes and reflects the carbon cost actually borne by regulated firms; 32 matched observations carry a gross rate of exactly zero and are retained under the "No price" category.

Throughout, the matched country–sector–year rate is an exposure proxy rather than a firm-specific marginal carbon price: multinationals produce across jurisdictions, emission sources are heterogeneous within sectors, and regulatory costs other than carbon pricing are not captured. We therefore describe events as *assigned* to an effective carbon rate.

Matching the carbon price

There are three dimensions that require a matching process between the OECD Effective Carbon Rates database and the PR sample: country, sector, and year. Sixty-six observations are excluded from the binned carbon-price figure due to missing country ECR data or missing sector-level entries in the matching table.

Country dimension. The matching covers most countries in our sample. We use country of incorporation as a proxy for the relevant climate pricing scheme.

Year dimension. The OECD ECR database provides two cross-sections: 2018 and 2021. We match each observation to the nearest available year: commitment years 2017–2020 are matched to the 2018 ECR values, and commitment years 2021–2023 are matched to the 2021 values. Our sample spans six years (2017, 2019, 2020, 2021, 2022, 2023). No retained event occurs in 2018.

Sector dimension. The sectoral matching is performed via a correspondence table that maps each firm's GICS industry classification to one of 30 OECD ECR user categories in two steps: GICS sectors and industries are first mapped to ISIC codes using the UNEP FI ISIC/GICS crosswalk [55], and the ISIC codes are then mapped to the OECD ECR user categories following the OECD/ISIC correspondence table provided by the OECD. The distribution of firms across OECD categories, and the GICS industries whose assignment is not one-to-one, are reported in Supplementary Table 4.

Missing values. Of the 561 observations, 66 could not be matched to an ECR value and are excluded from the carbon-pricing analysis: 49 are incorporated in jurisdictions absent from the OECD ECR database, and 17 have a covered country but no entry for their country×sector cell. Supplementary Table 5 reports the breakdown.

Abnormal returns related to firms' commitment and CAAR estimation.

For each commitment, we establish the relationship between the firm's return and that of a synthetic index measuring market average return before the shock, and use it to predict the firm's returns absent the announcement. Figure 1 summarizes the timing conventions.

In detail, let t_0 denote the announcement day of event i (some firms have several commitment releases, we nonetheless use the index i interchangeably for commitments and firms, without loss of generality), and let $\tau_A = t_0 - 83$, $\tau_B = t_0 - 8$ and $\tau_C = t_0 + 15$ be the three dates (in trading days) marked in Fig. 1: the calibration window runs over $(\tau_A, \tau_B]$ and the abnormal-return window over $[\tau_B, \tau_C]$. We first run the following regression for each firm and each commitment to calibrate the market model (based on the specific market index of the country of exchange of the firm's main equity) and obtain firm i's α_i and β_i parameters:

$$R_{i,t} = \alpha_i + \beta_i R^m_{c,t} + \varepsilon_{i,t}, \qquad t \in (\tau_A, \tau_B] \tag{1}$$

where the calibration window $(\tau_A, \tau_B]$ comprises the 75 trading days up to and including τ_B, the first day of the abnormal-return window, $R_{i,t}$ is the stock return on day t of the firm emitting commitment i, $R^m_{c,t}$ is the market return on day t for country c and $\varepsilon_{i,t}$ is the zero-mean disturbance term, assumed normally distributed and uncorrelated with the market return. All returns are daily log returns. Using the market model is standard in the event-study literature [56]. In the baseline there is no buffer between the calibration window and the abnormal-return window: they meet at $\tau_B = t_0 - 8$. Robustness checks insert an 8- or 16-day buffer; the headline CAR(0,2) is essentially unchanged (see the robustness forest).

In turn, it allows us to compute the abnormal return $AR_{i,\tau}$ for each commitment i on every trading day τ of the abnormal-return window $[\tau_B, \tau_C]$ around the event day t_0, as an out-of-sample prediction:

$$\widehat{AR}_{i,\tau} = R_{i,\tau} - \left(\hat{\alpha}_i + \hat{\beta}_i R^m_{c,\tau}\right), \qquad \tau \in [\tau_B, \tau_C] \tag{2}$$

where $\hat{\alpha}_i$ and $\hat{\beta}_i$ are estimated through the market model for each commitment i. The abnormal return is the disturbance term of the market model calculated on an out-of-sample basis. We winsorize (clamp, not drop) 5% of the distribution on each side for each event day; robustness checks vary this winsorization rate between 0% and 15%. The estimate varies only between 0.23% and 0.29% across the 0–15% grid (Extended Data Fig. 2). The winsorization is load-bearing for precision rather than magnitude: without censoring, the same estimator gives 0.27% with a 95% confidence interval of −0.04 to 0.57% ($t = 1.72$) — significant at the 10% but not the 5% level — because clamping removes roughly a third of the cross-sectional sampling standard error (over half of the sampling variance), and the reported confidence intervals do not account for the data-dependence of the clamp. The 5%-level significance of the headline estimate therefore holds at the 5–15% grid points but not at 0% or 1% censoring, where the estimate is significant at the 10% level only.

These abnormal returns then allow for an aggregation along two dimensions, across days and across firms, where two aggregation schemes coexist (the balanced and AAR-cumulate series):

(i) Firm-level ("balanced") aggregation: $AR \rightarrow CAR_i \rightarrow CAAR$. Each event's abnormal returns are first cumulated into its own $\widehat{CAR}_i(t_1, t_2)$; the event enters only if $\widehat{AR}_{i,\tau}$ is observed on *every* day $\tau \in [t_1, t_2]$ (complete cases). The CAAR is then the cross-event mean,

$$\widehat{CAAR}^{bal}(t_1, t_2) = \frac{1}{N_{bal}} \sum_{i=1}^{N_{bal}} \widehat{CAR}_i(t_1, t_2), \tag{3}$$

where

$$\widehat{CAR}_i(t_1, t_2) = \sum_{\tau=t_1}^{t_2} \widehat{AR}_{i,\tau} \tag{4}$$

with Brown–Warner cross-sectional standard errors, $SE = sd(\widehat{CAR_i})/\sqrt{N_{bal}}$.

(ii) Calendar-day ("AAR-cumulate") aggregation: $AR \to AAR_\tau \to CAAR$. Abnormal returns are first averaged across events day by day, over the N_τ events with an observed abnormal return on day τ, and the daily averages are then cumulated:

$$\widehat{AAR_\tau} = \frac{1}{N_\tau}\sum_{i=1}^{N_\tau}\widehat{AR}_{i,\tau} \quad \text{and} \quad \widehat{CAAR}(t_1, t_2) = \sum_{\tau=t_1}^{t_2}\widehat{AAR_\tau}, \tag{5}$$

with cross-period-independence (CPI) standard errors, $SE = \sqrt{\sum_{\tau=t_1}^{t_2}\widehat{Var}(\widehat{AAR_\tau})}$.

The two logics differ only in how missing firm-days are handled: the balanced rule drops any event with a missing day in the cumulation window ($N_{bal} \leq N_\tau$), while the AAR-cumulate rule uses every available event-day observation.

Pre-event and dependence diagnostics. A Wald test on the vector of the five daily pre-event average abnormal returns (days -5 to -1), computed on the $n = 479$ events observed on all five days, does not reject the null of no pre-announcement reaction ($\chi^2(5) = 1.8$, $p = 0.87$). Clustered-inference and coarser-cluster robustness checks are reported in Supplementary Note 5.

Multivariate specification

For the multivariate analysis, we compute each event's CAR(0,2) as the complete-case sum of its daily abnormal returns,

$$CAR_i(t_1, t_2) = \sum_{\tau=t_1}^{t_2}\widehat{AR}_{i,\tau}, \tag{6}$$

and regress it by OLS on a set of category dummies:

$$CAR_i(0,2) = \alpha + \mathbf{Country}_i'\boldsymbol{\beta} + \mathbf{Label}_i'\boldsymbol{\gamma} + \mathbf{Sector}_i'\boldsymbol{\delta} + \theta\,\mathbf{COP}_i + \varepsilon_i, \tag{7}$$

where $\mathbf{Country}_i$ is a vector of region dummies (EU, US, non-OECD; reference: other OECD countries), $\mathbf{Label}_i$ a vector of commitment-label dummies (GFANZ signatory, SBTi not-yet-validated target, SBTi validated target, unlabelled target; reference: target modifications), Sector_i is a vector of sector dummies covering four groups formed from the LSEG TRBC economic sectors (Supplementary Table 2; reference: Industry & Utilities), and COP_i indicates an announcement released on the exact COP conference days (COP23–COP28); the COP-exclusion robustness checks additionally use a ±7-day window. Standard errors are CR2 cluster-robust, clustered on the country of incorporation. P-values on CR2 standard errors use classical cluster-robust t-statistics with df $= G - 1$ throughout the main text, Extended Data and SI primary tables. The calibration the CR2 estimator is designed to pair with [57, 58]. Every such table (the multivariate specification, the anti-ESG DiD tables, and the carbon-price tables) has a companion SI table reporting the same estimates under Satterthwaite small-sample degrees of freedom (clubSandwich) instead.

We use a regression-adjusted version of this contrast, recoding the multivariate specification's Country factor (Equation (7)) into a single US indicator (Extended Data Table 2). We successively test with all US commitments (§1) and excluding the commitments exposed to the anti-ESG backlash (§2; announced on or after 1 January 2022 by anti-ESG-state firms (see below)). Finally, we add Parent, Sector and COP controls (similarly as in the main multivariate specification). We use a one-sided test as our main hypothesis (H_1: US > ROW), pre-specified on the strength of both our own univariate finding above (significantly positive returns for US-incorporated firms against a null ROW estimate) and the positive US announcement effect documented by prior literature [29].

Extended Data Table 1 reports the five clustering schemes under classical cluster-robust degrees of freedom (df $= G - 1$); Supplementary Table 3 repeats them under Satterthwaite degrees of freedom. On the pooled sample, the US coefficient (0.42%) is only marginally significant: it reaches the 10% level under listing-based clustering (columns 2 and 5) under classical inference, and only under listing $\times$ sector clustering (column 5) under Satterthwaite inference; it is not significant under incorporation-country clustering, the main specification (column 1, $p = 0.10$ classical, $p = 0.16$ Satterthwaite).

As a general rule, we cluster at the level at which the treatment or contrast of interest varies: the country of incorporation for the cross-country contrasts (region, commitment label, sector and the carbon-price regressions below), and the state of headquarters for the within-US anti-ESG design (below), where treatment is assigned at the state level. Cell sizes follow the univariate AAR-cumulate convention (the per-label $N_{\min}$); the multivariate regression uses a slightly smaller complete-case sample.

Estimation sample for the region coefficients. Equation (7) is estimated on the pooled sample of 519 complete-case events. On this pooled sample the US region coefficient is 0.42 percentage points but only marginally significant ($p = 0.10$, df $= G - 1$; Extended Data Table 1), because it averages a positive pre-backlash premium with a null post-backlash one across the two state groups (Fig. 5): pooling describes a mixture of two regimes rather than a single, stable US effect. The within-US polarization that drives this instability is analyzed directly by the anti-ESG difference-in-differences design below.

The heterogeneity analysis in stock market reaction: carbon exposure

To quantify the relation between the announcement reaction and carbon-pricing exposure, we estimate the following OLS regression on the subsample of events with a matched OECD Effective Carbon Rate:

$$\begin{aligned} CAR_i = \alpha &+ \beta_1 \log_{10}(1 + \mathrm{ECR}_{c,s,y}) + \beta_2 \log_{10}(1 + \mathrm{Emissions}_{i,y}) \\ &+ \beta_3 \times \mathrm{ShareCommitted}_{c,s,y} + \varepsilon_{c,s,i,y} \end{aligned} \tag{8}$$

where $ECR_{c,s,y}$ is the Effective Carbon Rate (in the headline columns, the rate *including free allocations*, `ecr_incl_fa`; an unadjusted-ECR column is reported as well) for country c, sector s and commitment year y; $Emissions_{i,y}$ are the firm's scope 1 & 2 GHG emissions (CO2eq); and $ShareCommitted_{c,s,y}$ is the share of LSEG-covered firms with a climate commitment in the firm's country-sector-year cell, a proxy for how expected a commitment is in the firm's competitive environment. The regressions add these regressors stepwise on a shared complete-case sample (N = 425); the full estimates are reported in Supplementary Table 6. A final specification adds an indicator for US-incorporated firms, since US events are concentrated in the low-ECR bins. Standard errors are CR2 cluster-robust (clubSandwich), clustered at the country-of-incorporation level, matching the headline multivariate specification; they do not propagate market-model AR-estimation variance. A timing-robustness companion (Supplementary Table 7) re-estimates Equation 8 with the peer share computed using only commitments from strictly earlier years (Year < event year), since LSEG records commitment timing mainly at annual precision.

Anti-ESG difference-in-differences

Cell-level estimates. We first compute average CAARs for each of four periods (pre-2021, 2021, 2022 and 2023) and for two groups of US states (anti-ESG states vs. pro-ESG & neutral states). These descriptive cell-level estimates are plotted in main-text Fig. 5, the between-group differences in Extended Data Fig. 5, and the underlying values in Extended Data Table 3.

Specification. We then estimate a cross-sectional difference-in-differences. Unlike the rest of the analysis, this design uses the state of headquarters rather than the state of incorporation. Firms choose their state of registration largely for corporate-law reasons (e.g.

Delaware accounts for a majority of listed US firms irrespective of where they operate), so incorporation carries almost no information about exposure to state-level political pressure and would introduce severe measurement error. Headquarters location is a better proxy, since firms typically site their headquarters where they have substantial operations or historical presence. It remains imperfect: it ignores the geographic spread of operations, customers and workforce, and any resulting mismeasurement will attenuate the estimated differential. The specification compares announcement returns for firms headquartered in pro-ESG or neutral states with those for firms headquartered in anti-ESG states, across the 2022 boundary:

$$\begin{aligned} CAR_i = \alpha + \beta_1 \times \mathbb{1}\{t \geq t_0\} + \beta_2 \text{ProESG}_i \\ + \beta_3 \text{ProESG}_i \times \mathbb{1}\{t \geq t_0\} + \mathbf{X_i}\eta + \varepsilon_i \end{aligned} \tag{9}$$

where $\mathbb{1}\{t \geq t_0\}$ and ProESG_i are respectively time- and space-varying binary indicators, ProESG_i equalling one if the firm's headquarters are located in a pro-ESG or neutral state. We label the indicator by the policy classification rather than by party, since the classification rests on the policy criteria set out below. We estimate Equation 9 on commitment releases by US-headquartered firms from 1 January 2018 onward. Of the 243 US-headquartered events carrying a state classification (86 anti-ESG, 157 pro-ESG or neutral), 242 fall within the 2018–2023 window, and the 228 with a complete-case CAR(0,2) enter the regression (81 anti-ESG, 147 pro-ESG or neutral); descriptive statistics by state group are reported in Extended Data Table 3.

Period boundaries. The DiD threshold t_0 is set at 1 January 2022, a clean year boundary approximating BlackRock's late-January 2022 letter to CEOs, which was followed within weeks by an escalation of the coordinated Republican-led backlash (state anti-ESG statutes, treasury divestment lists and attorney-general investigations [59]). The 2021 period opens at Biden's inauguration (20 January 2021), so releases from 1–19 January 2021 fall in the pre-2021 bin; the same boundary serves as the placebo cutoff in the falsification test (Extended Data Table 6, col. 6).

Timing and trend robustness. Three further columns of Extended Data Table 6 probe that timing. State legislation predates the 2022 escalation: Texas SB 13 and SB 19 were signed in June 2021. Re-dating t_0 to June 2021 halves the interaction to +1.1 percentage points and renders it insignificant (Satterthwaite $p = 0.18$; col. 2), so the timing of any repricing is not precisely identified. Two columns then re-estimate the baseline no-controls specification: restricting the pre-period to 2021 onward (from 20 January 2021), which drops the small and opposite-signed pre-2021 cells, leaves the interaction at +1.7 percentage points ($p = 0.003$, df $= G - 1$; Satterthwaite $p = 0.007$; col. 4), and adding group-specific linear time trends (years since 1 January 2018) leaves it at +2.1 ($p = 0.031$; Satterthwaite $p = 0.042$; col. 5).

State classification. The criteria are designed to record whether a state, acting through an officer empowered to commit it, had taken a formal position against ESG-linked finance by the end of the sample window; these actions would be considered by investors as political exposure for a firm headquartered there, because it signals that state contracting decisions, litigation or legislative attention may follow. We classify as *anti-ESG* all states that, as of 31 December 2023, (i) had enacted a statute restricting the consideration of ESG factors in state contracting or (ii) signed the multi-state letter asking Congress to overturn the Department of Labor's ESG investing rule [60]. Interestingly, all 19 states that joined Governor Ron DeSantis' anti-ESG alliance [61] are included in our classification. Isolated positions taken by state financial officers (treasurers, comptrollers and auditors) and not supported by the state government were excluded, most visibly the coalition letters addressed to Congress and to the Securities and Exchange Commission over 2021–2023. The resulting classification is listed in Supplementary Table 10.

Alternatively, we can classify states by the party of the governor at the end of our sample window (December 2023). This is a less precise proxy for the state ESG policy environment, since it ignores the actions taken by governors and attorneys general in the anti-ESG coalition, and it is also less balanced: 24 of the 34 states in the regression sample are governed by Democrats, leaving only 10 Republican-governed states. However, it enables a check the robustness of the effect using an indisputable classification. The resulting interaction coefficient is +1.95 percentage points ($p = 0.001$, df $= G - 1$; Extended Data Table 6, col. 8; Satterthwaite $p = 0.004$).

Relabelling Robustness. The classification is a single end-2023 snapshot applied uniformly to all events, so states whose ESG actions were initiated and then discontinued within the sample window are coded pro-ESG or neutral. This concerns Arizona, whose attorney-general-led actions of 2022 were discontinued in 2023 under a new attorney general; re-coding Arizona to the anti-ESG group leaves the interaction at +1.6 percentage points ($p = 0.012$, df $= G - 1$; Extended Data Table 6, col. 7; Satterthwaite $p = 0.020$). At the other end, Virginia and North Carolina, both governed by Democrats when the key legislation passed, are the most debatable entries in the anti-ESG group; re-coding them to the pro-ESG/neutral group leaves the interaction at +2.0 percentage points ($p = 0.004$, df $= G - 1$; Satterthwaite

$p = 0.015$; Extended Data Table 6, col. 3). Re-coding either the most contestable pro-ESG state (Arizona) or the two most contestable anti-ESG states (Virginia, North Carolina) therefore moves the estimate in the same direction as expected from their reassignment, by 0.2 to 0.6 percentage points, without changing the significance.

Because the interaction is large relative to the average announcement effect and the number of clusters is modest, we run two further diagnostics on the baseline (no-controls) specification. First, leave-one-state-out estimates (Supplementary Fig. 8): re-estimating the specification 34 times, dropping one headquarters state at a time, leaves the interaction between +1.8 and +2.2 percentage points (the largest single-state shifts come from dropping Pennsylvania or Texas, which lower the estimate, and New York or California, which raise it), significant at the 1% level. Second, randomization inference: we reassign the anti-ESG/pro-ESG-or-neutral label across the 34 headquarters states present in the regression sample, freely and without political or geographic stratification, holding the number of states in each group fixed at the observed split (15 anti-ESG, 19 pro-ESG-or-neutral). Each state keeps its own announcements, so unequal event counts per state are preserved by construction. For each of 10,000 reassignments we re-estimate Equation 9 without controls and record the interaction coefficient, so the test statistic is identical to the reported estimator. The observed interaction exceeds, in absolute value, all but 5 of the 10,000 placebo interactions, a randomization p-value of 0.0005.

Controls and inference. X_i is a set of firm- and commitment-level controls: an SBTi indicator, log market capitalization, sector dummies and CO_2 emissions (company fixed effects are unavailable, the design being cross-sectional). Standard errors are CR2 cluster-robust, clustered by *state of headquarters* (the level at which the state ESG policy environment varies) and inference uses classical CR2 t-statistics with df $= G - 1$, as throughout; a Satterthwaite small-sample-df version is reported as an SI robustness check (Supplementary Table 14). Extended Data Table 4 reports the main results. Extended Data Table 5 reports an inference sensitivity analysis: the interaction remains significant at the 1% level under heteroskedasticity-robust (HC1) standard errors; under CR2 clustering on the country of incorporation the interaction remains significant at the 5% level in the baseline specification and at the 1% level with controls. However, within a US-headquartered sample that scheme leaves only G = 7 highly unbalanced clusters, where small-sample cluster-robust inference is known to be unreliable in either direction, so this column should be read with caution regardless of the p-value it returns.

Composition. The design compares largely different firms across periods: of the 196 firms in the regression sample, only 16 announce both before and after the boundary. In addition, the announcer mix shifts differentially: the Energy & Mining share of anti-ESG-state announcements falls from 34% to 12%, while pro-ESG-state announcements shift toward SBTi and Finance & Tech announcers (Supplementary Table 11). Two specifications hold sector composition fixed: sector $\times$ post-period fixed effects leave the interaction at +1.9 percentage points (Satterthwaite $p = 0.010$), and excluding Energy & Mining leaves it at +1.8 ($p = 0.002$; Supplementary Table 12). Because exit from announcing is itself a plausible response to the backlash, these composition-constant estimates bound sector-composition effects but do not separate repricing from selection into announcing. More generally, the post-Fink's letter (called post-2022 for simplicity) indicator bundles every shock that loads differentially on the two state groups' announcers over 2022–2023; the energy-sector channel is the only one tested directly.

Reporting summary

Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

Use of large language models

Anthropic's Claude (Claude Code, Opus 5) was used to draft, refactor and document parts of the R replication code and to assist with debugging. All analytical choices (sample construction, variable definitions, model specification, estimation and inference) were made by the authors. Every model-assisted script was read, tested and reconciled against the results reported here by the authors, who take full responsibility for the content of this paper.

Data availability

Press-release event data (announcement dates, headlines and Refinitiv Identifier Codes), firm characteristics and fundamentals, daily equity close prices, the country FR Equity Price Return Indices and the S&P 500 were obtained from LSEG (formerly Refinitiv) under a commercial licence, through the LSEG Workspace environment (Press aggregator, Screener and Monitor apps, Datastream); the exact `TR.*` field codes are listed in Supplementary Note 4. These data are proprietary and cannot be redistributed by the authors, but qualified researchers can obtain them through an institutional LSEG subscription or a direct licence agreement (https://www.lseg.com/en/data-analytics). Effective carbon rates come from the OECD Detailed Effective Carbon Rates database (2023 edition, covering 2018–2021), which was not publicly downloadable at the time of analysis; the OECD supplied it free of charge on request (see Acknowledgements), and other researchers can obtain it on the same terms from the OECD Centre for Tax Policy and Administration.

All derived data required to reproduce the reported results are deposited in a private GitHub repository, to which editors and referees are granted access for the purpose of peer review only. Because the underlying announcements are public press releases, the repository releases the event list in full (company name, announcement (Day 0) date, target-label classification and US state-of-headquarters ESG classification) together with the authors' anti-ESG state classification and the estimated daily abnormal returns and event-level CAR(0,2) underlying every figure and table. The LSEG-proprietary RICs and the licensed price and fundamentals series are included for referee verification only and are not redistributable. Source data for the main-text figures are provided with this paper. Requests regarding the derived data should be addressed to the corresponding author.

Code availability

All analyses were conducted in R version 4.3.3. The complete replication code (event-panel construction, market-model estimation and abnormal-return computation, and the multivariate, carbon-pricing and anti-ESG difference-in-differences regressions, together with all robustness checks, figures and tables) is provided in the same private GitHub repository, to which the editors and referees are granted access for peer review. The repository includes a README that documents the software environment (R 4.3.3 and the versions of the principal packages used) and the exact LSEG data items and OECD variables required as inputs, so that researchers holding the necessary licences can reproduce the results end-to-end from the raw sources. The derived event list and abnormal returns needed to reproduce the reported estimates without access to the raw LSEG and OECD data are included in the same repository (see Data availability).

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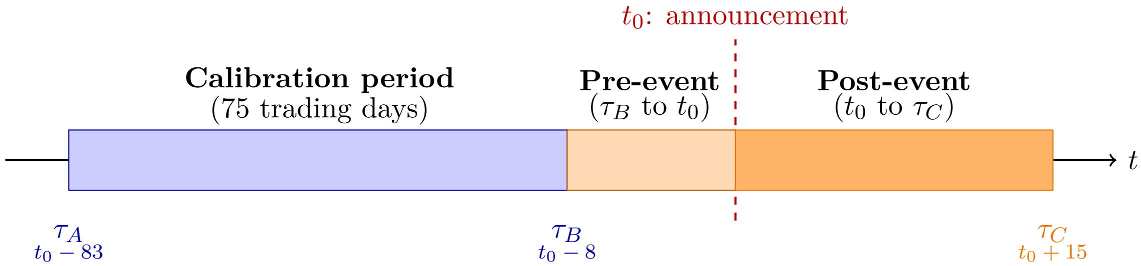
Author contributions

M.A.: Data curation, Methodology, Formal analysis, Writing – original draft. R.C.: Conceptualization, Methodology, Formal analysis, Writing – review & editing, Supervision.

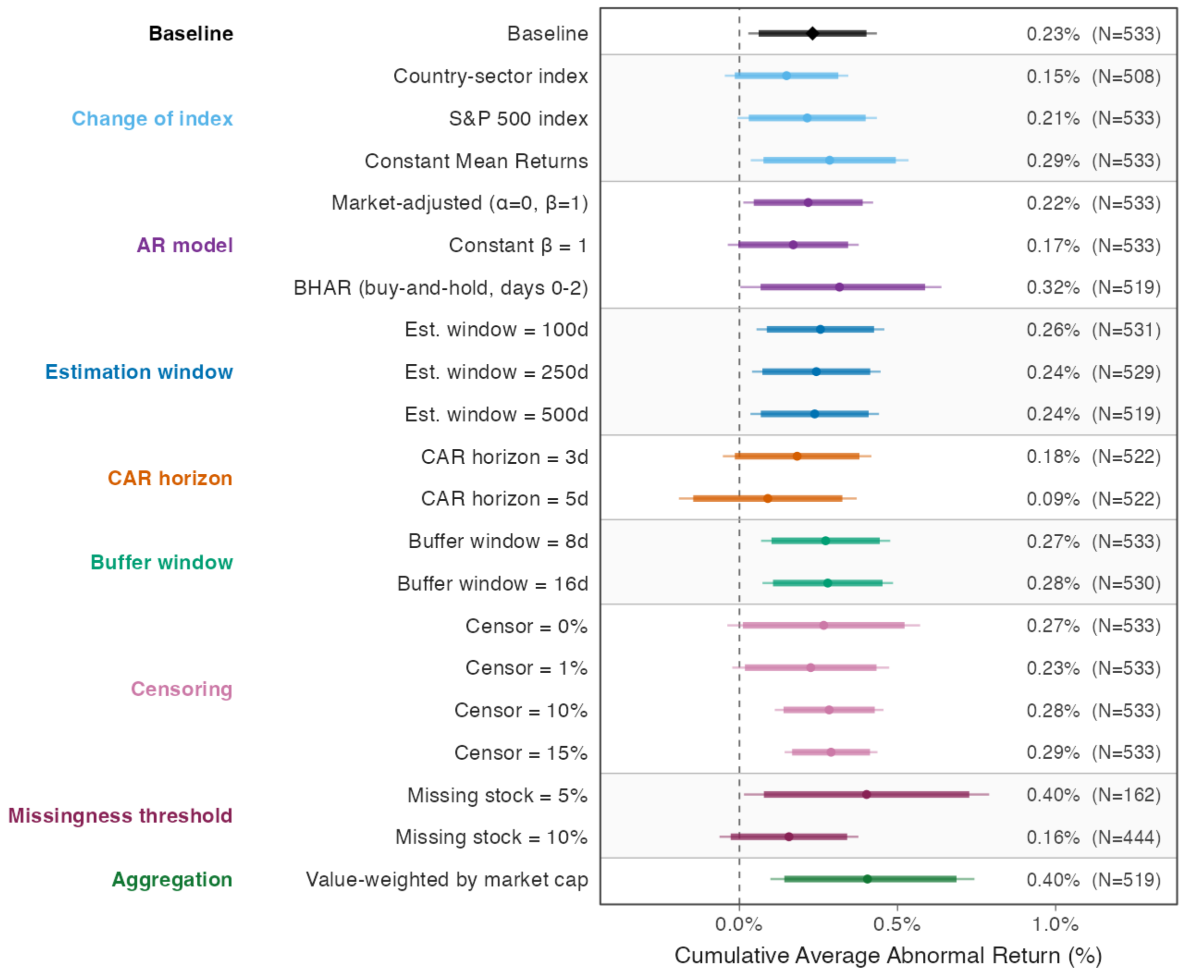
Both authors contributed to the interpretation of the results, critically revised the manuscript, and approved the final version for publication.

Competing interests

The authors declare no competing interests.

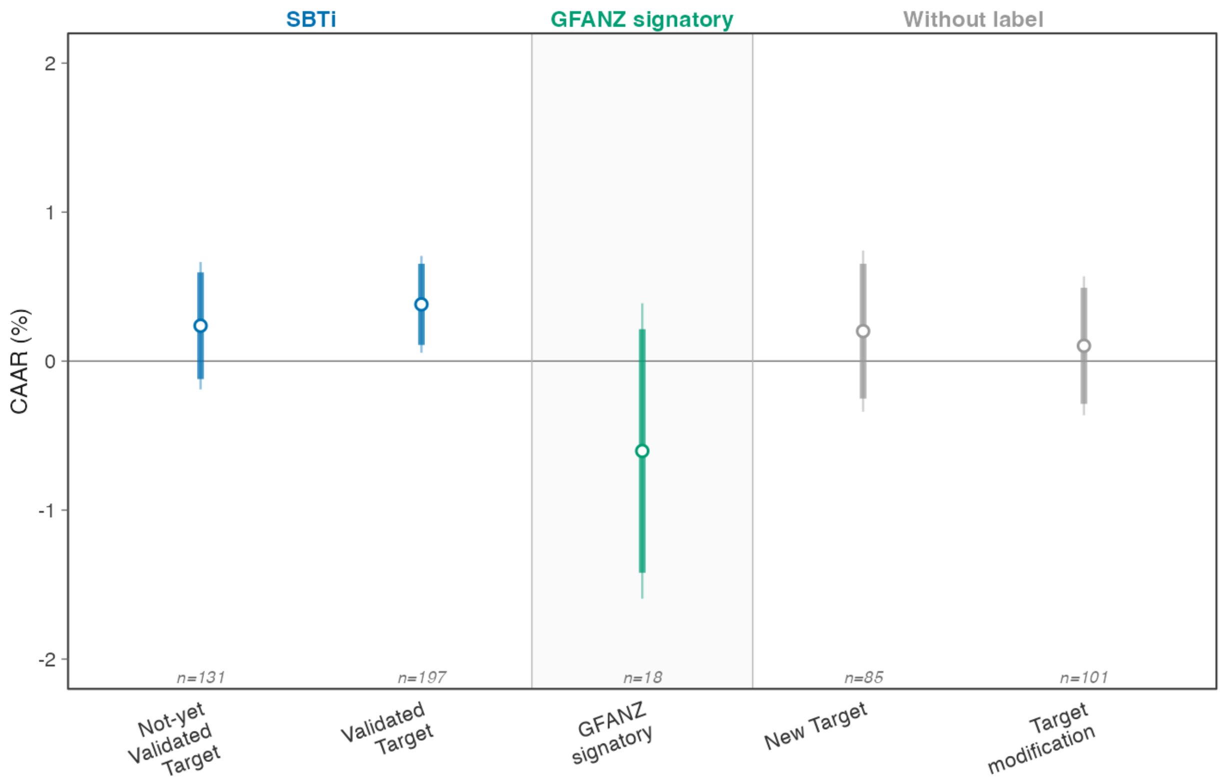


Extended Data Fig. 1: Event-study timeline. Trading-day indexing relative to the announcement day t_0 (horizontal axis not to scale). Three dates organize the design: the calibration (estimation) window runs over $(\tau_A, \tau_B]$, with $\tau_A = t_0 - 83$ and $\tau_B = t_0 - 8$ (75 trading days); abnormal returns are computed out of sample over $[\tau_B, \tau_C]$, with $\tau_C = t_0 + 15$; the headline CAR cumulates days t_0 to $t_0 + 2$; and the figures display $t_0 \pm 5$ days. The market-model parameters $(\hat{\alpha}_i, \hat{\beta}_i)$ are estimated on the calibration period and used to construct out-of-sample abnormal returns over the pre- and post-event windows. In the baseline there is no buffer between the calibration window and the abnormal-return window (they meet at τ_B), so the abnormal return plotted at τ_B is mechanically attenuated; the headline CAR(0,2) is unaffected. Robustness checks insert an 8- or 16-day buffer between the calibration window and τ_B.



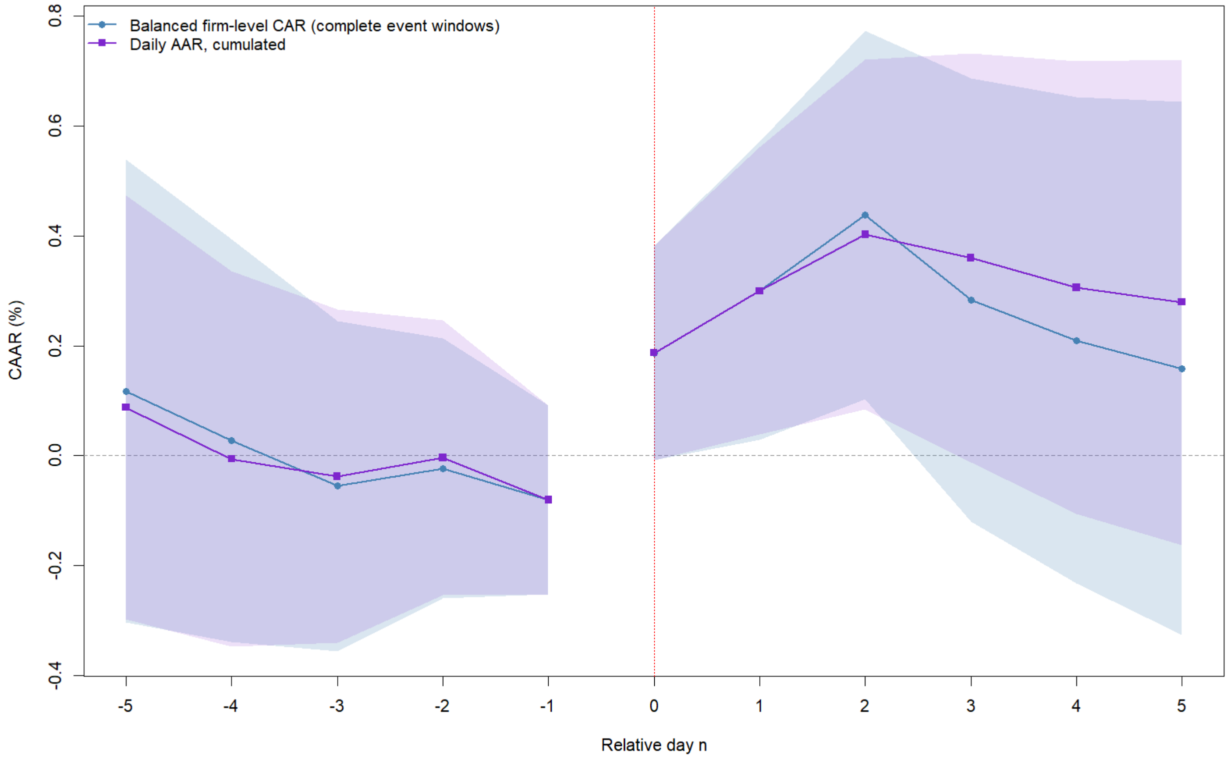
Extended Data Fig. 2: Robustness checks: Alternative modeling of abnormal returns

Notes: Forest plot of the CAAR(0,2) across specifications; thick and thin bars are 90% and 95% confidence intervals. Baseline: CAR(0,2), country benchmark index, 75-trading-day estimation window, no estimation–event buffer, 5% per-tail censoring of daily abnormal returns, 15% maximum missing share in estimation-window returns. Light blue: benchmark index (country–sector index, S&P 500, constant-mean returns). Purple: abnormal-return model (market-adjusted, constant $\beta = 1$, buy-and-hold); the BHAR row uses simple compounded returns bought at the day -1 close, is not winsorized, and retains complete-price cases only. Orange: CAR horizon (3- and 5-day windows). Dark blue: estimation window (100/250/500 days). Green: buffer window (8/16 days). Pink: censoring rate (0/1/10/15%). Dark red: maximum missing-return share (5/10%). Green (bottom): value-weighted CAAR. BHAR and value-weighted rows use percentile-bootstrap confidence intervals; all other rows use cross-period-independence standard errors.



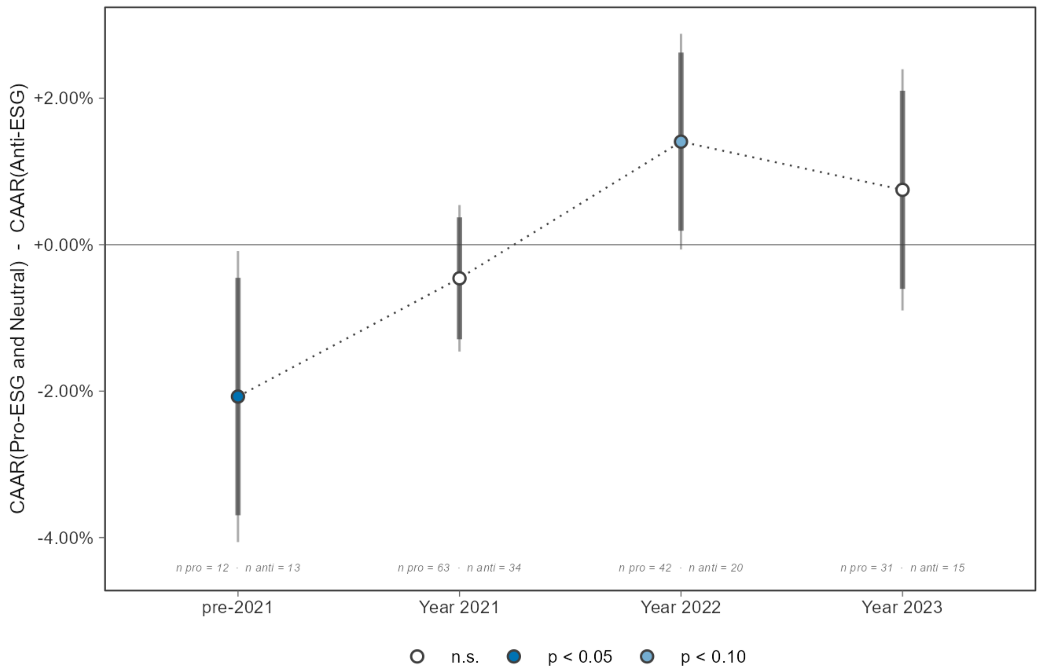
Extended Data Fig. 3: Heterogeneity by certification label

Notes: CAAR(0,2) by commitment type (AAR-cumulate rule; thick and thin bars are 90% and 95% confidence intervals, cross-period-independence standard errors). SBTi not-yet-validated pools the SBTi commitment and submission steps (N = 131); SBTi-validated targets, N = 197; GFANZ signatories (NZIA, NZAM, NZBA), N = 18; unlabelled new targets, N = 85; target modifications, N = 101. Cross-label differences are not statistically significant at the 5% level; the GFANZ cell is small and its negative point estimate should be read with caution. Confidence intervals are not adjusted for multiple comparisons.



Extended Data Fig. 4: Cumulative average abnormal returns around commitment releases, US-incorporated firms only: balanced firm-level CAR versus AAR-cumulate

Notes: Same construction as the headline butterfly figure in the main text, restricted to announcements by US-incorporated firms (239 events enter the AR estimation and 237 are retained). Daily abnormal returns are the same market-model residuals as in the full sample, including the single global winsorization pass (5% per tail per event day, bounds from the full event panel); only the cross-sectional aggregation is restricted to the US subsample. Blue: balanced firm-level CAR (complete-day firms; Brown–Warner cross-sectional standard errors); at day +2 the CAAR is 0.44% (N = 223). Purple: daily AARs cumulated over the window (cross-period-independence standard errors); at day +2 the CAAR is 0.40% (N = 227). Shaded bands are 95% confidence intervals. Neither interval propagates market-model estimation variance.



Extended Data Fig. 5: Difference in announcement returns between pro-ESG/neutral and anti-ESG state groups, by period

Notes: Pro-ESG-or-neutral minus anti-ESG difference in CAAR(0,2) (percentage points) for the four periods of main-text Fig. 5; the underlying cell estimates are reported in Extended Data Table 3. Thick and thin bars are 90% and 95% confidence intervals from independent-samples standard errors ($\sqrt{SE^2_{\text{pro}} + SE^2_{\text{anti}}}$, $df = \min(N) - 1$; the two state buckets are disjoint event sets). The pre-2021 difference (-2.1 percentage points) is significant at the 5% level and opposite in sign to the post-2022 differences; the 2022 difference is significant at the 10% level only. Standard errors do not propagate market-model AR-estimation variance.

Extended Data Table 1: Multivariate analysis: alternative standard-error clustering schemes

	Dependent variable:				
	CAR(0,2)				
	Incorporation (main)	Listing	Region × sector	Incorp. × sector	Listing × sector
EU	0.001	0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Non-OECD	0.003	0.003	0.003	0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
US	0.004	0.004*	0.004	0.004	0.004*
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
GFANZ signatory	−0.009*	−0.009*	−0.009***	−0.009	−0.009
	(0.004)	(0.005)	(0.003)	(0.005)	(0.006)
SBTi not-yet-validated target	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
SBTi-validated target	0.001	0.001	0.001	0.001	0.001
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Unlabeled target	−0.001	−0.001	−0.001	−0.001	−0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Customer Services	0.001	0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Energy and Mining	0.002	0.002	0.002	0.002	0.002
	(0.004)	(0.007)	(0.003)	(0.004)	(0.005)
Finance and Tech	0.003	0.003	0.003**	0.003	0.003
	(0.002)	(0.003)	(0.001)	(0.003)	(0.004)
COP	−0.002	−0.002	−0.002	−0.002	−0.002
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
Constant	−0.002	−0.002	−0.002	−0.002	−0.002
	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)
p-value (US)	0.101	0.094	0.108	0.101	0.072
Clusters G	39	32	16	85	75
Observations	519	519	519	519	519
R^2	0.013	0.013	0.013	0.013	0.013

Note: *p<0.1; **p<0.05; ***p<0.01, based on CR2 t-statistics with df = G-1 (classical cluster-robust; clubSandwich sandwich variance); an SI robustness twin reports Satterthwaite small-sample-df p-values/stars on the same estimates (Supplementary Table 3). All five columns report the same OLS regression (Equation 7); the dependent variable is the event-level CAR(0,2), the complete-case sum of daily abnormal returns over days 0–2. Pooled US sample (no anti-ESG/post-Fink exclusion). Only the cluster definition for the CR2 standard errors varies across columns. Column (1), the main specification, clusters on country of incorporation (G = 39); column (2) on country of exchange listing (G = 32); columns (3)–(5) on country × sector cells (column (3) uses the four macro-regions, columns (4)–(5) the raw countries). All five are one-way CR2 clusterings rather than Cameron–Gelbach–Miller two-way inference. Standard errors are computed over event-level CAR outcomes and do not propagate market-model AR-estimation variance from the daily abnormal-return step.

Extended Data Table 2: US-versus-rest-of-world contrast: sensitivity to anti-ESG backlash exposure and controls

	Dependent variable:		
	CAR(0,2)		
	Pooled (backlash-exposed included)	Excl. backlash-exposed	Excl. backlash-exposed + controls
	(1)	(2)	(3)
US ($\mathbb{1}$)	0.003*	0.005***	0.005***
	(0.002)	(0.002)	(0.002)
p-value (one-sided H_1: US > ROW)	0.026	0.003	0.005
p-value (two-sided)	0.053	0.006	0.009
p-value (one-sided, Satterthwaite)	0.032	0.005	0.008
p-value (two-sided, Satterthwaite)	0.065	0.011	0.016
Controls (Parent, Sector, COP)	No	No	Yes
Clusters G	39	39	39
Observations	519	489	489
R^2	0.004	0.008	0.019

Note: *p<0.1; **p<0.05; ***p<0.01, two-sided, based on CR2 t-statistics with df = G-1 (classical cluster-robust; clubSandwich sandwich variance), clustered on country of incorporation; a Satterthwaite small-sample-df version of both p-values is reported as an additional pair of rows below (same point estimates and standard errors, only the reference distribution differs). In all columns, the four-level Country factor of the main multivariate regression (Equation 7; EU, Non-OECD, US; reference: Other OECD) is recoded into a single US indicator, directly testing the US-versus-rest-of-world contrast referenced in the main text, rather than comparing two separate subsample significance tests. Cols. (1)–(2) report the bivariate US indicator alone, with no controls, so as not to bundle the sample-exposure comparison with a controls comparison. Col. (1): pooled sample, all US commitments (N = 519). Col. (2): US commitments announced on or after 1 January 2022 by firms headquartered in anti-ESG states (”backlash-exposed”) excluded (N = 489). Col. (3): same excluded sample as col. (2), with the Parent, Sector and COP controls of the main multivariate specification added (coefficients omitted for brevity), to show the contrast is not an artifact of omitting them. One-sided p tests H_1: US > ROW, pre-specified given the positive US finding in prior literature [62]; two-sided p is the primary test. Standard errors are computed over event-level CAR outcomes and do not propagate market-model AR-estimation variance.

Extended Data Table 3: Anti-ESG heterogeneity: point estimates behind Figure 5

Period	ESG-state bucket	N	CAAR (%)	SE (%)	90% CI (%)	95% CI (%)	Share (%)
pre-2021	Anti-ESG States	13	1.26	0.65	[0.11, 2.41]	[-0.15, 2.67]	27.1
pre-2021	Pro-ESG and Neutral States	12	-0.82	0.63	[-1.95, 0.32]	[-2.20, 0.57]	27.1
Year 2021	Anti-ESG States	34	0.77	0.38	[0.13, 1.41]	[0.00, 1.54]	19.0
Year 2021	Pro-ESG and Neutral States	63	0.31	0.31	[-0.21, 0.84]	[-0.32, 0.94]	35.3
Year 2022	Anti-ESG States	20	-0.78	0.60	[-1.83, 0.27]	[-2.05, 0.49]	13.7
Year 2022	Pro-ESG and Neutral States	42	0.63	0.36	[0.02, 1.23]	[-0.10, 1.35]	27.3
Year 2023	Anti-ESG States	15	0.07	0.61	[-1.01, 1.15]	[-1.24, 1.39]	9.7
Year 2023	Pro-ESG and Neutral States	31	0.82	0.46	[0.03, 1.61]	[-0.12, 1.77]	20.6

Note: Sample restricted to events whose firm is headquartered in a US state with a classified ESG stance. CAAR(0,2) is computed by cumulating daily AARs within each cell. SE uses the cross-period-independence rule $\sqrt{\sum_k Var(AAR_k)}$ and Student-t critical values with $N_{min} - 1$ df, where N_{min} is the smallest daily cell count over the cumulation window. Share = the cell's fraction of *all* commitment announcements in the period (US + non-US), so Pro-ESG + Anti-ESG shares sum to the US share of the period.

N convention: N is the minimum daily cross-sectional count over the $\text{CAR}(0, 2)$ window (AAR-cumulate rule). The DiD regression (Table 4) uses complete-case $\text{CAR}(0, 2)$, so its N may be 1–2 events smaller. Share values use raw period totals computed before the CAR availability filter; identical share values in two cells of the same period reflect equal raw group counts. Analysis period: 2018-01-01 (period start) through end of sample; 2017 events excluded.

Extended Data Table 4: Anti-ESG DiD on US-headquartered commitment releases

	Dependent variable:			
	CAR(0,2)			
	(1)	(2)	(3)	(4)
Pro-ESG/neutral state ($\mathbb{1}$)	-0.008^{**}	-0.008^{**}	-0.009^{*}	-0.010^{**}
	(0.003)	(0.003)	(0.004)	(0.005)
Post-Fink ($\mathbb{1}$[Date $\geq$ 2022-01-01])	-0.013^{***}	-0.013^{***}	-0.014^{***}	-0.015^{***}
	(0.003)	(0.003)	(0.003)	(0.003)
Pro-ESG/neutral $\times$ Post-Fink (β_3)	0.020^{***}	0.020^{***}	0.021^{***}	0.022^{***}
	(0.005)	(0.005)	(0.005)	(0.006)
log(1 + Market Cap)		-0.001	-0.001	-0.002
		(0.003)	(0.003)	(0.003)
log(1 + CO_2 Sc. 1+2)			-0.001	-0.0004
			(0.002)	(0.002)
SBTi indicator				-0.002
				(0.004)
Constant	0.009^{***}	0.018	0.025	0.036
	(0.002)	(0.036)	(0.041)	(0.043)
Sector fixed effects	No	No	No	Yes
Observations	228	228	214	214
R^2	0.037	0.037	0.039	0.048

Dependent variable is event-level CAR(0,2), not CAAR.

Note: *p<0.1; **p<0.05; ***p<0.01. CR2 cluster-robust standard errors (clubSandwich::vcovCR) clustered on US state of headquarters ($G \approx 34$) in parentheses. Stars based on CR2 cluster-robust t-statistics with df = G-1 (classical cluster-robust); a Satterthwaite small-sample-df version is reported as an SI robustness check (Supplementary Table 14). Regression SEs are over event-level CAR outcomes and do not propagate market-model AR-estimation variance. Sample: US-headquartered events with a state classification; the sample window starts 2018-01-01 (no retained event occurs in 2018). Pro-ESG and Neutral States vs Anti-ESG States (see Methods, State classification). Post threshold: 1 January 2022 (approximating Fink's January 2022 letter).

Extended Data Table 5: Anti-ESG DiD: inference sensitivity

	Dependent variable:					
	CAR(0,2)					
	(1)	(2)	(3)	(4)	(5)	(6)
Pro-ESG/neutral state ($\mathbb{1}$)	-0.008^{**}	-0.008^{*}	-0.008^{*}	-0.010^{**}	-0.010^{**}	-0.010^{*}
	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)
Post-Fink ($\mathbb{1}$[Date $\geq$ 2022-01-01])	-0.013^{***}	-0.013^{*}	-0.013^{**}	-0.015^{***}	-0.015^{*}	-0.015^{***}
	(0.003)	(0.006)	(0.005)	(0.003)	(0.006)	(0.006)
Pro-ESG/neutral $\times$ Post-Fink (β_3)	0.020^{***}	0.020^{**}	0.020^{***}	0.022^{***}	0.022^{***}	0.022^{***}
	(0.005)	(0.006)	(0.007)	(0.006)	(0.005)	(0.007)
log(1 + Market Cap)				-0.002	-0.002	-0.002
				(0.003)	(0.003)	(0.003)
log(1 + CO_2 Sc. 1+2)				-0.0004	-0.0004	-0.0004
				(0.002)	(0.001)	(0.002)
SBTi indicator				-0.002	-0.002	-0.002
				(0.004)	(0.005)	(0.004)
Constant	0.009^{***}	0.009^{*}	0.009^{***}	0.036	0.036	0.036
	(0.002)	(0.004)	(0.003)	(0.043)	(0.045)	(0.038)
Sector fixed effects	No	No	No	Yes	Yes	Yes
SE scheme	HQ-state CR2	Inc.-country CR2	HC1	HQ-state CR2	Inc.-country CR2	HC1
p-value (β_3)	0.000	0.017	0.003	0.000	0.003	0.002
p-value (β_3, Satterthwaite)	0.001	0.152	—	0.002	0.097	—
Clusters G	34	7	—	33	7	—
Observations	228	228	228	214	214	214
R^2	0.037	0.037	0.037	0.048	0.048	0.048

Note: Columns (1)–(3) re-estimate the baseline DiD and columns (4)–(6) the full-controls specification of the main anti-ESG table under three inference schemes: CR2 cluster-robust SEs clustered by state of headquarters (primary), CR2 clustered by country of incorporation, and heteroskedasticity-robust HC1. Cluster-robust stars use CR2 t-statistics with df = G-1 (classical cluster-robust); HC1 stars use $df = N - k$. Coefficients are identical across schemes by construction; only the SEs and stars vary. A Satterthwaite small-sample-df version of the cluster-robust p-values is reported as an additional row (HC1 has no Satterthwaite analogue, shown as —).

Extended Data Table 6: Anti-ESG DiD robustness: treatment date, state classification, pre-trends and a pre-period placebo

	Dependent variable:							
	CAR(0,2)							
	Full controls (Post ≥ 2022-01-01)	Post ≥ 2021-06-01 (Texas SB 13/19)	VA & NC re-coded pro	Pre-period 2021 only	Group-specific trends	Placebo: pre-2022 sample	Arizona re-coded anti	Republican governor (Dec 2023)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pro-ESG/neutral state ($\mathbb{1}$)	-0.010^{**}	-0.008	-0.010^{**}	-0.005	-0.007	-0.021^{***}	-0.009^{***}	-0.009^{***}
	(0.005)	(0.007)	(0.005)	(0.004)	(0.013)	(0.006)	(0.003)	(0.003)
Post (column-specific cutoff)	-0.015^{***}	-0.010^{***}	-0.015^{***}	-0.012^{***}	-0.015^{**}	-0.005	-0.010^{**}	-0.013^{***}
	(0.003)	(0.004)	(0.004)	(0.003)	(0.007)	(0.004)	(0.005)	(0.003)
Pro-ESG/neutral × Post	0.022^{***}	0.011	0.020^{***}	0.017^{***}	0.021^{**}	0.016^{**}	0.016^{**}	0.020^{***}
	(0.006)	(0.007)	(0.006)	(0.005)	(0.009)	(0.007)	(0.006)	(0.005)
log(1 + Market Cap)	-0.002	-0.001	-0.002					
	(0.003)	(0.003)	(0.003)					
log(1 + CO_2 Sc. 1+2)	-0.0004	0.0001	-0.001					
	(0.002)	(0.002)	(0.002)					
SBTi indicator	-0.002	-0.001	-0.003					
	(0.004)	(0.004)	(0.004)					
Time trend (years)					0.001			
					(0.003)			
Pro-ESG/neutral × trend					-0.0003			
					(0.004)			
Constant	0.036	0.030	0.038	0.008^{***}	0.005	0.013^{***}	0.009^{***}	0.010^{***}
	(0.043)	(0.038)	(0.044)	(0.002)	(0.009)	(0.003)	(0.001)	(0.001)
Sector FE	Yes	Yes	Yes	No	No	No	No	No
p-value (β_3)	0.000	0.159	0.004	0.003	0.031	0.031	0.012	0.000
p-value (β_3, Satterthwaite)	0.002	0.177	0.015	0.007	0.042	0.046	0.020	0.004
State cluster (G)	33	33	33	32	34	31	34	34
Observations	214	214	214	203	228	122	228	228
R^2	0.048	0.016	0.035	0.033	0.038	0.046	0.024	0.031

Note: *p<0.1; **p<0.05; ***p<0.01. CR2 cluster-robust SEs, clustered by US state of headquarters, in parentheses; stars use CR2 t-statistics with df = G-1 (classical cluster-robust); a Satterthwaite small-sample-df version of the *p*-value is reported as an additional row below (same point estimates and standard errors, only the reference distribution differs). Col. (1): the specification with full controls (col. (4) of Table 4), Post = $\mathbb{1}$[Date ≥ 2022-01-01]. Col. (2): Post re-defined as $\mathbb{1}$[Date ≥ 2021-06-01] to capture Texas SB 13 (signed June 2021), which predates the Fink letter. Col. (3): Virginia and North Carolina — both had Democratic governors at the time of key legislation, the most contestable Anti-ESG-bucket classifications — re-coded to the Pro-ESG/neutral group, mirroring col. (7)'s Arizona re-code. Cols. (4)–(8) use the no-controls specification (col. (1) of Table 4). Col. (4) restricts the pre-period to 2021 (from 20 January 2021), dropping the small, opposite-signed pre-2021 cells. Col. (5) adds group-specific linear time trends (years since 1 January 2018). Col. (6) is a falsification test: pre-2022 events only, with a placebo cutoff at Biden's inauguration (20 January 2021); its significant interaction indicates that part of the relative divergence between the two state groups predates the 2022 backlash. Col. (7) re-codes Arizona — the most contestable pro/neutral classification under criterion (iii) of the state classification, its 2022 attorney-general-led ESG actions having been discontinued in 2023 — to the anti-ESG group. Col. (8) replaces the criteria (i)–(iii) state classification with a single alternative rule: a state is Anti-ESG if and only if it had a Republican governor as of 31 December 2023 (26 states; Louisiana is coded Democratic, since Jeff Landry (R) was not inaugurated until 8 January 2024). This flips four states with a Democratic governor out of the criteria (i)–(iii) Anti-ESG bucket (Kansas, Kentucky, Louisiana, North Carolina) and two states with a Republican governor into it (Nevada, Vermont), net −2 states relative to the main classification.

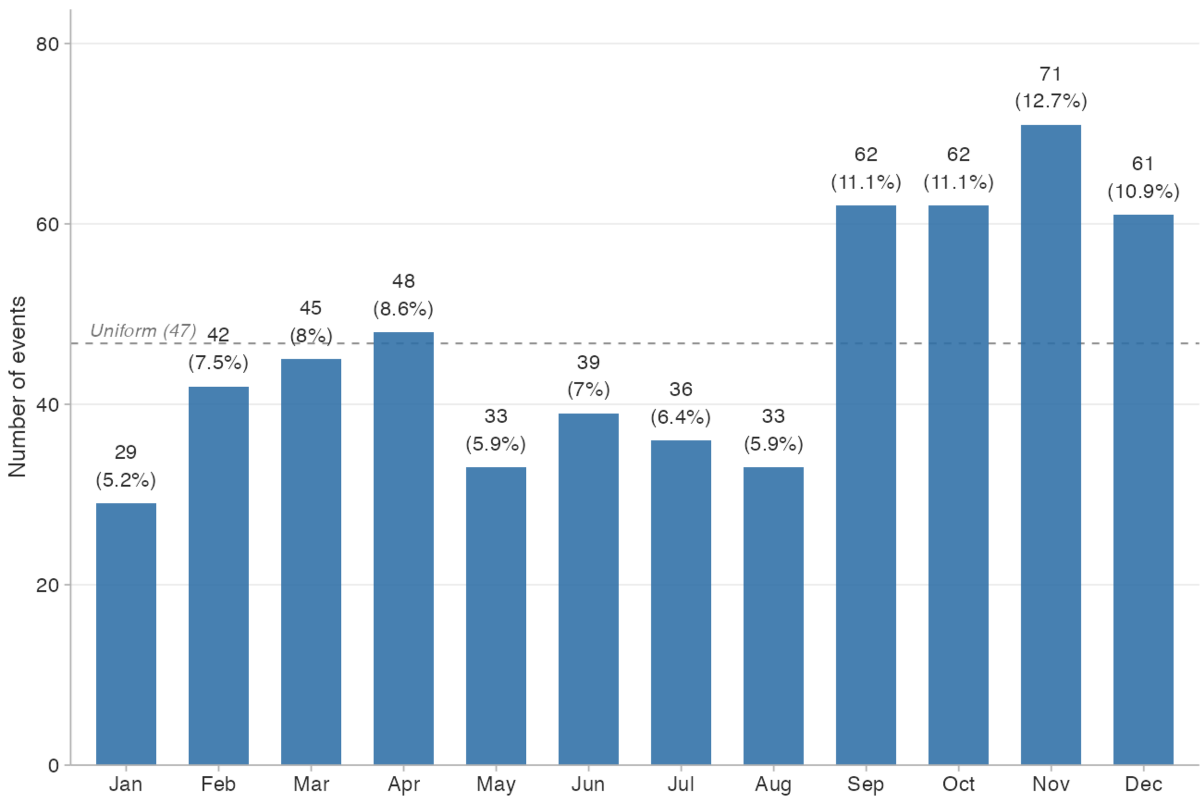
Supplementary Information

Modest and politically contingent stock market value for corporate climate pledges

Mathias Abitbol Renaud Coulomb

Extended Figures and Tables

Data description



Supplementary Figure 1: Distribution of commitment announcements by month

Notes: Number of retained commitment announcements by calendar month of release (N = 561 events). The dashed line is the expected count under a uniform distribution across months. Elevated counts in September–December coincide with the run-up to and holding of the annual COP meetings.

Press-release documentation

(a)

June 13, 2022

Helios Technologies Sets Goal of Achieving Net Zero GHG Emissions By 2050

SARASOTA, Fla.--(BUSINESS WIRE)-- Helios Technologies, Inc. (NYSE: HLIO) ("Helios" or the "Company"), a global leader in highly engineered motion control and electronic controls technology for diverse end markets, announced today it has set a long-term commitment to achieve net zero greenhouse gas emissions (GHG) by 2050 for its operated assets.

"We take our societal responsibility seriously and this newest commitment adds to our earlier recognition that Helios Technologies' environmental, social and governance (ESG) responsibilities are the living, operating principles by which we measure ourselves and shape our behavior on a daily basis," commented Josef Matosevic, President and Chief Executive Officer. "Initial efforts and steps are underway as we develop more detailed plans to reach this long-term commitment. At the Board level, we established an ESG Committee as a component of our governance structure to assist the Company in its oversight of corporate social responsibilities, significant public policy issues, health and safety, and climate-change related trends and other global ESG matters.

"We believe this is an important step forward in building a more sustainable company that can create long-term value for all stakeholders."

Helios has completed its first Scope 1 and Scope 2 greenhouse gas inventory and is working on detailed emission-reduction roadmaps for its major operated assets.

Read more about the Company's progress and follow its ESG journey at https://heliostechnologies.com/esg

About Helios Technologies
Helios Technologies is a global leader in highly engineered motion control and electronic controls technology for diverse end markets, including construction, material handling, agriculture, energy, recreational vehicles, marine, health, and wellness. Helios sells its products to customers in over 90 countries around the world. Its strategy for growth is to be the leading provider in niche markets, with premier products and solutions through innovative product development and acquisition. The Company has paid a cash dividend to its shareholders every quarter since becoming a public company in 1997. For more information please visit: www.heliostechnologies.com.

View source version on businesswire.com:
https://www.businesswire.com/news/home/20220613005167/en/

(b)

Nikon Group's Net-zero Targets Approved by Science Based Targets (SBT) initiative

March 22, 2024

SCIENCE BASED TARGETS
DRIVING AMBITIOUS CORPORATE CLIMATE ACTION

TOKYO - Nikon Corporation (Nikon) received approval for its net-zero targets from the Science Based Targets (SBT) initiative. The Nikon Group has been setting a target of achieving carbon neutrality by FY2050. This time, the Nikon Group has set a new long-term target of achieving zero greenhouse gas (GHG) emissions*1 throughout the value chain following SBT initiative, which received approval from the SBT initiative. In addition, the GHG emissions reduction targets for FY 2030 (Near-Term Targets), which had previously been certified have been updated in line with the latest criteria set by the SBT initiative, and have been re-certified as the "1.5 degrees Celsius target."
Nikon will bring the target forward by 20 years for switching to 100% renewable energy for electricity used in our business activities, moving it from FY 2050 to FY 2030, to achieve the GHG emissions reduction targets.

The SBT Initiative is an initiative jointly established in 2015 by the United Nations Global Compact, WRI (World Resources Institute), and others to encourage companies to set science-based GHG reduction targets to limit the rise in global average temperatures due to climate change to 1.5°C above pre-industrial levels.

SBT Initiative website

Nikon Group's GHG emissions reduction targets approved by SBT initiative

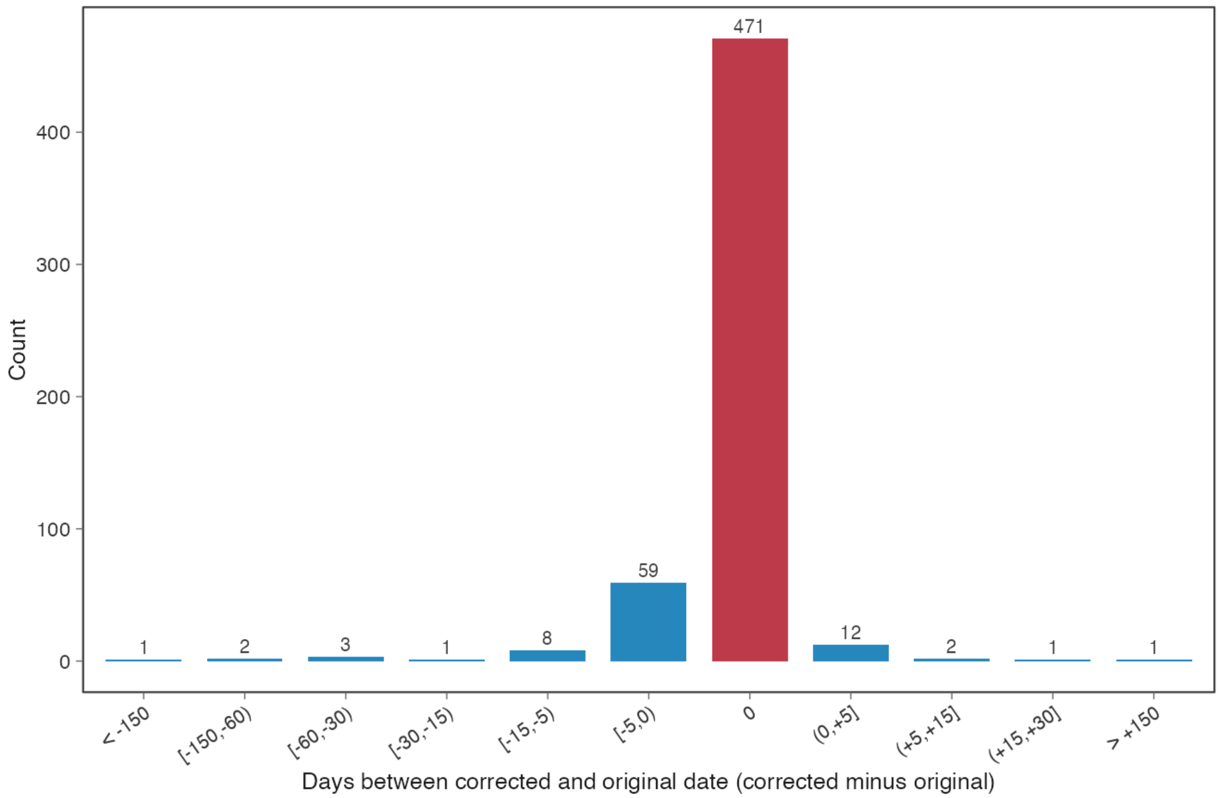
- Net-Zero Target
  Reach net-zero GHG emissions across the value chain by FY2050.
  Near-Term Targets
- Reduce absolute scope 1*2 and 2*3 GHG emissions 57% by FY2030 from a FY2022 base year.
  Reduce absolute scope 3*4 GHG emissions 25% by FY2030 from a FY2022 base year.

At the Nikon Group, in accordance with our Corporate Philosophy of Trustworthiness and Creativity, we meet society's expectations to win "trust" in each area of the environment, society/labor, and governance. Furthermore, we aim to contribute to the achievement of the realization of a sustainable society where humans and machines co-create seamlessly by "creating" new value through business activities. We continue to promote these efforts.

*1 Net-zero means reducing GHG emissions (Scope 1, 2, and 3) across the value chain by 90% and neutralizing remaining emissions in accordance with standards set by the SBT Initiative.
*2 Direct GHG emissions due to the use of fuel on site.
*3 Indirect GHG emissions from consumption of purchased electricity heat or steam.
*4 Other, indirect emissions of GHG emissions (excluding Scope 1 and 2).

Supplementary Figure 2: Examples of press releases announcing corporate climate commitments

Notes: Panel (a) shows an example of a press release announcing a new net-zero commitment (Helios Technologies, June 2022; Business Wire release, indexed by the LSEG press aggregator). Panel (b) shows an example of a press release announcing the validation of a target by the Science Based Targets initiative (Nikon, March 2024; an illustrative example postdating the sample window).



Supplementary Figure 3: Distribution of gaps between verified and recorded release dates

Notes: Distribution of the gap (in days) between the manually verified date on which the information became public and the date initially recorded by the LSEG press aggregator (corrected minus original; negative values indicate that the verified public date precedes the recorded date). The red bar marks exact matches.

Descriptive Statistics

Supplementary Table 1: Descriptive statistics of the PR sample

General characteristics

Variable	n	Mean	SD	d1	d5	d9	N_{firm}
Releases per firm	455	1.14	0.39	1.00	1.00	2.00	455
Market Capitalization (in B$)	519	34.28	100.51	1.25	12.04	75.28	455
GHG Emissions, sc.1&2 (in Mt)	475	4.46	16.45	0.01	0.28	6.74	412
GHG Emissions int. (in tons/M$)	474	302.99	884.77	3.08	29.50	612.06	411
Carbon Price (in €)	463	27.70	42.41	0.25	5.94	77.91	406
Carbon Price inc. FA (in €)	463	23.60	40.87	0.03	1.21	73.19	406

GHG Emissions Trend

Metric	N_{firm}	Y-3	Y-2	Y-1	Y0	Y+1	Y+2
Sc.1 ($MtCO_2$)	296	5.659	5.478	5.293	4.943	4.568	4.484
Av. Sc.1&2 ($MtCO_2$)	291	5.930	5.819	5.613	5.310	5.039	4.918
Δ ind.-adj. Sc.1&2 (%)	289	0.250	-0.553	0.291	0.074	-0.122	0.341
Δ YoY Sc.1&2 (%)	289	—	-1.302	1.240	4.632	-0.509	1.531
Sc.1&2 int. (tCO_2/M$)	252	385.933	358.438	360.439	362.456	290.061	266.679

Sectors

Sector	n	pct	N_{firm}
Finance & Tech	173	33.30	155
Customer Services	133	25.60	117
Industry & Utilities	115	22.20	101
Energy & Mining	98	18.90	82

Regions

Region	n	pct	N_{firm}
US	223	43.00	193
Other OECD	130	25.00	118
EU	126	24.30	110
Non-OECD	40	7.70	34

Type of label

Label	n	pct	N_{firm}
No label	83	16.00	83
SBTi	321	61.80	288
inc. commitment	88	17.00	87
inc. submission	37	7.10	37
inc. validation	196	37.80	188
GFANZ signatory	18	3.50	18
Target modification	97	18.70	90

Release Year

Year	n	pct	N_{firm}
2017	3	0.6	3
2018	—	—	—
2019	11	2.1	11
2020	33	6.4	33
2021	170	32.8	163
2022	149	28.7	147
2023	153	29.5	151

Note: The general-characteristics, sectors, regions, type-of-label and release-year panels describe the 519 events with a complete-case CAR(0,2); n is the number of commitment events and N the number of distinct companies. The CO_2-trend panel is built from the full 561-event sample, retaining one release per company (the first) with complete emission data over the window [Y−3, Y+2] and passing basic plausibility screens (minimum emission levels, changing year-to-year totals, non-zero revenue for the intensity rows). The emission variables follow [1]. $d1/d5/d9$ denote the first, fifth and ninth deciles.

Sector Grouping

Supplementary Table 2: Grouping of LSEG TRBC economic sectors into the four analysis sectors

TRBC economic sector	Analysis group	Events
Energy	Energy & Mining	98
Basic Materials	Energy & Mining	
Industrials	Industry & Utilities	115
Utilities	Industry & Utilities	
Consumer Cyclicals	Customer Services	
Consumer Non-Cyclicals	Customer Services	133
Healthcare	Customer Services	
Financials	Finance & Tech	
Technology	Finance & Tech	173
Real Estate	Finance & Tech	

Notes: The four analysis groups are used throughout the multivariate, robustness and composition specifications; the headline multivariate specification further excludes 30 US anti-ESG-state events announced on or after 1 January 2022 (Methods). Reference category in the multivariate regression (Equation 7) is Industry & Utilities. There are eleven TRBC sectors, but "Academic & Educational Services" is not applicable in our sample.

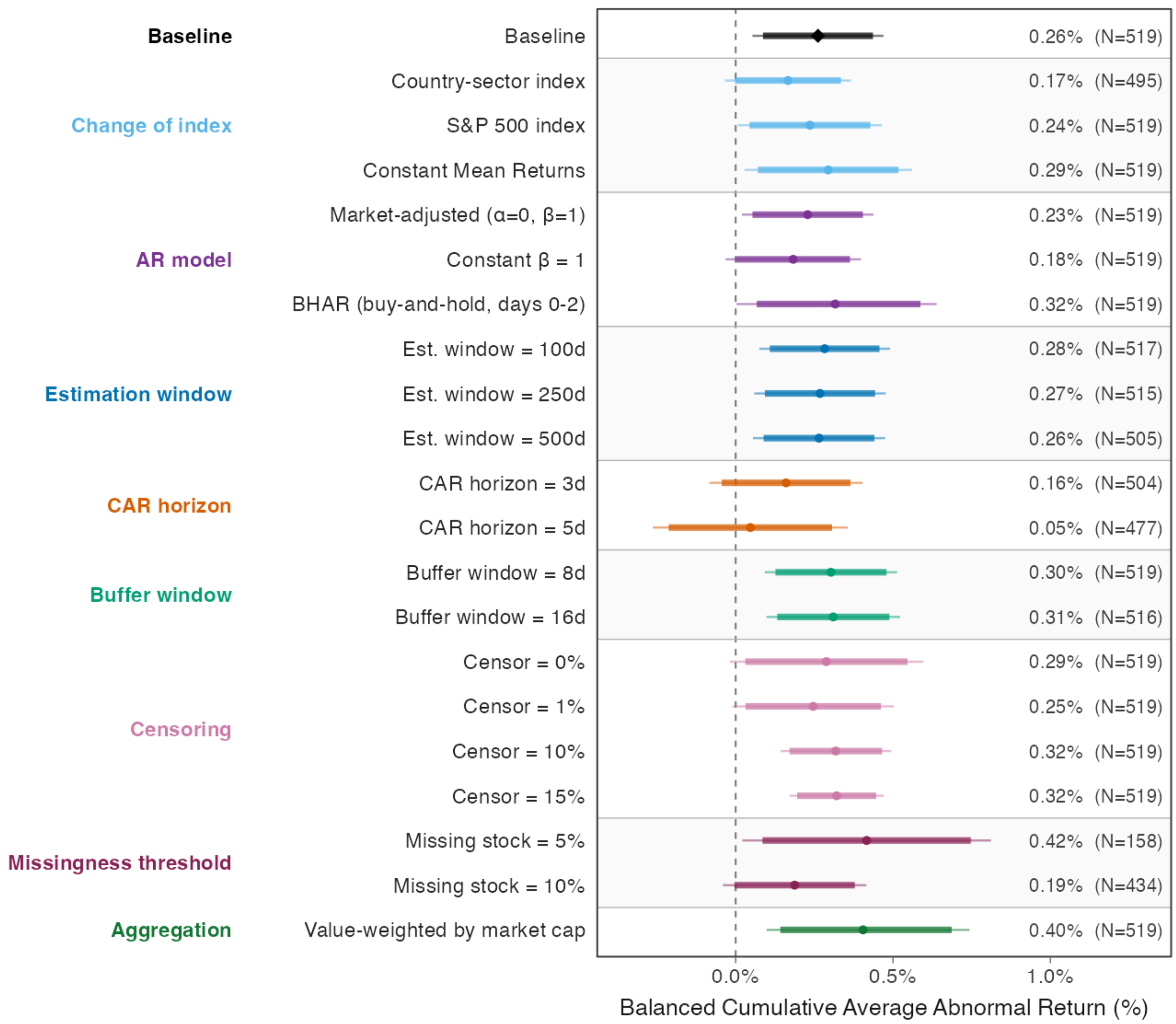
Multivariate analysis: sample and inference robustness

Supplementary Table 3: Multivariate analysis (SI robustness): Satterthwaite standard errors

	Dependent variable:				
	CAR(0,2)				
	Incorporation (main)	Listing	Region × sector	Incorp. × sector	Listing × sector
EU	0.001	0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Non-OECD	0.003	0.003	0.003	0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
US	0.004	0.004	0.004	0.004	0.004*
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
GFANZ signatory	−0.009*	−0.009	−0.009*	−0.009	−0.009
	(0.004)	(0.005)	(0.003)	(0.005)	(0.006)
SBTi not-yet-validated target	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
SBTi-validated target	0.001	0.001	0.001	0.001	0.001
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Unlabeled target	−0.001	−0.001	−0.001	−0.001	−0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Customer Services	0.001	0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Energy and Mining	0.002	0.002	0.002	0.002	0.002
	(0.004)	(0.007)	(0.003)	(0.004)	(0.005)
Finance and Tech	0.003	0.003	0.003*	0.003	0.003
	(0.002)	(0.003)	(0.001)	(0.003)	(0.004)
COP	−0.002	−0.002	−0.002	−0.002	−0.002
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
Constant	−0.002	−0.002	−0.002	−0.002	−0.002
	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)
p-value (US)	0.164	0.160	0.153	0.122	0.093
Clusters G	39	32	16	85	75
Observations	519	519	519	519	519
R^2	0.013	0.013	0.013	0.013	0.013

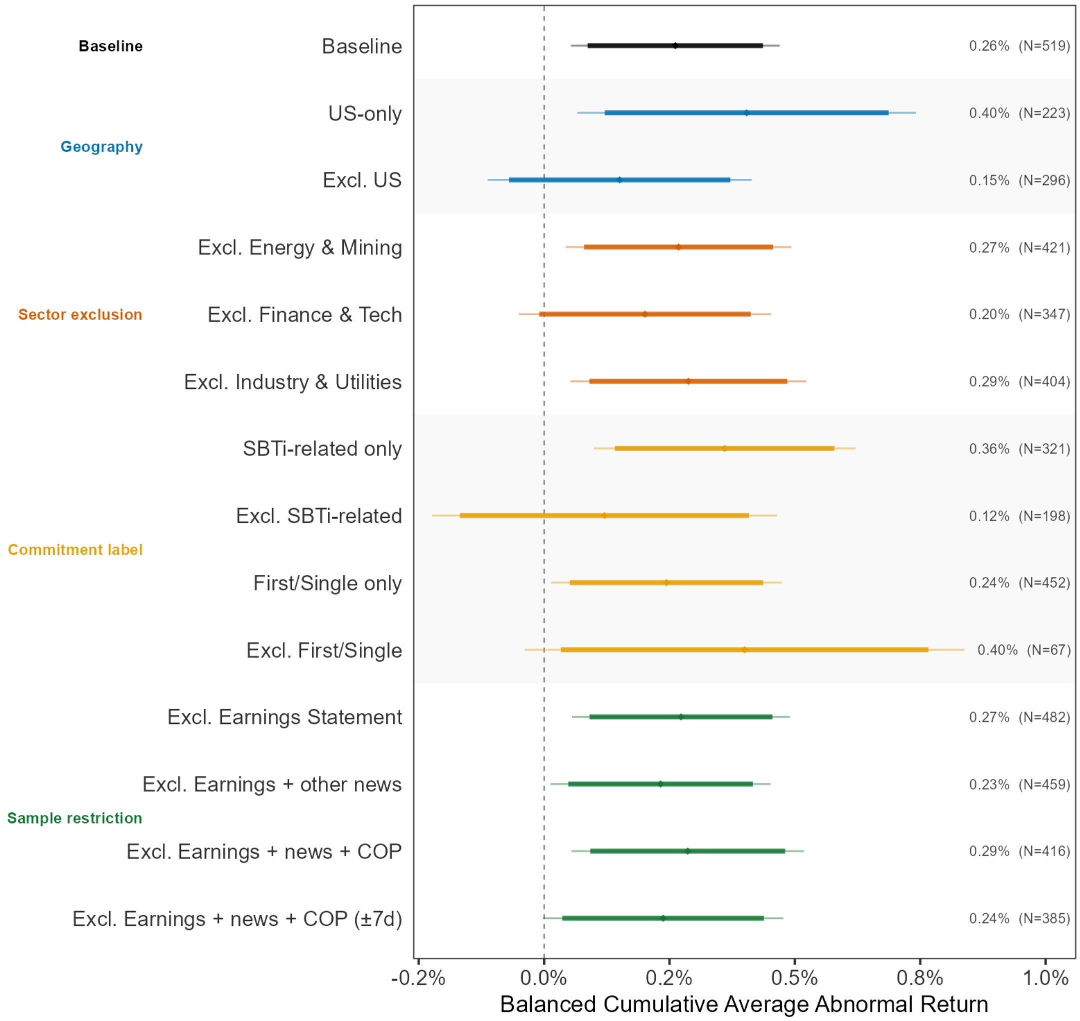
Note: *p<0.1; **p<0.05; ***p<0.01, based on CR2 t-statistics with Satterthwaite degrees of freedom (clubSandwich). Same point estimates, standard errors and sample as Extended Data Table 1 (pooled US sample, no anti-ESG/post-Fink exclusion); only the reference distribution for the p-values/stars differs (Satterthwaite per-coefficient df here vs. classical df = G-1 in the main text). All five columns report the same OLS regression (Equation 7). Standard errors are computed over event-level CAR outcomes and do not propagate market-model AR-estimation variance from the daily abnormal-return step.

Balanced CAR Robustness



Supplementary Figure 4: Robustness checks under the balanced firm-level CAR rule

Notes: Same specifications as the main robustness forest (Extended Data Fig. 2), but every headline point is re-estimated under the balanced firm-level CAR rule: per-event CAR(0,2) is the sum of daily abnormal returns with no missing day allowed, the specification CAAR is the cross-firm mean, and confidence intervals use Brown–Warner cross-sectional standard errors ($df = N - 1$). Thick and thin bars are 90% and 95% confidence intervals. BHAR and value-weighted rows keep their percentile-bootstrap intervals.



Supplementary Figure 5: Extended robustness checks under the balanced firm-level CAR rule

Notes: Same specifications as the extended robustness forest (Fig. 2 in the main text), re-estimated under the balanced firm-level CAR rule (complete event-window abnormal returns only; Brown–Warner cross-sectional standard errors, $df = N - 1$). Thick and thin bars are 90% and 95% confidence intervals.

Carbon-price regressions

Supplementary Table 4: Matching firms to OECD Effective Carbon Rate user categories

Panel A. Most populated OECD ECR user categories

Code	Description	Distinct firms
COMMPUB	Commercial and public services	238
MACHINE	Machinery and equipment	82
CHEMICAL	Chemicals	52
ELEC	Electricity producers	35
FOODPRO	Food processing	28
MINING	Mining	25
WOODPRO	Wood products	24

Panel B. Notable GICS-to-OECD mappings

GICS industry	OECD category
Oil, Gas & Consumable Fuels	ADJDISTLOSS (gas distribution) DOMESAIR (fuel for air transport) NONROAD (pipeline transportation) COMMPUB (commercial services)
Electric utilities; independent power producers; multi-utilities	ELEC (uniform)
Metals & Mining	MINING or NONFERR

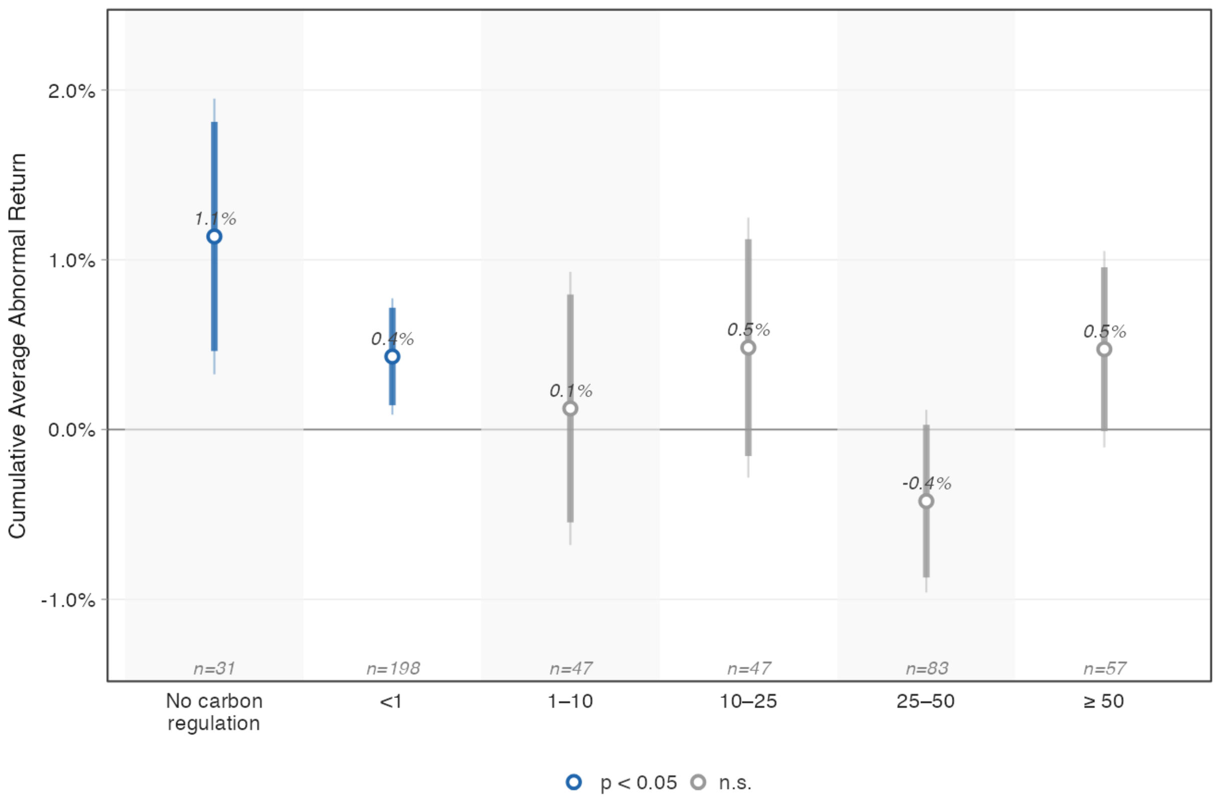
Notes: Each firm's GICS industry is mapped to ISIC codes using the UNEP FI ISIC/GICS crosswalk, and ISIC codes are then mapped to one of the 30 OECD ECR user categories following the OECD/ISIC correspondence table (Methods). Panel A reports the number of distinct firms assigned to each of the seven most populated categories. Panel B lists the GICS industries whose assignment is not one-to-one: Oil, Gas & Consumable Fuels resolves to one of four categories depending on the granular ISIC code, and Metals & Mining splits between MINING and NONFERR.

Supplementary Table 5: Events without a matched OECD Effective Carbon Rate

Panel A. Country of incorporation absent from the OECD ECR database (49 events)	
Jurisdiction	Events
Singapore	9
United Arab Emirates	8
Bermuda	7
Hong Kong	7
Taiwan	7
China (Mainland)	3
Curaçao	2
Jersey	2
Cayman Islands	1
Liberia	1
Malta	1
Saudi Arabia	1
Subtotal	*49*

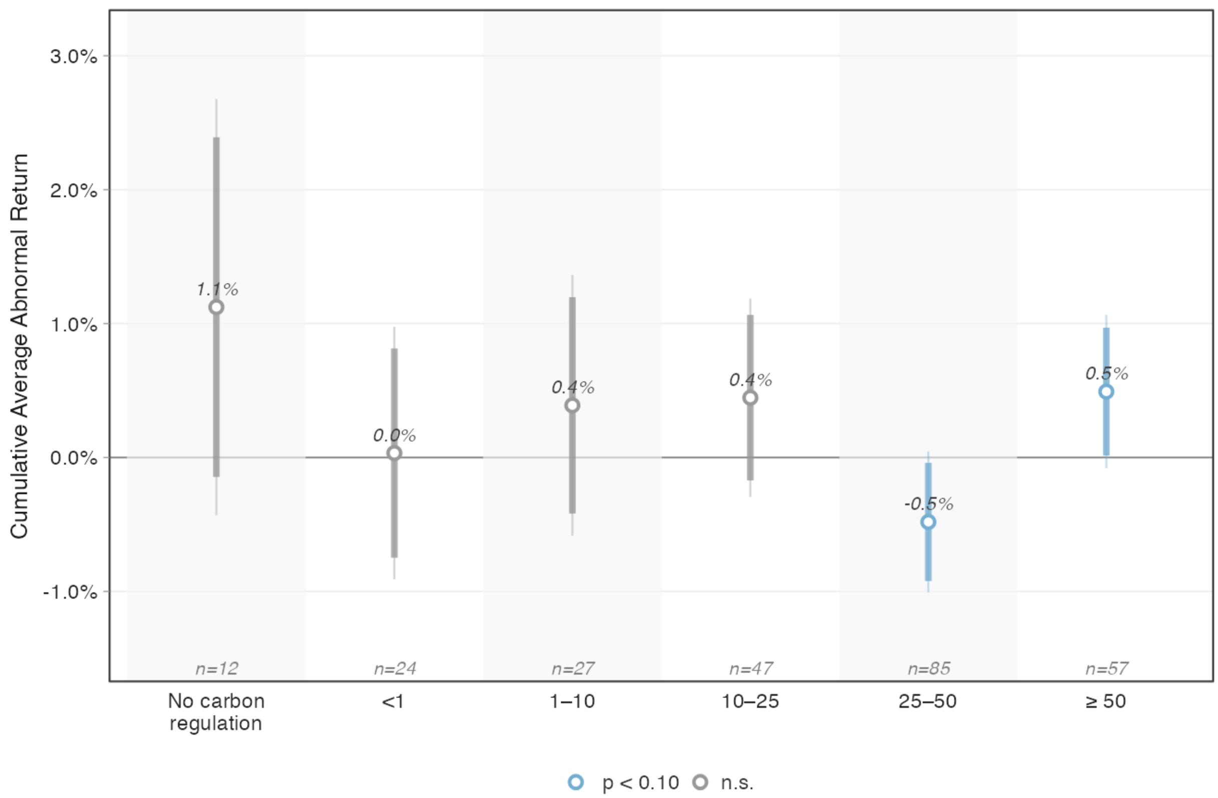
Panel B. Country covered but country $\times$ sector cell absent (17 events)		
Country	OECD user category	Events
United States	`NA`	7
Switzerland	`MINING`	3
United Kingdom	`NA`	3
Australia	`HEAT`	1
France	`NA`	1
Israel	`CHEMICAL`	1
Japan	`NA`	1
Subtotal		*17*
Total unmatched		**66**

Notes: Of the 561 events in the main event-study sample, 66 carry no matched OECD Effective Carbon Rate and are excluded from the carbon-pricing analysis, leaving 495. Panel A lists jurisdictions outside the OECD ECR framework altogether. Panel B lists events whose country is covered but whose specific country $\times$ sector cell is absent, either because the OECD computes no ECR for that sector in that country (`MINING`, `HEAT`, `CHEMICAL` rows) or because the firm's OECD user category could not be determined from the correspondence table (`user_OECD` = `NA`). The unresolved GICS industries are online services, business support services, paper packaging, passenger ground transport, UK investment trusts, and IT services; none maps unambiguously to an OECD energy-user category.



Supplementary Figure 6: CAAR(0,2) by effective carbon rate, balanced firm-level CAR rule

Notes: Balanced-rule companion to main-text Fig. 4. CAAR(0,2) by bin of the OECD Effective Carbon Rate including free allocations (€/tCO_2), restricted to events with a complete-case CAR(0,2) (an event is dropped if any day in the cumulation window is missing), for the 463 of 561 events with a matched ECR and a complete CAR. Per-bin CAAR uses the same AAR-cumulate rule and cross-period-independence standard errors ($df = N_{\min} - 1$) as the main-text figure, computed within the complete-case subsample. Thick and thin bars are 90% and 95% confidence intervals. Bin sample sizes range from 31 (no carbon regulation) to 198 (<1 €). Confidence intervals are not adjusted for multiple comparisons across bins.



Supplementary Figure 7: CAAR(0,2) by effective carbon rate, non-US events

Notes: Same construction as main-text Fig. 4 (AAR-cumulate rule, cross-period-independence standard errors, $df = N_{\min} - 1$), restricted to events by non-US-incorporated firms. Excluding US events thins the low-ECR bins (the no-carbon-pricing bin retains 12 events, the below-1 € bin 24); two bins differ from zero at the 10% level — the ≥50 € bin (+0.5%, N = 57) and the 25–50 € bin (−0.5%, N = 85) — and none at the 5% level. Thick and thin bars are 90% and 95% confidence intervals; confidence intervals are not adjusted for multiple comparisons across bins.

Supplementary Table 6: Market reaction to mitigation announcements and the effective carbon rate (OECD/ECR-coverage subsample)

	Dependent variable:				
	CAR(0,2)				
	(1)	(2)	(3)	(4)	(5)
Effective Carbon Rate, EMCR (log)	−0.002				
	(0.002)				
Effective Carbon Rate, EACR (log)		−0.002	−0.002	−0.002	−0.002
		(0.002)	(0.002)	(0.002)	(0.002)
CO_2 Sc. 1+2 (log)			−0.0003	−0.0003	−0.0003
			(0.001)	(0.001)	(0.001)
Share Committed (LSEG)				0.001	0.001
				(0.006)	(0.006)
US-incorporated ($\mathbb{1}$)					0.0004
					(0.003)
Constant	0.005^{***}	0.005^{***}	0.007	0.007	0.007
	(0.001)	(0.001)	(0.007)	(0.007)	(0.008)
Discounting Free Allocations	No	Yes	Yes	Yes	Yes
Controlling for Carbon Emissions	No	No	Yes	Yes	Yes
Share Committed	No	No	No	Yes	Yes
US indicator	No	No	No	No	Yes
Observations	425	425	425	425	425
R^2	0.005	0.005	0.005	0.005	0.005

Note: *p<0.1; **p<0.05; ***p<0.01, based on CR2 t-statistics with df = G-1 (classical cluster-robust); a Satterthwaite small-sample-df version is reported as an SI robustness check (Supplementary Table 8). Each observation is a new net-zero commitment or an upward target revision. The analysis uses the PR sample restricted to OECD/ECR-covered complete cases (N = 425 of 561 PR events). Effective carbon rates are drawn from the OECD database; the "Discounting Free Allocations" variable refers to an OECD ECR measure that adjusts for free allowances. Sector classification maps each firm's GICS industry to ISIC codes (UNEP FI ISIC/GICS crosswalk) and then to OECD ECR categories via the OECD/ISIC correspondence table. The dependent variable is event-level CAR(0,2), computed as a complete-case sum of daily abnormal returns. Standard errors are CR2 cluster-robust (clubSandwich::vcovCR), clustered at the country-of-incorporation level, matching the headline multivariate table; they do not propagate market-model AR-estimation variance. EMCR = effective marginal carbon rate (free allocations not discounted); EACR = effective average carbon rate (free allocations discounted). Column (5) adds a US-incorporation indicator to assess whether the carbon-price gradient merely reflects the US concentration of low-ECR events.

Carbon-price robustness: peer-share timing

Supplementary Table 7: Carbon-price robustness: annual LSEG peer-share timing

	Dependent variable:	
	CAR(0,2)	
	(1)	(2)
Effective Carbon Rate, EACR (log)	−0.002	−0.002
	(0.002)	(0.002)
CO_2 Sc. 1+2 (log)	−0.0003	−0.0002
	(0.001)	(0.001)
Share Committed (LSEG, event-year included)	0.001	
	(0.006)	
Share Committed (LSEG, prior years only)		−0.001
		(0.007)
Constant	0.007	0.007
	(0.007)	(0.007)
Peer-share timing	Year $\leq$ event year	Year < event year
Conservative pre-treatment control	No	Yes
Observations	425	425
R^2	0.005	0.005

Note: Both columns use the same OECD/ECR-covered complete-case PR sample (N = 425 of 561 PR events). LSEG records commitment timing mainly at annual precision, so the conservative robustness excludes all same-year peer commitments by using Year ¡ event year. Standard errors are CR2 cluster-robust SEs clustered at the country-of-incorporation level; stars use df = G-1 (classical cluster-robust). A Satterthwaite small-sample-df version is reported as an SI robustness check (Supplementary Table 9).

Supplementary Table 8: Market reaction to mitigation announcements and the effective carbon rate (SI robustness: Satterthwaite standard errors)

	Dependent variable:				
	CAR(0,2)				
	(1)	(2)	(3)	(4)	(5)
Effective Carbon Rate, EMCR (log)	−0.002				
	(0.002)				
Effective Carbon Rate, EACR (log)		−0.002	−0.002	−0.002	−0.002
		(0.002)	(0.002)	(0.002)	(0.002)
CO_2 Sc. 1+2 (log)			−0.0003	−0.0003	−0.0003
			(0.001)	(0.001)	(0.001)
Share Committed (LSEG)				0.001	0.001
				(0.006)	(0.006)
US-incorporated ($\mathbb{1}$)					0.0004
					(0.003)
Constant	0.005*	0.005*	0.007	0.007	0.007
	(0.001)	(0.001)	(0.007)	(0.007)	(0.008)
Discounting Free Allocations	No	Yes	Yes	Yes	Yes
Controlling for Carbon Emissions	No	No	Yes	Yes	Yes
Share Committed	No	No	No	Yes	Yes
US indicator	No	No	No	No	Yes
Observations	425	425	425	425	425
R^2	0.005	0.005	0.005	0.005	0.005

Note: *p<0.1; **p<0.05; ***p<0.01. Same point estimates and CR2 cluster-robust SEs (clustered at the country-of-incorporation level) as Supplementary Table 6; only the reference distribution for the p-values/stars differs (Satterthwaite small-sample degrees of freedom, clubSandwich, instead of classical df = G-1). Sample: N = 425 of 561 PR events. EMCR = effective marginal carbon rate (free allocations not discounted); EACR = effective average carbon rate (free allocations discounted).

Supplementary Table 9: Carbon-price robustness: annual LSEG peer-share timing (SI robustness: Satterthwaite standard errors)

	Dependent variable:	
	CAR(0,2)	
	(1)	(2)
Effective Carbon Rate, EACR (log)	−0.002	−0.002
	(0.002)	(0.002)
CO_2 Sc. 1+2 (log)	−0.0003	−0.0002
	(0.001)	(0.001)
Share Committed (LSEG, event-year included)	0.001	
	(0.006)	
Share Committed (LSEG, prior years only)		−0.001
		(0.007)
Constant	0.007	0.007
	(0.007)	(0.007)
Peer-share timing	Year ≤ event year	Year < event year
Conservative pre-treatment control	No	Yes
Observations	425	425
R^2	0.005	0.005

Note: Same point estimates and CR2 cluster-robust SEs (clustered at the country-of-incorporation level) as Supplementary Table 7; only the reference distribution for the p-values/stars differs (Satterthwaite small-sample degrees of freedom, clubSandwich, instead of classical df = G-1). Both columns use the same OECD/ECR-covered complete-case PR sample (N = 425 of 561 PR events).

Anti-ESG DiD: balance and composition

Supplementary Table 10: Classification of US states by ESG policy stance, as of 31 December 2023

Anti-ESG states (28)	**Pro-ESG and neutral states (23)**
Alabama, Alaska, Arkansas, Florida, Georgia, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Mississippi, Missouri, Montana, Nebraska, New Hampshire, North Carolina, North Dakota, Ohio, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, West Virginia, Wyoming	Arizona, California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Maine, Maryland, Massachusetts, Michigan, Minnesota, Nevada, New Jersey, New Mexico, New York, Oregon, Pennsylvania, Rhode Island, Vermont, Washington, Washington DC, Wisconsin

Notes: A state is classified as anti-ESG if, as of 31 December 2023, it signed the multi-state letter asking Congress to overturn the Department of Labor's ESG investing rule. All remaining states, together with the District of Columbia, are counted as pro-ESG or neutral. The classification is applied uniformly to all events as a single end-2023 snapshot. The borderline cases are discussed in the Methods.

Supplementary Table 11: Anti-ESG DiD: announcer composition by state group and period

	Anti-ESG states		Pro-ESG/neutral states	
	Pre-2022	Post-2022	Pre-2022	Post-2022
Events (N)	47	34	75	72
Distinct firms	43	31	70	68
$\log_{10}(1 + \text{Market Cap})$, mean	12.28	12.07	12.35	12.16
SBTi share (%)	63.8	67.6	56.0	81.9
Emissions coverage (%)	95.7	100.0	89.3	94.4
Energy & Mining (%)	34.0	11.8	17.3	11.1
Industry & Utilities (%)	31.9	35.3	24.0	18.1
Customer Services (%)	27.7	26.5	26.7	30.6
Finance & Tech (%)	6.4	26.5	32.0	40.3
Other sectors (%)	0.0	0.0	0.0	0.0

Note: Composition of the DiD regression sample (N = 228 events, 196 distinct firms) by state group and period (cutoff 1 January 2022). Only 16 firms announce both before and after the cutoff, so the interaction compares largely different announcers across periods. Sector shares use the four coarse groups of the multivariate analysis. Emissions coverage = share of events with non-missing scope 1+2 emissions.

Supplementary Table 12: Anti-ESG DiD: composition-robust specifications

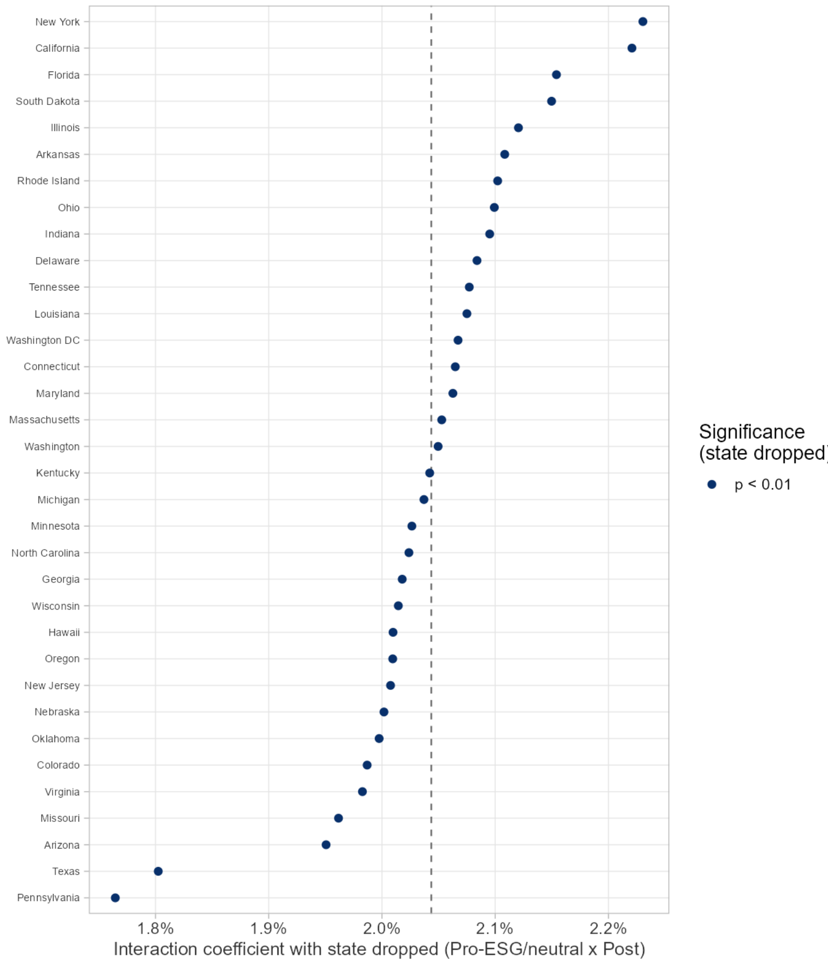
	Dependent variable:		
	CAR(0,2)		
	Baseline	Sector × post FE	Excl. Energy & Mining
	(1)	(2)	(3)
Pro-ESG/neutral state ($\mathbb{1}$)	-0.008^{**}	-0.008^{*}	-0.006^{**}
	(0.003)	(0.004)	(0.003)
Post ($\mathbb{1}$[Date ≥ 2022-01-01])	-0.013^{***}	-0.011	-0.011^{***}
	(0.003)	(0.007)	(0.003)
Pro-ESG/neutral × Post	0.020^{***}	0.019^{***}	0.018^{***}
	(0.005)	(0.006)	(0.005)
Constant	0.009^{***}	0.008^{**}	0.008^{***}
	(0.002)	(0.004)	(0.002)
Sector × post FE	No	Yes	No
State cluster (G)	34	34	32
Observations	228	228	187
R^2	0.037	0.051	0.031

Note: CR2 cluster-robust SEs, clustered by US state of headquarters, in parentheses; stars use CR2 t-statistics with df = G-1 (classical cluster-robust); a Satterthwaite small-sample-df version is reported as an SI robustness check (Supplementary Table 13). Col. (1): baseline specification. Col. (2) adds sector × post-period fixed effects (four coarse sector groups plus Other), absorbing sector-specific post-2022 shocks such as the energy-price boom. Col. (3) excludes Energy & Mining events. Companion balance panel: Table 11. Because exit from announcing is itself a plausible response to the backlash, composition-constant estimates bound sector-composition effects but do not separate repricing from selection into announcing.

Supplementary Table 13: Anti-ESG DiD: composition-robust specifications (SI robustness: Satterthwaite standard errors)

	Dependent variable:		
	CAR(0,2)		
	Baseline	Sector × post FE	Excl. Energy & Mining
	(1)	(2)	(3)
Pro-ESG/neutral state ($\mathbb{1}$)	-0.008^{**}	-0.008^{*}	-0.006^{*}
	(0.003)	(0.004)	(0.003)
Post ($\mathbb{1}$[Date $\geq$ 2022-01-01])	-0.013^{***}	-0.011	-0.011^{**}
	(0.003)	(0.007)	(0.003)
Pro-ESG/neutral × Post	0.020^{***}	0.019^{**}	0.018^{***}
	(0.005)	(0.006)	(0.005)
Constant	0.009^{***}	0.008^{**}	0.008^{**}
	(0.002)	(0.004)	(0.002)
Sector × post FE	No	Yes	No
State cluster (G)	34	34	32
Observations	228	228	187
R^2	0.037	0.051	0.031

Note: Same point estimates and CR2 cluster-robust SEs (clustered by US state of headquarters) as Supplementary Table 12; only the reference distribution for the p-values/stars differs (Satterthwaite small-sample degrees of freedom, clubSandwich). Col. (1): baseline specification. Col. (2) adds sector × post-period fixed effects (four coarse sector groups plus Other), absorbing sector-specific post-2022 shocks such as the energy-price boom. Col. (3) excludes Energy & Mining events.



Supplementary Figure 8: Leave-one-state-out sensitivity of the anti-ESG DiD interaction

Notes: Baseline (no-controls) Pro-ESG/neutral × Post interaction (Equation 9), re-estimated 34 times, dropping one headquarters state at a time. The dashed line marks the full-sample estimate (+2.0 percentage points). Points are coloured depending on the significance level (here 1% in all scenarios); dropping Pennsylvania or Texas lowers it, dropping New York or California raises it. The full range across all 34 states is +1.8 to +2.2 percentage points.

Supplementary Table 14: Anti-ESG DiD on US-headquartered commitment releases (SI robustness: Satterthwaite standard errors)

	Dependent variable:			
	CAR(0,2)			
	(1)	(2)	(3)	(4)
Pro-ESG/neutral state ($\mathbb{1}$)	-0.008^{**}	-0.008^{**}	-0.009^{*}	-0.010^{*}
	(0.003)	(0.003)	(0.004)	(0.005)
Post-Fink ($\mathbb{1}$[Date $\geq$ 2022-01-01])	-0.013^{***}	-0.013^{***}	-0.014^{***}	-0.015^{***}
	(0.003)	(0.003)	(0.003)	(0.003)
Pro-ESG/neutral $\times$ Post-Fink (β_3)	0.020^{***}	0.020^{***}	0.021^{***}	0.022^{***}
	(0.005)	(0.005)	(0.005)	(0.006)
log(1 + Market Cap)		-0.001	-0.001	-0.002
		(0.003)	(0.003)	(0.003)
log(1 + CO_2 Sc. 1+2)			-0.001	-0.0004
			(0.002)	(0.002)
SBTi indicator				-0.002
				(0.004)
Constant	0.009^{***}	0.018	0.025	0.036
	(0.002)	(0.036)	(0.041)	(0.043)
Sector fixed effects	No	No	No	Yes
Observations	228	228	214	214
R^2	0.037	0.037	0.039	0.048

Dependent variable is event-level CAR(0,2), not CAAR.

Note: *p<0.1; **p<0.05; ***p<0.01. Same point estimates and CR2 cluster-robust standard errors (clubSandwich::vcovCR, clustered on US state of headquarters) as Extended Data Table 4; only the reference distribution for the p-values/stars differs. Stars based on CR2 cluster-robust t-statistics with Satterthwaite small-sample degrees of freedom (clubSandwich). Regression SEs are over event-level CAR outcomes and do not propagate market-model AR-estimation variance. Sample: US-headquartered events with a state classification; the sample window starts 2018-01-01 (no retained event occurs in 2018). Pro-ESG and Neutral States vs Anti-ESG States (see Methods, State classification). Post threshold: 1 January 2022 (approximating Fink's January 2022 letter).

Supplementary Note 1: Related literature and prior announcement-return estimates

Several strands of evidence bear on announcement pricing beyond those summarized in the main text. Sustainability-linked bonds now tie financing costs directly to emission targets [2]. Jiang et al. [3] track the market response when targets succeed or fail, and Dahlström et al. [4] and Bendig et al. [5] study the longer-horizon financial performance of committed firms. Desai et al. [6] find that net-zero pledges by US oil and gas producers can be penalized unless perceived as credible, for example through near-term target dates.

The GFANZ cell in the label analysis warrants a note. GFANZ-affiliated announcements show the one negative point estimate (-0.6% in the subgroup figure; -0.9 percentage points, significant at the 10% level, in the multivariate specification), but the cell contains only 18 events and its confidence intervals are wide, so we read it with caution; it is nonetheless consistent with banking-sector evidence that, as the Net-Zero Banking Alliance rose and unraveled, only founding members' joining announcements moved prices, while later signings and the wave of exits left markets unmoved [7]. Bank net-zero commitments themselves appear to have left lending and engagement behavior largely unchanged [8], reinforcing the gap between announcement and delivery. Examining the cohort of corporate emissions targets that came due in 2020, Jiang et al. [3] find that almost one in ten failed and nearly a third quietly disappeared, yet the failures attracted almost no media coverage and no detectable market reaction, whereas the initial announcements of those same targets had been rewarded with improved media sentiment and environmental scores.

The propensity to make a public commitment is also informative: commitment drives have so far drawn mainly the "willing" — lower-emission firms — rather than the firms most exposed to carbon pricing [9]. In stringent-regulation environments, where peer commitment rates are higher, an individual announcement accordingly carries less news relative to what peer behavior already implies — consistent with the anticipation reading of the paper's cross-country results.

Two prior studies are closest to ours. Bauer et al. [10] identify US corporate "green pledges" from news text and document positive announcement returns that persist over the following trading days. Our estimates are consistent with theirs in sign and, for US-incorporated firms, in magnitude: restricted to the United States, our announcement effect (0.37–0.44%, statistically significant) is comparable to the positive US reactions they document. The two designs differ in three respects. First, coverage: our press-release sample is global and multi-sector, and because the average response outside the United States is close to zero, the global average (0.23%) is smaller than US-only estimates. Second, horizon: their US reactions show no reversal over the days following the announcement, whereas in our daily design the cumulative abnormal return declines after day $+2$; our global average is smaller than their cumulated multi-day effects. Third, within-US heterogeneity: we document the post-2022 divergence between anti-ESG and other states, which a pooled US design does not examine. Relative to Kim [11], who links investor responses to US net-zero pledge disclosures to the preferences of the investor base, our contribution is the global, press-release-timed, multi-sector sample and the policy and political-economy heterogeneity.

Supplementary Note 2: Power and equivalence bounds for the certification-premium null

The main text reports that the absence of a certification premium is informative rather than underpowered. This note details the two quantities behind that claim, the minimum detectable effect (MDE) and the equivalence bound, both computed from the label-level CAARs.

Let $\widehat{C}_v$ and $\widehat{C}_u$ denote the three-day CAAR(0,2) values for SBTi-validated targets ($N_v = 197$) and unlabelled new targets ($N_u = 85$), estimated under the AAR-cumulate rule with cross-period-independence standard errors s_v and s_u (Methods Eq. 5). The certification contrast and its standard error are

$$\hat{\delta} \;=\; \widehat{C}_v - \widehat{C}_u, \qquad s_\delta \;=\; \sqrt{s_v^2 + s_u^2}, \tag{S1}$$

treating the two label groups as independent cross-sections. In the analysis sample, $\hat{\delta} = 0.18$ pp with $s_\delta = 0.32$ pp, giving the 95% confidence interval $\hat{\delta} \pm z_{0.975}\, s_\delta = [-0.44,\, +0.80]$ pp.

Minimum detectable effect. For a two-sided test of $H_0\colon \delta = 0$ at the 5% level, the smallest true premium detectable with 80% power is

$$\mathrm{MDE}_{80} \;=\; (z_{0.975} + z_{0.80})\, s_\delta \;=\; 2.80 \times s_\delta \;=\; 0.89 \text{ pp}. \tag{S2}$$

A certification premium of that size or larger would thus have been detected with at least 80% probability; the observed contrast is barely a fifth of it.

Equivalence bound. We complement the power calculation with two one-sided tests (TOST) [12]. For a margin $\Delta > 0$, the test rejects the hypothesis of a non-negligible premium, $H_0\colon |\delta| \geq \Delta$, at the 5% level whenever the 90% confidence interval for δ lies entirely within $(-\Delta, \Delta)$. The smallest margin for which this rejection obtains is

$$\Delta^* \;=\; |\hat{\delta}| + z_{0.95}\, s_\delta \;=\; 0.70 \text{ pp}, \tag{S3}$$

so the data reject, at the 5% level, any announcement-stage certification premium larger than 0.70 percentage points.

Caveats. Both quantities use normal critical values, an accurate approximation at these cell sizes. They inherit the assumptions of the underlying standard errors: independence of abnormal returns across label groups (label groups are non-overlapping sets of events, but events overlap in calendar time, so residual cross-sectional correlation would understate s_δ), and, as throughout the paper, no propagation of market-model estimation variance from the daily abnormal-return step. The bounds concern the SBTi-validated-versus-unlabelled contrast only and are not adjusted for multiple comparisons across the other label cells of Extended Data Fig. 3.

Supplementary Note 3: Press-release search terms

We search press-release headlines for any of the following terms: “Climate Pledge”, “science-based”, “science based”, “SBTi”, “carbon target”, “net-zero”, “Net Zero”, “Climate Commitments”, “emissions target”, “emission target”, “emissions reduction target”, “emissions reduction goal” and “Environmental Commitment”; we also retain releases categorised by LSEG as related to “carbon neutrality”. New net-zero commitments follow standard headline wording (Supplementary Fig. 1), so the margin of error in this categorisation is limited.

Supplementary Note 4: Firm characteristics and matching on LSEG

The *LSEG Monitor App* data universe comprises 67,900 active listed firms. This database is used for all firms' time-invariant or yearly characteristics (when the variable of interest is year-variant, we retrieve the data from the year of the commitment). We extracted and used the following variables (in parentheses, the LSEG field associated with each variable): the identifier i.e. the RIC of the main equity, the ISIN Code (TR.Code), the company name (TR.CompanyName), the company location in terms of Country of Exchange (TR.ExchangeCountry), Country of Incorporation (TR.RegistrationCountry) and Province or State of Incorporation (TR.RegStateProvince) and Province or State of Headquarters (TR.HQStateProvince), the main sector of activity following the TRBC classification in economic sectors (TR.TRBCEconomicSector) and in industries (TR.TRBCIndustry), the Company Market Capitalization (TR.CompanyMarketCap) (we extracted the Company Market Cap and not the Main Equity Market Cap, because the latter variable is not standardized across firms), CO_2 emissions in scope 1 (TR.CO2DirectScope1) and in scope 2 (TR.CO2IndirectScope2). We also construct *Share Committed*, which denotes the proportion of LSEG-covered firms having made a net-zero or science-based climate commitment within a given country–sector–year cell, and serves as a proxy for the degree of climate maturity within a firm's competitive environment.

We match the event data with the LSEG financial data collected through the *Screener App*. The matching process relies on the use of the Refinitiv Identifier Code (RIC) for the PR sample. The relevant RIC is in general associated with the news release. When it is not, or when the RIC was not associated with the ordinary share of the firm, we completed this work manually. The RIC is an identifier at the equity and market level. However, in *LSEG Screener App*, each listed firm is associated with a unique reference RIC, which happens to be the RIC of its primary security, excluding dual listings. This allows us to rule out any risk of double counting or mismatching.

Supplementary Note 5: Pre-event and dependence diagnostics.

The pre-announcement test reported in the main text is a Wald test on the vector of the five daily pre-event average abnormal returns (days −5 to −1): with $\bar{m}$ the vector of daily AARs and $\hat{V}$ the sample covariance matrix of the daily abnormal returns across the $n = 479$ events observed on all five pre-event days, the statistic $\bar{m}'(\hat{V}/n)^{-1}\bar{m}$ is referred to a $\chi^2(5)$ distribution ($W = 1.8$, $p = 0.87$). Unlike the confidence bands, this test uses the estimated covariance across event days rather than assuming cross-period independence; it is computed on the daily averages themselves and is therefore independent of the backward-cumulation convention used to plot pre-event values in the butterfly figures. The clustered-inference check regresses the complete-case CAR(0,2) on a constant (N = 519) and computes cluster-robust standard errors by firm (G = 455 distinct firms), by announcement date (G = 354 distinct dates), and two-way by firm and date (by inclusion–exclusion); the mean of 0.26% carries t-statistics of 2.46, 2.43 and 2.42, respectively, against 2.47 under cross-sectional independence. As a coarser-cluster robustness check — absorbing dependence across adjacent days within overlapping event windows — clustering by ISO week gives $t = 2.38$ (G = 167) and by calendar month $t = 2.16$ (G = 54; 95% CI 0.02–0.50%): the mean remains significant at the 5% level under both, with the month-clustered interval predictably the widest.

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