Emissions, Liquidity, and Institutional Ownership

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Abstract

We examine how the introduction of standardized carbon emissions information affects equity-market liquidity and ownership. Using Trucost coverage data from 2010–2023 and a staggered difference-in-differences design, we find that first-time emissions information narrows bid-ask spreads by 2%, reduces Amihud illiquidity by 4%, and increases dollar trading volume and turnover by 4%. Institutional ownership increases by approximately 0.3 percentage points, representing roughly $30 billion in capital reallocated to newly covered firms. This ownership shift partially explains the liquidity improvement: a one-percentage-point increase in institutional ownership is associated with an additional 0.2% reduction in spreads. Liquidity gains are strongest for high-emission firms and late disclosers. Our results indicate that non-financial information, when standardized and comparable, is financially consequential: it attracts institutional capital, reduces adverse-selection costs, and enhances market efficiency.

JEL Classification: G12, G14, G23, Q54

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1 Introduction

Regulators worldwide are betting that standardized environmental disclosure will improve capital markets. The EU's Corporate Sustainability Reporting Directive (CSRD), the ISSB standards, and the SEC's climate disclosure rules all rest on the premise that making corporate carbon emissions observable and comparable reduces information frictions, lowers trading costs, and broadens investor participation. Yet, causal evidence for this premise remains scarce.

A large theoretical and empirical literature links financial disclosure to market liquidity. In the canonical framework of Diamond and Verrecchia (1991), the release of value-relevant information narrows the dispersion of beliefs, reduces market makers' adverse-selection risk, and tightens bid-ask spreads (e.g., Bushee and Noe 2000; Balakrishnan, Billings, Kelly, and Ljungqvist 2014; Boone and White 2007; Bird and Karolyi 2016). Whether non-financial information—information that does not appear on income statements or balance sheets—operates through the same channels, remains less clear. Carbon emissions are a particularly compelling test case: among environmental data, they are the most widely estimated by third parties, the most directly tied to regulatory and financial risk through carbon pricing and stranded-asset exposure, and the most amenable to standardized, cross-firm comparison. Understanding whether information on carbon emissions affects market quality is therefore central to evaluating regulatory disclosure mandates.

This paper studies how the first market-wide availability of standardized firm-level emissions information affects equity-market liquidity and identifies the mechanism through which it does so: institutional ownership. Once comparable emissions data become available for a previously uncovered firm, institutional investors reallocate capital toward it, and this ownership shift partially explains the observed

liquidity gains. The effect of non-financial information on market quality therefore operates not only through the direct adverse-selection channel emphasized by Diamond and Verrecchia (1991), but also indirectly, through the composition of the investor base.

Our empirical setting exploits the staggered rollout of Trucost coverage—one of the most widely used providers of corporate emissions data—between 2010 and 2023. Trucost combines firm-reported Scope 1 and Scope 2 carbon emissions with model-based estimates for non-reporting firms and distributes harmonized and comparable emissions data used by institutional investors, regulators, and financial data aggregators. As such, Trucost provides a close proxy for the information set of market participants at the time emissions data become observable. We define an information-introduction event as the earliest date on which Trucost provides emissions information for a firm it did not previously cover.

A key feature of this setting is that, for the vast majority of firms, first-time emissions coverage reflects third-party estimation rather than voluntary (or mandatory) firm disclosure. Approximately 95% of first-coverage events in our sample arise from Trucost's proprietary estimation methodology. This feature is central for identification. It allows us to separate the market response to the availability of standardized emissions information from the endogenous disclosure incentives that shape voluntary reporting decisions (Banerjee and Prokopev 2025). Firms generally do not self-select into treatment based on favorable information; instead, coverage begins when Trucost's data sources and models permit estimation. As a result, the timing of information introduction is largely orthogonal to firms' contemporaneous liquidity and ownership dynamics.

Because Trucost retrospectively harmonized pre-2008 emissions data in 2009, we restrict attention to first-time coverage events from 2010 to 2023 to ensure con-

sistent timing. Our final sample comprises 16,976 firm-year observations for U.S. and Canadian equities, including 2,946 treated firms that receive first-time emissions coverage during the sample window. We estimate treatment effects using a staggered difference-in-differences design following Callaway and Sant'Anna (2021). For each treated firm, we construct a matched control group using propensity score matching based on market capitalization, bid-ask spreads, and trading volume measured 90 days before the event. Control firms are drawn from the set of firms that do not receive emissions coverage within two years of matching. Our event window spans approximately 60 trading days before and after the information-introduction date.

The availability of standardized emissions information produces economically meaningful improvements in equity-market liquidity. Bid-ask spreads decline by approximately 2% relative to matched control firms, the Amihud illiquidity measure falls by 4%, and trading volume and turnover each increase by roughly 4%. These effects emerge shortly after information introduction, are not preceded by differential pre-trends, and are robust to a comprehensive set of controls and fixed-effects specifications, supporting a causal interpretation.

These liquidity gains coincide with a shift in ownership composition. The share of institutional holdings rises by approximately 0.3 percentage points relative to matched non-disclosing firms, corresponding in aggregate to roughly $30 billion of institutional capital reallocated toward newly covered firms during our sample period. The increase is concentrated among investment advisors and banks, long-horizon investors with fiduciary obligations and ESG integration mandates, while hedge fund holdings show no significant response, and private-equity and individual retail holdings decline. This documents a selection effect that complements the push effect identified by Ilhan, Krueger, Sautner, and Starks (2023): where

they show that institutional investors demand climate transparency, we show that institutional investors select into firms once that transparency arrives.

Does this reallocation merely coincide with improved liquidity, or does it help explain it? In a pure Diamond and Verrecchia (1991) account, information reduces adverse-selection costs directly, and ownership plays no explanatory role. But a large literature shows that institutional participation independently improves market quality by expanding order flow, lowering inventory costs for market makers, and intensifying information production (Rubin 2007; Kini and Mian 1995; Boone and White 2007; Bird and Karolyi 2016). To distinguish these channels, we augment our baseline specification with a triple interaction between the treatment indicator, the post-treatment dummy, and the change in institutional ownership around the event. The coefficient on this term is approximately -0.2, implying that a one-percentage-point increase in institutional ownership is associated with an additional 0.2% reduction in bid-ask spreads, beyond the direct effect of information introduction. Institutional investors do not merely respond to emissions transparency; they amplify its effect on market quality. Ownership is, in this sense, a channel.

Both effects, the liquidity improvement and the ownership reallocation, are strongest where ex ante uncertainty is greatest. Firms with above-median emissions and late disclosers within their industry show interaction coefficients of approximately -0.03, roughly doubling the baseline effect. High emitters face elevated uncertainty about regulatory exposure and compliance costs; late disclosers are more difficult for investors to benchmark against industry peers. Revealing emissions information in these cases resolves larger information gaps. We also find that firms whose disclosed emissions deviate most from sector medians, a proxy for estimation difficulty, experience the strongest liquidity responses. By contrast, the

effects are not confined to climate-salient subsamples: they hold across U.S. states with below-median climate change beliefs, states without emissions trading systems, and sectors in which GHG emissions are not classified as financially material by SASB standards. The pronounced response of high emitters, combined with the breadth of the effect across climate-salient and non-climate-salient settings, is consistent with institutional capital reallocation driven by the informational content of emissions data rather than by investor climate preferences per se.

Our paper contributes to three strands of the literature. First, we extend research linking firm-level data availability to equity-market liquidity (Bushee and Noe 2000; Balakrishnan et al. 2014; Krueger, Sautner, Tang, and Zhong 2024) by showing that non-financial information operates through the same channels as financial disclosure, and by identifying ownership as a mechanism. While Krueger et al. (2024) study mandatory ESG disclosure regimes across countries, our identification isolates the effect of information availability on a firm-by-firm basis, holding the regulatory environment fixed. Second, we contribute to the literature on environmental information and asset prices (Bolton and Kacperczyk 2021; Hsu, Li, and Tsou 2021; Ilhan, Sautner, and Vilkov 2021) by shifting the focus from investors' valuation of emissions to intermediaries' pricing of firm equity. Third, we complement Ilhan et al. (2023), who document a push effect in which institutional investors demand climate transparency, by identifying the corresponding selection effect: institutional capital flows to firms when transparency is realized, and this flow is itself consequential for market quality.

In sum, we provide causal evidence that standardized carbon emissions information improves equity-market liquidity and broadens the investor base. Non-financial information, when credible, is financially consequential: it attracts institutional capital, reduces adverse-selection costs, and enhances the efficiency of equity

markets.

2 Literature Review

This paper contributes to three strands of literature: (1) the effect of firm-level information releases on capital markets quality, (2) the way how capital markets incorporate environmental risk, and (3) the interaction between disclosure and institutional ownership. The literature review is organized to parallel the three contributions outlined in the introduction.

An extensive literature examines how firm disclosure affects capital markets quality. Theoretically, disclosure reduces information asymmetry between insiders and outside investors and, as shown by Diamond and Verrecchia (1991), thereby narrows bid-ask spreads and lowers the cost of capital. Goldstein and Yang (2017) provide a unified analytical framework showing that disclosure's effect on market quality depends on whether information is exogenous to investors' private information acquisition; the conventional positive effect obtains precisely in the exogenous-information case, which corresponds to standardized third-party emissions data. Empirically, Easley and O'Hara (2004) show that greater disclosure lowers information-gathering and monitoring costs, and Balakrishnan et al. (2014) establish that firms can causally improve the liquidity of their equity through voluntary disclosure. This literature has increasingly extended to non-financial information. Matsumura, Prakash, and Vera-Munoz (2014) document that firms disclosing carbon emissions tend to be significantly larger than comparable non-disclosing firms, and Bolton and Kacperczyk (2026) show that voluntary carbon disclosure is associated with lower realized stock returns. For mandatory regimes, Grewal, Riedl, and Serafeim (2019) find heterogeneous equity-market reactions to

the EU's nonfinancial disclosure directive, with the most negative reactions concentrated among firms with poor ex-ante ESG performance, while Chen, Hung, and Wang (2018) show that mandatory CSR disclosure in China reduces firm profitability but also reduces pollution externalities, underscoring that disclosure mandates can have real effects beyond capital markets. Krueger et al. (2024) further establish that mandatory ESG disclosure improves firm-level equity-market liquidity across countries, and Christensen, Hail, and Leuz (2021) survey the broader economic consequences of mandatory CSR and sustainability reporting regimes. Our paper examines the market impact of standardized, predominantly third-party-estimated emissions information, a setting in which the timing of information introduction is largely orthogonal to firm-level disclosure incentives. We extend this literature by showing that non-financial information operates through the same adverse-selection channel as financial disclosure, and by identifying institutional ownership as a mechanism linking information availability to liquidity.

A growing literature examines how capital markets process environmental risk data. Most closely related to our setting, Rehse, Riordan, Rottke, and Zietz (2019) find that environmental uncertainty—specifically, exposure to Hurricane Sandy—widens bid-ask spreads, consistent with the adverse-selection mechanism we study. In the disclosure literature, Schiemann and Sakhel (2019) and Fan, Tang, and Pan (2021) find that climate risk disclosure and third-party assurance reduce information asymmetry. Whereas this literature primarily studies how markets value realized environmental risk or the quality of existing disclosures, our paper studies the complementary question of how the introduction of standardized emissions information reshapes trading costs and ownership composition.

Research on institutional ownership and disclosure documents that institutional investors prioritize corporate disclosure and actively advocate for its expansion

(Boone and White 2015; Bird and Karolyi 2016). A similar investor-driven "push effect" regarding climate risk disclosure has been documented by Ilhan et al. (2023), who exploit a 2015 French regulatory shift that required institutional investors to disclose their own climate risk exposure. They do not rule out the possibility that a "selection effect" operates simultaneously—that is, that institutional investors reallocate capital into firms once transparency is realized, rather than only pushing firms toward disclosure. Our paper identifies exactly this selection effect: institutional capital flows to firms when standardized emissions information becomes available, and this reallocation partially explains the observed improvement in equity-market liquidity.

3 Empirical Design

3.1 Treatment: Trucost Emissions Coverage Events

Our treatment variable is constructed from Trucost, a leading provider of corporate carbon-emissions information to institutional investors, regulators, and financial data aggregators. Trucost reports emissions for each covered firm from two sources: (i) direct firm disclosure, when the firm voluntarily publishes its Scope 1 and Scope 2 emissions; and (ii) Trucost's proprietary estimation model, which imputes emissions from sector benchmarks, production-process data, and firm-level operational inputs when firms do not report themselves. Both enter the market through the same data feed, at the same granularity, and are used interchangeably by downstream data aggregators; from the standpoint of market participants, they are equally observable.

We define the treatment event as Trucost's *first-coverage date*, i.e., the earliest date

on which any emissions information—whether firm-reported or Trucost-estimated—becomes observable through the Trucost data feed for a firm that was not previously covered. Of the 2,946 first-coverage events in our sample, approximately 95% correspond to Trucost estimates rather than voluntary firm reports.[1]

This composition is central to our identification strategy. In a setting dominated by voluntary firm disclosure, treatment timing would be endogenous to firm-specific liquidity conditions, managerial incentives, and unobserved news. In our setting, by contrast, the timing of coverage is determined primarily by Trucost's own estimation pipeline—the schedule with which the vendor extends model coverage to new firms as sector benchmarks, production-process inputs, and other operational data become available. Coverage timing is therefore driven by vendor-side data infrastructure rather than by firm-side disclosure decisions, and is largely orthogonal to firms' contemporaneous liquidity and ownership dynamics.

Because Trucost retrospectively revised all pre-2008 data in May 2009 (Zhang 2025), we restrict our observation period to first-coverage events between 2010 and 2023 to ensure consistent event timing. Firms that were already covered by Trucost before 2010 are excluded from the treated sample and do not appear as information-introduction events in our analysis. Within the 2010–2023 window, we observe substantial heterogeneity in the timing of Trucost's first-coverage events across North American publicly listed firms. The upper panel of Figure 1 plots the cumulative number of first-coverage events by industry. Coverage activity accelerates after 2015, consistent with the rise in climate-related regulation and institutional-investor demand for standardized emissions data following the Paris

[1] In the broader Trucost panel, direct firm reporting accounts for a larger share of observations than it does among first-coverage events. This is because Trucost's model-based estimation typically precedes voluntary firm reporting by several years; by the time a firm begins reporting its own emissions, Trucost has usually been estimating them for some time. First-coverage events are therefore concentrated in the estimation-based subsample.

Agreement; both forces increase demand for Trucost's coverage and, in turn, prompt the vendor to extend its estimation pipeline to a growing share of the corporate universe. The largest increases in coverage occur in the financial, health care, and information technology sectors, which start from comparatively low baseline coverage. Carbon-intensive sectors such as Energy and Materials expand more slowly, reflecting their already-higher pre-2015 coverage rates. This cross-industry and cross-time variation in the timing of Trucost's coverage decisions provides the staggered treatment assignment that underlies our identification design. The lower panel of Figure 1 shows the share of sample firms with Trucost coverage by industry. The pattern tracks the absolute counts: most industries move from roughly 20% coverage before 2015 to 40–80% coverage after 2015, while the utilities sector—where regulatory disclosure pressure and sector-specific emissions data predate our sample—already exceeded 40% coverage by 2010.

3.2 Liquidity Measures, Fundamentals, and Institutional Ownership

We collect daily U.S. and Canadian stock prices and trading volume from the Center for Research in Security Prices (CRSP) and firm fundamentals (number of shares outstanding, property, plant, and equipment (PPE), debt, stockholders' equity, common equity, net income) from Compustat. We merge the CRSP/Compustat data with monthly consolidated 13F filings from LSEG Workspace to measure firm-level institutional ownership.[2] Although 13F filings are reported quarterly, the LSEG consolidation delivers monthly updates, and the staggered distribution of first-coverage events across firms generates substantial variation in the time

[2]Table A1 provides descriptions of the variables used in our analyses.

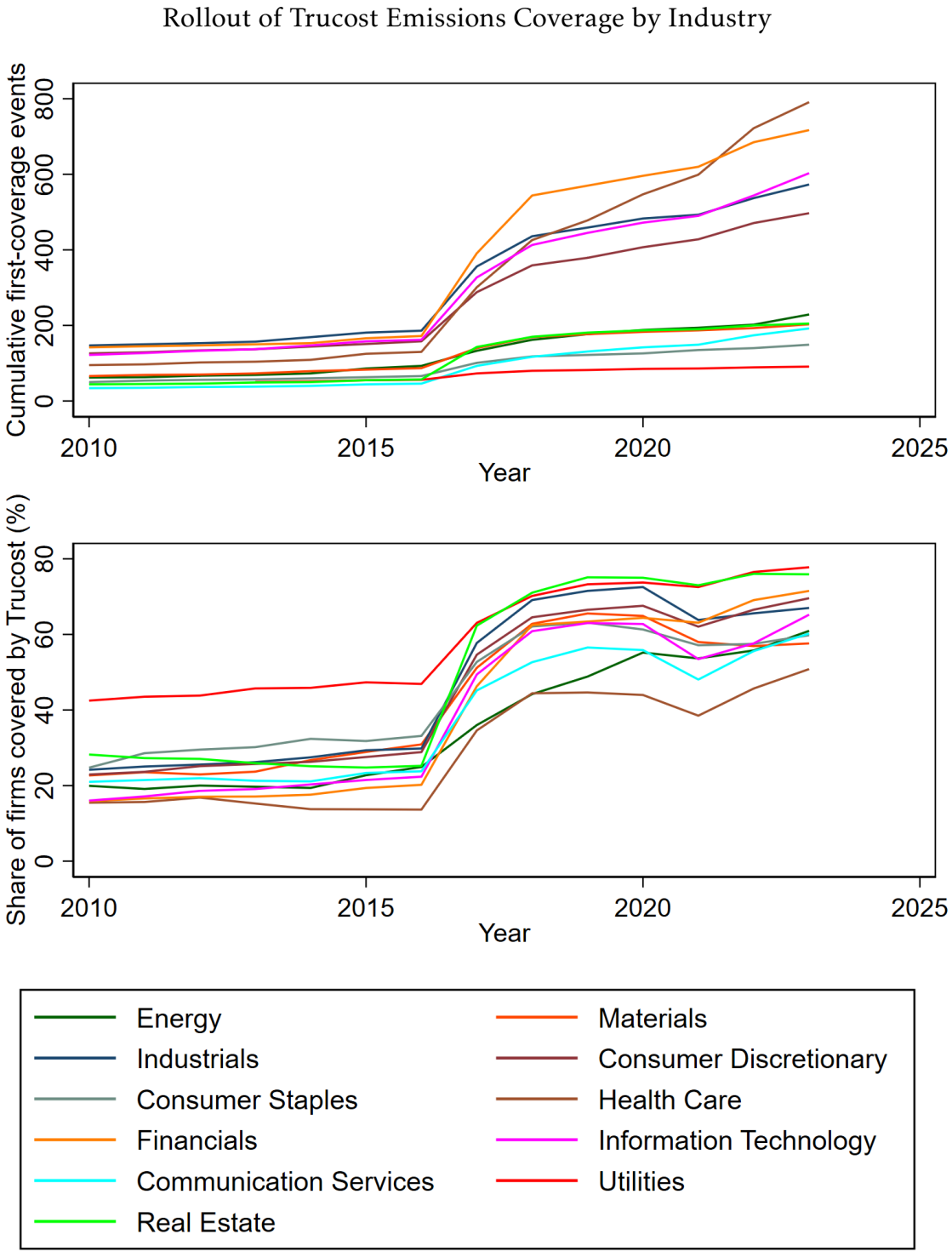


Figure 1: This figure shows the cumulative number of first-time Trucost coverage events (upper panel) and the share of sample firms with Trucost emissions coverage (lower panel), by industry and year, 2010–2023. A first-coverage event is defined as the first date on which Trucost publishes emissions information for a firm that was not previously covered, irrespective of whether the underlying emissions figure is firm-reported or Trucost-estimated; approximately 95% of first-coverage events in our sample are Trucost estimates.

between an event and its nearest 13F reporting date. This variation permits us to estimate dynamic treatment effects on institutional ownership at the bin (weekly) frequency used for liquidity outcomes.

We measure equity-market liquidity by four variables: (1) the logarithm of the relative bid-ask spread, computed as the bid-ask spread divided by the midpoint of the two quotes, which we refer to as the *Spread*; (2) the logarithm of the Amihud illiquidity ratio (Amihud 2002), defined as the average absolute return scaled by trading volume in dollars over a 30-day rolling window—a horizon long enough to stabilize the daily absolute-return-to-volume ratio while remaining short relative to our event window; (3) the logarithm of dollar trading volume; and (4) the logarithm of turnover, measured as shares traded divided by shares outstanding. Together these measures capture both the cost and the intensity of trading, allowing us to assess whether information introduction mitigates information asymmetry and improves market quality.

We aggregate daily observations into non-overlapping five-trading-day bins and conduct all analyses at the bin level, following Bessembinder (2003). We refer to each five-trading-day unit as a *bin*: trading days one through five constitute bin one, days six through ten constitute bin two, and so on. All variables are computed as averages of the daily observations within each bin. Consistent with Pástor and Stambaugh (2003), we construct all logarithmic variables (*Spread*, Amihud illiquidity, trading volume, turnover, market capitalization, and PPE) by first aggregating absolute values and then taking logarithms. Binning reduces idiosyncratic noise and day-of-the-week effects (Chordia, Roll, and Subrahmanyam 2001), yielding a more robust estimate of the treatment effect on liquidity. Our findings are robust to alternative aggregation intervals (three and ten trading days) and to the use of daily data.

3.3 Methodology

We analyze the effect of first-time emissions information introduction on equity-market liquidity and institutional ownership using a difference-in-differences (DiD) approach, as the timing of Trucost coverage is unlikely to be strictly exogenous with respect to firm characteristics. In applying a DiD design to liquidity outcomes, we follow Brogaard, Ringgenberg, and Roesch (2025), who examine the effect of the suspension of floor trading on equity-market liquidity.

For each treated firm (i.e., those for which Trucost publishes emissions information for the first time), we study the treatment effect over an interval spanning 11 bins before to 11 bins after the bin containing the event date (the *treatment bin*), which we refer to as the *event window*. This corresponds roughly to 60 trading days on each side of the event.

Because first-coverage timing varies across firms, we employ the staggered DiD framework of Callaway and Sant'Anna (2021), implemented via the `csdid2` Stata package, which allows us to estimate dynamic average treatment effects on the treated (ATT) in an event-study setting. We use the doubly robust DiD estimator based on stabilized inverse probability weighting and ordinary least squares developed by Sant'Anna and Zhao (2020). This estimator is robust to the heterogeneous-treatment-effect problems that can bias two-way fixed effects (TWFE) DiD regressions in staggered designs, as discussed by Goodman-Bacon (2021) and Baker, Larcker, and Wang (2022). In our setting, these problems are further attenuated because the control group is restricted to not-yet-treated firms, so we additionally report TWFE DiD regressions to demonstrate that our conclusions do not hinge on the choice of estimator.

We construct a matched control group for each treated firm using propensity

score matching, based on data from the calendar month that includes the day 90 days before the event. The set of potential control firms consists of all firms that do not receive Trucost coverage before or within two years after the matching date. That is, if matching takes place in year y, any untreated firm that is first covered by year $y+2$ or earlier is excluded from the control pool. We additionally exclude firms with stock prices below one dollar (following Peress and Schmidt 2020) and firms delisted during the event window, which ensures a constant sample size over the window. The matching variables are the logarithms of monthly average market capitalization, monthly average relative spread, and monthly average dollar trading volume, with a caliper of 0.005 (following Balakrishnan et al. 2014). For any given day d, the final sample comprises all treated firms whose event window contains day d, together with the corresponding control firms drawn from the treated firm's event window. We exclude treated firms for which no suitable control firms are matched and obtain, on average, 4.8 control firms per treated firm. Our results are robust to matching one or three control firms per treated firm instead of five.

A single firm may appear in the final sample more than once, for instance when it is matched as a control to multiple treated firms with overlapping event windows, or when it serves first as a control and subsequently as a treated firm. We retain these repeat observations, treating each appearance as an independent unit, and cluster standard errors at the firm level to account for within-firm correlation across appearances. Because the matching procedure cannot generate perfect counterfactuals, we include control variables in all DiD specifications to absorb residual differences between treated and control firms.

3.4 Descriptive Statistics

After merging the Trucost, CRSP/Compustat, and LSEG datasets and dropping observations from firms with incomplete data (e.g., missing sector information), our sample comprises 8,756 U.S. and Canadian firms over the period 2010–2023. Table 1 reports the industry and country composition of the sample, separating treated from control firms. Industry classifications follow Compustat's GSECTOR variable, and country identification is based on the reporting currency. The final dataset contains 16,976 treated-firm–control-firm units, of which 2,946 are treated and 14,030 are control observations; these 14,030 control observations correspond to 2,624 unique control firms, reflecting an average of approximately 5.4 matched appearances per unique control firm. The largest sectors are Health Care (20.4%), Financials (16.1%), and Industrials (10.4%), broadly mirroring the sector composition of the North American equity market. Sectoral and geographic distributions are balanced across treated and control groups, indicating that the two groups are comparable along observable dimensions.

Table 2 reports means and standard deviations of the liquidity measures and other financial variables used in the analysis, separately for treated and control firms. All continuous variables are winsorized at the 1st and 99th percentiles to limit the influence of outliers; our results are robust to alternative winsorization thresholds. All statistics are computed at the treatment bin, i.e., the bin containing the first-coverage date (for control firms, the bin containing the event date of the matched treated firm). The last column reports the mean difference between control and treated firms together with the result of a t-test on the difference. Treated and control firms are broadly comparable across most dimensions, although treated firms exhibit somewhat higher baseline institutional ownership (69.18% vs.

Table 1: Industry and Country

	Treatment Group					
	Total		*Control*		*Treated*	
	N	Share	N	Share	N	Share
N	16,976		14,030		2,946	
Sector						
Energy	1,761	(10.4%)	1,610	(11.5%)	151	(5.1%)
Materials	879	(5.2%)	751	(5.4%)	128	(4.3%)
Industrials	1,772	(10.4%)	1,380	(9.8%)	392	(13.3%)
Consumer Discretionary	1,771	(10.4%)	1,446	(10.3%)	325	(11.0%)
Consumer Staples	603	(3.6%)	510	(3.6%)	93	(3.2%)
Health Care	3,467	(20.4%)	2,883	(20.5%)	584	(19.8%)
Financials	2,737	(16.1%)	2,206	(15.7%)	531	(18.0%)
Information Technology	1,838	(10.8%)	1,427	(10.2%)	411	(14.0%)
Communication Services	988	(5.8%)	848	(6.0%)	140	(4.8%)
Utilities	391	(2.3%)	351	(2.5%)	40	(1.4%)
Real Estate	769	(4.5%)	618	(4.4%)	151	(5.1%)
Country						
USA	16,649	(98.1%)	13,717	(97.8%)	2,932	(99.5%)
Canada	327	(1.9%)	313	(2.2%)	14	(0.5%)

Notes: This table reports the industry and country of each firm, differentiating between control and treated firms. Firms are classified by industry using the GSECTOR variable from Compustat. The country is determined based on the currency (CURCD) variable from Compustat.

54.66%) and slightly higher market capitalization. We do not explicitly match on institutional ownership because doing so would introduce post-treatment bias in the ownership analysis of Section 5; instead, we rely on firm fixed effects in all DiD specifications to absorb time-invariant level differences between groups.

Table 2: Summary Statistics

	Treatment Group						
	Total		*Control*		*Treated*		
	Mean	SD	Mean	SD	Mean	SD	Diff.
Spread	2.70	(1.36)	2.71	(1.39)	2.62	(1.21)	0.09***
Log Amihud Illiq.	−19.38	(2.54)	−19.35	(2.64)	−19.55	(2.03)	0.20***
Log Volume	15.41	(2.25)	15.38	(2.32)	15.55	(1.88)	−0.17***
Log Turnover	−5.37	(1.22)	−5.40	(1.25)	−5.25	(1.04)	−0.15***
Market Cap. in bn	7.50	(21.29)	8.40	(22.95)	3.17	(8.90)	5.23***
Log Market Cap.	20.76	(2.03)	20.76	(2.15)	20.80	(1.37)	−0.04
Log PPE	5.59	(2.91)	5.68	(3.04)	5.17	(2.12)	0.51***
Leverage	0.89	(2.87)	0.88	(2.92)	0.91	(2.61)	−0.03
B/M	0.57	(0.63)	0.58	(0.65)	0.52	(0.52)	0.07***
Return on Equity	−0.06	(0.36)	−0.07	(0.36)	−0.03	(0.34)	−0.04***
Price	28.01	(37.35)	27.34	(37.83)	31.21	(34.80)	−3.87***
Inst. Own. in %	57.18	(34.34)	54.66	(35.01)	69.18	(27.98)	−14.51***

Notes: This table reports the mean and standard deviation for all financial variables in the data sample around the treatment date (or for control firms, around the treatment date of the respective treated firm), differentiating between control and treated firms. The last column reports the result of a t-test on the difference between control and treated firms. Each observation is obtained by aggregating five consecutive trading days. The statistics are calculated using the observation containing the treatment date. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.
The equity-market liquidity measures are introduced in Section 3. The *market capitalization* is the product of the number of common shares outstanding and the closing stock price. The *property, plant, and equipment (PPE)* comes from Compustat. The *leverage* is the sum of long-term debt and debt in current liabilities, divided by stockholders' equity. The *book-to-market ratio (B/M)* is the amount of common equity divided by the market capitalization. The *return on equity* is the net income divided by the market capitalization. The *price* is the closing stock price, and *institutional ownership* is from LSEG.

4 Emissions Information and Liquidity

We begin by testing whether the introduction of Trucost emissions coverage improves equity-market liquidity. Our baseline outcome is the logarithmic relative bid-ask spread (*Spread*); we extend to additional measures below. We first present dynamic treatment effects using the estimator of Callaway and Sant'Anna (2021), and then complement these with fixed-effects panel regressions that yield average treatment effects and allow us to evaluate robustness across specifications.

Figure 2 plots the dynamic average treatment effects (ATT) on the *Spread*, with each bar representing one bin. Pre-treatment ATTs are statistically indistinguishable from zero, supporting the parallel-trends assumption. The post-treatment ATT is negative, significant, and grows monotonically in event time. It becomes statistically significant within approximately 15 trading days (three bins) after first coverage and reaches roughly −0.03 by the end of the event window, corresponding to a 3% reduction in the relative bid-ask spread of newly covered firms relative to matched controls.[3]

To obtain a single average treatment effect and to test robustness across specifications, we complement the dynamic estimates with a fixed-effects panel regression of the form

$$\begin{aligned} Spread_{i,t} = \delta_1 \times Treated_{i,t} \times Post_{i,t} + \delta_2 \times Post_{i,t} + \delta_3 \times Treated_{i,t} \\ + \sum_{k=1}^{n} \beta_k \times \text{Control}_{k,i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}, \end{aligned} \tag{1}$$

where *Treated* equals 1 for firms that receive first-time Trucost coverage within the event window and 0 for matched control firms, and *Post* equals 1 in bins after

[3] Results are similar when we use the relative spread rather than its logarithmic transformation.

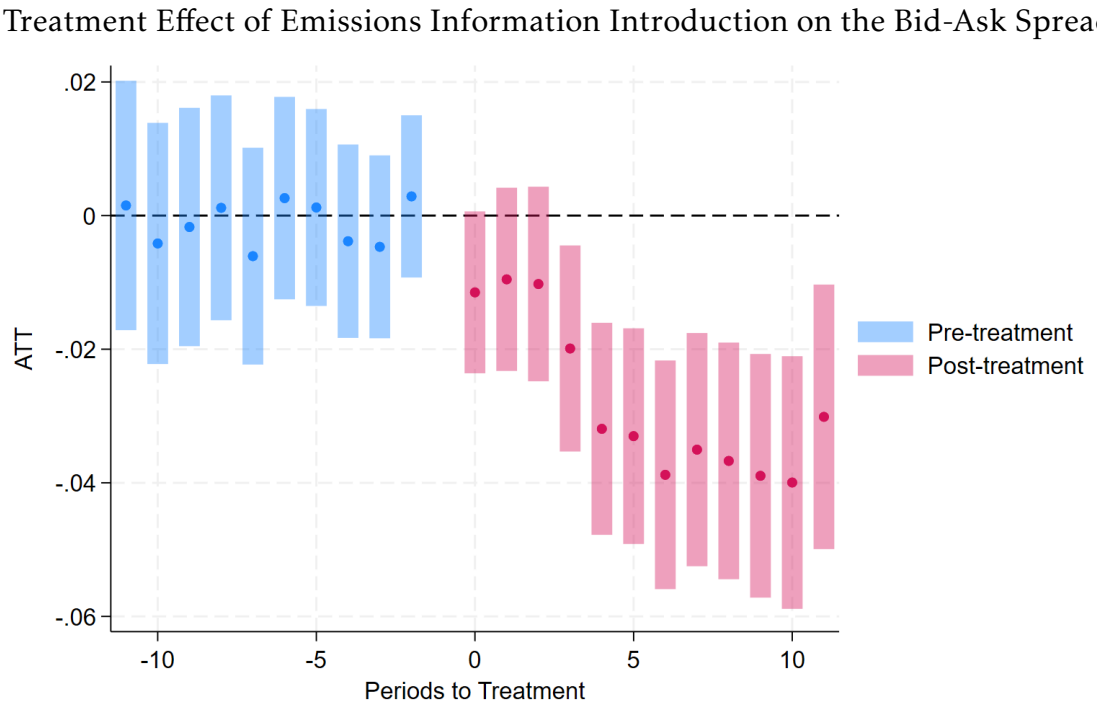


Figure 2: Using the DiD estimator of Callaway and Sant'Anna (2021), we estimate the effect of first-time Trucost emissions coverage on the logarithmic relative bid-ask spread (*Spread*). The ATT is depicted for each period relative to the event, along with the 95% confidence interval. One period corresponds to five consecutive trading days. Control variables are the logarithmic market capitalization, 1/price, leverage, book-to-market ratio, return on equity, and logarithmic trading volume.

the event date (for control firms, after the event date of the matched treated firm) and 0 otherwise. The interaction *Treated* × *Post* is the DiD term of interest, and its coefficient δ_1 estimates the average differential effect of information introduction on equity-market liquidity. Time fixed effects γ_t are defined at the bin level. Firm fixed effects α_i are assigned separately to each appearance of a firm in the final dataset: when a firm is matched as a control to several treated firms, or appears first as a control and later as a treated firm, each appearance is treated as an independent unit. In specifications with firm fixed effects, the *Treated* dummy is absorbed. Our control set includes market capitalization, leverage, book-to-market ratio, and return on equity, similar to Brogaard et al. (2025) and Zhang (2025). Standard errors are clustered at the firm and time level.

Table 3 reports the results. Column (1), without fixed effects or controls, yields a *Treated* × *Post* coefficient of −0.02, significant at the 1% level: the relative bid-ask spread declines by approximately 2% following the introduction of emissions information, relative to the change in the control group over the same period. The coefficient is stable across Columns (2) through (7), which progressively add time fixed effects, firm fixed effects, and firm-level controls. Columns (8) through (10) additionally include logarithmic trading volume as a control. This is a conservative test—volume is itself a liquidity-related variable, so conditioning on it mechanically absorbs part of the treatment effect—but the coefficient remains negative and statistically significant at −0.01. Taken together, the DiD regression estimates a 1–2% average reduction in bid-ask spreads across the event window. This is somewhat smaller in magnitude than the end-of-window dynamic ATT of −0.03 in Figure 2, which is expected: the regression averages the treatment effect over all post-event bins, including those immediately following the event in which the effect has not yet fully materialized, whereas the dynamic ATT captures the effect at its strongest

point.

We next extend the analysis to additional liquidity measures. Table 4 reports DiD regressions using the logarithmic Amihud illiquidity ratio, logarithmic dollar trading volume, and logarithmic turnover as dependent variables. Columns (1) and (2) show that the Amihud illiquidity ratio declines by approximately 0.04 following first coverage, corresponding to a 4% reduction in illiquidity; the coefficient is statistically significant at the 1% level and mirrors the pattern for the *Spread*. Columns (3) through (6) show that dollar trading volume and turnover each increase by approximately 0.04 following first coverage, indicating a 4% rise in trading activity. The four measures move in the directions predicted by a reduction in information asymmetry—lower costs of trade and higher intensity of trade—and collectively confirm that the introduction of standardized emissions information improves equity-market liquidity.

5 Emissions Information and Institutional Ownership

Having established that the introduction of Trucost emissions coverage improves equity-market liquidity, we now examine whether it also reshapes the ownership of newly covered firms. If institutional investors systematically select into firms once standardized emissions information becomes available—as the intuition of a selection effect would predict—then first coverage should coincide with an increase in institutional ownership relative to matched non-covered firms.

Figure 3 plots the dynamic ATT on the share of institutional ownership, using the same Callaway and Sant'Anna (2021) staggered DiD framework. Pre-treatment ATTs are statistically indistinguishable from zero, consistent with parallel trends. Following first coverage, institutional ownership rises steadily and reaches approxi-

Table 3: DiD Analysis on the Bid-Ask Spread around Emissions Information Introduction

Dep. Variable = *Spread*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	−0.08*	−0.07*								
	(0.04)	(0.04)								
Post	0.03	−0.11***	0.03***	0.00	0.02***	0.00	0.00	0.00	0.00	0.00
	(0.03)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treated × *Post*	−0.02***	−0.02***	−0.02***	−0.02***	−0.02***	−0.02***	−0.02***	−0.01***	−0.01**	−0.01**
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log Market Cap.					−0.49***	−0.46***	−0.45***	−0.31***	−0.26***	−0.29***
					(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
B/M					0.06***	0.04**		0.00	0.01	0.02
					(0.02)	(0.02)		(0.01)	(0.01)	(0.02)
Return on Equity					0.04*	0.04*		0.05***	0.04***	0.04***
					(0.02)	(0.02)		(0.01)	(0.01)	(0.01)
1/Price								1.30***	1.06***	0.99***
								(0.09)	(0.08)	(0.09)
Log Volume									−0.08***	−0.07***
									(0.00)	(0.00)
Leverage										−0.00
										(0.00)
Log PPE										−0.01
										(0.01)
Constant	2.69***	2.77***	2.68***	2.69***	12.84***	12.14***	12.00***	8.92***	9.19***	9.78***
	(0.05)	(0.04)	(0.00)	(0.00)	(0.59)	(0.60)	(0.56)	(0.64)	(0.62)	(0.66)
Time fixed effects	No	Yes	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	390279	390279	390279	390279	383693	383693	390279	383693	383693	309455
r2	0.00	0.13	0.95	0.96	0.96	0.96	0.96	0.96	0.96	0.96

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results of a fixed effects regression of the logarithmic relative bid-ask spread (*Spread*) on several firm characteristics around first-time Trucost emissions coverage. *Treated* is 1 for treated firms and 0 for control firms. *Post* is 1 after treatment (for control firms: after treatment of the respective treated firm) and 0 before treatment. Each observation corresponds to five consecutive firm-trading days. The period we consider spans from eleven bins before to eleven bins after the event, where each bin accordingly comprises five trading days. Standard errors are clustered at the firm and time level.

Table 4: DiD Analysis on Alternative Liquidity Measures around Emissions Information Introduction

	(1) Log Amihud Illiq.	(2) Log Amihud Illiq.	(3) Log Volume	(4) Log Volume	(5) Log Turnover	(6) Log Turnover
Post	0.01***	0.01**	−0.00	−0.00	−0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Treated × *Post*	−0.06***	−0.04***	0.05***	0.04***	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log Market Cap.		−0.08*		0.24***		−0.66***
		(0.05)		(0.08)		(0.07)
Leverage		−0.00		0.00		0.00
		(0.00)		(0.00)		(0.00)
B/M		−0.01		0.07**		0.07***
		(0.02)		(0.03)		(0.02)
Return on Equity		0.03		−0.09***		−0.06
		(0.02)		(0.02)		(0.04)
Log Price		−0.82***		0.91***		0.78***
		(0.05)		(0.08)		(0.07)
Constant	−19.39***	−15.46***	15.42***	7.96***	−5.36***	6.23***
	(0.00)	(0.88)	(0.00)	(1.38)	(0.00)	(1.28)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	390272	381969	390279	381976	390220	381976
r2	0.98	0.98	0.94	0.95	0.84	0.84

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results of a fixed effects regression of the logarithmic Amihud illiquidity measure, the logarithmic dollar trading volume, and the logarithmic turnover on several firm characteristics around first-time Trucost emissions coverage. *Treated* is 1 for treated firms and 0 for control firms. *Post* is 1 after treatment (for control firms: after treatment of the respective treated firm) and 0 before treatment. Each observation corresponds to five consecutive firm-trading days. The period we consider spans from eleven bins before to eleven bins after the event, where each bin accordingly comprises five trading days. Standard errors are clustered at the firm and time level.

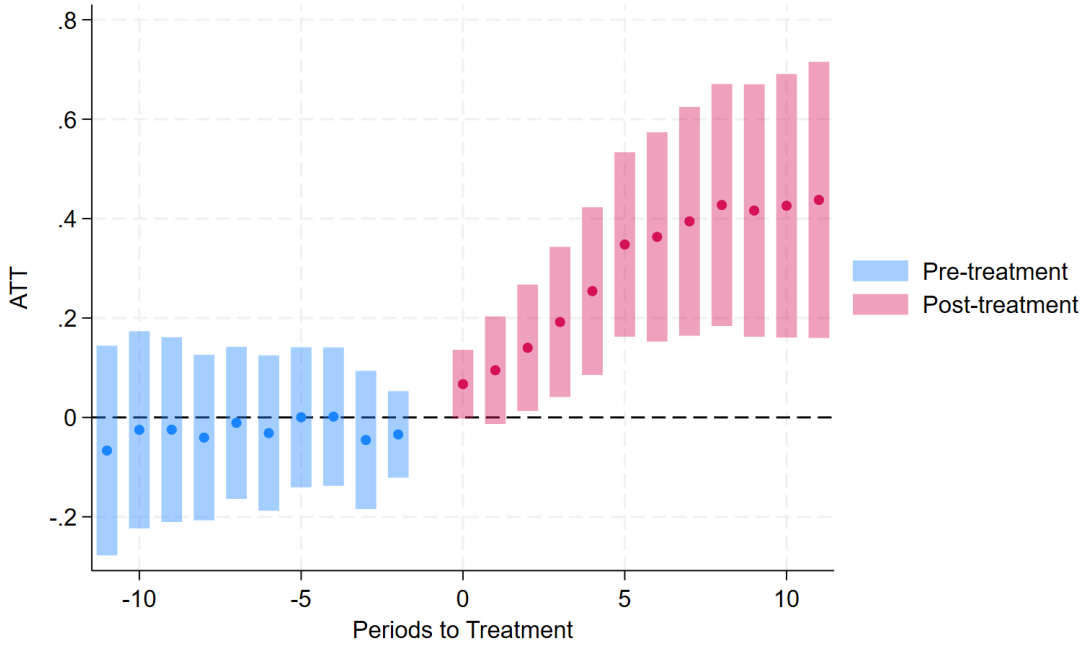


Figure 3: Using the DiD method from Callaway and Sant'Anna (2021), we estimate the effect of first-time Trucost emissions coverage on the share of institutional ownership in %. The ATT is depicted for each period relative to the event, along with the 95% confidence interval. One period corresponds to five consecutive trading days. No control variables were used.

mately 0.4 percentage points by the end of the event window, indicating that newly covered firms attract roughly 40 additional basis points of institutional ownership relative to matched controls.

To obtain a single average effect and test robustness, we estimate the fixed-effects panel regression

$$\begin{aligned} \text{Inst. Own. in } \%_{i,t} = \delta_1 \times Treated_{i,t} \times Post_{i,t} + \delta_2 \times Post_{i,t} + \delta_3 \times Treated_{i,t} \\ + \sum_{k=1}^{n} \beta_k \times \text{Control}_{k,i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}, \end{aligned} \tag{2}$$

where the dependent variable is the share of institutional ownership in percent.

Table 5 reports the results. The coefficient on *Treated* × *Post* indicates that the share of institutional ownership rises by approximately 0.3 percentage points following first coverage, relative to matched control firms. This estimate corroborates the dynamic pattern in Figure 3: the regression coefficient averages the effect over all post-event bins, while the end-of-window ATT of 0.4 percentage points reflects the effect at its strongest point. Both measures confirm a positive, statistically robust differential increase in institutional ownership following the introduction of standardized emissions information.

The effect is economically meaningful. Applied to the market capitalizations of treated firms at the time of first coverage, the 0.3 percentage-point coefficient corresponds to approximately $30 billion of institutional capital reallocated toward newly covered firms over our sample period from 2010 to 2023.

Table A2 in the appendix disaggregates the overall effect by investor type, distinguishing between investment advisors, banks and trusts, hedge funds, private equity, and individual investors. The overall increase is driven primarily by investment advisors and by banks and trusts—long-horizon institutional investors with fiduciary obligations and ESG integration mandates (Krueger, Sautner, and Starks 2020; Pástor, Stambaugh, and Taylor 2021). Hedge fund holdings show no significant response, while private-equity and individual retail holdings decline. This pattern is consistent with a rotation in the ownership base: as long-horizon institutional capital flows into newly covered firms, earlier holders that operate with different informational technologies—private-equity vehicles whose comparative advantage lies in private information, and retail investors whose participation is typically concentrated in less transparent firms—reduce their exposure. First coverage therefore not only attracts new capital but also shifts the composition of the shareholder base toward investor classes that place greater weight on standardized

Table 5: DiD Analysis on the Share of Institutional Ownership around Emissions Information Introduction

	(1) Inst. Own. in %	(2) Inst. Own. in %	(3) Inst. Own. in %
Post	−0.11***	−0.11***	−0.11***
	(0.04)	(0.04)	(0.04)
Treated × *Post*	0.34***	0.31***	0.32***
	(0.12)	(0.12)	(0.12)
Log Market Cap.		1.78***	1.59***
		(0.26)	(0.29)
Leverage			0.03
			(0.05)
B/M			−0.01
			(0.22)
Return on Equity			0.30
			(0.20)
Constant	57.16***	20.19***	24.20***
	(0.01)	(5.47)	(6.13)
Time fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Observations	386497	386497	378862
r2	0.99	0.99	0.99

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results of a fixed effects regression of the share of institutional ownership on several firm characteristics around first-time Trucost emissions coverage. *Treated* is 1 for treated firms and 0 for control firms. *Post* is 1 after treatment (for control firms: after treatment of the respective treated firm) and 0 before treatment. Each observation corresponds to five consecutive firm-trading days. The period we consider spans from eleven bins before to eleven bins after the event, where each bin accordingly comprises five trading days. Standard errors are clustered at the firm and time level.

environmental information.

6 Heterogeneity

In this section, we examine whether the two effects, i.e., the improvement of equity-market liquidity (Section 4) and the rise in institutional ownership (Section 5), are causally linked, where they are strongest, and whether they are robust. We first test whether the ownership reallocation partially explains the liquidity improvement—the mechanism test that underlies our paper's central claim. We then examine how the liquidity effect varies with the level of disclosed emissions, with the timing of Trucost coverage, and across sectors and regions that differ in their ex-ante climate salience. A final subsection reports a battery of robustness tests.

6.1 Variation across Institutional Ownership

The two effects documented in Sections 4 and 5 could, in principle, be two parallel consequences of information introduction operating through independent channels. In a pure Diamond and Verrecchia (1991) account, information reduces adverse-selection costs directly, and the composition of the investor base plays no explanatory role. But a separate literature documents that institutional participation independently improves market quality by increasing monitoring, broadening order flow, and reducing inventory costs for market makers (Rubin 2007; Kini and Mian 1995). If this second channel is active in our setting, then firms that experience larger increases in institutional ownership around first coverage should also exhibit larger liquidity improvements. The cross-sectional correlation between the two effects therefore identifies whether ownership operates as an explanatory channel for liquidity, rather than as a coincident outcome.

We test this by augmenting Model 1 with a triple interaction between the DiD indicator and the change in institutional ownership around the event. We denote this change as Δ*Inst. Own.*, defined as the difference in institutional ownership between three months before and three months after the event date for each treated firm; the variable is expressed in units where a value of 1 corresponds to a 100-percentage-point change, so a 1-percentage-point change is coded as 0.01. Control firms are assigned the same value as their matched treated firm.

Table 6 reports the results. The coefficient on *Treated* $\times$ *Post* $\times$ Δ*Inst. Own.* is approximately -0.2, negative and statistically significant. Applying the scaling, a 1-percentage-point larger increase in institutional ownership around first coverage is associated with an additional reduction in the *Spread* of $0.01 \times 0.2 = 0.002$ log points, or approximately 0.2% in levels, beyond the direct effect of information introduction. Institutional investors therefore do not merely respond to emissions transparency; they amplify its effect on market quality. Ownership is, in this sense, a channel linking information to liquidity.

To confirm that this result is not driven by outliers, we repeat the specification in Table A3 of the appendix using a dummy variable Δ *Inst. Own. above median*, which equals 1 if a treated firm's change in institutional ownership exceeds the median change of +0.56 percentage points across all treated firms, and 0 otherwise. Control firms take the same value as their matched treated firm. The result holds: the interaction is driven by firms with a relatively pronounced increase in institutional ownership, rather than by an influential tail.

Table 6: DiD Analysis on the Bid-Ask Spread around Emissions Information Introduction, Dependent on Change in Institutional Ownership

	(1) *Spread*	(2) *Spread*	(3) *Spread*
Post	0.00	0.00	−0.00
	(0.00)	(0.00)	(0.00)
Treated × *Post* × Δ Inst. Own.	−0.24***	−0.13*	−0.13*
	(0.08)	(0.07)	(0.07)
Treated × *Post*	−0.02***	−0.02***	−0.02***
	(0.01)	(0.01)	(0.01)
Post × Δ Inst. Own.	0.02	0.00	−0.00
	(0.03)	(0.03)	(0.03)
Log Market Cap.		−0.45***	−0.45***
		(0.03)	(0.03)
B/M			0.04**
			(0.02)
Return on Equity			0.04*
			(0.02)
Constant	2.70***	11.97***	12.10***
	(0.00)	(0.57)	(0.61)
Time fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Observations	386898	386898	380473
r2	0.96	0.96	0.96

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results of a fixed effects regression of the logarithmic relative bid-ask spread (*Spread*) on several firm characteristics around first-time Trucost emissions coverage. *Treated* is 1 for treated firms and 0 for control firms. *Post* is 1 after treatment (for control firms: after treatment of the respective treated firm) and 0 before treatment. Δ Inst. Own. is the change in institutional ownership from 3 months before the event to 3 months after the event; it is expressed in units where 1 equals 100 percentage points. Each observation corresponds to five consecutive firm-trading days. The period we consider spans from eleven bins before to eleven bins after the event, where each bin accordingly comprises five trading days. Standard errors are clustered at the firm and time level.

6.2 Variation across Carbon Emissions

The liquidity effect of first coverage may vary with the level of emissions disclosed. Ex ante, the direction of this heterogeneity is ambiguous. On one hand, high emitters may exhibit greater opacity before coverage, consistent with evidence that weaker CSR performers tend to withhold environmental information (Cho, Lee, and Pfeiffer 2013); in this case, the resolution of information asymmetry would be larger when their emissions are revealed. On the other hand, high emitters may face elevated regulatory and stakeholder pressure that pushes them toward greater transparency before Trucost coverage begins; in this case, first coverage would be less incrementally informative. Whether the net effect is stronger or weaker for high emitters is therefore an empirical question.

To test this, we interact the DiD indicator in Model 1 with *High Emissions*, a dummy that equals 1 if the Scope 1 emissions revealed at first coverage exceed the sector median and 0 otherwise; control firms are assigned the same value as their matched treated firm. Table 7 reports the results. The baseline *Treated* × *Post* coefficient remains negative, and the interaction *Treated* × *Post* × *High Emissions* is −0.03, statistically significant at the 1% level. The total liquidity effect for high emitters roughly doubles the baseline, consistent with the first interpretation: first coverage resolves a larger information gap for firms with greater carbon exposure.

Beyond the level of emissions, the market reaction may depend on how accurately investors could estimate emissions before Trucost coverage. Using sector-median emissions as a naive benchmark, we compute each treated firm's estimation error as the absolute difference between its first-covered emissions and the sector-median emissions. We partition treated firms into quartiles Q1 (smallest error) through Q4 (largest error) and estimate the baseline DiD model separately within

Table 7: DiD Analysis on the Bid-Ask Spread around Emissions Information Introduction, Dependent on Emission Levels

	(1) *Spread*	(2) *Spread*	(3) *Spread*
Post	−0.00	−0.00	−0.00
	(0.00)	(0.00)	(0.00)
Treated × *Post* × *High Emissions*	−0.03***	−0.03***	−0.03***
	(0.01)	(0.01)	(0.01)
Treated × *Post*	−0.02**	−0.01	−0.01
	(0.01)	(0.01)	(0.01)
Post × *High Emissions*	0.01**	0.01***	0.01**
	(0.01)	(0.00)	(0.00)
Log Market Cap.		−0.45***	−0.46***
		(0.03)	(0.03)
B/M			0.04**
			(0.02)
Return on Equity			0.04*
			(0.02)
Constant	2.69***	12.00***	12.14***
	(0.00)	(0.56)	(0.60)
Time fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Observations	390279	390279	383693
r2	0.96	0.96	0.96

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results of a fixed effects regression of the logarithmic relative bid-ask spread (*Spread*) on several firm characteristics around first-time Trucost emissions coverage. *Treated* is 1 for treated firms and 0 for control firms. *Post* is 1 after treatment (for control firms: after treatment of the respective treated firm) and 0 before treatment. *High Emissions* is 1 for firms disclosing emissions that are higher than the industry median. Each observation corresponds to five consecutive firm-trading days. The period we consider spans from eleven bins before to eleven bins after the event, where each bin accordingly comprises five trading days. Standard errors are clustered at the firm and time level.

each quartile, together with the matched control firms. Table 8 reports the results. Firms with the smallest estimation errors (Q1) show no significant liquidity effect, while firms in Q2 through Q4 exhibit negative and statistically significant *Treated* × *Post* coefficients. The liquidity response is strongest where investors' ex-ante emissions estimates were most likely to be wrong, reinforcing the interpretation that first coverage resolves larger information gaps when emissions are harder to predict.

6.3 Variation across Timing of Emissions Information Introduction

While the timing of first-time emissions information introduction may influence market reactions due to the varying amounts of new information released, the net effect is ex-ante ambiguous and could manifest in two opposing ways. On the one hand, firms covered early in their industry may exhibit greater information asymmetry than firms covered later, because market participants have not yet been able to build industry-level expectations. Once some firms in a sector are covered, investors can form sharper priors about the likely emissions of remaining firms, and Trucost's own estimation models improve as more industry data become available. On the other hand, early-covered firms may be those whose emissions are relatively easy to estimate—for example, because their disclosures, production processes, or asset bases are more transparent—leading Trucost to cover them sooner. If so, the set of not-yet-covered firms has progressively more uncertain emissions profiles, and information introduction for these firms resolves larger informational gaps. Early coverage may also reveal systematically lower emissions in a sector, prompting

Table 8: DiD Analysis on the Bid-Ask Spread around Emissions Information Introduction, Dependent on Emission Estimation Error

	Estimation Error			
	Est. Error Q1	Est. Error Q2	Est. Error Q3	Est. Error Q4
Dep. Variable = *Spread*	(1)	(2)	(3)	(4)
Post	−0.01	0.01	−0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Treated × *Post*	0.00	−0.03***	−0.03***	−0.01*
	(0.01)	(0.01)	(0.01)	(0.01)
Log Market Cap.	−0.41***	−0.44***	−0.51***	−0.48***
	(0.04)	(0.04)	(0.03)	(0.03)
B/M	0.06*	0.01	0.04*	0.06***
	(0.03)	(0.03)	(0.02)	(0.02)
Return on Equity	0.05**	−0.00	0.06*	−0.02
	(0.02)	(0.04)	(0.03)	(0.02)
Constant	11.33***	11.93***	13.17***	12.49***
	(0.88)	(0.79)	(0.73)	(0.70)
Time fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Observations	96594	95629	95494	95976
r2	0.96	0.96	0.96	0.96

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results of a fixed effects regression of the logarithmic relative bid-ask spread (*Spread*) on several firm characteristics around first-time Trucost emissions coverage. Treated firms are sorted into four quartiles (Q1, Q2, Q3, and Q4) based on their emission estimation errors (absolute difference between disclosed emissions and median sector emissions). *Treated* is 1 for treated firms and 0 for control firms. *Post* is 1 after treatment (for control firms: after treatment of the respective treated firm) and 0 before treatment. Each observation corresponds to five consecutive firm-trading days. The period we consider spans from eleven bins before to eleven bins after the event, where each bin accordingly comprises five trading days. Standard errors are clustered at the firm and time level.

investors to revise their beliefs about the remaining uncovered firms downward.[4] Krueger et al. (2024) find that, under ESG disclosure mandates, voluntary late adopters experience greater liquidity improvements than early adopters, consistent with the second channel.

We partition treated firms by timing: *early disclosers* are the first 50% of firms within an industry to receive Trucost coverage, and *late disclosers* are the remaining 50%. The median coverage date is September 2017 for early disclosers and August 2020 for late disclosers. Table 9 reports the distribution of emissions levels, type of first reporting, and industries/countries across the two groups. First coverage is overwhelmingly estimation-based for both groups (97% for early, 94% for late). The share of high-emitting firms is slightly higher among early disclosers than among late disclosers, but the difference is small, implying that any timing heterogeneity documented below does not primarily reflect differences between high- and low-emitters already captured in Section 6.2.

Moreover, Table A4 reports financial and ownership characteristics of early and late disclosers. The two groups differ along several dimensions: early disclosers are somewhat larger ($3.44bn vs. $2.90bn in market capitalization), have markedly higher baseline institutional ownership (76.51% vs. 61.74%), and exhibit tighter pre-event liquidity (average *Spread* of 2.27 vs. 2.98). These differences indicate that early- and late-covered firms are not identical populations: Trucost's coverage pipeline reaches large, liquid, and institutionally-held firms earlier. Our DiD specification with firm fixed effects absorbs these time-invariant level differences, so the timing heterogeneity we document below captures the differential change in liquidity around the event, not the cross-sectional level difference between groups.

[4] In a related but distinct vein, Yan, Li, Huang, and Li (2020) show that the consistency of carbon performance and carbon information disclosure significantly affects enterprise value.

Table 9: Firm Characteristics by Timing of Trucost Emissions Coverage

	Disclosure Timing			
	Early		*Late*	
	N	Share	N	Share
N	1,480		1,466	
Emissions Relative to Industry Median				
Low Emissions	1,035	(69.9%)	1,100	(75.0%)
High Emissions	445	(30.1%)	366	(25.0%)
Type of Disclosure				
Disclosed by Firm	49	(3.3%)	85	(5.8%)
Estimated	1,431	(96.7%)	1,381	(94.2%)
Sector				
Energy	76	(5.1%)	75	(5.1%)
Materials	64	(4.3%)	64	(4.4%)
Industrials	196	(13.2%)	196	(13.4%)
Consumer Discretionary	163	(11.0%)	162	(11.1%)
Consumer Staples	47	(3.2%)	46	(3.1%)
Health Care	292	(19.7%)	292	(19.9%)
Financials	266	(18.0%)	265	(18.1%)
Information Technology	209	(14.1%)	202	(13.8%)
Communication Services	70	(4.7%)	70	(4.8%)
Utilities	21	(1.4%)	19	(1.3%)
Real Estate	76	(5.1%)	75	(5.1%)
Country				
USA	1,470	(99.3%)	1,462	(99.7%)
Canada	10	(0.7%)	4	(0.3%)

Notes: This table reports the level of Scope 1 emissions relative to the industry median type of first carbon reporting, industry, and country of each firm, differentiating between early and late disclosing firms. Firms are classified by industry using the GSECTOR variable from Compustat. The country is determined based on the currency (CURCD) variable from Compustat.

We formalize the timing test by adding the interaction *Treated* × *Post* × *Late* to Model 1, where *Late* equals 1 for firms in the late-covered half and 0 otherwise; control firms are assigned the value of their matched treated firm. Table 10 reports the results. The triple-interaction coefficient in Columns (2) and (3) is −0.03, statistically significant at the 1% level: the liquidity improvement is substantially larger for late-covered firms. Columns (4) and (5) report DiD regressions restricted to the early and late subsamples separately, together with their matched controls. The *Treated* × *Post* coefficient is insignificant for the early subsample and significantly negative at the 1% level for the late subsample. These results are consistent with the second channel outlined above: late-covered firms have harder-to-predict emissions, so first coverage resolves a larger information gap for them. A practical implication is that firms yet to be covered by Trucost can still expect meaningful liquidity benefits when coverage arrives; the effect is not confined to early movers.

6.4 Variation across States, Sectors, and Analyst Coverage

A distinctive implication of our paper's interpretation—that institutional capital reallocation and liquidity improvements are driven primarily by the informational content of standardized emissions data rather than by investor climate preferences—is that the liquidity effect should also appear in subsamples where climate salience is low. We test this prediction along four dimensions of ex-ante climate salience: local climate beliefs, state-level carbon pricing, sectoral materiality, and analyst coverage. Table A5 in the appendix reports the DiD regressions within each subsample.

Columns (1) and (2) split firms by the climate beliefs of their headquarters state, using U.S. state-level survey data from Marlon, Wang, Bergquist, Howe, Leiserowitz, Maibach, Mildenberger, and Rosenthal (2022). *Yale Low* firms are headquartered in

Table 10: DiD Analysis on the Bid-Ask Spread around Emissions Information Introduction, Dependent on Timing of Emissions Coverage

	Disclosure Timing				
	All			*Early*	*Late*
	(1)	(2)	(3)	(4)	(5)
	Spread	*Spread*	*Spread*	*Spread*	*Spread*
Post	−0.00	−0.00	−0.01	−0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treated × *Post* × *Late*	−0.02	−0.03***	−0.03***		
	(0.01)	(0.01)	(0.01)		
Treated × *Post*	−0.01*	−0.00	−0.00	0.00	−0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Post × *Late*	0.01	0.01*	0.01**		
	(0.01)	(0.01)	(0.01)		
Log Market Cap.		−0.45***	−0.46***	−0.61***	−0.36***
		(0.03)	(0.03)	(0.03)	(0.03)
B/M			0.04**	0.04	0.05**
			(0.02)	(0.04)	(0.02)
Return on Equity			0.04*	−0.05	0.03
			(0.02)	(0.04)	(0.02)
Constant	2.69***	12.00***	12.15***	15.08***	10.43***
	(0.00)	(0.56)	(0.60)	(0.74)	(0.63)
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	390279	390279	383693	200120	183573
r2	0.96	0.96	0.96	0.96	0.95

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results of a fixed effects regression of the logarithmic relative bid-ask spread on several firm characteristics around first-time Trucost emissions coverage. *Treated* is 1 for treated firms and 0 for control firms. *Post* is 1 after treatment (for control firms: after treatment of the respective treated firm) and 0 before treatment. The dummy variable *Late* is 1 if a disclosing firm is among the last 50% of firms within the same industry to disclose emissions, and 0 otherwise. Each observation corresponds to five consecutive firm-trading days. The period we consider spans from eleven bins before to eleven bins after the event, where each bin accordingly comprises five trading days. Standard errors are clustered at the firm and time level.

states with a below-median share of residents who believe climate change is occurring; *Yale High* firms are headquartered in above-median states. The *Treated* × *Post* coefficient is negative and statistically significant in both subsamples, indicating that the liquidity improvement does not depend on local climate sentiment.

Columns (3) and (4) split firms by whether their state had implemented an Emissions Trading System (ETS) by 2023, as classified by the World Bank's Carbon Pricing Dashboard; Canadian firms are included with the ETS group. The effect holds in both subsamples, showing that the result is not exclusively driven by firms operating under a local carbon-pricing regime.

Columns (5) and (6) split firms by sectoral materiality of greenhouse gas emissions, using the Sustainability Accounting Standards Board (SASB) materiality map. The effect is statistically significant and of comparable magnitude for both firms in sectors where GHG emissions are not classified as financially material (Column (5)) and firms in the remaining sectors (Column (6)). This is the sharpest of the four cuts: even when emissions are not formally material to a sector's financial performance, the introduction of standardized third-party emissions information measurably improves liquidity.

Columns (7) and (8) split firms by analyst coverage. The treatment effect persists for firms with both below-median and above-median analyst counts, indicating that the liquidity improvement is not confined to lightly-monitored firms whose emissions are hard to learn through other channels, nor to heavily-monitored firms whose public information environment is already rich.

Taken together, these results support the paper's central interpretation: the liquidity gains following first-time emissions information introduction reflect the informational content of the data itself rather than investor climate preferences, regulatory pressure, or prior information asymmetry specific to a subset of firms.

6.5 Robustness Tests

We conduct a series of robustness tests to ensure the baseline findings are not driven by specification choices or confounding events.

Matching and specification. Sector affiliation is not included in our baseline matching procedure. To verify that sector-specific changes in liquidity are not driving the result, we re-estimate the main regressions after adding sector affiliation to the matching variables, and obtain coefficients consistent with our baseline. The findings are also robust to including Industry × Year fixed effects in the TWFE DiD regressions and to adding capital expenditure intensity as an alternative measure of asset tangibility.

Reporting-season timing. A potential concern is that first-coverage events are temporally correlated with firms' own financial disclosures, which would raise the possibility that our estimates capture standard earnings-announcement effects. Figure A1 in the appendix shows that Trucost coverage events are dispersed throughout the calendar year, rather than clustered around quarterly reporting dates. We additionally re-estimate the baseline specifications after excluding firms whose first-coverage event occurs during the reporting-season months (January, April, July, October), and obtain consistent results.

Placebo tests. To rule out confounding information events unrelated to Trucost coverage, we conduct a placebo test using randomized treatment dates. For firms not covered by Trucost prior to 2010, we assign pseudo-treatment dates drawn from a uniform distribution over 2010–2024, calibrated so that the number of placebo-treated firms approximately matches the count of actual first-coverage events in our primary sample. We repeat the procedure 10,000 times, re-estimate the DiD regressions from Equations 1 and 2, and record the share of statistically

significant coefficients.

Table 11: Placebo Test Results

	All	10% Level		5% Level		1% Level	
	Pos.	Sign.	Pos.	Sign.	Pos.	Sign.	Pos.
Spread	53.35%	9.00%	58.56%	4.29%	60.61%	0.82%	63.41%
Inst. Own.	54.55%	9.98%	60.62%	5.26%	59.89%	1.18%	65.25%

Notes: This table reports the results of a placebo test, where we rerun the estimation of Equations 1 and 2 10,000 times, using uniformly distributed placebo treatments. The first column (All) reports the share of positive DiD coefficients across all placebo iterations. We consider three significance levels (10 %, 5%, and 1%). For each level, we report the number of statistically significant coefficients (Sign.) along with the proportion of positive coefficients (Pos.) within the group of significant coefficients at the respective level. The first row (*Spread*) reports results for the regression specification from Column (6) of Table 3, while the second row (Inst. Own.) reports results for the specification from Column (3) of Table 5.

Table 11 reports the results. At the 10%, 5%, and 1% significance levels, the share of statistically significant placebo coefficients is very close to the proportion expected under the null hypothesis of no treatment effect. For example, under the specification from Column (6) of Table 3, 9.00% of placebo iterations yield a coefficient significant at the 10% level; under Column (3) of Table 5, the figure is 9.98%. The rejection rates at the 5% and 1% levels are similarly close to their nominal thresholds, and we obtain comparable results for the remaining specifications from Tables 3 and 5 involving time and firm fixed effects. These null results confirm that the liquidity and ownership effects documented in the main analysis are specifically associated with Trucost coverage events rather than with contemporaneous confounding information shocks.

7 Conclusion

This paper provides causal evidence that the introduction of standardized carbon emissions information improves equity-market liquidity and reshapes the ownership composition of newly covered firms. Using the staggered rollout of Trucost coverage from 2010 to 2023, we show that first-time emissions information leads to a 2% decline in bid-ask spreads, a 4% reduction in Amihud illiquidity, and a 4% increase in trading volume and turnover. Institutional ownership rises by approximately 0.3 percentage points, corresponding to an aggregate reallocation of roughly $30 billion toward newly covered firms. The liquidity improvements are partially explained by these ownership shifts: a one-percentage-point increase in institutional holdings is associated with an additional 0.2% reduction in spreads. Effects are strongest for high-emission firms and late disclosers—precisely the cases where ex-ante uncertainty is greatest—indicating that the market-quality benefits of disclosure are largest where opacity is highest.

Our findings speak directly to the ongoing debate over mandatory climate disclosure. The EU's Corporate Sustainability Reporting Directive, the ISSB standards, and the SEC's climate disclosure rules all rest on the premise that standardized environmental information benefits capital markets. We provide evidence for a stronger claim: even in the absence of mandatory firm reporting, the mere availability of comparable third-party-estimated emissions data attracts institutional capital and improves trading conditions. Mandates may not be necessary to realize the market-quality benefits of standardization; standardized coverage may suffice. Importantly, these effects are not confined to climate-salient sectors or regions with strong climate beliefs; they hold broadly, suggesting that the informational content of emissions data, rather than investor climate preferences per se, drives the market

response. For firms, the increasing environmental transparency that we document improves liquidity, even when the information is unfavorable, and previous work has shown that this can lead to lower cost of equity.

Several limitations warrant mention. First, our estimates capture short- to medium-term market reactions around information-introduction events. As disclosure becomes universal under CSRD and similar regimes, our estimates may understate the long-run benefits to the extent that ongoing improvements in data availability continue to attract institutional capital; alternatively, marginal transparency effects may diminish as coverage saturates. Distinguishing these possibilities is an important direction for future work. Second, although the predominance of third-party estimation in our sample substantially reduces selection concerns, we cannot entirely rule out that Trucost's coverage decisions correlate with unobserved firm characteristics. Third, our analysis focuses on U.S. and Canadian equities, whose institutional structures and disclosure norms may differ from those in other markets.

These limitations point to several avenues for future research. Linking the liquidity gains documented here to firms' cost of equity capital would quantify the real-economy benefits of standardized environmental disclosure. Examining how the quality and assurance of emissions data affect market responses would help distinguish the value of information availability from information credibility. Analyzing how emissions information interacts with other channels of information production—analyst coverage, short-selling activity, and derivative markets—could illuminate whether standardized data complement or substitute for existing information technologies. Finally, investigating whether similar mechanisms operate for social and governance disclosures would clarify whether carbon data are uniquely informative or whether the information-to-ownership-to-liquidity pathway we

identify extends to the broader ESG agenda.

In sum, we find that non-financial disclosure, when standardized and comparable, is financially consequential. Carbon emissions information operates through similar channels as traditional financial disclosure: it reduces information asymmetry, attracts institutional capital, and enhances market efficiency. These results suggest that capital markets internalize the informational content of environmental data even before explicit carbon pricing or mandatory reporting takes effect. For policymakers weighing the costs and benefits of climate disclosure mandates, our evidence indicates that the market-quality benefits are real, economically meaningful, and extend beyond firms and sectors where climate risk is most salient.

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Appendix

A Additional Tables

Table A1: Variable Descriptions

Variable	Description	Data Source
Spread	Logarithmic relative bid-ask spread in bp	CRSP, calculated
Log Amihud Illiq.	Logarithmic Amihud illiquidity measure	CRSP, calculated
Log Volume	Logarithmic trading volume in dollars	CRSP, calculated
Log Turnover	Logarithmic turnover	CRSP, calculated
Market Cap. in bn	Market capitalization in billion dollars	CRSP
Log Market Cap.	Logarithmic market capitalization in dollars	CRSP, calculated
Log PPE	Logarithmic property, plant, and equipment	Compustat
Leverage	Debt divided by stockholders' equity	Compustat, calculated
B/M	Book-to-market ratio	Compustat, calculated
Return on Equity	Net income divided by market capitalization	Compustat, calculated
Price	Stock price in dollars	CRSP
Inst. Own. in %	Share of institutional ownership in %	LSEG Data
Scope 1 Emissions	Scope 1 Emissions in tonnes CO_2e	Trucost
Scope 2 Emissions	Scope 2 Emissions in tonnes CO_2e	Trucost
Log Intensity	Logarithmic Scope 1 Emissions intensity	Trucost
Sector	Sector classification	Compustat
Country	Country based on reporting currency	Compustat

Notes: This table reports the descriptions of all variables and their respective data source.

Table A2: DiD Analysis on the Share of Ownership by Investor Types around Emissions Information Introduction

	(1) Investment Advisor	(2) Bank and Trust	(3) Hedge Fund	(4) Private Equity	(5) Individual Investor
Post	−0.03	−0.01**	−0.05	0.00	−0.02
	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)
Treated × *Post*	0.15**	0.04**	0.03	−0.21***	−0.11***
	(0.07)	(0.02)	(0.11)	(0.06)	(0.04)
Log Market Cap.	1.01***	−0.10**	0.75***	0.19	0.03
	(0.16)	(0.04)	(0.23)	(0.25)	(0.13)
B/M	0.10	−0.07*	−0.14	0.12	−0.09
	(0.12)	(0.04)	(0.20)	(0.13)	(0.09)
Return on Equity	0.15	−0.01	0.15	−0.10	0.06
	(0.15)	(0.03)	(0.13)	(0.11)	(0.05)
Constant	2.48	4.61***	13.36***	0.57	7.04**
	(3.45)	(0.93)	(4.85)	(5.16)	(2.74)
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	383693	383693	383693	383693	338641
r2	0.99	0.97	0.98	0.98	0.98

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results of a fixed effects regression of the share of ownership by investor types on several firm characteristics around first-time Trucost emissions coverage. Ownership data are retrieved from LSEG Workspace. For simplicity, Investment Advisor also includes Mutual Funds, Pension Funds, and Insurance Companies, Bank and Trust also contains Research Firms, Hedge Fund contains the category Investment Advisor/Hedge Fund, and Private Equity includes Holding Companies and Venture Capital. *Treated* is 1 for treated firms and 0 for control firms. *Post* is 1 after treatment (for control firms: after treatment of the respective treated firm) and 0 before treatment. Each observation corresponds to five consecutive firm-trading days. The period we consider spans from eleven bins before to eleven bins after the event, where each bin accordingly comprises five trading days. Standard errors are clustered at the firm and time level.

Table A3: DiD analysis on the Bid-Ask Spread around Emissions Information Introduction, Dependent on Change in Institutional Ownership

	(1) *Spread*	(2) *Spread*	(3) *Spread*
Post	−0.00	−0.00	−0.00
	(0.00)	(0.00)	(0.00)
Treated × *Post* × Δ *Inst. Own. above median*	−0.04***	−0.02**	−0.02**
	(0.01)	(0.01)	(0.01)
Treated × *Post*	−0.00	−0.01	−0.01
	(0.01)	(0.01)	(0.01)
Post × Δ *Inst. Own. above median*	0.01*	0.01	0.01
	(0.00)	(0.00)	(0.00)
Log Market Cap.		−0.45***	−0.45***
		(0.03)	(0.03)
B/M			0.04**
			(0.02)
Return on Equity			0.04*
			(0.02)
Constant	2.70***	11.97***	12.10***
	(0.00)	(0.57)	(0.61)
Time fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Observations	386898	386898	380473
r2	0.96	0.96	0.96

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results of a fixed effects regression of the logarithmic relative bid-ask spread (*Spread*) on several firm characteristics around first-time Trucost emissions coverage. *Treated* is 1 for treated firms and 0 for control firms. *Post* is 1 after treatment (for control firms: after treatment of the respective treated firm) and 0 before treatment. Δ *Inst. Own. above median* is 1 if the increase in institutional ownership from 3 months before the event to 3 months after the event is greater than the median increase of all treated firms. Each observation corresponds to five consecutive firm-trading days. The period we consider spans from eleven bins before to eleven bins after the event, where each bin accordingly comprises five trading days. Standard errors are clustered at the firm and time level.

Table A4: Summary Statistics

	Disclosure Timing			
	Early		*Late*	
	Mean	SD	Mean	SD
Spread	2.27	(1.15)	2.98	(1.17)
Log Amihud Illiq.	−20.02	(1.87)	−19.08	(2.08)
Log Trading Vol.	15.79	(1.71)	15.31	(2.00)
Log Turnover	−5.20	(0.91)	−5.30	(1.15)
Market Cap. in bn	3.44	(9.37)	2.90	(8.39)
Log Market Cap.	20.98	(1.31)	20.61	(1.40)
Log PPE	5.45	(2.11)	4.89	(2.09)
Leverage	0.93	(2.44)	0.89	(2.77)
B/M	0.50	(0.44)	0.53	(0.59)
Return on Equity	0.00	(0.15)	−0.06	(0.46)
Price	36.40	(35.90)	25.96	(32.84)
Inst. Own. in %	76.51	(24.72)	61.74	(29.11)
Scope 1 Emissions	293,516.92	(2.58e+06)	239,531.65	(2.46e+06)
Scope 2 Emissions	77,707.94	(687,111.88)	69,738.35	(672,264.82)

Notes: This table reports the mean and standard deviation for all financial and emissions variables in the data sample around the treatment date (or for control firms, around the treatment date of the respective treated firm), differentiating between early and late covered firms. Each observation is obtained by aggregating five consecutive trading days. The statistics are calculated using the observation containing the treatment date.

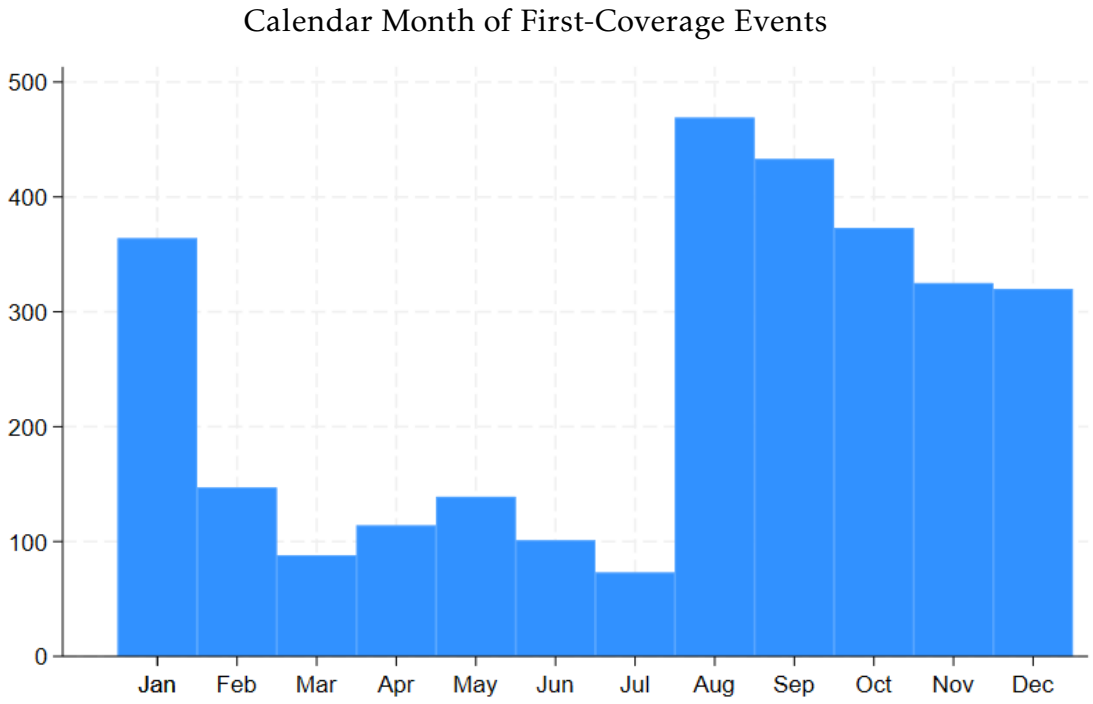


Figure A1: This figure shows the distribution of first-time Trucost coverage events throughout the year within the final sample.

Table A5: DiD Analysis on the Bid-Ask Spread around Emissions Information Introduction, Dependent on State, Industry, and Number of Analysts

	(1) Yale Low	(2) Yale High	(3) No ETS	(4) ETS	(5) Not Material	(6) Other	(7) Analysts Low	(8) Analysts High
Post	0.00	−0.00	0.00	−0.00	−0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treated × *Post*	−0.02***	−0.02**	−0.02**	−0.02**	−0.02**	−0.02**	−0.02**	−0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log Market Cap.	−0.47***	−0.46***	−0.49***	−0.44***	−0.45***	−0.46***	−0.44***	−0.49***
	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
B/M	0.04**	0.06**	0.05**	0.06**	0.04*	0.05**	0.04*	0.06**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Return on Equity	0.02	0.05**	0.02	0.04*	0.05**	0.01	0.04*	0.05*
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
Constant	12.41***	12.17***	12.81***	11.74***	12.17***	12.12***	11.87***	12.59***
	(0.68)	(0.74)	(0.63)	(0.78)	(0.66)	(0.77)	(0.69)	(0.77)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	167355	155229	187248	145040	247440	136253	155229	145040
r2	0.96	0.96	0.96	0.96	0.96	0.96	0.96	0.96

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the results of a fixed effects regression of the logarithmic relative bid-ask spread on several firm characteristics around first-time Trucost emissions coverage. The regression in Column (1) includes only firms with a below-median share of state inhabitants believing that climate change is happening, while Column (2) considers firms from states with an above-median belief in climate change. Column (3) uses firms from US states that did not introduce an ETS by 2023, while Column (4) covers firms from U.S. states that did, as well as Canadian firms. The regression in Column (5) includes only firms from sectors for which greenhouse gas emissions are not material, according to the Sustainability Accounting Standards Board (SASB), and Column (6) covers firms from the remaining sectors. Column (7) includes firms with a below-median number of analysts, and Column (8) includes firms with an above-median number of analysts.

B Stock Market Reaction to Emissions Information Introduction

The main text of the paper documents the liquidity and ownership consequences of first-time Trucost emissions information introduction. In this supplementary analysis, we examine the short-term equity-value implications of the same events. This analysis plays a confirmatory role for the main results: if information introduction resolves genuine information asymmetry about firms' environmental risk exposure, we should observe negative cumulative abnormal returns (CARs) around first-coverage events, as investors revise their priors to incorporate previously-unpriced regulatory and transition risk. This prediction is consistent with prior evidence on corporate social responsibility announcements more generally (Krüger 2015) and, more specifically, with studies of UK firms that document negative market reactions to carbon disclosure announcements, particularly among carbon-intensive firms with substantial institutional ownership (Florackis, Muktadir-Al-Mukit, Sainani, and Zhang 2025).

We compute CARs around first-coverage events using the standard market model

$$r_{i,t} = \alpha_i + \beta_i \times r_{m,t} + \varepsilon_{i,t}, \tag{3}$$

where $r_{i,t}$ denotes the firm return and $r_{m,t}$ denotes the CRSP value-weighted return, which serves as the proxy for the market portfolio. Model parameters $\hat{\alpha}_i$ and $\hat{\beta}_i$ are estimated over the 200-trading-day window from 250 to 50 trading days before each firm's first-coverage event. Abnormal returns (ARs) are defined as the difference

between realized and estimated returns

$$\mathrm{AR}_{i,t} = r_{i,t} - \hat{\alpha}_i - \hat{\beta}_i \times r_{m,t}. \tag{4}$$

We calculate CARs over the event windows $[-1,1]$, $[-5,5]$, $[-10,10]$, and $[-20,20]$ by aggregating daily abnormal returns. The pre-event portion accommodates potential reporting lags in Trucost's dissemination pipeline, while the extended post-event windows capture possibly delayed market reactions. We assess statistical significance using the ADJ-BMP t-test of Kolari and Pynnönen (2010), which accounts for event-induced volatility, autocorrelation, and cross-sectional correlation, and we additionally report the non-parametric generalized rank (GRANK-T) test of Kolari and Pynnönen (2011).

Table B1: Cumulative Abnormal Returns

	Mean	t-Stat	1st Perc.	Med.	99th Perc.	Perc. pos.	GRANK	N
[−1,1]	−0.35***	−3.01	−14.81	−0.26	14.71	45.75	−3.04	3012
[−5,5]	−0.37**	−2.04	−26.51	−0.57	29.38	46.12	−2.34	3012
[−10,10]	−1.11***	−4.15	−38.52	−1.26	40.92	44.02	−4.54	3012
[−20,20]	−2.45***	−5.83	−56.96	−1.78	52.94	44.92	−5.53	3012

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports statistics on cumulative abnormal returns (CARs) in % over different event windows. The column Mean reports the mean CAR, t-Stat the ADJ-BMP t-test statistic introduced in Kolari and Pynnönen (2010). 1st Perc. shows the first percentile, Med. the median, 99th Perc. the 99th percentile, Perc. pos. the percentage of positive CARs, and GRANK the GRANK-T test statistic from Kolari and Pynnönen (2011).

Table B1 reports mean CARs over the four event windows. All four are negative and statistically significant under both the parametric ADJ-BMP and non-parametric GRANK-T tests. The mean CAR is −0.35% over the three-day window $[-1,1]$ and grows monotonically in event length, reaching −2.45% over the 41-day window $[-20,20]$. The negative short-window reaction is consistent with investors

revising valuations downward as they incorporate previously unobservable information about firms' regulatory and transition-risk exposure; the persistence of the effect in longer windows suggests that this revision is not fully impounded on the day of first coverage alone.

We next examine cross-sectional variation in CARs with respect to emission intensity and institutional ownership, following the specification of Florackis et al. (2025). Emission intensity is defined as the first-covered Scope 1 emissions divided by total revenues. Table B2 reports OLS regressions of CARs on the logarithmic emission intensity, institutional ownership, their interaction, and firm-level controls. The interaction term is negative across all event windows and statistically significant at the 5% level for the three-day window $[-1, 1]$, though not for the longer windows. This pattern—stronger negative returns for high emitters with high institutional ownership—parallels our main-text finding in Section 6.2 that the liquidity improvement is concentrated in high-emission firms. Both results are consistent with a single economic mechanism: first coverage resolves greater information asymmetry for firms whose emissions exposure was ex-ante most uncertain, producing both stronger downward valuation responses and larger market-quality improvements. The CAR evidence therefore corroborates the adverse-selection channel underlying the liquidity and ownership results in the main paper.

Table B2: Cumulative Abnormal Returns, Emission Intensity, and Institutional Ownership

Dep. Var. = CAR	(1) [-1,1]	(2) [-5,5]	(3) [-10,10]	(4) [-20,20]	(5) [-1,1]	(6) [-5,5]	(7) [-10,10]	(8) [-20,20]
Log Intensity	21.28	11.24	−10.45	0.40	18.78	9.49	−6.71	3.15
	(1.64)	(0.45)	(−0.30)	(0.01)	(1.43)	(0.37)	(−0.19)	(0.06)
Inst. Own. in %	0.57	0.54	−0.06	2.39	0.56	0.88	0.56	2.61
	(1.00)	(0.49)	(−0.04)	(1.06)	(0.94)	(0.77)	(0.34)	(1.11)
Log Intensity × Inst. Own. in %	−0.40**	−0.24	−0.08	−0.29	−0.38**	−0.23	−0.13	−0.32
	(−2.14)	(−0.68)	(−0.16)	(−0.40)	(−2.00)	(−0.63)	(−0.26)	(−0.44)
Log Market Cap.					−5.13	−8.94	−5.79	23.36
					(−0.69)	(−0.62)	(−0.29)	(0.79)
Leverage					−2.19	−0.02	0.93	12.30
					(−0.60)	(−0.00)	(0.09)	(0.85)
B/M					−16.26	77.16**	147.84***	209.85***
					(−0.88)	(2.16)	(2.95)	(2.86)
Constant	−58.56	−60.25	−70.83	−368.33**	61.18	62.72	−75.12	−997.94
	(−1.48)	(−0.79)	(−0.66)	(−2.35)	(0.40)	(0.21)	(−0.18)	(−1.64)
Observations	2979	2979	2979	2979	2959	2959	2959	2959
r2	0.002	0.000	0.000	0.001	0.003	0.002	0.004	0.004

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the results of a regression of CARs (expressed in basis points) on the logarithmic emission intensity (Log intensity), where the emission intensity is defined as the first disclosed Scope 1 emissions divided by revenues. Further independent variables are institutional ownership, the interaction of emission intensity and institutional ownership, and control variables.