Disclosure and Liquidity: The Ownership Channel

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Abstract

We examine the impact of corporate carbon disclosure on equity-market liquidity and ownership. Using Trucost data from 2010–2023 and a staggered difference-in-differences design, we find that first-time carbon emission disclosures narrow bid–ask spreads by 3%, reduce Amihud illiquidity by 4%, and increase trading volume and turnover by 4%. We find evidence of a selection effect by institutional investors, as institutional ownership increases by approximately $45 billion following disclosure events, corresponding to about 0.4% of the total market capitalization of newly disclosing firms. Liquidity gains are most substantial for high-emission and late-reporting firms. Our results indicate that environmental transparency, though non-financial in nature, is financially material: it attracts institutional capital, alleviates information asymmetry, and enhances market efficiency.

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1 Introduction

Information is a cornerstone of market efficiency. By reducing information asymmetry between informed and uninformed investors, transparency improves price discovery and market liquidity. In the canonical framework of Diamond and Verrecchia (1991a), the release of value-relevant information narrows the dispersion of beliefs, reduces market makers' adverse-selection risk, and tightens bid–ask spreads. A large empirical literature confirms that improvements in firms' information environments are associated with lower trading costs and a broader investor base, often through changes in institutional participation and information production (e.g., Bushee and Noe 2000; Balakrishnan, Billings, Kelly, and Ljungqvist 2014; Boone and White 2007; Bird and Karolyi 2016). Because liquidity affects firms' cost of capital and access to financing, disclosure policy plays a central role in the functioning of financial markets.

The informational role of non-financial transparency remains less well understood, even as it has become increasingly important for investors and regulators. Environmental information is not reported on financial statements, yet it can be financially relevant because it informs assessments of regulatory exposure, operating efficiency, and long-run climate risk. Corporate greenhouse gas (GHG) emissions are a leading example. For many years, emissions data were neither consistently observable nor comparable across firms, allowing issuers to withhold or delay reporting with limited immediate capital-market consequences. As policy initiatives such as the EU's Corporate Sustainability Reporting Directive (CSRD), the SEC's climate disclosure proposal, and the Task Force on Climate-related Financial Disclosures (TCFD) push toward standardized reporting, it becomes important to understand whether, when, and through which channels emissions information

affects trading costs, investor participation, and valuation. Prior work shows that mandatory ESG disclosure can improve the information environment (Krueger, Sautner, Tang, and Zhong 2024) and that institutional investors can influence firms' climate risk disclosure (Ilhan, Krueger, Sautner, and Starks 2023). We complement this evidence by identifying the market mechanism: we show how the introduction of standardized emissions information induces institutional capital reallocation and how these ownership shifts partially explain improved equity-market liquidity.

This paper studies how the first market-wide availability of standardized firm-level emissions information affects equity-market liquidity and ownership. Our empirical setting leverages data from Trucost for the period 2010–2023, one of the most widely used providers of corporate emissions information. Trucost combines firm-reported Scope 1 and Scope 2 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 that was previously not covered.

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 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 (**?**). 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 consistent 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.

Our analysis yields four main findings. First, the introduction of emissions information leads to economically meaningful improvements in equity-market liquidity. Bid–ask spreads decline by approximately 3% 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 and are not preceded by differential pre-trends, supporting a causal interpretation.

Second, institutional ownership increases significantly following the availability of emissions information. In our difference-in-differences specification, the share of institutional holdings rises by approximately 0.3 percentage points relative to matched non-disclosing firms. This effect is economically meaningful: for the median treated firm, it corresponds to roughly $15 million in incremental institutional investment, and in aggregate to approximately $45 billion reallocated toward newly

covered firms during our sample period. The effect represents about 0.4% of the total market capitalization of disclosing firms. Disaggregating by investor type, we find that the ownership increase is concentrated among investment advisors and banks—long-horizon investors with fiduciary obligations and ESG integration mandates. Hedge fund holdings show no significant response, while private equity and individual retail holdings decline. These patterns are consistent with evidence that institutional investors systematically incorporate sustainability-related information into portfolio allocation decisions (Krueger, Sautner, and Starks 2020; Pástor, Stambaugh, and Taylor 2021).

Third, the liquidity improvements are partially explained by these ownership shifts. When we interact the treatment indicator with the change in institutional ownership around the disclosure event, the coefficient on the triple interaction term is approximately −0.2. Because the ownership-change variable is scaled in units of $\frac{1}{100}$ per percentage point, this estimate implies that a one-percentage-point increase in institutional ownership is associated with an additional 0.002 decline in the log relative bid–ask spread, or roughly 0.2% in levels. Institutional investors therefore not only respond to emissions transparency but also amplify its effects on market quality. This mechanism is consistent with evidence that institutional ownership enhances liquidity and reduces adverse-selection costs (Rubin 2007; Kini and Mian 1995; Bushee and Noe 2000; Boone and White 2007; Bird and Karolyi 2016; Ilhan et al. 2023).

Fourth, the effects exhibit economically intuitive heterogeneity. Liquidity improvements are strongest for firms with above-median emissions and for late disclosers within their industry, with interaction coefficients of approximately −0.03 in both cases—roughly doubling the baseline effect. These are precisely the settings in which ex ante uncertainty about environmental performance is greatest. Firms with

high emissions face elevated uncertainty about regulatory exposure and compliance costs, while late disclosers are more difficult for investors to benchmark against industry peers. Revealing emissions information in these cases therefore resolves larger informational frictions. We also find that firms whose disclosed emissions deviate most from sector medians—a proxy for estimation difficulty—experience the strongest liquidity responses, with significant effects concentrated in the upper three quartiles of estimation error. Consistent with the view that disclosure resolves uncertainty, we observe negative cumulative abnormal returns around information-introduction events, with three-day CARs of approximately −0.35% and 41-day CARs reaching −2.45%, suggesting that markets, on average, revise valuations downward as emissions information is incorporated.

Taken together, the evidence indicates that disclosure is most valuable where opacity is greatest. Transparency yields larger market-quality improvements when uncertainty about firm fundamentals is high or when information has been delayed. At the same time, the pronounced effects for high-emitting firms suggest that institutional capital reallocation is driven primarily by the informational content of emissions data rather than by investor climate preferences per se. Consistent with this interpretation, our results are robust across U.S. states and sectors, including states with below-median climate change beliefs (as measured by the Yale Climate Opinion Survey), states without emissions trading systems, and sectors in which greenhouse gas emissions are not classified as financially material by SASB standards.

Our estimates capture short- to medium-term market reactions around first-time information-introduction events. The persistence of these liquidity gains remains an open question as emissions disclosure becomes more widespread and standardized. Nonetheless, the results demonstrate that even non-financial information,

when standardized and comparable, is incorporated by capital markets through conventional information channels.

In sum, we provide causal evidence that standardized carbon emissions information improves equity-market liquidity and broadens the investor base. Non-financial disclosure, 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 streams of literature, i.e., research on (1) the impact of firm disclosure on financials, (2) the importance of environmental firm characteristics on investor decision-making, and (3) the relationship between disclosure and institutional investors.

An extensive number of studies has investigated the impact of firm disclosures on a number of important financial variables, including equity-market liquidity. From a theoretical perspective, firms' disclosures can reduce information asymmetry between managers or insiders and intermediaries, leading to increased equity-market liquidity (Diamond and Verrecchia (1991b)). According to Easley and O'Hara (2004), increased disclosure reduces information gathering costs and, as a result, monitoring costs. Firms can causally impact the equity-market liquidity of their equity via voluntary disclosure (Balakrishnan et al. (2014)). Studying the impact of carbon disclosure on stock returns, Bolton and Kacperczyk (2021b) finds that voluntary disclosure leads to lower stock returns for disclosing firms relative to non-disclosing firms. Firms that disclose their carbon emissions tend to be larger (about a $2.3 billion higher median value) than comparable non-disclosing firms

(Matsumura, Prakash, and Vera-Munoz (2014)). In the context of non-financial disclosure, Krueger et al. (2024) provide evidence that mandatory ESG disclosure enhances firm-level equity-market liquidity. Our paper introduces first-time carbon emission disclosure as voluntary non-financial disclosure events and studies their impact on market efficiency.

Literature on how financial market participants incorporate information about the environmental performance of firms has substantially increased in quantity and quality in the past decade. Bolton and Kacperczyk (2021a) find that U.S. firms with higher CO_2 emissions earn higher returns. Moreover, higher carbon emissions increase downside risk (Ilhan, Sautner, and Vilkov (2021)). High-polluting firms also exhibit higher returns (Hsu, Li, and Tsou (2021)) and environmental uncertainty is reflected in the bid–ask spread of firms (Rehse, Riordan, Rottke, and Zietz (2019)). However, disclosing climate change-related physical risk leads to lower information asymmetry (Schiemann and Sakhel (2019)). Fan, Tang, and Pan (2021) discuss the information asymmetry decreasing impact of assurance in carbon disclosure. Our paper shifts the focus from investors' perceptions of carbon emissions to the role of carbon emissions and their disclosure in shaping how intermediaries determine buy and sell prices for firms' equity.

Research on institutional ownership and disclosure highlights 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). Specifically, Ilhan et al. (2023) provide causal evidence for this push effect (also considered to be an engagement effect) by examining a 2015 French regulatory shift that required institutional investors to disclose their own climate risk exposure. However, they do not rule out the possibility that a "selection effect" may be

occurring simultaneously. Our paper contributes to this discourse by specifically examining the selection effects that arise when institutional investors rebalance their portfolios after disclosure events.

3 Empirical Design

3.1 Trucost Carbon Disclosure Dates

Our primary variable is obtained from Trucost. The Trucost dataset includes firm-level carbon emissions data as well as the corresponding disclosure dates. For each firm, we take the earliest disclosure date of carbon emissions as the point in time when emission data was first revealed to the market (i.e., the first disclosure). The disclosed carbon emissions in the Trucost dataset are either firm-reported or estimated data. We do not distinguish between firm-reported and estimated emissions in this paper, although about 95% of first disclosures of carbon emission in our sample are based on estimates.

Because Trucost retrospectively revised all pre-2008 data in May 2009 (Zhang (2025)), we restrict the observation period of our study to disclosure events between 2010 and 2023 to ensure accurate timing. During this period, we observe heterogeneity in the timing of the first-time disclosure of carbon emissions for the North American publicly listed firms. The upper graph of Figure 1 shows the cumulative number of first-time carbon emission disclosures by industry from 2010 to 2023 for these firms. Disclosure activity accelerates after 2015, consistent with the growing prominence of climate-related regulation and investor demand for transparency following the Paris Agreement. The strongest increases in the number of disclosing firms within one industry occur in the financial sector, the health care

sector, and the information technology sector. Carbon-intensive sectors such as Energy and Materials expand at a slower pace. This variation across industries and over time provides meaningful heterogeneity in disclosure timing that underlies our identification design. The lower graph of Figure 1 shows the share of firms in our sample with disclosed emissions by industry. The development of the shares of disclosing firms shows a similar pattern as the absolute numbers. While most industries experience a substantial increase in the share of disclosing firms after 2015 (from about 20% before 2015 to 40%-80% after 2015), more than 40% of the firms in the utilities sector had already disclosed their carbon emissions before 2010.

[Figure 1 about here.]

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.[1]

We measure equity-market liquidity by (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

[1] Table 12 provides descriptions of the variables used in our analyses.

in dollars over a 30-day rolling window, (3) the logarithm of trading volume in dollars, and (4) the logarithm of turnover measured as shares traded relative to shares outstanding. The combination of these measures captures both the cost and intensity of trading, allowing us to evaluate whether emission disclosures mitigate information asymmetry and enhance equity-market liquidity.

We conduct all analyses based on a lower-frequency dataset constructed by aggregating every five consecutive trading days into a single observation, thereby effectively generating a weekly dataset following the approach of Bessembinder (2003), which studies the impact of decimalization on bid–ask spreads based on weekly frequency. We refer to these periods of five trading days as *bins*. That is, trading days one through five constitute bin one, days six through ten constitute bin two, and so on. All variables are calculated as averages of the daily observations in the respective bin. Consistent with the approach of Pástor and Stambaugh (2003) for the average trading volume, we construct all logarithmic variables (*Spread*, Amihud illiquidity, trading volume, turnover, market capitalization, and PPE) based on the aggregated absolute values. This procedure allows for a more robust estimation of the effect of first-time carbon emission disclosure on equity-market liquidity, as it reduces idiosyncratic noise and mitigates the impact of outliers and day-of-the-week effects (Chordia, Roll, and Subrahmanyam (2001)). Nevertheless, our findings are robust to alternative aggregation intervals, such as periods of three or ten trading days instead of five, and to the use of daily data instead of aggregated data.

3.3 Methodology

We analyze the effect of first-time carbon emission disclosure on equity-market liquidity and the share of institutional ownership using a difference-in-differences (DiD) approach, as the decision to disclose carbon emissions or have them externally estimated by third parties might not be exogenous to firm characteristics. By applying a DiD approach to identify the differential effect on equity-market liquidity, we follow Brogaard, Ringgenberg, and Roesch (2025), who recently examined the effect of the suspension of floor trading on equity-market liquidity.

We define the treatment as the first disclosure date on which Trucost provides publicly available emission disclosures for each respective firm. The timing of each firm's first-time carbon emission disclosure is identified using Trucost's records on the effective disclosure date. We study the treatment effect of the disclosure in an interval spanning 11 bins before to 11 bins after the bin containing the disclosure date (the *treatment bin*) and refer to this interval as the *event window*, which includes the respective pre-treatment and post-treatment periods. The event window roughly corresponds to the period spanning 60 trading days before to 60 trading days after the disclosure date.

We assign up to five non-disclosing control firms 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 disclose emissions before or within two years after the matching takes place. That is, if we match in year y, we exclude all untreated firms that disclose emissions by year $y + 2$ or earlier from the set of potential control firms. Furthermore, we exclude firms with stock prices below one dollar (following Peress and Schmidt 2020) and firms that were delisted during the event window to ensure

a constant sample size over the event window. We use the logarithms of monthly average market capitalization, monthly average relative spread, and monthly average trading volume in dollars as matching variables and apply 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 and the corresponding control firms during the event window of the respective treated firm. We exclude treated firms to which no control firms were matched and obtain, on average, 4.8 control firms for each treated firm. Our results are robust to using a different control sample size, e.g., by matching one or three instead of five control firms to each disclosing firm. We do not remove duplicates, i.e., a firm may appear several times in the final sample. Since the matching might not generate perfect counterfactuals of the treated firms, we use control variables in the DiD models to account for any remaining differences between treated and control firms.

Since disclosure dates differ across firms, we employ a staggered DiD framework to account for variation in treatment timing. We follow Callaway and Sant'Anna (2021) by using their DiD approach implemented in the csdid2 Stata package, which allows us to examine dynamic *average treatment effects on the treated (ATT)* in an event-study framework. For estimating the treatment effect, we use the doubly robust DiD estimator based on stabilized inverse probability weighting and ordinary least squares presented in Sant'Anna and Zhao (2020).

The approach proposed by Callaway and Sant'Anna (2021) addresses problems arising from heterogeneous treatment effects in staggered DiD settings. The applicability of two-way fixed effects (TWFE) DiD regressions in such contexts has recently been questioned by Goodman-Bacon (2021) and Baker, Larcker, and Wang (2022). However, by the construction of the control group, which contains only firms that are not yet treated, these issues are unlikely to compromise the interpretability of

TWFE DiD regressions in our setting. To complement our results obtained from the Callaway and Sant'Anna (2021) method, we conduct several TWFE DiD regressions using equity-market liquidity and institutional ownership as dependent variables.

3.4 Descriptive Statistics

After matching 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 for the period from 2010 to 2023. Table 1 reports the industry and country composition of the sample, differentiating between treated and control firms. Industry classifications follow Compustat's GSECTOR variable, and country identification is based on the reporting currency. The final dataset comprises 16,976 units, of which 2,946 are treated and 14,030 are control firms, corresponding to a total 2,624 unique control firms. The largest sectors are Health Care (20.4%), Industrials (10.4%), and Financials (16.1%), broadly mirroring the composition of the North American equity market. Sectoral and geographic distributions are balanced across the treatment and control groups, indicating that treated firms and control firms are comparable along observable dimensions.

[Table 1 about here.]

Table 2 shows the mean and standard deviation for various equity-market liquidity measures and other relevant financial variables, distinguishing between control firms and treated firms. All continuous dependent and independent variables are winsorized at the 1st and 99th percentiles to mitigate the influence of outliers. However, our results are robust to different levels of winsorization. All statistics are calculated at the time of first-time carbon emission disclosure; that is, we measure

all variables at the treatment bin. Despite some differences, control firms and treated firms are broadly comparable. In the last column, the mean difference between control firms and treated firms is reported, along with the result of a t-test on this difference.

[Table 2 about here.]

4 Carbon Disclosure and Liquidity

Following the staggered DiD framework proposed by Callaway and Sant'Anna (2021), Figure 2 plots the average treatment effects (ATT) on the *Spread*, with each bar representing one bin in our setting. Since we observe no statistically significant pre-treatment differences in *Spread* between the disclosing firms and the control group (see the blue bars in Figure 2), the parallel trends assumption appears to hold.

[Figure 2 about here.]

The post-treatment differential effect on the *Spread* is negative and significant, suggesting that corporate first-time carbon emission disclosure leads to a marked improvement in equity-market liquidity relative to a control group of non-disclosing firms. At the end of the event window, the ATT on the *Spread* is approximately -0.03, corresponding to a reduction in the relative bid—ask spread of about 3%. The effect becomes statistically significant within 15 trading days following the disclosure. Using the relative spread instead of its logarithmic version yields similar results.

Statistical inference is obtained by estimating our baseline DiD model using a

fixed effects panel regression of the following 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* is a carbon reporting dummy variable that takes the value 1 for firms disclosing emissions within the event window and 0 for control firms. The *Post* dummy equals 1 after treatment for the treated firms and the set of control firms, and 0 before treatment. The interaction of *Treated* and *Post* serves as the DiD variable. The coefficient δ_1 on *Treated* × *Post* estimates the differential effect of first-time carbon emission disclosure on equity-market liquidity, compared to the control group. Where appropriate, we include both time and firm-fixed effects. Time-fixed effects γ_t are defined at the bin level. Each time a firm appears in the final dataset comprising treated and control firms, a separate firm-fixed effect α_i is assigned. For instance, if a firm is matched to several disclosing firms or appears first as a control and later as a treated firm, we treat each occurrence as an independent unit. Accordingly, in the specifications using firm fixed effects, the *Treated* dummy vanishes. We use several control variables, including market cap, capital structure, book-to-market ratio, and return on equity ((similar to Brogaard et al. 2025; Zhang 2025)).

[Table 3 about here.]

Table 3 reports the results. The coefficient of −0.02 for the DiD variable *Treated* × *Post* in Column (1) is statistically significant at the 1% significance level, indicating that the *Spread* decreases following first-time carbon emission disclosure, which supports the pattern shown in Figure 2. As the logarithmic relative spread

decreases by −0.02, relative spreads, and therefore also trading costs, decline by approximately 2% following disclosure, compared to the change in the control group over the same period. This result is robust across alternative fixed effects specifications and various sets of control variables (see Columns (2) to (10) in Table 3).

The results for the alternative measures of equity-market liquidity–namely, the Amihud illiquidity ratio, logarithmic trading volume, and logarithmic turnover–are consistent with those found for the *Spread*: Columns (1) and (2) of Table 4 present the results obtained using the Amihud illiquidity measure. The coefficient of the interaction term *Treated* × *Post* is negative and statistically significant at the 1% level, mirroring our previous findings for the *Spread* variable. The coefficient indicates a 0.04 decrease in the logarithmic Amihud illiquidity measure, translating to an approximate 4% reduction in illiquidity following the first-time carbon emission disclosure.

[Table 4 about here.]

Columns (3) through (6) report results using logarithmic dollar trading volume and logarithmic turnover as dependent variables. We find that both measures increase by approximately 0.04 following disclosure, representing a 4% rise in absolute trading activity. Taken together, these results consistently indicate that corporate first-time carbon emission disclosure significantly enhances equity-market liquidity.

5 Carbon Disclosure and Institutional Ownership

Having identified significant improvements in equity-market liquidity following first-time carbon emission disclosure, we next evaluate the relationship between these disclosure events and institutional ownership. Using the same methodology as in our primary analysis, we plot the ATT on institutional ownership using the staggered DiD approach of Callaway and Sant'Anna (2021). Figure 3 confirms the presence of parallel trends during the pre-treatment period, followed by a significant and sustained differential increase in institutional ownership after disclosure.

[Figure 3 about here.]

The ATT reaches 0.4 percentage points by the end of the event window, indicating that institutional ownership in disclosing firms increases by approximately 40 basis points compared to the control group following the disclosure event.

We use the same specification as above (Model 1) to evaluate the significance of the effect:

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

with the share of institutional ownership in a firm as the dependent variable. Table 5 reports the results. The coefficient on the interaction term *Treated* × *Post* suggests that the share of institutional ownership in a firm increases by approximately 0.3 percentage points following the disclosure of carbon emissions, compared to control firms. This result corroborates the visual evidence presented in Figure 3, confirming a positive and statistically robust differential shift in institutional ownership following the initial disclosure of carbon emissions.

[Table 5 about here.]

The effect is both statistically and economically significant. In total, institutional investors allocated an additional $45 billion to treated firms compared to the control group within a six-month window centered on the initial disclosure event. This figure represents the cumulative capital inflow across our sample from 2010 to 2023. On average, this effect translates into an additional $15 million in institutional investment per firm following the first-time carbon emission disclosure. A more conservative estimate of approximately $30 billion is derived by multiplying the total market capitalization of all disclosing firms at the time of disclosure by the *Treated* × *Post* coefficient reported in Table 5.

Table 13 in the appendix provides detailed information on the reaction of different institutional investor types to first-time carbon emission disclosure. We distinguish between investment advisors, bank and trusts, hedge funds, private equity, and individual investors. The overall institutional ownership effect is largely driven by a significant increase in the categories of investment advisors as well as banks and trusts. In contrast, we find no significant change in hedge fund holdings, along with a significant reduction in private equity involvement. Consistent with these shifts and our previous findings, we also document a significant reduction in the share held by individual (retail) investors.

6 Heterogeneity

In this section, we examine whether the impact of first-time carbon emission disclosure on equity-market liquidity varies based on concurrent changes in institutional ownership, the level of emissions disclosed, and the timing of the disclosures. The objective is to identify the mechanisms that drive market sensitivity during a firm's

first-time carbon emission disclosure. To achieve this, we augment our baseline DiD model with interaction terms to test for heterogeneity across these key dimensions.

6.1 Variation across Institutional Ownership

The results in Sections 4 and 5 establish that first-time carbon emission disclosure leads to enhanced equity-market liquidity and increased institutional ownership. We will now examine whether these two phenomena are linked. Prior research indicates that greater institutional ownership is associated with tighter bid–ask spreads (Rubin (2007); Kini and Mian (1995)). We therefore test whether firms that experience larger increases in institutional ownership around the time of disclosure also exhibit larger improvements in equity-market liquidity. In doing so, we assess whether changes in ownership composition help to explain the cross-sectional variation in equity-market liquidity responses to first-time carbon emission disclosure.

Table 6 reports results from a modified version of Model 1, in which we interact the DiD indicator with the change in institutional ownership around the time of disclosure. We denote this change as $\Delta Inst.\ Own.$, defined as the difference in institutional ownership between three months before and three months after the disclosure date for the treated firm. The variable is expressed in units of $\frac{1}{100}$ per percentage point, with the same value assigned to the control firms.

The coefficient on $Treated \times Post \times \Delta Inst.\ Own.$ is approximately -0.2, indicating that firms experiencing a larger increase in institutional ownership also exhibit a greater decline in bid–ask spreads. Quantitatively, a 1 percentage point increase in institutional ownership corresponds to an additional change in *Spread* of $\frac{1}{100} \cdot (-0.2) = -0.002$, thereby confirming that ownership changes help explain cross-sectional variation in equity-market liquidity improvements following disclosure.

[Table 6 about here.]

To ensure this result is not influenced by outliers, we include Table 14 in the appendix, where we define a dummy variable Δ *Inst. Own. above median*, which equals 1 if the change in institutional ownership for a treated firm exceeds the median change in institutional ownership (+0.56 percentage points) observed among all treated firms. Control firms are assigned the same number (0 or 1) as the corresponding treated firm. The results indicate that the effect is primarily driven by firms with a relatively pronounced increase in institutional ownership.

6.2 Variation across Carbon Emissions

Information asymmetry is likely to be more pronounced among firms with higher carbon emissions. Firms with emissions below their industry median are generally expected to face fewer concerns regarding their environmental performance and, therefore, have weaker incentives to withhold information. In contrast, high emitters may be subject to greater scrutiny and exhibit higher opacity before first-time carbon emission disclosure. This intuition aligns with evidence from the broader corporate social responsibility (CSR) literature, which shows that weaker performers tend to be less transparent (Cho, Lee, and Pfeiffer (2013)).

To test this hypothesis in our context, we interact the DiD variable from Model 1 with the dummy variable *High Emissions*, defined as 1 if the Scope 1 carbon emissions revealed in a firm's first disclosure are higher than the median carbon emissions within the same sector, and 0 otherwise. Control firms are assigned the same number (0 or 1) as the corresponding treated firm. Table 7 shows the results of this analysis. While the coefficient on *Treated* $\times$ *Post* remains negative, consistent with the main analysis, the coefficient of -0.03 on *Treated* $\times$ *Post* $\times$ *High Emissions* is

significant at the 1% level. This indicates a particularly significant improvement in equity-market liquidity when a firm discloses its high carbon emission levels or when these levels are estimated.

[Table 7 about here.]

Apart from the level of disclosed emissions, the reaction to first-time carbon emission disclosure may also depend on how market participants estimated emissions prior to disclosure. We use the median sector emissions as a benchmark for estimating firm emissions. By computing the absolute difference between disclosed emissions and median sector emissions, we create a proxy for the estimation error of carbon emissions made by investors. We partition the sample into quartiles, where Q1 contains firms with the smallest estimation errors, and Q4 contains firms with the largest estimation errors. Table 8 reports the results of the TWFE DiD regressions restricted to the quartiles Q1 through Q4. While we do not observe an equity-market liquidity effect for firms with small estimation errors (Q1), the $Treated \times Post$ coefficient for the remaining three quartiles is negative and significant. The results indicate that equity-market liquidity improves more for firms with carbon emissions that are difficult to estimate.

[Table 8 about here.]

6.3 Variation across Carbon Disclosure Timing

While the timing of first-time carbon emission disclosure 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 that disclose information early in their industry may face greater information

asymmetry than firms that disclose information later because market participants may not have been able to build industry-level expectations. Market participants may form expectations about the emissions of late-disclosing firms once some firms have already disclosed information, allowing for the development of emission estimation techniques. On the other hand, early-disclosing firms may have better-than-expected carbon emissions or emissions that are relatively easy to predict and, therefore, prefer to report them sooner. This may imply that the remaining non-disclosing firms have more uncertain emissions profiles. As a result, market participants may revise their beliefs and infer that overall emissions are lower than initially expected, as early disclosers reported relatively low emission levels.[2] Krueger et al. (2024) find that, in the context of ESG disclosure mandates, voluntary late adopters experience greater improvements in equity-market liquidity than early adopters.

To investigate whether potential variations in the effects of treatment over time are driven by differences in first-time carbon emission disclosure timing, we compare the average levels of early and late disclosure. Table 9 contains sample statistics for *early* and *late disclosers*, where early disclosers are defined as the first 50% firms to disclose within an industry, and late disclosers are the last 50%. The majority of first emissions data available for a firm comes from third-party estimations for both early and late disclosers. While the share of firms with low emissions among late disclosers is slightly higher than that among early disclosers, there is no observable pattern suggesting that early disclosers, on average, have higher or lower emissions than late disclosers. This implies that the analysis in Section 6.2 does not capture timing heterogeneity due to differences between high-

[2] In a similar manner, but with a different objective, Yan, Li, Huang, and Li (2020) show that also the consistency of carbon performance and carbon information disclosure has a significant impact on enterprise value.

and low-emitting firms. Moreover, descriptive statistics (see Table 15) show that both groups are fairly similar, although early disclosers are slightly larger ($3.44bn vs. $2.90bn), have a higher share of institutional ownership (76.51% vs. 61.74%), and exhibit higher equity-market liquidity (e.g., the average Spreads are 2.27 vs. 2.98). The median date of the first-time carbon emission disclosure date for early disclosers is September 2017, while the median date for late disclosing firms is August 2020.

[Table 9 about here.]

We estimate timing effects using Model 1, to which we add the interaction term between the DiD variable and the dummy variable *Late*, defined as 0 for early disclosers and 1 for late disclosers. Control firms are assigned the same value for the dummy as the corresponding treated firm. Table 10 reports the results. The coefficients of −0.03 for *Treated* × *Post* × *Late* in Columns (2) and (3) are negative and statistically significant at the 1% level. They suggest that the reduction in Spread is greater for late disclosing firms than for early disclosing firms. Thus, the improvement in equity-market liquidity compared to the control firms occurs primarily for late disclosers. The results of DiD regressions restricted to early or late disclosers and their respective control firms in Columns (4) and (5) of Table 10 confirm this finding, as the coefficient of *Treated* × *Post* in the regressions for the subset of late disclosers is negative and statistically significant on the 1% level. This suggests that the emissions of late-disclosing firms are more difficult to predict than those of early disclosers. The results indicate that firms that have not yet reported can still enhance their equity-market liquidity by disclosing emissions, as the observed effects are not confined to early disclosers.

[Table 10 about here.]

7 Robustness Tests

We continue by performing a series of robustness checks to ensure the validity of our findings. One potential concern is that sector-specific changes in equity-market liquidity may influence our main results since sector affiliation is not explicitly accounted for in the matching procedure. As part of our robustness checks, we include sector affiliation among the matching variables and find results consistent with our previous findings. Our results also remain robust to the inclusion of Industry × Year fixed effects in the TWFE DiD regressions. The findings also hold when we include additional control variables, such as capital expenditure intensity as an alternative measure of asset tangibility.

Another potential concern is that first-time carbon emission disclosure may be temporally correlated with financial disclosures. We are confident that this does not pose an actual challenge to our identification strategy since carbon emissions reporting exhibits dispersed timing throughout the year (see Figure 4 in the appendix). Moreover, we excluded firms that disclose during the reporting season (January, April, July, and October) and find further support for the validity of our findings.

Next, we evaluate whether the equity-market liquidity improvement is limited to certain geographic regions or sectors. We first investigate whether there is geographical variation in the equity-market liquidity improvement by considering local beliefs regarding climate change. Therefore, we use U.S. state level panel data on opinions regarding climate change from Marlon, Wang, Bergquist, Howe, Leiserowitz, Maibach, Mildenberger, and Rosenthal (2022). The relevant variable for defining *Yale Low* is the estimated percentage of inhabitants who trust that global warming is happening. Column (1) of Table 16 in the appendix shows

the results of a DiD regression restricted to firms with a below-median share of state inhabitants who believe that climate change is occuring *(Yale Low)*. Column (2) reports the results for firms with headquarters in states with above-median beliefs in climate change *(Yale High)*. As evident, our results hold for both groups, indicating that the equity-market liquidity improvement is not limited to states with strong beliefs about climate change. Columns (3) and (4) differentiate between firms from states that did not introduce an Emissions Trading System (ETS) by 2023 and firms from states that did. We obtain this information from the World Bank's Carbon Pricing Dashboard. This further supports our previous finding by demonstrating that the results are not exclusively driven by states that introduced an Emissions Trading System.

Additionally, we want to rule out that the effect is driven by firms for which carbon emissions are considered to be financially material. Therefore, we classified the treated firms in our sample with respect to the materiality map according to the Sustainability Accounting Standards Board (SASB). Column (5) indicates a significant treatment effect even for firms in sectors where greenhouse gas emissions are not considered financially material. The effect is of the same magnitude as that for firms in the remaining sectors, as shown in Column (6).

Furthermore, to ensure that our results are not influenced by other recurring information-providing events, we conduct placebo tests using randomized treatment dates. Specifically, for firms that did not disclose prior to 2010, we assign pseudo-treatment dates that are uniformly distributed between 2010 and 2024. We calibrate this distribution so that the number of placebo-treated firms approximately matches the count of actual disclosing firms in our primary sample. We conduct this procedure 10,000 times, using the generated placebo treatments to re-estimate potential effects on our two variables of interest (*Spread* and *Inst. Own*)

using Equations 1 and 2, and report the share of statistically significant DiD coefficients.

[Table 11 about here.]

Table 11 shows that, at the 10%, 5%, and 1% significance levels, the share of statistically significant coefficients is very close to the proportion expected under the null hypothesis of no treatment effects. For instance, the share of coefficients that are significant at the 10% level using the same specification as Column (6) of Table 3 is 9.00%, while for the specification from Column (3) of Table 5 it is 9.98%. Under the null hypothesis, we would expect 10% of the iterations to return a statistically significant coefficient. Our results for the significance levels at 5% and 1% align with this finding. Furthermore, we obtain similar results for the remaining specifications from Tables 3 and 5 involving time and firm fixed effects. These null results provide further support for the validity of our empirical design and confirm that the observed equity-market liquidity effects are specifically associated with the disclosure events rather than with confounding information shocks.

8 Conclusion

This paper provides causal evidence that the introduction of standardized carbon emissions information improves equity-market liquidity and increases institutional ownership. Using the staggered rollout of Trucost coverage from 2010 to 2023, we show that first-time emissions information leads to a 3% 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 $45 billion toward newly cov-

ered firms. The liquidity improvements are partially explained by these ownership shifts: each 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 disclosure is most valuable where opacity is highest.

Our findings have implications for the ongoing debate over mandatory climate disclosure. Regulations such as the EU's Corporate Sustainability Reporting Directive, the ISSB standards, and the SEC's climate disclosure rules rest on the premise that standardized environmental information benefits capital markets. We provide direct evidence for this premise. Even in the absence of mandatory firm reporting, the availability of comparable emissions data attracts institutional capital and reduces trading costs. 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 results imply that environmental transparency can lower the cost of equity by improving liquidity and broadening the shareholder base, even when the disclosed information is unfavorable.

Several limitations warrant mention. First, our estimates capture short- to medium-term market reactions around information-introduction events. As emissions disclosure becomes universal and standardized, the incremental value of additional transparency may diminish, and we cannot speak to long-run effects. Second, although the predominance of third-party estimation in our sample 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; the effects may differ in markets with different institutional structures, disclosure norms, or climate policy environments.

These limitations point to directions for future research. Studying the persistence of liquidity gains as disclosure becomes more widespread would illuminate whether early-mover advantages exist or whether benefits accrue primarily during the transition to full coverage. Examining how the quality and assurance of emissions data affect market responses would help distinguish the value of information availability from information credibility. Extending the analysis to international markets, particularly those subject to mandatory disclosure regimes, would permit comparisons of voluntary and regulatory channels. Finally, investigating how emissions information interacts with other ESG dimensions could clarify whether carbon data are uniquely informative or whether similar mechanisms operate for social and governance disclosures.

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 informed 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

[Table 12 about here.]

[Table 13 about here.]

[Table 14 about here.]

[Table 15 about here.]

[Figure 4 about here.]

[Table 16 about here.]

A Stock Market Reaction to Carbon Disclosure

As demonstrated in the previous sections, both equity-market liquidity and institutional ownership increase following first-time carbon emission disclosure. In this section, we examine the firm-value implications. Krüger (2015) finds a negative market reaction to CSR firm events, regardless of positive and negative news. Studies of UK firms have documented negative reactions to carbon disclosure announcements, particularly among firms from carbon-intensive industries and firms with substantial institutional ownership (Florackis, Muktadir-Al-Mukit, Sainani, and Zhang (2025)).

We aim to capture the stock price effects by assessing cumulative abnormal returns (CARs) around first-time carbon emission disclosures. To estimate expected stock returns, we employ the market model

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

where $r_{i,t}$ represents the firm return and $r_{m,t}$ denotes the CRSP value-weighted return, serving as the proxy for the market portfolio. We estimate the model parameters $\hat{\alpha}_i$ and $\hat{\beta}_i$ using data from 250 trading days to 50 trading days prior to each respective disclosure event. Abnormal returns (ARs) are defined as the difference between realized stock returns 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 for the event windows $[-1,1]$, $[-5,5]$, $[-10,10]$, and $[-20,20]$ by aggregating daily abnormal returns. The inclusion of a pre-event period accounts for potential reporting lags by Trucost, while the extended post-event windows capture possibly delayed market reactions. We assess significance using the ADJ-BMP t-test statistic from Kolari and Pynnönen (2010), which accounts for event-induced volatility, autocorrelation, and cross-sectional correlation. Additionally, we employ the non-parametric generalized rank (GRANK) test from Kolari and Pynnönen (2011).

[Table 17 about here.]

The first column of Table 17 reports mean CARs over the aforementioned event windows, all of which are consistently negative and statistically significant. The mean CAR for the three-trading-day window $[-1,1]$ is -0.35%, indicating an immediate response to first-time carbon emission disclosure. The effect persists in the longer windows, reaching -2.45% over a period of 41 trading days surrounding the disclosure.

Our findings align with the result of Florackis et al. (2025) showing significantly lower event-day abnormal returns for firms with high emission intensity and high institutional ownership in a UK sample.

[Table 18 about here.]

Table 18 reports our results of an OLS regression examining the cross-sectional dependence of CARs on emission intensity and institutional ownership. The emission intensity is defined as the first disclosed Scope 1 emissions divided by total revenues. As reported by Florackis et al. (2025) for event-day abnormal returns, the coefficient on the interaction term of the logarithmic emission intensity and institutional ownership is negative and statistically significant for the shortest event window $[-1, 1]$. For the extended event windows, the coefficient remains negative but is no longer statistically significant. Consistent with our equity-market liquidity findings, these results suggest that short-term shareholder wealth effects are most pronounced for high carbon emitters that face substantial scrutiny from institutional investors.

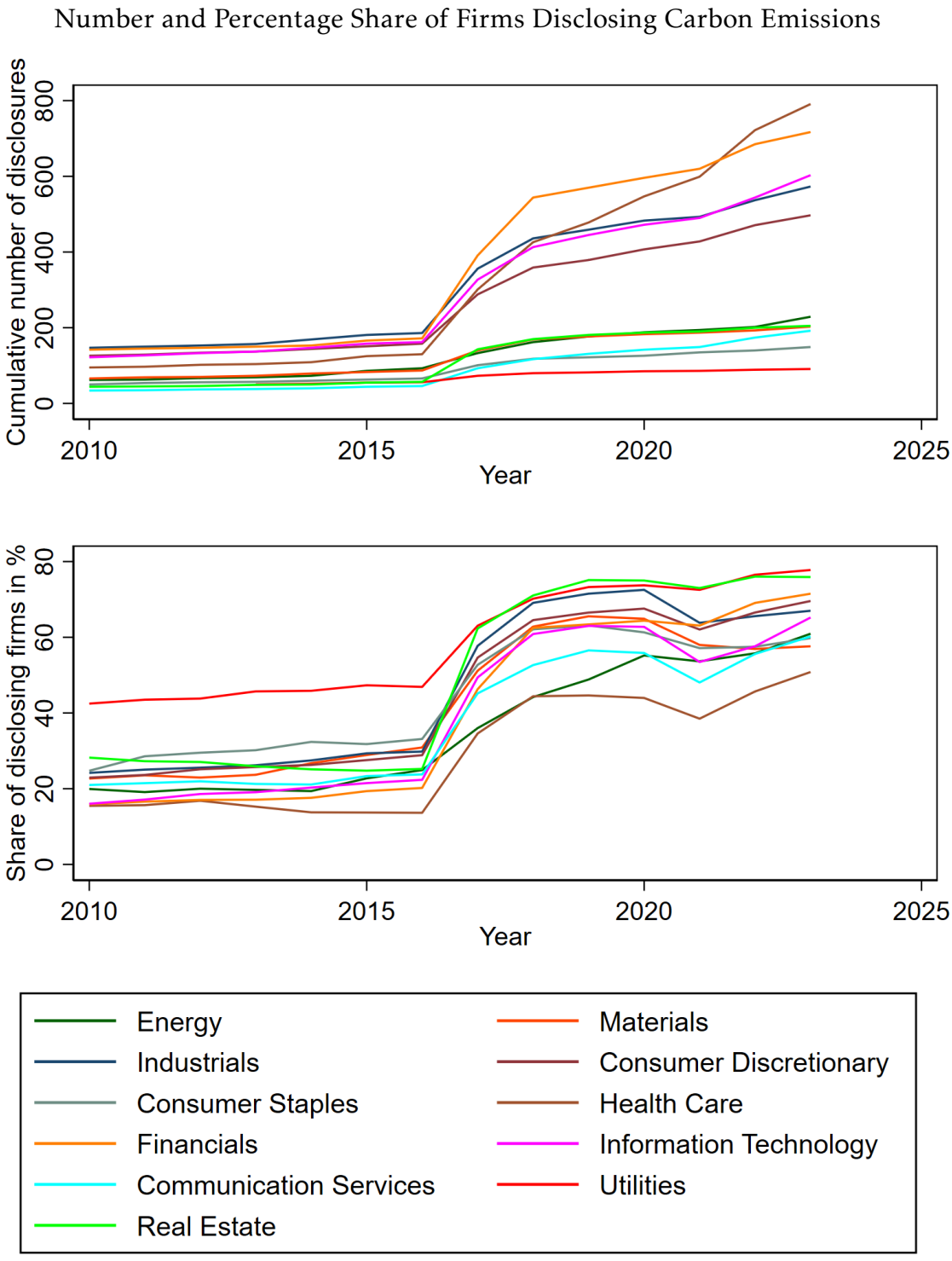


Figure 1: This figure presents the cumulative number of disclosures in our dataset and the share of disclosing firms by industry and year, 2010–2023.

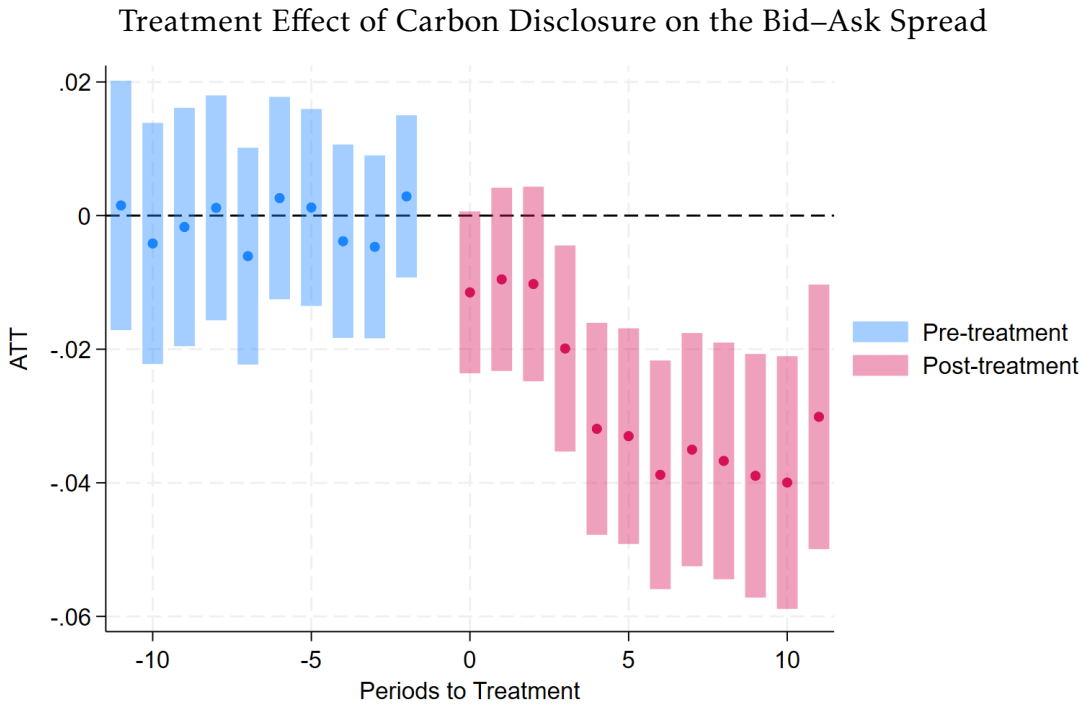


Figure 2: Using the DiD method from Callaway and Sant'Anna (2021), we estimate the effect of first-time carbon emission disclosure on the logarithmic relative bid—ask spread (*Spread*). The ATT is depicted for each period relative to the event, along with the 95% confidence interval. One period corresponds 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.

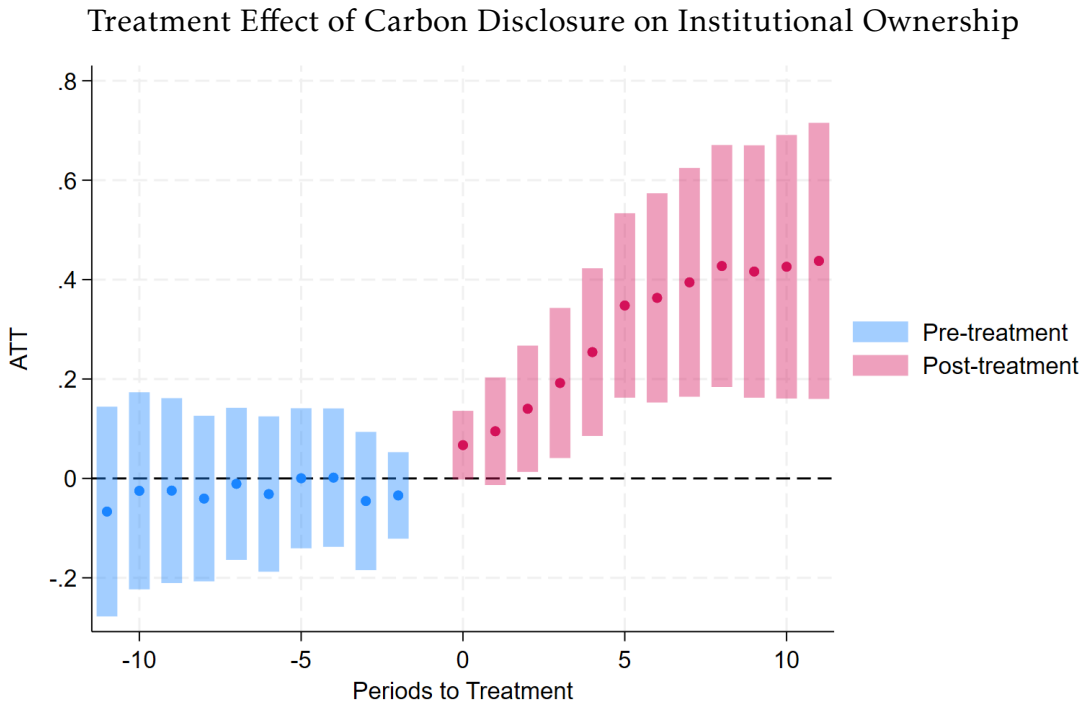


Figure 3: Using the DiD method from Callaway and Sant'Anna (2021), we estimate the effect of first-time carbon emission disclosure on the share of institutional ownership in %. The ATT is depicted for each period relative to the event, along with the 95% confidence interval. One period corresponds to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

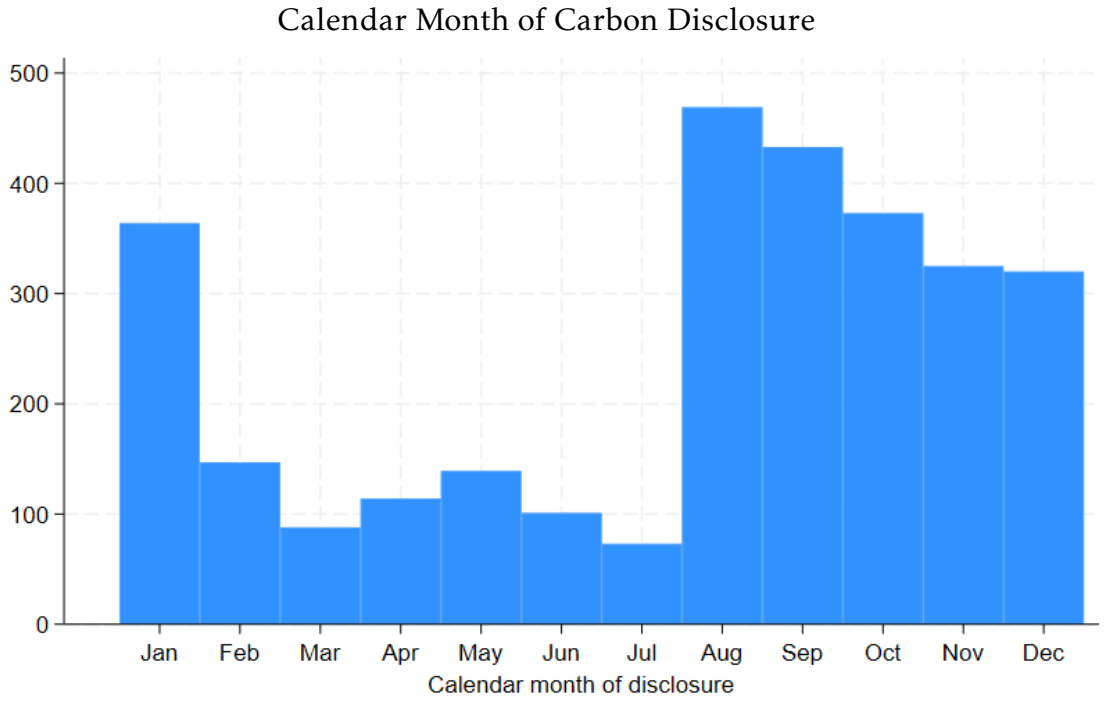


Figure 4: This figure shows the distribution of first-time carbon emissions disclosure dates throughout the year within the final sample.

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.

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.

Table 3: DiD Analysis on the Bid–Ask Spread around First-Time Carbon Emissions Disclosure

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 carbon emission disclosure. *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 First-Time Carbon Emissions Disclosure

	(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 carbon emission disclosure. *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 5: DiD Analysis on the Share of Institutional Ownership around First-Time Carbon Emissions Disclosure

	(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 carbon emission disclosure. *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 6: DiD Analysis on the Bid—Ask Spread around First-Time Carbon Emissions Disclosure, 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 carbon emission disclosure. *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.

Table 7: DiD Analysis on the Bid—Ask Spread around First-Time Carbon Emissions Disclosure, 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 carbon emission disclosure. *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.

Table 8: DiD Analysis on the Bid—Ask Spread around First-Time Carbon Emissions Disclosure, 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 carbon emission disclosure. 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.

Table 9: Firm Characteristics by Timing of Disclosure

	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.

Table 10: DiD Analysis on the Bid–Ask Spread around First-Time Carbon Emissions Disclosure, Dependent on Disclosure Timing

	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 carbon emissions disclosure. *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.

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 12: 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 13: DiD Analysis on the Share of Institutional Ownership around First-Time Carbon Emissions Disclosure

	(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 carbon emissions disclosure. 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 14: DiD analysis on the Bid–Ask Spread around First-Time Carbon Emissions Disclosure, 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 carbon emissions disclosure. *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 15: 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 disclosing firms. Each observation is obtained by aggregating five consecutive trading days. The statistics are calculated using the observation containing the treatment date.

Table 16: DiD Analysis on the Bid–Ask Spread around First-Time Carbon Emissions Disclosure, Dependent on State and Industry

	(1) Yale Low	(2) Yale High	(3) No ETS	(4) ETS	(5) Not Material	(6) Other
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)
Treated × *Post*	−0.02***	−0.02**	−0.02**	−0.02**	−0.02**	−0.02**
	(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***
	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
B/M	0.04**	0.06**	0.05**	0.06**	0.04*	0.05**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Return on Equity	0.02	0.05**	0.02	0.04*	0.05**	0.01
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Constant	12.41***	12.17***	12.81***	11.74***	12.17***	12.12***
	(0.68)	(0.74)	(0.63)	(0.78)	(0.66)	(0.77)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	167355	155229	187248	145040	247440	136253
r2	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 carbon emission disclosure. 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.

Table 17: 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 18: 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	21.277	11.239	−10.449	0.402	18.778	9.493	−6.709	3.154
	(1.64)	(0.45)	(−0.30)	(0.01)	(1.43)	(0.37)	(−0.19)	(0.06)
Inst. Own. in %	0.567	0.537	−0.058	2.394	0.561	0.882	0.556	2.615
	(1.00)	(0.49)	(−0.04)	(1.06)	(0.94)	(0.77)	(0.34)	(1.11)
Log Intensity × Inst. Own. in %	−0.397**	−0.242	−0.083	−0.292	−0.375**	−0.230	−0.130	−0.323
	(−2.14)	(−0.68)	(−0.16)	(−0.40)	(−2.00)	(−0.63)	(−0.26)	(−0.44)
Log Market Cap.					−5.129	−8.936	−5.788	23.364
					(−0.69)	(−0.62)	(−0.29)	(0.79)
Leverage					−2.191	−0.018	0.932	12.300
					(−0.60)	(−0.00)	(0.09)	(0.85)
B/M					−16.258	77.161**	147.838***	209.848***
					(−0.88)	(2.16)	(2.95)	(2.86)
Constant	−58.555	−60.253	−70.830	−368.328**	61.181	62.720	−75.115	−997.939
	(−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.