Do Firms Manipulate Their Carbon Emissions Reporting?

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Abstract: We document that firms underreport carbon emissions in the aftermath of firm-specific, high-profile climate controversies. To tie underreporting to manipulation, we show that firms underreport more when they avoid costly decarbonization, enjoy greater reporting discretion, and experience greater stakeholder backlash. Additional analysis suggests that firms manipulate primarily to avoid reputational damage. Following controversies, firms increase their use of third-party assurance, consistent with attempts to bolster the credibility of reported emissions. Yet such assurance does not reduce subsequent underreporting unless board oversight exists. In another setting, we additionally find evidence that firms selectively apply carbon accounting rules to restate historical emissions in ways that create favorable trends. Together, we provide large sample evidence consistent with firms intentionally misreporting their carbon emissions.

JEL Classifications: *G18; G30; M14; M41; M42; M48*

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Introduction

Firms increasingly disclose their carbon emissions, both voluntarily and in response to new regulations. Because carbon disclosure guides decarbonization and economy-wide capital-risk allocation (e.g., Bolton and Kacperczyk, 2021, 2023; Ilhan et al., 2023), its credibility is crucial. Prior studies and media articles highlight frequent under- and over-reporting, often attributed to *innocuous errors* as firms are still learning to account for emissions (The Guardian, 2011; Gipper et al., 2025b). Yet some inaccuracies may reflect loophole exploitation or deliberate *manipulation*. Detecting such manipulation is difficult because firms rarely disclose how they calculate emissions. As a result, while journalistic investigations provide anecdotal evidence, systematic large-sample evidence of "managers manipulating emission metrics… has not been documented" (Glenk, 2025).

This study provides large sample evidence consistent with firms intentionally underreporting their emissions after climate controversies. The climate controversies we study are firm-specific incidents involving atmospheric pollution and emissions that attract significant media and stakeholder scrutiny.[1] Backlashes from stakeholders could damage managements' reputation and draw increased attention to reported emissions, prompting managers to repair their image via claims of improved environmental performance. Climate controversies could also generate contractual incentives because a significant portion of managers' compensation depends on stock returns (e.g., Murphy, 1985), which could decline after climate controversies, and some plans explicitly link pay to environmental performance (Cohen et al., 2023; Li, 2024). These incentives likely prompt firms to pursue a range of responses that improve their perceived environmental performance, including lowering reported emissions.

[1] One example is ExxonMobil. On October 5, 2020, leaked internal documents revealed that the firm planned to increase oil production by 2025, resulting in a yearly increase of 21 million metric tons (or 15%) of carbon dioxide and other greenhouse gases. This contradicted their own public targets to reduce pollution and emissions. After Bloomberg reporting, ExxonMobil received significant backlash from stakeholders. See Appendix A.

To lower reported emissions, managers can either incur real costs—such as divesting high-polluting facilities—or understate emissions through carbon accounting choices, such as excluding certain plants from calculations or making unrealistic assumptions. Although climate controversies increase attention from stakeholders and the media, this scrutiny is unlikely to improve detection. This is because, different from financial reporting where future cash flows ultimately constrain discretion in accruals, carbon accounting lacks an equivalent verification mechanism. Moreover, the assumptions and data underlying emissions calculations are generally unobservable to outsiders, and this verification gap extends even to regulators, who themselves struggle to identify underreporting (ProPublica, 2025).[2] These features suggest an increase in the potential economic benefits of underreporting when responding to climate controversies without a commensurate increase in detection risk. Therefore, we hypothesize that firms deliberately underreport carbon emissions following climate controversies, consistent with evidence from other settings where managers favor low-cost strategies (e.g., Dechow et al., 2010; Slemrod, 1992).

To test our prediction, we examine whether firms are more likely to underreport direct emissions in the four years following a climate controversy, compared to all other years within firm. We identify a comprehensive sample of climate controversies for public firms from 2012 to 2021 using data from RepRisk, which systematically tracks ESG-related controversies reported by global media, NGOs, regulators, etc. We focus on severe, high-profile firm-specific environmental incidents—typically covered by major outlets and known to trigger market and stakeholder reactions. To detect underreporting, we follow Gipper et al. (2025b) and compare reported emissions to a model-predicted benchmark of "expected" emissions based on observable

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[2] Stakeholders may speculatively infer underreporting; however, these stakeholders do not observe key inputs and assumptions, and underreporting is likely coupled with other tactics (e.g., voluntary assurance) to bolster reporting credibility. These frictions likely prevent *suspicion* from leading to *true detection* of underreporting.

production characteristics, grounded in engineering research documenting that emissions scale with production output and technology. We measure underreporting as the numerical difference between disclosed and expected emissions.

The findings support our hypothesis. In the four years following a climate controversy, firms are 6.1 percent more likely to underreport their direct emissions, and the magnitude of underreporting equals nearly 20 percent of its standard deviation. This result is robust to alternative event windows and measures of underreporting, a difference-in-differences (DiD) design per Cengiz et al. (2019) and Baker et al. (2022), and does not hold for controversies unrelated to emissions, such as water-related pollution or DEI scandals. Event-time analysis shows limited underreporting before controversies and higher underreporting afterward. Together, the results are consistent with firms deliberately understating their emissions.

Although underreporting may arise from innocuous errors, several of our findings suggest it reflects manipulation. First, the central premise of our hypothesis is that underreporting is less costly and easier to implement than genuine decarbonization. In contrast to the strong evidence of underreporting, we find little indication that firms increase divestments or green investments after climate controversies. Second, in the cross section, underreporting is (i) concentrated among firms that forgo tangible decarbonization actions, consistent with manipulation and decarbonization acting as substitutes; (ii) more prevalent when firms have more opportunities to manipulate—for instance, among firms with more complex ownership structures or more subsidiaries where excluding certain facilities is easier to carry out; and (iii) stronger when the risks of backlash and stakeholder scrutiny are higher. Additional analysis suggests that both reputational and contractual incentives contribute to underreporting, with reputational concerns playing a more prominent role.

We explicitly consider alternative explanations. One possibility is a latent operational shock that both triggers a climate controversy and disrupts a firm's carbon accounting systems, leading to incomplete emissions scoping and thus unintentional underreporting. Another is a decline in the application of the "conservatism principle" of carbon accounting, which encourages firms to err on the side of overstating emissions under uncertainty. To the extent that climate controversies lead to subsequent improvements in carbon accounting systems, firms would overreport less, which we could measure as underreporting in the analysis. Inconsistent with these alternative stories, we find no systematic changes in carbon accounting systems around climate controversies.

How do firms make understated emissions appear credible to stakeholders? Although our analysis centers on underreporting, firms likely deploy a basket of tactics to bolster their reputation. A natural candidate is voluntary carbon assurance, which prior work identifies as a key signaling device in sustainability reporting (Gipper et al., 2025a). Indeed, we observe an increase in assurance adoption after climate controversies. Furthermore, we find no evidence that assurance curbs underreporting after climate controversies, consistent with management using assurance as a signaling tactic. This finding contrasts with prior research showing that carbon assurance reduces potentially innocuous errors (Gipper et al., 2025b) and with the role of financial audits as a defense against misreporting (DeFond and Zhang, 2014). A likely reason is that carbon assurance is voluntary and management-engaged, making it useful when managers seek higher reporting quality but vulnerable to agency problems when managers behave opportunistically.

While our event-study approach provides rich cross-sectional variation to link underreporting to manipulations, we cannot document *how* firms misreport, a question complicated by the lack of disclosure of calculation methods and input parameters, and our reliance on the expected-emissions model. In our last analysis, we exploit a unique feature of the carbon accounting rules and show—in another setting—that firms restate historical emissions after mergers and acquisitions but not after similar-sized divestments, even though rules require restatements after both. This asymmetry suggests that firms selectively restate to justify increases in emissions while avoiding restatements that would explain decreases, thereby creating a misleading appearance of decarbonization.

The central contribution of this study is to the nascent literature on *accounting* for carbon. Some studies examine how to design carbon accounting rules (e.g., Reichelstein, 2024; Penman, 2025; Glenk, 2025; Reichelstein et al., 2025; Easton et al., 2025). Other studies focus on corporate carbon reporting under the existing carbon accounting rules. Most closely related to our work are studies that evaluate the quality of carbon reporting and document under- and overreporting.[3] While these studies collectively highlight rampant inconsistencies, they either do not take a stance on the drivers or attribute the findings to errors or tailoring disclosures to different stakeholder audiences. Glenk (2025) goes further by conjecturing biased reporting but notes that "evidence of managers manipulating emission metrics… has not been documented." We fill this gap by studying underreporting after climate controversies in a large sample. Importantly, we link these patterns to managerial incentives and exploit institutional features of carbon accounting rules to distinguish deliberate misreporting from unintentional errors.

This study is closely related to—but distinct from—the ESG-washing literature, which has usually been defined as "a gap between symbolic and substantive actions" (Siano et al., 2017). That literature emphasizes two forms of deception. The first is marketing-based, where qualitative statements diverge from underlying actions (e.g., Baker et al., 2024; Raghunandan and Rajgopal, 2024). The second is operational deception, where firms shift polluting or socially undesirable activities to jurisdictions with less scrutiny (Bartram et al., 2022; Deng et al., 2024; Cascino and

[3] See Depoers et al. (2016); Klaaben and Stoll (2021); Li et al. (2024); Gipper et al. (2025b); Cohen et al. (2025); Lepere et al. (2025); Aobdia et al. (2025).

Correia, 2024; Ecker and Keeve, 2025; Duchin et al., 2025). By contrast, we examine accounting-based deception: firms exploiting or breaking carbon accounting rules to misstate quantitative emissions. While symbolic and operational greenwashing may be widely acknowledged, documenting accounting-based manipulation is critical because it produces the figures on which regulators and investors base key decisions.[4]

Our study also relates to the broader literature on earnings management, audit, and governance. For example, our use of event study is consistent with studies exploiting periods with strong managerial incentives to identify financial misreporting (e.g., Jones, 1991), and we extend this literature by focusing on a newer form of accounting. As another example, we examine governance mechanisms and show that carbon assurance is not associated with reduced underreporting, in contrast to its effectiveness in addressing innocuous errors in emissions reporting (Gipper et al., 2025b) and to financial statement audits, where even voluntary audits constrain misreporting, perhaps due to features like auditor independence and board oversight (Kim et al., 2011; Minnis, 2011; DeFond and Zhang, 2014). This comparison highlights institutional frictions that limit the effectiveness of carbon assurance in its current form, and the potential role of sustainability governance in improving carbon reporting credibility.

The findings have implications for standard setters, regulators, and stakeholders. Glenk (2025) notes biased disclosure as a potential flaw of the existing carbon accounting rules. We provide direct evidence of such behavior, which can inform ongoing improvements to the Protocol

4 There are two studies related to ours. First, Bailey et al. (2025), which to our knowledge is the only prior work to document accounting-based deception in corporate social responsibility reporting, in their case in U.K. gender pay gap disclosures. We extend this inquiry to environmental reporting and provide a systematic analysis of accounting-based manipulation of emissions reporting, which is arguably the most consequential sustainability metric for stakeholders. Second, Gipper et al. (2026) show that small decreases in reported emissions intensity is related to increasing emissions levels later, lower likelihood to meet emissions reduction targets, and meet-or-beat financial reporting behavior. We consider this paper to be complementary to ours by showing that relevant ex post outcomes correlate with strategic emissions reporting behavior while we show that strategic emissions reporting can take the form of accounting-based manipulation, especially following climate controversies.

design as well as efforts to adopt alternative reporting standards.[5] As mandatory carbon-reporting regimes proliferate, our findings suggest regulators and investors must look beyond innocent mistakes to intentional manipulation when allocating ESG capital and pricing carbon risk. The existence of misreporting also underscores the need for greater standardization in carbon accounting, such as through clearer measurement rules, enhanced comparability, and the consistent application of reporting boundaries. Moreover, our evidence that voluntary assurance provides limited protection against deliberate misreporting highlights the need for stronger sustainability governance—potentially through mandatory assurance, stricter enforcement, and board oversight.

1. Institutional Setting

1.1 The Current State of Carbon Accounting and Reporting

Carbon accounting, or Greenhouse Gas accounting, is a method by which firms measure and disclose their historical quantitative emissions. The vast majority of the firms that report carbon emissions follow the GHG Protocol, which is a carbon accounting standard developed by the World Resource Institute and the World Business Council for Sustainable Development in 1998 and has been updated several times since.[6] The GHG Protocol classifies emissions into three categories (or "scopes"): direct emissions from sources controlled or owned by the reporting firm (Scope 1), indirect emissions associated with the purchase of electricity and other energy (Scope 2), and other indirect emissions along the value chain, such as emissions by suppliers (Scope 3).

Firms calculate reported emissions by scope. The calculation proceeds in three steps. The first step is to choose the organizational boundary and determine which entities' emissions will be

[5] For example, in September 2024, World Resource Institute (WRI) and World Business Council for Sustainable Development announced four technical working groups to revise and update the corporate carbon accounting standards (WRI, 2024). In addition, a coalition of academics and practitioners formed the Carbon Accounting Standards Initiative, which explores the adoption of alternative carbon accounting methods.

[6] According to the World Resource Institute, 97% of the disclosing S&P 500 companies report their emissions using the GHG Protocol. See https://ghgprotocol.org/about-us.

included. The second step is to classify emissions into the three scopes mentioned above. The third step is to calculate emissions, which is done by multiplying activity data with emissions conversion factors. While there are different types of greenhouse gases (e.g., CO_2, methane), all GHG emissions are subsequently converted to carbon dioxide equivalents. This is why we use "GHG emissions" and "carbon emissions" interchangeably, as is conventional.

1.2 Carbon Accounting Quality and Misreporting Opportunities

High-quality carbon information is essential in stakeholder decision making (Ilhan et al., 2023). A nascent strand of research studies—both theoretically and empirically—the quality of carbon accounting. Gipper et al. (2025b) propose a framework to evaluate carbon accounting quality. In their framework, carbon accounting quality is a function of (1) measurement standard quality, and (2) implementation quality. The former refers to the ability of the GHG Protocol to track a firm's carbon footprint. Because the GHG Protocol is so prevalently used, studies examining the measurement standard itself focus on conceptual and theoretical critiques and solutions, e.g., Reichelstein (2024), Penman (2025), Glenk (2025).

Research on implementation quality focuses on firms' carbon reporting within the GHG Protocol. Implementation issues could include both innocuous errors and intentional misreporting. Several recent studies highlight inaccuracies in emissions reporting, but they typically treat errors as innocuous or remain agnostic about intent. Cohen et al. (2025) analyze 900 disclosures from 276 firms and find that 60% are later restated, with understatements slightly more common, underscoring the pervasiveness of implementation problems. Studies of Scope 3 emissions (Klaaben and Stoll, 2021; Li et al., 2024) likewise reveal systematic misreporting, but do not ascribe it to deliberate behavior. Depoers et al. (2016) show that emissions reported in various venues differ from each other and argue that firms tailor disclosures to different stakeholder audiences. Gipper et al. (2025b) attribute improvements from external assurance to better carbon accounting systems that reduce unintentional errors. While findings in some of these studies could be theoretically consistent with intentional misreporting, they do not explicitly investigate whether inaccurate emissions reporting is partially driven by manipulations.

Firms have several ways to manipulate and misreport their emissions. Recall that the first step of calculating emissions is setting a firm's organizational boundary by deciding which facilities and subsidiaries should be included in the calculation. The GHG Protocol allows firms to pick one of three methods (i.e., financial control, operational control, equity share). A firm can strategically choose a method or aggressively apply discretion within a chosen method that results in the lowest emissions estimate. For example, they could exclude heavy-polluting facilities by arguing that they have no financial or operational control. They can even simply ignore certain facilities from their calculations by deciding that data is unavailable or of low quality, given limited oversight and opacity regarding these decisions.

Firms can also manipulate their activity data and emissions conversion factors. For example, if a firm rents half of a building without separate utility bills and estimates electricity use for their office space, the firm can either conservatively estimate its utility use or ignore the office space altogether. Similarly, because it is common to estimate emissions with sampling, reporting firms can select non-representative samples. In addition, because there are often multiple versions of the conversion factors, a firm can choose a less accurate factor that results in lower reported emissions. Firms rarely disclose these input parameters and, again, experience limited oversight over the reporting process, so firms can exploit these opportunities to misreport emissions.

We focus on the underreporting of Scope 1 emissions because they are the most directly attributable to firm operations and therefore the clearest setting in which managers have both responsibility and discretion. Unlike Scope 2 and—especially—Scope 3 emissions, which rely heavily on external data, estimation, and value-chain assumptions, Scope 1 reporting is closer to "hard" operational data and forms the core of firms' carbon disclosures. Moreover, manipulation of Scope 1 emissions is more likely to be perceived as credible by stakeholders, because reductions in reported Scope 3 emissions can be attributed to, e.g., discretion in life cycle analysis rather than genuine product-based emissions improvements. As such, evidence of underreporting in Scope 1 thus provides a cleaner test of intentional misreporting and avoids confounding explanations from measurement noise or needed assumptions that are more pronounced in Scopes 2 and 3.

2. Empirical Strategy

2.1 Research Design

To test our prediction, we estimate the following regression model:

$$Underreport_{i,t} = \alpha + \beta\ Climate\ Controversy_{i,t} + Controls + Firm,\ Year\ FE + \varepsilon_{i,t} \qquad (1)$$

In Eq. (1), *Underreport* is a measure of underreporting of Scope 1 emissions for firm *i* in year *t*, which we discuss in Section 3.2. *Climate Controversy* is an indicator equal to one if there is one or more severe or high publicity events related to atmospheric pollution in the current year or the previous three years.[7] We illustrate the coding of this variable in Appendix B. If firms are more likely to underreport emissions, the coefficient on *Climate Controversy* (i.e., β) should be positive. We control for common firm characteristics that may affect carbon accounting quality, including firm size, book-to-market, leverage, loss indicator, return on assets, and firm age (Gipper et al., 2025b). We include firm and year fixed effects. Firm fixed effects control for unobservable heterogeneity that is invariant within a firm over time. Year fixed effects remove yearly shocks

[7] Climate controversies need not pertain specifically to a firm's Scope 1 emissions to influence its reporting. Such controversies often raise doubts about the credibility of a firm's overall climate strategy and disclosures, prompting broader revisions to the firm's disclosure strategy, including the most scrutinized and comparable category: Scope 1 emissions.

common to all firms in our sample. Our research design compares underreporting in the four years following a climate controversy to other years of the same firm, while eliminating time trends. This setup is analogous to the research design of Akey et al. (2024), Christensen et al. (2023), and Larcker et al. (2025). We select a four-year window to be consistent with those studies, but our results are insensitive to this choice. Throughout the study, we base our inferences on standard errors clustered by firm.

2.2 Measuring Underreporting

Conceptually, underreporting occurs when reported emissions are lower than actual emissions. Direct detection of underreporting requires a comparison of reported and actual emissions, which are unobservable. Prior research relies on prediction models, which are based on the idea that emissions are a byproduct of production and hence can be expressed as a function of production output and technology. The more output a company produces, the higher the emissions; the cleaner the production technology is, the lower the emissions. Variants of emissions models have been used in accounting, finance, economics, and environmental engineering studies (Goldhammer et al., 2017; Griffin et al., 2017; Downar et al., 2021; Bolton and Kacperczyk, 2021; Nguyen et al., 2021).[8]

We rely on Gipper et al. (2025b), which estimates the following model:

$$\text{Ln}(Emissions_{i,t}) = \beta_{industry(i)}\ Determinants_{i,t} + \text{Industry-Year FE} + \varepsilon_{i,t}. \quad (2)$$

The dependent variable is the natural logarithm of Scope 1 emissions for firm *i* in year *t.* The

[8] We favor this approach over emissions derived from satellite images for two reasons. First, satellite images do not fully capture emissions from non-manufacturing plants and, therefore, cannot speak to a broad cross section of firms. Second, satellites are ineffective in capturing CO_2 emissions (which constitutes the vast majority of GHG emissions) *at the plant level* because (1) CO_2 is well-mixed in the atmosphere making it hard to detect changes driven by firm production activities, (2) spatial and temporal coverage of current CO_2 sensors (e.g., OCO-2) are too sparse (e.g., Reuter et al. 2014, 2019), (3) proper measurement requires high frequency temporal coverage and favorable atmospheric conditions (e.g., low cloud cover, limited aerosol interference, and stable wind patterns to prevent rapid dilution).

determinants capture either production output or production technology, including firm sales, changes in inventory, cost of goods sold (COGS), firm size, net property, plant, and equipment (PP&E), fixed asset tangibility (net PP&E divided by assets), capital expenditure, asset age, number of countries and states with operations, the number of employees, and lagged emissions. The coefficient for each determinant varies by industry to account for the fact that production differs by industry. The model includes industry-year fixed effects to capture the industry-wide heterogeneities in production over time. Like Gipper et al. (2025b), we interpret the fitted values generated by Eq. (2) as a proxy for actual emissions.[9]

After estimating the emissions model in Eq. (2), we detect underreporting by comparing the difference between the reported emissions and the model-estimated benchmark as follows:

$$Underreport = \text{Ln}(\widehat{Emissions_{i,t}}) - \text{Ln}(Emissions_{i,t}). \quad (3)$$

Eq. (3) is the negative of the residual value in Eq. (2), with higher values indicating reported emissions being substantively lower than the benchmark and greater degrees of underreporting. Empirically, our residuals exhibit both positive and negative values in roughly similar proportions, a pattern consistent Cohen et al. (2025)'s finding that restatements arising from over- and under-reporting are roughly evenly split. This alignment suggests that the benchmark emissions model generates a plausible distribution of deviations.

By using the signed distribution of deviation from the benchmark, we measure the *degree* of potential underreporting rather than relying on binary classification. This approach reduces sensitivity to the precise specification of the emissions model on the classification margin and hence potentially mitigates model specification concerns. We further mitigate these concerns by

[9] Gipper et al. (2025b) document that deviations of reported emissions from model expectations are associated with lower environmental disclosure scores from Bloomberg and Refinitiv and higher managerial uncertainty of emissions reporting accuracy. The measure does not correlate with earnings management proxies such as "meet or beat" and abnormal accruals. Additionally, firms that report to the CDP or the U.S. Environmental Protection Agency have smaller deviations. They interpret these results as corroborating the model's validity.

exploring different emissions models, as well as alternative measures of underreporting, including an indicator for whether *Underreport* is positive, and a truncated version which sets *Underreport* to 0 when reported emissions are higher than the model-estimated benchmark.[10]

2.3 Data

Our study draws on multiple data sources. Climate controversies come from RepRisk, which compiles ESG incidents from news outlets, social media, government agencies, and other channels. For each incident, we observe the date and characteristics such as severity and reach. We focus on high-profile events involving climate change, GHG emissions, or atmospheric pollution, i.e., those with the highest severity or reach, affecting many people, reflecting systematic intent, and/or receiving global media coverage (e.g., *The New York Times*). We expect misreporting incentives to arise from stakeholder backlash, which is most likely when events are both visible and severe.

In addition to the event-level data, we use a separate firm-date panel from RepRisk to capture firms' reputational standing. RepRisk employs a proprietary algorithm to dynamically quantify each firm's reputational risk exposure at the daily level. The resulting measure—RepRisk Index (RRI)—ranges from 0 to 100, with higher values indicating greater reputational damage and heightened perceived risk for stakeholders when considering interactions with the firm. This dataset allows us to investigate the underlying sources of manipulation incentives.

We incorporate several other datasets. We exploit environmental disclosure data from CDP (formerly the Carbon Disclosure Project), which annually surveys the world's largest firms for

[10] There are four clarifications for interpreting our model-based underreporting measure. First, we acknowledge that declining reported emissions, even relative to a benchmark model, is not *per se* underreporting. Hence, we support our interpretation with a variety of additional tests. Second, underreporting does not require emissions levels to fall; underreporting can increase if actual emissions rise more than reported emissions. Third, firms can reinforce the perception of actual decarbonization with other tactics such as qualitative ESG disclosures or carbon assurance (see Table 8). Finally, it is unclear whether stakeholders rely on models like ours, likely because building a model requires large data panels. Stakeholders often evaluate emissions within a firm's time series using historical emissions and some scalar. Table IA-1 provides evidence that emissions—controlling only for lagged emissions and sales—decline following controversies, plausibly perceived by a typical stakeholder as decarbonization.

detailed information on carbon emissions and broader environmental practices. We further use emissions data from Trucost, carbon assurance data from Gipper et al. (2025a), sustainability governance data from LSEG ESG (formerly Refinitiv), mergers and acquisitions data from SDC Platinum, firm characteristics from Compustat, and stock market variables from CRSP.

To create our main sample, we begin with all firms in Compustat, which we then intersect with the underreporting measures. This intersection represents the largest sample attrition because many public firms do not disclose emissions. We additionally merge this panel with RepRisk events data and eventually drop observations with missing control variables. Our final sample is an annual panel dataset uniquely identified by firm and year from 2012 through 2021.[11] This sample includes 896 unique firms and 5,370 observations, with 480 climate controversies occurring in 112 firms and 235 firm years. We note that for subsequent tests sample sizes vary due to data availability. For example, certain variables rely on data from CDP questionnaires, but some questions are not included in the surveys for all years in our sample. We winsorize all continuous variables at the 1st and 99th percentiles to mitigate the influence of outliers. We conduct all tests using the full sample of observations, conditional on data availability.

2.4 Descriptive Statistics

Figure 1 Panel A plots the yearly number of high-profile climate controversies and the number of firms with climate controversies in our sample. Before 2015, climate controversies are uncommon. Since 2016, both variables have exhibited strong upward trends. The breakpoint coincides with the signing of the Paris Agreement in April 2016, which likely increased awareness of these events, and hence, the reporting of them. As mentioned in the previous section, we retain

[11] Our sample begins in 2012 because our RepRisk data starts in 2009. By setting the beginning year to 2012, we ensure consistent coding of the *Climate Controversy* variable, i.e., all years in our sample incorporates environmental controversies in the current and previous three years.

high-profile events that have either high reach or high severity. In Panel B, we plot the number of events in each category. Although both types of events exhibit increasing time trends, they do not overlap, indicating that not all severe events are widely disseminated, and vice versa. Table 1 presents summary statistics for the main variables in our sample. The mean value of *Underreport* is close to zero, which is expected because OLS residuals average out to zero by construction. The distributions of other variables are consistent with prior literature (Gipper et al., 2025b).

To validate that the climate controversies in our sample are material, we examine how stakeholders respond to these events in the [-5, 10] day window (e.g., Flammer, 2021). First, we focus on shareholders and plot the average cumulative abnormal returns (CAR) using a CAPM model. As we show in Panel A of Figure 2, whereas there is a general downward trend in CAR, the trend becomes particularly salient after the revelation of climate controversies at $t = 0$ and does not show signs of reversion. Next, we focus on other stakeholders and examine how the RepRisk Index (RRI) changes in the same event window. As we show in Panel B, sample firms' reputational risk exposure increases significantly around climate controversies and remains stable afterwards. Together, Figure 2 validates that the events we study result in both reputational and financial damage, which, in turn, could incentivize misreporting.

3. Main Results

3.1 Climate controversies and underreporting

In Table 2, we report results from estimating Eq. (1) with several measures of underreporting. In column (1), the outcome variable is an indicator variable ($Underreport^{Ind}$) equal to one if reported emissions is smaller than the model-estimated benchmark, i.e., *Underreport*>0. This measure does not capture the degree of underreporting, but it allows us to estimate the probability of underreporting. The coefficient is positive and statistically significant, suggesting an increase in the probability of underreporting by a sizable 6.1%, which is equivalent to 12.2% of the standard deviation. In column (2), the outcome variable (*Underreport*^{Trunc}) is a truncated variant of *Underreport*, which we set to zero when reported emissions are higher than the benchmark. The intuition is that there should not be any underreporting when a firm overreports its emissions. If the emissions model in Eq. (2) is well-specified, then this approach reduces noise and accurately captures the degree of underreporting. We continue to find that firms are more likely to underreport after climate controversies. In terms of economic magnitude, the coefficient on *Climate Controversy* indicates an increase in the degree of underreporting by 0.050, equivalent to 13.9% of the standard deviation.

In column (3), we report results using our preferred underreporting measure, the continuous variable *Underreport*. We favor this variable over the truncated measure because some firms may overreport their emissions but remain unaware of the issue given their poor carbon accounting system. If those firms intentionally underreport their emissions after climate controversies, such "underreporting" might still be considered overreporting relative to the model benchmark. In cases like this, we would not detect underreporting with our truncated measure but would with our continuous measure. The regression estimate suggests an increase in the degree of underreporting by 0.069, or 13.3% of the standard deviation. This economic magnitude is highly similar to columns (1)-(2). Together, this analysis provides preliminary evidence that firms underreport their emissions in the four years after a climate controversy.

We conduct a battery of robustness checks to strengthen our inference. As we show in Internet Appendix Table IA-1, our findings are robust to using alternative definitions of post-event periods, alternative ways to establish the expected level of emissions, and inclusion of industry-specific time-trends. We additionally perform falsification tests using environmental scandals unrelated to carbon emissions (e.g., incidents involving water pollution or hazardous waste), as well as workplace scandals centered on social issues (e.g., gender or racial discrimination). We find no evidence of underreporting following either type of event. These results reinforce our interpretation that emissions underreporting is a targeted response to climate-related controversies, rather than a general reaction to reputational shocks or ESG-related events. In Table IA-2, quantile regressions show underreporting effects at the 40th, 60th, and 80th percentiles but not at the 1st, 20th and 100th, suggesting firms understate emissions enough to benefit from perceptibly lower emissions but also in moderation to limit accusations of erroneous reporting or detection risk.

To the extent that climate controversies are non-random, our results are prone to alternative explanations. Our identifying assumption is that in lieu of climate controversies, the treated and control firms would have similar trends in emissions underreporting.[12] We further sharpen our inference by examining underreporting in event time, analogous to the stacked cohort DiD analysis in Cengiz et al. (2019).[13] As we show in Table IA-3, our inference remains unchanged with this alternative design. This finding also addresses the concern regarding incorrect inferences arising from panel regressions with high dimensional fixed effects (Baker et al., 2022).

Turning to the event time analysis, as we show in Figure 3 Panels A-C, in the years prior to climate controversy, the coefficients are small, statistically insignificant, and mostly exhibit no obvious trends. In the event year, we observe an increase in the degree of underreporting and the pattern persists throughout the post period. Because firms often face multiple environmental

[12] In an untabulated analysis, we find that our underreporting measures do not predict future controversies, suggesting that firms do not proactively underreport to avoid climate controversies.

[13] Specifically, for each year in our sample, we include firms with climate controversies in that year (i.e., treated firms) and firms without any climate controversy in our sample period (i.e., control firms). For each year cohort, we retain observations from years t−3 to t+3. We then stack the cohorts to create a panel dataset. Because roughly 50% of the sample firms can have multiple climate controversies, we assign each firm to the cohort of its first severe, high-profile controversy and ignore subsequent controversies, so that no firm-year is simultaneously in the post-period for one controversy and the pre-period for another.

incidents, we cannot completely rule out anticipatory underreporting in response to lower-severity controversies. This should bias against finding sharp post-event effects when focusing on the most severe events, implying our estimates are conservative, or potentially contribute to modest pre-trends. Overall, the patterns in Figure 3 confirm the validity of the common trends assumption and provide further insights regarding the dynamics of underreporting.

3.2 Underreporting and Costly Decarbonization

Because underreporting can arise from both innocuous errors and manipulations, our main finding could be driven by omitted variables that result in both climate controversies and fragile carbon accounting systems and hence, underreporting due to error. We perform several tests to tie underreporting with manipulation. Our first set of analyses examines the substitution between costly decarbonization efforts and intentional underreporting. Prior research suggests that while many firms engage in greenwashing in response to environmental scrutiny (e.g., Siano et al. 2017), some undertake substantive decarbonization actions—such as divesting heavily polluting plants and facilities (Tomar, 2023; Duchin et al. 2025). These differing potential responses motivate two testable predictions. First, we expect to observe limited evidence of firms engaging in costly decarbonization following climate controversies. Second, if underreporting and real decarbonization are substitutes, the effect of climate controversies on underreporting should be more pronounced among firms that do not decarbonize.

As a first step, we examine whether firms undertake decarbonization efforts following climate controversies. We consider multiple costly decarbonization channels—structural, investment, and innovative—to ensure our results are not driven by a narrow definition. Prior research highlights several common strategies, including divesting polluting assets (divestment); investing in tangible assets that produce fewer emissions (asset renewals); and pursuing innovative activities aimed at reducing or preventing pollution (green innovation) (OECD, 2011; Tomar, 2023). We capture divestment using an indicator for whether the firm divests any assets in a given year. To evaluate investment in cleaner technology, we create an indicator variable equal to one if a firm's asset age decreases and its capital expenditure is above the industry median. This measure relies on the assumption that newer fixed assets are more likely to incorporate cleaner, more energy-efficient technologies, while the above-median spending filter excludes trivial or routine renewals. Finally, we capture green innovation using an indicator for whether a firm develops any green patents in a year.

We estimate Eq. (1) using these decarbonization outcomes as dependent variables and report the results in Panel A of Table 3. The coefficient on *Climate Controversy* is statistically insignificant and economically small across all three columns. This stands in sharp contrast to our main finding in Table 2 that firms underreport emissions following climate controversies. Taken together, the results are consistent with firms favoring underreporting over costly real actions.

Although the coefficient on *Climate Controversy* is insignificant, untabulated statistics show that a small subset of firms indeed takes costly decarbonization actions.[14] To examine whether such firms are less likely to underreport, we conduct a cross-sectional analysis by partitioning *Climate Controversy* into non-overlapping groups based on (1) whether the firm divests in a given year, (2) whether its asset age decreases relative to the prior year alongside with above median capital expenditure, reflecting substantive investment in cleaner production, and (3) whether it files for any green patents during the year. For this analysis, we additionally control for the split indicators to account for inherent differences in underreporting for groups with and without

[14] Although manipulation is generally less costly than decarbonization, it is not costless. Firms differ in the expected costs and benefits of both decarbonization and misreporting, such as reputational concerns, stakeholder pressure, regulatory scrutiny, and idiosyncratic manager preferences. These heterogeneous trade-offs could explain why some firms choose costly decarbonization instead of underreporting.

decarbonization efforts.[15] The results are reported in Panel B of Table 3. Across all three decarbonization measures, we find strong evidence of substitution: firms that undertake costly real decarbonization actions are significantly less likely to underreport emissions after climate controversies. Taken together, the findings in Table 3 reinforce our interpretation that emissions underreporting in response to climate controversies reflects intentional manipulation rather than inadvertent reporting issues.

3.3 Underreporting and Easiness to Manipulate

In our second set of analyses, we focus on the easiness to manipulate emissions. In Section 2, we list several ways through which firms can intentionally understate their emissions, including exploiting organizational boundary choices, excluding emissions sources, or estimating emissions using favorable assumptions or outdated emissions factors. To the extent that firms manipulate emissions disclosures in response to climate controversies, we expect the magnitude of manipulation to be larger when firms face fewer constraints and have greater opportunity and discretion to do so, i.e., managers at such firms find it easier to manipulate.

We proxy for this discretion using four structural characteristics. First, we use the number of reported subsidiaries as a proxy for operational fragmentation, which may make oversight of facility-level emissions reporting more difficult. Second, we consider the amount of noncontrolling interests—defined as the absolute value of noncontrolling interests divided by total equity—which reflects ownership complexity and the potential to exclude partially owned, emissions-intensive entities from Scope 1 boundaries. Third, we classify firms into industries that necessarily rely on

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[15] This specification is a reparameterization of an interacted model in which one group's effect is treated as the baseline and the other group's effect is measured relative to it. In this formulation, the effect of climate controversy for the "low" group is given directly by the baseline coefficient, and the effect for the "high" group is obtained by adding the baseline and the interaction term. Similar setups have been used in prior studies such as Ye et al. (2023) and Krueger et al. (2024).

estimation-based reporting, where a greater portion of emissions are typically calculated using activity data and standardized emission factors, rather than measured directly. Lastly, we consider the proportion of a firm's emissions coming from Scope 1, because if a firm's emissions are primarily along the value chain, then there are not much direct emissions to manipulate *ex ante*.

For each characteristic, we partition *Climate Controversies* into two non-overlapping indicators that classify events as occurring in the above- or below-median group (or in a high-estimation industry or not). Table 4 reports the results. Across all four dimensions, we find strong evidence that underreporting is concentrated among firms where manipulation is easier to carry out. We test differences in coefficients using Wald tests of equality. Because we have directional predictions, we rely on one-sided p-values, all of which fall below the 0.1 threshold. Overall, the evidence is consistent with the view that managers exploit discretion and organizational complexity to misreport when detection risk is lower. That is, manipulation is not randomly distributed but is systematically more prevalent in environments where rules are harder to enforce and monitoring is more costly (e.g., Leuz et al., 2003; Honigsberg et al. 2022).[16]

4. Additional Analysis

4.1 Variation in Stakeholder Response Likelihood

If managers intentionally underreport emissions following climate controversies, we expect the extent of underreporting to increase with the strength of their incentives. In our setting, managerial incentives come from stakeholder pressure. As such, we predict that our findings are

[16] In Table IA-4, we provide additional evidence of manipulation by examining the disclosure of omitted emissions sources. CDP occasionally prompts firms to indicate whether emissions sources are omitted, and prior work suggest omissions are disclosed truthfully when errors are innocuous (Gipper et al., 2025b). If underreporting after climate controversies reflects accidental issues in data collections, firms should acknowledge more omissions, while strategic manipulation implies withholding such information. We find no increase in omission disclosure after climate controversies, inconsistent with inadvertent disruptions to data collection or estimation procedures. More importantly, firms that disclose omissions are significantly less likely to underreport after climate controversies, supporting the view that underreporting following controversies reflects strategic rather than accidental behavior driven by limitations in the firm's carbon accounting infrastructure.

stronger when stakeholders are more likely to respond, e.g., when the events are particularly severe, when more stakeholders are aware of the events, and when events are unambiguously tied to firm actions. In our main design, we focus already on these high-profile events that are severe and widely disseminated. To generate cross-sectional variation, we augment Eq. (1) by adding *Other Event* as an additional independent variable as follows:

$$Underreport_{i,t} = \alpha + \beta_1\, Climate\ Controversy_{i,t} + \beta_2\, Other\ Event_{i,t}$$
$$+\ Controls + Fixed\ Effects + \varepsilon_{i,t}. \qquad (4)$$

We consider three alternative versions of *Other Event*: events that are not severe, not widely circulated, and events that are related to but not definitively tied to firm actions. Importantly, these events are climate-related, just not high-stakes enough to be included in *Climate Controversy*. We predict that stakeholders are less likely to respond to these alternative events, i.e., $\beta_1 > \beta_2$.

We report the results in columns (1)-(3) of Table 5, respectively. In all three columns, the coefficient on *Climate Controversy* continues to be statistically significant and the magnitude remains similar. For these alternative types of events, none of the coefficients are statistically significant and the economic magnitudes are close to zero. Wald tests of coefficient equality confirm that they are statistically significant from each other at conventional levels.

Whereas our existing analysis relies on event characteristics to generate variation in the likelihood of stakeholder response, we also use an alternative approach by focusing on underreporting before and after the Paris Agreement, a legally binding cross-country treaty on climate change. Drafted in late 2015 and adopted in early 2016, the Agreement aims to limit global temperature rise. Consistent with the Paris Agreement significantly increasing stakeholders' awareness of climate matters, Bolton and Kacperczyk (2023) show that investors are more likely to price carbon-transition risk after 2015. We partition *Climate Controversies* into two indicators, one for events prior to 2015, and another for events after 2015. Column (4) shows that firms are more likely to underreport in the post-Paris Agreement years. Together, the findings in Table 5 are consistent with underreporting being a possible response to stakeholder backlashes.

4.2 Contractual vs. Reputational Incentives

Our next set of tests explores the sources of underreporting incentives. Research in corporate finance and accounting highlights contractual and reputational forces as the two primary drivers of managerial behavior (Edmans et al., 2023). In our setting, both reputational and contractual pressures can motivate intentional underreporting. These incentives are not mutually exclusive—climate controversies that cause reputational harm may also trigger negative market reactions. Accordingly, our analysis focuses on identifying their presence.

We modify Eq. (1) by partitioning *Climate Controversy* into two sets of non-overlapping indicators: events with high versus low reputational risk exposure, and events with large versus small stock market reactions. To capture contractual incentives, we classify events based on whether the cumulative stock return in the [–5, +10] day window around the event is above or below the sample median. To assess reputational incentives, we partition events based on whether the percentage change in the RepRisk Reputation Index (RRI) over the same window is above or below the median. We select this window to align with the event-study horizon in Figure 2, though our results are not sensitive to the choice of window.

We present the results in Table 6. Column (1) examines whether underreporting following climate controversies varies by reputational damage. We find stronger results among firms experiencing greater reputational exposure, suggesting that reputational concerns influence managers' decisions to underreport. In column (2), we turn to contractual incentives, focusing on potential CEO compensation losses from variable pay tied to stock performance. We find underreporting following events with both large and small market reactions, suggesting that contractual incentives alone cannot fully explain managers' behavior.

To further disentangle these effects, we partition *Climate Controversy* into four non-overlapping categories: events with above- and below-median reputational exposure changes, and within each group, events with above- and below-median stock market reactions. Column (3) shows that when reputational damage is high, underreporting occurs regardless of the magnitude of the market reaction. In contrast, when reputational exposure is low, firms underreport more following events with larger negative stock returns.[17] These patterns suggest that both reputational and contractual incentives contribute to underreporting, with reputational concerns playing a more dominant role.[18]

4.3 Carbon Accounting System Changes

Although our findings point to manipulation, it is possible that an unobservable factor simultaneously caused climate controversies and damaged the firm's carbon accounting system, which results in flawed emissions measurement and therefore innocuous underreporting. Another possibility is the application of the conservatism principle in carbon accounting. In particular, the GHG Protocol recommends that firms overstate emissions when faced with high uncertainty. If climate controversies prompt firms to improve emissions measurements, then the more precise carbon accounting system can result in a decrease in uncertainty and less overstatement of emissions. In this case, the documented underreporting result would not be driven by manipulation but rather by a mechanical decrease in managerial uncertainty.

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[17] One possible explanation is that reputational threats could be longer lasting, with a story that is difficult to control and may directly target the credibility of a firm's sustainability narrative, whereas stock market reactions may be shorter-lived. Indeed, we find (untabulated) that stock price slowly reverses to the pre-event level in 3 months.

[18] In an untabulated analysis, we also examine the role of linking CEO pay to climate performance. Searching for keywords such as "emissions," "climate," and "carbon," we identified 30 firm-years with such metrics, consistent with the limited use of climate factors in Cohen et al. (2023). We find little evidence that firms are more likely to underreport when CEO pay is tied to climate metrics.

To investigate these potential explanations, we examine whether carbon accounting systems change after climate controversies using three measures. Our first measure is the number of days each firm takes to assemble environmental data and report to the CDP (*Filing Lag*). In most years of our sample, we observe when firms submit their responses to the CDP and back out the preparation period. This measure is analogous to using the number of days between fiscal year-end and 10-K release dates as a proxy for internal accounting quality (Gipper et al., 2025b). Our second and third measures capture whether there are issues with underlying data gaps (*Data Gaps*) and the number of issues related to reporting carbon emissions (*Issues*). Both measures come from CDP and are available in some years of the sample because CDP questionnaires vary by year. As we show in Table 7, we find no evidence that carbon accounting systems deteriorate nor improve after climate controversies. This finding lends support to the manipulation interpretation of underreporting.

4.4 External Assurance as a Signaling Device

For underreporting to be effective, firms must make their disclosed emissions appear credible to external audiences. A natural mechanism is third-party carbon assurance, which prior research argues can serve as a voluntary signal of reporting credibility and commitment to sustainability (Gipper et al., 2025a; Peters and Romi, 2015). If firms use assurance strategically to bolster the believability of understated emissions, we should observe greater assurance uptake following climate controversies. Consistent with this possibility, Figure 3 Panel D and Table IA-3 show that firms become more likely to obtain carbon assurance after such events.

To provide further evidence that firms use voluntary assurance opportunistically, we examine whether underreporting behavior differs for firms with and without assurance. Our analysis is motivated by prior research showing that carbon assurance can improve the quality of emissions measurement when firms seek higher reporting accuracy (Gipper et al., 2025b), and that improved measurement of emissions is essential in corporate decarbonization (Downar et al., 2021). Furthermore, the financial audit is important in constraining financial misreporting (DeFond and Zhang, 2014). If firms genuinely seek to improve their environmental performance, we expect firms with assurance to underreport less.

To do so, we modify Eq. (1) by partitioning *Climate Controversy* into non-overlapping indicators for firms with and without carbon assurance. Because our assurance data ends in 2020, we first re-estimate Eq. (1) using a subsample of firm-years without missing assurance data in Table 8 column (1) and confirm that our main finding continues to hold in this subsample. More importantly, in column (2), we find no difference in underreporting for firms with and without assurance, consistent with firms intentionally adopting assurance to improve the credibility of underreported emissions figures. This is possible because carbon assurance is largely voluntary, and the third-party assuror is often hired by the CFO, controller, or public relations teams, which are subordinate to the CEO with limited board oversight. Therefore, while voluntary assurance can be informative when firms seek genuine reporting improvements, its ability to curb manipulation appears limited in settings where managerial incentives favor understating emissions.[19]

5. Alternative Setting: Restatements After Organizational Boundary Changes

Thus far, our analysis establishes that firms intentionally underreport their emissions but is

[19] It is possible that some firms seek assurance to genuinely improve their emissions measurement and environmental performance after climate controversies. Therefore, we test whether the role of assurance differs with board oversight. We expect assurors hired by these firms to be more independent and less influenced by managers. Using Refinitiv, we identify firms with a sustainability board committee and split the assurance group into firms with and without such a committee. As we show in Table IA-5, firms with such a committee and assurance are not more likely to underreport after climate controversies, whereas firms without board oversight continue to underreport despite obtaining assurance. This pattern is consistent with pooling across two types of firms: those that use assurance primarily as a signaling or reputation tool that complements misreporting, and those that embed assurance in governance, analogous to financial audits. Because outsiders cannot easily distinguish these types, voluntary carbon assurance may exhibit a lemons problem.

silent on *how* firms misreport. This is an inherent limitation because assumptions and input parameters are rarely disclosed in a large sample. In our last analysis, we leverage a unique feature of the GHG Protocol and showcase how managers can manipulate emissions reporting in an alternative setting.

One of the fundamental principles of the GHG Protocol is "consistency," which emphasizes comparability of emissions numbers within-firm over time. To implement the consistency principle, the GHG Protocol requires reported emissions to be benchmarked against previous years so that stakeholders can track a firm's emissions over time. To do so, firms need to establish a "base year." Base year emissions are rarely included in firms' sustainability reports. Instead, firms report emissions in the current and most recent previous year, just like how firms report current and previous year numbers in financial statements. See Figure 4 for two examples from Apple and Estee Lauder's sustainability reports.

According to the GHG Protocol, a firm can restate its historical emissions. Emissions restatements are quite different from financial statement restatements and can come from three sources: (1) major calculation errors or a series of smaller errors that are collectively material, (2) improvements in calculation methodologies, and (3) changes in the organizational boundary (i.e., (1) is similar to a financial statement restatement while (2) and (3) are not). When mergers, acquisitions (M&A), or divestiture occur in a firm, its organizational boundary changes. The reporting firm then needs to update its historical emissions to ensure within-firm comparability.

Firms' restatement incentives likely vary. On the one hand, firms have an incentive to follow the GHG Protocol and restate emissions after M&A because such restatements allow firms to justify why their emissions have gone up after M&A. Such a restatement would reduce an *increasing* trend in emissions. On the other hand, firms may have an incentive to not restate historical emissions after divestments because divestments reduce reported emissions in current and future periods. Such a restatement would reduce a *decreasing* trend in emissions. If the same firm restates after M&A but does not restate after divestment, this asymmetric carbon accounting behavior is likely evidence of intentional misreporting.

To test this conjecture, we combine corporate restructuring data from SDC Platinum with carbon reporting data from the CDP, and estimate the following regression equation:

$$Restatement_{i,t} = \alpha + \beta_1\, M\&A_{i,t} + \beta_2\, Divest_{i,t} + Controls + Fixed\ Effects + \varepsilon_{i,t}. \quad (6)$$

The dependent variable, *Restatement*, is an indicator variable equal to one if a firm *i* restates any of its historical emissions numbers in year *t*.[20] *M&A* and *Divest* are either indicators or the natural logarithm of the dollar value of mergers and divestitures for firm *i* in year *t*. The control variables are identical to the ones we use in other tests, including firm size, ROA, book-to-market, leverage, loss indicator, and age. We include firm and year fixed effects. With firm fixed effects, we exploit how within-firm intertemporal variations in mergers and divestitures lead to different restatement decisions. Our coefficients of interest are on *M&A* and *Divest*. We predict a positive and significant β_1 and a small, statistically insignificant, and potentially negative β_2. That is, relative to years without any corporate structural changes, we examine whether the same firm is more likely to restate historical emissions in years with mergers and less likely to restate historical emissions in years with divestitures.

We find evidence consistent with this conjecture in Table 9. In columns (1) and (2), we measure M&A and divestments using either indicators or the natural logarithm of deal value. The

[20] We note that the CDP-based restatement measure understates the true frequency of restatements because CDP limits firms to reporting up to four years of historical emissions and such disclosure is not mandatory. Measures based on sustainability reports show higher rates of restatements (e.g., Cohen et al., 2025). Nonetheless, our measure is informative: it is positively correlated with assurance (Gipper et al., 2025b) and consistent with evidence in Ballou et al. (2018) and Maroun (2019).

coefficient on *M&A* is consistently positive and significant, suggesting that firms are more likely to restate historical emissions after M&A, possibly to justify why their emissions go up. The coefficient on *Divest* is consistently negative, albeit insignificant, suggesting that firms do not restate historical emissions after divestitures, possibly to signal decarbonization. However, one concern with this finding is that M&As are generally larger than divestments, so restatement decisions could be driven by deal size differences. To mitigate this concern, we redefine M&A to either exclude mergers of equals or only keep the M&A deal size smaller than 20% of the acquiror's asset. As we show in columns (3) and (4), our findings continue to hold. Together, this analysis provides further evidence of intentional misreporting.

6. Conclusion

This study examines whether firms intentionally underreport their emissions. Focusing on disclosures in the aftermath of high-profile climate controversies, we find evidence consistent with event-driven underreporting. To strengthen the case for intentional misreporting, we show that underreporting is more pronounced when firms do not undertake real decarbonization actions, when opportunities for manipulation are greater, and when firms face heightened stakeholder scrutiny. Additional analyses indicate that reputational concerns are an important driver of this behavior. Finally, we show that firms break carbon accounting rules by selectively restating historical emissions after mergers and acquisitions but not after divestments to show more favorable emissions trends. Collectively, these findings point to intentional misreporting of emissions rather than innocuous reporting errors.

Our study is not without limitations. We do not observe firms' internal reporting inputs (e.g., conversion factors), which prevents us from pinpointing the precise mechanisms managers may use to underreport. Moreover, our inferences rely on an emissions benchmark and residual measure that contain measurement error because true emissions are unobservable. If these errors are largely unrelated to the timing of climate controversies and our cross-sectional split variables, they mainly add noise and attenuate estimated responses, making our estimates conservative. Systematic benchmark errors correlated with controversies or specific firm types would be more problematic, but we mitigate this concern with consistent results in cross-sectional tests and with the asymmetric M&A versus divestiture restatement evidence, which speaks to reporting behavior without relying on the benchmark. This limitation also opens up opportunities for future research to study underreporting using data closer to firms' reporting choices. Finally, our analysis focuses on underreporting and does not address the possibility of intentional overreporting, e.g., inflating baseline emissions before adopting reduction targets to demonstrate subsequent progress ("cookie jar carbon accounting"). Exploring these dimensions, and the role of regulation in constraining misreporting, offers fruitful avenues for future research.

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Appendix A. Examples of Climate Controversies

Panel A: Exxon Mobil Case Study

Event: Exxon Mobil to increase annual carbon dioxide emissions by 2025

Date: October 5, 2020.

Source: Bloomberg

Synopsis: Leaked internal documents from Exxon Mobil have reportedly revealed the company's plans to invest USD 210 billion to increase oil production by 2025. Apparently, the move would contradict international efforts to slow global warming, as well as its own targets to reduce pollution and methane emissions. The company would reportedly emit 143 million tons of carbon dioxide equivalent annually, representing a yearly increase of 21 million metric tons, once its investment plan is initiated. It is also estimated that if Scope 3 emissions are also accounted for, the full climate impact of Exxon Mobil's growth plan is likely to be 100 million tons of additional carbon dioxide. In 2019, it was estimated that Exxon Mobil's total carbon dioxide emission, which includes Scope 3 emissions, was 528 million metric tons.

Aftermath: In the subsequent proxy season (2021), an activist investment management firm (Engine No. 1) started a proxy fight. BlackRock, Vanguard, and State Street voted against Exxon's leadership and installed three directors nominated by Engine No. 1 to the board.

On January 21, 2024, Exxon filed a lawsuit in the Fifth Circuit Court in Texas suing two shareholders, Arjuna Capital and FollowThis. The two shareholders filed a proposal requesting that the company do more to reduce greenhouse gas emissions.

October 5, 2020 (Bloomberg)

Exxon's Plan for Surging Carbon Emissions Revealed in Leaked Documents

Internal projections from one of world's largest oil producers show an increase in its enormous contribution to global warming

December 7, 2020 (Reuters)

Exxon faces proxy fight launched by new activist firm Engine No. 1

May 27, 2021 (The Economist)

Business | The little Engine that could

ExxonMobil loses a proxy fight with green investors

An activist hedge fund succeeds in nominating at least two climate-friendly directors to the energy giant's board

Appendix A. Examples of Climate Controversies—Continued

Panel B. Additional Examples

Event: EPA identifies Southern Company coal plants as largest emitters of greenhouse gases

Date: January 12, 2012

Source: EPA

Synopsis: The Environmental Protection Agency unveiled a new website that identifies most of the nation's biggest emitters of carbon dioxide, methane and other greenhouse gases. Two Southern Company coal-fired electric generating plants near Atlanta are identified as the biggest contributors to U.S. global greenhouse gases, with a third Southern plant in Alabama identified as the third-biggest emitter.

Event: Google workers call on company to adopt aggressive climate plan

Date: November 4, 2019

Source: The Guardian

Synopsis: More than 1,000 Google workers have signed a public letter calling on their employer to commit to an aggressive "company-wide climate plan" that includes canceling contracts with the fossil fuel industry and halting its donations to climate change deniers.

Event: Boeing jets emissions data highlights industry's green challenge

Date: July 26, 2021

Source: Reuters

Synopsis: Commercial jets delivered by Boeing Co last year will account on average for emissions equivalent to 1 million tonnes of carbon dioxide each over their 20-year-plus lifespans, a new report from the planemaker shows. The report sheds further light on the task facing global aviation as it faces calls by environmental groups for curbs to air travel and growing political pressure to cut emissions, after Europe's Airbus issued similar data in February.

Event: McDonald's net zero carbon restaurant prompts greenwashing backlash

Date: December 10, 2021

Source: Sky News (concurrent coverage of McDonald's greenwashing by Bloomberg and The Guardian)

Synopsis: McDonald's has today opened its first net zero restaurant, but campaigners are unconvinced by the company's environmental claims. Anna Jones from Greenpeace UK said: "If meat and dairy are still the main course on McDonald's menu, then this new restaurant initiative can only be labelled as it is: McGreenwash."

For the past decade, McDonald's has vowed to address the planet-warming problem behind its most popular menu item. In October 2021, it announced it would zero out its entire climate footprint by 2050. But an in-depth examination of the company's headway — including a review of its beef sustainability pilot projects as well as dozens of interviews with current and former McDonald's executives, cattle ranchers, industry experts and scientists — shows that the world's biggest hamburger chain so far hasn't reduced the climate impact of its beef.

Appendix B. Variable Definitions

B1. Variable Definitions

This table presents variable definitions for our empirical tests.

Variable	Definition
	Climate Controversy Variables
Climate Controversy	An indicator variable equal to one if a firm has at least one severe or high publicity climate controversy in the last four years, and zero otherwise. We identify such controversies using RepRisk, restricting attention to events classified under "Climate change, GHG emissions, and global pollution" or tagged with "Greenhouse gas (GHG) emissions." An event qualifies only if it has severity = 3 or reach = 3, the highest category in the RepRisk scale.
Low-reach Controversy	An indicator variable equal to one if a firm has at least one low publicity (reach = 1 or 2) climate controversy in the last four years, and zero otherwise.
Non-severe Controversy	An indicator variable equal to one if a firm has at least one non-severe (severity = 1 or 2) climate controversy in the last four years, and zero otherwise.
Pre-Paris Controversy	An indicator variable equal to one for *Climate Controversy* prior to 2016.
Unsharp Controversy	An indicator variable equal to one if a firm has at least one unsharp climate controversy in the last four years, and zero otherwise. Unsharp controversies are those which the firm is mentioned but the criticism is not precisely defined and definitely attributable to the firm's wrongdoing.
	Underreporting Variables
Underreport	The difference between a firm's reported emissions and the model estimated emissions, multiplied by -1. Reported emissions come from Trucost Environmental. Model estimated emissions come from the Gipper et al. (2025) model. The difference is multiplied by -1 so that higher values indicate greater degrees of underreporting.
$Underreport^{Ind}$	An indicator variable equal to one if *Underreport* is positive, and zero otherwise.
$Underreport^{Trunc}$	Equal to *Underreport* when *Underreport* is greater than zero, and zero otherwise.
	Firm Characteristics
Firm Age	The natural logarithm of firm age, from Compustat.
Book to Market	The ratio of the book value of equity to the market value of equity, measured at the fiscal year-end, from Compustat.
Leverage	The ratio of total liabilities to total equity, measured at the fiscal year-end, from Compustat.
Loss	An indicator variable equal to one when basic earnings per share excluding extraordinary items from Compustat is less than zero, and zero otherwise, from Compustat.
ROA	Earnings before extraordinary items divided by average total assets, from Compustat.
Size	The natural logarithm of the total assets measured at fiscal year-end, from Compustat.
	Decarbonization Variables
Asset Renewal	An indicator variable equal to one if a firm's asset age is less than the previous year and its capital expenditure is above the industry-year median. Asset age is computed as the difference between gross property, plant, and equipment (PP&E) and net PP&E divided by annual depreciation, from Compustat.
Divestment	An indicator variable equal to one if a firm divests its assets in a year, and zero otherwise, from SDC Platinum.
Green Patents	An indicator variable equal to one if a firm files green patents in a year, and zero otherwise.
	Reaction Variables
Small (Large) RRI	An indicator variable equal to one if the percentage change in the RepRisk Reputation Index over the [-5, +10] day window around a climate controversy is below (above) the sample median.
Small (Large) Return	An indicator variable equal to one if the cumulative market-adjusted return around a climate controversy is less (more) negative than the sample median.

(continued)

Appendix B. Variable Definitions—Continued

Variable	Definition
	Indicators of Carbon Accounting System
Data Gaps	An indicator variable equal to one if a firm discloses data gap issues in their carbon accounting system, and zero otherwise.
Filing Lag	The natural logarithm of the difference between the date a firm submits its response to the CDP questionnaire and that year's CDP questionnaire release date, from CDP.
Issues	Number of issues management identified with the carbon reporting system used to generate emissions, from CDP.
Omissions	An indicator variable equal to one if a firm discloses that emission sources have been omitted from their reported emissions, from CDP.
	Manipulation Feasibility Measures
NCI	Noncontrolling interests divided by total shareholders' equity, from Compustat
Subsidiaries	Total number of subsidiaries, from WRDS Relationships
Scopes 1/3 Emissions	The ratio of Scope 1 to Scope 3 emissions, from Trucost
High Discretion Industries	An indicator equal to one for firms in the Fama–French 12 industry groups Business Equipment (7), Telecommunications (9), Utilities (10), or Other (12), and zero otherwise.
	Other Variables
Assurance	A scope-specific indicator variable equal to one if either Gipper et al. (2025a) data or CDP indicates that a firm obtains assurance over emissions for Scope 1 emissions in a year, and zero otherwise, from Gipper et al. (2025a)
Board	An indicator variable equal to one if a firm has a sustainability committee on the board, and zero otherwise, from Refinitiv.
Divestment	An indicator variable equal to one if a firm divests its assets in a year, and zero otherwise, from SDC Platinum.
Ln(*Divestment*)	The natural logarithm of the dollar value of divestitures in a year, and zero otherwise, from SDC Platinum.
Ln(*M&A*)	The natural logarithm of the dollar value of M&As in a year, and zero otherwise, from SDC Platinum.
M&A	An indicator equal to one if a firm has at least one M&A deals in a year, and zero otherwise, from SDC Platinum.
Restatement	An indicator variable equal to one if a firm revised its historical emissions number(s), and zero otherwise, from CDP.

B.2. Coding of Climate Controversy

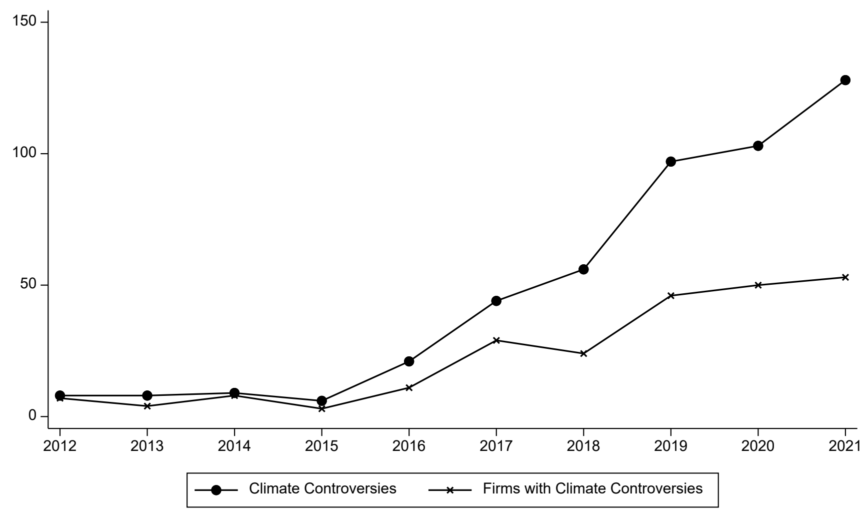
This table illustrates the coding of our *Climate Controversy* variable. The highlighted cells are the firm-years with one or more climate controversies in our sample. Firm A's climate controversy occurs in 2014. Therefore, for years 2014-2017, *Climate Controversy* is coded as one because it fits the definition of having one or more climate controversies in the current or the previous three years. Firm B has no climate controversy during the sample period, so *Climate Controversy* equals zero for all years. Firm C has two climate controversies, one in 2015, and one in 2017. *Climate Controversy* equals one for 2015-2018 because of the controversy in 2015 and equals one for 2017-2019 because of the controversy in 2017. In addition, although our sample begins in 2012, we have available RepRisk data dating back to 2009, allowing us to fully capture the controversies occurring prior to the beginning of the sample period.

	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021
Firm A	0	0	1	1	1	1	0	0	0	0
Firm B	0	0	0	0	0	0	0	0	0	0
Firm C	0	0	0	1	1	1	1	1	1	0

Figure 1. Time Trends in Climate Controversies

This figure presents time trends of climate controversies in our sample. Panel A reports the number of climate controversies in a year and the number of firms with climate controversies. Panel B reports the number of controversies with high reach and with high severity separately.

Panel A. Climate Controversies



Panel B. Reach and Severity

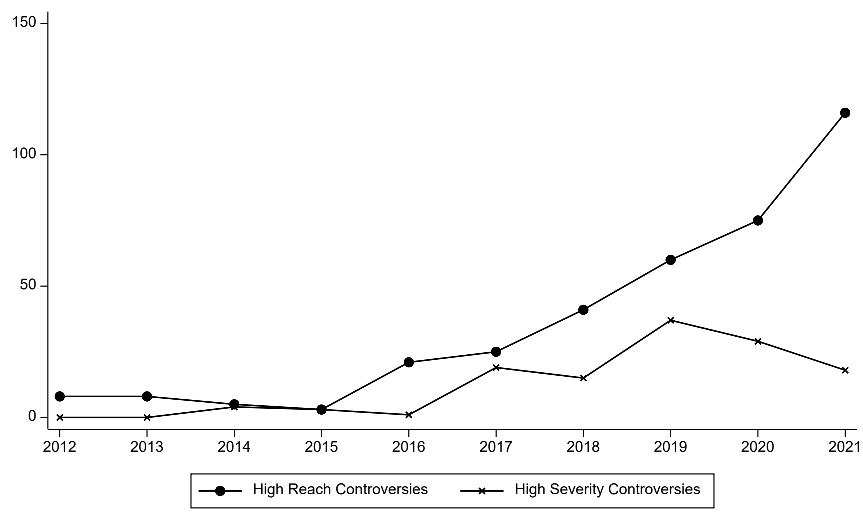
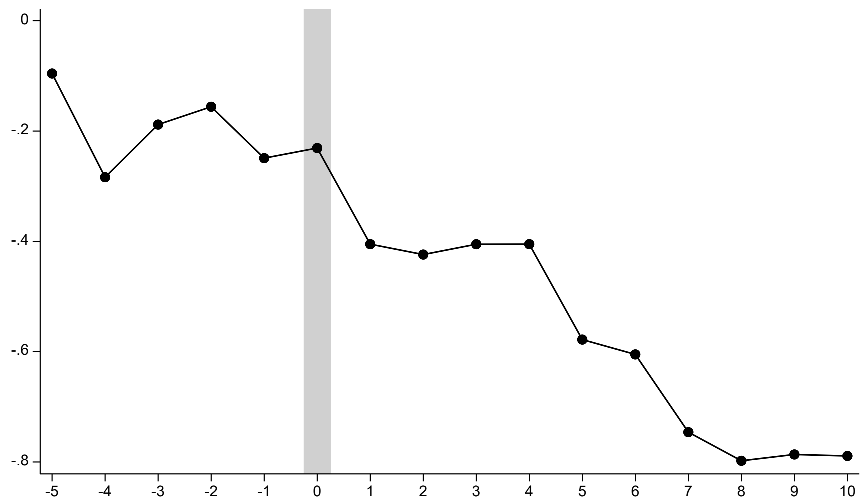


Figure 2. Stakeholder Reactions to Climate Controversies

This figure presents stakeholder reactions to climate controversies in the [-5, 10] day event window for all climate controversies in our sample, with day 0 being the date of each climate controversy. Panel A reports the average cumulative abnormal return (in percentages). Cumulative abnormal returns are calculated as the sum of daily abnormal returns using a market model. Panel B reports the average RepRisk Index (RRI). RRI is a daily measure of firms' reputational risk exposure and ranges from 0 to 100, with higher numbers indicating more risk exposure.

Panel A. Cumulative Abnormal Return



Panel B. RepRisk Index

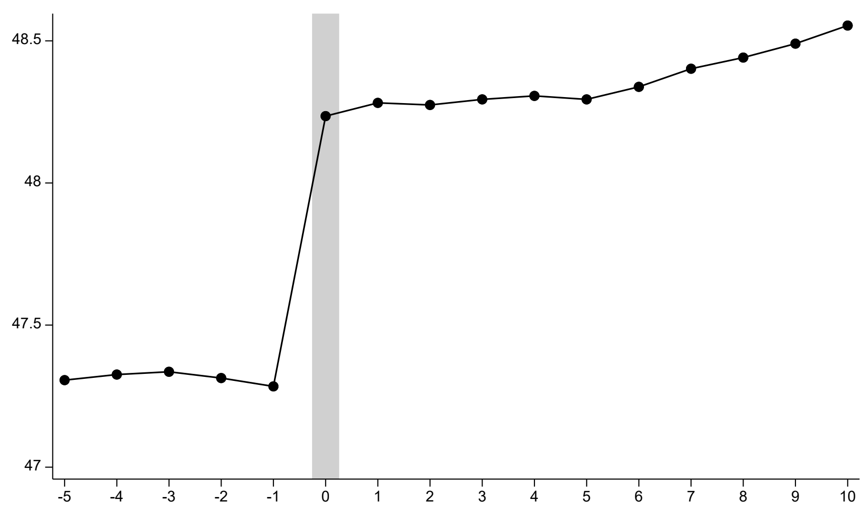


Figure 3. Event Time Analysis

This figure reports underreporting and assurance adoption around climate controversies in event time. Because firms in our sample can have multiple climate controversies, we only consider the first severe, high-profile climate controversy from each firm for this analysis. For each year in the sample, firms experiencing a controversy in that year are designated as treated firms, while firms without any climate controversies during the sample period serve as control firms. We retain observations in a seven-year window from t−3 to t+3 around each controversy. We then stack these cohorts and estimate stacked cohort difference-in-differences regressions per Cengiz et al. (2019) and Baker et al. (2022). Event cohorts occurring in the first and last two years of the sample are excluded to ensure the availability of both pre- and post-event observations. The controversy year is denoted as t=0. All coefficients are measured relative to year t=−3. Coefficient estimates (dots), along with 90% confidence intervals (bars), are shown for each year.

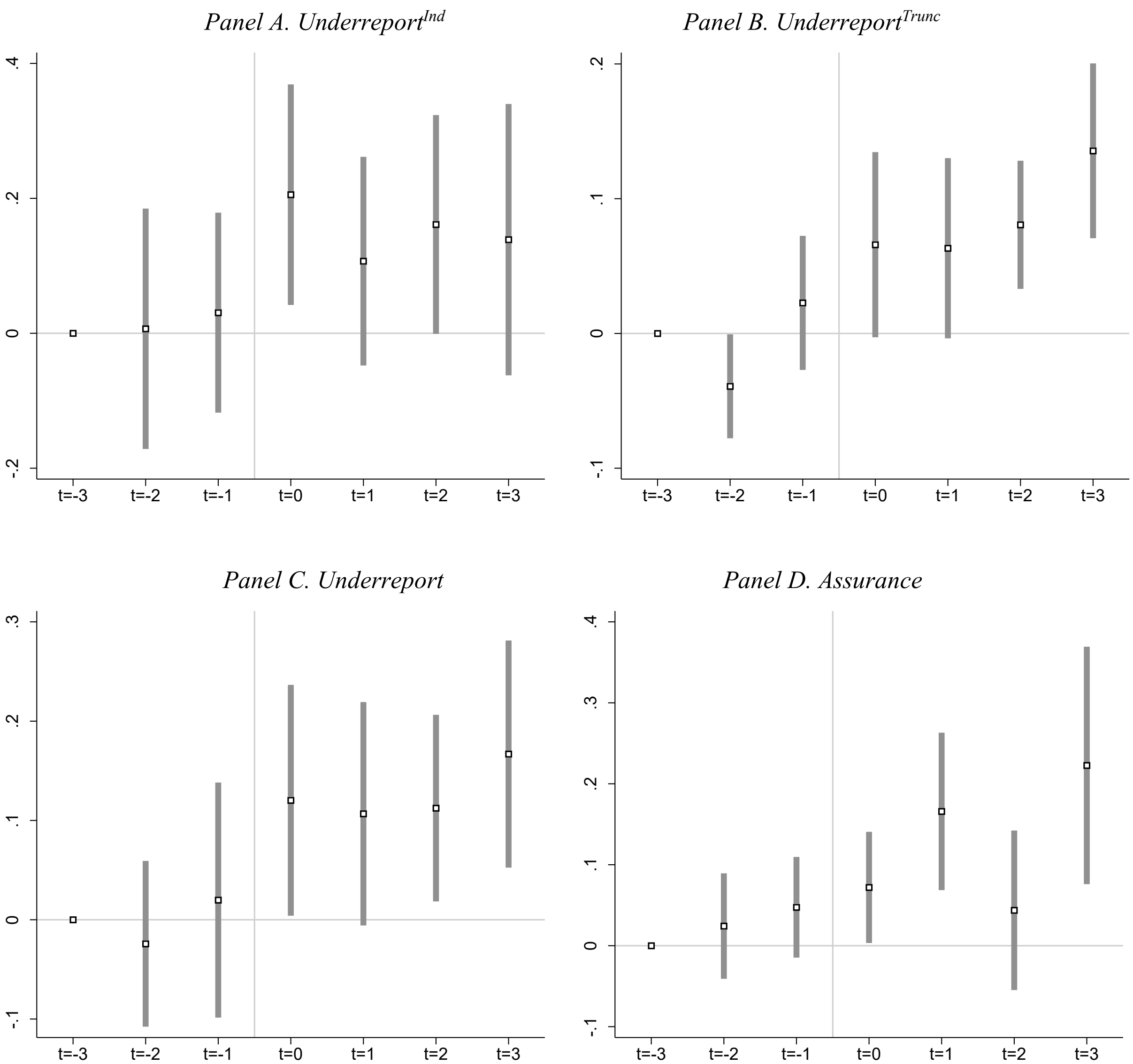


Figure 4. Examples of Corporate Carbon Reporting

This figure presents examples of carbon emissions reporting in corporate ESG reports. The examples in Panels A and B are Apple and Estee Lauder, respectively.

Panel A. Apple 2024 Environmental Progress Report

		Fiscal year	
		2023	**2022**
Corporate emissions (metric tons CO_2e)[2]	**Gross emissions**	**324,100**	**324,000**
	Scope 1	**55,200**	**55,200**
	Natural gas, diesel, propane	35,300	39,700
	Fleet vehicles	17,000	12,600
	Other emissions[3]	2,900	2,900
	Scope 2 (market-based)[4]	**3,400**	**3,000**
	Electricity	0	0
	Steam, heating, and cooling[5]	3,400	3,000
	Scope 3	**412,800**	**265,800**
	Business travel	225,700	113,500
	Employee commute[6]	164,100	134,200
	Upstream impacts (scope 1)	18,300	10,600
	Work from home (market-based)	4,700	7,500
	Transmission and distribution loss (market-based)	N/A	N/A
	Third-party cloud (market-based)	0	0
	Carbon removals		
	Corporate carbon offsets[7]	-471,400	-324,100[8]

Panel B. Estee Lauder 2023 Social Impact & Sustainability Report

		FY23	FY22
GREENHOUSE GAS (GHG) EMISSIONS: SCOPE 1 AND 2 (THOUSAND METRIC TONS CO_2 EQUIVALENT EXCEPT FOR PERCENTAGES)	Scope 1[19]	29.5*	27.8+
	Scope 2 Market-based[20]	1.3*	1.3+
	Scope 2 Location-based[20]	60.2*	54.8+
	GHG intensity (normalized to million dollars of net sales)[21]	0.0*	0.0+
	% Carbon Neutral	100%	100%
	% Scope 1 and 2 reduction[22]	51%	54%
	Reduction of emissions due to conservation and efficiency measures[23]	0.7*	0.7+

Table 1. Descriptive statistics

This table summarizes key variables and sample composition. Panel A reports descriptive statistics for the key variables in our sample. Panel B compares the distribution of firms in our sample with Compustat. Definitions of all variables are as described in Appendix B.

Panel A. Summary Statistics

	(1) Mean	(2) SD	(3) 25th	(4) 50th	(5) 75th
Firm Age	3.78	0.60	3.40	3.85	4.29
Book to Market	0.55	0.56	0.21	0.40	0.71
Climate Controversy	0.08	0.28	0.00	0.00	0.00
Leverage	0.64	0.20	0.51	0.64	0.77
Loss	0.15	0.36	0.00	0.00	0.00
ROA	0.04	0.08	0.01	0.04	0.08
Size	9.80	1.49	8.74	9.74	10.84
Underreport	-0.01	0.52	-0.21	-0.02	0.16
$Underreport^{Ind}$	0.47	0.50	0.00	0.00	1.00
$Underreport^{Trunc}$	0.15	0.36	0.00	0.00	0.16

Panel B. Industry Distribution

Fama French 12 Industry Classification	(1) Sample	(2) Compustat
Business Equipment (e.g., Computer, Software, Electronic Equipment)	15.70%	16.23%
Other (e.g., Equipment, Bus. Service)	15.47%	13.42%
Manufacturing	13.17%	7.85%
Finance	10.39%	22.99%
Consumer Non-Durables	7.65%	3.65%
Utilities	7.06%	2.10%
Oil, Gas, and Coal Extraction and Products	6.63%	4.33%
Healthcare, Medical Equipment, and Drugs	5.98%	15.54%
Chemicals and Allied Products	5.81%	2.14%
Wholesale, Retail, and Some Services	5.55%	7.25%
Telephone and Television Transmission	3.67%	2.36%
Consumer Durables	2.92%	2.14%

Table 2. Climate Controversies and Emissions Underreporting

This table presents an analysis of whether firms underreport in the four years following climate controversies using three alternative measures of underreporting. Definitions of all variables are as described in Appendix B. t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)
		Dependent Variable:	
	$Underreport^{Ind}$	$Underreport^{Trunc}$	*Underreport*
Climate Controversy	0.061*	0.050***	0.069***
	(1.82)	(3.04)	(2.82)
Controls:			
Size	-0.023	-0.061**	-0.106***
	(-0.72)	(-2.56)	(-2.75)
Firm Age	0.278**	0.097	0.248**
	(2.38)	(1.44)	(2.29)
ROA	-0.174	-0.078	-0.164
	(-1.28)	(-0.68)	(-0.89)
Book to Market	-0.031	-0.017	-0.024
	(-1.01)	(-0.85)	(-0.82)
Leverage	-0.013	-0.065	-0.145
	(-0.14)	(-0.75)	(-1.19)
Loss	0.022	0.031	0.022
	(0.81)	(1.29)	(0.63)
Firm, Year Fixed Effects	Yes	Yes	Yes
Observations	5,370	5,370	5,370
R-squared	0.364	0.342	0.318

Table 3. Costly Decarbonization Activities

This table presents analyses of firms' decarbonization activities and underreporting around climate controversies. Panel A tests whether firms are more likely to decarbonize in the four years following climate controversies by regressing measures of decarbonization activities on *Climate Controversy*. Panel B tests whether firms that decarbonize after climate controversies are less likely to underreport. Definitions of all variables are as described in Appendix B. All regressions include firm size, book-to-market, leverage, loss indicator, return on assets, and firm age as control variables, as well as firm, year fixed effects. p-values are derived using Wald tests of coefficient equality. t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A. Climate Controversies and Decarbonization

	(1)	(2)	(2)
		Dependent Variable:	
	Divestment	*Asset Renewal*	*Green Patents*
Climate Controversy	0.020	-0.033	-0.007
	(0.80)	(-1.02)	(-0.26)
Controls	Yes	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes	Yes
Observations	5,370	5,370	5,370
R-squared	0.284	0.308	0.702

Panel B. Substitution Between Underreporting and Decarbonization

	(1)	(2)	(3)
	Dependent Variable: *Underreport*		
Action =	Divestment	Asset Renewal	Increased Green Innovation
[1] *Action = 0 × Climate Controversy*	0.083***	0.090***	0.088***
	(3.17)	(3.36)	(3.46)
[2] *Action = 1 × Climate Controversy*	-0.021	-0.006	0.009
	(-0.51)	(-0.17)	(0.20)
Pr([1]>[2]) p-value	0.010	0.004	0.046
Control for *Action*	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes	Yes
Observations	5,370	5,370	5,370
R-squared	0.319	0.319	0.319

Table 4. Variation in Manipulation Easiness

This table reports an analysis of whether firms are more likely to underreport after climate controversies when manipulation is easier to carry out. Definitions of all variables are as described in Appendix B. All regressions include firm size, book-to-market, leverage, loss indicator, return on assets, and firm age as control variables, as well as firm, year fixed effects. p-values are derived using Wald tests of coefficient equality. t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)
	Dependent Variable: *Underreport*			
Easy =	Above Median Subsidiaries	Above Median Noncontrolling Interests	High Discretion Industries	Above Median Scopes 1 / 3 Emissions
[1] *Easy = 1× Climate Controversy*	0.089***	0.098***	0.140***	0.113***
	(2.94)	(2.93)	(3.63)	(3.68)
[2] *Easy = 0 × Climate Controversy*	0.026	0.042	0.047*	0.017
	(0.88)	(1.41)	(1.69)	(0.51)
Pr([1]>[2]) p-value	0.049	0.083	0.021	0.013
Control for *Easy*	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	5,370	5,370	5,370	5,370
R-squared	0.319	0.319	0.319	0.335

Table 5. Variation in Stakeholder Response Likelihood

This table presents an analysis of whether underreporting after climate controversies varies with the likelihood of stakeholder responses. Definitions of all variables are as described in Appendix B. All regressions include firm size, book-to-market, leverage, loss indicator, return on assets, and firm age as control variables, as well as firm, year fixed effects. p-values are derived using Wald tests of coefficient equality. t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)
	Dependent Variable: *Underreport*			
[1] *Climate Controversy*	0.069***	0.069***	0.076***	0.082***
	(2.84)	(2.83)	(3.32)	(3.09)
Other Types of Events:				
[2] *Non-severe Controversy*	-0.006	-	-	-
	(-0.22)			
[2] *Low-reach Controversy*	-	-0.004	-	-
		(-0.15)		
[2] *Unsharp Controversy*	-	-	-0.015	-
			(-0.46)	
[2] *Pre-Paris Controversy*	-	-	-	0.017
				(0.48)
Pr([1]>[2]) p-value	0.027	0.026	0.020	0.039
Controls	Yes	Yes	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	5,370	5,370	5,370	5,370
R-squared	0.318	0.318	0.318	0.319

Table 6. Contractual vs. Reputational Incentives

This table presents results from an analysis of whether underreporting after climate controversies varies with the stock market reaction (*Return*) and change in reputational exposure (*RRI*) around the controversy. Definitions of all variables are as described in Appendix B. All regressions include firm size, book-to-market, leverage, loss indicator, return on assets, and firm age as control variables, as well as firm, year fixed effects. p-values are derived using Wald tests of coefficient equality. t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)
	Dependent Variable: *Underreport*		
[1] *Climate Controversy _ Small RRI*	0.036	-	-
	(1.35)		
[2] *Climate Controversy _ Large RRI*	0.113***	-	-
	(3.63)		
[1] *Climate Controversy _ Small Return*	-	0.067**	-
		(2.03)	
[2] *Climate Controversy _ Large Return*	-	0.071***	-
		(2.89)	
Climate Controversy _ Small RRI Small Return	-	-	0.020
			(0.58)
Climate Controversy _ Small RRI Large Return	-	-	0.057**
			(1.97)
Climate Controversy _ Large RRI Small Return	-	-	0.138***
			(2.86)
Climate Controversy _ Large RRI Large Return	-	-	0.088***
			(3.10)
Pr([1]<[2]) p-value	0.009	0.447	-
Controls	Yes	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes	Yes
Observations	5,370	5,370	5,370
R-squared	0.319	0.318	0.319

Table 7. Alternative Explanation: Carbon Accounting System Changes

This table presents analyses of whether a firm's carbon accounting systems change after climate controversies. Definitions of all variables are as described in Appendix B. All regressions include firm size, book-to-market, leverage, loss indicator, return on assets, and firm age as control variables, as well as firm, year fixed effects. t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)
		Dependent Variable:	
	Filing Lag	*Data Gaps*	*Issues*
Climate Controversy	0.002	-0.002	0.156
	(0.17)	(-0.03)	(0.99)
Controls	Yes	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes	Yes
Observations	2,964	1,656	1,656
R-squared	0.872	0.828	0.840

Table 8. Voluntary Carbon Assurance and Emissions Underreporting

This table presents an analysis of whether voluntary carbon assurance is associated with less emissions underreporting. Assurance data comes from Gipper et al. (2025b) and ends in 2020. Column (1) reexamines the association between *Climate Controversy* and *Underreport* (i.e., the analysis in column 3 of Table 2) using a subsample of firms with non-missing assurance data. Column (2) examines whether the association between climate controversies and underreporting varies with assurance. t–statistics appear in parentheses and are clustered by firm. p-values are derived using Wald tests of coefficient equality. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)
	Dependent Variable: *Underreport*	
Climate Controversy	0.078***	-
	(2.78)	
[1] *Assurance = 0 × Climate Controversy*	-	0.085*
		(1.80)
[2] *Assurance = 1 × Climate Controversy*	-	0.077**
		(2.50)
Pr([2]>[1]) p-value	-	0.442
Control for *Assurance*	N/A	Yes
Controls	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes
Observations	4,567	4,567
R-squared	0.322	0.322

Table 9. Alternative Setting: Organizational Boundary Changes

This table presents results from an analysis of whether firms' tendency to restate carbon emissions varies by whether firms have mergers or divestitures. t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)
	Dependent Variable: *Restatement*			
M&A	0.028***	-	0.030***	0.029***
	(3.16)		(3.35)	(3.37)
Divestment	-0.003	-	-0.002	-0.004
	(-0.36)		(-0.25)	(-0.43)
Ln(*M&A*)	-	0.028***	-	-
		(2.78)		
Ln(*Divest*)	-	-0.007	-	-
		(-0.54)		
Sample	Full	Full	Excluding Mergers	M&A Deal Size < 0.2*Assets
Controls	Yes	Yes	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,186	4,186	4,186	4,186
R-squared	0.405	0.405	0.406	0.406

Do Firms Manipulate Their Carbon Emissions Reporting?

Internet Appendix

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Table of Contents

Table IA-1. Main Result Robustness

This table presents results from various robustness and falsification tests. Specifically, we estimate variants of Eq. (1), with changes to the dependent variable, the independent variable, or the fixed effect structure. The changes are described in the leftmost column, and the coefficient (t-statistics) of *Climate Controversy* (or its variations) is reported in columns (1), (2), and (3). In modification [1], we reconstruct the dependent variables using a naïve model that includes only firm sales and lagged emissions. In modifications [2]-[4], instead of measuring *Climate Controversy* using a 4-year window, we consider 2-year and 3-year windows, as well as setting the variable equal to 1 for all years after the firm's first climate controversy. In modifications [5] and [6], we reconstruct the dependent variables using two non-linear, machine learning approaches (Neutral Networks, Random Forest). In modification [7], we replace year fixed effects with industry-year fixed effects. In modifications [8] and [9], we perform falsification tests by replacing *Climate Controversy* with non-emissions-related environmental controversies (*Non-climate Environmental Controversy*) and controversies related to social issues (*Social Controversy*). t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)
	Coefficient on *Climate Controversy*		
Dependent Variable:	$Underreport^{Ind}$	$Underreport^{Trunc}$	*Underreport*
Modification:			
Simple Model of Benchmark Emissions			
[1] Using sales and lagged emissions as the only factors to predict expected level of emissions	0.035	0.051***	0.065**
	(1.08)	(2.55)	(2.46)
Alternative Ways to Define Treatment			
[2] *Climate Controversy* measured using a 2-year window	0.044	0.036**	0.056**
	(1.38)	(2.21)	(2.42)
[3] *Climate Controversy* measured using a 3-year window	0.078**	0.044***	0.070***
	(2.35)	(2.65)	(3.01)
[4] *Climate Controversy* = 1 for all years after the first climate controversy of the firm	0.102**	0.070***	0.106***
	(2.53)	(3.95)	(3.59)
Machine Learning Models of Benchmark Emissions			
[5] Using Neural Networks to predict expected level of emissions	0.024	0.040*	0.057**
	(0.67)	(1.87)	(2.07)
[6] Using Random Forest to predict expected level of emissions	0.078*	0.019*	0.023*
	(1.75)	(1.87)	(1.83)
Alternative Fixed Effect Structures			
[7] Including firm, industry-year fixed effects	0.065*	0.048***	0.064**
	(1.90)	(2.71)	(2.48)
Falsification			
[8] *Non-climate Environmental Controversy* as the treatment	-0.009	-0.009	-0.006
	(-0.26)	(-0.34)	(-0.17)
[9] *Social Controversy* as the treatment	-0.016	0.003	0.009
	(-0.64)	(0.11)	(0.28)
Controls	Yes	Yes	Yes
Firm, Year Fixed Effects (Except for [7])	Yes	Yes	Yes

Table IA-2. Quantile Regressions

This table presents results from estimating Eq. (1) using quantile regressions for the 1st, 20th, 40th, 60th, 80th, and 100th percentiles of *Underreport*. Definitions of all variables are as described in Appendix B. z–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: *Underreport*					
Quantile:	1st	20th	40th	60th	80th	100th
Climate Controversy	-0.016	0.043	0.058**	0.076***	0.094***	-0.016
	(-0.25)	(1.47)	(2.30)	(3.01)	(2.99)	(-0.25)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,555	5,555	5,555	5,555	5,555	5,555

Table IA-3. Stacked Cohort Difference-in-Differences Analysis

This table presents our underreporting and assurance analysis using a stacked cohort difference-in-difference design. Because firms in our sample can have multiple climate controversies, we only consider the first severe, high-profile climate controversy from each firm for this analysis. For each year in the sample, firms experiencing a controversy in that year are designated as treated firms, while firms without any climate controversies during the sample period serve as control firms. We retain observations in a seven-year window from t−3 to t+3 around each controversy. We then stack these cohorts and estimate stacked cohort difference-in-differences regressions per Cengiz et al. (2019) and Baker et al. (2022). Event cohorts occurring in the first and last two years of the sample are excluded to ensure the availability of both pre- and post-event observations. t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)
	Dependent Variable:			
	Underreport^{Ind}	*Underreport*^{Trunc}	*Underreport*	*Assurance*
Treat × Post	0.141***	0.083***	0.121***	0.088***
	(2.96)	(3.65)	(2.92)	(2.60)
Controls	Yes	Yes	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	14,340	14,340	14,340	14,513
R-squared	0.400	0.379	0.350	0.736

Table IA-4. Climate Controversies, Emissions Underreporting, and Disclosure

This table presents analyses of whether average underreporting firms disclose omissions (column 1), whether firm disclosure of carbon emissions calculation omissions changes in the aftermath of climate controversies (column 2), and whether the association between climate controversies and underreporting varies by disclosure of omissions (column 3). Definitions of all variables are as described in Appendix B. All regressions include firm size, book-to-market, leverage, loss indicator, return on assets, and firm age as control variables, as well as firm, year fixed effects. p-values are derived using Wald tests of coefficient equality. t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)
		Dependent Variable:	
	Omissions	*Omissions*	*Underreport*
Underreport	0.033*	-	-
	(1.78)		
Climate Controversy	-	-0.010	-
		(-0.26)	
[1] *Climate Controversy_ No Omissions*	-	-	0.110**
			(2.11)
[2] *Climate Controversy_ Omissions*	-	-	0.027
			(1.12)
Pr([1]>[2]) p-value	-	-	0.055
Controls	Yes	Yes	Yes
Firm, Year Fixed Effects	Yes	Yes	Yes
Observations	2,964	2,964	2,964
R-squared	0.695	0.694	0.396

Table IA-5. Voluntary Carbon Assurance and Emissions Underreporting: Variation in Board Involvement

This table presents an analysis of whether the association between climate controversies and underreporting for assurance and non-assurance firms varies with board involvement. All regressions include firm size, book-to-market, leverage, loss indicator, return on assets, and firm age as control variables, as well as firm, year fixed effects. p-values are derived using Wald tests of coefficient equality. t–statistics appear in parentheses and are clustered by firm. *, **, *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively.

	(1) Dependent Variable: *Underreport*
[1] *Climate Controversy _No Assurance*	0.035
	(0.44)
[2] *Climate Controversy _ Assurance _ Board*	-0.318
	(-1.55)
[3] *Climate Controversy _ Assurance _ No Board*	0.379*
	(1.86)
Pr([3]>[2]) p value	0.044
Lower Order Effects	Yes
Controls	Yes
Firm, Year Fixed Effects	Yes
Observations	3,101
R-squared	0.345