Rewarding Green Goals: The Effects of Environmental Incentives for CEOs

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Abstract

This paper examines whether tying CEO compensation to environmental performance reduces firms' greenhouse gas emissions. Using global firm-level data from 2015–2024, we exploit the staggered adoption of environmental bonus components and regulatory-driven variation, to estimate how emissions change after incentives are introduced. Across specifications, the estimated effects are small and statistically non-significant. To interpret this null result, we document that environmental components are typically minor relative to total compensation and rely largely on qualitative, non-verifiable criteria rather than quantified emissions targets. Consistent with weak contractibility and limited monitoring capacity, firms appear more likely to adjust disclosure and qualitative representations of performance than operational emissions. Our results suggest that ESG-linked compensation schemes, as currently designed, function more as symbolic commitments than as effective drivers of decarbonization, highlighting the need for clearer, quantitatively grounded, and verifiable metrics if pay is intended to align executives with climate goals.

Keywords: Climate policy, Executive compensation, Scope 1 emissions

JEL Codes: D22, G30, M14, Q56, Q58

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1 Introduction

As environmental, social, and governance (ESG) considerations gained prominence among investors, regulators, and the public during the late 2010s, firms increasingly incorporated sustainability metrics into executive pay. While subsequent years have seen a partial rollback of ESG initiatives, ESG-linked compensation has remained a common feature of executive pay structures (Cohen et al., 2023; Zhu et al., 2024). A growing share of chief executive officer (CEO) bonus structures now includes climate-related performance criteria, reflecting widespread expectations that compensation should incentivize credible action on climate change. Yet despite the rapid diffusion of these mechanisms, prior research has largely examined their relationship with ESG ratings, disclosure, and formal governance structures, rather than with firms' verifiable environmental outcomes. Most existing studies assess sustainability performance through ESG ratings or composite indices, which capture changes in reporting and governance but do not directly measure firms' environmental footprint. As a result, it remains unclear whether linking CEO pay to environmental criteria leads to measurable reductions in operational emissions.

The question of whether ESG-linked compensation affects real environmental outcomes is also highly relevant from a policy perspective. Together with regulatory instruments such as emissions pricing, technology mandates, and performance standards, recent climate policy increasingly relies on disclosure-based frameworks and internal governance mechanisms to steer corporate behavior. In the European Union, the Corporate Sustainability Reporting Directive (CSRD) substantially expands mandatory climate disclosures with the explicit aim of improving accountability and incentivizing operational change. These approaches rest on the implicit assumption that greater transparency and alignment of managerial incentives with sustainability objectives can induce firms to reduce emissions. Assessing whether these governance-based approaches translate into measurable emissions reductions is therefore of first-order policy importance.

To address this gap, we examine whether linking CEO compensation to environmental targets leads to measurable reductions in greenhouse gas (GHG) emissions. Using global firm-level data from the Carbon Disclosure Project (CDP) from 2015 to 2024 and three complementary identification strategies that exploit within-firm variation, diffusion in compensation practices, and policy-induced changes in disclosure requirements, we study how Scope 1 emissions evolve following the introduction of environmental bonus components in CEO pay. Across all specifications, we find no evidence of economically meaningful reductions in direct operational emissions. These results are consistent across identification strategies and sample restrictions, indicating that the absence of effects is not driven by a particular empirical design.

Rather than interpreting this null result as evidence that incentives cannot matter, we show that it reflects how environmental incentives are currently designed. We document that environmental bonus components are typically small relative to total compensation, concentrated in short term pay, and tied primarily to qualitative and non-verifiable objectives such as progress on sustainability strategy or the advancement of climate commitments, rather than to quantified emissions targets. Combined with limited technical environmental expertise at the board level, these design features weaken contractibility and monitoring, reducing the ability of compensation schemes to motivate costly operational change. In this sense, ESG linked compensation functions less as a binding incentive mechanism and more as a symbolic governance device that allows firms to signal responsiveness to stakeholder pressure without materially altering emissions trajectories.

This paper contributes to three strands of literature. First, it advances research on executive compensation by providing causal evidence that environmental incentives, as currently structured, do not affect real operational outcomes. Second, it informs debates on corporate climate responsibility by showing that the formal adoption of ESG linked pay does not translate into lower Scope 1 emissions. Third, it contributes to broader discussions on the effectiveness of ESG mechanisms by highlighting the role of incentive design, contractibility, and governance capacity in determining whether sustainability oriented compensation schemes operate substantively or symbolically.

The remainder of the paper proceeds as follows. Section 2 provides the theoretical background and a literature review. Section 3 describes the empirical strategies and data. Section 4 presents the results and section 5 details the results for scope 2 emissions. Section 6 investigates the mechanisms. Section 7 discusses the findings and Section 8 concludes.

2 Background

2.1 Theoretical Background

Several theoretical perspectives offer competing predictions about whether linking CEO compensation to environmental performance should affect firms' emissions trajectories. From a classic agency perspective, performance based incentives can align managerial actions with shareholders' long term objectives when CEOs would otherwise underinvest in emissions abatement that yields limited short run returns (Jensen and Meckling, 1976; Holmstrom, 1979). Under this view, tying compensation to environmental outcomes should motivate managers to undertake costly abatement efforts, particularly in operational domains under their direct control.

Attention based theory reinforces this mechanism by emphasizing managerial cognitive constraints and competing priorities. Issues tied to meaningful rewards receive disproportionate attention (Ocasio, 1997). Embedding environmental targets into compensation contracts can therefore elevate the salience of climate related decisions, encouraging managers to allocate time and organizational resources toward emissions reduction even when incentive weights are modest (Holmstrom and Milgrom, 1991).

However, insights from behavioral economics and signaling theory highlight important limitations. Incentives are effective only when they are sufficiently strong, clearly defined, and verifiable. Weak or poorly specified incentives may crowd out intrinsic motivation, encourage strategic behavior, or induce goal displacement rather than genuine performance improvements (Deci et al., 1999; Gneezy and Rustichini, 2000). In the environmental domain, compensation schemes based on qualitative or discretionary criteria leave substantial room for interpretation, diluting their motivational content.

From a signaling perspective, such qualitative environmental incentives resemble cheap talk. They allow firms to signal environmental commitment without incurring the costs of substantive abatement (Crawford and Sobel, 1982). Consistent with this view, empirical evidence shows that firms operating under voluntary or weak evaluation frameworks emphasize disclosure and qualitative climate actions, with limited operational change and higher emissions growth (Bingler et al., 2022, 2024). Environmental performance pay may therefore function symbolically rather than as a binding commitment.

These perspectives highlight a central tension in the design of environmental incentives. While agency and attention based theories predict that environmental bonuses can motivate emissions reductions, behavioral and signaling theories suggest that weak, qualitative, or non-verifiable measures are unlikely to induce real change. Instead, such designs may reflect an institutional response to conflicting stakeholder pressures, allowing firms to demonstrate responsiveness while preserving flexibility in core business decisions (Cho et al., 2015).

Finally, incentive effectiveness depends on the governance environment in which contracts operate. When ownership is informed, engaged, and capable of monitoring managerial behavior, performance based incentives can support complex long horizon investments. For example, Aghion et al. (2013) show that stronger institutional ownership enhances innovation incentives by disciplining managers and sustaining long term investment. By contrast, in settings with weak oversight or limited verifiability, formal incentive adoption is less likely to translate into substantive operational change.

2.2 Executive Compensation and Incentive Effectiveness

A large literature in economics examines how executive compensation shapes managerial behavior. Early work on pay-for-performance emphasizes linking CEO pay to financial outcomes to align managerial incentives with shareholder value (Jensen and Murphy, 1990). While some studies document substantial pay–performance sensitivity, for example, showing large compensation responses to firm performance and risk-taking incentives that mitigate agency conflicts (Hall and Liebman, 1998), others find more muted or unstable effects (Ataay, 2018). Boards do adjust compensation in response to performance, but these effects often decay over time and vary across governance structures, ownership regimes, and CEO characteristics (Murphy, 2013; Ozkan, 2011). Overall, the evidence suggests that financial incentives can influence managerial behavior, but their effectiveness is highly context-dependent.

Building on these mixed findings, a subsequent wave of research examines whether linking executive compensation to corporate social responsibility (CSR) can better align managerial decisions with broader stakeholder objectives. Several studies report positive effects: CSR-linked pay is associated with higher CSR performance across large international samples (Derchi et al., 2021; Ma et al., 2015), with effects that strengthen over time and, in some cases, extend to environmental outcomes (Al-Shaer et al., 2023). CSR contracting has also been linked to improved stakeholder relations and a longer-term managerial orientation, consistent with the growing prevalence of CSR criteria in executive pay (Flammer et al., 2019). However, effectiveness remains conditional. CSR incentives perform better in settings with strong shareholder oversight (Hong et al., 2015), may partly serve to justify higher equity-based compensation (Kumari and Pattanayak, 2016), and can depend on employee perceptions of managerial credibility (Li et al., 2022). Other studies document symbolic responses, such as expanded CSR policies without reductions in irresponsible behavior, and even unintended negative effects (Thompson, 2021; Fabrizi et al., 2014).

More recent work extends this logic to executive compensation schemes linked to environmental, social, and governance (ESG) metrics. Unlike earlier CSR contracts tied to explicit performance targets, ESG-linked pay typically relies on ratings-based indicators, disclosure measures, or governance milestones. Empirical studies find that ESG-linked incentives are associated with higher ESG and environmental ratings, increased climate disclosure, and the formalization of internal climate governance structures (Ikram et al., 2023; Al-Shaer et al., 2023; Cohen et al., 2023). By contrast, earlier CSR contracts tied to concrete environmental and social targets are more directly linked to operational outcomes, including reductions in non-GHG pollution and increases in green innovation (Flammer et al., 2019). This evidence suggests that ESG-linked incentives primarily elevate managerial attention and organizational focus on sustainability rather than directly inducing operational change.

A growing critique emphasizes the symbolic nature of ESG-linked pay. ESG targets are often qualitative or weakly verifiable, granting managers substantial discretion and weakening the connection between compensation and real environmental outcomes (Bebchuk and Tallarita, 2022). Consistent with this view, Hazarika et al. (2023) find that ESG incentives are associated primarily with improvements in social scores, with limited evidence of environmental effects. Even when environmental dimensions are considered, outcomes are typically measured using aggregate ESG ratings rather than direct indicators of emissions or abatement. Similarly, Eliwa et al. (2025) show that ESG-linked pay reduces gaps between disclosure and ESG scores, reflecting improved reporting alignment rather than substantive operational improvements. As a result, much of the existing literature evaluates sustainability performance through ratings and composite indices rather than through verifiable environmental outcomes such as GHG emissions.

3 Empirical Analysis

3.1 Empirical Strategy

Identifying the causal effect of CEO environmental incentives on corporate emissions is empirically challenging because the adoption of such incentives is endogenous. Firms self-select into environmental bonus schemes in response to regulatory pressure, investor scrutiny, or unobserved managerial preferences, and these same factors may independently affect emissions trajectories. As a result, simple comparisons between adopters and non-adopters are likely to be biased.

To address this endogeneity, we adopt an empirical strategy that combines three complementary research designs, each leveraging different sources of variation in the adoption of environmental compensation incentives. First, we implement a matched difference-in-differences (DiD) design that compares changes in emissions within firms before and after the introduction of environmental bonuses, relative to a matched control group of non-adopting firms. Second, we exploit peer effects in compensation practices by constructing an instrumental variable based on the adoption of environmental incentives among economically or institutionally similar firms, capturing governance spillovers in executive pay. Third, we use exposure to the EU Corporate Sustainability Reporting Directive (CSRD), following Hazarika et al. (2023), as a policy-based instrument that increases the salience and verifiability of ESG performance for U.S. firms with EU subsidiaries, thereby inducing the adoption of environmental criteria in executive compensation.

We view these approaches as complementary rather than hierarchical. Each design relies on distinct identifying assumptions and samples, and therefore provides an independent source of variation. The consistency of results across these strategies strengthens the credibility of the empirical findings despite the inherent challenges of identifying causal effects in this setting.

3.1.1 Difference-in-Differences Design

We construct a matched panel of firms that adopt environmental criteria in CEO bonus structures and comparable firms that do not adopt. Firms are matched on pre-treatment characteristics, including firm size, baseline Scope 1 emissions, industry, country, and governance attributes, to improve comparability between treated and control groups.

We then estimate Difference-in-Differences (DiD) models on the matched sample to identify the effect of environmental bonus adoption on firm-level emissions. The empirical strategy compares within-firm changes in Scope 1 emissions before and after adoption relative to matched control firms. Event study estimates show no evidence of differential pre-trends between treated and control firms, supporting the parallel trends assumption.[1] The baseline specification is:

$$\log(\text{Emissions}_{it}) = \alpha + \beta_1 \text{Treated}_i + \beta_2 \text{Post}_{it} + \delta(\text{Treated}_i \times \text{Post}_{it}) + \mathbf{X}'_{it}\gamma + \varepsilon_{it}, \quad (1)$$

where Emissions_{it} denotes Scope 1 emissions for firm i in year t. The indicator Treated_i equals one for firms that introduce environmental performance criteria in CEO compensation, and Post_{it} equals one for years at or after adoption. The coefficient δ captures the average treatment effect on emissions. The vector $\mathbf{X}_{it}$ includes year, country, and industry fixed effects, and time-varying firm-level controls. Standard errors are clustered at the firm level.

Because treatment timing varies across firms, we also implement staggered DiD estimators following Callaway and Sant'Anna (2021), which allow for heterogeneous treatment effects across cohorts and over time. We report both dynamic event-study estimates and overall average treatment effects. Additional implementation details are provided in Appendix C.5.

3.1.2 Spillover Instrumental Variables Design

While the matched DiD design addresses selection on observables and time invariant unobserved heterogeneity, concerns remain that time varying unobservables, such as evolving managerial preferences, investor pressure, or regulatory expectations, may simultaneously

[1] Appendix C provides detailed implementation of the matching procedure, balance diagnostics, and additional robustness checks.

influence both the adoption of environmental incentives and emissions outcomes. To address this concern, we implement an instrumental variables strategy that exploits peer and industry spillovers in the diffusion of CEO environmental compensation practices.

The identifying assumption is that, conditional on firm and year fixed effects and controls, variation in peer and industry adoption affects a firm's emissions only through its influence on the adoption of environmental compensation incentives. The relevance of the instruments is supported by the well documented tendency of firms to align executive compensation practices with those of peer firms and broader governance trends. The exclusion restriction is plausible insofar as the instruments are constructed from firms outside the focal firm's decision environment and, in the case of the industry spillover measure, explicitly exclude firms in the same industry, thereby limiting exposure to common environmental shocks that could directly affect emissions.

The first instrument captures peer spillovers and is defined as the lagged share of other firms within the same emissions group and country that have already adopted environmental bonus criteria:

$$\text{PeerRate}_{i,g,c,t-1} = \frac{\sum_{j \in (g,c,t-1),\, j \neq i} \text{entitlement}^{\text{CEO}}_{j,t-1}}{N_{g,c,t-1} - 1} \tag{2}$$

The second instrument captures industry spillovers and is defined as the lagged prevalence of CEO environmental compensation among firms in other industries within the same country:

$$\text{IndRate}_{i,c,s,t-1} = \frac{\sum_{j \in (c,t-1),\, \text{industry}_j \neq \text{industry}_i} \text{entitlement}^{\text{CEO}}_{j,t-1}}{N_{c,t-1} - N_{c,s,t-1}}. \tag{3}$$

These instruments capture the diffusion of governance practices driven by peer imitation and broader institutional forces, including investor stewardship norms and national governance codes. Because the industry spillover instrument excludes firms in the same industry, it mitigates contamination from industry specific environmental shocks that could directly affect emissions. The first stage regression is given by:

$$\text{Entitlement}^{\text{CEO}}_{it} = \alpha_0 + \alpha_1 \text{PeerRate}_{i,t-1} + \alpha_2 \text{IndRate}_{i,t-1} + \mathbf{X}'_{it}\boldsymbol{\alpha} + \varepsilon_{it}, \tag{4}$$

where $\mathbf{X}_{it}$ includes firm and year fixed effects and additional controls. Predicted values from the first stage are used in the second stage:

$$\log(\text{Scope1}_{it}) = \beta_0 + \beta_1 \widehat{\text{Entitlement}}^{\text{CEO}}_{it} + \mathbf{X}'_{it}\boldsymbol{\beta} + u_{it}, \tag{5}$$

which identifies the local average treatment effect for firms whose adoption decisions are influenced by peer or industry spillovers. This design therefore isolates variation in incentive adoption that is plausibly orthogonal to firm specific emissions shocks.

3.1.3 CSRD Instrumental Variables Design

As an additional source of identification, we exploit regulatory variation induced by the EU Corporate Sustainability Reporting Directive (CSRD), which increases the salience and verifiability of ESG performance for firms with operations in the European Union. This design provides policy driven variation in the adoption of CEO environmental compensation incentives that is distinct from the within firm and spillover based approaches described above.

We construct a time varying indicator of CSRD exposure equal to one for U.S. firms with at least one EU 27 subsidiary in the post 2022 period and zero otherwise. The underlying intuition is that the CSRD raises disclosure requirements and regulatory scrutiny for firms with EU subsidiaries, thereby increasing the incentives to formalize environmental performance metrics in executive compensation. Because the policy originates outside the United States and applies through multinational subsidiary structures, it generates differential exposure across otherwise similar firms. Formally, let $EUsub_i$ indicate whether firm i owns at least one EU 27 subsidiary and define $post\_CSRD_t = \mathbb{1}\{t \geq 2022\}$. The instrument is

$$z_{it}^{\text{CSRD}} = EUsub_i \times post\_CSRD_t, \tag{6}$$

which equals one for exposed firms after the CSRD and zero otherwise. Identification relies on the assumption that, conditional on controls and fixed effects, differential exposure to CSRD affects Scope 1 emissions primarily through its effect on the adoption of CEO environmental bonus entitlement.

The analysis uses the U.S. subsample of CDP Climate Change questionnaires for firms reporting between 2019 and 2024, which brackets the pre and post CSRD period. CDP firms are linked to ORBIS to recover ownership structures and subsidiary locations. EU subsidiary status is invariant at the firm level over the sample period, so identification exploits differential exposure to the timing of CSRD implementation rather than changes in multinational structure. The outcome variable is forward looking Scope 1 emissions. For each firm year, we construct

$$y_{i,t+1} = \log\bigl(\text{Scope1}_{i,t+1} + 1\bigr), \tag{7}$$

and estimate a two stage least squares model. The first stage is

$$CEO\ Entitlement_{it} = \pi\, z_{it}^{\text{CSRD}} + X_{it}'\gamma + \lambda_{g(i)} + \lambda_t + u_{it}, \tag{8}$$

and the second stage is

$$y_{i,t+1} = \beta\ CEO\ \widehat{Entitlement}_{it} + X_{it}'\delta + \lambda_{g(i)} + \lambda_t + \varepsilon_{it}, \tag{9}$$

where $CEO\ Entitlement_{it}$ is an indicator for CEO environmental bonus entitlement; X_{it} includes monetary and non monetary environmental incentives, a dummy for board level climate oversight, and emissions group indicators; $\lambda_{g(i)}$ are emissions group fixed effects; and λ_t are year fixed effects. Standard errors are clustered at the firm level.

We interpret these approaches as providing complementary sources of variation rather than as hierarchical identification strategies. Each design relies on distinct assumptions and samples, and the consistency of results across them strengthens the credibility of the empirical findings.

3.2 Data

We use firm-level data from the CDP Climate Change Questionnaires (2015–2024), an annual survey covering corporate climate governance and emissions (CDP, 2024). Our empirical analysis relies on two related but distinct samples drawn from these data. The matched global sample consists of 512 firms from 23 countries and it underlies the descriptive analysis, the PSM and DiD estimates, and the spillover IV specifications; its construction is described in Section C.1. The second is a subsample consisting only of U.S. firms used for the CSRD-based instrumental variables strategy, where exposure to the EU Corporate Sustainability Reporting Directive can be credibly defined based on subsidiary ownership.

Table 1: Summary Statistics: Global Sample

Variable	Mean	SD	Min	Max	N
Log Scope 1 Emissions	10.400	2.180	0.000	15.400	4,671
CEO Entitlement	0.204	0.403	0.000	1.000	4,461
Monetary incentive	0.883	0.322	0.000	1.000	4,461
Non-monetary incentive	0.394	0.489	0.000	1.000	4,461
Board-level oversight	0.946	0.226	0.000	1.000	4,667

Notes: This table provides summary statistics for main variables used in the empirical analysis using the matched global sample.

The data include self-reported Scope 1 and Scope 2 emissions and indicators for whether

CEOs receive monetary or non-monetary climate-linked incentives. These incentive variables allow us to identify the first year a CEO becomes eligible for an environmental bonus, which defines treatment in the empirical analysis. The construction of the CSRD sample is detailed in Appendix A.2. Variable definitions, treatment construction, and sample restrictions are described in detail in Appendix A. In particular, Appendix A.3 clarifies how CEO environmental bonus entitlement is defined using CDP disclosures, and Appendix A.4 documents our trimming and winsorization procedures for emissions data.

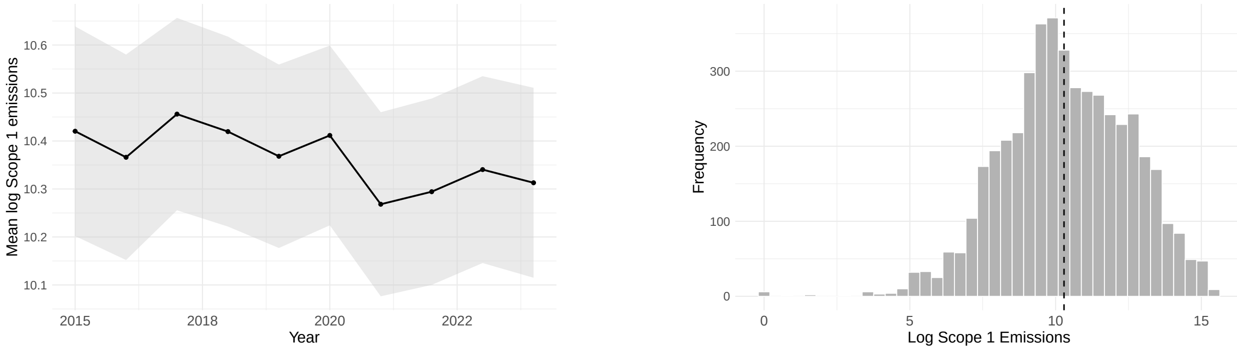
Table 2: Adoption and Timing of CEO Environmental Bonus Entitlement

	Value
Number of firms	512
Share ever adopting CEO bonus entitlement	0.562
Mean year of first adoption	2021
Share adopting pre-2022	0.556
Share adopting post-2022	0.444

Notes: The table summarizes firm-level adoption of CEO environmental bonus in the global sample.

Our primary outcome is annual Scope 1 emissions (metric tons of CO_2-equivalent), self-reported to CDP and log-transformed in regressions. Controls include firm-level indicators for monetary and non-monetary environmental incentives and board-level climate oversight. Table 1 reports summary statistics for the global sample. The average Log Scope 1 emissions display substantial dispersion across firms, reflecting pronounced heterogeneity in emissions intensity. Governance-related variables are widely prevalent in the sample: approximately 88% of firm-year observations report monetary climate incentives, 39% report non-monetary incentives, and nearly all firms indicate board-level oversight of climate issues.

Table 2 documents the prevalence and timing of CEO environmental bonus entitlement in the matched global sample. A majority of firms (56%) adopt CEO environmental bonus entitlement at some point during the sample period, with adoption occurring over time. The mean year of first adoption is 2021, with roughly equal shares of firms adopting before and after 2022. Figures 1a and 1b provide descriptive context on the evolution and distribution of Scope 1 emissions. The left panel shows that average emissions are relatively stable over time at the aggregate level, with modest year-to-year variation and overlapping confidence intervals. The right panel highlights substantial cross-sectional heterogeneity in emissions across firms, even after log transformation.



(a) Time evolution

(b) Cross-sectional distribution

Figure 1: Scope 1 emissions in the matched global sample

Notes: The left panel plots annual averages of log scope 1 emissions over time, with shaded areas denoting 95% confidence intervals. The right panel shows the cross-sectional distribution, with the dashed vertical line indicating the median. Both panels are based on the matched global sample.

4 Results

We present results across three complementary empirical approaches that exploit different sources of variation in the adoption of CEO environmental compensation incentives. The matched Difference in Differences design leverages within firm changes in adoption timing in a global sample, the spillover based instrumental variables approach isolates variation driven by peer and institutional diffusion, and the CSRD design exploits policy induced variation for U.S. firms with EU subsidiaries.

4.1 Difference in Differences Results

Table 3 reports baseline Difference in Differences estimates for Scope 1 emissions using the matched global sample. The first three columns present results from a traditional two-way fixed-effects (TWFE) specification, which we report for comparability but interpret with caution given known biases under staggered treatment timing and heterogeneous effects.

Table 3: DiD Results: Log Scope 1 Emissions

		DiD		**CS-DiD**
	All Firms	**Trimmed**	**Winsorized**	
CEO Entitlement	-0.077**	-0.071*	-0.081**	-0.125
	(0.038)	(0.039)	(0.036)	(0.095)
Observations	4,645	4,124	4,725	4,124
Adj. R2	0.954	0.935	0.955	

Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Global matched sample trimmed and winsorized at 95% and 1% percentiles. CS-DiD uses the trimmed sample.

Across all TWFE specifications, the estimated average treatment effect on the treated (ATT) is negative and statistically significant at conventional levels, suggesting modest declines in emissions following the adoption of environmental bonus entitlement. However, these estimates may be biased in settings with treatment effect heterogeneity.

We therefore focus on the Callaway–Sant'Anna estimator, which accounts for staggered adoption. The corresponding estimate (last column) remains negative but is no longer statistically significant. This indicates that once treatment timing and heterogeneity are properly accounted for, there is no robust evidence of a reduction in Scope 1 emissions.

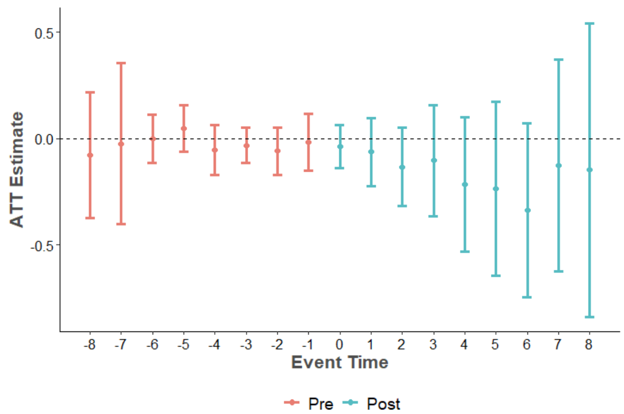


Figure 2: Event Study Plot Modern DiD: Log Scope 1 Emissions

Figure 2 presents event-study estimates from the staggered DiD specification. Pre-treatment coefficients are close to zero and statistically insignificant, providing support for the parallel trends assumption. Post-treatment estimates are generally negative and become more negative over event time, but remain small in magnitude and statistically indistinguishable from zero. Appendix Table 17 confirms that none of the dynamic effects are statistically significant.

4.2 Spillover Instrumental Variables Results

We next exploit variation in incentive adoption driven by peer behavior and broader governance norms. This approach isolates variation in CEO environmental compensation that arises from diffusion in executive pay practices, thereby addressing concerns that time-varying firm-level factors jointly influence both incentive adoption and emissions outcomes.

Table 4 reports results from the two-instrument specification. The first stage uses Peer Spillover, defined as adoption among firms in the same emissions group and country, and Industry Spillover, defined as adoption in other industries within the same country. Peer Spillover is consistently strong in the first stage, while Industry Spillover provides additional cross-industry variation and enables overidentification tests. In the second stage, estimated effects of CEO environmental bonus entitlement on Scope 1 emissions are negative but small across all specifications. The magnitude of the coefficients is modest and statistically indistinguishable from zero, indicating no measurable effect on emissions.

Table 5 reports results using only the Peer Spillover instrument. The first stage remains strong, and second-stage estimates are similar in magnitude and precision to the two-instrument specification. The spillover IV results are consistent with the Difference in Differences estimates and provide no evidence that exogenously induced adoption of environmental CEO incentives leads to economically meaningful reductions in Scope 1 emissions.

Table 4: IV Results: Peer and Industry Spillovers, and Log Scope 1 Emissions

	All Firms	**Trimmed**	**Winsorized**
(A) First Stage			
Peer Spillover ($t-1$)	0.349***	0.332***	0.334***
	(0.058)	(0.066)	(0.059)
Industry Spillover ($t-1$)	0.000	-0.026	-0.001
	(0.073)	(0.082)	(0.074)
(B) Second Stage			
CEO Entitlement	-0.166	-0.215	-0.099
	(0.287)	(0.333)	(0.298)
Observations	3914	3347	3980
Adj. R^2	0.958	0.941	0.959
F-statistic	18.37***	12.48***	15.89***

Notes: Standard errors clustered at the firm level in parentheses. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The first stage includes two lagged instruments: (1) *Peer Spillover*, defined as the prior-year adoption rate of CEO environmental incentives among firms in the same emissions group and country; and (2) *Industry Spillover*, defined as the prior-year adoption rate among firms in all other industries within the same country-year. Weak-IV F-statistic is the Kleibergen–Paap Wald statistic. All specifications include firm and year fixed effects and the full set of control variables.

4.3 CSRD Instrumental Variables Results

Table 6 reports results from the CSRD-based instrumental variables design, which exploits policy-induced variation in exposure to EU sustainability disclosure requirements among U.S. firms with EU subsidiaries. The first stage shows that CSRD exposure significantly increases the probability that a CEO becomes entitled to an environmental bonus, indicating that the policy affects the adoption of environmental compensation incentives.

In the second stage, the estimated effect of CEO environmental bonus entitlement on Scope 1 emissions is negative but small and statistically indistinguishable from zero, indicat-

Table 5: IV Results: Peer Spillovers, and Log Scope 1 Emissions

	All Firms	**Trimmed**	**Winsorized**
	(A) First Stage		
Peer Spillover ($t-1$)	0.341***	0.359***	0.333***
	(0.053)	(0.057)	(0.054)
	(B) Second Stage		
CEO Entitlement	-0.139	-0.065	-0.066
	(0.272)	(0.271)	(0.276)
Observations	4263	3778	4334
Adj. R^2	0.956	0.938	0.957
F-statistic	42.17***	40.14***	38.55***

Notes: Standard errors clustered at the firm level in parentheses. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The first stage includes *Peer Spillover*, defined as the prior-year adoption rate of CEO environmental incentives among firms in the same emissions group and country. Weak-IV F-statistic is the Kleibergen–Paap Wald statistic. All specifications include firm and year fixed effects and the full set of control variables.

ing no measurable effect on emissions. As in any instrumental variables design, identification relies on the exclusion restriction. A potential concern is that EU regulatory exposure could directly affect emissions through subsidiaries. However, event-study evidence shows no differential pre-trends in Scope 1 emissions between firms with and without EU subsidiaries prior to the implementation of the CSRD (see Figure 1 in Appendix B.2), which would be expected if such a channel were present.

These results are consistent with the Difference in Differences and spillover IV estimates. While the CSRD induces changes in executive compensation structures, the resulting variation does not translate into economically meaningful reductions in firms' direct emissions.

Across the three empirical approaches, the evidence points to a consistent conclusion. While the adoption of CEO environmental bonus entitlement is sometimes associated with negative point estimates, these effects are small and statistically indistinguishable from zero, particularly in specifications that account for staggered treatment timing or isolate exogenous variation in adoption. Overall, the results suggest that environmental CEO incentives, as currently designed, do not generate measurable reductions in firms' direct emissions.

5 Scope 2 Emissions

We also examine Scope 2 location-based emissions to assess whether CEO environmental incentives affect electricity-related emissions. We include Scope 2 emissions not only as an additional outcome, but to illustrate the identification limits of governance-based incentives

Table 6: IV Results: CSRD Exposure

	Log Scope 1 Emissions
(A) First Stage	
CSRD Exposure	0.102**
	(0.037)
(B) Second Stage	
CEO Entitlement	-0.415
	(2.300)
Observations	3,580
Adj. R^2	0.303
F-statistic	19.38***

Note: Standard errors in parentheses, clustered at the firm level. $^+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is log Scope 1 emissions measured at time (t+1). The instrument and all regressors are measured at (t).

for electricity-related emissions. The results of the CSRD-based IV strategy yields large point estimates that are robust to trimming, winsorization, and alternative functional forms (Appendix Tables 18 and 19), but diagnostic tests suggest these effects are not causal.

First, firms with and without EU subsidiaries differ markedly in pre policy Scope 2 emissions levels and trends. Event study estimates show pronounced positive pre trends well before the introduction of the CSRD, and placebo reduced form tests yield large and statistically significant effects at artificial policy dates. These patterns indicate that CSRD exposure predicts Scope 2 emissions independently of policy timing, capturing persistent structural differences rather than a policy induced shock. Second, CSRD exposure directly affects Scope 2 reporting. Firms with EU subsidiaries become significantly more likely to report Scope 2 emissions after the CSRD, while Scope 1 reporting remains largely unchanged (Table 23). This creates a direct channel through which the instrument affects observed Scope 2 emissions independently of CEO incentive structures, violating the exclusion restriction.

These failures are consistent with the construction of Scope 2 location-based emissions, which aggregate electricity use across locations using local grid emission factors. Firms with EU subsidiaries differ systematically in geographic footprint, electricity-grid exposure, and reporting coverage. These are exactly the dimensions that drive Scope 2 outcomes. Therefore, the CSRD instrument largely captures pre-existing differences rather than responses to CEO incentives. Consistent with this interpretation, within-firm strategies (DiD and peer-effects IV with firm fixed effects) yield small, statistically non-significant estimates for Scope 2 (Tables 20 and 21). Given these violations, we refrain from causal claims for Scope 2 emissions and focus the main causal analysis on Scope 1 outcomes; credible identification for Scope 2 would require designs that explicitly account for geography and electricity sourcing.

6 Why Do CEO Environmental Bonuses Fail?

Our results show no statistically significant reduction in Scope 1 emissions after firms link CEO pay to environmental performance. We therefore examine three mechanisms that may limit the effectiveness of these bonuses. First, the environmental component is small relative to total compensation. Second, the targets are often qualitative or loosely defined, and third, boards may lack the technical expertise to monitor and enforce performance. These features suggest ESG-linked pay may be more symbolic than operational.[2]

6.1 Are CEO Environmental Bonuses Large Enough to Matter?

A first limitation of CEO environmental incentives concerns the way these bonuses are designed. Figure 3 shows that in both the US and non-US samples, the most common form of "green" incentive is a simple cash bonus, typically expressed as a percentage of salary, rather than equity-based or long-term compensation instruments. This already suggests that firms rely on low-powered reward structures when linking executive pay to environmental objectives.

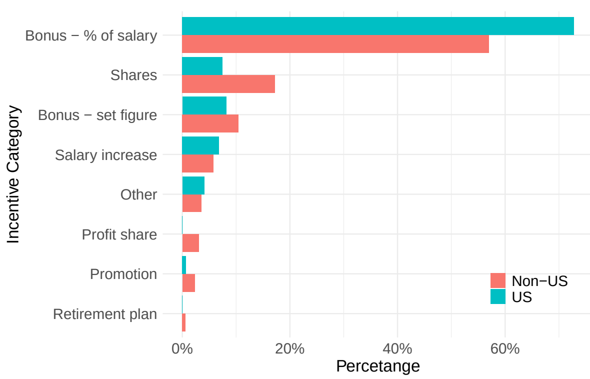


Figure 3: How is the Green Bonus Granted?

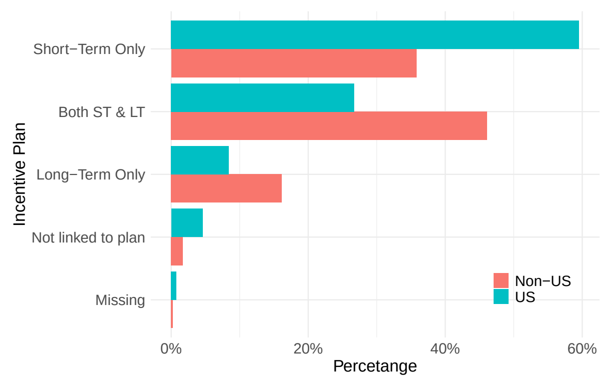


Figure 4: Types of Green Bonus

A second limitation is the time horizon of these incentives. Figure 4 shows that in the U.S., most green bonuses are short-term (annual) and not embedded in multi-year long-term incentive plans (LTI). Because short-term bonuses are easily adjusted and provide

[2]Details on the classification of emission-related incentive metrics, board experience, and governance mechanisms are provided in Appendix E.

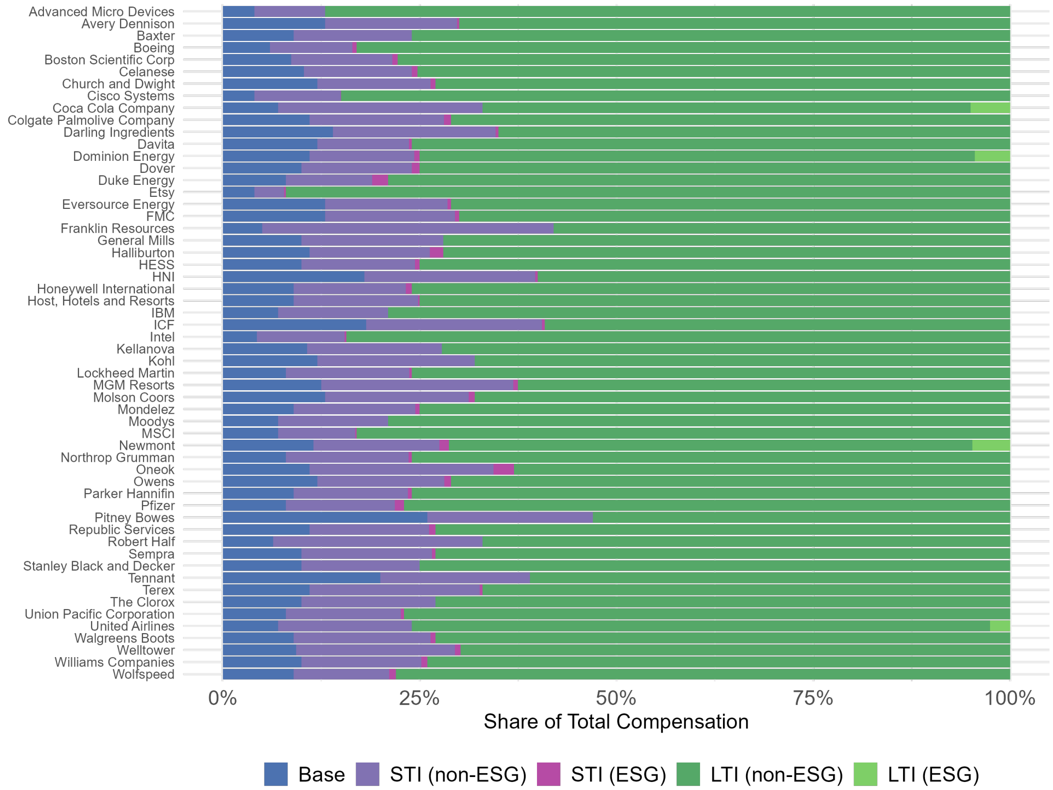


Figure 5: Green bonus as part of the CEO compensation scheme

Note: The figure shows the composition of CEO compensation for a stratified random sample of 50 firms adopting environmental CEO bonus entitlement in 2023. Components are expressed as shares of total CEO pay. STI denotes short-term incentives, LTI long-term incentives, and ESG environmental, social, and governance criteria. Firms are sampled proportionally by industry and emissions intensity from the CSRD IV sample.

weak commitment, they are unlikely to influence decisions requiring sustained investment, consistent with incentive theory on delayed and hard-to-measure tasks and with evidence linking sustainability investment to long-term governance (Holmstrom and Milgrom, 1991; Flammer and Bansal, 2017).

A third limitation is the small weight of green incentives in total pay. Figure 5 shows that the environmental component is typically tiny relative to salary, short-term incentives (STI), and especially LTI, which dominate pay and are tied mainly to financial metrics. In classical agency models, such a small, short-term, and weakly enforced component is unlikely to offset the organizational and financial costs of meaningful CO_2 reductions.

6.2 Are Environmental Targets Measurable and Enforceable?

A second mechanism weakening the effectiveness of green bonuses concerns the measurability of the underlying performance criteria. While firms increasingly reference environmental considerations in their compensation frameworks, many of the targets reported in the CDP disclosures remain broad, strategic, and difficult to verify. Descriptions such as "advancing the sustainability strategy" or "strengthening climate commitments" provide ample narrative flexibility but little in the way of concrete, year-on-year emissions expectations. Qualitative goals of this type are easier to satisfy ex post and harder for boards or shareholders to monitor, allowing CEOs to receive environmental credit without undertaking actions that reduce emissions in a measurable way.

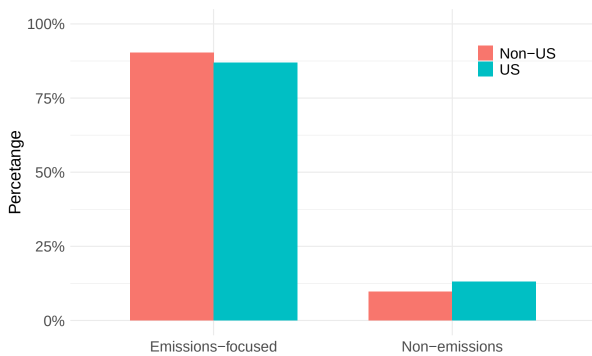


Figure 6: Emissions-Related Metrics

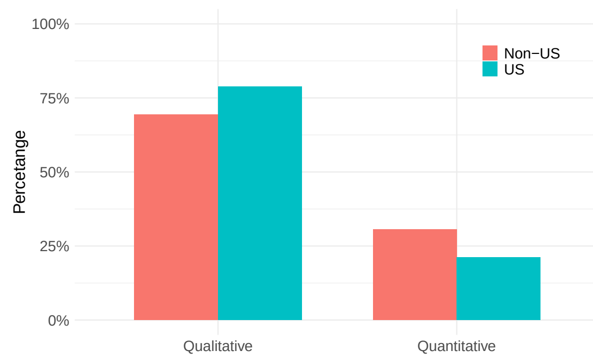


Figure 7: Qualitative vs. Quantitative

Figure 6 shows that many firms classify at least part of their CEO incentive metrics as emissions related, suggesting an apparent alignment between compensation and climate outcomes. In practice, this alignment is often more formal than substantive. Metrics labeled as emissions related frequently combine concrete indicators, such as reductions in absolute or intensity based emissions, with broader sustainability objectives, including progress on environmental initiatives or general climate goals. This blending of operational and strategic elements obscures the behaviors being incentivized and weakens the link between bonuses and actual emissions performance.

Figure 7 helps explain this disconnect. Most emissions related metrics rely on qualitative assessments rather than clear numerical benchmarks. Only a minority of firms sets explicit quantitative targets, such as reductions in absolute emissions, intensity ratios, or renewable energy shares. Most instead reward actions like initiating projects or advancing environmental strategies without specifying measurable thresholds or timelines. These designs permit bonus payouts even when emissions trajectories do not change, encouraging symbolic compliance rather than behavioral shifts capable of producing detectable CO_2 reductions. A detailed classification of these metrics is reported in Table 24 in the Appendix.

6.3 What Type of Climate Expertise Do Corporate Boards Have?

The effectiveness of CEO environmental incentives depends in part on the board's capacity to evaluate the credibility of reported climate performance. Boards, and in particular compensation committees, set environmental targets, monitor progress, and approve bonus payouts, so enforcement hinges on the depth of their environmental expertise.

Figure 8 shows that most firms report some form of board competency on climate issues. While this suggests an ability to oversee environmental performance, self reported competency provides little information about its substance. Figure 9 shows that board expertise is drawn primarily from experience rather than academic training or certified environmental education. Such experience supports strategic oversight and stakeholder engagement but does not necessarily provide the technical knowledge required to assess emissions measurement, verification practices, or decarbonization pathways. As a result, much of the reported competency reflects high level sustainability oversight rather than the technical expertise needed to evaluate whether reported CO_2 reductions are methodologically sound or operationally meaningful.

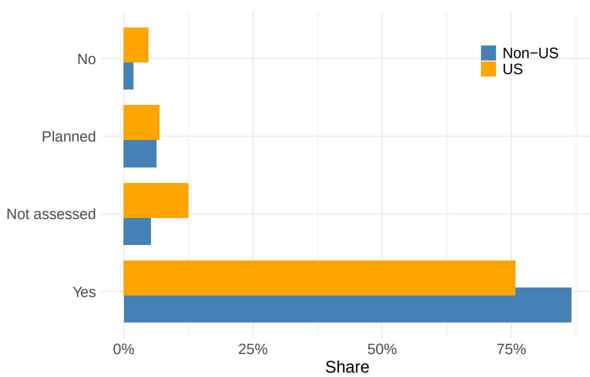


Figure 8: Board Competency on Climate

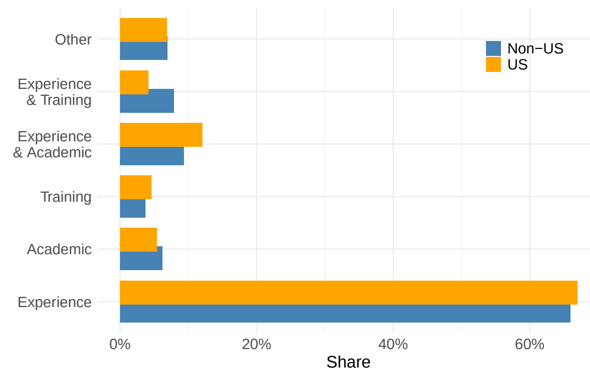


Figure 9: Board's Competency Background?

Figure 10 further decomposes experience-based competency into its main categories. Most board experience stems from managerial exposure to sustainability, including executive or management roles in ESG functions and participation in environmental committees, rather than from technical roles. Experience conferring deeper technical capacity, such as work in government environmental agencies, academic positions in environmental science, or involvement in complex sustainability transitions, is comparatively rare. Figure 11 shows that boards primarily maintain or update this competency through informal mechanisms, such as external engagement, internal work groups, or general knowledge-sharing, while structured technical training is uncommon. These patterns suggest that even when boards possess relevant experience, they may not systematically update or deepen their understanding of emissions accounting or decarbonization strategies. Detailed classifications are provided in Tables 25 and 26 in the Appendix.

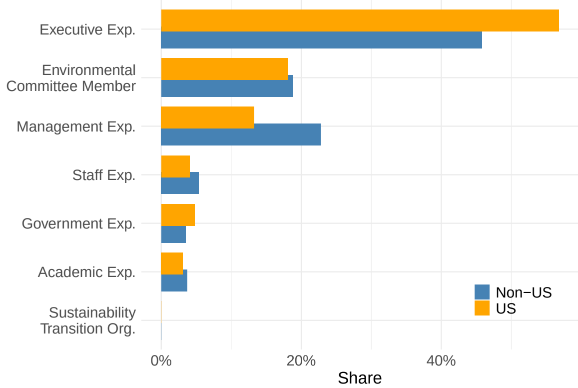


Figure 10: Board's Experience Types?

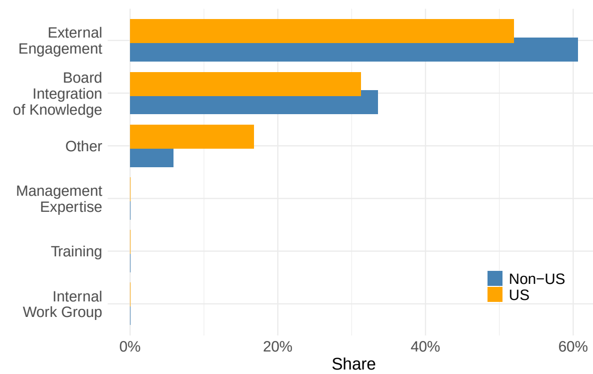


Figure 11: How is competency updated?

These patterns suggest that while boards frequently claim environmental competency, this competency is largely experiential, managerial, and informally maintained, rather than technical, academically grounded, or systematically updated. As a result, boards may lack the depth of expertise required to rigorously assess emissions metrics or to challenge management on the credibility of reported reductions, allowing qualitative narratives to substitute for measurable performance in bonus evaluations.

This does not imply that experience-based knowledge is irrelevant or that boards are unable to engage with climate issues. Firms may also rely on external experts or independent verifiers when assessing performance. However, external assurance cannot substitute for the board's core responsibility of designing incentive targets. The evidence shows that most environmental key performance indicators (KPI) embedded in CEO bonuses are qualitative or strategic rather than quantitatively tied to emissions outcomes. This design mirrors the composition of board expertise, which is concentrated in ESG and sustainability oversight roles, while technical expertise in emissions accounting, climate science, or environmental regulation remains relatively rare. Such experience supports broad sustainability governance but is less suited to setting measurable, time-bound decarbonization targets. Consequently, even when verification is outsourced, incentive schemes remain loosely defined and weakly linked to actual CO_2 reductions. The constraint, therefore, is not a lack of experience per se, but how board expertise shapes incentive design toward qualitative assessment rather than rigorous emissions accountability.

7 Discussion

This study examines whether linking CEO compensation to environmental performance affects firms' greenhouse gas emissions. Focusing on Scope 1 emissions, those most directly under managerial control, we find no statistically significant effect of CEO environmental bonus adoption across multiple complementary identification strategies that exploit distinct sources of variation in incentive adoption. The consistency of these results across designs suggests that ESG-linked bonuses, as currently implemented, do not materially alter firms' operational emissions trajectories.

These findings point to a clear interpretation. In practice, environmental components of CEO compensation are typically small relative to total pay and are tied to loosely defined, often qualitative objectives rather than verifiable emissions targets. Such incentives lack both the financial salience and contractual precision required to meaningfully shift managerial trade-offs or justify costly operational adjustments. As a result, while firms may formally adopt environmental performance criteria in executive pay, these mechanisms do not generate sufficient incentive strength to induce measurable emissions reductions.

This interpretation also helps rationalize the magnitude of the estimated effects. Given the relatively small financial stakes and qualitative nature of most environmental bonus components, large changes in operational emissions are not expected ex ante. Consistent with this, estimated effects are economically small across specifications, suggesting that any impact of these incentives on emissions is limited in magnitude rather than obscured by statistical uncertainty.

It also aligns with standard agency and attention-based theories. Incentives influence behavior when they are salient, well-defined, and tied to outcomes that managers can affect and be held accountable for. Weak or qualitative targets are unlikely to overcome the structural and financial barriers associated with substantive abatement, nor to command sustained managerial attention relative to competing strategic objectives. From a behavioral perspective, low-powered or weakly specified incentives may instead encourage symbolic compliance, allowing firms to signal environmental commitment without altering core production decisions.

Importantly, these results do not imply that firms ignore environmental considerations following bonus adoption. Rather, they indicate that the current design of ESG-linked compensation schemes is insufficient to generate detectable changes in quantitative outcomes such as emissions. More meaningful effects would likely require incentive contracts that assign greater financial weight to environmental performance, rely on clearly defined and verifiable metrics, and tie rewards explicitly to quantitative emissions reductions rather than to broad assessments of sustainability engagement.

Several limitations should be considered when interpreting these findings. First, the analysis relies on CDP disclosures, which are voluntary and more common among larger and sustainability-engaged firms. This introduces potential selection bias, as the sample may not reflect the broader population of companies with weaker reporting practices or fewer climate commitments. Second, the measurement of emissions and related outcomes depends on self-reported data, which may be subject to estimation error, optimism bias, or inconsistencies across firms and years. Although Scope 1 emissions are widely reported, the absence of standardized, mandatory reporting may attenuate estimated effects.

From an econometric perspective, the validity of the matched Difference-in-Differences estimates depends on the quality of the matching procedure and the assumption of parallel trends. While event-study evidence shows no systematic pre-trends, unobserved differences between treated and control firms may still affect emissions trajectories. In addition, treatment timing is inferred from the first observed year of CEO environmental bonus entitlement, which may not perfectly coincide with when targets were designed, communicated, or internalized by managers. Lagged responses or anticipatory adjustments could therefore attenuate estimated effects. Moreover, the estimated coefficients reflect average treatment effects and may mask heterogeneity across firms with stronger or more quantitatively defined incentive schemes.

Finally, the analysis focuses on short- to medium-term effects. The influence of environmental incentives on corporate strategy may emerge only gradually, particularly when emissions reductions require long-term investments. Our empirical strategy therefore concentrates on Scope 1 emissions, where identification is most credible. For Scope 2 emissions, regulatory exposure affects both reporting behavior and geographic coverage, limiting causal interpretation. More broadly, the small magnitude and qualitative nature of most environmental bonus components suggest that even long-run effects may remain difficult to detect without more detailed data on incentive strength and target specificity.

8 Conclusions

This paper studies whether tying CEO pay to environmental performance reduces operational Scope 1 emissions. Using multiple complementary identification strategies that exploit within-firm variation, diffusion in compensation practices, and policy-induced changes in disclosure requirements, we find no statistically significant effect of adopting environmental performance bonuses. Rather than indicating that incentives cannot matter, these results reflect how such incentives are currently designed. Environmental components of CEO compensation are typically small relative to total pay and are tied to qualitative or loosely defined objectives, limiting their ability to shift managerial effort toward costly operational abatement.

The results highlight a broader design problem in the use of ESG linked compensation as a climate policy instrument. Our analysis focuses on emissions that are directly under managerial control, where incentive effects are most likely to operate, and therefore provides a stringent test of whether internal governance mechanisms can drive real environmental outcomes. For executive pay to meaningfully support decarbonization, incentive contracts would need to rely on clearly defined and verifiable emissions metrics, carry meaningful financial stakes, and operate within governance structures capable of monitoring and enforcing performance. Absent these features, environmental bonus adoption is likely to function primarily as a symbolic governance response rather than as an effective mechanism for reducing corporate greenhouse gas emissions.

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Appendix

A Data Construction

A.1 Definitions

Table 7: Variable definitions

Variable	Definition
Log Scope 1 emissions	Natural logarithm of a firm's gross global Scope 1 greenhouse gas emissions, defined as direct emissions from owned or controlled sources, reported in metric tons of CO_2e.
Log Scope 2 emissions	Natural logarithm of a firm's gross global Scope 2 greenhouse gas emissions (location-based), defined as indirect emissions from the generation of purchased or acquired electricity, heat, steam, or cooling, reported in metric tons of CO_2e.
CEO Entitlement	Indicator capturing whether the Chief Executive Officer (CEO) is entitled to climate-related incentives, including monetary or non-monetary rewards linked to climate performance indicators or targets.
CSO Entitlement	Indicator capturing whether the Chief Sustainability Officer (CSO) is entitled to climate-related incentives, including monetary or non-monetary rewards linked to climate performance indicators or targets.
Climate board oversight	Indicator for the existence of board-level oversight of climate-related issues, including governance structures, assigned responsibilities, and integration of climate-related risks and impacts into board decision-making.
Carbon credit use	Indicator for whether the firm uses carbon credits for compliance or voluntary offsetting purposes, including purchased or issued credits from recognized carbon-crediting programs.
Regulated carbon market	Indicator for participation in regulated (compliance) carbon pricing systems, such as emissions trading schemes or carbon taxes, where carbon credits or allowances are required by regulation.
Internal carbon market	Indicator for whether the firm applies an internal price on carbon to inform investment decisions, risk management, or strategic planning.
Scope 1 third-party verification	Indicator for whether reported Scope 1 greenhouse gas emissions are verified or assured by an independent third party.
Scope 2 third-party verification	Indicator for whether reported Scope 2 greenhouse gas emissions are verified or assured by an independent third party.

A.2 Construction of the CSRD IV Subsample

The CSRD IV analysis is conducted on a subsample of U.S. firms reporting to the CDP Climate Change Questionnaires. We define a pre-period from 2019 to 2021 and a post-period from 2022 to 2024. This sspecification restricts attention to firms with at least two observations in each window. The CSRD IV estimation sample consists of 558 firms.

To measure CSRD exposure, CDP firms are matched to ORBIS data using firm names in order to recover ownership structures and subsidiary locations. A firm is classified as exposed if it owns at least one active subsidiary located in an EU-27 country at any point in the sample period. Ownership shares are not restricted, and exposure is time-invariant at the firm level. The instrument is defined as the interaction between an indicator for owning at least one EU subsidiary and a post-CSRD indicator equal to one from 2022 onward.

Table 8: EU-27 countries used to define CSRD exposure

ISO-2	**Country**	**ISO-2**	**Country**	**ISO-2**	**Country**
AT	Austria	BE	Belgium	BG	Bulgaria
HR	Croatia	CY	Cyprus	CZ	Czech Republic
DK	Denmark	EE	Estonia	FI	Finland
FR	France	DE	Germany	GR	Greece
HU	Hungary	IE	Ireland	IT	Italy
LV	Latvia	LT	Lithuania	LU	Luxembourg
MT	Malta	NL	Netherlands	PL	Poland
PT	Portugal	RO	Romania	SK	Slovakia
SI	Slovenia	ES	Spain	SE	Sweden

Notes: The table lists the EU-27 member states used to define CSRD exposure through subsidiary presence. The United Kingdom (GB) is excluded, consistent with the post-Brexit EU-27 definition.

A.3 Definition of Treatment Status: CEO Entitlement

Treatment is defined using firms' annual disclosure of CEO environmental bonus entitlement in the CDP Climate Change Questionnaires. For each firm-year, we observe whether the CEO is reported as eligible for an environmental performance–linked bonus.

Firms are classified into three mutually exclusive groups. *Treated* firms report at least one year without entitlement and at least one subsequent year with entitlement. *Never treated* firms never report entitlement, while *always treated* firms report entitlement in all observed years. For Treated, the treatment year is defined as the first year in which CEO environmental bonus entitlement is reported. We define the post-treatment indicator as one for years weakly after this first-entitlement year. Finally, we restrict attention to firms with sufficiently long panels to support pre/post comparisons.

A.4 Handling Outliers: Trimming and Winsorization

The dataset contains extreme values, especially in the upper tail, which often appear to reflect reporting or data-entry errors. To limit their influence, we report results using three variants of each outcome: the full sample, a winsorized sample (capping values at the 1st and 95th percentiles), and a trimmed sample (dropping observations outside the 1st and 95th percentiles). Winsorization preserves the sample but retains potentially incorrect values in capped form, whereas trimming removes suspect observations at the cost of sample size. We treat the trimmed specification as the preferred benchmark.

Table 9 reports the number of firms and firm-year observations in the matched sample for each variant. Because winsorization changes the distribution of the matching variable, the set of matched treated–control pairs can differ slightly across variants.

Table 9: Matched Sample Size Across Data Variants

Data Variant	**Unique Firms**	**Sample Size**
Full Sample	517	4,646
Trimmed Sample	458	4,125
Winsorized Sample	525	4,726

Note: Counts reflect the matched panel prior to DiD estimation. Matching was performed on scope 1 emissions, with trimming and winsorizing based on the 1st and 95th percentiles.

B Preliminary Analysis

B.1 Descriptive Analysis

Table 10: Summary Statistics: CSRD IV Estimation Sample

Variable	Mean	SD	Min	Max	N
Log Scope 1 Emissions	11.000	3.410	0.000	18.700	3,660
Log Scope 2 Emissions	11.600	2.330	0.000	20.800	3,282
CEO bonus entitlement	0.141	0.348	0.000	1.000	3,592
Monetary incentive	0.752	0.432	0.000	1.000	3,592
Non-monetary incentive	0.415	0.493	0.000	1.000	3,592
Board-level oversight	0.886	0.317	0.000	1.000	3,695
EU subsidiary	0.340	0.474	0.000	1.000	3,711

Notes: This table provides the summary statistics of the CSRD IV estimation sample.

Table 11: Correlation Matrix: CSRD IV Estimation Sample

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
(1) Log Scope 1 Emissions	1.00 ***	0.71 ***	0.15 ***	0.31 ***	0.09 ***	0.13 ***	0.03 *
(2) Log Scope 2 Emissions	0.71 ***	1.00 ***	0.12 ***	0.36 ***	0.15 ***	0.11 ***	0.14 ***
(3) CEO Bonus Entitlement	0.15 ***	0.12 ***	1.00 ***	0.21 ***	0.04 ***	0.10 ***	0.09 ***
(4) Monetary Incentive	0.31 ***	0.36 ***	0.21 ***	1.00 ***	0.14 ***	0.13 ***	0.11 ***
(5) Non-Monetary Incentive	0.09 ***	0.15 ***	0.04 ***	0.14 ***	1.00 ***	−0.01	0.10 ***
(6) Board-Level Oversight	0.13 ***	0.11 ***	0.10 ***	0.13 ***	−0.01	1.00 ***	0.00
(7) EU Subsidiary	0.03 *	0.14 ***	0.09 ***	0.11 ***	0.10 ***	0.00	1.00 ***

Notes: Pearson correlation coefficients are reported for the CSRD IV estimation sample. Significance levels are based on pairwise correlation tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Pre-policy Treated-Control Balance

Table 12 compares pre-CSRD firm characteristics for firms with versus without EU subsidiaries, using firm-level averages over the pre-policy period (through 2021). For each variable, we report group means and the standardized mean difference (SMD); values up to 0.25 indicate small to moderate baseline differences. Firms with EU subsidiaries are larger and more emissions-intensive before CSRD (especially for Scope 2 emissions) and are somewhat more likely to have environmental incentives and verification in place. In contrast, governance characteristics such as board-level climate oversight are well balanced across groups, consistent with the multinational profile of EU-exposed firms.

Our IV strategy hinges on parallel pre-policy dynamics rather than baseline levels. Figure 1 reports reduced-form event-study estimates comparing emissions trends for firms with versus without EU subsidiaries before CSRD. Scope 1 coefficients prior to 2022 are small and centered around zero, indicating no differential pre-trends. By contrast, Scope 2 shows clear positive pre-trends, suggesting that the exclusion restriction is more plausible for Scope 1 than for Scope 2 outcomes.

Table 12: Pre-policy Balance: Firms With and Without EU Subsidiaries

	No EU subsidiary	EU subsidiary	SMD
Log Scope 1 emissions	10.18	10.97	0.23
Log Scope 2 emissions	10.93	11.91	0.42
CEO Entitlement	0.08	0.13	0.17
CSO Entitlement	0.06	0.06	0.05
Climate board oversight	0.84	0.85	0.04
Carbon credit use	0.17	0.21	0.13
Regulated carbon market	0.24	0.32	0.21
Internal carbon market	0.18	0.17	0.05
Scope 1 third-party verification	0.51	0.63	0.27
Scope 2 third-party verification	0.48	0.61	0.27
Observations	397	169	

Notes: The table reports firm-level averages computed using pre-policy data (years up to 2021). Standardized mean differences (SMD) measure differences in baseline characteristics between groups.

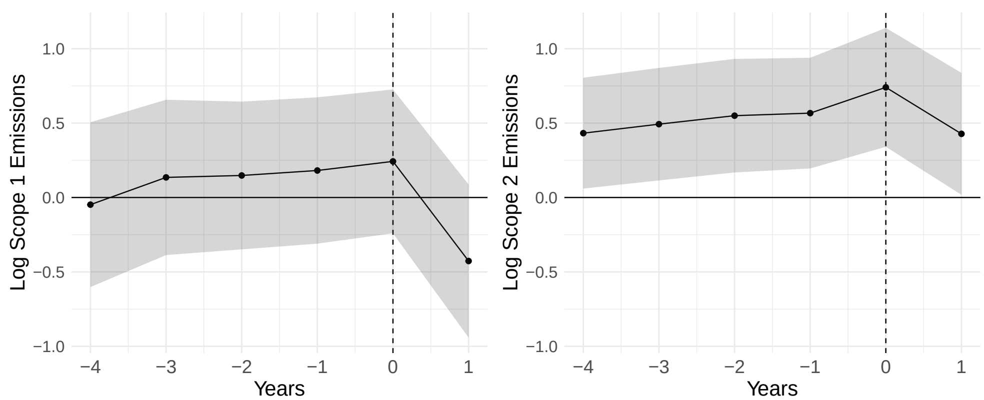

Figure 1: Pre-policy trends in Scope 1 and Scope 2 emissions.
The figure plots reduced-form event-study coefficients comparing firms with and without EU subsidiaries. Pre-policy coefficients for Scope 1 (left) are small and centered around zero, while Scope 2 (right) exhibits pronounced positive pre-trends before 2022.

B.3 FE for IV CSRD Sample

Table 13: Fixed Effects Regression: Next-Year Scope 1 Verification

	FE Estimate
CEO Entitlement	-0.007142
	(0.050549)
Observations	3580
Adj. R^2	0.969

Notes: Using the CSRD IV sample, we first estimate a firm fixed-effects model with firm and year fixed effects. Standard errors clustered at the firm level in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Details on Alternative Identification Strategies

C.1 Propensity Score Matching

This subsection describes the construction of the matched global sample used in the descriptive analysis and the PSM–DiD estimates. We construct a matched panel of treated and control firms using nearest neighbor propensity score matching. Treated firms are those that adopt environmental criteria in CEO bonus structures, while control firms never adopt such incentives. To align treatment timing, baseline covariates are defined in the year prior to adoption for treated firms, and corresponding baseline years are assigned to control firms.

We perform one to one matching without replacement using a caliper of 0.2 on the estimated propensity score and exact matching on industry group, country, and year. Propensity scores are estimated from a logit model in which the covariate vector X_i includes the logarithm of baseline Scope 1 emissions and a set of firm level and contextual characteristics:

$$P(T_i = 1 \mid X_i) = \Pr(\text{Bonus}_i = 1 \mid X_i) = \frac{\exp(X_i'\beta)}{1 + \exp(X_i'\beta)}. \tag{10}$$

To ensure that firms contribute observations both before and after treatment, we retain only firms with at least six years of available data. Matched pairs are tracked using subclass identifiers. Matching is performed separately on the full sample, a trimmed sample excluding the first and ninety fifth percentiles of emissions, and a winsorized sample.

Balance diagnostics indicate substantial improvements in covariate balance following matching. Love plots reported in Appendix Figure 3 show standardized mean differences below 0.1 for nearly all covariates in the trimmed sample, which is used as the main specification. The full and winsorized samples are used for robustness checks. Because emissions are right skewed, all regressions use logarithmic emissions measures. Figure 2 shows similar post matching emissions distributions across treated and control firms.

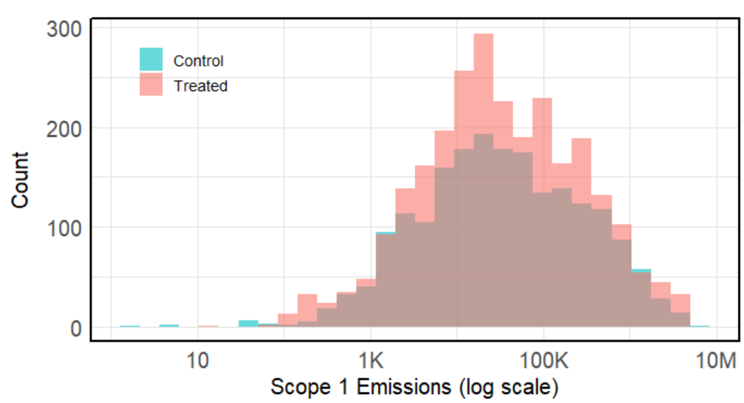

Figure 2: Distribution of log-transformed Scope 1 emissions

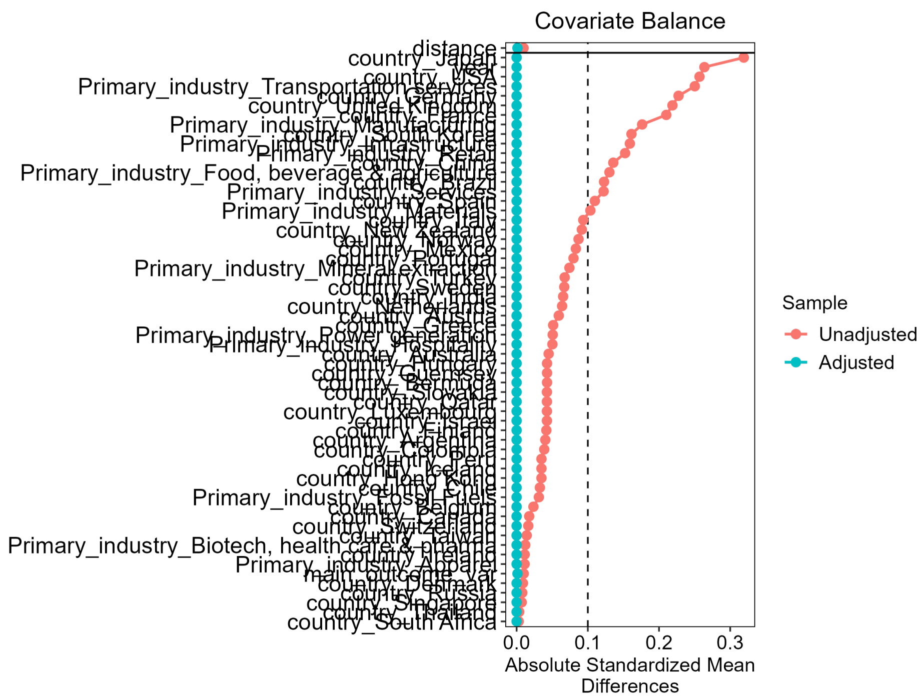

Figure 3: Average Scope 1 Emissions Love Plot (Trimmed Sample)

C.2 Two-Way Fixed Effects Model

e estimate a two-way fixed effects panel regression on the global matched sample. The specification includes firm and year fixed effects. The main coefficient of interest captures the within-firm association between CEO environmental bonus entitlement and emissions outcomes. The regression control for emissions-group classifications and governance characteristics, including monetary and non-monetary climate incentives and board-level climate oversight. Standard errors are clustered at the firm level.

Table 14: Fixed Effects Regression: Log of Scope 1 Emissions

	Scope 1
CEO Entitlement	-0.0166
	(0.0370)
Observations	4,645
Adj. R^2	0.954

Notes: Estimates use the global matched sample. The dependent variable is log(Scope 1 + 1). The model includes firm and year fixed effects and controls for emissions group, monetary and non-monetary climate incentives, and board-level climate oversight. Standard errors clustered at the firm level in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

C.3 Placebo Test

To assess whether spurious timing patterns could generate treatment-like effects, we conduct a placebo test using only never-treated firms from the global matched sample. Each control firm is randomly assigned a pseudo-treatment year drawn from the distribution of treatment years observed among treated firms. A placebo post-treatment indicator is then defined for years at or after the assigned pseudo-treatment year. It includes year fixed effects and the same set of control variables used in the main specifications. Because the placebo treatment is assigned independently of firms' actual incentive adoption, the absence of statistically significant effects supports the interpretation that the main results are not driven by coincidental timing or aggregate trends.

Table 15: Placebo Regression: Emissions

	Scope 1
Placebo	0.001
	(0.127)
Observations	2010
Adj. R^2	0.247

Notes: Estimates use only never-treated firms from the global matched sample. The dependent variable is log(Scope 1+1). PlaceboPost_{it} is constructed using randomly assigned pseudo-treatment years drawn from treated-firm years. The model includes year and emissions fixed effects and control variables. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

C.4 Double Machine Learning Model

We apply a Double Machine Learning (DML) approach that allows for flexible, high-dimensional controls and reduces sensitivity to functional-form assumptions. Following the partialling-out framework of Chernozhukov et al. (2018), both the outcome and the treatment are residualized with respect to a rich set of covariates using machine-learning methods.

Table 16: Double Machine Learning (LASSO)

	Estimate
CEO Entitlement	-0.022 (0.0278)
95% CI	[-0.0766, 0.0325]

Note: Double Machine Learning (partialling-out, PLR) with LASSO nuisance models and firm fixed effects. Standard errors are cluster-robust. The results use the global matched sample.

C.5 Staggered Difference-in-Differences

We estimate staggered Difference-in-Differences models using the `did` package in `R`, following Callaway and Sant'Anna (2021). Treatment timing is defined as the first year in which a CEO environmental bonus is adopted, with never-adopters serving as the control group. We use the doubly robust estimator with time-varying covariates, report overall average treatment effects and dynamic event-time estimates, and visualize treatment dynamics using `ggdid()`. Standard errors are computed using the package's default bootstrap.

Table 17: Dynamic (Event-Time) ATT Estimates

Event Time	**Estimate**	**Std. Error**	**95% CI**
−7	-0.004	0.141	[-0.281, 0.272]
−6	0.006	0.042	[-0.075, 0.088]
−5	0.046	0.041	[-0.034, 0.127]
−4	-0.022	0.030	[-0.080, 0.036]
−3	-0.040	0.042	[-0.123, 0.043]
−2	-0.012	0.062	[-0.134, 0.110]
−1	0.046	0.070	[-0.090, 0.182]
0	0.042	0.045	[-0.047, 0.130]
1	-0.030	0.121	[-0.267, 0.208]
2	-0.072	0.108	[-0.284, 0.140]
3	-0.111	0.126	[-0.358, 0.135]
4	-0.178	0.148	[-0.469, 0.113]
5	-0.218	0.141	[-0.494, 0.058]
6	-0.337*	0.158	[-0.648, -0.027]
7	-0.088	0.165	[-0.413, 0.236]

Note: Event-time estimates from Callaway and Sant'Anna (2021). Confidence intervals use firm-clustered standard errors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The results use the global matched sample.

D Scope 2 Emissions Regression Results

D.1 CSRD IV Results

Table 18: CSRD IV Results: Scope 2 Emissions Location-Based

	All Firms	**Winsorized**	**Trimmed**
	(A) Second Stage		
CEO Entitlement	5.941**	5.360**	4.461**
	(2.592)	(2.354)	(2.069)
	(B) First Stage		
CSRD Exposure	0.106***	0.106***	0.108***
	(0.039)	(0.039)	(0.040)
Observations	2506	2506	2314
F-statistic	17.26	17.26	16.56
Weak-IV test (p-value)	< 0.001	< 0.001	< 0.001

Notes: The dependent variable is log(Scope 2 emissions $(\text{location-based})_{t+1}+1$), measured using `scope_2_emissions_location`. Column (1) uses the raw measure; column (2) winsorizes the underlying emissions and (3) trims them, both at the 1st and 95th percentiles, before logging. All specifications include emissions-group and year fixed effects and control variables. Standard errors clustered at the firm level are reported in parentheses. Weak-IV F-statistic corresponds to the Kleibergen–Paap Wald statistic. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 19: CSRD IV Results: Scope 2 Emissions Location-Based (Log Growth)

	All Firms	**Winsorized**	**Trimmed**
	(A) Second Stage		
CEO Entitlement	0.465	0.335	0.273
	(0.327)	(0.283)	(0.278)
	(B) First Stage		
CSRD Exposure	0.107***	0.109***	0.111***
	(0.039)	(0.039)	(0.040)
Observations	2463	2443	2311
F-statistic	17.36	17.85	17.78
Weak-IV test (p-value)	< 0.001	< 0.001	< 0.001

Notes: The dependent variable is log growth in location-based Scope 2 emissions, defined as $g_{\log} = \log(\text{Scope2Loc}_{t+1}+1) - \log(\text{Scope2Loc}_{t}+1)$, where `Scope2Loc` corresponds to `scope_2_emissions_location`. Column (1) uses the raw measure; column (2) winsorizes the underlying emissions and (3) trims them, both at the 1st and 95th percentiles, before logging. All specifications include emissions-group and year fixed effects and control variables. Standard errors clustered at the firm level are reported in parentheses. Weak-IV F-statistic corresponds to the Kleibergen–Paap Wald statistic. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

D.2 PSM-DiD Results

Table 20: DiD Results: Scope 2 Emissions Location-Based

	DiD			CS-DiD
	All Firms	Trimmed	Winsorized	
ATT	-0.014	0.012	-0.026	0.007
	(0.084)	(0.078)	(0.077)	(0.156)
Observations	1607	1449	1703	1449
Adjusted R^2	0.874	0.885	0.878	

Note: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sample trimmed and winsorized at the 95% and 1% percentiles. CS-DiD uses the trimmed sample.

D.3 Peer and Industry Spillover IV Results

Table 21: IV Results: Peer and Industry Spillovers, and Scope 2 Emissions (Location-Based)

	All Firms	Trimmed	Winsorized
(A) First Stage			
Peer Spillover $(t-1)$	0.366***	0.340**	0.329***
	(0.099)	(0.106)	(0.095)
Industry Spillover $(t-1)$	-0.096	-0.109	-0.103
	(0.091)	(0.102)	(0.091)
(B) Second Stage			
CEO Entitlement (2SLS)	-0.015	0.166	0.101
	(0.403)	(0.470)	(0.402)
Observations	1,301	1,160	1,393
F-statistic	9.50***	7.47***	8.48***

Notes: Standard errors clustered at the firm level in parentheses. $^+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The first stage includes two lagged instruments: (i) *Peer Spillover*, defined as the prior-year adoption rate of CEO environmental incentives among firms in the same emissions group and country; and (ii) *Industry Spillover*, defined as the prior-year adoption rate among firms in all other industries within the same country-year. The reported F-statistic is the Kleibergen–Paap Wald statistic for weak identification. All specifications include firm and year fixed effects and the full set of control variables.

D.4 Pre-policy Placebo Tests

Our IV strategy requires that CSRD exposure affects emissions only through CEO incentive structures, implying no systematic relationship between exposure and emissions prior to the directive. We test this using pre-policy placebo regressions: restricting to pre-2022 data, we re-estimate the reduced form using placebo policy years 2018, 2019, and 2020, for which the exposure effect should be close to zero.

For Scope 1 emissions, placebo estimates are small, vary in sign, and are statistically insignificant, indicating no differential pre-trends for EU-exposed firms. For Scope 2 (location-based), placebo effects are large, positive, and significant across placebo years, suggesting systematic pre-policy differences unrelated to CSRD. Overall, CSRD exposure appears plausibly exogenous for Scope 1 outcomes but not for Scope 2 outcomes.

Table 22: Pre-policy Placebo Reduced-Form Estimates

	Scope 1 Emissions			Scope 2 Emissions		
	2018	2019	2020	2018	2019	2020
$\text{CRSD}_{\text{placebo}}$	-0.098	0.029	0.048	0.414^{***}	0.478^{***}	0.530^{***}
	(0.249)	(0.247)	(0.265)	(0.158)	(0.165)	(0.178)
Observations	Pre-2022 sample			Pre-2022 sample		

Notes: Each column reports a reduced-form regression of next-period log emissions on a placebo policy indicator equal to one for firms with EU subsidiaries in years greater than or equal to the indicated placebo cutoff. All regressions use only pre-2022 data and include year fixed effects, emissions-group fixed effects, and governance controls. Standard errors clustered at the firm level. $^{***}p < 0.01$.

D.5 CSRD Exposure and Emissions Reporting Behavior

A key concern is whether CSRD exposure changes not only real emissions behavior but also reporting. If disclosure responds to the policy, observed emissions changes may partly reflect reporting rather than underlying emissions intensity. Table 23 tests this by using binary outcomes equal to one if a firm reports a finite (non-missing) Scope 1 or Scope 2 (location-based) value in a given year. Reporting responses differ by scope. CSRD exposure has a small, statistically insignificant effect on Scope 1 reporting, but increases the probability of reporting Scope 2 by about 3.3 percentage points. Thus, CSRD exposure materially affects Scope 2 disclosure, while Scope 1 reporting is largely unaffected, supporting Scope 1 as the primary outcome for the IV analysis.

Table 23: Effect of CSRD Exposure on Emissions Reporting

	Scope 1 Reporting	Scope 2 Reporting
CSRD Exposure	0.010	0.033^{***}
	(0.007)	(0.013)
Controls	Yes	Yes
Firm-clustered SE	Yes	Yes

Notes: The dependent variable is an indicator equal to one if the firm reports a finite, non-missing value for the corresponding emissions measure in a given year, and zero otherwise. CSRD exposure is defined as an indicator for firms with EU subsidiaries in years 2022 and later. All regressions include governance controls (CEO monetary and non-monetary incentives, board-level climate oversight, carbon credit use, and verification indicators), industry fixed effects, and year fixed effects. Standard errors are clustered at the firm level. $^{***}p < 0.01$.

E Mechanisms: Details and Definitions

Table 24: Classification of Emission-Related CEO Incentive Metrics

Metric	Type
Reduction in absolute emissions	Quantitative
Reduction in emissions intensity	Quantitative
Increased share of renewable energy in total energy consumption	Quantitative
Reduction in absolute emissions in line with net-zero target	Quantitative
Emissions reductions across portfolio companies	Quantitative
Implementation of an emissions reduction initiative	Qualitative
Other emission reduction-related metrics, please specify	Qualitative
Progress towards environmental targets	Qualitative
Achievement of environmental targets	Qualitative

Table 25: Classification of Board's Experience Categories

CDP Description	Label
Executive-level experience in a role focused on environmental issues	Executive Exp.
Management-level experience in a role focused on environmental issues	Management Exp.
Staff-level experience in a role focused on environmental issues	Staff Exp.
Experience in an academic role focused on environmental issues	Academic Exp.
Experience in the environmental department of a government	Government Exp.
Experience in an organization that is exposed to environmental scrutiny and is going through a sustainability transition	Sustainability Transition Org.
Active member of an environmental committee or organization	Environmental Committee Member

Table 26: Classification of Board's Competency Updates Categories

CDP Description	Label
Consulting regularly with an internal, permanent, subject-expert working group	Internal WG
Engaging regularly with external stakeholders and experts on environmental issues	External Engagement
Regular training at management level on environmental issues, industry best practice, and standards (e.g., TCFD, SBTi)	Training
Having at least one individual at management level with expertise on this environmental issue	Expertise in Management
Other	Other