

Article

Mandate or Market? Climate-Disclosure Regulation and the Quality of Corporate Climate Governance: Evidence from Staggered Adoption Across Four Economies

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Abstract

Climate-related disclosure has moved rapidly from a voluntary practice to a legal obligation, yet whether mandates improve the substance of corporate climate governance—rather than the volume of reporting—remains an open question. We exploit the staggered introduction of climate-disclosure mandates in the United Kingdom (2022) and France (2025), with the United States and Australia serving as never-treated and not-yet-treated comparison groups, to estimate the effect of mandatory disclosure on structured climate-governance quality. The outcome is the Transition Pathway Initiative (TPI) Management Quality score, an externally assessed 0–5 staircase of climate-governance practices, observed for 837 listed firms over 2015–2025 (2841 firm-year observations). Using two-way fixed effects, Sun–Abraham and Callaway–Sant’Anna estimators implemented in RStudio, we find no statistically significant effect of disclosure mandates on climate-governance quality: the preferred Callaway–Sant’Anna estimate is 0.082 levels (95% CI −0.221 to 0.385), precise enough to rule out effects larger than about 0.4 levels (≈ 0.4 standard deviations). Meanwhile, never-mandated US firms improved from a mean Management Quality of 1.4 to 3.1 over the decade, converging toward mandated jurisdictions. An anticipation-adjusted specification yields a positive and significant effect, but it benchmarks against pandemic-year assessments and should be read with caution. Disaggregating the score into its underlying indicators shows that board-level oversight and basic disclosure are near-universal among mandated firms, whereas transition-planning and capital-alignment practices remain rare in every jurisdiction, locating the unfinished governance agenda at the top of the staircase. The evidence suggests that disclosure mandates largely codified governance practices that investor- and market-driven pressures had already diffused, implying that regulators should pair disclosure requirements with substantive transition-plan obligations if the policy goal is governance change rather than transparency alone.



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1. Introduction

Few areas of corporate regulation have moved as quickly as climate-related disclosure. Within five years of the Task Force on Climate-related Financial Disclosures (TCFD) issuing

its voluntary recommendations [1], jurisdictions accounting for a large share of global market capitalization converted those recommendations into binding law. The premise underlying this regulatory wave is that transparency disciplines behavior: investors price climate risk and demand credible management of it [2–5], and mandatory disclosure is expected to force firms to build the governance machinery—board oversight, executive accountability, targets, and transition plans—that voluntary regimes left optional. The United Kingdom required TCFD-aligned reporting for premium-listed companies from financial years beginning in 2021, with the first mandated annual reports published in 2022 [6,7]; the European Union’s Corporate Sustainability Reporting Directive (CSRD) extended mandatory sustainability reporting to large firms with first reports in 2025 [8]; Australia legislated mandatory climate reporting for its largest entities with first reports due in 2026 [9]; and the International Sustainability Standards Board consolidated the TCFD architecture into IFRS S2 [10]. The United States, by contrast, adopted a federal climate-disclosure rule in 2024 that was stayed in litigation and never came into force during our sample period [11], leaving large listed US firms without a binding economy-wide climate-disclosure mandate.

Whether these mandates change what firms do, rather than what they say, is the question this paper addresses. A substantial literature evaluates the economics of disclosure regulation [12,13] and, more recently, the consequences of the UK TCFD mandate specifically, finding effects on financial performance [14] and on the volume and tone of climate-related claims [15]. Yet the outcome that mandates are ultimately meant to move—the quality of corporate climate governance—has rarely been measured directly. Reporting volume is an imperfect proxy: firms can produce longer climate sections without strengthening board oversight or adopting credible targets. We instead use the Transition Pathway Initiative (TPI) Management Quality (MQ) score, an externally assessed, analyst-verified staircase that classifies firms from Level 0 (unaware of climate change as a business issue) to Level 5 (strategic assessment), based on indicators covering policy commitments, emissions disclosure, target-setting, executive responsibility, and transition planning [16–18]. Because MQ is constructed by an independent academic center using publicly disclosed information under a consistent methodology, it provides a structured, comparable measure of climate-governance quality across firms, countries, and years.

Our research design exploits the staggered timing of climate-disclosure mandates across four advanced economies whose firms are continuously assessed by TPI: the United Kingdom (first mandated reports observable in 2022 assessments), France (CSRD wave-one reports observable in 2025), Australia (first reports due only in 2026, outside our window, so Australian firms serve as not-yet-treated controls), and the United States (never treated within the sample). We assemble an unbalanced firm-year panel of 837 firms and 2841 firm-year observations spanning TPI assessment years 2015–2025. Identification follows the modern staggered difference-in-differences (DiD) literature: alongside the two-way fixed-effects (TWFE) benchmark, we estimate the interaction-weighted estimator of Sun and Abraham and the group-time average treatment effects of Callaway and Sant’Anna, which are robust to treatment-effect heterogeneity and the negative-weighting problems that contaminate TWFE under staggered adoption [19–24].

The answer the data give is largely “market.” Across every primary specification, the estimated effect of disclosure mandates on climate-governance quality is small and statistically indistinguishable from zero: 0.114 levels (SE 0.092) under TWFE, 0.074 (SE 0.104) under Sun–Abraham, and 0.082 (95% CI −0.221 to 0.385) under Callaway–Sant’Anna. These are not underpowered non-results: the confidence intervals exclude effects larger than roughly 0.3–0.4 MQ levels, less than half a standard deviation. The null survives a battery of robustness checks—alternative control groups, alternative French treatment dating,

alternative deduplication of assessment vintages, a high-MQ indicator addressing ceiling effects, two-way clustering, and a placebo mandate placed in the pre-period. At the same time, the descriptive record shows powerful market-driven improvement: never-mandated US firms climbed from a mean MQ of 1.4 in 2015 to 3.1 in 2025, converging toward UK and French firms, which already averaged above 3.4 before any mandate took effect. One specification—an anticipation-adjusted Callaway–Sant’Anna estimator that treats the UK announcement year as already affected—yields a positive and significant effect of 0.68 levels; we report it transparently but show that it benchmarks against pandemic-year assessments and is therefore fragile.

There are three pieces of literature in which this paper is contributing. First, it enriches the rewritten literature of the real effects of mandatory sustainability disclosure, which have focused on financial performance and emissions reporting and reporting behavior [14,15,25–29]. Second, our evidence corroborates the “mandate versus market” debates in sustainability regulation [30,31] to suggest that these investor-led initiatives—engagement coalitions, index stewardship, and climate-risk pricing—have helped to disseminate the practice of climate-governance in advance of existing legal regulation [32–35]. Thirdly, it provides a methodology template for policy evaluation based on externally assessed governance scores, thus indicating how sensitive findings are to anticipation assumptions and using currently available best practice estimators [19,20,24,36,37]. The findings give a dismal policy signal to the jurisdictions just implementing climate-reporting regimes: the disclosure of plans may reinforce rather than shape substantive change, while regulators aiming for the latter could be required to legislate for the former.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background, reviews the literature, and develops hypotheses. Section 3 presents the data, variables, and empirical strategy. Section 4 reports the results, including event studies and robustness tests. Section 5 discusses mechanisms and implications, and Section 6 concludes.

2. Institutional Background, Literature, and Hypotheses

2.1. From Voluntary Recommendation to Legal Mandate

The TCFD framework, issued in 2017 under the auspices of the Financial Stability Board, structured climate-related disclosure around governance, strategy, risk management, and metrics and targets [1]. Adoption was initially voluntary and investor-driven. The United Kingdom moved first toward compulsion: the Financial Conduct Authority’s Listing Rule 9.8.6R(8) required premium-listed commercial companies to report against the TCFD recommendations on a comply-or-explain basis for financial years beginning on or after 1 January 2021, with the first mandated annual reports published in spring 2022 [6]; the Companies (Strategic Report) (Climate-related Financial Disclosure) Regulations 2022 extended climate-related reporting duties to large companies for financial years beginning on or after 6 April 2022 [7]. France entered the panel pre-treated in a weaker sense—Article 173 of the 2015 Energy Transition Law required institutional and large-company climate reporting from fiscal year 2016—but its binding firm-level regime in the sense studied here arrives with CSRD wave one, whose first reports were published in 2025 [8]. Australia’s Treasury Laws Amendment (Financial Market Infrastructure and Other Measures) Act 2024 introduced mandatory climate reporting under AASB S2 for Group 1 entities for financial years beginning on or after 1 January 2025, so the first Australian mandated reports fall in 2026, outside our sample window [9]. In the United States, the Securities and Exchange Commission adopted a climate-disclosure rule in March 2024, but the rule was immediately stayed pending litigation and did not take effect during our sample period [11]. This sequencing—UK 2022, France 2025, Australia 2026, US untreated—creates the staggered adoption design that our estimators exploit.

2.2. Theory: Why Mandated Disclosure Might (or Might Not) Improve Governance

There are two theoretical lines that argue that disclosure requirements positively affect the underlying governance. Information-economics models of discretionary disclosure [38,39] suggest that it is precisely the firms that have less good information to reveal that would engage in such behavior in a mandatory regime, and the unraveling results in Grossman and Milgrom's models [40,41] suggest that the effect of mandates is to increase the cost of signaling they possess nothing credible to disclose, which should encourage firms to actually invest in good governance. In the empirical sense, there is a systematic connection between voluntary environmental disclosure and performance and capital-market benefits [42,43] with an understanding of legitimacy theory that disclosure is one of the instruments that firms use to secure their social license [44,45]. The second tradition is provided by institutional theory, which provides a framework of "canonical isomorphic forces" organizing towards standardization of practice [46,47] and provides with coercive determinants from regulation a logical basis for board-level oversight of climate issues alongside agency or stakeholder rationales [48,49]. These accounts should ramp up board oversight, targeting, and transition planning—all practices measured by the TPI Management Quality staircase. As with the governance channel, board-level characteristics and climate-related incentives also correlate with better carbon performance [50,51], and capital markets have a lower appetite for carbon emissions that are not disclosed or quantified [52].

But there are also solid reasons to hope for little or nothing. First, companies are going the extra mile in anticipation of the regulation and of investor pressure [30,53], and studies indicate that in some instances the voluntary and market pressures/personal preferences have preceded mandate regulation [31]. If large listed firms are already at the top of the governance staircase, as many institutional investors, engagement coalitions like Climate Action 100+, and index stewardship may have worked to spread climate-governance practice among large listed firms [32–35], it means the marginal firm required by a 2022 mandate may already be toward the top. Second, disclosure requirements regulate the reporting rather than the substance: disclosure requirements enable firms to "green wash" amounts of climate-related disclosure without changing governance structures, as studied in the field of greenwashing and selective disclosure [54–57]. Thirdly, the disclosure-based outcome itself could already be visible: TPI makes assessments on public disclosures, so companies that score high on TPI will likely be high even without a disclosure mandate. What is the answer to this question of dominance? (This is an empirical question).

2.3. Evidence on Mandatory Sustainability Disclosure

The empirical literature on mandatory sustainability disclosure documents real effects in several settings. Mandatory non-financial disclosure in the EU produced negative announcement returns for affected firms, but subsequent improvements in some outcomes [25] and the EU CSR Directive generated real effects on firm behavior [26]. Carbon-specific mandates matter: the UK's 2013 greenhouse-gas reporting mandate reduced emissions among affected firms [27], facility-level disclosure induces benchmarking and abatement [28], and cross-country evidence associates ESG disclosure mandates with improved information environments [29,58]. France's Article 173 shifted financial institutions away from fossil-energy financing [59]. Climate-risk disclosure has also been linked to green innovation in emerging-market settings [60]. On the UK TCFD mandate specifically, recent work finds improved financial performance among treated non-financial firms [14] and a steady increase in TCFD-aligned climate claims that began before the mandate was in force [15]. What this literature has not established is whether mandates improve externally assessed climate-governance quality—the structured practices, rather than the prose. The

TPI Management Quality framework, used in academic assessments of corporate climate action [16,17], permits exactly that test.

2.4. Hypotheses

Following the coercive-isomorphism and unraveling logics, our primary hypothesis is stated in the affirmative, against which the data can speak:

Hypothesis 1 (H1). *The introduction of a mandatory climate-disclosure regime increases the climate-governance quality (TPI Management Quality level) of treated firms relative to untreated firms.*

Because the UK rule was announced in December 2020 and applied to financial years beginning in 2021, while the first mandated reports—and hence the first TPI assessments observing them—appear in 2022, treated firms had a regulatory ramp-up window in which anticipatory governance investments could occur [24,36]. This motivates a secondary hypothesis:

Hypothesis 2 (H2). *Any mandate effect on climate-governance quality is concentrated in the announcement-to-first-report window, such that estimators allowing for anticipation recover larger effects than no-anticipation estimators.*

3. Data and Methods

3.1. Sample and Outcome Variable

The sample comprises all firms headquartered in the United Kingdom, the United States, France, and Australia that received a TPI Management Quality assessment in any year from 2015 to 2025. TPI, an academic initiative based at the London School of Economics, assesses publicly listed, predominantly high-emitting companies using a published methodology applied consistently by trained analysts to firms' public disclosures [18]. The MQ framework assigns each firm-year an ordinal level from 0 to 5: Level 0 (unaware of climate change as a business issue), Level 1 (acknowledging climate change as a business issue), Level 2 (building capacity), Level 3 (integrating into operational decision-making), Level 4 (strategic assessment), and, in later methodology vintages, Level 5 (transition planning and implementation). Underlying indicators include the existence of a climate policy, emissions disclosure and verification, quantitative targets, board oversight and executive responsibility for climate issues, and disclosure of transition plans. Because assessments are external, criteria-based, and comparable across firms and years, MQ measures the quality of climate governance rather than the quantity of climate reporting, and—being a transparent staircase applied by a single academic assessor—avoids the aggregation divergence documented for commercial ESG ratings [61,62]. TPI also publishes the binary sub-indicators (e.g., existence of board-level oversight, quantitative targets, scenario planning, capital-expenditure alignment) that underlie each firm's level for its most recent assessment; we exploit these in a component-level analysis (Section 4.8) to locate where governance headroom remains.

The raw extract contains 4130 assessment records for 837 firms. TPI periodically re-assesses firms within the same year under successive methodology vintages (v1–v5); 1289 records are same-firm-same-year duplicates arising from overlapping vintages, and the vintages disagree on the MQ level in 140 of these firm years. We retain one observation per firm-year, keeping the assessment under the most recent methodology vintage, which yields an unbalanced estimation panel of 2841 firm-year observations; retaining the earliest vintage instead leaves results unchanged (Section 4.4). Table 1 reports the panel composition: 663 US firms (2161 firm years), 63 UK firms (246), 61 Australian firms (263), and 50 French firms (171). Coverage expands markedly from 2023 as TPI broadened its

universe, a feature the unbalanced-panel estimators accommodate and which motivates a balanced-subsample robustness check.

Table 1. Sample composition: firm-year observations by country and TPI assessment year.

Country	2015	2016	2017	2018	2019	2020	2021	2022	2023	2024	2025
Australia	0	4	6	17	17	9	30	31	39	57	53
France	1	0	4	7	10	6	11	12	27	47	46
United Kingdom	0	6	6	10	17	9	19	22	36	61	60
United States	7	26	34	63	100	71	121	156	331	635	617
Total	8	36	50	97	144	95	181	221	433	800	776

Notes: Firm-year observations after retaining one assessment per firm-year (latest methodology vintage). Total $N = 2841$ across 837 firms (Australia 61, France 50, United Kingdom 63, United States 663). Source: authors' calculations from TPI Management Quality assessments.

3.2. Treatment Definition and Timing

Treatment is defined at the country level as the first TPI assessment year in which mandated climate disclosures are observable. For the United Kingdom, this is 2022: FCA-mandated TCFD reports for financial years beginning in 2021 were published in spring 2022 and entered that year's assessments [6,7]. For France, the primary cohort is 2025; the first year CSRD wave-one reports are observable [8]; because France was partially pre-treated by Article 173 from fiscal year 2016, we re-estimate all models dating French treatment to 2017 as a robustness check. Australia's cohort year is 2026, outside the sample window, so Australian firms contribute only never-treated-in-sample observations and enter the not-yet-treated control set. US firms are never treated [11]. The resulting treatment indicator equals one for UK firm years from 2022 (179 treated firm years) and French firm years in 2025 (46 treated firm years) and zero otherwise.

Two features of this coding warrant emphasis. First, France enters the panel only weakly pre-treated: Article 173 of the 2015 Energy Transition Law bound institutional investors and large-company climate reporting from fiscal year 2016, but not the firm-level, comply-or-explain climate-governance regime studied here, which arrives with CSRD wave one whose first reports are observable in 2025; dating French treatment to 2017 tests sensitivity to this ambiguity and leaves the null intact (a robustness check reported in Section 4.4). Second, because Australia's first mandated reports fall in 2026, Australian firms are genuinely not-yet-treated within our window. Any pre-2026 anticipatory preparation by Australian firms would, if anything, contaminate the control group in the direction of a spuriously positive mandate effect, so treating them as clean not-yet-treated controls is conservative for our null. Because France contributes a single post-treatment year and Australia none, the causal estimate is identified principally from the UK 2022 cohort—a limitation of the identifying variation that we make explicit in Sections 4 and 5 rather than obscure.

3.3. Empirical Strategy

The benchmark specification is the two-way fixed-effects (TWFE) difference-in-differences regression $MQ_{it} = \beta \cdot Post_{it} + \alpha_i + \lambda_t + \varepsilon_{it}$, where α_i are firm fixed effects, λ_t are assessment-year fixed effects, and standard errors are clustered at the firm level [63,64]. We augment the benchmark with methodology-vintage fixed effects, to absorb scoring changes across TPI methodology versions, and with sector-by-year fixed effects, which compare firms within the same TPI sector and year. Because TWFE is biased under staggered adoption with heterogeneous treatment effects [21–23], our preferred estimates come from two heterogeneity-robust estimators: the interaction-weighted event-study estimator

of Sun and Abraham [20], implemented with never-treated and not-yet-treated cohorts as controls, and the group-time average treatment effects of Callaway and Sant’Anna [19], aggregated to overall, group, and dynamic (event-time) effects, with doubly robust estimation, the not-yet-treated as the primary control group, a universal base period (event time -1), and inference from a multiplier bootstrap with 1000 iterations. We probe pre-trends through the event-study coefficients and treat them with the caution urged by the recent econometrics literature [24,36,37,65], reporting an anticipation-adjusted specification motivated by the December 2020 announcement of the UK rule (H2).

Because the MQ level is an ordinal 0–5 staircase rather than a cardinal variable, we complement the linear specifications with three checks that do not treat the levels as equally spaced. First, we estimate pooled ordered logit and ordered probit models with assessment-year and country effects, which allow the treatment to shift the entire ordinal distribution non-linearly. Second, we estimate the fixed-effects ordered logit of Baetschmann, Staub, and Winkelmann [66]—the “blow-up-and-cluster” (BUC) estimator—which dichotomizes the outcome at every cutpoint and applies conditional (within-firm) logit, thereby retaining firm fixed effects while respecting the ordinal scale. Third, we re-estimate the model on a binary upward-transition outcome equal to one when a firm moves up at least one MQ level relative to its previous assessment. We also probe treatment-effect heterogeneity by re-estimating the benchmark separately for high-emitting sectors (energy, utilities, materials and mining, and heavy transport) and for lower-emitting sectors, and by interacting treatment with an indicator for firms whose first observed MQ level was low (≤ 2), where governance headroom was greatest. Finally, because treatment is assigned at the country level and only two jurisdictions are effectively treated, firm-level clustering could misstate precision; we therefore report a wild-cluster bootstrap clustered on country [67] as a check on the null, recognizing that with four clusters bootstrap p -values are themselves imprecise.

All data preparation, estimation, and graphics were conducted in RStudio with R version 4.3.3 [68,69], using the fixest package (version 0.14.1) for fixed-effects and Sun–Abraham estimation [70], the did package (version 2.3.0) for Callaway–Sant’Anna estimation [19], data.table (1.18.4) for data management, and ggplot2 (3.4.4) for all figures [71]. Ordinal and wild-bootstrap checks were computed with the corresponding standard R implementations. Replication code and the estimation panel are available from the authors.

3.4. Variables

Table 2 defines every variable used in the estimation and states how each is constructed from the underlying TPI assessment files.

Table 2. Variable definitions.

Variable	Definition/Construction
MQ level	TPI Management Quality level (0–5), externally assessed climate-governance quality; one observation per firm-year (latest methodology vintage).
Post (D_{it})	Indicator equal to 1 from a country’s first treated assessment year onward: United Kingdom 2022; France 2025 (2017 in robustness); Australia 2026 (outside window); United States never treated.
Cohort year (g)	First, treated assessment year of the firm’s country; 0 for never-treated-in-sample firms in the Callaway–Sant’Anna estimation.
Event time	Assessment year minus cohort year.
High-MQ	Indicator equal to 1 if MQ level ≥ 4 (robustness outcome addressing ceiling effects).
Methodology vintage	TPI methodology version (v1–v5) under which the assessment was made; used as fixed effects.
Sector	TPI sector classification (58 sectors); used in sector-by-year fixed effects.

Notes: All variables are constructed from the TPI assessment files; no additional data were collected.

4. Results

4.1. Descriptive Evidence

Table 3 reports descriptive statistics. The mean MQ level across the pooled panel is 3.15 (SD 0.93); the distribution is concentrated at Level 3 (1479 firm years), with 672 firm years at Level 4 and 200 at Level 5. Firms in ever-treated countries (UK and France) average 3.59, against 3.01 for never-treated US firms—a large pre-existing level difference that firm fixed effects absorb. Figure 1 plots mean MQ by country and year and contains the paper’s central descriptive fact: never-mandated US firms improved from 1.43 in 2015 to 3.11 in 2025, a gain of 1.7 levels achieved entirely without a binding mandate, while UK and French firms entered the panel already above 3.4 and improved modestly toward 4.0. The simple 2×2 comparison around the UK mandate makes the same point: UK mean MQ rose from 3.46 before 2022 to 3.84 after, a gain of 0.375, while US mean MQ rose from 2.75 to 3.07 over the same split, a gain of 0.324—a raw difference-in-differences of just +0.051 levels.

Table 3. Descriptive statistics for the MQ level.

Group	N	Mean	SD	Median	P75
Full panel	2841	3.15	0.93	3	4
Ever treated (UK, France)	680	3.59	0.90	4	4
Never treated (US)	2161	3.01	0.89	3	3
MQ distribution (levels 0/1/2/3/4/5)	22/124/344/1479/672/200				

Notes: Australia is grouped with never-treated-in-sample observations in the Callaway–Sant’Anna estimation because its cohort year (2026) lies outside the panel window. P75 = 75th percentile.

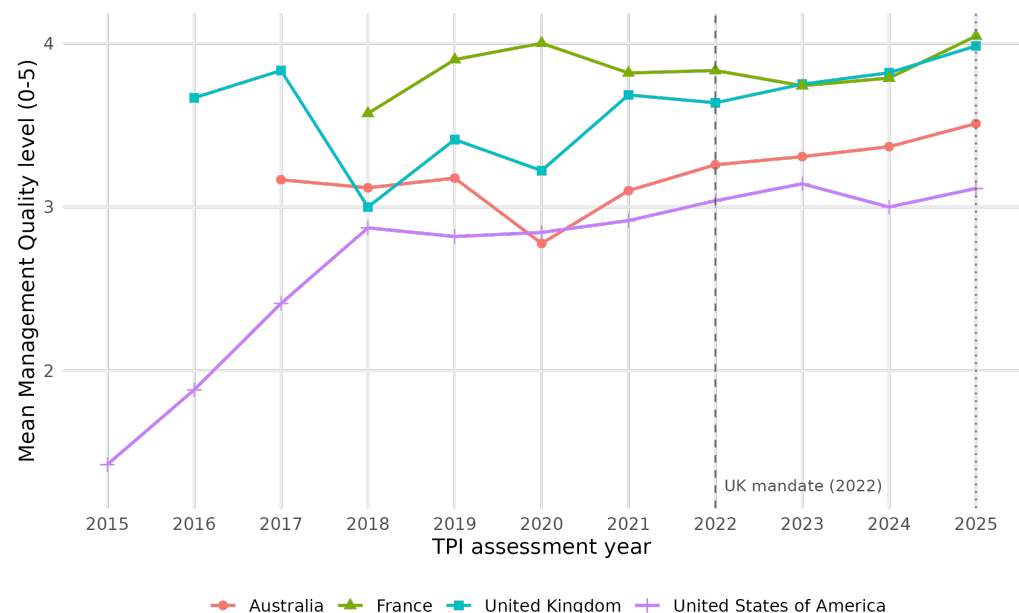


Figure 1. Mean TPI Management Quality level by country and assessment year, 2015–2025. The dashed vertical line marks the first UK assessments observing mandated TCFD reports (2022); the dotted line marks the first French assessments observing CSRD reports (2025). Cells with fewer than five observations are suppressed. Produced in RStudio with ggplot2.

4.2. Main Difference-in-Differences Estimates

Table 4 reports the main estimates. The TWFE benchmark yields a post coefficient of 0.114 (clustered SE 0.092; $p = 0.216$): treated firms gained about a tenth of an MQ level relative to controls, an effect not distinguishable from zero. Adding methodology-vintage fixed effects leaves the estimate unchanged (0.113, SE 0.092), and the within- R^2 of 0.002 shows the mandate explains essentially none of the within-firm variation in governance

quality. Comparing firms within sector-year cells raises the point estimate to 0.153 (SE 0.082; $p = 0.064$), marginally significant at the 10% level—the only conventional specification to approach significance, and modest in magnitude (about one-sixth of a standard deviation). The heterogeneity-robust estimators sit below the TWFE benchmark: the Sun–Abraham aggregated ATT is 0.074 (SE 0.104; $p = 0.480$), and the Callaway–Sant’Anna overall ATT is 0.082 (bootstrap SE 0.155), with a 95% confidence interval of -0.221 to 0.385 . Using only never-treated US firms as controls produces an identical point estimate (0.082, SE 0.165). Because the French 2025 cohort contributes a single post-treatment year, the Callaway–Sant’Anna group aggregation is identified from the UK 2022 cohort (group ATT 0.082, SE 0.161); the French group-time effect could not be estimated with a finite standard error and is excluded from aggregation. H1 is therefore not supported: across estimators, mandates are associated with no statistically significant improvement in climate-governance quality, and the interval estimates exclude effects larger than roughly 0.3–0.4 levels (about 0.4 standard deviations).

Table 4. Effect of climate-disclosure mandates on the TPI Management Quality level.

	(1) TWFE	(2) TWFE	(3) TWFE	(4) SA	(5) CS, NYT	(6) CS, NT
Post (ATT)	0.114 (0.092)	0.113 (0.092)	0.153 [†] (0.082)	0.074 (0.104)	0.082 (0.155)	0.082 (0.165)
95% CI	—	—	—	—	[$-0.221, 0.385$]	[$-0.241, 0.406$]
Firm FE	Yes	Yes	Yes	Yes	—	—
Year FE	Yes	Yes	—	Yes	—	—
Vintage FE	No	Yes	No	No	—	—
Sector $\times$ Year FE	No	No	Yes	No	—	—
Observations	2812	2812	2749	2812	2841	2841
Adjusted R ²	0.722	0.722	0.729	0.725	—	—

Notes: The dependent variable is the MQ level (0–5). Columns (1)–(4) are estimated with fixest in RStudio; standard errors (in parentheses) are clustered by firm; singleton fixed-effect observations are dropped. Column (4) reports the aggregated ATT from the Sun–Abraham interaction-weighted event-study estimator. Columns (5)–(6) report the overall ATT from the Callaway–Sant’Anna estimator (did package; doubly robust; universal base period; multiplier bootstrap, 1000 iterations) with not-yet-treated (NYT) and never-treated (NT) controls, respectively. Em dashes (—) indicate the entry is not applicable to that column (e.g., a 95% CI is not reported for the TWFE and Sun–Abraham columns, and the firm/year/vintage/sector fixed-effects rows do not apply to the Callaway–Sant’Anna columns). [†] $p < 0.10$; no estimate is significant at the 5% level.

4.3. Event-Study Dynamics and Pre-Trends

Figure 2 plots the Callaway–Sant’Anna dynamic effects, Figure 3 the corresponding Sun–Abraham event study, and Table 5 reports the underlying coefficients for both. Post-treatment effects are flat and insignificant in both frameworks: at event times 0, +1, and +2 the Callaway–Sant’Anna estimates are -0.063 (SE 0.175), 0.143 (SE 0.217), and 0.166 (SE 0.199), and the Sun–Abraham counterparts at event times 0 through +3 range from -0.029 to 0.172 , all insignificant. There is no evidence of a treatment effect that builds with exposure. The pre-period, however, is not innocuous. Relative to the reference year (event time -1 , i.e., UK assessments in 2021), several earlier pre-treatment coefficients are negative—Sun–Abraham estimates of -0.317 (SE 0.145) at -4 and -0.254 (SE 0.122) at -3 , and a Callaway–Sant’Anna estimate of -0.644 (SE 0.239) at -2 whose simultaneous confidence band excludes zero. The pattern indicates that treated firms’ governance quality rose relative to controls during 2020–2021, the window between the announcement of the UK rule (December 2020) and the first mandated reports. Two readings are possible: anticipatory compliance investments (consistent with H2) or a pandemic-year dip in UK assessments in 2020 followed by recovery. We return to this in Section 4.5; under either reading, the standard no-anticipation estimates in Table 4 are the conservative benchmark.

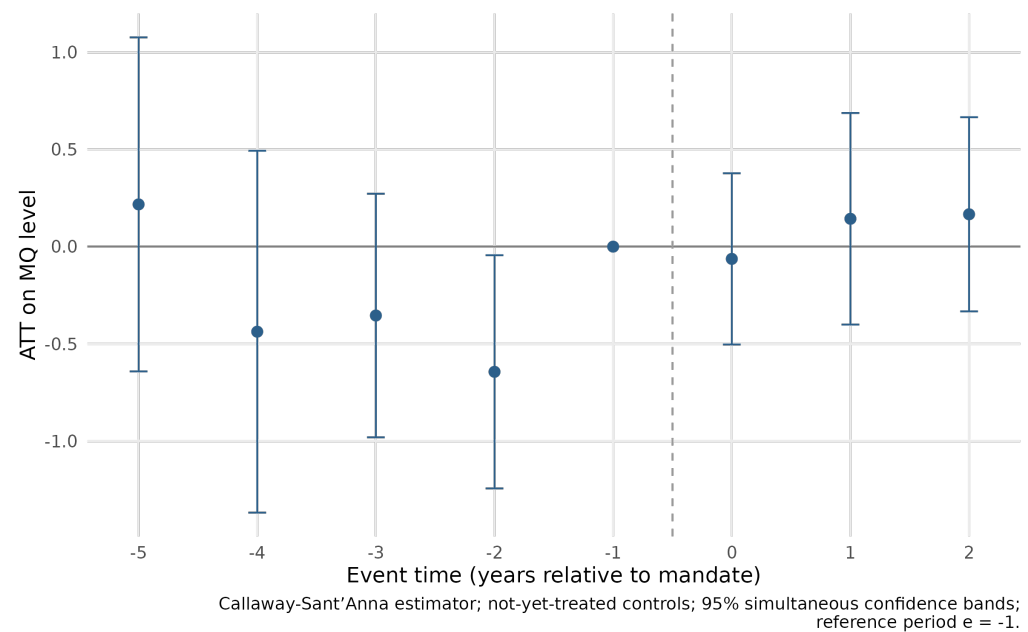


Figure 2. Callaway–Sant’Anna dynamic treatment effects on the MQ level, with not-yet-treated controls and a universal base period (event time -1). Blue markers are the estimated ATT at each event time and the vertical bars are the 95% simultaneous confidence bands from a multiplier bootstrap (1000 iterations); the vertical dashed line marks the reference period (event time -1). Produced in RStudio with ggplot2.

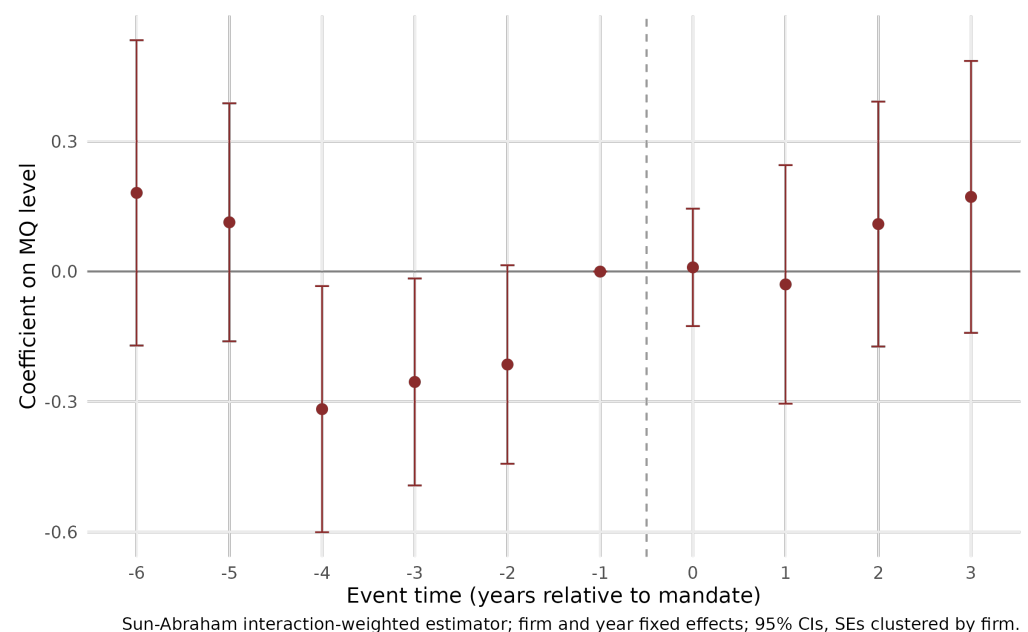


Figure 3. Sun–Abraham interaction-weighted event-study estimates with firm and year fixed effects. Dark-red markers are the interaction-weighted coefficients at each event time and the vertical bars are the 95% confidence intervals based on firm-clustered standard errors; the vertical dashed line marks the reference period (event time -1). Produced in RStudio with ggplot2.

4.4. Robustness

Table 6 and Figure 4 show a battery of robustness tests on the null; all the conventional specifications reproduce it. The Post coefficient in a comparison of the cleanest two-country designs (UK treated firms and US firms that were never treated) is 0.018 (at SE 0.161, $p = 0.911$). The corresponding Callaway–Sant’Anna group effect for the 2017 cohort could not be estimated within a finite standard error, as only one French firm was found before

2017, so the French margin should be used from the TWFE specification. Considering the oldest instead of the most recent methodology, vintage per firm-year yields virtually the same estimate: 0.114 (with SE 0.090). Focusing on the boundary where already high-scoring UK firms are able to move, by using a high-MQ indicator ($MQ \geq 4$) instead of the ordinal, results in 0.032 (SE 0.040; $p = 0.427$) or an insignificant 3.2-percentage-point shift in the probability of strategic-level governance. Using the pre-mandate sample (2015–2021), the placebo mandated to UK firms is small, negative, and insignificant (-0.143 , SE 0.159; $p = 0.372$), which supports the parallel-trends benchmark in the years preceding the announcement window. Standard errors are larger when the clustering is done on both firm and year (0.114, SE 0.102), and when the clustering period is limited to the 174 firms that have been assessed in at least five years, the outcome is 0.070 (SE 0.130, $p = 0.589$). The TWFE analysis of the UK's treatment of the announcement-affected assessment year 2021 is 0.128 (SE 0.098; $p = 0.191$). Finally, because treatment is assigned at the country level, we address the concern that firm-level clustering understates the true sampling variability of a country-level intervention [63]. Clustering the standard errors on country (four clusters) leaves the point estimate unchanged and yields a country-clustered standard error of 0.067; a wild-cluster bootstrap with the null imposed (Rademacher weights) returns $t = 1.65$ and a bootstrap p -value of 0.078 (Table 6, row 13). The coefficient thus remains statistically insignificant at conventional 5% levels under country-level inference, so the null is not an artifact of the level at which standard errors are clustered; we caution, however, that with only four clusters—two of them effectively treated—bootstrap inference is itself imprecise [67], and we therefore rely on the firm-clustered specifications as our primary inference.

Table 5. Event-study coefficients (dependent variable: MQ level).

Event Time	CS Estimate	CS SE	SA Estimate	SA SE
−6	—	—	0.181	(0.179)
−5	0.217	(0.342)	0.114	(0.140)
−4	−0.437	(0.370)	−0.317 *	(0.145)
−3	−0.354	(0.249)	−0.254 *	(0.122)
−2	−0.644 *	(0.239)	−0.214 †	(0.117)
−1 (ref.)	0	—	0	—
0	−0.063	(0.175)	0.010	(0.069)
+1	0.143	(0.217)	−0.029	(0.140)
+2	0.166	(0.199)	0.110	(0.144)
+3	—	—	0.172	(0.160)

Notes: CS = Callaway–Sant’Anna dynamic aggregation (not-yet-treated controls, universal base period; the CS asterisk denotes a 95% simultaneous confidence band excluding zero). SA = Sun–Abraham interaction-weighted estimator (firm and year fixed effects; firm-clustered SEs; * $p < 0.05$, † $p < 0.10$). An em dash (—) indicates the coefficient is not estimated at that event time under the corresponding estimator. Sun–Abraham coefficients at event times −10, −8, and −7, identified from very few observations, are estimated imprecisely (0.232, 0.109, and −0.446, respectively) and omitted from the figure for readability.

4.5. The Anticipation Specification

Row (12) of Table 6 reports the one specification that departs from the null and bears directly on H2. Treating event time −1 (the 2021 assessment year, after the December 2020 announcement of the UK rule but before the first mandated reports) as already affected, the Callaway–Sant’Anna estimator with one year of anticipation benchmarks treated–control comparisons against event time −2 and recovers an overall ATT of 0.684 levels (SE 0.220; 95% CI 0.253 to 1.115)—a large, statistically significant effect concentrated in the announcement-to-first-report window. Taken at face value, this supports H2: firms upgraded their climate governance when compliance became certain, not when

the first mandated report was filed, and no-anticipation estimators absorb that upgrade into the baseline. We urge caution, however, before adopting this as the headline estimate. The shifted base period is 2020, a pandemic year in which the mean UK MQ dipped to 3.22 (from 3.41 in 2019) while US assessments were flat (2.85 versus 2.82); re-benchmarking against a temporarily depressed treated-group year mechanically inflates the estimated effect. The earlier pre-period coefficients in Table 5 also drift over 2018–2019, so the flat-pre-trend condition holds only approximately under the shifted base period [36,37]. The honest characterization is that the data are consistent with either a genuine announcement effect of roughly 0.6–0.7 levels or with no effect and a pandemic-induced dip-and-recovery in UK assessments; the post-implementation evidence, which is robustly null, cannot distinguish them. We therefore present the anticipation estimate as suggestive support for H2 rather than as the paper’s central finding.

Table 6. Robustness of the mandate effect (dependent variable: MQ level unless noted).

Specification	Estimate	SE	p-Value	N
(1) Baseline TWFE	0.114	(0.092)	0.216	2812
(2) +Methodology-vintage FE	0.113	(0.092)	0.220	2812
(3) Sector $\times$ year FE	0.153	(0.082)	0.064	2749
(4) UK vs. US only	0.018	(0.161)	0.911	2382
(5) France cohort = 2017 (Art. 173)	0.021	(0.153)	0.892	2812
(6) Earliest-vintage deduplication	0.114	(0.090)	0.206	2812
(7) High-MQ indicator (MQ $\geq$ 4)	0.032	(0.040)	0.427	2812
(8) Placebo: UK mandate in 2019 (pre-period only)	−0.143	(0.159)	0.372	590
(9) Two-way clustering (firm and year)	0.114	(0.102)	0.289	2812
(10) Firms with $\geq$ 5 assessment years	0.070	(0.130)	0.589	1264
(11) UK treatment dated from 2021 announcement	0.128	(0.098)	0.191	2812
(12) CS with one year of anticipation	0.684 *	(0.220)	[0.253, 1.115]	2841
(13) Country-clustered wild bootstrap	0.114	(0.067)	0.078	2812

Notes: Rows (1)–(11) are TWFE estimates with firm and assessment-year fixed effects (except as noted) and firm-clustered standard errors, estimated with fixest in RStudio. Row (12) is the Callaway–Sant’Anna overall ATT allowing one year of anticipation (base period shifted to event time -2); the bracketed range is the 95% confidence interval. Row (13) re-estimates the baseline with standard errors clustered on country (four clusters); the SE shown is the CRV1 country-clustered standard error, and the reported p -value is from a wild-cluster bootstrap with the null imposed (Rademacher weights, 999 replications), which with only four clusters should itself be read as approximate. * 95% confidence interval excludes zero. The placebo in row (8) restricts the sample to 2015–2021 and assigns a counterfactual UK mandate in 2019.

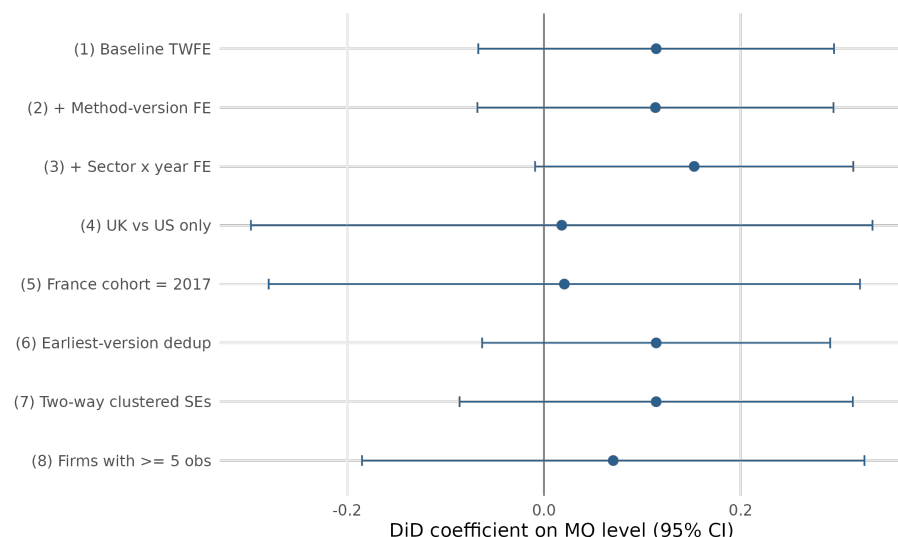


Figure 4. Robustness of the difference-in-differences coefficient across specifications (rows (1)–(6) and (9)–(10) of Table 6). Blue markers are the point estimates for each specification and the horizontal bars are the 95% confidence intervals based on the reported standard errors; the vertical reference line marks zero (no effect). Produced in RStudio with ggplot2.

4.6. Ordinal-Outcome and Upward-Transition Robustness

Because the MQ level is an ordinal staircase, the linear specifications in Tables 4–6 could in principle mask non-linear movement in the governance distribution. Table 7 reports three checks that respect the ordinal scale. A pooled ordered logit with assessment-year and country effects yields a post coefficient of 0.131 (SE 0.224; $p = 0.558$), and the ordered probit counterpart is 0.139 (SE 0.127; $p = 0.271$); in neither model does the mandate shift the ordinal distribution significantly. The fixed-effects (blow-up-and-cluster) ordered logit of Baetschmann, Staub, and Winkelmann [66], which retains within-firm variation while treating the outcome ordinally, returns 0.273 (SE 0.148; $p = 0.065$)—marginally significant at the 10% level and closely mirroring the sector-by-year TWFE estimate in magnitude and precision, but not significant at conventional 5% levels. We also test the reviewers' suggestion of an explicit level-transition outcome. A linear probability model for moving up at least one MQ level relative to the previous assessment gives -0.014 (SE 0.056; $p = 0.802$) with firm and year fixed effects, against an unconditional upgrade rate of 18.5%; a cumulative indicator for scoring above the firm's own first-observed level is 0.069 (SE 0.052; $p = 0.183$); and a pooled probit on the year-on-year upgrade indicator is likewise insignificant (0.171, SE 0.188; $p = 0.363$). Across ordinal and transition-based formulations, the conclusion is unchanged: mandates are not associated with a statistically significant improvement in climate-governance quality, and the linear approximation does not conceal a non-linear transition.

Table 7. Ordinal-outcome and upward-transition robustness (dependent variable as noted).

Specification	Estimate	SE	p-Value	N
(1) Ordered logit (pooled; year + country)	0.131	(0.224)	0.558	2841
(2) Ordered probit (pooled; year + country)	0.139	(0.127)	0.271	2841
(3) FE ordered logit (BUC; firm effects)	0.273	(0.148)	0.065	818 ^a
(4) Moved up ≥ 1 level, YoY (LPM; firm + year FE)	-0.014	(0.056)	0.802	1612
(5) Above own first-observed level (LPM; firm + year FE)	0.069	(0.052)	0.183	2812
(6) Moved up ≥ 1 level, YoY (probit; pooled)	0.171	(0.188)	0.363	1612

Notes: Rows (1), (2), and (6) are pooled maximum-likelihood models with assessment-year and country indicators. Row (3) is the Baetschmann–Staub–Winkelmann fixed-effects ordered logit [66], which dichotomizes the outcome at every cutpoint and applies conditional within-firm logit; ^a the reported figure is the number of within-firm strata contributing identifying variation. Rows (4)–(5) are linear probability models with firm and assessment-year fixed effects and firm-clustered standard errors. The dependent variable in rows (4) and (6) equals one if a firm's MQ level exceeds its level at the previous assessment; in row (5), it equals one if the current level exceeds the firm's first observed level. No estimate is significant at the 5% level.

4.7. Sectoral and Baseline Heterogeneity

The pooled null could conceal offsetting effects across firms with different exposure to climate regulation. We therefore split the sample by sector emission intensity and by pre-mandate governance level (Table 8). Estimated separately, high-emitting sectors (energy, utilities, materials and mining, and heavy transport) show no mandate effect (-0.011 , SE 0.132; $p = 0.936$), while lower-emitting sectors show a positive and significant coefficient (0.316, SE 0.107; $p = 0.003$). A pooled interaction model, however, shows that the differential between the two groups is not statistically significant: with the low-emitting group as the baseline, the Post $\times$ high-emitting interaction is -0.155 (SE 0.165; $p = 0.348$). The low-emitting effect is also fragile to the identifying comparison—it retains significance under two-way clustering (0.316, $p = 0.022$) and sector-by-year fixed effects (0.319, $p = 0.001$) but loses it when the comparison is restricted to UK-treated versus US never-treated firms (0.403, SE 0.289; $p = 0.164$). A second split, interacting treatment with an indicator for firms whose first observed MQ level was low (≤ 2), returns a large coefficient on the interaction

(0.891, SE 0.176; $p < 0.001$) but is identified from only a handful of low-baseline treated firms and should be read as descriptive of where headroom existed rather than as a precise causal estimate. Taken together, these results are suggestive that any governance response to mandates was concentrated among lower-intensity or lower-starting firms with more room to move, while the high-emitting firms that dominate the TPI universe—and that disclosure mandates first targeted—show no detectable effect. We present this heterogeneity as a qualification of the headline null, not as evidence of a robust conditional treatment effect.

Table 8. Sectoral and baseline heterogeneity in the mandate effect (dependent variable: MQ level).

Specification	Estimate	SE	p-Value	N
(1) High-emitting sectors only (TWFE)	−0.011	(0.132)	0.936	1547
(2) Lower-emitting sectors only (TWFE)	0.316	(0.107)	0.003	1265
(3) Interaction: Post (lower-emitting baseline)	0.210	(0.105)	0.045	2812
(4) Interaction: Post × high-emitting (differential)	−0.155	(0.165)	0.348	2812
(5) Interaction: Post (high initial MQ baseline)	0.013	(0.084)	0.879	2812
(6) Interaction: Post × low initial MQ (≤ 2)	0.891	(0.176)	0.000	2812

Notes: All models are TWFE regressions with firm and assessment-year fixed effects and firm-clustered standard errors, estimated with fixest in RStudio. High-emitting sectors are energy, utilities, materials and mining, and heavy transport; lower-emitting sectors are the remainder of the TPI universe. Rows (3)–(4) report a single interacted regression in which Post measures the effect for lower-emitting firms and Post × high-emitting the differential; rows (5)–(6) report a separate interacted regression in which low initial MQ equals one if the firm's first observed level was ≤ 2 . The low-emitting effect in row (2) is significant under two-way clustering ($p = 0.022$) and sector-by-year fixed effects ($p = 0.001$) but not under a UK-versus-US-only comparison (0.403, SE 0.289; $p = 0.164$). The row-(6) interaction is identified from a few low-baseline treated firms and is descriptive.

4.8. Which Governance Components Move?

The policy prescription that disclosure should be paired with substantive obligations is sharper if we can say which governance components remain weak. TPI's binary sub-indicators, which underlie each firm's MQ level, allow exactly this decomposition. Table 9 and Figure 5 report, for each indicator, the share of firms meeting it in the most recent assessment, by country. The pattern is stark and consistent with the codification reading of our main result. The foundational and mid-staircase components are effectively saturated among mandated firms: every UK firm has a climate policy, discloses Scope 1 and 2 emissions, sets quantitative targets, and—critically for the governance question—maintains board-level oversight of climate issues (100% in the UK and France, versus 88.9% in the US), so there was almost no room for a mandate to raise these margins further. Where firms fall away, in every jurisdiction, is at the top of the staircase, on precisely the transition-planning substance that disclosure rules do not compel: only 46.7% of UK firms (and 7.9% of US firms) quantify the financial implications of their transition strategy, only 60.0% (versus 21.0% in the US) use an internal carbon price, and the capital-alignment indicators collapse everywhere—13.3% of UK firms and 3.2% of US firms align capital expenditure with decarbonization, and 10.0% of UK firms and 2.4% of US firms disclose a plan to phase out carbon-intensive assets. In other words, mandated firms had already climbed to universal board oversight and basic disclosure before the mandate, while the components that would represent genuine forward-looking governance—quantified strategy, internal carbon pricing, and capital-expenditure alignment—remain rare under both mandatory and voluntary regimes. This locates the unfinished agenda squarely at the transition-plan margin and gives concrete content to the recommendation that disclosure obligations be paired with substantive transition-plan requirements.

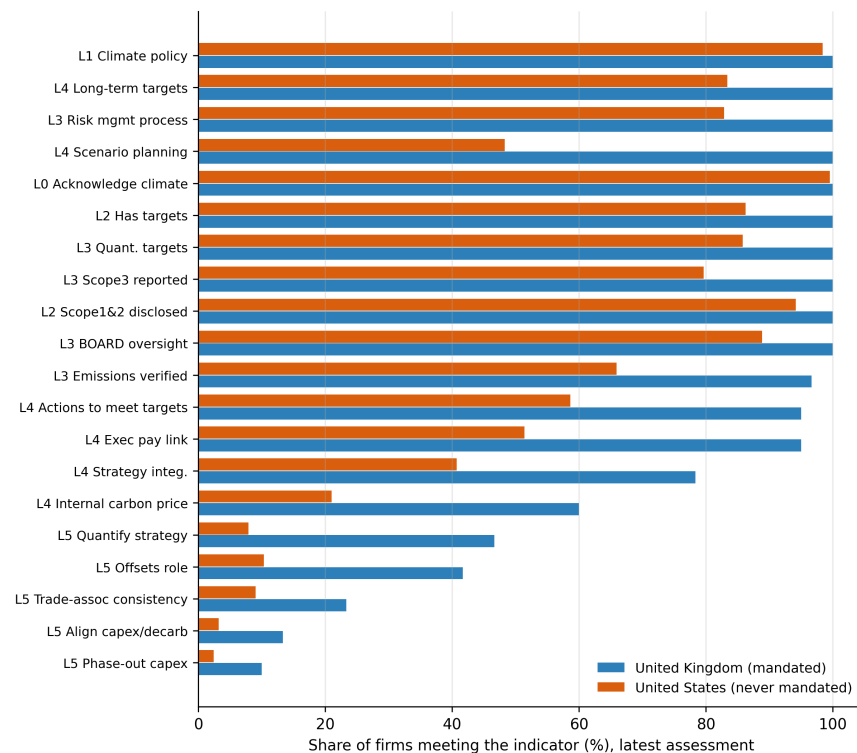


Figure 5. Prevalence of TPI governance sub-indicators, United Kingdom versus United States, most recent assessment. Bars show the share of firms meeting each indicator; indicators are ordered from foundational (top) to transition-planning (bottom). Board oversight and basic disclosure are near-universal among mandated UK firms, while transition-plan and capital-alignment indicators are rare in both jurisdictions.

Table 9. Prevalence of TPI governance sub-indicators by country, most recent assessment (% of firms meeting the indicator).

Governance Sub-Indicator	UK	US	FR	AU	UK – US
Acknowledge climate as business issue	100.0	99.5	100.0	100.0	0.5
Climate policy in place	100.0	98.4	100.0	100.0	1.6
Emission-reduction targets	100.0	86.3	100.0	93.0	13.7
Scope 1 & 2 emissions disclosed	100.0	94.2	100.0	100.0	5.8
Board/executive oversight of climate	100.0	88.9	100.0	96.5	11.1
Quantitative targets set	100.0	85.8	100.0	93.0	14.2
Scope 3 emissions reported	100.0	79.6	100.0	78.9	20.4
Emissions verified/assured	96.7	65.9	95.7	84.2	30.8
Climate risk-management process	100.0	82.9	100.0	96.5	17.1
Long-term (2050) targets	100.0	83.4	100.0	91.2	16.6
Executive remuneration linked to climate	95.0	51.4	100.0	61.4	43.6
Climate integrated into strategy	78.3	40.7	91.3	19.3	37.6
Scenario planning conducted	100.0	48.3	93.5	68.4	51.7
Internal carbon price used	60.0	21.0	69.6	35.1	39.0
Actions disclosed to meet targets	95.0	58.6	100.0	68.4	36.4
Quantify financial implications of strategy	46.7	7.9	30.4	15.8	38.8
Role of offsets disclosed	41.7	10.3	39.1	10.5	31.3
Plan to phase out carbon-intensive assets	10.0	2.4	21.7	1.8	7.6
Capital expenditure aligned with decarbonization	13.3	3.2	19.6	14.0	10.1
Trade-association positions consistent with climate	23.3	9.0	17.4	22.8	14.3

Notes: Each cell reports the percentage of a country's firms meeting the indicator in their most recent TPI assessment, computed from the binary sub-indicators that TPI publishes alongside each firm's Management Quality level. Indicators are ordered approximately from the bottom (Level 0–1) to the top (Level 5) of the MQ staircase. The final column is the UK-minus-US percentage-point gap. Foundational and mid-staircase indicators (policy, disclosure, targets, board oversight) are near-universal among mandated firms, whereas top-of-staircase transition-planning indicators (quantified strategy, internal carbon price, capital-expenditure alignment, asset phase-out) remain rare in every jurisdiction.

5. Discussion

Three facts organize the interpretation. First, mandates arrive late on the governance staircase: UK firms averaged MQ 3.46 before a single mandated report was filed, meaning the median treated firm had already integrated climate change into operational decision-making under purely voluntary and investor-driven regimes. Second, never-mandated US firms achieved the largest governance improvement in the panel—1.7 levels over a decade—during precisely the period in which institutional investors organized around climate risk: climate-risk pricing and disclosure demand by institutional investors [2,3,72–75], engagement by large asset managers and coalitions [32–35], and the mainstreaming of ESG information in investment processes [76–78] all operated economy-wide and without legal compulsion. Third, the mandate itself moved nothing detectable at the margin once those forces had operated. Together these facts favor the market reading of our title question: for large listed firms in advanced economies—the firms TPI assesses and the firms disclosure mandates initially target—the binding constraint on climate-governance quality by the early 2020s was not the absence of a legal duty to disclose.

This interpretation does not imply that disclosure regulation is ineffective in general. The settings in which mandates have documented real effects—emissions reductions after the UK carbon-reporting mandate [27], benchmarking after facility-level disclosure [28], portfolio shifts after Article 173 [59], information-environment improvements across countries [29], and efficiency gains under market-based instruments such as energy-quota trading [79]—typically involve outcome margins (emissions, financing flows, energy efficiency, information asymmetry) or firm populations (smaller, less scrutinized firms) different from ours. Our result is also consistent with recent UK evidence: climate claims were rising before the mandate took force [15], and financial-performance effects [14] can coexist with unchanged governance scores if the mandate changed investor perceptions or internal resource allocation without moving firms across MQ levels. What our evidence resists is the stronger claim that disclosure mandates, by themselves, build climate governance in firms already subject to intense investor scrutiny. The governance-relevant question for such firms is whether mandates change the substance of transition plans—a margin that disclosure-based scores capture only at the top of the staircase, and that newer instruments (transition-plan requirements under UK and EU rules, IFRS S2 implementation guidance [10]) target directly.

The component decomposition in Section 4.8 gives this argument concrete content and sharpens the policy prescription. Mandated firms had already saturated the foundational and mid-staircase practices that a disclosure rule could plausibly compel—climate policy, emissions disclosure, quantitative targets, and, most tellingly for the governance question, board-level oversight, which every UK and French firm already maintained before any mandate bound them. The components on which firms fall short are uniformly at the top of the staircase and are exactly those that disclosure rules do not require in substance: quantifying the financial implications of the transition strategy, using an internal carbon price, and—most starkly aligning capital expenditure with decarbonization and committing to phase out carbon-intensive assets, practices that fewer than one in seven firms meet in any jurisdiction. A null average effect is therefore not surprising once the margins are separated: the mandate operated on components that were already near-universal and left untouched the components on which real governance headroom remains. The suggestive heterogeneity in Section 4.7, in which any response concentrates among lower-intensity and lower-starting firms rather than the high-emitting firms that dominate the sample, tells the same story from the cross-section: movement is possible only where firms had not already climbed the staircase, and the firms mandates were first targeted. For regulators, the implication is specific rather than general—pairing disclosure with obligations that

bite on capital allocation and quantified transition planning, not simply requiring more reporting of practices firms have already adopted.

It bears emphasis that a null effect on governance quality does not render disclosure mandates valueless. Standardized, externally comparable climate information improves transparency, comparability across firms, and the quality of the information environment on which investors, lenders, and market participants rely [12,29], and a growing body of work links richer firm-level information environments to market quality, liquidity, and price efficiency more broadly [80–82], with corresponding real consequences for the financing conditions firms face [83]. Our finding is narrower and specific to a single margin: for heavily scrutinized large firms, mandatory disclosure did not, on average, raise externally assessed governance quality beyond what market forces had already achieved. The transparency, accountability, and comparability rationales for disclosure regulation stand independent of that result.

The anticipation pattern deserves its own discussion because it carries a methodological lesson for the rapidly growing DiD literature on sustainability regulation. Climate-disclosure mandates are announced years before first compliance, and firms face strong incentives to be ready on day one. Evaluations that date treatment from first mandated reports therefore risk loading genuine effects into the pre-period baseline—our anticipation-adjusted estimate is five times the no-anticipation estimate—while evaluations that date treatment from announcement risk conflating policy effects with contemporaneous shocks, in our setting the pandemic. Researchers should report both, as we do, and be explicit that the choice is consequential [24,36,37]. For greenwashing-adjacent concerns [54–57], the null on governance quality alongside documented increases in reporting volume in the same jurisdiction [15] is itself informative: more climate prose without higher externally assessed governance quality is the signature symbolic compliance would leave in the data, although our design cannot rule out that firms near the top of the staircase made substantive improvements invisible to a six-level scale. The component evidence in Section 4.8 strengthens this reading: the practices that saturate under the mandate are the reportable, easily verifiable ones (policy statements, board committees, disclosed targets), while the substantive, costly transition-plan commitments that would be hardest to fabricate remain rare—a distribution consistent with mandates codifying the appearance of governance faster than its substance.

Limitations follow from the data and design. The TPI universe over-represents large, high-emitting listed firms, so the null generalizes to the population mandates first targeted, not to the broader corporate sector that later compliance waves will reach. France contributes a single post-treatment year and Australia none, so the causal estimate is identified principally from the UK cohort; the French and Australian regimes deserve re-evaluation when post-2025 assessment years accumulate. The MQ scale is ordinal and coarse, and ceiling effects compress the treated group's room to improve, although the high-MQ-indicator robustness check and the ordinal-outcome models (Section 4.6) address the margin where movement remained possible. Country-level treatment with two effective clusters limits design-based country-clustered inference; we report a wild-cluster bootstrap on country (Table 6, row 13) that leaves the null intact but is itself imprecise with four clusters, and firm-level clustering—standard in this setting—assumes away country-level shocks correlated with treatment timing, a caveat the placebo test only partially relieves [63,64]. The sectoral and baseline heterogeneity in Section 4.7 is suggestive rather than conclusive: the high-versus-low-emitting differential is not statistically significant, and the low-baseline interaction is identified from a few treated firms. The component analysis in Section 4.8 uses the cross-sectional snapshot of sub-indicators from each firm's most recent assessment rather than a firm-year panel, so it characterizes where governance headroom remains but

does not itself identify a treatment effect on individual components. Finally, we deliberately add no firm-level financial controls because covariates measured post-announcement are potentially endogenous to treatment; firm fixed effects absorb time-invariant heterogeneity, and the sector-by-year specification absorbs industry shocks, but unobserved time-varying firm-level confounders cannot be excluded.

6. Conclusions

Exploiting the staggered arrival of climate-disclosure mandates across the United Kingdom, France, Australia, and the United States, and measuring outcomes with an externally assessed climate-governance score for 837 firms over 2015–2025, this paper asked whether mandating disclosure builds corporate climate governance. The answer, estimated with current heterogeneity-robust difference-in-differences methods in RStudio, is that it did not detectably do so: the preferred estimate is 0.08 MQ levels with a confidence interval excluding effects larger than 0.4 levels, robust across estimators, control groups, treatment datings, outcome codings, placebo tests, and sample restrictions. The decade's genuine governance story unfolded among never-mandated US firms, whose scores converged from 1.4 toward the mandated jurisdictions' levels under investor and market pressure alone. An anticipation-adjusted estimator suggests mandates may have accelerated governance upgrades in the announcement window, but the estimate is confounded by pandemic-year assessments and is offered as a hypothesis for future work rather than a conclusion. Decomposing the governance score into its underlying indicators clarifies why the average effect is null: mandated firms had already reached near-universal board oversight and basic disclosure, while the forward-looking transition-plan practices that would represent substantive governance—quantified strategy, internal carbon pricing, and alignment of capital expenditure with decarbonization—remain rare under both mandatory and voluntary regimes.

For regulators, the implication is not that disclosure mandates are pointless—comparable, audited climate information has value to capital markets independent of governance effects [12,16], improving transparency, comparability, and the information available to investors and lenders even where it does not raise governance scores—but that transparency obligations imposed on heavily scrutinized firms should not be expected to transform their governance because markets had largely completed that work first. Jurisdictions whose objective is substantive corporate transition should focus on the margins mandates have not yet reached, which our component analysis identifies concretely: smaller firms entering later compliance waves, and—at the top of the governance staircase—the credibility and implementation of transition plans, internal carbon pricing, and capital-expenditure alignment, through outcome-based obligations that disclosure alone cannot substitute for. For researchers, the paper demonstrates both the value of externally assessed governance scores as policy-evaluation outcomes and the sensitivity of staggered DiD conclusions to anticipation assumptions in settings where regulation is announced long before it binds. As post-2025 assessment years accumulate for France and Australia, the design developed here can be re-estimated to test whether later mandates, arriving in a more mature stewardship environment, replicate the UK's null.

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