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Disclosure incentives, ESG visibility, and market valuation: Evidence from Korea's Corporate Value-Up Program

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Abstract

We examine how expanding participation through policy incentives changes the information conveyed by corporate disclosure and whether a firm's prior environmental, social, and governance (ESG) visibility shapes the market response. The Korea Corporate Value-Up Program offers a useful setting because a December 2025 tax amendment substantially increased participation without changing the underlying disclosure framework. Our analysis covers the full text of 1,086 filings and compares 145 firms that disclosed under the initial voluntary regime with 541 firms that disclosed after the incentive was introduced. Event-study estimates are evaluated against propensity-score-matched non-disclosing firms. The three-day matched-pair cumulative average abnormal return is 1.008 percentage points before the amendment but only 0.005 percentage points afterward. During the voluntary period, announcement returns are smaller for firms with higher prior ESG ratings, suggesting that disclosure provides less new information when a firm is already closely followed. Once participation becomes widespread, the announcement effect disappears. Instead, investors respond to the specificity of disclosed commitments, but only when those commitments come from firms with higher ESG ratings. These findings show that disclosure operates through two distinct channels: the decision to disclose and the credibility of what is disclosed. Policies that increase participation may therefore broaden disclosure coverage while weakening participation itself as a market signal.

Keywords: disclosure incentives; ESG visibility; event study; propensity-score matching; market valuation

JEL classification: G14; G34; G38; M41

1 Introduction

Over the past decade, policymakers have increasingly turned to disclosure-based regulation as a tool for promoting more sustainable and responsible corporate behavior. Rather than relying solely on prescriptive rules or direct intervention, governments and regulators now encourage firms to disclose information regarding their strategies, risks, commitments, and long-term plans. Examples include climate-related disclosures, transition plans, ESG reporting requirements, and other sustainability-related transparency initiatives. The underlying assumption is that greater transparency allows investors and stakeholders to make better-informed decisions, thereby creating market incentives for improved corporate behavior.

Despite the growing popularity of such policies, an important question remains unresolved. Why do some disclosures appear to influence investor perceptions and corporate value while others generate little response? Transparency alone may not be sufficient. Investors are unlikely to react merely because information is released; they are more likely to react when they have reason to believe that disclosed commitments will be implemented. In this sense, the effectiveness of disclosure-based policy may depend not only on information availability but also on credibility.

We approach this question by distinguishing two channels through which a voluntary disclosure may convey information. The first is the information channel. What travels through it is the fact that a firm chose to disclose at all. When participation is limited, the decision to disclose separates firms from their non-disclosing peers, and the announcement may be informative regardless of what the document contains. The second is the content channel. What travels through it is the substance of what was promised. Plans committing the firm to numerical targets or to actions such as stock cancellation impose costs that narrative statements of intent do not. A policy

that broadens participation weakens the information channel but leaves the content channel intact. As participation rises, then, the market should price the substance of what is disclosed rather than the act of disclosing itself.

Testing this requires a setting in which the disclosure regime is held constant while participation changes for reasons exogenous to any individual firm. The Korea Corporate Value-Up Program provides one. Introduced in February 2024, the program encourages listed firms to voluntarily disclose value-enhancement plans covering capital allocation, shareholder return policies, and long-term performance targets, with the Korea Exchange issuing a formal guideline in May 2024. In December 2025, an amendment to the Restriction of Special Taxation Act introduced separate taxation of dividend income for high-dividend firms, effective January 1, 2026. An enforcement decree then designated the Value-Up disclosure as the means of satisfying the associated disclosure requirement. Firms seeking the tax treatment were thereby obliged to file a plan while the program remained voluntary for others, and participation rose from 145 firms to 541 without any change in the disclosure instrument.

Our treatment of ESG requires comment. We do not use ESG ratings as a proxy for the quality of a firm's information environment, since such ratings blend disclosure practice, firm size, industry composition, and rating-agency methodology. We adopt a narrower reading, taking the rating as an indicator of how closely a firm is already observed and, correspondingly, of how costly it would be to renege on a public commitment. Under this reading, prior visibility may reduce the news content of the act of disclosing while raising the credibility of what is promised, so that the same variable moderates the two channels in opposite directions.

The content variables are collected from the full text of all 1,086 Value-Up filings registered during the sample period. For each disclosure we measure the number of guideline indicator areas containing a quantified target and whether the plan commits to stock cancellation in definite terms. We combine these into a specificity index and estimate market reactions using an event-study design in which each disclosing firm is matched to three non-disclosing firms by propensity score, with exact matching on the month preceding the event. All inference clusters on the event date, and because filings cluster on a small number of days, we report conventional test statistics alongside statistics adjusted for the intraclass correlation and a calendar-time portfolio test.

Three findings emerge. First, a disclosure effect is evident only in Regime I. Matched-pair cumulative average abnormal returns (CAAR) over the three-day window are 1.008 percentage points in Regime I and 0.005 percentage points in Regime II, and the corresponding cumulative abnormal return (CAR) regression estimates are 1.242 and 0.037. The abnormal return observed among disclosing firms in Regime II is not evident once matched non-disclosing firms are used as the benchmark. Second, within Regime I the effect is smaller for firms with a stronger ESG rating, with a one standard deviation increase associated with a reduction of 1.338 percentage points; the same interaction is not evident in Regime II. Third, within Regime II the specificity index shows no significant unconditional association with returns but is associated with higher returns among firms with a stronger ESG rating, with an interaction coefficient of 1.760. A placebo procedure shifting matched-pair event dates five hundred times yields empirical p-values of 0.012, 0.042, and 0.018 for the three coefficients under the market model, and 0.004, 0.068, and 0.028 under the market-adjusted model.

The study contributes to the literature in three ways. First, it separates the market response to voluntary disclosure into an information channel and a content channel, and shows that a policy incentive can shift the response from one channel to the other. Second, it offers quasi-experimental evidence on how measures intended to broaden participation affect the informativeness of the disclosures they elicit. Third, it provides new evidence from a closely watched recent disclosure-based policy in Asia, documenting an asymmetric moderating role for ESG rating that is obscured when the ESG rating is treated as a single performance measure. More broadly, the results suggest that the informativeness of a disclosure regime may depend on its institutional environment rather than on the disclosure form alone. They also suggest that the success of disclosure-based regulation depends not only on information provision but on whether investors have reason to believe what they are told.

2 Literature review and hypothesis development

2.1 Disclosure-based policy and corporate responses

Disclosure has become one of the most widely used policy instruments in contemporary sustainability and corporate governance regulation. Rather than mandating specific corporate actions, regulators increasingly rely on transparency-based mechanisms that encourage firms to disclose information regarding their strategies, risks, commitments, and future plans. Recent examples include the Task Force on Climate-related Financial Disclosures (TCFD), the International Sustainability Standards Board (ISSB), and the European Union's Corporate Sustainability Reporting Directive (CSRD). All three rely heavily on disclosure-based mechanisms to influence corporate behavior (TCFD, 2017; ISSB, 2023; European Commission, 2022).

The underlying rationale is that improved transparency enables investors and stakeholders to monitor firms more effectively, thereby creating market-based incentives for better corporate behavior (Healy and Palepu, 2001; Christensen et al., 2021). In environmental and sustainability policy, disclosure is often viewed as a flexible alternative to command-and-control regulation. It allows firms to determine how best to respond while still being held accountable by capital markets and stakeholders (Eccles et al., 2014; Ioannou and Serafeim, 2015).

Prior studies nonetheless provide mixed evidence regarding effectiveness. While some find that disclosure improves market efficiency and reduces information asymmetry (Diamond and Verrecchia, 1991), others argue that disclosure may have limited effects when investors perceive it as symbolic rather than substantive (Michelon et al., 2015). A growing literature therefore emphasizes credibility in determining whether disclosure-based policies achieve their intended objectives. Investors must evaluate not only the content of disclosures but also the likelihood that firms will implement their stated commitments (Beyer et al., 2010; Leuz and Wysocki, 2016).

This literature typically takes the disclosure regime as fixed and asks how firms differ within it. A less examined question is what happens to the informativeness of the same regime when the population of participants changes, and settings in which participation shifts discontinuously while the instrument is held constant are uncommon. The Korea Corporate Value-Up Program is useful for both questions. Introduced in 2024, it encourages listed firms to voluntarily disclose value-enhancement plans without binding implementation requirements, so investors face uncertainty as to whether disclosed plans represent genuine commitments or symbolic communication. If investors perceive Value-Up disclosures as credible signals of future corporate actions, such disclosures should generate positive market reactions.

Hypothesis 1 (Disclosure Effect). *Policy-driven disclosure is positively associated with firm value, as reflected in abnormal stock returns.*

2.2 The information channel and the content channel

In a voluntary regime, an investor may learn from a filing along two dimensions that are conceptually separable. The first is the firm's decision to file. Signaling and self-selection arguments imply that when disclosure is costly and participation is partial, the decision itself is informative, since firms expecting favorable evaluation are more willing to submit to it (Spence, 1973; Verrecchia, 1983; Dye, 1985). We refer to this as the information channel. Acquiring and processing information about a firm is costly, so monitoring effort is allocated selectively across firms (Grossman and Stiglitz, 1980). The strength of this channel should also be inversely related to participation. If nearly every firm discloses, observing that a particular firm has done so conveys little about that firm. The second dimension is what the filing promises. A plan that commits the firm to a numerical target, or to canceling repurchased shares on a specified scale and schedule, imposes a cost that a mere statement of intent does not carry. We refer to this as the content channel: its strength depends on what the filing itself commits to, rather than on how many other firms have disclosed.

The two channels are difficult to separate within a single regime, because they operate together there. A change that increases participation while holding the disclosure instrument constant offers one way to separate them. Such a change dilutes the information channel almost by definition, since a higher participation rate makes any single firm's decision to disclose less distinctive; it leaves the content channel unaffected, since what a given filing

commits to does not depend on how many other firms have filed. The two Korean regimes described in Section 3.1 provide exactly this kind of variation. Each of the hypotheses that follow is therefore tested in the regime where the relevant channel is expected to be active.

2.3 ESG rating as a measure of market visibility

A second stream of literature focuses on the role of information environments in shaping the value of disclosure, suggesting that the economic impact of new information depends on how much is already available to investors (Verrecchia, 1983; Dye, 1985). Firms with a strong ESG rating are generally subject to closer external observation through sustainability reports, ESG disclosures, stakeholder engagement, and non-financial reporting (Dhaliwal et al., 2011; Christensen et al., 2021), so additional policy-driven disclosure may offer relatively little incremental information. This follows from the notion of diminishing marginal informational value. When investors already possess substantial information about a firm's governance practices, sustainability initiatives, and strategic direction, the contribution of an additional disclosure is smaller.

We depart from prior work in how we interpret the ESG measure. Composite ESG ratings combine disclosure practice, firm size, industry composition, governance attributes, and proprietary weighting choices, which makes them difficult to defend as construct-valid measures of information-environment quality or underlying ESG performance (Friede et al., 2015). In our data, the correlation between the composite rating and the governance sub-score is 0.909, and the correlation between the composite rating and firm size is 0.634. These magnitudes require a deliberately narrow interpretation. We use the rating as an imperfect proxy for market visibility: inclusion in the rating process indicates that rating agencies and other information intermediaries repeatedly collect, standardize, and disseminate information about the firm. Visibility is therefore an observational condition, not a claim that the firm is better governed or that its information environment is intrinsically of higher quality. Because larger firms are more likely to be rated and closely followed, all main specifications control for log total assets, and the robustness analysis adds a size interaction. The remaining overlap means that the ESG coefficient cannot be interpreted as a pure visibility parameter; its role is to test whether the predicted pattern is strongest among firms already embedded in external monitoring networks.

Section 4.6 examines whether substituting the governance sub-score, analyst coverage, or foreign ownership alters the conclusions. The alternatives are informative because they overlap with visibility in different ways. Governance ratings reflect assessed monitoring structures as well as rating-agency scrutiny; analyst coverage directly records the production of firm-specific information; foreign ownership records who owns the shares but not necessarily whether those owners generate or disseminate monitoring information. The visibility interpretation therefore predicts closer correspondence for governance and analyst coverage than for foreign ownership. Applied to the information channel, the announcement should be less valuable when the market has already formed a view of the firm.

Hypothesis 2 (Information Substitution Effect). *In the voluntary regime, the positive effect of policy-driven disclosure on firm value is weaker for firms with a higher ESG rating.*

2.4 Disclosure content and credibility

A separate question concerns whether what is disclosed is believed. Because disclosure-based policies typically rely on voluntary participation and limited enforcement, investors must assess whether firms will translate announced intentions into behavior. Mechanisms that make deviation more observable and costly should increase the credibility of a commitment (Healy and Palepu, 2001; Beyer et al., 2010). Repeated observation by rating agencies and analysts creates a public record against which later conduct can be compared, whereas share ownership alone need not produce comparable information. A foreign investor may hold a position without issuing forecasts, ratings, or public assessments. This distinction is central to the content channel: the relevant mechanism is the continuing production of observable information and the reputational cost it creates, not ownership identity by itself.

This consideration should matter most once the information channel has been diluted. When participation is close to universal, filing no longer sets a firm apart from its peers, so the only thing left for the market to evaluate is the substance of the plan. Because that substance is a promise, its value to an investor should depend on how likely the firm is to keep it. The prediction is therefore not that content is priced across the board, but that it is priced conditionally — more heavily for firms whose commitments investors already have reason to trust.

Hypothesis 3 (Conditional Credibility Effect). *In the incentive-mandated regime, the market response to the specificity index of a disclosed plan is stronger for firms with a higher ESG rating.*

Hypotheses 2 and 3 predict opposite signs for the same moderating variable. This follows from the argument rather than contradicting it, because the two hypotheses concern different objects. H2 concerns the disclosure decision, whose news content prior visibility should reduce. H3 concerns the disclosed commitment, whose credibility prior visibility should raise.

2.5 Conceptual framework

Figure 1 summarizes the argument. The horizontal dimension is the institutional regime, and it determines which of the two channels is expected to operate. The vertical dimension is the ESG rating, which moderates whichever channel is active. The framework implies four cells with distinct predictions. In the voluntary regime, the announcement effect should be larger for firms with a weaker ESG rating and attenuated for firms with a stronger ESG rating. In the incentive-mandated regime, no announcement effect is expected in either cell. There the content effect should be concentrated among firms with a stronger ESG rating.

3 Data and methodology

3.1 Institutional background and sample construction

The Korea Corporate Value-Up Program and the Disclosure Guideline

This study uses the Korea Corporate Value-Up Program as an institutional setting in which disclosure incentives change sharply over time. Introduced in February 2024, the program encourages listed firms to voluntarily disclose value-enhancement plans, including capital allocation strategies, shareholder return policies, and performance targets. The Korea Exchange finalized a guideline setting out how such a plan should be organized on 24 May 2024, effective from 27 May (Korea Exchange, 2024). Filings are submitted through the exchange's electronic disclosure system as voluntary disclosures. Participation is not compulsory and no penalty attaches to declining to file. The guideline states that firms retain discretion over both whether to participate and what to include. The subsequent tax amendment provides policy-timed variation, but the empirical design is not a randomized experiment; the identifying assumptions and concurrent institutional changes are set out below.

The guideline organizes a plan into six chapters: company overview, diagnosis of current condition, target setting, plan formulation, implementation assessment, and communication. Two of these constitute the plan proper, since target setting states what the firm intends to achieve and plan formulation states how it intends to get there. The remaining four situate the targets against the firm's current position, report progress against previously stated goals, and describe shareholder engagement, and thus make the plan checkable by an outside reader. This is the guideline's own division, and we adopt it in Section 3.4. Within the target-setting chapter, the guideline groups the candidate performance indicators into five areas. Four of these are financial: market valuation, capital efficiency, shareholder return, and growth. The fifth is non-financial, covering governance, dividend procedure, and environmental and social indicators.

The 2026 Tax Amendment and the Two Regimes

On December 23, 2025 the National Assembly amended the Restriction of Special Taxation Act with effect

from January 1, 2026. Article 104-27 provides that dividend income received from qualifying high-dividend firms is taxed separately at preferential rates rather than aggregated with the shareholder's other income. A firm qualifies if its dividends have not fallen relative to the fiscal year containing December 31, 2024 and, in addition, either its payout ratio reaches forty percent, or its payout ratio reaches twenty-five percent and its total dividends increased by at least ten percent relative to the fiscal year two years prior. Paragraph four of the same article obliges a qualifying firm to disclose that it satisfies these conditions. The deadline is the day after the annual general meeting resolves the dividend. An enforcement decree amendment approved on February 24, 2026 designated the Value-Up plan disclosure as the vehicle for that obligation.

The arrangement is best described as a conditional mandate. The disclosure remained voluntary for firms with no interest in the tax treatment. For firms whose shareholders stood to benefit, it became a precondition for the benefit. The regulatory instrument was unchanged, since the same guideline, filing system, and disclosure category continued to apply. Two implications follow for the empirical design. Whether a firm disclosed in 2026 is likely determined more by its dividend policy and its shareholders' tax position than by its own valuation judgment. Because the deadline is tied to the annual general meeting, the timing of filings also follows the corporate calendar.

We accordingly divide the sample at December 31, 2025, the day before the amended statute took effect, and refer to the two periods as Regime I, the voluntary regime, and Regime II, the incentive-mandated regime. The boundary is fixed by law rather than estimated from returns. Table 1 reports the composition of the two regimes. Regime I comprises 145 firms disclosing on 87 event dates between May 31, 2024 and December 30, 2025. Regime II comprises 541 firms disclosing on only 67 event dates between January 7 and August 7, 2026, and 352 of those filings occur in March, when most annual general meetings are held. This concentration is not incidental: the statutory deadline follows the shareholder meeting that resolves the dividend. Regime II therefore combines a change in disclosure incentives with a calendar-driven compression of event dates. Common market movements during the March meeting season can mechanically resemble a disclosure response when hundreds of firm observations share a small number of dates. We address this dependence with CR2 event-date clustered standard errors, calendar-time portfolio tests, matched controls assigned the same event date, and March-exclusion analyses, but these procedures cannot turn the timing rule into random assignment. The Regime II estimates should consequently be read as evidence from the first incentive-linked filing season rather than as an unrestricted causal effect of the tax amendment.

Because 2026 was the first year in which high-dividend firms would file, the exchange permitted an abbreviated format (Korea Exchange, 2026). A firm could record the core plan directly in the disclosure form without attaching a full plan document, thereby omitting the four chapters that make the plan externally checkable. Use of this format rose from 4.1 to 80.0 percent. Panel B of Table 2 shows that completion of target setting and plan formulation increased while completion of the four verification chapters fell sharply. The incentive change and the format change therefore occur together in Regime II. This prevents a clean attribution of the altered market response solely to broader participation: reduced verifiability may also weaken the disclosure signal or make credibility more dependent on prior monitoring. We report estimates that exclude abbreviated filings, while recognizing that the resulting Regime II sample is too small to separate these mechanisms precisely.

Sample Construction

We define the population as the 2,551 securities with daily return data over our 800-trading-day sample period. Removing the two market indices leaves 2,549 listed securities. Financial firms are excluded because they follow a different accounting framework: leverage, return on assets, and price-to-book are computed under industry-specific accounting rules that are not comparable to those of non-financial firms, and the pool of non-disclosing financial firms available as controls is in any case small.

We further require a December fiscal year end and positive book equity, leaving 2,451 firms, and after removing 42 disclosing firms lacking an industry classification the disclosing sample comprises 686 firms in eleven industry groups. The control pool consists of firms that never filed a Value-Up disclosure and were neither suspended from trading nor delisted, yielding 41,130 firm-period candidates from 1,573 distinct firms. Stock price

and financial data are obtained from DataGuide, disclosure records from the Korea Exchange's KIND system, and ESG ratings from the Korea Institute of Corporate Governance and Sustainability (KCGS).

3.2 Event-study methodology

To evaluate the capital market reaction to policy-driven disclosure, this study employs a standard event study methodology, which assumes that new information is incorporated into prices without systematic delay (Fama, 1970). The event date is the firm-level announcement date of the Value-Up disclosure. Where the filing was submitted after the market close or on a non-trading day, the event date is moved forward to the next trading session. This adjustment matters because 51.6 percent of filings in our sample are submitted after 15:30. We treat only original plan filings as events, excluding advance notices, implementation reports, corrections, and resubmissions, and retain the earliest qualifying filing for each firm.

Abnormal returns are estimated using both the market model and the market-adjusted model. Under the market model, the return-generating process is specified as

$$R_{i,t} = \alpha_i + \beta_i R_{m,t} + \varepsilon_{i,t}$$

where $R_{i,t}$ denotes the return of firm i on day t and $R_{m,t}$ the return on the composite index of the market on which the firm is listed. Parameters are estimated over a window running from 250 to 21 trading days before the event, requiring at least 60 valid observations. Abnormal returns are the difference between actual and expected returns; under the market-adjusted model they are the raw return less the index return.

Two statistics are used throughout and are kept distinct. The cumulative abnormal return (CAR) is the abnormal return of a single firm cumulated over an event window; it is the dependent variable in every regression. The cumulative average abnormal return (CAAR) is the cross-sectional mean of CAR over a group of observations; it is the statistic reported in the event-study tables. Where the two are compared, both are stated in percentage points.

CAR is computed over four event windows of three, seven, eleven, and twenty-one days centered on the event date, with the main analysis focusing on the three-day window and longer windows used for robustness. Observations whose event window contains a trading suspension are dropped. CAAR is computed on unwinsorized data; regressions use CAR winsorized at the first and ninety-ninth percentiles.

To account for cross-sectional dependence among firms disclosing on the same date, standard errors are clustered at the event-date level. This dependence is pronounced in Regime II, where many firms disclose on the same day. We therefore report three test statistics for CAAR. The first is the conventional firm-level statistic. The second divides that statistic by the square root of the design effect, which follows from the intraclass correlation of abnormal returns within event dates and the average cluster size. The third is a calendar-time portfolio statistic that treats the mean abnormal return on each event date as a single observation. For the three-day window, estimated intraclass correlations in Regime II are 0.213 under the market model and 0.314 under the market-adjusted model, implying design effects of 2.51 and 3.22. An event study on disclosing firms alone cannot readily distinguish a disclosure effect from a market-wide movement on days when filings cluster. We therefore report CAAR for disclosing firms and matched-pair differences in separate panels.

3.3 Baseline Regression Model

To examine the determinants of market reactions, we estimate cross-sectional regressions of CAR on disclosure indicators, content measures, and characteristics, structured to correspond to Hypotheses 1 through 3. Control variables in every specification comprise firm size measured as log total assets, leverage, price-to-book, KOSPI/KOSDAQ, return on assets, and foreign ownership, together with industry fixed effects, and all specifications apply the matching weights described in Section 3.5.

Baseline Model for H1: Disclosure Effect

To test whether policy-driven disclosure generates positive market reactions, the following model is estimated separately by regime on the matched sample:

$$CAR_i = \beta_0 + \beta_1 \, Treatment_i + \beta_2 \, Controls_i + \varepsilon_i$$

where CAR_i denotes the cumulative abnormal return for firm i and $Treatment_i$ equals one if the firm discloses a Value-Up plan. A significantly positive β1 supports the hypothesis that policy-driven disclosure conveys value-relevant information. Estimating the model separately by regime allows the coefficient to differ across the two institutional environments.

Model for H2: Information Substitution Effect

To examine whether the ESG rating moderates the market reaction to disclosure, we extend the baseline model:

$$CAR_i = \beta_0 + \beta_1 \, Treatment_i + \beta_2 \, ESG_i + \beta_3 \, (Treatment_i \times ESG_i) + \beta_4 \, Controls_i + \varepsilon_i$$

where ESG_i is the standardized ESG rating. A significantly negative β3 indicates an information substitution effect. The hypothesis is located in Regime I, where the information channel is expected to be intact; the same specification is estimated on Regime II and on the pooled sample as a comparison.

Model for H3: Conditional Credibility Effect

To test whether firms with a higher ESG rating see a larger price reaction to what their plan promises, we estimate:

$$CAR_i = \beta_0 + \beta_1 \, Specificity_i + \beta_2 \, ESG_i + \beta_3 \, (Specificity_i \times ESG_i) + \beta_4 \, Controls_i + \varepsilon_i$$

where $Specificity_i$ is the specificity index. A significantly positive β3 indicates that, for a given level of the specificity index, commitments are priced more heavily when made by firms already subject to close market scrutiny. This specification is estimated on disclosing firms only, since content is undefined for firms that did not disclose. The hypothesis is located in Regime II, and the same specification is estimated on Regime I as a comparison.

Standard Error Estimation

This study employs cluster-robust standard errors to account for cross-sectional dependence among firms disclosing on the same event date. All reported specifications use the CR2 cluster-robust estimator with a Satterthwaite degrees-of-freedom correction, which remains valid when the number of clusters is small (Bell and McCaffrey, 2002).

3.4 Measurement of Key Variables

ESG Rating and Alternative Moderating Variables

The ESG rating is measured from the composite ESG rating published by KCGS. It is converted to an ordinal score running from one for the lowest to seven for the highest, and is then standardized; the standardized variable is denoted ESG_std. In the matched sample the underlying ordinal score has a mean of 2.99 and a standard deviation of 1.62. Three alternative measures are used in robustness tests. The first is the KCGS governance sub-score, which incorporates board independence, shareholder protection, ownership structure, and monitoring mechanisms. The second is the natural logarithm of one plus the number of analyst opinions, with zero assigned to firms without coverage. The third is foreign ownership as a percentage of shares outstanding. All four are

standardized on the full matched sample rather than within subsamples, so that coefficients remain comparable across tables and across moderators. Firm size enters interaction specifications in mean-centered form.

Three terms recur throughout and are used in a fixed sense. *ESG rating* denotes the standardized composite rating just described and is the moderating variable in every reported specification. *Governance sub-score* denotes the governance component of that same composite rating and is used only as an alternative moderator in Section 4.6. *Specificity index* denotes the content measure defined in subsection (3), and is distinct from *disclosure content*, which we use as an informal cover term for everything a filing contains.

Timing of Accounting and Rating Data

Two features of the reporting calendar require separate treatment. Because annual reports are not filed until the end of March, financial data for the immediately preceding fiscal year are not yet public for events occurring in January through March. For these 409 events we use the fiscal year two years prior. Because the rating agency publishes ESG ratings each October, the rating in force at any event date is the one assigned in the preceding calendar year. As a result, the financial-statement and ESG-rating covariates for a given event are drawn from different reference years.

Disclosure Content Variables

Content variables are collected from the full text of every Value-Up filing registered during the sample period, 1,086 in total, of which 1,063 match a security in the population.[1] Two variables enter the hypotheses. Quantified target areas records how many of the guideline's five indicator areas contain an explicit numerical target. The variable can in principle take any value from zero to five; the highest value observed in our data is four, because no filing states a numerical target in all five areas. Stock cancellation equals one only where the filing states the commitment in definite terms; a plan describing cancellation as under review is set to zero. Of the 233 filings mentioning cancellation, 213 state it definitively. We standardize each variable and take their mean to form the specificity index. A further variable indicates whether the plan makes any quantifiable commitment at all, coded one if either quantified target areas or stock cancellation is positive. We use this binary indicator only as a robustness check, to assess whether our results depend on measuring the specificity index as a continuous measure rather than as a simple yes/no distinction.

Two variables describe the structure of the document rather than the substance of the promise and are reported in Panel B of Table 2 without entering any regression. Form compliance counts how many of the two chapters constituting the plan are completed, and substantive effort counts how many of the four verification chapters are completed. A filing is classified as abbreviated where form compliance is positive but substantive effort is zero,

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[1)] Disclosure content variables are collected from the full text of every Value-Up plan filing registered with the Korea Exchange during the sample period, May 2024 through August 2026. Quantified target areas is based on the five indicator areas set out in the Korea Exchange guideline. Four are financial. They are market valuation (price-to-book, price-to-earnings), capital efficiency (return on equity, return on invested capital, cost of equity, weighted average cost of capital), shareholder return (dividend amount, payout ratio, dividend yield, treasury shares, total shareholder return, shareholder return ratio) and growth (revenue, operating income, asset and research expenditure growth). The fifth is non-financial and covers governance, dividend procedure, environmental and social indicators. The variable records how many of these areas (0–5) contain an explicit numerical target. The residual financial category the guideline lists as other, comprising asset portfolio composition, free cash flow and leverage, is not counted as a separate area. Counting the presence of items to form an index follows the checklist approach to measuring disclosure level (Botosan, 1997), and treating numerical targets as more informative than narrative statements follows evidence on how the form of quantitative information affects users' judgments (Hutton, Miller, & Skinner, 2003). Stock cancellation is recorded only where a definite expression specifying scale or timing appears, such as a statement that cancellation has been resolved, that the board has approved it, or that repurchased shares will be cancelled; expressions indicating that cancellation is under review, planned or undetermined are treated as absent. Plan completeness is measured by whether each of the six chapters set out in the guideline is present. Of these, target setting and plan formulation constitute the plan proper and are combined into form compliance (0–2), while company overview, diagnosis of current condition, implementation assessment and communication increase the verifiability of the promise and are combined into substantive effort (0–4).

which is a structural signature of the abbreviated format rather than its legal definition.

We exclude substantive effort from the specificity index. Control firms are assigned a missing value rather than zero for all content variables, since a firm that did not disclose has no plan whose content could be measured.

3.5 Identification Strategy

A key empirical challenge is potential selection bias, as firms voluntarily choose whether and when to disclose. To mitigate this concern we adopt several strategies.

First, we employ propensity score matching (Rosenbaum and Rubin, 1983). Each disclosing firm is matched to three non-disclosing firms without replacement by nearest-neighbor matching on the linear predictor of a logistic propensity model, using a caliper of 0.2 standard deviations of the linear predictor (Rosenbaum and Rubin, 1985). Matching covariates are log assets, leverage, log price-to-book, KOSPI/KOSDAQ, return on assets, and foreign ownership.

Second, matching is exact on the month preceding the event, so treated and control firms are observed under the same market conditions in that month. Because Regime I and Regime II fall in different months, this also separates the matching problem by regime automatically. Matched control firms are assigned the event date of the disclosing firm to which they are matched. The procedure matches 143 of 145 disclosing firms in Regime I and 535 of 541 in Regime II, yielding 678 treated and 1,872 control observations — an average of 2.76 controls per treated firm.

Third, we include a comprehensive set of control variables and industry fixed effects. Panel A of Table 3 reports covariate balance: the maximum absolute standardized mean difference falls from 1.023 before matching to 0.150 after. The residual imbalance is on price-to-book and exceeds the conventional 0.1 threshold. Section 4.6 therefore tightens the price-to-book caliper directly, reducing the standardized difference from 0.150 to 0.002.

These adjustments address observable firm selection but not the common-date shock created by clustered filing. Our primary inference therefore treats the event date, rather than the firm, as the independent sampling unit. The regressions use CR2 event-date clustered standard errors with a small-sample degrees-of-freedom correction, and the calendar-time portfolio test collapses all firms filing on a date into one portfolio observation. The matched comparison further subtracts the return of non-disclosing firms assigned the same date. Because 352 Regime II events occur in March, we also report March-only and March-excluded estimates. Agreement across these designs is more informative than the conventional firm-level event-study statistic, particularly in Regime II.

Fourth, because many control candidates have near-identical propensity scores, which of them is selected is partly arbitrary. To gauge the resulting sampling variation we re-draw the matched sample twenty times with the matching order randomized, and re-estimate the three principal coefficients each time. Panel A of Table 8 reports the results.

3.6 Additional Tests and Robustness Checks

To assess the validity of our findings, Table 7 varies the dependent variable, matching design, sample, controls, moderator, and specificity measure. The sample checks are designed around the two institutional confounds identified above: one excludes March 2026 and another excludes abbreviated filings. Table 8 then evaluates sensitivity to the particular matched draw, deletes each event date in turn, and shifts entire matched pairs to placebo dates in 500 repetitions while preserving the observed clustering structure. Together with CR2 inference and the calendar-time portfolio test, these exercises ask whether the results survive dependence across firms, alternative event composition, and the small cells underlying the interaction estimates.

4 Results and Discussion

4.1 Descriptive statistics and matching quality

Table 1 reports the composition of the two regimes and Table 2 the content and format of the filings. Filings rose from 145 to 541 while the specificity index fell from 0.808 to −0.217, a difference of 1.024, significant at the 1 percent level. Both components move together. Quantified target areas fall from 1.497 to 0.172, and the share of plans committing definitively to stock cancellation falls from 0.303 to 0.115, both significant at the 1 percent level. The standardized ESG rating among filers falls from 0.760 to −0.093, significant at the 1 percent level, indicating that the firms drawn in by the tax incentive were on average less closely observed.

Panel A of Table 3 reports descriptive statistics for the matched sample alongside covariate balance. Among disclosing firms the mean number of quantified target areas is 0.45 out of five and only 15.5 percent of filings commit definitively to stock cancellation, so the specificity index is concentrated in a minority of filings. This matches the prevalence of the abbreviated format, which fills in the plan's core sections without substantiating them and accounts for 64.2 percent of filings in the pooled matched sample and 80.4 percent in Regime II.

Panel B of Table 3 reports correlations among disclosing firms. The correlation between the specificity index and firm size is 0.361, and between stock cancellation and firm size 0.192, suggesting that the content measure is not simply a proxy for size.

The right-hand columns of the same panel report the matching diagnostics. They show that the maximum absolute standardized mean difference falling from 1.023 to 0.150, with the residual imbalance concentrated in raw price-to-book. Panel A of Table 8 reports the twenty re-drawn matched samples. Under the market model the Regime I disclosure effect lies between 1.194 and 1.386 across draws, the Regime I interaction between −1.454 and −1.201, and the Regime II interaction between 1.728 and 1.902. The corresponding ranges under the market-adjusted model are 1.374 to 1.602, −1.330 to −1.076, and 1.779 to 1.900. No draw reverses a sign under either model.

4.2 Market Reaction to Value-Up Disclosures (H1)

Table 4 reports CAAR and Table 5 the corresponding CAR regressions. In Regime I the three-day market-model CAAR among disclosing firms is 1.164 percentage points. The conventional statistic, which treats each of the 145 disclosing firms as an independent observation, is t = 2.76 (significant at the 1 percent level). Because these firms disclose on only 87 distinct dates, returns are correlated within a date (intraclass correlation = 0.235); adjusting the standard error for this clustering lowers the statistic to t = 2.57, still significant at the 5 percent level. Collapsing each event date into a single average return — the calendar-time portfolio test — yields t = 1.83, significant at the 10 percent level. The matched-pair difference in CAAR in Panel B of Table 4 is 1.008 percentage points (t = 2.05) and the CAR regression estimate in Table 5 is 1.242. The three inference approaches agree in sign, though the calendar-time test is weaker than the other two, and H1 is supported in the voluntary regime.

The Regime II estimates require a different evidentiary threshold. Because 352 of 541 filings occur in March 2026, firm-level significance can be overstated even when standard event-study assumptions appear to hold. We therefore interpret Regime II only after comparing conventional, CR2-adjusted, calendar-time, and matched-control results. A return that appears significant only when firms sharing the same date are treated as independent is not taken as evidence of a disclosure effect.

Regime II presents a different picture. Under the market-adjusted model, disclosing firms show a three-day CAAR of 1.421 percentage points with a conventional t statistic of 5.07. But the intraclass correlation here is far higher, 0.314, because 541 firms disclose on only 67 dates and 352 of them file in March 2026 alone; adjusting for this clustering lowers the statistic to t = 2.83, and the calendar-time portfolio test — which treats each disclosure date as one observation — falls to t = 1.08, no longer significant. The matched-pair difference in CAAR over the same window is 0.464 percentage points (t = 1.37, not significant), and the corresponding market-adjusted CAR regression estimate is 0.421, also not significant. The disclosing and control columns of that panel show why: disclosing firms earned 1.451 percentage points while their matched non-disclosing firms earned 0.986.

Under the market model the gap closes almost entirely, with a matched-pair difference of 0.005 percentage points and a CAR regression estimate of 0.037. Taken together, these estimates suggest that the abnormal return observed among disclosing firms in this regime reflects a market-wide movement concentrated on a few heavily-subscribed filing dates rather than an increment attributable to disclosure itself.

The divergence between the two designs is informative in itself. An event study conducted on disclosing firms alone and a matched comparison against non-disclosing firms agree in Regime I but not in Regime II. This is what would be expected given the cross-sectional dependence created by the concentration of filings. We therefore base our conclusions on the matched comparison. A related caution applies to wider windows. In Regime II the twenty-one-day market-model CAAR reaches 3.952 percentage points, but once the 537 firm-level observations are collapsed into the event dates behind them the calendar-time portfolio statistic is −0.01, so this return is not statistically distinguishable from no effect at all. With only 67 event dates behind 541 firms, a window this long is dominated by which days the market happened to move rather than by the disclosure itself, and our conclusions rest on the three-day window.

The pooled column of Table 5 shows what is lost by not separating the two regimes. Pooling them, the matched-pair difference in CAAR is 0.216 percentage points under the market model and the CAR regression estimate 0.241, neither significant, while the corresponding market-adjusted estimates are 0.625 and 0.609 and significant at the five percent level. A pooled reading would therefore record a weak and model-dependent average effect, and would not reveal that the effect is confined to one regime.

H1 is thus supported in the voluntary regime and not in the incentive-mandated regime. This is what the information channel described in Section 2.2 implies. The act of disclosing appears to have ceased carrying information once the tax incentive brought most eligible firms into the disclosing population.

4.3 ESG Rating and the Information Substitution Effect (H2)

The H2 block of Table 5 tests the information substitution effect. In Regime I the interaction between disclosure and the ESG rating is −1.338 under the market model and −1.229 under the market-adjusted model, both significant at the five percent level, with a disclosure main effect of 2.001 at mean ESG rating. The disclosure effect is therefore about 2.0 percentage points for a firm at average ESG rating. Each one-standard-deviation increase in the ESG rating reduces this effect by roughly 1.2 to 1.3 percentage points, so the effect shrinks to essentially zero for firms whose ESG rating is about one and a half standard deviations above the mean. H2 is supported: firms the market already watches closely show little to no price reaction to a Value-Up announcement, consistent with the disclosure carrying less new information for them.

The corresponding interaction in Regime II is 0.066 and is not significant, nor is the pooled estimate. If the ESG rating attenuated the announcement effect through a channel unrelated to selection, some attenuation would be expected to persist after the tax amendment.

4.4 The Specificity Index and the Credibility Effect (H3)

The H3 block of Table 5 tests the conditional credibility effect on Regime II disclosing firms. The interaction between the specificity index and the ESG rating is 1.760 under the market model and 1.780 under the market-adjusted model, both significant at the five percent level. The main effect of the specificity index is 1.689 and is not significant, and the main effect of the ESG rating is 0.406 and is not significant. H3 is supported.

The pattern of significance points to a narrower claim than the general proposition that content matters. In this regime the specificity index shows no significant unconditional association with returns, and is associated with higher returns only among firms the market already observes closely. Columns 1 and 2 provide the corresponding comparisons. Pooling the two regimes, the interaction is weak and not significant under either the market model or the market-adjusted model. Estimated on Regime I disclosing firms alone it is also weak and not significant. Both patterns are what would be expected if the effect is confined to the regime in which the information channel has been diluted. In that column the main effect of the ESG rating is significant and negative, the counterpart of

the substitution result in the H2 block estimated within disclosing firms.

Because the interaction is identified from the contrast between filings with and without content, the relevant cell sizes are central to inference. Table 5 shows that only 21 Regime II firms combine a high ESG rating with an offered commitment, compared with 75 high-rated firms without a commitment, 31 low-rated firms with a commitment, and 113 low-rated firms without one. The H3 estimate therefore depends materially on a 21-firm cell. The interaction is statistically identifiable, but its precision and external validity are limited: it should not be generalized to the much larger population of unrated Regime II filers or interpreted as an average effect for all high-visibility firms. Section 4.6 consequently treats H3 as conditional evidence and emphasizes sign stability, event-date deletion, placebo inference, and alternative sample definitions.

4.5 Disclosure content and subsequent implementation

Panel A of Table 6 examines whether disclosure content is associated with returns without conditioning on the ESG rating. Pooling both regimes, the specificity index attracts a coefficient of 0.930, significant at the ten percent level under the market model. The second block of the panel enters the two components separately. Quantified target areas attract 0.731 and are significant at the five percent level while stock cancellation attracts 0.840 and is not, which suggests that the association is attributable mainly to numerical targets. Splitting by regime places that association in Regime II, where quantified target areas attract 2.443 and are significant at the ten percent level while the cancellation pledge remains insignificant. The decomposition identifies the likely source rather than settling it, since the components are estimated on 240 Regime II observations.

Panel B of the same table examines whether the initial disclosures were followed by implementation reports. Among Regime I disclosing firms with an ESG rating, the proportion that subsequently filed an implementation report was 57.8 percent for high-rating firms offering a commitment and 54.5 percent for those offering none. The corresponding figures for low-rating firms were 47.4 and 30.0 percent. The difference associated with offering a commitment is therefore confined to firms with a low ESG rating. In a logistic regression entering all three jointly, neither the initial CAR (0.010), the ESG rating (0.291) nor the specificity index (0.156) predicts subsequent implementation reporting.

Two limitations weaken this test. First, we can only observe whether a firm filed a follow-up report, not whether it actually carried out its plan; a firm that cancelled shares without filing a separate report would be wrongly coded as non-compliant. Second, the regression rests on the 123 Regime I firms carrying an ESG rating, among which 64 filed a report and 59 did not, against ten parameters — 5.9 events per parameter, below the usual rule of thumb of ten — so the estimates are unreliable at this sample size. For these reasons, the test cannot tell us whether the market's reaction at announcement predicts what a firm does afterward.

4.6 Robustness Check

Table 7 checks whether the three main results survive six classes of changes, and Table 8 provides inference diagnostics targeted at the clustered dates and small H3 cell. The stability tests are reported before drawing broader policy conclusions because the baseline significance of H3, by itself, is not sufficient evidence when 21 firms carry substantial identifying weight.

First, on the dependent variable. The results are insensitive to the treatment of the three-day window itself, but the substitution result (H2) is specific to it: at seven days and eleven days the interaction is no longer present. This fits the idea that the substitution effect comes from the market's immediate reaction to new information, not from a slower reassessment over the following weeks.

Second, on the matching design. Matching firms one-to-one instead of one-to-three, matching more strictly within industry, tightening the match on valuation, and replacing matching altogether with entropy balancing all leave the three main findings essentially unchanged.

Third, the sample restrictions clarify what the baseline H3 estimate can and cannot establish. Excluding March

2026 increases the H3 coefficient from 1.760 to 2.495 under the market model and from 1.780 to 2.244 under the market-adjusted model, but the sample falls from 240 to 68 and the standard errors rise to 1.771 and 2.006. Restricting the sample to March yields smaller positive coefficients of 1.358 and 1.403 on 172 observations. The loss of significance outside March is therefore not evidence of a sign reversal; it reflects the removal of the season that supplies most rated observations and most variation in plan content. Economically, this indicates that the first incentive-linked filing season provides limited independent support outside the annual-meeting cycle. The baseline result captures an average across a calendar-compressed institutional episode, and a second filing year is needed to determine whether the credibility channel persists when event timing is more dispersed.

Fourth, the control checks address the possibility that the ESG interaction is a size or eligibility effect. Adding high-dividend and index-membership controls leaves all three main findings with the predicted signs. Allowing the disclosure effect to vary with firm size also leaves H1 and H2 negative or positive as predicted; for H3, the coefficient remains positive at 1.742 under the market model and 1.930 under the market-adjusted model, but is significant only in the latter at the ten percent level. Thus, observed size does not account for the sign of the credibility interaction, although the loss of precision cautions against claiming that the composite ESG rating isolates visibility independently of size. The high correlation between rating coverage and firm size makes multicollinearity a plausible contributor to the wider standard errors.

Fifth, alternative moderators provide a demanding test of the visibility interpretation. Replacing the composite ESG rating with the governance sub-score preserves H2 under both return models, albeit at the ten percent level; analyst coverage preserves the predicted negative sign and reaches the ten percent level only under the market model. For H3, the governance and analyst interactions remain positive (1.157 and 0.745 under the market model) but are not significant. These patterns are consistent with a common monitoring component but do not establish interchangeable measures of a single latent construct. Governance ratings combine monitoring structures with rating-agency assessment, while analyst coverage captures information production more directly. By contrast, foreign ownership produces neither the predicted H2 nor H3 pattern. This difference supports the theoretical distinction in Section 2: ownership identifies the holder of cash-flow rights, whereas visibility requires repeated observation and the production of information against which commitments can later be evaluated. Nonetheless, because the H3 result is strongest for the composite rating and is estimated from a small cell, we treat it as suggestive evidence of conditional credibility rather than proof of a general visibility mechanism.

Sixth, on the specificity index. Breaking the index into its two building blocks, quantified target areas and stock cancellation, each piece separately reproduces the credibility result (H3) under the market model, and neither one alone explains the result better than the combined index. The index itself remains the more precise measure. If we instead use a simple yes or no indicator of whether the firm made any commitment at all, the result disappears, suggesting that what matters is not merely whether a firm promised something, but how much it promised.

Table 8 provides the strongest defense against dependence on a particular matched draw or event date. Across twenty randomized matching draws, the H3 coefficient remains positive within narrow ranges: 1.728–1.902 under the market model and 1.779–1.900 under the market-adjusted model. Deleting each event date in turn never reverses the sign of H2 or H3; the H3 coefficient ranges from 1.172 to 1.953 under the market model and from 1.158 to 1.965 under the market-adjusted model. In 500 clustered placebo assignments, the empirical p-values for H3 are 0.018 and 0.028, while those for H1 are 0.012 and 0.004 and those for H2 are 0.042 and 0.068. These exercises show that the reported signs are not generated by one filing date or one matched-control draw. They strengthen internal robustness, but they do not enlarge the 21-firm identifying cell or resolve the institutional co-occurrence of the tax incentive, March filing calendar, and abbreviated format.

4.7 Discussion: The Two Channels and the Effectiveness of Disclosure-Based Policy

The estimates assembled above describe a regime whose informational character changed while its formal design did not. The number of firms filing a Value-Up plan rose from 145 in the voluntary period, Regime I, to 541 in the incentive-mandated period, Regime II, a factor of 3.7, even though the number of distinct filing dates fell from 87 to 67. Over the same boundary, the average specificity of what firms promised fell sharply: the specificity index dropped from 0.808 in Regime I to −0.217 in Regime II, the average number of quantified target areas fell from 1.497 to 0.172 out of five, and the share of firms committing definitively to stock cancellation fell

from 30.3 to 11.5 percent. The guideline, the filing system, and the disclosure category were unchanged throughout. What changed was who filed, why, and in what form.

The information channel operates through the disclosure decision itself. In Regime I, the matched-pair CAAR is 1.008 percentage points and the CAR regression estimate is a similar 1.242 percentage points. This reaction is not the same for every firm: it shrinks as the ESG rating rises. Concretely, for a firm at the average level of ESG rating, disclosing a plan is associated with a price gain of about 2.0 percentage points; for a firm whose ESG rating is about one and a half standard deviations above average, meaning a firm already closely watched by the market, that gain shrinks to essentially nothing. The announcement therefore appears to matter mainly for firms the market follows less closely, consistent with the idea that its informational value comes from separating disclosing firms from non-disclosing ones.

In Regime II, neither part of this pattern appears. The matched-pair CAAR is only 0.005 percentage points under the market model, essentially the same return for disclosing and non-disclosing firms, and the size of the effect no longer depends on the ESG rating at all. Because the tax provision made filing a precondition for a shareholder tax benefit, 506 of the 541 filers in Regime II were high-dividend firms, so the disclosing population came to resemble nearly the entire eligible population rather than a self-selected subset. The announcement effect and its dependence on the ESG rating disappear together, exactly what we would expect once disclosure stops being a distinguishing choice and becomes something almost everyone eligible does. If we pool the two regimes together instead of separating them, both effects become statistically indistinguishable from zero. In other words, splitting the sample into Regime I and Regime II is not a minor refinement; it is the only way either result becomes visible at all.

The March concentration qualifies this interpretation. Regime II does not provide 541 independent disclosure events; it provides 541 firm observations distributed across 67 dates, with almost two thirds of the filings concentrated in one annual-meeting month. The disappearance of H1 after matching and calendar-time aggregation is consistent with dilution of the information channel, but it is also consistent with common March returns overwhelming a small incremental disclosure response. The March-excluded H3 coefficient remains positive and larger than the baseline, yet is too imprecise to be decisive because only 68 rated observations remain. The economic conclusion is therefore comparative rather than fully causal: the voluntary regime contains a distinct firm-level announcement signal, whereas the first incentive-linked season does not reveal one once common-date movements are removed.

The content channel operates through what a plan promises, not through the decision to file. In Regime II, the specificity index has no significant effect on price on its own, but that effect depends heavily on the firm's ESG rating. For a firm with a high ESG rating, one standard deviation above the average, the implied price effect of a one-standard-deviation increase in the specificity index is a substantial 3.449 percentage points. For a firm with a low ESG rating, one standard deviation below the average, the implied effect is essentially zero. A given commitment is priced by the market only when made by a firm it already watches closely. The same pattern estimated in Regime I is small and not significant, consistent with this content effect being hard to detect there, since the announcement effect dominates in that regime instead.

The decline in content across the regime boundary is closely associated with the abbreviated format. Completion of target setting and plan formulation increased, but completion of the four verification sections—company overview, current-condition diagnosis, implementation assessment, and communication—fell sharply as abbreviated filings rose from 4.1 to 80.0 percent. This is not merely a descriptive side effect. If investors require contextual and follow-up information to evaluate a promise, the format change can itself weaken H1 and make the pricing of specificity more dependent on prior visibility. Table 7 excludes abbreviated filings to probe this possibility. H1 and H2 remain similar, but the Regime II H3 coefficient, although positive and larger at 2.430–2.524, is not significant because only 50 observations remain and standard errors approach two percentage points. The exercise therefore cannot distinguish an incentive effect from a verifiability effect with adequate power. Future filings under a common format, or a design exploiting variation in format eligibility, are needed to separate the two mechanisms.

Taken together, the evidence is consistent with two channels operating under different institutional conditions, but the Korean setting does not vary one institutional feature at a time. Regime II simultaneously expands

participation, compresses filings around the annual-meeting calendar, changes the composition of filers, and permits an abbreviated format. The ESG rating moderates the disclosure decision in Regime I and the disclosed commitment in Regime II in opposite directions, as the framework predicts. The stronger inference is therefore about the joint informational character of the two regimes, not the isolated structural effect of any single policy component. A disclosure regime cannot be evaluated by participation alone: usefulness depends on whether filing remains informative, whether the plan remains verifiable, and whether investors possess an observable record against which the promise can be judged.

4.8 Implications for disclosure policy and practice

First, participation alone may not be a good way to judge whether a voluntary disclosure regime is working. By that measure, the 2026 tax amendment was a clear success: the number of filers rose by a factor of 3.7, from 145 firms in Regime I to 541 in Regime II, and 506 of those 541 Regime II filers were high-dividend firms taking advantage of the new tax benefit. But over that same shift from Regime I to Regime II, the price reaction to disclosure disappeared, and the average specificity of what firms promised dropped sharply. A regulator who judges the program by how many firms participated would call it a success. A regulator who judges it by how informative the disclosures actually are would reach the opposite conclusion, from the very same episode. This tension seems built into any policy tool that depends on voluntary participation for its signaling value: the more successful the policy is at raising participation, the weaker that signal becomes.

Second, simplifying the filing format may trade off verifiability against coverage. Panel B of Table 2 shows that the abbreviated format, used by most Regime II filers, did not make plans less complete overall; it just changed what kind of completeness firms provided. The two sections that state the actual plan, its targets and how to reach them, were filled in more often than before. But the four sections that let an outside reader check the plan, background, diagnosis, follow-up, and investor communication, were filled in far less often. Related signs point the same way: fewer filings pointed to a published investor-relations document, and the disclosure text itself got shorter on average. In short, plans became more likely to state a target and less likely to back it up, a trade-off regulators should weigh explicitly if this simplified format is kept or expanded.

Third, the ESG rating helps different types of firms in opposite ways, depending on which regime they are in. In Regime I, a lesser-known firm gains more from simply announcing a plan than a well-known firm does, because the announcement itself is news for the lesser-known firm but not for the well-known one. In Regime II, the opposite holds for the plan's content: a well-known firm's specific commitment moves its stock price, while an identical commitment from a lesser-known firm barely moves the price at all, presumably because the market has no track record to judge whether that firm will actually follow through. Put simply, being under the radar helps a firm when it is only deciding whether to disclose, but hurts it once the market starts judging what it promised.

Fourth, who Regime II brought into the disclosing population matters for how to read its results. The KOSPI-listed share of filers fell from 73.1 percent in Regime I to 37.5 percent in Regime II, and fewer than half of the Regime II filers even carried an ESG rating. The incentive therefore disproportionately pulled in firms that analysts and rating agencies do not closely cover. If market credibility comes from being closely observed, then a policy that recruits unobserved firms will tend to produce disclosures the market has little basis for evaluating. Expanding who discloses and expanding who is watched by raters and analysts may need to happen together, not as substitutes for one another.

Fifth, we currently cannot observe whether firms actually follow through on their promises. A firm that cancels shares but never files a separate implementation report looks, in our data, identical to a firm that never acted at all. Section 4.5 can therefore only measure whether firms reported following up, not whether they actually did. Closing this gap would require a disclosure framework with a reporting requirement built in.

5 Conclusions and limitations

This study examines the effectiveness of policy-driven disclosure by distinguishing the informational value of

the decision to disclose from the credibility of what is disclosed. The Korea Corporate Value-Up Program provides the setting, together with a 2026 tax change that raised participation sharply without altering the disclosure rules themselves.

Three results follow. Announcing a plan raised stock prices only in the voluntary period, Regime I, and mainly for firms the market did not already follow closely; that price effect, and its dependence on the ESG rating, both disappeared once the tax incentive made disclosure close to universal in Regime II. What emerged instead in Regime II was a content effect: a plan's specificity index moved prices only when the disclosing firm already had a high ESG rating.

Taken together, these results show that the market impact of policy-driven disclosure is not explained by transparency alone. Disclosure mattered as a signal only while it remained a distinguishing choice; once nearly every eligible firm disclosed, what mattered instead was whether investors had reason to believe what a firm promised. The regime did not simply become less informative. It became informative in a different way, and only for firms the market already watched closely.

The accompanying decline in the specificity index coincides with the abbreviated filing format: completion of the plan's core sections rose while completion of its verification sections collapsed. This pattern is consistent with a format-induced loss of verifiability, but the format and incentive changes occur together, so their separate contributions cannot be identified from the current filing season.

These findings separate the informational value of a disclosure decision from the credibility of a disclosed commitment, and show that a policy incentive can shift the market's response from one to the other. They also suggest that ESG ratings are better read as a measure of how closely a firm is watched than as a proxy for the quality of its information environment, a reading under which the study's two seemingly contradictory findings, that visibility both weakens and strengthens investor response, become consistent with each other.

This study has four related limitations. First, 352 of the 541 Regime II filings occur in March 2026 because the statutory deadline follows the annual general meeting. CR2 standard errors, calendar-time portfolios, date-matched controls, and March-exclusion estimates reduce the risk of treating common-date returns as independent, but they cannot eliminate the calendar effect or create additional non-March variation. Second, the abbreviated format is used by 80.0 percent of Regime II filers and changes the availability of verification content at the same time that participation expands. Excluding abbreviated filings leaves only 50 rated Regime II observations, so the study lacks power to separate incentive dilution from reduced verifiability. Third, H3 is identified materially by 21 high-rated firms that offer a commitment. Jackknife and clustered placebo tests show that no single event date generates the result, but they do not remove the limitation on precision or generalizability. Fourth, ESG rating coverage falls from 84.8 percent of Regime I filers to 44.5 percent of Regime II filers. The credibility test therefore applies to the rated minority and may not extend to the unrated firms brought into the program by the tax incentive. The rating's correlation with governance and size also means that visibility cannot be isolated perfectly from these related firm characteristics.

These limitations point to specific next steps: observe additional filing seasons with more dispersed event dates; compare full and abbreviated filings under a design with credible variation in format use; expand the number of rated firms offering substantive commitments; and track realized corporate actions rather than implementation reports alone. Such evidence would allow the participation, timing, format, and monitoring mechanisms that move together in the first Regime II season to be distinguished more cleanly.

In conclusion, disclosure-based policy works best when what is disclosed is both new and believable. Disclosure informs investors, but credibility determines whether a commitment is priced, and the institutional environment determines which of the two is doing the work at any given time. The success of disclosure-based regulation therefore depends not only on how many firms disclose, but on whether their disclosures still tell investors something they did not already know.

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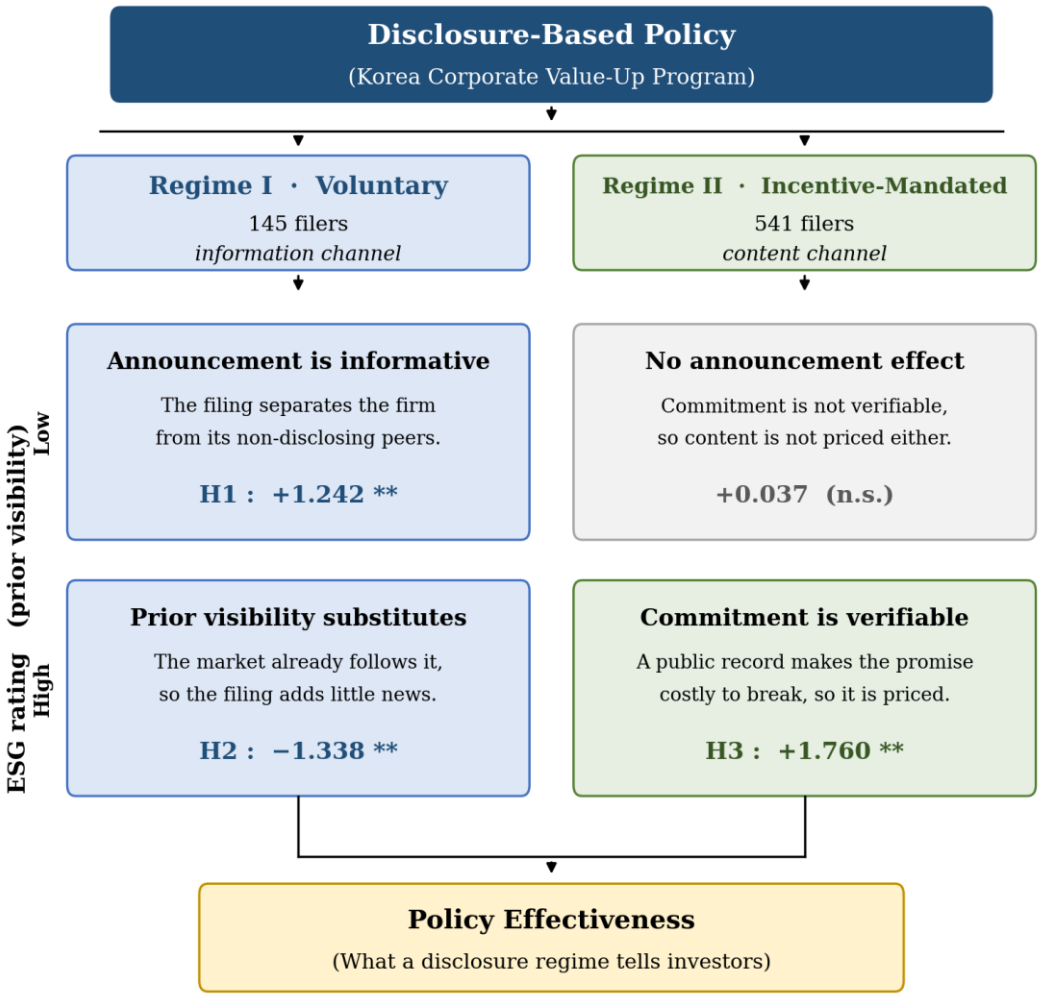
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Fig. 1 Conceptual Framework



Note: The regime determines which channel operates, and the ESG rating moderates whichever channel is active. In Regime I the market prices the decision to disclose, and prior visibility weakens that response. In Regime II the decision no longer distinguishes firms, so the market prices the specificity index instead, and prior visibility strengthens that response.

Table 1 Sample Composition

Regime I covers filings dated on or before December 31, 2025, the day before the amended Restriction of Special Taxation Act took effect; Regime II covers filings dated thereafter. Months is the span between the first and last event date of each sample, so the counts are cumulative totals rather than rates. Abbreviated filings complete at least one chapter of the plan proper and none of the four verification chapters.

Sample	Period	Months	Firms	KOSPI	KOSDAQ	Event dates	ESG rated (%)	High-dividend	Abbreviated (%)
Pooled	2024-05 to 2026-08	27	686	309	377	154	53.1	506	64.0
Regime I	2024-05 to 2025-12	20	145	106	39	87	84.8	0	4.1
Regime II	2026-01 to 2026-08	8	541	203	338	67	44.5	506	80.0

After matching the sample comprises 678 disclosing firms, 143 in Regime I and 535 in Regime II, paired with 1,872 non-disclosing observations. Of the 541 Regime II filings, 352 fall in March 2026, when the statutory deadline follows the annual general meeting. Only the earliest original filing of each firm is treated as an event, so the two regimes contain disjoint sets of firms.

Table 2 Disclosure Content and Format across Regimes

Panel A reports the content variables that enter the hypotheses; Panel B reports the format variables, which are descriptive only. The specificity index is the mean of the standardized quantified target areas and the standardized stock-cancellation indicator, both measured from the plan document accompanying each filing. In Panel B the first two rows are the chapters constituting the plan proper and their sum is form compliance; the next four make a plan verifiable and their sum is substantive effort. Text length is the character count of the disclosure form body. Differences are Regime I minus Regime II, with p from a two-sided t-test.

Panel A. Disclosure content

Variable	N	Pooled	SD	Regime I	Regime II	Difference	p
Specificity index	686	0.000	0.797	0.808	−0.217	1.024	<0.001
Quantified target areas	686	0.453	0.868	1.497	0.172	1.325	<0.001
Stock cancellation	686	0.155	0.362	0.303	0.115	0.189	<0.001
ESG rating (standardized)	364	0.195	0.990	0.760	−0.093	0.853	<0.001

Panel B. Disclosure format

Variable	N	Pooled	SD	Regime I	Regime II	Difference	p
Target setting	686	0.899	0.301	0.766	0.935	−0.170	<0.001
Plan formulation	686	0.870	0.336	0.669	0.924	−0.255	<0.001
Form compliance (0–2)	686	1.770	0.558	1.434	1.860	−0.425	<0.001
Company overview	686	0.235	0.424	0.828	0.076	0.752	<0.001
Diagnosis of current condition	686	0.179	0.384	0.655	0.052	0.603	<0.001
Implementation assessment	686	0.016	0.126	0.028	0.013	0.015	0.313
Communication	686	0.278	0.449	0.800	0.139	0.661	<0.001
Substantive effort (0–4)	686	0.708	1.136	2.310	0.279	2.031	<0.001
Areas mentioned (0–5)	686	1.764	1.041	2.876	1.466	1.410	<0.001
Investor-relations material	686	0.297	0.457	0.938	0.126	0.812	<0.001
Text length (characters)	686	281.6	144.3	347.1	264.1	83.0	<0.001

The two groups of format variables move in opposite directions: the chapters constituting the plan become more complete while the chapters that would make the promise verifiable collapse. This matches the structure of the abbreviated form rather than a uniform decline in effort.

Table 3 Sample Characteristics

Panel A reports descriptive statistics for disclosing firms alongside the covariate balance achieved by matching; control means are weighted by the matching weights. The standardized mean difference (SMD) is the disclosing-minus-control difference scaled by the disclosing standard deviation, with 0.1 as the conventional threshold. Control variables are winsorized at the first and ninety-ninth percentiles. Panel B reports pairwise Pearson correlations among disclosing firms. High-dividend status is unreported in seven Regime II filings, which is why its N is 671.

Panel A. Descriptive statistics and covariate balance

Variable	N	Mean	SD	Regime I	Regime II	Control N	Control mean	SMD before	SMD after
Log total assets	678	13.096	1.599	14.652	12.680	1,872	13.083	0.549	0.008
Leverage (%)	678	35.773	19.962	42.941	33.857	1,872	36.251	−0.269	−0.024
Price-to-book	678	1.620	2.723	1.771	1.580	1,872	2.029	−0.248	−0.150
KOSPI listing	678	0.448	0.498	0.727	0.374	1,872	0.456	0.386	−0.016
Return on assets (%)	678	4.660	5.829	5.275	4.496	1,872	4.362	1.023	0.051
Foreign ownership (%)	678	9.242	10.522	16.889	7.199	1,872	9.122	0.440	0.011
Log price-to-book	678	−0.061	0.932	−0.044	−0.066	1,872	0.003	−0.204	−0.069
ESG rating	361	0.188	0.988	0.757	−0.099	975	−0.012	0.517	0.203
Governance sub-score	361	0.205	0.979	0.747	−0.068	975	−0.024	0.479	0.234
Analyst coverage	678	0.096	1.050	1.199	−0.199	1,872	0.049	0.393	0.044
High-dividend firm	671	0.748	0.434	0.000	0.951	1,872	0.000	1.714	1.722
Index constituent	678	0.261	0.440	0.699	0.144	1,872	0.000	0.598	0.594
Abbreviated filing	678	0.642	0.480	0.035	0.804	1,872	0.000	1.332	1.337
Quantified target areas	678	0.453	0.870	1.510	0.170	—	—	—	—
Stock cancellation	678	0.155	0.362	0.301	0.116	—	—	—	—
Specificity index	678	0.001	0.798	0.812	−0.216	—	—	—	—

Across the six matching covariates the maximum absolute SMD falls from 1.023 to 0.150, and the matched sample pairs 678 disclosing firms with 1,872 controls, an average of 2.76 per disclosing firm. The propensity score is fitted on log price-to-book; the residual imbalance in raw price-to-book is addressed in Table 7 [B] by tightening the caliper on that variable. Variables below the matching covariates are reported for completeness and are not matched on. Disclosure content is undefined for firms that did not disclose, so no control mean or SMD exists.

Panel B. Correlation matrix, disclosing firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) ESG rating	1.000							
(2) Governance sub-score	0.909***	1.000						
(3) Analyst coverage	0.552***	0.481***	1.000					
(4) Foreign ownership	0.317***	0.308***	0.603***	1.000				
(5) Log total assets	0.634***	0.523***	0.696***	0.500***	1.000			
(6) Quantified target areas	0.347***	0.352***	0.387***	0.249***	0.383***	1.000		
(7) Stock cancellation	0.101*	0.110**	0.133***	0.150***	0.192***	0.270***	1.000	
(8) Specificity index	0.285***	0.294***	0.326***	0.250***	0.361***	0.797***	0.796***	1.000

The composite ESG rating and its governance sub-score correlate at 0.909 and are effectively the same construct, and the composite correlates with firm size at 0.634. The body therefore uses the composite rating throughout and reports the alternatives in Table 7 [E]. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4 Cumulative Average Abnormal Returns (CAAR)

CAAR is reported in percentage points on raw, unwinsorized abnormal returns. MM is the market model estimated over [−250, −21] with at least sixty valid returns; MA is the market-adjusted model. Panel A covers disclosing firms; Panel B reports the matched-pair difference with the disclosing and control means alongside, since a significant disclosing-firm CAAR is not an incremental effect if matched controls moved with it on the same day. ICC is the intraclass correlation of abnormal returns within event dates; Adj. t divides the conventional statistic by the square root of the resulting design effect; Port. t collapses each event date to a single observation.

Panel A. Disclosing firms

Model	Sample	Window	N	CAAR	t	ICC	Adj. t	Port. t
MM	Pooled	(−1,+1)	686	0.569**	2.56	0.187	2.00	2.02
MM	Pooled	(−3,+3)	686	1.196***	3.81	0.033	3.61	0.84
MM	Pooled	(−5,+5)	684	1.338***	3.50	0.029	3.33	0.32
MM	Pooled	(−10,+10)	682	3.143***	4.94	0.000	4.94	−0.37
MM	Regime I	(−1,+1)	145	1.164***	2.76	0.235	2.57	1.83
MM	Regime I	(−3,+3)	145	0.851	1.46	0.197	1.37	0.53
MM	Regime I	(−5,+5)	145	0.520	0.74	0.157	0.70	−0.11
MM	Regime I	(−10,+10)	145	0.146	0.15	0.027	0.15	−0.52
MM	Regime II	(−1,+1)	541	0.409	1.59	0.213	1.00	1.18
MM	Regime II	(−3,+3)	541	1.289***	3.52	0.059	2.96	0.66
MM	Regime II	(−5,+5)	539	1.558***	3.49	0.059	2.93	0.54
MM	Regime II	(−10,+10)	537	3.952***	5.20	0.024	4.80	−0.01
MA	Pooled	(−1,+1)	686	1.419***	5.94	0.272	4.26	2.11
MA	Pooled	(−3,+3)	686	1.757***	5.33	0.103	4.58	0.82
MA	Pooled	(−5,+5)	684	2.038***	5.02	0.113	4.25	0.06
MA	Pooled	(−10,+10)	682	1.841***	2.77	0.002	2.76	−0.26
MA	Regime I	(−1,+1)	145	1.412***	3.28	0.229	3.05	2.32
MA	Regime I	(−3,+3)	145	1.145*	1.95	0.205	1.83	1.05
MA	Regime I	(−5,+5)	145	0.901	1.29	0.124	1.24	0.48
MA	Regime I	(−10,+10)	145	0.867	0.91	0.000	0.91	0.27
MA	Regime II	(−1,+1)	541	1.421***	5.07	0.314	2.83	1.08
MA	Regime II	(−3,+3)	541	1.921***	4.96	0.139	3.52	0.17
MA	Regime II	(−5,+5)	539	2.344***	4.89	0.160	3.34	−0.32
MA	Regime II	(−10,+10)	537	2.103***	2.61	0.080	2.07	−0.58

Panel B. Matched-pair difference

Model	Sample	Window	N	Disclosing	Control	Difference	t	ICC	Adj. t	Port. t
MM	Pooled	(−1,+1)	678	0.573	0.357	0.216	0.79	0.067	0.71	1.70
MM	Pooled	(−3,+3)	678	1.198	0.744	0.454	1.22	0.000	1.22	0.48
MM	Pooled	(−5,+5)	676	1.343	0.862	0.480	1.04	0.000	1.04	0.55
MM	Pooled	(−10,+10)	674	3.148	1.745	1.402*	1.91	0.000	1.91	−0.48
MM	Regime I	(−1,+1)	143	1.073	0.066	1.008**	2.05	0.159	1.95	1.80
MM	Regime I	(−3,+3)	143	0.748	−0.219	0.966	1.48	0.131	1.42	1.16
MM	Regime I	(−5,+5)	143	0.441	−0.062	0.503	0.61	0.079	0.59	0.56
MM	Regime I	(−10,+10)	143	0.106	0.462	−0.357	−0.30	0.000	−0.30	−0.44
MM	Regime II	(−1,+1)	535	0.439	0.434	0.005	0.02	0.101	0.01	0.76
MM	Regime II	(−3,+3)	535	1.319	1.002	0.317	0.73	0.031	0.66	−0.43
MM	Regime II	(−5,+5)	533	1.585	1.110	0.474	0.87	0.016	0.82	0.23
MM	Regime II	(−10,+10)	531	3.967	2.091	1.876**	2.14	0.000	2.14	−0.22
MA	Pooled	(−1,+1)	678	1.422	0.797	0.625**	2.18	0.111	1.85	1.87
MA	Pooled	(−3,+3)	678	1.754	1.056	0.698*	1.83	0.006	1.81	0.87
MA	Pooled	(−5,+5)	676	2.045	1.126	0.918*	1.94	0.000	1.94	0.76
MA	Pooled	(−10,+10)	674	1.844	0.202	1.642**	2.17	0.000	2.17	0.34
MA	Regime I	(−1,+1)	143	1.314	0.088	1.225**	2.45	0.150	2.34	2.23
MA	Regime I	(−3,+3)	143	1.010	−0.235	1.245*	1.94	0.072	1.89	1.75
MA	Regime I	(−5,+5)	143	0.794	−0.121	0.915	1.15	0.022	1.14	1.25
MA	Regime I	(−10,+10)	143	0.794	0.661	0.133	0.12	0.000	0.12	0.33
MA	Regime II	(−1,+1)	535	1.451	0.986	0.464	1.37	0.155	0.94	0.78
MA	Regime II	(−3,+3)	535	1.953	1.401	0.552	1.22	0.058	1.02	−0.27
MA	Regime II	(−5,+5)	533	2.380	1.461	0.919	1.64	0.048	1.40	−0.02
MA	Regime II	(−10,+10)	531	2.127	0.079	2.049**	2.25	0.000	2.25	0.15

Panel B is the decisive comparison, since a disclosing-firm CAAR is informative only against what matched controls earned on the same day. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5 Main Results: The Two Channels

The dependent variable is CAR(−1,+1) in percentage points, winsorized at the first and ninety-ninth percentiles. Every column includes the six controls (log total assets, leverage, price-to-book, KOSPI listing, return on assets, foreign ownership) and industry fixed effects, and applies the matching weights. The three blocks use different samples: H1 the full matched sample, H2 the rated subsample, and H3 disclosing firms only, since content is undefined for firms that did not disclose. Of the 2,550 matched observations, eleven controls lack a valid abnormal return, leaving 2,539. The ESG rating is standardized, so its coefficients are per standard deviation. Standard errors in parentheses are clustered on the event date using the CR2 correction with Satterthwaite degrees of freedom, which vary across specifications, so the significance markers are not a monotone function of the t ratio.

Panel A. Market model

	(1) Pooled	(2) Regime I	(3) Regime II
H1. Disclosure effect — predicted b > 0			
Disclosure	0.241	1.242**	0.037
	(0.241)	(0.536)	(0.256)
N (event-date clusters)	2,539 (150)	544 (86)	1,995 (64)
H2. Information substitution — predicted b < 0 in Regime I			
Disclosure	−0.097	2.001***	−0.468
	(0.407)	(0.683)	(0.498)
ESG rating	−0.032	−0.029	0.153
	(0.202)	(0.310)	(0.250)
Disclosure × ESG rating	−0.114	−1.338**	0.066
	(0.251)	(0.536)	(0.348)
N (event-date clusters)	1,336 (144)	409 (86)	927 (58)
H3. Conditional credibility — predicted b > 0 in Regime II			
Specificity index	0.929	0.238	1.689
	(0.609)	(0.687)	(1.001)
ESG rating	−0.449	−1.580**	0.406
	(0.320)	(0.659)	(0.374)
Specificity index × ESG rating	0.001	−0.159	1.760**
	(0.488)	(0.593)	(0.719)
N (event-date clusters)	361 (119)	121 (77)	240 (42)

Panel B. Market-adjusted model

	(1) Pooled	(2) Regime I	(3) Regime II
H1. Disclosure effect — predicted b > 0			
Disclosure	0.609**	1.419**	0.421
	(0.266)	(0.563)	(0.298)
N (event-date clusters)	2,539 (150)	544 (86)	1,995 (64)
H2. Information substitution — predicted b < 0 in Regime I			
Disclosure	0.573	2.095***	0.236
	(0.412)	(0.697)	(0.548)
ESG rating	0.003	−0.021	0.130
	(0.241)	(0.323)	(0.311)
Disclosure × ESG rating	−0.434	−1.229**	−0.125
	(0.324)	(0.535)	(0.447)
N (event-date clusters)	1,336 (144)	409 (86)	927 (58)
H3. Conditional credibility — predicted b > 0 in Regime II			
Specificity index	0.535	0.305	1.270
	(0.664)	(0.708)	(1.103)
ESG rating	−0.750*	−1.592*	0.031
	(0.395)	(0.767)	(0.497)
Specificity index × ESG rating	0.096	−0.080	1.780**
	(0.484)	(0.635)	(0.797)
N (event-date clusters)	361 (119)	121 (77)	240 (42)

Cells identifying the H3 interaction

Regime	ESG rating	No commitment	Commitment offered
Regime I	High	14	80
Regime I	Low	6	21
Regime II	High	75	21
Regime II	Low	113	31

High and low are defined by the median ESG rating among all rated disclosing firms. A firm offers a commitment if it states at least one quantified target or a definite stock cancellation. The Regime II interaction is identified from a cell of twenty-one firms, which is the binding constraint on precision. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6 Supplementary Analyses

Neither panel tests a hypothesis. Panel A asks whether disclosure content is priced without conditioning on the ESG rating, first as the combined index and then as its two components entered jointly. Panel B asks whether the initial disclosures were followed by implementation reports. Panel A uses the market model and follows Table 5 in controls, fixed effects, weights and standard errors.

Panel A. Direct effect of disclosure content, market model

	(1) Pooled	(2) Regime I	(3) Regime II
A-1. Combined index			
Specificity index	0.930*	0.117	1.681
	(0.490)	(0.433)	(1.147)
ESG rating	−0.449	−1.700***	0.015
	(0.308)	(0.508)	(0.372)
N (event-date clusters)	361 (119)	121 (77)	240 (42)
A-2. Components entered jointly			
Quantified target areas	0.731**	0.293	2.443*
	(0.335)	(0.394)	(1.230)
Stock cancellation	0.840	−0.348	0.617
	(1.026)	(0.849)	(1.764)
ESG rating	−0.482	−1.817***	0.099
	(0.326)	(0.539)	(0.373)
N (event-date clusters)	361 (119)	121 (77)	240 (42)

Of the two components it is the quantified target count, not the cancellation pledge, that carries the pooled association, and in Regime II the target count alone is positive and significant at the ten percent level.

Panel B. Subsequent implementation reporting, Regime I disclosing firms

ESG rating	Plan content	Firms	Filed a report	Share (%)
High	Commitment offered	64	37	57.8
High	No commitment	11	6	54.5
Low	Commitment offered	38	18	47.4
Low	No commitment	10	3	30.0

Explanatory variable	Coefficient (SE)
Initial CAR(−1,+1)	0.010 (0.045)
ESG rating	0.291 (0.292)
Specificity index	0.156 (0.219)

Logistic regression on all 123 Regime I disclosing firms carrying an ESG rating, with the six controls and all three explanatory variables entered jointly; heteroskedasticity-robust standard errors in parentheses, without industry fixed effects, matching weights or event-date clustering. High and low are defined by the median ESG rating within this sample. With 64 events, 59 non-events and ten parameters the ratio of events per variable is 5.9, below the conventional threshold of ten. The dependent variable is whether a separate implementation report was filed, not whether the plan was implemented.

Table 7 Robustness

This table reports the three principal coefficients under alternative settings: [A] the dependent variable, [B] the matching design, [C] the sample, [D] additional controls, [E] the moderating variable, and [F] the definition of the specificity index. The market model and the market-adjusted model appear side by side. Entries marked n.a. indicate that the setting does not bear on that coefficient: the two March 2026 rows restrict the sample to Regime II, [E] requires a moderator, and [F] alters the specificity index. N is the number of observations behind the H3 interaction. Standard errors in parentheses are clustered on the event date; Satterthwaite degrees of freedom vary across specifications, so the significance markers are not a monotone function of the t ratio.

Specification	H1 Regime I (MM)	H1 Regime I (MA)	H2 Regime I (MM)	H2 Regime I (MA)	H3 Regime II (MM)	H3 Regime II (MA)	N
Baseline (1% winsorized, 1:3 matching)	1.242** (0.536)	1.419** (0.563)	−1.338** (0.536)	−1.229** (0.535)	1.760** (0.719)	1.780** (0.797)	240
[A] No winsorization	1.156** (0.565)	1.336** (0.592)	−1.498** (0.596)	−1.385** (0.601)	1.991* (0.906)	2.067* (1.013)	240
[A] Window (−3,+3)	1.294* (0.728)	1.482* (0.753)	0.223 (0.698)	0.574 (0.712)	1.334 (1.349)	0.643 (1.436)	240
[A] Window (−5,+5)	1.007 (0.958)	1.187 (0.957)	−1.063 (1.125)	−0.313 (1.138)	1.314 (1.272)	1.183 (1.292)	238
[B] One-to-one matching	1.860*** (0.649)	2.173*** (0.692)	−1.673*** (0.536)	−1.402** (0.583)	1.773** (0.729)	1.819* (0.837)	240
[B] Exact matching on industry	1.071* (0.560)	1.177* (0.596)	−1.473** (0.604)	−1.369** (0.570)	1.942** (0.714)	2.033** (0.783)	226
[B] Price-to-book caliper 0.5 SD (SMD 0.002)	1.415** (0.585)	1.478** (0.617)	−1.605*** (0.512)	−1.425** (0.556)	1.873** (0.840)	1.762 (0.991)	234
[B] Entropy balancing (max SMD 0.064)	1.188** (0.555)	1.309** (0.580)	−1.370** (0.561)	−1.326** (0.551)	1.760** (0.719)	1.780** (0.797)	240
[C] Excluding March 2026	n.a.	n.a.	n.a.	n.a.	2.495 (1.771)	2.244 (2.006)	68
[C] March 2026 only	n.a.	n.a.	n.a.	n.a.	1.358 (0.724)	1.403 (0.817)	172
[C] Excluding abbreviated filings	1.300** (0.528)	1.473** (0.557)	−1.243** (0.540)	−1.122** (0.542)	2.430 (1.970)	2.524 (1.928)	50
[D] High-dividend and index controls	2.209** (1.035)	2.348** (1.077)	−1.215** (0.553)	−1.120* (0.564)	1.272** (0.533)	1.306* (0.721)	237
[D] Size interaction control	1.616* (0.900)	1.866* (0.942)	−1.300** (0.629)	−1.200* (0.704)	1.742 (0.887)	1.930* (0.972)	240
[E] Moderator = governance sub-score	n.a.	n.a.	−1.119* (0.565)	−0.959* (0.561)	1.157 (0.810)	1.283 (0.819)	240
[E] Moderator = analyst coverage	n.a.	n.a.	−0.841* (0.481)	−0.701 (0.484)	0.745 (1.019)	0.419 (1.184)	240
[E] Moderator = foreign ownership	n.a.	n.a.	0.107 (0.438)	0.029 (0.461)	−0.119 (0.637)	−0.515 (0.774)	240
[F] Quantified target areas only	n.a.	n.a.	n.a.	n.a.	1.787* (0.886)	1.799 (1.006)	240
[F] Stock cancellation only	n.a.	n.a.	n.a.	n.a.	1.000* (0.458)	1.002* (0.491)	240
[F] Any commitment (binary)	n.a.	n.a.	n.a.	n.a.	1.467 (1.162)	1.237 (1.374)	240

Entropy balancing weights the control group to match the disclosing firms on the first moment of every matching covariate rather than discarding observations, and reproduces the matched-sample estimates. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8 Inference Stability

Panel A re-draws the matched sample twenty times with the matching order randomized. Panel B deletes each event date in turn and re-estimates. Panel C shifts the event date of each matched pair by a random interval of thirty to two hundred and sixty trading days in either direction, preserving the pair structure and the clustering of filings, and repeats this five hundred times; the empirical p-value is the share of placebo draws whose absolute coefficient equals or exceeds the actual one.

Panel A. Matching sampling variation, twenty re-draws

Coefficient	MM min	MM median	MM max	MM SD	MA min	MA median	MA max	MA SD
H1 Disclosure effect (Regime I)	1.194	1.280	1.386	0.060	1.374	1.480	1.602	0.060
H2 Disclosure × ESG (Regime I)	−1.454	−1.364	−1.201	0.079	−1.330	−1.227	−1.076	0.082
H3 Specificity × ESG (Regime II)	1.728	1.808	1.902	0.039	1.779	1.827	1.900	0.032

Panel B. Event-date jackknife

Coefficient	MM min	MM median	MM max	MM SD	MA min	MA median	MA max	MA SD
H2 Disclosure × ESG (Regime I)	−1.505	−1.339	−1.091	0.062	−1.433	−1.231	−0.994	0.061
H3 Specificity × ESG (Regime II)	1.172	1.760	1.953	0.102	1.158	1.780	1.965	0.112

Panel C. Placebo test, five hundred re-draws

Coefficient	MM actual	MM placebo mean	MM placebo SD	MM empirical p	MA actual	MA placebo mean	MA placebo SD	MA empirical p
H1 Disclosure effect (Regime I)	1.242	0.071	0.505	0.012	1.419	0.062	0.514	0.004
H2 Disclosure × ESG (Regime I)	−1.338	0.055	0.633	0.042	−1.229	0.087	0.642	0.068
H3 Specificity × ESG (Regime II)	1.760	−0.024	0.705	0.018	1.780	−0.005	0.805	0.028

No re-draw and no single-date deletion reverses a sign under either return model. All six placebo p-values fall below ten percent.