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Philippines Publicly Listed Property Companies' Potential for Greenwashing and Earnings Management and the Moderating Effect of Profitability: Evidence from Published Sustainability and Financial Reports

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Abstract. This study examined a disclosure-based proxy for potential greenwashing and its association with accrual-based earnings management among 40 publicly listed Philippine property firms during 2022–2024, comprising 120 firm-year observations. A 35-item environmental disclosure index classified information as absent, symbolic, or substantive. The gap between overall and substantive-scored disclosure equals the number of symbolic items divided by 70. Coding used a shared handbook and author cross-review, but inter-rater reliability was not quantified. Discretionary accruals were estimated using the Modified Jones Model. The main random-effects model used firm-level pairs-bootstrap uncertainty and wild-bootstrap p-values. The mean gap was .1171. Signed mean discretionary accruals were .0239, −.0020, and .0057 across 2022–2024. Neither the disclosure coefficient ($b = -.0235$, $p = .933$) nor the profitability interaction ($b = 8.044$, $p = .135$) was statistically significant; reported R^2 was .228. Year indicators and lagged ROA did not support the hypotheses. Some exploratory outlier treatments yielded significant interactions, indicating specification sensitivity. Results concern symbolic disclosure and model-unexplained accruals; they neither validate the score as a measure of environmental misrepresentation nor establish the absence of a relationship between actual greenwashing and earnings management.

Keywords: Greenwashing, Earnings Management, Sustainability Disclosure, Property Companies, panel regression

1 Introduction

Because of rapid urbanization and the growing demand for transparency, there has been increased focus on sustainability and on environmental, social, and governance (ESG) reporting [27], [12]. As a result, the property sector is subjected to very strict ESG disclosure requirements and is under increasing pressure to incorporate green buildings and energy-efficient designs [2], [32], [14]. The need to project a sustainable image without sacrificing financial performance might provide scope for strategic reporting. In the Philippines, the Securities and Exchange Commission (SEC)'s 2019 sustainability reporting guidelines introduced a "comply or explain" approach for companies that are listed on the stock exchange [28]. Moreover, since the property sector's accounting methods rely on managerial judgment as a result of its capital-intensive character and dependence on long-term assets [19], [20], there is room for flexibility in both financial and sustainability reporting. For this reason, the present study looks at the relationship between possible greenwashing (GW) and earnings management (EM) among publicly listed property firms in the Philippines.

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Even though sustainability reporting has become more institutionalized, there are still worries regarding the credibility of ESG disclosures and the possibility of sustainability reporting being used as a strategic means to affect stakeholder perceptions [3], [13]. Companies might engage in greenwashing by overemphasizing their environmental commitments while hiding their poor actual performance, thus producing a talk–walk gap between what is disclosed and what is actually implemented [7]. Meanwhile, earnings management continues to be a problem in financial reporting, with managerial discretion being employed to affect the reported financial performance [22]. There is already evidence to indicate that sustainability disclosures and earnings management may be linked, since firms can use ESG reporting strategically together with their financial reporting practices to influence stakeholder perceptions [29], [25].

Although earlier studies have looked into ESG disclosure and earnings management, only a small number of them have directly examined greenwashing as a separate concept connected to financial reporting behavior [17], [30]. A great deal of the existing literature focuses on the amount of ESG disclosures rather than on the authenticity or quality of the sustainability claims, and this could lead to the neglect of symbolic reporting practices [8], [4]. Furthermore, researchers tend to study sustainability reporting and earnings management separately, which hinders an understanding of the relationship between non-financial and financial manipulation within companies.

The study examines whether a symbolic environmental disclosure score, as a disclosure-based proxy for potential greenwashing (GW), is associated with the discretionary-accrual proxy for earnings management (EM) and whether profitability moderates that relationship. The specific objectives are: (1) to describe the extent of symbolic disclosure under the study's 35-item coding rubric; (2) to describe the levels and directions of Modified Jones Model discretionary accruals; (3) to estimate the association between the continuous disclosure score and discretionary accruals; and (4) to test the interaction between that score and profitability. The environmental measure is not an independently verified talk–walk discrepancy, and discretionary accruals do not estimate the probability of intentional manipulation.

The study contributes evidence about the relationship between environmental disclosure specificity and an accounting proxy in the Philippine property sector. Its findings can inform a cautious assessment of sustainability and financial reporting, while recognizing that the selected measures establish neither the credibility of environmental outcomes nor managerial intent.

Agency theory links information asymmetry and managerial discretion to the possibility of opportunism in both sustainability and financial reporting, while legitimacy and stakeholder theories provide an explanation for why companies might choose to disclose sustainability information in order to meet external expectations [16], [31], [11], [22].

Greenwashing measures need to distinguish reported information from environmental outcomes. Marquis et al. [6] compare disclosure coverage with a ratio weighted by estimated environmental damage. Lublóy et al. [4] review varied greenwashing measures. The present index draws on the general distinction between symbolic and substantive information. It directly measures symbolic environmental disclosure, and its use as a potential GW proxy requires a narrower interpretation.

Earnings management includes some accounting judgment which can change the reported performance [22]. The discretionary accruals derived from the Modified Jones Model show accruals that are not explained, even though they might also be due to legitimate business operations [5], [21].

The profitability obtained by means of the return on assets (ROA) can influence the incentives and pressures associated with both kinds of reporting. The evidence regarding sustainability and earnings management is mixed, with some studies finding no significant relationship between the two [18], [15]. Firm age is included as a control to take into account organizational maturity [9].

The study examines two directional hypotheses: H1 proposes a positive relationship between potential GW, operationalized as the symbolic disclosure score, and discretionary accruals; H2 proposes that profitability strengthens that relationship. These hypotheses concern the specified proxies in an observational panel. Their tests do not identify causal effects or establish a relationship between independently verified environmental misrepresentation and financial manipulation.

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2 Methodology

2.1 Research design and sample

The study adopts a quantitative and explanatory approach in order to look at the relationship between potential GW and EM among publicly listed property companies in the Philippines. For this purpose, it employs a non-experimental, archival, and analytical approach, making use of secondary financial and ESG data drawn from audited annual reports and publicly available sustainability disclosures, since these constitute the companies' ESG disclosures within the Philippine SEC sustainability reporting framework [6]. The analysis is carried out at the level of the firm, employing a panel data design covering the period from 2022 to 2024 that keeps a continuous record of the same property firms over a number of different time periods in order to observe the changes that take place over time.

The starting number of firms in the property sector of the Philippine Stock Exchange was 51. One firm was excluded from the study because it was not consistently listed between 2022 and 2024; nine subsidiaries were omitted since their parent companies were already included; and one firm was left out due to a lack of adequate data for the Modified Jones calculation. The resulting balanced panel consisted of 40 firms and 120 firm-year observations (Table 1).

Table 1. Purposive selection of Philippine listed property firms

No	Criteria	Amount
1	Total Population (publicly listed companies in the property sector)	51
2	Companies not consistently listed during the study period (2022–2024)	(1)
3	Companies that are subsidiaries of other property firms included in the sample	(9)
4	Companies with incomplete or unavailable sustainability/annual reports	0
5	Companies with insufficient variables required for MJM computation	(1)
6	Companies delisted during the study period	0
	Sample (n)	40
	Total observations (n × 3 years)	120

The secondary data were obtained from audited financial statements, annual reports, company profiles, and the published sustainability disclosures. The study put the financial variables and environmental indicators into a spreadsheet and removed those firm-years which had insufficient numbers of the required variables.

2.2 Measures and disclosure coding

The study used a 35-item environmental disclosure index (EDI) covering eight categories: policy and governance, energy and emissions, water, waste and pollution, biodiversity and land use, compliance and legal issues, environmental financial information, and property-sector indicators. Each item received 0 for absent, unrelated, or unsupported disclosure; 1 for an explicit relevant statement that did not meet the item's substantive criteria; or 2 for the indicator-specific evidence in Appendix A. Long et al. [10] inform graded disclosure scoring, and Cao et al. [23] inform the symbolic–substantive distinction. No EDI item was excluded from the denominator.

Coding applied to one indicator for one firm in one reporting year. Only explicit information attributable to the reporting boundary received credit. Coders did not carry statements forward from another year, infer one indicator from a related activity, or calculate an intensity metric that the company had not disclosed. A blank or unsupported not-applicable entry was not treated as a measured zero. A parent or sponsor initiative required explicit attribution to the reporting entity; included subsidiary activities could be credited within a stated consolidated boundary.

For quantitative indicators, score 2 requires an explicitly disclosed quantity or intensity metric, including a measured zero where appropriate. Other indicators permit detailed policies, measurable targets, implemented programs, or achieved certifications without requiring a numerical outcome. Thus, "qualitative" and "symbolic" are not interchangeable, and a substantive target need not have been achieved. Appendix A provides all 35 indicator-specific substantive criteria and illustrative applications.

The authors participated in coding. The authors cross-checked other companies' coding, and the shared handbook incorporated clarifications arising from discussion of disagreements. These were review and reconciliation procedures. The study does not report a separate formal coder-training or pilot-calibration exercise, a prespecified escalation rule for unresolved disagreements, or an independent adjudicator.

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No pre-reconciliation inter-rater reliability coefficient is reported: Cohen's kappa, Krippendorff's alpha, ICC, or another agreement statistic was not quantified in the reported analysis. Manual cross-checking and agreement after discussion do not estimate how reproducibly coders would classify the same evidence independently.

For each firm-year, N_1 denotes the number of items coded 1 and N_2 the number coded 2. The common maximum is 35 items multiplied by 2 points, or 70. Equations (1)–(3) express the reported ratios and their algebraic identity:

$$ADR = \frac{N_1 + 2N_2}{70} \quad (1)$$

$$SDR = \frac{2N_2}{70} \quad (2)$$

$$Gap = ADR - SDR = \frac{N_1}{70} \quad (3)$$

Substantive items contribute two points to both ADR and SDR and cancel in the difference; symbolic items contribute one point only to ADR. The gap therefore equals N_1 divided by 70 and ranges from 0 to 0.5. With a fixed 35-item checklist, it is a rescaled count of symbolic items, not their proportion relative to substantive items or to all disclosed items. A higher score indicates more items classified as symbolic under the rubric.

An entirely undisclosed checklist and an entirely substantive checklist both yield a zero gap. Adding substantive disclosure while leaving symbolic items unchanged also leaves the gap unchanged. For illustration, eight symbolic items yield 0.1143 whether the remaining 27 items are absent or substantive. These are algebraic examples, not sample observations. A low score therefore cannot establish disclosure completeness, strong environmental performance, or the absence of greenwashing.

Annual quartiles provide relative groups within the sample: upper groups contain higher symbolic-item counts under the same rubric. Gregory [24] provides a precedent for distribution-based grouping. Quartiles are not thresholds for environmental misconduct, and upper-group membership does not establish a higher probability of verified greenwashing.

Discretionary accruals were estimated using the Modified Jones Model [5], [33]. Total accruals use the balance-sheet approach; short-term debt includes the current portion of long-term debt. The change in current assets was corrected to use consecutive current-asset balances. All ratios use internally consistent monetary units, and lagged assets in the reciprocal-assets regressor were standardized to pesos, including four firms reporting in thousands of pesos. The total-accrual equation is:

$$TA_{it} = \Delta CA_{it} - \Delta Cash_{it} - \Delta CL_{it} + \Delta STD_{it} - DAE_{it}$$

For each year, 2022–2024, the first-stage regression was estimated cross-sectionally over the 40 firms. The change in revenue enters this estimation without subtracting the change in receivables:

$$\frac{TA_{it}}{A_{i,t-1}} = \alpha_0 + \alpha_1 \frac{1}{A_{i,t-1}} + \beta_1 \frac{\Delta R_{it}}{A_{i,t-1}} + \beta_2 \frac{PPE_{it}}{A_{i,t-1}} + \varepsilon_{it}$$

Nondiscretionary accruals retain the estimated intercept and apply the receivables adjustment when constructing the expected accrual component:

$$NDA_{it} = \hat{\alpha}_0 + \hat{\alpha}_1 \frac{1}{A_{i,t-1}} + \hat{\beta}_1 \frac{\Delta R_{it} - \Delta AR_{it}}{A_{i,t-1}} + \hat{\beta}_2 \frac{PPE_{it}}{A_{i,t-1}}$$

$$DA_{it} = \frac{TA_{it}}{A_{i,t-1}} - NDA_{it}$$

TA denotes total accruals; CA current assets; Cash cash balances; CL current liabilities; STD debt included in current liabilities; DAE depreciation and amortization; A total assets; R revenue; AR receivables; and PPE gross property, plant and equipment. Changes compare consecutive financial years. The per-year DA series remains the main dependent variable, with all 120 firm-years retained.

Positive discretionary accruals indicate adjustments which increase income, negative values indicate adjustments which reduce income, and the greater the absolute magnitude, the greater the unexplained accrual activity. Since these are indirect measures, they do not prove that there has been deliberate manipulation [21], [26].

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ROA was obtained by dividing net income by average total assets (the mean of opening and closing total assets); firm age was defined as the study year minus the year of incorporation. The panel model included the mean-centered gap and ROA, their interaction, and firm age; therefore, the coefficients on the mean-centered terms captured the conditional associations.

2.3 Statistical analysis

Pooled OLS, firm fixed effects, and Swamy–Arora random effects were compared using an F test for firm intercepts, the Breusch–Pagan LM test for random effects, its one-sided Honda counterpart, and the Hausman test. A Mundlak test with firm-clustered covariance additionally assessed the joint contribution of firm means of the regressors. Random effects was retained as the primary specification for continuity with the study design, with pooled OLS and fixed effects informing sensitivity assessment. A non-significant Hausman or LM result was not treated as proof that random effects was preferable or that the firm effect was exogenous.

Diagnostics comprised variance inflation factors, the studentized Koenker–Breusch–Pagan test, within-firm Breusch–Godfrey tests of orders 1 and 2, the Wooldridge first-difference test, Pesaran CD with 10,000 within-firm permutations of time order, and Shapiro–Wilk testing. These are specification diagnostics, and non-rejection does not establish that a violation is absent. The permutation reference for CD was used because only three years were observed.

Standard errors and percentile 95% confidence intervals were obtained from 10,000 firm-level pairs-bootstrap replications: firms were sampled with replacement, all three years of each sampled firm were retained, duplicated draws were treated as distinct clusters, and the random-effects model and variance components were re-estimated. Predictors retained the original sample centering. Primary two-sided coefficient p-values use a restricted wild cluster bootstrap-t with the null imposed, 9,999 Rademacher draws, CR1 studentization, and the fitted GLS transformation held fixed. Webb weights and CR1 t tests provided additional comparisons. The main analysis used seed 1001 for pairs resampling and seeds 1003/1004 for the wild checks; corresponding lagged-model seeds were 2001 and 2003/2004. No pairs-bootstrap replication failed. The wild-bootstrap approach follows Cameron et al. [34].

Sensitivity analyses added year indicators, compared firm fixed effects, inspected Cook's D, studentized residuals, and leave-one-firm-out estimates, and winsorized DA alone or DA and ROA at pooled 1st/99th and 5th/95th percentiles. Interactions were recomputed from re-centered variables after winsorization; exclusions removed whole firms to retain balanced panels. A lagged-ROA analysis used 39 firms and 117 firm-years with DA re-estimated on those firms and a contemporaneous benchmark using the same sample and DA. Approximate minimum detectable interactions and power at specified benchmark effects were calculated from observed standard errors, a two-sided 5% level, and $G - 1$ degrees of freedom. These are sensitivity calculations, not a prospective sample-size design or equivalence test.

The additional estimators and resampling procedures were implemented in Gretl. Their numerical implementation and treatment of short-panel diagnostics form part of the reproducibility requirements. Statistical decisions used $\alpha = .05$.

3 Results and Discussion

3.1 Disclosure gap and discretionary accruals

Table 2 reports mean discretionary accruals of .0239 in 2022, −.0020 in 2023, and .0057 in 2024. The series has no monotonic downward mean trend. Medians were −.0365, −.0032 and .0145, respectively. These are signed summaries: opposite directions can offset, so means near zero do not establish low absolute accrual magnitude or a low probability of earnings management.

Annual standard deviations were .1928, .1079, and .0657. The largest value, .8736, occurred in 2022; the minimum, −.4641, occurred in 2023. The 2024 range was −.2941 to .1044. Dispersion was descriptively narrower in later years, but this does not establish improved reporting integrity or reduced intentional manipulation. Table 2 includes the pooled summary for all 120 firm-years.

Table 2. Descriptive statistics of discretionary accruals by year

Year	N	Mean	Median	SD	Minimum	Maximum
2022	40	0.0239	−0.0365	0.1928	−0.1275	0.8736
2023	40	−0.0020	−0.0032	0.1079	−0.4641	0.2541

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Year	N	Mean	Median	SD	Minimum	Maximum
2024	40	0.0057	0.0145	0.0657	−0.2941	0.1044
Overall	120	0.0092	−0.0018	0.1324	−0.4641	0.8736

As shown in Table 3, the pooled mean disclosure score was .1171, and the median was 0.1143. These values summarize symbolic-item contributions to the 70-point checklist maximum; they are not percentages of false claims or estimates of the prevalence of greenwashing.

The pooled standard deviation was .0488, and scores ranged from 0.0143 to 0.2286. Under the fixed denominator, these differences reflect variation in the number of items classified as symbolic across firm-years. They do not measure the distance between reported commitments and actual environmental outcomes.

Table 3 reports pooled 25th and 75th percentiles of 0.0857 and 0.1429. The middle 50% of scores therefore lay between approximately 0.086 and 0.143. Higher positions indicate larger symbolic-item counts, without establishing symbolic dominance over substantive disclosure or worse environmental performance.

Annual mean scores in Table 3 remained close to 0.12 during 2022–2024. Table 4 records 57 of the 120 firm-year observations (47.5%) in Q3 or Q4 of their annual distributions, including 29 (24.2%) in Q4. These totals combine annual classifications; they describe relative positions rather than an independently validated greenwashing-risk threshold. Discrete scores and ties can produce unequal group sizes.

The descriptive findings establish variation in symbolic environmental disclosure under the study's coding definitions. They do not identify verified greenwashing incidents, demonstrate a preference for symbolic over substantive reporting, or establish a discrepancy between claims and independently observed environmental performance. Interpretation of low scores also requires attention to non-disclosure, which contributes zero to the gap.

Table 3. Descriptive statistics of potential GW

Year	N	Mean	Median	SD	Min.	Q1	Q3	Max.
2022	40	0.1182	0.1143	0.0491	0.0286	0.0857	0.1464	0.2286
2023	40	0.1150	0.1143	0.0486	0.0286	0.0857	0.1429	0.2143
2024	40	0.1182	0.1214	0.0500	0.0143	0.0857	0.1464	0.2143
Overall	120	0.1171	0.1143	0.0488	0.0143	0.0857	0.1429	0.2286

Table 4. Annual quartile groups of the disclosure score (inclusive cutoffs)

Year	Q1	Q2	Q3	Q4	Total
2022	14 (35.0%)	7 (17.5%)	9 (22.5%)	10 (25.0%)	40 (100%)
2023	14 (35.0%)	8 (20.0%)	9 (22.5%)	9 (22.5%)	40 (100%)
2024	11 (27.5%)	9 (22.5%)	10 (25.0%)	10 (25.0%)	40 (100%)
Overall	39 (32.5%)	24 (20.0%)	28 (23.3%)	29 (24.2%)	120 (100%)

3.2 Accrual measurement and influential observations

The largest 2022 DA, 0.8736, belongs to a newly incorporated property-sector entity with a small opening asset base and a major property-for-share transaction. Its reported assets increased approximately 54-fold. Scaling by small lagged assets makes the accrual estimate sensitive to this structural change. This provides an operating and measurement explanation requiring scrutiny; it does not establish either deliberate manipulation or its absence. The firm remains in the main sample because it meets the study's sector and data criteria.

A second flagged entity experienced large balance-sheet changes and a 2024 ROA of approximately 0.584. Its DA values, approximately −0.276 in 2023 and 0.104 in 2024, are not the largest accrual extremes. Panel influence diagnostics and exclusions are discussed separately from first-stage MJM influence; their Cook's D statistics must not be treated as interchangeable.

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For the lagged-ROA analysis, the entity without 2020 financial statements could not supply 2021 ROA based on average opening and closing assets. Per-year MJM estimates were therefore recalculated on the remaining 39 firms. For the same 117 firm-years, the 39-firm and main DA series have Pearson correlation .979 and Spearman correlation .920, with a mean absolute difference of .0094. The 39-firm DA standard deviations are .1342, .1093 and .0665 for 2022–2024. These comparisons assess the available re-estimated series and do not establish stability under every possible first-stage specification.

3.3 Model selection and diagnostics

Table 5 reports the estimator comparisons. The firm-effects F test ($p = .243$), random-effects LM test ($p = .836$) and one-sided Honda test ($p = .418$) did not establish a firm-level variance component. Estimated firm variance was .0004015 and $\theta = .0428$, so random effects was close to pooled OLS, without being numerically identical. These results do not establish superiority over pooled OLS.

The Hausman test yielded $\chi^2(4) = 8.82$, $p = .066$. Its null was not rejected at 5%, but the firm-clustered Mundlak test rejected ($\chi^2(4) = 26.84$, $p < .001$), questioning random-effects exogeneity. The Mundlak p-value was .074 after the most influential firm was excluded. Estimator evidence is therefore mixed. Random effects remains the primary specification for continuity, with fixed effects reported as a sensitivity comparison.

The studentized Koenker–Breusch–Pagan test gave $\chi^2(4) = 8.16$, $p = .086$. Heteroskedasticity was not detected at the 5% level, which does not prove constant error variance. Breusch–Godfrey tests of orders 1 and 2 yielded $p = .661$ and .886, and the Wooldridge first-difference test gave $p = .533$. They did not detect serial correlation in this short panel.

In the main model, Pesaran CD was 1.62, with permutation $p = .102$, so cross-firm dependence was not detected at 5%. It was detected in the year-indicator and lagged-ROA sensitivity models, however. Firm-clustered inference does not by itself protect against cross-firm common errors; those sensitivity results require additional caution.

Shapiro–Wilk gave $W = .786$, $p < .001$, indicating residual non-normality. Main-model coefficient inference uses the firm-level pairs bootstrap, wild cluster bootstrap-t and CR1 comparisons described in Section 2.3. These methods do not correct endogeneity, first-stage accrual-model misspecification, or the construct limitations of the environmental disclosure score.

Table 6 reports VIFs from 1.020 to 1.566. These indicate low collinearity among the included predictors. Mean-centering also defines the reference points of the conditional coefficients; it does not validate the underlying measures or establish other modeling assumptions.

Table 5. Model selection and diagnostic tests

Test	Statistic	p	Interpretation
F test for firm effects	$F(39,76) = 1.20$	.243	Firm effects not detected against pooled OLS.
LM for random effects	$\chi^2(1) = 0.04$	.836	Positive firm variance not established.
Honda one-sided LM	$z = 0.21$	.418	Positive firm variance not established.
Hausman	$\chi^2(4) = 8.82$	.066	Null not rejected at 5%.
Mundlak with firm-clustered covariance	$\chi^2(4) = 26.84$	< .001	RE exogeneity questioned; p = .074 when the most influential firm is excluded.
Koenker–Breusch–Pagan	$\chi^2(4) = 8.16$	.086	Heteroskedasticity not detected at 5%.
Breusch–Godfrey order 1	$LM(1) = 0.19$	.661	Serial correlation not detected.
Breusch–Godfrey order 2	$LM(2) = 0.24$	.886	Serial correlation not detected.
Wooldridge first difference	$F(1,39) = 0.395$	.533	Serial correlation not detected.
Pesaran CD with permutation inference	$CD = 1.62$	.102	Cross-firm dependence not detected in the main model.
Shapiro–Wilk	$W = 0.786$	< .001	Residual normality rejected.

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Table 6. Variance inflation factors

Predictor	VIF
Centered disclosure score (GW)	1.020
Centered ROA	1.529
Firm age	1.066
Centered GW × centered ROA	1.566

3.4 Panel regression

The main random-effects model has reported transformed-model $R^2 = .228$ and adjusted $R^2 = .201$ (120 firm-years, 40 firms). Table 7 reports point estimates, pairs-bootstrap standard errors and percentile intervals, CR1 p-values, and the wild cluster bootstrap-t p-values used for primary hypothesis evaluation. The pairs bootstrap used 10,000 replications and the wild procedure 9,999. No main-model coefficient was significant at 5% under these methods.

Table 7. Random-effects estimates with firm-level bootstrap inference

Predictor	Estimate	Pairs boot SE	95% percentile CI	p CR1	p wild
Intercept	0.0082	0.0228	−0.0321 to 0.0572	0.689	0.710
Disclosure score (GW-centered)	−0.0235	0.2440	−0.5358 to 0.4199	0.919	0.933
Profitability (ROA-centered)	−0.4264	0.3048	−0.8910 to 0.2312	0.138	0.369
Firm age	0.00008	0.00053	−0.00120 to 0.00092	0.863	0.864
GW-centered × ROA-centered	8.0444	5.8058	−4.6130 to 18.4465	0.121	0.135

Note. DA is the outcome. N = 120, G = 40. $R^2 = .228$ and adjusted $R^2 = .201$. CI denotes the pairs-bootstrap percentile interval. CR1 uses firm-clustered covariance with 39 t degrees of freedom. Wild p-values use the restricted Rademacher bootstrap-t. The disclosure coefficient is conditional on mean ROA and the ROA coefficient on mean disclosure.

The centered disclosure score had a negative but statistically non-significant coefficient (b = −.0235, p = .933) at mean profitability. H1 was not supported. The estimate concerns symbolic-item counts on the study's scale, and neither demonstrates a positive relationship nor establishes its absence. Reports of non-significant sustainability–EM associations [18], [15] offer contextual comparisons, but use different constructs and do not validate the present proxy.

The coefficient for profitability was negative but not significant (b = −.4264, p = .369); likewise, that for firm age was also not significant (b = .00008, p = .864). The estimates obtained do not offer sufficient evidence of the relevant conditional relationships within this model. Another study looking at Indonesian listed property and real estate companies also found no significant link between ROA and earnings management [1], even though the data and model used in that study differ from the present one.

The interaction between the disclosure score and ROA was positive but statistically non-significant (b = 8.044, p = .135). H2 was not supported: the analysis did not establish that profitability changed the relationship between the selected disclosure and accrual proxies. Neither the positive point estimate nor its non-significance establishes how actual greenwashing relates to earnings management.

The interaction's 95% pairs-bootstrap interval is −4.61 to 18.45. Conventional standard errors would give ROA p = .0007, while the firm-clustered CR1, pairs-bootstrap and wild procedures do not establish a significant ROA coefficient. Influence diagnostics show concentration in one firm-year. The robust comparisons, rather than conventional significance alone, guide the main conclusion.

Overall, the reported model did not provide sufficient evidence for the hypothesized relationships between the selected proxies. Construct limitations and unquantified coding reliability constrain that inference. The estimates cannot be generalized to independently verified greenwashing or treated as proof that environmental and financial reporting practices are unrelated.

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3.5 Sensitivity of the panel results

Adding 2023 and 2024 indicators, with 2022 as the reference, left the main slopes nearly unchanged. Their joint firm-clustered Wald test was $F(2,39) = 0.154$, $p = .858$. The interaction estimate was 8.09, with wild-bootstrap $p = .154$. The Friedman repeated-rank test across years yielded $\chi^2(2) = 9.80$, $p = .007$; this concerns within-firm rank differences and is not a test of the year coefficients in the conditional mean regression. Changing medians and non-significant year indicators therefore address different questions.

Firm fixed effects produced an interaction estimate of 12.46, with wild-bootstrap $p = .153$. The disclosure, ROA and age coefficients were also non-significant under that inference. With three years, within-firm information is limited, and firm age enters the fixed-effects model through its time trend. These estimates provide a comparison in light of the Mundlak rejection; they do not resolve the exogeneity of the primary random-effects specification.

Panel influence is concentrated. Eight observations exceed the Cook's D screening value $4/120 = .0333$, four have absolute studentized residuals above 3, and seven have absolute robust z-scores above 3.5. The newly incorporated entity's 2022 observation has Cook's $D = 3.112$, compared with .331 for the next largest value. Leave-one-firm-out interaction estimates range from approximately 5.13 to 11.95. These diagnostics concern the second-stage panel model, not the unreported first-stage MJM leverage distribution.

The interaction is significant in four exploratory specifications: winsorizing DA and ROA at the 1st/99th percentiles ($b = 9.31$, $p = .040$), winsorizing DA at the 5th/95th percentiles ($b = 5.92$, $p = .028$), winsorizing both at those percentiles ($b = 5.68$, $p = .035$), and excluding four firms with absolute studentized residuals above 3 ($b = 8.53$, $p = .042$). Winsorizing only DA at the 1st/99th percentiles gives $p = .064$, while excluding only the most influential firm gives $p = .233$. Age is also significant under DA-only 5th/95th winsorization ($p = .016$). These results show specification sensitivity, rather than stable confirmatory support for H2.

The trimming rules use observed outcomes and several specifications were examined. Their isolated p-values are therefore interpreted exploratorily, with attention to selection and multiple-testing concerns. In addition, the year-indicator model retains detectable cross-firm residual dependence under the permutation CD test ($p = .0279$). Firm-level resampling addresses dependence within firms and does not by itself accommodate common errors across firms.

Table 8. Sensitivity to year indicators, firm fixed effects and extreme observations

Specification	N / firms	GW b (p)	ROA b (p)	GW × ROA b (p)	R^2
Main random effects	120 / 40	−0.023 (.933)	−0.426 (.369)	8.04 (.135)	.228
Add year indicators	120 / 40	−0.023 (.932)	−0.420 (.362)	8.09 (.154)	.229
Firm fixed effects	120 / 40	−0.711 (.594)	−0.504 (.295)	12.46 (.153)	—
DA winsorized at 1st/99th percentiles	120 / 40	0.050 (.802)	−0.257 (.436)	9.15 (.064)	.190
DA and ROA winsorized at 1st/99th	120 / 40	0.081 (.707)	−0.216 (.482)	9.31 (.040)	.093
DA winsorized at 5th/95th	120 / 40	0.135 (.240)	0.001 (.983)	5.92 (.028)	.106
DA and ROA winsorized at 5th/95th	120 / 40	0.141 (.239)	−0.040 (.715)	5.68 (.035)	.065
Most influential firm excluded; main DA retained	117 / 39	0.103 (.762)	−0.063 (.779)	6.67 (.233)	.040
Four firms with |studentized residual| > 3 excluded	108 / 36	0.271 (.052)	−0.006 (.959)	8.53 (.042)	.155

Note. Cells report coefficients and wild-bootstrap p-values in parentheses. Age is included in every specification. Winsorization uses pooled percentiles; exclusions remove complete firms. The 117-observation exclusion row retains the main DA series and differs from the re-estimated-DA benchmark in the next table. The table reports exploratory comparisons; individual p-values are not adjusted for the multiple specifications examined.

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3.6 Lagged profitability sensitivity analysis

ROA was lagged one year to reduce contemporaneous overlap with accrual-based DA: 2022 uses 2021 ROA, 2023 uses 2022 ROA, and 2024 uses 2023 ROA. The entity lacking the opening statements needed for 2021 ROA was excluded. DA was re-estimated on the remaining 39 firms, giving 117 firm-years. A contemporaneous-ROA benchmark uses these same observations and DA, so that the lag comparison does not combine a timing change with a sample or dependent-variable change.

The contemporaneous benchmark interaction was 6.33 (wild p = .259), compared with 0.77 (wild p = .748) under lagged ROA. Lagged ROA itself was 0.046 (p = .646), and the lagged-model R^2 was .002. The DA–profitability correlation was −.450 in the 120-observation main model, −.104 in the 117-observation contemporaneous benchmark and .007 with lagged ROA. These changes show sensitivity to the influential entity, sample and timing; they do not establish the absence of accounting overlap or endogeneity.

The lagged specification has estimated firm variance of zero and is numerically equivalent to pooled OLS. Its Hausman p-value is .059 and Mundlak p-value .064. Permutation CD detects cross-firm dependence (p = .0076), which remains after adding year indicators (p = .024). Consequently, its firm-clustered bootstrap inference requires caution. The high 2021 ROA value of .9146 also warrants reconciliation with the source statements; excluding that firm gives an interaction of approximately .55 with wild p = .859 (114 observations, 38 firms). This exclusion check does not validate the underlying ROA input.

Table 9. Contemporaneous and lagged profitability using comparable samples

Quantity	Main contemporaneous ROA	Contemporaneous ROA on 39 firms	Lagged ROA on 39 firms
Disclosure score b (wild p)	−0.023 (.933)	0.095 (.775)	0.066 (.747)
ROA or lagged ROA b (wild p)	−0.426 (.369)	−0.031 (.887)	0.046 (.646)
Firm age b (wild p)	0.00008 (.864)	0.00011 (.635)	−0.00009 (.900)
Interaction b (wild p)	8.04 (.135)	6.33 (.259)	0.77 (.748)
Interaction 95% pairs-bootstrap CI	−4.61 to 18.45	−5.58 to 15.11	−4.15 to 13.41
Pearson r of DA and profitability used	−.450	−.104	.007
R^2	.228	.031	.002
N / firms	120 / 40	117 / 39	117 / 39

Note. Coefficient p-values are restricted wild-bootstrap values. The two 39-firm columns use the same re-estimated DA and the same 117 firm-years. GW retains the study-defined disclosure-score interpretation. A lagged predictor reduces contemporaneous overlap but does not establish causal identification. Cross-firm dependence remains a limitation of the lagged specification.

3.7 Precision of the moderation test

The main interaction ranges from −.01675 to .01264, with standard deviation .00297. Its estimate is 8.044, with a CR1 95% t interval of −2.22 to 18.31 and a pairs-bootstrap percentile interval of −4.61 to 18.45. Both include zero and positive values of appreciable size. These intervals describe uncertainty on the study's measurement scale and must not be read as percentages.

Using the observed CR1 standard error, the approximate minimum detectable interaction at 80% power and a two-sided 5% level is 14.58 raw units, or .327 after standardizing by SD(interaction)/SD(DA). The corresponding pairs-bootstrap values are 16.68 and .374. At a standardized benchmark of .20, estimated power is 40.4% using CR1 and 32.3% using the pairs-bootstrap SE. The lagged-model standardized thresholds are approximately .39 and .73, respectively. These calculations indicate limited sensitivity to smaller effects; they do not show that a non-significant coefficient rules out a large effect, nor do they constitute equivalence testing.

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Table 10. Observed interaction range and approximate detection sensitivity

Quantity	Main-model value
Centered interaction range; SD	−0.01675 to 0.01264; 0.00297
Interaction coefficient; CR1 SE; pairs-bootstrap SE	8.044; 5.074; 5.806
95% CR1 t interval; pairs-bootstrap percentile interval	−2.22 to 18.31; −4.61 to 18.45
Approximate MDE at 80% power using CR1 SE	14.58 raw units; 0.327 standardized
Approximate MDE at 80% power using pairs-bootstrap SE	16.68 raw units; 0.374 standardized
Power at standardized interaction 0.1 / 0.2 / 0.3 / 0.5 using CR1 SE	13.8% / 40.4% / 73.0% / 99.0%

Note. MDE is approximately (t.975,G−1 + t.80,G−1) × SE. Standardized effects scale the coefficient by SD(interaction)/SD(DA). Benchmark power uses a noncentral t reference. These calculations condition on observed standard errors and are not an equivalence test. Statistical detection at a chosen effect size is not guaranteed in an individual sample.

4 Conclusion and Recommendations

The sampled firms' reports contained varying numbers of items classified as symbolic during 2022–2024. Because the gap equals that count divided by 70, the findings concern a feature of published disclosure rather than a verified talk–walk discrepancy. Discretionary accruals also varied, with greater dispersion in 2022. Neither proxy establishes deliberate greenwashing or accounting manipulation.

The model did not support a positive relationship between the disclosure score and discretionary accruals at mean profitability (H1), or a strengthening interaction with profitability (H2). Profitability and firm age were also statistically non-significant, and the reported R^2 was .228. These findings are limited to the selected measures and specification; they do not demonstrate independence or the absence of a meaningful relationship between actual greenwashing and earnings management.

The corrected DA means are close to zero and do not follow the downward pattern of the original calculation. H1 and H2 remain unsupported in the primary full-sample model. Some outcome-based winsorization and exclusion choices produce a significant positive interaction, so the moderation conclusion is sensitive to influential observations and specification. The lagged-ROA comparison also provides no significant interaction, with the additional limitation of detected cross-firm dependence.

Companies can improve environmental reporting by stating reporting boundaries, presenting comparable measurements and progress against targets, and distinguishing plans from implemented activities. Independent assurance can add evidence within its stated scope. Future research should compare disclosure with independent environmental outcomes, examine non-disclosure and alternative item weights, and extend the sector and time coverage. Such work would assess the broader construct that the present symbolic disclosure score cannot establish.

The EDI is based on corporate reports and researcher-assigned categories. The symbolic-item count has not been validated against independent environmental performance or verified misrepresentation, and its equal item weights do not capture environmental materiality. Substantive classification denotes sufficient reported detail rather than independently verified implementation or favorable outcomes. Coding reproducibility remains unquantified, so cross-review cannot support a claim of statistically demonstrated inter-rater reliability. The observational design, accrual-model limitations, purposive sample, short period, and potentially omitted firm characteristics further constrain interpretation.

The per-year MJM uses only 40 firms, and complete first-stage coefficient, fit, leverage, and alternative-estimation evidence is needed to establish its stability. The panel bootstrap conditions on the calculated DA series and does not propagate uncertainty from re-estimating the first-stage accrual model in each resample. The model includes firm age as its sole control; size, leverage, growth, and governance were not incorporated. Their omission can affect identification and is not resolved by the short panel or by resampling. Cash-based profitability, performance-matched accruals and real earnings-management measures were not tested.

Estimated interaction uncertainty remains substantial. The design-sensitivity calculations do not support treating non-significance as equivalence. The Mundlak rejection in the main sample, cross-firm dependence in sensitivity models, and the flagged high 2021 ROA input constrain inference. Future work should reconcile unusual inputs, retain reproducible estimation code and first-stage outputs, and assess dependence and omitted influences with longer panels.

A subsequent reliability assessment should use independent item-level ratings recorded before reconciliation, identify the coders and common subset of firm-years, and report category frequencies, disagreement patterns, an appropriate chance-corrected agreement coefficient, and uncertainty. Ordinal Krippendorff's alpha can accommodate multiple coders with overlapping ratings; weighted Cohen's kappa is appropriate for a defined pair. Aggregate-score ICC, if reported, would answer a different question from agreement on the 0–2 categories. Where independent original ratings are unavailable, a new coding exercise must be identified as a subsequent assessment.

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Conflict of Interest

The authors declare no conflicts of interest regarding the research, authorship, and publication of this article.

Author Contributions

M.A.M.: Theoretical framework development, investigation, data curation, validation, interpretation, and writing—original draft and review and editing. K.N.: Statistical methodology and earnings management measurement, investigation, data curation, validation, interpretation, and writing—original draft and review and editing. C.G.N.: Theoretical framework development, investigation, data curation, validation, interpretation, and writing—original draft and review and editing. A.F.O.: Conceptualization, methodology, development and operationalization of the potential greenwashing measure, investigation, data curation, validation, interpretation, writing—original draft and review and editing, and journal correspondence. J.V.: Conceptualization, methodology, development and operationalization of the earnings management measure, investigation, data curation, validation, interpretation, and writing—original draft and review and editing. M.M.E.: Supervision and research guidance as thesis adviser.

The five student authors participated in the manual coding and cross-checking of firm-year reports, contributed to the literature review and conclusions, and reviewed and approved the final manuscript. A separate statistician assisted with the statistical analysis.

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Artificial Intelligence (AI) Use Statement

For grammar editing, the authors used Grammarly; for coding assistance, Copilot; and for literature discovery, Consensus. The authors used ChatGPT to help restructure and edit the manuscript language to suit the journal format. However, the authors remain responsible for checking the manuscript's accuracy, citations, originality, and final content.

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Appendix A Environmental Disclosure Coding Rules

Each firm-year indicator receives one score. Score 0 applies when indicator-specific information is absent, unrelated, blank, or unsupported. Score 1 applies when a relevant statement is present but does not meet the substantive condition below. Score 2 requires the listed indicator-specific evidence. The rules concern disclosed information and do not independently establish its truth or environmental effectiveness.

Table A1 Indicator-specific evidence required for score 2

Indicator	Substantive evidence required
A1 Environmental policy statement	A formal policy with objectives, principles, responsibilities, or environmental content.
A2 Environmental management system	An explicit formal system with scope, monitoring, audits, responsibilities, or corrective action.
A3 Environmental management certification	An achieved ISO 14001 or equivalent environmental management certification; a building certification is different.
A4 Board oversight of environmental issues	An explicit board environmental responsibility, committee, mandate, or regular environmental review.
A5 SDG alignment	Specific Sustainable Development Goals mapped to operations, programs, or outcomes.
B1 Energy consumption	An actual energy quantity; a saving percentage alone does not establish consumption.
B2 Renewable energy commitment and usage	Actual renewable use or operational capacity; an explicitly measured zero quantity can be disclosed.
B3 GHG emissions	An explicitly quantified emissions inventory or scope disclosure.
B4 Emission or carbon reduction targets	A specific measurable target or identified target framework; achievement is not implied.
B5 Energy efficiency initiatives	Specific implemented efficiency measures or technologies.
B6 Energy intensity	An explicitly reported intensity metric; coders do not calculate a missing intensity.
B7 Climate-related disclosure	An explicit climate disclosure structure covering governance, strategy, risk management, and metrics or targets.
C1 Water consumption	Actual withdrawal or consumption quantities; savings are not equivalent to consumption.
C2 Water recycling or reuse	Actual reuse or an explicit measured reuse quantity; treatment alone is insufficient.
C3 Water risk disclosure	Specific water-risk assessment or evaluated exposure.
C4 Water conservation policies or programs	Implemented conservation systems, measures, or programs.
C5 Water intensity	An explicitly reported normalized water metric; no metric is inferred from separate quantities.
D1 Waste generation	An actual waste quantity.
D2 Waste reduction and recycling	Implemented recycling, diversion, composting, or waste-reduction systems or outcomes.
D3 Pollution control measures	Specific implemented pollution-prevention, treatment, monitoring, or control measures.

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Indicator	Substantive evidence required
D4 Hazardous waste	An explicit hazardous-waste quantity or measured disposal information, including an explicit zero.
E1 Land-use impacts	Quantified impacts or a site-specific assessment; property area alone is insufficient.
E2 Biodiversity protection initiatives	Identifiable implemented conservation, restoration, protection, or ecosystem initiatives with results.
E3 Sustainable land-use policies	An explicit land-use framework, development exclusion, screening rule, or sustainable site-selection policy.
F1 Environmental fines or penalties	Explicit fines, sanctions, cases, amounts, or measured nil outcomes; blank entries are different.
F2 Regulatory compliance	Identified environmental regulations, regulators, permits, standards, audits, or compliance mechanisms.
F3 Litigation or environmental legal risk	Specific environmental legal or regulatory exposure; general climate discussion is insufficient.
G1 Environmental expenditures	An explicitly attributed environmental expenditure amount.
G2 Environmental investments	Implemented environmental technologies, infrastructure, or assets; future investment plans are different.
G3 Cost savings from sustainability	Quantified financial benefits explicitly tied to environmental activities; energy savings do not imply monetary savings.
H1 Green-building certifications	An achieved identified building certification such as LEED, BERDE, or EDGE.
H2 Sustainable construction	Implemented sustainable materials, methods, or construction technologies.
H3 Climate-resilient infrastructure	Implemented physical adaptation or resilience systems; studies and planning alone do not establish infrastructure.
H4 Tenant sustainability	Structured tenant environmental programs, monitoring, campaigns, education, or engagement.
H5 Green leasing	Actual sustainability lease clauses or provisions; a reported zero share of green leases does not establish their existence.

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Table A2 Illustrative applications of the coding rules

The examples are hypothetical applications of the stated rubric, not quotations or observations from sampled companies and not evidence of a coder-training exercise. Score 2 denotes sufficient disclosed detail under the relevant item rule; it does not establish that a claim is accurate, independently assured, or environmentally favorable.

Indicator	Score 0	Score 1	Score 2
B1 Energy consumption	No attributable energy-consumption disclosure.	A general commitment to reduce energy use, without a consumption quantity.	An explicit reporting-year electricity consumption figure, such as 1,200 MWh, within the stated boundary.
A1 Environmental policy	No attributable environmental policy information.	A general pledge to protect the environment.	A formal policy specifying environmental objectives and responsibilities; a numerical outcome is not required.
A3 Management certification	Only a green-building certificate, with no environmental management certification information.	A stated intention to obtain environmental management certification.	An achieved ISO 14001 or equivalent environmental management certification within the reporting boundary.
F1 Environmental fines	A blank fines field or no attributable fines information.	A general claim of environmental compliance without specific fines or nil-outcome information.	An explicit reporting-year statement of environmental fines of PHP 0, or a specified fines amount.
H5 Green leasing	Only tenant environmental engagement, with no lease-related disclosure.	A stated intention to introduce green leasing.	An actual lease provision requiring tenant environmental action, such as reporting electricity use.