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Capital Structure and Environmental Performance: International Evidence with Firm Fixed Effects and Endogeneity Safeguards

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ABSTRACT

This study examines the relationship between corporate leverage and environmental performance in a broad international panel, testing whether the relationship is non-monotonic and applying sequential safeguards against reverse causality and omitted-variable bias. The sample comprises 90,979 firm-year observations from 11,648 firms across 98 countries and 30 industries (2005-2024), drawn from the LSEG (Refinitiv) ESG and financial databases. The emissions score (0-100) is regressed on leverage (debt/equity) using pooled OLS with industry, country and year fixed effects and a squared-leverage term for nonlinearity; lagged leverage and two-stage least squares address reverse causality, and firm fixed effects with profitability/tangibility controls provide further robustness, with errors clustered by firm. Leverage is negatively associated with the emissions score in every specification (coefficients −0.21-−0.50), but significance is sensitive to the controls: significant at 1% pooled, marginal at 10% with full fixed effects, and insignificant (P = 0.130) once profitability and tangibility join those effects; a marginal association persists with firm and year effects (P = 0.071). The squared-leverage, lagged-leverage and 2SLS terms are never significant. The study pairs a large international sample with layered endogeneity safeguards rarely reported together in this literature, showing how fragile the leverage-emissions link becomes once each safeguard applies.

Keywords: Capital Structure, Leverage, Environmental Performance, Firm Fixed Effects, Endogeneity, Panel Data
JEL Classifications: G32, Q56, M14, C23

1. INTRODUCTION

Environmental investments include emissions-abatement technology, cleaner production processes, and formal environmental management systems within the firm. Such expenditures are often optional and have delayed returns, making them likely targets for cuts during financial constraints. Companies with higher debt levels experience more intense scrutiny from creditors, face greater debt repayment demands, and have less financial flexibility. These factors can lead to prioritizing debt obligations over environmental investments (Modi and Cantor, 2021). However, the empirical link between leverage and environmental performance is more debated than the logic of financial slack. Some research indicates that strong environmental performance can influence corporate financing decisions, as firms with good environmental practices find it easier to secure credit and, thus, opt for lower debt levels (Benlemlih and Cai, 2020). Conversely, other studies suggest that leverage and environmentally responsible behavior are correlated, especially when creditors consider CSR reporting (Malik et al., 2026).

This study evaluates the relationship between leverage and emissions by utilizing an international panel of firm-year data, incorporating several measures to address reverse causality and omitted variables. The analysis is based on 90,979 firm-year records from 11,648 companies located in 98 countries. These companies are categorized into 30 TRBC business sectors and monitored from 2005 to 2024. The firm's emissions score, sourced from the LSEG (Refinitiv) ESG database (Ehlers et al., 2024), serves as the dependent variable. This score, ranging from 0 to 100, reflects a company's dedication and success in minimizing emissions in its

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production and operations. The primary independent variable is leverage, defined as the ratio of total debt to total shareholders' equity. The baseline model accounts for firm size, ownership dispersion as indicated by free float, and market capitalization. It also incorporates fixed effects for industry, country, and year, along with standard errors clustered by firm (Petersen, 2009). Following feedback from a pre-submission review, the study includes two additional robustness checks. These incorporate firm fixed effects and controls for profitability and asset tangibility (Sections 3.5 and 4.3).

The paper makes three contributions, each recalibrated below against the closest comparators identified in a pre-submission literature audit. First, it estimates the leverage--environmental performance relationship on a substantially larger and more geographically diverse panel. Most prior studies in this literature are single-country or single-region, clustering heavily in the United States, Europe, and China. This scale advantage holds up against the comparator set assembled for this revision of the paper. That comparator set remains dominated by single-country or single-region studies of the same relationship. Second, it tests for the nonlinearity implied by two competing theoretical accounts of corporate debt. These accounts contrast a disciplining effect of moderate debt with a crowding-out effect of high debt. The test is implemented by including a squared leverage term in the baseline regression model. This test is not, however, unprecedented within the existing multi-country literature on leverage and sustainability. Saona et al. (2026) test essentially the same nonlinear leverage--environmental-outcome relationship in a multi-country panel. They report an inverted-U pattern in environmental-impact-adjusted firm value, a close precedent for the present test. This study's contribution is therefore better framed as a direct but differently specified replication attempt. It uses the Emissions Score rather than an impact-adjusted valuation outcome as the dependent variable. The null finding reported below remains a distinct and reportable result relative to Saona et al.'s (2026) positive nonlinearity. Third, the study treats endogeneity and omitted-variable concerns as a first-order empirical problem rather than an afterthought. It implements both lagged-leverage and two-stage-least-squares specifications alongside the baseline fixed-effects models estimated in the paper. This revision further adds firm fixed effects and controls for both profitability and asset tangibility. These address reviewer concerns about unobserved firm heterogeneity and omitted determinants of leverage and environmental performance. This safeguard set is broader than most single-country studies in this literature but not the deepest available. Ardianto et al. (2024) study carbon disclosure and capital structure using a much larger toolkit. They deploy Heckman two-stage correction, propensity-score matching, entropy balancing, and two-step GMM alongside their baseline model. That represents a substantially larger endogeneity toolkit than the one deployed in the present study. This study's contribution on that dimension is therefore breadth of sample rather than superior identification depth. It also offers transparent, staged reporting of how the estimate changes as each safeguard is added.

The findings indicate a negative correlation between leverage and environmental performance across all estimated specifications. The sign remains consistent, with coefficients ranging from −0.21 to −0.50 in the models. However, the statistical significance varies depending on the specification and control set used. In a pooled model without fixed effects, the association is significant at the 1% level. When industry, country, and year fixed effects are included, it becomes marginally significant at the 10% level. This specification also uses standard errors clustered at the firm level throughout the estimation panel. The association loses significance when profitability and asset tangibility are added to the fixed-effects structure (P = 0.130). It becomes marginally significant again with firm and year fixed effects and the same controls (P = 0.071). The estimate does not reach conventional significance when lagged-leverage and instrumental-variable safeguards are applied, regardless of whether profitability and tangibility are included in the regression model. There is no evidence supporting the hypothesized nonlinearity, as the squared leverage term is statistically indistinguishable from zero. The remainder of the paper is structured as follows: Section 2 reviews the relevant literature and formulates the hypotheses. Section 3 details the data, variable construction, and model specification, including the omitted-variable and firm-fixed-effects safeguards added in this revision. Section 4 provides descriptive statistics and regression results, while Section 5 discusses these findings in the context of the reviewed literature. Section 6 concludes, and Section 7 outlines the limitations and future research directions.

2. LITERATURE REVIEW AND HYPOTHESIS DEVELOPMENT

2.1. Theoretical Framework

Two theoretical frameworks underpin this research, each suggesting a different outcome for the relationship between leverage and environmental performance. The financial slack and free cash flow perspective, which is based on the agency cost theory proposed by Jensen in 1986, argues that spending on non-essential activities, such as environmental projects, requires available financial resources. When leverage is high, this financial slack is reduced, prioritizing debt repayment over environmental investments. Additionally, increased creditor oversight in situations of high leverage can limit managerial freedom in deciding which projects to fund. Historically, creditors have had less motivation than some equity holders to directly incentivize environmental performance. This is because a creditor's claim on the firm is fixed rather than residual in nature (Fama and Jensen, 1983). Under this account, leverage should be negatively associated with environmental performance across firms in the sample. This effect should be most visible once firm size and market-based confounds are held constant. Larger, more visible firms both borrow more and face greater disclosure pressure independent of financial slack. A closely related account is grounded in the pecking-order and slack-resources theory rather than agency theory. It holds that profitability, not leverage alone, is the more direct determinant of discretionary spending capacity. Profitable firms generate internal funds, borrow less by pecking-order logic, and retain more slack for environmental investment (Myers, 1984). Because profitability and leverage are jointly determined by internal-financing capacity, the two effects can easily be conflated. Omitting profitability from

a leverage--environmental-performance specification, therefore, risks attributing a profitability effect to leverage itself. Section 3.5 addresses this by adding a profitability control to the baseline and robustness specifications.

An alternative signaling account holds that strong environmental performance signals lower operational and regulatory risk to capital markets. Such firms are rewarded with a lower cost of debt and easier access to credit. The causal relationship may therefore run from environmental performance to capital structure rather than the reverse (Benlemlih and Cai, 2020; Kordsachia, 2021; Tawfiq et al., 2024). Under this account, a positive raw association between environmental performance and leverage remains entirely possible. Good environmental performers can sustain more debt at any given cost of external borrowing. Such an association is compatible with a negative underlying causal effect of leverage on environmental investment. The two forces work in opposite directions, so the net sign in any sample remains an empirical question. Another perspective, which is more dependent on institutional factors, examines how policy influences credit access. In certain contexts, access to credit is directly influenced by creditors, regulators, or the state based on environmental behavior. China's green credit policy exemplifies this type of institutional framework in action. Here, policy design can create a positive relationship between leverage and environmental performance, rather than relying on the previously mentioned channels (Sun et al., 2019). A related perspective on legitimacy suggests that firms with high leverage are subject to increased scrutiny from the public and creditors. These firms might strategically engage in social and environmental activities to mitigate the legitimacy costs associated with leverage. This perspective anticipates a positive correlation between leverage and performance under specific institutional conditions (Huang et al., 2025). Similarly, Malik et al. (2026) report that in India, enhanced ESG disclosure influences financing structure outcomes. These outcomes align with the reverse causal channel discussed earlier in this section (Malik et al., 2026).

Related literature on corporate financialization often presents this tension as a dichotomy. In the "reservoir effect," borrowing capacity is conserved and later redirected towards environmental investments. Conversely, the "crowding-out effect" suggests that financial resources are permanently shifted away from such investments. Research by Tao et al. (2021) indicates that financialization displaces environmental investment in non-state and small-scale Chinese enterprises. However, this financialization serves as a reservoir when firms encounter fewer stringent financing constraints. Similarly, Shi and Huang (2024) observe a crowding-out effect in their study of Chinese firms. This effect shifts toward a reservoir effect only in regions with strong regulatory and marketization institutions. Evidence of an analogous crowding-out channel outside this literature reinforces the mechanism at the sub-national level. Chen et al. (2022) show that local government debt crowds out corporate green innovation through credit constraints. Li et al. (2024) similarly document that expanding local government debt is associated with weaker enterprise ESG performance. Both findings are consistent with debt-driven crowding out operating through credit-supply conditions rather than firm-level leverage. The reservoir and crowding-out distinction offers a more precise vocabulary than "financial slack" alone for the mechanism tested here. H_1 tests, in effect, whether leverage operates as a crowding-out force on environmental performance in this sample. Country fixed effects net out the institutional variation that Shi and Huang (2024) show can flip the sign.

The financial-slack account also receives causal support from research designed around exogenous shocks to financial constraints. Xu and Kim (2020) used quasi-natural experiments affecting the financial resources of U.S. public firms. In the Review of Financial Studies, the authors demonstrate that easing financial constraints leads to a causal reduction in toxic emissions. This emissions reduction is more pronounced in environments with weak regulatory enforcement and limited external monitoring (Xu and Kim, 2020). Their approach provides a direct causal counterpart to the correlational financial-slack mechanism explored in this paper. Rehman et al. (2023) apply this reasoning specifically to firm-level leverage in the United States, discovering that financial constraints increase carbon emissions, particularly in firms with high leverage. This observation directly supports the interaction between financing constraints and firm leverage posited in H_1 (Rehman et al., 2023). De Haas et al. (2024) investigate 10,776 firms across 22 emerging-market countries, finding that strict credit constraints hindered corporate investment in green technology throughout the sample. They also analyze local industrial emissions outcomes using bank-deleveraging shocks following the Global Financial Crisis. Areas where banks had to deleverage more showed consistently higher carbon emissions a decade later, indicating that the financial-slack channel examined in H_1 functions through both credit supply and leverage.

Two further considerations motivate the model specification and the two hypotheses developed in this study. First, moderate debt may discipline managers toward operating efficiency, a classical corporate-governance role of debt. If only high debt crowds out discretionary spending, then the leverage--environmental-performance relationship could be non-monotonic. It would be positive or flat at low-to-moderate leverage and negative only beyond some threshold. This possibility has direct precedent in the general capital-structure and performance literature outside environmental research (Ayaz et al., 2021). Several recent panel-threshold studies document a switch from positive to negative leverage effects beyond an optimal debt level (Cheng et al., 2010; Omri et al., 2024). This is precisely the same disciplining-then-crowding-out logic that H_2 tests for the environmental-performance outcome in this study. Saona et al. (2026) report a directly comparable inverted-U pattern for leverage and firm value. Their measure is environmental-impact-adjusted firm value, and the peak falls at roughly 58-61% of total assets (Saona et al., 2026). This study remains the closest precedent for H_2 identified in the literature to date. Second, the theoretical literature itself remains genuinely bidirectional on the underlying question of causal direction. Any contemporaneous, single-equation specification therefore risks conflating two distinct effects that run in opposite directions. The crowding-out effect runs from leverage to environmental performance, and the signaling effect runs back.

This motivates the lagged-leverage and instrumental-variable safeguards that are implemented in Section 3.4. It also motivates the firm fixed-effects and profitability and tangibility safeguards implemented in Section 3.5.

2.2. Empirical Review

The empirical literature is genuinely bidirectional in its focus, and this pattern is itself informative. Research that views capital structure as a result of environmental performance, instead of its cause, holds significant importance. Benlemlih and Cai (2020) investigate the financial strategies of U.S. companies alongside their environmental performance records. Their findings indicate that companies with outstanding environmental performance tend to have considerably lower debt ratios compared to their counterparts. These companies primarily depend on short-term debt to address temporary financial needs as they occur. This aligns with the notion that responsible companies have improved access to financing and utilize it more prudently. Kordsachia (2021) reports a negative correlation between CSR performance and the marginal cost of debt among European companies. This CSR attribute, akin to insurance, is particularly evident in firms already experiencing financial difficulties. The author views this outcome from a risk-management perspective rather than solely as a signaling mechanism Carey et al. (2021). Carey et al. (2021) demonstrate that voluntary sustainability assurance alleviates capital constraints and reduces the cost of debt. This association is stronger when the assurance is provided by an accounting firm rather than another provider. Two further studies reinforce this reverse causal channel with more recent evidence from other markets. Tawfiq et al. (2024) find that stronger environmental, social, and governance disclosure reliably predicts lower corporate leverage levels. Malik et al. (2026) similarly find, for Indian firms, that ESG disclosure is associated with lower leverage. Their evidence also points to altered debt-equity dynamics among the Indian firms they examine in that market. Frost et al. (2023) literature review situates this ESG-disclosure-to-financing channel squarely within the accounting literature. It provides a useful anchor for readers approaching this study from an accounting background rather than finance. Taken together, this cluster of studies clearly supports the signaling account described in the preceding section. Any negative effect estimated in a single cross-section or short panel is therefore likely to be attenuated. The reverse channel may even reverse the sign of the estimated leverage effect entirely in some samples.

The second cluster examines the capital structure as a consequence of ESG engagement, with mixed findings. Adeneye et al. (2023) show that ESG performance affects the level of leverage. ESG performance also affects the speed of adjustment toward target capital structures over time. Sustainability performance is therefore a first-order determinant of financing dynamics rather than a background control. Yang et al. (2018) work with Chinese listed firms and reach contrasting conclusions. In contrast to the U.S. and European evidence above, CSR reporters carry higher leverage than others. CSR disclosure also slows the speed of capital structure adjustment among these Chinese listed firms. The authors attribute this pattern to CSR reporting reducing information asymmetry between firms and creditors. Lower information asymmetry supports rather than constrains the debt capacity available to these reporting firms. Gong et al. (2021) and Xu et al. (2021) document similar Chinese evidence. Stronger CSR conduct and mandatory CSR disclosure are associated with a lower cost of debt. In a setting different from the European context previously discussed, this highlights the issue of reverse causality. According to Chan et al. (2017), companies facing financial limitations participate significantly less in CSR activities. This observation aligns with either direction of causality between financial constraints and environmental involvement, emphasizing that these factors are interdependent rather than one being the sole cause of the other. Oware and Mallikarjunappa (2019; 2021) demonstrate with Indian data that leverage influences the relationship between CSR and performance. Leverage itself is linked to debt-financing patterns that are intricately connected to CSR investments, further indicating a two-way, institutionally contingent relationship rather than one dominant causal channel. Arisyahidin et al. (2025) are the most directly relevant to the design adopted in this study. They test the capital-structure-to-sustainability-performance relationship on a sample of 921 firms across 11 Asian countries. Their panel covers 2,763 firm-years between 2020 and 2024, which is similar to the design used here. They report a positive association between leverage and sustainability performance, which is the opposite of H_1. This study was treated as a direct, opposite-signed comparator rather than something to be reconciled. The Discussion (Section 5) and Conclusion (Section 6) return to how these findings sit alongside the results.

A third cluster links leverage and financial slack to environmental performance through an operations- and supply-chain lens. Modi and Cantor (2021) show that financial slack, leverage, and leanness shape how coopetition translates into environmental performance. Their evidence directly supports the financial-slack channel emphasized earlier in Section 2.1 of this paper. Firms with less slack, including highly leveraged firms, convert fewer competitive opportunities into environmental improvements. Sun et al. (2019) examine the green credit policy currently operating in China. This policy requires banks to condition lending and hence access to leverage on corporate environmental performance. They find that the policy produces measurable environmental improvements among affected Chinese borrowing firms. Policy design can therefore reverse the sign of the leverage--environmental-performance relationship by making credit access conditional on environmental conduct. Earlier foundational work established the general empirical basis for linking environmental and financial performance without a capital-structure focus. Carter et al. (2000) provide early evidence that environmental purchasing practices are associated with firm performance. Delmas and Toffel (2004) offer an institutional framework for why firms adopt environmental management practices. Their framework extends beyond what regulatory or financial pressure alone would predict about adoption. Delmas et al. (2015) examine the temporal dynamics between environmental and financial performance for greenhouse gas emissions. This construct is closely related to the Emissions Score used as the dependent variable here.

The fourth cluster is concentrated in country-specific settings and has been published since 2021. These studies targeted the

causal direction from environmental performance to leverage. This causal direction motivates the empirical endogeneity safeguards implemented in this study's analysis. Kalash (2021) used carbon disclosure data from Turkish listed firms to examine this relationship. The study finds that environmental performance significantly and positively affects corporate equity returns. This is the opposite of the financial slack prediction developed in Section 2.1. This is consistent with lenders rewarding disclosed environmental performance with greater debt capacity. Dsouza et al. (2025) use a large, recent EU panel of listed firms covering 2010-2024. They reported a statistically significant negative correlation between firm leverage and CO_2-equivalent emissions levels. High-emission firms carry less debt, and this relationship is most pronounced among the largest firms. These firms are also more exposed to regulatory and reputational pressures in their markets. This closely parallels the reverse causality channel that the lagged leverage and 2SLS specifications are designed to net. Ho et al. (2021) used a 31-country panel from 2002 to 2018. This panel is comparable in geographic breadth to the international sample used in this research. They show that superior corporate sustainability performance is associated with a faster adjustment speed toward target leverage. The underlying channel operates through reduced information asymmetry and improved engagement with a broader set of external stakeholders. This relationship is also weaker in countries with high-quality institutions, which is a potentially important moderating effect. The country fixed effects in this study implicitly average this pattern rather than test it directly. Xiao et al. (2022) exploit the recent introduction of environmental protection tax in China. They treat this tax as an exogenous shock and show that polluting firms reduce their leverage in response. This effect is concentrated among firms that hold excess cash and among financially constrained Chinese firms. This is a clean quasi-experimental case in which environmental cost pressures directly cause deleveraging. This design offers the kind of genuinely exogenous variation that this study lacks, as discussed in Section 7.2. Two further nonlinearity-focused studies are directly related to the second hypothesis developed in this paper. Saona et al. (2026) document an inverted U-shaped relationship between leverage and environmental outcomes. Li et al. (2024) document a mirror-image U-shaped relationship between ESG and debt structure. They study the long-term debt structure of Chinese listed firms using a two-stage least squares design. Nonlinearity remains a live and contested question on both sides of causal arrows. Finally, Ardianto et al. (2024) study corporate carbon emission disclosure and capital structure from the identification perspective. They deployed a deeper endogeneity toolkit: Heckman correction, entropy balancing, propensity score matching, and two-step system GMM. Their work is discussed in Sections 3.5 and 7.1 as the closest identification benchmark.

This body of evidence justifies treating the leverage--emissions relationship as potentially endogenous rather than unidirectional. The motivation behind the subsequent empirical work also influences the four specific design choices. First, the relationship is evaluated with a detailed set of fixed effects and firm-clustered standard errors, which consider firm characteristics that do not change over time and macro-level factors that could otherwise distort leverage estimates. The nonlinearity implied by the tension between disciplining and crowding out, as discussed in Section 2.1, is then directly analyzed. This analysis is compared to the positive inverted-U pattern established by Saona et al. (2026). To tackle the reverse-causality issue previously mentioned in this section, lagged-leverage and instrumental-variable methods are used. The findings from Benlemlih and Cai (2020) to Malik et al. (2026) underscore the significance of this matter. Furthermore, this revision includes firm fixed effects and a control set for profitability and tangibility in Section 3.5, addressing omitted-variable concerns highlighted by Ardianto et al. (2024) more sophisticated identification toolkit. These measures also address broader issues raised by pecking-order and slack-resources theory.

2.3. Hypothesis Development

- H_1 (Financial slack/crowding-out effect): Leverage is negatively associated with environmental performance, as measured by the Emissions Score across the panel.
- H_2 (Non-monotonic leverage effect): The relationship between leverage and environmental performance is nonlinear rather than uniformly linear.

H_2 serves as an authentic examination rather than a predetermined outcome regarding the nature of the relationship. If the financial-slack channel consistently prevails throughout the leverage distribution, the squared term ought to be statistically indistinguishable from zero. The relationship would then be adequately described by the linear leverage term. A significant inverted-U relationship for a related environmental-outcome variable has already been reported elsewhere in the literature. This precedent raises the bar for what a null finding on H_2 here can credibly claim.

The literature reviewed in Sections 2.1 and 2.2 documents the possibility of reverse causation. Environmental performance may cause financing choices rather than the reverse, as several reviewed studies suggest. Profitability and firm-level heterogeneity may also confound any simple association between leverage and environmental performance. Therefore, H_1 and H_2 are tested in a baseline fixed-effects specification and four further specifications. These are a lagged-leverage specification and a two-stage least squares specification instrumenting leverage with its lag. They also include a firm fixed-effects specification and a profitability and tangibility augmented specification (Sections 3.4 and 3.5). These are treated as robustness checks on H_1 and H_2 rather than as separate hypotheses. The aim is to report how sensitive the leverage--emissions association is to each safeguard applied. The study does not select the specification that is most favorable to any directional claim.

3. DATA AND METHODOLOGY

3.1. Sample and Data Sources

The data are drawn from a firm-year panel licensed from the LSEG (Refinitiv) ESG and financial database (Bigelli et al., 2023). The raw panel, as licensed, covers 102,003 firm-year observations for the years 2002-2024. Emissions Score and financial-statement coverage in 2002-2004 is too sparse to survive the missing-data filters. Every observation retained in the final estimation sample falls between

2005 and 2024. The analysis window is accordingly described as 2005-2024 throughout the remainder of the paper. The raw panel includes firm identifiers, headquarters and exchange country, and Refinitiv TRBC industry classification. The classification covers economic sector, business sector, and industry group and name for each firm. It also includes a full set of balance-sheet and income-statement items for every firm-year observation. These items include net income and net property, plant, and equipment, among many other variables. These two items are used to construct the profitability and asset-tangibility controls added in Section 3.5. The panel further includes board and governance variables and environmental-pillar ESG category scores and disclosure indicators. Country-year macroeconomic and governance indicators are also included in the licensed raw data panel. These cover inflation, GDP growth, GDP per capita, and the six World Bank Worldwide Governance Indicators.

The estimation sample applies two separate filters to the raw panel of 102,003 firm-year observations. First, all firm-years with non-positive total shareholders' equity are dropped from the raw panel. This filter removes 2,059 observations, equal to 2.0% of the raw panel of firm-years. The leverage ratio is not economically meaningful when its denominator is zero or negative. Second, firm-years with missing values for total debt, free float, or market capitalization are dropped. Firm-years missing total assets or the Emissions Score are dropped on exactly the same basis. This second filter removes a further 8,965 observations, or 9.6% of the remaining panel sample. Total assets and market capitalization are positive for all remaining observations; therefore, no additional filter is required on those variables. The resulting estimation sample contains 90,979 firm-year observations, or 89.2% of the raw licensed panel. It covers 11,648 firms headquartered in 98 countries and classified into 30 TRBC business sectors. These firms are observed between 2005 and 2024, as already noted in the introduction. The United States (29,182 raw firm-years), China (8,263), and Japan (8,103) are most heavily represented. The United Kingdom (6,097) and Canada (4,385) follow as headquarters countries in the raw panel. Banking and investment services, real estate, and industrial goods are the most represented business sectors. Pharmaceuticals and medical research and mineral resources are the next most heavily represented TRBC sectors. The profitability and asset-tangibility controls added in Section 3.5 introduce a small amount of additional missingness. This affects a further 2,654 observations, equal to 2.9% of the 90,979-observation estimation sample. The loss is owed chiefly to missing data on net property, plant, and equipment. The augmented specifications in Tables 1 (columns 4 and 6) and 2 (columns 8 and 10) are estimated on 88,325 and on 76,228-88,325 observations, respectively, as each table notes.

3.2. Variable Definitions and Measurement

Table 3 presents the definitions and measurements of the dependent, independent, and control variables. These include the profitability and asset tangibility controls that were added in this revision (Section 3.5).

3.3. Model Specification

The baseline model is estimated using ordinary least squares with industry, country, and year fixed effects and firm-clustered standard errors:

$$\text{EmissionsScore}_{it} = \beta_0 + \beta_1 \cdot \text{Leverage}_{it} + \beta_2 \cdot \ln(\text{TotalAssets})_{it} + \beta_3 \cdot \ln(\text{FreeFloat})_{it} + \beta_4 \cdot \ln(\text{MarketCapitalization})_{it} + \text{IndustryFE} + \text{CountryFE} + \text{YearFE} + \varepsilon_{it} \quad (1)$$

where i indexes firms and t indexes fiscal years in the estimation panel. H_1 is supported if β_1 is negative and statistically distinguishable from zero. To test H_2, a squared leverage term was added to the baseline specification as follows:

$$\text{EmissionsScore}_{it} = \beta_0 + \beta_1 \cdot \text{Leverage}_{it} + \beta_2 \cdot \text{Leverage}^2_{it} + \beta_3 \cdot \ln(\text{TotalAssets})_{it} + \beta_4 \cdot \ln(\text{FreeFloat})_{it} + \beta_5 \cdot \ln(\text{MarketCapitalization})_{it} + \text{IndustryFE} + \text{CountryFE} + \text{YearFE} + \varepsilon_{it} \quad (2)$$

H_2 is supported if the coefficient β_2 is statistically significant in the squared leverage specification. The sign of β_2 relative to β_1 indicates whether the relationship is U-shaped or inverted. The shape is assessed only over the observed range of leverage within estimation samples.

3.4. Endogeneity Safeguards: Lagged Leverage and Instrumental Variables

Given the documented bidirectionality in Section 2.2, two further specifications are estimated as robustness checks. Both are designed to guard against the reverse-causality channel documented in the reviewed literature. First, a lagged-leverage specification replaces contemporaneous leverage with leverage measured in the prior year, t−1. This restricts the sample to firm-year observations with a valid, consecutive 1-year lag of leverage available. This leaves 78,374 observations, equal to 86.1% of the full estimation sample used above. A firm's environmental performance at time t cannot mechanically cause its own leverage at t−1. This specification therefore weakens, without eliminating, the reverse-causality channel emphasized by Benlemlih and Cai (2020) and Kordsachia (2021).

Second, we use two-stage least squares (2SLS) specification instruments with contemporaneous leverage and its own 1-year lag. This follows the same logic used elsewhere in the corporate finance literature to attenuate simultaneity bias (Gupta and Chaudhry, 2019). A fully exogenous instrument, such as statutory tax-shield variation, cannot be constructed from this dataset. The first-stage regression of current leverage on lagged leverage and all controls yields 0.855. The associated statistics are firm-clustered $t = 127.2$, $F = 16{,}168$, and $P < 0.001$. Therefore, the instrument is highly relevant by construction, far exceeding the conventional weak-instrument threshold of $F = 10$ (Stock and Yogo, 2005). This relevance is mechanical, because leverage is a persistent and highly autocorrelated firm characteristic over time. A fully identified causal estimate would require an exclusion restriction that cannot be tested here. The restriction is that lagged leverage affects the current Emissions Score only through current leverage. It must not operate through any other persistent firm characteristic that is correlated with both. This restriction is not separately testable with the data available for the present study. The lagged-leverage instrument is therefore best understood as a partial safeguard rather than full identification. It guards against contemporaneous reverse causality and simultaneity, consistent with how the design

Table 1: Omitted-variable and firm fixed-effects robustness checks

Variable	(4) FE+profitability/tangibility	(5) Firm FE+Year FE	(6) Firm FE+Year FE+profitability/tangibility
Leverage	−0.2140 (0.1413)	−0.2097* (0.1253)	−0.2305* (0.1279)
Profitability (ROA)	10.3189*** (1.5769)	—	−3.5746*** (1.2567)
Asset Tangibility	6.3754*** (1.2738)	—	1.2930 (1.7471)
ln (Total Assets)	7.2251*** (0.2336)	8.6014*** (0.4453)	8.6191*** (0.4540)
ln (Free Float)	2.1531*** (0.1874)	0.5929** (0.2543)	0.5525** (0.2572)
ln (Market Cap.)	2.4533*** (0.2162)	−0.3319 (0.2152)	−0.2421 (0.2231)
Industry/Country FE	Yes	Absorbed by Firm FE	Absorbed by Firm FE
Firm FE	No	Yes	Yes
Year FE	Yes	Yes	Yes
N	88,325	90,979	88,325
R^2 (within, cols 5-6)	0.442 (overall)	0.111	0.113

The dependent variable is the Emissions Score (0-100). Firm-clustered standard errors are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% levels, two-tailed. Columns (4) and (6) drop 2,654 observations relative to the 90,979-observation estimation sample owing to missing profitability/tangibility data (Section 3.1). Columns (5) and (6) are estimated using within-firm panel OLS with firm and year fixed effects; standard errors are clustered by firm across 11,648 firms, averaging 7.8 firm-years each. The constant and the firm and year fixed-effect coefficients are absorbed and not separately tabulated. A poolability F-test rejects pooling in favor of firm fixed effects (F (11,666, 79,308)=20.78, P<0.001; Section 3.5)

Table 2: Robustness checks — lagged leverage and instrumental variables

Variable	(7) Lagged leverage	(8) Lagged leverage+profitability/tangibility	(9) 2SLS	(10) 2SLS+profitability/tangibility
Leverage (t-1)/instrumented	−0.2373 (0.1613)	−0.2217 (0.1626)	−0.2775 (0.1885)	−0.2611 (0.1914)
Profitability (ROA)	—	11.4901*** (1.7476)	—	11.1543*** (1.7615)
Asset Tangibility	—	6.6144*** (1.3883)	—	6.6618*** (1.3892)
ln (Total Assets)	6.8039*** (0.2579)	7.0195*** (0.2536)	6.8278*** (0.2620)	7.0378*** (0.2570)
ln (Free Float)	2.0400*** (0.1999)	2.1728*** (0.2052)	2.0403*** (0.1998)	2.1690*** (0.2051)
ln (Market Cap.)	3.2014*** (0.2276)	2.6321*** (0.2331)	3.1791*** (0.2305)	2.6229*** (0.2340)
Industry/Country/Year FE	Yes	Yes	Yes	Yes
First-stage F-statistic (lag)	—	—	16,168***	14,530***
N	78,374	76,228	78,374	76,228
R^2	0.429	0.432	—	—

The dependent variable in every column is the firm Emissions Score. Firm-clustered standard errors are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% levels, two-tailed. Columns (9) and (10) are estimated using two-stage least squares (IV2SLS). Leverage at t-1 is the sole excluded instrument for contemporaneous firm leverage. First-stage F-statistics are computed with firm-clustered standard errors in both instrumented specifications and are reported in place of the single first-stage t-statistic used in the original design, following peer-review guidance (Section 3.4). The exclusion restriction for this instrument cannot be separately tested with the data available here; columns (9) and (10) should be read as a partial safeguard against reverse causality, not a fully identified causal estimate

Table 3: Variable definitions

Variable	Definition	Source/construction
Emissions score	A 0-100 score reflecting a company's commitment and effectiveness in reducing environmental emissions in its production and operational processes.	LSEG (Refinitiv) ESG database, emissions score
Leverage	Total debt scaled by total shareholders' equity (including minority interest and hybrid debt). Winsorized at the 1st and 99th percentiles.	Debt - Total/Total shareholders' equity.
Profitability (ROA)	Net income after tax scaled by total assets. Winsorized at the 1st and 99th percentiles. Added in this revision as an omitted-variable safeguard (Section 3.5).	Net Income after Tax/Total Assets.
Asset tangibility	Net property, plant, and equipment scaled by total assets. Winsorized at the 1st and 99th percentiles. Added in this revision as a standard capital-structure control (Section 3.5).	Property, Plant and Equipment - Net/ Total Assets.
ln (Total Assets)	Natural logarithm of total assets; proxies for firm size.	ln (Total Assets).
ln (Free Float)	Natural logarithm of the market value of shares freely available for trading; proxies for ownership dispersion and liquidity.	ln (Free Float).
ln (Market Capitalization)	Natural logarithm of market capitalization; proxies for firm scale and investor visibility.	ln (Market Capitalization).
Industry FE	Fixed effects for the TRBC Business Sector (30 categories). Absorbed by Firm FE where the latter is used (Section 3.5).	TRBC Business Sector Name.
Country FE	Fixed effects for the headquarters country (98 categories). Absorbed by Firm FE where the latter is used (Section 3.5).	Country of Headquarters.
Year FE	Fixed effects for the fiscal year (2005-2024).	Year.
Firm FE	Fixed effects for each of the 11,648 firms in the panel, absorbing all time-invariant firm heterogeneity (management quality, business model, sector, and country membership). Added in this revision (Section 3.5).	Firm identifier (RIC).

Free float and market capitalization enter the model in natural-log form to address extreme skew; raw dollar values span roughly ten orders of magnitude across the sample. ln (Total Assets) follows the same convention. Leverage, Profitability, and Asset Tangibility are each winsorized at the 1st and 99th percentiles; unlike the log-transformed variables, these three are simple ratios whose extreme outliers a log transform alone does not tame (e.g., firm-years with near-zero equity or total assets). Standard errors are clustered by firm in all regressions reported in this paper

is used elsewhere. The results from this specification are therefore interpreted accordingly in Section 4.4 of the paper.

3.5. Omitted Variable and Firm Fixed Effects Safeguards

A pre-submission peer review of this manuscript identified two design choices that require closer attention. Reviewers in this literature are likely to treat both as baseline expectations rather than as extensions. The two choices are the absence of firm fixed effects and profitability control. Both are addressed directly in this revision as additional robustness checks, as reported in Table 1.

First, a firm fixed-effects specification replaces the industry and country fixed effects used in the baseline. Both are time-invariant within a firm and are therefore mechanically absorbed by the firm fixed effects. A separate fixed effect is estimated for each of the 11,648 firms in the panel. The specification retains year fixed effects and firm-clustered standard errors and is written as follows:

$$EmissionsScore_{it} = \beta_0 + \beta_1 \bullet Leverage_{it} + \beta_2 \bullet \ln(TotalAssets)_{it} + \beta_3 \bullet \ln(FreeFloat)_{it} + \beta_4 \bullet \ln(MarketCapitalization)_{it} + FirmFE + YearFE + \varepsilon_{it} \quad (3)$$

This specification absorbs all time-invariant firm heterogeneity that could confound the estimated leverage relationship. Examples include management quality, business model, sector niche, and any persistent form of "ESG culture." Such unobserved heterogeneity plausibly drives both leverage and environmental performance simultaneously. The specification is estimated only from within-firm variation in leverage over the full sample period. The panel averages 7.8 firm-years per firm, with a median of 6 and a maximum of 20. Within-firm leverage variation is present but more limited than the full cross-sectional variation used earlier. This is expected to widen the standard errors relative to Table 4, column (2), even where point estimates are similar. A formal F-test for poolability rejects the null that firm fixed effects are jointly zero. The reported test statistic is F(11666, 79308) = 20.78, with an associated P-value below.001. This supports a fixed-effects rather than a pooled or random-effects specification for the present panel data. A Hausman-type argument reinforces this modelling choice on theoretical rather than purely statistical grounds. The random-effects estimator assumes that unobserved firm heterogeneity is uncorrelated with leverage in the estimated model. Section 2.1 argues that assumption is unlikely to hold for a firm's persistent "ESG culture."

Second, profitability control (return on assets, ROA) and asset tangibility controls are added. Both enter the industry, country, and year fixed-effects specifications and the firm fixed-effects specification. Profitability is a textbook determinant of leverage through pecking-order logic in capital structure literature. Profitable firms generate internal funds and therefore borrow less than otherwise comparable listed firms. Profitability also determines environmental investment through slack resource logic because profitable firms hold more discretionary resources for non-mandatory spending (Frank and Goyal, 2003; Hang et al., 2017). This is the same family of arguments that H_1 invokes but in the opposite direction. Omitting profitability therefore risks conflating a leverage effect with a profitability effect, exactly as Section 2.1 notes. Asset tangibility is added alongside profitability as a standard capital-structure control in these models. Tangible assets support debt capacity through their collateral value in most corporate lending decisions. They may also correlate independently with the scale of physical operations subject to environmental scrutiny. Both new variables are constructed exactly as described in Table 3 earlier in this paper. They are winsorized at the 1st and 99th percentiles on the same basis as leverage.

4. EMPIRICAL RESULTS

4.1. Descriptive Statistics and Correlations

Table 5 presents descriptive statistics for the estimation sample of 90,979 firm-year observations described above. The profitability and asset tangibility variables added in Section 3.5 cover 88,325 firm-year observations. This difference reflects the additional missingness noted above in Section 3.1. The mean Emissions Score is 42.29, with a median of 41.43 and standard deviation 32.92. The scores span nearly the full 0-100 range available on the underlying LSEG scale. This indicates substantial cross-firm variation in both emissions performance and emissions disclosure across the panel. Mean winsorized leverage, total debt to total shareholders' equity, is 0.96 with a median of 0.54 and SD 1.42. The winsorized range runs from 0 to 9.57 across the firm-years in the estimation sample. This reflects the truncation of extreme

Table 4: Regression results — leverage and emissions score (H_1, H_2)

Variable	(1) Pooled OLS	(2) Full FE (baseline)	(3) Squared term
Leverage	−0.4995*** (0.1640)	−0.2709* (0.1395)	−0.3941 (0.3703)
Leverage^2	—	—	0.0162 (0.0407)
ln (Total Assets)	4.4159*** (0.2172)	7.0436*** (0.2381)	7.0594*** (0.2460)
ln (Free Float)	1.6507*** (0.1554)	2.0197*** (0.1822)	2.0196*** (0.1822)
ln (Market Cap.)	3.3323*** (0.2221)	2.9335*** (0.2145)	2.9251*** (0.2175)
Constant	−158.481*** (3.085)	−228.751*** (5.848)	−228.853*** (5.857)
Industry FE	No	Yes	Yes
Country FE	No	Yes	Yes
Year FE	No	Yes	Yes
N	90,979	90,979	90,979
R^2	0.215	0.444	0.444

The dependent variable is the Emissions Score (0-100). Firm-clustered standard errors are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% levels, two-tailed. Coefficients are reported to two decimal places in the text and four decimal places here; the additional precision is substantively relevant when marginal statistical significance is being discussed (column 2)

values driven by firms with equity close to zero. Mean profitability (ROA) is 2.9% of total assets, with a median value of 3.4%. Mean asset tangibility is 25.3% of total assets, with a median value of 18.2%. Both figures are broadly typical of a large international, cross-industry panel such as this.

Table 6 reports the pairwise correlations among the continuous regressors and the dependent variable, including profitability and asset tangibility. All firm size and market scale variables are positively and significantly correlated with the Emissions Score in the sample. The variable ln(Total Assets) shows the strongest association, with a correlation coefficient of 0.441. Larger, more visible firms face greater disclosure and scrutiny pressure independent of any leverage channel. Leverage is positively but weakly correlated with the Emissions Score in raw pairwise terms (r = 0.056). Leverage is also moderately correlated with ln(Total Assets), at a coefficient of 0.230 overall. Profitability is more strongly correlated with the Emissions Score (r = 0.161) than leverage is. It is also negatively correlated with leverage (r = −0.112), consistent with the pecking-order theory (Ramalho and Silva, 2009). Given the sample size, correlations as small as 0.01 were statistically significant at conventional levels. All correlations reported in Table 6 are therefore significant at P < 0.001. The variance inflation factors for the six continuous regressors in the augmented model range from 1.02 to 2.73. The lowest value is asset tangibility, and the highest is ln(Total Assets) in that model. These values are well below the conventional thresholds for concern, which fall between VIF values of 5 and 10. Multicollinearity is not a material issue, even after adding profitability and tangibility to the regression.

4.2. Baseline Results: H_1 and H_2

Table 4 presents the baseline regression results, which were unchanged from the original study design. Column (1) reports a pooled OLS specification estimated without any fixed effects. Column (2) adds industry, country, and year fixed effects, the baseline specification used for H_1. Column (3) adds the squared leverage term in order to test the second hypothesis, H_2. In the pooled specification, leverage carries a negative and significant coefficient (β = −0.4995, SE = 0.1640, P = 0.002). Once industry, country, and year fixed effects are introduced, the coefficient shrinks but remains negative. It falls to marginal significance at the 10% level (β = −0.2709, SE = 0.1395, P = 0.052). The R^2 rises sharply from 0.215 to 0.444 once the fixed effects are added to the model. Industry, country, and year effects absorb a substantial share of the cross-sectional variation in the Emissions Score. This is consistent with the documented pattern that emissions performance varies systematically by sector and jurisdiction (Dietz et al., 2018). Therefore, H_1 is only weakly supported in the baseline design reported in Table 4 above. The leverage--emissions association is negative and economically similar in magnitude across the two specifications. Statistical confidence in that association is only marginal once the full fixed-effects structure is in place. As Section 4.3 shows, this marginal significance does not survive the addition of further controls. Adding profitability and tangibility controls to the same fixed-effects structure removes that significance entirely.

Column (3) of Table 4 adds the squared leverage term directly to test hypothesis H_2. Neither the linear term (β = −0.3941, SE = 0.3703, P = 0.287) nor the squared term is significant. The squared term is estimated at β = 0.0162, SE = 0.0407, with P = 0.690. The model's R^2 is unchanged from column (2) to the third decimal place shown. Therefore, H_2 is not supported by the results reported for this international panel sample. There is no evidence of a disciplining effect of moderate leverage that reverses beyond some threshold. This holds notwithstanding the inverted-U pattern Saona et al. (2026) report for a related variable. The linear specification in column (2) is not statistically improved by allowing a non-monotonic relationship.

The control variables are precisely estimated and stable in sign across all three reported columns. ln(Total Assets) carries a large,

Table 5: Descriptive statistics

Variable	n	Mean	SD	Min	P25	Median	P75	Max
Emissions score	90,979	42.291	32.923	0.000	8.365	41.430	71.675	99.940
Leverage (D/E, winsorized)	90,979	0.957	1.420	0.000	0.203	0.542	1.094	9.573
Profitability (ROA, winsorized)	88,325	0.029	0.105	-0.542	0.008	0.034	0.072	0.273
Asset tangibility (winsorized)	88,325	0.253	0.241	0.000	0.044	0.182	0.399	0.899
ln (Total Assets)	90,979	22.113	1.951	12.330	20.902	22.082	23.310	29.471
ln (1+Free Float)	90,979	18.981	1.828	0.000	17.787	18.945	20.219	29.978
ln (Market Capitalization)	90,979	21.687	1.808	5.050	20.645	21.779	22.810	33.862

Leverage, profitability, and asset tangibility are winsorized at the 1st and 99th percentiles of their distributions, following restriction of the sample to firm-years with positive total shareholders' equity. The unwinsorized leverage ratio has a mean of 1.52 and SD of 50.68, reflecting a small number of firm-years with near-zero equity in the raw licensed panel. ln (Total Assets), ln (Free Float), and ln (Market Capitalization) are not separately winsorized; the log transform already compresses their right tail (see Table 3 note)

Table 6: Correlation matrix

	Emissions	Leverage	ROA	Tangibility	ln (Assets)	ln (Free Float)	ln (Market Cap.)
Leverage	0.056	1.000					
ROA	0.161	−0.112	1.000				
Tangibility	0.076	0.029	0.056	1.000			
ln (Assets)	0.441	0.230	0.162	−0.059	1.000		
ln (Free Float)	0.287	0.073	0.050	0.046	0.474	1.000	
ln (Market Cap.)	0.415	0.034	0.322	−0.004	0.746	0.412	1.000

n=88,325 (the profitability- and tangibility-augmented sample described in Section 3.1). All correlations reported are statistically significant at P<0.001 given the large sample size. Each row reports the correlation of that variable with the columns to its left; no cell is duplicated between the lower and upper triangles of the matrix

positive, and highly significant coefficient throughout (β = 7.044, P < 0.001). The same holds for ln(Free Float) (β = 2.020) and ln(Market Capitalization) (β = 2.934), both of which are highly significant. Larger, more widely held, and more highly valued firms report substantially higher Emissions Scores, independent of leverage.

4.3. Omitted-Variable and Firm Fixed-Effects Robustness Checks

Table 1 reports the three additional specifications added in this revision in response to peer review (Section 3.5). Column (4) adds profitability (ROA) and asset tangibility to the industry, country, and year fixed-effects baseline. Column (5) replaces industry and country fixed effects with firm fixed effects, retaining year fixed effects. Column (6) combines firm and year fixed effects with profitability and tangibility controls, the most demanding specification reported here.

In column (4), profitability and tangibility are added to Table 4, column (2) baseline. The leverage coefficient shrinks from −0.2709 to −0.2140 and falls below conventional significance thresholds. The associated statistics are SE = 0.1413, P = 0.130, and n = 88,325 observations. Both added controls are precisely estimated and strongly positive in this augmented fixed-effects specification. Profitability is estimated at β = 10.32 (SE = 1.58) and tangibility at β = 6.38 (SE = 1.27). Both of these coefficients are significant at the 0.1% level in that regression model. This is consistent with slack-resources theory, as set out earlier in Section 2.1 above. Profitable, asset-heavy firms hold more discretionary resources and collateralizable capacity for non-mandatory environmental investment (Hang et al., 2017). This confirms the peer-review concern that Table 4 baseline partly conflated a leverage effect with an omitted profitability effect.

In Column (5), the industry and country fixed effects were replaced with firm fixed effects. The year fixed effects were retained, and the leverage coefficient remained close to the baseline in Table 4. The estimate is β = −0.2097 (SE = 0.1253, P = 0.094, N = 90,979), which is marginally significant at 10%. This specification uses only within-firm variation in leverage over time, averaging 7.8 firm-years. Therefore, it absorbs all time-invariant firm heterogeneity present in the estimation panels. The within-R^2 is 0.111, which is lower than the overall R^2 reported for the earlier fixed-effects specifications. A within-firm estimator cannot explain the variation driven by stable between-firm differences in sectors, countries, and scales. This between-firm variation is absorbed by firm fixed effects rather than explained by them.

Column (6) combines both safeguards: firm and year-fixed effects with profitability and tangibility controls. Here the leverage coefficient is −0.2305 (SE = 0.1279, P = 0.071, n = 88,325, within-R^2 = 0.113). It is marginally significant at the 10% level, the same threshold as columns (2) and (5). It is the only profitability-augmented specification in Tables 1 and 2, where leverage remains significant. Within this specification, profitability turns negative and statistically significant (β = −3.57, SE = 1.26, P = 0.004). This sign reversal from column (4) is itself informative about the underlying economic mechanism. Once time-invariant firm heterogeneity is absorbed, the remaining within-firm variation captures only short-run profitability fluctuations. It no longer captures the persistent slack-resource endowment of the firm over the sample period. Firms plausibly trim discretionary environmental spending in leaner years, net of their usual resource base. This is a distinct, shorter-horizon mechanism from the between-firm profitability effect estimated in column (4). Asset tangibility is not statistically significant in this specification (β = 1.29, SE = 1.75, P = 0.459).

Reading together, columns (4) to (6) give a more demanding picture than Table 4 alone. The leverage--emissions association is not robust to adding profitability and tangibility to the fixed-effects design in column (4). It survives at the 10% level under both of the most stringent safeguards recommended in the review. These are firm fixed effects in column (5) and firm fixed effects with controls in column (6). This pattern is inconsistent with the baseline being driven primarily by unobserved, time-invariant firm heterogeneity. Firm fixed effects would have absorbed and eliminated any such heterogeneity from the estimates. The pattern is consistent with profitability explaining part, though not all, of the estimated leverage effect.

5. ROBUSTNESS CHECKS: ENDOGENEITY SAFEGUARDS

Table 2 reports the lagged-leverage and 2SLS specifications that were described earlier in Section 3.4. Each is estimated with the original controls in columns 7 and 9, unchanged from the original design. Each is also estimated with profitability and asset tangibility added, in columns 8 and 10, new in this revision. In the lagged-leverage specification with original controls (column 7), the coefficient on lagged leverage is −0.2373. The associated statistics are SE = 0.1613, P = 0.141, and n = 78,374 observations. This is similar in magnitude to the fixed-effects baseline in Table 4 but not statistically significant. Adding profitability and tangibility in column 8 leaves the point estimate broadly similar and insignificant. The estimate is β = −0.2217 (SE = 0.1626, P = 0.173, n = 76,228) in that specification. Profitability and tangibility are both positive and significant here (β = 11.49 and 6.61, both P < 0.001). This finding is consistent with the pattern reported in column (4) of Table 1 above.

In the 2SLS specification with original controls (column 9), contemporaneous leverage is instrumented with its lag. The coefficient on the instrumented leverage is −0.2775 (SE = 0.1885, P = 0.141, n = 78,374). This is again close in magnitude to the OLS fixed-effects estimate but not statistically significant. Adding profitability and tangibility in column 10 yields β = −0.2611 (SE = 0.1914, P = 0.172). This estimate is based on 76,228 observations and is again not significant at conventional levels. Profitability and tangibility are strongly positive and significant in the specifications. The first-stage relationship between current and lagged leverage remains extremely strong regardless of the control set. The statistic is F = 16,168 with the original controls and F = 14,530 with the additions. Both statistics are firm-clustered

and lie far above the conventional weak-instrument threshold of 10 (Stock and Yogo, 2005).

Two patterns are worth emphasizing across the full set of ten specifications now reported in Tables 1, 2 and 4. First, the point estimates remain remarkably stable, ranging narrowly from −0.21 to −0.50 across the specifications. The estimates never change the sign in any of the ten specifications reported in this study. This is inconsistent with the leverage--emissions association being purely an artifact of reverse causality alone. Suppose the true underlying relationship were driven entirely by environmental performance causing lower leverage. That mechanism is documented by Benlemlih and Cai (2020), Kordsachia (2021), Tawfiq et al. (2024), and Malik et al. (2026). The lagged, instrumented, and firm-fixed-effects specifications each weaken a different candidate confounding channel. Each would then be expected to shrink the coefficient toward zero or flip its sign. None of them do so in the results reported across Tables 1, 2 and 4. Second, statistical significance is considerably more fragile here than the original two-safeguard design suggested. Leverage is significant at conventional levels in four of the ten specifications, columns 1, 2, 5, and 6. It is not significant in the remaining six specifications reported across the three tables here. These include both endogeneity-safeguarded specifications, regardless of whether profitability and tangibility are added. This is a materially more cautious picture than the framing of the original manuscript. Sections 5 and 6 are revised accordingly to reflect that more cautious empirical picture throughout.

6. DISCUSSION

H_1 predicts that leverage is negatively associated with environmental performance in this study's international panel. The prediction is framed net of size, ownership dispersion, market capitalization, profitability, and asset tangibility. It is also framed net of industry, country, and year effects, or of firm effects. The prediction is supported in the direction across all ten specifications estimated in Tables 1, 2 and 4. However, only four of the ten estimated specifications are statistically supported. Leverage is significant at the 1% level in the pooled model without fixed effects. It is marginal at the 10% level with full industry, country, and year fixed-effects structure. It is marginal again at 10% under firm and year fixed effects, with or without the controls. However, it is not significant once profitability and tangibility join the industry, country, and year design. It is also insignificant once lagged leverage or 2SLS safeguards are applied, regardless of controls. This overall pattern of results is informative in two distinct and separable ways. A coefficient that changed sign or collapsed toward zero once the safeguards were introduced would be significant. This suggests that the association in Table 4 was largely an artifact of one of two things. First, environmentally responsible firms choose lower leverage, as documented by Benlemlih and Cai (2020). The second explanation is persistent, unobserved firm heterogeneity that affects both leverage and environmental performance. Neither pattern occurs in any of the ten specifications reported in this paper. The coefficient remains negative throughout and within a narrow band, from − 0.21 to − 0.50. This rules out the strongest form of concern regarding the estimated leverage association. However, the statistical significance is lost in six of the ten specifications reported in this study. These include specifications that combine profitability controls with industry, country, and year design. That is, column (4) of Table 1, which addresses a central peer-review baseline expectation. Therefore, the evidence for H_1 is better described as a modest, imprecisely estimated financial-slack signal. This is not a robust, high-confidence finding regarding the effect of leverage on emissions. This is a materially more cautious conclusion than what the original single-safeguard design could support. This conclusion is carried forward into Section 6 of the present paper.

The raw pairwise correlation of leverage with the Emissions Score is positive at r = 0.056. The size-adjusted regression coefficient on leverage is negative in all specifications. That sharp contrast is itself a substantively important finding in its own right here. It remains the clearest illustration in this paper of why multivariate controls matter in this literature. Firm size is positively correlated with both leverage (r = 0.230) and the Emissions Score (r = 0.441). Larger firms borrow more in both absolute and relative terms across this large international sample. They also face greater disclosure and scrutiny pressure that raises measured emissions performance independent of financing. Failing to control for firm size would therefore mask the within-size financial-slack channel entirely. It could even reverse the apparent sign of the channel these specifications are designed to isolate. Table 1 extends this logic one further step in the empirical analysis reported here. Profitability, not leverage, has the stronger raw correlation with the Emissions Score (r = 0.161). Profitability is also negatively correlated with leverage itself, at r = −0.112 in the sample. A specification controlling for size but omitting profitability, as Table 4 did, is therefore vulnerable. It risks attributing to leverage part of an effect that actually runs through profitability instead. That is precisely the pattern observed in column (4) of Table 1 of this paper. Adding profitability in that column shrinks the leverage coefficient and removes its statistical significance.

H_2 predicts a non-monotonic relationship between firm leverage and measured environmental performance in the sample. Moderate leverage might discipline managers toward efficiency without crowding out any environmental investment. Only high leverage would then crowd out that discretionary environmental spending under this account. This hypothesis is not supported by the results reported in Table 4, column (3) above. The squared leverage term is statistically indistinguishable from zero in that particular estimated specification. The overall model fit does not improve when the squared term is added. Leverage in this sample is winsorized between 0 and 9.57, with a median of 0.54. Within that observed range, a single linear crowding-out relationship describes the data, as well as a quadratic one. This null result sits alongside, rather than replicating, the inverted-U finding of Saona et al. (2026). The two studies test related but distinct outcome constructs, which limits any direct comparison between them. This study uses a disclosure-based Emissions Score, while that study uses an impact-adjusted valuation measure. The divergence may therefore reflect a difference in what is measured rather than in economics. Neither of the two findings should be read as superseding the other in this literature. The null result does not rule out a disciplining

effect of debt on other performance margins. Operating efficiency and free-cash-flow discipline are two obvious examples of such margins. Any such effect simply does not translate into detectable nonlinearity in the emissions outcome measured here.

The consistently strong, positive coefficients on firm size, free float, and market capitalization are unsurprising. The relationship between firm visibility and ESG disclosure and performance is well documented (Gamerschlag et al., 2011). These coefficients remain a useful economic-magnitude benchmark for the size of the leverage effect. A one-standard-deviation increase in leverage, of 1.42 units, is associated with a 0.38-point decrease in the Emissions Score. This estimate is based directly on the leverage coefficient reported in Table 4, column (2). A one-standard-deviation increase in ln(Total Assets), of 1.95 units, raises the Score by 13.74 points. --- a standardized comparison that replaces the raw-coefficient comparison used in the original design and shows leverage's economic magnitude to be roughly 3% (2.8%) that of firm size, smaller than the raw comparison alone conveyed. Firm scale and visibility remain the dominant observable correlates of the Emissions Score in this sample. This is consistent with the legitimacy and stakeholder-pressure accounts of environmental disclosure more generally (Chithambo et al., 2020). Leverage, even where it is statistically significant, is best understood as a second-order correlate. It is not a primary driver of measured environmental performance in this large international sample.

Finally, these results are in direct tension with the findings of Arisyahidin et al. (2025). They report a positive capital structure--sustainability performance association in an 11-country Asian panel of comparable design ambitions. Three explanations are worth distinguishing here, although these data cannot adjudicate between them directly. The first explanation is institutional heterogeneity between the two samples being compared. Their sample is regionally concentrated in Asia, unlike the broader panel used in this study. Turkish evidence from Kalash (2021) and Chinese evidence reviewed in Section 2.2 (Sun et al., 2019) also document positive leverage--environmental associations. The opposite sign may therefore reflect a genuinely different average institutional environment in Asia. Examples include more prevalent policy-conditioned credit and legitimacy-driven bundling of CSR and leverage decisions. That is a substantive difference rather than a methodological disagreement between the two studies. The country fixed effects here average over, rather than isolate, this kind of regional heterogeneity. The second explanation concerns differences in the outcome constructs used by the two studies compared. Arisyahidin et al. (2025) measure sustainability performance through modified leverage and a broader sustainability index. This study instead uses a narrower, disclosure-anchored Emissions Score as the dependent variable throughout. The two constructs need not move together, even under a single shared underlying causal structure. The third explanation, most directly addressed by this revision, concerns differences in omitted variables. Their design may not separately control for profitability alongside leverage in the estimated regression model. Part of their positive association could then reflect the same profitability confound identified in this paper. Table 1, column (4) shows that confound can flip the significance, though not the sign. Distinguishing among these explanations is a natural extension for future research, as Section 7.2 notes. It is not a question that the design of this study can resolve on its own.

7. CONCLUSION

This study examines whether corporate leverage is associated with environmental performance across an international panel. The panel contains 90,979 firm-year observations spanning 98 countries and the 2005-2024 period. This revision layers firm fixed-effects and profitability and tangibility safeguards onto the original study design. Leverage is negatively associated with the firm's Emissions Score across all ten specifications estimated here. The coefficients range narrowly from −0.21 to −0.50 across those ten estimated specifications. This is consistent with H_1 and with the financial-slack and crowding-out channel described earlier. Under that channel, higher debt-service obligations and creditor monitoring divert resources away from environmental investment. Statistical confidence in this association is nonetheless fragile across the full set of ten specifications. It is significant at conventional levels in four of the ten specifications estimated here. Those four are pooled OLS, the fixed-effects baseline, and both firm-and-year fixed-effects specifications reported. It is not significant in the remaining six specifications reported across the three tables. These include the specification adding profitability and tangibility to the industry, country, and year baseline. They also include both the lagged-leverage and 2SLS specifications, regardless of the control set. The point estimate does not meaningfully change in magnitude or sign as safeguards are introduced. This pattern is more consistent with a real but modest and imprecisely estimated financial-slack effect. It is less consistent with a purely spurious raw correlation between the two variables. It is also less consistent with an association fully explained by reverse causality or unobserved heterogeneity. The fragility of statistical significance means this conclusion should be held with real caution throughout. H_2 predicted a disciplining nonlinearity at moderate leverage, and it is not supported by these results. The squared leverage term is statistically insignificant throughout all of the specifications estimated here. This holds notwithstanding the related inverted-U pattern that Saona et al. (2026) report for a different environmental-outcome construct.

7.1. Theoretical Contribution

This study contributes to the capital structure and environmental performance literature in three main ways. Each contribution is recalibrated in this revision against a fuller set of comparators and specifications. First, prior single-country studies have reported the leverage--environmental performance relationship in sharply conflicting directions. The relationship is positive in some Chinese and Asian settings (e.g., Yang et al., 2018; Sun et al., 2019; Arisyahidin et al., 2025). It is negative or reversed-causal in the U.S. and European evidence (e.g., Benlemlih and Cai, 2020; Kordsachia, 2021; Tawfiq et al., 2024; Malik et al., 2026). This study shows that divergence is not simply an artifact of differing institutional settings alone. In a pooled international panel with country or firm fixed effects, the average relationship is negative. These fixed effects absorb time-invariant institutional and firm-level differences across the full sample of firms. This aligns more

closely with the financial-slack account than with legitimacy or policy-conditioned credit accounts. The opposite-signed finding of Arisyahidin et al. (2025) comes from a comparably international Asian panel of firms. This claim should therefore be read as evidence about the average relationship in this sample. It is not a resolution of the broader cross-study disagreement discussed in Section 5. Second, it demonstrates empirically, rather than merely asserting, that the size confound is materially large. The confound is large enough to affect the observed leverage--environmental-performance association reported in this literature. Table 1 in this revision shows that an omitted profitability confound operates in a similar way. Both underscore the importance of the fixed-effects and control-variable structure used here and elsewhere. They underscore specifically the value of the firm fixed-effects and profitability safeguards added after review. Third, the study tests for a squared-leverage nonlinearity and does not find one anywhere. This narrows the range of plausible theoretical mechanisms for this particular outcome construct. The data are more consistent with a monotonic financial-slack channel than with a disciplining account. That disciplining-then-crowding-out account concerns the role of debt in the Emissions Score measured here. Inverted-U findings for a related but distinct outcome suggest the nonlinearity question remains open elsewhere. That related outcome is environmental-impact-adjusted firm value rather than the disclosure-based Emissions Score used here.

7.2. Practical and Policy Contribution

For creditors, institutional investors, and ESG-linked lenders, leverage may carry modest information about environmental performance. That information holds net of firm size and market visibility in all of these specifications. It is directionally consistent with, but numerically much smaller than, the information carried by scale. Its statistical reliability depends materially on whether profitability and tangibility are controlled for here. It also depends on whether firm-level heterogeneity is absorbed, as this revision clearly shows. Some policymakers are interested in using credit conditions to influence corporate environmental conduct directly. China's green credit policy, examined by Sun et al. (2019). (Sun et al., 2019) the evidence in this study is more limited than the original design suggested it could support. The negative baseline association is marginal at P = 0.052 in Table 4, column 2. It is consistent with unconditional increases in leverage not improving, and possibly modestly harming, performance. This policy-relevant claim rests specifically on the fixed-effects baseline and the firm-fixed-effects specifications reported above. Those are Table 1, columns 5 and 6, both marginal at P of roughly 0.07-0.09. The claim does not rest on the two specifications genuinely safeguarded against reverse causality here. Those are the lagged-leverage and 2SLS specifications that are reported in Table 2 above. Both are statistically insignificant whether or not profitability and tangibility are included (P = 0.14-0.17). Readers, including policymakers, should treat the policy implication as a plausible but unconfirmed hypothesis for now. It is grounded in the fixed-effects associations rather than in a causally identified finding. It should be weighed alongside other evidence from a comparable international Asian panel (Section 5). Readers should do so before drawing firm conclusions for any one specific institutional setting. For corporate financial managers, increasing leverage may carry a modest trade-off against environmental investment capacity. That trade-off is distinct from, and additive to, the reputational and cost-of-capital consequences of leverage. Those other consequences are documented elsewhere in the wider capital-structure literature reviewed in this paper. The size of this trade-off is sensitive to which controls and fixed effects are used. Even its presence at conventional statistical confidence is sensitive, as Tables 1, 2 and 4 show.

7.3. Limitations and Future Research

7.3.1. Limitations

Several limitations qualify these findings: Four carried forward from the original design and two added in this revision.

First, the lagged-leverage and 2SLS specifications in Section 4.4 provide only a partial safeguard against reverse causality. Lagged leverage is a strong first-stage predictor of current leverage by mechanical construction alone. Leverage is highly autocorrelated from 1 year to the next within a given firm. The exclusion restriction required for a fully identified causal estimate cannot be tested directly here. The restriction is that lagged leverage affects current environmental performance only through current leverage. A genuinely exogenous instrument, such as statutory tax-shield variation, was not available in this dataset. Ardianto et al. (2024) address a closely related identification problem, namely carbon-emission disclosure and capital structure. They use a substantially larger toolkit than the one deployed in the present study. The toolkit includes the Heckman two-stage correction, propensity-score matching, entropy balancing, and two-step system GMM. A reader comparing the two studies should weigh the contribution of this paper accordingly. It is the breadth of the sample and layered safeguards, as Section 1 explains, not identification depth.

Second, the Emissions Score is a widely used and continuous measure of environmental performance. It is itself partly a disclosure-based construct rather than a purely objective performance measure. Firms that do not report relevant emissions data may receive lower or missing scores. This may reflect their disclosure practices rather than their underlying environmental performance. The missing-data filters described in Section 3.1 necessarily drop any firm-years without adequate coverage. This could bias the estimation sample toward larger, more disclosure-active firms in the panel. Relatedly, the Emissions Score here is drawn from a single data provider, LSEG or Refinitiv. ESG ratings from different providers are documented to correlate only moderately with one another. This is the well-known "aggregate confusion" problem described by Berg et al. (2022). They show that MSCI, Sustainalytics, S and P Global, and other major providers frequently disagree on the same firm-year. This study does not have access to a second ESG data source for any comparison. It cannot report whether the estimated association replicates using MSCI, Sustainalytics, or S and P Global data. Several central estimates here sit at or near the edge of conventional statistical significance. Provider-specific measurement is therefore a plausible source of fragility alongside the specification sensitivity documented throughout this paper. A second-provider replication is therefore identified as a priority extension in Section 7.2 below.

Third, industry fixed effects are specified at the TRBC Business Sector level, with 30 categories. They are not specified at the more granular TRBC Industry level, which has 140 categories. The original design noted that a firm-fixed-effects specification was a natural extension for future work. Such a specification would additionally absorb all time-invariant firm heterogeneity present in the panel. This revision now reports the specification directly in Table 1, columns (5) and (6). It is therefore no longer an unaddressed limitation, although it carries its own caveat. The within-firm estimator relies only on within-firm variation in leverage over the sample period. This variation is more limited than the full cross-sectional variation used elsewhere in this paper. The panel averages only 7.8 firm-years per firm across the 11,648 firms it includes. This is the most plausible explanation for the modestly wider standard errors in those columns.

Fourth, the sample is broad in country coverage but is not a random sample worldwide. It reflects the coverage of the LSEG (Refinitiv) ESG database used throughout this study. The database skews toward larger, publicly listed firms with more developed disclosure practices overall (Dobrick et al., 2023). The results should be interpreted as applying to this population rather than to private or micro-cap firms.

Fifth, and added in this revision, the profitability and asset-tangibility controls are accounting-based constructs. They are measured contemporaneously with both leverage and the Emissions Score in each firm year. They are therefore potentially subject to a version of the same reverse-causality concern noted earlier. This concern motivates the lagged-leverage and 2SLS safeguards applied to leverage itself above. Environmental investment could, in principle, affect short-run profitability or the composition of tangible assets. This study does not lag or instrument the profitability and tangibility controls it adds. The estimates in Tables 1 and 2 should be read as controlling for these channels. They should not be read as causally isolating either of those two particular channels.

Sixth, also added in this revision, the poolability F-test is reported in Section 3.5. It formally rejects pooling in favor of firm fixed effects across this estimation panel. This study does not report a formal Hausman test comparing fixed against random effects. The theoretical argument in Section 3.5 is offered in place of that formal test. That argument is that unobserved firm heterogeneity is unlikely to be uncorrelated with leverage. This is consistent with how the comparison is often handled elsewhere in this literature. Random effects are not a serious contender on theoretical grounds in the present setting. A reader who wants the formal statistic should treat its absence as a minor gap.

7.3.2. Future research

Future research should pursue five extensions, each mapped to the specific limitation it addresses.

- First, a genuinely exogenous instrument for leverage would allow for a more fully identified causal estimate. Possible candidates include staggered changes in statutory tax shields, local banking-sector shocks, and capital requirements. Regulatory capital requirements affecting corporate lenders are a further plausible source of exogenous variation. Such an instrument would improve the lagged-leverage safeguard used in this study. This would directly address the exclusion-restriction limitation described in Section 7.1 above. The environmental-protection-tax shock exploited by Xiao et al. (2022) in China is one concrete template. The quasi-natural experiments exploited by Xu et al. (2020) in the United States are another. Both are concrete templates for this kind of design in a large international panel.
- Second, replicating the central estimates using a second ESG data provider would be valuable. MSCI, Sustainalytics, or S and P Global would each serve well for such a replication exercise. Even a subsample where coverage overlaps with LSEG or Refinitiv would be informative here. This would directly address the ratings-divergence limitation that is described second in Section 7.1. It would clarify whether the marginal significance throughout Tables 2 and 4 reflects the economics. The alternative is that it is partly an artifact of relying on a single instrument.
- Third, leverage could be usefully decomposed by maturity into short-term versus long-term debt. One alternative method involves categorizing by source, differentiating between bank loans and debt from public markets. Such an analysis might determine whether the financial-slack channel operates uniformly across all debt categories or is more significant in certain categories. This study would build upon the maturity-centered findings of Benlemlih and Cai (2020) and Tan et al. (2020). Furthermore, it would help clarify why similar estimates emerge from within-firm and fixed-effects models, which integrate all debt maturities and origins while considering various confounding variables.
- Fourth, the literature review underscores notable variations in institutional frameworks across the countries and regions studied. This issue is directly addressed by the differing outcomes reported by Arisyahidin et al. (2025), as discussed in Section 5. Implementing a formal cross-country moderation analysis would extend the pooled average-effect methodology used in this study. This would test whether creditor rights, banking sector structure, or green credit policy explains the sign variation. This is in the spirit of the institutional moderation documented by Ho et al. (2021). They document the moderation of leverage adjustment speed more generally across many countries. This would address the institutional heterogeneity question regarding the country and firm fixed effects.
- Fifth, an explicit panel-threshold or reservoir-versus-crowding-out framework can be usefully applied to these data (Shi and Huang, 2024; Tao et al., 2021). Replicating the inverted-U specification of Saona et al. (2025) on the Emissions Score is another option. Either approach could better capture regime-dependent nonlinearity than the squared-leverage specification tested here. That specification found no evidence of nonlinearity anywhere in this international panel of firms. It may, however, lack the power or functional form to detect a threshold effect. Either approach would directly reconcile the H_2 null result with the positive finding of Saona et al. (2026) (Section 5).

8. DATA AVAILABILITY STATEMENT

The firm-level ESG and financial data used in this study were obtained under licence from the LSEG (Refinitiv) ESG database. The authors are unable to share the underlying firm-level data directly; researchers with an institutional subscription to LSEG (Refinitiv) can access equivalent data. All regressions reported in this paper were estimated in Python 3, using pandas for data construction, statsmodels for OLS and dummy-variable fixed-effects estimation, and linearmodels for the panel firm-and-year fixed-effects and IV2SLS specifications; standard errors are firm-clustered throughout. Analysis code and derived summary statistics are available from the corresponding author on request.

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