International Journal of Economic Practices and Theories (IJEPT)
ISSN: 2247-7225
Volume 2026, Issue 1

Is the ESG Penalty Real? A Propensity Score Matching Analysis of Indian Listed Firms

Prakhar Wadhwa

Research Scholar, Department of Commerce, University of Delhi, New Delhi, India

Prof. Vipin Kumar Aggarwal

Principal, Sri Aurobindo College (Evening), University of Delhi, New Delhi, India

Abstract

This paper investigates whether the mostly reported negative association between ESG classification and firm valuation reflects a genuine causal effect or a statistical artefact. Using Bloomberg data for 487 listed Indian firms (84 ESG-classified, 403 non-ESG) over 2005–2023, a Difference-in-Differences framework reveals a significant cross-sectional ESG penalty on Tobin's Q ($\beta = -0.695$, $p < 0.01$). Similar to Difference-in-Differences framework, propensity score matching on firm size, matching each ESG firm to two non-ESG firms of comparable log market capitalisation, eliminates this penalty ($\beta = -0.662$, $p > 0.10$). The result holds across most performance measures (ROA, ROE, DuPont ROE) and survives lagged dependent variable controls, winsorisation, placebo tests, and dynamic event study validation. The ESG × Post2015 DID interaction is null across all specifications, confirming that India's post-2015 regulatory shift produced no measurable differential financial effect for ESG firms. The apparent ESG penalty is a size artefact. ESG firms are larger as a group, and larger firms trade at lower Tobin's Q for structural reasons unrelated to ESG. Cross-sectional ESG performance correlations should not be interpreted without adequate counterfactual construction.

Keywords: ESG, financial performance, propensity score matching, size artefact, Tobin's Q, India, difference-in-differences, BRSR

JEL Classification: G14, G32, G38, M14, Q56

1. Introduction

The rapid expansion of Environmental, Social, and Governance (ESG) investing has prompted a growing academic debate over whether ESG classification translates into measurable financial value. Meta-analytic evidence compiled by Friede, Busch, and Bassen (2015), aggregating over 2,200 individual studies, reported that approximately 90 per cent of studies found a non-negative relationship between ESG and corporate financial performance.

Whelan et al. (2021), reviewed over 1,000 studies from 2015 to 2020, found 58 per cent positive, 13 per cent neutral, 21 per cent mixed, and 8 per cent negative. These combined figures have been cited as evidence of a positive ESG performance consensus.

Yet the consensus is still fragile than it appears. Whether the relationship is positive, negative, or negligible depends on a many other factors like the geographic context, the ESG metric used, the financial outcome being measured, the time horizon, and how endogeneity is handled.

Berg, Koelbel, and Rigobon (2022) show that correlations between ESG ratings of major providers average 0.54, a level of disagreement they describe as "aggregate confusion". If the measurement of ESG itself is so noisy, the claimed relationship between ESG and financial performance warrants scepticism.

The choice of financial outcome also matters, market-based measures like Tobin's Q capture future looking investor expectations, while accounting based measures like ROA and ROE reflect past operational performance and may take ESG related costs or efficiencies only with a lag.

A major source of endogeneity is the non-random nature of ESG classification. Firms that enter ESG indices or obtain ESG ratings differ systematically from those that do not. In most markets, ESG classified firms tend to be larger, profitable, liquid, and internationally visible. If these characteristics independently predict

financial performance, any observed correlation between ESG status and firm value will merge the ESG effect with a selection effect. This paper shows that this combination matters.

Using Bloomberg data for 487 listed Indian firms (84 ESG-classified, 403 non-ESG) over the period 2005–2023, this paper begins with a standard Difference-in-Differences (DID) framework that produces a statistically significant negative ESG coefficient on Tobin's Q ($\beta = -0.695$, $p < 0.01$) that is the apparent ESG penalty.

It then demonstrates that this penalty disappears entirely when propensity score matching (PSM) is used to compare ESG firms only to non-ESG firms of comparable size. In the matched sample of 2,046 firm-year observations, the ESG coefficient drops to -0.662 ($p > 0.10$), a near-identical point estimate rendered insignificant by the elimination of size-based between-group variation.

The same null result holds for ROA, ROE, and DuPont ROE. The ESG × Post2015 DID interaction is insignificant across all specifications and samples, indicating that India's post-2015 regulatory environment produced no measurable differential financial effect.

India provides an informative setting for this analysis. It is the world's fifth-largest economy, home to over 5,000 listed companies, and has seen substantial regulatory changes in ESG disclosure. The Ministry of Corporate Affairs issued voluntary CSR guidelines in 2009, followed by mandatory CSR spending under Section 135 of the Companies Act 2013.

SEBI mandated Business Responsibility Reporting (BRR) for the top 100 listed entities in 2012, expanded coverage to the top 500 by 2015 and the top 1,000 by 2019, and replaced the BRR with the Business Responsibility and Sustainability Reporting (BRSR) framework in May 2021, mandatory from FY 2022–23.

The BRSR Core, requiring independent third-party assurance on nine key performance indicators, was introduced in July 2023. This phased regulatory build-up, coinciding with India's adoption of the Sustainable Development Goals in 2015, makes the post-2015 period a credible treatment window for the DID framework. Few other emerging markets went through a comparable sequence of regulatory escalation over the same period.

The paper is motivated by competing predictions from stakeholder theory (Freeman, 1984), which predicts that ESG engagement builds relational assets that translate into superior financial performance, and signalling theory (Spence, 1973), which suggests that ESG disclosure may serve as a costly signal to separate genuine ESG performers from greenwashers. Against these, agency theory (Jensen and Meckling, 1976) raises the concern that ESG spending may reflect managerial self-interest, while Cornell's (2021) equilibrium argument implies that any ESG premium should be competed away once ESG information is widely available. The DID framework helps adjudicate among these predictions by comparing the same firms before and after the regulatory shift, controlling for time-invariant firm characteristics and common time trends.

The paper makes three contributions. First, it provides the first PSM-based analysis of the ESG–financial performance relationship for Indian listed firms, demonstrating that the cross-sectional ESG penalty is entirely a size artefact. Second, it extends the DID identification strategy validated by a dynamic event study confirming parallel pre-trends and a placebo test, to the Indian ESG context, a quasi-experimental approach that is rare in the India-specific literature, which has relied predominantly on correlational designs. Third, the results carry a methodological warning for the broader ESG performance literature: studies that report negative (or positive) ESG performance associations without adequately addressing the size confound may be capturing the well-documented size effect rather than an ESG-specific phenomenon. The remainder of the paper is organised as follows. Section 2 reviews the literature. Sections 3 and 4 describe the data and methodology. Section 5 presents the results. Section 6 documents robustness checks. Section 7 discusses the findings, and Section 8 concludes.

2. Literature Review

2.1 The ESG–Financial Performance Debate

The theoretical case for a positive ESG performance link draws on stakeholder theory (Freeman, 1984; Donaldson and Preston, 1995), which posits that firms attending to the interests of employees, communities, and the

environment build relational assets that translate into superior financial outcomes. This theory predicts that ESG engagement reduces risk, lowers the cost of capital (El Ghoul et al., 2011; Chava, 2014), and generates a stakeholder insurance effect during crises (Lins, Servaes, and Tamayo, 2017).

The resource-based view (Barney, 1991; Wernerfelt, 1984) complements stakeholder theory by suggesting that ESG capabilities constitute intangible resources that are difficult to imitate and can therefore sustain competitive advantage.

Against the positive view, agency theory (Jensen and Meckling, 1976) warns that ESG spending may reflect managerial self-interest i.e. executives pursuing reputational benefits at shareholders' expense, rather than value creation. Friedman's (1970) classical shareholder view goes further, arguing that any corporate expenditure beyond profit maximisation represents an agency cost.

The efficient market hypothesis offers a third perspective: if markets efficiently incorporate ESG information into asset prices, no residual return differential should persist (Cornell, 2021; Pástor, Stambaugh, and Taylor, 2021). Under this view, the null result is the theoretically predicted outcome.

The empirical record is mixed. Friede, Busch, and Bassen's (2015) meta-analysis reported a non-negative association, but this aggregate finding masks substantial heterogeneity across regions, time periods, and methodologies. Auer and Schuhmacher (2016) found no significant ESG return premium after controlling for risk in their international sample.

The COVID-19 pandemic generated a wave of studies on ESG resilience. Albuquerque et al. (2020) documented that high-ESG firms experienced less negative stock returns during the crash, while Broadstock et al. (2021) reported ESG resilience in China.

However, Demers et al. (2021) showed that the COVID-era resilience effect was driven by intangible asset intensity rather than ESG per se, and El Khoury et al. (2022) found mixed evidence across G20 nations. These divergent findings suggest that the ESG performance relationship is conditional dependent on context, measurement, and methodology rather than a stable empirical regularity.

2.2 Selection Bias and the Size Confound

A recurring problem in ESG research is endogeneity. ESG adoption is not randomly assigned, larger, visible, and profitable firms self-select into ESG classification. Instrumental variable approaches, GMM estimators (Arellano and Bond, 1991), and propensity score matching have been employed to address this concern, but no approach completely resolves it.

The size confound is problematic because larger firms are both more likely to receive ESG classification through higher data availability, greater analyst coverage, and index inclusion thresholds and more likely to exhibit lower Tobin's Q due to decreased growth optionality and diversification discounts.

Hong and Kacperczyk (2009) demonstrated that the exclusion of sin stocks from ESG portfolios alters portfolio characteristics in ways that confound ESG performance acknowledgement. Bebchuk and Cohen (2005) showed that governance effects on firm value are sensitive to the inclusion of size controls. Chatterji et al. (2016) documented that ESG ratings diverge substantially across providers, raising the question of what ESG scores are actually measuring.

Despite these warnings, much of the ESG–performance literature continues to rely on OLS or panel regressions without matching, leaving the size confound unaddressed. The DID methodology has gained some traction Flammer (2015) used a regression discontinuity approach for CSR proposals, and Flammer and Ioannou (2021) employed DID with matched samples in emerging markets, but such designs remain the exception rather than the rule, particularly in the Indian context.

2.3 ESG in the Indian Context

The India-specific evidence is thin and inconclusive. Sharma, Bhattacharya, and Thukral (2019) found a negative influence of ESG on accounting and market measures for BSE-500 companies. Rao et al. (2023) documented that

the environmental and governance pillars negatively affected ROE across most quantiles for Nifty 50 firms. Dalal and Thaker (2019) reported positive associations but without a causal identification strategy.

Chelawat and Trivedi (2016) found mixed results in an early Indian sample. Dharmapala and Khanna (2018) examined India's mandatory CSR spending requirement under Section 135 and found limited evidence of positive financial impacts, while Manchiraju and Rajgopal (2017) reported a negative market reaction to the CSR mandate. More recently, Dutt et al. (2025), Biju, Thomas, and Garg (2025), and Goud (2025) have explored ESG performance linkages using newer frameworks, with similarly mixed conclusions. Maji and Lohia (2023) found partial positive effects for Indian firms, while Shobhwani and Lodha (2024) reported mixed results across different performance measures.

What these Indian studies share is the absence of quasi-experimental identification. Most employ cross-sectional regressions or basic panel models without a reliable treatment control comparison.

No prior study, to my knowledge, used a DID framework exploiting the post-2015 regulatory shift combined with PSM to address size-based selection. This matters because, as Dumitrescu and Zakriya (2021) demonstrated, ESG scores have weaker predictive power for financial performance in emerging markets relative to developed markets, and the mechanisms underlying any ESG performance relationship may differ across institutional contexts.

2.4 Research Hypotheses

The paper tests two primary hypotheses.

H_1: ESG classified firms exhibit a statistically significant negative coefficient on Tobin's Q in unmatched regressions, reflecting an apparent ESG penalty.

H_2: ESG penalty disappears after propensity score matching on firm size, confirming that it is a size artefact rather than a causal effect.

A subsidiary hypothesis tests whether the post-2015 regulatory shift produced a differential performance change for ESG firms (the DID null):

H_3: The ESG × Post2015 DID interaction is statistically insignificant, indicating that the Indian ESG regulatory framework did not generate measurable financial returns for ESG classified firms.

3. Data and Sample

3.1 Data Sources and Sample Construction

The empirical analysis draws on firm-level financial and ESG classification data from the Bloomberg Terminal for Indian companies listed on the National Stock Exchange (NSE). Bloomberg provides consistent coverage of both financial variables and ESG index membership, and its binary ESG classification avoids the measurement noise associated with continuous ESG scores from providers whose methodologies differ substantially (Berg, Koelbel, and Rigobon, 2022). The sample period spans 2005 to 2023, providing ten years of pre-treatment data (2005–2014) and nine years of post-treatment data (2015–2023).

Financial-sector firms were excluded. Firms with fewer than three consecutive years of non-missing observations were dropped, and extreme values beyond the 1st and 99th percentiles were winsorised. Market capitalisation values of zero for 78 firm-years were imputed via within-firm linear interpolation (61 cases successfully imputed, 17 removed). Bloomberg-reported ROE of zero for 189 observations where net income was non-zero was replaced with calculated ROE. The final sample comprises 487 unique firms (84 ESG-classified, 403 non-ESG), yielding 6,716 firm-year observations. The ESG classification is time-invariant.

3.2 Variables

The primary dependent variable is Tobin's Q (mean 2.79), with secondary measures ROA (mean 7.6 per cent), ROE (mean 15.1 per cent), and DuPont ROE. The treatment variable is a binary ESG indicator. Controls are log

market capitalisation and asset turnover. Year fixed effects and firm-clustered standard errors are used throughout. Table 1 reports the mean comparison between ESG and non-ESG firms.

Table 1: Mean Comparison ESG (N = 84) vs Non-ESG (N = 403) Firms

Variable	ESG Mean	Non-ESG Mean	Diff.	t-stat	p (Welch)	p (Wilcoxon)
Tobin's Q	3.04	2.73	+0.31	1.42	0.157	0.089
ROA	0.081	0.075	+0.006	1.08	0.281	0.312
ROE	0.173	0.146	+0.027	2.18	0.031	0.024
Asset Turnover	0.781	0.806	−0.025	−0.62	0.537	0.614
Log(Mkt Cap)	8.88	6.87	+2.01	14.82	<0.001	<0.001
Total Assets (Cr)	30,256	3,949	+26,307	8.41	<0.001	<0.001

Notes: Firm-level means. ESG firms substantially larger (log market cap: 8.88 vs. 6.87, $p < 0.001$).

ESG firms are substantially larger: mean log market capitalisation is 8.88 versus 6.87 for non-ESG firms ($t = 14.82$, $p < 0.001$), corresponding to mean total assets of approximately ₹30,256 crore versus ₹3,949 crore a roughly 7.7-fold difference. ESG firms report slightly higher Tobin's Q (3.04 versus 2.73, $p = 0.157$) and significantly higher ROE (17.3 versus 14.6 per cent, $p = 0.031$), but lower asset turnover (0.78 versus 0.81). The size gap is the central fact motivating the PSM analysis.

4. Methodology

4.1 The Identification Problem

The main challenge in estimating the causal effect of ESG classification on firm financial performance is that ESG adoption is not randomly assigned. Firms that enter ESG indices or obtain ESG ratings differ systematically from those that do not, and many of these differences, particularly firm size, are themselves correlated with the outcome variables. A standard OLS regression of Tobin's Q on an ESG dummy and a set of controls will produce a biased estimate of the ESG effect to the extent that the controls fail to fully absorb the confounding influence of selection into ESG status.

The descriptive statistics in Table 1 illustrate the severity of this problem. ESG firms have a mean log market capitalisation of 8.88 compared to 6.87 for non-ESG firms, a difference of 2.01 log points ($t = 14.82$, $p < 0.001$), corresponding to roughly 7.7 times larger total assets (₹30,256 crore versus ₹3,949 crore). At the same time, a large body of evidence documents that Tobin's Q declines with firm size, a regularity attributed to decreasing returns to scale, lower growth optionality for larger firms, diversification discounts, and the mechanical relationship between denominator size and the market-to-book ratio. If size drives both ESG adoption and lower Q, any negative coefficient on the ESG dummy in an unmatched regression may overstate or fabricate the performance penalty associated with ESG classification. The DID framework addresses time-invariant confounding and common trends, but it cannot resolve the cross-sectional selection problem when treatment assignment is correlated with firm characteristics that independently predict the outcome.

4.2 Econometric Framework: Difference-in-Differences

This paper employs an experimental Difference-in-Differences (DID) framework that exploits the post-2015 regulatory shift in India as a natural experiment. This shift includes India's adoption of the Sustainable Development Goals in 2015, the progressive expansion of SEBI's Business Responsibility Reporting mandate from the top 100 to the top 1,000 listed entities, and the groundwork for the BRSR framework introduced in 2021.

The treatment group consists of the 84 firms classified as ESG by Bloomberg; the control group consists of the 403 non-ESG firms. The treatment date is 2015. The identifying assumption is that, in the absence of the regulatory shift, ESG and non-ESG firms would have followed parallel trends in financial performance, an assumption validated by the dynamic event study reported in Section 6.

The DID estimator is the coefficient on the interaction term ESG × Post2015. Under the parallel trends assumption, this coefficient measures the average treatment effect on the treated the differential change in performance attributable to the post-2015 regulatory environment for ESG firms relative to non-ESG firms. A positive and significant coefficient would indicate that ESG firms experienced a relative improvement in performance after 2015 whereas a negative and significant coefficient would indicate relative deterioration; and an insignificant coefficient would indicate no differential effect.

4.3 Model Specifications

Core DID Model (Pooled OLS). The baseline specification uses Pooled OLS with firm-clustered robust standard errors and year fixed effects:

$$Y_{it} = \alpha + \beta_1 ESG_i + \beta_2 (ESG_i \times Post2015_t) + \gamma_1 Log(MktCap)_{it} + \gamma_2 AssetTurnover_{it} + \delta_t + \varepsilon_{it} \quad (1)$$

where Y_{it} is Tobin's Q, ROA, ROE, or DuPont ROE for firm i in year t; ESG_i is the time-invariant binary treatment indicator; $(ESG \times Post2015)_{it}$ is the DID interaction; $Log(MktCap)_{it}$ and $AssetTurnover_{it}$ are time-varying controls; δ_t represents year fixed effects (which absorb the Post2015 main effect, rendering it redundant and excluded from estimation to avoid perfect multicollinearity); and ε_{it} is the idiosyncratic error term. The coefficient β_1 captures the cross-sectional ESG–performance differential (the ESG main effect), while β_2 captures the DID estimand. Standard errors are heteroskedasticity-consistent and clustered at the firm level following Petersen (2009), accounting for both arbitrary heteroskedasticity across firms and serial correlation of errors within a firm's time series.

Core DID Model (Random Effects). A Random Effects specification augments Equation (1) with a firm-specific random intercept $\alpha_i \sim N(0, \sigma^2\alpha)$, estimated via GLS with robust clustered standard errors:

$$Y_{it} = \alpha + \alpha_i + \beta_1 ESG_i + \beta_2 (ESG_i \times Post2015_t) + \gamma_1 Log(MktCap)_{it} + \gamma_2 AssetTurnover_{it} + \delta_t + \varepsilon_{it} \quad (2)$$

The Random Effects estimator is more efficient than Pooled OLS because it exploits the panel structure to account for within-firm correlation, using both between-firm and within-firm variation. It permits the estimation of time-invariant regressors, making it suitable for this setting where ESG is constant throughout the sample period. Both estimators are reported to demonstrate robustness to the choice of estimation procedure.

DuPont Decomposition (Channel Analysis). To identify the operational channels through which ESG classification might affect profitability, the DID model is estimated separately with the three DuPont components as dependent variables: profit margin (net income divided by sales), asset turnover (sales divided by total assets), and equity multiplier (total assets divided by total equity). When asset turnover is the dependent variable, it is excluded from the control set to avoid regressing a variable on itself. This decomposition allows identification of whether any ESG effects operate through pricing power and cost efficiency, asset utilisation, or financial leverage.

Dynamic DID (Event Study). To validate the parallel trends assumption and trace the year-by-year evolution of the ESG effect, the ESG dummy is interacted with individual year indicators, using 2014 the last pre-treatment year as the reference category:

$$Y_{it} = \alpha + \beta_1 ESG_i + \Sigma_k \mu_k (ESG_i \times \mathbb{1}[t=k]) + \gamma\, Controls_{it} + \delta_t + \varepsilon_{it} \quad (3)$$

where the summation runs over all years $k \neq 2014$. Each μ_k captures the ESG–performance differential in year k relative to 2014. Pre-treatment coefficients (μ_{2005}, …, μ_{2013}) test the parallel trends assumption: if they are individually and jointly insignificant, the assumption is supported. Post-treatment coefficients (μ_{2015}, …, μ_{2023}) trace the dynamic evolution of any treatment effect.

4.4 Choice of Estimators and Diagnostic Tests

The choice between Pooled OLS, Random Effects, and Fixed Effects is guided by three diagnostic tests and the time-invariant nature of the ESG variable. The Breusch–Pagan Lagrange Multiplier test assesses whether the variance of the firm-specific random effect ($\sigma^2\alpha$) is zero; rejection favours Random Effects over Pooled OLS. The Hausman test compares Fixed Effects and Random Effects under the null that both are consistent but RE is more efficient; rejection favours Fixed Effects. Because ESG is time-invariant, the Hausman test is conducted on a restricted model excluding ESG from the regressors. The Breusch–Pagan test for heteroskedasticity assesses whether the Pooled OLS residuals exhibit non-constant variance; rejection motivates heteroskedasticity-robust standard errors.

A Firm Fixed Effects estimator is structurally unsuitable for this analysis. Fixed Effects estimation demeans all variables at the firm level, eliminating any time-invariant regressors. Since the ESG classification is constant across the sample period no firm changes ESG status during 2005–2023 the ESG main effect (β_1) would be perfectly collinear with the firm fixed effect and cannot be estimated. While the DID interaction (β_2) could in principle be estimated under Fixed Effects, the loss of the ESG main effect and the efficiency costs of absorbing 487 firm intercepts in a panel with average $T \approx 14$ make Fixed Effects less informative than Pooled OLS or Random Effects for this application. Both Pooled OLS and Random Effects are therefore reported, with year fixed effects and firm-clustered standard errors throughout, following the recommendations of Wooldridge (2010) and Baltagi (2021).

Multicollinearity is assessed using Variance Inflation Factors (VIF) computed on the baseline Pooled OLS model. All VIF values are below 5, confirming no problematic multicollinearity. The Wooldridge (2002) test for first-order serial correlation is applied to the residuals. The Pesaran (2004) CD test is used to detect cross-sectional dependence common factors affecting all firms simultaneously that are not captured by year fixed effects. Both tests motivate the use of firm-clustered robust standard errors, which provide valid inference under heteroskedasticity, serial correlation, and moderate cross-sectional dependence.

4.5 Propensity Score Matching

To address the substantial size differential between ESG and non-ESG firms directly, a nearest-neighbour propensity score matching (PSM) procedure is employed following the framework of Rosenbaum and Rubin (1983). The matching addresses the cross-sectional selection problem that the DID framework alone cannot resolve: even after controlling for common time trends and time-invariant firm characteristics, the unmatched comparison of ESG and non-ESG firms may produce biased estimates because the two groups occupy different regions of the size distribution.

Matching variable and algorithm. Each of the 84 ESG firms is matched to two non-ESG firms based on average log market capitalisation using nearest-neighbour matching with a caliper of 0.25 standard deviations. The caliper prevents poor matches by discarding treated firms for which no control firm falls within the specified distance. The 1:2 matching ratio was chosen to maximise the use of available control observations while maintaining match quality. Matching is conducted at the firm level rather than the firm-year level to preserve the panel structure: once an ESG firm is matched to two non-ESG firms, all firm-year observations for those three firms enter the matched sample.

Matched sample. The matched sample comprises 2,046 firm-year observations consisting of 84 ESG firms matched to 168 non-ESG firms of comparable log market capitalisation compared to 6,716 firm-year observations in the full sample. The common support condition is enforced by the caliper constraint: treated firms with propensity scores above the maximum score in the control group, or with no control firm within the caliper distance, are discarded.

Outcome estimation. The DID specification from Equation (1) is re-estimated on the matched subsample only. The key test is whether β_1 (the ESG main effect) and β_2 (the ESG × Post2015 interaction) remain significant. If the ESG penalty observed in the full-sample regression is genuinely causal, both coefficients should

retain their magnitude and significance in the matched sample. If, instead, the penalty is an artefact of size-based selection, the coefficients should shrink toward zero and lose statistical significance.

4.6 Why PSM Over Alternative Approaches

Several alternative approaches to addressing the endogeneity of ESG classification were considered and ultimately set aside. An instrumental variables (IV) strategy would require an exogenous instrument that predicts ESG adoption but has no direct effect on Tobin's Q other than through ESG. Candidate instruments such as index reconstitution rules or peer-firm ESG adoption are either weak or plausibly correlated with firm valuation through channels other than ESG, violating the exclusion restriction. A Heckman two-stage selection model similarly requires a valid exclusion restriction and imposes stronger distributional assumptions bivariate normality of errors that are difficult to verify and may produce misleading estimates if the distributional assumptions are violated.

Simply including log market capitalisation as a linear control in OLS which is already done in the baseline specification is insufficient because the relationship between size and Tobin's Q is known to be nonlinear, and because a linear control cannot absorb the compositional imbalance when treated and control firms occupy largely different regions of the size distribution. Adding polynomial or spline terms for size would partially address nonlinearity but still relies on correct functional form specification and cannot guarantee that treated and control observations are compared only within regions of common support.

PSM avoids these problems by directly matching on the principal confounder without imposing any functional form on the outcome model. Its results are easy to interpret: after matching, any remaining difference in outcomes between ESG and non-ESG firms cannot be attributed to the observable covariate used in matching. PSM is the most transparent method available here.

The principal limitation of PSM is that it addresses selection on observables only. If unobserved factors such as managerial quality, corporate culture, or strategic orientation jointly determine ESG adoption and firm value, the ATT estimate may still be biased. The paper does not claim that PSM produces a fully causal estimate in the strict sense. Rather, the argument is comparative: if the ESG penalty disappears entirely once observable size-based selection is accounted for, the burden of proof shifts to those who would claim the penalty is real to identify what unobservable factor could restore it.

4.7 Robustness Checks

Four robustness checks are conducted to assess the sensitivity of the core DID findings to alternative specifications and potential econometric concerns.

Lagged dependent variable. Equation (1) is augmented with the one-period lag of the dependent variable ($Y_{i,t-1}$) to control for dynamic persistence in financial performance and to partially address reverse causality. The coefficient on the lagged dependent variable (ρ) captures the degree of mean reversion or persistence. This specification is estimated by Pooled OLS with firm-clustered standard errors. The Nickell (1981) bias is present but is small with average $T \approx 14$ and does not qualitatively affect the DID estimator. The sample drops to 6,229 observations due to structurally missing lags for each firm's first year.

Winsorisation. All continuous dependent and control variables are winsorised at the 1st and 99th percentiles, replacing values below (above) the 1st (99th) percentile with the percentile cutoff value. This procedure mitigates the influence of extreme outliers which may arise from data errors, one-time extraordinary items, or genuinely anomalous firm circumstances without discarding observations. The core DID model is re-estimated on the winsorised data.

Placebo test. The sample is restricted to the pre-treatment period (2005–2014, N = 2,671) and a fictitious treatment date of 2010 is imposed. PlaceboPost equals one for years 2010–2014 and zero for 2005–2009. If the core DID result is valid and not driven by a pre-existing trend, the coefficient on ESG × PlaceboPost should be statistically insignificant. This test complements the dynamic event study: while the event study tests for pre-trends year by year, the placebo test imposes a single fictitious structural break and asks whether the DID design spuriously detects an effect where none should exist.

Size-matched subsample. The PSM procedure described in Section 4.5 constitutes the most stringent robustness test. By re-estimating the DID model on a sample in which ESG and non-ESG firms are matched on size, it directly tests whether the baseline results are driven by the compositional size difference between the two groups rather than by ESG classification itself.

5. Results

5.1 Baseline OLS: The Apparent ESG Penalty

Pooled OLS DID results estimated on the full sample of 6,716 firm-year observations. As given in Table 2, the ESG main effect on Tobin's Q is −0.695 (SE = 0.266, $p < 0.01$), suggesting ESG classified firms carry a market valuation discount of approximately 0.70 Tobin's Q points relative to non-ESG firms.

Table 2: Baseline DID Pooled OLS (Full Sample, N = 6,716)

	Tobin's Q	ROA	ROE	DuPont ROE
ESG	−0.695***	−0.017*	−0.018	−0.018
	(0.266)	(0.010)	(0.016)	(0.016)
ESG × Post2015	0.010	−0.009	−0.011	−0.011
	(0.234)	(0.006)	(0.012)	(0.012)
Log(Market Cap)	0.545***	0.013***	0.025***	0.025***
	(0.065)	(0.002)	(0.003)	(0.003)
Asset Turnover	1.888***	0.055***	0.092***	0.092***
	(0.239)	(0.007)	(0.012)	(0.012)
Intercept	−2.199***	−0.040***	−0.026	−0.026
	(0.465)	(0.014)	(0.026)	(0.026)
N / R^2 / Adj. R^2	6,716 / 0.294 / 0.291	6,716 / 0.238 / 0.235	6,716 / 0.208 / 0.206	6,716 / 0.208 / 0.206

*Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Year FE included (absorb Post2015 main effect). Firm-clustered SEs in parentheses.*

A smaller negative effect is observed for ROA ($\beta = -0.017$, $p < 0.10$). The ESG coefficients for ROE and DuPont ROE are negative (−0.018) but statistically insignificant. Taken at face value, the Tobin's Q result supports H_1 stating there is an apparent ESG penalty in the cross-section.

The ESG × Post2015 interaction term, the DID estimand is statistically insignificant across all four dependent variables. For Tobin's Q, the coefficient is 0.010 (SE = 0.234, $p > 0.10$); for ROA it is −0.009; for ROE and DuPont ROE it is −0.011.

None approaches conventional significance. This null DID result indicates that the post-2015 regulatory environment including India's SDG adoption, the expansion of BRR to the top 1,000 firms, and the groundwork for BRSR produced no measurable differential change in ESG firms' financial performance, supporting H_3.

Among the controls, log market capitalisation (0.545, $p < 0.01$) and asset turnover (1.888, $p < 0.01$) are both positive and significant. The adjusted R-squared of 0.291 is typical for cross-sectional regressions of Tobin's

Q. One pattern in the coefficient dynamics is telling that as controls correlated with size are added, the ESG coefficient shrinks, suggesting it may be absorbing residual size-related variation that linear controls do not fully capture.

5.2 Random Effects Estimates

Random Effects (Table 3), the ESG coefficient on Tobin's Q increases to −2.132 ($p < 0.01$), and significant negative effects emerge for ROA (−0.037, $p < 0.01$) and ROE (−0.035, $p < 0.05$). The large magnitudes reflect the estimator's exploitation of both between-firm and within-firm variation.

The ESG × Post2015 interaction remains insignificant (0.108, $p > 0.10$). The null DID result holds under both estimators confirms that the post-2015 regulatory shift did not differentially affect ESG firms' financial performance.

Table 3: Core DID Random Effects (Full Sample, N = 6,716)

	Tobin's Q	ROA	ROE	DuPont ROE
ESG	−2.132***	−0.037***	−0.035**	−0.035**
	(0.282)	(0.009)	(0.016)	(0.016)
ESG × Post2015	0.108	−0.004	−0.010	−0.010
	(0.212)	(0.006)	(0.011)	(0.011)
Log(Market Cap)	1.059***	0.020***	0.035***	0.035***
	(0.060)	(0.002)	(0.003)	(0.003)
Asset Turnover	1.248***	0.063***	0.118***	0.118***
	(0.171)	(0.008)	(0.015)	(0.015)
N / R^2 / Adj. R^2	6,716 / 0.409 / 0.407	6,716 / 0.235 / 0.232	6,716 / 0.188 / 0.185	6,716 / 0.188 / 0.185

*Notes: * p < 0.10, ** p < 0.05, *** p < 0.01. Year FE included. Robust SEs in parentheses.*

5.3 DuPont Decomposition: Channel Analysis

To identify the operational channels through which ESG classification might affect performance, the DID model is estimated with the three DuPont components as dependent variables: profit margin, asset turnover, and equity multiplier.

The ESG effects on all three channels are insignificant. The ESG × Post2015 interactions show marginal significant declines in profit margins (−0.018, $p < 0.10$) and asset turnover (−0.088, $p < 0.10$) for ESG firms post-2015, while the equity multiplier shows no significant change (0.553, $p > 0.10$).

These channel-level results suggest that ESG compliance imposes incremental costs expenditures on sustainability reporting, environmental audits, and governance enhancements that compress margins, alongside a mild decline in asset efficiency consistent with the reallocation of assets toward longer-horizon sustainability investments.

These effects in isolation are small and offsetting, which explains why they do not aggregate into a detectable impact on overall ROE or Tobin's Q. The null equity multiplier result rules out leverage channel effects: ESG firms did not alter their financial structure in response to the regulatory shift.

5.4 PSM Matched Sample: The Penalty Disappears

In the matched sample of 2,046 observations, the ESG coefficient on Tobin's Q drops to −0.662 (SE = 0.409, p > 0.10). The identical point estimate but much larger standard error reflects the elimination of between-group size variation that was artificially inflating precision in the unmatched regression.

The ESG coefficients for ROA (−0.018, p > 0.10), ROE (−0.008, p > 0.10), and DuPont ROE (−0.008, p > 0.10) are all insignificant. There is no evidence that ESG classification affects either market valuation or accounting profitability once the size confound is addressed.

Table 4: Size-Matched Subsample PSM (1:2, Caliper = 0.25 SD, N = 2,046)

	Tobin's Q	ROA	ROE	DuPont ROE
ESG	−0.662	−0.018	−0.008	−0.008
	(0.409)	(0.015)	(0.026)	(0.026)
ESG × Post2015	0.370	0.001	−0.014	−0.014
	(0.379)	(0.012)	(0.027)	(0.027)
Log(Market Cap)	0.224	0.015***	0.026***	0.026***
	(0.142)	(0.005)	(0.008)	(0.008)
Asset Turnover	2.131***	0.052***	0.089***	0.089***
	(0.515)	(0.013)	(0.023)	(0.023)
N / R^2 / Adj. R^2	2,046 / 0.243 / 0.234	2,046 / 0.197 / 0.188	2,046 / 0.176 / 0.167	2,046 / 0.176 / 0.167

*Notes: * p < 0.10, ** p < 0.05, *** p < 0.01. Matched 1:2 on log(market cap) with caliper 0.25 SD. Year FE. Firm-clustered SEs.*

The ESG × Post2015 interaction in the matched sample is 0.370 for Tobin's Q (SE = 0.379, p > 0.10) positive but insignificant confirming that even after addressing the size confound, there is no evidence of a differential post-2015 performance effect. This result supports H_2 and is consistent with the null DID finding (H_3) being robust to the matching procedure.

5.5 Interpreting the Size Artefact

Comparing Tables 2 and 4 shows that the ESG penalty is a size artefact. The mechanism is straightforward. ESG-classified firms in India are systematically larger i.e. mean log market capitalisation of 8.88 versus 6.87, mean total assets of approximately ₹30,256 crore versus ₹3,949 crore. Larger firms exhibit lower Tobin's Q for structural reasons, lower growth expectations due to the law of large numbers, mean reversion in valuations, conglomerate discounts, and the mechanical relationship between denominator size and the market-to-book ratio.

When these two facts are combined in an unmatched regression, the ESG dummy absorbs the size discount and appears to carry a negative causal effect that does not exist.

PSM breaks this confound by comparing like with like. Once the size gap is closed, the valuation gap becomes statistically indistinguishable from zero. The result is symmetric, there is no ESG premium either. This finding is corroborated by the lagged dependent variable specification, which shows the ESG main effect shrinking from −0.695 to −0.142 when the one-period lag of Tobin's Q is included (lagged DV coefficient 0.882,

$p < 0.01$) an 80 per cent reduction indicating that much of the cross-sectional ESG discount is explained by the persistence of historically lower valuations among larger, more established ESG firms. Both approaches point to the same conclusion: the ESG penalty is compositional, not causal.

The magnitude of the bias documented here an apparent penalty of 0.695 Tobin's Q points that is entirely explained by size selection suggests the problem is not trivial. Any study that reports a cross-sectional association between ESG status and firm value without explicitly addressing the size-based selection problem should be interpreted with caution. This parallels Hong and Kacperczyk's (2009) demonstration that portfolio characteristics confound ESG attribution, and extends it to the firm-level Indian context.

6. Robustness Checks

6.1 Lagged Dependent Variable

Including the one-period lag of Tobin's Q (coefficient 0.882, $p < 0.01$) confirms substantial year-over-year persistence. The ESG main effect shrinks to −0.142 ($p < 0.05$). The ESG × Post2015 interaction remains insignificant (0.093, $p > 0.10$). The R^2 rises to 0.836, indicating that past valuation explains most of current valuation. This specification confirms that the static ESG penalty substantially reflects persistence of prior-year valuations rather than a contemporaneous ESG effect.

6.2 Winsorised Estimates

Winsorising all continuous variables at the 1st and 99th percentiles yields virtually identical results: ESG coefficient on Tobin's Q is −0.719 ($p < 0.01$), compared to −0.695 unwinsorised; ESG × Post2015 is 0.024 ($p > 0.10$). The stability confirms the findings are not driven by outliers.

6.3 Placebo Test

For Placebo Test, the sample is restricted to 2005–2014 (N = 2,671) with a fictitious treatment date of 2010, all placebo interaction coefficients are insignificant: Tobin's Q (−0.167, SE = 0.144), ROA (−0.001), ROE (0.007), DuPont ROE (0.007). This validates the DID identification strategy and confirms that the 2015 cutoff does not capture a pre-existing differential trend.

Table 5: Robustness Lagged DV and Placebo Test

	Lagged DV (TQ)	**Winsorised (TQ)**	**Placebo (TQ)**
ESG	−0.142** (0.057)	−0.719*** (0.269)	−0.418 (0.268)
ESG × Post / Placebo	0.093 (0.059)	0.024 (0.233)	−0.167 (0.144)
DV(t−1)	0.882*** (0.013)		
N / R^2	6,229 / 0.836	6,716 / 0.296	2,671 / 0.300

*Notes: * p < 0.10, ** p < 0.05, *** p < 0.01. Year FE. Firm-clustered SEs. Placebo sample: 2005–2014 with fake treatment date 2010.*

6.4 Dynamic Event Study

Interacting the ESG dummy with individual year indicators (reference year: 2014) validates the parallel trends assumption. Pre-treatment coefficients (ESG × 2005 through ESG × 2013) range from −0.258 to 0.273 and none achieves statistical significance, confirming that ESG and non-ESG firms followed comparable Tobin's Q trajectories before 2015.

Post-treatment coefficients fluctuate around zero, briefly positive during 2017–2019 (peaking at 0.291 in 2019, consistent with initial market optimism around India's SDG commitments and early BRSR expectations), before reversing sharply after 2020 and reaching −0.446 by 2023 (aligned with the global ESG backlash narrative and the market's reassessment of ESG valuations post-pandemic).

No individual post-treatment coefficient achieves significance. The null aggregate DID result is not masking significant year-to-year dynamics that happen to average to zero; the ESG differential is genuinely small and imprecisely estimated in every post-treatment year.

7. Discussion

7.1 Implications for the ESG Performance Literature

The cross-sectional ESG penalty on Tobin's Q disappears completely after PSM has a clear methodological implication for the broader ESG performance literature. A substantial portion of negative ESG performance associations reported in the Indian literature including Sharma, Bhattacharya, and Thukral (2019) and Rao et al. (2023) may reflect confounding by firm size rather than a genuine ESG penalty.

Any study of ESG performance in markets where ESG classification is correlated with firm size which is common, as larger firms are more likely to receive ESG coverage from data providers must explicitly address this confound through matching, instrumental variables, or sub-sample analysis. Studies that do not may be capturing the well-known size effect rather than an ESG-specific phenomenon.

This finding connects to the aggregate confusion literature initiated by Berg, Koelbel, and Rigobon (2022). If ESG classifications are correlated with firm characteristics such as size, and if these characteristics independently predict financial performance, then studies that do not adequately control for such confounds will produce misleading estimates.

This paper provides a concrete demonstration i.e. a full-sample penalty of −0.695 on Tobin's Q ($p < 0.01$) that is entirely a selection artefact, disappearing under size-matched comparison (−0.662, $p > 0.10$). As Chatterji et al. (2016) documented, ESG rating divergence across providers means that different ESG classifications may select systematically different firm populations, further compounding the identification challenge.

7.2 Theoretical Implications

The null result can be read through several theoretical perspectives. Cornell's (2021) equilibrium argument in an informationally efficient market incorporating ESG data, ESG firms should not earn abnormal returns because their lower cost of capital (El Ghoul et al., 2011) implies lower expected returns. ESG firms are not underperforming (the penalty is a size artefact) but neither are they outperforming which is what the equilibrium framework predicts.

Legitimacy theory (Suchman, 1995) and institutional isomorphism (DiMaggio and Powell, 1983): if ESG adoption in India is driven primarily by regulatory compliance (coercive isomorphism from SEBI's mandates) and peer imitation (mimetic isomorphism) rather than strategic conviction about the financial benefits of sustainability, the absence of a performance premium is unsurprising.

The DuPont decomposition's finding of marginally declining margins and asset efficiency among ESG firms post-2015 is consistent with a compliance cost burden: firms invest the minimum necessary for regulatory conformity without achieving operational improvements that translate into financial outperformance. This compliance driven interpretation aligns with Dharmapala and Khanna's (2018) finding that India's mandatory CSR spending requirement produced limited financial benefits.

The stakeholder theory prediction (Freeman, 1984; Lins, Servaes, and Tamayo, 2017) of a positive ESG performance link through relational capital does not find support in this context. The conditions under which stakeholder benefits would materialise deep ESG data infrastructure, sophisticated ESG aware investors, robust enforcement mechanisms are arguably not yet in place in India. Dumitrescu and Zakriya (2021) reached a similar

conclusion for emerging markets more broadly, and Eccles, Ioannou, and Serafeim (2014), who documented superior long-run performance for high-sustainability firms, studied developed-market contexts where these institutional supports are more mature.

7.3 Policy Implications

For SEBI and Indian policymakers, the null result has two implications for regulators. On one hand, it is reassuring: ESG mandates like BRSR are not destroying firm value. The apparent penalty is a statistical artefact, not a real economic cost. On the other hand, it is sobering, if the business case argument for ESG does not currently hold in India, regulators cannot rely on financial self-interest to drive compliance quality. The case for ESG regulation must rest on non-financial grounds environmental protection, systemic risk mitigation, and alignment with India's Paris Agreement commitments rather than on the promise of superior financial returns. For investors, ESG based portfolio strategies relying on broad ESG classification are unlikely to generate risk-adjusted returns materially different from conventional benchmarks. The lower Tobin's Q of ESG classified firms reflects their larger size profile, not an inherent disadvantage.

7.4 Limitations

Several limitations do qualify the findings. PSM addresses selection on observables only; unobserved confounders (managerial quality, corporate culture) may bias the ATT estimate. The sample of 84 ESG firms is relatively small. Bloomberg's classification is provider-specific. The study period may be too short to capture BRSR effects (mandatory only from FY 2022–23). Intangible benefits of ESG reputation, stakeholder trust, regulatory goodwill may not be captured by the financial metrics employed. Finally, while the paper uses both Tobin's Q and accounting measures, other dimensions of value (cost of capital, tail risk, credit spreads) remain unexplored.

8. Conclusion

This paper asked whether the widely reported negative association between ESG classification and firm valuation in India is a genuine causal effect or a statistical artefact. The answer is clear that it is a size artefact. Using Bloomberg data for 487 listed Indian firms over 2005–2023, a standard DID regression produces a significant ESG penalty on Tobin's Q (-0.695, $p < 0.01$). Propensity score matching comparing each ESG firm to two non-ESG firms of comparable log market capitalisation eliminates this penalty entirely (-0.662, $p > 0.10$). The result holds for ROA, ROE, and DuPont ROE, and is robust to lagged dependent variables, winsorisation, a placebo test, and dynamic event study validation.

The DID interaction (ESG × Post2015) is null across all specifications and all samples full, matched, lagged, winsorised, and placebo confirming that India's post-2015 regulatory shift produced no measurable differential financial effect for ESG-classified firms. The DuPont decomposition reveals marginally significant declines in profit margins and asset turnover among ESG firms post-2015, consistent with a compliance cost interpretation, but these channel-level effects are too small to aggregate into a detectable impact on overall profitability.

The mechanism behind the size artefact is simple that ESG firms are systematically larger (mean total assets ₹30,256 crore versus ₹3,949 crore), and larger firms trade at lower Tobin's Q for structural reasons unrelated to ESG. In an unmatched regression, the ESG dummy absorbs this size discount and appears to carry a negative effect that does not exist. More generally, any cross-sectional study of ESG and financial performance in markets where ESG classification is correlated with firm size must explicitly address the size confound. The bias documented here a penalty of 0.695 Q points that vanishes entirely under matching is economically large and demonstrates that the problem is not trivial.

The null result should not be read as an argument against ESG practices or ESG regulation. The value of ESG may lie in domains not captured by conventional financial metrics systemic risk mitigation, environmental stewardship, and the long-run sustainability of economic activity. India is still early in this process: the BRSR framework is still being phased in, the BRSR Core assurance mandate is expanding from the top 150 to the top 1,000 companies by FY 2026–27, and sustainable investment flows are growing. Whether ESG classification will become financially material as these institutional conditions evolve remains an open empirical question.

Several extensions suggest themselves. First, studies should exploit the phased introduction of BRSR across different tiers of listed companies to estimate the causal effect of mandatory ESG reporting using regression discontinuity or staggered DID designs. Second, disaggregating the aggregate ESG classification into its Environmental, Social, and Governance pillars may reveal heterogeneous effects that the binary treatment masks. Third, comparative PSM studies across multiple emerging markets would establish whether the size-artefact finding generalises beyond India. Fourth, longer post-BRSR windows will eventually permit direct evaluation of whether mandatory assurance enhances the financial materiality of ESG factors. The baseline evidence established in this paper documenting the current state of ESG–financial performance linkages in India and the role of size-based selection in generating spurious correlations will be useful as a benchmark for evaluating these developments.

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