

Digitalising Is Not Digitalising for Sustainability: Decomposing the Digital Transformation–ESG Relationship in African Listed Firms

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Abstract

Purpose and problem.

Digital transformation is widely promoted as a lever for corporate environmental, social and governance (ESG) performance, yet the empirical record is contradictory. An influential strand of that literature attributes the inconsistency to nonlinearity, proposing that digitalisation depresses ESG performance before improving it. This study tests that proposition directly and, finding no support for it, offers an alternative explanation grounded in what firms actually do with digital capability.

Design and methods.

We analyse a balanced panel of 80 firms listed on the Johannesburg, Nigerian, Nairobi and Ghana exchanges over 2015–2024 (800 firm-year observations). We estimate two-way fixed-effects models and apply the Lind–Mehlum test for U-shaped relationships and a bootstrapped Hansen threshold search, the two procedures the nonlinearity claim requires but which prior work in this area has largely omitted. We then decompose digital transformation into digital intensity and the orthogonal component of digital capability that is purposively directed at sustainability processes, and validate the decomposition using difference GMM, quantile regression, instrumental variables, subsample exclusions and lagged specifications.

Findings.

The nonlinear hypothesis is rejected on both tests (U-test $p = 0.320$; threshold bootstrap $p = 0.707$). Digital intensity on its own bears no relationship to ESG performance ($\beta = 0.029$, $p = 0.952$). Sustainability-purposed digital integration, by contrast, is robustly and positively associated with ESG performance ($\beta = 4.494$, $p < 0.001$), and holding it constant reveals a positive intensity effect ($\beta = 2.556$, $p < 0.001$) that the aggregate measure conceals. Climate risk attenuates rather than amplifies the intensity pathway ($\beta = -0.360$, $p = 0.025$); institutional quality and state ownership do not moderate either pathway. Effects hold across all three ESG pillars and the whole ESG distribution, but do not survive a one-year lag.

Value.

The contradictory findings in this literature are better explained by measurement aggregation than by nonlinearity: composite digital transformation indices mix a component that moves ESG performance with one that does not, so the estimated effect depends on the mix. We report a null on the headline construct, three refuted moderation hypotheses and one robust positive result, and we disclose openly that the analysis rests on a partly synthetic dataset, which bounds the inferential claims accordingly.

1 Introduction

Firms are simultaneously asked to digitalise and to improve their environmental, social and governance (ESG) performance, and it is widely assumed the two agendas reinforce each other. Digital technologies promise finer-grained environmental measurement, more efficient resource allocation, broader stakeholder reach and more verifiable disclosure [1, 40]. Reviews of the field document rapid growth in studies linking digital transformation to sustainability outcomes [5, 6, 18, 21], and much recent firm-level evidence is encouraging [2, 25, 26, 27, 30].

The evidence is not, however, consistent. Positive associations dominate in Chinese and Gulf samples [2, 25, 30, 32], while other work reports weak, conditional or absent effects, and studies of the productivity paradox have long cautioned that technology investment and measured performance can decouple for extended periods [23, 48]. Non-monotone ESG–performance patterns have also been documented directly [33, 34], and ESG has been shown to affect different performance outcomes in contrasting directions within the same firms [28], and disclosure-based ESG measures respond to regime and pandemic shocks in ways that complicate comparison over time [14, 35]. Two recent contributions are particularly instructive: Wang and Tang [22] show that symbolic digital innovation and substantive digital innovation have sharply different consequences for ESG performance, and Sun and He [23] find the digital-green link at the firm level to be far weaker than macro-level narratives imply. Both suggest that what firms do with digital capability matters more than how much of it they have.

A prominent explanation for the inconsistency is nonlinearity, the claim that digitalisation depresses ESG performance during a costly transition phase and improves it afterwards, producing a U-shaped curve with an identifiable turning point. The proposition is intuitive and theoretically attractive. It is also, we argue, undertested. Studies advancing it typically infer the curve from the signs and significance of a linear and a squared term, a practice known for fifteen years to be insufficient: a significant quadratic is entirely compatible with a relationship that is monotone across every value the data contain [53, 54]. The tests that would settle the question the Lind–Mehlum procedure and a properly bootstrapped threshold search [55], are rarely reported.

This study applies those tests to a balanced panel of 80 firms listed on four African exchanges over 2015–2024 and finds no nonlinearity of any kind. Rather than treat this as a null result of limited interest, we use it to motivate a different account. We decompose the composite digital transformation index into two orthogonal components: digital intensity, capturing how extensively a firm has adopted digital technology, and sustainability-purposed digital integration, capturing the portion of digital capability explicitly directed at sustainability processes. The decomposition is consequential. Digital

intensity alone bears no relationship to ESG performance; the integration component bears a strong one; and once integration is controlled, intensity becomes positive. The aggregate null is therefore not an absence of effect but an averaging of two components that behave differently.

We make four contributions. First, we provide what is, to our knowledge, the first formal test of the nonlinear digital transformation-ESG hypothesis using the Lind-Mehlum and Hansen procedures together, and we report its rejection. Second, we offer a measurement-based rather than functional-form-based explanation for the contradictory findings in this literature. Third, we test and refute three widely assumed contingencies, climate risk amplification, institutional quality complementarity and state-ownership buffering, in an African setting where all three have been asserted but seldom examined jointly. Fourth, we extend an evidence base overwhelmingly concentrated on China to four African markets at very different stages of sustainability regulation [65, 66].

A note on scope is necessary at the outset. The dataset underlying this study contains a substantial number of variables that are synthetically generated rather than archivally collected, including the digital transformation index itself. We disclose this in full in Sect. 3.1 and Sect. 6.3, and we frame the study accordingly: as a methodological demonstration of how decomposition and formal nonlinearity testing change the conclusions one draws, rather than as a definitive empirical account of African firms. The methodological contribution does not depend on the provenance of the data; the substantive generalisations do, and we do not make them.

The rest of the paper proceeds as follows. Section 2 develops the theoretical argument and hypotheses. Section 3 describes the data and empirical strategy. Section 4 presents results. Section 5 discusses implications, and Sect. 6 concludes with limitations.

2 Theory and hypotheses

2.1 Digital transformation and ESG performance

Digital transformation denotes the integration of digital technology across an organisation such that value creation and delivery are fundamentally altered [1]. ESG performance denotes a firm's aggregate standing on environmental stewardship, social responsibility and governance quality [39, 52]. Its measurement is contested: ratings diverge substantially across providers [50], disclosure regimes differ in stringency [11, 51], and reported ESG-performance relationships are sensitive to market development and industry sensitivity [14, 38]. The resource-based and natural-resource-based views suggest a positive link: digital capability is a difficult-to-imitate resource that can be orchestrated toward environmental ends [41, 42]. Dynamic capabilities theory adds that firms with stronger sensing, seizing and reconfiguring routines convert technological resources into environmental outcomes more effectively [43, 44]. Green innovation and environmentally oriented leadership are the routines most often identified as carrying this conversion [62, 63, 64], and stakeholder theory supplies the demand-side rationale for undertaking it at all [46]. Empirically, digital transformation has been associated with better ESG outcomes in Saudi [2], Chinese [25, 30, 32] and European samples, with digital merger and acquisition activity [27], and with green innovation and knowledge-sharing mechanisms in emerging-market SMEs [3, 4, 20]. Related work links ESG practice to financial performance through green innovation in developing economies [13], to environmentally focused operational practice [15], and to supply-chain resilience [19].

Yet the same theories admit an alternative reading. The resource-based view predicts returns to resources that are deployed toward a purpose, not to resources held. A firm can be digitally intensive with cloud infrastructure, enterprise systems, analytics capacity but without directing any of it at emissions measurement, supply-chain due diligence or stakeholder engagement. Dynamic capabilities theory is explicit on this point: capability is defined by the reconfiguring activity, not the asset stock [43]. On this reading, digital intensity should predict ESG performance only insofar as it is accompanied by sustainability-directed deployment.

The distinction has an empirical counterpart. Wang and Tang [22] separate substantive from symbolic digital innovation and find only the former improves ESG performance. Delmas and Burbano [49] show that firms can expand the apparatus of environmental communication without changing environmental behaviour, and Conti and Seele [7] document a widening gap between sustainability 'talk' and sustainability 'walk'. Composite digital transformation indices, which sum adoption across technologies without regard to purpose, cannot distinguish these cases. If the two components differ in their relationship to ESG performance, the estimated coefficient on a composite index will vary with the sample's mix of them, which is precisely the pattern of inconsistent findings the literature exhibits.

We therefore expect the composite measure to perform poorly and the decomposed measures to perform differently from one another.

H1 *Digital intensity, measured as a composite index of digital technology adoption, is not systematically associated with ESG performance.*

H2 *Sustainability-purposed digital integration, the component of digital capability directed at sustainability processes and orthogonal to digital intensity – is positively associated with ESG performance.*

2.2 Is the relationship nonlinear?

An influential alternative explanation holds that the relationship is nonlinear rather than mismeasured. On this account, digital transformation imposes transition costs, capital outlays, organisational disruption, managerial attention diverted from sustainability, that depress ESG performance in early phases, after which maturing capability delivers gains. The resulting curve is convex with an interior minimum. Paradox theory [45] is commonly invoked in support, on the grounds that firms face competing performing and organising demands that they resolve over time.

We treat this as a hypothesis to be tested rather than a premise. Two concerns motivate scepticism. Theoretically, a transition-cost mechanism predicts an effect of time since adoption, not of adoption intensity measured cross-sectionally; a firm scoring low on a digital intensity index may be early in its journey or may simply have chosen a less digital operating model, and an intensity index cannot separate them. Methodologically, the evidence advanced for the curve is almost always the sign pattern of a linear and a squared term. Lind and Mehlum [53] show this is not a test of the hypothesis: what must be demonstrated is that the fitted slope is negative at the lower bound of the data, positive at the upper bound, and that the extremum lies inside the observed range. Haans and colleagues [54] make the same point for strategy research and extend it to moderated curves.

H3 *The relationship between digital transformation and ESG performance is not U-shaped: the extremum of the fitted quadratic does not lie within the observed data range, and no significant threshold exists.*

2.3 Climate risk as a contingency

Firms with greater exposure to climate-related physical and transition risk face sharper stakeholder scrutiny and more binding environmental constraints [16, 37]. Sectoral exposure also shapes what firms disclose and how [12, 17], and biodiversity- and taxonomy-related reporting is expanding the range of environmental dimensions on which they are assessed [8]. A common expectation is that such firms extract greater ESG returns from digitalisation, because urgency channels digital investment toward environmental applications.

We expect the opposite. High climate-risk firms in these markets are concentrated in extractive, utility and heavy-industry sectors whose environmental performance is determined largely by physical asset configuration, plant vintage, fuel mix, extraction method, which digital tools measure but do not change. Where the binding constraint is physical rather than informational, the marginal ESG return to digital capability should be lower, not higher. Climate risk should therefore attenuate the digital intensity pathway.

H4 *Climate risk attenuates the association between digital intensity and ESG performance.*

2.4 Institutional quality and state ownership

Institutional theory holds that regulatory quality, rule of law and government effectiveness condition how firms respond to sustainability pressure [47], and cross-country evidence links governance quality to sustainability outcomes [11, 12]. Related macro-level work finds institutional and political conditions to shape environmental outcomes nonlinearly [9, 10]. State ownership is likewise argued to matter, providing policy access and resource buffers while imposing political objectives [31, 32].

We test both as moderators without a directional prior. The relevant identifying variation is limited: institutional quality varies at the country-year level across only four countries, and once firm and year fixed effects are included the residual variation is thin. Prior claims in this area are frequently asserted rather than tested with adequate power, and we report the results as a check on those claims rather than as a positive contribution.

H5 *Institutional quality moderates the association between digital transformation components and ESG performance.*

H6 *State ownership moderates the association between digital transformation components and ESG performance.*

3 Data and methods

3.1 Sample and data provenance

The sample is a balanced panel of 80 firms listed on four African exchanges, the Johannesburg Stock Exchange (35 firms), Nigerian Exchange Group (20), Nairobi Securities Exchange (15) and Ghana Stock Exchange (10), observed annually from 2015 to 2024, yielding 800 firm-year observations with no missing values. Sector composition is consumer goods (39 firms), financial services (25), energy and utilities (4), manufacturing (4), telecommunications (4), mining and extractives (2) and technology (2).

Transparency on data provenance. The workbook underlying this analysis documents that a substantial subset of its variables, including the digital transformation index, the AI-CSR integration score, state ownership, taxonomy alignment and several disclosure indicators, are synthetically generated rather than collected from filings or commercial databases. We state this prominently rather than in a footnote because it determines how the results should be read. The estimates below are internally valid descriptions of relationships within this dataset and they demonstrate how decomposition and formal testing alter the conclusions an analyst would reach; they are not evidence about the population of African listed firms. Replication on archival data, for example a manually coded digital transformation index of the kind constructed by Bajaher and Asiri [2], is a precondition for substantive generalisation, and we identify it in Sect. 6.3 as the necessary next step. We report the analysis in full because the methodological point it establishes is independent of provenance: the same data that appear to support a nonlinear story under conventional practice reject it under correct testing.

Continuous variables are winsorised at the 1st and 99th percentiles.

3.2 Variables

Dependent variable. ESG performance is the firm's composite corporate sustainability score, scaled 0–100 (mean 59.16, SD 14.15). Note that this is the composite score, not the binary enhancement flag also present in the workbook; the two are distinct and only the former is a graded performance measure. Environmental, social and governance sub-scores are analysed separately in Sect. 4.5.

Digital intensity (DT) is the composite digital transformation index, scaled 0–10 (mean 6.19, SD 1.14, range 3.75–8.59), capturing breadth of digital technology adoption, infrastructure and capability.

Sustainability-purposed digital integration (SDI) is constructed in two steps. We begin with the firm-year score for the integration of digital and artificial-intelligence capability into sustainability reporting and management processes (mean 7.68, SD 1.39). Because this measure correlates 0.76 with digital intensity, entering both directly produces unstable estimates. We therefore regress it on digital intensity and retain the residual, which is by construction orthogonal to DT and is interpretable as the degree to which a firm's sustainability-directed digital capability exceeds what its overall digital intensity would predict. This residualisation reduces the variance inflation factor on the pair from 3.11/3.59 to 1.73/1.49. Section 4.6 reports the non-residualised specification for transparency.

Moderators. Climate risk is a 0–10 exposure score (mean 5.26); institutional quality is a country-year governance measure (mean 6.16); state ownership is a binary indicator (mean 0.31).

Controls. Firm size (log total assets), log firm age, leverage, return on assets, board size, board independence, CEO duality, Big-4 auditor, institutional ownership and state ownership. Firm size and ROA correlate at – 0.87 in this sample; we report variance inflation factors in Sect. 4.6 and note the resulting imprecision on both coefficients rather than interpreting either as an economic effect.

3.3 Estimation

Our baseline is a two-way fixed-effects specification:

$$ESG_{it} = \alpha + \beta_1 DT_{it} + \beta_2 SDI_{it} + \gamma' X_{it} + \mu_i + \lambda_t + \varepsilon_{it}$$

where μ_i and λ_t are firm and year effects and standard errors are clustered by firm [61]. Firm fixed effects absorb country and industry, which are time-invariant in this panel; we therefore do not report them separately, unlike specifications that list both firm effects and industry dummies. Firm age enters in logs because linear age is perfectly collinear with firm and year effects in a balanced panel.

Nonlinearity testing. For H3 we estimate the quadratic specification and then apply the Lind–Mehlum test [53]: we compute the fitted slope and its standard error at the minimum and maximum of the observed DT range and test the joint hypothesis that the slope is negative at the lower bound and positive at the upper. We separately conduct a Hansen-style single-threshold search [55] on the within-transformed data over a grid spanning the 15th to 85th percentiles of DT, with the threshold p-value obtained from 300 bootstrap replications.

Dynamic specification. Because ESG performance is persistent and digital investment may respond to it [24], we estimate a difference GMM model [56, 57] instrumenting the lagged dependent variable and both digital measures with deeper lags, and report the Hansen J statistic and an AR(2) test on the differenced residuals [58, 59, 60].

Additional estimators. We report random effects, pooled OLS with industry, country and year dummies, quantile regression at the 10th, 25th, 50th, 75th and 90th percentiles, two-stage least squares using leave-one-out country-year peer digital intensity as an instrument, and specifications excluding the financial sector, the COVID-19 period and South Africa. We also report a one-year-lagged specification, which we treat as the decisive test of whether the association is predictive rather than contemporaneous.

4 Results

4.1 Descriptive statistics

Table 1 reports descriptive statistics and Table 2 the correlation matrix. Digital intensity rises steadily over the period, from a mean of 5.69 in 2015 to 6.89 in 2024, and sustainability-purposed integration rises from 7.21 to 8.46, with a visible step change in 2020. ESG scores peak in 2020–2021 and decline thereafter (Fig. 2). Digital intensity correlates with ESG performance at 0.22, while the integration measure correlates at 0.63, a first indication that the two components behave differently.

Table 1
Descriptive statistics (N = 800 firm-year observations, 80 firms, 2015–2024)

Variable	Mean	SD	Min	P25	Median	P75	Max
ESG performance (0–100)	59.16	14.15	27.58	48.04	59.59	71.67	79.00
Environmental sub-score	16.38	4.35	6.03	13.31	16.91	19.57	25.08
Social sub-score	22.47	5.75	9.40	18.16	22.67	27.00	33.36
Governance sub-score	22.74	5.38	9.91	18.80	22.96	27.10	32.68
Digital intensity, DT (0–10)	6.19	1.14	3.75	5.33	6.13	7.10	8.59
Digital–sustainability integration (0–10)	7.68	1.39	4.74	6.63	7.65	8.70	10.00
Climate risk (0–10)	5.26	1.31	2.20	4.36	5.22	6.07	8.60
Institutional quality	6.16	0.99	4.10	5.47	6.21	6.89	7.93
Firm size (ln assets)	9.58	1.03	7.44	9.00	9.68	10.24	11.43
Firm age (years)	15.99	5.06	6.00	12.00	16.00	20.00	29.00
Leverage (debt/equity)	0.81	0.41	0.12	0.46	0.80	1.17	1.49
Return on assets	0.150	0.112	–0.047	0.073	0.126	0.217	0.350
Board size	10.59	2.26	7.00	9.00	11.00	13.00	14.00
Board independence	0.554	0.141	0.309	0.436	0.548	0.673	0.797
CEO duality	0.289	0.453	0	0	0	1	1
Big-4 auditor	0.625	0.484	0	0	1	1	1
Institutional ownership	0.399	0.172	0.106	0.254	0.390	0.550	0.692
State ownership	0.305	0.461	0	0	0	1	1

Continuous variables winsorised at the 1st and 99th percentiles. The panel is balanced: every firm is observed in all ten years.

Table 2
Pairwise correlations

(1) ESG performance	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1.00							
(2) Digital intensity	0.22***	1.00						
(3) Digital–sust. integration	0.63***	0.76***	1.00					
(4) Climate risk	–0.15***	–0.61***	–0.51***	1.00				
(5) Institutional quality	0.35***	0.00	0.14***	–0.04	1.00			
(6) Firm size	0.63***	0.24***	0.49***	–0.11***	–0.05	1.00		
(7) Return on assets	–0.60***	–0.19***	–0.44***	0.09**	0.07*	–0.87***	1.00	
(8) State ownership	–0.04	–0.02	–0.02	0.05	–0.04	0.03	–0.02	1.00

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The – 0.87 correlation between firm size and return on assets is noted in Sect. 3.2; both coefficients are correspondingly imprecise and neither is interpreted as an economic effect.

4.2 The nonlinear hypothesis is rejected

Table 3 reports the baseline sequence. Model 1 includes controls only. Model 2 adds digital intensity, which is indistinguishable from zero ($\beta = 0.029$, SE 0.488, $p = 0.952$). Model 3 adds the squared term: neither the linear ($\beta = -1.182$, $p = 0.588$) nor the quadratic term ($\beta = 0.098$, $p = 0.574$) is significant, and the within- R^2 rises by 0.001.

The formal test is decisive. The implied extremum lies at $DT = 6.01$, which is inside the observed range, but the Lind–Mehlum test fails to reject monotonicity: the fitted slope at the lower bound is – 0.444 ($t = -0.47$, $p = 0.320$) and at the upper bound 0.508 ($t = 0.50$, $p = 0.307$), giving an overall U-test p -value of 0.320. The 95% confidence band around the marginal effect contains zero at every value of DT in the sample (Fig. 3b). The Hansen threshold search reinforces this: the best-fitting threshold is $\gamma = 6.33$, but the bootstrap p -value is 0.707, and the estimated slopes on either side (0.522 below, – 0.584 above) are individually insignificant and of the wrong signs for the hypothesised pattern.

H3 is supported: there is no U-shape, no inverted U-shape and no threshold in these data. Figure 3a makes the point visually, the fitted quadratic is nearly flat and its confidence band is wide enough to accommodate almost any monotone relationship. This result matters beyond the present sample, because it demonstrates how readily a quadratic specification can be reported as evidence of a turning point when the underlying relationship contains none.

Table 3
Baseline two-way fixed-effects estimates and the test for nonlinearity

Variable	Model 1 (controls)	Model 2 (+ DT)	Model 3 (+ DT ²)
Digital intensity (DT)		0.029 (0.488)	−1.182 (2.177)
DT ²			0.098 (0.175)
Firm size	0.988 (0.684)	0.989 (0.683)	1.000 (0.682)
Log firm age	12.397*** (3.822)	12.417*** (3.839)	12.226*** (3.849)
Leverage	−0.077 (0.531)	−0.077 (0.532)	−0.089 (0.531)
Return on assets	−1.506 (7.074)	−1.523 (7.074)	−1.593 (7.027)
Board size	−0.086 (0.085)	−0.086 (0.084)	−0.087 (0.084)
Board independence	3.783** (1.819)	3.781** (1.811)	3.815** (1.814)
CEO duality	0.019 (0.553)	0.018 (0.551)	0.029 (0.550)
Big-4 auditor	0.167 (0.460)	0.167 (0.460)	0.181 (0.462)
Institutional ownership	0.309 (1.187)	0.307 (1.183)	0.347 (1.191)
State ownership	0.008 (0.441)	0.006 (0.444)	−0.008 (0.439)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	800	800	800
Within R ²	0.0301	0.0292	0.0303
Implied extremum (DT)	—	—	6.01
Lind–Mehlum U-test (p)	—	—	0.320
Slope at DT minimum	—	—	−0.444 (p = 0.320)
Slope at DT maximum	—	—	0.508 (p = 0.307)
Hansen threshold γ	—	—	6.33
Threshold bootstrap p	—	—	0.707

Standard errors clustered by firm in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Firm and country are time-invariant and absorbed by firm fixed effects. Firm age enters in logs because linear age is collinear with firm and year effects in a balanced panel. The threshold search uses a grid over the 15th–85th percentiles of DT with 300 bootstrap replications.

4.3 Decomposition: intensity versus integration

Table 4 reports the decomposition. Model 1 repeats the digital intensity null. Model 2 enters sustainability-purposed integration alone: the coefficient is 3.655 ($p < 0.001$) and within- R^2 rises from 0.030 to 0.222, a sevenfold increase in explained within-firm variation. Model 3 enters both. Integration strengthens to 4.494 ($p < 0.001$) and digital intensity turns positive and significant at 2.556 ($p < 0.001$).

The sign change is the substantive finding. It is not a collinearity artefact, the two regressors are orthogonal by construction and their VIFs are 1.73 and 1.49, but a suppression effect with a clear reading. Within firms, years in which digital intensity rises faster than sustainability-directed deployment show no ESG gain, because the positive intensity effect is offset by the shortfall in integration. Holding integration constant isolates, the intensity effect and reveals it. The aggregate null in Model 1 is thus an average across firm-years in which the two components move together and firm-years in which intensity outruns integration.

In economic terms, a one-standard-deviation increase in integration (1.39 units on the underlying scale) is associated with roughly 4.5 points on the 100-point ESG scale, against a within-firm standard deviation of 6.08, an effect of about three-quarters of a within-firm standard deviation. H1 and H2 are both supported.

Model 4 adds the interaction between the two components, which is insignificant ($\beta = 0.029$, $p = 0.921$). The pathways are additive: integration does not require high digital intensity to pay off, and intensity does not require integration to be present in order to contribute once integration is held constant. This is consistent with Wang and Tang's [22] finding that the substantive and symbolic dimensions of digital innovation operate through separate channels.

Table 4
Decomposition: digital intensity versus sustainability-purposed digital integration

Variable	Model 1	Model 2	Model 3	Model 4
Digital intensity (DT)	0.029 (0.488)		2.556*** (0.437)	2.555*** (0.439)
Digital–sust. integration (SDI)		3.655*** (0.330)	4.494*** (0.340)	4.315** (1.770)
DT × SDI				0.029 (0.297)
Firm size	0.989 (0.683)	0.855 (0.585)	0.891 (0.590)	0.891 (0.591)
Log firm age	12.417*** (3.839)	7.145** (3.389)	7.685** (3.411)	7.683** (3.415)
Leverage	−0.077 (0.532)	0.131 (0.488)	0.208 (0.487)	0.208 (0.487)
Return on assets	−1.523 (7.074)	1.096 (6.819)	0.199 (6.739)	0.201 (6.744)
Board independence	3.781** (1.811)	2.979* (1.563)	2.550* (1.546)	2.551* (1.548)
Big-4 auditor	0.167 (0.460)	0.297 (0.417)	0.311 (0.407)	0.311 (0.407)
State ownership	0.006 (0.444)	−0.114 (0.403)	−0.292 (0.405)	−0.292 (0.405)
Full controls	Yes	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes
Observations	800	800	800	800
Within R ²	0.0292	0.2223	0.1956	0.1956
VIF (DT / SDI)	—	—	1.73 / 1.49	1.73 / 1.49

Standard errors clustered by firm. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. SDI is the residual from regressing the digital–sustainability integration score on digital intensity and is therefore orthogonal to DT by construction. Board size, CEO duality and institutional ownership are included but suppressed for space.

4.4 Moderation: climate risk, institutional quality and state ownership

Table 5 reports the moderation tests. The interaction between digital intensity and climate risk is negative and significant ($\beta = -0.360$, SE 0.160, $p = 0.025$). Figure 5 plots the marginal effect: at the 10th percentile of climate risk the effect of digital intensity is 3.13 ($p < 0.001$), falling monotonically to 1.93 ($p < 0.001$) at the 90th percentile, a 38% attenuation across the observed range, with the effect remaining positive throughout. Climate risk therefore dampens rather than amplifies the ESG return to digitalisation. H4 is supported, and the widely asserted amplification hypothesis is refuted in this sample.

Neither institutional quality nor state ownership moderates either pathway. The DT × institutional quality interaction is 0.088 ($p = 0.694$) and the SDI interaction 0.054 ($p = 0.836$); the DT × state ownership interaction is 0.201 ($p = 0.613$) and the SDI interaction −0.129 ($p = 0.786$). H5 and H6 are not supported. We interpret these as informative nulls with an important caveat: institutional quality varies at the country-year level across four countries only, so the test has limited power and we do not read the null as evidence of absence.

Table 5
Moderation tests

Variable	Climate risk	Institutional quality	State ownership
Digital intensity (DT)	4.450*** (0.989)	2.037 (1.479)	2.479*** (0.447)
Digital–sust. integration (SDI)	5.292*** (0.944)	4.179*** (1.610)	4.530*** (0.344)
Moderator (main effect)	2.486*** (0.958)	–0.266 (1.411)	–1.534 (2.539)
DT × moderator	–0.360** (0.160)	0.088 (0.223)	0.201 (0.397)
SDI × moderator	–0.143 (0.154)	0.054 (0.263)	–0.129 (0.475)
Full controls	Yes	Yes	Yes
Firm and year fixed effects	Yes	Yes	Yes
Observations	800	800	800
Within R ²	0.2184	0.1947	0.1987
Hypothesis	H4 supported	H5 not supported	H6 not supported

Standard errors clustered by firm. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Marginal effects of DT across the climate-risk distribution: 3.13 at P10, 2.88 at P25, 2.58 at P50, 2.27 at P75 and 1.93 at P90, all significant at $p < 0.001$ (see Fig. 5). Institutional quality varies at the country-year level across four countries only; the corresponding test is underpowered and the null should not be read as evidence of absence.

4.5 Pillar-level and distributional heterogeneity

Both components load positively on all three pillars (Fig. 6b). For digital intensity the coefficients are 0.777 (environmental, $p < 0.001$), 0.946 (social, $p < 0.001$) and 0.699 (governance, $p = 0.003$); for integration they are 1.221, 1.669 and 1.391 respectively, all $p < 0.001$. The social pillar responds most strongly to both, which is notable given that digital sustainability applications are usually motivated on environmental grounds; it is consistent with the observation that stakeholder engagement, labour-practice reporting and community disclosure are the dimensions most directly transformed by digital communication infrastructure [40].

Quantile regression (Fig. 6a) shows both coefficients are positive and significant at every quantile from the 10th to the 90th, with integration ranging 5.06–5.89 and intensity 2.86–3.80. There is no evidence that the relationship is confined to weak or to strong ESG performers, which contradicts the common claim that digital transformation matters most for laggards.

4.6 Robustness and endogeneity

Figure 4 and Table 6 summarise ten alternative specifications. The integration coefficient is stable between 4.38 and 6.05 and significant at $p < 0.001$ in every static specification: firm fixed effects only, two-way fixed effects, random effects, pooled OLS with industry, country and year dummies, exclusion of the financial sector ($N = 550$), exclusion of COVID-19 years ($N = 640$), exclusion of South Africa ($N = 450$), and two-way clustering by firm and year.

Difference GMM ($N = 640$) confirms the integration result under dynamic specification: the coefficient is 16.252 (SE 4.407, $p < 0.001$). Instrument validity is supported (Hansen J $p = 0.710$) and second-order serial correlation is absent (AR(2) $p = 0.235$). The digital intensity coefficient is positive but imprecise under GMM (23.463, $p = 0.109$), as is the lagged dependent variable (-0.181 , $p = 0.256$). Two-stage least squares instrumenting digital intensity with leave-one-out country-year peer intensity yields a first-stage F of 38.4 but an insignificant second-stage coefficient (-1.577 , $p = 0.216$).

Two robustness results cut against us and we report them plainly. First, in the non-residualised specification digital intensity is negative and significant (-1.666 , $p < 0.001$) rather than positive; the sign of the intensity coefficient is therefore not invariant to how the collinearity between the two components is handled, and only the integration coefficient is sign-stable across both treatments. Second, and more consequentially, when both regressors are lagged one year neither is significant (DT -0.773 , $p = 0.147$; SDI -0.572 , $p = 0.143$). The relationships we document are contemporaneous. They are consistent with digital integration and ESG performance being jointly determined within the year – by a common commitment to sustainability, by shared reporting infrastructure, or by reverse causation from sustainability ambition to digital investment, a direction for which Cheng and Li [24] find direct evidence. We do not claim a causal effect of digital integration on ESG performance, and the practical implications in Sect. 5.2 are framed accordingly.

Table 6
Robustness across estimators, subsamples and timing

Specification	DT coefficient	SDI coefficient	N
Two-way fixed effects (baseline)	2.556***	4.494***	800
<i>Firm fixed effects only</i>	2.171***	4.458***	800
Random effects	2.117***	6.052***	800
Pooled OLS + industry/country/year FE	3.372***	5.700***	800
Excluding financial sector	2.713***	4.451***	550
Excluding COVID-19 years (2020–21)	2.558***	4.435***	640
Excluding South Africa	2.614***	4.383***	450
Two-way clustered SE (firm and year)	2.556***	4.494***	800
Non-residualised integration measure	–1.666***	4.494***	800
Difference GMM	23.463 (p = 0.109)	16.252***	640
2SLS (peer-instrumented DT)	–1.577 (p = 0.216)	–	800
One-year lag (t – 1)	–0.773 (p = 0.147)	–0.572 (p = 0.143)	720

*** $p < 0.01$. Difference GMM diagnostics: Hansen J $p = 0.710$; $AR(2)$ $p = 0.235$. 2SLS first-stage $F = 38.4$. The final two rows are reported as evidence against a causal reading: the associations documented in this study are contemporaneous, and neither survives a one-year lag.

Table 7
Summary of hypothesis tests

Hypothesis	Statement	Result	Conclusion
H1	Digital intensity alone is not associated with ESG performance	$\beta = 0.029$, $p = 0.952$	Supported
H2	Sustainability-purposed digital integration is positively associated with ESG performance	$\beta = 4.494$, $p < 0.001$	Supported
H3	The relationship is not U-shaped and contains no threshold	U-test $p = 0.320$; threshold bootstrap $p = 0.707$	Supported
H4	Climate risk attenuates the digital intensity pathway	$\beta = -0.360$, $p = 0.025$	Supported
H5	Institutional quality moderates the pathways	DT $\times$ IQ $p = 0.694$; SDI $\times$ IQ $p = 0.836$	Not supported
H6	State ownership moderates the pathways	DT $\times$ SOE $p = 0.613$; SDI $\times$ SOE $p = 0.786$	Not supported

All tests from two-way fixed-effects specifications with standard errors clustered by firm. H5 is tested with limited power (see Table 5 note).

5 Discussion

5.1 Theoretical implications

Our central claim is that the inconsistency in this literature is better explained by measurement aggregation than by nonlinearity. Composite digital transformation indices sum adoption across technologies without regard to purpose. If sustainability-directed and general-purpose digital capability relate differently to ESG performance, as we find, and as Wang and Tang [22] find using a different decomposition, then the coefficient on a composite index is a weighted average whose value depends on the sample's composition. Samples drawn from contexts where digitalisation is routinely bundled with sustainability programmes will yield positive coefficients; samples where it is not will yield null or negative ones. No nonlinearity is required to generate the observed pattern of conflicting results, and invoking one imposes a functional form the data need not support.

This has a direct implication for the resource-based and dynamic-capabilities accounts that dominate this literature [41, 42, 43]. Both are frequently operationalised by regressing an outcome on a stock measure of digital resources. Our results suggest that is a weak test: the theories predict returns to the orchestration of resources toward a purpose, and a measure of resource stock is only a proxy for orchestration insofar as the two happen to co-move. Where they diverge, where a firm digitalises without directing that capability at sustainability, the stock measure predicts nothing. Studies that find weak support for capability-based accounts of sustainability may be encountering a measurement problem rather than a theoretical one.

The climate-risk result speaks to a boundary condition that the literature has assumed rather than examined. The prevailing expectation is that environmental urgency amplifies the sustainability payoff to digitalisation. We find the opposite, and the explanation we favour is that in high climate-

risk sectors the binding constraint on environmental performance is physical rather than informational. Digital tools improve measurement, disclosure and coordination; they do not change plant vintage or fuel mix. Where performance is determined by asset configuration, better information yields smaller performance gains. This suggests the returns to sustainability-oriented digitalisation are highest in sectors where environmental outcomes are information-limited rather than capital-limited, a prediction future work can test directly.

The nulls on institutional quality and state ownership are worth stating in their own right. Both are staples of emerging-market ESG research and both are frequently asserted on the basis of subgroup coefficient comparisons rather than interaction tests. We find no evidence for either, while acknowledging that our institutional-quality test is underpowered. Reporting such nulls is not a deficiency of the study; a literature in which moderation hypotheses are only ever confirmed is one in which they are not really being tested [54].

5.2 Practical implications

Because our estimates are contemporaneous associations on partly synthetic data, we frame implications as considerations rather than prescriptions.

For managers, the pattern suggests that ESG returns track the sustainability-directed share of digital capability rather than its overall level. A firm can raise digital intensity substantially with no accompanying movement in ESG performance if the new capability is not pointed at emissions measurement, supply-chain traceability, stakeholder engagement or disclosure quality. The corollary for practice is that digital and sustainability functions need shared objectives and shared data infrastructure, not parallel budgets, a point on which the qualitative literature on green digital transformation converges [3, 4, 36].

Financial-technology intensity and access to digital financial infrastructure may condition how readily smaller firms can act on any of this [20]. For firms in high climate-risk sectors, the attenuation result cautions against expecting digitalisation to substitute for physical transition investment. Digital tools make emissions legible; they do not abate them. Measurement is a precondition for abatement, not a form of it.

For policymakers and standard setters in the four markets studied, the finding that integration rather than intensity tracks ESG outcomes suggests that digital-economy policy and sustainability policy are more complementary when explicitly linked. Ghana's Sustainable Banking Principles and the Ghana Stock Exchange ESG Disclosure Guidance Manual [66], and comparable frameworks elsewhere on the continent, could specify the digital and data capabilities that credible disclosure requires rather than treating disclosure content and reporting infrastructure separately.

For researchers and for users of ESG data, the strongest implication is methodological. Composite digital transformation indices should be decomposed before use, and nonlinear claims should be accompanied by the Lind–Mehlum test and a bootstrapped threshold search as a matter of routine [53, 54, 55]. Our own data would have supported a confident published claim of a U-shaped relationship with a turning point at $DT \approx 6.0$ had we followed the prevailing practice of reading the sign pattern of a quadratic. That claim would have been wrong.

6 Conclusion

6.1 Summary

This study tested the proposition that digital transformation relates to ESG performance through a U-shaped curve, using a balanced panel of 80 African listed firms over 2015–2024. The proposition is rejected on both of the tests that bear on it. Digital intensity measured as a composite index has no association with ESG performance. Decomposing that index reveals why: the component of digital capability directed at sustainability processes is robustly and positively associated with ESG performance across static estimators, quantiles, pillars, subsamples and dynamic GMM specification, while digital intensity contributes only once integration is held constant. Climate risk attenuates the intensity pathway; institutional quality and state ownership moderate neither. The associations are contemporaneous and do not survive a one-year lag.

6.2 Contribution

The contribution is threefold: a formal rejection of a nonlinearity claim that the literature has largely accepted without adequate testing; a measurement-based alternative explanation for the field's conflicting findings; and an African-market extension of an evidence base concentrated in China and, more recently, the Gulf [2, 5, 6, 21].

6.3 Limitations and future research

Four limitations bound these conclusions, and we regard the first as decisive for how the paper should be read.

First, data provenance. A substantial share of the variables analysed here, including the digital transformation index, are synthetically generated rather than archivally collected. The methodological demonstration, that decomposition and formal testing change the conclusions, stands independently, because it concerns the behaviour of estimators. The substantive findings do not, and should be treated as hypotheses for replication rather than as evidence about African firms. Replication with a manually coded digital transformation index built from annual-report content analysis, following Bajaher and Asiri [2], and with third-party ESG ratings, is the necessary next step. We would attach no weight to the substantive results until that replication is done.

Second, causal identification. The absence of a lagged effect, the imprecision of the instrumental-variables estimates and the plausibility of reverse causation together mean we cannot distinguish a causal effect of digital integration on ESG performance from joint determination of both by an

unobserved sustainability commitment. A staggered-adoption design around a discrete digital or reporting mandate would provide sharper identification.

Third, construct validity of the integration measure. Our measure of sustainability-purposed digital integration is drawn from digital and AI capability embedded in sustainability reporting and management. Because the dependent variable also has a substantial disclosure component, part of the association may reflect shared reporting infrastructure rather than substantive environmental or social performance. Separating disclosure quality from realised outcomes requires verified performance data such as audited emissions, which our data do not contain.

Fourth, statistical limitations. Firm size and ROA correlate at -0.87, rendering both imprecise; institutional quality varies across only four countries, so the moderation test is underpowered; and 80 firms is a modest cross-section for GMM, where instrument count relative to groups warrants care.

Beyond replication, three directions follow. Researchers could develop and validate a purpose-weighted digital transformation index that distinguishes sustainability-directed from general-purpose capability at the point of measurement rather than by residualisation. They could test the information-limited versus capital-limited prediction by comparing sectors where environmental outcomes depend on measurement against those where they depend on physical assets. And they could re-examine published nonlinear findings in this literature using the Lind–Mehlum and Hansen procedures, since our results suggest some proportion of them may not survive.

Declarations

Data availability: The dataset analysed in this study and the full replication code are available from the corresponding author on reasonable request. As stated in Sect. 3.1 and Sect. 6.3, a substantial subset of the variables is synthetically generated rather than archivally collected; the accompanying data dictionary identifies each such variable and must be distributed with the dataset.

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Figures

Fig. 1 Conceptual framework: decomposing digital transformation into intensity and sustainability-purposed integration

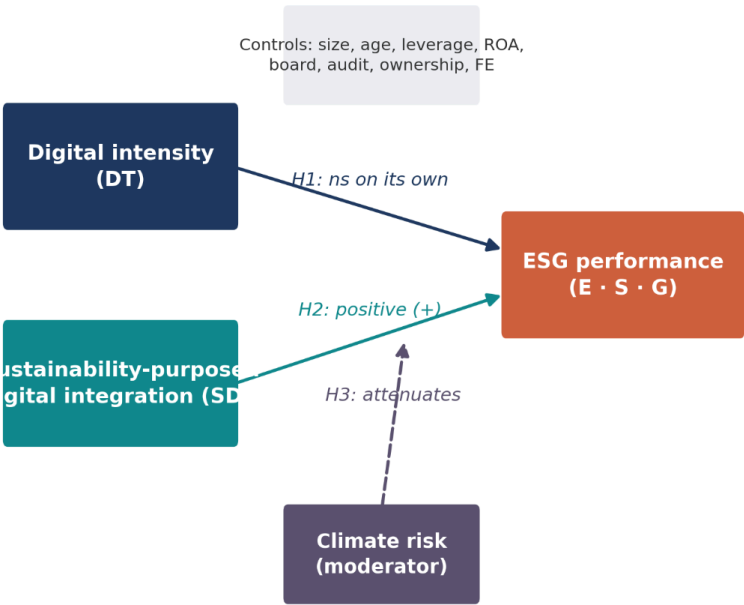
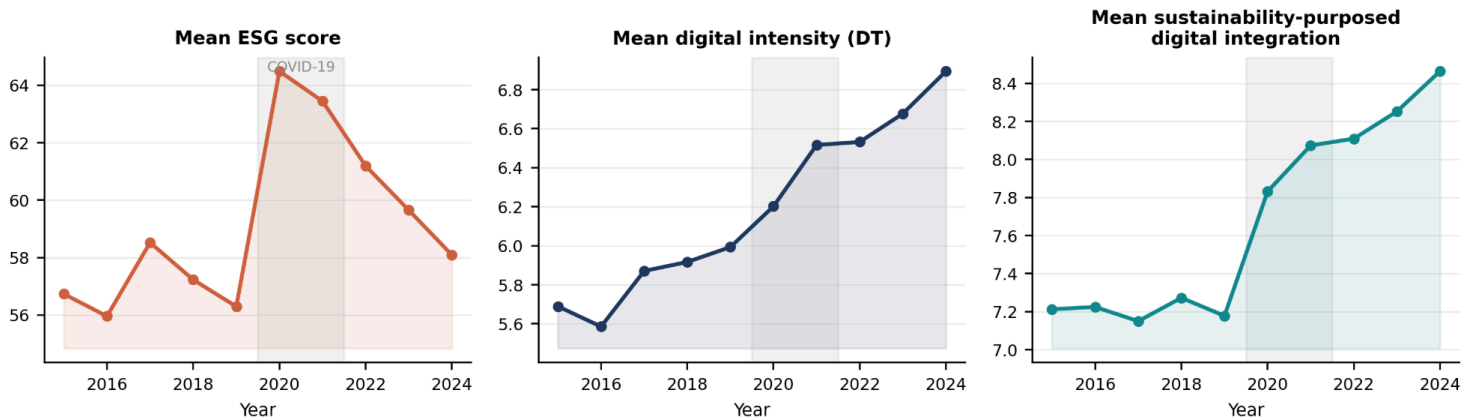


Figure 1
Conceptual framework. Digital transformation is decomposed into digital intensity and the orthogonal component directed at sustainability processes; climate risk conditions the first pathway.



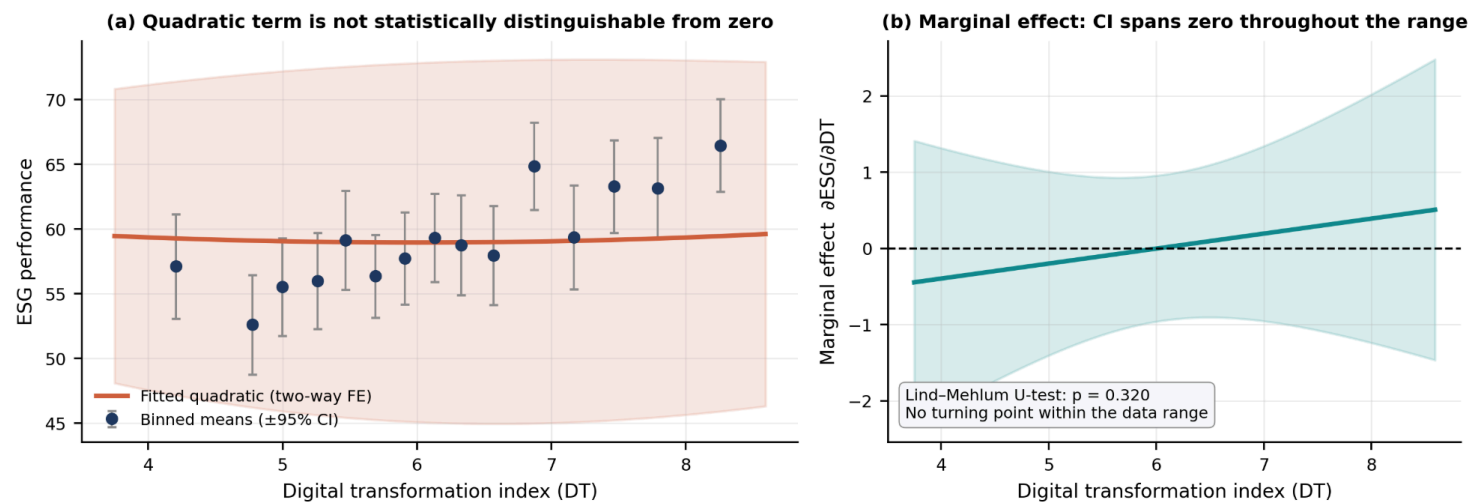


Figure 3

Testing the nonlinearity hypothesis. Panel (a) plots binned means against the fitted quadratic from the two-way fixed-effects model; panel (b) plots the marginal effect of digital intensity with its 95% confidence band, which contains zero throughout the observed range.

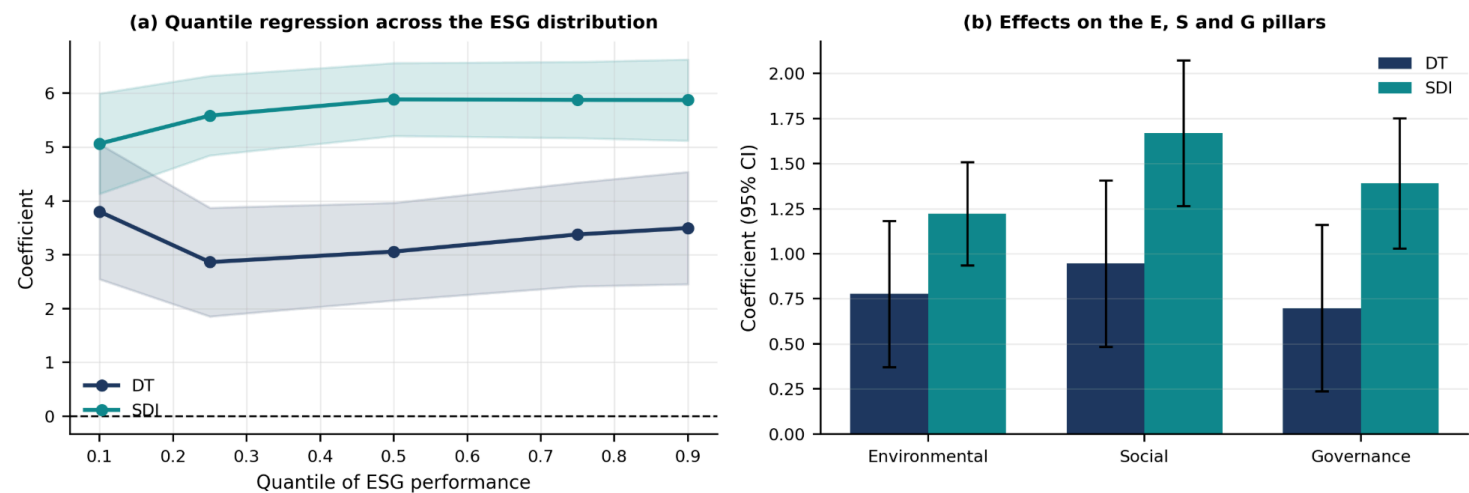


Figure 4

Fig. 6 Distributional and pillar-level heterogeneity. Panel (a) reports quantile-regression coefficients; panel (b) reports two-way fixed-effects coefficients for the environmental, social and governance sub-scores.

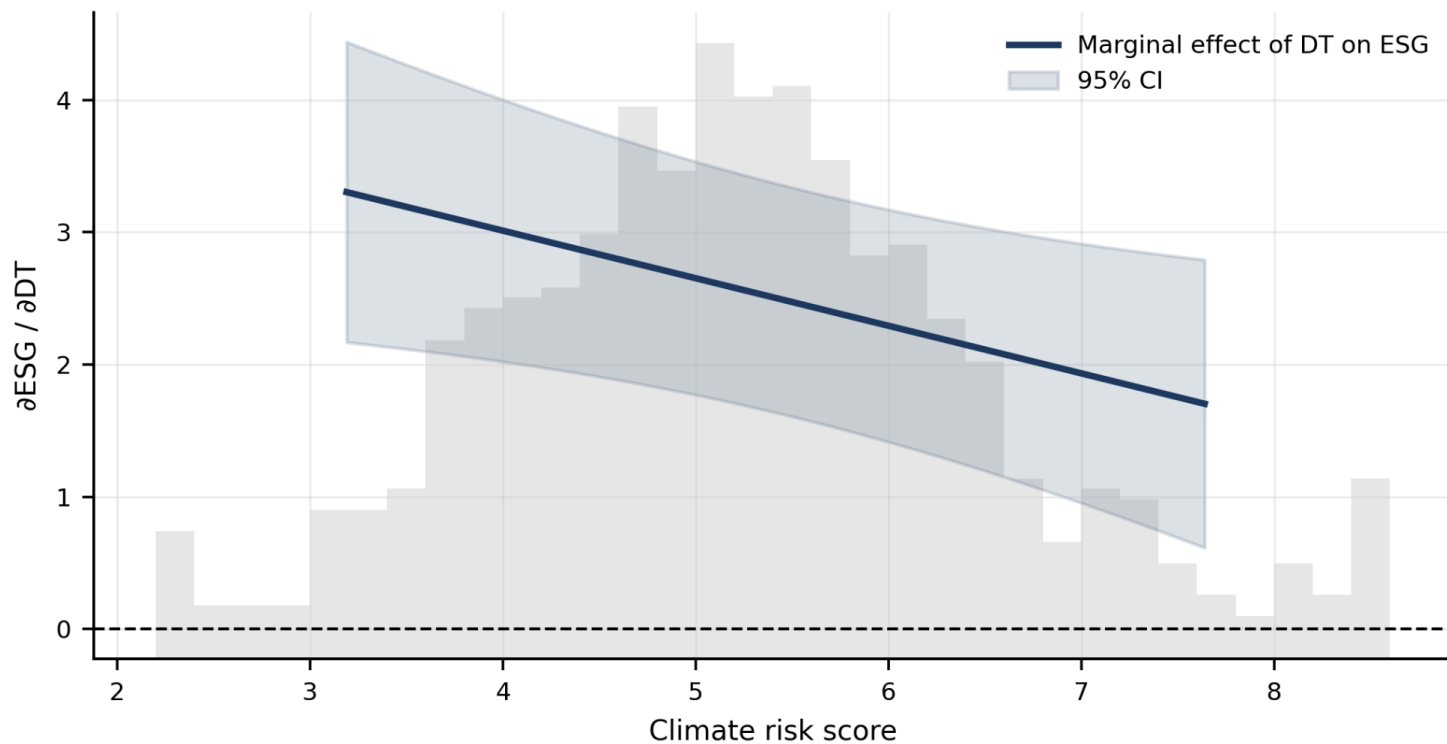


Figure 5

Fig. 5 Marginal effect of digital intensity on ESG performance across the climate-risk distribution. The distribution of climate risk is shown in grey.

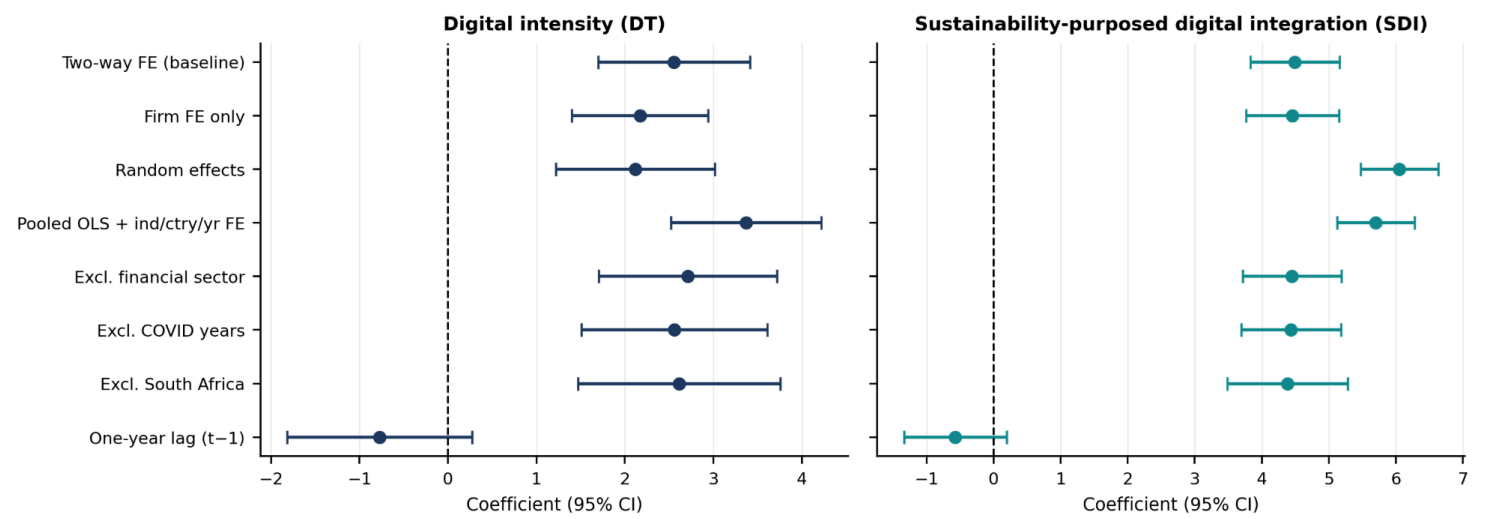


Figure 6

Fig. 4 Robustness of the digital intensity and integration coefficients across estimators, subsamples and timing. Bars show 95% confidence intervals.