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From Innovation Intensity to Green Action: Cross-Economy Evidence on Energy Management and the Carbon-Monitoring Gap

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Abstract

This study examines whether economy-level innovation intensity is associated with environmental action and whether that association differs across green outcomes and levels of adoption. Using harmonized World Bank Enterprise Survey indicators for 89 economies observed in 2023–2024, the analysis relates the prevalence of highly innovation-active firms to energy-management adoption and CO_2 monitoring. The empirical strategy combines HC3-robust OLS, beta regression, MM-robust estimation, bootstrap inference, continent controls, leave-one-economy-out diagnostics, quantile regression, alternative innovation measures, and formal cross-outcome coefficient tests. Innovation intensity is positively associated with energy-management adoption, although precision varies across specifications. Construct robustness is strongest for process innovation and a standardized additive innovation index; product innovation is also significant in the macro-control sample, whereas R&D alone is not. Quantile point estimates are largest at the 25th conditional quantile, but a joint 5000-replication bootstrap yields only suggestive global evidence of heterogeneity ($p = 0.077$), with no pairwise differences significant under sign-based bootstrap inference. The clearest result is the Carbon-Monitoring Gap: the innovation slope is significantly weaker for CO_2 monitoring than for energy management in the full sample, after outcome-specific continent adjustment, with standardized outcomes, after influential-case exclusion, and in every leave-one-economy-out re-estimation. The findings indicate that aggregate innovation intensity is associated more strongly with operational green practices than with carbon measurement, while the significant management-outcome interaction suggests that this outcome heterogeneity is not unique to innovation.

Keywords: innovation intensity; green action; energy management; carbon monitoring; outcome heterogeneity

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1. Introduction

The low-carbon transition is changing the content of corporate capability. Firms are increasingly expected not only to comply with environmental regulation but also to redesign production processes, improve resource use, establish energy-management routines, measure greenhouse-gas emissions, and produce information that can support credible decarbonization. These demands bring innovation, management, and environmental governance into the same organizational problem. The natural-resource-based view argues that environmental constraints can generate capability-building opportunities when pollution prevention, resource productivity, and sustainable development become embedded

in organizational routines [1]. The Porter perspective similarly emphasizes that environmental pressures can induce innovation and offset part of the cost of compliance through productivity and resource-efficiency improvements [2,3]. At the economy level, the present study does not treat these firm-level mechanisms as directly observed behavior; instead, it asks whether economies with a higher prevalence of innovation-active firms also display broader adoption of environmental practices.

A large eco-innovation literature has established that research and development, technological knowledge, organizational learning, regulation, market demand, and cost-saving incentives influence the development of environmental innovation [4–7]. Environmental innovators also rely more intensively on R&D cooperation and external knowledge, indicating that absorptive and collaborative capabilities matter for translating knowledge into greener production [8]. At the same time, the economic consequences of environmental innovation vary by type. Resource- and energy-efficiency innovations may improve profitability through lower input costs, whereas other environmental innovations operate through compliance, reputation, or strategic positioning [9]. Recent research continues to emphasize the interaction between organizational capabilities, green innovation, regulation, and sustainable performance [10].

Despite this progress, the empirical literature remains asymmetric. Most studies ask what causes firms to innovate environmentally, or whether green practices subsequently improve productivity, export participation, or innovation. Paschoaleto and Martínez-Zarzoso [11], for example, use the Green Economy module of the World Bank Enterprise Surveys (WBES) to construct a harmonized environmental performance index and show that environmental practices can be compared systematically across economies. Phan [12] reports a positive association between green-economy practices and product and process innovation in WBES data. Skhirtladze [13] likewise identifies innovation, R&D, training, finance, and quality certification among the firm-level correlates of green practices. This evidence establishes a close connection between innovation and environmental behavior, but the aggregate cross-economy relationship remains less developed: do business environments with greater innovation intensity also exhibit broader environmental action?

A second gap concerns the multidimensional nature of environmental action. Energy management and carbon monitoring are often grouped under a broad green-management label, yet they represent distinct organizational requirements. Energy-management measures can be implemented through process optimization, equipment upgrading, maintenance routines, energy audits, target setting, and formal energy-management systems. Carbon monitoring requires additional information infrastructure: organizational and operational boundaries, emissions factors, metering, accounting routines, data governance, verification, and reporting capacity. Environmental management accounting research shows that specialized accounting tools can improve carbon management and disclosure quality [14], while mandatory carbon disclosure can affect subsequent emissions by changing organizational incentives and accountability [15]. This distinction also connects to the broader problem of ESG information quality and environmental transparency: operational change can precede the systems needed to generate consistent and auditable environmental information. Aggregate innovation intensity may therefore be more strongly associated with operational environmental upgrading than with formal carbon measurement.

Recent research on environmental, social, and governance (ESG) performance further highlights the need to distinguish environmental practices from the information systems used to communicate and evaluate them. Sun and Li [16] examine the informational dimension of ESG performance, whereas Ma et al. [17] examine greenwashing in relation to institutional and cultural conditions. Taken together, these contributions provide a useful setting for distinguishing operational environmental action, carbon-measurement adoption,

and the credibility of sustainability information. In the present cross-economy analysis, the Carbon-Monitoring Gap refers specifically to a difference between aggregate associations with energy-management practices and CO_2-monitoring adoption. It does not measure the accuracy of ESG reporting, disclosure credibility, or greenwashing. Nevertheless, it suggests a relevant distinction between the diffusion of operational environmental practices and the organizational infrastructure needed to produce carbon information.

A third issue is distributional heterogeneity. The marginal relevance of innovation may differ across the conditional distribution of green action. Economies situated in the lower conditional distribution of energy-management adoption, relative to their observed covariates, may offer greater scope for technological diffusion, process upgrading, and organizational learning to be associated with broader adoption. At higher conditional quantiles, the remaining constraints may be more institutional, sectoral, regulatory, or measurement-specific. This logic motivates a lower-tail hypothesis in which the innovation–green action association may be stronger at lower conditional quantiles. Because such a pattern cannot be inferred from mean regression alone, quantile regression is used as a complementary, explicitly exploratory distributional test.

This study addresses these questions using harmonized economy-level WBES indicators for 89 economies observed in their latest compatible survey wave in 2023 or 2024. The main dependent variable is the share of firms adopting energy-management measures to reduce emissions; the alternative outcome is the share monitoring their own CO_2 emissions. The focal construct is economy-level innovation intensity, measured by a deliberately stringent WBES indicator identifying the share of firms that simultaneously introduced a new product or service, introduced a process innovation, and spent on R&D. This aggregate measure should not be interpreted as an individual firm capability. It captures the prevalence of highly innovation-active firms within each economy and is used as an indicator of the innovation intensity of the business environment. The analysis further controls for management quality, bank finance, export orientation, foreign ownership, survey year, real GDP per capita, manufacturing intensity, trade openness, and continent effects. Because the data are cross-sectional economy-level aggregates, all interpretations are explicitly associational rather than causal or firm-level.

The empirical design is intentionally layered. Baseline and extended OLS specifications use HC3 heteroskedasticity-consistent standard errors. Beta regression addresses the bounded nature of the environmental outcomes, while high-breakdown MM estimation, Cook's distance diagnostics, winsorization, leave-one-economy-out re-estimation, and nonparametric bootstrap inference assess sensitivity to influential observations (see Appendix A, Figure A1). Construct robustness re-estimates the models with separate product-innovation, process-innovation, and R&D shares and with an equal-weight standardized additive index. Quantile regression evaluates possible distributional heterogeneity using a common 5000-replication economy-level bootstrap. Crucially, the paper goes beyond comparing significance levels across separate regressions: it formally tests whether the innovation coefficient differs between energy-management adoption and CO_2 monitoring using stacked-outcome specifications with country-clustered CR2 inference.

The results establish three findings. First, economy-level innovation intensity is positively associated with energy-management adoption, although statistical precision varies across estimators and samples. The relationship is not an artifact of the stringent intersection measure: process innovation and the standardized additive innovation index are significant in the full sample, and product innovation, process innovation, and the additive index are significant in the macro-control sample; R&D alone is not. Second, the innovation coefficient is much larger at the 25th conditional quantile than at the median or 75th quantile, but the common-bootstrap global equality test is only suggestive ($p = 0.077$) and no pairwise

difference is significant under sign-based bootstrap inference (although two are marginal under a normal approximation). The distributional evidence is therefore descriptive rather than a demonstrated catch-up mechanism. Third, the innovation slope is significantly weaker for CO_2 monitoring than for energy management. This Carbon-Monitoring Gap survives full-sample estimation, outcome standardization, outcome-specific continent adjustment, influential-case exclusion, and every leave-one-economy-out re-estimation. A joint test also indicates broader outcome-specific slope heterogeneity, including a significant management-outcome interaction.

The study contributes to the literature in four respects. First, it reframes the innovation–environment relationship at the economy level, focusing on whether the prevalence of innovation-active firms is associated with broader environmental practice adoption while explicitly avoiding firm-level ecological interpretation. Second, it distinguishes operational green action from carbon measurement and demonstrates formally that these outcomes need not share the same aggregate innovation relationship. Third, it adds a distributional perspective while calibrating the evidence to the formal bootstrap tests rather than treating lower-tail point estimates as proof of a dynamic catch-up process. Fourth, it shows that the central outcome contrast is robust to alternative innovation measures and several influence and scale checks, while also documenting that the outcome heterogeneity is not unique to innovation. The policy implication is consequently conditional: innovation-support policies may facilitate operational greening, but carbon accountability plausibly requires complementary measurement, accounting, verification, reporting, and institutional capabilities.

2. Materials and Methods

The theoretical link between innovation intensity and environmental action can be motivated by capability-based views of the firm while recognizing that the empirical unit is the economy. Hart's [1] natural-resource-based view extends the resource-based framework by arguing that capabilities related to pollution prevention, product stewardship, and sustainable development can become sources of competitive advantage. Aragón-Correa and Sharma [18] add a contingent perspective: the environmental value of organizational resources depends on the competitive and institutional context in which firms operate. In parallel, the Porter hypothesis emphasizes that properly designed environmental pressures may stimulate innovation that improves resource productivity and partially offsets compliance costs [2,3]. At the aggregate level used here, these firm-level mechanisms motivate an economy-level expectation: where innovation-active firms are more prevalent, technological search, diffusion, and implementation may also be more widespread, but the data do not identify within-firm mechanisms directly.

The national innovation systems perspective provides an additional rationale for studying these relationships at the economy level. Lundvall [19] emphasizes interactive learning among firms and institutions, while Nelson [20] highlights the role of national institutional arrangements in shaping technological innovation. From this perspective, economy-level innovation intensity reflects the prevalence of highly innovation-active firms within a wider business environment, not a direct capability measurement for an individual establishment. Such environments may also support technological diffusion, knowledge exchange, managerial learning, and adoption of environmentally relevant practices. Yet observed cross-economy associations may also reflect industrial composition, regulatory arrangements, institutional quality, and complementary infrastructure. Consequently, this theoretical perspective motivates the aggregate hypothesis without establishing that the firms undertaking innovation are necessarily the same firms adopting environmental practices, or that the observed associations are causal.

Eco-innovation research identifies technological knowledge and R&D as core components of this process. Rennings [7] conceptualizes eco-innovation broadly as technological, organizational, social, and institutional change that reduces environmental burdens. Horbach [4] finds that knowledge capital and R&D capability are important determinants of environmental innovation, while Horbach et al. [5] show that the relative importance of regulation, technology push, market pull, and cost savings differs across environmental objectives. Evidence from the United Kingdom similarly indicates that environmental innovation reflects a combination of regulation, capabilities, and expected cost savings [6]. De Marchi [8] further shows that environmental innovators rely more heavily on external R&D cooperation, reinforcing the importance of absorptive and collaborative capacity.

Operational environmental practices are particularly plausible correlates of aggregate innovation intensity because they often require technological search, process redesign, equipment adaptation, and organizational learning. Resource- and energy-efficiency innovations can create direct economic value by lowering material and energy inputs, although returns differ across types of environmental innovation [9]. Javed et al. [10] likewise emphasize the importance of organizational capabilities and green innovation for sustainable performance under environmental regulation. At the cross-economy level, a higher prevalence of firms combining product innovation, process innovation, and R&D is therefore interpreted as greater innovation intensity rather than as a direct measure of the capability of any particular firm.

The WBES literature reinforces the plausibility of this aggregate relationship. Paschoaleto and Martínez-Zarzoso [11] demonstrate that environmental practices can be harmonized and compared across firms and economies using the Green Economy module. Phan [12] identifies a positive relationship between green-economy practices and product and process innovation. Skhirtladze [13] reports that innovation-related internal capabilities are among the firm-level correlates of CO_2 monitoring and energy-management adoption. The present study therefore evaluates whether cross-economy variation in the prevalence of innovation-active firms is systematically associated with broader environmental-practice adoption, without interpreting the aggregate coefficient as a firm-level behavioral effect.

H1. *Economy-level innovation intensity is positively associated with the adoption of energy-management measures to reduce emissions.*

Innovation intensity does not operate in an organizational vacuum. Firms must also coordinate information, set targets, monitor deviations, allocate responsibility, and sustain new routines. Bansal and Roth [21] show that ecological responsiveness reflects competitiveness, legitimacy, and ecological-responsibility motives, but organizational context determines how those motives are translated into action. Delmas and Toffel [22] similarly demonstrate that facilities facing comparable environmental pressures can respond differently because internal structures and influential organizational functions filter external demands. These firm-level arguments motivate the inclusion of economy-level management quality as a distinct aggregate capability indicator.

Management quality is directly relevant to energy use. Bloom et al. [23] match management-practice measures to energy-use data for UK manufacturing establishments and report that better-managed firms are less energy intensive. Martin et al. [24] similarly connect management practices and organizational structure to energy efficiency, while Grover and Karplus [25] show that the energy-management nexus depends on which practices are adopted and in which firms. Experimental evidence from India further indicates that modern management practices can generate substantial productivity and operational-efficiency improvements [26]. These findings justify treating management quality as a core organizational capability and control. Christmann [27] shows at the firm level that comple-

mentary assets condition the returns to environmental practices; such complementarities, however, need not appear as a statistically significant multiplicative interaction between economy-level aggregates. Accordingly, management is modeled primarily as an enabling capability rather than as the paper's central moderation mechanism.

The average association between innovation intensity and environmental action may conceal distributional heterogeneity. Economies at lower conditional quantiles of energy-management adoption have lower modeled adoption relative to the covariate distribution, not necessarily lower absolute adoption rates. In such settings, new production technologies, process innovation, technical learning, and organizational diffusion may relax basic informational and implementation barriers. By contrast, at higher conditional quantiles, residual barriers may involve regulation, reporting standards, sector composition, energy prices, or specialized environmental-management systems. This motivates, but does not by itself establish, the possibility that the marginal association of innovation intensity declines as the conditional level of green action rises.

Quantile methods are useful because they permit the relationship between covariates and the outcome to vary across the conditional distribution rather than imposing a single mean coefficient. Kesidou and Demirel [6] use quantile-based reasoning to show that determinants of eco-innovation can differ across the innovation distribution. Extending that logic to cross-economy environmental practice, a larger lower-quantile innovation coefficient would be consistent with lower-tail heterogeneity. Importantly, however, a low conditional quantile should not be equated with an economy being absolutely "lagging," and significance at one quantile but not another does not establish coefficient inequality. The empirical analysis therefore relies on formal global and pairwise bootstrap tests before drawing any distributional conclusion.

H2. *The positive association between innovation intensity and energy-management adoption is stronger at lower conditional quantiles of the energy-management distribution.*

Energy-management adoption and carbon monitoring are related but distinct organizational outcomes. Operational energy management can be integrated directly into production and maintenance through equipment upgrades, process optimization, energy audits, target setting, and routine monitoring of energy inputs. Carbon monitoring requires an additional layer of measurement and accounting infrastructure. Firms must define organizational boundaries, identify emission sources, collect activity data, apply emissions factors, maintain consistent records, verify calculations, and often prepare information for external reporting.

This distinction is supported by the environmental-management-accounting literature. Qian et al. [14] show that environmental management accounting can improve carbon management and disclosure quality, indicating that specialized information systems matter beyond general operational capability. Downar et al. [15] demonstrate that mandatory carbon disclosure can produce real emissions responses, highlighting the role of formal monitoring and accountability regimes. More broadly, the distinction is consistent with concerns about environmental-information quality and transparency: an economy can display substantial technological and process innovation without equally developed systems for carbon measurement and reporting. If so, aggregate innovation intensity should have a stronger association with energy-management adoption than with CO_2 monitoring. This proposition is tested directly through a cross-outcome coefficient restriction rather than by comparing p-values from separate regressions.

This operational-versus-informational distinction also connects the present hypotheses to the literature on ESG information quality and greenwashing. Energy-management adoption can reflect investments in production practices, whereas carbon monitoring requires

additional measurement routines, accounting systems, and organizational coordination. Sun and Li [16] address the informational role of ESG performance, while Ma et al. [17] highlight the institutional context of environmental claims. These studies reinforce the importance of not equating the adoption of operational practices with credible environmental information. Because the present WBES indicators capture the aggregate prevalence of energy-management and CO_2-monitoring practices, rather than reporting accuracy or environmental misrepresentation, H3 tests outcome-specific associations and does not constitute a test of greenwashing.

H3. *The association between economy-level innovation intensity and energy-management adoption is stronger than its association with CO_2 monitoring.*

3. Data and Methodology

The empirical analysis combines harmonized indicators from the World Bank Enterprise Surveys with macroeconomic controls from the World Development Indicators (WDI) [28]. WBES are nationally representative surveys of formal private-sector establishments conducted using standardized questionnaires that cover business conditions, innovation, management, finance, internationalization, and environmental practices [29]. Rather than pooling confidential firm-level microdata, the study uses publicly disseminated economy-level WBES indicators. The unit of observation is therefore the country/economy. All dependent variables, innovation measures, and business controls are aggregate shares or indices, so the estimated coefficients describe cross-economy associations among business-environment aggregates and cannot be interpreted as within-firm behavioral relationships.

The core sample consists of 89 economies observed in their latest compatible WBES wave in 2023 or 2024. Forty-two economies are observed in 2023 and 47 in 2024. The main outcome is the percentage of firms adopting energy-management measures to reduce emissions, and the alternative outcome is the percentage monitoring their own CO_2 emissions. The focal innovation-intensity indicator is intentionally demanding: it measures the share of firms that simultaneously introduced a new product or service, introduced a process innovation, and spent on R&D (Table 1). The intersection is used to capture the prevalence of highly innovation-active firms rather than to claim that any single activity alone represents capability. To assess sensitivity to this construction, the robustness analysis separately uses economy-level product-innovation, process-innovation, and R&D shares and an equal-weight additive index formed from their standardized values. Because the broader component measures and the stringent intersection measure are not identical constructs, these tests are interpreted as construct robustness rather than exact measurement substitutions.

Management quality is captured by the WBES management-practices index. Additional business-environment controls are the share of firms with a bank loan or line of credit, the share directly exporting at least 10% of sales, and the share with at least 10% foreign ownership. Macroeconomic controls are real GDP per capita in constant 2015 US dollars, manufacturing value added as a share of GDP, and trade in goods and services as a share of GDP. GDP per capita is log transformed before standardization. To avoid using future information, macro controls are matched using the survey year or the latest available observation at or before that year with a maximum three-year look-back. The common macro-control sample contains 79 economies. Continent indicators for Africa, the Americas, Asia, Europe, and Oceania capture broad geographical heterogeneity, with Africa used as the reference group in the reported fixed-effect specification. Data were retrieved and processed in August 2026.

Table 1. Variable definitions, measurement, and sources.

Variable	Definition/Transformation	Source/Code
Energy management	Firms adopting energy-management measures to reduce emissions (% of firms); main outcome	WBES/WDI, IC.FRM.ENGM.ZS
CO_2 monitoring	Firms monitoring their own CO_2 emissions (% of firms); alternative outcome	WBES/WDI, IC.FRM.CO_2.ZS
Innovation intensity (strict)	Firms introducing a new product/service and process and spending on R&D (% of firms); standardized in regressions; focal stringent intersection measure	WBES/WDI, IC.FRM.NPRD.ZS
Product innovation	Firms introducing a new product/service (% of firms); standardized; construct-robustness measure	WBES country profiles
Process innovation	Firms introducing a process innovation (% of firms); standardized; construct-robustness measure	WBES country profiles
R&D activity	Firms spending on R&D (% of firms); standardized; construct-robustness measure	WBES country profiles
Additive innovation index	Equal-weight mean of standardized product innovation, process innovation, and R&D shares; re-standardized	Author's construction
Management quality	Management practices index; standardized	WBES/WDI, IC.MNG.IND
Bank finance	Firms with a bank loan/line of credit (% of firms)	WBES/WDI, IC.FRM.BNKL.ZS
Export orientation	Firms exporting directly at least 10% of sales (% of firms)	WBES/WDI, IC.FRM.EXS.ZS
Foreign ownership	Firms with at least 10% foreign ownership (% of firms)	WBES/WDI, IC.FRM.FO.ZS
GDP per capita	GDP per capita, constant 2015 US$; natural log and standardized	WDI, NY.GDP.PCAP.KD
Manufacturing share	Manufacturing value added (% of GDP); standardized	WDI, NV.IND.MANF.ZS
Trade openness	Trade in goods and services (% of GDP); standardized	WDI, NE.TRD.GNFS.ZS
Survey year	Indicator equal to 1 for 2024 and 0 for 2023	Author's construction
Continent fixed effects	Africa, Americas, Asia, Europe, Oceania; Africa omitted	Author's construction from ISO country codes

Note. WBES indicators are economy-level aggregates constructed from nationally representative firm surveys. WDI controls are matched at or before the survey year, with a maximum three-year look-back.

Innovation intensity and management quality are standardized to facilitate interpretation. In the linear models, the innovation coefficient therefore represents the percentage-point difference in the environmental outcome associated with a one-standard-deviation difference in the economy-level innovation indicator, conditional on the included controls. The baseline specification is written as

$$GreenAction_i = \alpha + \beta_1 Innovation_i + \beta_2 Management_i + \gamma' X_i + \delta Year2024_i + \varepsilon_i \quad (1)$$

where $GreenAction_i$ is the percentage of firms adopting energy-management measures in economy i; $Innovation_i$ and $Management_i$ are standardized economy-level indicators; and X_i contains finance, export orientation, and foreign-ownership controls. The expanded specification adds log GDP per capita, manufacturing share, trade openness, and continent effects. Because the sample is a moderate cross-section of heterogeneous economies, all OLS inference is based on HC3 heteroskedasticity-consistent standard errors [30], which provide additional protection against leverage-related finite-sample distortions.

$$GreenAction_i = \alpha + \beta_1 Innovation_i + \beta_2 Management_i + \gamma' X_i + \theta' M_i + \lambda' Continent_i + \varepsilon_i \quad (2)$$

The environmental outcomes are percentages bounded between zero and 100. OLS is retained as the main specification because coefficients are directly interpretable in percentage points, but beta regression is estimated as a functional-form robustness check after rescaling the dependent variable to the unit interval [31]. The sample contains no exact boundary observations at zero or one for the environmental outcomes, so no boundary adjustment is required. Average marginal effects are calculated to translate beta-regression results back into percentage-point terms.

Influential observations are addressed in complementary ways rather than through automatic deletion. Cook's distance identifies observations exerting disproportionate leverage on the OLS coefficient vector [32]. High-breakdown MM regression reduces the influence of atypical observations while retaining the complete sample [33]. Additional checks include 1–99% winsorization, exclusion of all Cook-flagged observations, and 89 leave-one-economy-out re-estimations. The latter is especially informative because it reveals whether any single economy changes the sign or approximate magnitude of the focal coefficient. Sampling uncertainty is further evaluated using 5000 nonparametric country-level bootstrap replications.

To examine whether innovation is differentially associated with green action across the conditional outcome distribution, the analysis estimates quantile regressions at $\tau = 0.25$, 0.50, and 0.75 using the Koenker–Bassett estimator [34]. The specification is

$$Q\tau(GreenAction_i|Z_i) = \alpha\tau + \beta\tau Innovation_i + \psi\tau' Z_i \quad (3)$$

All quantile quantities are evaluated with a common 5000-replication economy-level nonparametric bootstrap in which economies are resampled with replacement and the Q25, Q50, and Q75 models are re-estimated within the same resample. The reported bootstrap standard error is the standard deviation of the bootstrap coefficient distribution, while the 95% interval is the percentile interval; these two summaries need not be algebraically identical in finite samples or under asymmetric resampling distributions. The covariate set in Equation (3) is the macro common-sample specification: innovation intensity, management quality, bank finance, export orientation, foreign ownership, survey year, log GDP per capita, manufacturing share, and trade openness. A global Wald test evaluates H0: $\beta 0.25 = \beta 0.50 = \beta 0.75$ using the joint bootstrap covariance, and pairwise differences are calculated from the same bootstrap draws. This unified procedure avoids interpreting significance at one quantile and insignificance at another as proof of coefficient heterogeneity.

Bootstrap inference and numerical reproducibility were further audited using the same 5000 economy-level resamples ($N = 79$) and two quantile-regression algorithms: the Barrodale–Roberts (BR) and Frisch–Newton (FN) methods. Both completed all 5000 joint replications without failed estimates and produced matching reported coefficients, standard errors, percentile intervals, pairwise contrasts, and global Wald statistics. The maximum absolute difference between corresponding bootstrap coefficients was 0.0000535 across the three quantiles. BR generated 32 nonunique-solution warnings across the resamples, whereas FN generated none; these warnings did not alter the reported results. The two-sided

bootstrap *p*-values in Table 2 are calculated as twice the smaller of the fractions of bootstrap estimates on either side of zero (capped at one), rather than by dividing the original point estimate by the bootstrap standard deviation. The global Wald *p*-value uses the joint bootstrap covariance matrix with an asymptotic chi-square reference distribution; it is not a bootstrap-calibrated tail probability. Because sign-based bootstrap inference and normal approximations based on bootstrap standard deviations yield different conclusions for some pairwise contrasts, the distributional evidence is treated as exploratory and inference-method sensitive.

Table 2. Quantile-regression estimates and formal coefficient-difference tests.

Panel/Test	Estimate	Bootstrap SE/Wald	95% CI	*p*
A. Q25 innovation-intensity coefficient	7.682	2.821	[−0.002, 11.012]	0.0504
A. Q50 innovation-intensity coefficient	1.927	3.034	[−1.926, 9.826]	0.2544
A. Q75 innovation-intensity coefficient	0.159	3.482	[−3.596, 10.219]	0.4424
B. Global equality: Q25 = Q50 = Q75	Wald = 5.126	df = 2	—	0.0771
C. Q25 − Q50	5.755	2.829	[−2.800, 8.452]	0.2628
C. Q25 − Q75	7.523	3.803	[−4.476, 10.518]	0.3236
C. Q50 − Q75	1.769	3.188	[−6.061, 6.512]	0.8208

Note. All quantities use the same 5000 economy-level bootstrap resamples. Panel A reports point estimates, bootstrap standard deviations, percentile 95% intervals, and two-sided bootstrap *p*-values. Panel B is a global Wald equality test based on the joint bootstrap covariance. Panel C reports pairwise coefficient differences from the same resamples. The evidence is suggestive of lower-tail heterogeneity but does not establish coefficient inequality at the 5% level. The quantile models use the $N = 79$ macro-control common sample. Pointwise bootstrap and joint Wald procedures rely on different inferential constructions; percentile intervals and Wald-based tests need not imply identical rejection decisions. Algorithmic robustness was checked by re-estimating all 5000 paired resamples with both BR and FN solvers; both generated identical reported results (maximum coefficient difference < 0.000054), with 32 nonunique-solution warnings under BR and none under FN. Two-sided bootstrap *p*-values are $2 \times \min\{\Pr^*(\text{estimate} \leq 0), \Pr^*(\text{estimate} \geq 0)\}$, capped at one. The global Wald *p*-value instead uses a chi-square(2) reference and the joint bootstrap covariance. Normal-approximation *p*-values based on estimate/bootstrap SD give different outcomes for Q25 − Q50 and Q25 − Q75; these discrepancies underscore the exploratory and inference-method-sensitive nature of the heterogeneity findings.

The central outcome-specificity proposition is tested directly rather than inferred from separate significance levels. The energy-management and CO_2-monitoring observations are stacked so that each economy contributes two outcome records. Energy management is the reference outcome, and the model interacts the CO_2-outcome indicator with innovation intensity and all other covariates. In additional robustness tests, continent effects are also interacted with the outcome indicator so that broad geographical differences are permitted to vary across the two environmental outcomes. The focal interaction coefficient is therefore the difference between the innovation slope for CO_2 monitoring and the innovation slope for energy management:

$$Y_{ij} = \alpha + \kappa CO2_j + \beta Innovation_i + \varphi\left(CO2_j \times Innovation_i\right) + \Gamma' Controls_{ij} + u_{ij} \quad (4)$$

Under this parameterization, $\varphi = \beta CO_2 - \beta Energy$. The null hypothesis H0: $\varphi = 0$ is tested using CR2 cluster-robust inference with Satterthwaite degrees-of-freedom correction, clustering by economy to account for paired outcomes. A negative and statistically significant interaction indicates that the innovation association is weaker for CO_2 monitoring. Robustness tests re-estimate φ in the full $N = 89$ sample, after standardizing each outcome separately, after allowing outcome-specific continent effects, after removing the six Cook-

flagged economies, and in 89 leave-one-economy-out samples. The analysis also jointly tests all outcome-specific slope interactions and reports the management-outcome interaction to assess whether the contrast is unique to innovation. The standard error of a paired coefficient difference can be smaller than the HC3 standard error of either standalone slope because $\mathrm{Var}(\beta\mathrm{Energy} - \beta\mathrm{CO_2})$ depends on the covariance between the paired estimates as well as their individual variances.

All analyses were implemented in R using the lmtest, sandwich, betareg, robustbase, boot, quantreg, WDI, countrycode, and clubSandwich packages. The empirical design remains associational throughout. Reverse causality and omitted institutional factors cannot be ruled out, and the economy-level aggregation does not permit firm-level causal interpretation.

4. Empirical Results

Table 3 shows substantial cross-economy dispersion in both environmental practices and innovation intensity. Energy-management adoption averages 32.50% of firms and ranges from 3.66% to 97.35%. CO_2 monitoring is considerably less widespread, with a mean of 13.28%, a median of 11.47%, and a maximum of 52.94%. The stringent innovation-intensity indicator is also highly dispersed: the mean is 5.66%, the median is 2.41%, and the maximum is 28.31%. The low median relative to the maximum reflects the demanding intersection construction and indicates that only a limited set of economies has a high prevalence of firms simultaneously combining product innovation, process innovation, and R&D expenditure.

Table 3. Descriptive statistics.

Variable	*N*	Mean	SD	Median	Min	Max
Energy-management adoption	89	32.497	18.692	29.580	3.660	97.350
CO_2 monitoring	89	13.279	9.632	11.470	1.590	52.940
Innovation intensity (strict)	89	5.660	6.644	2.410	0.000	28.310
Management practices index	89	52.673	8.037	52.390	28.070	68.600
Bank loan/line of credit	89	38.109	17.499	39.580	8.700	88.520
Direct exporter (≥10% sales)	89	11.800	8.330	9.600	0.690	33.280
Foreign ownership (≥10%)	89	10.278	8.592	8.080	0.000	49.070

Note. Percentages are shares of firms except the management-practices index. Source: author's calculations from World Bank Enterprise Surveys/World Development Indicators.

The bivariate patterns anticipate the central empirical contrast. Energy-management adoption is positively correlated with the stringent innovation-intensity measure ($r = 0.288$, $p = 0.006$) and management quality ($r = 0.353$, $p < 0.001$). By contrast, innovation intensity is only weakly correlated with CO_2 monitoring ($r = 0.050$, $p = 0.639$). Figure 1 illustrates this divergence: the unconditional innovation–energy-management relationship slopes upward, whereas the innovation–CO_2-monitoring relationship is nearly flat. These descriptive patterns do not control for covariates and do not establish firm-level mechanisms, but they motivate the outcome distinction developed in the multivariate analysis.

Table 4 reports the baseline HC3 regressions. In Model 1, a one-standard-deviation increase in the stringent innovation-intensity measure is associated with 5.480 percentage points higher energy-management adoption ($p = 0.0038$). Adding management quality in Model 2 reduces the innovation coefficient modestly to 4.638 ($p = 0.016$), while management itself is positive and significant (5.945, $p = 0.014$). This pattern is consistent with the

interpretation that aggregate innovation intensity and managerial implementation quality are related but empirically distinct.

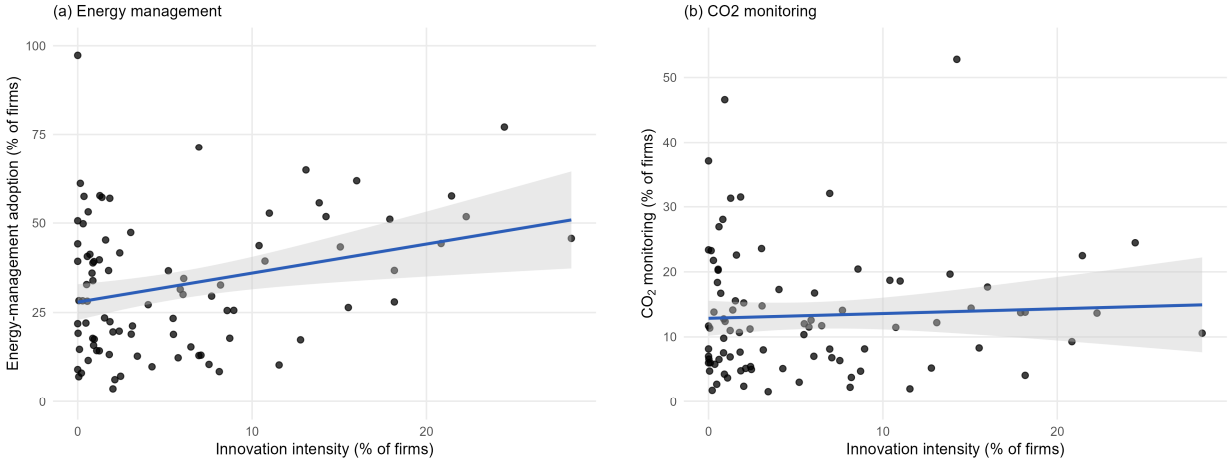


Figure 1. Innovation intensity and alternative environmental outcomes. Note. (**a**) panel: energy-management adoption. (**b**) panel: CO_2 monitoring. Lines are unconditional OLS fits with 95% confidence bands; the figure is descriptive and not causal.

Table 4. Baseline regressions for energy-management adoption (HC3 robust standard errors).

Variable	M1	M2	M3	M4	M5	M6
Innovation intensity	5.480 *** (1.842)	4.638 ** (1.894)	4.001 * (2.076)	4.433 ** (1.759)	3.964 * (2.070)	4.035 * (2.148)
Management quality	—	5.945 ** (2.377)	5.605 ** (2.680)	5.227 ** (2.595)	5.331 * (2.894)	5.282 * (2.806)
Bank finance	—	—	0.109 (0.169)	—	0.056 (0.170)	0.056 (0.175)
Export orientation	—	—	—	0.339 (0.245)	0.312 (0.255)	0.311 (0.262)
Foreign ownership	—	—	—	—	−0.152 (0.239)	−0.154 (0.242)
Survey year: 2024	−0.668 (3.876)	−0.918 (3.761)	−0.503 (4.102)	−0.502 (3.805)	−0.237 (4.121)	−0.243 (4.218)
Innovation × Management	—	—	—	—	—	−0.293 (3.540)
R^2	0.083	0.182	0.191	0.203	0.212	0.212
Adjusted R^2	0.062	0.153	0.153	0.165	0.154	0.144

Note. Dependent variable: firms adopting energy-management measures to reduce emissions (% of firms). HC3 standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. $N = 89$ in all models.

Adding bank finance or export orientation separately changes precision more than magnitude. The innovation-intensity coefficient equals 4.001 ($p = 0.057$) in the bank-finance specification and 4.433 ($p = 0.014$) in the export specification. In the fully controlled business-environment model, it remains positive at 3.964 ($p = 0.0589$), while management equals 5.331 ($p = 0.0691$). Bank finance, export orientation, foreign ownership, and the 2024 survey-wave indicator are individually insignificant. The innovation-by-management interaction is −0.293 ($p = 0.934$), providing no evidence that the two aggregate indicators

operate through a simple multiplicative moderation channel. Accordingly, management is retained as an important covariate rather than used to define the central contribution.

A key concern in cross-economy analysis is that innovation intensity may proxy for development level or economic structure. Sequential macro controls (Figure 2) do not materially alter the focal coefficient. It is 3.964 in the original full model (Table 5), 3.780 after adding GDP per capita, 3.980 after adding manufacturing share, and 4.429 after adding trade openness. In the common 79-economy sample, the corresponding same-sample baseline coefficient is 4.890 ($p = 0.0423$), while the fully macro-controlled coefficient is 4.429 ($p = 0.0737$). The change is modest in economic magnitude, although precision falls as controls are added.

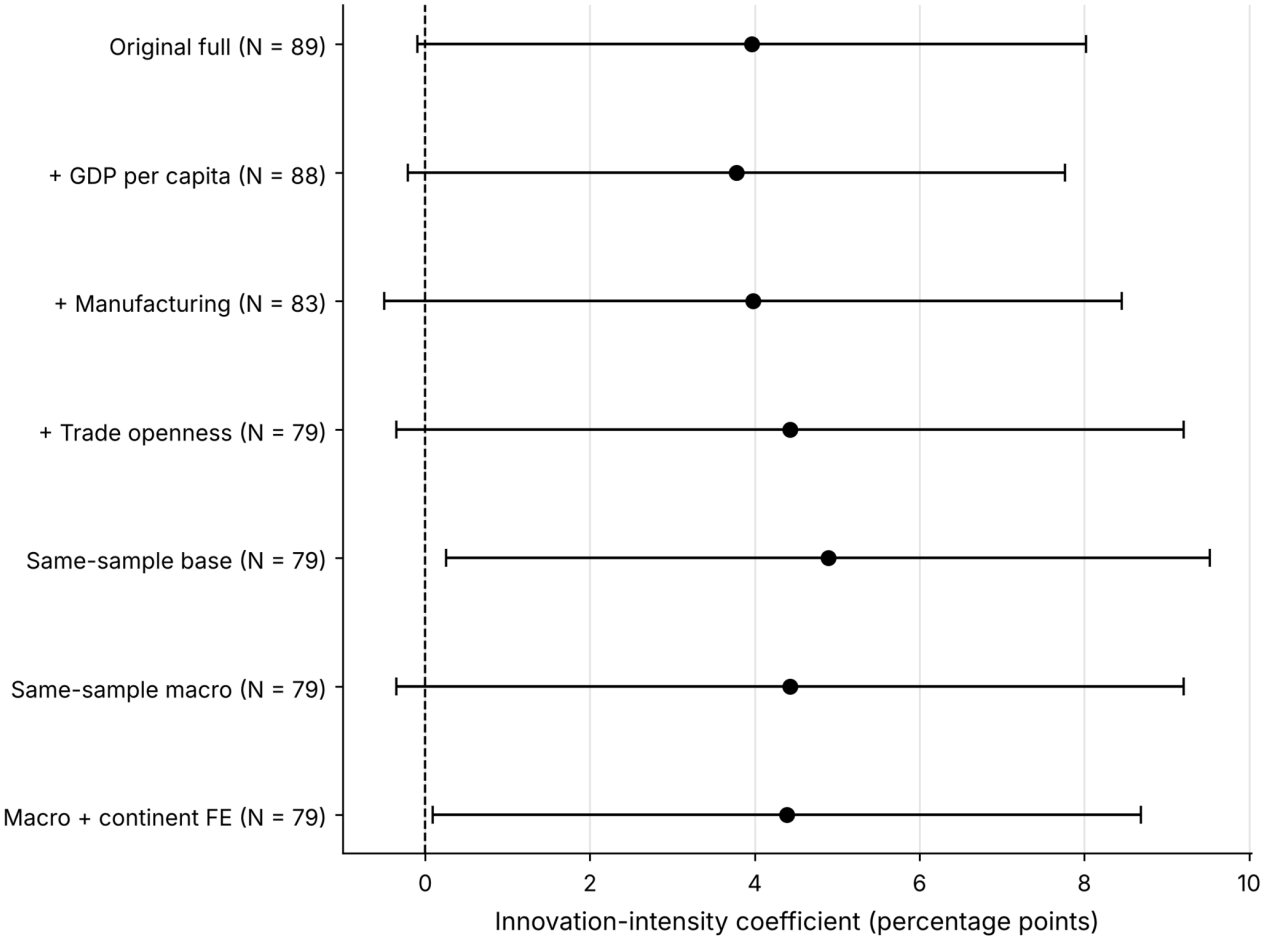


Figure 2. Stability of the innovation-intensity coefficient across macro-control specifications. Note. Points show HC3 innovation-intensity coefficients; horizontal bars are approximate 95% confidence intervals (±1.96 × HC3 SE). The dashed vertical line marks zero.

Continent fixed effects strengthen rather than weaken the focal estimate. In the macro-plus-continent specification, the innovation coefficient is 4.391 with $p = 0.0495$, while R^2 rises to 0.387. The result suggests that the baseline relationship is not explained solely by broad continental differences. The macro specification also passes the Ramsey RESET test ($p = 0.246$), and the maximum variance-inflation factor is approximately 2.38, indicating no severe multicollinearity.

Alternative estimators tell a similar but not uniformly significant story. In the full-sample beta regression, the innovation-intensity coefficient is 0.186 ($p = 0.0355$), corresponding to an average marginal effect of approximately 4.00 percentage points; management is also positive (0.264, $p = 0.0029$; AME ≈ 5.68 percentage points). MM robust regres-

sion produces an innovation coefficient of 3.959 ($p = 0.0435$). Winsorization yields 3.938 ($p = 0.0562$). A 5000-replication bootstrap centers the innovation coefficient at 3.813 with a 95% percentile interval of $[-0.087, 7.321]$ and $p = 0.0564$ (Table 6). Thus, the appropriate interpretation is stability in sign and approximate magnitude rather than uniform 5% significance.

Table 5. Stability of the innovation coefficient under macro and continent controls.

Specification	*N*	Innovation Intensity	HC3 SE	*p*	R^2	Adj. R^2
Original full	89	3.964	2.070	0.0589	0.212	0.154
+GDP per capita	88	3.780	2.035	0.0669	0.237	0.170
+Manufacturing	83	3.980	2.285	0.0857	0.240	0.157
+Trade openness	79	4.429	2.438	0.0737	0.262	0.166
Same-sample base	79	4.890	2.365	0.0423	0.234	0.170
Same-sample macro	79	4.429	2.438	0.0737	0.262	0.166
Macro + continent FE	79	4.391	2.193	0.0495	0.387	0.265

Note. Continent fixed effects use Africa as the omitted category.

Table 6. Estimator and influence robustness for innovation intensity and management.

Estimator/Sample	Innovation Intensity	*p*	Management	*p*	Comment
OLS-HC3, full sample	3.964 *	0.0589	5.331 *	0.0691	$N = 89$
Beta regression	0.186 **	0.0355	0.264 ***	0.0029	AME: +4.00 pp/+5.68 pp
MM robust regression	3.959 **	0.0435	4.121 **	0.0356	High-breakdown estimator
1–99% winsorized	3.938 *	0.0562	4.581 *	0.0687	$N = 89$
Influential cases removed	2.367	0.1461	4.994 ***	0.0044	$N = 83$
Bootstrap, 5000 reps.	3.813 *	0.0564	5.450 **	0.0344	95% CI innovation intensity: $[-0.087, 7.321]$

Note. Beta-regression coefficients are on the logit scale; average marginal effects are reported in percentage points. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Cook's distance diagnostics flag Cambodia, Israel, Equatorial Guinea, Belgium, Tajikistan, and Bosnia and Herzegovina. Removing all six simultaneously lowers the energy-management innovation coefficient to 2.367 ($p = 0.146$), showing that the average-effect estimate is sensitive to a severe joint deletion exercise (Table 6). The leave-one-economy-out analysis (Figure 3) is more reassuring: across 89 re-estimations, the coefficient remains positive in 100% of cases, ranges from 2.806 to 4.407, and has a median of 3.974. It remains significant at the 10% level in 96.6% of the re-estimations, though only 14.6% meet the 5% threshold. No single economy therefore reverses the relationship, but the sample does not support overstatement of statistical certainty. Table 7 reports the same specifications re-estimated with alternative innovation measures (product innovation, process innovation, R&D, and a standardized additive index).

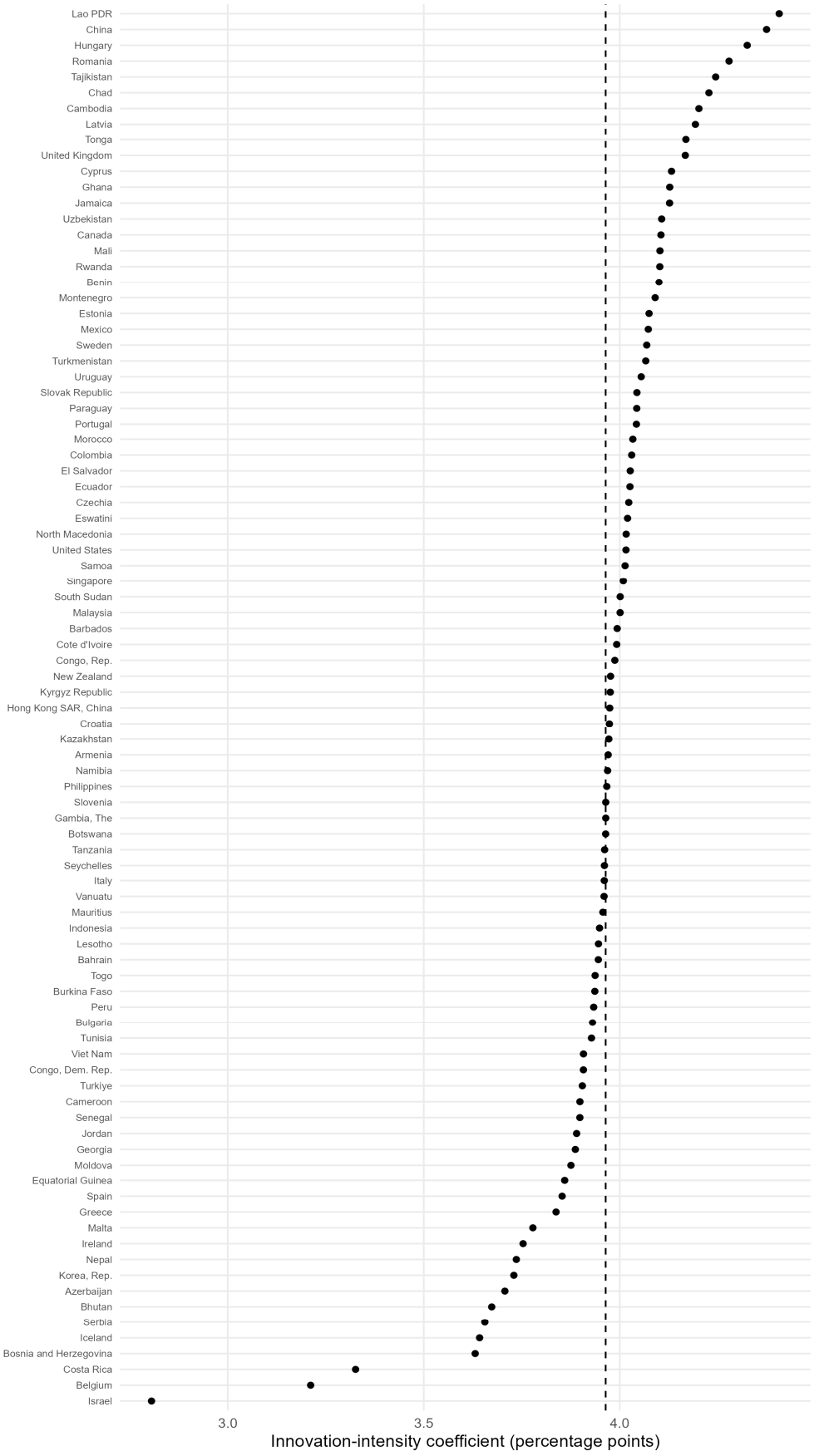


Figure 3. Leave-one-economy-out stability of the innovation-intensity coefficient. Note. Each point is obtained after omitting the economy shown. The dashed line marks the full-sample coefficient.

Table 7. Alternative innovation-measure robustness.

Panel/Measure	*N*	Energy Coefficient	HC3 SE	*p*	CO_2 − Energy Gap	CR2 *p*
A. Strict intersection (t11)	89	3.964	2.070	0.0589	−3.945	0.0120
A. Product innovation	89	4.239	2.407	0.0819	−4.420	0.0114
A. Process innovation	89	5.733	2.369	0.0177	−4.926	0.0075
A. R&D	89	4.259	3.405	0.2145	−2.196	0.3529
A. Standardized additive index	89	5.832	2.777	0.0388	−4.905	0.0148
B. Strict intersection (t11)	79	4.429	2.438	0.0737	−4.381	0.0135
B. Product innovation	79	5.383	2.632	0.0446	−5.030	0.0082
B. Process innovation	79	6.123	2.576	0.0202	−5.196	0.0066
B. R&D	79	4.519	4.129	0.2775	−2.299	0.4175
B. Standardized additive index	79	6.344	3.017	0.0392	−5.171	0.0146

Note. Panel A uses the full $N = 89$ business-control sample. Panel B uses the $N = 79$ macro common sample. Energy coefficients are HC3 estimates for a one-standard-deviation increase in each innovation measure. CO_2 − Energy is the stacked CR2 interaction; negative values indicate a weaker innovation slope for CO_2 monitoring. The alternative measures are construct-robustness checks and are not treated as exact substitutes for the stringent intersection indicator. The alternative-measure results show that the main energy-management association is not confined to the stringent intersection definition. In the full sample, process innovation and the additive index are significant at 5%, while product innovation is positive at the 10% level and R&D alone is imprecise. In the macro common sample, product innovation, process innovation, and the additive index are significant at 5%. The Carbon-Monitoring Gap is also significant for the strict, product, process, and additive measures in both samples, but not for R&D alone. This pattern supports construct robustness while cautioning against a claim of uniformity across every innovation dimension.

The quantile point estimates display a pronounced lower-tail pattern. At the 25th conditional quantile of energy-management adoption, the innovation-intensity coefficient is 7.682. It falls to 1.927 at the median and to 0.159 at the 75th quantile. However, a low conditional quantile is not equivalent to an economy being absolutely "lagging," and the declining point estimates should be interpreted as possible distributional heterogeneity rather than as evidence of a dynamic catch-up process.

The unified 5000-replication bootstrap reinforces this cautious interpretation. The Q25 coefficient has bootstrap SE = 2.821, a 95% percentile interval of [−0.002, 11.012], and $p = 0.0504$; the Q50 and Q75 coefficients are imprecisely estimated ($p = 0.2544$ and $p = 0.4424$). The global equality test yields Wald = 5.126 with $p = 0.0771$, providing suggestive evidence at the 10% level but not at 5%. Pairwise differences are also insignificant: Q25 − Q50 = 5.755 ($p = 0.2628$), Q25 − Q75 = 7.523 ($p = 0.3236$), and Q50 − Q75 = 1.769 ($p = 0.8208$), with all 95% percentile intervals including zero. H2 therefore receives only suggestive support, and the paper does not interpret the pattern as a demonstrated catch-up effect.

As a numerical robustness check, the complete bootstrap analysis was independently re-estimated with BR and FN quantile-regression solvers using identical resamples. Across 5000 successful replications for each solver, the reported Q25, Q50 and Q75 results, all pairwise differences and the global Wald test were unchanged (Figure 4). Although BR issued 32 nonunique-solution warnings, FN issued none and the maximum absolute coefficient difference across paired fits was below 0.000054. This supports numerical stability with respect to the fitting algorithm. It does not resolve differences between sign-based bootstrap and normal-approximation inference: for example, Q25 − Q50 and Q25 − Q75 yield sign-based *p*-values of 0.2628 and 0.3236, compared with normal-approximation *p*-values

of 0.0419 and 0.0479. Therefore, the substantive interpretation remains conservative and does not claim statistically established quantile heterogeneity at the 5% level under every inferential procedure.

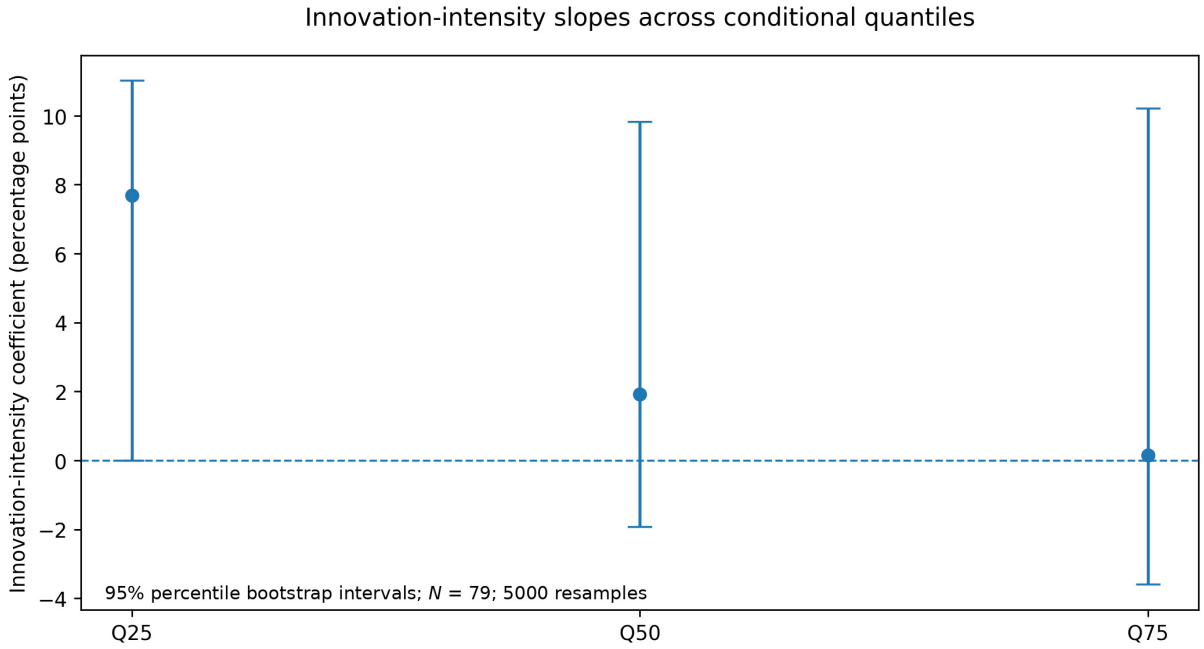


Figure 4. Innovation-intensity coefficients across the conditional distribution of energy-management adoption. Note. Vertical bars summarize bootstrap uncertainty. The point-estimate decline is pronounced, but formal cross-quantile differences are not significant at the 5% level.

The strongest result concerns the difference between energy-management adoption and CO_2 monitoring. In the original common-sample macro model, the innovation-intensity coefficient for energy management is 4.429 (HC3 SE = 2.438, $p = 0.0737$), whereas the corresponding CO_2-monitoring coefficient is 0.048 (HC3 SE = 1.272, $p = 0.9700$) (Figure 5). The latter estimate is close to zero but statistically imprecise; its confidence range does not justify an equivalence-to-zero claim. The formal analysis therefore focuses on the directly estimated difference between outcome-specific slopes rather than on separate significance levels.

In the $N = 79$ macro stacked model, the CO_2 × Innovation interaction equals −4.381 (CR2 SE = 1.623, $p = 0.0135$). In the full $N = 89$ business-control sample, the corresponding difference is −3.945 (SE = 1.455, $p = 0.0120$). The result is not driven by outcome scaling: after separately standardizing energy management and CO_2 monitoring, the interaction remains negative and significant (−0.210 SD units, SE = 0.086, $p = 0.0219$). Nor is it an artifact of imposing identical continent effects across outcomes: allowing outcome-specific continent effects yields −3.502 (SE = 1.294, $p = 0.0117$). The CR2 standard error of the paired slope difference can be smaller than the HC3 standard error of the standalone energy slope because the variance of a difference subtracts twice the covariance between the paired slope estimates.

Influence diagnostics provide further support for the outcome contrast. Removing all six Cook-flagged economies yields a CO_2-minus-energy innovation-slope difference of −2.756 (SE = 1.245, $p = 0.0369$). Across 89 leave-one-economy-out re-estimations, the interaction remains negative in every case, ranges from −4.357 to −3.056, and remains significant at the 5% level in 100% of re-estimations. The joint CR2 test rejects equality of all six outcome-specific slope interactions, $F(6, 30.3) = 5.65$, $p < 0.001$. Importantly, the management-outcome interaction is also significant (−4.265, SE = 1.815, $p = 0.0276$), indi-

cating that the operational-versus-monitoring contrast is not uniquely specific to innovation. H3 is nevertheless supported because the innovation slope is consistently and significantly weaker for CO_2 monitoring in the direct cross-outcome tests.

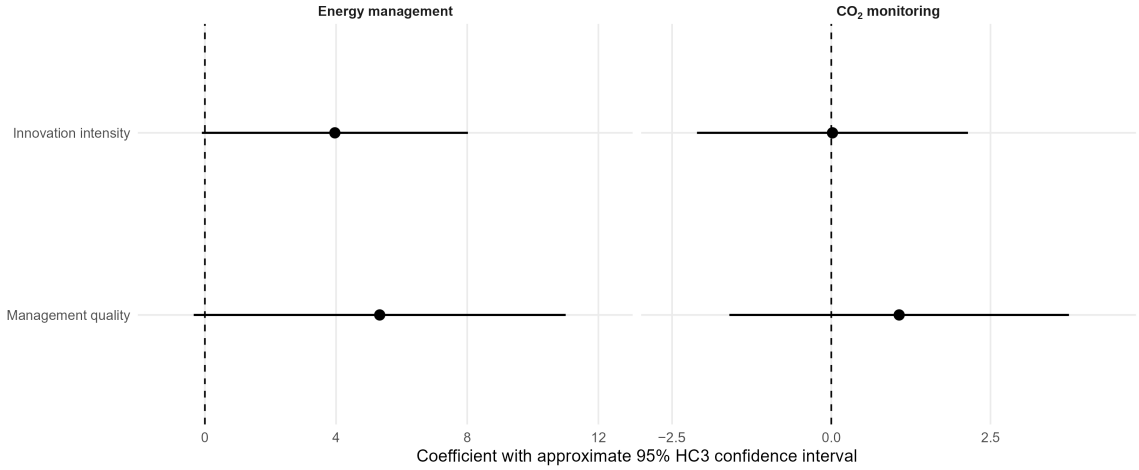


Figure 5. Innovation-intensity and management coefficients across environmental outcomes. Note. Points show estimates from fully controlled models with approximate 95% confidence intervals. Formal cross-outcome inference and robustness tests are reported in Table 8.

Table 8. Formal and robustness tests of the Carbon-Monitoring Gap.

Test/Quantity	Estimate	SE/Statistic	95% CI/Range	*p*
Macro sample: Energy innovation slope	4.429	2.438	—	0.0737
Macro sample: CO_2 innovation slope	0.048	1.272	—	0.9700
Macro CR2: CO_2 − Energy ($N = 79$)	−4.381	1.623	—	0.0135
Full-sample CR2: CO_2 − Energy ($N = 89$)	−3.945	1.455	—	0.0120
Standardized outcomes: CO_2 − Energy	−0.210	0.086	—	0.0219
Outcome × continent: CO_2 − Energy	−3.502	1.294	—	0.0117
Cook-six removed: CO_2 − Energy ($N = 83$)	−2.756	1.245	—	0.0369
Leave-one-out φ (89 re-estimations)	Median −3.945	—	[−4.357, −3.056]	100% $p < 0.05$
Joint outcome-specific slope interactions	F = 5.65	df = 6, 30.3	—	<0.001
Management × outcome: CO_2 − Energy	−4.265	1.815	—	0.0276

Note. Negative values indicate a weaker innovation-intensity slope for CO_2 monitoring than for energy-management adoption. CR2 inference clusters by economy with Satterthwaite correction. The standardized-outcome model scales each outcome separately. The outcome × continent specification permits continent effects to differ across outcomes. Leave-one-out results summarize 89 re-estimations (Figure 3). The joint test evaluates all outcome-specific slope interactions simultaneously. The management-by-outcome coefficient is included to assess whether outcome-specific heterogeneity extends beyond innovation.

5. Discussion

The results support a differentiated interpretation of aggregate innovation and environmental action. Economy-level innovation intensity is positively associated with energy-management adoption, but the strength and precision of that relationship vary across estimators and measurement choices. The central result is therefore not that every innovation measure produces the same coefficient, but that the positive association survives several conceptually distinct measures and robustness designs, especially process innovation and the standardized additive index.

The alternative-measure analysis directly addresses the possibility that the focal intersection indicator mechanically generates the result. In the full sample, process innovation and the additive index are significant predictors of energy-management adoption, while product innovation is positive but marginal and R&D alone is imprecise. In the macro-control sample, product innovation, process innovation, and the additive index are significant, whereas R&D alone remains insignificant. The pattern suggests that the energy-management relationship is more closely aligned with the breadth and implementation side of innovation than with R&D expenditure considered in isolation. Because product and process innovation are highly correlated, the separate-indicator results should not be interpreted as isolated causal contributions of each innovation activity.

The magnitude is also substantively meaningful. In the fully macro-controlled model, a one-standard-deviation difference in innovation intensity is associated with approximately 4.4 percentage points higher energy-management adoption. Relative to a sample mean of about 32.5%, this is not a trivial difference. The beta-regression marginal effect is of similar size, and the leave-one-economy-out exercise shows that the sign does not depend on any single observation. The strongest interpretation is therefore not that innovation causally increases energy management by a precise number of percentage points, but that innovation-intensive business environments systematically exhibit greater operational green-action adoption.

Management quality plays a supporting rather than dominant role. It is significant in several parsimonious and alternative-estimator specifications but becomes less precise once broader macro controls are introduced. This result is consistent with prior work showing that structured management can improve energy discipline, while the environmental consequences of generic management practices remain heterogeneous across firms and contexts [23–25]. The insignificant innovation-by-management interaction further suggests that the available economy-level indicators do not support a simple multiplicative complementarity mechanism.

The quantile estimates add a distributional perspective but should be interpreted cautiously. The Q25 innovation coefficient is substantially larger than the median and upper-quantile estimates, which is consistent with stronger association where the conditional level of energy-management adoption is lower. However, the bootstrap interval for Q25 narrowly includes zero, pairwise differences are insignificant under sign-based bootstrap inference, and the global equality test reaches only the 10% level. The evidence therefore supports a lower-tail pattern, not a verified dynamic catch-up mechanism.

This calibration matters conceptually. Conditional quantiles describe positions in the conditional outcome distribution and should not be equated automatically with absolute levels of development or adoption. Moreover, a cross-sectional design cannot establish "catch-up," which is inherently dynamic. Future panel or repeated-firm research with larger effective samples could test whether the lower-tail pattern reflects diffusion, convergence, or other adjustment processes over time.

The Carbon-Monitoring Gap is the clearest empirical contribution. The innovation-intensity slope is significantly weaker for CO_2 monitoring than for energy-management

adoption in the $N = 79$ macro model and the full $N = 89$ sample, after standardizing the outcomes, after allowing continent effects to vary by outcome, after deleting all six Cook-flagged economies, and in every leave-one-economy-out re-estimation. These results support an outcome-specific association, but they should not be read as showing that innovation is uniquely responsible for the gap: management quality also exhibits a significant outcome interaction, and the joint test indicates broader cross-outcome slope heterogeneity.

The organizational logic is consistent with this broader interpretation. Innovation intensity can be associated with new products, redesigned processes, equipment upgrading, experimentation, and operational efficiency. These activities may coincide with greater use of energy-management practices even where environmental reporting systems remain incomplete. Carbon monitoring, by contrast, requires measurement boundaries, reliable activity data, emissions factors, specialized accounting routines, verification, data governance, and often regulatory or stakeholder pressure to standardize reporting. The environmental-management-accounting literature emphasizes these specialized systems [14], and evidence from carbon-disclosure mandates shows that monitoring and accountability institutions can alter organizational behavior [15]. The result also has implications for ESG information quality: operational environmental activity and the production of auditable environmental information are related but analytically distinct.

The policy implication is therefore one of possible sequencing and complementarity rather than a direct prescription. Innovation-support policies may facilitate operational green upgrading, but the cross-sectional associations do not establish that such policies cause energy-management adoption. The findings are consistent with the potential value of pairing innovation ecosystems with carbon-accounting standards, technical assistance, metering infrastructure, verification capacity, disclosure frameworks, and human-capital development. These implications should be treated as plausible extensions of the observed associations rather than as policy effects identified by the present design.

For managers, the findings suggest that investment in innovation and investment in environmental measurement address related but different organizational problems. Product and process innovation can facilitate operational change, whereas carbon accountability requires dedicated measurement and reporting routines. One plausible, untested organizational interpretation is that technological upgrading can coincide with improved operational practices without comparably developed carbon-measurement systems; this mechanism cannot be established from the aggregate data. Cross-functional links among operations, finance, accounting, sustainability, and information systems may help bridge that organizational divide, although the present aggregate data cannot test this mechanism directly.

For policymakers, the results caution against treating innovation policy as a sufficient decarbonization strategy. Programs that support R&D, technology adoption, process modernization, and knowledge diffusion may be environmentally useful, yet the Carbon-Monitoring Gap is consistent with a need for complementary carbon-information infrastructure. Carbon-accounting guidance, standardized emissions factors, digital reporting systems, training, third-party verification, and proportionate disclosure requirements are plausible complements. The lower-tail quantile pattern may also motivate further investigation of whether innovation support is especially relevant where environmental-management adoption is less developed, but the present cross-sectional evidence does not establish that dynamic claim.

Several limitations define the scope of inference. First, the study is cross-sectional and observational. Reverse causality is plausible: economies with more widespread environmental practices may create stronger incentives for innovation, just as innovation intensity may facilitate environmental upgrading. The coefficient estimates should therefore be read as associations rather than treatment effects. Second, the use of economy-level aggregates

creates an ecological-inference limitation. The data describe the prevalence of firm practices within economies but do not identify how innovation and environmental practices co-vary within individual firms. The economy-level innovation-intensity construct is consequently not a substitute for firm-level innovation intensity. Continent indicators absorb broad geographic differences but are not substitutes for direct environmental-policy or institutional measures. The availability and comparability of these measures across the 89-economy WBES sample remain incomplete.

Third, the 2023–2024 survey structure limits dynamic inference. The survey-year indicator is not significant, but two cross-sectional survey waves cannot reveal whether innovation precedes environmental adoption, establish a structural break, or show whether associations persist over time. Fourth, macro controls are matched to the survey year using the most recent non-future observation within a three-year window, which introduces modest timing differences across economies. Fifth, the final specifications do not include direct measures of institutional quality, environmental-policy stringency, carbon taxes, energy prices, or carbon-reporting regulation. These factors may shape both innovation and environmental-practice adoption and could confound the reported associations. Future work should merge such measures with WBES-based indicators where comparable coverage is available.

Future work could extend the analysis using WBES firm-level microdata to estimate multilevel models with firms nested within economies. Such data would permit direct investigation of heterogeneity by firm size, sector, ownership, export status, and technological intensity while avoiding ecological interpretation. Panel data would help establish temporal ordering and test whether the observed lower-tail pattern reflects genuine diffusion or convergence over time. Future research could also incorporate environmental-policy stringency, regulatory quality, carbon taxes, energy prices, mandatory disclosure regimes, renewable-energy availability, and sectoral carbon intensity to identify why aggregate innovation intensity is associated differently with operational action and carbon monitoring.

6. Conclusions

This study examines whether economy-level innovation intensity is associated with environmental action and whether that association differs across dimensions and levels of green practice. Using harmonized World Bank Enterprise Survey indicators for 89 economies observed in 2023–2024, the analysis distinguishes between the adoption of energy-management measures and the monitoring of firm-level CO_2 emissions. The empirical unit is the economy, so the results concern cross-economy variation in the prevalence of firm practices rather than within-firm behavioral effects.

The evidence indicates a positive but not uniformly precise association between innovation intensity and energy-management adoption. The stringent intersection measure remains positive across HC3 regressions, beta regression, MM robust estimation, macro controls, continent effects, winsorization, bootstrap inference, and leave-one-economy-out re-estimation. Alternative innovation measures further show that the relationship is not confined to the intersection definition: process innovation and the standardized additive index are significant in the full sample, and product innovation, process innovation, and the additive index are significant in the macro-control sample. R&D alone is not statistically significant, so the evidence should not be described as invariant across all measures.

The distributional analysis is suggestive rather than definitive. The innovation coefficient is largest at the 25th conditional quantile, but the common-bootstrap global equality test is only marginal at the 10% level ($p = 0.077$); no pairwise difference is significant under sign-based bootstrap inference, and the Q25 percentile interval narrowly includes zero.

These results indicate a lower-tail pattern in the conditional association, not a demonstrated lower-tail mechanism.

The strongest conclusion concerns the Carbon-Monitoring Gap. The estimated CO_2-monitoring innovation slope in the macro model is close to zero and imprecise, while direct cross-outcome tests show that the slope is significantly weaker for CO_2 monitoring than for energy-management adoption. In the full $N = 89$ sample, the CO_2-minus-energy interaction is -3.945 ($p = 0.012$); with standardized outcomes it remains significant at -0.210 SD units ($p = 0.022$); with outcome-specific continent effects it equals -3.502 ($p = 0.012$); and after removal of the six Cook-flagged economies it equals -2.756 ($p = 0.037$). The interaction remains negative and significant in every leave-one-economy-out re-estimation. Because management quality also differs significantly across the two outcomes, the evidence points to broader operational-versus-monitoring heterogeneity rather than to an innovation-exclusive mechanism.

The broader implication is that technological upgrading and carbon accountability are related but distinct dimensions of the green transition. The results are consistent with the view that innovation-intensive business environments may be more conducive to operational greening, while credible carbon measurement requires additional information, accounting, verification, reporting, and institutional capacities. Given the observational design, these implications are best treated as hypotheses for policy design and future causal research rather than as estimated policy effects.

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Data Availability Statement: The data supporting the findings of this study are publicly available. Economy-level aggregate indicators were obtained from the World Bank Enterprise Surveys (https://www.enterprisesurveys.org, accessed on 1 August 2026) and macroeconomic indicators were obtained from the World Bank World Development Indicators (https://databank.worldbank.org/source/world-development-indicators, accessed on 1 August 2026). No confidential microdata were used.

Acknowledgments: The statistical analyses reported in this study were conducted using R 4.5.0.

Conflicts of Interest: The author declares no conflicts of interest.

Appendix A

Figure A1 reports the Cook's distance diagnostic and the quadratic innovation specification used during model selection. The Cook's distance plot identifies a small number of influential economy observations, while the quadratic specification tests whether a simple linear relationship masks systematic curvature. The squared innovation term is not statistically significant ($p = 0.222$), supporting the use of the linear average-effect specification together with the more informative quantile analysis.

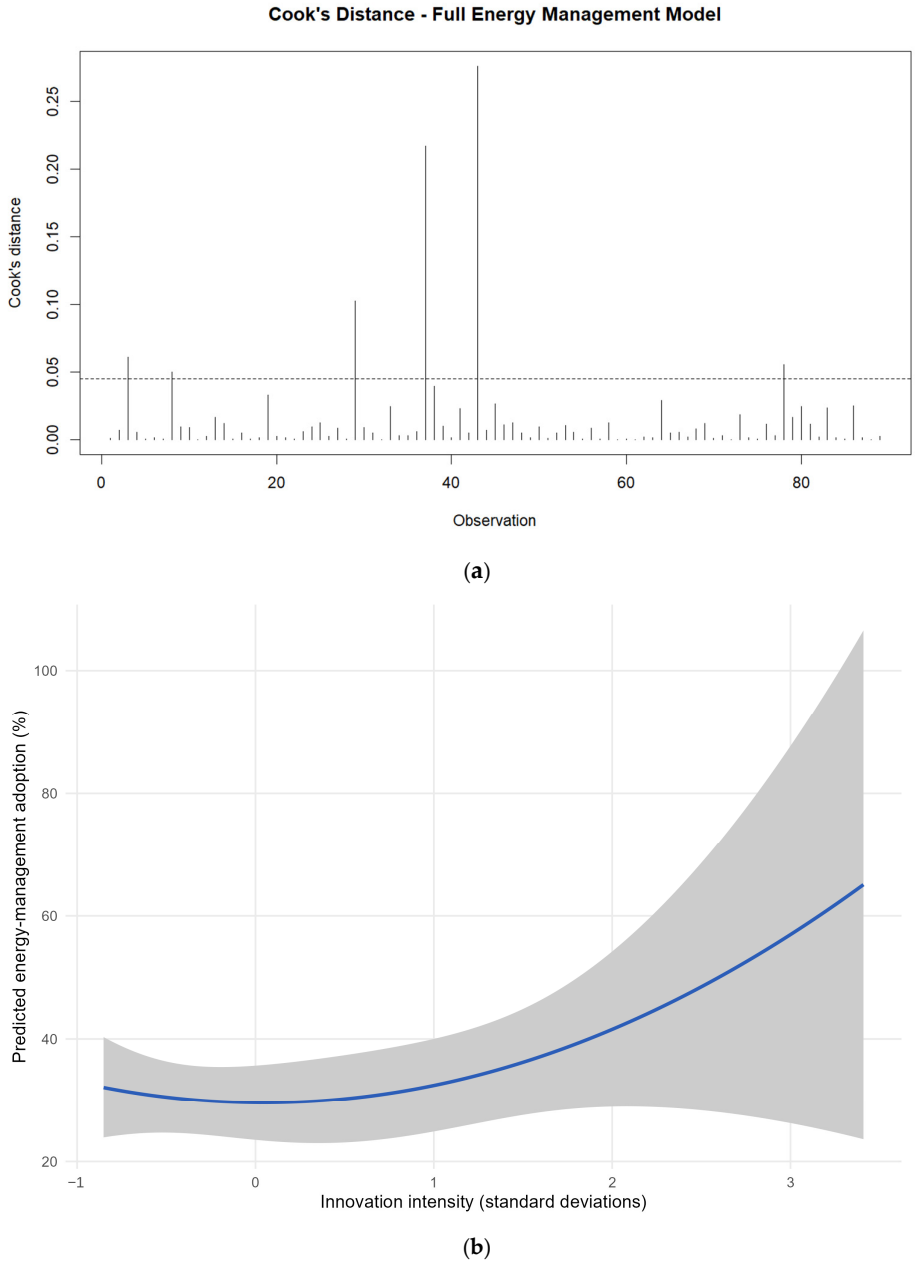


Figure A1. (**a**) Cook's distance diagnostic. (**b**) Quadratic innovation specification.

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