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Trade Openness and Carbon Productivity Across Climate Policy Conditions: A Specification-Sensitivity Analysis

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ABSTRACT: The environmental consequences of international trade may depend on the domestic policy conditions under which trade takes place. This study examines whether the relationship between trade openness and carbon productivity varies across climate-policy conditions using annual panel data for 49 economies over 2010–2023. In the principal economy and year fixed-effects specification, the interaction between trade openness and one-year-lagged climate policy stringency is positive and statistically significant. However, the positive trade association is statistically distinguishable from zero only in the upper part of the observed policy distribution, covering about 11% of country-year observations. More demanding specifications provide substantially weaker evidence. Economy-specific trends eliminate the positive interaction, while a future-policy placebo remains significant, raising concerns about temporal identification. First-difference results are also specification-sensitive, and richer institutional and structural controls attenuate the estimate. The interaction is not reproduced consistently across alternative trade measures, consumption-based hard tests, or a denominator-free specification. Overall, the positive conditional association identified in the principal fixed-effects framework does not survive the majority of the more demanding dynamic, structural, and measurement specifications. The evidence therefore does not establish a robust or causal relationship between trade openness, climate policy stringency, and carbon productivity.

KEYWORDS: Carbon productivity; climate policy stringency; consumption-based emissions; panel data; trade openness.

1. INTRODUCTION

International trade can affect environmental performance through competing mechanisms. Greater openness may expand production, transportation, and energy use, increasing carbon pressure. At the same time, trade can facilitate access to cleaner technologies, more efficient intermediate inputs, and international production knowledge. The environmental relationship associated with trade is therefore unlikely to be uniform across economies and may depend on the domestic conditions under which international integration occurs.

This ambiguity is particularly relevant for carbon productivity, which captures the amount of economic output generated relative to carbon emissions. Compared with total emissions, carbon productivity provides an efficiency-oriented perspective on the relationship between economic activity and carbon pressure. Existing studies show that trade can be associated with changes in carbon productivity, but the direction and magnitude of this relationship vary across countries and economic settings (Mahmood et al., 2024; Xu et al., 2024). Moreover, improvements in production-based carbon productivity do not necessarily imply equivalent reductions in global carbon pressure because trade can alter the geographical location of emission-intensive production and embodied emissions (Liu & Oka, 2024).

Climate policy stringency may be one condition associated with this heterogeneity. Trade expands access to technologies, inputs, and markets, while stronger climate policies can alter the relative cost of carbon-intensive activities and strengthen incentives for cleaner production. These mechanisms suggest that the trade–carbon productivity relationship may become more favourable under more stringent climate-policy conditions. However, the opposite or a weaker relationship is also plausible. Environmental regulation can impose adjustment costs, encourage production relocation, and evolve alongside broader changes in institutions, finance, technology, energy systems, and industrial structure. Climate policy stringency may therefore act as part of a wider transition process rather than as an isolated conditioning factor.

Despite substantial research on trade and environmental performance and a growing literature on environmental policy stringency, relatively little evidence directly examines whether the **trade–carbon productivity relationship itself varies across climate-policy conditions**. More importantly, a positive interaction estimated in a conventional panel model need not represent a stable policy relationship. It may depend on country-specific trends, the timing of policy changes, correlated structural developments, the construction of trade and carbon-productivity measures, or the treatment of cross-border emissions. Assessing these sources of sensitivity is therefore important for distinguishing a recurring empirical relationship from one that appears only under a particular specification.

This study examines these issues using annual panel data for 49 economies over 2010–2023. The principal analysis estimates the interaction between trade openness and one-year-lagged climate policy stringency within an economy and year fixed-effects framework and evaluates the conditional trade association across the observed policy distribution. The analysis then examines whether the empirical relationship is preserved when economy-specific dynamics, broader institutional and structural conditions, alternative measures of trade exposure, consumption-based carbon accounting, and alternative model formulations are considered. Because the study does not exploit an exogenous source of variation in climate policy stringency, all estimates are interpreted as conditional within-economy associations rather than causal effects.

The study contributes in two related ways. First, it connects the trade–environment and environmental-policy literatures by examining climate policy stringency as a condition potentially associated with heterogeneity in the trade–carbon productivity relationship. Second, rather than relying on a single interaction estimate, it evaluates how dependent that relationship is on dynamic assumptions, structural controls, trade measurement, and carbon accounting. This distinction is important because an empirical interaction observed in a standard fixed-effects framework may have substantially different implications if it is not reproduced under more demanding specifications. The study therefore provides a deliberately cautious assessment of both the apparent conditional relationship and the limits of interpreting it as a stable or causal policy mechanism.

2. LITERATURE REVIEW

The environmental consequences of international trade are commonly explained through the scale, composition, and technique channels. Trade can increase emissions by expanding economic activity and energy demand, while changes in production structure and access to cleaner technologies may offset part of this pressure (Copeland & Taylor, 2004). Empirical evidence therefore does not support a uniform relationship between trade openness and environmental performance. Dou et al. (2023), for example, document nonlinear and heterogeneous relationships between trade openness and carbon emissions, reinforcing the view that the environmental implications of trade depend on country characteristics and economic conditions.

Carbon productivity provides a related but distinct perspective because it captures economic output relative to carbon emissions rather than emissions alone. Recent evidence suggests that international trade can be associated with carbon productivity, although the relationship differs across settings. Xu et al. (2024) report a positive association between international trade and carbon productivity among Belt and Road economies, with differences across income groups. Mahmood et al. (2024), using evidence from MENA economies, find that trade openness is negatively associated with domestic carbon productivity but generates positive spatial spillovers, producing a more favourable regional relationship. These findings suggest that the trade–carbon productivity relationship is heterogeneous rather than universal.

An additional complication is that production-based carbon productivity may not fully represent the environmental implications of trade. International integration allows economies to import carbon-intensive goods and relocate emission-intensive production. Improvements in domestic carbon productivity may therefore reflect technological upgrading, structural reallocation, or changes in the geographical distribution of emissions. Liu and Oka (2024) show that changes in carbon productivity under international trade can be connected to transfers of production factors and embodied emissions. Consequently, a favourable domestic trade–carbon productivity relationship should not automatically be interpreted as an improvement in global carbon efficiency.

Environmental policy may help explain part of this heterogeneity. The Porter hypothesis proposes that well-designed environmental regulation can stimulate innovation and efficiency improvements that partly offset compliance costs (Porter & van der Linde, 1995). Empirical evidence, however, remains mixed. Albrizio et al. (2017) show that productivity responses to environmental policy vary with firms' technological position and adjustment capacity. Yasmeen et al. (2024) find that environmental policy stringency is associated with higher carbon, energy, and non-energy productivity in emerging economies. In contrast, Arjomandi et al. (2023) report that tighter environmental policies can reduce green economic and productivity growth over longer horizons, while Benatti et al. (2024) find adverse productivity responses among highly polluting firms, with considerable heterogeneity across firms and policy conditions. Stronger environmental policy therefore cannot be assumed to improve productivity uniformly.

These mixed findings suggest that climate policy may be relevant not only through its direct relationship with environmental performance but also through the conditions it creates for adjustment to trade. Trade can expand access to technologies, intermediate inputs, and international knowledge, whereas stronger climate policies can increase the relative cost of carbon-intensive production. Under more stringent policy conditions, firms may therefore have stronger incentives to use trade-related opportunities for cleaner production and efficiency improvements. Related evidence supports the possibility of such conditional relationships. Fatima et al. (2024) find that environmental policy stringency can strengthen the environmental contribution of energy transition. However, the relationship need not always be favourable: Kim et al. (2025) show that greater policy stringency can weaken the positive association between trade openness and green technological development under some conditions.

Carbon leakage provides an important competing mechanism. More stringent environmental policies can encourage domestic upgrading but may also increase incentives to relocate emission-intensive production or substitute toward carbon-intensive imports. Olasehinde-Williams and Akadiri (2025) provide evidence linking environmental policy stringency with cross-border carbon leakage. A positive interaction between trade openness and policy stringency in a production-based carbon-productivity model could therefore represent cleaner domestic production, structural change, international displacement of emissions, or some combination of these channels. This ambiguity limits causal interpretation and motivates the use of consumption-based carbon measures as a complementary outcome.

Overall, the existing literature has developed substantial evidence on trade and environmental performance and, separately, on environmental policy stringency and productivity. Much less evidence directly examines whether the trade–carbon productivity relationship itself varies across climate-policy conditions. This study addresses that gap by treating climate policy stringency as a conditioning feature of the empirical relationship between trade openness and carbon productivity, while recognising that policy stringency may also be correlated with broader institutional, financial, technological, and energy conditions.

Accordingly, the study proposes the following hypothesis:

Hypothesis: The marginal association between trade openness and carbon productivity becomes more positive as climate policy stringency increases.

This hypothesis concerns a conditional empirical association rather than a causal policy effect. It does not imply that increasing climate policy stringency necessarily causes trade openness to improve carbon productivity.

3. METHOD

This study uses annual country-level data for 49 economies over 2010–2023. Climate policy stringency is obtained from the OECD Climate Actions and Policies Measurement Framework (CAPMF), while the principal macroeconomic and structural variables are drawn from the World Bank's World Development Indicators (WDI). Additional institutional indicators are obtained from the Worldwide Governance Indicators (WGI), and consumption-based CO_2 emissions are taken from the Global Carbon Budget data distributed through Our World in Data. The principal estimation sample contains 685 country-year observations.

The dependent variable is the natural logarithm of carbon productivity. Production-based carbon productivity is defined as the reciprocal of carbon intensity:

$$\mathrm{CP_{it}} = \frac{1}{\mathrm{CI_{it}}} \tag{1}$$

The principal explanatory variable is trade openness, measured as exports plus imports relative to GDP. Climate policy stringency is measured by the OECD CAPMF composite index, which ranges from 0 to 10, with higher values representing greater policy stringency. The principal specification uses the one-year-lagged policy measure to establish temporal ordering, although this lag is not interpreted as providing exogenous policy variation.

To facilitate interpretation of the interaction, trade openness and lagged climate policy stringency are mean-centred:

$$TO^{c}_{\mathrm{it}} = TO_{\mathrm{it}} - \overline{\mathrm{TO}};$$

$$\mathrm{CPS}^{c}_{\mathrm{i,t-1}} = \mathrm{CPS_{i,t-1}} - \overline{\mathrm{CPS}^{L1}} \tag{2}$$

The principal empirical model is:

$$\ln(\mathrm{CP_{it}}) = \alpha + \beta_1 \mathrm{TO}^{c}_{\mathrm{it}} + \beta_2 \mathrm{CPS}^{c}_{\mathrm{i,t-1}} + \beta_3\left(\mathrm{TO}^{c}_{\mathrm{it}} \times \mathrm{CPS}^{c}_{\mathrm{i,t-1}}\right) + \gamma_1 \ln(\mathrm{GDPpc_{it}}) + \gamma_2 \mathrm{IND_{it}} + \gamma_3 \mathrm{URB_{it}} + \mu_i + \lambda_t + \varepsilon_{it} \tag{3}$$

Here, μ_i and λ_t denote economy and year fixed effects, respectively. The control variables are real GDP per capita, industry value added as a share of GDP, and the urban population share. Standard errors are clustered at the economy level. The coefficient of primary interest is β_3, which indicates whether the trade–carbon productivity relationship varies with lagged climate policy stringency.

Before constructing the interaction term, trade openness and lagged climate policy stringency are mean-centred using their estimation-sample means. Mean-centering changes the reference point of the constituent lower-order coefficients but does not alter the interaction coefficient, fitted values, or marginal relationships implied by the model. The variables are not standardised by their standard deviations; consequently, trade openness remains measured in percentage points of GDP and climate policy stringency remains expressed in its original CAPMF units. In the interaction specification, the coefficient on trade openness therefore represents its conditional association with carbon productivity when climate policy stringency is at its centering value, rather than an unconditional average association.

Because interaction coefficients alone do not reveal the conditional relationship between trade openness and carbon productivity, the marginal association of trade is evaluated across the observed policy distribution:

$$\frac{\partial \ln(\mathrm{CP_{it}})}{\partial \mathrm{TO}^{c}_{\mathrm{it}}} = \beta_1 + \beta_3 \mathrm{CPS}^{c}_{\mathrm{i,t-1}} \tag{4}$$

The Johnson–Neyman procedure is used to identify the range of policy stringency over which this marginal association is statistically distinguishable from zero.

Table 1. Variable definitions and data sources

Variable	Symbol	Measurement	Source
Carbon productivity	ln(CP)	Log reciprocal of production-based carbon intensity	World Bank WDI
Trade openness	TO	Exports plus imports as % of GDP	World Bank WDI
Climate policy stringency	CPS	CAPMF composite index, 0–10	OECD CAPMF
Lagged policy stringency	$\mathrm{CPS_{t-1}}$	CPS lagged one year	OECD CAPMF; authors' calculation
Trade × policy	INT	Centred trade × centred lagged CPS	Authors' calculation
Real GDP per capita	Ln(GDPpc)	Natural logarithm of real GDP per capita	World Bank WDI
Industry share	IND	Industry value added as % of GDP	World Bank WDI
Urbanisation	URB	Urban population as % of total population	World Bank WDI
Institutional quality	$\mathrm{INST_{t-1}}$	Lagged mean of rule of law, government effectiveness, and control of corruption	World Bank WGI; authors' calculation
Financial development	$\mathrm{FIN_{t-1}}$	Lagged private-sector credit as % of GDP	World Bank WDI; authors' calculation
Renewable-energy share	$\mathrm{REN_{t-1}}$	Lagged renewable share of final energy consumption	World Bank WDI; authors' calculation
Consumption-based CP	$\ln(\mathrm{CP^{cons}})$	Log real GDP divided by consumption-based CO_2	World Bank WDI; Global Carbon Budget/Our World in Data

Real trade	ln(RT)	Log real exports plus imports	World Bank WDI; authors' calculation
Real trade per capita	ln(RTPC)	Log real trade divided by population	World Bank WDI; authors' calculation
Real exports/imports	ln(EX)\ln(IM)	Log real exports and imports	World Bank WDI

Source: Authors' compilation from OECD CAPMF, World Bank WDI and WGI, and Global Carbon Budget/Our World in Data.

Several sensitivity analyses are used to evaluate how dependent the principal interaction is on specification and measurement choices. Inference is examined using wild-cluster bootstrap and Driscoll–Kraay standard errors. Dynamic sensitivity is assessed using economy-specific linear trends, future-policy placebo interactions, and two distinct first-difference formulations. The first-difference-outcome specification relates changes in carbon productivity to one-year-lagged explanatory variables expressed in levels. The strict first-difference specification instead applies the first-difference transformation to the dependent variable and each right-hand-side term in Equation (3), including the interaction term. Because the two formulations represent different short-run relationships, they are reported separately. To distinguish differences in specification from differences in sample composition, the two models are also compared on an identical estimation sample. Additional specifications introduce lagged institutional quality, financial development, and renewable-energy structure, both separately and jointly.

Measurement sensitivity is assessed using real trade, real trade per capita, real exports, and real imports in place of the conventional trade-openness ratio. Carbon productivity is also reconstructed using consumption-based CO_2 emissions:

$$\ln(\mathrm{CP}_{\mathrm{it}}^{\mathrm{cons}}) = \ln\left(\frac{\mathrm{GDP}_{\mathrm{it}}^{2015}}{\mathrm{CO}_{2,\mathrm{it}}^{\mathrm{cons}}}\right) \tag{5}$$

Finally, because both conventional trade openness and production-based carbon productivity contain GDP components, an additional component specification avoids a common GDP denominator. It relates log consumption-based CO_2 emissions to log real trade, lagged policy stringency, their interaction, and separate linear and quadratic controls for log real GDP, together with the remaining controls and fixed effects. This specification is treated as a measurement sensitivity test rather than as an alternative causal model.

All estimations are conducted in Stata. Given the observational design and the absence of an exogenous source of variation in climate policy stringency, all estimated relationships are interpreted as conditional within-economy associations rather than causal effects.

4. EMPIRICAL RESULTS

Table 2 reports descriptive statistics for the principal estimation sample of 685 economy-year observations. Logged carbon productivity averages 1.245, while trade openness averages 96.77% of GDP and varies substantially across the sample. Climate policy stringency averages 3.91, and its one-year-lagged value averages 3.71. The dispersion in both trade openness and policy stringency provides the within-sample variation used to examine their conditional relationship.

Table 2. Descriptive statistics

Variable	Observations	Mean	Standard deviation	Minimum	Maximum
Log carbon productivity	685	1.2446	0.7275	−0.3988	3.2003
Carbon intensity	685	0.3772	0.3059	0.0407	1.4900
Trade openness (% of GDP)	685	96.7726	60.7041	22.4862	412.1772
Climate policy stringency	685	3.9062	1.2305	0.5203	7.0338
Lagged climate policy stringency	685	3.7137	1.2528	0.5203	6.7697
Log real GDP per capita	685	9.9766	0.8739	7.1190	11.6161
Industry value added (% of GDP)	685	25.5620	7.5618	9.1138	63.0804
Urban population (% of total)	685	75.1181	13.3806	30.9042	95.6272

Notes: Lagged climate policy stringency for 2010 uses pre-sample 2009 CAPMF observations.
Source: Authors' calculations based on OECD CAPMF and World Bank WDI data.

Table 3 presents the fixed-effects estimates. Model 1 excludes the interaction term. Trade openness is positively associated with carbon productivity in this specification, whereas contemporaneous climate policy stringency is statistically insignificant. Model 2 introduces the interaction between trade openness and contemporaneous policy stringency. Model 3 is the principal specification and replaces contemporaneous policy stringency with its one-year lag.

In Model 3, the trade openness × lagged policy stringency interaction is positive and statistically significant (β=0.000700, SE = 0.000202, p = 0.001). The lower-order coefficients of trade openness and lagged policy stringency are statistically insignificant. Because the interacting variables are mean-centred, these coefficients describe conditional associations evaluated at the mean of the other interacting variable rather than unconditional relationships.

Table 3. Fixed-effects estimates of trade openness, climate policy stringency, and carbon productivity

Variables	Model 1: Baseline	Model 2: Current interaction	Model 3: Lagged interaction
Trade openness	0.003517* (0.001141)	0.001536 (0.001304)	0.001454** (0.001372)
Current climate policy stringency	0.018393 (0.031964)	0.019317 (0.027220)	—
Lagged climate policy stringency	—	—	0.018316 (0.023239)
Trade × current policy stringency	—	0.000699*** (0.000210)	—
Trade × lagged policy stringency	—	—	0.000700* (0.000202)
Log real GDP per capita	0.494939* (0.131435)	0.407702* (0.122910)	0.414616* (0.125378)
Industry value added	0.000126 (0.003450)	0.000831 (0.003397)	0.000882 (0.003469)
Urban population share	−0.008389* (0.004579)	−0.008147** (0.003836)	−0.008024** (0.003842)
Economy fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Economy-clustered standard errors	Yes	Yes	Yes
Observations	685	685	685
Economies	49	49	49
Within R^2	0.7753	0.7990	0.8008

Notes: Dependent variable: log carbon productivity. Economy-clustered standard errors in parentheses. *** p<0.01; ** p<0.05; * p<0.10.
Source: Authors' estimations.

The positive interaction coefficient does not imply that trade openness is positively associated with carbon productivity throughout the observed policy distribution. Table 4 therefore reports conditional marginal associations from the principal specification. The estimated trade association is statistically insignificant at the 25th percentile, the sample mean, and the 75th percentile of lagged policy stringency. It becomes statistically distinguishable from zero only toward the upper end of the observed distribution.

Table 4. Conditional marginal association of trade openness

Policy condition	Lagged policy stringency	Marginal association	Standard error	p-value	95% confidence interval
25th percentile	2.908	0.000887	0.001447	0.543	[−0.002023, 0.003797]
Sample mean	3.714	0.001451	0.001372	0.296	[−0.001308, 0.004210]
75th percentile	4.541	0.002030	0.001312	0.128	[−0.000608, 0.004668]
Maximum observed level	6.770	0.003590***	0.001251	0.006	[0.001076, 0.006105]

Notes: Marginal associations are based on Model 3. *** p<0.01.
Source: Authors' calculations from the principal fixed-effects specification.

A fine-grid Johnson–Neyman calculation places the statistical boundary at approximately 5.299. Only 75 of the 685 observations, representing about 11% of the principal sample and 20 of the 49 economies, lie at or above this value. The region in which the positive trade association is statistically distinguishable from zero is therefore relatively narrow.

The model-implied magnitudes should be interpreted in the same conditional manner. A 10-percentage-point increase in trade openness corresponds to approximately 1.46% higher carbon productivity at the sample-mean policy level, although the underlying marginal association is statistically insignificant there. The corresponding association is approximately 2.59% at the Johnson–Neyman boundary and 3.66% at the maximum observed policy value. These magnitudes are not negligible in percentage terms, but their substantive relevance is limited by the narrow policy region over which the association is statistically distinguishable from zero and by the specification sensitivity documented below.

Alternative inference procedures do not by themselves overturn the statistical significance of the principal interaction. Wild-cluster bootstrap inference gives p = 0.002, and Driscoll–Kraay standard errors also retain statistical significance. The formal Pesaran CD test does not reject cross-sectional independence (p = 0.988), but the average absolute pairwise residual correlation is 0.428. The latter indicates substantial residual comovement and suggests that the near-zero signed-average CD statistic may mask correlations of opposite signs. Driscoll–Kraay estimates are therefore treated as complementary inference checks rather than evidence that cross-sectional dependence is absent. Given the short time dimension of the panel, these results should also be interpreted cautiously.

Several conventional sensitivity exercises preserve the positive interaction. The estimate remains positive after winsorising trade openness at the first and ninety-ninth percentiles, excluding observations with trade openness above 200% of GDP, lagging all explanatory variables, excluding 2020–2021, and sequentially removing individual economies. These checks indicate that the principal estimate is not driven solely by extreme trade observations, the pandemic period, or a single economy. They do not, however, address the central identification concerns arising from gradual economy-specific trajectories and policy timing.

The economy-specific-trend specification provides the first major qualification. Once individual linear trends are added, the interaction changes sign to −0.000331 and becomes statistically insignificant under both conventional clustered and wild-cluster bootstrap inference. This result indicates that the positive principal estimate cannot be clearly separated from gradual economy-specific trajectories in the available data.

The future-policy placebo provides an additional and particularly important identification challenge. On the same restricted estimation sample, the interaction with lagged policy stringency is statistically insignificant, whereas the interaction involving future policy stringency remains positive and statistically significant. This pattern is inconsistent with a clean temporal interpretation in which prior policy conditions generate the principal relationship. Instead, it is consistent with the possibility that trade openness, climate policy stringency, and carbon productivity move together as part of broader time-varying national transition processes.

The first-difference results also depend strongly on model formulation. The first-difference-outcome specification, which relates changes in carbon productivity to lagged explanatory variables in levels, produces a negative and statistically insignificant interaction. A strict first-difference transformation of Equation (3), in contrast, produces a positive interaction under conventional clustered inference. Re-estimating both formulations on the identical sample of 636 observations leaves this contrast essentially unchanged: the first-difference-outcome estimate is −0.000063 ($p = 0.210$), whereas the strict first-difference estimate is 0.000339 ($p = 0.006$). The contrast is therefore not explained by sample composition. However, wild-cluster bootstrap inference for the strict estimate gives $p = 0.052$, and its bootstrap confidence set crosses zero. The first-difference evidence consequently indicates sensitivity to the treatment of short-run dynamics rather than independent confirmation of the principal fixed-effects result.

Broader institutional and structural controls lead to a similar qualification. Adding institutional quality, financial development, and renewable-energy structure separately preserves a positive interaction. When these variables are included jointly, however, the coefficient falls substantially. On the same 604-observation sample, the principal model yields an interaction of 0.000609, whereas the full-control specification yields 0.000304, with $p = 0.054$ under clustered inference and $p = 0.068$ under wild-cluster bootstrap inference. The attenuation is therefore not driven simply by the smaller estimation sample. More importantly, combining the full control set with economy-specific trends produces an interaction of −0.000146, with clustered $p = 0.624$ and wild-bootstrap $p = 0.770$. This demanding combined specification provides no evidence supporting the hypothesised positive relationship.

Measurement sensitivity further limits the generality of the principal estimate. On a common sample, the conventional trade-openness interaction remains positive and statistically significant, as does the interaction based on real trade per capita. In contrast, interactions based on aggregate real trade, real exports, and real imports are statistically insignificant. The relationship is therefore not invariant to the measurement of trade exposure.

The consumption-based results provide a complementary test of carbon accounting. The positive interaction is reproduced when carbon productivity is constructed using consumption-based CO_2 emissions in the principal fixed-effects specification. However, it is not reproduced when economy-specific trends or first-difference formulations are introduced. Neither the lagged nor the future-policy interaction is statistically significant in the corresponding consumption-based placebo specification. The consumption-based evidence therefore shows that the principal pattern is not confined entirely to production-based accounting, but it does not provide robust confirmation under the more demanding dynamic specifications.

Finally, the denominator-free component specification removes the common GDP denominator by modelling log consumption-based CO_2 emissions as a function of log real trade and separate GDP controls. The trade × policy interaction is negative, which is directionally consistent with the hypothesised relationship in an emissions equation, but is statistically insignificant under both clustered and wild-cluster bootstrap inference. The denominator-free model therefore does not confirm the principal interaction and leaves the possibility of common-denominator influence unresolved.

Table 5. Inference, specification, and measurement sensitivity

Specification	Interaction coefficient	Standard error	Clustered p	Wild p	N
Panel A. Dynamic and identification sensitivity					
Principal two-way fixed effects	0.000700	0.000202	0.001	0.002	685
Economy-specific trends	−0.000331	0.000389	0.399	0.459	685
First-difference outcome specification	−0.000063	0.000049	0.210	0.410	636
Strict first differences	0.000339	0.000117	0.006	0.052	636
Placebo sample: main lag	0.000348	0.000218	0.116	0.174	637
Future-policy placebo	0.000362	0.000177	0.046	0.046	637
Panel B. Extended controls					
+ Institutional quality	0.000699	0.000199	0.001	—	685

+ Financial development	0.000608	0.000183	0.002	—	649
+ Renewable-energy share	0.000396	0.000160	0.017	—	637
Baseline, full-control common sample	0.000609	0.000208	0.005	—	604
Full extended controls	0.000304	0.000154	0.054	0.068	604
Full controls + economy trends	−0.000146	0.000296	0.624	0.770	604
Panel C. Measurement sensitivity					
Original trade openness, common sample	0.000725	0.000211	0.001	—	670
Log real trade × policy	0.002841	0.007115	0.691	—	670
Log real trade per capita × policy	0.045128	0.005240	<0.001	—	670
Log real exports × policy	−0.000167	0.007856	0.983	—	670
Log real imports × policy	0.003901	0.007041	0.582	—	670
Consumption CP: principal FE	0.000433	0.000137	0.003	0.017	671
Consumption CP: economy trends	0.000095	0.000309	0.761	0.699	671
Consumption CP: strict first differences	0.000163	0.000170	0.343	0.394	623
Consumption placebo: main lag	0.000175	0.000240	0.469	0.508	624
Consumption placebo: future policy	0.000271	0.000264	0.310	0.644	624
Denominator-free component model	−0.012870	0.009152	0.166	0.251	656

Notes: Entries report the interaction between the relevant trade measure and lagged climate policy stringency unless otherwise indicated. Wild p-values are based on 9,999 wild-cluster bootstrap replications. The two production first-difference models are reported on an identical sample of 636 observations. Alternative trade measures and the denominator-free model use different scales and are therefore not directly comparable in coefficient magnitude. In the denominator-free emissions specification, a negative interaction is directionally consistent with the proposed relationship.

Source: Authors' estimations.

Because these exercises evaluate the same substantive hypothesis across a relatively large set of correlated specifications and measurement choices, statistical significance near conventional thresholds should not be treated as multiple independent confirmations. The sensitivity exercises are used to assess the stability of the relationship rather than to accumulate evidence through repeated hypothesis testing. Borderline p-values are therefore interpreted cautiously.

Taken together, the evidence is substantially weaker outside the principal fixed-effects framework. The positive interaction appears clearly in the conventional two-way fixed-effects specification, but the majority of the more demanding dynamic, structural, and measurement specifications do not reproduce the hypothesised relationship. The disappearance of the interaction under economy-specific trends, the significant future-policy placebo, the attenuation under richer controls, the contrasting first-difference results, and the sensitivity to alternative trade and carbon measures all limit the interpretation of the principal estimate. The empirical results therefore document an apparent conditional association in the principal fixed-effects framework, while also showing that this relationship is not sufficiently stable to support a robust or causal policy interpretation.

5. DISCUSSION AND IMPLICATIONS

The results reveal an important distinction between the relationship identified in the principal fixed-effects model and the broader body of evidence. In the principal specification, the association between trade openness and carbon productivity becomes more positive as climate policy stringency increases. However, this relationship is statistically distinguishable from zero only in the upper part of the observed policy distribution, covering about 11% of country-year observations. More importantly, the positive interaction is not reproduced consistently once stronger assumptions about country-specific dynamics, structural conditions, and measurement are imposed. The principal estimate should therefore be interpreted as a conditional empirical pattern rather than as evidence of a stable policy relationship.

The dynamic tests provide the strongest reason for caution. Economy-specific trends eliminate the positive interaction, indicating that the principal estimate cannot be clearly separated from gradual economy-specific trajectories in the available data. The future-policy placebo raises an additional concern: the future-policy interaction is statistically significant while the lagged interaction is insignificant on the same restricted sample. This pattern is consistent with the possibility that the principal relationship partly captures broader national transition processes that evolve jointly with trade integration, climate-policy stringency, and carbon productivity. Such processes may include technological upgrading, changes in industrial structure, energy transition, institutional development, and wider decarbonisation trends.

The first-difference results reinforce this sensitivity rather than resolving it. A strict first-difference transformation produces a positive interaction under conventional clustered inference, whereas the first-difference-outcome formulation remains statistically insignificant when both are estimated on the same sample. The strict estimate also fails to meet the conventional 5% threshold under wild-cluster bootstrap inference. The difference between the two formulations is therefore best interpreted as sensitivity to the modelling of short-run dynamics rather than as independent confirmation of the principal fixed-effects result.

Broader controls and alternative measurements lead to a similar conclusion. Institutional quality, financial development, and renewable-energy structure individually leave the principal interaction largely intact, but their joint inclusion substantially attenuates the estimate. When the full control set is combined with economy-specific trends, no evidence of the hypothesised positive interaction remains. This suggests that climate policy stringency is closely associated with broader institutional and structural transformations and should not be interpreted as an isolated conditioning factor.

Measurement choices also remain consequential. The interaction is not reproduced consistently when aggregate real trade, exports, or imports replace the conventional trade-openness ratio. This matter because both trade openness and carbon productivity incorporate GDP components. The denominator-free specification does not confirm the principal relationship, leaving the possibility of common-denominator influence unresolved. Similarly, the positive interaction obtained with consumption-based carbon productivity shows that the baseline pattern is not confined entirely to production-based accounting, but its disappearance under more demanding trend and first-difference specifications limits the strength of this evidence. The results therefore cannot establish that stronger climate policy prevents carbon leakage or produces broader reductions in global carbon pressure.

These findings are consistent with the heterogeneous evidence in the existing literature. Trade can generate scale, composition, and technique effects whose balance differs across economic environments (Copeland & Taylor, 2004). Likewise, environmental regulation has been associated with productivity improvements in some settings and adjustment costs in others (Albrizio et al., 2017; Arjomandi et al., 2023; Benatti et al., 2024; Yasmeen et al., 2024). The present results add to this literature by showing that even an apparently favourable trade–policy interaction in a conventional fixed-effects framework can be highly dependent on dynamic assumptions, structural controls, and variable construction.

The economic interpretation should also remain proportionate to the evidence. At relatively high policy-stringency values, the estimated magnitudes from the principal model are not negligible in percentage terms. However, their practical relevance is limited by the narrow portion of the policy distribution over which the marginal trade association is statistically distinguishable from zero and by the instability of the interaction across demanding specifications. These model-implied magnitudes should therefore not be interpreted as reliable policy effects.

The policy implications are consequently limited. The findings do not support trade liberalisation as an environmental strategy in itself, nor do they imply that governments should target a particular value of the CAPMF index. The Johnson–Neyman boundary is a sample-specific statistical region rather than an optimal policy threshold. Moreover, CAPMF combines multiple instruments, so the analysis cannot identify whether carbon pricing, regulatory standards, subsidies, information measures, or other policy components are responsible for the principal interaction.

Generalisability is another limitation. Forty of the 49 economies in the sample are classified as high income, so the estimates primarily reflect variation among relatively advanced economies. The results should therefore not be generalised to developing economies without additional evidence. More broadly, the observational design does not provide an exogenous source of variation in either climate policy stringency or trade exposure. The study consequently identifies conditional associations and their sensitivity rather than causal policy effects.

The contribution of the study is therefore deliberately qualified. It documents a positive trade–policy interaction in a conventional fixed-effects framework while showing that this relationship is not robust to the majority of more demanding dynamic, structural, and measurement specifications. This combination is informative because it demonstrates both the apparent conditional pattern and the limits of interpreting such a pattern as a stable environmental-policy relationship.

6. CONCLUSION

This study examines whether the relationship between trade openness and carbon productivity varies across climate-policy conditions using annual panel data for 49 economies over 2010–2023. The principal economy and year fixed-effects specification identifies a positive interaction between trade openness and lagged climate policy stringency. However, the positive trade association is statistically distinguishable from zero only in a relatively narrow upper portion of the observed policy distribution, and the relationship does not survive the majority of the more demanding identification and measurement checks.

The strongest qualifications arise from the dynamic specifications. Economy-specific trends eliminate the positive interaction, while a future-policy placebo remains statistically significant even though the lagged interaction is insignificant on the same sample. First-difference results are also dependent on model formulation: the strict transformation is positive under conventional clustered inference but does not meet the 5% threshold under wild-cluster bootstrap inference, whereas the alternative first-difference-outcome specification remains null. These results do not provide a clean basis for separating the principal interaction from broader time-varying economy-specific processes.

Structural and measurement tests lead to the same general conclusion. The interaction attenuates under richer institutional, financial, and energy controls and disappears when these controls are combined with economy-specific trends. Alternative trade measures provide inconsistent evidence, consumption-based carbon productivity does not reproduce the relationship under demanding dynamic specifications, and the denominator-free model provides no statistically precise confirmation.

The overall evidence therefore does not establish a robust relationship in which greater climate policy stringency systematically strengthens the association between trade openness and carbon productivity. The more limited conclusion is that a positive conditional pattern appears in the principal fixed-effects framework but is sensitive to country-specific dynamics, broader structural conditions, carbon accounting, and the measurement of trade exposure.

Future research could strengthen causal identification by exploiting discrete and plausibly exogenous changes in specific climate-policy instruments, such as major emissions-trading reforms or the introduction of carbon-border measures, using event-study or difference-in-differences designs with appropriate sectoral exposure and comparison groups. Extending the time horizon would be particularly important for evaluating reforms with only a short post-policy period in the present sample.

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Appendix

Appendix A. Exploratory Income-Group Heterogeneity

Income-group heterogeneity is examined as an exploratory extension of the principal specification. Separate fixed-effects estimates yield positive trade–policy interaction coefficients for both high-income and non-high-income economies, with a larger point estimate for the latter group. However, the pooled specification provides no evidence that the interaction differs statistically between the two groups. The coefficient on the three-way interaction between trade openness, lagged climate policy stringency, and the non-high-income indicator is 0.000306 ($p = 0.630$), while the corresponding wild-cluster bootstrap p-value is 0.604. Given that the non-high-income subsample contains only nine economies, the subgroup estimates should therefore be interpreted descriptively rather than as evidence of systematic income-group heterogeneity.

Table A1. Exploratory income-group heterogeneity

Specification	Interaction coefficient	Standard error	p-value	Wild p-value	Observations	Economies
High-income economies	0.000403**	0.000190	0.040	—	559	40
Non-high-income economies	0.001918**	0.000604	0.013	—	126	9
Pooled income-group difference	0.000306	—	0.630	0.604	685	49

Notes: The first two rows report the trade openness × lagged climate policy stringency interaction estimated separately by income group. The pooled difference is the coefficient on the three-way interaction between trade openness, lagged policy stringency, and the non-high-income indicator. The wild p-value is based on wild-cluster bootstrap inference. ** p<0.05.
Source: Authors' estimations.

Appendix B. Sample economies by income group

Income group	Economies
High income (40)	Australia; Austria; Belgium; Bulgaria; Canada; Chile; Costa Rica; Croatia; Czechia; Denmark; Estonia; Finland; France; Germany; Greece; Hungary; Iceland; Ireland; Israel; Italy; Japan; Korea, Rep.; Latvia; Lithuania; Luxembourg; Malta; Netherlands; New Zealand; Norway; Poland; Portugal; Romania; Russian Federation; Saudi Arabia; Slovak Republic; Slovenia; Spain; Sweden; Switzerland; United Kingdom
Non-high income (9)	Argentina; China; Colombia; India; Indonesia; Mexico; Peru; South Africa; Turkiye

Notes: Income classification follows the classification used in the dataset. The non-high-income group comprises eight upper-middle-income economies and one lower-middle-income economy (India). The principal sample contains 40 high-income and nine non-high-income economies.

Source: Authors' compilation.