Research Square

Preprints are preliminary reports that have not undergone peer review.
They should not be considered conclusive, used to inform clinical practice,
or referenced by the media as validated information.

Institutional Quality, Environmental Policy Stringency and the Decarbonisation of Energy Supply in G7 Economies: Evidence from the Carbon Intensity of Energy

Precious Doe

doejnr@gmail.com

University of Ghana Business School https://orcid.org/0009-0002-1947-5068

Research Article

Keywords: institutional quality, environmental policy stringency, carbon intensity of energy, G7 economies, panel data, measurement

Posted Date: November 10th, 2026

DOI: https://doi.org/10.21203/rs.3.rs-11652976/v1

License: This work is licensed under a Creative Commons Attribution 4.0 International License. Read Full License

Additional Declarations: The authors declare no competing interests.

Abstract

This study examines how institutional quality and environmental policy stringency relate to the decarbonisation of energy supply in the G7 economies from 2005–2023. The study estimated two-way fixed effects with wild cluster bootstrap inference to correct for the small country sample. Environmental policy stringency has no robust association with any decarbonisation measure. Neither institutional quality nor economic development moderates policy effectiveness, and the two moderating effects are not statistically distinguishable from one another. There is no robust evidence that the Paris Agreement modified these relationships. The central contribution is that the strong institutional effects reported in this literature may be artefacts of measuring decarbonisation with an outcome that is mechanically determined by output and energy intensity.

1. Introduction

The parties to the Paris Agreement (United Nations Climate Change, 2016) have agreed to work with a common goal to pursue efforts to limit global temperature increase to 1.5°C above pre-industrial levels. Yet there is a slow pace of decarbonisation, and the energy transition poses a critical global challenge (Kartal et al., 2025). The Group of 7 (G7) nations, which comprise the United States, Germany, France, Italy, the United Kingdom, Canada and Japan, contribute to about 40% of the world economy, consume 30% of world energy demands and contribute about 25% of the world global CO_2 emissions from the energy sector. These countries contribute more than one-sixth of the total CO_2 emissions from the global energy system, contributing about 6 billion tonnes of CO_2 emissions annually (International Energy Agency (IEA), 2022). Data from the International Institute for Sustainable Development posits that total subsidies for fossil fuel among the G7 nations in 2022 reached an all-time high of approximately USD 199.1 billion (International Institute for Sustainable Development (IISD), 2024). The G7 nations are noted for their economic heft and technological advancement. Therefore, it is necessary to understand how the institutional structures of the G7 nations can accelerate energy transitions.

Scholars (Degirmenci et al., 2024; Kousar et al., 2024) noted that environmental policies and decarbonisation efforts are influenced by the quality of institutional frameworks in countries. The quality of the institutions in charge of designing, implementing, and enforcing environmental regulations determines to a large extent the success of energy transitions and reductions in CO_2 emissions (Boateng et al., 2024; Omri & Boubaker, 2024). The G7 economies are noted to have a high degree of complex institutional frameworks, yet they have significant difficulties in the implementation of their commitments to global climate change efforts. As noted by Danish and Ulucak (2020), Maji et al. (2022) and Ameer et al. (2022), large economies, despite their relatively developed institutions, struggle to take decisive actions to achieve climate-related goals due to their already established energy systems and conflicting economic interests.

This study departs from that literature on a point of measurement rather than of theory. Almost all of these studies operationalise decarbonisation as the level of greenhouse gas or carbon dioxide emissions. Energy-related emissions, however, are linked to output and energy use by an accounting identity. For country *i* in year *t*,

$$GHG_{it} \equiv GDP_{it} \times \frac{E_{it}}{GDP_{it}} \times \frac{GHG_{it}}{E_{it}} \quad (1)$$

where *GHG* denotes energy-related greenhouse gas emissions, *GDP* denotes output, and *E* denotes primary energy consumption. The second term is energy intensity, *EI*, and the third is the carbon intensity of energy

supply, *CIE*. Taking natural logarithms gives

$$\ln GHG_{it} = \ln GDP_{it} + \ln EI_{it} + \ln CIE_{it} \quad (2)$$

Equation (2) is an identity rather than an estimated relationship, and it holds exactly in any dataset. The specification used throughout this literature regresses emissions on output, energy use and a governance or policy variable,

$$\ln GHG_{it} = \beta_0 + \beta_1 \ln GDP_{it} + \beta_2 \ln EI_{it} + \beta_3 X_{it} + \beta_4 Z_{it} + \lambda_i + \epsilon_{it} \quad (3)$$

where *X* is the institutional or policy variable of interest and *Z* a vector of controls. Substituting (2) into (3) and rearranging gives

$$\ln CIE_{it} = \beta_0 + (\beta_1 - 1) \ln GDP_{it} + (\beta_2 - 1) \ln EI_{it} + \beta_3 X_{it} + \beta_4 Z_{it} + \lambda_i + \epsilon_{it} \quad (4)$$

Equation (4) shows what Eq. (3) estimates once the identity is removed. The coefficients on output and energy intensity have population values of unity irrespective of any behavioural relationship, and the institutional variable is left to explain movements in carbon intensity alone. The explanatory power reported for such models belongs to the identity rather than to governance.

The implication is that an emissions level is a measure of an environmental outcome, not a measure of decarbonisation. Decarbonisation is properly a change in the carbon content of the energy system: emissions per unit of energy supplied. This study therefore takes the carbon intensity of energy supply as its principal dependent variable, retaining energy-related emissions and the emissions intensity of output as secondary measures so that the results can be compared with the existing literature. This change of measure is the study's central methodological contribution and, as the results show, it substantially alters what can be concluded about the role of institutions.

Given the identified knowledge gaps, this study answers the following research questions.

1. How do institutional quality and environmental policy stringency relate to the carbon intensity of energy supply in G7 nations, and how does this differ from what is found when emissions levels are used instead?
2. Do environmental policy stringency, economic development and energy intensity transmit institutional effects to decarbonisation outcomes, and does institutional quality or economic development moderate the effectiveness of environmental policy more strongly?
3. Is there evidence that the Paris Agreement modified these relationships?

The study makes three contributions. First, it identifies and quantifies an accounting artefact in a widely used empirical design and proposes a measure that is not subject to it. Second, it re-examines the institutional and policy determinants of G7 decarbonisation using that measure, with inference appropriate to a panel of seven countries. Third, it reports, transparently, that most of the relationships claimed in this literature do not survive either change. The study advances SDG 13 (Climate Action), SDG 7 (Affordable and Clean Energy) and SDG 16 (Peace, Justice and Strong Institutions) by clarifying what can and cannot be inferred about institutional influence on energy decarbonisation from national panel data.

The remainder of the paper is structured as follows. Section 2 reviews the relevant literature and develops the hypotheses. Section 3 sets out the data, the models and the econometric strategy. Section 4 presents the

results. Section 5 discusses them, and Section 6 concludes.

2. Literature review

2.1 Theoretical framework

Two theoretical frameworks inform this study. Environmental political economy theory addresses how political institutions shape environmental policy implementation and drive governance mechanisms in a decarbonisation effort (Omri & Boubaker, 2024). The theory highlights the relationship between institutional quality and environmental outcomes, helping scholars to understand the intricacies of policy implementation in advanced economies (Adebayo et al., 2022). Institutional theory complements it by explaining how institutional quality affects policy effectiveness and facilitates environmental governance (Vatalis et al., 2022), and by allowing examination of both formal and informal institutional aspects of decarbonisation (Groves et al., 2024).

The two theories generate testable predictions rather than general expectations. Environmental political economy theory holds that political institutions shape environmental outcomes principally through the design and enforcement of policy. If that is correct, environmental policy stringency should be associated with lower carbon intensity, and the association between institutional quality and carbon intensity should operate through policy stringency rather than around it. Institutional theory holds that institutional capacity determines the effectiveness with which policy is converted into outcomes. If that is correct, the effect of policy stringency should be conditional on institutional quality, which implies a negative interaction between the two. These predictions are stated as hypotheses at the end of Section 2 and are tested directly.

2.2 Related literature

2.2.1 Institutional quality and emissions

Research focusing on G7 economies by Adebayo et al. (2024), Degirmenci et al. (2024) and Kousar et al. (2024) shows how institutional quality reduces carbon emissions through improved regulatory enforcement and policy implementation. Institutions affect environmental outcomes through three main channels: they strengthen policy enforcement, increase accountability, and improve resource allocation. In G7 nations, Ameer et al. (2022), Danish and Ulucak (2020) and Maji et al. (2022) found that high institutional quality enables stricter environmental standards, directly lowering emission levels. Yuan et al. (2023) find that effectively functioning institutions may enhance environmental quality even in low-income countries, and Hunjra et al. (2023) demonstrate that quality institutions are more inclined to advocate for laws and rules that encourage environmentally sustainable development. Bakhsh et al. (2024) emphasise the significance of institutional quality in mitigating long-term environmental degradation.

A feature of this literature that has not been remarked upon is that most of these studies specify emissions levels, or emissions per capita, as the dependent variable while including output and energy use among the regressors. The accounting relationship set out in Section 1 applies to all of them. This does not mean their findings are wrong, but it does mean that the reported magnitudes on output and energy variables are uninformative, and that the institutional estimates are conditional on a specification whose explanatory power is largely mechanical. The present study treats this as an open question to be tested rather than an assumption.

2.2.2 Environmental policy stringency and emission reduction

According to Gil and Bernardo (2020), achieving decarbonisation requires a comprehensive approach that incorporates creative efforts, carbon sequestration, and sustainability-oriented legislation. Salvia et al. (2021), examining 327 European cities, found that metropolitan regions demonstrate stronger commitment to environmental conservation compared to suburban areas, though most European cities may struggle to meet the Paris Agreement's target of net-zero emissions by mid-century. Murray and Rivers (2015) provide empirical evidence from British Columbia indicating that increasing the carbon tax from $10 to $30 led to a 5–15% reduction in regional carbon emissions, while having minimal impact on economic development.

2.2.3 Transmission channels

Empirical evidence suggests various pathways through which environmental policies translate into emission reductions. The effectiveness of environmental policies often operates through institutional channels (Yu et al., 2024), and there is a positive relationship with CO_2 emissions reduction when strong policy implementation mechanisms exist (Maji et al., 2023). Bhattacharya et al. (2017) demonstrate that restrictions on fossil fuel consumption gradually increase efficiency through economic development pathways, and Liu et al. (2023) provide evidence that rising incomes mediate the relationship between institutional quality and environmental improvements through increased R&D investment and technological change. Xu et al. (2024) conclude that institutional quality influences environmental outcomes through both fossil and renewable energy channels, and Karaaslan and Çamkaya (2022) show how energy efficiency mediates the relationship between institutional quality and environmental outcomes.

2.2.4 Interactive effects

Hasan et al. (2024), employing a quantile approach in oil-consuming economies, find significant interactive effects between institutional quality and environmental policies, and Jiao et al. (2023) demonstrate through NARDL analysis that policy effectiveness is contingent upon institutional strength. Gyamfi et al. (2024) conclude that the effectiveness of environmental policies is moderated by economic development levels. Hussain and Dogan (2021) provide contrasting evidence from BRICS economies. Wang et al. (2022) and Karim et al. (2022) demonstrate that institutional quality significantly moderates the relationship between environmental policies and outcomes, while Khan et al. (2023) show that in CEMAC countries government effectiveness and regulatory quality can affect environmental results, and Azam et al. (2021) suggest the moderating effect is more pronounced in lower-middle-income countries.

Comparisons of moderating strength in this literature are frequently drawn between coefficients that are not on comparable scales, and between models estimated separately, so that the difference between them is never tested. Where an income variable enters in logarithms and a governance variable enters as a bounded index, a unit change means something entirely different in each case, and the raw coefficients cannot be ranked. The present study addresses this by standardising all variables, entering both interactions in a single equation, and testing the equality of the two moderating effects with a Wald test.

2.3 Hypotheses

H1. Improvements in institutional quality are associated with reductions in the carbon intensity of energy supply.

H2. Greater environmental policy stringency is associated with reductions in the carbon intensity of energy supply.

H3. The association between institutional quality and carbon intensity is transmitted through environmental policy stringency.

H4. The effect of environmental policy stringency on carbon intensity is conditional on institutional quality (a negative interaction).

H5. The moderating effect of institutional quality differs from that of economic development.

H6. The relationships in H1 to H4 changed after the Paris Agreement.

3. Data, model and econometric strategy

3.1 Variables and data sources

The panel covers the seven G7 countries annually from 2005 to 2023. Variable definitions and sources are given in Table 1.

The principal dependent variable is the carbon intensity of energy supply, defined as energy-related greenhouse gas emissions divided by primary energy consumption. Because energy intensity is measured as primary energy per unit of output, energy consumption is recovered as the product of energy intensity and output, and carbon intensity follows as emissions divided by that product. Two secondary dependent variables are retained for comparability with the literature: energy-related greenhouse gas emissions and the emissions intensity of output. Note that once output is included among the regressors, the emissions-intensity-of-output specification is algebraically equivalent to the emissions specification, differing only by unity in the coefficient on output; it is reported for completeness but adds no independent information.

Institutional quality is measured by the World Bank's six Worldwide Governance Indicators: Voice and Accountability, Political Stability, Government Effectiveness, Regulatory Quality, Rule of Law and Control of Corruption. Because these indicators are highly collinear (Section 4.2), they are entered one at a time, and a composite index is additionally constructed as the first principal component of the six. Environmental policy stringency is the OECD Environmental Policy Stringency Index, ranging from 0 to 6. Controls are output, trade openness, energy intensity and urban population growth.

The panel is unbalanced. The OECD Environmental Policy Stringency archive series ends in 2020, and the governance, trade and energy series end in 2022. Estimation samples are therefore 126 observations for models containing governance variables, 112 for models containing policy stringency, and 119 and 105 respectively for the corresponding first-differenced models. The figure of 133 country-year observations applies only to the descriptive statistics of emissions, output and urbanisation.

Table 1
Variable definitions, units and sources

Variable	Code	Role	Definition and unit	Source
Carbon intensity of energy supply	CIE	Dependent (principal)	Energy-related GHG emissions per unit of primary energy; Mt CO_2e per kWh. Constructed as GHG / (EI × GDP).	Authors' construction from IEA and Our World in Data
Energy-related GHG emissions	GHG	Dependent (secondary)	Total greenhouse gas emissions from energy, national total, Mt CO_2e	IEA Greenhouse Gas Emissions from Energy Data Explorer
Emissions intensity of GDP	CIGDP	Dependent (secondary)	GHG emissions per unit of output, Mt CO_2e per constant 2015 US$	Authors' construction
Worldwide Governance Indicators	VOA, POL, GOV, REG, RUL, COR	Independent	Voice and Accountability, Political Stability, Government Effectiveness, Regulatory Quality, Rule of Law, Control of Corruption; index, approx. −2.5 to 2.5	World Bank Worldwide Governance Indicators
Composite institutional quality	INST	Independent	First principal component of the six WGI dimensions (72.5% of variance)	Authors' construction
Environmental policy stringency	EPS	Independent	OECD composite index of 14 environmental instruments; 0 (least) to 6 (most stringent); available 2005–2020	OECD Environmental Policy Stringency Index
Economic development	GDP	Control	Gross domestic product, national total, constant 2015 US$	World Bank national accounts; OECD National Accounts
Trade openness	TO	Control	Trade as a percentage of GDP	World Bank World Development Indicators
Energy intensity	EI	Control	Primary energy consumption per unit of GDP, kWh per US$	Our World in Data
Urbanisation	URB	Control	Urban population growth, annual %	World Bank (SP.URB.GROW)

Notes: All monetary and physical quantities are national totals. Logarithmic transformations are natural logarithms and are applied to GHG, CIE, CIGDP, GDP, TO and EI only; the governance indicators, EPS and URB enter in levels.

3.2 Empirical models

Three sets of models are estimated. The direct-effect model relates each decarbonisation measure to one institutional or policy variable at a time, with controls:

$$Y_{i,t} = \beta_0 + \beta_1 X_{i,t} + \beta_2 Controls_{i,t} + \lambda_i + \tau_t + \varepsilon_{i,t} \quad (5)$$

where $Y_{i,t}$ is the log of one of the three decarbonisation measures for country i in year t, $X_{i,t}$ is one governance dimension, the composite institutional index, or environmental policy stringency, $Controls_{i,t}$ contains log output, log trade openness and urban population growth, and λ_i and τ_t are country and year fixed effects. The Environmental Kuznets Curve is tested by adding a squared output term to Eq. (5).

The mediation model is specified for each of three candidate channels M; environmental policy stringency, economic development and energy intensity

$$M_{i,t} = \alpha_0 + \alpha_1 X_{i,t} + \alpha_2 Controls_{i,t} + \lambda_i + \tau_t + u_{i,t} \quad (6)$$

$$Y_{i,t} = \gamma_0 + \gamma_1 X_{i,t} + \gamma_2 M_{i,t} + \gamma_3 Controls_{i,t} + \lambda_i + \tau_t + v_{i,t} \quad (7)$$

$$\text{Indirect effect} = \alpha_1 \times \gamma_2 \quad (8)$$

Critically, Controls in equations (5) and (6) contains only trade openness and urbanisation. Output and energy intensity are excluded from the control set whenever they serve as mediators, since conditioning on a mediator blocks the path whose indirect effect is being estimated.

The moderation model enters both interactions in a single equation, with every variable standardised to zero mean and unit variance so that the two interaction coefficients are on a common scale and their difference can be tested:

$$Y_{i,t} = \beta_0 + \beta_1 zX_{i,t} + \beta_2 zEPS_{i,t} + \beta_3 zGDP_{i,t} + \beta_4 (zX \times zEPS)_{i,t} + \beta_5 (zGDP \times zEPS)_{i,t} + \beta_6 Controls_{i,t} + \lambda_i + \tau_t + \varepsilon_{i,t} \quad (9)$$

Hypothesis H5 is tested as the linear restriction $\beta_4 = \beta_5$ by a Wald test. The Paris Agreement model adds an indicator $POST_t$ equal to one from the treatment year onward, together with all two-way and three-way interactions:

$$Y_{i,t} = \beta_0 + \beta_1 zX + \beta_2 zEPS + \beta_3 zGDP + \beta_4 (zX \times zEPS) + \beta_5 (zGDP \times zEPS) + \beta_6 (zX \times POST) + \beta_7 (zEPS \times POST) + \beta_8 (zGDP \times POST) + \beta_9 (zX \times zEPS \times POST) + \beta_{10} (zGDP \times zEPS \times POST) + \beta_{11} Controls + \lambda_i + \tau_t + \varepsilon_{i,t} \quad (10)$$

The hypothesis that the Agreement modified the relationships is the joint restriction $\beta_9 = \beta_{10} = 0$, tested by a Wald test. The treatment year is set at 2016, the year the Agreement entered into force on 4 November 2016, with 2015 (adoption, 12 December 2015) and 2017 reported as sensitivity. Because year fixed effects are retained, the level effect of POST is absorbed along with all other common shocks; only the interactions are identified, which is what the claim that the Agreement modified a relationship actually asserts.

3.3 Econometric strategy

Panel unit root tests are reported for every variable rather than for the dependent variable alone, in levels and in first differences, using the Im–Pesaran–Shin test with moments simulated by Monte Carlo for the exact panel dimension and lag order used here, together with the Maddala–Wu Fisher chi-square and Choi's inverse-normal statistic. Because all series prove to be integrated of order one, residual-based cointegration tests are reported; where no cointegrating relationship is found, first differences are the appropriate specification and nothing of long-run interest is discarded by differencing.

The principal specification is two-way fixed effects with country and year effects. Year effects are essential here: cross-sectional dependence tests reject the null of independence for every variable in the panel (Section 4.2), so common time shocks are present and omitting them would bias the institutional and policy estimates. With seven cross-sectional units, dynamic panel GMM requires more instruments than there are countries and has no credible asymptotic justification; two-way fixed effects is used instead.

With only seven clusters, conventional cluster-robust standard errors are severely downward-biased and reject the null far too often. All inference reported here therefore uses a restricted wild cluster bootstrap with Rademacher weights, enumerating all $2^7 = 128$ possible sign vectors exactly rather than sampling them, which gives an exact bootstrap distribution with a minimum attainable p-value of $1/128 = 0.008$. Heteroskedasticity-robust, cluster-robust and Driscoll–Kraay p-values are reported alongside for comparison in Table 10. Mediation indirect effects are tested by bias-corrected 95 per cent confidence intervals from 2,000 country-block bootstrap replications rather than by the sequential-significance logic of Baron and Kenny (1986). Estimation was carried out in Python 3.11 using pandas, NumPy, SciPy, statsmodels and linearmodels. All headline estimates were reproduced independently in a second estimation library, agreeing to within 10^{-15}.

4. Results

4.1 Descriptive statistics

Table 2 reports descriptive statistics. All logarithmic variables are in natural logarithms, and the number of observations is stated for each variable because the panel is unbalanced. Environmental policy stringency ranges from 1.222 to 4.889, indicating considerable diversity of regulatory approach within the G7, and the governance indicators show the high values expected of advanced economies, with Political Stability the weakest dimension.

Table 2
Descriptive statistics

Variable	N	Mean	Median	SD	Min	Max
lnGHG	133	6.612	6.387	0.914	5.590	8.703
lnCIE	126	-22.471	-22.379	0.221	-22.926	-22.160
lnCIGDP	133	-22.243	-22.210	0.393	-23.090	-21.536
VOA	126	1.203	1.212	0.182	0.845	1.507
POL	126	0.629	0.575	0.326	-0.106	1.262
GOV	126	1.379	1.513	0.420	0.192	1.892
REG	126	1.376	1.442	0.334	0.488	1.879
RUL	126	1.394	1.545	0.447	0.208	1.886
COR	126	1.393	1.533	0.521	0.006	2.062
INST	126	0.000	0.291	2.086	-4.822	2.802
EPS	112	3.200	3.194	0.656	1.222	4.889
lnGDP	133	28.855	28.671	0.759	27.910	30.712
lnTO	126	3.942	4.070	0.378	3.140	4.604
lnEI	126	0.244	0.146	0.346	-0.259	1.063
URB	133	0.823	0.950	0.512	-0.731	3.067

Notes: All logs are natural logarithms. N differs across variables because the panel is unbalanced: EPS is available only to 2020 and the governance, trade and energy series only to 2022.

4.2 Diagnostics

Table 3 reports panel unit root tests for all fifteen variables. Every variable fails to reject the unit root null in levels and rejects it decisively in first differences on all three tests. The evidence is uniform, and first differencing is therefore justified.

Table 3
Panel unit root tests, all variables

Variable	Form	t-bar	IPS W	p	Fisher χ^2	p	Verdict
lnGHG	level	-0.749	2.047	0.980	8.487	0.862	unit root
Δ lnGHG	Δ	-4.410	-7.750	0.000	120.973	0.000	I(0)
lnCIE	level	-1.369	0.359	0.640	13.049	0.523	unit root
Δ lnCIE	Δ	-2.783	-3.363	0.000	53.431	0.000	I(0)
lnCIGDP	level	0.496	5.394	1.000	0.400	1.000	unit root
Δ lnCIGDP	Δ	-4.076	-6.859	0.000	107.491	0.000	I(0)
VOA	level	-1.736	-0.619	0.268	17.709	0.220	unit root
Δ VOA	Δ	-4.031	-6.623	0.000	98.706	0.000	I(0)
POL	level	-1.518	-0.039	0.484	13.917	0.456	unit root
Δ POL	Δ	-2.908	-3.691	0.000	44.924	0.000	I(0)
GOV	level	-1.173	0.882	0.811	10.385	0.734	unit root
Δ GOV	Δ	-3.809	-6.044	0.000	89.761	0.000	I(0)
REG	level	-1.749	-0.653	0.257	17.251	0.243	unit root
Δ REG	Δ	-3.303	-4.721	0.000	61.277	0.000	I(0)
RUL	level	-0.308	3.189	0.999	3.103	0.999	unit root
Δ RUL	Δ	-3.287	-4.680	0.000	64.679	0.000	I(0)
COR	level	-1.489	0.039	0.515	10.571	0.719	unit root
Δ COR	Δ	-2.651	-3.018	0.001	39.001	0.000	I(0)
INST	level	-1.044	1.227	0.890	8.638	0.854	unit root
Δ INST	Δ	-2.758	-3.299	0.000	40.113	0.000	I(0)
EPS	level	-1.256	0.600	0.726	8.706	0.849	unit root
Δ EPS	Δ	-3.343	-4.694	0.000	71.874	0.000	I(0)
lnGDP	level	-0.612	2.415	0.992	5.259	0.982	unit root
Δ lnGDP	Δ	-3.902	-6.396	0.000	89.370	0.000	I(0)
lnTO	level	-1.527	-0.062	0.475	12.437	0.571	unit root
Δ lnTO	Δ	-3.626	-5.566	0.000	74.460	0.000	I(0)
lnEI	level	0.053	4.151	1.000	2.801	0.999	unit root

Notes: H0: all panels contain a unit root. IPS W standardised using Monte Carlo moments simulated for T = 19, one lag, intercept, 20,000 replications. Fisher is the Maddala–Wu chi-square. The Levin–Lin–Chu test is not reported because it imposes a common autoregressive root across countries.

Variable	Form	t-bar	IPS W	p	Fisher χ^2	p	Verdict
Δ lnEI	Δ	-3.899	-6.280	0.000	98.378	0.000	I(0)
URB	level	-2.091	-1.562	0.059	20.721	0.109	unit root
Δ URB	Δ	-4.245	-7.311	0.000	112.300	0.000	I(0)

Notes: H0: all panels contain a unit root. IPS W standardised using Monte Carlo moments simulated for T = 19, one lag, intercept, 20,000 replications. Fisher is the Maddala–Wu chi-square. The Levin–Lin–Chu test is not reported because it imposes a common autoregressive root across countries.

Residual-based cointegration tests for the principal dependent variable do not reject the null of no cointegration (IPS W = − 0.226, p = 0.411; Fisher χ^2 = 13.16, p = 0.514), nor do they for energy-related emissions (W = 1.235, p = 0.892). A cointegrating relationship is found only for the emissions intensity of output (W = − 1.565, p = 0.059; Fisher χ^2 = 29.50, p = 0.009).

Table 4 reports cross-sectional dependence tests. The null of cross-sectional independence is rejected for every variable, with Pesaran CD statistics between 4.16 and 18.81 and average absolute pairwise correlations between 0.41 and 0.97.

Table 4
Cross-sectional dependence (Pesaran CD test)

Variable	CD statistic	p-value	Average |r|	Maximum |r|	Pairs
ln GHG	11.562	< 0.001	0.655	0.966	21
ln CIE	4.158	< 0.001	0.414	0.968	21
GOV	7.539	< 0.001	0.454	0.853	21
INST	5.083	< 0.001	0.506	0.854	21
EPS	7.264	< 0.001	0.447	0.829	21
ln GDP	13.590	< 0.001	0.702	0.989	21
ln EI	18.812	< 0.001	0.968	0.984	21

Notes: H0: no cross-sectional dependence. The null is rejected for every variable, contrary to the result reported in the submitted version.

The six governance indicators are severely collinear. Government Effectiveness and Rule of Law correlate at 0.957, Rule of Law and Control of Corruption at 0.941, and Government Effectiveness and Control of Corruption at 0.913. When all six enter a single equation with the controls, the variance inflation factors are 32.71 for Rule of Law, 19.23 for Government Effectiveness, 18.06 for Control of Corruption, 8.02 for Voice and Accountability, 5.81 for Regulatory Quality and 1.88 for Political Stability. Two remedies are adopted: each governance dimension is entered separately, and a principal-components index is used as a composite measure.

4.3 The accounting identity

Energy-related emissions satisfy the identity GHG ≡ GDP × (Energy/GDP) × (GHG/Energy), which in natural logarithms is ln GHG = ln GDP + ln EI + ln CIE and holds in these data to within 4×10^{-15}. A pooled regression of

log emissions on log output and log energy intensity returns coefficients of 1.094 and 0.919 against identity values of unity, with an R-squared of 0.948. The share of the variance of log emissions attributable to the output and energy-intensity terms alone is 94.1 per cent.

The coefficients on output of 1.118 to 1.136 and on energy intensity of 0.954 to 0.965, reported as stable across four models with R-squared between 0.831 and 0.849, are the identity recovered numerically rather than an estimated behavioural relationship. The same applies to the output and energy-intensity mediation channels, whose mediator-to-outcome paths of 1.136 and 0.954 are the identity coefficients. The carbon intensity of energy supply removes both terms by construction and is used as the principal dependent variable for that reason.

4.4 Direct effects

Table 5 reports the direct-effect estimates. A negative coefficient denotes lower carbon intensity or lower emissions, and therefore greater decarbonization.

Table 5
Direct effects on the carbon intensity of energy supply, two-way fixed effects

Specification	Variable	Coefficient	Cluster SE	Cluster p	Wild cluster p	N	Within R^2
Levels	INST	0.0401	0.0225	0.0744	0.0781	126	0.9679
Levels	GOV	0.2069	0.0995	0.0376	0.1250	126	0.9699
Levels	REG	0.1361	0.0599	0.0231	0.1094	126	0.9685
Levels	RUL	0.2361	0.0929	0.0111	0.1875	126	0.9694
Levels	COR	-0.0259	0.0617	0.6744	0.7734	126	0.9649
Levels	VOA	0.0755	0.1287	0.5573	0.5156	126	0.9650
Levels	POL	-0.0228	0.0432	0.5971	0.6797	126	0.9649
Levels	EPS	-0.0301	0.0232	0.1946	0.1406	112	0.9716
First differences	INST	0.0053	0.0043	0.2222	0.2031	119	0.3622
First differences	GOV	-0.0240	0.0079	0.0023	0.0156	119	0.3656
First differences	REG	-0.0060	0.0118	0.6108	0.8125	119	0.3605
First differences	RUL	-0.0008	0.0377	0.9840	0.9688	119	0.3600
First differences	COR	-0.0039	0.0202	0.8451	0.8281	119	0.3602
First differences	VOA	0.0666	0.0323	0.0390	0.1562	119	0.3696
First differences	POL	0.0309	0.0117	0.0086	0.1094	119	0.3825
First differences	EPS	-0.0028	0.0037	0.4601	0.3750	105	0.3672

Notes: Country and year fixed effects throughout. One institutional or policy variable per model; controls are log GDP, log GDP squared, log trade openness and urban population growth. The wild cluster bootstrap p-value is the basis for inference; the cluster p-value is shown only for comparison. A negative coefficient indicates greater decarbonisation.

In levels, none of the governance dimensions is significant under valid inference, though several would appear significant if cluster-robust standard errors were used. Where the point estimates are positive, they indicate that country-years with higher governance scores exhibit higher carbon intensity; a cross-sectional pattern rather than evidence about the effect of institutional change. In first differences, which is the specification supported by the unit root and cointegration evidence, improvements in government effectiveness are associated with a reduction in the carbon intensity of energy supply of 0.025 log points (wild cluster p = 0.016). This is the one institutional result that survives, and it supports H1 for a single governance dimension only. The composite institutional index is not significant, nor is any other individual dimension.

Environmental policy stringency is not significantly associated with any decarbonisation measure in either specification. In levels its coefficient on carbon intensity is − 0.030 with a wild cluster bootstrap p-value of 0.141, and in first differences − 0.003 with p = 0.375. H2 is not supported. The divergence between levels and differences for government effectiveness is reported rather than resolved: in the cross-section higher

governance is associated with higher carbon intensity, while changes in governance are associated with reductions.

4.5 Mediation

Table 6 reports the indirect effects of institutional quality through the three candidate channels, with bias-corrected bootstrap confidence intervals. Across forty-two combinations of institution, channel and dependent variable, one interval excludes zero, which is approximately what would be expected by chance at the 5 per cent level. H3 is not supported: environmental policy stringency does not transmit institutional effects to decarbonisation outcomes in these data.

Table 6
Mediation: indirect effects with bias-corrected bootstrap confidence intervals

Channel	Institution	a path	b path	Indirect	CI lower	CI upper	Sig.
Policy stringency	INST	0.1709	-0.0039	-0.0007	-0.0061	0.0001	No
Policy stringency	GOV	0.3744	-0.0019	-0.0007	-0.0064	0.0011	No
Policy stringency	REG	0.6396	-0.0039	-0.0025	-0.0149	0.0013	No
Policy stringency	RUL	-0.1663	-0.0026	0.0004	-0.0008	0.0125	No
Policy stringency	COR	0.3703	-0.0025	-0.0009	-0.0066	0.0006	No
Policy stringency	VOA	-1.6008	-0.0018	0.0030	-0.0091	0.0214	No
Policy stringency	POL	0.3013	-0.0050	-0.0015	-0.0088	0.0005	No
Econ. development	INST	0.0019	0.2680	0.0005	-0.0016	0.0047	No
Econ. development	GOV	-0.0032	0.2676	-0.0009	-0.0057	0.0056	No
Econ. development	REG	0.0129	0.2759	0.0036	-0.0006	0.0169	No
Econ. development	RUL	0.0109	0.2716	0.0030	-0.0067	0.0152	No
Econ. development	COR	-0.0029	0.2708	-0.0008	-0.0081	0.0087	No
Econ. development	VOA	-0.0135	0.2803	-0.0038	-0.0187	0.0086	No
Econ. development	POL	0.0006	0.2694	0.0002	-0.0023	0.0035	No
Energy intensity	INST	0.0068	-0.3040	-0.0021	-0.0083	0.0017	No
Energy intensity	GOV	0.0115	-0.2933	-0.0034	-0.0302	0.0062	No
Energy intensity	REG	0.0187	-0.2987	-0.0056	-0.0224	0.0045	No
Energy intensity	RUL	-0.0014	-0.2974	0.0004	-0.0340	0.0201	No
Energy intensity	COR	0.0272	-0.2984	-0.0081	-0.0238	-0.0013	Yes
Energy intensity	VOA	-0.0136	-0.2947	0.0040	-0.0079	0.0275	No
Energy intensity	POL	-0.0006	-0.2967	0.0002	-0.0038	0.0114	No

Notes: Dependent variable: change in log carbon intensity of energy supply. Country and year fixed effects. Controls are trade openness and urbanisation only; output and energy intensity are excluded from the control set when they serve as mediators. Intervals are bias-corrected 95% intervals from 2,000 country-block bootstrap replications. With seven clusters these intervals are weak and should be read as indicative.

4.6 Moderation

Table 7 tests H4 and H5. Both interactions are entered in a single equation with all variables standardised, so that the two moderating effects are on a common scale and their difference is testable.

Table 7
Moderation on standardised scales, with a test of equality of moderating effects

Form	Dependent variable	Institution	N	Inst × EPS	p (WCB)	GDP × EPS	p (WCB)	Wald p
Levels	Carbon intensity	INST	112	-0.0159	0.4219	-0.0039	0.6719	0.4613
Levels	Carbon intensity	GOV	112	0.0016	0.8750	-0.0025	0.7188	0.7567
Levels	GHG emissions	INST	112	-0.0298	0.2656	0.0013	0.8438	0.1297
Levels	GHG emissions	GOV	112	-0.0084	0.4844	0.0042	0.6719	0.4447
First diff.	Carbon intensity	INST	105	0.0016	0.4531	-0.0012	0.8906	0.6145
First diff.	Carbon intensity	GOV	105	0.0009	0.7656	0.0005	0.9375	0.9264
First diff.	GHG emissions	INST	105	0.0012	0.6250	-0.0012	0.8594	0.5564
First diff.	GHG emissions	GOV	105	-0.0000	0.9844	0.0001	1.0000	0.9743

Notes: All variables standardised to zero mean and unit variance. Country and year fixed effects. The Wald p-value tests H0: the two moderating effects are equal. p (WCB) is the wild cluster bootstrap p-value.

Not one interaction term is significant in any of the eight specifications, so H4 is not supported. The test of equality between the two moderating effects is not rejected, with Wald p-values ranging from 0.130 to 0.974, so H5 is not supported either.

4.7 The Paris Agreement

Table 8 reports the structural-break specification.

Table 8
Paris Agreement: triple interactions and treatment-year sensitivity

Cut year	Form	Dependent variable	N	INST×EPS×POST	p (WCB)	GDP×EPS×POST	p (WCB)	Joint Wald p
2015	Levels	Carbon intensity	112	-0.0135	0.7656	-0.0166	0.1094	0.2295
2015	Levels	GHG emissions	112	-0.0287	0.5469	-0.0128	0.5156	0.6840
2015	First diff.	Carbon intensity	105	-0.0017	0.6250	0.0286	0.2031	0.0245
2015	First diff.	GHG emissions	105	0.0025	0.6562	0.0357	0.0156	0.0000
2016	Levels	Carbon intensity	112	-0.0199	0.6875	-0.0261	0.0469	0.0117
2016	Levels	GHG emissions	112	-0.0376	0.5312	-0.0187	0.3594	0.4992
2016	First diff.	Carbon intensity	105	-0.0015	0.6875	0.0285	0.1250	0.0108
2016	First diff.	GHG emissions	105	0.0018	0.7656	0.0359	0.0156	0.0000
2017	Levels	Carbon intensity	112	-0.0443	0.2188	-0.0319	0.0781	0.0230
2017	Levels	GHG emissions	112	-0.0620	0.2031	-0.0250	0.3906	0.3402
2017	First diff.	Carbon intensity	105	0.0012	0.7969	0.0253	0.2344	0.0865
2017	First diff.	GHG emissions	105	0.0042	0.6250	0.0322	0.1562	0.0012

Notes: Country and year fixed effects throughout, so the level effect of POST is absorbed with all other common time shocks and only the interactions are identified. The joint Wald test is of H0: both triple interactions equal zero.

The joint Wald test rejects in first differences but not in levels; the two triple-interaction terms carry opposite signs across dependent variables; and the interaction of output with policy stringency and the post-Paris indicator is significant at cut-years 2016 and 2017 but not at 2015, so the conclusion turns on a treatment date. H6 is therefore not supported robustly. Research question 3 is accordingly answered descriptively rather than causally.

4.8 Robustness and the sensitivity of inference

Table 9 subjects the one surviving institutional result to eighteen alternative specifications. The point estimate is notably stable, lying between − 0.019 and − 0.044 and never changing sign, across changes to the fixed-effects structure, the control set, the sample of countries and the sample of years. Statistical significance is not stable, with wild cluster bootstrap p-values ranging from 0.016 to 0.219.

Table 9
Robustness of the government effectiveness estimate

Specification	Coefficient	Cluster SE	Cluster p	Wild cluster p	N
Baseline (two-way FE, FD)	-0.0246	0.0087	0.0046	0.0156	119
Country FE only (no year FE)	-0.0249	0.0126	0.0476	0.1094	119
Add ΔEPS as control	-0.0256	0.0137	0.0620	0.1250	105
Add ΔlnEI as control	-0.0220	0.0100	0.0274	0.0781	119
Add EKC quadratic	-0.0240	0.0079	0.0023	0.0156	119
Drop all controls	-0.0235	0.0111	0.0337	0.0312	119
Exclude Canada	-0.0227	0.0101	0.0241	0.0312	102
Exclude France	-0.0215	0.0096	0.0257	0.1250	102
Exclude Germany	-0.0249	0.0112	0.0267	0.0312	102
Exclude Italy	-0.0188	0.0101	0.0614	0.2188	102
Exclude Japan	-0.0209	0.0152	0.1693	0.0938	102
Exclude United Kingdom	-0.0300	0.0158	0.0575	0.0625	102
Exclude United States	-0.0306	0.0121	0.0112	0.0312	102
2005–2020 only (EPS window)	-0.0264	0.0133	0.0469	0.1406	105
Exclude 2020–2021 (COVID)	-0.0437	0.0172	0.0112	0.0625	105
DV = Δ emissions intensity of GDP	-0.0148	0.0271	0.5847	0.6094	119
DV = Δ energy-related GHG	-0.0148	0.0271	0.5847	0.6094	119
Focus = Δ governance index (PCA)	0.0047	0.0041	0.2498	0.2031	119

Notes: Dependent variable: change in log carbon intensity of energy supply. Focal regressor: change in government effectiveness. Baseline is two-way fixed effects in first differences. GMM is not reported; with seven cross-sectional units it has no credible asymptotic justification.

Table 10 makes explicit why the choice of standard error matters here. For the same coefficient, heteroskedasticity-robust, cluster-robust and Driscoll–Kraay p-values are systematically smaller than the wild cluster bootstrap p-value, in several cases by an order of magnitude. Government effectiveness in levels, for instance, has a cluster-robust p-value of 0.095 and a Driscoll–Kraay p-value below 0.001, but a wild cluster bootstrap p-value of 0.266.

Table 10
Comparison of inference methods

Dependent variable	Variable	Form	Coefficient	p (HC1)	p (cluster)	p (D-K)	p (wild cluster)	N
Carbon intensity	GOV	Levels	0.1863	0.0012	0.0952	0.0001	0.2656	126
Carbon intensity	GOV	First diff.	-0.0246	0.3910	0.0046	0.2569	0.0156	119
Carbon intensity	INST	Levels	0.0513	0.0000	0.0206	0.0012	0.0781	126
Carbon intensity	INST	First diff.	0.0047	0.5848	0.2498	0.4119	0.2031	119
Carbon intensity	EPS	Levels	-0.0352	0.0060	0.1235	0.0026	0.1562	112
Carbon intensity	EPS	First diff.	-0.0027	0.6618	0.4583	0.5422	0.3750	105
GHG emissions	GOV	Levels	0.2786	0.0000	0.0632	0.0005	0.0312	126
GHG emissions	GOV	First diff.	-0.0148	0.5992	0.5847	0.5354	0.6094	119
GHG emissions	INST	Levels	0.0646	0.0000	0.0190	0.0056	0.0156	126
GHG emissions	INST	First diff.	0.0125	0.1766	0.2853	0.1484	0.3281	119
GHG emissions	EPS	Levels	-0.0546	0.0007	0.1186	0.0002	0.1406	112
GHG emissions	EPS	First diff.	0.0002	0.9792	0.9780	0.9725	0.9688	105

Notes: Identical coefficient estimates under four inference procedures. With G = 7 clusters, cluster-robust and Driscoll–Kraay standard errors over-reject substantially; the wild cluster bootstrap, enumerating all 128 Rademacher sign vectors, is the appropriate procedure.

5. Discussion

The first research question asked how institutional quality and environmental policy stringency relate to the carbon intensity of energy supply, and how this differs from what is found using emissions levels. The difference is substantial. Using emissions levels, output and energy intensity appear as a strong determinants, with elasticities near unity and models explaining more than eighty per cent of variation. Those magnitudes are an accounting identity, and the apparent explanatory power of the model belongs to it. Using the carbon intensity of energy supply, which removes both terms by construction, the result is far more modest: one governance dimension, government effectiveness, is associated with reductions in carbon intensity when measured in changes, and nothing else is robust.

This finding is consistent with the broad direction of Adebayo et al. (2024), Degirmenci et al. (2024) and Kousar et al. (2024), who report emission-reducing effects of institutional quality in the G7, but it is much weaker than those studies suggest, and it is confined to a single dimension of governance. It is worth noting that government effectiveness; the capacity of the public administration to formulate and implement policy is precisely the dimension one would expect to matter for the technical work of changing an energy system, as distinct from dimensions capturing political voice or corruption control. The result is suggestive rather than decisive, and it should be treated as such until it can be tested on a larger panel.

The absence of any robust association between environmental policy stringency and decarbonisation deserves comment, because it holds even under the corrected measure. G7 energy systems carry large sunk costs and long asset lifetimes that resist policy pressure, so that stringency on paper does not translate into carbon intensity in practice. Also, the OECD index, which aggregates fourteen instruments into a single score, is too coarse to capture the specific instruments that bear on the carbon content of energy supply. The present analysis cannot distinguish between these, and the paper does not claim to.

The second research question concerned transmission and moderation, and the results locate a precise break in the mechanism that environmental political economy theory proposes. Countries with more capable public administrations and stronger regulatory quality maintain more stringent environmental policy, which is consistent with Hunjra et al. (2023) and Bakhsh et al. (2024), who argue that quality institutions produce rules favouring sustainable outcomes. The second link does not hold. Policy stringency shows no association with the carbon content of energy supply, and because an indirect effect is the product of two paths, the transmission collapses however strong the first link may be.

Murray and Rivers (2015) show that a specific instrument at a specific price changed emissions in British Columbia, and Gil and Bernardo (2020) argue that decarbonisation requires legislation working alongside innovation and sequestration. Neither claim concerns regulatory ambition in the aggregate. The OECD index averages fourteen instruments of unequal potency, so a country that tightens vehicle standards and one that prices carbon across the economy can register comparable scores while their energy systems diverge. What the index captures well is the stance of the regulatory state; what it cannot capture is whether the instruments that govern fuel choice have been deployed at levels that alter investment. Salvia et al. (2021) report the same disjunction among European cities, where commitment to climate targets outruns the trajectory needed to meet them. The present results extend that observation from municipal pledges to national regulatory ambition, and suggest the gap is structural rather than a failure of intent.

There is a further reason to expect the second link to be weak in these economies specifically. The carbon content of energy supply is determined by generation fleets, grids, and fuel supply arrangements whose lifetimes run to decades. Regulatory stringency alters prices and permissions at the margin, but the composition of the capital stock responds through investment decisions that are slow, lumpy and shaped by considerations beyond environmental rules. Danish and Ulucak (2020) and Ameer et al. (2022) make this argument about advanced economies with established energy systems, and Bhattacharya et al. (2017) find that restrictions on fossil consumption raise efficiency gradually rather than abruptly. Where a literature has treated stringency as acting on emissions within the period observed, the mechanism it describes more plausibly operates over horizons longer than any panel of this length can trace.

The moderation results require a similar reading. Hasan et al. (2024), Jiao et al. (2023), Wang et al. (2022) and Karim et al. (2022) report that institutional quality conditions the effectiveness of environmental policy, and the present study finds no such conditioning, whether by institutional quality or by income. Rather than a contradiction, this is what the sharper contributions to that literature predict. Azam et al. (2021) find the moderating role of institutions most pronounced in lower-middle-income countries, and Gyamfi et al. (2024) argue that institutions and policies must pass a threshold before they improve environmental quality. Both imply that conditioning operates where institutional quality and income vary widely and where some countries fall below the threshold. The G7 occupies the upper tail of both distributions. If the moderating relationship is one of diminishing returns above a threshold that all seven countries have long since crossed, then its absence here confirms the shape of the relationship rather than denying it. The claim that economic capacity enhances policy effectiveness may therefore be a claim about development, not about advanced economies, and it should be tested where the relevant variation exists.

What survives is a direct association between government effectiveness and falling carbon intensity that does not travel through measured policy. This points towards an explanation of institutional influence that the regulatory framing has obscured. Government effectiveness measures the capacity of the state to formulate and implement, which in the energy domain means building transmission, procuring generation, permitting projects and funding research. Xu et al. (2024) find institutional quality operating through fossil and renewable energy channels rather than through rules, Liu et al. (2023) find income working through research and technological change, and Karaaslan and Çamkaya (2022) find efficiency carrying the effect. These authors describe a state that acts within the energy system rather than one that regulates it from outside. The evidence here is consistent with that view, and it suggests environmental political economy theory is half-supported: its account of how institutions shape policy holds, while its account of policy as the route from institutions to environmental outcomes does not survive contact with a mature energy system.

The third research question concerned the Paris Agreement, and the evidence does not support the claim that it modified the relationships examined here. The result speaks to research design as much as to the Agreement itself. A universal treaty ratified by almost every party leaves no untreated comparison group, so every country in the panel is exposed at the same moment. The year effects that absorb common shocks necessarily absorb the treaty alongside them, and what remains identifies only differences in how countries responded rather than whether the Agreement mattered. This limitation is structural rather than a shortage of observations, and it applies to any study inferring treaty effects from a pre-and-post comparison of national panels.

There are also substantive reasons to expect little. The Agreement rests on pledge and review rather than binding national targets, so it alters the reporting and commitment environment rather than the instruments that determine fuel choice. Salvia et al. (2021) document this gap at municipal level, where declared ambition outruns the trajectory required to deliver it. Omri and Boubaker (2024) argue that climate legislation and clean energy policies matter under particular conditions rather than universally, which is not a prediction that a framework treaty will shift outcomes across the board. Kartal et al. (2025) show that stringency in the G7 evolved over a long period, and much of the architecture governing energy in these countries, from emissions trading to statutory carbon budgets, predates 2015. On that argument, the Agreement codified a direction of travel rather than initiating one, and a sharp break at ratification would have been surprising. The implication is not that the Agreement is inconsequential, but that its influence cannot be recovered from aggregate national

panels. Establishing it requires designs built on variation the treaty itself creates, such as differences in the ambition of nationally determined contributions or in the timing of instruments adopted to meet them

6. Conclusions

This study asked whether institutional quality and environmental policy stringency drive the decarbonisation of energy supply in the G7. Its principal finding is methodological. Energy-related emissions, the outcome variable used throughout this literature, are bound to output and energy intensity by an accounting identity that accounts for 94.1 per cent of their variation in this panel. Regressions of emissions on institutional and policy variables therefore recover an accounting relationship, and the near-unit elasticities on output and energy use that such studies report as substantive findings are that identity. Substituting the carbon intensity of energy supply removes the problem and materially changes the conclusions.

With two-way fixed effects and inference valid for seven clusters, improvements in government effectiveness are associated with reductions in the carbon intensity of energy supply. The estimate is stable in sign and magnitude across eighteen specifications but not uniformly significant, and it is reported as suggestive. Environmental policy stringency shows no robust association with any decarbonisation measure. Institutional quality does not transmit its effect through policy stringency, neither institutional quality nor economic development moderates policy effectiveness, and the two moderating effects are never distinguishable from one another. There is no robust evidence that the Paris Agreement modified these relationships.

6.1 Theoretical implications

The results set a limit on environmental political economy theory. The theory holds that political institutions shape environmental outcomes through the design and enforcement of policy. That route is not visible in this panel. Policy stringency neither moves carbon intensity nor carries the institutional effect, and the association that survives runs outside the policy channel. This does not refute the theory. It indicates that in advanced economies with mature energy systems, institutional influence sits somewhere other than regulation, and that theory should look to technological capacity and market structure.

For institutional theory, the results argue for treating governance dimensions separately. Of the six examined, only government effectiveness shows an association with decarbonisation. Composite measures, including the index built here, hide that result rather than reveal it. Treating institutional quality as a single construct may explain why estimated effects vary across studies.

The study also offers a methodological caution with theoretical consequences. A design that measures decarbonisation by emissions levels while controlling for output and energy use will produce large and stable coefficients on those controls, and will assign what remains to institutions. Theoretical claims built on that pattern describe an accounting relationship rather than the behaviour of institutions.

6.2 Practical implications

For policymakers, the defensible implication is narrow. Government effectiveness, rather than institutional quality in general, is the dimension linked to a falling carbon content of energy supply. The evidence supports

investment in the capacity to formulate and deliver policy more than it supports reform of governance as a whole.

Monitoring frameworks that express targets as emissions levels will record improvement that comes from slower output growth or from efficiency gains, neither of which changes the carbon content of energy. Carbon intensity of energy supply, fossil and renewable shares, and clean electricity shares track decarbonisation itself, and should be reported alongside emissions.

The weakness of the policy channel carries a further lesson. A composite stringency score can rise through instruments that leave fuel choice untouched, so regulatory ambition in the aggregate is a poor guide to delivery. Governments and those assessing them should track the instruments that govern generation and fuel supply rather than a single index.

6.3 Limitations and directions for future research

The panel covers seven countries over nineteen years, which limits statistical power. This is why several estimates that hold steady in size across specifications cannot be shown to differ from zero, and readers should weigh the stability of those estimates alongside the p-values. Extending the analysis to the OECD countries, or to a wider set of high-income economies, would establish whether the government effectiveness result holds or reflects this small sample.

Three features of the data constrain what can be asked. Emissions and output enter as national totals because the dataset holds no population series, so a per capita specification and a properly specified test of the Environmental Kuznets Curve remain out of reach. The OECD stringency series ends in 2020, which shortens the post-Paris window. Carbon intensity is constructed from energy intensity and output rather than observed, so error in either component carries into it. Adding population and a current stringency series is the most immediate extension.

National aggregates also hide differences between sectors, and institutions may act on electricity in ways they do not act on industry or transport. Sectoral and sub-national work would identify the mechanism that the design used here can only point towards. A wider task remains. Re-estimating published studies of institutions and emissions with carbon intensity measures would show how much of that literature survives the measurement problem set out in this paper.

Declarations

Competing interests.

The author has no competing interests to declare that are relevant to the content of this article.

Funding.

The author received no financial support for the research, authorship or publication of this article.

Data availability. All data analysed in this study are derived from publicly available sources listed in Table 1: the IEA Greenhouse Gas Emissions from Energy Data Explorer, the World Bank Worldwide Governance Indicators

and World Development Indicators, the OECD Environmental Policy Stringency Index, and Our World in Data.

Ethical approval. This study did not involve human participants or animals, and ethical approval was therefore not required.

Consent to participate and consent for publication. Not applicable.

Author contributions.

The sole author was responsible for conceptualisation, methodology, data curation, formal analysis, writing of the original draft, and review and editing.

References

References shown in blue are newly added, principally the methodological sources cited but not previously listed. All entries now carry complete bibliographic details and a DOI or stable URL.

1. Adebayo, T. S., Ağa, M., Agyekum, E. B., Kamel, S., & El-Naggar, M. F. (2022). Do renewable energy consumption and financial development contribute to environmental quality in MINT nations? Implications for sustainable development. Frontiers in Environmental Science, 10, 1068379. https://doi.org/10.3389/fenvs.2022.1068379
2. Adebayo, T. S., Alola, A. A., & Ullah, S. (2024). Probing the carbon neutrality drive of environmental-related technologies and energy transition in France and Germany: a novel time–frequency technique. Clean Technologies and Environmental Policy. https://doi.org/10.1007/s10098-024-02816-z
3. Ameer, W., Amin, A., & Xu, H. (2022). Does institutional quality, natural resources, globalization, and renewable energy contribute to environmental pollution in China? Role of financialization. Frontiers in Public Health, 10, 849946. https://doi.org/10.3389/fpubh.2022.849946
4. Azam, M., Liu, L., & Ahmad, N. (2021). Impact of institutional quality on environment and energy consumption: evidence from developing world. Environment, Development and Sustainability, 23(2), 1646–1667. https://doi.org/10.1007/s10668-020-00644-x
5. Bakhsh, S., Zhang, W., Ali, K., & Anas, M. (2024). Energy transition and environmental stability prospects for OECD economies. Journal of Environmental Management, 354, 120358. https://doi.org/10.1016/j.jenvman.2024.120358
6. Baltagi, B. H., Feng, Q., & Kao, C. (2012). A Lagrange multiplier test for cross-sectional dependence in a fixed effects panel data model. Journal of Econometrics, 170(1), 164–177. https://doi.org/10.1016/j.jeconom.2012.04.004
7. Baron, R. M., & Kenny, D. A. (1986). The moderator–mediator variable distinction in social psychological research: Conceptual, strategic, and statistical considerations. Journal of Personality and Social Psychology, 51(6), 1173–1182. https://doi.org/10.1037/0022-3514.51.6.1173
8. Bhattacharya, M., Churchill, S. A., & Paramati, S. R. (2017). The dynamic impact of renewable energy and institutions on economic output and CO2 emissions across regions. Renewable Energy, 111, 157–167. https://doi.org/10.1016/j.renene.2017.03.102

9. Boateng, E., Annor, C. B., Amponsah, M., & Ayibor, R. E. (2024). Does FDI mitigate CO2 emissions intensity? Not when institutional quality is weak. Journal of Environmental Management, 354, 120386. https://doi.org/10.1016/j.jenvman.2024.120386
10. Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2008). Bootstrap-based improvements for inference with clustered errors. Review of Economics and Statistics, 90(3), 414–427. https://doi.org/10.1162/rest.90.3.414
11. Choi, I. (2001). Unit root tests for panel data. Journal of International Money and Finance, 20(2), 249–272. https://doi.org/10.1016/S0261-5606(00)00048-6
12. Danish, & Ulucak, R. (2020). The pathway toward pollution mitigation: Does institutional quality make a difference? Business Strategy and the Environment, 29(8), 3571–3583. https://doi.org/10.1002/bse.2597
13. Degirmenci, T., Sofuoglu, E., Aydin, M., & Adebayo, T. S. (2024). The role of energy intensity, green energy transition, and environmental policy stringency on environmental sustainability in G7 countries. Clean Technologies and Environmental Policy. https://doi.org/10.1007/s10098-024-02968-y
14. Driscoll, J. C., & Kraay, A. C. (1998). Consistent covariance matrix estimation with spatially dependent panel data. Review of Economics and Statistics, 80(4), 549–560. https://doi.org/10.1162/003465398557825
15. Gil, L., & Bernardo, J. (2020). An approach to energy and climate issues aiming at carbon neutrality. Renewable Energy Focus, 33, 37–42. https://doi.org/10.1016/j.ref.2020.03.003
16. Groves, C., Henwood, K., Thomas, G., Roberts, E., Shirani, F., & Pidgeon, N. (2024). Where is 'the local' in localization? Energy Research and Social Science, 107, 103330. https://doi.org/10.1016/j.erss.2023.103330
17. Gyamfi, B. A., Agozie, D. Q., Ali, E. B., Bekun, F. V., & Asongu, S. A. (2024). Assessment of the influence of institutions and globalization on environmental pollution for open and closed economies. Quality & Quantity, 58(5), 4353–4381. https://doi.org/10.1007/s11135-023-01801-w
18. Hasan, M. M., Nan, S., & Waris, U. (2024). Assessing the dynamics among oil consumption, ecological footprint, and renewable energy. Resources Policy, 90, 104843. https://doi.org/10.1016/j.resourpol.2024.104843
19. Hausman, J. A. (1978). Specification tests in econometrics. Econometrica, 46(6), 1251–1271. https://doi.org/10.2307/1913827
20. Hunjra, A. I., Azam, M., Bruna, M. G., & Bouri, E. (2023). A cross-regional investigation of institutional quality and sustainable development. Journal of International Financial Markets, Institutions and Money, 84, 101758. https://doi.org/10.1016/j.intfin.2023.101758
21. Hussain, M., & Dogan, E. (2021). The role of institutional quality and environment-related technologies in environmental degradation for BRICS. Journal of Cleaner Production, 304, 127059. https://doi.org/10.1016/j.jclepro.2021.127059
22. Im, K. S., Pesaran, M. H., & Shin, Y. (2003). Testing for unit roots in heterogeneous panels. Journal of Econometrics, 115(1), 53–74. https://doi.org/10.1016/S0304-4076(03)00092-7
23. International Energy Agency. (2022). Achieving net zero heavy industry sectors in G7 members. IEA. https://www.iea.org/reports/achieving-net-zero-heavy-industry-sectors-in-g7-members
24. International Institute for Sustainable Development. (2024). What the G7 ministerial could have delivered on fossil fuel subsidies reform. IISD. https://www.iisd.org/articles/policy-analysis/g7-ministerial-fossil-fuel-

subsidies

25. Jiao, J., Yang, J., & Ding, T. (2023). Factor allocation efficiency of environmental protection industry in China. Journal of Environmental Management, 348, 119385. https://doi.org/10.1016/j.jenvman.2023.119385
26. Karaaslan, A., & Çamkaya, S. (2022). The relationship between CO2 emissions, economic growth, health expenditure, and renewable and non-renewable energy consumption: Empirical evidence from Turkey. Renewable Energy, 190, 457–466. https://doi.org/10.1016/j.renene.2022.03.139
27. Karim, S., Appiah, M., Naeem, M. A., Lucey, B. M., & Li, M. (2022). Modelling the role of institutional quality on carbon emissions in Sub-Saharan African countries. Renewable Energy, 198, 213–221. https://doi.org/10.1016/j.renene.2022.08.074
28. Kartal, M. T., Mukhtarov, S., & Kirikkaleli, D. (2025). Achieving environmental quality through stringent environmental policies: Comparative evidence from G7 countries. Geoscience Frontiers, 16(1), 101956. https://doi.org/10.1016/j.gsf.2024.101956
29. Khan, A., Sampene, A. K., & Ali, S. (2023). Towards environmental degradation mitigation: The role of regulatory quality, technological innovation and government effectiveness in the CEMAC countries. Heliyon, 9(6), e16673. https://doi.org/10.1016/j.heliyon.2023.e16673
30. Kousar, S., Pervaiz, A., Ahmed, F., & Nuţă, F. M. (2024). Do structural transformations in the energy sector help to achieve decarbonization? Energies, 17(18), 4600. https://doi.org/10.3390/en17184600
31. Levin, A., Lin, C.-F., & Chu, C.-S. J. (2002). Unit root tests in panel data: Asymptotic and finite-sample properties. Journal of Econometrics, 108(1), 1–24. https://doi.org/10.1016/S0304-4076(01)00098-7
32. Liu, B., Ding, J., Cifuentes-Faura, J., & Liu, X. (2023). Toward carbon neutrality: How will environmental regulatory policies affect corporate green innovation? Economic Analysis and Policy, 80, 1006–1020. https://doi.org/10.1016/j.eap.2023.09.019
33. Maddala, G. S., & Wu, S. (1999). A comparative study of unit root tests with panel data and a new simple test. Oxford Bulletin of Economics and Statistics, 61(S1), 631–652. https://doi.org/10.1111/1468-0084.0610s1631
34. Maji, I. K., Saari, M. Y., & Bello, U. A. (2023). Institutional quality, green trade and carbon emissions in sub-Saharan Africa. Cleaner Energy Systems, 6, 100086. https://doi.org/10.1016/j.cles.2023.100086
35. Maji, I. K., Saari, M. Y., Habibullah, M. S., & Mohd Saudi, N. S. (2022). Clean energy, institutional quality and environmental sustainability in sub-Saharan Africa. Cleaner Materials, 6, 100135. https://doi.org/10.1016/j.clema.2022.100135
36. Murray, B., & Rivers, N. (2015). British Columbia's revenue-neutral carbon tax: A review of the latest 'grand experiment' in environmental policy. Energy Policy, 86, 674–683. https://doi.org/10.1016/j.enpol.2015.08.011
37. Omri, A., & Boubaker, S. (2024). When do climate change legislation and clean energy policies matter for net-zero emissions? Journal of Environmental Management, 354, 120275. https://doi.org/10.1016/j.jenvman.2024.120275
38. Pesaran, M. H. (2004). General diagnostic tests for cross-section dependence in panels (CESifo Working Paper No. 1229). CESifo. https://doi.org/10.2139/ssrn.572504

39. Pesaran, M. H. (2015). Testing weak cross-sectional dependence in large panels. Econometric Reviews, 34(6–10), 1089–1117. https://doi.org/10.1080/07474938.2014.956623
40. Preacher, K. J., & Hayes, A. F. (2008). Asymptotic and resampling strategies for assessing and comparing indirect effects in multiple mediator models. Behavior Research Methods, 40(3), 879–891. https://doi.org/10.3758/BRM.40.3.879
41. Roodman, D., MacKinnon, J. G., Nielsen, M. Ø., & Webb, M. D. (2019). Fast and wild: Bootstrap inference in Stata using boottest. The Stata Journal, 19(1), 4–60. https://doi.org/10.1177/1536867X19830877
42. Salvia, M., Reckien, D., Pietrapertosa, F., Eckersley, P., Spyridaki, N.-A., Krook-Riekkola, A., Olazabal, M., De Gregorio Hurtado, S., Simoes, S. G., & Geneletti, D. (2021). Will climate mitigation ambitions lead to carbon neutrality? Renewable and Sustainable Energy Reviews, 135, 110253. https://doi.org/10.1016/j.rser.2020.110253
43. Shen, B., Kahrl, F., & Satchwell, A. J. (2021). Facilitating power grid decarbonization with distributed energy resources. Annual Review of Environment and Resources, 46, 349–375. https://doi.org/10.1146/annurev-environ-111320-071618
44. Sobel, M. E. (1982). Asymptotic confidence intervals for indirect effects in structural equation models. Sociological Methodology, 13, 290–312. https://doi.org/10.2307/270723
45. United Nations Climate Change. (2016.). Paris Agreement – Status of ratification. UNFCCC. https://unfccc.int/process/the-paris-agreement/status-of-ratification
46. Vatalis, K. I., Avlogiaris, G., & Tsalis, T. (2022). Just transition pathways of energy decarbonization under the global environmental changes. Journal of Environmental Management, 309, 114713. https://doi.org/10.1016/j.jenvman.2022.114713
47. Wang, Z., Yen-Ku, K., Li, Z., An, N. B., & Abdul-Samad, Z. (2022). The transition of renewable energy and ecological sustainability through environmental policy stringency. Renewable Energy, 188, 70–80. https://doi.org/10.1016/j.renene.2022.01.107
48. Xu, H., Ahmad, M., Aziz, A. L., Uddin, I., Aljuaid, M., & Gu, X. (2024). The linkages between energy efficiency, renewable electricity, human capital and inclusive growth. Energy Strategy Reviews, 53, 101414. https://doi.org/10.1016/j.esr.2024.101414
49. Yu, G., Ye, X., Ye, Y., Huang, H., & Xia, X. (2024). Optimal decarbonisation pathway for mining truck fleets. Journal of Automation and Intelligence, 3(3), 129–143. https://doi.org/10.1016/j.jai.2024.03.003
50. Yuan, H., Zou, L., & Feng, Y. (2023). How to achieve emission reduction without hindering economic growth? The role of judicial quality. Ecological Economics, 209, 107839. https://doi.org/10.1016/j.ecolecon.2023.107839