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Has Economic Growth Decoupled from Energy Demand in Latin America? Evidence for Sustainable Energy Transitions

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Abstract

Energy efficiency gains do not necessarily mean that economic growth requires less additional energy. We assess this distinction in ten Latin American economies during 2010–2022, using 122 complete country–year observations and accounting for persistent country differences and shared annual shocks. All ten countries contribute through 2021; only Brazil and Mexico enter in 2022. The estimated income elasticity of energy use per person is 1.001, with an enumerated country wild-bootstrap p-value of 0.023 against zero. To distinguish compatibility with unity from evidence of proportional coupling, we enumerate all 1024 country-level sign patterns separately at each boundary of the illustrative equivalence band [0.9, 1.1]. The resulting equivalence p-value is 0.387: proportional coupling is not established, and the directional test also does not establish relative decoupling. Alternative covariance and lagged-income specifications retain positive associations. Observed fuel-price sensitivities cover 30 observations and remain imprecise. Renewable-share moderation is not supported directionally, while country-level Tapio trajectories vary across periods. With only ten country clusters, limited inferential power constrains the economic classification. The findings support separate monitoring of energy efficiency, per-capita demand and supply decarbonization rather than treating improvements in one as evidence of progress in all three. The estimates describe associations and do not identify causal policy effects.

Keywords: income–energy coupling; Tapio decoupling; energy efficiency; renewable energy; sustainable energy transition; Latin America; exhaustively enumerated wild-cluster bootstrap; SDG 7; SDG 8

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1. Introduction

Latin America and the Caribbean combine abundant hydropower, solar and wind potential, fossil-fuel producers, critical minerals and rapidly changing electricity systems. Yet the region also faces infrastructure bottlenecks, fiscal constraints, energy-security risks and growing demand for mobility, cooling, digital services and urban infrastructure. These pressures make the energy transition simultaneously an environmental, productive and social-development challenge [1–3].

The sustainability question is therefore not simply whether the region is becoming more energy efficient, but whether economic growth is becoming less dependent on additional energy demand. This distinction maps directly onto the Sustainable Development

Goals. SDG 7.3 calls for faster improvements in energy efficiency; SDG 8.4 explicitly calls for improved resource efficiency and the decoupling of economic growth from environmental degradation; SDG 12 focuses on sustainable consumption and production; and SDG 13 places these transformations within the climate-action agenda [4].

Energy intensity per unit of GDP can decline while energy use per person continues to rise with income. A country may become more efficient in converting energy into output and nevertheless remain coupled in per-capita demand if scale effects exceed efficiency gains. Consequently, evidence of lower energy intensity is not equivalent to evidence that development has become compatible with a lower energy-demand trajectory.

Existing evidence addresses complementary outcomes and time horizons. Balza et al. [5], using Latin American and Caribbean panel data for 1971–2019, report income associations of approximately 0.4 in the short run and 0.9 in the long run. Chen and Steinbuks [6] instead examine energy-intensity improvements and the potential contribution of efficiency policies; Carvajal et al. [7] place regional efficiency progress within the energy transition. A long-run elasticity below unity, lower energy intensity and lower energy use per person are different findings. Their coexistence motivates testing the present within-country elasticity explicitly, rather than transferring a decoupling label across outcomes, samples or time horizons.

This article addresses a specific measurement and inference gap: whether regional improvements in energy intensity also imply a less-than-proportional within-country association between income and energy use per person. ENER is measured in kilograms of oil equivalent per person, whereas the official primary-energy-intensity indicator measures energy per unit of output. Testing an elasticity against unity and testing practical equivalence to unity answer different questions; failure to reject equality cannot establish proportional coupling. The contribution is a transparent comparison of these concepts and of regional regression estimates with country-level trajectories, rather than a claim that the positive income–energy association is new.

The empirical sample comprises Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Mexico, Paraguay, Peru and Uruguay. The revised main database is unbalanced from 2010 to 2022 and contains 122 complete observations: all ten countries are observed through 2021, and Brazil and Mexico contribute complete six-variable observations in 2022. Missing 2022 country-years are not imputed. The design therefore extends the evidence as far as the verified official data allow without manufacturing balance. Regional data context is provided by WDIs [8].

This paper makes six contributions. First, it separates per-capita energy demand from energy intensity. Second, it uses the verified 2010–2022 unbalanced panel while disclosing sparse final-year coverage. Third, it combines tests against unity, an illustrative equivalence assessment and Tapio's eight-state taxonomy. Fourth, it reports exhaustive enumeration of all 2^{10}; country-cluster Rademacher sign patterns, eliminating Monte Carlo sampling error in the bootstrap enumeration, without claiming exact finite-sample validity. Fifth, it examines whether renewable-energy share moderates the conditional income association. Sixth, it compares per-capita demand with official primary-energy intensity and relates the distinction to SDGs 7.3, 8.4, 12 and 13.

1.1. Scale, Composition and Technique Effects

Energy demand responds to development through scale, composition and technique effects. The scale effect raises demand as production, mobility, appliance ownership and infrastructure expand. Composition effects depend on whether activity shifts toward mining, manufacturing and transport-intensive sectors or toward less energy-intensive services. Technique effects reflect capital renewal, efficiency standards, technology and fuel substitution [9–12].

The Environmental Kuznets Curve (EKC) provides a competing theoretical possibility: technique and composition changes may eventually outweigh the scale of development. An EKC concerns a nonlinear relationship between income and an environmental outcome; it does not imply that total energy use must fall when carbon-intensive energy is replaced. Urbanization and industrialization can strengthen scale effects or enable efficiency gains, depending on development and sector composition [11]. The present constant-elasticity model contains no income-squared term and cannot locate an EKC turning point or assign any sampled economy to an ascending or descending EKC segment. Such positions are therefore not asserted.

Development-stage heterogeneity also matters for interpreting the EKC debate. Li and Lin [11] find that urbanization and industrialization have different associations with energy use and emissions across income groups. Thus, a common sign for urbanization or industry cannot be assumed in economies undergoing different structural transitions. In the scale–composition–technique framework, access to energy services can expand alongside efficiency improvements, while industrial restructuring can change energy requirements per unit of output. This makes the direction and magnitude of the income association an empirical question. Our linear specification assesses an average conditional elasticity; it does not test the nonlinear turning-point proposition of the EKC.

In a log–log energy-demand equation, the GDP coefficient is an income elasticity. Values between zero and one describe less-than-proportional growth in energy use per person; unity describes proportional growth. Statistical evidence for either statement requires adequate precision. For a compatible energy/output ratio, the accounting identity $\Delta\ln(E/Y) = \Delta\ln(E/P) - \Delta\ln(Y/P)$ shows why intensity can fall while energy use per person rises. This identity provides conceptual guidance, not an identity imposed on the separately reported official intensity series or on the Section 3.5 figure's arithmetic country averages. Tapio endpoint ratios and conditional regression elasticities also differ in their time aggregation and adjustment for covariates.

1.2. Renewable Transition, Urbanization, Industry and Trade

Renewable energy may displace fossil fuels while total energy demand continues to expand. Latin American evidence emphasizes heterogeneous resource endowments, technology, finance and spatial spillovers [13–17]. A higher renewable share can therefore decarbonize supply without necessarily reducing the income elasticity of total energy use.

Resource endowments become operating capacity through financing, technology and policy implementation [1–3,15–17]. Institutional quality and environmental-policy ambition are distinct concepts. Kaufmann, Kraay and Mastruzzi [18] distinguish public-service effectiveness, regulatory quality and the rule of law, and they emphasize measurement uncertainty. Those broad governance constructs cannot be substituted automatically for environmental enforcement or climate-policy stringency. Official national records document different legal and planning frameworks (see Section 4.1), including Chile's 2022 statutory neutrality objective and other countries' NDCs or long-term strategies [19–25]. They provide historical context rather than a comparable annual policy index. Fixed effects absorb persistent differences, but not changes in enforcement. Commitment dates and targets are not treated as observed compliance or causal explanations of the estimated elasticities.

These studies imply that policy ambition and realized demand adjustment should be analyzed separately. The efficiency-policy perspective in Chen and Steinbuks [6] concerns the conditions under which energy requirements per unit of output may improve, whereas the broader governance concepts in Kaufmann et al. [18] concern institutional capacity and measurement. Neither provides a direct annual measure of environmental enforcement for this panel. Renewable endowments likewise create opportunities, but

their translation into generation and final consumption depends on investment, technology and implementation [13–17]. The historical policy inventory therefore supplies context for regional heterogeneity rather than a ranking of regulatory stringency or a measured policy effect. This distinction connects the literature to our associational design and to the limitations of the renewable interaction.

Urbanization can increase housing, transport, cooling and infrastructure demand, but density and public transport may generate efficiency gains [26,27]. Industry and trade are similarly ambiguous because sector composition, imported technology, productivity and logistics intensity can push energy demand in opposite directions [11,28].

1.3. Hypotheses and Research Questions

H1. Income scale association: $\beta GDP > 0$. The expansion of energy services with income suggests a positive association, but composition and technique changes can offset it [9–12]. H1 provides a benchmark rather than the principal novelty of this study. The regional question is whether those offsets are sufficiently strong to support H2 or whether the available sample remains too imprecise to distinguish responses around unity.

H2. Relative decoupling: $0 < \beta GDP < 1$. The directional test evaluates H0: $\beta GDP \geq 1$ against $\beta GDP < 1$; a positive elasticity is additionally required to describe relative rather than absolute decoupling. A separate two-one-sided-test (TOST) sensitivity evaluates practical equivalence within [0.9, 1.1]. This band treats a 1% income change as associated with approximately 0.9–1.1% energy-demand change. It is the reviewer-proposed illustrative tolerance, not a prespecified or universally valid policy threshold. Failure of either test leaves the corresponding claim unestablished.

H3. Renewable moderation: $\beta GDP \times REN < 0$. Renewable technologies may accompany efficiency and structural change, motivating a negative interaction, whereas additional energy-service demand, electrification and supply expansion can offset that channel [13–17]. A renewable share measures the fuel mix rather than efficiency itself. Thus, the interaction hypothesis is empirically contestable and is evaluated as a conditional association, not as a causal mechanism.

RQ1. How do Tapio decoupling states differ across countries and between 2010–2019, 2010–2020, and the verified 2010–2022 endpoints?

RQ2. Can official declines in primary-energy intensity coexist with a regional income–energy elasticity whose precision does not establish decoupling or equivalence to unity? Table 1 summarizes how this study relates to the preceding literature.

Table 1. Positioning of this study relative to the literature.

Study	Coverage	Energy Concept	Method	Main Message	Difference Here
Balza et al. [5]	LAC; long panel	Energy consumption	Panel econometrics	Long-run income elasticity ≈ 0.9	No formal β = 1/Tapio comparison
Chen and Steinbuks [6]	25 LAC countries	Energy intensity	Decomposition + regressions	Sectoral efficiency gains reduce intensity	Different outcome: energy/output
Carvajal et al. [7]	LAC; 1990–2022	Energy efficiency	Regional assessment	Efficiency gains/decoupling narrative	Regional aggregate
Tapio [29]	Decoupling framework	Energy/activity growth	Elasticity taxonomy	Eight decoupling states	Applied here to LAC energy demand
This study	10 economies; 2010–2022	Per-capita energy use	TWFE + enumerated wild + Tapio	Positive association; equivalence to unity not established	Separates inference on demand, intensity and SDGs

2. Materials and Methods

2.1. Data, Sample and Measurement Discipline

The revised main econometric sample is an unbalanced country–year panel covering 2010–2022 with 122 complete observations. All ten economies are observed from 2010 through 2021. In 2022, Brazil and Mexico have complete values for all six model variables and enter the regression; other country-years remain outside the six-variable estimation when at least one required official observation is unavailable. This is deliberate: no missing value is interpolated, extrapolated, simulated or replaced by zero.

The dependent variable, ENER, is energy use per capita (kg of oil equivalent per person) [30]. GDP is GDP per capita at PPP in constant 2021 international dollars [31]. Urbanization is the urban share of total population [32]. Renewable energy is renewable consumption as a percentage of total final energy consumption [33,34]. Industry value added and trade openness are percentages of GDP [35,36]. All baseline model variables enter in natural logarithms.

For Mexico, the urbanization series is placed on one current WDI/UN World Urbanization Prospects vintage over 2010–2022 to avoid an artificial 2020–2021 break caused by mixing successive UN population revisions. Source-vintage notes are retained at the observation level in the replication workbook.

The independent efficiency comparison uses the official World Bank indicator EG.EGY.PRIM.PP.KD—primary-energy intensity in MJ per 2021 PPP GDP [37]. It replaces the earlier internally constructed energy/GDP proxy. The figure in Section 3.5 uses a constant four-country subset (Argentina, Brazil, Chile and Mexico) over 2013–2022 so that the index comparison is not driven by changing country composition.

2.2. Econometric Specification and Exhaustively Enumerated Small-Cluster Inference

The preferred model is as follows:

$$\ln(\mathrm{ENER}_{it}) = \beta_1 \ln(\mathrm{GDP}_{it}) + \beta_2 \ln(\mathrm{URB}_{it}) + \beta_3 \ln(\mathrm{REN}_{it}) + \beta_4 \ln(\mathrm{IND}_{it}) + \beta_5 \ln(\mathrm{TRD}_{it}) + \alpha_i + \lambda_t + \varepsilon_{it}. \quad (1)$$

Country fixed effects absorb time-invariant structural heterogeneity, and year fixed effects absorb common shocks. Coefficients are interpreted as conditional within-country associations, not causal effects. Identification relies on variation remaining after both sets of fixed effects and the included controls. Income and energy use can be jointly determined, and time-varying omitted factors can affect both. Neither fixed effects nor robust covariance estimates resolve that endogeneity. Accordingly, the coefficient is not interpreted as the causal effect of economic growth.

Because there are only ten country clusters, primary hypothesis decisions use country-clustered studentization and null-imposed Rademacher wild-cluster bootstrap inference [38,39]. With $G = 10$, all $2^{10} = 1024$ country sign patterns are enumerated. The same procedure is used for GDP against zero, equality with unity, the directional relative-decoupling test, both equivalence-boundary tests and the renewable interaction. Country-clustered standard errors and conventional t-based intervals remain labeled companion summaries; the equivalence decision no longer uses a parametric t distribution. Exhaustive enumeration removes Monte Carlo sampling error, not small-sample approximation error.

The equality test H0: $\beta\mathrm{GDP} = 1$ assesses compatibility with unity, not equivalence. For relative decoupling, the lower-tail enumerated test is evaluated at the null boundary $\beta\mathrm{GDP} = 1$. For practical equivalence, TOST evaluates H0L: $\beta\mathrm{GDP} \leq 0.9$ against $\beta\mathrm{GDP} > 0.9$ and H0U: $\beta\mathrm{GDP} \geq 1.1$ against $\beta\mathrm{GDP} < 1.1$. Each component separately imposes its boundary value, re-estimates nuisance slopes and fixed effects under that restriction, and forms bootstrap outcomes from restricted fitted values plus restricted residuals multiplied by

a common sign within each country. The unrestricted model and its country-clustered standard error are re-estimated for every bootstrap outcome. With tL = (β^GDP − 0.9)/SE and tU = (β^GDP − 1.1)/SE, pL is the fraction of lower-bound bootstrap statistics at least tL, and pU is the fraction of upper-bound statistics at most tU. Inclusive comparisons allow an absolute numerical tolerance of 10^{-9}. The TOST *p*-value is max(pL, pU); equivalence requires both one-sided *p*-values below 0.05. The same 1024 sign vectors are reused under two distinct restricted data-generating processes, not pooled into one distribution. No percentile interval, parametric t tail or Monte Carlo correction enters the TOST decision.

2.3. Tapio Decoupling Taxonomy

Country heterogeneity is evaluated using Tapio's eight-state decoupling taxonomy [29]. For each available endpoint period, the elasticity is as follows:

$$D = [(E_1 - E_0)/E_0]/[(GDP_1 - GDP_0)/GDP_0]. \quad (2)$$

When GDP grows, $D < 0$ denotes strong decoupling; $0 \leq D < 0.8$ weak decoupling; $0.8 \leq D \leq 1.2$ expansive coupling; and $D > 1.2$ expansive negative decoupling. When GDP declines, the corresponding states are strong negative decoupling, weak negative decoupling, recessive coupling and recessive decoupling. This paper reports 2010–2019 to avoid pandemic contamination, 2010–2020 for direct comparison with the earlier endpoint, and 2010–2022 wherever a verified 2022 energy-use and GDP endpoint exists.

2.4. Renewable-Energy Moderation Mechanism

To test H3, a mechanism specification replaces logged renewable share with its centered percentage-point level and includes a centered interaction:

$$\ln(ENER_{it}) = \gamma_1 \ln(GDP)^c_{it} + \gamma_2 REN^c_{it} + \gamma_3 [\ln(GDP)^c_{it} \times REN^c_{it}] + controls + \alpha_i + \lambda_t + \upsilon_{it}. \quad (3)$$

Mean-centering does not change the interaction coefficient, but it makes the main effects interpretable near the sample mean. H3 predicts $\gamma_3 < 0$, meaning that a higher renewable share weakens the income elasticity of energy demand.

Renewable share is renewable consumption as a percentage of total final energy consumption, not primary-energy consumption (Table 2). The share and its income interaction may be endogenous: renewable supply and total demand jointly determine the ratio, while hydropower conditions, industrial structure and energy prices may affect both the share and demand. The direction of bias is not identified. Expanding non-renewable demand can lower the renewable share, creating a negative association; hydroelectric expansion or omitted demand growth accompanied by renewable deployment can create a positive association. These channels can bias the interaction in different directions. Time-invariant geographic endowments are absorbed by country fixed effects and cannot alone instrument a within-country model. An interacted or time-varying instrument would need demonstrated relevance and an exclusion restriction; hydrology can directly affect supply and demand. No suitable instrument is supplied, so no IV result is reported. We also screen observed two-year lagged income, renewable share and their product as internal candidates on 102 rows. After residualizing country/year effects and current controls, the excluded-instrument matrix and its cross-product with the three candidate endogenous regressors each have rank three. This is an algebraic identification condition, not a strength or exclusion test. Residual serial correlation (see Section 3.1) makes lag exogeneity questionable. No internally lagged instrument is therefore promoted to a validated causal design. Static hydropower potential or solar endowments remain absorbed by country effects; adding interactions with shocks would introduce new relevance and exclusion assumptions rather

than automatically solve the problem. In particular, the sign of a simple association between the renewable share and total demand does not determine the sign of bias in the GDP–renewable interaction: that bias also depends on how the omitted factor covaries with income and the residualized product term. The available data do not identify those covariances, so a unique, likely biased direction cannot be established.

Table 2. Variables, measurement and official sources.

Variable	Concept	Unit	Code/Source	Transform	Role
ENER	Energy use per capita	kg oil equivalent per capita	EG.USE.PCAP.KG.OE [30]	ln	Outcome
GDP	GDP per capita, PPP	constant 2021 int. $	NY.GDP.PCAP.PP.KD [31]	ln	Income scale
URB	Urban population share	% population	SP.URB.TOTL.IN.ZS [32]	ln	Control
REN	Renewable energy consumption	% final energy consumption	EG.FEC.RNEW.ZS/ SDG 7.2.1 [33,34]	ln; level in interaction	Transition
IND	Industry value added	% GDP	NV.IND.TOTL.ZS [35]	ln	Control
TRD	Trade openness	% GDP	NE.TRD.GNFS.ZS [36]	ln	Control
EI	Primary-energy intensity	MJ/$2021 PPP GDP	EG.EGY.PRIM.PP.KD [37]	index	Independent comparison

2.5. Diagnostics

Groupwise heteroskedasticity is assessed with a Modified Wald procedure [40], and Breusch–Pagan is retained as a pooled diagnostic [41]. First-order serial correlation is evaluated using the Wooldridge–Drukker procedure [42], residual cross-sectional dependence is evaluated using Pesaran's CD statistic [43], and multicollinearity is evaluated using variance inflation factors. Because residual dependence is detected, Section 3.6 now report Driscoll–Kraay [44], year-clustered and two-way country/year-clustered covariance sensitivities computed from the supplied main panel. These checks address inference, not omitted-variable bias.

For reproducibility, covariance sensitivities use the same OLS equation with explicit country and year dummies (122 observations; 27 parameters, including the intercept). One-way cluster covariances use $G/(G-1) \times (n-1)/(n-K)$. The two-way covariance adds country and year cluster covariances and subtracts the intersection covariance, which here is HC1 because each country–year is unique. The Driscoll–Kraay calculation uses time sums of regressor–residual scores, Bartlett weights, two annual lags and $n/(n-K)$ scaling. Tests against zero use t12 for year clustering and Driscoll–Kraay, and t9 for country and two-way clustering. With only 13 annual periods and ten countries, these are sensitivity calculations, not definitive asymptotic guarantees. A further specification replaces contemporaneous logged GDP with its observed one-year lag, retaining current controls and both fixed effects; the first year of each country is dropped, leaving 112 observations. No lag is imputed. Lagging income does not eliminate endogeneity under persistent omitted shocks.

The price sensitivity adds one logged observed retail fuel price at a time to Equation (1). GIZ's surveys report mid-November super-gasoline and diesel prices in nominal US cents per liter for 2010, 2012 and 2014 [45,46]. Dividing by 100 gives USD per liter; all 30 matched rows are observed. We also re-estimate the baseline on exactly those rows to separate sample selection from adding a price control. Neither interpolation nor an international common oil-price series is substituted for missing national prices. Nominal USD prices

mix pricing, exchange-rate and inflation influences and are not an annual real index of total energy costs. This is a limited transport-fuel sensitivity, not a price-adjusted 122-row baseline. Separate price controls avoid conflating gasoline and diesel and do not identify their causal demand effects.

2.6. Software and Reproducibility

The replication package contains the unchanged original workbook as a historical provenance archive and a canonical Round 2 workbook. The executable Python script recomputes the baseline, covariance and lagged-income sensitivities, observed-price models and all enumerated wild-bootstrap tests. Separate files retain the 1024 studentized statistics at $\beta GDP = 0.9$ and $\beta GDP = 1.1$. An independent implementation using explicit country/year dummy variables verifies every TOST bootstrap statistic and both tail counts against the residualized implementation. The canonical TOST replaces the Round 1 parametric assessment. No random sampling, imputation or seed is used. Original sample observations and model coefficients are retained; enumeration does not confer exact finite-sample validity [38,39]. The Supplementary Materials accompanying this article provide the canonical Round 2 workbook, original provenance data, source register, executable Python scripts, bootstrap boundary distributions and figure inputs (see Supplementary Materials statement). The Supplementary Materials cited in this section provide the data, code, numerical verification outputs, and figure-generation files needed to reproduce the reported analyses.

3. Results

3.1. Descriptive Evidence and Diagnostics

The sample contains 122 complete observations. All ten countries enter in 2010–2021, whereas only Brazil and Mexico enter in 2022. The final-year dummy consequently draws information from two economies conditional on the common slope specification; it is not a representative regional 2022 shock estimate. No missing row is imputed. The exclude-2022 check in Section 3.6 yields $\beta GDP = 1.001$ (SE = 0.273), essentially unchanged, reducing concern that the regional point estimate is driven by the final year. Figure 1 displays the observed country-level energy-use trajectories; Table 3 summarizes the model variables, and Table 4 reports the diagnostic tests.

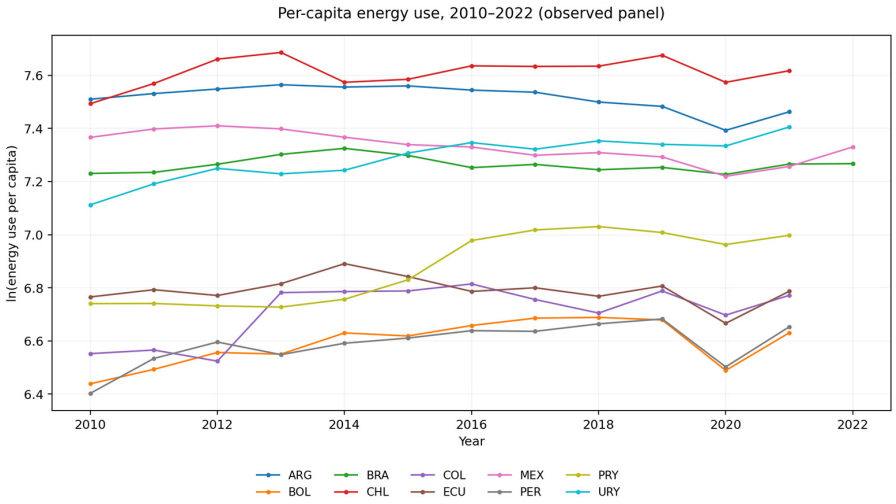

Figure 1. Logged per-capita energy use in the unbalanced 2010–2022 estimation panel. Lines end at each country's last complete main-sample observation. Line crossings and coincident segments reflect the observed values; they do not represent duplicated or interpolated country–year observations.

Table 3. Descriptive statistics for the 2010–2022 unbalanced main sample.

Variable	N	Mean	SD	Min.	Max.
logENER	122	7.061	0.374	6.403	7.686
logGDP	122	9.789	0.362	8.932	10.309
logURB	122	4.357	0.145	4.082	4.560
logREN	122	3.203	0.656	2.152	4.153
logIND	122	3.324	0.203	2.854	3.631
logTRD	122	3.896	0.359	3.113	4.486

Table 4. Diagnostics for the preferred specification.

Diagnostic	Statistic	*p*-Value	Conclusion
Modified Wald groupwise heteroskedasticity	268.225	<0.001	Heteroskedasticity
Breusch–Pagan diagnostic	52.569	<0.001	Heteroskedasticity
Wooldridge–Drukker serial-correlation test	F = 13.892	0.0047	First-order serial correlation
Pesaran CD	−2.461	0.014	Residual cross-sectional dependence
Maximum VIF	4.797	—	No severe multicollinearity

3.2. Main Estimates and Formal Decoupling Test

The GDP elasticity is 1.001 and differs from zero under exhaustive enumeration ($p = 0.023$). Equality with unity is not rejected (t = 0.003; enumerated two-sided $p = 1.000$), but the companion 95% country-clustered t interval [0.381, 1.620] includes substantial relative decoupling and more-than-proportional responses. The lower-tail enumerated test of βGDP < 1 gives $p = 513/1024 = 0.500977$. For equivalence within [0.9, 1.1], the lower-bound statistic is 0.367906, with 387/1024 bootstrap statistics in its upper tail (pL = 0.377930); the upper-bound statistic is −0.362173, with 396/1024 in its lower tail (pU = 0.386719). Hence, TOST $p = 0.386719$, and neither component rejects at 5%. The enumerated test does not establish practical equivalence. Neither relative decoupling nor proportional coupling is demonstrated; the estimate is compatible with unity without proving it. Table 5 reports the main estimates and the formal inference tests; Figure 2 displays the corresponding country-clustered confidence intervals.

Table 5. Main two-way fixed-effects model with enumeration-based small-cluster inference.

Variable	Coef.	Cluster SE	Cluster *p*	Wild *p* (Enum.)	95% Cluster CI
GDP per capita	1.001	0.274	0.005	0.023	[0.381, 1.620]
Urbanization	0.460	1.781	0.802	0.811	[−3.569, 4.488]
Renewable-energy share	−0.066	0.100	0.526	0.590	[−0.293, 0.161]
Industry value added	−0.095	0.149	0.539	0.615	[−0.432, 0.241]
Trade openness	0.076	0.128	0.569	0.656	[−0.214, 0.365]
Countries/clusters	10				
Observations	122				
Within R^2	0.469				
Joint F (5, 9)	4.426	$p = 0.026$			
H0: βGDP = 1	t = 0.003			1.000	Unity not rejected; equivalence not established
H2: βGDP < 1 (enumerated)	t = 0.003			0.501	Relative decoupling not established
TOST H0L: βGDP ≤ 0.9	t = 0.368			0.378	387/1024; upper tail
TOST H0U: βGDP ≥ 1.1	t = −0.362			0.387	396/1024; lower tail
TOST joint decision				0.387	max(pL, pU); equivalence not established

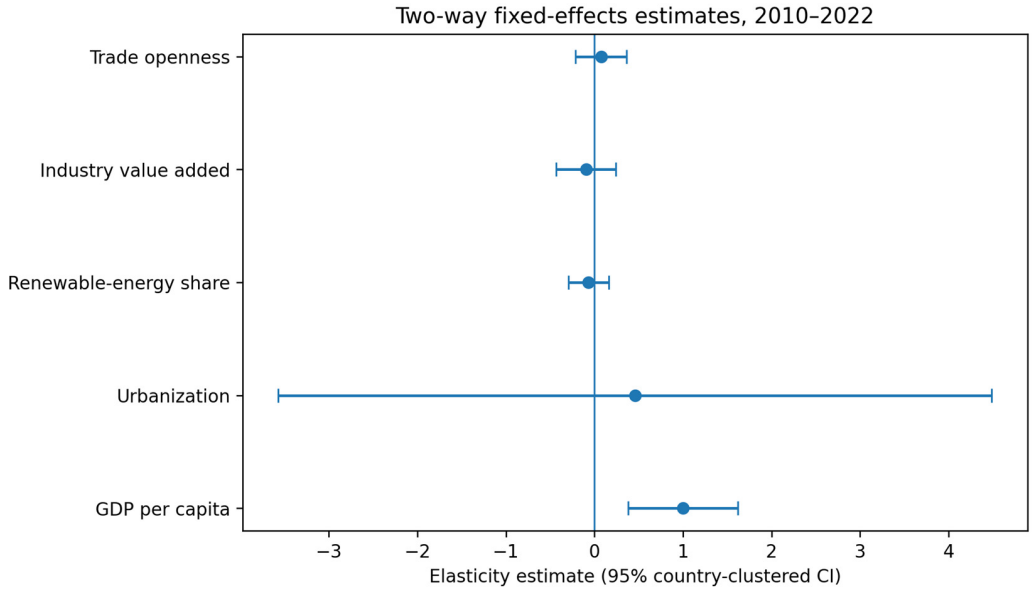

Figure 2. Two-way fixed-effects coefficients with 95% country-clustered confidence intervals, 2010–2022.

Urbanization, renewable share, industry and trade have wide confidence intervals in the baseline (Table 5). Fixed effects remove persistent cross-country differences and common trends; slowly evolving shares can consequently provide limited identifying variation. There are only ten clusters, and the broad intervals cannot distinguish opposing scale, density, composition and technique channels. These explanations are plausible identification constraints, not measured variance decompositions. The negative industry coefficient (-0.095; 95% CI $[-0.432, 0.241]$) does not establish an energy-saving effect. Statistical insignificance of the controls does not justify excluding them or treating their underlying mechanisms as absent. Industry value added is measured as a share of GDP, not as the level of industrial output or its energy consumption. Conditional on income and the other controls, the negative point estimate therefore cannot be read as evidence that expanding industrial production reduces energy demand. Omitted national energy prices may affect this estimate as well as the income coefficient; the matched-price exercise is a sensitivity check rather than a correction proven to eliminate that bias.

3.3. Tapio Decoupling States

The Tapio results show why a single endpoint classification is insufficient. Before the pandemic, Mexico and Ecuador exhibit strong or weak decoupling, whereas Paraguay and Uruguay show expansive negative decoupling and several countries remain in expansive coupling. Including 2020 changes several states because the pandemic simultaneously reduced GDP and energy demand. For the verified 2010–2022 endpoints, Mexico shows strong decoupling; Colombia, weak decoupling; Chile, expansive coupling; Brazil, expansive negative decoupling; and Argentina, recessive coupling. The remaining five 2022 states are left unclassified because the supplied official source layer does not contain verified 2022 energy-use and GDP endpoints for them. Table 6 reports the country-level classifications for each endpoint period.

Table 6. Tapio decoupling elasticity and eight-state classification by country and period.

Country	2010–2019	2010–2020	2010–2022
Argentina	D = 0.524; Weak negative decoupling	D = 0.742; Weak negative decoupling	D = 1.149; Recessive coupling
Bolivia	D = 0.853; Expansive coupling	D = 0.273; Weak decoupling	NA
Brazil	D = −9.495; Strong negative decoupling	D = 0.092; Weak negative decoupling	D = 1.375; Expansive negative decoupling
Chile	D = 1.174; Expansive coupling	D = 0.948; Expansive coupling	D = 0.810; Expansive coupling
Colombia	D = 1.133; Expansive coupling	D = 1.201; Expansive negative decoupling	D = 0.682; Weak decoupling
Ecuador	D = 0.223; Weak decoupling	D = −1.424; Strong decoupling	NA
Mexico	D = −0.864; Strong decoupling	D = 8.279; Recessive decoupling	D = −0.533; Strong decoupling
Paraguay	D = 1.515; Expansive negative decoupling	D = 1.409; Expansive negative decoupling	NA
Peru	D = 1.144; Expansive coupling	D = 0.815; Expansive coupling	NA
Uruguay	D = 1.240; Expansive negative decoupling	D = 2.124; Expansive negative decoupling	NA

3.4. Does Renewable Energy Weaken Income–Energy Coupling?

The interaction coefficient is positive (0.0154) rather than negative. Its enumeration-based two-sided wild-bootstrap *p*-value is 0.090, while the one-sided enumeration-based wild-bootstrap *p*-value for the prespecified directional hypothesis H3: $\gamma 3 < 0$ is 0.956. H3 is therefore not supported. The data do not show that a higher renewable-energy share weakens the coupling between GDP and total per-capita energy use. This does not imply that renewables fail to decarbonize supply; it indicates that fuel-mix change and demand decoupling are distinct mechanisms. Table 7 provides the estimated renewable-share moderation coefficients.

Table 7. Renewable-energy moderation mechanism.

Variable	Coef.	Cluster SE	Cluster *p*	Wild *p* (Enum.)	One-Sided H3 *p*
GDP (centered log)	0.9502	0.1600	0.000	0.006	—
REN share (centered pp)	−0.0027	0.0033	0.428	0.512	—
GDP × REN	0.0154	0.0049	0.012	0.090	0.956
logURB	0.3176	1.1288	0.785	0.746	—
logIND	−0.1101	0.0896	0.250	0.334	—
logTRD	0.0483	0.1078	0.665	0.701	—

The implied GDP elasticity rises from approximately 0.69 at the first quartile of renewable share to 1.19 at the third quartile. Because the interaction is not significant at the 5% level under the enumeration-based two-sided wild-bootstrap test, this gradient is treated as suggestive rather than structural evidence. Figure 3 illustrates the estimated marginal income elasticity across renewable shares.

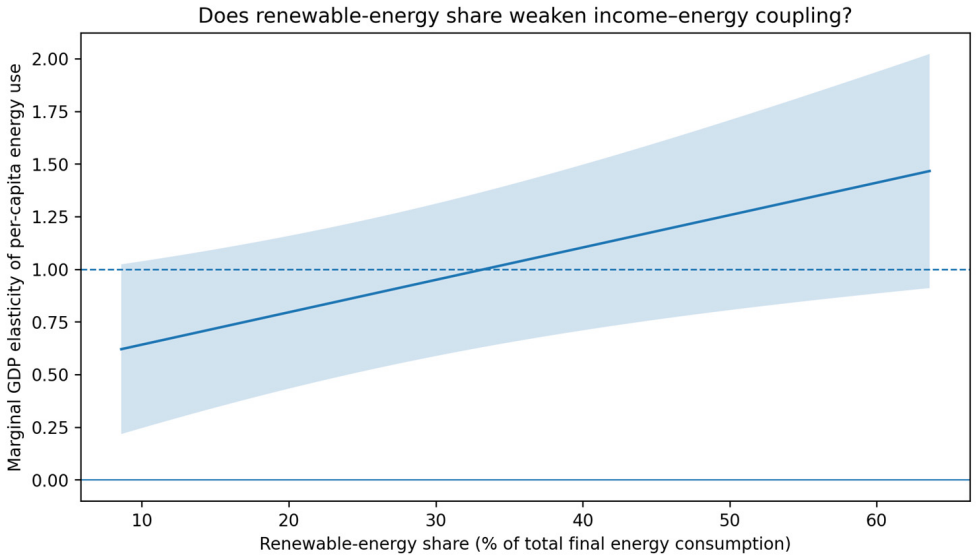

Figure 3. Marginal GDP elasticity across the observed renewable-energy share. Shaded area, 95% country-clustered confidence interval; dashed line, unit elasticity.

3.5. Official Energy Intensity and the Efficiency–Demand Distinction

The earlier constructed energy/GDP proxy is removed from the empirical evidence. Figure 4 instead uses the official WDI primary-energy-intensity indicator, EG.EGY.PRIM.PP.KD, for a constant four-country subset—Argentina, Brazil, Chile and Mexico—from 2013 to 2022 [37]. Using a constant subset avoids an artificial trend caused by changing country composition. The figure compares independently reported energy intensity with the same countries' GDP per capita and per-capita energy use.

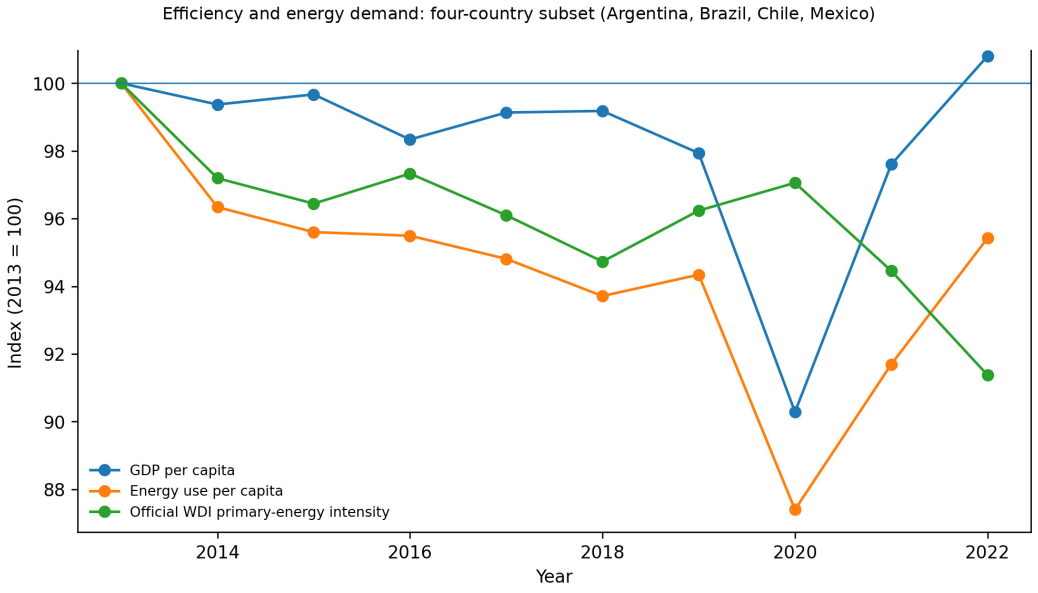

Figure 4. GDP per capita, per-capita energy use and official WDI primary-energy intensity, four-country subset (Argentina, Brazil, Chile and Mexico), 2013–2022; index 2013 = 100.

The official-intensity comparison illustrates distinct income, energy-use and intensity trajectories. In Figure 4, average per-capita energy use and official primary-energy intensity both end below their 2013 values, whereas GDP per capita ends slightly above its initial level. During 2021–2022, energy use per person rises while intensity falls, illustrating how these margins can diverge within the same subset. The figure does not independently establish a

conditional regression elasticity of unity, and it represents four countries rather than the full ten-country panel. Progress toward SDG 7.3 must therefore be assessed separately from the broader resource-efficiency objective under SDG 8.4; these indicators are not comprehensive measures of environmental degradation.

3.6. Robustness and Sensitivity

The GDP coefficient remains close to one when 2022 is removed, when the sample is restricted to the pre-pandemic period through 2019, and when Brazil and Mexico are removed individually or jointly. The 2022 observations therefore improve temporal currency but do not drive the core result: excluding 2022 yields $\beta = 1.001$ (SE = 0.273; cluster $p = 0.005$), essentially identical to the main estimate. This sensitivity is important because only Brazil and Mexico contribute complete six-variable observations in 2022.

With the baseline coefficient held at 1.001, the Driscoll–Kraay SE is 0.117, the year-clustered SE is 0.115 and the two-way country/year-clustered SE is 0.268 (Table 8). Each comparison retains evidence of a positive association against zero, but none establishes equivalence to unity. The lagged-GDP model gives $\beta GDP(t-1) = 0.826$ (country-clustered SE = 0.289; $p = 0.019$ against zero; 95% t9 CI [0.173, 1.479]). Its point estimate is lower, but the interval still contains unity, and the one-sided test for an elasticity below one yields $p = 0.281$. This different timing and sample should not be treated as a causal estimate or as proof of relative decoupling. These covariance comparisons use the same contemporaneous-income specification and 122 observations, so coefficient differences are not introduced by changing the error covariance alone. By contrast, the lagged-income specification uses 112 observations and a different timing assumption; its coefficient cannot be compared with the main estimate as a pure correction for simultaneity.

Table 8. Robustness of the GDP–energy association.

Check	N	GDP Coefficient	SE	*p*-Value Against Zero Unless Specified
Main 2010–2022 unbalanced	122	1.001	0.274	0.005
Exclude 2022	120	1.001	0.273	0.005
Pre-pandemic through 2019	100	0.931	0.277	0.008
Exclude Brazil	109	1.112	0.388	0.021
Exclude Mexico	109	1.066	0.220	0.001
Exclude Brazil and Mexico	96	1.004	0.205	0.002
Wild bootstrap H0: βGDP = 0 (enumerated)	122	1.001	0.274	0.023
Wild bootstrap H0: βGDP = 1 (enumerated)	122	1.001	0.274	1.000
Driscoll–Kraay (Bartlett; lag 2)	122	1.001	0.117	<0.001; t12, H0: β = 0
Year-clustered covariance	122	1.001	0.115	<0.001; t12, H0: β = 0
Country/year two-way clustering	122	1.001	0.268	0.005; t9, H0: β = 0
One-year lagged GDP; country clusters	112	0.826	0.289	0.019; t9, H0: β = 0
Observed-price same-sample baseline	30	1.069	0.384	Wild $p = 0.008$; β = 0
Add logged gasoline price	30	1.114	0.420	Wild $p = 0.010$; β = 0
Add logged diesel price	30	1.118	0.405	Wild $p = 0.010$; β = 0

In the observed-price subpanel, the same-sample baseline elasticity is 1.069 (SE = 0.384). Adding gasoline gives 1.114 (SE = 0.420; 95% CI [0.165, 2.063]); adding diesel gives 1.118 (SE = 0.405; 95% CI [0.201, 2.034]). Corresponding income tests against zero have enumerated *p*-values 0.010 in each price model (Table 8). The fuel-price coefficients are imprecise: gasoline, 0.039 (SE = 0.171; country-clustered $p = 0.824$); and diesel, 0.073 (SE = 0.196; $p = 0.720$). Their positive point estimates are not interpreted as upward-sloping causal

demand. The short, three-survey-year subpanel and nominal transport-fuel measurement restrict this check; it cannot show that omitted energy prices are harmless in the full panel.

4. Discussion

The regional regression shows a positive conditional income–energy association. Its point estimate is near unity, yet the confidence interval and failed equivalence test do not establish proportional coupling. Equally, the directional test does not establish a less-than-proportional response. The substantive conclusion is limited precision around a positive association, rather than proof that Latin America has or has not decoupled. Alternative covariance estimates preserve the positive baseline association, and the lagged-income model emphasizes uncertainty about the magnitude and timing of the relationship.

This result is compatible with Balza et al. [5], who report a long-run income elasticity near 0.9, but the present paper asks a different inferential question: whether the coefficient is demonstrably below unity. It is also compatible with Chen and Steinbuks [6] and Carvajal et al. [7], because reductions in energy intensity do not necessarily imply falling or less-than-proportional per-capita energy demand.

Tapio trajectories complement the regional model without identifying country-specific conditional elasticities. In 2010–2022, Mexico shows strong decoupling ($D = -0.533$); Colombia, weak decoupling (0.682); Chile, expansive coupling (0.810); Brazil, expansive negative decoupling (1.375); and Argentina, recessive coupling (1.149). These are observed endpoint growth ratios, whereas TWFE adjusts for controls and common annual shocks using within-country variation. In particular, recessive coupling describes simultaneous contraction rather than growth with energy-demand reduction. A common regression slope can coexist with different endpoint states; it does not demonstrate identical country responses. Differences in sector structure, renewable endowments, institutions and policy are plausible explanations [1–3,11,13–17], but those channels are not separately measured here. Pandemic endpoint sensitivity and small GDP changes can produce unstable Tapio ratios, so the classifications cannot establish which national policies caused decoupling.

The positive interaction point estimate is suggestive but does not support the negative moderation hypothesis at the stated significance level. Rebound, electrification and infrastructure expansion, or renewable deployment occurring alongside demand growth could generate such a pattern. However, the main panel lacks an annual real energy-service-price measure, sectoral electrification, infrastructure or rebound measurements to distinguish these explanations. Moreover, the renewable share includes all renewable final consumption rather than only new renewable electricity. Reverse causality and omitted factors can affect the interaction sign and magnitude. Failure to support H3 is not evidence that renewables are ineffective or that any one mechanism dominates. The 30-row same-sample mechanism gives an interaction of -0.00757 (SE = 0.01114; country-clustered $p = 0.514$), compared with -0.00749 (SE = 0.01182; $p = 0.542$) when gasoline is added. The sign reversal relative to the full panel already occurs before adding price. It should therefore be attributed to sensitivity to sample composition, not identified price-induced moderation. Neither subpanel estimate supplies significant evidence for the directional renewable-moderation claim.

From a sustainability perspective, energy-efficiency progress under SDG 7.3 and resource efficiency under SDG 8.4 require distinct monitoring. The present results neither establish demand decoupling nor rule it out at the regional regression level. SDGs 12 and 13 motivate considering demand management together with supply decarbonization, but the model does not estimate emissions or causal policy effectiveness.

Econometrically, exhaustive enumeration materially improves transparency with ten clusters. Because every possible Rademacher sign vector is evaluated, the reported p-values

do not depend on an arbitrary number of repeated bootstrap draws. Nevertheless, the panel remains short and unbalanced, so this paper maintains an associational interpretation.

4.1. Policy Implications

First, energy-efficiency policy should be evaluated not only by declines in MJ per unit of GDP but also by whether those gains reduce the income elasticity of per-capita energy demand. This distinction is central to aligning SDG 7.3 with SDG 8.4.

Second, renewable deployment can change the supply mix, while the present interaction estimate does not establish demand-reducing moderation. Grid modernization, storage, electrification and efficiency standards may be considered complementary policy instruments [1,2,7]. Their effects are not estimated here, and electrification can itself expand energy services; policy appraisal must distinguish total demand from fossil-fuel use and emissions.

Third, the Tapio states show that policy should be country-specific. Economies displaying strong or weak decoupling warrant investigation of the mechanisms involved; countries in expansive negative decoupling require attention to transport, industrial structure, technology and energy pricing.

Fourth, SDG 12 motivates evaluating energy-service demand alongside the fuel mix. Building codes, public transport, industrial energy management and efficient equipment are relevant policy options discussed in the regional energy-efficiency context [6,7]. This study supports monitoring both intensity and per-capita demand, but does not quantify the effectiveness or cost of these instruments.

Table 9 records national frameworks documented by the end of the estimation period. Chile's legal objective, long-term strategies and shorter-horizon NDC targets differ in instrument type and conditionality. They cannot be ranked from their percentages alone, because baselines, coverage, finance and legal status differ. None measures enforcement or establishes that the framework caused a Tapio state. Regional inference consequently remains distinct from national policy evaluation [19–25].

Table 9. Historical national climate-policy evidence and interpretation limits.

Country	Historical Documentary Evidence	Interpretation Boundary
Argentina	Long-term strategy submitted 6 November 2022 [19]	Strategy record; not an enforcement score
Bolivia	First NDC submitted 5 October 2016 [24]	Verified national submission; no inferred neutrality deadline
Brazil	NDC states long-term climate-neutrality objective for 2050 [25]	Commitment; not demonstrated compliance
Chile	Law 21455 establishes GHG neutrality by 2050 [20]	Statutory framework differs from a voluntary strategy
Colombia	E2050 strategy submitted 12 November 2021 [19]	Long-term planning; not a measured demand effect
Ecuador	NDC: 9% domestic or 20.9% conditional reduction by 2025 against BAU in specified sectors [21]	Conditional support and sector scope differ from economy-wide neutrality
Mexico	Mid-century strategy submitted 16 November 2016 [19]	Long-term planning; not proof of energy-demand decoupling
Paraguay	NDC: 20% BAU in 2030 reduction; 10% domestic plus 10% conditional [22]	Financial conditionality limits comparison of headline ambitions
Peru	Official NDC update announcement: 40% reduction by 2030; maximum 179 $MtCO_2eq$ [23]	Announced mitigation ambition; not an enforcement score
Uruguay	Long-term strategy submitted 28 December 2021 [19]	Long-term planning; demand need not follow supply decarbonization

4.2. Limitations and Future Research

The 2010–2022 main panel is unbalanced. Although all ten countries are observed through 2021, only Brazil and Mexico have complete six-variable observations in 2022. Year fixed effects therefore use limited cross-sectional information in the final year. This paper reports this explicitly and retains an exclude-2022 robustness estimate.

Official renewable-energy coverage is the binding constraint on a broader complete six-variable 2022 sample. Future releases should permit a more even extension. No current missing observation has been filled by interpolation or extrapolation.

The 2010–2022 Tapio classification is available only for countries with verified endpoint values in the supplied official source layer. The taxonomy is descriptive and should not be interpreted as causal evidence of policy effectiveness.

The renewable interaction measures conditional association, not a causal treatment effect. Observed national fuel prices now support a matched 30-row sensitivity, but full-period real energy-price coverage is not established. Year effects absorb common shocks, not national retail prices, subsidies or pass-through. Time-varying omitted prices can affect both income and interaction estimates; their bias cannot be signed from these data. The national policy inventory documents institutional context but does not create a comparable annual enforcement measure. Economic complexity and sector-specific electrification, infrastructure and rebound measurements are not included. The interpretation remains limited accordingly.

The instrument screening does not certify causal identification. The observed two-year lags satisfy a rank condition on 102 rows, but serially correlated disturbances and plausible persistent confounding make their exclusion uncertain. No 2SLS or GMM effect is presented as a causal result. Geographic endowments alone are time-invariant and absorbed by country fixed effects; weather interactions can directly influence energy supply, demand and production. Without a defensible exclusion argument, adding them would weaken rather than improve identification. With only ten countries, automatic Difference or System GMM adoption also risks unreliable large-N approximations and instrument proliferation. These are substantive identification limits, not a claim that an omitted instrument was successfully added.

Small-sample power is a central limitation. The ten country clusters provide few independent units for country-cluster inference, despite 122 country–year observations. Within-country persistence, the short time dimension and limited variation after removing fixed effects further restrict precision. These constraints help explain the wide country-clustered intervals and limit sensitivity to departures from unity. Equivalence is particularly demanding: both boundaries of the narrow $[0.9, 1.1]$ band must be rejected, whereas the conventional standard error of 0.274 exceeds the band half-width of 0.1. Enumerating every sign pattern makes the reported tail fractions deterministic, but it does not add information, increase the number of independent countries or guarantee adequate power [38,39]. Its p-value resolution $(1/1024)$ is distinct from statistical power. We do not estimate achieved power or attribute all uncertainty to cluster count. Failure to establish equivalence does not establish non-equivalence, just as failure to reject unity does not establish proportional coupling. Residual cross-country dependence also limits a country-cluster bootstrap despite common year effects; the alternative covariance estimates are sensitivities rather than a cure. Longer panels and more independent economies would help improve precision, subject to comparable measurements.

5. Conclusions

The ten-country panel identifies a positive conditional association between income and per-capita energy use, with a point elasticity of 1.001. The fully enumerated wild-

bootstrap TOST within [0.9, 1.1] yields $p = 0.386719$, so practical equivalence to unity is not established. The enumerated directional test also fails to establish relative decoupling. These findings reflect uncertainty about the economic classification and should not be read as proof of either coupling or its absence. Only ten country clusters, a short unbalanced panel, and persistent disturbances restrict test sensitivity. Sparse final-year coverage does not drive the point estimate; covariance, lagged-income and observed-price sensitivities preserve positive but differently precise associations. Tapio trajectories remain country- and period-specific, and official intensity comparisons separate efficiency from demand. Renewable-share moderation remains unsupported directionally and cannot be interpreted causally. Monitoring efficiency, total demand and supply decarbonization separately offers a more defensible assessment of sustainability than transferring conclusions between these outcomes. More precise and better identified evidence is required to attribute national trajectories to specific policies.

Supplementary Materials: The following supporting information can be downloaded at: https://www.mdpi.com/article/10.3390/su182110955/s1, the replication package includes the canonical observed panel, provenance workbook, input data, executable Python scripts, TOST boundary distributions, source documentation, and files for recreating Figures 1–4. The repository record is https://doi.org/10.6084/m9.figshare.33471220; the updated Round 2 archive accompanies the revised submission.

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Abbreviations

GDP, gross domestic product; LAC, Latin America and the Caribbean; TWFE, two-way fixed effects; WDI, World Development Indicators; SDG, Sustainable Development Goal; UTPL, Universidad Técnica Particular de Loja; UIDE, Universidad Internacional del Ecuador.

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