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Industrial Robot Import Intensity and Carbon Performance: Country-Composition Heterogeneity in OECD Countries (2010–2021)

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Abstract. This study examines the conditional associations between industrial robot import intensity and carbon performance in OECD countries during 2010–2021. Robot import intensity is measured as the logarithm of HS 847950 robot import values relative to manufacturing value added; because the equal-and-opposite numerator–denominator restriction is rejected, additional models enter both components separately. Two-way fixed-effects models with country-clustered and wild-cluster bootstrap inference are estimated for a core cohort of 34 countries that had joined the OECD by 2010 and a retrospective expanded panel of 38 countries. The ratio-based robot coefficient is insignificant in the core sample ($\beta = 0.0046$, $p = 0.657$) but positive in the expanded sample ($\beta = 0.0251$, $p = 0.009$). In an unrestricted model, the additional robot import slope for the four later entrants is positive ($\beta = 0.0613$; country-clustered $p = 0.005$; wild-bootstrap $p = 0.016$). Matched-country models based on 2010 economic characteristics yield similarly positive slope differentials ($\beta = 0.0565$–0.0588), but their wild-bootstrap p-values (0.179 and 0.116) do not establish conventional statistical significance. The level association with energy intensity is not corroborated in first differences, and the environmental policy stringency analysis excludes the four late entrants. These findings indicate sample-composition sensitivity and heterogeneous conditional associations rather than causal effects, established energy-use mechanisms, or directly measured sustainable industrial transformation.

Keywords: Carbon intensity, CO_2 emissions, energy intensity, industrial robots, OECD

1 Introduction

The climate crisis and the rapid transformation of production technologies are unfolding simultaneously. Although industrial robots are regarded as a technology capable of improving environmental performance through productivity gains, process standardization, energy management, and factor allocation, their potential to expand production scale, increase electricity demand, and encourage the use of energy-intensive capital suggests that the same technology may also generate environmental costs. Therefore, the direction of the relationship between robotization and decarbonization cannot be determined solely by the presence of technology adoption. While some recent studies have demonstrated that robot adoption reduces carbon intensity or emissions [1]–[4], others have reported increases in total emissions or identified conditional effects [5]–[7].

These divergent findings highlight three methodological issues. First, the measurement of robotization varies across studies. Research based on International Federation of Robotics (IFR) data employs stock and flow indicators, including robot installations, operational stock, and robot density per employee. In contrast, import-based studies track robot acquisition via international trade records [8]. Second, the choice of environmental outcome matters. A reduction in carbon intensity does not necessarily imply a corresponding reduction in total emissions, a distinction that is particularly relevant to rebound and scale effects [6], [9]. Third, the environmental consequences of robotization may depend on complementary conditions such as the energy mix, institutional quality, and environmental policy [7], [10], [11].

This study contributes to the literature in three ways. First, it distinguishes nominal trade-based robot acquisition from operational robot adoption and tests the sensitivity of the results to the construction of the import-to-manufacturing-value-added ratio, including an unrestricted numerator–denominator decomposition. Second, it separates the 34-country core OECD cohort from the retrospective panel of 38 countries that were members by 2021 and estimates explicit slope differences for the four later entrants. Third, it supplements country-specific trends, common-factor checks, leave-one-country-out diagnostics, and wild-cluster inference with economically matched comparison countries and matching-frequency weights. This design evaluates

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whether the observed associations depend on the country set and the estimation procedure; it does not directly identify the effects of income, energy systems, or industrial restructuring as causal mechanisms.

The results reject a uniform interpretation of the robot–carbon association. The ratio-based coefficient is near zero in the core cohort ($\beta = 0.0046$, $p = 0.657$), positive in the 38-country retrospective panel ($\beta = 0.0251$, $p = 0.009$), and insignificant in the membership-consistent sample ($\beta = 0.0104$, $p = 0.327$). In a model that separately includes robot import values and manufacturing value added, the additional import slope for the four late entrants is 0.0613 (country-clustered $p = 0.005$; wild-bootstrap $p = 0.016$). Its magnitude remains positive in matched samples, but statistical significance is not robust to wild-cluster inference with only 10 country clusters. These findings motivate a distinction between the stability of the estimated sign and the stability of statistical precision. The study therefore treats country-group heterogeneity as a descriptive pattern that could reflect observed and unobserved structural conditions rather than an effect of OECD accession.

The remainder of the article is organized as follows. Section 1.1 reviews the literature on robotization, carbon intensity, energy use, rebound effects, and environmental policy and develops the research hypotheses. Section 2 describes the data, variables, and econometric methodology. Section 3 presents the empirical findings and robustness analyses and discusses them in relation to the existing literature. Finally, Section 4 summarizes the main conclusions, outlines the policy implications, acknowledges the study's limitations, and identifies directions for future research.

1.1 Literature Review, Theoretical Expectations, and Hypotheses

A substantial proportion of cross-country studies conclude that industrial robots improve carbon performance. Li et al. [1] reported that robot applications reduce carbon intensity across 35 countries, with the effects operating through green total factor productivity and energy intensity. Yao et al. [3] reported that robot adoption reduces CO_2 emissions in an OECD–BRICS sample, particularly in higher-emission quantiles and advanced economies. Jin [4] reached a similar conclusion for consumption-based embodied carbon intensity, whereas Long et al. [12] demonstrated that robots support low-carbon green performance in countries participating in the Belt and Road Initiative.

Energy efficiency and technological transformation are frequently cited mechanisms underlying the favorable environmental outcomes of robotization. Wang, Lee, and Li [13] reported that robots improve manufacturing energy intensity across 38 countries and 17 manufacturing sectors. Lin and Xu [14] emphasized technological innovation and electrification, whereas Chen et al. [15] reported that artificial intelligence applications can reduce carbon emission intensity in Chinese cities. Zhou et al. [16] also provided evidence on energy-efficiency and green-technology channels. Studies of Chinese cities and firms generally identify green innovation, human capital, cleaner production, and emission-reduction investment as important channels [2], [17]–[19].

Nevertheless, the findings are not uniformly favorable. Zhang and Zhu [5] identified positive associations between robot installations and density and total carbon emissions in the United States, China, and Japan. Sharfaei et al. [7] showed that although a more stringent EU climate policy accelerates robotization, the reconfiguration of value chains does not automatically yield lower CO_2 emissions. These results caution against technological determinism: robotization can transform production processes, but environmental outcomes may deviate from expectations when the energy mix and the scale of production are unfavorable.

Firm- and regional-level research also yields heterogeneous results. Li and Tian [20], Zhang and Shen [21], Han et al. [22], Qi et al. [8], Xu et al. [18], and Zhang et al. [23] identified various mechanisms through which robot use can reduce emissions or emission intensity. However, much of this evidence is drawn from Chinese microeconomic or subnational datasets. Given the greater diversity in energy structures, institutional capacity, and industrial composition across OECD countries, a common country-level coefficient should not be assumed to be universal.

Increases in productivity and energy efficiency driven by robots do not guarantee a decline in aggregate energy consumption or output. Wang et al. [6] reported that robots can generate an energy rebound effect in Chinese cities even while reducing total carbon emissions. Zhao and Liu [9] reported that the reduction in total CO_2 attributable to robots may be apparent in the short and medium term but weaker in the long term, whereas the carbon-intensity effect is more persistent. This distinction warrants examining carbon intensity and total emissions in separate specifications.

Evidence of nonlinearity is also accumulating. Using quadratic and threshold models, Liu et al. [24] demonstrated that the direction of the robot–emissions relationship may change with the level of development. Li, Zhang, and Wu [11] documented a U-shaped relationship for the carbon intensity embodied in manufacturing exports, whereas Lin et al. [25] identified a nonlinear pattern between robotic manufacturing

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development and urban emissions. Spatial dependence and spillovers also matter: Nie et al. [26] found that robot adoption can produce carbon-reducing effects in neighboring regions.

Institutional and policy contexts constitute additional dimensions that may alter the environmental consequences of robots. Wang, Li, and Li [10] identified threshold patterns associated with natural-resource rents and the control of corruption. Li, Zhang, and Wu [11] reported that environmental regulation can strengthen robots' carbon-reducing effects. Hashmi and Alam [27] established dynamic relationships among environmental regulation, innovation, and emissions in OECD countries, whereas Tao et al. [28] emphasized nonlinear relationships between OECD carbon intensity and financial development and information and communication technologies. These studies support treating environmental policy not merely as a control variable but also as a complementary institutional condition shaping the environmental outcomes of technological adoption.

The OECD carbon-intensity literature suggests that per capita income, industrialization, renewable energy, trade openness, and institutional and policy indicators should not be omitted from the model. Pan et al. [29] demonstrated cross-country variation in the determinants of carbon intensity and highlighted economic scale and industrial structure. Doğan et al. [30] emphasized the emission-reducing role of economic complexity and renewable energy in developed countries. Hashmi and Alam [27] explicitly addressed heteroskedasticity, serial correlation, and cross-sectional dependence in an OECD panel by applying Driscoll–Kraay corrections. These studies motivate the inclusion of the renewable energy share, income per capita, manufacturing share, trade openness, and regulatory quality as controls.

In terms of robot measurement, this study differs from IFR stock-based approaches. Qi et al. [8] provided an important example of using robot imports as an indicator of adoption at the firm level. Nevertheless, annual import flows may be more volatile than stocks at the country level. Accordingly, in addition to annual robot import intensity, this study employs a consistent cumulative measure that divides total robot imports over three years by total manufacturing value added over the same three years. This allows the sensitivity of the findings to the measurement horizon to be assessed directly.

The literature indicates that the relationship between robotization and carbon performance may vary with energy efficiency, production scale, economic structure, and institutional conditions. Against this background, the first hypothesis is formulated as follows:

> *H1:* An increase in industrial robot acquisition/import intensity reduces carbon intensity in OECD countries.

Carbon intensity and total CO_2 emissions need not move in the same direction. Rather than stipulating a rebound hypothesis with a predetermined sign, the following research question is posed:

> *RQ1:* Does the relationship between robot acquisition/import intensity and total CO_2 emissions differ from that observed for carbon intensity in direction, magnitude, and statistical robustness?
>
> *H2:* An increase in robot acquisition intensity reduces energy intensity.
>
> *RQ2 (exploratory):* Within the OECD core-country subsample for which environmental policy stringency (EPS) is observed, does EPS moderate the conditional relationship between robot import intensity and carbon intensity?

A negative Robot × EPS interaction would be consistent with the proposed carbon-reducing complementarity, but the restricted data do not permit a test of this proposition for the four late entrants. No separate directional hypothesis is formulated concerning the potential nonlinearity of the robot–carbon relationship. Instead, a quadratic specification is employed as an exploratory robustness analysis in light of the U-shaped, inverted U-shaped, and threshold relationships reported in the literature.

2 Methodology

2.1 Sample and Data Sources

The analysis uses two complementary sample definitions. The reference sample is a core cohort of 34 countries that had joined the OECD by the end of 2010, observed from 2010 to 2021 (408 country-year observations). The expanded sample is a balanced retrospective panel covering the same period for all 38 countries that were OECD members as of 2021 (456 country-year observations). Latvia, Lithuania, Colombia, and Costa Rica joined in 2016, 2018, 2020, and 2021, respectively; therefore, the expanded panel includes preaccession observations for these four countries [31]. It is thus interpreted as a retrospective panel of the 2021 OECD country group, not as an effect specific to OECD membership periods. A further membership-consistent sample that retains only membership-year observations for the four late entrants comprises 421 observations.

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Industrial robot imports were retrieved from UN Comtrade under HS 847950 via import flows and the world trading partner. HS 847950 captures cross-border acquisition within this trade category, not overall robot use or operational robot stock. Because the HS 2022 revision also introduced a new industrial robot subheading under 8428.70, using HS 847950 alone provides a more limited representation of broader robot imports after 2022. Carbon, energy, and macroeconomic indicators were obtained from the World Bank, and EPS data were obtained from the OECD.

The primary robot indicator is the natural logarithm of the ratio of current HS 847950 robot imports to current manufacturing value added. Both components are positive for all 456 observations, so no constant was added, and no imputation was used for the logarithmic transformation. Additional models include the numerator and denominator as separate logarithmic regressors. The alternative three-year indicator divides the sum of robot imports in years t, t − 1, and t − 2 by the sum of manufacturing value added over the same three years. EPS analyses use 374 observations for 34 countries from 2010 to 2020. Import values are nominal monetary flows rather than counts of installed robots; they can respond to robot prices, product composition, trade routing, and exchange-rate valuation. The ratio is consequently an acquisition-intensity proxy, not an estimate of operational robot density. Because the coefficient restriction implied by a single logged ratio is rejected in the reported data ($F = 12.37$, $p = 0.0012$), unrestricted specifications with separate ln(imports) and ln(manufacturing value added) regressors are used as a key sensitivity analysis. The variables, sources, and descriptive statistics are summarized in Table 1.

Table 1. Variables, data sources, and descriptive statistics.

Variable	Definition/source	N	Mean	SD	Min	Max
ln carbon intensity	Log of carbon intensity (kg CO_2e per constant 2015 US dollar of GDP); World Bank EN.GHG.CO2.RT.GDP.KD	456	−1.426	0.567	−3.046	0.130
ln robot ratio	Log of HS 847950 robot imports/current manufacturing value added; UN Comtrade + World Bank NV.IND.MANF.CD	456	−7.696	1.066	−11.114	−3.802
ln total CO_2	Log of total CO_2 emissions excluding LULUCF (Mt CO_2e); World Bank EN.GHG.CO2.MT.CE.AR5	456	4.510	1.577	1.122	8.616
ln energy intensity	Log of energy intensity; World Bank EG.EGY.PRIM.PP.KD	456	1.262	0.390	0.086	2.750
Renewable energy (%)	Share of renewables in final energy consumption; EG.FEC.RNEW.ZS	456	22.257	16.195	1.300	82.900
ln GDP per capita	Log of real GDP per capita (constant 2015 USD); NY.GDP.PCAP.KD	456	10.279	0.682	8.559	11.616
Manufacturing share (%)	Manufacturing value added/GDP; NV.IND.MANF.ZS	456	14.178	5.425	4.554	34.891
Trade openness (%)	Trade in goods and services/GDP; NE.TRD.GNFS.ZS	456	100.155	61.215	23.140	397.506
Regulatory quality	WGI Regulatory Quality estimate; GOV_WGI_RQ_EST	456	1.195	0.544	−0.191	2.080
EPS	OECD Environmental Policy Stringency (0–6)	374	2.913	0.912	0.556	4.889
ln population	Log of total population; SP.POP.TOTL	456	16.381	1.515	12.670	19.621
ln three-year robot ratio	Log of Σ robot imports (t, t − 1, t − 2)/Σ manufacturing VA (t, t − 1, t − 2)	380	−7.631	1.010	−10.927	−4.065

Note. Descriptive statistics refer to the 2010–2021 retrospective panel of 38 countries that were OECD members as of 2021. The reference core cohort of 34 countries that had joined by the end of 2010 is reported separately in the main analysis. Because the EPS sample excludes the four late-accession countries, it is restricted to 34 countries from 2010 to 2020. The robot indicator is a proxy for import/acquisition intensity normalized by manufacturing scale, not the operational robot stock.

2.2 Econometric Strategy

The overall research design, from data sources and sample definitions to the estimated outcomes and robustness checks, is summarized in Figure 1. The baseline specification uses two-way fixed effects to control for time-invariant unobserved country characteristics and annual shocks common to all countries. The carbon-intensity equation is specified in Equation (1).

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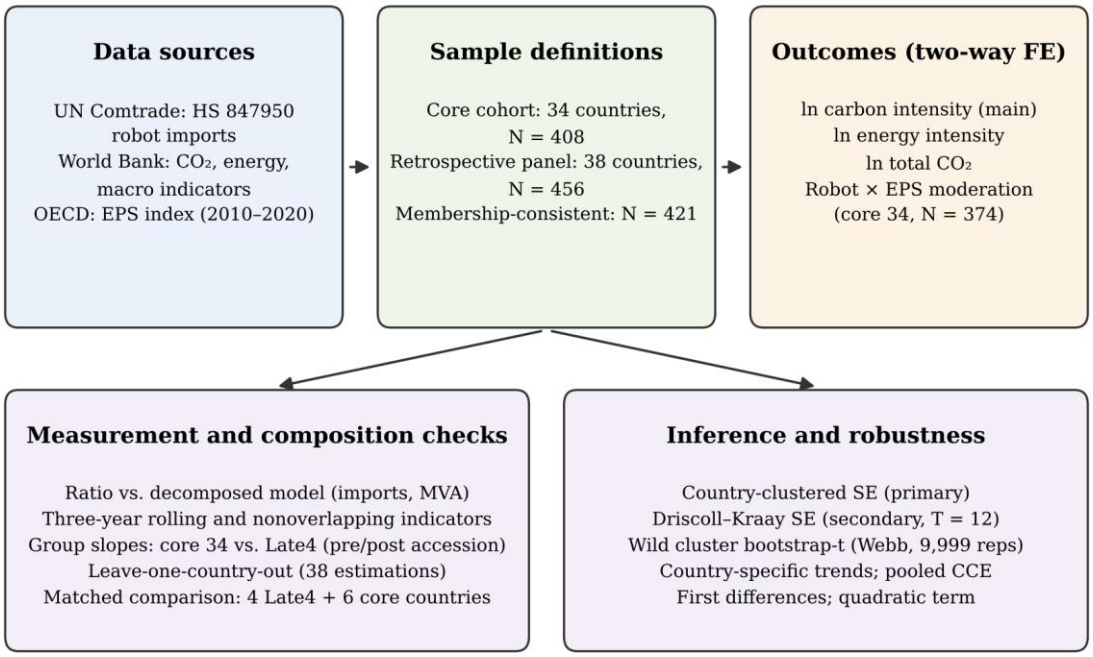


Figure 1. Research design and analytical framework.

$$\ln(CI_{it}) = \beta_1 \ln(Robot_{it}) + \beta_2 Renewable_{it} + \beta_3 \ln(GDPpc_{it}) + \beta_4 Manufacturing_{it} + \beta_5 Trade_{it} + \beta_6 RegulatoryQuality_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \quad (1)$$

Here, α_i denotes country fixed effects, and λ_t denotes year fixed effects. Energy intensity is initially excluded from the baseline carbon-intensity equation and examined as a separate outcome in Equation (2). The inclusion of energy intensity in the carbon equation serves solely as a complementary coefficient-sensitivity analysis; it is not interpreted as a formal or causal mediation test.

$$\ln(EnergyIntensity_{it}) = \gamma_1 \ln(Robot_{it}) + \gamma_2 X_{it} + \alpha_i + \lambda_t + \upsilon_{it} \quad (2)$$

The total CO_2 model in Equation (3) parallels the carbon-intensity specification. To control for country scale, the annual level model includes real GDP per capita and population jointly; the first-difference robustness analysis employs an aggregate real economic scale, defined as ln(real GDP) = ln(GDP per capita) + ln(population).

$$\ln(CO_{2,it}) = \delta_1 \ln(Robot_{it}) + \delta_2 X_{it} + \alpha_i + \lambda_t + \mu_{it} \quad (3)$$

The environmental policy specification in Equation (4) is an exploratory moderation analysis restricted to the 34-country core cohort observed in the EPS series (374 country-year observations, 2010–2020). Its estimated interaction describes within-sample conditional variation only and cannot be generalized to the four later entrants.

$$\ln(CI_{it}) = \theta_1 \ln(Robot_{it}) + \theta_2 EPS_{it} + \theta_3 [\ln(Robot_{it}) \times EPS_{it}] + \theta_4 X_{it} + \alpha_i + \lambda_t + \xi_{it} \quad (4)$$

Pooled ordinary least squares (OLS), fixed effects (FE), and random effects (RE) are compared for model selection. The hypothesis that all country effects are jointly zero is strongly rejected in the FE model, and the Breusch–Pagan Lagrange multiplier test also rejects pooled OLS. The conventional Hausman test is not considered a reliable model-selection criterion because the difference in variance matrices is not positive definite and a rank warning is generated. Mundlak correlated random-effects specifications are applied instead, and the null hypothesis of joint insignificance of the country means cannot be rejected. Nevertheless, FE is retained as the main estimator because the research question concerns within-country change, and the specification conservatively absorbs time-invariant country heterogeneity. RE findings are therefore not treated as the decisive basis for model selection. The panel diagnostics are summarized in Table 2.

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Error-structure diagnostics point to heteroskedasticity and serial correlation. Consequently, primary statistical inference relies on country-clustered standard errors. Driscoll–Kraay (DK) estimates are also reported, but, given T = 12, they are treated as secondary robustness checks. The sensitivity of the main robot coefficient to the number of clusters is additionally assessed with a wild cluster bootstrap-t test using 38 country clusters, Webb weights, and 9,999 replications. Inference is evaluated along two distinct dimensions: whether coefficient signs remain similar across reasonable models and whether estimated effects remain statistically distinguishable from zero. A positive coefficient under common correlated effects (CCE) or matched-sample estimation is not by itself evidence of stable statistical precision; the treatment-like Late4 subgroup also contains only four country clusters.

Country-specific linear trends and a parsimonious pooled CCE specification are used to address cross-sectional dependence and common shocks. Three additional designs assess sample composition: (i) 38 leave-one-country-out estimations, successively excluding each country; (ii) a membership-consistent panel retaining only the OECD membership years of the four late entrants; and (iii) a group-interaction model estimating separate robot slopes for the core 34 countries, the preaccession years of late entrants, and their postaccession years. Because group intercept indicators are collinear with country fixed effects, interpretation concerns differences in robot slopes rather than group levels. These exercises identify country-composition heterogeneity and do not estimate causal effects of OECD membership.

An additional unrestricted slope specification includes ln(robot import value), ln(manufacturing value added), the interactions of each term with the Late4 indicator, the established controls, and country and year fixed effects. The Late4 × ln(imports) coefficient is therefore a conditional group difference in the import-value slope rather than the coefficient on the original import-to-value-added ratio. We retain both estimands and do not treat their coefficients or bootstrap p-values as directly interchangeable.

For the matched-country sensitivity exercise, baseline log GDP per capita and manufacturing value added as a share of GDP were standardized using the 2010 cross-country distribution. The three nearest core-country neighbors of each of the four late entrants were identified by squared Euclidean distance, allowing reuse of comparison countries. The 12 pairwise matches yield six unique core countries: Türkiye, Poland, Mexico, Estonia, Chile, and Hungary. The unweighted specification includes each unique country once; the weighted specification assigns comparison countries weights equal to their matching frequencies (three, three, two, two, one, and one, respectively) and a weight of three to each late entrant, balancing the total group weights. Matching is used only for a sensitivity comparison, not as causal treatment assignment or evidence of exact covariate balance.

Slope-difference hypotheses in both full and matched samples are tested using country-clustered standard errors and null-imposed wild cluster bootstrap-t tests with 9,999 replications and Webb weights. As the matched design includes only ten countries, with four late entrants, bootstrap-based p-values are interpreted cautiously. Baseline balance is inspected separately for the matching characteristics. The available models do not estimate robot interactions with each proposed structural characteristic separately; matching can assess sample-composition sensitivity but cannot attribute the residual Late4 difference to particular structural mechanisms.

Table 2. Panel diagnostics, multicollinearity, and summary of CIPS results.

Test/variable	Statistic	p-value/result	Interpretation
Maximum VIF	3.01	—	No serious multicollinearity
Mean VIF	1.90	—	No serious multicollinearity
FE: all country effects = 0	$F(37, 401) = 738.42$	< 0.001	Pooled OLS rejected
Breusch–Pagan LM	$\bar{\chi}^2 = 2364.30$	< 0.001	Panel effects are present
Mundlak joint means	$\chi^2(6) = 7.11$	0.3105	RE assumption cannot be rejected; FE is the conservative baseline
Modified Wald	$\chi^2(38) = 2584.47$	< 0.001	Groupwise heteroskedasticity
Wooldridge AR(1)	$F(1, 37) = 80.004$	< 0.001	Serial correlation
Pesaran CD in FE residuals	−1.71	0.088	No strong residual CD evidence at the 5% level
CDw+ in FE residuals	736.35	< 0.001	Enhanced test signals common dependence
ln robot ratio, CIPS	Stationary at levels	p < 0.01	Evidence for I(0)
Renewable energy, CIPS	Stationary at levels	p < 0.01	Evidence for I(0)
ln carbon intensity, CIPS	Specification-sensitive	First difference p < 0.001	Borderline/I(1); robustness required

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Test/variable	Statistic	p-value/result	Interpretation
ln GDP per capita, CIPS	Level sensitive to trend	First difference p < 0.001	Evidence for I(1)
Manufacturing share, CIPS	Nonstationary at levels	First difference p < 0.001 (lag 0)	Evidence for I(1)
Trade openness, CIPS	Nonstationary at levels	First difference p < 0.001 (lag 0)	Evidence for I(1)
Regulatory quality, CIPS	Nonstationary at levels	First difference p = 0.009	Evidence for I(1)
ln energy intensity, CIPS	Nonstationary at levels	First difference p < 0.001	Evidence for I(1)
ln total CO_2, CIPS	Nonstationary at levels	First difference p = 0.001	Evidence for I(1)

Note. CIPS = cross-sectionally augmented Im–Pesaran–Shin panel unit root test. Given the short time dimension, the CIPS results are interpreted cautiously because they depend on lag and trend specifications. Classification draws on the combined assessment of levels and first-difference sensitivity checks rather than any single test result.

3 Results and Discussion

3.1 Descriptive Findings and Preliminary Tests

The untransformed robot import intensity measure is strongly right-skewed (skewness = 8.26; kurtosis = 88.51). The highest observations occur primarily in Luxembourg and some Central European manufacturing economies. The logarithmic transformation substantially improves the distribution: the skewness and kurtosis of the log robot ratio are −0.226 and 4.246, respectively. The logarithmic transformation also considerably improves the distribution of carbon intensity. Accordingly, the baseline models use logarithmic robot and carbon measures.

In the correlation matrix, the correlation coefficient between the robot measure and trade openness is 0.520, whereas that between per capita income and regulatory quality is 0.794. Nevertheless, the maximum variance inflation factor (VIF) is 3.01 and the mean VIF is 1.90, suggesting that there is no serious multicollinearity (Table 2). Panel diagnostics indicate substantial heteroskedasticity and serial correlation, whereas cross-sectional dependence is widespread in the raw series. These findings motivate the use of robust standard errors.

3.2 Main Results: Core Sample and Country-Composition Heterogeneity

Table 3 shows that the definition of the sample substantially changes the main robot coefficient. For the core cohort of 34 countries that had joined the OECD by the end of 2010, the coefficient of robot import intensity is 0.0046 and statistically insignificant (SE = 0.0102, p = 0.657, N = 408). In contrast, the retrospective 2010–2021 panel of 38 countries that were members as of 2021 yields a positive and significant coefficient of 0.0251 (SE = 0.0090, p = 0.009, N = 456). When only the OECD membership years are retained for the four late entrants, the common coefficient decreases to 0.0104 and becomes insignificant (p = 0.327, N = 421). The positive average coefficient in the 38-country panel therefore cannot be interpreted as a homogeneous OECD-wide association.

The group-specific slope model further clarifies the source of this sensitivity to sample composition. The robot slope for the 34 core countries is 0.0031 (p = 0.752). For Latvia, Lithuania, Colombia, and Costa Rica, the total robot slope is 0.0657 (p < 0.001) during the preaccession period and 0.0704 (p < 0.001) after accession. The difference between these late-entrant slopes is only 0.0047 and is statistically insignificant (F(1, 37) = 0.17, p = 0.680). Hence, the stronger positive slope is not confined to preaccession observations and cannot be attributed to a break induced by OECD accession. Figure 2 visualizes these coefficients and their 95% confidence intervals.

This finding indicates that H1 is not supported in the core OECD cohort. The positive coefficient in the expanded panel must instead be read alongside the different slopes of the four late entrants relative to the core group. Although renewable energy and per capita income are negatively associated with carbon intensity in the expanded model, the central interpretation is based on sample and group heterogeneity rather than a single pooled OECD coefficient.

Table 3. Carbon intensity: sample definition and group-specific slope heterogeneity.

Sample/specification	Robot β	SE	p	N	Interpretation

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Sample/specification	Robot β	SE	p	N	Interpretation
Core 34 countries, 2010–2021	0.0046	0.0102	0.657	408	Reference; no significant association
Expanded 38 countries, 2010–2021	0.0251	0.0090	0.009	456	Positive; composition-sensitive
Membership-consistent 38 countries	0.0104	0.0105	0.327	421	Pooled coefficient insignificant
Group slope: core 34	0.0031	0.0096	0.752	456	Conditional slope
Group slope: Late4 preaccession	0.0657	0.0148	< 0.001	456	Strong positive slope
Group slope: Late4 postaccession	0.0704	0.0180	< 0.001	456	Strong positive slope
Late4 post- minus preaccession slope	0.0047	0.0114	0.680	456	No significant difference

Note. All models include country and year fixed effects with country-clustered standard errors. The group-specific slope rows are conditional slopes from the same interaction model with 456 observations. The late entrants are Latvia, Lithuania, Colombia, and Costa Rica. The membership-consistent sample retains only their OECD membership years. The coefficients are not interpreted as causal effects of OECD membership.

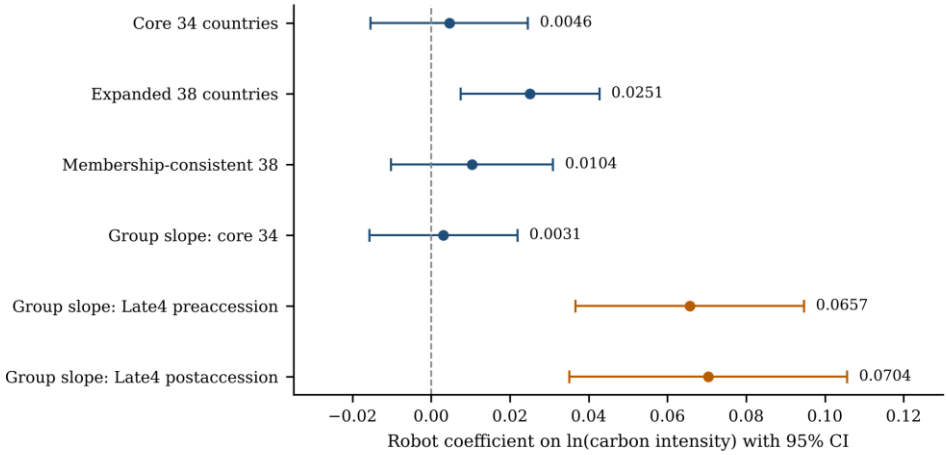


Figure 2. Robot coefficients on ln(carbon intensity) across sample definitions and group-specific slopes.

Note. Points are the estimates reported in Table 3; horizontal lines are 95% confidence intervals computed as β ± 1.96 × SE from country-clustered standard errors. The dashed vertical line marks zero.

3.3 Leave-One-Country-Out Analysis, Sample Sensitivity, and Robustness Checks

The leave-one-country-out analysis indicates that the positive coefficient in the expanded 38-country panel is not driven by any single country (Figure 3). As each country is excluded in turn, the robot coefficient remains positive, ranging from 0.0177 to 0.0290, and is significant at the 5% level in 37 of the 38 estimates. Only the exclusion of Lithuania yields β = 0.0177 with p = 0.052. Individually excluding Colombia, Costa Rica, Latvia, or Lithuania does not eliminate the positive coefficient; however, excluding all four countries jointly reduces it to 0.0046. This pattern points to systematic country-group composition rather than a single outlier.

Accession timing is examined separately for the four late entrants. In the original group-interaction model, pre- and postaccession robot slopes are 0.0657 and 0.0704, respectively, with an insignificant difference (p = 0.680). Because the later entrants provide only 13 postaccession country-year observations in total, this test has limited precision. Failure to reject a break at accession does not demonstrate that the two periods are equivalent. The Late4 designation is therefore a descriptive country-group indicator rather than evidence of a membership-induced change or an identified structural mechanism.

The results are also robust to alternative measurement horizons. With the three-year rolling robot indicator, the FE coefficient is 0.0334 (p = 0.038), and the first-difference coefficient is 0.0235 (p = 0.042). For the four nonoverlapping three-year blocks, the coefficient is 0.0390 (p = 0.025). When robot imports and manufacturing value added are modeled as separate logarithmic variables, the coefficient on robot imports remains positive (β = 0.0188, p = 0.048), but the ratio restriction is rejected (F = 12.37, p = 0.0012). The robot ratio should therefore be interpreted as acquisition intensity normalized by manufacturing scale rather than a pure robotization effect.

Robustness must be assessed separately in terms of coefficient sign and statistical precision. In the original expanded-panel ratio specification, the robot coefficient remains positive under country-specific linear trends (β

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= 0.0152, p = 0.006), but the parsimonious pooled CCE estimate weakens to β = 0.0131 (p = 0.065). The ratio-model wild-cluster bootstrap p-value is 0.046, close to the conventional 5% threshold. These results support positive-sign persistence but not uniformly strong statistical precision when common factors and small-cluster inference are considered. They are separate from the bootstrap tests of the Late4 × ln(imports) slope in the unrestricted decomposition. Table 4 summarizes these sample-composition and robustness analyses.

Table 4. Sample composition and robustness analyses.

Specification	Coefficient/statistic	SE/test	p	N	Interpretation
Panel A. Sample composition					
38 countries, 2010–2020	0.0243	0.0087	0.009	418	Persists when 2021 is excluded
Core 34, 2010–2020	0.0075	0.0104	0.476	374	Not significant
Leave-one-country-out	β range 0.0177–0.0290	38 estimates	37/38 < 0.05	444	No single country drives result
Late4 × robot: additional slope	0.0743	0.0234	0.003	456	Different from core cohort
Late4 × robot: wild bootstrap	t = 3.174	Webb, 9,999	0.0105	456	Slope difference persists
Late4 post- minus preaccession	0.0047	0.0114	0.680	456	No accession-related break
Panel B. Measurement and econometric robustness					
Three-year rolling FE	0.0334	0.0155	0.038	380	Positive
Three-year rolling first difference	0.0235	0.0112	0.042	342	Positive
Three-year nonoverlapping blocks	0.0390	0.0166	0.025	152	Robust to overlap
ln robot imports, decomposed model	0.0188	0.0092	0.048	456	Positive
ln manufacturing value added, same model	−0.2661	0.0716	0.001	456	Denominator also matters
Ratio restriction test	F = 12.37	—	0.0012	456	Equal-and-opposite restriction rejected
Country-specific linear trends	0.0152	0.0052	0.006	456	Expanded panel
Parsimonious pooled CCE	0.0131	0.0071	0.065	456	Precision weakens
38-country wild cluster bootstrap	0.0251	t = 2.778	0.046	456	Borderline significant at 5%

Note. FE models include country and year fixed effects with country-clustered standard errors. The leave-one-country-out summary is based on 38 separate estimates; all country-level coefficients are displayed in Figure 3. Late4 comprises Latvia, Lithuania, Colombia, and Costa Rica, which joined the OECD after 2010. Wild bootstrap inference uses 38 clusters, Webb weights, and 9,999 replications. The CCE and wild-bootstrap rows test the pooled logged robot ratio; the decomposed Late4 slope-difference results, which address a different estimand, appear separately in Table 5.

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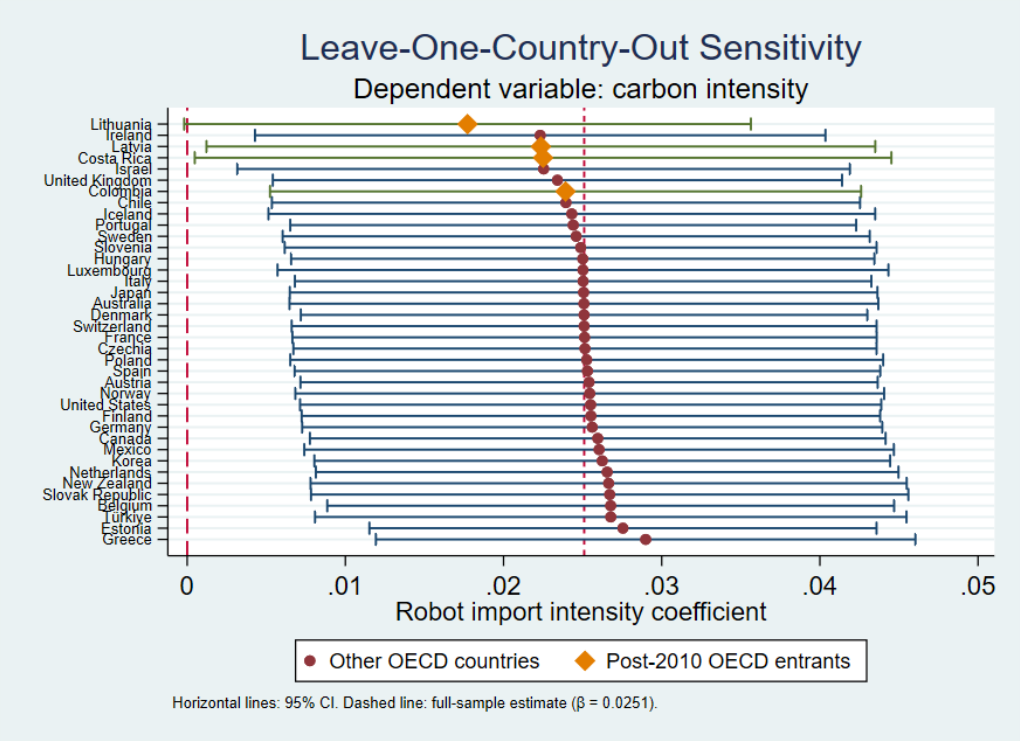


Figure 3. Leave-one-country-out sensitivity analysis (38 separate FE estimates).

Note. Points represent the estimated robot coefficient when each country is excluded in turn; horizontal lines indicate approximate 95% confidence intervals. The dashed vertical line denotes the full-sample 38-country estimate (β = 0.0251). Countries marked with an asterisk are the four countries that joined the OECD after 2010.

Additional country-composition checks based on the unrestricted import-value specification show a positive additional Late4 import slope of 0.0613 (SE = 0.0208, conventional p = 0.0054). The corresponding null-imposed wild-cluster bootstrap p-value is 0.0162. Re-estimating this specification after removing each of the 38 countries yields positive slope-difference estimates ranging from 0.0455 (without Estonia) to 0.0702 (without Costa Rica); all conventional p-values are below 0.05. This leave-one-country-out range concerns the decomposed interaction, not the pooled ratio-model range shown in Figure 3. Although the coefficient sign is stable, country exclusions change its magnitude, and the number of late-entrant clusters remains small.

In the matched ten-country comparison, the unweighted Late4 × ln(imports) slope differential is 0.0565 (SE = 0.0286, conventional p = 0.0800; wild-bootstrap p = 0.1788). Applying matching-frequency weights gives 0.0588 (SE = 0.0247, conventional p = 0.0412; wild-bootstrap p = 0.1160). Thus, the direction and approximate magnitude are similar across specifications, but neither matched-sample difference is significant under the chosen bootstrap procedure (Table 5). The joint test of the Late4 import and manufacturing-value-added interactions likewise does not reject at 5% in the weighted matched sample (p = 0.0965).

Matching markedly reduces the 2010 difference in average log GDP per capita between Late4 and comparison countries (absolute mean gap 1.2343 before matching versus 0.2066 without weights and 0.1764 with matching-frequency weights). It does not improve balance in manufacturing share: the absolute gap rises from 0.2259 percentage points in the full sample to 0.8088 and 1.0807 points in the unweighted and weighted matched samples, respectively. Thus, economic comparability improves along the income dimension, but the design neither balances all observed structures nor shows whether development, manufacturing composition, renewable energy, or trade characteristics account for the remaining group distinction.

Table 5. Additional decomposed-model slope-difference, matching, and bootstrap sensitivity results.

Specification (N / G)	β_{diff}	p_{CR}	p_{WCB}
Full sample, unrestricted TWFE (456 / 38)	0.06131	0.0054	0.0162
Matched, unweighted (120 / 10)	0.05651	0.0800	0.1788
Matched, frequency-weighted (120 / 10)	0.05883	0.0412	0.1160

Note. N / G = country-year observations / countries; p_{CR} = conventional country-clustered p-value; p_{WCB} = wild-cluster bootstrap p-value. The dependent variable is ln(carbon intensity). Each specification includes ln(imports), ln(manufacturing

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value added), separate Late4 interactions with both, the baseline controls, and country and year fixed effects. Bootstrap p-values come from null-imposed wild cluster bootstrap-t tests (9,999 replications; Webb weights). The full-sample decomposed interaction differs from the original pooled logged import-to-manufacturing-value-added ratio coefficient. The four late entrants provide only four clusters in all three models; matched models include six distinct comparison-country clusters.

3.4 Complementary Findings on Energy Intensity

Robot import intensity has a positive and statistically significant coefficient in the energy-intensity level model ($\beta = 0.0180$, $p = 0.007$), which is also retained under DK inference with one lag (DK(1); $p = 0.020$). When energy intensity is added to the carbon-intensity equation, its coefficient is strongly positive ($\beta = 0.8623$, $p < 0.001$), whereas the robot coefficient decreases to 0.0096 and becomes insignificant ($p = 0.130$). Although this coefficient change suggests close comovement among the variables, it does not establish causal mediation.

The first-difference findings are more cautious: the coefficient relating the annual change in robot import intensity to the change in energy intensity is 0.0076 ($p = 0.245$), and lagged changes in robotization are also insignificant ($p = 0.696$). In contrast, the change in energy intensity is strongly associated with the change in carbon intensity ($\beta = 0.5859$, $p < 0.001$). Energy results should therefore be treated as complementary correlational evidence on the energy-use dimension, not as a formal test of a causal mechanism. The one-year lag of the change in robot intensity does not provide convincing evidence that robot acquisitions precede subsequent changes in energy use; a contemporaneous association in levels can reflect common trends or jointly determined macroeconomic adjustments. Dedicated distributed-lag and dynamic-panel specifications, including lagged level measures and formally assessed cumulative lag effects, have not been estimated here. Consequently, the direction and timing of the robot–energy–carbon relationship remain unresolved, and the attenuation of the robot coefficient upon adding energy intensity cannot establish a causal pathway.

3.5 Environmental Policy Stringency and Nonlinearity

The EPS analysis is explicitly exploratory because its 374 observations span only the 34 core OECD countries during 2010–2020. The four later entrants, whose inclusion is central to the main cross-group heterogeneity finding, are not represented. The robot coefficient is already near zero and insignificant in this restricted sample. The Robot × EPS coefficient is negative ($\beta = -0.0074$) but not significant ($p = 0.235$), and the marginal robot coefficient at the sample mean of EPS is small ($\beta = 0.0021$, $p = 0.824$). Accordingly, this subsample cannot test whether environmental policy explains or moderates the stronger slopes observed among the four later entrants.

Estimated marginal effects are 0.0045 ($p = 0.627$), 0.0014 ($p = 0.879$), and −0.0031 ($p = 0.764$) at the 25th, 50th, and 75th percentiles of EPS, respectively; each confidence interval contains zero. Thus, exploratory RQ2 yields no statistically persuasive evidence of moderation within the observed core cohort. These null results do not establish that environmental policy is ineffective, nor do they adjudicate policy moderation in the late-entrant countries excluded from the EPS dataset.

In the exploratory nonlinear specification, the squared robot term is −0.0033 ($p = 0.239$) and remains insignificant under Driscoll–Kraay inference ($p = 0.248$). The available OECD sample therefore provides insufficient statistical evidence of a clear U-shaped or inverted U-shaped pattern. However, the short time dimension precludes ruling out nonlinearity more generally.

3.6 Total CO~2~ Emissions and Scale Results

In the total CO_2 level model, annual robot import intensity is positively and significantly associated with emissions ($\beta = 0.0208$, $p = 0.018$), and the result is retained under DK(1) inference ($p = 0.015$). The renewable energy share is negatively associated with total CO_2, whereas the economic-scale indicators exhibit positive associations.

In contrast, the first-difference coefficient of the annual robot indicator is 0.0067 ($p = 0.125$), and the coefficient of its lagged difference is 0.0001 ($p = 0.976$). For the three-year robot indicator, the total CO_2 level coefficient is 0.0258 ($p = 0.092$), and the first-difference coefficient is 0.0212 ($p = 0.064$). Thus, although the association with total emissions is positive, it has lower statistical precision than the carbon-intensity findings.

With respect to RQ1, carbon intensity and total CO_2 emissions do not exhibit identical empirical patterns. While robot import intensity is positively associated with both outcomes in levels, the total CO_2 results are weaker, particularly in models of short-run changes. Because the analysis does not identify an initial improvement in carbon intensity, this pattern is not interpreted as confirmation of the conventional rebound mechanism. Table 6 summarizes the energy-intensity, policy-moderation, and total CO_2 results.

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Table 6. Complementary results for energy intensity, policy moderation, and total CO_2 emissions.

Analysis	Coefficient/marginal effect	SE	p	N	Finding
Energy intensity FE: ln robot	0.0180	0.0063	0.007	456	Positive levels association
Energy intensity DK(1): ln robot	0.0180	0.0066	0.020	456	Levels result persists
Energy intensity FD: Δ ln robot	0.0076	0.0065	0.245	418	Short-run channel not confirmed
Carbon + ln energy: ln robot	0.0096	0.0062	0.130	456	Robot insignificant after energy added
Carbon + ln energy: ln energy	0.8623	0.0864	< 0.001	456	Strong positive energy intensity
Robot × EPS	−0.0074	0.0061	0.235	374	No significant moderation evidence
Robot effect, EPS 25th percentile	0.0045	0.0093	0.627	374	Not significant
Robot effect, EPS median	0.0014	0.0093	0.879	374	Not significant
Robot effect, EPS 75th percentile	−0.0031	0.0105	0.764	374	Not significant
Total CO_2 FE: annual robot	0.0208	0.0084	0.018	456	Positive at 5%
Total CO_2 FD: annual robot	0.0067	0.0043	0.125	418	Short-run result insignificant
Total CO_2 FE: three-year robot	0.0258	0.0149	0.092	380	Positive at 10%
Total CO_2 FD: three-year robot	0.0212	0.0111	0.064	342	Positive at 10%

Note. Energy and total CO_2 models are complementary analyses in the expanded retrospective panel of 38 countries and should not be generalized homogeneously to all OECD countries given the sample heterogeneity identified for the core cohort. Energy specifications are not formal tests of causal mediation. The EPS model is based on 374 observations for the 34 core countries from 2010 to 2020; it is an exploratory core-sample analysis and cannot address Late4 policy moderation.

3.7 Discussion

The core-cohort result contrasts with cross-country studies that report carbon-reducing effects of robot adoption based on IFR installation and stock measures [1], [3], [4], [12]. The absence of a significant association among the 34 core members, together with the positive slope in the retrospective 38-country panel, is closer to studies that document positive or conditional associations [5], [7] and supports the view that country-level coefficients should not be treated as universal [10], [11]. Two features of the present design may help reconcile these differences. First, the robot indicator is a nominal import-acquisition proxy rather than an operational stock measure [8], and the rejected ratio restriction shows that it partly reflects manufacturing scale. Second, the sensitivity to sample composition indicates that pooled estimates can be shaped by a small group of countries with distinct structural characteristics.

The analysis identifies Latvia, Lithuania, Colombia, and Costa Rica as important contributors to this heterogeneity. When robot imports and manufacturing value added are included as separate logarithmic regressors, the additional robot import slope for these four countries is positive ($\beta = 0.0613$). Country-level influence diagnostics indicate that this difference is not attributable to any single country, with the estimated interaction coefficient ranging from 0.0455 to 0.0702 across 38 leave-one-country-out specifications. Nevertheless, the Late4 classification should not be interpreted as identifying a distinct causal mechanism. These countries differ from the core OECD cohort in economic development, industrial composition, energy systems, and trade characteristics, which may partly explain the observed differences in the robot–carbon relationship. Moreover, the absence of a statistically significant difference between pre- and postaccession slopes does not establish equivalence or imply that OECD membership has no effect, particularly because only 13 country-year observations are available after accession.

The matched-country analyses provide further insight into the sensitivity of the estimated group differences. The estimated robot import slope differential remains positive in both the unweighted ($\beta = 0.0565$) and matching-frequency-weighted ($\beta = 0.0588$) specifications. However, although matching substantially reduces the baseline income gap, it does not improve balance in manufacturing share, and differences in energy composition, trade patterns, and other structural characteristics remain insufficiently addressed. The matched-sample results therefore indicate that the positive association is not eliminated by restricting the comparison to countries with more similar observed economic characteristics, but they cannot establish whether the remaining differential reflects specific structural conditions or unobserved country heterogeneity.

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A distinction between the stability of coefficient signs and the robustness of statistical significance is particularly important. In the full 38-country decomposed interaction model, the additional Late4 slope remains statistically significant under wild cluster bootstrap inference ($p = 0.0162$), but the corresponding bootstrap p-values increase to 0.1788 and 0.1160 in the unweighted and weighted matched samples, respectively. Similarly, the original pooled robot-ratio specification retains a positive coefficient under the parsimonious CCE estimator, although its statistical significance weakens ($p = 0.065$), while the wild cluster bootstrap produces a borderline result ($p = 0.046$). These estimates originate from different model specifications and should not be interpreted interchangeably. Taken together, the results suggest that the positive direction of the robot–carbon association is more stable than its statistical precision.

The energy-intensity results also differ from studies reporting that robots lower manufacturing or firm energy intensity [13], [14]. Although robot import intensity is positively associated with energy intensity in levels, the relationship is insignificant in first differences, and adding energy intensity to the carbon equation renders the robot coefficient insignificant. These patterns are consistent with robot imports, energy consumption, and carbon performance responding to common economic developments or changes in production scale, but they do not establish that robot acquisition precedes changes in energy use. Accordingly, H2 is not supported, and the results should not be presented as evidence of a confirmed energy-transmission or mediation mechanism.

The policy-moderation results are subject to important sample restrictions. Unlike evidence that environmental regulation strengthens robots' carbon-reducing effects in manufacturing exports [11] or shapes emissions dynamics in OECD countries [27], the Robot × EPS interaction and the marginal effects at different stringency levels are insignificant within the 34-country core sample. Because EPS data exclude the four late entrants and the baseline robot coefficient is already insignificant in this sample, the result should not be read as evidence that environmental regulation is ineffective. Likewise, the exploratory quadratic specification provides insufficient evidence of the nonlinear patterns reported in subnational studies [24], [25].

Finally, the total CO_2 results reinforce the importance of distinguishing between environmental outcomes [6], [9]. The association with total emissions is positive but weaker and less consistent than that observed for carbon intensity, particularly in first differences. Because the analysis does not establish an initial carbon-intensity reduction attributable to robotization, the results do not support a conventional rebound interpretation. Table 7 summarizes the evidence for each hypothesis and research question.

Table 7. Summary of evidence for the hypotheses and research questions.

Hypothesis/question	Key evidence	Assessment
H1: Robot acquisition reduces carbon intensity	Core 34: $\beta = 0.0046$ ($p = 0.657$); expanded 38: $\beta = 0.0251$ ($p = 0.009$); Late4 slopes positive (Table 3)	Not supported
RQ1: Total CO_2 versus carbon intensity	Levels: $\beta = 0.0208$ ($p = 0.018$); first difference: $\beta = 0.0067$ ($p = 0.125$) (Table 6)	Patterns differ; weaker precision for total CO_2
H2: Robot acquisition reduces energy intensity	Levels: $\beta = 0.0180$ ($p = 0.007$), positive; first difference: $\beta = 0.0076$ ($p = 0.245$) (Table 6)	Not supported
RQ2 (exploratory): EPS moderation	Robot × EPS: $\beta = -0.0074$ ($p = 0.235$); core 34 only, $N = 374$ (Table 6)	No evidence of moderation
Nonlinearity (exploratory)	Squared robot term: $\beta = -0.0033$ ($p = 0.239$); DK $p = 0.248$	Insufficient evidence

Note. Assessments refer to conditional associations in the samples analyzed and do not imply causal effects.

4 Conclusion

This study examined the relationship between industrial robot import intensity and carbon performance across OECD countries during 2010–2021, with particular attention to country-composition heterogeneity, measurement sensitivity, and the robustness of statistical inference. The findings do not support a homogeneous relationship between robot acquisition and carbon intensity across OECD countries. In the core cohort of 34 countries that had joined the OECD by the end of 2010, the robot import intensity coefficient is statistically insignificant ($\beta = 0.0046$, $p = 0.657$), whereas the retrospective panel of 38 countries that were OECD members as of 2021 yields a positive and significant coefficient ($\beta = 0.0251$, $p = 0.009$). The disappearance of statistical significance in the membership-consistent specification further demonstrates that the estimated association is sensitive to sample composition rather than representing a uniform OECD-wide relationship. The positive association is concentrated among the four later entrants, and its direction is more stable than its statistical

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precision. H1 and H2 are not supported, and the exploratory analyses provide no statistically robust evidence of policy moderation or nonlinearity.

These findings have several implications for the evaluation of automation and environmental policy. Industrial robot acquisition should not be regarded as inherently carbon-reducing or carbon-increasing across countries. Its environmental associations must be considered in relation to energy supply, industrial structure, economic development, production scale, and the technological characteristics of the acquired equipment. The distinction between nominal robot imports and actual robot deployment is especially important. Import values may reflect price changes, exchange-rate movements, and international trade arrangements rather than changes in the operational stock of industrial robots. Furthermore, the rejection of the equal-and-opposite coefficient restriction for robot imports and manufacturing value added indicates that the import-to-manufacturing-value-added ratio cannot be interpreted as a pure measure of robotization. These measurement limitations reinforce the need for caution when translating the estimated associations into policy recommendations.

Several limitations should be acknowledged. First, the reliance on nominal imports classified under HS 847950 does not directly capture robot installations, operational stocks, or their technological characteristics. Second, the observational panel design and relatively short time dimension ($T = 12$) limit causal identification and the analysis of dynamic relationships. Third, the small number of late-accession countries creates substantial uncertainty in subgroup inference, even when country-clustered standard errors and wild cluster bootstrap procedures are employed. Fourth, the matching exercises do not fully balance the relevant structural characteristics, and the existing specifications cannot determine the extent to which income, industrial composition, energy systems, and trade patterns account for the observed heterogeneity. Fifth, the restricted EPS coverage prevents an assessment of environmental policy moderation in the four late-accession countries. Finally, although the study considers carbon intensity, total CO_2 emissions, and energy intensity, it does not directly measure manufacturing upgrading, green productivity, clean-technology adoption, or sustainable industrial restructuring.

Future research could address these limitations by incorporating physical robot adoption indicators, longer country panels, sector-level manufacturing data, and more comprehensive measures of industrial and energy-system transformation. Explicit interactions between robot acquisition and structural country characteristics, together with distributed-lag and appropriately specified dynamic-panel models, would also help clarify whether the observed relationships reflect persistent structural conditions or changes unfolding over time.

In conclusion, the study's principal contribution lies in demonstrating that the relationship between industrial robot imports and carbon performance is sensitive to country composition and econometric specification. The positive robot–carbon association observed in the expanded OECD panel is not representative of the core OECD cohort, and the estimated differences associated with the four late-accession countries remain subject to structural and statistical uncertainty. The findings therefore highlight the importance of accounting for cross-country heterogeneity and measurement limitations rather than drawing generalized or causal conclusions about the environmental consequences of robotization.

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All authors have read and approved the final version of the manuscript.

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Conflict of Interest
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Use of Artificial Intelligence (AI) and AI-Assisted Technologies
During the preparation of this manuscript, the authors used Grammarly for language refinement and proofreading. The authors reviewed and verified all AI-assisted output and take full responsibility for the accuracy, originality, integrity, and final content of the manuscript.

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Data Availability Statement
The data supporting the findings of this study are available from the corresponding author upon reasonable request. The underlying country-level series are publicly available from UN Comtrade, the World Bank, and the OECD.

Ethics Statement
Ethical review and approval were not required for this study because it used only publicly available country-level secondary data and did not involve human participants or animals.

Informed Consent Statement
Not applicable.

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