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Agricultural Trade, Eco-Efficiency, and Greenhouse-Gas Emissions in the EU-27: Evidence from a Dynamic Panel Analysis, 1991–2023

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Abstract

This study examines short-run associations among agricultural trade, production, and environmental performance across the 27 current European Union (EU) member states during 1991–2023. The unbalanced panel includes pre-accession observations; Common Agricultural Policy effects are not separately identified. Eco-efficiency is measured as the reciprocal of FAOSTAT agricultural greenhouse-gas (GHG) emissions per unit of production value, with total GHG emissions as an alternative outcome. Pesaran CD, CIPS, and Westerlund tests assess cross-sectional dependence, unit roots, and cointegration. As cointegration is not established, first-differenced dynamic common-correlated-effects mean-group and fixed-effects models with Driscoll–Kraay standard errors are estimated. Conditional on production and common-factor controls, total trade, imports, and exports show no statistically significant average association with eco-efficiency at the 5% level. Production is positively associated with eco-efficiency, although their shared production component warrants caution. In the alternative emissions model, the estimated production elasticity is 0.1325 (conventional 95% confidence interval: 0.0758–0.1893), indicating a positive but less-than-proportional emissions response, consistent with relative rather than absolute decoupling. The findings support evaluating agricultural trade alongside production and absolute emissions rather than treating trade expansion as evidence of environmental improvement.

Keywords: agricultural trade; sustainable land use; eco-efficiency; greenhouse-gas emissions; environmental decoupling; sustainable intensification; EU-27; dynamic CCE-MG

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1. Introduction

Agricultural environmental sustainability has become a central challenge for contemporary food systems, requiring the reconciliation of agricultural productivity, economic competitiveness, food security, and environmental protection. Agricultural production contributes to greenhouse-gas (GHG) emissions and places substantial pressure on land, water, biodiversity, and other natural resources. Consequently, improving agricultural sustainability requires not only maintaining or increasing production but also limiting the environmental pressures associated with agricultural activities. This challenge is particularly relevant in the context of climate change, growing resource constraints, and the transition towards more sustainable and resilient agricultural systems [1–4].

Eco-efficiency provides an important analytical perspective for examining the relationship between agricultural production and environmental performance. Broadly defined, eco-efficiency reflects the ability to generate economic or productive value while limiting associated environmental pressures. In agriculture, improvements in productive efficiency do not necessarily coincide with improvements in environmental efficiency, particularly when higher output is accompanied by greater emissions or resource consumption. Recent evidence from European agriculture highlights the importance of distinguishing conventional productive efficiency from eco-efficiency when assessing the environmental performance of agricultural systems [5]. Accordingly, the relationship between agricultural production and GHG emissions represents an important dimension of agricultural sustainability, although it cannot capture all relevant environmental impacts.

International agricultural trade introduces additional complexity into this relationship. Trade integration may influence environmental performance through changes in production scale, commodity composition, resource allocation, technological adoption, and the geographical distribution of agricultural activities. Export expansion may stimulate domestic production and associated environmental pressures, whereas imports may substitute for domestic production while transferring part of the environmental burden to supplying countries. Recent studies demonstrate that agricultural trade can redistribute pressures on land, water, biodiversity, and other environmental resources across national boundaries [6,7]. A systematic review further highlights the multiple, potentially opposing mechanisms through which agricultural trade affects environmental outcomes [8]. Consequently, neither greater trade integration nor increasing agricultural exports and imports can be assumed to produce uniform environmental benefits.

These relationships are particularly relevant to the European Union, where agricultural systems operate within an integrated market and a common regulatory and policy environment but exhibit substantial heterogeneity in production structures, resource intensity, technological development, and environmental performance. Previous European research has examined the relationships among agricultural imports, environmental pressures, energy-related emissions, and sustainable production practices [9–11]. Other studies suggest that the environmental consequences of agricultural exports depend on accompanying changes in technology, production intensity, and economic structure [12,13]. Recent evidence also identifies considerable variation in agricultural eco-efficiency across European countries, reinforcing the importance of examining productive and environmental performance jointly [5,14].

From a theoretical perspective, the environmental consequences of trade are commonly understood through scale, composition, and technique effects. Trade expansion may increase environmental pressure by stimulating production, modifying the distribution of economic activities across sectors and countries, or facilitating technological and organizational improvements that reduce environmental intensity. The relative importance of these channels determines whether market integration is associated with environmental deterioration or improvement. Foundational and subsequent contributions to the trade–environment literature demonstrate that these effects may operate simultaneously and differ substantially across economic activities and institutional settings [15–19]. More specifically, recent reviews of agricultural trade liberalization identify heterogeneous environmental consequences related to production expansion, structural adjustment, technological change, and the geographical displacement of environmental burdens [20].

An important implication is that changes in agricultural emissions intensity must be distinguished from changes in absolute emissions. A reduction in emissions per unit of agricultural output may indicate improved relative environmental performance even when total emissions continue to increase. This distinction is reflected in the concept of environmental

decoupling, which differentiates between relative decoupling, where emissions increase more slowly than agricultural output, and absolute decoupling, where emissions remain stable or decline while agricultural output expands. Under this definition, unchanged emissions represent the zero-growth boundary case of absolute decoupling, whereas actual emissions reductions require a decline in total emissions. Both forms of decoupling are relevant to assessing the environmental implications of agricultural development. Nevertheless, improvements in emissions intensity do not necessarily imply reductions in absolute environmental pressures or establish comprehensive agricultural sustainability, particularly when other environmental impacts remain unmeasured.

Despite the growing literature on agricultural trade and environmental performance, several empirical issues warrant further investigation. First, previous studies have examined agricultural trade, eco-efficiency, and emissions dynamics from different perspectives, but their interrelationships are not always evaluated within a common empirical framework that explicitly accounts for domestic agricultural production. Consequently, it remains important to establish whether aggregate trade retains an independent short-run association with environmental performance once production dynamics are controlled for. Second, total agricultural trade may conceal differences between imports and exports, whose environmental associations can vary according to production structures and trade composition. Third, European agricultural systems are exposed to common macroeconomic, technological, commodity-market, and environmental shocks, creating cross-sectional dependence that should be accommodated in panel estimation. Finally, eco-efficiency indicators incorporating agricultural production value share an accounting component with the production variables included among the regressors. This relationship requires explicit verification and careful coefficient interpretation alongside alternative environmental outcomes.

Against this background, the present study investigates the short-run relationships among agricultural trade, agricultural production, eco-efficiency, and GHG emissions in 27 European countries corresponding to the current composition of the European Union over 1991–2023. The period includes observations preceding the EU accession of several member states; consequently, the analysis does not identify the causal effects of EU membership or Common Agricultural Policy reforms. Agricultural environmental performance is primarily measured through an eco-efficiency indicator defined as the reciprocal of FAOSTAT GHG emissions intensity per value of agricultural production. Total agricultural GHG emissions are additionally considered as an alternative dependent variable, allowing the analysis to distinguish changes in measured eco-efficiency from changes in absolute environmental pressures.

The empirical strategy explicitly accounts for important features of the European agricultural panel, including cross-sectional dependence, non-stationarity, and heterogeneity across countries. Following diagnostic testing, the analysis focuses on short-run relationships in first differences, employing dynamic common-correlated-effects mean-group (CCE-MG) estimation and complementary fixed-effects specifications with Driscoll–Kraay standard errors. Separate specifications for total agricultural trade, imports, and exports are estimated, while alternative environmental outcomes and specifications excluding agricultural production are used to examine the sensitivity of the results.

The study addresses three research questions:

RQ1. Is agricultural trade systematically associated with short-run changes in agricultural eco-efficiency in the EU-27 after accounting for agricultural production dynamics, country heterogeneity, and common cross-country shocks?

RQ2. What short-run associations do agricultural imports and exports, examined separately, exhibit with agricultural environmental performance?

RQ3. How does the estimated production–environment relationship change when total agricultural GHG emissions replace eco-efficiency as the dependent variable, and are the resulting estimates consistent with relative environmental decoupling?

The study's contribution is primarily empirical and integrative, rather than the development of a new econometric estimator. Four aspects distinguish its contribution to the agricultural sustainability literature.

First, the study jointly investigates trade integration, agricultural production, and GHG-based environmental performance, providing evidence on whether aggregate trade indicators retain an identifiable short-run association with eco-efficiency after production dynamics and common shocks are taken into account.

Second, it uses a relatively long historical panel covering 1991–2023 for the fixed set of current EU-27 member states. This allows short-run relationships to be examined across diverse agricultural systems and periods of economic, institutional, and environmental change, while recognizing differences in countries' historical EU membership status.

Third, the empirical design combines heterogeneous country-specific estimation with procedures addressing cross-sectional dependence and non-stationarity. This is particularly relevant to integrated European agricultural markets, where common disturbances may affect countries simultaneously but with different magnitudes.

Fourth, and importantly, the study explicitly investigates the potential conceptual overlap between agricultural production and the construction of the eco-efficiency indicator. By considering absolute agricultural GHG emissions as an alternative outcome and assessing specifications excluding agricultural production, the analysis provides a more differentiated interpretation of measured eco-efficiency and production–emissions relationships. This distinction is essential for interpreting evidence of relative environmental decoupling without equating improvements in emissions intensity with absolute emissions reductions.

Overall, the study contributes to the understanding of agricultural environmental sustainability by separating the short-run associations of trade integration and domestic production with GHG-based environmental performance. It also highlights the importance of distinguishing eco-efficiency improvements from absolute emissions outcomes when evaluating agricultural sustainability. The findings can inform broader discussions of environmentally sustainable agricultural development and resource-efficient production, while their implications for specific policy instruments and land-management practices require additional evidence.

The remainder of the paper is organized as follows. Section 2 reviews the relevant literature and establishes the empirical research gap. Section 3 presents the data and econometric methodology. Section 4 reports the empirical findings and robustness analyses. Section 5 discusses their interpretation, sustainability implications, and limitations, while Section 6 concludes.

2. Literature Review

2.1. Trade Integration and Environmental Pressures

International agricultural trade affects environmental outcomes through scale, composition and technique effects, and these mechanisms may act in opposing directions. Increased market access can expand production and associated input demand (scale), redistribute output across commodities, regions and technologies (composition), or facilitate changes in emissions intensity through innovation and regulation (technique). The trade–environment literature therefore offers no general prediction that larger aggregate trade flows will improve or worsen environmental performance [15–19]. More recent synthesis specific to agricultural trade liberalization likewise identifies multiple pathways and context-dependent environmental outcomes [20].

This distinction is important for the EU agricultural sector, which combines integrated markets and common policy rules with heterogeneous national production systems. Specialization in crops rather than livestock, differences in energy and fertilizer requirements, and the stringency of environmental standards can produce different emissions responses to comparable changes in trade. An insignificant coefficient on aggregate trade cannot therefore exclude offsetting commodity- or country-specific effects, nor can it identify a particular causal trade mechanism [17,18,20].

2.2. Environmental Burdens Embodied in Trade

Production-based inventories assign environmental pressures to the place of production, whereas consumption- or trade-adjusted perspectives account for cross-border displacement. International trade may transfer emissions and resource demands between countries even when their domestic emissions trends appear favorable [21–23]. In agriculture, trade can also redistribute virtual land and water requirements and create indirect incentives for land conversion beyond the importing economy [24–26].

Recent agricultural evidence shows why this accounting distinction matters. Foong et al. [27] demonstrate the policy relevance of adjusting agricultural emissions for trade, while Zhao et al. [28] quantify greenhouse gases embodied in international agricultural trade. Cabernard et al. [6] trace biodiversity impacts associated with recent land-use change driven by rising agri-food imports. These studies indicate that a national aggregate trade coefficient represents only a partial view of environmental consequences: the analysis in this paper addresses domestic agricultural GHG performance rather than the complete trade-embodied footprint.

2.3. Eco-Efficiency, Output Measurement and Production Dynamics

The concept of agricultural eco-efficiency evaluates the ability of an agricultural system to generate economic or productive output while minimizing resource consumption and associated environmental pressures [5,29,30]. Quantified eco-efficiency approaches and frontier-based measures provide complementary perspectives, and should not be confused with conventional productive efficiency [31–33]. Agricultural systems may raise output per unit of GHG emissions while still increasing total emissions.

The indicator employed here is the reciprocal of FAOSTAT emissions per value of agricultural production. Consequently, its logarithmic variation contains changes in agricultural production value as well as changes in emissions. The explanatory production measure, in contrast, is a physical production-volume index. Both measures are based on constant-price agricultural production, and the verified near-identity between their annual logarithmic changes and GHG emissions changes has direct implications for interpreting the production coefficient. Its positive association with ratio-based eco-efficiency should not be treated as independent evidence of improved environmental performance. Examining total agricultural GHG emissions as an alternative dependent variable is therefore an essential complementary specification, but it does not eliminate every source of endogeneity or omitted-variable bias.

Recent evidence provides additional context for this distinction. Ait Sidhoum et al. [34] evaluate agri-environment schemes and farm eco-efficiency in EU countries, finding that observed eco-efficiency improvements cannot be assumed to follow automatically from scheme participation. Yılmaz [30] documents variation in macro-level agricultural eco-efficiency across EU member states and Türkiye. Together, these studies reinforce the need to separate measured eco-efficiency, economic production and the mechanisms that potentially influence both.

2.4. Decoupling and the Interpretation of Emissions Elasticities

Decoupling describes the relationship between changes in environmental pressure and the economic activity driving it. Relative decoupling occurs when agricultural output and emissions both increase, but emissions grow at a slower rate than output. Absolute decoupling occurs when agricultural output increases while emissions remain stable or decline. Under this definition, unchanged emissions represent the zero-growth boundary case of absolute decoupling, whereas an actual reduction in total emissions requires a negative emissions growth rate. These distinctions are widely applied in environmental assessment but do not, by themselves, establish causal relationships or demonstrate comprehensive environmental sustainability [35,36].

For the present short-run log-difference specification, a positive production–emissions elasticity below unity is consistent with relative decoupling, conditional on the included regressors and model specification, as emissions increase less than proportionately with agricultural output. However, this estimated relationship does not establish absolute decoupling, which requires stable or declining total emissions alongside expanding agricultural output. Unchanged emissions represent the zero-growth boundary of absolute decoupling, whereas actual emissions reductions require a decline in total emissions. Moreover, the estimated association does not identify whether changes in emissions intensity arise from technological improvements, farm management practices, livestock composition, or shifts in agricultural production. Sectoral GHG inventories also omit environmental pressures relevant to land-use sustainability. Evidence from Andrei et al. [37], examining agricultural CO_2, CH_4, and N_2O decoupling across European countries, further indicates that decoupling patterns may vary by greenhouse gas, period, and national production conditions.

2.5. Agricultural Land, Intensification and Environmental Trade-Offs

The sustainability relevance of agricultural output expansion depends on whether additional production is obtained through the conversion of land, higher output per hectare or changes in commodity mix. Increasing yields on existing farmland can potentially moderate pressure for expansion, but greater intensification may also increase nutrient surpluses, soil degradation, water use or biodiversity losses [38–43]. Sustainable intensification is therefore a conditional goal rather than an automatic outcome of productivity gains.

Food-system environmental boundaries extend beyond the agricultural emissions inventory. Evidence on global food production and mitigation pathways shows potentially substantial trade-offs across GHG emissions, land demand and other ecological pressures [44–48]. Boakes et al. [49] quantify biodiversity impacts associated both with food-system land use and its emissions, illustrating why a single emissions indicator cannot capture all environmental dimensions. The IPCC [47] likewise treats land carbon dynamics and agricultural emissions within a broader mitigation context. Although the present analysis offers evidence on agricultural production-related GHG outcomes, it does not directly measure land conversion, soil carbon, biodiversity or the adoption of land-management practices.

2.6. Research Gap and Empirical Contribution

The literature reveals three analytical challenges relevant to the EU-27. First, an aggregate trade coefficient combines potentially competing scale, composition and technique pathways; the independent short-run relationship with domestic eco-efficiency remains an empirical question. Second, the use of agricultural production as an explanatory factor alongside a production-value-based eco-efficiency indicator requires explicit recognition of their near-accounting identity, as verified in Section 3.2. Third, evidence of relative

emissions decoupling should be distinguished from absolute emissions reductions and from broader land-use sustainability claims.

This study addresses these questions with a 1991–2023 EU-27 panel that accommodates cross-sectional dependence and heterogeneous country responses. It compares total trade with imports and exports, examines the production–eco-efficiency relationship, and uses total agricultural GHG emissions as an alternative outcome. These choices provide evidence on short-run aggregate associations rather than identifying the separate trade channels or the environmental consequences of particular management practices. The resulting contribution is an empirically qualified distinction between trade integration, measured eco-efficiency and production-related emissions decoupling.

3. Materials and Methods

3.1. Data and Sample

The empirical analysis covers 27 countries corresponding to the current membership composition of the European Union (EU-27), using annual observations for 1991–2023. The sample includes Austria, Belgium, Bulgaria, Croatia, Cyprus, Czechia, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, and Sweden. The designation EU-27 defines a fixed geographical sample rather than the historical composition of EU membership. During the study period, successive enlargements occurred in 1995, 2004, 2007, and 2013, meaning that the dataset includes pre-accession observations for several countries. The United Kingdom is excluded from the sample. Consequently, the analysis examines agricultural and environmental relationships across the selected countries without explicitly identifying the effects of EU accession, Common Agricultural Policy (CAP) implementation, or specific agricultural-policy reforms.

Annual data were obtained from FAOSTAT [50]. Agricultural production is represented by the FAOSTAT Gross Production Index Number for Agriculture (2014–2016 = 100), which captures changes in agricultural production volume independently of fluctuations in output prices. Environmental performance is measured using the FAOSTAT indicator Emissions per Value of Agricultural Production, expressed in kg CO_2-equivalent per international dollar and referring to IPCC Agriculture. This indicator measures greenhouse-gas emissions intensity relative to agricultural production value. Its reciprocal is used to construct the agricultural eco-efficiency indicator, with higher values indicating greater production value per unit of emissions. Total agricultural greenhouse-gas emissions are additionally considered as an alternative environmental outcome to assess the relationship between agricultural production and absolute emission levels.

Agricultural trade comprises imports and exports of crops and livestock products, originally reported by FAOSTAT in thousands of current US dollars. To reduce the influence of price changes, nominal trade values are deflated using country-specific FAOSTAT GDP deflators expressed in US-dollar terms (2015 = 100), yielding trade values in thousands of constant 2015 US dollars. Total agricultural trade is calculated as the sum of real imports and exports.

For reproducibility, the FAOSTAT domains, item codes and element codes of the main agricultural production and emissions indicators are summarized below in Table 1.

The initial panel comprises 27 countries over 33 years, corresponding to a maximum of 891 country-year observations. However, the panel is mildly unbalanced due to differences in historical data availability. Agricultural production and environmental-performance indicators are available for 855 observations (95.96% coverage), while agricultural imports, exports, and total greenhouse-gas emissions are available for 864 observations (96.97% coverage). The joint dataset contains 855 complete country-year observations before econometric

transformations. Missing observations are concentrated in the early years, particularly for Belgium and Luxembourg (1991–1999), Bulgaria (1991–1997), and several Central and Eastern European countries during 1991–1993. From 2000 onward, complete observations are available for all 27 countries across the principal variables. These differences in data coverage are distinct from changes in EU membership, as pre-accession observations are retained wherever available. The final estimation sample may be further reduced by first-differencing and the inclusion of lagged variables.

Table 1. FAOSTAT data sources and corresponding domain, item, and element codes for the agricultural production and environmental indicators used in the analysis.

Variable	FAOSTAT Domain	Item Code	Element Code
Agricultural production index (PROD)	QI	F2051	432
Emissions intensity (EI)	EM	1711	72791
Agricultural GHG emissions (GHG)	GT	1711	723113
Gross production value (QV)	QV	F2051	152

3.2. Variable Construction

EE Agricultural eco-efficiency (EE_{it}) is the principal dependent variable. Given that FAOSTAT reports emissions per value of agricultural production, eco-efficiency is defined as the reciprocal of emissions intensity (Equation (1)):

$$EE_{it} = \frac{1}{EI_{it}} = \frac{V_{it}}{GHG_{it}} \quad (1)$$

where EI_{it} denotes agricultural emissions intensity in country i and year t and V_{it} is the gross value of agricultural production at constant prices. Higher values of EE_{it} therefore indicate greater agricultural production value per unit of greenhouse-gas emissions. Since EE_{it} and EI_{it} are exact inverses, their logarithmic transformations satisfy Equation (2):

$$ln(EEit) = -ln(EIit) = lnV_{it} - \ln(GHG_{it}) \quad (2)$$

An important measurement consideration arises from the construction of the agricultural eco-efficiency indicator. According to the FAOSTAT methodology, emissions intensity is calculated as agricultural greenhouse-gas emissions divided by the gross value of agricultural production at constant 2014–2016 international prices. Agricultural eco-efficiency is defined as the reciprocal of this measure. The agricultural production index is also based on constant-price agricultural output, normalized to the 2014–2016 reference period.

Consequently, the eco-efficiency and production indicators are closely related through an accounting relationship. In logarithmic first differences, this relationship can be expressed as Equation (3):

$$\Delta\ln EE_{it} = \Delta\ln Prod_{it} - \Delta\ln GHG_{it} + \varepsilon_{it} \quad (3)$$

where ε_{it} captures departures from the exact identity, including potential differences between the production index and the production-value series underlying emissions intensity, and differences in precision or source vintages. Agricultural production is represented by the Gross Production Index ($PROD_{it}$), with 2014–2016 = 100.

To examine this relationship, an additional verification was conducted using the processed EU-27 dataset. The analysis covered 828 matched annual changes for 27 countries over 1992–2023. The correlation between observed eco-efficiency growth and the difference between production growth and emissions growth was 0.9977. The mean absolute discrepancy was 0.0028 log points, and 98.55% of observations exhibited an absolute discrepancy below 0.01.

Only 12 matched annual changes exhibited absolute discrepancies of at least 0.01 log points. These exceptional observations require source-level verification; their presence does not justify treating the production and eco-efficiency indicators as independent.

Although the accounting relationship holds approximately for the overwhelming majority of observations, its implications are explicitly considered when interpreting the econometric findings. Specifically, the estimated production coefficient in the eco-efficiency model partly reflects the shared accounting structure and should not be interpreted as independent evidence of environmental improvements. Accordingly, the eco-efficiency model provides complementary information on emissions performance relative to agricultural output, whereas the specification employing total agricultural GHG emissions as the dependent variable provides the principal evidence concerning the production–emissions relationship and relative decoupling.

The reciprocal emissions-intensity measure provides a transparent indicator of agricultural production value generated per unit of greenhouse-gas emissions. However, it should be distinguished from frontier-based approaches that explicitly incorporate undesirable environmental outputs. For example, Guo and Li [51] employed an undesirable-output slack-based measure (SBM) to evaluate energy-saving and emissions-reduction efficiency, accounting for input–output slacks and environmental burdens relative to a production frontier. Unlike such multidimensional efficiency measures, the indicator employed in the present study does not estimate a production frontier, incorporate agricultural resource inputs explicitly, or identify technical inefficiency. Instead, it captures a specific dimension of agricultural environmental performance, namely the relationship between production value and associated GHG emissions. Its advantage lies in its transparent construction and consistent application across the EU-27 panel over the study period. Nevertheless, an increase in this indicator does not necessarily imply a reduction in absolute emissions or an improvement in overall resource-use efficiency, since it may also reflect changes in production value, sectoral composition, or the relative growth of output and emissions. Accordingly, the term *eco-efficiency* is used here in the restricted sense of emissions-adjusted agricultural production value rather than frontier-based environmental efficiency.

Nominal agricultural imports and exports are converted into constant 2015 US dollars according to Equations (4) and (5), respectively:

$$IMP_{it}^{R} = \frac{IMP_{it}}{DEF_{it}/100} \mathrm{IMP}_{\mathrm{it}}^{\mathrm{R}} = \mathrm{IMP}_{\mathrm{it}}^{\mathrm{N}} \times 100/\mathrm{DEF}_{\mathrm{it}} \quad (4)$$

and

$$EXP_{it}^{R} = \frac{EXP_{it}}{DEF_{it}/100}, \ \mathrm{EXP}_{\mathrm{it}}^{\mathrm{R}} = \mathrm{EXP}_{\mathrm{it}}^{\mathrm{N}} \times 100/\mathrm{DEF}_{\mathrm{it}} \quad (5)$$

DEF_{it} where $\mathrm{DEF}_{\mathrm{it}}$ denotes the country-specific GDP deflator (2015 = 100), $\mathrm{IMP}^{\mathrm{N}}{}_{\mathrm{it}}$ and $\mathrm{EXP}^{\mathrm{N}}{}_{\mathrm{it}}$ represent nominal agricultural imports and exports, respectively, and superscript R denotes GDP-deflated trade values. Total real agricultural trade is calculated as the sum of GDP-deflated imports and exports (Equation (6)). Since the GDP deflator is an economy-wide price index rather than an agricultural commodity-specific deflator, these measures do not fully account for changes in agricultural product prices, commodity composition, or relative prices. Moreover, GDP-deflated monetary trade values do not represent physical trade volumes. Consequently, the estimated trade coefficients capture conditional associations with changes in aggregate GDP-deflated agricultural trade values rather than changes in physical trade quantities or commodity-specific trade flows. This distinction should be considered when interpreting the trade–environment relationships (Equation (6)):

$$\mathrm{TRADE}^{\mathrm{R}}{}_{\mathrm{it}} = \mathrm{IMP}^{\mathrm{R}}{}_{\mathrm{it}} + \mathrm{EXP}^{\mathrm{R}}{}_{\mathrm{it}} \quad (6)$$

All strictly positive continuous variables are expressed in natural logarithms. The principal variables are $\ln(EE)$, $\ln(PROD)$, $\ln(IMP^R)$, $\ln(EXP^R)$, and $\ln(TRADE^R)$.

Figure 1 illustrates the evolution of the variables employed in the econometric analysis.

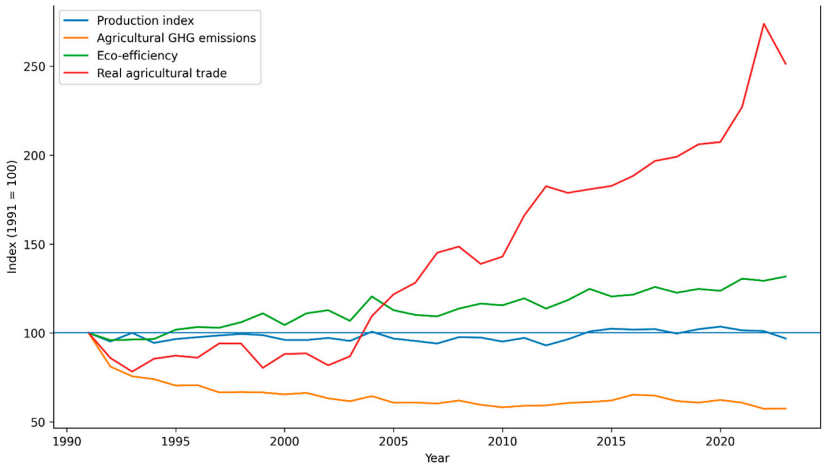


Figure 1. Evolution of agricultural production, greenhouse-gas (GHG) emissions, eco-efficiency, and real agricultural trade in the EU-27, 1991–2023. Source: Authors' calculations based on FAOSTAT data [50].

Following the aggregate trends presented in Figure 1, the country-specific series provide further insights into differences across EU member states. Figure 2 illustrates the evolution of agricultural greenhouse gas emissions across the EU-27 during 1991–2023, revealing substantial cross-country differences. Most countries exhibit declining emissions, particularly Germany, France, Poland, Greece, and Romania, while Ireland, Spain, and Portugal display relatively stable trajectories with some fluctuations.

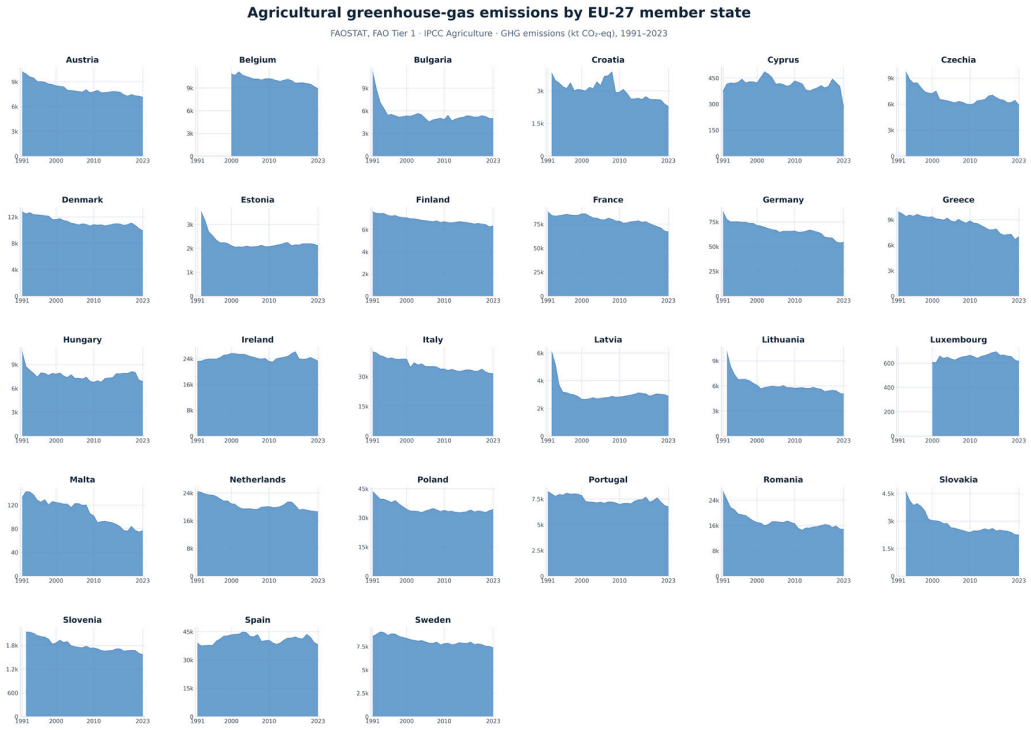


Figure 2. Agricultural greenhouse gas emissions by country EU-27, 1991–2023. Source: Authors' presentation based on FAOSTAT [50]. FAO Tier 1 emissions (CO_2-equivalent, AR5), IPCC Agriculture, kilotonnes; the country series are shown only where observations are available.

Figure 3 illustrates the evolution of agricultural eco-efficiency across the EU-27 during 1991–2023, highlighting substantial cross-country heterogeneity. Most countries exhibit an upward trend, particularly Austria, Denmark, Germany, the Netherlands, and Spain, suggesting improvements in agricultural production value relative to greenhouse gas emissions. However, several countries display considerable fluctuations and less consistent patterns. These descriptive trends indicate varying degrees of improvement in agricultural eco-efficiency but do not establish causal relationships with agricultural trade or environmental policies.

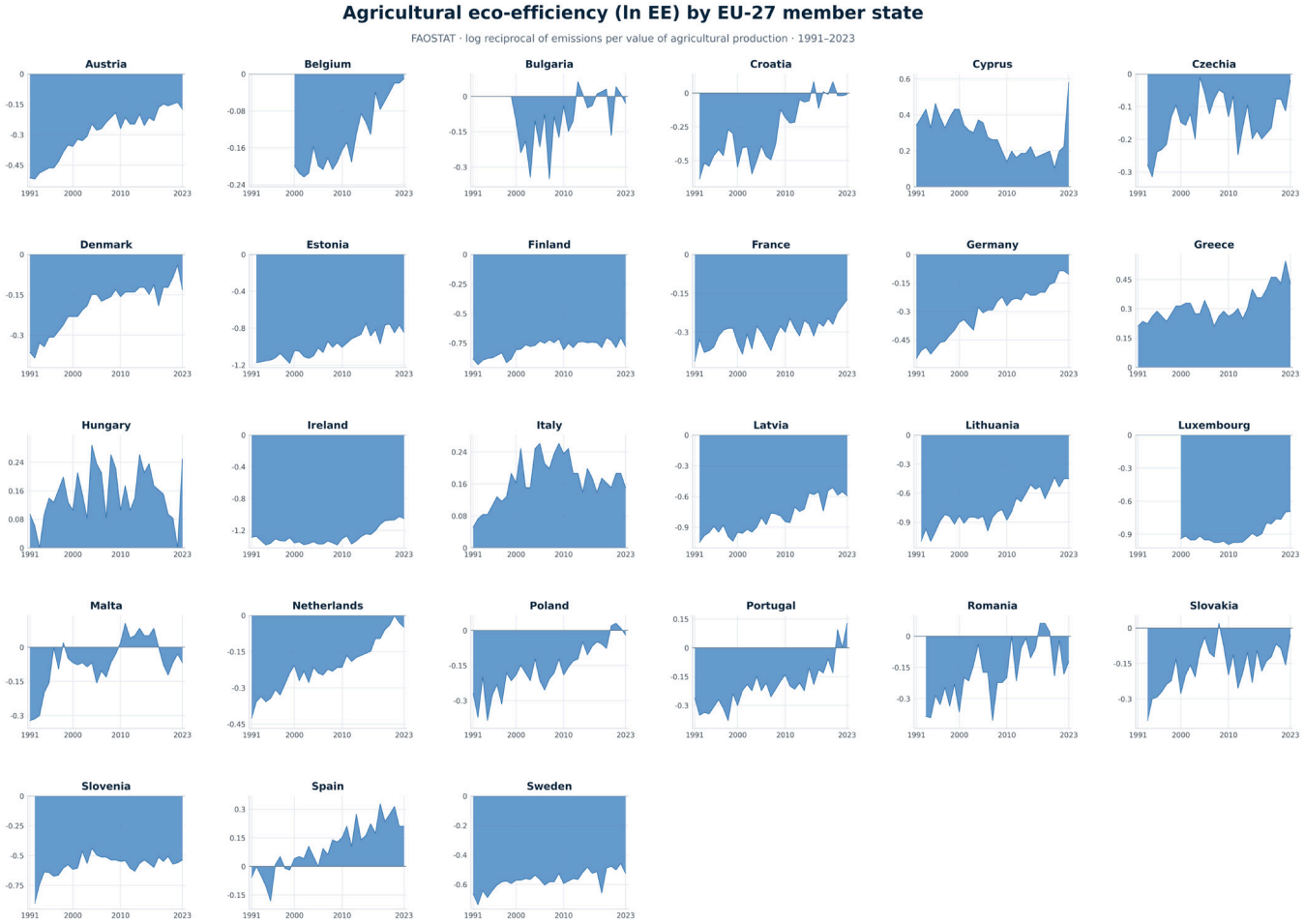


Figure 3. Agricultural eco-efficiency by country in the EU-27, 1991–2023. Source: Authors' presentation based on FAOSTAT [50]. Eco-efficiency is the reciprocal of FAOSTAT emissions per value of agricultural production (international dollars per kg CO_2-equivalent). The panels reproduce the supplied logarithmic eco-efficiency series (ln EE); values should not be read as untransformed levels.

Figure 4 illustrates country-specific developments in real agricultural trade across the EU-27 during 1991–2023. The series provide a comparative perspective on differences in trade volumes and their evolution over time, reflecting the heterogeneous positions of member states within agricultural markets. Differences in the levels and trajectories of real trade may be associated with variations in agricultural production structures, market integration, and trade specialization. However, the descriptive evidence does not establish whether changes in agricultural trade are associated with improvements or deterioration in environmental performance. This relationship is examined more systematically in the subsequent econometric analysis.

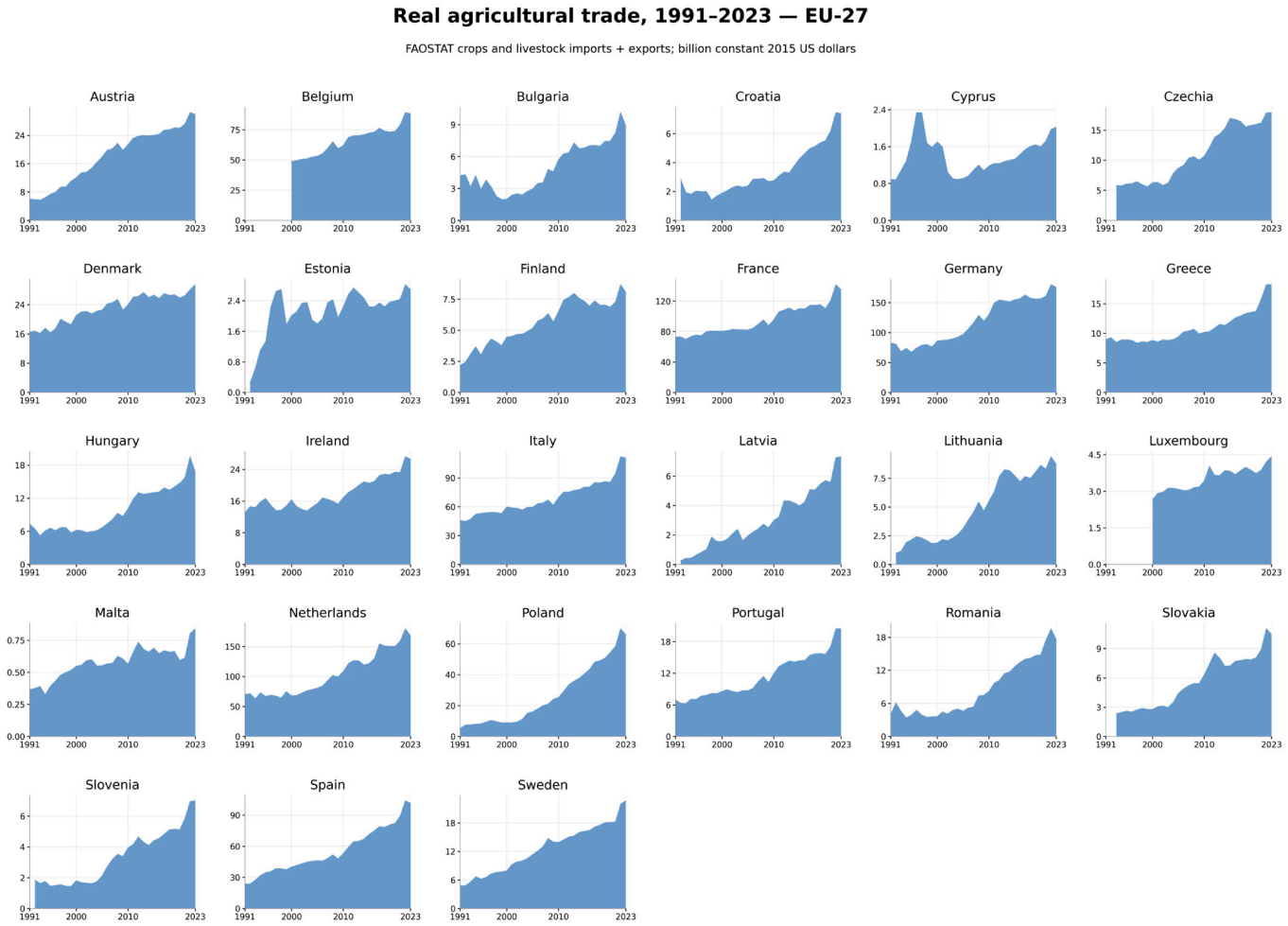


Figure 4. Real agricultural trade by country in the EU-27, 1991–2023. Source: Authors' presentation based on FAOSTAT [50]. Total trade is real agricultural imports plus real exports of crops and livestock products, recorded in thousands of constant 2015 US dollars and rescaled to billions of constant 2015 US dollars for plotting using country-specific GDP deflators. The panels reproduce the supplied real-trade series.

Figure 5 presents the evolution of agricultural greenhouse gas emissions intensity across EU member states. The country-specific series reveals considerable variation in emissions generated per unit of agricultural production value. Several countries exhibit declining emissions intensity, indicating improvements in the relationship between agricultural output value and environmental pressures. Other countries display fluctuations or comparatively stable patterns, suggesting that improvements have not occurred uniformly. Because emissions intensity and eco-efficiency are exact reciprocals, their logarithmic transformations exhibit opposite trajectories. Accordingly, the patterns in Figure 5 provide an alternative representation of those in Figure 3 rather than independent evidence of environmental improvement.

3.3. Econometric Strategy

The econometric sequence addresses cross-sectional dependence, non-stationarity and country heterogeneity. Common agricultural-policy, technological, commodity-market and macroeconomic shocks may generate correlations across countries; regressions in non-stationary levels may also be spurious in the absence of cointegration. Cross-sectional dependence is therefore assessed using Pesaran's CD test [52], integration properties using the cross-sectionally augmented IPS (CIPS) test [53], and potential long-run relationships

using Westerlund's error-correction-based cointegration tests [54]. Because the reported tests do not establish a stable cointegrating relationship, the principal estimations use logarithmic first differences. The Pesaran CD and CIPS tests were implemented in R using the plm package. The Pesaran CD statistics were calculated from pairwise correlations over available overlapping country-year observations. The CIPS procedure employed cross-sectionally augmented Dickey–Fuller regressions with two augmentation lags, including an intercept and deterministic linear trend in levels and an intercept only in first differences. The untruncated cross-sectionally augmented mean-group statistic was used, with missing observations excluded according to data availability. Westerlund's [54], error-correction-based cointegration tests included country-specific intercepts and linear trends, with lag and lead orders selected using the Akaike Information Criterion within the ranges of 1–2 lags and 0–1 leads, respectively. Long-run variance estimation employed a Bartlett kernel with a window parameter of two, while inference was based on 999 bootstrap replications to account for cross-sectional dependence. An independent Python implementation (Westerlund package, version 0.1.2) reproduced all 12 reported Westerlund test statistics to three decimal places. Although differences were observed in the bootstrap *p*-values, potentially reflecting implementation-specific resampling procedures and numerical differences, all tests yielded identical inferential conclusions, failing to reject the null hypothesis of no cointegration at the 5% significance level.

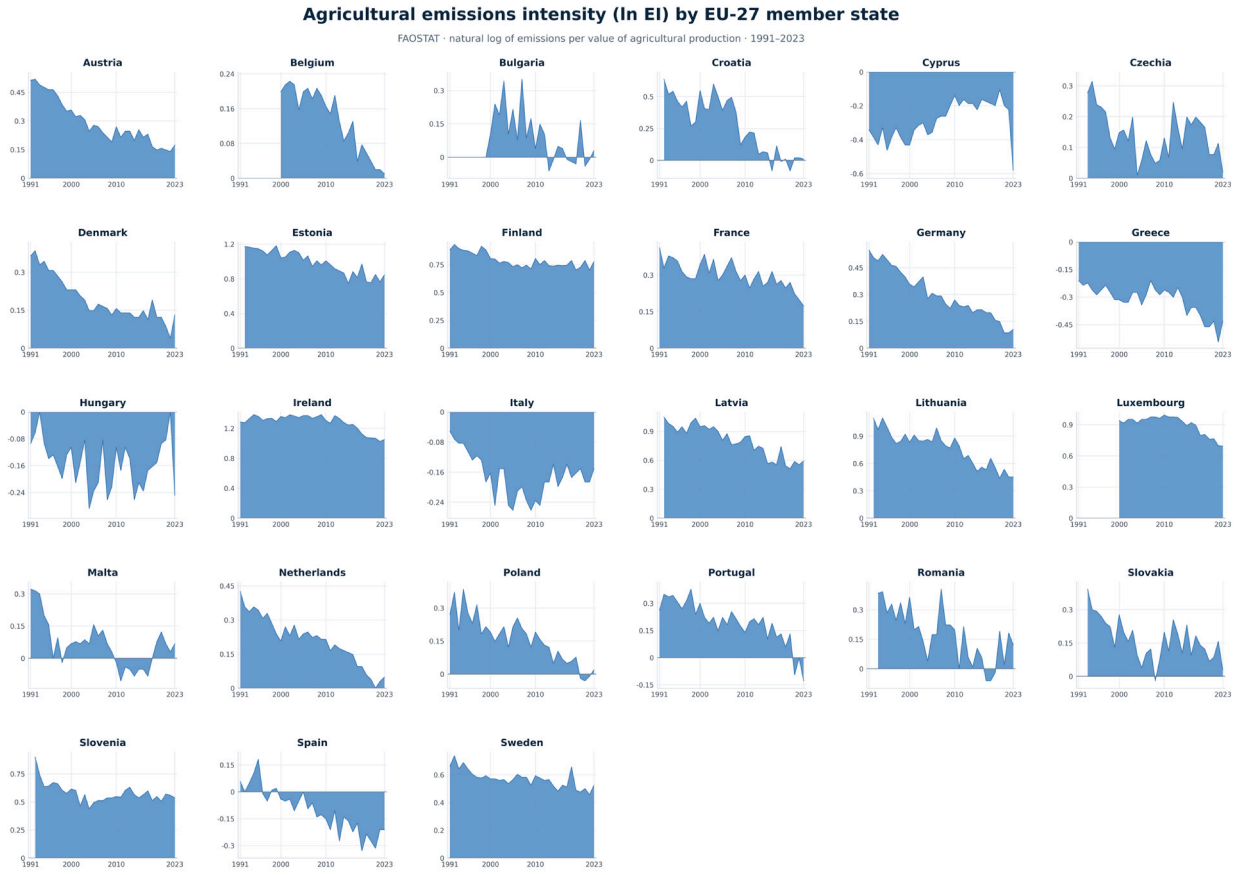


Figure 5. Agricultural greenhouse-gas emissions intensity by country in the EU-27, 1991–2023. Source: Authors' presentation based on FAOSTAT [50]. The panels reproduce the supplied logarithmic FAOSTAT emissions-intensity series (ln EI), whose untransformed unit is kg CO_2-equivalent per international dollar of agricultural production value.

Absolute agricultural GHG emissions are analyzed as an alternative outcome because the eco-efficiency measure includes agricultural production value in its construc-

tion, whereas PROD is a production-volume index; the near-accounting relationship between their annual logarithmic changes is explicitly verified in Section 3.2 and considered when interpreting the production coefficient. The methodological framework is illustrated in Figure 6.

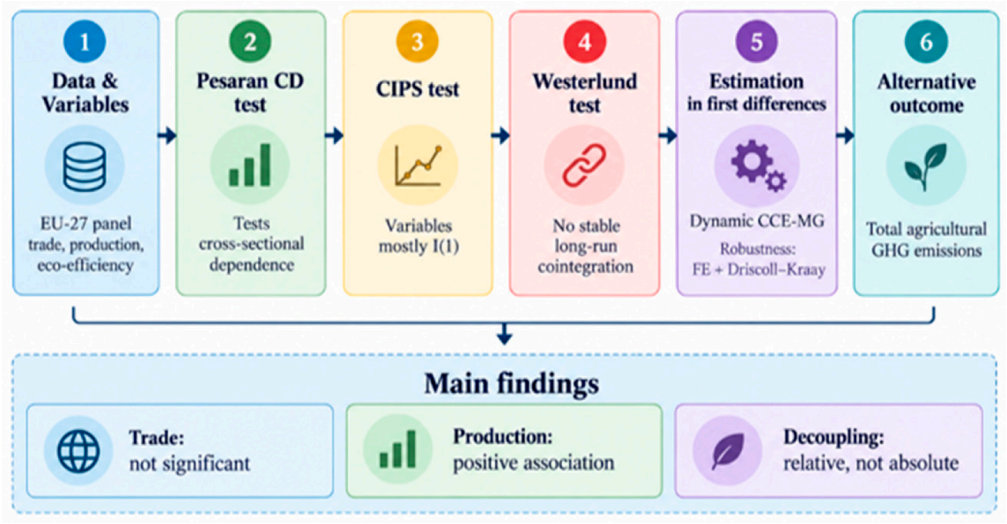


Figure 6. Overview of the empirical strategy.

3.4. Cross-Sectional Dependence and Panel Unit-Root Testing

The Pesaran CD statistic [52] tests the null hypothesis of cross-sectional independence. Its pairwise-correlation-based formulation is evaluated over available overlapping observations; rejection is interpreted as evidence of correlated country disturbances, not necessarily as identification of a specific common factor. In view of this dependence, panel unit-root properties are examined using the CIPS test [53], which averages country-specific cross-sectionally augmented Dickey–Fuller (CADF) statistics. CADF regressions include cross-sectional averages of the dependent variable and its first difference to approximate unobserved common shocks.

Levels tests include an intercept and deterministic trend; first-difference tests include an intercept. The reported implementation uses two augmentation lags in both cases and a conventional 5% significance threshold.

3.5. Westerlund Error-Correction Panel Cointegration Test

For series classified as integrated of order one, Westerlund's error-correction-based panel tests [54] investigate the null hypothesis of no cointegration. The generic country-specific representation can be written as Equation (7):

$$\Delta y_{it} = a_i + \varphi_i(y_{i,t-1} - \theta_i' x_{i,t-1}) + \Sigma_j \lambda_{ij}\Delta y_{i,t-j} + \Sigma_j \gamma_{ij}'\Delta x_{i,t-j} + u_{it} \quad (7)$$

In Equation (7), $y_{it} = \ln(EE_{it})$; x_{it} is the vector containing $\ln(TRADE^R_{it})$ and $\ln(PROD_{it})$ in the baseline model; a_i is a country-specific intercept; θ_i is the vector of long-run coefficients; φ_i is the error-correction coefficient; λ_{ij} and γ_{ij} represent short-run lag coefficients; u_{it} is the disturbance; and Δ denotes the one-year first-difference operator. The summation limits correspond to the lag order selected in the implemented test. Evidence of error correction is associated with $\varphi_i < 0$. The four reported statistics, G_t, G_a, P_t and P_a, comprise two group-mean and two panel statistics, respectively; their common null is no cointegration. Rejection criteria and interpretation depend on the statistic-specific alternatives.

Given cross-sectional dependence, inference uses a residual bootstrap with 999 replications rather than relying exclusively on asymptotic *p*-values. The principal candidate long-run relationship is specified as follows Equation (8):

$$\ln(EE_{it}) = a_i + \theta_{1i} \ln(TRADE^R_{it}) + \theta_{2i} \ln(PROD_{it}) + e_{it} \quad (8)$$

In Equation (8), the intercept a_i and long-run slopes θ_{1i} and θ_{2i} may be country-specific, and e_{it} denotes the deviation from the candidate equilibrium. Alternative candidate relationships replace total real trade with real imports or real exports. Testing for a potential long-run relationship does not imply that such a relationship has been established. In the present study, the cointegration tests do not provide evidence of a long-run equilibrium relationship; therefore, the subsequent analysis focuses on short-run dynamics using first-differenced variables: in the reported results, cointegration is not supported and the short-run analysis is therefore undertaken in differences.

3.6. Dynamic Common-Correlated-Effects Model in First Differences

The preferred short-run estimator is dynamic common-correlated-effects mean-group (dynamic CCE-MG) [55,56]. It allows country-specific short-run responses while augmenting each country equation with cross-sectional averages of the modeled variables to proxy common factors. The baseline specification is Equation (9):

$$\Delta\ln(EE_{it}) = a_i + \rho_i \Delta\ln(EE_{i,t-1}) + \beta_i \Delta\ln(TRADE^R_{it}) + \gamma_i \Delta\ln(PROD_{it}) + \delta_i' \bar{Z}_t + \Sigma_{s=1}^{2} \psi_{is}' \bar{Z}_{t-s} + \varepsilon_{it} \quad (9)$$

In Equation (9), a_i denotes the country-specific intercept; ρ_i captures persistence in annual changes in log eco-efficiency; β_i and γ_i are country-specific short-run trade and production elasticities; ε_{it} is the idiosyncratic error; and $\bar{Z}_t$ is the vector of contemporaneous cross-sectional averages of $\Delta\ln(EE)$, $\Delta\ln(TRADE^R)$ and $\Delta\ln(PROD)$. δ_i and ψ_{is} are country-specific coefficient vectors associated with the contemporaneous and lagged averages, respectively. Two lags of cross-sectional averages are included ($s = 1, 2$). The superscript R denotes constant-2015-dollar trade, and a bar denotes an average across available countries at time t.

The baseline specification includes total real agricultural trade and production; alternative specifications substitute imports and exports, individually or jointly. The country regressions include contemporaneous cross-sectional averages and two annual lags of those averages. Mean-group standard errors are calculated as the cross-country standard deviation of the estimated coefficients divided by the square root of the number of estimable countries, while two-sided *p*-values are obtained from a Student's t-distribution with $N - 1$ degrees of freedom. This conventional inference procedure relies on the assumption that country-specific coefficient estimates are sufficiently independent across countries. Although the CCE augmentation accounts for unobserved common factors through cross-sectional averages, residual cross-sectional dependence remains statistically significant in the two-lag specifications, potentially affecting the reliability of conventional mean-group standard errors and associated *p*-values. Consequently, statistical significance should be interpreted cautiously, as these standard errors do not explicitly correct for the remaining cross-country dependence. The two-lag eco-efficiency specifications include 27 country equations and 785 observations (22–30 per country), with the baseline country regressions retaining 9–17 residual degrees of freedom.

The positive coefficient of agricultural production in the eco-efficiency specification should be interpreted in light of the accounting relationship between agricultural output and emissions intensity. Because eco-efficiency incorporates agricultural production value in its construction, the estimated coefficient partly reflects this structural relationship rather than an independent effect of production on environmental performance. Accordingly, the

result describes the conditional association between production growth and the output-to-emissions relationship, without establishing that increased production independently improves environmental efficiency. To obtain a more direct assessment of environmental pressures, the analysis additionally considers total agricultural GHG emissions as the dependent variable.

To assess whether the positive production association is driven by the construction of the reciprocal emissions-intensity indicator, we estimated supplementary models using total agricultural greenhouse-gas emissions (FAOSTAT, FAO Tier 1; kt CO_2-equivalent) as the dependent variable. Unlike the reciprocal intensity measure, the logarithm of total GHG emissions contains no agricultural production-value term. The supplementary specifications model the annual change in log GHG emissions as a function of annual changes in the Gross Production Index and real agricultural trade, using country fixed effects and Driscoll–Kraay standard errors with a Bartlett kernel and a three-year bandwidth. Estimates were obtained for the full available first-difference sample (828 observations) and for the exact 785-observation sample used in the main eco-efficiency CCE-MG specification. Bandwidths of one, five and eight years were examined for sensitivity; import-only, export-only, and joint-trade specifications were additionally estimated. The emissions outcome measures absolute environmental pressure rather than technical efficiency and is therefore interpreted as a complementary measurement check, not an alternative frontier-based efficiency index.

The dynamic CCE-MG eco-efficiency models and associated sensitivity analyses were estimated in Python 3.13 using the pandas, NumPy, SciPy, and statsmodels libraries. Country-specific regressions were estimated using ordinary least squares and mean-group coefficients and standard errors were calculated from the country-level estimates. The processed panel contains no interpolated observations; regressions use complete country-year observations after first-differencing and lag construction. Cross-sectional dependence, panel unit-root, and cointegration diagnostics were conducted separately, as described in Sections 3.4 and 3.5.

3.7. Robustness Estimation

As a robustness check, the first-differenced relationship is estimated using country and year fixed effects with Driscoll–Kraay standard errors [57]. Country fixed effects capture country-specific intercept differences in the differenced specification, while year effects absorb common annual disturbances. The Driscoll–Kraay covariance estimator allows for heteroskedasticity and serial and cross-sectional dependence under its regularity conditions. A Bartlett kernel with bandwidth three years is used. The static specification is given by Equation (10):

$$\Delta \ln(EE_{it}) = a_i + \tau_t + \beta \Delta \ln(TRADE^R_{it}) + \gamma \Delta \ln(PROD_{it}) + \varepsilon_{it} \quad (10)$$

In Equation (10), a_i and τ_t denote country and year fixed effects, respectively; β and γ are common-slope coefficients for changes in real agricultural trade and production, and ε_{it} is the error term. Separate specifications substitute real imports and exports for total real trade. The fixed-effects robustness specification is static and does not include a lagged dependent variable, unlike the dy-namic CCE-MG model.

Total agricultural GHG emissions are also used as an alternative dependent variable in a dynamic CCE-MG model. This specification uses $\Delta \ln(GHG_{it})$ in place of $\Delta \ln(EE_{it})$, with analogous lagged outcome, real-trade and production regressors and cross-sectional averages. A positive production coefficient below unity is consistent with less-than-proportionate emissions growth and hence relative, not absolute, decoupling.

3.8. Finite-Sample Sensitivity and Residual-Dependence Assessment

Given the relatively short time dimension of the dynamic CCE-MG regressions and the presence of statistically detectable residual cross-sectional dependence, additional analyses were undertaken to assess finite-sample sensitivity and the reliability of conventional mean-group inference. The baseline total-trade, import, and export specifications contain 13 estimated parameters per country, while the joint import–export specification contains 17. With 22–30 observations per country, the limited residual degrees of freedom may affect coefficient precision and conventional inference.

Finite-sample sensitivity was investigated through a model-calibrated Monte Carlo simulation comprising 1100 replications per specification. The simulation retained the observed panel structure, country-specific fitted coefficients, and explanatory-variable trajectories. Disturbances were resampled using synchronized four-year blocks from the common residual observation period (2002–2023), with a degree-of-freedom adjustment to account for the number of country-specific parameters. Simulated dependent-variable trajectories were generated recursively, and contemporaneous and lagged cross-sectional averages were recalculated during re-estimation. Simulation bias was calculated as the difference between the mean simulated coefficient and the original estimate used to calibrate the data-generating process. This procedure provides a conditional assessment of finite-sample behavior rather than a formal bias correction under an independently established data-generating process.

Residual cross-sectional dependence was evaluated using the conventional pairwise-overlapping Pesaran CD statistic and an additional randomized weighted-covariance diagnostic motivated by potential incidental-parameter distortions in factor-augmented panel models [58–60]. The weighted diagnostic employed 30 independent Rademacher-weight draws over the balanced common residual period 2002–2023 (27 countries and 22 years), using a prespecified random seed. Randomization sensitivity was further examined across 100 alternative seeds, and mean absolute pairwise residual correlations were also reported. Because the weighted statistic employs an approximate normalization, it is interpreted as an exploratory CDw-style diagnostic rather than a formally validated bias-corrected test. Accordingly, non-rejection under a particular randomization is not considered evidence that residual cross-sectional dependence has been eliminated.

To assess the sensitivity of coefficient inference to remaining dependence, two complementary bootstrap procedures were implemented. First, a country-trajectory CCE bootstrap was conducted with 4000 replications. In each replication, 27 complete country time series were sampled with replacement, preserving their original temporal ordering. Model-specific contemporaneous and two-lag cross-sectional averages were recalculated, country-specific dynamic CCE regressions were re-estimated, and mean-group coefficients were recomputed. Two-sided 95% percentile confidence intervals were constructed from the resulting empirical distributions. This procedure incorporates uncertainty associated with heterogeneous country trajectories and estimated common-factor proxies, although its interpretation depends on adequately capturing common factors and sufficiently weak remaining idiosyncratic cross-sectional dependence [55,58].

Second, a synchronized moving-block-score bootstrap was implemented using the country-aggregated OLS influence contributions to the mean-group coefficients. Common sequences of annual blocks were sampled across countries to preserve empirical contemporaneous cross-country covariance. The baseline analysis employed 10,000 replications with four-year blocks, while three- and five-year blocks were examined as sensitivity checks. Because the procedure holds the fitted regressor and common-factor proxy design fixed, it constitutes a conditional, first-order approximation rather than a full re-estimation bootstrap for the heterogeneous dynamic model. Consequently, the country-trajectory and synchro-

nized block-score procedures provide complementary assessments of coefficient uncertainty, but neither guarantees fully dependence-robust inference in the presence of unresolved residual cross-sectional dependence and relatively short country-specific estimation periods.

4. Results

4.1. Cross-Sectional Dependence and Integration Properties

As reported in Table 2, Pesaran's CD test rejects cross-sectional independence for all five principal variables ($p < 0.001$). The largest CD statistics occur for real imports (89.818), total real trade (87.161), and real exports (75.566); the corresponding statistic for eco-efficiency is 50.826. These findings correlated national series but do not identify the source of the common variation.

Table 2. Cross-sectional dependence and CIPS panel unit-root tests.

Variable	Pesaran CD	CD *p*-Value	CIPS Level	*p*-Value	CIPS Δ	*p*-Value	Order
ln Production index	8.428	<0.001	−2.229	>0.10	−3.297	<0.01	I(1)
ln Eco-efficiency	50.826	<0.001	−1.645	>0.10	−3.284	<0.01	I(1)
ln Real imports	89.818	<0.001	−2.597	≈0.090	−3.087	<0.01	I(1)
ln Real exports	75.566	<0.001	−2.095	>0.10	−3.078	<0.01	I(1)
ln Total real trade	87.161	<0.001	−1.929	>0.10	−2.806	<0.01	I(1)

Notes: The null hypotheses are cross-sectional independence (Pesaran CD) and a unit root (CIPS). The logarithm of eco-efficiency is the negative of log emissions intensity, so their unit-root classifications coincide by construction.

The cross-sectional dependence documented in Table 2 motivates the use of second-generation panel tests and common-factor controls. The descriptive country trajectories in Figures 2–5 illustrate substantial differences in the levels and evolution of emissions, eco-efficiency, trade, and emissions intensity, but do not by themselves identify the drivers of these differences.

Table 2 also reports CIPS results. At the 5% level, the unit-root null is not rejected in levels for production (−2.229), eco-efficiency (−1.645), imports (−2.597), exports (−2.095), or total real trade (−1.929). For imports, the reported level test *p*-value is approximately 0.090, indicating rejection at 10% but not 5%.

In first differences, the CIPS statistics range from −3.297 to −2.806, with reported *p*-values below 0.01 for all five variables (Table 2). The variables are consequently treated as predominantly integrated of order one, I(1), within the adopted test specifications.

4.2. Westerlund Panel Cointegration Tests

Table 3 reports the Westerlund error-correction tests for eco-efficiency, agricultural production, and alternative trade measures, with bootstrap *p*-values based on 999 replications.

For the total-trade specification, the four test statistics are Gt = −2.361 (bootstrap $p = 0.942$), Ga = −7.242 ($p = 0.994$), Pt = −8.835 ($p = 0.963$), and Pa = −7.100 ($p = 0.883$). None rejects the null of no cointegration at the 5% level (Table 3).

The result is similar when total trade is replaced by imports or exports: all corresponding bootstrap *p*-values are greater than 0.83 (Table 3). Failure to reject the null is not proof that no long-run relation exists; rather, these tests provide insufficient evidence for cointegration in the examined specifications.

The Westerlund results do not provide sufficient evidence to establish a stable long-run equilibrium linking agricultural trade, production, and eco-efficiency. Plausible explanations include heterogeneous agricultural specialization, technology, environmental management, and counteracting scale, composition, and technique effects of

trade [15–18,20,29,30,34]. However, the tests do not identify these mechanisms, and failure to reject no cointegration is not proof that no long-run relationship exists. Given the predominantly I(1) properties of the variables and the absence of established cointegration, first-differenced models are used to examine conditional annual relationships between trade, agricultural production, eco-efficiency, and greenhouse-gas emissions (Tables 4–8). These models estimate short-run associations and potential relative emissions decoupling, not causal policy effects or long-run equilibrium adjustment.

Table 3. Westerlund error-correction panel cointegration tests.

Specification	Statistic	Test Statistic	Bootstrap *p*-Value
Total trade + production	Gt	−2.361	0.942
	Ga	−7.242	0.994
	Pt	−8.835	0.963
	Pa	−7.100	0.883
Imports + production	Gt	−2.303	0.941
	Ga	−6.616	0.998
	Pt	−8.687	0.905
	Pa	−6.716	0.833
Exports + production	Gt	−2.132	0.977
	Ga	−7.419	0.988
	Pt	−8.291	0.950
	Pa	−6.825	0.881

Notes: Gt and Ga are group-mean statistics; Pt and Pa are panel statistics. The null hypothesis is no cointegration. Reported *p*-values use 999 bootstrap replications.

Table 4. Dynamic CCE-MG estimates in first differences (two lags of cross-sectional averages).

Specification	Lagged ΔlnEE	Trade Variable	Δln Production	Residual CD *p*-Value
Total trade + production	−0.0478 (0.073)	−0.0114 (0.685)	0.8367 *** (<0.001)	0.0054
Imports + production	−0.0484 (0.070)	−0.0149 (0.589)	0.8285 *** (<0.001)	0.0129
Exports + production	−0.0465 (0.180)	−0.0255 (0.331)	0.8501 *** (<0.001)	0.0052
Imports + exports + production	−0.0606 (0.171)	IMP: −0.0210 (0.625) EXP: −0.0029 (0.916)	0.8326 *** (<0.001)	0.0082

Notes: Dependent variable: Δln(EE). Estimates are mean-group coefficients, with conventional standard errors calculated from cross-country variation in country-specific estimates. Two-sided *p*-values are reported in parentheses using 26 degrees of freedom; *** denotes $p < 0.01$. Each specification includes 27 country regressions and 785 observations. Significant residual cross-sectional dependence warrants caution when interpreting conventional mean-group inference.

Table 5. First-difference fixed-effects estimates with Driscoll–Kraay standard errors.

Specification	Trade Coefficient	Δln Agricultural Production	Observations
Total trade	0.031 (0.051)	0.827 *** (<0.001)	828
Imports	0.043 (0.086)	0.834 *** (<0.001)	828
Exports	0.006 (0.465)	0.826 *** (<0.001)	828
Imports + exports	IMP: 0.049 (0.112); EXP: −0.011 (0.415)	0.838 *** (<0.001)	828

Notes: *p*-values are reported in parentheses. *** $p < 0.01$. Driscoll–Kraay standard errors are calculated using a Bartlett kernel with bandwidth three.

Table 6. Dynamic CCE-MG robustness specification: total agricultural GHG emissions (dependent variable: Δln GHG).

Variable	Coefficient	SE	t-Statistic	*p*-Value
Lagged Δln GHG emissions	−0.1044	0.0512	−2.042	0.051
Δln total real agricultural trade	0.0042	0.0275	0.153	0.880
Δln agricultural production	0.1325	0.0276	4.800	<0.001

Table 7. Dynamic CCE-MG robustness specification: eco-efficiency excluding the production regressor (dependent variable: Δln EE).

Variable	Coefficient	SE	*p*-Value
Lagged Δln eco-efficiency	−0.3995 ***	0.0388	<0.001
Δln total real agricultural trade	0.0018	0.0469	0.970

Notes: *** denotes statistical significance at the 1% level. The specification excludes agricultural production to examine the sensitivity of the estimated trade–eco-efficiency relationship to the inclusion of the production regressor.

Table 8. Matched-sample lag sensitivity and confidence intervals for the dynamic CCE-MG agricultural GHG emissions model.

Variable	One lag	Two Lags
Lagged Δln GHG	−0.0213	−0.1044
Standard error	0.0450	0.0512
Total-trade coefficient	0.0079	0.0042
Trade standard error	0.0245	0.0275
Trade 95% CI	[−0.0424, 0.0582]	[−0.0524, 0.0608]
Production elasticity	0.1322	0.1325
Production standard error	0.0268	0.0276
Production 95% CI	[0.0770, 0.1874]	[0.0758, 0.1893]
Residual CD *p*-value	0.1938	0.0201
Observations	787	787
Countries	27	27
Parameters per country	10	13

Notes: Both dynamic CCE-MG specifications are estimated using the same 787 country-year observations from 27 EU countries. The one-lag and two-lag specifications differ in the number of lags of the cross-sectional averages included to account for unobserved common factors. Reported standard errors are conventional mean-group standard errors, and the 95% confidence intervals are based on Student's t inference with 26 degrees of freedom. Confidence intervals may differ slightly from calculations using the displayed values because of rounding. Residual CD *p*-values assess remaining cross-sectional dependence. The significant residual dependence in the two-lag specification ($p = 0.0201$) warrants caution when interpreting conventional confidence intervals.

4.3. Dynamic CCE-MG Estimates in First Differences

Table 4 presents the first-difference dynamic CCE-MG estimates for total agricultural trade, imports, exports, and their joint specification. All four models were reproduced using a consistent Python mean-group procedure, aligned country-year observations, and model-specific cross-sectional averages with two lags. Each specification includes 27 countries and 785 observations. Conventional mean-group *p*-values and residual Pesaran CD diagnostics are reported, with particular attention to the inferential limitations arising from remaining cross-sectional dependence.

The estimated total-trade coefficient is negative but statistically insignificant at the 5% level ($\beta = -0.0114$, $p = 0.685$). Similarly, imports ($\beta = -0.0149$, $p = 0.589$) and exports ($\beta = -0.0255$, $p = 0.331$) do not exhibit statistically significant average short-run associations with agricultural eco-efficiency when examined separately. In the joint specification,

the import and export coefficients are −0.0210 ($p = 0.625$) and −0.0029 ($p = 0.916$), respectively. These findings do not establish a statistically significant average trade–eco-efficiency association at the 5% level. However, they neither demonstrate environmental neutrality nor exclude economically meaningful or heterogeneous country-specific effects. Moreover, the separate import and export estimates do not establish equivalence between their associations, as no formal equality test was conducted.

Agricultural production is positively associated with the constructed eco-efficiency indicator across all four specifications, with coefficients ranging from 0.8285 to 0.8501 (all conventional $p < 0.001$). In the total-trade model, a 1% annual increase in the production index is associated with approximately a 0.84% contemporaneous increase in measured eco-efficiency, conditional on the included regressors and common-factor proxies. Nevertheless, this association must be interpreted cautiously because agricultural production value also enters the construction of the eco-efficiency measure. Consequently, the positive coefficient partly reflects a shared accounting component and should not be interpreted as independent causal evidence of environmental improvement. The alternative GHG emissions specifications are examined separately in Section 4.4.

The coefficients on lagged annual changes in eco-efficiency are negative across all four models but do not attain statistical significance at the 5% level using conventional mean-group inference. For the total-trade specification, the coefficient is −0.0478 ($p = 0.073$). Given the limited residual degrees of freedom and remaining cross-sectional dependence, these estimates do not provide sufficiently robust evidence for strong conclusions concerning short-run adjustment dynamics.

The matched-sample lag-order sensitivity results are presented in Supplementary Table S1. Both one- and two-lag dynamic CCE-MG specifications were estimated using the same 785 country-year observations from 27 countries. Agricultural trade remained statistically insignificant ($\beta = -0.0129$ and −0.0114, respectively), while the production coefficient remained positive ($\beta = 0.8513$ and 0.8367, respectively). Residual Pesaran CD tests indicated statistically significant cross-sectional dependence in both specifications ($p = 0.0456$ and 0.0054, respectively), suggesting that conventional mean-group inference should be interpreted cautiously.

The bootstrap sensitivity analyses provide complementary information on coefficient uncertainty (Supplementary Table S2). For total trade, the 95% country-trajectory bootstrap and synchronized four-year block-score confidence intervals are [−0.0655, 0.0448] and [−0.0514, 0.0180], respectively. The corresponding intervals are [−0.0643, 0.0438] and [−0.0678, 0.0180] for imports, and [−0.0629, 0.0373] and [−0.0496, 0.0042] for exports. All these intervals include zero, as do the corresponding intervals for both coefficients in the joint import–export specification. For agricultural production in the total-trade model, the country-trajectory and four-year block-score confidence intervals are [0.7805, 0.9064] and [0.7807, 0.8914], respectively; the corresponding production intervals also exclude zero in the other specifications. These results support the stability of the positive production association while indicating considerable uncertainty regarding the magnitude and direction of the average trade coefficients.

Block-length sensitivity generally supports this interpretation, although the five-year block-score confidence interval for the export-only coefficient [−0.0479, −0.0013] narrowly excludes zero, unlike the three-year, four-year, and country-trajectory intervals (Supplementary Table S3). Accordingly, the evidence is characterized as showing no stable average short-run trade association across the principal uncertainty procedures, rather than establishing the absence of a trade effect. The bootstrap methods also have distinct inferential limitations, as described in Section 3.8.

Residual Pesaran CD tests reject the null of cross-sectional independence for all four specifications ($p = 0.0052$–0.0129), indicating that the cross-sectional-average augmentation does not fully eliminate detectable residual dependence. The exploratory randomized CDw-style diagnostic does not reject under the prespecified weight realizations ($p = 0.228$–0.927); however, its results are sensitive to randomization, with 9–16% of 100 alternative-seed realizations rejecting at the 5% level. Mean absolute pairwise residual correlations remain approximately 0.20 (Supplementary Table S4). Therefore, non-rejection by the weighted diagnostic should not be interpreted as evidence that residual dependence has been eliminated. Taken together, these findings reinforce the need to interpret the estimated coefficients as conditional short-run associations and to regard the complementary bootstrap intervals as sensitivity assessments rather than definitive dependence-robust inference.

4.4. Fixed-Effects and Alternative-Outcome Robustness Checks

To assess the robustness of the dynamic CCE-MG findings, the analysis employs an alternative estimation approach based on first-difference fixed-effects models with Driscoll–Kraay standard errors. This estimator provides a complementary assessment of the trade–eco-efficiency relationship while accounting for heteroskedasticity and temporal and cross-sectional dependence in the error structure. Unlike the dynamic CCE-MG framework, these specifications impose common-slope coefficients across countries and do not include a lagged dependent variable.

Table 5 presents the results of the fixed-effects robustness analysis. The total-trade coefficient is positive but does not reach statistical significance at the 5% level ($\beta = 0.031$, $p = 0.051$). The import coefficient is also positive and statistically insignificant at 5% ($\beta = 0.043$, $p = 0.086$), while the export coefficient is small ($\beta = 0.006$, $p = 0.465$). When imports and exports are included jointly, neither coefficient is significant at the 5% level (imports: $\beta = 0.049$, $p = 0.112$; exports: $\beta = -0.011$, $p = 0.415$). Agricultural production remains positively associated with the constructed eco-efficiency indicator across all four specifications, with estimated coefficients of 0.826–0.838 (all $p < 0.001$). The proximity of the total-trade and import p-values to conventional thresholds, together with the assumptions underlying asymptotic inference in a relatively short panel, warrants caution: these results do not establish the absence of an economically meaningful or heterogeneous trade association.

Two further specifications address the distinction between the eco-efficiency ratio and an outcome based on total agricultural GHG emissions and assess the trade coefficient without a production control (Tables 6 and 7).

4.4.1. Alternative Dependent Variable: Agricultural GHG Emissions

The first specification replaces eco-efficiency with FAOSTAT total agricultural GHG emissions (kt CO_2-equivalent), which do not embed agricultural production value in the definition of the outcome. Its reported dynamic CCE-MG estimates are presented in Table 6.

The findings report an insignificant coefficient on total real trade (0.0042; $p = 0.880$) and a positive production coefficient (0.1325; $p < 0.001$). The latter is an elasticity estimate conditional on the included model controls, rather than evidence that production increases necessarily cause emissions to increase.

The estimated production elasticity is below unity, and the approximate 95% confidence interval [0.0758, 0.1893] is also below one (Table 8). The coefficient estimate itself is reported in Table 6. This finding is consistent with relative rather than absolute short-run emissions decoupling, conditional on model controls; it does not establish improvements in land-use sustainability.

The reported GHG model in Table 6 concerns total real trade; import-only, export-only, and joint-trade estimates are not displayed in that table and are not interpreted here.

4.4.2. Production-Control Sensitivity and Complementary Robustness Analyses

A second specification removes agricultural production from the eco-efficiency regression altogether.

Removing agricultural production from the dynamic CCE-MG eco-efficiency specification (Table 7) produces an insignificant total-trade coefficient ($\beta = 0.0018$, conventional $p = 0.970$) and a more negative coefficient on lagged eco-efficiency (-0.3995, SE = 0.0388) than in the full specification (-0.0478, SE = 0.0256; Table 4). Both regressions use the same 785 country-year observations from 27 countries, an identical lag of the dependent variable, and two lags of the cross-sectional averages. Excluding production removes both its country-specific regressor and its contemporaneous and two lagged cross-sectional averages, reducing the number of country-level parameters from 13 to nine, including the intercept. Two intermediate, matched-sample specifications indicate that removing only country-specific production yields a lag coefficient of -0.4514, whereas removing only production averages yields -0.0544. Hence, most of the change follows from omitting the country-specific production regressor. The restricted model should be interpreted as a sensitivity analysis, not as evidence of faster structural adjustment. Residual dependence remains statistically detectable (CD $p = 0.0089$), so conventional inference remains qualified.

In the dynamic CCE-MG model with total agricultural GHG emissions as the dependent variable, the estimated production elasticity is positive and statistically significant ($\beta = 0.1325$, $p < 0.001$), compared with 0.8367 in the ratio-based eco-efficiency model. Complementary first-difference fixed-effects estimates with Driscoll–Kraay standard errors confirm a positive production–emissions association, yielding an elasticity of 0.1825 (95% CI: [0.1062, 0.2588]; N = 828). Restricting the estimation to the 785 observations used in the baseline eco-efficiency CCE-MG model produces a similar estimate of 0.1638 (95% CI: [0.1048, 0.2227]). In this restricted specification, the total-trade coefficient remains statistically insignificant ($\beta = -0.0244$; 95% CI: [-0.0819, 0.0332]). Across these alternative emissions specifications, agricultural production is positively associated with absolute GHG emissions, although the estimated elasticities remain below unity. These conditional associations are consistent with relative short-run emissions decoupling but do not establish absolute emissions reductions or causal environmental improvements. Furthermore, differences between the production coefficients in the emissions and eco-efficiency models should not be interpreted as an exact accounting decomposition, since the Gross Production Index is not identical to the production-value component underlying the eco-efficiency indicator.

Taken together, Tables 4–7 show no statistically significant average total-trade coefficient at 5% across the re-estimated eco-efficiency specifications and reported alternative-outcome checks. Production has a positive conditional association with measured eco-efficiency, but the construction of the indicator may contribute to this association. The alternative GHG emissions model (Tables 6 and 8) yields a positive production–emissions elasticity below unity, consistent with relative decoupling, not absolute emissions reductions. Residual cross-sectional dependence in the eco-efficiency models remains a limitation.

As a matched-sample sensitivity analysis, the agricultural GHG emissions model was re-estimated using one rather than two lags of the cross-sectional averages, with both specifications evaluated on the same 787 country-year observations. The one-lag model produced an insignificant total-trade coefficient ($\beta = 0.0079$) and a positive production elasticity ($\beta = 0.1322$). The corresponding two-lag estimates were $\beta = 0.0042$ and $\beta = 0.1325$, respectively. The conventional residual Pesaran CD test did not reject the null of cross-sectional independence in the one-lag model ($p = 0.1938$), whereas it rejected this null

in the two-lag model ($p = 0.0201$; Table 8). However, non-rejection does not establish the absence of residual dependence. Both production elasticity estimates are positive and below unity, consistent with a conditional relationship of relative rather than absolute emissions decoupling. The conventional mean-group confidence intervals remain subject to the inferential limitations discussed above.

For the two-lag agricultural GHG emissions specification, the estimated total-trade coefficient is statistically insignificant ($\beta = 0.0042$, $p = 0.880$), with 95% country-trajectory bootstrap and synchronized four-year block-score confidence intervals of $[-0.0498, 0.0569]$ and $[-0.0221, 0.0463]$, respectively. The estimated production–emissions elasticity is positive ($\beta = 0.1325$), with corresponding confidence intervals of $[0.0742, 0.1907]$ and $[0.0951, 0.1740]$, both lying above zero and below unity. This conditional relationship is consistent with relative rather than absolute emissions decoupling, as it indicates a less-than-proportional emissions response to agricultural production changes without establishing stable or declining total emissions. The conventional t-based confidence intervals reported in Table 8 and the supplementary bootstrap intervals provide complementary assessments of statistical uncertainty. Nevertheless, the bootstrap procedures have distinct inferential limitations and should be interpreted as sensitivity analyses rather than definitive corrections for residual cross-sectional dependence.

4.5. Finite-Sample and Dependence Sensitivity Analyses

The additional analyses provide evidence on the sensitivity of the dynamic CCE-MG results to finite-sample estimation and residual cross-sectional dependence (Supplementary Tables S2–S5). The model-calibrated Monte Carlo assessment, based on 1100 replications per specification, produced relatively small simulated biases for agricultural production, ranging from −0.0082 to +0.0015 across the examined models. In the baseline total-trade specification, the simulated biases were +0.0080 for trade, −0.0036 for production. (Supplementary Table S5). Although the absolute magnitude of these biases is generally modest, the trade-related biases are non-negligible relative to the small estimated trade coefficients. The joint import–export specification exhibited greater sensitivity, particularly for the export coefficient (simulated bias = +0.0302), reflecting the greater parameterization and uncertainty of this specification. These findings indicate that finite-sample effects may be more consequential for the interpretation of trade coefficients than for the comparatively stable production associations. However, the simulated biases are conditional on the fitted data-generating process and should not be interpreted as formal corrections to the reported estimates.

The residual-dependence diagnostics provide a complementary qualification. Conventional residual Pesaran CD tests reject cross-sectional independence across the principal specifications ($p = 0.0052$–0.0201). In contrast, the exploratory randomized weighted-covariance diagnostics do not reject at the 5% level under the prespecified randomization ($p = 0.228$–0.927). Nevertheless, the results exhibit sensitivity to alternative random weights, while mean absolute pairwise residual correlations remain approximately 0.20. Consequently, the conventional CD rejections cannot be interpreted as definitive evidence of inadequate factor control, but the additional diagnostics do not establish that residual dependence has been eliminated.

The resampling sensitivity analyses generally reinforce the cautious interpretation of the main findings. For the baseline total-trade coefficient ($\beta = -0.0114$), the 95% country-trajectory bootstrap interval $[-0.0655, 0.0448]$ and synchronized four-year block-score interval $[-0.0514, 0.0180]$ both include zero. The corresponding production coefficient ($\beta = 0.8367$) remains positive, with confidence intervals excluding zero under both procedures. A qualification arises for the export-only specification, where the five-year block-score

interval narrowly excludes zero, unlike the country-trajectory and shorter-block results. Thus, the evidence does not establish a stable average trade association across the examined uncertainty procedures, but neither does it demonstrate the absence of trade effects.

In the alternative GHG emissions specification, the estimated production elasticity ($\beta = 0.1325$) remains positive and below unity, with country-trajectory and four-year block-score intervals of [0.0742, 0.1907] and [0.0951, 0.1740], respectively. These conditional sensitivity results are consistent with relative emissions decoupling but do not establish absolute emissions reductions. Given the limitations of the resampling procedures and the short country-specific time dimension, the findings should be interpreted as evidence of sensitivity patterns rather than fully dependence-robust statistical inference.

4.6. Synthesis of the Empirical Evidence

Three findings emerge from the reported estimates. First, significant cross-country dependence is observed in the underlying variables (Table 2), while the country trajectories in Figures 2–5 also illustrate heterogeneity. Second, the Westerlund statistics do not reject the null of no cointegration for the candidate relationships (Table 3).

Third, the consistently re-estimated dynamic CCE-MG eco-efficiency models show no statistically significant average aggregate trade coefficient, whereas agricultural production exhibits a strong positive conditional association (Table 4). Residual CD tests reveal detectable remaining dependence in the two-lag models. The static FE–Driscoll–Kraay specifications also exhibit positive production coefficients, while their total-trade coefficient is near the 5% significance threshold (Table 5).

The GHG alternative-outcome analysis yields a small, non-significant trade estimate and a positive production elasticity below one (Tables 6 and 8), consistent with relative, rather than absolute, decoupling. The production-excluded eco-efficiency model also yields a non-significant trade estimate (Table 7).

These are short-run, aggregate associations for the sampled countries. The estimates do not identify the effects of particular CAP reforms, trade interventions, farm technologies, or land-management measures; nor do the descriptive country figures provide causal evidence about those mechanisms.

5. Discussion: Agricultural Trade, Eco-Efficiency, and Environmental Sustainability

The findings provide evidence on conditional short-run relationships among agricultural trade, production, and environmental performance across the EU-27. The preferred dynamic CCE-MG specifications do not identify statistically significant average associations between total agricultural trade, imports, or exports and the reciprocal emissions-intensity indicator. Complementary fixed-effects models with Driscoll–Kraay standard errors generally support this finding, although the total-trade coefficient approaches the conventional 5% significance threshold ($\beta = 0.031$, $p = 0.051$). Differences in coefficient magnitudes and signs across estimators indicate that a stable average short-run trade–eco-efficiency relationship cannot be established within the examined framework.

This finding should not be interpreted as evidence that agricultural trade is environmentally neutral. Trade–environment relationships may reflect competing scale, composition, and technique mechanisms, which can differ across agricultural products, production systems, and countries. Turan [12], for example, identifies a negative long-run relationship between agricultural exports and carbon emissions in Türkiye, whereas the present analysis does not establish a comparable average short-run association across the EU-27. These findings are not necessarily contradictory, given differences in geographical coverage, observation periods, environmental indicators, and econometric specifications.

Similarly, Chen et al. [8] highlight how agricultural trade may redistribute environmental pressures through production adjustments and internationally embodied resource demands. Such mechanisms cannot be separately identified using the aggregate trade variables employed here.

Agricultural production exhibits a consistently positive association with the reciprocal emissions-intensity indicator, with estimated coefficients of approximately 0.83–0.85. However, this association must be interpreted within the accounting relationship underlying the eco-efficiency measure. As established by the empirical verification presented in Section 3.2, changes in agricultural eco-efficiency closely correspond to changes in agricultural production minus changes in total GHG emissions. This relationship arises because the emissions-intensity indicator incorporates gross agricultural production value, while the Gross Production Index is derived from the corresponding constant-price production framework. Consequently, the estimated production coefficient partly reflects the accounting structure of the dependent variable rather than an independently identified environmental effect. Its positive magnitude is consistent with a less-than-proportional emissions response relative to production growth, although an exact coefficient relationship cannot be assumed across differently specified econometric models. Accordingly, the eco-efficiency results should be interpreted as descriptive evidence of output-relative emissions performance, rather than as independent evidence that agricultural expansion improves technical or multidimensional environmental efficiency.

The specification employing total agricultural GHG emissions as the dependent variable provides the principal basis for interpreting the environmental implications of agricultural production and trade. Unlike the reciprocal emissions-intensity indicator, total GHG emissions do not incorporate agricultural production value as an accounting component, thereby avoiding the mechanical relationship identified in the eco-efficiency specification. The estimated production–emissions elasticity is positive but below unity (approximately 0.13), indicating a less-than-proportional conditional emissions response to changes in agricultural production. This pattern is consistent with relative emissions decoupling, rather than absolute reductions in emissions or a causal improvement in environmental performance. It also reinforces the distinction between production efficiency and environmental performance discussed by Richterová and Richter [5]. Although technological development, resource management, and agricultural production practices may influence these relationships [14], their individual contributions cannot be established from the present specifications.

The robustness analyses further clarify the scope and reliability of these findings. Excluding agricultural production from the eco-efficiency equation yields a statistically insignificant total-trade coefficient ($\beta = 0.002$, $p = 0.970$), while the alternative emissions-outcome analysis also identifies no statistically significant aggregate trade association. Nevertheless, residual cross-sectional dependence remains detectable in some dynamic CCE-MG specifications, warranting caution regarding conventional mean-group inference. These results therefore support the absence of a consistently identified average short-run trade association under the examined specifications, rather than demonstrating the absence of heterogeneous or indirect environmental effects.

The additional finite-sample and dependence sensitivity analyses qualify these conclusions. Model-calibrated simulations suggest that finite-sample bias is relatively limited for the production coefficient under the examined assumptions, whereas its magnitude is more consequential for the smaller trade coefficients, particularly when imports and exports are included jointly. Moreover, differences between conventional residual cross-sectional dependence tests and exploratory randomized weighted diagnostics underscore the challenges of assessing residual dependence in factor-augmented models. Although complementary

bootstrap analyses generally support the stability of the positive production association and the absence of a consistently distinguishable average trade coefficient, these findings remain conditional on the adopted resampling procedures and model assumptions. Taken together, the results highlight the importance of distinguishing accounting relationships from independently estimated environmental associations, while recognizing coefficient uncertainty, model dependence, and the limitations of causal interpretation. The principal environmental conclusion is therefore drawn from the GHG emissions specification, which provides evidence consistent with relative decoupling of agricultural production and emissions, without establishing absolute emissions reductions or causal environmental improvements.

5.1. Implications for Sustainable Agricultural Development and Environmental Policy

The findings highlight the importance of evaluating agricultural competitiveness and environmental performance through complementary indicators rather than relying on aggregate trade expansion as a proxy for environmental improvement. The absence of a consistently significant average trade association, combined with finite-sample and inferential uncertainty, does not establish that agricultural trade is environmentally neutral. Instead, it underscores the need for a more disaggregated monitoring of agricultural production, trade composition, and environmental outcomes. Policy assessment should therefore distinguish changes in GDP-deflated monetary trade values from physical commodity flows and consider both production-related emissions intensity and absolute GHG emissions.

The positive production–emissions elasticity below unity is consistent with relative decoupling within the estimated conditional framework, but it does not establish stable or declining total emissions. Consequently, agricultural sustainability monitoring would benefit from combining productivity indicators with measures of absolute emissions, nutrient balances, water use, soil conditions, and biodiversity pressures. Within the Common Agricultural Policy (CAP), relevant policy options include support for resource-efficient production practices, improved nutrient management, precision agriculture, advisory services, and low-emission technologies. However, these measures should be regarded as potential policy instruments rather than interventions whose effectiveness has been demonstrated by the present analysis.

Furthermore, the inclusion of pre-accession observations and the absence of explicitly identified CAP interventions prevent the estimation of accession-related or policy-specific effects. Future assessments of CAP effectiveness should therefore incorporate disaggregated policy expenditure, participation in environmental schemes, and appropriate counterfactual evaluation designs. Such approaches would allow agricultural productivity, environmental efficiency, and absolute emissions outcomes to be evaluated separately while accounting for differences in national agricultural structures and policy exposure.

The production-related findings also underscore the importance of distinguishing emissions intensity from absolute environmental performance. The positive association between agricultural production and the reciprocal emissions-intensity measure may partly reflect the construction of the outcome, while the alternative GHG specification indicates a positive production–emissions elasticity below unity. This result is consistent with relative decoupling but does not establish declining absolute emissions or improvements in independently measured technical environmental efficiency. Environmental monitoring would therefore benefit from jointly considering agricultural production, emissions intensity, and total GHG emissions, rather than relying on a single ratio-based indicator.

Furthermore, agricultural GHG emissions represent only one dimension of environmental sustainability. The analysis does not directly measure land-use changes, water consumption, nutrient losses, pesticide-related impacts, soil degradation, or biodiversity pressures. Consequently, the results cannot establish whether the observed relationships

correspond to improvements in overall agricultural sustainability. A broader monitoring framework incorporating these environmental dimensions would allow a more comprehensive assessment of agricultural production and trade patterns.

Finally, differences in agricultural structures, production technologies, and environmental conditions across EU member states suggest the relevance of context-specific monitoring and evaluation. However, the present models do not identify the effects of individual Common Agricultural Policy (CAP) instruments, regulatory measures, or technological interventions. Moreover, the historical sample includes observations preceding EU accession for several countries currently belonging to the EU-27. The findings therefore cannot be interpreted as direct evidence of CAP effectiveness. Instead, they offer a descriptive empirical basis for identifying relevant environmental indicators and motivating more detailed policy evaluation.

Overall, the policy relevance of the study lies in highlighting the limitations of aggregate trade measures and ratio-based environmental indicators. Distinguishing trade-related associations from commodity-specific environmental effects, relative emissions decoupling from absolute reductions, and observational relationships from causal policy impacts can contribute to more transparent and comprehensive agricultural sustainability assessment.

5.2. Limitations and Future Research

Several methodological and data-related limitations should be considered when interpreting the findings. First, the analysis relies on aggregate national observations for the countries constituting the current EU-27 over 1991–2023. Although this framework facilitates cross-country comparisons, it does not capture differences among individual commodities, farms, or production systems. Agricultural trade is measured through GDP-deflated monetary values rather than physical quantities or embodied environmental footprints. Consequently, the estimates cannot separately identify commodity-composition effects, changes in resource intensity, or environmental pressures transferred through international trade.

Second, the econometric framework estimates conditional short-run associations rather than causal effects. Potential endogeneity, omitted time-varying determinants, measurement uncertainty, and unobserved institutional or technological changes cannot be fully excluded. Although the empirical approach incorporates cross-sectional dependence diagnostics, dynamic CCE-MG estimation, and complementary fixed-effects specifications with Driscoll–Kraay standard errors, residual dependence remains detectable in some models. The limited country-level degrees of freedom also warrant caution concerning conventional mean-group inference. The robustness analyses provide complementary evidence but do not eliminate these identification and inference limitations.

Third, the reciprocal emissions-intensity indicator captures agricultural production value per unit of GHG emissions rather than independently measured technical environmental efficiency. Unlike multidimensional frontier-based approaches, including the undesirable-output SBM framework employed by Guo and Li [51], the indicator does not explicitly incorporate multiple production inputs, undesirable outputs within a production frontier, or technical inefficiency. Its construction may also contribute to the estimated positive production coefficient. The alternative total GHG specifications address part of this concern by excluding production value from the outcome, but they do not substitute for a comprehensive assessment of environmental efficiency.

Fourth, the analysis focuses on GHG-related environmental performance and does not directly examine other potentially important agricultural pressures, including water use, fertilizer runoff, soil degradation, land-use change, and biodiversity loss. Furthermore, while the results are relevant to environmental monitoring, they cannot establish the effectiveness of specific CAP reforms or other policy interventions.

An additional methodological limitation concerns the finite-sample properties of the dynamic CCE-MG estimator. The country-specific regressions contain a relatively large number of parameters compared with the available annual observations, particularly in the joint import–export specification. Although model-calibrated Monte Carlo simulations and complementary resampling procedures were employed to assess coefficient sensitivity and statistical uncertainty, these analyses do not constitute definitive finite-sample bias corrections or establish universally valid inference under arbitrary residual cross-sectional dependence. Furthermore, the exploratory randomized weighted-CD results are sensitive to the choice of random weights and do not conclusively demonstrate the elimination of residual dependence. Consequently, the estimated coefficients and associated uncertainty measures should be interpreted conditionally on the adopted specifications and diagnostic procedures. Future studies could investigate alternative factor-augmented estimators, formally validated bias-corrected dependence diagnostics, and resampling methods with established properties for heterogeneous dynamic panels with short time dimensions.

Future research could address these limitations by incorporating commodity-level trade flows, physical trade quantities, embodied emissions, and additional environmental indicators. Farm-level or regional analyses could investigate differences in agricultural technologies, resource use, and production composition. Multidimensional efficiency approaches, including undesirable-output SBM models, could provide complementary measures of environmental performance. Finally, research employing explicit policy identification strategies and detailed information on CAP implementation could examine the environmental consequences of particular interventions more directly.

6. Conclusions

This study investigated the short-run relationships among agricultural trade, agricultural production, eco-efficiency, and greenhouse-gas (GHG) emissions across the current EU-27 countries during 1991–2023. By combining dynamic common-correlated-effects mean-group estimation with complementary fixed-effects robustness checks, the analysis accounted for heterogeneous country dynamics and common cross-sectional influences while examining alternative measures of agricultural environmental performance.

The findings reveal no statistically significant average short-run association between aggregate agricultural trade and measured eco-efficiency at the 5% level across the preferred specifications. Imports and exports, examined separately, likewise exhibit statistically insignificant associations, although the results do not establish equivalence between their coefficients or the absence of commodity-specific environmental effects. Agricultural production, by contrast, is positively associated with measured eco-efficiency, although the construction of the indicator requires caution in interpreting this relationship. When total agricultural GHG emissions are considered, the estimated production elasticity is positive but substantially below unity (0.1325), consistent with relative rather than absolute emissions decoupling. This relationship is conditional on the model specification and does not establish declining total emissions or causal environmental improvements.

The principal scientific contribution lies in distinguishing the environmental associations of agricultural trade from those of domestic production while explicitly addressing the measurement limitations of ratio-based eco-efficiency indicators. By integrating complementary environmental outcomes within a heterogeneous-panel framework, the study demonstrates the importance of separating improvements in measured emissions intensity from changes in absolute environmental pressures. This distinction contributes to a more precise interpretation of the agricultural trade–environment relationship and the empirical assessment of relative emissions decoupling.

From a policy perspective, the findings highlight the importance of evaluating agricultural competitiveness and environmental performance through complementary indicators rather than relying exclusively on trade expansion or emissions-intensity improvements. Monitoring both production-related emissions intensity and absolute GHG emissions would provide a more informative basis for sustainability assessment. Nevertheless, the analysis does not identify causal effects of specific Common Agricultural Policy (CAP) instruments or other interventions.

Additional finite-sample simulations and dependence-sensitive resampling analyses broadly support the stability of the principal production associations, while highlighting greater uncertainty surrounding the estimated trade coefficients and the limitations of conventional mean-group inference.

The conclusions remain subject to limitations arising from aggregate monetary trade measures, unobserved commodity composition and environmental pressures, potential endogeneity, and remaining residual cross-sectional dependence. Future research incorporating disaggregated trade data, broader environmental indicators, and explicit policy-evaluation approaches could provide further insight into the mechanisms underlying agricultural environmental performance.

Overall, the findings demonstrate that agricultural trade expansion, improvements in measured eco-efficiency, and reductions in absolute environmental pressures are distinct outcomes that should not be treated as interchangeable indicators of sustainability. Their careful differentiation is essential for interpreting agricultural environmental performance and developing evidence-informed sustainability assessments across the EU.

Supplementary Materials: The following supporting information can be downloaded at: https://www.mdpi.com/article/10.3390/su182111423/s1, Table S1: Sensitivity of dynamic CCE-MG estimates to the number of cross-sectional augmentation lags (matched sample); Table S2: Bootstrap confidence intervals for the dynamic CCE-MG estimates; Table S3: Sensitivity of trade coefficient confidence intervals to the time-block length; Table S4: Residual cross-sectional dependence diagnostics for the dynamic CCE-MG specifications; Table S5: Model-calibrated Monte Carlo assessment of finite-sample sensitivity in the dynamic CCE-MG estimates.

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