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# Energy Transition, Economic Growth and Foreign Direct Investment: Unveiling the Renewable–Non-Renewable Energy Nexus in Saudi Arabia Using an ARDL Approach

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## Abstract

Whether renewable energy is reshaping the growth model of hydrocarbon exporters is central to sustainable development and Sustainable Development Goal 7. We examine interactions among renewable-energy consumption (REC), non-renewable energy consumption, economic growth, foreign direct investment (FDI) and trade openness in Saudi Arabia over 1980–2021. We apply bootstrap ARDL bounds tests and Toda–Yamamoto tests corrected for serial correlation and multiple testing. We find no statistically detectable long-run relationship in which REC adjusts to growth, fossil-energy use, FDI or trade; this is robust to alternative specifications and to the treatment of missing pre-1990 REC data, but inconclusive with observed data only (1990–2021). A conditional long-run relationship normalised on GDP is driven mainly by trade openness, and the positive GDP–REC association disappears once the pre-1990 imputation is controlled for. Trade openness exhibits a robust Granger-causal effect on FDI, whereas no variable, including FDI, robustly predicts REC, and causality from FDI or REC to GDP is not robust. Renewables, below 0.1% of final energy use, are not yet detectably linked to Saudi macroeconomic dynamics: the data show no evidence that low-carbon energy follows from growth or FDI and cannot indicate whether the Vision 2030 targets will be met, so sustainability progress must be monitored directly.

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## 1. Introduction

Saudi Arabia is the world's largest crude-oil exporter and one of the most energy-intensive economies. Fossil fuels still account for almost all of its primary energy supply—oil 64.2% and natural gas 35.7%—whereas solar, wind and other renewable sources contribute about 0.1% [1]. Electricity and heat production and transport generate 49% and 26% of the country's energy-related $CO_2$ emissions, respectively, and Saudi Arabia accounts for about 1.6% of global energy-related $CO_2$ emissions [1]. At the same time, Vision 2030 has placed economic diversification, the deployment of renewable energy, the attraction of foreign investment and the reduction of hydrocarbon dependence at the centre of the national development strategy. These objectives are also commitments to sustainable development: the United Nations 2030 Agenda calls for a substantial increase in the share of renewable energy in the global energy mix (Sustainable Development Goal (SDG) target 7.2), for sustained per capita growth that is progressively decoupled from environmental degradation (SDG targets 8.1 and 8.4) and for urgent action to combat climate change (SDG 13) [2], and Saudi Arabia has announced the objectives of net-zero greenhouse-gas emissions by 2060 and of a 50% share of renewable sources in its electricity capacity by 2030 [3]. Whether the macroeconomy and the energy mix are moving together in a way that supports this strategy is therefore a first-order policy question.

The interactions examined in this study are therefore central to the sustainability of Saudi development, because each variable relates to a dimension of sustainable development. The share of renewables in final energy consumption is the official indicator used to monitor SDG target 7.2 (indicator 7.2.1) [4]; fossil-energy use is the main source of the country's greenhouse-gas emissions and of the environmental pressure of growth; GDP per capita captures the economic dimension of sustained growth; and FDI and trade openness are the external channels through which finance, technology and diversification opportunities can be mobilised for the transition. Testing whether these variables are linked in the long run and in a predictive sense is a way of measuring whether the economy is moving toward a sustainable energy–growth path, in which growth, investment and openness support the deployment of renewables, or whether the renewable transition remains disconnected from macroeconomic dynamics. The answer matters for the design and monitoring of sustainability policies in hydrocarbon-exporting economies, where the transition must reconcile export revenues, energy security and decarbonisation.

Economic theory suggests several channels through which renewable and non-renewable-energy use, output, foreign direct investment (FDI) and trade openness may interact. Non-renewable energy is an essential input for industrial production, transport and hydrocarbon exports, so output and fossil-fuel use are expected to move together in a resource-based economy. Renewable energy may raise output by diversifying the energy supply and releasing oil for exports, but it may also be a consequence of growth if higher income and fiscal revenues are what fund renewable energy projects. FDI can foster renewable deployment through technology transfer and green finance, but it can also expand energy-intensive activities; similarly, trade openness can operate through scale, composition and technique effects with opposite implications for the energy mix. Which of these channels dominates is an empirical question, and the answer is likely to be country-specific.

The empirical energy–growth literature, organised around the growth, conservation, feedback and neutrality hypotheses, has not reached a consensus [5]. Three features limit what is known about Saudi Arabia in particular. First, most country studies examine aggregate energy, electricity or oil-product consumption [6–8] and therefore cannot tell whether renewable and non-renewable energy respond differently to the macroeconomy. Second, the few studies that disaggregate the energy mix for Saudi Arabia do not model it jointly with FDI and trade openness [9], although these external-sector variables are the channels through which Vision 2030 expects the transition to be financed. Third, inference is often based on small samples with mixed-integration orders and structural breaks (oil-price collapses, the 2008–2009 financial crisis, the 2014–2016 oil-price slump, the launch of Vision 2030 and COVID-19), which calls for careful distinction between long-run-level relationships and predictive causality, and for explicit robustness checks.

This study addresses these gaps and makes three contributions. First, it analyses renewable and non-renewable-energy consumption, economic growth, FDI and trade openness jointly rather than in separate bivariate or trivariate models. This joint treatment is informative for three reasons: (i) because renewable and non-renewable-energy are components of the same energy system, modelling them together shows whether renewables respond to the macroeconomy in the same way as fossil energy or follow a distinct path, which studies of aggregate energy cannot reveal; (ii) because FDI and trade are the channels through which Vision 2030 expects capital and technology for the transition to be acquired, including them shows whether these channels are linked to renewable deployment, to fossil-energy use or only to growth in general; and (iii) because omitting relevant variables can create spurious relationships or conceal genuine ones [5], the joint system reduces omitted-variable bias in the long-run and causality tests. Second, it asks whether, in the largest hydrocarbon exporter at an early stage of its energy transition, renewable energy use is driven by growth and foreign investment (a demand-following transition), is itself a source of growth (an energy-led transition), or is still unrelated to macroeconomic dynamics (neutrality), and whether FDI and trade act as channels of renewable diffusion or of growth more generally; in doing so, it extends previous Saudi evidence on oil products and growth [8] to the renewable/non-renewable composition of energy use and to the external sector, using data for 1980–2021. Third, methodologically, it applies the bootstrap ARDL bounds test [10], which provides finite-sample inference and guards against degenerate cases, together with Toda–Yamamoto causality tests corrected for serial correlation and multiple testing, and it assesses robustness to lag selection, structural breaks, the sample period and the treatment of missing renewable-energy data. The analysis is guided by the following research questions:

1. Is there a long-run-level relationship among renewable energy consumption, non-renewable energy consumption, economic growth, FDI and trade openness in Saudi Arabia?
2. What are the long-run and short-run associations between renewable-energy consumption and its determinants?
3. What is the direction of Granger (predictive) causality among these variables?
4. What policy implications follow for Saudi Arabia's sustainable energy transition and economic diversification under Vision 2030?

The objective of the study is therefore to determine whether renewable-energy consumption in Saudi Arabia is linked to economic growth, fossil-energy use, FDI and trade openness in the long run (RQ1 and RQ2) and in a predictive sense (RQ3), and to derive policy implications only to the extent that such links are established (RQ4). Two complementary methods are used because they answer different questions. The bootstrap ARDL bounds test answers RQ1 and RQ2: it tests whether a long-run-level relationship exists and identifies which variable adjusts to it; it is valid for a mixture of I(0) and I(1) variables;

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and the bootstrap provides finite-sample critical values and detects degenerate cases [10]. The Toda–Yamamoto procedure answers RQ3: it treats all five variables as endogenous and tests the direction of predictive (Granger) causality within a level VAR framework, without pre-testing for cointegration (Section 3.6), and thus complements the single-equation ARDL tests, which condition on the regressors and are silent on feedback among them. Agreement between the two approaches strengthens a conclusion, whereas disagreement indicates that a result should be treated with caution. By establishing which links are and are not supported by the data, the study also provides a baseline against which progress toward SDG 7 and the national sustainability targets can be monitored as new data become available.

The remainder of the paper is organised as follows. Section 2 synthesises the related literature and identifies the research gap. Section 3 describes the data and the econometric strategy. Section 4 reports the empirical results, the robustness checks and the sensitivity of the results to the treatment of the data. Section 5 discusses the economic interpretation of the findings, and Section 6 concludes with the implications for sustainable development, the policy implications and limitations of the study.

## 2. Literature Review

This section synthesises the evidence most directly relevant to the research questions. Section 2.1 reviews the aggregate energy–growth nexus and the reasons for why its results diverge; Section 2.2 examines what is gained by separating renewable from non-renewable energy; Section 2.3 considers FDI and trade openness as channels linking the energy mix to the macroeconomy; and Section 2.4 summarises the evidence for Saudi Arabia and states the research gap. Table 1 summarises the studies most closely related to the present one.

### *2.1. The Energy–Growth Nexus: Hypotheses and Sources of Divergent Evidence*

Four hypotheses structure the literature. Under the growth hypothesis, energy use causes output, so energy conservation may harm growth; under the conservation hypothesis, causality runs from output to energy; the feedback hypothesis posits bidirectional causality; and the neutrality hypothesis posits no causal link [5]. The seminal bivariate study of Kraft and Kraft [11] found causality from GNP to energy consumption in the United States, supporting the conservation hypothesis. Stern [12,13], however, showed that once capital and labour are included and energy is quality-adjusted, energy Granger-causes GDP, and Narayan and Smyth [14] found that energy consumption and capital drive real GDP in the long run in the G7 once structural breaks are accounted for. Apergis and Payne [15] likewise reported long-run causality from energy to output in Central America in a multivariate panel framework.

The comparison of these studies reveals a consistent pattern rather than a mere list of contradictory results: bivariate specifications and short samples tend to produce conservation or neutrality results, whereas multivariate models that control for other production factors and for structural breaks more often support the growth or feedback hypotheses [5]. The divergence therefore stems largely from omitted variables, structural instability and specification choices such as lag length. This has two implications for the present study: the energy mix should be analysed in a multivariate framework, and the robustness of the results to lag selection and structural breaks must be verified explicitly.

### *2.2. Renewable Versus Non-Renewable Energy and Growth*

Disaggregating energy by source has shown that the two components do not play the same role. For OECD countries, Apergis and Payne [16] found bidirectional causality between renewable-energy consumption and growth, and Bhattacharya et al. [17]

reported a significant positive effect of renewable energy on output in 57% of the 38 leading renewable-energy-consuming countries. Kasperowicz et al. [18] found a long-run positive link in 29 European countries, whereas Smolović et al. [19] found a positive long-run effect in the older EU members but a negative short-run effect in the new member states. In contrast, Maji and Sulaiman [20] found that renewable energy—largely traditional biomass—reduced growth in West Africa, Lee and Jung [21] reported a negative effect for South Korea, and Tuna and Tuna [22] found that only non-renewable energy is linked to growth in the ASEAN-5. Rahman and Velayutham [23] likewise found that non-renewable energy is more closely associated with growth than renewable energy in South Asia, while Cevik et al. [24] showed that the nexus in the United States is regime-dependent.

Taken together, these findings indicate that renewable energy tends to contribute to growth where it represents a substantial and technologically modern share of the energy mix, whereas where its share is small or dominated by traditional sources its effect is neutral or negative and non-renewable energy remains the growth-relevant input. In several such economies, causality has been found to run from income to renewable energy rather than the reverse [9,25]. Given that renewables account for about 0.1% of Saudi primary energy [1], this literature leads us to expect—as a hypothesis to be tested rather than as a premise—that renewable-energy use in Saudi Arabia is more likely to respond to economic growth, or to be unrelated to it, than to drive it, and that the interaction between the renewable and non-renewable components may reflect substitution within total energy use [26].

Three factors help to explain why the renewable–growth results differ across countries. The first is the size and nature of the renewable share: in OECD and European economies, the variable mainly measures modern renewables (hydro, wind and solar) that compete with fossil fuels in power generation [16,18], whereas in West Africa it is dominated by traditional biomass, whose use declines as income rises [20]; the same label therefore captures different economic processes. The second is the role of fossil energy in the economy: where fossil fuels remain the dominant and cheapest input, non-renewable energy is the growth-relevant component and renewables are too small to affect aggregate output [22,23]. The third is the policy and market regime: when renewable deployment is driven by discrete public programmes rather than by market responses to prices and income, renewables expand in steps that are not tied to short-run fluctuations in income, and causal links become regime-dependent or undetectable [24]. Methodological differences—panel versus country-specific designs, linear versus regime-switching models and the treatment of structural breaks—add to these substantive sources of heterogeneity [5,22,24]. For Saudi Arabia, these differences imply that results obtained for economies with large and modern renewable shares cannot be transferred directly: the renewable share is tiny, renewables do not yet compete with oil and gas at scale in power generation, and deployment has been driven by state programmes. Neutrality, or causality running from income to renewables, would therefore be consistent with the international evidence, whereas a strong growth effect of renewables would be surprising.

### *2.3. FDI and Trade Openness as Transmission Channels*

FDI and trade openness are the main external channels through which technology and capital for the energy transition can be acquired. Fan and Hao [27] found a long-run equilibrium relationship among renewable-energy consumption, GDP per capita and FDI across Chinese provinces, but no significant short-run effect of FDI on renewable energy. Using a bootstrap ARDL approach, Ghazouani et al. [28] found that the links among trade openness, renewable energy and growth in Asia–Pacific countries are strongly country-specific. Zafar et al. [29] showed that trade openness, like non-renewable energy, increases $CO_2$ emissions in emerging economies, while Rasoulinezhad and Saboori [30] found short-

run causality from trade openness to emissions in the Commonwealth of Independent States. The common thread is that the effect of FDI and trade on the energy mix depends on the sectoral composition of foreign investment and exports: when FDI and trade are concentrated in energy-intensive activities, they may expand fossil-energy use rather than renewable use. This question has received little attention in hydrocarbon-exporting economies, where FDI has historically been oriented toward petrochemicals and heavy industry. The divergent findings therefore have a clear interpretation: FDI and trade are not inherently 'green' or 'brown', and their association with the energy mix depends on the sectors they finance. For Saudi Arabia, this implies that a link between FDI and renewable energy cannot be presumed; it is an empirical question that the joint model is designed to test.

### *2.4. Evidence for Saudi Arabia and Research Gap*

Studies on Saudi Arabia consistently show that economic activity is closely tied to hydrocarbon energy. Mahalik et al. [6] found that financial development and growth increase energy demand, Belal et al. [7] found unidirectional causality from growth to industrial electricity consumption and neutrality for residential consumption, and Daly et al. [8], using ARDL and Toda–Yamamoto tests for 1970–2021, found a positive long-run relationship between oil-product consumption and GDP with causality from several oil products to growth. For renewables, Kahia et al. [9] reported unidirectional causality from growth to renewable-energy consumption over 1990–2016 in a simultaneous-equation framework. Recent studies have begun to examine the energy transition under Vision 2030 more directly. Amran et al. [31] review the current status and prospects of renewable energy in Saudi Arabia and show that, despite large solar and wind potential, renewable deployment has been recent and driven by national programmes. Derouez [32], using ARDL and VECM models for 1990–2024, finds that economic growth and technological progress are positively, and fossil-fuel consumption negatively, associated with solar electricity production. Elmahgop et al. [33], using ARDL and wavelet-coherence methods for 1990–2024, find that the effect of the renewable share on real GDP depends on the level of financial development: it is positive but insignificant at low levels and negative at high levels. Both studies start in 1990, the first year for which renewable consumption data are available, and neither model renewable and non-renewable energy jointly with FDI and trade openness.

Three conclusions emerge. What we know is that Saudi growth has been closely linked to fossil energy and that renewable energy remains marginal. What we do not know is (i) whether renewable and non-renewable energy respond differently to growth, FDI and trade when these variables are modelled jointly over a long period that includes the major oil-market shocks and the first years of Vision 2030; (ii) whether FDI and trade act as channels of renewable diffusion or of fossil-energy expansion; and (iii) whether the direction of the links is robust to the small-sample, mixed-integration and structural-break issues that characterise the Saudi data. The present study is designed to address these three questions. It differs from the recent Saudi studies [9,32,33] in three respects: it models renewable and non-renewable energy jointly with FDI and trade openness; it combines bootstrap ARDL tests under all five normalisations with Toda–Yamamoto tests that are corrected for serial correlation and multiple testing; and it extends the sample back to 1980 while examining explicitly how the treatment of the missing pre-1990 renewable data affects the conclusions.

**Table 1.** Selected empirical studies most closely related to the present study.

| Study | Period | Country/Region | Method | Main Finding |
|---|---|---|---|---|
| Kraft and Kraft [11] | 1947–1974 | United States | Bivariate Granger causality | GDP → E (conservation) |
| Stern [12,13] | 1948–1990 | United States | Multivariate VAR, cointegration | E → GDP (growth) |
| Narayan and Smyth [14] | 1972–2002 | G7 | Panel cointegration with structural breaks | E → GDP in the long run (growth) |
| Apergis and Payne [16] | 1980–2007 | OECD countries | Panel cointegration, ECM | REC ↔ GDP (feedback) |
| Tuna and Tuna [22] | 1980–2015 | ASEAN-5 | Asymmetric causality | NREC → GDP; REC ↛ GDP |
| Rahman and Velayutham [23] | 1990–2018 | South Asia (5) | Panel cointegration and causality | NREC more strongly linked to GDP than REC |
| Fan and Hao [27] | 2000–2015 | 31 Chinese provinces | Panel cointegration, Granger | Long-run REC–GDP–FDI link; FDI ↛ REC (short run) |
| Ghazouani et al. [28] | 1980–2017 | Asia–Pacific (7) | Bootstrap ARDL | Country-specific TR–REC–GDP links |
| Mahalik et al. [6] | 1971–2011 | Saudi Arabia | Cointegration | Financial development, GDP → E |
| Belal et al. [7] | 1990–2019 | Saudi Arabia | VECM | GDP → industrial electricity; neutrality (residential) |
| Kahia et al. [9] | 1990–2016 | Saudi Arabia | Simultaneous equations | GDP → REC |
| Daly et al. [8] | 1970–2021 | Saudi Arabia | ARDL, Toda–Yamamoto | Oil products → GDP; positive long-run link |
| Derouez [32] | 1990–2024 | Saudi Arabia | ARDL, VECM | GDP, technology (+) and fossil fuels (−) → solar electricity production |
| Elmahgop et al. [33] | 1990–2024 | Saudi Arabia | ARDL, wavelet coherence | REC effect on GDP depends on financial development |

Note: E = total energy consumption; REC/NREC = renewable/non-renewable-energy consumption; TR = trade openness; → unidirectional causality; ↔ bidirectional causality; and ↛ no causality.

## 3. Data and Methodology

### *3.1. Data*

The study uses annual data for Saudi Arabia over 1980–2021 on five variables, defined in Table 2: renewable-energy consumption (REC), measured as the share of renewable energy in total final energy consumption (%), which corresponds to SDG indicator 7.2.1 [4]; non-renewable-energy consumption (NREC), measured by energy use per capita (kilograms of oil equivalent per capita; WDI indicator EG.USE.PCAP.KG.OE), which in Saudi Arabia consists almost entirely of fossil fuels, and entered in natural logarithms (lnNREC); economic growth (GDP), proxied by real GDP per capita (constant 2015 US$); foreign direct investment (FDI), measured as net inflows (% of GDP); and trade openness (TR), measured as the sum of exports and imports of goods and services (% of GDP). The series were obtained from the World Bank's World Development Indicators (WDIs) [34]; the provenance of the NREC series is documented in detail in the next paragraph. The 42-year span covers the oil-price collapse of 1986, the 2008–2009 global financial crisis, the 2014–2016 oil-price slump, the launch of Vision 2030 and the COVID-19 shock, and therefore allows the interactions among the variables to be examined over several oil-market cycles.

Two data issues require explicit treatment. First, REC is not reported for Saudi Arabia before 1990. In the baseline sample, the missing values for 1980–1989 were set to zero. This choice preserves the full 1980–2021 sample, which includes the 1986 oil-price collapse and

the low-growth years that followed and therefore increases the power of the tests, and it reflects the fact that modern renewable sources were negligible in Saudi Arabia in the 1980s. It is, however, an imputation, not an observation: the absence of data does not show that renewable consumption was exactly zero, and the imputation creates a jump in REC in 1990 that may affect the unit-root, bounds and causality tests. We therefore do not rely on the imputed series alone. Section 4.8 reports the main results under four alternatives: (i) the baseline series with a step dummy for 1990, which absorbs the jump created by the imputation; (ii) a backcast series in which the 1980–1989 values are set equal to the first observed value (1990); (iii) the observed data only (1990–2021), with REC in levels; and (iv) the observed data with REC in logarithms. Because the observed-data sample contains only 32 annual observations, it is analysed as a complete parallel set of estimates rather than adopted as the baseline. Second, NREC is measured in physical units per capita, not as a percentage of the total: the mean of lnNREC (8.60; Table 3) corresponds to about 5400 kg of oil equivalent per capita. Its provenance is as follows. For 1980–2014, the values of our dataset coincide, to the second decimal, with the WDI EG.USE.PCAP.KG.OE (energy use, kg of oil equivalent per capita) in the edition based on IEA Statistics (© OECD/IEA 2014), whose last observation is 2014 (e.g., 3209.05 in 1980 and 6905.84 in 2014). That edition contains no values after 2014, and the current WDI edition, which is based on revised IEA energy balances, reports different levels (7959.91 in 2014 and 7796.22 in 2015; retrieved on 23 October 2026). The 2015–2021 values of our dataset (from 10,546.53 in 2016 to 10,634.64 in 2021) could therefore not be traced to the same WDI series; they were appended when the dataset was compiled, and their higher level explains the shift after 2014 (Zivot–Andrews break in 2014 over the full sample, Table 6, and in 2015 over 1990–2021). The 2015 entry of the compiled dataset (95,432.55) does not appear in either WDI edition. Because it is about ten times larger than the adjacent entries (6905.84 in 2014 and 10,546.53 in 2016), it was attributed to a misplaced decimal separator during data compilation, not to an error in the WDI source, and was set to 9543.25 in the baseline. As this corrected value cannot be verified against a published source, the conclusions should not depend on it. We therefore re-estimated the models (i) with the 2015 value replaced by the log-linear interpolation of the adjacent entries (8534.21); (ii) with a step dummy from 2015; (iii) with a fully traceable series that uses the IEA-based WDI edition for 1980–2014 and extends it to 2015–2021 with the growth rates of the current WDI edition (2015 value: 6763.83); and (iv), for 1990–2021, with the current WDI series itself [34]. The original and replacement values are listed in the note to Table 12, and the results are reported in Section 4.8.

**Table 2.** Variables and definitions.

| Variable | Description | Measurement Unit and Transformation | Source |
|---|---|---|---|
| GDP | Real GDP per capita | Constant 2015 US$; in natural logarithms (lnGDP) | WDI [34] |
| REC | Renewable energy consumption | % of total final energy consumption; in levels. Available from 1990; set to zero for 1980–1989 in the baseline (see Section 3.1) | WDI [34] |
| NREC | Non-renewable energy consumption | Energy use per capita (WDI EG.USE.PCAP.KG.OE, IEA-based edition, 1980–2014; for 2015–2021 see Section 3.1), kg of oil equivalent per capita; in natural logarithms (lnNREC) | WDI [34] |
| FDI | Foreign direct investment, net inflows | Net inflows of equity capital, reinvested earnings and other capital from foreign direct investors, % of GDP; in levels | WDI [34] |
| TR | Trade openness | Exports plus imports of goods and services, % of GDP; in natural logarithms (lnTR) | WDI [34] |

Source: World Bank, World Development Indicators (https://databank.worldbank.org/source/world-development-indicators) (accessed on 3 October 2026).

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### *3.2. Econometric Strategy*

The empirical strategy consists of four sequential steps, each answering a distinct question. Step 1 (unit-root tests) determines the order of integration of each series and verifies that none is I(2), which is the condition under which the ARDL bounds test is valid. Step 2 (bootstrap ARDL bounds tests) addresses RQ1, i.e., whether a long-run-level relationship exists and which variable adjusts to it. Step 3 (long-run coefficients and error-correction or first-difference models) addresses RQ2. Step 4 (Toda–Yamamoto causality tests) addresses RQ3 by examining the direction of Granger causality in a system in which all variables are treated as endogenous. The ARDL framework is therefore the main estimation strategy, and the Toda–Yamamoto procedure is the only causality test used. Robustness checks and the sensitivity analyses for the data issues discussed in Section 3.1 are described in Section 3.6.

The baseline model treats REC as the dependent variable, consistent with the objective of explaining renewable-energy use during the transition. The bounds tests are also computed under the four alternative normalisations, in which NREC, GDP, FDI and TR are in turn the dependent variable. These five equations are estimated separately as conditional single-equation models; they are neither a system of simultaneous equations nor five independent structural models. Their role is diagnostic: the bounds test of Pesaran et al. [35] assumes that the regressors are long-run-forcing, so that the null hypothesis of no level relationship should be rejected only in the normalisation whose dependent variable adjusts to the long-run relationship. Comparing the five normalisations therefore indicates which variables error-correct; the direction of short-run predictive links is analysed separately with the Toda–Yamamoto procedure. The Johansen [36] system cointegration test is not used, because it requires all series to be integrated of the same order, I(1), which is not the case here (Section 4.2). A further point concerns the interpretation of the GDP normalisation. When the dependent variable is I(0), as GDP is over the full sample (Section 4.2), a significant $t_{dep}$ may simply reflect its stationarity. Rejection of the $F_{ind}$ null then indicates that the lagged levels of the regressors help to explain GDP conditional on its own past, i.e., that GDP reverts toward a level that co-moves with the regressors. We therefore describe such a result as a conditional long-run (level) relationship normalised on GDP, and not as evidence that GDP adjusts to an equilibrium in the sense of cointegration among I(1) variables.

### *3.3. Unit-Root Testing Procedure*

The order of integration is determined with the augmented Dickey–Fuller (ADF) test [37], with lag length selected by the Schwarz information criterion (SIC), and the Phillips–Perron (PP) test [38], which corrects non-parametrically for serial correlation and heteroskedasticity; both are computed using two specifications: one with a constant and another with a constant and a linear trend. Because conventional tests lose power in the presence of structural breaks, we also apply the Zivot and Andrews [39] test, which allows for one endogenously determined break in the intercept (Model A) or in the intercept and trend (Model C), with 15% trimming and lag length selected by the SIC. In all tests, the null hypothesis is that the series has a unit root.

### *3.4. Model Specification*

Following previous studies of the energy–growth–investment nexus [8,27,28], renewable-energy consumption is modelled as a function of economic growth, non-renewable-energy consumption, FDI and trade openness:

$$REC_t = f(GDP_t, NREC_t, FDI_t, TR_t) \quad (1)$$

The empirical counterpart of Equation (1) is:

$$REC_t = \alpha + \beta_1\, lnGDP_t + \beta_2\, lnNREC_t + \beta_3\, FDI_t + \beta_4\, lnTR_t + \varepsilon_t \quad (2)$$

where $\alpha$ is the intercept, $\beta_1 - \beta_4$ are the long-run coefficients, t = 1980, …, 2021, and $\varepsilon_t$ is an error term assumed to be serially uncorrelated and homoskedastic. Because REC and FDI enter in levels and GDP, NREC and TR in logarithms, $\beta_1, \beta_2$ and $\beta_4$ measure the change in REC associated with a 1% change in the corresponding regressor, and $\beta_3$ measures the change in REC associated with a one-percentage-point change in FDI. $\beta_1$ is expected to be positive (higher income increases both the demand for cleaner energy and the fiscal capacity to finance it). The signs of $\beta_2$, $\beta_3$ and $\beta_4$ are theoretically ambiguous: NREC may crowd out renewables or complement them within the power system; FDI may transfer clean technologies or expand energy-intensive activities; and trade openness may act through scale, composition and technique effects with opposite signs. No transformation other than the logarithm was applied to the variables; multicollinearity is assessed directly with variance inflation factors (Section 4.1).

### *3.5. Bootstrap ARDL Bounds-Testing Approach*

The ARDL bounds-testing approach [35] is used because it is valid for a mixture of I(0) and I(1) regressors, provided none is I(2), and because it estimates the short-run and long-run parameters jointly. With 42 annual observations, however, the asymptotic critical values may be unreliable, and the F-test may signal cointegration in degenerate cases, in particular when the dependent variable is I(0). We therefore follow the bootstrap ARDL approach of McNown et al. [10]. Three statistics are computed from the unrestricted error-correction model (UECM) in Equation (3): the overall F-test on all lagged level terms ($F_{overall}$), the *t*-test on the lagged dependent variable ($t_{dep}$) and the F-test on the lagged levels of the regressors ($F_{ind}$). Their critical values and *p*-values are obtained by a residual bootstrap. For each test, the UECM is re-estimated with the corresponding null hypothesis imposed; the centred residuals of this restricted model are resampled with replacement; a bootstrap series of the dependent variable is generated recursively from the restricted model, holding the regressors at their observed values; and the unrestricted UECM is re-estimated on the bootstrap sample to compute the test statistic. This procedure is repeated B = 2000 times for each statistic and each normalisation. The bootstrap critical values are the 90th, 95th and 99th percentiles of the resulting distributions (the 10th, 5th and 1st percentiles for $t_{dep}$), and the bootstrap *p*-value is the share of bootstrap statistics that are more extreme than the observed one. The asymptotic bounds of Pesaran et al. [35] are reported for comparison. The UECM of the baseline equation is:

$$\Delta REC_t = \alpha_0 + \sum_{i=1}^{p} \alpha_{1i} \Delta\, REC_{t-i} + \sum_{i=0}^{q_1} \alpha_{2i} \Delta\, lnNREC_{t-i} + \sum_{i=0}^{q_2} \alpha_{3i} \Delta\, lnGDP_{t-i} + \sum_{i=0}^{q_3} \alpha_{4i} \Delta\, FDI_{t-i} + \sum_{i=0}^{q_4} \alpha_{5i} \Delta\, lnTR_{t-i} + \beta_1\, REC_{t-1} + \beta_2 lnNREC_{t-1} + \beta_3 lnGDP_{t-1} + \beta_4 FDI_{t-1} + \beta_5 lnTR_{t-1} + \varepsilon_t \quad (3)$$

Three null hypotheses are tested in Equation (3): $F_{overall}$ tests $H_0: \beta_1 = \beta_2 = \beta_3 = \beta_4 = \beta_5 = 0$ (no long-run level relationship) against the alternative that at least one $\beta_j \neq 0$; $t_{dep}$ tests $H_0: \beta_1 = 0$; and $F_{ind}$ tests $H_0: \beta_2 = \beta_3 = \beta_4 = \beta_5 = 0$. All models include an unrestricted intercept and no trend (Case III of Pesaran et al. [35]). A long-run level relationship is concluded only when all three null hypotheses are rejected. If $F_{overall}$ and $t_{dep}$ reject but $F_{ind}$ does not, the result is a degenerate case of the first type, in which the significance of the lagged dependent variable merely reflects its stationarity; if $F_{overall}$ and $F_{ind}$ reject but $t_{dep}$ does not, it is a degenerate case of the second type [10].

The lag orders (p, $q_1$, …, $q_4$) of each ARDL equation were selected ex ante by the Akaike information criterion (AIC) among all combinations with p = 1, 2 for the dependent variable and $q_j$ = 0, 1, 2 for each regressor, using a common estimation sample (1982–2021). The maximum of two lags reflects the sample size: with five variables, a maximum of three

lags would imply up to 17 parameters for 39 observations. The selected specifications were not modified after the diagnostic tests. Different lag orders across the five normalisations are to be expected, because each equation has a different dependent variable with its own adjustment dynamics. Specifications selected by the SIC and with a maximum of three lags are reported as robustness checks. The VAR criteria in Table 7 are used only to select the lag length of the Toda–Yamamoto VAR (Section 3.6).

If a long-run relationship is found, the short-run dynamics are estimated with the error-correction model:

$$\Delta REC_t = \beta_0 + \sum_{i=1}^{p-1} \gamma_{1i} \Delta\, REC_{t-i} + \sum_{i=0}^{q_1} \gamma_{2i} \Delta\, lnNREC_{t-i} + \sum_{i=0}^{q_2} \gamma_{3i} \Delta\, lnGDP_{t-i} + \sum_{i=0}^{q_3} \gamma_{4i} \Delta\, FDI_{t-i} + \sum_{i=0}^{q_4} \gamma_{5i} \Delta\, lnTR_{t-i} + \tau\, ECT_{t-1} + \varepsilon_t \quad (4)$$

where the $\gamma$ coefficients capture the short-run dynamics and $\tau$ is the coefficient of the lagged error-correction term ($ECT_{t-1}$), which measures the speed of adjustment toward the long-run equilibrium. If no long-run relationship is found, the long-run coefficients are not interpreted, and the short-run dynamics are estimated with an ARDL model in first differences without an error-correction term. The residuals are tested for serial correlation (Breusch–Godfrey LM test), heteroskedasticity (Breusch–Pagan–Godfrey and ARCH tests), normality [40] and functional form (Ramsey RESET), and parameter stability is examined with the CUSUM and CUSUM-of-squares tests [41].

### *3.6. Toda–Yamamoto Causality Test and Robustness Checks*

Granger causality is tested with the procedure of Toda and Yamamoto [42]. A VAR of order k + $d_{max}$ is estimated in levels for (REC, lnNREC, lnGDP, FDI, lnTR), where k is the optimal lag length and $d_{max}$ = 1 is the maximum order of integration found in Section 4.2. For each pair of variables, the null hypothesis that x does not Granger-cause y ($H_0$: the coefficients on the first k lags of x in the equation for y are jointly zero) is tested with a modified Wald (MWALD) statistic, which is asymptotically $\chi^2$(k) distributed irrespective of the integration and cointegration properties of the series; the additional $d_{max}$ lag is not restricted. Because this distribution requires serially uncorrelated errors, k must be chosen so that the residuals of every equation pass the serial-correlation tests. The information criteria in Table 7 select k = 1 (SIC and HQ) or k = 3 (AIC and FPE). In a VAR that contains only a constant, no lag length satisfies this requirement: in the augmented VAR with k = 2, the Breusch–Godfrey LM(2) test rejects the null of no serial correlation in the FDI equation ($p$ = 0.005) and in the lnGDP equation ($p$ = 0.018), and k = 1 and k = 3 leave serial correlation in the FDI equation and, for k = 3, also in the REC and lnNREC equations. The FDI residuals show that this serial correlation stems from the sharp rise in FDI inflows after Saudi Arabia's accession to the World Trade Organization in December 2005 and their subsequent decline, which a constant-only VAR cannot accommodate; the Zivot–Andrews test also locates a break in FDI in 2005 (Table 6). The baseline VAR therefore includes, as exogenous deterministic terms, an impulse dummy for 2005 together with the event dummies used in the ARDL robustness checks (1986, 2009, 2015, 2016 and 2020) and a step dummy for 1990, the first year of observed REC data. With these terms and k = 2, the Breusch–Godfrey LM(1) and LM(2) tests do not reject the null of no serial correlation in any of the five equations (all $p \geq 0.16$), whereas with k = 1 and k = 3 serial correlation remains in at least one equation. We therefore use k = 2 and $d_{max}$ = 1, i.e., an augmented VAR(3) estimated over 1983–2021 (39 observations), and report the specification without dummies and k = 1 and k = 3 as robustness checks. Because Granger causality measures incremental predictive content, a rejection of $H_0$ is interpreted as evidence that past values of x help to predict y, not as evidence of a structural causal mechanism. The Toda–Yamamoto procedure is a test in the level VAR and does not distinguish between short-run and

long-run causality; the long-run dimension of the analysis is captured by the bootstrap ARDL bounds tests and the error-correction term.

Two further steps address the small sample and the large number of tests. First, because the asymptotic $\chi^2$ approximation may be inaccurate with 39 observations and 23 parameters per equation, we also report bootstrap *p*-values obtained from a residual bootstrap under the null hypothesis (B = 999 replications), following Hacker and Hatemi-J [43]. Second, the 20 pairwise tests are carried out simultaneously, so that some rejections may occur by chance: if all 20 null hypotheses were true and the tests were independent, the probability of at least one rejection at the 5% level would be about 64%. We therefore also report *p*-values adjusted with the Holm [44] procedure, which controls the family-wise error rate, and with the Benjamini–Hochberg [45] procedure, which controls the false discovery rate. Both adjustments are applied separately to the 20 asymptotic *p*-values and to the 20 bootstrap *p*-values, and both sets of adjusted *p*-values are reported in Table 10. A predictive relationship is described as robust only if it remains significant after these adjustments and across alternative lag lengths; relationships that are significant only at unadjusted conventional levels are reported as tentative and are not given the same weight in the interpretation.

Six robustness checks are performed: (i) the ARDL lag orders are selected by the SIC; (ii) a maximum of three lags is allowed; (iii) impulse dummies for the 1986 oil-price collapse, the 2009 global financial crisis, the 2015–2016 oil-price slump and launch of Vision 2030, and the 2020 COVID-19 shock, together with a step dummy for the start of the REC data in 1990, are added to all ARDL equations; (iv) the models are re-estimated on the 1990–2021 subsample with REC in logarithms; (v) the Toda–Yamamoto tests are repeated without the deterministic dummies, with k = 1 and k = 3, and with k = 2 on the 1990–2021 subsample; and (vi) the bootstrap ARDL tests are repeated under the alternative treatments of the missing REC observations and of NREC described in Section 3.1 (step dummy for 1990, backcast series, observed data for 1990–2021 with REC in levels, step dummy for the 2015 level shift in NREC, interpolated 2015 value of NREC, a fully traceable spliced NREC series and the WDI energy-use series as an alternative NREC measure).

## 4. Results

### *4.1. Descriptive Statistics and Multicollinearity Diagnostics*

Table 3 reports the descriptive statistics of the variables used in estimation. The Jarque–Bera test rejects normality at the 5% level for REC ($p < 0.001$), lnGDP ($p < 0.001$) and FDI ($p = 0.008$), but not for lnNREC ($p = 0.078$) or lnTR ($p = 0.431$). The non-normality of lnGDP reflects the oil-boom years at the beginning of the sample (kurtosis 9.43), and that of REC the zero values imputed for 1980–1989. Non-normality of the variables themselves does not invalidate the inference, which depends on the regression residuals. The residuals of the GDP equation are normal, whereas those of the REC equation are not, reflecting the jump in REC in 1990; this is one reason for relying on bootstrap rather than asymptotic critical values and for the 1990–2021 robustness check.

**Table 3.** Descriptive statistics.

| | REC | lnNREC | lnGDP | FDI | lnTR |
|---|---|---|---|---|---|
| Mean | 0.0130 | 8.5967 | 9.8552 | 1.5901 | 4.2810 |
| Maximum | 0.0500 | 9.3637 | 10.4739 | 8.4964 | 4.5654 |
| Minimum | 0.0000 | 8.0737 | 9.6244 | −1.9401 | 3.9527 |
| Std. Dev. | 0.0129 | 0.3588 | 0.1638 | 2.5391 | 0.1578 |
| Skewness | 1.410 | 0.845 | 2.280 | 1.137 | 0.116 |
| Kurtosis | 4.532 | 2.749 | 9.427 | 3.540 | 2.047 |

| | | | | | |
|---|---|---|---|---|---|
| Jarque–Bera | 18.017 | 5.108 | 108.663 | 9.559 | 1.683 |
| Probability | <0.001 | 0.078 | <0.001 | 0.008 | 0.431 |
| Observations | 42 | 42 | 42 | 42 | 42 |

Note: lnNREC, lnGDP and lnTR are in natural logarithms; REC and FDI are in levels (%). NREC is measured in kg of oil equivalent per capita; its 2015 value was corrected for a data-entry error (95,432.55 replaced by 9543.25; see Section 3.1). Sample: 1980–2021, Saudi Arabia.

Table 4 reports the pairwise correlation coefficients, with t-statistics in parentheses. The largest correlations in absolute value are 0.602 (FDI–lnTR), 0.464 (lnGDP–lnTR), −0.442 (REC–lnTR) and 0.441 (REC–lnNREC); no coefficient exceeds 0.61 in absolute value. Because pairwise correlations cannot reveal multicollinearity involving more than two regressors, variance inflation factors (VIFs) were computed for the regressors of Equation (2): 1.18 for lnNREC, 1.39 for lnGDP, 1.77 for FDI and 2.36 for lnTR (mean VIF = 1.68). All values are well below the conservative threshold of 5 and the conventional threshold of 10 [46]; multicollinearity is therefore not a concern. The correlations are reported for descriptive purposes only and are not interpreted as evidence of economic relationships.

**Table 4.** Pairwise correlation coefficients.

| | REC | lnNREC | lnGDP | FDI | lnTR |
|---|---|---|---|---|---|
| REC | 1 | | | | |
| lnNREC | 0.441 *** (3.108) | 1 | | | |
| lnGDP | −0.087 (−0.551) | 0.061 (0.389) | 1 | | |
| FDI | −0.188 (−1.209) | 0.076 (0.482) | 0.155 (0.993) | 1 | |
| lnTR | −0.442 *** (−3.114) | −0.206 (−1.333) | 0.464 *** (3.309) | 0.602 *** (4.765) | 1 |

Note: Pairwise correlation coefficients, t-statistics in parentheses; *** significant at the 1% level. Sample: 1980–2021 (42 observations).

### *4.2. Unit-Root Test Results*

Table 5 reports the ADF and PP tests, and Table 6 the Zivot–Andrews tests. REC and lnTR are non-stationary in levels and stationary in first differences under all tests, i.e., I(1), whereas lnGDP is stationary in levels under all tests, i.e., I(0). For lnNREC and FDI, the ADF and PP tests do not reject the unit-root hypothesis at the 5% level (for FDI, $p = 0.061$ with a constant), but the Zivot–Andrews test rejects it when a structural break is allowed: in 2014 (Model A) or 2009 (Model C) for lnNREC, and in 2005 (Model C) for FDI. These two series are therefore I(1) according to the conventional tests and I(0) around a structural break. All series are stationary in first differences, so none is I(2). This mixture of I(0) and I(1) series satisfies the conditions of the ARDL bounds test, rules out the Johansen procedure, and implies $d_{max} = 1$ in the Toda–Yamamoto VAR. The dependent variable of the baseline model, REC, is I(1), as required for the bounds test to be informative. For the GDP normalisation, whose dependent variable is I(0), the $t_{dep}$ test is not informative on its own, and the conclusion rests on the $F_{ind}$ test. Over the observed-data sample 1990–2021, REC is I(1) according to the PP test and the ADF test with a trend, and lnGDP is I(1) rather than I(0) (ADF $p \geq 0.29$; PP $p \geq 0.23$; Zivot–Andrews not significant): the stationarity of lnGDP over the full sample reflects mainly the decline of GDP per capita in the 1980s. This difference is taken into account in the sensitivity analysis of Section 4.8. Figure 1 shows the trend and cyclical components of the series extracted with the Hodrick–Prescott filter ($\lambda = 100$); it is presented for descriptive purposes only.

**Table 5.** Unit-root tests without structural breaks (ADF and PP).

| Variable | ADF (c) | ADF (c, t) | PP (c) | PP (c, t) | Decision |
|---|---|---|---|---|---|
| REC | −1.267 [0.644] | −1.760 [0.724] | −1.054 [0.733] | −1.728 [0.739] | I(1) |

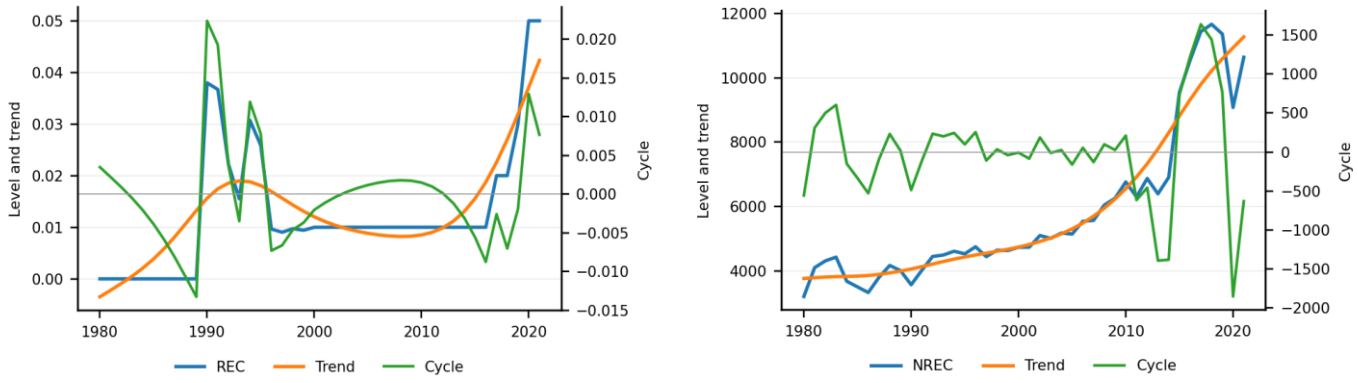
| | | | | | |
|---|---|---|---|---|---|
| ΔREC | −5.994 *** [<0.001] | −6.029 *** [<0.001] | −6.005 *** [<0.001] | −6.018 *** [<0.001] | |
| lnNREC | −0.467 [0.898] | −2.117 [0.537] | −0.131 [0.946] | −1.966 [0.620] | I(1) † |
| ΔlnNREC | −6.255 *** [<0.001] | −6.285 *** [<0.001] | −6.595 *** [<0.001] | −8.076 *** [<0.001] | |
| lnGDP | −4.712 *** [<0.001] | −5.840 *** [<0.001] | −4.462 *** [<0.001] | −6.137 *** [<0.001] | I(0) |
| ΔlnGDP | −4.477 *** [<0.001] | −4.845 *** [<0.001] | −4.446 *** [<0.001] | −4.890 *** [<0.001] | |
| FDI | −2.779 * [0.061] | −2.694 [0.239] | −2.786 * [0.060] | −2.754 [0.214] | I(1) † |
| ΔFDI | −6.238 *** [<0.001] | −6.107 *** [<0.001] | −7.140 *** [<0.001] | −6.943 *** [<0.001] | |
| lnTR | −1.815 [0.373] | −1.798 [0.706] | −1.973 [0.299] | −1.974 [0.616] | I(1) |
| ΔlnTR | −5.268 *** [<0.001] | −5.178 *** [<0.001] | −5.172 *** [<0.001] | −5.075 *** [<0.001] | |

Note: Test statistics with MacKinnon *p*-values in brackets; (c) = constant, (c, t) = constant and linear trend; and ADF lag length selected by the SIC. *** and * denote rejection of the unit-root null at the 1% and 10% levels. † I(1) according to ADF and PP, but I(0) around a structural break according to the Zivot–Andrews test (Table 6).

**Table 6.** Zivot–Andrews unit-root tests with one endogenous structural break.

| Variable | Model A: t-Stat. | Break (A) | Model C: t-Stat. | Break (C) |
|---|---|---|---|---|
| REC | −2.666 | 1996 | −2.957 | 2014 |
| ΔREC | −7.452 *** | 1993 | −7.758 *** | 1992 |
| lnNREC | −4.937 ** | 2014 | −5.201 ** | 2009 |
| ΔlnNREC | −6.368 *** | 1987 | −7.080 *** | 2014 |
| lnGDP | −6.205 *** | 2011 | −6.427 *** | 2011 |
| ΔlnGDP | −4.453 | 2013 | −10.852 *** | 1992 |
| FDI | −4.533 | 2005 | −5.538 ** | 2005 |
| ΔFDI | −7.561 *** | 2010 | −7.698 *** | 2010 |
| lnTR | −3.234 | 2003 | −3.331 | 2004 |
| ΔlnTR | −6.105 *** | 2009 | −6.015 *** | 2009 |

Note: Model A allows a break in the intercept, Model C in the intercept and trend; 15% trimming; lag length selected by the SIC (maximum 2). 5% critical values: −4.80 (Model A) and −5.08 (Model C) [39]. *** and ** denote rejection of the unit-root null at the 1% and 5% levels.



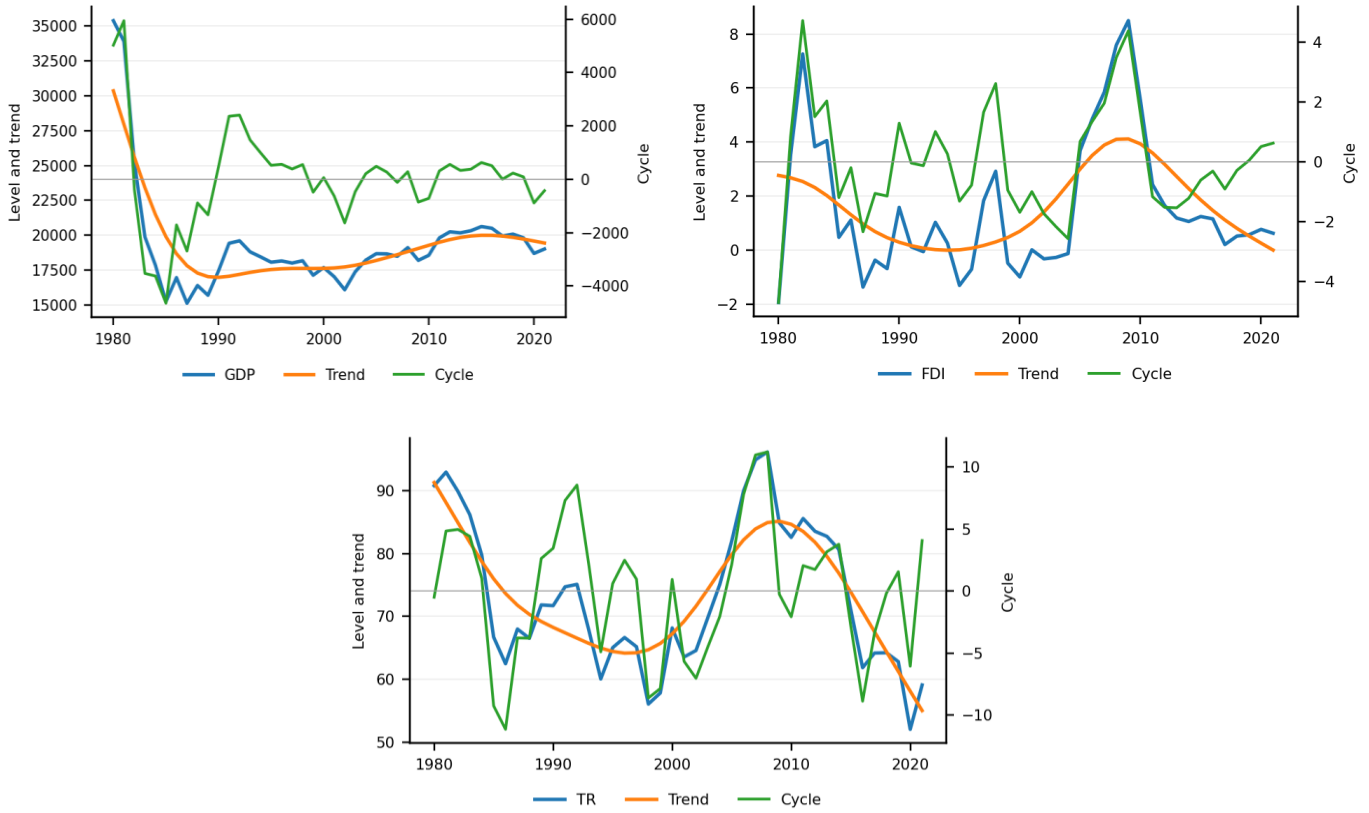



**Figure 1.** Hodrick–Prescott filter ($\lambda$ = 100): level, trend and cycle of renewable-energy consumption (REC), non-renewable-energy consumption (NREC), GDP per capita, foreign direct investment (FDI) and trade openness (TR), 1980–2021.

### *4.3. Bootstrap ARDL Bounds Tests for Cointegration*

Table 8 reports the bootstrap bounds tests for the baseline model and the four alternative normalisations. For the baseline model (REC as the dependent variable; ARDL(1,2,1,2,0)), $F_{overall}$ = 3.122 (bootstrap $p$ = 0.102), $t_{dep}$ = −3.327 ($p$ = 0.065) and $F_{ind}$ = 3.089 ($p$ = 0.079). None of the three statistics rejects its null hypothesis at the 5% level, and $F_{overall}$ does not reject even at the 10% level. The asymptotic bounds give the same answer: $F_{overall}$ and $t_{dep}$ lie between the I(0) and I(1) bounds at the 10% level and are therefore inconclusive. We conclude that there is no statistically detectable long-run level relationship in which renewable-energy consumption adjusts to GDP, NREC, FDI and trade openness (RQ1). This conclusion holds in the five robustness specifications of Table 11 and in all but one of the alternative treatments of the data (Section 4.8): when only observed data (1990–2021) are used with REC in levels, $F_{overall}$ and $F_{ind}$ reject at the 5% level but $t_{dep}$ only at the 10% level, so that the evidence becomes inconclusive rather than negative. Failure to reject the null hypothesis does not prove that renewable energy is unrelated to the other variables: with a short sample and a renewable share that is almost constant for much of the period, the tests have limited power to detect such a relationship.

In contrast, all three statistics reject their null hypotheses when GDP is the dependent variable ($F_{overall}$ = 14.509, $t_{dep}$ = −7.527, $F_{ind}$ = 7.047; bootstrap $p \leq 0.001$) and when FDI is the dependent variable ($F_{overall}$ = 6.170, $t_{dep}$ = −4.773, $F_{ind}$ = 5.153; $p$ = 0.008, 0.004 and 0.018). No level relationship is found for NREC and TR. Because GDP is I(0), the significance of $t_{dep}$ alone could merely reflect the stationarity of GDP; the significance of $F_{ind}$, however, shows that the lagged levels of the regressors enter the GDP equation, so the relationship is not degenerate. Level relationships are therefore found when GDP or FDI is the dependent variable, but not when REC, NREC or TR is. For GDP, which is I(0) over the full sample,

the result is a conditional level relationship: the lagged levels of the regressors, mainly trade openness (Section 4.4), help to explain GDP given its own past, and GDP reverts toward the level associated with them; it should not be read as cointegration among I(1) variables (Section 3.2). This result is robust to SIC lag selection, but it weakens when three lags or shock dummies are allowed ($F_{ind}$ is then not significant for GDP) and in the 1990–2021 subsample, in which GDP is I(1) and only $F_{ind}$ rejects at the 5% level (Section 4.8), so it should be interpreted with caution.

**Table 7.** VAR lag-order selection criteria (used to select k in the Toda–Yamamoto VAR).

| Lag | LogL | LR | FPE | AIC | SIC | HQ |
|---|---|---|---|---|---|---|
| 0 | 110.250 | — | $3.116 \times 10^{-9}$ | −5.397 | −5.184 | −5.321 |
| 1 | 231.853 | 205.789 | $2.225 \times 10^{-11}$ | −10.351 | −9.072 * | −9.892 * |
| 2 | 255.541 | 34.014 | $2.543 \times 10^{-11}$ | −10.284 | −7.938 | −9.442 |
| 3 | 287.315 | 37.477 | $2.147 \times 10^{-11}$ * | −10.632 * | −7.219 | −9.407 |

Note: * indicates the lag order selected by each criterion. LR: Sequential modified likelihood-ratio statistic; FPE: Final prediction error; AIC, SIC, HQ: Akaike, Schwarz and Hannan–Quinn criteria. Common sample 1983–2021 (39 observations); variables REC, lnNREC, lnGDP, FDI and lnTR in levels. With the deterministic dummies of the baseline Toda–Yamamoto VAR (Section 3.6), the SIC still selects k = 1 and the AIC and FPE k = 3, whereas the HQ criterion selects k = 3.

**Table 8.** Bootstrap ARDL bounds tests under alternative normalisations (1980–2021).

| Dependent Variable | ARDL Order (AIC) | $F_{overall}$ | $t_{dep}$ | $F_{ind}$ | Long-Run Level Relationship |
|---|---|---|---|---|---|
| REC | (1,2,1,2,0) | 3.122 (0.102) [3.831] | −3.327 * (0.065) [−3.449] | 3.089 * (0.079) [3.704] | No |
| lnNREC | (1,1,1,2,0) | 0.732 (0.650) [2.825] | 0.233 (0.806) [−2.519] | 0.787 (0.565) [3.066] | No |
| lnGDP | (1,0,2,2,0) | 14.509 *** (<0.001) [4.135] | −7.527 *** (<0.001) [−3.186] | 7.047 *** (<0.001) [2.955] | Yes |
| FDI | (2,0,1,1,1) | 6.170 *** (0.008) [4.495] | −4.773 *** (0.004) [−3.767] | 5.153 ** (0.018) [3.726] | Yes |
| lnTR | (2,0,0,1,1) | 1.908 (0.495) [4.381] | −2.913 (0.190) [−3.811] | 1.374 (0.478) [3.840] | No |
| Asymptotic bounds (Case III, k = 4) | | F: I(0) | F: I(1) | t: I(0) | t: I(1) |
| 10% | | 2.45 | 3.52 | −2.57 | −3.66 |
| 5% | | 2.86 | 4.01 | −2.86 | −3.99 |
| 1% | | 3.74 | 5.06 | −3.43 | −4.60 |

Note: Bootstrap *p*-values in parentheses and bootstrap 5% critical values in brackets (B = 2000 replications; for $t_{dep}$, the lower-tail critical value). ***, ** and * denote rejection of the null hypothesis at the 1%, 5% and 10% levels, respectively. Lag orders selected by the AIC (maximum of two lags); Case III (unrestricted intercept, no trend); effective sample: 1982–2021 (40 observations).

### *4.4. Long-Run and Short-Run Estimates*

Table 9 reports the estimates for the baseline REC equation (Panel A) and for the GDP equation, the normalisation for which a long-run relationship is supported (Panel B). In Panel A, consistent with the absence of a statistically detectable level relationship, none of the long-run coefficients of the REC equation is significant (all $p > 0.12$); they are reported for completeness but are not interpreted. The short-run dynamics of REC are therefore estimated with an ARDL model in first differences. The change in lnNREC is negatively associated with the change in REC (−0.0397, t = −2.74, $p = 0.010$), whereas the changes in lnGDP ($p = 0.098$) and lagged FDI ($p = 0.091$) are significant only at the 10% level. In Panel B, lnGDP is positively associated in the long run with REC (5.018, t = 3.17, $p = 0.003$) and with lnTR (0.305, t = 2.26, $p = 0.031$), whereas lnNREC and FDI are not significant. Because REC is measured in percentage points and varies only between 0 and 0.05, the REC coefficient implies that a 0.01-point higher renewable share is associated with a long-run GDP

per capita about 5% higher; given this very small variation, the coefficient should be read as an association between the emergence of renewable energy after 1990 and the post-1990 growth path rather than as an effect. The sensitivity analysis confirms this interpretation: the REC coefficient becomes small and insignificant when a step dummy for 1990 is added (1.07, $p = 0.66$), when the 1980–1989 values are backcast (−0.67, $p = 0.72$) and when only observed data are used (2.10, $p = 0.24$), whereas the coefficient of trade openness remains positive in all cases. The long-run GDP–REC coefficient is therefore not interpreted as a substantive long-run effect of renewable energy on output. The error-correction term is negative and highly significant (−0.515, t = −7.57), implying that about half of any deviation of GDP from the level associated with its regressors is corrected within one year. In the short run, the change in FDI is positively associated with GDP growth (0.0098, $p = 0.020$). For the FDI normalisation, the long-run coefficient of lnTR is positive and significant (11.50, $p = 0.001$), but the RESET test rejects the functional form of that equation, so it is not interpreted further. These coefficients are conditional associations; their economic interpretation is discussed in Section 5.

**Table 9.** Estimates for the REC equation (Panel A) and the GDP equation (Panel B), 1980–2021.

| Regressor | Coefficient | t-Statistic | *p*-Value |
|---|---|---|---|
| **Panel A. REC equation, ARDL(1,2,1,2,0): long-run coefficients (not interpreted: no statistically detectable level relationship)** | | | |
| lnNREC | 0.0174 | 1.601 | 0.120 |
| lnGDP | 0.0554 | 1.369 | 0.182 |
| FDI | −0.0008 | −0.428 | 0.672 |
| lnTR | −0.0375 | −1.317 | 0.198 |
| C | −0.5126 | −1.478 | 0.150 |
| **Panel A (cont.). Short-run model in first differences (dependent variable: ΔREC)** | | | |
| Constant | 0.0026 * | 1.846 | 0.074 |
| ΔREC(−1) | 0.0282 | 0.162 | 0.872 |
| ΔlnNREC | −0.0397 *** | −2.744 | 0.010 |
| ΔlnNREC(−1) | −0.0093 | −0.651 | 0.520 |
| ΔlnGDP | 0.0350 * | 1.706 | 0.098 |
| ΔFDI | 0.0002 | 0.220 | 0.827 |
| ΔFDI(−1) | 0.0015 * | 1.743 | 0.091 |
| ΔlnTR | −0.0198 | −1.255 | 0.219 |
| Diagnostics (levels ARDL): BG LM(2): F = 0.506 [$p = 0.609$]; BPG: F = 1.872 [$p = 0.092$]; ARCH(1): F = 0.194 [$p = 0.663$]; Jarque–Bera = 49.763 [$p \leq 0.001$]; RESET: F = 3.115 [$p = 0.088$] | | | |
| **Panel B. GDP equation, ARDL(1,0,2,2,0): long-run coefficients** | | | |
| REC | 5.0176 *** | 3.170 | 0.003 |
| lnNREC | 0.0691 | 1.375 | 0.179 |
| FDI | 0.0011 | 0.121 | 0.904 |
| lnTR | 0.3050 ** | 2.257 | 0.031 |
| C | 7.8296 *** | 10.150 | <0.001 |
| **Panel B (cont.). Error-correction model (dependent variable: ΔlnGDP)** | | | |
| Constant | 4.0345 *** | 7.550 | <0.001 |
| ΔlnNREC | 0.1373 * | 1.843 | 0.074 |
| ΔlnNREC(−1) | 0.0909 | 1.146 | 0.260 |
| ΔFDI | 0.0098 ** | 2.437 | 0.020 |
| ΔFDI(−1) | −0.0093 ** | −2.293 | 0.028 |
| ECT(−1) | −0.5146 *** | −7.574 | <0.001 |
| Diagnostics (levels ARDL): BG LM(2): F = 0.114 [$p = 0.893$]; BPG: F = 1.911 [$p = 0.089$]; ARCH(1): F = 0.129 [$p = 0.721$]; Jarque–Bera = 0.480 [$p = 0.787$]; RESET: F = 0.026 [$p = 0.872$] | | | |

Note: ***, ** and * denote significance at the 1%, 5% and 10% levels. Long-run coefficients and standard errors obtained from the UECM (delta method). BG: Breusch–Godfrey; BPG: Breusch–Pagan–Godfrey. Effective sample: 1982–2021 (40 observations).

### *4.5. Diagnostic and Stability Tests*

The lower rows of each panel of Table 9 report the residual diagnostics of the ARDL models together with their *p*-values. For the GDP equation, the null hypotheses of no serial correlation (Breusch–Godfrey, $p = 0.893$), homoskedasticity (Breusch–Pagan–Godfrey, $p = 0.089$; ARCH, $p = 0.721$), normality (Jarque–Bera, $p = 0.787$) and correct functional form (RESET, $p = 0.872$) are not rejected at the 5% level. For the REC equation, serial correlation ($p = 0.609$), heteroskedasticity ($p = 0.092$; ARCH, $p = 0.663$) and misspecification ($p = 0.088$) are not detected, but normality is rejected (Jarque–Bera = 49.76) because of the jump in REC in 1990. The bootstrap inference used in Table 8 does not rely on normal errors, and the conclusion for REC is unchanged in the 1990–2021 subsample and under the alternative treatments of the pre-1990 data (Section 4.8). The CUSUM and CUSUM-of-squares statistics of both equations remain within the 5% bounds (Figures 2 and 3); the CUSUM of squares of the REC equation comes close to the upper bound in the late 1990s, again reflecting the 1990 jump.

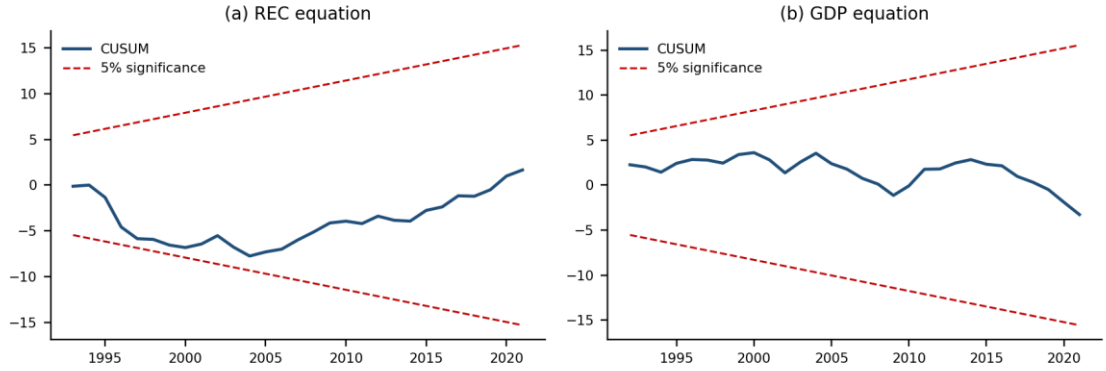



**Figure 2.** CUSUM statistics with 5% significance bounds: (**a**) REC equation; (**b**) GDP equation.

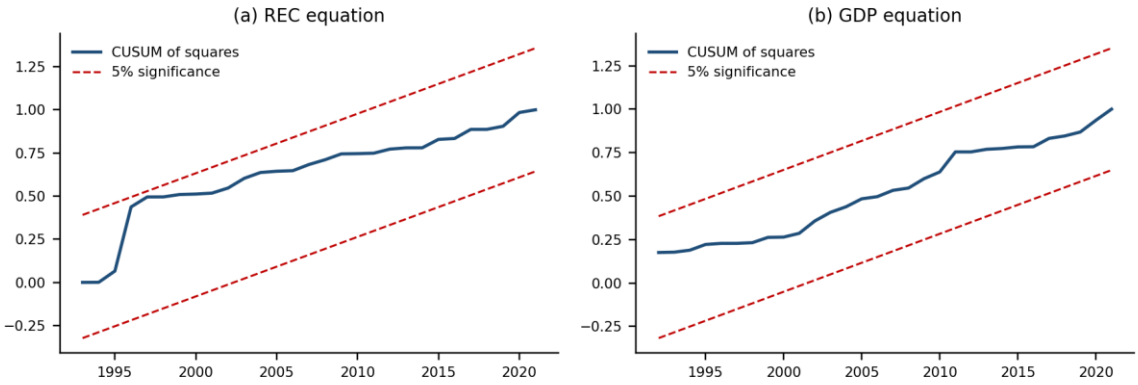



**Figure 3.** CUSUM-of-squares statistics with 5% significance bounds: (**a**) REC equation; (**b**) GDP equation.

### *4.6. Toda–Yamamoto Causality Tests*

Table 10 reports the MWALD statistics from the baseline augmented VAR(3) (k = 2, $d_{max} = 1$, with the deterministic dummies described in Section 3.6; 39 observations), together with bootstrap *p*-values, *p*-values adjusted for multiple testing and, in the last four columns, the asymptotic *p*-values of the robustness specifications. The residuals of all five equations of the baseline VAR are free of serial correlation (Breusch–Godfrey LM(2) *p*-values: REC 0.80, lnNREC 0.48, lnGDP 0.63, FDI 0.20, lnTR 0.50). Using unadjusted asymptotic *p*-values, three null hypotheses are rejected at the 5% level: trade openness

Granger-causes FDI ($\chi^2 = 36.54$, $p < 0.001$), trade openness Granger-causes GDP ($\chi^2 = 13.54$, $p = 0.001$) and REC Granger-causes GDP ($\chi^2 = 9.16$, $p = 0.010$). FDI → GDP is significant at the 5% level asymptotically ($p = 0.042$) but only at the 10% level with the bootstrap ($p = 0.065$). None of the other 16 null hypotheses is rejected at the 5% level; in particular, neither GDP, NREC, FDI nor trade openness helps to predict REC (all $p \geq 0.24$).

After adjustment of the asymptotic $p$-values for multiple testing (first entry in the Holm and BH columns of Table 10), only two relationships remain significant at the 5% level: TR → FDI (Holm- and Benjamini–Hochberg-adjusted $p < 0.001$) and TR → GDP (Holm $p = 0.022$; Benjamini–Hochberg $p = 0.011$), whereas REC → GDP (Holm $p = 0.19$; Benjamini–Hochberg $p = 0.068$) and FDI → GDP (Holm $p = 0.71$; Benjamini–Hochberg $p = 0.21$) do not survive. When the adjustments are applied to the bootstrap $p$-values (second entry in the Holm and BH columns of Table 10), TR → FDI remains significant at the 5% level (Holm $p = 0.020$; Benjamini–Hochberg $p = 0.020$), whereas TR → GDP is significant only at the 10% level under the Benjamini–Hochberg procedure ($p = 0.100$) and not under the Holm procedure ($p = 0.190$), and REC → GDP and FDI → GDP are not significant under either procedure (Holm $p \geq 0.61$; Benjamini–Hochberg $p \geq 0.23$). The robustness columns give the same ranking. TR → FDI is significant in every specification ($p \leq 0.001$), including the 1990–2021 subsample, and survives the Holm adjustment in each of them. TR → GDP is significant with k = 1, without dummies and in the subsample, but not with k = 3 ($p = 0.18$). By contrast, REC → GDP is significant with k = 1 ($p = 0.001$) but not with k = 3 ($p = 0.92$) or without dummies ($p = 0.081$), and FDI → GDP is significant only in the VAR without dummies ($p = 0.005$), whose FDI equation is serially correlated; it is not significant with k = 1 ($p = 0.085$), with k = 3 ($p = 0.12$) or in the subsample ($p = 0.37$). We therefore regard TR → FDI as robust, TR → GDP as probable, and REC → GDP and FDI → GDP as tentative. FDI does not Granger-cause REC in any specification ($p \geq 0.42$). These results identify predictive relationships: past values of trade openness contain information that helps to forecast FDI and GDP, but the tests do not by themselves establish the economic mechanism behind this predictive content.

**Table 10.** Toda–Yamamoto Granger causality tests (baseline: augmented VAR(3), k = 2, $d_{max} = 1$, with deterministic dummies).

| Null Hypothesis: x ↛ y | MWALD $\chi^2(2)$ | $p$ (Asym.) | $p$ (Boot.) | Holm: Asym./Boot. | BH: Asym./Boot. | No Dummies | k = 1 | k = 3 | 1990–2021 |
|---|---|---|---|---|---|---|---|---|---|
| REC → lnNREC | 2.375 | 0.305 | 0.320 | 1.000/1.000 | 0.678/0.700 | 0.867 | 0.179 | 0.214 | 0.080 * |
| REC → lnGDP | 9.160 ** | 0.010 | 0.034 | 0.185/0.612 | 0.068/0.227 | 0.081 * | 0.001 *** | 0.920 | 0.181 |
| REC → FDI | 0.779 | 0.678 | 0.655 | 1.000/1.000 | 0.797/0.802 | 0.565 | 0.782 | 0.510 | 0.998 |
| REC → lnTR | 0.216 | 0.898 | 0.880 | 1.000/1.000 | 0.914/0.926 | 0.984 | 0.114 | 0.400 | 0.392 |
| lnNREC → REC | 2.840 | 0.242 | 0.252 | 1.000/1.000 | 0.669/0.700 | 0.068 * | 0.759 | 0.015 ** | 0.405 |
| lnNREC → lnGDP | 1.672 | 0.433 | 0.491 | 1.000/1.000 | 0.763/0.802 | 0.777 | 0.041 ** | 0.617 | 0.234 |
| lnNREC → FDI | 1.042 | 0.594 | 0.636 | 1.000/1.000 | 0.797/0.802 | 0.997 | 0.473 | 0.868 | 0.511 |
| lnNREC → lnTR | 0.180 | 0.914 | 0.931 | 1.000/1.000 | 0.914/0.931 | 0.391 | 0.218 | 0.670 | 0.991 |
| lnGDP → REC | 0.505 | 0.777 | 0.766 | 1.000/1.000 | 0.863/0.851 | 0.831 | 0.193 | 0.874 | 0.860 |
| lnGDP → lnNREC | 4.617 * | 0.099 | 0.134 | 1.000/1.000 | 0.398/0.536 | 0.453 | 0.219 | 0.798 | 0.047 ** |
| lnGDP → FDI | 2.138 | 0.343 | 0.350 | 1.000/1.000 | 0.687/0.700 | 0.139 | 0.422 | 0.848 | 0.419 |
| lnGDP → lnTR | 0.835 | 0.659 | 0.682 | 1.000/1.000 | 0.797/0.802 | 0.629 | 0.473 | 0.635 | 0.984 |
| FDI → REC | 1.403 | 0.496 | 0.550 | 1.000/1.000 | 0.763/0.802 | 0.693 | 0.422 | 0.867 | 0.950 |
| FDI → lnNREC | 3.846 | 0.146 | 0.164 | 1.000/1.000 | 0.487/0.547 | 0.369 | 0.104 | 0.641 | 0.346 |
| FDI → lnGDP | 6.363 ** | 0.042 | 0.065 | 0.706/1.000 | 0.208/0.325 | 0.005 *** | 0.085 * | 0.123 | 0.368 |
| FDI → lnTR | 1.477 | 0.478 | 0.524 | 1.000/1.000 | 0.763/0.802 | 0.186 | 0.425 | 0.697 | 0.576 |
| lnTR → REC | 0.990 | 0.610 | 0.619 | 1.000/1.000 | 0.797/0.802 | 0.747 | 0.429 | 0.531 | 0.431 |
| lnTR → lnNREC | 2.637 | 0.267 | 0.329 | 1.000/1.000 | 0.669/0.700 | 0.613 | 0.879 | 0.730 | 0.986 |
| lnTR → lnGDP | 13.539 *** | 0.001 | 0.010 | 0.022/0.190 | 0.011/0.100 | 0.024 ** | 0.018 ** | 0.181 | <0.001 *** |
| lnTR → FDI | 36.543 *** | <0.001 | 0.001 | <0.001/0.020 | <0.001/0.020 | <0.001 *** | <0.001 *** | 0.001 *** | <0.001 *** |

Note: MWALD statistics are asymptotically $\chi^2(k)$ under $H_0$: x does not Granger-cause y. Baseline: VAR with a constant, impulse dummies for 1986, 2005, 2009, 2015, 2016 and 2020 and a step dummy for 1990; 39 observations (1983–2021). Boot.: bootstrap $p$-value (B = 999). Holm and BH: $p$-values

adjusted for the 20 tests with the Holm and Benjamini–Hochberg procedures; in each cell, the first entry adjusts the 20 asymptotic *p*-values (*p* asym.) and the second entry adjusts the 20 bootstrap *p*-values (*p* boot.). Bootstrap *p*-values cannot be smaller than 1/(B + 1) = 0.001, so the adjusted bootstrap *p*-values of the strongest relationships are conservative. Robustness columns report asymptotic *p*-values: no dummies (k = 2, constant only; the FDI and lnGDP equations are serially correlated, BG $p = 0.005$ and 0.018); k = 1 (40 observations); k = 3 (38 observations); 1990–2021 (k = 2, dummies within the subsample, 29 observations; serial correlation remains in several equations, so this column is indicative only). ***, ** and * denote rejection of $H_0$ at the 1%, 5% and 10% levels, respectively (asymptotic *p*-values).

### *4.7. Robustness Checks*

Table 11 summarises the bootstrap bounds tests under the five specifications. The absence of a statistically detectable long-run level relationship for REC is robust: $F_{overall}$ is never significant at the 10% level, and the three statistics never jointly reject, whether the lags are selected by the SIC, three lags are allowed, shock dummies are added or the sample is restricted to 1990–2021 with REC in logarithms. The long-run relationship for GDP is confirmed by all three statistics with SIC lag selection, but $F_{ind}$ loses significance when three lags or shock dummies are allowed, and in the 1990–2021 subsample $F_{overall}$ is significant only at the 10% level. The relationship for FDI is confirmed with SIC selection and with three lags, but not with shock dummies or in the subsample. The Toda–Yamamoto results are discussed in Section 4.6. Overall, the main conclusion—that there is no statistically detectable long-run relationship in which renewable-energy consumption adjusts to growth, fossil-energy use, FDI or trade—is robust to these specifications, whereas the long-run relationships for GDP and FDI are sensitive to the treatment of structural shocks.

**Table 11.** Robustness of the bootstrap ARDL bounds tests (bootstrap *p*-values).

| Specification | REC Equation | GDP Equation | FDI Equation |
|---|---|---|---|
| Baseline: AIC, max. 2 lags, 1980–2021 | (1,2,1,2,0): 0.102/0.065/0.079 | (1,0,2,2,0): <0.001/<0.001/<0.001 | (2,0,1,1,1): 0.008/0.004/0.018 |
| SIC, max. 2 lags | (1,2,0,0,0): 0.277/0.234/0.210 | (1,0,1,2,0): <0.001/<0.001/0.001 | (2,0,0,0,1): 0.011/0.004/0.014 |
| AIC, max. 3 lags | (2,3,3,0,0): 0.125/0.088/0.140 | (3,3,0,3,1): 0.048/0.011/0.214 | (3,3,2,1,1): 0.014/0.003/0.012 |
| AIC, max. 2 lags + shock dummies | (1,2,1,2,0): 0.300/0.121/0.270 | (1,2,2,2,1): <0.001/<0.001/0.107 | (2,2,0,0,2): 0.285/0.278/0.289 |
| 1990–2021, REC in logarithm | (1,2,0,0,0): 0.133/0.146/0.085 | (1,0,0,0,0): 0.064/0.130/0.040 | (2,0,0,0,2): 0.481/0.280/0.666 |

Note: Each cell reports the ARDL order and the bootstrap *p*-values of $F_{overall}/t_{dep}/F_{ind}$ (B = 999 replications; 2000 for the baseline). Shock dummies: Impulse dummies for 1986, 2009, 2015, 2016 and 2020 and a step dummy from 1990.

### *4.8. Sensitivity to the Treatment of Missing REC Data and of NREC*

Table 12 reports the bootstrap bounds tests for the REC, GDP and FDI equations, together with the long-run coefficients of REC and lnTR in the GDP equation, under the alternative treatments of the data described in Section 3.1. Three results stand out. First, the absence of a long-run relationship for REC is not an artefact of the zero imputation: $F_{overall}$ does not reject even at the 10% level, and the three statistics never reject jointly, when a step dummy for 1990 is added, when the 1980–1989 values are backcast, when NREC is corrected for its 2015 level shift, when REC is in logarithms or when the WDI energy-use series is used for NREC. The only exception is the observed-data sample with REC in levels, in which $F_{overall}$ ($p = 0.027$) and $F_{ind}$ ($p = 0.014$) reject but $t_{dep}$ rejects only at the 10% level ($p = 0.095$); by the criterion of Section 3.5, this is a degenerate case at the 5% level, and the long-run coefficients of this equation are insignificant at the 5% level. The evidence for REC therefore ranges from 'no relationship' to 'inconclusive', but in no specification does it support a well-determined long-run relationship.

Second, the large positive long-run GDP–REC coefficient of the baseline depends on the imputation. It remains significant when only the 2015 shift in NREC is controlled for (5.58, $p = 0.003$), but it becomes insignificant when the 1990 jump is absorbed by a step dummy (1.07, $p = 0.66$), when the 1980–1989 values are backcast (−0.67, $p = 0.72$) and in the observed-data samples ($p \geq 0.17$). In contrast, the long-run coefficient of trade openness in the GDP equation is positive in every specification (0.19–0.57) and significant at the 5% level in four of the seven. Third, the GDP and FDI level relationships are confirmed by all three statistics in the full sample under the backcast series and the NREC step dummy, although the GDP relationship becomes degenerate when the 1990 step dummy is added ($F_{ind}$ $p = 0.41$); both weaken in the observed-data samples, in which GDP is I(1) and only $F_{ind}$ rejects for GDP at the 5% level, and no relationship is found for FDI. These results show that the conclusions about REC are robust to the treatment of the missing data, whereas the conclusions about the GDP and FDI equations rest partly on the information contained in the 1980s.

The two additional NREC specifications show that the corrected 2015 value does not drive the results. When the 2015 value is replaced by the interpolation of the adjacent entries, the bounds-test conclusions and long-run coefficients are virtually identical to those of the baseline (Table 12). When the fully traceable spliced series is used, the absence of a long-run relationship for REC is confirmed ($F_{overall}$ $p = 0.819$), and the conditional level relationship for GDP is confirmed by all three statistics, with a positive long-run coefficient of trade openness (0.31, $p = 0.040$); the level relationship for FDI, however, is no longer found ($F_{overall}$ $p = 0.272$), which confirms that the FDI normalisation is fragile. In the baseline Toda–Yamamoto VAR re-estimated with the traceable series, TR → FDI remains significant after the Holm adjustment ($p < 0.001$), no variable Granger-causes REC (all $p \geq 0.45$), and FDI Granger-causes neither REC ($p = 0.56$) nor GDP at the 5% level ($p = 0.074$); REC → GDP is significant ($p = 0.003$; Holm $p = 0.048$), but, given its sensitivity to the lag length in the baseline, it remains classified as tentative (the FDI equation of this VAR shows marginal serial correlation, Breusch–Godfrey $p = 0.045$).

**Table 12.** Sensitivity of the bootstrap ARDL results to the treatment of missing REC data and of NREC.

| Specification | Sample | REC Equation | GDP Equation | FDI Equation | LR Coef. of REC (GDP Eq.) | LR Coef. of lnTR (GDP Eq.) |
|---|---|---|---|---|---|---|
| Baseline (REC = 0 in 1980–1989) | 1980–2021 | 0.102/0.065/0.079 | <0.001/<0.001/<0.001 | 0.008/0.004/0.018 | 5.018 *** (0.003) | 0.305 ** (0.031) |
| Step dummy for 1990 | 1980–2021 | 0.151/0.150/0.138 | <0.001/<0.001/0.410 | 0.028/0.011/0.022 | 1.073 (0.660) | 0.209 (0.184) |
| Backcast: 1980–1989 = 1990 value | 1980–2021 | 0.122/0.762/0.101 | <0.001/<0.001/0.022 | 0.015/0.008/0.018 | −0.669 (0.718) | 0.513 ** (0.017) |
| Step dummy for NREC shift (2015) | 1980–2021 | 0.181/0.043/0.760 | <0.001/<0.001/0.001 | 0.012/0.013/0.011 | 5.577 *** (0.003) | 0.188 (0.242) |
| Observed data, REC in levels | 1990–2021 | 0.027/0.095/0.014 | 0.066/0.134/0.042 | 0.455/0.276/0.609 | 2.104 (0.236) | 0.574 ** (0.021) |
| Observed data, REC in logarithms | 1990–2021 | 0.133/0.146/0.085 | 0.064/0.130/0.040 | 0.481/0.280/0.666 | 0.043 (0.222) | 0.543 ** (0.017) |
| Observed data, NREC = WDI energy use | 1990–2021 | 0.379/0.775/0.307 | 0.092/0.156/0.072 | 0.350/0.262/0.595 | 2.625 (0.169) | 0.513 * (0.064) |
| NREC 2015 interpolated | 1980–2021 | 0.117/0.078/0.099 | <0.001/<0.001/<0.001 | 0.009/0.004/0.010 | 4.482 ** (0.010) | 0.335 ** (0.028) |
| Traceable NREC (WDI, spliced) | 1980–2021 | 0.819/0.412/0.737 | <0.001/<0.001/<0.001 | 0.272/0.225/0.304 | 3.698 ** (0.015) | 0.308 ** (0.040) |

Note: Bootstrap $p$-values of $F_{overall}/t_{dep}/F_{ind}$ (B = 999; 2000 for the baseline); ARDL orders selected by the AIC with a maximum of two lags. LR coef.: long-run coefficient in the GDP equation, with $p$-

value in parentheses (delta method); for the specification with REC in logarithms, the coefficient refers to lnREC. Backcast: 1980–1989 values set to the 1990 value (0.038%). WDI energy use: EG.USE.PCAP.KG.OE, kg of oil equivalent per capita, current edition (1990–2021). NREC 2015 values: dataset entry 95,432.55; baseline (decimal correction) 9543.25; interpolated, exp[(ln 6905.84 + ln 10,546.53)/2] = 8534.21; traceable series 6763.83 = 7796.22 × (6905.84/7959.91), where 6905.84 is the IEA-based WDI value for 2014 and 7796.22 and 7959.91 are the current WDI values for 2015 and 2014; the traceable series applies the same ratio (0.8676) to all current WDI values for 2015–2021. ***, ** and * denote significance at the 1%, 5% and 10% levels.

## 5. Discussion

The central result is that we find no statistically detectable long-run relationship in which renewable-energy consumption in Saudi Arabia adjusts to growth, fossil-energy use, FDI and trade, and no robust evidence that these variables help to predict it. This pattern is consistent with—but does not prove—the neutrality hypothesis for renewables and resembles the findings for economies in which renewables account for a very small or traditional share of energy use [22,23]. It differs from Kahia et al. [9], who reported causality from growth to renewable energy in Saudi Arabia over 1990–2016 in a simultaneous-equation framework; the difference may reflect the sample period, the method and the fact that the renewable share remained almost constant at about 0.01% between 1996 and 2016. A plausible interpretation, which our tests do not identify, is that renewable deployment in Saudi Arabia has so far been driven by discrete policy decisions and a few large projects at the very end of the sample, rather than by market responses to income or investment.

The negative short-run association between changes in NREC and REC should be interpreted with care. Because REC is measured as a share of total final energy consumption, faster growth of fossil-energy use mechanically lowers the renewable share unless renewable use grows at least as fast. The weak and lag-dependent predictive causality from NREC to REC does not allow us to conclude that the fossil-energy system supports the expansion of renewables.

The conditional level relationship identified in the GDP equation is driven mainly by trade openness, whose long-run coefficient is positive in all specifications (Table 12), and is consistent with a trade-based growth model in which trade openness largely reflects hydrocarbon exports. The positive long-run association between GDP and REC in the baseline disappears when the 1990 jump created by the imputed zeros is controlled for or when only observed data are used (Section 4.8). It therefore reflects the coincidence of the first observed renewable data with the post-1990 growth path rather than an effect of renewable energy, and, because REC → GDP predictive causality is not robust to the lag length or to multiple testing, the evidence does not support the growth hypothesis for renewable energy.

Trade openness is the most robust predictor in the system: it Granger-causes FDI in every specification and after correction for multiple testing, and it Granger-causes GDP in most specifications. By contrast, the predictive link from FDI to GDP is significant only in the VAR without deterministic dummies, whose FDI equation is serially correlated, and it does not survive once this misspecification is corrected or the tests are adjusted for multiplicity. The link from openness to FDI is compatible with the view that openness attracts foreign investment—for example through export-oriented and market-seeking investment—but this mechanism is an interpretation consistent with the evidence rather than a conclusion established by the tests. FDI does not Granger-cause renewable-energy consumption in any specification and does not enter the long-run REC equation significantly. The data therefore do not show that FDI has contributed to the development of renewable energy in Saudi Arabia; that FDI could become such a channel—for example through

foreign participation in large solar and wind projects—is a possibility that the present results neither establish nor rule out.

## 6. Conclusions, Sustainability and Policy Implications, and Limitations

This study examined the dynamic interactions among renewable and non-renewable-energy consumption, economic growth, FDI and trade openness in Saudi Arabia over 1980–2021, using bootstrap ARDL bounds tests and Toda–Yamamoto causality tests. Four findings are directly supported by the data. First, there is no statistically detectable long-run level relationship in which renewable-energy consumption adjusts to growth, fossil-energy use, FDI or trade; this result holds across lag selection, shock dummies and alternative treatments of the missing REC data, although the evidence becomes inconclusive when only observed data with REC in levels are used. Second, a conditional long-run relationship normalised on GDP exists in the full sample and is driven mainly by trade openness; it is sensitive to the treatment of structural shocks and to the sample period, and the positive GDP–REC coefficient of the baseline is not robust to the treatment of the pre-1990 data and is therefore not interpreted as an effect of renewable-energy. Third, trade openness Granger-causes FDI in all specifications and after adjustment for multiple testing, and Granger-causes GDP in most specifications, whereas no variable—including FDI—robustly predicts renewable energy consumption. Fourth, predictive causality from renewable energy and from FDI to GDP is weak and not robust to the lag length, to the correction of serial correlation or to multiple testing.

The study contributes to the literature by showing that, in the largest hydrocarbon exporter, there is not yet any statistically detectable evidence that renewable energy has become integrated into macroeconomic dynamics: it neither robustly follows growth and foreign investment nor robustly predicts growth. It also shows how conclusions about the energy–growth nexus depend on consistent data handling and the treatment of missing observations, on distinguishing conditional long-run associations from predictive causality, and on inference that is appropriate for small samples.

Contribution to sustainable development. The results contribute to the sustainability debate in four ways. First, they quantify the starting point of the Saudi energy transition: over four decades, the renewable share—the indicator used to monitor SDG target 7.2—has remained too small and too disconnected from growth, fossil-energy use, FDI and trade for a robust macroeconomic link to be detected. Progress toward SDG 7 and toward the national renewable-capacity and net-zero targets therefore cannot be inferred from the expansion of the economy and should be monitored directly with the renewable share and related sustainability indicators. Second, the conditional long-run relationship for GDP is driven mainly by trade openness, which largely reflects hydrocarbon exports, while fossil fuels still supply almost all final energy. This pattern is consistent with a growth model that remains dependent on hydrocarbons and indicates that sustained growth progressively decoupled from fossil-fuel dependence (SDG target 8.4) remains a challenge rather than an achievement of the sample period. Third, the absence of a predictive link from FDI to renewable energy suggests that aligning investment promotion with sustainability objectives, so that foreign capital and technology are directed toward clean-energy projects, is an open policy issue rather than an outcome that can be taken for granted. Fourth, the empirical framework—bootstrap bounds tests under all normalisations, Toda–Yamamoto tests corrected for serial correlation and multiple testing, and explicit sensitivity analysis for data gaps—is transparent and replicable; it can be re-estimated as post-2021 data become available, or applied to other hydrocarbon exporters, to monitor whether the energy–growth–investment nexus moves toward a sustainable path. In this sense, the study provides a tool for measuring and monitoring the macroeconomic

integration of renewable energy, which is a precondition for a sustainable, low-carbon development model.

Other statements in this paper are interpretations that are consistent with these findings but are not established by them. The absence of a detectable link between renewables and the macroeconomy may reflect the fact that renewable deployment has so far been driven by discrete public programmes and a few large projects at the end of the sample; the predictive link from trade openness to FDI may reflect export-oriented investment; and FDI may in future become a channel of renewable diffusion. In particular, the results do not show that FDI has contributed to the development of renewable energy in Saudi Arabia. These explanations are hypotheses for future research rather than conclusions of the present study.

Some cautious policy implications follow; they are implications of the absence of detectable links, not results of a policy evaluation. Because the historical data show no robust response of renewable energy to growth or FDI, they provide no basis for expecting the renewable targets of Vision 2030 to be reached as a by-product of economic expansion or foreign investment. They do not, however, allow an assessment of whether these targets will be met: the sample ends in 2021, contains only a few years after the launch of Vision 2030 and precedes most large renewable projects, and the observed-data evidence for REC is inconclusive. Dedicated instruments—such as competitive auctions, long-term power-purchase agreements or incentives that direct FDI toward renewable projects—are plausible ways of supporting renewable deployment, but the present models estimate associations and predictive relationships, not the effects of these instruments, whose evaluation would require project-level or policy-specific data. The robust predictive role of trade openness for FDI supports continued openness as part of diversification. These implications are suggestive: they are derived from associations and predictive relationships, not from estimated structural effects of policy.

The study has several limitations. The sample contains only 42 annual observations, which limits the power of the tests, so that the failure to detect a relationship should not be equated with evidence of its absence. REC is not available before 1990 and was set to zero for 1980–1989 in the baseline; the conclusions about REC are robust to alternative treatments of these years, but the observed-data evidence is inconclusive and the baseline GDP–REC coefficient depends on the imputation. REC also varies very little after 1990, and the NREC series combines IEA-based WDI data for 1980–2014 with 2015–2021 values that could not be traced to the same series, which produces a level shift after 2014, and its 2015 entry required a correction that cannot be verified against a published source. Interpolating the 2015 value, controlling for the shift or using fully traceable WDI series leaves the conclusions about REC and GDP unchanged, but the FDI level relationship is not found with the traceable series. The causality analysis involves 20 pairwise tests with 39 observations, so isolated rejections may occur by chance; only the relationships that survive multiple-testing adjustment are therefore emphasised. Granger causality captures predictive content and not structural causation, and potentially relevant variables such as oil prices, energy subsidies, capital stock and $CO_2$ emissions are omitted. Finally, the post-2016 Vision 2030 period covers only a few observations, so the effects of the most recent renewable projects cannot yet be identified and the results cannot be used to assess whether the Vision 2030 targets will be achieved. Future research could use quarterly or sectoral data, nonlinear or Fourier ARDL models and longer post-2020 samples to identify the mechanisms suggested here.

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