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# The Natural Resources Curse and Critical Raw Materials: Comparing Dependence and Abundance Measures

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# The Natural Resources Curse and Critical Raw Materials: Comparing Dependence and Abundance Measures

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**Abstract**

This paper revisits the Natural Resource Curse (NRC) in the case of Critical Raw Materials (CRM), a strategic input for the energy transition. Using USGS production data for the 1970–2019 period, we distinguish between dependence (CRM production as a share of GDP) and abundance (CRM production per capita). We argue that dependence is endogenous and a misleading measure of resource endowment. Our cross-sectional model averaging estimates show no robust impact of CRM on growth. Panel estimations for 44 countries, using advanced static and dynamic estimators, point to the same conclusion: weak and inconsistent results, with the apparent negative effect of CRM dependence largely driven by endogeneity. Overall, our findings reject the idea of a universal CRM curse. Instead, they highlight how measurement choices and endogeneity critically shape the NRC debate. While appropriate policies should address potential negative externalities and encourage diversification as well as the capture of greater value added along the CRM supply chain, our findings suggest that, to ensure long-term growth, traditional factors–such as human capital accumulation, infrastructure, and institutional quality and stability–should remain central.

**Keywords:** natural resources curse; critical raw materials; natural resources abundance; natural resources dependence

**JEL Classifications:** O13, Q32

## 1 Introduction

In recent years, Critical Raw Materials (CRM) have gained growing attention from policymakers due to their essential role in present and future manufacturing industries. By definition, CRM are strategic inputs for economic growth, national security, and the transition towards a net-zero

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economy. Without them, electrification and digitalization would be impossible. Their criticality stems both from their economic importance and from the high supply risks they involve (European Commission, 2017). The International Energy Agency projects that demand for CRM may outpace supply in the coming decades, raising concerns about future shortages and vulnerabilities (IEA, 2024). In this context, each country is identifying these materials and responding accordingly. Through policy initiatives, they aim to minimize supply risks as well as maximize potential benefits from resource extraction amid growing geopolitical tensions.

In response, governments have launched ambitious initiatives. The European Union adopted the Critical Raw Materials Act in 2024, while the United States issued an executive order in March 2025 to increase domestic extraction. Resource-rich countries such as Australia and Canada have also set out strategies to capture more value from their mineral endowments. Yet, what seems to have become an all-out race between economic superpowers to secure access to CRM is not necessarily a win–win endeavor (Arezki and Van der Ploeg, 2025). For producing countries, especially less industrialized ones, the hope is to leverage these resources to move up the global value chain. However, export restrictions and resource nationalism may generate tensions and, at the global level, even slow down the green transition (Kowalski and Legendre, 2023).

This policy debate echoes a long-standing question in economics: are natural resources a "boon" or a "curse"? Since the seminal work of Sachs and Warner (1995), the so-called Natural Resource Curse (NRC) hypothesis has fueled a vast literature. While many studies have documented a negative relationship between resource dependence and growth, results remain highly heterogeneous (Frankel, 2010; Van der Ploeg, 2011; Havranek et al., 2016; Badeeb et al., 2017; Dauvin and Guerreiro, 2017). A key reason lies in the measurement of resource endowment. Most contributions rely on dependence measures—such as primary exports or rents as a share of GDP—that are highly endogenous. By contrast, few studies explore abundance measures—such as production or reserves per capita—that may better capture exogenous variation in resource wealth (Brunnschweiler and Bulte, 2008; Shahbaz et al., 2019). To date, however, the NRC has not been examined through the lens of CRM, despite their unique economic and geopolitical relevance.

This paper aims to fill this gap. Using newly compiled data on CRM production between 1970 and 2019, we revisit the NRC hypothesis by explicitly comparing dependence and abundance measures. We also exploit both cross-sectional and panel approaches to assess the robustness of results across time and methodologies. Our contributions are fourfold. First, we provide the first cross-country assessment of the NRC in the case of CRM. Second, we clarify the informational content of dependence versus abundance measures, showing that they capture very different economic realities. Third, we reassess the stability of the NRC over time using rolling-window estimations. Fourth, we apply advanced panel estimators that account for slope heterogeneity and cross-sectional dependence, highlighting the risks of endogeneity in standard approaches.

Overall, our results suggest that CRM cannot be considered a robust source of a resource curse. While dependence measures sometimes yield negative effects, these appear largely driven by endogeneity rather than by genuine causal mechanisms. By contrast, abundance measures provide little evidence of adverse long-run impacts. These findings underline the importance of carefully distinguishing between dependence and abundance when analyzing the economic consequences of resource wealth, and they call for caution in drawing strong policy conclusions about a potential "CRM curse."

The rest of the paper is organized as follows. Section 2 reviews the NRC literature. Section 3 presents our data, and Section 4 describes the methodology. Results, including robustness checks, are discussed in Section 5. In section 6, we further discuss how these results can be articulated with the existing literature as well as recent geostrategic and geo-economic trends. Section 7 concludes the paper.

## 2 Literature review

The Natural Resource Curse (NRC) literature has developed along several major lines. Its first foundation is the Dutch Disease (DD) hypothesis, which became prominent with the work of Corden and Neary (1982). The DD emphasizes how a resource boom can weaken the competitiveness of the non-resource tradable sector—whether agricultural or industrial—often regarded as the main driver of productivity and long-term growth (Mien and Goujon, 2022).

This line of research was extended by Auty (1993), who coined the expression natural resource curse thesis in reference to Gelb (1988), highlighting how resource-rich countries could be trapped in suboptimal policies, while resource-poor countries were forced into more efficient specialization (Auty, 1994).

Building on this perspective, Sachs and Warner (1995) provided the first comprehensive cross-country econometric analysis of the relationship between resource wealth and growth. Although they referred to resource abundance in their title, their main explanatory variable was the share of primary exports in GDP (SXP), which is more properly defined as a measure of dependence. Controlling for initial income, investment, openness, and institutional quality, they showed a robust negative correlation between natural resource dependence and growth during 1971–1989. Despite revisions, their work ultimately culminated in Sachs and Warner (2001), where they reaffirmed that the NRC is a "reasonably solid fact."

From this starting point, the literature expanded to investigate two fundamental questions: *Is the NRC true? If so, through which mechanisms?* Various channels have been identified[1], including the price trends hypothesis, Dutch Disease effects, and the volatility channel (Van der Ploeg and Poelhekke, 2009; Cavalcanti, Mohaddes, et al., 2011). Others have stressed political economy and institutional explanations: resource-rich countries may adopt procyclical or inefficient policies

[1]For a review, see Frankel (2010), Van der Ploeg (2011) and Badeeb et al. (2017)

(Auty, 1994; Frankel, 2010; Badeeb et al., 2017), underinvest in education (Gylfason, 2001), or fall prey to corruption, rent-seeking, and increased conflict risk. For these reasons, empirical work generally controls for institutional quality, human capital, investment, and openness, following Sachs and Warner (1995).

Despite extensive research, consensus remains elusive. Havranek et al. (2016) report in their meta-analysis that the mean effect is "negligible." Dauvin and Guerreiro (2017) similarly identify a "soft" NRC that applies mainly to developing countries. Both meta-analyses highlight sources of heterogeneity, including institutional interactions, investment controls, time horizons, dataset types, and above all, measurement choices.

The distinction between *dependence* and *abundance* has been particularly influential. Early studies often conflated the two, treating exports or rents as proxies for endowment. Critics, however, noted that such measures are endogenous: dependence reflects the lack of alternative income sources rather than exogenous resource wealth (Stijns, 2005; Ding and Field, 2004; Brunnschweiler and Bulte, 2008). Brunnschweiler and Bulte (2008) famously described the NRC as a "red herring," showing that resource abundance, measured by World Bank (1997) subsoil wealth, is positively correlated with growth, and that once dependence is instrumented by abundance, its negative effect disappears. They stressed the elevated risk of a negative omitted-variable bias when using dependence measures. In a cross-sectional framework, which is most common in the NRC literature, these measures are likely to absorb the effects of any unobserved variable that explain both initial increased reliance on natural resources extraction and lower subsequent growth.

More generally, measurement choices have been at the center of the debate in this literature. While abundance is often defined as a stock (reserves), and dependence as a flow (exports as % of GDP), normalization plays a decisive role. For example, Gylfason (2001) used natural capital as a share of total capital, but this variable can also be endogenous, since countries with low human and physical capital appear more resource-abundant by construction[2]. Stijns (2005) further argued that production and reserves are highly correlated, making production a reasonable proxy for abundance if not normalized in an endogenous way. Norman (2009) attempted to reconstruct stock measures by combining reserve bases with historical production, while Van der Ploeg and Poelhekke (2010) stressed the limitations of World Bank estimates of natural capital. In sum, dependence measures tend to produce evidence of a curse, whereas abundance measures often suggest positive or insignificant effects (Brunnschweiler and Bulte, 2008; Alexeev and Conrad, 2009; Van der Ploeg and Poelhekke, 2010; Smith, 2015; Badeeb et al., 2017).

Finally, outcomes also depend on the type of resource considered. Havranek et al. (2016) note that studies on oil are less likely to find evidence of a curse. Sachs and Warner (1995) aggregated agriculture, fuel, and non-fuel mining, but subsequent work has emphasized the need to distinguish

[2] Especially when, due to data availability, natural capital is measured at the end of the growth period studied.

among them (Stijns, 2005). Recent studies focusing on minerals and metals yield mixed evidence[3]: Stijns (2005) found no effect of non-fuel mineral reserves, Boschini et al. (2007) reported that institutional quality mediates the effect of mineral dependence, Norman (2009) and Van der Ploeg and Poelhekke (2010) obtained contrasting results depending on specification, while more recent panel analyses offer diverging conclusions (Shahbaz et al., 2019; Sharma and Pal, 2021; Bildirici and Gokmenoglu, 2020; Ben-Salha et al., 2021; Ozcan et al., 2023). Similarly, results at the regional level are mixed (Cavalcanti, Da Mata, et al., 2019; Corrocher et al., 2020; Zuo and Zhong, 2020).

In summary, the NRC literature demonstrates that the relationship between natural resources and growth is highly sensitive to methodology, measurement, and context. While early evidence suggested a universal curse, more recent studies stress heterogeneity and caution against generalization. This is particularly relevant for CRM, which differ from traditional resources and remain underexplored in the NRC framework.

Taking a step back from NRC-focused studies, broader analyses on empirical growth predictors based on cross-sectional data yield mixed evidence regarding the NRC. Bayesian Model Averaging (BMA) analyses for the 1960-1992 time-period, show that primary exports as a share of total exports are not a key determinant of growth once model uncertainty is taken into account (Fernández et al., 2001; Sala-I-Martin, 1997). Ranking regressors according to posterior inclusion probabilities, these studies show that primary exports are the $20^{th}$ and $11^{th}$ most relevant variables respectively. More importantly, mining dependence (which includes fuel but accounts for production rather than exports) is consistently identified as a more robust, positive driver of growth within the same framework. It should however be noted that mining dependence is computed for 1988, i.e. toward the end of the sample period, which calls for caution in interpretation. Nonetheless, the resulting endogeneity bias is likely to be negative. At face value, this would make the NRC a matter of trade specialization rather than natural resources extraction.

# 3 Data

## 3.1 CRM production measures: dependence and abundance

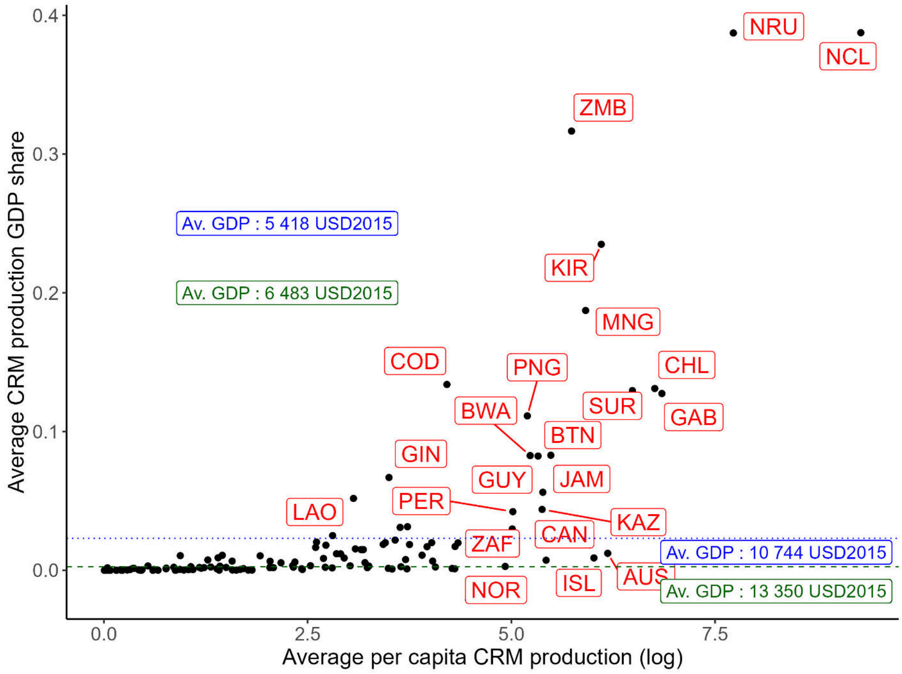
As outlined above, the distinction between resource dependence and resource abundance is central to the NRC debate. In this paper, we construct two CRM production measures using data from the U.S. Geological Survey (USGS Mineral Yearbooks (USGS, 2025a) and Mineral Commodity Summaries (USGS, 2025b)) over the 1970–2019 period, covering 30 of the 34 CRM identified by the European Commission (2023).[4]

To aggregate CRM production across minerals, we use the trend of the unit value per ton provided by the USGS (Kelly and Matos, 2022). This trend is extracted with a Hodrick–Prescott

[3] A summary of papers closest to our scope and/or methodology is given in Table A.1

[4] For more details see Appendix B. Critical raw materials production data collection

Figure 1: Average of dependence and abundance measures 1970-2019




Note: This figure displays CRM dependence (y-axis) and abundance (x-axis) measures. Each point represents a country, positioned according to its average CRM dependence and abundance over the period 1970–2019. Missing values were excluded when calculating the averages. The blue dotted line is the average CRM dependence while the green dashed line is the median. Boxed GDP values report the average real per capita GDP level above and below these thresholds. Country ISO codes are used as labels (see Table D.2).
Source: Author's calculations based on data extracted from the USGS and WDI

(HP) filter (Hodrick and Prescott, 1981), which smooths short-term fluctuations while capturing the long-term dynamics of commodity prices (see Appendix C. Critical raw materials production data aggregation for more details). The resulting series, expressed in current USD, serves as a common weighting scheme to aggregate CRM production into a single monetary measure.

From this aggregate, we derive two indicators: (i) CRM dependence, defined as CRM production expressed as a share of GDP, and (ii) CRM abundance, defined as the logarithm of CRM production per capita in 2015 USD. GDP data are extracted from the World Development Indicators (WDI) database (World Bank).

This approach follows the reasoning of Shahbaz et al. (2019), who argue that the distinction between dependence and abundance stems mainly from normalization rather than from opposing stocks and flows. While dependence measures tend to reflect the lack of alternative sources of income and are therefore more likely to be endogenous, abundance measures are closer to exogenous endowments.

Figure 1 illustrates the correlation between the two measures. Among the studied countries, high CRM production as a share of GDP often equates to high production value per capita; however the reverse does not always hold. High-income countries such as Australia (AUS), Canada (CAN), Iceland (ISL), and Norway (NOR) display high abundance levels but relatively low dependence.

On average, real GDP per capita in our sample is USD 9,890, while CRM production accounts for 2.3% of GDP. Among the 21 countries above the average dependence level, mean GDP per capita is USD 5,418, compared to USD 10,744 for the 106 countries below. This gap becomes even more pronounced when comparing countries above and below the median dependence level.

## 3.2 Dependent and control variables

Our dependent variable is the real GDP per capita (expressed in 2015 USD), extracted from WDI data. This indicator captures long-term economic performance and is consistent with the growth literature following Sachs and Warner (1995).

In addition to our CRM production measures, we introduce several control variables to account for the main growth determinants as proposed in Fernández et al. (2001) and Sala-I-Martin (1997) and NRC transmission channels identified in the literature (see Section 2):

- Investment: the share of gross capital formation in GDP, drawn from WDI. This variable captures capital accumulation, a well-known key driver of growth. In our panel estimations, we use the log of the real per capita gross capital formation in the manner of Shahbaz et al. (2019).

- Trade and economic openness: two measures are included. We use the log of real trade per capita as a *de facto* indicator of trade intensity, and the KOF Economic Globalisation Index[5] (Gygli et al., 2019), which combines *de jure* and *de facto* indicators of trade and financial integration. This hybrid index has become a standard measure of openness due to its wide coverage and conceptual clarity (Gräbner et al., 2021).

- Institutional quality: the rule of law index from the Varieties of Democracy (V-Dem) project (Coppedge et al., 2025). This variable proxies institutional stability and governance quality, which are crucial in NRC-related mechanisms such as rent-seeking and corruption.

- Human capital: the education index from the UNDP Human Development Reports,[6] based on expected years of schooling (EYS) and mean years of schooling (MYS). As data are only available from 1990 onward, earlier values are reconstructed through linear interpolation using data points for 1980 and 1985 (Education Index Dataset 1980-2013). This ensures continuity in our cross-sectional analysis, though the interpolated series is excluded from panel regressions due to data unavailability from 1970 to 1979. As an alternative control for human capital, we use life expectancy at birth from WDI.

- Primary exports: the share of agricultural raw materials, fuels, ores, and metals in GDP, computed using WDI data. This measure replicates the well-known "SXP" indicator of Sachs

[5] KOF Time Series Explorer (accessed 2025-02-28)
[6] HDR-UNDP data center (accessed 2025-02-27)

and Warner (1995), allowing direct comparability with the early NRC literature. Based on this data, we compute real per capita primary exports to distinguish between primary exports abundance and dependence.

- Cultural differences: the population shares of Confucians, Muslims and Protestant Christians from Maoz and Henderson (2013), which are found to be relevant in BMA analyses.
- Geographic controls: dummy variables for Latin American and Sub-Saharan countries.

Not all variables are included in each of our empirical approaches. For a clear summary, see Table D.1.

Using these variables, we construct a balanced panel of 44 countries over the period 1970–2019.[7] This large dataset provides both broad cross-country coverage and sufficient time variation to apply panel estimators that address slope heterogeneity and cross-sectional dependence, two critical issues in the NRC literature.

# 4 Methodology

To assess the relationship between CRM and economic growth, we adopt a two-step empirical strategy. First, we conduct a cross-sectional analysis in the spirit of Sachs and Warner (1995), which allows us to benchmark our results against the early NRC literature and to explore the robustness of dependence and abundance measures. Second, we turn to panel data methods, which make it possible to exploit both the time and country dimensions of our dataset, and to address issues such as slope heterogeneity and cross-sectional dependence that are often overlooked in cross-section studies.

## 4.1 Cross-section analysis

Building upon the Sachs and Warner (1995) framework, we consider the following specification:

$$GDP_i = \beta_0 + \beta_1 NatRes_i' + \beta_X X_i' + \varepsilon_i \tag{1}$$

Where $GDP_i$ is the per annum real per capita growth rate of GDP, $NatRes_i$ a vector of primary exports and CRM abundance and dependence measures, $X_i$ a vector of controls and $\beta_X$ the vector of corresponding coefficients. As in Magnus, Powell, et al. (2010), we include only initial values instead of averages over the sample for every variable, except investment to limit endogeneity. However, as we are constrained by data availability, we define the initial value as the first non-missing value contained in the first half of the estimation window.

[7] The list of included countries in the balanced panel data is given in Table D.2 while descriptive statistics for our variables are given in Table D.3

As model uncertainty is a long-standing problem in growth literature, we compare standard OLS regressions, controlling for initial GDP, economic openness, institutional quality, human capital and investment (closest in spirit with Sachs and Warner (1995)) with the Weighted-Averaged Least Squares (WALS) method (Magnus, Powell, et al., 2010) with additional geographic and cultural controls. As a Bayesian combination of frequentist estimators, this model averaging method allows for a time-efficient computation with proper handling of uncertainty. As pointed out in Section 2, there is some evidence that NRC results may be time-sensitive and/or that a negative significant result is less likely for larger time samples. Thus, we leverage our fifty-year sample to estimate coefficients using a rolling-window approach. We present results for 20-, 30- and 40-year windows.

For our WALS estimations, we include inital GDP, investment, and the share of Confucians as focus regressors (which are variables forced into the estimated model at all times), based on results of Fernández et al. (2001) and Sala-I-Martin (1997). All other regressors are treated as auxiliary, reflecting uncertainty regarding their inclusion in the model, with each natural resources variables separately. We employ the reflected Weibull prior with the default parameter values proposed by Magnus and De Luca (2016) which is both neutral and robust.

## 4.2 Panel analysis

Turning to the panel analysis and given the presence of cross-sectional dependence[8] and the dimensions of our panel datasets, we first consider three dynamic estimators : the cross-sectionally augmented distributed lag (CS-DL) estimator, the cross-sectionally augmented autoregressive distributed lag (CS-ARDL) estimator (Chudik et al., 2015) and the Pooled Mean Group (PMG) estimator (Pesaran, Shin, et al., 1999).[9] The CS-DL and CS-ARDL both assume full slope heterogeneity, while the PMG estimator assumes long-run slope homogeneity and the existence of a long-run relationship between the dependent variable and the regressors. Starting from the simplified case of the following ARDL(1,1) in which we omit cross-sectional averages:

$$y_{i,t} = \mu_i + \delta_{i0} X_{i,t} + \delta_{i1} X_{i,t-1} + \lambda_i y_{i,t-1} + \varepsilon_{i,t} \tag{2}$$

The error-correction equation can be written as:

$$\Delta y_{i,t} = \phi_i (y_{i,t-1} - \theta_{i0} - \theta_{i1} X_{i,t}) - \delta_{i1} \Delta X_{i,t} + \varepsilon_{i,t} \tag{3}$$

And the long-run coefficients are:

$$\theta_{i0} = \frac{\mu_i}{1-\lambda_i}, \theta_{i1} = \frac{\delta_{i0} + \delta_{i1}}{1-\lambda_i}, \phi_i = -(1-\lambda_i) \tag{4}$$

[8]The results of the CD test from Pesaran (2015) are displayed in Table D.4, and show that the null hypothesis of weak cross-sectional dependence is always rejected.

[9]These estimators have been proposed to account for slope heterogeneity and cross-sectional dependence, as standard fixed effect estimators are biased when the time and individual dimensions grow large.

In accordance with the PMG estimator assumptions, $\theta_{xi} = \theta_x, \forall i, x$, meaning that all countries share the same long-run relationship between GDP and the regressors.

The CS-DL estimator allows for a direct estimation of long-run effects:

$$y_{it} = c_{yi} + \theta_i' x_{it} + \sum_{l=0}^{p-1} \delta_{il} \Delta x_{i,t-l} + \sum_{l=0}^{p_{\bar{y}}} \omega_{y,il} \bar{y}_{t-l} + \sum_{l=0}^{p_{\bar{x}}} \omega_{x,il}' \bar{x}_{t-l} + e_{it} \tag{5}$$

Where $\theta_i'$ denotes the long-run effect of $x_i$, and $\bar{y}$ and $\bar{x}$ are the cross-sectional averages. Chudik et al. (2015) recommend using the following lag order: $p_{\bar{x}} = \left[T^{1/3}\right]$, $p_{\bar{y}} = 0$ and $p = p_{\bar{x}}$. For T = 50, this means that we should optimally include 3 lags of the cross-sectional averages. The CS-DL estimator has two advantages. First, it has better performance for panels with a smaller time dimension ($30 < T < 100$). Second, it is not as sensitive as the CS-ARDL to lag-order specification. However, it is biased in case of feedback loops between lagged values of the dependent variable and the regressors. Since GDP is a component of the CRM dependence measure, this could be problematic because of the mechanical feedback loop between lagged values of GDP and lagged values of CRM dependence. The CS-ARDL estimator allows us to compute long-run effects in two steps. First, short-run coefficients are estimated using the following equation:

$$y_{it} = c_{yi}^* + \sum_{l=1}^{p_y} \varphi_{il} y_{i,t-l} + \sum_{l=0}^{p_x} \beta_{il}' x_{i,t-l} + \sum_{l=0}^{p_{\bar{z}}} \psi_{il}' \bar{z}_{t-l} + e_{it}^* \tag{6}$$

With $\bar{z}_t = (\bar{y}_t, \bar{x}_t')'$ and $p_{\bar{z}} = \left[T^{1/3}\right]$. Then, the long-run effect is obtained by computing:

$$\hat{\theta}_{CS-ARDL,i} = \frac{\sum_{l=0}^{p_x} \hat{\beta}_{il}}{1 - \sum_{l=1}^{p_y} \hat{\varphi}_{il}} \tag{7}$$

According to Chudik et al. (2015), this estimator is robust to joint determination of the dependent variable and the regressors. It should be robust to reverse causality, which would make it particularly adapted to estimate the effect of CRM dependence on GDP. However, this estimator can be sensitive to the lag-order specification, especially with smaller panels.

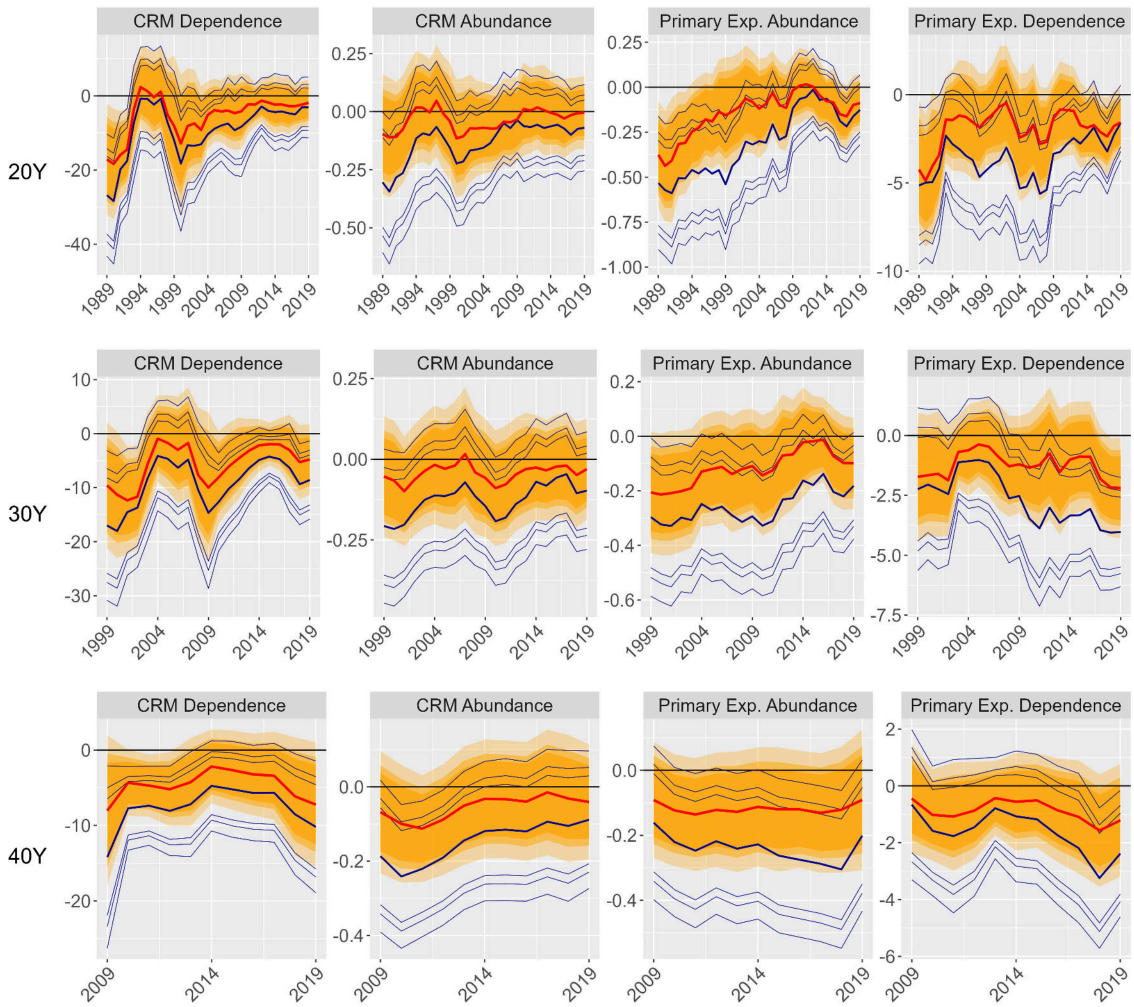
Finally, for the sake of comparison, we also provide the results obtained using static mean group (MG) and common correlated effects mean group (CCEMG) estimators (Pesaran and Smith, 1995; Pesaran, 2006), in line with Shahbaz et al. (2019).

# 5 Empirical results and robustness checks

## 5.1 Rolling-window regressions

Figure 2 reports the results of our rolling-window estimations with 20-, 30-, and 40-year windows for our four natural resources variables. Introducing more controls and accounting for model uncertainty yields less negative and significant coefficients consistently across window sizes and variables, implying some degree of omitted variable bias. We find that the OLS coefficient for primary export dependence is less likely to be significant when expanding the window size, in line with the results of Havranek et al. (2016). However, we find an opposite trend for CRM dependence. Our WALS results suggest some support for a CRM dependence negative effect but no effect of CRM abundance. On the contrary, we find similar results for both normalization strategies in the case of primary exports. The abundance measure even yields slightly more significant results.

Figure 2: WALS and OLS coefficients




Note: This figure displays the WALS and OLS rolling-window estimation results based on a 20-, 30-, and 40-year rolling window. WALS coefficients are traced in red. The 90%, 95% and 99% confidence intervals are shown in orange. OLS coefficients are traced in blue with outer thin blue lines corresponding to the same confidence intervals. Years on the x-axis indicate the last year included in the estimation window.

Figures E.1 to E.6 report our results for control variables, estimated when including either CRM or primary export dependence[10]. Across specifications, the most consistent finding is the strong and statistically significant positive effect of investment and the negative effect of initial

[10]Regarding controls, there is little to no variation between dependence or abundance measures. Any estimation results that are not presented here for the sake of brevity are available upon request.

GDP. These results are fully in line with the broader growth literature. Other control variables yield more mixed evidence. The positive effect of the share of Confucians and the negative effect of the Sub-Saharan Africa dummy become more statistically significant as the window size increases. A similar pattern is found for the positive effect of life expectancy, but it is less significant when including CRM measures. Rule of Law, Education and Globalization Index (ECGI) are often positively signed but only intermittently significant. Finally, the share of Protestants and Muslims and the Latin American dummy almost never reach significance.

Importantly, these rolling-window results should not be interpreted as genuine time variation in coefficients. Rather, they highlight the sensitivity of the findings to sample coverage and data availability, which expand over time. The lack of consistent statistical significance for CRM variables across most windows strongly suggests that CRM production has no stable effect on growth. This observation also calls into question earlier studies relying on short samples: the significant negative coefficients for CRM dependence for the five first 20-year windows are insufficient to claim evidence of a CRM curse.

In sum, our cross-sectional rolling-window analysis provides little to no support for the existence of a robust CRM curse or traditional NRC.

## 5.2 Panel regressions

We begin our panel analysis with static estimators. The Mean Group (MG) results (Table 1) indicate no significant effect of CRM production—whether measured as abundance or dependence—on GDP growth. As expected, trade per capita and investment per capita exert positive and significant effects, while institutional quality is not significant. Using the CCEMG estimator (Table 2) to account for cross-sectional dependence, we again find positive effects of trade and investment, but a significant negative effect of CRM dependence across specifications. A negative effect of CRM abundance emerges in the simplest model but disappears once additional controls are included.

To compare the CS-ARDL and PMG estimators, we use the Hausman specification test (Hausman, 1978). The test results indicate that the CS-ARDL and CS-DL estmators—both of which allow for full slope heterogeneity—are more appropriate[11]

As shown in Table 3, CS-ARDL results provide no evidence of a CRM abundance blessing, nor do they offer robust support for the existence of a CRM dependence curse. CS-DL estimates (Table 4) reveal a more consistent negative effect of CRM dependence while the effect of CRM abundance is equally fragile.

As noted earlier, given the size and structure of our panel, the CS-DL estimator should be the most reliable. Taken at face value, our results therefore suggest the existence of a CRM dependence

[11]The results of the Hausman test are available upon request. Furthermore, when using the Pooled Mean Group (PMG) estimator the error-correction term is not significant (as can be seen in Table E.1), implying no long-run relationship between our dependent variable and the regressors.

Table 1: MG Static panel estimation

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| (Intercept) | $4.69^{***}$ | $3.89^{***}$ | $2.35^{*}$ | $4.67^{***}$ | $3.89^{***}$ | $2.95^{***}$ |
| | (0.23) | (0.23) | (1.21) | (0.25) | (0.24) | (1.08) |
| CRM abundance | 0.03 | 0.02 | 0.01 | | | |
| | (0.03) | (0.02) | (0.02) | | | |
| Trade | $0.49^{***}$ | $0.33^{***}$ | $0.31^{***}$ | $0.52^{***}$ | $0.35^{***}$ | $0.33^{***}$ |
| | (0.03) | (0.03) | (0.04) | (0.03) | (0.03) | (0.03) |
| Investment | | $0.27^{***}$ | $0.28^{***}$ | | $0.28^{***}$ | $0.28^{***}$ |
| | | (0.03) | (0.03) | | (0.03) | (0.03) |
| Institutional qual. | | | 1.37 | | | 0.71 |
| | | | (1.22) | | | (1.13) |
| CRM dependence | | | | $-129.42^{*}$ | $-94.09$ | $-97.55$ |
| | | | | (67.65) | (61.24) | (60.45) |
| Observations | 2200 | 2200 | 2200 | 2200 | 2200 | 2200 |

This table reports the mean group (MG) estimation results

Standard errors are in parentheses, $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 2: CCEMG Static panel estimation

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| CRM abundance | $-0.03^{**}$ | $-0.00$ | 0.00 | | | |
| | (0.01) | (0.01) | (0.01) | | | |
| Trade | $0.26^{***}$ | $0.10^{***}$ | $0.08^{***}$ | $0.28^{***}$ | $0.12^{***}$ | $0.09^{***}$ |
| | (0.03) | (0.03) | (0.02) | (0.03) | (0.03) | (0.02) |
| Investment | | $0.24^{***}$ | $0.21^{***}$ | | $0.23^{***}$ | $0.20^{***}$ |
| | | (0.02) | (0.02) | | (0.02) | (0.02) |
| Institutional qual. | | | 0.28 | | | 0.31 |
| | | | (0.46) | | | (0.42) |
| CRM dependence | | | | $-140.81^{**}$ | $-48.64^{**}$ | $-31.46^{**}$ |
| | | | | (66.05) | (19.39) | (13.94) |
| Observations | 2200 | 2200 | 2200 | 2200 | 2200 | 2200 |

This table reports the common correlated effects mean group (CCEMG) estimation results.

Standard errors are in parentheses, $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 3: CS-ARDL dynamic long-run effect panel estimation

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| GDP (lagged) | $-0.136^{***}$ | $-0.240^{***}$ | $-0.322^{***}$ | $-0.399^{***}$ | $-0.130^{***}$ | $-0.250^{***}$ | $-0.344^{***}$ | $-0.441^{***}$ |
| | (-6.02) | (-7.33) | (-7.82) | (-7.64) | (-8.72) | (-8.29) | (-7.50) | (-8.10) |
| CRM abundance | 0.161 | $-0.0701^{*}$ | 0.144 | -0.395 | | | | |
| | (0.60) | (-1.85) | (1.20) | (-1.03) | | | | |
| Trade | | $0.826^{***}$ | -0.488 | -0.182 | | $0.618^{***}$ | 0.161 | $0.320^{**}$ |
| | | (3.49) | (-0.86) | (-0.15) | | (4.01) | (0.82) | (2.22) |
| Investment | | | 0.245 | 0.342 | | | $0.342^{***}$ | $0.265^{***}$ |
| | | | (1.51) | (1.46) | | | (3.73) | (3.84) |
| Institutional qual. | | | | 7.267 | | | | -2.654 |
| | | | | (0.82) | | | | (-0.93) |
| CRM dependence | | | | | $-137.4^{*}$ | $-193.5^{**}$ | -9.549 | 105.2 |
| | | | | | (-1.85) | (-2.32) | (-0.09) | (0.76) |
| Observations | 2024 | 2024 | 2024 | 2024 | 2024 | 2024 | 2024 | 2024 |

This table reports the cross-sectionally augmented autoregressive distributed lag (CS-ARDL) estimation results.

t-statistics are in parentheses, $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 4: CS-DL dynamic long-run effect panel estimation

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| CRM abundance | -0.00805 | -0.0468*** | -0.00920 | -0.00320 | | | | |
| | (-0.38) | (-2.64) | (-0.74) | (-0.32) | | | | |
| Trade | | 0.312*** | 0.128*** | 0.0952*** | | 0.315*** | 0.154*** | 0.0957*** |
| | | (7.77) | (2.99) | (3.52) | | (9.37) | (4.20) | (3.73) |
| Investment | | | 0.240*** | 0.200*** | | | 0.239*** | 0.207*** |
| | | | (8.10) | (10.58) | | | (8.98) | (11.91) |
| Institutional qual. | | | | 0.459 | | | | -0.224 |
| | | | | (0.66) | | | | (-0.25) |
| CRM dependence | | | | | -192.9** | -197.2** | -96.15** | -50.13** |
| | | | | | (-2.29) | (-2.27) | (-2.23) | (-2.57) |
| Observations | 1980 | 1980 | 1980 | 1980 | 1980 | 1980 | 1980 | 1980 |

This table reports the cross-sectionally augmented distributed lag (CS-DL) estimation results.
t-statistics are in parentheses, $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

curse. However, we have consistently argued that dependence measures are problematic because of their endogeneity. Specifically, these measures tend to be negatively biased due to their inverse correlation with GDP: countries with lower income levels appear more resource-dependent. Our descriptive analysis confirmed that CRM dependence is indeed associated with low GDP per capita. In line with this reasoning, the CS-ARDL estimates, for which reverse causality is a smaller cause for concern, are less significant and negative.

To further explore endogeneity issues, we examined country-specific coefficients from column (8) of the CS-DL estimation (see Table E.2). These results reveal large variations in magnitude and, more importantly, unexpected patterns: the largest negative coefficients are found in high-income economies such as Austria, France, Germany, Italy, Japan, the United Kingdom, and the United States. In our view, this outcome is best explained by downward endogeneity bias. There is no convincing theoretical rationale for such results, which contradict both established NRC mechanisms and the broader literature. For example, it is implausible that a sector representing less than a fraction of a percent of GDP in countries like Japan or the UK could generate significant exchange rate pressures, foster rent-seeking institutions, or create growth-dampening volatility. The country-specific coefficients from column (4), using our CRM abundance measure, do not follow such a pattern.

To test this interpretation, we re-estimated the model excluding these seven high-income countries. As shown in Table 5, this adjustment sharply reduced the magnitude of the coefficients and rendered the long-run effect of CRM dependence statistically insignificant in two out of four specifications. This strongly suggests that the apparent negative long-run effect is an artifact of endogeneity rather than genuine evidence of a CRM dependence curse.

As another way to substantiate our claim regarding CRM dependence endogeneity affecting our results, we use the two-step system GMM framework from Arellano and Bond (1991). This

Table 5: CS-DL long-run effects dynamic panel estimation (AUT, DEU, FRA, GBR, ITA, JPN, USA removed)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| CRM dependence | -25.39 | -28.95* | -10.06 | -8.210** |
| | (-1.42) | (-1.86) | (-1.57) | (-2.09) |
| Trade | | 0.308*** | 0.144*** | 0.119*** |
| | | (7.96) | (3.74) | (4.00) |
| Investment | | | 0.221*** | 0.184*** |
| | | | (8.63) | (9.14) |
| Institutional qual. | | | | -0.242 |
| | | | | (-0.25) |
| Observations | 1665 | 1665 | 1665 | 1665 |

This table reports the CS-DL estimation results with outliers excluded.
t-statistics are in parentheses, $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

approach has been widely applied in empirical research upon economic growth to address endogeneity concerns (Beck and Levine, 2004; Chang et al., 2008; Compton et al., 2011; Darku and Yeboah, 2018; Hoeffler, 2002; Moshirian and Wu, 2012). As we are interested in long-term effects, we construct 10 periods by averaging our variables over 5 years. This also allows us to avoid the problem of instrument proliferation which threatens the validity of the GMM approach as the panel widens (Roodman, 2009). With both time and country fixed effects, we instrument the lagged value of GDP and our CRM variables using further lags as is standard in the literature. This choice is justified by the lack of a reliable external instrument for CRM dependence. Natural resources abundance variables have been used in the literature to instrument resources dependence (Brunnschweiler and Bulte, 2008; Van der Ploeg and Poelhekke, 2010), but the exclusion hypothesis is called into question. The results in Table 6 show the absence of significant results for CRM dependence. The null hypothesis of no correlation between the instruments and the residuals cannot be rejected, while the null of joint insignificance is rejected across all specifications. Overall, these results further support the endogeneity hypothesis.

Table 6: System GMM panel estimation

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| GDP (lagged) | 0.96*** | 0.64*** | 0.62*** | 0.64*** | 0.95*** | 0.63*** | 0.64*** | 0.65*** |
| | (0.03) | (0.11) | (0.05) | (0.05) | (0.03) | (0.08) | (0.05) | (0.05) |
| CRM abundance | 0.00 | 0.03 | 0.01 | 0.02* | | | | |
| | (0.01) | (0.02) | (0.01) | (0.01) | | | | |
| Trade | | 0.31*** | 0.15*** | 0.11** | | 0.33*** | 0.09 | 0.07 |
| | | (0.09) | (0.05) | (0.05) | | (0.07) | (0.06) | (0.07) |
| Investment | | | 0.20*** | 0.21*** | | | 0.25*** | 0.27*** |
| | | | (0.04) | (0.05) | | | (0.03) | (0.03) |
| Institutional qual. | | | | 0.06 | | | | −0.02 |
| | | | | (0.12) | | | | (0.13) |
| CRM dependence | | | | | −0.09 | −0.20 | −0.14 | 0.07 |
| | | | | | (0.35) | (0.73) | (0.51) | (0.64) |
| Observations | 440 | 440 | 440 | 440 | 440 | 440 | 440 | 440 |
| Sargan Test (p-value) | 0.12 | 0.69 | 1.00 | 1.00 | 0.16 | 0.93 | 0.98 | 1.00 |
| Wald Test coeff. (p-value) | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |

This table reports the system GMM estimation results

Standard errors are in parentheses, $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

# 6 Discussion

So far, our analysis has approached the possibility of a CRM curse from a purely empirical standpoint. While we do not find evidence of a robust empirical regularity across years and countries, these results do not allow us to reject the causal mechanisms highlighted in the literature. However, when addressing the NRC as a concept, it is important to distinguish between what constitutes a challenge and what may constitute a curse among the proposed pathways. Our findings do not suggest that high exposition to a volatile commodity or currency appreciation following a resource boom cannot be harmful, but rather that such effects are not systematic. They are not systematic because appropriate public policies and socio-economic environments can mitigate–if not fully offset–the harm associated with resource extraction. A compelling example is the countercyclical fiscal framework implemented in Chile to manage copper-related windfalls (Frankel, 2010; Braunstein and Chuchko, 2025). In that sense, copper, lithium or any other CRM cannot be considered a curse from a macroeconomic standpoint. Leveraging such resources for economic development is a challenge, but it does not entail inevitability. Historically, natural resources have featured in both successful development trajectories and missed opportunities or traps (Wright and Czelusta, 2004).

By contrast, the relationship between natural resources endowment and political stability or institutional quality offers a more compelling case for a curse. No appropriate public policy can be expected in a country rife with corruption, opaque resource management or even armed conflict around point resources. There is some evidence suggesting a downward pressure on institutional quality through rent seeking, corruption and instability in the presence of natural resources (Badeeb et al., 2017). As this mechanism primarily affects countries with weak institutional foundations, natural resources seem to contribute to a development trap while not being its root cause.

In that line of reasoning, Arezki and Van der Ploeg (2025) draw an interesting connection with the seminal paper by Acemoglu et al. (2001) characterizing the effect of *extractive institutions* on current economic development. Interestingly, their argument rests on the idea that variation in institutions implemented by colonial powers could be explained through other factors than natural resources endowment. It remains to be shown that the US, Australia and the Democratic Republic of Congo inherited different institutional frameworks because of differences in crop yields or subsoil wealth.

Turning back to critical raw materials, the ongoing race between major powers to secure their supply could exacerbate this institutional pressure in producing countries. Indeed, such countries can be ill-equipped to face pressures from China or the US, who have recently demonstrated again their willingness to prey on vulnerable countries as in the case of Ukraine. Another CRM specific issue is the uncertainty surrounding the future economic importance of those materials. Criticality is context-dependent and potentially transient. For instance, the value of lithium stems from its role in lithium-ion batteries, a relatively recent technology. If breakthroughs in magnesium or aluminum-based technologies provided more efficient or profitable alternatives (Olabi et al., 2023), the value of lithium could decline sharply. Likewise, battery technologies have become progressively less cobalt-intensive (Lebdioui and Riofrancos, 2025). Additionally, the immaturity of certain CRM markets increases opacity and price volatility, while limiting available risk-management instruments (Braunstein and Chuchko, 2025). For instance, lithium price variations killed a Chinese plant project in Chile this year (Reuters, 2025a). With these factors in mind, holistic, farsighted public policies may be even more challenging to implement for CRM.

All in all, leveraging CRM extraction for economic development is a complex issue requiring careful, holistic public policies. Perceiving a systematic "resource curse"–that neither the existing literature on mineral resources nor our results supports– makes resource nationalism appear to be a natural answer. However, its consequences remain unclear. While still unfolding, the Indonesian example shows quite clearly how the same export restrictions can prompt industrialization or backfire depending on the context (Lebdioui and Riofrancos, 2025). Without unique market power, CRM resources by themselves are not sufficient to offset the human capital endowment, infrastructures, political stability and regulatory framework requirements to capture activities generating more value-added along the global value chain. As of today, many African economies contend with this reality as shown by recent developments regarding lithium mining and processing (Boafo et al., 2024). For instance, Zimbabwe faces headwinds as a landlocked country in Sub-Saharan Africa–where land-transport-infrastructures issues have been shown to disadvantage export-oriented industries (Christ and Ferrantino, 2011)–among other aforementioned challenges. Without massive sway over the global lithium supply, Zimbabwe's export restrictions strategy may allow for lithium processing localization (Reuters, 2025b), but it is unlikely to yield such results for a battery manufacturing industry. The lithium triangle is another example showing that, while variation in state involvement in the extraction sector is possible, overly ambitious Bolivian resource nationalism has yet to achieve convincing results (Johnson et al., 2024).

Beyond macroeconomic considerations, we acknowledge the substantial socio-environmental challenges inherent in natural resource extraction (Boafo et al., 2024; Lebdioui and Riofrancos, 2025). Although policy can mitigate part of these impacts, mining remains socially and environmentally costly. Furthermore, the persistent transfer of natural resources from the *Global South* to the *Global North* must not be overlooked (Givens et al., 2019).

# 7 Conclusion

In this paper, we investigated the Natural Resource Curse (NRC) in the specific case of Critical Raw Materials (CRM). Using production data from 1970 to 2019, we constructed two distinct measures: CRM abundance, defined as real per capita production, and CRM dependence, defined as the share of CRM production in GDP. Our analysis shows that dependence measures are highly endogenous and capture more information about a country's income level than about its actual endowment.

Across a range of empirical strategies, including cross-section and panel estimators accounting for slope heterogeneity and cross-sectional dependence, we find no robust evidence of a CRM curse. Cross-sectional regressions suggest that CRM production has no systematic effect on growth, while panel results provide mixed evidence. Some specifications point to a negative long-run effect of CRM dependence, but this result is largely driven by high-income, low-dependence countries and most likely reflects endogeneity bias rather than a genuine causal mechanism.

Overall, our findings confirm that the NRC cannot be considered a general empirical regularity in the case of CRM, and relying on dependence measures without properly addressing endogeneity risks producing misleading results.

Looking forward, future research should focus on improving data quality—particularly with respect to production and reserves—and on developing robust identification strategies to tackle endogeneity. Approaches based on geospatial data, country-level mining information, or credible instruments for dependence could provide promising avenues. More generally, while CRM extraction may create local challenges, our results do not support the notion of a universal CRM curse at the macroeconomic level. While appropriate policies should address potential negative externalities and encourage diversification as well as the capture of greater value added along the CRM supply chain, our findings suggest that, to ensure long-term growth, traditional factors–such as human capital accumulation, infrastructure, and institutional quality and stability–should remain central.

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# Appendix A. Literature summary

Table A.1: Summary of the Natural Resources Curse literature (Mineral an/or panel data)

| Study | Dataset / Countries | Period | Resource Measure | Method | Findings |
|---|---|---|---|---|---|
| Stijns (2005) | ∼70–90 countries | 1970–1989 | 57 mineral reserves (USGS, 1999), aggregated via PCA into two indexes | Cross-country growth regressions | No significant effect of non-fuel mineral reserves on growth. |
| Boschini et al. (2007) | 80 countries | 1975–1998 | Share of ore & metal exports in GDP; mineral production in GNP; gold, silver, diamond production in GDP | Growth regressions with interaction terms | Negative direct effect of mineral dependence on growth; positive marginal effect when interacted with institutional quality |
| Norman (2009) | ∼70 countries | 1970–1989 | Reconstructed reserves of 35 minerals as share of GDP | 2SLS (rule of law instrumented) | Negative direct effect of mineral stock on growth; indirect effect via institutions. |
| Van der Ploeg and Poelhekke (2010) | 60–82 countries | 1970–1989 | Norman (2009) mineral reserves, normalized per capita | 3SLS | No significant direct effect; positive direct effect offset by negative indirect effect through volatility. |
| Bildirici and Gokmenoglu (2020) | 7 largest gold, silver, copper producers in 2016 | 1960–2020 | Gold, silver, copper production (USGS Minerals Yearbooks), no aggregation | Markov switching VECM | Mixed results across metals. |
| Ozcan et al. (2023) | 19 top exporters of precious metals in 2021 | 2001–2021 | Precious metals & concentrates share of GDP | Panel mean group estimator | Negative correlation with growth; effect insignificant when interacted with institutions or human capital. |
| Sharma and Pal (2021) | 111 countries | 1996–2015 | Natural resource rents (% GDP); ores & metal exports (% of merchandise exports) | CS-ARDL; CS-DL | No significant long-run effect; authors argue NRC supported, but evidence suggests otherwise. |
| Shahbaz et al. (2019) | 35 countries | 1980–2015 | Rents per capita vs. rents as % GDP | MG and CCEMG estimators | Rents per capita positively correlated with growth; rents as % GDP negatively correlated. |
| Ben-Salha et al. (2021) | 8 most resource-rich countries | 1970–2013 | Oil, gas, coal, mineral, and forest rents (% GDP) | Panel mean group estimator | Significant positive long-run effect of resource rents on GDP per capita growth. |

# Appendix B. Critical raw materials production data collection

The USGS provides two main sources of annual data on global CRM production and reserves. Mineral commodity summaries (MCS) are concise reports that include production and reserves data (and occasionally approximate resource figures) for the most significant producing countries. MCS are available from 1996 onward, released each January or February, and cover data from 1994 onward. An MCS published in year n reports production for n–1 (preliminary, subject to revision) and n–2. By contrast, Minerals Yearbooks (MYB) are more comprehensive and typically avoid grouping smaller producers under "other countries." An MYB released in year n covers production from n–4 to n, implying that for a given year the MYB generally provides more accurate data than the MCS if discrepancies arise. MYBs are available from 1931 to around 2022, making them the only source for the 1970–1993 period. However, they rarely contain reserves data. Whenever both sources overlap, MYB data should therefore be preferred.

In some cases, discrepancies between MYB and MCS go beyond revisions or country coverage. Lithium production is counted in gross weight in MYB and in elemental content in MCS. Because ore content varies widely, cross-country comparisons based on gross weight would be misleading, we applied the following conversion for data between 1970 and 1993: Chilean ore is considered to contain 20% of lithium (because of lithium brine extraction), while other countries' ore is considered to contain 2% of lithium. For Russia, the ratio of MCS to MYB data is implausible; we therefore applied a 40% content ratio for 1993 and the standard 2% for 1992, which yields more reasonable values.

To ensure consistency during aggregation, all production data were converted to tons. For helium, we used the ratio of 0.1785 kilograms per cubic meter. Additional adjustments were necessary: ferrosilicon and silicon metal are often not distinguished and were therefore combined; platinum-group metals (platinum, palladium, iridium, rhodium, ruthenium) were aggregated due to limited disaggregated data; beryl was converted to beryllium content using the USGS factor of 4%; and arsenic trioxide to arsenic content using the factor of 75.71%. German data prior to reunification were aggregated across East and West. Finally, when USGS reported production as "less than half a unit," we standardized this to a quarter of a unit.

Because bauxite and alumina are reported together, but only the extraction stage is considered critical, we only retain bauxite. For the same reason, we only retain manganese ore production and not ferromanganese and silicomanganese production. Similarly, as only titanium metal is considered critical and not titanium as a whole, we retained only titanium sponge production. Also, no detailed data is provided for rare earths, which are considered one material. Table B.1 lists all materials included in our measure of CRM production.

A further challenge arises from missing values. Since all production data are collected from published reports rather than a centralized database, the absence of reported production does not automatically imply zero output for all non-producing countries in a given year. This is particularly relevant in the case of MYB data, where no "other countries" residual is reported. It is reasonable to assume that most countries not listed produce little or nothing, but this cannot be verified with certainty. Consequently, assigning a value of zero to all unreported cases would be misleading, yet treating them as missing at random would also be inappropriate. A country is far more likely to be missing because it produces no significant amount of a given material. This limitation, common in resource data, is especially pronounced here. As a result, estimates based on these data may carry some degree of bias, since missing values are excluded from aggregation.

Finally, U.S. CRM production figures are downward biased by confidentiality restrictions. The USGS does not publish U.S. production when disclosure would reveal proprietary information. This does not apply, however, to copper and phosphate rock.

Table B.1: Critical Raw Materials Coverage

| | | | |
|---|---|---|---|
| Antimony | Arsenic | Barite | Bauxite |
| Beryllium | Bismuth | Boron | Cobalt |
| **Coking coal** | Copper | Feldspar | Fluorspar |
| Gallium | Germanium | **Hafnium** | Helium |
| Lithium | Rare earths | Magnesium | Manganese |
| Natural graphite | Nickel | Niobium | Phosphate rock |
| **Phosphorus** | Platinum-group metals | **Scandium** | Silicon |
| Strontium | Tantalum | Titanium sponge | Tungsten |
| Vanadium | | | |

Note: This table provides the list of CRM according to the European Commission (2023).

Materials not included in our analysis are in red and bold.

# Appendix C. Critical raw materials production data aggregation

When aggregating production data (measured by weight) for 30 heterogeneous materials, two main options are available. The first is to apply a non-monetary aggregation method such as that proposed by Stijns (2005). However, using a principal component analysis to build an index makes economic interpretation less straightforward. A second possibility would be to weight production using the Economic Importance (EI) index developed by the European Commission. Yet this approach suffers from two major limitations. First, the EI index is only available from 2011 onwards, and at three-year intervals. Extending it backward to cover earlier decades would clearly misrepresent historical patterns. Second, the EI index does not correct for the substantial differences in material weights. As a result, any aggregate measure based on EI would be disproportionately driven by heavy, low-value materials such as phosphate rock or bauxite. Without additional data, this limitation cannot be resolved, making the method unsuitable for our purposes.

Another method is to convert production data into monetary values, which can easily be added together. This solution is nevertheless constrained by limited price data. Public exchange prices—such as those from the London Metal Exchange or the Shanghai Metals Market—cover only a subset of materials. Through Datastream, we retrieved data only for copper, aluminum, lead, zinc, tin, nickel, silver, gold, palladium, platinum, lithium, and cobalt. These series refer to industrial-grade metals, implicitly assuming that refining costs and quality differences are negligible across materials. Moreover, temporal coverage is highly uneven: cobalt prices are available only from 2010 onward, lithium from 2012, and nickel from 1993...

Another price source is the Primary Commodity Price System from the International Monetary Fund.[12] It includes prices for only 16 materials, six of which fall outside our CRM definition. Furthermore, time coverage remains very limited: for instance, prices for lithium, manganese, rare earths, silicon, and vanadium are available only from 2013 onward.

A third option would have been to use trade data reported in both volume and value to derive an implicit price. We tested this approach with the BACI database (Gaulier and Zignago, 2010) version HS92. However, data availability proved problematic both in terms of time coverage and material coverage. In addition, only limited information is available on whether countries are actually exporting ores, alloys, or metals, and on their respective grades or contents. As a result, this method would have required highly speculative assumptions with little empirical foundation

After reviewing all potential data sources, we concluded that the unit value per ton estimated by the USGS offers the best combination of coverage, efficiency, and transparency. The USGS provides unit value data for all 30 materials included in our CRM production measure from 1900

[12] IMF PCPS Data explorer

to 2019 (and beyond for some materials). The only exceptions are niobium and fluorspar, for which unit value data are missing after 2007. The unit value per ton is defined as the value of the apparent consumption of one ton of a given material. Apparent consumption, in turn, is defined as:

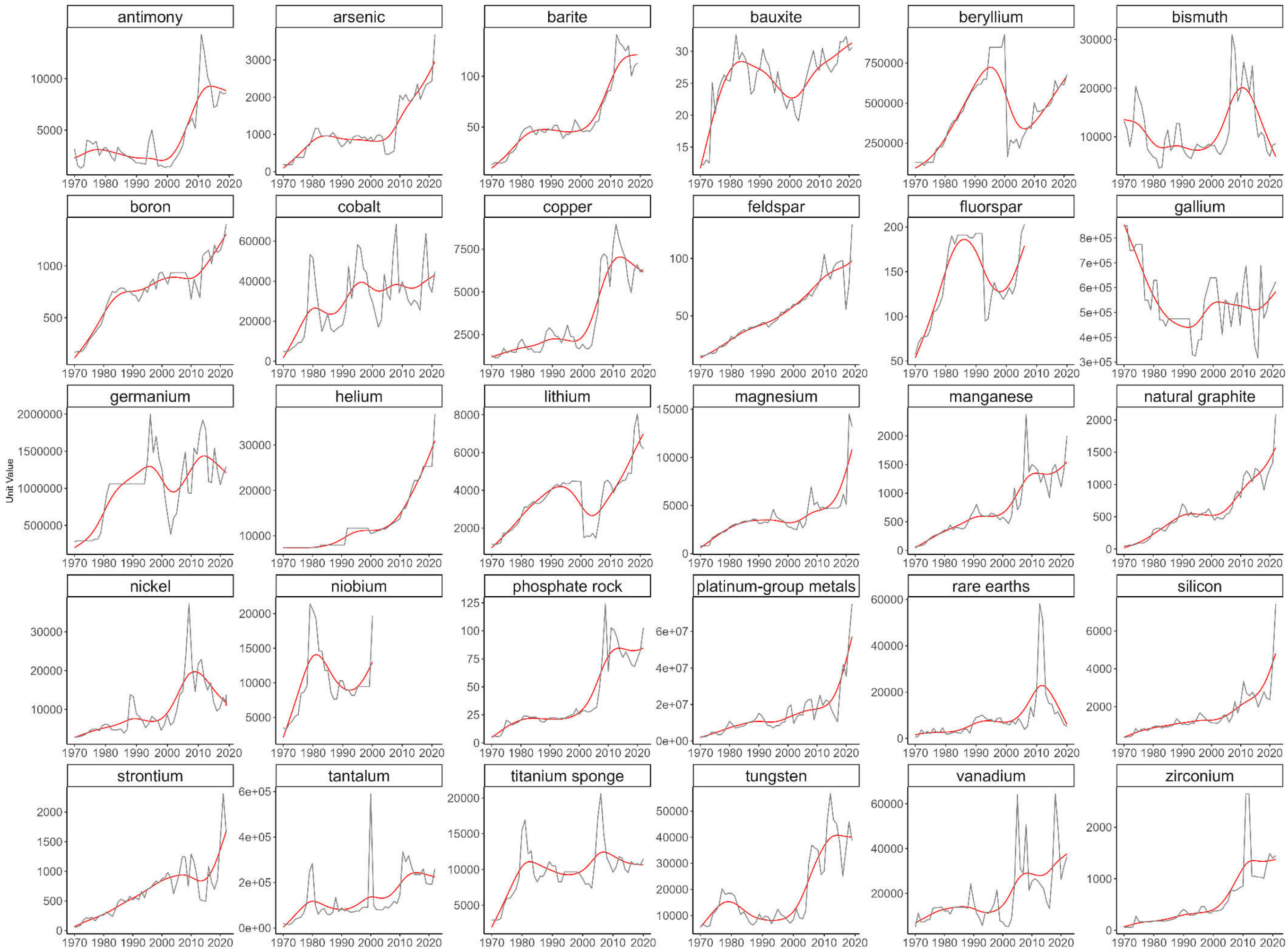
$$Apparent\ Consumption = Primary\ Production + Secondary\ Production + Imports - Exports$$

In practice, this value is often based on import and export prices, as well as market exchange prices for materials with larger trade volumes. The USGS unit value, however, has one important limitation: it is derived from U.S. data and therefore does not perfectly reflect global prices. For materials with significant variation in product quality or grade (notably manganese and silicon), the measure largely depends on the composition of U.S. imports and exports.

It is important to emphasize that our objective is not to measure rents or the actual gains a country derives from CRM production. Using USGS data for such a purpose would be ill-advised, as these figures are best regarded as order-of-magnitude estimates rather than precise values. Indeed, USGS data range from directly reported figures to approximations, with the aim of providing broad benchmarks rather than exact production values.

As highlighted in the literature review, much of the NRC effect relates to long-term structural dynamics. As we are not trying to take advantage of some documented natural resources endowment shocks, it appears justified to construct a prudent, smoothed natural resources variable. For example, it would be implausible to claim that tantalum producers saw their resource base increase sixfold in 1999, then fall back to one-sixth in 2001. In reality, this reflected a demand shock to which supply could not adjust in the short term (Simandl, 2002). For this reason, we extracted the trend component of the unit value series using the Hodrick–Prescott filter (Hodrick and Prescott, 1981) with $\lambda = 100$ as is usually advised for annual data. Figure C.1 reports the resulting smoothed trend together with the original yearly unit values for all included materials

Figure C.1: Unit value per ton: Actual and Trend




Note: This figure displays the actual value (in grey) and the trend (in red) of CRM unit values.

Source: Author's calculations based on data extracted from USGS.

# Appendix D. Supplementary data details

Table D.1: Variable inclusion summary

| Variable | Cross-section OLS | Cross-section WALS | Panel regressions |
|---|---|---|---|
| CRM abundance | ✓ | ✓ | ✓ |
| CRM dependence | ✓ | ✓ | ✓ |
| Primary exp. abundance | ✓ | ✓ | |
| Primary exp. dependence | ✓ | ✓ | |
| Real per capita GDP | ✓ | ✓ | ✓ |
| Rule of Law | ✓ | ✓ | ✓ |
| GFCF (share of GDP) | ✓ | ✓ | |
| Real GFCF per capita | | | ✓ |
| Globalization index | ✓ | ✓ | |
| Real trade per capita | | | ✓ |
| Initial real per capita GDP | ✓ | ✓ | |
| Life expectancy | | ✓ | |
| Confucians (share of Pop.) | | ✓ | |
| Muslims (share of Pop.) | | ✓ | |
| Protestants (share of Pop.) | | ✓ | |
| Latin America | | ✓ | |
| Sub-Saharan Africa | | ✓ | |

Table D.2: List of countries

| | | | | |
|---|---|---|---|---|
| Argentina (ARG) | Australia (AUS) | Austria (AUT) | Bolivia (BOL) | Brazil (BRA) |
| Canada (CAN) | Chile (CHL) | China (CHN) | Colombia (COL) | Cuba (CUB) |
| Germany (DEU) | Dominican Republic (DOM) | Algeria (DZA) | Ecuador (ECU) | Egypt (EGY) |
| Spain (ESP) | Finland (FIN) | France (FRA) | United Kingdom (GBR) | Greece (GRC) |
| Guatemala (GTM) | Indonesia (IDN) | India (IND) | Iran (IRN) | Israel (ISR) |
| Italy (ITA) | Jamaica (JAM) | Japan (JPN) | South Korea (KOR) | Morocco (MAR) |
| Madagascar (MDG) | Mexico (MEX) | Malaysia (MYS) | Norway (NOR) | Pakistan (PAK) |
| Peru (PER) | Portugal (PRT) | Rwanda (RWA) | Sweden (SWE) | Thailand (THA) |
| Tunisia (TUN) | Turkey (TUR) | United States (USA) | South Africa (ZAF) | |

Table D.3: Balanced panel descriptive statistics

| Variable | N | Mean | SD | Min | Max |
|---|---|---|---|---|---|
| GDP | 2200 | 8.841 | 1.336 | 5.245 | 11.248 |
| CRM abundance | 2200 | 2.786 | 1.758 | 0 | 7.74 |
| CRM dependence | 2200 | 0.01 | 0.024 | 0 | 0.225 |
| Trade | 2200 | 8.07 | 1.524 | 2.688 | 10.942 |
| Investment | 2200 | 7.379 | 1.412 | 2.987 | 10.031 |
| Institutional quality | 2200 | 0.628 | 0.306 | 0.041 | 0.995 |

Note : *GDP* is the log of real GDP per capita in 2015 USD, *CRM abundance* the log of real CRM production per capita in 2015 USD, *CRM dependence* the share of CRM production in GDP, *Trade* the log of per capita real trade in 2015 USD, *Investment* the log of per capita gross capital formation in 2015 USD and *Institutional quality* the V-DEM institute rule of law indicator.

Table D.4: Cross-sectional dependence test

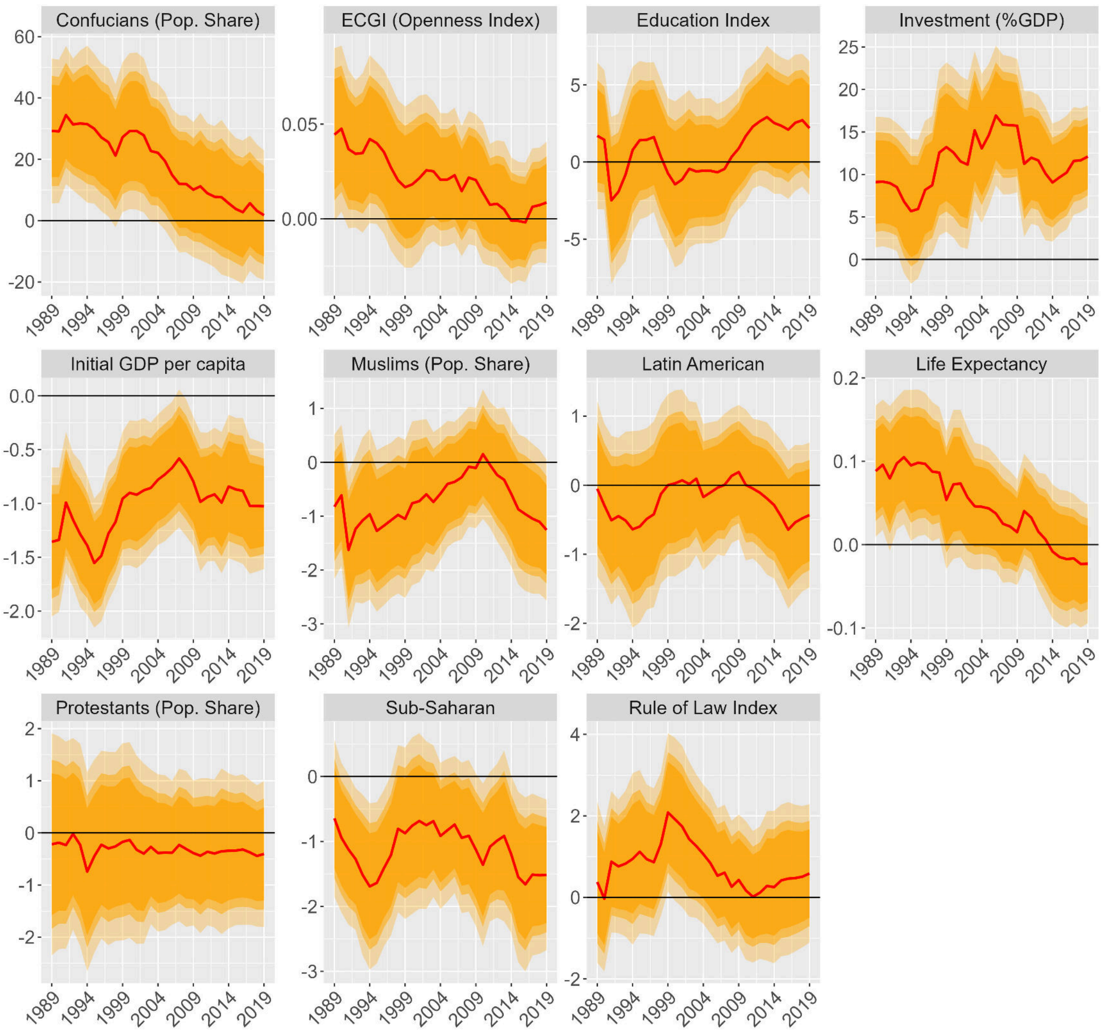
H0: weak cross-sectional dependence

| | GDP | CRM abundance | CRM dependence | Trade | Investment | Institutional quality |
|---|---|---|---|---|---|---|
| CD | 217.277 | 190.557 | 148.898 | 216.773 | 216.821 | 204.621 |
| P-Value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

Note : This table reports the results of the CD test (Pesaran, 2015).

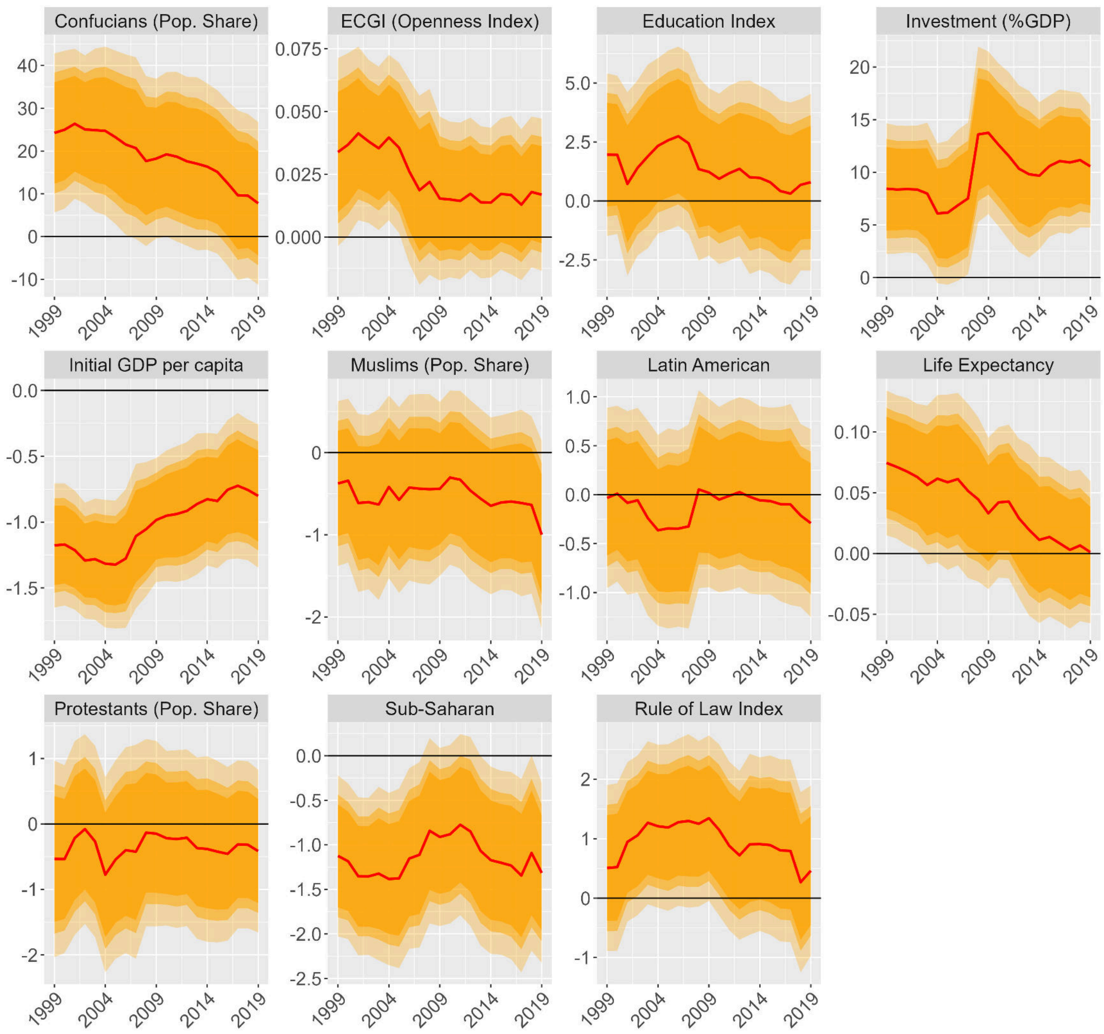
# Appendix E. Complementary results

Figure E.1: WALS controls coefficients (including CRM dependence): 20-year rolling window



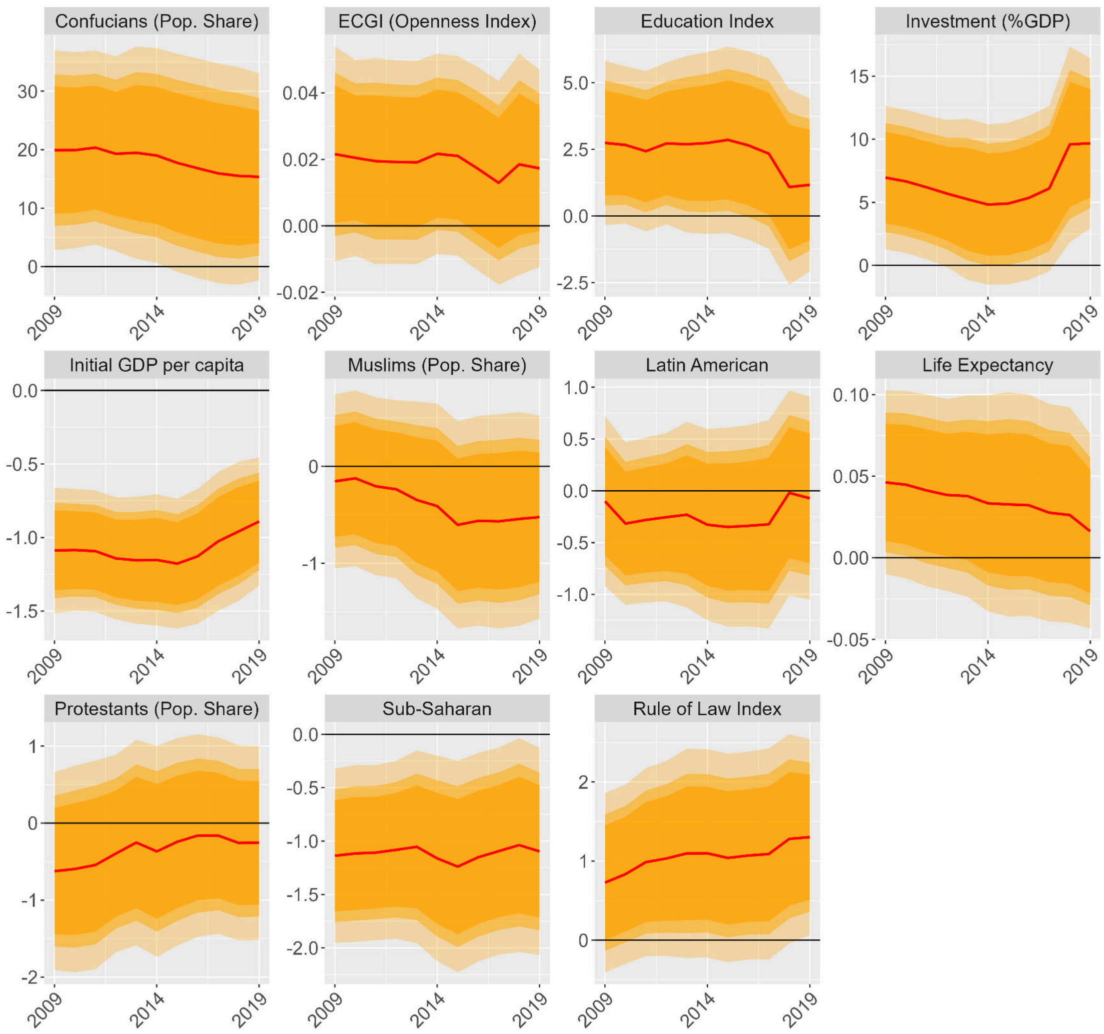

Note: This figure displays the WALS rolling-window estimation results based on a 20-year rolling window. The coefficients are traced in red. The 90%, 95% and 99% confidence intervals are shown in orange. Years on the x-axis indicate the last year included in the estimation window.

Figure E.2: WALS controls coefficients (including CRM dependence): 30-year rolling window



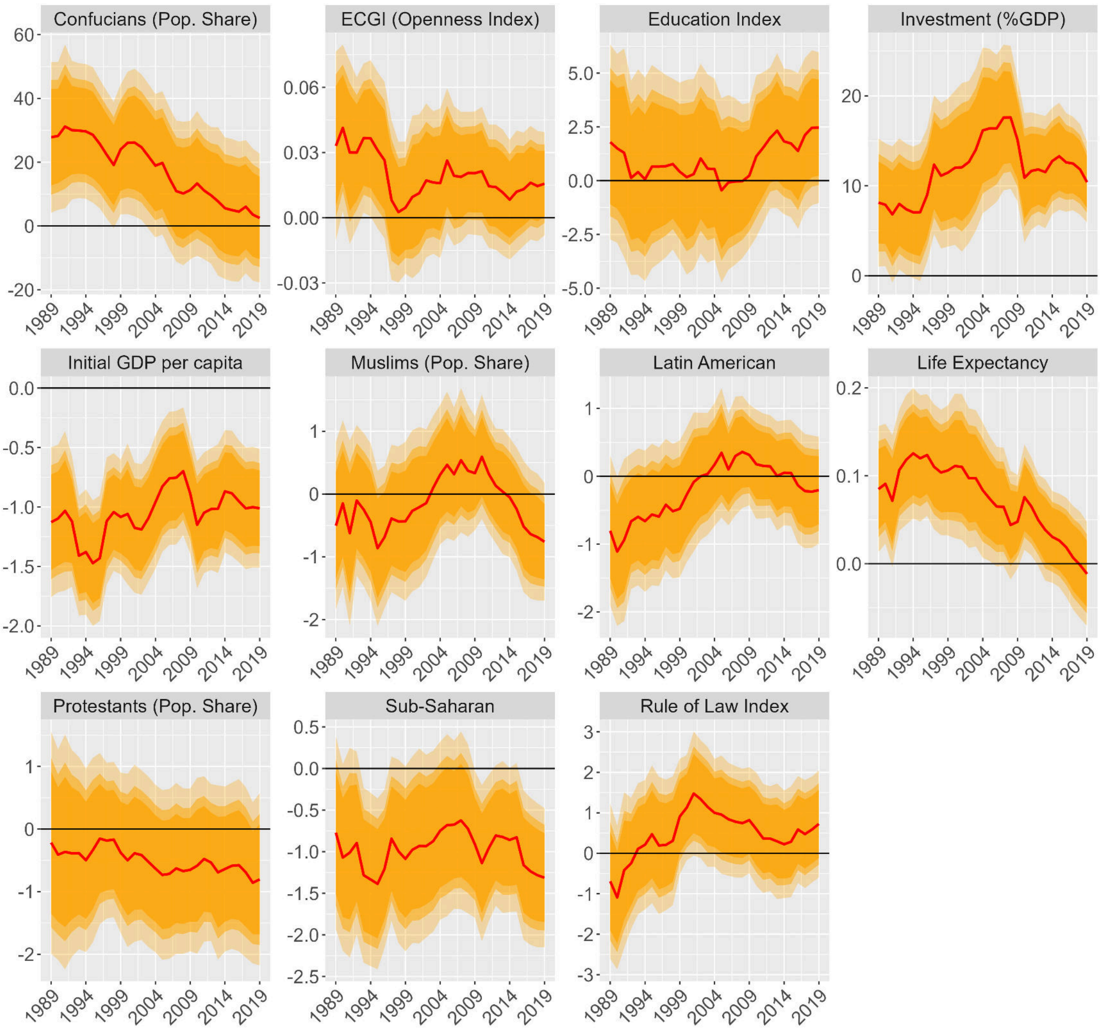

Note: This figure displays the WALS rolling-window estimation results based on a 30-year rolling window. The coefficients are traced in red. The 90%, 95% and 99% confidence intervals are shown in orange. Years on the x-axis indicate the last year included in the estimation window.

Figure E.3: WALS controls coefficients (including CRM dependence): 40-year rolling window



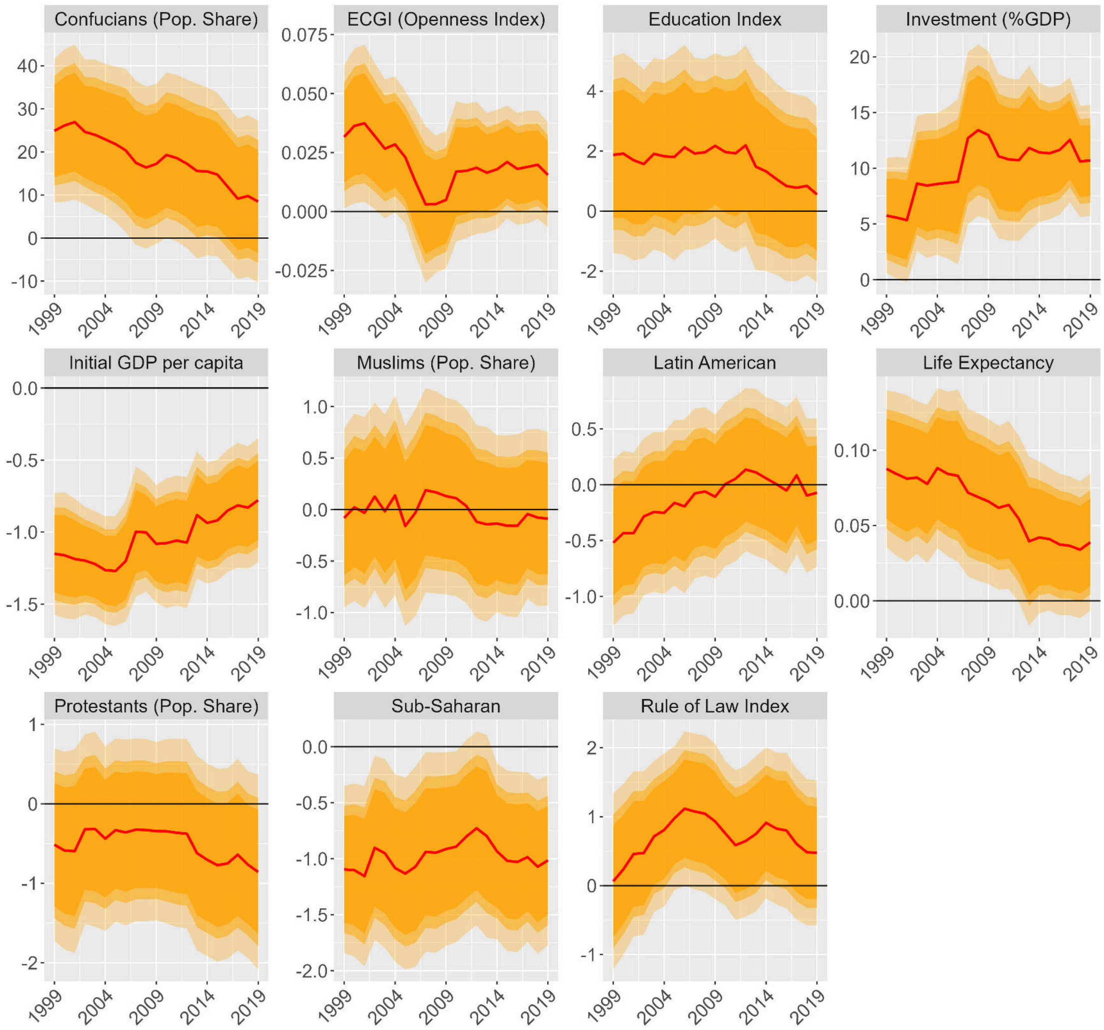

Note: This figure displays the WALS rolling-window estimation results based on a 40-year rolling window. The coefficients are traced in red. The 90%, 95% and 99% confidence intervals are shown in orange. Years on the x-axis indicate the last year included in the estimation window.

Figure E.4: WALS controls coefficients (including primary exports dependence): 20-year rolling window



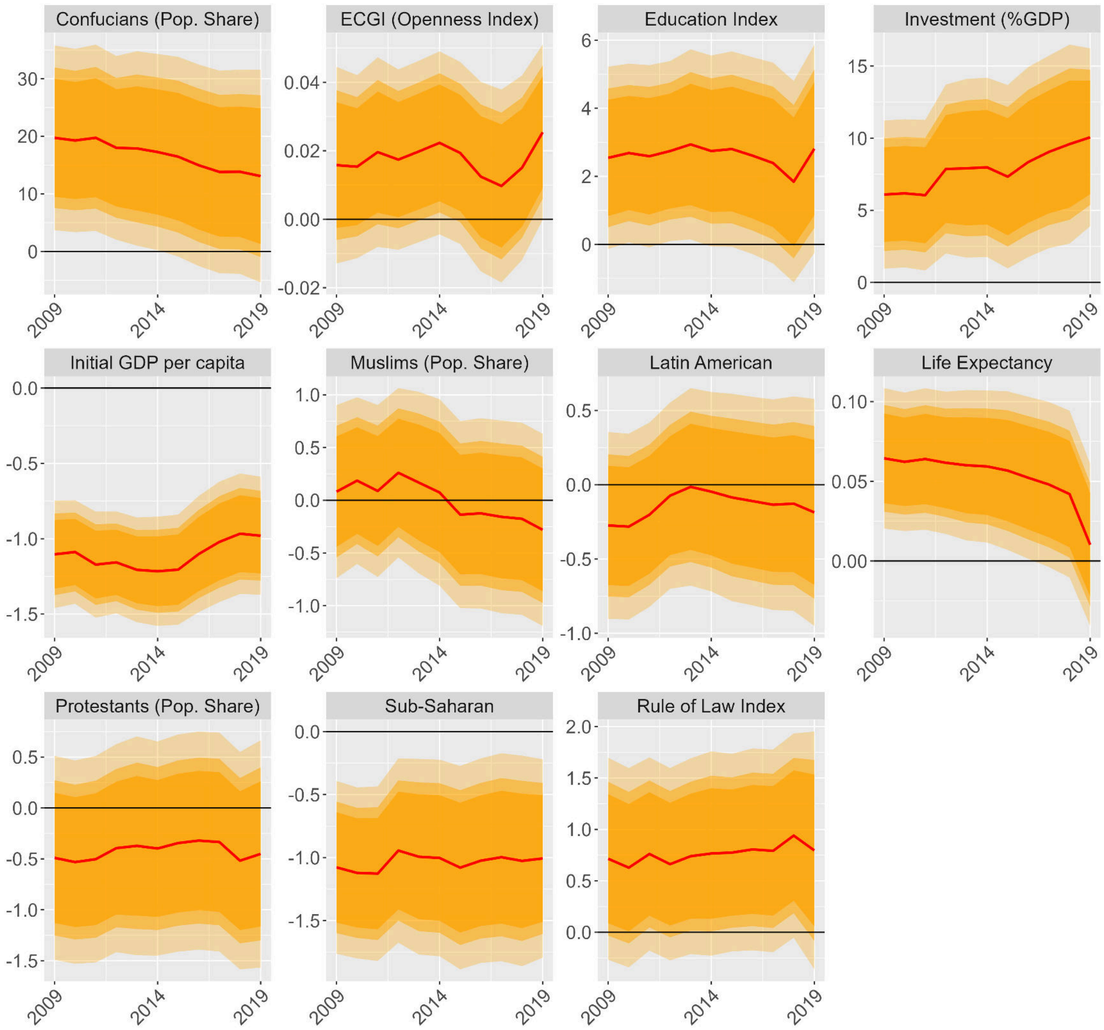

Note: This figure displays the WALS rolling-window estimation results based on a 20-year rolling window. The coefficients are traced in red. The 90%, 95% and 99% confidence intervals are shown in orange. Years on the x-axis indicate the last year included in the estimation window.

Figure E.5: WALS controls coefficients (including primary exports dependence): 30-year rolling window




Note: This figure displays the WALS rolling-window estimation results based on a 30-year rolling window. The coefficients are traced in red. The 90%, 95% and 99% confidence intervals are shown in orange. Years on the x-axis indicate the last year included in the estimation window.

Figure E.6: WALS controls coefficients (including primary exports dependence): 40-year rolling window




Note: This figure displays the WALS rolling-window estimation results based on a 40-year rolling window. The coefficients are traced in red. The 90%, 95% and 99% confidence intervals are shown in orange. Years on the x-axis indicate the last year included in the estimation window.

Table E.1: PMG dynamic long-run effect panel estimation

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Error correction | -0.0119 | -0.0335 | -0.0885 | -0.0464 | -0.00783 | -0.0324 | -0.0830 | -0.0420 |
| | (-0.05) | (-0.05) | (-0.18) | (-0.05) | (-0.01) | (-0.04) | (-0.04) | (-0.04) |
| CRM abundance | 0.554** | 0.112 | 0.0303 | 0.0902 | | | | |
| | (2.07) | (0.89) | (0.19) | (0.44) | | | | |
| Trade | | 0.547* | 0.209 | 0.327 | | 0.606 | 0.215 | 0.353 |
| | | (1.68) | (0.46) | (0.43) | | (0.60) | (0.14) | (0.52) |
| Investment | | | 0.257 | 0.301 | | | 0.262 | 0.296 |
| | | | (0.51) | (0.51) | | | (0.21) | (0.38) |
| Institutional qual. | | | | -0.0323 | | | | 0.0876 |
| | | | | (-0.01) | | | | (0.02) |
| CRM dependence | | | | | 16.89 | 1.404 | 0.621 | 4.087 |
| | | | | | (0.08) | (0.03) | (0.00) | (0.10) |
| Observations | 1980 | 1980 | 1980 | 1980 | 1980 | 1980 | 1980 | 1980 |

This table reports the pooled mean group (PMG) estimation results

t-statistics are in parentheses, $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table E.2: CS-DL country specific coefficients

| | Model CRM abundance | | | | Model CRM dependence | | | |
|---|---|---|---|---|---|---|---|---|
| | CRM abundance | Trade | Investment | Institutional qual. | CRM dependence | Trade | Investment | Institutional qual. |
| ARG | 0.002 (0.06) | 0.011 (0.15) | **0.433** (8.74) | 0.093 (1.38) | -20.698 (-1.42) | 0.115 (1.28) | **0.285** (2.85) | 0.039 (0.51) |
| AUS | 0.045 (1.12) | 0.130 (0.99) | **0.210** (2.01) | -0.734 (-0.09) | 0.402 (0.03) | 0.164 (0.64) | 0.149 (1.35) | 5.603 (0.68) |
| AUT | 0.021 (0.48) | 0.031 (0.09) | 0.277 (1.23) | -1.015 (-0.59) | -124.215 (-0.77) | -0.043 (-0.15) | **0.298** (1.87) | -2.282 (-1.15) |
| BOL | **0.093** (2.87) | **-0.109** (-1.72) | **0.150** (3.18) | **-0.351** (-2.09) | 1.704 (0.71) | 0.012 (0.19) | 0.072 (1.14) | **-0.686** (-5.49) |
| BRA | 0.048 (1.09) | -0.072 (-0.67) | **0.292** (4.51) | -0.227 (-0.93) | 7.404 (0.42) | **-0.194** (-1.68) | **0.282** (3.88) | **-0.292** (-1.75) |
| CAN | 0.021 (0.39) | **0.113** (1.83) | **0.197** (1.91) | 1.590 (0.28) | -2.074 (-0.38) | **0.163** (2.64) | **0.213** (2.69) | 4.196 (0.71) |
| CHL | 0.088 (1.19) | -0.087 (-0.40) | **0.399** (4.70) | **0.205** (3.22) | 0.202 (0.35) | -0.033 (-0.19) | **0.402** (5.11) | **0.124** (2.34) |
| CHN | **-0.268** (-3.50) | **0.187** (2.31) | **0.555** (6.34) | 0.206 (0.41) | **-32.395** (-4.35) | **0.243** (2.40) | **0.380** (4.86) | -0.137 (-0.36) |
| COL | -0.016 (-0.88) | **0.212** (2.03) | **0.191** (4.94) | -0.219 (-0.71) | -11.714 (-1.65) | **0.186** (2.12) | **0.202** (5.20) | -0.353 (-1.06) |
| CUB | 0.010 (0.24) | **0.277** (7.47) | 0.026 (0.68) | -2.086 (-1.03) | **-9.637** (-5.79) | **0.223** (5.82) | **0.122** (4.05) | -1.935 (-1.26) |
| DEU | -0.006 (-0.15) | 0.125 (0.64) | 0.215 (1.50) | 15.103 (0.72) | -210.510 (-0.69) | 0.025 (0.12) | **0.282** (2.03) | 9.674 (0.60) |
| DOM | -0.004 (-0.22) | 0.036 (0.47) | **0.131** (2.46) | **0.337** (2.65) | **-3.642** (-2.42) | 0.077 (0.97) | **0.140** (3.11) | 0.210 (1.58) |
| DZA | 0.008 (0.12) | 0.051 (0.55) | 0.057 (0.69) | 0.062 (0.12) | -16.052 (-0.21) | -0.083 (-1.02) | **0.247** (3.75) | -0.297 (-0.65) |

Note : This table reports the CS-DL estimation results for each country.

t-statistics in parentheses, coefficients are in bold if the associated t-statistics are above 1.65 in absolute value

| | Model CRM abundance | | | | Model CRM dependence | | | |
|---|---|---|---|---|---|---|---|---|
| | CRM abundance | Trade | Investment | Institutional qual. | CRM dependence | Trade | Investment | Institutional qual. |
| ECU | -0.006 (-0.54) | 0.090 (0.62) | **0.174** (2.18) | 0.330 (1.63) | **11.535** (2.47) | -0.072 (-0.52) | **0.178** (3.10) | 0.187 (0.76) |
| EGY | -0.015 (-0.24) | -0.007 (-0.08) | **0.211** (4.39) | -0.196 (-0.17) | -33.732 (-1.13) | 0.039 (0.65) | **0.219** (5.46) | 0.375 (0.62) |
| ESP | -0.057 (-0.94) | 0.244 (1.07) | **0.222** (1.68) | 0.022 (0.20) | -28.501 (-0.45) | 0.100 (0.50) | **0.304** (2.98) | 0.035 (0.32) |
| FIN | 0.002 (0.06) | **0.250** (2.09) | **0.238** (3.24) | -1.268 (-0.08) | **-27.902** (-1.71) | **0.306** (2.76) | **0.127** (1.79) | -18.432 (-1.18) |
| FRA | -0.038 (-0.67) | 0.327 (1.46) | 0.028 (0.20) | -4.589 (-1.17) | -256.415 (-0.86) | 0.166 (0.75) | 0.126 (0.71) | -4.755 (-1.23) |
| GBR | -0.054 (-0.64) | -0.036 (-0.20) | **0.228** (2.46) | 3.960 (0.66) | -644.943 (-0.66) | -0.019 (-0.09) | **0.275** (1.88) | 0.567 (0.09) |
| GRC | -0.065 (-1.23) | 0.038 (0.42) | **0.350** (10.02) | 0.194 (0.30) | **-42.123** (-2.46) | -0.016 (-0.21) | **0.305** (9.32) | -0.040 (-0.06) |
| GTM | **0.033** (3.07) | 0.010 (0.21) | 0.052 (0.93) | -0.470 (-1.53) | **6.137** (1.80) | 0.051 (1.06) | 0.083 (1.57) | **-0.677** (-1.97) |
| IDN | **-0.071** (-2.21) | **0.196** (2.50) | 0.113 (1.24) | -0.169 (-1.04) | -3.126 (-1.48) | **0.159** (1.89) | 0.044 (0.52) | **-0.301** (-2.22) |
| IND | -0.007 (-0.07) | -0.024 (-0.20) | 0.124 (1.14) | **-0.688** (-3.13) | 10.636 (0.27) | 0.013 (0.12) | 0.155 (1.08) | 0.095 (0.47) |
| IRN | **0.098** (2.76) | **0.101** (1.92) | **0.395** (6.99) | **-0.672** (-4.59) | **21.248** (2.00) | **0.126** (2.95) | **0.223** (3.90) | -0.121 (-0.87) |
| ISR | **0.082** (2.49) | **0.288** (1.93) | **0.136** (1.82) | **-5.811** (-1.98) | 5.800 (0.29) | 0.252 (1.42) | **0.136** (1.87) | -1.314 (-0.49) |
| ITA | -0.025 (-0.61) | 0.176 (1.64) | **0.209** (2.10) | 0.185 (0.25) | **-426.456** (-2.00) | 0.003 (0.03) | **0.326** (3.50) | 0.192 (0.26) |
| JAM | **0.128** (2.14) | **-0.370** (-3.69) | **0.186** (3.67) | 1.903 (1.65) | **-2.543** (-2.88) | -0.108 (-1.51) | **0.095** (1.90) | **5.936** (8.43) |
| JPN | -0.000 (-0.01) | 0.108 (1.19) | **0.255** (1.67) | -3.639 (-0.82) | -289.445 (-0.79) | 0.026 (0.42) | **0.416** (4.57) | 3.666 (0.96) |
| KOR | 0.020 (0.46) | 0.001 (0.01) | **0.194** (2.32) | 0.085 (0.59) | 69.856 (1.55) | 0.020 (0.31) | **0.338** (3.88) | **0.305** (2.71) |
| MAR | -0.003 (-0.11) | **0.408** (3.79) | 0.012 (0.11) | **-1.828** (-1.99) | -0.847 (-1.03) | **0.344** (3.79) | 0.067 (0.73) | **-1.944** (-2.25) |
| MDG | -0.015 (-0.94) | **0.073** (1.86) | -0.015 (-0.52) | -0.071 (-0.64) | 0.481 (0.55) | 0.016 (0.42) | -0.028 (-0.71) | -0.116 (-1.28) |
| MEX | -0.095 (-1.15) | **0.394** (3.63) | **0.220** (1.84) | **-0.809** (-2.54) | **-34.917** (-2.37) | **0.451** (5.07) | **0.191** (1.81) | **-0.693** (-3.76) |
| MYS | 0.010 (0.36) | **0.222** (2.10) | **0.169** (4.45) | -0.042 (-0.17) | 1.129 (0.04) | **0.175** (2.56) | **0.182** (6.90) | 0.077 (0.36) |
| NOR | -0.005 (-0.26) | **0.669** (3.76) | -0.017 (-0.28) | -4.031 (-0.20) | -11.069 (-1.14) | **0.596** (4.07) | -0.006 (-0.11) | -25.588 (-1.33) |
| PAK | -0.038 (-0.59) | 0.081 (1.33) | **0.223** (3.02) | 0.129 (0.23) | -56.587 (-0.67) | 0.104 (1.12) | **0.195** (2.58) | 0.432 (1.58) |
| PER | 0.060 (1.23) | 0.060 (0.60) | **0.197** (3.09) | 0.076 (0.90) | **-3.663** (-2.92) | **0.191** (2.02) | **0.399** (4.77) | **-0.210** (-2.28) |
| PRT | **0.033** (2.55) | 0.016 (0.17) | **0.282** (4.69) | -0.696 (-0.58) | **20.169** (2.88) | **-0.173** (-1.68) | **0.346** (8.34) | -0.445 (-0.40) |
| RWA | **-0.132** (-3.88) | **0.128** (1.94) | **0.118** (1.85) | **1.016** (3.94) | **12.063** (3.83) | **0.385** (8.02) | 0.055 (0.88) | 0.307 (0.97) |
| SWE | -0.002 (-0.07) | 0.136 (1.00) | 0.119 (1.28) | 23.685 (0.87) | -5.355 (-0.21) | 0.147 (1.23) | 0.124 (1.34) | 17.943 (0.68) |
| THA | -0.025 (-0.63) | 0.063 (0.80) | **0.178** (4.77) | **-0.483** (-2.14) | -11.079 (-0.37) | **0.216** (4.57) | **0.190** (5.13) | -0.229 (-1.12) |
| TUN | **0.038** (2.17) | **-0.315** (-2.34) | 0.034 (0.33) | -0.053 (-0.77) | **5.961** (2.75) | **-0.292** (-2.45) | 0.068 (0.67) | -0.051 (-1.20) |
| TUR | 0.007 (0.17) | -0.012 (-0.26) | **0.330** (5.28) | 0.027 (0.16) | 7.196 (1.63) | 0.008 (0.17) | **0.351** (5.91) | 0.048 (0.33) |
| USA | -0.101 (-1.49) | -0.128 (-0.62) | **0.408** (3.42) | 1.322 (1.08) | -76.700 (-1.61) | -0.054 (-0.21) | **0.355** (2.61) | 1.027 (0.71) |
| ZAF | 0.064 (0.79) | 0.097 (0.71) | **0.302** (2.64) | -0.188 (-1.30) | -1.160 (-0.49) | 0.196 (1.56) | **0.206** (2.27) | -0.010 (-0.06) |

Note : This table reports the CS-DL estimation results for each country.

t-statistics in parentheses, coefficients are in bold if the associated t-statistics are above 1.65 in absolute value