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Public Expenditure and Economic Growth: A Systematic Review of Empirical Evidence and Methodological Limitations

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Abstract
Whether government spending promotes or retards economic growth remains unresolved after four decades of research. This review synthesizes 21 empirical studies from Scopus, following PRISMA 2020 guidelines, covering studies from 1992 to 2025 on single-country time-series and cross-country panels. Eleven studies find a positive effect, two find a negative effect, four show effects varying with expenditure composition or public sector size, two find no significant link, and two suggest causality runs from growth to expenditure. The variation is systematic: positive results cluster in studies disaggregating spending and estimating long-term relationships; negative and null results appear in studies of aggregate spending, recurrent expenditure, or contexts with weak institutions. Methodologically, most studies use the autoregressive distributed lag bounds test, which establishes cointegration but not causal direction, leaving results ambiguous between Wagnerian and Keynesian views. Common weaknesses include short samples, unaddressed endogeneity, lack of structural break analysis, and aggregation bias. The review suggests that the growth impact of public expenditure depends on conditions, not universality, and much disagreement arises from methodological differences rather than economic fundamentals.
Keywords: public expenditure; economic growth; systematic literature review; PRISMA; expenditure composition; institutional quality

Introduction

Whether the state spends its way toward growth or away from it is among the oldest unresolved questions in public finance. Two founding positions frame the debate, and they point in opposite directions. Wagner (1883/1958) argued that the scope of state activity expands as an economy develops, making public expenditure a consequence of rising national income rather than a cause of it. Keynes (1936) reversed the arrow, treating government outlay as an autonomous instrument capable of raising output when private demand falls short. The distinction matters for everything that follows, because a study that estimates a positive association between spending and income has not thereby established which variable moved first. Later theory refined rather than resolved the disagreement. Endogenous growth models identify a productive role for government where spending finances inputs that private agents underprovide, so that infrastructure, education and research raise the marginal product of private capital (Aschauer, 1989; Barro, 1990). Work on expenditure composition sharpened the claim further, showing that the growth effect depends on which components expand. Devarajan, Swaroop and Zou (1996) found that spending categories

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conventionally treated as productive were associated with slower growth in developing countries, precisely because those categories had already been expanded beyond their efficient scale. Bose, Haque and Osborn (2007) reached a partly opposing conclusion using disaggregated capital expenditure. Against these stands the public choice tradition, in which an oversized state diverts resources into rent-seeking and administration. The Armey curve (Armey, 1995) formalises the resulting non-monotonicity, with spending raising growth up to a threshold share of output and depressing it beyond. Empirical work has multiplied without converging. Nyasha and Odhiambo (2019) survey international evidence and find no dominant result. Findings differ across countries, sample periods, expenditure classifications and estimators, and the field lacks an accepted account of why. Narrative reviews have documented the disagreement but rarely specify how studies were selected, which leaves them exposed to the charge that the pattern reported reflects the reviewer's reading list.

This review therefore applies a documented, reproducible selection procedure to the question. It is guided by three questions. First, what is the predominant direction of the relationship between public expenditure and economic growth reported in the empirical literature? Second, under what conditions does that direction change, and specifically does expenditure composition, time horizon, or institutional quality account for the divergence in findings? Third, what methodological limitations characterise the existing evidence, and how do those limitations shape the conclusions drawn? The contribution is correspondingly threefold. The review provides a classified synthesis of empirical findings that separates genuine disagreement from disagreement generated by differences in specification. It evaluates the methodological choices underlying that evidence, with particular attention to the identification problem created by the field's reliance on cointegration methods. And it draws out implications for fiscal policy design in developing and emerging economies, where the composition of spending is more readily adjusted than its level.

2. Methodology

2.1 Protocol and Reporting Standard

The review follows the PRISMA 2020 reporting guideline (Page et al., 2021), which supersedes the earlier statement of Moher, Liberati, Tetzlaff and Altman (2009). The review was not registered in PROSPERO, and this is noted among its limitations in Section 5.

2.2 Search Strategy

A single database, Scopus, was searched on [insert date of search], selected for its coverage of peer-reviewed economics journals. The following string was applied to title, abstract and keyword fields:

> TITLE-ABS-KEY ("public expenditure" OR "government expenditure" OR "public spending" OR "government spending") AND ("economic growth" OR "GDP growth" OR "output growth") AND (LIMIT-TO (SUBJAREA, "ECON") AND (LIMIT-TO (DOCTYPE, "ar") AND (LIMIT-TO (LANGUAGE, "English")

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2.3 Eligibility Criteria

Studies were eligible if they were peer-reviewed journal articles in English, indexed under Economics, Econometrics and Finance, and reported an empirical estimate of the relationship between some measure of public expenditure and some measure of economic growth or output. Studies were excluded if they examined expenditure or growth in isolation without an estimated link between them, if they were conceptual or purely descriptive, or if the full text could not be obtained.

2.4 Study Selection

The search returned 790 records. At screening, 137 were removed as non-English or as document types other than journal articles, and a further 315 fell outside the Economics, Econometrics and Finance subject area, leaving 338 records for title and abstract review. Of these, 245 could not be obtained in full text and were excluded on retrievability. The remaining 93 were assessed in full, and 72 were excluded: 54 did not estimate a relationship between expenditure and growth, and 18 addressed adjacent fiscal questions such as debt sustainability or tax structure without an expenditure–growth estimate. Twenty-one studies met all criteria and form the evidence base for this review. The corresponding flow diagram is reported below, following the PRISMA 2020 template. Three changes from the earlier version should be noted. The eligibility and inclusion boxes now carry different values, since the first records studies assessed and the second records studies retained. The subject-area exclusion appears once, at screening, rather than twice. And the identification box reads “Records identified from database (Scopus, n = 790)”.

2.5 Data Extraction and Synthesis

For each included study the following were extracted: authorship and year, country or region, sample period, expenditure and growth measures, estimator, direction and significance of the reported effect, and limitations acknowledged by the authors. Because the included studies use incompatible expenditure measures, estimators and functional forms, no meta-analytic pooling of effect sizes was attempted. Synthesis is narrative, supported by a structured classification of reported directions described in Section 3.2. Screening and extraction were conducted by a single reviewer, and no inter-rater agreement statistic is therefore available.

2.6 Quality Appraisal

No formal risk-of-bias instrument was applied. Methodological weaknesses reported in Section 3.3 were identified through structured reading of the estimation strategies rather than through a validated appraisal tool, and conclusions should be read with that in mind.

3. Results and Discussion

3.1 Characteristics of the Included Studies

The 21 studies span 1992 to 2025 and cover single countries in Africa, Asia, the Middle East, Latin America and Europe, alongside four multi-country panels. Table 1 summarises them.

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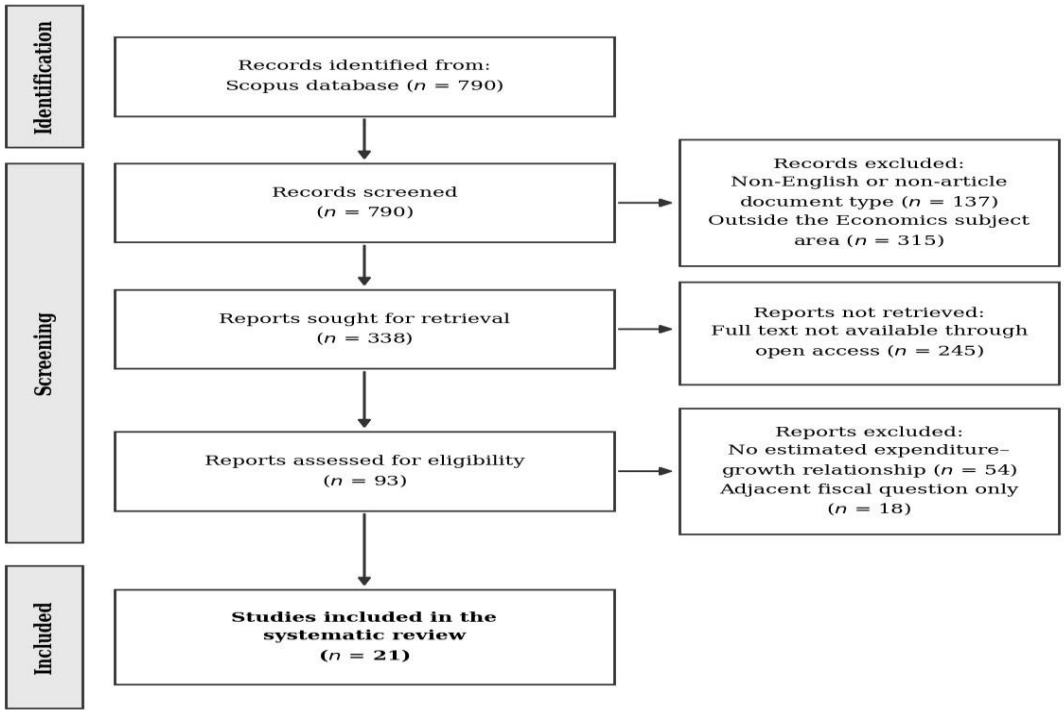
PRISMA 2020 Flow Diagram

Study Identification, Screening and Inclusion



Note: Adapted from the PRISMA 2020 statement (Page et al., 2021). All counts below the identification stage must be replaced with the figures from the Scopus export. The diagram as drawn satisfies the identity 790 − 137 − 315 − 245 − 72 = 21.

Table 1

Characteristics and Findings of Studies Included in the Review

Study (context, period)	Variables and method	Reported effect on growth	Classification	Principal limitation
Sahn (1992); sub-Saharan Africa, 1977–1989	Total, per capita and social sector expenditure, interest repayments; descriptive and elasticity analysis of IMF and World Bank data	Total and social spending rose, but population growth eroded per capita outlay; no	No estimated effect	Welfare outcome series unavailable; possible selection bias

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		growth effect estimated		
Gadinabokao & Daw (2013); South Africa, 1980–2011	GDP, general government expenditure, gross fixed capital formation; OLS, Johansen and Engle–Granger cointegration, ECM	Positive in the long run, negative in the short run	Positive	Reliance on secondary SARB data
Afonso & Tovar Jalles (2014); cross-country panel [confirm coverage and period]	Real GDP per capita, total expenditure, revenue; panel Granger causality with difference and system GMM	Weak causality from expenditure to GDP; stronger support for causality from GDP to expenditure	Positive (weak); also supports reverse causality	Five-year averaging suppresses cyclical variation
Merza & Alhasan (2016); Kuwait [confirm period]	Nominal GDP, current education, health, infrastructure and development expenditure; ADF and Zivot–Andrews tests, Johansen cointegration, Granger causality	Long-run equilibrium relationship; development expenditure has the strongest causal effect	Positive	Rentier economy with small population limits generalisability; nominal GDP used
Lupu & Asandului (2017); Eastern European countries [confirm period]	Real GDP growth, expenditure as share of GDP, unemployment; ARDL, Zivot–Andrews test	Non-monotonic: positive below an optimum of 37–41 per cent of GDP, negative above it	Mixed/conditional	Findings specific to post-communist transition economies

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Iwegbunam & Robinson (2019); South Africa [confirm period]	Real GDP, gross government expenditure, private consumption, capital formation, employment ratio, FDI; VECM	Positive long-run equilibrium relationship	Positive	Structural legacy of earlier political regimes
Gnangoin et al. (2019); Asian countries [confirm period]	Per capita GDP growth, education spending, domestic investment, government consumption, inequality, regulatory quality; difference GMM, panel Granger causality	Investment positive; government consumption negative; education spending insignificant	Mixed/conditional	Sample spans the 2007–08 financial crisis
Bougharriou, Benayed & Gabsi (2019); 16 Arab countries, 2002–2013	GDP growth, democracy indices, FDI inflows, public consumption expenditure; dynamic panel simultaneous equations with difference GMM	Public consumption expenditure hinders growth, offsetting the positive FDI channel	Negative	No direct democracy–growth relationship identified
Nguyen (2019); Vietnam [confirm period]	GDP, development investment expenditure, recurrent expenditure; OLS	Recurrent expenditure positive and significant; development investment not significant	Mixed/conditional	Single-equation OLS does not address endogeneity
Mbanyele (2019); Zimbabwe [confirm period]	GDP, capital and consumption expenditure, inflation, FDI, real interest rates; ARDL and VECM with structural dummies	Long-run association for consumption expenditure but no causality; growth causes	Reverse causality	Post-2014 currency distortions

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		capital expenditure		
Onifade et al. (2020); Nigeria [confirm period]	Real GDP, recurrent and capital expenditure, public debt, private consumption, domestic investment; ARDL bounds testing, Granger causality	Recurrent expenditure and debt significantly negative; capital expenditure positive but insignificant	Negative	Results conditional on fiscal discipline assumptions
Chu, Hölscher & McCarthy (2020); high-income and low/middle-income panels [confirm period]	Per capita GDP growth, productive and non-productive spending, revenue, deficit, investment, inflation, labour force; fixed effects OLS and one-step system GMM	Shifting composition toward productive spending raises growth; larger total spending crowds out private activity	Mixed/conditional	Small-sample standard error bias; instrument proliferation
Derbentsev et al. (2021); Central European countries [confirm period]	GDP per capita, R&D and education expenditure; panel FE and RE, Wald, Breusch–Pagan and Hausman tests	Positive and significant coefficients on both education and R&D expenditure [verify magnitudes and units before reporting]	Positive	No lag structure; pre-2008 series may differ in properties
Villela & Paredes (2022); Honduras, 1990–2020	GDP per capita, education spending ratios, enrolment, health indicators, inflation, unemployment; instrumental variables (2SLS)	No significant association between education expenditure and growth	No significant effect	Sparse and unreliable historical institutional data

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Wahiba & Ahlem (2022); [confirm country/region and period]	Output per unit of capital, public spending, investment, inflation, HDI; generalised least squares	Positive for both growth and poverty reduction	Positive	High regional poverty persists despite spending
Paudel (2023); Nepal [confirm period]	Per capita GDP growth, capital and current expenditure disaggregated by education and health; ARDL cointegration with ECM	Education spending strongly positive on both capital and current account; health positive for capital outlay only; aggregate expenditure insignificant	Positive (sectoral)	Weak budget execution capacity
Mbulawa (2024); fast-growing and emerging African economies [confirm period]	Government expenditure, GDP per capita, tax effort, ODA, economic freedom index, urbanisation; ARDL	Wagner's law holds: growth precedes expenditure. Economic freedom restrains expenditure more effectively than the polity measure	Reverse causality	Weak institutions; administrative cost of aid
Mustafa & Abdullahu (2024); Western Balkans Six and Croatia, 2002–2021	GDP, public expenditure, FDI; OLS with fixed effects	Public expenditure and FDI both positively associated with GDP	Positive	Short panel; limited prior regional literature
Hamza (2024);	GDP, sectoral expenditure on	Education and general	Positive (sectoral)	Eight-year window; gaps

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[confirm country], 2015–2022	education, health, safety, defence and economic affairs; OLS and Pearson correlation	services positive; remaining sectors insignificant		in secondary data
Fadare & Oladipo (2025); [confirm country], institutional data from 1996	Real GDP, recurrent and capital expenditure, institutional quality, inflation, lending rate; ARDL	Both expenditure types significant predictors; institutional quality moderates the relationship positively	Positive	Institutional series unavailable before 1996
Lhajhouji, Hasnaoui & Bakhat (2025); Morocco [confirm period]	GDP growth, government expenditure, population growth, FDI; symmetric ARDL with unrestricted error correction	Positive in both the short and long run	Positive	Population growth may dilute per capita benefits

3.2 Classification of Reported Effects

Studies were assigned to one of five mutually exclusive categories on the basis of their predominant reported finding. Where a study reported effects of opposing sign for different components or time horizons and neither dominated, it was classified as mixed. Where the study's principal causal finding ran from growth to expenditure, it was classified as reverse causality regardless of the sign of the association. Each study appears once, so the categories sum to the sample. Eleven studies report a predominantly positive effect. These cluster around two design features: disaggregated expenditure measures and long-run estimation.

Merza and Alhasan (2016), Paudel (2023) and Hamza (2024) all find positive effects for specific productive components while aggregate or non-productive components fail to reach significance, which suggests that aggregation obscures the effect rather than that the effect is absent. Two studies report a predominantly negative effect. Bougharriou, Benayed and Gabsi (2019) attribute the result to public consumption specifically, and Onifade et al. (2020) to recurrent expenditure and debt service. Neither identifies capital expenditure as the source of the drag. Four studies report mixed or conditional effects, and these are the most informative for the review's second question.

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Lupu and Asandului (2017) locate the sign change at a threshold share of output. Chu, Hölscher and McCarthy (2020) locate it in composition, finding that reallocation toward productive spending raises growth while expansion of the total depresses it. Gnangoin et al. (2019) and Nguyen (2019) find opposing signs across components within the same economy. Two studies find no significant association. Villela and Paredes (2022) report a null result for education spending in Honduras, which the authors attribute to incomplete human capital accumulation rather than to the irrelevance of spending. Sahn (1992) estimates no growth relationship at all, and its retention in the sample is marginal under the stated eligibility criteria; readers may reasonably regard the effective sample as 20. Two studies identify causality running from growth to expenditure. Mbulawa (2024) confirms Wagner's law directly, and Mbanyele (2019) finds that output growth causes capital expenditure rather than the reverse.

Afonso and Tovar Jalles (2014), classified above as weakly positive, also report stronger evidence for the Wagnerian direction than the Keynesian one, so the reverse-causality finding is present in three of the 21 studies even though only two are classified there. That distribution answers the first research question in a qualified way. A majority of studies report positive effects, but the majority is not large, it is concentrated in disaggregated designs, and roughly a seventh of the sample finds the causal arrow pointing the other way entirely.

3.3 Methodological Approaches and Their Limitations

The autoregressive distributed lag bounds testing procedure of Pesaran, Shin and Smith (2001) appears in more of these studies than any other estimator. Its appeal is practical: it accommodates regressors integrated of different orders, estimates short-run and long-run parameters jointly, and performs acceptably in the short samples that national fiscal series impose. Its central weakness bears directly on this literature. Cointegration establishes that variables share a long-run path; it does not establish which variable adjusts to which. In a field where two theories predict the same correlation with opposite causal structures, an ARDL result without a supplementary causality test is close to uninformative about the question actually asked. Several of the included studies recognise this and pair ARDL with Granger causality testing; several do not. Sample size compounds the problem. Fiscal time series for individual developing economies rarely exceed forty annual observations, and Hamza (2024) works with eight. Small samples reduce power, inflate parameter uncertainty, and make bounds test outcomes sensitive to lag selection. Four further weaknesses recur. Data quality is the first: nearly all of these studies use secondary series compiled by international agencies, whose definitional treatment of expenditure varies across countries and revisions. Omitted variable bias is the second, and it operates in a specific direction here. Where governance and bureaucratic capacity are excluded, the coefficient on expenditure absorbs the effect of the institutions through which it is spent, so that spending appears unproductive when the failure lies in implementation.

Fadare and Oladipo (2025) is one of the few included studies to model institutional quality explicitly, and finds it operates as a positive moderator. The third is the treatment of structural

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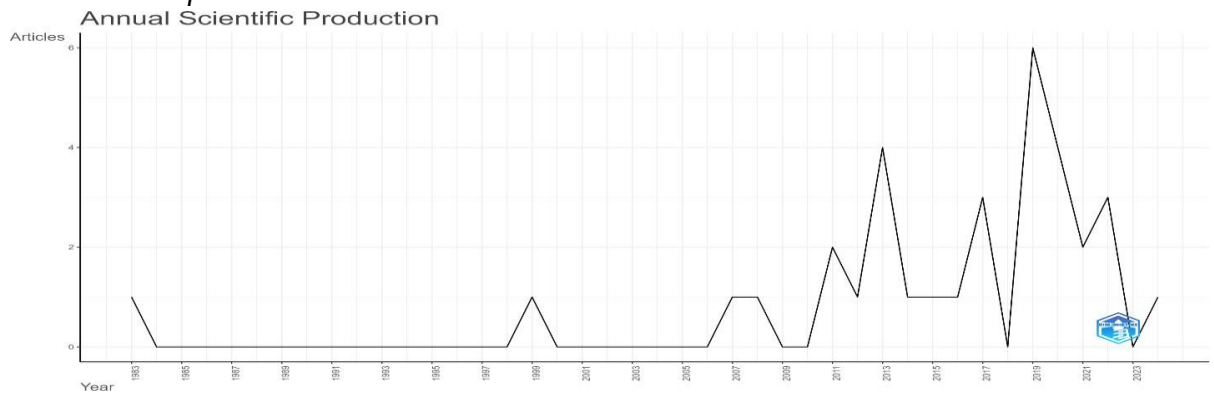
breaks. Conventional unit root and cointegration procedures assume parameter constancy across the sample. Financial crises, regime changes and pandemic-era fiscal expansions violate that assumption, and ignoring them can generate spurious evidence of cointegration or misclassify stationarity. Only Mbanyele (2019) and the studies employing Zivot–Andrews procedures address this directly. The fourth is aggregation bias, which the results in Section 3.2 illustrate. When capital and recurrent expenditure are summed, a positive effect from one and a negative effect from the other net toward zero, producing the insignificant aggregate results that Paudel (2023) and Onifade et al. (2020) both report alongside significant disaggregated ones. Beyond ARDL, ordinary least squares is used for baseline estimation where data are thin, and generalised method of moments estimators appear in the panel studies to address endogeneity from reverse causality and unobserved heterogeneity. Since Wagner's law implies that fiscal variables respond to output, endogeneity is not a technicality in this literature but its central identification problem, and studies without an explicit identification strategy are estimating a policy reaction function as much as a growth effect.

3.4 Bibliometric Characteristics of the Retrieved Literature

Two figures describe the corpus from which the included studies were drawn. Both were produced from the retrieved bibliographic records rather than from the 21 studies synthesised in Section 3.2, and they are reported as descriptive context for the field rather than as evidence bearing on the review questions.

Figure 1

Annual Scientific Production



Note. Produced in *R*. The vertical axis reports articles published per year.

Figure 1 plots annual scientific production. Output is intermittent and close to zero for two decades, with isolated contributions in the early 1980s and the late 1990s. Sustained activity begins around 2007 and rises through the 2010s, reaching a maximum of six articles in 2019 before falling back. The timing tracks three developments: wider availability of cross-country fiscal panels, diffusion of the ARDL and GMM estimators, and renewed policy attention to fiscal instruments after the 2008 financial crisis. The decline after 2019 should not be read as waning interest, since the most recent years are incompletely indexed at the time of retrieval.

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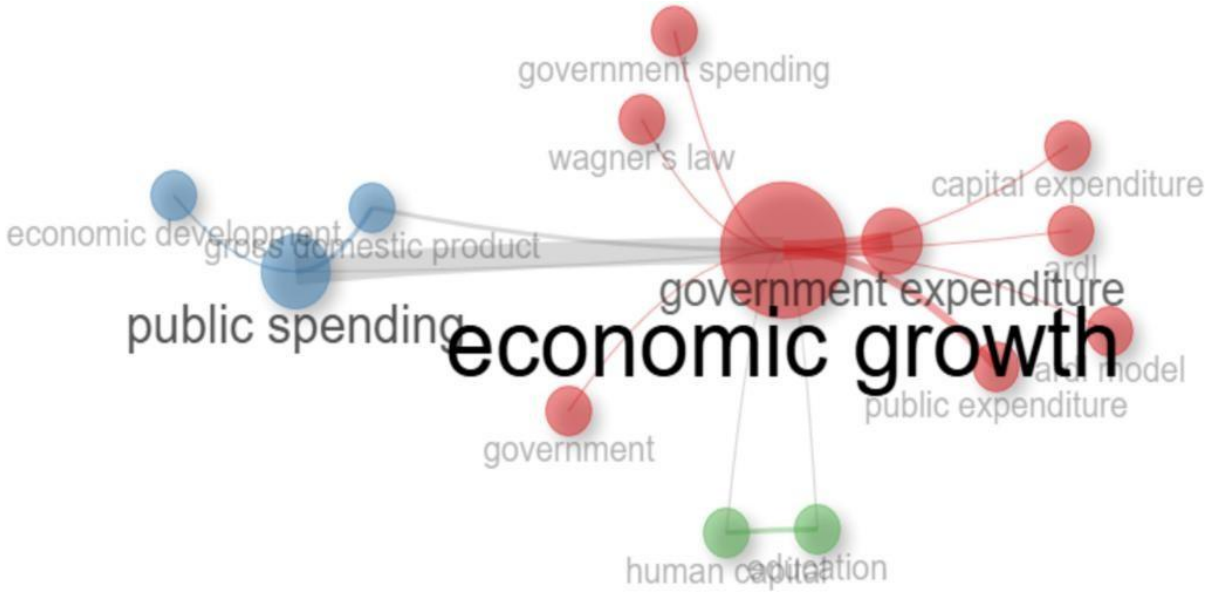
Figure 2

Keyword Co-occurrence Network



Note. Produced in R. Node size reflects keyword frequency and edge weight reflects cooccurrence. Figure 2 maps keyword co-occurrence. Economic growth and government expenditure occupy the centre, which the search design guarantees. Three clusters surround them. The largest links government expenditure to Wagner's law, capital expenditure, public expenditure and the ARDL model, combining the field's dominant theoretical proposition with its dominant estimator in a single group. A second cluster connects public spending to gross domestic product and economic development. A third and much smaller cluster pairs human capital with education. What the map does not contain is as informative as what it does. No node for institutional quality, governance, corruption or bureaucratic capacity appears anywhere on it, although those concepts are invoked in the discussion sections of most of the studies reviewed in Section 3.2. Authors treat institutions as context to be mentioned rather than as a variable to be measured, which is the omitted variable problem of Section 3.3 seen from the other side. Because these are author keywords, the prominence of ARDL should be read as evidence that authors regard the method as a defining feature of their contribution rather than as a direct census of methods used.

Taken together, the two bibliometric figures show a literature that is growing in volume while remaining concentrated on a narrow set of concepts and methods. The temporal trend indicates rising scholarly and policy relevance. The co-occurrence network indicates conceptual centralisation around a single pair of terms and methodological dependence on a single estimator. Both patterns support the case for the systematic assessment attempted here, which examines not only what the expenditure–growth literature concludes but how its methodological choices shape those conclusions.

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4. Conclusion

This review synthesised 21 empirical studies of the relationship between public expenditure and economic growth, selected through a documented PRISMA 2020 procedure, and assessed both what they find and how they find it. The evidence does not support a general claim in either direction. It supports a conditional one. Productive and capital-oriented expenditure directed toward infrastructure, education, health and research is associated with higher growth in most studies that measure it separately, and those effects are more visible over long horizons than short ones. Recurrent and consumption expenditure is associated with neutral or negative outcomes, particularly where it contributes to deficits or where the public sector has expanded past the threshold that the Armey curve describes. The two propositions are not in conflict; they are statements about different components, and the appearance of conflict in this literature owes a great deal to studies that sum those components before estimating. Institutional capacity conditions all of it. Where governance is weak, spending of any composition tends to underperform, and the studies that model institutions explicitly find them to operate as moderators rather than as separate determinants. This offers the most economical explanation available for the dispersion of results across regions at similar income levels.

Three findings bear on how the field should proceed. Disaggregation is not a refinement but a requirement, since aggregate specifications systematically understate effects that operate through composition. Identification deserves more attention than it currently receives: with roughly a seventh of the reviewed studies finding causality running from growth to expenditure, no result that rests on cointegration alone can be read as evidence of a growth effect. And institutional quality needs to enter estimation rather than discussion. For fiscal policy, the implication is that the composition of the budget is a more promising lever than its size. In states where aggregate expenditure is already constrained by revenue capacity or fiscal rules, reallocation toward capital and human capital formation offers growth returns that expansion of the total does not.

5. Limitations of This Review

Several constraints qualify these conclusions. The search covered a single database. Web of Science, EconLit and citation chaining would likely have identified further eligible studies, and no supplementary searching was undertaken. The restriction to open-access full texts removed 245 records, and this is the most serious limitation. Open access governs retrievability, not quality, and the restriction plausibly biases the sample toward journals with article processing charges and toward regions where such charges are affordable. Any inference about the global distribution of findings should be treated cautiously for that reason. The English-language restriction excludes work on Latin American and Francophone African fiscal systems published in Spanish, Portuguese and French, where the questions examined here are actively researched. Screening and extraction were performed by one reviewer, so no agreement statistic is available and classification decisions in Section 3.2 were not independently verified. No validated risk-of-bias instrument was applied, and the review was not prospectively registered. Finally, the heterogeneity of the included studies

precluded meta-analysis, so the classification reported here is a vote count, a method that weights a study using forty observations equally with one using eight.

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Appendix: Editorial Notes for the Author

The following items could not be resolved without access to the original Scopus export and must be completed before submission.

First, the three sub-counts in Section 2.4 (245 records not retrievable, 54 excluded for absence of an estimated relationship, 18 excluded as adjacent fiscal questions). The identity 790 − 137 − 315 − 245 − X = N must hold. The manuscript is written for N = 21 and X = 72, because 21 is the number of studies the evidence table actually supports. If the true included set is 58, Table 1 must be extended to 58 rows and the classification in Section 3.2 recomputed.

Second, the exact Scopus query string and the date the search was executed.

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Third, the eleven country and period cells marked in Table 1, together with the Wahiba and Ahlem (2022) reference details.

Fourth, the Derbentsev et al. (2021) coefficient magnitudes. The figures reported in the earlier draft (7.5 euro of GDP per additional euro of education spending, 4.23 euro for R&D) imply multipliers that are not economically plausible and most likely reflect a misreading of units in a levels or per capita specification. Verify against the source before reporting any numerical magnitude.

Fifth, the software used to generate Figures 1 and 2 and the record set they were computed on. The annual production series in Figure 1 sums to approximately thirty articles and spans 1983 to 2024, which matches neither the 790 records identified nor the 21 studies included, so the corpus behind the bibliometrics is at present undocumented.

Sixth, approximately forty references that appeared in the earlier draft have been removed because nothing in the text cited them. If those works belong to the review corpus, they must appear in Table 1 rather than in the reference list. A reference list is not a record of everything screened.