Oil-Price Exposure and Budget Deficit Persistence: Evidence from Oil-Exposed Economies

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Abstract

Large deficits can reflect persistent structural imbalances or temporary external disturbances to fiscal resources. This paper asks whether a large externally generated fiscal deterioration remains visible in subsequent annual fiscal balances. I study 14 oil-exposed economies over 2009-2023 using a differential-exposure design: annual Brent-price growth is interacted with each country's average oil rents as a share of GDP during 2006-2008, fixed before the estimation window. Country and year fixed effects absorb persistent country heterogeneity and common global conditions. Because identification is driven by only 15 annual common price movements, inference uses two-way clustering and complete-enumeration wild-cluster bootstrap tests by year. Adverse exposure-adjusted oil-price movements are associated with a large contemporaneous deterioration in the fiscal balance: the preferred coefficient is -0.511, with a complete-enumeration year-cluster bootstrap p-value of 0.023. The estimate is stable to major-episode exclusions, country-specific trends, leave-one-country-out estimation, source expansion, and alternative pre-period exposure definitions. Yet the adverse response is near zero one year later and is not negative at horizons two and three; adding lagged fiscal balances and lagged shock controls leads to the same conclusion. Favorable movements are weak on impact but are associated with stronger balances one year later. A separate eight-country mechanism test finds no robust evidence that compensation-based expenditure rigidity amplifies the adverse response. These estimates concern persistence in the annual fiscal-balance response, not reversal of the debt stock: an impact deficit can raise debt even if subsequent balances recover. The evidence therefore distinguishes acute fiscal deterioration from propagation: adverse oil-price exposure can worsen balances sharply on impact, but sustained deterioration requires mechanisms beyond the initial movement.

Keywords: budget deficits; fiscal balance; oil-price movements; differential exposure; fiscal persistence; fiscal rigidity

JEL classifications: C23; E62; H62; Q43

1. Introduction

Long-term fiscal sustainability is usually discussed in terms of slow-moving forces: demographic change, tax capacity, entitlement commitments, debt-service costs, and the institutions that govern spending and borrowing. Yet fiscal trajectories are also punctuated by large external disturbances. A sharp loss of revenue can move a government from surplus to deficit within a single budget year. The harder empirical question is not whether the balance deteriorates on impact, but whether that deterioration remains visible after the initial disturbance has passed.

This distinction connects the paper to the broader literature on deficit and debt sustainability. In fiscal-reaction approaches, debt dynamics depend not only on current deficits but also on whether governments respond systematically to rising debt and whether primary balances adjust over time (Bohn, 1998). Earlier sustainability frameworks similarly separate temporary budget disturbances from fiscal paths that violate the

intertemporal budget constraint (Blanchard, Chouraqui, Hagemann, and Sartor, 1990). The present paper does not estimate a full debt-sustainability model. It studies the prior flow question: whether a large externally generated deterioration in the annual fiscal balance remains visible in later annual balances. A temporary deficit can still raise the debt stock, so absence of a persistent balance response should not be read as evidence that the cumulative debt effect disappears.

This paper examines that distinction in a setting where externally generated fiscal pressure is large and where differential exposure is measurable before the outcome period. Oil-exposed economies face a common world price but differ sharply in how strongly oil income enters the domestic economy. A global decline in Brent crude is therefore not a uniform treatment: the same price movement represents a much larger exposure shock for an economy that entered the period with oil rents equal to half of GDP than for one in which oil rents were only a small share of national income. That cross-country heterogeneity provides the identifying variation for separating immediate fiscal deterioration from its subsequent propagation.

The question is related to, but distinct from, the established literature on fiscal procyclicality in commodity exporters. Oil-producing governments often expand expenditure during booms and confront difficult adjustment during busts (Villafuerte and Lopez-Murphy, 2010; Arezki and Ismail, 2013). Commodity-price shocks affect both revenue and expenditure, making their net effect on the budget balance an empirical question (Samake and Spatafora, 2012). Recent work has also documented nonlinear fiscal behavior across oil- and gas-revenue regimes (Durand-Lasserve and Karanfil, 2023) and has shown that structurally different oil shocks can change the probability of a fiscal-adjustment episode (Valencia, Gamboa-Arbelaez, and Sanchez, 2025). Fiscal institutions matter as well: rules and institutional design can influence procyclicality, adjustment, and the composition of spending (Frankel, 2011; Ardanaz et al., 2021; Apeti, Basdevant, and Salins, 2023).

The present analysis does not ask whether oil prices matter for public finances in general. It asks a narrower question that is directly relevant to persistent deficits: when an adverse exposure-adjusted oil-price movement worsens the headline fiscal balance, how much of that differential deterioration remains one, two, and three years later? A second question is whether compensation-based recurrent expenditure rigidity amplifies the adverse response. The paper treats these as distinct empirical questions: impact, persistence of the annual balance response, and one candidate propagation mechanism.

The empirical design combines a common annual Brent-price movement with a country-specific exposure measure fixed before the estimation period. Oil exposure is the average of oil rents as a share of GDP during 2006-2008. Annual Brent-price growth over 2009-2023 is interacted with that predetermined measure. The resulting variable is an exposure-scaled global oil-price movement. It is not a structurally identified oil-supply shock and it is not a direct measure of government oil revenue. Rather, it is a reduced-form treatment-intensity measure: year fixed effects absorb the common price movement, and identification comes from whether fiscal balances move differently in countries with greater pre-existing oil exposure.

The headline sample contains 14 countries whose complete 2009-2023 general-government fiscal-balance histories are verified within the April 2026 IMF World Economic Outlook vintage. The panel is balanced, with 210 country-year observations. A broader source audit contains 24 oil-exposed economies with the same 15-year analytical window; ten require IMF historical mirrors for part of their fiscal histories. Those ten are excluded from the headline estimates and retained only for source-sensitivity analysis. Appendix A reports both the strict sample and the provenance of the expanded sample. The 2009-2023 window is held fixed across specifications and source tiers rather than selectively appending later observations to particular countries.

The econometric sequence is deliberately transparent. Pooled OLS is reported only as a benchmark. Country fixed effects absorb time-invariant institutional and structural differences, while year fixed effects absorb global events common to all countries, including the direct level effect of Brent-price movements. The main specification separates favorable and adverse exposure-adjusted movements. Persistence is examined with local projections at horizons from zero through three years. Because the common oil-price component supplies only 15 annual innovations in the impact equation, conventional country clustering alone can overstate precision. The paper therefore reports two-way country-and-year clustered standard errors and complete-enumeration wild-cluster bootstrap tests across both cluster dimensions.

The evidence is sharp on impact. The estimated coefficient on the magnitude of an adverse exposure-adjusted oil movement is -0.511 in the country-and-year fixed-effects model. The estimate remains close to -0.51 when 2020 is excluded, when the 2014-2015 oil-price collapse is excluded, and when country-specific linear trends are added; it remains negative in all 14 leave-one-country-out estimations. Complete-enumeration wild-cluster bootstrap inference by year gives a p-value of 0.0228. Reconstructing exposure with the 2006-2008 median or with any individual pre-period year also leaves the adverse coefficient tightly clustered between -0.537 and -0.472.

The dynamic response changes the interpretation of that impact result. The adverse coefficient is essentially zero one year later. At horizons two and three it becomes positive rather than remaining negative, although longer-horizon estimates are imprecise under complete-enumeration year-cluster inference because the number of shock years falls. The data therefore do not support a persistent negative annual fiscal-balance response generated by the initial exposure-adjusted oil-price movement alone. Favorable movements follow a different timing pattern: their impact coefficient is small and imprecise, while the one-year response is positive and precisely estimated.

A separate mechanism exercise evaluates one possible reason why a temporary fiscal deterioration could become persistent. On an eight-country panel with audited expenditure-composition data, the shock is interacted with lagged compensation of government employees as a share of government expense. The adverse-shock interaction is positive rather than negative and is statistically imprecise under complete-enumeration eight-cluster wild-bootstrap inference. Compensation-based fiscal rigidity therefore does not provide a robust amplification channel in this sample.

The paper contributes in four ways. First, it uses predetermined differential exposure: a common global price movement is scaled by oil dependence measured before the outcome period, so the identifying comparison is between more- and less-exposed economies facing the same annual Brent movement. Second, it traces the continuous general-government fiscal balance rather than only spending components or a discrete fiscal-adjustment event. Third, it separates impact from persistence using local projections and explicitly defines persistence as persistence in the annual balance response rather than persistence in the debt stock. Fourth, it aligns inference with the effective number of common annual shocks through two-way clustering and complete-enumeration wild-cluster bootstrap tests. The robustness design additionally shows that the main timing pattern is not tied to one crisis episode, one country, one source-vintage choice, or one construction of the pre-period exposure measure.

These contributions should be read with appropriate limits. The design identifies differential exposure to a common global price movement; it does not identify the structural source of each oil-price innovation. Oil rents as a share of GDP proxy economic oil exposure rather than the government's marginal oil-revenue share, so the coefficient should not be interpreted as a universal revenue elasticity. The 24-country source audit is the analytical set assembled for this draft, not a claim to exhaust the universe of oil-producing economies. Finally, the outcome is the annual headline fiscal balance, not debt. A temporary impact deficit

can permanently raise the level of public debt even when later annual balances recover. The paper's claim is therefore deliberately narrower: the initial exposure-adjusted price movement does not, by itself, generate a persistently negative fiscal-balance response in this sample.

The rest of the paper is organized as follows. Section 2 relates the analysis to the literature and develops the empirical predictions. Section 3 describes the data and construction of the exposure measure. Section 4 sets out the econometric strategy and inference. Section 5 reports the main results and dynamic responses. Section 6 presents robustness tests and the expanded-sample evidence. Section 7 evaluates the expenditure-rigidity mechanism. Section 8 discusses what the estimates do and do not imply for the study of persistent deficits. Section 9 concludes.

2. Related Literature and Empirical Predictions

2.1 Commodity shocks and fiscal outcomes

The fiscal consequences of commodity cycles have been studied from several angles. A broad political-economy literature documents that fiscal policy is frequently procyclical in developing economies, with institutional weaknesses helping to explain why governments spend more during good times and have less room to stabilize activity during downturns (Alesina, Campante, and Tabellini, 2008). Commodity exporters are especially exposed because a large portion of national income, foreign exchange, and often government revenue can move with world prices.

Evidence from oil-producing countries shows that expenditure patterns respond strongly to boom-bust cycles. Villafuerte and Lopez-Murphy (2010) document large changes in fiscal balances, expenditure growth, and the use of oil windfalls across the 2004-2008 boom and subsequent decline. El Anshasy and Bradley (2012) find that oil prices affect fiscal policy in oil exporters, while Samake and Spatafora (2012) show that commodity-price shocks influence both revenue and spending in developing economies. Because both sides of the budget adjust, the effect on the headline deficit need not be a mechanical mirror of the price movement.

Arezki and Ismail (2013) emphasize asymmetry in expenditure adjustment. In their analysis, current spending is downwardly sticky during busts while capital expenditure bears more of the adjustment. Durand-Lasserve and Karanfil (2023) likewise find that fiscal behavior depends on the configuration of oil and gas revenue, including whether revenue is high or low and rising or falling. These studies make clear that the fiscal response to a commodity movement is state dependent and that spending categories do not adjust symmetrically.

Recent work by Valencia, Gamboa-Arbelaez, and Sanchez (2025) moves beyond price changes alone by distinguishing structural oil supply and demand shocks and examining the probability of fiscal adjustment. Their outcome is a discrete adjustment event. The present study is complementary. It does not seek to identify structural supply and demand innovations. Instead, it asks how a continuous headline fiscal balance moves when a common global price change is more or less important across countries because of predetermined exposure.

2.2 Fiscal institutions, debt, and persistence

The literature on fiscal institutions is relevant because persistence cannot be understood from the initial fiscal disturbance alone. Fiscal sustainability depends on whether primary balances eventually respond to

debt and whether budget rules, political institutions, and expenditure commitments allow deficits to adjust after the initial shock (Blanchard et al., 1990; Bohn, 1998). In resource-rich economies, institutional design is also central to the management of revenue volatility. Frankel (2011) shows how Chile's structural-budget institutions helped reduce procyclicality in a commodity-exporting setting. Ardanaz et al. (2021) demonstrate that the design of fiscal rules affects whether public investment is compressed during consolidations. Apeti, Basdevant, and Salins (2023) find that fiscal rules in resource-rich countries are associated with stronger fiscal discipline and lower expenditure procyclicality.

One intuitive mechanism is recurrent expenditure rigidity. Salaries and other contractual or politically difficult-to-cut items may adjust more slowly than revenue. If a negative oil shock lowers fiscal resources while recurrent commitments remain fixed, the headline balance can absorb the gap. This mechanism is plausible, but it should be tested rather than assumed. Compensation of employees is only one part of rigidity; debt service, transfers, subsidies, intergovernmental commitments, and institutional constraints may also matter.

2.3 From impact to persistence

For the purpose of studying long-term deficits, the key distinction is between an **impact effect** and a **persistent path**. Let the fiscal balance be

$$Y_{it} = R_{it} - G_{it}, \tag{1}$$

where R_{it} denotes government revenue and G_{it} government expenditure. A negative oil-price exposure movement can lower fiscal resources immediately. If expenditure and non-oil revenue do not adjust one-for-one within the same year, the fiscal balance deteriorates on impact.

Persistence requires more than an impact loss. The initial fiscal gap must be propagated through future budgets, financing costs, expenditure commitments, weak revenue recovery, or institutional behavior. The empirical design is therefore organized around three predictions.

Impact prediction. Larger adverse exposure-adjusted oil movements should be associated with weaker contemporaneous fiscal balances.

Persistence prediction. If the exposure-adjusted oil-price movement itself creates a persistent deficit process, the adverse coefficient should remain negative at horizons beyond the impact year.

Rigidity prediction. If compensation-based recurrent expenditure rigidity is an amplification mechanism, the adverse-shock effect should become more negative as lagged compensation expenditure rises.

The first prediction concerns whether the exposure-adjusted movement creates an acute fiscal deterioration. The second asks whether the annual balance response remains negative after impact. The third evaluates one candidate mechanism for propagation. This separation is central to the paper: a large impact coefficient is not, by itself, evidence of a structural deficit process.

Throughout the paper, 'persistence' refers to persistence in the annual fiscal-balance response, not to reversal of the cumulative public-debt stock. If the impact-year deficit is debt financed, public debt can remain permanently higher even when subsequent annual balances return to their prior path. The local projections therefore answer a flow-response question; debt-stock persistence would require a separate outcome and is not inferred here.

3. Data and Measurement

3.1 Fiscal-balance outcome and sample construction

The dependent variable is general-government net lending/borrowing as a percentage of GDP from the IMF World Economic Outlook (WEO). Positive observations indicate net lending or a surplus; negative observations indicate net borrowing or a deficit. A negative regression coefficient therefore represents deterioration in the headline fiscal position.

The empirical files for this draft use a common 2009-2023 outcome window. The source audit contains 24 oil-exposed economies with non-missing 2006-2008 oil-rent exposure and 15 annual fiscal-balance observations assembled from IMF WEO materials. Fourteen countries have complete 2009-2023 histories verified within the April 2026 WEO vintage itself; these observations form the strict headline panel of 210 country-years. The strict classification is based on source provenance and completeness, not on estimated coefficients.

The remaining ten countries require IMF historical mirrors for at least part of the 2009-2023 fiscal history. They are retained in a 24-country, 360-observation source-sensitivity panel rather than pooled into the headline sample. Appendix A lists the provenance category for every country in the expanded audit. The 2009-2023 endpoint is kept common across all specifications in this draft; later WEO observations are not selectively appended to particular series because doing so would change the validated estimation window and source audit.

3.2 Predetermined oil exposure

Cross-country treatment intensity is based on oil rents as a percentage of GDP from the World Bank's World Development Indicators, indicator NY.GDP.PETR.RT.ZS. The World Bank defines oil rents as the difference between the value of crude-oil production at regional prices and total costs of production. The measure therefore captures the economic importance of oil extraction relative to GDP; it is not a direct measure of the government's take from the oil sector.

For each country, exposure is fixed at the mean of oil rents over 2006-2008:

$$E_i = \frac{OR_{i,2006} + OR_{i,2007} + OR_{i,2008}}{3}. \tag{2}$$

Using a three-year pre-period average reduces sensitivity to an individual base year and, more importantly, prevents the exposure measure from being updated in response to fiscal outcomes during the estimation period. The fixed measure is used to rank pre-existing exposure rather than to impose a common mapping from oil rents to government revenue. Section 6.4 verifies that the headline timing pattern is also obtained when exposure is constructed from the 2006-2008 median or from any one of the three pre-period years.

3.3 Exposure-adjusted global oil-price movement

Let P_t be the annual Brent price from the IMF Primary Commodity Prices series distributed through the Federal Reserve Bank of St. Louis. Annual price growth is

$$g_t = \frac{P_t - P_{t-1}}{P_{t-1}}. \tag{3}$$

The country-year treatment variable is

$$S_{it} = E_i g_t. \tag{4}$$

The scaling is economically transparent. Suppose pre-period oil rents equal 25 percent of GDP and Brent falls by 40 percent. Equation (4) gives $S_{it} = -10$. The same price decline generates a shock of only -2 for a country with oil rents equal to 5 percent of GDP.

This variable should be interpreted as an exposure-adjusted global oil-price movement, not as a structurally identified oil supply shock. Brent movements reflect supply, global demand, precautionary demand, and other forces. Year fixed effects absorb the common component of those forces and the direct level effect of Brent growth. Identification comes from their differential relevance across countries with different pre-period oil exposure. Because fiscal regimes differ in how oil rents translate into government revenue, the resulting coefficient is a reduced-form exposure gradient rather than a structural revenue elasticity.

To allow favorable and adverse movements to differ, the shock is decomposed as

$$S_{it}^F = \max(S_{it}, 0), \qquad S_{it}^A = \max(-S_{it}, 0). \tag{5}$$

Both components are non-negative. A positive coefficient on S^F means that favorable oil-price exposure strengthens the fiscal balance. A negative coefficient on S^A means that a larger adverse exposure-adjusted movement worsens it.

3.4 Descriptive statistics

Table 1 summarizes the strict-vintage sample. The average fiscal balance is -3.03 percent of GDP, with a standard deviation of 5.73 percentage points. Pre-period oil exposure averages 21.46 percent of GDP but varies widely, from 2.34 to 57.71 percent. That dispersion is essential for the research design: the annual Brent movement is common, but its implied fiscal exposure differs substantially across countries.

Table 1. Descriptive statistics: strict-vintage headline sample

Variable	N	Mean	Std. dev.	Min	Median	Max
General-government fiscal balance (% GDP)	210	-3.033	5.733	-19.600	-3.350	13.800
Pre-period oil exposure (% GDP)	210	21.464	18.118	2.337	13.683	57.713
Brent annual growth (%)	210	3.710	31.209	-46.628	-2.723	63.455
Exposure-adjusted movement, S	210	0.796	8.783	-26.911	-0.237	36.622
Favorable component, S^F	210	3.223	6.284	0.000	0.000	36.622
Adverse magnitude, S^A	210	2.427	4.683	0.000	0.237	26.911

Notes: The panel contains 14 countries observed from 2009 through 2023. Exposure is the country-specific mean of World Bank oil rents (% GDP) during 2006-2008. The fiscal balance is IMF WEO general-government net lending/borrowing (% GDP). No fiscal observations are interpolated.

Pairwise correlation and variance-inflation checks do not indicate a collinearity problem. The VIF is 1.11 for both S^F and S^A in the asymmetric impact specification. Because the positive and negative components are mutually exclusive within a year, they are not mechanically close to linear dependence.

4. Econometric Strategy

4.1 Benchmark specifications

The analysis begins with a pooled symmetric benchmark:

$$Y_{it} = \alpha + \beta S_{it} + \varepsilon_{it}. \tag{6}$$

Pooled OLS is not the preferred model because countries differ persistently in fiscal institutions, resource structures, and average budget positions. The next specification therefore introduces country fixed effects:

$$Y_{it} = \alpha_i + \beta S_{it} + \varepsilon_{it}. \tag{7}$$

The principal symmetric benchmark also adds year fixed effects:

$$Y_{it} = \alpha_i + \lambda_t + \beta S_{it} + \varepsilon_{it}. \tag{8}$$

The role of year effects requires emphasis. Raw Brent-price growth varies only over time and is absorbed by the year effects. The coefficient on the exposure-adjusted movement is identified from the interaction of that common annual movement with predetermined cross-country exposure. The estimand is therefore a differential response: in the same year, does the fiscal balance move more strongly in countries that entered the period with greater oil exposure?

Country effects absorb time-invariant differences, including average resource dependence, fiscal regimes, and persistent institutions. Year effects absorb common global shocks, including worldwide financial conditions and the direct level effect of the oil-price movement. The remaining identifying assumption is that there is no omitted country-specific fiscal shock whose covariance with the annual Brent movement systematically scales with pre-period oil exposure. Country-specific trends address a smooth version of this concern, while episode exclusions and leave-one-country-out estimates test whether the result is concentrated in particular years or economies.

The design is consequently best described as a reduced-form differential-exposure design. It is more informative than a simple correlation between Brent prices and budget balances because the common time-series price movement is absorbed by year effects and exposure is predetermined. It does not, however, justify treating the exposure-adjusted movement as a fully structural causal shock or interpreting the coefficient as a policy-invariant fiscal elasticity.

4.2 Asymmetric impact specification

The main impact model is

$$Y_{it} = \alpha_i + \lambda_t + \beta_F S_{it}^F + \beta_A S_{it}^A + \varepsilon_{it}. \tag{9}$$

Because S^A is the positive magnitude of an adverse movement, the impact prediction is $\beta_A < 0$. Same-period symmetry in the response to a favorable and adverse movement of equal magnitude implies

$$H_0\colon\ \beta_F + \beta_A = 0. \tag{10}$$

This restriction is tested directly. The paper does not infer asymmetry merely from one coefficient being statistically significant and the other not.

The 2009-2023 window contains seven years of positive Brent-price growth and eight years of negative growth. That modest number of sign regimes is another reason to treat same-period asymmetry as secondary and to emphasize the timing pattern and the conservative year-cluster inference.

The baseline specifications do not control for contemporaneous GDP growth or inflation. Both variables may themselves respond to oil-price movements and can form part of the fiscal transmission channel. Conditioning on them would therefore risk blocking part of the response that the reduced-form design is intended to measure. Country-specific trends are instead introduced as a robustness check for smooth differential fiscal trajectories.

4.3 Dynamic persistence with local projections

To distinguish an acute deficit from persistence, the impact model is extended using local projections in the spirit of Jorda (2005). For horizon $h = 0,1,2,3$,

$$Y_{i,t+h} = \alpha_i^h + \lambda_t^h + \beta_h^F S_{it}^F + \beta_h^A S_{it}^A + u_{i,t+h}. \tag{11}$$

The impact coefficient is the contemporaneous response. If adverse exposure-adjusted oil-price movements create a persistent negative annual fiscal-balance response, the adverse coefficient should remain below zero at subsequent horizons. A coefficient that returns toward zero indicates that the differential balance deterioration is no longer visible; a later positive coefficient is consistent with reversal or recovery in the annual balance. None of these responses, by itself, establishes that the public-debt stock returns to its pre-shock path.

The horizon structure is fixed at zero through three years. The paper does not extend or shorten the horizon in response to significance. As h increases, the number of usable shock years falls from 15 at impact to 12 at $h = 3$, so longer-horizon inference is necessarily less precise.

As a dynamic-control robustness check, the local projection is also estimated after adding the lagged fiscal balance and lagged favorable and adverse movements:

$$\begin{aligned} Y_{i,t+h} = \alpha_i^h + \lambda_t^h + \beta_h^F S_{it}^F + \beta_h^A S_{it}^A + \rho_h Y_{i,t-1} \\ + \phi_h^F S_{i,t-1}^F + \phi_h^A S_{i,t-1}^A + u_{i,t+h}. \end{aligned} \tag{12}$$

This specification is not used to select a preferred horizon. It is reported as a robustness check because fiscal balances are persistent and the previous year's oil-price movement may affect the fiscal position observed at later horizons.

4.4 Fiscal-rigidity mechanism

A secondary mechanism test asks whether compensation-based expenditure rigidity magnifies adverse shocks. The rigidity measure is

$$\mathrm{FR}_{i,t-1} = \frac{\text{Compensation of employees}_{i,t-1}}{\text{Government expense}_{i,t-1}} \times 100. \tag{13}$$

It is lagged by one year and centered at the mechanism-sample mean. The estimated equation is

$$Y_{it} = \alpha_i + \lambda_t + \beta_F S_{it}^F + \beta_A S_{it}^A + \delta \mathrm{FR}_{i,t-1}^c + \theta_F \left(S_{it}^F \times \mathrm{FR}_{i,t-1}^c\right) + \theta_A \left(S_{it}^A \times \mathrm{FR}_{i,t-1}^c\right) + \varepsilon_{it}. \quad (14)$$

The amplification prediction is $\theta_A < 0$. This model is estimated only on the eight countries with consistently audited rigidity data, giving 100 observations. It is treated as a mechanism exercise, not as the headline identification sample.

4.5 Inference with a common global shock

Inference is unusually important in this design. Observations can be correlated within countries over time, but the treatment also contains a common annual price movement. A large number of country-year observations does not create an equally large number of independent oil-price innovations. This is an aggregate-shock version of the inference problem emphasized by Moulton (1990).

The main tables therefore report two-way clustered standard errors by country and year following the logic of Cameron, Gelbach, and Miller (2011). For the central coefficients, complete-enumeration wild-cluster bootstrap tests are also calculated separately over country clusters and year clusters using Rademacher sign enumeration. MacKinnon and Webb (2017, 2018) show why bootstrap procedures are useful when cluster counts are limited, though no procedure eliminates the information constraint created by a small number of independent shocks.

For the impact model there are 14 country clusters and 15 year clusters, allowing complete sign enumeration of all 2^{14} and 2^{15} sign patterns. For the one-year response there are 14 shock years. At longer horizons, the number of independent years falls further; this is why the paper treats the $h = 2$ and $h = 3$ estimates as descriptive evidence about the shape of adjustment rather than as high-powered tests.

5. Main Results

5.1 From pooled OLS to two-way fixed effects

Table 2 reports the model sequence. The symmetric coefficient is positive throughout. Pooled OLS gives 0.285; country fixed effects give 0.276; and the country-and-year fixed-effects estimate is 0.269. The point estimate is therefore not an artifact of cross-sectional differences between high- and low-oil countries. Model fit rises substantially when fixed effects are introduced, from an R-squared of 0.191 in pooled OLS to 0.672 in the two-way fixed-effects model.

The asymmetric specification is more informative. The impact coefficient on favorable movements is 0.103 and imprecisely estimated. The coefficient on the magnitude of adverse movements is -0.511, with a two-way clustered standard error of 0.069. The formal symmetry restriction $\beta_F + \beta_A = 0$ is rejected under two-way clustered inference at the 5 percent level ($p = 0.036$), but the same restriction is not rejected under complete-enumeration year-cluster bootstrap inference ($p = 0.156$). For that reason, the paper emphasizes the timing pattern rather than a strong claim about unequal same-period magnitudes.

Table 2. Benchmark and asymmetric fiscal-balance estimates

	(1) Pooled OLS	(2) Country FE	(3) Country + year FE	(4) Current asymmetry	(5) One-year response
Symmetric exposure movement, S	0.285	0.276	0.269		

	(1) Pooled OLS	(2) Country FE	(3) Country + year FE	(4) Current asymmetry	(5) One-year response
	(0.070)	(0.083)	(0.075)		
Favorable movement, S^F				0.103	
				(0.123)	
Adverse magnitude, S^A				**-0.511**	
				(0.069)	
Favorable movement, S^F_{t-1}					**0.410**
					(0.102)
Adverse magnitude, S^A_{t-1}					-0.046
					(0.264)
Observations	210	210	210	210	196
Countries	14	14	14	14	14
Country fixed effects	No	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes
R^2	0.191	0.490	0.672	0.682	0.699

Notes: Dependent variable is general-government net lending/borrowing (% GDP). Standard errors in parentheses are two-way clustered by country and year. S^A is coded as the positive magnitude of an adverse movement, so a negative coefficient indicates fiscal deterioration. Column (5) is the $h = 1$ local-projection response to the shock dated t.

The magnitude of the adverse coefficient is economically meaningful. Consider a 40 percent Brent decline for a country with pre-period oil rents equal to 25 percent of GDP. The exposure-adjusted adverse magnitude is 10. The point estimate implies a contemporaneous fiscal-balance change of roughly

$$10 \times (-0.511) = -5.11$$

percentage points of GDP. This is an interpretation of the regression scale, not a claim that every price decline produces an identical mechanical budget effect. The actual fiscal response depends on the country's institutional, tax, ownership, and expenditure structure, much of which is absorbed only imperfectly by the reduced-form design.

5.2 Inference for the impact response

The impact result is not dependent on a single standard-error convention. Under two-way clustering, the p-value on S^A is below 0.001. Complete-enumeration wild-cluster bootstrap inference across the 14 countries gives $p = 0.0004$. More importantly for a design built from a common annual price series, complete sign enumeration across the 15 year clusters gives $p = 0.0228$.

Table 3. Few-cluster inference for the principal coefficients

Response	Coefficient	Two-way cluster p	Complete-enumeration WCB p, country	Complete-enumeration WCB p, year
Current adverse movement, S^A_t	-0.511	<0.001	0.0004	0.0228
One-year favorable movement, S^F_t	0.410	0.0015	0.0002	0.0122

Notes: Complete-enumeration wild-cluster bootstrap (WCB) p-values enumerate all Rademacher sign patterns over the relevant cluster dimension. The current specification has 14 country and 15 year clusters; the one-year response has 14 shock-year clusters. The table reports the two coefficients that define the principal timing pattern.

The year-cluster result is the more conservative check because it recognizes that the common oil-price component offers only 15 independent annual movements. The central conclusion therefore does not rest on treating 210 observations as 210 independent shocks. The formal same-period symmetry restriction has a two-way clustered p-value of 0.036, but its complete-enumeration year-cluster bootstrap p-value is 0.156. The analogous one-year symmetry test has a year-bootstrap p-value of 0.362. Thus the clearest evidence concerns timing rather than definitive structural asymmetry in equal-sized positive and negative movements.

5.3 Dynamic response and deficit persistence

The one-year response changes the interpretation of the impact result. The coefficient on the adverse magnitude falls from -0.511 at $h = 0$ to -0.046 at $h = 1$, with a two-way clustered p-value of 0.863. The initial deterioration is therefore not detectable in the next year's headline balance.

Favorable shocks show the opposite timing. Their impact coefficient is 0.103 and statistically weak, but the one-year coefficient is 0.410, with a two-way clustered p-value of 0.0015 and an complete-enumeration year-cluster bootstrap p-value of 0.0122. In the data, fiscal gains from favorable oil conditions appear more clearly after a delay than in the impact year.

Figure 1 and Table 4 extend the response to three years. The adverse response is negative only on impact. It moves close to zero at $h = 1$, turns positive at $h = 2$, and is positive at $h = 3$. The two-way clustered interval at $h = 3$ excludes zero, but the complete-enumeration year-cluster bootstrap p-value is 0.087 with only 12 shock-year clusters. The longer-horizon reversal should therefore be described as suggestive rather than definitive.

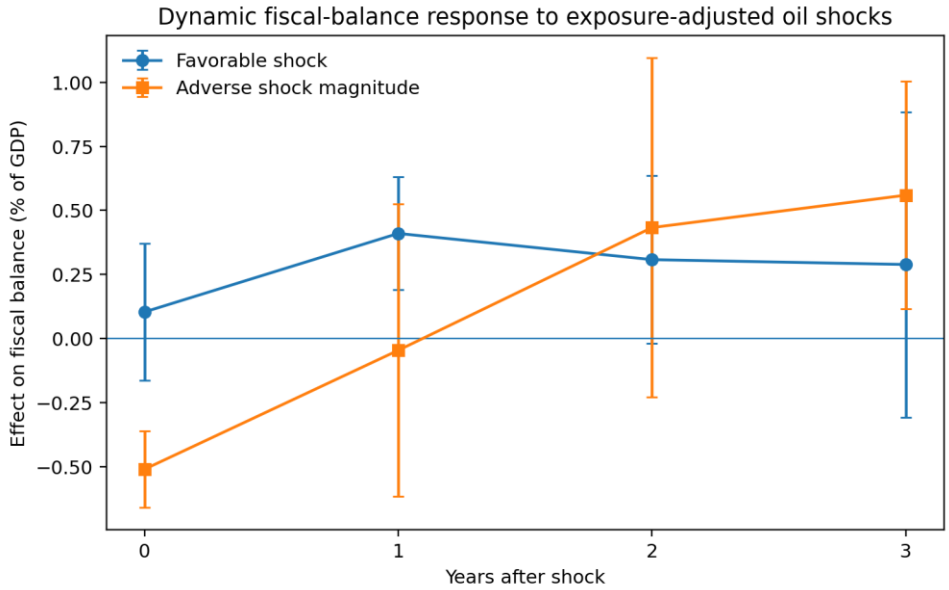


Figure 1. Dynamic fiscal-balance response to exposure-adjusted oil movements. Points are local-projection coefficients; error bars are 95% confidence intervals based on two-way country-and-year clustered standard errors.

Table 4. Local-projection responses, horizons 0-3

Horizon (years)	Favorable coefficient	Two-way SE	Two-way p	Adverse coefficient	Two-way SE	Two-way p

Horizon (years)	Favorable coefficient	Two-way SE	Two-way p	Adverse coefficient	Two-way SE	Two-way p
0	0.103	0.123	0.418	**-0.511**	0.069	<0.001
1	**0.410**	0.102	0.001	-0.046	0.264	0.863
2	0.308	0.150	0.063	0.433	0.304	0.180
3	0.289	0.271	0.309	0.559	0.202	0.018

Notes: Each row is a separate local projection of the fiscal balance at $t + h$ on shocks dated t, with country and shock-year fixed effects. The sample falls from 210 observations at $h = 0$ to 168 at $h = 3$. Complete-enumeration year-cluster WCB p-values at $h = 2$ are 0.142 for the favorable component and 0.192 for the adverse component; at $h = 3$ they are 0.295 and 0.087, respectively. Longer-horizon estimates should therefore be interpreted cautiously.

Table 5 reports the prespecified dynamic-control robustness check. Adding the lagged fiscal balance and lagged favorable and adverse movements reduces the impact adverse coefficient from -0.511 to -0.371, but the effect remains negative and survives complete-enumeration year-cluster bootstrap inference. The one-year adverse response remains statistically unsupported. The favorable response remains strongest at the one-year horizon. The persistence conclusion is therefore not an artifact of omitting the previous fiscal balance or the previous oil-price movement from the local projection.

Table 5. Dynamic-control local-projection robustness

Horizon (years)	Favorable coefficient	Year WCB p	Adverse coefficient	Year WCB p	Observations
0	0.253	0.022	**-0.371**	0.021	196
1	**0.371**	0.031	-0.217	0.548	182
2	0.208	0.188	0.398	0.131	168
3	0.234	0.461	0.605	0.065	154

Notes: Each row augments the local projection with the lagged fiscal balance and lagged favorable and adverse movements. P-values are complete-enumeration wild-cluster bootstrap p-values over shock-year clusters. The sample starts in 2010 because the robustness equation includes lagged controls.

The dynamic evidence is central for the deficit-persistence question. A large adverse exposure-adjusted movement can generate a sharp impact-year fiscal deterioration without producing a persistently negative annual balance response over the following one to three years. This is different from saying that deficits in oil exporters are generally temporary, and it is also different from saying that the debt consequences disappear. Persistent deficits can arise from many sources, while an impact deficit can raise the debt stock even if later balances recover. The narrower result is that the initial oil-price exposure movement is not sufficient, in this sample, to account for a persistently negative balance response once the impact year has passed.

6. Robustness and Source Sensitivity

6.1 Major oil episodes

The impact estimate could in principle be dominated by the extraordinary 2020 collapse in activity and oil demand or by the 2014-2015 oil-price decline. Table 6 shows that this is not the case. Excluding 2020 leaves the adverse coefficient almost unchanged at -0.504. Excluding 2014 and 2015 leaves it at -0.515. Both remain precisely estimated under two-way clustering.

This differs from the earlier 24-country mixed-vintage sample, where removing 2014-2015 weakened precision more noticeably. In the strict 14-country sample, the adverse impact response is not dependent on either of those episodes.

6.2 Country-specific trends and influential countries

A concern in exposure designs is that highly oil-dependent economies may follow different fiscal trends for reasons unrelated to annual oil prices. Adding country-specific linear trends addresses smooth differential trajectories. The adverse coefficient is -0.511, virtually identical to the baseline -0.511, with a two-way clustered p-value below 0.001.

Leave-one-country-out estimation provides a second influence check. Re-estimating the impact equation 14 times, omitting one country each time, yields adverse coefficients between -0.552 and -0.462. The sign never changes and the largest two-way p-value is approximately 0.009. The result is therefore not driven by Iraq, Saudi Arabia, Russia, or any other single large oil producer. The one-year favorable coefficient is similarly stable, ranging from 0.366 to 0.448 across the leave-one-out exercises.

6.3 Expanded 24-country sample

The expanded source audit contains 24 countries with analytically usable 2009-2023 fiscal histories. Ten rely on IMF historical mirrors for at least part of the period, whereas the 14 headline countries have their full histories verified within the April 2026 WEO vintage. The additional countries are deliberately retained as a source-sensitivity exercise rather than mixed into the headline sample. Appendix Table A2 reports the country-level provenance categories.

In the 24-country, 360-observation sample, the current adverse coefficient remains negative at -0.386, with a two-way clustered p-value of 0.007. The one-year favorable coefficient is 0.344, with $p = 0.003$. The qualitative timing pattern therefore survives the sample expansion. Complete-enumeration year-cluster inference for the 24-country impact estimate is more conservative ($p = 0.075$), reinforcing the decision to base the main claims on the fully verified strict-vintage sample rather than the larger mixed-source panel.

6.4 Sensitivity to the pre-period exposure definition

The headline exposure measure is the 2006-2008 mean, but the result is not driven by that averaging choice. Replacing the mean with the three-year median produces a current adverse coefficient of -0.525 and a one-year favorable coefficient of 0.417. Using 2006, 2007, or 2008 exposure separately yields current adverse coefficients between -0.537 and -0.472 and one-year favorable coefficients between 0.387 and 0.436. All four alternative constructions preserve the principal timing pattern. Table 6 reports the corresponding two-way clustered estimates.

Table 6. Robustness of the principal timing pattern

Specification	Current adverse coef.	Two-way SE	p	One-year favorable coef.	Two-way SE	p
Strict sample: baseline	-0.511	0.069	<0.001	0.410	0.102	0.001
Excluding 2020	-0.504	0.064	<0.001	0.375	0.098	0.002
Excluding 2014-2015	-0.515	0.087	<0.001	0.311	0.097	0.008
Country-specific linear trends	-0.511	0.087	<0.001	–	–	–
Expanded 24-country sample	-0.386	0.122	0.007	0.344	0.094	0.003

Specification	Current adverse coef.	Two-way SE	p	One-year favorable coef.	Two-way SE	p
Exposure: 2006-2008 median	-0.525	0.072	<0.001	0.417	0.107	0.002
Exposure: 2006 only	-0.472	0.061	<0.001	0.387	0.091	0.001
Exposure: 2007 only	-0.537	0.079	<0.001	0.436	0.109	0.002
Exposure: 2008 only	-0.521	0.069	<0.001	0.404	0.108	0.002
Leave-one-country-out range	[-0.552, -0.462]	–	max 0.009	[0.366, 0.448]	–	max 0.022

Notes: The expanded sample retains 24 countries but includes IMF historical mirrors for earlier fiscal observations in ten countries. The strict sample is therefore the preferred headline specification. Country-specific trends are included only in the current asymmetric equation. Alternative-exposure rows hold the strict sample and model fixed while replacing the 2006-2008 mean exposure with the median or one constituent pre-period year.

The robustness exercises address distinct threats. Episode exclusions test sensitivity to unusually large global movements. Country-specific trends address smooth differential fiscal trajectories. Leave-one-country-out estimates address influential economies. The expanded sample addresses source provenance and sample breadth. Alternative exposure definitions address sensitivity to the pre-period scaling choice. Across these exercises, the adverse impact remains negative and the principal timing pattern is preserved without selecting a new lag structure after observing the estimates.

7. Fiscal Rigidity as an Amplification Mechanism

The headline result establishes that adverse exposure-adjusted oil movements worsen fiscal balances on impact. It does not identify why some governments may find adjustment easier than others. A plausible mechanism is recurrent expenditure rigidity: if salaries absorb a large fraction of government expense and are difficult to reduce quickly, revenue losses may pass more strongly into deficits.

The mechanism test uses eight countries with consistently audited compensation and government-expense data and 100 country-year observations. Lagged compensation as a share of government expense averages 20.86 percent in this subsample. The variable is centered before the interactions are constructed.

Table 7 reports the estimates. The current adverse coefficient remains negative at -0.382. The interaction between adverse shocks and fiscal rigidity is +0.0183. This sign is opposite the simple amplification prediction, and the estimate is not statistically distinguishable from zero under two-way clustering or complete-enumeration eight-country wild-bootstrap inference. The complete-enumeration bootstrap p-value is 0.281. The favorable-shock interaction is also imprecise. The maximum VIF is 2.42, so the absence of an interaction is not explained by severe multicollinearity.

Table 7. Compensation-based fiscal-rigidity mechanism

Variable	Coefficient	Two-way SE	Two-way p	Complete-enumeration 8-country WCB p
Centered lagged fiscal rigidity	-0.009	0.233	0.969	–
Favorable movement, S^F	0.385	0.166	0.053	0.039
Adverse magnitude, S^A	-0.382	0.185	0.077	0.027
$S^F \times$ fiscal rigidity	0.0024	0.0060	0.698	0.688

Variable	Coefficient	Two-way SE	Two-way p	Complete-enumeration 8-country WCB p
$S^A \times$ fiscal rigidity	0.0183	0.0101	0.114	0.281

Notes: N=100, eight countries. Country and year fixed effects are included. Fiscal rigidity is compensation of employees divided by government expense, lagged one year and centered. Complete-enumeration wild-cluster bootstrap p-values enumerate all $2^8 = 256$ country sign patterns for the reported shock coefficients and interactions.

The null interaction should not be read as evidence that expenditure commitments are irrelevant. Compensation is only one component of fiscal rigidity, and the mechanism sample is small. The defensible conclusion is narrower: this compensation-share measure does not robustly explain why adverse oil exposure would propagate into a more negative headline fiscal-balance response. Other forms of rigidity may matter, but searching across alternative definitions until an interaction becomes significant would change the estimand after observing the data and is deliberately avoided.

8. Discussion: Acute Shocks versus Persistent Deficits

The empirical pattern draws a useful line between impact-year fiscal deterioration and persistence in the annual balance response. Exposure to a large adverse world oil-price movement is associated with a rapid and economically large weakening of the general-government balance. That relationship survives data-vintage restrictions, major-episode exclusions, trend controls, influence diagnostics, alternative exposure definitions, and inference procedures that respect the limited number of common annual price movements.

The local projections tell a different story after impact. The adverse coefficient collapses toward zero at one year and does not remain negative at horizons two and three. The appropriate interpretation is that the differential annual balance deterioration is no longer detectable after the impact year; it is not that the impact-year deficit is mechanically undone. If the initial gap is debt financed, the level of public debt can remain higher even when subsequent annual balances recover. Persistence in the flow therefore requires a propagation mechanism, while persistence in the stock is a separate question.

That distinction is useful in the broader analysis of long-term deficits. Structural deficits can reflect durable differences between revenue capacity and expenditure commitments, demographic pressures, rising interest costs, or institutional rules that make adjustment difficult. A temporary revenue shock can expose those weaknesses, but it is analytically separate from them. The oil-exporter setting makes the distinction visible because the external revenue disturbance is large and its exposure can be measured before the outcome period.

The compensation-rigidity test is informative in precisely this sense. It evaluates one candidate mechanism for propagation and does not find support for it. This does not resolve the question of which institutions create persistent deficits. It rules out a simple version of one story in this sample. Other mechanisms could matter: debt-service obligations, transfers, subsidies, public employment contracts not captured by the compensation ratio, sovereign financing access, stabilization funds, fiscal rules, or the political process through which temporary spending increases become permanent.

The favorable-shock response also deserves careful interpretation. Fiscal improvement appears more strongly one year after favorable oil movements than contemporaneously. This pattern is consistent with several mechanisms: revenue recognition and budget cycles, lagged saving, expenditure responses during

the boom year, or delayed rebuilding of fiscal buffers. The design does not distinguish among them, so the result should be treated as a timing fact rather than a structural behavioral parameter.

Several limitations follow directly from the design. First, Brent-price growth is not decomposed into structural oil supply and demand shocks, so source-specific structural interpretations are beyond the paper's scope. Second, the strict sample contains only 15 annual common price movements, limiting power at longer horizons even with 210 country-year observations. Third, oil rents as a share of GDP are an exposure proxy rather than a direct measure of government oil revenue; fiscal regimes differ in how extraction rents translate into public revenue. Fourth, the 24-country audit is the analytical sample assembled for this draft rather than an exhaustive universe of oil-producing economies. Fifth, the outcome is the headline general-government balance, not its revenue and spending components, financing flows, or the debt-to-GDP stock. Those decompositions and stock outcomes would be needed to identify the accounting channel and cumulative debt consequences in detail.

These limits are also why the paper avoids policy prescriptions. The estimates establish a reduced-form timing pattern in annual fiscal balances; they do not identify a universal institutional remedy, a structural oil-shock elasticity, or a debt-sustainability rule. Questions about how persistent deficits should be reduced require additional evidence on taxes, spending composition, financing, debt dynamics, and welfare trade-offs that lies outside this design.

9. Conclusion

This paper asks whether a large externally generated fiscal deterioration remains visible in subsequent annual fiscal balances. The setting is a panel of oil-exposed economies in which a common global price movement has different fiscal relevance because countries entered the period with very different oil dependence.

Using 14 countries with fully verified April 2026 IMF WEO fiscal histories over 2009-2023, the analysis interacts annual Brent-price growth with average 2006-2008 oil rents as a share of GDP. The cross-country exposure component is fixed before the estimation window. Country and year fixed effects isolate differential responses to the common movement, while two-way clustering and complete-enumeration wild-cluster bootstrap procedures align inference with the limited number of independent annual shocks.

The principal result is a large contemporaneous deterioration in fiscal balances following adverse exposure-adjusted oil movements. The preferred coefficient is -0.511, and it remains stable when 2020 is removed, when 2014-2015 is removed, when country-specific trends are added, when individual countries are omitted, when the source sample is expanded, and when pre-period exposure is reconstructed using the median or any individual 2006-2008 base year. Complete-enumeration year-cluster bootstrap inference continues to reject a zero impact effect in the strict sample.

The persistence evidence is different. The adverse annual-balance response is essentially absent one year later. At horizons two and three, the estimates are positive rather than negative, although longer-horizon inference is weaker because the number of shock years declines. The data therefore do not support a persistently negative fiscal-balance response generated by the initial oil-price exposure movement alone. Favorable movements have little contemporaneous effect but are associated with stronger balances one year later.

A secondary mechanism test finds no robust evidence that compensation-based expenditure rigidity amplifies the adverse response. The result is useful precisely because it separates a strong impact relationship from a mechanism that the data do not support.

The broader implication is empirical rather than prescriptive. Large external oil-price exposure can create substantial current deficits, but an acute deficit does not establish a persistently negative annual balance response. Nor does recovery in the annual balance imply that the debt accumulated during the impact year has disappeared. Understanding long-term fiscal trajectories therefore requires separate identification of the mechanisms that propagate flow deterioration and the mechanisms that govern debt accumulation and adjustment. In this sample, the impact response is strong; persistence of the negative annual balance response is not.

Data and Replication Statement

The fiscal-balance data are from the IMF World Economic Outlook database, April 2026 vintage, supplemented only in the expanded source-sensitivity panel by documented IMF historical mirrors. Pre-period oil exposure is constructed from the World Bank World Development Indicators oil-rents series (NY.GDP.PETR.RT.ZS). Brent prices are from the IMF Primary Commodity Prices series distributed through FRED. The strict-vintage estimation file contains no interpolated fiscal-balance observations. Three transformed analysis files accompany the draft: nber_interim_panel.csv for the strict 14-country panel, nber_deficit_stage2_panel.csv for the 24-country source-sensitivity panel, and nber_deficit_m3_mechanism_panel.csv for the eight-country rigidity exercise. A replication script accompanies these files and reproduces the headline fixed-effect estimates, two-way clustered standard errors, complete-enumeration country- and year-cluster wild-bootstrap tests for the principal coefficients, local projections, source-sensitivity estimates, alternative exposure constructions, and the compensation-rigidity regression.

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Appendix A. Strict-Vintage Headline Sample

The headline sample is defined by source provenance: each country must have a complete 2009-2023 fiscal-balance history verified within the April 2026 WEO vintage and non-missing 2006-2008 oil-rent exposure. Table A1 lists the 14 headline countries. The expanded source audit contains ten additional countries whose 2009-2023 histories require IMF historical mirrors for at least part of the period. Table A2 reports the full 24-country audit and its provenance categories. Sample-tier assignment is therefore based on data source and completeness rather than the estimated sign or significance of the oil-exposure coefficient.

Table A1. Countries in the strict-vintage headline sample

Country	ISO3	Mean oil rents, 2006-2008 (% GDP)	Fiscal-balance observations	Fiscal source status
Algeria	DZA	29.96	15	Full April 2026 WEO history verified
Azerbaijan	AZE	37.73	15	Full April 2026 WEO history verified
Bahrain	BHR	3.94	15	Full April 2026 WEO history verified
Brazil	BRA	2.34	15	Full April 2026 WEO history verified
Colombia	COL	5.01	15	Full April 2026 WEO history verified
Iraq	IRQ	57.71	15	Full April 2026 WEO history verified
Malaysia	MYS	6.89	15	Full April 2026 WEO history verified
Mexico	MEX	5.43	15	Full April 2026 WEO history verified
Nigeria	NGA	15.81	15	Full April 2026 WEO history verified
Oman	OMN	39.38	15	Full April 2026 WEO history verified
Papua New Guinea	PNG	8.53	15	Full April 2026 WEO history verified
Russian Federation	RUS	11.56	15	Full April 2026 WEO history verified
Saudi Arabia	SAU	51.11	15	Full April 2026 WEO history verified
United Arab Emirates	ARE	25.10	15	Full April 2026 WEO history verified

Notes: All countries have 15 annual fiscal-balance observations for 2009-2023. Exposure is fixed before the estimation period.

Table A2. Country provenance in the expanded 24-country source audit

Country	ISO3	Mean oil rents, 2006-2008 (% GDP)	Fiscal-balance observations	Fiscal source status
Algeria	DZA	29.96	15	Full Apr-2026 WEO

Country	ISO3	Mean oil rents, 2006-2008 (% GDP)	Fiscal-balance observations	Fiscal source status
Angola	AGO	48.06	15	Apr-2026 (2018-23) + IMF mirror (2009-17)
Azerbaijan	AZE	37.73	15	Full Apr-2026 WEO
Bahrain	BHR	3.94	15	Full Apr-2026 WEO
Brazil	BRA	2.34	15	Full Apr-2026 WEO
Cameroon	CMR	7.30	15	Apr-2026 (2018-23) + IMF mirror (2009-17)
Colombia	COL	5.01	15	Full Apr-2026 WEO
Ecuador	ECU	17.88	15	Apr-2026 (2018-23) + IMF mirror (2009-17)
Equatorial Guinea	GNQ	48.94	15	Apr-2026 (2018-23) + IMF mirror (2009-17)
Gabon	GAB	37.99	15	Apr-2026 (2018-23) + IMF mirror (2009-17)
Iraq	IRQ	57.71	15	Full Apr-2026 WEO
Kazakhstan	KAZ	20.40	15	Apr-2026 (2018-23) + IMF mirror (2009-17)
Kuwait	KWT	51.71	15	Apr-2026 recent + WEO historical mirror
Malaysia	MYS	6.89	15	Full Apr-2026 WEO
Mexico	MEX	5.43	15	Full Apr-2026 WEO
Nigeria	NGA	15.81	15	Full Apr-2026 WEO
Oman	OMN	39.38	15	Full Apr-2026 WEO
Papua New Guinea	PNG	8.53	15	Full Apr-2026 WEO
Qatar	QAT	32.15	15	Apr-2026 recent + WEO historical mirror
Republic of Congo	COG	51.53	15	Apr-2026 (2018-23) + IMF mirror (2009-17)
Russian Federation	RUS	11.56	15	Full Apr-2026 WEO
Saudi Arabia	SAU	51.11	15	Full Apr-2026 WEO
Trinidad and Tobago	TTO	10.37	15	Apr-2026 (2018-23) + IMF mirror (2009-17)
United Arab Emirates	ARE	25.10	15	Full Apr-2026 WEO

Notes: All countries have 15 annual fiscal-balance observations for 2009-2023 and non-missing 2006-2008 oil-rent exposure. "Full Apr-2026 WEO" identifies the 14-country headline tier. The other labels indicate use of IMF historical mirrors for part of the 2009-2023 history and those countries enter only the source-sensitivity exercise. The expanded audit is the analytical set assembled for this draft and is not presented as an exhaustive universe of oil-producing economies.

Appendix B. Additional Diagnostics

B.1 Correlation and multicollinearity

The asymmetric impact variables have low variance-inflation factors. VIF(S^F)=1.113 and VIF(S^A)=1.113. This is well below conventional thresholds and confirms that the interaction-free asymmetric specification is not hindered by multicollinearity.

Table B1. Collinearity diagnostics for the asymmetric impact variables

Variable	Variance-inflation factor (VIF)
Favorable movement, S+	1.113
Adverse magnitude, S−	1.113

Notes: The diagnostic applies to the asymmetric impact specification. Values reproduce the VIFs reported in Appendix B.1; both are well below conventional thresholds for problematic multicollinearity.

B.2 Formal symmetry tests

In the strict-vintage impact model, the null $\beta_F + \beta_A = 0$ yields an estimated restriction of -0.408. The country-clustered p-value is 0.0055 and the two-way clustered p-value is 0.0361. However, the complete-enumeration year-cluster bootstrap p-value is 0.156. The paper therefore treats the same-period magnitude asymmetry as suggestive under conventional inference but not robust under the most conservative common-shock inference.

For the one-year responses, the analogous restriction has a two-way clustered p-value of 0.290 and a complete-enumeration year-cluster bootstrap p-value of 0.362. The paper therefore does not claim a formally established lagged asymmetry even though the favorable coefficient is individually positive and the adverse coefficient is near zero.

Table B2. Formal symmetry tests

Response	Favorable coef.	Adverse coef.	Restriction estimate	Country-cluster p	Two-way p	Year WCB p
Impact (h=0)	0.103	−0.511	−0.408	0.0055	0.0361	0.156
One-year (h=1)	0.410	−0.046	0.364	–	0.290	0.362

Notes: Because S− is coded as the positive magnitude of an adverse movement, same-magnitude symmetry implies β+ + β− = 0. The country-cluster p-value for the one-year restriction is not separately reported in the manuscript. WCB denotes complete-enumeration wild-cluster bootstrap over year clusters.

B.3 Expanded-sample provenance

The 24-country analytical panel is used only as a source-sensitivity sample because ten countries rely on IMF historical mirrors for part of the 2009-2023 fiscal history. Table A2 makes that provenance explicit. The expanded estimates preserve the principal timing pattern, but complete-enumeration year-cluster inference is weaker. Keeping the 14-country fully verified panel as the headline sample therefore makes both the data hierarchy and the inferential hierarchy transparent.

Table B3. Expanded-sample fiscal-data provenance summary

Fiscal-data provenance	Countries	Observations	Analytical role
Full April 2026 WEO history verified	14	210	Headline sample
April 2026 (2018–2023) + IMF historical mirror (2009–2017)	8	120	Robustness only
April 2026 recent + current WEO historical mirror	2	30	Robustness only
Expanded total	24	360	Source-sensitivity sample

Notes: Counts are taken from the expanded analytical panel. Country-level provenance is reported in Table A2. The headline estimates use only the 14 countries with fully verified April 2026 WEO histories.

B.4 Replication and exposure-construction check

The accompanying replication script reconstructs the headline coefficient (-0.511), the complete-enumeration year-cluster wild-bootstrap p-value (0.0228), and the one-year favorable coefficient (0.410) directly from the supplied transformed files. Replacing mean 2006-2008 exposure with the three-year median or any constituent pre-period year leaves the current adverse estimate between -0.537 and -0.472 and the one-year favorable estimate between 0.387 and 0.436. This check addresses sensitivity to the particular averaging rule used to fix pre-period exposure.

Table B4. Replication and alternative exposure-construction checks

Exposure definition	Current adverse coef.	Two-way SE	p	One-year favorable coef.	Two-way SE	p
Mean, 2006–2008	−0.511	0.069	<0.001	0.410	0.102	0.0015
Median, 2006–2008	−0.525	0.072	<0.001	0.417	0.107	0.0019
2006 only	−0.472	0.061	<0.001	0.387	0.091	0.0010
2007 only	−0.537	0.079	<0.001	0.436	0.109	0.0015
2008 only	−0.521	0.069	<0.001	0.404	0.108	0.0025

Notes: All specifications use the strict 14-country sample and the same country and year fixed-effects structure. The table verifies that the principal timing pattern is not sensitive to using the 2006–2008 mean, the corresponding median, or any single pre-period year as the predetermined exposure measure.