Debt payoffs from fiscal consolidations: can fiscal rules make a difference?*

Martín Ardanaz Kenji Moreno Jorge Puig

Abstract

This paper examines the role of fiscal rules in shaping the response of public debt to fiscal consolidations. Using local projections and panel data for 140 countries, we identify consolidation episodes using fiscal balance forecast errors. Results show that consolidations are followed by a gradual decline in the debt-to-GDP ratio on average. However, this decline is substantially larger when a fiscal rule is in place: consolidations outside a fiscal rule framework generate small and imprecise debt reductions, whereas those undertaken in the presence of fiscal rules lower debt by 2 to 2.5 percentage points of GDP at medium horizons. Finally, the quality of fiscal rule design matters: consolidations accompanied by low-quality rules are ineffective in reducing debt ratios, while strong or high-quality rules generate large and persistent debt declines, reaching between 5 to 6 percentage points of GDP over the medium term. The findings underscore the relevance of rules-based fiscal frameworks to support the implementation of credible fiscal consolidation strategies.

JEL Classification: E62; H61; H63.
Keywords: fiscal consolidation; public debt; fiscal rules

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1 Introduction

High levels of public debt and fiscal sustainability concerns have returned to the forefront of macroeconomic policy making. Against the backdrop of multiple shocks such as the COVID-19 pandemic, the energy crisis, and the rise in global interest rates that followed the inflation surge, governments around the world face increasing pressure to rebuild fiscal space while preserving economic growth. In the current high debt environment, fiscal consolidation is one of the policy options available to policymakers to address the challenge of restoring public finances (IMF, 2026; Kose *et al.*, 2022). However, previous research shows that fiscal adjustment efforts do not always succeed in reducing or stabilizing debt, in part because policy reversals—such as a shift to loose fiscal policy after initial tightening—are relatively common (Alesina *et al.*, 2015; Mauro & Villafuerte, 2013). What institutional arrangements can make fiscal consolidations more effective in controlling public debt over time?

This paper examines the extent to which fiscal rules condition the impact of consolidation efforts on public debt dynamics. Fiscal rules impose long-lasting constraints on fiscal policy by introducing numerical limits on budgetary aggregates (Kopits & Symansky, 1998). Fiscal consolidations are multi-year attempts by governments to reduce budget deficits and hence improve debt sustainability. Given their intertemporal nature, expectations play a central role in determining the consequences of consolidation (Bi *et al.*, 2013; Alesina *et al.*, 2019). In this context, fiscal rules have the potential to anchor expectations about future fiscal policy (Hatchondo *et al.*, 2022). In particular, fiscal rules may increase the credibility of fiscal consolidation plans by making the initial adjustment more persistent or difficult to reverse over time. However, rules may also be weakly enforced or poorly designed, undermining the sustainability of consolidation efforts (Alesina & Perotti, 1996; Wyplosz, 2012). Thus, whether rules help fiscal consolidations reduce debt over the medium term remains an open empirical question.

We identify fiscal consolidations by exploiting differences between projected and actual fiscal balances, following the forecast-error approach of Auerbach & Gorodnichenko (2012) and Blanchard & Leigh (2013). We then estimate the dynamic response of public debt to fiscal adjustments using state-dependent local projections (Cloyne *et al.*, 2023; Jordà, 2005). The analysis introduces fiscal rules as an institutional state variable and asks whether the debt response to adjustments differs when a fiscal rule is in place. To address the potential endogeneity of fiscal rule adoption, we complement OLS estimates with an IV strategy that instruments a country's rule adoption with adoption in neighboring countries, motivated by spatial-diffusion channels through which institutional and policy reforms tend to spread (Caselli & Reynaud, 2020; Giuliano *et al.*, 2013). Finally, we examine whether the quality

of rule design matters in conditioning the impact of fiscal consolidations on public debt.

The analysis yields three sets of results. First, fiscal consolidations are followed by a delayed decline in public debt on average. The debt-to-GDP ratio falls moderately on impact, with debt declines above 1 percentage point at medium horizons. Second, fiscal rules amplify the response of public debt to fiscal adjustments. While consolidations outside a fiscal rule framework generate small and imprecise debt reductions, consolidations under fiscal rules lower debt by 2 to 2.5 percentage points of GDP at medium horizons. Third, the quality of fiscal rules is especially important: consolidations accompanied by low-quality rules are associated with modest and imprecisely estimated debt responses, whereas strong or high-quality rules generate large and persistent debt declines, reaching between 5 to 6 percentage points of GDP over the medium term. Taken together, the results suggest that the institutional setting of a fiscal consolidation is a key determinant of its success in lowering public debt.

The paper contributes to two strands of literature. The first examines the outcomes of fiscal consolidations and their determinants. Previous research mostly focuses on the macroeconomic consequences of fiscal adjustments. This literature has made important progress in the identification of fiscal adjustment shocks over the last decade (see Ramey (2019) for a review). While early studies typically relied on measures such as the improvement in the cyclically adjusted fiscal balance to define a fiscal adjustment period (Alesina & Ardagna, 2010, 2013), more recent work draws on the narrative approach or forecast errors to identify exogenous fiscal policy actions (Auerbach & Gorodnichenko, 2013; Guajardo *et al.*, 2014; Jordà & Taylor, 2016; Alesina *et al.*, 2019; Beetsma *et al.*, 2021). We extend this line of work by focusing on the effects of fiscal consolidations on public debt dynamics, the objective that motivates adjustments in the first place.

Prior work also examines the determinants of *successful* fiscal consolidations, that is, the degree to which adjustment efforts translate into more or less permanent improvements in the country's fiscal position (von Hagen & Strauch, 2001). For instance, the design or composition of fiscal adjustment has been found to be a key determinant of success: consolidations based mostly on spending cuts tend to be more persistent than those based mostly on tax increases (Alesina & Perotti, 1997; Alesina & Ardagna, 2013). We contribute to this literature by focusing on the medium-term debt payoffs of fiscal consolidations. This margin is still empirically unsettled: while Ando *et al.* (2025) find that the typical fiscal adjustment may have limited effects on debt ratios, evidence in Frangiamore *et al.* (2025) suggests that some types of consolidations can be effective at lowering debt at particular points in the debt distribution.

We also contribute to a large literature on the effectiveness of fiscal rules. Existing empirical work typically explores the *direct* effect of rule presence or rule features on various outcomes of interest: whether rules improve fiscal balances, debt, or the cyclical behavior of fiscal policy.[1] For instance, Acalin *et al.* (2025) find that certain characteristics of rules, such as robust correction mechanisms, can reduce fiscal risks reflected in sovereign spreads, while Guerguil *et al.* (2017) show that flexibility provisions can reduce procyclical policy biases. Rather than focusing on the direct impact of rules, we ask how rule presence and quality condition the response of debt-to-GDP ratios to fiscal adjustments.

Recent research on the relationship between fiscal rules and fiscal consolidation outcomes has been limited in scope, often focusing on a narrow set of countries or on specific types of fiscal adjustment. For example, Chrysanthakopoulos & Tagkalakis (2023) examine the role of different rule characteristics and the presence of fiscal councils in the probability of initiating successful fiscal adjustments in 40 advanced countries. While Frangiamore *et al.* (2025) analyze the impact of consolidations on public debt on a broad sample of countries, their focus is on a subset of consolidation episodes (e.g. expenditure shocks) that are used to study the effects of adjustments on the debt distribution. They find expenditure shocks to be especially effective in reducing debt-at-risk (the 95th percentile of the debt distribution).[2] We extend this literature in various ways. First, we study fiscal consolidation episodes in general rather than expenditure consolidations alone. Second, we focus on a broad sample of countries at different levels of economic development. Finally, we place fiscal rules at the center of the analysis. Leveraging heterogeneity in rule adoption and quality, we assess the extent to which rule-based fiscal frameworks shape the medium-term debt consequences of fiscal adjustment episodes.

The rest of the paper is organized as follows. Section 2 presents the empirical strategy. Section 3 provides key variable definitions, and descriptive evidence. Section 4 presents the main results, robustness exercises, and heterogeneity analysis. Section 5 concludes.

[1] See Potrafke (2025) for a recent overview and Heinemann *et al.* (2018) for a meta-analysis of the empirical literature.

[2] The authors explore potential non-linearities in the relationship between consolidations and debt and consider fiscal rules among several factors.

2 Empirical strategy

2.1 Public debt response to fiscal consolidations

The impact of fiscal consolidations on debt is estimated using the single-equation approach of Jordà (2005). We estimate a separate regression for each horizon $h = 0, \ldots, 5$. Let $d_{i,t}$ denote the gross public debt-to-GDP ratio and define $D^h_{i,t} \equiv d_{i,t+h} - d_{i,t-1}$. The dependent variable is therefore the cumulative change in the debt ratio between the year preceding the consolidation and horizon h. The variable $FC_{i,t}$ represents the size of the fiscal consolidation shock, expressed in percentage points of GDP and set equal to zero outside consolidation episodes. Writing $\widetilde{X}_{i,t} \equiv X_{i,t} - \overline{X}_i$ for the vector of centered predetermined states, we estimate Equation (1) as follows:

$$D^h_{i,t} = \alpha^h_i + \lambda^h_t + \beta_h FC_{i,t} + \widetilde{X}'_{i,t}\boldsymbol{\gamma}_h + FC_{i,t}\widetilde{X}'_{i,t}\theta_h + \varepsilon^h_{i,t+h}. \tag{1}$$

Following Cloyne *et al.* (2020, 2023), each centered state-variable enters both directly and interacted with the contemporaneous shock $FC_{i,t}$. This makes β_h the cumulative debt response to a one-percentage-point-of-GDP consolidation evaluated at the country's average predetermined state. Initially, a parsimonious state vector includes two lags of $D_{i,t}$, $FC_{i,t}$, and the debt-to-GDP ratio. A fully controlled specification then adds lags of the change in the primary balance, inflation, real GDP growth, and the change in the implicit interest rate on public debt. Country fixed effects, α^h_i, absorb persistent cross-country differences, while year fixed effects, λ^h_t, absorb unrestricted shocks common to all countries.

Identification of fiscal consolidations. Observed changes in fiscal balances are not suitable treatments by themselves because they combine anticipated policy choices, automatic responses to the business cycle, and genuinely unexpected fiscal actions. The forecast-error (FE) approach isolates the last component by comparing the realized or actual change in the fiscal balance with the forecasted value. In particular, by drawing on historical World Economic Outlook (WEO) vintages, we first construct the unanticipated fiscal variable shock (FE^{FB}), as the difference between the change in the actual ($FB^A_{i,t}$) overall fiscal balance, expressed as percentage of GDP and calculated based on the October WEO for the following year, and the change in the forecast value ($FB^F_{i,t}$), calculated based on the October WEO for the current year: $FE^{FB}_{i,t} = \Delta FB^A_{i,t} - \Delta FB^F_{i,t}$.

Following the forecast error literature, we residualize this forecast error with respect to real GDP growth and inflation forecast errors and lagged macro-fiscal variables. Given that a positive forecast error implies fiscal policy is contractionary, we define $FC_{i,t}$ as an improve-

ment above 0.5 percentage points in the fiscal shock measure to identify fiscal policy changes. In particular, if $\widehat{\eta}_{i,t}$ denotes the resulting residual, the treatment is $FC_{i,t} = \widehat{\eta}_{i,t}\,\mathbf{1}\{\widehat{\eta}_{i,t} > 0.5\}$. The simple difference between the actual balance and its forecast removes the component of fiscal policy that was predictable using information available when the forecast was made; residualizing the variable allows us to take into account any changes due to surprises in the business cycle and the part of the shock that can be predicted from past observations. This logic, following Auerbach & Gorodnichenko (2012) and Blanchard & Leigh (2013), makes the conditional exogeneity assumption $\mathbb{E}\left[FC_{i,t}\varepsilon^{h}_{i,t+h} \mid \alpha^{h}_{i}, \lambda^{h}_{t}, X_{i,t}\right] = 0$ substantively more plausible than it would be for observed fiscal-balance changes. Section 3 describes the construction and timing of the forecast-error measure in detail.

Equation (1) is estimated by OLS. Standard errors are clustered by country. Macroeconomic and fiscal variables come from historical vintages of the IMF WEO database. In addition to public debt data and forecast errors, the WEO database also provides the overall and primary balances, real GDP, inflation, and the inputs required to construct the implicit interest rate on public debt. The sample is an unbalanced panel of 140 countries over 1990–2019. The Appendix lists the 140 countries in that sample, and Appendix Table A1 reports summary statistics.

2.2 Fiscal rule adoption

To allow the debt response to depend on whether a fiscal rule is in place at the time of the fiscal adjustment, we extend Equation (1) as follows:

$$\begin{aligned} D^{h}_{i,t} = \alpha^{h}_{i} + \lambda^{h}_{t} + \beta_h FC_{i,t} + \delta_h FR_{i,t} + \phi_h \left(FC_{i,t} \times FR_{i,t}\right) \\ + \widetilde{X}'_{i,t}\gamma_h + FC_{i,t}\widetilde{X}'_{i,t}\theta_h + \varepsilon^{h}_{i,t+h}, \end{aligned} \tag{2}$$

where $FR_{i,t}$ equals one if a fiscal rule is active, as recorded in the IMF Fiscal Rules Dataset (Alonso *et al.*, 2025). The debt response in the absence of a rule is β_h, the response under an active rule is $\beta_h + \phi_h$, and ϕ_h is the rule–no-rule differential. Standard errors and confidence intervals for reported response combinations and contrasts are computed using the delta method and the full estimated variance–covariance matrix.

Rule adoption may be correlated with unobserved determinants of debt dynamics. We therefore complement OLS with an IV specification leveraging rule adoption in neighboring countries (Caselli & Reynaud, 2020; Ardanaz *et al.*, 2021). Let $Z_{i,t}$ be the contemporaneous share of country i's land-border neighbors that operate a fiscal rule, excluding country i

itself.[3] We treat $FR_{i,t}$ and $FC_{i,t} \times FR_{i,t}$ as endogenous and instrument them with $Z_{i,t}$ and $FC_{i,t} \times Z_{i,t}$, respectively. Relevance requires neighboring rule adoption to predict domestic rule status and the associated consolidation interaction. The exclusion restriction requires that, conditional on country and year fixed effects and the common predetermined control set, neighboring-country rule exposure affect the domestic debt response only through domestic rule status. The Appendix reports conventional first-stage and underidentification diagnostics, weak-identification-robust inference, and a broad set of falsification and robustness exercises aimed at this assumption.[4]

2.3 Fiscal rule quality

Our third specification examines whether the quality or strength of the fiscal rule framework further conditions the debt payoff from consolidations. We summarize fiscal rule strength with a time-invariant country state. Let Q_i denote country i's average Fiscal Rules Strength Index (FRSI), computed over the years in which a fiscal rule is active. The FRSI is constructed following the criteria in Alonso-Albarran *et al.* (2025).[5] Appendix Table A9 provides a mapping of the indicators, scoring rules, and data sources used to construct the aggregate FRSI along each of its four constituent dimensions: (i) the legal basis of fiscal rules; (ii) the monitoring framework; (iii) enforcement procedures and presence of correction mechanisms; and (iv) flexibility provisions in response to shocks.

Using the country average rather than contemporaneous quality reduces the risk that current debt changes mechanically alter the measured institutional state. Define the active-quality state as $S_{i,t} \equiv FR_{i,t} \times Q_i$. Hence, $S_{i,t} = 0$ when no rule is in force and equals the country's average rule quality during active-rule years. We estimate the following specification:

$$\begin{aligned} D^h_{i,t} = \alpha^h_i + \lambda^h_t + \beta_h FC_{i,t} + \delta_h FR_{i,t} + \eta_h S_{i,t} \\ + \phi_h \left(FC_{i,t} \times FR_{i,t}\right) + \omega_h \left(FC_{i,t} \times S_{i,t}\right) \\ + \widetilde{X}'_{i,t}\gamma_h + FC_{i,t}\widetilde{X}'_{i,t}\theta_h + \varepsilon^h_{i,t+h}. \end{aligned} \tag{3}$$

[3]The border instrument is constructed from the share of land neighbors with an active rule using the public GeoDataSource Country Borders database; the own country is excluded by construction.

[4]Given that the instrument is available only for countries with usable land-border exposure, the IV estimates use a fixed 124-country sample. To distinguish changes in estimation from changes in sample composition, we re-estimate Equation (2) by OLS on exactly the IV estimation support and compare results with the baseline 140-country sample.

[5]Rule-quality measures combine IMF Fiscal Rules Dataset with the IMF Fiscal Council Dataset.

Including the lower-order term $S_{i,t}$ ensures that the interaction $FC_{i,t} \times S_{i,t}$ does not proxy for a direct association between active rule quality and debt dynamics. A separate level effect of Q_i is absorbed by country fixed effects. Unlike rule status, Q_i is not instrumented. We therefore interpret the quality estimates as OLS heterogeneity, not as the causal effect of redesigning a rule.

Equation (3) implies a consolidation response of β_h when no rule is in force and $\beta_h + \phi_h + \omega_h q$ under an active rule of quality q. The difference between high and low-quality rules is therefore $\omega_h(q_H - q_L)$. We evaluate the response at selected percentiles of the country-level overall quality distribution and at its four components: legal basis, monitoring, enforcement, and flexibility. Standard errors and confidence intervals for these evaluated responses, quality components, and percentile contrasts are computed by the delta method using the full estimated variance–covariance matrix.

3 Descriptive evidence

3.1 Fiscal consolidations

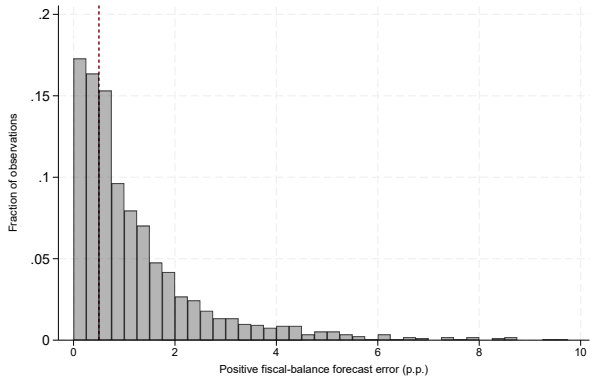
The fiscal consolidation variable $FC_{i,t}$ is constructed from the October WEO forecast error in the annual change of the overall fiscal balance. Based on the simple difference between actual and forecasted balances, we refine the forecat error by regressing this variable on real-GDP-growth and inflation forecast errors and lagged macro-fiscal predictors. Appendix Figure A1 compares the simple and refined forecast-error measures: the cleaning recenters the shock but leaves its overall shape largely unchanged. A fiscal consolidation is an observation for which the refined forecast error exceeds 0.5 percentage points of GDP, and $FC_{i,t}$ equals that error in those observations and zero otherwise.[6]

Figure 1 describes the magnitude and timing of the resulting consolidations. Panel A restricts attention to the 1,724 positive forecast errors and marks the 0.5 threshold. Of these observations, 1,144 exceed the threshold and are classified as fiscal consolidations. Conditional on being identified as a consolidation, the mean shock is 1.72 percentage points of GDP and the median is 1.26, as reported in Appendix Table A1. The right-skewed histogram indicates that most consolidations are moderate, with a progressively thinner tail of large adjustments. Panel B shows that the identified consolidations span the sample period rather than being concentrated in particular years.

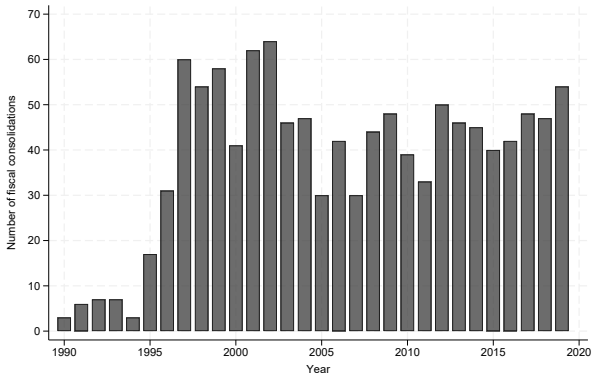
[6]For robustness purposes, the empirical analysis that follows works with alternative thresholds to define the occurrence of a fiscal consolidation.

Figure 1: Fiscal consolidation shocks: distribution and timing

Panel A. Positive forecast errors



Panel B. Fiscal consolidations over time



Source: Authors' elaboration. *Notes:* The figure uses October WEO forecast errors in the sample over 1990–2019. Panel A plots positive fiscal-balance forecast errors; the vertical line marks the 0.5-percentage-point threshold used to identify a fiscal consolidation. Panel B reports the number of country-year observations above that threshold in each year.

Consecutive consolidation years within a country can also be grouped into episodes. The 1,144 consolidation years form 711 distinct episodes: 440 last one year, 165 last two years, and 106 last three years or more. Thus, the typical episode is short—mean duration is 1.61 years and median duration is one year. Appendix Figure A2 reports the distributions of cumulative episode magnitude and duration.

3.2 Fiscal rule presence and design quality

We define $FR_{i,t}$ with a dummy variable equal to one whenever a national fiscal rule is active. Table 1 connects this institutional classification to the fiscal consolidations. Of the 1,144 consolidation years, 440 occur under an active rule and 704 without one. At the episode level, 294 of the 711 episodes coincide with at least one fiscal rule in place.[7] While consolidations without a rule are somewhat larger on average, the starting debt-to-GDP ratio in both scenarios is relatively similar.

[7]Fifteen multi-year episodes cross a rule-adoption date, so the no-rule and any-rule episode counts are not mutually exclusive.

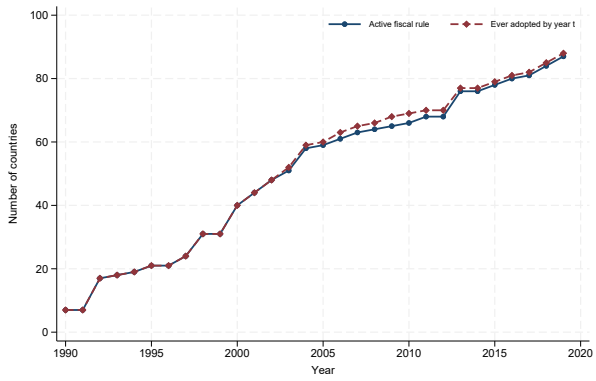
Table 1: Fiscal consolidations across fiscal rule environments

Environment	Consolidation years	Distinct episodes	Episode share (%)	Mean shock	Median shock	Initial debt
All consolidations	1,144	711	100.00	1.72	1.26	50.89
No fiscal rule	704	432	60.76	1.92	1.38	49.64
Any fiscal rule	440	294	41.35	1.41	1.11	52.71

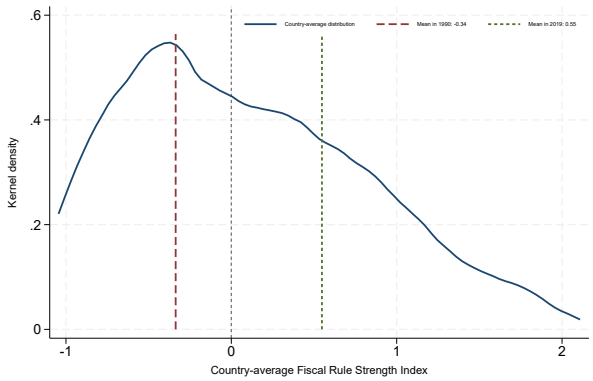
Source: Authors' elaboration. *Notes:* The table uses the 140-country sample during 1990–2019. A distinct episode is a sequence of consecutive consolidation years within a country and is assigned to an environment if at least one of its years occurs in that environment. The no-rule and any-rule rows overlap for 15 episodes that cross a rule-adoption date. Episode shares use 711 distinct episodes as the denominator and therefore need not sum to 100 percent. Mean and median shocks are annual consolidation magnitudes in percentage points of GDP; initial debt is measured at $t-1$.

Figure 2: Fiscal rule adoption and design quality

Panel A. Fiscal rule adoption over time



Panel B. Country-average rule quality



Source: Authors' elaboration. *Notes:* Panel A uses the IMF Fiscal Rules Dataset and reports, within the broad sample, the number of countries with an active rule and the cumulative number that had adopted a rule by each year. Panel B combines the IMF Fiscal Rules and Fiscal Council datasets and replicates the aggregation in Alonso-Albarran *et al.* (2025). The density contains one country-average FRSI observation for each of the 85 countries on the usable active-rule quality support. The black vertical line marks zero; the red and green lines mark the mean annual FRSI among active rules in this support in 1990 (−0.34, seven countries) and 2019 (0.55, 84 countries), respectively.

Panel A in Figure 2 shows fiscal rules have become an increasingly popular policy tool, with more than 80 countries adopting at least one national rule by the end of the sample period. Fiscal rule frameworks also differ markedly in their design quality or strength. Following the criteria in Alonso-Albarran *et al.* (2025), we generate an annual standardized FRSI score and average it within country over active-rule years, $Q_i = (T_i^R)^{-1} \sum_{t:FR_{i,t}=1} Q_{i,t}$. Panel B, in Figure 2, gives equal weight to each of the 85 countries on the active-rule estimation support.[8] The distribution is dispersed and moderately right-skewed: the standardized index

[8]Within our sample, 92 countries operated a rule at some point according to the IMF database, but four

mean is 0.09, its median is −0.02, and its 10th and 90th percentiles are −0.78 and 1.04. The average index rises from −0.34 in 1990 to 0.55 in 2019. The quality analysis therefore draws on substantial cross-country variation and on institutional improvement over time.

4 Results

4.1 Public debt response to fiscal consolidation

Figure 3 reports the cumulative response of public debt to a one-percentage-point-of-GDP fiscal consolidation. The parsimonious and fully controlled specifications yield similar debt paths. The parsimonious estimate reaches 0.86 percentage points at $h = 3$, 1.17 at $h = 4$, and 0.95 at $h = 5$. Adding predetermined fiscal and macroeconomic states therefore slightly moderates the response at longer horizons without changing its timing or qualitative interpretation. Appendix Table A2 reports the underlying coefficients and corresponding standard errors.

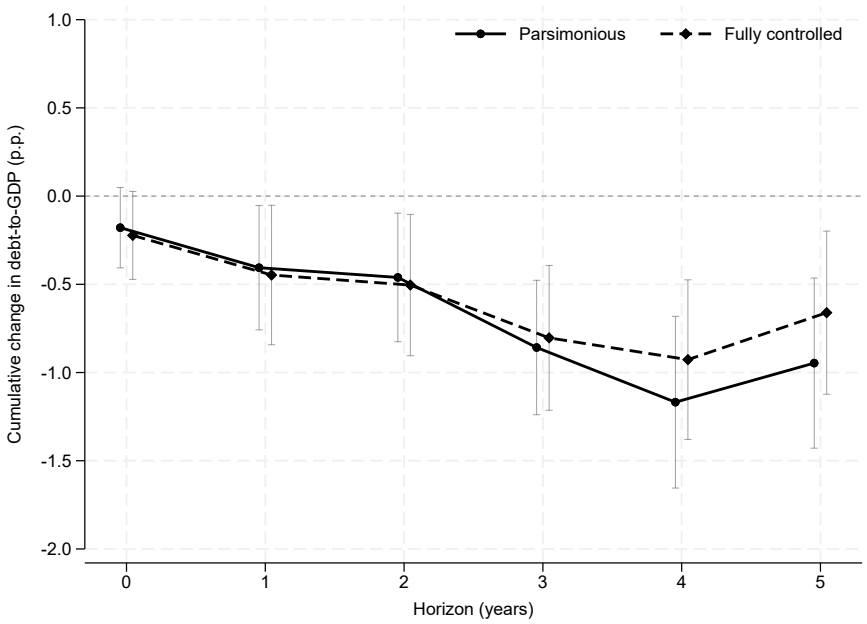
The estimates support a gradual debt payoff on average: the reduction is not concentrated in the year in which the consolidation occurs, but accumulates over the following three to five years. This timing is consistent with debt stock–flow arithmetic. A consolidation improves a fiscal flow, whereas the debt ratio reflects the cumulative primary balance together with inherited interest costs, nominal growth, and stock–flow adjustments; the effect of a persistent fiscal improvement can therefore build over several years rather than appear fully on impact (Escolano, 2010). The dynamic pattern is also broadly in line with Frangiamore *et al.* (2025), who study expenditure-based consolidations in a large cross-country sample and find that their effects on the debt distribution build over subsequent years.

Figure 4 examines the robustness of the average response of debt. Panel A replaces the debt ratio with cumulative real public-debt growth. The real debt stock is between 2.3 and 2.8 percent lower at horizons three through five. The decline is statistically significant at the 5 percent level from $h = 1$ onward and at the 1 percent level from $h = 2$ through $h = 4$. Moreover, Appendix Figure A3 shows no systematic response of real GDP. Taken together, these results indicate that the baseline fall in the debt ratio reflects slower accumulation of the real debt stock rather than ensuing macroeconomic dynamics.

Panel B changes the treatment of common shocks. Replacing the baseline year fixed

adopted it only after 2019, leaving 88 countries with usable active-rule observations during the estimation window. In the active-rule-only fixed-effects exercise, Azerbaijan, Barbados, and Cambodia each contribute only one active-rule observation (in 2019) and are thus removed as fixed-effect singletons, leaving a final sample of 85 countries.

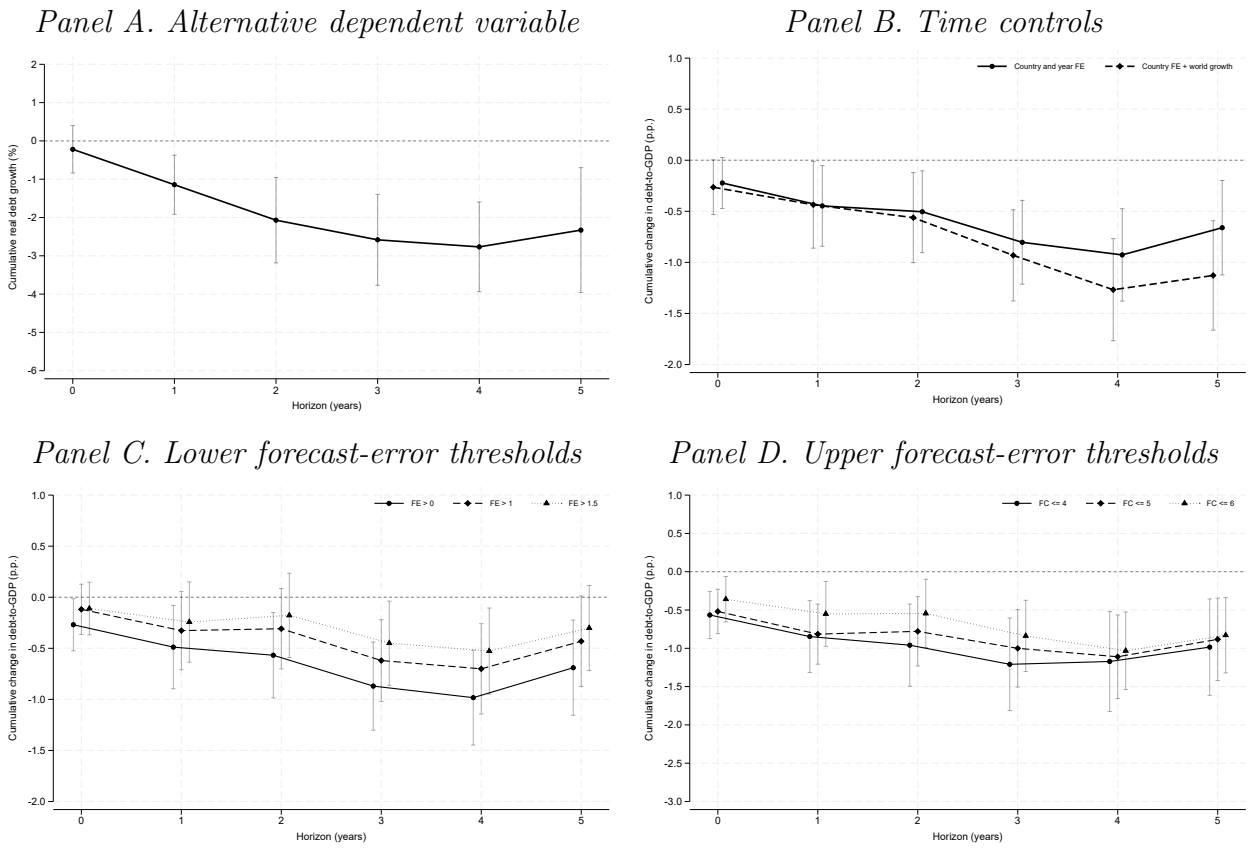
Figure 3: The effect of fiscal consolidation on public debt



Source: Authors' elaboration. *Notes:* The figure reports the cumulative change in gross public debt as a share of GDP from $t-1$ to $t+h$ following a fiscal consolidation of one percentage point of GDP. The parsimonious specification includes two lags of $D_{i,t}$, $FC_{i,t}$, and the debt-to-GDP ratio. It also includes country and year fixed effects. The fully controlled specification additionally includes lags of the change in the primary balance, inflation, real GDP growth, and the change in the implicit interest rate. Capped bars are 90 percent confidence intervals based on standard errors clustered by country.

effects with centered world growth, entered directly and interacted with the consolidation as in Cloyne *et al.* (2020, 2023), leaves the shape and statistical precision of the response intact and produces somewhat larger medium-run estimates: the debt ratio declines by 0.93 percentage points at $h = 3$, 1.27 at $h = 4$, and 1.13 at $h = 5$. The average response is therefore not driven by the particular treatment of common shocks. Panels C and D assess the influence of the fiscal consolidation episode threshold definition. Lower thresholds generate the same delayed negative profile, but the magnitude attenuates somewhat as the minimum forecast error rises. By contrast, capping positive consolidations at 4, 5, or 6 percentage points produces uniformly negative and precisely estimated responses. Overall, the baseline debt response is not an artifact of choosing a particular threshold definition.

Figure 4: Robustness of the average debt response



Source: Authors' elaboration. *Notes:* Panel A replaces the dependent variable with cumulative real public-debt growth. Panel B replaces the baseline year fixed effects with centered world growth. Panel C varies the lower forecast-error threshold; Panel D retains the baseline lower threshold of 0.5 and imposes upper forecast-error thresholds. Every specification retains the fully controlled vector. Capped bars are 90 percent confidence intervals based on standard errors clustered by country. Panels use different vertical scales where required by the units of the outcome and the lower confidence limits.

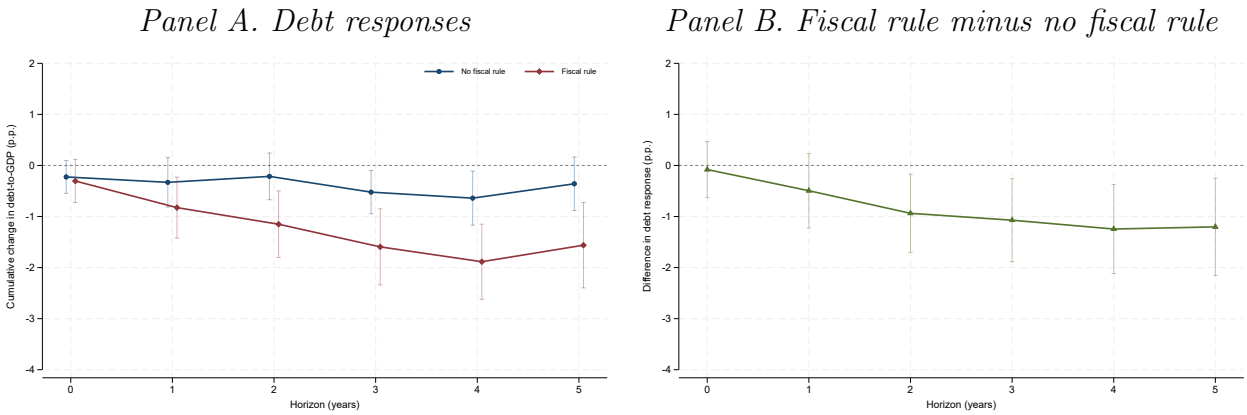
Finally, we test the sensitivity of our results to the timing surrounding the release of WEO

vintages.[9] Appendix Figure A5 reconstructs the treatment using the April rather than the October WEO vintage. Both measures produce a persistent fall in debt. The baseline debt response is therefore robust to alternative forecast timings.

4.2 The role of fiscal rules

Our main question is whether fiscal rule adoption conditions the debt payoff from consolidation. Figure 5 begins with OLS estimates on the baseline 140-country sample. Panel A reports the responses without and with an active fiscal rule, while Panel B reports their difference. In the absence of a rule, the debt response is small and generally imprecise, remaining below 0.7 percentage points in absolute value throughout the horizon. Under the presence of a fiscal rule, debt falls between 1.5 and 2 percentage points at medium horizons. The rule vs. no-rule differential remains between 1.07 and 1.25 percentage points over $h = 3$–5. It is statistically significant at the 5 percent level from $h = 2$ onward. Appendix Table A3 reports the underlying estimates.

Figure 5: Fiscal consolidations with and without a fiscal rule: OLS evidence



Source: Authors' elaboration. *Notes:* Panel A reports the cumulative response of gross public debt as a share of GDP to a fiscal consolidation of one percentage point of GDP without a fiscal rule (blue) and with an active fiscal rule (red). Panel B reports the difference between those responses (fiscal rule minus no fiscal rule) in green. Capped bars are 90 percent confidence intervals based on standard errors clustered by country; confidence intervals for the with-rule response and the rule–no-rule contrast use the delta method.

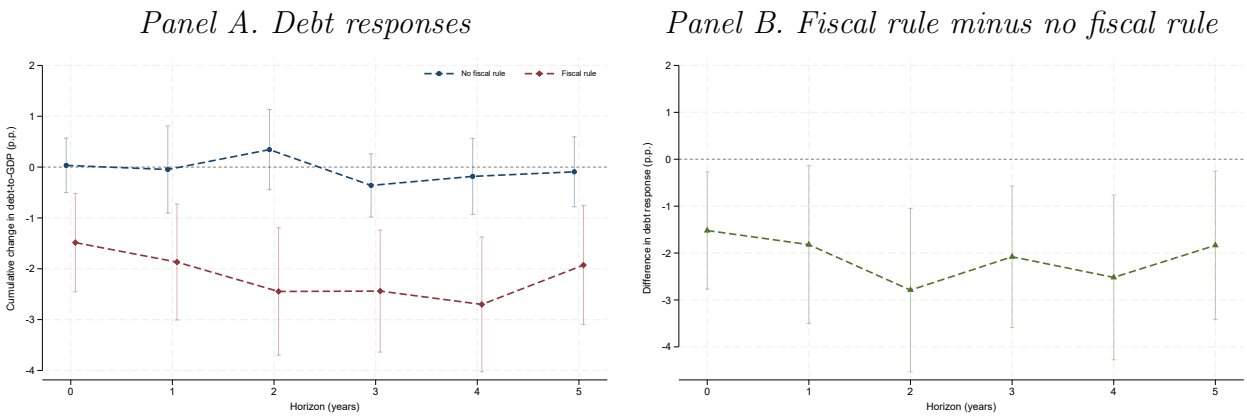
Countries may adopt rules in response to debt stress, or fiscal rule adoption may coincide with broader institutional reforms that also affect debt dynamics. To address endogeneity concerns, Figure 6 instruments the fiscal rule indicator and its interaction with fiscal consoli-

[9] Appendix Figure A4 shows that the April and October vintages contain meaningfully different information: over their common support, the correlation between the two forecast errors is 0.28 and the Jaccard overlap of positive consolidation observations is 0.36.

dation using land-neighbor rule exposure and its interaction with the consolidation. Because the instrument requires usable border exposure, the IV sample contains 124 countries.

The IV estimates reinforce the OLS pattern. The response without a rule remains statistically indistinguishable from zero throughout the horizon. The active-rule response is 1.49 percentage points on impact, exceeds 2.4 percentage points at horizons two through four, and remains 1.93 percentage points after five years. The estimated differential ranges from 1.52 to 2.79 percentage points. It is statistically significant at the 5 percent level at $h = 0$ and from $h = 2$ through $h = 4$, and at the 10 percent level at $h = 1$ and $h = 5$. Appendix Table A4 provides the complete coefficient paths.

Figure 6: Fiscal consolidations with and without a fiscal rule: IV evidence



Source: Authors' elaboration. *Notes:* Panel A reports the cumulative response of gross public debt as a share of GDP to a fiscal consolidation of one percentage point of GDP without a fiscal rule (blue) and with an active fiscal rule (red). Panel B reports the difference between those responses (fiscal rule minus no fiscal rule) in green. Capped bars are 90 percent confidence intervals based on standard errors clustered by country; confidence intervals for the with-rule response and the rule–no-rule contrast use the delta method.

Table A5 and Figure A7 in the Appendix provide a battery of IV diagnostic tests suggesting instrument relevance.[10] Additional checks availabe in Appendix Figures A6–A8 and Tables A6–A7 probe the exclusion restriction and the influence of particular observations. The estimates are stable when common shocks are absorbed by IMF economic-group-by-year fixed effects, when predetermined averages of neighboring countries' growth and fiscal

[10] At $h = 0$, the conditional Sanderson–Windmeijer first-stage statistics are 19.53 for rule status and 39.08 for its interaction with fiscal consolidation. The joint cluster-robust Kleibergen–Paap rk Wald F statistic is 7.72, and the corresponding underidentification test rejects the null with a p-value below 0.001. The homoskedastic Cragg–Donald statistic is 85.78 and exceeds the Stock–Yogo 10 percent maximal IV size reference value of 7.03 for two endogenous regressors and two excluded instruments. Because Stock–Yogo critical values assume i.i.d. errors, they are not formal cutoffs for the cluster-robust Kleibergen–Paap statistic. We therefore complement conventional Wald inference with targeted Anderson–Rubin tests. Their p-values closely track the baseline inference and remain below 0.10 at every horizon. These results support instrument relevance. Full results appear in Appendix Table A5 and Appendix Figure A7.

conditions are included, and when both adjustments enter jointly. They also survive dating rule status and the instruments at $t-1$, excluding observations around domestic rule transitions, and requiring at least two land neighbors, although estimates become less precise as the network-support restriction reduces the sample. Predetermined-outcome placebos do not reject jointly after correcting for multiple testing, either in the full IV sample or before domestic rule adoption. Exposure permutations place the observed first stages in the far tail of placebo assignments. Finally, all 124 conventional leave-one-country-out estimates and all 124 strict network leave-one-out estimates remain negative; at $h = 0$ they range from approximately -1.78 to -1.13.[11] Taken together, the results show that fiscal consolidations are followed by larger and more persistent debt reductions when a fiscal rule is active, even after potential endogeneity concerns are explicitly taken into account.

4.3 Does fiscal rule quality matter?

We now analyze whether the design quality or overall strength of the rule further conditions the debt response. Equation (3) retains the full 140-country sample, including country-years without an active rule. Fiscal rule quality is measured by the country-level average of the Fiscal Rule Strength Index (FRSI) over years in which a rule is active, following the index construction criteria in Alonso-Albarran *et al.* (2025). Figure 7 evaluates the consolidation response at the 5th and 95th percentiles of the FRSI distribution and reports their difference.

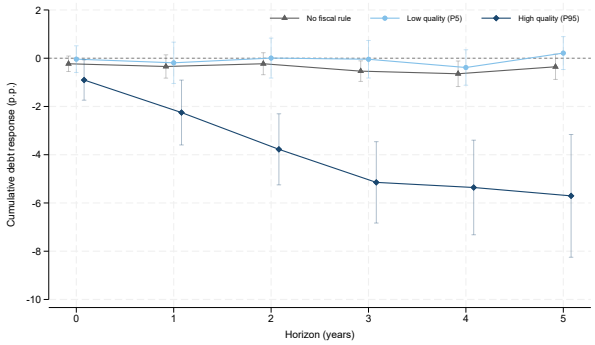
Results show substantial heterogeneity across quality regimes. Under low-quality rules, the debt response to fiscal adjustments is modest and imprecisely estimated throughout the horizon. Instead, in the presence of high-quality rules, debt falls by between 5 and 5.7 points at horizons three through five. Panel B directly tests the relevant contrast. The high vs. low quality differential is statistically indistinguishable from zero on impact, reaches -2.1 percentage points at $h = 1$ and -3.8 at $h = 2$, and ranges from -5.0 to -5.9 points between $h = 3$ and $h = 5$. It is marginally significant at $h = 1$ and statistically significant at the 1 percent level from $h = 2$ onward. Overall, the results suggest that stronger fiscal frameworks are associated with more persistent debt responses. Appendix Table A8 reports the underlying estimates.

While the previous result considers variation at two specific points of the quality distribution, Figure 8 uses the full quality distribution. Panel A evaluates the dynamic debt response at five FRSI percentiles; Panel B traces at $h = 4$ the component of the response attributable

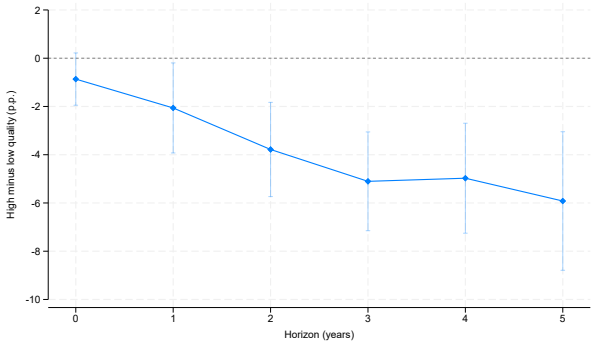
[11] Appendix Figures A6–A8 and Tables A6–A7 report the complete battery of sensitivity checks and instrument diagnostic tests, including alternative control sets, consolidation thresholds and upper caps, two-way clustering, timing conventions, and network-support restrictions.

Figure 7: Fiscal consolidations and debt dynamics by fiscal rule quality

Panel A. Debt responses by rule quality



Panel B. High- minus low-quality response

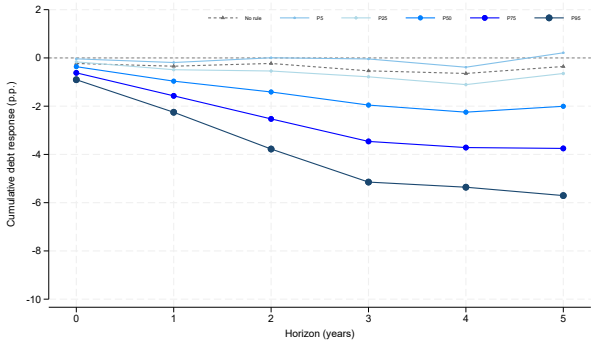


Source: Authors' elaboration. *Notes:* The figure reports estimates from Equation (3) using the common fully controlled vector dated at $t-1$. Low- and high-quality rules are evaluated at the 5th and 95th percentiles of the country-level FRSI distribution. Panel A also reports the response when no fiscal rule is active. Panel B reports the high- minus low-quality response. Capped bars are 90 percent confidence intervals based on standard errors clustered by country and computed by the delta method for the evaluated responses and their contrasts.

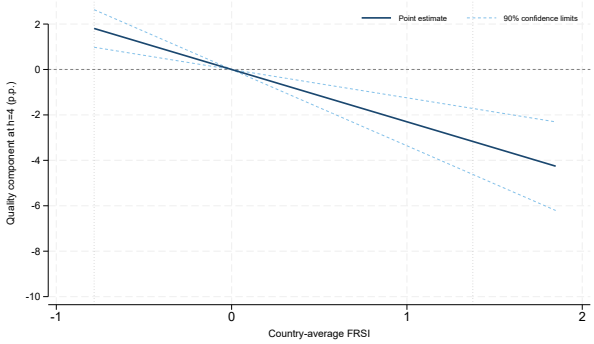
to rule quality, relative to a framework with standardized quality equal to zero. The increasingly negative responses across the ordered percentile distribution and the downward-sloping $h = 4$ profile show that result is not an artifact of choosing a particular low–high quality cutoff.

Figure 8: Debt responses across the fiscal rule quality distribution

Panel A. Varying percentiles of FRSI



Panel B. Quality component at $h = 4$



Source: Authors' elaboration. *Notes:* Panel A evaluates the active-rule response at the 5th, 25th, 50th, 75th, and 95th percentiles of the country-level FRSI distribution; marker size increases with quality. The no-rule response is shown as a reference. Panel B reports $q\widehat{\omega}_4$, the estimated quality component of the consolidation response at $h = 4$, across the observed common-support distribution of standardized FRSI. The solid line is the point estimate, dashed lines are the 90 percent confidence interval, and vertical lines mark the 5th and 95th percentiles. Standard errors are clustered by country; confidence intervals for the evaluated responses and quality components use the delta method.

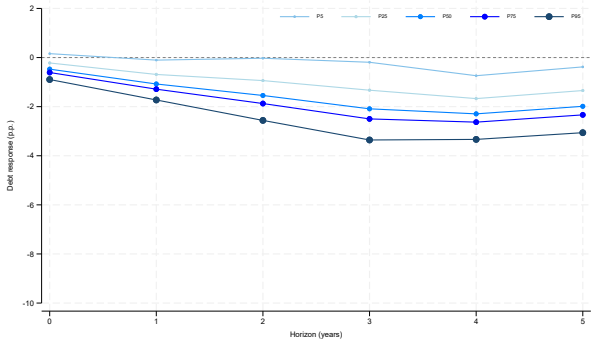
To analyze the different components of fiscal rule quality, we proceed in two steps. First,

we examine each of the four FRSI dimensions separately. Figure 9 repeats the distributional exercises for legal basis, monitoring arrangements, enforcement and correction mechanisms, and flexibility provisions. Each dimension replaces the aggregate FRSI score in an otherwise identical regression that includes both the active-quality level term and its interaction with fiscal consolidation. Because the four dimensions may covary, these estimates should be interpreted as dimension-specific associations rather than as mutually independent contributions.

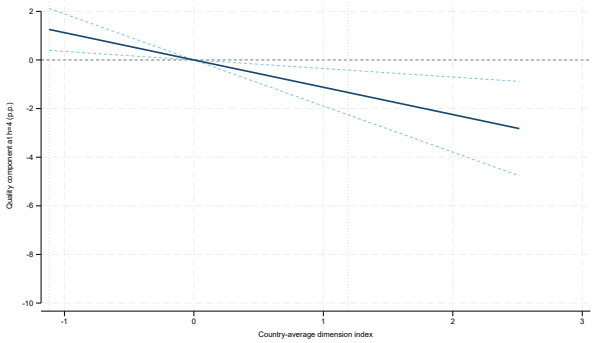
Results show that each dimension points in the same direction, although not every dimension is equally precise at every horizon. Flexibility displays the earliest and most consistent gradient, beginning at $h = 1$, whereas enforcement and correction mechanisms produces the largest medium- and long-run differential. At $h = 4$, moving from the 5th to the 95th percentile is associated with an additional debt decline of approximately 2.6 percentage points for the legal dimension, 3.0 points for monitoring, 5.1 points for enforcement, and 3.9 points for flexibility.

Figure 9: Fiscal rule quality dimensions and the debt response

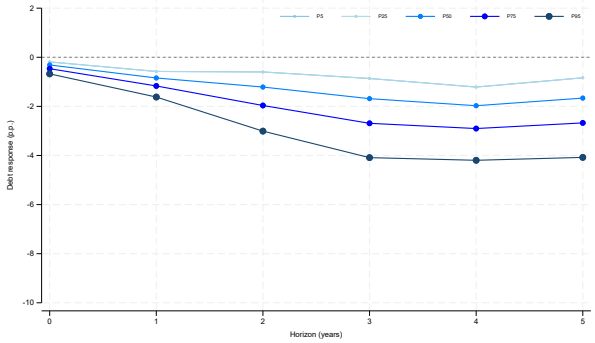
Panel A. Legal basis and coverage



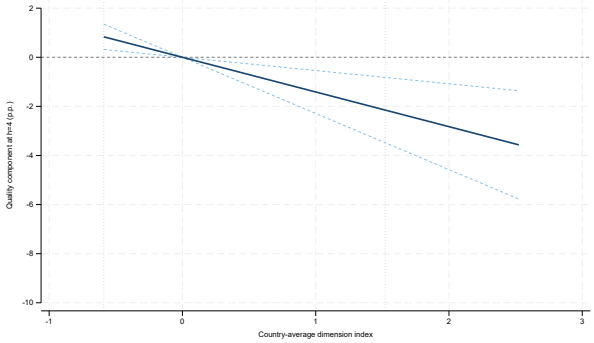
Panel B. Legal dimension at $h = 4$



Panel C. Monitoring arrangements



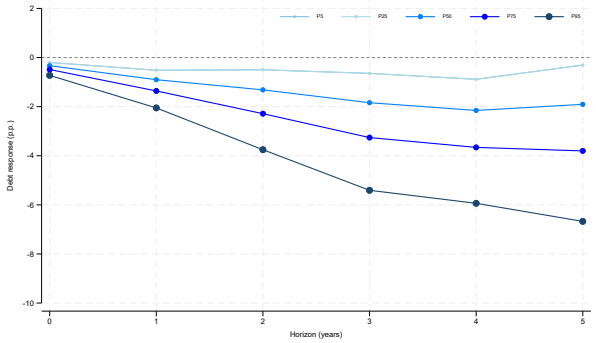
Panel D. Monitoring dimension at $h = 4$



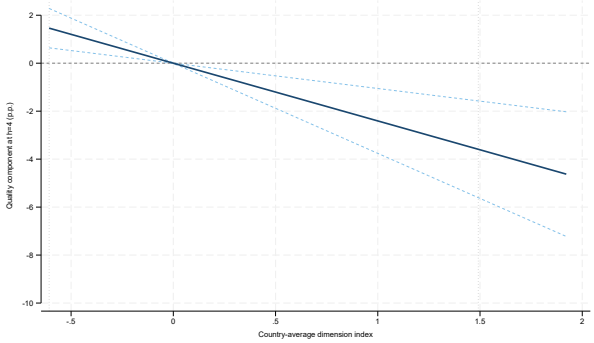
Source: Authors' elaboration. *Notes:* Left-hand panels evaluate active-rule responses at the 5th, 25th, 50th, 75th, and 95th percentiles of each country-level quality dimension. Right-hand panels report the corresponding quality component of the response at $h = 4$. Capped bars and dashed lines denote 90 percent confidence intervals based on standard errors clustered by country and computed by the delta method for evaluated responses and quality components.

Figure 9: fiscal rule quality dimensions and the debt response (continued)

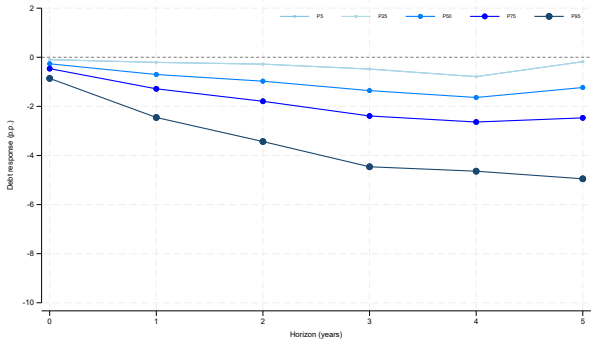
Panel E. Correction and enforcement



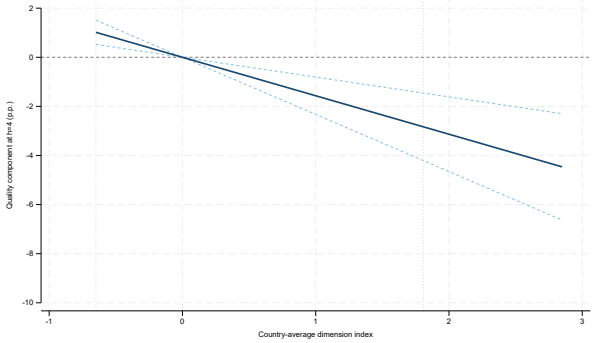
Panel F. Enforcement dimension at $h = 4$



Panel G. Flexibility provisions



Panel H. Flexibility dimension at $h = 4$



Source: Authors' elaboration. *Notes:* Left-hand panels evaluate active-rule responses at the 5th, 25th, 50th, 75th, and 95th percentiles of each country-level quality dimension. Right-hand panels report the corresponding quality component of the response at $h = 4$. Capped bars and dashed lines denote 90 percent confidence intervals based on standard errors clustered by country and computed by the delta method for evaluated responses and quality components.

Finally, we assess the relative contribution of each dimension by entering all four dimensions simultaneously. Table 2 reports the results for the full 140-country specification. The specification includes the four active-quality level terms and their four interactions with fiscal consolidation, while retaining the binary rule terms and the common control set. The four interaction terms are jointly significant from $h = 2$ through $h = 5$ and marginally significant at $h = 1$. Flexibility and enforcement appear as the the most relevant dimensions. In the case of flexibility, its high–low differential remains negative and is statistically significant at $h = 5$, with additional marginally significant estimates at $h = 1$ and $h = 4$. In addition, enforcement and correction mechanisms retain a large negative differential at longer horizons, whereas the legal and monitoring components are no longer statistically distinguishable from zero. These findings are in line with recent literature stressing the relevance of complementing flexibility provisions with correction mechanisms for enhancing the effectiveness of rules-based fiscal frameworks (Acalin *et al.*, 2025; Alonso-Albarran *et al.*, 2025).

Table 2: Fiscal rule quality dimensions entered jointly

	$h = 0$	$h = 1$	$h = 2$	$h = 3$	$h = 4$	$h = 5$
Panel A. Full 140-country specification						
Legal basis	−1.315	−0.788	−1.037	−0.906	0.188	1.383
	(1.234)	(1.359)	(1.371)	(1.479)	(1.398)	(1.410)
Monitoring arrangements	0.269	0.135	−0.571	−0.871	−0.951	−1.014
	(0.857)	(1.092)	(1.373)	(1.380)	(1.444)	(1.834)
Enforcement and correction	0.154	−0.005	−1.345	−2.617	−3.279*	−4.364*
	(0.762)	(1.175)	(1.444)	(1.709)	(1.970)	(2.394)
Flexibility provisions	−0.050	−1.724*	−1.616	−1.836	−2.054*	−3.201***
	(0.723)	(0.962)	(1.079)	(1.224)	(1.166)	(1.215)
Joint test p-value	0.477	0.051	0.007	< 0.001	0.002	0.003
Observations	2,610	2,609	2,608	2,607	2,606	2,604
Countries	140	140	140	140	140	140

Source: Authors' elaboration. *Notes:* Each entry reports the partial high–low differential in the cumulative debt response associated with moving the indicated dimension from its 5th to its 95th percentile, holding the other three dimensions constant. Panel A estimates the hierarchically saturated specification on the full sample, including the four active-quality level terms, their interactions with fiscal consolidation, fiscal rule status, and the consolidation–rule interaction. The joint-test row reports the p-value for the null that the four consolidation–quality interactions are jointly zero. All regressions include the common predetermined control vector and country and year fixed effects. Standard errors clustered by country are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5 Conclusions and policy implications

Fiscal consolidations are undertaken to restore fiscal space and place public debt on a more sustainable path, but their debt payoff is usually not guaranteed. Previous literature addresses various design elements of fiscal adjustments themselves that could explain their success, but such emphasis tends to overlook key contextual features surrounding these policy shocks.

This paper studies whether the institutional environment under which consolidations take place can affect ensuing debt dynamics. Using fiscal-balance forecast errors and local projections for an unbalanced panel of 140 countries over 1990–2019, we estimate the average cumulative public debt response to consolidation and then examine how it varies with fiscal rule presence and quality.

First, the average response is gradual. A one-percentage-point-of-GDP consolidation has a small and statistically imprecise effect on impact, but the debt ratio falls by approximately 1 percentage point at longer horizons. The absence of a systematic real-GDP response indicates that the baseline result does not arise from an expansion of the denominator.

Second, the presence of fiscal rules amplifies the debt payoff. The baseline-sample OLS differential becomes statistically significant at horizon two and remains between 0.9 and 1.2 percentage points over horizons two through five. An IV design yields the same negative sign and medium-run timing, with larger point estimates.

Third, the binary distinction between rule and no rule masks substantial heterogeneity in institutional design. Differences in debt declines between high- and low-quality frameworks are economically large, reaching 5 to 6 percentage points of GDP over the medium term. Among the quality components, flexibility provisions and enforcement/correction mechanisms appear as the most relevant dimensions driving the results. Thus, merely adopting a numerical rule is unlikely to be sufficient by itself to anchor expectations. In contrast, the capacity to accommodate shocks while preserving commitment to fiscal sustainability are central to lock-in the gains of fiscal adjustment in the medium turn.

Overall, the findings suggest that governments interested in addressing debt sustainability concerns should not only focus on defining the size and composition of consolidation packages, but also on the design of the institutional framework that affects whether those packages are sustained over time.

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Appendix

List of countries

Afghanistan, Albania, Algeria, Angola, Antigua and Barbuda, Armenia, Australia, Austria, Azerbaijan, Barbados, Belarus, Belgium, Belize, Benin, Bolivia, Bosnia and Herzegovina, Brazil, Brunei Darussalam, Bulgaria, Burkina Faso, Burundi, Cabo Verde, Cambodia, Cameroon, Canada, Chile, China, Colombia, Costa Rica, Cote d'Ivoire, Croatia, Cyprus, Czech Republic, Denmark, Dominica, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Estonia, Eswatini, Fiji, Finland, France, Georgia, Germany, Ghana, Grenada, Guatemala, Guinea, Guyana, Haiti, Honduras, Hong Kong SAR, Hungary, India, Indonesia, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Kiribati, Korea, Kuwait, Kyrgyz Republic, Lao P.D.R., Lesotho, Liberia, Lithuania, Luxembourg, Madagascar, Malawi, Malaysia, Malta, Mauritania, Mauritius, Mexico, Moldova, Mongolia, Morocco, Mozambique, Myanmar, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, North Macedonia, Norway, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Russia, Rwanda, Saudi Arabia, Senegal, Serbia, Sierra Leone, Slovak Republic, Slovenia, South Africa, Spain, Sri Lanka, Switzerland, Syria, Tajikistan, Tanzania, Thailand, The Bahamas, The Gambia, Togo, Tonga, Trinidad and Tobago, Tunisia, Turkiye, Uganda, Ukraine, United Arab Emirates, United Kingdom, United States, Uruguay, Uzbekistan, Vanuatu, Vietnam, Yemen, Zambia.

Table A1: Descriptive statistics

Variable	N	Mean	SD	P10	Median	P90
Panel A. Country-year variables (fully controlled specification at h=0; N=2,610)						
Gross public debt (percent of GDP)	2,610	50.86	34.62	18.14	44.13	88.35
Raw fiscal-balance forecast error (p.p.)	2,610	0.20	1.87	-1.67	0.21	2.08
Baseline fiscal-balance forecast error (p.p.)	2,610	0.00	1.75	-1.75	0.03	1.77
Fiscal-consolidation magnitude (p.p.)	2,610	0.54	1.06	0.00	0.00	1.77
Fiscal-consolidation indicator	2,610	0.33	0.47	0.00	0.00	1.00
Any fiscal rule	2,610	0.51	0.50	0.00	1.00	1.00
Primary balance (percent of GDP)	2,610	-0.27	3.70	-4.11	-0.53	3.67
Change in primary balance (p.p.)	2,610	-0.02	2.86	-2.67	0.07	2.43
Real GDP growth (percent)	2,610	3.60	3.90	-0.02	3.63	7.33
Inflation (percent)	2,610	4.77	5.30	0.52	3.32	10.67
Implicit interest rate (percent)	2,610	-1.01	90.27	0.72	3.29	6.93
Change in implicit interest rate (p.p.)	2,610	-1.13	38.44	-0.94	-0.05	0.82
Panel B. Identified fiscal-consolidation years in the broad sample						
Fiscal-consolidation magnitude, FC years (p.p.)	1,144	1.72	1.41	0.58	1.26	3.56
Panel C. Country-average fiscal-rule quality (N=85 countries)						
Country-average FRSI	85	0.09	0.70	-0.78	-0.02	1.04
Legal-basis dimension	85	0.15	0.80	-1.12	0.29	1.06
Monitoring dimension	85	0.07	0.79	-0.59	-0.26	1.21
Enforcement and correction dimension	85	0.01	0.72	-0.61	-0.19	1.20
Flexibility dimension	85	0.15	0.87	-0.65	-0.11	1.16

Source: Authors' elaboration. *Notes:* Panel A uses exactly the 2,610 country-year observations entering the fully controlled local projection at horizon zero; every statistic therefore has identical support. In Panel A, the fiscal-consolidation measure equals the baseline forecast error when it exceeds 0.5 percentage points and zero otherwise. Panel B conditions on the 1,144 identified consolidation years in the broad 140-country universe over 1990–2019, so its mean and median describe the size of a consolidation rather than a variable containing zeros. Panel C contains one observation per country in the common 85-country active-rule quality support. Quality variables are standardized Alonso-Albarran *et al.* (2025)-replication scores averaged within country over active-rule years.

Table A2: Fiscal consolidations and the cumulative response of debt-to-GDP

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
Parsimonious	-0.179	-0.406^{*}	-0.461^{**}	-0.858^{***}	-1.168^{***}	-0.947^{***}
	(0.139)	(0.214)	(0.222)	(0.232)	(0.296)	(0.293)
Fully controlled	-0.223	-0.447^{*}	-0.504^{**}	-0.804^{***}	-0.927^{***}	-0.661^{**}
	(0.152)	(0.240)	(0.244)	(0.250)	(0.275)	(0.281)
Observations	2,610	2,609	2,608	2,607	2,606	2,604
Countries	140	140	140	140	140	140

Source: Authors' elaboration. *Notes:* The table reports the estimated coefficient on $FC_{i,t}$ from Equation (1). The dependent variable is the cumulative change in gross public debt as a share of GDP from $t-1$ to $t+h$. The parsimonious and fully controlled specifications are defined in the note to Figure 3. Country-clustered standard errors are reported in parentheses. Statistical significance is indicated by $^{*}p<0.10$, $^{**}p<0.05$, and $^{***}p<0.01$.

Table A3: OLS estimates of fiscal-rule heterogeneity on the sample

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
No fiscal rule	-0.225	-0.331	-0.214	-0.522^{**}	-0.640^{**}	-0.359
	(0.195)	(0.294)	(0.278)	(0.258)	(0.322)	(0.319)
Active fiscal rule	-0.305	-0.826^{**}	-1.151^{***}	-1.594^{***}	-1.885^{***}	-1.562^{***}
	(0.257)	(0.363)	(0.397)	(0.453)	(0.448)	(0.508)
Fiscal rule minus no fiscal rule	-0.080	-0.495	-0.937^{**}	-1.072^{**}	-1.245^{**}	-1.202^{**}
	(0.335)	(0.443)	(0.466)	(0.493)	(0.530)	(0.578)
Observations	2,610	2,609	2,608	2,607	2,606	2,604
Countries	140	140	140	140	140	140

Source: Authors' elaboration. *Notes:* Each column is a separate local projection estimated by OLS on the 140-country sample. The first two rows report the cumulative debt response to a one-percentage-point-of-GDP fiscal consolidation without and with an active fiscal rule. The third row is their difference. The specification includes the common fully controlled vector dated at $t-1$ and country and year fixed effects. Country-clustered standard errors are in parentheses. Statistical significance is indicated by $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Table A4: IV estimates of fiscal-rule heterogeneity on the final IV sample

	$h=0$	$h=1$	$h=2$	$h=3$	$h=4$	$h=5$
No fiscal rule	0.034	−0.047	0.344	−0.360	−0.182	−0.092
	(0.326)	(0.521)	(0.480)	(0.378)	(0.455)	(0.418)
Active fiscal rule	−1.485**	−1.868***	−2.446***	−2.439***	−2.702***	−1.928***
	(0.589)	(0.693)	(0.762)	(0.730)	(0.806)	(0.710)
Fiscal rule minus no fiscal rule	−1.520**	−1.821*	−2.790***	−2.079**	−2.519**	−1.835*
	(0.760)	(1.022)	(1.059)	(0.917)	(1.068)	(0.960)
Observations	2,288	2,287	2,286	2,285	2,284	2,282
Countries	124	124	124	124	124	124

Source: Authors' elaboration. *Notes:* Each column is a separate local projection estimated by IV on the fixed 124-country sample. The fiscal-rule indicator and its interaction with fiscal consolidation are instrumented using land-neighbor fiscal-rule exposure and its interaction with the consolidation. The first two rows report the cumulative debt response without and with an active rule; the third row is their difference. The specification includes the common fully controlled vector dated at $t-1$ and country and year fixed effects. Country-clustered standard errors are in parentheses. Statistical significance is indicated by $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Table A5: Baseline IV estimates and identification diagnostics

	$h = 0$	$h = 1$	$h = 2$	$h = 3$	$h = 4$	$h = 5$
Fiscal rule − no fiscal rule	-1.520^{**}	-1.821^{*}	-2.790^{***}	-2.079^{**}	-2.519^{**}	-1.835^{*}
	(0.760)	(1.022)	(1.059)	(0.917)	(1.068)	(0.960)
Wald p-value	0.045	0.075	0.008	0.023	0.018	0.056
Targeted AR p-value	0.051	0.068	0.006	0.024	0.014	0.051
Two-way-clustered Wald p-value	0.092	0.096	0.005	0.031	0.028	0.052
Observations	2,288	2,287	2,286	2,285	2,284	2,282
Countries	124	124	124	124	124	124
Identification diagnostics at $h = 0$						
Sanderson–Windmeijer F: fiscal rule						19.53
Sanderson–Windmeijer F: FC × fiscal rule						39.08
Partial R^2: fiscal rule / FC × fiscal rule						0.075 / 0.259
Kleibergen–Paap rk Wald F						7.72
Kleibergen–Paap underidentification p-value						< 0.001
Cragg–Donald Wald F						85.78
Exposure-permutation p: rule / FC × rule						0.005 / 0.010

Source: Authors' elaboration. *Notes:* The upper panel reports the active-rule minus no-rule response. The targeted Anderson–Rubin test evaluates the interaction coefficient while treating the level fiscal rule regressor as strongly identified. At $h = 0$, the Stock–Yogo 10-percent maximal-IV-size critical value for two endogenous regressors and two excluded instruments is 7.03; the Cragg–Donald statistic exceeds it. Stock–Yogo values assume i.i.d. errors and therefore do not provide a formal cutoff for the cluster-robust Kleibergen–Paap statistic. For a single endogenous regressor, the corresponding 10- and 15-percent maximal-size critical values are 19.93 and 11.59: the conditional rule first stage lies just below the former and above the latter, while the interaction first stage exceeds the former. Permutation p-values use 199 within-IMF-economic-group-by-year reshufflings plus the observed assignment. Country-clustered standard errors are in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A6: IV robustness of the fiscal rule minus no-rule response

	$h = 0$	$h = 3$	$h = 5$	KP F at $h = 0$	Obs. at $h = 0$
Baseline	-1.520^{**}	-2.079^{**}	-1.835^{*}	7.72	2,288
	(0.760)	(0.917)	(0.960)		
Common-support baseline	-1.668^{**}	-1.963^{**}	-1.781^{*}	6.23	2,226
	(0.786)	(0.865)	(0.955)		
IMF group × year fixed effects	-1.612^{**}	-2.147^{**}	-2.120^{**}	4.39	2,226
	(0.726)	(0.911)	(1.066)		
Predetermined neighbor controls	-1.467^{**}	-1.817^{**}	-1.627^{*}	6.12	2,226
	(0.737)	(0.820)	(0.891)		
Group-year FE and neighbor controls	-1.364^{**}	-1.874^{**}	-1.736^{*}	4.04	2,226
	(0.648)	(0.814)	(0.909)		
Lagged rule and instrument	-1.334	-2.003^{**}	-1.877^{*}	8.57	2,288
	(0.817)	(0.966)	(1.012)		
Outside rule-transition window	-1.642^{*}	-2.296^{**}	-2.032^{*}	5.39	2,123
	(0.854)	(0.938)	(1.052)		
At least two land neighbors	-1.764^{*}	-1.650	-1.191	5.72	2,030
	(0.951)	(1.034)	(0.962)		
At least three land neighbors	-1.671	-2.142	-2.018	4.66	1,662
	(1.200)	(1.447)	(1.426)		
Country and year clustering	-1.520^{*}	-2.079^{**}	-1.835^{*}	7.56	2,288
	(0.902)	(0.964)	(0.944)		

Source: Authors' elaboration. *Notes:* Every coefficient is the IV estimate of the active-rule response minus the no-rule response; negative values indicate a larger debt reduction under a fiscal rule. Parentheses contain the corresponding standard errors. The common-support rows hold fixed observations with available neighbor controls. The lagged design dates both the rule and excluded instruments at $t - 1$; the stable design excludes a one-year window around changes in own-rule status. The final row reports two-way country-and-year clustered inference; point estimates and first-stage statistics are unchanged. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: IV validity and influence diagnostics

Panel A. Joint placebo tests for excluded instruments				
	Full IV sample		Pre-adoption sample	
Predetermined outcome	Raw p	Holm p	Raw p	Holm p
Debt-to-GDP	0.229	0.915	0.885	1.000
Fiscal consolidation	0.794	1.000	0.414	1.000
Real GDP growth	0.049	0.246	0.050	0.248
Primary balance	0.965	1.000	0.807	1.000
Change in implicit interest rate	0.343	1.000	0.345	1.000
Panel B. Leave-one-country-out sensitivity at $h = 0$				
	Full estimate	LOO minimum	LOO maximum	Maximum absolute change
Conventional LOO	-1.520	-1.781	-1.134	0.386
Strict network LOO	-1.520	-1.741	-1.131	0.388
Panel C. Exposure-randomization inference (199 reshufflings)				
First stage for rule level		Permutation $p = 0.005$		
First stage for rule × consolidation		Permutation $p = 0.010$		

Source: Authors' elaboration. *Notes:* Panel A tests whether the two excluded instruments jointly predict outcomes dated $t-1$, conditional on states dated $t-2$. Holm adjustments are computed across the five outcomes within each sample. Panel B reports the fiscal-rule minus no-rule coefficient from all 124 leave-one-country-out estimates. Strict network LOO also removes the omitted country from every remaining country's neighbor exposure. Panel C reports randomization-inference p-values from 199 exposure reshufflings within IMF economic-group-by-year cells plus the observed assignment.

Table A8: Fiscal-rule quality and the cumulative response of debt

	$h = 0$	$h = 1$	$h = 2$	$h = 3$	$h = 4$	$h = 5$
No fiscal rule	−0.230	−0.343	−0.229	−0.535**	−0.644**	−0.355
	(0.195)	(0.293)	(0.277)	(0.259)	(0.322)	(0.319)
Active rule at FRSI P5	−0.039	−0.190	0.006	−0.043	−0.385	0.213
	(0.336)	(0.520)	(0.503)	(0.473)	(0.447)	(0.414)
Active rule at FRSI P95	−0.904*	−2.252***	−3.777***	−5.146***	−5.358***	−5.706***
	(0.508)	(0.817)	(0.895)	(1.025)	(1.192)	(1.546)
FRSI P95 minus P5	−0.865	−2.061*	−3.783***	−5.103***	−4.973***	−5.919***
	(0.659)	(1.134)	(1.188)	(1.245)	(1.386)	(1.746)
Observations	2,610	2,609	2,608	2,607	2,606	2,604
Countries in full equation	140	140	140	140	140	140
Countries identifying quality			85			

Source: Authors' elaboration. *Notes:* The table reports the no-rule response and active-rule responses evaluated at the 5th and 95th percentiles of the country-average Alonso-Albarran *et al.* (2025)-replication FRSI. The last coefficient row is the P95–P5 difference. The hierarchically saturated regression includes all $FR = 0$ observations, the active-quality state directly, its interaction with fiscal consolidation, the common fully controlled vector dated at $t-1$, and country and year fixed effects. The equation contains 140 countries; the quality gradient is identified by the 85-country usable active-rule support. Country-clustered standard errors are in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

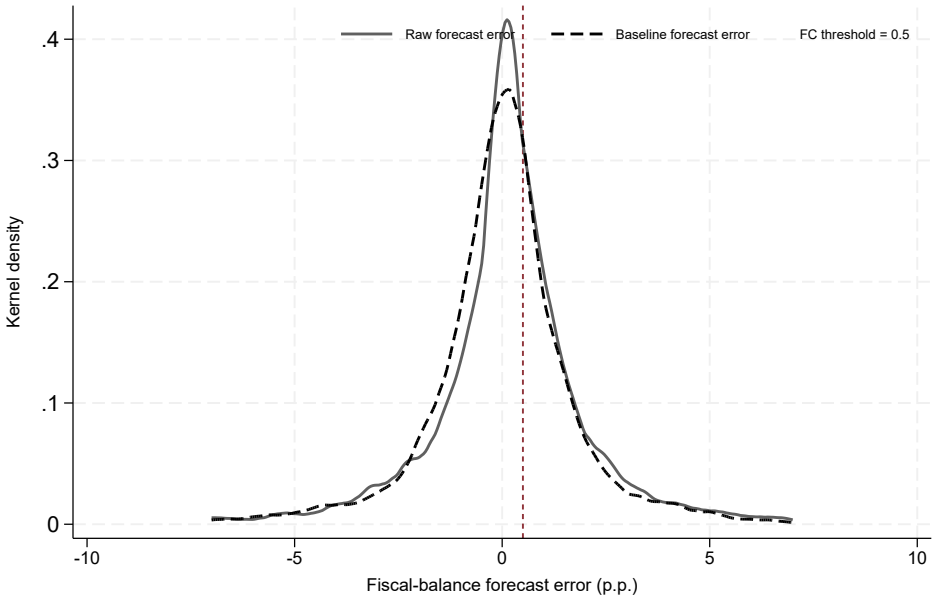
Table A9: Fiscal Rule Strength Index: Measurement criteria

Dimension	Indicator	Score	Source
1. Legal basis	Legal basis	$[0, 1]$	FR dataset
2. Monitoring of fiscal rules	Monitoring mechanism outside the government	$[0, 1]$	FR dataset
	Monitoring of fiscal rules	$[0, 1]$	Fiscal Council dataset
	Independence – legal and operational	$[0, 1]$	Fiscal Council dataset
	Independent body setting budget assumptions and monitoring budget implementation	$[0, 1]$	FR dataset
3. Enforcement and correction mechanism	Formal enforcement procedure; fiscal responsibility law	$[0, 1]$	FR dataset
	Presence of correction mechanism in case of deviation from the rule	$[0, 1]$	FR dataset
	Type of correction	$[0, 1]$	FR dataset
4. Flexibility and resilience against shocks	Presence of escape clauses	$[0, 1]$	FR dataset
	Characteristics of escape clauses (activation procedures; conditions for invoking the clause; accountability mechanism)	$[0, 1]$	FR dataset
	Cyclical adjustment of the numerical constraint (adds to the escape-clause indicator where applicable)	—	FR dataset

Source: Authors' elaboration. *Notes:* The measurement criteria are based on Alonso *et al.* (2025). FR denotes fiscal rule.

Additional figures

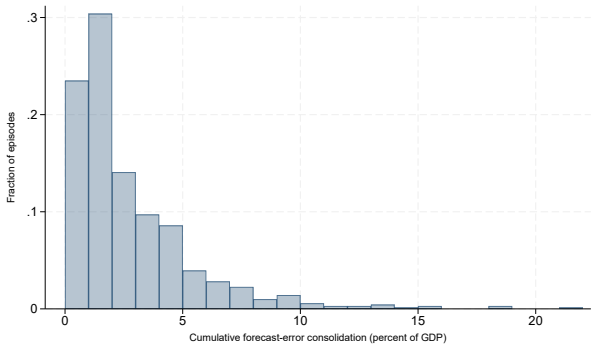
Figure A1: Raw and baseline fiscal balance forecast errors



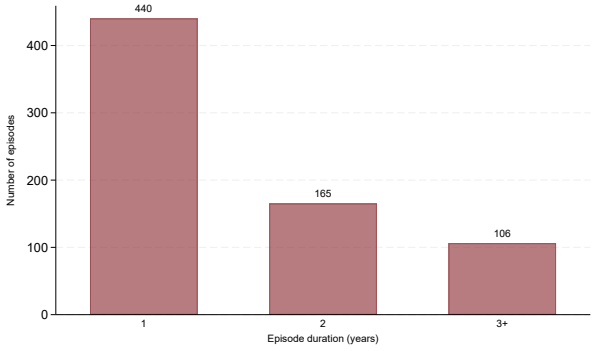
Source: Authors' elaboration. *Notes:* The figure reports kernel densities of the raw October-WEO forecast error in the annual change of the overall fiscal balance and the baseline forecast error after removing real-GDP-growth and inflation forecast errors and lagged macro-fiscal predictors. The vertical line marks the 0.5-percentage-point fiscal-consolidation threshold.

Figure A2: Magnitude and duration of fiscal-consolidation episodes

Panel A. Episode magnitude

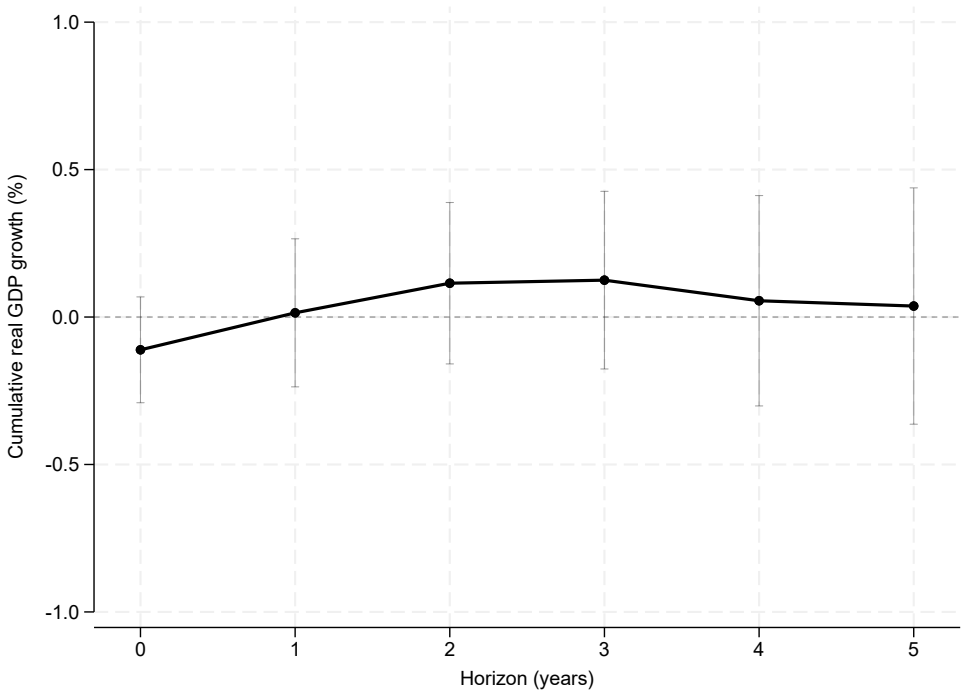


Panel B. Episode duration



Source: Authors' elaboration. *Notes:* Consecutive consolidation years within a country form one episode. Panel A reports the distribution of cumulative forecast-error magnitudes across the 711 episodes. Panel B reports the number of episodes lasting one year, two years, or three or more years. The broad sample contains 1,144 consolidation years over 1990–2019.

Figure A3: The response of real GDP to fiscal consolidation

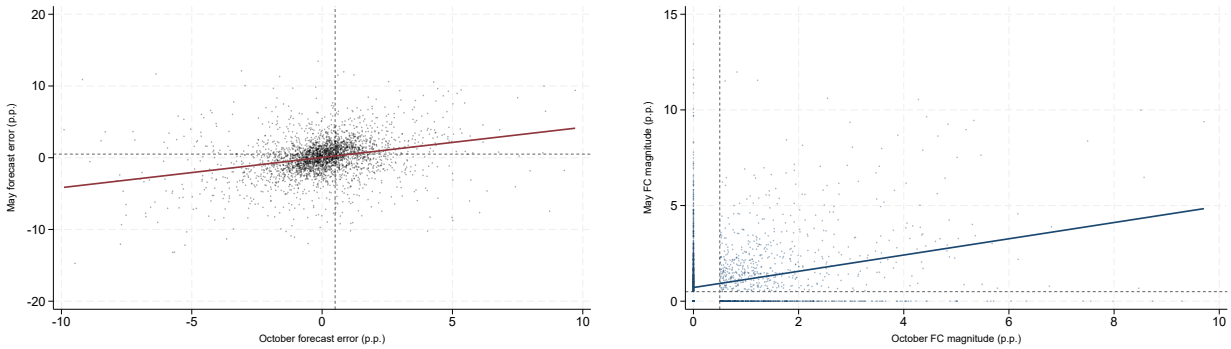


Source: Authors' elaboration. *Notes:* The dependent variable is cumulative real GDP growth from $t-1$ to $t+h$. The specification uses the fully controlled predetermined vector and country and year fixed effects, with every predetermined domestic state entering directly and interacted with the contemporaneous fiscal consolidation. Capped bars are 90 percent confidence intervals based on standard errors clustered by country.

Figure A4: May and October WEO forecast-error measures

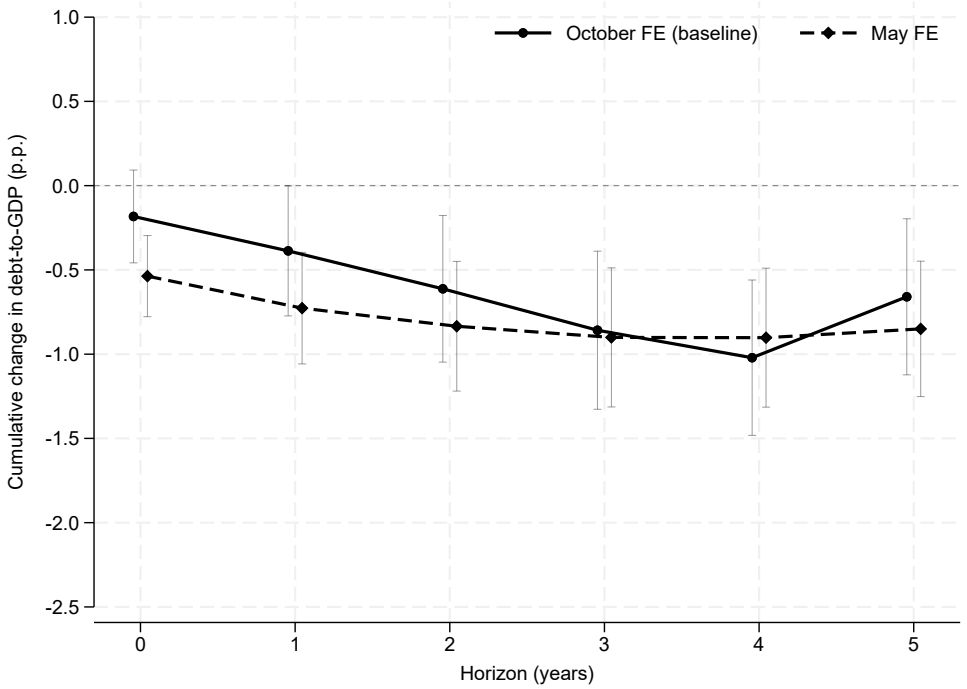
Panel A. Forecast errors

Panel B. Thresholded consolidations



Source: Authors' elaboration. *Notes:* The panels compare the May and October WEO constructions on 3,004 common country-year observations over 1994–2019. In both panels the correlation is 0.28. The Jaccard overlap of positive consolidation observations is 0.36; dashed lines mark the 0.5-percentage-point threshold.

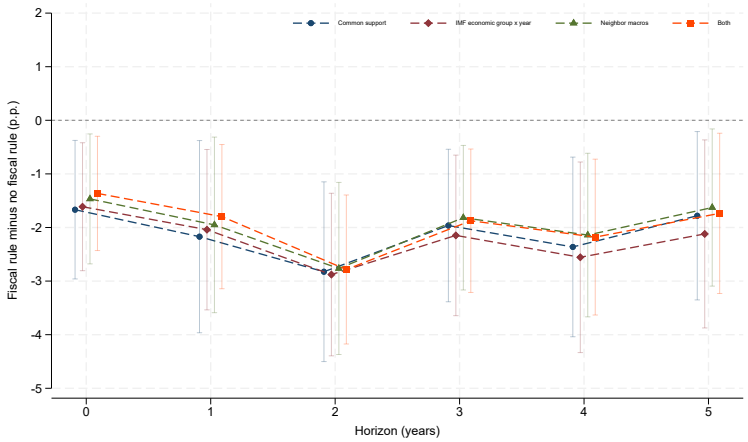
Figure A5: Alternative WEO forecast-error vintage



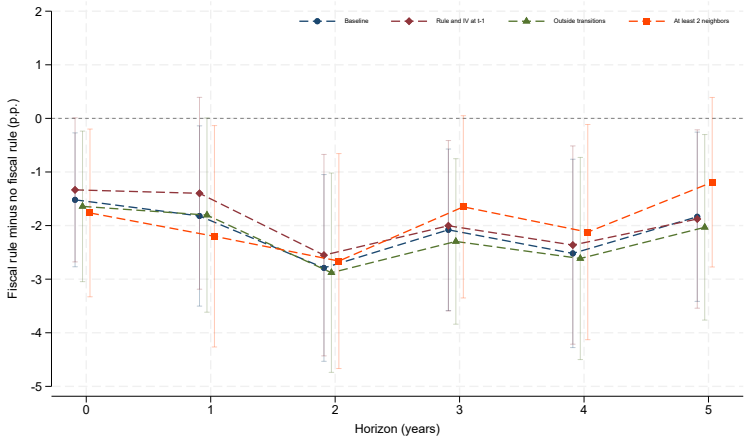
Source: Authors' elaboration. *Notes:* The figure compares fiscal consolidations constructed from the October WEO vintage used in the baseline with consolidations constructed from the May WEO vintage. Both series are estimated over their identical 1994–2019 support using the fully controlled specification and 138 countries. Capped bars are 90 percent confidence intervals based on standard errors clustered by country.

Figure A6: IV robustness

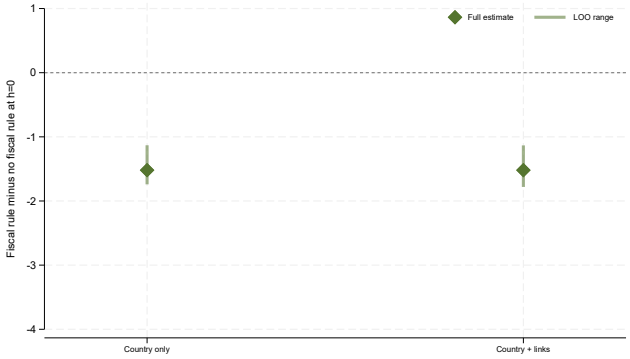
Panel A. Common-shock controls



Panel B. Timing and network support



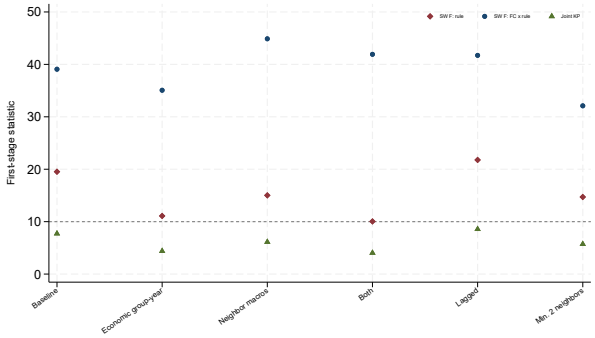
Panel C. Influence of countries and border links



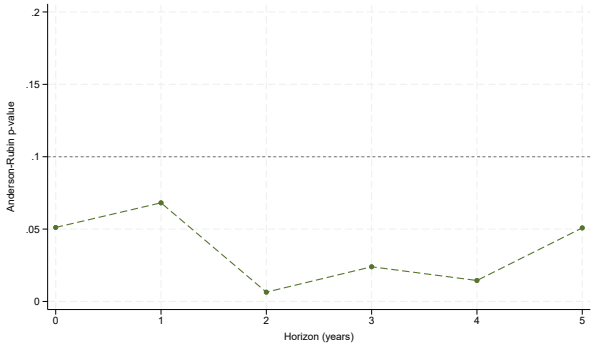
Source: Authors' elaboration. *Notes:* Panels A and B report the IV difference between the debt response with and without a fiscal rule under alternative common-shock controls, instrument timing, transition windows, and minimum network support. Panel C reports the exact range of the $h = 0$ coefficient from 124 conventional country-LOO estimations and 124 strict network-LOO estimations; the latter also removes the omitted country from every remaining country's neighbor exposure. Capped bars are 90 percent confidence intervals based on standard errors clustered by country.

Figure A7: Identification, weak-IV inference, and falsification exercises

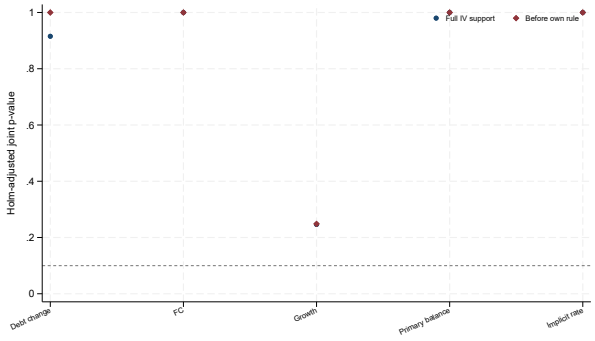
Panel A. Conditional first stages at $h = 0$



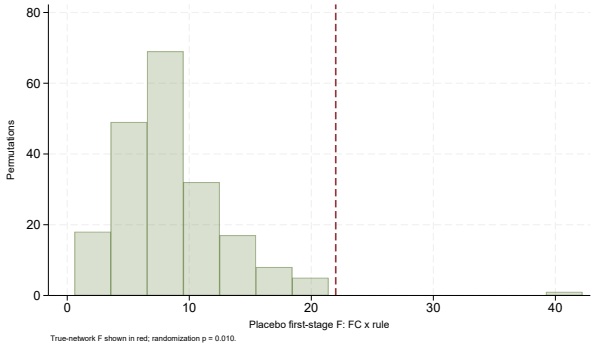
Panel B. Weak-IV-robust inference



Panel C. Predetermined-outcome placebos



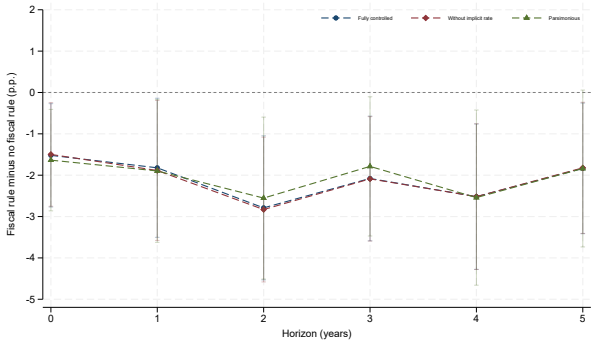
Panel D. Exposure permutation



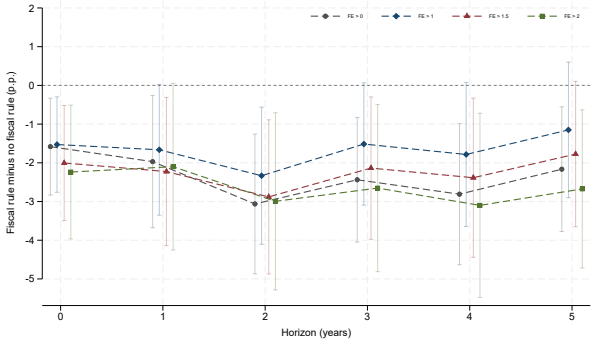
Source: Authors' elaboration. *Notes:* Panel A reports the conditional Sanderson–Windmeijer first-stage statistics and the joint cluster-robust Kleibergen–Paap statistic; the horizontal line at 10 is a visual reference, not a formal critical value. Panel B reports targeted Anderson–Rubin p-values. Panel C tests whether the excluded instruments predict outcomes dated $t-1$, conditional on states dated $t-2$; diamonds restrict the sample to observations before domestic rule adoption. The reported joint p-values are Holm-adjusted across outcomes. Panel D compares the observed first-stage statistic for the interaction with the distribution from 199 exposure reshufflings within IMF economic-group-by-year cells.

Figure A8: Additional IV specification checks

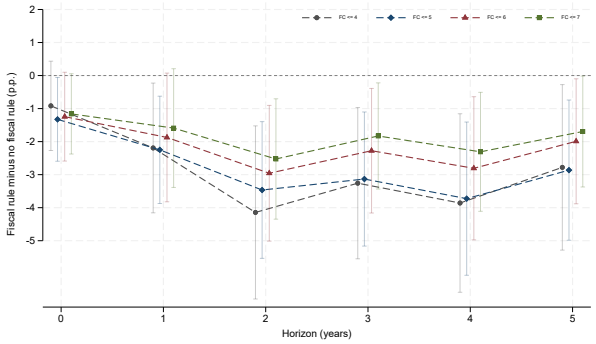
Panel A. Control sets



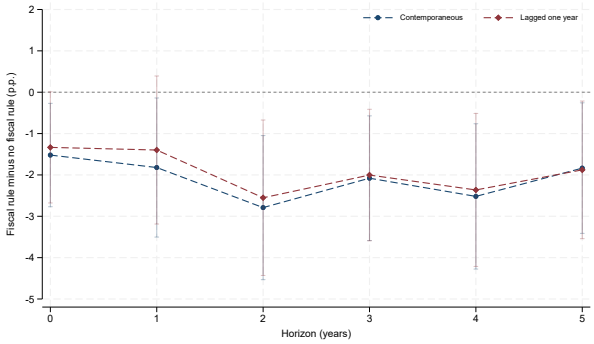
Panel B. Lower forecast-error thresholds



Panel C. Upper consolidation caps



Panel D. Instrument timing



Source: Authors' elaboration. *Notes:* Panel A varies the predetermined control set. Panel B varies the lower threshold used to define a fiscal consolidation. Panel C caps positive consolidations at alternative upper thresholds. Panel D compares contemporaneous rule and instrument timing with one-year-lagged timing. Outcomes, consolidation measures, and their associated interactions are reconstructed consistently within each exercise. Capped bars are 90 percent confidence intervals based on standard errors clustered by country. A separate two-way country-and-year clustering check leaves point estimates unchanged and yields p-values below 0.10 at every horizon.