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Central Bank Independence and Sovereign Borrowing

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Abstract

We study how reforms that tighten statutory limits on central-bank lending to government affect sovereign borrowing in 155 countries over 1972–2023. Using instrumental-variable local projections, we estimate their effects on public debt and real effective implicit interest rates. We find that in developing economies tighter constraints raise debt-to-GDP while reducing real borrowing costs. A median-sized reform temporarily increases debt-to-GDP by about 12 percentage points at its two-year peak, and lowers the real effective implicit interest rate by roughly 6 percentage points after five years, an effect that persists over the ten-year horizon. Foreign-held external public debt also rises persistently. Advanced economies show a smaller debt response of the opposite sign and no decline in borrowing costs. The real-rate decline persists under alternative debt denominators and relative to the United States. State-dependent estimates show that lower borrowing costs are strongest in high-inflation developing economies, consistent with statutory lending constraints improving sovereign borrowing conditions where credibility gains are greatest.

Keywords: central bank independence, government debt, interest rate, sovereign lending, central bank governance.

JEL classification: E58, G28, N20.

*The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the Central Bank of Ireland, the International Bank for Reconstruction and Development/World Bank, and its affiliated organisations, or those of the Executive Directors of the World Bank or the governments they represent.

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> "I want the *Banque* [*de France*] to be enough in the hands of the Government, but not too much. I do not ask that it lends money to the Government, but that it provides it with facilities."[1]
>
> *Napoleon Bonaparte, 1806*

1 Introduction

The relationship between central banks and fiscal authorities is at the heart of the debate on central bank independence. Historically, the first central banks were created as fiscal agents to the sovereign, mainly with the purpose of financing war spending.[2] In this context, central bank independence was completely absent. This close fiscal-monetary linkage remained largely unchallenged until the late 1980s, when institutional reforms began limiting central bank lending to the government. Figure 1 shows the distribution of reforms that increased (blue bars) or decreased (red bars) the conditions on central bank lending to the government over the past five decades, for a sample of 155 countries. The chart is based on an index measuring the legal provisions that shield the central bank from lending to the government. Throughout 1972-2023, reforms introducing tighter restrictions on lending to the government were steadily implemented. The independence of central banks in lending to the government accelerated in the late 1980s and early 1990s. However, we also notice a few cases that go in the opposite direction, reducing central bank independence.

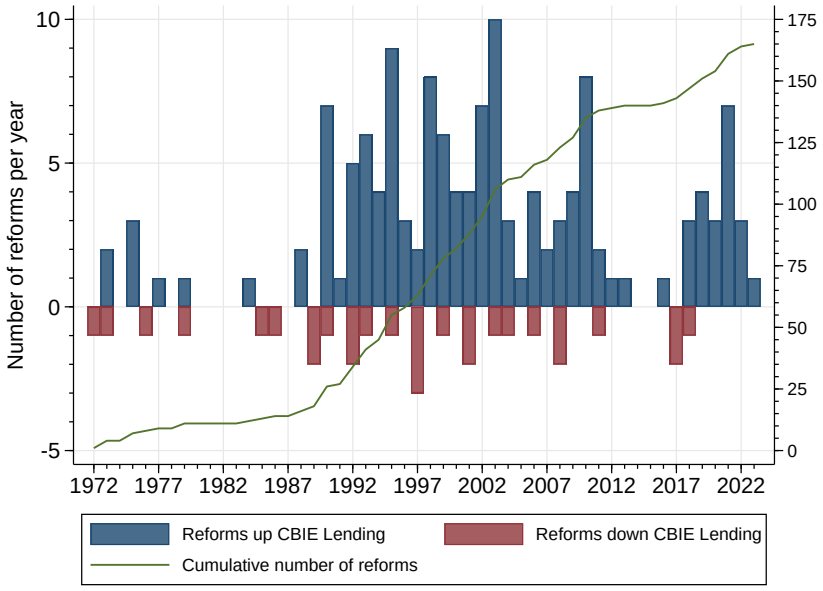
Against this backdrop, we examine how reforms that restrict central-bank lending to governments affect sovereign borrowing. Specifically, we study how reform-induced changes in statutory lending constraints affect the public-debt-to-GDP ratio and the real effective implicit interest rate paid on government debt. The latter measure is central to our interpretation because it captures the real cost of servicing the outstanding debt stock, rather than merely the nominal interest bill.

Whether central banks should finance government debt remains a contested policy ques-

[1] Original text: "Je veux que la Banque soit assez dans les mains du Gouvernement et n'y soit pas trop. Je ne demande pas qu'elle lui prête de l'argent, mais qu'elle lui procure des facilités," Ramon (1929).

[2] The Bank of England was established in 1694 with the purpose of waging the English government's war against France in the Low Countries (Encyclopædia Britannica, 2025). The Swedish Riksbank, established in 1668, was mainly used to finance war expenditure throughout the 17th and 18th centuries (Hendrickson, 2020). Napoleon Bonaparte, who spearheaded the creation of the Banque de France in 1800, later pressured the central bank to provide favorable lending to war contractors (Rouanet, 2021; Chadha and Newby, 2013). As Broz (1998, pp. 206) put it, "the main societal public good [of central banks] was fiscal in nature and involved improving government creditworthiness during wartime".

Figure 1: Reforms increasing or decreasing the central bank's lending limits to the government (1972-2023)



Note: The figure shows the frequency of reforms that decrease/increase lending limits to the government, together with the cumulative number of reforms between 1972 and 2023.
Source: Romelli (2022, 2025).

tion. Quantitative easing, rising sovereign-debt levels, and the coordinated monetary and fiscal expansions deployed during the COVID-19 pandemic reignited debate about the role of central banks as fiscal agents (Blanchard, 2023; Moschella, 2024; Adrian et al., 2024). Scholars have consequently re-examined the role of monetary financing in supporting government interventions (Blanchard, 2019; Galí, 2020; Blanchard and Pisani-Ferry, 2020; Adrian et al., 2021), including its potential use for *helicopter money* (Galí, 2020; Yashiv, 2020). Recent debates over central-bank governance further underscore that the boundary between monetary and fiscal policy remains under renewed scrutiny (Financial Times, 2026). These developments revive long-standing questions about monetary–fiscal coordination and about the conditions under which fiscal considerations come to dominate monetary policy (Leeper, 1991), particularly when the interest rate on public debt remains below the growth rate for extended periods (Blanchard, 2019).

However, existing studies on the link between central bank independence and sovereign debt dynamics present mixed findings. Previous literature found that higher central bank independence leads to lower debt (Grilli et al., 1991; von Hagen and Harden, 1995), government deficits (Grilli et al., 1991; Bodea, 2013a; Bodea and Higashijima, 2017), and monetary

financing (Hooley et al., 2021). These results, though, hold only in countries with strong democratic institutions, such as the rule of law or constraints on executive power (Bodea and Higashijima, 2017; Jung et al., 2025a). Other studies found less clear results, such as no significant relationship between central bank independence and government deficits (Sikken and de Haan, 1998) or interest rates (Alesina and Summers, 1993). Another study found that the lack of central bank independence in developing countries does not result in monetization of election-related fiscal expansions (Alpanda and Honig, 2009).

A key common factor among these analyses is that many approaches rely on a broad index of central bank independence, which encompasses a wide range of elements beyond the specific restrictions on monetary financing of public debt. For example, the index developed by Cukierman et al. (1992), which remains the most widely used in the literature, incorporates dimensions of central bank independence that are not strictly related to monetary financing, such as the central bank's objectives and the procedures for appointing or dismissing its governor. According to this index, only 50 percent of the criteria are strictly related to lending to the government.

We expand on previous studies by focusing on independence specifically in terms of the limitations of the central bank's lending to the government. To this end, we use an index of central bank independence that focuses solely on the restrictions around the central bank's ability to finance sovereign debt. This index is based on the Central Bank Independence - Extended (CBIE) index of Romelli (2025), which builds on existing indices of central bank independence to develop the most comprehensive index of independence. In doing so, we focus only on aspects related to monetary financing, using the CBIE Lending sub-index, which measures central bank independence in terms of lending to the government. For example, this indicator captures whether legal limits exist on central bank lending and which institution determines the government's financing conditions. This index allows us to evaluate the benefits that stem specifically from reforms in central banks' independence in lending to the government.

From a theoretical perspective, this raises the question of what we should expect such reforms to do. Holding the fiscal stance unchanged, the *direct* effect of restricting central bank lending requires the government to turn to private financial markets for financing. In theory, this additional demand for private funding should put upward pressure on the cost of government debt. In settings in which seignorage is an important source of public revenue,

tighter limits on monetary financing can require greater reliance on interest-bearing market debt and, when demand for government paper is not perfectly elastic, raise the interest rate required to place that debt (Cottarelli, 1993; Coutinho, 1989). This channel is less relevant when the monetary base is a small share of government liabilities and when interest is paid on reserves. Working in the opposite direction are the *indirect* effects that operate through the enhanced credibility of the central bank. To the extent that investors perceive the reduction in monetisation as lowering the risk that debt will be inflated away, tighter restrictions on central bank lending can increase the supply of private funding to the government and lower the interest rate it is required to pay. Because the two channels push borrowing costs in opposite directions, the net effect of restricting central bank lending is *a priori* ambiguous, and the question is ultimately an empirical one. Which channel dominates should depend on how much credibility a reform can buy. Where monetary financing has historically been large and inflation high, investors attach a substantial probability to the debt being inflated away, so a legal commitment not to monetise lowers the compensation they require and the indirect effect should prevail. Where the central bank is already credible, and inflation is low, little monetization risk is left to remove, so the direct effect of losing access to central bank credit should prevail, and borrowing costs should, if anything, rise. The real effective implicit interest rate is the natural place to adjudicate between the two, because it measures the real cost of servicing the outstanding debt stock and is not mechanically affected by the inflation compensation embedded in nominal rates.

Nevertheless, it remains an empirical question which of these forces are more prevalent. To estimate these effects on 155 countries over 1972–2023, we use instrumental-variable local projections. This approach allows us to assess not only the magnitude of the effect but also its duration over time.

We find a sharp divergence across country groups. In developing economies, tighter restrictions on central-bank lending to government raise debt-to-GDP and lower real borrowing costs. For a median-sized reform, i.e., an increase of 0.34 in the 0–1 CBIE Lending index, the debt-to-GDP ratio rises by about 12 percentage points at its two-year peak before gradually receding, while the real effective implicit interest rate falls by about 6 percentage points after five years and remains lower over the ten-year horizon. In advanced economies, by contrast, debt-to-GDP falls modestly and temporarily, while real borrowing costs show no economically meaningful change. In developing economies, the decline in borrowing costs is

strongest in high-inflation countries, and foreign-held external public debt rises persistently, by roughly 10 percentage points of GDP at its peak, consistent with improved access to external creditors. The real-rate decline persists under alternative debt denominators and relative to the United States, and non-IV local projections point in the same direction.

These estimates are consistent with the credibility channel dominating the direct quantitative effect: stricter rules on central bank lending improve borrowing conditions for developing countries, which are able to carry more debt at a lower real cost. The contrast across country groups is consistent with this reading. In advanced economies, where there is little credibility to gain, the real effective rate does not fall and drifts slightly upward, while among developing economies the decline is strongest in countries with high inflation. By committing not to finance the government, the sovereign gains credibility as a borrower. Investors, more confident that the government is constrained from inflating debt away through monetisation, may be more willing to hold sovereign debt and require a lower return, which is consistent with the persistent decline in the real effective implicit interest rate. Faced with a durably cheaper real cost of borrowing, the government has more room to issue debt than it would have absent the reform, even though the debt ratio itself eventually reverts towards its pre-reform path. Read this way, the transitory debt expansion and the persistent fall in the real cost of servicing debt can be seen as two sides of the same credibility effect, rather than two separate findings.

Our paper contributes to three related strands of literature on central-bank institutions and sovereign borrowing. First, it contributes to work on central-bank independence and fiscal outcomes. The long-standing CBI literature focuses primarily on inflation and monetary discipline (Alesina and Gatti, 1995; Loungani and Sheets, 1997; Cukierman et al., 1998; Cukierman and Lippi, 1999; Hayo, 1998; Lim, 2021; Athanasopoulos et al., 2025; Jung et al., 2025b), while a more limited literature studies public debt, deficits and monetary financing (Grilli et al., 1991; von Hagen and Harden, 1995; Sikken and de Haan, 1998; Bodea, 2013b; Bodea and Higashijima, 2017; Alpanda and Honig, 2009; Hooley et al., 2021). Recent work moves closer to sovereign financing conditions: Nöh (2019) studies debt maturity, while Alter et al. (2026) finds that higher de jure CBI is associated with lower local-currency sovereign yields in emerging and developing economies. We complement this work by isolating one specific institutional margin, i.e., legal restrictions on central-bank lending to the government, and studying reform-induced changes in the debt stock, the real effective

implicit cost of servicing that debt, and external public borrowing over a long international panel.

Second, the paper relates to the literature on debt intolerance, limited commitment, reputation, and institutional graduation. Reinhart et al. (2003) argue that countries with adverse default and inflation histories can face financing stress at debt ratios that are manageable for more credible borrowers. Aguiar et al. (2013) show how weak inflation credibility can raise tranquil-period borrowing costs and alter sovereign borrowing limits and crisis vulnerability. Frankel et al. (2013) document that improvements in institutional quality can allow developing economies to graduate from pro-cyclical fiscal policy. Morelli and Moretti (2023) show that sovereign reputation is priced in spreads, while Schreger et al. (2023) show that stronger central-bank credibility can have non-monotonic effects on debt issuance because it both increases real debt capacity and changes fiscal incentives. In general, the literature has found that independence increases the credibility of central bank monetary policy (Blinder, 2000; Garriga and Rodriguez, 2020; Du et al., 2020a; Reis, 2019, 2022; Willems and Zettelmeyer, 2022; Romelli, 2022; Jung et al., 2025a). Our results speak to this literature by providing dynamic evidence that a specific legal commitment device is followed, in developing economies, by lower real debt-servicing costs and greater borrowing from external creditors. This evidence is consistent with greater debt-carrying capacity, but we do not directly estimate a sustainable debt limit or fiscal reaction function.

Third, our paper relates to the literature on original sin and the ability of emerging-market sovereigns to attract financing in forms that reduce monetary and currency risk. Burger and Warnock (2006) link stable inflation and stronger institutions to deeper local-currency bond markets. Du et al. (2020b) provide a particularly close mechanism: without monetary-policy commitment, investors price the government's incentive to inflate into nominal local-currency debt, raising its required premium and affecting debt composition. Du and Schreger (2022), Onen et al. (2025), and Han et al. (2026) document the subsequent, but uneven, expansion of emerging-market local-currency borrowing and foreign participation. Our external-debt evidence adds a related quantity margin: after reforms that tighten central-bank lending restrictions, foreign-held external public debt rises persistently.

The rest of the paper is organized as follows. In section 2, we describe our data, while section 3 presents the empirical strategies employed in the paper. Section 4 presents the results, and section 5 concludes.

2 Data and descriptive statistics

This section describes the data used in the analysis. The index of central bank independence used in the paper is presented in Section 2.1. Section 2.2 discusses our dependent variables, i.e., the debt-to-GDP ratio and the interest rate on government debt, while Section 2.3 describes the additional control variables used in the analysis.

2.1 Index of central bank lending to the government

In this paper, we use information from the Central Bank Independence - Extended (CBIE) index introduced in Romelli (2022), and updated by Romelli (2025) for a sample of 155 countries over the period 1972 to 2023.[3] This index combines the classical indices of central bank independence, i.e., the Grilli et al. (1991) and Cukierman et al. (1992) indices, to develop a comprehensive index of central bank independence that covers a wider range of central bank characteristics. In its most disaggregated format, this new index provides information on 42 criteria of central bank institutional design.

In our empirical analysis, we use the sub-index of CBIE that refers to the limitations on central bank lending to the government (CBIE Lending, hereafter). This index captures the legal framework governing central bank lending to the government, based on eight key criteria: i) restrictions on advances to the government; ii) limitations on central bank purchases of government securities in the primary or secondary market; iii) constraints on the central bank's ability to trade in these markets; iv) authority over the terms and conditions of lending; v) the scope of government-related entities eligible to receive central bank credit; vi) the legal definition of quantitative limits on such lending; vii) the maturity of the loans; and viii) the applicable interest rate conditions. Exact coding rules are defined for each category, and a rating between zero and one is assigned to each criterion. We provide the details on the coding of each criterion used in the construction of the sub-index on limitations on lending to the government in Appendix B. The degree of CBIE Lending is computed as the average of these criteria and its value ranges from 0 (lowest independence and loose limits) to 1 (maximum independence and stricter constraints).[4]

Table 1 provides an overview of the evolution of the CBIE Lending index over time. As central banks became increasingly independent, the limitations on lending to the government

[3] See Appendix Table A for the full set of countries and information on data availability.

[4] These criteria are also those reported in the 'Limitations on lending to the government' sub-index of the widely used Cukierman et al. (1992) index of CBI.

increased. The average level of the index rises over the period of our analysis, after a slight dip in the 1980s. The limited number of reversals underscores the broad-based acceptance of the need to restrict government monetary financing across the world.

Table 1: Summary statistics on the evolution of the CBIE Lending index

Period	# of Countries	# of Obs	# of Reforms	# of Reversals	Mean	Min	Max
Full sample							
1972-1979	93	719	7	4	0.396	0	1
1980-1989	110	1034	3	4	0.385	0	1
1990-1999	149	1365	51	9	0.449	0	1
2000-2009	155	1539	42	7	0.577	0	1
2010-2019	155	1550	20	4	0.644	0	1
2020-2023	155	620	14	0	0.686	0	1
Advanced economies							
1972-1979	25	193	3	1	0.36	0	0.938
1980-1989	26	257	1	3	0.344	0	0.938
1990-1999	34	321	16	0	0.485	0	1
2000-2009	34	340	8	0	0.735	0	1
2010-2019	34	340	1	0	0.768	0	1
2020-2023	34	136	0	0	0.788	0	1
Developing countries							
1972-1979	68	526	4	3	0.409	0	1
1980-1989	84	777	2	1	0.398	0	1
1990-1999	115	1044	35	9	0.438	0	1
2000-2009	121	1199	34	7	0.533	0	1
2010-2019	121	1210	19	4	0.61	0	1
2020-2023	121	484	14	0	0.658	0	1

Notes: Table provides summary statistics for the CBIE Lending index of limitation on central bank lending to the government from Romelli (2022, 2025). The sample of Advanced economies follows the IMF World Economic Outlook classification of advanced economies. All remaining countries in the sample are classified as Developing.

2.2 Government debt and interest rates

Our main outcomes of interest are the government debt-to-GDP ratio and the real effective implicit interest rate on government debt. With these variables, we can explore whether restricting the central bank's ability to finance the government affects both the size of public debt and the real cost of servicing that debt. We measure gross public debt as a share of GDP using the IMF Historical Public Debt Database (Abbas et al., 2010), extended to the most recent years with general government gross debt from the IMF World Economic Outlook.[5] Data on interest expenses on public debt are from the Public Finances in Modern

[5]The vintage of the Historical Public Debt Database that we use ends in 2019, so all observations from 2020 onwards are taken from the World Economic Outlook. Isolated missing years within a country series are filled by linear interpolation between adjacent observations, which affects less than 1 percent of the

History database of the IMF (Mauro et al., 2015). Crossing this information with the CBIE Lending indicator allows us to cover 153 countries for the debt-to-GDP ratio. Coverage falls to 125 countries for the implicit interest rate, which additionally requires data on interest expenses. Our interest-rate outcome is the real effective implicit interest rate, which we obtain by applying the Fisher adjustment to the nominal implicit rate $r^{nom}_{c,t}$:

$$r^{real}_{c,t} = 100 \left[(1 + r^{nom}_{c,t}/100)/(1 + \pi_{c,t}/100) - 1 \right] \tag{1}$$

where $\pi_{c,t}$ is the CPI inflation rate. The nominal implicit rate is the ratio between government interest expenses in year t and the average of the public debt stock at the end of years t and $t-1$.[6] Both series are expressed as percentages of GDP, so the GDP denominator cancels out. This real measure captures the average real cost of the outstanding debt stock. We keep the nominal implicit interest rate, alternative debt denominators, and implicit-rate spreads relative to the United States as comparison and robustness outcomes.

Three considerations motivate the choice of the real effective implicit rate as our main interest-rate outcome.

First, the object that determines fiscal space is the real cost of servicing debt. In the standard debt-accumulation identity, the evolution of the debt ratio depends on the differential between the real interest rate and the real growth rate, so it is the real rate, and not the nominal interest bill, that enters the arithmetic of debt sustainability (Ghosh et al., 2013; Blanchard, 2019). Since our question is whether restrictions on central bank lending expand a government's capacity to carry debt, the real rate is the quantity to which the theory speaks.

Second, the nominal implicit rate mechanically embeds inflation compensation. The implicit rate is the ratio of interest expenditure to the outstanding stock of debt, and in inflationary periods coupon payments include a component that compensates creditors for the erosion of the real value of principal. In our sample inflation ranges from deflation to episodes of hyperinflation, so nominal implicit rates observed in different countries and periods contain very different inflation-compensation components and are not directly com-

observations in the database. The World Economic Outlook series refers to general government, while sector coverage in the Historical Public Debt Database varies across countries and years.

[6] We average the debt stock because interest paid during a year accrues on the debt outstanding over that year, as in the Eurostat convention for the apparent cost of government debt, and because the end-of-period stock responds to the reforms we study, so scaling by it would mechanically depress the measured rate at exactly the horizons where the debt response is largest. Section 4.6 reports the lagged stock and other denominators, and the real-rate response is similar across them.

parable.

Third, and most important for our design, the treatment itself moves the deflator. Reforms that restrict central bank lending are associated with lower inflation in the literature on central bank independence, and our own estimates point the same way for the price level (Appendix Figure F.4), although the response mainly reflects high-inflation episodes. The response of the nominal implicit rate therefore combines a change in the real cost of borrowing with a change in the inflation compensation embedded in interest payments, and cannot on its own be read as a measure of the change in borrowing conditions. Deflating by realised inflation isolates the first component, which is the object of interest.

2.3 Control variables

To capture possible confounding effects, we include a set of additional control variables. These controls can be grouped into three main categories: business cycle, economic openness, and institutional factors.[7]

First, to capture business-cycle effects, we include real GDP growth and CPI inflation. We further control for three expenditure measures: government expenditure, gross fixed capital formation, and final consumption expenditure, each expressed as a percentage of GDP, since government spending and investment cycles can mechanically drive public-debt ratios.

Second, we account for a country's openness and external financing environment by including the following indicators: a country's degree of openness to trade, net FDI inflows (% of GDP), exchange rate stability index, and the Chinn–Ito index of de jure financial openness.

Finally, to account for the level of institutional quality, we include two additional variables. First, the overall Polity Score index, which measures the strength of democratic institutions, with lower values indicating a more autocratic regime. Second, the index of legal constraints on the executive, previously used in Acemoglu et al. (2008) to measure the potential interference of governments on central banks. Both indicators are from the Polity IV dataset (PolityIV, 2014). For the executive-constraints index, we set the Polity special codes for foreign interruption, interregnum, and transition periods to missing, so that only the 1–7 institutional scale enters the analysis. These institutional covariates ensure that our

[7] See Appendix Table C.1 for definitions and data sources. Appendix Table C.2 reports summary statistics.

estimates are not simply a reflection of different institutional regimes.

3 Empirical Strategy

This section outlines our empirical strategy aimed at investigating the long-run effects of reforms that tighten controls on central bank lending to the government on the debt-to-GDP ratio and the real effective implicit interest rate on government debt.

3.1 Instrumental-variable local projections

We trace the dynamic effects of these reforms using local projections (Jordà, 2005), which estimate the response of an outcome at each horizon separately by projecting outcomes at $t+h$ on information dated $t-1$. Specifically, we estimate the following system of h equations:

$$y_{c,t+h} - y_{c,t-1} = \alpha_c^h + \gamma_t^h + \Lambda^h \Delta CBIEL_{c,t-1}^{Reform} + \sum_{l=1}^{p} \beta_{1lh}(y_{c,t-l} - y_{c,t-1-l}) + \\ + \sum_{f=1}^{h} \beta_{2fh} \Delta CBIEL_{c,t+f}^{Reform} + \beta_{3h}' X_{c,t} + u_{c,t+h} \tag{2}$$

where c indexes the countries in our sample and $h = 0, 1, \ldots, 10$ the forecast horizons. The dependent variable, y, is the cumulative change in the outcome between *t-1* and *t+h*. The outcome variable is the government debt-to-GDP ratio, the real effective implicit interest rate on government debt, or the government interest-expense ratio depending on the specification. In the extensions, the outcome is instead foreign-held external public debt, the price level, the nominal implicit rate or an implicit-rate spread relative to the United States.

The treatment variable, $\Delta CBIEL_{c,t}^{Reform}$, is a reform episode that changes the legal constraints on central bank lending to the government. It is defined as the change in the CBIE Lending index implied by the most recent reform in country c. Once a reform occurs, the magnitude of the change remains in place in subsequent years until a new reform updates the index, or, in the case of a reversal, moves it in the opposite direction. The variable is zero before a country's first reform. This persistent-treatment coding is the natural representation of an institutional change that stays on the books until it is amended, and it follows the treatment of major institutional reforms in Acemoglu et al. (2008). It differs

from a reform-year dummy, which would set the treatment back to zero immediately after the reform year even though the legal provisions remain in force, and from the level of the index, which compares countries with different institutional histories rather than isolating changes in central bank design. In our sample, 114 of the 155 countries have reformed their degree of CBIE Lending since 1972, implementing 137 reforms that tightened restrictions on lending to the government and 28 reforms that relaxed them. We distinguish advanced economies, following the IMF World Economic Outlook classification (34 countries in our sample), from developing countries (the remaining 121). Appendix D presents the evolution of the index for the countries in our sample. The reform variable enters with one lag, so that Λ^h traces the response of the outcome between $t-1$ and $t+h$ to the reform-induced change in CBIE Lending observed at $t-1$. The lag order of the differenced outcome variables is fixed at $p = 4$ at every horizon. In the debt-to-GDP equation this means conditioning on past debt dynamics, while in the interest-rate equation we include four lags of the change in the real effective implicit rate and four lags of the change in debt-to-GDP, so that debt dynamics can affect borrowing costs over the forecast window.

Following Teulings and Zubanov (2014), we also include leads of the treatment up to the relevant horizon to absorb reforms that occur between the impulse and the horizon being estimated. Country-specific intercepts, α_c^h, absorb time-invariant country characteristics, while year fixed effects, γ_t^h, account for factors common across countries in a given year. The vector $X_{c,t}$ collects the time-varying controls described in Section 2.3, which enter contemporaneously in each horizon's regression. Inference is based on spatial correlation-corrected standard errors (Driscoll and Kraay, 1998).[8]

A key challenge is that reforms in central bank design are unlikely to be strictly exogenous. Identifying Λ^h by ordinary least squares would require $\mathbb{E}[u_{c,t+h} \mid \Delta CBIEL_{c,t-1}^{Reform}, X_{c,t}] = 0$, which is implausible in this setting. As Romelli (2022) shows, reform episodes are systematically related to the institutional status quo, convergence towards regional peers, and external pressures such as IMF programs or inflation crises. Since these factors plausibly affect sovereign borrowing directly, we estimate an instrumental-variable local projection (Jordà et al., 2015), instrumenting the lagged reform variable with predetermined drivers of institutional change. This is the specification behind our baseline results, and it mirrors

[8]Following Montiel Olea and Plagborg-Møller (2021), lag augmentation removes the need to correct for serial correlation in local-projection residuals. We nevertheless use Driscoll–Kraay standard errors to allow for cross-sectional dependence; conventional heteroskedasticity-robust inference yields similar conclusions.

the identification strategy in Athanasopoulos et al. (2025).

For each horizon h, we estimate a two-stage least squares system with the same lags, leads, controls, and fixed effects as in equation (2). The first stage is:

$$\Delta CBIEL_{c,t-1}^{Reform} = \phi_c^h + \tau_t^h + \theta^{h\prime} Z_{c,t-1} + \sum_{l=1}^{p} \kappa_{1lh}(y_{c,t-l} - y_{c,t-1-l}) + \\ + \sum_{f=1}^{h} \kappa_{2fh} \Delta CBIEL_{c,t+f}^{Reform} + \kappa_{3h}' X_{c,t} + v_{c,t-1}^h \tag{3}$$

where $Z_{c,t-1}$ contains the four excluded instruments: (i) the level of the CBIE Lending index lagged two years, (ii) regional reform pressure lagged two years, measured as the gap between the country's CBIE Lending level and the 75th percentile of the index within its region,[9] (iii) an indicator equal to one if the country was under an IMF program in the previous two years, and (iv) an indicator equal to one if the country experienced an inflation crisis, defined as annual inflation above 30 percent, in the previous two years.

Identification rests on the standard instrumental-variable assumptions of instrument relevance and exclusion restriction: the instruments must predict reform adoption, and must affect sovereign borrowing only through the reform. The lagged level of CBIE Lending captures persistence in the institutional status quo, since countries with weaker existing restrictions have more room to tighten them. Regional pressure captures peer effects in institutional design: Romelli (2022) shows empirically that regional convergence is a significant driver of reforms in central bank independence, which supports the relevance of this instrument. The exclusion restriction requires the further assumption that, conditional on the controls and the fixed effects, a country's distance from its regional peers affects its debt or borrowing costs only through its effect on reform incentives, via imitation or competitive pressure, and not directly. The IMF-program and inflation-crisis indicators capture episodes of external pressure and macroeconomic stabilization that trigger legal reform, and are assumed to affect the outcomes over the horizons considered only through their effect on reform adoption. Both are dated in the two years preceding the treatment, and the specification conditions on year fixed effects, so that global crisis dynamics common across countries are absorbed.

We emphasize that the only *instruments excluded* are the four predetermined variables

[9] Regional peer groups are identified using the classification in Acemoglu et al. (2019).

in $Z_{c,t-1}$. The leads of the reform variable, $\sum_{f=1}^{h} \Delta CBIEL_{c,t+f}^{Reform}$, are *not* instruments. They enter as control variables in both stages, exactly as in equation (2).

The second stage is:

$$y_{c,t+h} - y_{c,t-1} = \alpha_c^h + \gamma_t^h + \Lambda^h \Delta \widehat{CBIEL}_{c,t-1}^{Reform} + \sum_{l=1}^{p} \beta_{1lh}(y_{c,t-l} - y_{c,t-1-l}) + \\ + \sum_{f=1}^{h} \beta_{2fh} \Delta CBIEL_{c,t+f}^{Reform} + \beta_{3h}' X_{c,t} + u_{c,t+h} \tag{4}$$

We estimate equations (3) and (4) horizon by horizon up to $h = 10$, and report the Kleibergen–Paap statistic at $h = 0$ as a check on instrument strength. The first-stage estimates are reported in Appendix Table E.1.

3.2 State-dependent IV local projections

The specification above assumes that the effects of tighter restrictions on lending to the government are the same on average across countries. These effects might instead be state-dependent, that is, conditional on the macroeconomic environment in which the reform takes place. Local projections provide a flexible framework to test these hypotheses.[10]

We estimate state dependence in interaction form. Letting $S_{c,t}$ denote a dummy variable indicating the state of the economy, the treatment is split into its high-state and low-state components within a single regression, which allows the response to differ across states while delivering a formal test of whether the two responses are equal. Equation 5 presents the specification:

$$y_{c,t+h} - y_{c,t-1} = \alpha_c^h + \gamma_t^h + \Lambda_H^h \, S_{c,t} \, \Delta \widehat{CBIEL}_{c,t-1}^{Reform} + \Lambda_L^h \, (1 - S_{c,t}) \, \Delta \widehat{CBIEL}_{c,t-1}^{Reform} + \delta^h S_{c,t} \\ + \sum_{l=1}^{p} \left[\beta_{1lh}^H S_{c,t} + \beta_{1lh}^L (1 - S_{c,t}) \right] (y_{c,t-l} - y_{c,t-1-l}) \\ + \sum_{f=1}^{h} \beta_{2fh} \Delta CBIEL_{c,t+f}^{Reform} + \beta_{3h}' X_{c,t} + u_{c,t+h} \tag{5}$$

Each of the two treatment terms is instrumented with the corresponding state interaction

[10] State-dependent local projections have recently gained popularity in macroeconomic research and have been extensively applied, for example, to analyzing the effects of austerity (Auerbach and Gorodnichenko, 2012; Ramey and Zubairy, 2018). de Haan and Wiese (2022) applied state-dependent models to estimate the effects of labor and product market reforms on economic growth.

of the instrument set $Z_{c,t-1}$, so that identification within a state uses the same predetermined drivers of reform as the baseline. The lags of the differenced outcome are also allowed to differ across states, while the controls, the leads and the fixed effects are common.[11] The coefficients Λ_H^h and Λ_L^h are the state-specific responses, and at each horizon we test $H_0 : \Lambda_H^h = \Lambda_L^h$. This test is the object of interest when asking whether the effect of reform genuinely differs across states, and it is not available when the two states are estimated on separate subsamples. We consider three state variables: high versus low inflation, whether the country was under an IMF programme, and high versus low debt, and we complement them with an institutional split by whether the country ever adopted inflation targeting.

3.3 Alternative specifications

We report three alternatives to the baseline, each estimated with the same cumulative outcomes, four fixed lags, leads, controls and year fixed effects.

The first estimates equation (2) directly by ordinary least squares, without instrumenting the reform variable. Comparing it with the instrumented estimates shows how much the endogeneity of reform adoption matters for the results.

The second replaces the continuous treatment with a dummy that switches on after the country's largest positive reform above the post-1972 median reform size and remains on thereafter, so that the response is identified by the timing of major reform episodes rather than by their magnitude.

The third addresses the coding of the treatment itself. Because $\Delta CBIEL_{c,t}^{Reform}$ records the magnitude of the legal change, the estimates could in principle be driven by the measured size of reforms rather than by their occurrence. We therefore re-estimate the baseline instrumented specification replacing the treatment with a signed reform indicator, equal to $+1$ after a reform that tightens lending restrictions and -1 after a reform that loosens them, carried forward on the same convention and zero before a country's first reform. This retains the direction of the institutional change while discarding its magnitude, so that comparing the two isolates the contribution of reform size.

[11] The inflation, debt and inflation-targeting states are country-level classifications, so in those three splits the state main effect $\delta^h S_{c,t}$ is collinear with the country fixed effects and is absorbed.

4 Results

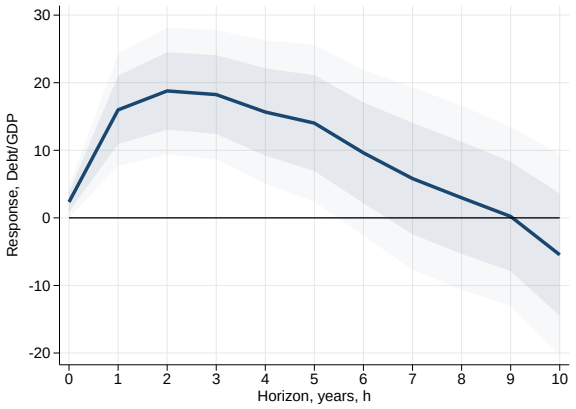
This section begins with the baseline IV local-projection estimates. As described in Section 3.1, these estimates instrument the reform-induced change in CBIE Lending before tracing its dynamic effects on debt-to-GDP and the real effective implicit interest rate. We first present the evidence for the full sample and then compare the responses in advanced and developing economies. This comparison motivates the focus on developing economies in the state-dependent analysis and robustness exercises that follow.

4.1 Baseline estimates

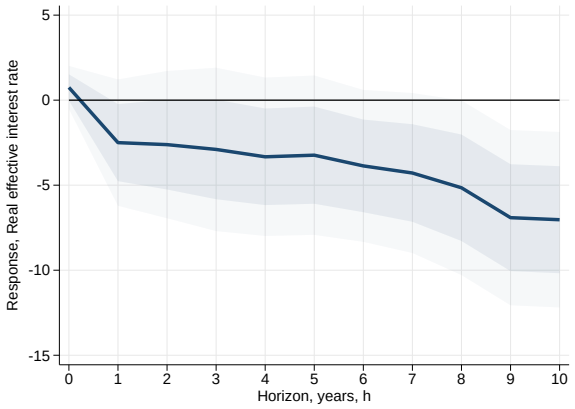
Figure 2 presents the preferred IV local-projection estimates for the full sample. Tighter legal restrictions on central bank lending are followed by a temporary increase in debt-to-GDP and a gradual decline in the real effective implicit interest rate that becomes statistically significant only at longer horizons. The pooled estimates therefore point to improved borrowing conditions, but they do not reveal whether this pattern is shared across groups of countries.

Figure 2: IV local projections, all countries, 1972–2023.

(a) Debt-to-GDP



(b) Real effective implicit interest rate



Notes: The figure shows the cumulative response of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for all countries, at horizon h (in years) on the x-axis. The reform variable is the change in CBIE Lending implied by a legal reform, carried forward until a subsequent reform or reversal. It is instrumented using lagged CBIE Lending, lagged regional pressure, IMF-program exposure in the previous two years, and inflation-crisis exposure in the previous two years, as described in Section 3.1. Responses are expressed per unit change in the 0–1 CBIE Lending index. The median positive reform in the full sample is 0.34. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

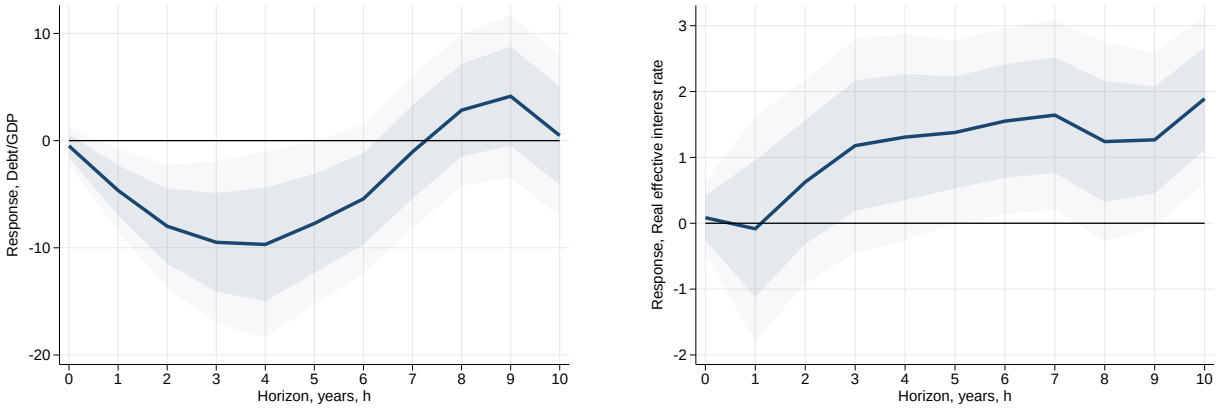
Figures 3 and 4 show that the pooled pattern masks a marked divergence between

advanced and developing economies. In advanced economies, for a reform of the full-sample median size (0.34 on the 0–1 index), debt-to-GDP falls temporarily by about 3 percentage points at its trough three to four years after the reform, before returning to zero. The real effective implicit interest rate instead drifts slightly upward, reaching about 0.7 percentage points at the ten-year horizon. Although it is marginally significant at some longer horizons, the effect is economically small. These responses do not support an expansion in debt-carrying capacity in advanced economies.

Figure 3: IV local projections, advanced economies, 1972–2023.

(a) Debt-to-GDP

(b) Real effective implicit interest rate

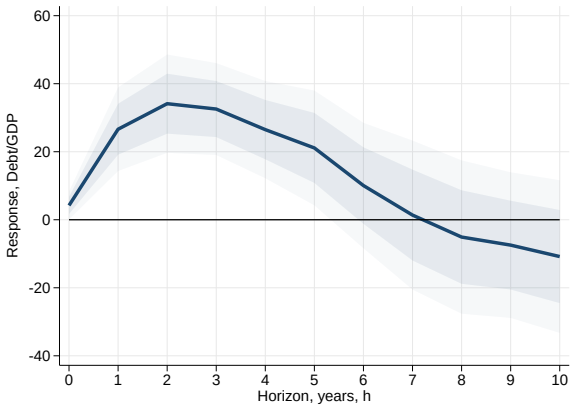


Notes: The figure shows the cumulative response of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for advanced economies, at horizon h (in years) on the x-axis. The reform variable is the change in CBIE Lending implied by a legal reform, carried forward until a subsequent reform or reversal. It is instrumented using lagged CBIE Lending, lagged regional pressure, IMF-program exposure in the previous two years, and inflation-crisis exposure in the previous two years, as described in Section 3.1. Responses are expressed per unit change in the 0–1 CBIE Lending index. The median positive reform in the full sample is 0.34. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

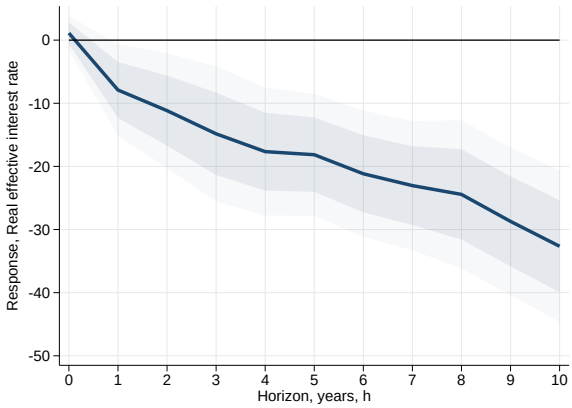
Figure 4 presents the corresponding estimates for developing economies. In this group, tighter legal restrictions on central bank lending are followed by an increase in debt-to-GDP and a decline in the real effective implicit interest rate. This combination is consistent with an expansion in debt-carrying capacity, in that governments can sustain higher public debt without a higher real servicing cost. Since the effects are economically larger in developing countries, where, plausibly, concerns about monetary financing and sovereign credibility are also more salient, we focus the state-dependent analysis that follows on this group.

Figure 4: IV local projections, developing countries, 1972–2023.

(a) Debt-to-GDP



(b) Real effective implicit interest rate



Notes: The figure shows the cumulative response of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for developing countries, at horizon h (in years) on the x-axis. The reform variable is the change in CBIE Lending implied by a legal reform, carried forward until a subsequent reform or reversal. It is instrumented using lagged CBIE Lending, lagged regional pressure, IMF-program exposure in the previous two years, and inflation-crisis exposure in the previous two years, as described in Section 3.1. Responses are expressed per unit change in the 0–1 CBIE Lending index. The median positive reform in the full sample is 0.34. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

The two responses differ in an economically informative way. The debt-to-GDP response is hump-shaped: for a median-sized tightening reform (0.34 on the 0–1 index), the debt-to-GDP ratio rises by about 9 percentage points one year after the reform, peaks at about 12 percentage points after two years, and then fades, becoming statistically indistinguishable from zero from the six-year horizon onwards. The real effective implicit interest rate instead declines persistently: the same median reform lowers the real rate by about 3 percentage points one year after the reform and by about 6 percentage points at the five-year horizon, and the decline is significant at the 90 percent level at every horizon from one to ten years. Because the outcome is the real effective implicit interest rate, it captures the real cost of servicing the outstanding debt stock rather than the nominal interest bill. Section 4.6 examines how this decline should be read. Read together, the estimates describe a transitory expansion of borrowing financed at a durably lower real cost: the debt ratio eventually reverts towards its pre-reform path, while the reduction in the real cost of servicing the outstanding debt stock persists.

4.2 State dependence

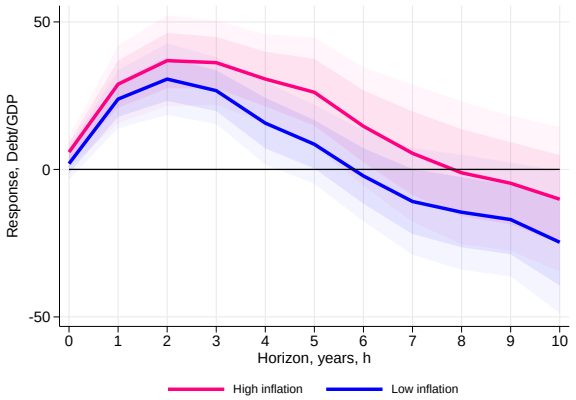
We next examine whether the baseline IV responses vary across macroeconomic states, using the interaction specification of equation (5) on the sample of developing countries. Because the two state-specific responses are estimated within a single regression, we can test at each horizon whether they are equal, rather than inferring state dependence from the visual overlap of two confidence bands. We consider three state variables: high versus low inflation, IMF-program exposure, and high versus low debt, and we complement them with an institutional split by whether the country ever adopted inflation targeting. Inflation, debt and inflation targeting are country-level states defined over the full sample period, including post-reform years, so they describe the type of country rather than conditions at the time of reform, while IMF-program exposure varies over time. Responses are per unit change in the CBIE Lending index throughout.

Inflation is the dimension along which the interest-rate response is state-dependent over the long run. Figure 5 shows that the real effective implicit interest rate falls in both states, but by more where inflation has been high, and the gap widens with the horizon: the response is -19.9 in high-inflation countries against -17.0 in low-inflation countries at the five-year horizon, reaching -33.5 against -12.3 at ten years.[12] The legal commitment not to monetize debt therefore appears most valuable in high-inflation contexts over the long run, consistent with investors having greater reason to fear that the government will inflate the debt away where that risk has been more salient historically. This reading is in line with recent evidence that episodes of deficit monetization leave a lasting imprint on the conduct of monetary policy (Jácome et al., 2026). The debt response also differs by inflation state, concentrated at intermediate horizons rather than in the long run. Debt-to-GDP rises by a comparable amount in both states over the first two years, and the paths diverge over the medium term, with the increase sustained longer in high-inflation countries; the null of equality is rejected at the ten percent level or better between the third and eighth horizons (p-values between 0.008 and 0.084) but not at the ten-year horizon (p-value of 0.168).

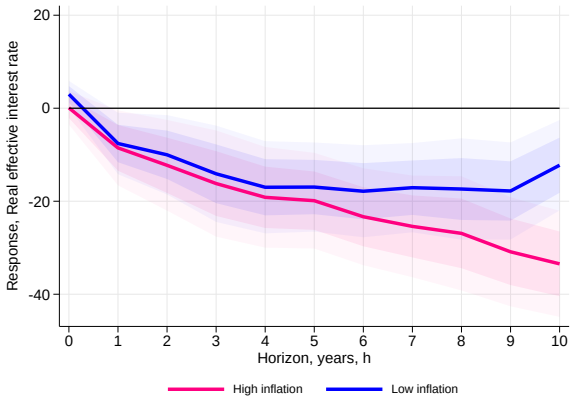
[12] The equality of the two responses is not rejected at conventional levels through the six-year horizon (p-value of 0.148), is rejected at the five percent level from seven years (p-value of 0.046), and is clearly rejected by the ten-year horizon (p-value below 0.001).

Figure 5: State-dependent IV local projections, by inflation, developing countries, 1972–2023.

(a) Debt-to-GDP



(b) Real effective implicit interest rate

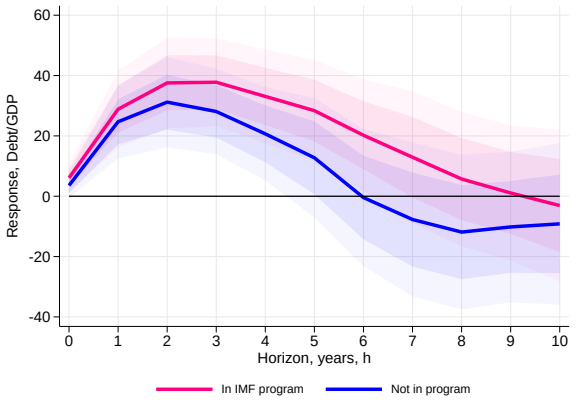


Notes: The figure shows state-dependent IV local-projection responses of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for developing countries, by high and low inflation, at horizon h (in years) on the x-axis. Countries are classified by a median split of each country's average CPI inflation over the sample period, the same inflation series used throughout the paper, including the Fisher adjustment in Section 2.2. The classification is time-invariant, assigning each country to a single state for its full sample. The split yields 60 high-inflation and 61 low-inflation countries, with a threshold of approximately 10 percent average annual inflation. The responses are estimated in the interaction form of equation (5), in which the treatment is split into its two state components within a single regression and each component is instrumented with the corresponding state interaction of the instrument set. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors. The equality of the two responses is tested at each horizon and the results are discussed in the text.

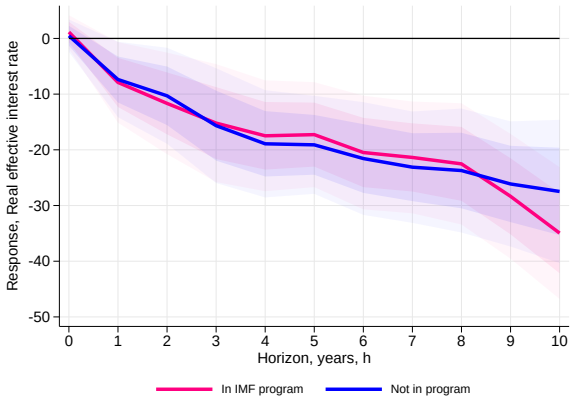
Figure 6 conditions the responses on whether the country was under an IMF program. This split speaks directly to the concern that central bank reforms may be bundled with broader stabilisation packages. The real-rate decline is of similar size in both groups, −17.3 against −19.1 at the five-year horizon, and the null of equality cannot be rejected at the five percent level at any horizon. The real-rate decline is therefore not specific to reforms adopted under an IMF program. The debt response is larger under an IMF program, with the difference significant at the five percent level between the fifth and eighth horizons.

Figure 6: State-dependent IV local projections, by IMF-program exposure, developing countries, 1972–2023.

(a) Debt-to-GDP



(b) Real effective implicit interest rate



Notes: The figure shows state-dependent IV local-projection responses of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for developing countries, by IMF-program exposure, at horizon h (in years) on the x-axis. IMF-program exposure is measured by the indicator used in the instrument set, which varies over time within countries. The responses are estimated in the interaction form of equation (5), in which the treatment is split into its two state components within a single regression and each component is instrumented with the corresponding state interaction of the instrument set. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors. The equality of the two responses is tested at each horizon and the results are discussed in the text.

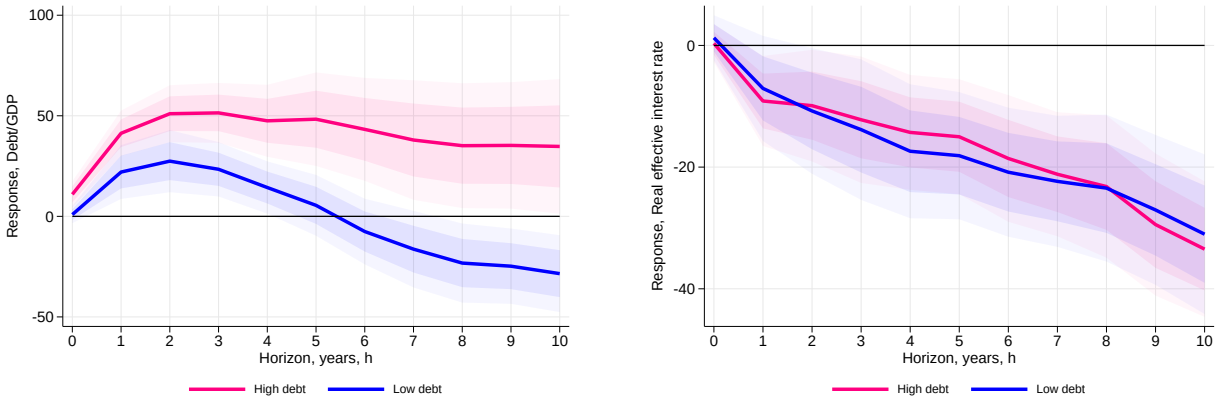
Next, Figure 7 splits developing countries by the level of debt, asking whether lending restrictions matter differently when fiscal constraints are already tight. The pattern is the reverse of the one found for inflation. The real-rate response is essentially the same in the two groups, -15.0 against -18.1 at the five-year horizon, and the null of equality is never rejected. The debt response, by contrast, differs sharply and at every horizon.[13] In high-debt countries the debt ratio rises by between 41 and 51 percentage points per unit of the index over the first five years and remains elevated ten years after the reform, while in low-debt countries it rises by about half as much at its peak, reverts, and turns significantly negative. The pattern is therefore consistent with reforms relaxing a constraint on debt issuance where debt is already high, without changing the real cost of servicing that debt differently from elsewhere.

[13]The p-value for the equality of the two responses is 0.002 at one year, 0.001 at two years, and below 0.001 from the third horizon onwards.

Figure 7: State-dependent IV local projections, by debt level, developing countries, 1972–2023.

(a) Debt-to-GDP

(b) Real effective implicit interest rate



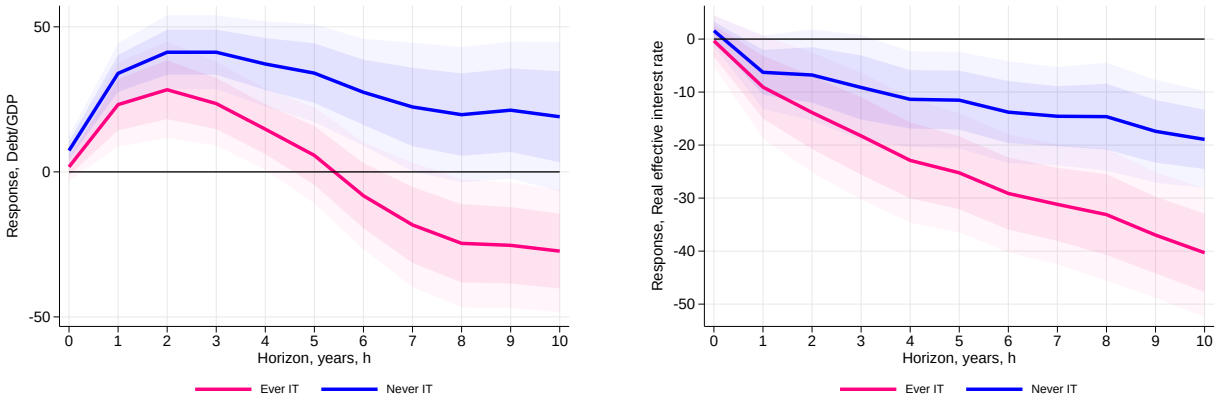
Notes: The figure shows state-dependent IV local-projection responses of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for developing countries, by high and low debt, at horizon h (in years) on the x-axis. Countries are classified by a median split of each country's average debt-to-GDP ratio over the sample period. The classification is time-invariant, assigning each country to a single state for its full sample. The split yields 59 high-debt and 60 low-debt countries, with a threshold of approximately 51 percent of GDP. Two developing countries without debt data are excluded. The responses are estimated in the interaction form of equation (5), in which the treatment is split into its two state components within a single regression and each component is instrumented with the corresponding state interaction of the instrument set. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors. The equality of the two responses is tested at each horizon and the results are discussed in the text.

Finally, Figure 8 splits developing countries by whether they ever adopted inflation targeting, an institutional counterpart to the inflation split. Because inflation targeting was typically adopted many years after the lending reform (a median of eight years), the split compares countries by the monetary framework they went on to build rather than the one in place when they reformed. The results show that the decline in the real effective rate is larger in countries that adopted inflation targeting (-25.2 against -11.5 at the five-year horizon), while the debt expansion is concentrated in countries that never did (34.0 against 5.7), and both differences are significant at the one percent level at that horizon. This pattern is consistent with the credibility gain showing up mainly as cheaper borrowing rather than additional debt in countries that went on to adopt inflation targeting.

Figure 8: State-dependent IV local projections, by inflation targeting, developing countries, 1972–2023.

(a) Debt-to-GDP

(b) Real effective implicit interest rate



Notes: The figure shows state-dependent IV local-projection responses of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for developing countries, by whether the country ever adopted inflation targeting, at horizon h (in years) on the x-axis. Countries are classified as ever IT if they were under an inflation-targeting regime in any year of the sample and as never IT otherwise. The classification is time-invariant, assigning each country to a single state for its full sample. The split yields 21 ever-IT and 100 never-IT countries. The responses are estimated in the interaction form of equation (5), in which the treatment is split into its two state components within a single regression and each component is instrumented with the corresponding state interaction of the instrument set. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors. The equality of the two responses is tested at each horizon, and the results are discussed in the text.

Taken together, the state-dependent estimates indicate where the credibility effect is concentrated. The real cost of servicing debt responds more strongly to reform where inflation has been high, and inflation is the only macroeconomic state for which the difference in the real-rate response is statistically significant, from the seven-year horizon onwards. Neither IMF-programme status nor the level of debt changes that response at the five percent level. What the level of debt changes is the quantity response: high-debt sovereigns expand their borrowing after reform and sustain it, while low-debt sovereigns do not. The inflation-targeting split reinforces the credibility reading, since about two-thirds of the countries that went on to adopt inflation targeting are in the high-inflation group. The evidence is therefore consistent with an inflation-credibility mechanism, and its consequences for debt accumulation are largest where fiscal space was most constrained to begin with.

Two episodes illustrate the contrast between high-inflation developing economies and low-inflation advanced economies. In Colombia, Law 31 of 1992, which implemented the central bank provisions of the 1991 Constitution, established the Banco de la República as an independent authority and restricted its lending to the government, raising CBIE

Lending from 0.47 to 0.67 (see Appendix D). The reform was domestically initiated and adopted outside any IMF-supported program. Colombia belongs to the high-inflation group in the estimates above, with inflation of about 27 percent in the reform year. The euro area provides a contrasting case. Between 1993 and 1998, the twelve countries that formed the euro area by 2001 raised their CBIE Lending scores to the maximum as part of the convergence towards monetary union. These reforms account for 12 of the 29 tightening reforms in advanced economies in our sample, and in eleven of the twelve cases inflation was below 5 percent in the reform year, with Greece the exception. Such reforms plausibly left little monetization risk to remove, which is consistent with the absence of a decline in real borrowing costs in advanced economies (Section 4.1). They were also part of a broader regime change, so they cannot be read as isolated tests.

4.3 Classical local-projection robustness

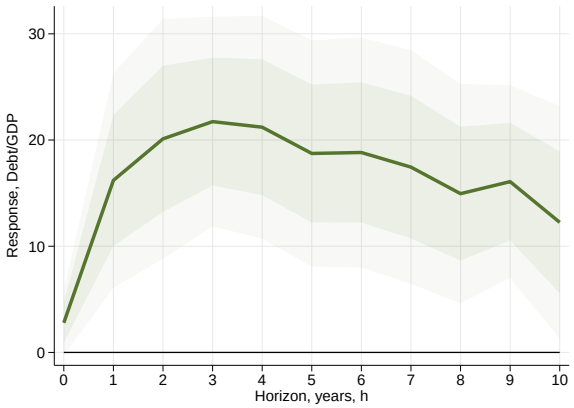
Figure 9 reports the corresponding classical, non-IV local-projection estimates for developing economies. These estimates use the same reform-induced change in CBIE Lending as the preferred IV specification and retain the baseline structure: cumulative responses from $t-1$ to $t+h$, year fixed effects, four fixed lags of the relevant differenced outcomes, and the real effective implicit interest rate as the interest-rate outcome. The difference from the baseline specification is that the reform variable is not instrumented.

The purpose of this exercise is not to replace the IV estimates, but to verify that the qualitative pattern is not created by the instrument set. The non-IV responses point in the same broad direction as the baseline IV evidence: in developing economies, reforms that tighten central bank lending restrictions are associated with higher debt-to-GDP and lower real borrowing costs. Because these estimates do not address the potential endogeneity of central bank reforms, we treat them as robustness and comparison evidence.

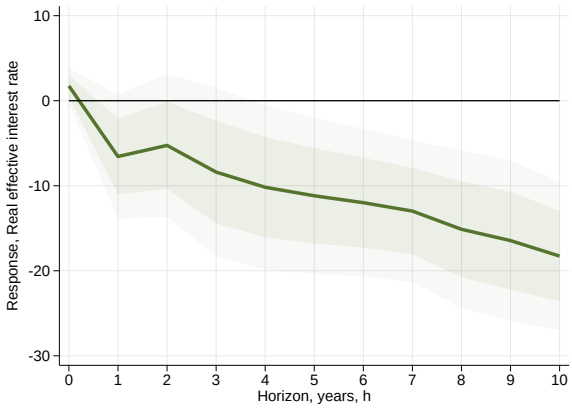
The non-IV estimates preserve the main qualitative distinction between developing and advanced economies. In developing economies, tighter lending restrictions are associated with higher debt-to-GDP and lower real borrowing costs, while the responses for advanced economies are more muted and less stable over time. We interpret these estimates as descriptive dynamic evidence that complements the IV analysis. They are also useful for comparison with the earlier version of the paper, where classical local projections were presented before the IV estimates. In the revised draft, however, the IV estimates carry the

Figure 9: Classical local projections, developing countries, 1972–2023.

(a) Debt-to-GDP



(b) Real effective implicit interest rate



Notes: The figure shows the cumulative response of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for developing countries, at horizon h (in years) on the x-axis, estimated by classical local projections. The reform variable is the change in CBIE Lending implied by a legal reform, carried forward until a subsequent reform or reversal, and it is not instrumented. The specification otherwise follows the baseline structure of Section 3.1: cumulative responses, year fixed effects and four fixed lags. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

main identification burden.

We also estimate the same classical local projections for the full sample and advanced economies, and for interest expenditures. The full-sample and advanced-economy estimates for debt-to-GDP and the real effective implicit interest rate are reported in Appendix Figure F.7 and are qualitatively consistent with the interpretation that the strongest effects are concentrated in developing economies.

4.4 Binary treatment-variable robustness

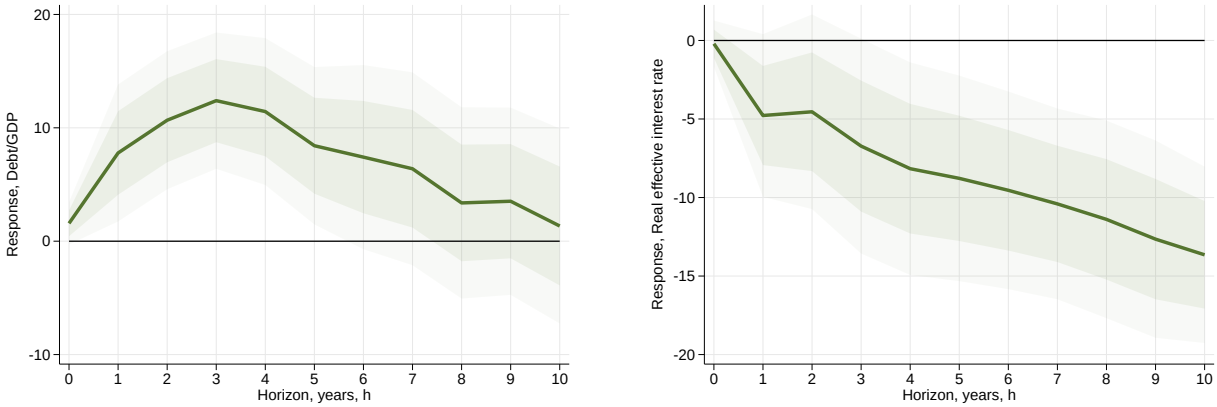
As a second robustness exercise, we replace the reform-magnitude treatment with a binary indicator for large CBIE Lending reforms. This variable switches on after the country's largest positive reform above the post-1972 median reform size. We estimate this binary specification using the same structure as above: cumulative outcomes, year fixed effects, four fixed lags, and the real effective implicit interest rate.

Figure 10 reports the binary local-projection results for developing economies. The estimates are less informative about reform intensity because they collapse reforms of different magnitudes into a single indicator. Nevertheless, they provide a useful check that the main pattern is not driven solely by the continuous scaling of the CBIE Lending reform vari-

able. We therefore interpret the binary estimates as robustness evidence, while retaining the reform-magnitude IV specification as the baseline.

Figure 10: Binary large-reform local projections, developing countries, 1972–2023.

(a) Debt-to-GDP (b) Real effective implicit interest rate



Notes: The figure shows the cumulative response of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a binary large-reform indicator, D_{med72}, for developing countries, at horizon h (in years) on the x-axis, estimated by classical local projections. The indicator switches on after the country's largest positive CBIE Lending reform above the post-1972 median reform size. It is not instrumented, and the specification otherwise follows the baseline structure of Section 3.1: cumulative responses, year fixed effects and four fixed lags. Responses are expressed per unit of the indicator and are therefore not directly comparable in magnitude with the estimates for the continuous reform variable, which are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

4.5 Signed reform indicator

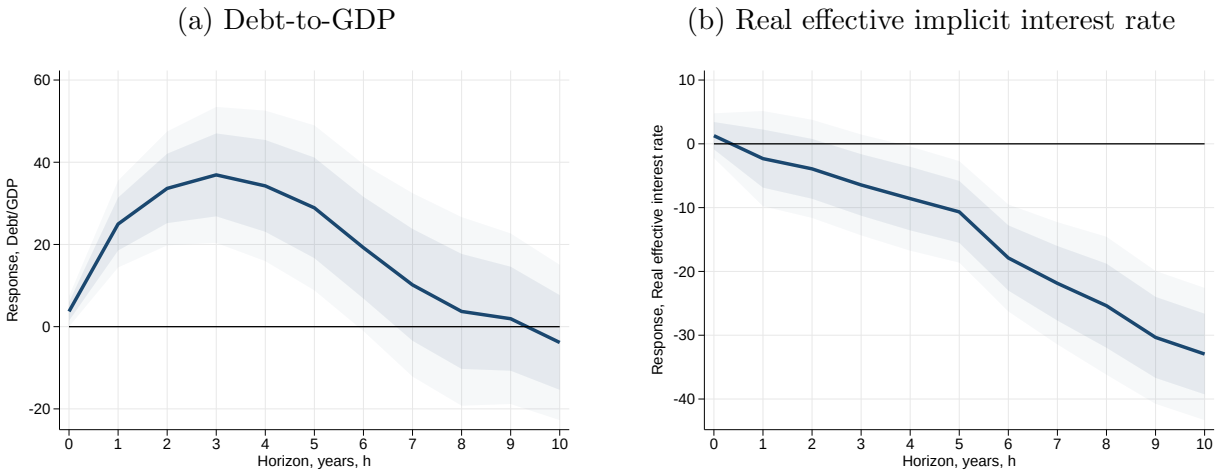
The baseline treatment records the magnitude of the legal change, so the estimated responses could in principle reflect the measured size of reforms rather than their occurrence. As a third robustness exercise, we re-estimate the baseline instrumented specification replacing the treatment with the signed reform indicator described in Section 3.3, equal to +1 after a reform that tightens lending restrictions and −1 after a reform that loosens them. Identification, controls, lags, leads, and instruments are unchanged, so the comparison isolates the coding of the treatment.

Figure 11 reports the results for developing economies, and the shape of both responses is preserved. Debt-to-GDP rises sharply after the reform, peaks at the three-year horizon, remains significant through the fifth year and is indistinguishable from zero from the sixth onwards. The real effective implicit interest rate declines progressively, becomes significant at the five-year horizon and reaches about −33 at ten years. Relative to the baseline, the short-horizon decline in the real rate is less precisely estimated, and it is no longer significant

at the one- and two-year horizons.

The coefficients are not directly comparable in magnitude with the baseline, since they measure the effect of a reform of unspecified size rather than the effect of a one-unit change in the index. Taken at face value, the signed-indicator coding implies a larger effect for a typical reform than the magnitude coding does, which suggests that the occurrence of a reform carries more of the response than its recorded size. We therefore retain the reform-magnitude specification as the baseline, since it is the more conservative of the two, and read this robustness exercise as evidence that the dynamic pattern we document, a transitory debt expansion alongside a persistent decline in the real cost of servicing debt, does not depend on the magnitudes recorded in the CBIE Lending index.

Figure 11: IV local projections with a signed reform indicator, developing countries, 1972–2023.



Notes: The figure shows the cumulative response of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a signed reform indicator for developing countries, at horizon h (in years) on the x-axis. The indicator equals +1 after a reform that tightens central bank lending restrictions and −1 after a reform that loosens them, is carried forward until a subsequent reform, and is zero before a country's first reform. It replaces the reform variable of the baseline IV specification in Section 3.1, and the instrument set, controls, lags and leads are unchanged. Responses are expressed per unit of the indicator and are therefore not directly comparable in magnitude with the baseline estimates, which are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

4.6 Additional robustness tests

This section examines the baseline responses along three lines: how the interest rate is measured, how the decline in the real rate should be read, and whether other changes that accompany lending reforms could explain them.

A data-quality concern specific to the interest-rate outcomes is that the implicit rate is a constructed ratio: extreme values arise mechanically in country-years with very small

recorded debt service, and in high-inflation episodes the Fisher adjustment amplifies measurement error in inflation. We therefore re-estimate the interest-rate responses excluding country-years in which the nominal implicit rate, the real effective implicit rate, or inflation lies outside its 1st–99th percentile range (Appendix Figure F.1). The exclusion affects about 4 percent of country-years, concentrated in high-inflation and crisis episodes, and no country leaves the sample entirely. Because these extremes reflect problems with the measured interest rate rather than with the debt ratio, the exclusion is motivated only for the interest-rate outcomes, and the baseline debt-to-GDP estimates retain the full sample. Panel (a) of the appendix figure nevertheless verifies that the debt-to-GDP response is similar on the trimmed sample, so the joint interpretation of the debt and interest-rate responses does not rest on the difference in samples. Panel (b) shows that the real-rate response is attenuated relative to the full sample, to about -7 at the five-year horizon against -18 on the full sample, and to about -10 against -33 at ten years, consistent with the excluded observations being concentrated in high-inflation episodes, where measurement error in the Fisher adjustment is largest. This attenuation is also consistent with the state-dependent estimates in Section 4.2, which identify high-inflation environments as those in which the effect on real borrowing costs is strongest.

The baseline responses are also not an artifact of how the implicit rate is scaled. We re-estimate the interest-rate specifications using seven alternative denominators: the end-of-period debt stock, the one-year lagged stock, and the average of the stocks at $t-1$ and $t-2$, all from the baseline debt-to-GDP series, together with the end-of-period and average stocks from the Public Finances and Global Macro Database debt series (Appendix Table F.1). The real-rate response is similar across all of them, ranging from -14 to -18 at the five-year horizon and from -25 to -33 at the ten-year horizon, and it is significant at the five percent level in every case. The smallest responses come from the two Public Finances debt measures. The nominal response is small and imprecisely estimated, and none of its estimates is significant at the five percent level under any denominator. Across the four versions built on the baseline debt-to-GDP series, its five-year point estimate rises as the denominator is dated further back, from 0.85 with the end-of-period stock to 3.71 with the average of the stocks at $t-1$ and $t-2$, as expected when interest payments are scaled by a debt stock that rises after the reform, but each step is smaller than the standard errors. This is consistent with the argument in Section 2.2 that the nominal rate cannot be read

on its own as a measure of borrowing conditions.

Two additional results sharpen the economic interpretation. First, the decline in real borrowing costs is also visible relative to a world benchmark. The response of the real implicit-rate spread vis-à-vis the United States mirrors the baseline real-rate response,[14] and a variant estimated without year fixed effects, in which the US benchmark is not absorbed by the time effects, shows the same persistent decline (Appendix Figure F.3). The nominal spread against the United States, by contrast, shows no significant movement at any horizon. The estimates are therefore consistent with a lower real cost of servicing sovereign debt relative to the world benchmark rather than with a nominal convergence, in line with evidence that central bank credibility and independence lower the returns investors require on sovereign debt (e.g., Du et al., 2020b; Willems and Zettelmeyer, 2022; Kashyap and Stein, 2023; Alter et al., 2026). Because implicit rates are constructed from interest payments and debt stocks rather than observed yields, we do not interpret the spread as a market risk premium.

Second, the same IV specification applied to the cumulative price level shows a significant decline in the CPI at medium and long horizons (Appendix Figure F.4), consistent with the disinflationary effects of central bank independence reforms documented in the literature (e.g., Grilli et al., 1991; Alesina and Gatti, 1995; Loungani and Sheets, 1997; Athanasopoulos et al., 2025; Jung et al., 2025b). The size of this response suggests that it reflects high-inflation episodes, which is also where the real-rate decline is strongest (Section 4.2). We therefore read the decline in the real effective rate as occurring together with disinflation, and we do not attempt to separate the two.

The remaining concern is that lending reforms rarely arrive alone. A reform may be adopted together with a change in the exchange-rate regime, in fiscal institutions or in the monetary-policy framework, any of which could affect borrowing conditions directly. For each of these, we ask how often reforms overlap with the other change and whether controlling for it, or dropping the countries where it overlaps, moves the estimates (Appendix Table F.2).[15] A natural first candidate is the exchange-rate regime, since pegged arrangements constrain monetary policy and may correlate with both reform adoption and borrowing

[14] With year fixed effects, the US benchmark rate is common to all countries within a year and is absorbed by the time effects, so the spread response coincides with the own-rate response by construction. The variant without year fixed effects is therefore the informative specification for the spread, and should be read as descriptive since it does not control for common global shocks.

[15] The impulse-response figures for the individual checks are available upon request.

conditions. The baseline controls already include the exchange-rate stability index and the Chinn–Ito index of financial openness. As a sharper check, we classify country-years using the Ilzetzki et al. (2019) de facto coarse classification and re-estimate the baseline, adding regime indicators (peg, freely falling, and dual-market regimes, with floating as the omitted category) to the control set. The responses are essentially unchanged (Appendix Figure F.5): the debt-to-GDP hump and the persistent decline in the real effective rate are not driven by exchange-rate-regime composition. Fiscal institutions are a second candidate. Using the IMF Fiscal Rules Dataset (Alonso et al., 2025), which starts in 1985, only 15 of the 124 reforms in developing economies since 1986 fall within one year of the adoption or major revision of a national or supranational fiscal rule, and six of these reflect a single episode, the 2003 reform of the regional central bank of the CEMAC countries. Excluding the 15 countries concerned, or adding lagged fiscal-rule status to the control set, leaves the real-rate response at the five-year horizon at -17.1 and -17.9, respectively, against -18.2 in the baseline, and the debt response is equally stable.

The monetary-policy framework is a third. Only one of the 132 reforms in developing economies since 1972 occurred under an inflation-targeting regime, since inflation targeting was typically adopted about a decade after the lending reform. Adding lagged inflation-targeting status to the control set leaves the debt response unchanged and reduces the real-rate response only slightly, from -18.2 to -16.6 at the five-year horizon, which remains significant at the one percent level. The responses by later adoption of inflation targeting are shown in Figure 8 and discussed in Section 4.2.

To mitigate concerns that our results might be driven by specific institutional or political circumstances surrounding central bank reform (i.e., that would introduce a third variables bias), we perform two additional tests. First, we address concerns that the observed legal reforms are part of an IMF program that aims to increase the independence of a country's national bank. Second, we verify that political partisanship of an incumbent government does not impact our baseline results, allowing us to mitigate concerns that the observed reforms are part of a larger set of reforms.

Starting with the role IMF-programs, if reforms were mostly imposed from outside, the estimates would reflect external pressure rather than domestic legislative choices. To gauge this, we cross-reference our reform episodes against the CBI-conditionality dataset of Kern et al. (2019), which records, for 1980–2012, whether an IMF-supported program included

conditions specifically targeting central-bank lending to the government. Of the 126 reform episodes that fall in country-years covered by the data (98 in developing economies), only 4 (3.2 percent) coincide with such conditionality in the same year, and 7 (5.6 percent) within a one-year window.[16] Few reforms therefore coincide with externally imposed conditions, and excluding the seven countries concerned leaves the baseline responses in developing economies unchanged in shape and, if anything, larger. The check is necessarily confined to the years for which the conditionality data are available.

The political orientation of the government that adopts a reform is the second. Tightening reforms are not confined to one orientation. Two prominent ones, in Argentina and Peru in 1992, followed episodes of monetary financing and hyperinflation (in Peru under the left-wing populist government of 1985–1990) but were adopted under governments that Funke et al. (2023) classify as right-wing populist, whereas the only reform adopted under a left-wing populist government in their classification, in Argentina in 2003, loosened the restrictions on central bank lending. Using the Database of Political Institutions (Beck et al., 2001), adding a lagged left-executive indicator to the control set leaves the baseline responses unchanged, and splitting reforms by the orientation of the executive in office shows the real effective rate falling under both left- and right-wing executives (-43.2 against -23.4 at the five-year horizon; Appendix Figure F.6), although the difference is significant at the five percent level only at the three- and four-year horizons. Because party orientation is unclassified for most developing-country executives, this split rests on less than half of the baseline sample.

Taken together, these checks leave the sign and shape of the baseline pattern intact: lending restrictions are followed by a transitory expansion of debt and a persistent decline in the real cost of servicing it, whether the interest rate is trimmed or rescaled, read against the world benchmark, or estimated while controlling for or excluding the other changes that may accompany reform. The size of the real-rate decline is sensitive to extreme observations and is strongest where inflation has been high.

4.7 Foreign-held (external) public debt

The baseline estimates describe a transitory expansion of debt financed at a persistently lower real cost. A natural question is who holds the additional debt, and in particular

[16] Counting conditions that cover any dimension of central bank independence also yields 7 episodes in the same year.

whether the foreign-held component of public debt rises. We measure foreign-held public debt as general government publicly guaranteed external debt stocks, that is, debt owed to nonresident creditors, from the World Bank International Debt Statistics, expressed as a share of GDP. The series is defined by the residence of the creditor and does not distinguish local- from foreign-currency debt. We estimate the response of this variable using the same IV local-projection structure as the baseline, focusing on developing economies, for which the external-debt data are available.

Figure 12 shows that reforms tightening central bank lending restrictions are followed by a marked and persistent increase in foreign-held public debt relative to GDP in developing economies. The cumulative response is positive, becomes statistically significant from the first year after the reform, rises until about the fifth year, and remains significant through the end of the projection window. For a median-sized tightening reform, foreign-held public debt rises by about 5 percentage points of GDP one year after the reform, peaks at close to 10 percentage points of GDP around the five-year horizon, and remains about 6 percentage points of GDP higher, and statistically significant, ten years out. Unlike total debt-to-GDP, which reverts towards its pre-reform path after about five years, foreign-held debt stays elevated, which points to a shift in the composition of public debt towards foreign creditors, although we do not estimate the domestic component.

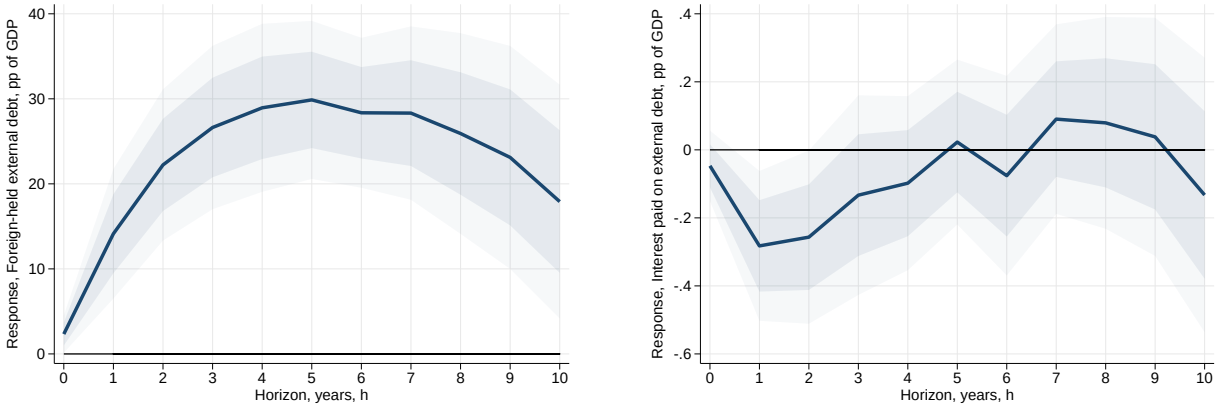
Panel (b) shows the corresponding response of interest paid on external public debt as a share of GDP. The point estimates are small throughout, marginally negative in the first two years and statistically indistinguishable from zero at every subsequent horizon, indicating that the expansion in external borrowing was not accompanied by a rising external interest burden. Read together with the baseline finding that debt-to-GDP rises while the real effective cost of debt service falls, this pattern suggests that the additional borrowing capacity created by these reforms is met to an important extent through external, foreign financing. This is consistent with limiting monetary financing improving the sovereign's access to external creditors without raising the external interest burden, although we cannot tell whether foreign creditors lend in local or foreign currency.

Because the external-debt data cover a smaller set of country-years than the baseline sample, we also re-estimate the baseline responses on the IDS estimation sample (Appendix Figure F.2). On this common sample, total debt-to-GDP rises by a magnitude comparable to the external-debt response over the first five years, and the real effective rate declines as

in the baseline. The foreign-held debt result is therefore not an artefact of the change in sample: on the sample where both variables are observed, the external component accounts for the bulk of the debt expansion over the first five years.

Figure 12: IV local projections: foreign-held (external) public debt, developing countries, 1972–2023.

(a) Foreign-held public debt (% of GDP) (b) Interest on external public debt (% of GDP)



Notes: The figure shows the cumulative response of foreign-held (external) public debt (panel a) and of interest paid on external public debt (panel b) to a reform-induced change in the CBIE Lending index for developing countries, at horizon h (in years) on the x-axis. The outcomes are general government publicly guaranteed external debt stocks and interest payments on external debt (World Bank International Debt Statistics), expressed as shares of GDP. The reform variable is the change in CBIE Lending implied by a legal reform, carried forward until a subsequent reform or reversal. It is instrumented using lagged CBIE Lending, lagged regional pressure, IMF-program exposure in the previous two years, and inflation-crisis exposure in the previous two years, as described in Section 3.1. Responses are expressed per unit change in the 0–1 CBIE Lending index. The median positive reform in the full sample is 0.34. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

5 Conclusions

This paper revisits a fundamental question in macroeconomic governance: how does central bank independence impact sovereign debt dynamics? Using a novel dataset covering 155 countries from 1972 to 2023, we show that institutional limits on monetary financing have significant long-run effects in developing countries. In particular, they ease governments' borrowing constraints by lowering the real cost of sovereign borrowing, thereby expanding fiscal space. In our estimates, a median-sized tightening reform temporarily raises the debt-to-GDP ratio by about 12 percentage points at its two-year peak, while the real effective cost of servicing public debt falls by around 6 percentage points at the five-year horizon and remains durably lower. These effects are strongest in countries with a history of high inflation, where credibility gains are largest, and we find no comparable decline in the

real cost of borrowing in advanced economies. Fiscal space does not expand because debt grows without limit, since the debt ratio eventually reverts towards its pre-reform path. It expands because the real cost of carrying the outstanding debt stock persistently declines and, it appears, because access to external creditors improves: foreign-held public debt rises markedly, with no accompanying increase in the external interest burden. The channels behind this compression of real borrowing costs deserve further study. Our external-debt evidence points to improved access to foreign creditors. A complementary channel is that the domestic financial sector absorbs more sovereign debt as the risk profile of government liabilities improves, in line with the literature on the bank–sovereign nexus (Gennaioli et al., 2014). We leave a direct examination of the composition of the domestic investor base to future work.

Our findings offer a word of caution for recent discussions about the future of macroeconomic management. Ultimately, it is the credibility of the macroeconomic policy framework that underwrites any large-scale "ad hoc" or "emergency" interventions, whether through quantitative easing, as in the decade following the Global Financial Crisis, or through more direct monetary financing of public debt, as some propose for the future. The paradox, however, is that such credibility can be depleted much faster than it can be restored (Blinder, 2007; Romelli, 2025; Kern et al., 2024). The fiscal space that credibility creates can also become a liability. As Martin (2015) shows, the additional debt and state capacity made possible by central bank reform and lower inflation can weigh on the central bank in future crises: the larger the accumulated debt, the greater the pressure to monetize it during a shock, which heightens the risk of fiscal dominance and erodes the value of debt as inflation returns to pre-reform levels.

From a policy perspective, there are three main implications. First, as Willems and Zettelmeyer (2022, page 77) observe, "cross-country differences in central bank credibility could be part of the explanation why all countries do not seem to be created equal when it comes to sustaining a given debt level. Countries that wish to enjoy greater fiscal space should invest in building central bank credibility." Second, to the extent that central bank independence and credibility have contributed to higher debt capacity at a lower real cost in developing countries, any proposals to expand the use of monetary financing and prolonged quantitative easing should be weighed against the risk of undermining this independence and jeopardizing the fiscal space gained over recent decades. As Willems and Zettelmeyer (2022)

put it, central bank independence is a "fiscal asset" that can strengthen state capacity but remains vulnerable to other policy choices. Finally, by the same logic, efforts to loosen the legal limits on central bank lending to the government and to blur the distinction between central banking and fiscal policy may worsen government borrowing conditions, particularly in countries with a history of high inflation, thus limiting the fiscal space essential for supporting vital policy programming.

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Appendix

A Economies

Table A.1: Analyzed economies

Economy, year of first analyzed legislation, and development cluster					
Afghanistan	2003	Dev	Dominica	1983	Dev
Albania	1992	Dev	Dominican Republic	1959	Dev
Algeria	1962	Dev	Ecuador	1957	Dev
Angola	1997	Dev	Egypt, Arab Rep.	1957	Dev
Anguilla	1987	Dev	Equatorial Guinea	1972	Dev
Antigua and Barbuda	1983	Dev	Estonia	1993	Adv
Argentina	1935	Dev	Ethiopia	1994	Dev
Australia	1959	Adv	Finland	1966	Adv
Austria	1955	Adv	France	1936	Adv
Azerbaijan	1996	Dev	Gabon	1972	Dev
Bahrain	1973	Dev	Gambia, The	1971	Dev
Bangladesh	2003	Dev	Georgia	1995	Dev
Belarus	1990	Dev	Germany	1957	Adv
Belgium	1948	Adv	Ghana	1975	Dev
Benin	1956	Dev	Greece	1959	Adv
Bolivia	1945	Dev	Grenada	1983	Dev
Bosnia and Herzegovina	1997	Dev	Guatemala	1959	Dev
Botswana	1975	Dev	Guinea-Bissau	1956	Dev
Brazil	1964	Dev	Guinea	1994	Dev
Brunei	1984	Dev	Haiti	1979	Dev
Bulgaria	1991	Dev	Hungary	1991	Dev
Burkina Faso	1956	Dev	Iceland	1966	Adv
Burundi	1965	Dev	India	1934	Dev
Cambodia	1954	Dev	Indonesia	1953	Dev
Cameroon	1972	Dev	Iran, Islamic Rep.	1972	Dev
Canada	1954	Adv	Iraq	1964	Dev
Central African Republic	1972	Dev	Ireland	1942	Adv
Chad	1972	Dev	Italy	1948	Adv
Chile	1953	Dev	Côte d'Ivoire	1956	Dev
China	1995	Dev	Jamaica	1992	Dev
Colombia	1923	Dev	Japan	1957	Adv
Comoros	1987	Dev	Jordan	1971	Dev
Costa Rica	1953	Dev	Kazakhstan	1993	Dev
Croatia	1991	Dev	Kenya	1984	Dev
Cuba	1959	Dev	Kuwait	1968	Dev
Cyprus	1963	Adv	Kyrgyzstan	1992	Dev
Czechia	1991	Adv	Lao PDR	1995	Dev
Congo, Dem. Rep.	1993	Dev	Latvia	1992	Adv
Denmark	1942	Adv	Lebanon	1969	Dev

Note: The table reports information on the list of analyzed economies, the first year of availability of the CBIE Lending index and their developmental cluster, i.e. *Adv* for advanced economies and *Dev* for developing economies.

Table A.1 Continued: Analyzed countries

Economy, year of first analyzed legislation, and development cluster					
Liberia	1974	Dev	Saint Kitts and Nevis	1983	Dev
Libya	1996	Dev	Saint Lucia	1983	Dev
Lithuania	1994	Adv	St. Vincent and the Grenadines	1983	Dev
Luxembourg	1983	Adv	Saudi Arabia	1957	Dev
Macao S.A.R	2000	Dev	Senegal	1956	Dev
Macedonia	1992	Dev	Seychelles	1986	Dev
Malawi	1989	Dev	Sierra Leone	1963	Dev
Malaysia	1982	Dev	Singapore	1991	Adv
Maldives	1982	Dev	Slovak Republic	1992	Adv
Mali	1984	Dev	Slovenia	1991	Adv
Malta	1994	Adv	Somalia	1960	Dev
Mauritania	1956	Dev	South Africa	1956	Dev
Mauritius	1966	Dev	Korea, Rep.	1950	Adv
Mexico	1960	Dev	Spain	1962	Adv
Moldova	1992	Dev	Sri Lanka	1953	Dev
Mongolia	1996	Dev	Sweden	1966	Adv
Montenegro	2005	Dev	Switzerland	1953	Adv
Morocco	1959	Dev	Taiwan, China	1979	Adv
Myanmar	1952	Dev	Thailand	1942	Dev
Namibia	1990	Dev	Bahamas, The	1974	Dev
Nepal	1955	Dev	Togo	1956	Dev
Netherlands	1948	Adv	Trinidad and Tobago	1964	Dev
New Zealand	1933	Adv	Tunisia	1958	Dev
Niger	1956	Dev	Türkiye	1970	Dev
Nigeria	1969	Dev	Turkmenistan	1994	Dev
Norway	1966	Adv	Uganda	1966	Dev
Oman	2000	Dev	Ukraine	1991	Dev
Pakistan	1972	Dev	United Arab Emirates	1980	Dev
Panama	1941	Dev	United Kingdom	1946	Adv
Paraguay	1952	Dev	Tanzania	1966	Dev
Peru	1962	Dev	United States of America	1951	Adv
Philippines	1948	Dev	Uruguay	1938	Dev
Poland	1997	Dev	Uzbekistan	2000	Dev
Portugal	1962	Adv	Venezuela, RB	1939	Dev
Qatar	1993	Dev	Viet Nam	1990	Dev
Congo, Rep.	1972	Dev	Yemen, Rep.	1971	Dev
Romania	1991	Dev	Zambia	1971	Dev
Russian Federation	1992	Dev	Zimbabwe	1956	Dev
Rwanda	1997	Dev			

Note: The table reports information on the list of analysed economies, the first year of availability of the CBIE Lending index and their developmental cluster, i.e. *Adv* for advanced economies and *Dev* for developing economies.

B Coding rules for the CBIE Lending index

This section summarizes the guiding principles used for the construction of the sub-index on Limitations on lending to the government (CBIE Lending index).

1)	Limitations on advances	
	Advances to government prohibited	1.00
	Advances permitted, but with strict limits (e.g., up to 15 percent of government revenue)	0.67
	Advances permitted, and the limits are loose (e.g., over 15 percent of government revenue)	0.33
	No legal limits on lending	0.00
2)	Lending to government	
	Not allowed	1.00
	In the secondary market with restricted limits	0.75
	In the secondary market with lax or without limits	0.50
	In the primary market with limits or approved by central bank board with a qualified majority	0.25
	In the primary market without limits	0.00
3)	Who decides financing conditions to government (maturity, interest, amount)?	
	Central bank defines terms and conditions	1.00
	Specified by the bank charter	0.67
	Agreed between the central bank and executive	0.33
	Decided by the executive branch alone	0.00
4)	Potential borrowers from the central bank	
	Only the government	1.00
	Government plus local governments	0.67
	All of the above plus public enterprises	0.33
	All of the above and to the private sector, also if it is not mentioned otherwise	0.00
5)	Limits on central bank lending defined	
	As an absolute cash amount	1.00
	As a percentage of central bank capital or other liabilities	0.67
	As a percentage of government revenues	0.33
	As a percentage of government expenditure	0.00
6)	Maturity of advances	
	Within 6 months	1.00
	Within 1 year	0.67
	More than 1 year	0.33
	No mention of maturity in the law	0.00
7)	Interest rates on advances	
	At market rates	1.00
	Interest rates not specified in law	0.50
	At below market rates	0.00
8)	Central bank prohibited from buying or selling government securities in the primary market	
	Yes	1.00
	No	0.00

C Data

Table C.1: Data and data sources

Variable	Definition	Data sources
	Dependent variable	
Debt to GDP	Gross public debt, percent of GDP. Observations from 2020 onwards are taken from the IMF World Economic Outlook measure of general government gross debt.	IMF - Historical Public Debt Database (Abbas et al., 2010) and World Economic Outlook
Real effective implicit interest rate on debt	Nominal implicit interest rate calculated as $100 \times \frac{\text{Interest paid on public debt in } t\text{, percent of GDP}}{\text{Average gross public debt in } t \text{ and } t-1\text{, percent of GDP}}$, then converted into a real effective rate using $100[(1 + r^{nom}/100)/(1 + \pi/100) - 1]$.	Interest paid: IMF - Public Finances in Modern History. Debt: as in the **Debt to GDP** row above. Inflation: as in the **Inflation** row below
	Explanatory variables	
CBIE Lending reform	Reform-induced change in the CBIE Lending index. The reform magnitude remains in place until a subsequent reform or reversal updates the index.	Romelli (2022, 2025)
Real GDP growth	Annual percentage growth rate of GDP at market prices based on constant local currency.	World Bank: NY.GDP.MKTP.KD.ZG
Inflation	Inflation, consumer prices (annual %)	World Bank: FP.CPI.TOTL.ZG
Government expenditure	Consolidated government expenditure, percent of GDP	Global Macro Database
Gross fixed capital formation	Gross fixed capital formation, percent of GDP	World Bank: NE.GDI.FTOT.ZS
Final consumption expenditure	Final consumption expenditure (% of GDP)	World Bank: NE.CON.TOTL.ZS
Openness to trade	Degree of openness to trade of a country. $Opnness_{c,t} = \frac{Imports_{c,t}+Exports_{c,t}}{GDP_{c,t}}$.	World Bank
Foreign direct investment	Foreign direct investment, net inflows (% of GDP)	World Bank: BX.KLT.DINV.WD.GD.ZS
Exchange Rate Stability	Measure of exchange rate stability, computed as the annual standard deviations of the monthly exchange rate between the home country and the base country.	Aizenman et al. (2008)
Chinn-Ito index	Index of capital account openness, Chinn–Ito de jure index (0–1).	Chinn and Ito (2008)
Polity	Index that measures the difference between the democratic and the autocratic score of a country, ranging from +10 (strongly democratic) to -10 (strongly autocratic).	PolityIV (2014)
Executive constraints	Constraints on the executive	PolityIV (2014)

Table C.1 Continued: Data sources

Variable	Definition	Data sources
	Additional data used in the extensions and robustness checks	
Foreign-held public debt	General government publicly guaranteed external debt stocks, expressed as a percent of GDP in current US dollars. Debt owed to nonresident creditors, not split by currency of denomination. Developing economies only.	World Bank: International Debt Statistics (DT.DOD.DEGG.CD); GDP from World Bank: World Development Indicators (NY.GDP.MKTP.CD)
Interest on external public debt	Interest payments on general government publicly guaranteed external debt, percent of GDP in current US dollars.	World Bank: International Debt Statistics (DT.INT.DEGG.CD); World Development Indicators (NY.GDP.MKTP.CD)
Alternative debt denominators	Gross government debt, percent of GDP, and consolidated government debt, percent of GDP, each used in end-of-period and average form.	IMF - Public Finances in Modern History (Mauro et al., 2015); Global Macro Database (globalmacrodata.com)
United States benchmark	Implicit interest rate of the United States, constructed as for all other countries and subtracted from each country's rate.	Same sources as the implicit rate
Fiscal rule in force; fiscal-rule change	Indicators for a fiscal rule in force and for the adoption or major revision of a rule, 1985–2024. Countries absent from the dataset are coded as having no rule.	IMF - Fiscal Rules Dataset (Alonso et al., 2025)
Exchange-rate regime	Indicators for peg (coarse categories 1–2), freely falling (5) and dual-market (6) regimes, with floating regimes (3–4) as the omitted category. The classification is available through 2019 and carried forward thereafter.	Ilzetzki et al. (2019) de facto coarse classification
Chief executive orientation	Party orientation of the chief executive (right, centre, left), 1975–2020. Unclassified executives are missing.	Database of Political Institutions (Beck et al., 2001)
IMF conditionality on central bank lending	Indicators for IMF-supported programmes with conditions targeting central bank lending to the government, and with conditions on any dimension of central bank independence, 1980–2012.	Kern et al. (2019)
Inflation-targeting regime	Indicator equal to one while the country operates an inflation-targeting regime, based on adoption and abandonment dates.	Jung et al. (2025b)
IMF programme exposure	IMF-programme indicator used in the instrument set, as defined in Section 3.1.	IMF and World Bank programmes data, 1970–2019 (Dreher, 2006)
Inflation crisis	Inflation-crisis indicator used in the instrument set, as defined in Section 3.1.	Constructed from CPI inflation

Table C.2: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Debt to GDP	6286	56.576	43.141	0.000	523.382
Real effective implicit interest rate	4558	-2.982	11.665	-99.846	47.059
CBIE Lending	6827	0.529	0.318	0.000	1.000
Real GDP growth	6657	3.588	6.784	-64.047	153.493
Inflation	6720	39.687	908.087	-72.729	65374.078
Government expenditure	6335	30.148	19.259	0.005	594.770
Gross fixed capital formation	5683	22.411	7.325	-2.424	93.547
Final consumption expenditure	5841	78.658	14.960	12.173	186.912
Openness to trade	5909	75.864	47.410	0.021	437.327
Foreign direct investment	6495	3.808	14.827	-440.131	449.083
Exchange Rate Stability	5576	0.628	0.326	0.001	1.000
Chinn-Ito index	6140	0.487	0.366	0.000	1.000
Polity	5474	2.545	7.141	-10.000	10.000
Executive constraints	5301	4.656	2.288	1.000	7.000

Note: The table provides summary statistics for the outcome and control variables included in the analysis, over 1972–2023 and restricted to country-years with CBIE Lending coverage. Real GDP growth, gross fixed capital formation, final consumption expenditure and foreign direct investment are from the World Bank World Development Indicators. Government expenditure is the consolidated government expenditure share of GDP from the Global Macro Database. Executive constraints are reported after setting the Polity special codes for foreign interruption, interregnum and transition periods to missing.

D Evolution of CBIE Lending index

Figure D.1: CBIE Lending index, advanced economies, 1972-2023

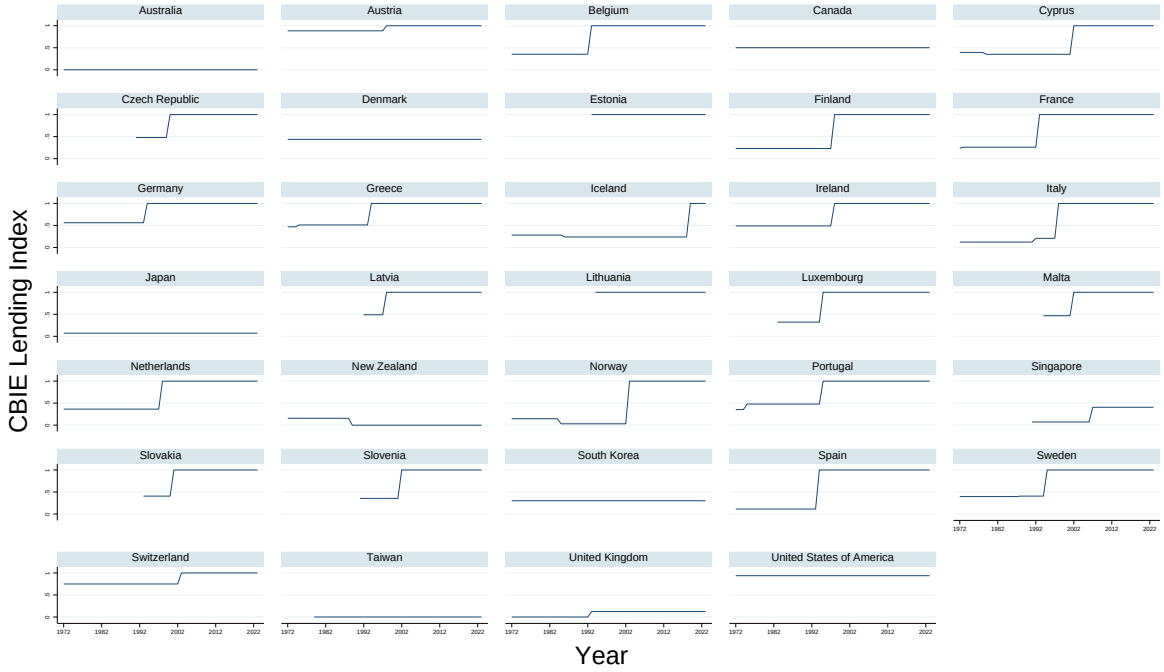


Figure D.2: CBIE Lending index, developing countries (Part I), 1972-2023

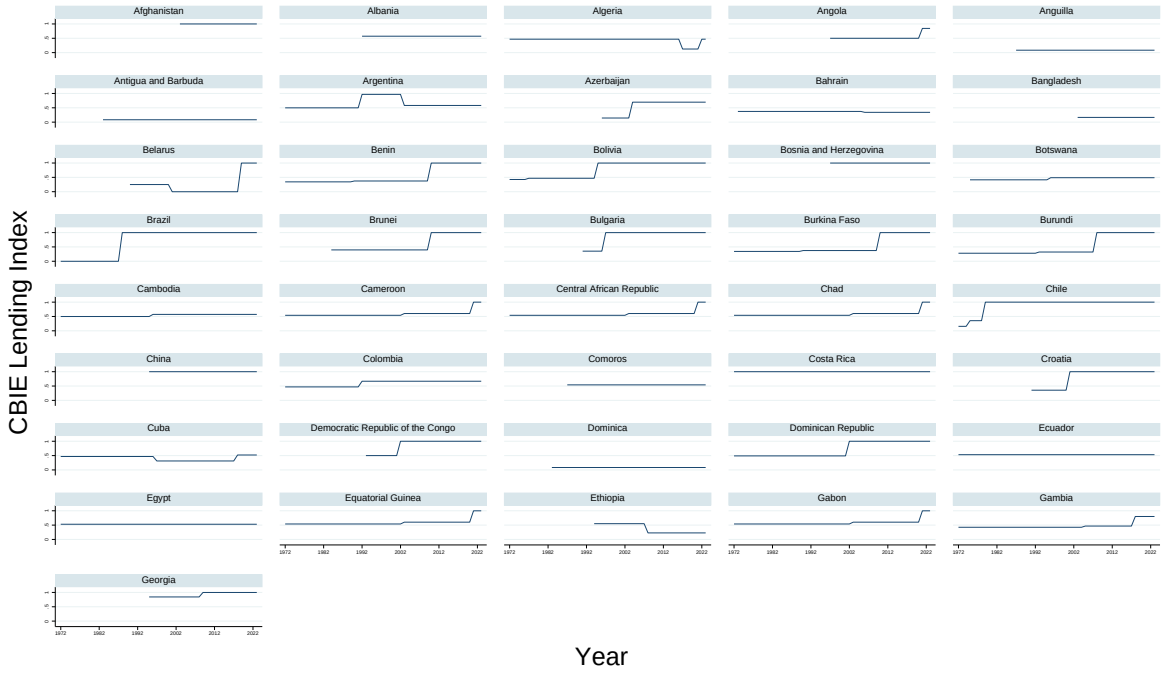


Figure D.3: CBIE Lending index, developing countries (Part II), 1972-2023

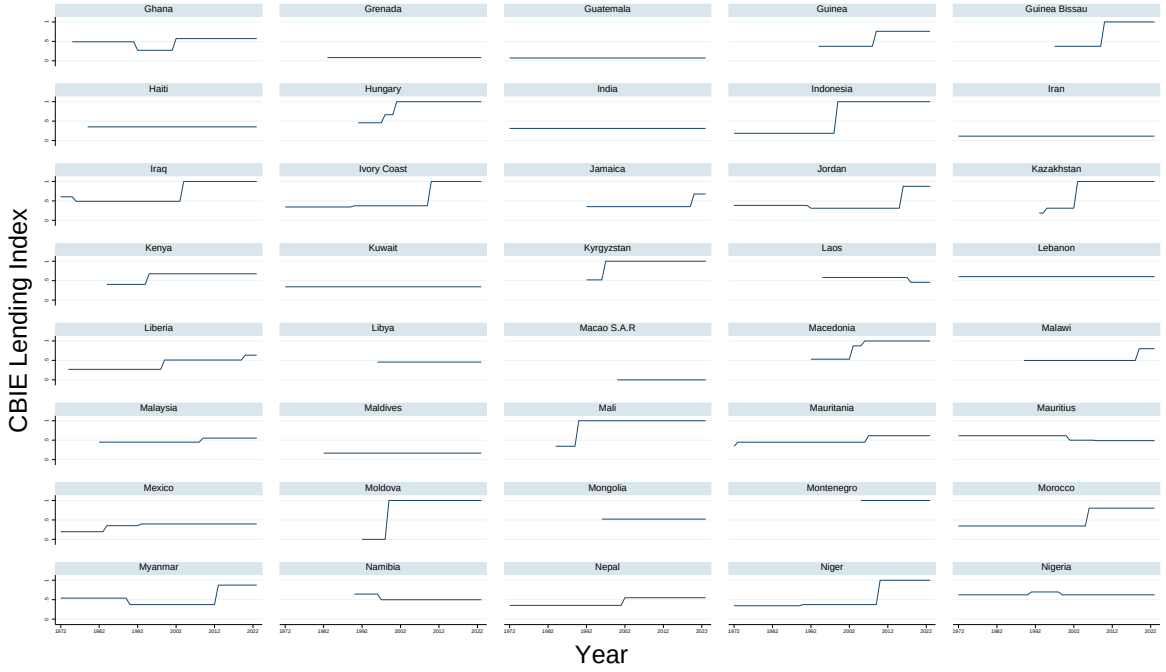
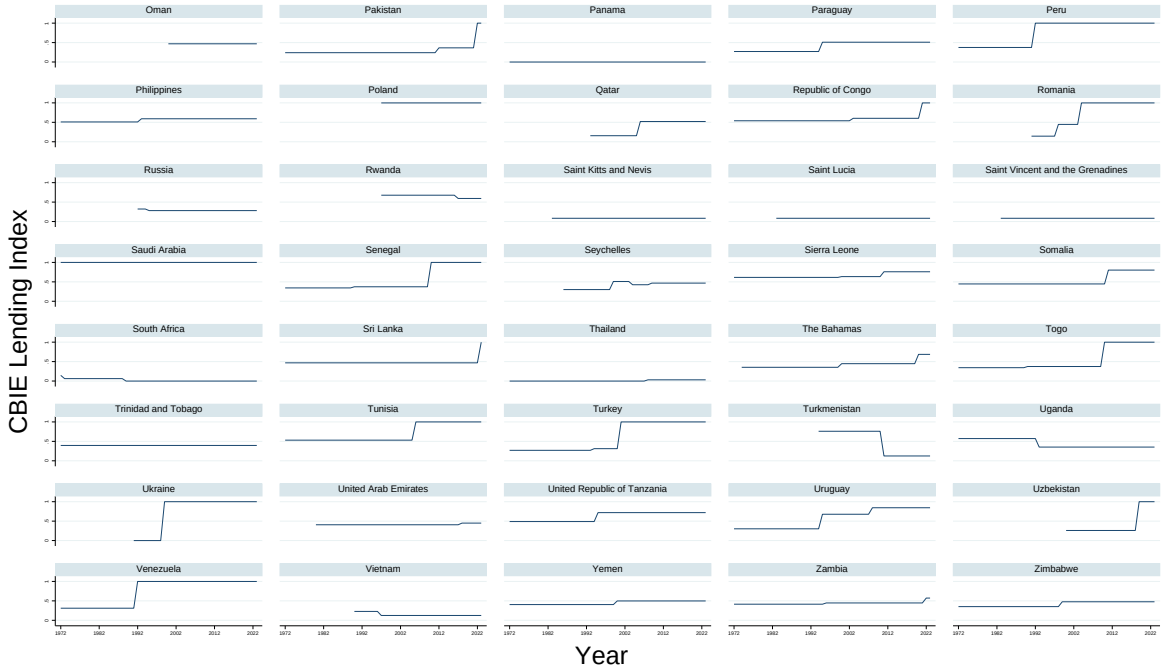


Figure D.4: CBIE Lending index, developing countries (Part III), 1972-2023



E IV Local projection estimation

Table E.1 presents the first stage of our IV estimation.

Table E.1: IV First stage estimates

	Debt-to-GDP			Real effective interest rate		
	All	Advanced	Developing	All	Advanced	Developing
CBIE Lending (t-2)	0.847***	0.862***	0.771***	0.838***	0.822***	0.772***
	(0.023)	(0.073)	(0.037)	(0.028)	(0.096)	(0.042)
Regional reform pressure (t-2)	0.050***	0.016	0.028*	0.043**	-0.022	0.033*
	(0.017)	(0.037)	(0.016)	(0.020)	(0.055)	(0.017)
IMF program exposure (t-1)	0.013***	-0.005	0.016***	0.012***	-0.005	0.016***
	(0.004)	(0.008)	(0.004)	(0.004)	(0.010)	(0.005)
Inflation crisis exposure (t-1)	0.028***		0.016***	0.029***		0.016*
	(0.007)		(0.006)	(0.010)		(0.009)
Kleibergen–Paap F	481.00	104.02	209.96	281.35	131.01	140.54
Observations	3445	1015	2430	2565	900	1665
Countries	122	29	93	105	27	78
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

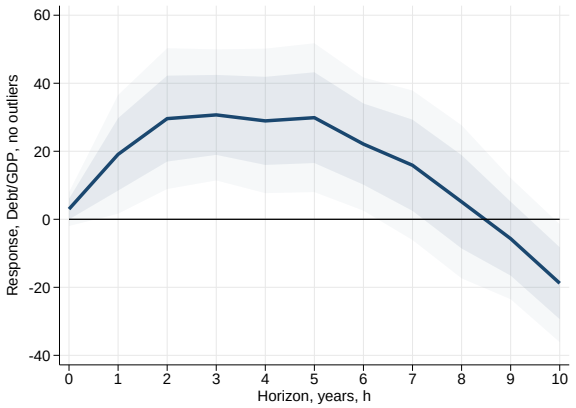
The table reports the first stage of the IV local projections at horizon $h = 0$, for the debt-to-GDP and rea effective-interest-rate specifications, in the full, advanced-economy, and developing-economy samples. The depe dent variable is the lagged reform-induced change in the CBIE Lending index. The excluded instruments are tl level of CBIE Lending lagged two years, regional reform pressure lagged two years, a lagged indicator for IM program exposure in the previous two years, and a lagged indicator for inflation crises (annual inflation abo 30%) in the previous two years. All specifications include the full set of controls, country fixed effects, and ye fixed effects. Driscoll and Kraay (1998) standard errors in parentheses. The Kleibergen–Paap statistic refers the corresponding $h = 0$ IV estimation. ***/**/* denote significance at the 1, 5, and 10 percent levels.

F Additional results

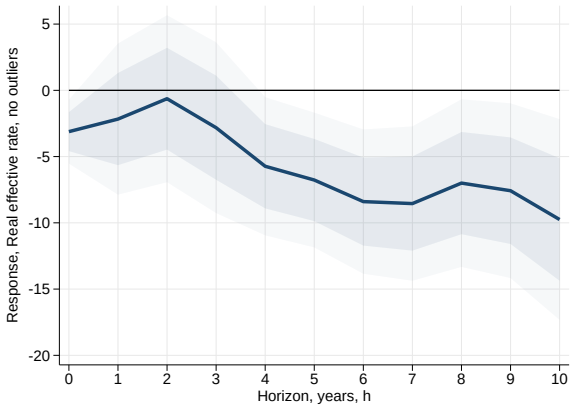
Appendix Figure F.1 shows the baseline IV local projections for developing economies when country-years with extreme implicit-rate or inflation observations (outside the 1st–99th percentile range) are excluded. Appendix Table F.1 reports the five- and ten-year responses of the real effective and the nominal implicit rate when only the debt stock in the denominator of the implicit rate is changed. Appendix Figure F.2 reports the response of total debt-to-GDP estimated on the subsample of country-years with external-debt coverage in the World Bank International Debt Statistics, which is the estimation sample of the foreign-held public debt result in Section 4.7. Appendix Figure F.3 reports the responses of the real and nominal implicit-rate spreads vis-à-vis the United States, estimated without year fixed effects so that the US benchmark is not absorbed by the time effects. Appendix Figure F.4 reports the response of the cumulative price level. Appendix Figure F.5 reports the baseline estimates with exchange-rate-regime indicators, based on the Ilzetzki et al. (2019) de facto classification, added to the control set. Appendix Table F.2 summarises the baseline estimates when fiscal-rule status, inflation-targeting status or the orientation of the chief executive is added as a control, and when the countries with a fiscal-rule change or IMF conditionality near a reform are excluded. Appendix Figure F.6 reports the responses by the orientation of the chief executive. Appendix Figure F.7 reports the classical, non-IV local projections of Section 4.3 for the full sample and for advanced economies.

Figure F.1: IV local projections excluding implicit-rate and inflation outliers, developing countries, 1972–2023.

(a) Debt-to-GDP



(b) Real effective implicit interest rate



Notes: The figure shows the cumulative response of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for developing countries, at horizon h (in years) on the x-axis. The estimates use the baseline IV specification of Section 3.1 after excluding country-years in which the nominal implicit rate, the real effective implicit rate, or inflation falls outside its 1st–99th percentile range. Extreme values of the implicit-rate measures arise mechanically in country-years with very small recorded debt service and in high-inflation episodes, where the Fisher adjustment amplifies measurement error in inflation. The exclusion therefore targets data-quality problems of the interest-rate outcomes, and the baseline debt-to-GDP estimates in the main text retain the full sample. In levels, the implied bounds exclude nominal implicit rates below 0.4 or above 19.4 percent, real effective rates below -60.1 or above 10.7 percent, and inflation below -3.2 or above 448 percent. The exclusion removes about 4 percent of country-year observations, concentrated in high-inflation and crisis episodes, and no country leaves the sample entirely. Panel (a) verifies as a sample-consistency check that the debt-to-GDP response preserves the baseline hump on the trimmed sample. Panel (b) shows that the real-rate response is attenuated relative to the full sample, consistent with the excluded observations being concentrated in high-inflation episodes, where measurement error in the Fisher adjustment is largest. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

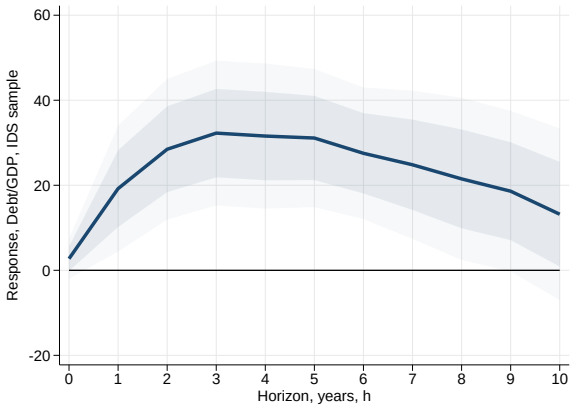
Table F.1: Implicit interest rate responses under alternative debt denominators, developing countries

	Real effective rate		Nominal rate		
Denominator of the implicit rate	5 years	10 years	5 years	10 years	Obs.
Debt-to-GDP ratio (baseline debt series)					
End-of-period stock, t	-18.03***	-31.76***	0.85	-0.60	1672
	(6.07)	(7.62)	(1.40)	(1.99)	
Average of the stocks at t and $t-1$ (baseline)	-18.15***	-32.65***	1.75	-0.04	1649
	(5.89)	(7.31)	(1.61)	(2.07)	
One-year lagged stock, $t-1$	-17.38***	-30.65***	2.51	0.10	1662
	(5.78)	(6.92)	(1.92)	(2.13)	
Average of the stocks at $t-1$ and $t-2$	-18.04***	-32.12***	3.71	0.75	1635
	(6.10)	(7.56)	(2.39)	(2.39)	
Public Finances in Modern History debt series					
End-of-period stock, t	-16.46**	-28.89***	1.48	1.10	1676
	(6.53)	(7.86)	(1.95)	(2.57)	
Average of the stocks at t and $t-1$	-14.01**	-25.06***	1.54	1.17	1639
	(6.00)	(7.01)	(1.90)	(2.33)	
Global Macro Database government debt series					
End-of-period stock, t	-17.63***	-29.79***	2.24	1.92	1694
	(6.07)	(7.23)	(1.83)	(2.38)	
Average of the stocks at t and $t-1$	-17.70***	-30.10***	3.71*	3.66	1681
	(6.00)	(7.12)	(2.18)	(2.65)	

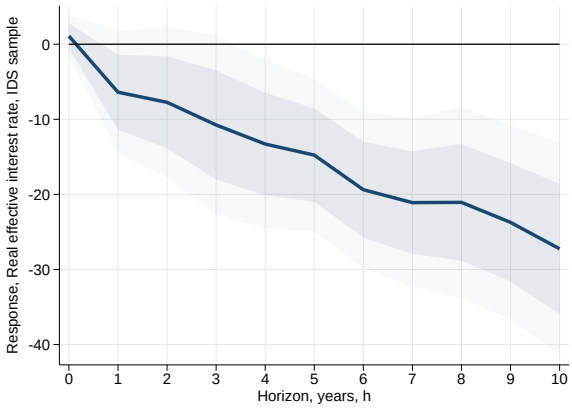
Notes: The table reports the cumulative response of the implicit interest rate on government debt, in percentage points, to a one-unit increase in the 0–1 CBIE Lending index at the five- and ten-year horizons. The estimates are IV local projections for developing countries and follow the baseline specification of Section 3.1, with the outcome replaced by the implicit rate in each row. Each row changes only the debt stock in the denominator of the implicit rate. The numerator is always interest expense from the IMF Public Finances in Modern History database (Mauro et al., 2015). The first four rows use the debt-to-GDP ratio of Section 2.2, the next two rows use the gross debt series of the Public Finances in Modern History database, and the last two rows use consolidated government debt from the Global Macro Database. The denominator is the stock at the end of year t, the average of the stocks at t and $t-1$, the stock at $t-1$, or the average of the stocks at $t-1$ and $t-2$, as indicated in each row. The real effective rate applies the Fisher adjustment with CPI inflation to the nominal rate in the same row. The baseline row is the specification used in the main text, and its real-rate responses are those plotted in panel (b) of Figure 4. Multiplying a response by 0.34, the median positive reform in the full sample, gives the response to a median-sized reform. Driscoll–Kraay standard errors are in parentheses. ***, ** and * denote significance at the 1, 5 and 10 percent levels, based on two-sided tests with normal critical values. Obs. is the number of country-years in the five-year-horizon regressions, which differs across rows because the debt series have different coverage.

Figure F.2: IV local projections on the IDS estimation sample, developing countries, 1972–2023.

(a) Debt-to-GDP



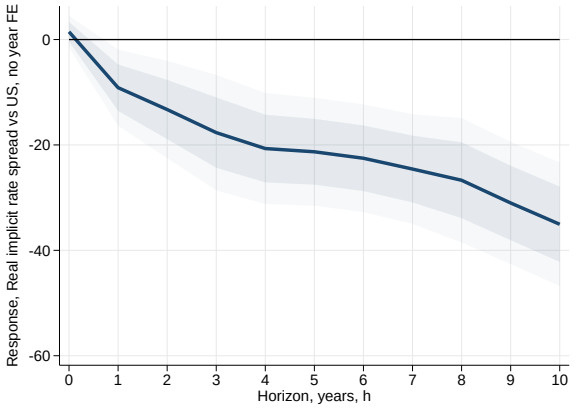
(b) Real effective implicit interest rate



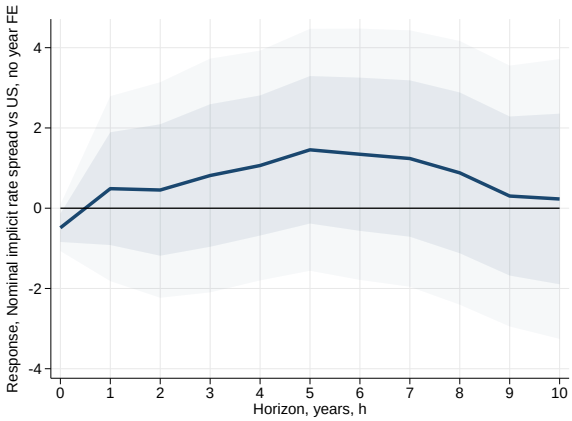
Notes: The figure shows the cumulative response of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for developing countries, at horizon h (in years) on the x-axis. The estimates use the baseline IV specification of Section 3.1 restricted to country-years with external-debt coverage in the World Bank International Debt Statistics, the estimation sample of the foreign-held public debt result in Section 4.7. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

Figure F.3: IV local projections: implicit-rate spreads vis-à-vis the United States, developing countries, 1972–2023.

(a) Real implicit-rate spread vs US



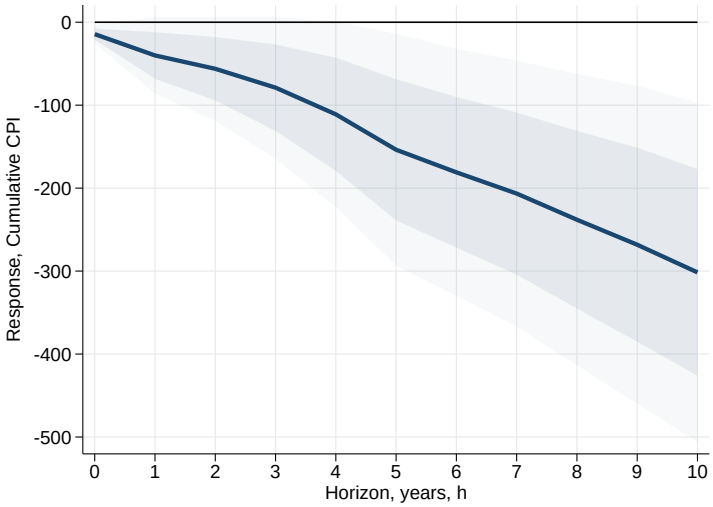
(b) Nominal implicit-rate spread vs US



Notes: The figure shows the cumulative response of the real (panel a) and nominal (panel b) implicit-rate spreads relative to the United States to a reform-induced change in the CBIE Lending index for developing countries, at horizon h (in years) on the x-axis. Both specifications are estimated without year fixed effects, since with year fixed effects the common US benchmark is absorbed by the time effects, and they are therefore descriptive complements to the baseline estimates. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

Figure F.4: IV local projections: cumulative price level, developing countries, 1972–2023.

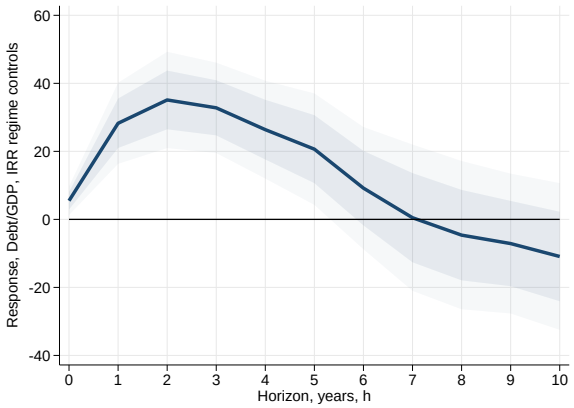
(a) Cumulative CPI (log points × 100)



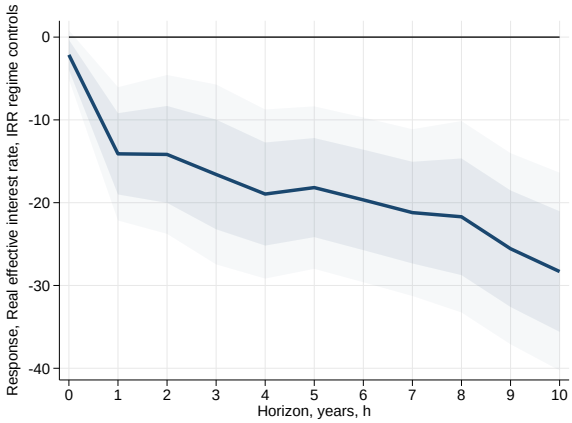
Notes: The figure shows the cumulative response of the log CPI (multiplied by 100) to a reform-induced change in the CBIE Lending index for developing countries, at horizon h (in years) on the x-axis. The estimates use the baseline IV specification of Section 3.1 with contemporaneous inflation removed from the control set. The decline in the price level at medium and long horizons is consistent with the disinflationary effects of central bank independence reforms. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

Figure F.5: IV local projections with exchange-rate-regime controls, developing countries, 1972–2023.

(a) Debt-to-GDP



(b) Real effective implicit interest rate



Notes: The figure shows the cumulative response of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for developing countries, at horizon h (in years) on the x-axis. The estimates use the baseline IV specification of Section 3.1 adding exchange-rate-regime indicators to the control set: peg (coarse categories 1–2), freely falling (5), and dual-market (6) regimes from the Ilzetzki et al. (2019) de facto coarse classification, with floating regimes (3–4) as the omitted category. The classification is available through 2019 and carried forward thereafter. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.

Table F.2: Responses with controls for, or exclusions of, other changes that may accompany reforms, developing countries

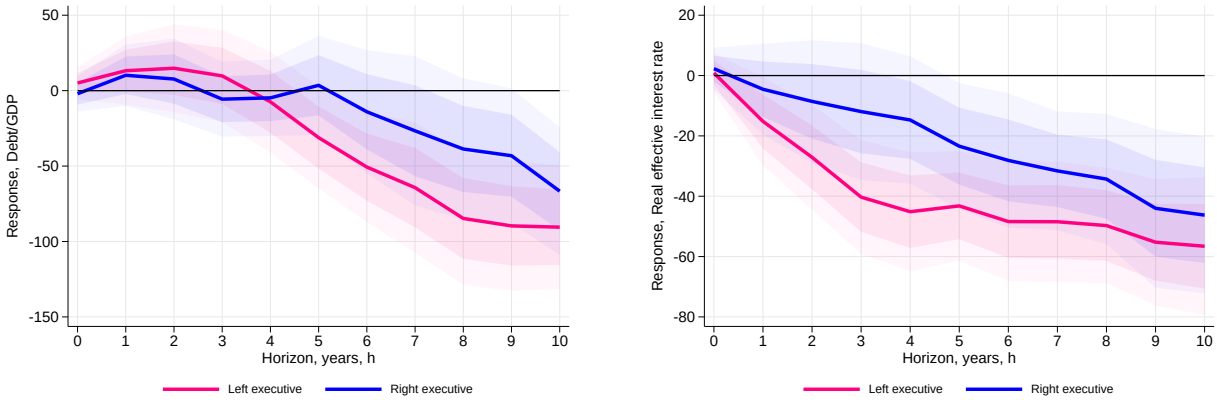
	Real effective rate		Debt-to-GDP		Obs.	
Specification	5 years	10 years	2 years	5 years	Real rate	Debt
Baseline specification						
Baseline	-18.15***	-32.65***	34.13***	21.11**	1649	2410
	(5.89)	(7.31)	(8.81)	(10.29)		
Control added to the baseline						
Exchange-rate-regime indicators	-18.18***	-28.32***	35.12***	20.65**	1649	2410
	(5.98)	(7.28)	(8.63)	(9.99)		
Lagged fiscal-rule status	-17.88***	-34.40***	33.71***	20.23**	1649	2410
	(5.93)	(7.54)	(8.87)	(10.26)		
Lagged inflation-targeting status	-16.57***	-29.57***	34.15***	20.56**	1649	2410
	(5.82)	(6.93)	(8.65)	(10.15)		
Lagged left-executive indicator	-18.32***	-32.73***	34.18***	21.32**	1649	2410
	(5.96)	(7.39)	(8.72)	(10.00)		
Countries excluded from the baseline sample						
Countries with a fiscal-rule change near a reform	-17.13***	-30.33***	36.20***	22.50**	1445	2022
	(5.97)	(7.33)	(8.94)	(9.45)		
Countries with IMF conditionality near a reform	-20.50***	-42.40***	41.04***	23.93**	1484	2227
	(7.41)	(9.38)	(8.56)	(11.40)		

Notes: The table reports the cumulative responses of the real effective implicit interest rate and of the debt-to-GDP ratio, in percentage points and in percentage points of GDP, to a one-unit increase in the 0–1 CBIE Lending index. The estimates are IV local projections for developing countries and follow the baseline specification of Section 3.1, which is the first row. Each further row changes one thing. In the second block a control is added to the baseline: indicators for pegged, freely falling and dual-market exchange-rate regimes, with floating regimes as the omitted category, from the Ilzetzki et al. (2019) de facto classification, lagged fiscal-rule status from the IMF Fiscal Rules Dataset (Alonso et al., 2025), lagged inflation-targeting status, or a lagged indicator that the chief executive is left-wing, from the Database of Political Institutions (Beck et al., 2001), with unclassified executives counted as non-left. In the third block the baseline is estimated without the 15 countries with a fiscal-rule adoption or major revision within one year of a reform, six of which are the 2003 reform of the CEMAC countries, or without the seven countries with a reform within one year of IMF conditionality targeting central bank lending, identified from the 1980–2012 data of Kern et al. (2019). Responses are per unit of the index, so multiplying by 0.34, the median positive reform in the full sample, gives the response to a median-sized reform. Driscoll–Kraay standard errors are in parentheses. ***, ** and * denote significance at the 1, 5 and 10 percent levels, based on two-sided tests with normal critical values. Obs. is the number of country-years at the five-year horizon in the real-rate and in the debt regressions. It is smaller for the real rate because interest data are missing for some country-years, and smaller in the third block because countries are excluded.

Figure F.6: State-dependent IV local projections, by orientation of the chief executive, developing countries, 1972–2023.

(a) Debt-to-GDP

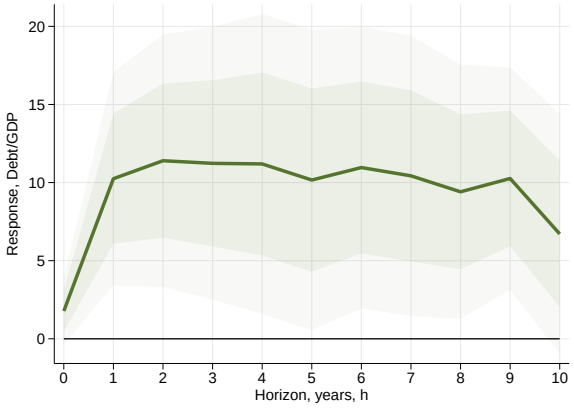
(b) Real effective implicit interest rate



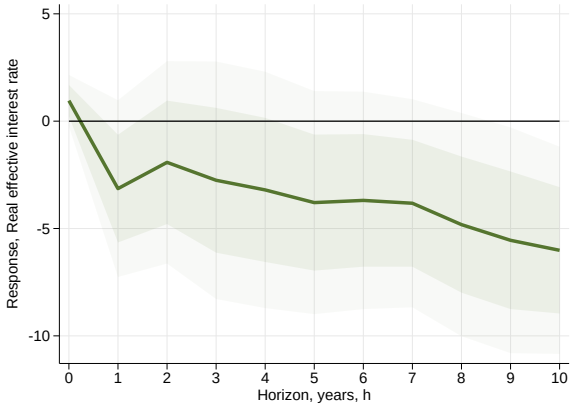
Notes: The figure shows state-dependent IV local-projection responses of the debt-to-GDP ratio (panel a) and the real effective implicit interest rate (panel b) to a reform-induced change in the CBIE Lending index for developing countries, by whether the chief executive was left- or right-wing, at horizon h (in years) on the x-axis. The orientation comes from the Database of Political Institutions (Beck et al., 2001), which classifies executives from 1975 to 2020. The state is the orientation of the executive in office in the year before the country's most recent reform, carried forward until the next reform and, before a country's first reform, the orientation in the previous year. Center and unclassified executives are excluded from the split, so it rests on 664 country-years for the real rate and 867 for the debt ratio at the five-year horizon, against 1,649 and 2,410 in the baseline, and the debt response is imprecisely estimated. The responses are estimated in the interaction form of equation (5), in which the treatment is split into its two state components within a single regression and each component is instrumented with the corresponding state interaction of the instrument set. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors. The equality of the two responses is tested at each horizon and the results are discussed in the text.

Figure F.7: Classical local projections, full sample and advanced economies, 1972–2023.

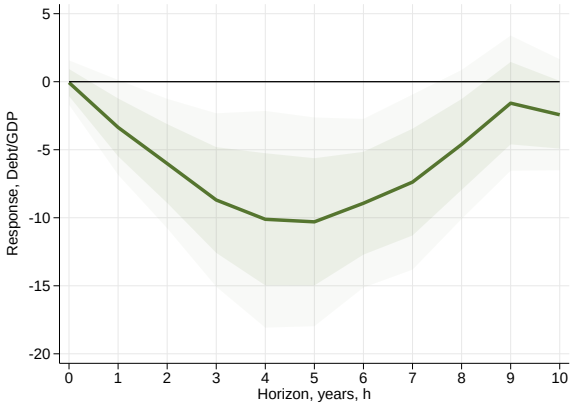
(a) Debt-to-GDP, all countries



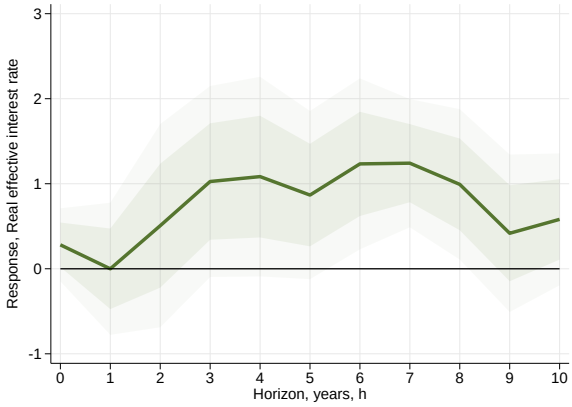
(b) Real effective implicit interest rate, all countries



(c) Debt-to-GDP, advanced economies



(d) Real effective implicit interest rate, advanced economies



Notes: The figure shows the cumulative response of the debt-to-GDP ratio (panels a and c) and the real effective implicit interest rate (panels b and d) to a reform-induced change in the CBIE Lending index, at horizon h (in years) on the x-axis, estimated by classical local projections. The top row pools all countries and the bottom row restricts the sample to advanced economies. The reform variable is the change in CBIE Lending implied by a legal reform, carried forward until a subsequent reform or reversal, and it is not instrumented. The specification otherwise follows the baseline structure of Section 3.1: cumulative responses, year fixed effects and four fixed lags. Responses are expressed per unit change in the 0–1 CBIE Lending index. Shaded areas denote the 68% and 90% confidence intervals using Driscoll and Kraay (1998) standard errors.