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# Fiscal policy and its watchers: do fiscal councils help close the tax gap?

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## Abstract

Independent fiscal institutions, or fiscal councils, have no authority to set or collect taxes, yet their scrutiny of fiscal policy may shape governments' incentives to mobilize revenue. This paper examines whether fiscal council adoption helps close the tax gap, defined as the share of estimated revenue potential that remains uncollected. Using a panel of 159 countries over 1990–2021, we estimate revenue potential with a stochastic frontier and exploit the staggered adoption of fiscal councils using a difference-in-differences estimator robust to heterogeneous treatment effects. On average, countries leave about 36% of their estimated revenue potential uncollected, with a mean tax gap of 0.38 in emerging and developing economies compared with 0.30 in advanced economies. We find that fiscal council adoption reduces the tax gap by 5.0 percentage points, or roughly 15% of its pre-adoption level. The effect builds up gradually after adoption and holds across a range of robustness checks, including alternative frontier specifications and estimators. It is concentrated in emerging and developing economies, where the scope for improving revenue mobilization is greatest, and remains significant across pre-adoption fiscal conditions, whether or not a fiscal rule is in place and whether public debt and deficits are high or low. The adjustment operates on the revenue side rather than the expenditure side, mainly through VAT, trade and property taxes, consistent with governments mobilizing revenue sources that were available but left untapped. Overall, the findings suggest that fiscal councils can support domestic revenue mobilization by strengthening fiscal scrutiny.

**Keywords:** Fiscal councils; Domestic revenue mobilization; Tax gap; Stochastic frontier analysis; Staggered difference-in-differences

**JEL Classification:** E62, H11, H20, H26, O23

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# 1 Introduction

In recent years, public finances have deteriorated significantly in many economies. Following successive responses to the Covid-19 pandemic and, more recently, rising geopolitical tensions, public debt has reached historically high levels, while interest payments absorb an increasing share of government budgets. According to the IMF, global public debt reached nearly 94% of world GDP in 2025 and, under unchanged policies, is projected to reach 100% of GDP by 2029, further increasing the burden of interest payments (IMF, 2026). These pressures on public finances are not merely cyclical. They also reflect structural financing needs related, among other factors, to demographic, security, and climate challenges.

In this context, governments' ability to mobilize domestic resources effectively has become a central component of fiscal sustainability, particularly in emerging and developing economies where access to external financing is becoming increasingly constrained. Official development assistance, for instance, declined by 23.1% in real terms in 2025, the largest annual contraction ever recorded, following an already significant decline in 2024, and the OECD projects a further decrease of around 7% in 2026 (OECD, 2026). The fiscal challenge facing governments therefore goes well beyond containing expenditure and public debt. It also involves mobilizing more effectively the resources that their economies already have at their disposal.

Against this background, many countries have progressively strengthened their fiscal institutions. Recent decades have witnessed the spread of fiscal rules, fiscal responsibility laws and, more recently, fiscal councils. Fiscal councils are independent public institutions that assess or produce macroeconomic and fiscal forecasts, analyze fiscal sustainability, evaluate the cost of fiscal measures, and monitor compliance with fiscal rules (Calmfors & Wren-Lewis, 2011; Debrun et al., 2013). Unlike tax administrations, however, they do not collect taxes and generally lack instruments to impose their recommendations on governments. Their influence operates primarily through independent information, fiscal scrutiny, public communication, and the reputational costs associated with non-credible fiscal policy.

The existing literature shows that these councils can improve the accuracy of fiscal forecasts, reduce optimism bias, and promote fiscal discipline and compliance with fiscal rules (Debrun & Kinda, 2017; Beetsma et al., 2019; Reuter, 2019; Căpraru et al., 2022). A distinct literature emphasizes the role of political and fiscal institutions in building fiscal capacity (Besley & Persson, 2009, 2011; Ricciuti et al., 2019), while recent evidence suggests that fiscal rules can foster revenue mobilization (Nomo Beyala, 2025). By contrast, the relationship between fiscal councils and revenue mobilization remains underexplored. This gap matters because an improvement in the fiscal balance alone does not reveal whether the adjustment reflects lower expenditure or more effective revenue mobilization.

This paper therefore examines whether fiscal councils help governments make better use of their available fiscal potential. Rather than focusing solely on observed tax revenues, we compare revenues actually collected with an estimated frontier conditional on each economy's structural characteristics. The tax gap thus captures the share of estimated fiscal potential that remains unexploited. Our research question can therefore be stated as follows: does the adoption of a fiscal council help countries move closer to their fiscal frontier and reduce their tax gap?

To answer this question, we use a panel of 159 countries over the 1990–2021 period, relying on the staggered difference-in-differences estimator of Callaway and Sant'Anna (2021), which compares adoption cohorts with countries not yet treated or never treated, conditional on pre-adoption covariates.

Our results show that fiscal council adoption reduces the tax gap by 5.0 percentage points, about

15% of its pre-adoption level. The effect builds up gradually in the years following adoption, and is robust to additional confounders, alternative frontier specifications, alternative revenue data, sample restrictions and alternative estimators. It is concentrated in emerging and developing economies, where the scope for additional revenue mobilization is greatest, and point estimates are also larger in resource-rich countries and where the initial tax gap is high. The effect does not depend on pre-adoption fiscal conditions, as it remains significant whether or not a fiscal rule was in place and whether public debt and deficits were high or low, nor does it depend on the measured strength of the council. The mechanism analysis shows that adjustment operates through revenues rather than expenditures. Total revenues rise by 2.4 percentage points of GDP, accounting for most of the 2.9-point improvement in the cyclically adjusted balance, mainly through VAT, import duties and property taxes, whereas income taxes and public expenditure show no significant response. The effect also tends to be larger in election years. These patterns are consistent with a simple conceptual framework in which fiscal councils raise the political cost of fiscal slippages and induce governments to adjust first through the revenue margins that are least costly to mobilize.

This paper makes three contributions. First, it extends the literature on fiscal councils, which has focused primarily on forecast accuracy, compliance with fiscal rules and fiscal balances, to the revenue side of fiscal policy. Second, it contributes to the literature on fiscal capacity and tax effort by identifying an oversight institution with no tax-collection authority as a potential driver of revenue mobilization, thereby complementing recent work on fiscal rules. Third, it combines a stochastic-frontier measure of the tax gap, constructed independently of treatment, with difference-in-differences estimators designed for staggered adoption and heterogeneous treatment effects, and shows that the choice of estimator is decisive in this setting. More broadly, our findings suggest that, at a time of rising financing needs and declining external assistance, stronger fiscal oversight institutions may help support domestic revenue mobilization, particularly where untapped fiscal potential is greatest.

The remainder of the paper is organized as follows. Section 2 presents the related literature and conceptual framework. Section 3 describes the data and the construction of the tax gap. Section 4 outlines the empirical strategy. Section 5 presents the main results, and Section 6 reports robustness checks. Section 7 examines treatment-effect heterogeneity, Section 8 investigates the underlying mechanisms, and Section 9 concludes.

## 2 Related literature and conceptual framework

### 2.1 Fiscal councils: information and independent fiscal scrutiny

Fiscal councils are non-partisan public institutions tasked with independently assessing fiscal policy and the sustainability of public finances (Debrun et al., 2013; Debrun & Kinda, 2017; Davoodi et al., 2022). Despite substantial institutional diversity, they generally share a formal mandate, an explicit role as fiscal watchdogs, and a responsibility to contribute to the public debate on fiscal policy. Their functions typically include producing or assessing macroeconomic and budgetary forecasts, evaluating the fiscal policy stance, monitoring compliance with fiscal rules, and costing new policy measures (Debrun et al., 2013).

Their development is intended to address the biases that may affect the conduct of fiscal policy. Common-pool problems, electoral incentives and time inconsistency can induce governments to favor short-term policies and accumulate excessive deficits (Alesina & Tabellini, 1990; Von Hagen & Harden, 1995; Krogstrup & Wyplosz, 2010; Wyplosz, 2012). Fiscal rules constitute a first

institutional response to these biases by imposing numerical constraints on selected fiscal aggregates (Kopits & Symansky, 1998). However, their effectiveness remains conditional on their design and on governments’ political commitment to comply with them, which cannot always be taken for granted (Debrun et al., 2009; Calmfors & Wren-Lewis, 2011; Schaechter et al., 2012; Debrun & Kinda, 2017; Beetsma et al., 2019). Moreover, rules may give rise to circumvention strategies or creative accounting when governments seek to satisfy formal constraints without substantially changing their underlying behavior (Milesi-Ferretti, 2004; Von Hagen & Wolff, 2006).

Fiscal councils address these limitations through a different channel: information and fiscal scrutiny. Governments may have incentives to rely on overly optimistic macroeconomic or budgetary assumptions, allowing them to present a more favorable fiscal trajectory and postpone necessary adjustments (Jonung & Larch, 2006; Frankel & Schreger, 2013). By producing independent assessments and making them publicly available, fiscal councils reduce information asymmetries between the executive, parliament and citizens, and make deviations from fiscal objectives more visible. Greater scrutiny may, in turn, increase the political and reputational costs of poor fiscal performance (Calmfors & Wren-Lewis, 2011; Beetsma & Debrun, 2016, 2018; Davoodi et al., 2022). Empirical studies broadly support this disciplinary role. The presence of a fiscal council has been associated with stronger fiscal balances (Maltritz & Wüste, 2015; Căpraru et al., 2022), greater compliance with fiscal rules (Beetsma et al., 2019; Reuter, 2019), more accurate and less optimistic forecasts (Debrun & Kinda, 2017; Beetsma et al., 2019), more frequent and durable fiscal adjustments (Chrysanthakopoulos & Tagkalakis, 2023), less procyclical fiscal policy (Chrysanthakopoulos & Tagkalakis, 2024), and lower sovereign risk premia, particularly when fiscal councils operate alongside fiscal rules (Afonso & Jalles, 2019; Căpraru et al., 2025).

However, this literature remains primarily focused on deficits, debt and expenditure. The revenue side has received considerably less attention, even though fiscal discipline may also be achieved through a more effective mobilization of existing resources. In particular, whether an independent oversight institution can influence government behavior regarding domestic revenue mobilization remains largely unexplored. This question is especially intriguing because fiscal councils neither collect taxes themselves nor exercise any direct authority over the tax administration. This study addresses this missing dimension.

## 2.2 From fiscal capacity to the tax gap

The central question of this paper relates to the concept of fiscal capacity, generally defined as the ability of the state to mobilize revenues from the available tax base (Besley & Persson, 2009, 2011, 2014). Fiscal capacity constitutes a core pillar of state capacity because it finances essential public goods, reduces dependence on external financing (Besley & Persson, 2014; Albers et al., 2023), supports long-run growth (Dincecco & Prado, 2012; Dincecco & Katz, 2016), and is associated with better institutional quality (Baskaran & Bigsten, 2013). Yet fiscal capacity remains highly uneven across countries and is particularly weak in many developing economies (Besley & Persson, 2014). Measuring fiscal capacity, however, raises a fundamental challenge. The tax revenue-to-GDP ratio, which is widely used, combines two distinct dimensions: what a country could potentially mobilize given its structural characteristics, and what it actually mobilizes. A low revenue level may therefore reflect either a structurally narrow tax base or a failure to fully exploit available fiscal potential. Likewise, the share of direct taxes, another commonly used measure (Besley & Persson, 2009; Dincecco & Prado, 2012; Nomo Beyala, 2025), provides information on the structure and sophistication of the tax system but does not directly capture the distance between observed revenues and their potential.

A growing literature on tax effort seeks precisely to distinguish these two dimensions by estimating a tax frontier, that is, the maximum level of revenue consistent with an economy’s structural characteristics, using stochastic frontier methods (Cyan et al., 2013; Fenochietto & Pessino, 2013; Mawejje & Sebudde, 2019; McNabb et al., 2021; Benitez et al., 2023). Tax effort measures actual revenues relative to this frontier, while the tax gap captures the distance between the two, that is, the share of estimated fiscal potential that remains unexploited. This concept should be distinguished from the compliance gap estimated by tax administrations, such as the VAT gap, which measures the difference between tax liabilities and actual payments. Here, the tax gap refers instead to the distance between actual revenues and the economy’s estimated fiscal potential. This measure is particularly well suited to our research question because, by conditioning revenue potential on the structural determinants of tax collection, it shifts attention away from the level of revenues toward the government’s ability to exploit the fiscal potential available to it. This is precisely the margin along which a fiscal institution can exert influence.

## 2.3 From fiscal scrutiny to revenue mobilization

How can an institution with no direct taxing authority influence the tax gap? We propose a simple conceptual chain to explain this relationship. The starting point is the information channel. By independently producing or assessing fiscal forecasts, a fiscal council makes it more difficult for governments to rely persistently on overly optimistic revenue assumptions. It thereby reveals more accurately financing needs and deviations between announced fiscal paths and realized outcomes. Existing studies show that fiscal councils can reduce forecast errors and optimism bias (Debrun & Kinda, 2017; Beetsma et al., 2019). Better information, in turn, increases the visibility of fiscal imbalances, making them easier for parliament, the media, financial markets and citizens to observe. This greater visibility raises the political and reputational costs of fiscal slippages (Beetsma & Debrun, 2016, 2018). Governments therefore have stronger incentives to undertake actual fiscal adjustment rather than rely on unrealistic revenue assumptions (Chrysanthakopoulos & Tagkalakis, 2023, 2024).

Fiscal adjustment can take the form of expenditure cuts, revenue increases, or a combination of both. The literature on fiscal consolidations has not established an optimal composition between the two (Afonso & Alves, 2023; Balasundharam et al., 2026), but it shows that the composition actually implemented often differs from what was initially announced. Consolidation plans frequently place greater emphasis on expenditure cuts, yet spending is often reduced by less than planned, leading governments to rely more heavily on revenue measures (Abbas et al., 2012). Expenditure rigidities, stemming from the weight of public wages, transfers and multi-year commitments, can partly account for this discrepancy. Revenue measures also tend to be more durable in countries where the revenue-to-GDP ratio is relatively low and untapped revenue sources remain available, such as VAT or property taxation (Adam et al., 2003; Gupta et al., 2004), whereas expenditure-based adjustments tend to be more persistent when initial revenue levels are already high (Tsibouris et al., 2006; Balasundharam et al., 2026).

It is important, however, to distinguish between raising taxes and improving their effective collection. Revenue-based consolidations that rely on higher tax rates may entail substantial electoral costs (Arias & Stasavage, 2019). By contrast, broadening tax bases and reducing fraud and evasion can generate sustainable revenue gains with more diffuse political costs (Cournède, 2013). It is this second margin that we expect to be particularly relevant when governments are subject to greater fiscal scrutiny: stronger efforts to mobilize revenue from existing tax bases, especially those that are easier to observe.

Finally, this mechanism should be particularly relevant around elections. Fiscal consolidations are less likely to succeed when an election is imminent (Gupta & Mulas-Granados, 2004; Clements et al., 2025), while electoral incentives can amplify deficit bias (Alesina & Tabellini, 1990; Brender & Drazen, 2005). By making fiscal slippages more visible precisely when voters evaluate government performance, a fiscal council may help constrain such behavior.

This mechanism can be summarized in a simple stylized framework. Let  $\bar{T}_j$  denote the revenue potential associated with tax instrument  $j$ , and let  $e_j \in [0, 1]$  denote the share of this potential that is effectively mobilized, so that

$$T_j = e_j \bar{T}_j. \quad (1)$$

Aggregate tax effort is then  $TE = \sum_j e_j \bar{T}_j / \sum_j \bar{T}_j$ , and the tax gap is  $1 - TE$ . The fiscal balance is given by

$$B = \sum_j e_j \bar{T}_j - G, \quad (2)$$

where  $G$  denotes public expenditure. Increasing effective revenue mobilization is costly, adjusting expenditure is also costly, and the government incurs a political cost when the fiscal balance falls short of its objective. The government therefore chooses  $\{e_j\}$  and  $G$  to minimize

$$\mathcal{L} = \sum_j \frac{\gamma_j}{2} e_j^2 + \frac{\kappa}{2} (G - \bar{G})^2 + \lambda (FC, E) \Phi(\bar{B} - B), \quad (3)$$

where  $\gamma_j$  captures the marginal cost of mobilizing additional revenue from instrument  $j$ ,  $\kappa$  the cost of adjusting expenditure relative to its desired level  $\bar{G}$ , and  $\Phi(\cdot)$ , increasing and convex, the political or reputational cost of deviations from the fiscal objective  $\bar{B}$ . The weight  $\lambda$  depends on the presence of a fiscal council ( $FC$ ) and on electoral salience ( $E$ ). By improving information and making fiscal deviations more visible, a fiscal council increases their political cost:

$$\frac{\partial \lambda}{\partial FC} > 0. \quad (4)$$

Denoting by  $m \equiv \lambda \Phi'(\bar{B} - B)$  the marginal political cost of a deviation, the first-order conditions yield

$$e_j^* = \frac{\bar{T}_j}{\gamma_j} m, \quad G^* = \bar{G} - \frac{m}{\kappa}. \quad (5)$$

Since  $m$  increases with  $\lambda$  (Appendix A.4), the adoption of a fiscal council raises effort on every tax instrument and lowers expenditure, in proportions governed by  $\gamma_j$  and  $\kappa$ .

This simple framework yields three empirical predictions. First, the adoption of a fiscal council strengthens fiscal adjustment and, when expenditure is relatively rigid ( $\kappa$  high), this adjustment operates primarily through stronger revenue mobilization rather than expenditure cuts, thereby reducing the tax gap. Second, since  $\partial e_j^* / \partial m = \bar{T}_j / \gamma_j$ , the response should be stronger for tax instruments with lower mobilization costs, typically those based on more observable tax bases, such as consumption and cross-border transactions, rather than direct taxation. Third, if the disciplining effect of a fiscal council is larger when fiscal performance is more salient ( $\partial^2 \lambda / \partial FC \partial E > 0$ ), its effect should be stronger during election periods. These predictions guide the mechanism analysis in Section 8.

## 3 Data

### 3.1 Main data sources

We analyse the effect of fiscal councils on the tax gap using a panel covering 159 countries over the period 1990–2021.

Our treatment variable,  $FC_{it}$ , takes the value one from the year in which the first fiscal council becomes operational in country  $i$ , and zero beforehand. It is drawn from the IMF Fiscal Council Dataset (Alonso-Albarran et al., 2025), which documents each council’s year of establishment, legal basis and mandate, its functions (macroeconomic and fiscal forecasting, policy costing, monitoring of fiscal rules and sustainability analysis), as well as its operational independence and communication policy. Since no council was abolished during the study period, treatment is absorbing. The five countries that already had a fiscal council in 1990 (Austria, Belgium, Denmark, the Netherlands and the United States) have no observed pre-treatment period; they contribute to the estimation of the fiscal frontier but not to the identification of the adoption effect. To characterize fiscal council design, we construct a Fiscal Council Strength Index (FCSI) following Alonso-Albarran et al. (2025). The index aggregates three sub-indices, each normalized between 0 and 1: tasks and instruments, independence and accountability, and communication policy. Because the index reflects the current characteristics of each council, it is time-invariant within adopters and is used to examine cross-council differences in the heterogeneity analysis.

Our dependent variable is the tax gap, defined as the share of estimated revenue potential that remains uncollected and derived from a stochastic frontier analysis. Estimating this measure requires internationally comparable tax revenue data. We therefore use the tax revenue-to-GDP ratio excluding social contributions from the IMF World Revenue Longitudinal Database (WoRLD) (Mansour et al., 2025), expressed in logarithms in the frontier estimation. It provides broad country and time coverage and harmonizes revenue data using a common methodology based on the Government Finance Statistics (GFS) framework, making it particularly suitable for cross-country comparisons of fiscal capacity and revenue mobilization. As a robustness check, we re-estimate the frontier and the resulting tax gap using the UNU-WIDER Government Revenue Dataset (UNU-WIDER, 2025), whose decomposition of tax revenues by category is also used in the mechanism analysis.

Following the literature (Fenochietto and Pessino, 2013; Mawejje and Sebudde, 2019; McNabb et al., 2021), the main structural determinants of the fiscal frontier, drawn from the World Bank’s World Development Indicators (WDI), include GDP growth, trade openness, the share of agriculture in value added, natural-resource rents and the urbanization rate. As a robustness check, we also estimate an augmented frontier that adds the level of development, measured by log GDP per capita and its square, the logarithm of population density, both from the WDI, and a human capital index from the Penn World Table (Feenstra et al., 2015).

In addition to these main sources, we draw on several complementary databases. Public debt, the cyclically adjusted balance, as well as total government revenue and expenditure come from the IMF Fiscal Monitor, while the presence of fiscal rules is taken from the Fiscal Rules Dataset. Finally, we construct an indicator of IMF lending, equal to one in years during which a country is under an IMF financing arrangement and zero otherwise, by combining the data of Vreeland (2007), which cover the earlier years, with the IMF Monitoring of Fund Arrangements (MONA) database, which covers programs approved from 2002 onward; we verify that both sources are consistent over their overlapping years. This indicator allows us to control for IMF conditionality, which may involve both reforms of fiscal institutions and revenue-mobilization measures.

### 3.2 Tax gap measurement

The tax gap, defined as the share of potential tax revenue that remains uncollected, is measured using a stochastic frontier approach based on the structural characteristics of each economy. Following the stochastic frontier approach of Aigner et al. (1977), applied to taxation by Fenochietto and Pessino (2013), tax revenues of country  $i$  in year  $t$  are modeled as

$$\ln T_{it} = \alpha + x'_{it}\beta + \lambda_t + v_{it} - u_{it}, \quad v_{it} \sim \mathcal{N}(0, \sigma_v^2), \quad u_{it} \sim \mathcal{N}^+(0, \sigma_u^2), \quad (6)$$

where  $T_{it}$  denotes tax revenue excluding social contributions as a percentage of GDP,  $x_{it}$  the structural determinants of the tax base, and  $\lambda_t$  year fixed effects. The error term has two components:  $v_{it}$ , a symmetric noise term capturing measurement error and transitory shocks, and  $u_{it} \geq 0$ , a one-sided term capturing the shortfall of observed revenue relative to its potential.

This asymmetry allows the model to distinguish inefficiency from random fluctuations. The noise term  $v_{it}$  can be positive or negative, so that transitory shocks may push observed revenue temporarily above or below the frontier, whereas the inefficiency term  $u_{it}$  can only reduce revenue; systematic shortfalls relative to the frontier are therefore attributed to inefficiency, while deviations in both directions are treated as noise.

The vector  $x_{it}$  includes GDP growth, trade openness, the share of agriculture in GDP, natural-resource rents and the urbanization rate, which characterize the size and taxability of the tax base (Fenochietto and Pessino, 2013; Mawejje and Sebudde, 2019; McNabb et al., 2021). GDP growth captures the cyclical position of the economy. Because tax revenues tend to rise in expansions and fall in recessions, controlling for it prevents temporary booms and downturns from being interpreted as changes in inefficiency. More urbanized and open economies generally provide broader and more traceable tax bases, particularly through formal firms and international trade, whereas large agricultural sectors are typically more difficult to tax because of informality, self-consumption, underreporting and preferential tax treatment. Natural-resource rents may weaken incentives to develop non-resource taxation, in line with the literature on the resource curse (Bornhorst et al., 2009; Besley and Persson, 2011; Crivelli and Gupta, 2014; Masi et al., 2024). Year fixed effects absorb shocks common to all countries, such as financial crises or waves of fiscal consolidation. The frontier is estimated independently of fiscal councils, so that the tax gap cannot be mechanically correlated with the treatment.

The inefficiency term  $u_{it}$  is not directly observed; only the composite residual  $\varepsilon_{it} = v_{it} - u_{it}$  is. Jondrow et al. (1982) show that  $u_{it}$  can be predicted from its distribution conditional on  $\varepsilon_{it}$ . Building on this result, Battese and Coelli (1988) derive the conditional expectation of technical efficiency, which we use as our measure of tax effort:

$$TE_{it} = \mathbb{E}[\exp(-u_{it}) \mid \varepsilon_{it}] \in (0, 1], \quad (7)$$

and we define the tax gap as

$$Gap_{it} = 1 - TE_{it}. \quad (8)$$

The tax gap is therefore an inefficiency score bounded between 0 and 1, which measures the share of estimated tax potential that is left untapped. For instance, a value of 0.30 indicates that a country collects approximately 70% of its revenue potential, conditional on its structural characteristics. Two features are important for its interpretation. First, the tax gap is a relative measure and does not rank countries according to their level of tax revenue. A country with relatively low revenues may thus operate close to its estimated frontier, while another with higher revenues may remain further from it. Second, the gap captures the overall shortfall from the estimated

frontier, regardless of whether it reflects limited administrative capacity and compliance or policy choices such as low statutory rates or extensive exemptions. Because our identification exploits within-country changes around fiscal council adoption, time-invariant cross-country differences in tax policy do not drive the estimated treatment effect.

Our baseline specification is a pooled stochastic frontier with half-normal inefficiency, estimated by maximum likelihood on country-year observations over 1990–2021. This choice is motivated by two considerations. First, the pooled frontier defines a common benchmark across countries, so that the tax gap measures how much a country collects relative to economies with comparable structural characteristics, which corresponds directly to our research question. Models with country-specific effects, such as the true fixed-effects model of Greene (2005), instead absorb all time-invariant characteristics into the frontier, potentially including part of persistent differences in tax effort. Second, inefficiency is allowed to vary freely over time within each country. This feature is essential to our identification strategy, since a model in which inefficiency were constant within countries would leave no within-country variation in the tax gap to relate to fiscal council adoption. We assess the sensitivity of our results to these choices in Section 6, using the true fixed-effects model, an exponential distribution for inefficiency, an alternative source of tax revenue data, as well as an augmented frontier.

### 3.3 Stylized facts

Figures 1, 2 and 3 highlight several descriptive features of the data that motivate the empirical analysis.

Figure 1 documents the diffusion and timing of fiscal council adoption over the sample period. Panel A shows that fiscal councils remained relatively uncommon until the mid-2000s, with only about ten countries operating one in 2006, before spreading rapidly over the following decade to reach 49 countries in 2021. Their early presence was concentrated among advanced economies, but adoption subsequently extended to emerging and developing economies, where it continued after adoption among advanced economies had levelled off. By the end of the sample period, fiscal councils were therefore no longer confined to a small group of advanced countries.

Panel B highlights the staggered nature of this diffusion. New adoptions are dispersed over time but are particularly concentrated between the global financial crisis and the mid-2010s, with 27 of the 44 adoptions observed within the sample window taking place between 2010 and 2015. European Union countries account for a large part of this wave, although several non-EU economies also established fiscal councils during the same period, and others did so before 2010 or after 2015. The timing of adoption therefore varies substantially across countries, while a sizeable share of reforms occurred during a common period of significant fiscal and institutional change. These features motivate an empirical strategy that explicitly accommodates staggered treatment timing and differences between adopting and non-adopting countries.

Figure 2 turns to the main outcome of interest. Panel A shows substantial dispersion in the estimated tax gap across country-year observations. Most observations lie between 0.15 and 0.45, with a mean of about 0.36, meaning that the average country collects roughly two-thirds of its estimated revenue potential. The pronounced right tail indicates that some countries remain considerably further from their estimated fiscal frontier. This variation points to substantial differences across countries and over time in the extent to which available fiscal potential is effectively mobilized.

Panel B reports the evolution of the average tax gap according to contemporaneous fiscal council status. Country-years with a fiscal council generally display a lower average tax gap than those without, and the difference becomes particularly visible after 2012, when the average gap among

countries with a council falls below 0.30 while it rises slightly among countries without one. This pattern is purely descriptive and should not be interpreted as a treatment effect. The composition of the two groups changes over time as countries adopt fiscal councils, and adopters may differ systematically from non-adopters in economic and institutional characteristics that also affect revenue mobilization. These descriptive differences therefore reinforce the need for an empirical strategy that accounts for staggered adoption and pre-treatment differences between treated and comparison countries.

Figure 3 maps the average tax gap by country and shows that the dispersion documented above has a clear geographical dimension. The largest average tax gaps are found among the oil producers of the Gulf, such as Iraq, Oman and Qatar, where resource revenues reduce the need to rely on ordinary taxation, as well as in several countries of sub-Saharan Africa and Asia. By contrast, the smallest gaps are concentrated among advanced economies, particularly in Europe, although some emerging and developing economies also collect a large share of their estimated potential. This pattern is consistent with the view that the scope for additional revenue mobilization is greatest in developing and resource-rich economies, a point we return to in the heterogeneity analysis.

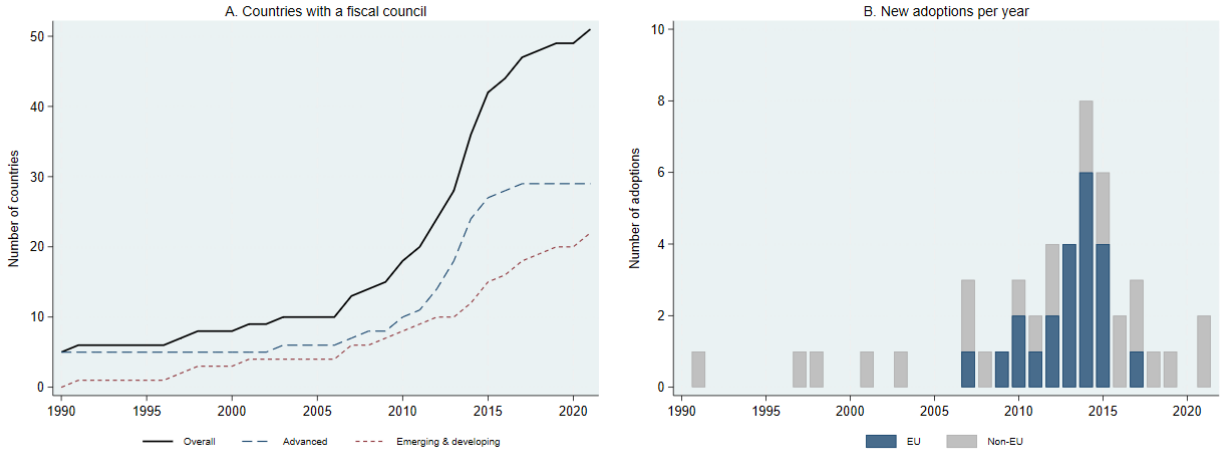


Figure 1: Diffusion and timing of fiscal council adoption, 1990–2021

*Notes:* Panel A reports the cumulative number of countries with an operational fiscal council, for the full sample and separately for advanced and emerging and developing economies. Panel B reports new fiscal council adoptions by year, distinguishing EU and non-EU countries. Countries with a fiscal council already in place at the beginning of the sample are included in the stock reported in Panel A but are not counted as new adoptions in Panel B. *Source:* Authors' calculations based on the IMF Fiscal Council Dataset.

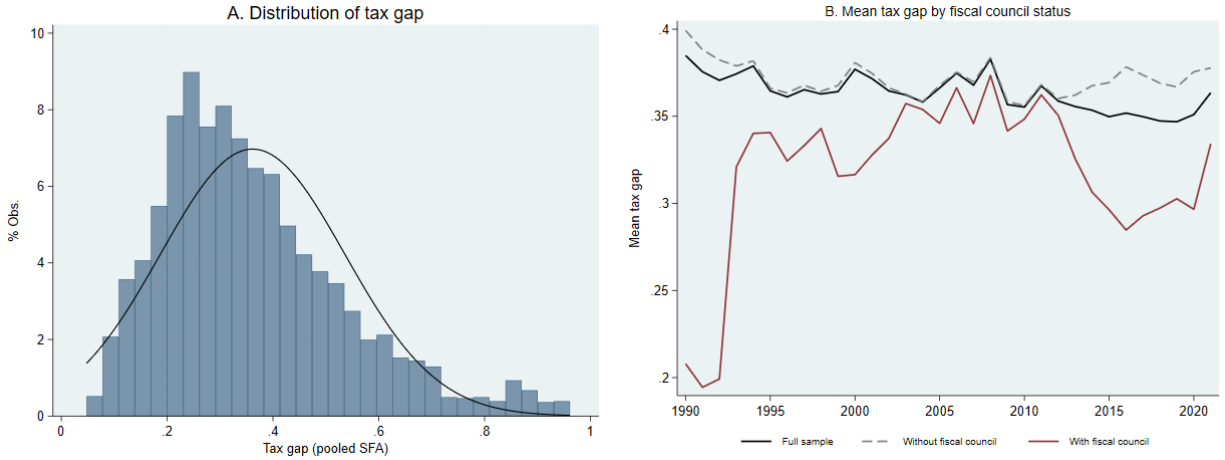


Figure 2: Distribution and evolution of the estimated tax gap, 1990–2021

*Notes:* Panel A reports the distribution of the tax gap estimated from the baseline pooled stochastic frontier. Panel B reports the annual mean tax gap for the full sample and separately for country-years with and without an operational fiscal council. Differences between the two groups are descriptive and should not be interpreted as treatment effects. *Source:* Authors' calculations.

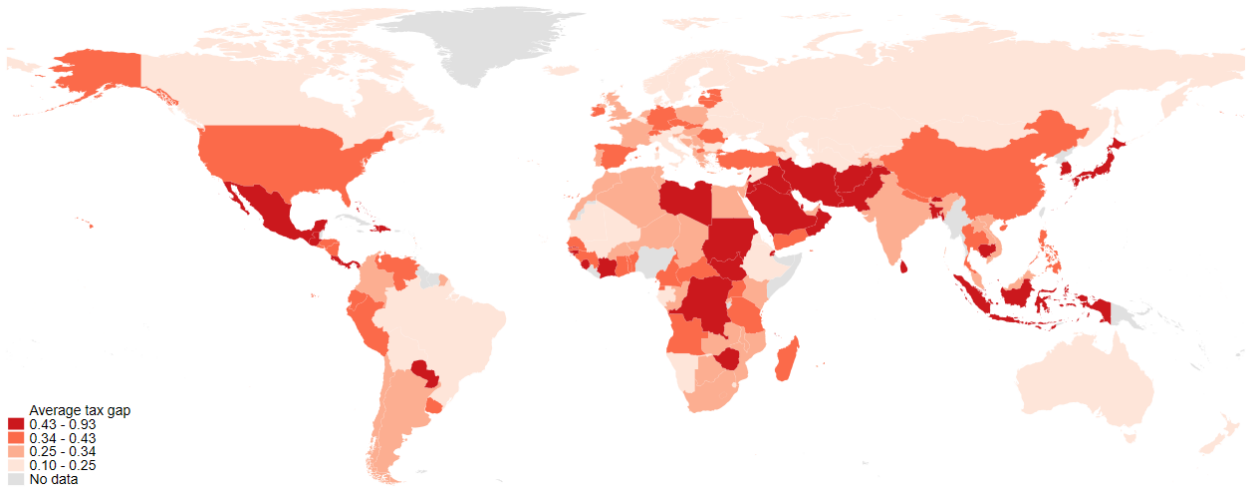


Figure 3: Average tax gap by country, 1990–2021

*Notes:* The map reports each country's average tax gap over 1990–2021, defined as the share of estimated revenue potential that remains uncollected. Countries are grouped into quartiles of the cross-country distribution, with darker shades indicating larger gaps. Grey areas indicate countries for which no tax gap estimate is available. *Source:* Authors' calculations based on the IMF World Revenue Longitudinal Data.

## 4 Empirical strategy

We exploit the temporal variation in the establishment of fiscal councils to estimate their effect on the tax gap. For each country  $i$ , let  $G_i$  denote the first year in which a fiscal council becomes operational, and define treatment status as

$$FC_{it} = \mathbf{1}(t \geq G_i). \quad (9)$$

Countries that do not establish a fiscal council during the sample period remain untreated. In our sample, once a fiscal council is established, it remains operational until the end of the observation period. The treatment is therefore irreversible or absorbing. Adoption dates, however, vary substantially across countries, placing our analysis in a staggered treatment adoption setting. Countries with a fiscal council already in place in 1990 (Austria, Belgium, Denmark, the Netherlands and the United States) do not contribute to the identification of the effect of adoption, since no pre-treatment trajectory is observed for them (Callaway & Sant’Anna, 2021).

Identifying the effect of fiscal councils raises two main challenges. The first concerns selection into adoption. Countries that establish a fiscal council tend to be more developed and to have stronger fiscal institutions. If these characteristics are also associated with improvements in revenue mobilization, the estimated effect of fiscal councils may be overstated. The second challenge stems from the strong concentration of European adoptions between 2010 and 2015 (Figure 1). This wave coincided with the sovereign debt crisis, fiscal adjustment programs supported by the IMF or the European Union, and the strengthening of European fiscal governance through the Fiscal Compact. The estimated effect could therefore partly capture a return to normal conditions following the crisis or the effects of other concurrent reforms.

Our setting is not well suited to the conventional two-way fixed-effects (TWFE) approach, because adoption occurs at different points in time across economies that are themselves highly heterogeneous, ranging from advanced to developing countries. When treatment effects are heterogeneous, TWFE estimates can be difficult to interpret and may be biased (de Chaisemartin and D’Haultfœuille (2020); Goodman-Bacon (2021); Callaway and Sant’Anna (2021); Sun and Abraham (2021); Borusyak et al. (2024)). With staggered treatment adoption, TWFE estimates a weighted average of treatment effects in which some weights may be negative, as already-treated units can serve as controls for units treated later. This complicates a causal interpretation of the resulting coefficient. We nevertheless report conventional TWFE estimates for comparison.

Several recent estimators address the difficulties created by staggered adoption and heterogeneous treatment effects (Callaway & Sant’Anna, 2021; Sun & Abraham, 2021; Borusyak et al., 2024; de Chaisemartin & D’Haultfœuille, 2026). Among them, the estimator developed by Callaway and Sant’Anna (2021) is particularly well suited to our design. Like other recent approaches, it estimates treatment effects specific to each adoption cohort and time period while using only units that are untreated at the time of comparison as counterfactuals, thereby avoiding the comparisons between early and late adopters that make conventional TWFE problematic. It is particularly attractive in our setting for three reasons.

First, it allows never-treated and not-yet-treated countries to be used jointly as comparison units, whereas Sun and Abraham (2021) rely on either never-treated units or a designated comparison cohort. This feature is particularly relevant in our sample, where countries that never establish a fiscal council differ substantially from adopters, while future adopters may provide a more institutionally comparable control group. Second, the estimator allows comparisons to be conditioned on characteristics measured prior to adoption, which is particularly useful given the strong observable selection into fiscal council adoption. Third, each adoption cohort is compared

with its relevant controls relative to the period preceding treatment, which makes the approach well suited to a setting in which countries differ substantially in their long-run trajectories over a sample spanning several decades.

Formally, let  $Y_{it}(g)$  denote the tax gap that country  $i$  would exhibit at time  $t$  if it adopted a fiscal council in year  $g$ , and let  $Y_{it}(0)$  denote its outcome in the absence of adoption. For each adoption cohort  $g$  and each period  $t \geq g$ , we estimate

$$ATT(g, t) = E[Y_{it}(g) - Y_{it}(0) | G_i = g], \quad (10)$$

by comparing the change in the tax gap of countries adopting in year  $g$  between  $g - 1$  and  $t$  with the corresponding change among never-treated and not-yet-treated countries over the same period. Identification relies on a conditional parallel trends assumption:

$$E[\Delta Y_{it}(0) | G_i = g, X_i] = E[\Delta Y_{it}(0) | G_i > t, X_i], \quad (11)$$

where  $\Delta Y_{it}(0) = Y_{it}(0) - Y_{i,t-1}(0)$  and  $X_i$  is a vector of covariates measured prior to adoption. It includes the logarithm of GDP per capita, which captures a major source of differences between adopters and non-adopters; inflation, which reflects macroeconomic stability; and the presence of a fiscal rule, which is closely related to the emergence of fiscal councils. These covariates are measured in the year preceding adoption, so that they cannot be affected by treatment. The structural determinants entering the tax frontier are not reintroduced, since they already condition the estimated revenue potential. In line with the Callaway and Sant’Anna (2021) framework, identification also requires limited anticipation and sufficient overlap between treated and comparison units.

The  $ATT(g, t)$  estimates are then aggregated to obtain an overall average treatment effect of fiscal council adoption as well as a dynamic treatment-effect profile in event time. The latter allows us to examine how the effect evolves with the duration of exposure to a fiscal council and to document the behavior of the tax gap in the years preceding adoption. Standard errors are clustered at the country level.

We complement these estimates with several robustness exercises. In particular, we isolate the effect of the European adoption wave, account for IMF-supported adjustment programs, and consider alternative estimation approaches, including the estimator of de Chaisemartin and D’Haultfœuille (2026) and the local-projections difference-in-differences approach of Dube et al. (2025).

## 5 Main results

Table 1 reports the average effect of fiscal council adoption on the tax gap. Columns (1) and (2) report conventional two-way fixed-effects (TWFE) estimates, without and with covariates. Columns (3) to (6) report estimates based on the Callaway and Sant’Anna (2021) estimator, which differ along two dimensions, namely the choice of comparison group and whether pre-treatment country characteristics are taken into account. Columns (3) and (4) include no covariates. The first compares adopters only with countries that never establish a fiscal council, while the second also includes countries that adopt later but are not yet treated at the time of comparison. Columns (5) and (6) use the same two comparison groups while conditioning the parallel trends assumption on the logarithm of GDP per capita, inflation and the presence of a fiscal rule, all measured in the year preceding adoption.

The contrast between the two estimators is striking. The conventional TWFE estimates are small, positive and statistically insignificant (0.010 and 0.009), suggesting that fiscal councils have no effect on the tax gap. Once staggered adoption and heterogeneous treatment effects are properly accounted for, however, the picture changes markedly. All four Callaway and Sant’Anna (2021) estimates are negative and statistically significant at the 1% level. Without covariates, fiscal council adoption reduces the tax gap by 3.2 percentage points when only never-treated countries are used as controls, and by 3.3 percentage points when not-yet-treated countries are also included. Conditioning on pre-adoption characteristics yields larger effects, of 5.4 percentage points with never-treated countries as the comparison group and 5.0 percentage points when all countries untreated at the time of comparison are used. This pattern is informative given the observable selection into adoption, as countries that establish a fiscal council differ substantially from non-adopters, particularly in terms of development and fiscal institutions. Once comparisons are restricted to countries that are more similar along these dimensions, the estimated effect becomes larger, suggesting that unconditional comparisons tend to understate it.

The divergence between the TWFE and Callaway and Sant’Anna (2021) estimates illustrates the problems discussed in Section 4. In a setting where adoption is staggered over three decades and concentrated in the early 2010s, the TWFE estimator relies heavily on comparisons in which countries that adopted a fiscal council earlier serve as controls for later adopters. When treatment effects build up over time, as documented below, these comparisons subtract part of the effect of early adopters from that of later ones and can mask, or even reverse, the underlying average effect (de Chaisemartin & D’Haultfœuille, 2020; Goodman-Bacon, 2021). The near-zero TWFE coefficient should therefore not be interpreted as evidence that fiscal councils are ineffective.

Our preferred specification is reported in column (6). It combines two features that are particularly appropriate in our setting. First, it conditions the comparison on important determinants of fiscal council adoption and fiscal outcomes. These are the level of development, which constitutes a major source of differences between adopters and non-adopters, inflation, which captures the macroeconomic environment, and the presence of a fiscal rule, another central component of the fiscal institutional framework that is closely related to the emergence of fiscal councils. Because these characteristics are measured before adoption, they cannot themselves be affected by treatment. Second, this specification uses as comparison units not only countries that never establish a fiscal council, but also future adopters as long as they have not yet been treated. These countries may provide more informative comparisons in a sample where never-adopters differ substantially from adopters, and their inclusion increases the number of available comparisons, especially for early adoption cohorts. In this specification, which covers 153 countries, 43 of which adopt a fiscal council within the sample window, fiscal council adoption reduces the tax gap by 5.0 percentage points, an effect statistically significant at the 1% level.

The dynamic treatment effects reported in Figure 4 complement these findings. The estimated coefficients in the periods preceding adoption are close to zero and are not statistically significant. We therefore find no significant evidence of systematic differences in tax gap trends between future adopters and comparison countries prior to fiscal council adoption, a pattern consistent with the parallel trends assumption underlying our identification strategy. Following adoption, the effect emerges relatively quickly and becomes progressively larger.

This gradual dynamic is consistent with the functioning of an institution whose influence builds over time as its assessments are produced, made public, and incorporated into fiscal decision-making and public debate.

The estimated magnitude is economically substantial. The average tax gap among adopters in the year preceding adoption is approximately 0.33, so a reduction of 5.0 percentage points

corresponds to roughly 15% of the initial distance from estimated fiscal potential. This result is consistent with the estimates reported in Section 8, which show an increase in total revenues following the establishment of a fiscal council. The central interpretation is therefore not that fiscal councils alter the structural determinants of fiscal capacity, but that they help reduce the distance between effectively mobilized revenues and estimated fiscal potential. In other words, fiscal council adoption is associated with a more complete use of existing fiscal capacity. This distinction matters from a policy perspective. Beyond long-run structural reforms aimed at expanding fiscal capacity itself, fiscal institutions may also improve the use of resources that are already potentially mobilizable, by strengthening information, scrutiny and the incentives faced by governments.

Table 1: Effect of fiscal council adoption on the tax gap

|                   | TWFE             |                  | Callaway and Sant'Anna |                      |                      |                      |
|-------------------|------------------|------------------|------------------------|----------------------|----------------------|----------------------|
|                   | (1)              | (2)              | (3)                    | (4)                  | (5)                  | (6)                  |
| Fiscal council    | 0.010<br>(0.012) | 0.009<br>(0.013) | -0.032***<br>(0.012)   | -0.033***<br>(0.012) | -0.054***<br>(0.015) | -0.050***<br>(0.015) |
| Comparison group  | –                | –                | Never                  | Not yet              | Never                | Not yet              |
| Covariates        | No               | Yes              | No                     | No                   | Yes                  | Yes                  |
| Observations      | 3863             | 3863             | 3701                   | 3701                 | 3701                 | 3701                 |
| Countries         | 159              | 159              | 153                    | 153                  | 153                  | 153                  |
| Treated countries | 44               | 44               | 43                     | 43                   | 43                   | 43                   |

*Notes:* Standard errors clustered at the country level in parentheses. Columns (1)–(2) report two-way fixed-effects estimates with country and year fixed effects. Columns (3)–(6) report Callaway & Sant'Anna (2021) estimates, with covariates measured the year before adoption. This estimator excludes the five countries that already had a fiscal council in 1990, which have no pre-treatment period, as well as adopters without an observation in the year preceding adoption. Never: never-treated controls; Not yet: never-treated and not-yet-treated controls. Treated countries: countries adopting a fiscal council within the sample window. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

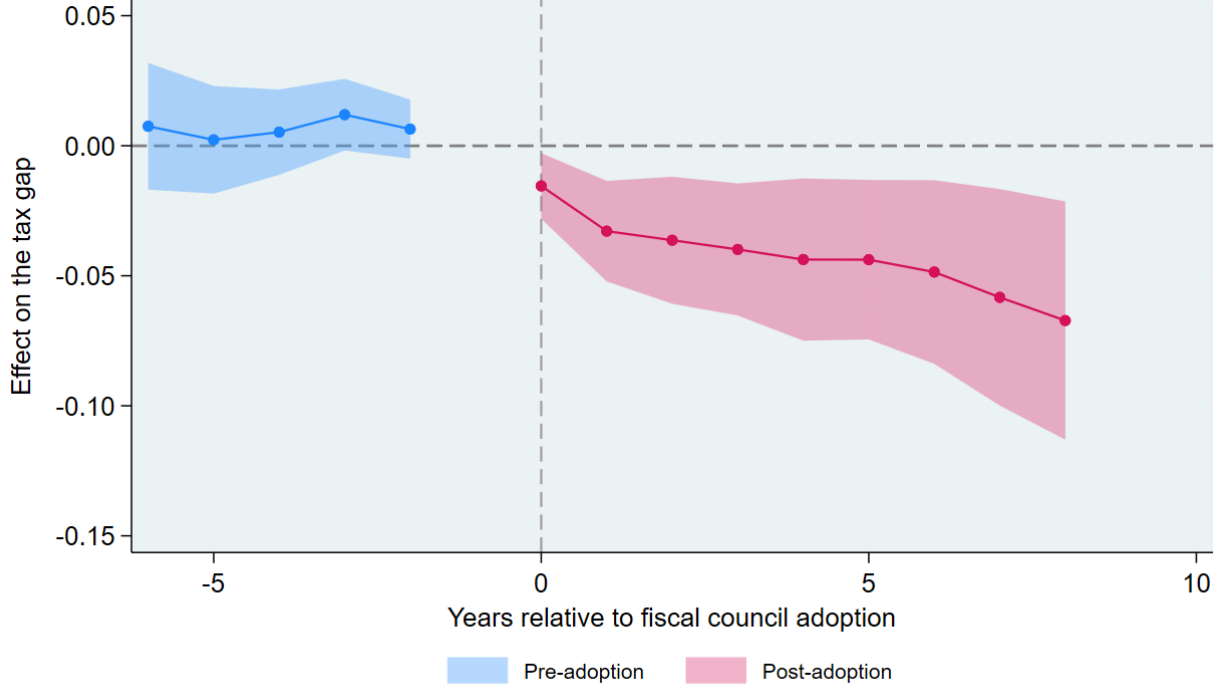


Figure 4: Dynamic effects of fiscal council adoption on the tax gap

*Notes:* Event-study estimates based on the Callaway & Sant’Anna (2021) estimator, using never-treated and not-yet-treated countries as comparison units and conditioning on log GDP per capita, inflation and the presence of a fiscal rule, measured in the year preceding adoption (preferred specification, column (6) of Table 1). Estimates are expressed relative to the year preceding adoption ( $t = -1$ ). Blue markers report pre-adoption coefficients and red markers post-adoption coefficients (95% confidence intervals based on standard errors clustered at the country level).

## 6 Robustness checks

This section examines whether the reduction in the tax gap associated with the adoption of a fiscal council is sensitive to the main choices underlying our baseline specification. We consider, in turn, additional potential confounders, the construction of the tax gap, the source and definition of revenue data, the composition of the sample and the study period, and finally the choice of estimator. All results are reported in Table 2. The estimated effect remains negative in all specifications, ranging from 2.1 to 5.3 percentage points compared with 5.0 percentage points in our preferred specification.

We begin by enriching the set of pre-treatment covariates. Fiscal council adoption may occur in a context of deteriorating public finances which, independently of the creation of the council, could also induce governments to strengthen revenue mobilization. Likewise, IMF programs may simultaneously involve reforms of fiscal institutions and measures aimed at increasing revenues, thereby creating a potential source of confounding. We therefore augment the baseline covariates with public debt, the fiscal balance and an indicator for participation in an IMF program, all

measured prior to fiscal council adoption so that they cannot themselves be affected by treatment. The estimated effect decreases in magnitude but remains negative and statistically significant at the 5% level, with fiscal council adoption associated with a reduction in the tax gap of approximately 2.5 percentage points (column 1). This attenuation suggests that part of the baseline effect is related to the initial fiscal conditions of adopting countries, although these conditions do not account for the entire effect. In the same spirit, we exclude countries that were under an IMF program prior to fiscal council adoption, to ensure that the result is not driven by IMF conditionality. The reduction in the tax gap then amounts to 5.3 percentage points (column 2), very close to the baseline estimate.

We next examine the sensitivity of the results to the construction of the tax gap. Because the tax gap is derived from an estimated fiscal frontier, its measurement necessarily depends on the assumptions used to model inefficiency and cross-country heterogeneity. We first replace the half-normal distribution of inefficiency with an exponential distribution. Under this alternative distributional assumption, fiscal council adoption reduces the tax gap by 4.1 percentage points (column 3). We then estimate the true fixed-effects model of Greene (2005), which separates permanent unobserved country heterogeneity from inefficiency itself and therefore reduces the risk that persistent structural differences across countries are interpreted as distance from the fiscal frontier. The estimated effect remains negative and statistically significant, at approximately 4.5 percentage points (column 4). Finally, we augment the frontier with the square of log GDP per capita, log population density and human capital, to account for nonlinearities in the relationship between development and revenue potential and for additional structural determinants of the tax base. The estimated effect remains negative and statistically significant at the 5% level, at 3.0 percentage points (column 5), although this specification covers a smaller sample of 125 countries because of data availability.

We also verify that the results are not driven by the source of the revenue data. We reconstruct the fiscal frontier and the tax gap using tax revenue data from the UNU-WIDER Government Revenue Dataset, which relies on harmonization and classification procedures that differ from those of the IMF database. The estimated effect remains negative and statistically significant, although smaller in magnitude, at approximately 2.8 percentage points (column 6). This estimate corroborates the baseline result and indicates that the relationship documented above is not driven by the particular source used to measure tax revenues. We also reconstruct the tax gap using non-resource tax revenues, to assess the role of resource-related taxes, which are particularly sensitive to commodity-price fluctuations. The effect remains negative, at 2.1 percentage points, but is smaller and no longer statistically significant (column 7). This suggests that part of the reduction in the tax gap operates through resource-related revenues, a finding consistent with the larger effect we document for resource-rich economies in Section 7.

We then assess the sensitivity of the estimates to the sample composition and study period. A substantial share of fiscal council adoptions occurred in European countries between 2010 and 2015, a period that coincided with the sovereign debt crisis, several fiscal adjustment programs and the strengthening of European fiscal governance. The estimated effect could therefore partly reflect this particular institutional context. We re-estimate the baseline specification after excluding European Union countries. The reduction in the tax gap remains 4.8 percentage points (column 8), indicating that the result is not driven by the European wave of fiscal council adoption. We also restrict the estimation period to 1990–2019 in order to exclude the exceptional disruptions to government revenues and expenditures associated with the Covid-19 pandemic. The estimated effect remains negative and statistically significant, at 4.8 percentage points (column 9).

Finally, we examine whether the results depend on the choice of estimator. We replicate

the analysis using two recent approaches designed for staggered treatment adoption settings with potentially heterogeneous treatment effects. These estimators rely on comparison groups and aggregation procedures that differ from those of Callaway and Sant’Anna (2021), allowing us to assess whether our main result is specific to the baseline estimator. The dynamic estimator of de Chaisemartin and D’Haultfœuille (2026) compares, at each horizon, changes in the tax gap among countries that adopt a fiscal council with those of countries whose treatment status remains unchanged. The resulting average total effect is 2.5 percentage points, significant at the 1% level (column 10). We also use the local-projections approach adapted to staggered treatment settings developed by Dube et al. (2025), which estimates treatment effects at different horizons using units that have not yet been treated at the relevant horizon as comparison groups. The pooled post-treatment effect is 3.4 percentage points, also significant at the 1% level (column 11), while the pooled pre-treatment coefficient is close to zero and statistically insignificant. Both estimators include the same covariates as our baseline specification. Their somewhat smaller magnitudes likely reflect differences in comparison groups, weighting and the way covariates enter each estimator.

Figure 5 reports the corresponding dynamic effects. For both alternative estimators, pre-adoption coefficients are close to zero and statistically insignificant, and the effect builds up gradually after adoption, closely mirroring the profile documented in Figure 4.

Overall, these exercises corroborate our baseline findings. The effect of fiscal councils on the tax gap remains negative and statistically significant when we enrich the set of pre-treatment covariates, modify the assumptions underlying the construction of the fiscal frontier, use an alternative source of revenue data, alter the sample composition and study period, or rely on alternative estimators. The magnitude of the effect ranges from 2.1 to 5.3 percentage points and is mainly attenuated when we control for fiscal conditions prior to adoption, use alternative revenue data or exclude resource-related revenues, without altering its direction.

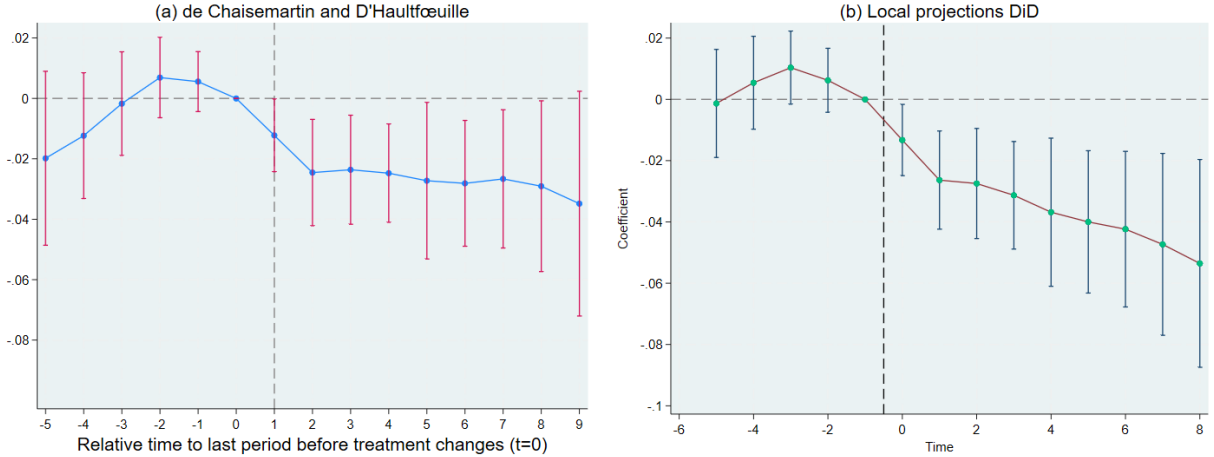


Figure 5: Dynamic effects of fiscal council adoption: alternative estimators

*Notes:* Event-study estimates of the effect of fiscal council adoption on the tax gap based on the estimators of de Chaisemartin & D’Haultfœuille (2026) and Dube et al. (2025). All estimates are expressed relative to the year preceding adoption ( $t = -1$ ), and markers are slightly shifted horizontally for readability. Vertical bars represent 95% confidence intervals based on standard errors clustered at the country level.

Table 2: Robustness checks

|                | Confounders             |                              |                                    | Tax gap measurement        |                              |                             | Sample                         |                      | Estimators           |                        |                      |
|----------------|-------------------------|------------------------------|------------------------------------|----------------------------|------------------------------|-----------------------------|--------------------------------|----------------------|----------------------|------------------------|----------------------|
|                | (1)<br>Add.<br>controls | (2)<br>Excl. IMF<br>programs | (3)<br>Exponential<br>inefficiency | (4)<br>True FE<br>frontier | (5)<br>Augmented<br>frontier | (6)<br>UNU-WIDER<br>revenue | (7)<br>Non-resource<br>revenue | (8)<br>Excl.<br>EU   | (9)<br>Pre-<br>COVID | (10)<br>dCdH<br>(2026) | (11)<br>LP-DiD       |
| Fiscal council | -0.025**<br>(0.013)     | -0.053***<br>(0.014)         | -0.041***<br>(0.014)               | -0.045**<br>(0.021)        | -0.030**<br>(0.013)          | -0.028**<br>(0.011)         | -0.021<br>(0.013)              | -0.048***<br>(0.019) | -0.048***<br>(0.014) | -0.025***<br>(0.008)   | -0.034***<br>(0.011) |
| Observations   | 3392                    | 3423                         | 3701                               | 3701                       | 3081                         | 3981                        | 3722                           | 3060                 | 3412                 | 3863                   | 1963                 |
| Countries      | 148                     | 142                          | 153                                | 153                        | 125                          | 149                         | 145                            | 130                  | 152                  | 159                    | 159                  |

*Notes:* Dependent variable: tax gap. Columns (1)–(9) report Callaway and Sant’Anna (2021) estimates with never-treated and not-yet-treated controls and covariates (log GDP per capita, inflation, fiscal rule) measured the year before adoption. Column (1) adds public debt, the fiscal balance and an IMF program indicator; column (2) excludes countries under an IMF program before adoption; columns (3)–(7) use alternative tax gap measures based on exponential inefficiency, a true fixed-effects frontier, a frontier augmented with the square of log GDP per capita, log population density and human capital, UNU-WIDER total tax revenue, and non-resource tax revenue; column (8) excludes EU member states; column (9) restricts the sample to 1990–2019. Column (10) reports the average total effect of de Chaisemartin and D’Haultfoeuille (2026), with the same covariates; column (11) reports the pooled post-treatment effect of the local-projections DiD estimator of Dube et al. (2025). Standard errors clustered at the country level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

## 7 Heterogeneity

Because fiscal councils are established in widely varying contexts, the average effect estimated so far may conceal substantial cross-country heterogeneity. To investigate these differences, we re-estimate our preferred specification across subsamples defined by several economic and institutional characteristics. The results, reported in Table 3, show that the effect is concentrated in emerging and developing economies, where the scope for improving revenue mobilization is greater, while it does not depend on pre-adoption fiscal conditions or on the measured strength of fiscal councils.

The first dimension we explore is the level of development. The effect of fiscal councils is concentrated in emerging and developing economies, where adoption reduces the tax gap by 5.6 percentage points, significant at the 1% level (column 2). By contrast, no statistically significant effect is detected in advanced economies, where the point estimate is close to zero (column 1). The estimate for advanced economies should nonetheless be interpreted with caution. Since most advanced economies in our sample eventually adopt a fiscal council, and only a handful never do, advanced adopters are compared with a small and shrinking group of advanced economies that have not yet adopted one. The pattern is nonetheless consistent with greater scope for improvement in emerging and developing economies, where available fiscal capacity is not always fully mobilized, whereas in advanced economies more developed tax administrations and already high levels of revenue mobilization may limit the marginal gains associated with a fiscal council. It is also consistent with the fiscal consolidation literature, which shows that adjustment episodes are more likely to rely on stronger revenue mobilization in emerging and developing economies because of the presence of untapped revenue sources (Adam et al., 2003; Gupta et al., 2004; Balasundharam et al., 2026).

The effect also varies with natural resource wealth. In resource-rich economies, fiscal council adoption is associated with a particularly large reduction in the tax gap, of 7.5 percentage points, significant at the 1% level (column 3), whereas the estimate for economies less dependent on natural resources is smaller and not statistically significant (column 4). Consistent with the literature on the fiscal resource curse, this pattern points to greater scope for improving revenue mobilization in economies where resource revenues have historically weakened incentives to develop ordinary tax revenues (Bornhorst et al., 2009; Crivelli & Gupta, 2014; Chachu, 2020; Masi et al., 2024). Masi et al. (2024) further show that stronger constraints on the executive can offset the adverse effect of resource rents on fiscal capacity. By strengthening fiscal scrutiny and making dependence on resource revenues more visible, a fiscal council may operate through a similar mechanism.

We next examine whether the effect depends on the pre-existing fiscal framework. The effect of fiscal councils is negative and statistically significant both in countries that already had a fiscal rule prior to adoption (4.6 percentage points, column 5) and in those that did not (5.9 percentage points, column 6). This result suggests that the two institutions perform distinct roles within the fiscal framework. While fiscal rules impose constraints on the conduct of fiscal policy, fiscal councils strengthen scrutiny, evaluation and transparency independently of the presence of a formal rule.

The effect does not depend on pre-adoption fiscal conditions either. Fiscal council adoption reduces the tax gap by 5.4 percentage points in countries where public debt was above the median before adoption and by 3.3 percentage points where it was below (columns 7 and 8). Similarly, the reduction amounts to 7.0 percentage points in countries with a high pre-adoption deficit and to 3.5 percentage points in those with a lower deficit (columns 9 and 10). All four estimates are statistically significant. Fiscal councils therefore appear to reduce the tax gap regardless of the fiscal position in which they are established, and not only in countries facing acute fiscal stress.

We also examine whether the effect depends on the design of the fiscal council. To do so, we

use the Fiscal Council Strength Index of Alonso-Albarran et al. (2025), constructed from the IMF Fiscal Council Dataset. The index aggregates three dimensions, namely the council’s tasks and instruments, which capture the breadth of its mandate, its independence and accountability, which reflect the institutional safeguards governing its operation, and its communication policy, which captures its ability to disseminate its analyses publicly. Because the index does not vary over time for a given adopter, the analysis compares councils with an index above the median with those below the median. The estimated effect is very similar in both groups, at 5.1 and 5.3 percentage points and significant at the 1% level (columns 11 and 12). This suggests that the effect is driven primarily by the existence of a fiscal council.

Finally, we examine whether the effect varies with the initial scope for improving revenue mobilization. Fiscal council adoption reduces the tax gap by 6.4 percentage points in countries where the pre-adoption gap was above the median, compared with 4.1 percentage points in countries with a lower initial gap, both estimates being significant at the 1% level (columns 13 and 14). This pattern is consistent with the idea that fiscal councils generate larger gains where untapped revenue-mobilization margins are initially greater.

Taken together, these results suggest that fiscal councils generate their largest gains where untapped revenue-mobilization margins are greatest. The effect is concentrated in emerging and developing economies, and it is also markedly larger in resource-rich countries and where the initial tax gap is high. This pattern is consistent with the idea that fiscal councils are most beneficial where available fiscal capacity remains underexploited. By contrast, the effect remains significant regardless of the prior existence of a fiscal rule, of pre-adoption levels of public debt and deficits, and of the measured institutional strength of fiscal councils.

## 8 Mechanisms

Fiscal councils do not collect taxes. According to the framework presented in Section 2, their effect on the tax gap therefore operates indirectly, through changes in governments’ fiscal behavior. By improving the information environment and strengthening fiscal scrutiny, fiscal councils make deviations between fiscal objectives and realized outcomes more visible and may thereby raise their political and reputational costs, strengthening governments’ incentives to correct their fiscal trajectories (Debrun & Kinda, 2017; Beetsma et al., 2019; Davoodi et al., 2022).

This framework yields three empirical implications. First, when expenditures are relatively rigid in the short run and untapped revenue-mobilization margins remain, the adjustment associated with the presence of a fiscal council should materialize through higher revenues rather than through expenditure cuts. Second, additional mobilization efforts should be concentrated on tax instruments offering the most accessible margins for revenue collection, particularly those relying on tax bases that are easier to observe or monitor. Third, if the disciplining effect of fiscal councils operates partly through the visibility of fiscal performance, their effect may differ when governments are more exposed to electoral scrutiny. We examine these three implications in turn.

We first examine the margin through which fiscal adjustment takes place following the establishment of a fiscal council (Table 4). If the reduction in the tax gap documented above reflects a more intensive use of existing fiscal capacity, it should be accompanied by an increase in effectively mobilized revenues. Consistent with this prediction, fiscal council adoption is followed by an improvement in the cyclically adjusted balance of 2.9 percentage points of GDP and a reduction in the fiscal deficit of 2.7 percentage points (columns 1 and 2). This adjustment occurs on the revenue side. Total revenues increase by 2.4 percentage points of GDP (column 3), whereas total

Table 3: Heterogeneity

|                | Development      |                       | Natural resources    |                   | Fiscal rule          |                      | Public debt          |                     | Fiscal deficit       |                      | Council strength     |                      | Initial tax gap      |                      |
|----------------|------------------|-----------------------|----------------------|-------------------|----------------------|----------------------|----------------------|---------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
|                | (1)              | (2)                   | (3)                  | (4)               | (5)                  | (6)                  | (7)                  | (8)                 | (9)                  | (10)                 | (11)                 | (12)                 | (13)                 | (14)                 |
|                | Advanced         | Emerging & developing | Resource-rich        | Non-resource      | Yes                  | No                   | High                 | Low                 | High                 | Low                  | Strong               | Weak                 | High                 | Low                  |
| Fiscal council | 0.011<br>(0.016) | -0.056***<br>(0.021)  | -0.075***<br>(0.028) | -0.022<br>(0.017) | -0.046***<br>(0.016) | -0.059***<br>(0.030) | -0.054***<br>(0.019) | -0.033**<br>(0.014) | -0.070***<br>(0.022) | -0.035***<br>(0.013) | -0.051***<br>(0.017) | -0.053***<br>(0.019) | -0.064***<br>(0.020) | -0.041***<br>(0.016) |
| Observations   | 844              | 2851                  | 1182                 | 2514              | 3500                 | 2674                 | 3115                 | 3086                | 3130                 | 3137                 | 3055                 | 3206                 | 3120                 | 3147                 |
| Countries      | 30               | 123                   | 50                   | 102               | 145                  | 116                  | 131                  | 129                 | 132                  | 131                  | 126                  | 135                  | 131                  | 132                  |

*Notes:* Standard errors clustered at the country level in parentheses. All columns report Callaway and Sant'Anna (2021) estimates with never-treated and not-yet-treated controls and covariates (log GDP per capita, inflation, fiscal rule) measured the year before adoption; the fiscal rule covariate is omitted in columns (5)–(6). In columns (1)–(4), both treated and comparison countries are restricted to the relevant group (development level or resource dependence). In columns (5)–(14), adopters are split according to the characteristic indicated while all never-treated countries are retained as controls. Resource-rich countries are those classified as resource-rich or fuel exporters by the IMF; they include oil and gas producers as well as exporters of minerals such as gold and diamonds. Public debt and the fiscal deficit (% of GDP; a high deficit means a larger deficit), as well as the initial tax gap, are averaged over the three years preceding adoption; council strength is measured by the Fiscal Council Strength Index. All are split at the median across adopters. p-value (equality): p-value of a test of equality between the two estimates of each pair, treating them as independent; for columns (5)–(14), which share the same control countries, the test is conservative. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

expenditure does not exhibit a statistically significant response (column 4). The improvement in the fiscal balance is therefore largely accounted for by the increase in revenues. This pattern echoes the literature on the composition of fiscal consolidations, which shows that adjustment plans often place greater emphasis on expenditure cuts, yet actual spending reductions tend to be smaller than planned, leading adjustment to rely more heavily on revenues (Abbas et al., 2012; Balasundharam et al., 2026), particularly when initial revenue mobilization is low and untapped revenue sources remain available. Fiscal councils therefore do not appear, in our sample, to be associated with expenditure-based adjustment; instead, improvements in the fiscal position coincide with stronger revenue mobilization, consistent with a more complete use of existing fiscal capacity.

We next examine where this additional revenue mobilization occurs, by decomposing tax revenues into broad categories (Table 5). The increase is concentrated in indirect taxes and revenues collected at the border. VAT revenues increase by 0.44 percentage points of GDP (significant at the 10% level), while taxes on international trade rise by 0.33 percentage points (significant at the 5% level), most of which comes from import duties (0.24 percentage points, significant at the 5% level). Property tax revenues also increase, more modestly, by 0.15 percentage points (significant at the 10% level). By contrast, we find no statistically significant effect on export duties, on income taxes as a whole, or on personal and corporate income taxes taken separately. This pattern is consistent with the second implication of the framework. In economies with more limited administrative capacity, revenue-mobilization opportunities depend in part on the observability of transactions and on the information available to the tax administration (Gordon & Li, 2009). VAT has a particular advantage in this respect, as the paper trail generated along the production chain facilitates the use of third-party information and can strengthen compliance (Pomeranz, 2015). Likewise, cross-border transactions are relatively concentrated and observable, so that the quality of customs enforcement can have important effects on evasion and effectively collected revenues (Chalendard et al., 2023). VAT and property taxes are also among the tax bases identified as offering substantial untapped revenue potential in countries with initially modest revenue-to-GDP ratios (Gupta et al., 2004; Balasundharam et al., 2026). By contrast, mobilizing direct taxes may require more detailed information on incomes, stronger administrative capacity, or more substantial changes to the tax system.

These results are consistent with the idea that adjustment occurs first along relatively accessible revenue-mobilization margins, but they do not allow us to identify precisely which policy instruments governments use. Higher VAT revenues may result from improved collection or compliance, but also from a broader tax base, fewer exemptions or changes in tax rates. Similarly, higher import-duty revenues may reflect stronger customs enforcement as well as changes in tariff rates or in the composition of trade flows. This distinction also matters politically. Explicit tax increases may entail substantial electoral costs (Arias & Stasavage, 2019), whereas broadening tax bases and reducing evasion may generate more durable revenue gains without necessarily taking the form of a general increase in tax rates (Cournède, 2013). Our data identify the tax components through which additional revenue mobilization materializes, but not the administrative, regulatory or legislative changes underlying these responses.

Finally, we examine whether the effect of fiscal councils varies with political incentives. The literature on political budget cycles shows that incentives to relax fiscal policy may intensify around elections (Alesina & Tabellini, 1990; Brender & Drazen, 2005), while fiscal adjustments are generally more difficult to implement when an election is imminent (Gupta & Mulas-Granados, 2004; Clements et al., 2025). If fiscal councils operate by making fiscal performance more visible, their presence may help constrain such incentives precisely when government actions are subject to greater scrutiny; as discussed in Section 2, however, the proximity of an election may also raise

the political cost of adjustment, so that the sign of this interaction is not determined a priori. Our estimates suggest that the reduction in the tax gap is larger in presidential and legislative election years, at 9.3 percentage points, than in non-election years, at 4.5 percentage points (Table 4, columns 5 and 6). This result should be interpreted as suggestive, since election years represent a relatively small subsample and we do not formally test the difference between the two estimates. It is nonetheless consistent with the view that independent fiscal scrutiny can alter government behavior by increasing the political cost of poor fiscal performance precisely when that cost is most salient.

Taken together, these results shed light on the mechanisms through which fiscal council adoption may translate into a reduction in the tax gap. They provide suggestive evidence that fiscal councils primarily affect the intensity with which existing fiscal capacity is mobilized, by strengthening fiscal scrutiny over government behavior, and that the resulting adjustment operates through relatively observable revenue bases rather than through expenditure cuts.

Table 4: Fiscal adjustment and electoral incentives

|                | Fiscal aggregates (% of GDP)      |                          |                         |                             | Tax gap                  |                              |
|----------------|-----------------------------------|--------------------------|-------------------------|-----------------------------|--------------------------|------------------------------|
|                | (1)<br>Cyclically adj.<br>balance | (2)<br>Fiscal<br>deficit | (3)<br>Total<br>revenue | (4)<br>Total<br>expenditure | (5)<br>Election<br>years | (6)<br>Non-election<br>years |
| Fiscal council | 2.874***<br>(1.083)               | -2.733***<br>(1.060)     | 2.359***<br>(0.820)     | -0.357<br>(1.039)           | -0.093***<br>(0.024)     | -0.045**<br>(0.022)          |
| Observations   | 4275                              | 4275                     | 4325                    | 4286                        | 742                      | 2457                         |
| Countries      | 154                               | 154                      | 154                     | 154                         | 121                      | 145                          |

*Notes:* All columns report Callaway & Sant’Anna (2021) estimates with never-treated and not-yet-treated controls and covariates (log GDP per capita, inflation, fiscal rule) measured the year before adoption. In columns (1)–(4), the dependent variable is the fiscal aggregate indicated, in percent of GDP; the fiscal deficit is expressed as a positive number, so that a negative coefficient indicates a lower deficit. In columns (5)–(6), the dependent variable is the tax gap, estimated on presidential or legislative election years and on non-election years, respectively. Standard errors clustered at the country level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

Table 5: Composition of tax revenues

|                | Indirect and trade taxes |                       |                         |                         | Property                 | Direct taxes           |                           |                            |
|----------------|--------------------------|-----------------------|-------------------------|-------------------------|--------------------------|------------------------|---------------------------|----------------------------|
|                | (1)<br>VAT               | (2)<br>Trade<br>taxes | (3)<br>Import<br>duties | (4)<br>Export<br>duties | (5)<br>Property<br>taxes | (6)<br>Income<br>taxes | (7)<br>Personal<br>income | (8)<br>Corporate<br>income |
| Fiscal council | 0.435*<br>(0.244)        | 0.330**<br>(0.136)    | 0.243**<br>(0.123)      | 0.153<br>(0.145)        | 0.145*<br>(0.083)        | 0.403<br>(0.303)       | 0.047<br>(0.259)          | 0.253<br>(0.174)           |
| Observations   | 2839                     | 4109                  | 3247                    | 768                     | 2823                     | 4000                   | 3300                      | 3460                       |
| Countries      | 124                      | 151                   | 131                     | 40                      | 123                      | 148                    | 136                       | 138                        |

*Notes:* Dependent variables are tax revenues by category, in percent of GDP, from the UNU-WIDER Government Revenue Dataset. All columns report Callaway & Sant’Anna (2021) estimates with never-treated and not-yet-treated controls and covariates (log GDP per capita, inflation, fiscal rule) measured the year before adoption. Standard errors clustered at the country level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

## 9 Conclusion

In a context of high public debt, rising interest burdens and growing financing needs, domestic revenue mobilization has become central to the sustainability of public finances. This paper examines whether fiscal councils, despite having no taxing authority, help governments make better use of the fiscal potential. We construct a tax gap from a stochastic revenue frontier and exploit the staggered adoption of fiscal councils across 159 countries over 1990–2021.

Our results show that fiscal council adoption reduces the tax gap by 5.0 percentage points, or about 15% of its pre-adoption level. The effect builds up gradually after adoption and remains robust across additional confounders, alternative frontier specifications, alternative revenue data, sample restrictions and alternative estimators. The effect is larger in emerging and developing economies, in resource-rich countries and where the initial tax gap is high, that is, where untapped revenue-mobilization margins are greatest.

The mechanism analysis shows that the adjustment operates through the revenue side. Total revenues rise by 2.4 percentage points of GDP, accounting for most of the 2.9-point improvement in the cyclically adjusted balance, while public expenditure shows no significant response. Additional revenues come mainly from VAT, taxes on international trade and property taxes, all relatively observable bases, while income taxes remain unchanged. The effect also tends to be larger in election years. These patterns are consistent with our conceptual framework, in which fiscal councils, by increasing the visibility of fiscal slippages, raise their political cost and encourage a fuller mobilization of existing fiscal capacity.

These findings contribute to two strands of literature. They extend the literature on fiscal councils, so far focused on forecasts, rule compliance and fiscal balances, to the revenue side, and they contribute to the literature on fiscal capacity and tax effort by identifying an oversight institution with no collection authority as a driver of revenue mobilization, complementing recent work on fiscal rules.

Several policy implications follow. First, fiscal councils appear to provide a relatively low-cost institutional lever for domestic revenue mobilization. Countries that have not yet established such institutions, particularly emerging and developing economies facing declining external assistance and substantial untapped fiscal potential, may therefore benefit from considering their introduction. Second, because their influence operates not through a binding instrument but through information, scrutiny and transparency, their effectiveness requires that their assessments be made public and subject to debate. Governments and parliaments should therefore ensure that fiscal councils have adequate access to fiscal and budgetary data and that their assessments are widely disseminated. Third, since the effect does not depend on the prior existence of a fiscal rule or on a particular institutional design, countries need not wait for a complete fiscal framework to benefit from establishing a credible and independent council. Fourth, fiscal councils are not substitutes for reforms of tax policy and tax administration but complements to them, since the gains we document are concentrated on relatively observable tax bases, while direct taxes do not respond. More broadly, our findings suggest that fiscal capacity depends not only on the resources that an economy is able to tax, but also on the institutions that shape governments' incentives to mobilize them.

# A Appendix

## A.1 List of countries

Table 6: Countries in the estimation sample

| Country                  | FC   | AE | RR | VAT | Country              | FC   | AE | RR | VAT |
|--------------------------|------|----|----|-----|----------------------|------|----|----|-----|
| Afghanistan              | —    |    |    |     | Kosovo               | —    |    |    | ✓   |
| Albania                  | —    |    |    | ✓   | Kuwait               | —    |    | ✓  |     |
| Algeria                  | —    |    | ✓  |     | Kyrgyz Republic      | —    |    |    | ✓   |
| Angola                   | —    |    | ✓  |     | Lao PDR              | —    |    | ✓  | ✓   |
| Argentina                | —    |    |    | ✓   | Latvia               | 2014 | ✓  |    | ✓   |
| Armenia                  | —    |    |    | ✓   | Lebanon              | —    |    |    | ✓   |
| Aruba                    | 2015 |    |    | ✓   | Lesotho              | —    |    |    | ✓   |
| Australia                | 2012 | ✓  | ✓  | ✓   | Libya                | —    |    | ✓  |     |
| Austria                  | 1990 | ✓  |    | ✓   | Lithuania            | 2015 | ✓  |    | ✓   |
| Azerbaijan               | —    |    | ✓  | ✓   | Luxembourg           | 2014 | ✓  |    | ✓   |
| Bahamas, The             | 2019 |    |    | ✓   | Madagascar           | —    |    |    | ✓   |
| Bahrain                  | —    |    | ✓  | ✓   | Malawi               | —    |    | ✓  | ✓   |
| Bangladesh               | —    |    |    | ✓   | Malaysia             | —    |    | ✓  | ✓   |
| Belarus                  | —    |    |    | ✓   | Mali                 | —    |    |    | ✓   |
| Belgium                  | 1990 | ✓  |    | ✓   | Malta                | 2015 | ✓  |    | ✓   |
| Benin                    | —    |    |    | ✓   | Mauritania           | —    |    | ✓  | ✓   |
| Bhutan                   | —    |    |    |     | Mauritius            | —    |    |    | ✓   |
| Bolivia                  | —    |    |    | ✓   | Mexico               | 1998 |    |    | ✓   |
| Bosnia and Herzegovina   | —    |    |    | ✓   | Moldova              | —    |    |    | ✓   |
| Botswana                 | —    |    |    |     | Mongolia             | 2017 |    | ✓  | ✓   |
| Brazil                   | 2016 |    |    | ✓   | Montenegro           | —    |    |    | ✓   |
| Brunei Darussalam        | —    |    | ✓  |     | Morocco              | —    |    |    | ✓   |
| Bulgaria                 | 2015 |    |    | ✓   | Mozambique           | —    |    | ✓  | ✓   |
| Burkina Faso             | —    |    | ✓  | ✓   | Namibia              | —    |    |    | ✓   |
| Cabo Verde               | —    |    | ✓  | ✓   | Nepal                | —    |    |    | ✓   |
| Cambodia                 | —    |    |    | ✓   | Netherlands          | 1990 | ✓  | ✓  | ✓   |
| Cameroon                 | —    |    | ✓  | ✓   | New Zealand          | —    | ✓  |    | ✓   |
| Canada                   | 2008 | ✓  | ✓  | ✓   | Nicaragua            | —    |    |    | ✓   |
| Central African Republic | —    |    | ✓  | ✓   | Niger                | —    |    |    | ✓   |
| Chad                     | —    |    | ✓  | ✓   | North Macedonia      | —    |    |    | ✓   |
| Chile                    | 2014 |    | ✓  | ✓   | Norway               | —    | ✓  | ✓  | ✓   |
| China                    | —    |    |    | ✓   | Oman                 | —    |    | ✓  |     |
| Colombia                 | 2012 |    |    |     | Pakistan             | —    |    |    | ✓   |
| Comoros                  | —    |    |    |     | Panama               | 2018 |    |    | ✓   |
| Congo, Dem. Rep.         | —    |    | ✓  | ✓   | Paraguay             | —    |    |    | ✓   |
| Congo, Rep.              | —    |    | ✓  |     | Peru                 | 2015 |    |    | ✓   |
| Costa Rica               | 2021 |    |    | ✓   | Philippines          | —    |    |    | ✓   |
| Cote d'Ivoire            | —    |    |    | ✓   | Poland               | —    |    |    | ✓   |
| Croatia                  | 2013 | ✓  |    | ✓   | Portugal             | 2012 | ✓  |    | ✓   |
| Cyprus                   | 2014 | ✓  |    | ✓   | Qatar                | —    |    | ✓  |     |
| Czechia                  | 2017 | ✓  |    | ✓   | Romania              | 2010 |    |    | ✓   |
| Denmark                  | 1990 | ✓  |    | ✓   | Russian Federation   | —    |    | ✓  | ✓   |
| Djibouti                 | —    |    |    |     | Rwanda               | —    |    |    | ✓   |
| Dominican Republic       | —    |    |    | ✓   | Saudi Arabia         | —    |    | ✓  |     |
| Ecuador                  | —    |    | ✓  | ✓   | Senegal              | —    |    |    | ✓   |
| Egypt, Arab Rep.         | —    |    | ✓  | ✓   | Serbia               | 2011 |    |    | ✓   |
| El Salvador              | —    |    |    | ✓   | Seychelles           | —    |    |    | ✓   |
| Equatorial Guinea        | —    |    | ✓  |     | Sierra Leone         | —    |    | ✓  |     |
| Eritrea                  | —    |    |    |     | Singapore            | —    | ✓  |    |     |
| Estonia                  | 2014 | ✓  |    | ✓   | Slovak Republic      | 2012 | ✓  |    | ✓   |
| Eswatini                 | —    |    |    | ✓   | Slovenia             | 2015 | ✓  |    | ✓   |
| Ethiopia                 | —    |    | ✓  |     | South Africa         | 2014 |    |    | ✓   |
| Finland                  | 2013 | ✓  |    | ✓   | South Sudan          | —    |    | ✓  |     |
| France                   | 2013 | ✓  |    | ✓   | Spain                | 2014 | ✓  |    | ✓   |
| Gabon                    | —    |    | ✓  | ✓   | Sri Lanka            | —    |    |    | ✓   |
| Gambia, The              | —    |    |    |     | Sudan                | —    |    |    | ✓   |
| Georgia                  | 1997 |    |    | ✓   | Sweden               | 2007 | ✓  |    | ✓   |
| Germany                  | 2013 | ✓  |    | ✓   | Switzerland          | —    | ✓  |    | ✓   |
| Ghana                    | —    |    | ✓  | ✓   | Syrian Arab Republic | —    |    |    |     |
| Greece                   | 2010 | ✓  |    | ✓   | Tajikistan           | —    |    |    | ✓   |
| Guatemala                | —    |    |    | ✓   | Tanzania             | —    |    |    | ✓   |

| Country            | FC   | AE | RR | VAT | Country              | FC   | AE | RR | VAT |
|--------------------|------|----|----|-----|----------------------|------|----|----|-----|
| Guinea             | –    |    | ✓  | ✓   | Thailand             | –    |    |    | ✓   |
| Guinea-Bissau      | –    |    | ✓  | ✓   | Togo                 | –    |    | ✓  | ✓   |
| Haiti              | –    |    |    |     | Tunisia              | –    |    |    | ✓   |
| Honduras           | –    |    |    | ✓   | Türkiye              | –    |    |    | ✓   |
| Hungary            | 2009 |    |    | ✓   | Turkmenistan         | –    |    | ✓  | ✓   |
| Iceland            | 2016 | ✓  |    | ✓   | Uganda               | 2001 |    | ✓  | ✓   |
| India              | –    |    |    |     | Ukraine              | –    |    |    | ✓   |
| Indonesia          | –    |    | ✓  | ✓   | United Arab Emirates | –    |    | ✓  |     |
| Iran, Islamic Rep. | 1991 |    | ✓  | ✓   | United Kingdom       | 2010 | ✓  | ✓  | ✓   |
| Iraq               | –    |    | ✓  |     | United States        | 1990 | ✓  | ✓  |     |
| Ireland            | 2011 | ✓  |    | ✓   | Uruguay              | 2021 |    |    | ✓   |
| Israel             | –    | ✓  |    | ✓   | Uzbekistan           | –    |    | ✓  | ✓   |
| Italy              | 2014 | ✓  |    | ✓   | Venezuela, RB        | –    |    | ✓  | ✓   |
| Japan              | –    | ✓  |    | ✓   | Viet Nam             | –    |    |    | ✓   |
| Jordan             | –    |    |    |     | West Bank and Gaza   | –    |    |    | ✓   |
| Kazakhstan         | –    |    | ✓  | ✓   | Yemen, Rep.          | –    |    | ✓  |     |
| Kenya              | 2007 |    |    | ✓   | Zambia               | –    |    | ✓  | ✓   |
| Kiribati           | –    |    |    | ✓   | Zimbabwe             | –    |    |    | ✓   |
| Korea, Rep.        | 2003 | ✓  |    | ✓   |                      |      |    |    |     |

*Notes:* The table lists the 159 countries included in the analysis. FC: year in which a fiscal council became operational (49 countries; – if none over 1990–2021); AE: advanced economy according to the IMF classification (35 countries); RR: resource-rich country or fuel exporter according to the IMF (53 countries); VAT: VAT revenue recorded in the UNU-WIDER Government Revenue Dataset (130 countries).

## A.2 Descriptives statistics

Table 7: Descriptive statistics

|                                | Obs. | Mean   | Std. dev. | Min     | Max     |
|--------------------------------|------|--------|-----------|---------|---------|
| Tax revenue (log, % GDP, IMF)  | 4280 | 2.611  | 0.708     | -1.918  | 3.907   |
| GDP per capita (log)           | 5046 | 8.428  | 1.468     | 5.233   | 11.630  |
| GDP growth                     | 5021 | 3.457  | 6.751     | -64.047 | 149.973 |
| Trade (% GDP)                  | 4746 | 79.825 | 47.859    | 0.000   | 437.327 |
| Agriculture (% GDP)            | 4770 | 12.342 | 11.330    | 0.000   | 62.277  |
| Natural resource rents (% GDP) | 4999 | 7.420  | 11.300    | 0.000   | 88.592  |
| Urban population (%)           | 5152 | 56.608 | 22.403    | 5.275   | 100.000 |
| Tax gap                        | 3863 | 0.362  | 0.174     | 0.048   | 0.961   |
| Fiscal council                 | 5152 | 0.122  | 0.327     | 0.000   | 1.000   |

*Notes:* Statistics are computed over country-year observations of the estimation sample (1990–2021). Tax revenue excludes social contributions (IMF). The tax gap is the inefficiency score from the baseline stochastic frontier. Fiscal council is a dummy equal to one from the year a fiscal council becomes operational.

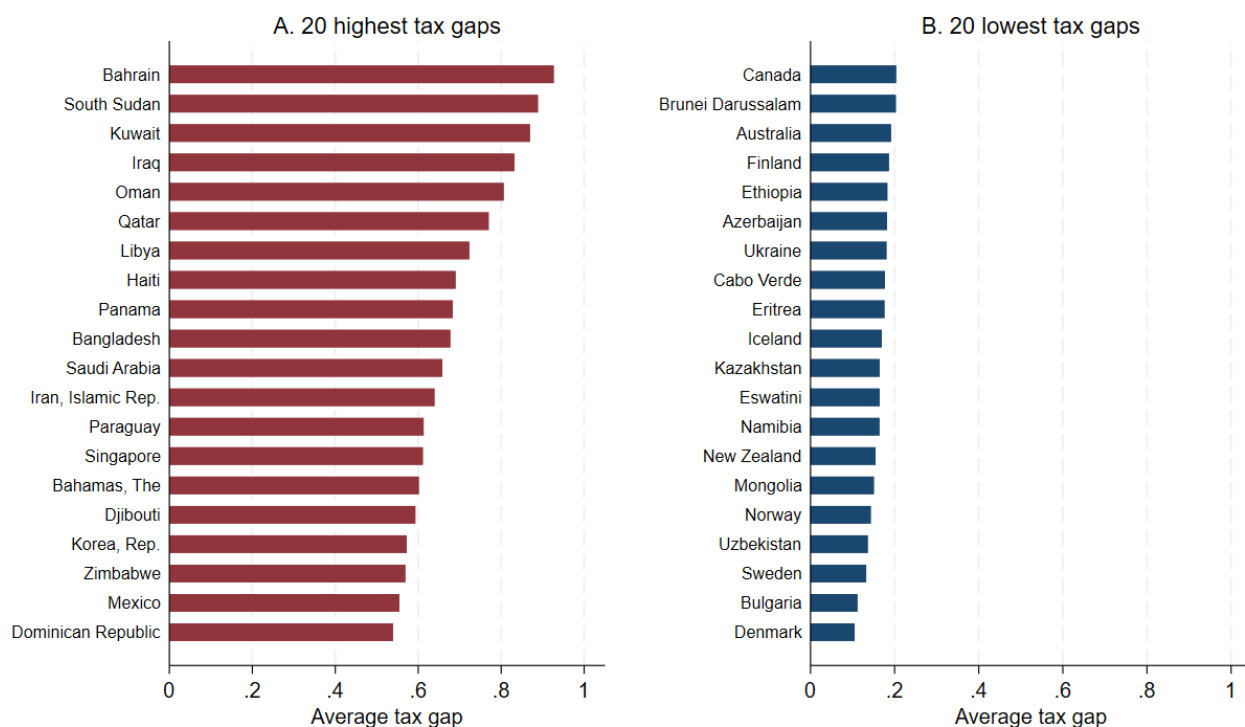


Figure 6: Countries with the highest and lowest average tax gaps, 1990–2021

*Notes:* The figure reports the 20 countries with the highest average tax gap (left) and the 20 countries with the lowest average tax gap (right) over 1990–2021. The tax gap is the share of estimated revenue potential that remains uncollected, obtained from the baseline pooled stochastic frontier, and is averaged over all available years for each country. A tax gap of 0.8, for instance, means that a country collects only 20% of its estimated revenue potential. *Source:* Authors' calculations based on the IMF World Revenue Longitudinal Data.

Table 8: Average tax gap by country group

|                                   | Mean tax gap | Countries |
|-----------------------------------|--------------|-----------|
| Full sample                       | 0.364        | 159       |
| <i>Development level</i>          |              |           |
| Advanced economies                | 0.303        | 35        |
| Emerging and developing economies | 0.382        | 124       |
| <i>Membership</i>                 |              |           |
| OECD                              | 0.310        | 38        |
| Non-OECD                          | 0.381        | 121       |
| European Union (EU)               | 0.288        | 27        |
| Non-EU                            | 0.380        | 132       |
| <i>Region (World Bank)</i>        |              |           |
| North America                     | 0.301        | 2         |
| Latin America and the Caribbean   | 0.444        | 20        |
| Europe and Central Asia           | 0.263        | 48        |
| Middle East and North Africa      | 0.514        | 22        |
| Sub-Saharan Africa                | 0.350        | 43        |
| South Asia                        | 0.471        | 7         |
| East Asia and the Pacific         | 0.355        | 16        |
| <i>Natural resources</i>          |              |           |
| Resource-rich                     | 0.387        | 53        |
| Non-resource-rich                 | 0.353        | 106       |

*Notes:* The tax gap is the inefficiency score from the pooled half-normal stochastic tax frontier, bounded between 0 (no gap) and 1. It measures the share of estimated revenue potential that remains uncollected given the country's economic structure. Each country's tax gap is first averaged over 1990–2021; the table reports the simple average of these country means within each group, which therefore differs slightly from the country-year mean reported in the summary statistics. Development level follows the IMF classification; resource-rich countries are those classified as resource-rich or fuel exporters by the IMF (oil, gas and minerals, including gold and diamonds).

### A.3 Additional checks

Table 9: Fiscal councils and the electoral cycle

|                | Year before election ( $t - 1$ ) |                       | Year after election ( $t + 1$ ) |                       |
|----------------|----------------------------------|-----------------------|---------------------------------|-----------------------|
|                | (1)<br>Pre-election<br>years     | (2)<br>Other<br>years | (3)<br>Post-election<br>years   | (4)<br>Other<br>years |
| Fiscal council | 0.157<br>(0.225)                 | -0.064***<br>(0.018)  | -0.097<br>(0.062)               | -0.047***<br>(0.017)  |
| Observations   | 635                              | 2349                  | 597                             | 2443                  |
| Countries      | 122                              | 144                   | 113                             | 143                   |

*Notes:* Dependent variable: tax gap. Callaway & Sant’Anna (2021) estimates with never-treated and not-yet-treated controls and covariates (log GDP per capita, inflation, fiscal rule) measured the year before adoption, estimated on the years indicated: years preceding a presidential or legislative election (columns 1–2), and years following an election (columns 3–4), each compared with all other years. Standard errors clustered at the country level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

Table 10: Fiscal adjustment and electoral incentives, by development level

|                                                   | Fiscal aggregates (% of GDP)      |                          |                         |                             | Tax gap                  |                              |
|---------------------------------------------------|-----------------------------------|--------------------------|-------------------------|-----------------------------|--------------------------|------------------------------|
|                                                   | (1)<br>Cyclically adj.<br>balance | (2)<br>Fiscal<br>deficit | (3)<br>Total<br>revenue | (4)<br>Total<br>expenditure | (5)<br>Election<br>years | (6)<br>Non-election<br>years |
| <i>Panel A. Advanced economies</i>                |                                   |                          |                         |                             |                          |                              |
| Fiscal council                                    | 1.185<br>(1.963)                  | -1.103<br>(1.932)        | 0.185<br>(1.200)        | -0.918<br>(1.902)           | -0.019<br>(0.040)        | 0.019<br>(0.028)             |
| Observations                                      | 884                               | 884                      | 884                     | 884                         | 150                      | 470                          |
| Countries                                         | 30                                | 30                       | 30                      | 30                          | 26                       | 27                           |
| <i>Panel B. Emerging and developing economies</i> |                                   |                          |                         |                             |                          |                              |
| Fiscal council                                    | 0.987<br>(1.193)                  | -0.739<br>(1.093)        | 1.781<br>(1.202)        | 1.112<br>(0.839)            | -0.285*<br>(0.151)       | -0.051<br>(0.033)            |
| Observations                                      | 3391                              | 3391                     | 3441                    | 3402                        | 249                      | 1937                         |
| Countries                                         | 124                               | 124                      | 124                     | 124                         | 69                       | 116                          |

*Notes:* Callaway & Sant’Anna (2021) estimates with never-treated and not-yet-treated controls and covariates (log GDP per capita, inflation, fiscal rule) measured the year before adoption, estimated separately for advanced economies (Panel A) and emerging and developing economies (Panel B); in each panel, both treated and comparison countries belong to the same group. In columns (1)–(4), the dependent variable is the fiscal aggregate indicated, in percent of GDP; the fiscal deficit is expressed as a positive number, so that a negative coefficient indicates a lower deficit. In columns (5)–(6), the dependent variable is the tax gap, estimated on presidential or legislative election years and on non-election years, respectively. Standard errors clustered at the country level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

Table 11: Composition of tax revenues, by development level

|                                                   | Indirect and trade taxes |                     |                   |                  | Property          | Direct taxes      |                   |                   |
|---------------------------------------------------|--------------------------|---------------------|-------------------|------------------|-------------------|-------------------|-------------------|-------------------|
|                                                   | (1)                      | (2)                 | (3)               | (4)              | (5)               | (6)               | (7)               | (8)               |
|                                                   | VAT                      | Trade taxes         | Import duties     | Export duties    | Property taxes    | Income taxes      | Personal income   | Corporate income  |
| <i>Panel A. Advanced economies</i>                |                          |                     |                   |                  |                   |                   |                   |                   |
| Fiscal council                                    | -0.113<br>(0.197)        | 0.001<br>(0.024)    | -0.001<br>(0.024) | —<br>—           | 0.226*<br>(0.125) | -0.669<br>(0.634) | -0.203<br>(0.319) | -0.484<br>(0.650) |
| Observations                                      | 863                      | 894                 | 889               | 36               | 923               | 927               | 904               | 908               |
| Countries                                         | 29                       | 30                  | 30                | 2                | 30                | 30                | 30                | 30                |
| <i>Panel B. Emerging and developing economies</i> |                          |                     |                   |                  |                   |                   |                   |                   |
| Fiscal council                                    | 0.759*<br>(0.446)        | 0.440***<br>(0.156) | 0.293<br>(0.247)  | 0.076<br>(0.108) | 0.099<br>(0.088)  | 0.755<br>(0.541)  | 0.224<br>(0.539)  | 0.342<br>(0.286)  |
| Observations                                      | 1973                     | 3215                | 2358              | 610              | 1820              | 3073              | 2395              | 2552              |
| Countries                                         | 95                       | 121                 | 101               | 35               | 88                | 118               | 106               | 108               |

*Notes:* Dependent variables are tax revenues by category, in percent of GDP, from the UNU-WIDER Government Revenue Dataset; VAT revenue is recorded only for countries that levy a value-added tax. Callaway & Sant’Anna (2021) estimates with never-treated and not-yet-treated controls and covariates (log GDP per capita, inflation, fiscal rule) measured the year before adoption, estimated separately for advanced economies (Panel A) and emerging and developing economies (Panel B); in each panel, both treated and comparison countries belong to the same group. Column 4 = not reported for advanced economies (two countries only). Standard errors clustered at the country level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

## A.4 Derivation of the conceptual framework

Substituting the first-order conditions (5) into the fiscal balance (2) gives

$$\bar{B} - B = \bar{B} + \bar{G} - mA, \quad A \equiv \sum_j \frac{\bar{T}_j^2}{\gamma_j} + \frac{1}{\kappa} > 0. \quad (12)$$

The marginal political cost  $m = \lambda \Phi'(\bar{B} - B)$  is therefore implicitly defined by  $m = \lambda \Phi'(\bar{B} + \bar{G} - mA)$ . Differentiating with respect to  $\lambda$ ,

$$\frac{dm}{d\lambda} = \frac{\Phi'}{1 + \lambda \Phi'' A} > 0, \quad (13)$$

since  $\Phi' > 0$  and  $\Phi'' \geq 0$ . Combined with (4), a fiscal council raises  $m$ .

**Prediction 1.** Total revenue is  $\sum_j e_j^* \bar{T}_j = A_T m$  with  $A_T \equiv \sum_j \bar{T}_j^2 / \gamma_j$ , while expenditure falls by  $m/\kappa$ . The share of the fiscal adjustment achieved through revenues is therefore

$$s_T = \frac{A_T}{A_T + 1/\kappa} = \frac{\kappa A_T}{1 + \kappa A_T}, \quad (14)$$

which tends to one as expenditure becomes rigid ( $\kappa \rightarrow \infty$ ). Higher effort  $e_j^*$  raises aggregate tax effort  $TE$  and reduces the tax gap  $1 - TE$ .

**Prediction 2.** From (5),  $\partial e_j^*/\partial m = \bar{T}_j/\gamma_j$ : for a given increase in  $m$ , effort rises more on instruments with low mobilization costs  $\gamma_j$ .

**Prediction 3.** The effect of a fiscal council on effort is

$$\frac{\partial e_j^*}{\partial FC} = \frac{\bar{T}_j}{\gamma_j} \frac{dm}{d\lambda} \frac{\partial \lambda}{\partial FC}, \quad (15)$$

which is larger in election periods if  $\partial^2 \lambda / \partial FC \partial E > 0$ , that is, if fiscal scrutiny is more consequential when fiscal performance is salient. If elections also raise the political cost of adjustment (higher  $\gamma_j$ ), the two forces work in opposite directions, and the sign of the interaction becomes an empirical question.

## A.5 Data sources

Table 12: Main variables, definitions and data sources

| Category              | Variable                                                                      | Definition                                                                                                                                                       | Source                                |
|-----------------------|-------------------------------------------------------------------------------|------------------------------------------------------------------------------------------------------------------------------------------------------------------|---------------------------------------|
| Outcome               | Tax gap                                                                       | Share of estimated tax potential not collected (1– tax effort), from a stochastic tax frontier                                                                   | Authors’ estimation based on IMF data |
|                       | Tax revenue                                                                   | Tax revenue excluding social contributions (% of GDP)                                                                                                            | IMF                                   |
| Treatment             | Fiscal council                                                                | Dummy equal to one from the year a fiscal council becomes operational                                                                                            | IMF Fiscal Council Dataset            |
|                       | Fiscal Council Strength Index                                                 | Index (0–3) of tasks and instruments, independence and accountability, and communication                                                                         | IMF Fiscal Council Dataset            |
| Frontier determinants | GDP growth, trade openness, agriculture, natural resource rents, urbanization | Real GDP growth (%); exports plus imports (% of GDP); agriculture value added (% of GDP); total natural resource rents (% of GDP); urban population (% of total) | World Development Indicators          |
| Covariates            | GDP per capita, inflation                                                     | Log of real GDP per capita; consumer price inflation (%)                                                                                                         | World Development Indicators          |
|                       | Fiscal rule                                                                   | Dummy equal to one if at least one national or supranational fiscal rule is in force                                                                             | IMF Fiscal Rules Dataset              |
|                       | Public debt, fiscal balance                                                   | General government gross debt and overall fiscal balance (% of GDP)                                                                                              | IMF Fiscal Monitor                    |
|                       | IMF program                                                                   | Dummy equal to one in years under an IMF lending arrangement                                                                                                     | Vreeland (2007); IMF MONA             |
| Mechanisms            | Fiscal aggregates                                                             | Cyclically adjusted balance, fiscal deficit, total revenue and total expenditure (% of GDP)                                                                      | IMF Fiscal Monitor                    |
|                       | Tax components                                                                | VAT, trade taxes (import and export duties), property taxes, personal and corporate income taxes (% of GDP)                                                      | UNU-WIDER GRD                         |
|                       | Election years                                                                | Dummy equal to one in years with a presidential or legislative election                                                                                          | Database of Political Institutions    |
| Alternative measures  | Tax revenue (alternative)                                                     | Total and non-resource tax revenue (% of GDP), used to re-estimate the tax gap                                                                                   | UNU-WIDER GRD                         |

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