# Does austerity pay off?*

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**Abstract**

We investigate empirically how fiscal shocks—unanticipated and exogenous changes of government consumption growth—impact the sovereign default premium. For this purpose we assemble a new data set for 38 emerging and developed economies. It contains approximately 3,000 observations for the sovereign default premium and three alternative measures of fiscal shocks. We condition our estimates on a) whether shocks are positive or negative and b) initial conditions in terms of fiscal stress. An increase of government consumption hardly affects the default premium. A reduction raises the premium if fiscal stress is severe, but decreases it if initial conditions are benign.

*Keywords:* Fiscal policy, austerity, default premium, fiscal stress, fiscal shocks, local projections, smooth transition

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# 1 Introduction

In the years following the global financial crisis, many European governments implemented sizeable austerity measures. These included spending cuts and tax increases and were meant to confront concerns about rising levels of public debt or outright solvency issues. In fact, yields on debt issued by several European sovereigns started to take off by 2010, reflecting sizeable default premiums (Krishnamurthy et al., 2018). Still, as austerity measures were implemented, default premiums kept rising. Also, along the cross section of countries, large spending cuts were associated with strong increases of the default premium.[1] Against this background, we ask whether austerity causes the default premium to decline, that is, whether austerity pays off.

We find that the answer depends on the conditions under which austerity takes place. Our main result can be summarized as follows. If the default premium is high to begin with, that is, if the economy is under severe *fiscal stress*, a reduction of government spending induces the default premium to increase further: austerity does not pay off. In contrast, if the default premium is initially moderate, cutting government spending induces the default premium to decline: austerity pays off during benign times. This result is robust across a variety of econometric specifications, modifications of our sample, and alternative ways to measure both fiscal stress and changes in government spending.

Throughout we focus on how financial markets respond to austerity measures and sidestep the issue of how such measures impact the actual health of government finances. While austerity impacts fiscal fundamentals such as the level of sovereign debt, these observed fundamentals typically fail to provide a sufficient statistic for assessing the sustainability of debt. For the ability and willingness of governments to service its debt obligations and to roll over liabilities also depends on market conditions (Calvo, 1988; Cole and Kehoe, 2000; Roch and Uhlig, 2018) and a number of country-specific, partly unobserved factors such as the ability to raise tax revenues (Bi, 2012; Lorenzoni and Werning, 2018; Trabandt and Uhlig, 2011). The same level of debt may thus have very different implications for debt sustainability at different times and in different countries. The default premium demanded by financial markets, instead, is based on a broader assessment and provides a more comprehensive picture.

To answer our research question, we assemble a new panel data set for the sovereign default premium in a large number of countries. Specifically, we construct time series for the sovereign default premium in 38 developed and emerging economies, covering the period since the early 1990s. We compute the default premium as the difference in sovereign yields

[1] See Figure A.2 in the online appendix.

vis-à-vis a riskless reference country where sovereign default can be ruled out for practical purposes. Importantly, we only consider yields on government securities issued in a common currency in order to eliminate confounding factors due to expectations of inflation and currency depreciation. For more recent observations, we also rely on data for credit default swap (CDS) spreads. In our sample we only include country-quarter observations right up to, but not including, periods of default. We analyze the variation of the default premium in some detail, both across countries and over time, and summarize it by the cumulative distribution function.

Our interest is on how austerity measures impact the sovereign default premium. Such measures include reductions of government consumption and transfers as well as tax increases. However, in our study we focus on government consumption, because identification is more straightforward in this case.[2] We identify fiscal shocks, that is, variations of government consumption that are a) unanticipated and b) do not represent a systematic response to the state of the economy. Our identification scheme is based on a two-step approach and builds on earlier work by Ramey (2011b) and Blanchard and Perotti (2002). In a first step, we compute three distinct measures of forecast errors of real government consumption growth in order to purify the changes of government spending growth of the anticipated component. For this purpose, we rely on data by professional forecasters, namely *Oxford Economics* and the OECD. Both provide forecasts for a large number of countries, although they do not cover all 38 countries in our sample. For this reason we also estimate a fairly conventional fiscal vector autoregression (VAR) in order to compute forecast errors of government consumption. Analyzing the distribution of forecast errors we find that all three measures are reasonably well behaved. In our baseline we rely on the data by *Oxford Economics*, since they are available at quarterly frequency and the country coverage is quite large.

In a second step, we use local projections à la Jordá (2005) and estimate the response of the default premium to the forecast error of government consumption growth. At this stage, we impose the identification assumption that unanticipated changes of government consumption are not itself an endogenous response to structural innovations. We assume, in other words, that the systematic component of government consumption is predetermined. This assumption is plausible because neither does government consumption automatically respond to the state of the economy, nor is it, because of decision lags, adjusted instantaneously in a discretionary manner. While we cannot test our identification assumption, we provide evidence which suggests that it is indeed satisfied in our sample. Moreover, we show that

[2]Some authors find that whether austerity is tax-based or spending-based is crucial for how it impacts the economy (for recent contributions see Alesina and Ardagna, 2013; Alesina, Barbiero, et al., 2015; Alesina, Favero, et al., 2015).

while our baseline is estimated on quarterly observations, our main result also obtains for a specification which exploits monthly forecast updates.

We allow the effect of austerity to depend on the amount of fiscal stress because austerity measures can be conducted under very different circumstances. Fiscal adjustments may take place under fairly calm conditions. However, often times, they are implemented when the sustainability of public finances is already in doubt and the default premium is high. The specific circumstances are likely to matter for how fiscal shocks propagate through the economy because mounting evidence suggests that the transmission of fiscal policy is state dependent (Auerbach and Gorodnichenko, 2012; Corsetti, Meier, et al., 2012; Ilzetzki et al., 2013). In order to measure fiscal stress we compare the default premium in the period prior to the fiscal shock to the other realizations in sample. In this regard it is key that our sample covers a large variety of countries and time episodes. Formally, we run a smooth transition regression where we use the cumulative distribution function of the sovereign default premium as an indicator function for the degree of fiscal stress.

We find that how fiscal shocks impact the default premium indeed depends on initial conditions in terms of fiscal stress. According to our baseline specification, a reduction of annualized government consumption growth by 1 percentage point *lowers* the default premium by about 30 basis points if fiscal stress is low to begin with. Instead, if fiscal stress is severe, the same shock *raises* the default premium by about 60 basis points. We define *benign times* as a level of the default premium sufficiently low for a reduction of government consumption to reduce the default premium. Our smooth transition model determines a threshold for benign times that differs for advanced and emerging economies, namely 30 and 200 basis points, respectively.

We shed some light on our findings by investigating the adjustment dynamics of important macroeconomic indicators to the shock. Government consumption growth declines somewhat persistently, implying that fiscal shocks tend to lower the level of government consumption permanently. The behavior of public debt and output growth in the aftermath of the shock depends strongly on initial conditions. A fiscal shock that materializes in times of severe stress raises the debt-to-output ratio and lowers output growth significantly. If the same shock takes place in the absence of stress, the debt ratio declines and output growth is unaffected. Lastly, we also assess the impact of fiscal shocks on the "term structure of default premiums" and find that it responds more strongly at the short end.

In terms of empirical contributions our paper relates to a number of studies which have explored the state dependence of the fiscal transmission mechanism. In particular, we build on the smooth transition framework of Auerbach and Gorodnichenko (2012, 2013). Early work by Perotti (1999) already established that fiscal policy affects the economy differently in "good times" and "bad". Giavazzi et al. (2000) suggest that the size and persistence of fiscal measures also matter for their effects. Earlier studies have also focused on how financial markets respond to fiscal policy measures. Ardagna (2009), for instance, reports that interest rates tend to decline in response to large fiscal consolidations. Laubach (2009) finds that future debt and deficits tend to raise U.S. interest rates. Akitoby and Stratmann (2008) focus on how sovereign yield spreads in emerging markets react to changes of fiscal indicators.

The remainder of the paper is organized as follows. We discuss our econometric model as well as our approach to identification in Section 2. Section 3 details the construction of our data set. Here, we also establish a number of basic facts regarding the time-series properties of the sovereign default premium and the series of fiscal shocks. Section 4 presents the results as well as an extensive robustness analysis. It also offers a brief discussion of our results in light of available theoretical models. Section 5 concludes.

# 2 Model specification and identification

In our analysis we rely on local projections, as introduced by Jordá (2005). In order to establish the causal effect of austerity measures on the sovereign default premium and other variables of interest we project these variables on fiscal shocks, that is, variations in fiscal policy that are unanticipated and not systematically related to the state of the economy. In what follows we first introduce our model specification and then explain how we measure fiscal shocks.

## 2.1 Local projections with smooth regime transitions

Local projections are quite flexible in accommodating a panel structure and offer a straightforward way to condition the short-run effects of fiscal shocks on the extent of *fiscal stress* that we measure by the initial level of the sovereign default premium.[3] In addition, local projections allow us to assess whether the sovereign default premium responds asymmetrically to positive and negative shocks. Auerbach and Gorodnichenko (2013), Owyang et al. (2013), and Ramey and Zubairy (2018), among others, also rely on local projections while analyzing fiscal policy. Their focus, however, is on the fiscal multiplier on output and on whether it changes with the business cycle or when interest rates are close to zero.

Formally, letting $x_{i,t+h}$ denote the response of a particular variable in country $i$ at time $t+h$ to a fiscal shock $\varepsilon^g_{i,t}$ at time $t$, we consider the following linear model as our point of

[3] In an earlier working paper version, we also consider VAR models. The results are very similar to those obtained with local projections (see Born, Müller, and Pfeifer, 2015).

departure:

$$x_{i,t+h} = \alpha_{i,h} + \eta_{t,h} + \psi_h \varepsilon^g_{i,t} + u_{i,t+h} \ , \tag{2.1}$$

where $\alpha_{i,h}$ and $\eta_{t,h}$ capture country- and time-fixed effects, respectively. At each horizon $h$, the response of the dependent variable to the shock is given by the coefficient $\psi_h$. An inclusion of further controls is not necessary to the extent that the fiscal shock is identified correctly.[4] We discuss how we measure fiscal shocks below. The error term $u_{i,t+h}$ is assumed to have a zero mean and strictly positive variance.

Our preferred specification is a version of model (2.1) which conditions the response of $x_{i,t+h}$ on the extent of fiscal stress. For this purpose we use a smooth transition model:

$$x_{i,t+h} = \alpha_{i,h} + \eta_{t,h} + \psi_{m,h} F\left(r_{i,t-1}\right) \varepsilon^g_{i,t} + \psi_{n,h} \left[1 - F\left(r_{i,t-1}\right)\right] \varepsilon^g_{i,t} + u_{i,t+h} \ . \tag{2.2}$$

Here, the response of the dependent variable to the shock is allowed to differ at each horizon $h$ across regimes "m" (maximum stress) and "n" (no stress), with the $\psi$-coefficients indexed accordingly. For each country-time observation, the indicator function $0 \leq F\left(r_{i,t-1}\right) \leq 1$ determines the weight of each regime. This indicator function depends on $r_{i,t-1}$, that is, the sovereign default premium in country $i$ at the end of the previous period. As a result the weights are predetermined with respect to the fiscal shock.

Projection (2.2) directly captures the dynamic effects of a fiscal innovation conditional on the circumstances under which it occurs. Formally, the response in period $t + h$ to a government consumption impulse in period $t$, $\varepsilon^g_{i,t}$, conditional on the economy experiencing a particular state today, indexed by $r_{i,t-1}$, is given by the regression coefficients on $\varepsilon^g_{i,t}$ in equation (2.2):

$$\left.\frac{\partial x_{i,t+h}}{\partial \varepsilon^g_{i,t}}\right|_{r_{i,t-1}} = \psi_{m,h} F\left(r_{i,t-1}\right) + \psi_{n,h} \left[1 - F\left(r_{i,t-1}\right)\right] \ . \tag{2.3}$$

This expression illustrates that computing impulse responses based on a single-equation approach does not require us to make additional assumptions on the economy staying in a particular regime (see also the discussion in Ramey and Zubairy, 2018). Rather, the local projection at time $t$ directly provides us with a measure of the average response of an economy in state $r_{i,t-1}$ going forward. Note also that equation (2.3) is just a linear combination of regression coefficients. We can thus rely on a Wald-type test to assess whether responses at a particular horizon are significantly different from each other as a result of different initial conditions.

Our framework also allows us to investigate the effect of positive as opposed to negative shocks by splitting the shock series according to their sign and including both as distinct

[4]Still, in a robustness check, we also include a vector of controls, $X_{i,t-1}$, which features lags of GDP growth, government spending growth, and the default premium.

regressors (Kilian and Vigfusson, 2011). In this case, we estimate the following model:

$$\begin{aligned} x_{i,t+h} &= \alpha_{i,h} + \eta_{t,h} + \psi^{+}_{m,h} F\left(r_{i,t-1}\right) \varepsilon^{g+}_{i,t} + \psi^{-}_{m,h} F\left(r_{i,t-1}\right) \varepsilon^{g-}_{i,t} \\ &\quad + \psi^{+}_{n,h} \left[1 - F\left(r_{i,t-1}\right)\right] \varepsilon^{g+}_{i,t} + \psi^{-}_{n,h} \left[1 - F\left(r_{i,t-1}\right)\right] \varepsilon^{g-}_{i,t} + u_{i,t+h} \ , \end{aligned} \tag{2.4}$$

where a positive shock is constructed as $\varepsilon^{g+}_{i,t} = \varepsilon^{g}_{i,t}$ if $\varepsilon^{g}_{i,t} \geq 0$ and 0 otherwise, and similarly for negative shocks. The rest of the notation follows model (2.2).

The specification of the transition function $F(\cdot)$ involves two steps: the choice of the indicator and the specification of the mapping of the indicator into weights from 0 to 1. First, we choose the (lagged) default premium as our indicator because we think that it provides a natural benchmark to measure the extent of fiscal stress for a given country-time observation. As a market-based measure, it is more comprehensive than specific fiscal indicators such as the debt-to-GDP ratio. Second, we specify the indicator function $F\left(r_{i,t-1}\right)$ on the basis of the empirical cumulative density function (CDF) in our sample. As a result we do not have to postulate a specific parametric indicator function as Auerbach and Gorodnichenko (2012) do in order to capture the state of the business cycle. Moreover, extreme values are given by observations which actually materialize in sample.[5] Formally, we have

$$F\left(r_{i,t-1}\right) = \frac{1}{N} \sum_{j=1}^{N} \mathbb{1}_{r_j \leq r_{i,t-1}} \ , \tag{2.5}$$

where $N$ is the number of country-time observations in our sample, $\mathbb{1}$ is an indicator function and $j$ indexes all country-time observations, separately for the group of advanced and emerging economies (baseline). $F\left(r_{i,t-1}\right)$ equals one if the premium is at the maximum of the sample: a situation of *maximum stress*. $F\left(r_{i,t-1}\right)$ equals zero if the premium is at the minimum: a situation of *no stress*. Of course, actual economies hardly ever operate in either of these two polar regimes. This notion is captured in the estimation, as, for each observation, the impact of the regressors is a weighted average of the dynamics in the two regimes. Put differently, regime transition is "smooth" as in Auerbach and Gorodnichenko (2012). Also, all observations contribute to identifying the dynamics that govern in the polar regimes.

## 2.2 Fiscal shocks

In our empirical analysis we focus on government consumption to construct a measure of fiscal shocks. Of course, actual austerity programs may have sizeable tax and transfer components, but identification is less thorny in case of government spending. Under our identification

[5] We check the robustness of our results by using a parametric logistic transition function and find that they are robust, see Table A.3 in the online appendix.

assumption (discussed below), a fiscal shock is an unanticipated innovation to the rate of change in government spending. The innovation represents fiscal "news" and can be captured by the forecast error (Ramey, 2011b). Formally, we have

$$\varepsilon^g_{i,t} = \Delta g_{i,t} - E_{t-1}\Delta g_{i,t} \ , \tag{2.6}$$

where $\Delta g_{i,t}$ is the realization of government spending growth and $E_{t-1}\Delta g_{i,t}$ is the previous period's forecast. It captures predictable changes due to, for instance, systematic responses to the state of the economy.

In our analysis we pursue three alternative approaches to compute $\varepsilon^g_{i,t}$. They differ in the source/method for obtaining the government spending forecast. The first two approaches closely follow Ramey (2011b) and use professional forecasts as a direct measure for $E_{t-1}\Delta g_{i,t}$. Ramey (2011b) in her analysis of U.S. time series relies on the Survey of Professional Forecasters, which is unavailable for our sample. We consider two different sets of professional forecasts instead.

In our baseline specification, we use proprietary data on quarter-on-quarter growth rate projections provided by *Oxford Economics*, a large forecasting firm that serves 1,500 clients, among them international corporations, financial institutions, government organizations, and universities. It employs some 200 economists and analysts. The reason we focus on growth rates rather than levels is that there are irregular base-year changes for the countries in our sample that would show up as structural breaks if we were considering levels.

Our second set of professional forecasts is compiled twice a year by the OECD and disseminated in its *Economic Outlook*. Forecasts are prepared at the end of an observation period, namely, in June and December of each year and tend to perform quite well (Auerbach and Gorodnichenko, 2012). While the coverage of some countries starts earlier than in the *Oxford Economics* sample, observations are available at lower frequency, that is, semi-annually, only.

Our third specification employs a panel VAR model to forecast government consumption growth and to compute the forecast error. Formally, let $X_{i,t}$ denote a vector of endogenous variables, which includes government spending growth, output growth, and the default premium. We estimate the following panel VAR:

$$X_{i,t} = \alpha_i + \eta_t + A(L)X_{i,t-1} + \nu_{i,t} \ , \tag{2.7}$$

where $A(L)$ is a lag polynomial and $\nu_{i,t}$ is a vector of reduced form disturbances with covariance matrix $E(\nu_{i,t}\nu'_{i,t}) = \Omega$. In our analysis below we allow for four lags since the model is estimated on quarterly data. Assuming i) a lower Cholesky factorization $\mathbf{L}$ of $\Omega$, and ii) that government consumption growth is ordered on top in the vector $X_{i,t}$, the structural shock $\varepsilon^g_{i,t}$ equals the (scaled) first element of the reduced form disturbance vector $\nu_{i,t}$, i.e. $\varepsilon^g_{i,t} = \mathbf{L}^{-1}\nu_{i,t}$.[6]

This forecasting model allows for the broadest coverage in terms of countries, time periods, and frequency as it relies on high-quality quarterly national accounts data (see Ilzetzki et al., 2013), but not on professional forecasts. However, influential contributions by Ramey (2011b) and Leeper et al. (2013) have highlighted the potential limitation of VAR models in accounting for "fiscal foresight". Intuitively, fiscal measures that are not predictable by the econometrician on the basis of conventionally observable time series may still be known in advance, for instance because it takes time to pass legislation. Hence, all things equal, we prefer to rely on professional forecasts that encompass the broadest possible information set to a finite-order VAR that may not span the relevant state space entirely. But given that professional forecasts are only available for a restricted sample in terms of coverage and frequency, we think that the VAR still serves as an important reference point. We will report results for all three forecasting models below.

In terms of identification, we assume for *all three specifications of the forecasting model* that the forecast error of government spending growth is not systematically caused by the contemporaneous state of the economy. Hence it represents a genuine fiscal shock. This identifying assumption—that the systematic component of government consumption is predetermined—goes back to Blanchard and Perotti (2002) but is also implicit in Ramey (2011b) when she considers news shocks based on professional forecasts (see also Ramey and Zubairy, 2018). To see this, assume, to the contrary, that government spending is adjusted instantaneously to other structural innovations in a systematic way. A positive technology shock may for example drive up output and, if fiscal policy can immediately react to output, result in an unforeseen change of government consumption. In this scenario a technology shock would give rise to "fiscal news". However, as originally argued by Blanchard and Perotti (2002), government consumption is unlikely a) to respond automatically to the cycle and b) to be adjusted instantaneously in a discretionary manner by policymakers. In this context it is important to note that our measure of government consumption is derived from national accounts and therefore accrual-based[7] and, unlike transfers, not composed of cyclical items. Discretionary changes of government spending, in turn, are subject to decision lags that

[6]The estimated shocks $\hat{\varepsilon}^g_{i,t}$ in this third specification are generated regressors in the second stage. However, as shown in Pagan (1984), the standard errors on the generated regressors are asymptotically valid under the null hypothesis that the coefficient is zero; see also Coibion and Gorodnichenko (2015), footnote 18, on this point.

[7]Hence, a change in cash flows which results from, say, a government deferring payments during a crisis is not recorded as a change of government consumption in our data set.

prevent policymakers from responding instantaneously to contemporaneous developments in the economy.

Anecdotal evidence suggests that this holds true also in times of fiscal stress. Still, we cannot rule out that policy measures—while debated for some time—are sometimes spurred by contemporaneous financial-market developments.[8] Against this background, we exploit the specific panel structure of our data set and assess whether policy makers adjust government consumption systematically in response to contemporaneous movements of the sovereign default premium. Importantly, we find that while the common component of the sovereign default premium induces the country-specific component of the premium to move on impact, it does not impact government consumption significantly (see Section 4.1). Hence, we maintain our identification assumption with some confidence.

Before we turn to the data, we briefly explain why the narrative approach to identify fiscal shocks is not suited to analyze the issue at hand. Following the work of C. D. Romer and D. H. Romer (2010) for the U.S., Devries et al. (2011) have constructed a data set of fiscal measures for a sample of OECD countries (see also Guajardo et al., 2011). These fiscal policy measures are identified as being orthogonal to the business cycle on narrative grounds. A large number of these measures are taken in order to reign in public debt or budget deficits which, in turn, move systematically with the sovereign default premium. These "shocks" are therefore likely to be endogenous with respect to the default premium and may not be used to identify the causal effect of fiscal policy on the former.

# 3 Data

Our analysis is based on a new data set. In addition to standard time-series data, it contains observations for the sovereign default premium and for fiscal shocks for up to 38 emerging and advanced economies.

## 3.1 The sovereign default premium

In what follows we detail the construction of the sovereign default premium. Since our focus is on how fiscal shocks impact the default premium, we consider only those countries for which quarterly observations on government consumption are available (see below). As stressed in the introduction, we construct a mostly spread-based measure using yields for securities issued in common currency. To the extent that financial markets are sufficiently integrated, we thus eliminate fluctuations in yield spreads due to inflation expectations and

[8] See the discussion in Online Appendix A.2.2.

the risk premiums associated with them. In addition to a default risk premium, if duration differs or drifts, yield spreads may still reflect a term premium (Broner et al., 2013). We try to minimize the term premium by constructing the yield spread on the basis of yields for bonds with a comparable maturity and coupon.[9] As a result, yield spreads should primarily reflect the probability and expected extent of a sovereign default—as assessed by market participants.[10]

We obtain our default-risk measure based on four distinct sources/strategies. First, for a subset of (formerly) emerging markets we directly rely on J.P. Morgan's Emerging Markets Bond Index (EMBI) spreads. Second, we add to these observations data for euro area countries based on the "long-term interest rate for convergence purposes". Third, we make use of the issuance of foreign-currency government bonds in many advanced economies during the 1990s and 2000s in order to extend our sample to non-euro area countries and the pre-euro period. Finally, in the more recent part of the sample, a direct measure of default risk has become available in the form of CDS spreads.[11]

The use of CDS spreads also allows us to include the benchmark countries United States (EMBI) and Germany (long-term convergence yields) in the sample. In order to get an absolute measure of default risk for the other countries, we add the CDS spread of the respective benchmark countries to the relative country spread. For the period before CDS data are available, we add the value of the average CDS spread of the period prior to the default of Lehman Brothers.[12] Online Appendix A.1.6 illustrates the construction of the default premium measure by means of an example. In our sample we only include country-quarter observations right up to, but not including, periods of actual default (see the note to Table A.1 in the online appendix for the excluded episodes). During default episodes, trading in secondary markets typically collapses and the information content of observed interest rates is limited. Excluding default episodes does not result in sample-selection bias, however, because we conduct inference about the effects of austerity when a country has not yet defaulted and austerity still is a choice.

Table A.1 provides basic descriptive statistics for the default premium, $r_t$. The total number of observations in our sample is 3013, of which 1648 are for developed economies

[9] We focus on long-term rates whenever possible. As they are closely linked to the average of expected future short-term rates, they are a more appropriate measure of governments' refinancing costs than short-term rates. We investigate the issue of different maturities in Section A.4.2 in the online appendix.

[10] In principle, spreads may also reflect a liquidity premium—an issue we ignore in what follows because we consider government debt traded in mature markets. See Online Appendix A.1.4 for a more detailed discussion.

[11] See Online Appendix A.1.1 for details.

[12] Before the Lehman Brothers default, German and U.S. CDS were below 8 basis points and thus virtually zero. After Lehman, they peak at about 70 basis points and slowly return to about 15 basis points.

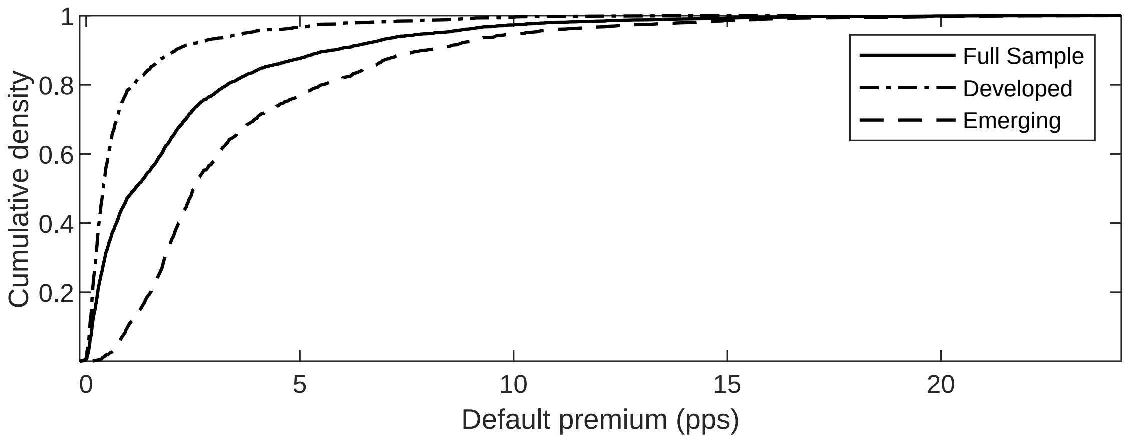


**Figure 1:** Sovereign default premium: empirical cumulative density function (CDF). Notes: horizontal axis measures the default premium in percentage points. Vertical axis measures fraction of observations for which the default premium is at most the value on the horizontal axis. Solid line displays CDF for full sample, dashed-dotted line: developed economies only, dashed line: emerging economies only.

and 1365 for emerging economies.[13] The default premium is measured at the end of the quarter in percentage points and varies considerably across our sample.[14] In a couple of euro area countries the lowest realizations of the default premium are slightly negative.[15] For the group of developed economies (see Table A.1 for the classification), we observe the highest premiums in Portugal (12 pps) and Greece (24 pps). For emerging economies, the highest values are reached in Brazil (24 pps), Ecuador (21 pps), and Argentina (20 pps).[16]

Compared to these values, most realizations of the default premium in our sample are small. This is apparent from the CDF plotted in Figure 1 for the entire sample (solid line), but also for the set of developed (dash-dotted line) and emerging economies (dashed line) in isolation. In each case, the mass of observations is very much concentrated on the left. For the full sample about 50 percent of the observations for the default premium are below 1.15 percentage points. Still, there are considerable differences across the two country groups: 99.4 percent of observations are below 10 percentage points in the sample of developed economies. The corresponding number is only 95 percent in the sample of emerging-market economies.

As explained in Section 2 above, the empirical CDF of the default premium provides a

[13]In order to construct the CDF, we rely on the broadest possible sample, that is, we do not restrict the default premiums to country-quarter observations where we have also have government spending forecast errors available (which would also differ across the three forecasting approaches).

[14]Results are unchanged when using the average over the quarter.

[15]The reason is that the long-term convergence yields are sometimes slightly lower than the German ones. This is presumably due to their construction not controlling for different bond duration characteristics and small maturity differences.

[16]During default episodes, spreads in secondary markets can achieve even higher values. In case of Argentina, the peak spread was 70 percentage points.

natural benchmark to measure the extent of fiscal stress for a given time-country observation. Importantly, it is a purely empirical benchmark and allows us to refrain from making specific and perhaps arbitrary assumptions. Also, as a market-based measure it is arguably more comprehensive than specific fiscal indicators such as the debt-to-GDP ratio. As a practical matter, we specify the function $F(\cdot)$ in projections (2.2) and (2.4) on the basis of the country-group-specific CDFs shown in Figure 1. Our indicator for stress takes on the highest value of 1 when a country at a given point in time experiences the highest default premium observed in our sample. In contrast, it takes a value of 0 when the country experiences the lowest premium observed in our sample.

Figures A.3 to A.5 in the online appendix show the fiscal stress indicator over time for the countries in our sample. They also show the empirical CDF of the smoothed output gap measure of Auerbach and Gorodnichenko (2012). While there is some correlation between fiscal stress and the state of the cycle, there are long periods during which the measures move in opposite directions.

In Table A.1 in the online appendix we also report the correlation of the sovereign default premium with output growth and the growth of government consumption, based on national accounts data. It turns out that the default premium is countercyclical in all countries, although sometimes the correlation is negligible. In contrast, the within-country correlation of the default premium and government consumption growth varies across countries. It is negative for most countries, but often weakly so.

## 3.2 Fiscal data

We now turn to the second original contribution in terms of data collection, namely the construction of fiscal shocks. These shocks capture unanticipated and non-systematic innovations in government consumption growth. As explained in Section 2 above, we construct three alternative measures.

For the first measure (baseline) we use *Oxford Economics* as a data source. *Oxford Economics* provides data on quarterly real government consumption. The data comes in monthly vintages and is unbalanced with some missing vintages in between. The earliest available vintage is November 1996. The latest available vintage used in our analysis is December 2017. Nowcasts and forecasts are produced by *Oxford Economics*, while the back data is provided to *Oxford Economics* by *Haver Analytics*, which in turn usually obtains its data from national statistical offices. The data come with a flag indicating whether the most recent vintage is seasonally adjusted. For the countries where this is not the case (Argentina, Malaysia, Thailand, and Turkey), we use Tramo/SEATS for seasonal adjustment. For the other countries, we visually inspect the vintages for the presence of a seasonal pattern and remove the seasonal figure in all vintages preceding the move to seasonal adjustment at the source.[17] A few countries move between exhibiting seasonal patterns and being seasonally adjusted several times. As there is no consistent way to identify the break points, we remove these countries from our sample. The first two columns of Table A.2 in the online appendix report the resulting sample.

*Oxford Economics* updates its quarterly forecasts for a number of macroeconomic aggregates, including real government consumption on a monthly basis. To compute the forecast error for a given quarter $t$, we compute the difference between the real-time value of government spending or GDP growth and the value forecasted for that quarter a quarter earlier. As *Oxford Economics* forecasts are usually made every month, we use a geometric average over the available monthly values in a given quarter $t$ to arrive at quarterly forecasts for spending growth next period $E_t \Delta g_{i,t+1}$ and real-time realizations of spending growth $\Delta g_{i,t}$. We also exploit monthly observation directly in the context of our sensitivity analysis.

Our second measure of fiscal shocks relies on data provided by the OECD in its Economic Outlook, which is published in June and December of each year. Before 1996S2, the forecast target period referred to half-years. Since 1996S2, the OECD forecast target refers to quarterly values. We sum the quarterly level observations for each half-year period in order to obtain a sample for which the frequency at which forecasts are made and the forecast target frequency align. This allows us to obtain observations at semi-annual frequency and to compute the growth-rate forecasts and real-time growth-rate realizations as well as the associated forecast errors.

Our third measure is based on an estimated VAR model that we rely on to forecast government consumption growth. For a long time, studies of the fiscal transmission mechanism have been limited to a small set of countries because high-quality quarterly data for government consumption was not available.[18] Rather, quarterly data was often derived from indirect sources using time disaggregation/interpolation. In a recent contribution, Ilzetzki et al. (2013) have collected quarterly data based on direct sources for government consumption for 44 countries. To estimate our VAR model, we collect quarterly data for government consumption expenditure based on national accounts/non-financial accounts of the government along the lines of Ilzetzki et al. (2013). Our sample differs to some extent, depending on the countries for which we are able to compute the sovereign default premium. We also extend their

[17] This is the case for Finland before 1999Q2, Ireland before 2004Q1, and Sweden before 1998Q2.

[18] Some studies have resorted to annual data (e.g., Beetsma et al., 2006, 2008; Bénétrix and Lane, 2013). In this case identification assumptions tend to be more restrictive. However, Born and Müller (2012) consider both quarterly and annual data for four OECD countries. They find that the estimated effects of government spending shocks do hardly differ.

**Table 1:** Forecast errors of government consumption growth: descriptive statistics

| | OE | OECD | VAR |
|---|---|---|---|
| Countries | 23 | 21 | 38 |
| T | 1696 | 887 | 2832 |
| Mean | -0.016 | -0.039 | 0 |
| RMSE | 0.616 | 0.402 | 2.947 |
| # significant constants | 4 | 1 | 1 |
| # significant LBQ-Tests | 5 | 6 | 7 |
| Wald $F$-statistic | 44.41 | 0.99 | 1790.86 |

*Notes:* Forecast errors measured in percentage points. Semi-annual OECD forecast errors are rescaled to reflect errors in the growth rate at a quarterly level. "# significant constants" refers to the number of countries where the mean forecast error is significantly different from 0. "# significant LBQ-Tests" refers to the number of countries where the Ljung and Box (1978) Q-statistic with 8 lags rejects the null hypothesis of the forecast error being white noise. The significance level for both tests is 5%. Kleibergen and Paap (2006) $rk$-Wald $F$-statistic computed using Stata's xtivreg2 in a first-stage regression of government consumption growth on the respective forecast error. Robust covariance estimator clustered at country and quarter level.

sample to include more recent observations and additional countries for which we were able to confirm with statistical agencies the availability of government consumption data based on direct sources.[19] The full sample coverage is shown in Table A.2 in the online appendix. Our earliest observation for which we obtain data on the default premium and on government consumption is 1991Q1, namely for Denmark and Italy. Our sample runs up to 2017Q4.

Table 1 displays descriptive statistics for the three forecast errors. All three forecasts perform reasonably well, but are not perfect. Over the full sample, the average forecast errors are close to zero for all three models. In the VAR model this is by construction. On an individual country basis, both the OECD and *Oxford Economics* produce forecasts with a relatively low root mean squared error (RMSE). The VAR forecasts exhibit the largest RMSE, but for a somewhat more challenging sample of countries. The OECD and VAR forecasts are unbiased, having only one country each with a significant mean forecast error.[20] *Oxford Economics* performs a bit worse in this regard. Regardless of the forecasting model, about 20% of the countries in our sample have autocorrelated forecast errors, as measured by a Ljung and Box (1978) Q-statistic test with eight lags. Our conjecture is that this result is partially driven by the depth of the Great Recession that caught most observers by surprise. This autocorrelation suggests that forecasts could be improved by including lags of previous

[19] For several European countries, we also include earlier observations for the 1990s whenever we are able to compute a default premium (see Online Appendix A.2.1.

[20] We run a regression of the forecast error on a constant and evaluate whether the coefficient is statistically significantly different from 0.

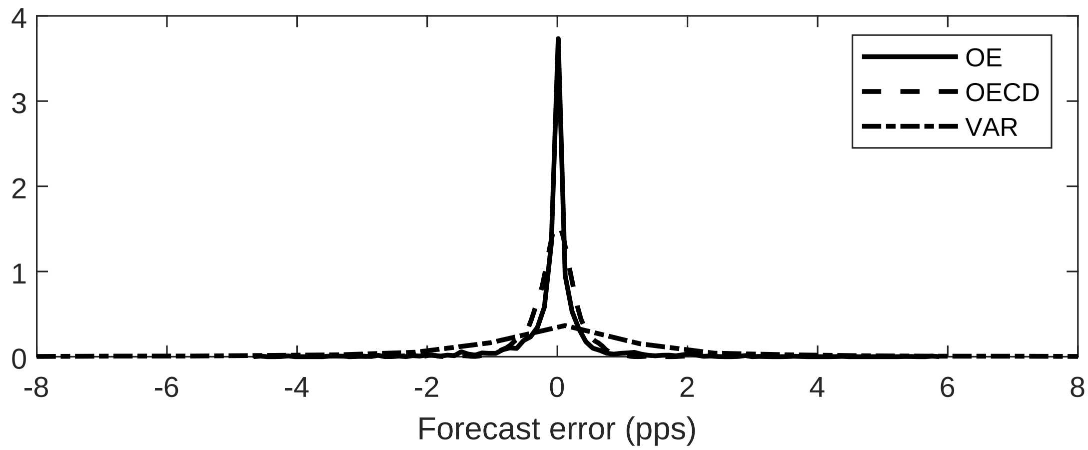


**Figure 2:** Distribution of forecast errors. *Notes:* lines correspond to kernel density smoother estimates for forecast errors based on *Oxford Economics* (OE, solid line), OECD (dashed), VAR (dashed-dotted).

forecast errors. In a robustness check below we find results similar to the baseline when we do so.

Figure 2 shows the kernel density estimates of the forecast errors. In line with Table 1, it illustrates that the forecast errors based on *Oxford Economics* and OECD forecasts are considerably less dispersed than the VAR-based forecast errors. Forecast errors based on *Oxford Economics* forecasts, however, exhibit somewhat fatter tails than those based on the OECD.

In the last row of Table 1 we report a measure of the predictive power of our identified shocks in the form of an F-statistic along the lines of the tests conducted in Ramey (2011b) and Ramey and Zubairy (2018).[21] Reassuringly, our baseline forecast errors based on the *Oxford Economics* forecasts, as well as the VAR-based forecast errors are comfortably above the rule-of-thumb threshold of 10 proposed by Staiger and Stock (1997).[22] The semi-annual OECD forecast errors, instead, do not seem to be a good predictor for semi-annual government consumption growth. Given their frequent use in the literature, we nevertheless report the results based on this forecasting model.

Finally, our data set also includes data on government debt for a subset of the countries in our sample. Here we draw on two sources. First, Eurostat provides data on gross quarterly government debt of the general government (*gov_10q_ggdebt*) for EU countries. Second, the

[21] Technically, given our panel structure with potentially non i.i.d. errors, we follow the suggestion in Baum et al. (2007) and check the predictive power of our identified shocks using the Kleibergen and Paap (2006) *rk* Wald $F$-statistic. It is computed in a "first-stage" panel fixed effects regression of the government consumption growth variable on the respective shock measure. Computing "naive" $F$-statistics in our pooled sample yields very similar values.

[22] The Montiel Olea and Pflueger (2013)-threshold for the 5 percent critical value for testing the null hypothesis that the 2SLS bias exceeds 10 percent of the OLS bias in our context is 23.1.

Worldbank's Quarterly Public Sector Debt (QPSD) Database provides data on gross general debt for some additional countries in our sample. If the latter database does not provide data for the general government but the central government, we use that series instead.

# 4 Results

In what follows we present the main results of our analysis. We first focus on the instantaneous response of the default premium to fiscal shocks. Afterwards we provide additional evidence on the adjustment dynamics and the transmission mechanism. Throughout we highlight the importance of initial conditions under which austerity takes place, namely the extent to which there is fiscal stress.

## 4.1 The impact of fiscal shocks on the default premium

In Table 2 we report our estimates for the response of the sovereign default premium to fiscal shocks. The different columns in the table show results for the alternative models that we use to compute the forecast error of government consumption growth, see equation (2.6) above. The different panels, in turn, refer to different specifications of the projections (2.1)–(2.4). In each case we focus on the instantaneous response of the premium ($h = 0$) and normalize coefficients so that they reflect the response of the default premium to an unanticipated *reduction* of government consumption growth by 1 percentage point (annualized). Standard errors are given in parentheses. They are robust with respect to heteroskedasticity as well as serial and cross-sectional correlation (Driscoll and Kraay, 1998).

Consider first the results for our baseline specification of the forecasting model, shown in the leftmost column. Here, we compute fiscal shocks as the difference of actual government consumption growth (as perceived in real time) and the forecast of *Oxford Economics*. In this case our sample covers 23 countries and consists of a total of 1515 observations. In the top panel, to set the stage, we show results for the *unconditional* linear projection (2.1), that is, we do not condition on initial conditions in terms of fiscal stress. We find that an unanticipated and exogenous reduction of government consumption raises the default premium significantly. Quantitatively, the effect is rather small: the default premium increases by 6 basis points. Once we estimate a version of model (2.4) and allow for different effects of spending cuts and hikes (but without accounting for fiscal stress) the effects of cuts are larger, namely about 16 basis points. There is no significant effect of spending increases (not shown).

In the second and third panel, we report results for the polar regimes of "maximum fiscal stress" and "no fiscal stress", respectively, which we obtain as a result of estimating

**Table 2:** Instantaneous response of default premium (basis points) to reduction of government consumption growth

| | Forecasting model | | | |
|---|---|---|---|---|
| | Oxford Econ. | OECD | VAR | VAR (max sample) |
| Unconditional projection | | | | |
| All | 6.02* | 8.28 | 0.74 | 0.35 |
| | (3.70) | (6.25) | (0.47) | (0.47) |
| Cuts only | 15.61** | 20.79** | 1.83* | 2.43** |
| | (7.78) | (10.32) | (1.01) | (1.05) |
| Maximum stress | | | | |
| All | 20.86 | 26.99*** | 1.52 | 0.58 |
| | (12.99) | (9.03) | (2.10) | (1.08) |
| Cuts only | 58.81*** | 55.27*** | 15.91*** | 10.40*** |
| | (17.34) | (9.99) | (5.25) | (3.21) |
| No fiscal stress | | | | |
| All | −6.99 | −20.18*** | 0.25 | −0.04 |
| | (4.98) | (6.17) | (0.77) | (0.66) |
| Cuts only | −33.40*** | −39.00*** | −7.66*** | −9.67*** |
| | (9.85) | (14.27) | (2.63) | (2.83) |
| Diff. max stress–no stress | | | | |
| All | 27.85 | 47.17*** | 1.27 | 0.62 |
| Cuts only | 92.21*** | 94.27*** | 23.57*** | 20.07*** |
| Countries | 23 | 21 | 23 | 38 |
| Observations | 1515 | 708 | 1443 | 2689 |

*Notes:* Response to unanticipated reduction of government spending growth by 1 percentage point (annualized). Estimates based on projections (2.1)–(2.4), with $h = 0$. Driscoll and Kraay (1998)-standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively. Forecast error for spending growth computed via different forecasting models: *Oxford Economics*, OECD and VAR model; right-most column uses largest possible sample, second to right-most column only uses sample for which forecasts by *Oxford Economics* are available.

the *conditional* model (2.2). Recall that we condition the response of the premium on the extent of fiscal stress to begin with (that is, in the previous quarter) relative to the empirical distribution in our sample. In the baseline, our measure of stress is computed on the basis of distinct distributions for advanced and emerging economies. It turns out that results are robust to modifying this assumption, as we discuss below.

For regime "maximum stress" (regime $m$), results are rather stark. Lumping together cuts and hikes in one time series does not yield significant results. However, if we allow for a differential effect of hikes and cuts as in model (2.4), we find that spending cuts induce the default premium to rise strongly. Severe fiscal stress alters the quantitative impact of cuts dramatically. We now find an increase of the premium by almost 60 basis points.

For regime "no stress" (regime $n$) we again find no significant effects if we do not distinguish between cuts and increases of government consumption. Spending cuts *per se*, however, tend to lower the default premium if stress is absent and quite strongly so. This result requires some qualification. Formally, we define "no stress" as a situation where the default premium is at its lowest point in the sample. Strictly speaking a further reduction of the default premium is hard to achieve.[23]

The strong reduction of the premium in regime $n$ reflects a more fundamental issue: the estimated projection (2.2) extrapolates from low stress observations to the pure regime of zero stress. Put differently, our finding suggests that reducing government consumption may actually bring down the default premium even if there is *some* fiscal stress. In what follows we refer to such a moderate level of fiscal stress as "benign times", defined as the region of the default premium for which a reduction of government consumption does not induce the default premium to rise. As we combine our estimates for regime $m$ and $n$, we obtain the threshold value of the indicator function and, in turn, the implied threshold for the default premium. For advanced economies benign times correspond to values of the default premium of up to approximately 30 basis points. The corresponding value for emerging economies is 200 basis points.

In sum, we find that whether austerity pays off or not depends on the initial conditions in terms of fiscal stress. If stress is at its maximum (historically speaking), a reduction of government consumption does not pay off. It raises the default premium strongly. If the reduction takes place while stress is low, that is during benign times, it tends to reduce the default premium.

The last panel of Table 2 underscores once more the importance of conditioning the response of the premium on initial conditions in terms of fiscal stress. It shows the difference in the response of the premium across regimes. It amounts to almost 1 percentage point (cuts only). A Wald-type test shows that the difference is also statistically significant.

Our result is robust to changes in the way we compute fiscal shocks. Specifically, in the second column we report results that obtain once we measure the forecast error of government consumption growth on the basis of OECD forecasts. Recall that in this case, forecasts for government consumption are compiled twice a year. Also, the country coverage is smaller than in the baseline. The sample consists of 20 countries and 708 semiannual observations. However, the results, shown in the second column of Table 2 are similar to the baseline.[24]

[23]In principle, a further reduction is possible only to the extent that even country-time observation like those for the US in the mid-2000s were not entirely risk free. This is unlikely to rationalize our estimate, which is quantitatively nontrivial.

[24]In order to ensure that results are quantitatively comparable to the baseline we adjust our estimates:

Importantly, this holds for all model specifications. We find once more that whether fiscal shocks raise or lower the default premium depends on the extent of fiscal stress.

The third forecasting model we consider is a conventional VAR model with four lags of government spending growth, real GDP growth, and the default premium. As discussed above, a potential downside of the VAR model is that it may fail to capture fiscal news that are known to market participants (but not the econometrician) and hence provide an incorrect measure of fiscal surprises. On the other hand, the country coverage of professional forecasters is limited. And indeed the sample for which we are able to compute quarterly VAR-based forecasts is considerably larger: it comprises 38 countries and 2689 country-time observations. We compute the unanticipated innovations of government spending growth on the basis of the VAR model and use these fiscal shocks to estimate projections (2.1)–(2.4). The two rightmost columns of Table 2 report the results. In the first of those columns we show VAR-based estimates for a restricted sample, namely the sample for which forecasts of government spending growth by *Oxford Economics* are available. In the second column we estimate the model on the largest possible sample.

We find that results are very similar across samples. Qualitatively, they are also similar to those of our baseline specification for the unconditional model as well as for benign times and times of severe fiscal stress. We find that reducing quarterly government spending growth raises the default premium if fiscal stress is very high to begin with. As before, spending cuts in the absence of stress tend to lower the default premium. However, compared to the forecasting models which employ professional forecasts, the effects are estimated to be quite a bit weaker. This may be due to the fact that the VAR does not capture all fiscal measures that are foreseen by market participants (Leeper et al., 2013; Ramey, 2011b).[25] That said, we note that our main result also obtains once we consider a much larger group of countries and rely on a VAR to compute forecast errors: the effects of spending cuts on the default premium depend on initial conditions.

## 4.2 Robustness

To the extent that our findings are surprising, notably for regime $m$, it may raise doubts about the identification assumption that we entertain throughout, namely, that the systematic component of government consumption is predetermined. A possible objection to our analysis

they reflect the response of the default premium to a change of government spending equivalent to a reduction of its growth rate by 1 percentage point (annualized).

[25] The larger RMSE reported in Table 1 indicates that professional forecasts indeed have a more complete information set. However, because our VAR model includes an inherently forward-looking variable, namely the sovereign default premium, potential problems due to fiscal foresight should at least be somewhat mitigated (Sims, 2012).

**Table 3:** Responses to global default premium component

| | Linear model | No fiscal stress | Maximum stress |
|---|---|---|---|
| Default premium in individual country | | | |
| Horizon 0 | 1.662 | −1.848 | 3.536* |
| | (1.355) | (2.292) | (1.988) |
| Horizon 1 | 0.969** | −2.005*** | 2.496** |
| | (0.428) | (0.422) | (1.072) |
| Forecast error of government consumption growth | | | |
| Horizon 0 | 0.001 | 0.005 | −0.007 |
| | (0.003) | (0.003) | (0.006) |
| Horizon 1 | 0.001 | 0.001 | 0.004 |
| | (0.002) | (0.004) | (0.006) |

*Notes:* Estimates based on forecast error for government consumption growth by *Oxford Economics* and common component of default premium (first principal component). Driscoll and Kraay (1998)-standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively.

is that results might be driven by reverse causality: as the sovereign default premium rises, governments may immediately cut government consumption, for instance, in order to calm financial markets. The panel structure of our data set allows us to assess this objection formally. We first extract a common factor in the default premium along the cross-sectional dimension of our panel by means of a principal component analysis (see Longstaff et al., 2011, for a similar approach), separately for developed and emerging economies. In a second step, we project the default premium and the forecast error on this common factor, as well as on its lags.[26] We consider the impact period as well as the impact in the next period ($h = \{0, 1\}$). Identification rests on the assumption that the common factor—variations of which may, for instance, reflect changes in the stochastic discount factor of global investors—is not contemporaneously affected by country-specific developments.[27]

We show results in Table 3. As before, we condition the effects on the extent of fiscal stress and normalize coefficients so that they capture the response to an increase of the common factor of the default premium by 1 percentage point. In the upper panel we show the response of the default premium measured in basis points. We observe that the "local" default premium increases significantly. Upon closer inspection we find that this result is

[26]We include four lags. In this ways, we seek to establish the effect of an innovation in the common factor on country-specific variables.

[27]We exclude the U.S. in the second stage as in this case the assumption is questionable.

**Table 4:** Instantaneous response of default premium (basis points) to fiscal shock: alternative specifications

| | Specification | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] |
| Unconditional projection | | | | | | | | |
| All | 2.07** | 2.57* | 5.50* | 3.09*** | 6.30 | 7.41** | 5.77* | 6.81 |
| | (1.01) | (1.58) | (3.10) | (1.18) | (3.95) | (3.31) | (3.39) | (4.38) |
| Cuts only | 2.34 | 8.25*** | 13.28** | 5.50** | 16.62** | 15.16** | 14.12** | 16.99** |
| | (1.76) | (3.28) | (6.89) | (2.38) | (8.38) | (7.39) | (7.18) | (7.84) |
| Maximum stress | | | | | | | | |
| All | 6.08** | 7.24 | 18.02** | 8.49** | 19.69* | 17.24* | 18.12 | 5.56 |
| | (3.14) | (5.01) | (7.59) | (3.58) | (11.96) | (10.02) | (11.69) | (8.11) |
| Cuts only | 13.13*** | 27.44*** | 54.40*** | 13.75*** | 47.15*** | 41.42*** | 46.95*** | 85.28*** |
| | (4.73) | (5.74) | (9.89) | (24.74) | (16.55) | (14.39) | (16.28) | (12.11) |
| No fiscal stress | | | | | | | | |
| All | −0.81 | −1.80 | −5.31** | −0.97 | −6.72 | −3.70 | −5.73 | 0.11 |
| | (1.25) | (2.23) | (2.73) | (1.43) | (5.64) | (4.08) | (5.07) | (22.86) |
| Cuts only | −6.93*** | −15.56*** | −32.22*** | −2.89*** | −24.62** | −20.64*** | −25.39*** | −169.47** |
| | (1.75) | (4.71) | (5.22) | (3.50) | (10.13) | (8.12) | (9.09) | (88.15) |
| Diff. max stress–no stress | | | | | | | | |
| All | 6.89* | 9.04 | 23.33** | 9.45** | 26.41 | 20.95 | 23.85 | 5.46 |
| Cuts only | 20.06*** | 43.00*** | 86.62*** | 16.64* | 71.77*** | 62.06*** | 72.34*** | 254.75*** |
| Countries | 23 | 23 | 23 | 23 | 23 | 23 | 23 | 23 |
| Observations | 4153 | 1515 | 1362 | 1226 | 1445 | 1504 | 1515 | 1515 |

*Notes:* Response to unanticipated reduction of government spending growth by 1 percentage point (annualized). Estimates based on projection (2.2), with $h = 0$ and using *Oxford Economics* forecasts. Driscoll and Kraay (1998)-standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively. 1= monthly model using fiscal news about the current quarter, 2 = spreads relative to country-group common factor as dependent variable, 3 = four lags of the forecast error included, 4 = four lags of the default premium, government spending growth, and GDP growth included, 5 = controlling for global factors instead of time-fixed effects (see text), 6 = fiscal deficit and trade balance (as shares of GDP) included, 7= ICRG political risk index included, 8 = pooled mean group estimator.

driven by regime $m$ (right column). In contrast, there is no significant response of the forecast error for government consumption growth (bottom panel), regardless of whether fiscal stress is severe or not. While not a formal test of our identification assumption, this evidence is highly suggestive.[28] It shows that in response to a higher default premium caused by global developments there is no systematic, unforeseen contemporaneous fiscal adjustment even if there is severe stress to begin with.

More generally, the assumption that the systematic component of government consumption is predetermined and may not be adjusted to the state of the economy within the period is less restrictive the shorter the period under consideration. Hence, we find it reassuring that our main result obtains also as we consider monthly updates of *Oxford Economics*' forecasts.

[28]It is not a genuine test because it rests on an identification assumption which is itself untested: that the common factor is contemporaneously unaffected by the country-specific developments.

Specifically, we construct a monthly measure of fiscal news for the current quarter: the change of the forecast for the current quarter in a given month compared to the last month when a forecast for this quarter was made.[29] Column [1] of Table 4 shows the results. The structure of the table mimics Table 2, except that we use the baseline measure for the forecast error by *Oxford Economics* throughout. The results for monthly news are qualitatively similar to our baseline for quarterly news. Quantitatively, the effects are smaller, but still significant.

Next, we turn to further robustness tests. In column [2] of Table 4 we report results for a specification where, instead of the default premium, we consider the difference between a country's default premium and the country-group common factor as the outcome variable. In this case, just like with a time fixed effect, the common factor captures changes in global risk valuation, but since each country has a different loading, we are able to capture different "country betas". Also in this case we find results are fairly similar to the baseline.

Our main result is also robust to further variations of our econometric specification, as we illustrate in columns [3]-[8] of Table 4. Here we report the results for six alternative specifications. First, given the evidence of autocorrelation in the government spending shocks, we include four lags of the forecast error. Second, we include four lags of government spending growth, GDP growth, and the default premium as additional regressors. Third, in light of evidence on the determinants of spread movements provided by Juvenal and Wiseman (2015), instead of time-fixed effects we include the following global factors as contemporaneous regressors: a Euro high yield index, the ECB shadow rate, the Fed Funds shadow rate, and the VIX. We also include a global commodities index. Fourth, we include four lags of the fiscal-deficit-to-GDP ratio and the trade-balance-to-GDP ratio to control for fiscal and external sustainability.[30] Fifth, we include the PRS Group's International Country Risk Guide (ICRG) political risk index (the sum of all components in their Table 3B). Last, we consider the pooled mean group estimator to account for the fact that our panel of countries is quite heterogeneous (Pesaran and Smith, 1995). Again, our main result obtains for all specifications.

Recall that we measure initial conditions on the basis of the empirical CDF of the default premium. Our results discussed above are based on distinct CDFs for emerging and advanced economies ("Country-group specific CDF") and the end-of-quarter observation of the premium. Table A.3 in the online appendix shows results for six different specifications for the indicator function, based on the average (rather than the end-of-quarter) premium, a joint empirical

[29] Thus, for the first month $m$ in a quarter $q$, the news refer to the revision in the forecast for quarter $q$, made in month $m-1$, which belonged to quarter $q-1$. For the other two months in the quarter $q$, the fiscal news refer to the revision in the nowcast for quarter $q$ made in the previous month belonging to the same quarter.

[30] Details on the data used can be found in Online Appendix A.4.1.

CDF for all countries, and a parametric specification of the indicator function. We focus on our baseline measure of the forecast error by *Oxford Economics* and find that our results are robust: in all instances a cut of government consumption lowers the default premium in benign times, but raises it significantly if fiscal stress is high.[31]

In the online appendix, we also explore more systematically to what extent our results also obtain for specific country groups—even though we stress that the mean group estimator allows for slope heterogeneity across countries and delivers results that are similar to the baseline (see column [8] in Table 4). By and large, the results in Table A.4 are similar to the baseline.

As a matter of fact, countries issue government debt with different maturities and, hence, correspondingly there is a "term structure of spreads". In our baseline, because of data limitations, we consider only *one* default premium for each country and investigate how it responds to fiscal shocks. For a limited number of time-country observations we are able to collect spreads for CDS of different maturities. The resulting sample starts after 2004 and is therefore dominated by the financial and euro area sovereign debt crises. With this limited sample at hand, we reestimate our baseline model for CDS spreads of different maturities.[32] Specifically, we report in Table 5 the impact response of a government spending shock in our baseline model on 1-year to 10-year CDS spreads.[33] Here we focus on a sample where we also have a bond-based spread measure available, the response of which is reported in the right-most column for the sake of comparison. We find that the response of the 10-year CDS spread is very similar to the bond-based measure. This is reassuring for the latter is dominated by bonds with a maturity of close to 10 years (see Section 3 above). More interestingly, we find that the short end of the spread curve responds more strongly to the shock. This implies that fiscal shocks alter the outlook in terms of default for the short run more strongly than for the long run.[34]

[31] Interestingly, we find that the response of the default premium is similarly sensitive to the state of the business cycle. If we condition the effect of spending cuts on booms and recessions as in Auerbach and Gorodnichenko (2012), we find that the premium rises during recessions and tends to decline during booms. Note that the recession indicator and the indicator for fiscal stress show quite distinct dynamics, see Figures A.3 – A.5 in the online appendix.

[32] As a practical matter, we discard CDS spread observations that are bigger than 40%. This threshold exceeds the largest value of the bond-based spread measure by 15 percentage points. Observations above this threshold are sometimes observed for periods of extreme stress during which price movements may be somewhat erratic.

[33] In Online Appendix A.4.2 we also discuss a related issue on the distinction between an increase of default risk (that is, the quantity of risk) or an increase of the "risk premium" (that is, the price of risk).

[34] Our findings regarding a country-specific government consumption shock are consistent with Augustin (2018) who reports that inverted CDS spread curves coincide with periods where country-specific factors as opposed to global factors drive the CDS spreads.

**Table 5:** Instantaneous response of default premium (basis points) to fiscal shock: CDS with different maturities vs. bond-based

| | CDS (columns represent maturities in years) | | | | | | | Spreads |
|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 7 | 10 | (bond-based) |
| Unconditional projection | | | | | | | | |
| All | 12.32** | 11.77** | 10.72** | 9.77** | 8.81* | 7.95* | 7.49* | 5.79 |
| | (6.13) | (5.90) | (5.50) | (5.05) | (4.68) | (4.21) | (3.91) | (3.60) |
| Cuts only | 20.44** | 20.18** | 18.83** | 17.36** | 15.74** | 15.37** | 14.16** | 13.37** |
| | (10.12) | (10.04) | (9.33) | (8.43) | (7.83) | (7.01) | (6.12) | (5.74) |
| Maximum stress | | | | | | | | |
| All | 49.99** | 49.97** | 43.47** | 39.33** | 35.41** | 31.86** | 29.01** | 23.22** |
| | (21.36) | (20.15) | (18.26) | (16.41) | (15.02) | (13.47) | (12.13) | (11.78) |
| Cuts only | 79.74*** | 78.94*** | 73.76*** | 67.89*** | 62.23*** | 58.66*** | 53.67*** | 50.87*** |
| | (25.51) | (25.05) | (22.98) | (20.51) | (18.95) | (16.54) | (14.63) | (12.66) |
| No fiscal stress | | | | | | | | |
| All | −21.42** | −20.66** | −18.63** | −16.71** | −15.01** | −13.46** | −11.80** | −9.91* |
| | (10.60) | (10.17) | (9.20) | (8.25) | (7.48) | (6.79) | (6.05) | (5.86) |
| Cuts only | −43.67*** | −43.57*** | −41.00*** | −37.80*** | −35.13** | −32.17*** | −29.26*** | −28.42*** |
| | (16.83) | (17.23) | (16.43) | (15.02) | (14.03) | (12.75) | (11.68) | (11.24) |
| Diff. max stress–no stress | | | | | | | | |
| All | 71.41** | 68.63** | 62.09** | 56.04** | 50.42** | 45.33** | 40.81** | 33.13* |
| Cuts only | 123.41*** | 122.51*** | 114.75*** | 105.69*** | 97.36*** | 90.83** | 82.92** | 79.30*** |
| Countries | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 |
| Observations | 676 | 676 | 676 | 676 | 676 | 676 | 676 | 676 |

*Notes:* Response to unanticipated reduction of government spending growth by 1 percentage point (annualized). Estimates based on projection (2.2), with $h = 0$ and using *Oxford Economics* forecasts. Driscoll and Kraay (1998)-standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively. The sample has been restricted to the country-time observations where CDS at all 7 maturities as well as a bond-based spread measure are available. The reported number of observations is for the unconditional model; for the conditional model, we lose an additional 2 observations where we cannot construct the stress indicator.

## 4.3 Adjustment dynamics

So far, we have focused on the impact response of the default premium to fiscal shocks. In order to shed some light on the adjustment dynamics, we estimate the local projection (2.2) also for $h = 1, \ldots, 8$. This means that we are considering the adjustment dynamics over a two-year horizon as we focus on the baseline forecasting model (*Oxford Economics*). In addition to the adjustment of the default premium over time, we study the response of other macroeconomic indicators to the fiscal shock.[35]

Figure 3 shows the estimated impulse response functions. The horizontal axis measures time in quarters. The vertical axis measures the deviation from the pre-shock level in percentage or basis points, depending on the variable. As before we distinguish initial conditions in terms of fiscal stress: the dashed line represents the response in times of

[35] Our sample is somewhat restricted relative to Section 4.1 because data for government debt is available only for a subset of countries.

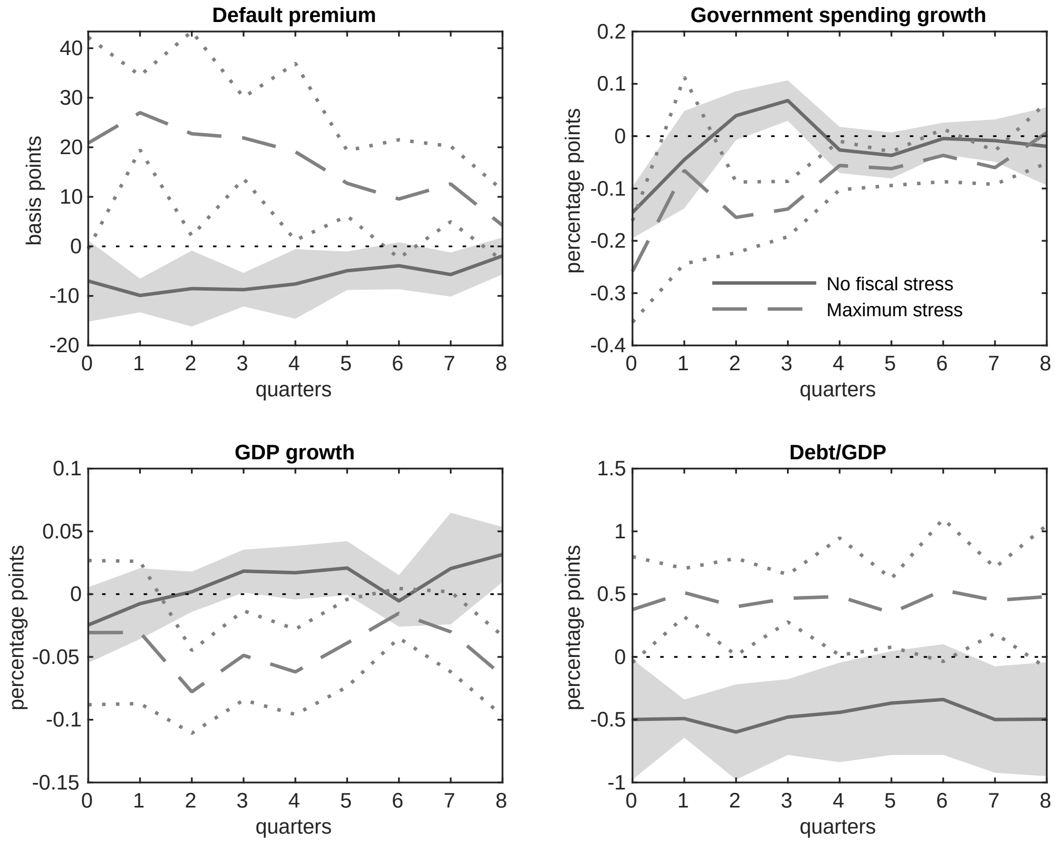


**Figure 3:** Dynamic adjustment to fiscal shock (reduction of government consumption by 1 percentage point annualized) in times of no fiscal stress (solid line) and in times of maximum stress (dashed line). *Notes:* horizontal axis measures quarters, vertical axis measures deviation from pre-shock path in percentages and basis points (default premium). Shaded areas and dotted lines indicate 90 percent confidence bounds. Results based on *Oxford Economics* forecast sample where debt data is available.

maximum fiscal stress (with dotted lines indicating 90 percent confidence bounds based on Driscoll and Kraay (1998)-standard errors), the solid line represents the response without stress (with shaded bands indicating 90 percent confidence bounds).

The upper-left panel shows the response of the default premium. For regime $m$, we observe that after the initial increase, the premium remains elevated. It declines only very gradually. Even towards the end of the two-year horizon under consideration, it is significantly above the pre-shock level. For regime $n$ we see that the decline of the premium is fairly persistent as well, but it is no longer significant at the end of the two-year horizon.

A natural question is how the default premium adjusts to the fiscal shock in the long run. However, local projections are not particularly well-suited to address this question because it is costly (in terms of degrees of freedom) to expand the horizon for which impulse responses are estimated. In an earlier version of this paper we estimated the effect of fiscal shocks on the default premium on the basis of a VAR model. The results for the short-run are similar to those obtained on the basis of local projections. In particular, in the VAR we also obtain an *immediate increase* of the premium in response to a reduction of government consumption provided fiscal stress is pervasive. And yet, *for the long-run the VAR predicts a significant decline* of the default premium for regime $m$ (Born, Müller, and Pfeifer, 2015).

The upper right panel of Figure 3 shows the response of real-time government consumption growth. It slows on impact and continues to be reduced relative to the pre-shock path for an extended period. The temporary decline of government consumption growth implies a reduction of the level of government consumption during the 2 year horizon under consideration. The dynamics are fairly similar across the two regimes, even though the growth decline is somewhat stronger in the second and third quarter in regime $m$.

In order to understand the differential response of the premium across regimes we thus turn to other macroeconomic variables. The sovereign default premium compensates investors for the probability that governments may repudiate part of their debt obligations. Recent models of sovereign default have highlighted the importance of two variables for a government's default decisions: output and the existing stock of debt. For this reason, we consider the impulse responses of both variables in the bottom panels of Figure 3.

The lower-left panel displays the response of real-time output growth. Here, the dynamics are markedly different across regimes. Initially, growth slows in both regimes. However, if fiscal stress is absent, output growth rebounds quickly. After about one year the initial effect on the output level is essentially undone. During times of maximum fiscal stress the dynamics are fundamentally different: output growth remains subdued for the entire horizon under consideration. Assuming an average government-consumption-to-GDP ratio of 20 percent, our estimates imply a cumulative fiscal multiplier on output for regime $m$ of about 1 after one quarter and of about 2 after 4 quarters. These values are perhaps high, but not unheard of (House et al., 2017; Ramey, 2011a).

In the lower-right panel we show how the debt-to-GDP ratio evolves over time in response to a cut of government consumption. Here, we again observe stark differences across regimes. In the absence of stress, the reduction of government consumption entails a sharp and lasting decline of the debt ratio. Instead, if fiscal stress is maximal the debt ratio rises in response to the shock. Put differently, we find evidence consistent with the view that austerity may be self-defeating *at times*. However, while Krugman (2010) outlines such a scenario for a liquidity trap, we provide evidence that the initial conditions in terms of fiscal stress are crucial in this regard. In a related recent study, Auerbach and Gorodnichenko (2017) find that fiscal stimulus in a slump can actually improve fiscal sustainability.

## 4.4 Discussion

A complete model-based analysis of our empirical results is beyond the scope of the present paper.[36] Here we offer a brief discussion in light of available theoretical models. Our focus is on the behavior of the sovereign default premium. According to fundamental no-arbitrage considerations, it compensates investors for the probability of (partial) default. Models in the tradition of Eaton and Gersovitz (1981) predict that, all else equal, sovereign default is more likely the higher the level of public debt and the lower the level of output (e.g., Arellano, 2008; Hatchondo, Martinez, and Sapriza, 2010). Hence, our finding of how the default premium on the one hand and the debt ratio and output growth on the other hand co-move in response to spending cuts aligns well with theory—both in the presence and in the absence of fiscal stress (see Figure 3).

More specifically, a few recent studies explicitly consider exogenous variation of either taxes or government consumption in models of sovereign default. Arellano and Bai (2017) calibrate a model of optimal default to match key features of the Greek economy and study an exogenous tax increase during a debt crisis. Their main result is particularly relevant in light of our findings above: austerity programs that increase distortionary taxes in times of fiscal stress can actually be self-defeating. They amplify the recession because higher taxes reduce the incentive to work. The default premium goes up as the recession deepens—reflecting stronger incentives for the government to default.

More closely related still is work by Bianchi et al. (2018). They study changes of government consumption and sovereign default in a two-sector small open economy that operates inside a currency union in the presence of downward nominal wage rigidity. There is an exogenous endowment of tradeable goods, while non-traded goods are produced using labor. The government consumes non-traded goods, which directly impacts production due to the nominal rigidity. Hence, an increase of government spending above the optimal level lowers unemployment. At the same time, it raises the default premium because spending is debt-financed. Conversely, a cut of government consumption lowers the default premium—in line with our findings in the absence of fiscal stress (see panel 3 of Table 2).

However, Bianchi et al. (2018) assume that taxes are lump-sum. This rules out adverse budgetary effects from reduced production. Such an adverse budgetary effect is at the heart of the analysis by Corsetti, Kuester, et al. (2013). In their model, sovereign default is not an optimal decision of the government, but is likely to take place as public debt gets close to the "fiscal limit", that is, the maximum level of debt that the government is able to

[36] In the working paper version of the paper we rationalize our findings on the basis of model simulations (Born, Müller, and Pfeifer, 2015). For this purpose we consider a variant of the model put forward by Arellano (2008).

service (Bi, 2012). Importantly, tax revenues move in proportion with output and government consumption impacts output directly because of nominal rigidities. In this environment a large fiscal multiplier obtains if monetary policy is constrained by the zero lower bound. A contractionary cut in government consumption then causes a decline of tax revenues that more than offsets the initial effect of reduced expenditures on the government budget. As a result, the default premium increases in response to a cut of government spending—in line with our findings for fiscal stress (see panel 2 of Table 2).[37]

Last, we briefly refer to work on the *optimal* adjustment of government consumption in the context of sovereign default (Cuadra et al., 2010; Hatchondo, Martinez, and Roch, 2017). Because in our empirical analysis we identify *exogenous* variations of government consumption, we are silent on the optimality of such measures. In fact, a one-time cut of government consumption is unlikely to be the optimal way to implement an austerity program. In this context, results by Hatchondo, Martinez, and Roch (2017) are remarkable, as they illustrate the benefits of credible medium-term consolidation strategies. Specifically, they study fiscal rules which impose a ceiling on the government budget balance when either debt or the default premium is high. It turns out that such fiscal rules allow governments to forego sharp fiscal adjustments because they provide an ex-ante fiscal anchor that mitigates concerns about the sustainability of debt.

In sum, available theoretical models provide important insights into the dynamics of the sovereign default premium, both unconditionally and conditional on fiscal shocks. They also allow us to rationalize our empirical results to a considerable degree. However, to the best of our knowledge, a comprehensive investigation of how initial conditions in terms of fiscal stress impact the fiscal transmission mechanism and, eventually, the response of the default premium to austerity measures is still lacking. Our empirical results may provide a useful reference point for such an investigation.

# 5 Conclusion

In this paper we make two distinct contributions. First, we set up a new data set. It comprises observations for 38 emerging and advanced economies since 1991, notably for the sovereign default premium and three alternative measures of fiscal shocks. We provide a detailed description of the data and establish a number of basic facts. Second, we assess how the default premium responds to a cut of government consumption. In doing so we account for

[37] Corsetti, Kuester, et al. (2013) also consider a "sovereign risk channel" whereby a higher sovereign default premium raises borrowing costs in the private sector. If this channel is strong, spending cuts tend to reduce the default premium.

initial conditions in terms of fiscal stress. We find that the premium rises in response to a cut of government consumption if stress is very severe, but declines if stress is low. This result is robust across a variety of alternative specifications and shock measures. It also holds for specific country groups in our sample.

Our results have important implications for policy. If confronted with a situation of severe fiscal stress, a reduction of government consumption is not rewarded by financial markets. In fact, financial markets may appear "schizophrenic" about austerity in that they demand austerity measures as public debt builds up, but fail to reward them as austerity slows down output growth (Blanchard, 2011; Cotarelli and Jaramillo, 2013). Under these circumstances, a commitment to a credible medium-term strategy might be more promising, along the lines of our discussion in Section 4.4 above. Moreover, to the extent that fiscal stress builds up gradually over time, our results suggest that austerity can actually pay off if implemented in a sufficiently timely manner. A main takeaway of our analysis is thus that delaying fiscal consolidations can be particularly harmful.

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# A Appendix

## A.1 Details on the construction of the default premium

In this Section, we provide additional information on the construction of default premiums and data sources.

### A.1.1 Sources and strategies

We obtain our default-risk measure based on four distinct sources/strategies. First, for a subset of (formerly) emerging markets we directly rely on J.P. Morgan's EMBI spreads, which measure the difference in yields between dollar-denominated government or government-guaranteed bonds of a particular country on the one hand and U.S. government bonds on the other hand.[38]

Second, we add to these observations data for euro area countries based on the "long-term interest rate for convergence purposes". They are computed as yields to maturity from "long-term government bonds or comparable securities" with a residual maturity of close to 10 years with sufficient liquidity (for details, see European Central Bank, 2004). For this country group, we use the German government bond yield as the risk-free benchmark rate and compute spreads relative to the German rate.[39]

Third, we make use of the issuance of foreign-currency government bonds in many advanced economies during the 1990s and 2000s in order to extend our sample to non-euro area countries and the pre-euro period. In case of countries like Denmark, Sweden, or the UK, this allows us to compute common-currency yield spreads, even though those countries are not members of the euro area. Drawing on earlier work by Bernoth et al. (2012), we identify bonds denominated in either U.S. dollar or Deutsche Mark of at least 5 years of maturity issued by developed economies. We compute the yield spread for those bonds relative to the yields of U.S. or German government bonds of comparable maturity and coupon yield.[40] Whenever possible, we aim to minimize the difference in coupon yield to 25 basis points

[38]See Section A.1.2 below for details on the EMBI.

[39]The bonds used for computing the "long-term interest rate for convergence purposes" are typically bonds issued in euro, but under national law. In this regard they differ from the securities on which the EMBI is based, which are typically issued under international law. This difference becomes important if the monetary union is believed to be reversible. In case of exit from the EMU, the euro bonds will most likely be converted into domestic-currency bonds, implying that they should carry a redenomination premium that is absent in case of international-law bonds. Still, even during the height of the European debt crisis, the redenomination premium accounted for a moderate fraction of sovereign yield spreads (Krishnamurthy et al., 2018; Kriwoluzky et al., 2015). Moreover, Bayer et al. (2018) find that redenomination risk, tends to show up at the short end of the yield curve. In any case, our main results also hold for a sample of emerging market countries.

[40]Yields on individual bonds are based on the yield to maturity at the midpoint as reported in Bloomberg or the yield to redemption in Datastream.

and the difference in maturity to one year. We omit the last 30 trading days before the earliest maturity date of either the benchmark or the government bond. As a result, our data should be free of artifacts due to trading drying up in the last days before redemption.[41] In case that several bonds are available for overlapping periods, we average over yield spreads using the geometric mean. This procedure mimics the creation of the EMBI spreads and the "long-term interest rate for convergence purposes". However, we necessarily rely on a smaller universe of foreign-currency bonds and cannot correct for maturity drift. Thus, we rely on the "long-term interest rate for convergence purposes" whenever they are available.[42]

Finally, in the more recent part of the sample, a direct measure of default risk has become available in the form of CDS spreads. Credit default swaps are insurance contracts that cover the repayment risk of an underlying bond. The CDS spread indicates the annual insurance premium to be paid by the buyer.[43] Accordingly, a higher perceived default probability on the underlying bond implies, ceteris paribus, a higher CDS spread. While well-suited to capture the market assessment of debt sustainability, CDS spread data are generally only available after 2003 (see Mengle, 2007). Unfortunately, trading in these markets was often thin before the financial crisis, price discovery often took place in bond markets, and CDS contracts are subject to counterparty risk (see Fontana and Scheicher, 2016). At the same time, CDS spreads have the advantage that they should not be affected by the flight to safety that may impact the bond spread relative to safe-haven countries (see e.g. Jiang et al., 2018) and that their pricing is typically less distorted by high margin costs (see e.g. Gârleanu and Pedersen, 2011). As there is no clear theoretical reason to prefer one or the other measure, we use bond spreads as they are consistently available for a longer period of time and use CDS spreads to measure default risk only when no spread-based default premium measure is available.[44]

### A.1.2 EMBI spreads

The J.P. Morgan EMBI is an emerging market debt benchmark that includes "U.S.-dollar-denominated Brady bonds, Eurobonds, traded loans, and local market debt instruments issued by sovereign and quasi-sovereign entities" (JP Morgan, 1999). For our purposes, it

[41]Still, in moving along the yield curve, we may pick up cross-country differences in the slope of the yield curve. In principle, this effect can be quantitatively significant (Broner et al., 2013). However, as we find our spread measure to co-move very strongly with CDS spreads (whenever they are available), we ignore the issue in the present paper.

[42]Because we focus on common-currency bonds, our spread measure is not affected by the convergence play observed for nominal yield spreads prior to the introduction of the euro.

[43]A no-arbitrage argument implies that the CDS spread should equal the spread between a par floating rate bond and the risk-free rate (Duffie, 1999).

[44]The CDS data construction is described in Section A.1.3 below. The correlation between CDS spreads and the yield-based default premium measures, when both are available, is typically above 0.9.

is important to note that debt instruments must have at least 2.5 years of maturity left for inclusion and remain in the index until 12 months before maturity. This implies that the maturity of the EMBI does not necessarily stay constant over time as the maturity of the underlying debt portfolio may change. The EMBI spread "corresponds to the weighted average of these securities' yield difference to the US Treasury securities with similar maturity, considered risk free. This risk premium is called in the market as the spread over Treasury of this portfolio" (Banco Central do Brasil, 2014). Inclusion of a bond into the EMBI requires a minimum bond issue size of $500 million. This ensures that the liquidity premium compared to U.S. bonds is not too large.[45]

The data is retrieved from Datastream. The mnemonic is JPMG followed by a three letter country identifier. We rely on stripped spreads (Datastream Mnemonic: SSPRD), which "strip" out collateral and guarantees from the calculation. For example, JPMGARG(SSPRD) is the mnemonic for the Argentinean EMBI spread.

### A.1.3 CDS spreads data

CDS spreads are from Datastream and spliced from two sources. Until 2010Q3, Datastream provides CDS spreads from Credit Market Analysis Limited (CMA), while Thomson Reuters, starting in 2008 provides CDS for an increasing number of issuers.[46] The contract type we choose is five years of maturity with complete restructuring (CR). The CMA CDS spreads are typically denominated in dollar, while the Thomson Reuters CDS spreads are often available in euro and dollar. Despite CDS spreads being theoretically unit free as they are measured in basis points, the choice of denomination currency choice can be relevant for sovereign entities. The reason is that, e.g., being reimbursed in U.S. dollar when Germany defaults may provide an insurance against exchange rate risk. (for more on this and CDS contracts in general, see, e.g., Buchholz and Tonzer, 2016; Fontana and Scheicher, 2016). To exclude an exchange rate risk premium, we use Thomson Reuters CDS spreads in U.S. dollar for all non-EMU countries and Thomson Reuters Euro CDS spreads in euro for euro area members after EMU accession. Unfortunately, for early time periods, the currency-specific Thomson Reuters CDS spreads are not always available. In this case, we rely on the CMA CDS spreads.

[45] For more information on the EMBI see JP Morgan (1999). Banco Central do Brasil (2014) provides a very accessible general introduction to the EMBI.

[46] Additional information on the distinction and the how to match the two series can be found at `http://extranet.datastream.com/data/CDS/`.

### A.1.4 Spread decomposition

In the main text, we use the difference between nominal yields on foreign-currency bonds and a risk-free reference bond to measure the default premium. We elaborate on this in the following.

For most practical purposes, the nominal yield to maturity of a bond, $r_t^{nom}$ can be decomposed as

$$r_t^{nom} = r_t^{real,riskfree} + E_t\left(\pi_{t+1}\right) + RP_t^{Infl} + E_t\left(\delta_{t+1}\right) + RP_t^{default} + RP_t^{term} + RP_t^{liqu} + \varepsilon_t\,, \quad \text{(A.1)}$$

where $r_t^{real,riskfree}$ is the real risk-free interest rate, $E_t\left(\pi_{t+1}\right)$ is the compensation for expected inflation, $RP_t^{Infl}$ denotes the premium for inflation risk, and $RP_t^{term}$ the term premium.[47] We are mostly interested in the next two components that we subsume under the heading "default premium": the compensation for expected default $E_t\left(\delta_{t+1}\right)$ and the default risk premium $RP_t^{default}$. The term $RP_t^{liqu}$ captures liquidity risk premia, while $\varepsilon_t$ captures other (higher order) terms. In order to isolate the terms of interest to us, we compute the yield spread between foreign-currency bonds and a default-risk free reference bond/bond index of a similar maturity. Under integrated financial markets, its yield, $r_t^{*,nom}$ , will be given by

$$r_t^{*,nom} = r_t^{real,riskfree} + E_t\left(\pi_{t+1}\right) + RP_t^{Infl} + RP_t^{term} + RP_t^{*,liqu} + \varepsilon_t^*\,. \quad \text{(A.2)}$$

The default-related terms are zero. The real risk-free interest rate, the inflation premium, and the term premium should be the same as in Equation (A.1), as we consider a bond denominated in the same currency and with the same maturity.[48] A yield spread computed this way will thus only contain the default-related premium and the difference of the liquidity risk premium as well as higher order terms. Unfortunately, it is not easy to isolate the difference in liquidity premia. However, we are quite confident that liquidity is not driving our results for three reasons. First, markets for government bonds are typically quite liquid so that any liquidity premium should be small. Second, the risk premium consist of the price of risk times the quantity of risk. With integrated financial markets, the price of risk tends to be a common factor that will be accounted for by our time-fixed effects, leaving only the quantity component of liquidity risk as a confounding factor (see also the discussion in Section A.1.5.). Finally, we find that our main results also obtain for a sample of developed economies where markets are very liquid. Results also hold up if we drop observations after the beginning of the recent financial crisis—a period when liquidity dried up considerably.

[47]This is a second order effect arising from the covariance of returns with the stochastic discount factor. It is absent if all investors are risk neutral.

[48]Regarding the term premium, it is actually the duration of expected cash flows that matters. This might introduce small differences of the term premium (see Broner et al., 2013).

### A.1.5 Price of risk and quantity of risk

Our measure of the default premium reflects the quantity of risk times the price of risk. The price of risk may be time-varying with global risk aversion (see, e.g., Bekaert et al., 2013). However, this should not be a problem in our setup as the price of risk-component should be global and is thus captured by time-fixed effects. This is equivalent to including the VIX as a control. However, our fiscal stress indicator is also based on default premia and thus depends on the price of risk as well. Thus, while the cross-section of our fiscal stress indicator is unaffected by the price of risk, the time series dimension may be affected as the price of risk will be simultaneously high for all countries at a particular point in time. However, results are robust to dropping the Great Recession period from our sample—a period when price of risk spiked.

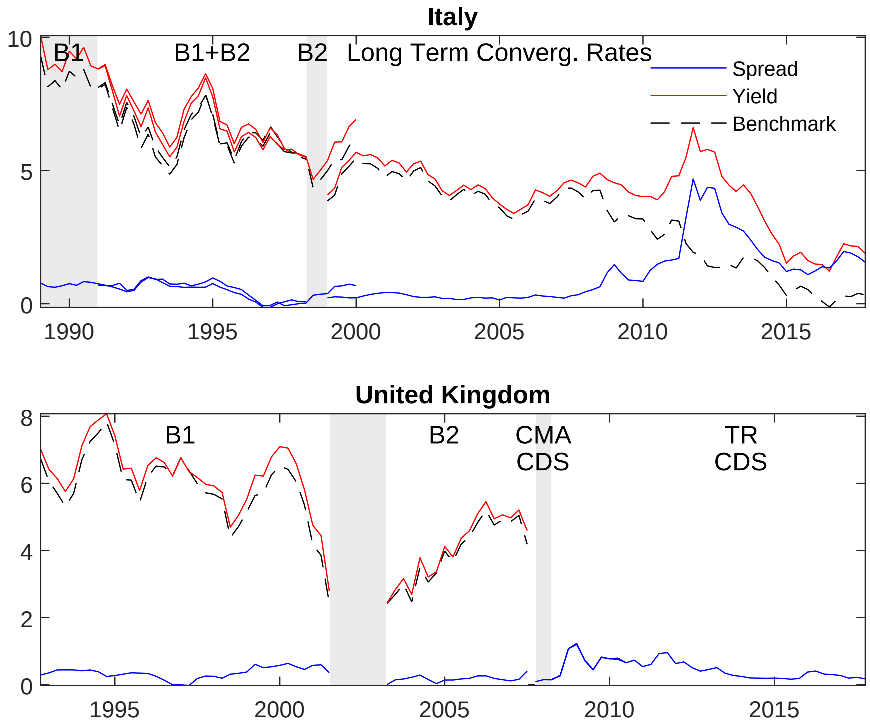


**Figure A.1:** Construction of sovereign yield spread: Italy and United Kingdom.

### A.1.6 Construction of data set: example

To illustrate the construction of our data set, Figure A.1 provides two examples, namely data for Italy (top) and the United Kingdom (bottom). Until 1991 only one Italian foreign currency-bond is available. Starting in 1992, we obtain a second bond and compute the yield spread as the average over those bonds. When the first bond matures in 1997, we are left with one bond until 1999. From that point on, we use the long-term convergence bond yields provided by the ECB. For the United Kingdom, we have two different bonds available to cover the early part of the sample, with missing values in between. From 2007 on, we rely on CMA CDS spreads, while in 2008 the Thomson Reuters CDS spreads become available, which are used for the rest of the sample.[49]

## A.2 Government spending data

### A.2.1 Government data for early 1990s

Government consumption data in the early 1990s used to be consistently available through Eurostat. But with the recent move to the European System of National Accounts 2010, the data often does not reach that far back. Thus, for Austria, Denmark, Italy, and Belgium we

[49]For details, see Appendix Section A.1.3.

splice the recent ESA2010 data from Eurostat with the earlier series available in the OECD's Quarterly National Accounts available through their bulk download facility (affix S2 for all countries, except Belgium, where it is S3). However, this data is not entirely based on direct sources, implying that the data falls short of the more recent Eurostat standards, firmly established since the mid-2000s only.

### A.2.2 Anecdotal evidence

Discretionary changes of government spending are arguably subject to decision lags that prevent policymakers from responding instantaneously to contemporaneous developments in the economy. Anecdotal evidence suggests that this holds true also in times of fiscal stress. For instance, in November 2009, European Commission (2009) stated regarding Greece: "in its recommendations of 27 April 2009 . . . the Council [of the European Union] did not consider the measures already announced by that time, to be sufficient to achieve the 2009 deficit target and recommended to the Greek authorities to "strengthen the fiscal adjustment in 2009 through permanent measures, mainly on the expenditure side". In response to these recommendations the Greek government announced, on 25 June 2009, an additional set of fiscal measures to be implemented in 2009 . . . . However, these measures . . . have not been implemented by the Greek authorities so far." In fact, it appears that significant measures were put in place not before 2010Q1, see Greek Ministry of Finance (2010).

Still, we cannot rule out that policy measures—while debated for some time—are sometimes spurred by contemporaneous financial-market developments. Consider the case of Italy: after some fiscal consolidation in 2010, the default premium kept on rising during the first quarter of 2011 and additional measures were approved by the cabinet on June 30. Finance minister Tremonti, in particular, pushed for severe austerity measures in order to "dispel any spectre of a Greek collapse in Italy" (The Economist, 2011). On July 8 prime minister Berlusconi stated that his finance minister "thinks he's a genius and everyone else is stupid", suggesting some modification to the austerity package. Arguably in response to these remarks, yields on Italian debt rose strongly, such that the package was approved in the Senate without much debate on July 14 (Time Magazine, 2011).

**Table A.1:** Basic properties of sovereign default premiums

| Country | Group | min | max | mean | std | $\rho(\Delta y_t, r_t)$ | $\rho(\Delta g_t, r_t)$ |
|---|---|---|---|---|---|---|---|
| Argentina | E | 2.13 | 19.50 | 7.55 | 3.55 | -0.57 | -0.11 |
| Australia | D | 0.03 | 1.30 | 0.31 | 0.31 | -0.32 | -0.38 |
| Austria | D | 0.02 | 1.75 | 0.39 | 0.37 | -0.44 | -0.13 |
| Belgium | D | 0.03 | 2.81 | 0.57 | 0.50 | -0.32 | -0.05 |
| Brazil | E | 1.64 | 24.22 | 5.40 | 3.92 | -0.11 | -0.03 |
| Bulgaria | E | 0.75 | 21.63 | 4.50 | 4.51 | -0.26 | -0.05 |
| Chile | E | 0.64 | 4.04 | 1.65 | 0.60 | -0.46 | -0.04 |
| Colombia | E | 1.28 | 10.75 | 3.51 | 2.03 | -0.31 | -0.15 |
| Croatia | E | 0.15 | 5.47 | 2.11 | 1.41 | -0.53 | -0.23 |
| Czech Republic | D | 0.05 | 2.08 | 0.57 | 0.47 | -0.80 | -0.16 |
| Denmark | D | 0.02 | 2.20 | 0.51 | 0.44 | -0.20 | -0.00 |
| Ecuador | E | 3.91 | 21.22 | 9.56 | 3.93 | -0.43 | -0.38 |
| El Salvador | E | 1.36 | 9.15 | 3.97 | 1.53 | -0.57 | 0.00 |
| Finland | D | -0.01 | 1.28 | 0.39 | 0.27 | -0.48 | -0.12 |
| France | D | 0.03 | 1.92 | 0.45 | 0.41 | -0.40 | -0.01 |
| Germany | D | 0.01 | 0.73 | 0.17 | 0.16 | -0.35 | 0.02 |
| Greece | D | 0.20 | 24.49 | 3.06 | 4.18 | -0.59 | -0.27 |
| Hungary | E | 0.19 | 6.37 | 1.99 | 1.57 | -0.57 | -0.07 |
| Ireland | D | -0.03 | 9.09 | 1.15 | 1.80 | -0.18 | -0.32 |
| Italy | D | -0.01 | 5.84 | 1.06 | 1.09 | -0.40 | -0.19 |
| Latvia | D | 0.05 | 10.01 | 2.11 | 2.12 | -0.67 | -0.25 |
| Lithuania | D | -0.07 | 7.25 | 1.86 | 1.72 | -0.60 | -0.31 |
| Malaysia | E | 0.55 | 10.64 | 1.98 | 1.33 | -0.59 | -0.03 |
| Mexico | E | 1.20 | 15.98 | 3.53 | 2.41 | -0.26 | 0.00 |
| Netherlands | D | -0.01 | 1.18 | 0.33 | 0.29 | -0.56 | -0.20 |
| Peru | E | 1.26 | 9.20 | 3.26 | 1.86 | -0.20 | 0.07 |
| Poland | E | 0.51 | 8.80 | 1.88 | 1.27 | -0.18 | -0.22 |
| Portugal | D | 0.03 | 12.15 | 1.69 | 2.57 | -0.49 | -0.67 |
| Slovakia | D | 0.04 | 4.03 | 1.07 | 1.07 | -0.35 | -0.31 |
| Slovenia | D | -0.15 | 5.36 | 1.48 | 1.56 | -0.49 | -0.49 |
| South Africa | E | 0.79 | 6.61 | 2.54 | 1.18 | -0.55 | -0.29 |
| Spain | D | 0.02 | 5.92 | 0.98 | 1.20 | -0.71 | -0.79 |
| Sweden | D | 0.01 | 1.20 | 0.36 | 0.24 | -0.34 | -0.08 |
| Thailand | E | 0.27 | 5.64 | 1.31 | 0.86 | -0.39 | 0.15 |
| Turkey | E | 1.48 | 10.75 | 4.09 | 2.23 | -0.29 | -0.09 |
| United Kingdom | D | -0.12 | 1.20 | 0.39 | 0.25 | -0.33 | -0.06 |
| United States | D | 0.00 | 0.61 | 0.23 | 0.11 | -0.47 | -0.09 |
| Uruguay | E | 1.51 | 16.52 | 3.64 | 2.84 | -0.35 | -0.35 |

*Notes:* Default premium $r_t$ is end-of-quarter observation, measured in percentage points. The last two columns report the correlation of default premiums with the growth rates of real GDP, $\Delta y_t$, and government consumption, $\Delta g_t$, respectively. Following IMF (2015, Tables B to D), group entry "D" denotes developed economies, while "E" denotes emerging economies. Excludes default episodes in Argentina (2001Q4–2005Q2, 2014Q3–2016Q2), Ecuador (1999Q3–2000Q3 and 2008Q4–2009Q2), and Greece (2012Q1–2012Q2, 2012Q4) as classified by Standard & Poor's (see Witte et al., 2018, Table 13).

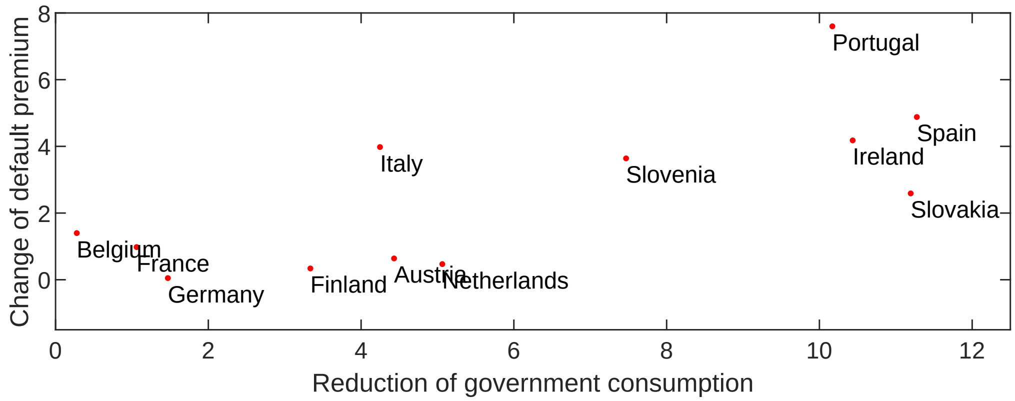


**Figure A.2:** Sovereign default premium and austerity in selected euro area economies: 2010Q1–2012Q2. Notes: vertical axis measures percentage change of default premium, horizontal axis measures reduction of real government consumption in percent, see Section 3 for a detailed data description. Greece, which defaulted in 2012Q1, is excluded.

## A.3 Additional figures and tables

## A.4 Robustness

### A.4.1 Data

The two interest rate measures combine the ECB policy rate (ECB Statistical Data Warehouse: BBK01.SU0202) and the effective FFR (FRED:FEDFUNDS) with the respective Wu and Xia (2016) shadow rates. The high yield index uses the ICE Benchmark Administration Limited (IBA) BofAML Euro High Yield Index Effective Yield index (FRED: BAMLHE00EHYIEY). The VIX is the Chicago Board Options Exchange (CBOE) Volatility Index (FRED: VIXCLS), while the commodity price index uses the International Monetary Fund Global Price Index of All Commodities (FRED: PALLFNFINDEXQ).

The deficit-to-GDP ratio combines quarterly data on general government net lending/borrowing from Eurostat with annual data from the IMF World Economic Outlook. The latter is linearly interpolated for non-Eurostat time periods and countries to achieve the largest available coverage.

**Table A.2:** Sample range for different forecasting models

| | Oxford Economics | | OECD | | VAR | |
|---|---|---|---|---|---|---|
| Country | Range | T | Range | T | Range | T |
| Argentina | 1999Q3-17Q4 | 51 | - | - | 1993Q3-17Q4 | 62 |
| Australia | 2003Q1-10Q3 | 28 | 2003S1-10S2 | 15 | 2002Q4-10Q3 | 16 |
| Austria | 1997Q1-17Q4 | 80 | 1997S1-17S2 | 31 | 1993Q3-17Q4 | 93 |
| Belgium | - | - | 1997S1-17S2 | 42 | 1991Q3-17Q4 | 101 |
| Brazil | - | - | - | - | 1996Q1-17Q4 | 83 |
| Bulgaria | - | - | - | - | 2000Q1-17Q4 | 67 |
| Chile | 1999Q3-17Q4 | 72 | - | - | 1999Q1-17Q4 | 71 |
| Colombia | - | - | - | - | 2000Q1-17Q4 | 67 |
| Croatia | - | - | - | - | 2003Q4-17Q4 | 52 |
| Czech Republic | 2004Q1-17Q4 | 56 | 2004S1-17S2 | 21 | 2003Q4-17Q4 | 52 |
| Denmark | 1997Q1-17Q4 | 73 | 1997S1-15S2 | 29 | 1991Q1-17Q4 | 90 |
| Ecuador | - | - | - | - | 1994Q4-17Q4 | 72 |
| El Salvador | - | - | - | - | 2002Q1-17Q3 | 58 |
| Finland | 1999Q2-17Q4 | 73 | 1997S1-17S2 | 42 | 1992Q1-17Q4 | 99 |
| France | 1999Q1-17Q4 | 74 | 1999S1-17S2 | 38 | 1998Q4-17Q4 | 72 |
| Germany | 2004Q1-17Q4 | 56 | 2004S1-17S2 | 28 | 2003Q4-17Q4 | 52 |
| Greece | 2001Q4-17Q4 | 60 | 1997S1-17S2 | 32 | 1995Q1-17Q4 | 79 |
| Hungary | 1999Q3-17Q4 | 72 | 1999S1-17S2 | 14 | 1998Q4-17Q4 | 72 |
| Ireland | 2004Q1-17Q4 | 56 | 1997S1-17S2 | 42 | 1995Q1-17Q4 | 87 |
| Italy | 1997Q1-17Q4 | 80 | 1989S1-17S2 | 58 | 1991Q1-17Q4 | 103 |
| Latvia | - | - | - | - | 2005Q4-17Q4 | 44 |
| Lithuania | - | - | - | - | 2005Q1-17Q4 | 47 |
| Malaysia | 1999Q3-17Q4 | 72 | - | - | 2000Q1-17Q4 | 67 |
| Mexico | - | - | 1997S1-16S2 | 40 | 1993Q3-17Q4 | 93 |
| Netherlands | 1999Q1-17Q4 | 74 | 1999S1-17S2 | 38 | 1998Q4-17Q4 | 72 |
| Peru | - | - | - | - | 1996Q4-17Q4 | 75 |
| Poland | - | - | 1997S1-13S2 | 27 | 2002Q1-17Q4 | 59 |
| Portugal | 1998Q4-17Q4 | 75 | 1997S1-17S2 | 42 | 1995Q1-17Q4 | 87 |
| Slovakia | 2005Q2-17Q4 | 51 | - | - | 2003Q4-17Q4 | 52 |
| Slovenia | - | - | - | - | 2002Q4-17Q4 | 56 |
| South Africa | - | - | - | - | 1994Q3-17Q4 | 89 |
| Spain | 1997Q1-17Q4 | 80 | 1997S1-17S2 | 42 | 1995Q1-17Q4 | 87 |
| Sweden | 1998Q3-17Q4 | 69 | 1997S1-17S2 | 39 | 1993Q1-17Q4 | 78 |
| Thailand | 1999Q3-17Q4 | 72 | - | - | 1997Q1-17Q4 | 79 |
| Turkey | 2000Q1-17Q4 | 70 | 1997S1-04S2 | 16 | 1998Q1-17Q4 | 75 |
| United Kingdom | 1997Q1-17Q4 | 80 | 1992S2-17S2 | 51 | 1995Q1-17Q4 | 87 |
| United States | 2007Q4-17Q4 | 41 | 2007S2-17S2 | 21 | 2007Q3-17Q3 | 36 |
| Uruguay | - | - | - | - | 2001Q1-17Q4 | 58 |
| Total | | 1515 | | 708 | | 2689 |

*Notes:* Range refers to the first and last observation available. Note that the VAR-approach requires 5 observations to construct 4 lags of growth rates. $T$ refers to the number of observations used for the particular country after accounting for missing values and lag construction in the unconditional model.

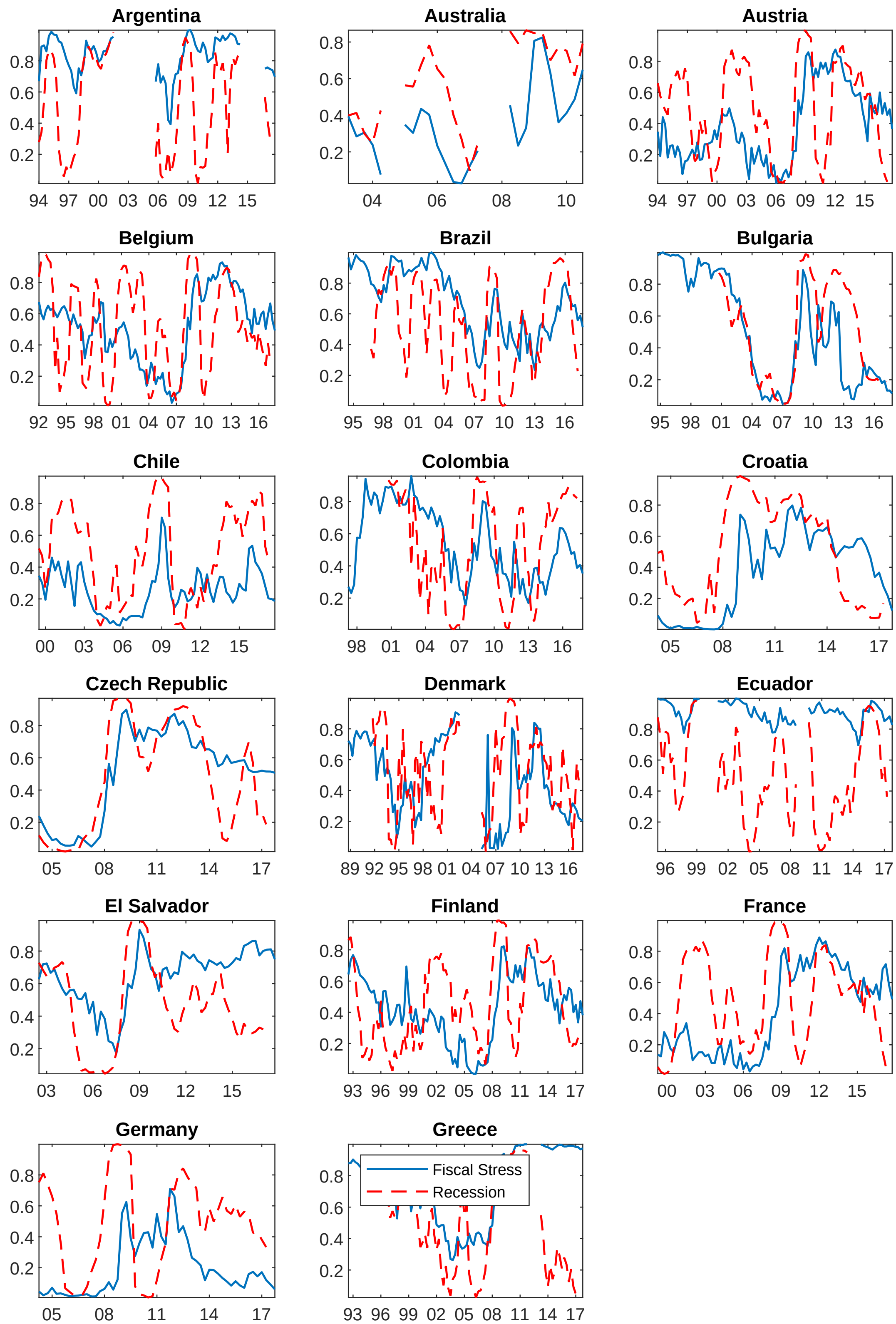


**Figure A.3:** Values of empirical CDF (Country group-specific) for lagged default premia and smoothed output gaps.

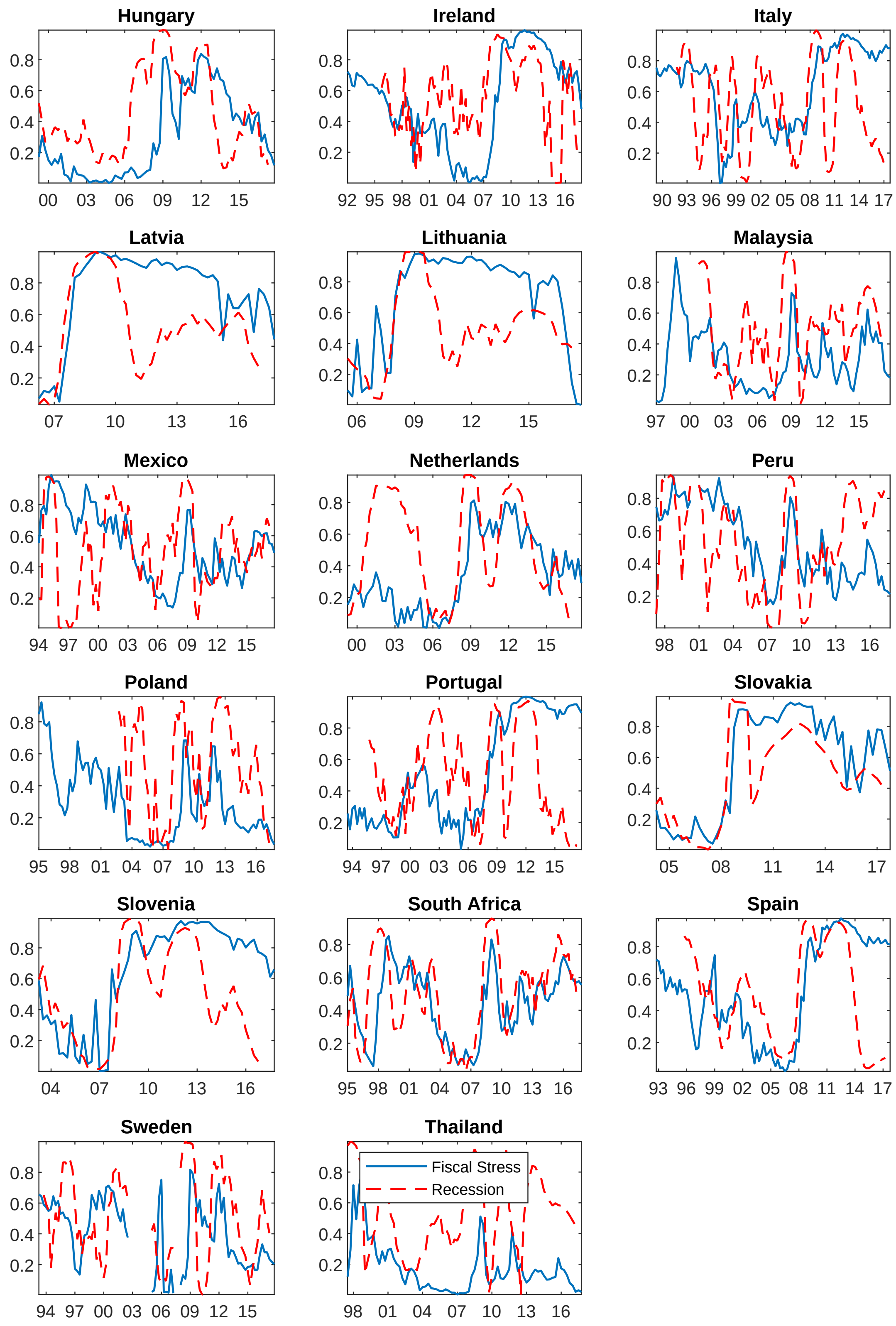


**Figure A.4:** Values of empirical CDF (Country group-specific) for lagged default premia and smoothed output gaps.

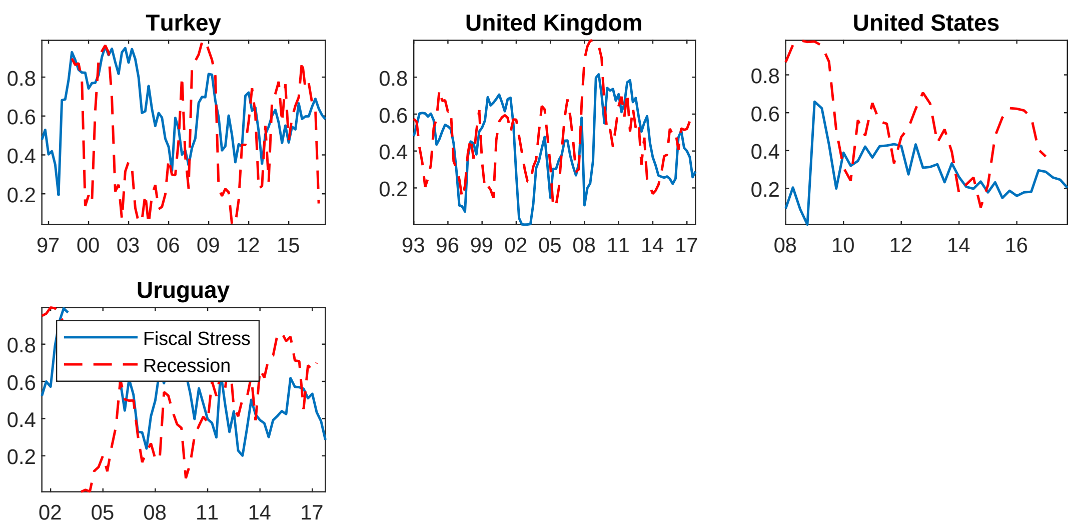


**Figure A.5:** Values of empirical CDF (Country group-specific) for lagged default premia and smoothed output gaps.

### A.4.2 Results

**Table A.3:** Instantaneous response of default premium to reduction of government spending: alternative stress indicators

| | Indicator | | | | |
|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] |
| Maximum stress | | | | | |
| All | 21.28 | 19.29* | 19.46* | 10.35* | 10.44* |
| | (13.46) | (11.80) | (11.56) | (5.72) | (5.77) |
| Cuts only | 60.95*** | 58.14*** | 57.22*** | 30.18*** | 30.05*** |
| | (18.47) | (16.84) | (15.99) | (10.80) | (10.74) |
| No fiscal stress | | | | | |
| All | −7.48 | −15.99 | −16.11 | −16.19 | −16.50 |
| | (5.47) | (11.13) | (10.66) | (10.18) | (10.23) |
| Cuts only | −34.23*** | −60.38*** | −60.49*** | −59.38*** | −60.25*** |
| | (10.71) | (18.80) | (18.00) | (19.28) | (19.09) |
| Diff. Max stress–no stress | | | | | |
| All | 28.76 | 35.28 | 35.58 | 26.53* | 26.94* |
| Cuts only | 95.18*** | 118.52*** | 117.71*** | 89.56*** | 90.30*** |
| Countries | 23 | 23 | 23 | 23 | 23 |
| Observations | 1498 | 1498 | 1498 | 1483 | 1483 |

*Notes:* Response to unanticipated reduction of government spending growth by 1 percentage point (annualized). Estimates based on projection (2.2), with $h = 0$ and using *Oxford Economics* forecasts. Standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively. 1 = country-group specific CDF with average premium over quarter, 2 = pooled empirical CDF with average premium over quarter, 3 = pooled empirical CDF with premium at the end of quarter, 4 = parametric indicator with average spread over quarter, 5 = parametric indicator with spread at the end of quarter. The parametric indicator uses a three-period backward-looking moving average of the spread, shifted by 0.5 percentage points to avoid negative values. The resulting average is transformed to have unbounded support by taking the logarithm and then transformed to range [0,1] via a logistic function with slope $\gamma = 3$.

#### Sample splits

Table A.4 shows the results for different country groups, based once more on our baseline measure for the forecast error by *Oxford Economics*. The first and second column show, respectively, the results for advanced and emerging countries in isolation. Column [3] reports results for a sample of euro-area countries only. Columns [4] and [5] are based on samples which exclude observations for the euro-area and financial-crisis countries, respectively.[50]

[50]We define the Great Recession as the time period from 2007Q2 until 2010Q4 and the Euro crisis countries as Greece, Ireland, Italy, Portugal, Slovenia, and Spain from 2009Q4 until 2014Q4.

**Table A.4:** Instantaneous response of default premium (basis points) to fiscal shock: selected country groups

| | Country group | | | | | |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Unconditional projection | | | | | | |
| All | 15.53 | 1.98** | 15.17 | 2.15** | 7.53* | 2.11** |
| | (13.02) | (0.96) | (13.98) | (1.08) | (4.50) | (0.92) |
| Cuts only | 31.49 | 4.22** | 30.28 | 4.25*** | 19.39** | 5.45*** |
| | (21.36) | (1.87) | (22.26) | (1.74) | (10.06) | (1.65) |
| Maximum stress | | | | | | |
| All | 38.45 | 6.89** | 36.40 | 6.94 | 26.34* | 6.12 |
| | (25.80) | (3.58) | (25.04) | (5.48) | (15.86) | (4.78) |
| Cuts only | 77.22*** | 21.90*** | 71.36*** | 29.86*** | 70.42*** | 24.65*** |
| | (25.31) | (7.21) | (24.74) | (8.08) | (22.19) | (6.47) |
| No fiscal stress | | | | | | |
| All | −27.73* | −0.94 | −33.36* | −1.10 | −8.40 | −0.38 |
| | (16.78) | (1.93) | (18.40) | (2.42) | (5.31) | (2.20) |
| Cuts only | −72.00*** | −9.34** | −77.19*** | −17.10*** | −39.06*** | −10.09*** |
| | (17.61) | (4.02) | (19.94) | (6.21) | (10.95) | (3.97) |
| Diff. max stress–no stress | | | | | | |
| All | 66.18* | 7.83 | 69.76* | 8.04 | 34.74* | 6.49 |
| Cuts only | 149.22*** | 31.24*** | 148.54*** | 46.96*** | 109.48*** | 34.73*** |
| Countries | 17 | 6 | 11 | 23 | 23 | 19 |
| Observations | 1106 | 409 | 727 | 1413 | 1176 | 797 |

*Notes:* Response to unanticipated reduction of government spending growth by 1 percentage point (annualized). Estimates based on projection (2.2), with $h = 0$ and using *Oxford Economics* forecasts. Driscoll and Kraay (1998)-standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively. 1 = Advanced economies, 2 = Emerging economies, 3 = Euro area countries, 4 = Excluding euro crisis countries, 5 = Excluding the financial crisis, 6 = Countries with their own legal tender.

Finally, column [6] refers to results based on a sample of countries that maintain at least some monetary policy independence: we consider only countries with their own legal tender (this excludes countries in the euro area). Some observers have argued that the sovereign default premium, notably during the recent euro-area crisis, is driven by "market sentiment" rather than "fundamentals". According to a popular narrative, the fact that euro area countries have completely surrendered monetary independence is crucial in this regard (see, e.g., De Grauwe and Ji, 2012). Independent central banks, so the argument goes, can act as a lender of last resort to governments and thereby rule out speculative runs on governments (see Farhi et al., 2013, for a formal treatment). Hence, whether a central bank is independent or not may matter for the dynamics of the default premium, at least the one paid on domestic-currency debt.

By and large, the results in Table A.4 are similar to the baseline (see left column of Table 2). Still, once we consider only emerging countries (column [2]), or once we exclude the observations for the crisis countries in the euro area (column [4]), the effect of spending cuts are quite a bit weaker during times of fiscal stress. The same holds for the results in column [6], that is, when we exclude countries without legal tender. However, in these instances we lose many observations that are characterized by fiscal stress, particularly in developed economies.[51]

**Further evidence on the term structure of spreads**

The increase of the default premium in response to fiscal shocks is consistent with two distinct scenarios: it may either reflect an increase of default risk (that is, the quantity of risk) or an increase of the "risk premium" (that is, the price of risk).[52] While we do control for changes in the global price of risk via time-fixed effects in our baseline specification, our results may still reflect country-specific fluctuations of the risk premium to the extent that financial markets are imperfectly integrated. To investigate this issue further, we compute a "forward spread", namely, the 1-year-1-year-forward $CDS^{1y}_{t+4,t+8}$ on the basis of the 2-year and 1-year CDS spreads:

$$CDS^{1y}_{t+4,t+8} = \frac{\left(1 + CDS^{2y}_t\right)^2}{1 + CDS^{1y}_t} - 1 \ ,$$

where the superscript indicates the maturity of the CDS.[53] We then compare the impact response of this forward spread after a fiscal shock to the response of the actual spread one year after the impact of the shock: $CDS^{1y}_{t+4}$. Assuming that all information is revealed upon impact, $CDS^{1y}_{t+4}$ provides a measure of the *expected* future short-term spread. We are thus in a position to test the hypothesis that the response of the risk premium is mostly driven by changes in the risk premium. For this to be the case, the response of the forward spread should differ from the response of the expected short-term spread.[54] Results, shown in Table A.5, allow us to reject the risk-premium hypothesis: the expected spread and the "forward

[51] Austerity programs are frequently part of the conditionality of IMF assistance, which is typically called upon when the sovereign default premium is high. To ensure that our results are not driven by such episodes, we run an additional check in which we drop all observations for which a country qualifies as an IMF "program country". We rely on the IMF's "History of Lending Arrangements" and classify countries as "program countries" if there is either a "Standby Arrangement" or an "Extended Fund Facility". Results are quite similar to those for the baseline sample (available on request).

[52] We thank an anonymous referee for stressing this aspect and for suggesting the strategy to explore it.

[53] Here we ignore the risk-free rate that would need to be added to obtain gross interest rates. As that rate is usually in the range of 2% annually, ignoring it only introduces a second-order measurement error.

[54] For a textbook treatment, see Bodie et al. (2017), Chapter 15.

**Table A.5:** Response of default premium (basis points) to fiscal shock: future spread vs. forward spread

| | $CDS^{1y}_{t+4}$ | $CDS^{1y}_{t+4,t+8}$ |
|---|---|---|
| Unconditional projection | | |
| All | 2.61*** | 3.50** |
| | (1.00) | (1.58) |
| Cuts only | 5.05*** | 8.04*** |
| | (1.81) | (2.48) |
| Maximum stress | | |
| All | 18.77*** | 16.84*** |
| | (5.17) | (6.84) |
| Cuts only | 31.12*** | 35.46*** |
| | (8.35) | (11.42) |
| No fiscal stress | | |
| All | −6.31*** | −3.85 |
| | (1.71) | (2.48) |
| Cuts only | −15.59*** | −13.47** |
| | (4.32) | (6.87) |
| Diff. max stress–no stress | | |
| All | 25.08** | 20.69** |
| Cuts only | 46.72** | 48.93*** |
| Countries | 23 | 23 |
| Observations | 903 | 903 |

*Notes:* Response of 1-year CDS spread at $h = 4$, $CDS^{1y}_{t+4}$, vs. 1-year forward spread between year 1 and 2, $CDS^{1y}_{t+4,t+8}$, to unanticipated reduction of government spending growth by 1 percentage point (annualized). Estimates based on projection (2.2), with $h = 0$ and using *Oxford Economics* forecasts. Driscoll and Kraay (1998)-standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent level, respectively. The sample has been restricted to the country-time observations where we have both the spot rate at $h = 4$ and the forward rate available. The reported number of observations is for the unconditional model; for the conditional model, we lose an additional 6 observations where we cannot construct the stress indicator.

spread" respond quite similarly to the fiscal shock.