Does austerity pay off?*

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Abstract

We ask whether cuts of government consumption lower or raise the sovereign default premium. To address this question, we set up a new data set for 38 emerging and advanced economies which contains quarterly time-series observations for sovereign default premia, government consumption, and output. We find that whether austerity pays off depends on a) initial conditions and b) the time-horizon under consideration. Spending cuts in times of fiscal stress raise default premia, but lower premia in benign times. These findings pertain to the short run. Austerity always pays off in the long run, but particularly so if initial conditions are bad.

Keywords: Fiscal policy, austerity, sovereign risk, default premium, local projections, panel VAR, fiscal stress

JEL-Codes: E62, E43, C32

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1 Introduction

In the years following the global financial crisis, many European governments implemented sizeable austerity measures in order to reduce budget deficits. These measures, which included spending cuts and tax increases, were meant to confront concerns about rising levels of public debt or outright solvency issues. In fact, the yields on debt issued by several European sovereigns started to take off by 2010, not least because of rising default premia (see, e.g., Lane, 2012). Yet the dismal growth performance in the following years, coupled with a further rise of yields, led many observers to question the wisdom of austerity. Against this background we ask whether austerity actually pays off and, if so, when and under which circumstances. More specifically, we ask whether austerity induces a rise or fall of the sovereign default premium.

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. In fact, while the response of fiscal indicators such as the level of sovereign debt is of first-order importance in this regard, it generally does not provide a sufficient statistic for assessing the sustainability of debt. For the willingness and the ability of governments to honor a given level of debt obligations depends on a number of country-specific, partly unobserved factors such as the ability to raise taxes. The same level of debt may thus have very different implications for debt sustainability in different countries (Bi, 2012; Eberhardt and Presbitero, 2013). In contrast, the default premium of sovereigns provides a more comprehensive statistic, both because of the immediate budgetary consequences of higher interest rates (see, e.g., Lorenzoni and Werning, 2014) and because they reflect a broader assessment of market participants.

Among the many factors which matter for such an assessment, output growth or, more generally, the level of economic activity plays a key role because it determines the amount of resources available for debt service (see, e.g., Arellano, 2008). In addition to debt levels and deficits, the growth performance of countries is therefore closely monitored by financial market participants: the default premium may rise or fall in response to austerity depending, among other things, on the joint response of debt levels and output growth to austerity. The premium is likely to increase if the growth effect of austerity is particularly adverse. Some observers indeed suggest that financial markets are "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, 2012).

However, the output effect of austerity measures, captured by the fiscal multiplier, is itself surrounded by considerable uncertainty. Recent contributions point to the state dependence of fiscal multipliers, that is, their tendency to change with the economic environment. Given the issue at hand, it is particularly noteworthy that a number of studies suggest that the multiplier is smaller or even negative whenever public debt is high (Auerbach and Gorodnichenko, 2013b; Corsetti, Meier, and Müller, 2012a; Ilzetzki, Mendoza, and Végh, 2013).[1] These results, however, are subject to the caveat that they condition multipliers on the level of public debt, rather than on a more comprehensive measure of "fiscal stress". Moreover, they rely on arbitrarily specified threshold levels for public debt in order to distinguish between low-debt and high-debt regimes. In our analysis below, we pursue an empirical strategy which overcomes both shortcomings while allowing for the possibility that the effects of austerity change with the level of fiscal stress.

As a first step of our analysis, we construct a new data set for sovereign default premia. Specifically, we collect time series for default premia for 38 advanced and emerging countries. We compute the default premium as the difference in sovereign yields 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 the confounding effects of inflation and depreciation expectations and to isolate market expectations of sovereign default. In some instances, we also rely on

[1] On the other hand, fiscal measures tend to impact the economy more strongly if there is pervasive slack in the economy (Auerbach and Gorodnichenko, 2012) or if the economy is stuck at the zero lower bound (Christiano, Eichenbaum, and Rebelo, 2011). Depending on the state of the economy, it may thus be beneficial in terms of macroeconomic outcomes to either frontload or to delay austerity measures (Corsetti et al., 2010). More extreme still, hysteresis effects may make austerity measures self defeating to the extent that contractionary fiscal measures may raise the financing costs of governments in the long run (De Long and Summers, 2012).

credit default swap spreads.

We establish a number of basic facts regarding default premia. First, premia vary considerably across time and countries. In some instances they are virtually zero, in others they are as high as 25 percentage points. The large number of observations allows us to compute the empirical density function. It increases sharply for low levels of the premium as the number of observations for which premia are high is limited. Second, default premia co-move negatively with economic activity. The correlation of default premia and output growth is strongly negative in all countries of our sample. Third, across countries there is no systematic correlation pattern of premia and government consumption.

In a second step, we provide estimates on the effects of austerity. We focus on cuts of government consumption for reasons of data availability. As a matter of fact, austerity packages typically comprise a variety of measures and our results are subject to the caveat that they pertain to the effects of spending cuts only.[2] In terms of identification, we assume that government consumption is predetermined within a given quarter. This assumption goes back to Blanchard and Perotti (2002) and is rationalized by the fact that changes in government consumption are subject to decision lags. A close reading of documents which detail austerity policies during the recent euro area crisis suggests that this also holds true in times of severe fiscal stress. We collect quarterly data for government expenditure following Ilzetzki, Mendoza, and Végh (2013), extending their data set to include additional countries and observations. For some countries, our observations for both quarterly government consumption as well as sovereign yield spreads date back to the beginning of the 1990s.

We pursue alternative econometric strategies to obtain estimates for how a variation of government consumption impacts the economy and, eventually, the sovereign default premium. In order to assess the short-run effect, we employ local projections (Jordá, 2005). This approach stands out in terms of flexibility and allows us to condition the effects of austerity on the extent of fiscal stress in a rather straightforward manner. It is less suited to assess the longer-term consequences of fiscal shocks. To study these, we rely

[2]According to some authors the composition of austerity is the key to its success (see, e.g., Alesina and Ardagna, 2013).

on panel vector autoregression (VAR) models.

We find two sets of results. First, how a cut of government consumption impacts sovereign default premia depends on whether the economy experiences benign times or fiscal stress. During benign times austerity induces a slight reduction of default premia. At the same time there is no significant output effect. During fiscal stress, on the other hand, cuts of government consumption raise default premia and lower output considerably. We show that these results—even though they may be puzzling at first sight—can be rationalized within a modified version of Arellano's (2008) model of optimal sovereign default. Second, these results obtain for the short run only (about 1.5 years). Starting after about 2 years, spreads tend to decline considerably and, in fact, more so if the economy experiences fiscal stress at the time austerity is implemented. Austerity pays off in the long run.

Our results are based on exogenous variations in government consumption, while austerity is typically a response to the state of the economy and to financial market developments. Still, identifying an exogenous variation in government consumption is key to isolate the impact of austerity as such rather than the joint effect of financial market developments and the accompanying austerity measures. That said, it is certainly possible that austerity measures impact the economy in different ways than a "regular" fiscal shock—perhaps because they are implemented under special circumstances or because the are particularly large. Conditioning the effects of spending cuts on the state of the economy is our strategy to address the first concern.[3]

Alternative and complementary approaches to assess the effects of fiscal consolidation episodes include case studies, notably those following up on the seminal work by Giavazzi and Pagano (1990). Yet another approach goes back to Alesina and Perotti (1995), recently applied by Alesina and Ardagna (2013). It identifies (large) fiscal adjustments as episodes during which the cyclically adjusted primary deficit falls relative to GDP by a certain amount. Finally, fiscal consolidations have also been identified on the basis of a narrative approach (Devries et al., 2011; Guajardo, Leigh, and Pescatori, 2011). Our

[3]Results by Giavazzi, Jappelli, and Pagano (2000) suggest that the size and persistence of fiscal measures also matters for their effects. We intend to take up this issue in future work.

paper is also not the first to study how fiscal policy affects financial markets. Related studies include numerous attempts to assess the effects of fiscal policy on interest rates. In particular, Ardagna (2009) finds that interest rates tend to decline in response to large fiscal consolidations. Laubach (2009) investigates how changes in the U.S. fiscal outlook affect interest rates. Finally, Akitoby and Stratmann (2008) focus on sovereign yield spreads in emerging markets.

The remainder of the paper is organized as follows. Section 2 details the construction of our data set. In this section, we also establish a number of basic facts regarding the time-series properties of sovereign default premia and their relationship to government consumption and output growth. In Section 3 we discuss our econometric specification and identification strategy. We present the main results of the paper and an extensive sensitivity analysis in Section 4. Section 5 suggests a structural interpretation through the lens of a model of optimal sovereign default. Section 6 concludes.

2 Data

Our analysis is based on a new data set. It contains quarterly observations for government consumption, output, and sovereign default premia in 38 emerging and advanced economies. While data on default premia are available at higher frequency, data on macroeconomic aggregates are not. For a long time, time-series 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.[4] Rather, quarterly data was often derived from indirect sources using time disaggregation/interpolation. In a recent contribution, Ilzetzki, Mendoza, and Végh (2013) have collected quarterly data based on direct sources for government consumption for 44 countries. Quarterly data of a comparable coverage for other fiscal variables such as taxes, transfers, or deficits are not available; hence our focus on government consumption.

[4] Some studies have resorted to annual data (e.g., Beetsma, Giuliodori, and Klaasen, 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.

We collect quarterly data for government consumption expenditure based on national accounts/non-financial accounts of the government along the lines of Ilzetzki, Mendoza, and Végh (2013). On the one hand, we limit our focus to those countries for which we are also able to compute a sovereign default premium. On the other hand, we extend their 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.[5] The full sample coverage is shown in Table 1. Our earliest observation for which we have both default premia and government consumption data is 1991Q1 for Denmark and Italy. Our sample runs up to 2014.

Table 1 also provides summary statistics for the government consumption-to-GDP ratio for our sample where both government consumption data and default premia are available. Government consumption from national accounts/non-financial accounts of the government is exhaustive government final consumption. It is accrual based and does not include transfer payments or government investment (see Lequiller and Blades, 2006, Chapter 9). Depending on the availability of quarterly time series, it pertains to either the general or the central government. The ratio of government consumption-to-GDP varies both across time and across countries. In case of general government data, government consumption fluctuates around 20 percent of GDP.

As a distinct contribution, we also construct a panel data set for sovereign default premia in order to measure the assessment of financial markets regarding the sustainability of public finances. Given observations on quarterly government consumption, we aim to construct measures of default risk for as many countries as possible. As stressed in the introduction, we construct a mostly spread-based measure using yields for securities issued in common currency. To the extent that goods and financial markets are sufficiently integrated, we thus eliminate fluctuations in yields due to changes in real interest rates, inflation expectations, and the risk premia associated with them. In addition to a default

[5]For several European countries, we opted to also include earlier observations during the 1990s, where default premia can be computed and countries experienced stronger variations in fiscal stress. In this case, government-consumption data is available through Eurostat. However, it 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. We therefore verify below that our results are robust with respect to employing a more conservative sample.

Table 1: Basic properties of government consumption-to-GDP ratio

Country	first obs	last obs	min	max	mean	std
Argentina	1994Q1	2013Q3	0.12	0.18	0.14	0.02
Australia	2003Q2	2010Q3	0.17	0.18	0.17	0.00
Austria	1994Q1	2014Q1	0.18	0.21	0.19	0.01
Belgium	1995Q1	2014Q1	0.21	0.25	0.23	0.01
Brazil	1995Q1	2014Q1	0.19	0.23	0.20	0.01
Bulgaria	1999Q1	2014Q1	0.14	0.20	0.18	0.02
Chile	1999Q3	2014Q2	0.05	0.06	0.06	0.00
Colombia	2000Q1	2014Q1	0.15	0.17	0.16	0.01
Croatia	2004Q2	2014Q1	0.18	0.21	0.20	0.01
Czech Republic	2004Q2	2014Q1	0.19	0.22	0.21	0.01
Denmark	1991Q1	2014Q1	0.25	0.30	0.26	0.01
Ecuador	1995Q2	2014Q1	0.09	0.14	0.12	0.02
El Salvador	2002Q3	2014Q1	0.06	0.09	0.07	0.01
Finland	1992Q3	2014Q1	0.20	0.25	0.22	0.02
France	1999Q2	2014Q1	0.23	0.25	0.24	0.01
Germany	2004Q2	2014Q1	0.18	0.20	0.19	0.01
Greece	2000Q1	2011Q1	0.17	0.22	0.18	0.01
Hungary	1999Q2	2014Q1	0.20	0.25	0.22	0.01
Ireland	1997Q1	2014Q1	0.14	0.21	0.17	0.02
Italy	1991Q1	2014Q1	0.17	0.22	0.19	0.01
Latvia	2006Q2	2014Q1	0.15	0.22	0.18	0.02
Lithuania	2005Q3	2014Q1	0.17	0.22	0.19	0.02
Malaysia	2000Q1	2014Q1	0.07	0.12	0.10	0.01
Mexico	1994Q1	2014Q2	0.00	0.00	0.00	0.00
Netherlands	1999Q2	2014Q2	0.20	0.27	0.24	0.02
Peru	1997Q2	2014Q2	0.07	0.09	0.08	0.01
Poland	1995Q1	2014Q1	0.17	0.20	0.18	0.01
Portugal	1995Q1	2014Q1	0.17	0.22	0.19	0.01
Slovakia	2004Q2	2014Q1	0.17	0.20	0.18	0.01
Slovenia	2003Q2	2014Q1	0.17	0.21	0.19	0.01
South Africa	1995Q1	2014Q1	0.18	0.23	0.20	0.01
Spain	1995Q1	2014Q1	0.17	0.22	0.19	0.02
Sweden	1993Q2	2014Q2	0.07	0.10	0.08	0.01
Thailand	1997Q3	2014Q2	0.10	0.14	0.12	0.01
Turkey	1998Q1	2014Q1	0.10	0.16	0.13	0.01
United Kingdom	1993Q1	2013Q4	0.17	0.23	0.20	0.02
United States	2008Q1	2014Q1	0.15	0.17	0.16	0.01
Uruguay	2001Q3	2014Q1	0.10	0.14	0.11	0.01

Notes: Government consumption is consumption of the general government except for Chile, El Salvador, Malaysia, Mexico, Peru, and Sweden, where it refers to central government consumption. The government consumption-to-GDP ratio is computed as the ratio of nominal variables, except for Uruguay, where we compute it as the ratio of real variables. For Mexico, the share of central government wages and goods and services purchases is only a very small share of GDP.

risk premium, if duration differs or drifts, yield spreads may still reflect a term premium (see Broner, Lorenzoni, and Schmukler, 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.[6] As a result, yield spreads should primarily reflect financial markets' assessment of the probability and extent of debt repudiation by a sovereign.[7]

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 Market Bond Index (EMBI) spreads, which measure the difference in yields between dollar-denominated government or government-guaranteed bonds of a country and U.S. government bonds.[8]

Second, we add to those observations data for euro area countries based on the "long-term interest rate for convergence purposes". Those 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.[9]

Third, we make use of the issuance of foreign currency government bonds in many advanced economies during the 1990s and 2000s 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, von Hagen, and Schuknecht (2012), we identify bonds denominated in either U.S. dollar or Deutsche mark of at least 5 years of maturity issued by advanced economies. We compute the yield spread for those bonds relative to the yields of U.S. or German government bonds of comparable

[6]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. Assessing the effects of austerity on the term structure is beyond the scope of the present study.

[7]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 Appendix A.1.3 for a more detailed discussion.

[8]See Appendix A.1.1 for details on the EMBI.

[9]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 depreciation/exchange rate premium that is absent in case of international law bonds. Still, even during the height of the European debt crisis, reversibility risk accounted for a small fraction of sovereign yield spreads in Greece (Kriwoluzky, Müller, and Wolf, 2014). In any case, our main results also hold for a sample of emerging market countries.

maturity and coupon yield.[10] Whenever possible, we aim to minimize the difference in coupon yield to 25 basis points and the difference in maturity to one year. In order to avoid artifacts introduced by trading drying up in the last days before redemption, we omit the last 30 trading days before the earliest maturity date of either the benchmark or the government bond.[11] In case of several bonds being available for overlapping periods, we average over yield spreads using the geometric mean. This procedure mimics the creation of the EMBI spreads and "long-term interest rate for convergence purposes". However, we necessarily rely on a smaller foreign currency bond universe and cannot correct for maturity drift. Thus, we rely on "long-term interest rate for convergence purposes" whenever they are available[12]

Finally, in the more recent part of the sample, a direct measure of default risk has become available in the form of credit default swaps (CDS) spreads. CDS 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.[13] Accordingly, a higher perceived default probability on the underlying bond implies, ceteris paribus, a higher CDS spread. While well-suited to capture market assessment of debt sustainability, CDS 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 are subject to counterparty risk (see Fontana and Scheicher, 2010). Thus, we use CDS to measure default risk only when no spread-based default premium measure is available.[14]

The use of CDS 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

[10] 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.

[11] 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, Lorenzoni, and Schmukler, 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.

[12] By focusing 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.

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

[14] The CDS data construction is described in Appendix A.1.2. The correlation between CDS spreads and the yield based default premium measures, when both are available, is typically above 0.9.

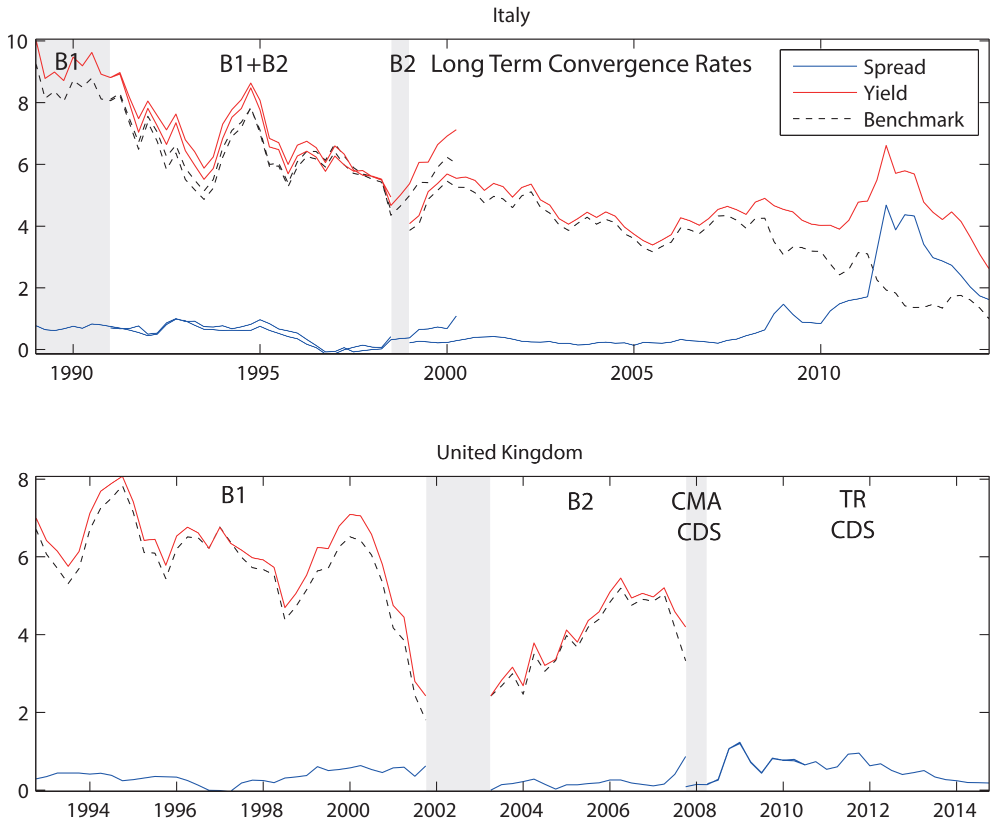


Figure 1: Construction of the Italian and United Kingdom default risk premia.

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 are available, we add the value of the average CDS spread of the period prior to the default of Lehman Brothers.[15]

To illustrate the construction of our data set, Figure 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

[15]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 and slowly return to about 15 basis points.

become available, which are used for the rest of the sample.[16]

Table 2 provides basic descriptive statistics for our absolute default premium measure. Default premia s_t are measured in percentage points and vary considerably across our sample. Periods of default on external debt are excluded from the sample.[17] In a couple of euro area countries the lowest realizations of the default premium are slightly negative.[18] For the advanced economies group,[19] we observe the highest premia in Portugal (12 pps) and Greece (10 pps). For the emerging economies, the highest values are reached in Brazil (24 pps), Ecuador (21 pps), and Argentina (20 pps).[20]

Compared to these values, most realizations of default premia in our sample are small. This is apparent from the empirical distribution function (CDF) plotted in Figure 2 for the entire sample (solid line), but also for the set of advanced (dashed-dotted line) and emerging economies in isolation (dashed line). The total number of observations in our sample is 2320, of which 1140 are for advanced economies and 1180 for emerging economies. 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 percentage point. Still, there are considerable differences across the two country groups: 99.7 percent of observations are below 10 percentage points in the sample of advanced economies. The corresponding number is only 95 percent in the sample of emerging market economies.

Finally, in the last two columns of Table 2 we report the correlation of sovereign default premia with output growth and the growth of government consumption, respectively. It turns out that default premia are countercyclical in all countries, although sometimes the correlation is negligible. In contrast, the correlation of default premia and government

[16]For details, see Appendix A.1.2.

[17]We follow the categorization by Standard & Poor's (see Chambers and Gurwitz, 2014, Table 2). Accordingly, in our sample Argentina (2001Q4–2005Q2), and Ecuador (1999Q3–2000Q3 and 2008Q4–2009Q2) were in default.

[18]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. These observations are best thought of as being 0.

[19]The assignment to country groups is shown in the second column of Table 2.

[20]During default episodes, spreads in secondary markets can achieve even higher values. In case of Argentina, the peak spread was 70 percentage points. Greek spreads were also higher shortly before and during the defaults (2012Q1–2012Q2, 2012Q4), but these observations are not included in our sample due to non-availability of corrected national accounts data.

Table 2: Basic properties of sovereign default premia

Country	Group	min	max	mean	std	$\rho(\Delta y_t, s_t)$	$\rho(\Delta g_t, s_t)$
Argentina	E	2.12	19.50	7.78	3.65	-0.55	-0.06
Australia	A	0.03	1.30	0.31	0.31	-0.38	-0.39
Austria	A	0.03	1.98	0.40	0.41	-0.47	-0.31
Belgium	A	0.03	2.94	0.59	0.59	-0.42	-0.20
Brazil	E	1.64	24.20	5.70	4.17	-0.05	-0.07
Bulgaria	E	0.73	9.18	3.24	2.53	-0.11	-0.04
Chile	E	0.62	4.04	1.60	0.63	-0.46	0.10
Colombia	E	1.26	10.73	3.56	2.07	-0.40	-0.17
Croatia	E	0.15	5.47	2.07	1.61	-0.66	-0.47
Czech Republic	E	0.05	2.08	0.62	0.54	-0.83	-0.05
Denmark	A	0.02	2.18	0.53	0.46	-0.20	-0.05
Ecuador	E	5.09	21.20	9.86	4.07	-0.44	-0.36
El Salvador	E	1.34	9.15	3.56	1.45	-0.75	0.04
Finland	A	-0.02	1.27	0.39	0.29	-0.50	-0.15
France	A	0.03	2.03	0.44	0.46	-0.41	0.01
Germany	A	0.02	0.73	0.20	0.18	-0.34	0.07
Greece	A	0.18	10.02	1.49	2.58	-0.61	-0.21
Hungary	E	0.17	6.37	2.00	1.75	-0.60	-0.05
Ireland	A	-0.02	9.09	1.41	2.15	-0.19	-0.39
Italy	A	-0.03	5.86	0.98	1.18	-0.42	-0.39
Latvia	E	0.05	10.01	2.75	2.30	-0.72	-0.74
Lithuania	E	0.06	7.25	2.32	1.83	-0.65	-0.23
Malaysia	E	0.74	4.31	1.70	0.71	-0.65	-0.05
Mexico	E	1.18	15.96	3.65	2.58	-0.28	-0.04
Netherlands	A	-0.01	1.18	0.34	0.32	-0.63	-0.28
Peru	E	1.24	9.18	3.52	1.93	-0.26	0.02
Poland	E	0.49	8.78	2.02	1.33	-0.05	-0.12
Portugal	A	0.03	12.28	1.63	2.84	-0.45	-0.42
Slovakia	A	0.04	4.10	1.22	1.21	-0.39	-0.23
Slovenia	A	-0.15	5.42	1.58	1.78	-0.47	-0.44
South Africa	E	0.77	6.59	2.42	1.25	-0.54	-0.18
Spain	A	-0.03	5.40	0.95	1.35	-0.65	-0.45
Sweden	A	0.01	1.20	0.39	0.24	-0.33	-0.07
Thailand	E	0.27	5.62	1.38	0.92	-0.38	0.13
Turkey	E	1.89	10.73	4.48	2.40	-0.33	-0.16
United Kingdom	A	0.05	1.20	0.45	0.24	-0.43	-0.06
United States	A	0.07	0.61	0.27	0.12	-0.48	0.12
Uruguay	E	1.51	16.50	4.02	3.13	-0.42	-0.38

Notes: Default premia s_t are end-of-quarter observations, measured in percentage points. The last two columns report the correlation of default premia with the growth rates of real GDP, Δy_t, and government consumption, Δg_t, respectively. Group entry "A" denotes advanced economies, while "E" denotes emerging economies. Excludes default episodes in Argentina (2001Q4–2005Q2), Ecuador (1999Q3–2000Q3 and 2008Q4–2009Q2), and Greece (2012Q1–2012Q2, 2012Q4).

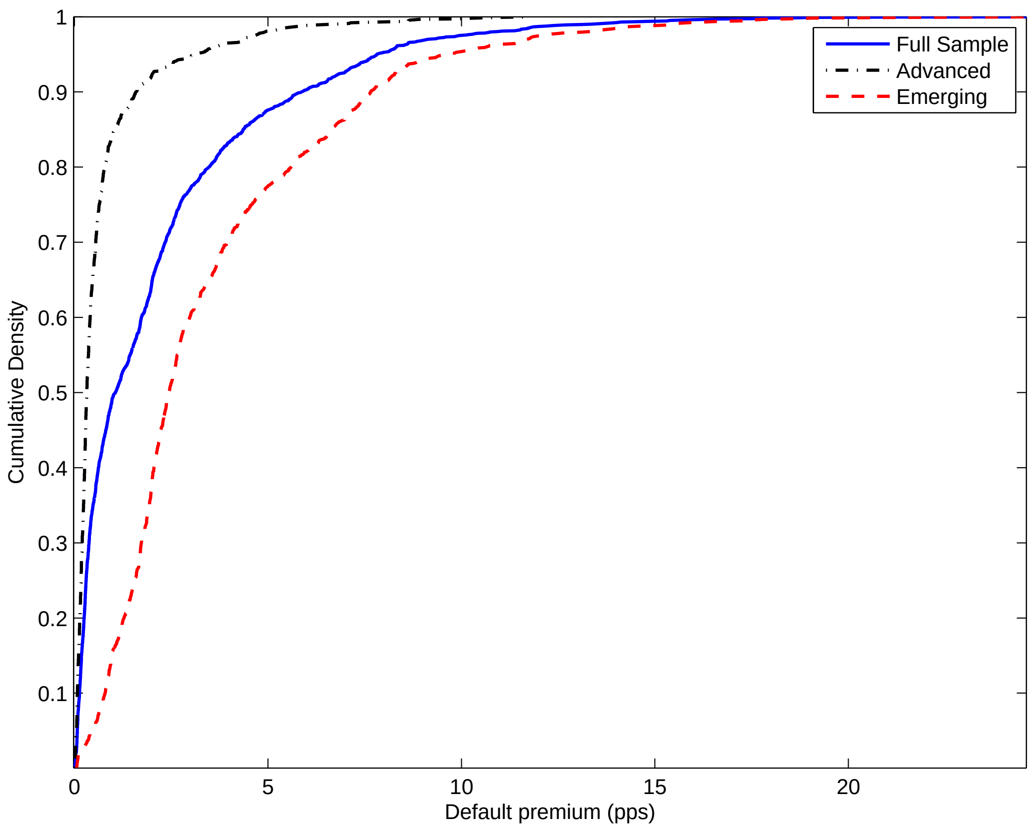


Figure 2: Sovereign default premia: empirical cumulative distribution function (CDF). Notes: horizontal axis measures default premia in percentage points. Vertical axis measures fraction of observations for which the lagged default premium is at most the value on the horizontal axis. Solid line displays CDF for full sample, dashed-dotted line: advanced economies only, dashed line: emerging economies only.

consumption growth varies across countries. It is negative for most of the countries, but often weakly so.

Eventually, we seek to establish the co-movement of default premia and government consumption conditional on an exogenous variation in government consumption. In order to do so, we rely on specific identification assumptions which are imposed within a particular econometric framework.

3 Empirical strategy

3.1 Setup

As our main tool, we rely on local projections to establish the effects of austerity on sovereign default premia as well as on other variables of interest. Relative to vector autoregression (VAR) models, local projections are more robust to model misspecification and are best linear projections even in the presence of nonlinearities (Jordá, 2005). Moreover, local projections prove highly flexible in accommodating a panel structure and, importantly, offer a very convenient way to account for state dependence—the focus of our analysis below. Earlier work by Auerbach and Gorodnichenko (2013b) and Owyang, Ramey, and Zubairy (2013) has illustrated this in the context of fiscal policy. More specifically, these studies employ a panel smooth transition autoregressive (STAR) model on which we rely in our analysis as well.

Formally, let $x_{i,t+h}$ denote the response of a particular variable at horizon h to an exogenous variation in government consumption at time t, with i indexing the countries in our sample. We estimate a local projection of $x_{i,t+h}$ on government consumption $g_{i,t}$ and a set of control variables $X_{i,t-1}$:

$$
\begin{aligned}
x_{i,t+h} = {} & \alpha_{i,h} + \beta_{i,h} t + \eta_{t,h} \\
& + F\left(z_{i,t}\right) \psi_{A,h} g_{i,t} + \left[1 - F\left(z_{i,t}\right)\right] \psi_{B,h} g_{i,t} \\
& + F\left(z_{i,t}\right) \Pi_{A,h}\left(L\right) X_{i,t-1} + \left[1 - F\left(z_{i,t}\right)\right] \Pi_{B,h}\left(L\right) X_{i,t-1} + u_{i,t} \, .
\end{aligned}
\tag{3.1}
$$

Here $\alpha_{i,h}$ and $\beta_{i,h}t$ are a country-specific constant and a country-specific trend, respectively,[21] and $\eta_{t,h}$ captures time fixed effects to control for common macro shocks.[22] The error term $u_{i,t}$ is assumed to have a zero mean and strictly positive variance. At each horizon, the response of the dependent variable to government consumption is allowed to differ across regimes "A" and "B", with the ψ-coefficients on the $g_{i,t}$ terms indexed accordingly. Similarly, $\Pi_{*,h}(L)$ is a lag polynomial of coefficient matrices capturing the impact of control variables in each regime.[23]

[21]Results are robust to using a quadratic time trend. We do not include a trend for the default premium.
[22]One of them is a possibly time-varying price of risk, see Appendix A.2.
[23]In our estimations, we set the lag length to four quarters. This is broadly in line with what information

We estimate model (3.1) using OLS where, in order to improve the efficiency of the estimates, we include the residual of the local projection at $t+h-1$ as an additional regressor in the regression for $t+h$ (see Jordá, 2005). For each forecast horizon, the sample is adjusted according to the available country-quarter observations.

3.2 Identification

The projection (3.1) does not provide a full description of the dependent variable's dynamics. It is not meant to. Instead, it seeks to capture the marginal effect of a fiscal shock over time. Identification therefore requires us to include as control variables only those variables which determine the evolution of government spending because we thereby isolate the dynamic effect of exogenous variations in government spending.

We follow Blanchard and Perotti (2002) and many others and assume that, within a given quarter, government consumption is predetermined.[24] This assumption is plausible because exhaustive government consumption is unlikely a) to respond automatically to the cycle and b) to be adjusted instantaneously in a discretionary manner by policy makers. To see this, recall that government consumption, unlike transfers, is not composed of cyclical items and, in addition, discretionary changes of government spending are subject to decision lags that prevent policymakers from responding to contemporaneous developments in the economy.[25]

Formally, we assume that government spending within a quarter is determined by the following process

$$g_{i,t} = \Gamma_*(L)X_{i,t-1} + \varepsilon^g_{i,t} \ , \tag{3.2}$$

criteria recommend. Our results are robust to varying the lag length.

[24]In fact, in terms of identification, our setup mimics a structural VAR with government spending ordered first (see also the discussion in Auerbach and Gorodnichenko, 2013a).

[25]Anecdotal evidence suggests that this holds true also in times of fiscal stress. For instance, in November 2009, European Commission (2009) states 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).

where $\Gamma_*(L)$ is a state-dependent lag polynomial of coefficient matrices capturing the impact of control variables. Deterministic terms are omitted to simplify the exposition. We generally allow for $g_{i,t} \in X_{i,t}$. $\varepsilon^g_{i,t}$ is an orthogonal innovation to government consumption. Eventually, we are interested in the response of a variable $x_{i,t+h}$ to such a shock while allowing for state dependence:

$$x_{i,t+h} = \alpha_{i,h} + \beta_{i,h}t + \eta_{t,h} + F\left(z_{i,t}\right)\psi_{A,h}\varepsilon^g_{i,t} + \left[1 - F\left(z_{i,t}\right)\right]\psi_{B,h}\varepsilon^g_{i,t} + u_{i,t}\ . \tag{3.3}$$

Using (3.2) to substitute for $\varepsilon^g_{i,t}$ in this expression and defining $\Pi_{*,h}(L) = -\psi_{*,h}\Gamma_*(L)$ yields the projection (3.1) on which we can rely to estimate the coefficients $\psi_{*,h}$.

Still, influential work by Ramey (2011b) and Leeper, Walker, and Yang (2013) has made clear that identification merely based on the assumption that fiscal policy measures are predetermined may fail to uncover the true effect of such measures whenever they are anticipated by market participants. The notion that fiscal policy measures are anticipated due to the legislative process and/or implementation lags is generally plausible. However, to what extent this matters quantitatively in the context of exhaustive government spending is unclear. In any case, controlling for anticipation using inherently forward-looking variables like default premia—as we do below—already goes a long way in mitigating potential problems due to foresight (see Sims, 2012). That being said, we also follow Ramey (2011b) and Auerbach and Gorodnichenko (2013b) and consider a specification of our model where we include forecast errors of government consumption rather than government consumption itself. Given data availability, we show—for a subset of our sample—that results do not change much relative to our baseline case.

Another popular approach is to identify fiscal shocks on a narrative basis. Following the work of Romer and Romer (2010) for the U.S., Devries et al. (2011) have constructed a data set of fiscal measures taken in a large sample of OECD countries. Importantly, these fiscal measures are identified on a narrative basis with a view to being orthogonal to the business cycle. A large number of these measures are thus taken in order to reign in public debt or budget deficits. To the extent that sovereign yield spreads co-move systematically with the latter, we stress that such "shocks" are not suited to investigate the effect of fiscal policy on the sovereign default premium.[26]

3.3 State-dependent dynamics

An important feature of projection (3.1) is that it allows us to capture the effects of fiscal shocks while accounting for the circumstances under which they take place. 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 z_{it}, is given by the regression coefficients on $g_{i,t}$ in equation (3.1):

$$\left.\frac{\partial x_{t+h}}{\partial g_{i,t}}\right|_{z_{i,t}} = F\left(z_{i,t}\right)\psi_{A,h} + \left[1 - F\left(z_{i,t}\right)\right]\psi_{B,h}\ . \tag{3.4}$$

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, 2014). Rather, the local projection at time t directly provides us with the average response of an economy in state $z_{i,t}$ going forward. Note also that equation (3.4) 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.

Regarding these conditions, it is conceptually convenient to distinguish two states or polar "regimes" which give rise to possibly different dynamics after a fiscal impulse. These polar cases are characterized by $F(z_{i,t})$ being equal to zero and one, respectively. It is quite unlikely, however, that actual economies operate in either of these two polar cases. Rather, they tend to be more or less close to one of the two. This notion is captured in the estimation, as the projection of the dependent variable at each horizon is a smoothly adjusted weighted average of the impact of government consumption as well as the controls. Specifically, the weights are a function $F(\cdot)$ of the indicator variable $z_{i,t}$, which provides information of where exactly the economy operates in between the two regimes. By using this weighted average, all observations between the two polar cases

[26] This applies to a lesser extent to studies which focus on the output effects of fiscal measures. See, for instance, Alesina, Favero, and Giavazzi (forthcoming) for an analysis of "fiscal consolidation plans".

contribute to identifying the dynamics in the two regimes.

In our estimation below we use lagged default premia $z_{i,t} = s_{i,t-1}$ as an indicator variable in order to measure how closely an economy operates to a regime of "fiscal stress". Using the lagged value of the default premium assures that the indicator is orthogonal to our identified government spending shocks. We weight regressors on the basis of the country-group specific empirical CDF (see Figure 2 above). Formally, we have

$$F(z_{i,t}) = \frac{1}{N}\sum_{j=1}^{N} \mathbb{1}_{z_j < z_{i,t}} \;, \tag{3.5}$$

where $\mathbb{1}$ denotes an indicator function and j indexes all country-time observations in each country group (advanced and emerging economies). The resulting indicator functions are displayed for each country in Figures A.1 to A.3 in the appendix. As an alternative to the empirical CDF, one may postulate a specific parametric function in order to attach weights to the indicator variable.[27] Using the empirical CDF (3.5), however, has two advantages. First, there are no degrees of freedom in specifying the transition function. Second, the polar cases are now given by states of the world that actually materialized in sample.[28]

3.4 Long-run effects

To the extent that one is interested in the long-run effects of fiscal shocks, projection (3.1) may be of limited practical use because the number of coefficients to estimate increases in the forecast horizon and quickly exhausts the degrees of freedom in the time dimension. When we take up the issue of possible long-run effects of fiscal shocks below, we therefore rely on VAR models. In this case the number of parameters to be estimated does not increase in the time horizon under consideration. Of course, as discussed above, this comes

[27] Auerbach and Gorodnichenko (2012) use a logistic cumulative density function $F(z_{i,t}) = \frac{\exp(-\gamma z_{i,t})}{1+\exp(-\gamma z_{i,t})}$ as their transition function so that $Prob(z < \bar{z}) = F(\bar{z})$. The parameter γ is set a priori such that 20 percent observations qualify as recessions.

[28] One may argue that only governments with relatively large financing needs issue foreign currency bonds and thus appear in our sample. As a consequence, our empirical CDF for fiscal stress may be skewed to extreme observations: those countries with large debt and thus default premia and euro area countries with historically low default premium observations. We check the robustness of our results by also using a logistic transition function and find that they are robust.

at the expense of additional cross-equation restrictions. Moreover, it less straightforward to account for state-dependence in panel VAR models.

Still, given the fairly rich cross-sectional dimension at our disposal we can rely on sample splits and estimate the VAR model on two different sets of country-time observations (see also, for instance, Ilzetzki, Mendoza, and Végh, 2013).[29] As with the local projection, we will rely on the empirical CDF of default premia to split observations in two groups. Formally, we estimate the following VAR by OLS

$$X_{i,t} = \mu_i + \alpha_i t + \Lambda(L) X_{i,t-1} + \nu_{i,t} \ , \tag{3.6}$$

where and μ_i and α_i are vectors containing country-specific constants and time trends. We use four lags. In terms of identification, we maintain the assumption that government consumption is predetermined. To impose this assumption, we order government spending first in $X_{i,t}$ and equate the first element in $\nu_{i,t}$ with a structural fiscal shock. As a practical matter, we assume a lower-triangular matrix B which maps reduced-form innovations $\nu_{i,t}$ into structural shocks $\varepsilon_{i,t} = B\nu_{i,t}$, where $\varepsilon_{i,t} \overset{iid}{\sim} (0, I)$. We attach no structural interpretation to the other elements in $\varepsilon_{i,t}$.

We follow the VAR literature on long-run effects (e.g., Blanchard and Quah, 1989) and allow for a long-run effect of fiscal shocks on default premia by including their first difference in $X_{i,t}$. The long-run response to a government spending shock can then be recovered from the total impact matrix (see Lütkepohl, 2005, Ch. 9.1.4)

$$\Omega_\infty = (I - \Lambda(1))^{-1} B^{-1} \begin{pmatrix} 1 & 0 & 0 \end{pmatrix}' . \tag{3.7}$$

Of course, a VAR in levels also allows for unit roots, but estimating a root to be exactly at 1 is a zero probability event. By including the spread in first differences, we essentially impose a unit root first and then use the long-run impact matrix to check whether the null hypothesis of no long-run impact can be rejected.[30] Confidence bands are obtained by bootstrapping.

[29]This implies, in contrast to the LP approach, that the impulse responses derived from the VARs for the sample groups are conditional on the assumption of staying in the same group going forward.

[30]This contrasts with the long-run identification scheme of Blanchard and Quah (1989) that excludes a long-run effect of certain shocks to identify the B matrix.

4 Results

4.1 Baseline

We estimate the local projection (3.1) on the full sample. In our baseline specification we include 4 lags of government consumption, the default premium, and output. We report the dynamic effect of a government spending shock on all three variables. The size of the shock is normalized such that government consumption declines by one percent of GDP.[31]

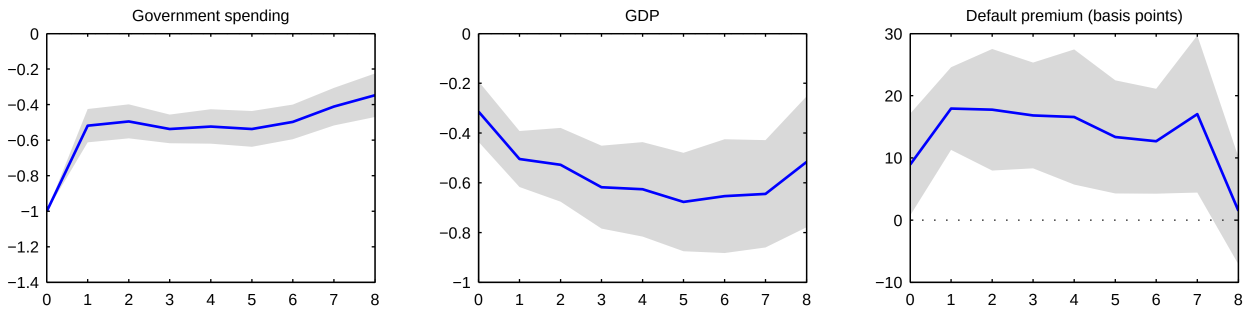
Figure 3 shows results. Here, and in the following, the horizontal axis measures time in quarters, while the vertical axis measures the deviation from the pre-shock path. The deviation is measured in percent of trend output, except for the default premium, which is measured in basis points. Solid and dashed lines represent the point estimates, while shaded areas and dotted lines indicate 90 percent confidence bounds.[32] Note that we restrict the horizon for which we report impulse responses to 8 quarters, because extending the horizon comes at the expense of degrees of freedom in the time-series dimension.

To set the stage, the upper panels of Figure 3 display the estimate of the local projection without conditioning on fiscal stress, that is, coefficient matrices in regimes A and B are restricted to be equal. As shown in the left panel, government spending, after an initial cut equal to one percent of GDP, remains depressed for an extended period, but eventually returns to its pre-shock level. The response of GDP is displayed in the middle panel. It declines by about 0.4 percentage points on impact, declines more strongly thereafter, and reaches a trough response of about -0.6 percent of GDP after 1.5 years. Given that we normalize the initial cut of government consumption to -1 percent of GDP, the (absolute value of our) estimate corresponds to the government spending multiplier on output (impact and peak-to-impact, respectively). Our estimates fall in the range of values frequently reported in the literature, if perhaps somewhat at the lower end (see, e.g., Barro and Redlick, 2011; Ramey, 2011a).

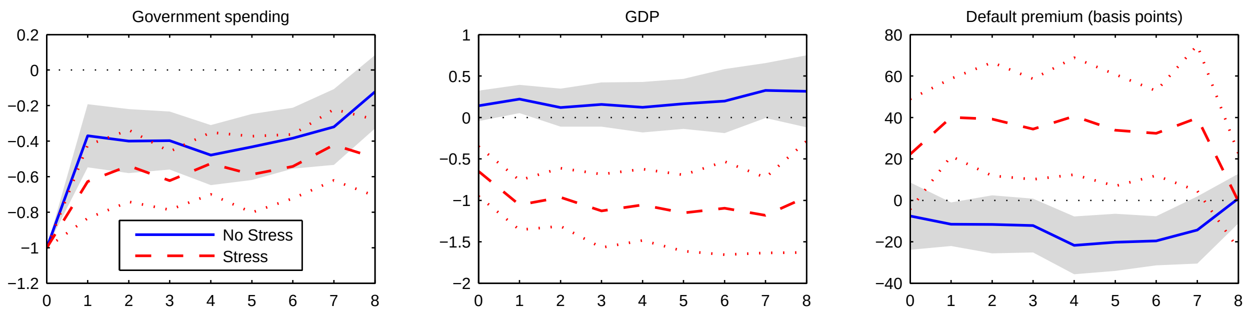
Finally, in the right panel we present estimates for the dynamic response of default

[31] We assume a government consumption to GDP ratio of 0.2 in line with the evidence reported in Table 1.

[32] Confidence bounds are based on Driscoll and Kraay (1998) cross-sectional correlation robust standard errors.



(a) Unconditional model



(b) Conditional on initial conditions

Figure 3: Dynamic response to an exogenous cut of government consumption by 1 percent of GDP: unconditional response (top) vs conditional on initial conditions (bottom). Notes: Horizontal axes represent quarters. Vertical axes represent deviation from pre-shock level in terms of trend output and basis points (default premium). Shaded areas and dotted lines indicate 90 percent confidence bounds. Weighting of initial conditions based on country group-specific CDF.

premia. We find that default premia do in fact increase in response to the spending cut. The impact and maximum response is about 10 and 20 basis points, respectively. It thus appears that austerity does not pay off: spending cuts fail to reassure investors about the sustainability of public finances.[33]

However, the above results do not condition on the state of the economy. They therefore mask heterogeneity of economic circumstances which may matter for how default premia respond to austerity, both across time and countries. For example, results by Bertola and Drazen (1993) and Perotti (1999) show that fiscal policy affects the economy differently in "good times" and "bad times". Recent evidence established by Corsetti, Meier, and Müller (2012a), Auerbach and Gorodnichenko (2013b), and Ilzetzki, Mendoza, and Végh (2013) suggests that the government spending multiplier on output tends to be

[33] The movements of default premia over time are not in conflict with the view that financial markets process information efficiently, see Section 5 below.

relatively low if debt is high. This is particularly relevant as austerity is often enacted in response to concerns about the sustainability of debt. However, as discussed above, public debt *per se* is an insufficient statistic to assess the sustainability of public finances, because fiscal capacity varies strongly with a number of country-specific factors, many of them unobserved. Instead, sovereign default premia provide more comprehensive information regarding the extent of "fiscal stress". They are the financial market participants' aggregate assessment of the observed and unobserved fundamentals determining fiscal sustainability and thus provide an indirect measure of the distance to the fiscal limit.

In what follows, we therefore estimate the state-dependent model (3.1) relying on lagged default premia s_{it} as an indicator variable with the country-group specific empirical CDF (3.5) used as the weighting function (advanced vs. emerging economies).[34] Thereby, the effects of spending cuts are allowed to differ depending on whether they take place in a regime of fiscal stress, evidenced by high default premia ($F(s_{i,t-1}) = 1$), or in "benign times" ($F(s_{i,t-1}) = 0$).

The second row of Figure 3 shows results for the baseline specification. Solid lines represent point estimates when conditioning on benign times. Dashed lines represent the results conditional on the presence of fiscal stress. Differences are rather stark: the dynamic adjustment of the economy under fiscal stress resembles that implied by the unconditional estimates, but the effects are quite a bit stronger. The point estimate for the multiplier now reaches a value of about unity, while default premia rise up to approximately 40 basis points in response to a cut of government consumption by one percent of GDP.

The effects of austerity in benign times, on the other hand, differ considerably from those obtained from the unconditional estimates. We now find that cutting government consumption actually raises output, although this effect is only marginally significant, both statistically and economically. Importantly, our estimates also suggest that default premia decline in response to cuts in government consumption, provided that the economy

[34]Using the empirical CDF obtained for each country group in isolation accounts for the observed heterogeneity in default premia across the set of advanced and emerging economies. We also run the estimation using the empirical CDF of the full sample. Results do not change qualitatively.

Table 3: Impact, peak, and long-run response of default premium (basis points)

	Local projection		VAR	
	Impact	Peak	Short-run peak	Long run
Unconditional	9	18	43	-29
Fiscal stress	22	40	36	-83
Benign times	-8	-22	-2	-8

Notes: Response to a cut of government consumption equal to 1 percent of GDP. The short-run peak is defined as the maximum of the absolute default premium response over the first 8 quarters.

enjoys more benign times. Also, premia decline gradually by about 20 basis points in this case. The impact and peak responses of default premia are summarized in the left panel of Table 3.

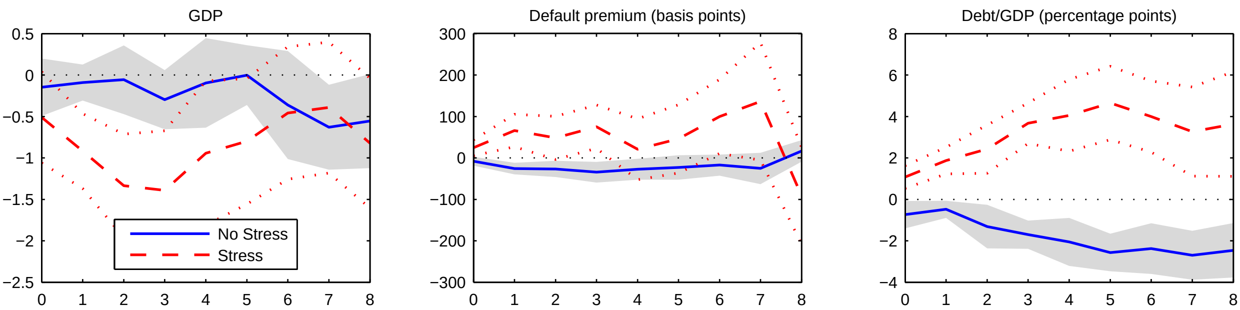
We also check whether the responses in both regimes are statistically significantly different from each other using a Wald-test. After correcting for multiple comparisons, the endogenous government spending response is not significantly different across regimes, while the null hypothesis that responses are equal is generally rejected for output and default premia at all horizons. Thus, while the responses of output and default premia differ significantly across regimes, this result is not driven by possible differences in the way austerity is implemented.

4.2 Transmission

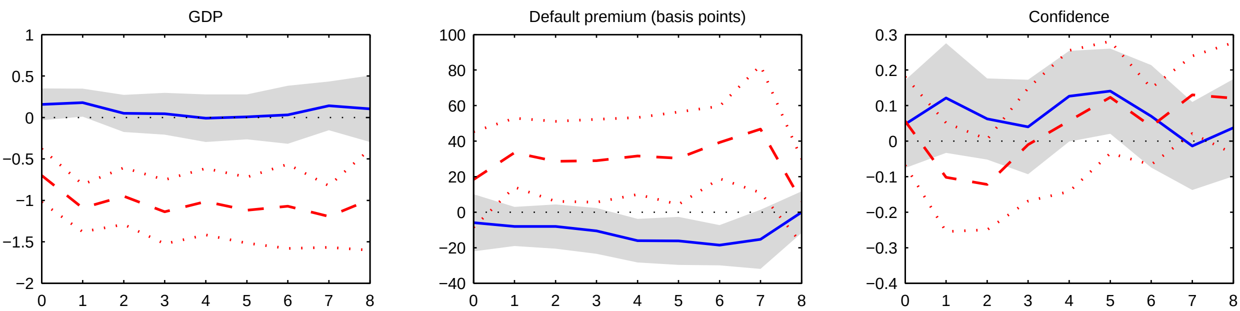
Austerity measures are often implemented during times of fiscal stress with a view towards reducing the default premium. Our finding that premia rise in response to spending cuts may thus appear puzzling. To shed further light on possible transmission channels, we consider four additional variables in our local projection. Specifically, we consider the debt-to-GDP ratio, a measure of confidence, private consumption, and private investment. At each horizon we project these variables on government spending and include their lags in the control vector.

Figure 4 shows the results. Panel (a) displays results for the specification with public debt.[35] Here, we consider a subsample that is considerably smaller than the full sample,

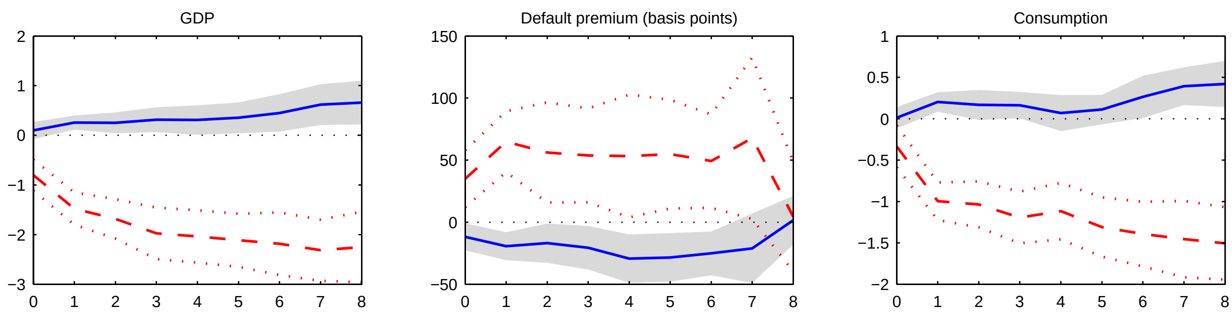
[35]It is total gross debt at nominal value outstanding at the end of each quarter between and within the



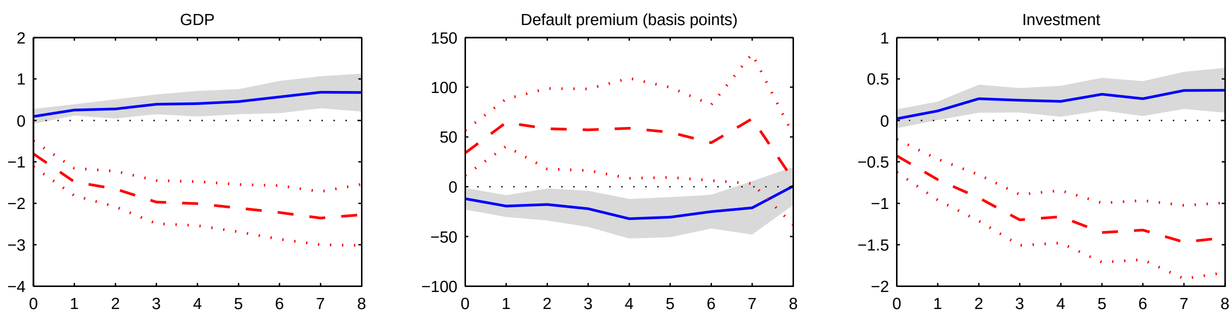
(a) Including debt/GDP ratio



(b) Including confidence



(c) Including private consumption



(d) Including private investment

Figure 4: Dynamic response to an exogenous cut of government consumption by 1 percent of GDP: additional variables. Notes: The responses of private consumption and investment are measured in percent of GDP.

because quarterly debt figures are not available for most countries and time periods. Yet,

sectors of general government.

the estimated effects of the fiscal shock on government spending (not shown), output, and default premia are fairly similar to those obtained for the baseline specification. This holds, in particular, for the differential impact of fiscal stress on the dynamics following a spending cut. The response of debt (relative to annual GDP) is quite informative: it rises in response to a spending cut if fiscal stress is high. It declines, albeit very gradually, if times are benign. This finding goes some way in accounting for the differential impact of austerity measures on default premia across the two regimes.[36]

Panel (b) of Figure 4 shows the results for the specification that features a measure of confidence provided by the Ifo World Economic Survey (WES), which surveys a number of experts for all countries in our sample.[37] Earlier research on the consequences of fiscal consolidations has argued that its impact on "confidence" is crucial (see, for instance, the discussion in Perotti, 2013). Bachmann and Sims (2012) find that confidence responds strongly to fiscal shocks during periods of economic slack. To gain a better understanding of what mechanism may drive our results, we also consider confidence in our model.[38] In times of fiscal stress (solid lines) confidence tends to decline in the short run; during benign times, in contrast, confidence tends to improve. These findings are consistent with the notion that austerity is less harmful to economic activity whenever it is associated with an improvement of confidence. In our setup, this coincides with a decline of the default premium.

Panels (c) and (d) show the responses obtained for the specifications which include, in turn, private consumption and private investment as a fourth variable. Including these variables does not fundamentally alter the responses of output and the default premium. The responses of private consumption and investment look very much alike: both co-move strongly with output. This holds for times of fiscal stress as well as for benign times.

[36] A similar picture emerges, once we consider the deficit ratio rather than the debt ratio (see Figure A.4)

[37] Respondents are asked to classify their expectations for the next six months using a grid ranging from 1 (deterioration) to 9 (improvement). 5 indicates that expectations are "satisfactory" (see, e.g., Kudymowa, Plenk, and Wohlrabe, 2014).

[38] Moreover, adding the forward-looking variable confidence enlarges the information set of the econometrician. This may mitigate possible problems due to fiscal foresight, an issue which we explore more systematically below.

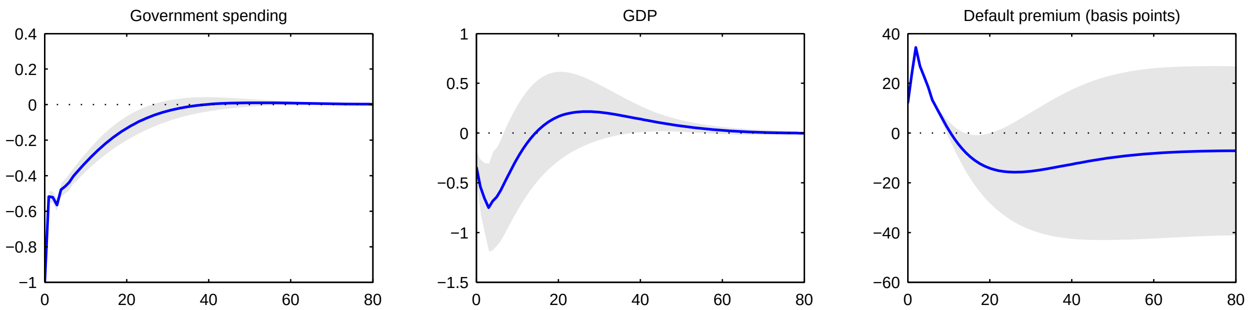
4.3 The default premium in the long run

Our results so far suggest that cutting government consumption does not pay off, at least if enacted during times of fiscal stress because it induces the default premium to rise. However, our focus has been on the short run, that is, on the first 1-2 years after the shock. A natural concern regarding our results is that more time may need to pass for the beneficial effects of austerity to materialize. As discussed in Section 3, assessing this issue on the basis of local projections can be excessively expensive in terms of degrees of freedom along the time dimension.

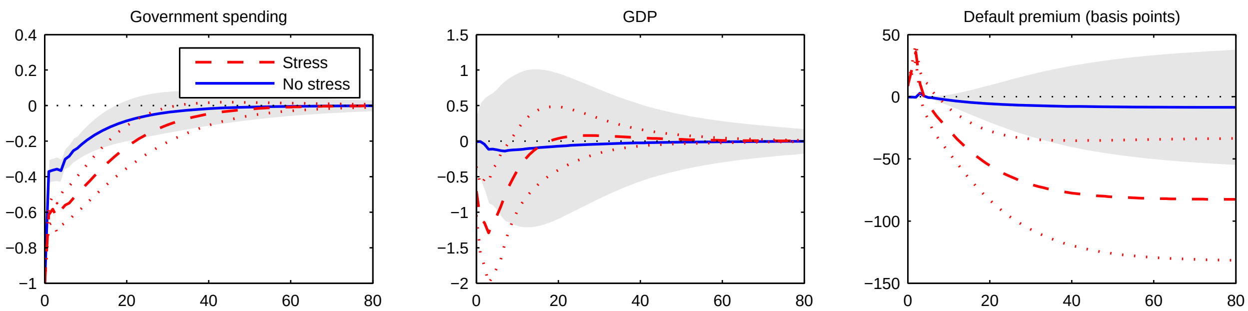
Hence, given the limited number of time-series observations at our disposal, we pursue an alternative strategy based on estimating a conventional panel VAR model. Specifically, we estimate VAR model (3.6) on time-series for government consumption, output, and the first difference of the sovereign default premium. We also conduct a sample split to distinguish between times of fiscal stress and benign times. Observations qualify as being characterized by fiscal stress if the empirical CDF of the default premium exceeds 0.7. Times qualify as benign if the indicator is below 0.3. This leaves us with 643 country-quarter observations for the stress regime and 635 observations for benign times.

Figure 5 displays the impulse responses to a cut of government consumption, covering a horizon of 20 years. The top panels show impulse responses for the unconditional sample. The right panel is of particular interest. It shows the cumulative response of the default premium. In terms of short-run dynamics, results are comparable to our baseline specification (see Figure 3). However, for the medium and long run we observe a considerable decline of default premia. The point estimate also suggests a permanent effect, but it is not significant.

Results are more clear cut in the bottom panels, where we distinguish between fiscal stress and benign times. Austerity during benign times (solid lines) lowers the default premium, but the effect is not significant in the long run. Things are different if austerity takes place in times of fiscal stress. As before, we find that the default premium increases significantly in the short run. However, we are now able to detect a significant decline afterwards: after about 1.5 years the premium starts to decline relative to the peak effect.



(a) Unconditional VAR



(b) Split: fiscal stress vs. benign times

Figure 5: Dynamic response to an exogenous cut of government consumption by 1 percent of GDP: possible long-run effect on default premium. Notes: estimates based on a structural panel VAR model with default premium entering in first differences. Horizontal axes measures quarters. Right panel shows cumulative response of default premia.

Moreover, it quickly falls below its pre-shock level and keeps on undershooting it in the very long run. We also note that the co-movement of the default premium and output is negative throughout: default premia peak at the time output is most depressed and start to decline at about the time when output rebounds.

Finally, note that the right panel of Table 3 summarizes the peak response and the long-run response of default premia obtained from the estimated VAR models. In terms of short-run effects, results are close to those obtained on the basis of local projections. In terms of long-run responses, the VAR model predicts a decline of the default premium by about 80 basis if government consumption is cut by one percent of GDP.

4.4 Sensitivity analysis

Fiscal Foresight

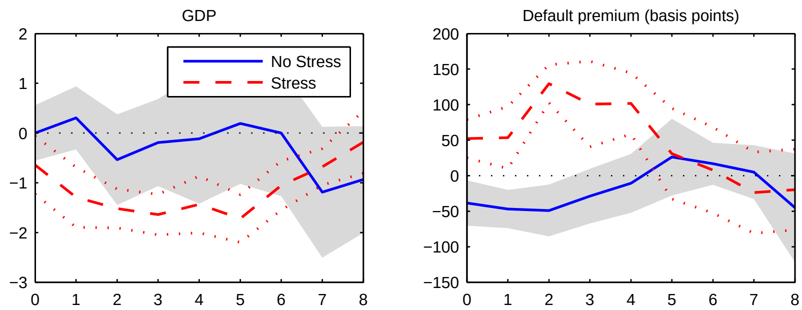
We explore the robustness of our findings across a range of alternative specifications and sample periods. A first set of experiments is aimed at exploring issues pertaining to fiscal foresight. Under the conventional Blanchard-Perotti approach, news and realizations of fiscal shocks are assumed to coincide. To the extent that fiscal shocks are known prior to implementation, estimates may be biased (Leeper, Walker, and Yang, 2013; Ramey, 2011b). To gauge the impact of possibly anticipated government spending shocks on our results, we turn to the OECD Economic Outlook data set, which contains semiannual observations for the period from 1986 to 2014 for an unbalanced panel of OECD countries. It contains explicit forecasts for government consumption spending, prepared by the OECD in June and December of each year, i.e., at the end of an observation period.[39]

Including the forecast error for government consumption in the local projection (3.1) rather than government consumption itself allows us to better identify the effects of unanticipated spending shocks in the presence of anticipated changes of government spending. Specifically, we replace the level of government consumption with the period-t forecast error of the growth rate of government spending.[40]

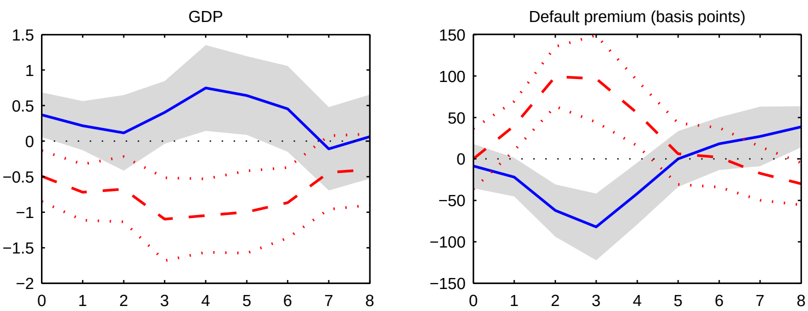
Figure 6 displays the results, obtained for the sample for which government spending forecasts are available. In the top row we show the results based on forecast errors, distinguishing fiscal stress and benign times. Results are quite similar to those reported for the baseline model in Figure 3 above. In order to isolate the effect of controlling for anticipation from the effect of varying the sample, we display in the bottom row results for when the baseline specification is estimated on the sample for which OECD forecasts are available. Again, it turns out that explicitly accounting for anticipation does not alter results very much. This confirms earlier findings by Beetsma and Giuliodori (2011), Born, Juessen, and Müller (2013), and Corsetti, Meier, and Müller (2012b).

[39] As discussed in detail by Auerbach and Gorodnichenko (2012), these forecasts have been shown to perform quite well.

[40] We use growth rates rather than levels, because the base year used by the OECD changes several times during our sample period.



(a) Identification using government consumption forecast error



(b) Identification using actual government consumption

Figure 6: Dynamic response to an exogenous cut of government consumption by 1 percent of GDP: semi-annual observations. Upper row: responses conditional on benign times (solid lines) and fiscal stress (dashed lines) using forecast error in local projection. Lower row: baseline specification estimated on OECD sample of semi-annual observations.

Cross-sectional heterogeneity

One might argue that, despite allowing for country fixed effects and state-dependent dynamics, the cross-sectional heterogeneity in our country sample is not sufficiently accounted for in our baseline model, leading to inconsistent estimates (see Pesaran and Smith, 1995). As a first step to address this concern, we conduct a number of sample splits. In doing so, we obtain results for a sample that includes only euro area countries, for a sample of euro area periphery countries which were hit hardest by the crisis (Greece, Ireland, Italy, Portugal, Slovenia, Spain), and for a sample of the remaining euro area countries.

Results, shown in Figure A.5, tend to be qualitatively similar to those obtained for the full sample—notably in terms of the differential impact of fiscal stress. The same holds for sub-samples comprising advanced and emerging economies only, see Panels (a) and (b) of Figure A.6. As a caveat, however, we note that there are sizeable differences in some instances, partially reflecting a strong decline in sample size. Similarly, we check whether results are driven by Great Recession. Panels (c) and (d) of Figure A.6, reporting estimates for a sample which ends in the second quarter of 2007, show that this is not the case.

A second, more formal approach controlling for cross-sectional heterogeneity is to estimate our baseline model using a mean-group estimator (Pesaran and Smith, 1995), that is, we estimate model (3.1) for each country separately and then average over the cross-section of coefficients.[41] As this leaves us with few degrees of freedom, we only estimate the model without conditioning on fiscal stress and use the panel model with slope homogeneity as our benchmark. Figure A.7(a) shows that our results are robust to allowing for cross-sectional heterogeneity in parameters.

Independent monetary policy

Some observers have argued that sovereign yield spreads, notably during the recent euro area crisis, are driven by "market sentiment" rather than "fundamentals" (see, e.g., De Grauwe and Ji, 2012). According to a popular narrative, the fact that euro area countries have surrendered monetary independence is crucial in this regard. 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. 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 reducing the likelihood of runs on domestic debt, it is very likely that there are spillover effects on the default premium paid on foreign-currency debt, too.

To explore this possibility for our data set, we consider results for countries that are either members of a monetary union or have officially dollarized.[42] Panels (b) and (c) of Figure A.7 show, respectively, the results for this country group and for countries that

[41]When doing so, we keep the model specification as in the benchmark model, except that we cannot include time fixed effects.

[42]Ecuador since 2000Q1 and El Salvador since 2001Q1 use the dollar as their official legal tender (see Levy Yeyati and Sturzenegger, 2002). We do not include hard pegs like the currency board in Argentina before 2001, because as this case shows, they are quite easily reversible.

have their own legal tender. We find that conditioning on monetary independence has little bearing on our results regarding the role of fiscal stress.

Measurement of default premium

In our baseline specification we measure the default premium in percentage points. Benign times are effectively characterized by a premium of close to zero. Impulse responses computed for the regime of benign times may then imply that the premium becomes potentially negative. Economically this makes little sense. We therefore consider an alternative specification where the premium is measured in logs. The results, shown in Figure A.8(a), are qualitatively similar, with the premium in the benign-times regime staying roughly constant.

Excluding benefits in kind

One justification for the predeterminedness assumption of government spending is that cyclical transfer components like food stamps are not included in the United States NIPA data on government consumption. However, government final consumption expenditure includes “Social benefits in kind corresponding to purchases of products supplied to households via market producers” (see Lequiller and Blades, 2006, Chapter 9). This item has the potential to be cyclical if the government for example provides unemployed persons with health care benefits that fall into this category. Ideally, one would like to exclude such items, but unfortunately this is impossible on a consistent cross-country basis due to institutional differences.[43] Still, we check the robustness of our results when excluding “benefits in kind provided via market producers” from our measure of government consumption (which can be done for European Union members). Figure A.8(b) presents the results. In general, they are similar to those obtained for our baseline specification.

[43]For details, see Appendix A.3.

Conservative sample

As discussed in Section 2, we include in our baseline sample some European countries in the 1990s because fiscal stress was quite variable in this sample of advanced economies. This implies that we partially rely on fiscal data that does not fully meet the more recent standards in the compilation of quarterly non-financial accounts of the government (see, e.g., Eurostat, 2011). To ensure the robustness of our results in terms of data quality, we estimate our model on a conservative subsample where the data quality is higher.[44] Results are shown in Figure A.8(c). They are very similar to our baseline sample.

Boom and recessions

Times of fiscal stress are mostly likely times of low output growth. Of course, the converse does not necessarily hold: a recession does not necessarily give rise to fiscal stress. Still, to put our results into perspective, it is useful to assess to what extent the effects of austerity on default premia change with the state of the business cycle. For this purpose, we estimate local projections, but instead of conditioning on fiscal stress we condition on the state of the cycle. As our indicator z_{it}, following Auerbach and Gorodnichenko (2013b), we use a measure of the output gap[45] and compute the empirical CDF as in the case of the sovereign default premium.

Figure A.8(d) shows the results. As in earlier work by Auerbach and Gorodnichenko (2013b) we find that the output effects of fiscal policy are considerably stronger during recessions. We also find that the default premium increases during recessions and falls during booms in response to austerity. We thus obtain a pattern of responses quite comparable to the one obtained once we condition on fiscal stress. Perhaps surprisingly, while conditioning on fiscal stress and recessions yields very similar results, we find that the overlap of stress and recession episodes is far from complete. In particular, the correlation

[44] We checked with national statistical agencies and adjusted the Ilzetzki, Mendoza, and Végh (2013) data sample where necessary. Using this conservative sample eliminates about 10% of our advanced economy observations.

[45] First, we compute a five-quarter moving average of the first difference of log output. The resulting series is then z-scored and filtered using an Hodrick-Prescott filter with smoothing parameter $\lambda = 160{,}000$. This is the value used in Auerbach and Gorodnichenko (2013b) adjusted for our quarterly sample following Ravn and Uhlig (2002).

of the empirical CDF which are used in the projection as weights is only moderate (see Table A.1 and Figures A.1 to A.3 in the appendix.).

5 Interpretation

Our empirical analysis reveals a number of robust patterns. Perhaps most puzzling are the dynamics in the short run: yield spreads rise in response to spending cuts if fiscal stress is high, but decline in the absence of fiscal stress. We now turn to a structural interpretation of these short-run dynamics, drawing on earlier work by Arellano (2008). We modify her framework to capture key aspects of our empirical setup, but minimize the departure from the original model in order to keep the analysis as transparent as possible. Specifically, we depart from the original model by allowing for a) exogenous variations in government spending and b) a multiplier effect of such variations on output.

In what follows, we briefly sketch the model. There is a small open economy whose government engages in intertemporal trade with international, risk-neutral investors in order to maximize the expected utility of the representative household:

$$E_0 \sum_{t=0}^{\infty} \beta^t U(c_t) \ . \tag{5.1}$$

Here $0 < \beta < 1$ is the discount factor and c_t is private consumption. Output y_t is given by

$$y_t = \bar{y} e^{\epsilon \hat{g}_t}, \tag{5.2}$$

where $\bar{y}$ is a positive constant and $\hat{g}_t$ is the percentage deviation of government consumption from its long-run value $\bar{g}$.[46] Government consumption varies exogenously and may impact household utility additively separable from c_t. ϵ is a reduced-form measure of the multiplier. We omit possible microfoundations of the multiplier based on, for instance, endogenous labor supply or a working capital constraint in order keep the analysis as focused as possible.[47]

[46] We abstract from exogenous variations in output due to endowment shocks as we are only interested in the dynamics conditional on shocks to government spending.

[47] Mendoza and Yue (2012) develop a model of optimal sovereign default and endogenous output determination.

Capital markets are incomplete and the government cannot commit to repaying its debt. Instead, it decides whether to repay the outstanding debt or not in each period. In case it repays, the resource constraint of the economy is given by

$$y_t + q_t d_{t+1} - d_t = c_t + \bar{g} e^{\hat{g}_t} \ , \tag{5.3}$$

where d_t is beginning-of-period debt, d_{t+1} is newly issued debt which matures in the next period and sells at price q_t. This price is determined in world capital markets by a no-arbitrage condition

$$q_t = \frac{1 - \delta_t}{1 + r} \ , \tag{5.4}$$

where δ_t is the probability of default and r is a risk-free return which international investors earn elsewhere.

In the event of default, the country is excluded from international financial markets and resorts to autarky. It will, however, be allowed to reenter financial markets with probability θ in each period thereafter. In addition, there is an asymmetric output cost, such that output in states of default is given by $y_t^{\text{def}} = \min(y_t, \bar{y}^{\text{def}})$. $\bar{y}^{\text{def}}$ is a constant defining the maximum output level during autarky. As a result, consumption during autarky is given by

$$c_t^{\text{def}} = y_t^{\text{def}} - \bar{g} e^{\hat{g}_t} \ . \tag{5.5}$$

To characterize the decision problem of the government that enters the current period with debt d_t and government spending g_t, it is useful to define the value of having the option to default, $v^o(d_t, g_t)$, as follows

$$v^o(d_t, g_t) = \max_{\{c_t, \text{def}\}} \left\{ v^c(d_t, g_t), v^{\text{def}}(g_t) \right\} \ . \tag{5.6}$$

Here, $v^c(d_t, g_t)$ is the continuation value associated with not defaulting, while $v^{\text{def}}(g_t)$ is the value of repudiating debt, that is, setting $d_{t+1} = 0$, defined recursively as

$$v^{\text{def}}(g_t) = U(g_t) + \beta \int_{g_{t+1}} \left[\theta v^o(0, g_{t+1}) + (1 - \theta) v^{\text{def}}(g_{t+1}) \right] f(g_{t+1}, g_t) dg_{t+1} \ . \tag{5.7}$$

Here g_{t+1} denotes next period's government consumption. The continuation value of not

Table 4: Parameter Values

Parameter	r	σ	β	θ	$\bar{y}$	$\bar{y}^{\text{def}}$	$\bar{g}$	ρ^g	σ_g
Value	1.70%	2	0.953	0.282	1	0.969	0.2	0.945	0.025

Note: Parameter values except for $\bar{g}$ are taken from Arellano (2008).

defaulting, in turn, is given by

$$v^c(d_t, g_t) = \max_{\{d_{t+1}\}} \Big\{ u\left(y_t + q(d_{t+1}, g_t)d_{t+1} - d_t - g_t\right) + \beta \int_{g_{t+1}} v^o(d_{t+1}, g_{t+1}) f(g_{t+1}, g_t) dg_{t+1} \Big\} . \tag{5.8}$$

Hence, exactly as in Arellano (2008), the government decides on the optimal level of borrowing and on whether to repay in order to maximize household utility. In doing so, it is also constrained not to run Ponzi schemes. In contrast to the original Arellano (2008) model, we study the effect of exogenous variations of government spending, which translate into output changes whenever $\epsilon > 0$.

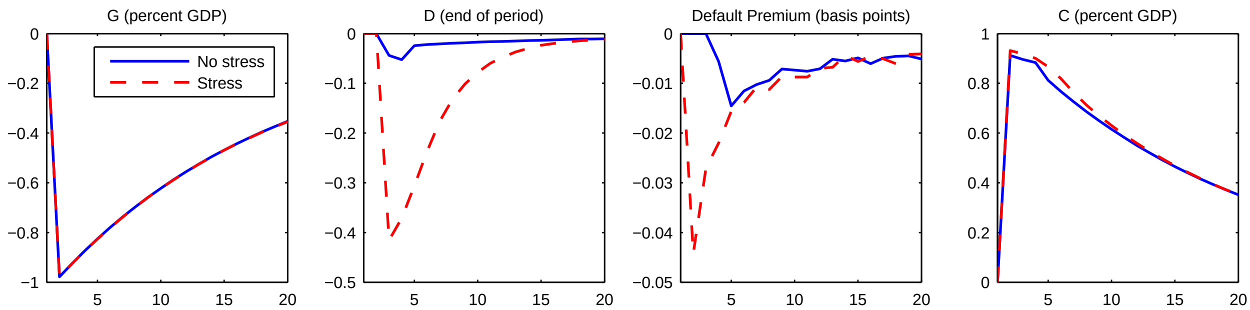
We skip the definition of a recursive equilibrium, because it is isomorphic to the one in Arellano (2008). We also adopt her assumptions regarding functional forms and choose $U(c_t) = c_t^{1-\sigma}/(1-\sigma)$. Regarding government spending, we assume an AR(1)-process

$$\hat{g}_t = \rho^g \hat{g}_{t-1} + \varepsilon_t^g,\ \varepsilon_t^g \sim \mathcal{N}(0, \sigma_g^2) . \tag{5.9}$$

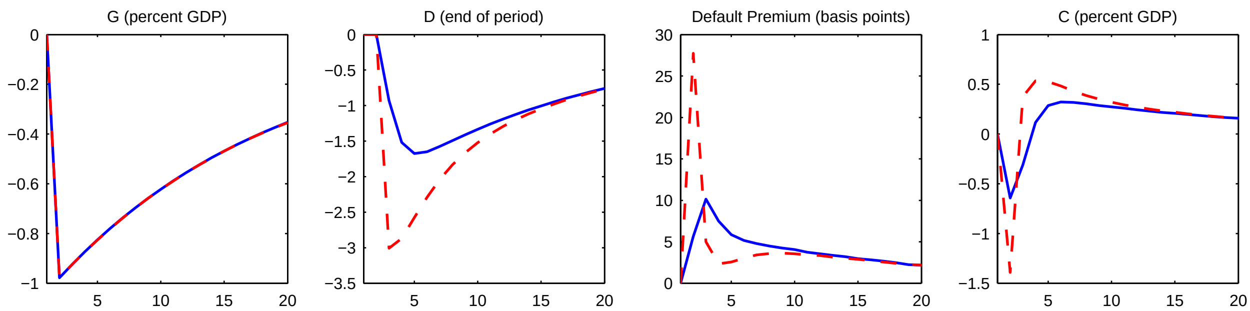
The model is set up at annual frequency. In terms of parameter values we also stick to Arellano's original calibration. Moreover, we assume $\bar{g} = 0.2$. The remaining parameter values are summarized in Table 4. The model is solved by discretizing the AR(1)-process into a 101-point Markov chain in the range of $\pm 4\ \sigma_g$ and using value function iteration on a 1000-point grid for debt on $[0, 0.33]$.[48]

Our empirical analysis provides two findings regarding the short run. First, the response of default premia to government spending shocks differs depending on the initial condition of fiscal sustainability. Premia increase in times of fiscal stress and fall in benign times. Second, the government spending multiplier on output tends to be large during

[48]Our grid is notably finer than the one in Arellano (2008), which alleviates the numerical issues discussed in Hatchondo, Martinez, and Sapriza (2010).



(a) $\epsilon = 0$



(b) $\epsilon = 0.7$

Figure 7: Dynamic response to an exogenous cut of government consumption by 1 percent of GDP: model simulations. Notes: Horizontal axes represent years. Vertical axes represent deviations from unshocked path. Solid line: debt initially low; dashed line: debt initially high.

times of fiscal stress and small in benign times. We now use the model to disentangle the role of the government spending multiplier and the role of the initial conditions for the response of default premia to cuts in government consumption.

In our model simulations, we contrast results for the case where there is no multiplier effect ($\epsilon = 0$) and the case where $\epsilon = 0.7$, which is in the middle of the range of estimates reported above. Regarding initial conditions, we also consider two possibilities. In the first case, the economy enjoys benign times, that is, debt and the default premium, given by $q^{-1} - (1 + r)$, are negligible. As a practical matter we assume that debt stands at one percent of the ergodic mean once government consumption is cut. In the second case, the level of debt is so high that the economy is at the brink of default, that is, debt is at the highest level outside the default set.

Figure 7 displays generalized impulse response functions which capture the dynamics triggered by a cut of government consumption equal to one percent of GDP.[49] The panels

[49] Generalized impulse responses account for the nonlinearity of the model (see Koop, Pesaran, and

in the top row are obtained assuming that there is no multiplier effect ($\epsilon = 0$). The bottom row shows results in the presence of a multiplier of 0.7. In each row, the first panel displays the dynamics of government spending, the second panel shows end-of-period debt, the third panel depicts the response of the default premium, while the final panel shows the response of private consumption. The default premium is measured in basis points, while the other variables are measured in percent of steady state output. The line style differentiates the initial condition in terms of fiscal stress: solid lines are computed under the assumption that austerity takes place during benign times; dashed lines represent the scenario of fiscal stress.

The dynamics of government spending are exogenous and identical across all four scenarios. There are perceptible differences in the adjustment of debt, however. Under all four scenarios, the economy's level of borrowing is reduced as government consumption declines. In the absence of a multiplier effect, that is, in the case depicted in the top row, the reduction of government consumption implies that more resources are available for private consumption and saving, that is, a reduction of debt. As can be seen from the graph, under the optimal policy we indeed observe some deleveraging as well as a sizable increase of private consumption. However, if the economy experiences fiscal stress, there is an additional incentive to deleverage in order to avoid potential default costs in the future. Debt and, associated with this, default premia decrease more strongly relative to a scenario of benign times.

Results differ fundamentally in the presence of a positive multiplier effect (bottom row). In this case output declines with government spending, offsetting the direct increase in resources available for private consumption and debt repayment. As a result, default becomes a relatively more attractive option. This is reflected in a rising default premium, in particular if the economy experiences fiscal stress to begin with. In order to avoid default, debt levels have to be reduced strongly. In fact, so much so that private consumption has to be cut as well. Note also that—in line with our empirical findings—the default

Potter, 1996). Specifically, responses are computed on the basis of stochastic simulations comparing the dynamics after a deterministic shock at time 1 and the dynamics in the absence of a shock. We set the initial $\hat{g}_t$ to its mean of 0 and average over 100,000 replications. To make the model responses comparable to the empirical responses, we exclude default episodes.

premium exhibits hump-shaped dynamics, even though investors are perfectly rational and market efficiency holds. This is because default incentives change gradually with the state of the economy.

Overall, our model simulations show that the empirical results for the short run can be rationalized within a version of the workhorse model of optimal sovereign default. As a caveat we note that the model is currently not rich enough for the multiplier to change endogenously with the level of fiscal stress and hence to mimic our empirical findings simultaneously along all dimensions. Moreover, our small open economy model endogenously features a debt-elastic interest rate premium and is thus stationary. It is therefore not able to capture permanent effects of temporary shocks. We intend to take this up in future work. Still, our model simulations give rise to an important insight: default incentives and, hence, default premia may increase in response to austerity whenever it impacts economic activity, and hence the overall amount of domestic resources, adversely. This effect is likely to be particularly strong if the economy is already operating at the brink of default.

6 Conclusion

Does austerity reduce sovereign default premia? In pursuing this question, we make two distinct contributions. First, we set up a new data set which contains data on sovereign default premia for 38 emerging and advanced economies. We assemble quarterly observations for an unbalanced panel from 1990 to 2014, not only for default premia, but also for government consumption and output. A first look at the data allows us to establish a number of basic facts. First, while there is a large variation in default premia, both across time and countries, they are moderate for the largest part of our sample. Second, default premia are strongly countercyclical. The correlation of default premia and current output growth is negative in all 38 countries. Third, across countries there is no systematic correlation pattern for default premia and government consumption.

As a second contribution, we assess how default premia respond to cuts of government consumption. If we do not condition on the state of the economy, we find that a cut of government consumption raises premia in the short run. At the same time output declines. However, it turns out that these results mask considerable heterogeneity. If we condition estimates on fiscal stress, captured by high default premia, we find that spending cuts have a stronger effect on default premia and output. In this case, reducing government consumption by one percent of GDP increases the default premium by about 40 basis points in the short run. The fiscal multiplier is around unity in times of fiscal stress. Instead, if the economy initially enjoys more benign times, spending cuts reduce default premia (by about 20 basis points) and leave output virtually unaffected.

The data thus reveal a very robust pattern: default premia tend to move negatively with output—both unconditionally and conditional on fiscal shocks. Hence, to the extent that austerity impacts economic activity adversely, it likely fails to bring about a reduction in default premia. We show that this finding can be rationalized within the workhorse model of optimal sovereign default. More generally, our finding supports the view that financial markets are primarily concerned with output growth.

This is also consistent with our findings for the long run. In particular, if cuts of government consumption take place in times of fiscal stress, we observe premia to reach their peak response after about 1.5 years. Afterwards they come down rather quickly, about at the same time as output rebounds. Importantly, premia undershoot their initial level considerably: a cut of government consumption by one percent of GDP reduces the default premium by some 80 basis points in the long run. Premia also decline in the long run if austerity takes place during benign times. In this case, however, the effect is not statistically significant.

In sum, austerity pays off. Yet, if it takes place during times of fiscal stress, benefits materialize only in the medium to long run. From the perspective of a structural model, this is not because investors are impatient or even schizophrenic about austerity. Rather, financial market participants understand that a government may be tempted to default on its debt obligations as long as austerity deprives the economy of much needed resources. Still, if a government manages to steer through the dire straits of austerity, it will be rewarded by reduced default premia in the long run.

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A Appendix

A.1 Details on the construction of the default premia

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

A.1.1 EMBI spreads

EMBI spreads are one of the four components used to construct the default premia. The J.P. Morgan Emerging Markets Bond Index (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 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, assuring that the liquidity premium compared to U.S. bonds is not too large.[50]

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.1.2 CDS data

We also use CDS spreads to measure default premia. They are taken from Datastream and spliced from two sources. Until 2010Q3, Datastream provides CDS from Credit Market Analysis Limited (CMA), while Thomson Reuters, starting in 2008 provides CDS for an increasing number of issuers.[51] The contract type we choose is five years of maturity with complete restructuring (CR). The CMA CDS are typically denominated in dollar, while the Thomson Reuters CDS are often available in euro and dollar. Despite CDS 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

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

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

reimbursed in U.S. dollar when Germany defaults may provide an insurance against associated exchange rate risks due to changes in value of the euro (for more on this and CDS in general, see, e.g., Buchholz and Tonzer, 2013; Fontana and Scheicher, 2010). To exclude exchange rate risk premia, we use Thomson Reuters CDS in U.S. dollar for all non-EMU countries and Thomson Reuters Euro CDS in euro for euro area members after EMU accession. Unfortunately, for early time periods, the currency-specific Thomson Reuters CDS are not always available. In this case, we rely on the CMA CDS spreads.

A.1.3 Spread decomposition

In the main text, we argue that taking the difference between nominal yields on foreign currency bonds and a risk-free reference bond should be sufficient to isolate default premia. 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

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

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.[52] 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 ε_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 premiium should be the same as in Equation (A.1) due to considering a bond in the same currency with the same maturity.[53] A yield spread computed this way will thus only contain the default-related premium and the difference in liquidity risk premia and higher order terms. Unfortunately, it is not easily possible 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, risk premia consist of the price of risk

[52] Like all risk premia, this is a second order effect arising from the covariance of returns with the stochastic discount factor. Thus, risk premia like this would be 0 if all investors were risk neutral.

[53] Regarding term premia, it is actually the duration of expected cash flows that matters. This might introduce small differences in term premia (see Broner, Lorenzoni, and Schmukler, 2013).

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.2.). Finally, even considering only a sample of advanced economies with very liquid markets or ending the sample before the recent financial crisis where liquidity did dry up does not qualitatively affect our results.

A.2 Price of risk and quantity of risk

Our measure of default premia 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, Hoerova, and Duca, 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 our 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, the time series dimension is affected as the price of risk will be simultaneously high for all countries at a particular point in time. However, results are similar when dropping the Great Recession where the price of risk suddenly spiked, alleviating concerns about this potential confounding factor.

A.3 Benefits in kind

Both the System of National Accounts 1993 (European Commission et al., 1993) and its more recent version, the System of National Accounts 2008 (European Commission et al., 2009) specify that "Social benefits in kind corresponding to purchases of products supplied to households via market producers" must be included in government consumption.[54] All countries in our sample use these frameworks. The item considered, [D6 311 + D63 121 + D63 131]/S13, covers for example the reimbursement of private households' consumption of health services by privately operating doctors through government run insurance systems. Private doctors are market producers and their services that are indirectly supplied to households by the government are social benefits provided in kind. Unfortunately, this item is potentially cyclical if the government for example provides unemployed persons with health care benefits that fall into this category. Ideally, one would like to exclude such items, but unfortunately this is impossible on a consistent cross-country basis due to institutional differences. For instance, some countries (e.g., the UK), provide such benefits in kind not via market producers, but (partially) via a nationalized health care system. In this case, excluding the benefits in kind provided via market producers is of no help, because the benefits provided are contained in the government wage bill.

[54] This also holds true for the EU implementation in the European System of National Accounts 1995 (European Commission, 1996) and 2010 (European Commission, 2013).

B Additional tables and figures

Table A.1: Descriptive statistics: indicator functions

Country	$mean\ (F^{stress})$	$mean\ (F^{recess})$	$corr\ (F^{stress}, F^{recess})$
Argentina	0.83	0.53	0.60
Australia	0.40	0.56	0.69
Austria	0.42	0.48	0.34
Belgium	0.53	0.51	0.40
Brazil	0.70	0.53	0.19
Bulgaria	0.48	0.52	0.70
Chile	0.28	0.51	0.63
Colombia	0.56	0.54	0.30
Croatia	0.34	0.45	0.53
Czech Republic	0.09	0.46	0.66
Denmark	0.52	0.52	0.32
Ecuador	0.92	0.50	0.05
El Salvador	0.61	0.45	0.55
Finland	0.47	0.44	0.31
France	0.41	0.47	0.26
Germany	0.28	0.49	0.10
Greece	0.60	0.55	0.70
Hungary	0.31	0.44	0.43
Ireland	0.50	0.51	0.13
Italy	0.62	0.51	0.25
Latvia	0.44	0.51	0.46
Lithuania	0.39	0.46	0.46
Malaysia	0.30	0.51	0.53
Mexico	0.57	0.49	0.11
Netherlands	0.38	0.50	0.37
Peru	0.58	0.56	0.30
Poland	0.34	0.53	0.44
Portugal	0.50	0.52	0.12
Slovakia	0.56	0.44	0.80
Slovenia	0.59	0.45	0.64
South Africa	0.43	0.48	0.56
Spain	0.49	0.51	0.55
Sweden	0.49	0.52	0.04
Thailand	0.23	0.52	0.40
Turkey	0.67	0.51	0.22
United Kingdom	0.56	0.44	0.34
United States	0.40	0.55	-0.16
Uruguay	0.60	0.51	-0.05

Notes: F^{stress} denotes the values of the country group-specific empirical CDF of the lagged default premium; F^{recess} denotes the empirical CDF of the the smoothed output gap, computed as the z-scored deviation of the 5 quarter moving average of the output growth rate from its HP-filtered trend ($\lambda = 160{,}000$). First column: average value of the fiscal stress indicator for the respective country. Second column: average value of the recession indicator for the respective country. Last column: correlation between the two indicators. Positive values indicate that fiscal stress is higher when the economy is deeper in a recession.

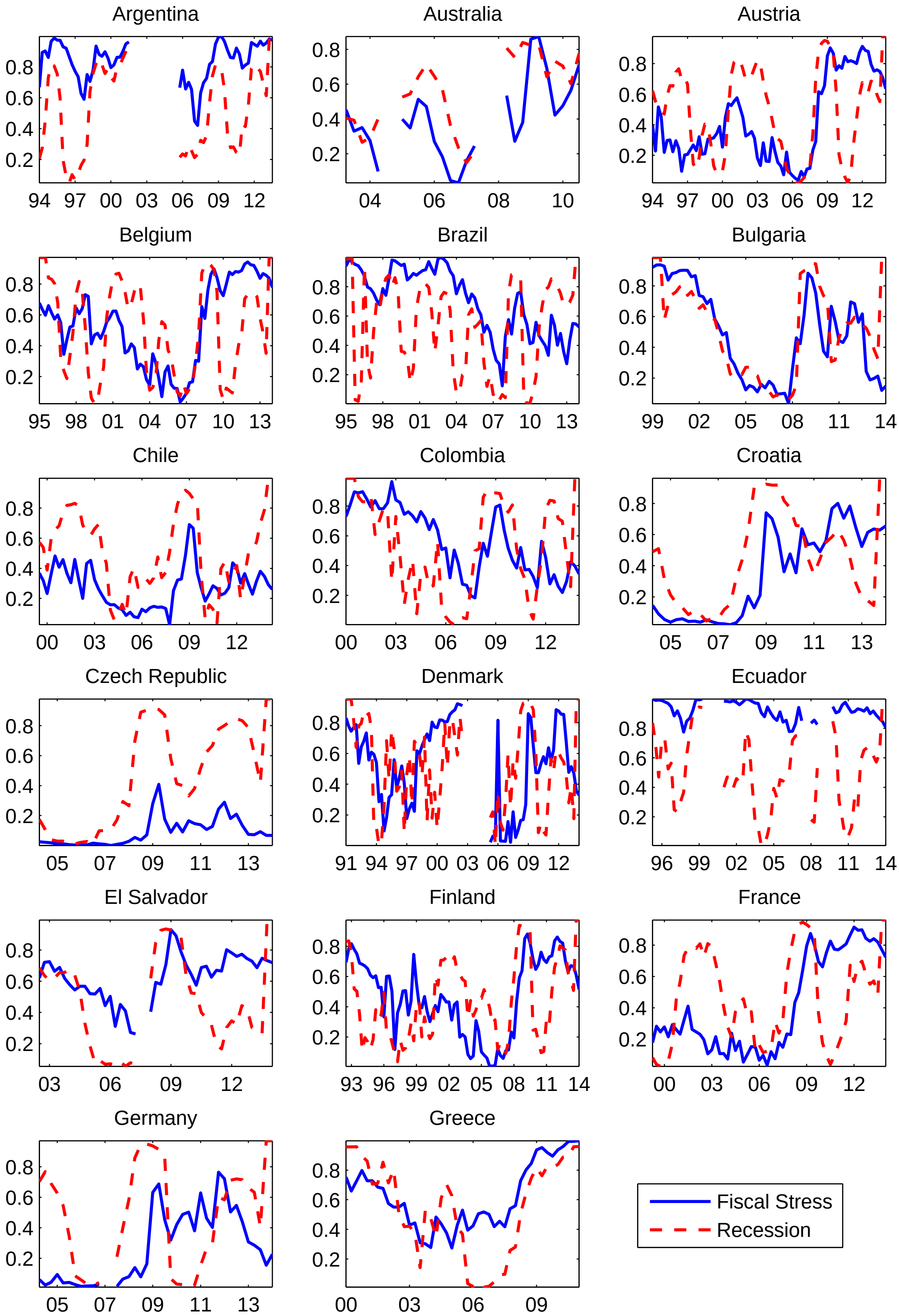


Figure A.1: Country group-specific empirical CDF values for lagged default premia and smoothed output gaps.

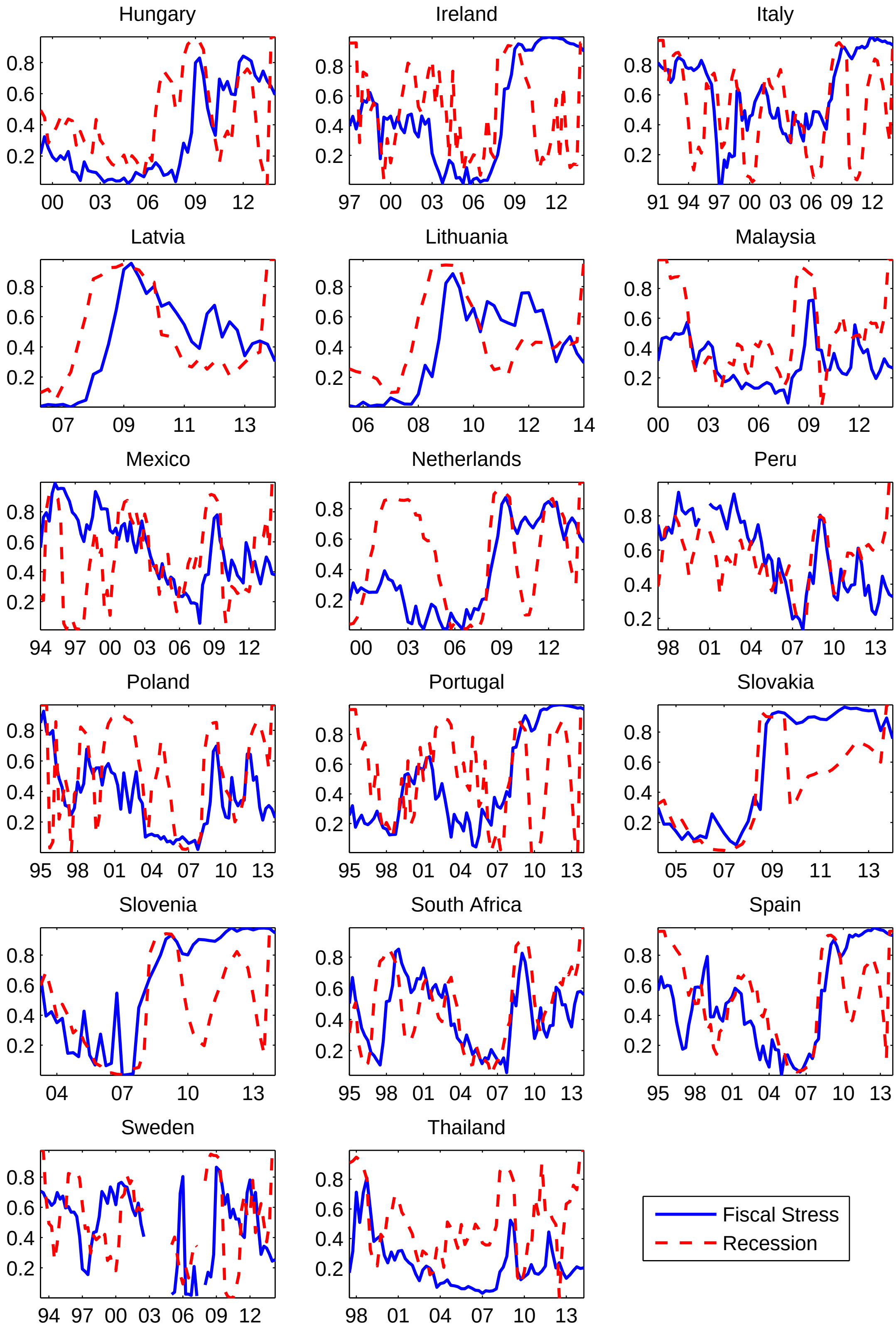


Figure A.2: Country group-specific empirical CDF values for lagged default premia and smoothed output gaps.

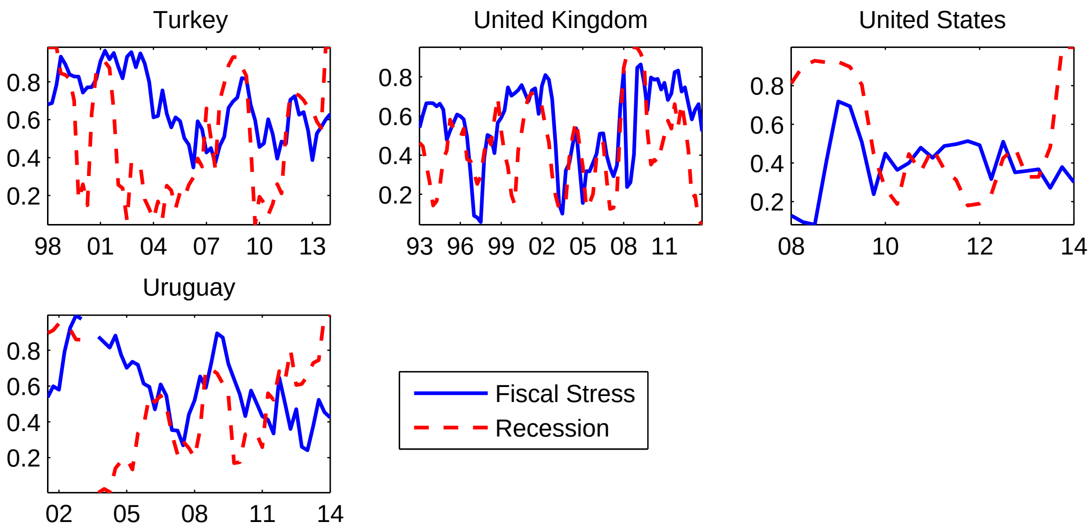


Figure A.3: Country group-specific empirical CDF values for lagged default premia and smoothed output gaps.

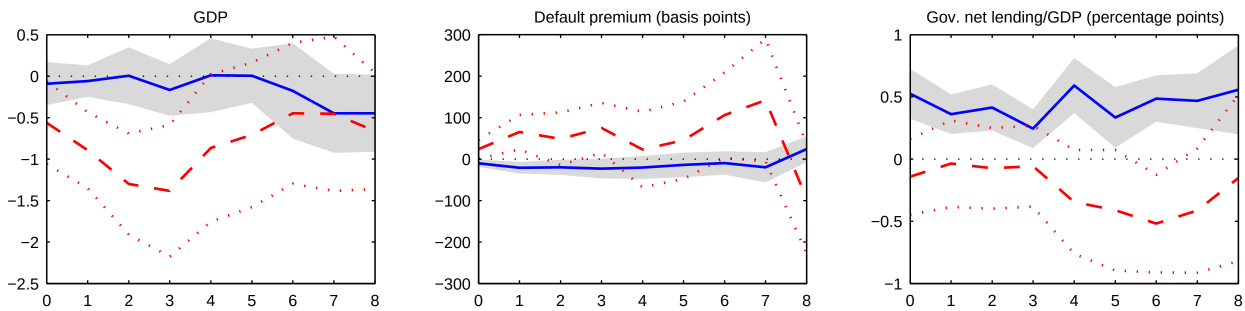
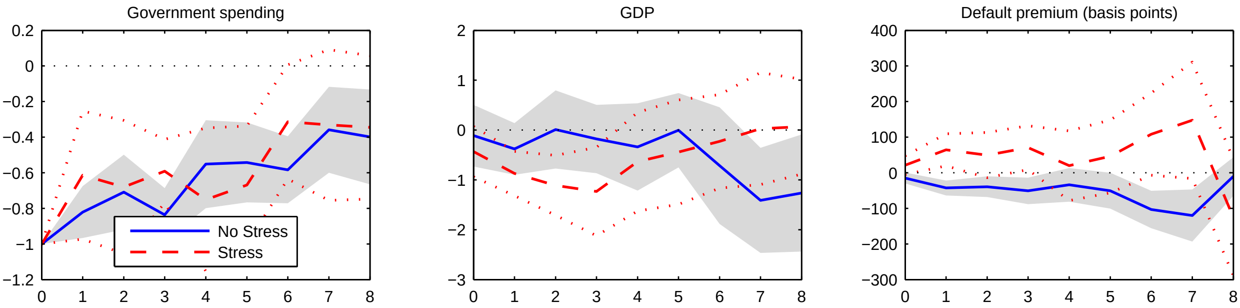
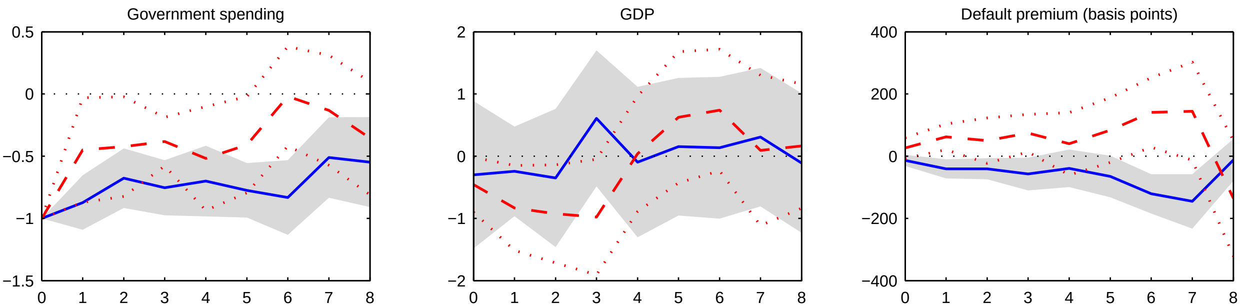


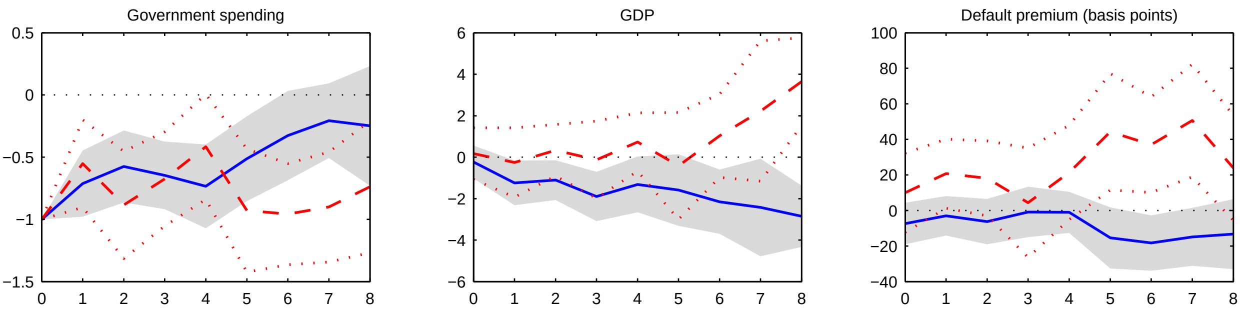
Figure A.4: Dynamic response to an exogenous cut of government consumption by 1 percent of GDP: model including the net-lending-to-GDP ratio.



(a) Euro area

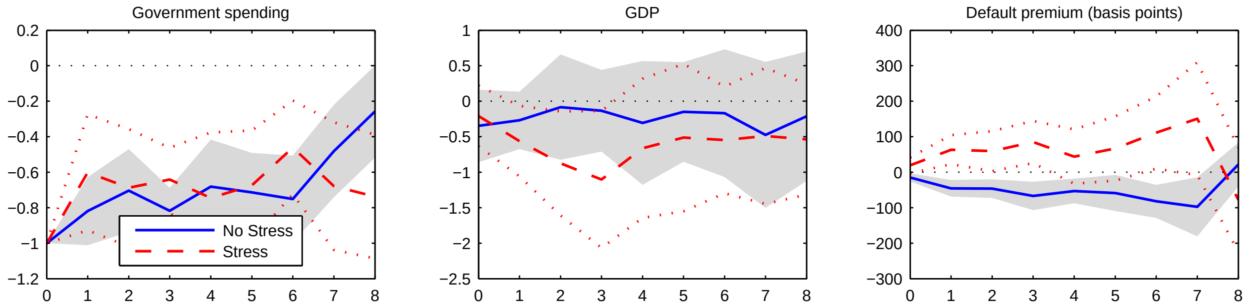


(b) Euro area: crisis countries

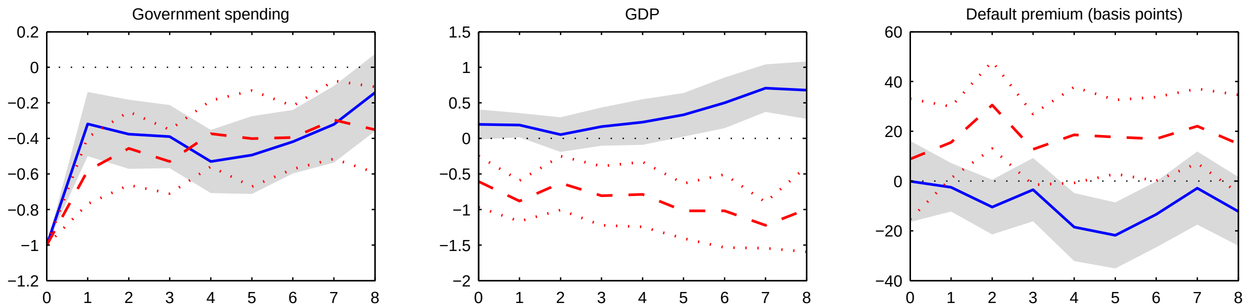


(c) Euro area: non-crisis countries

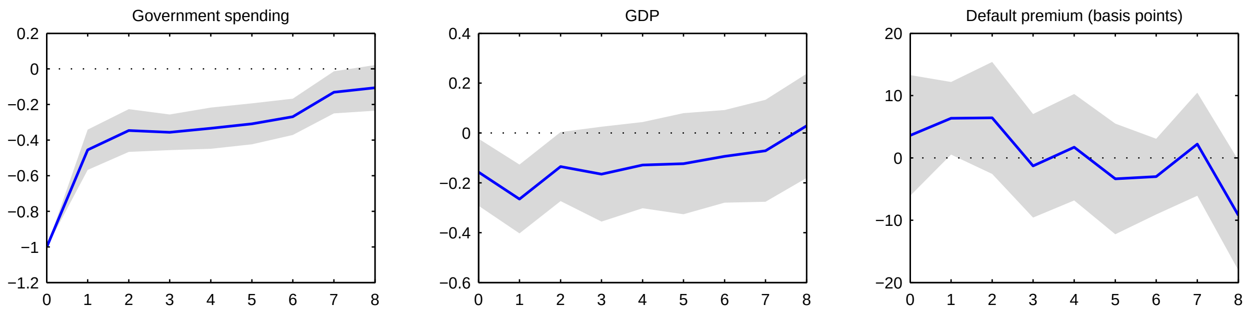
Figure A.5: Dynamic response to an exogenous cut of government consumption by 1 percent of GDP: euro area samples.



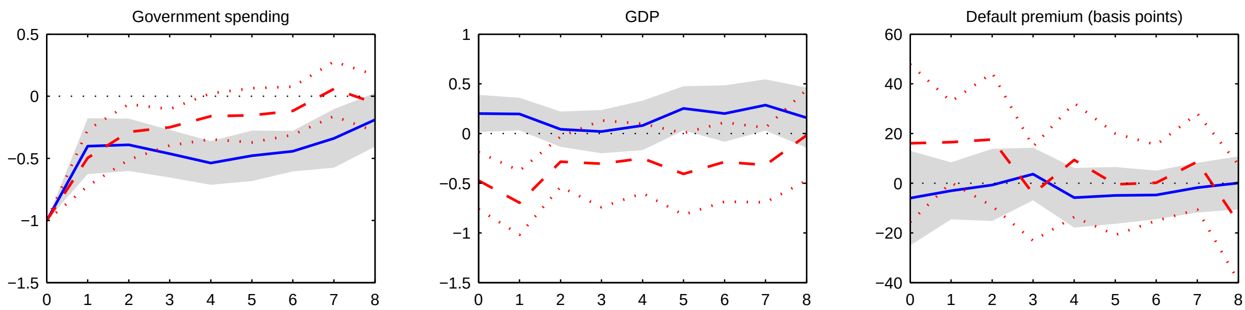
(a) Advanced economies



(b) Emerging economies

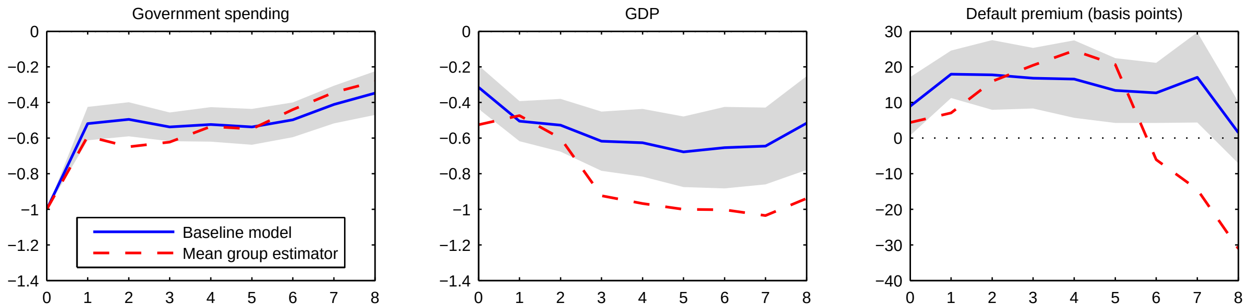


(c) Excluding Great Recession: unconditional

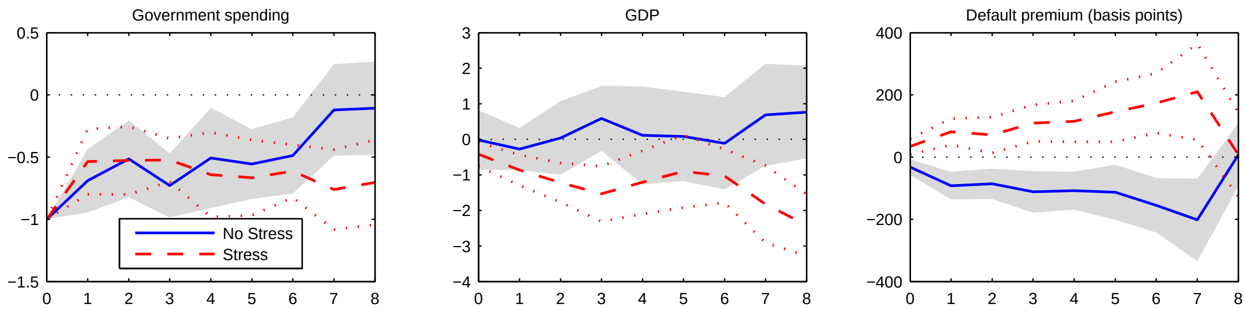


(d) Excluding Great Recession: conditional on fiscal stress

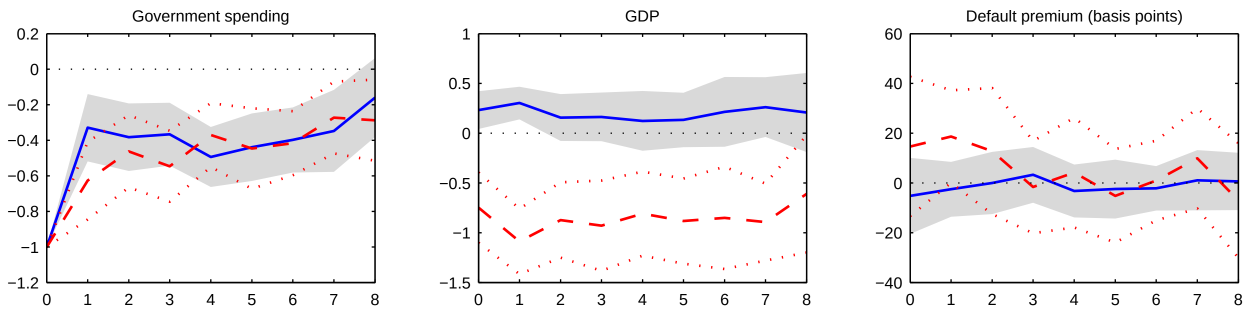
Figure A.6: Dynamic response to an exogenous cut of government consumption by 1 percent of GDP: advanced and emerging economies separately (Panels (a) and (b)) and excluding the Great Recession (Panels (c) and (d)).



(a) Unconditional model: baseline vs. mean group estimator

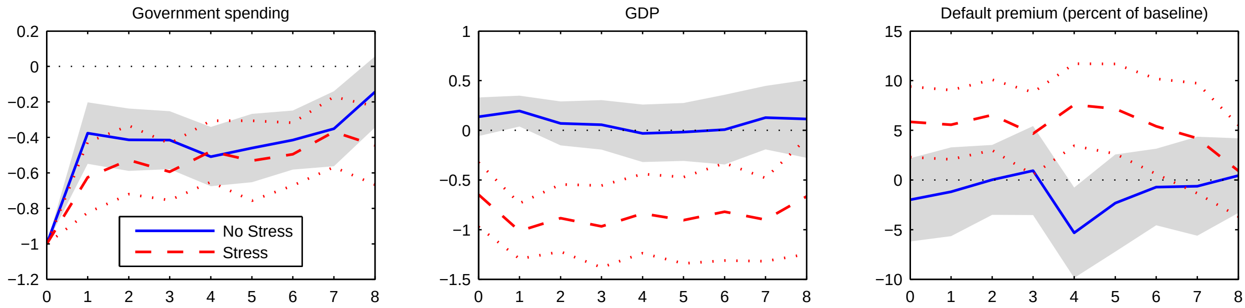


(b) Monetary union or dollarization

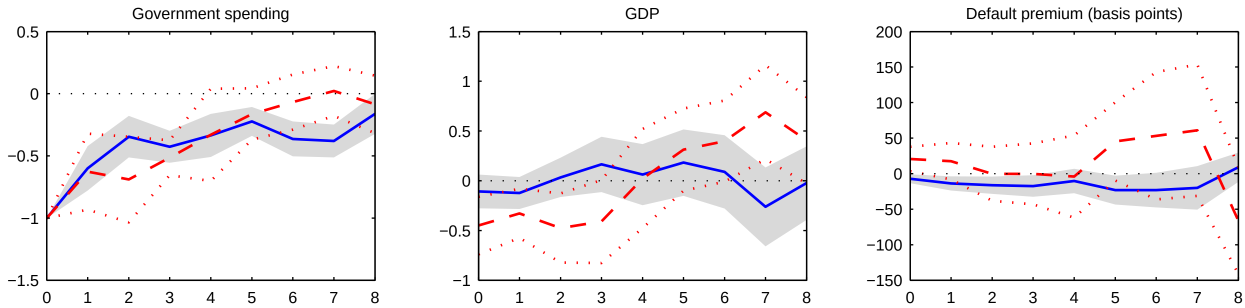


(c) Countries with their own legal tender

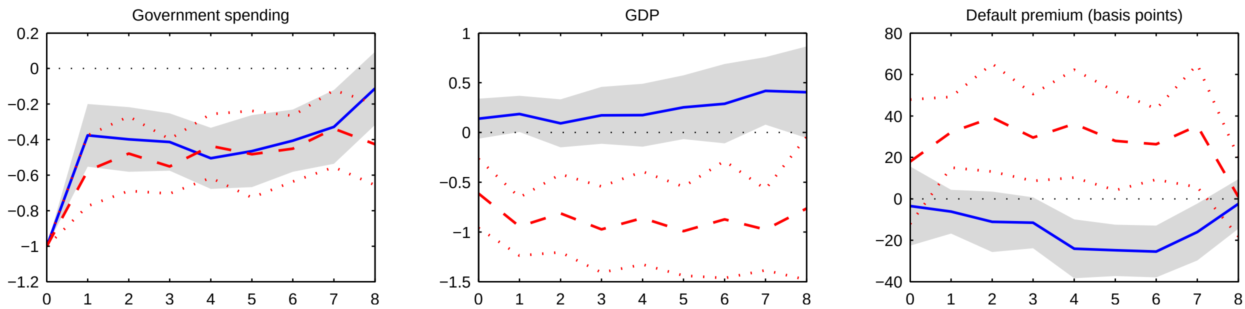
Figure A.7: Dynamic response to an exogenous cut of government consumption by 1 percent of GDP. Panel (a): comparison of unconditional baseline estimates with those obtained by mean group estimation. Panel (b): restricting the sample to country-quarter observations that were members of monetary unions or de jure dollarized. Panel (c): restricting the sample to country-quarter observations with their own legal tender.



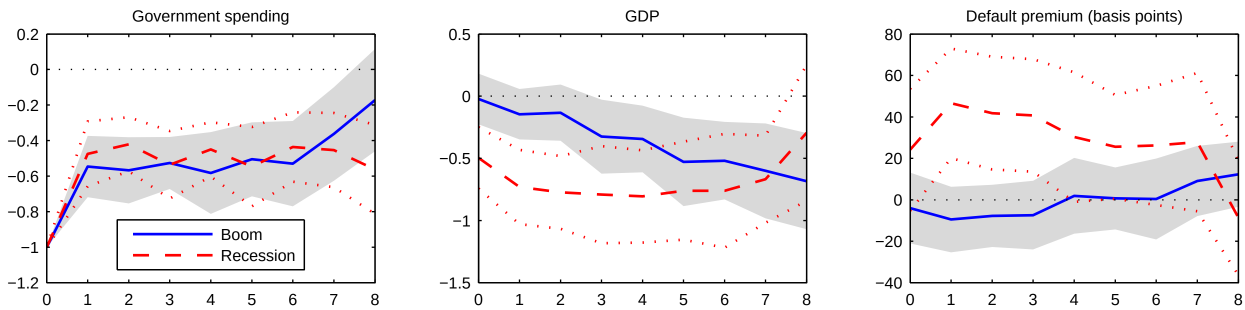
(a) Default premia measured in logs



(b) Government consumption without benefits in kind



(c) Conservative sample for government consumption



(d) Boom vs. recession

Figure A.8: Dynamic response to an exogenous cut of government consumption by 1 percent of GDP. Panel (a): default premia measured in logs. Panel (b): using government consumption without benefits in kind. Panel (c): conservative sample where we could confirm that government spending data was derived from direct sources. Panel (d): conditioning on booms and recessions (output gap used as indicator variable).