

Does austerity pay off?*

Benjamin Born, Gernot J. Müller, Johannes Pfeifer

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Abstract

Austerity measures are frequently enacted when the sustainability of public finances is in doubt. Such doubts are reflected in high sovereign yield spreads and put further strain on government finances. Is austerity successful in restoring market confidence, bringing about a reduction in yield spreads? We employ a new panel data set which contains sovereign yield spreads for 26 emerging and advanced economies and estimate the effects of cuts of government consumption on yield spreads and economic activity. The conditions under which austerity takes place are crucial. During fiscal stress or during recessions we find that it fails to bring about a reduction in spreads. In contrast, austerity pays off, if conditions are more benign.

Keywords: Fiscal Policy, austerity, sovereign risk, yield spreads

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*Prepared for the SAFE Research Conference “Austerity and Growth: Concepts for Europe”. Born: University of Mannheim and CESifo, born@uni-mannheim.de, Müller: University of Bonn and CEPR, gernot.mueller@uni-bonn.de, Pfeifer: University of Mannheim, pfeifer@uni-mannheim.de. We thank Kerstin Bernoth for very helpful discussions. Andreas Born, Diana Schüler and Alexander Scheer have provided excellent research assistance. Gernot Müller also thanks the German Science Foundation (DFG) for financial support under the Priority Program 1578. The usual disclaimer applies.

“Debt is a slow-moving variable that cannot—and in general should not—be brought down too quickly. But interest rates can change much more quickly than fiscal policy and debt.” (C. Reinhart and K. Rogoff 2013)

“[F]inancial investors are schizophrenic about fiscal consolidation and growth. They react positively to news of fiscal consolidation, but then react negatively later, when consolidation leads to lower growth—which it often does.” (O. Blanchard 2011)

1 Introduction

In the years following the global financial crisis, many European countries have been implementing sizeable austerity measures. These measures were taken in order to confront mounting concerns about rising levels of public debt and deficits or outright solvency issues. In fact, in a number of euro area countries sovereign yield spreads relative to Germany started to take off by 2010, arguably leaving policy makers with no alternative course of action. Yet a further deterioration of financial market conditions, coupled with dismal growth performance in the following years, lead many observers to question the wisdom of austerity. In this paper, we ask whether austerity pays off, that is, whether it actually helps restoring market confidence in the sustainability of public finances as reflected by sovereign yield spreads.

In our analysis we sidestep the question of how austerity measures impact the actual health of government finances. Rather, we focus on financial markets’ assessment, that is, we investigate whether yield spreads rise or fall in response to austerity. Some observers suggest that financial markets are “schizophrenic” about austerity in that they seemingly demand austerity measures, but fail to reward them, because consolidation slows down output growth (Blanchard, 2011; Cotarelli and Jaramillo, 2012). Output growth is a key factor determining debt sustainability not only because it improves the ability of debtors to service the debt, but also because it is likely to raise their willingness to do so (see, e.g., Bi, 2012; Arellano, 2008).

Of course, in order to stabilize public finances there seem to be few alternatives to austerity, that is, lowering government expenditures or raising taxes.¹ Still, given that output growth is crucial for (the perceived) debt sustainability, ill-timed austerity may fail to reach its

¹Historically, in addition to primary surpluses, output growth as well as negative real interest rates have contributed to the reduction of debt-to-GDP ratios (Hall and Sargent, 2011). Real interest rates in turn may have been depressed due to “financial repression” (Reinhart and Sbrancia, 2014).

objective, because the output effects of austerity are likely to depend on the state of the economy. For instance, if there is pervasive slack in the economy, contractionary fiscal measures may have particularly adverse effects on economic activity (Auerbach and Gorodnichenko, 2012). In this case, delaying austerity may be beneficial in terms of macroeconomic outcomes (Corsetti, Kuester, Meier, and Müller, 2010). More extreme still, hysteresis effects may make austerity measures self-defeating to the extent that contractionary fiscal measures may raise the long-run financing costs of governments (De Long and Summers, 2012).

In order to measure financial markets' assessment of debt sustainability we rely on sovereign yield spreads. In a first step of our analysis, we compute spreads for 26 countries as the difference in sovereign yields vis-à-vis a "riskless" reference country where sovereign default is considered to be extremely unlikely. 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 an outright default. It turns out that our spread measure moves in close sync with credit default swap spreads which are available only for parts of our sample. Moreover, our measure of yield spreads directly speaks to the question of whether austerity pays off, as it captures directly how financial market expectations of government solvency affect real borrowing costs.

In a first step of our analysis, we establish a number of basic facts. First, yield spreads do not exhibit a particular behavior during times which have been classified as fiscal consolidation episodes in earlier work by Alesina and Ardagna (2013). Second, yield spreads comove negatively with economic activity. The correlation of yield spreads and current output growth is negative in all countries of our sample, but Sweden. Third, there is no systematic correlation pattern emerging for yield spreads and government consumption.

In a second step, we investigate the effects of cuts in government consumption in a panel of 26 emerging and advanced economies. We focus on the effects of government consumption cuts, as identification is somewhat less controversial in this case. Specifically, 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 spending cannot be agreed upon without a considerable decision lag. We collect quarterly data for government expenditure while drawing on earlier work by Ilzetzki, Mendoza, and Végh (2013) and extending their data set to include observations up to 2013. 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 government consumption impacts the economy and, eventually, sovereign yield spreads. Following a large empirical literature on the fiscal transmission mechanism, we rely on estimated panel vector autoregressions (VAR) to compute impulse response functions. In addition, we also employ local projections which, following Jordá (2005), have become very popular in recent years. For both models we compute unconditional estimates for the effects of fiscal shocks as well as estimates conditional on the state of the economy. In this regard, local projects stand out in terms of flexibility (Auerbach and Gorodnichenko, 2013b).

Our main results can be summarized as follows. We find that austerity, or more precisely, cuts of government consumption, tend to raise sovereign yield spreads, if we do not condition our estimates on the state of the economy. At the same time output declines considerably. Upon closer inspection, however, these results mask considerably heterogeneity. It turns out that spending cuts reduce spreads, provided that countries experience benign times, that is the absence of fiscal stress or during booms. The same holds for flexible exchange rate regimes. At the same time, we find fiscal multipliers much reduced. In fact, we cannot reject the hypothesis that government consumption leaves economic activity unaffected under these conditions. On the other hand, in the presence of fiscal stress, during recessions or under fixed exchange rate regimes, spending cuts raise spreads and depress economic activity. Hence, and perhaps surprisingly, we find the comovement of spreads and economic activity remains negative, once we condition on fiscal shocks. This holds even independently of the economic environment.

Our results are based on exogenous variations in government consumption, while austerity is typically considered to be an endogenous response to, say, financial market developments. That said, note that identifying an exogenous variation in government consumption is key to isolate the impact of austerity on the variables of interest *per se*, rather than the joint effect of financial market developments *cum* austerity. Still, 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. Conditioning on the effects of spending cuts on the state of the economy is our strategy to address such concerns.

Alternative, and complementary, approaches to assess the effects of fiscal consolidation episodes include case studies, notably the classic analysis of Giavazzi and Pagano (1990), or, more recently, Perotti (2013). 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 in which specific consolidation episodes are singled out through a close reading of the actual policy process (Devries, Guajardo, Leigh, and Pescatori, 2011).

Our analysis provides new results by investigating the effects of government spending cuts on sovereign yield spreads for a large panel of advanced and emerging economies. Related studies include numerous attempts to assess the effects of fiscal policy on interest rates. In particular, Ardagna (2009), using the Alesina and Perotti (1995) approach, shows 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) use a similar measure for sovereign yield spreads as we use in the present paper. They focus on emerging market economies, however, and assess the contemporaneous impact of fiscal variables on spreads within a given year.

The remainder of the paper is organized as follows. Section 2 provides a detailed analysis of our data set. In particular, in this section we aim at establishing a number of basic facts regarding the time-series properties of sovereign yield spreads 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 in Section 4. Section 5 concludes.

2 Data

Our analysis is based on a new data set. It contains quarterly data for government consumption, output, and sovereign yield spreads for 26 emerging and advanced economies. The use of quarterly data is key to our analysis below. While data on yield spreads are available at higher frequency, data on macroeconomic aggregates are not. In fact, for a long time, time-series studies of the fiscal transmission mechanism have been limited to a small set of countries, because data for government consumption has not been available at (non-interpolated) quarterly frequency.² In an important contribution, Ilzetzki, Mendoza, and Végh (2013) have collected original quarterly data for government consumption for 44 countries, as opposed to interpolating data from annual observations.

We reconstruct quarterly data for government consumption along the lines of Ilzetzki,

²Some studies have resorted to annual data (e.g. Beetsma, Giuliodori, and Klaasen, 2006, 2008; Bénétix and Lane, 2013). In this case identification assumptions tend to be more restrictive. However, Born and Müller (2012) consider both quarterly and annual data for four OECD countries. They find that the estimated effects of government spending shocks do hardly differ.

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

Country	first obs	last obs	min	max	mean	std
Belgium	1995.00	2013.25	0.24	0.25	0.25	0.00
Denmark	1991.00	2013.25	0.25	0.29	0.27	0.01
Finland	1990.00	2013.25	0.18	0.26	0.21	0.02
France	1986.00	2013.25	0.23	0.27	0.25	0.01
Greece	2000.00	2011.00	0.17	0.23	0.18	0.01
Hungary	1995.00	2013.25	0.21	0.28	0.23	0.02
Ireland	1997.00	2013.25	0.16	0.18	0.17	0.01
Italy	1991.00	2013.25	0.19	0.22	0.20	0.01
Netherlands	1988.00	2013.25	0.23	0.28	0.25	0.01
Poland	1995.00	2013.25	0.17	0.21	0.18	0.01
Portugal	1995.00	2013.25	0.19	0.22	0.20	0.01
Slovenia	1995.00	2013.25	0.17	0.22	0.19	0.01
Spain	1995.00	2013.25	0.16	0.22	0.18	0.02
Sweden	1993.00	2013.25	0.07	0.12	0.09	0.02
United Kingdom	1986.00	2013.25	0.21	0.28	0.23	0.02
Argentina	1993.00	2013.25	0.12	0.15	0.13	0.01
Chile	1989.00	2012.75	NaN	NaN	NaN	NaN
Colombia	2000.00	2013.25	0.15	0.17	0.16	0.00
Ecuador	2000.00	2013.25	0.10	0.13	0.12	0.01
El Salvador	1994.00	2013.25	0.06	0.09	0.07	0.01
Malaysia	2000.00	2013.00	NaN	NaN	NaN	NaN
Peru	1995.00	2013.25	NaN	NaN	NaN	NaN
South Africa	1993.00	2013.25	0.13	0.18	0.15	0.02
Thailand	1993.00	2013.25	0.07	0.11	0.09	0.01
Turkey	1998.00	2013.25	0.09	0.12	0.11	0.01
Uruguay	1988.00	2013.25	NaN	NaN	NaN	NaN

Notes: Government consumption is consumption of the general government except for Chile, El Salvador, Malaysia, Peru, and Sweden, where it refers to central government consumption. For Chile, Malaysia, Uruguay, and Peru, we do not have information about the *level* of quarterly non-interpolated government consumption.

Mendoza, and Végh (2013) for the subset of countries for which we are also able to compute sovereign yield spreads. In the process, we also extend the sample to include more recent observations. Our earliest observations for which we have both spread and government consumption data is 1990Q1 for Denmark and Ecuador. Our sample runs up to 2013.

Table 1 provides summary statistics for the ratio of government consumption-to-GDP. Government consumption is exhaustive government consumption and does not include transfer payments. As in Ilzetzki, Mendoza, and Végh (2013), 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 yield spreads in order to measure market confidence in the sustainability of public finances. Given observations on quarterly government consumption, we aim to construct measures of yield spreads for as many countries as possible. As stressed in the introduction, we construct yield spreads using yields for securities issued in common currency. To the extent that goods and financial markets are sufficiently integrated, we are thereby eliminating fluctuations in yields due to changes in real interest rates, inflation expectations and the risk premia associated with them. In addition to a default risk premium, yield spreads may still reflect a term and liquidity premium. However, 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. Moreover, any liquidity premium is likely to be small in our sample.³ As a result, yield spreads should reflect primarily financial markets' assessment of the probability and extent of debt repudiation by a sovereign.

We obtain spreads following three distinct 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 of dollar-denominated government or government-guaranteed bonds of a country relative to those of U.S. government bonds.⁴ 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" according to the International Securities Market Association (ISMA) formula 6.3 from "long-term government bonds or comparable securities" with a residual maturity of close to 10 years (ideally 9.5 to 10.5 years) with a sufficient liquidity (see European Central Bank, 2004, for details). In case more than one bond is included in the sample, simple averaging over yields is performed to obtain a representative rate. 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.⁵

³Inclusion of a bond into the EMBI requires a minimum bond issue size of \$500 million, assuring that the liquidity premium compared to US bonds is not too large.

⁴In particular, we rely on stripped spreads (Datastream Mnemonic: SSPRD), which "strip" out collateral and guarantees from the calculation.

⁵The bonds used for computing the "long-term interest rate for convergence purposes" are typically bonds issued in the common currency but under national domestic law. They differ in this dimension from the EMBI and foreign currency bonds we use, 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

Finally, we also 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 countries and the pre-EMU period (see, e.g., Bernoth, von Hagen, and Schuknecht, 2012). In particular, we identify bonds denominated in either US dollar or Deutsche mark of at least 5 years of maturity by developed countries. We compute the spread of yields for those bonds relative to the yields of US or German government bonds of comparable maturity and coupon yield in order to have comparable duration and thus term premia.⁶ 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 thirty trading days before the earliest maturity date of either benchmark or the government bond.⁷ In case of several bonds being available for overlapping periods, we average over yield spreads. In order to construct quarterly time-series data, we average yield spreads over the trading days of a given quarter.⁸

Figure 1 provides some examples for how sovereign yield spreads are constructed. Considering four countries, it displays the yields of foreign currency bonds jointly with their associated benchmark bonds. For three countries (Italy, Denmark, UK), we consider bonds denominated in dollars, while for Greece we consider a bond issued in Deutsche mark.⁹ Note

EMU, the euro bonds will be converted into domestic currency bonds, implying that they should carry a depreciation/exchange rate premium that is absent in the 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).

⁶Yields on individual bonds are based on the yield to maturity at the midpoint as reported in Bloomberg or the yield to redemption in Datastream.

⁷Still, our spread measure is not obtained for constant maturities. In moving along the yield curve, we may thus pick up cross-country differences in the slope of the yield curve. In principle, this effect can be quantitatively significant (Broner, Lorenzoni, and Schmunkler, 2013). Still, as we find our spread measure to comove very strongly with CDS spreads (whenever they are available), we ignore the issue in the present paper.

⁸In applying our procedure, we seek to mimic the creation of the EMBI spreads and “long-term interest rate for convergence purposes”. We note, however, that we rely on a smaller foreign currency bond universe and cannot correct for maturity drift (Broner, Lorenzoni, and Schmunkler, 2013). Because of this, we use these data only for the shortest necessary sample, i.e. for EMU countries until the euro is introduced and the “long-term interest rate for convergence purposes” becomes available.

⁹Italy: 10year \$US bond issued on 08/02/1991 with coupon 8.75% (XS0030152895); benchmark bond: US 10 year Treasury note issued on 15/11/1990 with coupon 8.25% (US912827ZN50). Denmark: 10year \$US zero coupon bond issued on 8/6/1986 (GB0042654961); benchmark bond: US 10 year Treasury note issued on 15/08/1998 with coupon 9.25% (ISIN: US912827WN87). UK: 10year \$US bond issued on 12/9/1992 with coupon 7.25% (XS0041132845); benchmark bond: US 10 year Treasury note issued on 06/05/1992 with coupon 7.5% (US912827F496). Greece: 10year DEM bond issued on 11/13/1996 with coupon 6.75% (DE0001349355); benchmark bond: German 15 year bond issued on 04/10/1996 with coupon 6.25% (DE0001135010).

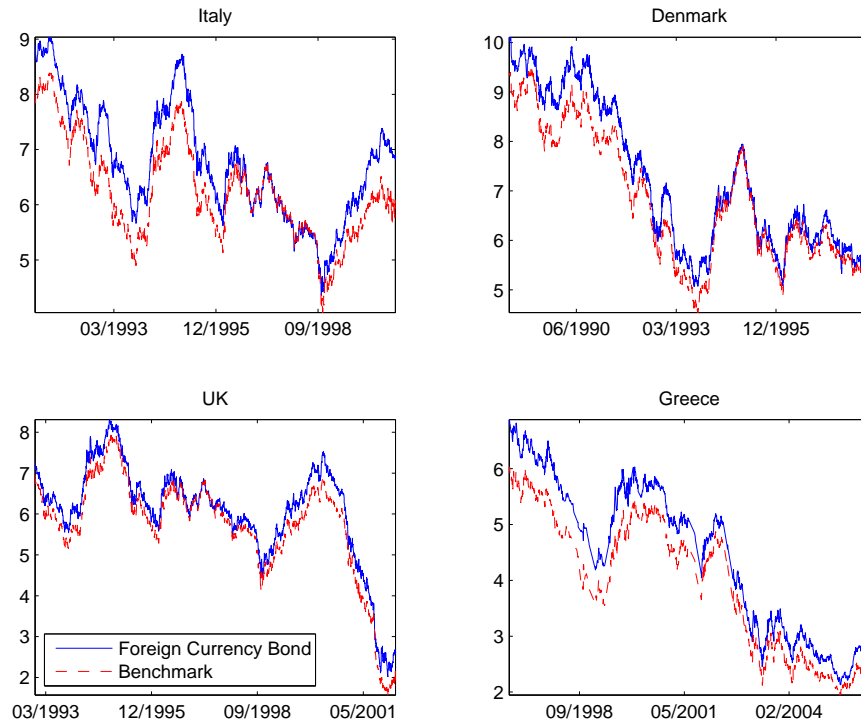


Figure 1: Bond Yields to Maturity for Selected Countries. Notes: blue solid line: yield to maturity/redemption yield of the respective domestic bond issued in foreign currency; red dashed line: yield to maturity/redemption yield of a “risk-free” benchmark bond with comparable coupon yield and maturity.

that yield spreads are typically small relative to the level of yields and vary considerably over time. For Italy and Greece, data on foreign currency bonds allow us to extend the sample to include observations prior to the introduction of the euro. In case of Denmark and the UK, they allow us to compute common-currency yield spreads, although those countries are not members of the euro zone.

Figure 2 details the construction for the case of Italy. Until 1991, only one foreign bond is available and its yield spread is taken. Starting in 1992, we have a second bond and their yield spreads are averaged. When the first bond matures in 1997, we are left with one bond until 1999 from where on we use the long-term convergence bond yields provided by the ECB. Table 2 provides information on the coverage of our spread sample and some basic descriptive statistics.¹⁰

¹⁰Figure 7 in the appendix plots the spreads for selected countries against fiscal consolidation episodes as classified by Alesina and Ardagna (2013). Interestingly, we do not find historical evidence that fiscal consolidations took place at times of high particularly high yield spreads.

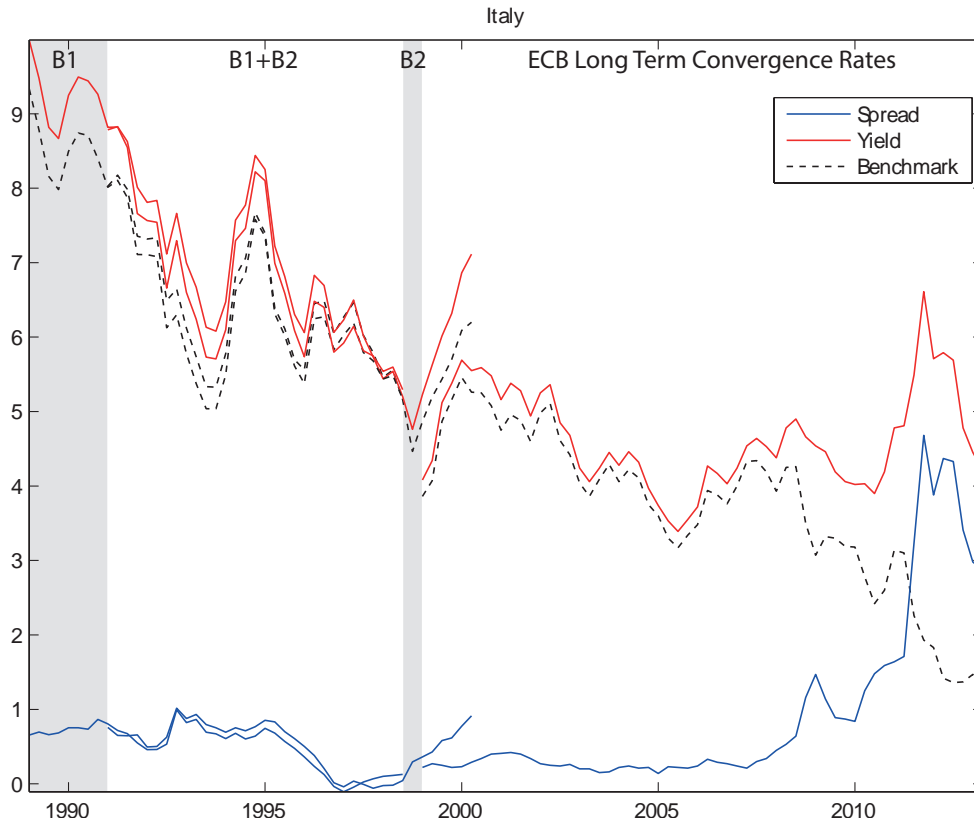


Figure 2: Construction of the Italian Yields Spread Series.

An alternative and frequently employed measure are credit default swap (CDS) spreads.¹¹ 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. 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 when a liquid market developed (see Mengle, 2007). To check the quality of our constructed spread measure, we compare it to yields of 5year CDS spreads. For the largest part of our sample the series move in close sync, with a correlation coefficient of 0.95 (see Figure 8 in the appendix).¹²

¹¹In a recent study, Longstaff, Pan, Pedersen, and Singleton (2011) find an important role of global factors in accounting for CDS spread dynamics of individual countries.

¹²The last column of Table 2 reports for country-by-country correlations. There are some outliers, most notable Uruguay, Sweden, El Salvador, and Thailand. But in these instances problems seem to be due to a lack of liquidity in the CDS market and Datastream recording the exact same CDS yield for up to 13 months.

Table 2: Basic properties of sovereign-yield spreads

Country	first obs	last obs	min	max	mean	std	$\rho(\text{CDS}_t, \text{Spread}_t)$
Belgium	1991.75	2013.25	0.04	2.53	0.45	0.44	0.94
Denmark	1988.50	2002.50	0.02	1.93	0.57	0.42	NaN
Finland	1992.25	2013.25	-0.04	0.80	0.27	0.18	0.87
France	1999.00	2013.25	0.02	1.35	0.27	0.32	0.88
Greece	1992.25	2013.25	0.15	23.98	2.80	5.24	0.98
Hungary	1999.00	2013.25	0.10	6.05	1.79	1.66	0.96
Ireland	1991.75	2013.25	-0.04	7.92	1.04	1.79	0.98
Italy	1989.00	2013.25	-0.07	4.68	0.77	0.98	0.97
Netherlands	1999.00	2013.25	0.00	0.67	0.19	0.17	0.93
Poland	1994.75	2013.25	0.42	8.71	1.97	1.43	0.95
Portugal	1993.25	2013.25	0.00	11.39	1.27	2.62	0.98
Slovenia	2006.00	2013.25	-0.17	5.13	1.59	1.59	0.93
Spain	1992.50	2013.25	0.01	5.07	0.71	1.15	0.98
Sweden	1986.00	2009.50	-0.95	2.95	0.90	0.94	-0.72
United Kingdom	1992.75	2007.75	-0.03	0.64	0.29	0.17	NaN
Argentina	1993.75	2013.25	2.04	70.78	15.80	18.74	NaN
Chile	1999.25	2013.25	0.55	3.43	1.45	0.58	0.88
Colombia	1997.00	2013.25	1.12	10.66	3.65	2.19	0.92
Ecuador	1995.00	2013.25	5.02	47.64	12.58	9.08	0.99
El Salvador	2002.25	2013.25	1.27	8.54	3.33	1.36	0.66
Malaysia	1996.75	2013.25	0.46	10.55	1.84	1.45	0.97
Peru	1997.00	2013.25	1.14	9.11	3.60	2.01	0.89
South Africa	1994.75	2013.25	0.70	6.52	2.28	1.24	0.94
Thailand	1997.25	2006.00	0.48	5.55	1.51	1.11	0.58
Turkey	1996.25	2013.25	1.39	10.66	4.19	2.44	0.86
Uruguay	2001.25	2013.25	1.27	16.43	4.07	3.18	-0.43

Notes: last column: correlation between 5year CDS spreads and constructed yields series during sample of overlap.

3 Econometric framework

In this section we discuss the econometric framework used to establish the effects of austerity on sovereign yield spreads. We first discuss identification and then turn to how we condition on the economic environment.

3.1 Identification

In terms of fiscal policy measures we focus on the dynamic effects of exhaustive government consumption for reasons of data availability. We obtain identification by assuming that, within a given quarter, government consumption is predetermined relative to the other variables included in our regressions. This assumption goes back to Blanchard and Perotti (2002) and has been widely applied in the empirical literature on the fiscal transmission mechanism. It is plausible, because 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 that discretionary government spending is subject to decision lags that prevent policymakers from responding to contemporaneous developments in the economy.¹³

Still, influential work by Ramey (2011b) and Leeper, Walker, and Yang (2013), has made clear that identification based on the assumption that government spending is predetermined may fail to uncover the true response of the economy to a government spending shock whenever such shocks have been anticipated by market participants. Of course, the notion that fiscal policy measures are anticipated, because they are the result of a legislative process and/or subject to implementation lags is plausible. Still, whether or not this invalidates the identification assumption is a quantitative matter. Results by Beetsma and Giuliodori (2011), Corsetti, Meier, and Müller (2012a), and Born, Juessen, and Müller (2013), for instance, suggest that the issue is of limited quantitative relevance as far as shocks to government spending are concerned. To address this issue, we 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 spending itself. Given data availability, we show—for a subset of our sample—that results do not change qualitatively relative to our baseline case.

A number of promising alternatives to identify shocks to government spending have been

¹³Anecdotic 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 Greece Ministry of Finance (2010).

pursued in the literature. Ramey (2011b) relies both war dates and forecast errors, while Devries, Guajardo, Leigh, and Pescatori (2011) use narrative evidence. These approaches cannot be applied to our sample for lack of narrative evidence or forecast errors at quarterly frequency.¹⁴ Also due to non-availability of appropriate tax data, we do not attempt to identify the effects of tax shocks. An alternative strand of the literature, following the lead of Alesina and Perotti (1995), identifies “fiscal adjustments” as episodes during which the cyclically adjusted primary deficit falls relative to GDP by a certain amount (see e.g. Alesina and Ardagna, 2010; IMF, 2010). In these studies, an episode of fiscal consolidation is assumed to take place over several years. Our focus, instead, is on the short run dynamics of spreads to cuts in government consumption.

3.2 Model specification

Our results are based on two alternative model specifications. Traditionally, the Blanchard-Perotti identification has been employed within a VAR context. More recently, it has also been used in panel VAR models, see, e.g., Ilzetzki, Mendoza, and Végh (2013) and Born, Juessen, and Müller (2013). Below we will also report estimates based on such a model. In this case, the vector of endogenous variables includes three variables: the log of real government consumption, $g_{i,t}$, the log of real GDP, $y_{i,t}$, and sovereign yield spreads, $s_{i,t}$, measured in percentage points. In what follows, i denotes the country and t the time period. The VAR model is given by

$$\begin{bmatrix} g_{i,t} \\ y_{i,t} \\ s_{i,t} \end{bmatrix} = \boldsymbol{\mu}_i + \boldsymbol{\alpha}_i t + \sum_{k=1}^K \mathbf{A}_k \begin{bmatrix} g_{i,t-k} \\ y_{i,t-k} \\ s_{i,t-k} \end{bmatrix} + \boldsymbol{\nu}_{i,t}, \quad (3.1)$$

where $\boldsymbol{\mu}_i$ and $\boldsymbol{\alpha}_i$ are vectors containing country-specific constants and time trends. The matrices \mathbf{A}_k capture the effect of past realizations on the current vector of endogenous variables. $\boldsymbol{\nu}_{i,t}$ is a vector of reduced form residuals. We estimate model (3.1) by OLS. Identification is based on mapping the reduced-form innovations $\boldsymbol{\nu}_{i,t}$ into structural shocks:

$$\boldsymbol{\varepsilon}_{i,t} = \mathbf{B}\boldsymbol{\nu}_{i,t}, \text{ with } \boldsymbol{\varepsilon}_{i,t} \stackrel{iid}{\sim} (0, \mathbf{I}).$$

¹⁴Another strand of the literature employs sign restrictions to identify fiscal shocks, see Mountford and Uhlig (2009). This is not feasible in the context of our analysis, as there is no consensus on the sign of the responses to a fiscal shock as far as the variables in our sample are concerned.

In the present context, identifying shocks to government consumption under the assumption that it predetermined boils down to equating the first element in $\nu_{i,t}$ with a structural fiscal shock.¹⁵

Recently, local projections have been a popular tool to complement VAR analysis. As argued by Jordá (2005), local projections are more robust to model misspecification as they do not impose cross-equation restrictions as in VAR models. Moreover, local projections prove highly flexible in accommodating a panel structure. Finally, and perhaps most importantly, they offer a very convenient way to account for state dependence—the focus of our analysis below. Earlier work by Auerbach and Gorodnichenko (2013b) has illustrated this in the context of fiscal policy.

More specifically, they suggest a panel smooth transition autoregressive (STAR) model on which we rely below. Defining the vector $X_{i,t} = [g_{i,t} \ y_{i,t} \ s_{i,t}]'$, the response of a variable $x_{i,t+h}$ at horizon h to $g_{i,t}$ can be obtained by locally projecting $x_{i,t+h}$ on time t government spending and a set of control variables/regressors. That is, the following relation is estimated:

$$\begin{aligned} x_{i,t+h} = & \alpha_{i,h} + \beta_{i,h}t + \eta_{t,h} \\ & + F(z_{it}) \psi_{A,h} g_{i,t} + [1 - F(z_{it})] \psi_{B,h} g_{i,t} \\ & + F(z_{it}) \Pi_{A,h}(L) X_{i,t-1} + [1 - F(z_{it})] \Pi_{B,h}(L) X_{i,t-1} + u_{it} . \end{aligned} \quad (3.2)$$

Here $\alpha_{i,h}$ and $\beta_{i,h}t$ are a country-specific constant and a country-specific trend, respectively. $\eta_{t,h}$ in turn captures time fixed effects, which we do not allow for in the VAR model above. u_{it} is an error term with strictly positive variance. L denotes the lag operator and $\Pi_{A,h}(L)$ is a lag polynomial of coefficient matrices capturing the dynamics of controls for a particular state of the economy (see below). Similarly, $\Pi_{B,h}(L)$ is the lag polynomial of coefficient matrices for the alternative state. Note that the dynamic response of the dependent variable to a government spending shock in the two states is captured by the Ψ -coefficients on the g_{it} terms. We estimate (3.2) using OLS, assuming that government spending is predetermined (see also Auerbach and Gorodnichenko, 2013a).

The distinct feature of model (3.2) is that the dynamic response of the dependent variable is a weighted average whereby the fiscal shock as well as the regressors are allowed to impact the dependent variable differently, depending the likelihood that the economy is in one of two states. The variable z_{it} is meant to provide the relevant information about the state, such as, for instance, current output growth which provides information about the state

¹⁵As a practical matter, we impose a lower-triangular structure on \mathbf{B} , attaching no structural interpretation to the other elements in $\varepsilon_{i,t}$.

of the business cycle (boom vs recession). A so-called “transition function” (Auerbach and Gorodnichenko, 2013b) maps z_{it} into weights which capture the probability that the economy operates in a particular state. In what follows we use the following specification of the transition function:

$$F(z_{it}) = \frac{\exp(-\gamma z_{it})}{1 + \exp(-\gamma z_{it})}, \quad \gamma > 0. \quad (3.3)$$

F is a logistic transformation of z_{it} and can be interpreted as a cumulative density function for the indicator, i.e. $Prob(z < \bar{z}) = F(\bar{z})$. In our estimation below we will consider alternative variables z_{it} while allowing for alternative distinctions regarding the relevant state of the economy.

4 Results

We now turn to our estimates regarding the effects of government consumption cuts. Our main focus is the dynamic response of sovereign spreads to such cuts. Still, as argued above, because the adjustment of output is likely to be a key determinant of spreads, we also report the response of output alongside that of government consumption and the spread. We normalize the size of the initial shock such that the cut in government consumption is equal to one percent of GDP.

In a first subsection, we report results for estimations which do not condition on the economic environment (or state of the economy). Afterwards we document that results differ considerably, depending on whether economies operate an exchange rate regime, experience fiscal stress or a recession. Recall that throughout we obtain identification by assuming that government spending is predetermined within a given quarter.

4.1 Baseline: unconditional effects

Figure 3 shows results for the baseline case: the specification which does not account for the economic environment. The panels in the upper row show impulse responses based on estimates obtained from local projections (LP). Here and in the following the horizontal axis measures time in quarters, while the vertical axis measures the deviation from the pre-shock path. For quantities it is measured in percent of trend output, for the spread it is measured in basis points. Solid lines represent the point estimates, while shaded areas

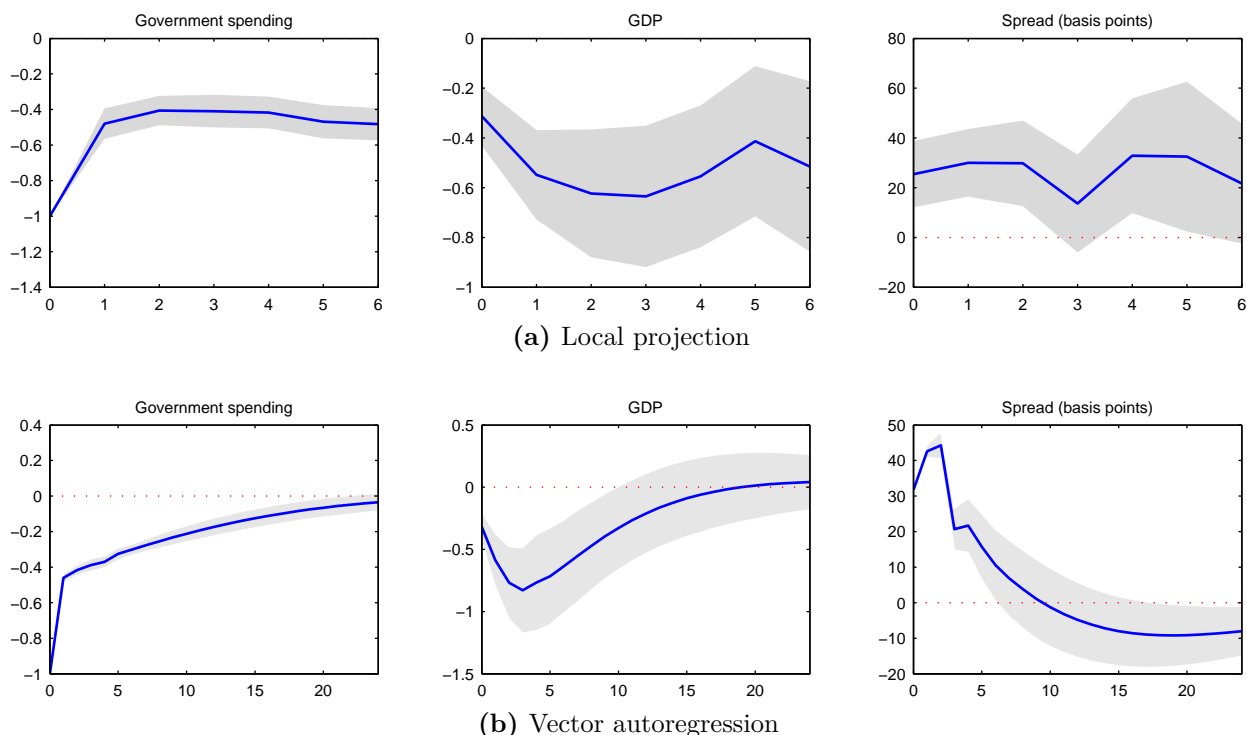


Figure 3: Unconditional dynamic response to a government spending shock. Notes: confidence bounds represent 68 percent standard errors. Horizontal axis represent quarters. Vertical axes represent deviation from pre shock level in terms of trend output and basis points (spread). Top panels show results based on local projections, bottom panels show results based on VAR.

indicate 68% standard errors.¹⁶ The second row shows the impulse responses obtained from the estimated panel vector autoregression (VAR) model. Note that the horizon for which we report impulse responses differs for the LP and the VAR case, as extending the horizon in the former case comes at the expense of degrees of freedom. Results are very similar, both from a qualitative and a quantitative point of view.

The first column of Figure 3 shows the dynamic adjustment of government consumption over time. After the initial cut, government consumption remains depressed for an extended period, but eventually returns to its pre-shock level, as evidenced by the VAR results (second row). The response of GDP is displayed in the panels of the second column. It declines by about 0.3 percentage points on impact, declines further and reaches a peak response of about -0.6 percent of GDP after about 3 quarters. Given that we normalize

¹⁶LP standard errors are clustered robust standard errors, VAR standard errors are bootstrapped using 100 bootstrap samples.

the initial cut in government consumption to -1 percent of GDP, these estimates can be interpreted as estimates of 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 (Ramey, 2011a).

Finally, in the third column, we present estimates for the dynamic response of spreads to the cut in government consumption. Here we find that spreads do, in fact, increase in response to the spending cut. The impact response varies between 25 basis points (LP) and 30 basis points (VAR). For both specifications we find a maximum effect of about 40 basis points. The VAR response shows that the spread returns to its pre-shock level, after mildly undershooting it for an extended periods. It thus appears that austerity doesn't pay off: spending cuts fail to reassure investors about the sustainability of public finances.

4.2 Conditional effects

The above result obtains for the entire sample, possibly masking heterogeneity of economic circumstances—both, across time and countries—which may impact how spreads respond to austerity. In fact, in this regard various dimensions of the economic environment are likely to be particularly relevant. Traditionally, the exchange rate regime maintained by a country has been identified as having a first order effect on the fiscal transmission mechanism. This holds for Mundell-Fleming-type models and, although to a somewhat lesser extent, for New Keynesian variants as well (Corsetti, Kuester, and Müller, 2013). In line with these considerations, earlier empirical work has established that fiscal multipliers tend to be higher in countries which operate a fixed exchange rate. A straightforward way to establish this is to estimate panel VAR models for different sub-samples (Ilzetki, Mendoza, and Végh, 2013; Born, Juessen, and Müller, 2013). A condition for this approach to make sense, however, is that countries do not change their exchange rate regime too often.¹⁷

In what follows we verify that this result obtains for our sample as well and establish new evidence for the behavior of spreads in response to austerity under different exchange rate regimes. Specifically, using the definition of exchange rate regimes in Ilzetki, Reinhart, and Rogoff (2009) we define those country-observations as a “fixed exchange rate regime” (or “peg”) for which countries operate a de facto crawling band that is narrower than or equal to $\pm 2\%$ or tighter. About two thirds of our 1534 country-quarter observations qualify as a peg. We estimate the panel VAR model on both country groups separately and report results in the second row of Figure 4.

¹⁷Corsetti, Meier, and Müller (2012b) pursue the issue using an alternative approach akin to LP.

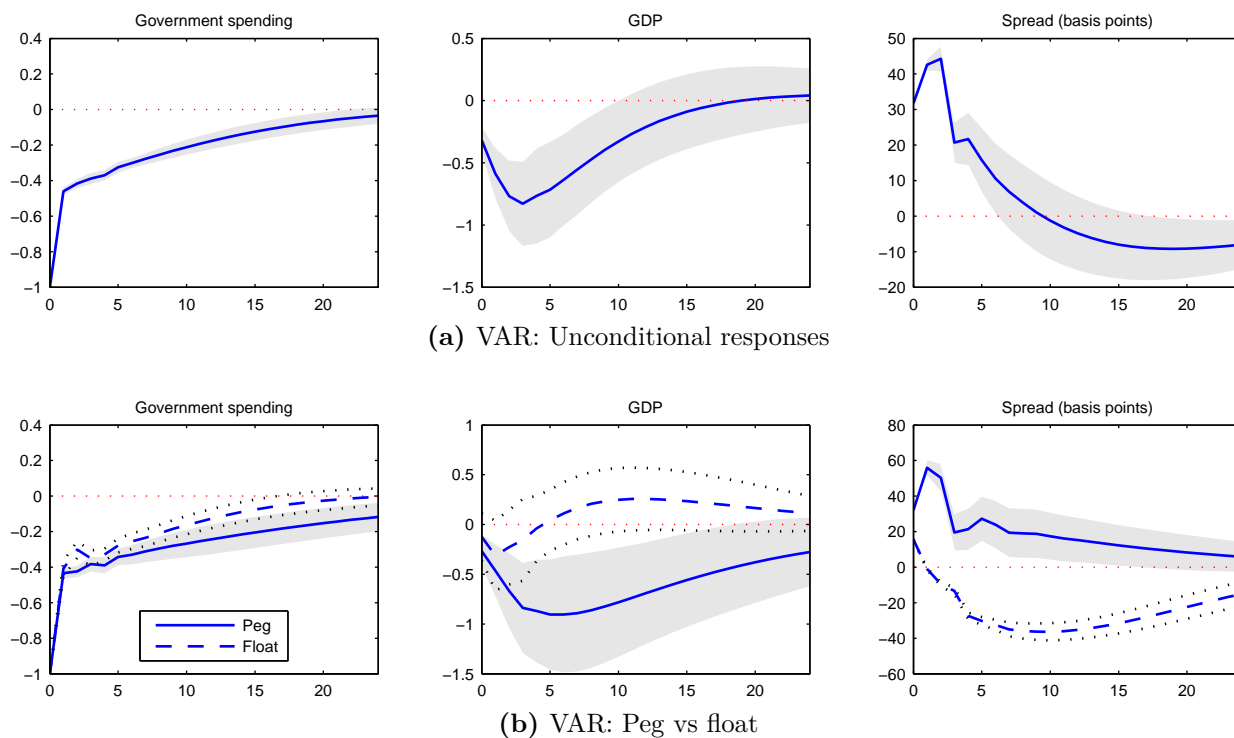


Figure 4: Dynamic Response to a Government Spending Shock derived from VAR (four lags). All standard errors are bootstrapped 68% standard errors.

Conditioning on the exchange rate regime yields a number of insights. First, comparing the results to the estimates for the unconditional responses (reproduced in the upper row of Figure 4), it appears that the observations for fixed exchange rate regimes dominate the sample, both in terms of dynamic adjustment patterns and in terms of quantitative results. Second, for country observations for which the exchange rate is flexible we find that the output multiplier is not significantly different from zero—in line with the predictions of the textbook version of the Mundell-Fleming model. Third, and perhaps more importantly still, we find that spreads tend to decline in response to government spending cuts in case the exchange rate regime is flexible. Hence, while the joint dynamics of output and spreads are turned upside down with the exchange rate regime, the negative co-movement of output and spreads is preserved under each exchange rate regime. This finding squares well with the observation that benign outcomes of austerity have been limited to periods of exchange rate flexibility, see, e.g., Perotti (2013).

So far we have established a number of results on the basis of VAR models estimated on different sub-samples. Yet, in case we aim at establishing the effects of features of the economic environment which change more frequently than the exchange rate regime the

VAR approach does not offer sufficient flexibility. In contrast, the LP approach is well equipped to capture time variation of the economic environment at higher frequency. Given the question at hand, namely whether austerity pays off, it is particularly interesting to condition on whether the economy experiences fiscal stress, as this often happens to be the case at times of austerity. In what follows, we measure fiscal stress on the basis of sovereign yield spreads.

In order to do so, we rely on the STAR model (3.2). This requires us, in a first step, to specify an indicator variable z_{it} based on sovereign yield spreads. Specifically, we compute a three-quarter moving average of the sovereign yield spread to filter out high-frequency noise and apply a log transformation to the result in order to allow the indicator variable to vary on an unrestricted domain.¹⁸ We consider a country to be in fiscal stress if $F(z_{it}) > 0.8$ and calibrate the transition function (3.3) in such a way that 20 percent of the observations in our sample are characterized by fiscal stress.¹⁹ Table 3 provides summary statistics as to the frequency with which countries have been suffering from fiscal stress. Figure 9 in the appendix provides an overview of the value the transition function takes for each observation in the sample F .

Given the calibrated transition function, we are in position to estimate model (3.2). It allows us to condition the effects of cuts of government consumption on whether the economy experiences fiscal stress or, alternatively, tranquil times. The second row of Figure 5 reports the results, contrasting it to the results for the unconditional estimates (reproduced in the first row of Figure 5). Results are rather stark: the dynamic adjustment of the economy under fiscal stress resembles those obtained for the unconditional estimates very closely. Fiscal stress episodes, as in case of fixed exchange rate observations, thus seem to dominate the overall sample. Only from a quantitative point of view we find that the effects under fiscal stress differ from the baseline case: they are considerably magnified. The point estimate for the multiplier now reaches a value of unity, while spreads rise to up to 100 basis points in response to a cut of government consumption.

The effects of austerity in tranquil times, on the other hand, differ considerably from those obtained from the unconditional estimates. As in the case of floating exchange rates, we now find the output effect not significantly different from zero. Importantly, our estimates

¹⁸Specifically, we compute $z_{it} = \log(0.2 + s_{it}^{filt})$, where the small additive constant takes care of small negative yield spreads occurring in the sample. Note also that our results are robust to using the country-demeaned moving average of the spreads, $z_{it} = s_{it}^{filt} - \bar{s}_i^{filt}$ as the indicator instead, see Figure 13 in the appendix.

¹⁹One can think of $F(z_{it})$ as a cumulative density function. In setting $\gamma = 1.3$ we make sure that 20% of the observations fulfil the criterion $F(z_{it}) > 0.8$.

Table 3: Descriptive Statistics: Transition functions

Country	$\rho(F(z_{it}^{stress}), F(z_{it}^{recess}))$	Share Rec.	Share Fisc. Stress	Share Peg
Belgium	0.21	0.22	0.00	1.00
Denmark	0.34	0.19	0.00	1.00
Finland	0.28	0.12	0.00	1.00
France	0.13	0.24	0.00	1.00
Greece	0.60	0.28	0.07	1.00
Hungary	0.25	0.11	0.24	1.00
Ireland	0.03	0.21	0.16	1.00
Italy	0.07	0.21	0.07	1.00
Netherlands	0.38	0.25	0.00	1.00
Poland	0.43	0.28	0.04	0.00
Portugal	0.14	0.23	0.14	1.00
Slovenia	0.47	0.22	0.19	1.00
Spain	0.05	0.16	0.09	1.00
Sweden	-0.12	0.19	0.00	0.00
United Kingdom	0.39	0.03	0.00	0.00
Argentina	0.33	0.25	0.97	0.75
Chile	0.71	0.17	0.00	0.00
Colombia	0.33	0.22	0.47	0.00
Ecuador	0.29	0.24	1.00	1.00
El Salvador	0.62	0.19	0.57	1.00
Malaysia	0.49	0.19	0.04	0.62
Peru	0.42	0.24	0.54	1.00
South Africa	0.56	0.17	0.28	0.00
Thailand	0.54	0.15	0.12	0.00
Turkey	0.35	0.18	0.61	0.00
Uruguay	0.07	0.22	0.50	0.00

Notes: First column: Correlation between fiscal stress and recession indicators. Second column: fraction of time during which the country was in a recession, based on $F(z_{it}) > 0.8$. Third column: fraction of time during which the country was in fiscal stress, based on $F(z_{it}) > 0.8$. Fourth column: fraction of time during which the country's exchange rate regime was a peg.

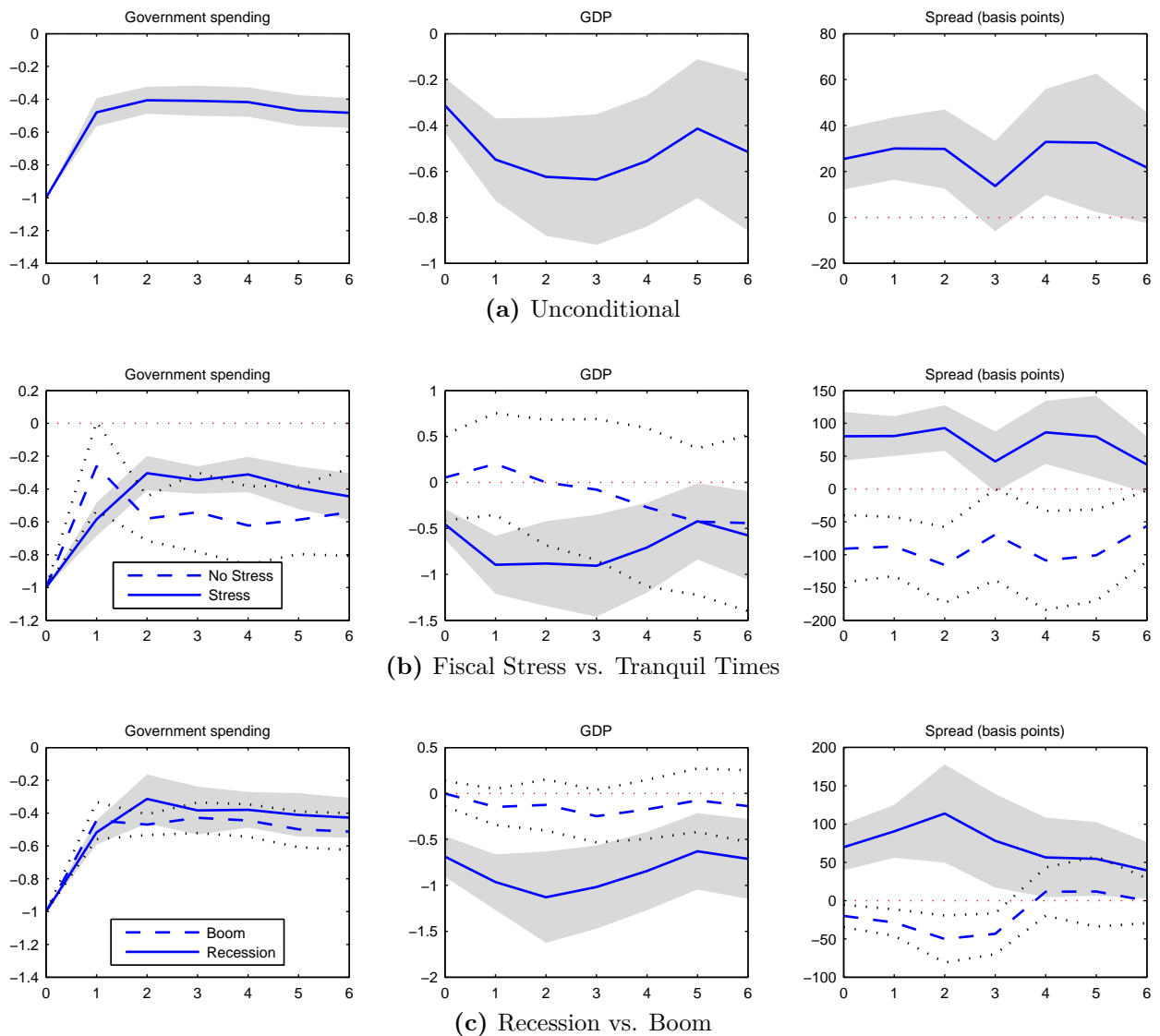


Figure 5: Dynamic response to a government spending shock derived from local projections (two lags). All standard errors are clustered robust 68% standard errors.

also suggests that spreads decline in response to cuts in government consumption, provided that the economy experiences tranquil times.

A consistent finding of our analysis is thus that the comovement of output and spreads remains negative, once we condition on fiscal shocks. This suggests an explanation for our findings regarding the role of fiscal stress. Episodes of fiscal stress are by definition circumstances where the ability and/or the willingness of governments to honor their debt obligations is doubted by market participants. From a theoretical point of view, one would

expect such doubts to increase whenever public debt rises and/or economic activity falls (Arellano, 2008; Bi, 2012). Hence, while—all else equal—austerity measures may reduce public debt, their adverse effect on economic activity appears to dominate the response of spreads. As a result, the fiscal outlook seems to deteriorate, at least in the short run under consideration in our analysis.

Under this interpretation it is crucial that cuts of government consumption impact economic activity particularly strongly during episodes of fiscal stress. This, in turn, may be the case, because episodes of fiscal stress tend to be episodes of low economic activity, because these are episodes when fiscal policy has been found to impact economic activity more strongly than in normal times (Auerbach and Gorodnichenko, 2012, 2013b). It is instructive to consider this possibility for our sample as well. For this purpose we consider an alternative specification of the STAR model. Specifically, following Auerbach and Gorodnichenko (2013b), we now condition the transition function on the state of the business cycle, rather than on fiscal stress. For this purpose, we replace z_{it} by a filtered measure of output growth. Following Auerbach and Gorodnichenko (2013b), we calibrate the transition function such that roughly 20 percent of all observations qualify as recession episodes, see Table 3.²⁰

Results are shown in the last row of Figure 5. Interestingly, we obtain a pattern of responses quite comparable to the one obtained once we condition on fiscal stress. During recessions, as in times of fiscal stress, the multiplier is about unity and spreads rise strongly in response to austerity. Conversely, during booms, as in fiscally tranquil times, the multiplier is basically zero and spreads decline in response to austerity. 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, there is large variation in the correlation of the transition obtained for stress and recessions, see Table 3. Moreover, the correlation of both indicators is about 0.32.

4.3 Robustness

We explore the robustness of our findings across a range of alternative specifications and sample periods. First, as discussed above, under the Blanchard-Perotti identification approach, news and realizations of fiscal shocks necessarily coincide. To the extent that fiscal shocks become known prior to an actual change in fiscal variables, estimates may

²⁰First, we compute a five-quarter moving average of the first difference of log output. The resulting series is then filtered using an Hodrick-Prescott filter with smoothing parameter $\lambda = 10,000$. A value for $\gamma = 2$ ensures that $F(z_{it}) < 0.8$ in 80 percent of the cases. This compares to 1.5 in Auerbach and Gorodnichenko (2013b).

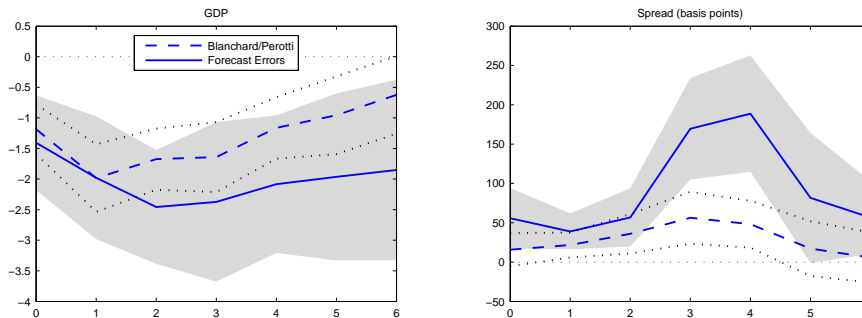


Figure 6: Dynamic response to a government spending shock derived from local projections (two lags). Solid line: Identification using OECD forecast errors. Dashed line: Blanchard/Perotti identification. All standard errors are clustered robust 68% standard errors. The countries sample is restricted to observations with available OECD forecast errors.

by biased. To gauge the impact of possibly anticipated government spending shocks on our results, we turn to the OECD Economic Outlook dataset, which contains semiannual observations for the period from 1986 to 2013 for an unbalanced panel of OECD countries. A key feature of this data set is that it contains explicit forecasts for government consumption spending. The OECD prepares these forecasts in June and December of each year, that is, at the end of an observation period.²¹

Including the forecast error for spending in the local projection model (3.2), rather than government spending allows us to better identify the effects of *unanticipated* spending shocks in the face of exogenous, but *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, because the base year used by the OECD changes several times during our sample period. Figure 6 displays the results, obtained for the sample for which government spending forecasts are available and compares them to the baseline local projections for the same sample. It turns out that explicitly accounting for anticipation does not alter results very much (see also Beetsma and Giuliodori, 2011; Corsetti, Meier, and Müller, 2012a; Born, Juessen, and Müller, 2013).

In a second experiment we exclude the Great Recession from our sample, that is, we consider only observations up to the second quarter of 2007. Figure 10 in the appendix shows the

²¹As discussed in detail by Auerbach and Gorodnichenko (2013b), these forecasts have been shown to perform quite well. Auerbach and Gorodnichenko (2013b) use these data to estimate government spending multipliers on the basis of local projections, contrasting results for recessions and booms. Beetsma and Giuliodori (2011) also control for anticipation effects when estimating the effects of government spending shocks. They consider annual data and include the budget forecast of the EU commission in their regression.

results based on the LP approach, distinguishing, as in Figure 5, unconditional estimates from those obtained once we condition on fiscal stress and the business cycle. Contrasting the results with those for the full sample, we conclude that results are not driven by the Great Recession.

Third, we consider the robustness of our results regarding the role of fiscal stress through a number of sample splits, obtaining results for a sample which includes only euro area countries, for a sample of euro area periphery countries 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 11, 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 Figure 12. As a caveat, however, we note that there are sizeable differences in some instances, reflecting perhaps also a strong decline in sample size.

A final experiment establishes robustness with regard to our measure of fiscal stress. For this purpose we change our calibration of the transition function. In the experiments so far we have considered the absolute value of spreads as the key variable to determine the level of fiscal stress, irrespective of the country under consideration. However, as our LP estimates allow for country-specific fixed effects, it may appear reasonable to do so as well in establishing the incidence of fiscal stress. In order to do so, we remove country-specific means from the spread prior to computing the value of the transition function. As a result we find considerably more variation in the incidence of fiscal stress in several emerging market economies where spreads tend to be higher on average, see Figure 9. Still, we find overall that the differential impact of fiscal stress on the fiscal transmission is largely unchanged. Results are reported in Figure 13 in the appendix.

5 Conclusion

Does austerity restore market confidence, bringing about a reduction in sovereign yield spreads? In pursuing this question, this paper makes two distinct contributions. First, we set up a new data set which contains data on sovereign yield spreads for 26 emerging and advanced economies. At a quarterly level we cover data from 1990 to 2013, not only for spreads, but also for government consumption and output.

A first look at the data allows us to establish a number of basic facts. First, yield spreads do not exhibit a particular behavior in the context of fiscal consolidation episodes as identified

by Alesina and Ardagna (2013). Second, yield spreads are strongly countercyclical. The correlation of yield spreads and current output growth is negative in all countries of our sample, but Sweden. Third, there is no systematic correlation pattern emerging for yield spreads and government consumption.

We thus turn to our second contribution: estimating the dynamic effects of austerity on spreads. In this regard, conditioning on the economic environment, notably on whether countries operate a flexible exchange rate regime, experience fiscal stress or a recession, yields important insights. First, results for the entire sample without accounting for the state of the economy appear to be dominated by observations for fixed exchange rates, recessions, and fiscal stress. In these instances, as well as the unconditional estimates, we find a government spending multiplier on output close to unity and, importantly, that cuts of government consumption tend to *raise* spreads. In fact, a cut of government consumption by one percent of GDP may induce a rise in spreads by up to 1 percentage point.

Instead, during fiscally tranquil times or booms, as well as for countries operating a flexible exchange rate regime, we find multipliers much reduced. In fact, they may not be different from zero in this case. Moreover, we also find that a reduction of government consumption induces a reduction in yield spreads by up to 1 percentage points. To rationalize these results, we stress that the data reveal a very robust pattern: yield spreads appear to comove negatively with output—both, unconditionally and conditional on fiscal shocks. To the extent that austerity exerts a strong negative impact on economic activity, it likely fails to bring about a reduction in yield spreads. This, according to our estimates, is more likely to happen in times of fiscal stress, during recessions and under fixed exchange rates.

Still, austerity may pay off—if only it is implemented during benign times. Under adverse economic conditions, instead, it may be beneficial to delay austerity measures. In this case, in order to reassure markets about the sustainability of public finances, one may rather enact policies directed towards boosting economic activity. While taken at face value, our results suggests that even expansionary fiscal policies may be beneficial in this regard, we caution against such conclusions, because of the possibly adverse long-term implications. These, in turn, warrant further investigation.

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A Appendix

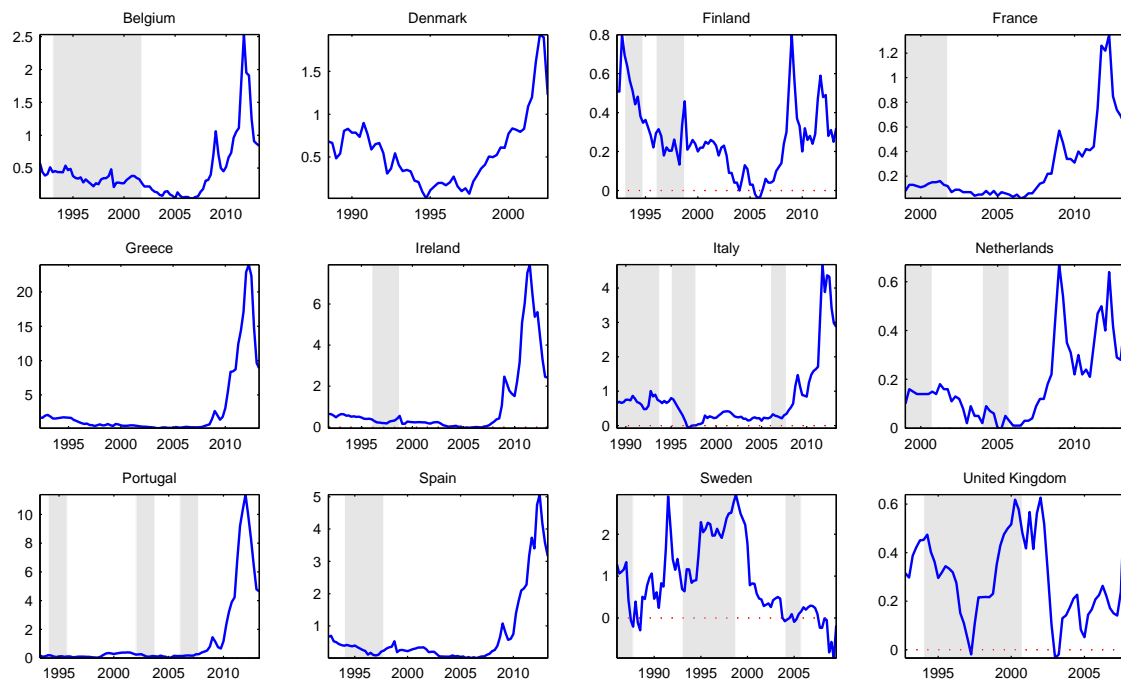


Figure 7: Spreads and Alesina/Ardagna (2013) consolidation episodes. Blue solid line: yield spread series on government bonds, constructed as described in the text. Grey shaded area: consolidation episodes as identified by Alesina and Ardagna (2013).

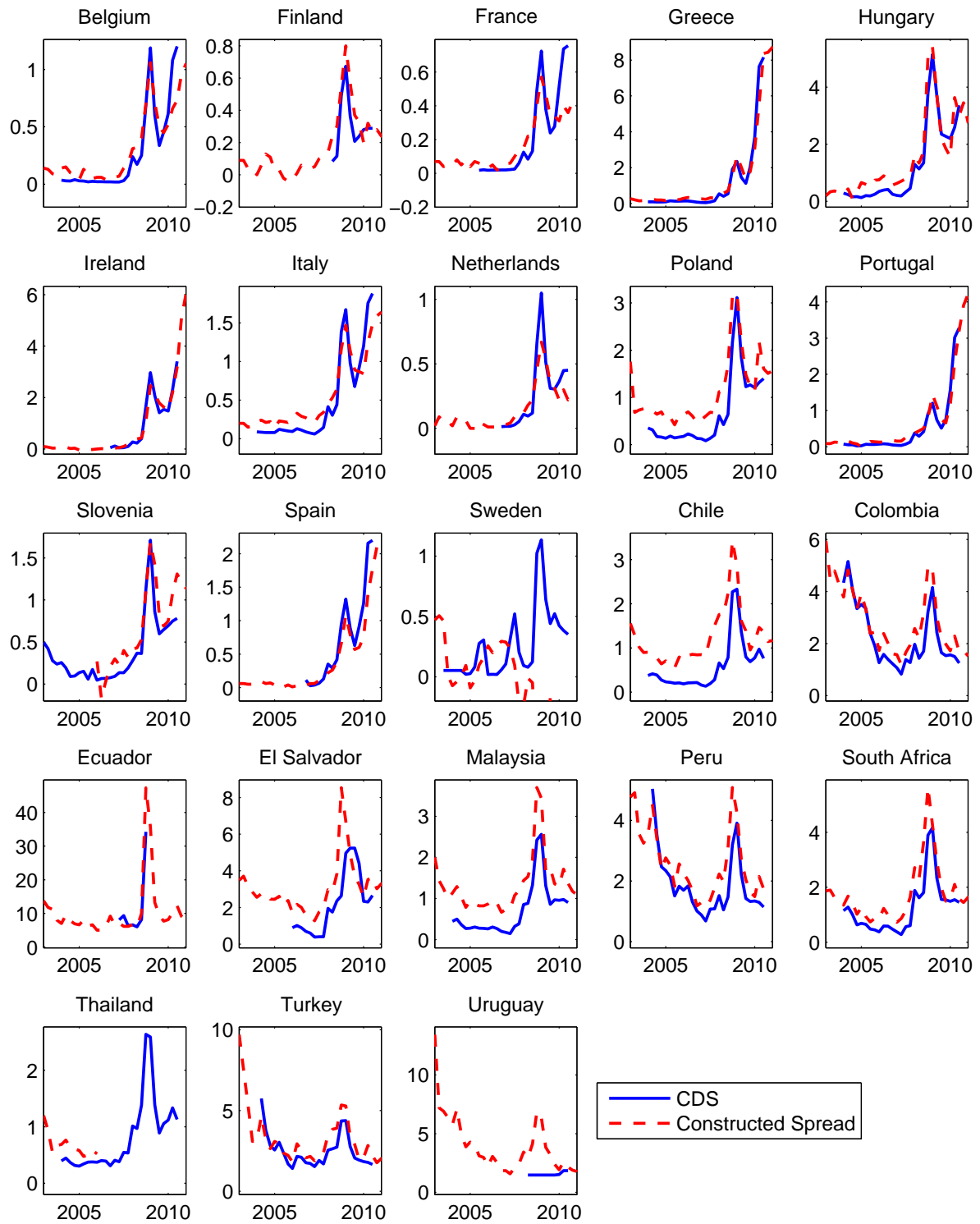


Figure 8: Comparison CDS vs constructed spreads for selected countries. Blue solid line: 5year CDS yields on government bonds. Red dashed line: yield spread series on government bonds, constructed as described in the text.

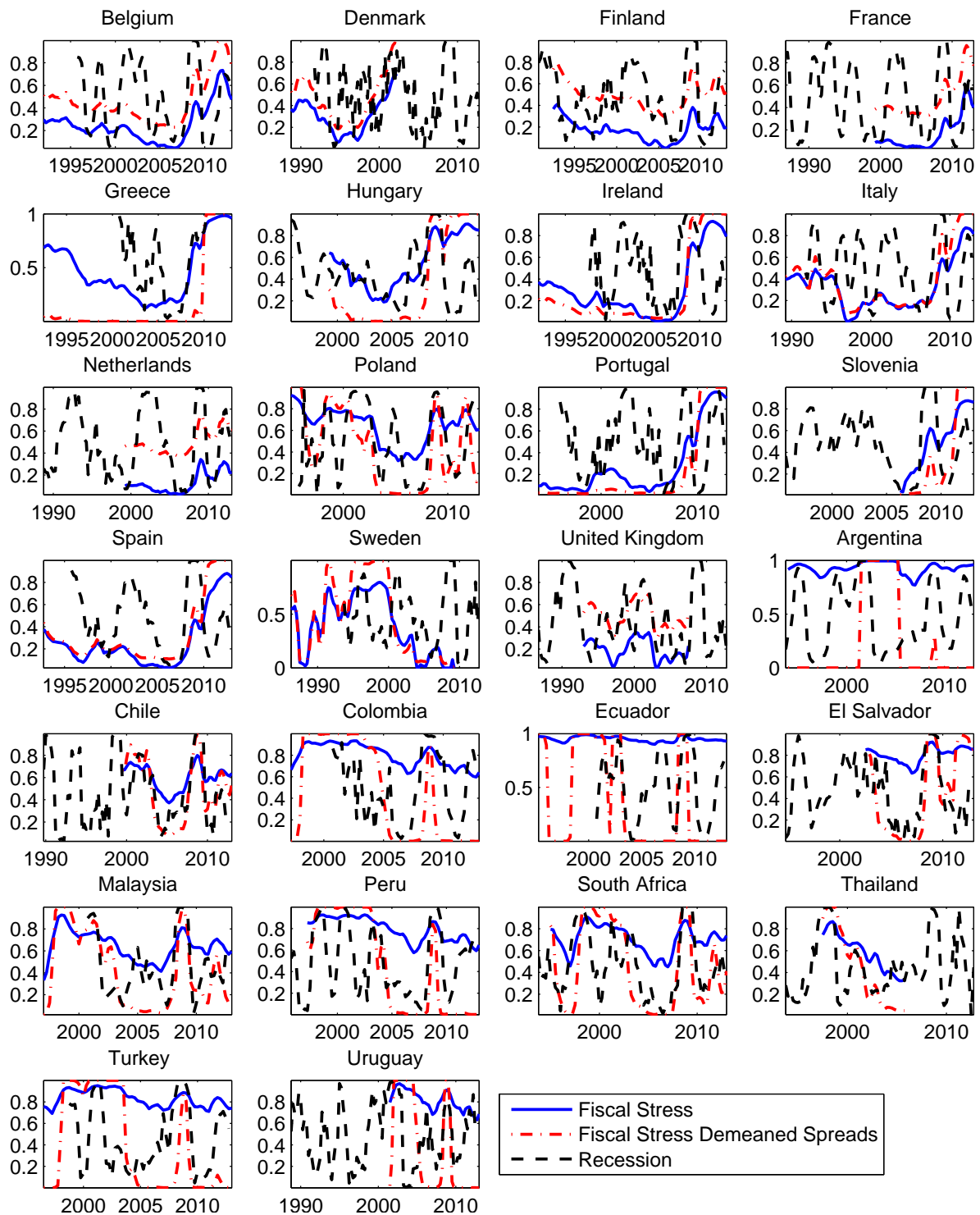


Figure 9: Transition Functions for Spreads and Recessions

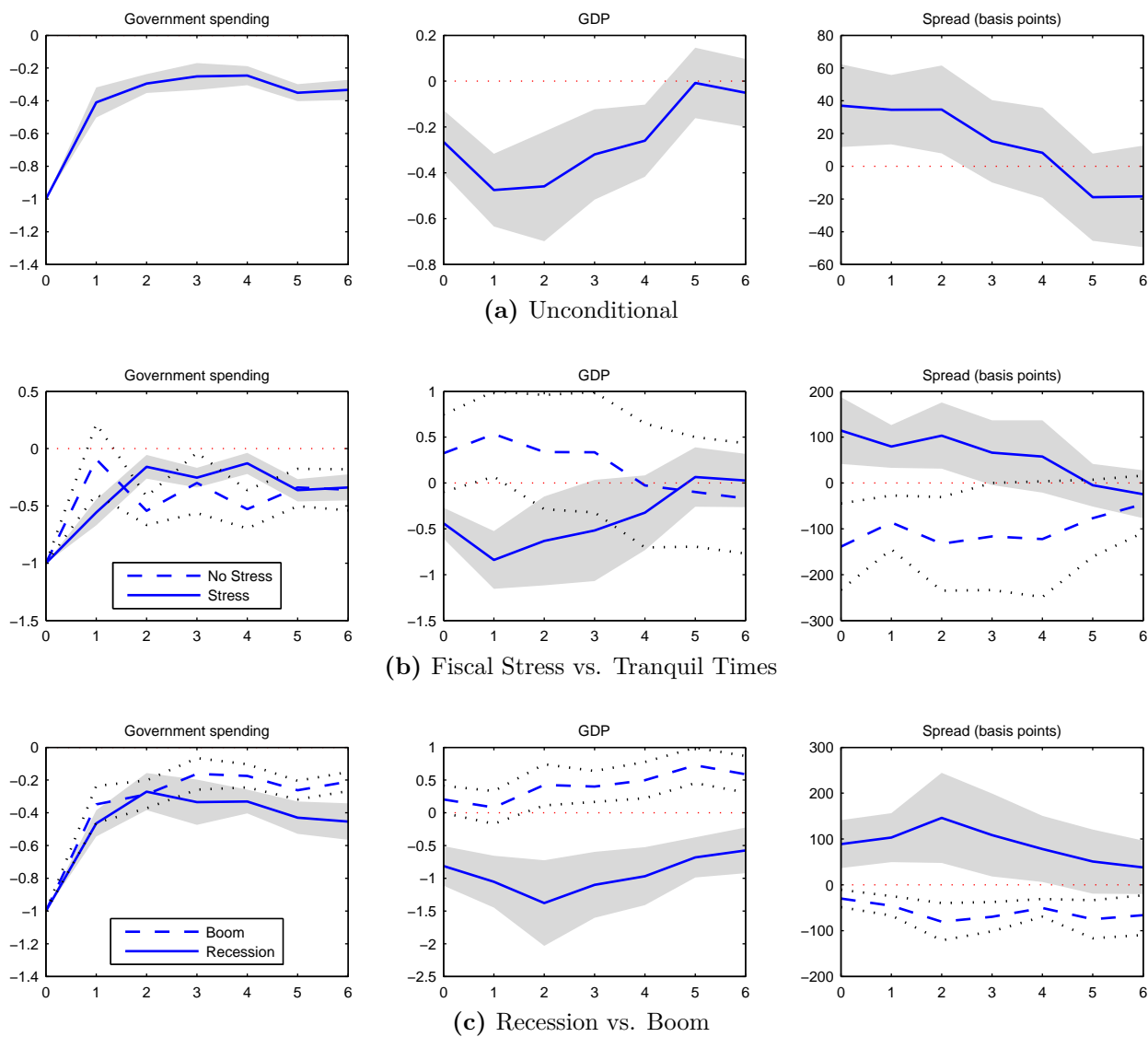


Figure 10: Dynamic Response to a Government Spending Shock derived from Local Projections (two lags) when excluding the Great Recession. All standard errors are clustered robust 68% standard errors.

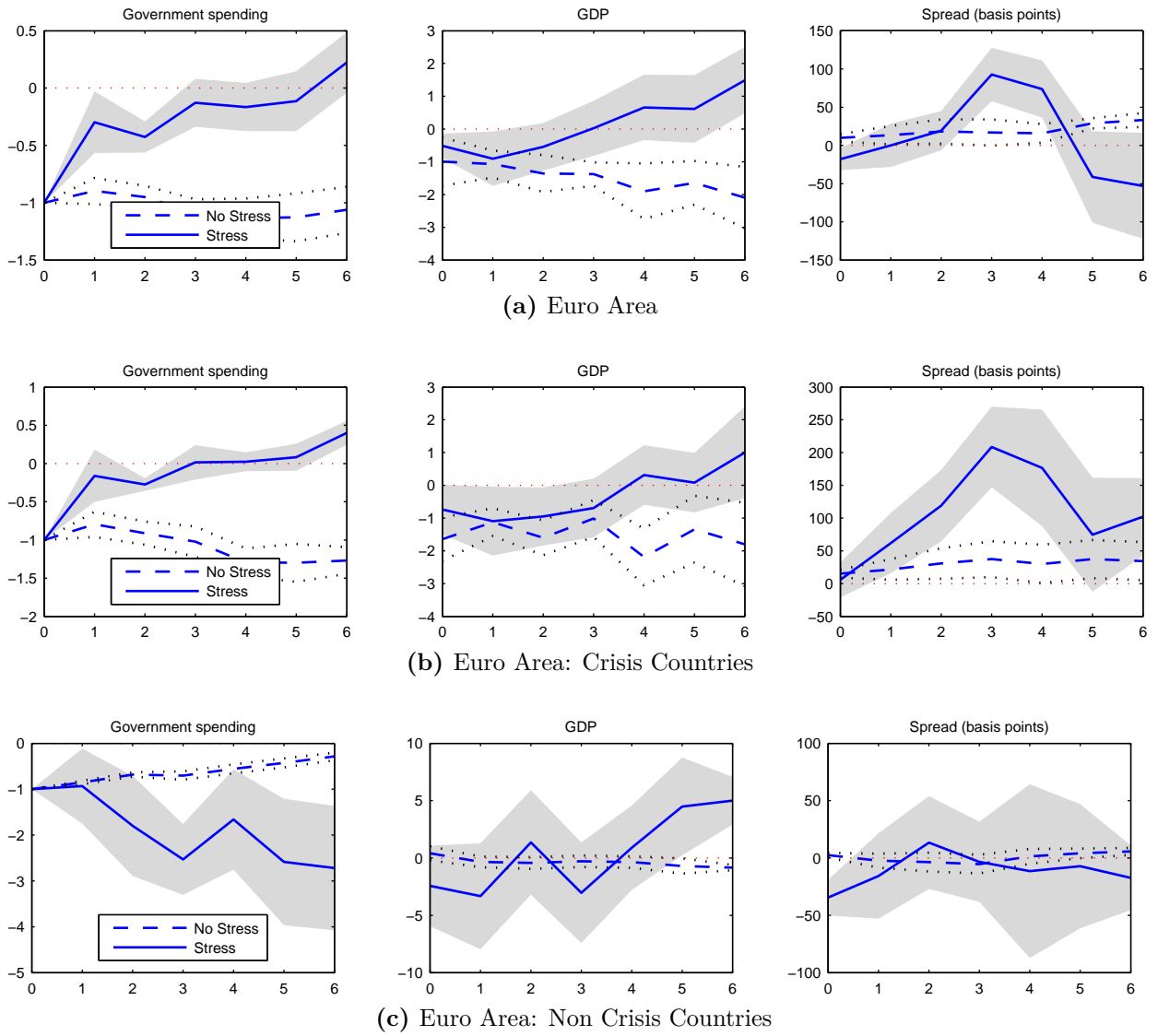


Figure 11: Dynamic Response to a Government Spending Shock derived from Local Projections for the whole sample and different euro area countries. All standard errors are clustered robust 68% standard errors.

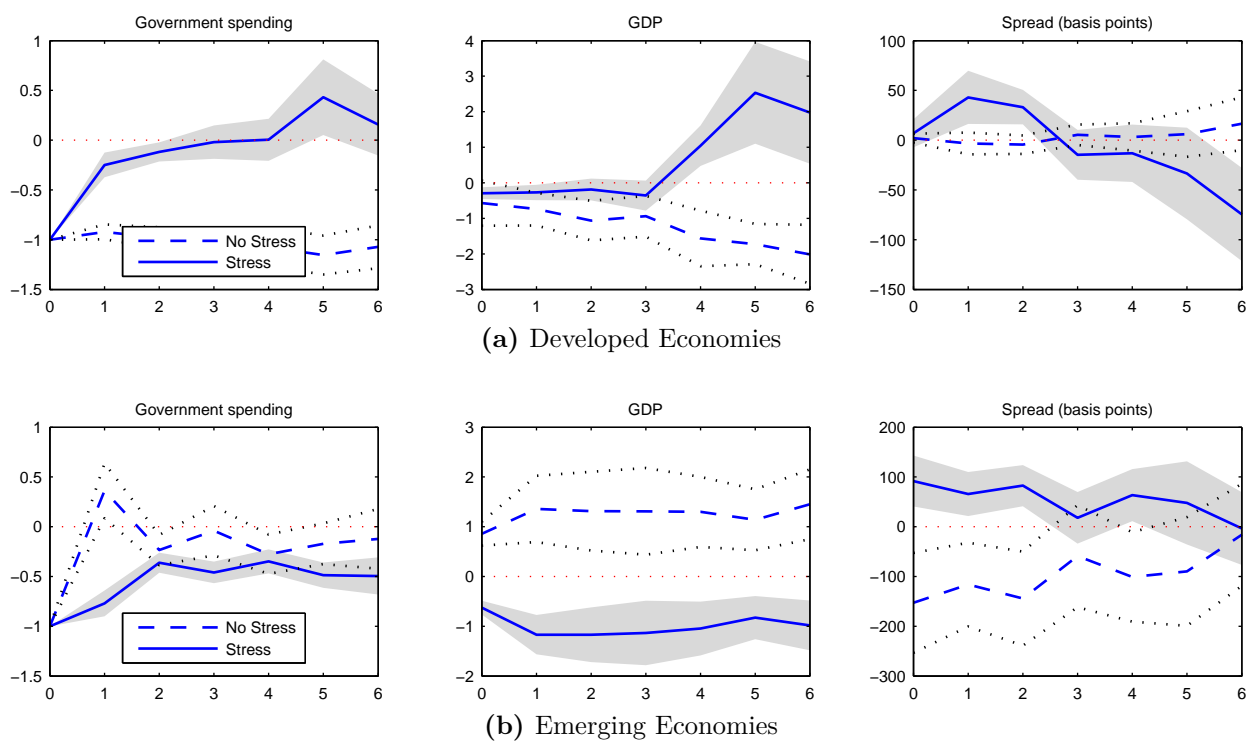


Figure 12: Dynamic Response to a Government Spending Shock derived from Local Projections for emerging and developed countries. All standard errors are clustered robust 68% standard errors.

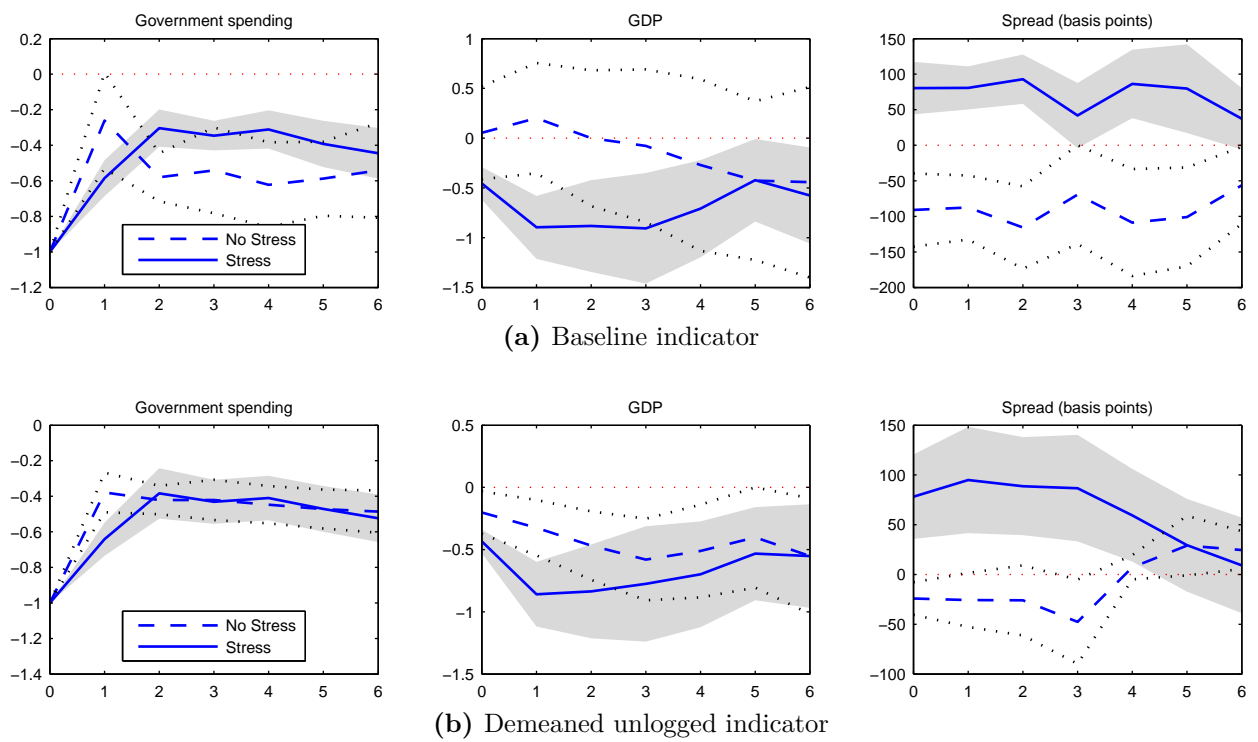


Figure 13: Dynamic Response to a Government Spending Shock derived from Local Projections for the two different indicators. All standard errors are clustered robust 68% standard errors.