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Does austerity pay off?

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Abstract

Policy makers often implement austerity measures when the sustainability of public finances is in doubt and, hence, sovereign yield spreads are high. Is austerity successful in bringing about a reduction in yield spreads? We employ a new panel data set which contains sovereign yield spreads for 31 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 times of fiscal stress, spreads rise in response to the spending cuts, at least in the short-run. In contrast, austerity pays off, if conditions are more benign.

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	confidence, panel VAR, local projections, fiscal stress
JEL-Codes:	E62, E43, C32

1 Introduction

In the years following the global financial crisis, many European governments implemented sizeable austerity measures: spending cuts and tax increases in order reduce budget deficits. These measures 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, measured relative to yields on German government debt, started to take off by 2010, arguably leaving policy makers with no alternative course of action. Yet the dismal growth performance in the following years, coupled with a further rise of yield spreads, lead many observers to question the wisdom of austerity. Against this background we ask whether austerity actually pays off and, if so, under which circumstances. More specifically, we ask whether austerity as such induces a rise or a fall of sovereign yield spreads.

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). Sovereign yield spreads, instead, provide a more comprehensive picture, 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: yield spreads may rise or fall in response to austerity depending, among other things, on the joint response of debt levels and output growth to austerity. Yield spreads are likely to increase, for instance, 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, in particular, 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 (Corsetti, Meier, and Müller, 2012a; Auerbach and Gorodnichenko, 2013a; 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 set up a new data set for sovereign yield spreads. Specifically, we compute spreads for 31 advanced and emerging countries 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 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 yield spreads on common-currency debt provide a clear-cut measure of the real borrowing costs of governments. Its response to austerity thus directly speaks to the question of whether austerity pays off.

We establish a number of basic facts regarding yield spreads. First, they vary considerably across time and countries. In some instances they are virtually zero, in others they are as high as 70 percentage points. A total of about 1850 observations also allows us to compute the empirical density function. It increases sharply for low levels of spreads, as the number of observations for which spreads are high is limited. Second, yield spreads co-move 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 of spreads and government consumption.

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[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 beneficial in terms of macroeconomic outcomes to either frontload or to delay austerity measures (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 financing costs of governments in the long run (De Long and Summers, 2012).

In a second step, we provide estimates on the effects of austerity. We focus on the effects of cuts to government consumption, as identification is somewhat less controversial in this case than in case of tax hikes. 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. A close reading of documents which detail austerity policies during the recent euro area crisis suggests that this holds true also 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 observations up to the year 2014. 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, sovereign yield spreads. Following a large empirical literature on the fiscal transmission mechanism, we rely partly on vector autoregressions (VAR), estimated on our panel data set. More important still, we also employ local projections as introduced by 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.

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 fiscal stress. At the same time output declines considerably. Upon closer inspection, however, this finding masks considerable heterogeneity. Namely, once we condition on fiscal stress, we find that spending cuts reduce yield spreads in the absence of fiscal stress. At the same time, we find fiscal multipliers much reduced or even slightly negative such that spending cuts raise economic activity. On the other hand, in the presence of fiscal stress, spending cuts raise spreads and depress economic activity strongly. These results also obtain for a subsample of countries for which quarterly observations for public debt are available. For this subsample, we document that that debt levels fall relative to output in response to spending cuts only in the absence of fiscal stress. They rise in response to spending cuts in the presence of fiscal stress. Finally, we also find that spreads tend to decline in response to spending cuts in the medium to 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, presumably more often than not, to financial market developments. Still, identifying an exogenous variation in government

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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 particularly large or perhaps because they are implemented under special circumstances. Conditioning the effects of spending cuts on the state of the economy is our strategy to address the second concern.[2]

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 (Guajardo, Leigh, and Pescatori, 2011; Devries, Guajardo, Leigh, and Pescatori, 2011).

Our analysis also provides new results on 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) 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) 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 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 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 and an extensive sensitivity analysis in Section 4. Section 5 concludes.

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[2]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.

2 Data

Our analysis is based on a new data set. It contains quarterly data for government consumption, output, and sovereign yield spreads for 31 emerging and advanced economies. Our econometric strategy below requires the use of quarterly data. 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.[3] In a recent contribution, Ilzetzki, Mendoza, and Végh (2013) have collected non-interpolated quarterly data for government consumption for 44 countries. We construct 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) for set of countries for which we are also able to compute sovereign yield spreads. In the process, we extend their sample to include more recent observations and additional countries where government consumption data based on direct sources is available. Our earliest observations for which we have both spread and government consumption data is 1990Q1 for Denmark and Ecuador. Our sample runs up to 2014.

Table 1 provides summary statistics for the government consumption-to-GDP ratio. 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). 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 the assessment of financial markets regarding 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

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[3]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.

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

Country	first obs	last obs	min	max	mean	std
Argentina	1993.00	2013.50	0.11	0.18	0.13	0.02
Austria	1988.00	2014.00	0.18	0.21	0.19	0.01
Belgium	1995.00	2014.00	0.21	0.25	0.23	0.01
Brazil	1995.00	2014.00	0.19	0.23	0.20	0.01
Bulgaria	1999.00	2014.00	0.14	0.20	0.18	0.02
Chile	1996.00	2014.25	0.05	0.06	0.06	0.00
Colombia	2000.00	2014.00	0.15	0.17	0.16	0.01
Denmark	1990.00	2014.00	0.25	0.30	0.26	0.01
Ecuador	2001.00	2014.00	0.09	0.14	0.12	0.02
El Salvador	1994.00	2014.00	0.06	0.09	0.07	0.01
Finland	1990.00	2014.00	0.20	0.25	0.22	0.02
France	1980.00	2014.00	0.21	0.25	0.23	0.01
Greece	2000.00	2011.00	0.17	0.22	0.18	0.01
Hungary	1995.00	2014.00	0.20	0.25	0.22	0.01
Ireland	1997.00	2013.50	0.14	0.21	0.17	0.02
Italy	1990.00	2014.00	0.17	0.22	0.19	0.01
Lithuania	1995.00	2014.00	0.17	0.28	0.21	0.03
Malaysia	2000.00	2014.00	0.07	0.12	0.10	0.01
Mexico	1993.00	2014.25	0.00	0.00	0.00	0.00
Netherlands	1988.00	2014.25	0.20	0.27	0.23	0.02
Peru	1995.00	2014.25	0.07	0.09	0.08	0.01
Poland	1995.00	2014.00	0.17	0.20	0.18	0.01
Portugal	1995.00	2013.50	0.17	0.22	0.19	0.01
Slovakia	1995.00	2014.00	0.17	0.24	0.20	0.02
Slovenia	1995.00	2014.00	0.17	0.21	0.19	0.01
South Africa	1980.00	2014.00	0.13	0.23	0.19	0.02
Spain	1980.00	2014.00	0.13	0.22	0.18	0.02
Sweden	1993.00	2014.25	0.07	0.10	0.08	0.01
Thailand	1993.00	2014.25	0.09	0.14	0.12	0.01
Turkey	1998.00	2014.00	0.10	0.16	0.13	0.01
Uruguay	1988.00	2014.00	0.10	0.15	0.12	0.01

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. The government consumption-to-GDP ratio is computed as the ratio of nominal variables, except for Uruguay, were we compute it as the ratio of real variables.

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, if duration differs or drifts, yield spreads may still reflect a term premium (see e.g. Broner, Lorenzoni, and Schmukler, 2013). 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.[4] As a result, yield spreads should reflect primarily financial markets' assessment of the probability and extent of debt repudiation by a sovereign.[5]

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.[6] 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, for details, European Central Bank, 2004). 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.[7]

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. In particular, drawing on earlier work by Bernoth, von Hagen, and Schuknecht (2012), we identify bonds denominated in either US dollar or Deutsche mark of at least 5 years of maturity by advanced economies. We compute the spread of yields for those bonds relative to the yields of US or German government bonds of

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

[5]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 for a more detailed discussion.

[6]Note that inclusion 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. Moreover, we rely on stripped spreads (Datastream Mnemonic: SSPRD), which "strip" out collateral and guarantees from the calculation. For more information on the EMBI, see JP Morgan (1999)

[7]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 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).

comparable maturity and coupon yield in order to have similar duration and thus term premia.[8] 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.[9] In case of several bonds being available for overlapping periods, we average over yield spreads using the geometric mean. In order to construct quarterly time-series data, we average yield spreads over the trading days of a given quarter.[10] Note also that 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.

Figure 1 provides some examples of how sovereign yield spreads are constructed. For four countries, it displays the yields of foreign currency bonds jointly with those of the associated benchmark bonds. For three countries (Italy, Denmark, UK), we consider bonds denominated in US dollars, while for Greece we consider a bond issued in Deutsche mark.[11] Note 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. Starting in 1992, we obtain a second bond and compute the yield spread as the

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[8]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.

[9]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 comove very strongly with CDS spreads (whenever they are available), we ignore the issue in the present paper.

[10]Our procedure thus mimics 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 Schmukler, 2013). Because of this, we use these data only for the shortest necessary sample, that is, for EMU countries we rely on the "long-term interest rate for convergence purposes" whenever available.

[11]Italy: 10year $US bond issued on 08/02/1991 with coupon 8.75 percent (XS0030152895); benchmark bond: US 10 year Treasury note issued on 15/11/1990 with coupon 8.25 percent (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 percent (ISIN: US912827WN87). UK: 10year $US bond issued on 12/9/1992 with coupon 7.25 percent (XS0041132845); benchmark bond: US 10 year Treasury note issued on 06/05/1992 with coupon 7.5 percent (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).
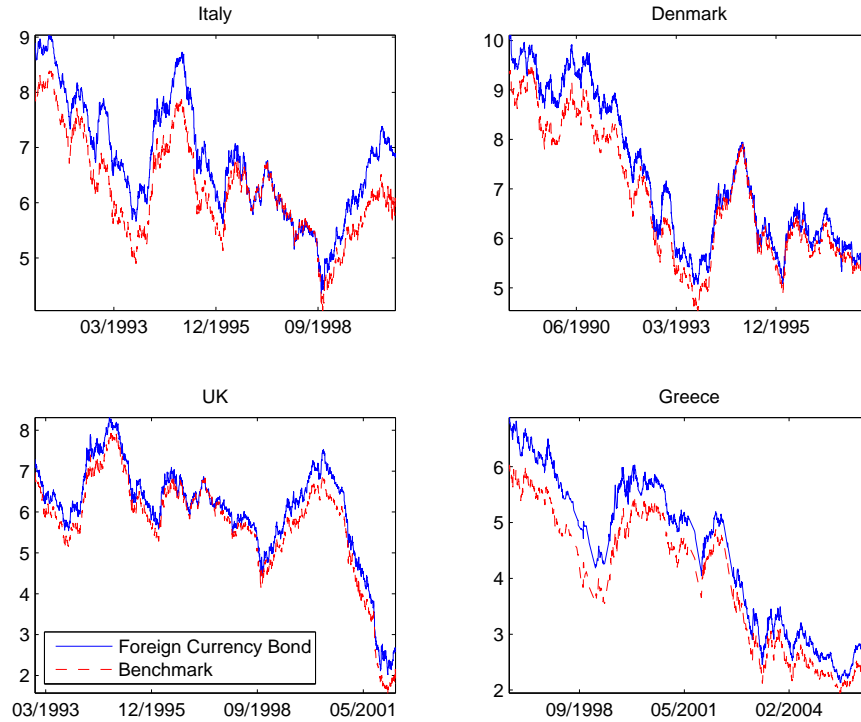
Figure 1: Bond yields to maturity for selected countries. Notes: blue solid line: yield to maturity/redemption yield of the respective domestic bond issued in foreign currency; red dashed line: yield to maturity/redemption yield of a "risk-free" benchmark bond with comparable coupon yield and maturity.

average over those of both 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.

Table 2 provides information on the coverage of our spread sample and some basic descriptive statistics.[12] Spreads s_t are measured in percentage points and vary considerably across our sample. In a couple of countries the lowest realization of the spread is negative, that is, yields fall below those of the reference bond. For the advanced economies group[13] we observe the highest spreads in Portugal (11) and Greece (24). For the emerging economies[14] the highest values are reached in Ecuador (48) and Argentina (71).

Measured relative to these values, most realizations of spreads in our sample are small.

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[12]Figures A.1 and A.2 in the appendix displays the time series on a country-by-country basis.

[13]Austria, Belgium, Denmark, Finland, France, Greece, Ireland, Italy, Netherlands, Portugal, Slovakia, Slovenia, Spain, Sweden, and United Kingdom.

[14]Argentina, Bulgaria, Chile, Colombia, Croatia, Ecuador, El Salvador, Hungary, Lithuania, Malaysia, Peru, Poland, South Africa, Thailand, Turkey, and Uruguay.

Table 2: Basic properties of sovereign yield spreads

Country	first obs	last obs	min	max	mean	std	$\rho(\Delta y_t, s_t)$	$\rho(\Delta g_t, s_t)$
Argentina	1993.75	2014.50	2.10	66.16	15.35	17.68	0.00	-0.20
Austria	1993.75	2014.50	0.00	1.21	0.28	0.26	-0.44	-0.32
Belgium	1991.75	2014.50	0.03	2.52	0.46	0.44	-0.38	-0.17
Brazil	1994.25	2014.50	1.48	18.95	5.64	3.93	-0.03	-0.08
Bulgaria	1994.50	2013.75	0.55	20.37	5.18	4.86	-0.09	-0.04
Chile	1999.25	2014.50	0.57	3.57	1.46	0.58	-0.48	0.19
Colombia	1997.00	2014.50	1.16	8.48	3.50	2.06	-0.40	-0.22
Denmark	1988.50	2002.50	0.02	1.93	0.57	0.42	-0.17	-0.01
Ecuador	1995.00	2014.50	3.97	39.38	12.11	8.33	-0.28	-0.02
El Salvador	2002.25	2014.50	1.32	7.52	3.34	1.23	-0.75	0.01
Finland	1992.25	2014.50	-0.04	0.80	0.27	0.18	-0.44	-0.23
France	1999.00	2014.50	0.02	1.36	0.29	0.31	-0.35	0.03
Greece	1992.25	2014.50	0.16	24.25	3.01	5.23	-0.60	-0.22
Hungary	1999.00	2014.50	0.20	5.90	1.81	1.55	-0.58	-0.07
Ireland	1991.75	2014.50	-0.04	7.93	1.07	1.75	-0.18	-0.39
Italy	1989.00	2014.50	-0.07	4.68	0.84	1.00	-0.41	-0.40
Lithuania	2009.75	2014.50	1.26	4.23	2.55	0.86	-0.14	-0.41
Malaysia	1996.75	2014.50	0.55	7.84	1.76	1.23	-0.61	-0.01
Mexico	1993.75	2014.50	1.02	14.02	3.47	2.54	-0.28	-0.05
Netherlands	1999.00	2014.50	-0.00	0.67	0.20	0.17	-0.65	-0.28
Peru	1997.00	2014.50	1.10	7.79	3.46	1.96	-0.33	-0.08
Poland	1994.75	2014.50	0.48	8.26	1.93	1.39	-0.02	-0.09
Portugal	1993.25	2014.50	0.00	11.39	1.40	2.61	-0.44	-0.40
Slovakia	2008.50	2014.50	0.73	3.40	1.67	0.79	-0.10	-0.16
Slovenia	2006.50	2014.50	0.04	5.11	1.92	1.62	-0.29	-0.40
South Africa	1994.75	2014.50	0.68	6.16	2.26	1.17	-0.50	-0.18
Spain	1992.50	2014.50	0.01	5.09	0.79	1.16	-0.61	-0.45
Sweden	1986.00	2009.50	-0.95	2.95	0.90	0.94	0.34	-0.07
Thailand	1997.25	2006.00	0.48	5.87	1.56	1.16	-0.47	0.19
Turkey	1996.25	2014.50	1.72	10.10	3.97	2.18	-0.34	-0.14
Uruguay	2001.25	2014.50	1.29	13.94	3.86	2.99	-0.25	-0.35

Notes: spreads s_t are geometric averages of daily observations per quarter, measured in percentage points. Last two columns refer to the growth rates of real GDP and government consumption, respectively
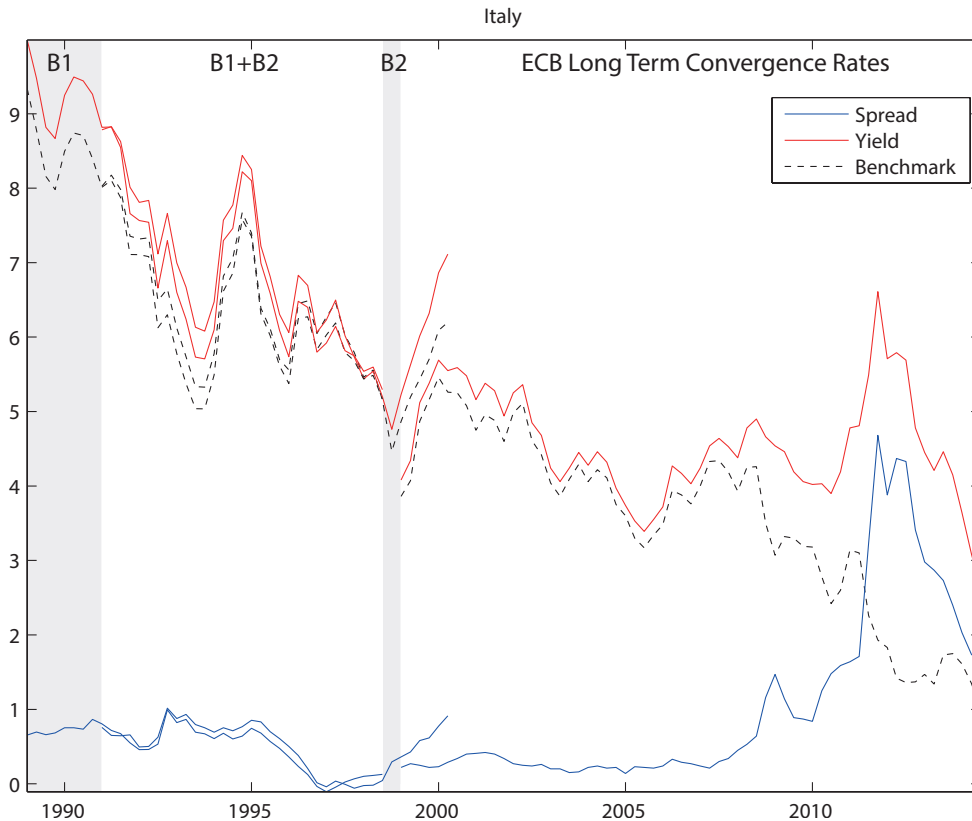
Figure 2: Construction of the Italian yield spread series.

This is apparent from the empirical distribution function (CDF) which we plot in Figure 3 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 1844, of which 846 are for advanced economies. In each case, the mass of observations is very much concentrated on the left. For the full sample, for instance, about 50 percent of the observations for the spread are below 1 percentage point. Still, there are considerable differences across the two country groups: 99.6 percent of observations are below 10 percentage points in the sample of advanced economies. The corresponding number is only 88 percent in the sample of emerging market economies.

An alternative and widely considered indicator of debt sustainability are credit default swap (CDS) spreads.[15] 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,

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[15]For instance, in 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.
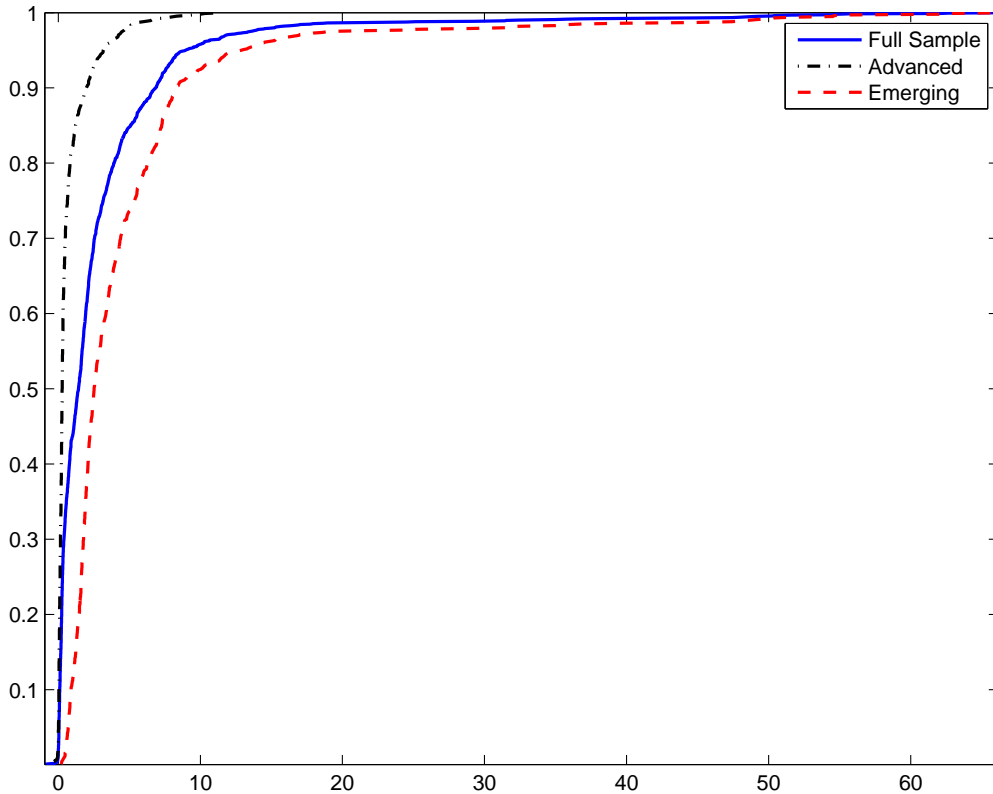
Figure 3: Sovereign yield spreads: empirical distribution function (CDF). Notes: horizontal axis measures spreads in percentage points. Vertical axis measures fraction of observations for which the lagged spread exceeds value on the horizontal axis. Solid line displays CDF for full sample (total number of observations: 1844), dashed-dotted line: advanced economies only (846), dashed line: emerging economies only (998).

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. We find a correlation of 0.92 (see also Figures A.1 and A.2).[16]

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

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[16]This correlation is obtained after discarding observations with spreads above 40 percent, that is, outliers related to the Argentinean default (see the top left panel in Figure A.1).

across countries. It is negative in about two thirds of the countries, but is generally weak. Eventually, we seek to establish the co-movement of spreads 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 Econometric framework

In this section we describe the econometric framework used to establish the effects of austerity on sovereign yield spreads. We first discuss identification and explain how we condition on the results on fiscal stress.

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.[17]

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

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

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.[18] To address this issue, we follow Ramey (2011b) and Auerbach and Gorodnichenko (2013a), 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.

Another popular approach is to identify fiscal shocks on a narrative basis. Following the work of Romer and Romer (2010) for the US, Devries, Guajardo, Leigh, and Pescatori (2011) have constructed a data set of fiscal shock for a large sample of OECD countries. However, these fiscal shocks are identified on a narrative basis with a view to being orthogonal to the business cycle. A large share of these shocks, however, reflects fiscal measures taken in order to reign in public debt or budget deficits. To the extent that sovereign yield spreads comove systematically with the latter, we stress that these "shocks" are not suited to investigate the effect of fiscal policy on sovereign yield spreads.[19]

Yet 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. Finally, note that due to non-availability of appropriate tax data, we do not attempt to identify the effects of tax shocks.

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

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[18]Still, whether or not this invalidates the identification assumption is a quantitative matter (Sims, 2012). Results by Beetsma and Giuliodori (2011), Corsetti, Meier, and Müller (2012b), 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.

[19]Another 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.

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

$$\begin{bmatrix} g_{i,t} \\ y_{i,t} \\ s_{i,t} \end{bmatrix} = \boldsymbol{\mu_i} + \boldsymbol{\alpha_i} t + \sum_{k=1}^{K} \boldsymbol{A_k} \begin{bmatrix} g_{i,t-k} \\ y_{i,t-k} \\ s_{i,t-k} \end{bmatrix} + \boldsymbol{\nu_{i,t}} \,, \tag{3.1}$$

where $\boldsymbol{\mu_i}$ and $\boldsymbol{\alpha_i}$ are vectors containing country-specific constants and time trends. The matrices $\boldsymbol{A_k}$ 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:

$$\boldsymbol{\varepsilon_{i,t}} = \boldsymbol{B}\boldsymbol{\nu_{i,t}} \,, \text{ with } \boldsymbol{\varepsilon_{i,t}} \overset{iid}{\sim} (0, \boldsymbol{I}) \,.$$

In the present context, identifying shocks to government consumption under the assumption that it is predetermined boils down to equating the first element in $\boldsymbol{\nu_{i,t}}$ with a structural fiscal shock.[20]

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 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 (2013a) and Owyang, Ramey, and Zubairy (2013) 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:

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

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

Here $\alpha_{i,h}$ and $\beta_{i,h}t$ are a country-specific constant and a country-specific trend, respectively.[21] $\eta_{t,h}$ in turn captures time fixed effects to control for common macro shocks,[22] which we do not allow for in the VAR model above. $u_{i,t}$ is an error term with strictly positive variance. L denotes the lag operator.[23] At each horizon, the response of the dependent variable to government spending 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. We estimate (3.2) using OLS, assuming that government spending is predetermined (see also Auerbach and Gorodnichenko, 2013b). 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.

The h-period ahead impulse response to a government spending shock, g_{it} conditional on being in a particular regime today, indexed by z_{it}, is given by

$$\left.\frac{\partial x_{t+h}}{\partial g_{it}}\right|_{z_{it}} = F\left(z_{i,t}\right)\psi_{A,h} + \left[1 - F\left(z_{i,t}\right)\right]\psi_{B,h} \tag{3.3}$$

In contrast to the VAR approach of Auerbach and Gorodnichenko (2012), computing the IRFs from our single equation local projection approach does not require assumptions on the economy being in a particular regime after time t (see also the discussion in Ramey and Zubairy, 2014). Rather, the local projection conducted at time t immediately provides the average response of an economy in state z_{it} going forward. Note also that equation (3.3) is just a linear combination of regression coefficients. Thus, it is straightforward to use a Wald-type test to test for the differences between IRFs at a particular horizon in two different states of the world to be significantly different.

Conceptually it is convenient to distinguish two 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 regimes. Rather, they tend to be more or less close to one of the two. This is captured in the estimation, as the projection of the dependent variable at

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[21]Results are robust to using a quadratic time trend.

[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 for both the VAR and the LP model. This is broadly in line with what information criteria recommended and our results are robust to varying the lag length.

each horizon is a weighted average, whereby government consumption as well as the control regressors are allowed to impact the dependent variable differently. The weights, in turn, are a function $F(\cdot)$ of an indicator variable $z_{i,t}$ which provides information of how close the economy is to one of the two regimes. By using this weighted average, all observations between the two polar help in identifying the dynamics in the two regimes.

In our estimation below we use lagged yield spreads $z_{i,t} = s_{i,t-1}$ 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 spread assures that the indicator is orthogonal to our identified government spending shocks. We weigh regressors on the basis of the country-group specific empirical CDF (see Figure 3 above). Formally, we have

$$F\left(z_{i,t}\right) = \frac{1}{N} \sum_{j=1}^{N} \mathbb{1}_{z_j < z_{i,t}} \tag{3.4}$$

where $\mathbb{1}$ denotes an indicator function and j indexes all country-time observations in the respective country group. As an alternative to the empirical CDF, one may assume a specific parametric function in order to map the indicator variable into specific weights.[24] Using the empirical CDF (3.4), 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 were actually obtained in-sample.

4 Results

We now turn to our estimates of 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 a linear model, not accounting for possible state dependence. Instead we contrast results for local projections with those obtained from a VAR model. Afterwards we document that results differ considerably, depending on the

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[24]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 $Prob(z < \bar{z}) = F(\bar{z})$. Parameter γ is set such that 20 percent observations qualify as recessions.

state of the economy. Recall that, in each instant, we obtain identification by assuming that government spending is predetermined within a given quarter.

4.1 Linear model

Figure 4 shows results for the baseline case: the specification which does not allow for state dependent effects, but constrains the effects of fiscal shocks to be linear. The panels in the upper row show impulse responses based on estimates obtained from local projections (LP). Here and in the following the horizonal axis measures time in quarters, while the vertical axis measures the deviation from the pre-shock path. For real 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 indicate 90 percent confidence bounds. The second row shows the impulse responses obtained from the estimated panel vector autoregression (VAR) model.[25] Note that the horizon for which we report impulse responses differs for the LP and the VAR model, as extending the horizon in the former case comes at the expense of degrees of freedom. The VAR results for the first 8 quarters are very similar to the LP results, both from a qualitative and a quantitative point of view.

The first column of Figure 4 shows the dynamic adjustment of government consumption over time. After the initial cut, equal to one percent of GDP, government consumption remains depressed for an extended period, but eventually returns to its pre-shock level (see, also the VAR results shown in the second row). The response of GDP is displayed in the panels of the second column. It declines by about 0.4 percentage points on impact, declines further and reaches a trough response of about -.8 percent of GDP after about 1.5 years. Given that we normalize 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 (see e.g. 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 20 basis points (LP) and 30 basis points (VAR). The maximum effect is about 60 and 45 basis points, respectively. The VAR response shows that the spread returns to its pre-shock level, after mildly undershooting it

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[25]LP confidence bounds are based on Driscoll and Kraay (1998) cross-sectional correlation robust standard errors, VAR confidence bounds are bootstrapped using 100 bootstrap samples.
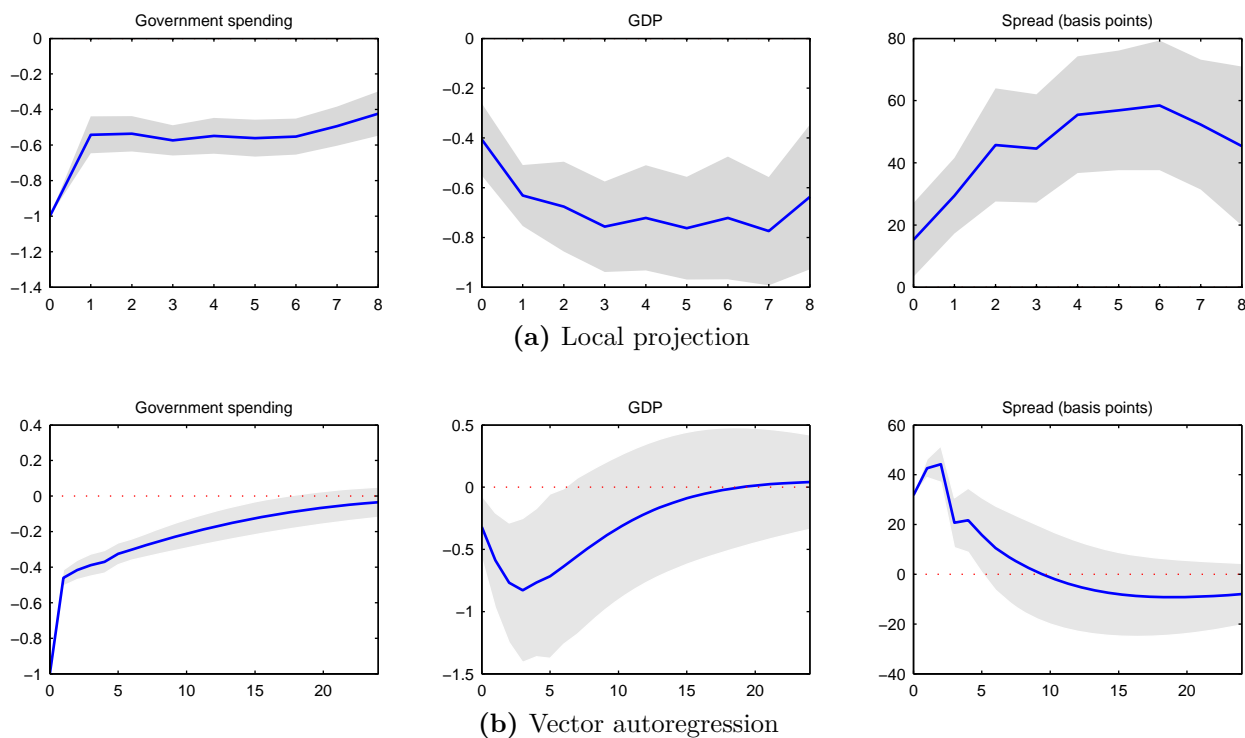
(a) Local projection

(b) Vector autoregression

Figure 4: Dynamic response to an exogenous cut in government spending by 1 percent of GDP in the linear model. Notes: Horizontal axes represent quarters. Vertical axes represent deviation from pre-shock level in terms of trend output and basis points (spread).

for an extended periods. It thus appears that austerity does not pay off: spending cuts fail to reassure investors about the sustainability of public finances.[26]

4.2 Fiscal stress vs benign times

The above result obtains for the entire sample, possibly masking heterogeneity of economic circumstances which may matter for how spreads respond to austerity, both across time and countries. In particular, earlier studies show that fiscal policy may affect the economy differently in "bad times" (Bertola and Drazen, 1993; Perotti, 1999). And indeed, recent evidence established by Corsetti, Meier, and Müller (2012a), Auerbach and Gorodnichenko (2013a) and Ilzetzki, Mendoza, and Végh (2013) suggests that the government spending multiplier on output tends to be relatively low, if debt is high. This

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[26]Perhaps it is also worth pointing out that the movements of spreads over time are not in conflict with the view that financial markets process information efficiently. Spreads move with fundamentals in basic models of sovereign default (see, e.g., Arellano, 2008).

(a) Linear model

(b) Conditional with country group-specific CDFs
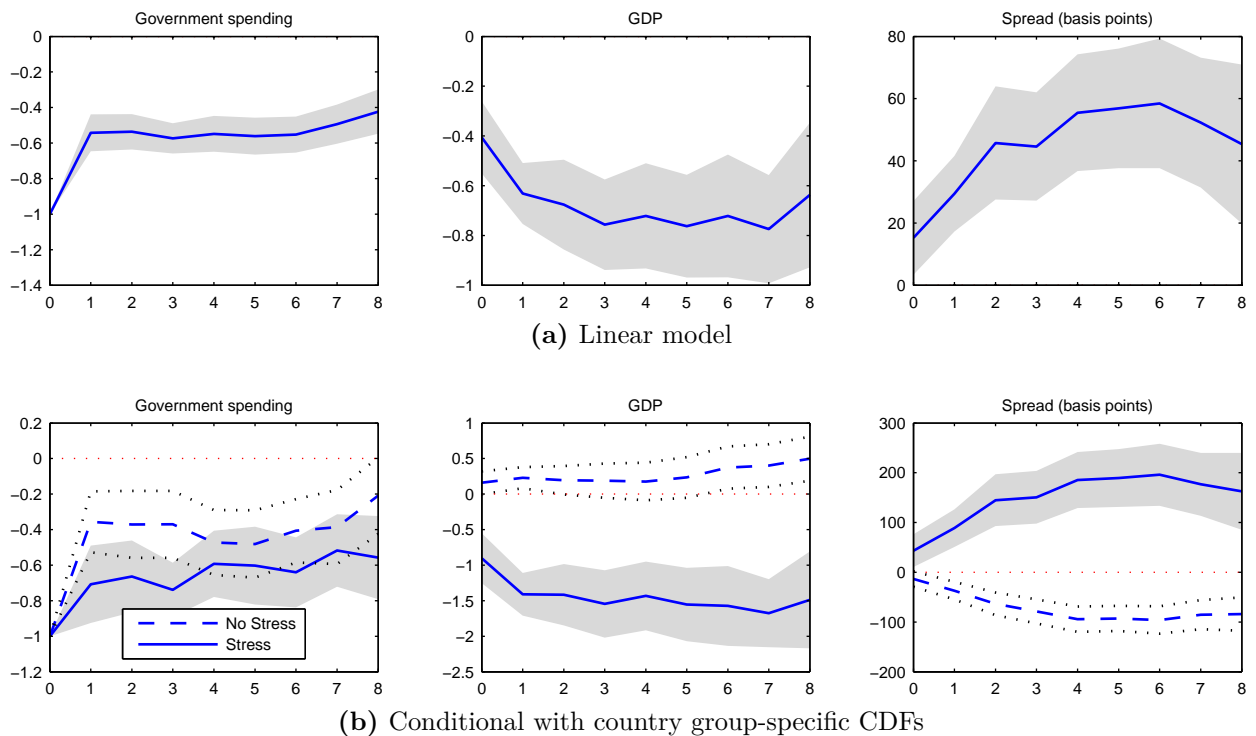
Figure 5: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags).

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. Instead, sovereign yield spreads provide more comprehensive information regarding the extent of "fiscal stress", both because of the underlying—arguably broader—assessment of financial market participants and, not least, because of the immediate budgetary consequences.

In what follows we therefore estimate the non-linear model (3.2) relying on the spreads as an indicator variable and their empirical CDF (3.4) as weighting function. We thus allow the effects of spending cuts cuts to differ depending on whether they take place in a regime of fiscal stress, evidenced by high spread or not ("benign times"). Recall, however, from our discussion in section 3 that we allow for a smooth transition across regimes, as we weigh observations according to the relative size of the spread during a given country-time observation. For the baseline specification, in order to account for fundamental heterogeneity across the set of advanced and emerging economies, we use the empirical CDF obtained for each country group in isolation.

The second row of Figure 5 reports the results for the baseline specification, contrasting it to the results for the linear model (reproduced in the first row of Figure 5). Solid lines represent point estimates for the regime of fiscal stress, with shaded areas indicating 90 percent confidence bounds. Dashed lines represent the estimates for the other polar case, the response to austerity during benign times. Results are rather stark: the dynamic adjustment of the economy under fiscal stress resembles closely that obtained for the unconditional estimates. Hence, fiscal stress episodes dominate estimates for the whole 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 about 1.5, while spreads rise to up to approximately 200 basis points in response to a cut of government consumption.

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 raises output. Although the effect is quantitatively moderated, a significant estimate for the multiplier of about -0.1 for benign times strikes us as remarkable. Importantly, our estimates also suggests that spreads decline in response to cuts in government consumption, provided that the economy enjoys more benign times. In this case spreads come down gradually by about 100 basis points.

We also checked whether the IRFs in both regimes are statistically significant using a Wald-test. After correcting for multiple comparisons, the endogenous government spending response is not significantly different across regimes, while the null hypothesis of equal IRFs is generally rejected for output and spreads at all horizons.

Given that austerity measures are often implemented during times of fiscal stress with a view towards reducing yield spreads, our finding that spreads rise in response to spending cuts may be puzzling. To shed further light on this issue, we also consider the debt-to-GDP ratio in our local projections and compute impulse responses. Figure 6 shows the results. They are obtained for a subsample for which quarterly observations of public debt are available. While it is considerably smaller than the full sample, the estimated effects of spending cuts on output and spreads are fairly similar to those obtained for the baseline specification, notably as regards the difference between fiscal stress and benign times. The response of the debt ratio, shown in the lower-right panel is quite informative: we find that debt rises relative to output 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 spreads across the two regimes. A similar
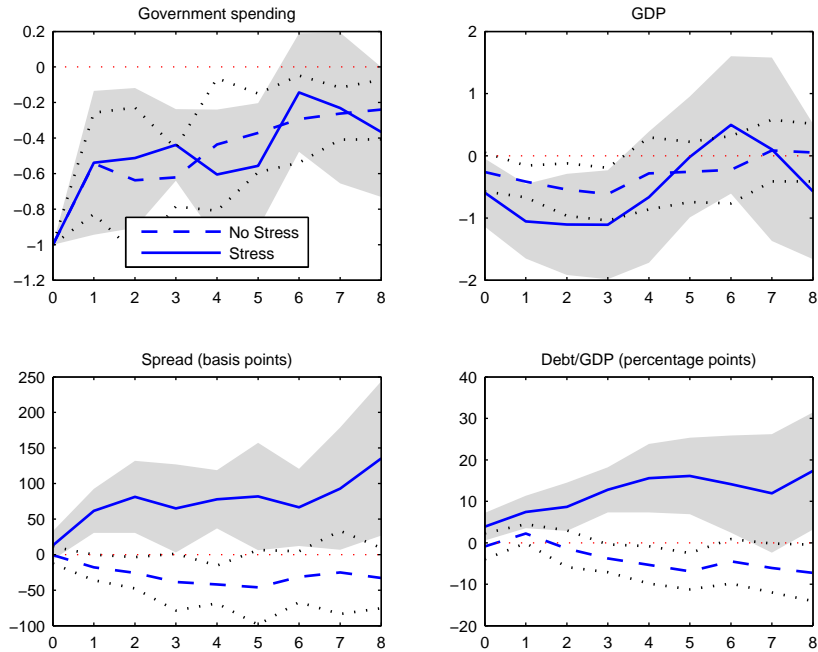
Figure 6: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags) when including the debt/GDP ratio.
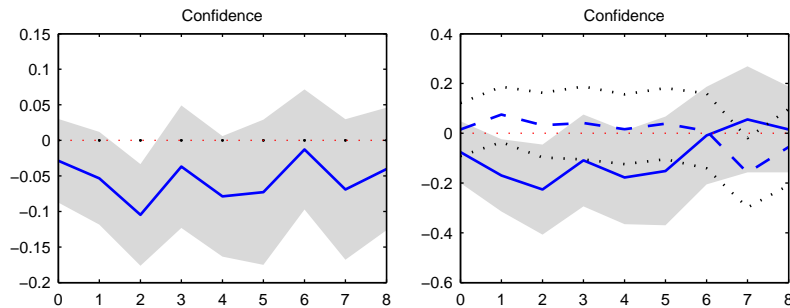

Figure 7: Confidence: dynamic response to an exogenous cut in government spending by 1 percent of GDP. Left panel: linear model (LP). Right panel: responses conditional on fiscal stress (solid lines) and benign times (dashed lines). Confidence, based on the Ifo World Economic Survey, pertains to expectations regarding economic conditions in the next 6 months.

picture emerges, once we consider the deficit ratio rather than the debt ratio (see Figure A.5).

The adjustment of the economy to the fiscal shock thus differs considerably, depending on the two regimes. 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 therefore estimate the dynamic response of confidence to a fiscal shock. For this purpose we rely on the Ifo World Economic Survey (WES), which surveys a number experts for all countries in our sample.[27]

Figure 7 displays the results. In the left panel we show results obtained for the linear model. As in Bachmann and Sims (2012) we find that confidence is fairly flat after a fiscal shock, although there is a marginally significant decline. Interestingly, however, once we condition on fiscal stress (right panel) results change. In times of fiscal stress (solid lines) confidence declines during benign times, in contrast, confidence tends to improve in response to a spending cut. 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 also with a decline in yield spreads.

Times of fiscal stress are mostly likely times of low output growth, for reasons discussed above. Of course, the converse does not necessarily hold: a recession may not give rise to fiscal stress if public finances are in good shape. Still, to put our results into perspective, it is useful to assess to what extent the effects of austerity on spreads change with the state of the business cycle. For this purpose we compute, following Auerbach and Gorodnichenko (2013a), a measure of the output gap.[28] We use it as an indicator variable and compute the empirical CDF as in the case of sovereign yield spreads.

Figure 8 shows the results.[29] 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 relatively large and spreads rise strongly in response to austerity. Conversely, during booms, as in fiscally benign 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, the correlation of the empirical CDF which are used in the projection as weights is only moderate (see Table A.1 in the appendix).

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[27]Respondents are asked to classify their expectations for the next six month using a grid ranging from 1 (deterioration) to 9 (improvement). 5 indicates that expectations are "satisfactory".

[28]First, 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 $\lambda = 160{,}000$. This is the value used in Auerbach and Gorodnichenko (2013a) adjusted for our quarterly sample following Ravn and Uhlig (2002).

[29]The figure looks almost identical if we use a logistic transition function instead of the empirical CDF
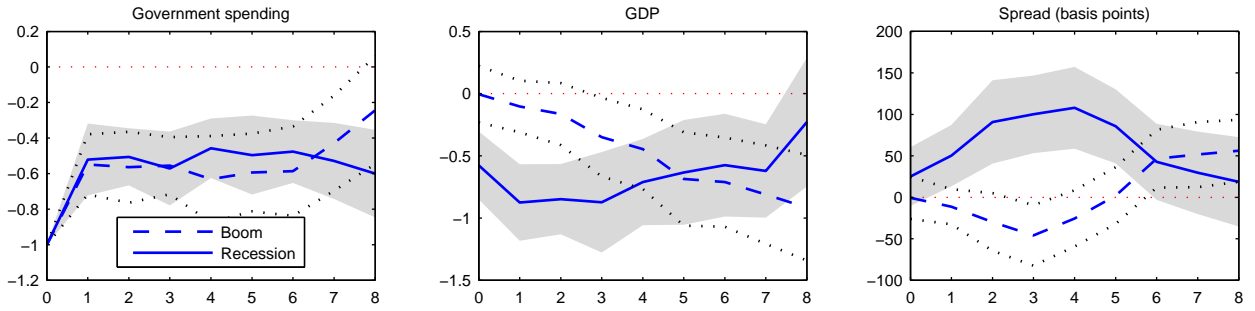
Figure 8: Dynamic response to an exogenous cut in government spending by 1 percent of GDP during booms and recessions. Notes: output gap used as indicator variable.
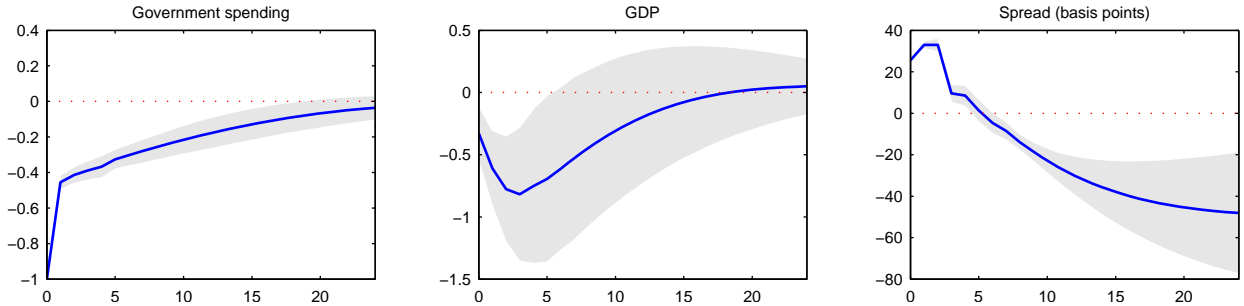

Figure 9: Responses to cut in government consumption with possible long-run effect on spread. Notes: estimates based on VAR model, with spread entering in first differences. Right panel shows cumulative response.

A robust finding of our analysis is that spending cuts during times of fiscal stress do not pay off: they induce spreads to rise. The same result obtains in the linear model. However, our focus so far has been on the short-run, that is, the first 1-2 years after the shock. Results for the VAR model, reported in the bottom row of Figure 4, instead, suggest that spreads may decline in the medium-term. In the long-run, however, spreads return to the pre shock level. This is unsurprising, as the spread enters the VAR model in levels. We therefore consider an alternative specification in which the spread enters the VAR in first differences. Figure 9 displays the impulse responses. The right panel is of particular interest. It shows the cumulative response of yield spreads. The short run dynamics are comparable to our baseline specification, but the medium-term dynamics are quite different. Spreads fall considerably. In the long-run, we find a permanent decline of about 50 percentage points.

to construct the indicator.

4.3 Sensitivity analysis

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 identification. Importantly, as discussed above, under the Blanchard-Perotti identification 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 (Ramey, 2011b; Leeper, Walker, and Yang, 2013). 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, that is, at the end of an observation period.[30]

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.[31] Figure 10 displays the results, obtained for the sample for which government spending forecasts are available. In the first row we contrast results based on forecast errors (solid lines) and those obtained under our baseline approach (dashed lines). It turns out that explicitly accounting for anticipation does not alter results very much (see also Beetsma and Giuliodori, 2011; Corsetti, Meier, and Müller, 2012b; Born, Juessen, and Müller, 2013). In the second row we show that the main result of our analysis, the differential effect of spending cuts during times of fiscal stress and benign times, also obtains, once identification is based on forecast errors.

Our results are based on the identification assumption that government spending is predetermined relative to output and yield spreads. This assumption is plausible to the extent that decision lags prevent an immediate discretionary policy response to either the cycle or fiscal stress. Still, in the light of our finding that spending cuts raise spreads, one may worry that we actually pick up reverse causation: spending falls as spreads rise—despite the fact

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

[31] We use growth rates rather than levels, because the base year used by the OECD changes several times during our sample period.
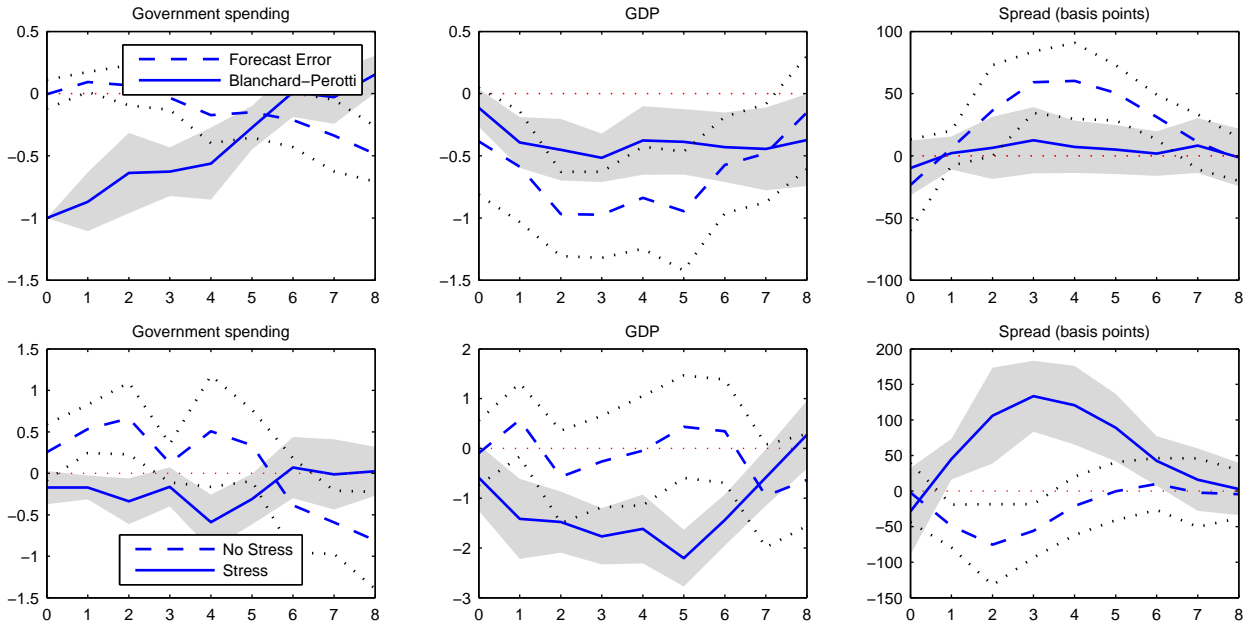
Figure 10: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections in the linear model (four lags). Upper row: Solid line: Identification using OECD forecast errors. Dashed line: Blanchard/Perotti identification. Lower row: responses conditional on fiscal stress (solid lines) and benign times (dashed lines) using forecast error identification. The country sample is restricted to observations with available OECD forecast errors.

that a within-quarter adjustment of spending is unlikely due to institutional constraints. To alleviate this concern, we consider an alternative specification, where we include the end of quarter value of the spread instead of the average quarter spread in the control vector so that GDP (a flow variable within the quarter) cannot react to it. Figure 11 displays the results, both for the linear model (top row) and the smooth transition model (bottom row). We find that results are qualitatively unchanged relative to those obtained for the baseline specification.

In a second set of experiments we explore the robustness of our results with respect to changes in the composition of the sample. First, we consider the full sample, but, in contrast to the baseline specification, use a common empirical CDF as a weighting function. Panel (a) of Figure A.6 in the appendix shows the results which are quite similar to those obtained for the baseline specification. Next, we exclude the Great Recession from our sample, that is, we consider only observations up to the second quarter of 2007. Figure A.7 in the appendix shows the results based on the LP approach, distinguishing unconditional
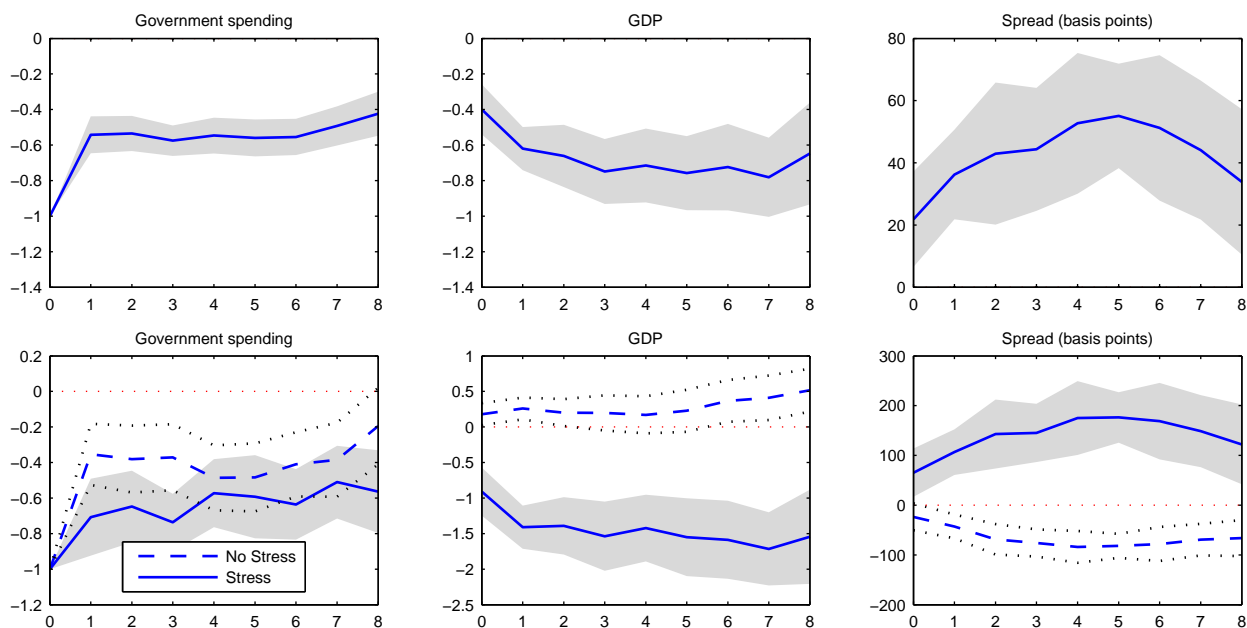
Figure 11: Dynamic response to a government spending shock derived from local projections (four lags). Top row: linear model; bottom row: responses conditional on fiscal stress (solid lines) and benign times (dashed lines). Spreads measured at the end of the quarter instead of quarterly average.

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.

In our baseline specification we measure spreads in percentage points. Benign times are effectively characterized by spreads of about zero. Impulse responses computed for regime of bening times, however, thus suggests that spreads fall below the risk-free benchmark, which is hard to interpret. To analyze the consequences of the potential misspecification of using a linear model for spreads (conditional on a fixed regime), we explore entering spreads in logs into our baseline regression. The results, shown in Figure A.8, are qualitatively similar, with spreads in the benign times regime staying roughly constant. Due to the easier interpretation of the IRFs from the spreads entered in levels as elasticities, we keep them as our baseline.

We also assess 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 A.9, 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 A.10. As a caveat, however, we note that there are sizeable differences in some instances, reflecting perhaps also a strong decline in sample size.

One may also be concerned with the issue of sample selection. Only governments with relatively large financing needs may 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 a large debt and thus spreads and Euro area countries with historically low spread observations. We check the robustness of our results by also using a logistic transition function (see also Auerbach and Gorodnichenko, 2012). Figure A.11 shows the results, which are similar to the baseline.

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).[32] 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.[33] 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.12 presents the results. In general, they are similar to those obtained for our baseline specification.

Finally, 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.,

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[32]Both the System of National Accounts 1993 (European Commission, 1993) (implemented in the EU as the European System of National Accounts 1995 (European Commission, 1996)) and its more recent version, the System of National Accounts 2008 (European Commission, 2009b) (implemented in the EU as the European System of National Accounts 2010 (European Commission, 2013)) specify this item to be included. All countries in our sample use on these frameworks. The item considered, [D6 311 + D63 121 + D63 131]/S13, covers for example the reimbursement of private households's 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.

[33]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.

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, because independent central banks, so the argument goes, could act as a lender of last resort to government. This possibility may be sufficient to rule out speculative runs on governments. To explore this possibility for our data set, we consider results of countries that are either members of a monetary union or have officially dollarized.[34] Figure A.13 shows the results for this case and for the case of countries that have their own legal tender.

5 Conclusion

Does austerity reduce 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 31 emerging and advanced economies. We assemble quarterly observations from 1990 to 2014, 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, while there is a large variation in yield spreads, both across time and countries, yield spreads are moderate for the largest part of our sample. Second, yield spreads are strongly countercyclical. The correlation of yield spreads and current output growth is negative in almost all countries of our sample. Third, there is no systematic correlation pattern emerging for yield spreads and government consumption.

As a second contribution, we assess how yield spreads react to austerity measures. If we do not condition on the state of the economy, we find that a cut of government consumption raises sovereign yield spreads in the short run. At the same time output declines considerably. A cut of government consumption by one percent of GDP raises spreads by some 60 basis points and reduces economic activity by about 0.8 percentage points. It turns out that these effects are driven by episodes of fiscal stress. If we condition estimates on fiscal stress, captured by high yield spreads, we find that spending cuts have an even stronger effect on spreads: they increase by about 200 basis points. Similarly, the adverse output effects are also amplified in this case.

Instead, if the economy enjoys more benign times, spending cuts pay off in that they bring about a sizeable reduction of yield spreads (about 100 basis points). In this case, output is hardly affected by the cut of government consumption. Moreover, for the linear model,

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[34]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.

we also find that spreads tend to decline in the long run once economic activity has fully recovered. In sum, the data reveal a very robust pattern: yield spreads tends 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 yield spreads. These findings are consistent with the view that financial markets are primarily concerned with output growth.[35]

Austerity may pay off in the long-term or if it is implemented during benign times. Under adverse fiscal 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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[35]Historically, 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). While it is unclear to what extent these factors will play an important role in stabilizing debt levels in the years to come, they are arguably no viable means in order to meet market pressures instantaneously.

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A Appendix

A.1 Spread decomposition

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

$$r_t^{nom} = r_t^{real,riskfree} + E_t(\pi_{t+1}) + RP_t^{Infl} + E_t(\delta_{t+1}) + RP_t^{default} + RP_t^{term} + RP^{liqu} + \varepsilon_t, \quad (1.5)$$

where $r_t^{real,riskfree}$ is the real risk-free interest rate, $E_t(\pi_{t+1})$ is the compensation for expected inflation, RP_t^{Infl} denotes the premia for inflation risk and RP_t^{term} the term premia.[36] We are mostly interested in the next two components, the compensation for expected default $E_t(\delta_{t+1})$ and the default risk premia $RP_t^{default}$. The term RP_t^{liqu} captures liquidity risk premia, while ε_t captures other (higher order) terms. In order to isolate the terms that interest 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

$$r_t^{*,nom} = r_t^{real,riskfree} + E_t(\pi_{t+1}) + RP_t^{Infl} + RP_t^{term} + RP_t^{*,liqu} + \varepsilon_t^*. \quad (1.6)$$

The default-related terms are zero. The real risk-free interest rate, the inflation premia, and the term premia will be the same as in Equation (1.5) due to considering a bond in the same currency with the same maturity. A yield spread computed this way will thus only contain the default-related premia 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 premia should be small. Second, risk premia consist of the price of risk times the quantity of risk. With integrated financial markets, the price of risk tends to be a common factor that will be accounted for by our time fixed effects, leaving only the quantity component of liquidity risk as a confounding factor. Finally, even when considering only a sample of advanced economies with very liquid markets or when ending the sample before the recent financial crisis where liquidity did dry up, does not qualitatively affect our results.

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[36]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.

A.2 Price of Risk and Quantity of Risk

Our spread measure reflects the quantity of risk times the price of risk. This price of risk may be time-varying with global risk aversion (see e.g. Bekaert, Hoerova, and Duca, 2013). This should not be a problem in our setup as the price of risk component should be global and thus is captured by our time fixed effects. This is equivalent to including the VIX as a control. However, our indicator is also based in spreads and thus depends on the price of risk as well. This would not affect the cross-section of our indicator, but the time series as the price of risk will be high for all countries at a particular point in time. However, even when dropping the Great Recession where the price of risk suddenly spiked, results are similar, alleviating concerns about this potential confounding factor.

A.3 Additional Figures and Tables

Table A.1: Descriptive statistics: indicators

Country	$mean\,(F^{stress})$	$mean\,(F^{recess})$	$corr\,(F^{stress}, F^{recess})$
Argentina	0.84	0.50	0.22
Austria	0.39	0.49	0.36
Belgium	0.51	0.50	0.38
Brazil	0.65	0.52	0.09
Bulgaria	0.43	0.49	0.80
Chile	0.22	0.51	0.73
Colombia	0.51	0.51	0.29
Denmark	0.57	0.54	0.23
Ecuador	0.90	0.52	0.12
El Salvador	0.55	0.44	0.52
Finland	0.43	0.45	0.28
France	0.36	0.47	0.22
Greece	0.61	0.52	0.66
Hungary	0.25	0.44	0.38
Ireland	0.48	0.49	0.11
Italy	0.60	0.50	0.19
Lithuania	0.50	0.41	0.03
Malaysia	0.23	0.48	0.51
Mexico	0.51	0.50	0.11
Netherlands	0.32	0.51	0.42
Peru	0.53	0.57	0.41
Poland	0.29	0.52	0.41
Portugal	0.49	0.51	0.16
Slovakia	0.88	0.66	0.21
Slovenia	0.77	0.51	0.54
South Africa	0.37	0.49	0.52
Spain	0.47	0.50	0.49
Sweden	0.59	0.54	-0.13
Thailand	0.23	0.50	0.44
Turkey	0.62	0.49	0.17
Uruguay	0.56	0.48	-0.17

Notes: F^{stress} denotes the values of the country group-specific empirical CDF of the lagged spread; 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.
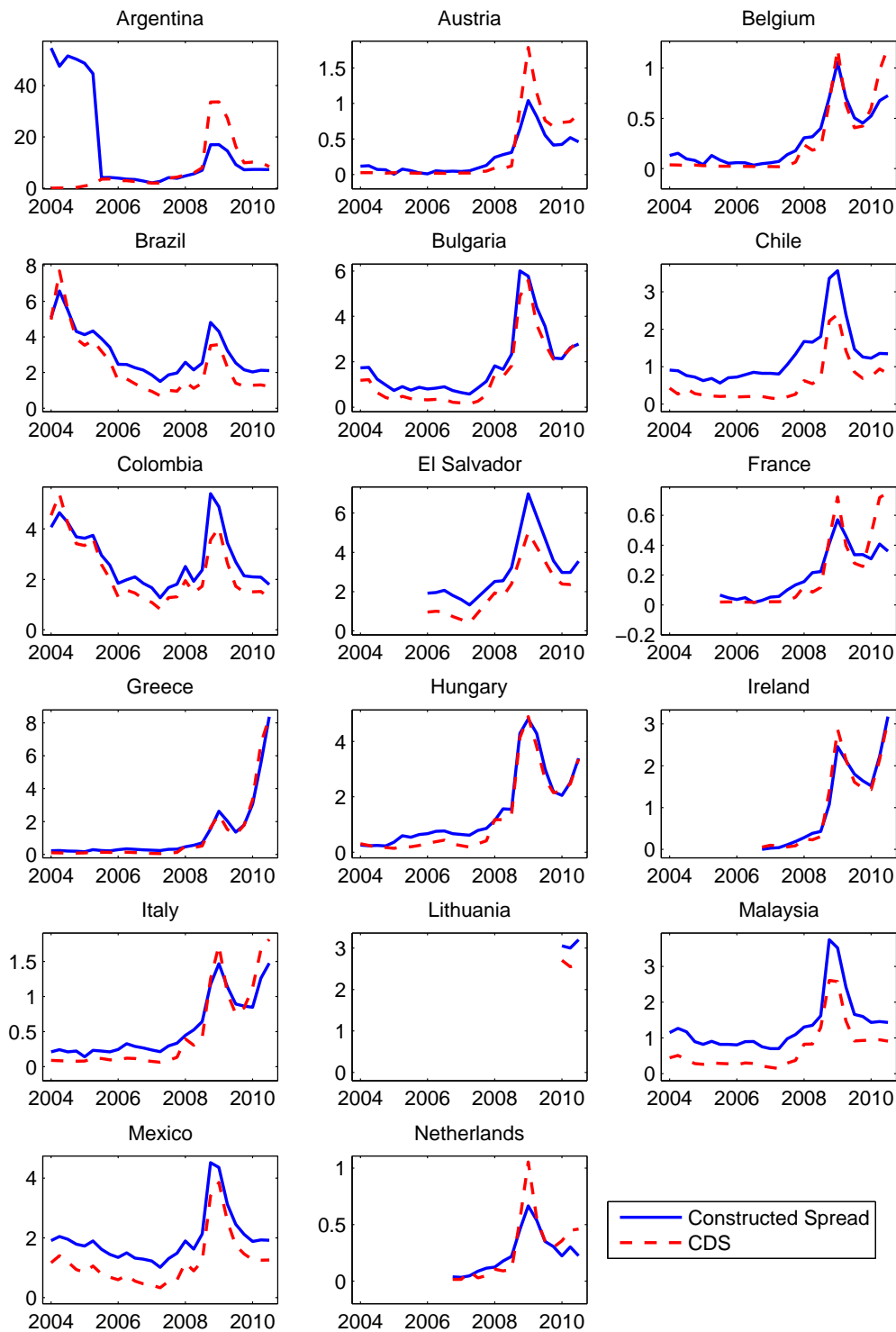
Figure A.1: Comparison CDS vs constructed spreads for selected countries. Blue solid line: 5year CDS yields on government bonds. Red dashed line: yield spread series on government bonds, constructed as described in the text.
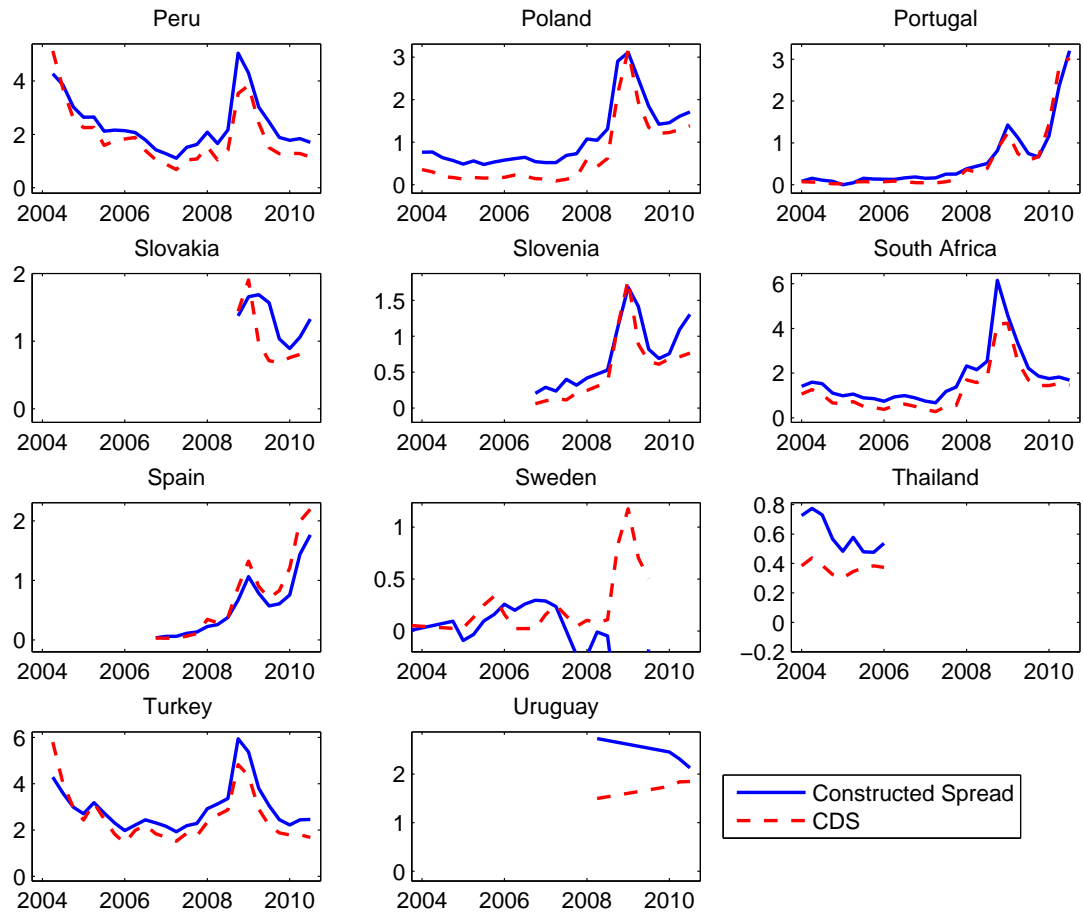
Figure A.2: Comparison CDS vs constructed spreads for selected countries. Blue solid line: 5year CDS yields on government bonds. Red dashed line: yield spread series on government bonds, constructed as described in the text.
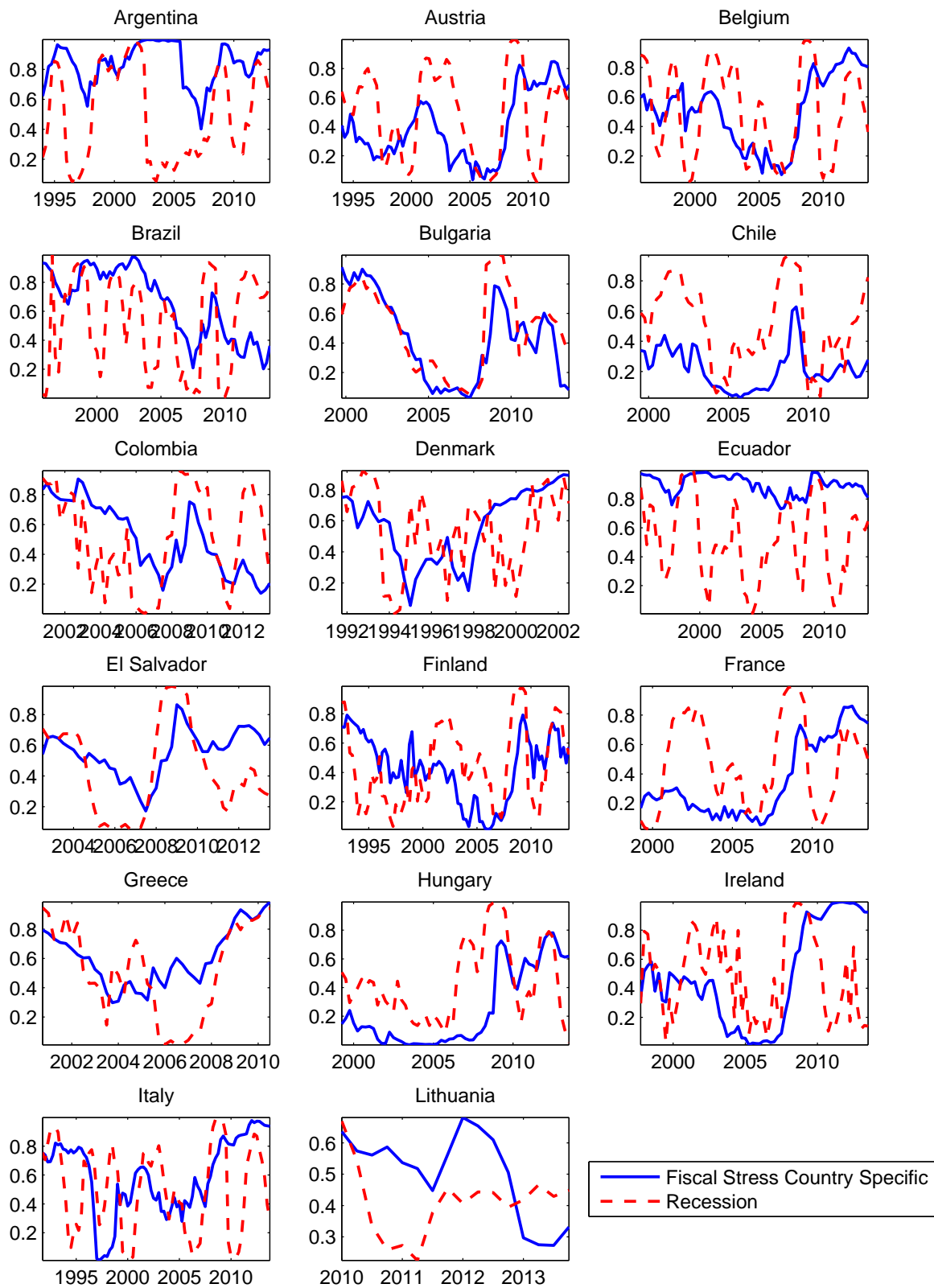
Figure A.3: Empirical CDF values for spreads and smoothed output gaps.
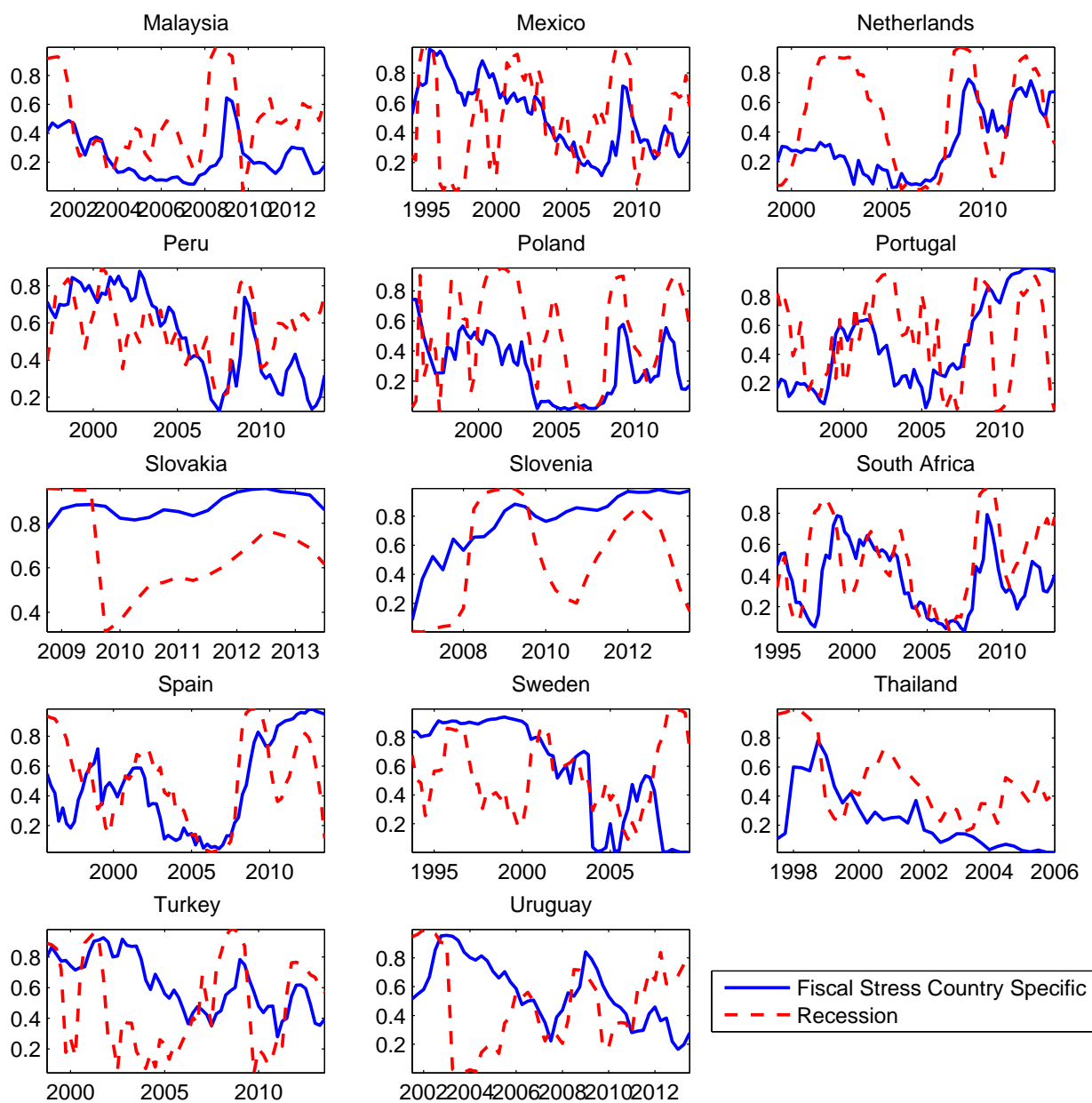
Figure A.4: Empirical CDF values for spreads and smoothed output gaps.

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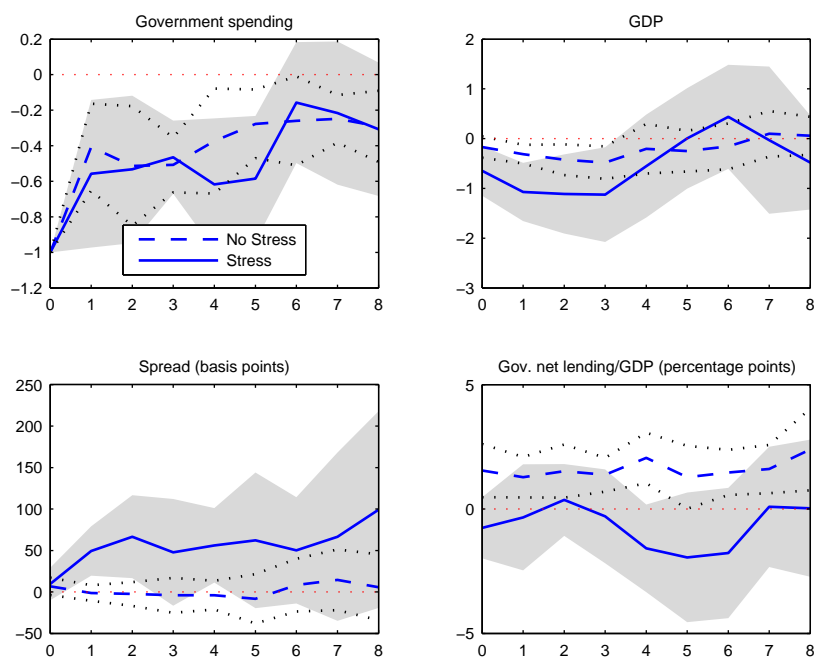
Figure A.5: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags) when including the deficit/GDP ratio.

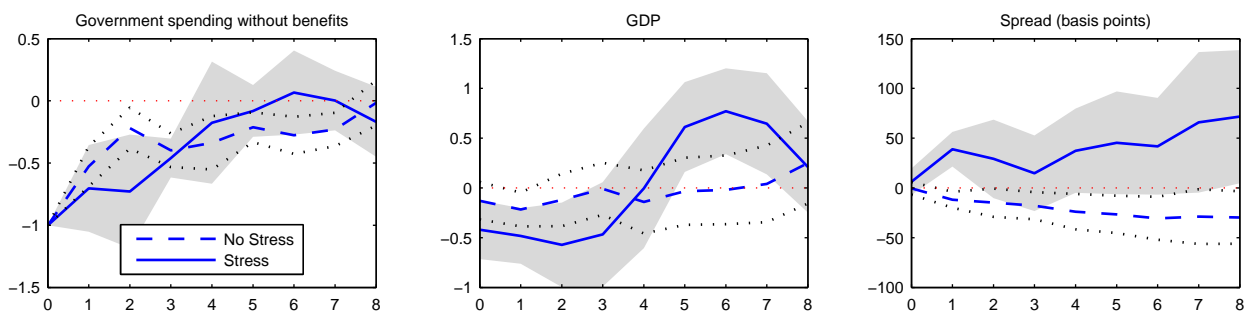**(a)** Pooled, average spread

(b) Pooled, end of quarter spread
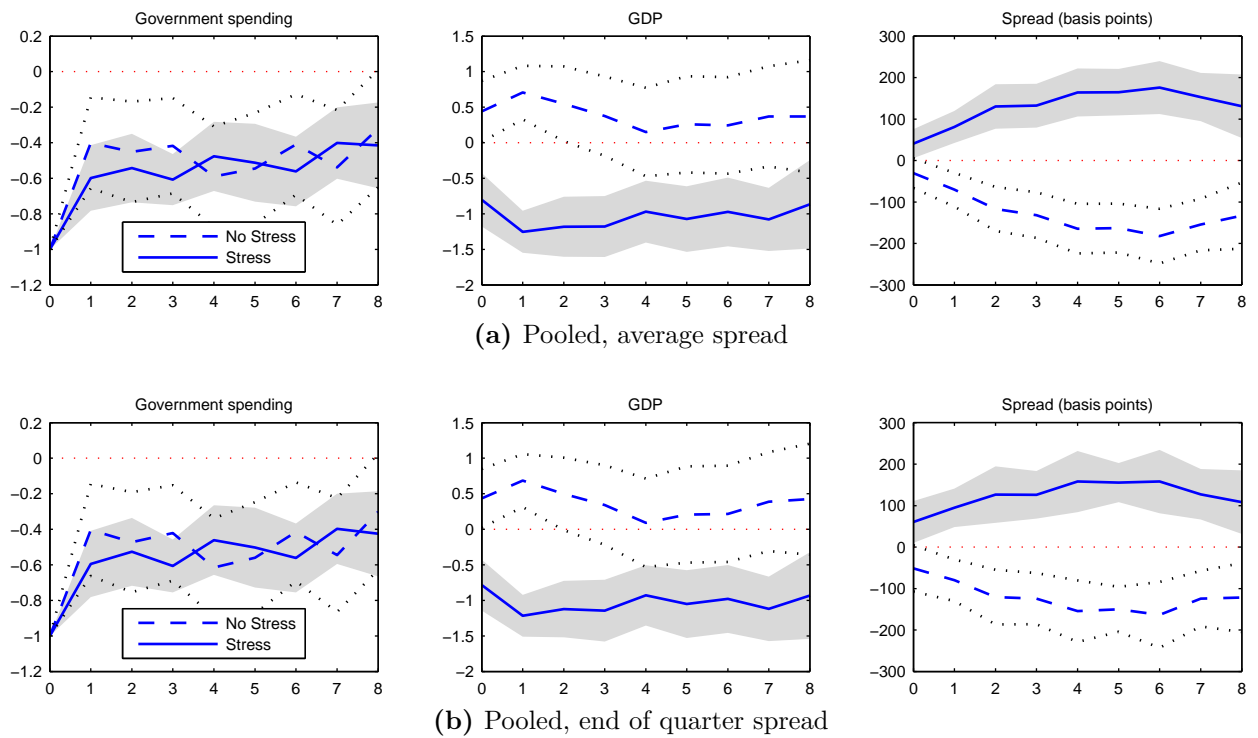
Figure A.6: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags). Panel (a): using a pooled empirical CDF based on average quarter spreads instead of a country group-specific one. Panel (b): using a pooled empirical CDF based on end of quarter spreads instead of a country group-specific one.
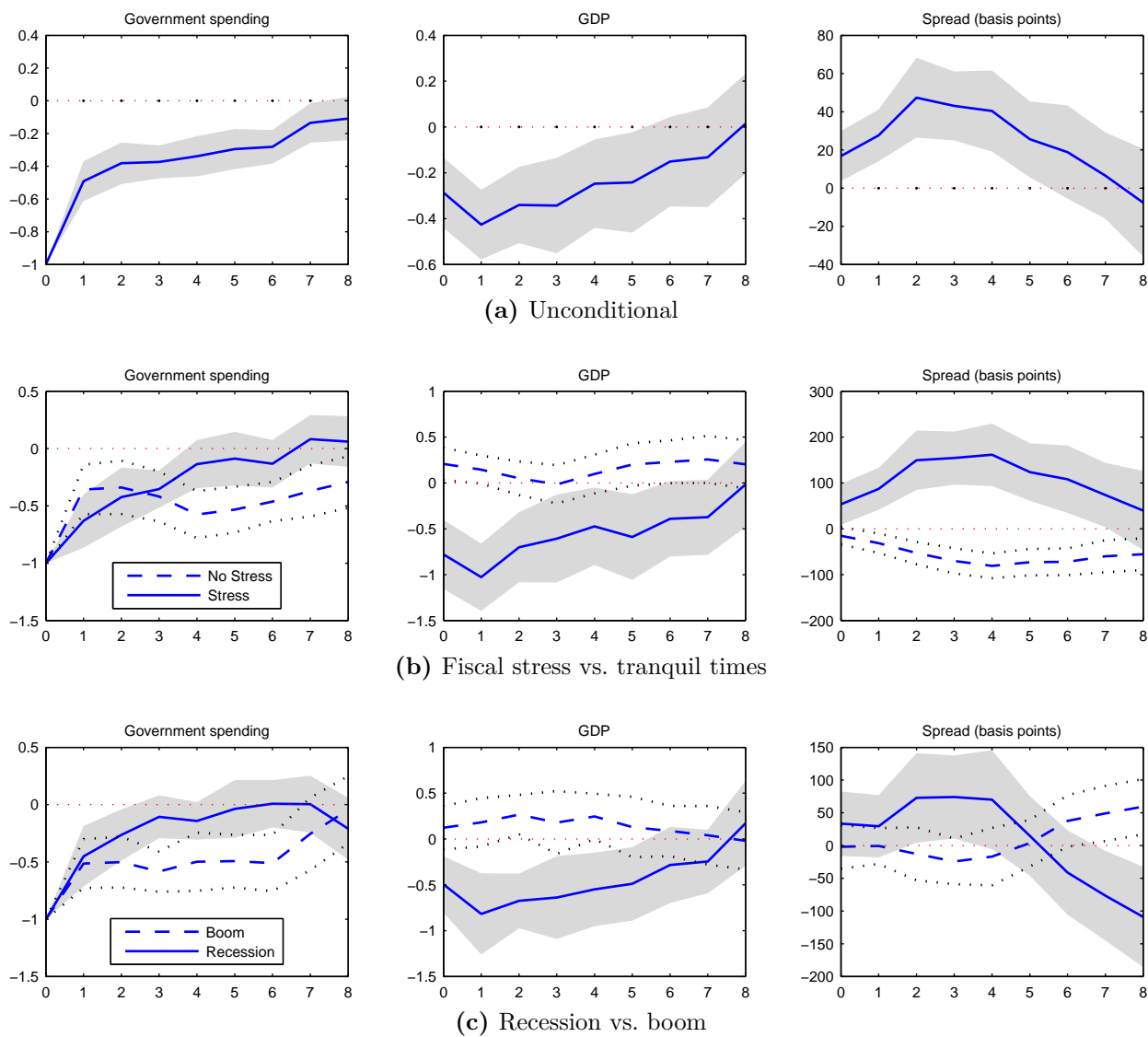
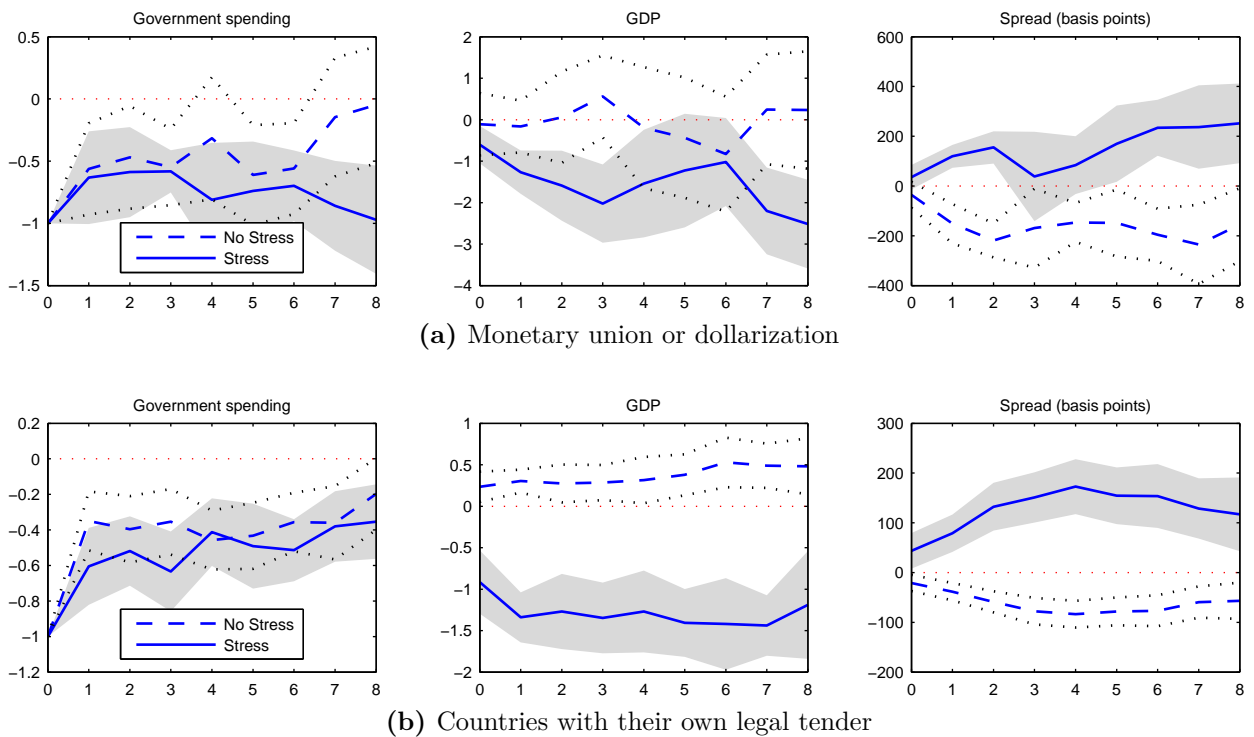**Figure A.7:** Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags) when excluding the Great Recession.
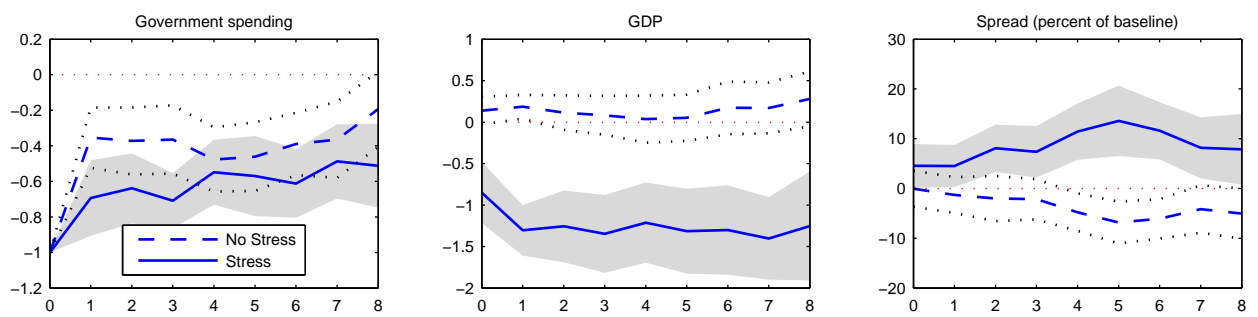
Figure A.8: Average quarter spreads in logs: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags).
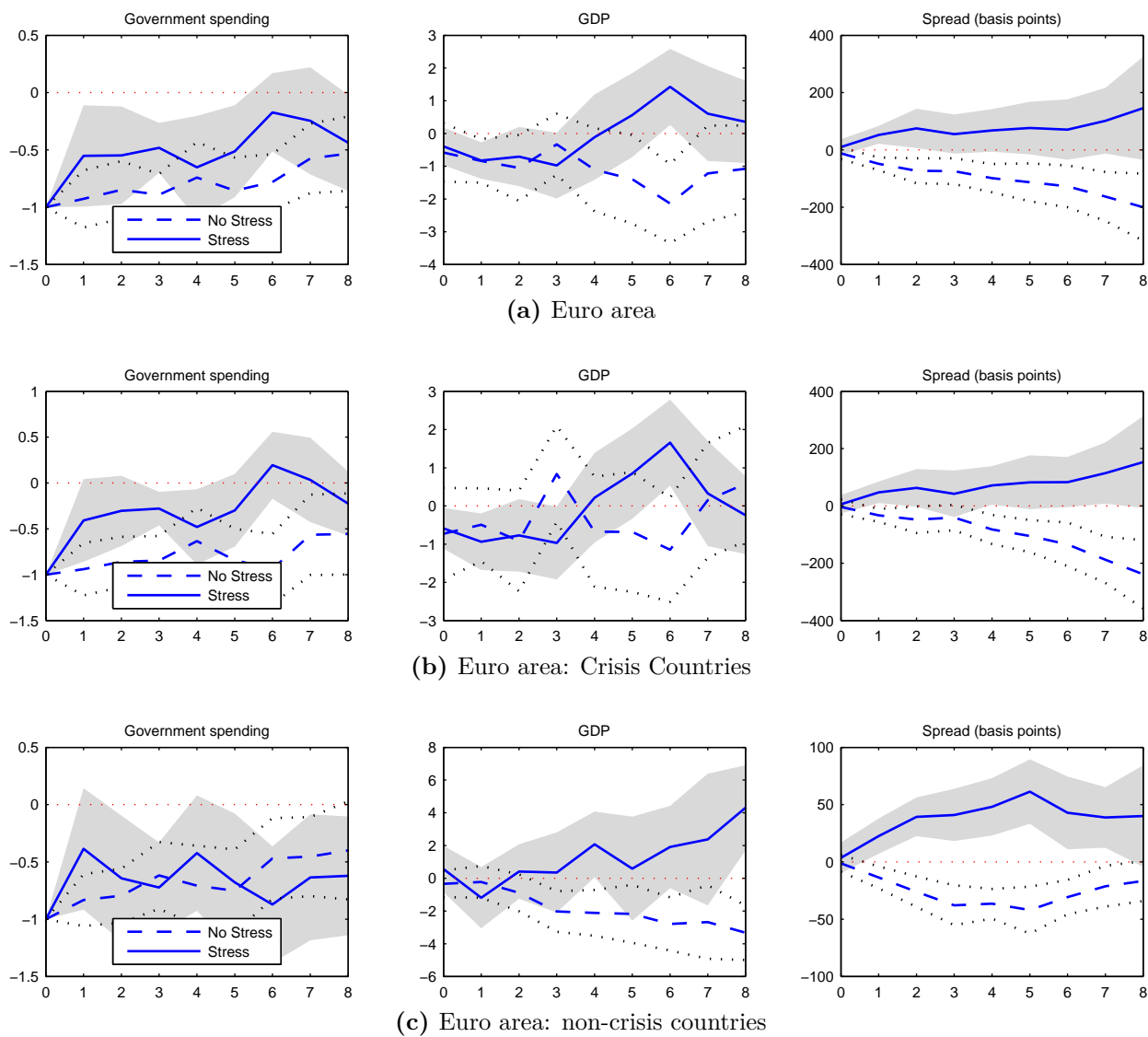
Figure A.9: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags) when excluding the Great Recession.
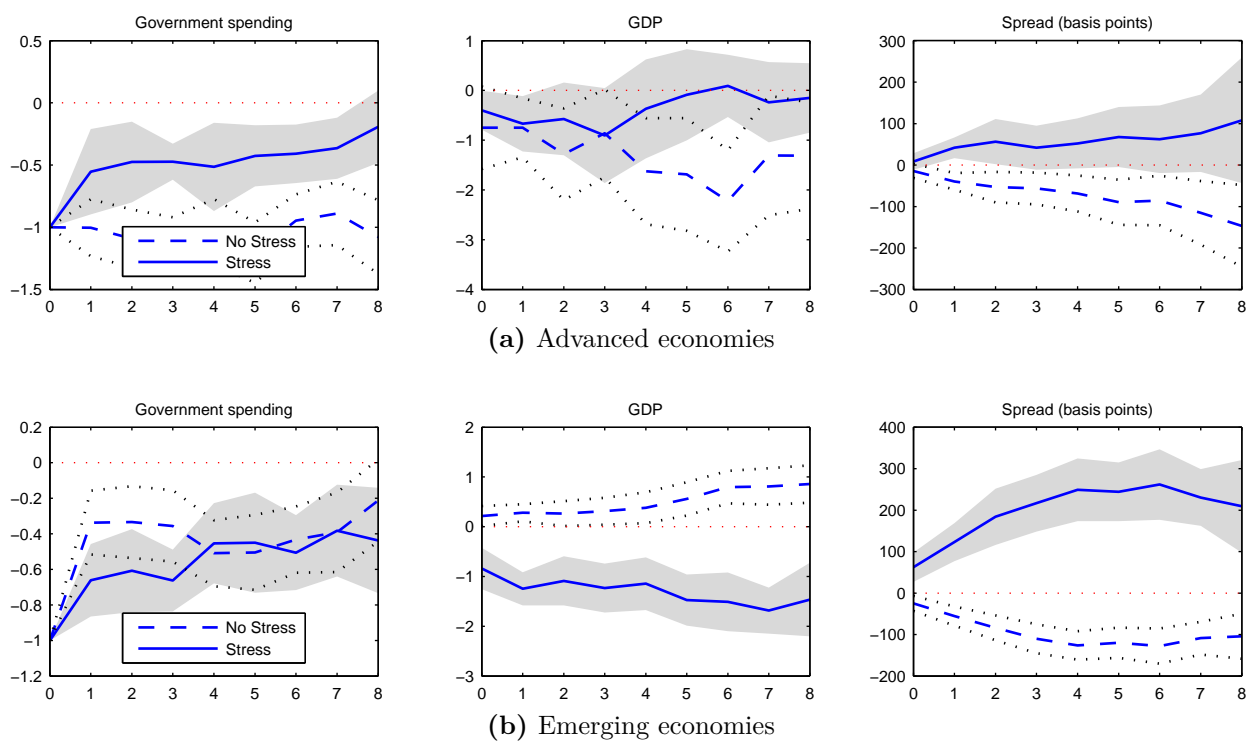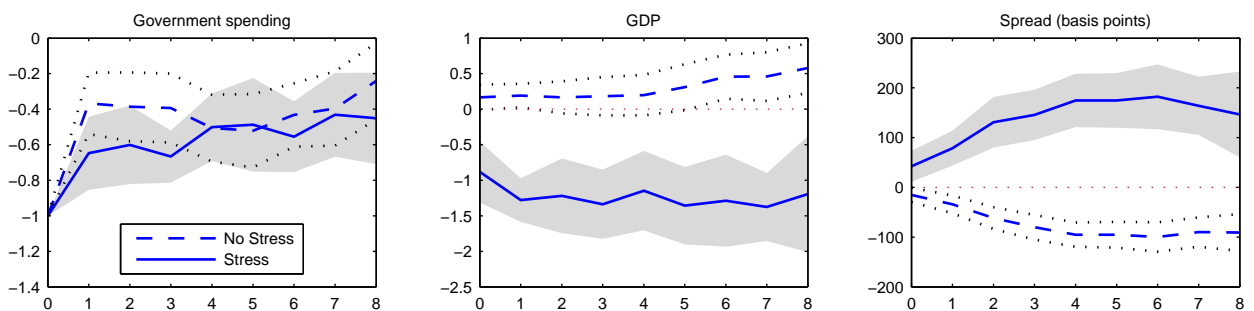
Figure A.10: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags) when excluding the Great Recession.
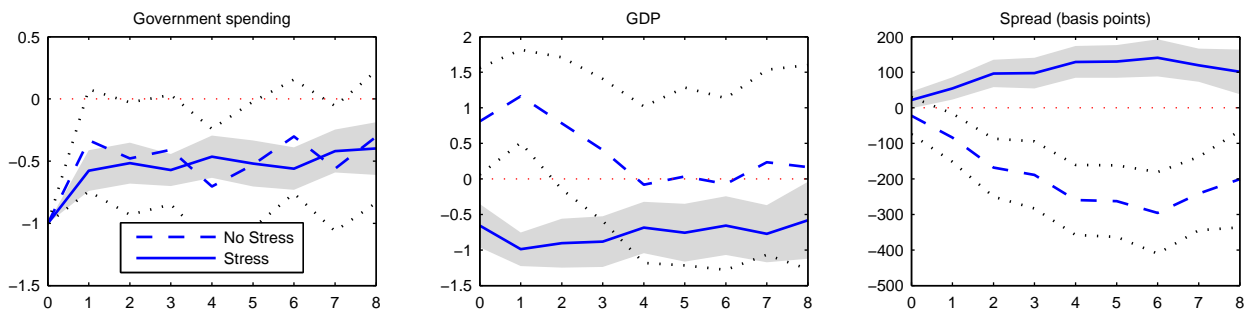
(a) Parametric indicator, average spread
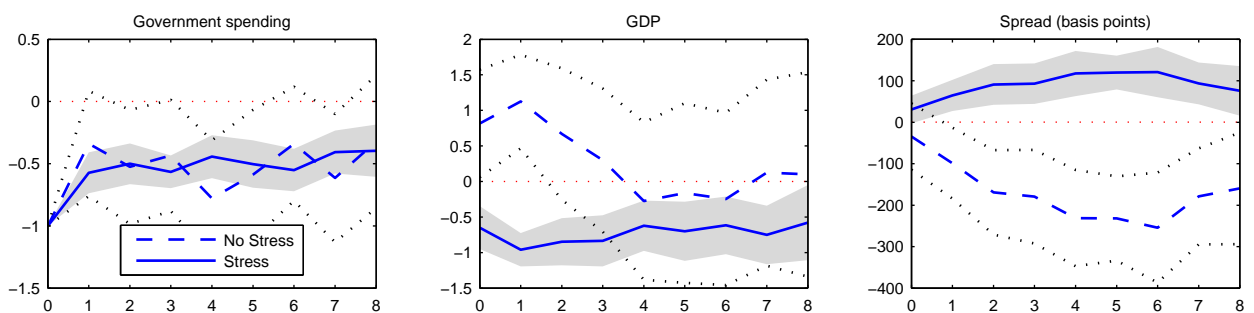
(b) Parametric indicator, end of quarter spread

Figure A.11: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags). Panel (a): using a parametric logistic transition function with $\gamma = 1.3$ based on average quarter spreads instead of a country group-specific empirical CDF. Panel (b): using a parametric logistic transition function with $\gamma = 1.3$ based on end of quarter spreads instead of a country group-specific empirical CDF.

Figure A.12: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags) when using government consumption without benefits in kind.

(a) Monetary union or dollarization

(b) Countries with their own legal tender

Figure A.13: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags). Top panel: restricting the sample to country-quarter observations that were members of monetary unions or de jure dollarized. Bottom panel: restricting the sample to country-quarter observations with their own legal tender.

Figure A.14: Dynamic response to an exogenous cut in government spending by 1 percent of GDP derived from local projections (four lags) using a conservative sample where we could confirm that government spending data was derived from direct sources.

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