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RESEARCH ARTICLE

Is it the "How" or the "When" that Matters in Fiscal Adjustments?

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Abstract Using data from 16 OECD countries from 1981 to 2014 we study the effects on output of fiscal adjustments as a function of the composition of the adjustment—that is, whether the adjustment is mostly based on spending cuts or on tax hikes—and of the state of the business cycle when the adjustment is implemented. We find that both the "how" and the "when" matter, but the heterogeneity related to the composition is more robust across different specifications. Adjustments based upon permanent spending cuts are consistently much less costly than those based upon permanent tax increases. Our results are generally not explained by different reactions of monetary policy. However, when the domestic central bank

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can set interest rates—that is outside of a currency union—it appears to be able to dampen the recessionary effects of consolidations implemented during a recession.

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1 Introduction

The empirical literature on the macroeconomic effects of fiscal policy finds that multipliers depend on the state of the economy and on the type of the adjustment: whether a shift in fiscal policy happens during an economic expansion or during a contraction makes a difference, but there is also evidence that adjustments based on increasing taxes have different effects compared to those based on cutting expenditures. So far the literature has studied the two aspects separately, thus running the risk of attributing to one source of nonlinearity effects that are in fact generated by the other. In this paper we study the macroeconomic effects of fiscal consolidations allowing both sources of nonlinearity—we refer to them as the "when" and the "how"—to operate simultaneously, thus avoiding the risk mentioned above. We analyze the effects of multi-year fiscal consolidation plans rather than isolated fiscal "shocks." The reason is that in the real-world governments typically adopt, and legislatures vote, multi-year budget laws which have little resemblance to the isolated fiscal "shocks" often studied in the literature. Our analysis focuses only on fiscal contractions: we have nothing to say about expansionary fiscal policies.

We find that regardless of whether they start in a recession or not, adjustments based upon permanent spending cuts are much less costly in terms of short-run output losses than those based upon permanent tax increases. The cumulative drop in output following a tax-based adjustment plan of one percent of GDP is three times more severe than the one associated with a spending-based adjustment plan of the same size. While we reject the null hypothesis that the effects of consolidations started during an economic downturn or an expansion are identical, we find that the difference between the two is small and less relevant than that between tax-based and spending-based consolidations. In our baseline model we allow the state of the economy to change endogenously after the start of a fiscal stabilization program. If, as sometimes done in the literature, we assume that the state of the economy does not change following the start of a fiscal adjustment—an assumption that misses the fact that the economy can start off in one state and then, during the stabilization program, transition to another—the initial state of the business cycle makes more of a difference: fiscal adjustments are costly when started during a downturn and they are not during an expansion. The evidence on the heterogeneity between expenditure-based and tax-based adjustments, however, is confirmed. We discuss below the pros and cons of these two different approaches.

Our results appear not to be systematically explained by a different reaction of monetary policy and, therefore, they should survive at the zero lower bound (ZLB) when monetary policy is constrained, or within monetary unions where monetary policy cannot respond to the fiscal policy of a specific member country. We find, however, that in one case the response of monetary policy appears to make a difference. When the domestic central bank can set interest rates—that is outside of a currency union—it appears to be able to dampen the recessionary effects of consolidations implemented during a recession.

It is important to remark that we are analyzing permanent shifts in fiscal variables: transitory and permanent fiscal measures may have different effects. For example, in standard New Keynesian models permanent tax hikes are much more contractionary than temporary ones, and permanent spending cuts are much less contractionary than transient ones (see Erceg and Lindé 2013; Alesina and others 2017). This may explain why our estimates of fiscal multipliers, which are determined by persistent fiscal plans and not by temporary shocks, have different magnitudes than those usually found in the literature and that typically refer to temporary shocks.

The extent to which fiscal multipliers depend on the state of the economy is a question that has received much attention in recent years, echoing earlier Keynesian arguments that government spending is likely to have larger expansionary effects in recessions than in expansions. The intuition is that, when the economy has slack, an increase in government spending is less likely to crowd out private consumption or investment and therefore has a stronger expansionary effect on output. In addition to slack in the labor market, larger frictions in financial markets and an increase in the number of liquidity constrained agents might also contribute to generate higher multipliers during recessions. In two important papers, Auerbach and Gorodnichenko (2012a, b, AG in what follows), starting from the model of taxes, government spending and output estimated and simulated by Blanchard and Perotti (2002), allow the effects of shifts in fiscal policy to differ depending on whether they are introduced during an expansion or a recession. Compared with Blanchard and Perotti (2002) AG adopt a more refined measure of unanticipated shifts in fiscal variables, in particular they purge fiscal variables of the “innovations” that were predicted by professional forecasters.[1] They find large differences in the size of spending multipliers between recessions and expansions with fiscal policy being considerably more effective in recessions. The expenditure multipliers in recession and in expansion are very different and very far from the “average” multiplier delivered by a model where multipliers are constrained to be identical, independently of the state of the economy. This heterogeneity extends also to tax multipliers: an increase in taxation has a small non-Keynesian expansionary effect in recession, while the effect is small but contractionary in expansion. The findings in AG—originally obtained using US data and found to be robust when an international panel is used—refer both to fiscal expansions and fiscal contractions: they are thus different from those presented in this paper that only studies fiscal consolidations.

The results obtained by Auerbach and Gorodnichenko (2012a) hinge on the assumption that the state of the economy remains constant for at least the 20 quarters over which multipliers are computed. Ramey and Zubairy (2017) have

[1] They do so using information from quarterly forecasts of fiscal and aggregate variables from the University of Michigan’s RSQE macroeconometric model, the Survey of Professional Forecasters and the forecasts prepared by the staff of the Federal Reserve Board for the meetings of the Federal Open Market Committee.

observed that this may be a reasonable approximation for expansions, which typically last for several years, but it is not a good approximation for recessions, which, in their sample, have a mean duration of only 3.3 quarters. To address this problem Ramey and Zubairy (2017) compute multipliers using the linear projection method proposed by Jordà (2005) which imposes no restrictions, including those implied by the nonlinear model adopted by AG, on the evolution of the cycle in response to fiscal shocks.[2] Using quarterly US data, covering multiple large wars and deep recessions, they find no evidence that government spending multipliers are particularly high during high unemployment periods. Most estimates of the spending multiplier are between 0.3 and 0.8. These authors apply two different identification schemes: the one adopted by Blanchard and Perotti (2002) and an updated version of Ramey's (2011) "military news" variable. They find a statistically significant difference in multipliers across states only when spending shocks are identified by Blanchard and Perotti (2002) approach. The difference is due not to high multipliers in the high unemployment state, but to very low multipliers in the low unemployment state.[3] Caggiano and others (2015) also allow a feedback from shifts in fiscal policy to the probability of the economy being in an expansion or a recession in a STVAR finding that fiscal multipliers are higher in recessions than in booms. Their results, however, depend upon "extreme" events, that is deep recessions and strong expansionary periods.

Another important dimension along which fiscal multipliers are found to differ is related to the composition of a fiscal adjustment, whether it is mostly based on tax increases or on spending cuts. Alesina and others (2015) find that the output effect of tax-based adjustments is much more recessionary than that of expenditure-based adjustments. Their results, however, do not allow multipliers to differ depending on the state of the economy. As mentioned above, the contribution of this paper is to allow both sources of nonlinearity—the "when" and the "how"—to operate simultaneously.

Ramey and Zubairy (2017) consider a further potentially important source of nonlinearity: whether interest rates are near the zero lower bound.[4] These results are

[2] They also consider a different measure of slack, related to unemployment rather than to output growth as in AG.

[3] Two related papers which use Canadian data (Owyang and others 2013; Ramey and Zubairy 2015) had found higher multipliers in high unemployment states. Revisiting those findings the authors (in work in progress) find that the difference between the US and Canadian results were probably due to the special circumstances of Canada's entry into WWII, when output responded to the news long before government spending actually rose.

[4] In a simple real business cycle model, such as Baxter and King (1993), the output multiplier of a positive shift in government spending is below one. In New Keynesian models the magnitude of the output multiplier depends on the nature of the shock that takes the economy to the ZLB. Woodford (2011), Eggertsson (2011) and Christiano and other (2011) consider the case in which the economy reaches the ZLB as a result of a "fundamental" shock. In this case the multiplier can be substantially larger than one as temporary government spending is inflationary and stimulates private consumption and investment by decreasing the real interest rate. Mertens and Ravn (2014) consider instead a situation in which the ZLB is reached following a "non-fundamental" confidence shock: they find that the output multiplier during the ZLB period is quite small. The reason is that, in this situation, government spending shocks are deflationary, raising the real interest rate and reducing private consumption and investment. Exploiting regional variation in military buildups Nakamura and Steinsson (2014) estimate an "open

more mixed than those comparing multipliers in expansions and contractions away from the ZLB. Multipliers at the ZLB are still generally low, but in a few specifications they are as high as 1.5. Miyamoto and others (2016) using data for Japan also investigate the effect on fiscal multipliers of the interaction between the slack in the economy and how close it is to the ZLB. However, the size of their sample does not allow them to address the two channels (slack and proximity to the ZLB) simultaneously: when they limit the analysis to periods close to the ZLB they find only weak evidence of asymmetry.

This paper is organized as follows. We start in Sect. 2 studying nonlinearities related to the "how." In Sect. 3 we study nonlinearities related to the "when." In Sect. 4 we first provide a descriptive analysis of the data to show the interaction between the "how" and the "when"; we then illustrate our empirical approach. Section 5 presents our main results and our robustness analyses. Section 6 concludes.

2 Fiscal Multipliers and the Composition of Fiscal Adjustments

A first source of nonlinearity in the output effect of a fiscal consolidation is related to its composition. In this section we first explain why we study fiscal plans rather than isolated shifts in individual fiscal variables, then we describe how such multi-year plans are constructed and how we distinguish between tax-based and expenditure-based adjustments. Finally, we discuss their exogeneity with respect to output growth.

2.1 Our Narrative Data

Our data contain detailed information on the fiscal consolidations implemented by 16 OECD countries (Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Portugal, Spain, Sweden, UK, USA) between 1981 and 2014. We address the potential endogeneity of shifts in fiscal variables using the "narrative" approach first introduced by Romer and Romer (2010), later applied to a number of OECD countries by Devries and others (2011) and extended by Alesina and others (2015). The fiscal consolidation measures in the Devries and others dataset are selected reading the records available in official documents to identify the size, timing and principal motivation for each fiscal

Footnote 4 continued
economy relative multiplier" of approximately 1.5. They claim their results are in line with New Keynesian models where government spending has a large effect on output, particularly when the economy is in a liquidity trap. Erceg and Lindé (2013) investigate the effects of a spending-based versus labor tax-based fiscal consolidation in a two-country DSGE model. They find that the effects depend on the degree of monetary accommodation. Under an independent monetary policy (no currency union) cuts in government spending are much less costly than tax hikes. Under a currency union the effect is partially reversed. Indeed, their model predicts that when monetary policy provides too little accommodation—given its focus on union wide aggregates—spending-based fiscal consolidations are more costly in terms of output losses in the short run. In the long run, however, spending cuts are still less harmful than tax hikes, because of real exchange rates and price levels adjustments. The adverse impact of spending-based consolidations (in the short run) is exacerbated when monetary policy is constrained at the ZLB.

action. They are "exogenous" because their adoption is not correlated with the economic cycle but rather (i) they are geared toward reducing an inherited budget deficit or are meant to correct its long-run trend, e.g., an increase in pension outlays induced by population aging, or (ii) are motivated by reasons which are independent of the state of the business cycle, thus excluding adjustments motivated by short-run counter-cyclical concerns. These consolidation measures are either tax increases or spending cuts and cover the period 1978–2009. Starting from the Devries and others dataset we have extended the sample of consolidation measures identifying those implemented between 2010 and 2014—a period characterized by frequent consolidation episodes especially in Europe—and we have collected additional information on every fiscal measure, including those selected before 2009. This was necessary, as we shall see, in order to use these measures to reconstruct fiscal plans. Overall we have analyzed and identified the legislative source of about 3500 different fiscal measures adopted in the 16 countries in our sample between 1981 and 2014.[5] Our database contains the details of each policy measure, e.g., "rise in VAT rate by 2 percent to be implemented in year *t*," "reduction in tax relief," "reduction of childbirth grants," "cut in public employees salaries."[6] These fiscal actions are then measured in terms of their impact (as estimated in the documents that accompany their introduction) on total revenues and expenditures relative to a baseline of no policy change, scaled by the level of the GDP the year before they are announced.[7]

In our dataset we distinguish between several categories of fiscal measures. For the analysis in this paper, however, we group measures in just two broad categories: spending, g, and taxes, τ. We classify as spending all measures related to government spending and investments. These include, for instance, current expenditure for goods and services, public sector salaries, managing cost of state provided services (such as education and healthcare), and government gross fixed capital formation expenditures. We also include in the spending category government transfers, that is payments made by the government to private entities, including subsidies, grants and other social benefits such as Social Security payments in the USA. We do not include interest payments. We classify as taxes all the changes in direct taxes—e.g., income, profits, capital gains and property taxes—and indirect taxes—e.g., VAT, sales tax, excise duties on goods, and stamp duties.

[5] Note that our sample of 16 OECD countries differs from the sample of 17 OECD countries considered by Devries and others. We have dropped the Netherlands, which is the only country for which the narrative identified fiscal adjustments are significantly correlated with past output growth. This is not surprising given that the budget rules in the Netherlands include the following provision "...The budget can respond to changes in the economy and measures need not be taken immediately if there is a windfall or setback..." (https://www.government.nl/topics/budget-day/contents/budget-rules). That is, the rule prescribes that the government sets fiscal targets at the beginning of its election term and in the following years deviations from these targets are only allowed for cyclical reasons.

[6] The data used in this paper, as well as the codes we wrote, are available on a dedicated space in the IGIER webpage: www.igier.unibocconi.it/fiscalplans.

[7] As a convention, we use the GDP of the previous period because this was the latest estimate for GDP known by policymakers at the time these fiscal measures were announced. Moreover, current GDP may be affected by contemporaneous fiscal shocks. Results (available upon request) are virtually identical when scaling with current GDP.

We consider all the measures affecting the amount of revenues collected by the government, both changes in tax rates and measures designed to broaden the tax base. We include transfers in g because, theoretically, we expect a cut in transfers to be less distortionary than an increase in taxes. Indeed, a particular feature of transfers is that they do not affect the marginal rate of substitution between consumption and leisure. Our choice is also supported by the results in Alesina and others (2017) who distinguish between consolidations mostly based on cuts to government consumption and investments (CB), consolidations mostly based on cuts to transfers (TRB) and consolidations mostly based on tax hikes (TB). They find that TRB consolidations are much less recessionary than TB ones, and their effects on output are very similar to those of CB consolidations.

2.2 Fiscal Plans

As mentioned in the Introduction we do not study the effects of isolated fiscal "shocks." Rather, we study the effects of fiscal "plans," that is of shifts in fiscal variables to be implemented over an horizon of several years. This approach was introduced in the literature by Alesina and others (2015) for two reasons. First, because in the real-world governments typically adopt, and legislatures vote, multi-year budget laws which have little resemblance to the isolated fiscal "shocks" often studied in the literature. Second, because, to the extent that expectations matter, the multi-year nature of these laws cannot be ignored.

Fiscal plans consist of a sequence of actions, some to be implemented at the time the legislation is adopted, some to be implemented in following periods. Plans are also a mix of measures, some affecting government expenditures, others affecting revenues. Typically legislatures start debating the overall size of an adjustment and then discuss its composition: by how much to cut spending (and which programs) and by how much to raise taxes (and which ones). The design of plans thus generates inter-temporal and intra-temporal correlations among fiscal variables. The inter-temporal correlation is the one between the announced (future) and the unanticipated (current) components of a plan. The intra-temporal correlation is the one between the changes in revenues and in spending that determines the composition of a plan, given its size. We assume that if a new plan is announced in period t the policies implemented in that period are unexpected. While a plan is debated in parliament, economic agents could form expectations of what it might look like. In practice, however—beyond the fact that measuring these expectations is virtually impossible—the composition of a plan is almost always the result of political deals which often are only resolved shortly before the plan is announced.

In order to construct fiscal plans, we re-classify the exogenous fiscal measures selected in our narrative analysis in three categories: measures that were immediately implemented ("unexpected" measures), measures that were written in the legislation but whose implementation was deferred to subsequent years (future "announced" measures) and measures that were implemented in a given year but had previously been announced, that is were part of legislation adopted in previous years.

Consider a fiscal plan coming into effect at the beginning of year t: it typically contains three components: (i) unexpected shifts in fiscal variables (announced and implemented at time t): we call them $e^u_{i,t}$, where i refers to the particular country implementing the fiscal correction; (ii) shifts implemented at time t but which had been announced in previous years: $e^a_{i,t-j,t}$, where j denotes the horizon of a fiscal plan; (iii) shifts announced at time t, to be implemented in future years $e^a_{i,t,t+j}$. Considering, for simplicity, the case in which the horizon of the plan is only 2 years, the current year, t, and the following one, $t+1$ $(j=1)$, and with reference to a specific country i, an overall correction $f_{i,t}$ to the primary budget surplus can thus be described as follows

$$\begin{aligned} f_{i,t} &= e^u_{i,t} + e^a_{i,t-1,t} + e^a_{i,t,t+1} \\ e^u_{i,t} &= \tau^u_{i,t} + g^u_{i,t} \\ e^a_{i,t,t+1} &= \phi_1 e^u_{i,t} + v_{1,i,t} \end{aligned}$$

The first equation breaks up the overall correction—a shift in the primary budget surplus—into its three components. The second equation explains that a fiscal correction consists of changes in taxes and in expenditures net of interest payments, thus $e^u_{i,t} = \tau^u_{i,t} + g^u_{i,t}$: the same holds for $e^a_{i,t-1,t}$ and $e^a_{i,t,t+1}$. The third equation captures the correlation between the immediately implemented and the announced parts of a plan. This is a crucial feature of fiscal plans: overlooking it would mean assuming that announcements are uncorrelated with unexpected policy shifts: as we shall see this assumption is violated in the data. Interestingly, different plans feature different correlations between announced measures and measures immediately implemented: expenditure-based plans tend to be frontloaded, while tax-based plans typically take the form of sequences of tax changes distributed over time. In order to correctly simulate the effect of a fiscal plan it is thus necessary to estimate this inter-temporal correlation: simulating an unexpected policy shift overlooking the accompanying announcements would not reflect the data we have used to estimate fiscal multipliers.

Sometimes it happens that fiscal plans are revised along the way: in that case, we classify a change in an announced measure as an unexpected shift in fiscal policy and we record the start of a new plan.

The above description highlights that fiscal plans generate "fiscal foresight": economic agents learn in advance (through announcements) measures that will be implemented in the future. As observed by Leeper and others (2008), fiscal foresight makes the moving average (MA) representation of a VAR non-invertible and thus prevents the identification of exogenous shifts in fiscal variables from VAR innovations: this is why narrative identification (which does not require extracting innovations from VAR residuals because innovations are identified reading official documents) becomes crucial.

To illustrate our approach with a specific example, and to allow a comparison between fiscal "plans" and the fiscal "shocks" used in the literature, Table 1[8]

[8] Tables and figures are obtained using the data described in Appendix 3 and in Sect. 2.1.

Table 1 The multi-year plan introduced in Belgium in 1992 (% of GDP)

Year	Devries and others			Our plans									Label
	$\widetilde{e_{i,t}}$	$\widetilde{\tau_{i,t}}$	$\widetilde{g_{i,t}}$	$e^u_{i,t}$	$e^a_{i,t-1,t}$	$e^a_{i,t,t+1}$	$\tau^u_{i,t}$	$\tau^a_{i,t-1,t}$	$\tau^a_{i,t,t+1}$	$g^u_{i,t}$	$g^a_{i,t-1,t}$	$g^a_{i,t,t+1}$	
1992	1.79	0.99	0.80	1.85	0	0.47	1.03	0	0.05	0.82	0	0.42	*EB*
1993	0.92	0.43	0.49	0.52	0.47	0.83	0.40	0.05	0.55	0.12	0.42	0.28	*TB*
1994	1.15	0.55	0.60	0.38	0.83	0	0	0.55	0	0.38	0.28	0	*EB*

Note. The table compares the fiscal shocks identified by Devries and others (2011) and our fiscal plans with reference to the fiscal correction implemented in Belgium between 1992 and 1994. The last column reports the label of the plan (EB or TB) obtained using our classification procedure shows—with reference to the fiscal correction implemented in Belgium between 1992 and 1994—the exogenous fiscal “shocks” identified by Devries and others (2011) and then used in Guajardo and others (2014) and the plans we reconstructed. We also report the tax and spending decomposition of Devries and others and our fiscal shifts. Devries and others overlook fiscal announcements and construct “fiscal shocks” (which we shall call $\widetilde{e_{i,t}}$) adding up shifts in fiscal variables that are unanticipated, $e^u_{i,t}$, with those that are implemented at time t but had been announced in previous periods, $e^a_{i,t-1,t}$. That is—keeping the simplifying assumption of a 2-year horizon—they assume $\widetilde{e_{i,t}} = e^u_{i,t} + e^a_{i,t-1,t}$. This variable and its components, $\widetilde{g_{i,t}}$ and $\widetilde{\tau_{i,t}}$, are shown in the first three columns of Table 1. Components entering our fiscal plans appear on the right-hand-side columns of Table 1. Notice that, differently from the Devries and others “shocks,” our plans also include announcements of future shifts in fiscal variables, $e^a_{i,t,t+1}$. The first set of columns reports the aggregate fiscal shifts and the remaining columns report their composition. Considering the row for 1992, $\widetilde{e_{i,t}} = 1.79$ and $e^u_{i,t} = 1.85$. The two corrections are not identical because our shifts in fiscal variables are measured in billions of the domestic currency and then scaled using the GDP of the previous period: we used the latest available GDP series which sometimes may have been revised since the time Devries and others accessed the data. The same holds for the following years and for the two subcomponents: for instance, remaining on row one, $\widetilde{\tau_{i,t}} = 0.99$ and $\tau^u_{i,t} = 1.03$.

In the last column of Table 1 we classify the plan considered in each row as tax-based (*TB*) or expenditure-based (*EB*): this classification is done summing all fiscal measures, unanticipated, implemented but previously announced and future announcements. A plan is labeled TB if the largest component of the fiscal correction (measured as a fraction of GDP the year before the budget law is introduced) is an increase in taxes; similarly, EB plans are those where expenditure cuts are the largest component of the fiscal correction. Formally, for each year t, plans are labeled following this convention

$$\text{if } \left(\tau^u_{i,t} + \tau^a_{i,t-1,t} + \sum_{j=1}^{\text{horiz}} \tau^a_{i,t,t+j}\right) > \left(g^u_{i,t} + g^a_{i,t-1,t} + \sum_{j=1}^{\text{horiz}} g^a_{i,t,t+j}\right)$$

$$\text{then } TB_{i,t} = 1 \quad \text{and} \quad \mathrm{EB}_{i,t} = 0, \quad \text{otherwise} \quad TB_{i,t} = 0 \quad \text{and} \quad \mathrm{EB}_{i,t} = 1$$

Note that the labeling of a plan depends on the full inter-temporal path of the correction and not only on the measures adopted in a specific year. For example, 1992 is classified as an *EB* plan despite the fact that the fiscal correction implemented in 1992 relies more heavily on taxation. The labeling of a plan can change if during its implementation changes are introduced with respect to the measures planned when it was first announced. This, indeed, happened in Belgium in 1993 and then again in 1994.

Descriptive statistics on the main characteristics of the fiscal plans we analyze are in Sect. 4.

2.3 Estimating and Simulating the Effect of Fiscal Plans

After having identified, via the narrative approach, consolidation measures that are exogenous with respect to output growth, and having used them to build consolidation plans, we derive impulse responses that describe the output effect of a given plan. To do this we need a model for simulating plans. We specify, for our panel of countries, a VAR in three variables: the growth rate of per capita output, the change in revenues as a fraction of GDP and change in primary government spending, also as a fraction of GDP. We also include in the VAR the three narratively identified components of a fiscal plan—which, if we limit the horizon to 2 years, are $e^{u}_{i,t}, e^{a}_{i,t-1,t}, e^{a}_{i,t,t+1}$—and we allow TB and EB plans to have different effects. Beyond the equations in the VAR, the model also includes a set of auxiliary equations that track the correlation between the announced and the unanticipated components of plans, and between shifts in taxes and in spending. These equations are crucial to be able to simulate plans because, as already mentioned, in the data unexpected shifts in fiscal variables do not happen in isolation, but are typically accompanied by announcements of future shifts. These auxiliary regressions allow to simulate the average plan estimated in the data in the sense that when the effects of an unanticipated shift in some fiscal variable is simulated, announcements move consistently with what has been observed in the sample. Similarly, when we simulate an EB or a TB plan, we do not move taxes (spending) keeping spending (taxes) constant because this almost never happens in the plans we use to estimate fiscal multipliers. Instead we move taxes and spending according to what we have observed, on average, in the EB or TB consolidations present in our sample. We compute impulse responses to fiscal plans whose size is one percent of GDP: we do this calibrating the shifts in individual fiscal variables so that the size of the adjustment plan is one percent of GDP. Finally, impulse responses are constructed by simulating the model in presence and in absence of a plan and by taking the difference between the two sets of simulated output paths.

This empirical strategy requires a few comments. First, our VAR is parsimonious in terms of included variables, which are only three. This does not affect the identification of the exogenous fiscal measures because these are not derived from VAR innovations but are directly observed. Our estimates of the output response to a fiscal plan, however, might also depend on the effect that plans have on variables not included in the VAR: this omission will not affect the measurement of the final effect but prevents the identification of different transmission channels. This is the reason why we shall devote a section of the paper to investigate the role of monetary policy in determining the fiscal multiplier that we obtain from our empirical analysis.

Second, specifying the VAR in first differences, our simulations measure the response to *permanent* fiscal adjustments. In principle a plan could be temporary even if the VAR is specified in differences. This would happen if announcements exactly canceled unanticipated shifts in fiscal variables. In our data this does not happen because announcements are mildly positively correlated with unanticipated shifts in fiscal variables, meaning not only that on average fiscal plans are permanent, but also that fiscal consolidation measures get reinforced over time. Transient changes in taxes or spending would instead have quite different effects.[9]

Third, our model deals with fiscal foresight by simulating plans that explicitly include announcements. Our strategy is thus different from the one adopted to deal with fiscal foresight when shocks are identified within the VAR, which consists in augmenting the VAR with real-time predictions of fiscal variables to clean the VAR innovations from the effect of the "news shocks." Last, but indeed not least, exogeneity of our narratively identified adjustment plans is critical. We address it in the next section.

2.4 The Exogeneity of Fiscal Plans

Discussing the data from Devries and others, De Cos and Moral-Benito (2016) and Jordà and Taylor (2016) have observed that some of their narratively identified fiscal adjustments are predictable, either by their own past or by past economic variables. This, they argue, is a threat to their exogeneity.

Consider first predictability by past fiscal adjustments: the approach followed here overcomes this problem analyzing plans rather than collapsing them into shocks. To understand why, assume one overlooked announcements and plans and instead considered $\widetilde{e_{i,t}} = e^u_{i,t} + e^a_{i,t-1,t}$, the fiscal "shocks" analyzed by Devries and others (2011) shown in Table 1 and found to be predictable by their own past. As we have illustrated in the previous section, within a plan, policy announcements are correlated with unanticipated policy shifts, that is $e^a_{i,t-1,t} = \phi_1 e^u_{i,t-1} + v_{1,i,t-1}$. Under the null that the $e^u_{i,t}$ are not correlated over time

$$\begin{aligned}\mathrm{Cov}\left(\widetilde{e_{i,t}}, \widetilde{e_{i,t-1}}\right) &= \mathrm{Cov}\left(\left(e^u_{i,t} + e^a_{i,t-1,t}\right), \left(e^u_{i,t-1} + e^a_{i,t-2,t-1}\right)\right) \\ &= \mathrm{Cov}\left(\left(e^u_{i,t} + \phi_1 e^u_{i,t-1} + v_{1,i,t-1}\right), \left(e^u_{i,t-1} + e^a_{i,t-2,t-1}\right)\right) \\ &= \phi_1 \mathrm{Var}\left(e^u_{i,t-1}\right)\end{aligned}$$

Finding $\mathrm{Cov}\left(\widetilde{e_{i,t}}, \widetilde{e_{i,t-1}}\right) \neq 0$ is therefore not surprising. In other words, predictability of $\widetilde{e_{i,t}}$ from their own past is a feature of multi-year fiscal plans and is properly dealt with analyzing plans rather than "shocks" such as $\widetilde{e_{i,t}}$. One could still be worried about predictability on the basis of past components of the primary deficit as a by-product of the narrative identification procedure that selects adjustments driven by the need to reduce the debt. This is not a problem at the estimation stage because consistent estimates of fiscal multipliers only require that innovations in output growth and shifts in fiscal variables are not correlated, an assumption which is not ruled out by predictability from past deficits. Simulation instead could be a problem, as it would be impossible to simulate unpredictable shift in fiscal policy. To address this potential problem we analyze fiscal plans within a panel

[9] Standard New Keynesian models imply that the effects of permanent changes are quite different from those of temporary changes; permanent tax hikes have stronger contractionary effects than transient ones, and permanent spending cuts are much less contractionary than transient ones (see Erceg and Lindé 2013; Alesina and others 2017).

VAR that includes three variables: output growth and the change in revenues e spending in percentage of GDP. The estimated coefficients of the narrative adjustments in this VAR measure the effect on output growth of the component of such adjustments that is orthogonal to lagged included variables (see Appendix 1). The estimated multipliers are thus not affected by the observed predictability.

Predictability of $\widetilde{e_{i,t}}$ by past economic variables raises a different issue. De Cos and Moral-Benito (2016) show that if the $\widetilde{e_{i,t}}$ are described by a dummy variable that takes the value of 1 when $\widetilde{e_{i,t}} \neq 0$, they are predictable based on information available at time $(t-1)$. This observation, however, does not take into account the fact that there are two sources of identification of narrative adjustments: the *timing* of a fiscal correction and its *size*. Transforming fiscal adjustments into a 0 / 1 dummy completely neglects the importance of size as a source of identification. To illustrate the practical relevance of this point we have run two simple regressions. Let I_t^a be an indicator variable that takes the value of 1 when an adjustment is implemented and 0 otherwise, and run on this indicator both unanticipated adjustments and announcements, that is run these two regressions

$$\begin{aligned} e_t^u &= \beta_1 I_t^a + \varepsilon_t \\ \sum_{j=1}^{2} e_{t,t+j}^a &= \beta_2 I_t^a + \eta_t \end{aligned} \tag{1}$$

If the only source of variation were the timing of the adjustment these regressions would produce an R^2 of 1. Table 2 reports the results: both R^2 are low, supporting the conjecture that the main source of identification is the size of the adjustment, not its timing. Summing up: evidence that the *timing* of narrative adjustments can be predicted does not imply that the fiscal correction itself is predictable because, as we have seen, its size cannot be predicted. It is useful to remember that fiscal policy is different from a medical treatment in which a group of patients are given the same dose of a medicine: if it was not, the above regression would produce an R^2 of 1.[10]

2.5 The Credibility of Fiscal Plans

Our results are derived under the assumption that plans are fully credible. The plans in our sample are often amended on the fly: when this happens we treat the amendment as a surprise and we count it as a new plan. The assumption that plans are fully credible is a strong one and one that cannot be easily tested.[11] Alesina and others (2017) investigate the importance of this assumption distinguishing between two categories of spending: transfers and government consumption and investment. Transfer-based plans, that often imply changes in social security legislation, should be more difficult to reverse, and thus more credible. They find that consolidations

[10] The non-predictability of fiscal corrections on the basis of output growth is documented by Alesina and others (2017) who verify that GDP does not Granger-cause the narrative fiscal consolidations shocks, according to the procedure by Toda and Yamamoto (1995) which shows no Granger causality on a panel VAR with one lag, and 10 percent Granger causality on a panel with two lags.

[11] Credibility of fiscal consolidations is discussed in Lemoine and Lindé (2016) and Corsetti and others (2012).

Table 2 Time versus size

	Coefficient	R^2	Observations
β_1	1.0245*** (0.0437)	0.4236	534
β_2	0.6945*** (0.0413)	0.2719	534

Note. The table reports the coefficients estimated from the two regressions in (1). Standard errors in parentheses

***Indicates statistical significance at the 1 percent level

mostly based on transfers cuts are slightly less recessionary that those mostly based on cuts to government consumption, but the difference is not statistically significant. Future research should investigate this issue further.

3 Fiscal Multipliers and the State of the Cycle

We now address the second source of nonlinearity: the possible dependence of multipliers on the state of the economic cycle.

The effects of this nonlinearity could be identified by simply separating fiscal consolidations initiated during an economic expansion from those that started during an economic downturn. This procedure, however, would miss the fact that the economy can start off in one state (for instance in a recession) and then, over time, transition to another (an expansion). For example, Belgium launched a large consolidation plan in 1982 following a year of recession, but while it was implemented the economy turned around and resumed positive growth. Ten years later, Belgium's 1992 multi-year consolidation plan began after a period of expansion, but in 1993 the Belgian economy entered a recession from which it recovered in 1994. For this reason we use an indicator of the state of the economy that can change over time.

To describe the state of the economy Auerbach and Gorodnichenko (2012a, b) have suggested using a logistic function $F(s_{i,t})$—where the index i refers to the country—which transforms the distribution of output growth, $\Delta y_{i,t}$ (more precisely of its standardized moving average), into a variable ranging between 0 and 1. This specification makes the transition between states of the economy smooth, with $F(s_{i,t})$ being the weight given to recessions and $1 - F(s_{i,t})$ the weight given to expansions. Formally, we define $F(s_{i,t})$ as follows

$$F(s_{i,t}) = \frac{\exp(-\gamma_i s_{i,t})}{1 + \exp(-\gamma_i s_{i,t})}, \quad \gamma_i > 0$$

$$s_{i,t} = \frac{\mu_{i,t} - \mathbb{E}(\mu_{i,t})}{\sigma(\mu_{i,t})}$$

$$\mu_{i,t} = \frac{\Delta y_{i,t-1} + \Delta y_{i,t-2}}{2}$$

where $\mu_{i,t}$ is the moving average—and $s_{i,t}$ its standardized version—of output growth during the previous 2 years and γ_i are the country-specific parameters of the logistic function. For comparison with AG we define an economy to be in recession if $F(s_{i,t}) > 0.8$. The parameters γ_i are then calibrated to match actual recession probabilities in the countries in our sample, defined as the percentage of years in which growth is negative over the sample, which consists of yearly data from 1979 to 2014. This calibration method allows us to use the same criterion for all countries in the sample, as most of them do not have Dating Committees.[12] In other words, we calibrate γ_i so that country i spends x_i percent of time in a recessionary regime—that is, $\mathbb{P}[F(s_{i,t}) > 0.8] = x_i$, where x_i is the ratio of the number of years of negative GDP growth for country i to the total number of years in the sample.

For example, since for the US this number is 17 percent, in order to have $\mathbb{P}[F(s_{US,t}) > 0.8] = 0.17$, we need to set $\gamma_{US} = 1.56$. This frequency of recession years for the US is also consistent with the number obtained using the NBER Business Cycle Dating Committee recession indicator over the sample 1979–2014, which is 19 percent.[13] In the case of Italy, to give another example, $\gamma_i = 2.24$ so that the country spends 22% of its time in recession: thus $\mathbb{P}[F(s_{IT,t}) > 0.8] = 0.22$. The $\gamma_i's$ obtained through this calibration procedure are reported in Table 3. In order to see how closely this method is able to match the data, Fig. 1 compares the dynamics of $F(s)$—the solid line—with actual recessions in the countries of our sample.[14] It is important to note that our state of the economy indicator, $F(s_{i,t})$, is a function of *lagged* output growth. The main advantage of this choice is that assuming a lagged feedback between GDP growth and $F(s_{i,t})$ permits us to treat this indicator as an endogenous variable when the model is simulated. This allows the state of the economy to change following a shift in fiscal policy—in other words it allows us to compute the response of $F(s_{i,t})$ to a fiscal adjustment. Once this is done, the impulse response of all variables in the VAR will reflect *both* the difference across states (expansion and recession) and the evolving expectation of recession. If instead $s_{i,t}$ was a function of *current* GDP growth (as in AG 2012b), $F(s_{i,t})$ and $\Delta y_{i,t}$ would be simultaneous and this would prevent us from allowing the state of the economy to respond to fiscal shocks. The regime would then remain constant throughout the fiscal adjustment program thus creating an asymmetry between the stage of estimation of the model (in which the regime is endogenous and time varying) and the stage of simulation in which the regime would be assumed to remain unchanged. Moreover, as pointed out by Ramey and Zubairy (2017), the assumption that the state of the economy stays constant for the horizon at which the model is simulated (in our case 5 years) cannot be a valid approximation for recession states, which have a mean duration of less than 1 year.

However, the use of a lagging indicator of the business cycle also implies a cost. Suppose that a fiscal correction at time t, implemented in recession, was able to

[12] To obtain values of $F(s)$ for the entire 1981–2014 sample we use data for output growth in the 2 years prior to the starting date of the estimation.

[13] We obtain this share by considering as years of recession those in which the number of months recorded as recessionary by the NBER is higher than 3.

[14] With $F(s_{i,t})$ we refer to the economic conditions prevailing at the beginning of the period in which the consolidation is implemented, thus reflecting economic growth in the two previous periods. Consistently with the way we constructed our indicator, in Fig. 1 we plot $F(s_{i,t+1})$ as a measure of the state of the cycle in period t for comparability with actual recessions.

Table 3 Calibrated γ_i's and years in recession

	γ	Years in recession (%)
AUS	1.14	14
AUT	1.53	14
BEL	1.13	14
CAN	1.09	17
DEU	1.31	17
DNK	1.72	19
ESP	1.70	25
FIN	4.92	22
FRA	1.59	14
GBR	1.43	19
IRL	1.68	14
ITA	2.24	22
JPN	1.65	17
PRT	1.60	22
SWE	1.92	19
USA	1.56	17

Note. The γ_i's are calibrated using the procedure described in the paragraph. The second column of the table reports the percentage of recession years (defined as years of negative growth) in every country computed over the period 1979–2014

change the state of the economy on impact. In this case the correction would be wrongly classified, based on the lagged indicator, as hitting the economy in a recession. This scenario, however, is unlikely in the case of our identified fiscal corrections because our narrative identification scheme excludes fiscal corrections driven by the cycle. Indeed, for a fiscal correction implemented in a recession to instantaneously move the economy into an expansion very strong non-Keynesian contemporaneous effects would be needed. Such effects are unlikely when the fiscal correction is selected to be exogenous with respect to output growth, i.e., it is not driven by the cycle. Finally, even if our procedure induced mislabeling of the regime, this would be only temporary.

Weighing these pros and cons we decided, in our main results, to opt for the backward-looking moving average which allows $F(s_{i,t})$ to be endogenous. In the robustness section we shall report the results obtained by making $F(s_{i,t})$ dependent on contemporaneous output growth, while holding the regime constant over the simulation horizon.

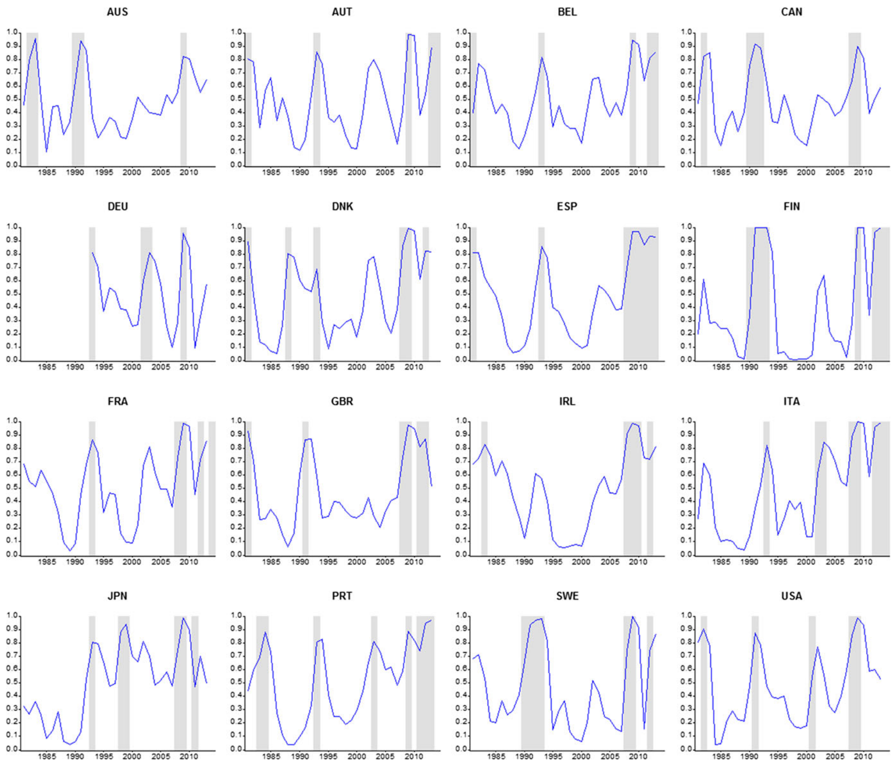


Fig. 1 Evolution of $F(s)$ and Recessions. Note. The graph shows the evolution of $F(s)$ for the countries in our sample and years of recession (years of negative growth per capita, shaded areas), 1981–2013

4 Allowing for the “When” and the “How” to be Studied Simultaneously

4.1 A First Look at the Data

The fiscal adjustment plans for the 16 countries in our sample are constructed as described in Sect. 2. Tables 4 through 8 illustrate the main features of our plans. Table 4 lists the number of plans that we have identified for each country over the sample of annual data spanning the years 1981–2014. A new plan is recorded whenever either a new adjustment is announced or previously announced measures are modified.

In total we have 170 plans and 216 episodes, that is years during which a fiscal consolidation is under way (a plan typically lasts more than 1 year). Of these about two-thirds are EB and one-third are TB. Table 5 documents the composition of fiscal plans showing the share of their main component, which determines the nature of the plan. As shown in the first column of Table 5, in half of TB plans taxes account for 75 percent or more of the total adjustment and the same holds for EB plans. The cases in which plans are labeled as EB or TB in the presence of a

Table 4 Plans classification, 1981–2014

Country	TB	EB
AUS	3	4
AUT	1	3
BEL	4	11
CAN	3	16
DEU	3	6
DNK	3	5
ESP	8	7
FIN	2	7
FRA	3	7
GBR	4	6
IRL	6	8
ITA	6	12
JPN	3	5
PRT	4	7
SWE	0	5
USA	4	4
Tot.	57	113

Note. Plans are classified according to the category that is most affected. The table reports new plans only

Table 5 The composition of fiscal adjustments

	≥ 0.75	<0.75	<0.65	<0.55
TB—57 plans	30	27	19	9
EB—113 plans	55	58	33	7
Tot. plans: 170		Tot. episodes: 216		

Note. The table reports the number of EB (TB) plans for which the spending (tax) share of the plan is higher or equal than 75 percent, lower than 75 percent, lower than 65 percent and lower than 55 percent. The last row reports the totals differentiating between *plans* and *episodes*. The latter are defined as years during which a fiscal consolidation is under way (a plan typically lasts more than 1 year)

marginally dominant component (e.g., either the spending share of EB plans or the tax share of TB ones are less than 55 percent) are rare, as shown in the last column of Table 5.

Table 6 analyzes the relative frequency of EB and TB plans and their relationship with the cycle. EB plans are more frequent than TB plans (113 vs. 57). The relative frequency of TB and EB plans when the recession indicator is high ($F(s_{i,t}) > 0.8$) or low ($F(s_{i,t}) \leq 0.2$) is not significantly different from the unconditional frequency. In other words, it is not the case that EB adjustments occur more frequently than TB

Table 6 Fiscal adjustments and the state of the economy

	$F(s_{i,t}) \leq 0.2$	$0.2 < F(s_{i,t}) \leq 0.5$	$0.5 < F(s_{i,t}) \leq 0.8$	$F(s_{i,t}) > 0.8$
TB—57 plans	3	14	18	22
EB—113 plans	10	31	32	40
Years in sample—515	94	189	133	99

Note. The table reports the number of EB and TB plans that started in a period of strong expansion ($F(s_{i,t}) \leq 0.2$), moderate expansion ($0.2 < F(s_{i,t}) \leq 0.5$), moderate recession ($0.5 < F(s_{i,t}) \leq 0.8$) and strong recession ($F(s_{i,t}) > 0.8$) together with the total number of years in every state. $F(s_{i,t})$ is defined as explained in Sect. 3

ones in a particular state of the economy (recession or expansion). For instance, of all the consolidations implemented in recession, two-thirds were EB and one-third TB. The same proportions hold in the full sample. This is important because it implies that, for instance, it is not the case that TB plans appear to be more recessionary because they are adopted more often during a recession.

Note also that, overall, adjustment plans—independently of their nature, EB or TB—are more likely to be introduced during a recession. There was a consolidation in 62 out of 99 years in which $F(s_{i,t}) > 0.8$, while only in 13 over 94 years in which $F(s_{i,t}) \leq 0.2$. This does not necessarily imply causality from the state of the cycle to the narratively identified fiscal adjustments, for two reasons. First, as already discussed in Sect. 2.4, there are two sources of identification of our fiscal corrections: the timing and the size. The information reported in Table 6 is limited to the timing. In other words, even if agents knew that the government was more likely to introduce fiscal consolidation measures during recessions they could not predict the size of the consolidation plan, as this varies a lot in our sample. Moreover it is also difficult to predict the nature (EB or TB) of a plan before its details are specified. Second, the fiscal measures we consider are exclusively aimed at increasing the primary surplus (being driven by past deficits). Thus endogeneity of the fiscal adjustments to the state of the cycle would mean that governments implement restrictive fiscal actions because the economy is in a recession. This strategy would be counterintuitive, given that the most likely output effect of fiscal consolidations is contractionary. The evidence reported in Table 6 would be much more worrisome if our exogenous fiscal measures were to contain some expansionary policies. Lastly, fiscal adjustments motivated by the objective of cooling down output growth, introduced when output is growing strongly, are excluded from our sample: remember that, following the narrative approach, we have selected fiscal corrections that are not motivated by the state of the cycle.

Table 7 shows the length of plans. Most plans have a 2-year horizon and, on average, *EB* plans last longer than *TB* ones—suggesting that cutting spending takes longer than raising taxes. Finally, Table 8 shows the magnitude of, respectively, the total shift in the primary surplus, the shift corresponding to the spending side and that corresponding to the tax side in the case of *EB*, *TB* and *all* plans. The average size of a plan is 1.83 percent of GDP and *EB* plans are larger than *TB* ones. The last two columns confirm that plans are well classified with our scheme: the spending part of *EB* plans is larger than that of *TB* ones and vice versa for taxes. Plans

Table 7 Time horizon of fiscal plans

	Horizon of plans (years)						
	1	2	3	4	5	6	Average
TB	16	20	6	7	7	1	2.51
EB	26	41	7	14	9	16	2.88
All	42	61	13	21	16	17	2.76

Note. The table reports the number of EB and TB plans lasting 1 up to 6 years and their average length. We define a plan to last 1 year if it only contains unexpected measures to be implemented immediately. A plan that includes measures to be implemented after 1 year has a 2 years horizon. Similarly for the others

Table 8 Size of fiscal plans

	Size of plans (%GDP)		
	Total	Spending	Taxes
All sample			
TB	1.60	0.49	1.10
EB	1.94	1.46	0.48
All plans	1.83	1.14	0.69
$F(s_{i,t}) \leq 0.2$			
TB	1.50	0.55	0.95
EB	1.50	1.34	0.16
All plans	1.50	1.16	0.34
$0.2 < F(s_{i,t}) \leq 0.5$			
TB	1.41	0.47	0.94
EB	1.37	1.10	0.27
All plans	1.38	0.90	0.48
$0.5 < F(s_{i,t}) \leq 0.8$			
TB	1.32	0.28	1.04
EB	2.07	1.40	0.66
All plans	1.80	1.00	0.80
$F(s_{i,t}) > 0.8$			
TB	1.95	0.67	1.28
EB	2.40	1.82	0.58
All plans	2.24	1.41	0.83

Note. The table reports the average total size and the average size of spending and tax measures for the plans in our sample, 1981–2014. Figures are presented for the whole sample and differentiating between states of the economy. $F(s_{i,t})$ is defined as explained in Sect. 3

implemented in recession ($F(s_{i,t}) > 0.8$) are larger than those implemented in expansion ($F(s_{i,t}) \leq 0.2$): the difference is about 0.9 percent of GDP for EB plans and about 0.5 percent for TB ones.

4.2 A Model with Two Sources of Nonlinearity

In this section we introduce a model that allows us to study, simultaneously, two nonlinearities in the effect of fiscal policy: one related to the state of the cycle, the other to the composition of the adjustment.

4.2.1 Estimation

The model we use is a Smooth Transition Panel VAR with two states, recession and expansion, and a nonlinearity associated with the composition of a fiscal plan. That is we allow multipliers to differ depending on whether the fiscal consolidation plan is tax-based or expenditure-based. The variables included in this panel VAR are the growth rate of per capita output ($\Delta y_{i,t}$), the change of tax revenues as a fraction of GDP ($\Delta\tau_{i,t}$) and that of primary government spending, also as a fraction of GDP ($\Delta g_{i,t}$). Define

$$\mathbf{z_{i,t}} = \begin{bmatrix} \Delta y_{i,t} \\ \Delta g_{i,t} \\ \Delta \tau_{i,t} \end{bmatrix} \qquad \mathbf{g}_{i,t} = \begin{bmatrix} g^u_{i,t} \\ g^a_{i,t-1,t} \end{bmatrix} \quad \tau_{i,t} = \begin{bmatrix} \tau^u_{i,t} \\ \tau^a_{i,t-1,t} \end{bmatrix} \qquad \mathbf{e}_{i,t} = \begin{bmatrix} e^u_{i,t} \\ e^a_{i,t-1,t} \\ e^a_{i,t,t+j} \end{bmatrix}$$

$$\beta_{\mathbf{h}} = \begin{bmatrix} \beta_{h1} & \beta_{h5} \\ \beta_{h2} & \beta_{h6} \\ \beta_{h3} & \beta_{h7} \\ \beta_{h4} & \beta_{h8} \end{bmatrix}' \qquad A^S_k = \begin{bmatrix} A^S_{k,1} \\ A^S_{k,2} \\ A^S_{k,3} \end{bmatrix}' \qquad \mathbf{a} = \begin{bmatrix} a_1 \\ a_2 \\ a_3 \end{bmatrix} \qquad \textit{similarly for } \mathbf{b}, \mathbf{c}, \mathbf{d}$$

for $h = 1, 2$, $k = 1, 2, 3$ and $S = E, R$.

Our model can thus be written as

$$\Delta y_{i,t} = \underbrace{\begin{bmatrix} 1 - F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} A^E_1 \\ A^R_1 \end{bmatrix} \mathbf{z_{i,t-1}}}_{\text{Dynamics}} + \underbrace{\begin{bmatrix} 1 - F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} \mathbf{a}'\mathbf{e}_{i,t} & \mathbf{b}'\mathbf{e}_{i,t} \\ \mathbf{c}'\mathbf{e}_{i,t} & \mathbf{d}'\mathbf{e}_{i,t} \end{bmatrix} \begin{bmatrix} TB_{i,t} \\ \mathrm{EB}_{i,t} \end{bmatrix}}_{\text{Impact at t}}$$
$$+ \lambda_{1,i} + \chi_{1,t} + u_{1,i,t}$$

$$\Delta g_{i,t} = \begin{bmatrix} 1 - F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} A^E_2 \\ A^R_2 \end{bmatrix} \mathbf{z_{i,t-1}} + \begin{bmatrix} 1 - F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \beta_{\mathbf{1}} \begin{bmatrix} \mathbf{g}_{i,t} \\ \tau_{i,t} \end{bmatrix} + \lambda_{2,i} + \chi_{2,t} + u_{2,i,t}$$

$$\Delta \tau_{i,t} = \begin{bmatrix} 1 - F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} A^E_3 \\ A^R_3 \end{bmatrix} \mathbf{z_{i,t-1}} + \begin{bmatrix} 1 - F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \beta_2 \begin{bmatrix} \mathbf{g}_{i,t} \\ \tau_{i,t} \end{bmatrix} + \lambda_{3,i} + \chi_{3,t} + u_{3,i,t} \tag{2}$$

In this specification nonlinearities affect output growth both on impact and through the dynamic response of the economy to a consolidation plan. First, we modify the autoregressive part of the VAR to allow our variables to follow different dynamics in expansion and in recession, as captured by the coefficient vectors A^E and A^R. Then, to estimate the impact of fiscal plans at time t, we augment the VAR equations with our narratively identified exogenous fiscal shifts. These enter as total fiscal shocks, $\mathbf{e}_{i,t} = \tau_{i,t} + \mathbf{g}_{i,t}$, in the output growth equation and as separate $\tau_{i,t}$ and $\mathbf{g}_{i,t}$ shocks in the equations for revenues and spending.

In the output growth equation, total fiscal shocks, $\mathbf{e}_{i,t}$, are interacted with two dummies, $TB_{i,t}$ and $\mathrm{EB}_{i,t}$, that take the value 1 if the plan we are analyzing is mostly based on tax hikes or on spending cuts, respectively.[15] The coefficient vectors $\mathbf{a}, \mathbf{b}, \mathbf{c}, \mathbf{d}$ capture the possible nonlinearities associated with the response (on impact) to a consolidation plan—both stemming from its composition and from the state of the economy: the effects of *TB* plans in booms and in recessions are described, respectively, by $\mathbf{a}$ and $\mathbf{c}$, while $\mathbf{b}$ and $\mathbf{d}$ capture the effects of *EB* plans in booms and recessions. The vector $\mathbf{e}_{i,t}$ contains three components $\left[e^u_{i,t} \quad e^a_{i,t-1,t} \quad e^a_{i,t,t+j} \right]'$: consequently $\mathbf{a} = \left[a_1 \quad a_2 \quad a_3 \right]'$ describes the heterogeneous effect of unexpected, announced and future announced fiscal shifts; similarly for $\mathbf{b}$, $\mathbf{c}$, and $\mathbf{d}$.[16]

In the equations for $\Delta g_{i,t}$ and $\Delta \tau_{i,t}$ we explicitly allow expenditure, $\mathbf{g}_{i,t}$, and revenue, $\tau_{i,t}$, corrections to have different coefficients. In these equations we include only fiscal shifts implemented in period t, either unexpected or previously announced: future announced corrections do not directly affect the dynamics of revenues and expenditures as their effect is not recorded in national accounts until they are implemented.

Each equation includes country, $\lambda_{j,i}$, and year, $\chi_{j,t}$ fixed effects. Finally, $u_{j,i,t}$ are unobservable VAR innovations: these are uninteresting for our analysis, as we do not need to extract from them any structural shock.[17]

There are some interesting points to note about our specification. First, our choice of interacting total shifts in fiscal variables with the TB and EB dummies allows us to decompose fiscal adjustments in two mutually exclusive components, which then can be simulated separately. This would not be possible if $\mathbf{g}_{i,t}$ and $\tau_{i,t}$ were directly included in the output growth equation because, as already observed, exogenous shifts in taxes and spending are correlated. While this correlation is not a problem at the estimation stage, at the simulation stage it would prevent from studying a pure spending or tax shock, since this would require to simulate a plan where the correlation between taxes and spending is zero, an assumption which is rejected by the data. Instead, by using the TB and EB dummies we are able to study the effect of tax-based and spending-based plans, allowing both $\mathbf{g}_{i,t}$ and $\tau_{i,t}$ to move according to the correlation observed in the data for each type of plans. Second, in our

15 See Sect. 2.2 for details on how we label plans.

16 At the estimation stage we include announced fiscal shift to be implemented up to *two years* in the future, that is, we include $e^a_{i,t,t+1}$ and $e^a_{i,t,t+2}$, and we restrict their coefficients to be equal. We impose this restriction to increase statistical power and because the dynamic effect of these shifts will then be captured by $e^a_{i,t.-1,t}$ as well.

17 Remember that we do not extract exogenous fiscal shocks from the VAR innovations: the components of our exogenous plans are identified outside the VAR using the narrative identification strategy and are included directly into the model.

specification we take into account the inter-temporal dimension of fiscal plans by including unexpected, announced and future announced fiscal shifts, allowing them to have heterogenous effects on our variables.[18] Third, the use of a VAR which includes the percentage change of revenues and spending (as a fraction of GDP) and tracks the impact of the narratively identified fiscal shocks on total revenues and total spending allows us to check the strength of our narratively identified instruments—for instance it allows us to verify if, following a positive shock to taxes, revenues indeed increase. Finally, our specification provides a straightforward way to verify the statistical relevance of the possible nonlinearities related to the timing and to the composition of fiscal plans. The following restrictions can be tested:

(i) $\mathbf{a} = \mathbf{c}$, $\mathbf{b} = \mathbf{d}$, $\beta_{i,j} = \beta_{i,j+4}$ for $i = 1, 2$ and $j = 1, 2, 3, 4$; the only source of nonlinearity in the contemporaneous effect of a plan arises from its type (EB vs. TB);

(ii) $\mathbf{a} = \mathbf{b}$, $\mathbf{c} = \mathbf{d}$; the only source of nonlinearity in the contemporaneous effect of a plan arises from the state of the cycle;

(iii) $\mathbf{a} = \mathbf{b} = \mathbf{c} = \mathbf{d}$, $\beta_{i,j} = \beta_{i,j+4}$ for $i = 1, 2$ and $j = 1, 2, 3, 4$; the impact effects of the introduction of a consolidation plan depend neither on the state of cycle nor on the type of plan;

(iv) $\mathbf{a} = \mathbf{b} = \mathbf{c} = \mathbf{d}$, $A_k^E = A_k^R$ for $k = 1, 2, 3$, given $\beta_{i,j} = \beta_{i,j+4}$ for $i = 1, 2$ and $j = 1, 2, 3, 4$; we are left with a standard linear VAR model without nonlinearities.

We will return to these tests in the results section. We now briefly describe how we use model (2) to simulate the response of the economy to a fiscal consolidation plan.

4.2.2 Simulation

As explained in Sect. 2.3, we want to simulate the response of the economy to plans rather than shocks. Fiscal plans have both an *intra-temporal* and an *inter-temporal* dimension: the first refers to the correlation between shifts in taxes, $\tau_{i,t}^u$, and government spending, $g_{i,t}^u$, in a given year, and the second is related to the correlation between current unexpected shifts, $e_{i,t}^u$, and announcements of future shifts in taxes and spending, $\tau_{i,t,t+j}^a$ and $g_{i,t,t+j}^a$. In order to simulate the effect of a plan we need a model for these correlations. Thus, as already discussed, we complete model (2) with a set of auxiliary equations describing the correlation between contemporaneous fiscal shifts and announcements, and modeling the share of tax and spending measures within a plan. We allow both correlations to be different according to the type of plan, *TB* versus *EB*. In other words, we allow plans

[18] Early narrative studies consider measures only upon implementation, using $f_{i,t} = e_{i,t}^u + e_{i,t-1,t}^a$ and neglecting $e_{i,t,t+j}^a$, as in Guajardo and others (2014) and Romer and Romer (2010).

to have a different *inter-temporal* and *intra-temporal* structure according to their type.[19] Our auxiliary regressions are

$$\begin{aligned}
\tau^{u}_{i,t} &= \delta^{TB}_{0} e^{u}_{i,t} * TB_{i,t} + \delta^{EB}_{0} e^{u}_{i,t} * EB_{i,t} + \epsilon_{0,i,t} \\
g^{u}_{i,t} &= \vartheta^{TB}_{0} e^{u}_{i,t} * TB_{i,t} + \vartheta^{EB}_{0} e^{u}_{i,t} * EB_{i,t} + \upsilon_{0,i,t} \\
\tau^{a}_{i,t,t+j} &= \delta^{TB}_{j} e^{u}_{i,t} * TB_{i,t} + \delta^{EB}_{j} e^{u}_{i,t} * EB_{i,t} + \epsilon_{j,i,t} \quad j = 1, 2 \\
g^{a}_{i,t,t+j} &= \vartheta^{TB}_{j} e^{u}_{i,t} * TB_{i,t} + \vartheta^{EB}_{j} e^{u}_{i,t} * EB_{i,t} + \upsilon_{j,i,t} \quad j = 1, 2
\end{aligned} \tag{3}$$

The first two equations are related to the *intra-temporal* dimension and describe the average tax (δ) and spending (ϑ) share of *EB* and *TB* plans. The next two equations model the *inter-temporal* structure and describe the relation between unexpected shifts and those announced for years $t + 1$ and $t + 2$, differentiating between *EB* and *TB* plans. Table 9 shows the estimated coefficients. On the basis of these auxiliary regressions we construct the $e^{a}_{i,t,t+1} = \tau^{a}_{i,t,t+1} + g^{a}_{i,t,t+1}$ and $e^{a}_{i,t,t+2} = \tau^{a}_{i,t,t+2} + g^{a}_{i,t,t+2}$ needed to compute impulse responses.[20]

Note that our procedure to derive impulse response functions from narrative shocks is different from the standard approach adopted in the recent empirical literature that derives impulse responses relying either on a truncated MA representation or on linear projection methods. We show in Appendix 2 that the standard application of the linear projections method does not fully exploit the nonlinearities of our statistical model.

5 Data and Results

Macrodata are from the OECD: Appendix 3 provides details on sources and on how we compute the variables used in the VAR. Our government expenditure variable is total government spending net of interest payments on the debt: that is we do not distinguish between government consumption, government investment, transfers

[19] Alternatively we could have allowed the inter-temporal structure of plans to be country—rather than plan-specific (see Alesina and others 2015). We opted for the latter to impose restrictions in the auxiliary regressions more similar to those in the main system—i.e., coefficients restricted across countries and unrestricted across types of plans.

[20] Given the presence of nonlinearities, impulse responses are constructed using the generalized method proposed by Koop and others (1996). This implies computing

$$I(\mathbf{z}_{i,t}, n, \delta, I_{t-1}) = E(\mathbf{z}_{i,t+n} \mid \mathbf{e}_{i,t} = \delta, I_{t-1}) - E(\mathbf{z}_{i,t+n} \mid \mathbf{e}_{i,t} = 0, I_{t-1})$$

using the following steps: (i) generate a baseline simulation for all variables by solving the full nonlinear system dynamically forward. This requires setting to zero all shocks for a number of periods equal to the horizon up to which impulse responses are computed, (ii) generate an alternative simulation for all variables by considering a particular plan and then solve dynamically forward the model up to the same horizon used in the baseline simulation, (iii) compute impulse responses to fiscal plans as the difference between the simulated values in the two steps above, (iv) compute confidence intervals by bootstrapping. In constructing the bootstrap we have to deal with dependence in the residuals of our system of 48 (3 variables and 16 countries) estimated equations. We do so by constructing a matrix 34×48 (our sample is 1981–2014 and it contains 34 annual observations) containing all the residuals in our system and by resampling the rows of such matrix.

Table 9 Estimated coefficients in the auxiliary equations

	Intra-temporal correlations		Inter-temporal correlations			
	TB	EB	TB		EB	
τ	δ_0^{TB}	δ_0^{EB}	δ_1^{TB}	δ_2^{TB}	δ_1^{EB}	δ_2^{EB}
	0.7823	0.3918	0.1552	0.0170	− 0.0415	0.0072
	(0.0175)	(0.0104)	(0.0278)	(0.0099)	(0.0165)	(0.0059)
g	ϑ_0^{TB}	ϑ_0^{EB}	ϑ_1^{TB}	ϑ_2^{TB}	ϑ_1^{EB}	ϑ_2^{EB}
	0.2177	0.6082	0.1290	0.0305	0.1590	0.0364
	(0.0175)	(0.0104)	(0.0315)	(0.0152)	(0.0187)	(0.0091)

Note. The table reports the coefficients estimated from the regressions in (3) distinguishing between *intra-temporal* and *inter-temporal* correlations. Standard errors in parentheses

(social security benefits, etc.) and other government outlays. Alesina and others (2017) investigate whether multipliers for government transfers differ from those for other spending items finding very moderate differences (see Sect. 2.1 for a more detailed discussion). The panel includes 16 OECD countries over the years 1981–2014.

5.1 Main Results

Before presenting our main results, it is useful to describe the structure of the fiscal shocks we simulate. We analyze the response of the economy to an EB and TB plan over five years (from $t = 0$ to $t = 4$). In both cases plans represent a *permanent* reduction of the primary deficit of 1 percent of GDP. Each plan contains an unexpected fiscal shock, announced and implemented at $t = 0$, and two shocks also announced at $t = 0$ but implemented at $t = 1$ and $t = 2$. These plans have a different *intra-temporal* and *inter-temporal* structure following the correlations reported in Table 9. Therefore, our simulated EB plan has, in percentage of GDP, $e_0^u = 0.86$, $e_{0,1}^a = 0.10$ and $e_{0,2}^a = 0.04$.[21] Similarly, our simulated TB plan has $e_0^u = 0.75$, $e_{0,1}^a = 0.21$, $e_{0,2}^a = 0.03$.[22]

We first show the impulse responses from the general unrestricted model (2) that allows for all nonlinearities.[23] The impulse responses of the variables in our VAR and of the indicator $F(s)$ are presented in Fig. 2. Lines with triangles and squares show the responses of the variables in the case, respectively, of an *EB* and a *TB* plan *introduced at a time when the economy is in an expansionary state* (defined as

[21] The *intra-temporal* structure of these EB shocks is as follows: $\tau_0^u = 0.34$, $g_0^u = 0.52$, $\tau_{0,1}^a = -0.04$, $g_{0,1}^a = 0.14$, $\tau_{0,2}^a = 0.01$, $g_{0,2}^a = 0.03$.

[22] The *intra-temporal* structure of these TB shocks is as follows: $\tau_0^u = 0.59$, $g_0^u = 0.16$, $\tau_{0,1}^a = 0.11$, $g_{0,1}^a = 0.10$, $\tau_{0,2}^a = 0.01$, $g_{0,2}^a = 0.02$.

[23] All the results we present are obtained using 1000 bootstrap repetitions.

$F(s) \simeq 0.2$); the responses of the economy to EB and TB fiscal consolidations *starting in a recessionary state* (defined as $F(s) \simeq 0.8$) are indicated by lines with circles and stars. It is important to remark that the state of the economy is affected by fiscal policy and can change as a plan evolves. Instead, the nature of the plan (TB, EB) is known upon announcement and does not change throughout the simulation. Responses are cumulated over time: thus our impulse response functions measure the deviation of an outcome from its level absent the change in fiscal policy.[24]

The first row of Fig. 2 shows that the stronger nonlinearity is that between *TB* and *EB* plans. In the case of an *EB* consolidation, the point estimates of the responses of output growth are almost identical across the two states of the economy, while in the case of a *TB* consolidation the point estimates are different, although the difference is not statistically significant. TB plans are always more recessionary than EB plans although the difference is larger when the plan is introduced in an expansion than in a recession.

The second and third rows of Fig. 2 show the responses of government revenues and government consumption (defined as explained at the top of this section and both measured as a fraction of GDP) to a *TB* and an *EB* plan starting from the two initial states. Observe that, on average, revenues increase by a larger amount during a TB consolidation, and spending decreases the most during an EB consolidation. This confirms that our classification of plans is trustworthy. Interestingly, we observe a positive response of revenues also to an EB consolidation, and a negative response of spending to a TB consolidation implemented in recession (while in expansion the response is just above zero). This confirms that spending and tax measures are not taken in isolation and thus supports our choice of analyzing plans rather than individual shifts in taxes and spending.

The fourth row of Fig. 2 shows the responses of $F(s)$: in all four cases a consolidation increases the recession indicator (the impulse response is always positive). There is, however, a significant difference between type of plans. During *TB* consolidations $F(s)$ increases much more than during *EB* ones and this holds both in expansions and recessions. Note that when a consolidation starts during a recession the difference in $F(s)$ between tax-based and expenditure-based adjustments initially is not statistically significant. It becomes significant 2 years after the start of the consolidation, indicating that TB consolidations worsen the state of the economy for a more prolonged period of time than EB ones.

Overall, the total effect on output growth—which is what matters and is the result of the effect going through the response of $F(s)$ as well as the effect going through all other coefficients in the model—is always statistically different between the two types of adjustment.

Table 10 presents the fiscal multipliers of an EB and TB permanent fiscal plan of one percent of GDP implemented starting in expansion or in recession. Multipliers are computed following two definitions: we show a cumulated multiplier and a multiplier defined as the sum of the output response over the simulation horizon, divided by the sum of the primary surplus response computed as $(\Delta\tau - \Delta g)$. The

[24] For $F(s)$ we report non-cumulated responses.

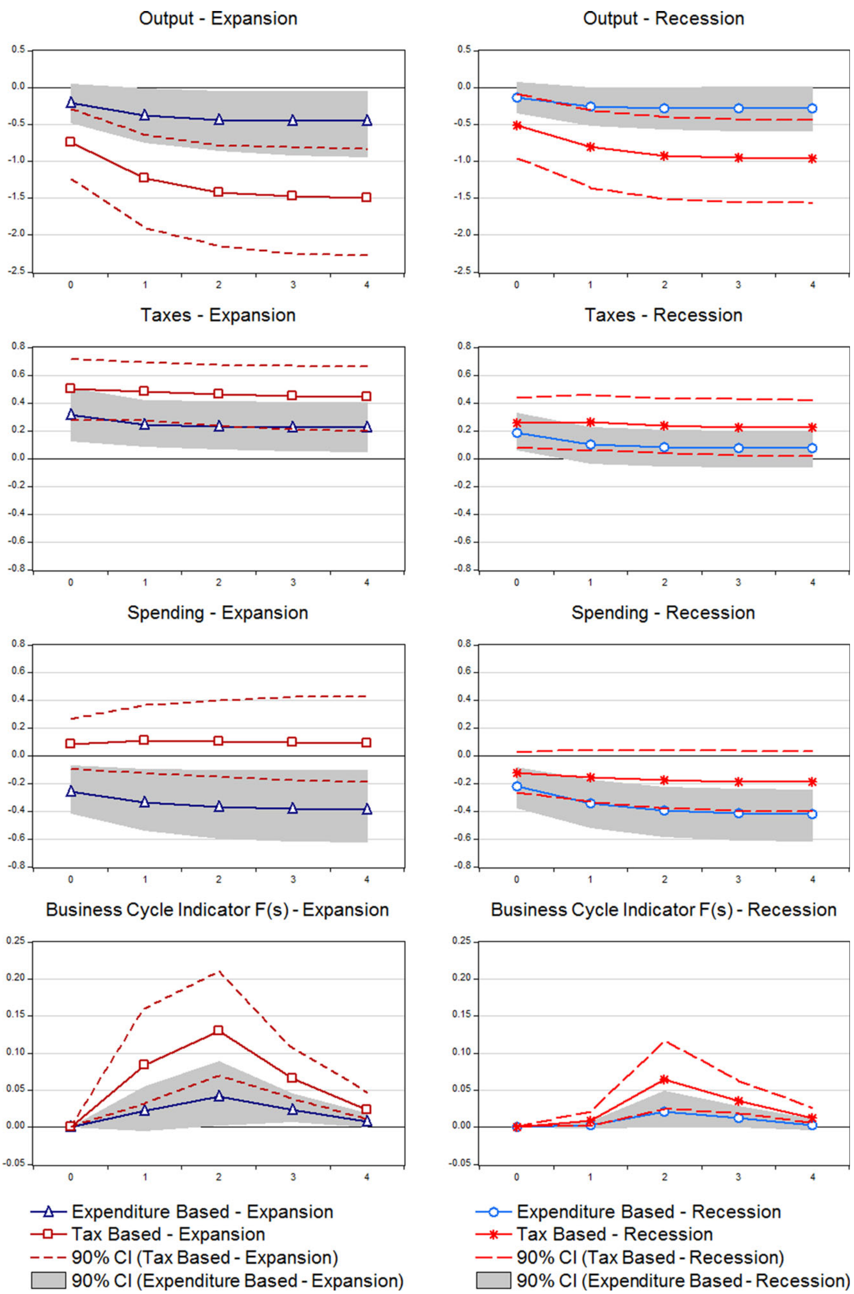


Fig. 2 Impulse responses from the model with heterogeneities between EB and TB plans and across states of the cycle. Note. The graph shows the response of VAR variables and $F(s)$ to a deficit reduction plan of 1 percent of GDP. IRFs for tax-based shock starting in recession labeled with stars, for tax-based shock starting in expansion labeled with squares, for expenditure-based shock starting in recession labeled with circles and expenditure-based shock starting from expansion labeled with triangles. Dashed lines and shaded areas are for bootstrapped (1000 repetitions) 90 percent confidence intervals

latter is based on the definition suggested by Woodford (2011) and used by Auerbach and Gorodnichenko (2012b) and has the advantage of taking into account the response of taxes and spending to the fiscal plan, as well as considering the persistence of fiscal shocks. Note that since our simulated plans contain both spending and tax measures—and both expenditure and receipts react to EB an TB plans—what we compute here is a *primary surplus* multiplier. Regardless of the definition, the multipliers of EB plans are much lower (in absolute value) than those of TB plans, both in expansion and in recession. In expansion, a tax-based fiscal consolidation plan of one percent of GDP has a cumulative multiplier of − 1.5, while the cumulative multiplier of a spending-based plan of the same size is − 0.46. Similarly, the *primary surplus multiplier* of a TB plan is − 3.70, while that of a EB plan is − 0.75. Multipliers in recessions are slightly lower (in absolute value) than in expansions, and exhibit a similar heterogeneity between TB and EB plans.

We formally test the relevance of the two sources of nonlinearity by bringing to the data the hypotheses introduced in Sect. 4.2. Table 11 reports the observed value of the likelihood ratio tests along with their probability under the null derived both on the basis of the asymptotic distribution and of a bootstrapped distribution that takes explicitly into account the (possibly small) size of our sample. All hypotheses are rejected at least at the 5 percent confidence level, regardless of the way we perform the tests. We conclude that all nonlinearities are statistically significant.

Next we consider two polar cases: the case in which only the "when" determines the output response to a fiscal adjustment and the case in which only the "how" is relevant. We study these cases notwithstanding the fact that the corresponding restrictions are rejected. The impulse responses reported in Fig. 3 are based on a model in which we remove the nonlinearity across types of plans (imposing $\mathbf{a} = \mathbf{b}$, $\mathbf{c} = \mathbf{d}$) while keeping that across states of the economy. As in our baseline simulations, the state of the economy is allowed to respond endogenously to the fiscal policy shift. Looking at the first row of the figure the response of output after the announcement of a fiscal consolidation plan does not appear to be strongly affected by the state of the economy: the two impulse responses are very similar and their confidence intervals overlap, thus confirming that the state of the economy—remember that here "state of the economy" refers to the state at the time the consolidation is first introduced—does not seem to be very relevant. In other words, overlooking the composition of the fiscal adjustment (*TB* or *EB*), fiscal multipliers do not appear to differ significantly when the economy starts from an expansion or a recession. This result confirms the finding reported in Owyang and others (2013) and in Ramey and Zubairy (2015, 2017). Of course this does not mean that the welfare effects are also similar: losing one percent of GDP when the economy is already in a recession can be more harmful compared to losing the same amount of output when the economy is expanding. The response of the indicator $F(s)$ in the fourth row confirms that implementing a consolidation always increases the recession indicator—slightly more when the economy starts from an expansion rather than a recession.

Finally, in Fig. 4 we keep only the nonlinearity across type of plans. In other words we replicate (using a panel VAR rather than estimating a truncated MA representation) the exercise performed in Alesina and others (2015). The strong similarity between the impulse responses reported here and those reported in the

Table 10 Output multipliers

	$\sum_{t=0}^{4} \Delta y_t$	$\frac{\sum_{t=0}^{4} \Delta y_t}{\sum_{t=0}^{4} (\Delta \tau_t - \Delta g_t)}$
Expansion		
EB	– 0.46	– 0.75
	(– 0.94; – 0.06)	(– 1.55; – 0.31)
TB	– 1.50	– 3.70
	(– 2.29; – 0.84)	(– 8.79; – 1.87)
Recession		
EB	– 0.28	– 0.58
	(– 0.59; 0.01)	(– 1.13; – 0.18)
TB	– 0.96	– 2.31
	(– 1.58; – 0.44)	(– 4.39; – 1.26)

Note. The table reports the cumulated and cumulated as fraction of cumulated primary surplus multipliers obtained from the model where we include all the nonlinearities. 90 percent bootstrapped (1000 repetitions) confidence intervals for the first column and one standard deviation for the second in parentheses

Table 11 Tests of hypotheses

H_0	Likelihood. ratio	Number of restrictions	Probability	
			Asymptotic test	Bootstrap test
(i)	27.7575	14	0.0153	0.0120
(ii)	20.1271	6	0.0026	0.0110
(iii)	45.2928	20	0.0010	0.0020
(iv)	70.9075	29	0.0000	0.0020

Note. The table reports the likelihood ratios, the number of imposed restrictions on parameters and the p-values (asymptotic and bootstrap computed with 1000 repetitions) for the four tests introduced in Sect. 4.2

previous paper suggests that the effect of predictability of the adjustments, which is addressed within a VAR but not in an MA, is minor.[25]

5.2 Robustness

In this section we evaluate the robustness of our results along two dimensions. First, we ask to what extent they depend on the accompanying monetary policy and thus what could be the effect of fiscal consolidations implemented at the ZLB. Second, we analyze the effect of adopting an alternative specification of $F(s)$, assuming that the state of the economy does not change following the shift in fiscal policy.

[25] Appendix 4 reports the multipliers for these alternative specifications.

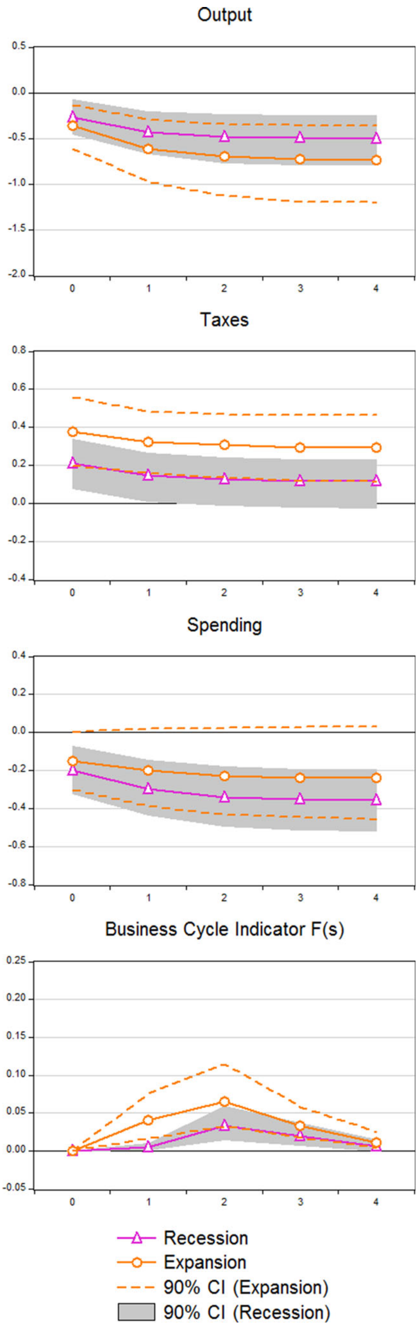


Fig. 3 Impulse responses from the model with heterogeneity only across states of the cycle. Note. The graph shows the response of VAR variables and $F(s)$ to a deficit reduction plan of 1 percent of GDP. IRFs for a plan starting in expansion labeled with circles, for a plan starting in recession labeled with triangles. Dashed lines and shaded areas are for bootstrapped (1000 repetitions) 90 percent confidence intervals

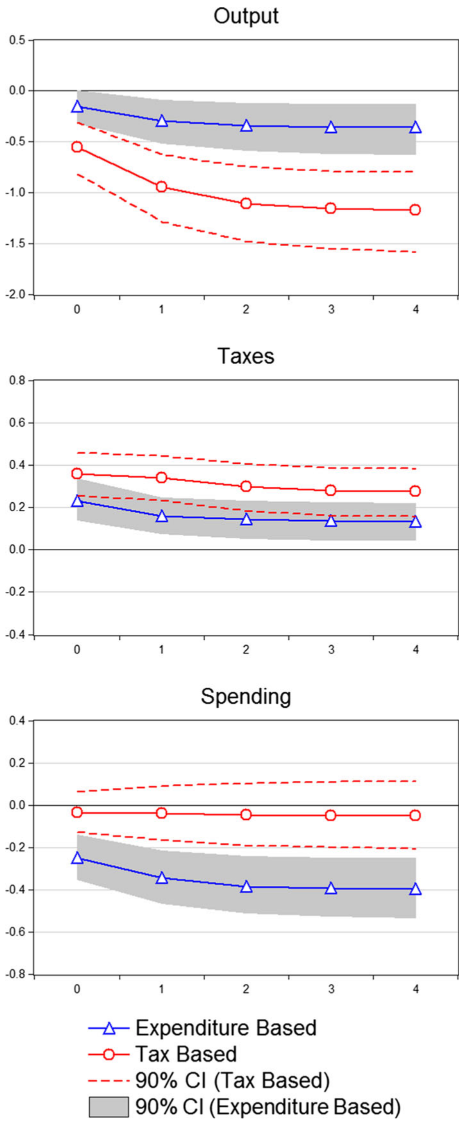


Fig. 4 Impulse responses from the model with heterogeneity only between EB and TB plans. Note. The graph shows the response of VAR variables to a deficit reduction plan of 1 percent of GDP. IRFs for tax-based shock labeled with circles, for expenditure-based shock labeled with triangles. Dashed lines and shaded areas are for bootstrapped (1000 repetitions) 90 percent confidence intervals

5.2.1 Fiscal Adjustments at the ZLB

Ideally, one would want to study how multipliers are affected not only by the cycle and the composition of a fiscal plan but also whether they occur at or close to the zero lower bound. Unfortunately, we do not have enough observations to consider all three factors (state of the economy, composition and ZLB) at the same time. What we can check, however, is whether the asymmetries we identified can be explained by a different (more or less constrained) response of monetary policy. If the asymmetries in fiscal multipliers were related to a different response of monetary policy our evidence could considerably change when monetary policy is constrained.

In order to assess the potential relevance of the monetary policy response (or lack thereof at the ZLB) we perform two exercises. First, we split our data in two subsamples: euro area countries (Austria, Belgium, France, Finland, Germany, Ireland, Italy, Portugal and Spain) from 1999 onward and non-euro area countries (Australia, Denmark, UK, Japan, Sweden, US and Canada) together with euro area countries before 1999. The motivation for this split is that the common currency prevents monetary policy from responding to fiscal developments in individual member countries. However, while it is true that monetary policy cannot respond at the country level, the ECB could still respond if fiscal consolidation happened in a large enough number of euro area countries at the same time. To capture this possible common response of monetary policy in the euro area, the specification also includes year fixed effects estimated on euro countries from 1999 onward. Model 2 is thus extended to

$$
\begin{aligned}
\Delta y_{i,t} = {} & \begin{bmatrix} 1 - F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} A_1^E \\ A_1^R \end{bmatrix} \mathbf{z_{i,t-1}} \\
& + \begin{bmatrix} [1 - F(s_{i,t})] \cdot Euro_{i,t} \\ F(s_{i,t}) \cdot Euro_{i,t} \\ [1 - F(s_{i,t})] \cdot (1 - Euro_{i,t}) \\ F(s_{i,t}) \cdot (1 - Euro_{i,t}) \end{bmatrix}' \begin{bmatrix} \mathbf{a}'_{Euro}\mathbf{e}_{i,t} & \mathbf{b}'_{Euro}\mathbf{e}_{i,t} \\ \mathbf{c}'_{Euro}\mathbf{e}_{i,t} & \mathbf{d}'_{Euro}\mathbf{e}_{i,t} \\ \mathbf{a}'_{NoEuro}\mathbf{e}_{i,t} & \mathbf{b}'_{NoEuro}\mathbf{e}_{i,t} \\ \mathbf{c}'_{NoEuro}\mathbf{e}_{i,t} & \mathbf{d}'_{NoEuro}\mathbf{e}_{i,t} \end{bmatrix} \begin{bmatrix} TB_{i,t} \\ \mathrm{EB}_{i,t} \end{bmatrix} \\
& + \lambda_i + \chi_t \cdot Euro_{i,t} + \partial_t \cdot (1 - Euro_{i,t}) + u_{i,t}
\end{aligned}
$$

where $\mathbf{a}_j = [\, a_{j,1} \quad a_{j,2} \quad a_{j,3} \,]'$ for $j = Euro, NoEuro$ (similarly for $\mathbf{b}_j$, $\mathbf{c}_j$, $\mathbf{d}_j$) and $Euro_{i,t} = 1 \; if \begin{cases} country = AUT, BEL, DEU, ESP, FIN, FRA, IRL, ITA, PRT \\ year \geq 1999 \end{cases}$

Figure 5 plots the impulse response functions from this model. For consolidations started during an expansion, the heterogeneity between EB and TB adjustments is clear regardless of whether monetary policy can respond or not. As in the baseline simulations, there is little evidence of heterogeneity across states of the cycle: no heterogeneity at all in the response of the economy to EB consolidations and some heterogeneity in the response to TB adjustments.

For consolidations started during a recession, instead, whether austerity is EB or TB seems to be irrelevant. But the ability of monetary policy to respond now makes a difference: the output cost of both types of plans is virtually zero if monetary policy is unconstrained, while both types of plans are equally recessionary if monetary policy is constrained. Remember that when we say ”starting in a recession” or ”starting in an expansion” we mean a probability, respectively, of 0.8 and 0.2 to be in a recession: the majority of consolidation episodes are not in these

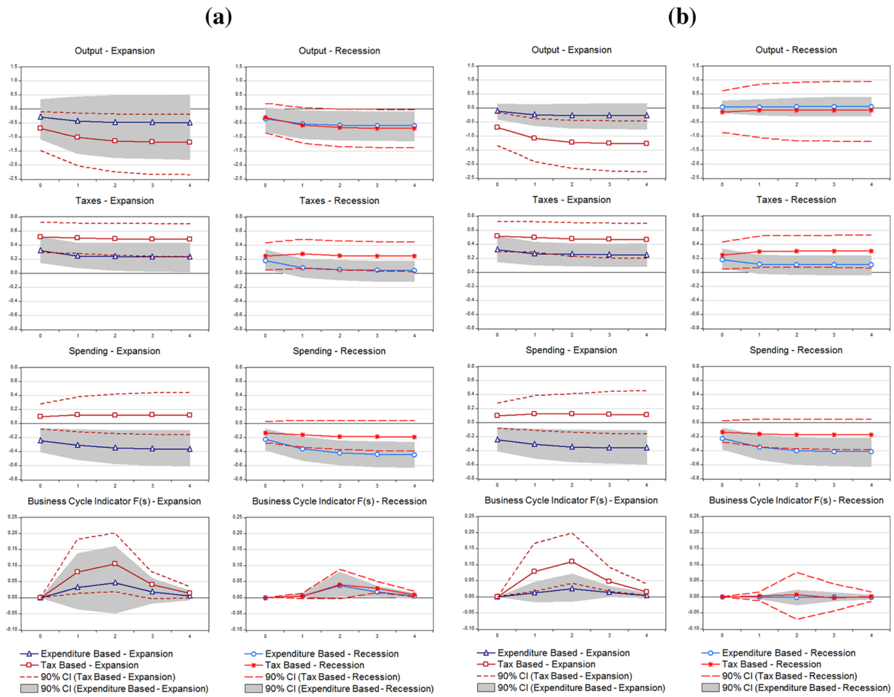


Fig. 5 Impulse responses from the model with heterogeneities between EB and TB plans and across states of the cycle, controlling for monetary policy. **a** Constrained Monetary Policy. **b** Unconstrained Monetary Policy. Note. The graph shows the response of VAR variables and $F(s)$ to a deficit reduction plan of 1 percent of GDP in euro area countries—under a common monetary policy—panel (**a**), and in the other countries—where monetary policy is free to adjust—panel (**b**). IRFs for tax-based shock starting in recession labeled with stars, for tax-based shock starting in expansion labeled with squares, for expenditure-based shock starting in recession labeled with circles and expenditure-based shock starting from expansion labeled with triangles. Dashed lines and shaded areas are for bootstrapped (1000 repetitions) 90 percent confidence intervals

two categories but in a situation of a ”neutral” cycles or mild downturns and mild expansions.

We should use some caution in interpreting these results, since by adding an additional layer of heterogeneity—euro versus non-euro—we are losing power and some of the coefficients are more imprecisely estimated, as witnessed by the wider confidence intervals of the impulse responses. Nevertheless it would be fair to conclude that European austerity between 2010 and 2014 might have been more costly because it was implemented while economies were in a downturn and monetary policy could not respond to country-specific fiscal plans.

As a further robustness check, we study whether the response of the economy to consolidations implemented while monetary policy is at the zero lower bound plays a significant role in influencing our results. Unfortunately, we cannot split our data between countries in years at the ZLB and countries in years out of the ZLB because the number of observations in the former group is too small. As an alternative we check the stability of our baseline results by removing the observations at the ZLB

Fig. 6 Impulse responses from the model with heterogeneities between EB and TB plans and across ▶ states of the cycle, excluding episodes at the ZLB. Note. The graph shows the response of VAR variables and $F(s)$ to a deficit reduction plan of 1 percent of GDP, from a model where we exclude episodes at the ZLB. IRFs for tax-based shock starting in recession labeled with stars, for tax-based shock starting in expansion labeled with squares, for expenditure-based shock starting in recession labeled with circles and expenditure-based shock starting from expansion labeled with triangles. Dashed lines and shaded areas are for bootstrapped (1000 repetitions) 90 percent confidence intervals

from our sample, i.e., we remove euro area countries in 2013 and 2014, the US from 2008 onward and Japan from 1996 onward.[26] The results of this exercise are presented in Fig. 6. The impulse response functions are very similar to the baseline case and this confirms that observations at the ZLB do not influence our findings significantly.[27]

Note that the findings that monetary accommodation is not very important might depend on the type of fiscal adjustment that we are considering. As we have already noted, we measure the effect of permanent adjustments, and the response of monetary policy to permanent and transitory adjustments is theoretically different. For instance, permanent adjustments to expenditure by their nature do not affect the natural real rate of interest.[28]

5.2.2 *An Alternative Specification of F(s)*

As already discussed, our smooth transition VAR describes the state of the economy using a "backward-looking" moving average and allows the indicator $F(s_{i,t})$ to evolve following the introduction of a fiscal plan. In this section we analyze how the results are affected by making $F(s_{i,t})$ function of contemporaneous output growth, while holding the state of the economy constant following the introduction of a plan. In practice, we keep our VAR specification unaltered but we change the definition of $\mu_{i,t}$ (and, hence, $F(s_{i,t})$) as follows:

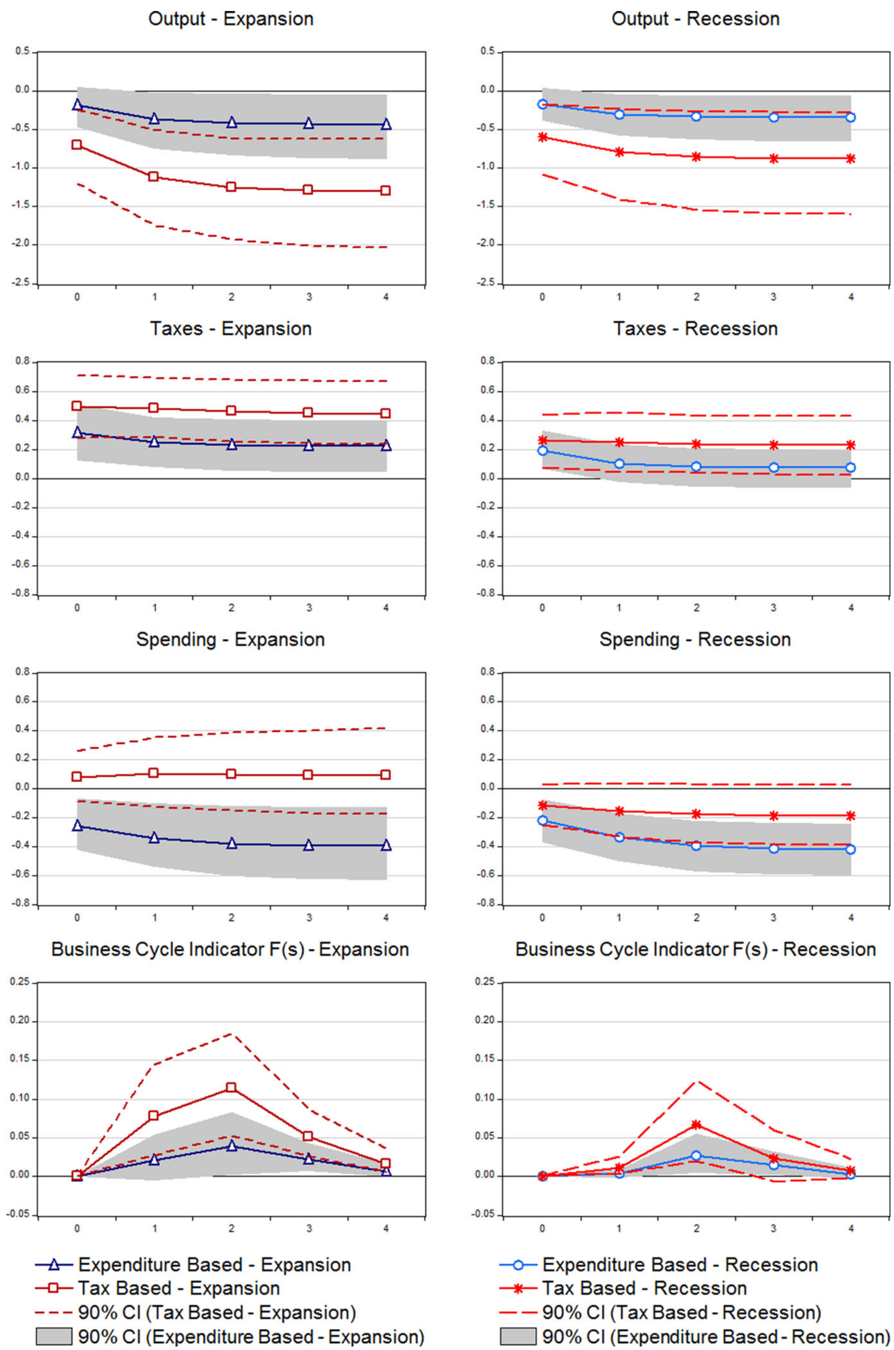
$$F(s_{i,t}) = \frac{\exp(-\gamma_i s_{i,t})}{1 + \exp(-\gamma_i s_{i,t})}, \quad \gamma_i > 0$$
$$s_{i,t} = \frac{\mu_{i,t} - \mathbb{E}(\mu_{i,t})}{\sigma(\mu_{i,t})}$$
$$\mu_{i,t} = \frac{\Delta y_{i,t} + \Delta y_{i,t-1}}{2}$$

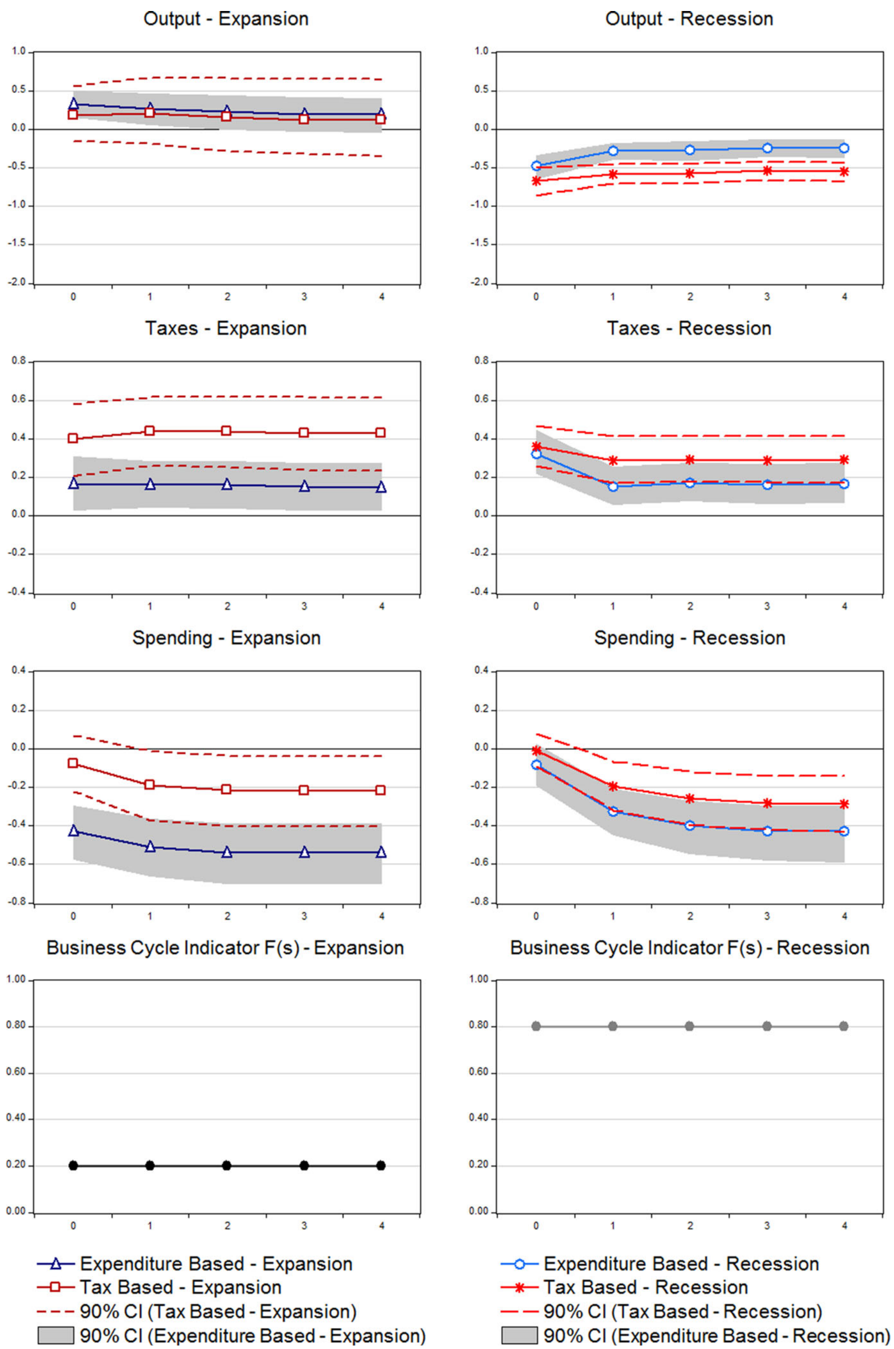
We report in Fig. 7 impulse responses obtained adopting this alternative specification. Note that instead of reporting the response of $F(s)$ to the fiscal shock, we plot its constant level. Indeed, as previously discussed in Sect. 3, in this framework we

[26] More precisely, we perform this check starting form the baseline model and interacting the fiscal shocks in the equation for output with a dummy equal to one for observations at the ZLB and another dummy which equals one for observations outside the ZLB. Then, we perform our simulation using the coefficients estimated on the latter. We do not present the IRFs for consolidations at the ZLB as they are unreliable, being estimated on a very limited number of observations.

[27] The multipliers computed under these two specifications are reported in Appendix 4.

[28] We are grateful to one of our referees for having made this point.


Output - Expansion
Output - Recession
Taxes - Expansion
Taxes - Recession
Spending - Expansion
Spending - Recession
Business Cycle Indicator F(s) - Expansion
Business Cycle Indicator F(s) - Recession
Expenditure Based - Expansion
Tax Based - Expansion
90% CI (Tax Based - Expansion)
90% CI (Expenditure Based - Expansion)
Expenditure Based - Recession
Tax Based - Recession
90% CI (Tax Based - Recession)
90% CI (Expenditure Based - Recession)



cannot allow $F(s)$ to move in response to the fiscal consolidation, since it is constructed as a function of the contemporaneous level of output. Hence we need to

◀**Fig. 7** Impulse responses from the model with heterogeneities between EB and TB plans and across states of the cycle, alternative specification of $F(s)$. Note. The graph shows the response of VAR variables to a deficit reduction plan of 1 percent of GDP, from a model where we use an alternative specification of $F(s)$ depending on current output growth. IRFs for tax-based shock starting in recession labeled with stars, for tax-based shock starting in expansion labeled with squares, for expenditure-based shock starting in recession labeled with circles and expenditure-based shock starting from expansion labeled with triangles. Dashed lines and shaded areas are for bootstrapped (1000 repetitions) 90 percent confidence intervals. The cycle indicator is held fixed throughout the simulation horizon

keep $F(s)$ fixed either at 0.2 (expansion) or 0.8 (recession) throughout the simulation.

These results confirm the findings, reported in Auerbach and Gorodnichenko (2012a, b), of an asymmetric output response to spending adjustments. We find a small, marginally significant, expansionary effect of EB adjustments started in expansion and a small, significant, recessionary effect of EB adjustments started in recession.[29] The output effect of EB adjustments started in recession is smaller than what reported by AG. This result is consistent with our identification strategy that selects, using narrative methods, only fiscal consolidations episodes—while Auerbach and Gorodnichenko (2012a, b) study both expansions and contractions. We do not expect our impulse responses to be of the same size of those derived considering both fiscal contractions and fiscal expansions. Importantly, whenever fiscal adjustments have a significant effect on output, that is when they start in a recession, the asymmetry between EB and TB plans is still present with TB plans being more recessionary than EB ones.

6 Conclusions

Fiscal consolidations can differ along three dimensions: their composition (increases in taxes vs. reductions in expenditures), the state of the business cycle when they are introduced and whether they occur at the ZLB or, more generally, whether monetary policy can respond to the consolidation. In this paper we mainly focussed on the first two aspects. We concluded that both the composition of a fiscal adjustment and the state of the business cycle matter, but the composition effect is much more robust. On average expenditure-based permanent adjustments have consistently much lower costs than tax-based permanent ones. The dynamic response of the economy to a consolidation plan does depend on whether this is adopted in a period of economic expansion or contraction, but the quantitative significance of this source of nonlinearity is small relative to the one which depends on the type of consolidation. The role of the ZLB is more difficult to assess given the low number of observations. However, our (admittedly not conclusive) evidence does point toward some but not overly large differences between episodes at or away from the ZLB, or more generally when monetary policy cannot react to a fiscal adjustment in a monetary union. This is an issue which deserves further research.

[29] See Appendix 4 for the exact multipliers computed under this alternative model.

Appendix 1: Predictability and Exogeneity

In a dynamic time-series model, estimation and simulation require, respectively, weak and strong exogeneity: these requirements are different from lack of predictability. To illustrate the point consider the following simplified model, which only includes the unanticipated component of fiscal plans:

$$\begin{aligned}\Delta y_t &= \beta_0 + \beta_1 e_t^u + \beta_3 \Delta y_{t-1} + \beta_4 \Delta\tau_{t-1} + \beta_5 \Delta g_{t-1} + u_{1t} \\ e_t^u &= \gamma_1 \Delta y_{t-1} + \gamma_2 \Delta\tau_{t-1} + \gamma_3 \Delta g_{t-1} + u_{2t} \\ \begin{pmatrix} u_{1t} \\ u_{2t} \end{pmatrix} &\sim N\left[\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma_{11} & \sigma_{12} \\ \sigma_{12} & \sigma_{22} \end{pmatrix}\right]\end{aligned}$$

The condition required for e_t^u to be weakly exogenous for the estimation of β_1 is $\sigma_{12} = 0$. This condition is independent of $\gamma_1, \gamma_2, \gamma_3$. In other words, when weak exogeneity is satisfied, the existence of predictability does not affect the consistency of the estimate of β_1. Moreover β_1 measures, by construction, the impact on Δy_t of u_{2t}, i.e., of the part of e_t^u that cannot be predicted by Δy_{t-1}, $\Delta\tau_{t-1}$ and Δg_{t-1}. In fact, by the partial regression theorem, estimating first $\hat{u}_{2t} = e_t^u - \hat{\gamma}_1 \Delta y_{t-1} + \hat{\gamma}_2 \Delta\tau_{t-1} + \hat{\gamma}_3 \Delta g_{t-1}$ and then δ_1, running $\Delta y_t = \delta_0 + \delta_1 \hat{u}_{2t} + v_t$, gives $\hat{\beta}_1 = \hat{\delta}_1$.

Appendix 2: MA's Versus VAR's

The VAR model described in the text model (2) is not the way impulse response functions are constructed in the recent empirical literature. In the literature the effect of narratively identified shifts in fiscal variables relies either on estimates of a truncated MA representation or on linear projection methods. The reason for these choices is that in the presence of multiple nonlinearities the MA representation of a VAR is much heavier than in the linear case—which means it could only be estimated imposing restrictions that limit the relevance of such nonlinearities. Consider for instance the following model in which fiscal adjustment plans have heterogenous effects according to the state of the cycle, but the VAR dynamics does not depend on the state of the economy, that is, using the terminology in the text, $A_i^E = A_i^R$. Assume also that *TB* and *EB* plans have identical effects.[30]

$$\mathbf{z}_{i,t} = A_1 \mathbf{z}_{i,t-1} + (1 - F(s_{i,t})) B_1 e_{i,t} + F(s_{i,t}) B_2 e_{i,t} + \mathbf{u}_{i,t} \tag{4}$$

where $\mathbf{z}_{i,t}$ is the vector containing output growth and the growth rates of taxes and spending, $e_{i,t}$ are, as in the main text, the narratively identified fiscal adjustments and $\mathbf{u}_{i,t}$ unobservable VAR innovations. From this VAR we would derive the following MA truncated representations:

[30] Allowing for the presence of *TB* and *EB* plans would strengthen our point but at the cost of making the algebra more complicated.

$$\mathbf{z}_{i,t} = \sum_{j=0}^{k} A_1^j \left((1 - F(s_{i,t-j})) B_1 e_{i,t-j} + F(s_{i,t-j}) B_2 e_{i,t-j} \right) + \sum_{j=0}^{k} A_1^j \mathbf{u}_{i,t-j} + A_1^{k+1} \mathbf{z}_{i,t-k-1}$$

Now apply to this framework the linear projection method. This would amount to deriving impulse responses for the relevant component of $\mathbf{z}_{i,t}$—say $\Delta y_{i,t}$—running the following set of regressions[31]:

$$\Delta y_{i,t+h} = \alpha_{i,h} + (1 - F(s_{i,t}))\beta_{h,1} e_{i,t} + F(s_{i,t})\beta_{h,2} e_{i,t} + \Gamma_h \mathbf{z}_{i,t} + \epsilon_{i,t} \tag{5}$$

Now compare this with the more general case in which the VAR dynamics is also affected by the state of the cycle—that is remove the restriction $A_i^E = A_i^R, (i = 1, 2, 3)$:

$$\mathbf{z}_{i,t} = (1 - F(s_{i,t}))A_1(L, E)\mathbf{z}_{i,t-1} + F(s_{i,t})A_1(L, R)\mathbf{z}_{i,t-1} + (1 - F(s_{i,t}))B_1 e_{i,t} \\ + F(s_{i,t})B_2 e_{i,t} + \mathbf{u}_{i,t}$$

In this case the truncated MA representation would be much more complicated than (5), as the response of $\mathbf{z}_{i,t+h}$ to $e_{i,t}$ would depend on all states of the economy between t and $t + h$. Estimating the correct linear projection would no longer be feasible.

To further illustrate the point observe that the correct linear projection to estimate the effect of $e_{i,t}$ on $\Delta y_{i,t+1}$:

$$\begin{aligned} \Delta y_{i,t+1} = \alpha_{i,1} &+ (1 - F(s_{i,t+1}))F(s_{i,t})\beta_{1,1} e_{i,t} + (1 - F(s_{i,t+1}))(1 - F(s_{i,t}))\beta_{1,2} e_{i,t} \\ &+ (F(s_{i,t+1}))F(s_{i,t})\beta_{1,3} e_{i,t} + (F(s_{i,t+1}))(1 - F(s_{i,t}))\beta_{1,4} e_{i,t} \\ &+ \Gamma_h \mathbf{z}_{i,t} + \epsilon_{i,t} \end{aligned} \tag{6}$$

is in general different from:

$$\Delta y_{i,t+1} = \alpha_{i,h} + (1 - F(s_{i,t}))\beta_{1,1} e_{i,t} + F(s_{i,t})\beta_{1,2} e_{i,t} + \Gamma_h \mathbf{z}_{i,t} + \epsilon_{i,t} \tag{7}$$

Note, in closing, that the cases in which the two representations coincide are very specific. Indeed, when (6) is the data generating process and (7) is estimated, the implied assumption is that the states $F(s_{i,t+1}) = 1$ and $F(s_{i,t+1}) = 0$ are observationally equivalent.

Summing up: if the data are generated by (6) the VAR representation is much more parsimonious than the linear projection which becomes practically not feasible unless very strong restrictions are imposed on the empirical model.

Appendix 3: Data Sources

See Table 12.

[31] This is the specification adopted by Auerbach and Gorodnichenko (2012a) to estimate a regime-dependent impulse response.

Table 12 Data sources

Variable	Label	Definition
Output (real)	*gdpv*	Gross domestic product, volume, market prices
Output (nominal)	*gdp*	Gross domestic product, value, market prices
Govt. consumption (real)	*cgv*	Govt. final consumption expenditure, volume
Govt. investment (real)	*igv*	Govt. gross fixed capital formation, volume
Revenues (nominal)	*yrg*	Current receipts, general govt., value
Social security (nominal)	*sspg*	Social security benefits paid by general govt., value
Other outlays (nominal)	*oco*	Other current outlays, general govt., value
Population	*popt*	Population, all ages, all persons

gdpv, **gdp**: OECD Economic Outlook n.97; for Ireland, IMF WEO April 2015;
cgv: OECD Economic Outlook n.97; for Ireland we used data from AMECO (final consumption expenditure of general government at current prices deflated in 2012 prices with the correspondent deflator series in the AMECO dataset—price deflator total final consumption expenditure of general government);
igv: OECD Economic Outlook n.97; for Austria missing data in the period 1978–1994; for Ireland, Italy, Portugal, Spain, we used data from AMECO (gross fixed capital formation at current prices: general government, deflated with correspondent deflator series in AMECO dataset—price deflator gross fixed capital formation: total economy); note that for Portugal and Ireland series are, respectively, in 2011 and 2012 prices;
yrg: OECD Economic Outlook n.98; for Australia in the period 1978–1988 and Ireland before 1990, Economic Outlook n.88;
sspg: OECD Economic Outlook n.98; for Australia in the period 1978–1988 and Ireland before 1990, Economic Outlook n.88;
oco: OECD Economic Outlook n.98; for Australia in the period 1978–1988 and Ireland before 1990, Economic Outlook n.88;
popt: OECD Historical Population Data and Projections (1950–2050).

The variables we use in the analysis are constructed as follows:

- GDP deflator

$$pgdp_{i,t} = \frac{gdp_{i,t}}{gdpv_{i,t}}$$

- Real per capita GDP growth

$$\Delta y_{i,t} = 100 * \left[\log\left(\frac{gdpv_{i,t}}{gdpv_{i,t-1}}\right) - \log\left(\frac{popt_{i,t}}{popt_{i,t-1}}\right)\right]$$

- Percentage Change of Government Spending (as fraction of GDP)

$$\Delta g_{i,t} = 100 * \left[\frac{\left(igv_{i,t} + cgv_{i,t}\right) + \frac{oco_{i,t} + sspg_{i,t}}{pgdp_{i,t}}}{gdpv_{i,t}} - \frac{\left(igv_{i,t-1} + cgv_{i,t-1}\right) + \frac{oco_{i,t-1} + sspg_{i,t-1}}{pgdp_{i,t-1}}}{gdpv_{i,t-1}} \right]$$

- Percentage Change of Government Revenues (as fraction of GDP)

$$\Delta \tau_{i,t} = 100 * \left[\frac{\frac{yrg_{i,t}}{pgdp_{i,t}}}{gdpv_{i,t}} - \frac{\frac{yrg_{i,t-1}}{pgdp_{i,t-1}}}{gdpv_{i,t-1}} \right]$$

Appendix 4: Multipliers in the Alternative Specifications

See Tables 13, 14, 15.

Table 13 Output multipliers in the alternative specifications

	$\sum_{t=0}^{4} \Delta y_t$	$\frac{\sum_{t=0}^{4} \Delta y_t}{\sum_{t=0}^{4} (\Delta \tau_t - \Delta g_t)}$
Nonlinearity in the state of the cycle		
Expansion	– 0.74	– 1.36
	(– 1.21; – 0.36)	(– 2.67; – 0.81)
Recession	– 0.50	– 1.07
	(– 0.79; – 0.25)	(– 1.72; – 0.67)
Nonlinearity in the composition of plan		
EB	– 0.36	– 0.69
	(– 0.63; – 0.13)	(– 1.15; – 0.36)
TB	– 1.17	– 3.56
	(– 1.59; – 0.81)	(– 6.26; – 2.29)

Note. The table reports the cumulated and cumulated as fraction of cumulated primary surplus multipliers estimated, respectively, in the models in which we include the nonlinearity in the business cycle (expansion vs. recession) and the nonlinearity in the composition of the plan (EB vs. TB). 90 percent bootstrapped (1000 repetitions) confidence intervals for the first column and one standard deviation for the second in parentheses

Table 14 Output multipliers controlling for monetary policy and excluding zero lower bound observations

	$\sum_{t=0}^{4} \Delta y_t$	$\frac{\sum_{t=0}^{4} \Delta y_t}{\sum_{t=0}^{4} (\Delta \tau_t - \Delta g_t)}$
Euro area		
Expansion		
EB	− 0.49	− 0.88
	(− 1.81; 0.50)	(− 2.51; 0.21)
TB	− 1.20	− 2.86
	(− 2.37; − 0.20)	(− 8.16; − 0.89)
Recession		
EB	− 0.60	− 1.22
	(− 1.14; − 0.09)	(− 2.37; − 0.54)
TB	− 0.69	− 1.57
	(− 1.39; − 0.04)	(− 3.20; − 0.63)
Non-Euro area		
Expansion		
EB	− 0.28	− 0.46
	(− 0.76; 0.17)	(− 1.13; − 0.04)
TB	− 1.28	− 3.23
	(− 2.30; − 0.47)	(− 8.04; − 1.15)
Recession		
EB	0.06	0.11
	(− 0.29; 0.40)	(− 0.35; 0.45)
TB	− 0.08	− 0.14
	(− 1.21; 0.94)	(− 1.91; 1.05)
Excluding zero lower bound		
Expansion		
EB	− 0.44	− 0.71
	(− 0.88; − 0.05)	(− 1.43; − 0.28)
TB	− 1.31	− 3.21
	(− 2.05; − 0.63)	(− 7.16; − 1.60)
Recession		
EB	− 0.34	− 0.71
	(− 0.65; − 0.06)	(− 1.25; − 0.30)
TB	− 0.88	− 1.99
	(− 1.60; − 0.28)	(− 4.11; − 1.00)

Note. The table reports the cumulated and cumulated as fraction of cumulated primary surplus multipliers estimated, respectively, in the model in which we control for monetary policy and when we exclude the observations at the zero lower bound. 90 percent bootstrapped (1000 repetitions) confidence intervals for the first column and one standard deviation for the second in parentheses

Table 15 Output multipliers under alternative specification of $F(s)$

	$\sum_{t=0}^{4} \Delta y_t$	$\frac{\sum_{t=0}^{4} \Delta y_t}{\sum_{t=0}^{4} (\Delta \tau_t - \Delta g_t)}$
Alternative specification for F(s)		
Expansion		
EB	0.19	0.27
	(−0.04; 0.40)	(0.07; 0.46)
TB	0.12	0.18
	(−0.35; 0.64)	(−0.29; 0.64)
Recession		
EB	−0.24	−0.41
	(−0.36; −0.14)	(−0.60; −0.28)
TB	−0.55	−0.94
	(−0.68; −0.43)	(−1.26; −0.73)

Note. The table reports the cumulated and cumulated as fraction of cumulated primary surplus multipliers estimated in the model in which we adopt the alternative specification of $F(s)$ including current output growth. 90 percent bootstrapped (1000 repetitions) confidence intervals for the first column and one standard deviation for the second in parentheses

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