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

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First draft: April 2016
Revised: April 2017

Abstract

Using data from 16 OECD countries from 1981 to 2014 we specify a model that determines the output effect of fiscal adjustments as a function of the composition of the adjustment and the state of the cycle. 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 spending cuts are consistently much less costly than those based upon tax increases. Our results are not explained by different reactions of monetary policy. However, 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 tax-based consolidations implemented during a recession.

*First presented at the 2016 IMF-ARC conference. We thank two anonymous referees, the editor Linda Tesar, and especially our discussant, Chris Erceg, for his very useful comments. We thank Giorgio Saponaro for his excellent research assistanship.

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1 Introduction

The empirical literature on the macroeconomic effects of fiscal policy finds that, under certain conditions, fiscal 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 non-linearity — for instance to the composition of a fiscal adjustment — effects that are in fact generated by the other. The analysis in this paper allows for both sources of non-linearity — we refer to them as the "when" and the "how" — to operate simultaneously, thus avoiding the risk mentioned above. In what follows we focus only on fiscal contractions, so we have nothing to say about expansionary fiscal policies.

We find that the difference between the effects of fiscal adjustments mostly based on raising taxes and those mostly based on cutting spending are independent of the state of the economy when the fiscal adjustment is implemented. When we allow for the state of the economy to vary in response to a simulated fiscal stabilization program, adjustments based upon spending cuts appear to be much less costly in terms of short run output losses — such losses are in fact on average close to zero — than those based upon tax increases, which instead are associated with large and prolonged recessions, and this is true regardless of whether the adjustment starts in a recession or not. In our baseline results we allow the state of the economy to change endogenously after the start of a fiscal stabilization program. If instead, as sometimes done in the literature, we assume that the state of the economy does not change following a the start of a fiscal adjustment, then the effect of fiscal policy on output growth vary as a function of the initial state of the business cycle. We will discuss below the details of these two different approaches. In any case, fiscal adjustments are costly during recessions and they are not during booms, but the evidence on the heterogeneity between expenditure based and taxation based adjustments is confirmed.

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 tax-based consolidations implemented during a recession. This finding could shed light on the recessionary effects of European "austerity", which was implemented inside a currency union and, for the countries mostly hardly hit in terms of growth (Italy and Portugal), was mostly tax-based (See Alesina et al. 2017).

Whether fiscal multipliers depend on the state of the economy is a question that has received much attention in recent years. The idea echoes 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 labour 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 (2012, 2013, AG in what follows), starting from the model of taxes, government spending and output estimated and simulated by Blanchard and Perotti 2002, allow for the effects of shifts in fiscal policy to differ depending on whether they are introduced during an expansion or a recession and adopt a more refined measure of unanticipated shocks to fiscal policy. In particular they use information from quarterly forecasts of fiscal and aggregate variables from the University of Michigan's RSQE macroeconometric model, Survey of Professional Forecasters (SPF) and the forecasts prepared by the staff of the Federal Reserve Board (FRB) for the meetings of the Federal Open Market Committee (FOMC) to purge fiscal variables of "innovations" that were predicted by professional forecasters. They find large differences in the size of spending multipliers in 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 results in a small non-Keynesian expansionary effect in recession, while the effect is small but contractionary in expansion. The findings in AG, which are originally obtained using U.S. data and found robust when an international panel is used, refer both to fiscal expansion and fiscal contractions: they are thus different from those presented in this paper that only studies fiscal consolidations.

The results obtained by AG hinge on the assumption that the state of the economy is held constant for at least the 20 quarters over which multipliers are computed. Ramey and Zubairy (2017) have argued 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 does not keep the state of the economy constant throughout the simulation [1]. Using quarterly U.S. 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 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 the 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.[2] Caggiano et al 2015 also allow for a feedback from shifts in fiscal policy to the probability of the economy being in an expansion or a recession, 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.

Ramey and Zubairy (2017) consider a further potentially important source of non-linearity: whether interest rates are near the zero-lower bound.[3] These

[1] Although, as we shall see, their approach does not make it endogenous with respect to the fiscal adjustment. They also consider a different measure of slack, related to unemployment rather than to output growth as in AB.

[2] Two related papers which use Canadian data (Owyang, Ramey and Zubairy 2013 and Ramey and Zubairy 2015) had found higher multipliers in high unemployment states. Rivisitng 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.

[3] 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, Eichenbaum and Rebelo 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. Erceg and Lindé 2013 investigate the effects of a spending-based *vs* labor tax-based fiscal consolidation in a two country DSGE model. They find that the effects depend on the degree of monetary

results are more mixed than those comparing multipliers in expansions and contractions away from the ZLB. Multipliers are still generally low, but in a few specifications they are as high as 1.5. Wataru, Miyamoto and Sergeyev 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.

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, Favero and Giavazzi (2015a) 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 for multipliers to be different depending on the state of the economy.[4] As mentioned above, the contribution of this paper is to allow for both sources of non-linearity — the "when" and the "how" — to operate simultaneously.

This paper is organized as follows. We start, in Section 2. studying non-linearities related to the "how". Here and throughout the paper, we analyze the effects of shifts in fiscal variables not studying isolated fiscal "shocks" but multi-year fiscal plans. 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. In Section 3 we study non-linearities related to the "when". In Section 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 analysis. Section 6 concludes.

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, the 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.

[4] See also the references cited therein.

2 Fiscal multipliers and the composition of fiscal adjustments

A first source of non-linearity 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 fiscal 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, United Kingdom, United States) 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 et al 2011 and extended by Alesina, Favero and Giavazzi (2015a). The fiscal consolidation measures in the Devries et al dataset are selected reading the records available in official documents to identify the size, timing and principal motivation for each fiscal action. They are “exogenous” because their adoption is not correlated with the economic cycle but rather (i) they are geared towards reducing an inherited budget deficit or are meant to correct its long run trend, e.g. increase in pension outlays induced by population aging, or (ii) are motivated by reasons which are independent of the state of the business cycle, excluding adjustments motivated by short-run counter-cyclical concerns. These consolidations measures are either tax increases or spending cuts and cover the period 1978-2009. Beyond extending the sample of consolidation measures identifying those implemented between 2010 and 2014, we have collected additional information on every fiscal measure, including those selected before 2009. This was necessary, as we shall see in the next Section, 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. Our database contains the details of each policy measure, e.g. “rise in VAT rate by 2% ”, “reduction in tax relief”, “reduction of childbirth grant”, “cut in public employees salaries”. These fiscal actions are then measured in terms of their impact (or expected impact for announced measures) on total revenues and expenditure scaled by the level of the GDP the year before they are announced[5]

2.2 Fiscal plans

As mentioned in the Introduction (and as first introduced in the literature in Alesina, Favero and Giavazzi (2015a)) 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 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 that 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 differed to subsequent years (measures "announced") and measures that were implemented in a given year but had previously been announced, that is were part of legislation adopted in previous years. We also distinguish between measures

[5]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

on the tax and on the spending side of the budget (interest payments are excluded and transfers are included among spending).

Consider a fiscal plan coming into effect at the beginning of year t. A plan 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 and *(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 two years, the current year t and the next year $t+1$ ($j=1$), 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 into its three components. The second equation explains that a fiscal correction consists of changes in taxes and in expenditures, 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 is an assumption violated in the data. Interestingly, different plans (for instance plans mostly based on tax hikes and plans mostly based on expenditure cuts) feature different correlations between announced measures and measures immediately implemented. 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.

It often happens that fiscal plans are revised along the way: in that case, we classify a modification of 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 et al 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 simply selected reading official documents) is 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 shows — with reference to the fiscal correction implemented in Belgium between 1992 and 1994 — on the left-hand side the exogenous fiscal "shocks" identified by DeVries et al and then used in Guajardo et al 2014 and, on the right-hand side, the plan we reconstructed. DeVries et al overlook fiscal announcements and construct the "fiscal shocks" whose effects they analyze (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 one-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 columns of Table 1. For instance, considering the row for 1992, $\widetilde{e_{i,t}} = 1.79$ and $e^u_{i,t} + e^a_{t,t-1,t} = 1.85$. The two corrections are not identical because shifts in fiscal variables are measured in billions of the domestic currency and then scaled using the GDP of the previous period. We use the latest available GDP series which sometimes may have been revised since the time DeVries et al accessed the data.[6] The same holds for the following years and for the two sub-components: for instance, remaining on row one, $\widetilde{\tau_{i,t}} = 0.99$ and $\tau^u_{i,t} + \tau^a_{t,t-1,t} = 1.03$. Components entering our fiscal plans appear on the right-hand-side columns of Table 1. Notice that, differently from the DeVries et al "shocks", our plans also include announcements of future shifts in fiscal variables.

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. Plans for which 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 is labelled *TB*; similarly, spending-based plans *EB* are those where the largest component of the fiscal correction consists of expenditure cuts. Note that the labelling 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 amount of fiscal correction actually implemented in 1992 relies more heavily on taxation. The labelling of a plan can

[6] 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. Results (available upon request) are essentially identical when scaling with current GDP.

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.

Table 1: Fiscal plan implemented by Belgium during 1992-1994

Year	$\widetilde{\tau_{i,t}}$	$\widetilde{g_{i,t}}$	$\widetilde{e_{i,t}}$	$e^u_{i,t}+e^a_{t,t-1,t}$	$\tau^u_{i,t}$	$\tau^a_{i,t-1,t}$	$\tau^a_{i,t,t+1}$	$g^u_{i,t}$	$g^a_{i,t-1,t}$	$g^a_{i,t,t+1}$	Label
1992	0.99	0.80	1.79	1.85	1.03	0	0.05	0.82	0	0.42	EB
1993	0.43	0.49	0.92	0.99	0.40	0.05	0.55	0.12	0.42	0.28	TB
1994	0.55	0.60	1.15	1.21	0	0.55	0	0.38	0.28	0	EB

for each year t, plans are labelled following this convention

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

$$then\ TB_{i,t} = 1\ and\ \ EB_{i,t} = 0,\ otherwise\ TB_{i,t} = 0\ \ and\ \ EB_{i,t} = 1$$

Descriptive statistics on the main characteristic of the fiscal plans we analyze are in Section 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 can derive impulse responses that describe the output effect of given plan. To do this we need a model for simulating plans. We specify, for our panel of countries, a trivariate VAR for 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. Each equation in this VAR is augmented by including the three narratively identified components of a fiscal plan and allowing for a different effect of TB and EB plans. The model is then closed by a set of auxiliary equations that track two sets of relationships: those between the announced and the unanticipated components of plans, and those between shifts in taxes and in spending. Auxiliary regressions are crucial to be able to simulate plans. In the data unexpected shifts in fiscal variables do not happen in isolation, but are typically accompanied by announcements of future shifts. These auxiliary equations allow to simulate the average plan estimated in the data in the sense that when an unanticipated component is simulated then the announced component moves 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 used to estimate fiscal multipliers. What we do is move taxes and spending according to what we observe on average in the EB or TB consolidations present in our sample. Finally, impulse responses are computed 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. Using the parameters estimated in the auxiliary equations, we calibrate the shifts in fiscal variables so that the size of the adjustment plan whose effects we simulate is one per cent of GDP.

This empirical strategy requires a few comments. First, our VAR is parsimonious in terms of included variables: only three, output growth and the change in revenues and spending in % of GDP. This however does not affect the identification of the exogenous fiscal measures which enter our plans because these are not derived from VAR innovations but are directly observed. Of course, the effects that we obtain might depend also on the effect plans have on variables that we omit from the VAR. This however will not affect the measurement of the final effect but will only prevent its categorization into different transmission channels. (This is the reason why we shall devote a section of the paper to the investigation of the importance of monetary policy in determining the fiscal multiplier that we obtain in our empirical analysis.)

Second, specifying the VAR in first differences, our simulations measure the response to "permanent" fiscal adjustments. There is only one qualification: in principle a plan can have a temporary effect even if the VAR is specified in differences. The condition for this to happen is that the announced part of the plan exactly cancels the part currently implemented. This does not happen in our data as the future and current components of plans are mildly positively correlated, meaning not only that on average fiscal plans are permanent, but also that fiscal consolidation measures get reinforced over time. It is important to remark that the effect of transient tax hikes and spending cuts could be very different. Standard New Keynesian models imply that the effects of permanent changes are quite different from temporary changes; permanent tax hikes have much more contractionary effects than transient ones, and permanent spending cuts are much less contractionary than transient ones (see Erceg Lindè 2013, Alesina et al. 2017).

Third, our model explicitly 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, and 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 paragraph.

2.4 The exogeneity of fiscal plans

The fact that some narratively identified fiscal adjustments are predictable, either by their own past or by past economic developments, has been considered by some authors (Hernandez de Cos and Moral-Benito 2016, Jorda and Taylor 2013) a threat to their exogeneity. Here we explain why this is not the case.

Assume you overlook announcements and plans and consider instead $\widetilde{e_{i,t}} = e^u_{i,t} + e^a_{i,t-1,t}$, the fiscal "shocks" analyzed by Devries et al (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

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

Finding $Cov\left(\widetilde{e_{i,t}}, \widetilde{e_{i,t-1}}\right) \neq 0$ 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 $\widetilde{e_{i,t}}$.

Predictability of $\widetilde{e_{i,t}}$ by past economic variables raises a separate issue. Hernandez 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^a_t 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: $e^u_t = \beta_1 I^a_t + \varepsilon_t$ and $\sum_j e^a_{t,t+j} = \beta_2 I^a_t + \eta_t$. 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 adjustment, not its timing.

Table 2: Time vs Size

	β_1	β_2
	1.0245***	0.6945***
	(0.0437)	(0.0413)
R^2	0.4236	0.2719
$\# \; of \; obs$	534	534

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.

Having said that, even considering the total narrative adjustments (as opposed to the zero/one dummy), some evidence of predictability of $e^u_{i,t}$, mainly on the basis of past government revenues and expenditures, remains. The non predictability of corrections on the basis of output growth is documented by Alesina et al. (2017) who verify that GDP does not Granger-cause the narrative fiscal consolidations shocks, according to the procedure by Toda and Taku (1995) which shows no Granger causality on a panel VAR with one lag, and 10% Granger causality on a panel with two lags[7]. The evidence of predictability on the basis of the past components of the deficits is a by-product of the narrative identification procedure that selects adjustments driven by past deficits. This is not a problem at the estimation stage because consistent estimates of fiscal multipliers require that innovations in output growth and the $e^u_{i,t}$ are not correlated, an assumption which is not ruled out by predictability from past information. When this condition is satisfied, the fact that the $e^u_{i,t}$ can be predicted based on past variables is irrelevant for the consistency of the estimated multipliers (see Appendix 1). Simulation instead could be a problem: you think you are simulating an unpredictable shift in fiscal policy, while it is not. To address this potential problem we analyze fiscal plans within a panel VAR that includes three variables: output growth and the change in revenues e spending in % of GDP. The estimated

[7]Note that our sample of 16 OECD countries differs from the sample of 17 OECD countries considered by DeVries et al. We have dropped the Netherlands, which is the only country for which the narrative identified fiscal adjustments can be predicted by past output growth. This is not suprising 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)

coefficients on the narrative adjustments in this VAR (see Appendix 1) measure the effect on output growth of the component of such adjustments that is orthogonal to lagged included variables. The estimated multipliers are thus not affected by the observed predictability.

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. In Alesina et al 2017 we investigate the importance of this assumption distinguishing between two categories of spending: transfers and current spending. We expect transfer-based plans, that often imply changes in social security legislation, to be less easily reversed, and thus more credible. Future research should investigate this issue more precisely [8]

3 Fiscal multipliers and the state of the cycle

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

This source of non-linearity can be identified by separating fiscal consolidations initiated during an economic expansion from those that started during recessions. 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 this reason we use an indicator of the state of the economy that can be used in an dynamic empirical model to allow for different dynamics in the state of recession and expansion.

To describe the state of the economy Auerbach and Gorodnichenko 2012, 2013 have suggested using a logistic function $F(s_{i,t})$ (where the index i refers to the country), which smooths the distribution of $\Delta y_{i,t-j}$ and transforms it into a variable ranging between 0 and 1. This allows for the transition between states of the economy to happen smoothly with $F(s_{i,t})$ being the weight given to recessions and $1 - F(s_{i,t})$ the weight given to expansions. Using, as a predictor of the state at time t, the weighted average of output growth over the previous two years, $F(s_{i,t})$ is

[8] Credibility of fiscal consolidations is discussed in Lemoine and Lindé (2014) and Corsetti et al. (2012).

$$
\begin{aligned}
F(s_{i,t}) &= \frac{\exp(-\gamma_i s_{i,t})}{1+\exp(-\gamma_i s_{i,t})}, \qquad \gamma_i > 0, \\
s_{i,t} &= \left(\mu_{i,t} - E\left(\mu_{i,t}\right)\right)/\sigma\left(\mu_{i,t}\right) \\
\mu_{i,t} &= \frac{\Delta y_{i,t-1} + \Delta y_{i,t-2}}{2}
\end{aligned}
$$

where $\mu_{i,t}$ is the moving average (and $s_{i,t}$ its standardized version) of output growth during the previous two years and γ_i are the country-specific parameters of the logistic function. For comparison with Auerbach and Gorodnichenko 2012 2013, 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, that is the percentage of years in which growth is negative over the sample, which consists of yearly data from 1979 to 2014. In other words, we calibrate γ_i so that country i spends x_i per cent of time in a recessionary regime — that is, $Pr(F(s_{i,t}) > 0.8) = x_i$, 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[9] .

For example, since for the US this number is 17%, in order to have $Pr(F(s_{US,t}) > 0.8) = 0.17$, we need to set $\gamma_{US} = 1.56$. This frequency of recession years for the US is also consistent with the NBER Dating Committee for a longer sample, extending back to 1946, which is of about 20%.[10] This choice allows us to use the same criterion for all countries in the sample, as most of them do not have Dating Committees. 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 $Pr(F(s_{IT,t}) > 0.8) = 0.22$. 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, Figure 1 compares the dynamics of $F(s)$ — the blue line — with actual recessions (defined as years of negative per capita output growth and denoted by the shaded areas) in the countries of our sample[11].

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

[10] 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.

[11] With $F(s_{i,t})$ we refer to the economic conditions prevailing at the beginning of the period in which the consolidation is implemented. Consistently with the way we constructed our indicator, in Figure 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: Calibration of γ_i

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

It is important to note that our state of the economy indicator $F(s_{i,t})$ is a function of lagged output growth. This indicator is better intepreted as an expected probability of recession at time t, given the information on GDP growth available at time $t-1$. This choice has advantages and costs. The main advantage 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, thus allowing the state of the economy to react to fiscal shocks. The impulse response function describing the response of $F(s_{i,t})$ to a fiscal adjustment can then be explicitly computed. Once this is done, the impulse response of all variables in the VAR can reflect *both* the difference across states (expansion and recession) and the evolving probability of (expected) recession. If instead $s_{i,t}$ was a function of *current* GDP growth (as in AG 2012), $F(s_{i,t})$ and $\Delta y_{i,t}$ would be simultaneous. This would prevent us from allowing the state of the economy to respond to fiscal shocks: it should be kept unchanged over the entire horizon of the impulse response. Holding the regime constant creates an asymmetry between the stage of estimation of the model (in which the regime is endogenous and time varying) and the stage of simulation of the model in which the regime is kept unchanged. This asymmetry, which in principle could be considered as legitimate, might have relevant consequences for the computed impulse responses. 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 one year.

The costs of using a lagging indicator of the business cycle emerges when fiscal corrections are able to affect contemporaneously the state of the economy. Suppose that a fiscal correction at time t, implemented in recession, is able to change the state of the economy at impact. In this case the correction would be wrongly classified, based on the lagged indicator, as hitting the economy in a high probability of recession regime. This scenario is unlikely in the case of our identified fiscal corrections because our narrative identification scheme excludes the fiscal corrections driven by the cycle. Indeed, for a fiscal correction implemented in a recession year 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, as we let the state of the economy respond endogenously to the fiscal correction, even if our procedure induced mislabelling of the regime, this would be only temporary.

Weighing these pros and cons we decided, in our main results, to opt for a 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.

4 Allowing for the "When" and the "How" to be studied simultaneously

4.1 A first look at the data

Fiscal adjustment plans for the 16 countries in our sample are constructed as described in Section 2 and shown in Appendix 3. Tables 4 through 7 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 178 plans and 225 episodes, that is years during which a fiscal consolidation is under way (a plan typically lasts more than one 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% or more of the total adjustment and the same holds for EB plans. The cases in which plans are labelled as EB or TB in the presence of a marginally dominant component (*e.g.* the spending share of EB plans and the tax share of TB ones less than 55%) are rare as shown in the last column of Table 5.

Table 7 allows to analyze the relative frequency of EB and TB plans and their relationship with the cycle. EB plans are more frequent than TB plans

(118 vs 60: we exclude observations where the cycle is not defined, such as Germany before 1993). The relative frequency of TB and EB plans when the probability of the recession is high ($F(s_{i,t}) > 0.8$) or low ($F(s_{i,t}) < 0.2$) is not significantly different from the unconditional one. In other words, it is not the case that EB adjustments occur more frequently than TB 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 that hold in the full sample. This is important because it says that, for example, it is not the case that TB plans are adopted more often during a recession and that this is the reason why they appear to be more recessionary.

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 61 out of 99 years in which $F(s_{i,t}) > 0.8$, while only in 14 over 101 years in which $F(s_{i,t}) < 0.2$. This however does not necessarily imply causality from the state of the cycle to the narratively identified fiscal adjustments, for two reasons. First, as already discussed, there are two sources of identification of our fiscal corrections: the timing and the intensity. The information reported in Table 7 is limited to the timing. In other words, even if agents knew that the government is more likely to introduce fiscal consolidation measures during recessions they cannot predict the size of the consolidation plan, as this varies a lot in our sample. Second, the fiscal actions we consider are measures exclusively aimed at increasing the primary surplus (being driven by past deficits) and their most likely output effect is contractionary, making a causal link between past recession and current measures counterintuitive. Endogeneity in our case would require that governments cut expenditure or increase taxation because the economy is in recession. The evidence reported in Table 7 would be much more worrisome if our exogenous fiscal measures were to contain some expansionary policies. Lastly, consider that fiscal adjustments introduced when output is growing strongly, motivated by the objective of cooling down output growth, are excluded from our sample, as, following the narrative approach we selected fiscal corrections that are not motivated by the state of the cycle.

In the light of this descriptive evidence it is important to analyze the impact of a fiscal consolidation plan taking into account *both* its composition (EB vs TB) and the state of the cycle (recession vs expansion) when it is introduced. Indeed, if EB or TB consolidations are relatively more frequent in expansions or recessions, by considering separately the "when", that is the state of the cycle, and the "how", that is the type of plan, one may find spurious results.

Finally, Table 7 shows the length and the size of plans. Most plans have a two year horizon and, on average, *EB* plans last longer than *TB* ones. The last three columns of Table 8 show 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 1.82% 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.

Table 4: Fiscal Adjustment Plans

	TB	EB		TB	EB
AUS	3	4	FRA	3	7
AUT	2	5	GBR	4	6
BEL	4	11	IRL	6	8
CAN	3	16	ITA	6	12
DEU	5	9	JPN	3	5
DNK	3	5	PRT	4	7
ESP	8	7	SWE	0	5
FIN	2	7	USA	4	4
Total TB:	60		Total EB:	118	

Table 5: The Composition of Fiscal Adjustments

	Share of Main Component			
Type of Plan	≥ 0.75	< 0.75	< 0.65	< 0.55
TB (60 plans)	30	30	19	9
EB (118 plans)	57	61	34	7
Total Plans: 178		Total Episodes: 225		

Table 6: Fiscal Adjustments and the State of the Economy

	$F(s_{i,t})$			
Type of Plan	< 0.2	< 0.5	≥ 0.5	> 0.8
TB (58 plans)	3	19	39	22
EB (115 plans)	11	44	71	39
Years in Sample - (531)	101	288	243	99

Table 7: Plans' Size and Length

	Horizon of plans (in years)							Size of plans (%GDP)		
Type of Plan	1	2	3	4	5	6	Average	Total	Spending	Taxes
TB	17	20	7	8	7	1	2.52	1.82	0.49	1.20
EB	28	43	8	14	9	16	2.84	1.94	1.46	0.48
All Plans	45	63	15	22	16	17	2.73	1.82	1.13	0.69

4.2 A model with two sources of non-linearity

In this section we introduce a model that allows us to study, simultaneously, two non-linearities in the effect of fiscal policy: one related to the state of the cycle, the other to the nature of the adjustment. The model is a Smooth Transition Panel VAR with two states: recession and expansion, and a non-linearity 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 ($\Delta g_{i,t}$) .

$$
\begin{aligned}
\Delta y_{i,t} &= (1-F(s_{i,t}))A_1^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_1^R(L)\,\mathbf{z}_{i,t-1} + \\
&\begin{bmatrix} 1-F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} \mathbf{a}'\mathbf{e}_{i,t} & \mathbf{b}'\mathbf{e}_{i,t} \\ \mathbf{c}'\mathbf{e}_{i,t} & \mathbf{d}'\mathbf{e}_{i,t} \end{bmatrix} \begin{bmatrix} TB_{i,t} \\ EB_{i,t} \end{bmatrix} \\
&+\lambda_{1,i} + \chi_{1,t} + u_{1,i,t} \qquad (1) \\
\Delta g_{i,t} &= (1-F(s_{i,t}))A_2^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_2^R(L)\,\mathbf{z}_{i,t-1} + \\
&+\begin{bmatrix} 1-F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} \beta_{11} & \beta_{12} & \beta_{13} & \beta_{14} \\ \beta_{15} & \beta_{16} & \beta_{17} & \beta_{18} \end{bmatrix} \begin{bmatrix} g^u_{i,t} \\ g^a_{i,t-1,t} \\ \tau^u_{i,t} \\ \tau^a_{i,t-1,t} \end{bmatrix} + \lambda_{2,i} + \chi_{2,t} + u_{2,i,t} \\
\Delta \tau_{i,t} &= (1-F(s_{i,t}))A_3^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_3^R(L)\,\mathbf{z}_{i,t-1} + \\
&+\begin{bmatrix} 1-F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} \beta_{21} & \beta_{22} & \beta_{23} & \beta_{24} \\ \beta_{25} & \beta_{26} & \beta_{27} & \beta_{28} \end{bmatrix} \begin{bmatrix} g^u_{i,t} \\ g^a_{i,t-1,t} \\ \tau^u_{i,t} \\ \tau^a_{i,t-1,t} \end{bmatrix} + \lambda_{3,i} + \chi_{3,t} + u_{3,i,t}
\end{aligned}
$$

where $\mathbf{z}_t : [\Delta y_t, \Delta g_t, \Delta \tau_t]$.

The narratively identified exogenous fiscal measures enter the estimation in two ways. In the output growth equation they enter as shifts in $e_{i,t}$; these are then interacted with the type of consolidation, TB or EB. The variable $\mathbf{e}_{i,t}$, has three components $\left[\begin{array}{ccc} e^u_{i,t} & e^a_{i,t-j,t} & e^a_{i,t,t+j} \end{array}\right]$ because, as we discussed, shifts in fiscal variables can be unanticipated, announced or implementation of previously announced measures.

Differently from the output growth equation, in the two equations for $\Delta g_{i,t}$ and $\Delta \tau_{i,t}$ we explicitly allow for expenditure and revenue corrections to have different coefficients and we also allow only the part of the narratively identified fiscal correction which is implemented in period t to affect the growth rates of revenues and expenditures to the part of the narratively identified fiscal correction which is implemented in period t. Future announced corrections do not directly affect the dynamics of revenues and expenditures as their effect is not recorded in national accounts at time t. Finally, the model also includes unobservable VAR innovations u_t: these are uninteresting for our analysis, in the sense that we do not need to extract from them any structural shock.

Interacting the shifts in fiscal variables with the TB and EB dummies allows to decompose fiscal adjustments in two mutually exclusive components, which then allows their effects to be simulated separately. This would not be possible if $g_{i,t}$ and $\tau_{i,t}$ were directly included in the output growth equation because, as already observed, exogenous shifts in taxes and spending are correlated. If we were to include them directly, rather than through orthogonal plans, we could only simulate the "average" adjustment plan, that is a plan that reproduces the average correlation between changes in taxes and spending observed in the estimation sample. Thus we would no longer be able to study the heterogenous effect of fiscal adjustments based on their composition.

In the model non-linearities with respect to the state of the economy and with respect to the composition of a fiscal plan affect output growth both on impact and through the dynamic response of the economy to a consolidation plan. On impact, the possible non-linearities associated with a consolidation plan — both stemming from its composition and from the state of the economy — are described by the coefficient vectors a, b, c, d in the first equation of model (1). The statistical relevance of these asymmetries can be assessed testing the following restrictions:

(i) $\mathbf{a} = \mathbf{c}$, $\mathbf{b} = \mathbf{d}$, $\beta_{ij} = \beta_{ij+4}$ for $i = 1, 2$ and $j = 1, 2, 3, 4$; the only source of non-linearity 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 non-linearity in the contemporaneous effect of a plan arises from the state of the cycle;

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

We shall return to these tests in the Results section below. [12],[13]

Model (1) must thus be accompanied by a set of auxiliary equations describing the response of announcements to contemporaneous corrections and the relative weights of tax and spending measures within a plan. We allow both correlations to be different according to the type of plan, TB vs EB. In other words, we allow for plans to have a different inter-temporal and infra-temporal structure according to their type.[14] Thus we complete our

[12] Note that our impulse responses describe the response of the economy to plans rather than shocks. Fiscal plans contain announcements of future shifts in taxes and spending, in order to simulate the effect of a plan and build the relevant impulse responses to measure the output effect of fiscal adjustment plans we need to model the response of "news innovations" to "current innovations". Moreover, since fiscal plans include measures both on the tax side and on the spending side, we also need to estimate the contemporaneous correlation between these two components.

[13] Given the presence of non-linearities, impulse responses are constructed using the generalized method proposed by Koop et al 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 non-linear 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 44x48 (our sample is 1981-2014 and it contains 44 annual observations) containing all the residuals in our system and by resampling the rows of such matrix.

[14] Alternatively we could have allowed the intertemporal structure of plans to be country- rather than plan-specific (see Alesina, Favero and Giavazzi 2015a). 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.

model for simulation with the following auxiliary regressions

$$
\begin{aligned}
\tau^u_{i,t} &= \delta_0^{TB} e^u_{i,t} * TB_{i,t} + \delta_0^{EB} e^u_{i,t} * EB_{i,t} + \epsilon_{0,i,t} \\
g^u_{i,t} &= \vartheta_0^{TB} e^u_{i,t} * TB_{i,t} + \vartheta_0^{EB} e^u_{i,t} * EB_{i,t} + \upsilon_{0,i,t} \\
\tau^a_{i,t,t+j} &= \delta_j^{TB} e^u_{i,t} * TB_{i,t} + \delta_j^{EB} e^u_{i,t} * EB_{i,t} + \epsilon_{j,i,t} \quad j = 1, 2 \\
g^a_{i,t,t+j} &= \vartheta_j^{TB} e^u_{i,t} * TB_{i,t} + \vartheta_j^{EB} e^u_{i,t} * EB_{i,t} + \upsilon_{j,i,t} \quad j = 1, 2
\end{aligned}
$$

where the first two equations describe the average tax (δ) and spending (ϑ) share of EB and TB plans. The next two equations describe the relation between unexpected shifts and those announced for years $t+1$ and $t+2$, differentiating between EB and TB plans. (These auxiliary regressions allow us to construct the $e^a_{i,t,t+j} = \tau^a_{i,t,t+j} + g^a_{i,t,t+j}$ needed to compute impulse responses). Table 8 shows the estimated coefficients

Table 8: Estimated coefficients in the auxiliary equations

δ_0^{TB}	δ_1^{TB}	δ_2^{TB}	δ_0^{EB}	δ_1^{EB}	δ_2^{EB}
0.7823	0.1552	0.0170	0.3918	−0.0415	0.0072
(0.0175)	(0.0278)	(0.0099)	(0.0104)	(0.0165)	(0.0059)

ϑ_0^{TB}	ϑ_1^{TB}	ϑ_2^{TB}	ϑ_0^{EB}	ϑ_1^{EB}	ϑ_2^{EB}
0.2177	0.1290	0.0305	0.6082	0.1590	0.0364
(0.0175)	(0.0315)	(0.0152)	(0.0104)	(0.0187)	(0.0091)

Before discussing the results, it is useful to make a few observations.

- While the state of the economy, *i.e.* the probability of being in an expansion or a recession, is affected by fiscal policy and can change as a plan evolves, the nature of the regime (TB, EB) is known upon announcement of the plan and does not change unless the plan is amended.
- The effect of fiscal measures when they are announced can be different from their effect as a plan is implemented. In particular:
  - in the first equation of model (1), defining $\mathbf{a} = \left[\begin{array}{ccc} a_{11} & a_{12} & a_{13} \end{array}\right]$ (and similarly for the $\mathbf{b}, \mathbf{c}$ and $\mathbf{d}$ coefficient vectors) the effect of a fiscal measure is fully exhausted when the measure is announced — that is nothing more happens upon implementation — if $a_{11} =$

a_{13}, $b_{11} = b_{13}$, $c_{11} = c_{13}$, $d_{11} = d_{13}$ and $a_{12} = b_{12} = c_{12} = d_{12} = 0$. When these restrictions are not rejected, plans can be collapsed into shocks of the type $f_{i,t} = e^u_{i,t} + e^a_{i,t,t+j}$. This is the assumption made in Romer and Romer 2010;

  - symmetrically, the null that a measure is effective only when it is implemented can be tested imposing the following restrictions $a_{11} = a_{12}$, $b_{11} = b_{12}$, $c_{11} = c_{12}$, $d_{11} = d_{12}$ and $a_{13} = b_{13} = c_{13} = d_{13} = 0$. When these restrictions are not rejected plans can be collapsed into shocks using the alternative definition $f_{i,t} = e^u_{i,t} + e^a_{i,t-1,t}$. This is the assumption made in Guajardo et al 2014.

- 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 measures on total revenues and total spending allows to check the strength of the narratively identified instruments — a check which usually is not carried out in studies which use an MA representation to project output growth on a distributed lag of the narratively identified adjustments. For instance it allows you to verify if, following a positive shock to taxes, revenues indeed increase. Note that the percentage 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 ($\Delta g_{i,t}$) are affected by the growth rate of GDP and it would be very hard to extract from the VAR innovations the exogenous components of the "news innovation" and the "current innovations" entering fiscal adjustment plans. We do not face this problem here, as the components of our exogenous plans are identified outside the VAR using the narrative identification strategy. they are included in the specification rather than being extracted from the specification as in standard Structural VARs.

- 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 non-linearities of our statistical model.

5 Data and results

5.1 Data and summary statistics

Macro data are from the OECD: Appendix 4 provides details on their sources and on how we compute the variables used in the analysis. 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 (social security benefits, etc.) and other government outlays. In Alesina et al 2017 we have investigated whether multipliers for government transfers differ from those for other spending items finding very moderate differences.

5.2 Main Results

We first show the impulse responses from the general unrestricted model that allows for all non-linearities. The impulse responses of the variables included in the VAR and of the indicator $F(s)$, the probability of being in a recession regime, are presented in Figure 2. Dark blue and dark red lines show the responses of the variables in the case, respectively, of an EB plan and a TB plan *introduced at a time when the economy is in an expansionary state* (defined as $F(s) \simeq 0.2$); light blue and light red lines *starting from a recessionary state* (defined as $F(s) \simeq 0.8$). The response of the state indicator $F(s)$ is computed as the difference between its simulated values following a fiscal adjustment which starts in a recession (expansion) and its simulation in the absence of a fiscal adjustment, starting from the same regime.

The upper left hand panel of Figure 2 shows that the stronger non-linearity is that between *TB* (red) and *EB* (blue) 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 their impact is more different in expansion than in recession.

The difference between *EB* and *TB* consolidations starting in any given state of the economy is a strong feature of the data with multipliers comparable to those estimated in Alesina, Favero and Giavazzi (2015a) abstracting from the state of the economy. Panels 2 and 3 of Figure 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: expansion and recession. Importantly, 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 on average 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.

Panel 4 of Figure 2 shows the responses of $F(s)$: in all four cases a consolidation increases the probability of experiencing a recession (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 (cycle-down regime) the difference in $F(s)$ between Tax-based and Expenditure-based adjustments is not statistically significant at impact. However it becomes significant two years after the implementation of the shock, showing 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 9 report the tests of the hypotheses introduced in Section 4.2

(i) $\mathbf{a} = \mathbf{c}$, $\mathbf{b} = \mathbf{d}$, $\beta_{ij} = \beta_{ij+4}$ for $i = 1, 2$ and $j = 1, 2, 3, 4$; the only source of non-linearity 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 non-linearity in the contemporaneous effect of a plan arises from the state of the cycle;

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

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

Table 9 reports the observed value of the likelihood ratio tests along with their probability under the null derived on 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 the hypotheses are rejected at least at the 5% confidence level, regardless of the way we perform the tests. We can conclude that all non-linearities are statistically significant.

Table 9: Hypotheses' Tests

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

To illustrate the economic relevance of imposing (statistically rejected) restrictions on our model, we consider two polar cases: the case in which only the "when" determines the output response to fiscal adjustment and the case in which only the "how" is relevant.

The impulse responses reported in Figure 3 are based on a model in which we remove the non-linearity across types of plans (imposing $\mathbf{a} = \mathbf{b}$, $\mathbf{c} = \mathbf{d}$) while keeping that across states of the economy. We thus generate impulse-response functions to fiscal adjustment plans by allowing these responses to differ according to the state of the economy, which also responds endogenously to the fiscal policy shift. Looking at the first panel 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 interval 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 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 Ramey and Zubairy 2013, 2015, 2017. Of course this does not mean that the welfare effects are also similar: losing one per cent 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 panel shows that implementing a consolidation always increases the probability of being in a recession — slightly more so when the economy starts from an expansion rather than a recession.

Finally, in Figure 4 we keep only the non-linearity 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, Favero and Giavazzi (2015a). The strong similarity between the impulse responses reported here and those reported in our previous paper suggests that the effect of predictability of the adjustments, which is dealt within a VAR but not in an MA, is minor.

5.3 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 restricting the state of the cycle, that is of assuming that the state of the economy does not change following the shift in fiscal policy.

5.3.1 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) together. What we can ask, 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) in determining the asymmetries we found above, we perform two exercises. First, we split our data in two sub-samples: euro area countries (Austria, Belgium, France, Finland, Germany, Ireland, Italy, Portugal and Spain) from 1999 onwards and non euro-area countries (Australia, Denmark, UK, Japan, Sweden, U.S. 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 onwards. Model (1) is thus extended to

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

$$with\ Euro_{i,t} = 1\ if \begin{cases} country = AUT, BEL, DEU, ESP, FIN, FRA, IRL, ITA, PRT \\ year \geq 1999 \end{cases}$$

Figures 5 and 6 plot the impulse response functions from this model. The results appear to be similar regardless of the response of monetary policy. The only difference is that TB consolidations started during a recession appear to be more harmful when monetary policy is constrained. Note that the findings that monetary accommodation is not important for spending cuts might depend on the type of fiscal adjustment that we are considering. As we have already noted fiscal plans in our VAR capture permanent adjustments. Permanent adjustments to expenditure by their nature do not affect the natural real rate of interest[15]. The finding that the response of monetary policy appears to dampen the recessionary effects of tax-based consolidations implemented during a recession could help understand the recessionary effects of European "austerity", which was mostly tax based for the countries mostly hardly hit by recession (Italy and Portugal) and implemented within a currency union.

Overall, however, our results do not appear to be driven by a different response of monetary policy to TB or EB adjustments, or to consolidations implemented in recession or expansion. The heterogeneity between EB and TB adjustments is in fact particularly clear when monetary policy cannot respond. 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.

As a further robustness check, we study whether the response of the economy to consolidations implemented while monetary policy is at the zero

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

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 from our sample, *i.e.* we remove euro area countries in 2013 and 2014, the US from 2008 and Japan from 1996 onward.[16] The results of this exercise are presented in Figure 7. 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. Note that these findings might depend on the type of fiscal adjustment that we are considering. As we have already noted fiscal plans in our VAR capture permanent adjustments that by their nature do not affect the natural real rate.

5.3.2 The Specification of $F(s)$.

As already discussed, our smooth transition VAR describes the state of economic using a "backward-looking" moving average and allows for 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 $\mu_{i,t}$ as follows:

$$\begin{aligned} F(s_{i,t}) &= \frac{\exp(-\gamma_i s_{i,t})}{1+\exp(-\gamma_i s_{i,t})}, \qquad \gamma_i > 0, \\ s_{i,t} &= \left(\mu_{i,t} - E\left(\mu_{i,t}\right)\right) / \sigma\left(\mu_{i,t}\right) \\ \mu_{i,t} &= \frac{\Delta y_{i,t} + \Delta y_{i,t-1}}{2} \end{aligned}$$

We report in Figure 8 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.[17] Indeed, as previously discussed,

[16] 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 perfom 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.

[17] For this reason the lines plotting the behavior of F(s) following a TB and an EB plan perfectly overlap.

in this framework we 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 keep $F(s)$ fixed either at 0.2 (expansion) or 0.8 (recession) throughout the simulation and the two lines for EB based and TB based corrections corresponding to the same state exactly overlap.

These results confirm the findings, reported in Auerbach and Gorodnichenko 2012, 2013, of an asymmetric output response to EB adjustments. We also find a small, marginally significant, expansionary effect of EB adjustments in expansion and a small, though significant, recessionary effect EB adjustments in recession. The output effect of EB adjustments in the recession regime is smaller than that reported by AG. This result is consistent with our identification strategy that selects, using narrative methods, only fiscal stabilization episodes. We do not expect our impulse responses to be of the same size of those derived considering both fiscal contractions and fiscal expansions. Interestingly, TB adjustments too are only significant in recession. Importantly, in the recession regime, the only one in which both TB and EB adjustments have a significant effect on output, the asymmetric effect between EB and TB adjustment is still present with TB adjustment being more recessionary than EB adjustments.

6 Conclusions

Fiscal consolidations can differ along three dimensions: their composition (increases in taxes vs reductions in expenditures), the state of the business cycles (whether a consolidation starts in a recession or in a boom) and whether or not they occur at a 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 more robust across different specifications. On average Expenditure-based adjustments have consistently much lower costs than Tax-based ones; the cost of the former are close to zero. 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 non-linearity 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 towards 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.

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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

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

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, when $\hat{u}_{2t} = e_t^u - \hat{\gamma}_1 \Delta y_{t-1} + \hat{\gamma}_2 \Delta \tau_{t-1} + \hat{\gamma}_3 \Delta g_{t-1}$ then estimating 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 vs VAR's

The VAR model described in the text (*model* (1)) 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 non-linearities 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 non-linearities. 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.[18]

$$\mathbf{z}_{i,t} = A_1 \mathbf{z}_{i,t-1} + (1 - F(s_{i,t})) B_1 e_{i,t} + F(s_{i,t}) B_2 e_{i,t} + \mathbf{u}_{i,t} \qquad (2)$$

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

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

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

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[19]

$$\Delta y_{i,t+h} = \alpha_{i,h} + (1 - F(s_{i,t}))\beta_{h,1} e_{i,t} + F(s_{i,t})\beta_{h,2} e_{i,t} + +\Gamma_h \mathbf{z}_{i,t} + \epsilon_{i,t} \quad (3)$$

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 (3), 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 $\Delta y_{i,t+1}$

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

is in general different from

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

Note, in closing, that the cases in which the two representations coincide are very specific. Indeed, when (4) is the data generating process and (5) 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 (4) 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.

[19] This is the specification adopted by Auerbach and Gorodnichenko 2013 to estimate a regime-dependent impulse response.

Appendix 3: Fiscal plans

Table A1: Classification of fiscal adjustments

		Tax					Spend						
		T^u_t	$T^a_{t-1,t}$	$T^a_{t,t+1}$	$T^a_{t,t+2}$	$T^a_{t,t+3}$	g^u_t	$g^a_{t-1,t}$	$g^a_{t,t+1}$	$g^a_{t,t+2}$	$g^a_{t,t+3}$	TB	EB
AUS	1985	0	0	0	0	0	0.2671	0	0.2671	0	0	0	1
AUS	1986	0.1658	0	-0.318	-0.484	0	0.3724	0.2671	0.2911	-0.081	0	0	1
AUS	1987	0	-0.318	-0.484	0	0	0.3994	0.2911	0.3181	0	0	0	1
AUS	1988	0	-0.484	0	0	0	0	0.3181	0	0	0	0	1
AUS	1993	0	0	0.2662	0.2662	0	0	0	0	0	0	1	0
AUS	1994	0	0.2662	0.2662	0	0	0	0	0	0	0	1	0
AUS	1995	0.4912	0.2662	0.4912	0	0	0	0	0	0	0	1	0
AUS	1996	0.0823	0.4912	0.2009	0.124	0.0054	0.3101	0	0.5544	0.2103	-0.034	0	1
AUS	1997	-0.005	0.2009	0.2311	0.1129	0.0708	-0.018	0.5544	0.1838	-0.058	-0.051	1	0
AUS	1998	0	0.2311	0.1129	0.0708	0.0752	0	0.1838	-0.058	-0.051	-0.035	1	0
AUS	1999	0	0.1129	0.0708	0.0752	0	0	-0.058	-0.051	-0.035	0	1	0
AUT	1980	0.1219	0	0	0	0	0.721	0	0	0	0	0	1
AUT	1981	0.5295	0	0	0	0	1.1251	0	0	0	0	0	1
AUT	1984	1.4915	0	0	0	0	0.6392	0	0	0	0	1	0
AUT	1996	0.9087	0	0.7311	0	0	1.5778	0	0.9128	0	0	0	1
AUT	1997	0	0.7311	0	0	0	0	0.9128	0	0	0	0	1
AUT	2001	0.912	0	-0.017	0	0	0.2246	0	1.1128	0	0	0	1
AUT	2002	0	-0.017	0	0	0	0	1.1128	0	0	0	0	1
AUT	2011	0.4033	0	0.1994	0.0613	0.0919	0.3022	0	0.1705	0.0643	0.0698	1	0
AUT	2012	0.3557	0.1994	0.3447	0.0255	0.0162	0.1688	0.1705	0.3309	0.6036	0.508	0	1
AUT	2013	0	0.3447	0.0255	0.0162	0.081	0	0.3309	0.6036	0.508	0.3492	0	1
AUT	2014	0.0549	0.0255	0.1117	0.2295	0.0136	0.0409	0.6036	0.3899	0.2686	0.009	0	1
BEL	1982	0	0	0	0	0	1.7677	0	0	0	0	0	1
BEL	1983	0.6155	0	0	0	0	0.9683	0	0	0	0	0	1
BEL	1984	0.2994	0	0.8179	0	0	0.4402	0	1.0346	0	0	0	1
BEL	1985	0	0.8179	0	0	0	0	1.0346	0	0	0	0	1
BEL	1986	0	0	0.1089	0	0	0	0	1.9837	0	0	0	1
BEL	1987	0	0.1089	0	0	0	0.2787	1.9837	0	0	0	0	1
BEL	1990	0.3849	0	0	0	0	0.0924	0	0	0	0	1	0
BEL	1992	1.0255	0	0.0485	0	0	0.8245	0	0.4192	0	0	0	1
BEL	1993	0.3959	0.0485	0.5543	0	0	0.1188	0.4192	0.2771	0	0	1	0
BEL	1994	0	0.5543	0	0	0	0.3844	0.2771	0	0	0	0	1
BEL	1996	0.7449	0	-0.233	0	0	0.4655	0	-0.233	0	0	1	0
BEL	1997	0.3796	-0.233	0	0	0	0.4601	-0.233	0	0	0	0	1
BEL	2010	0.2145	0	0.2917	0	0	0.8298	0	0.0841	0	0	0	1
BEL	2011	0.2108	0.2917	0	0	0	0.254	0.0841	0	0	0	1	0
BEL	2012	0.8512	0	0.1106	0.1616	0	1.5808	0	0.527	0.6133	0	0	1
BEL	2013	0.5258	0.1106	0.1616	0	0	0.5503	0.527	0.6133	0	0	0	1
BEL	2014	0	0.1616	0	0	0	0	0.6133	0	0	0	0	1
CAN	1983	0	0	0.1917	0.3863	0.2641	0	0	0	0	0	1	0
CAN	1984	0	0.1917	0.3863	0.2641	0.0514	0	0	0	0	0	1	0
CAN	1985	0.1767	0.3863	0.8197	0.217	0	0.5156	0	0.2823	0.2197	0.2438	0	1
CAN	1986	0.2883	0.8197	0.4697	0.1032	0.017	0.1211	0.2823	0.2903	0.2771	0.2515	1	0
CAN	1987	0	0.4697	0.323	-0.266	0.0242	0	0.2903	0.2282	0.2324	0.2392	0	1
CAN	1988	0.0297	0.323	-0.246	0.0275	0.0994	0	0.2282	0.2324	0.2392	-0.003	0	1
CAN	1989	0.445	-0.246	0.5678	0.2301	0.0172	0.165	0.2324	0.3561	0.018	-5E-04	1	0
CAN	1990	-0.243	0.5678	0.2604	0.2493	0.065	0.2076	0.3561	0.2291	0.0963	0.0165	0	1
CAN	1991	0	0.2604	0.2493	0.065	0	0.1104	0.2291	0.2464	0.148	0.0312	0	1
CAN	1992	-0.058	0.2493	0.048	0.0427	0.014	0	0.2464	0.148	0.0312	0	0	1
CAN	1993	0	0.048	0.0427	0.014	0	0.237	0.148	0.1988	0.1442	0.0382	0	1
CAN	1994	0.0582	0.0427	0.1163	0.0393	0.0039	0.2216	0.1988	0.5501	0.3379	0.133	0	1
CAN	1995	0.0896	0.1163	0.1011	0.0303	0.0052	0.3687	0.5501	0.6517	0.3253	0.0662	0	1
CAN	1996	0.0032	0.1011	0.0313	0.0052	0	-0.082	0.6517	0.3944	0.0984	0	0	1
CAN	1997	-0.036	0.0313	-0.013	-0.018	-0.005	-0.014	0.3944	0.0807	-0.01	-0.002	0	1
CAN	2010	0.018	0	0.0091	0.0039	0.0041	0.0216	0	0.0615	0.1088	0.0754	0	1
CAN	2011	0.0108	0.0091	0.0296	0.0279	0.0069	0.0088	0.0615	0.1603	0.1502	0.1566	0	1
CAN	2012	0	0.0296	0.0279	0.0069	-0.002	0.0624	0.1603	0.2383	0.2671	0.0704	0	1
CAN	2013	0.013	0.0279	0.0346	0.0214	0.0097	0.0091	0.2383	0.2883	0.0778	0.0006	0	1
CAN	2014	0.0019	0.0346	0.0356	0.0162	0.0075	0.1279	0.2883	0.1193	-0.033	-0.061	0	1

Table A1: Classification of fiscal adjustments

		Tax					Spend					TB	EB
		T^u_t	$T^a_{t-1,t}$	$T^a_{t,t+1}$	$T^a_{t,t+2}$	$T^a_{t,t+3}$	g^u_t	$g^a_{t-1,t}$	$g^a_{t,t+1}$	$g^a_{t,t+2}$	$g^a_{t,t+3}$		
DEU	1982	0.6343	0	0	-0.354	0	0.7008	0	0	0	0	0	1
DEU	1983	0.3467	0	-0.354	0	0	0.6455	0	0	0	0	0	1
DEU	1984	0	-0.354	0	0	0	0.6729	0	0	0	0	0	1
DEU	1991	1.1776	0	0.4114	0.1189	0.0585	0.0421	0	0.1755	0.2047	0.1852	1	0
DEU	1992	0	0.4114	0.1189	0.0585	0	0	0.1755	0.2047	0.1852	0	1	0
DEU	1993	0	0.1189	0.0585	0.8445	0	0	0.2047	0.1852	0.1178	0	1	0
DEU	1994	0.0819	0.0585	0.9146	0	0	0.6579	0.1852	0.2611	0	0	0	1
DEU	1995	0	0.9146	0	0	0	0	0.2611	0	0	0	0	1
DEU	1997	0.5313	0	0	0	0	0.9935	0	-0.08	0	0	0	1
DEU	1998	0.1015	0	0	0	0	0	-0.08	0	0	0	1	0
DEU	1999	0	0	0.1313	0	0	0	0	0.5917	0	0	0	1
DEU	2000	0	0.1313	0	0	-0.381	0	0.5917	0	0	0	0	1
DEU	2003	1.4821	-0.381	-0.68	0	0	0	0	0	0	0	1	0
DEU	2004	0	-0.68	0	0	0	1.0532	0	0	0.3039	0	0	1
DEU	2005	0	0	0	0	0	0	0	0.3039	0	0	0	1
DEU	2006	0	0	0.4042	0	0	0	0.3039	0.5053	0	0	0	1
DEU	2007	0	0.4042	0	0	0	0	0.5053	0	0	0	0	1
DEU	2011	0.3299	0	-0.019	0	0	0.229	-0.122	0.1263	-0.122	0	1	0
DEU	2012	-0.074	-0.019	-0.193	0	0	0.5632	0.1263	-0.033	0	0	0	1
DEU	2013	0	-0.193	0	0	0	0	-0.033	0	0	0	0	1
DNK	1982	0	0	0.1144	0	0	0	0	0	0	0	1	0
DNK	1983	1.0015	0.1144	0	0	0	1.9029	0	1.2018	0	0	0	1
DNK	1984	-0.218	0	0.9084	0	0	0.763	1.2018	0.9084	0	0	0	1
DNK	1985	0	0.9084	0	0	0	0	0.9084	0	0	0	0	1
DNK	1994	0	0	0.0432	0	0	0	0	0	0	0	1	0
DNK	1995	0	0.0432	0	0	0	0.1208	0	0	0	0	0	1
DNK	2009	0	0	0	0.0975	0	0	0	0	0	0	1	0
DNK	2010	0	0	0.3889	0.0971	0.4872	0	0	0.5827	0.5827	0.5827	0	1
DNK	2011	0	0.3889	0.0971	0.4872	0	0	0.5827	0.5827	0.5827	0	0	1
DNK	2012	0.1955	0.0971	0.585	0	0	0	0.5827	0.5827	0	0	0	1
DNK	2013	0	0.585	0	0	0	0	0.5827	0	0	0	0	1
ESP	1983	1.7616	0	0	0	0	0	0	0	0	0	1	0
ESP	1984	0.409	0	0	0	0	0.8179	0	0	0	0	0	1
ESP	1989	1.0791	0	-0.309	0	0	0.0915	0	0	0	0	1	0
ESP	1990	0	-0.309	0	0	0	0	0	0	0	0	1	0
ESP	1992	0.8245	-0.603	0.4581	0	0	0.3665	0	0.2884	0	0	1	0
ESP	1993	0.2741	0.4581	0	0	0	0	0.2884	0	0	0	1	0
ESP	1994	0	0	0	0	0	1.5526	0	0	0	0	0	1
ESP	1995	0	0	0	0	0	0.776	0	0	0	0	0	1
ESP	1996	0.1928	0	0	0	0	1.0602	0	0	0	0	0	1
ESP	1997	0.0907	0	0	0	0	1.3608	0	0	0	0	0	1
ESP	2009	0.2924	0	0	0	0	0	0	0	0	0	1	0
ESP	2010	0.4851	0	0	0	0	1.1695	0	0.5616	0	0	0	1
ESP	2011	0	0	0	0	0	0.9807	0.5616	0	0	0	0	1
ESP	2012	1.6662	0	0.8371	0	0	1.5005	0	0.4469	0.2684	0.2105	1	0
ESP	2013	2.0485	0.8371	0.5853	0.2926	0	-0.332	0.4469	0.2022	0.1337	0	1	0
ESP	2014	0.9068	0.5853	0.4389	-0.078	0	-0.028	0.2022	0.773	0	0	1	0
FIN	1992	0	0	0	0	0	0.8672	0	1.934	0	0	0	1
FIN	1993	0	0	0	0	0	1.6848	1.934	0	0	0	0	1
FIN	1994	1.6868	0	-0.706	0	0	1.7653	0	0	0	0	0	1
FIN	1995	0	-0.706	0	0	0	2.4088	0	1.6028	0	0	0	1
FIN	1996	0	0	-0.273	0	0	0	1.6028	0	0	0	0	1
FIN	1997	-0.478	-0.273	0	0	0	0.9888	0	0	0	0	0	1
FIN	2010	0	0	0.6463	0.1215	0	0	0	0	0	0	1	0
FIN	2011	0	0.6463	0.1215	0	0	0	0	0	0	0	1	0
FIN	2012	-0.054	0.1215	1.0331	0.0807	0.0172	0.2291	0	0.1945	0.2377	0.2581	1	0
FIN	2013	0	1.0331	0.0807	0.0172	0.2438	0	0.1945	0.2377	0.2581	0	1	0
FIN	2014	0	0.0807	0.2786	0.2438	0	0	0.2377	0.6962	0.0193	0.0755	0	1

Table A1: Classification of fiscal adjustments

		Tax					Spend					TB	EB
		τ^u_t	$\tau^a_{t-1,t}$	$\tau^a_{t,t+1}$	$\tau^a_{t,t+2}$	$\tau^a_{t,t+3}$	g^u_t	$g^a_{t-1,t}$	$g^a_{t,t+1}$	$g^a_{t,t+2}$	$g^a_{t,t+3}$		
FRA	1979	0.9588	0	0	0	0	0	0	0	0	0	1	0
FRA	1987	-0.265	-0.26	0	-0.194	0	0.7502	0	0	-0.005	0	0	1
FRA	1988	0	0	-0.194	0	0	0	0	-0.005	0	0	0	1
FRA	1989	0	-0.194	0	0	0	0	-0.005	0	0	0	0	1
FRA	1991	0.0864	0	-0.058	0	0	0.2188	0	0	0	0	0	1
FRA	1992	0	-0.058	0	0	0	0	0	0	0	0	0	1
FRA	1995	0.4007	0	0.5067	0	0	-0.118	0	0	0	0	1	0
FRA	1996	0.4162	0.5067	0.1033	0	0	0.4012	0	0.2103	0	0	1	0
FRA	1997	0.2905	0.1033	0	-0.097	-0.194	0	0.2103	0	0	0	0	1
FRA	1998	0	0	-0.097	-0.194	0	0	0	0	0	0	0	1
FRA	1999	0	-0.097	-0.194	0	0	0	0	0	0	0	0	1
FRA	2000	0	-0.194	0	0	0	0	0	0	0	0	0	1
FRA	2010	0	0	0	0	0	0	0	0.3558	0	0	0	1
FRA	2011	0.661	0	0.6119	0	0	0.5358	0.3558	0.5758	0.1052	0.0551	0	1
FRA	2012	0.5911	0.6119	0.4409	0.01	0	0.1215	0.5758	0.1325	0.0536	0.1502	1	0
FRA	2013	1.3422	0.4409	-0.182	0	0	0.5752	0.1325	0.4371	0.1502	0.3135	0	1
FRA	2014	0.1224	-0.182	-0.165	-0.349	-0.118	0.8582	0.4371	1.0823	1.0237	0.577	0	1
GBR	1979	-0.493	0	-0.164	0	0	0.739	0	0.2463	0	0	0	1
GBR	1980	0	-0.164	0	0	0	0	0.2463	0	0	0	0	1
GBR	1981	1.1107	0	0.3702	0	0	0.1234	0	0.0411	0	0	1	0
GBR	1982	0	0.3702	0	0	0	0	0.0411	0	0	0	1	0
GBR	1993	0	0	0.5205	0.1735	0	0	0	0	0	0	1	0
GBR	1994	0.177	0.5205	0.2325	0	0	0.1261	0	0.042	0	0	1	0
GBR	1995	0	0.2325	0	0	0	0	0.042	0	0	0	1	0
GBR	1996	0	0	0	0	0	0.3161	0	0.1054	0	0	0	1
GBR	1997	0.4633	0	0.3437	0.2398	0.0589	0.2278	0.1054	0.058	-0.006	0	1	0
GBR	1998	0	0.3437	0.2398	0.0589	0	0	0.058	-0.006	0	0	1	0
GBR	1999	0	0.2398	0.0589	0	0	0	-0.006	0	0	0	1	0
GBR	2010	0.1457	0	0.7011	0.3703	0.246	0.2629	0	0.2981	0.4054	0.4685	0	1
GBR	2011	0.0462	0.7011	0.3879	0.2898	0.1448	-0.004	0.2981	0.5252	0.7168	0.5982	0	1
GBR	2012	-0.079	0.3879	0.3135	0.2414	0.008	-0.011	0.5252	0.7216	0.6579	0.1829	0	1
GBR	2013	-0.049	0.3135	0.3108	0.1231	-0.043	0.0727	0.7216	0.6715	0.1608	0.0262	0	1
GBR	2014	-0.029	0.3108	0.1166	-0.101	-0.037	-0.011	0.6715	0.1754	0.1172	0.0454	0	1
IRL	1982	2.9483	0	0	0	0	0.3033	0	0	0	0	1	0
IRL	1983	2.6459	0	0	0	0	0.0669	0	0	0	0	1	0
IRL	1984	0.3127	0	0	0	0	0	0	0	0	0	1	0
IRL	1985	0.1316	0	0	0	0	0	0	0	0	0	1	0
IRL	1986	0.5607	0	0	0	0	0	0	0	0	0	1	0
IRL	1987	0.4188	0	0	0	0	1.1986	0	0	0	0	0	1
IRL	1988	0	0	0	0	0	2.0879	0	0	0	0	0	1
IRL	2008	0	0	0	0	0	0	0	0.2846	0	0	0	1
IRL	2009	2.8437	0	0.8922	0	0	1.2085	0.2846	0.8045	0	0	1	0
IRL	2010	0.0119	0.8922	0.0315	0	0	2.4086	0.8045	0.1105	0	0	0	1
IRL	2011	0.7932	0.0315	0.6245	0	0	2.5554	0.1105	0.5748	0	0	0	1
IRL	2012	0.6224	0.6245	0.1311	0	0	1.327	0.5748	0.3657	0	0	0	1
IRL	2013	0.6503	0.1311	0.3589	0	0	0.8829	0.3657	0.2606	0	0	0	1
IRL	2014	0.1917	0.3589	-0.034	0	0	0.7086	0.2606	0.0011	0	0	0	1
ITA	1991	1.7626	0	-1.062	0	0	0.9203	0	0	0	0	0	1
ITA	1992	2.5155	-1.062	-1.899	0	0	1.6204	0	0	0	0	0	1
ITA	1993	3.25	-1.899	-0.678	0	0	2.917	0	0	0	0	0	1
ITA	1994	0.2575	-0.678	0	0	0	1.5389	0	0	0	0	0	1
ITA	1995	2.2616	0	-1.515	0	0	1.6623	0	0.0565	0	0	0	1
ITA	1996	1.4769	-1.515	-0.395	0	0	1.063	0.0565	0	0	0	0	1
ITA	1997	1.2673	-0.395	-0.569	0	0	0.901	0	0	0	0	0	1
ITA	1998	0.6162	-0.569	0	0	0	0.567	0	0	0	0	0	1
ITA	2004	0.9018	0	-0.288	0	0	0.3449	0	0	0	0	1	0
ITA	2005	0.351	-0.288	0	0	0	0.8085	0	0	0	0	0	1
ITA	2006	0.5232	0	0	0	0	0.8841	0	0	0	0	0	1
ITA	2007	1.1981	0	0	0	0	-0.36	0	0	0	0	1	0
ITA	2009	0	0	0.1133	-0.023	-0.027	0	0	0.0075	0.0012	-6E-04	1	0

Table A1: Classification of fiscal adjustments

		Tax					Spend					TB	EB
		T^u_t	$T^a_{t-1,t}$	$T^a_{t,t+1}$	$T^a_{t,t+2}$	$T^a_{t,t+3}$	g^u_t	$g^a_{t-1,t}$	$g^a_{t,t+1}$	$g^a_{t,t+2}$	$g^a_{t,t+3}$		
ITA	2011	0.2226	0.1754	1.2884	0.7647	0.2124	0.2335	0.6763	0.6835	0.8533	0.1503	1	0
ITA	2012	0.9684	1.2884	0.7526	0.0931	0	0.3741	0.6835	1.2754	0.4812	0.0405	1	0
ITA	2013	0.3141	0.7526	0.231	0.0487	0.0032	0.0372	1.2754	0.4562	0.0092	-0.031	0	1
ITA	2014	-0.039	0.231	0.0806	0.268	0.0274	-0.11	0.4562	0.2956	-0.05	0.0107	1	0
JPN	1979	0.1207	0	0.1399	0.0383	0	0	0	0	0	0	1	0
JPN	1980	0.0901	0.1399	0.1027	0	0	0	0	0	0	0	1	0
JPN	1981	0.3337	0.1027	0.2384	0	0	0	0	0	0	0	1	0
JPN	1982	0.0771	0.2384	0.055	0	0	0.6842	0	0.0629	0	0	0	1
JPN	1983	0	0.055	0	0	0	0.2758	0.0629	0	0	0	0	1
JPN	1997	0.9816	0	0.3272	0	0	0.4395	0	0.1465	0	0	1	0
JPN	1998	0	0.3272	0	0	0	0	0.1465	0	0	0	1	0
JPN	2003	0	0	0	0	0	0.4884	0	0	0	0	0	1
JPN	2004	0.1804	0	0.0601	0	0	0.454	0	0	0	0	0	1
JPN	2005	0	0.0601	0	0	0	0.2207	0	0	0	0	0	1
JPN	2006	0.4763	0	0.1588	0	0	0.2735	0	0	0	0	1	0
JPN	2007	0	0.1588	0	0	0	0	0	0	0	0	1	0
PRT	1983	1.0304	0	0	0	0	0.7295	0	0	0	0	1	0
PRT	2000	0	0	0	0	0	0.4628	0	0	0	0	0	1
PRT	2002	1.0348	0	0	0	0	0.2587	0	0	0	0	1	0
PRT	2005	0.5316	0	0.4528	0	0	0.082	0	0.4765	0.9201	0	0	1
PRT	2006	0.5704	0.4528	0.4916	0	0	0.786	0.4765	1.3128	0	0	0	1
PRT	2007	0	0.4916	0	0	0	0.388	1.3128	0	0	0	0	1
PRT	2010	0.6052	0	1.3832	0	0	0.5091	0	1.3832	0	0	1	0
PRT	2011	0.4804	1.3832	0.8646	0.4804	0	0.538	1.3832	2.9782	1.345	0	0	1
PRT	2012	0.3886	0.8646	2.6174	0	0	0.7771	2.9782	2.1221	0	0	0	1
PRT	2013	0.3922	2.6174	-0.392	0	0	0.0981	2.1221	0	0	0	1	0
PRT	2014	0.5437	-0.392	0.0679	0	0	1.5213	0	-0.027	0	0	0	1
SWE	1984	0.2269	0	0	0	0	0.7312	0	0	0	0	0	1
SWE	1993	0.4046	0	0.1962	0	0	1.0176	0	0.7601	0	0	0	1
SWE	1994	0	0.1962	0.1961	0.1121	0.0841	0	0.7601	0.2942	0.1681	0.1261	0	1
SWE	1995	1.0645	0.1961	0.7208	0.5407	0.3607	1.5988	0.2942	1.0812	0.8111	0.5409	0	1
SWE	1996	0	0.7208	0.5407	0.3607	0	0	1.0812	0.9492	0.6789	0.1381	0	1
SWE	1997	0	0.5407	0.3607	0	0	0	0.9492	0.6789	0.1381	0	0	1
SWE	1998	0	0.3607	0	0	0	0	0.6789	0.1381	0	0	0	1
USA	1978	0.139	0	0	0.0815	0.8246	0	0	0	0	0	1	0
USA	1979	0	0	0.0815	0.8246	0	0	0	0	0	0	1	0
USA	1980	0	0.0815	0.8246	0	0	0	0	0	0	0	1	0
USA	1981	-0.311	0.8246	0	0	0	0	0	0	0	0	1	0
USA	1983	0	0	0	0.1913	0.1106	0	0	0	0	0	1	0
USA	1984	0	0	0.1913	0.1106	0	0	0	0	0	0	1	0
USA	1985	0	0.1913	0.1106	0	0.4395	0	0	0	0	0	1	0
USA	1986	0	0.1106	0	0.4395	0	0	0	0	0	0	1	0
USA	1987	0	0	0.2826	0	0	0	0	0	0	0	1	0
USA	1988	0.2382	0.2826	0	0	0	0.2731	0	0	0	0	1	0
USA	1990	0.2263	0	0.3005	0.1944	-0.004	0.0742	0	0.304	0.3252	0.3183	0	1
USA	1991	0	0.3005	0.1944	-0.004	0.0751	0	0.304	0.3252	0.3183	0.4808	0	1
USA	1992	0	0.1944	-0.004	0.0751	0.0265	0	0.3252	0.3183	0.4808	0.2384	0	1
USA	1993	0.0929	-0.004	0.4009	0.1764	0.0856	0.021	0.3183	0.5725	0.4036	0.3028	0	1
USA	1994	0	0.4009	0.1764	0.0856	0.0612	0	0.5725	0.4036	0.3028	0.341	0	1
USA	1995	0	0.1764	0.0856	0.0612	-0.034	0	0.4036	0.3028	0.341	0.237	0	1
USA	1996	0	0.0856	0.0612	-0.034	0	0	0.3028	0.341	0.237	0	0	1
USA	1997	0	0.0612	-0.034	0	0	0	0.341	0.237	0	0	0	1
USA	1998	0	-0.034	0	0	0	0	0.237	0	0	0	0	1
USA	2011	0	0	0	0	0	0.0368	0	0.142	0.1203	0.0785	0	1
USA	2012	0	0	0	0	0	0	0.142	0.1203	0.0785	0.0501	0	1
USA	2013	0.1732	0	0.1237	0	0	0.2642	0.1203	0.0785	0.0501	0.0434	0	1

Appendix 4: Data sources

Table 10: Macroeconomic 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. Inventment (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)

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

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

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

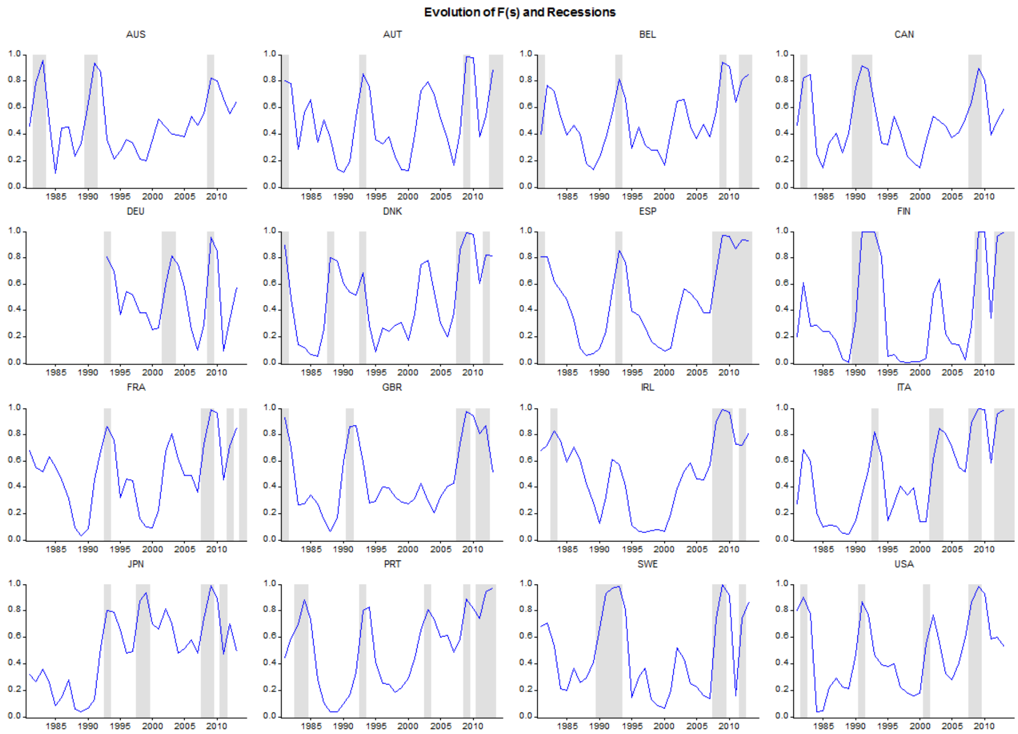

Figure 1: Evolution of $F(s)$ for the countries in our sample and years of recession (years negative growth per capita, shaded areas), 1981-2013.

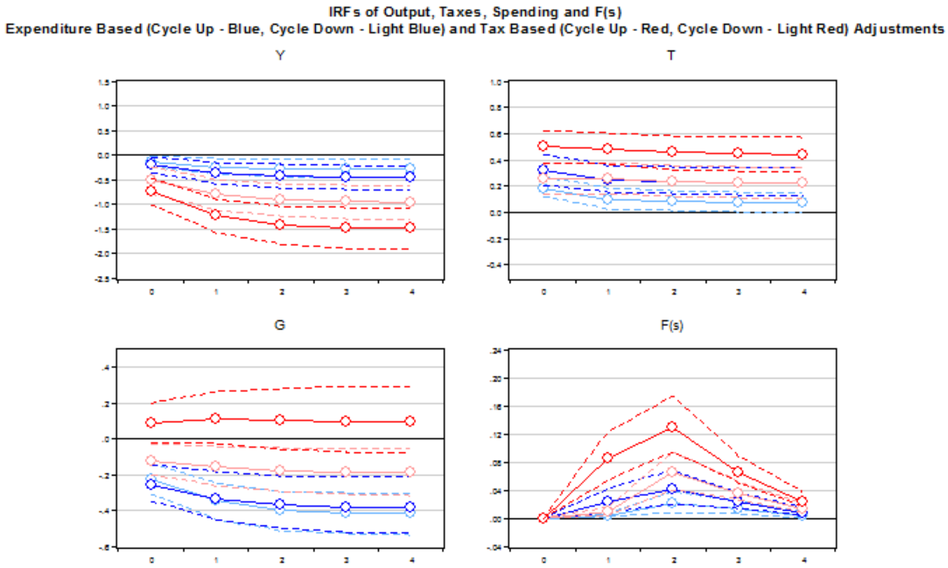

Figure 2: Allowing for heterogeneity between EB and TB plans and across states of the cycle.

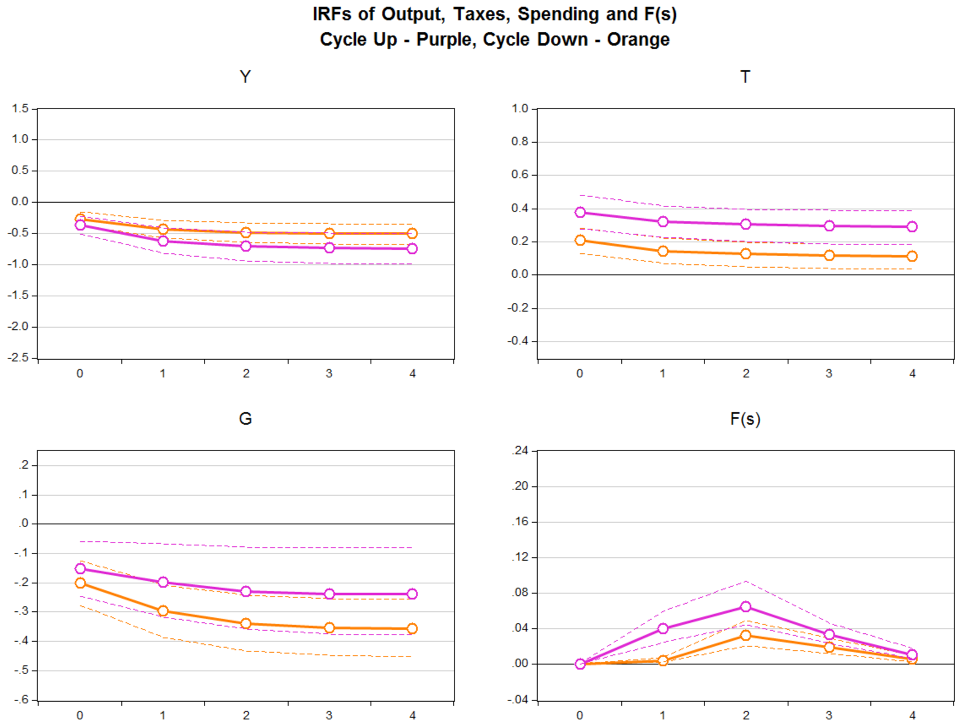

Figure 3: Allowing for heterogeneity across states of the cycle only.

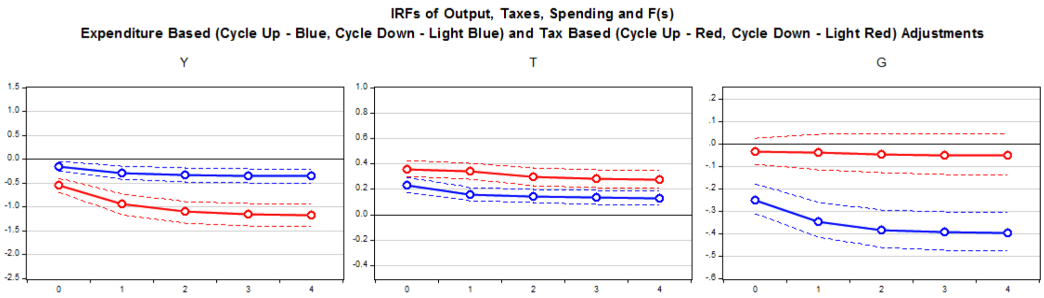

Figure 4: Allowing for heterogeneity only between EB and TB plans.

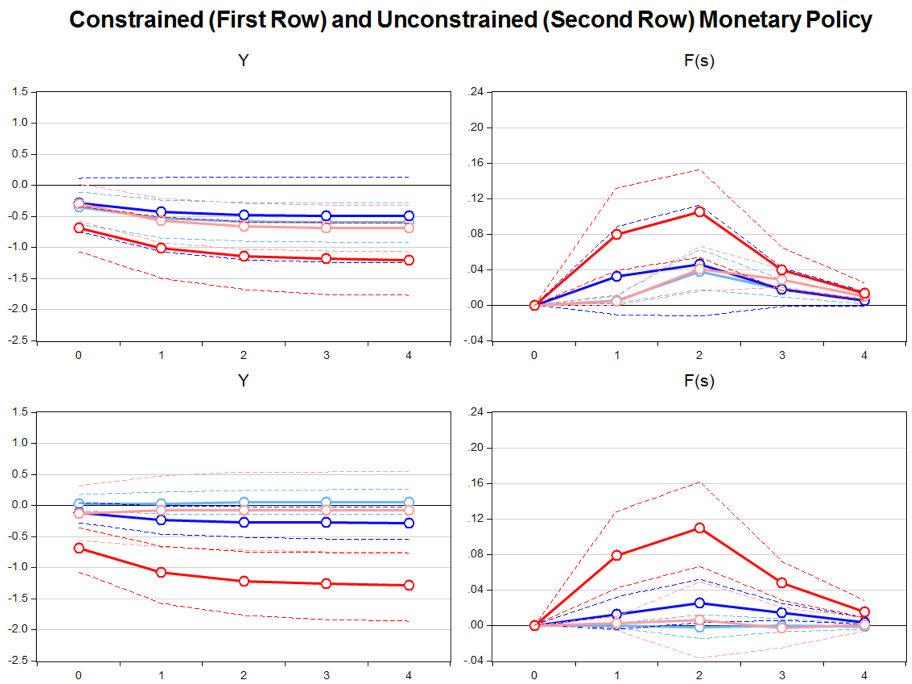

Figure 5: Euro Area vs Non-Euro Area countries.

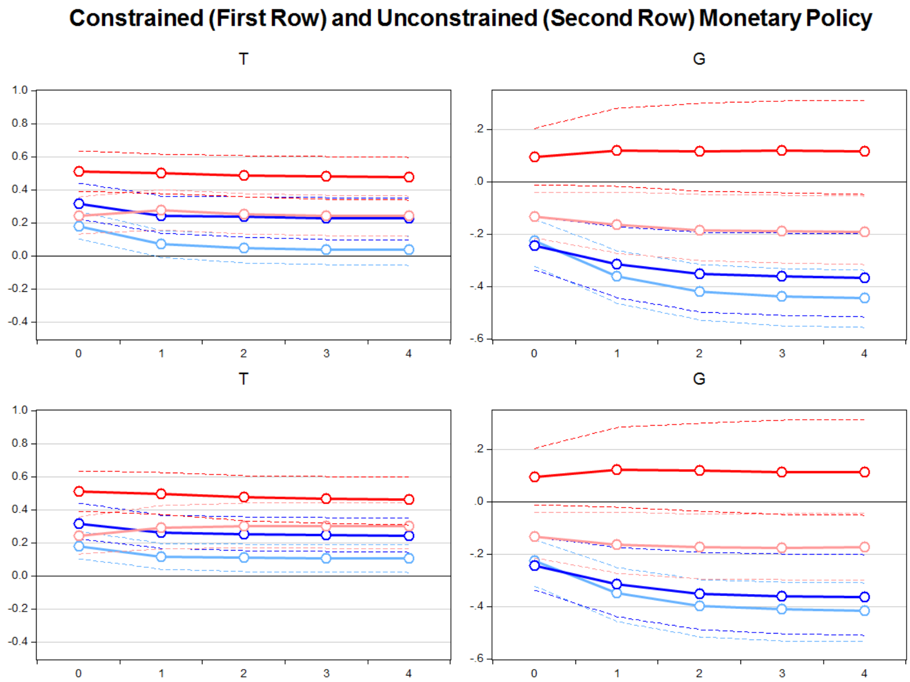

Figure 6: Euro Area vs Non-Euro countries.

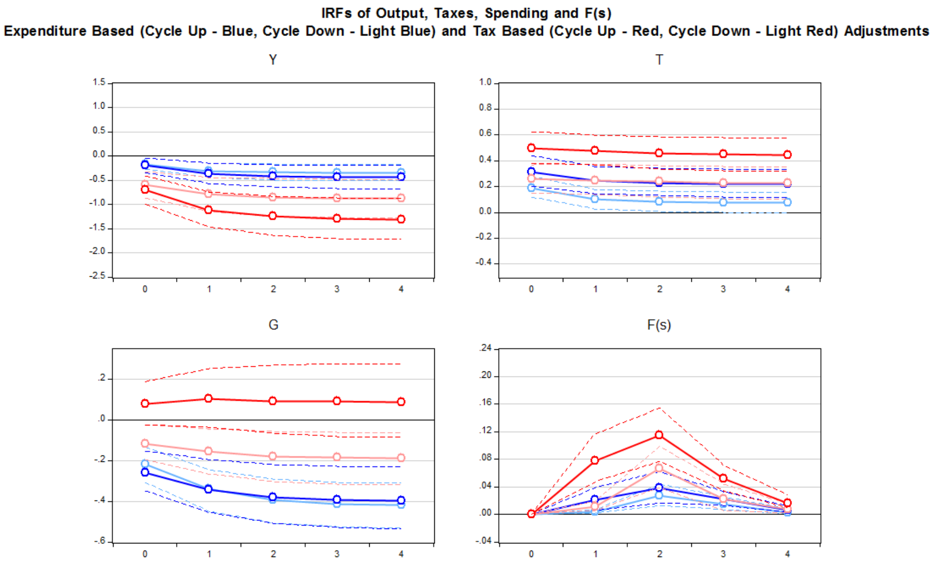

Figure 7: Excluding Episodes at the ZLB.

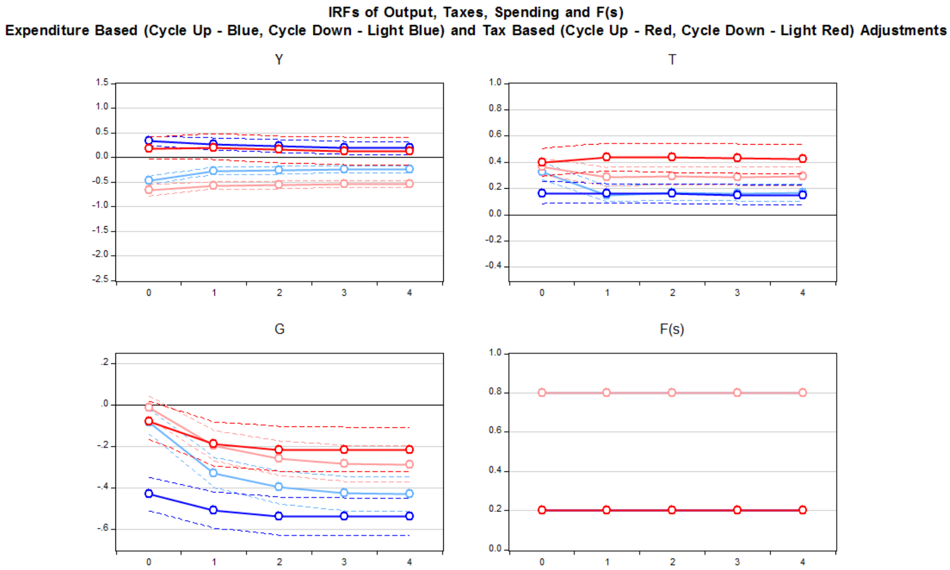

Figure 8: Impulses responses with coincident indicator of the state of the economy and constant regime under simulation