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ABSTRACT

The present paper argues that the correct experiment to evaluate the effects of a fiscal adjustment is the simulation of fiscal plans rather than of individual fiscal shocks. The simulation of the fiscal plans adopted by 16 OECD countries over a 30-year period supports the hypothesis that the effects of consolidations depend on their design. Fiscal adjustments based upon spending cuts are much less costly, in terms of output losses, than tax-based ones. Fiscal adjustments have especially low output costs when they consist of permanent rather than stop and go. The difference cannot be explained by accompanying policies, including monetary policy, and appears to be mainly due to the different response of business confidence and private investment.

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1 Introduction

Do sharp reductions of deficits and government debts (labeled fiscal adjustments or fiscal consolidations) cause large output losses? This paper argues that the correct methodology to answer this question requires studying fiscal plans, rather than individual fiscal shocks as it is normally done in the literature. Large fiscal consolidations are typically multi-year processes in which a government announces and then implements deficit reduction policies. Often these plans are revised and adjusted over the course of their implementation generating a complex interaction of expected and unexpected policy actions which we can account for. We argue that simulating shocks, *i.e.* the effect of isolated shifts in government revenues or in spending (as it is typically done in the literature) is not the right kind of experiment when the focus of interest is to gather evidence on the response of the economy to fiscal actions. The empirical analysis of the effects of fiscal policy, especially of major fiscal adjustments which never occur in a single year, should be based on the analysis of multi-period plans [1].

Fiscal shocks are typically used in the modern approach to policy analysis because they allow to select, from the empirical evidence, the facts that should be matched by the relevant theoretical Dynamic Sthocastic General Equilibrum Models (DSGE) model—which is the tool that should be used to address policy questions. Because the solution of a DSGE model can be well approximated by a Vector AutoRegressive model (VAR), empirical models have become the natural tool for selecting among different DSGE models, based on their ability to replicate the reduced form-based evidence. Such reduced form evidence, however, should be produced in the context of valid experiments, which means *(i)* identify exogenous policy actions that can be simulated keeping the parameters of the estimated empirical model constant, *(ii)* simulate experiments that do not change the correlation in the data used to estimate the parameters in the empirical model. Orthogonalized shocks obtained imposing restrictions on a VAR, satisfy these two conditions and their simulation is commonly considered the correct experiment[2]. We argue that this is not the case when analyzing fiscal policy.

This paper argues that the correct experiment for assessing the output effects of a fiscal consolidation—and establish the stylized facts that a the-

[1] Drautzburg and Uhlig (2013) take a first step in the direction of the identification of plans by allowing VAR-identified shocks to be correlated.

[2] This is the strategy used in traditional fiscal VAR, such as Blanchard-Perotti (2002).

oretical model should match—consists in simulating plans, not individual fiscal shocks. In fact fiscal consoldiations are indeed multi year plans in the intentions of the polciymakers and in the eyes of individuals.

The problem that arises when analyzing fiscal plans (as clearly illustrated by Leeper et al 2008, Leeper 2010) is that fiscal foresight—the fact that agents are aware of future, but not yet realized, fiscal adjustments—causes a misalignment between the information set used by the econometrician in a VAR and that available to the economic agents (see Lippi and Reichlin, 1994). The consequence is that the exogenous combination of unanticipated and announced fiscal corrections that characterizes a plan cannot be uniquely recovered from VAR innovations. The solution is to adopt the "narrative" approach introduced by Romer and Romer (2010) which does not suffer from this problem because exogenous shifts in fiscal policy are not reconstructed via the inversion of the moving average representation of a VAR, but directly observed using official documents to identify the size, timing, and motivation for the fiscal actions taken or announced by the government. This approach obviously relies on an accurate reading of policymakers' intentions. We use narratively identified fiscal adjustments to build exogenous plans. Plans consist of the announcement of a sequence of fiscal actions, some to be implemented the same year of the announcement (unanticipated) and some to be implemented in following years (announced). If a plan starts in period t we define policy changes in that period as unanticipated. The announcements for future periods are taken as a measure of anticipated policy changes. In principle even a plan which is announced and starts in period t could have been anticipated before t: our narrative measure does not allow for this possibility. However, the main focus of our analysis is the composition of fiscal adjustments. This is often the result of a complex political game, which makes predicting the composition of an adjustment very difficult.

Importantly, the design of plans generates inter-temporal and intra-temporal correlations among fiscal variables. The inter-temporal correlation is the one between the announced (future) and unanticipated (current) components of a plan. The intra-temporal correlation is that between the changes in revenues and spending that determines the composition of a plan—-what we shall call the "style" of a plan. As argued by Ramey (2011a, b) distinguishing between announced and unanticipated shifts in fiscal policy, and allowing them to have different effects on macroeconomic variables, is crucial for evaluating fiscal multipliers. The literature, however (see, for example, Mertens and Ravn 2011) has so far studied the different effects of anticipated and unanticipated shocks assuming that they are orthogonal. This is not the case in our sample where the correlation between anticipated and unanticipated shocks reflects the style of a fiscal plan. Thus an experiment designed to study, via a dynamic simulation, the effects of announced and unanticipated shocks should not violate the correlation between the two.

We build our data set upon the international database of narratively identified fiscal adjustments constructed at the IMF by Devries et al (2011) (D&al) which covers 17 OECD countries between 1978 and 2009.. Among all stabilization episodes these authors have selected those that were designed to reduce a budget deficit and to put the public debt on a sustainable path. This should guarantee their "exogeneity" since such fiscal actions represent a response to past decisions and economic conditions rather than to prospective conditions. As a result, they are unlikely to be systematically correlated with other developments affecting output in the short term, and are thus valid for estimating the short-term effects of fiscal consolidation on economic activity. The D&al dataset, however, only reports year by year fiscal shocks without distinguishing between announced an unanticipated policy shifts. We extend the dataset by clustering individual shifts in taxes or spending into multi-year fiscal plans consisting of announced and unanticipated exogenous fiscal shocks. We build such plans using the information – either found within D&al or using the original sources thereby indicated – about the dates in which each policy shift is approved and then implemented[3]. This allows us to distinguish between unanticipated and announced (up to 3-years ahead) shocks. The sum of the two components is always equal to the original shock coded in D&al except for seven cases in which, after careful examination of the original sources used to construct the dataset, we have decided to change their coding because it appeared to be incorrect.[4]. .

Countries adopt different styles for their fiscal consolidations. The degree of correlation between the unanticipated and announced part of a plan varies from being very strong and positive, to being negative. We have "reversal plans", where, for example, a fiscal tightening in the year a plan is first introduced, is accompanied by the announcement of looser fiscal policy in subsequent years. On the contrary, in "persistent plans" unanticipated and

[3] We test for exogeneity and we find that most of these plans, with one exception, which we drop, are indeed uncorrelated with past realizations of output.

[4] Our results are not driven by these seven changes and are robust if the (D&al) original coding is used. Detailed reasons for each change to the dataset are explained in the Appendix labelled Description of the Dataset.

announced fiscal actions go in the same direction.

Having identified exogenous fiscal plans we classify them in tax-based (TB) and expenditure-based (EB) on the basis of the relative importance of tax increases and spending cuts in each plan. This allows us to capture the intra-temporal correlation between tax hikes and expenditure cuts. Allowing tax hikes and expenditure cuts to be correlated is crucial for evaluating fiscal multipliers—for the same reason that it is crucial to allow for anticipated and unanticipated shocks to be correlated. Think of regressing output growth on distributed lags of exogenous changes in taxes and in expenditures and then using the estimated coefficients (for instance on taxes) to dynamically simulate the effect of a change in taxes keeping expenditures constant. This is not a valid experiment when changes in taxes and expenditure are correlated. The reason is that the estimated coefficients reflect the correlation between the two variables. If this is different from zero, then the coefficients cannot be used to run an experiment that instead assumes that the two variables are orthogonal.[5]

To analyze the impact of fiscal plans on macroeconomic variables we adopt the truncated MA representation that is normally used (see, for example, Romer and Romer 2010) when narrative shocks are identified. If we were to study the output effects of fiscal consolidations using plans for only one country we would have too few observations. This is why, in order to obtain more precise estimates, we pool together fiscal adjustments from different countries. Pooling, however, is problematic in the presence of heterogeneity (see, for example, Favero, Giavazzi and Perego 2011). We thus estimate a quasi-panel model pooling the international evidence but allowing for two sources of heterogeneity: *(i)* different styles of fiscal consolidations across countries and *(ii)* different effects of TB and EB plans.

The empirical evidence suggests that the effects of fiscal consolidations depend on their design and in particular on two characteristics: their composition (tax hikes *vs.* spending cuts) and their consistency over time (*i.e.* whether spending cuts are permanent or transitory). Spending-based adjustments have been associated on average with mild and short-lived recessions, in many cases with no recession at all. Instead, tax-based adjustments have been followed by prolonged and deep recessions. It is worth emphasizing

[5]Note that this problem is relevant for narrative shocks but does not arise in the case of fiscal shocks identified imposing restrictions on VAR residuals, since such shocks are orthogonalized to one another.

that these are averages of several plans: so an average of small or zero recession can be the result of some bigger recessionary episodes and even some case of expansionary fiscal adjustments. We find evidence which hints to the fact that adjustments may be associated with especially low output costs when they are permanent rather than stop and go. The difference between spending-based and tax-based adjustments is remarkable in its size and cannot be explained by different monetary policy responses. The difference in the output effects of the two types of fiscal adjustment is mainly due to the response of private investment, rather than that of consumption growth.[6] Interestingly, the responses of business and consumers' confidence to different types of fiscal adjustment show the same asymmetry as investment and consumption: business confidence (unlike consumer confidence) picks up immediately after the start of an expenditure-based adjustment.

The result that spending-based fiscal adjustments are, on average, non-recessionary or only very mildly recessionary, brings support to a vast literature started by Giavazzi and Pagano (1990) and recently extended and summarized by Alesina and Ardagna (2010, 2012). This literature, using simple data analysis and case studies, suggested that indeed spending-based fiscal adjustments—differently from tax-based ones—can have very small or no output costs at all.[7] Those results, as indeed ours, were obtained studying periods during which nominal interest rates had not fallen to zero and therefore the central bank could accompany the fiscal contraction with a monetary expansion. To rule out the possibility that our results are determined by an heterogenous endogenous response of monetary policy to tax-based and spending-based adjustments, we split our sample in two groups of observations: euro area countries from 1999 onwards, and non-euro area countries pooled with euro area countries before 1999. In euro area countries from 1999 onwards the response of monetary policy is constrained, in that the ECB sets its policy by looking at general euro area conditions and should not respond

[6] This result is consistent with Alesina et al (2002).

[7] Alesina and Ardagna (2010) and the literature which they summarize identified stabilization episodes using measures of large changes in cyclically adjusted budget deficits. Large reductions in this variable were assumed exogenous to output fluctuations, and thus an indication of active policies to reduce deficits. This, admittedly imperfect, approach was criticized by Guajardo et al.(2011) who then set out to build their dataset. Interestingly, while Guajardo et al.(2011) were critical of the possibility of costless fiscal adjustments, the results of the present paper show that a careful analysis using their own data leads to a picture which is remarkably similar to that of the previous literature reviewed by Alesina and Ardagna (2010).

to macroeconomic events idiosyncratic to member countries. The empirical evidence of heterogeneity between tax-based and expenditure-based adjustments is robust when the output response to fiscal policy is allowed to be different between EMU and non-EMU countries. To investigate the role of monetary policy we also run a counter-factual experiment. We shut down the response of innovations in monetary policy to exogenous fiscal contractions, thus investigating what the output response to a fiscal contraction would be in the case of no reaction of monetary policy to fiscal adjustments. We find that the differences are minor and that spending-based adjustments are less costly than tax-based ones even when monetary policy is not allowed to react to the adjustment. Finally, we also show that the difference between tax-based and expenditure-based adjustments is not driven by their timing relative to the business cycle.

The paper is organized as follows. The next section briefly reviews the theory and the empirical evidence on fiscal adjustments Section 3 describes the data and illustrates our strategy for the construction of the fiscal plans. Section 4 illustrates and discusses our estimation strategy. Section 5 reports our results. Section 6 discusses a number of robustness checks. The last section concludes.

2 Tax-based and spending-based stabilizations: theory and empirical evidence

Gathering empirical evidence on the effect on output of a fiscal stabilization is particularly relevant given that alternative theories have different predictions on this effect. Wealth effects, intertemporal substitution and distortions determine the effect of fiscal policy on output in neoclassical models (see Baxter and King,1993). These three channels operate differently in the case of tax increases or expenditure cuts. With lump sum taxes, and when agents derive no benefits from public spending, a reduction in government spending raises private wealth because future expected taxes fall. Private consumption increases and (if leisure and consumption are normal goods) labor supply falls. Because in this model labor demand does not change when government spending changes, hours worked decrease, the real wage increases and output falls. For output to increase following a reduction in wasteful government spending, taxes need to be distortionary and the intertemporal substitution elasticity sufficiently high. Intuitively this happens because, when the intertemporal substitution elasticity is high, the wealth effect produced by a cut in government spending is small relative to the substitution effect generated by the reduction in distortionary taxes, that increases the net return to investments and/or labor.

The literature considering the effects of fiscal policy on the components of aggregate demand has typically focused on consumption. An exception is the paper by Alesina et al. (2002) which analyzes (theoretically and empirically) the differential effects of spending cuts and tax increases on private investment. These authors show that lower government spending may imply lower taxes on capital, higher investment and possibly higher output. The size of these effects will depend upon the transitory or permanent nature of the change in expenditure (Corsetti and Meier 2009). An increase in taxation will instead have an unambiguous contractionary effect on output as the negative wealth effect on the demand side (both on consumption and on investment) is combined with the negative effect of increased distortions on the supply side. A reduction in government employment could instead be expansionary. Consider first a competitive labour market: the reduction in government employment generates a positive wealth effect. If both leisure and consumption are normal goods, consumption and leisure will increase and labour supply will decrease, but not enough to completely offset the lower demand for government employment. Hence, we should observe a reduction in real wages: the resulting increase in profits will raise investment, both during the transition and in steady state. When wages are bargained between firms and unions, a reduction in government employment may affect real wages both in the public and in the private sector. In a similar vein, Alesina and Perotti (1997) show how, in unionized economies, increases in income taxes translate into higher wage demand by unions, higher unit labor costs and a loss of competitiveness for domestic firms.

Confidence and uncertainty may also influence output fluctuations (Bloom 2009, Dixit and Pindyck 1994). Fluctuations in the degree of uncertainty produce rapid drops and rebounds in aggregate output and employment as higher uncertainty causes firms to temporarily pause their investment and hiring; productivity growth also falls as this pause in activity freezes reallocation across units. Again, for virtually all the channels discussed above it should matter a lot whether the spending cuts are perceived as permanent or transitory. Wealth effects will be larger for permanent spending cuts.

Textbook Keynesian models and new Keynesian models with less than perfectly flexible prices, predict that spending cuts are always recessionary (see e.g. De Long and Summers 2012, Galì, Lopez-Salido and Valles 2007) and the multiplier for government spending should be larger in theory than that for taxes. Recent research has pointed out this type of results might reemerge also in different context when nominal monetary policy rate are close to their lower bound.

Christiano et al. (2011) calculate that when the zero lower bound is binding, the spending multiplier turns positive (spending cuts reduce output) and, in their calibration, as large as 3.7. The channel through which this can happen is the expectation of future deflation. If prices are sticky, consumers expect prices to fall, when firms will be able to adjust them. This raises the real interest rate inducing them to postpone consumption. Eggerston (2010) similarly, and through the same mechanism, finds that the multiplier for a cut in labor taxes flips sign at the ZLB. In his calibration a 1% cut in labor taxes switches from being positive to negative, at -1.02. Our episodes do not include periods of ZLB, but we show that our results, regarding the lower costs of expenditure based adjustments verus tax based ones should survive at zero lower bound, since they do not depend on different repsonses of monetary policy to the two types of adjustments. The accumulation of data on these recent episodes will allow progress on this specific issue. The empirically literature based on identified shocks suggests that tax multipliers are larger than spending multipliers (see Ramey 2013 for a survey). Multipliers are also found to be larger during recessions (Auerbach and Gorodnichenko 2012, Giavazzi and McMahon 2013, Ramey 2013).

Finally, a different strand of the literature emphasizes the role of accompanying policies. One, as we already discussed, is of course monetary policy Guajardo et al.(2011), Alesina and Ardagna (1998, 2012) and Perotti (2013) show that certain supply-side polices, such as labor market and product market liberalization, wage agreements with the unions and reduction in unionization level, can help reduce or even eliminate the output losses associated with spending cuts.

3 Identification of exogenous fiscal plans

We construct multi-year exogenous fiscal plans re-classifying the fiscal policy shocks identified by D&al using the narrative method on a sample of OECD countries. For each episode of fiscal stabilization the narrative approach allows us to identify the unanticipated and the anticipated components of a plan. The data suggest that stabilizations come in different styles, depending on the persistence of the shifts in fiscal policy. Permanent shifts in fiscal policy occur when we observe a positive (or zero) correlation between the unanticipated corrections introduced when a plan is announced and those announced for the following years. When instead this correlation is negative, the fiscal measures are stop-and-go, *i.e.* temporary because the fiscal corrections introduced upon the announcement of a plan are at least partially reversed in the following years. As observed in the introduction, the literature so far (see *e.g.* Mertens and Ravn 2011), has studied the different effects of anticipated and unanticipated shocks assuming that the two are orthogonal. This is not the case in our sample where the correlation between anticipated and unanticipated shocks reflects the "style" of a fiscal plan.

If a plan starts in period t we define policy changes in that period as unanticipated. The announcements for future periods are taken as a measure of anticipated policy changes. In principle even a plan which is announced and starts in period t could have been anticipated: we have no way of measuring this possibility. We think however that this occurrence is unlikely since the composition of fiscal adjustments is often the result of a complex political game, which is quite hard to anticipate with a reasonable amount of certainty until the plan is announced and approved.

3.1 The data

We use the fiscal consolidation episodes identified in D&al for 17 OECD countries: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, the Netherlands, Portugal, Spain, Sweden, the United Kingdom, and the United States. The frequency of the data is annual and the sample runs from 1978 to 2009.[8] D&al use the records available in official documents to identify the size, timing and principal motivation for the fiscal actions taken by each country. In particular, they examine policymakers' intentions and actions as described in contemporaneous policy documents, that represent a response to past decisions and economic conditions rather than to current or prospective conditions. They emphasize that *"If a consolidation is motivated primarily by restraining domestic de-*

[8] The dataset is available on the IMF website (http://www.imf.org/external/pubs/cat/longres.aspx?sk=24892.0).

mand, we do not include it in our database". The historical sources examined include Budget Reports, Budget Speeches, Central Bank Reports, Convergence and Stability Programs submitted by EU governments to the European Commission, IMF Reports and OECD Economic Surveys. In addition, they examine country specific sources, such as, among other, various reports by the Congressional Budget Office and the Economic Reports of the President for the United States, the Journal Officiel de la Republique Francaise for France. Two examples of such exogenous fiscal plans are the U.S. 1993 Omnibus Budget Reconciliation Act, which involved raising taxes and cutting spending *"not to reduce the risk of economic overheating, but because policymakers saw it as a prudent policy change with potential long-term benefits"* and the European plans adopted in the second part of the 1990s to meet the Maastricht criteria and join the euro. For most countries the concept of government adopted is the "general government", which includes both the central State administration and all levels of local governments. For three federal countries (Canada, Australia and the United States) the data only refer to the central government (e.g. the Federal government for the US). This would affect the results if local government systematically moved their budget, for instance to offset the effect of changes in the central budget.

The shifts in fiscal policy recorded in this data include, as the Romer and Romer (2010) dataset, both unanticipated and anticipated policy shifts, that is tax increases or spending cuts announced in year t, to be implemented in year $t + i$. A few measures that were announced but for which *"the historical record shows that they were not implemented at all"* are dropped from the D&al database and cannot easily be recovered. Fortunately there are only five instances in our sample in which this happened—that is individual announcements were not recorded because never implemented—one each in Japan, Italy, Germany, the UK and the Netherlands (a case which is irrelevant for us since, as we discuss below, we drop this country). All other announcements are assumed to be credible and thus recorded.

This identification strategy applies to a panel of countries the idea originally proposed by Romer and Romer (2010) for the U.S. to identify major tax policy changes not dictated by business cycle fluctuations. In the D&al data tax increases are measured, as in Romer and Romer, by the expected revenue effect of each change in the tax code, as a percent of GDP. Spending cuts (also measured as percent of GDP) are changes in expenditure relative to the level that was expected absent the policy shift, not relative to the previous year. Thus a spending cut for year $t + 1$ does not necessarily imply a reduction in government spending relative to year t, but only relative to what would have happened in year $t+1$ absent the policy shift.[9] This is the correct way to measure spending cuts if we want to capture the effect of new information.

However, the criteria used by D&al to identify the relevant shocks differ from those adopted by Romer and Romer (2010) in two important dimensions. The latter focus only on revenue shocks and identify two main types of legislated exogenous tax changes: those driven by long-run motives, such as to foster long-run growth, and those aiming to deal with budget deficits. D&al, instead, consider both expenditure and revenue shocks and focus only on fiscal actions motivated by the primary objective of reducing the budget deficit[10]. This means that the identified shocks do not have zero mean: only shocks which have a negative impact on the deficit are recorded, that is only tax increases and expenditure cuts. Having a series of adjustments that occur always in the same direction (we do not consider fiscal expansions) raises naturally the possibility that the series is truncated. However, given the authors' identification criteria, these truncated shocks should correspond to tax cuts or increases in expenditure engineered because the deficit was perceived as too low or the surplus too high. These cases are quite unlikely[11].

Finally we run a simple check to assess whether the adjustments identified by D&al are indeed exogenous, by regressing them on a distributed lag of output growth. A shift in spending or taxes is exogenous for the estimation of our parameters of interest if it cannot not be predicted by past variables. The only country for which the narrative identified fiscal adjustments can be

[9] This way to measure spending cuts is the one that was used in the United States in 2013 to measure the effect of the so-called "Sequester".

[10] In fact, if fiscal consolidation is offset by fiscal actions not primarily motivated by cyclical fluctuations, they compute the sum of the two measures (deficit-driven and long-run growth) and conclude that consolidation occurred if the overall change in policy yields budgetary savings.

[11] Although we cannot check for truncation for all the countries in our sample, we can for the U.S., comparing the Devries et al with the Romer and Romer shocks. The latter include both positive and negative observations, and are constructed aggregating tax shocks that are deficit-driven and tax shocks driven by a long-run growth motive. Deficit-driven fiscal expansions never occur in the Romer and Romer sample because all tax shocks driven by the long-run motive are expansionary (i.e. negative tax shocks), and all the deficit-driven tax shocks are contractionary (i.e. positive tax shocks). Therefore, the Romer and Romer deficit-driven shocks, which are directly comparable to those identified by Devries et al, show no evidence of truncation.

predicted by past output growth is the Netherlands, which we drop from the sample[12].

The construction of our plans is such that the sum of the anticipated and the unanticipated components of each policy shift is always equal to the original shocks coded in D&al, except for 7 cases. In such cases we have decided to change their coding, after a check based on the original sources. Detailed reasons are explained in the Data Appendix.[13]

Summing up. The D&al data contain, over the period 1978-2009, a total of 563 individual exogenous shifts in government spending and tax revenues (unanticipated and anticipated) for seventeen countries. We construct plans based upon the annual information available. We drop the Netherlands for the reason illustrated above. In our baseline results we also drop Sweden and Finland because we lack data on confidence for these countries. The results including Sweden and Finland for the variables for which data are available are essentially identical, as we show below. Thus in our baseline results we use 14 countries: four non-European countries (the U.S., Canada, Australia and Japan), two EU countries that are not members of the monetary union (Denmark and the U.K.) and eight Euro area countries (Germany, France, Italy, Spain, Austria, Belgium, Ireland, Portugal). [14]

[12] Note that the exclusion of Netherlands is not crucial to determine our results. Note also that our results are slightly different from those reported in de Cos and Mora (2012) who find some correlation between a dummy set to one on occasion of the fiscal adjustments identified by Devries et al. and zero everywhere else and past output growth. The difference arises from the use of a dummy instead of a continuous variable which reflects the actual change in fiscal variables, as we do here.

[13] For example, according to Devries et al. (2011) the Italian Delegation Law of 1993 included L 31 trillion of expenditure-based fiscal consolidation (1994 OECD Economic Surveys, p.44-45). However, the OECD report quantifies the expenditure cuts of the Delegation Law to actually be 43.5 trillion. The L 31 trillion was the targeted primary surplus decided in May 1993 and it was incorrently regarded as the total amount of expenditure cuts in entry 1993 for Italy. As a consequence, when we encountered such cases we revised the magnitude of the shocks according to the sources cited within the text.

[14] Mertens and Ravn (2012) have pointed out the potential measurement error associated with the measurement of fiscal shocks. Unfortunately the solution they propose—using narrative shocks as instruments for the true unobservable shocks—is not applicable to plans that include both anticipated and unanticipated components. Nevertheless, we believe that the issue raised by these authors is a relevant one, and an extension of their approach to plans is a very interesting agenda for future research.

3.2 Constructing plans

We identify plans as sequences of fiscal corrections announced at time t to be implemented between time t and time $t+k$; we call k the anticipation horizon. We define the unanticipated fiscal shocks at time t for country i as the surprise change in the primary surplus at time t:

$$e_{i,t}^{u} = \tau_{i,t}^{u} + g_{i,t}^{u}$$

where $\tau_{i,t}^{u}$ is the surprise increase in taxes announced at time t and implemented in the same year, and $g_{i,t}^{u}$ is the surprise reduction in government expenditure also announced at time t and implemented in the same year. We denote instead as $\tau_{i,t,j}^{a}$and $g_{i,t,j}^{a}$ the tax and expenditure changes announced by the fiscal authorities of country i at date t with an anticipation horizon of j years (*i.e.* to be implemented in year $t+j$). In the D&al dataset fiscal plans almost never extend beyond a 3-year horizon: thus we take $j=3$ as the maximum anticipation horizon [15]. We therefore define the observed anticipated shocks in period t as follows

$$\begin{aligned}
\tau_{i,t,0}^{a} &= \tau_{i,t-1,1}^{a} \\
\tau_{i,t,j}^{a} &= \tau_{i,t-1,j+1}^{a} + \left(\tau_{i,t,j}^{a} - \tau_{i,t-1,j+1}^{a}\right) \;\; j \geqslant 1 \\
g_{i,t,0}^{a} &= g_{i,t-1,1}^{a} \\
g_{i,t,j}^{a} &= g_{i,t-1,j+1}^{a} + \left(g_{i,t,j}^{a} - g_{i,t-1,j+1}^{a}\right) \;\; j \geqslant 1 \\
e_{i,t,j}^{a} &= \tau_{i,t,.j}^{a} + g_{i,t,.j}^{a}
\end{aligned}$$

We shall illustrate how we use our classification of fiscal shocks to construct fiscal plans with two examples: the plans introduced in Italy in 1991 and in Australia in 1984. The case of Italy is illustrated in Table 1. D&al state that *"...The narrative analysis leads to the conclusion that in 1991 fiscal consolidation amounted to 2.77 percent of GDP, with tax hikes worth 1.69 percent of GDP and spending cuts of 1.08 percent of GDP. Fiscal consolidation was motivated by government debt reduction, as the Bank of Italy Annual Report 1990 (p. 69) explains ... However, as reported by the IMF in*

[15] In the sample there are a few occurences of policy shifts anticipated four and five years ahead. Their number is too small to allow us to include them in our estimation.

its 1992 Recent Economic Developments document (p. 21), a number of the tax measures introduced in 1991-Lit 19.4 trillion (1.26 of GDP)-were of a one-off nature.... The expiration in 1993 of one-off tax measures introduced in previous years was worth 1.20 percent of GDP....". The first row of Table 1 illustrates our classification of this narrative record.

Insert Table 1 here

Note that the plan introduced in 1991 was subsequently modified, in 1992 and in 1993, with the introduction of further unanticipated tax hikes of 2.85 and 3.2 per cent of GDP respectively, and additional spending cuts worth 1.9 and 2.48 per cent of the GDP. As we highlight below Italy is indeed a country which normally does not stick to announced plans.These modifications are illustrated in the second and third rows of Table 1. We label fiscal adjustments respectively as "tax-based" (TB) and "expenditure-based" (EB) if the sum of the unexpected plus the announced tax (expenditure) changes (measured as percent of GDP in the year the plan is introduced) is larger than the sum of the unexpected plus the announced expenditure (tax) changes. Table 1 illustrates that this classification strategy leads to label the 1991-1993 Italian adjustment as EB. Note that this happens because the tax hike introduced in 1991, despite being larger than the corresponding spending cuts, is transitory, while the spending cut is permanent. This multi-year labelling strategy does not lead to marginal cases – in which a label is attributed on the basis of a negligible difference between the share of tax hikes and expenditure cuts in the overall adjustment. The data show that in most cases a political decision was made as to the nature of the fiscal consolidation: EB or TB. Table 1 in the Data Appendix lists our classification of all episodes in TB and EB.

We cannot observe the realizations of announced plans, because the narrative method allows to identify exogenous corrections at the time when they are announced but only total expenditure and receipts are observed upon implementation. Thus we cannot control-because we do not observe it-for the possibility that the composition of an adjustment changes (relative to what had been announced) when it is implemented. We do observe, however, the total adjustment if this differs from what had been announced.

Our second example is Australia. The plan which was introduced in 1985 with a series of sequential adjustments, lasted until 1988. After the December 1984 elections — in which the Labour party surprisingly defeated the sitting liberals — the government announced a sequence of medium-term spending cuts aimed at reducing a large inherited budget deficit. Table 2 illustrates this episode. The plan announced in 1984 featured no change in taxation and spending cuts of $0,45$ per cent of GDP each year in 1985 and 1986. In 1986 the plan was revised: the new plan called for additional spending cuts of 0.4 of GDP in 1986, of 0.26 in 1987 and a very small reversal of -0.08 in 1988. In the revised plan revenue increases were also introduced: a tax increase of 0.17 of GDP in 1986, a further increase of 0.19 of GDP in 1987 and an almost complete reversal (-0.29) in 1988. All four years are labelled as periods of EB adjustment. Note that because the revisions for 1988 were announced in as part of a multi-year plan, 1988 is labelled as a year of EB adjustment even if in that year we observe an (anticipated) reduction in taxation larger that the (anticipated) increase in expenditure. This would not be the case if we (incorrectly) overlooked plans and only considered year-to-year fiscal adjustments.

As the Australian and Italian examples illustrate, the procedure used to label corrections as TB or EB uses only information available in real time: the labelling of each plan is decided on the basis of information available when the plan is announced and implemented. This labelling can therefore be used to estimate and simulate the real time effects of the adoption of a plan and to detect potential differences between EB and TB plans. [16]

Insert Table 2 here

The results of our classification of episodes for each country is reported in Table 3. Sometimes fiscal plans change nature over time: for instance they start as an EB plan and at some point turn into a TB plan. One example of a policy reversal is Canada in 1991. A plan initially labelled as TB was modified, after some time, to deliver the majority of corrections on the expenditure side. At the time of the announcement we label such a plan TB, but it then shifts to EB when the new announcement is made and tax hikes are replaced by spending cuts. The coding of different episodes is

[16] This would not be possible with alternative classification schemes. For instance, using the success of adjustments, say in terms of their ability to stabilize the debt/GDP ratio to identify their status. Success can be a useful classification criterion within sample, but it is useless for out-of-sample analyses, since the success of a plan cannot be determined upon its announcement.

implemented using two dummies, EB and TB, that take values of one when the relevant adjustment is implemented, and zero otherwise.

Insert Table 3 here

As already noted, fiscal plans – at least those in our dataset – differ not only in their composition (EB *vs.* TB) but also in the correlation between unanticipated and anticipated shifts in fiscal variables–what we have labelled the "style" of a plan. This is determined by the observed correlation between unanticipated and anticipated shifts announced at time t. A permanent fiscal correction is characterized by zero or positive correlation between e_t^u and $e_{t,t+,j}^a$ $(j > 1)$. Instead, stop-and-go adjustments display a negative correlation between $e_{i,t}^u$ and $e_{i,t,j}^a$ $(j > 1)$.

4 Specification of the empirical model

We estimate the effect of fiscal adjustments on several variables: GDP growth (all growth rates are annual), private consumption growth, the growth in private fixed capital formation[17], the change in short-term ($3-$month) interest rates, inflation, the (log of) the Economic Sentiment Indicator (ESI) for both consumers and firms computed by the OECD or the European Commission. The sources of our data and all data transformations are described in Table A2 in the Appendix.

The model we estimate, (1), is a (truncated) moving average representation of the variable of interest, $\Delta z_{i,t}$ (in turn GDP growth, private consumption growth, etc.). We estimate a quasi-panel which allows for two types of heterogeneity: within-country heterogeneity in the effects of TB and EB plans on the left-hand-side variable, and between-country heterogeneity in the style of a plan

[17]Except for Italy and Spain where lack of separate data on private investment at the beginning of the sample forces us to study total investment: private plus public. Our results are unaffected if we drop these two countries.

$$
\begin{aligned}
\Delta z_{i,t} &= \alpha + B_1(L)e^u_{i,t} * TB_{i,t} + B_2(L)e^a_{i,t,0} * TB_{i,t} + \\
&\quad C_1(L)e^u_{i,t} * EB_{i,t} + C_2(L)e^a_{i,t,0} * EB_{i,t} + \\
&\quad + \sum_{j=1}^{3} \gamma_j e^a_{i,t,j} * EB_{i,t} + \sum_{j=1}^{3} \delta_j e^a_{i,t,j} * TB_{i,t} + \lambda_i + \chi_t + u_{i,t}
\end{aligned} \tag{1}
$$

$$
\begin{aligned}
e^a_{i,t,1} &= \varphi_{1,i}\ e^u_{i,t} + v_{1,i,t} \\
e^a_{i,t,2} &= \varphi_{2,i}\ e^u_{i,t} + v_{2,i,t} \\
e^a_{i,t,3} &= \varphi_{3,i}\ e^u_{i,t} + v_{3,i,t}
\end{aligned}
$$

where λ_i and χ_t are country and time fixed effects.

In (1) shifts in fiscal policy affect the economy through three components. First, unanticipated changes in fiscal stance, $e^u_{i,t}$, announced at time t and implemented at time t; second the implementation at time t of policy shifts that had been announced in the past, $e^a_{i,t,0}$; third, the anticipation of future changes in fiscal policy, announced at time t, to be implemented at a future date, $e^a_{i,t,j}$ for $j = 1, 2, 3$. Our moving average representation is truncated because the length of the $B(L)$ and $C(L)$ polynomials is limited to three-years. This truncation, however, does not affect the possibility of correctly estimating the fiscal multipliers, as all omitted shocks and all information lagged $t-4$ and earlier are orthogonal to the variables included in our specification[18]. $\varphi_{1,i}, \varphi_{2,i}, \varphi_{3,i}$ are estimated on a country by country basis on the time series of the narrative fiscal shocks.

[18] (1) differs from a VAR. The usual practice in VAR models is to derive impulse responses first by estimating the model in autoregressive form, then by identifying structural shocks from the VAR residuals, and finally inverting the VAR representation to obtain the infinite MA representation in which all variables included in the VAR are expressed as linear functions of a distributed lag of structural shocks. The coefficients in this representation (that are not directly estimated) define the impulse response function. In our case, since we observe the structural shocks from the narrative method, we can directly compute impulse responses, thus following the estimation procedure adopted by Romer and Romer (2010). The advantage of observable narrative shocks is that they allow to compute impulse responses omitting – differently from a standard VAR – a large amount of information which would be orthogonal to the shocks included in the regression. Therefore, parsimony in the specification is paired with consistent (though not efficient) estimation:. We pay a cost in terms of precision, as the omitted information affects the size of the confidence intervals of the impulse response functions.

We compute impulse responses taking into account the correlation between unanticipated shocks in year t and shocks announced in year t to be implemented in years $t+1$, $t+2$ and $t+3$. That is when we simulate the response to an anticipated shock we take into account the fact that such a shock typically does not occur in isolation but is accompanied by the contemporaneous announcement of future shifts in fiscal variables according to our estimates of the φ parameters. Impulse responses to correlated shocks can be computed using the Generalized Impulse Response Functions (GIRF) discussed in Garratt et al. (2012), where contemporaneous linkages across shocks are based on the estimated covariances of the error terms. Following a similar approach we first estimate the φ coefficients which describe the response of anticipated shocks to unanticipated ones. Then, when we simulate the impact of a realization of $e^u_{i,t}$, we also change $e^a_{i,t,t+1}$ (by $\varphi_{1,i}$), $e^a_{i,t,t+2}$ (by $\varphi_{2,i}$),and $e^a_{i,t,t+3}$ (by $\varphi_{3,i}$) [19]. In other words (1) is a quasi-panel: we impose cross-country restrictions on the B, C and γ coefficients, but we allow for within- and between-country heterogeneity. "Within" because impulse responses will be different for TB and EB adjustments. "Between" because they will also differ across countries as the $\varphi' s$ differ, according to each country's specific style. We compute impulse responses to a shock in the unanticipated component of the fiscal corrections, $e^u_{i,t}$, equal to one per cent of GDP. The total size of the adjustment, however, will differ across countries as the response of anticipated corrections to unanticipated ones differs from one country to another. Finally, the effects of different style of fiscal adjustments can be gauged by comparing the impulse responses of different countries. The model is estimated by SUR (Seemingly Unrelated Regressions).

The overall model contains a total of 56 equations: 4 equations for each of the 14 countries. The total number of estimated parameters is 100: 18 common parameters, 14 country fixed effects, 26 time dummies and (14*3) parameters in the equations linking unexpected to expected shocks. We compute impulse responses by simulation as illustrated in Appendix 2 . We expect that our specification will deliver much more precise estimates of the impulse response functions than those normally obtained in VAR for a number of reasons. First, what we estimate is a quasi-panel version of the

[19] Our estimates of the φ parameters are simply meant to capture the correlation between observable anticipated and unanticipated corrections. Thus, for our purposes, there is no need to instrument the regressors to obtain valid estimates.

truncated MA representation adopted by Romer and Romer (2010) for U.S. data and the cross-sectional dimension allows us to significantly enlarge the sample size and the precision of the estimates. Second, consistently with the SURE estimation of the quasi-panel model, we bootstrap residuals by taking into account the fact that there is cross-sectional correlation among them. Third, plans identified by the narrative records, differently from shocks identified in a VARs, are observable and they are therefore not resampled when confidence intervals are constructed by bootstrap methods. Finally, allowing for an heterogenous effect of TB and EB plans reduces the size of residuals in the estimated model.

4.1 Discussion

Our proposed specification is meant to capture in a parsimonious way the impact on fiscal multipliers of within and between country heterogeneity in the composition of a fiscal adjustment. There are several aspects of our specification that are worth discussing to illustrate the way in which we have solved the trade-off between parsimony and the risk of under-parameterization.

The direct way of modelling heterogeneity between tax and spending multipliers is a specification that distinguishes between the effect of exogenous tax adjustment and exogenous expenditure adjustments

$$\begin{aligned}\Delta z_{i,t} &= \alpha + B_1(L)\tau^u_{i,t} + B_2(L)\tau^a_{i,t,0} + \\ &\quad C_1(L)g^u_{i,t} + C_2(L)g^a_{i,t,0} + \\ &\quad + \sum_{j=1}^{3}\gamma^a_j\tau^a_{i,t,j} + \sum_{j=1}^{3}\delta_j g^a_{i,t,j} + \lambda_i + \chi_t + u_{i,t}\end{aligned} \tag{2}$$

$$\begin{array}{ll}
\tau^a_{i,t,1} = \varphi_{1,i}\ \tau^u_{i,t} + v_{1,i,t} & \tau^a_{i,t,1} = \varphi_{7,i}\ g^u_{i,t} + v_{7,i,t} \\
\tau^a_{i,t,2} = \varphi_{2,i}\ \tau^u_{i,t} + v_{2,i,t} & \tau^a_{i,t,2} = \varphi_{8,i}\ g^u_{i,t} + v_{8,i,t} \\
\tau^a_{i,t,3} = \varphi_{3,i}\ \tau^u_{i,t} + v_{3,i,t} & \tau^a_{i,t,3} = \varphi_{9,i}\ g^u_{i,t} + v_{9,i,t} \\
g^a_{i,t,1} = \varphi_{4,i}\ g^u_{i,t} + v_{4,i,t} & g^a_{i,t,1} = \varphi_{10,i}\ \tau^u_{i,t} + v_{10,i,t} \\
g^a_{i,t,2} = \varphi_{5,i}\ g^u_{i,t} + v_{5,i,t} & g^a_{i,t,2} = \varphi_{11,i}\ \tau^u_{i,t} + v_{11,i,t} \\
g^a_{i,t,3} = \varphi_{6,i}\ g^u_{i,t} + v_{6,i,t} & g^a_{i,t,3} = \varphi_{12,i}\ \tau^u_{i,t} + v_{12,i,t} \\
g^u_{i,t} = \varphi_{13,i}\ \tau^u_{i,t} + v_{13,i,t} &
\end{array}$$

there are clearly many more parameters in (2) than in our baseline specification. However, the estimation of all the relevant $\varphi's$ in this specification is not feasible given the available observations on the components of fiscal plans. Note that estimates of the $\varphi's$ are essential to measure tax and spending multipliers. Consider, for the sake of illustration, the case of a researcher interested in the output effect of an unanticipated tax change $\tau^u_{i,t}$. $B_1(L)$ would correctly measure this multiplier only if $\varphi_{1,i} = \varphi_{2,i} = ... = \varphi_{13,i} = 0$. In fact, only in this case the experiment of introducing a shock to $\tau^u_{i,t}$ setting all the other innovations to zero would be a valid one. If, for example, $\varphi_{13,i} \neq 0$, one cannot set $g^u_{i,t} = 0$ when simulating the effect of an unanticipated tax shock $\tau^u_{i,t}$. In other words, since the parameters in our model are estimated allowing for the sample correlation between changes in taxes and spending as well as between unanticipated and anticipated changes, such correlations cannot be assumed away when the model is simulated.

Our specification saves degrees of freedom first by studying the correlation between unanticipated and anticipated total adjustments, that is by estimating in (1) three $\varphi's$ parameters instead of thirteen as in (2) and then by distinguishing between tax-based and expenditure-based adjustments.

Finally, our specification also imposes the restriction that all anticipated shocks occurring at time t have the same impact on the dependent variable independently on how far back they had been announced. This is why we use a single variable, $e^a_{i,t,0}$. As already argued (Mertens and Ravn 2011), this seems a very reasonable way to save on degrees of freedom.

5 Results

In this section we present our baseline results from the estimation of (1) and the associated equations used to estimate the $\varphi's$. The estimation runs from 1981 to 2007 (we observe exogenous shifts in fiscal variables over the period 1978-2009, but we lose observations from the presence of leads and lags of the fiscal variables).

Table 4 illustrates the difference in the style of fiscal adjustments in the various countries. In this table (where we also report the results for Sweden and Finland which are not in the baseline regressions because for these two countries we lack data on confidence) we report the estimates of $\varphi_{1,i}$, $\varphi_{2,i}$, $\varphi_{3,i}$ with their standard errors in brackets. We show a coefficient of zero, with no standard error, whenever there are too few observations available for estimation. Canada and Australia and Sweden record a cumulative response (sum of the responses of one-, two- and three-year ahead announcements to an unanticipated correction) which is in the region of unity and higher than one for Canada., Austria, Denmark, France, Japan and the U.K. feature a positive but milder response of anticipated corrections to current unanticipated ones with coefficients ranging from 0.12 to 0.85. This correlation becomes not statistically different from zero in Belgium, Finland, Germany, Ireland, Japan, Ireland, Portugal, Spain, and the U.S., where fiscal policy corrections are implemented mainly via unanticipated shocks[20]. At the opposite end of the spectrum lies Italy, where one and two–year ahead anticipations are negatively correlated with unanticipated shocks (significantly at the one-year horizon). This suggests that at least part of a typical Italian stabilization is transitory.

Insert Table 4 here

Figure 1 illustrates visually the potential importance of this point by reporting $e^u_{i,t}$ and $e^a_{i,t,t+1}$ for all countries in our sample. The figure shows a significant heterogeneity across countries in the design of their fiscal plans and confirms the results of Table 4. Compare, for instance, the results for Sweden and Italy. In Sweden the continuous and the dotted lines move together, indicating that unanticipated (the continuous line) and 1-year ahead anticipated (the dotted line) shifts in fiscal stance move in the same direction. That is, unanticipated tightenings are accompanied by the announcement of more tightening one year down the road. The opposite happens in Italy.

Insert Figure 1 here

Figure 2 reports impulse responses of output growth to EB and TB fiscal plans where, as everywhere else in this paper for comparability with the available empirical literature, we report *one standard errors* bands. Countries are ordered starting from those that feature a positive but mild correlation between future anticipated and current unanticipated corrections (Australia, Austria, Denmark, France, the U.K. and Japan). Next we list the countries for which this correlation is close to zero (Belgium, Germany, Ireland, Portugal, Spain, and the U.S.). Finally the two opposite ends of the spectrum

[20]Table 4 reports a zero with no standard error where the number of observations was not large enough to estimate the relevant $\varphi' s$.

in terms of the relation between anticipated and unanticipated fiscal adjustments, Canada and Italy. The patterns differ across countries (because of the heterogeneity in styles) but in all of them the difference between EB and TB adjustments is large and statically significant. In all countries TB adjustments are recessionary and there is no sign of recovery for at least the three years following the start of a plan. In the case of EB adjustments recessions are on average much smaller and short-lived. Note that this is an average which could result from some bigger EB induced recessions and some expansionary EB adjustments. Interestingly, Canada features the largest difference between TB and EB plans while the smallest is observed in the case of Italy. This is not surprising because an unanticipated shift in taxes equal to 1% of GDP (our experiment) in Italy is partly offset by the anticipation of future shifts in the opposite direction. This comparison hints at the fact that adjustments have especially low cost when the announcement of a spending cut is not accompanied by that of a future reversal. On the contrary they are less effective when they are stop-and-go.[21]

Insert Figure 2 here

Figures 3 and 4 show the response of households' consumption on durables and non-durables and of business investment [22]. The results indicate that the different effect on output growth of TB and EB adjustments is to be attributed more to the response of private investment, than to that of private consumption. Consumption growth typically responds less hetrogenously than investment to TB and EB adjustments.

[21] Guajardo et al (2011) also use the (D&al) data and also distinguish between EB and TB adjustments. Compared with our results, however, the impulse responses reported in that paper are constructed overlooking plans and country-specific styles *i.e.* overlooking the correlation between unanticipated and anticipated shifts in taxes and spending. Although the general message is similar—EB adjustments are less recessionary than TB ones—overlooking plans results in much wider confidence intervals. Note that Guajardo et al (2011) report, in their Figure 9, *one standard error* bands, with 64 per cent confidence intervals. To allow comparability we do the same in this paper (in a version of this paper previously circulated labels on confidence bound were wrongly indicating the width of our bands at two stadard deviations). It is probably worth noting that the difference between the effect of EB ant TB plans on output remains significat also if *two standard errors* bands, with 95 per cent confidence intervals are considered.

[22] The data refer to private capital formation for all countries except for Spain and Italy where, for the early part of the sample, we only have data total capital formation which includes both private and public capital formation. Our results are unchanged if we drop these two countries in our estimation.

Insert Figures 3 and 4

Figures 5 and 6 report the responses of the indicators of consumer and business confidence. The evidence of heterogeneity between TB and EB adjustments is confirmed in the response of consumer confidence, and more strongly confirmed for business confidence. The evidence from the responses of business confidence and investment is consistent with a causal relation running from business confidence to investment and output.

Insert Figures 5 and 6

Finally, we consider the response of monetary policy and of inflation, which are reported in Figures 7 and 8.

Insert Figures 7 and 8

Overall, monetary policy (the change in 3-month interest rates) is more expansionary in the case of EB adjustments than in the case of TB ones. The differences in the responses of monetary policy to fiscal plans, however, appears to be too small to explain the large differences in output responses; moreover the pattern of cross-country heterogeneity in the responses of monetary policy to fiscal plans does not match the one observed for output.

The response of inflation helps understand why monetary policy might be tighter during TB plans. Figure 8 shows that TB adjustments are in general more inflationary than EB ones. One possibility, as discussed in Alesina and Perotti (1997), is that TB plans include increases in indirect taxes and in income taxes which trigger a response of wages. This evidence raises the issue of the importance of accompanying monetary policy in determining the heterogenous effects on output of TB and EB plans. Could it be that EB plans are less recessionary precisely because monetary policy is more expansionary during such plans? If this were the case the heterogeneity between the two types of plans could disappear at the zero lower bound, where interest rates are prevented from falling. We address this issue in the next section where we show that monetary policy cannot be the main explanation for the difference in the effects of TB *vs* EB adjustments.

Before turning to our robustness analysis it is worth comparing once again the results for Canada and Italy. These two countries, as we discussed above, are at the opposite ends of the spectrum in terms of their styles of adjustments. In Canada the government typically announces fiscal plans that are consistent over time. Italy, on the contrary, is the quintessential example of stop-and-go policies. Interestingly, the evidence for Canada suggests that EB adjustments, when they are part of a consistent plan, might be expansionary, driven by a surge in private investment. In Italy, instead, the difference between EB and TB plans is the smallest among the countries in our sample, and EB plans don't feature positive effects on output. This observation suggests that consistent plans over time seem to be superior to stop-and-go, largely unpredictable policies.[23]

6 Robustness

6.1 Monetary policy

Does the accompanying monetary policy explain the difference between EB and TB adjustments? In this section we show that the answer is negative[24]. We show this results in two ways: first, we exploit the fact that for a sizeable part of our sample monetary policy is constrained, since in the Euro area it is conducted by the ECB and it cannot respond to country-specific fiscal adjustments. Next, we design a counterfactual experiment aimed at evaluating what the effect of fiscal adjustment would be if policy rates remained unchanged.

6.1.1 The effect of fiscal policy in euro area countries

We now separate our observations in two groups: in the first group we include observations for euro area countries (Austria, Belgium, France, Finland, Germany, Ireland, Italy, Portugal and Spain) from 1999; in the second group all other observations: non euro-area countries (Australia, Denmark, U.K.,

[23]The policy reversals which are part of Italian plans might suggest the presence of intertemporal effects. For instance, if taxes are high today, but expected to fall tomorrow, labor supply and output migh increase today. This does not seem to be the case because policy reversals in Italy are typically the result of temporary measures such as temporary tax amnesties.

[24]Guajardo et al (2011) also compare TB and EB adjustments and claim that this is the case. Their evidence, however, is based on the analysis of isolated shocks, rather than plans, a procedure which we have argued is incorrect, at least with these data.

Japan, Sweden, U.S. and Canada) and euro-area countries before 1999. The motivation for this divisions is that the common currency prevents monetary policy from responding to fiscal developments in member countries[25]. We therefore proceed to the estimation of the following system

$$
\begin{aligned}
\Delta z_{i,t} &= \alpha + \delta_k (L) \Delta i_t + B_{1k}(L) e^u_{i,t} * TB_{i,t} + B_{2k}(L) e^a_{i,t} * TB_{i,t} + \\
&\quad C_{1k}(L) e^u_{i,t} * EB_{i,t} + C_{2k}(L) e^a_{i,t} * EB_{i,t} + \\
&\quad + \sum_{j=1}^{3} \gamma_{jk} e^a_{i,t,j} * EB_{i,t} + \sum_{j=1}^{3} \delta_{jk} e^a_{i,t,j} * TB_{i,t} + \lambda_i + \chi_t + u_{i,t} \\
e^a_{i,t,1} &= \varphi_{1,i} e^u_{i,t} + v_{1,i,t} \\
e^a_{i,t,2} &= \varphi_{2,i} e^u_{i,t} + v_{2,i,t} \\
e^a_{i,t,3} &= \varphi_{3,i} e^u_{i,t} + v_{3,i,t} \\
k &= EMU,\ non\ EU\ plus\ Europe\ before\ EMU
\end{aligned}
\tag{3}
$$

In (3) the coefficients describing the responses of the relevant macro variables to fiscal plans are restricted to be equal within each group, euro and non-euro members, respectively. No restrictions are imposed between the two groups.

The impulse responses for output generated by the unrestricted system, reported in Figure 9, confirms the robustness of our baseline results showing an heterogenous effect of EB and TB plans. Interestingly, this robustness results emerge even if the panel restrictions are rejected[26].

Insert Figure 9

[25] A similar analysis is conducted by Jalil (2012). This paper considers fiscal shocks rather than fiscal plans and finds that the tax multiplier is of about 3, while the spending multiplier is close to zero in countries where monetary authorities are constrained in their ability to counteract shocks because they are in either a monetary union or a liquidity trap.

[26] In this estimation we have extended the sample to Sweden and Finland, the two countries which so far we had been excluded because of lack of some data. Introducing these two countries–and even doing so in a less restricted system— leaves the main result unaltered. This is confirmed when Sweden and Finland are included in the restricted model. The results are available by the authors upon request.

6.1.2 Counterfactual Evidence

Consider a simplified representation of the joint dynamics of output growth, Δy_t, the monetary policy variable (which for simplicity we denote MP_t), and of our narrative fiscal corrections consisting of both unanticipated and anticipated components

$$\begin{bmatrix} \Delta y_t \\ MP_t \end{bmatrix} = \begin{bmatrix} a_{11} & a_{12} \\ a_{21} & a_{22} \end{bmatrix} \begin{bmatrix} \Delta y_{t-1} \\ MP_{t-1} \end{bmatrix} + \begin{bmatrix} \varepsilon_t^y \\ \varepsilon_t^m \end{bmatrix}$$

$$\varepsilon_t^y = \beta_1 e_t^f * TB_t + \beta_2 e_t^f * EB_t + \beta_3 e_t^{nf} + e_t^y$$

$$\varepsilon_t^m = \gamma_1 e_t^f * TB_t + \gamma_2 e_t^f * EB_t + \gamma_3 e_t^{nf} + \gamma_4 e_t^y + e_t^m$$

The VAR innovations in the output growth equation, ε_t^y, include the narrative (structural) fiscal shocks, that are allowed to have heterogenous effects according to their nature, $e_t^f * TB_t$ and $e_t^f * EB_t$, non-fiscal shocks, e_t^{nf}, and a residual output shock e_t^y.that for our purposes we do not need to identify. The VAR innovations in the equation for the monetary policy variable, ε_t^m, include the same structural shocks affecting the output innovations, and a structural monetary shock e_t^m. This model makes the (usual) recursive assumption between macroeconomic variables and monetary policy — that is we assume that monetary policy reacts contemporaneously to macro shocks, but it takes at least one lag before monetary policy can affect macroeconomic outcomes. This assumption is standard in VAR models of the monetary transmission mechanism. In principle, the recursive assumption become less plausible the lower the frequency at which the data are observed. We shall check its plausibility by comparing the response of output to monetary policy derived in our extended empirical model with those available in the literature and based on higher frequency data.

The moving average representation for output growth, consistent with the above representation and truncated after two periods can be written as follows

$$
\begin{aligned}
\Delta y_t &= \beta_1 e_t^f TB_t + \beta_2 e_t^f EB_t + +\beta_3 e_t^{nf} + e_t^y + \\
&+a_{11}\left(\beta_1 e_{t-1}^f * TB_{t-1} + \beta_2 e_{t-1}^f * EB_{t-1} + \beta_3 e_{t-1}^{nf} + e_{t-1}^y\right) + \\
&+a_{12}\left(\gamma_1 e_{t-1}^f TB_{t-1} + \gamma_2 e_t^f EB_{t-1} + \gamma_3 e_{t-1}^{nf} + \gamma_4 e_{t-1}^y + e_{t-1}^m\right) + \\
&\left(a_{11}^2 + a_{12}a_{21}\right)\left(\beta_1 e_{t-2}^f TB_{t-2} + \beta_2 e_{t-2}^f EB_{t-2} + \beta_3 e_{t-2}^{nf} + e_{t-2}^y\right) + \\
&\left(a_{11}a_{12} + a_{12}a_{22}\right)\left(\gamma_1 e_{t-2}^f TB_{t-2} + \gamma_2 e_{t-2}^f EB_{t-2} + \gamma_3 e_{t-2}^{nf} + \gamma_4 e_{t-2}^y + e_{t-2}^m\right)
\end{aligned}
$$

As structural shocks are orthogonal to each other, projecting Δy_t on e_t^f, e_{t-1}^f and e_{t-2}^f allows us to obtain consistent estimates of the impulse responses of output growth to TB and EB adjustments

$$
\Delta y_t = \sum_{i=1}^{3} \hat{\delta}_{i,TB} e_{t-i+1}^f * TB_{t-i+1} + \sum_{i=1}^{3} \hat{\delta}_{i,EB} e_{t-i+1}^f * EB_{t-i+1} + v_{1t}, \tag{4}
$$

This regression is equivalent (in the context of our example) to the output growth equation estimated in (1) in Section 3.5. Its coefficients reflect both the *direct effect* of fiscal policy on output (that depends on β_1 and β_2) and the *indirect effect* that depends on the response of monetary policy to the fiscal adjustment, namely γ_1 and γ_2. These two channels can be separated by estimating the following augmented moving average model where we allow output growth to respond directly to lagged monetary policy innovations through the coefficients $\hat{\pi}$. This augmented specification allows to "counterfactually" shut down the indirect monetary policy channel and therefore assess its importance in determining the heterogenous effect of EB and TB adjustments on output

$$
\begin{aligned}
\Delta y_t &= \sum_{i=1}^{3} \hat{\pi}_{i,TB} e_{t-i+1}^f * TB_{t-i+1} + \sum_{i=1}^{3} \hat{\pi}_{i,EB} e_{t-i+1}^f * EB_{t-i+1} + \\
&+ \sum_{i=1}^{2} \hat{\pi}_{i,MP} \varepsilon_{t-i}^m + v_{2t}
\end{aligned} \tag{5}
$$

The following table compares the expected values of the coefficients estimated in (4) and (5) and illustrates how our augmented specification can be used to estimate the direct effect of fiscal policy on output controlling for the response of monetary policy to fiscal adjustments. [27]

Closing the Monetary Policy Channel				
baseline specification				
		$\frac{\partial \Delta y_t}{\partial e^f_t * F_i}$	$\frac{\partial \Delta y_t}{\partial e^f_{t-1} * F_i}$	$\frac{\partial \Delta y_t}{\partial e^f_{t-2} * F_i}$
$F_i = TB$		β_1	$a_{11}\beta_1 + a_{12}\gamma_1$	$(a_{11}^2 + a_{12}a_{21})\,\beta_1 + (a_{11}a_{12} + a_{121}a_{22})\,\gamma_1$
$F_i = EB$		β_2	$a_{11}\beta_2 + a_{12}\gamma_2$	$(a_{11}^2 + a_{12}a_{21})\,\beta_2 + (a_{11}a_{12} + a_{12}a_{22})\,\gamma_2$
augmented specification				
		$\frac{\partial \Delta y_t}{\partial e^f_t * F_i}\Big\vert_{\varepsilon^m_t = 0}$	$\frac{\partial \Delta y_t}{\partial e^f_{t-1} * F_i}\Big\vert_{\varepsilon^m_{t-1} = 0}$	$\frac{\partial \Delta y_t}{\partial e^f_{t-2} * F_i}\Big\vert_{\varepsilon^m_{t-2} = 0}$
$F_i = TB$		β_1	$a_{11}\beta_1$	$(a_{11}^2 + a_{12}a_{21})\,\beta_1$
$F_i = EB$		β_2	$a_{11}\beta_2$	$(a_{11}^2 + a_{12}a_{21})\,\beta_2$
		$\frac{\partial \Delta y_t}{\partial e^m_{t-1}}$	$\frac{\partial \Delta y_t}{\partial e^m_{t-2}}$	
		a_{12}	$(a_{11}a_{12} + a_{12}a_{22})$	
		a_{12}	$(a_{11}a_{12} + a_{12}a_{22})$	

[27] First moments of all estimated parameters are conditonal upon the regressors in the relevant specification.

Based on this analysis we have estimated an augmented version of (1) using Δi_t as a proxy for monetary innovations[28]

$$\begin{aligned}
\Delta z_{i,t} &= \alpha + \sum_{k=1}^{3} \delta_k \Delta i_{t-k} + B_1(L) e^u_{i,t} * TB_{i,t} + B_2(L) e^a_{i,t} * TB_{i,t} + \quad (6) \\
& C_1(L) e^u_{i,t} * EB_{i,t} + C_2(L) e^a_{i,t} * EB_{i,t} + \\
& + \sum_{j=1}^{3} \gamma_j e^a_{i,t,j} * EB_{i,t} + \sum_{j=1}^{3} \delta_j e^a_{i,t,j} * TB_{i,t} + \lambda_i + \chi_t + u_{i,t} \\
e^a_{i,t,1} &= \varphi_{1,i} e^u_{i,t} + v_{1,i,t} \\
e^a_{i,t,2} &= \varphi_{2,i} e^u_{i,t} + v_{2,i,t} \\
e^a_{i,t,3} &= \varphi_{3,i} e^u_{i,t} + v_{3,i,t}
\end{aligned}$$

Augmenting our baseline specification with lags of Δi_t allows us to compute the impulse response to the fiscal plans by zeroing the response of monetary policy to all innovations and in particular to fiscal adjustments. The distributed lag of Δi_t is significant in our output growth equation, but the effect of innovations in monetary policy on output are small relative to that of fiscal adjustments. The dynamic responses of output growth to the change in interest rates are described in the following table

The Dynamic Response of Δy_t to Δi_{t-i}			
period	*i=1*	*i=2*	*i=3*
coeff	*-0.22*	*-0.15*	*-0.12*
t-stat	*-8.73*	*-6.69*	*-4.73*

These coefficients show a significant negative but small response of output growth to changes in the monetary policy rate. Technically speaking the response described by the coefficients in the table is not directly comparable with usual impulse responses describing the effect of monetary policy on output, because they are responses to monetary policy innovations and not to exogenous monetary policy shocks. However, taking into account the well established fact that monetary policy innovations are strongly correlated to exogenous monetary policy shocks (see *e.g.*. Rudebusch 1998) it is interesting

[28] Using a proxy for monetary policy innovations we are able to capture a more general monetary policy reaction function than that adopted in the illustrative example above.

to note that the response implied by our estimated coefficients lies in between the typical response obtained on U.S. data (see *e.g.*. Christiano et al. 1998) and that obtained on euro area data, which is smaller than that observed for the U.S. (see *e.g.*. Peersman and Smets 2001).

The counterfactual exercise aimed at shutting down the response of monetary policy to fiscal innovations is implemented by setting Δi_{t-i} to zero. The impulse responses thus computed are reported in Figure 10 along with the responses obtained in the baseline model. The results in Figure 10 confirm the indications obtained estimating the baseline model. The conclusion is that the differential response of monetary policy to EB and TB adjustments cannot fully explain the different effect on output growth of EB and TB plans.[29]

Insert Figure 10 here

We have repeated this counterfactual experiment limiting the sample to the countries belonging to the euro area. Figure 11 shows that both the main evidence and the results of the counterfactual obtained by setting to zero the response of monetary policy to fiscal adjustments remain robust.

Insert Figure 11

6.2 Is the choice between TB and EB plans related to the cycle, or to accompanying reforms?

Some authors have found that the effects of fiscal contractions on output growth are asymmetric during economic expansions and recessions (see Auerbach and Gorodnichenko 2012, Bachmann and Sims 2011, Barro and Redlick 2011)[30]. Could the asymmetry we have documented between TB and EB plans be explained by the fact that the choice between the two types of adjustment is related to the cycle? In other words, is it the case that TB adjustments are chosen during recessions while EB ones are chosen during

[29]Note that some of these countries adopted the Euro therefore had an identical monetary policy for part of the period under consideration. Unfortunately we do not have enough cases of fiscal adjustment in the first decade of the Euro to use this feature of the data. it is in fact well known that after entering the monetary union, many countries relaxed rather than tighten their fiscal stance.

[30]A different results is however obtained by Ramey et al.(2013).

periods of economic expansions? In principle the narrative approach should eliminate the correlation of the adjustments with the cycle, but the point is more subtle. The type of fiscal consolidation may be unrelated to the cycle when it is decided, but somehow it could happen that EB are chosen during booms and TB during recessions, possibly by chance. We will show below that this is not the case. A second concern may arise because of the possibility that the asymmetry between TB and EB plans might be explained by the fact that EB plans (differently from TB ones) often are adopted as part of a wider set of market-oriented reforms, such as labor and product market liberalizations. It could be that such reforms, rather than the character of the fiscal plan, is the reason for the milder effects on output growth.

To address the first concern we use a measure of the cycle, defined as the deviation of output from its Hodrick-Prescott trend. To address the second we use an index of labor market reforms constructed by the OECD. We then run a binary choice (panel) probit regression of the dummies identifying TB and EB episodes on these two measures separately. We find no evidence of a relation between the cycle or the degree of labor market reforms and the choice whether to implement a TB adjustment. The coefficient on the cyclical variable is 0.04 with an associated standard error of 0.73. The McFadden R-square of the regression is 0.001. There is instead very mild evidence for an higher likelihood to choose an EB plan during a recession: the coefficient on the cyclical variable is -0.16 with an associated standard error of 0.07; the McFadden R-square is 0.01. Interestingly, the marginal significance of the cycle variable disappears when time dummies, capturing common shocks, are included in the specification. Summing up. Our main result is not driven by the endogeneity of the type of adjustment to the cycle.

Similar results are obtained studying the relation between the choice whether to adopt an EB or a TB plan and the OECD index of labor market reforms. Note that this result is not inconsistent with the evidence and the case studies in Perotti (2013) and Alesina and Ardagna (1998, 2012). These papers argue that amongst all the fiscal adjustments the least costly are those accompanied but some supply side reforms and by wage moderation. So, for instance, amongst the EB adjustments those which are the least costly or even expansionary are those accompanied by such reforms. Our result is different. What we find is that the difference between EB and TB cannot be explained by supply side reforms.

7 Conclusions

The main result of this paper is that while tax-based adjustments are associated with deep and long lasting recessions, expenditure-based adjustments are not. The output losses associated with the latter are very small, on average close to zero. This average is likely to be the result of some episodes of fiscal adjustment which are characterized by small output costs, and other which are accompanied by a (small) expansion. The aggregate demand component which reflects more closely the difference in the response of output to expenditure-based and tax-based adjustments is private investment. The confidence of investors also does not fall much after an expenditure-based adjustment, and promptly recovers and increases above the baseline. Instead it falls for several years after a tax-based adjustment. The differences between the two types of adjustments is not to be explained by a different response of monetary policy, and therefore it should not vanish at the zero lower bound. Nor is it explained by the cycle, or by systematically different choices of the supply side reforms that may accompany a fiscal correction. Finally, and importantly, we have shown that the correct methodology to answer the question What are the output effects of fiscal consolidations? is studying fiscal plans, rather than individual fiscal shocks as normally done in the literature.

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Table 1: Stabilization plans in Italy (i=IT) 1991-1993

time	$\tau^u_{i,t}$	$\tau^a_{i,t,0}$	$\tau^a_{i,t,1}$	$\tau^a_{i,t,2}$	$\tau^a_{i,t,3}$	$g^u_{i,t}$	$g^a_{i,t,0}$	$g^a_{i,t,1}$	$g^a_{i,t,2}$	$g^a_{i,t,3}$	TB	EB
1991	1.69	0	-1.26	0	0	1.08	0	0	0	0	0	1
1992	2.85	-1.26	-1.2	0	0	1.92	0	0	0	0	0	1
1993	3.2	-1.2	-0.57	0	0	3.12	0	0	0	0	0	1

Table 2: Stabilization plan in Australia (i=AU) in 1984

time	$\tau^u_{i,t}$	$\tau^a_{i,t,0}$	$\tau^a_{i,t,1}$	$\tau^a_{i,t,2}$	$\tau^a_{i,t,3}$	$g^u_{i,t}$	$g^a_{i,t,0}$	$g^a_{i,t,1}$	$g^a_{i,t,2}$	$g^a_{i,t,3}$	TB	EB
1985	0	0	0	0	0	0.45	0	0.45	0	0	0	1
1986	0.17	0	0.19	-0.27	0	0.4	0.45	0.26	-0.08	0	0	1
1987	0	0.19	-0.27	0	0	0.45	0.26	0.37	0	0	0	1
1988	0	-0.27	0	0	0	0	0.37	0	0	0	0	1

Table 3	Anticipated and unanticipated fiscal adjustments										years		plans
country	τ^u	$\tau^a_{i,t,0}$	$\tau^a_{i,t,1}$	$\tau^a_{i,t,2}$	$\tau^a_{i,t,3}$	$g^u_{i,t}$	$g^a_{i,t,0}$	$g^a_{i,t,1}$	$g^a_{i,t,2}$	$g^a_{i,t,3}$	TB	EB	
AU	4	7	7	3	1	5	6	6	3	1	2	8	5
OE	5	1	1	0	0	5	2	2	0	0	3	4	2
BG	7	4	4	0	0	10	4	4	0	0	3	8	3
CN	9	14	14	10	8	10	12	12	11	8	8	7	10
DE	9	6	6	3	0	9	6	6	3	1	8	8	3
DK	4	0	0	0	0	3	1	1	0	0	2	2	2
ES	7	3	3	0	0	7	2	2	0	0	5	5	2
FN	2	1	1	0	0	5	2	2	0	0	0	6	2
FR	5	5	5	3	1	4	2	2	0	0	7	5	4
IR	6	0	0	0	0	3	1	1	0	0	5	2	0
IT	12	6	6	0	0	12	0	0	0	0	3	9	5
JP	7	7	7	1	0	7	2	2	0	0	7	5	7
NL	9	2	2	0	0	11	2	2	0	0	1	12	4
PT	4	2	2	0	0	4	2	2	0	0	5	2	2
SW	3	4	4	2	1	3	4	4	2	1	0	7	2
UK	4	5	5	1	0	5	6	6	1	0	7	3	4
US	5	12	12	10	7	3	8	8	7	6	5	10	3
Tot.	102	79	79	33	18	106	62	62	27	17	71	103	60

NB A plan occurs when some unanticipated and anticipated adjustments are observed simulataneously or when some future adjuments are announced for the first time.

Table 4 Cross countries heterogeneity in the design of multi - year plans

	CAN	AUS	SWE	GBR	AUT	DNK	JPN	FRA
$\varphi_{1,i}$	0.99 (0.19)	0.85 (0.12)	0.48 (0.09)	0.34 (0.02)	0.31 (0.06)	0.24 (0.03)	0.27 (0.03)	0.18 (0.08)
$\varphi_{2,i}$	0.59 (0.097)	-0.14 (0.08)	0.31 (0.06)	0.04 (0.02)	0	0	-0.0005 (0.003)	-0.02 (0.04)
$\varphi_{3,i}$	0.022 (0.04)	-0.02 (0.01)	0.21 (0.02)	0	0	0	0	-0.03 (0.03)

	DEU	FIN	POR	USA	ESP	BEL	IRL	ITA
$\varphi_{1,i}$	0.11 (0.11)	0.11 (0.11)	0.07 (0.14)	0.07 (0.23)	0.06 (0.06)	0.04 (0.09)	0	-0.22 (0.04)
$\varphi_{2,i}$	-0.096 (0.08)	0	0	0.07 (0.16)	0	0	0	0
$\varphi_{3,i}$	0.03 (0.01)	0	0	-0.01 (0.12)	0	0	0	0

The following equations are estimated

$e^a_{i,t,1} = \varphi_{1,i} e^u_{i,t} + \upsilon_{1,i,t}$

$e^a_{i,t,2} = \varphi_{2,i} e^u_{i,t} + \upsilon_{2,i,t}$

$e^a_{i,t,3} = \varphi_{3,i} e^u_{i,t} + \upsilon_{3,i,t}$

$e^a_{i,t,j}$ are the corrections announced by the fiscal authorities of country i at date t with an anticipation horizon of j years (i.e. to be implemented in year $t + j$) for country i, $e^u_{i,t}$ are instead the unanticipated fiscal correction announced and implemented in year t by the fiscal authorities of country i.

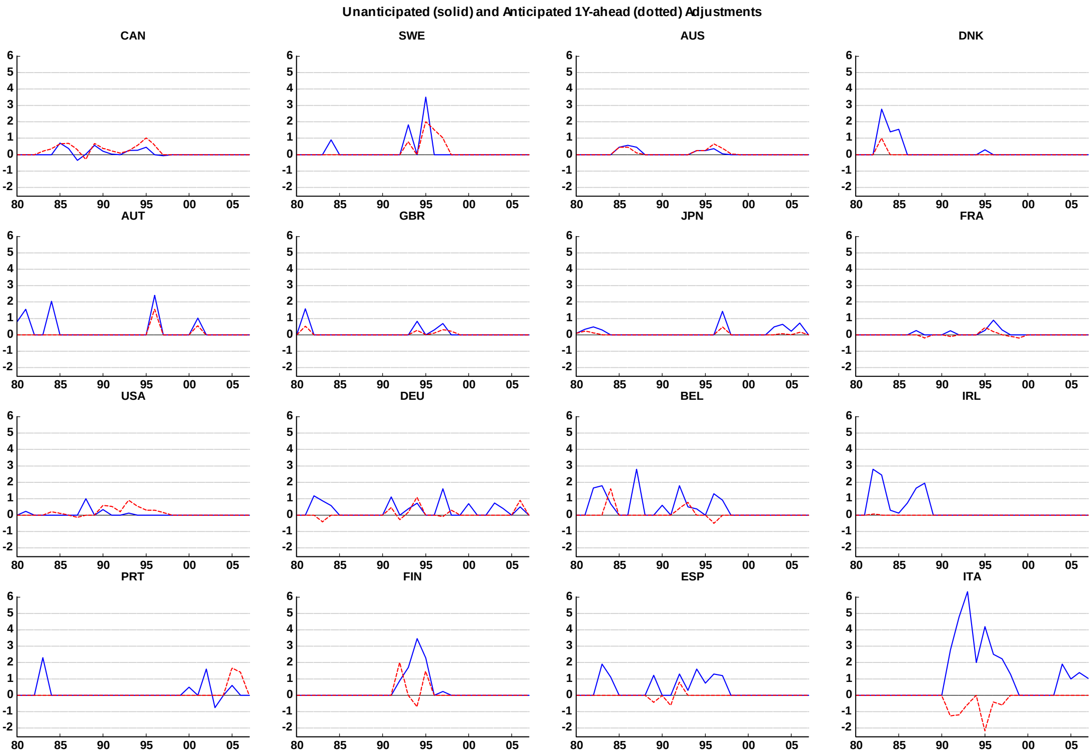

Figure 1: Unanticipated and Anticipated Fiscal Adjustments

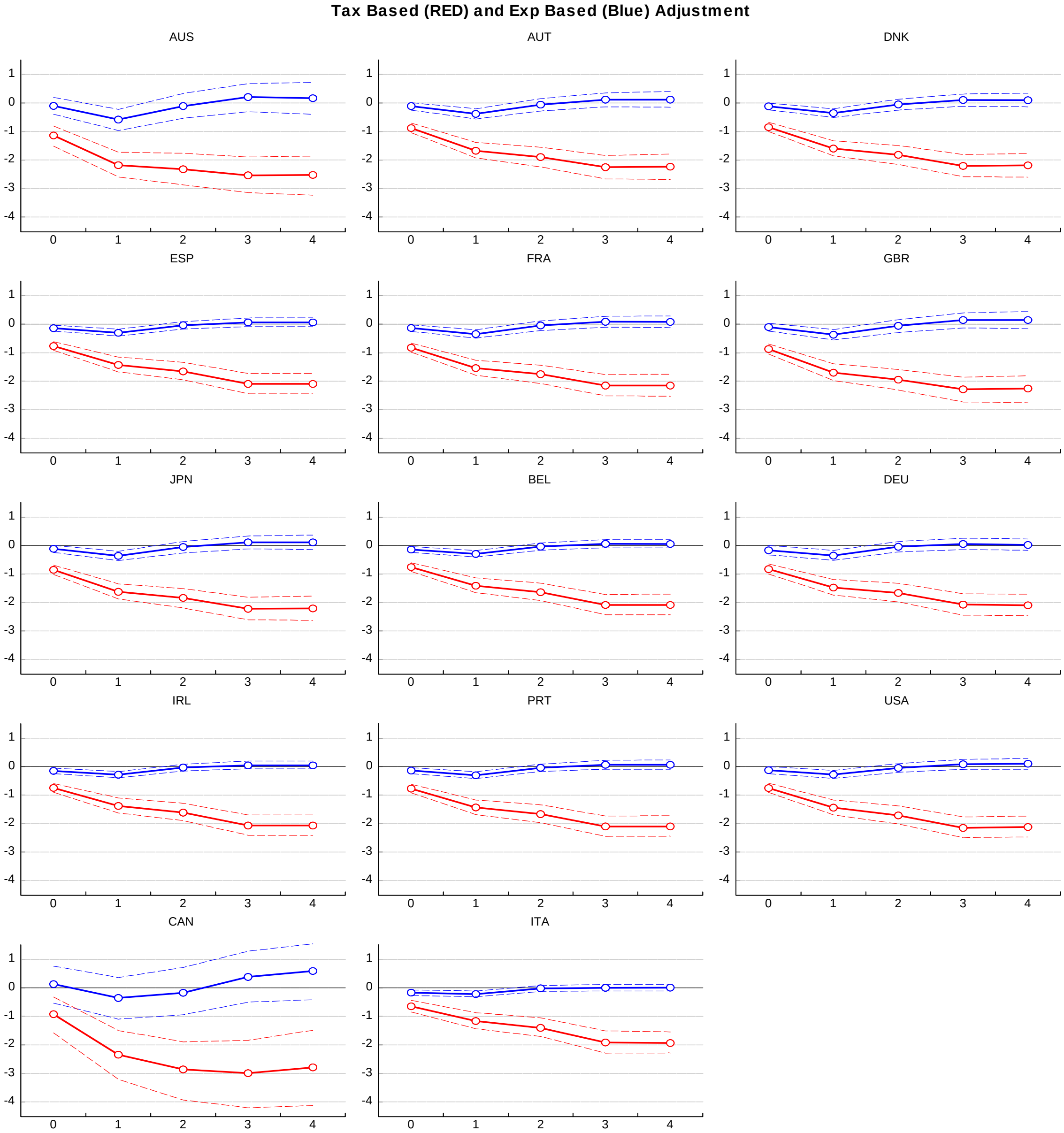

Figure 2: The effect of TB and EB adjustments on output growth

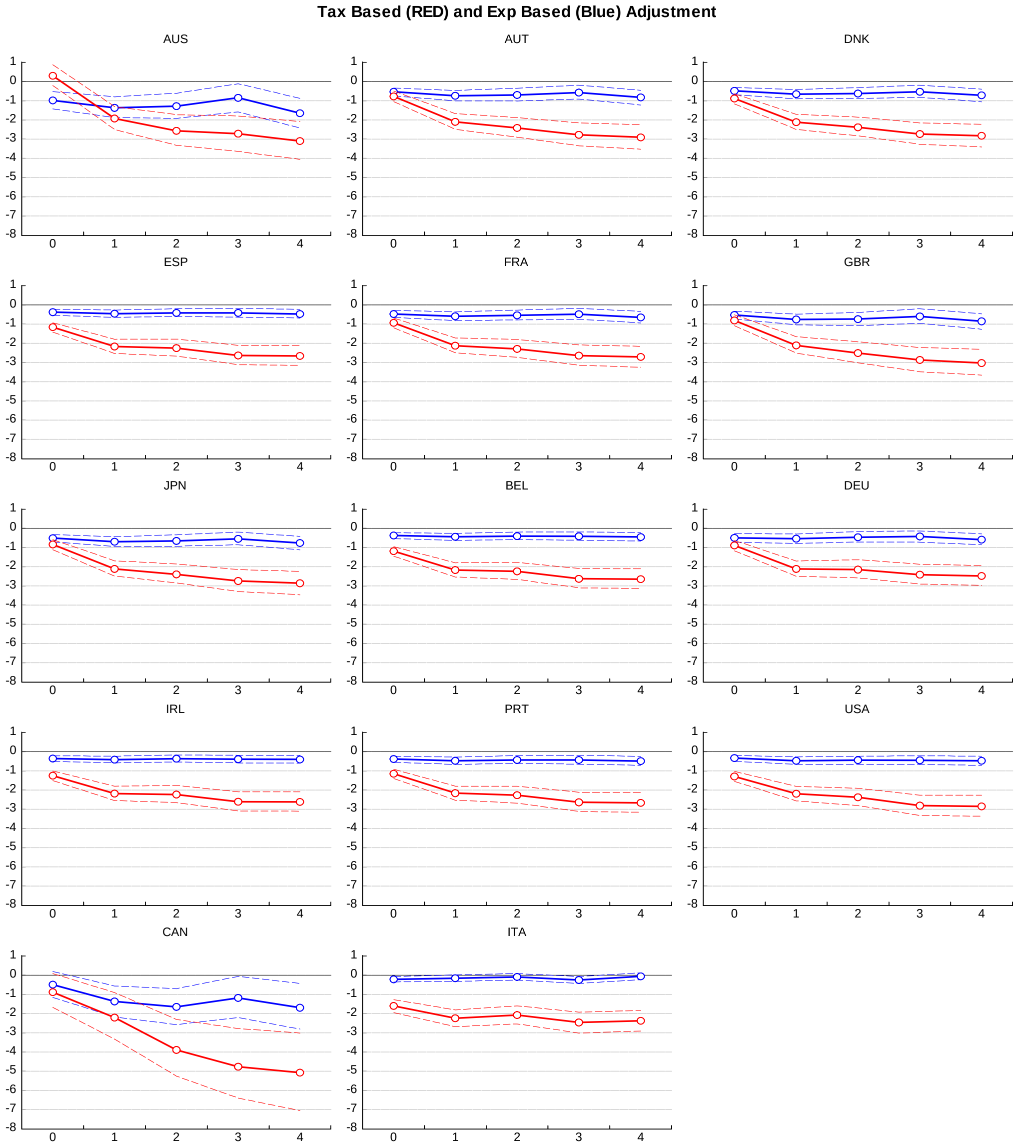

Figure 3: The effect of TB and EB adjustments on consumption growth

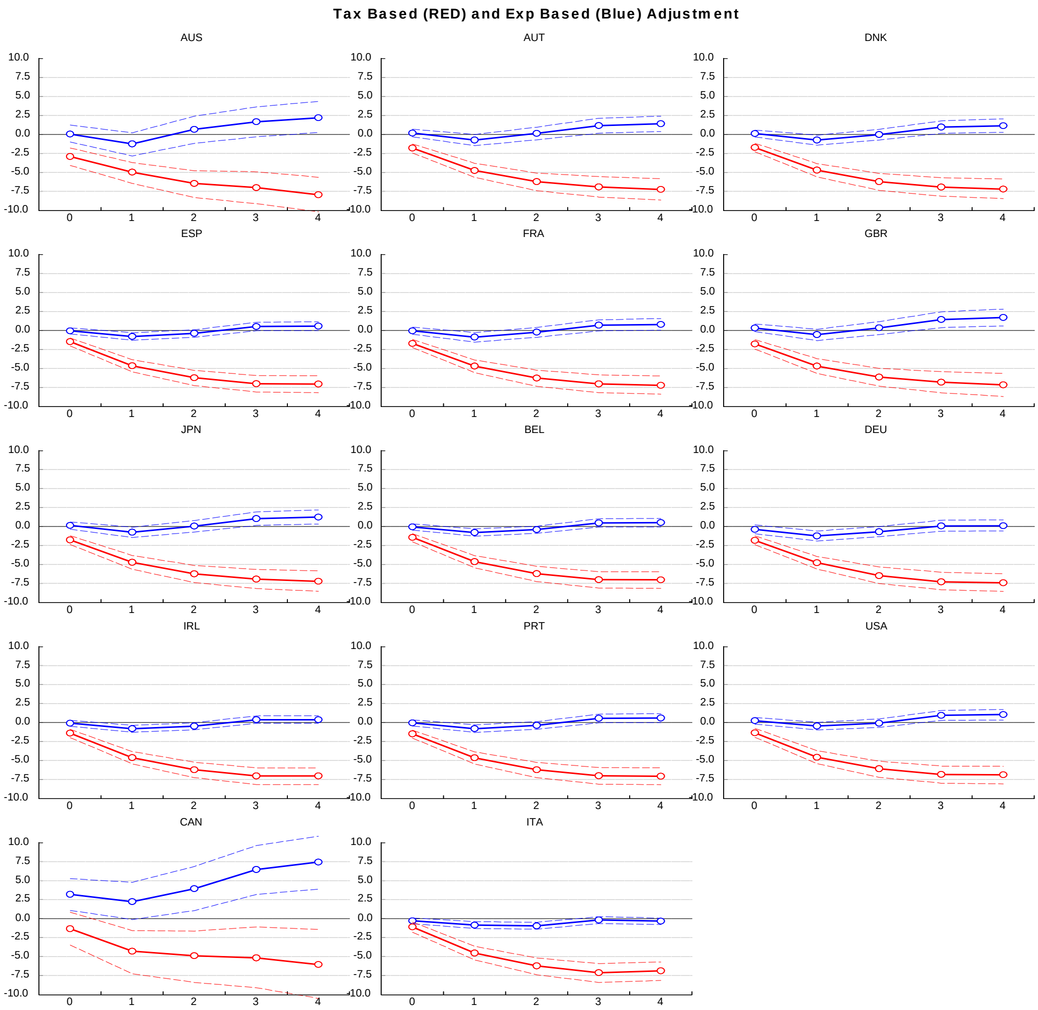

Figure 4: The effect of TB and EB adjustments on fixed capital formation growth

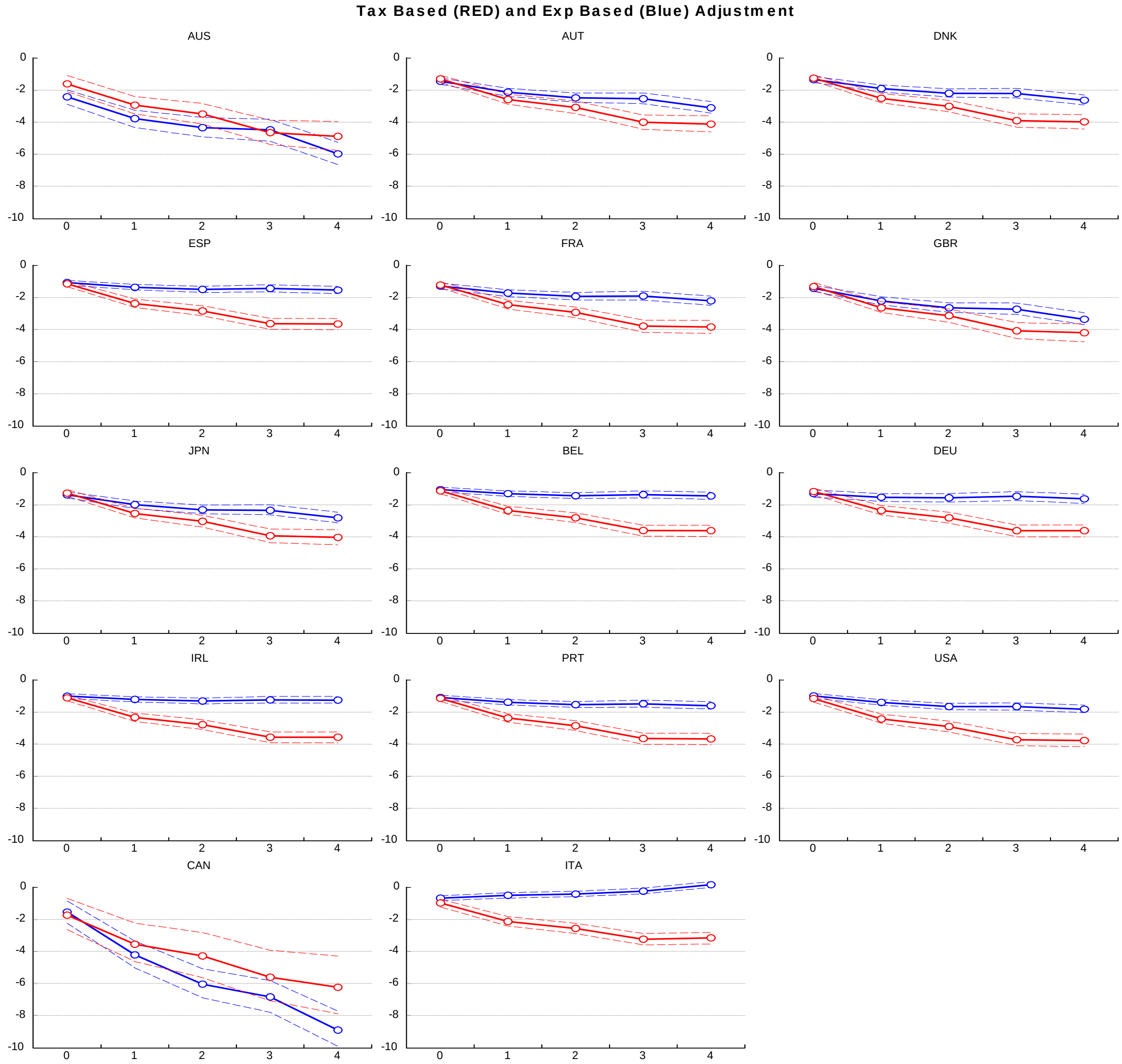

Figure 5: The effect of TB and EB adjustments on ESI Consumer Confidence

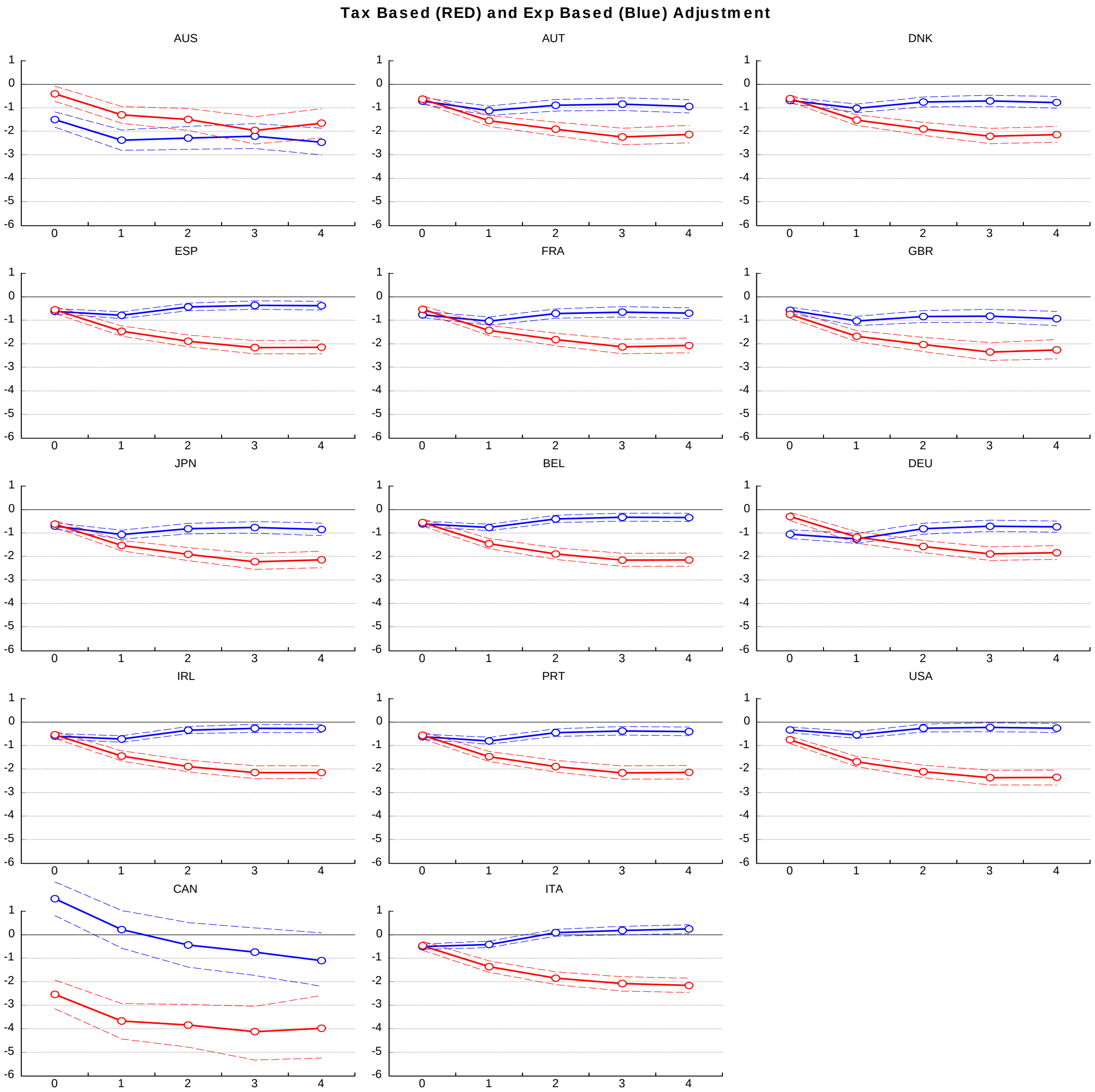

Figure 6: The effect of TB and EB adjustments on ESI Business Confidence

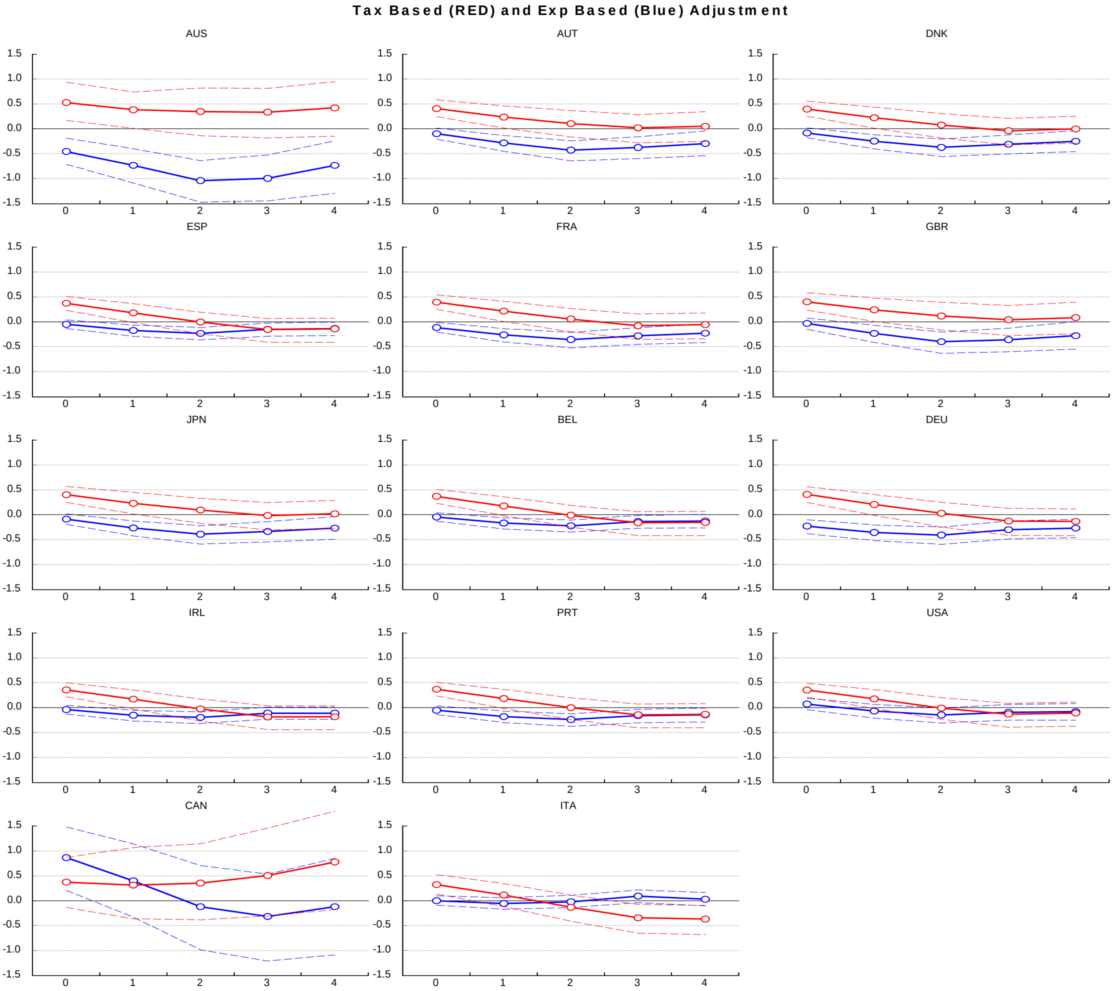

Figure 7: The effect of TB and EB adjustments on monetary policy (change in the 3M TBills Rates)

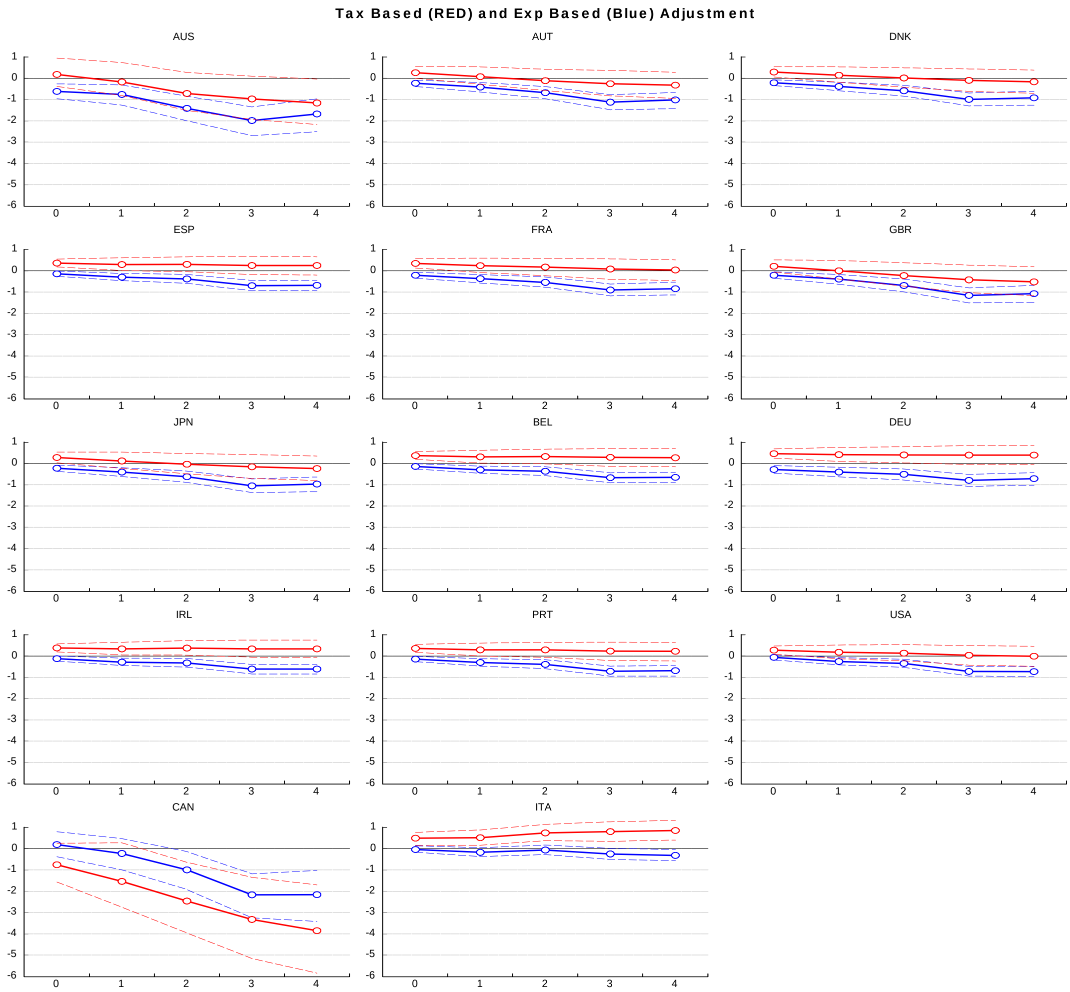

Figure 8: The effect of TB and EB adjustments on inflation (GDP deflator)

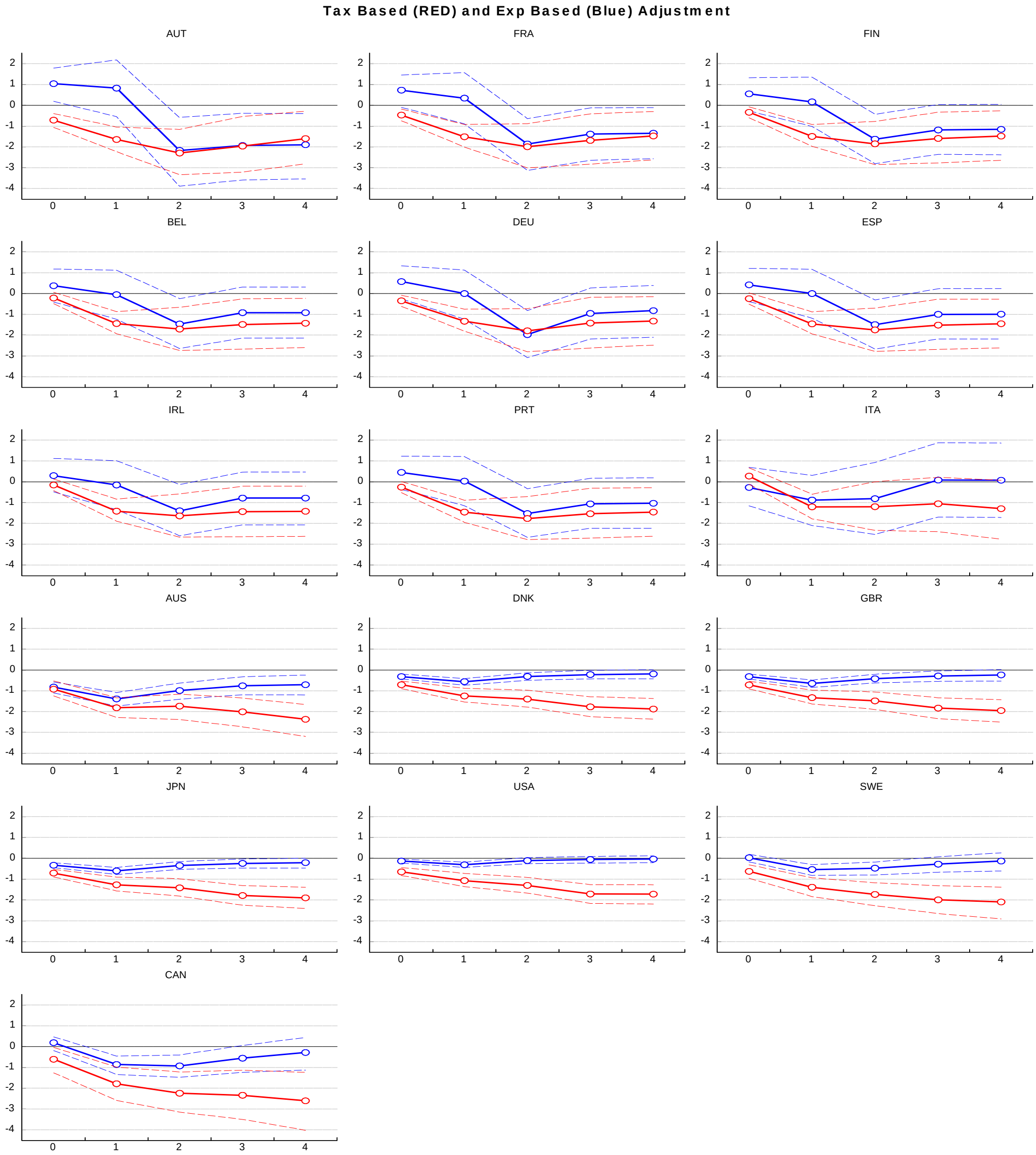

Figure 9: Impulse responses of output allowing for different coefficients in the euro area (top 9 countries) and non-euro area (bottom 7 countries)

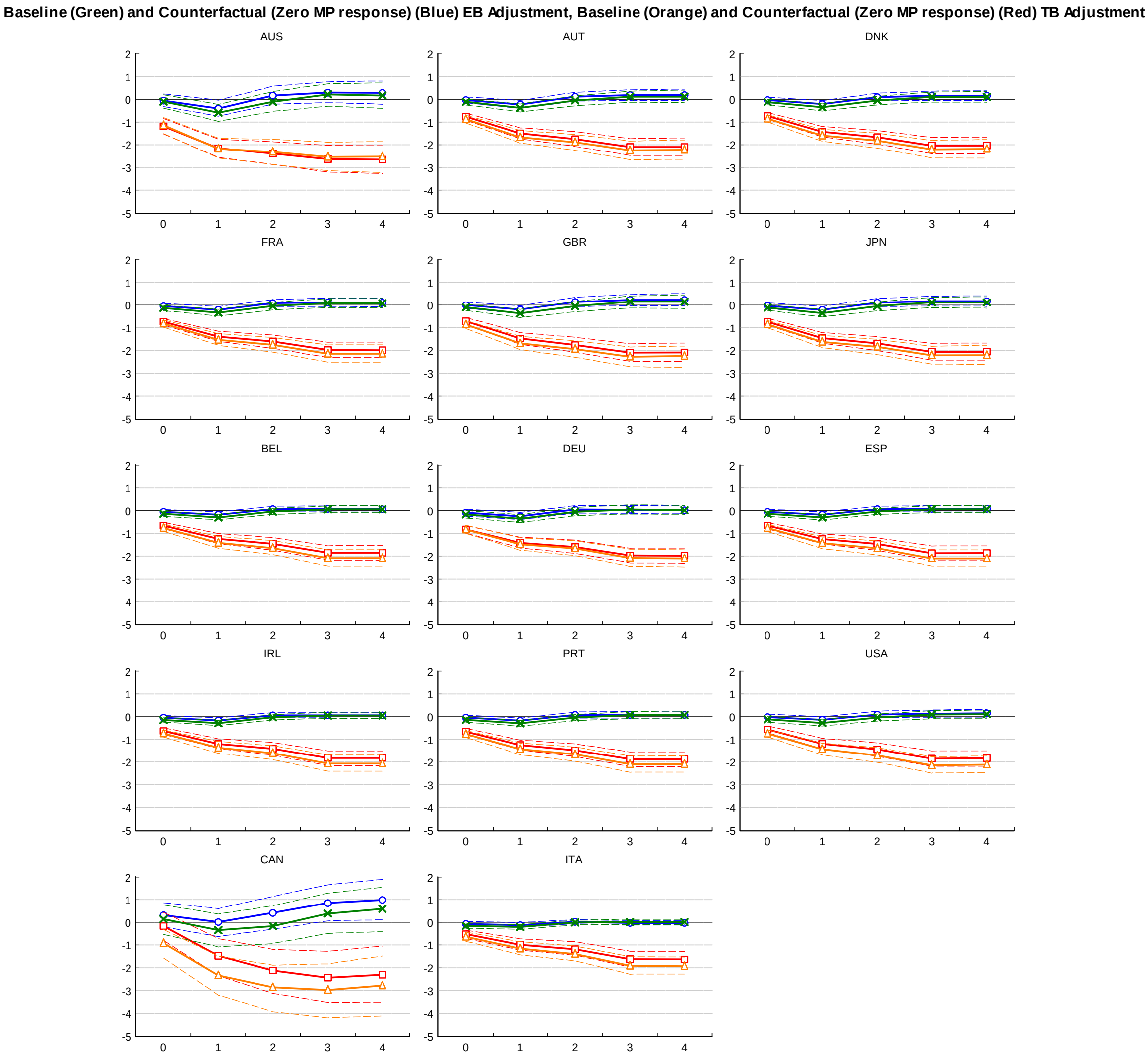

Figure 10: The effect of TB and EB adjustments: Baseline and Counterfactual

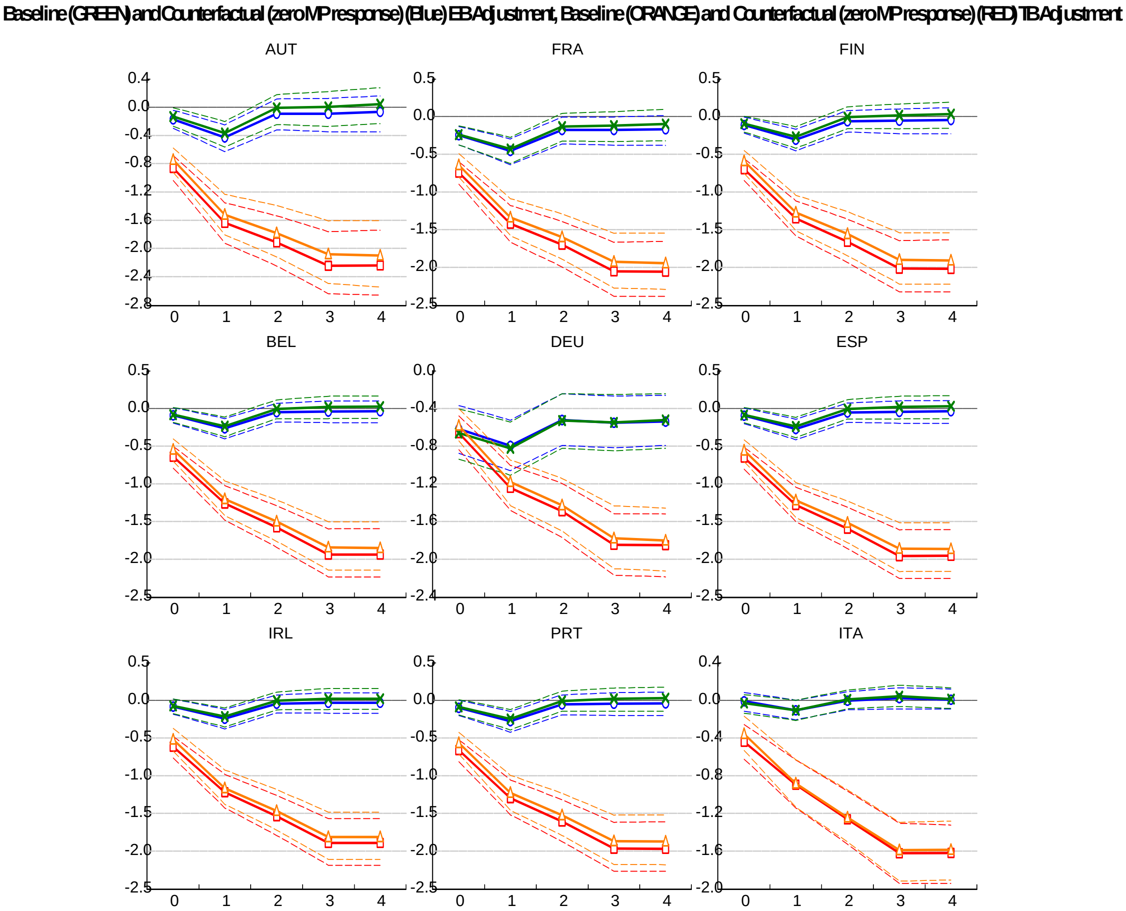

Figure 11: The effect of TB and EB adjustment both Baseline and Counterfactual for Europe

9 Data Appendix

Our data come from different public sources such as Thomson Reuters Datastream, the OECD Economic Outlook database, the Action-based Dataset of Fiscal Consolidations compiled by DeVries et al (2011), which provide us with the fiscal consolidation episodes, and the IMF International Financial Statistics (IFS). Datastream was used to obtain time series of the Economic Sentiment Indicators originally produced by the European Commission. This confidence index was integrated with national sources. The series for private final consumption expenditure and gross fixed capital formation are from IFS. The other macroeconomic variables from the OECD Economic Outlook database.

Macroeconomic and Confidence Data Sources		
Variable	Definition	Source
Consumer Confidence indicator	Economic Sentiment Indicator	European Commissio
Business Confidence Indicator	Economic Sentiment Indicator	European Commissio
Long Term Interest rate	10-Y Government bonds YTM	IMF IFS
Short-Term Interest rate	3-M Treasury Bill YTM	IMF IFS
Consumption	Total Final Consumption Expenditure	IMF IFS
Investment	Gross Private fixed Capital Formation	IMF IFS
Output	Gross Domestic Product	OECD
Population	Total Resident Population	OECD

The variables included as dependent variables, for each country i, in the multy country moving average specification to compute the dynamic effects of fiscal adjustments where the following:

1. Real per capita GDP growth is defined as

$$dy_{i,t} = log(\frac{y_{i,t}}{y_{i,t,-1}}) - log(\frac{popt_{i,t}}{popt_{i,t-1}})$$

   where $y_{i,t}$ is the real gdp at time t and $popt_{i,t}$ is the total population at time t.

2. Final per capita real consumption expenditure growth is

$$dfce_{i,t} = log(\frac{fce_{i,t}}{fce_{i,t-1}}) - log(\frac{popt_{i,t}}{popt_{i,t-1}})$$

   where $fce_{i,t}$ is the final real consumption expenditure at time t.

3. Gross capital formation per capita growth is the change in the log of real gross capital formation

$$dgcf_{i,t} = log(\frac{gcf_{i,t}}{gcf_{i,t-1}}) - log(\frac{popt_{i,t}}{popt_{i,t-1}})$$

where $dgcf_{i,t}$ is the real gross capital formation growth from time t-1 to time t and $gcf_{i,t}$ is the gross fixed capital formation at time t.

4. Consumer and business confidence indicators were defined in terms of logs.

$$lc_{i,t} = log(c_{i,t})$$
$$lb_{i,t} = log(b_{i,t})$$

where $lc_{i,t}$ is the log of the consumer confidence indicator at time t, $c_{i,t}$ is the consumer confidence indicator at time t, $lb_{i,t}$ is the log of the business confidence indicator, and b_t is the business confidence indicator at time t.

5. Term spreads are computed between the yield on long-term government bonds (ten-year) and the yield on short-term (three-month) bills

$$s_{i,t} = irl_{i,t} - irs_{i,t}$$

where $s_{i,t}$ is the spread at time t, $irl_{i,t}$ is the long-term government bond (ten-year) at time t, and $irs_{i,t}$ is the short-term (three-month) bill at time t.

9.1 From the Action-based Dataset of Fiscal Consolidations to Fiscal Plans

Table 1 illustrates how we obtained fiscal plans by reclassifying adjustments contained in the Action-based Dataset of Fiscal Consolidations of Devries et al. (2011).

The original database contains exogenous fiscal shifts in public revenues and spending with respect to the previous year. The episodes capture the changes in policy having effect in year t, compared to a baseline scenario of no policy change with respect to year t-1.

Although Devries et al. (2011) often specify the date of approval of the fiscal plans, when computing their impact they do not distinguish between measures that were announced in previous years or within the year of implementation. Hence, in order to take into account the multi-periodal feature of the fiscal plans, we exploit the information about the date of approval to distinguish between unexpected and expected shocks. The date of announcement is either found within the text or using the sources indicated by Devries et al. (2011). Due to the annual nature of the data we define as 'unanticipated' all the fiscal plans which have impact on the calendar year t and are approved between September of year t-1 and December of year t. All the plans approved before this window of time and supposed to have effect within the calendar year t are coded as 'anticipated'. For example, the Omnibus Budget Reconciliation Act of 1993 (OBRA-93) in the US was enacted on August 10, 1993 and had a budgetary impact (in percent of GDP) of 0.12 in 1993, 0.40 in 1994, 0.26 in 1995, 0.29 in 1996, 0.30 in 1997; 0.15 in 1998 (Devries et al. 2011). Hence, the impact in 1993 is coded as unexpected, while all the other shocks are coded as announced components with an impact horizon from one to five years.

Thanks to this classification we can consider the combined effect, in a given year, of current and future policies with an impact horizon of up to five years. After summing up these components for both taxes and expenditures, we take the largest between the two in order to label the episodes to either be expenditure-based or tax-based.

The sum of the 'unanticipated' and 'anticipated' components of the fiscal episodes is always equal to the original shocks by Devries et al.(2011).except for 7 cases in which we slightly diverge from Devries et al. (2011) for the following reasons:

Belgium 1996

Tax hikes of 0.5 percent of GDP revised in tax hikes of 0.8 percent of GDP. In entry 1996, Devries et al. (2011) compute a tax shock totaling 0.5 percent of GDP. However, as can be read at p.19, the budgetary impact of tax-based deficit-reduction measures in the 1996 Budget (1996 IMF Recent Economic Developments, p.11) were of 0.9 percent of GDP (of which 0.1 percent of GDP consisting in sales of buildings and not considered in the analysis). As a consequence the shock in revenues is of 0.8 percent of GDP rather than 0.5 percent of GDP.

Belgium 1997

Spending cuts of 0.5 percent of GDP revised in spending cuts of 0.25

percent of GDP and tax hikes of 0.41 percent of GDP revised in tax hikes of 0.16 percent of GDP. As clearly stated at p.19 of Devries et al. (2011), the Budget 1996 included one-off measures equal to 0.5 percent of GDP, which should be allocated equally across spending and tax measures. However, Devries et al. (2011) neglect to apply the expiration of these one-off measures in 1997. As a consequence, in 1997 the spending cuts should total 0.25 percent of GDP (0.5 percent of GDP from the 1997 Budget and -0.25 from the one-off measures expired in 1997) and tax hikes should be equal to 0.16 (0.41 percent of GDP introduced in the 1997 Budget and -0.25 from the expiration of previous one-off measures).

Canada 1984

Tax hikes of 0.27 percent of GDP revised in tax hikes of 0.2 percent of GDP. According to Table 1 p. 28 of Devries et al. (2011) the 1983 Budget approved C$ 1,215 million with impact in the fiscal year 1984-85. In order to allocate this amount in the calendar years 1984 and 1985 Devries et al. (2011) should impute C$ 3/4 billion in 1984 and C$ 1/4 billion in 1985. However, as can be seen in Table 1, all of the measures are allocated in 1984 and, in addition, C$ 1/4 are allocated in 1985. This procedure ends up counting 1/4 of the measures twice. As a consequence, we calculate again the impact of the 1993 Budget in 1994, with tax hikes of 0.20 percent of GDP instead of 0.27 percent of GDP.

Italy 1993

Spending cuts of 2 percent of GDP revised in spending cuts of 3.1 percent of GDP. Fiscal consolidation in 1993 was the result of two different packages: the Delegation Law and the May 1993 package. According to Devries et al. (2011) the Delegation Law included L 31 trillion of expenditure-based fiscal consolidation (1994 OECD Economic Surveys, p.44-45). However, the OECD report quantifies the expenditure cuts of the Delegation Law to actually be 43.5 trillion. The L 31 trillion is the primary deficit surplus of that year. As a consequence we revise the amount of spending cuts to be equal to 3.1 percent of GDP instead of 2 percent of GDP.

Italy 2004

Tax hikes of 0.67 percent of GDP revised in tax hikes of 1 percent of GDP and spending cuts of 0.63 percent of GDP revised in spending cuts of 0.9 percent of GDP. Devries et al. (2011) state at p.53 that the 2004

Budget introduced savings of €16 billion (0.7 percent of GDP) and additional measures decided in July 2004 amounting to €7.6 billion (0.6 percent of GDP). It is clear that the proportion between the total amounts of the two measures and their percentage over GDP is not consistent. As a consequence we revised the fiscal consolidation shock in 2004 to be 1 percent of GDP of tax hikes (instead of 0.67) and 0.9 percent of GDP of spending cuts (instead of 0.63).

Netherlands 1993

Tax cuts of 0.16 of GDP revised in tax hikes of 0.04 and spending cuts of 0.28 percent of GDP revised in spending cuts of 0.88 percent of GDP. At the very end of entry 1993 (p.64 of Devries et al. 2011) the total amount of tax hikes is computed as the sum of -0.39 percent of GDP coming from tax cuts decided in the 1993 Budget, +0.43 percent of GDP from additional measures introduced in 1993 and -0.2 percent of GDP that are not mentioned in the text. Probably, this -0.2 is considered to be the effect of the expiration of previous measures introduced in 1992. However, in entry 1992 these measures are declared to be exclusively spending cuts and indeed they are applied in the computation of the spending shock for 1993. As a consequence we revise the amount of tax hikes to be 0.04 percent of GDP (-0.39+0.43).

On the spending side we figured that there is a typo in the computation at the very end of entry 1993 at p.64. Indeed, 0.78+0.3-0.2 is equal to 0.88 percent of GDP rather than 0.28 percent of GDP.

United Kingdom 1997

Spending cuts of 0.16 percent of GDP revised in spending cuts of 0.26 percent of GDP. Devries et al. (2011) neglect to apply the impact of 0.1 percent of GDP of spending cuts corresponding to a quarter of the measures decided in the FSBR 1996-97 (see entry for 1996 and 1997 p. 77). As a consequence, we consider the total amount of spending cuts introduced in 1997 to be 0.26 percent of GDP (0.16+0.1).

Table 1: Classification of fiscal adjustments

		Total	Tax	Spend	Tax					Spend					TB	EB
					u,t	a,t	a,t+1	a,t+2	a,t+3	**u,t**	a,t	a,t+1	a,t+2	a,t+3		
AUS	1985	0,45	0,00	0,45	0	0	0	0	0	0,45	0	0,45	0	0	0	1
AUS	1986	1,02	0,17	0,85	0,17	0	0,19	-0,27	0	0,4	0,45	0,26	-0,08	0	0	1
AUS	1987	0,90	0,19	0,71	0	0,19	-0,27	0	0	0,45	0,26	0,37	0	0	0	1
AUS	1988	0,10	-0,27	0,37	0	-0,27	0	0	0	0	0,37	0	0	0	0	1
AUS	1994	0,25	0,25	0,00	0,25	0	0,25	0	0	0	0	0	0	0	1	0
AUS	1995	0,50	0,50	0,00	0,25	0,25	0,25	0	0	0	0	0	0	0	1	0
AUS	1996	0,62	0,34	0,28	0,09	0,25	0,175	0,05	-0,04	0,275	0	0,475	0,17	-0,03	0	1
AUS	1997	0,70	0,18	0,53	0	0,175	0,05	-0,04	0	0,05	0,475	0,32	0,07	0	0	1
AUS	1998	0,37	0,05	0,32	0	0,05	-0,04	0	0	0	0,32	0,07	0	0	0	1
AUS	1999	0,04	-0,04	0,07	0	-0,04	0	0	0	0	0,07	0	0	0	0	1
AUT	1980	0,80	0,11	0,69	0,11	0	0	0	0	0,69	0	0	0	0	0	1
AUT	1981	1,56	0,50	1,06	0,5	0	0	0	0	1,06	0	0	0	0	0	1
AUT	1984	2,04	1,30	0,74	1,3	0	0	0	0	0,74	0	0	0	0	1	0
AUT	1996	2,41	0,88	1,53	0,88	0	0,44	0	0	1,53	0	1,12	0	0	0	1
AUT	1997	1,56	0,44	1,12	0	0,44	0	0	0	0	1,12	0	0	0	0	1
AUT	2001	1,02	0,90	0,12	0,9	0	0	0	0	0,12	0	0,55	0	0	1	0
AUT	2002	0,55	0,00	0,55	0	0	0	0	0	0	0,55	0	0	0	0	1
BEL	1982	1,66	0,00	1,66	0	0	0	0	0	1,66	0	0	0	0	0	1
BEL	1983	1,79	0,69	1,10	0,69	0	0	0	0	1,1	0	0	0	0	0	1
BEL	1984	0,69	0,28	0,41	0,28	0	0,73	0	0	0,41	0	0,88	0	0	0	1
BEL	1985	1,61	0,73	0,88	0	0,73	0	0	0	0	0,88	0	0	0	0	1
BEL	1987	2,80	0,00	2,80	0	0	0	0	0	2,8	0	0	0	0	0	1
BEL	1990	0,60	0,40	0,20	0,4	0	0	0	0	0,2	0	0	0	0	1	0
BEL	1992	1,79	0,99	0,80	0,99	0	0,03	0	0	0,8	0	0,39	0	0	0	1
BEL	1993	0,92	0,43	0,49	0,4	0,03	0,55	0	0	0,1	0,39	0,23	0	0	1	0
BEL	1994	1,15	0,55	0,60	0	0,55	0	0	0	0,37	0,23	0	0	0	0	1
BEL	**1996**	**1,30**	**0,80**	**0,50**	**0,8**	**0**	**-0,25**	**0**	**0**	**0,5**	**0**	**-0,25**	**0**	**0**	**1**	**0**
BEL	**1997**	**0,41**	**0,16**	**0,25**	**0,41**	**-0,25**	**0**	**0**	**0**	**0,5**	**-0,25**	**0**	**0**	**0**	**0**	**1**
CAN	**1983**	**0,00**	**0,00**	**0,00**	**0**	**0**	**0,203**	**0,351**	**0,227**	**0**	**0**	**0**	**0**	**0**	**1**	**0**
CAN	**1984**	**0,20**	**0,20**	**0,00**	**0**	**0,203**	**0,351**	**0,227**	**0,044**	**0**	**0**	**0**	**0**	**0**	**1**	**0**
CAN	1985	1,03	0,53	0,50	0,203	0,325	0,594	0,308	0,046	0,502	0	0,053	0,071	0,0365	1	0
CAN	1986	0,99	0,84	0,15	0,279	0,563	0,537	0,148	0,018	0,1	0,05	0,147	0,055	0,0017	1	0
CAN	1987	0,28	0,14	0,14	-0,35	0,492	0,32	-0,23	0,088	0	0,135	-0,03	-0,095	-0,0284	1	0
CAN	1988	0,30	0,33	-0,03	0,034	0,292	-0,2	0,082	2E-04	0	-0,03	-0,09	-0,026	0	1	0
CAN	1989	0,31	0,24	0,08	0,421	-0,18	0,588	0,127	1E-03	0,156	-0,08	0,087	0,052	0,0108	1	0
CAN	1990	0,86	0,57	0,29	0	0,569	0,123	9E-04	0	0,207	0,084	0,25	0,042	-0,0041	1	0
CAN	1991	0,40	0,13	0,27	0,011	0,122	-0,01	0	0	0,022	0,248	0,228	0,093	0,0196	0	1
CAN	1992	0,21	-0,01	0,22	0	-0,01	-0,01	0	0	0	0,224	0,099	0,019	0	0	1
CAN	1993	0,35	-0,01	0,36	0	-0,01	-0,02	0	0	0,263	0,095	0,255	0,083	0,0146	0	1
CAN	1994	0,49	0,04	0,45	0,056	-0,02	0,098	0,04	0,004	0,213	0,24	0,469	0,302	0,0606	0	1
CAN	1995	0,99	0,18	0,81	0,082	0,094	0,098	0,03	0	0,368	0,446	0,917	0,525	0	0	1
CAN	1996	0,97	0,09	0,88	0	0,095	0,029	0	0	-0,01	0,888	0,538	0	0	0	1
CAN	1997	0,47	0,01	0,47	-0,02	0,027	0	0	0	-0,04	0,51	0	0	0	0	1
DEU	1982	1,18	0,56	0,62	0,56	0	0	-0,41	0	0,62	0	0	0	0	0	1
DEU	1983	0,87	0,30	0,57	0,3	0	-0,41	0	0	0,57	0	0	0	0	0	1
DEU	1984	0,18	-0,41	0,59	0	-0,41	0	0	0	0,59	0	0	0	0	0	1
DEU	1991	1,11	1,08	0,03	1,08	0	0,27	-0,46	0	0,03	0	0,19	0,18	0,18	1	0
DEU	1992	0,46	0,27	0,19	0	0,27	-0,46	0	0	0	0,19	0,18	0,18	0	0	1
DEU	1993	0,11	-0,07	0,18	0,39	-0,46	0	0,77	0	0	0,18	0,18	0,11	0	1	0
DEU	1994	0,91	0,08	0,83	0,08	0	0,84	0	0	0,65	0,18	0,245	0	0	0	1
DEU	1995	1,09	0,84	0,25	0	0,84	0	0	0	0	0,245	0	0	0	1	0
DEU	1997	1,60	0,50	1,10	0,5	0	0	0	0	1,1	0	-0,1	0	0	0	1
DEU	1998	-0,10	0,00	-0,10	0	0	0,3	0	0	0	-0,1	0	0	0	1	0
DEU	1999	0,30	0,30	0,00	0	0,3	0	0	0	0	0	0	0	0	1	0
DEU	2000	0,70	-0,05	0,75	-0,05	0	0	0	0	0,75	0	0	0	0	0	1
DEU	2003	0,74	0,74	0,00	0,74	0	0	0	0	0	0	0	0	0	1	0
DEU	2004	0,40	-0,70	1,10	-0,7	0	0	0	0	1,1	0	0	0	0	0	1
DEU	2006	0,50	0,00	0,50	0	0	0,5	0	0	0,5	0	0,4	0	0	0	1

Table 1: Classification of fiscal adjustments

		Total	Tax	Spend	Tax					Spend					TB	EB
					u,t	a,t	a,t+1	a,t+2	a,t+3	**u,t**	a,t	a,t+1	a,t+2	a,t+3		
DNK	1983	2,77	0,92	1,85	0,92	0	0	0	0	1,85	0	1	0	0	0	1
DNK	1984	2,38	0,67	1,71	0,67	0	0	0	0	0,71	1	0	0	0	0	1
DNK	1985	1,54	0,77	0,77	0,77	0	0	0	0	0,77	0	0	0	0	1	0
DNK	1995	0,30	0,30	0,00	0,3	0	0	0	0	0	0	0	0	0	1	0
ESP	1983	1,90	1,90	0,00	1,9	0	0	0	0	0	0	0	0	0	1	0
ESP	1984	1,12	0,37	0,75	0,37	0	0	0	0	0,75	0	0	0	0	0	1
ESP	1989	1,22	0,98	0,24	0,98	0	-0,28	0	0	0,24	0	-0,15	0	0	1	0
ESP	1990	-0,40	-0,25	-0,15	0	-0,25	0	0	0	0	-0,15	0	0	0	1	0
ESP	1991	0,00	0,00	0,00	0	0	-0,6	0	0	0	0	0	0	0	1	0
ESP	1992	0,70	0,30	0,40	0,9	-0,6	0,5	0	0	0,4	0	0,3	0	0	1	0
ESP	1993	1,10	0,80	0,30	0,3	0,5	0	0	0	0	0,3	0	0	0	1	0
ESP	1994	2,40	0,80	1,60	0,3	0,5	0	0	0	1,6	0	0	0	0	0	1
ESP	1995	0,74	0,00	0,74	0	0	0	0	0	0,74	0	0	0	0	0	1
ESP	1996	1,30	0,20	1,10	0,2	0	0	0	0	1,1	0	0	0	0	0	1
ESP	1997	1,20	0,10	1,10	0,1	0	0	0	0	1,1	0	0	0	0	0	1
FIN	1992	0,91	0,00	0,91	0	0	0	0	0	0,91	0	2,005	0	0	0	1
FIN	1993	3,71	0,00	3,71	0	0	0	0	0	1,705	2,005	0	0	0	0	1
FIN	1994	3,46	0,69	2,77	0,69	0	-0,69	0	0	2,77	0	0	0	0	0	1
FIN	1995	1,65	-0,63	2,28	0	-0,63	0	0	0	2,28	0	1,47	0	0	0	1
FIN	1996	1,47	0,00	1,47	0	0	0	0	0	0	1,47	0	0	0	0	1
FIN	1997	0,23	-0,70	0,93	-0,7	0	0	0	0	0,93	0	0	0	0	0	1
FRA	1979	0,85	0,85	0,00	0,85	0	0	0	0	0	0	0	0	0	1	0
FRA	1987	0,26	-0,50	0,76	-0,5	0	0	-0,2	0	0,76	0	0	0	0	0	1
FRA	1988	0,00	0,00	0,00	0	0	-0,2	0	0	0	0	0	0	0	1	0
FRA	1989	-0,20	-0,20	0,00	0	-0,2	0	0	0	0	0	0	0	0	1	0
FRA	1991	0,25	0,00	0,25	0	0	0	0	0	0,25	0	-0,1	0	0	0	1
FRA	1992	-0,10	0,00	-0,10	0	0	0	0	0	0	-0,1	0	0	0	0	1
FRA	1995	0,28	0,43	-0,15	0,43	0	0,45	0	0	-0,15	0	0	0	0	1	0
FRA	1996	1,34	0,87	0,47	0,42	0,45	0,11	0	0	0,47	0	0,09	0	0	1	0
FRA	1997	0,50	0,41	0,09	0,3	0,11	0	-0,1	-0,2	0	0,09	0	0	0	1	0
FRA	1998	0,00	0,00	0,00	0	0	-0,1	-0,2	0	0	0	0	0	0	1	0
FRA	1999	-0,10	-0,10	0,00	0	-0,1	-0,2	0	0	0	0	0	0	0	1	0
FRA	2000	-0,20	-0,20	0,00	0	-0,2	0	0	0	0	0	0	0	0	1	0
GBR	1979	0,27	-0,45	0,72	-0,45	0	-0,13	0	0	0,72	0	0,21	0	0	0	1
GBR	1980	0,08	-0,13	0,21	0	-0,13	0	0	0	0	0,21	0	0	0	0	1
GBR	1981	1,58	1,43	0,16	1,425	0	0,475	0	0	0,155	0	0,053	0	0	1	0
GBR	1982	0,53	0,48	0,05	0	0,475	0	0	0	0	0,053	0	0	0	1	0
GBR	1994	0,83	0,68	0,15	0,675	0	0,225	0	0	0,15	0	0,05	0	0	1	0
GBR	1995	0,28	0,23	0,05	0	0,225	0	0	0	0	0,05	0	0	0	1	0
GBR	1996	0,30	0,00	0,30	0	0	0	0	0	0,3	0	0,1	0	0	0	1
GBR	**1997**	**0,79**	**0,53**	**0,26**	**0,53**	**0**	**0,3**	**0,206**	**0**	**0,16**	**0,1**	**0,01**	**0,005**	**0**	**1**	**0**
GBR	1998	0,31	0,30	0,01	0	0,3	0,206	0	0	0	0,01	0,005	0	0	1	0
GBR	1999	0,21	0,21	0,01	0	0,206	0	0	0	0	0,005	0	0	0	1	0
IRL	1982	2,80	2,54	0,26	2,54	0	0	0	0	0,26	0	0,06	0	0	1	0
IRL	1983	2,50	2,44	0,06	2,44	0	0	0	0	0	0,06	0	0	0	1	0
IRL	1984	0,29	0,29	0,00	0,29	0	0	0	0	0	0	0	0	0	1	0
IRL	1985	0,12	0,12	0,00	0,12	0	0	0	0	0	0	0	0	0	1	0
IRL	1986	0,74	0,74	0,00	0,74	0	0	0	0	0	0	0	0	0	1	0
IRL	1987	1,65	0,53	1,12	0,53	0	0	0	0	1,12	0	0	0	0	0	1
IRL	1988	1,95	0,00	1,95	0	0	0	0	0	1,95	0	0	0	0	0	1
ITA	1991	2,77	1,69	1,08	1,69	0	-1,26	0	0	1,08	0	0	0	0	0	1
ITA	1992	3,51	1,59	1,92	2,85	-1,26	-1,2	0	0	1,92	0	0	0	0	0	1
ITA	**1993**	**5,12**	**2,00**	**3,12**	**3,2**	**-1,2**	**-0,57**	**0**	**0**	**3,12**	**0**	**0**	**0**	**0**	**0**	**1**
ITA	1994	1,43	-0,27	1,70	0,3	-0,57	0	0	0	1,7	0	0	0	0	0	1
ITA	1995	4,20	2,41	1,79	2,41	0	-2,16	0	0	1,79	0	0	0	0	0	1
ITA	1996	0,35	-0,74	1,09	1,42	-2,16	-0,41	0	0	1,09	0	0	0	0	1	0
ITA	1997	1,82	0,89	0,93	1,3	-0,41	-0,6	0	0	0,93	0	0	0	0	0	1
ITA	1998	0,68	0,01	0,67	0,61	-0,6	0	0	0	0,67	0	0	0	0	0	1

Table 1: Classification of fiscal adjustments

		Total	Tax	Spend	Tax					Spend					TB	EB
					u,t	a,t	a,t+1	a,t+2	a,t+3	**u,t**	a,t	a,t+1	a,t+2	a,t+3		
JPN	1979	0,12	0,12	0,00	0,115	0	0,123	0,031	0	0	0	0	0	0	1	0
JPN	1980	0,21	0,21	0,00	0,09	0,123	0,091	0	0	0	0	0	0	0	1	0
JPN	1981	0,43	0,43	0,00	0,342	0,091	0,227	0	0	0	0	0	0	0	1	0
JPN	1982	0,71	0,31	0,40	0,085	0,227	0,057	0	0	0,398	0	0,065	0	0	0	1
JPN	1983	0,42	0,06	0,37	0	0,057	0	0	0	0,3	0,065	0	0	0	0	1
JPN	1997	1,43	0,98	0,45	0,975	0	0,325	0	0	0,45	0	0,15	0	0	1	0
JPN	1998	0,48	0,33	0,15	0	0,325	0	0	0	0	0,15	0	0	0	1	0
JPN	2003	0,48	0,00	0,48	0	0	0	0	0	0,48	0	0	0	0	0	1
JPN	2004	0,64	0,19	0,45	0,188	0	0,063	0	0	0,45	0	0	0	0	0	1
JPN	2005	0,28	0,06	0,22	0	0,063	0	0	0	0,22	0	0	0	0	0	1
JPN	2006	0,72	0,45	0,27	0,45	0	0,15	0	0	0,27	0	0	0	0	1	0
JPN	2007	0,15	0,15	0,00	0	0,15	0	0	0	0	0	0	0	0	1	0
NLD	1981	1,75	0,53	1,22	0,53	0	0	0	0	1,22	0	0	0	0	0	1
NLD	1982	1,71	0,00	1,71	0	0	0	0	0	1,71	0	0	0	0	0	1
NLD	1983	3,24	0,49	2,75	0,49	0	0	0	0	2,75	0	0	0	0	0	1
NLD	1984	1,76	0,00	1,76	0	0	0	0	0	1,76	0	0	0	0	0	1
NLD	1985	1,24	0,00	1,24	0	0	0	0	0	1,24	0	0	0	0	0	1
NLD	1986	1,74	0,00	1,74	0	0	0	0	0	1,74	0	0	0	0	0	1
NLD	1987	1,48	1,48	0,00	1,48	0	-0,3	0	0	0	0	0	0	0	1	0
NLD	1988	0,05	-0,70	0,75	-0,4	-0,3	0	0	0	0,75	0	0	0	0	0	1
NLD	1991	0,87	0,87	0,00	0,87	0	-0,61	0	0	0	0	0,82	0	0	0	1
NLD	1992	0,74	-0,58	1,32	0,03	-0,61	0	0	0	0,5	0,82	-0,2	0	0	0	1
NLD	**1993**	**0,92**	**0,04**	**0,88**	**0,04**	**0**	**0**	**0**	**0**	**1,08**	**-0,2**	**0**	**0**	**0**	**0**	**1**
NLD	2004	1,70	0,40	1,30	0,4	0	0	0	0	1,3	0	0	0	0	0	1
NLD	2005	0,50	0,20	0,30	0,2	0	0	0	0	0,3	0	0	0	0	0	1
PRT	1983	2,30	1,35	0,95	1,35	0	0	0	0	0,95	0	0	0	0	1	0
PRT	2000	0,50	0,00	0,50	0	0	0	0	0	0,5	0	0	0	0	0	1
PRT	2002	1,60	1,20	0,40	1,2	0	0	0	0	0,4	0	0	0	0	1	0
PRT	2003	-0,75	-0,75	0,00	-0,75	0	0	0	0	0	0	0	0	0	1	0
PRT	2005	0,60	0,52	0,08	0,52	0	1,1	0	0	0,08	0	0,55	0	0	1	0
PRT	2006	1,65	1,10	0,55	0	1,1	0,5	0	0	0	0,55	0,9	0	0	1	0
PRT	2007	1,40	0,50	0,90	0	0,5	0	0	0	0	0,9	0	0	0	0	1
SWE	1984	0,90	0,21	0,69	0,21	0	0	0	0	0,69	0	0	0	0	0	1
SWE	1993	1,81	0,42	1,39	0,42	0	0,19	0	0	1,392	0	0,586	0	0	0	1
SWE	1994	0,78	0,19	0,59	0	0,19	0	0	0	0	0,586	0	0	0	0	1
SWE	1995	3,50	1,40	2,10	1,4	0	0,8	0,6	0,4	2,1	0	1,2	0,9	0,6	0	1
SWE	1996	2,00	0,80	1,20	0	0,8	0,6	0,4	0	0	1,2	0,9	0,6	0	0	1
SWE	1997	1,50	0,60	0,90	0	0,6	0,4	0	0	0	0,9	0,6	0	0	0	1
SWE	1998	1,00	0,40	0,60	0	0,4	0	0	0	0	0,6	0	0	0	0	1
USA	1978	0,14	0,14	0,00	0,135	0	0	0,062	0	0	0	0	0	0	1	0
USA	1979	0,00	0,00	0,00	0	0	0,062	0	0	0	0	0	0	0	1	0
USA	1980	0,06	0,06	0,00	0	0,062	0	0	0	0	0	0	0	0	1	0
USA	1981	0,23	0,23	0,00	0,23	0	0	0	0	0	0	0	0	0	1	0
USA	1983	0,00	0,00	0,00	0	0	0	0,21	0,096	0	0	0	0	0	1	0
USA	1984	0,00	0,00	0,00	0	0	0,21	0,096	0	0	0	0	0	0	1	0
USA	1985	0,21	0,21	0,00	0	0,21	0,096	0	0	0	0	0	0	0	1	0
USA	1986	0,10	0,10	0,00	0	0,096	0	0	0	0	0	0	0	0	1	0
USA	1987	0,00	0,00	0,00	0	0	-0,15	0	0	0	0	0	0	0	1	0
USA	1988	0,85	0,39	0,46	0,54	-0,15	0	0	0	0,46	0	0	0	0	0	1
USA	1990	0,33	0,26	0,07	0,26	0	0,29	0,24	-0,02	0,07	0	0,29	0,29	0,214	0	1
USA	1991	0,58	0,29	0,29	0	0,29	0,24	-0,02	0,07	0	0,29	0,29	0,214	0,43	0	1
USA	1992	0,53	0,24	0,29	0	0,24	-0,02	0,07	0,02	0	0,29	0,214	0,43	0,25	0	1
USA	1993	0,32	0,08	0,23	0,1	-0,02	0,4	0,19	0,075	0,02	0,214	0,5	0,34	0,215	0	1
USA	1994	0,90	0,40	0,50	0	0,4	0,19	0,075	0,06	0	0,5	0,34	0,215	0,24	0	1
USA	1995	0,53	0,19	0,34	0	0,19	0,075	0,06	-0,02	0	0,34	0,215	0,24	0,17	0	1
USA	1996	0,29	0,08	0,22	0	0,075	0,06	-0,02	0	0	0,215	0,24	0,17	0	0	1
USA	1997	0,30	0,06	0,24	0	0,06	-0,02	0	0	0	0,24	0,17	0	0	0	1
USA	1998	0,15	-0,02	0,17	0	-0,02	0	0	0	0	0,17	0	0	0	0	1

10 Appendix 2: The computation of impulse responses

We compute impulse responses for our empirical model as the difference between two model based forecasts: those obtained conditionally upon a fiscal adjustment plan and those obtained when there is no fiscal plan.

1. generation of a baseline simulation for all variables by solving dynamically forward the estimated system setting all shocks to zero;

2. generation of an alternative simulation for all variables by giving a one per cent of GDP shock to $e^u_{i,t}$, and letting all anticipated shocks react endogenously according to the φ coefficients. Solve dynamically forward the model for the alternative scenarios up to the same horizon used in the baseline simulation;

3. computation of impulse responses as the difference between the simulated values in the two steps described above;

4. computation of confidence intervals by block bootstrapping[31], preserving the cross-country correlation between the $\mu_{i,t}$ in each replication of the bootstrap–that is bootstrapping two rows of residuals at the time.[32]

[31] As suggested by Oscar Jorda, we use block bootstrap to take into account the possibility of autocorrelation in the residuals of the estimated system. In fact, the evidence for autocorrelation in the residuals is very weak and block bootstrapping makes very little difference for our empirical results.

[32] Bootstrapping requires saving the residuals from the estimated model and then iterating the following steps: a) re-sample rows of the saved residuals and generate a set of observations for all variables, b) re-estimate the model; c) compute impulse responses going through the steps described in the text; d) go back to step a). By going thruogh 1,000 iterations we produce bootstrapped distributions for impulse responses and compute confidence intervals.