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IGIER – Università Bocconi, Via Guglielmo Röntgen 1, 20136 Milano –Italy
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The output effect of fiscal consolidations

Alberto Alesina, Carlo Favero and Francesco Giavazzi *

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Abstract

Fiscal consolidations achieved by means of spending cuts are much less costly in terms of output losses than tax-based ones. The difference cannot be explained by accompanying policies, including monetary policy, and it is mainly due to the different response of business confidence and private investment. We obtain these results by studying the effects of the adoption of fiscal consolidation plans (rather than isolated shocks), that is combinations of tax increases and spending cuts, some unanticipated, other anticipated, in a sample of 16 OECD economies..

Keywords: fiscal adjustment, output, confidence, investment
JEL Classification: H60, E62

*Alberto Alesina: Harvard University, IGIER-Bocconi, CEPR and NBER. aalesina@harvard.edu. Carlo Favero: Deutsche Bank Chair, IGIER-Bocconi, and CEPR. carlo.favero@unibocconi.it. Francesco Giavazzi: IGIER-Bocconi, CEPR and NBER, francesco.giavazzi@unibocconi.it. We thank Silvia Ardagna, Olivier Blanchard, Giancarlo Corsetti, Marco Del Negro, Daniel Leigh, Roberto Perotti, Andrea Pescatori, , Christopher Sims, Guido Tabellini, and participants in seminars at European Central Bank, Bank of Italy, Federal Reserve Bank of San Francisco, Federal Reserve Bank of New York, London Buisness School, Northwestern university, NYU Stern, Bocconi, and the NBER conferences on Sovereign Debt and Financial Crises and The European Crises. Diana Morales, Madina Karamysheva and Andrea Passalacqua provided outstanding research assistance. This paper is part of the Growth and Sustanaibility Policies for Europe (GRASP) project (#244725) funded by the European Commission's 7th Framework Programme.

1 Introduction

Do sharp reductions of deficits and government debts (labeled "fiscal adjustments" or "fiscal consolidations") cause large output losses? The present paper shows that the answer depends on how the consolidation occurs. We analyze the experience of sixteen OECD countries over a quarter of a century (1978 to 2009) and we find that spending-based adjustments have been associated, on average, with mild and short-lived recessions, in many cases with no recession. Instead, tax-based adjustments have been followed by prolonged and deep recessions. The difference is remarkable in its size and we find that it cannot be explained by different monetary policies during the two types of adjustment. This suggests that this difference could still hold at the zero-lower-bound (ZLB) when the central bank is prevented from accompanying the fiscal contraction with a cut in interest rates. It is certainly possible that at the ZLB both types of adjustment might be more costly because the central bank cannot help as much, but the difference between tax-based and spending-based adjustments should persist even when nominal interest rates are stuck at zero, given that monetary policy has relatively little to do with it. The heterogeneity in the effects of the two types of fiscal adjustment (tax-based and spending-based) appears to be mainly due to the response of private investment, rather than that of consumption.[1] 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 expenditure-based adjustments.

Measuring the effect of fiscal consolidations requires to identify a sample of episodes of exogenous shifts in fiscal stance. This has been done in two ways. One treats exogenous shifts in fiscal policy as unobservables, and identifies them by imposing restrictions on reduced form dynamic specifications of macroeconomic and fiscal variables. The relevant restrictions can be derived by exploiting institutional features of the fiscal system (Blanchard and Perotti, 2002), or by imposing sign restrictions — derived from economic theory — on the shape of the response of the economy to the fiscal stimulus (Mountford and Uhlig, 2009). A different line of research adopts a narrative approach to identify observable exogenous shifts in fiscal stance, and then estimates their effect on macroeconomic variables running simple regressions

[1] This result is consistent with Alesina et al (2007).

(Romer and Romer 2010, Devries at al. 2011).

In the present paper we follow the narrative approach but we introduce an important innovation: we consider multi year fiscal adjustment plans rather than yearly fiscal shocks. In fact fiscal consolidations almost never consist of isolated shifts in either taxes or government spending. Most often they come in the form of fiscal plans which extend over a number of years and include both unanticipated changes in taxes and spending and the announcement of changes that will be implemented in subsequent years (anticipated). In order to construct these plans we use the information in Devries et al. (2011), a data set which documents exogenous shifts in fiscal policy applying the narrative approach to a set of seventeen countries. These policy shifts are documented using the records available in official documents to identify the size, timing and principal motivation for the fiscal actions taken or announced by each country. Amongst all these fiscal actions, these authors have selected those that were designed to reduce a budget deficit and/or to put the public debt on a sustainable path. They excluded all fiscal policy actions motivated by the current state of the economy, *e.g.* a fiscal contraction adopted because the economy was over-heating. This should guarantee their "exogeneity" for the estimation of the output multipliers. In fact, we test for exogeneity and we find that — in all countries with the exception of the Netherlands which we drop — these fiscal policy shifts are indeed uncorrelated with past realizations of output.

Different countries adopt different styles when it comes to designing multi-year fiscal consolidation plans. Some follow stop and go policies other countries adopts fiscal policy plans followed consistently over a number of years. We will show that it makes a difference. Also the analysis of multi-year fiscal plans allows us to make progress on the question of anticipated versus unanticipated shifts in fiscal policy (an issue whose importance has been highlighted by Ramey 2011a), and permanent versus transitory shifts. In our thirty-year sample there are only a few plans per country. Thus, in order to obtain precise-enough estimates, we pool together fiscal adjustments from different countries. [2] We allow two sources of heterogeneity. The first is a within-country heterogeneity with respect to the type of fiscal adjustments (tax-based or spending-based), the second is between-countries heterogeneity in the way fiscal policy is conducted (stop-and-go or multi-year consistent

[2] For a detailed discussion of issues of pooling in empirical models of fiscal policy see Favero, Giavazzi and Perego (2012).

plans). By allowing for these sources of heterogeneity we find that spending-based fiscal adjustments are much less recessionary than tax-based ones.

These findings are consistent with the literature, opened 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 spending based-fiscal adjustments—differently from tax-based ones—can have very small or no output costs at all. [3] Those results were typically 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. Thus, in order to rule out the possibility that our results have been determined by an heterogenous endogenous response of monetary policy to the different types of fiscal adjustment, we run a counter-factual experiment. We shut down the response of interest rates to the fiscal contraction, thus investigating what the output response to a fiscal contraction would be if interest rates were prevented from falling. We continue to find that spending-based adjustments are less costly than tax-based ones even when monetary policy is not allowed to react to the adjustment.

Can the difference between tax based and spending based adjustment be a spurious effect due the cycle? In principle the narrative approach should eliminate the correlation of the fiscal adjustment to the cycle but it could be that, even if unrelated to the cycle when they are decided, spending-based adjustments are chosen during booms and tax-based ones during recessions, possibly by chance. We investigate this possibility and we show that the possibility that our findings are driven by the endogeneity of the type of adjustment to the cycle can be excluded. Similarly we investigate, and we exclude, the possibility that our findings are driven by the endogeneity of the type of adjustment to other concomitant reforms, labor market reforms in particular.

[3] 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 unlikely to be endogenous to output fluctuations and thus an indication of active policies to reduce deficits. This, admittedly imperfect, approach was criticized by Devries et al (2011) who then set out to build their narrative data. Interestingly, while Devires 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).

The paper is organized as follows. The next section briefly reviews the theory behind the effects of different types of fiscal adjustment. Section 3 describes the data and shows the results obtained from a first pass at the data. Section 4 describes the way we used the data to construct fiscal plans and illustrates our estimation strategy. Section 4 reports our results. Section 5 discusses a number of robustness checks. The last section concludes.

2 Tax-based and spending-based stabilizations: a brief literature review

In neoclassical models the direct effect of fiscal policy on output, generated by accounting identities, is compounded with effects working through wealth, intertemporal substitution and distortions. These three channels operate differently in the case of tax-based or expenditure-based adjustments. In a simple real business cycle model with lump sum taxes, where 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. This result however can vanish if taxes are distortionary, for instance if government spending is financed with distortionary taxes which directly or indirectly affect the return to capital.

The literature considering the effects of fiscal policy on the components of aggregate demand has typically focused on consumption. An exception is Alesina et al. (2002) who analyze (theoretically and empirically) the differential effects of spending cuts and tax increases on investment. Now lower government spending means lower taxes on capital, higher investment and possibly higher output. The size of such effects will be different according to the transitory or permanent nature of the change in expenditure (Baxter and King 1993). 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. Also 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 could also play a role on investment (and perhaps on consumption as well). In fact a related strand of the literature emphasizing the importance of uncertainty for output fluctuations (Bloom 2009, Dixit and Pindyck 1994), paves the way to the possibility of an heterogenous effect of different types of fiscal adjustments, mainly through an investment channel. 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. The reduction in uncertainty regarding fiscal sustainabilty is also related to the degree of persistence of shifts in fiscal policy. On the contrary, stop-and-go policies may increase rather than decrease uncertainty.

Recent research on the effects of fiscal policy focuses on what might be different at ZLB. When interest rates are stuck at zero, and prices are inflexible, as in the New Keynesian model, the effects of fiscal policy come to resemble those predicted by the textbook Keynesian model where 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. Christiano, Eichenbaum and Rebelo (2011) calculate that when the ZLB 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 because not all firms can adjust prices all the time: 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. The empirically literature gives a different message, suggesting 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), suggesting that fiscal adjustments are less likely to be costless if started during a downturn.

Finally a different strand of the literature emphasizes the role of accompanying policies. One, as we already discussed, is of course monetary policy (DeVries et al. 2011). Alesina and Ardagna (1998, 2012) and Perotti (2012) 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. Fiscal adjustments are often complex policy "packages". Permanent cuts in government spending are often a sign of a decisive government willing to undertake sharp and courageous reform programs. On the contrary, temporary measures, for instance the announcement that spending cuts will be reversed, could signal less courageous reform programs.

3 Identification of Exogenous Shifts in Fiscal Policy

Recent contributions to the literature on the effects of fiscal policy have identified exogenous policy shifts either adopting structural VAR methods or "narrative" approaches.[4] We follow the second strategy for several reasons. First, as mentioned above, fiscal adjustments are typically introduced via multi-year plans, which include unanticipated and anticipated components, and only the narrative approach allows us to identify these two components.[5]

[4] For a useful review of the literature see Ramey (2013), the discussion by Perotti (2013) and the Introduction in Alesina and Giavazzi (2013).

[5] As is well known, using the narrative record to identify fiscal shocks we do not need to invert the MA representation of a VAR. This is important because fiscal foresight might make the MA representation of a VAR non inevertible, thus preventing the identification of shocks. In other words, the VAR-based identification of shocks relies on the assumption

Second, with the narrative approach we can distinguish between different types of stabilizations, more or less persistent or stop and go. Permanent shifts in fiscal policy occur when we observe a positive 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: the fiscal corrections introduced upon the announcement of a plan are at least partially reversed in the following years. Note that the assumption underlying traditional analysis of the effect of non anticipated fiscal shocks (see, for example, Mertens and Ravn 2011) is a lack of correlation between the unanticipated and the future anticipated shifts in taxes or spending; we show below that this assumption is violated. Third, the shocks identified via a narrative method are model independent and therefore are not affected by the possibility that some variables might be omitted in the estimation. Obviously the narrative method relies on an accurate reading of the intention and action of the policymakers.

3.1 The data

We use the fiscal consolidation episodes identified in DeVries et al. (2011) 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.[6] DeVries et al. (2011) 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 demand, 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 gov-

that the agents' and the econometrician's information sets are aligned, an assumption that fails in the presence of anticipated shfits in policy. Leeper et al (2008) illustrate that fiscal foresight could cause a misalignment of the two information sets, thus making it impossible to extract meaningful shocks from statistical innovations in the VAR.

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

ernments 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 (CBO) and, for instance, the Economic Report of the President for the United States and the Journal Officiel de la Republique Francaise for France. Two examples 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 Euro Area fiscal 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 government. For three federal countries (Canada, Australia and the United States) the data only refer to the central government (e.g. the Federal government or 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 database and cannot easily be recovered. Fortunately there are only fiver instances in our sample in which this happened, that is individual announcements were not recorded, 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 De-Vries et al. (2010) 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.[7] This is the correct way to measure spending cuts if we want to capture the effect of new information.

However, the criteria used by DeVries et al. (2011) 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. DeVries et al. (2011), instead, consider both expenditure and revenue shocks and focus only on fiscal actions motivated by the objective of reducing a budget deficit. 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.[8] Finally we run a simple check to assess whether the adjustments identified by DeVries et al. (2011) 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 predicted by past output growth is the Netherlands, which we drop from the sample. [9]

Summing up. The DeVries et al. (2011) data contain, over the period1978-

[7] 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".

[8] 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.

[9] 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.

2009, a total of 563 individual shifts in government spending tax revenues (unanticipated and anticipated) for seventeen countries. 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 the remaining 14 countries: four non European countries, the United States, Canada, Australia and Japan; two EU countries that are not members of the monetary union, Denmark and the United Kingdom and eight Euro area countries: Germany, France, Italy, Spain, Austria, Belgium, Ireland, Portugal.

3.2 A first pass at the data: overlooking fiscal plans

In order to facilitate the comparison of our findings with the existing literature we start by estimating a model which is as close as possible to the one estimated by Romer and Romer (2010). We first reproduce the Romer and Romer results over a sample of quarterly data (1980:1–2006:2) that matches the one used in this paper, which is shorter. The results are reported in equation (1). The left-hand side variable is output growth in year t, τ_t^{RR} are the tax shocks identified by Romer and Romer (2010), defined as the expected revenue effect of each exogenous change in the tax code, measured as a percent of year t GDP. These shocks sum up unanticipated shifts in taxes (the effect on year t of measures announced and implemented in year t) and shocks also announced in year t but to be implemented in subsequent years. The regression estimates directly a moving average representation truncated at 12 quarters. The results we obtain are very similar to those reported by Romer and Romer (2010). The coefficients on tax increases are negative, large in absolute value and highly statistically significant.

$$
\begin{aligned}
\Delta y_t \quad = \quad & \underset{(0.32)}{-0.34}\tau_t^{RR} \underset{(0.32)}{-0.33}\,\tau_{t-1}^{RR} \underset{(0.32)}{+0.11}\,\tau_{t-2}^{RR} \underset{(0.32)}{-0.34}\,\tau_{t-3}^{RR} \\
& \underset{(0.32)}{-0.22}\tau_{t-4}^{RR} \underset{(0.32)}{+0.08}\,\tau_{t-5}^{RR} \underset{(0.33)}{-0.64}\,\tau_{t-6}^{RR} \underset{(0.33)}{-0.53}\,\tau_{t-7}^{RR} \\
& \underset{(0.32)}{-0.18}\tau_{t-8}^{RR} \underset{(0.33)}{+0.38}\,\tau_{t-8}^{RR} \underset{(0.33)}{-0.10}\,\tau_{t-10}^{RR} \underset{(0.32)}{+0.64}\,\tau_{t-11}^{RR} \\
& \underset{(0.40)}{-0.84}\tau_{t-12}^{RR} + \lambda_{US} + u_t
\end{aligned}
\qquad (1)
$$

We next run a similar regression using the shifts in taxes and spending identified by DeVries et al (2011). For the time being we overlook fiscal plans and consider only unanticipated shifts in taxes ($\tau^u_{US,t}$) and spending ($g^u_{US,t}$). Equation (1) shows the results which are obtained estimating a panel which includes the 14 countries in our sample. The estimated coefficients are thus constrained to be the same for all countries.

$$\begin{aligned}\Delta y_{i,t} \quad = \quad & \underset{(0.117)}{-0.51}\tau^u_{i,t} \underset{(0.120)}{-0.41}\,\tau^u_{i,t-1} \underset{(0.118)}{-0.22}\,\tau^u_{i,t-2} \underset{(0.119)}{-0.18}\,\tau^u_{i,t-3} \\ & + \underset{(0.112)}{0.11}\,g^u_{i,t} \underset{(0.132)}{+0.08}\,g^u_{i,t-1} + \underset{(0.129)}{0.33}\,g^u_{i,t-2} + \underset{(0.124)}{0.11}\,g^u_{i,t-3} \\ & +\lambda_i + \chi_t + u_{i,t}\end{aligned} \qquad (2)$$

The effect of tax shocks is similar to that estimated in (1), the effect of expenditure adjustments is strikingly different All coefficients are more precisely estimated because we are estimating a panel rather than a single equation. (1).

These results are only illustrative, as they cannot be interpreted as multipliers and cannot be directly compared with those reported in Romer and Romer (2010). The reason is that in our data fiscal consolidations typically consist of correlated tax increases and spending cuts. Shifts in taxes and spending are not orthogonol to each other. This makes it impossible to compute two separate fiscal multipliers, one for tax hikes, another for spending cuts, simply adding spending shocks, $g_{i,t}$, to (1), precisely because the two almost never happen in isolation. In other words, if fiscal adjustments are normally implemented acting contemporaneously on taxes and expenditures, then computing the effect of a tax adjustment keeping the expenditure unaltered is not a valid simulation.

To correctly compute multipliers we thus must abandon the analysis of individual shifts in taxes or spending, and instead simulate the effect of a typical adjustment, which comprises both a tax increases and a spending cut. What we can do, to come as close as possible to estimating tax and spending multipliers, is distinguish such adjustments between those that are predominantly made of tax increases (which we label TB, tax-based) and those that are predominantly made of spending cuts (which we label EB, expenditure-based). To be precise we label TB (EB) a fiscal shock if, in the year in which it happened, its tax (expenditure) component was larger than its expenditure (tax) component. Thus we define e^{tot}_t the overall shift in fiscal policy in year t: tax increases plus spending cuts (measured as a percent of year t GDP), both unanticipated and announced for the future. We then interact this variable with dummies that categorize consolidations into tax-based or expenditure-based accordingly to the predominant nature of the adjustment. What we thus estimate is not, for instance, the effect of an isolated increase in taxes (which almost never happens in the data) but of a typical consolidation implemented mostly by raising taxes, with a smaller component of spending cuts—and vice-versa. Adopting this strategy delivers the following results:

$$\begin{aligned}\Delta y_{i,t} = & \underset{(0.077)}{-0.65} e_{i.t}^{tot} * TB_{i,t} \underset{(0.079)}{-0.42} e_{i,t-1}^{tot} * TB_{i,t-1} \underset{(0.077)}{-0.11} e_{i,t-2}^{tot} * TB_{i,t-2} \underset{(0.077)}{-0.42} e_{i,t-3}^{tot} * TB_{i,t-3} \\ & \underset{(0.076)}{-0.077} e_{i,t}^{tot} * EB_{i,t} \underset{(0.084)}{-0.16} e_{i,t-1}^{tot} * EB_{i,t-1} \underset{(0.084)}{+0.36} e_{i,t-2}^{tot} * EB_{i,t-2} \underset{(0.077)}{+0.18} e_{i,t-3}^{tot} * EB_{i,t-3} \\ & +\lambda_i + \chi_t + u_t \end{aligned} \quad (3)$$

The panel regression includes time and country fixed effects as above, but the coefficients are constrained to be identical across all countries. As in the Romer and Romer specification the sum of the coefficients on the included lags measures the effect on the level of output .Tax-based adjustments are recessionary with a multiplier of -0.65 after one-year, of about -1.1 after two years and of -1.5 after three years. These values are comparable, but smaller than those reported in Romer and Romer, in which the multiplier takes a value of about -1 after one-year, that increases to -2.2 after two years and reaches value in between -2.5 and 3 per cent after three years Spending-based adjustments have no significant effect on output for two years but then, two years after the policy shift, become significantly expansionary.[10] For the case of expenditure based adjustments only the coefficient on (t-1) is borderline

[10]By summing, in e_t^{tot}, anticipated and unanticipated shifts in fiscal policy, (**??**) assumes (like Romer and Romer 2010) that the two have identical effects on the economy. Theory tells that in general this in general is not the case. The economy is likely to respond differently to a tax increase announced the day it is implemented, compared to one announced the same day, but to be implemented a few years down the road. The same for changes in public spending. Moreover, anticipated shifts in policy may have one effect the moment they are announced and another the moment they are implemented, for instance if some agents are liquidity constrained. None of this is allowed in (**??**). Simply splitting e_t^{tot} in its two component (anticipated and unanticipated) — the procedure adopted by Mertens and Ravn 2011 using the Romer and Romer 2010 data — would be incorrect for the same reason we could not split this variable entering separately tax hikes and spending cuts: anticipated and unanticipated shifts in policy almost never happen in isolation.

insignificantly different form zero and it is very small in absolute value. The coefficients on (t-2) and (t-3) are positive but insignificant. According to this regression, spending cuts have virtually no output effects while tax increases have large negative effects on output.

3.3 Fiscal plans

The results discussed in the previous section do not allow for heterogeneity across countries. Studying the effects of fiscal plans, rather than isolated fiscal shocks, allows to remove this constraint. 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.

Thus, 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 surprise 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 DeVries et al. (2011) data fiscal plans almost never extend beyond a 3 year horizon: thus we take $j = 3$ as the maximum anticipation horizon [11]. We therefore define the observed anticipated shocks in period t as follows:

[11] 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 procedure.

$$
\begin{aligned}
\tau_{i,t}^{a} &= \tau_{i,t,t+1}^{a} + \tau_{i,t,t+2}^{a} + \tau_{i,t,t+3}^{a} \\
g_{i,t}^{a} &= g_{i,t,t+1}^{a} + g_{i,t,t+2}^{a} + g_{i,t,t+3}^{a} \\
e_{i,t}^{a} &= \tau_{i}^{a} + g_{i,t}^{a}
\end{aligned}
$$

We shall illustrate how we use our classification of fiscal shocks to construct fiscal plans with two example: the fiscal plan introduced in Italy in 1991 and that introduced in Australia in 1984. The case of Italy is illustrated in Table 1. DeVries et 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 its 1992 Recent Economic Developments document (p. 21), a number of the tax measures introduced in 1991—-Lit 19.4trillion (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 evidence.

Insert Table 1 here

Note that the plan introduced in 1991 was subsequently modified, in 1992 and 1993, with the introduction of further unanticipated tax hikes of 2.85 and 3.2 per cent of GDP respectively, and additional spending cuts of 1.9 and 2.48 per cent of the GDP. These modifications are illustrated in the second and the third rows of Table 1. We label fiscal adjustments respectively as "tax based" (TB) and "expenditure based" (EB) if the sum of the unexpected and announced tax (expenditure) changes (measured as percent of GDP) is larger than the sum of the unexpected and announced expenditure (tax) adjustments. Table 1 illustrates that this strategy leads to label the 1991-1993 adjustment in Italy 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 A1 in the Appendix lists our classification of episodes in TB and EB. Note that we cannot observe directly realizations of announced plans, as the narrative method allows to identify exogenous corrections when they are announced but only total expenditure and receipts are observed upon implementation.

Here is the second example: the plan which started in Australia in 1985 and, with a series of subsequent adjustments, lasted until 1988. After the December 1984 elections — in which the Labour party surprisingly defeated the sitting liberals — the government announced a set of medium-term fiscal policy spending cuts aimed at reducing a large inherited budget deficit. Table 2 illustrates this episode. The announced plan 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 expenditure-based adjustments. Note that because the revision introduced in 1986 for 1988 occurs as part of a multi-year plan, 1988 is labelled as a year of tax-based fiscal adjustment even if in that year we observe an (anticipated) reduction in taxation larger that the (anticipated) increase in expenditure.

Insert Table 2 here

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 given on the basis of information available when the plan is announced and implemented. This labelling can therefore be used in the estimation and simulation of the real time effects of the adoption of a fiscal plan and to detect potential differences between EB and TB plans. [12]

[12] 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.

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. Policy reversals are present in our data. For instance in Canada, in 1991, a fiscal correction initially labelled as TB, after some time was modified to deliver the majority of corrections on the expenditure side. At the time of the announcement we would label such a plan TB, but it would then shift to EB when the new announcement is made and tax hikes are replaced by spending cuts. The coding of different episodes is 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

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. We call the latter characteristic the "style" of a fiscal 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 $e_{i,t,j}^a$ $(j > 1)$.

3.4 Estimation

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[13] , 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 and the European Commission. Our baseline estimates are, as we explained, limited to 14 countries since we do not have data on confidence for Sweden and Finland. The sources of our data and all data transformations are described in Table A2 in the Appendix.

Our system includes for all countries a (truncated) moving average representation of the variable of interest, $\Delta z_{i,t}$ (in turn GDP growth, private

[13]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.

consumption growth, etc.). We use a quasi-panel which allows for two types of heterogeneity: a within country heterogeneity in the effects of TB and EB plans on the left-hand-side variable, and a between country heterogeneity in the design of fiscal plans. It is described by (4)

$$\begin{aligned} \Delta z_{i,t} &= \alpha + B_1(L)e^u_{i,t} * TB_{i,t} + B_2(L)e^a_{i,t,0} * TB_{i,t} + \\ & C_1(L)e^u_{i,t} * EB_{i,t} + C_2(L)e^a_{i,t,0} * EB_{i,t} + \\ & + \sum_{j=1}^{3} \gamma_j e^a_{i,t,j} * EB_{i,t} + \sum_{j=1}^{3} \delta_j e^a_{i,t,j} * TB_{i,t} + \lambda_i + \chi_t + u_{i,t} \end{aligned} \tag{4}$$

$$\begin{aligned} e^a_{i,t,0} &= e^a_{i,t-1,1} + e^a_{i,t-2,2} + e^a_{i,t-3,3} \\ e^a_{i,t,t+1} &= \varphi_{1,i}\ e^u_{i,t} + v_{1,i,t} \\ e^a_{i,t,t+2} &= \varphi_{2,i}\ e^u_{i,t} + v_{2,i,t} \\ e^a_{i,t,t+3} &= \varphi_{3,i}\ e^u_{i,t} + v_{3,i,t} \end{aligned} \tag{5}$$

where λ_i and χ_t are country and time fixed effects.

In equation (4) shifts in fiscal policy affect the economy through three components. Unanticipated changes in fiscal stance, $e^u_{i,t}$, announced at time t and implemented at time t; the realization at time t of policy shifts that had been announced in the past, $e^a_{i,t,0}$; 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 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. [14]. $\varphi_{1,i}, \varphi_{2,i}, \varphi_{3,i}$ are estimated on a country by country basis on the time series of the narrative fiscal shocks.

[14] (4) 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 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 $t+3$. Impulse responses to correlated shocks can be computed using the Generalized Impulse Response Functions (GIRF) discussed in Garratt et al. (2006), 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 $\varphi_{3,i}$) [15].In other words (4) 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. Impulse responses will be different for TB and EB adjustments. They will also differ across countries because the $\varphi' s$ differ across countries, describing 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 permanent *vs.* transitory 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—those we use in our baseline estimation. 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 following these four steps:

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

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

we pay a cost in terms of precision, as the omitted information affects the size of the confidence intervals of the impulse response functions.

[15] 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.

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[16], 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.[17]

4 Results

In this section we present our baseline results from the estimation of (4) and the associated equations used to estimate the $\varphi's$. The estimation runs from 1981 to 2007 since the model includes leads and lags of the fiscal variables, observing policy shifts over the period 1978-2009 allows us to estimate the model over the sample 1981-2007.

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 Sweden record a cumulative response of anticipated fiscal shocks to unanticipated corrections which is in the region of unity and higher than one for Canada. Australia, Austria, Denmark, France, and the United Kingdom feature a positive but milder response of anticipated corrections to current unanticipated ones with coefficients ranging from 0.12 to 0.85.

[16] 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.

[17] 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.

This correlation becomes not statistically different from zero in the cases of Belgium, Finland, Germany, Ireland, Japan, Ireland, Portugal, Spain, and the United States, where fiscal policy corrections are implemented mainly via unanticipated shocks (in fact, in the case of Portugal and Ireland adjustments occur almost exclusively via unanticipated shocks—and thus we do not have a sufficient number of observations to to estimate the $\varphi' s$). 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 Italy's stabilization plans are 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.

Insert Figure 1 here

The figure shows a significant heterogeneity across countries in the design of their fiscal plans and confirms the results of Table 4. To understand the figure, compare the results for Sweden and Italy. In Sweden the continuous and the dotted line 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.

Figure 2 reports the impulse responses of output growth to EB and TB fiscal plans where, as everywhere else in the paper, we report *two standard errors* bands, with 95 per cent confidence intervals. 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 United Kingdom and Japan); next we list the countries for which this correlation is close to zero (Belgium, Germany, Ireland, Portugal, Spain, and the United States) and finally the two opposite ends of the spectrum in terms of the relation between anticipated and unanticipated fiscal adjustments, Canada and Italy. The patterns differ across countries (because of the heterogeneity in plans) but in all of them the difference between EB and TB adjustments is large and statically significantly. In all countries TB adjustments are recessionary and there is no sign of recovery for at least the three years following the start of the plan. In the case of EB adjustments recessions are on average typically much smaller and short-lived. Note that this is an average which can result form some bigger EB induced recessions and sone 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 or spending 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 EB adjustments have especially low cost when they are clearly announced with no subsequent revisions. On the contrary they are less effective when they are stop-and-go.[18]

Insert Figure 2 here

Figures 3 and 4 show the response of households' consumption on durables and non-durables and of business investment [19]. The results clearly indicate that the different effect on output growth of TB and EB adjustments is to be attributed to the response of private investment, rather than to that of private consumption. Consumption growth typically responds quite similarly to TB and EB adjustments. US and Canada are the exceptions: in these two countries the responses of consumption and of investment are similar.

Insert Figures 3 and 4

Figures 5 and 6 report the responses of the indicators of consumer and business confidence. There is some mild heterogeneity between TB and EB

[18] Guajardo et al (2011) also use the DeVries et al (2011) data and also distinguish between EB and TB adjustments. Compared with our results, however, the impulse responses reported in that paper are constructed overlooking the country-specific styles of fiscal plans, *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, while throughout this paper we have reported *two standard errors* bands, with 95 per cent confidence intervals.

[19] 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.

adjustments in the response of consumer confidence, while a strong heterogeneity emerges 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, are much smaller than those of output, and the pattern of cross-country heterogeneity also does not match the one observed for output. The response of inflation helps understand why monetary policy might be slightly tighter during TB plans. Figure 8 shows that TB adjustments are 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 ZLB where interest rates are prevented from falling. We address this issue in the next section where we show that monetary policy cannot be the only or the main reason which distinguishes TB versus EB..

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 end 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 of private investment. In Italy, instead, the difference between EB and TB plans is the smallest, and EB plans don't feature positive effects on output. Pre-announced plans that are consistent over time thus seem to be superior to stop-and-go, largely unpredictable policies.[20]

5 Robustness

5.1 Monetary policy

We saw in figure 7 that monetary policy is on average slightly more expansionary during EB than TB adjustments, possibly in response to inflation. Can this be the reason why EB adjustments have much lower costs in terms of output losses? In this section we show that the answer is negative.[21].

We have designed a counterfactual aimed at evaluating what the effect of fiscal adjustments would be if policy rates remained unchanged, *i.e.* if the central bank was prevented from responding to the shift in fiscal policy. Consider a simplified representation of the joint dynamics of output growth, Δy_t, of the monetary policy variable (which for simplicity we denote MP_t), and of our narrative fiscal corrections consisting of both unanticipated and anticipated components

$$\begin{aligned} \begin{bmatrix} \Delta y_t \\ MP_t \end{bmatrix} &= \begin{bmatrix} a_{11} & a_{12} \\ a_{21} & a_{22} \end{bmatrix} \begin{bmatrix} \Delta y_{t-1} \\ MP_{t-1} \end{bmatrix} + \begin{bmatrix} \varepsilon_t^y \\ \varepsilon_t^m \end{bmatrix} \\ \varepsilon_t^y &= \beta_1 e_t^f * TB_t + \beta_2 e_t^f * EB_t + \beta_3 e_t^{nf} + e_t^y \\ \varepsilon_t^m &= \gamma_1 e_t^f * TB_t + \gamma_2 e_t^f * EB_t + \gamma_3 e_t^{nf} + \gamma_4 e_t^y + e_t^m \end{aligned}$$

The VAR innovations in the output growth equation, ε_t^y, depends on the narrative fiscal structural shocks that are allowed to have heterogenous effect according to their nature, $e_t^f * TB_t$ and $e_t^f * EB_t$, on non-fiscal shocks, e_t^{nf}, and on a residual output shock that we do not need to identify for our purposes e_t^y.

[20]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.

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

The VAR innovations in the equation for the monetary policy instruments, ε_t^m depend on the same structural shocks affecting the output innovations, and on a structural monetary shock e_t^m. This model makes the (usual) recursivity 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.

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

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

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

$$\Delta y_t = \sum_{i=1}^{3} \hat{\delta}_{i,TB} e_{t-i+1}^f * TB_{t-i+1} + \sum_{i=1}^{3} \hat{\delta}_{i,EB} e_{t-i+1}^f * EB_{t-i+1} + v_{1t}, \quad (6)$$

This regression is equivalent (in the context of our example) to the output growth equation estimated in (4) 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* of fiscal adjustment on output that depends on the responses 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

$$\Delta y_t = \sum_{i=1}^{3} \hat{\pi}_{i,TB} TB_{t-i+1} e^f_{t-i+1} + \sum_{i=1}^{3} \hat{\pi}_{i,EB} EB_{t-i+1} e^f_{t-i+1} + \quad (7)$$
$$+ \sum_{i=1}^{2} \hat{\pi}_{i,MP} \varepsilon^m_{t-i} + v_{2t}$$

The following table compares the expected values of the coefficients estimated in (6) and (7) 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. [22]

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	$a_{11}\beta_1 + a_{12}\gamma_1$	$(a^2_{11} + a_{12}a_{21})\,\beta_1 + (a_{11}a_{12} + a_{21}a_{22})\,\gamma_1$
$F_i = EB$		β_2	$a_{11}\beta_2 + a_{12}\gamma_2$	$(a^2_{11} + a_{12}a_{21})\,\beta_2 + (a_{11}a_{12} + a_{21}a_{22})\,\gamma_2$
Augmented specification				
		$\frac{\partial \Delta y_t}{\partial e^f_t * F_i}\,\vert_{\varepsilon^m_t = 0}$	$\frac{\partial \Delta y_t}{\partial e^f_{t-1} * F_i}\,\vert_{\varepsilon^m_{t-1} = 0}$	$\frac{\partial \Delta y_t}{\partial e^f_{t-2} * F_i}\,\vert_{\varepsilon^m_{t-2} = 0}$
$F_i = TB$		β_1	$a_{11}\beta_1$	$(a^2_{11} + a_{12}a_{21})\,\beta_1$
$F_i = EB$		β_2	$a_{11}\beta_2$	$(a^2_{11} + a_{12}a_{21})\,\beta_2$
		$\frac{\partial \Delta y_t}{\partial e^m_{t-1}}$	$\frac{\partial \Delta y_t}{\partial e^m_{t-2}}$	
		a_{12}	$(a_{11}a_{12} + a_{21}a_{22})$	
		a_{12}	$(a_{11}a_{12} + a_{21}a_{22})$	

[22] 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 (4) using Δi_t as a proxy for monetary innovations[23]

$$\begin{aligned}
\Delta z_{i,t} &= \alpha + \sum_{k=1}^{3} \delta_k \Delta i_{t-k} + B_1(L) e^u_{i,t} * TB_{i,t} + B_2(L) e^a_{i,t} * TB_{i,t} + \\
&\quad C_1(L) e^u_{i,t} * EB_{i,t} + C_2(L) e^a_{i,t} * EB_{i,t} + \\
&\quad + \sum_{j=1}^{3} \gamma_j e^a_{i,t,j} * EB_{i,t} + \sum_{j=1}^{3} \delta_j e^a_{i,t,j} * TB_{i,t} + \lambda_i + \chi_t + u_{i,t} \qquad (8) \\
e^a_{i,t,1} &= \varphi_{1,i} e^u_{i,t} + v_{1,i,t} \\
e^a_{i,t,2} &= \varphi_{2,i} e^u_{i,t} + v_{2,i,t} \\
e^a_{i,t,3} &= \varphi_{3,i} e^u_{i,t} + v_{3,i,t} \\
e^a_{i,t} &= e^a_{i,t-1,1} + e^a_{i,t-2,2} + e^a_{i,t-3,3}
\end{aligned}$$

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

[23] 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.

exogenous monetary policy shocks (see *e.g.*. Rudebusch 1998) it is interesting 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 9 along with the responses obtained in the baseline model. The results in Figure 9 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 the two type of fiscal plans.[24]

Insert Figure 9 here

5.2 Assessing the validity of the panel restrictions

Our baseline specification allows for within country heterogeneity in the effect of TB and EB plans and for between countries heterogeneity in the style of fiscal policy, but imposes panel restrictions on the coefficients of the MA representation used to construct impulse responses. Testing these restrictions estimating a fully unrestricted system is not possible because we do not have enough observations. We go one step in this direction separating countries in two blocks: euro area countries (Austria, Belgium, France, Finland, Germany, Ireland, Italy, Portugal and Spain)[25] and non euro-area countries (Australia, Denmark, United Kingdom, Japan, Sweden, U.S. and Canada). The motivation for this divisions is that presumably countries which adopted the same currency were more "similar" that those which did not even before the euro was introduces. We therefore proceed to the estimation of the following system

[24]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.

[25]Euro area countries are defined as such even for the pre Euro period in our sample. As discussed in a previous footnote we do not have enough observations of fiscal adjustments during the Euro area in our sample.

$$
\begin{aligned}
\Delta z_{i,t} &= \alpha + \delta_k (L) \Delta i_t + B_{1k}(L) e^u_{i,t} * TB_{i,t} + B_{2k}(L) e^a_{i,t} * TB_{i,t} + \\
& C_{1k}(L) e^u_{i,t} * EB_{i,t} + C_{2k}(L) e^a_{i,t} * EB_{i,t} + \\
& + \sum_{j=1}^{3} \gamma_{jk} e^a_{i,t,j} * EB_{i,t} + \sum_{j=1}^{3} \delta_{jk} e^a_{i,t,j} * TB_{i,t} + \lambda_i + \chi_t + u_{i,t} \\
e^a_{i,t,1} &= \varphi_{1,i} e^u_{i,t} + \upsilon_{1,i,t} \\
e^a_{i,t,2} &= \varphi_{2,i} e^u_{i,t} + \upsilon_{2,i,t} \\
e^a_{i,t,3} &= \varphi_{3,i} e^u_{i,t} + \upsilon_{3,i,t} \\
e^a_{i,t} &= e^a_{i,t-1,1} + e^a_{i,t-2,2} + e^a_{i,t-3,3} \\
k &= EU - EMU,\ NON\ EU - EMU
\end{aligned}
\tag{9}
$$

In (9) the coefficients describing the responses of the relevant 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 10, strengthen our baseline results by showing a more heterogenous effect of the EB and TB plans. Interestingly, this increased heterogeneity causes a rejection of the panel restrictions (the χ^2 test with twenty degrees of freedom for the null of equal coefficients across the two blocs takes a value of 88.05).[26]

Insert Figure 10

In the light of these results it seems interesting to run the counterfactual to evaluate the importance of accompanying monetary policy by estimating the model exclusively on the subsample of countries that belong to the euro area. Figure 11 shows that both the main evidence and the results of the counterfactual obtained by zeroing the response of monetary policy to fiscal adjustments are robust.

Insert Figure 11

[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.

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

There is empirical evidence which suggests an asymmetric effect of fiscal policy on confidence and output growth during economic expansions and recessions (see Auerbach and Gorodnichenko 2012, Bachmann and Sims 2011, Barro and Redlick 2011). Could the asymmetry 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 so that have large multipliers and EB one are chosen during booms so that they have small ones? In principle the narrative approach should eliminate the correlation of the adjustments to the cycle but this is point more subtle. The size of fiscal consolidations may be unrelated to the cycle when they are decided, but their type is and somehow it happens 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 mild 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 one we use the 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 in 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.

Although the choice between EB and TB plans appears not to be related to the cycle, it is possible that the output effects of either plan differ depending on the state of the economy: expansions vs recessions (Auerbach and Gorodnichenko 2012, Bachmann and Sims 2011, Barro and Redlick 2011)

Similar results are obtained when the relation between the choice between EB and TB plans and the OECD index of labor market reforms is considered. Thus our findings are not driven by the endogeneity of the type of adjustment to the cycle.

Note that this result is not inconsistent with the evidence and case studies of Alesina Ardagna (1998 2012) and Perotti (2012). These papers argue that amongst all the fiscal adjustment those which are least costly are those accompanied but some supply side reforms and wage moderation. So, for instance, amongst the EB adjustments those which are the least costly or not costly at all 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.

6 Conclusions

The critical 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 cases with small output costs and cases of small expansionary effects of fiscal adjustments. 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 and promptly recovers and increases above baseline soon after an expenditure based adjustment. Instead it falls for several years after a tax-based one. 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 in a zero lower bound situation. Finally, the difference between the effects of the two types of adjustments cannot be explained by the cycle nor by systematically different choices of accompanying additional supply side reforms.

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

time	$\tau^u_{i,t}$	$\tau^a_{i,t,}$	$\tau^a_{i,t,1}$	$\tau^a_{i,t,2}$	$\tau^a_{i,t,3}$	$g^u_{i,t}$	$g^a_{i,t}$	$g^a_{i,t,1}$	$g^a_{i,t,2}$	$g^a_{i,t,3}$	TB	EB
1991	1.69	0	-1.26	-1.2	0	1.08	0	0	0	0	0	1
1992	2.85	-1.26	-1.2	0	0	1.9	0	0	0	0	0	1
1993	3.2	-1.2	0	0	0	2.48	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	0.5	0	0.45	0	0	0	1
1986	0.17	0	0.19	-0.29	0	0.4	0.45	0.26	-0.08	0	0	1
1987	0	0.19	-0.29	0	0	0	0.26	-0.08	0	0	0	1
1988	0	-0.29	0	0	0	0	-0.08	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	3	3	0	0	10	3	3	0	0	4	7	3
CN	12	12	12	10	6	12	13	13	11	9	6	7	10
DK	3	2	2	0	0	2	1	1	0	0	1	4	2
FN	2	1	1	0	0	6	1	1	0	0	0	6	2
FR	5	4	4	3	1	4	2	2	0	0	7	5	4
BD	12	4	4	2	0	12	4	4	2	1	6	10	3
IR	7	1	0	0	0	5	1	0	0	0	5	2	0
IT	12	5	5	1	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	3	3	0	0	11	1	1	0	0	2	11	4
PT	6	0	0	0	0	6	0	0	0	0	5	2	0
ES	7	1	1	0	0	7	2	2	0	0	4	6	2
SW	3	4	4	2	1	3	4	4	2	1	0	7	2
UK	6	3	3	0	0	7	3	4	0	0	7	3	4
US	8	8	8	7	6	3	8	8	7	6	5	10	2

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 3 Cross countries heterogeneity in the design of multi - year plans

	CAN	SWE	AUS	DNK	AUT	GBR	JPN	FRA
$\varphi_{1,i}$	1.424	0.49	0.85	0.55	0.31	0.29	0.27	0.12
	(0.28)	(0.09)	(0.12)	(0.11)	(0.06)	(0.02)	(0.03)	(0.04)
$\varphi_{2,i}$	0.74	0.31	-0.14	0	0	0	-0.001	-0.011
	(0.12)	(0.06)	(0.08)				(0.003)	(0.03)
$\varphi_{3,i}$	0.058	0.22	-0.02	0	0	0	0	-0.02
	(0.05)	(0.02)	(0.01)					(0.02)

	USA	DEU	BEL	IRE	POR	FIN	ESP	ITA
$\varphi_{1,i}$	0.08	0.051	0.015	0	0	-0.041	-0.024	-0.2
	(0.26)	(0.054)	(0.09)			(0.088)	(0.03)	(0.04)
$\varphi_{2,i}$	0.08	-0.098	0	0	0	0	0	-0.03
	(0.19)	(0.03)						(0.03)
$\varphi_{3,i}$	-0.02	0.02	0	0	0	0	0	0
	(0.14)	(0.01)						

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.

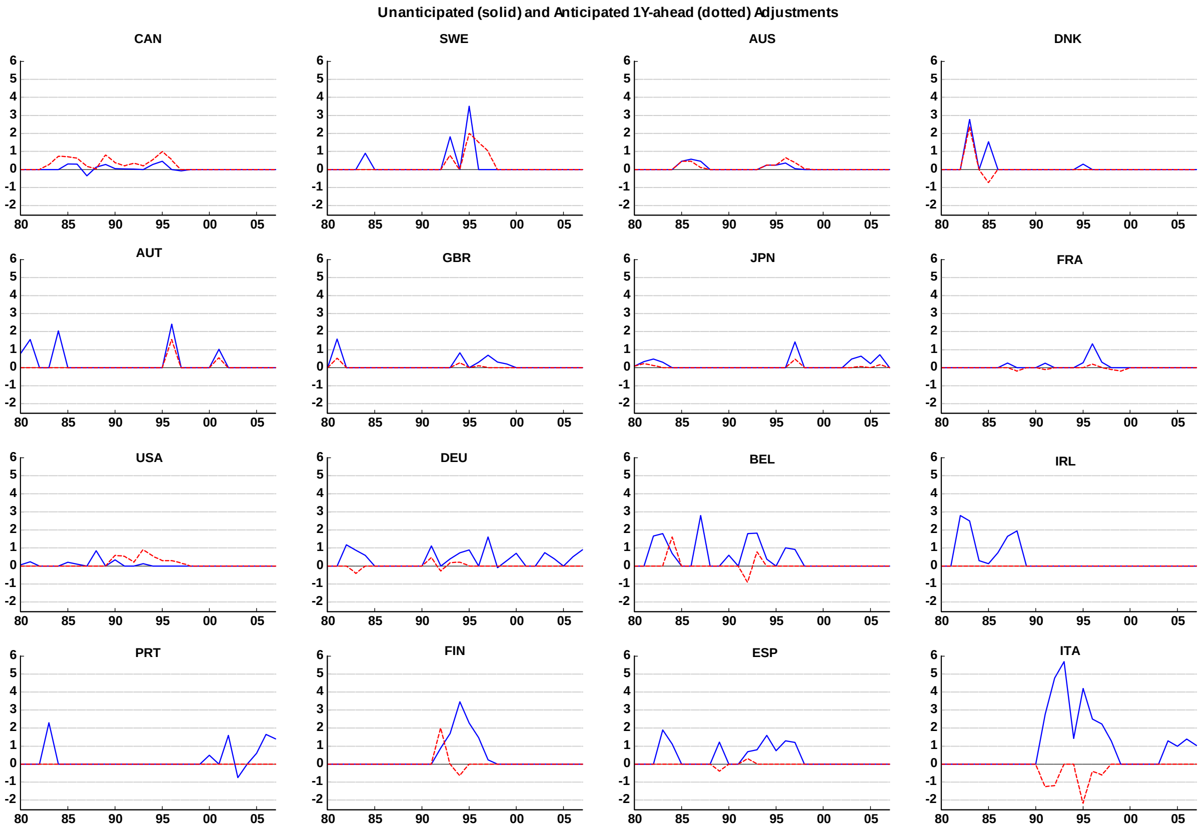

Figure 1: Unanticipated and Anticipated Fiscal Adjustments

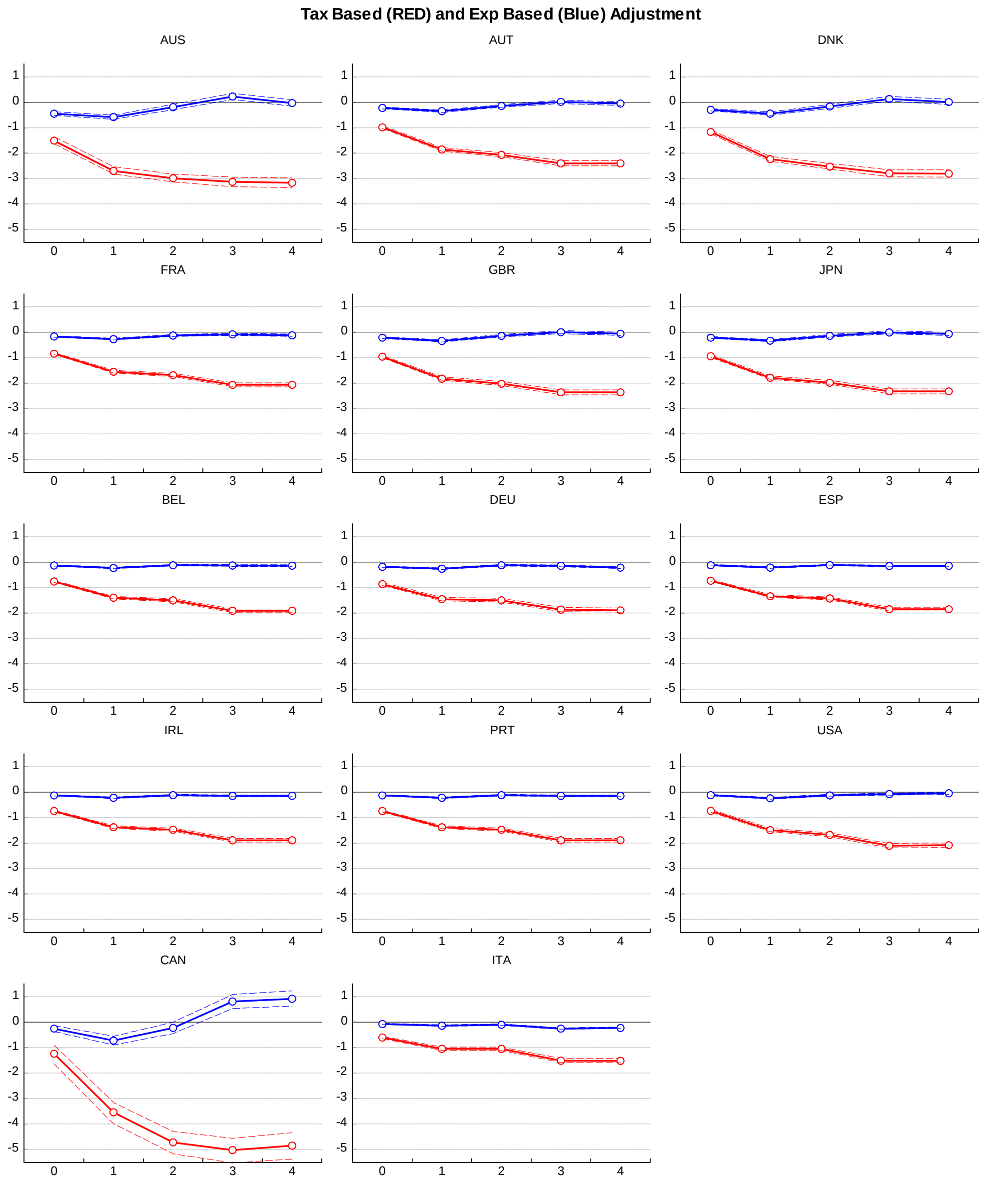

Figure 2: The effect of TB and EB adjustment on output growth

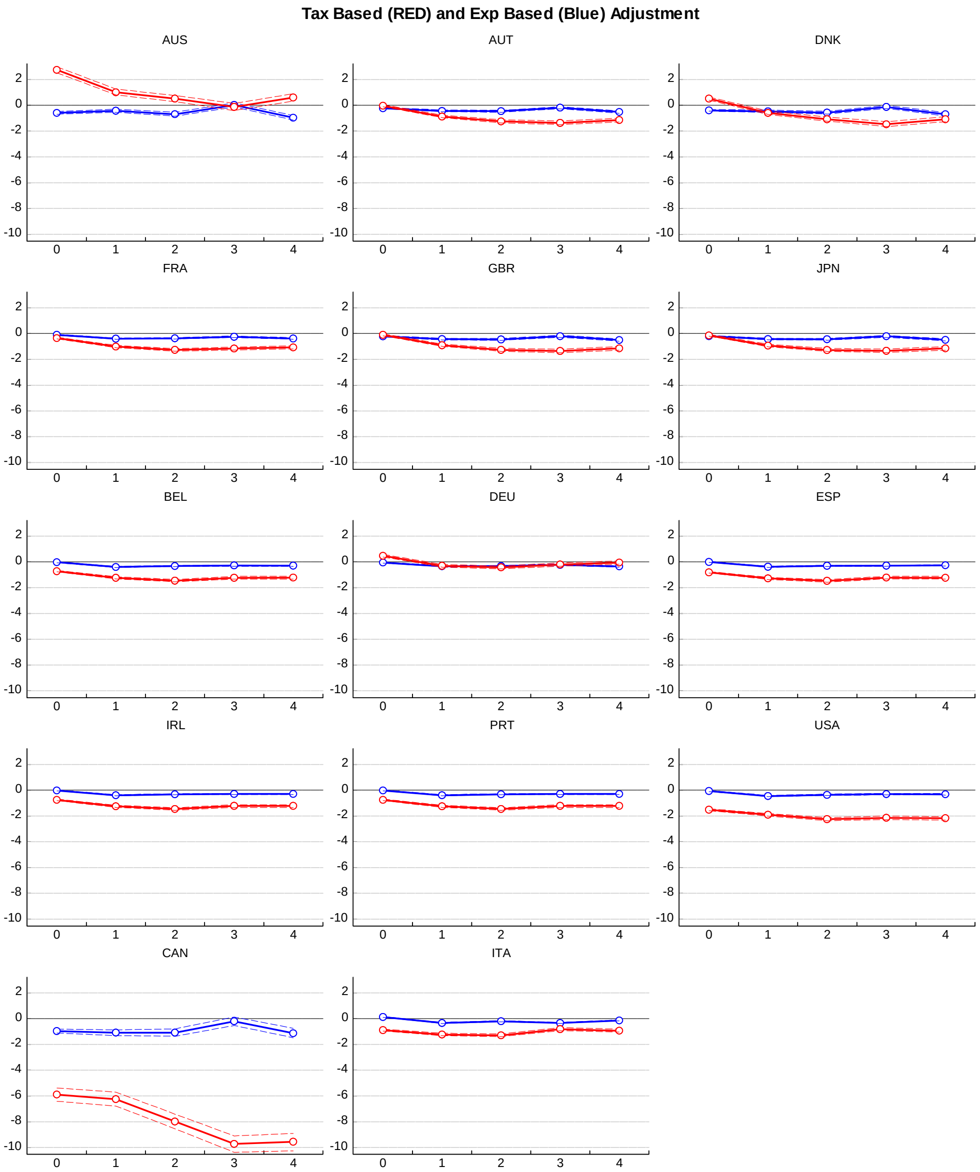

Figure 3: The effect of TB and EB adjustment on consumption growth

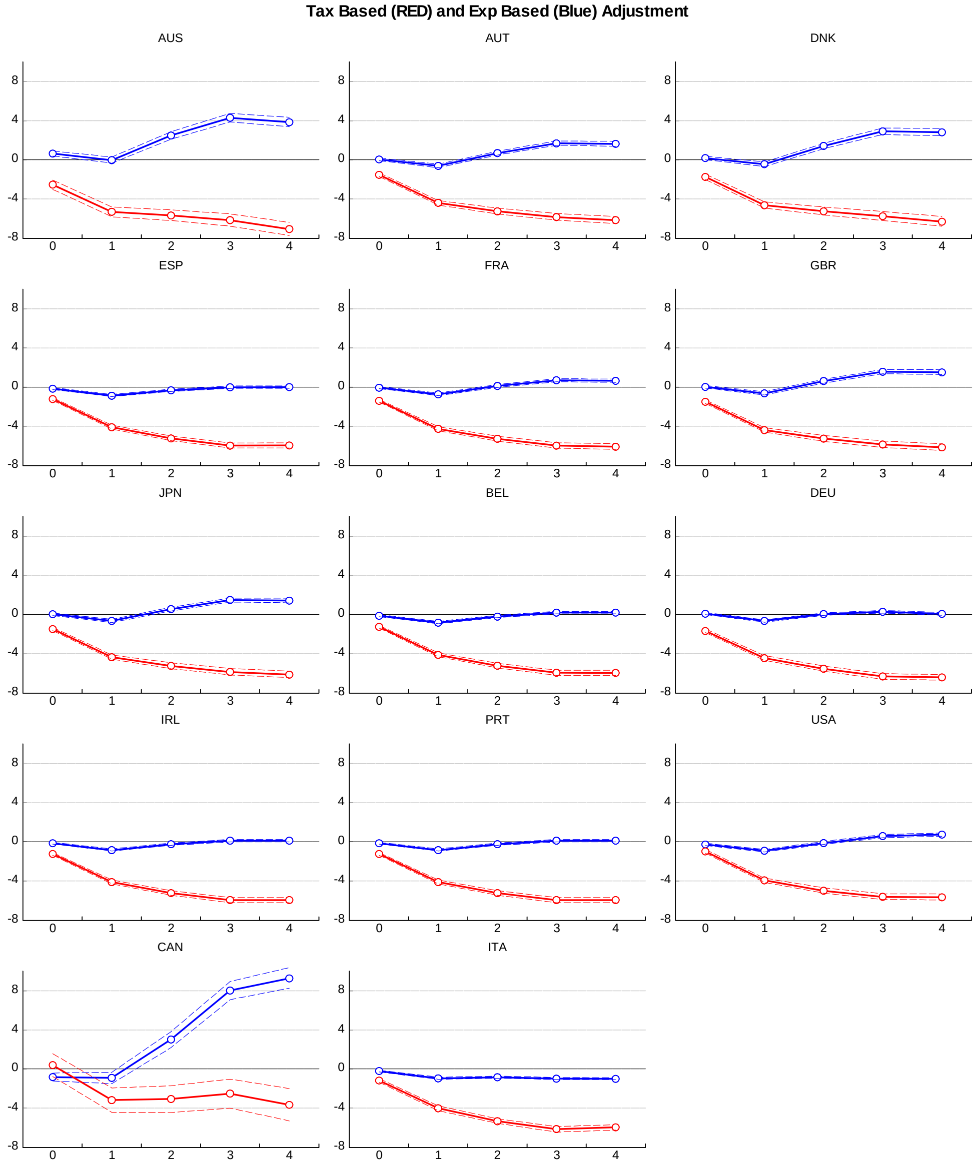

Figure 4: The effect of TB and EB adjustment on fixed capital formation growth

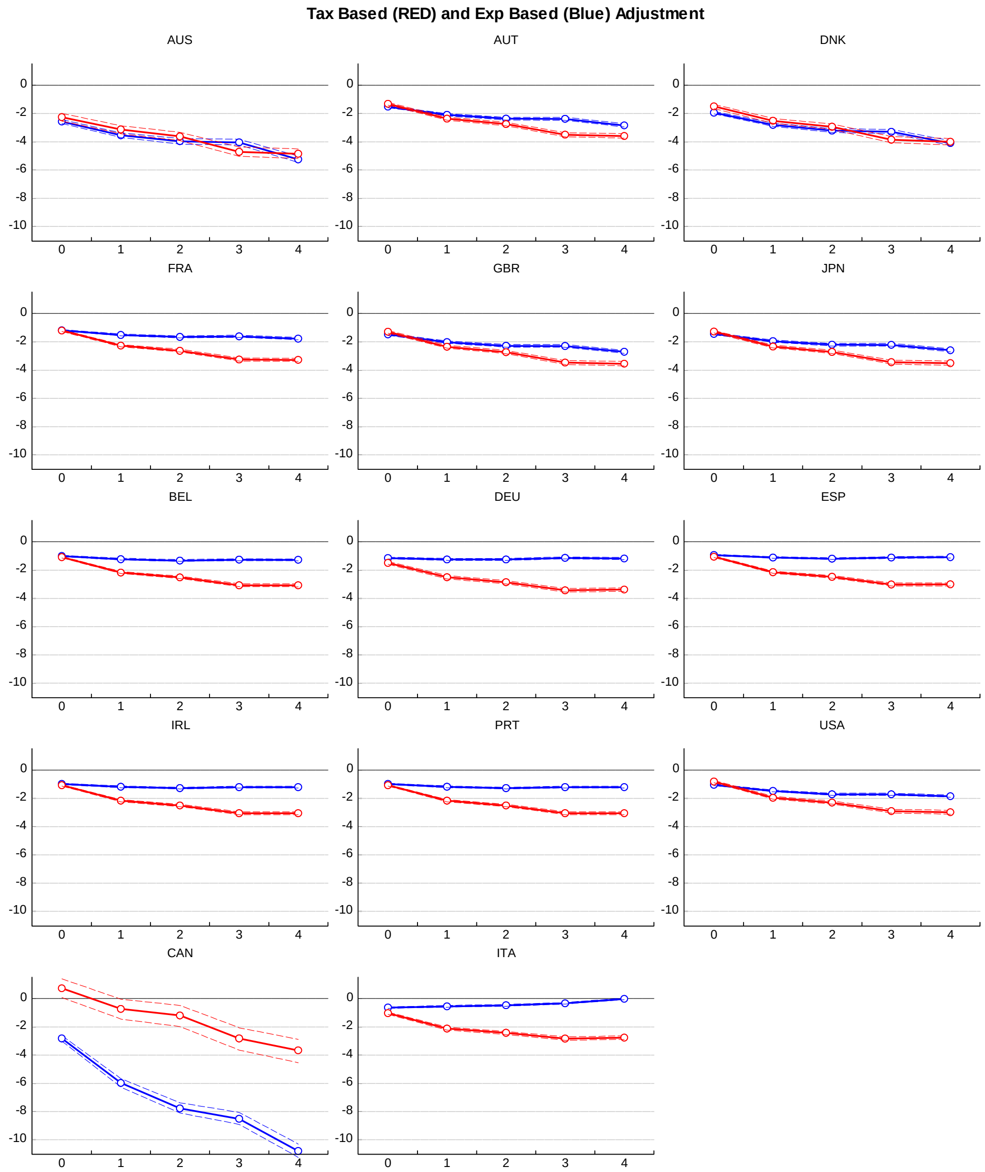

Figure 5: The effect of TB and EB adjustment on ESI Consumer Confidence

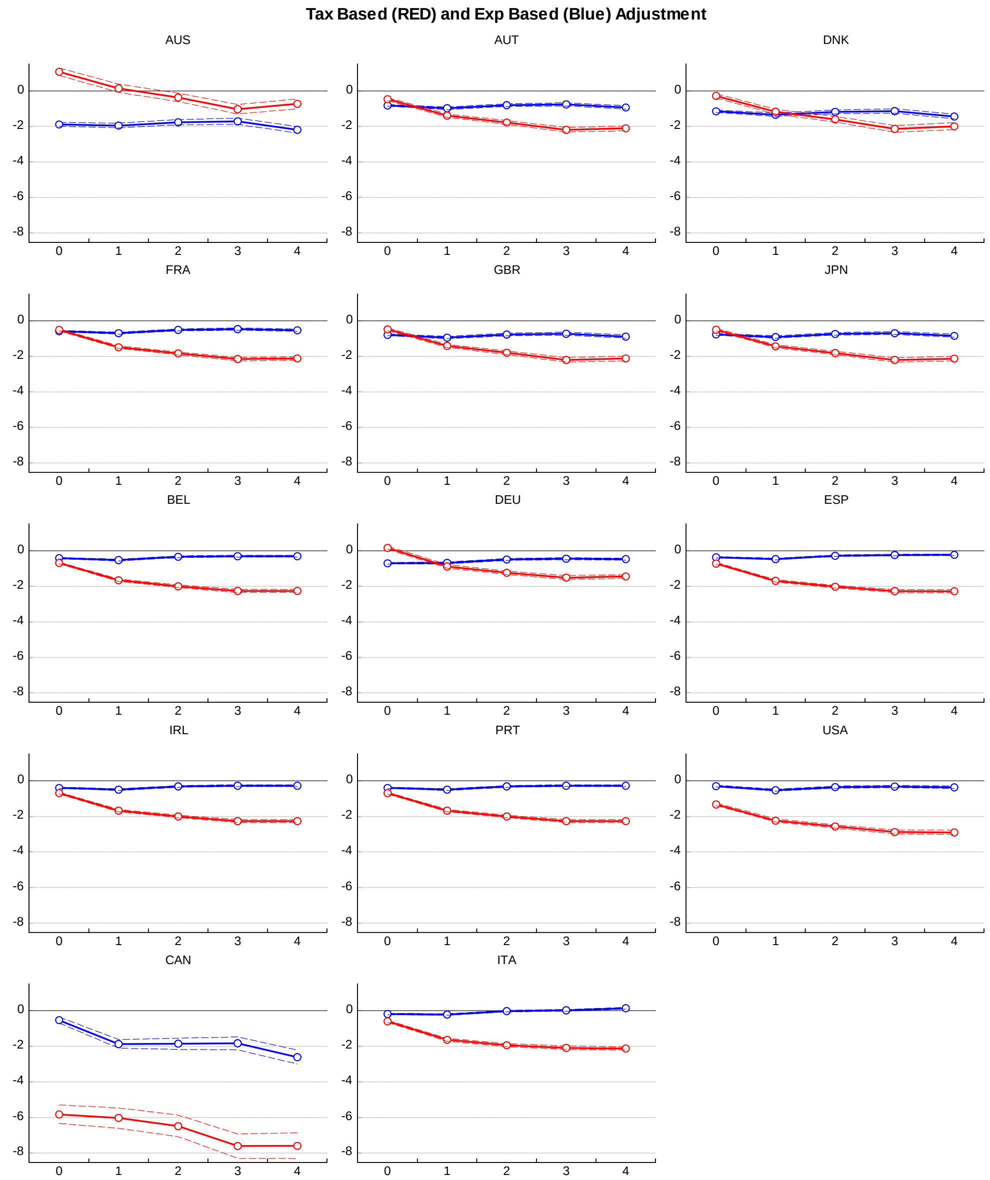

Figure 6: The effect of TB and EB adjustment on ESI Business Confidence

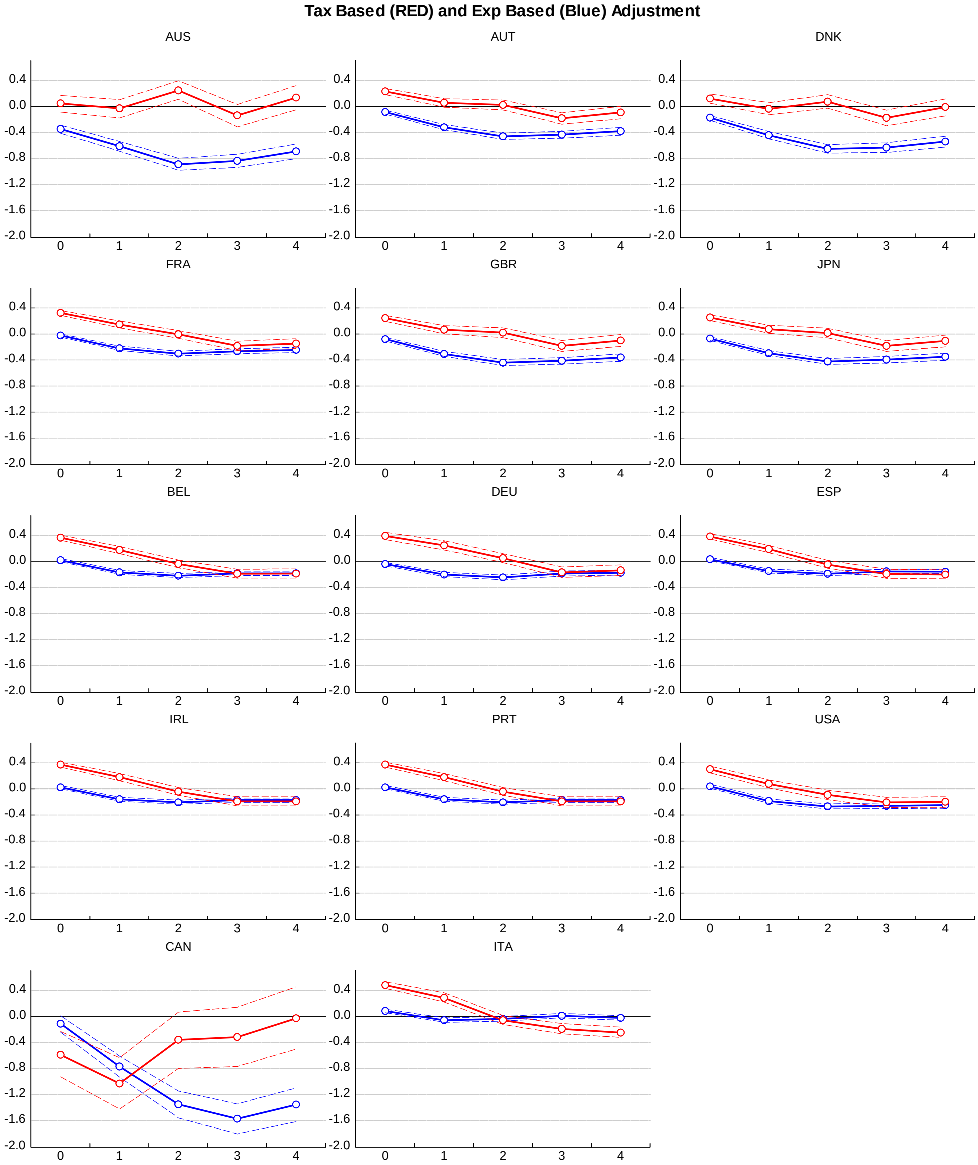

Figure 7: The effect of TB and EB adjustments on monetary policy (change in the 3M TBills Rates)

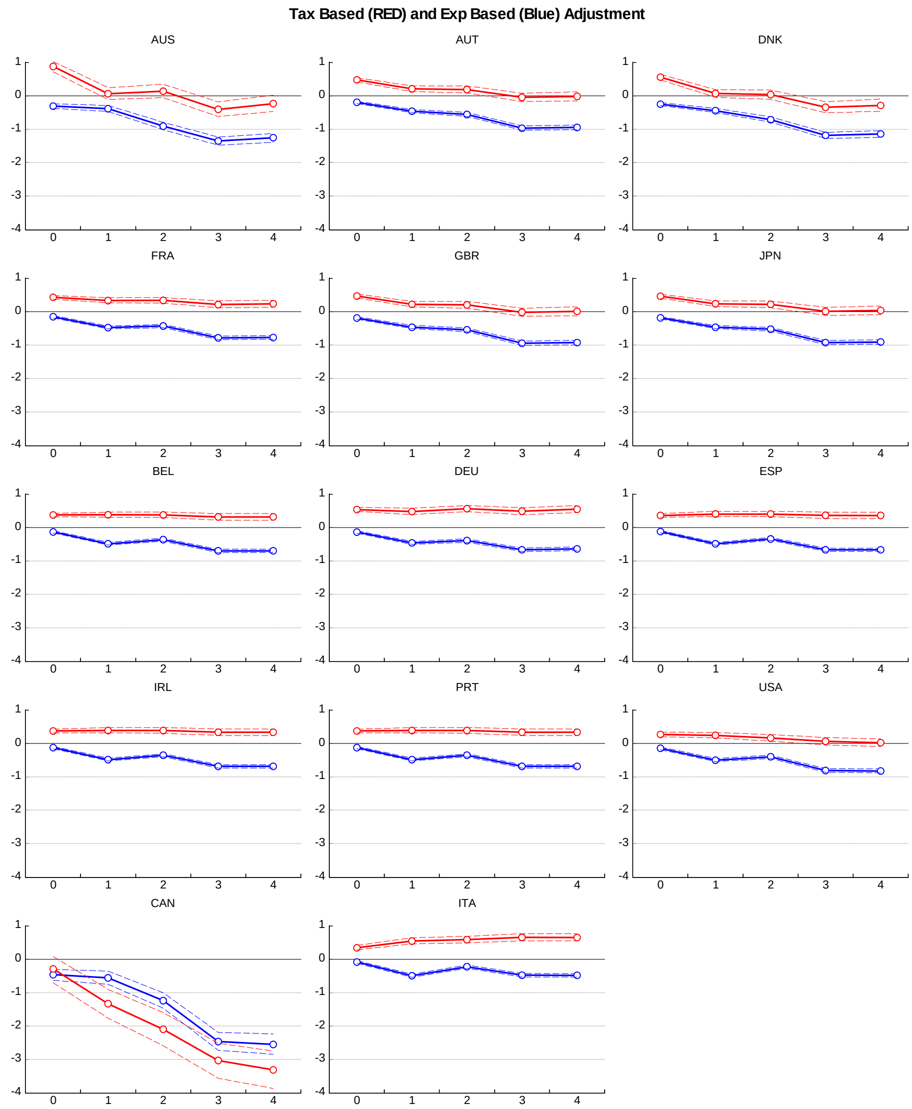

Figure 8: The effect of TB and EB adjustments on inflation (GDP deflator)

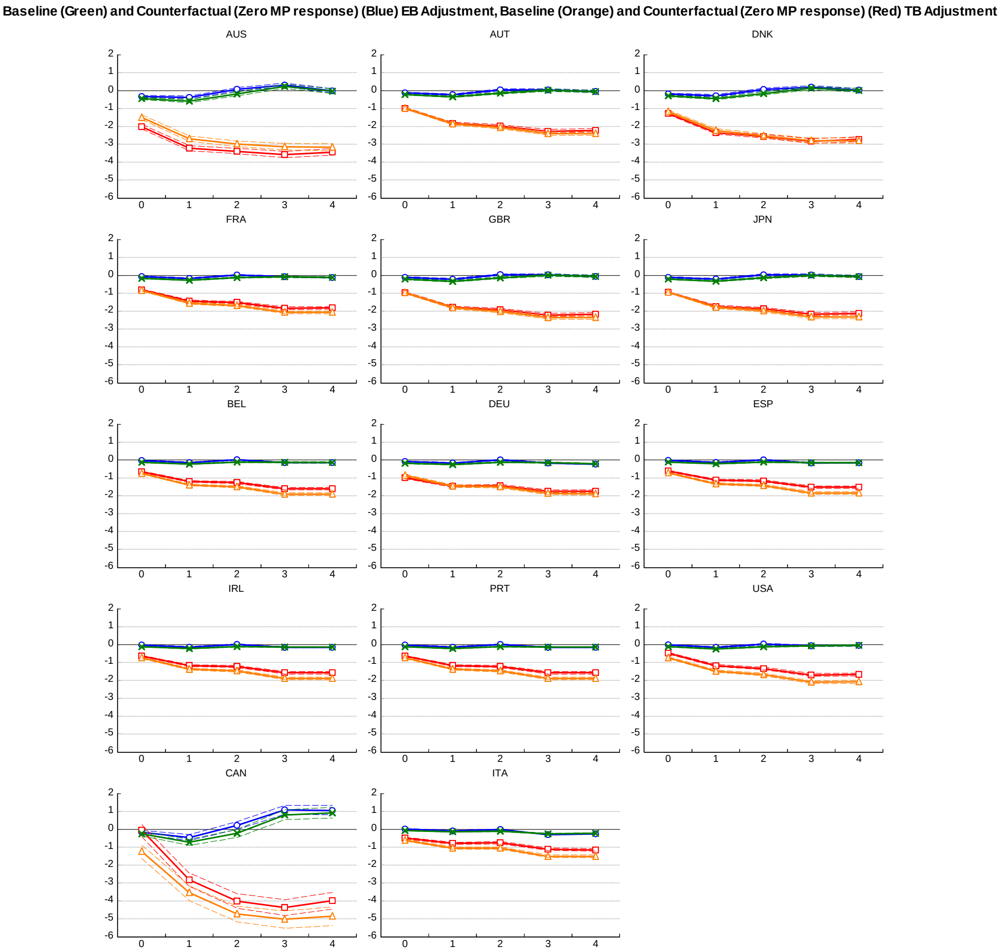

Figure 9: The effect of TB and EB adjustment: Baseline and Counterfactual

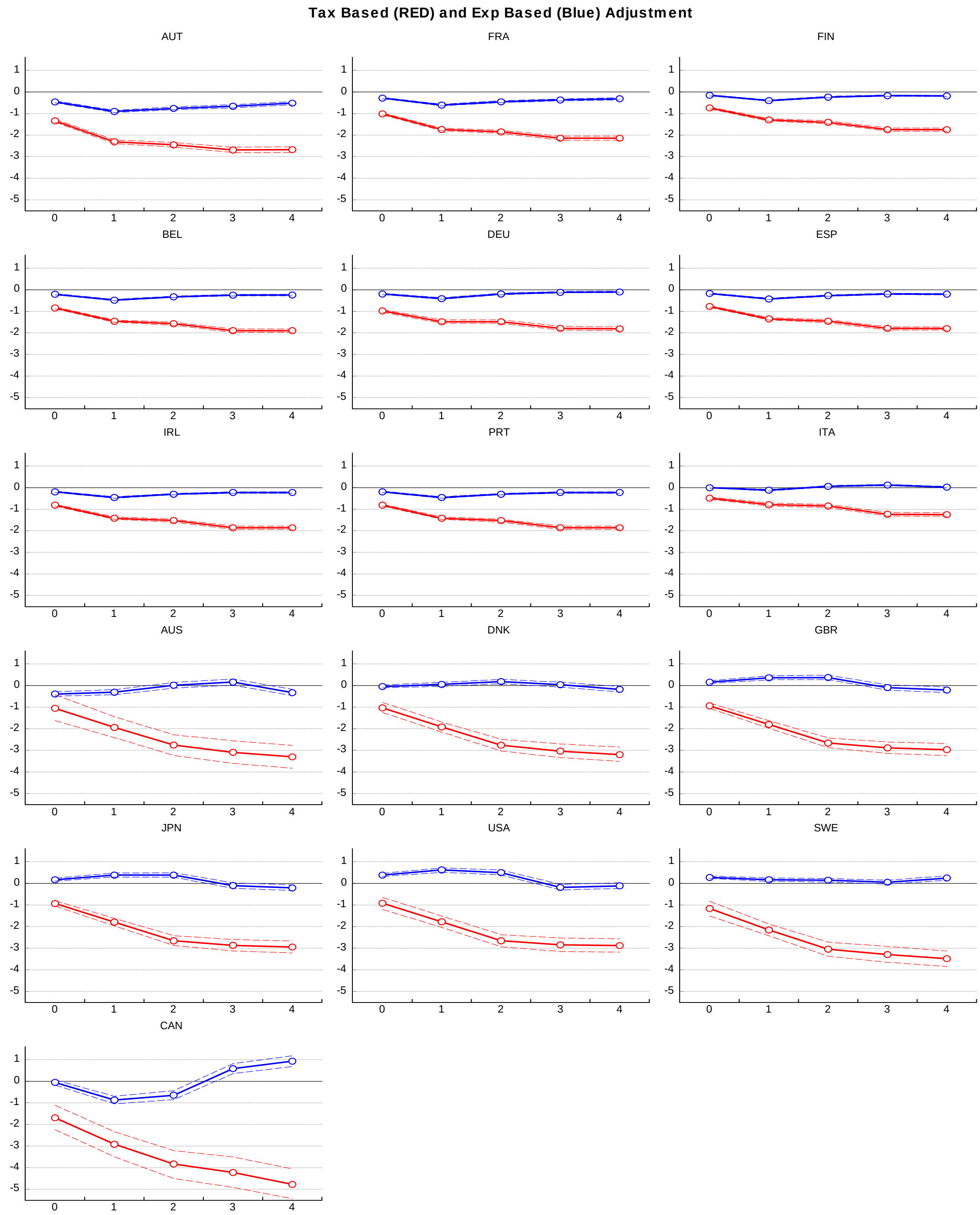

Figure 10: Impulse responses of output allowing for different coefficients in euro area and non-euro area countries

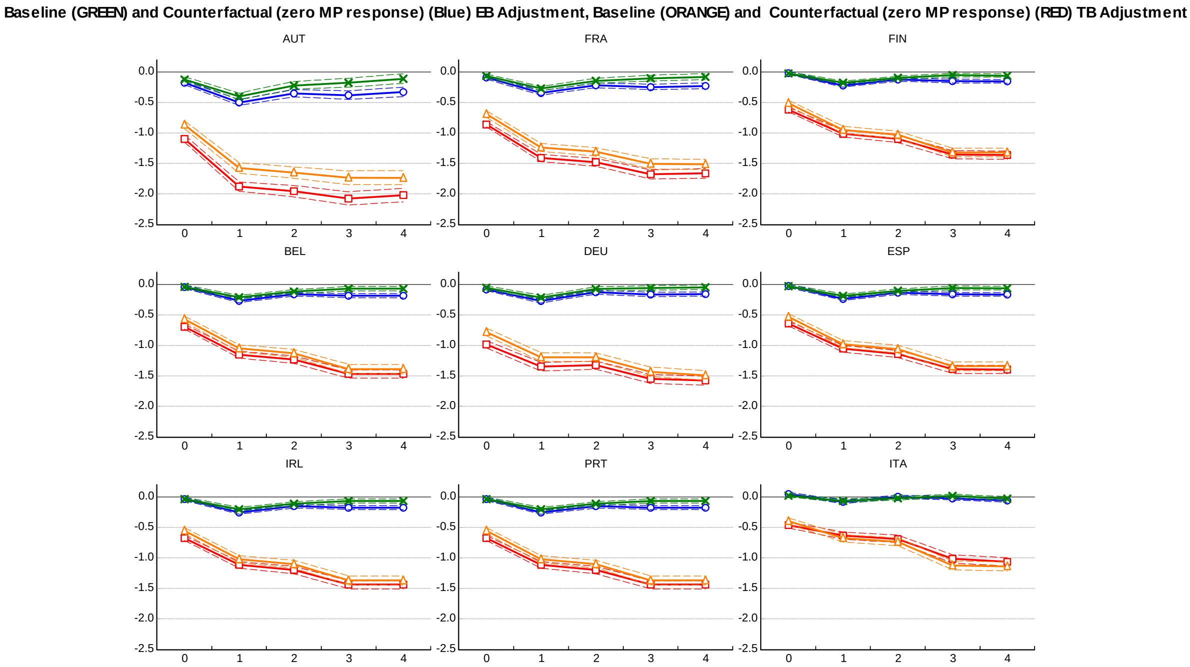

Figure 11: The effect of TB and EB adjustment both Baseline and Counterfactual for Europe

8 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.

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	1	0
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	-0.5	0	0	0.8	0	-0.4	0	0	1	0
BEL	1993	0.92	0.43	0.49	0.93	-0.5	0.55	0	0	0.89	-0.4	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	1.00	0.50	0.50	0.5	0	0	0	0	0.5	0	0	0	0	1	0
BEL	1997	0.91	0.41	0.50	0.41	0	0	0	0	0.5	0	0	0	0	0	1
CAN	1983	0.00	0.00	0.00	0	0	0.27	0.325	0.199	0	0	0	0	0	1	0
CAN	1984	0.27	0.27	0.00	0	0.27	0.355	0.208	0.036	0	0	0.373	-0.16	-0.09	1	0
CAN	1985	1.03	0.53	0.50	0.174	0.355	0.65	0.268	0.036	0.129	0.373	0.051	0.062	0.029	1	0
CAN	1986	0.99	0.84	0.15	0.192	0.65	0.492	0.124	0.014	0.1	0.051	0.135	0.046	0.001	1	0
CAN	1987	0.28	0.14	0.14	-0.35	0.492	0.124	0.014	0	0	0.135	0.046	0.001	0	1	0
CAN	1988	0.30	0.33	-0.03	0.202	0.124	0.027	0.001	0	-0.07	0.046	0.001	0	0	1	0
CAN	1989	0.31	0.24	0.08	0.21	0.027	0.496	0.121	0.01	0.074	0.001	0.314	0.248	0.04	1	0
CAN	1990	0.86	0.57	0.29	0.072	0.496	0.121	0.01	0	-0.02	0.314	0.248	0.04	-0	1	0
CAN	1991	0.40	0.13	0.27	0.011	0.121	0.01	0	0	0.022	0.248	0.188	0.087	0.017	0	1
CAN	1992	0.21	-0.01	0.22	-0.01	0.01	0	0	0	0.035	0.188	0.35	0.211	0.075	0	1
CAN	1993	0.35	-0.01	0.36	-0.01	0	0	0	0	0.008	0.35	0.211	0.075	0.013	0	1
CAN	1994	0.49	0.04	0.45	0.036	0	0.094	0.037	0.004	0.242	0.211	0.446	0.279	0.053	0	1
CAN	1995	0.99	0.18	0.81	0.087	0.094	0.095	0.028	0	0.368	0.446	0.889	0.482	0	0	1
CAN	1996	0.97	0.09	0.88	0	0.095	0.028	0	0	-0.01	0.889	0.51	0	0	0	1
CAN	1997	0.47	0.01	0.47	-0.02	0.028	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	1	0
DEU	1993	0.11	-0.07	0.18	0.39	-0.46	0	0	0	0	0.18	0.18	0	0	0	1
DEU	1994	0.91	0.08	0.83	0.08	0	0.07	0	0	0.65	0.18	0.135	0	0	0	1
DEU	1995	1.08	0.84	0.24	0.77	0.07	0	0	0	0.11	0.135	0	0	0	1	0
DEU	1997	1.60	0.50	1.10	0.5	0	0	0	0	1.1	0	0	0	0	0	1
DEU	1998	-0.10	0.00	-0.10	0	0	0	0	0	-0.1	0	0	0	0	0	1
DEU	1999	0.30	0.30	0.00	0.3	0	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	0	0	0.5	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		
DNK	1983	2.77	0.92	1.85	0.92	0	0.67	0	0	1.85	0	1.71	0	0	0	1
DNK	1984	2.38	0.67	1.71	0	0.67	0	0	0	0	1.71	0	0	0	0	1
DNK	1985	1.54	0.77	0.77	0.77	0	-0.72	0	0	0.77	0	0	0	0	0	1
DNK	1986	-0.72	-0.72	0.00	0	-0.72	0	0	0	0	0	0	0	0	0	1
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	-0.25	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	1992	0.70	0.30	0.40	0.3	0	0	0	0	0.4	0	0.3	0	0	0	1
ESP	1993	1.10	0.80	0.30	0.8	0	0	0	0	0	0.3	0	0	0	1	0
ESP	1994	1.60	0.00	1.60	0	0	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	-0.63	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	0	0	0	0	1
FIN	1996	1.47	0.00	1.47	0	0	0	0	0	1.47	0	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	0	1
FRA	1989	-0.20	-0.20	0.00	0	-0.2	0	0	0	0	0	0	0	0	0	1
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	0	0	-0.15	0	0	0	0	1	0
FRA	1996	1.33	0.86	0.47	0.86	0	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	0.69	0.53	0.16	0.533	0	0	0	0	0.156	0	0	0	0	1	0
GBR	1998	0.31	0.30	0.01	0.297	0	0	0	0	0.014	0	0	0	0	1	0
GBR	1999	0.21	0.21	0.01	0.206	0	0	0	0	0.005	0	0	0	0	1	0
IRL	1982	2.80	2.54	0.26	2.54	0	0	0	0	0.26	0	0	0	0	1	0
IRL	1983	2.50	2.44	0.06	2.44	0	0	0	0	0.06	0	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	-1.2	0	1.08	0	0	0	0	0	1
ITA	1992	3.50	1.60	1.90	2.85	-1.26	-1.2	0	0	1.92	0	0	0	0	0	1
ITA	1993	4.49	2.00	2.49	3.2	-1.2	0	0	0	2.49	0	0	0	0	0	1
ITA	1994	1.43	-0.27	1.70	-0.27	0	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	0.34	-0.74	1.08	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		
ITA	2004	1.30	0.67	0.63	0.67	0	0	0	0	0.63	0	0	0	0	1	0
ITA	2005	1.00	0.40	0.60	0.4	0	0	0	0	0.6	0	0	0	0	0	1
ITA	2006	1.39	0.50	0.89	0.5	0	0	0	0	0.89	0	0	0	0	0	1
ITA	2007	1.03	1.32	-0.29	1.32	0	0	0	0	-0.29	0	0	0	0	1	0
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	1.23	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	0.06	-0.69	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	-0.87	0	0	0	0	0	0	0	1	0
NLD	1992	0.74	-0.58	1.32	0.29	-0.87	0.23	0	0	1.32	0	-0.2	0	0	0	1
NLD	1993	0.12	-0.16	0.28	-0.39	0.23	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	0	0	0	0.08	0	0	0	0	1	0
PRT	2006	1.65	1.10	0.55	1.1	0	0	0	0	0.55	0	0	0	0	1	0
PRT	2007	1.40	0.50	0.90	0.5	0	0	0	0	0.9	0	0	0	0	0	1
SWE	1984	0.9	0.21	0.69	0.21	0	0	0	0	0.69	0	0	0	0	0	1
SWE	1993	1.812	0.42	1.3917	0.42	0	0.19	0	0	1.392	0	0.586	0	0	0	1
SWE	1994	0.777	0.19	0.5863	0	0.19	0	0	0	0	0.586	0	0	0	0	1
SWE	1995	3.5	1.4	2.1	1.4	0	0.8	0.6	0.4	2.1	0	1.2	0.9	0.6	0	1
SWE	1996	2	0.8	1.2	0	0.8	0.6	0.4	0	0	1.2	0.9	0.6	0	0	1
SWE	1997	1.5	0.6	0.9	0	0.6	0.4	0	0	0	0.9	0.6	0	0	0	1
SWE	1998	1	0.4	0.6	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	0	0	0	0	0	0	1	0
USA	1980	0.06	0.06	0.00	0.062	0	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	1985	0.21	0.21	0.00	0.21	0	0	0	0	0	0	0	0	0	1	0
USA	1986	0.10	0.10	0.00	0.096	0	0	0	0	0	0	0	0	0	1	0
USA	1988	0.85	0.39	0.46	0.39	0	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	0.52	0.24	0.28	0	0.24	-0.02	0.07	0.02	0	0.28	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	0.20	0.33	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	0.00	0.15	0	-0.02	0	0	0	0	0.17	0	0	0	0	1