The output effect of fiscal consolidations

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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 17 OECD economies..

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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. Fiscal adjustments based upon spending cuts are much less costly, in terms of output losses, than tax-based ones. Studying the experience of sixteen OECD countries over a quarter of a century (1978 to 2009) 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 type of adjustments. 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. *I*n other words it is possible that at zero lower bound both types of adjustments might be more costly because the central bank cannot help as much, but the difference between tax based and spending based adjustments should persist also at ZLB given that monetary policy has 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 to consumption growth.[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, for identification purposes, to identify a sample of episodes of exogenous shifts in fiscal stance.Following the approach pioneered by Romer and Romer (2010), Devries at al (2011) have collected and described the multi-year fiscal consolidation plans (tax increases and spending cuts) announced (and then implemented or revised) by seventeen[2] OECD countries between 1978 and 2009. These plans 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. Among all stabilization plans these authors have selected

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

[2] As we discuss below we have to drop one country from the sample so we are left with sixteen.

those that were designed to reduce a budget deficit and to put the public debt on a sustainable path: this should guarantee their "exogeneity" for the estimation of the output multipliers.[3]

The fiscal consolidation plans that can be constructed using the Devries et al (2011) data usually consist of some "unanticipated" correction, to be implemented in the same year the plan is first announced, and a series of anticipated corrections to be implemented in the following years. These unanticipated and anticipated shifts in taxes and spending appear to be correlated, but such correlations differ from one country to another. In some countries fiscal consolidations are typically permanent policy shifts, *i.e.* unanticipated corrections are positively correlated with the anticipated ones. Other countries, instead, typically announce plans that have transitory elements.

If we were to study only one country we would have very few observations This is why, in order to obtain more precise estimates, we pool together fiscal shocks from different countries. However, pooling is problematic in the presence of heterogeneity (see, for example, Favero, Giavazzi and Perego (2012)). In the present paper we propose a model that pools the international evidence on the effect of fiscal policy by allowing two sources of heterogeneity. The first is a within country heterogeneity with respect to the type of fiscal adjustments, the second is between countries heterogeneity in the way fiscal policy is conducted. Rather than simulating the effect of isolated, exogenous shifts in either taxes or government spending—as the literature has so far typically done—we study the response of output (and of consumption, investment and confidence) to multi-period fiscal consolidation plans, sequences of tax increases and spending cuts, announced in some year and then implemented or revised in subsequent years. When a multi period consolidation plans is introduced an unanticipated correction is implemented in the current year and a series of future corrections are announced for the following years. 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.

The result that spending based fiscal adjustments are non-recessionary on average bring support to a vast literature opened by Giavazzi and Pagano (1990) and recently extended and summarized by Alesina and Ardagna (

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

2010). This literature, using simple data analysis and case studies, suggested that indeed spending based-fiscal adjustments—differently from tax-based ones—can have very small or no output costs at all.[4] 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 type of fiscal adjustments 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 find that the differences are minor and that spending-based adjustments are less costly than tax-based ones even when monetary policy is not allowed to react to the adjustment.

Given that the very large difference between tax-based and spending-based fiscal adjustments appears not to depend on monetary policy, what accounts for it? Some explanations could be the "standard" neoclassical ones: the distortionary supply-side effects of taxation, wealth effects associated with expectations of lower taxes in the future thanks to spending cuts. The role of accompanying non-monetary policies could also play a role: some case studies by Alesina and Ardagna (1998, 2012) and Perotti (2012) show that the spending-based consolidations which have been especially favorable to growth are those that have been accompanied by supply-side reforms, goods and labor market liberalization and wage moderation. [5]

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 our data and our statistical approach. Section 4 illustrates our

[4] 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 dataset. 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).

[5] Alesina and Ardagna (2012) show that these policies, rather nominal exchange rate devalutaions, are what helped exports during expansionary, spending-based episodes of fiscal adjustments.

results, Section 5 reports the evidence from a number of robustness checks we have conducted and 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 depending on wealth, intertemporal substitution and distortions. These three channels operate differently in the case of tax-based or expenditure-based adjustments. A reduction in government spending has a positive wealth effect on individuals (via the reduction in future expected taxes) and therefore an expansionary effect on consumption. As a consequence of the positive wealth effect, labour supply shifts inward, hours worked decrease and the real wage increases. This static effect is combined with a dynamic effect that depends on the impact that a cut in government expenditure has on the future stock of capital available to the economy. The size of such an effect is 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.

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. Because of tax distortions and their negative effect on profitability, one can derive a straightforward negative response of private investment to a tax-based adjustment. 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. [6]

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 adjustment, mainly through an investment-related channel. In this framework fluctuations in 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.

For virtually all the channels discussed above it should matter a lot whether the spending cuts are perceived as permanent or transitory. In particular, wealth effects will be larger for permanent spending cuts, and the elimination of uncertainty regarding fiscal sustainabilty is also of course much more relevant. 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 that 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. 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, and 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.

[6] *Alesina and Perotti (1997) show how in unionized economies increases in income taxes translate into increasing wage demand by unions and increasing unit labor costs and loss of comptetiveness for domestic firms.*

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 sometimes 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.

A different strand of the literature emphasizes the role of accompanying policies. One 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 levels 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 and Estimation

3.1 Identification

Recent contributions to the literature on the effect of fiscal policy have adopted either structural VAR methods or "narrative" approaches.[7] We follow the second strategy for several reasons. First, as fiscal adjustments are typically introduced via multi-year plans, which include unanticipated and anticipated components, only the narrative approach allows us to identify these two components.[8] Second we can distinguish between different

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

[8] 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 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.

type of stabilization, determined by the structure of fiscal plans and the correlation between the current unanticipated part of the plan and the future announced one. Permanent shifts in fiscal policy occur when we observe a positive correlation between the unanticipated corrections introduced when a plan is announced and the anticipated ones scheduled for the following years. When instead the correlation is negative the fiscal measures are stop and go, i.e. temporary since : the fiscal corrections introduced upon the announcement of a plan are at least partially reversed in the following years. The no correlation between the unanticipated and the future anticipated part of plans is the assumption underlying the traditional analysis of the effect of non anticipated fiscal shocks (see, for example, Mertens and Ravn(2011); we show below that this assumption is violated. Third , 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. Consider for example the case of a macroeconomic model which contains macro and fiscal variables, but does not include financial variables. By imposing some identifying restrictions on the contemporaneous correlation among the included variables (as for example in Blanchard and Perotti 2002), structural fiscal shocks can be identified by making the VAR innovations orthogonal to fluctuations in output. But this overlooks the fact that asset price fluctuations could induce a correlation between cyclically-adjusted fiscal shocks and output. For instance, a stock market boom could induce a shift in cyclically-adjusted taxes by increasing the revenue from capital-gain taxation, while at the same time affecting aggregate demand and thus output. Omitting financial variables could therefore generate a bias in the estimates of fiscal multipliers.

We use the fiscal consolidation episodes identified in Devries et al (2011) for 17 OECD countries and shown in Table 1. The countries considered are 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.[9] 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. This identification strategy applies to a panel of countries the idea originally proposed by Romer and

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

Romer (2010) for the U.S. to identify major tax policy changes not dictated by business cycle fluctuations. However, the Devries et al. (2011) shocks differ from those identified by Romer and Romer (2010) in two important dimensions. Romer and Romer 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 an inherited budget deficit. 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. This raises the possibility that the shock series is truncated. A truncation would arise if exogenous shocks that increase the deficit have occurred in the sample, but have not been included in the identified series. In practice, given the authors' strategy, 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.[10]

We first check whether the shocks identified by Devries et al (2011) are indeed exogenous, regressing them on a distributed lag of output growth. A shift in spending or taxes is exogenous for the estimation of the parameters of our interest if it cannot not be predicted by past variables. The only country for which the narrative identified fiscal shocks can be predicted by past output growth is the Netherlands, which we drop from the sample. [11]

[10] 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.

[11] 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.

3.2 Fiscal plans

Multi-year fiscal plans include, when they are announced, measures that go into effect immediately and other that are foreseen to take place at some future date. Therefore, they contain both unanticipated and anticipated shifts in taxes and expenditure. In principle even fiscal changes which are implemented today as part of a new plan could have been anticipated, but we have no way of measuring this possibility. We think however that this is unlikely to occur since the composition of fiscal adjustments is often the result of a complex political game, the result of which is quite hard to anticipate with a reasonable amount of certainty until the plan is announced and approved.

Shifts in taxes or spending (anticipated or unanticipated) are rescaled and expressed as a share of GDP at the implementation date. We define the unanticipated fiscal shocks at time t for country i as the surprise change in the primary surplus at time t:

$$e^u_{i,t} = \tau^u_{i,t} + g^u_{i,t}$$

where $\tau^u_{i,t}$ is the surprise increase in taxes announced at time t and implemented in the same year, and $g^u_{i,t}$ is the surprise reduction in government expenditure also announced at time t and implemented in the same year. We denote instead as $\tau^a_{i,t,j}$and $g^a_{i,t,j}$ 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: we thus take $i=3$ as the maximum anticipation horizon [12]. We therefore define the anticipated shocks in period t as follows

$$\begin{aligned} \tau^a_{i,t} &= \tau^a_{i,t-1,1} + \tau^a_{i,t-2,2} + \tau^a_{i,t-3,3} \\ g^a_{i,t} &= g^a_{i,t-1,1} + g^a_{i,t-2,2} + g^a_{i,t-3,3} \\ e^a_{i,t} &= \tau^a_{i,t} + g^a_{i,t} \end{aligned}$$

We label fiscal adjustments as "tax based" (TB) and "expenditure based" (EB) if the sum of the unexpected and announced tax (expenditure) changes

[12] 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.

(measured as pecent of GDP) is larger than the sum of the unexpected and announced expenditure (tax) adjustments.[13] 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 suggest that in most cases a political decision was made as to the nature of the fiscal consolidation: EB or TB. Policy reversals are possible, and are present in our data. Namely, a fiscal correction may be initially labelled as TB and then after some time the nature of the plan is changed to deliver tha majority of corrections on the expenditure side (such a shift occurred, for example, in 1991 in Canada). .At the time of the announcement we would label such a plan EB, but it would then shift to TB 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. Table A1 in the Appendix lists our classification of episodes in TB and EB. Note that we cannot observe 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.

To illustrate our classification using a specific example we consider the Australian multi-year plan which was announced in December 1984 and, with a series of subsequent adjustments, lasted until 1988. Table 1 illustrates this case

Insert Table 1 here

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 partial 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)

[13] This procedures is identical to that used by Devries et al (2011).

reduction in taxation larger that the (anticipated) increase in expenditure. Finally, it is worth noting that 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. 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 2.

Insert Table 2 here

3.3 Different types of fiscal plans

The consolidation plans which we study 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. Our coding of shocks implies that $e^u_{i,t}$ is orthogonal to $e^a_{i,t}$, because $e^a_{i,t}$ depends on information dated $t-1$ and earlier (which is obviously not the case for $e^u_{i,t}$), while there is no reason to believe that orthogonality also holds between $e^u_{i,t}$ and $e^a_{i,t,j}$ $(j > 1)$ as they all depend on information available at time t. The observed correlation between unanticipated and anticipated shifts announced at time t characterizes different fiscal policy styles. A government that typically introduces permanent fiscal plans will be characterized by zero or positive correlation between e^u_t and $e^a_{t,j}$ $(j > 1)$. Instead, a government that operates via temporary fiscal corrections will be characterized by a negative correlation between $e^u_{i,t}$ and $e^a_{i,t,j}$ $(j > 1)$. When simulating the effects of a fiscal plan we take into account a country's style. In other words, we do not consider the effect of isolated shifts in either taxes or spending–which almost never occur in our data—but we study the effect of stabilization plans which take into account the country-specific correlation, observed in the data, between unanticipated shifts and shifts announced for the future. Simulating the effects of a plan is thus different than considering

individual fiscal shocks and then assuming that their effects are identical for all countries.

3.4 Estimation

We study 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, the change in short-term (3−month) interest rates and inflation. Our baseline estimates are limited to 14 countries. We drop the Netherlands for the reason discussed above (the fiscal plans of Devries et al. (2011) are not orthogonal to observables). In the baseline we also drop Sweden and Finland because for these two countries we do not have the data on confidence. We shall show in the robustness section that our results on the macro variables are robust to the inclusions of these two countries. Since one of the channels often mentioned as a possible explanation for "non-contractionary fiscal adjustments" is, as discussed above, confidence, we also consider the (log of) the Economic Sentiment Indicator (ESI) for both consumers and firms computed by the OECD and the European Commission countries and corresponding confidence measures for other countries. The sources of our data and all data transformations are described in Table A2 in the Appendix.

We estimate a system that includes for all countries a (truncated) moving average representation of the variable of interest, $\Delta z_{i,t}$ (in turn GDP growth, private consumption growth,etc.). The system is estimated in a quasi-panel form as it explicitly allows for two types of heterogeneity: a within country heterogeneity in the effects of TB and EB plans on the dependent variable and a between country heterogeneity in the design of fiscal plans. To be able to allow efficiently for between country heterogeneity we use as indicator of fiscal corrections anticipated and unanticipated changes in the primary budget surplus, e, the sum of shocks to g and τ. In practice, the following

specification is considered:

$$
\begin{aligned}
\Delta z_{i,t} &= \alpha + B_1(L)e^u_{i,t} * TB_{i,t} + B_2(L)e^a_{i,t,0} * TB_{i,t} + \\
&\quad C_1(L)e^u_{i,t} * EB_{i,t} + C_2(L)e^a_{i,t,0} * EB_{i,t} + \\
&\quad + \sum_{j=1}^{3} \gamma_j e^a_{i,t,j} * EB_{i,t} + \sum_{j=1}^{3} \delta_j e^a_{i,t,j} * TB_{i,t} + \lambda_i + \chi_t + u_{i,t} \\
e^a_{i,t,1} &= \varphi_{1,i} e^u_{i,t} + v_{1,i,t} \\
e^a_{i,t,2} &= \varphi_{2,i} e^u_{i,t} + v_{2,i,t} \\
e^a_{i,t,3} &= \varphi_{3,i} e^u_{i,t} + v_{3,i,t} \\
e^a_{i,t} &= e^a_{i,t-1,1} + e^a_{i,t-2,2} + e^a_{i,t-3,3}
\end{aligned}
\tag{1}
$$

where λ_i and χ_t are country and time fixed effects.

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

We compute impulse responses *t*aking into account the correlation between unanticipated shocks in year t and anticipated shocks announced in year t for 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 an anticipated shocks to an unanticipated ones. Then, when we simulate the impact of a realization of $e^u_{i,t}$, we also change $e^a_{i,t,1}$ (by $\varphi_{1,i}$), $e^a_{i,t,2}$ (by $\varphi_{2,i}$),and $e^a_{i,t,3}$ (by $\varphi_{3,i}$). Note that since $e^a_{i,t}$ is orthogonal to $e^u_{i,t}$, it does not change in year t but it does in years $t+1$, $t+2$, and $t+3$, consistently with its definition.[14]In other words (1) is a quasi-panel: we impose cross-country restrictions on the equation linking $\Delta z_{i,t}$ to the fiscal variables, but we allow for two forms of within and between country heterogeneity. Impulse responses will be different for TB and EB adjustment and they will also reflect the different styles of fiscal correction typically adopted by the countries in our sample. Note that, as our impulse responses are computed by introducing a one per-cent shock in the unanticipated fiscal corrections, the total size of the adjustment will not be homogenous across countries as the response of anticipated corrections to unanticipated ones is allowed to be different across countries.

Finally, note also that the effects of permanent *vs* transitory fiscal adjustments can be gauged by comparing the impulse responses of different countries: for instance of Canada, which have normally adopted permanent adjustments (the estimates of all $\varphi's$ are positive and their sum is higher than one), with Italy, which has typically adopted transitory adjustments (φ_1.is negative for Italy).

Technically our estimation strategy is carried out as follows:

- We estimate different $\varphi_i's$ for the different countries to gauge the correlation between anticipated corrections (as of year t, for years $t+1$, $t+2$ and $t+3$) and unanticipated corrections introduced in year t. These coefficients describe the different styles of fiscal correction;

- The system is then augmented with the equations for $\Delta z_{i,t}$, (1), and estimation is carried out via SUR (Seemingly Unrelated Regressions)

[14]Note that our estimates of the $\varphi_{1,i}$, $\varphi_{2,i}$ and $\varphi_{3,i}$ parameters are simply meant to capture correlation between observable anticipated and unanticipated corrections and there is therefore no need to instrument the regressors to obtain valid estimates for our purpose.

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 in the system for $\Delta z_{i,t}$, and 14*3 parameters in the equations linking unexpected to expected shocks. Then we assess the effects of fiscal stabilizations on the path of macroeconomic variables computing impulse responses to a shift of the primary surplus (as a ratio to GDP) equivalent to one per cent of GDP. 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 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[15], 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.[16]

4 Results

In this section we present our baseline results from the estimation of (1) and the associated equations used to estimate the $\varphi's$. The estimation sample

[15] 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.

[16] 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.

includes 14 countries[17] and runs from 1981 to 2007[18] .

Table 3 illustrates the difference in the style of fiscal adjustments in various countries. In this table we report the estimates of $\varphi_{1,i}$, $\varphi_{2,i}$, $\varphi_{3,i}$ and their standard errors within brackets. We report 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 .12 to .85. 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 occurs almost exclusively via unanticipated shocks—and thus we do not have a sufficient number of observations to to estimate the $\varphi's$). At the other end of the spectrum lies Italy, where one and two–years ahead anticipations are significantly and negatively correlated with unanticipated shocks: as a consequence at least part of Italy's stabilization plans are transitory.

Insert Table 3 here

Figure 1 illustrates visually the potential importance of this point by reporting $e^u_{i,t}$ and $e^a_{i,t,1}$ for all countries in our sample.

Insert Figure 1 here

The figure shows a remarkable heterogeneity across countries in the design of their fiscal plans and confirms the results of Table 3.

[17] As discussed above we have dropped the Netherlands throughout because some of the Dutch fiscal shocks appear not to be exogenous. In our baseline results we have also dropped Finland and Sweden because data on consumer and business confidence are only available for a short sample for these two countries. As the model includes leads and lags of relevant variables the availability of data from 1978 to 2009 allows us to estimated our model over the sample 1981: 2007.

[18] Leads and lags of the fiscal corrections are included in the specification and therefore observing corrections over the period 1978-2009 allows us to estimate the model over the sampe 1981-2007.

Figure 2 gives an illustration of the different impact of TB and EB based plans by comparing the output effect of two types of adjustment implemented at the same time in different countries. We consider the output effect of the EB based adjustment implemented in Canada and of the TB based adjustment implemented in France that occurred over the same period (1995-1998): the much stronger EB based adjustment in Canada has less contractionary effect than the contemporaneous TB adjustment in France.

Insert Figure 2 here

Figure 3 reports the impulse responses of output growth to EB and TB fiscal corrections plans (in line with the literature cumulative effect on growth are reported to gauge the impact on the level of output). In Figure 3, as in all the results we show in the paper, we report *two standard errors* bands, with 95 per cent confidence intervals. Responses are reported in the figure by considering first the countries that feature a positive but mild correlation between future anticipated and current unanticipated corrections, namely Australia, Austria, Denmark, France, the United Kingdom and Japan, next we list the countries for which this correlation becomes close to zero, Belgium, Finland, Germany, Ireland, Portugal, Spain, and the United States, finally we consider the two opposite ends of the spectrum in terms of the relation between anticipated and unanticipated fiscal adjustments, i.e. 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 typically much smaller and short-lived. Interestingly, Canada features the largest difference between TB and EB plans while the smallest is observed in the case of Italy. 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.[19]

Insert Figure 3 here

[19] *An additional factor in explaining Italy is that often EB plans include cuts in transfers from the national government to local governments. But these "cuts" are then compensated by tax increases at the local level.*

Figures 4 and 5 show the response of households' consumption on durables and non-durables and business investment. 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 exception in that the response of consumption and of investment is similar.

Insert Figures 4 and 5

Figures 6 and 7 report the responses of the ESI indicators for consumer confidence and business confidence: there is some heterogeneity between TB and EB adjustments in the responses of consumer confidence, while a strong heterogeneity emerges for business confidence. The evidence from the responses of business confidence and investment is consitenty with a causal relation running from business confidence to investment and output.

Insert Figures 6 and 7

Finally, let us consider the response of monetary policy and inflation , which is reported in Figure 8-9.

Insert Figures 8 and 9

Overall, there is some evidence that monetary policy is more expansionary in the case of EB ajustment with respect to TB adjustment.[20] However, the differences in the monetary policy responses to fiscal adjustments are much smaller than those of output and the pattern of cross-country heterogeneity does not match the one observed for output. The response of monetary policy to the two types of adjustment can be justified by the response of inflation reported in Figure 9, that illustrates how tax based adjustmens are more inflationary than expenditure based adjustments. Alesina and Perotti (1997) rationalize this evidence in terms of the inflationary consequences of higher indirect taxes and the effect of higher income taxes on wages

[20] This evidence is consistent with a stronger impact on inflation of TB plans that is observable when impulse responses to inflation are analyzed (we do not report this set of impulse responses that are obtained in the replication package of all the results in the paper available from the authors' webpage)

This evidence raises potentialy the issue of the importance of accompanying monetary policy in determining the heterogenous output responses of TB abd EB based plans. We shall explicitly address this issue in the robustness section where we show more formally that monetary policy cannot explain the observed difference in output responses to EB and TB adjustments.[21]

5 Robustness

In this section we show, one by one, a number of robustness tests. We shall start from the role of monetary policy. Does the difference between TB and EB plans depend on how monetary policy responds to the two types of fiscal adjustment? Because if it did, then our results could vanish at the ZLB when the central bank can no longer reduce nominal interst rates. We shall then try to test our panel restrictions estimating the model for subsets of countries and comparing our results with those obtained by Romer and Romer (2010), albeit only for shifts in taxes and overlooking the difference between anticipated and unanticipated shfts. Next we check whether the asymmetry between the effects of TB and EB plans might be explained by the fact that the choice between the two types of adjustment is related to the cycle or to accompanying reforms, such as a liberalization of the labor market. What could be a source of concern is that the cycle, for instance, could give rise to a potential endogeneity not due to the relation between the fiscal adjustment and the cycle *(*which is ruled out by the way narrative shocks are identified*)*, but rather from a relation between the type of adjustment chosen and the cycle. The same could be true for the correlation with other reforms. Finally, we shall return to the importance of simulating plans and allowing for a difference between TB and EB plans, by comparing our results with those typically shown in the literature and based on shocks rather than

[21] 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 adjustments—overlooking plans results in much wider confidence. 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.

on plans.[22]

5.1 Monetary policy

If the asymmetry between TB and EB plans could be explained by the fact that monetary policy responds differently to the two type of plans, the apparent superiority of EB plans might vanish at the ZLB[23]. The impulse responses of output growth and monetary policy discussed in the previous section show that that the common pattern across countries of the heterogenous effect of TB and EB plans is not matched by an analogue common pattern in the response of monetary policy to the two types of adjustment. This is a first indication that the heterogenous output effect of TB and EB plans cannot be entirely abscribed to the different monetary policy response to EB and TB plans. To provide more evidence on this issue we have designed a counterfactual aimed at evaluating what would be the effect of fiscal adjustments if policy rates remained unchanged, *i.e.* if the central bank was prevented from responding to the shifts in fiscal policy. To run this counterfactual we proceed as follows. Define e^f fiscal correction (comprising both unanticipated and anticipated components), Δi_t the change in monetary policy rates, $\mathbf{e}_t^{nf}$ a vector of non-fiscal shocks, orthogonal to e^f, to which monetary policy endogenously reacts and ε_t^m exogenous monetary policy shocks. The monetary policy reaction function is described by

$$\Delta i_t = \beta_1 e_t^f + \beta_2 \mathbf{e}_t^{nf} + \varepsilon_t^m$$

What we estimate for each country is essentially the following equation

[22] 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 adjustments—overlooking plans results in much wider confidence. 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.

[23] Guajardo et al. (2011) claim that this is the case when they perform an empirical estimation of the difference in the output effect of tax based and expenditure based adjustment *Their empirical analysis is incorrect sicne it is based on shocks and not on plans like ours.*

$$\Delta y_t = \gamma(L) e_t^f + \varepsilon_t^y$$

If the specification for the monetary policy reaction function is the correct one, than estimates of γ will be a convolution of the direct response of output to fiscal shocks and to the indirect response of output to fiscal shocks that summarizes the reaction of monetary policy to fiscal shocks and the reaction of output to monetary policy. To disentagle the relative importance of the direct and the indirect channel we estimate the following augmented specification of our baseline model:

$$\begin{aligned}
\Delta z_{i,t} &= \alpha + \delta(L)\Delta i_t + B_1(L) e_{i,t}^u * TB_{i,t} + B_2(L) e_{i,t,0}^a * TB_{i,t} + \\
&\quad C_1(L) e_{i,t}^u * EB_{i,t} + C_2(L) e_{i,t,0}^a * EB_{i,t} + \\
&\quad + \sum_{j=1}^{3} \gamma_j e_{i,t,j}^a * EB_{i,t} + \sum_{j=1}^{3} \delta_j e_{i,t,j}^a * TB_{i,t} + \lambda_i + \chi_t + u_{i,t} \\
e_{i,t,1}^a &= \varphi_{1,i} e_{i,t}^u + v_{1,i,t} \\
e_{i,t,2}^a &= \varphi_{2,i} e_{i,t}^u + v_{2,i,t} \\
e_{i,t,3}^a &= \varphi_{3,i} e_{i,t}^u + v_{3,i,t} \\
e_{i,t}^a &= e_{i,t-1,1}^a + e_{i,t-2,2}^a + e_{i,t-3,3}^a
\end{aligned} \tag{2}$$

Augmenting our baseline specification with $\delta(L)\Delta i_t$ 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 corrections. The only requirement to obtain valid estimates of $\delta(L)$ is that monetary policy does not affect fiscal corrections. The distributed lag of Δi_t is significant in our ouput equation but the effect of innovations in monetary policy on output are small.

The dynamic responses of output are described in the following table:

The dynamic response of Δy_t to Δi_t			
period	t-1	t-2	t-3
coeff	-0.22	-0.15	-0.12
t-stat	-8.73	-6.69	-4.73

The impulse responses computed in this counterfactual exercise are reported together with the responses obtained in the baseline model in Figure 10.

Insert Figure 10 here

The results in Figure 10 confirms the indications obtained from the estimation of the baseline model that the different effect on output growth observed for EB and TB plans are very little explained by the different response of monetary policy to TB and EB plans.

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 coefficient of the moving average representation used to derive impulse responses. A natural question arises on the validity of such restrictions. Ideally a fully unrestricted system should be estimated in which the cross-equation restrictions adopted in the panel estimation are then explicitly tested. Unfortunately, this is not possible because the number of available observations does not allow to estimate all parameters in the unrestricted system. However, some step in this direction can be taken by separating our original system in two blocs and testing if the cross-equation restrictions are valid across the two blocs. We implement this by separating our set of countries in EMU (Austria, Belgium, France, Finland, Germany, Ireland, Italy, Portugal and Spain) and non-EMU (Australia, Denmark, United Kingdom, Japan, Sweden, US and Canada) countries. We therefore proceed to the estimation of the following system:

$$
\begin{aligned}
\Delta z_{i,t} &= \alpha + \delta_k(L)\Delta i_t + B_{1k}(L)e^u_{i,t} * TB_{i,t} + B_{2k}(L)e^a_{i,t,0} * TB_{i,t} + \\
&\quad C_{1k}(L)e^u_{i,t} * EB_{i,t} + C_{2k}(L)e^a_{i,t,0} * EB_{i,t} + \\
&\quad + \sum_{j=1}^{3}\gamma_{jk}e^a_{i,t,j} * EB_{i,t} + \sum_{j=1}^{3}\delta_{jk}e^a_{i,t,j} * TB_{i,t} + \lambda_i + \chi_t + u_{i,t} \qquad (3)\\
e^a_{i,t,1} &= \varphi_{1,i}e^u_{i,t} + v_{1,i,t}\\
e^a_{i,t,2} &= \varphi_{2,i}e^u_{i,t} + v_{2,i,t}\\
e^a_{i,t,3} &= \varphi_{3,i}e^u_{i,t} + v_{3,i,t}\\
e^a_{i,t} &= e^a_{i,t-1,1} + e^a_{i,t-2,2} + e^a_{i,t-3,3}\\
k &= 1,\ 2
\end{aligned}
$$

The impulse response for output generated by the unrestricted system reported in Figure 11 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).

Insert Figure 11

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.

The results reported in Figure 12 show 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 12

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

The empirical evidence of 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), suggests that the asymmetry between TB and EB plans might be explained by the fact that the choice between the two types of adjustment is related to the cycle. This points to a potential endogeneity problem that could arise not from the relation between the fiscal adjustment and the cycle *(*which is ruled out by the way narrative shocks are identified*)*, but rather from a relation between the type of adjustment chosen and the cycle.

A related problem is suggested by 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 these concerns we use two measures: one of the cycle, defined as the deviation of output

from its Hodrick-Prescott trend, the othet is the index of labor market reforms cosntructed 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 martket 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. Similar results are obtained when the relation between the choice between EB and TB plans and the OECD index of labout market reforms is considered.This is a rather decisive result that allows us to exclude that our findings are 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. We are saying that the difference between EB and TB cannot be explained by supply side refroms

5.4 Results with no country heterogeneity

Our paper consider a multi-country version of the approach adopted by Romer and Romer(2010) in which tax multipliers are obtained via the direct estimation of Moving Average representations[24] that projects the variable of interest (outptut growth in the baseline case) on a distributed lag of the observable shocks identified via the narrative methods.[25]. In order to facilitate the comparison of our results with the evidence available we consider in this section a simplified version of our specification that can be considered as a

[24] As we do not estimate a VAR, so it does not make any sense to compare our confidence intervals with those reported in VAR studies.

[25] *Remember however the differences in the identification strategy of the shacks between Romer and Romer (20120 and Devries et al. (2011)*

multicountry analogue of what Romer and Romer have done for the US. In practice we use our data to estimate the response of output growth to unanticipated shifts in taxes or spending. In practice, we have estimated over the sample of annual data 1981-2007 the following simplified version of our general model (1)

$$\begin{aligned}\Delta y_{i,t} &= \underset{(0.117)}{-0.51}\tau^u_{i,t} \underset{(0.120)}{-0.41}\,\tau^u_{i,t-1} \underset{(0.118)}{-0.22}\,\tau^u_{i,t-2} \underset{(0.119)}{-0.18}\,\tau^u_{i,t-3} \\ &+ \underset{(0.112)}{0.11}\,g^u_{i,t} \underset{(0.132)}{+0.08}\,g^u_{i,t-1} + \underset{(0.129)}{0.33}\,g^u_{i,t-2} + \underset{(0.124)}{0.11}\,g^u_{i,t-3} \\ &+ \lambda_i + \chi_t + u_{i,t}\end{aligned} \tag{4}$$

These results can be compared with those reported in Romer and Romer(2010) and based on the sample 1980:1-2006:2 of quarterly observations for the US :

$$\begin{aligned}\Delta y_{US\,t} &= \underset{(0.32)}{-0.34}\tau^{RR}_t \underset{(0.32)}{-0.33}\,\tau^{RR}_{t-1} \underset{(0.32)}{+0.11}\,\tau^{RR}_{t-2} \underset{(0.32)}{-0.34}\,\tau^{RR}_{t-3} \\ &\underset{(0.32)}{-0.22}\tau^{RR}_{t-4} \underset{(0.32)}{+0.08}\,\tau^{RR}_{t-5} \underset{(0.33)}{-0.64}\,\tau^{RR}_{t-6} \underset{(0.33)}{-0.53}\,\tau^{RR}_{t-7} \\ &\underset{(0.32)}{-0.18}\tau^{RR}_{t-8} \underset{(0.33)}{+0.38}\,\tau^{RR}_{t-8} \underset{(0.33)}{-0.10}\,\tau^{RR}_{t-10} \underset{(0.32)}{+0.64}\,\tau^{RR}_{t-11} \\ &\underset{(0.40)}{-0.84}\tau^{RR}_{t-12} + \lambda_{US} + u_{US,t}\end{aligned} \tag{5}$$

The estimate of the tax multiplier produced by the simplified version of our model are very similar to those generated by the R&R specifications with the main difference that the panel analysis delivers much more precise estimates. However ther are a few important caveat. First, while the tax multiplier can be immediately read off the coefficients in the R&R specification, this cannot be done in model (4) as the $g^u_{i,t}$ shocks are not orthogonal to $\tau^u_{i,t}$. In this specification multipliers can be computed only via a simulation of the model that takes into account the correlation between the tax based and the expenditure based adjustment.. If fiscal adjustment are normally implemented via coupled modification in expenditure and revenue, computing the effect a a tax adjustment keeping the expenditure unaltered is not a valid simulation. For this very reason one should also resist the tempation to conclude that the coefficients reported in model (4) suggest some mild evidence of non-keynesian effect of expenditure cuts.

To read impulse reponses directly from coefficient the adopted specification should project output change on the total adjustments (obtain by aggregating anticipated and unanticipated tax hikes and expenditure cuts) by interacting them with dummies that categorize adjustments into tax based or expenditure based accordingly to the predominant nature of the adjustment. Adopting this strategy delivers the following results:

$$
\begin{aligned}
\Delta y_{i,t} &= \underset{(0.077)}{-0.65} e^{tot}_{i,t} TB_t \underset{(0.079)}{-0.42}\, e^{tot}_{i,t-1} TB_{t-1} \underset{(0.077)}{-0.11}\, e^{tot}_{i,t-2} TB_{t-2} \underset{(0.077)}{-0.42}\, e^{tot}_{i,t-3} TB_{t-3} \\
&\quad \underset{(0.076)}{-0.077} e^{tot}_{i,t} EB_t \underset{(0.084)}{-0.16}\, e^{tot}_{i,t-1} EB_{t-1} \underset{(0.084)}{+0.36}\, e^{tot}_{i,t-2} EB_{t-2} \underset{(0.077)}{+0.18}\, e^{tot}_{i,t-3} EB_{t-3} \\
&\quad + \lambda_i + \chi_t + u_{i,t} \\
e^{tot}_{i,t} &= e^{u}_{i,t} + e^{a}_{i,t} \\
e^{u}_{i,t} &= \tau^{u}_{i,t} + g^{u}_{i,t} \\
e^{a}_{i,t} &= \tau^{a}_{i,t} + g^{a}_{i,t} \\
TB_t &= 1 \; if \quad \left(\tau^{u}_{i,t} + \tau^{a}_{i,t}\right) > \left(g^{u}_{i,t} + g^{a}_{i,t}\right) > 0 \\
TB_t &= 0 \quad otherwise \\
EB_t &= 1 \; if \quad \left(g^{u}_{i,t} + g^{a}_{i,t}\right) > \left(\tau^{u}_{i,t} + \tau^{a}_{i,t}\right) > 0 \\
EB_t &= 0 \quad otherwise
\end{aligned}
$$

Second, as pointed out by Mertens-Ravn (2011), fiscal adjustments works through plans that are partly anticipated partly not anticipated, the omission of anticipated fiscal adjustment from (4) can be a source of misspecifation. In fact, our evidence illustrates that the degree of such misspecification is variable across different countries as a consequence of the different styles with which fiscal policy is conducted. There is no misspecification in the US and in all the countries in which there is no correlation between the anticipated and the unanticiapted corrections but the case is very different from countries like Canada who tend to operate fiscal policy through plans pairing the introduction of a current adjustment with the annoucementes of a series of future measures.

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 very close to zero. 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 recovers much sooner after a spending-based adjustment than 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 thus should not vanish in a zero lower bound situation. Finally, the difference between the effects of the two types of adjustment cannot be explained by the cycle nor by systematically different choices of accompanying additional supply side reforms.

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Table 1: The multi-year stabilization plan introduced 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 2: Number of anticipated and unanticipated fiscal adjustments

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
OE	5	1	1	0	0	5	2	2	0	0	3	4
BG	7	3	3	0	0	10	3	3	0	0	4	7
CN	12	12	12	10	6	12	13	13	11	9	6	7
DK	3	2	2	0	0	2	1	1	0	0	1	4
FN	2	1	1	0	0	6	1	1	0	0	0	6
FR	5	4	4	3	1	4	2	2	0	0	7	5
BD	12	4	4	2	0	12	4	4	2	1	6	10
IR	7	1	1	0	0	5	1	1	0	0	5	2
IT	12	5	5	1	0	12	0	0	0	0	3	9
JP	7	7	7	1	0	7	2	2	0	0	7	5
NL	9	3	3	0	0	11	1	1	0	0	2	11
PT	6	0	0	0	0	6	0	0	0	0	5	2
ES	7	1	1	0	0	7	2	2	0	0	4	6
SW	3	4	4	2	1	3	4	4	2	1	0	7
UK	6	3	3	0	0	7	3	4	0	0	7	3
US	8	8	8	7	6	3	8	8	7	6	5	10

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.

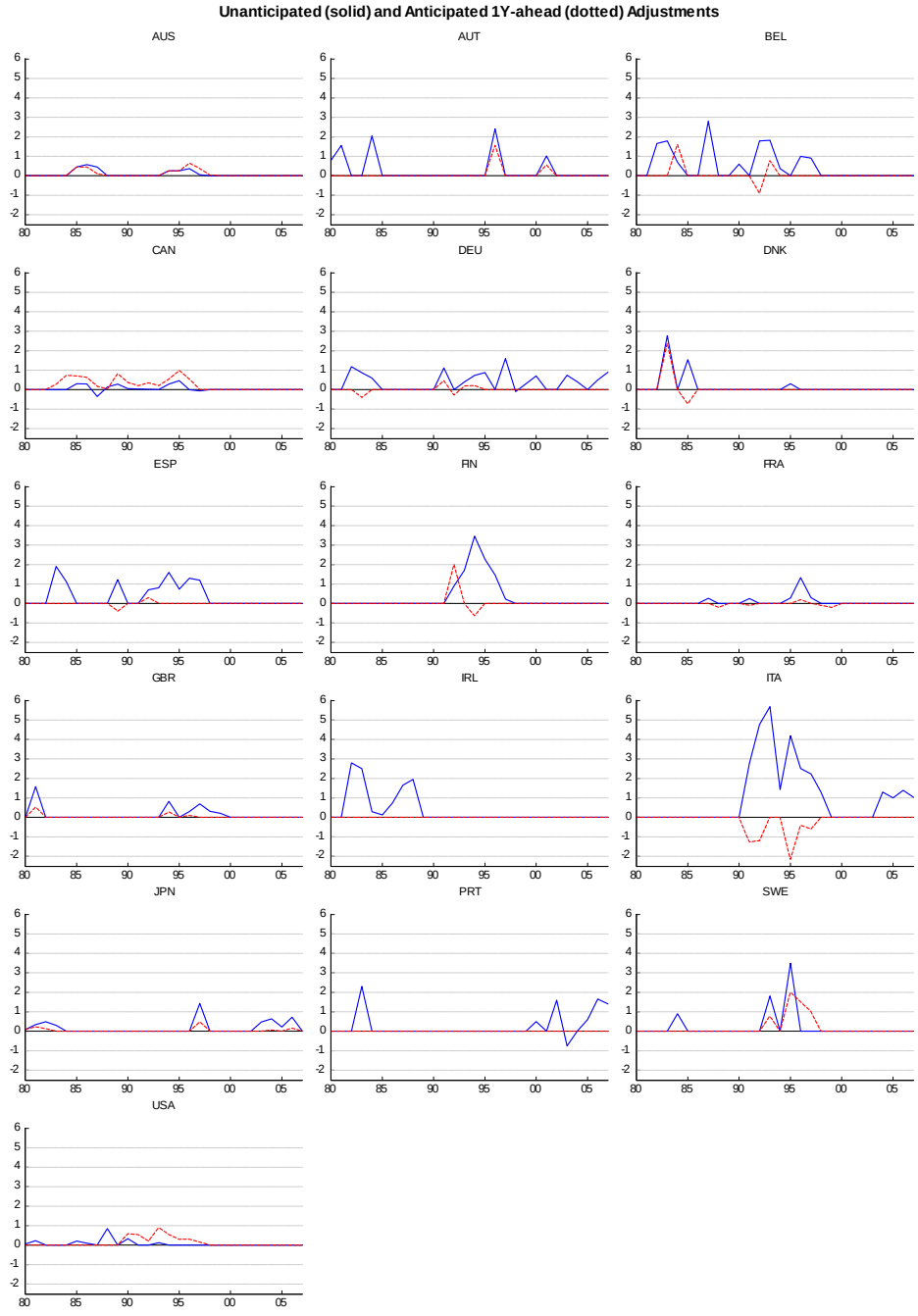

Figure 1: Unanticipated and Anticipated Fiscal Adjustments

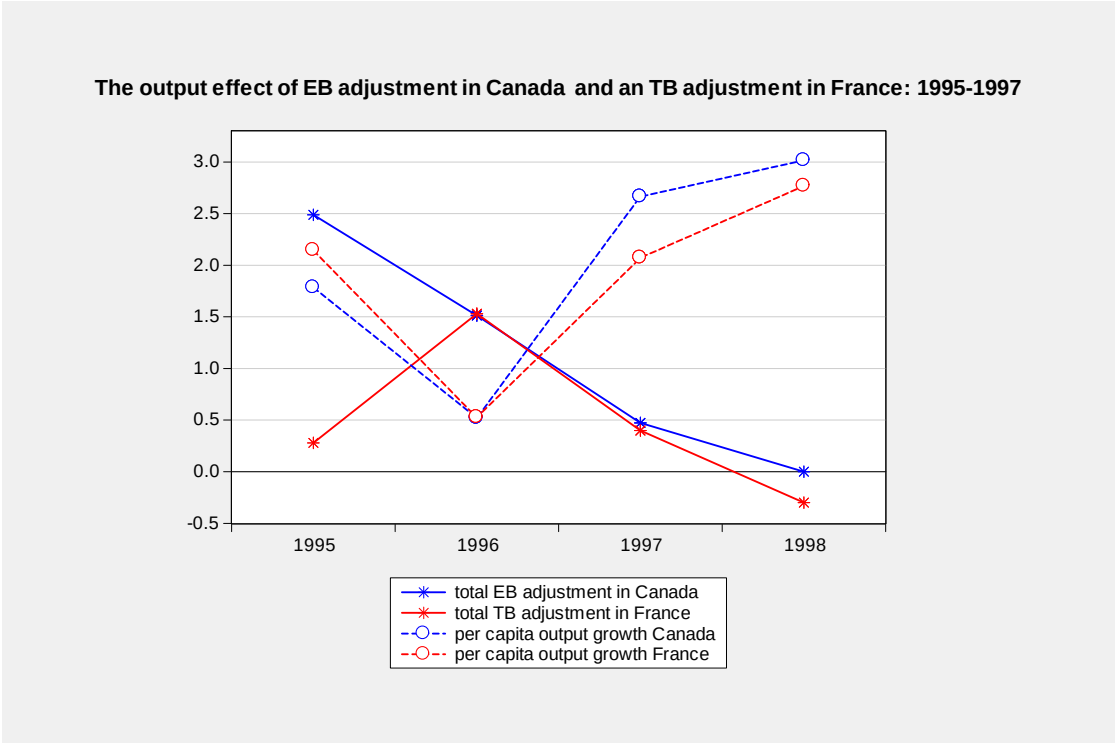

Figure 2: An episode of TB adjustment in France and of an EB adjustment in Canada

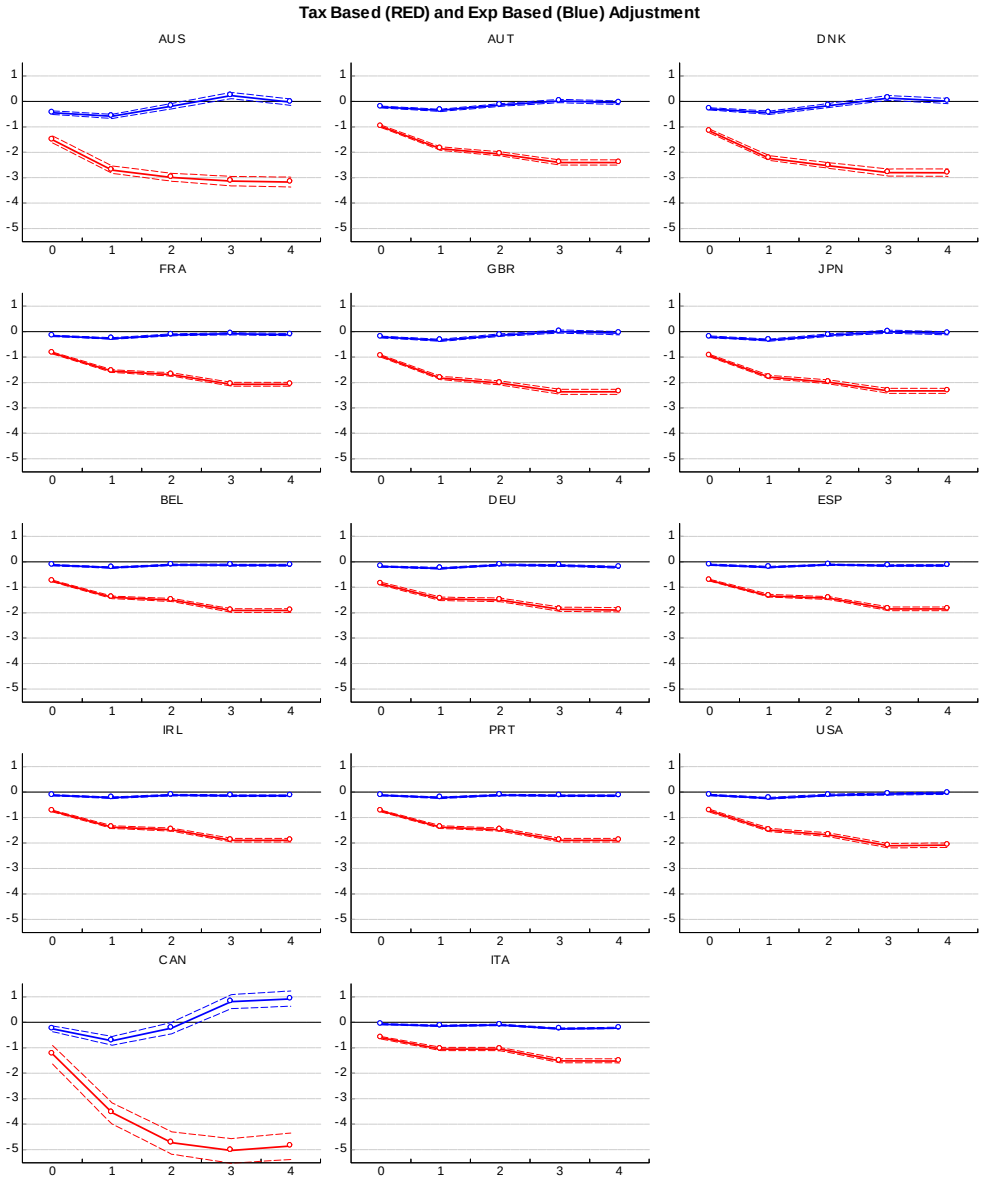

Figure 3: The effect of TB and EB adjustment on output growth

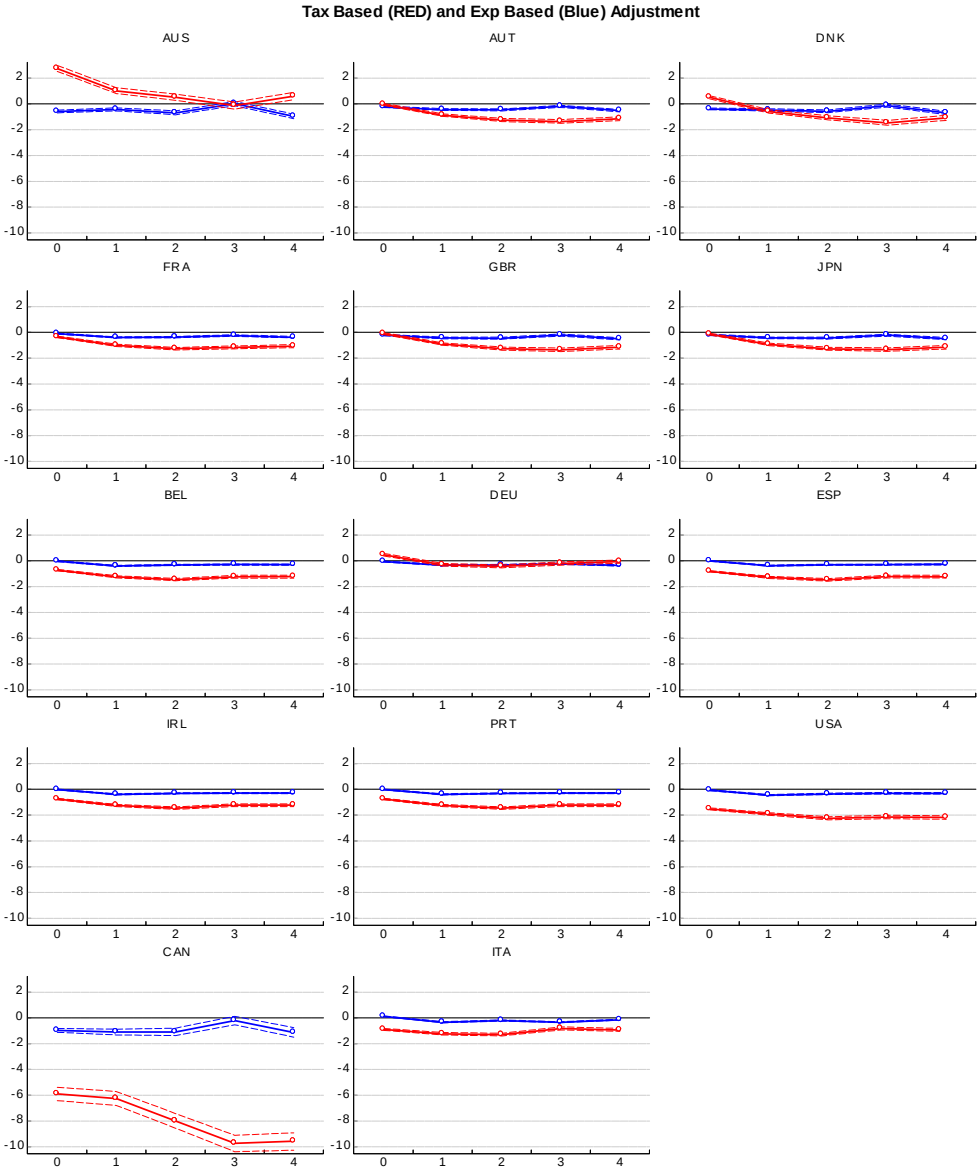

Figure 4: The effect of TB and EB adjustment on consumption growth

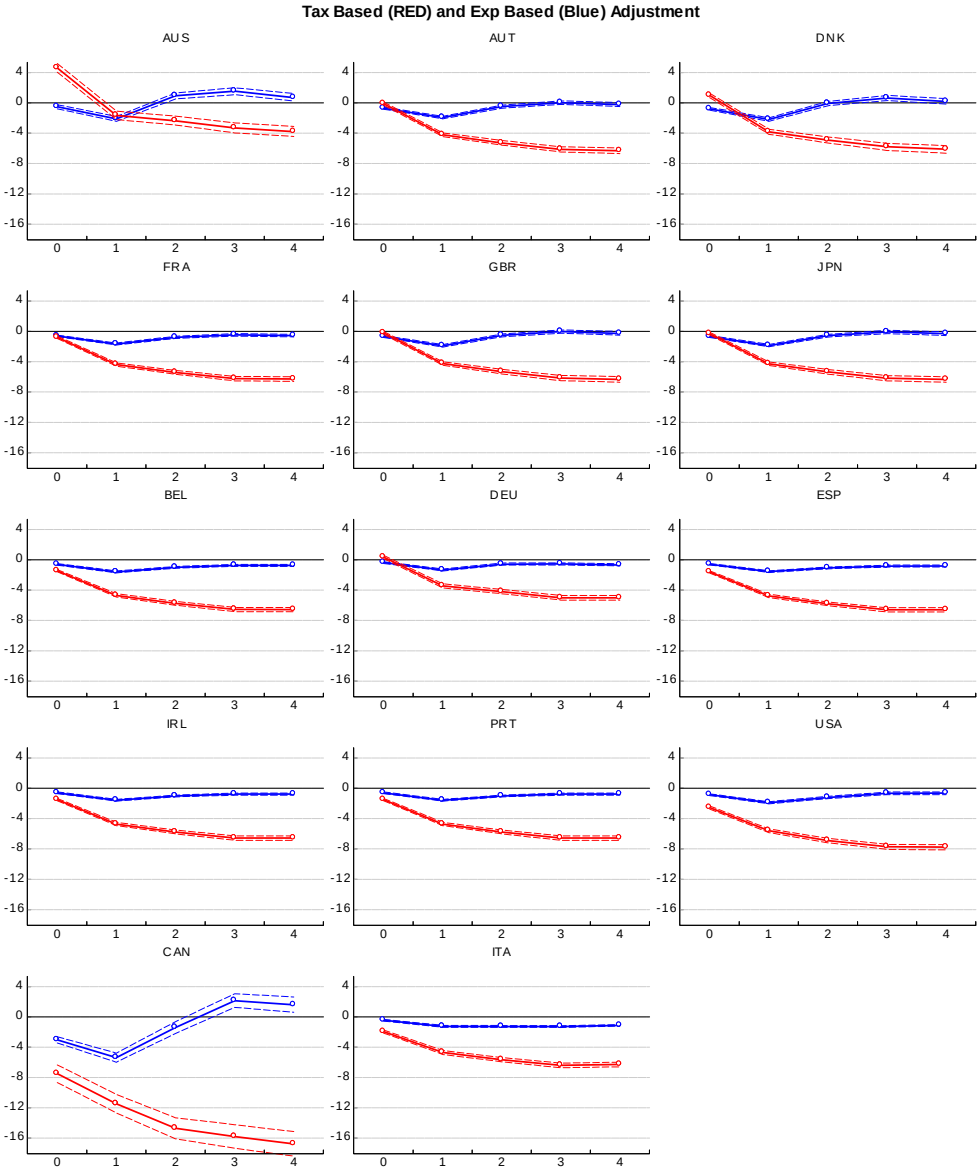

Figure 5: The effect of TB and EB adjustment on fixed capital formation growth

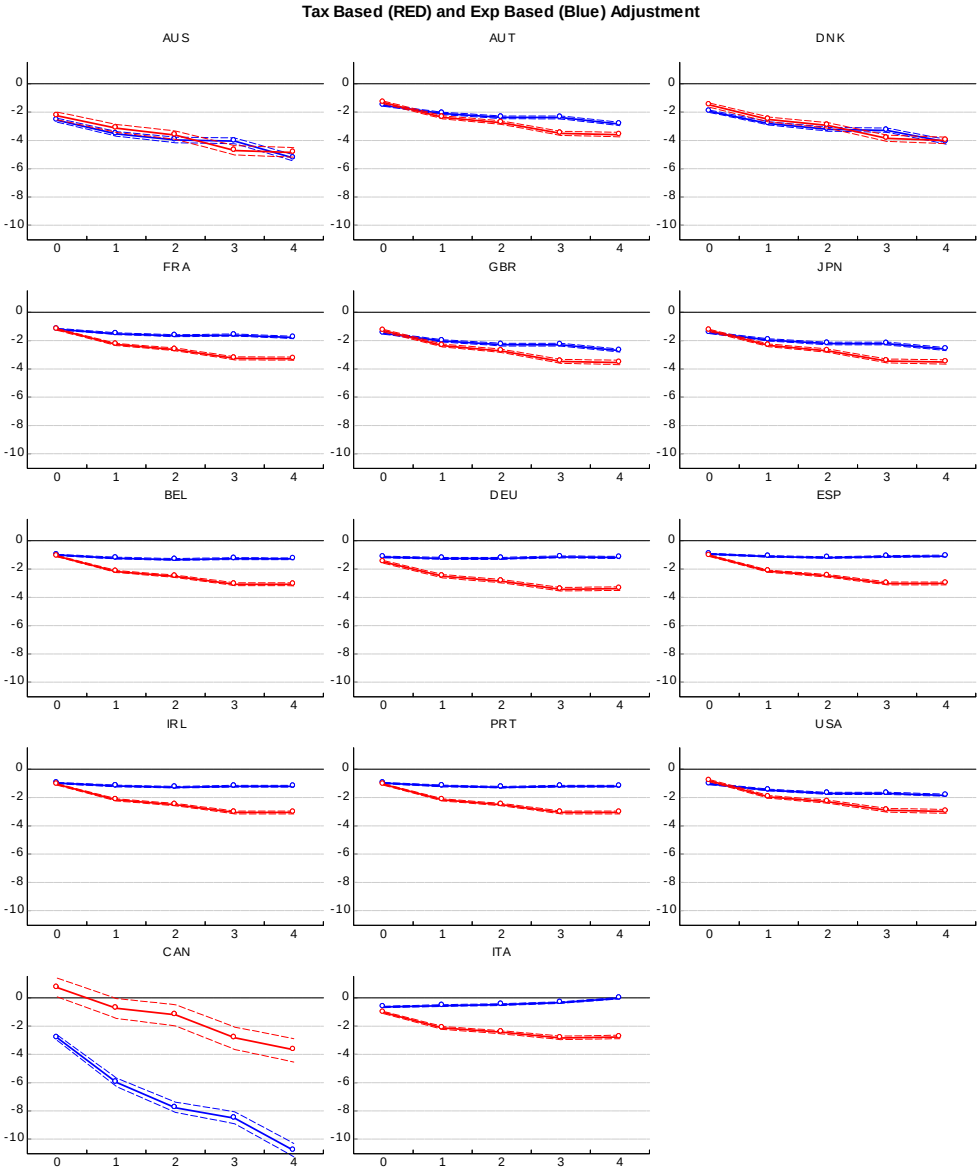

Figure 6: The effect of TB and EB adjustment on ESI Consumer Confidence

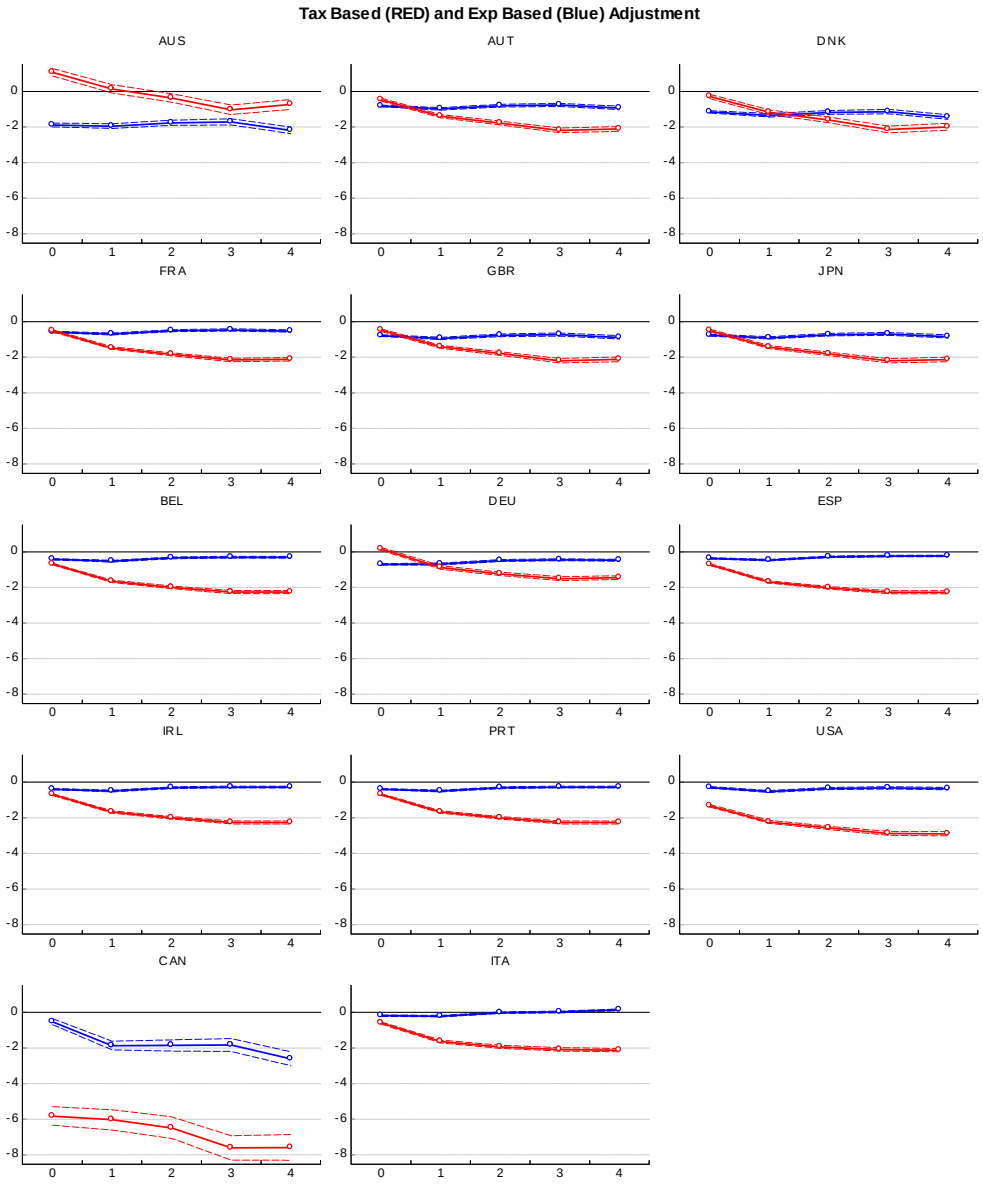

Figure 7: The effect of TB and EB adjustment on ESI Business Confidence

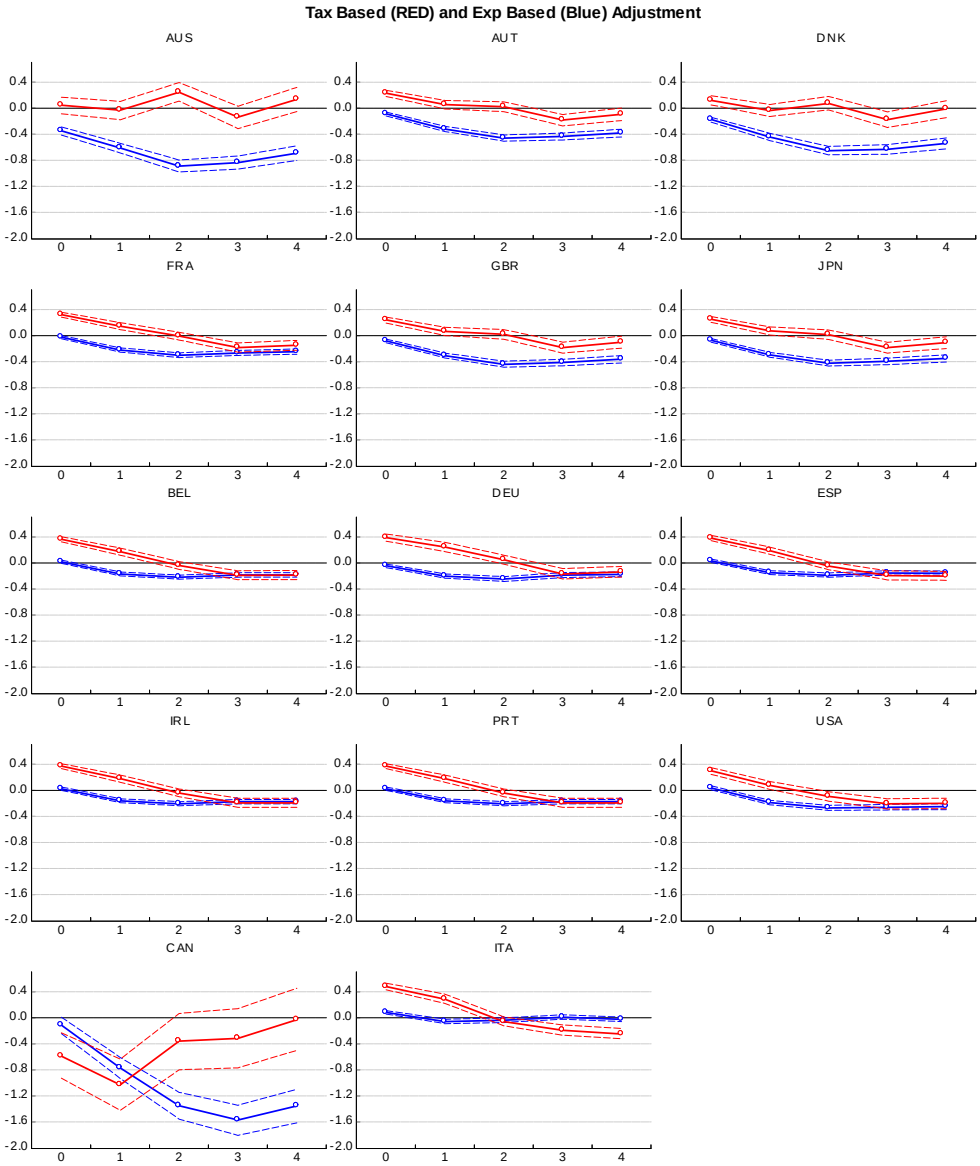

Figure 8: The effect of TB and EB adjustments on monetary policy (change in the 3M TBills Rates)

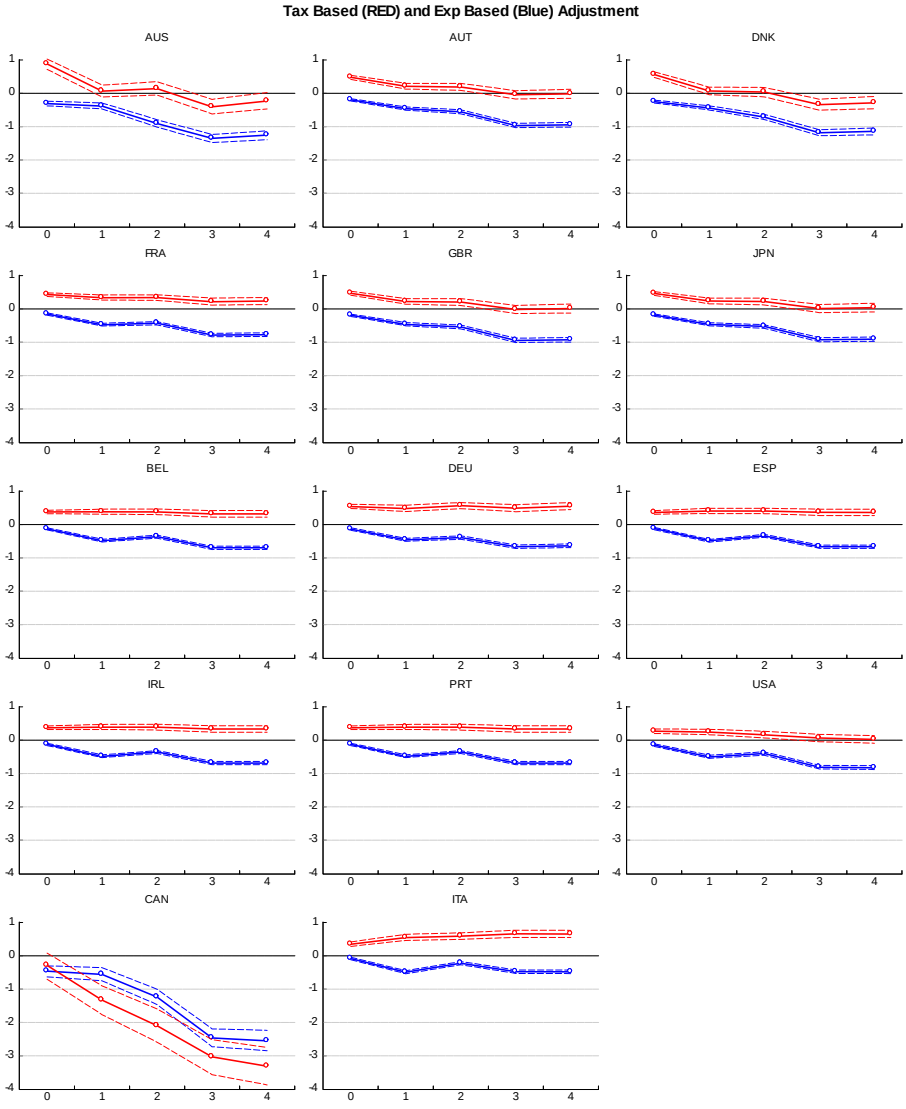

Figure 9: The effect of TB and EB adjustments on inflation (GDP deflator)

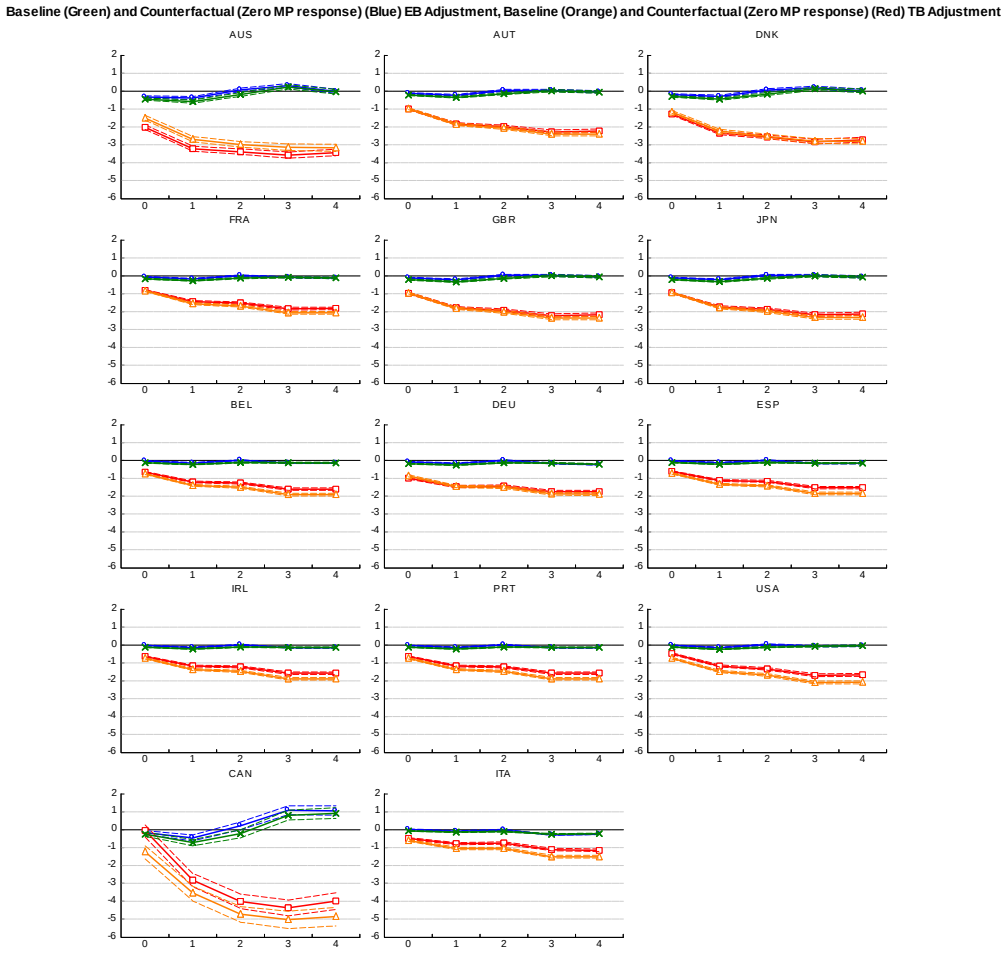

Figure 10: The effect of TB and EB adjustment: Baseline and Counterfactual

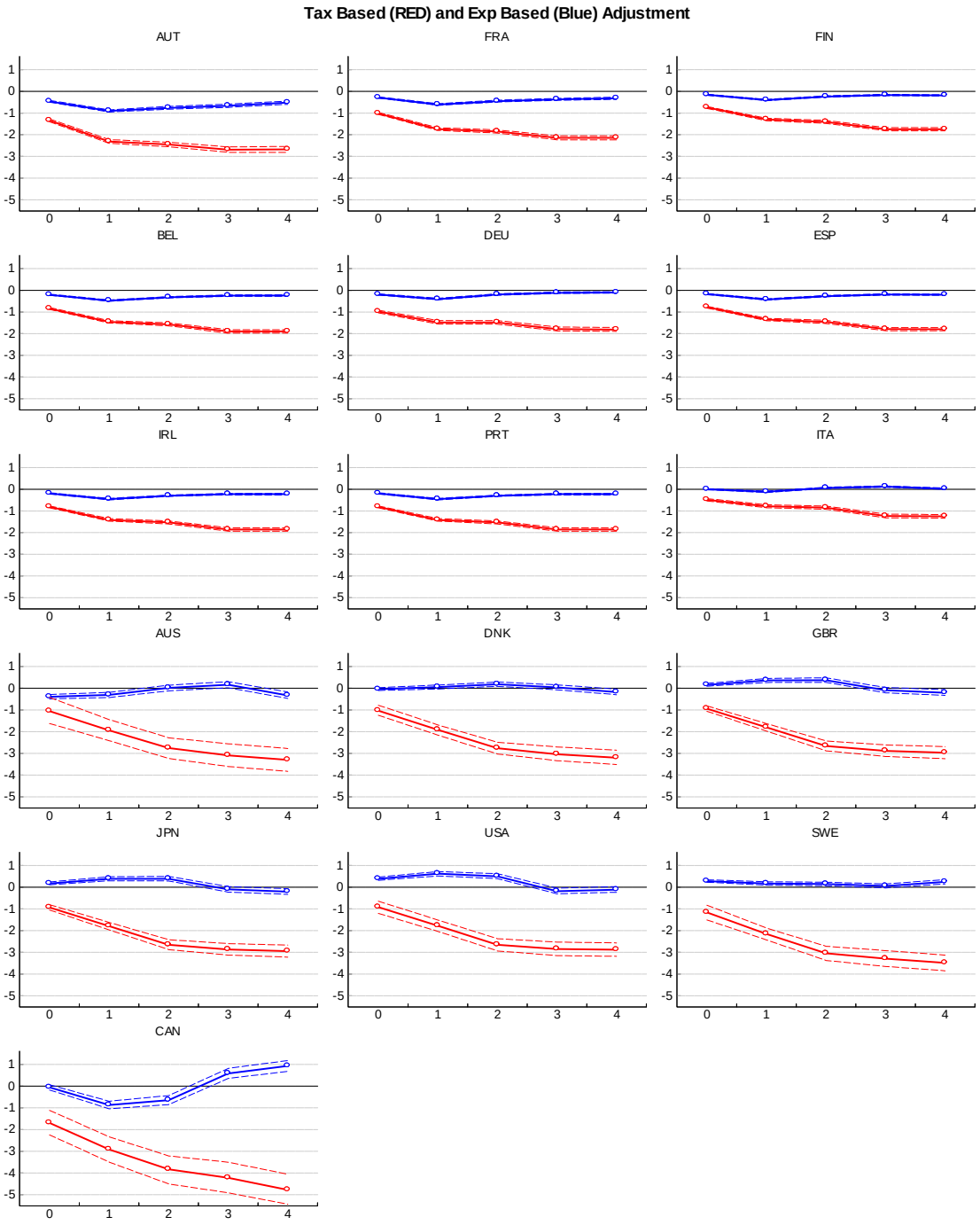
Tax Based (RED) and Exp Based (Blue) Adjustment
AUT
FRA
FIN
BEL
DEU
ESP
IRL
PRT
ITA
AUS
DNK
GBR
JPN
USA
SWE
CAN

Figure 11: Impulse responses of output allowing for different coefficients in euro area and non-euro area countries

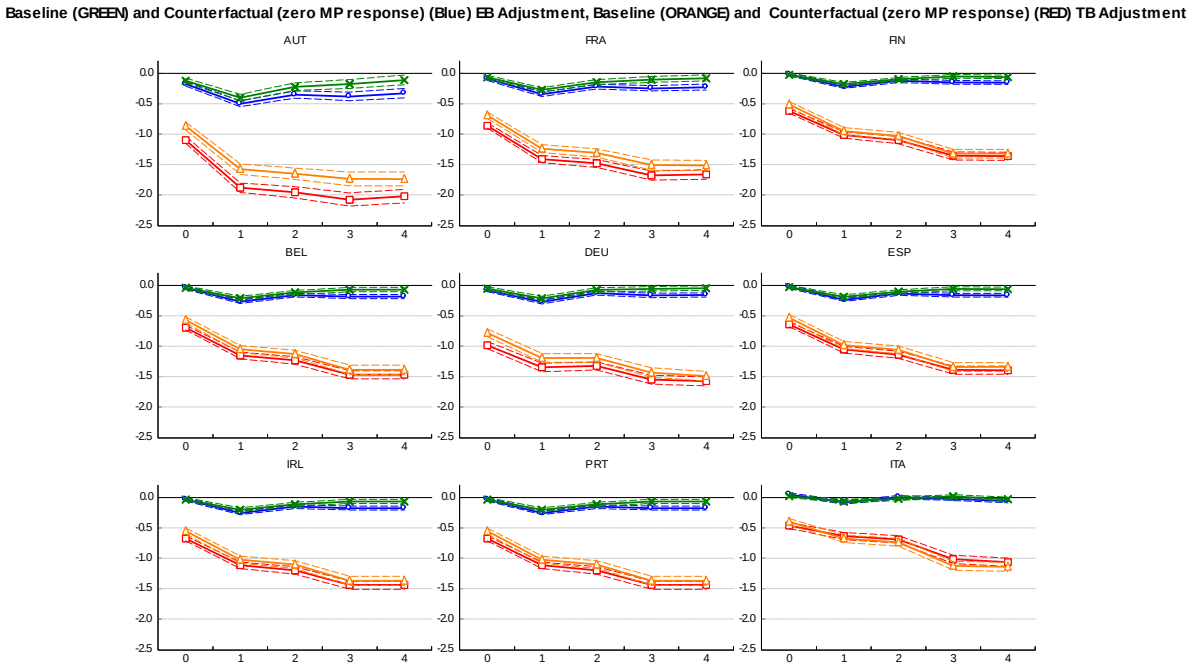

Figure 12: The effect of TB and EB adjustment both Baseline and Counterfactual for Europe

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 Commission
Business Confidence Indicator	Economic Sentiment Indicator	European Commission
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
TA	1991	2.77	1.69	1.08	1.69	0	-1.26	-1.2	0	1.08	0	0	0	0	0	1
TA	1992	3.50	1.60	1.90	2.85	-1.26	-1.2	0	0	1.92	0	0	0	0	0	1
TA	1993	4.49	2.00	2.49	3.2	-1.2	0	0	0	2.49	0	0	0	0	0	1
TA	1994	1.43	-0.27	1.70	-0.27	0	0	0	0	1.7	0	0	0	0	0	1
TA	1995	4.20	2.41	1.79	2.41	0	-2.16	0	0	1.79	0	0	0	0	0	1
TA	1996	0.34	-0.74	1.08	1.42	-2.16	-0.41	0	0	1.09	0	0	0	0	1	0
TA	1997	1.82	0.89	0.93	1.3	-0.41	-0.6	0	0	0.93	0	0	0	0	0	1
TA	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