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ABSTRACT

This paper studies whether fiscal consolidations cause large output losses. We find that it matters crucially how the fiscal correction occurs. Adjustments based upon spending cuts are much less costly in terms of output losses than tax-based ones. Spending-based adjustments have been associated with mild and short-lived recessions, in many cases with no recession at all. Tax-based adjustments have been associated with prolonged and deep recessions. The difference cannot be explained by different monetary policies during the two types of fiscal adjustments, and is mainly due to the different response of private investment. Rather than studying the effects of individual shifts in taxes and spending, as the literature has so far typically done, we study the effects of the adoption of a fiscal consolidation plan, that is a combination of tax increases and spending cuts, some unanticipated, other anticipated, all announced at the same date. This allows us to obtain much more precise estimates of tax and spending multipliers and is important also because isolated shifts in taxes or spending occur very rarely—almost never in our sample. We find that the correlation between unanticipated and anticipated shifts in taxes and spending is heterogeneous across countries, suggesting that the degree of persistence of fiscal corrections varies.

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1 Introduction

Do sharp reductions of deficits and government debts (labeled "fiscal adjustments") cause large output losses? This question is at the forefront of the policy debate, given that many OECD countries sooner or later will have to reduce their public debts. The answer which this paper provides is that it matters crucially how the consolidation occurs. Fiscal adjustments based upon spending cuts are much less costly in terms of output losses than tax-based ones. In particular, spending-based adjustments have been associated with mild and short-lived recessions, in many cases with no recession at all. Instead, tax-based adjustments have been followed but prolonged and deep recessions. The difference is remarkable in its size and cannot be explained by different monetary policies during the two type of adjustments. In fact, we find that the mild asymmetric (and lagged) response of short-term rates cannot explain the difference between the two types of adjustments: heterogeneity in the response of monetary policy appears with a lag of one to two years, while the heterogenous response of output growth to EB and TB adjustments is immediate. We find that the heterogeneity in the effects of the two types of fiscal adjustment (tax-based and spending-based) is 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. More research is needed to explore causality, above and beyond this interesting set of correlations between confidence of investors and investment and growth.

The strength and the statistical significance of our results depend crucially on the innovative approach adopted in this paper to simulate the impact of fiscal adjustments. Rather than simulating the impact of exogenous fiscal shocks, we study the response of output (and of the other variables of interest) to multi-period fiscal consolidation plans — that is sequences of tax increases and spending cuts, announced in some year and then implemented or revised in subsequent years. We allow for differences in the "style" of these plans across countries, and we show that these differences are a critical factor in order to obtain more precise estimates of the response of the economy to a consolidation plan.

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

Thus non-recessionary fiscal adjustments (or in some cases even expansionary) are possible and bring support to a vast 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, has shown that indeed spending based-fiscal adjustments can be successful and have very small or no output costs at all.[2]

The key question in estimating the effects on output of shifts in taxes or government spending is how to identify shifts that are "exogenous", that is are not a response to the state of output — as would be the case, for instance, of a fiscal expansion induced by a fall in output. 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 OECD countries over a quarter of a century (1980-2005).[3] These plans are reconstructed using the records available in official documents to identify the size, timing, and principal motivation for the fiscal actions taken by each country. Among all stabilization plans these authors have selected those that were designed to reduce a budget deficit and to put the public debt on a sustainable path: this should guarantee their "exogeneity" for the estimation of the output multipliers.

The Devries et al (2011) data allow us to study the effects of the adoption of a fiscal consolidation plan — that is a combination of tax increases and spending cuts, some unanticipated, other anticipated, all announced at the same date — rather than of individual shifts in taxes and spending, as the literature has so far typically done. This is important because individual shifts in taxes and spending occur very rarely: actually almost never in the Devries et al (2011) sample. Studying individual shifts in taxes or spend-

[2]Recent work by Perotti (2012) and Alesina and Ardagna (2012) have explored, using case studies, which accompanying policies are more likely to deliver successful and not too costly fiscal ajdustments.

[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 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).

ing thus means investigating the effects of a style of fiscal consolidation that (almost) never occurs in the data. The study of multi-year fiscal plans also allows us to make progress on question of anticipated versus unanticipated shifts in fiscal policy and permanent versus transitory shifts. A plan usually consist of some "unanticipated" correction, to be implemented in the same year of the announcement, and a series of anticipated corrections to be implemented in the following years. Importantly, there is strong evidence of a correlation between unanticipated and anticipated shifts in taxes and spending that is heterogenous across different countries. Fiscal consolidations are typically permanent policy shifts in some countries, where unanticipated corrections are positively correlated with the following anticipated ones. Other countries, instead, typically announce plans that have transitory elements, so that a negative correlation emerges between anticipated and unanticipated corrections. While it is always interesting to study the effects of announcements of future changes in taxes or spending, we highlight one particular — and so far unexplored — aspect of such anticipations: the possibility that they may signal a policy reversal. [4]

Allowing for this heterogeneity in the style of fiscal consolidations results in much more precise estimates of tax and spending multipliers. Interestingly, however, the wide variety of styles produces results that — although slightly different across countries — yield a strong common message: tax-based plans induce prolonged and deep recessions, while spending-based plans are associated with very mild and short-lived recessions, in some cases with no recession at all.

Given that the very large difference between tax-based and spending-based fiscal adjustments does not depend on the cycle or on monetary policy, what explains 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 policies could also play a role: Alesina and Ardagna (2012) and the case studies by Alesina and Ardagna (1998) 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 liberation and wage moder-

[4] Mertens and Ravn (2011), using the episodes identified in Romer and Romer (2010), investigate the effects of anticipated and unanticipated shifts in taxes, but do not consider the possibility that the two are related signalling a policy reversal.

ation. These accompanying reforms may signal a "change of regime", that is a policy switch towards a more market friendly policy stance, less taxation, liberalizations etc., perhaps in some cases agreed upon with the unions. [5] These results would be consistent with what we also find, namely a very different reaction of business confidence during spending-based and tax-based adjustments, much more negative in the latter Also, Alesina and Ardagna (2012) find that the expansionary spending-based adjustments are those in which current spending, rather than public investment is reduced. At this stage we cannot pursue this line of analysis with the Devries et al (2011) data because neither the composition of changes in taxes and spending, nor accompanying policies are recorded. Future research will need to evaluate the contribution of all these different channels to what seems a very robust result: tax-based adjustments are much more costly in terms of output losses than spending-based ones.

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 statistical procedures Section 4 illustrates our results and the last section concludes.

2 Tax-based and spending-based stabilizations: what should we observe?

In neoclassical models fiscal policy affects output through wealth effects, 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 taxation) and therefore an expansionary effect on consumption. As a consequence of the positive wealth effect, labour supply shifts upward, 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 expendi-

[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.

ture (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 (see also Baxter and King 1993). 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, as discussed for instance in Ardagna (2001). This may increase profits and therefore once again investment. As noted by Giavazzi and Pagano (1990) the positive output effect of a stabilization program can also stem from a reduction in the term spread, if the impact of the fiscal consolidation on the risk premium dominates the effect of higher consumption and investment on expected monetary policy.

Confidence could also play a role on investment (and perhaps on consumption as well). Imagine an investor unsure about the future course of taxes. The announcement of a permanent spending cut could eliminate such uncertainty and, in addition to all the other channels emphasized above, lead to an increase in his propensity to invest. In fact a related strand of the literature emphasizing the importance of uncertainty for output fluctuations (Bloom 2009, Bloom, Bond and Van Reenen 2007, Bloom and Floteotto 2007, 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 (for instance about future taxation) 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.

The "standard" Keynesian view argues, instead, that all of the above is fairly irrelevant and spending cuts are always recessionary (see e.g. De Long and Summers 2012). In models in the Keynesian tradition the multiplier for government spending is typically larger than that for taxes (Galì, Lopez-Salido and Valles 2007), although the assumption of sticky prices within a neoclassical framework tends to reduce the size of Keynesian multipliers. The empirically literature gives a different message, suggesting that tax multipliers are larger than spending multipliers (see Ramey 2012 for a survey). Multipliers are also found to be larger during recessions (Auerbach and Gorodnichenko 2012, Giavazzi and McMahon 2012), 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. The most obvious example is monetary policy. Obviously a fiscal adjustment can have different effects depending on how monetary policy responds. The latter is likely to be endogenous, since the central bank might react differently when facing a permanent and credible fiscal adjustment, or a temporary unseasonable one. Similar considerations apply to exchange rate movements [6] Monetary policy is not the only example. 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 in-

[6] See Lambertini and Tavares (2003) and Alesina and Ardagna (2012). According to the latter the role of exchange rate movements in explaing the success, or lack thereof, of fiscal adjustment is overblown by the policy discussion.

stance the announcement that spending cuts will be reversed, could signal less courageous reform programs. Alesina and Ardagna (2012) in particular find (studying both episodes defined by changes in the full-employment deficit and the Devries et al 2011 episodes) that what makes successful spending-based adjustments different from recessionary tax-based adjustments is not monetary policy but a more general "pro-reform" stance of the government, on the supply side as well as on the spending side.

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 and related to that, we can distinguish between stabilizations based on permanent shifts in fiscal policy, from those based on transitory shifts. Permanent shifts in fiscal policy occur in presence of a positive correlation between the unanticipated corrections introduced when the plan is announced and the anticipated ones scheduled for the following years. When instead the correlation between unanticipated and anticipated corrections becomes negative we are in presence of temporary measures: the fiscal corrections introduced upon the announcement of a plan are at least partially undone in the following years. 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.

[7] For a useful review of the literature see Ramey (2012), the discussion by Perotti(2012) and the Introduction in Alesina and Giavazzi (2012).

[8] As is well known, using the narrative record to identify fiscal shocks we do not need to invert the moving average 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.

Consider for example the case of a simple 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 thus generate a bias in the estimates of fiscal multipliers.

We use the fiscal consolidation episodes identified in Devries et al (2011) for 15 OECD countries and shown in Table 1.

Insert Table 1 here

The countries included are Australia, Austria, Belgium, Canada, Denmark, France, Germany, Ireland, Italy, Japan, the Netherlands, Portugal, Spain, the United Kingdom, and the United States.[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 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

[9]The dataset is available on the IMF website (http://www.imf.org/external/pubs/cat/longres.aspx?sk=24892.0). We have dropped Finland and Sweden from the sample because data on consumer and business confidence are only available for a short sample for these two countries. The results for Finland and Sweden, and the average across countries for all the other variables (excluding the confidence data), are qualitatively identical to the results that we present below. The results for Sweden and Finland are available upon request from the authors.

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 with a positive impact on 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 extremely unlikely.[10]

Measures of fiscal shocks based upon *actual* rather than *planned* shifts in fiscal variables do not allow to discriminate between anticipated *versus* unanticipated, and permanent *versus* transitory shocks, distinctions which are instead possible using the Devries et (2011) episodes and which we now describe.

3.2 Fiscal plans

Fiscal consolidations are almost always multi-year processes which include, at the time of announcement, immediate measures and future ones. Therefore, we have 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 it 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.

The size of the shocks is measured as the change in taxes and expenditures

[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.

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 at date t with an anticipation horizon of j years (*i.e.* to be implemented in year $t + j$) for country i. Consistently with the evidence from the Devries et al (2011) database, we take $i = 3$ as the maximum anticipation horizon [11]. We therefore define the anticipated shocks in period t as follows

$$\begin{aligned}
\tau^a_{i,t,0} &= \tau^a_{i,t-1,1} + \tau^a_{i,t-2,2} + \tau^a_{i,t-3,3} \\
g^a_{i,t,0} &= g^a_{i,t-1,1} + g^a_{i,t-2,2} + g^a_{i,t-3,3} \\
e^a_{i,t,0} &= \tau^a_{i,t,0} + g^a_{i,t,0}
\end{aligned}$$

Table 2 illustrates our labelling of shocks using the example of a hypothetical multi-year fiscal plan

Insert Table 2 here

Consider the case in which the fiscal authorities announce, in year 1, a multi-year plan with a three-year horizon. In year 1 there is an immediate increase in taxes of 1 per cent of GDP, followed by another increase of 0.6 per cent of GDP in year 2, no change of taxation in year 3 and a reduction in taxation of 0.4 per cent of GDP in year 4. At the same time expenditures are cut by 0.5 per cent of GDP in year 1, with further cuts of 0.4 per cent in year 2 and 0.5 per cent in year 3 and a compensatory expansion of 0.6 per cent of GDP in year 4.

In year 1 the plan is coded taking into account all unanticipated and anticipated shocks. From year 2 onwards all unanticipated shocks take the

[11] In the sample there are very few occurences of shocks anticipated four and five year ahead . Their number is too small to allow to include them in our estimation procedure.

value of zero as no further announcement is made, while anticipated shocks will change as the announcements made in year 1 travel through time. This hypothetical stabilization plan is only partially permanent, as fiscal policy moves in the opposite direction are announced for year 4. In this example there are no deviations from the year-1 announcements, that is the plan is never revised. Deviations from an initial plan can however be easily accommodated in this framework by attributing the appropriate values to unanticipated and anticipated shocks when the initial plan is revised and a new one is introduced.

3.3 Tax-based and spending-based adjustments

We label fiscal adjustments as "tax based" (TB) and "expenditure based" (EB) if the sum of the unexpected and announced tax (expenditure) changes is larger than the sum of the unexpected and announced expenditure (tax) adjustments.[12] Importantly, our 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. We account for policy reversals in the way described above. Namely, a fiscal correction may come with the announcement of a sequence of unanticipated spending cuts, but then deliver only tax increases. At the time of the announcement this plan would be labelled EB, but it would then shift to TB when spending cuts fail to materialize and are replaced by tax hikes. 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 1 lists our classification of episodes in TB and EB.

To illustrate our classification using a specific example we consider the Australian multi-year plan which was announced in 1984 and, with a series of subsequent adjustments, lasted until 1988. Table 3 illustrates this case

Insert Table 3 here

In 1984 a fiscal stabilization plan was announced featuring no change in taxation and spending cuts of $0,45$ per cent of GDP each year in 1985

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

and 1986. In 1986 the plan was revised: the new plan featured 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) 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 4.

Insert Table 4 here

3.4 The heterogeneity of fiscal plans

The consolidation plans 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. When simulating the effects of a fiscal plan we take into account a country's style. Simulating the effects of a plan is much more precise than considering individual fiscal shocks and then assuming that their effects are identical for all countries. Plans take into account the country-specific link, observed in the data, between unanticipated shifts and shifts announced for the future when the unanticipated shifts are introduced. Figure 1 illustrates visually the potential importance of this point by reporting $e^u_{i,t}$ and $e^a_{i,t,1}$ for all 15 countries in our sample.

Insert Figure 1 here

In general the correlation between unanticipated and 1-year ahead anticipated shocks is mildly positive, pointing towards a general tendency for permanent corrections, but with some cross-country heterogeneity in the degree of correlation. Italy however stands out as a clear outlier: fiscal adjustments in Italy are typically temporary affairs, the correlation between $e^u_{i,t}$ and $e^a_{i,t,1}$ is -0.18 and statistically significant. [13]On the other hand, in the U.S. and Canada fiscal corrections have a clear permanent nature: the correlation between anticipated and unanticipated shocks is positive and stronger than average.

Our coding of shocks implies that $e^u_{i,t}$ is orthogonal to $e^a_{i,t,0}$, because $e^u_{i,t,0}$ and $e^a_{i,t,0}$ depend on information dated $t-1$ and earlier, 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 $e^a_{i,t,j}$ $(j > 1)$. We shall exploit these feature of the data by modelling fiscal stabilization plans that take into account the response of anticipated shifts to unanticipated shifts observed in the sample. Consistently with what the data in Figure 1 suggest, such responses will be allowed to be heterogenous across different countries: this allows us to assess the different fiscal multipliers generated by different styles of fiscal adjustments.

3.5 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 spread between the yield on long-term government bonds (10-year) and 3-month bills and the changes in short-term (3−month) interest rates. Since one of the channels often mentioned as a possible cause of "non-contractionary fiscal adjustments", as discussed

[13]It is perhaps because of this characteristics of its fiscal plans that Italy has not been able to reduce its high level of debt over GDP in the last two decades.

above, is confidence, we also consider the (log of) the Economic Sentiment Indicator (ESI) for both consumers and firms computed by the European Commission for European countries and corresponding confidence measures for other countries. The sources of our data and all data transformations are described in Table 5 which appears in the next section.

Our specification allows for heterogeneity in the effects of TB and EB stabilizations and can accommodate different styles in the implementation of a fiscal plan, *i.e.* different correlations between unanticipated and anticipated fiscal shocks across countries. We estimate a (truncated) moving average representation of the variable of interest, $\Delta z_{i,t}$ (say the growth rate of GDP), allowing for heterogeneity in the effects of anticipated and unanticipated TB and EB adjustments

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

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 variables included. Therefore, parsimony in the specification is

paired with consistent (though not efficient) estimation. Of course 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 and no other shocks, except fiscal corrections, are included in the specification. 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. Note that although panel restrictions are imposed in the equation linking $\Delta z_{i,t}$ to the fiscal variables, the estimated system allows for cross-country heterogeneity in the style of fiscal plans.

In computing impulse responses we allow for the different styles of fiscal stabilization (permanent vs transitory) observed in the data taking into account the correlation between unanticipated shocks in year t and anticipated shocks announced in year t for years $t+1$, $t+2$ and $t+3$. In other words, our fiscal shocks are not single realizations of unanticipated or anticipated shocks, as typically done in the literature, but combinations of the two, constructed taking into account their correlation as observed in the data. 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 one. 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 $e^a_{i,t,0}$ 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. This way of simulating shocks introduces cross-country differences in impulse responses that reflect the different styles of fiscal correction adopted by the various countries. Within this framework, only the effects of fiscal adjustments that have been effectively implemented are analyzed. For instance we do not estimate the effect of a single unanticipated spending shock if such a shock has never occurred in the sample, because the country considered has always adopted plans that combine unanticipated with anticipated shocks, moving, at the same time, both taxes and spending.

The effects of permanent *vs* transitory adjustments can be gauged by

comparing the impulse responses of different countries: for instance of the U.S. and Canada, which have normally adopted permanent adjustments, with Italy, which has typically adopted transitory adjustments.

To summarize: our estimation strategy imposes cross-equation (panel) restrictions while at the same time allowing for heterogeneity in the style (permanent *vs.* temporary) of fiscal corrections, and is carried out in two steps:

- we first estimate — separately from (1) and allowing coefficients to differ across countries — the response of anticipated corrections (as of year t, for years $t+1$, $t+2$ and $t+3$) to unanticipated corrections introduced in year t. The estimation of country-specific coefficients allows the flexibility necessary for the model to be able to describe the different styles of fiscal correction adopted by the countries in our sample and illustrated in the previous sections;

- next we estimate the system for $\Delta z_{i,t}$, (1), using Seemingly Unrelated Regressions (SURE).

The overall model contains a total of 60 equations: 4 equations for each of the 15 countries. The total number of estimated parameters is 78: 18 common parameters, 15 country fixed effects in the system for $\Delta z_{i,t}$, 15*3 parameters in the equations linking unexpected to expected shocks.

Having done this 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. Impulse responses are computed simulating our estimated system of equations 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 shocks 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 via bootstrap methods, allowing explicitly for the correlation between the $\mu_{i,t}$ in each replication of the bootstrap.[14]

4 Empirical results

4.1 Data and basic statistics

Table 5 summarizes the definition of the variables which we use and their sources.

Insert Table 5 here

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. The confidence index was integrated for non European countries by adding the corresponding series available from Datastream 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.

4.2 Results

4.2.1 Basic results with no country heterogeneity

To highlight the potential of our proposed methodology we set a baseline by estimating a simple specification which uses the Devries et al (2011) shocks as a measure of fiscal adjustment, but no country heterogeneity is allowed. This is done by simulating the effects of individual unanticipated shocks,

[14] Bootstrapping requires saving the residuals from the estimated model and then iterating the following steps: a) re-sample from the saved residuals and generate a set of observation 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 1. By going thruogh 1,000 iterations we produce bootstrapped distributions for impulse responses and compute confidence intervals.

rather than plans. In this experiment — which is what the literature that uses narrative shocks typically does — we thus overlook the different styles of fiscal adjustment. We distinguish, however, between TB and EB adjustments according to the definition discussed above. In practice we estimate the following simplified version of our general model (1)

$$\Delta z_{i,t} = B_1(L)e_{i,t}^{u,IMF} * TB_{i,t} + C_1(L)e_{i,t}^{u,IMF} * EB_{i,t} + \lambda_i + \mu_{i,t} \qquad (2)$$

Figure 2.1 here

Figure 2.1 presents the basic results. These replicate and extend Figure 9 in Guajardo et al (2011), which also uses the Devries et al (2011) data and also distinguish between EB and TB adjustments. There is an important difference between all impulse responses reported in this paper and those in Figure 9 of Guajardo et al (2011): we report *two standard errors* bands, with 95 per cent confidence intervals, while Guajardo et al (2011) report *one standard error* bands, with 64 per cent confidence intervals. We find that TB and EB adjustments have effects on output which are mildly statistically different (top right panel of Figure 2.1). In the case of EB adjustments output falls slightly for a year; after about one year it is statistically indistinguishable from the pre–adjustment level, and after two years it is above the pre-adjustment level. Note that the speed of the recovery could be even faster if one were to take into account that an EB plan may start not in January of year 0 but a few months later. On the contrary, TB adjustments are followed by a more severe recession which lasts for the entire three years of our horizon. Standard errors, however, are relatively large: confidence intervals for EB and TB plans overlap in the year the plan is introduced and remain close, though not overlapping, in the following years.

The component of aggregate demand which comes closer to explain this difference between the two types of fiscal adjustments is private investment (see the other panels of Figure 2.1). The latter recovers very quickly after an EB adjustment and is above the initial level after a little more than a year. Private consumption seems to recover a little sooner in EB than in TB adjustments, but the difference is much less clear than for investment and it is statistically insignificant. The results for confidence are broadly consistent with this pattern. Business confidence (*i.e.* the confidence of investors) takes

a dip in the case of TB adjustments, while it is unaffected and then increases after an EB one. Instead we don't see much of a difference in the response of consumer confidence following either type of adjustment. Unfortunately it is impossible, with yearly data, to precisely assess the direction of causality between business confidence and output. What is certain is that the three variables, business confidence, investment and output move together. Finally, when we look at the term structure of interest rates and at the change in short-term interest rates we do not detect any significant difference between the two types of adjustment.

The basic specification discussed so far does not include time fixed effects. We report in Figure 2.2 results which are identical to those reported in Figure 2.1 except that we now include time fixed effects. Looking at the effects on output, the difference between TB and EB adjustments does not disappear, in fact it becomes even stronger. TB adjustments appear to create even deeper recessions when we control for time fixed effects, while EB ones continue to produce much smaller recessions, if any at all. The only difference is that in year 3 growth picks up less in Figure 2.2 than in 2.1. We return to why this might be the case in the section on robustness.

Figure 2.2 here

Guajardo et al. (2011) claim that this mild evidence (once one considers two standard error bands) of a difference between TB and EB adjustments can be totally explained by the different accompanying policies, in particular monetary policy. The response of monetary policy to the two types of fiscal adjustments is reported in Figure 2.2. We find a mild significant difference with more restrictive monetray policies being associated with TB-based consolidations, and more expansionary policies being associated with EB-based ones. Is this mild evidence sufficient to discard an asymmetric effect of TB and EB adjustments and to abscribe this asymmetry to accompanying monetary policies? We shall further investigate this isssue after having introduced our innovation, that is when we simulate plans rather than shocks and we allow for heterogeneity among plans and our answer will be "no" : this small difference in monetary polciy cannot explain the differcne in the repsonse of output to EB and TB adjustments..

4.2.2 Allowing for heterogeneity in the style of fiscal plans

The average results described in the previous sub-section overlook fiscal plans: we only considered unanticipated shifts in taxes or spending, overlooking the anticipated shifts that are contemporaneously announced. We now return to the specification in (1). As already discussed, this specification allows for the simulation of "plans" rather than individual shocks. This is important because unanticipated and anticipated shocks are not independent: they move together according to the style of implementation of the plans adopted in the sample by each country. Simulating the effects of individual shocks (unanticipated or anticipated) would mean studying fiscal experiments that the countries in our sample have never run. Styles differ across countries and heterogeneity is driven by the different estimated parameters $\varphi_{1,i}$, $\varphi_{2,i}$, $\varphi_{3,i}$ (see 1) that describe the correlations between $e^a_{i,t,j}$and $e^a_{i,t,j}$, *i.e.* between the corrections announced by the fiscal authorities of country i at date t, with an anticipation horizon of j years, and $e^u_{i,t}$,the unanticipated fiscal correction announced and implemented in year t.

The importance of cross-country of heterogeneity in fiscal plans is illustrated in Table 6. 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 (in most cases none) available for estimation. The analysis of the response of anticipated to unanticipated fiscal shocks reveals interesting cross-country heterogeneity in fiscal plans. At one end of the spectrum we have the U.S. and Canada where one-year ahead and two-year ahead anticipations are significantly and positively correlated with unanticipated shocks: in these countries stabilization plans are *permanent* corrections. At the other end of the spectrum lies Italy, where one-year ahead anticipations are significantly and negatively correlated with unanticipated shocks: as a consequence at least part of Italy's stabilization plans are *transitory*. Inbetween these two extremes lie most countries, with a low but significant positive response of one-year ahead expected corrections to current ones. Portugal and Ireland are exceptions in that adjustments occurs almost exclusively via unanticipated shocks.

Insert Table 6 here

To sum up: in the experiments that we shall now report, panel cross-country restrictions are imposed on all coefficients except, obviously, for the

country fixed effects. However, the response of all variables to fiscal shocks is allowed to be different in TB and EB episodes, and cross-country heterogeneity is allowed in the response of anticipated shocks to unanticipated ones.

Figure 3 reports the impulse responses of output growth to EB and TB fiscal corrections plans. The patterns differ across countries 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 the three years of the time horizon. In the case of EB adjustments in some countries output does no move, *i.e.* there is no recession, in others (U.S. and Canada, for example) there is a short recession and then in year 2 output is above the pre-adjustment level.

Insert Figure 3 here

Figures 4 and 5 show the response of households' consumption and business investment. The results clearly indicate that the heterogeneous effect on output growth of TB and EB adjustments is to be attributed to the dynamics of gross fixed capital formation, rather than to that of private consumption. There is no evidence of heterogeneity in the response of consumption growth to TB and EB adjustments, while the response of investment growth mirrors that of output.

Insert Figures 4 and 5

Figures 6 and 7 report the responses of the ESI indicator for consumer confidence and business confidence: there is no heterogeneity in the responses of consumer confidence, while a strong heterogeneity emerges for business confidence between TB and EB adjustments. One interpretation is that causality runs from business confidence to investment and output, but a more refined analysis looking at monthly data would be necessary to disentangle what leads what.

Insert Figures 6 and 7 here

Finally, let us consider monetary policy. This is important since the heterogeneity we observe between TB- and EB-based adjustments could be the product of a different response of monetary policy to the two types of adjustments — a point we raised in discussing Figures 2.1 and especially 2.2. Figure 9 shows the response of monetary policy rates (3-month interest rates); Figure 8 that of the spread between long (10-year) and short (3-month) rates The results rule out the possibility that the channel for heterogeneity runs through monetary policy, either via the spread between long-term and short-term interest rates (that does not show any significant difference in the response to TB- and EB-based adjustments), nor via monetary policy rates, whose response also displays no heterogeneity. This result provides evidence against the claims in Guayardo et al (2011). Importantly, in order to break the link between the asymmetric effect on output of TB and EB adjustments and accompanying monetary policies it is key to allow for cross-country heterogeneity. While the evidence of asymmetric effects of TB and EB adjustments on ouput growth is uniform across countries, the accompanying monetary policies are very different across countries: in several cases the size and the timing of the asymmetry observed in monetary policy between the TB and EB adjustment cannot explain the output effects of fiscal adjustments. We shall return once again to this point in the next section on robustness.

Summing up. Estimating the effects of fiscal plans, rather than individual fiscal shocks, we obtain much more precise estimates of tax and spending multipliers. Interestingly, however, the wide variety of fiscal styles produces results that — although slightly different across countries [15] — yield a strong common message.

Insert Figures 8 and 9 here

4.2.3 Robustness

Time fixed effects

[15] The fact that results are not that different across countries should not come as a surprise. Remember that the system is estimated imposing cross-country resrictions, that is the parameters in the model are resricted to be identical across countries.The only differences arise from the differences across countries in the correlation between unanticipated and anticipated shocks.

In this section we investigate the sensitivity of our results to key aspects of our identification strategy. First, we claimed that the direct estimation of the moving average representation limits the impact of omitted variables, because, if our identification strategy is valid, any omitted information is orthogonal to the anticipated and unanticipated shocks included in the model: thus it should not affect the point estimates of our impulse response functions. To provide further evidence on this issue we have re-run our original specification augmenting it with time dummies. Time dummies capture any common shocks affecting all countries in our panel. Not surprisingly time dummies are significant in all equations of our specification. Figure 10 to 16 illustrate the point reporting the responses of all variables to TB and EB adjustments based on a model augmented with time dummies. The heterogeneity in the response of output to the two types of adjustments is virtually unaltered. There is no more evidence of an expansionary effect of EB adjustments, but the heterogeneity between the effects of TB and EB adjustments is clearly robust. Also robust is the evidence suggesting that the crucial channel for heterogeneity goes through business confidence and investment.

Monetary policy

Looking at monetary policy, we now observe some heterogeneity in the response of monetary policy to EB and TB adjustments. This evidence, however, is not robust across countries, while the heterogeneity in the effects on output of TB and EB corrections remains strongly robust in all countries. Consider, for example, the cases of Italy and the U.S.. In both countries there is signifcant and strong evidence of an heterogenous effect on output growth of TB and EB adjustments. Instead, when we consider the effect of TB and EB adjustments on monetary policies, important differences emerge between the two countries. In the case of Italy the accompanying monetary policy, when TB adjustment are implemented, is initially slightly more restrictive, but the differences between the accompanying monetary policies disappears over time. In the case of the U.S. the pattern is very different, with very little initial difference in the response to the two types of adjustment, that becomes sizeable over time when the monetary policy that accompanies TB adjustments becomes much more restrictive than that accompanying EB adjustments.

To sum up: we observe a very similar differential effect on output growth of TB and EB adjustments in Italy and the U.S., while the impact on monetary policy of TB and EB adjustments in the two countries is very different.

This evidence rules out the possibility that the asymmetric output effect of TB and EB adjustments is driven by the accompanying monetary policy. Note that one could not come to this conclusion using the Gujardoet al (2011) methodology, since it does not allow for country heterogeneity when simulating the effect of fiscal adjustments. Moreover, the heterogeneity in the response of monetary policy is not reflected in a similar heterogeneity in the response of the term spread. Importantly, heterogeneity in the response of monetary policy appears with a lag of one to two years, while the heterogenous response of output growth to EB and TB adjustments is immediate. Incidentally note that the response of monetary policy to a fiscal plan is not exogenous to the nature of the plan. For instance a central bank may be more willing to ease if it perceives a "change of regime" in the stance of the fiscal authority which engages in a aggressive EB adjustment, stopping for instance the growth of entitlements and other automatic spending programs On the contrary, the same monetary authority may be worried by fiscal plans based upon one-off tax increases.

Expansions and recessions

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. To address this concern we have constructed a measure of the cycle, defined as the deviation of output from its Hodrick-Prescott trend. We then run a binary choice (panel) probit regression of the dummies identifying TB and EB episodes on this measure of the cycle. We find no evidence of a relation between the cycle 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. 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. As shown above, the heterogeneity of the effects of TB and EB plans is indeed robust to the inclusion of time dummies in the specification.

Insert Figures 10 to 16

5 Conclusions

We have studied the effects on the economy of fiscal consolidation plans identifying such plans with the narrative method. Allowing for cross country heterogeneity in the style of fiscal adjustments delivers estimates which are much more precise than those obtained studying the effects of individual fiscal shocks within an aggregate cross country analysis. The key result is that while expenditure-based adjustments are not recessionary, tax-based ones create deep and long lasting recessions. The aggregate demand component which reflects more closely the difference in the response of output to ECB and TB adjustments is private investment. The confidence of investors proceeds with the economy and therefore recovers much sooner after a spending-based adjustment than after a tax-based one. The differences between the two types of adjustments appears not to be explained by a different response of monetary policy. These results are consistent with the descriptive statistics presented in Alesina and Ardagna (2012) who show that the fiscal stabilizations which have the mildest effect on output are those that are accompanied by a set of structural reforms which signal a "decisive" policy change. They (like us) do not find any difference in the monetary policy stance between spending-based and tax-based adjustments, but mostly differences in the policy packages regarding supply side reforms and liberalizations.

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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	0	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	0	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
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
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

Table 1: Classification of fiscal adjustments

		Total	Tax	Spend	Tax					Spend					TB	EB
					u,t	a,t	a,t+1	a,t+2	a,t+3	**u,t**	a,t	a,t+1	a,t+2	a,t+3		
JPN	1979	0,12	0,12	0,00	0,115	0	0,123	0,031	0	0	0	0	0	0	1	0
JPN	1980	0,21	0,21	0,00	0,09	0,123	0,091	0	0	0	0	0	0	0	1	0
JPN	1981	0,43	0,43	0,00	0,342	0,091	0,227	0	0	0	0	0	0	0	1	0
JPN	1982	0,71	0,31	0,40	0,085	0,227	0,057	0	0	0,398	0	0,065	0	0	0	1
JPN	1983	0,42	0,06	0,37	0	0,057	0	0	0	0,3	0,065	0	0	0	0	1
JPN	1997	1,43	0,98	0,45	0,975	0	0,325	0	0	0,45	0	0,15	0	0	1	0
JPN	1998	0,48	0,33	0,15	0	0,325	0	0	0	0	0,15	0	0	0	1	0
JPN	2003	0,48	0,00	0,48	0	0	0	0	0	0,48	0	0	0	0	0	1
JPN	2004	0,64	0,19	0,45	0,188	0	0,063	0	0	0,45	0	0	0	0	0	1
JPN	2005	0,28	0,06	0,22	0	0,063	0	0	0	0,22	0	0	0	0	0	1
JPN	2006	0,72	0,45	0,27	0,45	0	0,15	0	0	0,27	0	0	0	0	1	0
JPN	2007	0,15	0,15	0,00	0	0,15	0	0	0	0	0	0	0	0	1	0
NLD	1981	1,75	0,53	1,22	0,53	0	0	0	0	1,23	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
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

Table 2: The classification of fiscal adjustments. An example

time	$\tau^u_{i,t}$	$\tau^a_{i,t,,0}$	$\tau^a_{i,t,1}$	$\tau^a_{i,t,2}$	$\tau^a_{i,t,3}$	$g^u_{i,t}$	$g^a_{i,t,0}$	$g^a_{i,t,1}$	$g^a_{i,t,2}$	$g^a_{i,t,3}$
1	1	0	0.6	0	-0.4	0.5	0	0.4	0.5	-0.6
2	0	0.6	0	-0.4	0	0	0.4	0.5	-0.6	0
3	0	0	-0.4	0	0	0	0.5	-0.6	0	0
4	0	-0.4	0	0	0	0	-0.6	0	0	0

Table 3: 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 4: 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
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 5: 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 6: Cross countries heterogeneity in the design of multi-year plans

	AU	OE	BG	CA	DK	DEU	FR	
$\varphi_{1,i}$	0.37 (0.02)	0.26 (0.03)	0.16 (0.056)	0.38 (0.02)	0.35 (0.04)	0.08 (0.04)	0.13 (0.025)	
$\varphi_{2,i}$	−0.004 (0.028)	0	0	0.16 (0.017)	0	−0.07 (0.03)	0.01 (0.02)	
$\varphi_{3,i}$	0	0	0	0.017 (0.009)	0	0	0	
	IR	IT	JP	NL	PT	SP	UK	US
$\varphi_{1,i}$	0	−0.18 (0.07)	0.21 (0.02)	0.07 (0.03)	0	0.0006 (0.02)	0.22 (0.02)	0.26 (0.015)
$\varphi_{2,i}$	0	0.005 (0.035)	0.004 (0.02)	0	0	0	0	0.18 (0.014)
$\varphi_{3,i}$	0	0	0	0	0	0	0	0.13 (0.011)

The following equations are estimated:

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

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

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

$e^a_{i,t,j}$and $e^a_{i,t,j}$ 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+i) 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.

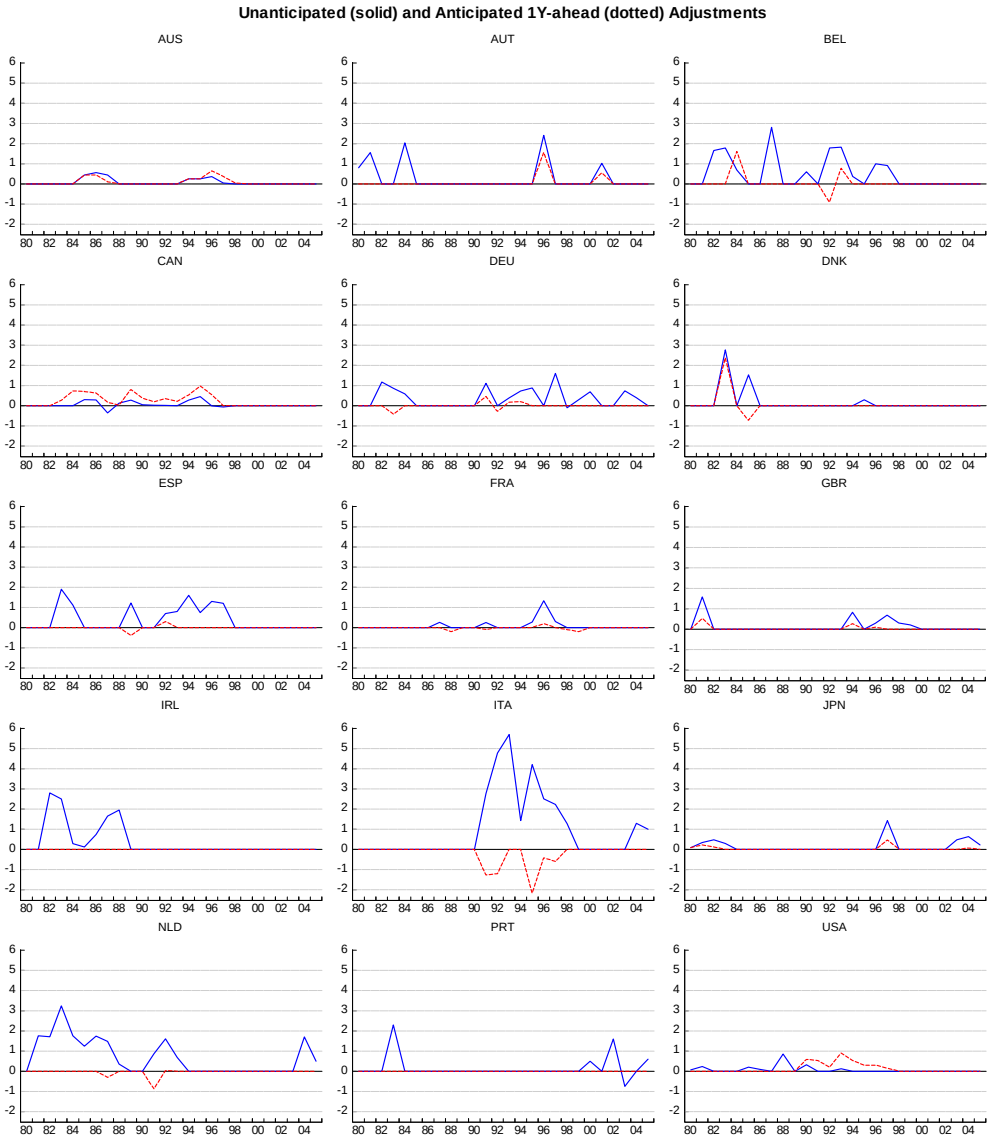

Figure 1: Unanticipated and Anticipated Fiscal Adjustments

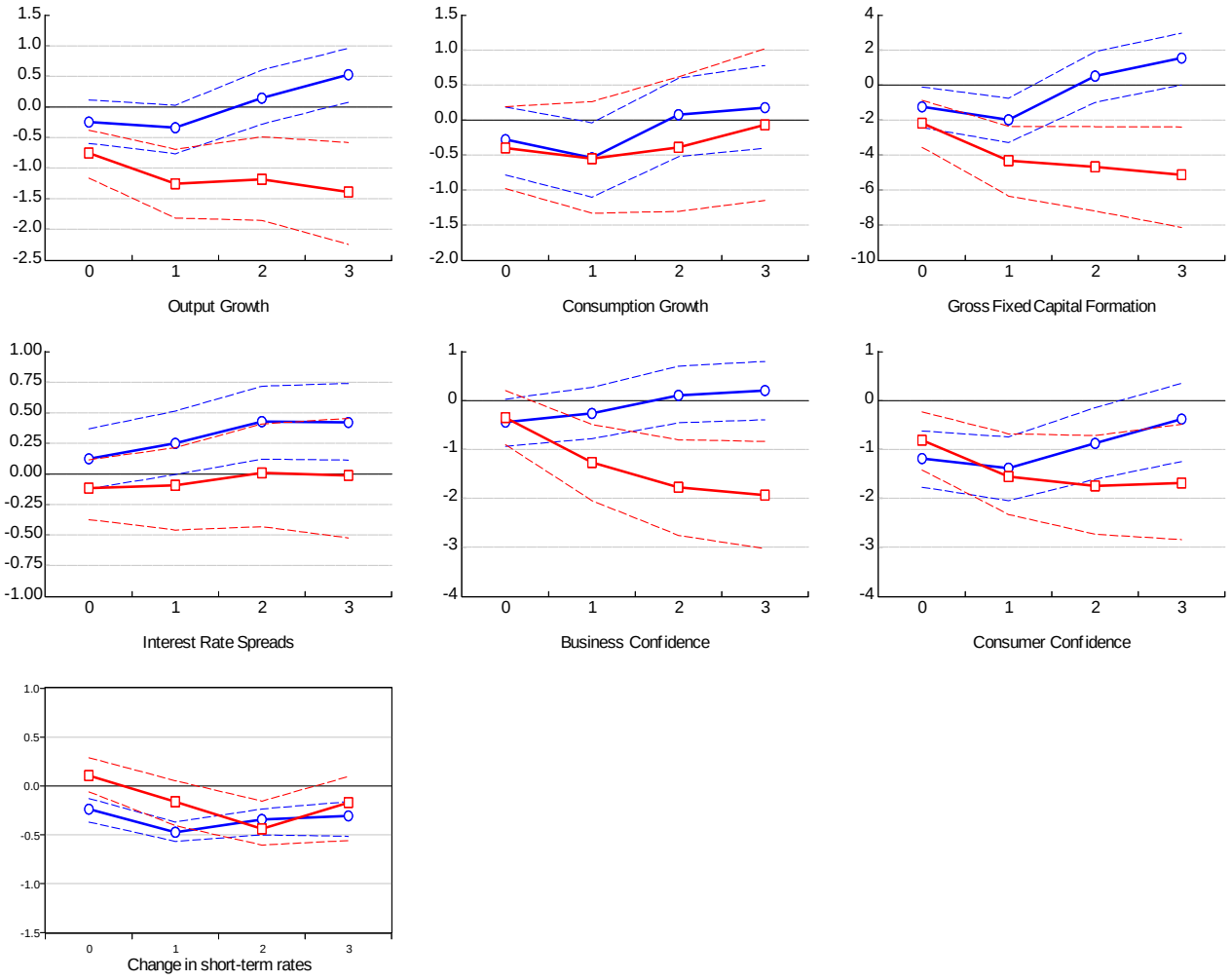

Figure 2.1: the effect of TB(squares) and EB(circles) adjustment based on the IMF shocks.No country heterogeneity, no plans, just shocks without time dummies

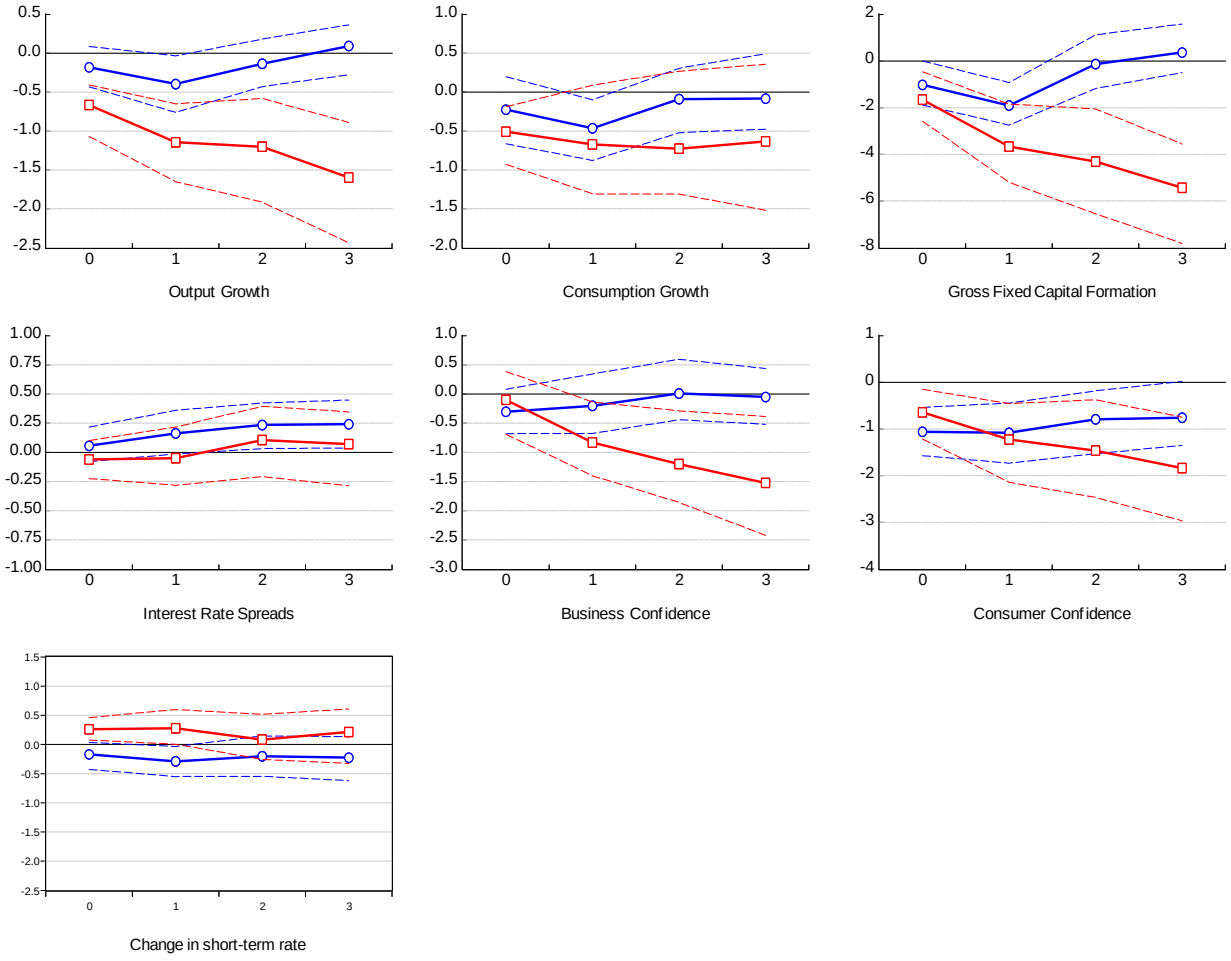

Figure 2.2: the effect of TB(squares) and EB(circles) adjustment based on the IMF shocks.No country heterogeneity, no plans, just shocks with time dummies

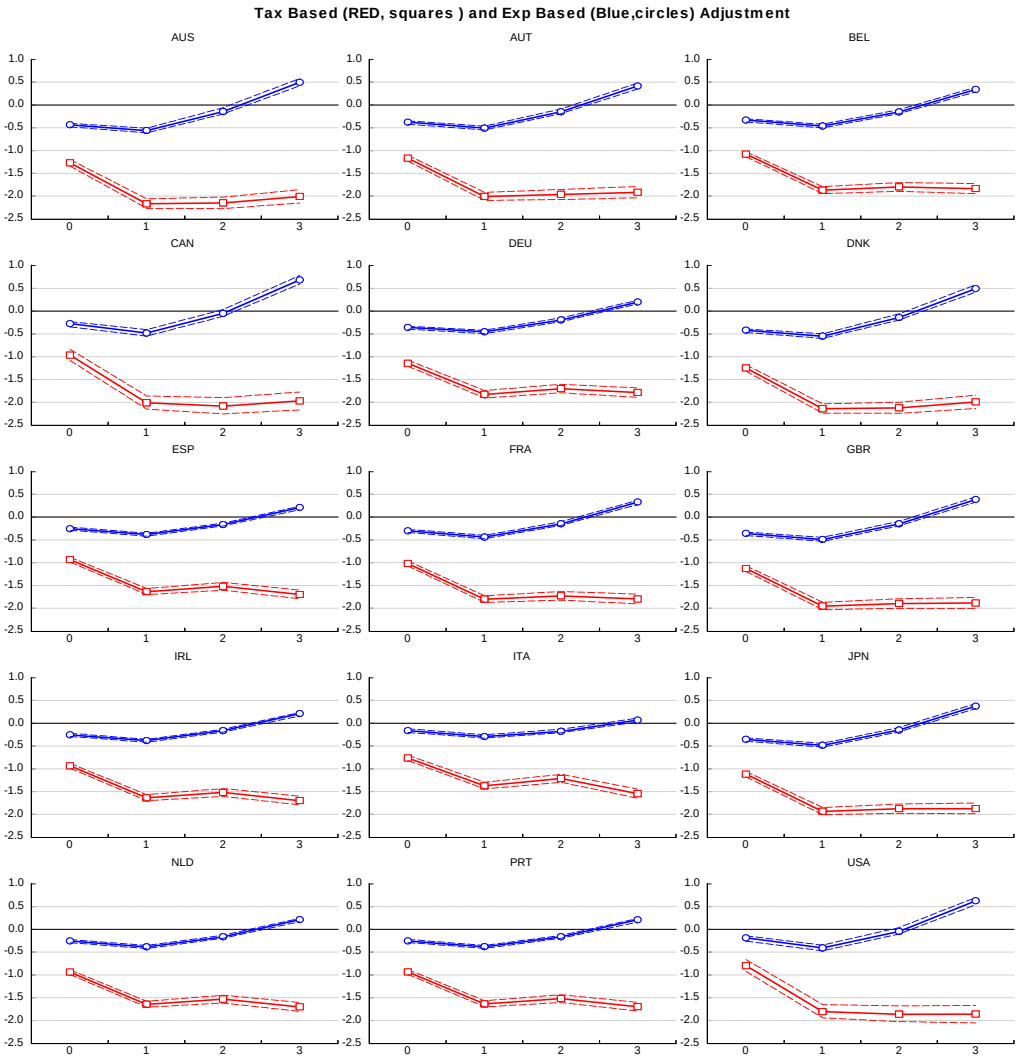

Figure 3: The effect of TB and EB adjustments on output growth

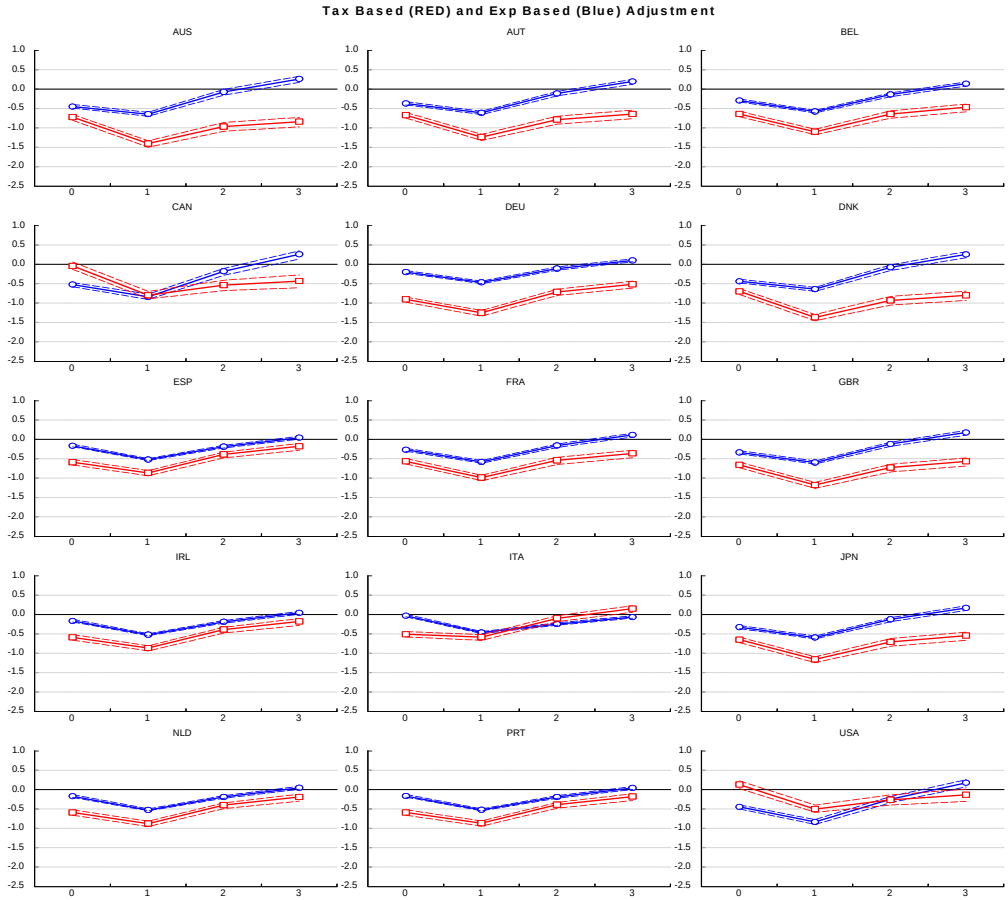

Figure 4: The effect of TB and EB adjustments on Consumption Growth

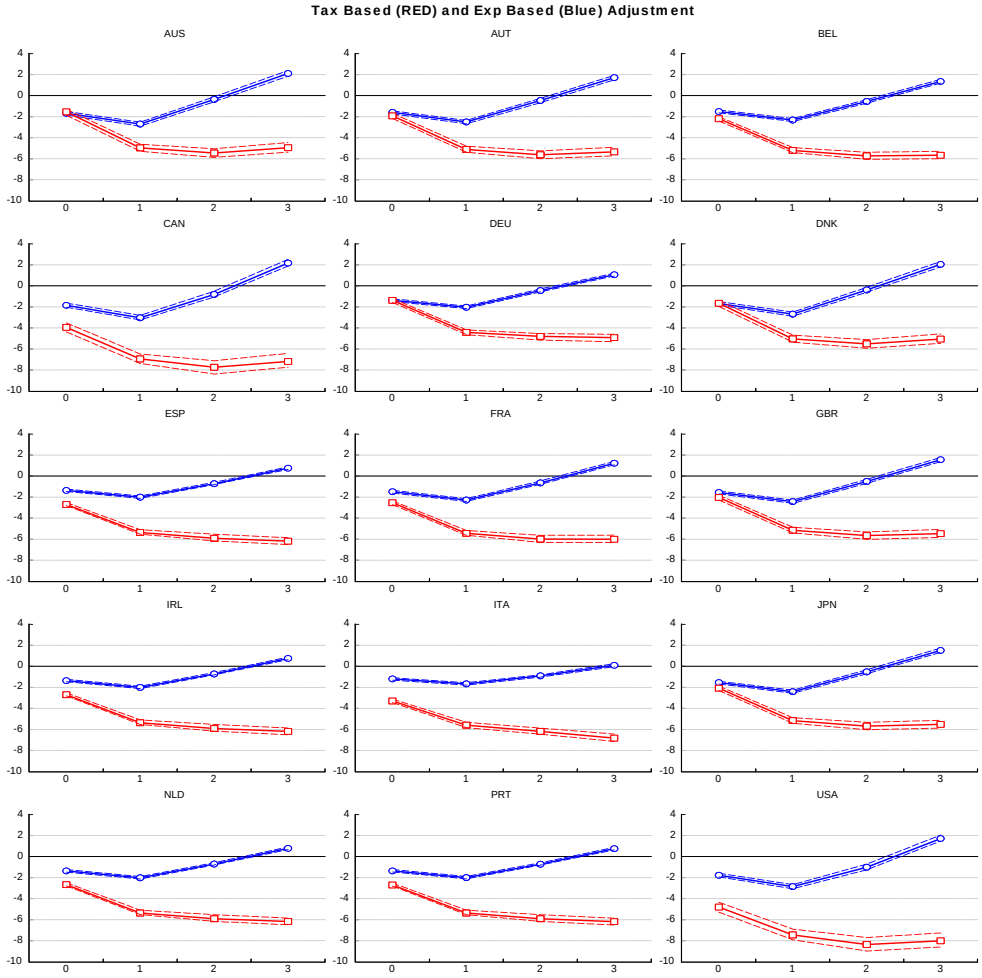

Figure 5: The effect of TB and EB adjustments on fixed capital formation growth

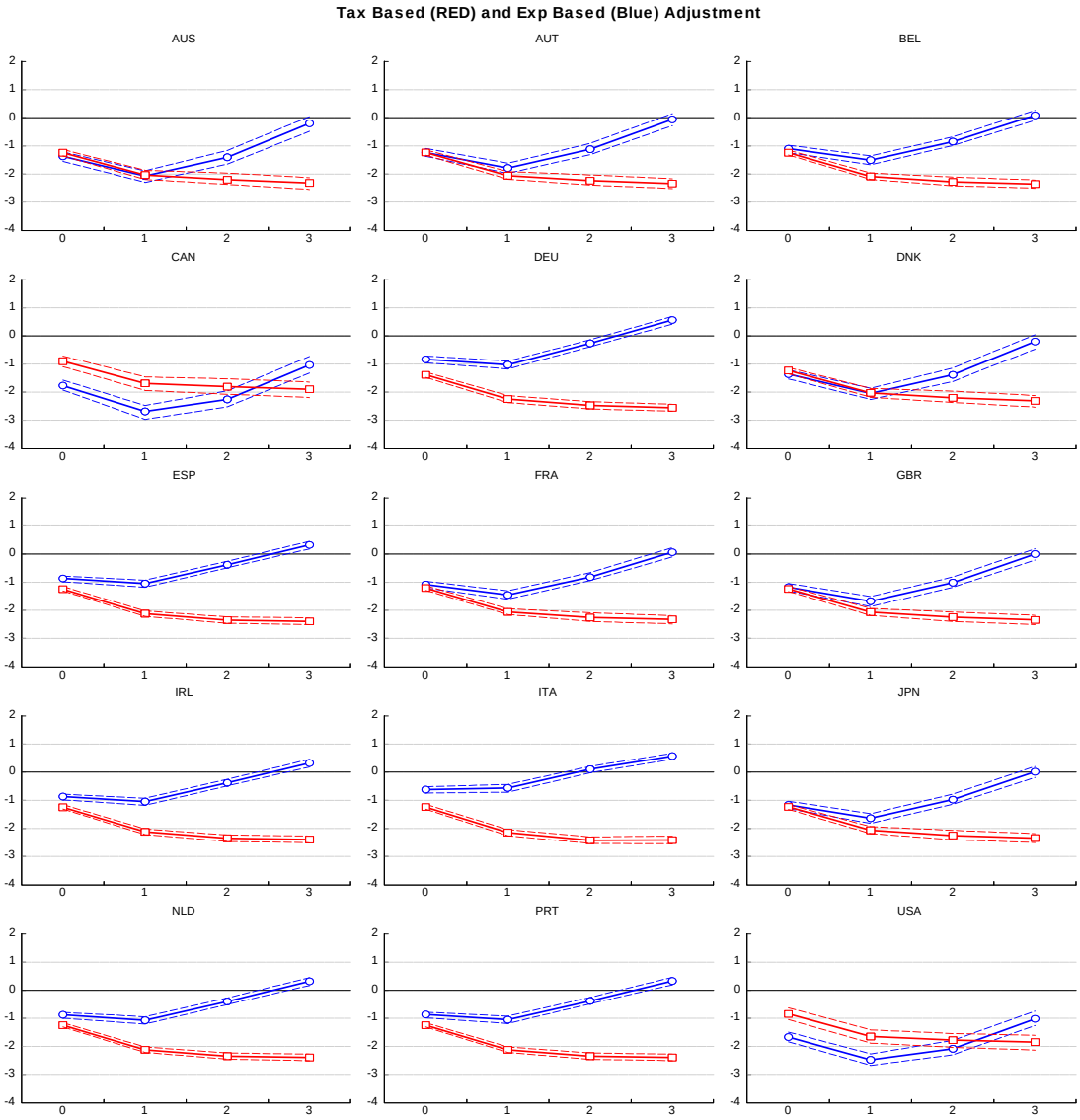

Figure 6: The effect of TB and EB adjustments on ESI Consumer Confidence

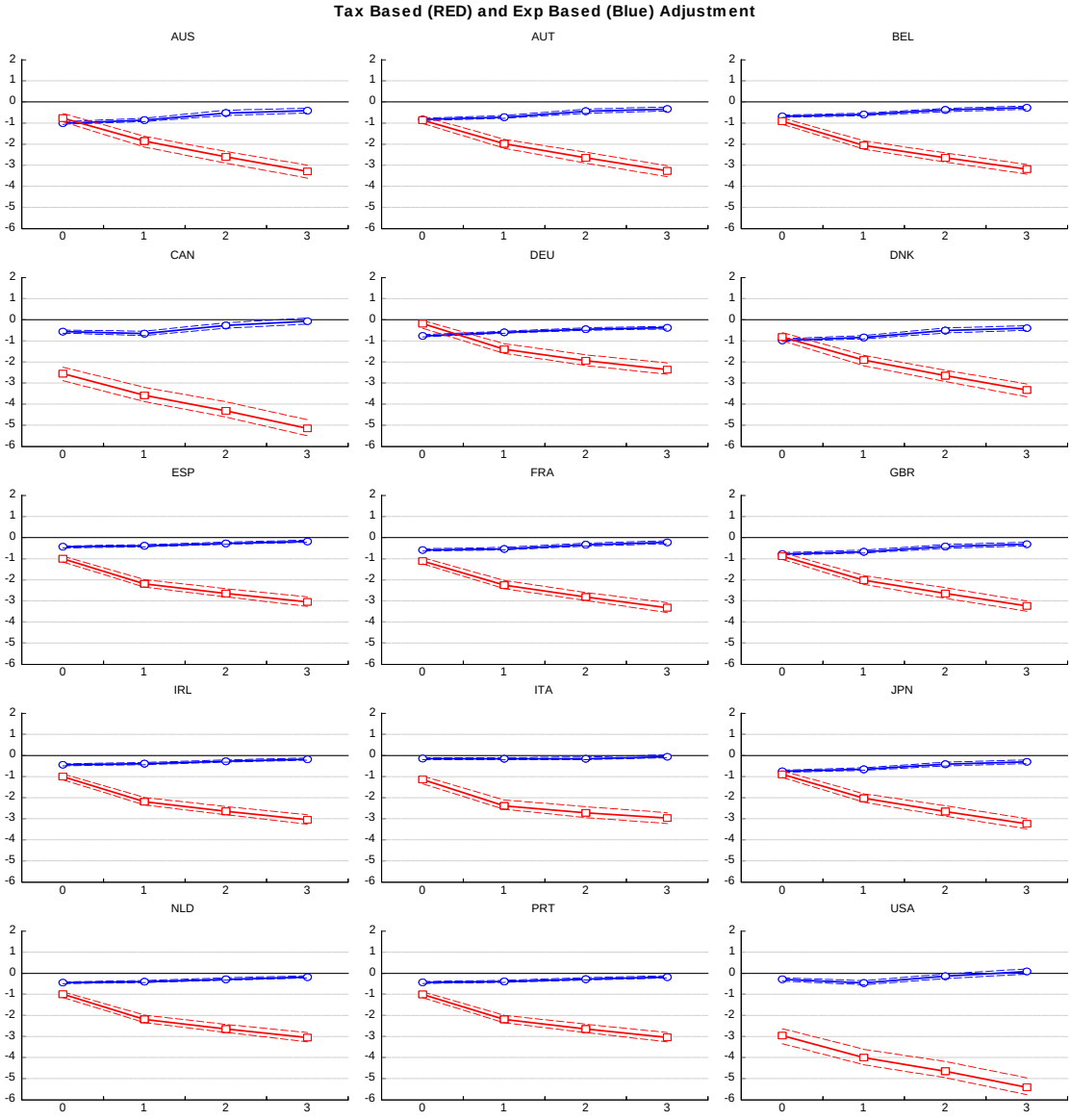

Figure 7: The effect of EB and TB adjustments on ESI Business Confidence

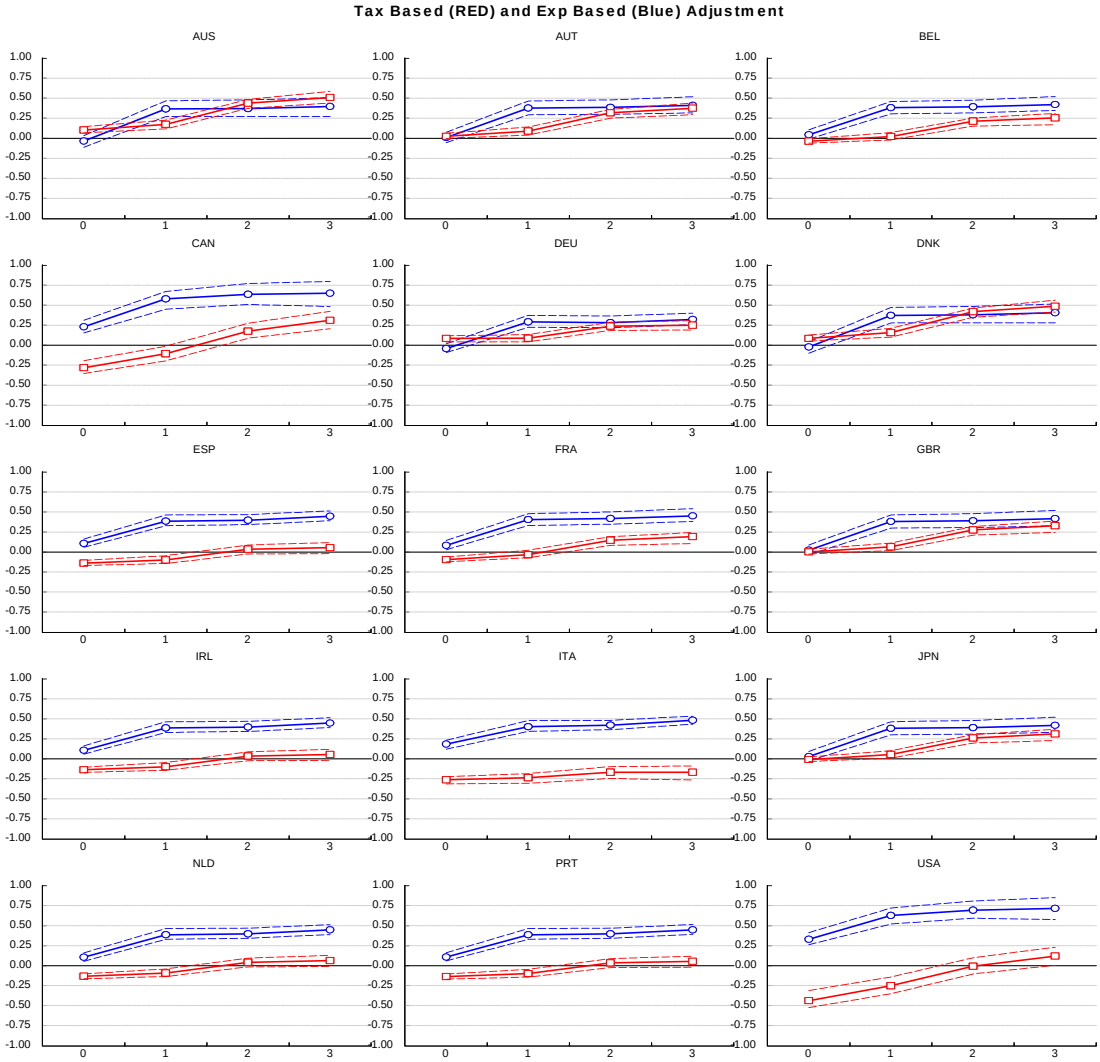

Figure 8: The effect of TB and EB adjustments on the term spread

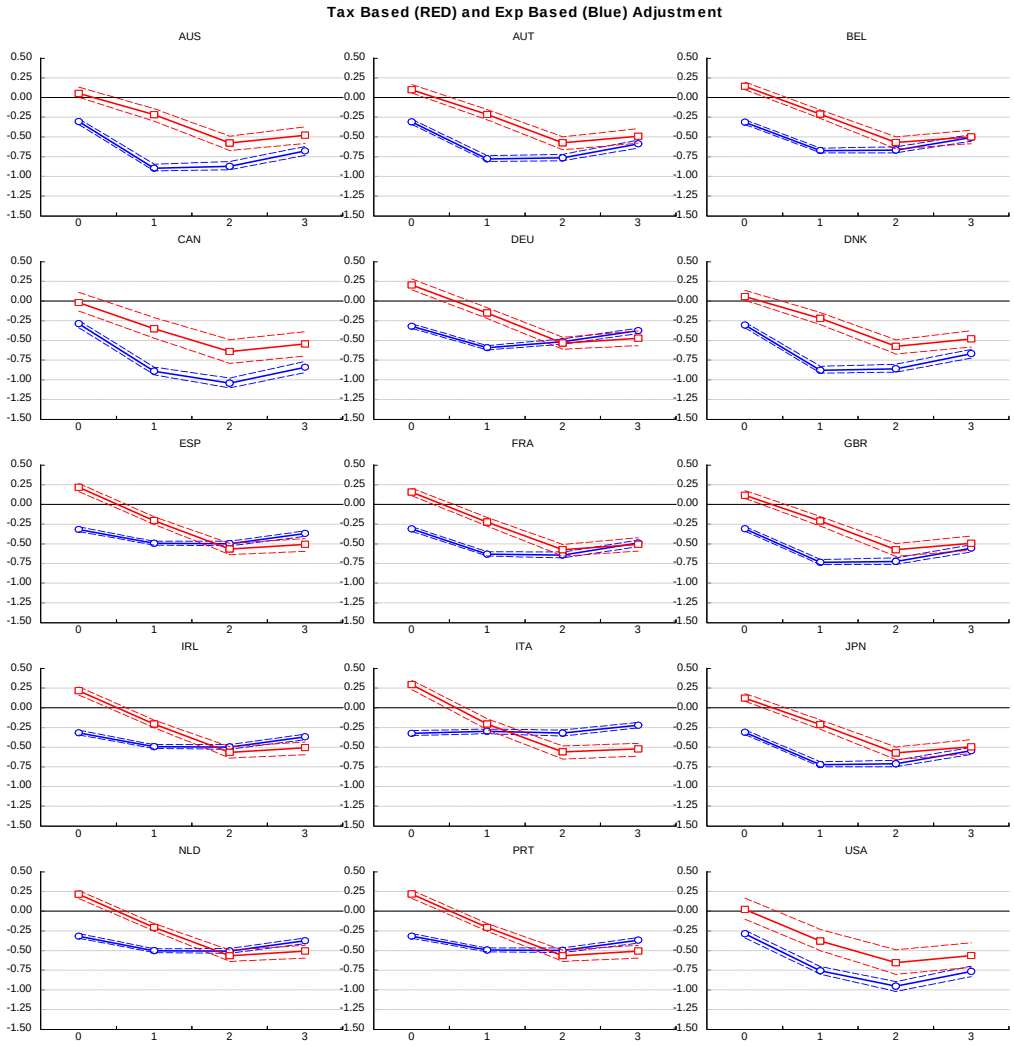

Figure 9: The effect of TB and EB adjustments on monetary policy (change in the 3M TBills Rates).

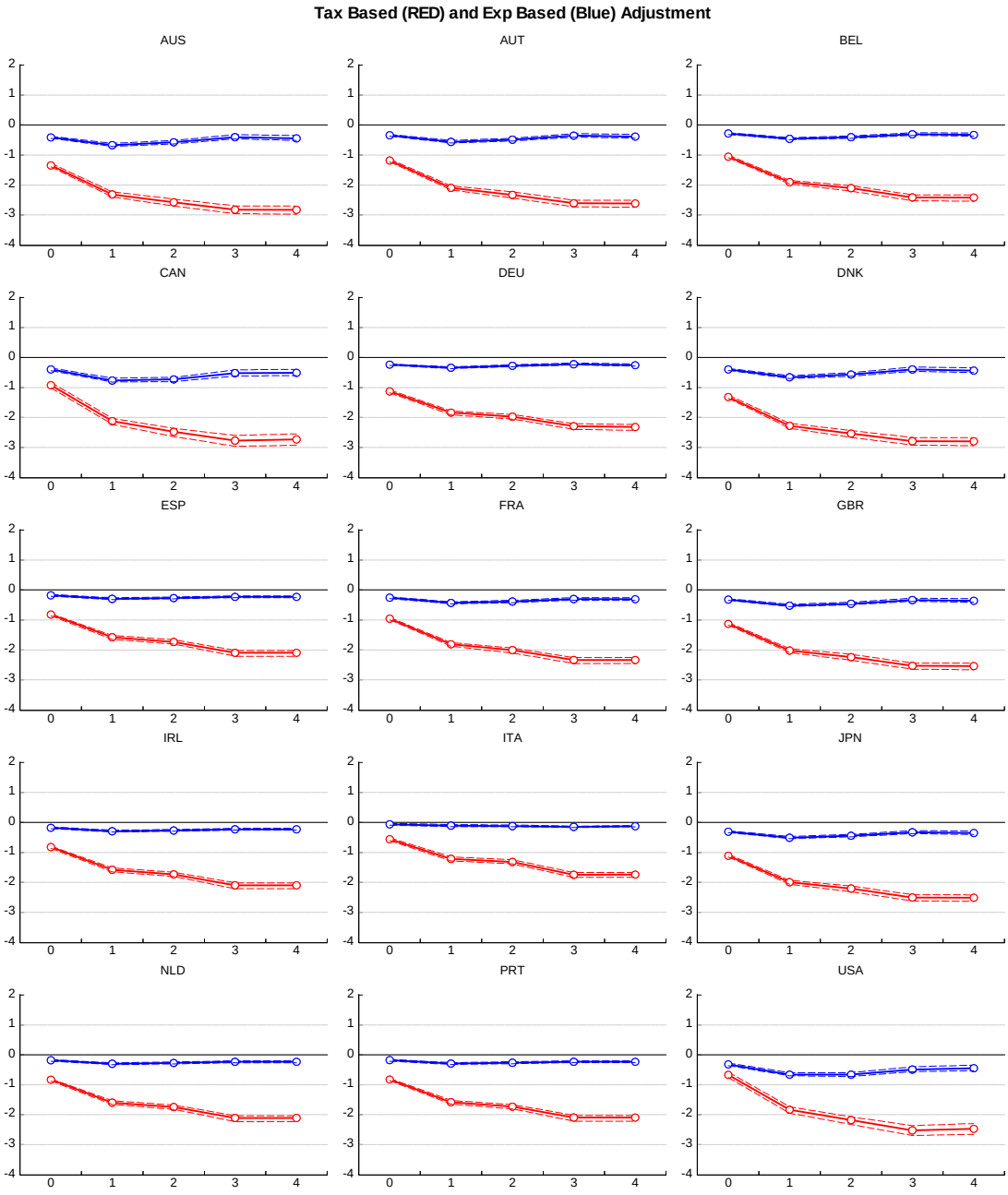

Figure 10: The effect of TB and EB adjustment on output growth in the model with time dummies

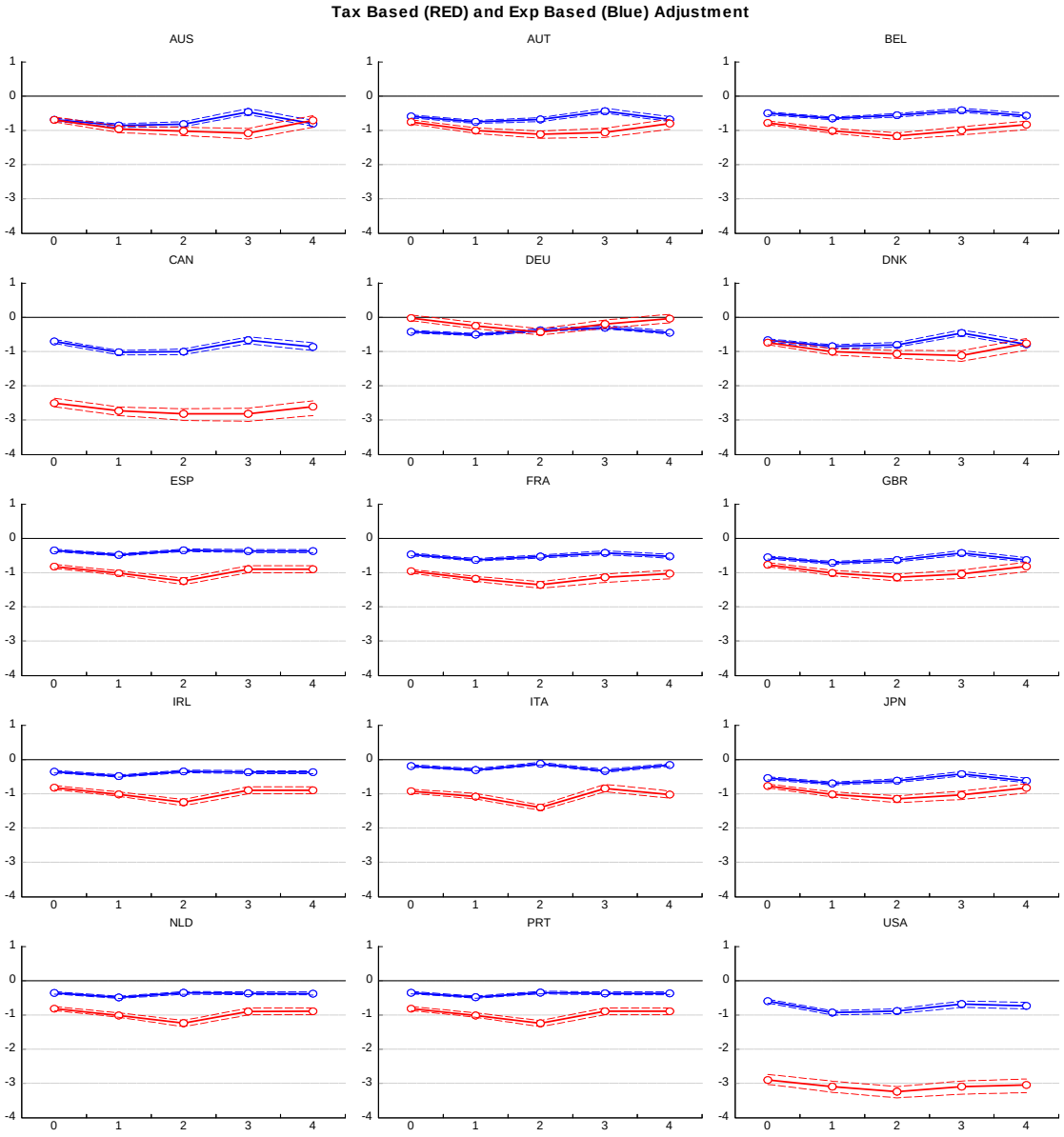

Figure 11: The effect of TB and EB adjustment on consumption growth in the model with time dummies

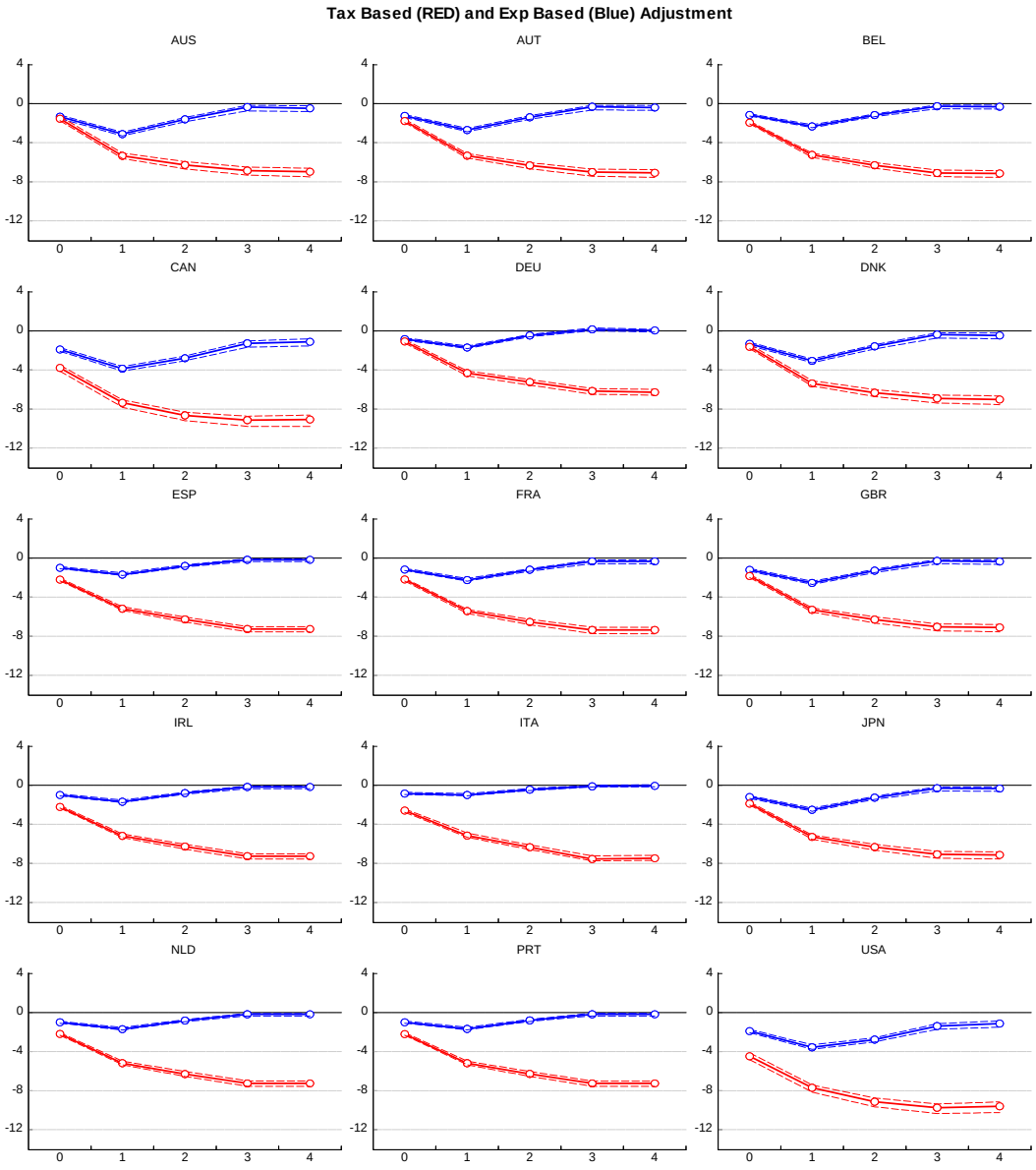

Figure 12: The effect of TB and EB adjustment on fixed capital formation growth in the model with time dummies

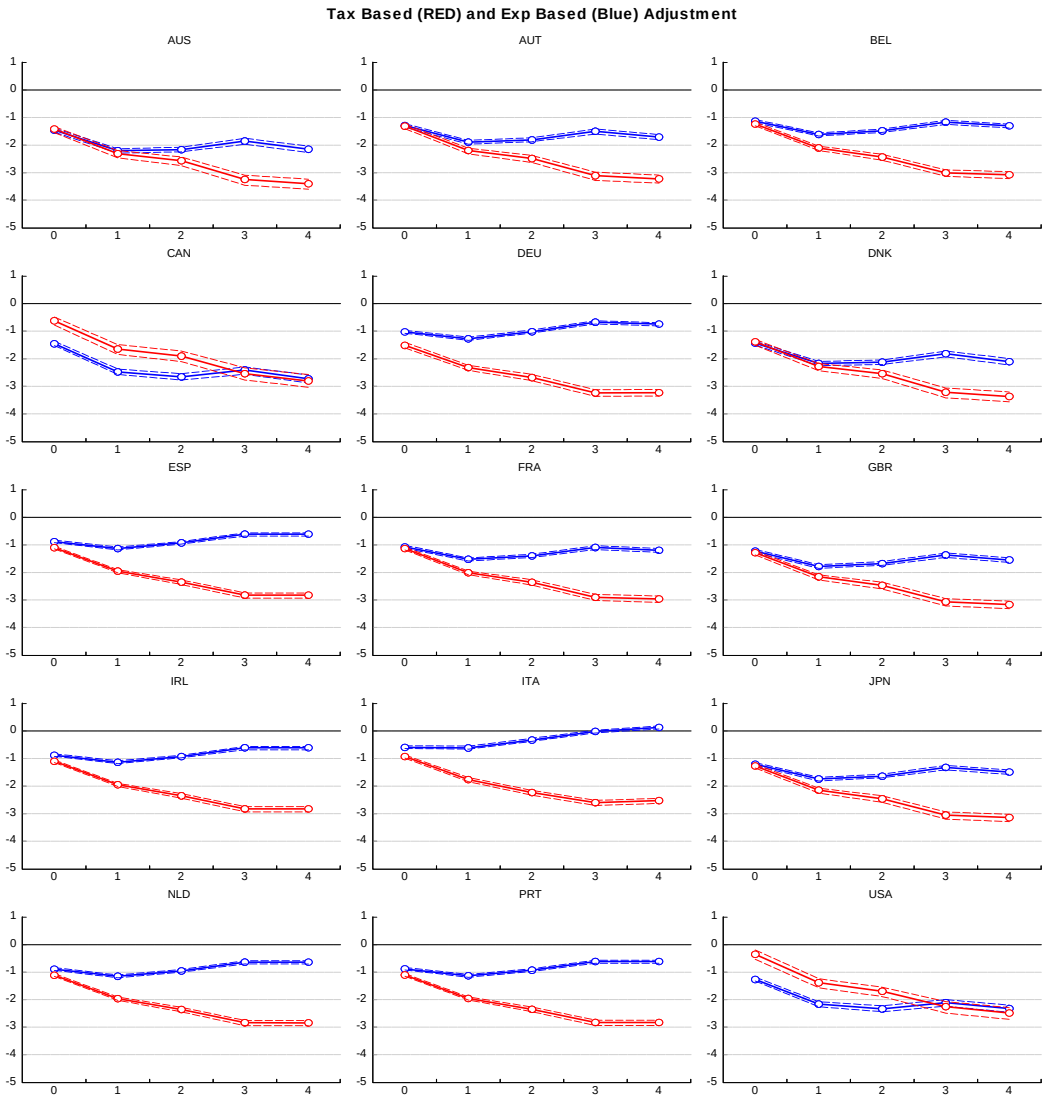

Figure 13: The effect of TB and EB adjustment on ESI Consumer Confidence in the model with time dummies

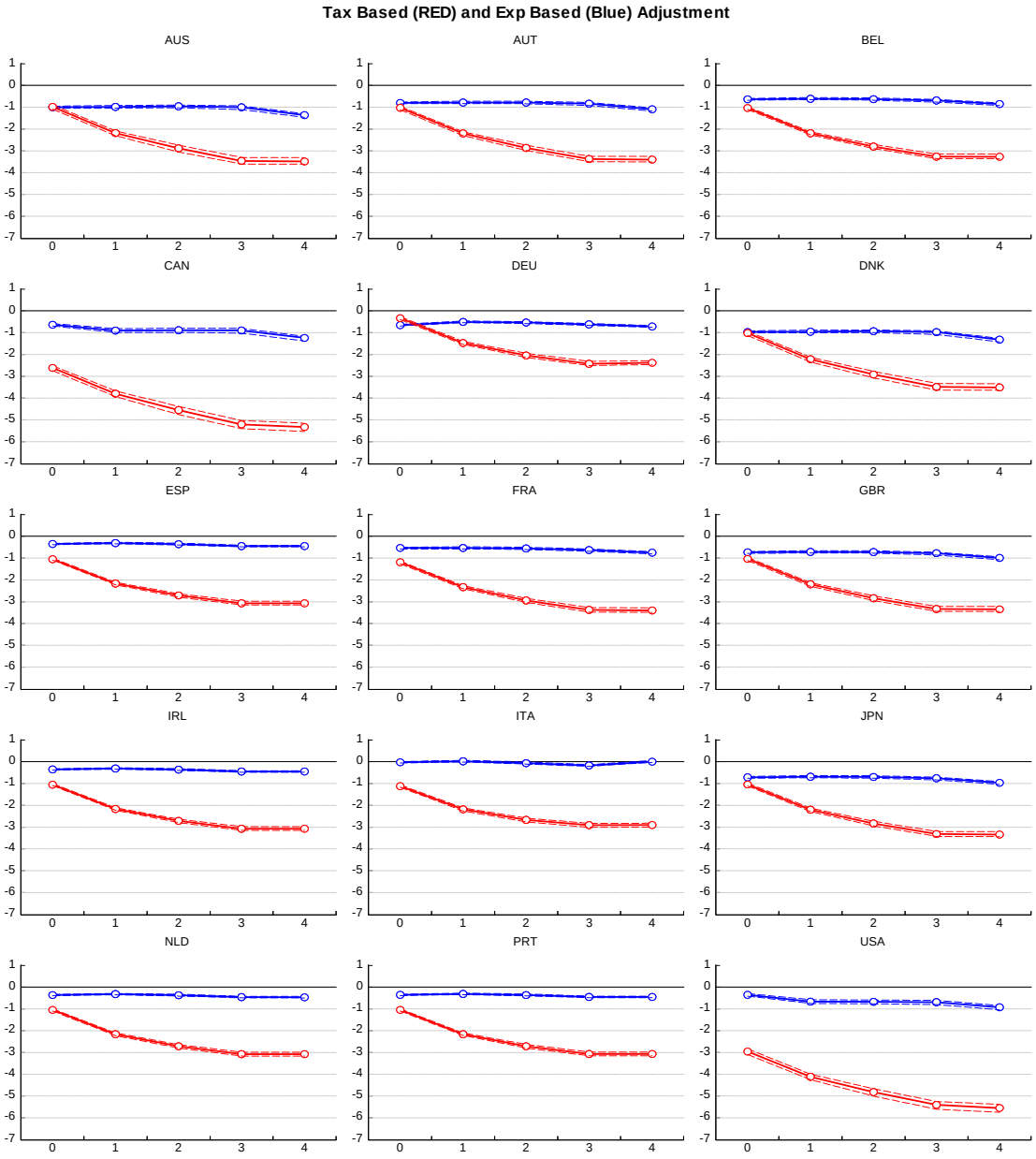

Figure 14: The effect of TB and EB adjustment on ESI Business Confidence in the model with time dummies

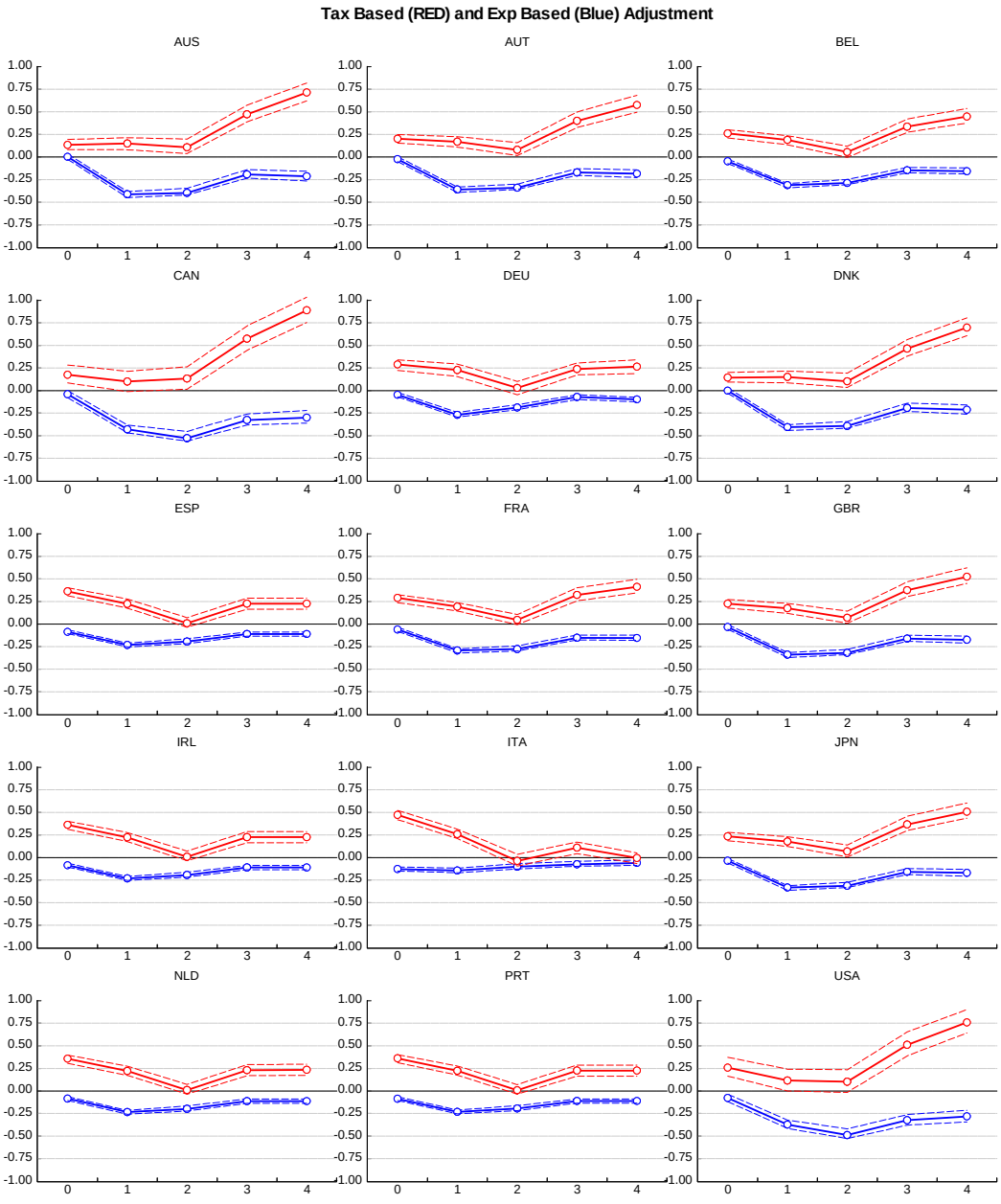

Figure 15: The effect of TB and EB adjustments on monetary policy (change in the 3M TBills Rates) in the model with time dummies

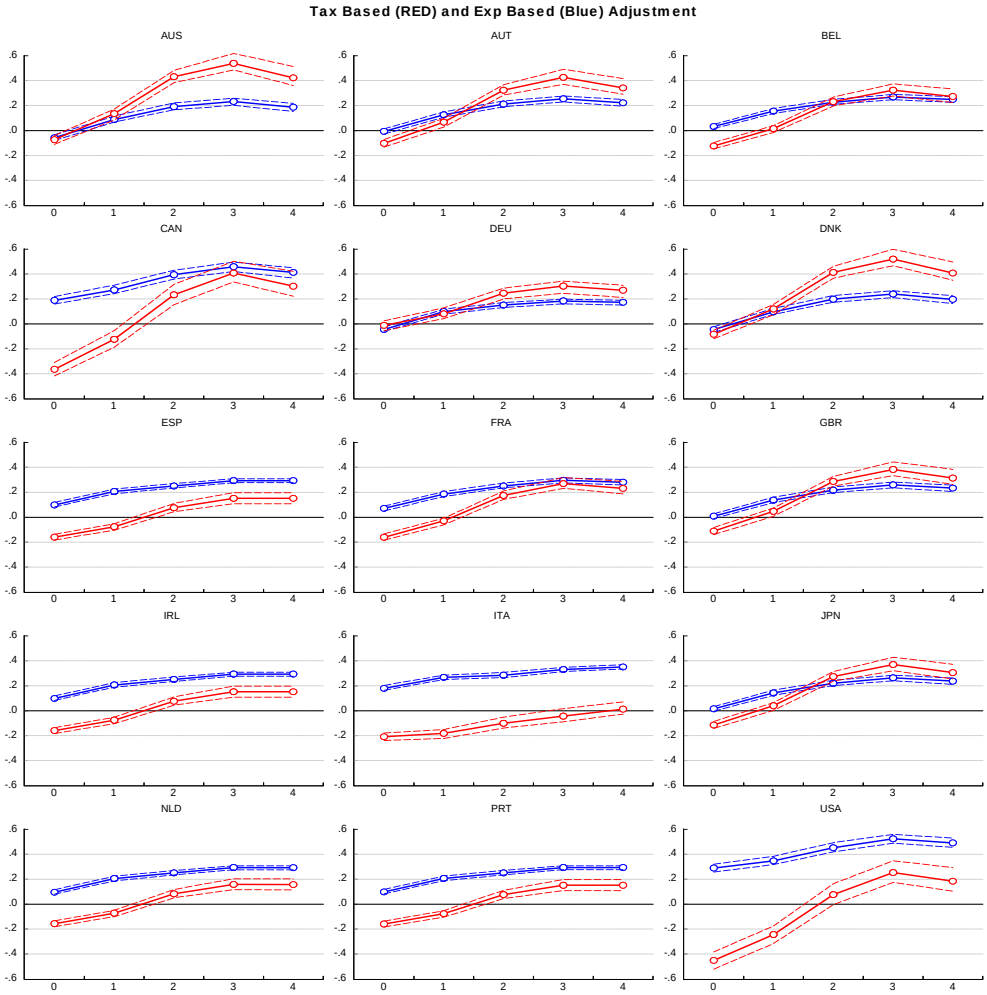

Figure 16: The effect of TB and EB adjustment on term spread in the model with time dummies