CHAPTER TWELVE

The Models in Our Book: A User's Guide

INTRODUCTION

Measuring multipliers requires two steps: (1) identify exogenous shifts in fiscal variables; and (2) analyze their effects on the economy, using an empirical model that allows tracking of the dynamic response of the economy to such exogenous shifts. For this second step, one needs to estimate the parameters of the model and then use the model to generate two alternative paths for the macroeconomic and policy variables, in the presence or absence of the shift in fiscal variables. The difference between these two paths is the "impulse response" that describes the dynamic reaction of the economy to the policy correction (the impulse).[1]

In specifying models for policy simulation there is an important trade-off: the simpler the model the easier it is to calculate the multipliers but the simpler the model the more likely it is that important relations among variables are missed. So the simpler the model the more likely that the model is wrong. To calculate fiscal multipliers a choice is required and a researcher must decide where to locate in the trade-off between simplicity and reliability. Computing a multiplier by running one regression and reading the coefficients it is almost surely a recipe for disaster: the economy is too complex to be described by one regression. Conversely, modeling the economy in all details almost certainly entails that there will be too many parameters to be estimated. A choice needs to be made and such a choice is surely a risky one.

In this chapter we shall illustrate all the models that we have used in this book and that we make available for replication. We shall start with an illustrative example and a general discussion to then review one by one the models used chapter by chapter.

WHY SIMULATING MODELS? A SIMPLE EXAMPLE

Consider the simplest possible specification of a model, like the one described in Chapter 5

$$\begin{aligned}
\Delta y_t &= \beta_0 + \beta_1 e_t^u + \beta_2 e_{t-1}^u + \gamma_1 e_{t,t-1}^a + \delta_1 e_{t,t+1}^a + u_t \\
e_{t,t+1}^a &= \varphi_1 e_t^u + v_t \\
e_{t+1,t}^a &= e_{t,t+1}^a
\end{aligned}$$

where Δy_t is output growth and the variables on the right-hand side are the unanticipated, announced, and implemented portions of a fiscal correction. To keep matters simple we limit the horizon of plans to one period, do not distinguish between tax-based (TB) and expenditure-based (EB) corrections, and limit the dynamic effects of plans also to one period. The parameters β_1 and β_2, describe the dynamic response of output growth to the unanticipated component of the plan; γ_1 describes the response to the implementation in year t of measures announced in year $t-1$; δ_1 the response to measures announced in year time t, to be implemented the following year, $t+1$; and finally β_0 measures the average rate of growth of the economy in absence of fiscal plans. The next two equations describe fiscal plans: the first measures the correlation between the announced and the unanticipated components of a plan; the second simply says that a measure announced, $e_{t+1,t}^a$, is subsequently implemented, showing up as $e_{t,t+1}^a$. Now assume that the data deliver the following estimated parameters:

$$\begin{aligned}
\Delta y_t &= 0.02 - 0.8 e_t^u - 0.6 e_{t-1}^u - 0.2 e_{t,t-1}^a - 0.3 e_{t,t+1}^a + \hat{u}_t \\
e_{t,t+1}^a &= 0.5 e_t^u + \hat{v}_t \\
e_{t+1,t}^a &= e_{t,t+1}^a
\end{aligned}$$

Note that the effects of a plan cannot be inferred simply by reading the coefficients of the first equation—because one needs to also take into account the correlation between announcements and unexpected shifts in fiscal variables. In other words, one needs to jointly simulate all three equations in the model and then compute impulse responses. Table 12.1 reports the result of this exercise

TABLE 12.1. A stylized example

	Impulse			Baseline	Alternative	Impulse response		Multiplier
	e_t^u	$e_{t,t-1}^a$	$e_{t,t+1}^a$	Δy_t	Δy_t	Δy_t	y_t	
t	$0.01\frac{2}{3}$	0	$0.01\frac{1}{3}$	0.02	0.013667	−0.00633	−0.00633	−0.633
$t+1$	0	$0.01\frac{2}{3}$	0	0.02	0.015333	−0.004667	−0.011	−1.1
$t+2$	0	0	0	0.02	0.02	0	−0.011	−1.1
$t+3$	0	0	0	0.02	0.02	0	−0.011	−1.1
$t+4$	0	0	0	0.02	0.02	0	−0.011	−1.1
$t+5$	0	0	0	0.02	0.02	0	−0.011	−1.1

The simulation in the table describes the effects of a fiscal correction worth 1% of GDP. Such a correction is implemented with a two-thirds share attributed to the unanticipated component and a one-third share announced at time t to be implemented in the following period. The output multiplier of the plan is −0.6333 in the first period and −1.1 from the second period onwards. These multipliers depend on the coefficients estimated in the first equation but also on the estimate of φ_1 (0.5 in the example). For example, the −0.633 delivered by the simulation in the first period is the sum of the impact effect of the unanticipated component $(-0.8 \cdot 0.01 \cdot \frac{2}{3})$ plus the effect of the announcement $(-0.03 \cdot 0.5 \cdot 0.01 \cdot \frac{2}{3})$.

OUR EMPIRICAL MODELS: AN OVERVIEW

More generally than in the simple case considered so far a model describes the behavior of a set of macro variables, $\mathbf{Y}_t$, as a function of their past values,$\mathbf{Y}_{t-1}$, the past values of a few policy variables $\mathbf{P}_{t-1}$ (in our case the fiscal policy variables) and macroeconomic shocks. Similarly, the dynamics of the policy variables can be decomposed into a "rule"—which describes the response of current policy to past policy and past macroeconomic conditions—and deviations from the rule, our fiscal plans:

$$\mathbf{Y}_t = f_1(\mathbf{Y}_{t-1}, \mathbf{P}_{t-1}, \Theta_1) + f_2(\mathbf{plan}_t, \Theta_2) + \mathbf{u}_{1t} \qquad (12.1)$$

$$\mathbf{P}_t = f_3(\mathbf{Y}_{t-1}, \mathbf{P}_{t-1}, \Theta_3) + f_4(\mathbf{plan}_t, \Theta_4) + \mathbf{u}_{2t} \qquad (12.2)$$

$$\mathbf{plan}_t = g\left(e^u_{i,t}, e^a_{i,t-1,t}, e^a_{i,t,t+1}, \Phi\right) + \mathbf{u}_{3t} \tag{12.3}$$

Once the variables to be included in $\mathbf{Y}_t$ and $\mathbf{P}_t$ are chosen (a choice, as we already mentioned in Chapter 5, that is limited by the scarcity of data), to use the model to run a simulation we need to decide on a functional form for the functions f_1, f_2, f_3, f_4 and to estimate the parameters $\Theta_1, \Theta_2, \Theta_3, \Theta_4$. Once the model is specified and estimated, the impact of fiscal plans on macroeconomic variables can be computed by constructing an impulse response (*IR*) computing the difference between two forecasts:

$$IR\,(t, s, d_i) = E\left(\mathbf{Y}_{i,t+s} \,\middle|\, \text{plans}_t;\; I_t\right) - E\left(\mathbf{Y}_{i,t+s} \,\middle|\, \text{no plans}_t;\; I_t\right)$$
$$s = 0, 1, 2, \ldots$$

Finally, multipliers can be calculated, as argued by Mountford and Uhlig (2009), Uhlig (2010), and Fisher and Peters (2010), as the integral of the output response divided by the integral of the change in fiscal variables.

THE MODELS USED IN THE LITERATURE TO MEASURE MULTIPLIERS

Beyond the narrative approach discussed in Chapter 4, many other techniques have been developed to deal with the identification, estimation, and simulation of the effects of a shift in taxes or spending.

The VAR Approach

Vector autoregressions (VAR) were one of the first techniques used to identify exogenous shifts in fiscal variables and to simulate their impact on the economy. Blanchard and Perotti (2002) were the first to adopt this empirical strategy.

VARs are systems of equations designed to analyze the linear interdependencies among multiple variables. In other words, rather than a single dynamic equation, VARs include a system of many dynamic equations. Moreover, (structural) VARs can be used to address the reverse causation problem. Structural VARs solve the problem by estimating a dynamic model that projects (linearly) both nonpolicy and policy variables on their past history: innovations in the equations for the policy variables thus represent deviations of these variables from their expected values, conditional on past information. These innovations contain two terms: the contemporaneous response of fiscal policy to the cycle and discretionary policy actions not related to the cycle. These discretionary policy actions are the "exogenous" policy shifts that researchers are interested in. Blanchard and Perotti (2002) recover such discretionary policy actions in two steps: (1) filtering out "the automatic stabilization component" from the VAR innovation, relying on institutional information about the automatic response of taxes, transfers, and spending to the state of the economy (although Caldara and Kamps (2017) show that estimated multipliers are quite sensitive to changes in the identification procedure); and (2) assuming that it takes at least one quarter for fiscal authorities to respond to the state of the economy, so that the current state of the cycle cannot influence the discretionary deviation of policy from the rule, a debatable assumption. Many variations on this theme have been implemented within this general framework.[2]

Once exogenous shifts in taxes or spending are recovered, their impact on macroeconomic variables can be constructed by comparing two different simulations of the VAR model: a baseline simulation and an alternative one. In the baseline simulation, it is assumed that the fiscal authority sticks to its rule; the alternative simulation instead introduces a discretionary deviation from the rule. Simulations of the model under the two different scenarios produce two paths for macroeconomic variables: their difference is the impulse response function that describes the response (over time) of the economy to an exogenous impulse given to policy variables. This approach naturally leads to computing multipliers as the ratio of the discounted sum of the output response to a shift in G or T to the total change in G or T (also discounted) because VAR impulse responses track the entire path of fiscal variables following an initial shift.

There are two main weaknesses in the use of this strategy to identify exogenous shifts in policy variables. First, the exogenous policy shifts

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depend on the particular specification of the model. For instance, if a relevant variable is omitted, innovations could be contaminated by this misspecification. The second is the validity of the identification assumption that allows the researcher to extract exogenous policy shifts from innovations in policy variables. For instance, Blanchard and Perotti (2002) assume that it takes at least one quarter for fiscal authorities to respond to the state of the economy.

Blanchard and Perotti (2002) estimate a three-variable VAR containing the log of tax revenue, of government spending, and of GDP (all in real per capita terms). This specification is very restrictive: only three variables are considered and the shifts in fiscal variables identified are combinations of unanticipated and announced fiscal corrections, which means that the estimates are obtained under the assumption that the responses to anticipated and unanticipated shifts in fiscal variables are identical. (The assumption that unanticipated and announced policy shifts have identical effects is inevitable if one chooses to identify fiscal innovations within a VAR, since this approach does not allow for the identification of policy announcements, the reason being that the Moving Average representation of a VAR cannot be inverted in the presence of future announced policy shifts). Distinguishing between anticipated and unanticipated shifts in fiscal variables, however, and allowing them to have different effects on output, is crucial for evaluating fiscal multipliers, as argued by Ramey (2011a, b) and confirmed by Mertens and Ravn (2013), who find that they do have different effects on output. In Blanchard and Perotti (2002), taxes are net of transfer payments to individuals and of interest paid by the government, and spending is defined as purchases of goods and services, adding up current and capital spending (transfers are not included in the analysis). Data (for the United States) are quarterly for the period 1947:1 to 1997:4. Multipliers are calculated comparing the peak of the output response to an initial shift in government spending or in taxes. Tax multipliers are close to -1 (between -1.3 and -0.8 depending on whether the variables are defined in first differences or levels) and similar in absolute value to spending multipliers (between 0.9 and 1.3). These results have been confirmed by Fatas and Mihov (2001); Perotti (2005); Galí, López-Salido, and Vallés (2007); and Pappa (2009).

Mountford and Uhlig (2009) estimate a much richer VAR which includes, beyond the two fiscal variables analyzed by Blanchard and Perotti (2002), many more: consumption, real wages, private nonresidential investment, interest rates, materials’ prices and the GDP deflator. Data (for the United States) are at a quarterly frequency from 1955 to 2000. Exogenous shifts in government revenue and expenditure are still identified within the VAR model, but also applying the methodology originally introduced by Uhlig (2005) to identify monetary policy shocks, that is, imposing sign restrictions on the effects of VAR innovations. The tax multiplier—defined as the ratio of the response of GDP at a given horizon (one or more quarters after the policy shift) to the initial movement of the fiscal variable—is almost three times larger than that computed by Blanchard and Perotti (2002): 3.57 (with a peak effect after 13 quarters). The deficit-financed spending multiplier is slightly lower than that estimated in Blanchard and Perotti (2002): 0.65 (with a peak effect upon impact). Linearly combining the two, the authors can analyze the effect of a balanced budget tax cut. Comparing these three experiments, they find that a surprise deficit-financed tax cut is the most effective at stimulating the economy, producing the largest present value multiplier (which, instead of measuring the effect of the shift in fiscal variables on impact, considers the cumulated response along the entire path of the response): five dollars of additional GDP for each dollar of cut in government revenue, 5 years after the shock.

Expectational VARs and Ramey’s News Variable

Recently some researchers have tried to overcome the problems that arise when exogenous shifts in fiscal variables are identified inside a VAR, in particular the fact that it is impossible to separate announcements from unexpected shifts in policy. They have done so using exogenous shifts identified outside the VAR model, for example, with narrative methods. The approach has been labeled “Expectational VARs.”

The outside variable used by Ramey and Shapiro (1998) is a dummy describing military buildups. They identify political events that led to unanticipated military buildups exogenous to the current state of the economy. These are called “war dates.” The macroeconomic impact of these "war dates" is then measured by estimating an equation for output growth that includes current and lagged values of war dates (as well as lags of the left-hand-side variable). This single equation approach is a valid approximation to a VAR "full information approach," that is an approach that includes many more variables than just output growth, under the assumption that the measurement error in "war dates" and innovations in the variables that are excluded from the model, for instance interest rates, are orthogonal. A number of follow-up papers (e.g., Edelberg et al. [1999], Burnside et al. [2004], and Cavallo [2005]) have embedded "war dates" in a VAR.[3] These experiments typically found government spending multipliers in the range 0.6–1.5, slightly higher than that found by Blanchard and Perotti (2002).[4]

Barro and Redlick (2011) introduced a second fiscal variable, marginal tax rates, and also allow for multipliers to differ depending on the level of unemployment. Their results cover both periods in which defense spending decreased (e.g., 1946–7 and 1954–5) and periods during which it increased (e.g., World War I, World War II, the Korean War). The estimated multiplier for defense spending (holding average marginal income tax rates fixed) is around 0.7. These estimates are derived under the assumption that the increase in expenditure is deficit financed. The results are obviously different in the case of tax-financed increases in spending. Since an increase in average marginal income tax rates has a significantly negative effect on GDP with an implied multiplier of 1.1, the balanced budget multiplier becomes negative.

Multipliers and the Government Intertemporal Budget Constraint

The response of economic agents to current shifts in fiscal policy depends on their expectations about how future fiscal policy will adjust to such shifts, an observation first made by Bohn (1991). This raises the issue of debt sustainability following a shift in fiscal policy—an issue typically overlooked in the articles discussed in the two previous sections. Chung, Davig, and Leeper (2007) impose debt sustainability on a VAR;

that is, they require that the real value of debt in the hands of the public must always be equal to the expected present value of surpluses. For any given shift in fiscal variables, they therefore can ask whether debt is sustainable. Using data for the United States over the period 1947:2 to 2006:2, they find that for some fiscal shifts, changes in the present value of surpluses are sufficient to guarantee debt sustainability. In other cases expected surpluses, instead, fail to adjust enough to guarantee debt sustainability for given discount rates. In particular, they find robust evidence in favor of a stabilizing role for the primary surplus following shifts in taxes, and similarly robust evidence of a stabilizing role for taxes following a shift in government spending. Conversely, the results point against a stabilizing role of changes in spending following either a shift in taxes or in spending. In all cases the horizon over which debt is stabilized is very long, around 50 years. Present values calculated up to any finite horizon fluctuate wildly, particularly following a shift in government spending or in transfers.

Favero and Giavazzi (2012) estimate a VAR model that includes the narrative tax shocks constructed by the Romers. They then keep track of the dynamics of the debt over GDP ratio in response to one of those shocks, appending to the VAR the identity that defines the change over time of the debt over GDP ratio. Estimated on post-World War II United States data (1950:1–2007:1), the model never delivers "unsustainable debt paths" and it produces multipliers that are very similar to those obtained from a VAR that omits the debt dynamics equation. This equivalence may not hold to other countries outside of the United States, therefore accounting for the debt dynamics in this case would be crucial.

In the same vein, using a standard new-Keynesian model, Corsetti, Meier, and Müller (2012b) analyze the effects of an increase in government spending under a plausible debt stabilizing policy: current increases in spending are accompanied by a subsequent period of spending reversals. They show that accounting for them is of crucial importance for the model to match the stylized facts of fiscal transmission. Their results suggest that for an increase in expenditure to be most effective, policymakers should accompany it with a credible commitment to cut expenditure over the medium term.

Fiscal Policy as an Average Treatment Effect

Impulse responses compute the average effect of a policy shift by simulating the dynamic path of output in the presence and absence of the policy shift. This is a procedure reminiscent of that used to analyze "average treatment effects" in randomized experiments, where researchers split the sample into two subgroups: a "treated" and a "control" group. After the population is randomly assigned to the two groups, a treatment is administered to one group and no treatment (a "placebo" in medical experiments) to the other. The effect of the treatment is then measured by analyzing the differences in outcomes for the two groups. Starting from this intuition, Jordà and Taylor (2016) have studied fiscal multipliers using the logic of treatments. To estimate average treatment effects, they use the set of exogenous shifts in taxes and spending constructed by Devries et al. (2011) and proceed as follows: (1) redefine the fiscal innovations as a 0/1 dummy; (2) estimate a *propensity score* computing the probability with which a correction is expected by regressing it on its own past and other predictors (this step is necessary because the Devries et al., 2011 corrections have been shown by De Cos and Moral-Benito, 2016 to be predictable) (3) use the propensity score to compute an Average Treatment Effect, that is, the average difference between output growth in the presence of a fiscal correction (weighted for their predictability, so that more unpredictable corrections carry more weight) and in their absence. They find that average treatment effects of a fiscal consolidation are not very different from those estimated (using the same data but with a different estimation strategy) by Guajardo et al. (2014). The peak effect 5 years after the consolidation is slightly larger than −1, and the cumulative effect after 5 years is about −3.

However, the transformation of shifts in taxes and spending into a 0/1 dummy is not innocuous because it overlooks the fact that there are two sources of identification of narrative adjustments: the timing of a fiscal correction and its size. Transforming fiscal adjustments into a 0/1 dummy neglects size as a source of identification, which is a major limitation of this methodology. Although this transformation is irrelevant in medical experiments, in which patients in the treated group are all administered the same dose of a medicine, it is not irrelevant in economics. This is a problem that VAR-based impulse responses do not have.

Local Multipliers

Nakamura and Steinsson (2014) and Giavazzi and McMahon (2013) studied the effect on state output and state private consumption of procurement contracts signed by the US Department of Defense with companies located in various US states. The identifying assumption is that states differ in the number of procurement contracts they are allocated from the Pentagon in ways that do not depend on their economic conditions, a reasonable but debatable assumption. Funding for such contracts comes from the federal budget and the cost thus falls on federal taxation, an effect that is captured (with many others, including changes in monetary policy, exchange rates, federal regulations) by the time fixed effect included in the estimated regression. The estimated multiplier therefore misses one element that determines consumers' responses to an increase in local defense spending: the anticipation of the taxes that they will need to pay to cover such an expenditure—except for the portion paid through federal taxes, which depends on the size of their state. In other words, the response of consumers takes into account only the fraction of the local defense expenditure that will be paid for by local consumers, but this is only a small part of the financing. For example, think of a large Pentagon project in Rhode Island, a very small state. Rhode Island residents will pay, through their taxes, for just a small fraction of the project. In this literature, the gap between estimated local multipliers and the total multiplier is typically filled by calibrating a model.

Similarly, Chodorow-Reich, Feiveson, Liscow, and Woolston (2012) and Wilson (2012) examined the impact on employment of expenditures related to the 2009 American Recovery and Reinvestment Act (ARRA), exploiting the fact that the distribution of grants was determined in a way that could not be predicted by economic conditions prevailing before 2008. This allows them to estimate state-level effects of the Act for all categories of expenditure covered by ARRA (with the exclusion of unemployment insurance). Shoag (2013) exploits the differential performances of the pension-fund investments of various

US states to generate exogenous shocks to state government spending. He finds large state-level multipliers, around 1.4. A problem with this estimate, however, is that it could be biased if excess returns in pension funds affect GDP, not only through the increase in expenditure following the revenue windfall, but also through a wealth effect on consumers. (For a review of what we have learned from the work on Local Multipliers see Chodorow-Reich [2017]).

THE MODELS IN CHAPTER 7

Our Baseline Specification

Our baseline specification for the dynamics of $\mathbf{Y}_t$ and $\mathbf{P}_t$ is a Vector Autoregressive Model (VAR), which in our case would be applied to a panel of 16 countries (the reason for using a panel, as mentioned in Chapter 5, is that plans are rare and estimates for a single country are thus impossible). So we have a panel VAR that in its most parsimonious specification would include the growth rate of per capita output ($\Delta y_{i,t}$) as the only $\mathbf{Y}_t$ variable, the change of tax revenues as a fraction of GDP ($\Delta \tau_{i,t}$) and that of primary government spending, also as a fraction of GDP ($\Delta g_{i,t}$) as the two $\mathbf{P}_t$ variables:

$$\mathbf{z}_{i,t} = \begin{bmatrix} \Delta y_{i,t} \\ \Delta g_{i,t} \\ \Delta \tau_{i,t} \end{bmatrix}, \mathbf{e}_{i,t} = \begin{bmatrix} e^u_{i,t} \\ e^a_{i,t-j,t} \\ e^a_{i,t,t+j} \end{bmatrix}, \mathbf{a} = \begin{bmatrix} a_1 \\ a_2 \\ a_3 \end{bmatrix} \text{ similarly for } \mathbf{b}$$

$$\Delta y_{i,t} = A_1(L)\,\mathbf{z}_{i,t-1} + \begin{bmatrix} \mathbf{a}'\mathbf{e}_{i,t} & \mathbf{b}'\mathbf{e}_{i,t} \end{bmatrix} \begin{bmatrix} TB_{i,t} \\ EB_{i,t} \end{bmatrix} + \lambda_{1,i} + \chi_{1,t} + u_{1,i,t}$$

$$\Delta g_{i,t} = A_2(L)\,\mathbf{z}_{i,t-1} + \begin{bmatrix} \beta_{11} & \beta_{12} & \beta_{13} & \beta_{14} \end{bmatrix} \begin{bmatrix} g^u_{i,t} \\ g^a_{i,t-1,t} \\ \tau^u_{i,t} \\ \tau^a_{i,t-1,t} \end{bmatrix} + \lambda_{2,i} + \chi_{2,t} + u_{2,i,t}$$

$$\Delta\tau_{i,t} = A_3(L)\,\mathbf{z}_{i,t-1} + \begin{bmatrix} \beta_{21} & \beta_{22} & \beta_{23} & \beta_{24} \end{bmatrix} \begin{bmatrix} g^u_{i,t} \\ g^a_{i,t-1,t} \\ \tau^u_{i,t} \\ \tau^a_{i,t-1,t} \end{bmatrix} + \lambda_{3,i} + \chi_{3,t} + u_{3,i,t}$$

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

Differently from the output growth equation, in the two equations for $\Delta g_{i,t}$ and $\Delta\tau_{i,t}$ we explicitly allow for expenditure and revenue corrections to have different coefficients In these equations we include only fiscal shifts implemented in period t, either unexpected or previously announced: future announced corrections do not directly affect the dynamics of revenues and expenditures as their effect is not recorded in national accounts until they are implemented. Each equation includes country, λ_i, and year, χ_t fixed effects. Finally, $u_{j;i;t}$ are unobservable VAR innovations: these are uninteresting for our analysis, as we do not need to extract from them any structural shock.

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

To be able to recover the effect of adjustment plans on the fiscal and macroeconomic variables, the empirical model for $\mathbf{Y}_t$ and $\mathbf{P}_t$ must be accompanied by a set of auxiliary equations describing the response of announcements to contemporaneous corrections and the relative weights of tax and spending measures within a plan. We allow both correlations to be different according to the type of plan, TB versus EB. In other words, we allow for plans to have a different intertemporal and intratemporal structure according to their type.[5] Thus we complete our model for simulation with the following auxiliary regressions:

$$
\begin{aligned}
\tau^u_{i,t} &= \delta_0^{\mathrm{TB}} e^u_{i,t} * \mathrm{TB}_{i,t} + \delta_0^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \epsilon_{0,i,t} \\
g^u_{i,t} &= \vartheta_0^{\mathrm{TB}} e^u_{i,t} * \mathrm{TB}_{i,t} + \vartheta_0^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \upsilon_{0,i,t} \\
\tau^a_{i,t,t+j} &= \delta_j^{\mathrm{TB}} e^u_{i,t} * \mathrm{TB}_{i,t} + \delta_j^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \epsilon_{j,i,t} \quad j = 1, 2 \\
g^a_{i,t,t+j} &= \vartheta_j^{\mathrm{TB}} e^u_{i,t} * \mathrm{TB}_{i,t} + \vartheta_j^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \upsilon_{j,i,t} \quad j = 1, 2
\end{aligned}
\tag{12.4}
$$

where the first two equations describe the average tax (δ) and spending (ϑ) share of EB and TB plans. The next two equations describe the relation between unexpected shifts and those announced for years $t+1$ and $t+2$, differentiating between EB and TB plans. (These auxiliary regressions allow us to construct the $e^a_{i,t,t+j} = \tau^a_{i,t,t+j} + g^a_{i,t,t+j}$ needed to compute impulse responses). The coefficients in the equations describing the dyanmic evolution of the plans are allowed to vary across the type of plan. This is to capture the fact that, as we shall see, TB plans tend to be front-loaded relative to EB plans because cutting expenditures takes longer than raising taxes. Alternatively, they could be allowed to vary across countries, capturing the possibility that different countries implement fiscal adjustments with different styles when it comes to the correlation between their unexpected and announced components. Both assumptions are interesting: unfortunately the data do not allow us to investigate both at the same time. Most of the results we present in Chapter 7 are obtained assuming plan-specific coefficients—a choice also motivated by consistency with the assumption that fiscal multipliers depend on the type of plan (EB vs TB). We report in Table 12.2 the estimated coefficients for our auxiliary model.

But we shall also show some results that allow country-specific coefficients. The complete model for the dynamics of the macroeconomic

TABLE 12.2. Estimated coefficients

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

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

variables, the fiscal variables and the plans is viable to estimation via a methods that takes into account the simultaneous cross-correlations of residuals (for example, Seemingly Unrelated Regressions [SUR]) stochastic simulation and bootstrap can then be applied to derive impulses responses and the uncertainty surrounding them. A more parsimonious specification of the VAR model can be obtained by estimating directly its Moving Average representation. In this case we would have

$$\begin{aligned}
\Delta y_{i,t} = {} & \alpha + B_1(L)e^u_{i,t} * \mathrm{TB}_{i,t} + B_2(L)e^a_{i,t,t-j} * \mathrm{TB}_{i,t} + C_1(L)e^u_{i,t} * \mathrm{EB}_{i,t} \\
& + C_2(L)e^a_{i,t,t-j} * \mathrm{EB}_{i,t} + \sum_{j=1}^{2}\gamma_j e^a_{i,t,j} * \mathrm{EB}_{i,t} + \sum_{j=1}^{2}\delta_j e^a_{i,t,j} * \mathrm{TB}_{i,t} \\
& + \lambda_i + \chi_t + u_{i,t} \\
e^a_{i,t,t+j} = {} & \varphi_j^{\mathrm{TB}} e^u_{i,t} * \mathrm{TB}_{i,t} + \varphi_j^{\mathrm{TB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \nu_{i,t,t+j} \quad j = 1, 2
\end{aligned} \tag{12.5}$$

We report the estimated coefficients from model (12.5) in Table 12.3.

There are many potentially omitted variables in the specification of (12.5). However, (1) the correct *measurement* of the effect of a fiscal adjustment only requires that the components of a plan are not correlated with the innovation in the left-hand side variables—and this is our assumption used to identify exogenous fiscal corrections; and (2) the correct *simulation* of the effects of a plan requires only that the plan is not predictable using past values of the right-hand-side variables. Condition (3), nonpredictability of corrections on the basis of past output growth, is discussed in Alesina, Barbiero, Favero, Giavazzi, and Paradisi (2017) The paper—using the procedure developed by Toda and Yamamoto

(1995) that shows no Granger causality on a panel VAR with one lag, and 10% Granger causality on a panel with two lags—shows that GDP does not Granger-cause the narratively identified fiscal consolidations. The Moving Average approach has the advantage of being parsimonious; the VAR compensates the need for more degrees of freedom with several advantages. First, using a VAR which includes changes in revenues and spending (as a fraction of GDP) and tracks the impact of the narratively identified shifts in fiscal variables on total revenues and total spending allows us to check the strength of our narratively identified instruments; for instance, it allows us to verify if, following a positive shift in taxes, revenues indeed increase. Second, in a VAR the estimated coefficients on the narratively identified shifts in fiscal variables measure

TABLE 12.3. Two-block estimation. Baseline Moving Average Representation

Dependent variable: GDP per capita growth			
$e^u_{i,t} * \mathrm{TB}_{i,t}$	−0.949799	$e^u_{i,t} * \mathrm{EB}_{i,t}$	−0.094089
	(0.125233)		(0.059393)
$e^a_{i,t,0} * \mathrm{TB}_{i,t}$	−0.733702	$e^a_{i,t,0} * \mathrm{EB}_{i,t}$	−0.371942
	(0.159449)		(0.101001)
$e^u_{i,t-1} * \mathrm{TB}_{i,t-1}$	−0.628784	$e^u_{i,t-1} * \mathrm{EB}_{i,t-1}$	−0.249209
	(0.120869)		(0.064568)
$e^a_{i,t-1} * \mathrm{TB}_{i,t-1}$	−0.287714	$e^a_{i,t-1} * \mathrm{EB}_{i,t-1}$	0.06087
	(0.149457)		(0.103290)
$e^u_{i,t-2} * \mathrm{TB}_{i,t-2}$	−0.105233	$e^u_{i,t-2} * \mathrm{EB}_{i,t-2}$	0.265
	(0.119698)		(0.066475)
$e^a_{i,t-2} * \mathrm{TB}_{i,t-2}$	−0.171947	$e^a_{i,t-2} * \mathrm{EB}_{i,t-2}$	0.089771
	(0.207754)		(0.104005)
$e^u_{i,t-3} * \mathrm{TB}_{i,t-3}$	−0.331351	$e^u_{i,t-3} * \mathrm{EB}_{i,t-3}$	0.073684
	(0.129430)		(0.062918)
$e^a_{i,t-3} * \mathrm{TB}_{i,t-3}$	−0.829834	$e^a_{i,t-3} * \mathrm{EB}_{i,t-3}$	0.107686
	(0.334179)		(0.109019)
$(e^a_{i,t,1} + e^a_{i,t,2}) * \mathrm{TB}_{i,t}$	0.267816	$(e^a_{i,t,1} + e^a_{i,t,2}) * \mathrm{EB}_{i,t}$	−0.348291
	(0.112263)		(0.078104)

φ_1^{TB}	φ_2^{TB}	φ_1^{EB}	φ_2^{EB}
0.284212	0.047558	0.117178	0.043429
(0.053105)	(0.021784)	(0.031600)	(0.012962)

the effect on output growth of the component of such adjustments that is orthogonal to lagged included variables: thus the estimated multipliers are not affected by the possible predictability of plans on the basis of the lagged information included in the VAR. Note that the narrative strategy adopted to identify exogenous fiscal corrections means that they can be predicted by past components of the deficit. This is fine because consistent estimates of fiscal multipliers require only that innovations in output growth and the components of fiscal adjustment plans are not correlated—an assumption that is not violated by predictability from past information. Simulation instead could be a problem, as the simulated shift in fiscal policy should not be those that agents have already predicted. The results in Chapter 7 are based on the more parsimonious MA representation. We use both models in Chapter 9 and find that they deliver very similar impulse responses, thus confirming that the predictability of fiscal plans on the basis of past deficits has a negligible empirical effect.

Predictability of Plans

Fiscal adjustments identified by the narrative approach are predictable by construction, either by their own past or by past economic data and this predictability could be a threat to their exogeneity. This point has been made by De Cos and Moral-Benito (2016) and Jordà and Taylor (2016) with reference to the narrative data constructed by Devries et al. (2011) and on which we have built—with many extensions—our dataset. Let us go through these arguments in turn.

First, finding that narratively identified fiscal "shocks" are predictable is not surprising: predictability is a feature of plans that contain the implementation in year t of measures that had been decided on in previous years and thus are by construction predictable. Assume you overlook announcements and plans and consider only the shifts in fiscal variables happening in year t, that is, $\widetilde{e_t} = e_t^u + e_{t-j,t}^a$, where the second term on the right-hand side measures the realization in year t of shifts in fiscal variables that had been announced j years before. Guajardo et al. (2014) analyzed the fiscal "shocks" $\widetilde{e_t}$ and found them to be predictable by their own past. As we explained in Chapter 5, within a plan policy announcements are correlated with unanticipated policy shifts.

Under the null, that the e_t^u are not correlated over time (and considering for simplicity one-year plans), $Cov\,(\widetilde{e}_t, \widetilde{e}_{t-1}) = \phi_1 \, Var\left(e_{t-1}^u\right)$.[6] Finding $Cov\,(\widetilde{e}_t, \widetilde{e}_t - 1) \neq 0$ is therefore not surprising. In other words, predictability of $\widetilde{e}_t$ from their own past is a feature of multiyear fiscal plans and is properly dealt with by analyzing plans rather than “shocks” such as $\widetilde{e}_t$. Predictability of $\widetilde{e}_t$ by past economic data raises a separate issue. De Cos and Moral-Benito (2016) show that if the $\widetilde{e}_t$ are described by a dummy variable that takes the value of 1 when $\widetilde{e}_t \neq 0$, they are predictable based on information available at time $(t-1)$. This observation, however, does not take into account the fact that there are two sources of identification of narrative adjustments: the *timing* of a fiscal correction and its *size*. Transforming fiscal adjustments into a 0/1 dummy neglects the importance of size as a source of identification. Second, predictability is different from exogeneity. To understand this, think of the following example (see Colacito, Hoffmann, and Phan [2016]). Seasonal temperatures have significant and systematic effects on the economy, both at the aggregate level and across a wide cross section of economic sectors. This effect is particularly strong during the summer: in the United States, for example, a 1 degree Fahrenheit increase in the average summer temperature is associated with a reduction in the annual growth rate of state-level output of 0.15 to 0.25 percentage points. Summer temperature is predictable, but this does not make it endogenous. In our context, what matters to avoid endogeneity is that narrative fiscal corrections are independent of the current state of the cycle: this is the criterion that drives narrative identification.

An Alternative Specification

Finally, as discussed in the concluding section of Chapter 5, we could have chosen an alternative specification. Instead of interacting the change in the primary deficit with the two dummies (EB and TB), we could have *directly* introduced unexpected, announced, and implemented changes in taxes and spending into the estimated regression, and from there derive multipliers for taxes and spending directly. However, this approach has an important drawback. Using τ_t and g_t directly in the empirical model would require estimating, in the second block, a much larger number of φ_j parameters: between announced and unexpected changes in taxes (and in spending) and all cross correlations, for example between announced changes in taxes and unexpected changes in spending, etc. In other words, since changes in taxes and in expenditures are correlated in the plans in our sample, it would be wrong to interpret the coefficients on taxes and spending as partial derivatives: wrong in the sense that we would be studying a style of fiscal actions that does not reflect what the countries actually did, at least in our sample. Constructing EB and TB plans is a way of greatly simplifying the estimation because the two type of plans are mutually exclusive.

In any case, a possible specification for this alternative model could be

$$\Delta y_{i,t} = \alpha + B_1(L)\tau^u_{i,t} + B_2(L)\tau^a_{i,t,t-j} + C_1(L)g^u_{i,t} + C_2(L)g^a_{i,t,t-j} + \sum_{j=1}^{2}\gamma_j\tau^a_{i,t,j} + \sum_{j=1}^{2}\delta_j g^a_{i,t,j} + \lambda_i + \chi_t + u_{i,t} \tag{12.6}$$

$$\begin{aligned} \tau^u_{i,t} &= \delta_0^{\mathrm{TB}} e^u_{i,t} * \mathrm{TB}_{i,t} + \delta_0^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \epsilon_{0,i,t} \\ g^u_{i,t} &= \vartheta_0^{\mathrm{TB}} e^u_{i,t} * \mathrm{TB}_{i,t} + \vartheta_0^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \upsilon_{0,i,t} \\ \tau^a_{i,t,j} &= \delta_j^{\mathrm{TB}} e^u_{i,t} * \mathrm{TB}_{i,t} + \delta_j^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \epsilon_{j,i,t} \quad j = 1,2 \\ g^a_{i,t,j} &= \vartheta_j^{\mathrm{TB}} e^u_{i,t} * \mathrm{TB}_{i,t} + \vartheta_j^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \upsilon_{j,i,t} \quad j = 1,2 \end{aligned} \tag{12.7}$$

In this alternative specification, the first bloc describes the macroeconomic variables directly as a function of the exogenous corrections in taxes and spending, rather than as a function of TB- and EB-based corrections. The second bloc models the intratemporal and the intertemporal structure of fiscal plans. Note that in this specification both the intratemporal and the intertemporal dimension of plans need to be modeled, because $\tau^u_{i,t}$ and $g^u_{i,t}$ are correlated and their effects cannot be simulated independently. Another way to say this is that we have an identification problem that is solved projecting $\tau^u_{i,t}$ and $g^u_{i,t}$ on $e^u_{i,t} * \mathrm{TB}_{i,t}$ and $e^u_{i,t} * \mathrm{EB}_{i,t}$, respectively. Because $e^u_{i,t} * \mathrm{TB}_{i,t}$ and $e^u_{i,t} * \mathrm{EB}_{i,t}$ are by construction mutually exclusive, it is natural to simulate the effect of one component keeping the other at zero (when $\mathrm{EB} = 1$, $\mathrm{TB} = 0$ by construction). One could avoid this identification problem only if $\tau^u_{i,t}$ and $g^u_{i,t}$ were orthogonal to each other, which is not the case in our sample. If we overlooked this, the estimated coefficients in the first bloc of the system could

TABLE 12.4. Two-block estimation. Alternative MA Representation

Dependent variable: Real GDP per capita growth			
τ_t^u	−0.495994	g_t^u	−0.405841
	(0.122127)		(0.128105)
$\tau_{t,0}^a$	−0.864720	$g_{t,0}^a$	−0.348672
	(0.182199)		(0.160364)
τ_{t-1}^u	−0.235163	g_{t-1}^u	−0.458218
	(0.123533)		(0.135375)
$\tau_{t-1,0}^a$	−0.432061	$g_{t-1,0}^a$	0.376504
	(0.186349)		(0.167767)
τ_{t-2}^u	−0.263732	$g_{t-2,0}^u$	0.694599
	(0.129703)		(0.136117)
$\tau_{t-2,0}^a$	−0.312234	$g_{t-2,0}^a$	0.260049
	(0.206196)		(0.167913)
τ_{t-3}^u	−0.418055	g_{t-3}^u	0.280307
	(0.133359)		(0.131676)
$\tau_{t-3,0}^a$	0.100652	$g_{t-3,0}^a$	−0.141484
	(0.216779)		(0.177044)
$\tau_{t,t+1}^a + \tau_{t,t+2}^a$	−0.499966	$g_{t,t+1}^a + g_{t,t+2}^a$	−0.178579
	(0.143745)		(0.117367)

not be interpreted as partial derivatives. The estimated coefficients from the alternative specification are reported in Table 12.4

FISCAL ADJUSTMENTS AND THE DYNAMICS OF DEBT OVER GDP

Simulating the effects of fiscal adjustments on the dynamics of the debt over GDP ratio (the debt ratio for short) requires a more structured empirical model. Our general description of the empirical model we used to simulate the effects of fiscal plans as follows: the model describes the behavior of a set of macro variables, $\mathbf{Y}_t$, as a function of their past values, $\mathbf{Y}_{t-1}$; the past values of a few policy variables $\mathbf{P}_{t-1}$; and macroeconomic shocks. Similarly, the dynamics of the policy variables can be decomposed into a "rule"—which describes the response of current policy to past policy and past macroeconomic conditions—and deviations from the rule, some of which are our fiscal plans. The dynamics of the debt ratio, d, for country i is

$$d_{it} = \frac{1 + i_{it}}{(1 + x_{it})} d_{it-1} + g_{i,t} - \tau_{i,t} + u_{6,i,t}$$
$$x_{it} \equiv \Delta p_{it} + \Delta y_{it} + \Delta p_{it} \Delta y_{it}$$

where i_{it} is the nominal average net cost of financing the debt , x_{it} nominal output growth, Δp_{it} is GDP inflation, $\tau_{i,t}$ is tax revenue as a fraction of GDP, and $g_{i,t}$ is primary government spending, also as a fraction of GDP. $u_{6,i,t}$ is a stock-flow adjustment, namely a term that tracks the difference between the actual change in the debt ratio and the change associated with the three variables in the foregoing equation. The need for stock-flow adjustment arises, for example, in the presence of revenue from sales or purchases of financial and nonfinancial assets; revaluations, in the case the debt is valued at market prices; debt write-offs, and so forth, all items that do not enter the definition of the primary surplus $\left(g_{i,t} - \tau_{i,t}\right)$.

To track the effect on the debt ratio of austerity plans the model must be extended so that $\mathbf{Y}_t = \left(\Delta y_{i,t}, \Delta p_{it}, i_{it}, d_{it}\right)$, $\mathbf{P}_t = \left(\Delta g_{i,t}, \Delta \tau_{i,t}\right)$. We therefore adopt the following specification:

$$\mathbf{z}_{i,t} = \begin{bmatrix} \Delta y_{i,t} \\ \Delta p_{i,t} \\ i_{i,t} \\ \Delta g_{i,t} \\ \Delta \tau_{i,t} \end{bmatrix}, \mathbf{e}_{i,t} = \begin{bmatrix} e^{u}_{i,t} \\ e^{a}_{i,t-j,t} \\ e^{a}_{i,t,t+j} \end{bmatrix}, \mathbf{a}_i = \begin{bmatrix} a_{1,i} \\ a_{2,i} \\ a_{3,i} \end{bmatrix} \text{ similarly for } \mathbf{b}_i$$

$$\Delta y_{i,t} = A_1 (L) \mathbf{z}_{i,t-1} + \begin{bmatrix} \mathbf{a}_1' \mathbf{e}_{i,t} & \mathbf{b}_1' \mathbf{e}_{i,t} \end{bmatrix} \begin{bmatrix} \mathrm{TB}_{i,t} \\ \mathrm{EB}_{i,t} \end{bmatrix} + \lambda_{1,i} + \chi_{1,t} + u_{1,i,t}$$

$$\Delta p_{i,t} = A_2 (L) \mathbf{z}_{i,t-1} + \begin{bmatrix} \mathbf{a}_2' \mathbf{e}_{i,t} & \mathbf{b}_2' \mathbf{e}_{i,t} \end{bmatrix} \begin{bmatrix} \mathrm{TB}_{i,t} \\ \mathrm{EB}_{i,t} \end{bmatrix} + \lambda_{2,i} + \chi_{2,t} + u_{2,i,t}$$

$$i_{it} = A_1 (L) \mathbf{z}_{i,t-1} + \begin{bmatrix} \mathbf{a}_3' \mathbf{e}_{i,t} & \mathbf{b}_3' \mathbf{e}_{i,t} \end{bmatrix} \begin{bmatrix} \mathrm{TB}_{i,t} \\ \mathrm{EB}_{i,t} \end{bmatrix} + \lambda_{3,i} + \chi_{3,t} + u_{3,i,t}$$

$$\Delta g_{i,t} = A_2(L)\,\mathbf{z}_{i,t-1} + \begin{bmatrix} \beta_{11} & \beta_{12} & \beta_{13} & \beta_{14} \end{bmatrix} \begin{bmatrix} g^u_{i,t} \\ g^a_{i,t-1,t} \\ \tau^u_{i,t} \\ \tau^a_{i,t-1,t} \end{bmatrix}$$

$$+ \lambda_{4,i} + \chi_{4,t} + u_{4,i,t}$$

$$\Delta \tau_{i,t} = A_3(L)\,\mathbf{z}_{i,t-1} + \begin{bmatrix} \beta_{21} & \beta_{22} & \beta_{23} & \beta_{24} \end{bmatrix} \begin{bmatrix} g^u_{i,t} \\ g^a_{i,t-1,t} \\ \tau^u_{i,t} \\ \tau^a_{i,t-1,t} \end{bmatrix}$$

$$+ \lambda_{5,i} + \chi_{5,t} + u_{5,i,t}$$

$$d_{it} = \frac{1 + i_{it}}{(1 + x_{it})} d_{it-1} + g_{i,t} - \tau_{i,t} + u_{6,i,t}$$

$$x_{it} \equiv \Delta p_{it} + \Delta y_{it} + \Delta p_{it} \Delta y_{it}$$

No modification is required for the bloc that models the fiscal plans. Thus, estimation and simulation of this extended model can then be implemented using exactly the same techniques described for the baseline VAR in Chapter 5.

TB VERSUS EB AUSTERITY IN A GENERAL EQUILIBRIUM MODEL

Standard new Keynesian models with less than perfectly flexible prices cannot explain our empirical results on the difference between TB and EB consolidations. These models predict that spending cuts are always recessionary (see, e.g., DeLong and Summers, 2012, Galí et al., 2007) and that the multiplier for government spending is larger (in absolute value) than that for taxes. Recent research (see e.g. Christiano, Eichenbaum, and Rebelo, 2011, Eggertsson, 2011) finds that this result is amplified at the zero lower bound. These models concentrate on the demand side: the output effects of fiscal policy, however, also depend on wealth effects; on intertemporal substitution effects; on the effects of distortions on the economy; and on the nature of public spending, in particular if it is a substitute for or a complement to private spending. These channels operate differently in the case of tax increases as opposed to expenditure cuts. When taxes are lump sum, and when agents derive no benefits from public spending, a reduction in government spending raises private wealth because future expected taxes fall. Private consumption increases and (if leisure and consumption are normal goods) labor supply falls. If labor demand does not change when government spending changes, then hours worked decrease, the real wage increases, and output falls. For output to increase after a reduction in government spending, taxes need to be distortionary, and the intertemporal substitution elasticity must be sufficiently high. Intuitively, this happens because when the intertemporal substitution elasticity is high, the wealth effect produced by a cut in government spending is small relative to the substitution effect generated by the reduction in distortionary taxes.

Alesina et al. (2017) extend the basic neo-Keynesian model with tax distortions by introducing TB and EB fiscal plans. They investigate the mechanism that could explain heterogeneity in the output effect of such plans, finding that the persistence of shifts in fiscal variables is the key to explaining the observed heterogeneous effects of different plans. EB plans are the least recessionary the longer lived is the reduction in government spending. Symmetrically, TB plans are more recessionary the longer lasting is the increase in tax burdens.

To grasp the intuition, think in terms of a simple supply and demand framework such as the one shown in Figures 12.1 and 12.2. Assume that the government budget is always balanced through compensating changes in nondistortionary transfers. A cut in government expenditure has two effects. The demand curve shifts inward, due to the direct effect of lower demand from the government. The supply curve also shifts inward: following a cut in government spending, consumers feel richer because they expect higher transfers in the future. This lowers labor supply, which in turn leads to an increase in firms' marginal costs. The shifts in aggregate supply and demand are functions of the persistence of fiscal adjustments: higher persistence implies both higher demand and higher supply elasticities, because the long-term nature of fiscal shocks makes consumers more sensitive to changes in prices, and firms more aggressive in their price settings. On the other hand, the present value

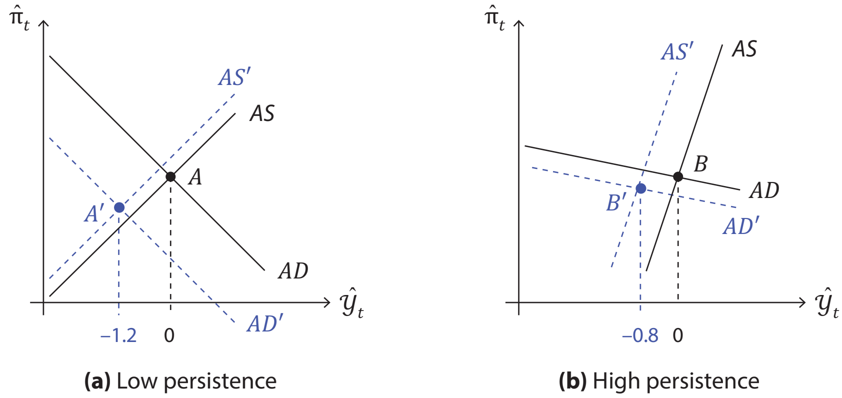

Figure 12.1. The output effect of a cut in government spending.

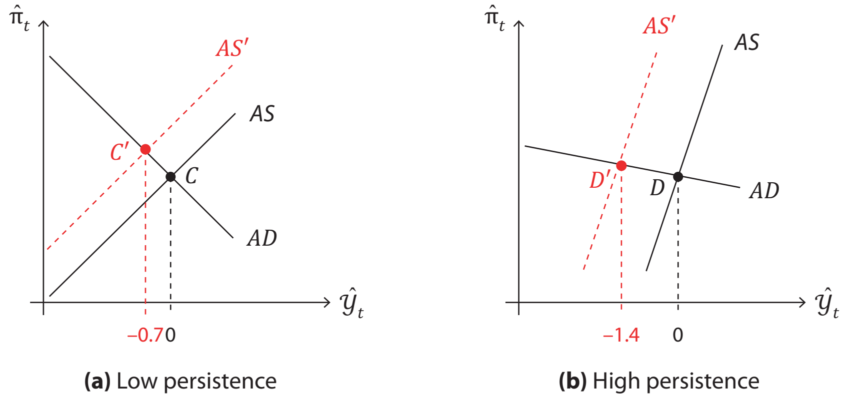

Figure 12.2. The output effect of an increase in wage taxes.

of transfers increases with the persistence of spending cuts. The result is that aggregate demand reacts less, but labor supply falls more because of the wealth effect. As shown in Figure 12.1, when persistence increases, the demand shift due to a cut in government expenditure starts to be dominated by the supply shift due to lower labor supply. However, the demand effect falls faster than the supply effect, so the government spending multiplier decreases with persistence.

Symmetrically, in the case of an increase in wage taxes, the multiplier increases with persistence. An increase in wage taxes has a direct effect only on aggregate supply. This is because a wage tax creates a wedge in the labor market but does not distort demand directly. As in the case of reductions in government consumption, higher persistence raises the elasticities of both supply and demand. However, it is clear that now the shift in supply dominates: as persistence rises, this shift amplifies. To put it simply, a persistent increase in labor taxes makes the static substitution effect between labor and leisure more permanent and this increases the wage tax multiplier.

RECONSIDERING BLANCHARD AND LEIGH (2014) IN CHAPTER 8

Blanchard and Leigh (2014, hereafter BL) address the stability of fiscal multipliers, investigating the relation between the International Monetary Fund growth forecast errors and the total amount of fiscal consolidations expected to be implemented in 2011, based on IMF forecasts. In practice, they run an ordinary least squares (OLS) regression on a cross section of 27 advanced economies, employing a cyclically adjusted measure of changes in the structural budget balance

$$\left(\frac{Y_{i,2011}-Y_{i,2009}}{Y_{i,2009}}-\frac{Y^{f}_{i,2011}-Y_{i,2009}}{Y_{i,2009}}\right)=\alpha+\beta\left(\frac{F^{f}_{i,2011}}{Y^{f,pot}_{i,2011}}-\frac{F_{i,2009}}{Y^{pot}_{i,2009}}\right)+\epsilon_i$$

The variable on the left hand side is the difference between the actual cumulated real GDP growth (year-over-year) during 2010–11 (based on the latest data) and the forecast prepared for the April 2010 IMF World Economic Outlook (WEO). This variable is regressed on the forecasted change in the general government fiscal balance as a percent of potential GDP during 2010–1 prepared for the same issue of the WEO.

Under the null hypothesis—that fiscal multipliers used for forecasting were accurate—the coefficient β should be 0. They found instead an estimated parameter equal to -1.095 (t-statistic = -4.294). This result suggests that for every additional percentage point of GDP of fiscal consolidation, GDP was about 1% lower than forecasted. They interpret the result as implying that fiscal multipliers in 2011 were higher than those predicted by forecasters: "Stronger planned fiscal consolidation

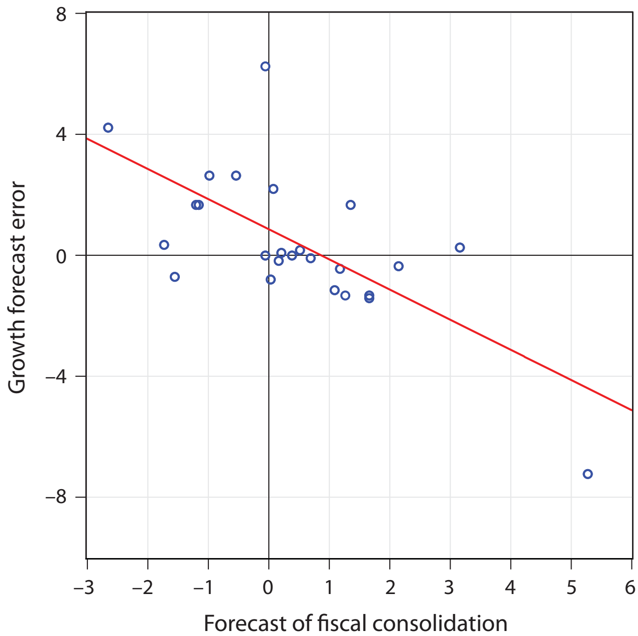

Figure 12.3. Blanchard and Leigh argument.

has been associated with lower growth than expected, with the relation being particularly strong, both statistically and economically, early in the crisis.”

Figure 12.3 illustrates the main result by plotting Growth Forecast errors (forecast errors for real GDP growth in 2010 and 2011 relative to forecasts made in the spring of 2010) versus Fiscal Consolidation Forecasts (forecasts of Fiscal Consolidation for 2010 and 2011 made in spring of 2010). The slope of the regression line, which is −1, illustrates the main point by the authors.

A closer look at Figure 12.3 suggests that for several countries in the sample what was forecasted was in fact a negative consolidation, that is, a fiscal expansion. It is therefore interesting to see if separating consolidations and expansions makes a difference. In columns 1 and 2 of Table 12.5 we compare (using the BL data) the results of the baseline model estimated by BL with one in which fiscal consolidations are interacted with a dummy identifying expansions and contractions.

TABLE 12.5. Blanchard and Leigh Regressions

	Baseline	*Model 1*	*Model 2*	*Model 3*	*Model 4*
const	0.775 (2.03)	1.319 (2.24)	1.014 (1.74)	0.195 (0.46)	0.817 (1.24)
$\left(\frac{F^{f}_{i,2011}}{Y^{f,pot}_{i,2011}} - \frac{F_{i,2009}}{Y^{pot}_{i,2009}}\right)$	−1.095 (−4.29)				
$\left(\frac{F^{f}_{i,2011}}{Y^{f,pot}_{i,2011}} - \frac{F_{i,2009}}{Y^{pot}_{i,2009}}\right) * D^{EXP}$		−0.394 (−0.65)	−0.461 (−0.74)		−0.507 (−0.79)
$\left(\frac{F^{f}_{i,2011}}{Y^{f,pot}_{i,2011}} - \frac{F_{i,2009}}{Y^{pot}_{i,2009}}\right) * (1 - D^{EXP})$		−1.401 (−3.95)	−1.183 (−3.55)		−0.922 (−1.86)
$\Delta 10Y_{i,2007/2009}$			−0.6433 (−4.06)	−0.95 (−2.42)	−0.675 (−3.07)
$\Delta 10Y_{i,2009/2011}$				−0.20 (−6.48)	−0.0647 (−1.08)
R^2	0.49	0.53	0.62	0.51	0.63
No. of obs.	26	26	24	24	24

While the first column of the table exactly replicates the results of BL, the second column illustrates that the bulk of the evidence is indeed generated by the episodes of fiscal consolidation, thus refining the conclusions of BL.

Note that in columns 1 and 2 the only regressor used to explain growth forecast errors are the forecasts of fiscal consolidation constructed in BL; it is therefore in principle possible that the fiscal adjustment variable acts as an instrument for other shocks that hit the economies during 2010–11. In this case the results obtained with this specification might change when one includes in the regression other variables that were in the information set of the forecasters and whose effect on output might have changed during the crisis. In fact, BL conducted an extensive analysis of potential variables included in forecasters' information set but omitted in these regressions: sovereign debt, financial sector stress, banking crises, fiscal consolidation in trading partners, precrisis external imbalances, and household debt overhang. Their main finding is that their main result is robust to these alternative specifications. However, they did not consider long-term yields on government bonds. Yields are a natural candidate to test the effect of potential omitted variables. First, they fluctuated in an important way during the subprime loan crisis. Hence including the change in interest rates in the period preceding 2010–11 could be an interesting way of testing that a change in the response of growth to fluctuations in long-term interest rates could also be responsible for the observed forecast errors. Second, the level of long-term interest rates was relatively low at the beginning of 2010 before the (unexpected) explosion of the Greek crisis. Over the course of 2011, long-term interest rates in some countries in the sample increased sharply and a large difference emerged between the realized level of long-term rates and the level expected at the beginning of 2010. If these prediction errors—that is, prediction errors on yields—were correlated with the planned fiscal consolidation in 2010, then the BL coefficient would also capture the output effect of the surprise increase in long-term rates. In this case, it would be hard to interpret it as a measure of the underestimation of the size of the fiscal multiplier and the policy implications of the correlation observed in BL would change rather drastically. Note that this interpretation is not implausible because if the subprime loan crisis induced an increase in the volatility of long-term rates and in the price of risk, high-debt and high-risk countries were forced to implement a fiscal adjustment as soon as their economies started recovering from the effects of the subprime loan crisis. Such a fiscal correction would have reduced the risk in a worst case scenario in which some new shocks would have caused long-term rates to spike again: a worst case scenario that was in fact sparked by the Greek announcement during 2010 that their deficit has been vastly underestimated.

To take into account the effect of changes in long-term interest rates, in column 3 of the table 12.5 we add, as a further regressor $\Delta 10Y_{i,2007/2009}$, the change in the yield to maturity of 10Y government bonds only between the end of 2007 and the end of 2009. All the regressors in this specification were in the information set of the forecasters. Both coefficients on $\Delta 10Y_{i,2007/2009}$ and $\left(\frac{F^{f}_{i,2011}}{Y^{f,pot}_{i,2011}} - \frac{F_{i,2009}}{Y^{pot}_{i,2009}}\right)$ are significant, thus pointing to the coexistence of two potentially separate channels explaining the GDP growth forecast errors. In column 4 of the same table we only consider as regressors the change in the yield to maturity of 10Y government bonds between the end of 2007 and the end of 2009 and between the end of 2011 and the beginning of 2010. This specification illustrates that (1) the argument applied by BL to the instability of fiscal multipliers could be also applied to the effects on growth of the change in long-term interest rates, as confirmed by the significance of the coefficient on $\Delta 10Y_{i,2007/2009}$; (2) shocks to long-term interest rates over the period 2010–11 played an important role in determining the forecast error for output growth over the same period, as demonstrated by the significance of the coefficient on $\Delta 10Y_{i,2009/2011}$. Interestingly when fiscal adjustment and fluctuations in interest rates (both before and during the forecast period) are considered, as in column 5, the coefficient on $\Delta 10Y_{i,2007/2009}$ remains significant while both the coefficients on $\Delta 10Y_{i,2009/2011}$ and $\left(\frac{F^f_{i,2011}}{Y^{f,pot}_{i,2011}} - \frac{F_{i,2009}}{Y^{pot}_{i,2009}}\right)$ become insignificant, proving the existence of correlation among these two variables. Therefore, the correlation between fiscal adjustments and fluctuations in long-term rate over 2010 and 2011 questions the interpretation that attributes the significance of the coefficient on fiscal adjustments in column 2 to the mismeasurement of the fiscal multipliers.[7]

We further assess these results by considering, as dependent variables, rather than the forecast errors constructed by BL, the forecast errors conditional on the implementation of fiscal adjustment plans, thus based on the methodology adopted throughout the book. We use our results in Chapter 8 to construct a panel of 11 economies over two periods (2010–11 and 2012–13):

$$\left(\frac{Y_{i,t} - Y_{i,t-2}}{Y_{i,t-2}} - \frac{Y^f_{i,t} - Y_{i,t-2}}{Y_{i,t-2}}\right) = \alpha + \beta_1 e_{it-2} + \beta_2 e^a_{it-2,t-1,t} + \beta_3 \Delta 10Y_{i,t-4/t-2} + \varepsilon_{it}$$

$$t = 2011, 2013$$

The results of the estimation of this model are reported in Table 12.6.

Whereas in columns 1 and 2 we consider only the impact on growth forecast errors of fiscal adjustments e_{it-2} and of their announcements $e^a_{it-2,t-1,t}$, in column 3 and 4 the effect of pre-austerity long-term interest rates changes is also assessed. The evidence reported confirms that the instability of the fiscal multipliers becomes much weaker when the role of the fluctuations in long-term interest rates before the forecast period and of other shocks (as captured by the time effects in column 4) is considered.

TABLE 12.6. Our Model

	Baseline	*Model 1*	*Model 2*	*Model 2 FE*
const	1.066	1.205	1.08	0.624
	(1.3)	(1.53)	(1.25)	(1.16)
e_{it-2}	−1.090	−0.812	−0.748	−0.291
	(−2.95)	−0.812	−0.748	−0.291
$e^a_{it-2,t-1,t}$		−0.538	−0.449	−0.379
		(−3.52)	(−1.13)	(−0.72)
$\Delta 10Y_{i,t-4/t-2}$			−0.098	−0.587
			(−0.27)	(−2.48)
R^2	0.29	0.34	0.34	0.29
No. of obs.	22	22	22	22

Note: Model 1 separates expansions from contractions, Model 2 adds pre-austerity yields, Model 3 considers only pre-austerity and contemporaneous yields, Model 4 adds pre-austerity and contemporaneous yields.

THE MODEL OF "HOW" AND "WHEN" IN CHAPTER 9

To allow for the impact of a fiscal adjustment to depend on the state of the economy, two ingredients are necessary: an indicator for the state of the economy and a model in which the dynamics of all variables depend on the state of the economy. This is the approach adopted by Auerbach and Gorodnichenko (2012a, 2013b) to produce different fiscal multipliers in expansion and recession. In practice we need to reconsider once more our general framework and choose a different specification for the relevant vaiables and the relevant functional forms f describing the relationships among them.

Consider the case in which one wants to allow the f functions to depend on the state of the economy. This is measured using a logistic function $F(s_{i,t})$ (where the index i refers to the country) that smooths the distribution of output growth, $\Delta y_{i,t-j}$ ($j = 1, 2, ...$) and transforms it into a variable ranging between 0 and 1. The transition between states of the economy happens smoothly, with $F(s_{i,t})$ being the weight given to recessions and $1 - F(s_{i,t})$ the weight given to expansions. The weighted average of output growth over the previous 2 years, $F(s_{i,t})$ is

$$F(s_{i,t}) = \frac{\exp(-\gamma_i s_{i,t})}{1 + \exp(-\gamma_i s_{i,t})}, \quad \gamma_i > 0, \tag{12.8}$$

$$s_{i,t} = \left(\mu_{i,t} - E\left(\mu_{i,t}\right)\right) / \sigma\left(\mu_{i,t}\right) \tag{12.9}$$

$$\mu_{i,t} = \frac{\Delta y_{i,t-1} + \Delta y_{i,t-2}}{2} \tag{12.10}$$

where $\mu_{i,t}$ is the moving average (and $s_{i,t}$ its standardized version) of output growth during the 2 years preceding the shift in fiscal policy and γ_i are the country-specific parameters of the logistic function. An economy is defined to be in recession if $F(s_{i,t}) > 0.8$. The parameters γ_i are calibrated to match actual recession probabilities in the countries in our sample: that is, the percentage of years in which growth is negative over the sample, which consists of yearly data from 1979 to 2014. In other words, we calibrate γ_i so that country i spends x_i percent of time in a recessionary regime. $Pr(F(s_{i,t}) > 0.8) = x_i$, where x_i is the ratio of the number of years of negative GDP growth for country i to the total number of years in the sample.

Next we specify a model for the dynamics of three variables—the growth rate of per capita output ($\Delta y_{i,t}$); the change of tax revenues as a fraction of GDP ($\Delta \tau_{i,t}$); and that of primary government spending, also as a fraction of GDP ($\Delta g_{i,t}$)—as a function of the state of the economy. In this specification, taxes and spending enter in two ways: as endogenous variables in the model—in this case they are total government spending and total revenue, which includes both their exogenous and endogenous components—and as narratively identified shifts in taxes and spending, which are exogenous

$$\begin{aligned}
\Delta y_{i,t} &= (1 - F(s_{i,t}))A_1^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_1^R(L)\,\mathbf{z}_{i,t-1} \\
&\quad + \lambda_{1,i} + \chi_{1,t} + u_{1,i,t} \qquad (12.11)\\
\Delta g_{i,t} &= (1 - F(s_{i,t}))A_2^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_2^R(L)\,\mathbf{z}_{i,t-1} \\
&\quad + \lambda_{2,i} + \chi_{2,t} + u_{2,i,t} \\
\Delta \tau_{i,t} &= (1 - F(s_{i,t}))A_3^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_3^R(L)\,\mathbf{z}_{i,t-1} \\
&\quad + \lambda_{3,i} + \chi_{3,t} + u_{3,i,t} \\
\mathbf{u}_{i,t} &= \begin{bmatrix} u_{1,i,t} \\ u_{2,i,t} \\ u_{3,i,t} \end{bmatrix} \sim N(0, \Sigma_t) \\
\Sigma_t &= \Sigma_E(1 - F(s_{t-1})) + \Sigma_R F(s_{t-1})
\end{aligned}$$

where $\mathbf{z}_{it}$: $[\Delta y_{it}, \Delta g_{it}, \Delta \tau_{it}]$. Auerbach and Gorodnichenko identify structural shocks to fiscal variables from VAR innovations using the approach of Blanchard and Perotti (2002). They then derive impulse responses to such shocks in the state of recession and expansion.

To allow simultaneously for nonlinearities related to the "How" and the "When," Alesina et al. (2018) have adopted a specification that includes the observed (narratively identified) fiscal measures directly, rather than identifying them from VAR innovations. In practice one replaces the standard VAR specification described in Chapter 5 with a nonlinear Smooth Transition VAR specification (STAR).

$$\mathbf{z}_{i,t} = \begin{bmatrix} \Delta y_{i,t} \\ \Delta g_{i,t} \\ \Delta \tau_{i,t} \end{bmatrix}, \mathbf{e}_{i,t} = \begin{bmatrix} e^{u}_{i,t} \\ e^{a}_{i,t-j,t} \\ e^{a}_{i,t,t+j} \end{bmatrix}, \mathbf{a} = \begin{bmatrix} a_1 \\ a_2 \\ a_3 \end{bmatrix} \text{ similarly for } \mathbf{b}, \mathbf{c}, \mathbf{d}$$

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

$$\tau^u_{i,t} = \delta_0^{\mathrm{TB}}\, e^u_{i,t} * \mathrm{TB}_{i,t} + \delta_0^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \epsilon_{0,i,t}$$

$$g^u_{i,t} = \vartheta_0^{\mathrm{TB}}\, e^u_{i,t} * \mathrm{TB}_{i,t} + \vartheta_0^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \upsilon_{0,i,t}$$

$$\tau^a_{i,t,t+j} = \delta_j^{\mathrm{TB}}\, e^u_{i,t} * \mathrm{TB}_{i,t} + \delta_j^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \epsilon_{j,i,t} \quad j = 1,2$$

$$g^a_{i,t,t+j} = \vartheta_j^{\mathrm{TB}}\, e^u_{i,t} * \mathrm{TB}_{i,t} + \vartheta_j^{\mathrm{EB}} e^u_{i,t} * \mathrm{EB}_{i,t} + \upsilon_{j,i,t} \quad j = 1,2$$

As it is the case with the VAR of Chapter 5, the narratively identified exogenous fiscal measures enter the estimation in two ways. In the output growth equation, they enter as shifts in $e_{i,t}$, the primary budget deficit. These are then interacted with the type of consolidation, TB or EB. The variable $\mathbf{e}_{i,t}$ has three components $\left[e^u_{i,t}\ e^a_{i,t-j,t}\ e^a_{i,t,t+j}\right]$ because shifts in fiscal variables can be unanticipated, announced, or an implementation of previously announced measures.

Unlike the output growth equation, in the two equations for $\Delta g_{i,t}$ and $\Delta\tau_{i,t}$ we allow for expenditure and revenue corrections to have different coefficients. Note that only the part of a narratively identified fiscal correction that is implemented in period t can affect the growth rates of revenues and expenditures: this is because future announced corrections do not directly affect total revenues and total expenditures; they are determined either by discretionary policy actions or by the endogenous response to fluctuations in output, not by announcements.

In the model, nonlinearities with respect to the state of the economy and with respect to the composition of a fiscal plan affect per capita output growth, both on impact and through the dynamic response of the economy to a consolidation plan. On impact, the possible nonlinearities associated with a consolidation plan—stemming from its composition and from the state of the economy—are described by the coefficient vectors **a**, **b**, **c**, **d** in the first equation of the model.

The structure of the fiscal adjustment plans is unaltered. The impulse responses reported in Figure 12.4 are constructed simulating the full model. They allow us to measure the effect of EB and TB plans adopted during an expansion or a recession.

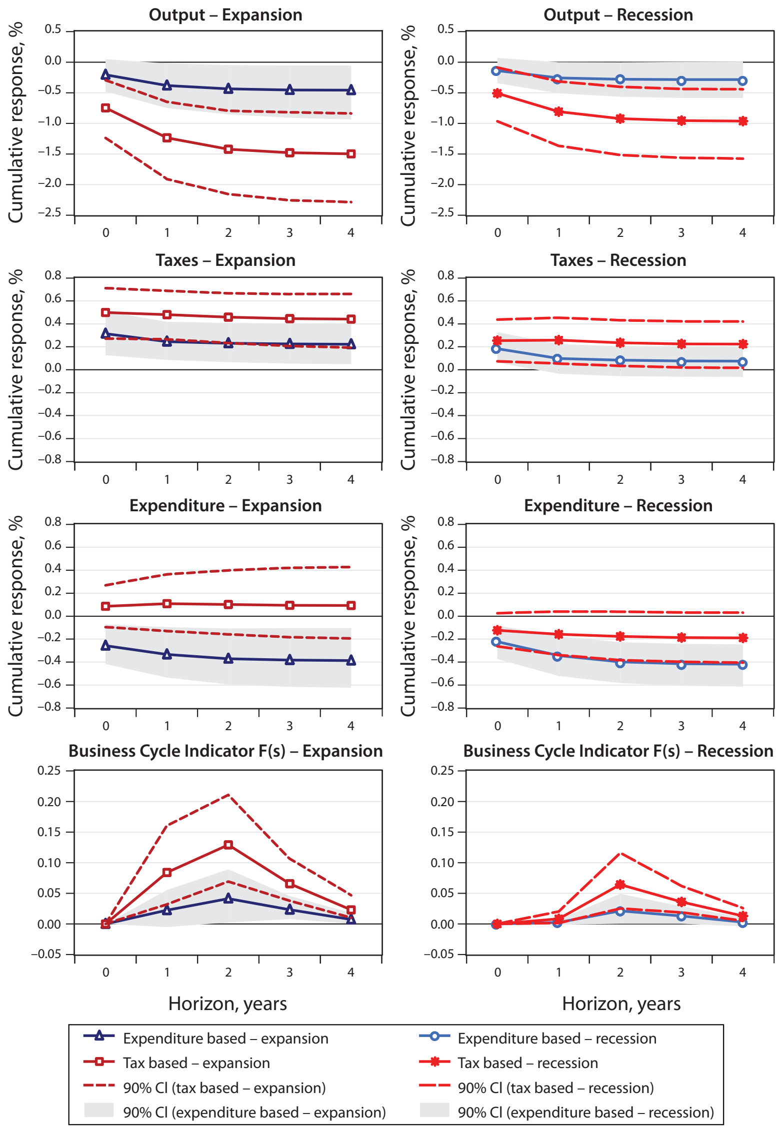

Figure 12.4. Impulse responses of output, taxes, spending and $F(s)$. *Source:* Alesina et al. (2018). *Note:* Allowing for heterogeneity between EB and TB plans and across states of the cycle.

Nonlinearities Related to Public Debt

When measuring the impact of fiscal adjustment it might be important to consider the possibility that the state of the economy is determined by the level of public debt. A fiscal consolidation began when debt is so high that investors consider it no longer sustainable could have different effects than one implemented when debt is rather low and/or stable. Following up on work by Ilzetzki, Mendoza, and Végh (2013), Huidrom et al. (2016) studied the relationship between fiscal multipliers and the fiscal position of the government in a panel of advanced and developing (19 advanced and 15 developing) economies using data at quarterly frequency over the period 1980:1–2014:1. They estimated a panel VAR in which lagged variables (real government consumption, the only fiscal instrument, real GDP, the real effective exchange rate, and the current account balance as a share of GDP) are interacted with a (lagged) moving average of the government debt over GDP ratio. Spending shocks are identified assuming that government consumption does not react contemporaneously to any other variable included in the VAR. Impulse responses show that government consumption multipliers do depend on the level of the debt ratio: multipliers tend to be as large as one when the fiscal position is strong (low debt over GDP ratio) and negative when the fiscal position is weak. These authors suggest that two channels could be at work: an interest rate channel, through which higher borrowing costs crowd out private investment; and a Ricardian channel, through which households reduce consumption in anticipation of future fiscal adjustment.

However, nonlinearities related to debt can be naturally studied by considering a STAR specification based on the VAR model with debt studied and illustrated in Chapter 7.

$$\mathbf{z}_{i,t} = \begin{bmatrix} \Delta y_{i,t} \\ \Delta p_{i,t} \\ i_{i,t} \\ \Delta g_{i,t} \\ \Delta \tau_{i,t} \end{bmatrix}, \mathbf{e}_{i,t} = \begin{bmatrix} e^{u}_{i,t} \\ e^{a}_{i,t-j,t} \\ e^{a}_{i,t,t+j} \end{bmatrix}, \mathbf{a}_i = \begin{bmatrix} a_{1,i} \\ a_{2,i} \\ a_{3,i} \end{bmatrix}$$

similarly for $\mathbf{b}_i, \mathbf{c}_i, \mathbf{d}_i$

$$
\begin{aligned}
\Delta y_{i,t} &= (1-F(s_{i,t}))A_1^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_1^R(L)\,\mathbf{z}_{i,t-1} \\
&\quad + \begin{bmatrix} 1-F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} \mathbf{a}_1'\mathbf{e}_{i,t} & \mathbf{b}_1'\mathbf{e}_{i,t} \\ \mathbf{c}_1'\mathbf{e}_{i,t} & \mathbf{d}_1'\mathbf{e}_{i,t} \end{bmatrix} \begin{bmatrix} \mathrm{TB}_{i,t} \\ \mathrm{EB}_{i,t} \end{bmatrix} \\
&\quad + \lambda_{1,i} + \chi_{1,t} + u_{1,i,t} \qquad (12.12) \\
\Delta p_{i,t} &= (1-F(s_{i,t}))A_1^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_1^R(L)\,\mathbf{z}_{i,t-1} \\
&\quad + \begin{bmatrix} 1-F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} \mathbf{a}_2'\mathbf{e}_{i,t} & \mathbf{b}_2'\mathbf{e}_{i,t} \\ \mathbf{c}_2'\mathbf{e}_{i,t} & \mathbf{d}_2'\mathbf{e}_{i,t} \end{bmatrix} \begin{bmatrix} \mathrm{TB}_{i,t} \\ \mathrm{EB}_{i,t} \end{bmatrix} \\
&\quad + \lambda_{2,i} + \chi_{2,t} + u_{2,i,t} \\
\Delta p_{i,t} &= (1-F(s_{i,t}))A_1^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_1^R(L)\,\mathbf{z}_{i,t-1} \\
&\quad + \begin{bmatrix} 1-F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} \mathbf{a}_3'\mathbf{e}_{i,t} & \mathbf{b}_3'\mathbf{e}_{i,t} \\ \mathbf{c}_3'\mathbf{e}_{i,t} & \mathbf{d}_3'\mathbf{e}_{i,t} \end{bmatrix} \begin{bmatrix} \mathrm{TB}_{i,t} \\ \mathrm{EB}_{i,t} \end{bmatrix} \\
&\quad + \lambda_{3,i} + \chi_{3,t} + u_{3,i,t} \\
\Delta g_{i,t} &= (1-F(s_{i,t}))A_2^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_2^R(L)\,\mathbf{z}_{i,t-1} \\
&\quad + \begin{bmatrix} 1-F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} \beta_{11} & \beta_{12} & \beta_{13} & \beta_{14} \\ \beta_{15} & \beta_{16} & \beta_{17} & \beta_{18} \end{bmatrix} \begin{bmatrix} g_{i,t}^u \\ g_{i,t-1,t}^a \\ \tau_{i,t}^u \\ \tau_{i,t-1,t}^a \end{bmatrix} \\
&\quad + \lambda_{4,i} + \chi_{4,t} + u_{5,i,t} \\
\Delta \tau_{i,t} &= (1-F(s_{i,t}))A_3^E(L)\,\mathbf{z}_{i,t-1} + F(s_{i,t})A_3^R(L)\,\mathbf{z}_{i,t-1} \\
&\quad + \begin{bmatrix} 1-F(s_{i,t}) \\ F(s_{i,t}) \end{bmatrix}' \begin{bmatrix} \beta_{21} & \beta_{22} & \beta_{23} & \beta_{24} \\ \beta_{25} & \beta_{26} & \beta_{27} & \beta_{28} \end{bmatrix} \begin{bmatrix} g_{i,t}^u \\ g_{i,t-1,t}^a \\ \tau_{i,t}^u \\ \tau_{i,t-1,t}^a \end{bmatrix} \\
&\quad + \lambda_{5,i} + \chi_{5,t} + u_{5,i,t} \\
d_{it} &= \frac{1+i_{it}}{(1+x_{it})} d_{it-1} + g_{i,t} - \tau_{i,t} + u_{6,i,t} \\
x_{it} &\equiv \Delta p_{it} + \Delta y_{it} + \Delta p_{it}\Delta y_{it} \\
\tau_{i,t}^u &= \delta_0^{\mathrm{TB}}\, e_{i,t}^u * \mathrm{TB}_{i,t} + \delta_0^{\mathrm{EB}}\, e_{i,t}^u * \mathrm{EB}_{i,t} + \epsilon_{0,i,t}
\end{aligned}
$$

$$g^{u}_{i,t} = \vartheta^{\mathrm{TB}}_{0}\, e^{u}_{i,t} * \mathrm{TB}_{i,t} + \vartheta^{\mathrm{EB}}_{0} e^{u}_{i,t} * \mathrm{EB}_{i,t} + \upsilon_{0,i,t}$$

$$\tau^{a}_{i,t,t+j} = \delta^{\mathrm{TB}}_{j}\, e^{u}_{i,t} * \mathrm{TB}_{i,t} + \delta^{\mathrm{EB}}_{j} e^{u}_{i,t} * \mathrm{EB}_{i,t} + \epsilon_{j,i,t} \quad j = 1, 2$$

$$g^{a}_{i,t,t+j} = \vartheta^{\mathrm{TB}}_{j}\, e^{u}_{i,t} * \mathrm{TB}_{i,t} + \vartheta^{\mathrm{EB}}_{j} e^{u}_{i,t} * \mathrm{EB}_{i,t} + \upsilon_{j,i,t} \quad j = 1, 2$$

Using a nonlinear model specified along these lines, Mei (2016) simultaneously studies two sources of nonlinearity: one associated with the composition of narratively identified fiscal plans (TB or EB adjustments) and one related to the growth rate of the debt over GDP ratio. For the latter, the two regimes to which the model attaches probabilities are high debt growth and stable debt (rather than expansion and recessions, as we did earlier in this chapter). More specifically, the model identifies a high debt growth regime as when the debt ratio increased by at least 3 percentage points on average in the 2 years preceding the fiscal correction. This threshold changes slightly in each country to reflect historical country-specific debt dynamics. Debt is instead defined as "stable" when it did not vary on average in the two years preceding the consolidation. Simulation of the model produces the impulse response functions reported in Figure 12.5.

The asymmetry between EB and TB adjustments is confirmed, independent of the state of debt growth, and is observed in both scenarios. When debt growth is relatively high, fiscal adjustments are less contractionary than when the debt over GDP ratio is stable. Interestingly, and differently from the results in Huidrom et al. (2016), the possibility of expansionary austerity for EB-based plans is a remote one also in the state of high debt growth.

Using an innovative and creative technique to derive impulse response functions, Barnichon and Matthes (2015) found that the multiplier associated with a negative shock to government spending is substantially above 1, while it is well below 1 in the case of a positive spending shock. Multipliers also could depend on the size of the government: cutting 1% of expenditure when it represents 57% of GDP, as in France, is very different than for a country like Ireland where the government spends only 30% of GDP. These are very interesting questions and remain a topic for future research.

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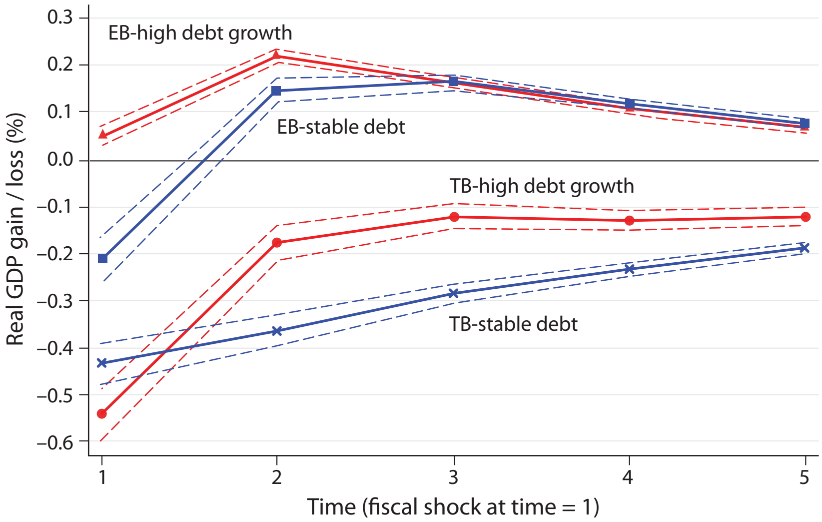

Figure 12.5. Impulse response: high vs. stable debt growth. *Source:* Mei (2016). *Note:* Real GDP generalized impulse response in high debt growth and stable debt scenarios. The initial values of *F* are respectively 0.8 (red simulations) and 0.5 (blue simulations). Red triangle = EB, high debt growth; red circle = TB, high debt growth; blue square = EB, stable debt; blue cross = TB, stable debt.

WHO ADJUSTS AND WHO WINS: MORE DETAILS ON THE MODEL FOR CHAPTER 10

Who Adjusts?

The variable "share_tenure" is the remaining number of years in office the government has divided by the total legislated duration of the term. As an example, a government in Italy in its first year in office will have a share_tenure of 0.8, because it still has four years to go (unless it gets kicked out earlier) on a total term of 5 years. We use this ratio, rather than the simple number of years left in office, because the duration of the executive term varies in the countries of our sample. We use an indicator variable (dummy) for coalition governments: that is, a dummy taking a value of 1 when the government is formed by a coalition of parties and otherwise 0. This variable is motivated by the literature on delayed stabilization that predicts that more divided governments will fail to promptly stabilize budget deficits.

−1—
0—
1—

TABLE 12.7. Probability of a new plan

	(1) *New positive plans*	*(2)* *TB new positive plans*	*(3)* *EB new positive plans*
Coalition dummy	−0.0327	−0.0746**	0.0262
	(0.04)	(0.03)	(0.04)
Share of tenure	0.1356**	0.0991**	0.0055
	(0.06)	(0.04)	(0.06)
Right-wing cabinet dummy	0.1010**	0.0711**	0.0043
	(0.04)	(0.03)	(0.04)
Lagged deficit	−0.0556***	−0.0074*	−0.0424***
	(0.01)	(0.00)	(0.01)
Lagged GDP growth per capita	−0.0460***	−0.0127**	−0.0217**
	(0.01)	(0.01)	(0.01)
Observations	509	509	509

Note: Standard errors in parentheses. ***p <0.01, **p <0.05, *p <0.1.

We also use a dummy for a right-wing cabinet. It equals 1 if the main government party is center right and 0 if the government is left of center.

Table 12.7 presents the results of our probit specification; we jointly evaluate the effects of these three variables on the probability of introducing a new plan of fiscal consolidation. In the first column, the dependent variable is a dummy with a value of one whenever new measures of fiscal restraint are introduced and implemented immediately or are announced for the future. It equals 0 when there is no fiscal consolidation in that year, or when the measures undertaken in that year are simply the execution of a plan announced previously. In the second and the third columns we further distinguish among new Tb or EB plans. In all three columns we add to the specification as economic controls the deficit and GDP growth in period $t - 1$.

The share_tenure is an important predictor of fiscal consolidations. The higher the share of years that a government still has in office—that is, the further off are the next scheduled elections—the higher is the probability that a government will implement a fiscal correction. This holds true even after controlling for lagged deficit. Looking at the second column, this result seems to be particularly driven by TB consolidations. We observe that coalition governments seem to implement fiscal

TABLE 12.8. Probability of a new large plan

	(1) *New positive large plans*	*(2)* *TB new positive large plans*	*(3)* *EB new positive large plans*
Coalition dummy	0.0151	−0.0081	0.0175
	(0.02)	(0.01)	(0.02)
Share of tenure	0.0592**	0.0338**	0.0226
	(0.03)	(0.01)	(0.02)
Right-wing cabinet dummy	0.0001	0.0049	−0.0046
	(0.02)	(0.01)	(0.02)
Lagged deficit	−0.0201***	−0.0020*	−0.0151***
	(0.00)	(0.00)	(0.00)
Lagged GDP growth per capita	−0.0115***	−0.0036*	−0.0070**
	(0.00)	(0.00)	(0.00)
Observations	509	509	509

Note: Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

restraints less often than single-party governments: the coefficient of the dummy is always negative and is strongly significant in the case of TB plans. Right-wing governments appear to initiate fiscal corrections more often than left and center governments. However, contrary to the common belief about right-wing government preferences regarding fiscal policy, we find that they tend to mostly implement consolidations based on tax hikes.

As a robustness check (Table 12.8), we repeat our analysis, redefining our dependent variable in order to capture only the largest fiscal adjustments in our sample—those larger than the 70th percentile. The right-wing and coalition dummies lose statistical significance, but share_tenure still has explanatory power, although its marginal effect is smaller.

Who Wins?

We measure the change in government with a dummy: at time t it is equal to one if the political orientation of the government has changed in either the second semester of year t or the first semester of year $t+1$. We obtain similar results (available from the authors) if we use any change in government cabinet (either a change in the political orientation or in the prime minister) as our dependent variable. The analysis has three different checks, shown in the three regression tables below, namely Tables 12.9, 12.10, and 12.11. Throughout the analysis we include as additional explanatory variables the level of GDP growth per capita in the previous year, the growth in the unemployment rate, the inflation rate (GDP deflator), the number of years the government has been in charge, and two political controls: whether the government is right wing and whether it is formed by a coalition. The first specification focuses on the association between the probability of a government being followed by another with different political ideology (either in regularly scheduled or unanticipated elections), and the same set of explanatory variables that were used to explain output.

We find that the probability of losing the office is a function of past expected and unexpected shocks that are part of previous fiscal plans up to 3 years back. It is also a function of current expected and unexpected shocks and of announcements about the next two years made in the current year or in previous years. One way of intepreting this specification is as follows: on the one hand, when judging the government, electors take into account what the government has done in the current and previous years. They distinguish its current decisions (unexpected shocks) from what it has just implemented, but had decided on earlier (anticipated shocks). On the other hand, the government is evaluated in light of its announced policies that are to be implemented in the near future. Each shock (expected and unexpected) is presumed to have different effects on the outcome, conditional on whether it is part of an EB or TB plan. Table 12.9 shows this analysis both with and without economic controls (first and second column respectively).

In the second column we see a seemingly significant finding: that an unexpected current contractionary measure within an EB plan predicts a higher probability of being replaced in office. However, a more detailed analysis shows that this result is not robust to the exclusion of two observations for Italy: the years 1991 and 1995. Qualitatively, this is similar to other findings reported in column 2: the effects of announcements within a TB plan, or of expected shocks in an EB plan 2 years earlier or in a TB plan 3 years earlier, are not robust.

TABLE 12.9. Change in ideology, all shocks (UT, AT, and announcements. Classification by total composition of the plan.)

	(1) Full sample w/o controls	*(2) Full sample*	*(3) Govt terminations*	*(4) Regular elections*
$e^u_{i,t} * EB_{i,t}$	0.0498**	0.0433**	0.0965	0.4012*
	(0.02)	(0.02)	(0.07)	(0.21)
$e^u_{i,t} * TB_{i,t}$	−0.0292	−0.037	0.1371	−0.484
	(0.04)	(0.04)	(0.28)	(0.75)
$e^a_{i,t,0} * EB_{i,t}$	0.0331	0.0278	0.0994	−0.2391
	(0.04)	(0.04)	(0.15)	(0.29)
$e^a_{i,t,0} * TB_{i,t}$	0.0316	0.0322	0.5894	1.0337
	(0.07)	(0.07)	(0.46)	(1.03)
$e^a_{i,t,1} * EB_{i,t} + e^a_{i,t,2} * EB_{i,t}$	−0.0409	−0.0424	−0.0413	0.1806
	(0.03)	(0.03)	(0.10)	(0.23)
$e^a_{i,t,1} * TB_{i,t} + e^a_{i,t,2} * TB_{i,t}$	0.1287**	0.1235**	0.7575*	1.0373
	(0.06)	(0.06)	(0.44)	(0.94)
$e^u_{i,t-1} * EB_{i,t-1}$	0.0113	0.0122	0.0073	
	(0.02)	(0.02)	(0.08)	
$e^u_{i,t-1} * TB_{i,t-1}$	−0.0207	−0.0174	0.016	
	(0.03)	(0.03)	(0.14)	
$e^a_{i,t-1,0} * EB_{i,t-1}$	−0.0679	−0.0547	−0.0081	
	(0.05)	(0.05)	(0.17)	
$e^a_{i,t-1,0} * TB_{i,t-1}$	−0.0699	−0.064	−0.6507	
	(0.10)	(0.10)	(0.49)	
$e^u_{i,t-2} * EB_{i,t-2}$	−0.0345	−0.0277	−0.1003	
	(0.03)	(0.03)	(0.13)	
$e^u_{i,t-2} * TB_{i,t-2}$	−0.0094	0.0038	0.0333	
	(0.03)	(0.03)	(0.13)	
$e^a_{i,t-2,0} * EB_{i,t-2}$	−0.0781*	−0.0753*	−0.1929	
	(0.04)	(0.04)	(0.14)	
$e^a_{i,t-2,0} * TB_{i,t-2}$	−0.0627	−0.0416	0.2185	
	(0.10)	(0.10)	(0.32)	
$e^u_{i,t-3} * EB_{i,t-3}$	−0.0061	−0.0016	0.0389	
	(0.03)	(0.02)	(0.10)	
$e^u_{i,t-3} * TB_{i,t-3}$	0.0155	0.0198		
	(0.03)	(0.05)		
$e^a_{i,t-3,0} * EB_{i,t-3}$	−0.0195	−0.0103		
	(0.04)	(0.04)		
$e^a_{i,t-3,0} * TB_{i,t-3}$	−0.4639**	−0.4303**		
	(0.22)	(0.22)		
GDP growth per capita		0.0035	−0.0048	−0.0046
		(0.01)	(0.03)	(0.03)
unempl. rate (Yearly variation)		0.0018	0.0078*	0.0104*
		(0.00)	(0.01)	(0.01)
inflation rate		0.0026	0.0119	−0.028
		(0.00)	(0.01)	(0.03)
years from gov. appointment	0.0652***	0.0625***		
	(0.01)	(0.01)		
coalition cabinet dummy		0.0380*	0.0804	0.0847
		(0.02)	(0.09)	(0.12)
right wing cabinet dummy		−0.0066	0.0101	
		(0.05)	(0.23)	
Observations	527	517	175	87

Note: Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE 12.10. Change in ideology, contemporaneous shocks (UT, AT only. Classification by composition of the contemporaneous total shock.)

	(1) *Full Sample w/o controls*	(2) *Full Sample*	(3) *Govt Terminations*	(4) *Regular Elections*
$e^u_{i,t} * \widehat{EB}_{i,t}$	0.022 (0.03)	0.012 (0.03)	0.0222 (0.08)	0.0000 (0.00)
$e^u_{i,t} * \widehat{TB}_{i,t}$	0.0546** (0.02)	0.0518** (0.03)	0.5860*** (0.21)	0.0000 (0.00)
$e^a_{i,t,0} * \widehat{EB}_{i,t}$	0.0045 (0.04)	−0.0055 (0.04)	0.1149 (0.14)	0.0000 (0.00)
$e^a_{i,t,0} * \widehat{TB}_{i,t}$	0.0525 (0.06)	0.0394 (0.06)	−0.1768 (0.35)	0.0000 (0.00)
$e^u_{i,t-1} * \widehat{EB}_{i,t-1}$	0.0593** (0.03)	0.0683** (0.03)	0.1888* (0.10)	
$e^u_{i,t-1} * \widehat{TB}_{i,t-1}$	−0.0206 (0.03)	−0.0104 (0.03)	−0.0052 (0.12)	
$e^a_{i,t-1,0} * \widehat{EB}_{i,t-1}$	−0.0251 (0.04)	−0.0166 (0.04)	0.07 (0.18)	
$e^a_{i,t-1,0} * \widehat{TB}_{i,t-1}$	−0.1123 (0.12)	−0.0784 (0.11)	−0.2637 (0.32)	
$e^u_{i,t-2} * \widehat{EB}_{i,t-2}$	−0.0849** (0.04)	−0.0764** (0.04)	−0.3518** (0.14)	
$e^u_{i,t-2} * \widehat{TB}_{i,t-2}$	−0.0232 (0.03)	−0.0227 (0.03)	−0.0659 (0.11)	
$e^a_{i,t-2,0} * \widehat{EB}_{i,t-2}$	−0.1390*** (0.05)	−0.1298** (0.05)	−0.2043 (0.14)	
$e^a_{i,t-2,0} * \widehat{TB}_{i,t-2}$	−0.0872 (0.10)	−0.0621 (0.09)	0.0073 (0.35)	
$e^u_{i,t-3} * \widehat{EB}_{i,t-3}$	0.0384 (0.03)	0.0379 (0.03)		
$e^u_{i,t-3} * \widehat{TB}_{i,t-3}$	−0.0208 (0.03)	−0.0421 (0.05)		
$e^a_{i,t-3,0} * \widehat{EB}_{i,t-3}$	0.004 (0.04)	0.0101 (0.04)		
$e^a_{i,t-3,0} * \widehat{TB}_{i,t-3}$	−0.0992 (0.10)	−0.0801 (0.10)		
GDP growth per capita		0.0038 (0.01)	−0.0055 (0.02)	0.0000 (0.00)
unempl. rate (yearly variation)		0.0020* (0.00)	0.0074* (0.00)	0.0000 (0.00)
inflation		0.003 (0.00)	0.0101 (0.01)	0.0000 (0.00)
years from gov. appointment	0.0701*** (0.01)	0.0663*** (0.01)		
coalition cabinet dummy		0.0397* (0.02)	0.0498 (0.08)	0.0000 (0.00)
right wing cabinet dummy		0.0003 (0.05)	0.0406 (0.25)	
Observations	527	517	175	87

Note: Marginal effects reported. Standard errors in parentheses. ***p <0.01, **p <0.05, *p <0.1.

—−1
—0
—1

TABLE 12.11. Change in ideology, cumulative new plans

	(1)	*(2)*	*(3)*	*(4)*
	Sum of plans		*Ideology*	
	Govt terminations	*Regular elections*	*Govt terminations*	*Regular elections*
Exp. based new plans	0.0006	−0.0183		
	−0.026	−0.04		
Tax based new plans	0.0111	−0.1322		
	−0.044	−0.09		
unempl. rate (avg. of mandate yearly var.)	0.0101**	0.0137	0.0099*	0.0135
	−0.005	−0.008	−0.005	−0.009
GDP growth per capita (avg. of mandate)	−0.0374	−0.0189	−0.0374	−0.0155
	−0.028	−0.047	−0.028	−0.047
inflation rate (avg. of mandate)	0.0167	−0.0076	0.0171	−0.0065
	−0.012	−0.022	−0.012	−0.022
coalition cabinet dummy	0.0437	0.0899	0.0502	0.0867
	−0.078	−0.111	−0.078	−0.111
right wing cabinet dummy	0.0879		0.0995	
	−0.21		−0.217	
Exp. based new plans (mandate avg.)			−0.0031	0.003
			−0.047	−0.137
Tax based new plans (mandate avg.)			0.0884	−0.4448
			−0.103	−0.308
Observations	175	87	175	87

Note: Marginal effects reported. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

−1
0
1

The second specification (Table 12.10) frames the event of a change in the ideology of the government as a function of the measures (both expected and unexpected) that have been implemented only in the current year or the three previous years. Here, each shock is presumed to have a different effect, not in light of the total direction of the plan it is part of (EB or TB plans), but of the total direction of the measures implemented in the same year. For example, an unexpected current shock in expenditures in a plan in which there are announced future larger shock in taxes would appear as a TB shock in the previous specification, but as an EB shock in this specification.

Therefore, we can think of this case in two ways: as a robustness check of the previous, not robust, results as to the definition of shocks; and as a test of voters’ reaction to a more plausible set of scenarios. That is, voters are more likely informed about present and past shocks than about announcements for the future. Moreover, they are more likely to perceive the quality of the shock according to the overall quality of the present shocks, which are more salient than future ones.

As a result of this different classification of shocks, the seemingly significant findings of column 1 are quite different from those of the previous case, showing that TB shocks predict a government’s higher probability of being replaced while earlier EB shocks do not. We have earlier had seen a positive association between contractionary shocks coming from a mainly EB plan and the probability of being replaced. But as before, this second result is due to a few observations: in particular, it relies only on Italy in 1995 and vanishes if we exclude it from the sample. The two negative coefficients on mainly EB measures taken 2 years before also lose significance as soon as we drop Italy or Belgium, while the positive coefficient on the expenditure side, unexpected measures taken the year before, reverts to statistical zero after dropping Germany. The same story can be told for the robustness of the results reported in the third column, restricting the analysis to only those years in which a government change actually happened. There, only 2-year-old unexpected EB shocks seem to predict a lower probability of the government being replaced with an ideologically different one.

While the message of the first two specifications is that no robust relationship can be found between fiscal adjustments and government change, a third more synthetic and intuitive specification (Table 12.11) confirms this. Now the sample is the list of all 16 countries' governments since 1981, with the exclusion of preunitary Germany and post-2010 United States. The outcome variable is a dummy that is 1 if the government of a country is followed by one with a different ideology and otherwise 0. The economic controls include unemployment variation, GDP per capita, and inflation, all measured as the average of the mandate of the government. The political controls capture whether the government is a coalition or not, and whether it is right wing or not. The explanatory variable is either the sum of new adjustment plans that have been decided by the government or the per-year-of-mandate average size of new plans decided by it. Both alternative explanatory variables, sum or average, are duplicated according to the overall composition of the cumulative new adjustment plans: if in its mandate the government has implemented or announced a total of spending cuts larger than tax increases, then the adjustment is classified as expenditure based.

Table 12.11 demonstrates that governments that undertake either TB or EB adjustment plans do not seem to be replaced by an adverse party more frequently than those who do not do this. This is also true when we restrict the analysis to governments that end at the time scheduled, as in column 4 (i.e., excluding early elections).

Note that the controls used in the regressions for estimating the probability of a new plan (Tables 12.7 and 12.8) are different from those explaining the probability of a turnover (Table 12.9 and 12.10) and also from those explaining the turnover as a function of the overall government performance (Table 12.11). This is because the controls used in the explanation of turnover (per capita GDP growth, yearly variation of unemployment rate, inflation rate) are believed to be endogenous to fiscal plans (given that we show estimates of the effects of plans on GDP in other chapters of this book). Therefore, they cannot be used to explain the probability of a plan taking place. Because the selection criterion of exogenous fiscal plans often has made us record deficit-driven consolidations, we include the lagged deficit as an explanatory variable of fiscal plans. Moreover, it can be shown that lagged GDP growth predicts the event of a plan, although not its size: therefore we include it. On the other hand, lagged deficit and GDP growth are not included in the explanation of the probability of turnover because the latter is strongly correlated with current GDP, included as a control, and the former with the lagged values of the fiscal shocks. For consistency, the final specification (i.e., the change in ideology as a function of the overall government performance during its tenure) requires using the same controls as the previous specification, averaged by the years the government has been in charge. In any event, our results are very robust to these alternative specifications and controls.

In summary, fiscal adjustments do not systematically predict replacement of governments with their rivals.