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Revenue- versus spending-based consolidation plans: The role of follow-up

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Revenue- versus spending-based
consolidation plans:
the role of follow-up

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Abstract

The literature on fiscal multipliers finds that spending-based fiscal consolidations tend to have more benign macro-economic consequences than revenue-based consolidations. By directly comparing *ex-post* data with consolidation plans, we present evidence of a systematically weaker follow-up of spending-based consolidation plans. Next, using a newly-developed dataset of consolidation *announcements*, panel VAR regressions confirm the weaker follow-up of spending-based plans and their more benign macro-economic effects compared to those of revenue-based plans. We disentangle the role of the difference in follow-up from that of the difference in the composition of revenue- and spending-based consolidations. While the latter channel, which works through the difference between revenue and spending multipliers, explains the largest fraction of the difference in economic trajectories, the difference in follow-up plays a non-negligible role as well.

Keywords: fiscal consolidation announcements, follow-up, fiscal multipliers, panel vector auto-regression, narrative identification

JEL classifications: E21, E62, H5

Non-technical summary

As a result of the global economic-financial crisis and the ensuing Eurozone debt crisis, many countries were forced to consolidate their public finances. These developments naturally prompted questions in both academic and policy circles. What are the macroeconomic effects of fiscal consolidations? Given that consolidation needs to take place, what is the “best” way to consolidate?

Researchers have investigated these questions using data on real-time narratively-identified consolidation plans. Such narrative identification of policy shocks is intended to limit the danger of selecting consolidation plans that are in fact endogenous responses to the business cycle. A rather robust result from studies using narrative identification is that revenue-based consolidations are more harmful for output dynamics than expenditure-based consolidations. The literature offers various explanations for this empirical result. One is based on monetary policy being more accommodative in the case of spending-based consolidations than in the case of revenue-based consolidations. A second explanation argues that, in comparison to revenue-based consolidation, spending-based consolidation has a positive effect on business confidence and private investment. A third explanation argues that the impact of persistent spending cuts on aggregate demand is mitigated by wealth effects.

This paper focuses on another channel of relevance for this aforementioned heterogeneity. The empirical evidence we present supports the hypothesis that the difference in macroeconomic performance following fiscal consolidation can at least partly be attributed to a better follow-up of revenue-based plans than of spending-based plans, thus resulting on average into larger negative effects on household disposable income under the former. Hence, a standard setting that features a central role for disposable income in individual expenditure decisions should in principle be able to rationalize at least part of the difference in economic performance associated with the two types of consolidation. The channel that we propose here naturally complements the role played by the composition of consolidation plans in terms of revenue and spending measures, combined with differences in the multipliers associated with these measures – a channel that has been highlighted before.

Our analysis proceeds in a number of steps. First we provide indicative evidence of differences in follow-up by directly comparing narratively-identified real-time consolidation measures from existing datasets with ex-post outcomes of fiscal contractions. We find a systematic shortfall of the latter relative to the narrative measures. Moreover, we show that this shortfall is systematically larger for spending-based measures. One-third to almost one-half of the difference in follow-up can be explained as an “automatic result” of over-optimistic output growth forecasts. Hence, we label this as “passive” non-follow-up. This contrasts with “active” non-follow-up, resulting from governments not fully implementing planned consolidation measures, and failing to do so more for spending reduction plans than revenue-raising plans. A possible explanation for a difference in active follow-up is that when it comes to actual implementation, political resistance to spending reductions is more likely to be prohibitive than in the case of revenue increases.

We proceed with a more in-depth analysis and, to do so, we first construct a new quarterly narrative dataset of fiscal consolidation announcements for thirteen European Union (EU) countries over the period 1978-2013. We try to assign these announcements as accurately as possible to the quarter in which this information becomes publicly available. The announcements are then entered as shocks into a quarterly panel vector auto-regression (VAR). A proper timing of announcement shocks allows us to take into account potential anticipatory actions by the private sector between the moment the plan becomes public information and its actual implementation. Our panel VAR confirms the existence of a difference in follow-up: announcements of revenue-based consolidation plan are on average followed by an imperfect, but substantially larger, follow-up in terms of an improved primary balance ratio of GDP than are announcements of spending-based consolidation plans. We also find that under the former, GDP, private consumption and consumer confidence decline significantly, while the long-term interest rate rises significantly. By contrast, after a spending-based consolidation announcement none of these variables react significantly.

However, these differences in macroeconomic responses may be caused not only by differences in follow-up, but also by differences in the composition of revenue and spending plans. Revenue-based plans tend to consist of combinations of revenue and spending measures, with the former dominating the latter. Vice versa, for spending-based plans. By combining the impulse responses to both types of announcement shocks, we can disentangle the effects of the difference in follow-up from those of the different

composition of the revenue- and spending-based consolidation plans in terms of their relative reliance on revenue versus spending measures. This way we are able to extract the multipliers for both revenues and spending. In line with the related literature, we find large and negative revenue multipliers and positive, but close to zero, spending multipliers. For a given trajectory of the primary balance, the relatively larger revenue content naturally leads to larger output contractions under revenue-based plans than under spending based plans. While this composition effect turns out to be the largest contributor to the difference in economic performance under the two types of consolidation plans, the contribution of the difference in follow-up turns out to be quantitatively relevant as well.

1. Introduction

What are the macroeconomic effects of fiscal consolidation plans? Do revenue-based consolidations affect the economy in a different way than spending-based consolidations and, if so, why? In recent years a number of papers (Alesina *et al.*, 2015a and 2015b, and Guajardo *et al.*, 2014) have tried to address these questions starting from the annual narrative dataset constructed by Devries *et al.* (2011). A robust result in the literature based on this dataset is that revenue-based consolidations are more harmful for output dynamics than expenditure-based consolidations. Different explanations have been put forward to explain this finding. The explanation by Guajardo *et al.* (2014) is based on monetary policy being accommodating in the case of spending-based consolidations. Alesina *et al.* (2015a, b) propose an explanation based on the positive effect of spending-based consolidations on business confidence and private investment. More recently, Alesina *et al.* (2017) confirm the heterogeneous effects of spending- versus revenue-based consolidation plans, while controlling for monetary policy. In particular, using a richer version of the narrative data of Devries *et al.* (2011), they show that revenue hikes result in larger output reductions than both cuts in government spending and transfers. Alesina *et al.* (2017) rationalize this heterogeneity in a new-Keynesian model with persistent fiscal shocks, where the impact of persistent spending cuts on aggregate demand is mitigated by wealth effects.¹

In this paper we provide evidence that differences in economic performance associated with expenditure-based versus revenue-based consolidation plans can at least partly be attributed to better follow up of revenue-based plans than of spending-based plans. Hence, a standard framework that assigns a role for disposable income in individual decisions could in principle explain at least part of the difference in economic performance associated with the two types of consolidation. This channel complements the role played by the different compositions of two types of consolidations in terms of revenues and spending measures combined with the differences in the revenue and spending multipliers - the channel that has been highlighted before.

In a first step we provide indicative evidence of differences in follow-up by *directly* comparing *ex-post* actual data from the OECD with the annual narratively-identified real-time consolidation measures of Devries *et al.* (2011) and Alesina *et al.* (2015a,b). We do this by very carefully matching the narrative measures with the appropriate variables for the *ex-post* outcomes. There is a systematic shortfall of the latter relative to the narrative measures. However, we also find that the

1 Their narrative dataset consists of permanent measures with a planning horizon roughly equal across revenue- and spending-based adjustments; governments usually front-load cuts in spending and implement revenues hikes by means of more gradual adjustments.

shortfall is systematically larger for spending-based measures. We offer two (potentially complementary) explanations for the weaker follow-up of spending plans. The first is what we refer to as “passive” non-follow-up and results from over-optimistic output growth forecasts. For standard estimates of elasticities and over-optimism in growth forecasts, this can explain one-third to almost one-half of the difference in follow-up. The second is what we refer to as “active” non-follow-up. It is the result of partially implementing planned consolidation measures. We can rationalize the lower degree of active follow-up exhibited by spending plans in a simple setting where the political resistance to consolidation plans is uncertain, but more likely to be prohibitive for spending than for revenue plans when it comes to actual implementation. Data on general strikes in Western Europe do indeed suggest that announcements of spending cuts are more frequently followed by socio-political unrest than announcements of revenue increases.

The indicative findings above motivate a deeper empirical analysis into the differential effects of spending-based versus revenue-based consolidation plans. To this end, we construct a new quarterly narrative dataset of fiscal consolidation *announcements* for thirteen European Union (EU) countries over the period 1978-2013. The dataset is based on assigning consolidation information as accurately as possible to the quarter in which it becomes publicly available. We then enter the announcements as shocks into a quarterly panel vector auto-regression (VAR). By using properly-timed announcement shocks we can account for potential private sector anticipation effects that may take place between the moment the plan becomes public information and its actual implementation. In other words, we can model the response of the economy to real-time news on planned consolidations. Existing datasets based on the narrative identification of consolidation plans largely fail to account for the combined effect of legislative and implementation lags in fiscal policy, which can take several years. For example, the annual dataset of Devries *et al.* (2011) assigns consolidation measures to the year when they are supposed to be implemented. Alesina *et al.* (2015a,b) distinguish between unanticipated and anticipated measures to improve inference. For instance, the measures implemented in a given year are classified as anticipated if they had been announced in the preceding fall as part of a multiannual consolidation plan. However, the authors do not identify the moment of the consolidation announcement, which is critical to account for potential anticipation effects.

Our panel VAR shows that announcements of revenue-based versus spending-based consolidations produce very different economic responses. Following a revenue-based announcement, GDP, private consumption and consumer confidence decline significantly, while the long-term interest rate rises significantly. By contrast, after a spending-based consolidation announcement none of these

variables reacts significantly. These findings are robust to a number of alterations of the baseline specification.

The estimates confirm the difference in follow-up: revenue-based consolidation announcements are on average followed by an imperfect, but substantially larger, follow-up in terms of an improved primary balance ratio of GDP than are spending-based consolidation announcements. The impulse responses allow us to disentangle and quantify the effects of the *difference in follow-up* from those of the *different composition* of the revenue- and spending-based consolidation plans in terms of their relative reliance on revenue versus spending measures. Even though both types of consolidation plans tend to simultaneously resort to revenue and spending measures, by combining the impulse responses to the two types of announcement shocks, we are able to extract the multipliers for both revenues and spending. In line with the relevant literature, we find large and negative revenue multipliers and positive, but close to zero, spending multipliers. For a given trajectory of the primary balance, the relatively larger revenue content naturally leads to larger output contractions under revenue-based plans than under spending based plans. While the composition effect turns out to be the largest contributor to the difference in economic performance, the contribution of the difference in follow-up between the two plan types is found to be quantitatively relevant as well.

The remainder of this paper is structured as follows. Section 2 provides a brief review of the relevant literature. Section 3 investigates the follow-up of the annual fiscal consolidation plans by direct comparison with *ex-post* data on revenues and spending, and discusses the roles of passive and active follow-up. Section 4 describes our newly-constructed quarterly dataset of fiscal consolidation announcements. Section 5 presents the results of our panel VAR analysis. Finally, Section 6 concludes. Appendices A and B provide further information on the macroeconomic data and the construction of the consolidation announcement data. Appendix C presents a simple framework rationalizing the “active” non-follow-up with some indirect evidence supporting it. Appendix D reports the figures of our robustness tests.

2. Literature review

This paper relates to three main strands of literature. First, it connects to the literature on the differential effects of expenditure-based and revenue-based consolidations. Second, it relates to studies that explore the deviations, and their determinants, of actual budgetary measures from planned measures. Finally, it connects to studies that emphasize the role of expectations in the transmission of policy changes.

The Great Recession has motivated a large body of work estimating the sign and magnitude of fiscal multipliers. Empirical evidence generally shows that positive revenue shocks are contractionary (Blanchard and Perotti, 2002; Romer and Romer, 2010; Barro and Redlick, 2011; Favero and Giavazzi, 2012), with output multipliers ranging between -0.5 and -5. Reductions in public wage expenditures lower disposable income directly, while reductions in non-wage public spending on goods and services lower disposable income by depressing the demand for private sector output and, hence, income generated in the private sector. These results are confirmed for narratively-identified consolidation measures: for a panel of OECD countries Guajardo *et al.* (2014) find that both the revenues and spending measures are associated with reductions in private consumption and GDP. However, there is evidence (e.g. Guajardo *et al.*, 2014, and Alesina *et al.*, 2015a and 2015b) that spending-based consolidations are more effective in reducing the public debt and are economically less harmful than revenue-based consolidations. The literature offers several arguments for why this may be the case. One argument, advanced by Guajardo *et al.* (2014) for example, is that monetary policy tends to be more accommodative in the case of spending-based consolidations. A second argument is that, because they are politically more costly, resorting to spending-based consolidations provides a stronger signal by the government to the private sector that it intends to improve its financial situation (Ardagna, 2004).² Third, Alesina *et al.* (2017) emphasize that in the presence of highly persistent fiscal shocks, a standard New-Keynesian model can explain the weaker output effects of government spending cuts as compared to tax increases.

The second line of literature closely connected to this paper consists of empirical studies that document sizable and systematic deviations of actual implementation from fiscal plans. Examples are Jonung and Larch (2006), Beetsma *et al.* (2009), Von Hagen (2010), Pina and Neves (2011), Cimadomo (2012), Beetsma *et al.* (2013), De Castro *et al.* (2013) and Debrun and Kinda (2017).³ Using data from the EU's Stability and Convergence Programs, Beetsma *et al.* (2009) show that actual budgetary adjustment falls systematically short of planned adjustment, and that the shortfall increases with the projection horizon. Related analysis by Von Hagen (2010) indicates that the form of fiscal governance and the tightness of fiscal rules can help to explain these shortfalls. Pina and Neves (2011) employ EU Excessive Deficit Procedure reporting data to conclude that budget balance forecasting errors are responsive to fiscal institutions and opportunistic political motivations. A related conclusion is reached by Beetsma *et al.* (2013), who distinguish between systematic shortfalls in implementation during the first year after the presentation of the budget and any

² The argument is related to Cukierman and Tommasi (1988) who argue that political decisions that are at odds with the preferences of the natural constituency of a party are most credible.

³ Cimadomo (2012) shows that OECD countries often plan a counter-cyclical fiscal stance, while fiscal outcomes tend to point towards a-cyclicity or pro-cyclicity.

potential further revision errors. They find that institutional quality – as measured by the tightness of national fiscal rules, the medium-term budgetary framework or budgetary transparency – improves budgetary reporting at both the planning stage and one year later. De Castro *et al.* (2013) go even further and carefully explore how data revisions gradually develop as the time distance to the original fiscal plan increases. In line with the literature, they find that preliminary deficit data releases are downward biased, with later data vintages exhibiting larger deficits. Countries try to systematically exploit the margins of acceptable reporting, but are subsequently corrected by Eurostat. Frankel and Schreger (2013) find that over-optimism in forecasting budgetary improvement is particularly strong when the deficit exceeds the 3% GDP limit at the moment that the forecast is constructed. However, the over-optimism is weaker for Eurozone countries that exhibit more ownership of fiscal discipline at the national level. For a broad panel of narratively-identified consolidation episodes across countries, Gupta *et al.* (2017) show that promise gaps are on average sizable. Both economic and political factors contribute to the gaps.

The third strand of relevant literature is the growing body of work that explores the role of news for short-term economic dynamics. Here, the crucial assumption is that short-run output fluctuations can be driven by changes in the information set of agents. New information about future (economic) developments affects the expectations of private sector agents, who start to adjust their behavior in anticipation of the future state of the economy (Beaudry and Portier, 2014). Expectations of fiscal consolidations may either moderate or exacerbate the contractionary effect of the actual measures on the real economy. On the one hand, adherents to the “expansionary austerity” view claim that positive expectations effects can mitigate the contractionary effects of fiscal consolidations: if private agents realize that the current fiscal consolidation prevents a future increase in taxation, the adjustment spurs optimism about the future path of public expenditure and tax burdens (Blanchard, 1990, Giavazzi and Pagano, 1990, and Alesina and Ardagna, 2010). On the other hand, Akerlof and Shiller (2009) posit the existence of a “confidence multiplier”, which may amplify the Keynesian effects of fiscal policy. This hypothesis is investigated in a recent study by Bachmann and Sims (2012), who find that during recessions in the United States the “confidence multiplier” reinforces the Keynesian effects of increases in government spending. Additionally, Ramey (2011) and Mertens and Ravn (2012) show that anticipation effects can play an important role in the identification of structural fiscal shocks and that the incorporation of narrative shocks in the empirical analysis produces results different from those based on standard techniques. Our dataset of fiscal consolidation announcements is particularly suited to addressing such expectational effects. From a methodological viewpoint, our work is a study on the link between news and short-term economic dynamics that uses explicitly identified shocks (such as, for instance, in Brückner and Pappa, 2015).

Thus, our work falls within the empirical literature on narratively-identified fiscal VAR models where our exogenous instrument consists of announcements of future fiscal austerity measures.

3. *Ex-post* deviations from real-time fiscal consolidation measures

This section explores to which extent *ex-post* measured fiscal changes compare to the real-time fiscal consolidation measures identified by Devries *et al.* (2011) and expanded by Alesina *et al.* (2015a,b). The dataset covers thirteen EU countries. For Austria, Belgium, Denmark, France, Germany, Ireland, Italy, Portugal, Spain and the United Kingdom the sample spans the period 1978 – 2013, whereas for Finland, Sweden and the Netherlands it covers the period 1978 – 2008. Indications of a systematic difference between the follow-up of real-time revenue and spending measures motivate the fully-fledged analysis in the ensuing sections.

3.1. Matching of *ex-post* data with the narrative consolidation data

The annual fiscal consolidation measures in the dataset of Devries *et al.* (2011) are narratively selected from policy documents such that their primary motivation is public finance sustainability and not a response to the business cycle. The identified measures together with their estimated budgetary impact reflect the “intentions and actions” of policymakers as described in the policy documents.⁴ Alesina *et al.* (2015a,b) distinguish between anticipated and unanticipated implementations and, in extending the dataset for the period 2009-2013, they follow the same approach as Devries *et al.* (2011). An important source of information used in particular by Alesina *et al.* (2015a,b) are the Stability and Convergence Programs submitted by EU member states; these documents contain both forecasts of the effects of fiscal plans for the coming years and real-time estimates of the impact of the measures taken in the current and preceding years. Therefore, in both the narrative dataset of Devries *et al.* (2011) and in its extension, the intended magnitude of a fiscal consolidation represents a mixture of forecasts and first-release data.

We compare changes in actual (i.e., *ex-post*) public revenues and spending with the estimated budgetary impact of the narratively-identified consolidation measures by Devries *et al.* (2011) and Alesina *et al.* (2015a,b) for each year. The comparison is served best by matching as well as possible the concepts of revenues and spending used in the narrative identification with our data from the OECD on actual revenues and spending. The narratively-identified revenue measures include the following items found in the OECD data: “direct taxes”, “indirect taxes”, “social security contributions received by government”, “other current receipts by government” and “capital tax and

⁴ The recorded budgetary impact is the estimated change in budgetary savings accounted for by all the measures implemented in a given year.

transfers receipts”. This is more narrow than (a subset of) the most comprehensive measure from the OECD “Total receipts, government”. The narratively-identified spending measures include the following series from the OECD data: “Government final consumption expenditure, appropriation account”, “Government fixed capital formation, appropriation account”, “Social security benefits paid by the government”, “Capital transfers paid and other capital payments” and “Other current outlays, government”. This is more narrow than the most comprehensive measure from the OECD “Total disbursements, government”. To demonstrate the robustness of our findings, we will compare the narratively-identified consolidation measures with the changes in both the most comprehensive and narrower actual series. Appendix A contains a full description of the annual budgetary data used in this section.

3.2. A simple accounting framework

We employ a simple accounting framework for the comparison between *ex-post* and planned fiscal changes. The starting point is the following expression:

$$\left(\frac{X_t^f}{Y_t^f} - \frac{X_{t-1}^f}{Y_{t-1}^f} \right) - \left(\frac{X_t^h}{Y_t^h} - \frac{X_{t-1}^h}{Y_{t-1}^h} \right), \text{ for } X = T, G \quad (1)$$

where T is nominal government revenues and G is nominal government spending. Here, $\left(\frac{X_t^f}{Y_t^f} - \frac{X_{t-1}^f}{Y_{t-1}^f} \right)$ is the change in component X as a share of GDP calculated *ex-post* using the final data vintage of the OECD Economic Outlook, while $\left(\frac{X_t^h}{Y_t^h} - \frac{X_{t-1}^h}{Y_{t-1}^h} \right)$ is the amount of consolidation in component X as a share of GDP announced in period $h \leq t-1$, which is obtained from the IMF consolidation dataset. Because consolidations concern discretionary measures to revenues and spending, we also calculate the *ex-post* deviations of the cyclically-adjusted part of component X :

$$\left(\left(\frac{X_t^f}{Y_t^f} \right)^{CA} - \left(\frac{X_{t-1}^f}{Y_{t-1}^f} \right)^{CA} \right) - \left(\frac{X_t^h}{Y_t^h} - \frac{X_{t-1}^h}{Y_{t-1}^h} \right), \text{ for } X = T, G \quad (2)$$

where superscript “CA” indicates the cyclically-adjusted component, which we obtain directly from the OECD Economic Outlook. For the revenues component we use “Cyclically adjusted current receipts excluding interest, general government, as a percentage of potential GDP” and for the spending component we use “Cyclically adjusted government current disbursements excluding interest, as a percentage of potential GDP”. We observe that the measures for which we calculate the *ex-post* cyclically-adjusted changes have a narrower definition than the most comprehensive

OECD measures of revenues and spending. The former correspond to our measures of current receipts and current disbursements, for which we will also report the figures calculated in expression (1).

3.3. Results of the comparison

Table 1 reports the results for revenues averaged per country over the consolidation years and averaged over all consolidation observations. Table 2 does the same for expenditures. Because there is no obvious one-to-one correspondence between the revenue and spending measures we construct from the OECD data and the universe of consolidation plans in our dataset, we report a variety of alternative average *ex-post* deviations from the real-time consolidation measures identified by Devries *et al.* (2011) and Alesina *et al.* (2015a,b). However, the conclusions of the comparison of the shortfalls for revenues and spending are the same in all instances. For the most comprehensive measure of revenues, “Total receipts, excluding gross interest receipts, government” (which in all likelihood covers all the items contained in the real-time consolidation data), we observe that the average shortfall over all consolidations is 0.15% of GDP. For the other revenues measures, i.e. “Current receipts, excluding gross interest receipts, government”, its cyclically-adjusted version, and “Total revenues (narrow definition)”, the average short-falls are slightly larger (up to a maximum of roughly 0.18% of GDP). By contrast, the average shortfalls for spending are substantially larger than for revenues. For the most comprehensive measure, “Total disbursements, excluding gross interests payments, government”, the average figure is 0.50% of GDP. For the other measures, i.e. “Current disbursements, excluding gross interests payments, government”, its cyclically-adjusted version, and for “Total expenditure (narrow definition)”, the average deviations are even larger. While the average size of the spending measures (0.93% of GDP) exceeds the average size of the revenues measures (0.62% of GDP), we find that the average shortfalls for spending are proportionally substantially larger than justified by the average size of the spending measures relative to the revenues measures. Based on the latter, the average size of the spending shortfall would be $(0.93/0.62) \cdot 0.154 = 0.23\%$ of GDP, less than half of the baseline actual figure of 0.50% of GDP.

Looking at the individual countries, where we average over the consolidation years, we observe that for the most comprehensive revenues measure, only 5 out of 13 countries exhibit a short-fall. This contrasts with the most comprehensive spending measure, for which we find that 10 out of 13 countries exhibit a short-fall. For the other revenue and spending measures we register a higher fraction of short-falls, but the spending measure is always characterized by weaker follow-up than the corresponding revenues measure.

Table 1: Average of *ex-post* deviations for revenues

Country	D_TREV	D_CREV	D_CACREV	D_NREV
Austria	0.251 (9)	0.045 (6)	-0.001 (6)	0.293 (9)
Belgium	0.007 (13)	-0.044 (13)	0.025 (10)	-0.027 (13)
Denmark	0.456 (8)	0.492 (8)	-0.105 (4)	0.575 (8)
Finland	0.126 (3)	0.133 (3)	-0.331 (3)	0.180 (3)
France	0.123 (11)	0.089 (11)	-0.022 (10)	0.090 (11)
Germany	0.058 (13)	0.042 (13)	-0.000 (13)	0.046 (13)
Ireland	-1.818 (5)	-1.648 (5)	-1.552 (5)	-1.786 (5)
Italy	-0.446 (16)	-0.365 (16)	-0.372 (16)	-0.435 (16)
Netherlands	0.207 (9)	0.204 (9)	0.214 (7)	0.211 (9)
Portugal	-0.069 (10)	-0.103 (10)	-0.113 (9)	-0.280 (10)
Spain	-0.253 (12)	-0.251 (12)	-0.174 (10)	-0.366 (12)
Sweden	-0.381 (7)	-0.385 (7)	-0.686 (6)	-0.340 (7)
UK	0.369 (13)	0.370 (13)	0.072 (9)	0.304 (13)
<i>Average</i>	<i>-0.154 (108)</i>	<i>-0.146 (108)</i>	<i>-0.184 (108)</i>	<i>-0.173 (108)</i>

Notes: (i) a *negative* number means a short-fall of actual implementation from the announcement. (ii) Averages are calculated over all consolidation years per country or over all (country, consolidation year) combinations. (iii) D_TREV = deviations for “Total receipts, excluding gross interest receipts, government”, D_CREV = deviations for “Current receipts, excluding gross interest receipts, government”, D_CACREV = deviations for cyclically-adjusted “Current receipts, excluding gross interest receipts, government”, and D_NREV = deviations for “Total revenues (narrow definition)”. (iv) The number in brackets is the number of consolidation observations per country.

Table 2: Average *ex-post* deviations for expenditures

Country	D_TEXP	D_CEXP	D_CACEXP	D_NEXP
Austria	0.348 (10)	0.495 (10)	0.086 (7)	0.426 (10)
Belgium	0.438 (15)	0.683 (15)	0.763 (11)	0.588 (15)
Denmark	-0.048 (6)	0.228 (6)	0.307 (3)	0.202 (6)
Finland	1.549 (6)	1.684(6)	1.621 (6)	1.715 (6)
France	0.756 (9)	0.879 (9)	0.792 (9)	0.768 (9)
Germany	0.204 (13)	0.138 (13)	0.277 (13)	0.147 (13)
Ireland	0.686 (5)	1.713 (5)	0.894 (5)	1.034 (5)
Italy	1.062 (15)	1.211 (15)	1.130 (15)	1.064 (15)
Netherlands	0.957 (11)	1.247(11)	0.663 (6)	0.982 (11)
Portugal	0.532 (10)	1.195 (10)	0.984 (9)	0.762 (10)
Spain	0.889 (13)	1.390 (13)	1.034 (12)	1.118 (13)
Sweden	-0.709 (7)	-0.472 (5)	0.368 (6)	0.204 (7)
UK	-0.302 (14)	0.022 (14)	-0.207 (10)	-0.087 (14)
<i>Average</i>	<i>0.501 (111)</i>	<i>0.768 (111)</i>	<i>0.684 (111)</i>	<i>0.645 (111)</i>

Notes: (i) a *positive* number means a short-fall of actual implementation from the announcement. (ii) D_TEXP = deviations for “Total disbursements, excluding gross interests payments, government”, D_CEXP = deviations for “Current disbursements, excluding gross interest payments, government”, D_CACEXP = deviations for cyclically-adjusted “Current disbursements, excluding gross interest payments, government”, and D_NEXP = deviations for “Total expenditure (narrow definition)”. (iii) Further, see the *Notes* to Table 1.

3.4. Results of the comparison

In the previous section we have documented that the follow-up of planned spending reductions is systematically weaker than the follow-up of planned revenue increases. In this section we explore plausible explanations that can account for at least part of this phenomenon.

“Passive” non-follow-up

A first explanation comes in the form of over-optimistic GDP growth forecasts at the time when consolidation measures are devised. For lack of a better name, because governments may be deliberately over-optimistic, we refer to this phenomenon as “passive” non-follow-up. Using a back of the envelope calculation, we show that systematically over-optimistic GDP growth forecasts account for a substantial fraction of the observed difference in follow-up between revenues and spending consolidation plans. The starting point is equation (1). Because we merely want to provide an order-of-magnitude of the role of over-optimism in GDP forecasts in this regard, we keep our set-up as simple as possible, and focus on the case of one-year ahead consolidation plans (hence, $h = t - 1$), while assuming that for a generic variable Z , $Z_{t-1}^{t-1} = Z_{t-1}^f$, which implies that now-cast estimates (i.e. estimates done for the current year) are equal to *ex-post* measures. Because forecasting inaccuracy increases with the horizon, the back-of-the-envelope numbers for the shortfalls that we calculate likely form a lower bound. Under these assumptions, the difference between the *ex-post* and planned change (1) reduces to $(X_t^f / Y_t^f) - (X_t^{t-1} / Y_t^{t-1})$, for $X = T, G$. In Beetsma *et al.* (2013) we show that this expression can be decomposed into a “growth effect” and a “denominator effect” according to the following formulation:⁵

$$(X_t^f / Y_t^f) - (X_t^{t-1} / Y_t^{t-1}) \approx \underbrace{\frac{X_{t-1}^f / Y_{t-1}^f}{(1+y_t^f)(1+y_t^{t-1})} (x_t^f - x_t^{t-1})}_{\text{growth effect}} - \underbrace{\frac{X_{t-1}^f / Y_{t-1}^f}{(1+y_t^f)(1+y_t^{t-1})} (y_t^f - y_t^{t-1})}_{\text{denominator effect}} \quad (3)$$

Here x_t^{t-1} is the planned growth in period $t - 1$ of nominal revenues (if $X = T$) or nominal expenditure (if $X = G$) for period t . Further, x_t^f is the corresponding *ex-post* growth rate over the same period. Finally, y_t^{t-1} is the projected nominal income growth rate in period $t - 1$ for period t

⁵ There is also a so-called “base effect” that is zero, however, under our assumptions and there is a residual effect that we ignore because it is of second-order importance – see Beetsma *et al.* (2013) for a discussion.

and y_t^f is the period t nominal income growth rate as measured *ex post*. Assuming that the elasticities ε_T and ε_G of revenues, respectively expenditures, with respect to output are constant, we have $x_t^f = \varepsilon_X y_t^f$ and $x_t^{f-1} = \varepsilon_X y_t^{f-1}$ (with $X = T, G$).

Frankel (2011) finds that the average optimism bias in output growth projections for EU countries is around 0.5%. Using the information in Table A.3 of Mourre *et al.* (2014), we are able to compute the average revenue and expenditure elasticities with respect to output of the thirteen EU countries in our sample. The resulting elasticities are $\varepsilon_T = 1.11$ and $\varepsilon_G = -0.16$. Finally, based on the *ex-post* measures available in the OECD Economic Outlook (November 2015), we know that the ratios of total revenues (narrow definition) and total expenditure (narrow definition) over GDP are $T_t^f / Y_t^f = 0.39$ and $G_t^f / Y_t^f = 0.39$, respectively. On the basis of this calibration, assuming an average *ex-post* nominal GDP growth y_t^f of 4.5% and (for consistency) an average nominal GDP growth forecast y_t^{f-1} of 5%, we can now calculate the average shortfall for both revenues and expenditure. In the case of revenues the shortfall is, in percent of GDP, $[0.39/((1+0.045)(1+0.050))][1.11*(-0.5)] \approx -0.20$ (growth effect) *minus* $[0.39/((1+0.045)(1+0.050))]*(-0.5) \approx -0.18$ (denominator effect), hence -0.02 percent of GDP. In other words, the *ex-post* revenue ratio of GDP is on average 0.02 percent lower than planned. In the case of expenditure the shortfall is, in percent of GDP, $[0.39/((1+0.045)(1+0.050))][(-0.16)*(-0.5)] \approx 0.03$ (growth effect) *minus* $[0.39/((1+0.045)(1+0.050))]*(-0.5) \approx -0.18$ (denominator effect), hence 0.21 percent of GDP. In other words, the *ex-post* spending ratio of GDP is on average 0.21 percent of GDP higher than planned. The above back-of-the-envelope calculations show that biases in the GDP growth forecasts lead to systematically larger shortfalls from plans for spending reductions than for revenue increases, which can explain a non-negligible fraction of the empirically-observed average difference between the shortfalls.

“Active” non-follow-up

Our second explanation concerns the “active” non-follow-up, which refers to the possibility that announced consolidation measures are only partially carried out. Appendix C rationalizes this phenomenon in a very simple two-stage framework, in which in the first stage the government sets up a consolidation plan that is communicated to the private sector and in the second stage decides whether or to what extent to actually carry out the plan. At the moment the consolidation plan is designed, the political costs of the measures are unclear, while closer to the actual implementation, there is a higher chance for spending reductions to be politically prohibitive than for revenues

increases. Under this assumption one can demonstrate that the average deviation of actual from planned consolidation measures is larger for spending than for revenues. This prediction is thus consistent with the empirical accounting evidence provided earlier in this section.

Appendix C provides indirect evidence for this assumption. In particular, using data from Hamann *et al.* (2013, 2016) for fifteen countries from the European Union plus Norway, we show that strikes associated with public spending cuts occur much more frequently than political unrest associated with revenue increases. To the extent that strikes form a proxy for the socio-political unrest created by the austerity measures, this provides indirect support for the mechanism laid out above. We find that disputes motivated by spending-cuts occur with a substantially higher frequency than disputes motivated by revenue increases: of the 159 disputes, 69 are spending-cut motivated (43 after excluding those in which the issue in dispute is pensions), while the number of strikes motivated by revenue increases is only 7. Excluding countries not present in our sample of consolidation plans, i.e. excluding Greece, Luxembourg and Norway, 85 strike episodes remain, of which 40 are motivated by spending cuts (23 upon exclusion of the pension-related disputes), 6 are motivated by revenue increases and 8 are motivated by both revenue increases and spending cuts.

Next, based on the narrative description both the strike and the consolidation, we are able to match twenty strikes to the consolidation plans in our dataset (one in Finland, one in France, one in Spain, one in the Netherlands, two in Portugal, four in Belgium and ten in Italy). Of these strikes, three were undertaken in response to revenue-based announcements (namely in Italy in September 2011, December 2011 and October 2013) and seventeen in response to spending-based announcements. Of course, it is possible that the larger number of protests against spending cuts is the result of proposals to cut spending occurring more frequently than proposals to raise revenues. Our narrative data do show that spending-based consolidation plans occur relatively more frequently than revenue-based plans, but not to the extent that strikes against spending cuts dominate strikes against revenue increases. Appendix C describes some further anecdotal evidence of protestors preferring revenue increases to spending cuts. Overall, our data suggest that plans to cut public spending are more likely to encounter public opposition than plans to raise revenues, thus providing indirect support for a potential role of “active” non-follow-up.

4. A new dataset of fiscal austerity announcements

In this section we discuss how we construct our novel dataset on *announcements* of fiscal austerity measures. Our dataset covers the thirteen EU countries mentioned earlier over the period 1978 - 2013. The announcements for the subsample period 1978-2008 are based on the narratively-

identified consolidation measures documented at the annual frequency in Devries *et al.* (2011), while the announcements for the subsample period 2009 - 2013 are based on the consolidation measures narratively identified by Alesina *et al.* (2015a,b) for ten out of the aforementioned thirteen EU countries over this period. We in turn expand the data on the announcements of narratively-identified consolidations with Finland, the Netherlands and Sweden for the period 2009 - 2013, as for this period these countries are not covered by Alesina *et al.* (2015a,b). The consolidation measures recorded in Devries *et al.* (2011) and Alesina *et al.* (2015a,b), and our expansion of the country sample for the period 2009-2013, are all identified from official contemporaneous government documents. The idea is that by carefully studying the motivation of each consolidation measure, one can single out the measures that are not intended as a response to macroeconomic fluctuations, but with the main goal of reducing the deficit and/or the debt level. Hence, the identified measures are in principle exogenous to the business cycle.

Alesina *et al.* (2015a,b) also convert the consolidation measures into “fiscal plans”. Total implementation in a given year is the sum of anticipated measures announced in previous years (and implemented in the current year) and unanticipated measures. If a measure is announced in the last quarter of the previous year or in the current year, it is considered to be unanticipated in the current calendar year. Measures approved earlier that are supposed to have an effect on the current year are coded as anticipated. A corresponding split is made for revenues and spending.

We map the narratively-identified annual consolidation measures into moments of announcements. In some instances, Devries *et al.* (2011) already provide the announcement dates, and in those cases we use these. In the other cases we work as follows. We start from the total implementation in a given year and try to find all the announcements behind this total – it may be the result of a number of measures announced at different points in time. For each measure, using official documents, we identify the month when it is first officially mentioned or proposed by the government. Appendix B provides further details and contains some examples.

We also try to quantify the magnitude of the measures. We do this by extracting, cross-checking and combining information from a variety of official documents, such as the OECD Economic Surveys, the OECD (2011, 2012) reports on restoring the public finances, national budgets, EU Stability and Convergence Plans, as well as from newspaper articles. The documents contain information on the projected effects of the various measures. By grouping the measures according to the date of their first official mention, we record the size of the announcement on that date as the sum of the budgetary effects of the various individual measures announced on that date. Concretely, the magnitude of the announcement on a given date is the sum of the marginal impacts on the primary

balance between now and six years ahead of the various new measures announced on that date. To give an example purely for the purpose of illustration, suppose two new measures are announced in September of year $t-1$. Measure 1 is expected to have a positive marginal effect of 0.5% of GDP on the primary balance from year t and on, while Measure 2 is expected to have a negative marginal effect on the primary balance of 0.2% of GDP from year $t+1$ and on. Then, the value of the announcement that we record for September of year $t-1$ is $0.5 - 0.2 = 0.3\%$ of GDP.

The resulting set of announcements constructed at the monthly frequency is aggregated to the quarterly frequency. The main reason for this conversion is that macro-economic and fiscal variables are (at best) only available at the quarterly frequency. In addition, this will mitigate potential anticipation effects because of information becoming available before the official consolidation announcement. It may be the case that a measure receives media attention before the first official announcement, for example, because information from discussions at the government level or in ministries is leaked to the press. However, pinpointing the first moments of media attention to such measures is virtually unfeasible given the coverage of the data in terms of countries and sample period. Moreover, initial discussions in the media generally provide only little information about the size and the composition of the measures. Nevertheless, extensive investigation in Beetsma *et al.* (2015) suggests that fiscal news recorded the way we do tends to be anticipated beforehand.⁶ By aggregating the monthly announcements to the quarterly frequency, we alleviate potential anticipation effects. To further pre-empt any potential anticipation effects further, we assign any announcement made in the first month of a quarter to the preceding quarter.⁷

It is worth mentioning that, owing to inaccuracies in the narrative data sources, the actual value assigned to an announcement can be a mix of *ex-ante* forecasts and real-time estimates of the impact of the measures on the primary balance.⁸ Hence, the assigned value to the announcement potentially measures the pure shock value of the consolidation plan with some error. Nevertheless, reporting a value has a substantial advantage over merely reporting a simple dummy for a fiscal

⁶ This was investigated by exploring the movements in consumer confidence around the official announcement dates. We observed that consumer confidence tends to move significantly already before the official announcement.

⁷ We find that our results are robust to assigning the announcement to the quarter in which it officially takes place (results are available upon request). Incidentally, note that Ramey (2011) also carries out an adjustment in the quarterly timing of the weekly defense shock. If the news occurs in the final two weeks of a quarter, it is assigned to the following quarter based on the assumption that it occurs too late to have a material effect on macroeconomic aggregates in the quarter in which it originates.

⁸ Most of the time, our sources (mainly the OECD Economic Surveys) provide an estimated impact of a plan at the moment of its announcement. However, there are instances when we do not have information about the estimated impact of a plan upon its announcement. In those cases, we use the impact as recorded by the EU's Stability and Convergence programs or IMF or OECD documents, some of which may have been issued after the consolidation started, thereby potentially providing a real-time assessment of the impact of a plan.

announcement. Despite potential concerns about measurement errors, using values implies that less information is thrown away and it allows us to exploit the possibility that larger consolidations elicit stronger responses than smaller consolidations. Moreover, it helps in more accurately classifying plans into whether they are revenue- or expenditure-based, namely not on the basis of the narrative description, but based on the relative estimated impact of the revenue versus the expenditure measures.

Summarizing, effectively our dataset extends the set of announcements used in Beetsma *et al.* (2015) with Finland, the Netherlands and Sweden for the additional years 2009 – 2013 and by assigning in most instances a value for the size of the announcement, instead of a simple dummy for the occurrence of the announcement.

Table 3 reports the magnitudes of the announced plans. Note that the figures refer to the annual size of the plans, while the plans themselves are dated to the quarter in which their announcement takes place, as described above. In total we have 211 fiscal austerity announcements. For 180 of them we are also able to establish the magnitude of their impact on the primary balance. The cumulative annual impact of the measures on the primary balance ranges between 0% and 9.3% of GDP over a maximum period of 6 years, with an average value of 1.37% of GDP in our country sample.⁹ The horizon of the consolidation plans ranges between 1.3 and 2.3 years.

Most consolidation plans combine measures on both the revenue and the expenditure side of the budget, which is why in Table 4 we classify plans as predominantly revenue- or expenditure-based using a 50% threshold. That is, if more than 50% of the total announced budgetary impact comes from the expenditure side, the plan is classified as “spending-based”, while if more than 50% comes from the revenue side, it is classified as “revenue-based”. The 5 cases in which the division between spending and revenue measures is equal will be dropped from the sample, whenever we study the two subsamples of spending- and revenue-based plans separately.

As Table 4 shows, the majority of the announcements in our sample are spending-based. In the group of expenditure-based announcements, the average announcement has a size of 1.42% of GDP, with an impact of 1.14% of GDP on the spending side and 0.28% on the revenue side. In the group of revenue-based announcements, the average announcement has a value of 1.26% of GDP, with an impact of 0.31% of GDP on the spending side and 0.95% of GDP on the revenue side.

⁹ The largest consolidations were announced for Ireland 2010:Q4 (9.3% of GDP), Sweden 1994:Q3 (8.4% of GDP) and Portugal 2011:Q3 (6.1% of GDP). Excluding these three consolidations, the average announcement has a value of 1.26% of GDP. For the average announcement, the cumulative impact of the revenue measures is 0.47% of GDP and that of the expenditure measures is 0.78% of GDP.

Table 3: Summary statistics of fiscal announcement data

Country	Number of consolidation plans	Average annual size - all measures	Average annual size – spending measures	Average annual size – revenue measures	Average horizon (years) of consolidation plans
Austria	7	1.98	1.21	0.77	2.3
Belgium	18	1.14	0.68	0.46	1.5
Denmark	6	1.35	0.85	0.50	1.5
Finland	10	1.47	1.37	0.10	1.6
France	15	0.87	0.44	0.43	1.8
Germany	16	0.92	0.56	0.36	1.7
Ireland	15	2.05	1.10	0.95	1.3
Italy	25	1.31	0.74	0.57	2.0
Netherlands	22	1.17	0.99	0.18	1.3
Portugal	10	2.09	1.19	0.90	1.8
Spain	19	1.57	0.91	0.66	1.7
Sweden	5	2.38	1.57	0.80	2.0
UK	12	0.79	0.41	0.39	2.3
<i>Total</i>	<i>180</i>	<i>1.37</i>	<i>0.85</i>	<i>0.51</i>	<i>1.7</i>

Table 4: Plans according to the predominance of their instruments

Country	Spending-based	Revenue-based	Equal	Total
Austria	5	2	0	7
Belgium	8	8	2	18
Denmark	2	3	1	6
Finland	8	2	0	10
France	10	5	0	15
Germany	10	6	0	16
Ireland	8	6	1	15
Italy	15	9	1	25
Netherlands	19	3	0	22
Portugal	5	5	0	10
Spain	11	8	0	19
Sweden	5	0	0	5
UK	8	4	0	12
<i>Total</i>	<i>114</i>	<i>61</i>	<i>5</i>	<i>180</i>

5. The panel vector auto regression (VAR) analysis

In Section 3 we have documented that follow-up is weaker for spending-based than for revenue-based consolidation plans. This section follows up on Section 3 by investigating the different macro-economic consequences of revenue-based and spending-based consolidation plans in a panel vector auto regression (VAR) where the announcements of the plans are introduced as shocks. Using this empirical model we will first confirm that more follow-up results from revenue-based consolidation announcements than from spending-based announcements. We also show that the former have much more adverse consequences for the economy than the latter. Most importantly, we disentangle the role of differences in follow-up and of differences in the composition of the revenue and spending plans for the divergence in the trajectories of the economies.

The advantage of combining our new dataset with the proposed model is that this allows us to take proper account of potential fiscal anticipation effects on the side of the private sector. A general complication with the empirical analysis of budgetary shocks is that real variables, such as private consumption, already adjust in anticipation before the actual execution of the plans. Not taking account of such anticipatory behavior may lead to bias (see Leeper *et al.*, 2013, for details). In contrast to many other datasets, our dating of consolidation announcements enables us to pinpoint with a higher degree of precision than before when new information about consolidation activity is released and, hence, allows us to take explicit account of the potential anticipation effects. This also allows us to explicitly explore the role of private sector confidence, as captured by the consumer confidence indicator and the long-term interest rate, which may react immediately to announcements and which many commentators believe to play an important role in the transmission of fiscal consolidations.

5.1. The empirical specification

We estimate a quarterly panel VAR model of the standard form:

$$Z_{i,t} = \sum_{l=1}^L A_l Z_{i,t-l} + U_{i,t},$$

where i indicates the country and t the period (expressed as year-quarter), $Z_{i,t}$ is a vector of endogenous variables, and $U_{i,t}$ is a vector of zero-mean, stationary reduced-form disturbances. L represents the number of lags included in the panel VAR and A_l is the matrix of coefficients associated with the l^{th} lag. From the formula, we suppress any exogenous explanatory variables that we assume under the baseline. The baseline specification features the following vector of endogenous variables:

$$Z_{i,t} = [F_{i,t}, \tau_{i,t}, g_{i,t}, y_{i,t}, c_{i,t}, LTI_{i,t}, CCONF_{i,t}]'.$$

Here, $F_{i,t}$ is the fiscal consolidation announcement, $\tau_{i,t}$ is revenues as a share of GDP, $g_{i,t}$ is government expenditure as a share of GDP, $y_{i,t}$ is real GDP, $c_{i,t}$ is real private consumption, $LTI_{i,t}$ is the long-term interest rate and $CCONF_{i,t}$ is consumer confidence. All macroeconomic variables are expressed in real terms and deflated using the GDP deflator.¹⁰ With the exception of the long-term interest rate and the government revenue and expenditure ratios, all series are expressed in logarithms and multiplied by 100, so that the impulse responses can be interpreted as percentage deviations from their original values. In the case of the government revenue and expenditure ratios, the impulse responses represent percentage point deviations from their original values, while in the

¹⁰ Appendix A describes the quarterly budgetary and macroeconomic variables we use in this section.

case of interest rates, they represent deviations in basis points from their original values. The deterministic components included in the baseline are seasonal dummies, country-fixed effects and country-specific linear trends. Importantly, because we are assessing the follow-up of consolidation announcements in terms of actual measures, the definitions of revenues and expenditures should correspond as closely as possible to the potential sets of measures included in the revenues respectively expenditure components of the fiscal consolidation. This implies in particular that $g_{i,t}$ will include transfers, and hence that $g_{i,t}$ is more broadly defined than merely government purchases.

We opt for a baseline specification containing four lags of the endogenous variables, hence amounting to a maximum lag length of one year. As shown below, the main results of the paper are robust to different choices of the lag structure and other configurations of the deterministic components.

5.2. Identification and other estimation issues

Empirical identification of fiscal policy shocks may be hampered by anticipation effects: the private sector learns about a policy change and responds to it before it is actually implemented. The legislative lag is the period between the official announcement of the policy measure and its legal implementation. Because the official announcement often coincides with the presentation of the new budget, we expect the legislative lag on average to be short. The implementation lag concerns the time between signing the relevant legislation and the moment when the new legislation comes into force. The sum of the two lags together can range from a couple of months to some years from the official announcement of a policy measure (Leeper *et al.*, 2013). In addition, media coverage of a new policy measure generally predates its official announcement. By looking at military spending, Ramey (2011) finds that news reports about war dates Granger-cause increases in defense spending, thus providing evidence of the anticipation of government spending shocks. If anticipated changes in revenues and public spending prompt economic agents to respond before the fiscal measures are actually implemented, the innovations identified in a structural VAR do not correspond to the true timing of the shocks. Formally, the moving-average representation of the VAR system is not invertible (Leeper *et al.*, 2013), leading to biased estimates. Moreover, according to Guajardo *et al.* (2014), anticipation effects could be an important explanation for the generally different findings of narrative and structural VARs.

Existing datasets based on narrative identification, such as Devries *et al.* (2011), tend to assign the impact of austerity measures to the years of implementation in accordance with the plan. Alesina *et*

al. (2015a, b) try to distinguish between the implementation of anticipated and unanticipated measures. However, news about anticipated measures has generally been released earlier. The same is usually the case for unanticipated measures, because these measures are mostly announced as part of the new budget prepared in the year preceding the year for which the unanticipated measure is reported. By timing austerity measures to the moment of their announcement, we take account of the potential legislative and implementation lags. As mentioned earlier, we also try to handle anticipation effects associated with earlier media coverage by assigning official announcements in the first month of a quarter to the preceding quarter.

De Cos and Moral-Benito (2013) and Jordà and Taylor (2016) find that the narrative shocks of Devries *et al.* (2011) can be predicted using a range of economic variables. Hence, it is conceivable that our fiscal consolidation announcements represent responses to past economic and financial conditions.¹¹ We therefore estimate our panel VAR model using a Cholesky decomposition with the fiscal consolidation announcements ordered first, allowing the austerity news to be predicted only by lags (of at least one quarter) of the economic and financial variables in the VAR. In doing so, the VAR equation corresponding to the fiscal consolidation announcement could be interpreted as a “policy announcement reaction function”, with its residuals representing the discretionary fiscal consolidation news.¹² The ordering of the other variables in the VAR has no bearing on the results.¹³

5.3. Baseline estimates

We estimate the panel VAR model at the quarterly frequency by means of ordinary least squares (OLS) on our sample of 13 European Union countries over the period 1978:Q1-2013:Q4.

Figure 1 reports the baseline responses when all consolidation announcements are included. Here, and in the sequel, the shock, which takes place at moment 0, is a consolidation announcement normalized to 1% of GDP. We observe a highly significant rise in public revenues by around 0.15% of GDP after about a year and a fall in public expenditures of roughly the same magnitude that becomes significant after about half a year. Both responses depress disposable income, which may help to explain the fall in GDP, which reaches a maximum of around 0.2% and gets close to

¹¹ In the spirit of De Cos and Moral-Benito (2013) and Jordà and Taylor (2016), we analyze the predictability of the consolidation announcements by means of logistic regressions and find that the announcement shocks are predicted by past debt dynamics and past values of long-term interest rates.

¹² We have also tried ordering the announcement variable last in the VAR. The impulse response are similar to those reported below with the announcement variable ordered first, which is consistent with the fact that the correlation coefficients between the residuals of the reduced-form equation for the consolidation announcement variable and the reduced-form residuals of the other variables in the VAR are very low.

¹³ As demonstrated in Christiano *et al.* (1999), for the Cholesky identification scheme under the recursion assumption, the impulse responses of the variables in the block following the announcement shock are invariant to the ordering of these variables *vis-à-vis* each other.

significance. Private consumption exhibits a significant maximum deterioration of around 0.4%, while the long-term interest rate exhibits a positive jump on impact and reaches a significant maximum of around 15 basis points, after which it converges back to its steady state within a period of 3 years. The long-term interest rate thus reacts immediately to the news. Finally, consumer confidence shows a significant maximum fall of around 1.5 percent.

Next, we split the news into announcements of revenue-based plans (Figure 2a) and spending-based plans (Figure 2b). For both types of plans, the shock is normalized to 1% of GDP. Clear differences show up between the two types of announcements. The announcement of a revenue-based plan produces a highly significant increase in revenues reaching a maximum of almost 0.6% of GDP, while GDP and private consumption exhibit significant reductions that reach maxima of around 1 and 1.5 percent, respectively. Public spending stays put, while the long-term interest rate rises by a maximum of about 40 basis points. Consumer confidence falls by a highly significant maximum of more than 5 percent. Note that both confidence indicators, the long-term interest rate and consumer confidence, even though they are forward-looking, reach their peak not immediately upon impact, but after half a year. By contrast, apart from public spending itself, none of the responses under spending-based announcements become significant.¹⁴ In particular, GDP and private consumption remain almost perfectly flat. Spending falls by a maximum of 0.20 – 0.25 percent. The fall is significant, but in magnitude it is less than half the rise in revenues over GDP under a revenue-based consolidation announcement, even though in both cases the announcement is normalized to one percent of GDP. Both the revenue and the spending ratios also take some time to return to their long-run level, although they do return faster to their equilibrium values than do real GDP and consumption.

5.4. Robustness of the baseline

We investigate the robustness of our baseline estimates in various ways. First, it could be argued that by including the recent crisis period in our time sample we capture an atypical period, during which the responses of economies to announcement shocks could differ from those in other periods. Therefore, Figures D.1a and D.1b in Appendix D report the impulse responses when we drop the period 2008 – 2013. However, these confirm the differences in the baseline responses for the two types of consolidation. Second, Figures D.2a and D.2b in Appendix D report the responses for the revenue- and spending-based plans when the (relevant) variables are expressed as shares of

¹⁴ For a panel based on a broad sample of advanced and emerging countries, Born *et al.* (2015) find that the effect of a cut in government consumption on the sovereign yield spread against a “riskless” reference country typically depends on the state of the economy.

potential output. The baseline results are both qualitatively and quantitatively essentially unchanged. Third, this is also the case when we replace the revenue and spending ratios of GDP by the logarithms of real revenues and real spending (Figures D.3a and D.3b in Appendix D). Fourth, Figures D.4a and D.4b in Appendix D report the responses when we restrict ourselves to revenues plans that contain at least 60% revenues measures and spending plans that contain at least 60% spending measures. Again, the responses are qualitatively and quantitatively very similar to those under the baseline. Fifth, this is also the case if we include a time dummy for each quarter in the sample – see Figures D.5a and D.5b in Appendix D – or allow for eight instead of four lags in the panel VAR – see Figures D.6a and D.6b in Appendix D. Sixth, following Favero and Giavazzi (2012) we include the lagged public debt as an exogenous variable in the baseline specification. This way we control for the fact that past debt dynamics may help to predict the announcement shocks. Again, the resulting impulse responses reported in Figures D.7a and D.7b in Appendix D are qualitatively and quantitatively essentially identical to those under the baseline. To check whether the baseline results are not driven by a specific country in our sample, our next robustness check drops one country at a time. Figures D.8a and D.8b in Appendix D show that the impulse responses are in all instances rather compactly clustered around the original responses, and in any case contained within the original 90% confidence intervals, thus suggesting that no individual country drives our main results.

Guajardo *et al.* (2014) suggest that the differences in impulse responses between revenue- and spending-based consolidations can be explained by monetary policy being more accommodative in the case of spending-based consolidations. However, over the largest part of the estimation period the majority of the countries in our sample had either a common currency or a stable exchange rate against the German mark. Hence, if we observe significant differences in the responses to revenue- and spending-based announcement consolidations, it is doubtful that these can be explained by differences in the monetary responses alone, because the ECB only responds to euro-area wide macroeconomic developments and not to those in individual countries. Likewise, in the period before EMU, the Bundesbank only responded to German developments and not to those in other countries pegging their exchange rate to the German mark. Nevertheless, to control for monetary policy, we replace the long-term interest rate with the short-term interest rate, which is closer to the central bank's policy instrument. However, Figures D.9a and D.9b in Appendix D show that the impulse responses are qualitatively and quantitatively essentially unaffected, except that the

response of the short-term interest rate to a revenue-based consolidation is insignificant and smaller than the baseline response of the long-term interest rate.¹⁵

An alternative to the current baseline would have been a specification with business confidence instead of consumer confidence. However, we have fewer observations on business confidence than on consumer confidence, and hence we would lose precision in comparison to our current baseline. Including business confidence alongside consumer confidence in the model would require estimation of even more parameters, and thus a further loss of degrees of freedom. Moreover, the question arises which indicator of confidence would be more important. *A priori*, to the extent that confidence affects the real economy, we expected consumer confidence to affect private consumption and business confidence to affect private investment. Private consumption is a substantially larger fraction of GDP; hence consumer confidence seems to be the more relevant variable to include in the baseline. Nevertheless, in this robustness check we replace the consumer confidence indicator with the business confidence indicator and private consumption with private investment. The results are depicted in Figures D.10a and D.10b in Appendix D. Compared to our original baseline we observe that in the case of a revenue-based consolidation, real GDP again exhibits a significant fall. However, the fall is substantially smaller than under the baseline. This suggests that the confidence channel may be relevant and that consumer confidence is more important in this regard than business confidence.

The long-term interest rate may be viewed as an indicator of financial market confidence. The baseline impulse responses show that, in response to announcements of revenue increases, the long-term interest rate rises and consumer confidence falls, while both variables stay put in response to announcements of spending reductions. Hence, as indicators of financial market and consumer confidence the two variables behave consistently *vis-à-vis* each other. However, we would ideally like to rule out the possibility that the behavior of the long-term interest rate is driven by factors other than confidence. In particular, consolidation announcements may affect the long-term interest rate through their effect on inflation expectations. *A priori*, if a consolidation announcement is not expected to stimulate the economy, we would also not expect it to raise the long-term interest rate by pushing up expected inflation.¹⁶ To confirm that the results are not driven by movements in inflation expectations, ideally one would include in the model the long-term interest rate purged of

¹⁵ The rise in the short-run interest rate following the announcement of a revenue-based consolidation is still not far from significant. However, it seems implausible that this rise is driven by a monetary tightening, because, if anything, we would expect monetary policy to become looser to avert the slow-down of the economy induced by the announcement. In any case, a counterfactual in which we force the short interest rate to stay constant does not affect the impulse responses.

¹⁶ A potential exception concerns a consolidation that takes place largely through an increase in indirect taxes, which has a direct, though temporary, positive effect on inflation.

expected inflation (over the lifetime of the long-term debt). However, while we have survey measures on expected inflation, these only indicate whether private agents expect inflation to go up or down. Hence, we lack a quantification of expected inflation from the surveys, so this route cannot be pursued. Therefore, we redo the baseline regressions by replacing the long-run interest rate with its difference with respect to realized CPI inflation, i.e. the *ex-post* long-run real interest rate. The impulse responses are found in Figures D.11a and D.11b in Appendix D. We observe that our baseline results are unaffected.

5.5. Follow-up versus differences in composition of consolidation plans

This subsection addresses the key questions of this paper: which channels explain the difference in economic performance following the announcement of revenue-based versus spending-based plans, and how important are these channels relative to each other? The estimates presented in the previous subsection suggest that follow-up under spending-based plans is weaker than under revenue-based plans. However, the relative importance of revenue and spending measures under the two types of plans is also different, while different budgetary instruments may feature different multipliers.

The responses are reported in Table 5, where the horizon h is expressed in quarters. As variables are forced to return to their baseline, we limit ourselves to a maximum horizon of 20 quarters. Follow-up after one year, as measured by the improvement in the primary balance, is more than 70 percent for revenue-based plans and only 25 percent for spending-based plans. Not surprisingly, for revenue-based plans, the largest fraction of the follow-up is an increase in revenues. Similarly, for spending-based plans the largest fraction of the follow-up is a reduction in spending. Besides differences in follow-up, we also observe differences in the cumulative primary balance multiplier, which for horizon h and revenue-based plans (henceforth indicated by subscript "r") we define as

$$m_{h,p,r} = \frac{\sum_{j=1}^h \hat{y}_{r,j}}{\sum_{j=1}^h pb_{r,j}}, \quad (4)$$

where $\hat{y}_{r,j}$ is the percent deviation of real output from its original value, hence $\sum_{j=1}^h \hat{y}_{r,j}$ is the cumulative percent deviation from the original value, $pb_{r,j}$ is the impulse response of the primary balance in percentage points of GDP for revenue-based plans after j quarters. The cumulative primary balance multiplier for spending-based plans (henceforth indicated by subscript "s") at horizon h , $m_{h,p,s}$, is defined analogously. In other words, the cumulative primary balance multiplier is the cumulative percent change of real output, divided by the cumulative improvement in the primary balance in percent of GDP. The cumulative primary balance multiplier is substantially larger

in absolute size for revenue-based than for spending-based consolidations, reaching a value of minus 3.6 after 5 years for the former, while for the latter it remains close to zero.

The revenues and spending multipliers cannot immediately be calculated, because generally revenue-based (spending-based) plans are partly composed of spending (revenue) measures. However, by combining the impulse responses to the two plan types, we can construct multipliers for each of the two sides of the public budget. To this end, we assume that the effect of a change in revenues (and similarly for spending) on output is the same for revenue-based and spending-based plans. Then, for horizon h of the response to a plan announcement in period 0 we end up with the following system that we can solve for the cumulative multipliers $m_{h,\tau}$ and $m_{h,g}$ for revenues, respectively spending:

$$\begin{aligned} m_{h,\tau} \sum_{j=1}^h \tau_{r,j} + m_{h,g} \sum_{j=1}^h g_{r,j} &= \sum_{j=1}^h \hat{y}_{r,j}, \\ m_{h,\tau} \sum_{j=1}^h \tau_{s,j} + m_{h,g} \sum_{j=1}^h g_{s,j} &= \sum_{j=1}^h \hat{y}_{s,j}, \end{aligned}$$

where $\sum_{j=1}^h \tau_{r,j}$ and $\sum_{j=1}^h g_{r,j}$ are the cumulative percentage point changes in revenue and spending over GDP. These are obtained directly from the impulse responses as reported in Table 5. The cumulative multiplier for revenues, also reported in Table 5, increases with the horizon h and reaches a maximum of 3.6 over the reported horizon, which is comparable in magnitude to the estimated effect of a 1 percent of GDP tax hike in Romer and Romer (2010). The cumulative multiplier for spending is actually negative,¹⁷ suggesting that a spending reduction benefits output, although the effect is only rather small.

The most important question for the purpose of this paper concerns the effect of differences in follow-up between revenue- and spending-based plans. Table 5 therefore also reports the output effects of a revenue-based consolidation if we counterfactually impose the same primary balance effects of a spending-based consolidation. Exploiting expression (4), we calculate the counterfactual cumulative output effect in percent (indicated by a tilde) under *revenue*-based plans for the counterfactual primary balance path of *spending*-based plans as:

$$\sum_{j=1}^h \widetilde{y}_{r,j} = m_{h,p,r} \sum_{j=1}^h p b_{s,j}.$$

Lagging this expression by one period, subtracting it from the original one, and rewriting, yields the counterfactual value of output growth after h quarters:

¹⁷ Corsetti *et al.* (2013) show that this may happen in the presence of default risk, when monetary policy is at the zero lower bound, hence unable to offset the impact of the default risk premium. However, for most of our sample monetary policy was not at the zero lower bound. An alternative explanation is based on the "expansionary austerity" view discussed in Section 2.

$$\widetilde{y}_{r,h} = m_{h,b,r} \sum_{j=1}^h pb_{s,j} - m_{h-1,b,r} \sum_{j=1}^{h-1} pb_{s,j}.$$

Not surprisingly, due to the counterfactual reduction in follow-up the fall in output shrinks. Output savings are particularly large shortly after the announcement shock has taken place. The fall in output after one year shrinks from 0.62 percent to 0.19 percent. The differences are rather small after two and three years, but widen again at longer horizons. Overall, we observe that the largest difference in the output trajectory between revenue- and spending-based plans is the result of the different plan compositions. Even so, differences in follow-up do play a non-negligible role.

Table 5: Cumulative multipliers at various horizons

Plan type	Description	Expression	$h=4$	$h=8$	$h=12$	$h=20$	Max (revenue), Min (spending, output)
Revenue-based	Output	\widehat{y}_h	-0.62	-1.04	-0.99	-0.69	-1.05
	Primary budget balance	$pb_h = \tau_h - g_h$	0.71	0.15	0.11	0.20	0.71
	Revenue ratio	τ_h	0.57	0.27	0.17	0.08	0.57
	Spending ratio	g_h	-0.14	0.12	0.06	-0.12	-0.14
	Average cumulative revenue ratio	$\sum_{j=1}^h \tau_j / j$	0.28	0.33	0.28	0.22	0.34
	Cumulative multiplier primary budget balance	$\sum_{j=1}^h \widehat{y}_j / \sum_{j=1}^h pb_j$	-1.00	-1.98	-3.06	-3.63	-3.63
Spending-based	Output	\widehat{y}_h	0.04	0.03	-0.02	-0.05	-0.05
	Primary budget balance	$pb_h = \tau_h - g_h$	0.25	0.22	0.14	0.06	0.28
	Revenue ratio	τ_h	0.02	0.01	0.00	-0.01	0.07
	Spending ratio	g_h	-0.23	-0.21	-0.14	-0.06	-0.23
	Average cumulative spending ratio	$\sum_{j=1}^h g_j / j$	-0.14	-0.18	-0.17	-0.14	-0.18
	Cumulative multiplier primary budget balance	$\sum_{j=1}^h \widehat{y}_j / \sum_{j=1}^h pb_j$	0.15	0.17	0.13	-0.01	0.18
Combining plan types	Cumulative multiplier revenues	$\sum_{j=1}^h \widehat{y}_j / \sum_{j=1}^h \tau_j$	-1.33	-1.92	-2.62	-3.64	-3.64
	Cumulative multiplier spending	$\sum_{j=1}^h \widehat{y}_j / \sum_{j=1}^h g_j$	-0.50	-0.55	-0.52	-0.35	-0.57
Counterfactual output of revenue-based consolidation announcements with primary balances of spending-based announcements		$\widetilde{y}_{r,h}$	-0.19	-0.90	-0.94	-0.19	-1.02

Notes: (i) The announcement shock always has a magnitude of 1 percent of GDP. (ii) horizon h is expressed in quarters. (iii) The final column reports the maximum or the minimum over the horizon of 20 quarters.

5.6. The role of the confidence channel

The impulse responses reported above show that, to the extent that movements in confidence are a reflection of the anticipated course of the economy, they are consistent with the latter for both types of consolidations: in the case of spending consolidations, neither financial market confidence as captured by the long-run interest rate nor consumer confidence are affected, in line with fact that

output and consumption turns out not to change. In the case of revenue consolidations, both financial market and consumer confidence deteriorate in line with the anticipated deterioration of the economy.

However, as documented in Section 2, it is sometimes argued that movements in confidence themselves constitute a separate transmission channel of the effects of consolidations. In contrast to other narrative fiscal datasets, our dataset of consolidation *announcements* offers a unique opportunity to explore the effects of consolidation plans on confidence. The reason is that our dataset allows us to detect the effect of unanticipated fiscal information on potential movements in confidence that happen at the moment the information is released. These movements, and their broader effect on the economy, would at most be partially accounted for in existing datasets that fail to record the new information when it actually becomes available.

To obtain some indication of the potential importance of the confidence channel we conduct a counterfactual in which we shut this channel off by fixing both the long-term interest rate and consumer confidence at their original values following the announcements – see Figures 3a and 3b. The deteriorations in real GDP and consumption following a revenue-based announcement are now substantially reduced. However, in view of the potential relevance of the Lucas critique, we take this finding only as indicative of the potential role of confidence in the transmission from consolidation announcements to the real economy.

6. Conclusions

Existing literature shows that narratively-identified spending-based consolidations have milder effects on the economy than revenue-based consolidations. This paper has focused on the role of differences in follow-up after announcements of revenue-based versus spending-based austerity measures. First, we provided “accounting evidence” that follow-up of consolidation plans is substantially larger for revenue increases than for spending cuts. Over-optimism in GDP growth forecasts can explain a non-negligible part of the difference. In addition, more uncertainty about the public’s acceptance of spending cut proposals could contribute further to explaining the higher likelihood that such proposals are not fully carried out.

We then constructed a narrative dataset on fiscal consolidation announcements which were entered as shocks into a panel VAR. The goal was to explore differences in the reaction of the macro-economy to the two types of consolidation announcements. In this way fiscal anticipation effects could be properly accounted for. The impulse responses confirmed the greater follow-up of revenue-based plans and showed that revenue-based consolidation announcements lead to a substantially

larger reduction in economic activity than spending-based consolidation announcements. We then went on to disentangle the respective roles of the differences in follow-up and the differences in composition of the two plan types. We found that, while the difference in the composition of the two plan types was the main contributor to the difference in economic performance (owing to substantially different multipliers for revenues than for spending), differences in follow-up were able to explain a substantial fraction as well.

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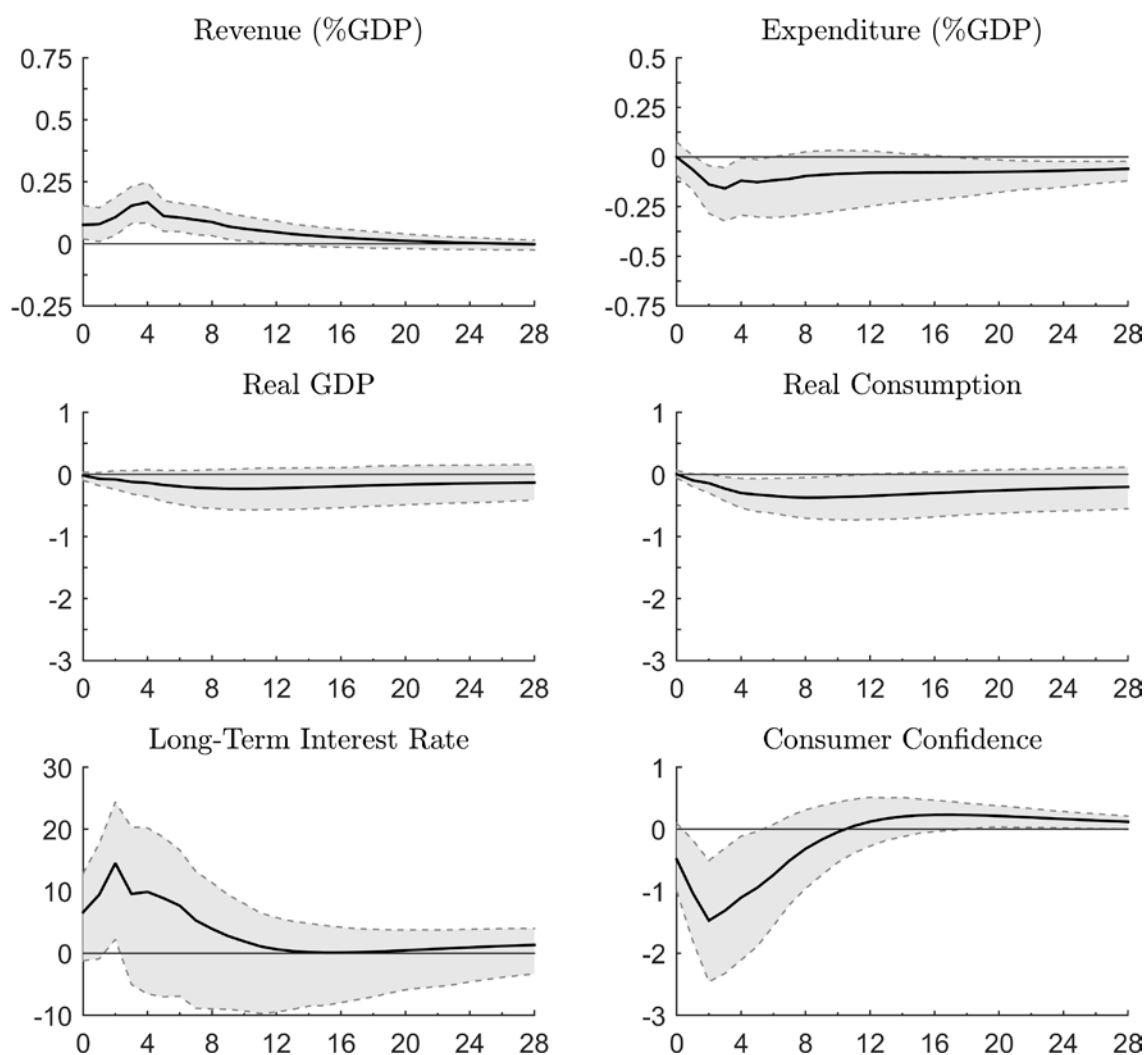
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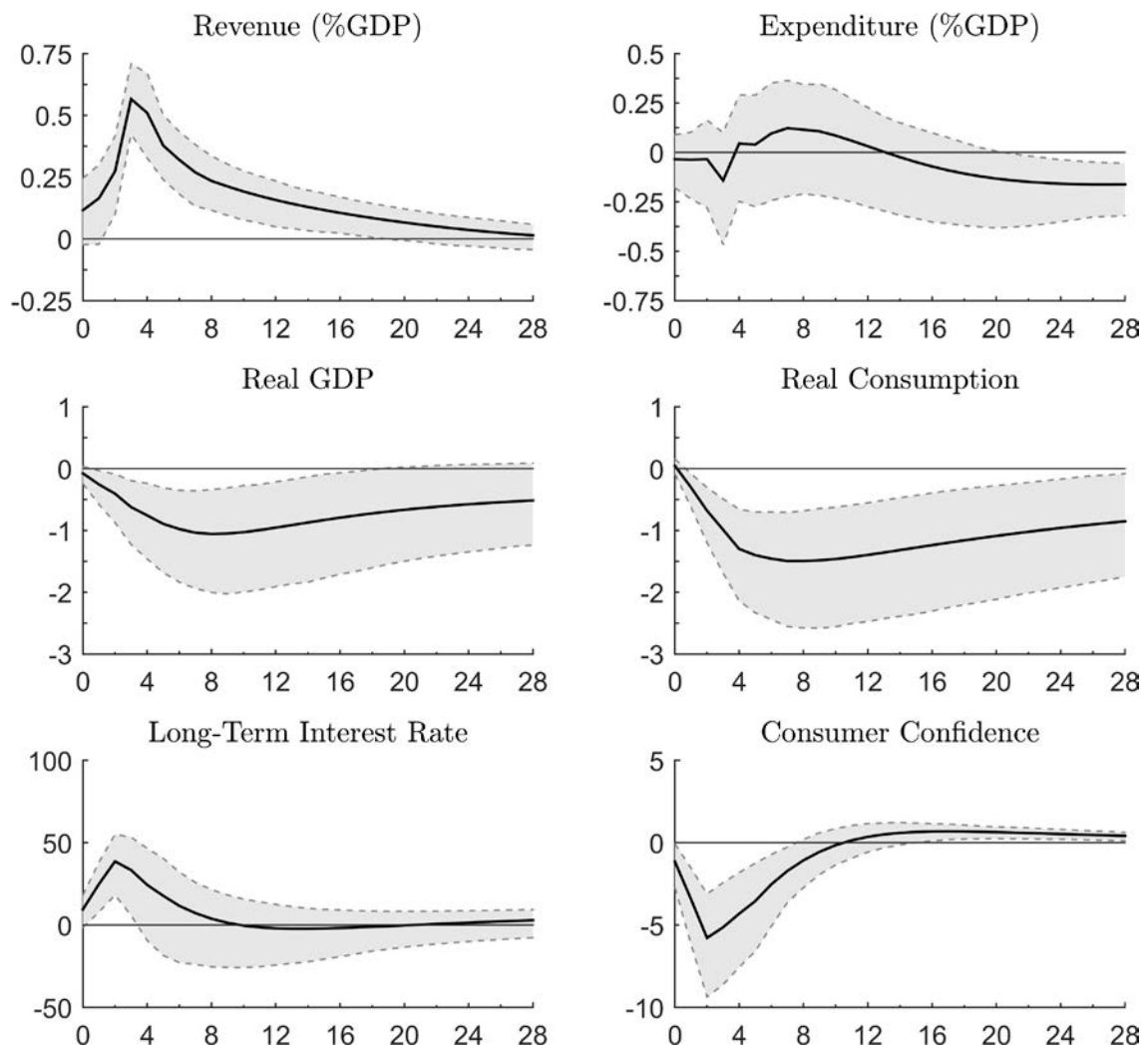
Figures:

Figure 1: Impulse responses baseline model – all announcements



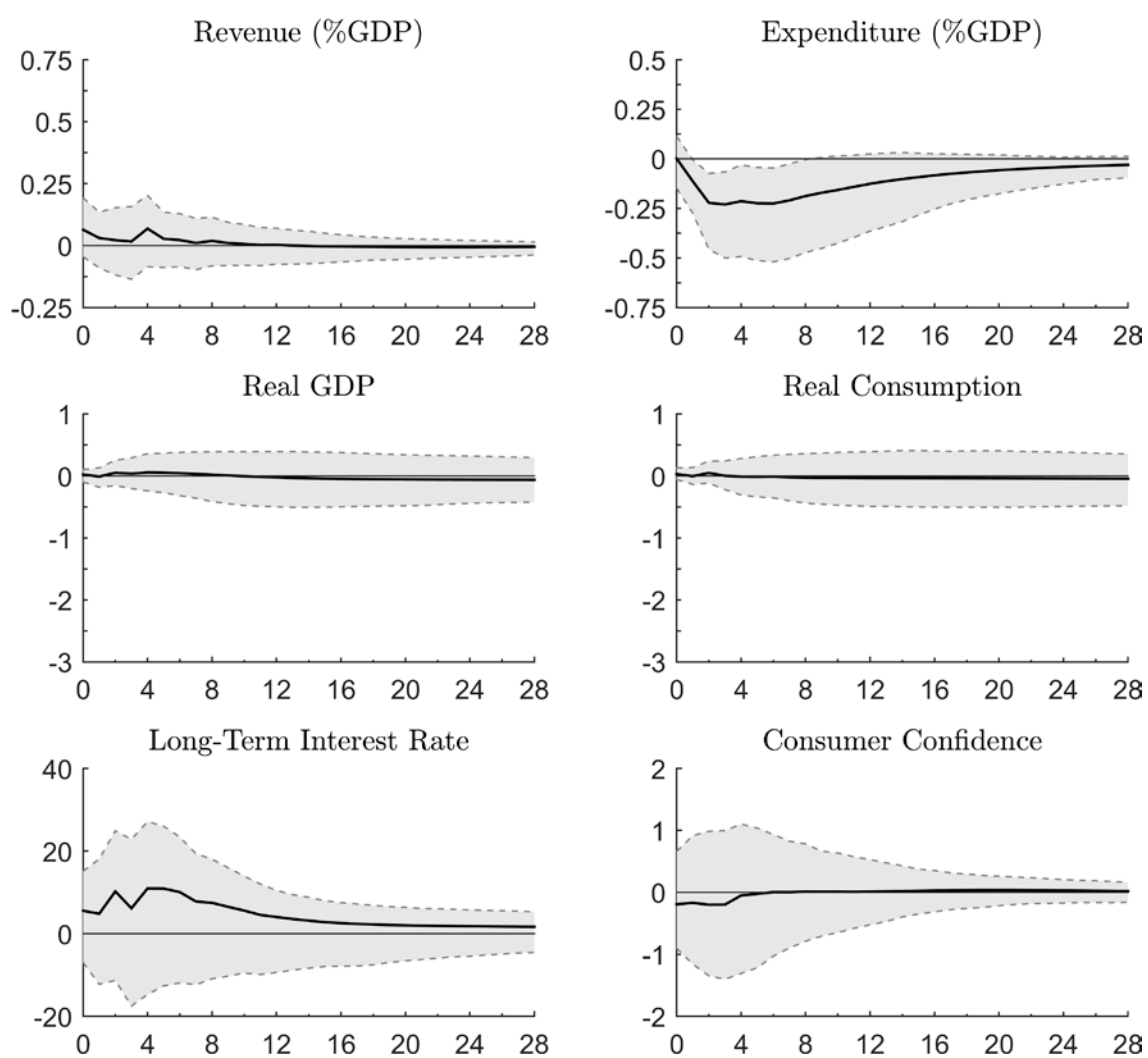
Notes: (i) The announcement shock (not portrayed) always has a magnitude of 1 percent of GDP. (ii) The mean impulse responses and their 90% confidence intervals are constructed with standard bootstrapping techniques and are based on 1000 replications. (iii) The impulse responses for revenues and spending are deviations in percentage points of GDP from their original values; real GDP, consumption and consumer confidence are deviations in percent from their original values; and the long-term interest rate is the deviation in basis points from its original value.

Figure 2a: Impulse responses baseline model – revenue-based plans



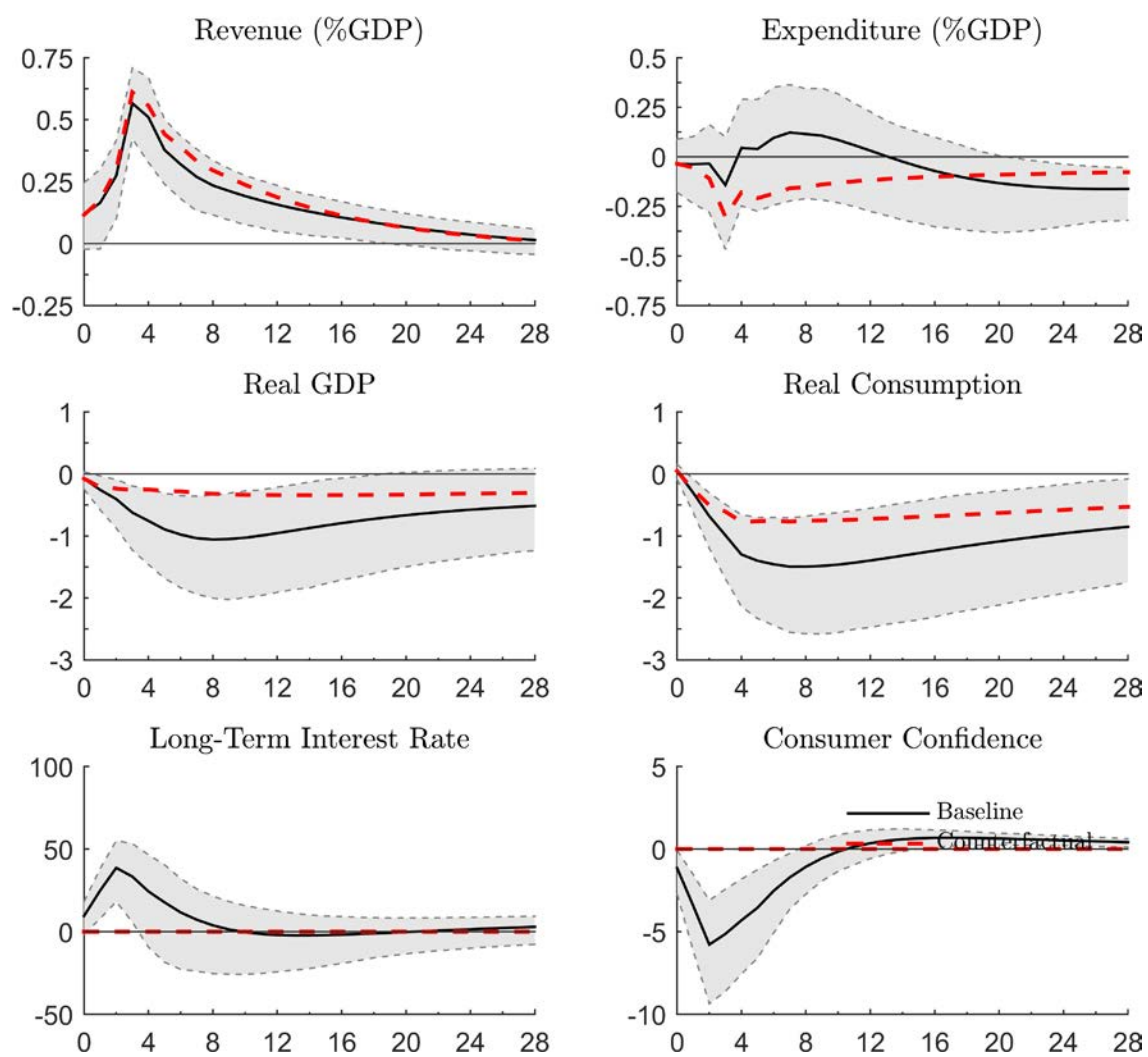
Notes: See Notes of Figure 1.

Figure 2b: Impulse responses baseline model – spending-based plans



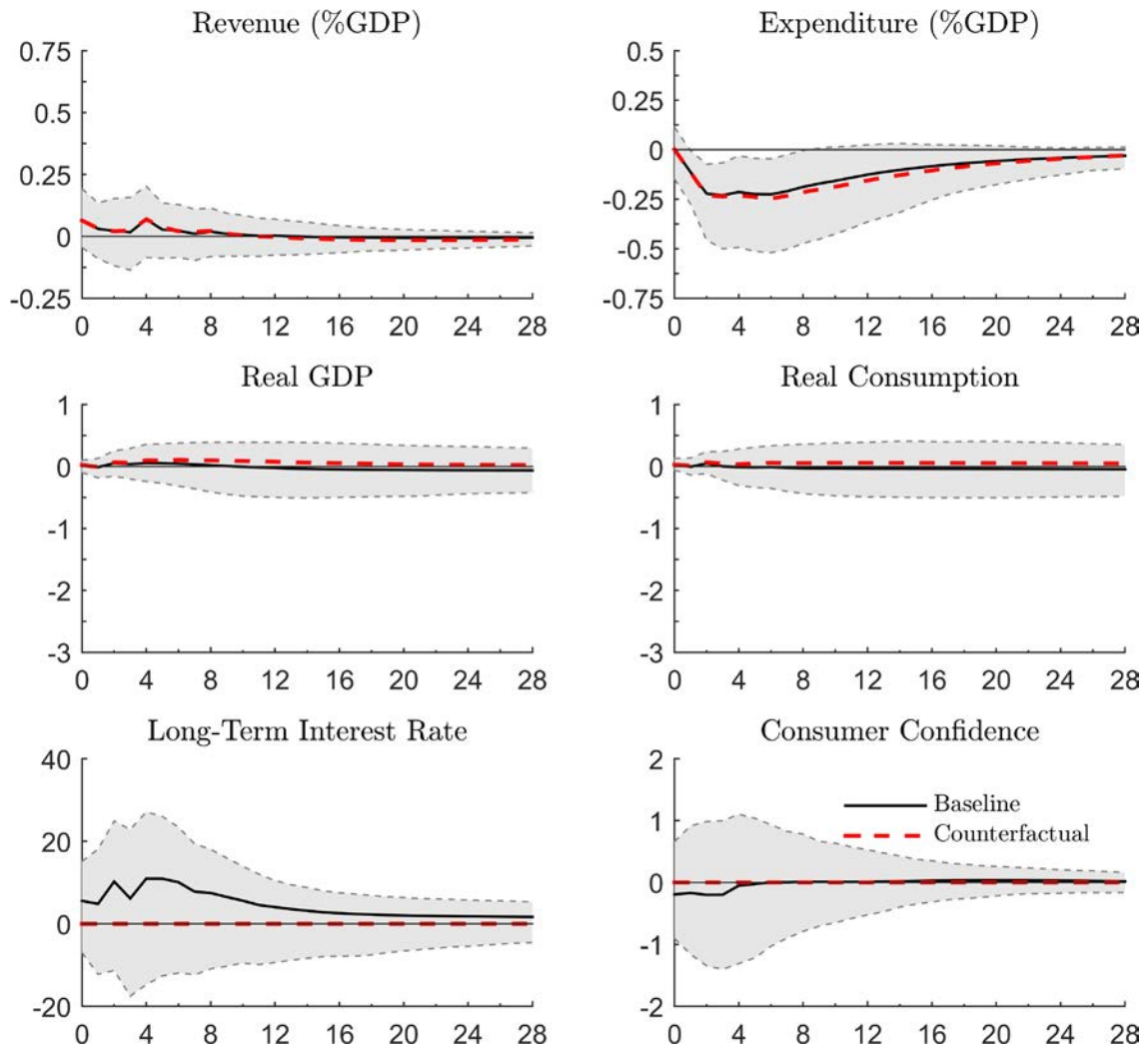
Notes: See Notes of Figure 1.

Figure 3a: Counterfactually shutting off the confidence channel – revenue-based plans



Notes: See Notes of Figure 1.

Figure 3b: Counterfactually shutting off the confidence channel – spending-based plans



Notes: See Notes of Figure 1.

Appendix

A: The actual (i.e., *ex-post*) data

A.1. Budgetary variables:

We obtain budgetary variables at both the annual and quarterly frequency. The annual budgetary data are taken from the November 2015 edition of the OECD Economic Outlook (EO). The quarterly data are from Eurostat. All the data have been compiled under the European System of Accounts, 2010 edition (ESA2010).

Correspondence Eurostat and EO series:

To construct appropriate quarterly data series, we have to make sure that the series extracted from Eurostat and the EO correspond to each other. To ensure maximum comparability of the OECD and Eurostat fiscal variables, we adopt the following procedure. First, we determine the correspondence between the budgetary components recorded at the annual frequency from the OECD with the annual data on the same components available from Eurostat. Based on the description of the data and the comparison of their numerical values, we are able to match perfectly a number of series observed at annual frequency between the two sources. The correspondences between codes from the two data sources are given in the follows:¹⁸

Table A.1: Correspondence between OECD and Eurostat series

		Code OECD	Code Eurostat
Revenue	Social security contributions received by government	SSRG	D61REC
	Indirect taxes	TIND	D2REC
	Total direct taxes	TY	D5REC
Expenditure	Government final consumption expenditure, appropriation account	CGAA	P3
	Government fixed capital formation, appropriation account	IGAA	P51G
	Social security benefits paid by the government	SSPG	D62PAY

Collection and construction of the quarterly series:

Then we collect the quarterly data from Eurostat using the same variable definitions. Hence, the quarterly data match the annual data from both Eurostat and the OECD. All quarterly data are

¹⁸ There are other components of government revenues and expenditures available from both sources that cannot be matched.

seasonally unadjusted and expressed in millions of euros or in local currency units. We multiply the series expressed in local currency units with the exchange rate against the euro and transform all the data in euros, after which we seasonally adjust the series using the X-11 procedure in EViews.

Unfortunately, we do not avail of quarterly data over the full sample period. The quarterly data have the following coverage: Austria from 2001, Belgium from 1995, Germany from 2002, Denmark from 1999, Spain from 1995, Finland from 1999, France from 1980, the UK from 1987, Ireland from 2002, Italy from 1999, the Netherlands from 1999, Portugal from 1999 and Sweden from 1995. We annualize the quarterly values by multiplying with a factor of four, deflate them using the quarterly GDP deflator and then append the resulting quarterly series to the annual series interpolated to the quarterly level in the period before the quarterly data become available. We interpolate annual OECD data to the quarterly frequency by means of a cubic spline interpolation. We append the quarterly to the interpolated annual data by scaling the annual observations with the ratio of the quarterly Eurostat and annual OECD observations in the first quarter of 2002. We choose 2002 to ensure that we use the same scaling factor for all countries and because in the case of Germany and Ireland the quarterly data is only available starting in 2002.

Construction of aggregate quarterly revenues and expenditure series:

We construct the following series of aggregate revenues and expenditures:

Total revenues (narrow definition) = Total direct taxes + Indirect taxes + Social security contributions received by government;

Total expenditure (narrow definition) = Government final consumption expenditure, appropriation account + Government fixed capital formation, appropriation account + Social security benefits paid by the government.

The relevant codes are found in Table A.1. These series are constructed both at annual and quarterly frequency. At the annual frequency, we also construct other series. The most comprehensive annual measure of public revenues is:

Total receipts, excluding gross interest receipts, government (OECD code: YRGT) = Current receipts, government (OECD code: YRG) + Capital tax and transfers receipts (OECD code: TKTRG) - Gross government interest receipts (OECD code: GGINTR).

Moreover we collect:

Current receipts, excluding gross interest receipts, government = Total revenue (narrow definition) + Other current receipts by government (OECD code: TOCR) + Property income received by government (OECD code: YPERG) – Gross government interest receipts (OECD code: GGINTR).

The most comprehensive measure of public spending that we use is:

Total disbursements, excluding gross interest payments, general government (OECD code: YPGTX) = Current disbursements, government (OECD code: YPG) + Government fixed capital formation, appropriation account (OECD code: IGAA) + Capital transfers paid and other capital payments (OECD code: TKPG) – Government consumption of fixed capital (OECD code: CFKG) – Gross government interest payments (OECD code: GGINTP).

Moreover we collect :

Current disbursements, excluding gross interest payments, government (OECD code YPGX) = Government final consumption expenditure, appropriation account (code OECD: CGAA) + Property income paid by the government (OECD code: YPEPG) + Social security benefits paid by the government (OECD code: SSPG) + Other current outlays, government (OECD code: YPOTG) – Gross government interest payments (OECD code: GGINTP).

A.2. Macroeconomic variables:

Most of our quarterly macroeconomic variables are extracted from the OECD Economic Outlook (2015). We retrieve the data (through Datastream) on private investment from the IMF International Financial Statistics database.¹⁹ When the data is not seasonally adjusted at the source, we transform the series with the standard X-11 procedure. Where necessary, we perform a nonlinear (quadratic) interpolation of the annual data to quarterly frequency, ensuring that the annual value is equal to the sum of the resulting quarterly observations for the year.

We obtain the following variables:

¹⁹ The precise series is “Gross fixed capital formation, corporations, households and non-profit institutions serving households (from gross domestic product by expenditure), nominal, current prices, not seasonally-adjusted”. For non-Eurozone countries we multiply with the exchange rate against the euro or the ecu (for the period preceding the Eurozone). Finally, we deflate all the series with the GDP deflator from the OECD Economic Outlook (2016).

Nominal GDP = Gross Domestic Product (market prices), value, annual and quarterly. The sources are the OECD Economic Outlook 96 of November 2014 EO96 (Ireland after 2013), the OECD Economic Outlook 95 of May 2014 (Spain) and the OECD Economic Outlook 88 of December 2010 (Ireland before 2013, Germany before 1991). We transform the series into millions and deflate it with the appropriate GDP deflator (market prices). In the cases where GDP is expressed in local currency units (Denmark, Sweden, United Kingdom), we transform it into euros by multiplying with the exchange rate;

Potential real GDP: we obtain this variable as the trend component resulting after first Hodrick-Prescott filtering the log of real GDP as defined above and then taking the exponential (inverse of the logarithm) of the resulting trend component of the series;

Real private consumption = Private Consumption expenditure, volume. The sources are the OECD Economic Outlook 96; the OECD Economic Outlook 95 (Spain); the OECD Economic Outlook 88 (Ireland, Germany before 1991). For Germany and Ireland we have to link the Economic Outlook 96 and 88 series. Because of this, we change the base year. To do so, we calculate the year average of the quarterly values in the year chosen as the base in both series: the series that uses this as the original base year and the series that uses another year as the base year. Then we multiply all values indexed to the other year by this factor. For example, Ireland has 2008 as the base year in the Economic Outlook 88 series and 2012 as the base year in the Economic Outlook 96 series. We choose 2008 as a base year, calculate the average of quarterly values in 2008 for both the Economic Outlook 96 and 88 series. Then we take the ratio of 2008 values to 2012 values (equivalent to price index 2012/2008) and multiply all values from 2012 and on by this factor;

CPI = Consumer Price Index All Items, change year-on-year, quarterly (OECD Main Economic Indicators);

Long-term interest rate = Long-term interest rate on government bonds, quarterly (OECD Economic Outlook EO96). Missing observations are taken from EO88 (also quarterly): Germany before 1991 (Western Germany) and Ireland before 1990;

Short-term interest rate = Short-term interest rate, quarterly (OECD Economic Outlook 96).²⁰ Missing observations are taken from EO88 (also quarterly): Germany before 1991 (Western Germany), Ireland between 1984 and 1990, and the UK between 1977 and 1978;

Exchange rate = Exchange Rate, quarterly: Swedish krona to euro, Danish krone to euro (ECB); Euro to pound (WM/Reuters and Datastream);

GDP deflator = Gross domestic product, deflator, market prices, annually and quarterly (OECD Economic Outlook EO96);

Public debt = General government gross financial liabilities, value (OECD Economic Outlook 96 and 88). We use OECD Economic Outlook 96, and supplement missing observations with values from OECD Economic Outlook 88. The data are in billions of euros. We append the OECD Economic Outlook 88 subsample by multiplying its numbers with the ratio of the values from the last year in which the OECD Economic Outlook 88 overlaps with the OECD Economic Outlook 96. For Germany, we link the series with that for West-Germany.

Private investment = Private gross fixed capital formation, volume. The International Monetary Fund (IMF) IFS database provides nominal, sometimes seasonally adjusted and sometimes non-seasonally-adjusted values in local currency units before 1999 and in euros after 1999. We use the IMF's IFS because from the OECD the data are missing entirely for Austria, Italy, Portugal and Spain. The IFS data are processed further for two reasons. First, for Italy, before 1999 the series was in trillions of lira (we multiplied by 1000) and for Portugal it was in billions escudo (we multiplied by 1000). For Ireland the linked series was in millions of euros (we divided the entire linked series by 1000). To link two series before and after 1999, we multiply the data in local currency units by the official conversion rate to the euro prevailing in 1999. The conversion rates are the ERM bilateral central rates to be used in determining the irrevocable conversion rates for the euro (see www.ecb.int, 2 May 1998). Second, the IFS data are not compiled in the same way for all the countries: some are seasonally adjusted, some are not. The latter need to be made comparable to the former. Because non-seasonally adjusted series are not available at all for some countries (France, Germany, Netherlands,

²⁰ The short-term interest rate is usually either the three-month interbank offer rate for loans between banks with an excess of liquidity and a shortage of liquidity, or the rate associated with Treasury bills, certificates of deposit or comparable instruments, always of three month maturity. For Euro-area countries the 3-month "European Interbank Offered Rate" is used from the date the country joined the euro.

Spain), we opt for using the seasonally-adjusted series. Those series that are only available as non-seasonally adjusted, we seasonally ourselves using the X-11 procedure (implemented in Eviews). Having harmonized the unit of currency and seasonally adjusted the non-seasonally adjusted series, we transform the series into real terms using the seasonally-adjusted deflator of gross fixed capital formation from the OECD.²¹

A.3. Confidence

A detailed description of the construction of the confidence variables is found in Beetsma *et al.* (2015). They are collected from the OECD, which in turn obtains them from other institutions, such as the national statistical institutes, and which standardizes them to make them comparable across the countries. Consumer confidence is based on questionnaires sent out to a random sample of the population. The questionnaires are based on answers to questions on whether or not the individual expects the personal and general economic situation to improve or not. The answers are aggregated to create an index. The business confidence indicator is also obtained from the OECD and constructed by aggregating the answers to a number of questions on business tendencies. The OECD standardizes the confidence series in a number of steps. Our impulse responses show the deviations in percent from the baseline.

B. Construction details of the consolidation announcements dataset

The materials in this appendix have been copied with slight modification from Beetsma *et al.* (2015). Regarding *what* is considered the announcement of a new consolidation, we have taken the following decisions:

- If a newly government explicitly signals its commitment to an existing fiscal plan, we consider this an announcement, the idea being that this should provide information on the likelihood that the plan will be carried out.
- We do not treat EU stability and convergence plans as announcements involving a consolidation.
- because the OECD data do not explicitly distinguish between the announcement and the implementation of measures, we have to interpret some verbs as signaling one or the other:

²¹ Both the GFCF deflator and the GDP deflator are discontinued for Germany in 1991. To link them, we take the ratio of prices for Germany (with base year 2010) and prices for Western Germany (with base year 1991), and average it over the quarters of 1991. We multiply by this factor all values for the price index with base 1991 (to transform their base to 2010).

- “a new tax is introduced” is treated as the implementation of a measure introduced in the budget for that year and the corresponding moment of announcement is the moment that the budget for that year was presented.
- “Excise duties are increased” is treated as the implementation of an earlier announced measure.
- “The Government takes additional fiscal measures” is treated as the announcement of a new measure.

Regarding the *exact timing* of announcement, we have taken the following decisions:

- We base the timing on the existing budgetary process in the country. The dating of the announcement of measures that are part of a new budget is the moment the government presents the budget to the parliament.
- The date the Parliament votes about the budget is not considered an announcement, unless the Parliament significantly modifies the plan of the Government. The dating of the announcement of such amendments is the moment of the vote on the budget in parliament or the moment they are reported if that is earlier.
- If the Parliament adopts the budget with “minor modifications” (as is commonly stated in documents), we do not consider this a separate announcement.

The Data Construction Appendix includes the description of each consolidation from the OECD Economic Surveys. We document the classification we have applied to the elements of the consolidation and the timing, i.e. the identification of the precise month of the year in which the announcement is made. Below we provide some examples.

Example 1: match of implementation in Devries *et al.* (2011) with OECD announcement information (Austria 1981):

Devries *et al.* (2011, p.13) discuss the fiscal consolidation implemented in Austria in 1981, “*the spending cuts fell on the pensions, while the tax hikes included a hike in the VAT rate on energy, a new tax on credit institutions and gasoline stations, and the suspension of part of the savings incentive system*”. The OECD describes the draft Budget for 1981, introduced in Parliament in October 1980. This comprises, among other measures, “*the cancelling of the interest subsidy scheme for investment, raise of VAT rate for energy from 8 to 13 per cent, introduction of special taxes on*

petrol stations and branch offices of credit institutions.” (OECD Economic Surveys, Austria 1981, p.58). Based on the composition of measures (VAT rise, taxes on gasoline stations and credit institutions) we identify that the policies mentioned by Devries *et al.* (2011) had first been proposed in the draft Budget for 1981, presented in October 1980.

Example 2: information from newspaper archives or national sources (Germany, 1993):

Devries *et al.* (2011, p.41) mention (in the description of the 1993 consolidation) the implementation of a VAT increase: *“there was an increase in the VAT rate from 14 to 15%, with an estimated impact of 0.39% of GDP in 1993”*. This was, in fact, proposed in September 1991. See the documentation from the German Parliament: <http://dip21.bundestag.de/dip21/btd/12/011/1201108.pdf>

Example 3: information from newspaper archives or national sources (Spain, 1992):

“The central government budget for 1992 projects a marked reduction in the deficit to almost 2 per cent of GDP. Budget consolidation is planned to be achieved by raising revenues in relation to GDP, with expenditure remaining at the level of 1991 (about 23 per cent of GDP) (...) The Budget includes large increases in indirect tax rates, notably the increase in the standard VAT rate by 1 percentage point to 13 per cent.” (OECD Economic Surveys, Spain 1992, p.40).

We have checked the *El Pais* newspaper archives and in an article released on October 7, 1991, we found information that a reform involving an increase in VAT was initiated in October 1991. This was expected according to the regular budgetary procedure and we used this information to assign the announcement of the 1992 Budget to October 1991 (see http://elpais.com/diario/1991/10/07/economia/686790014_850215.html).

C. A framework and indirect evidence for “active” non-follow-up

This appendix presents a very simple model to rationalize differences in “active” non-follow-up between plans for revenue-based consolidation versus spending-based consolidation. The model is merely intended to organize our understanding of a potential explanation of the observed differences. Hence, we abstract from all possible features that are not strictly necessary to produce suggested mechanism. The model is based on the assumption that the uncertainty that a plan eventually turns out to be politically prohibitive is higher for spending- than for revenue-based plans.

We also provide some indirect evidence for the potential relevance of the mechanism presented here.

There are two periods, period 0 and period 1. GDP is assumed constant and normalized to one, while the real interest rate is assumed to be constant at zero. In period 0, public spending and public revenues (as shares of GDP) are given by g_0 , respectively τ_0 . Together with the debt ratio $d_0 > 0$ at the start of period 0, they produce a new debt ratio $d_1 = d_0 + g_0 - \tau_0$ at the start of period 1. We assume that $g_0 > \tau_0$. In other words, in the absence of a correction in public spending and/or revenues, the public debt ratio continues to rise. Therefore, in period 0 the government announces a consolidation plan (g_1^a, τ_1^a) for spending and revenues in period 1. Below, we will show that the optimal consolidation plan implies $g_1^a < g_0$ and $\tau_1^a > \tau_0$.

In period 1, the government has the option to carry out the announced spending consolidation or stick to the spending level in the previous period. Similarly, it has the option to carry out the announced increase in revenues or stick to the revenues level in the previous period. Carrying out consolidation measures is politically costly. However, ending period 1 with public debt is also costly. This will not be explicitly modeled, but it may be the result of politically-costly consolidation measures that are expected to be needed in the future. Hence, in period 1 the government features a loss from carrying out the combination (g_1, τ_1) of:

$$L = \frac{1}{2} \left[\lambda (g_0 - g_1)^2 + \mu (\tau_0 - \tau_1)^2 + d_2^2 \right], \quad \lambda, \mu > 0,$$

where $d_2 = d_1 + g_1 - \tau_1 = d_0 + g_0 - \tau_0 + g_1 - \tau_1$. In addition, there are “lump sum” stochastic political costs Δ_g and Δ_τ of carrying out the announced spending, respectively revenues consolidation measures. Concretely, we assume that:

$$\Delta_g = \begin{cases} 0, & \text{with probability } 1 - \pi_g \\ \bar{\Delta} > 0, & \text{with probability } \pi_g \end{cases} \quad \Delta_\tau = \begin{cases} 0, & \text{with probability } 1 - \pi_\tau \\ \bar{\Delta} > 0, & \text{with probability } \pi_\tau \end{cases}$$

The constant $\bar{\Delta}$ is sufficiently large that the political cost of consolidating spending, respectively revenues, is prohibitively high, and the government is forced to stick to the spending, respectively revenues levels in period 0. Finally, Δ_g and Δ_τ are assumed to be statistically independent.

The timing of events is as follows. In period 0, the government announces (g_1^a, τ_1^a) . Then, the economy moves to period 1. At the start of period 1, the values of Δ_g and Δ_τ become known. Finally, the government sets (g_1, τ_1) .

The following table indicates the probabilities and the possible outcomes for spending and revenues in period 1:

	$(1 - \pi_g)$	π_g
$(1 - \pi_\tau)$	$(g_1, \tau_1) = (g_1^a, \tau_1^a)$	$(g_1, \tau_1) = (g_0, \tau_1^a)$
π_τ	$(g_1, \tau_1) = (g_1^a, \tau_0)$	$(g_1, \tau_1) = (g_0, \tau_0)$

Hence, exploiting the loss function L , in period 0 the government sets (g_1^a, τ_1^a) so as to minimize:

$$\begin{aligned} & \frac{1}{2}(1 - \pi_g)(1 - \pi_\tau)[\lambda(g_0 - g_1^a)^2 + \mu(\tau_1^a - \tau_0)^2 + (d_1 + g_1^a - \tau_1^a)^2] + \frac{1}{2}(1 - \pi_g)\pi_\tau[\lambda(g_0 - g_1^a)^2 + (d_1 + g_1^a - \tau_0)^2] \\ & + \frac{1}{2}\pi_g(1 - \pi_\tau)[\mu(\tau_1^a - \tau_0)^2 + (d_1 + g_0 - \tau_1^a)^2] + \frac{1}{2}\pi_g\pi_\tau(d_1 + g_0 - \tau_0)^2 \end{aligned}$$

Some algebra yields the following outcomes for the consolidation announcements:

$$g_1^a = \frac{(\lambda - \mu + \lambda\mu + \pi_g - \pi_\tau - \pi_\tau\pi_g)g_0 + (\mu + \pi_\tau)(2\tau_0 - d_0)}{\mu + \lambda + \mu\lambda + \pi_g + \pi_\tau - \pi_g\pi_\tau}$$

$$\tau_1^a = \frac{(\mu - \lambda + \lambda\mu + \pi_\tau - \pi_g - \pi_\tau\pi_g)\tau_0 + (\lambda + \pi_g)(2g_0 + d_0)}{\mu + \lambda + \mu\lambda + \pi_g + \pi_\tau - \pi_g\pi_\tau}$$

We can now calculate the average deviations of announced spending and revenues from their realizations as:

$$E(g_1 - g_1^a) = \pi_g(g_0 - g_1^a) = \frac{\pi_g(\mu + \pi_\tau)[2(g_0 - \tau_0) + d_0]}{\mu + \lambda + \mu\lambda + \pi_g + \pi_\tau - \pi_g\pi_\tau} > 0$$

$$E(\tau_1^a - \tau_1) = \pi_\tau(\tau_1^a - \tau_0) = \frac{\pi_\tau(\lambda + \pi_g)[2(g_0 - \tau_0) + d_0]}{\mu + \lambda + \mu\lambda + \pi_g + \pi_\tau - \pi_g\pi_\tau} > 0.$$

It is easy to see that, not surprisingly, $E(g_1 - g_1^a)$ and $E(\tau_1^a - \tau_1)$ are increasing in the probabilities π_g , respectively π_τ , that the announced consolidation measures will not be carried out. Most importantly, we find that $E(g_1 - g_1^a) > E(\tau_1^a - \tau_1)$ if and only if $\pi_g\mu > \pi_\tau\lambda$. If deviations in

revenues and spending from their initial values feature equal weights in loss function L , i.e. $\mu = \lambda$, this condition reduces to $\pi_g > \pi_\tau$. Hence, the average deviation of the actual from the announced spending reduction exceeds the average deviation of the actual from the announced revenues increase when the chance of not carrying out the spending consolidation exceeds that of not carrying out the revenues consolidation measures.

Our simple theoretical framework thus predicts that, *ceteris paribus*, if the chance that a spending cut turns out to have a prohibitive political cost is higher than the chance that a revenue increase turns out to have a prohibitive political cost, the average deviation of actual from planned consolidation measures is larger for spending than for revenues.

Here, we provide some indirect data support for the mechanism suggested by our model based on data from Hamann *et al.* (2013, 2016) on 159 episodes of general strikes in the European Union plus Norway over the period 1980-2006. The data contains information about the country where the strike occurs, the exact date of the strike, the main governing party, the issue in dispute and the outcome of the strike in terms of concessions. Examples of issues in dispute are “Labour law reform”, “Austerity”, “Pensions”, “Economic policy” and “Public spending”. In a number of instances the description of the issue in dispute makes it quite clear whether the strike is associated with public spending cuts or tax increases.²² However, in many instances this is not clear. For example, when the issue in dispute is “Austerity”, this can be result of spending cuts, revenues increases or both. Hence, we check all strikes to get more information, especially in cases where the motivation is “Economic policy”, “Public spending” and “Austerity”. In particular, we look for newspaper articles documenting the strike and try to deduce what its motivation is. A substantial number of disputes are about pensions. We classify them as “spending cut motivated”, as we expect that pension measures are typically aimed at reducing expenditures on public pensions. Indirectly, this is also the case for (planned) increases in the retirement age, which will also result in reduced spending on pension benefits, *ceteris paribus*. In cases where the strike was against an austerity budget comprising changes in both revenue and spending, we have characterized the protest as against both categories of measures. At the end of this appendix we provide a few examples of the assignment of issues in dispute in the strikes.

The dataset considers a slightly larger set of countries than our austerity announcement data and it also includes an outlier in terms of the number of general strikes: out of the total of 159 episodes, 69

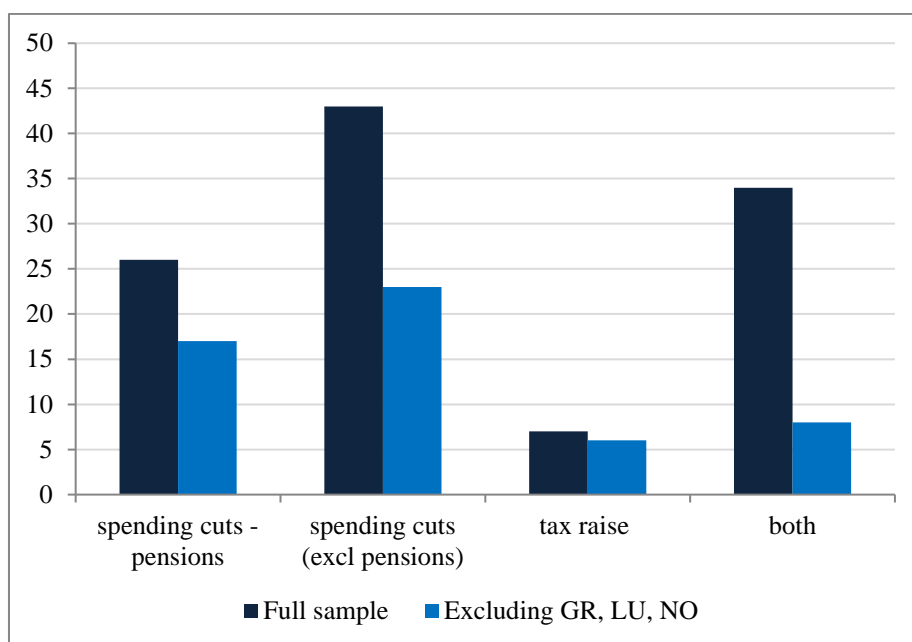
²² We assume that the protests are never against expansionary budgetary measures. For example, if the issue in dispute is “Public spending”, we assume that the protests are against public spending cuts and not spending expansions.

are registered in Greece. We start by analyzing the dataset in full and then restrict our attention to the country sample matching our 13 European OECD countries.

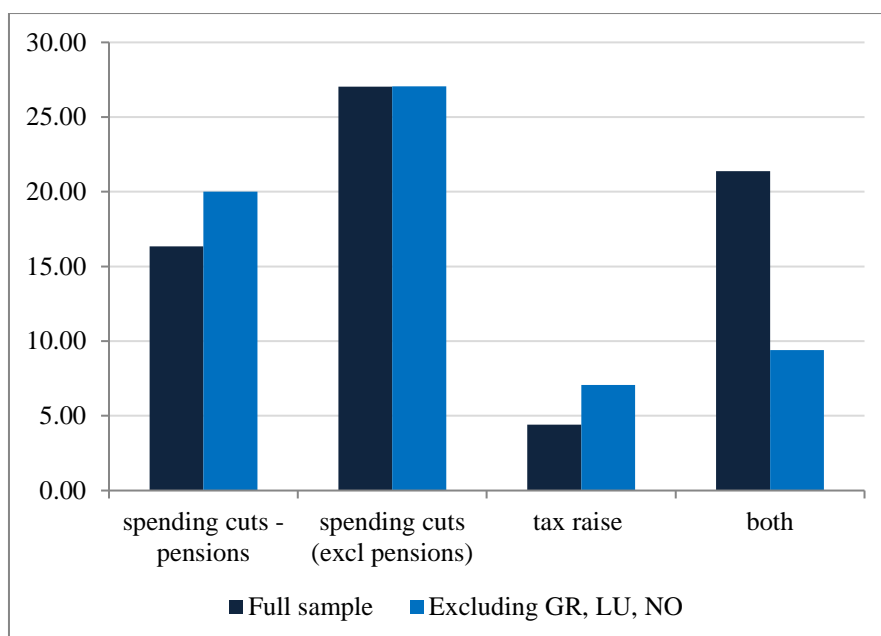
Out of the 159 disputes, we find that 69 are spending-cut motivated, 43 after excluding those where the issue in dispute is pensions, while the number of revenue-raise motivated strikes is only 7. Hence, disputes motivated by spending-cuts occur with a substantially higher frequency than disputes motivated by revenue increases. We also observe 34 strikes against austerity in general, hence aimed at adjustments in both taxes and spending. Excluding Greece, Luxembourg and Norway, 85 strike episodes remain, of which 40 are motivated by spending cuts (23 upon exclusion of the pension-related disputes), 6 are motivated by revenue increases and 8 are motivated by both revenue increases and spending cuts. The information is summarized in Figures C.1a and C.1b.

Figure C.1: Strikes in Western Europe by issue in dispute

(a) Absolute numbers



(b) Percentages



As a next step, we select only those strikes that took place before 2014 in our sample countries and we obtain data for general strikes in eight countries: Austria, Belgium, Finland, France, Italy, Netherlands, Portugal, Spain. After removing the strikes that cannot be assigned specifically to austerity measures, we are left with twenty strikes that can be matched with the consolidation announcements in our dataset on the basis of the narrative description of the strike and the consolidation (one in Finland, one in France, one in Spain, one in the Netherlands, two in Portugal, four in Belgium and ten in Italy). Out of the twenty strikes, three were undertaken in reaction to the same austerity announcement (in December 2011 in Italy). Three were undertaken in response to revenue-based announcements (namely in Italy in September 2011, December 2011 and October 2013) and 17 in response to spending-based announcements. Moreover, out of these 17 spending-based announcements eight have a revenue component of zero. From Table C.1 we observe that the spending-reduction component in the consolidation plan is on average relatively larger for consolidation announcements that can be matched to a general strike than for the other consolidation announcements.

Table C.1: Links between strikes and announcements

Strike following announcement	Average reduction in spending	Average increase in revenues	Total value of announcement
no	0.82 (64.6%)	0.52 (35.4%)	1.34
yes	0.94 (72.7%)	0.29 (27.3%)	1.23

Notes: In brackets we report the share of the total value of the announcement accounted for on average by the spending or the revenues component.

Interestingly, for some of the general strikes we consider, newspaper articles and online sources that discuss them contain information that participants in a strike have a preference for tax increases relative to government spending cuts. For example, in November 1992, Finnish unions countered a governmental proposal of reduced unemployment benefits with the threat of a general strike. The conflict remained unresolved until the centre-right government "agreed not to reduce the unemployment benefits, and instead reluctantly accepted the union's demand for increased taxes" (Sundberg, 1993, quoted in Uwe Becker- The Changing Political Economies of Small West European Countries, p. 51). Another example concerns a pension reform initiated in 2004 in Italy. It seems the protesters perceived tax cuts as a more efficient electoral tool and felt that the pension reform was undertaken to create the possibility for tax cuts in view of the election. "The conservative government of the prime minister, Silvio Berlusconi, has already watered down the pension reform bill to try to appease the unions while still aiming to save more than £6bn a year. Union leaders say the government only wants to save on pensions so it can reduce taxes to boost its chances at the polls." (<https://www.theguardian.com/world/2004/mar/26/italy>; http://www.corriere.it/Primo_Piano/Cronache/2004/03_Marzo/26/sciopero.shtml).

Examples of assignment of issues in dispute in the strikes:

Here, we provide some examples of how we assign issues in dispute to public spending cuts, revenue increases, a combination of both, or some other matter.

Example 1: Greece, 8 December 2016, issue in dispute is "Labour law reform". On the basis of additional information from www.aljazeera.com/news/2016/12/greeks-strike-repressive-austerity-161208081056974.html "Greece's leading unions have launched a general strike that shut down several key sectors in protest over planned new pay cuts and taxes called for by international creditors" we classify this as "both spending cut and revenue increase".

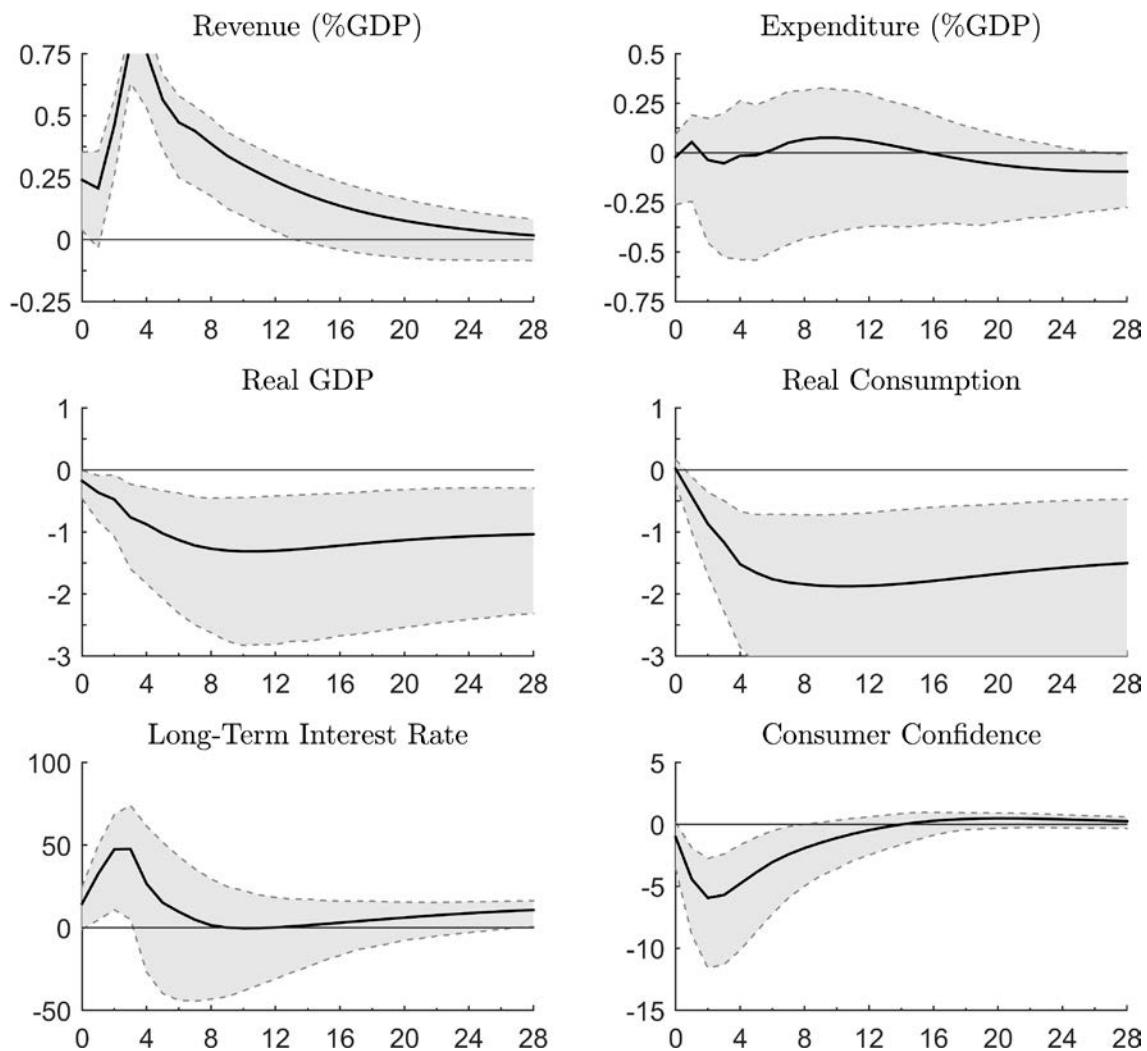
Example 2: Belgium, 24 June 2016, issue in dispute is "Austerity". On the basis of additional information from www.telesurtv.net/english/news/Thousands-Continue-Strikes-Across-Belgium--20160601-0001.html "Workers are protesting against the government's social and economic policies, which includes budget cuts. A number of trade unions have been protesting against government changes to labor laws including plans to increase the retirement age; to make it easier for companies to employ workers on part-time and short-term contracts; and to extend the working-week to 45 hours" we classify this as "spending cut motivated".

Example 3: Finland, 18 September 2015, issue in dispute is “Austerity”. On the basis of additional information from <http://www.bbc.com/news/business-34287816> “Strikers are protesting against government cutbacks, including limits to benefits and overtime pay. The plans included cutting back holidays, reducing pensioners' housing allowances, and reductions in employees' overtime and Sunday pay.” we classify this as “spending cut motivated”.

D: Results of robustness exercises

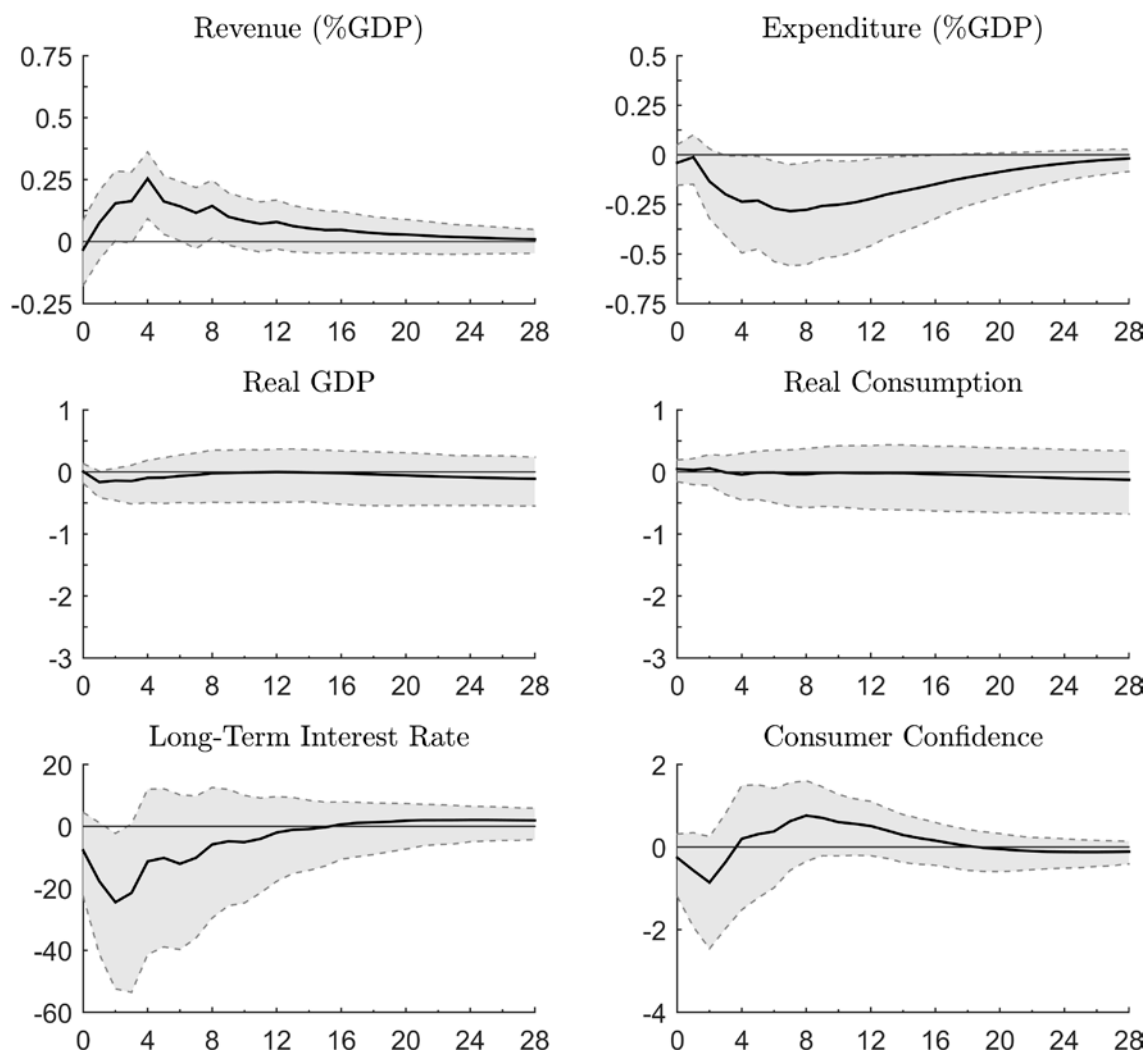
This appendix presents the figures for the various robustness exercises.

Figure D.1a: Excluding the crisis period 2008-2013 – revenue-based plans



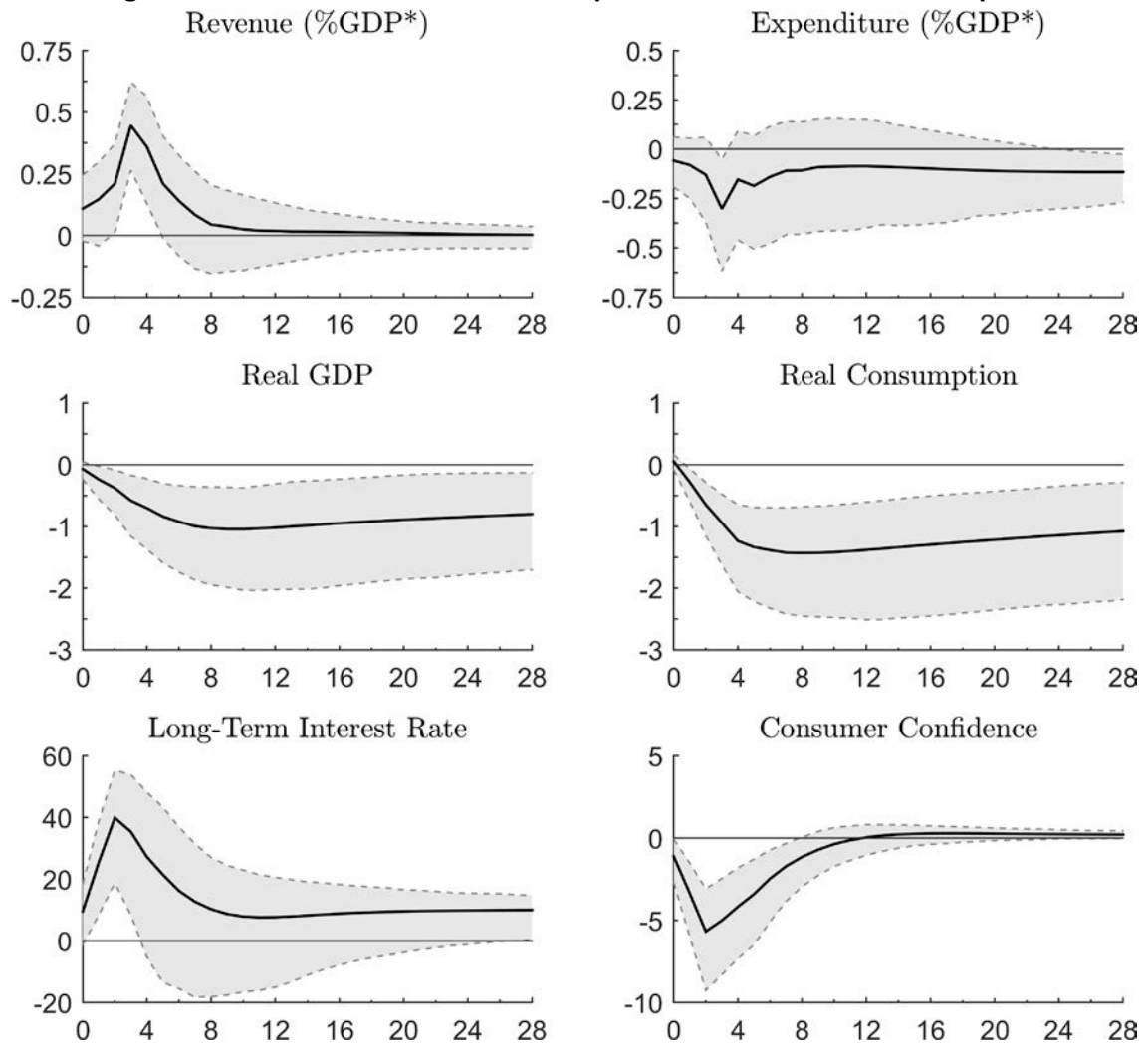
Notes: See Notes of Figure 1.

Figure D.1b: Excluding the crisis period 2008-2013 – spending-based plans



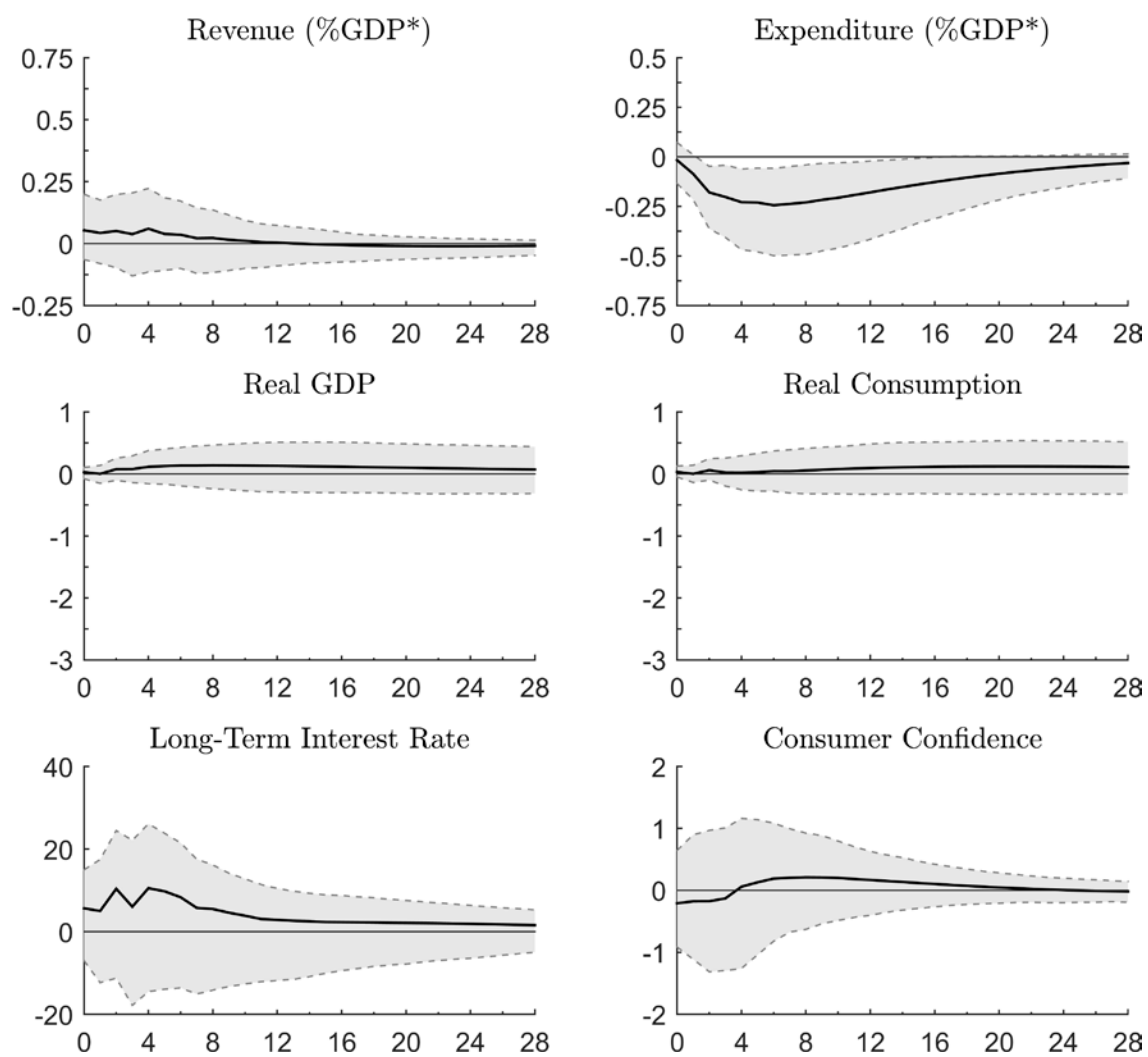
Notes: See Notes of Figure 1.

Figure D.2a: Baseline model in shares of potential GDP – revenue-based plans



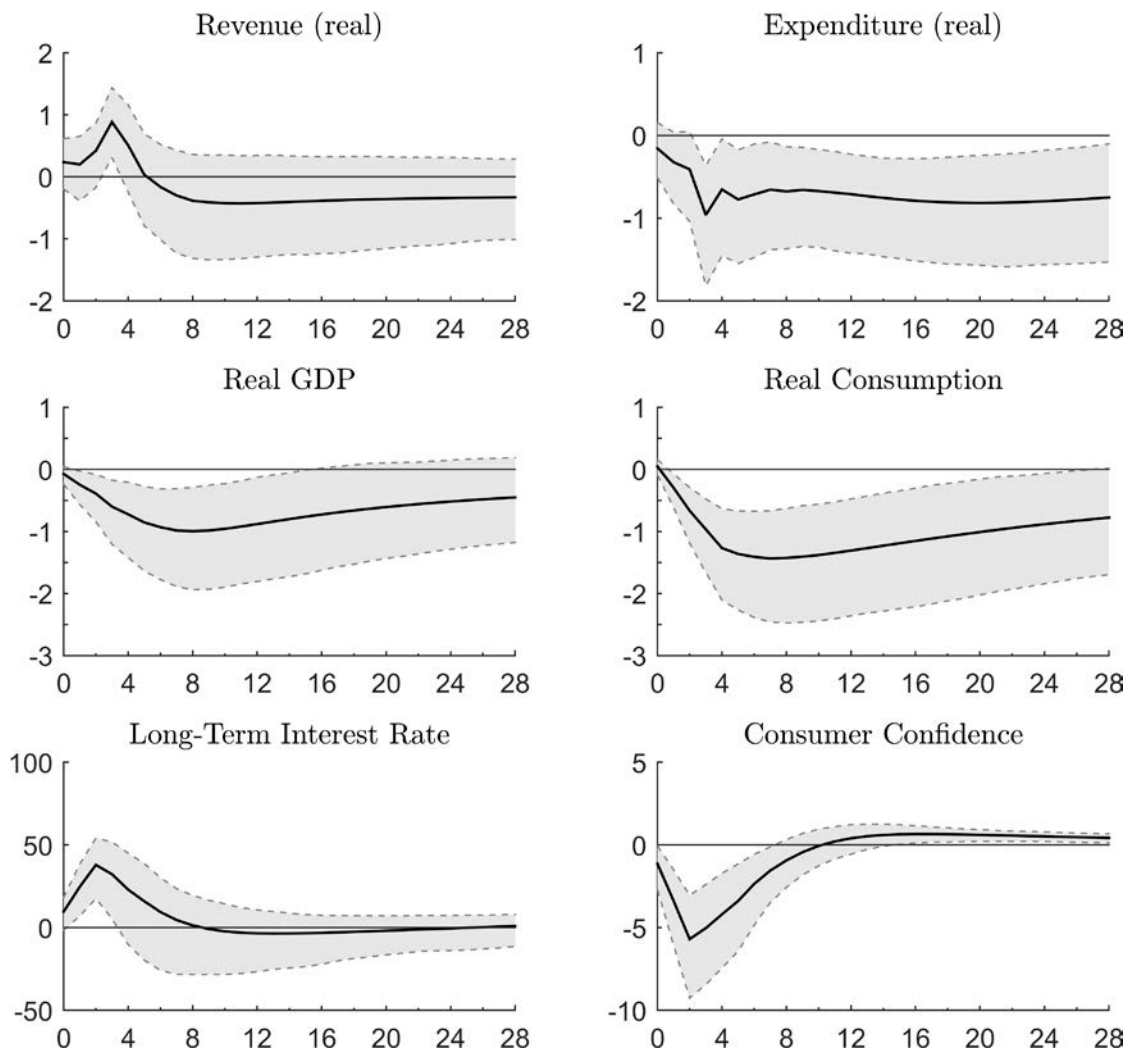
Notes: See Notes of Figure 1.

Figure D.2b: Baseline model in shares of potential GDP – spending-based plans



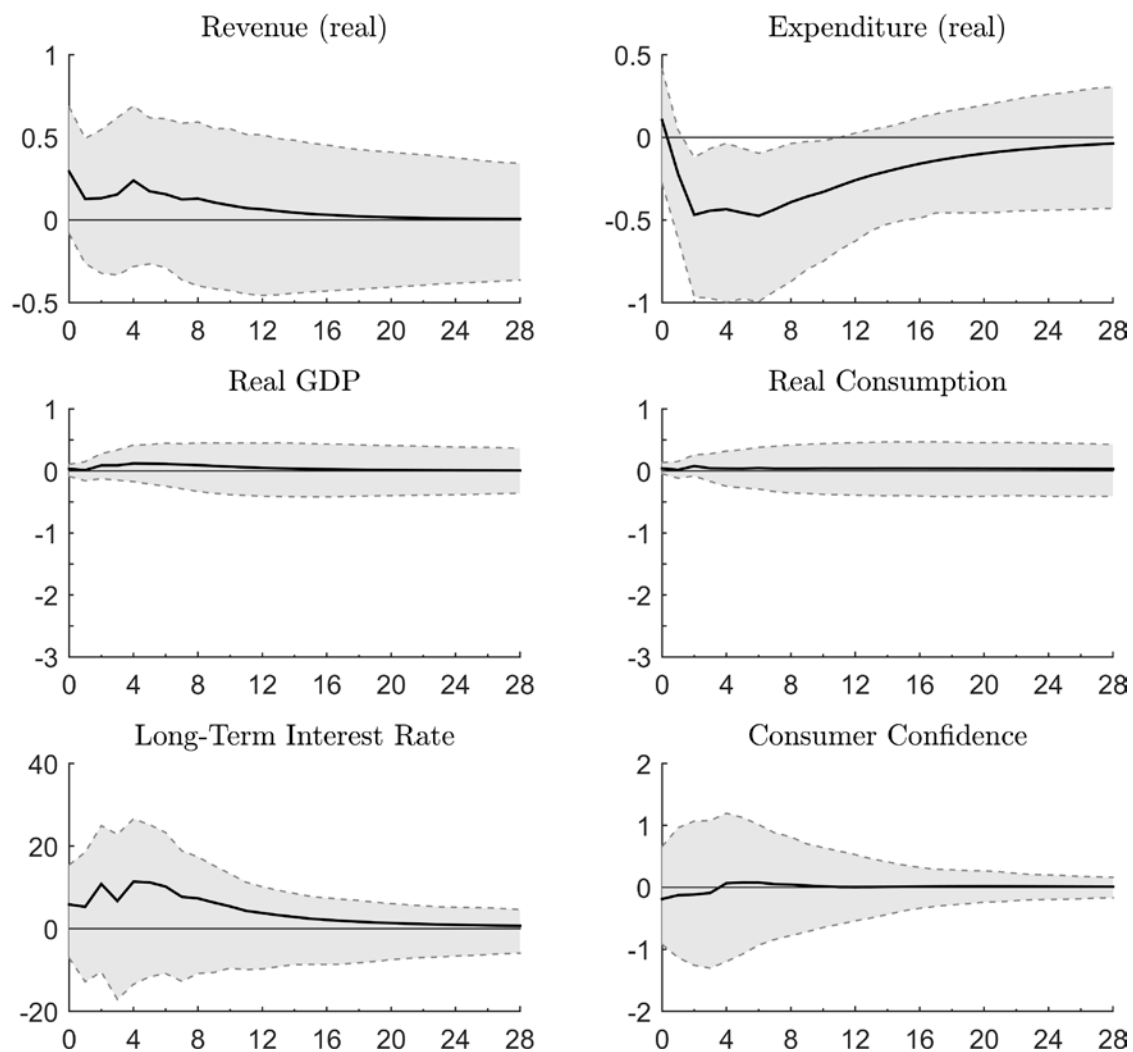
Notes: See Notes of Figure 1.

Figure D.3a: logs of revenues and spending – revenue-based plans



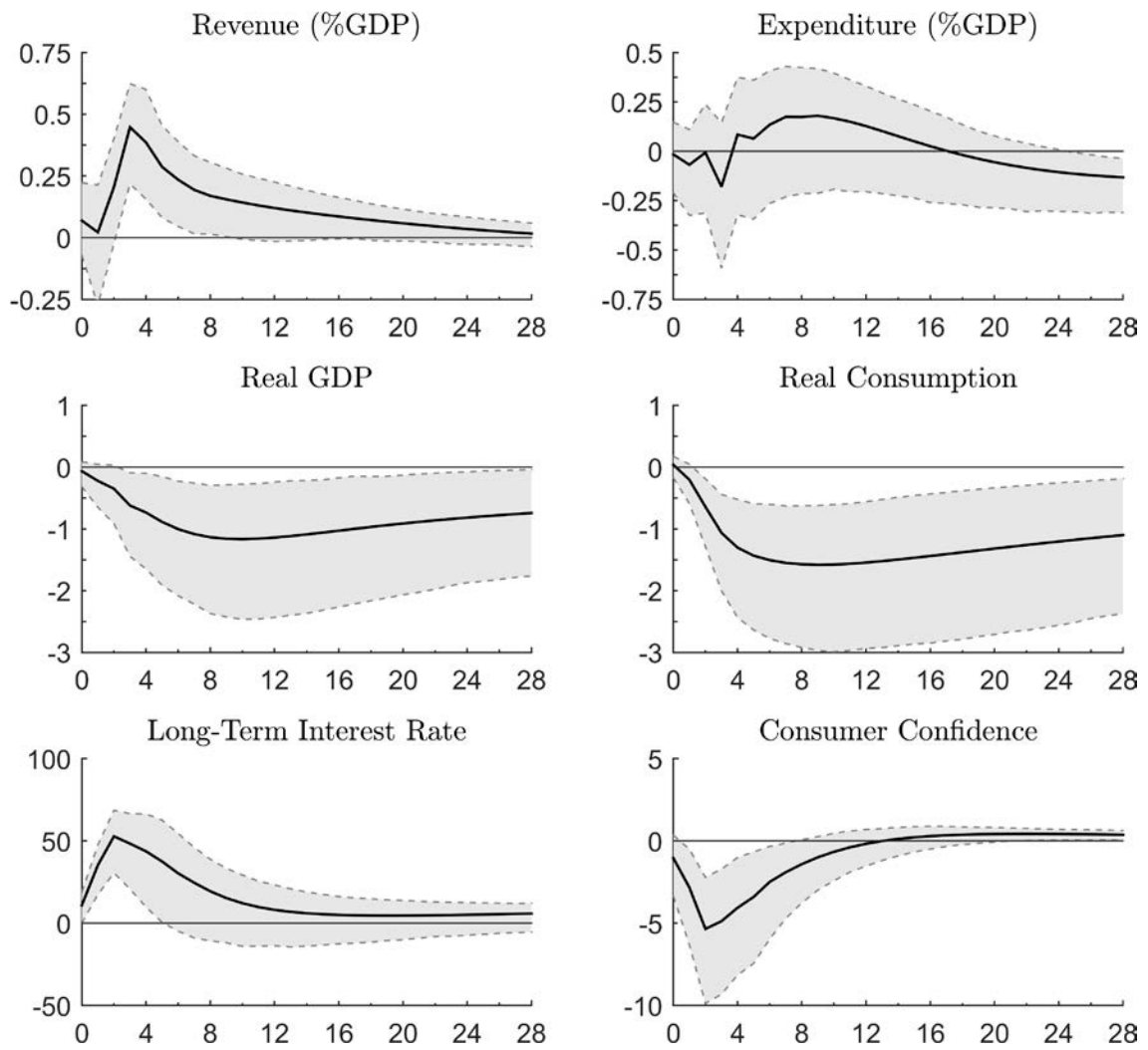
Notes: See *Notes* of Figure 1. The impulse responses for revenues and spending now measure the percent deviations from their original levels in euros.

Figure D.3b: logs of revenues and spending – spending-based plans



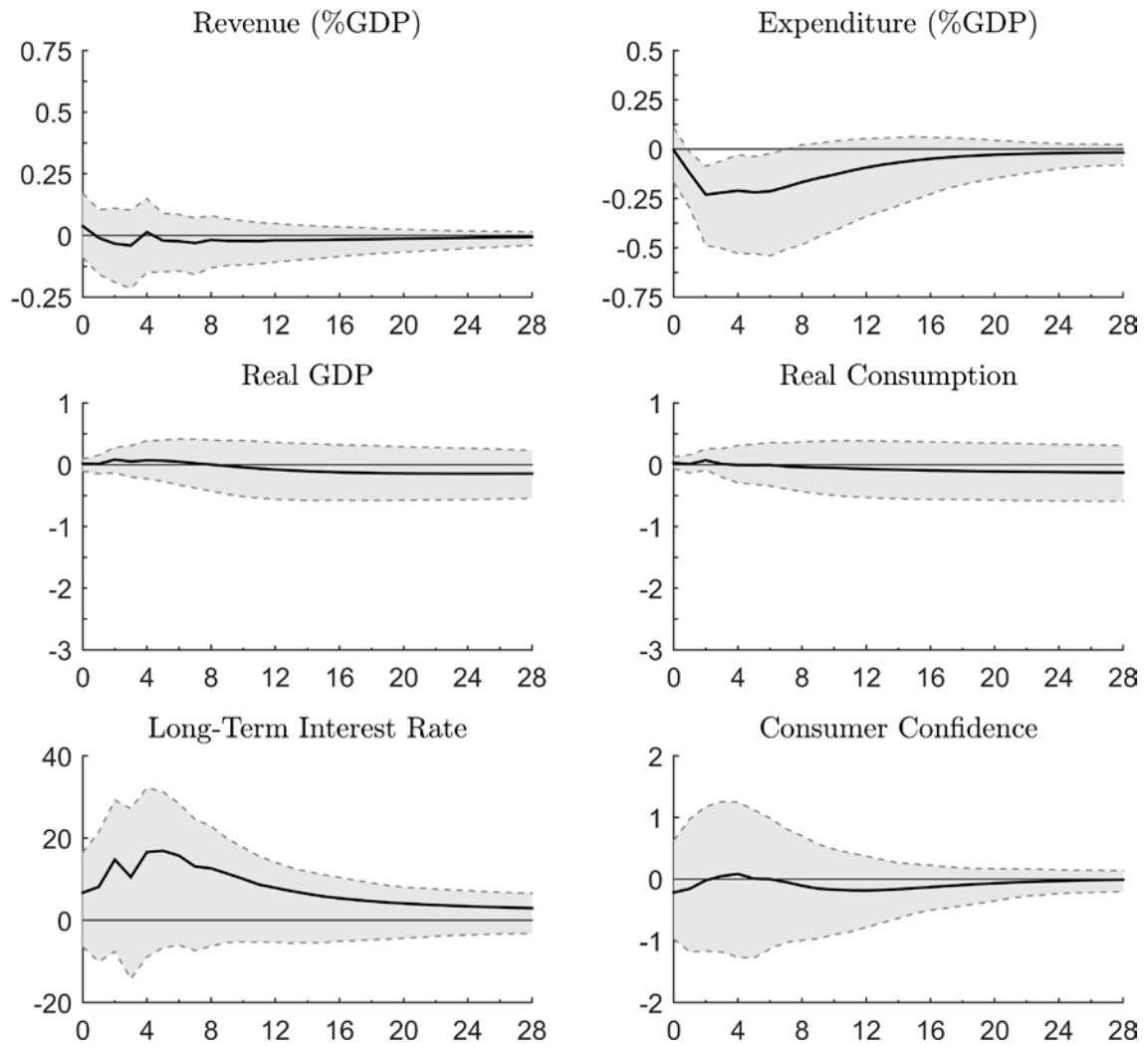
Notes: See *Notes* of Figure 1. The impulse responses for revenues and spending now measure the percent deviations from their original levels in euros.

Figure D.4a: Baseline model – revenue-based plans with at least 60% revenue measures



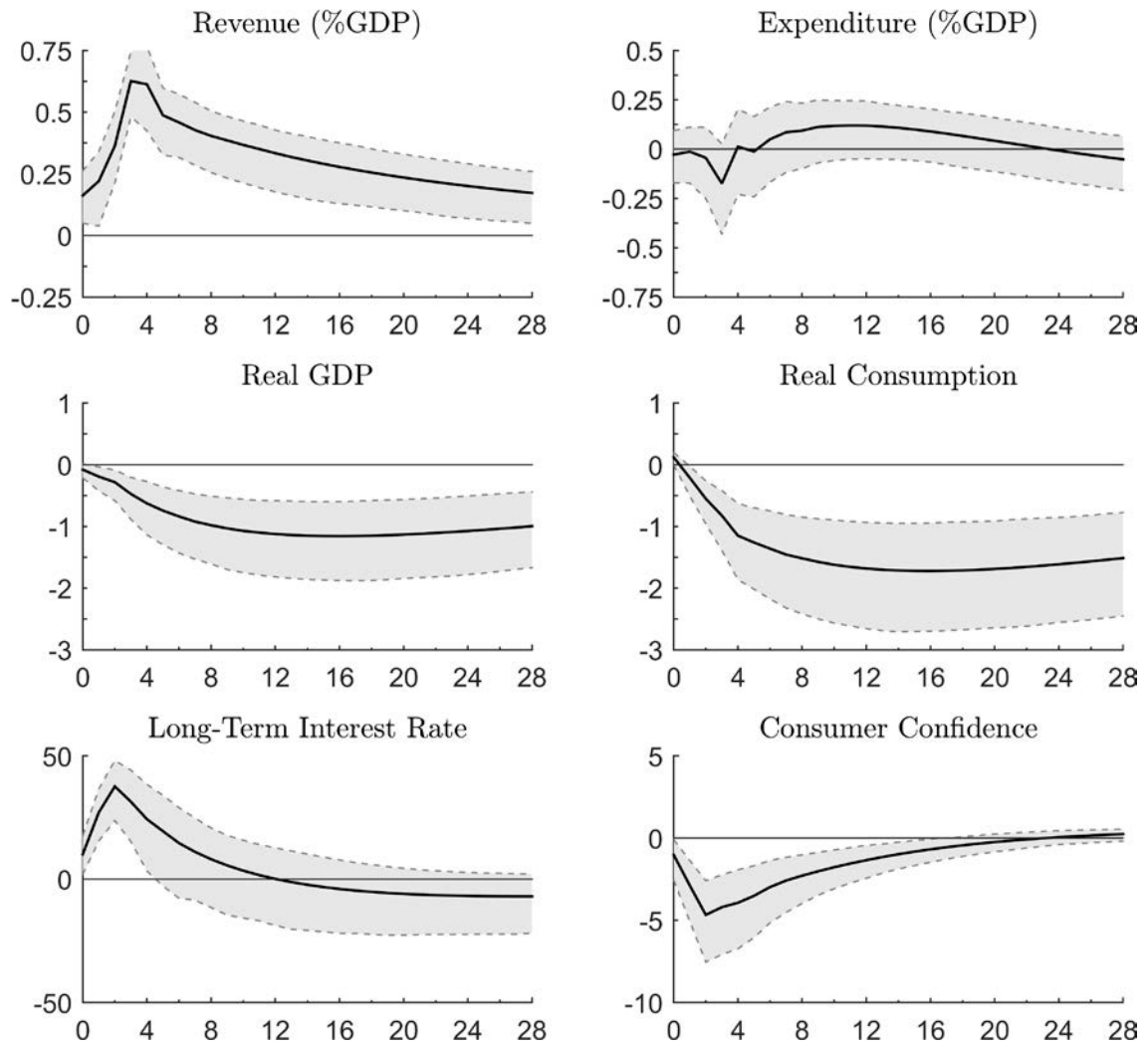
Notes: See Notes of Figure 1.

Figure D.4b: Baseline model – spending-based plans with at least 60% spending measures



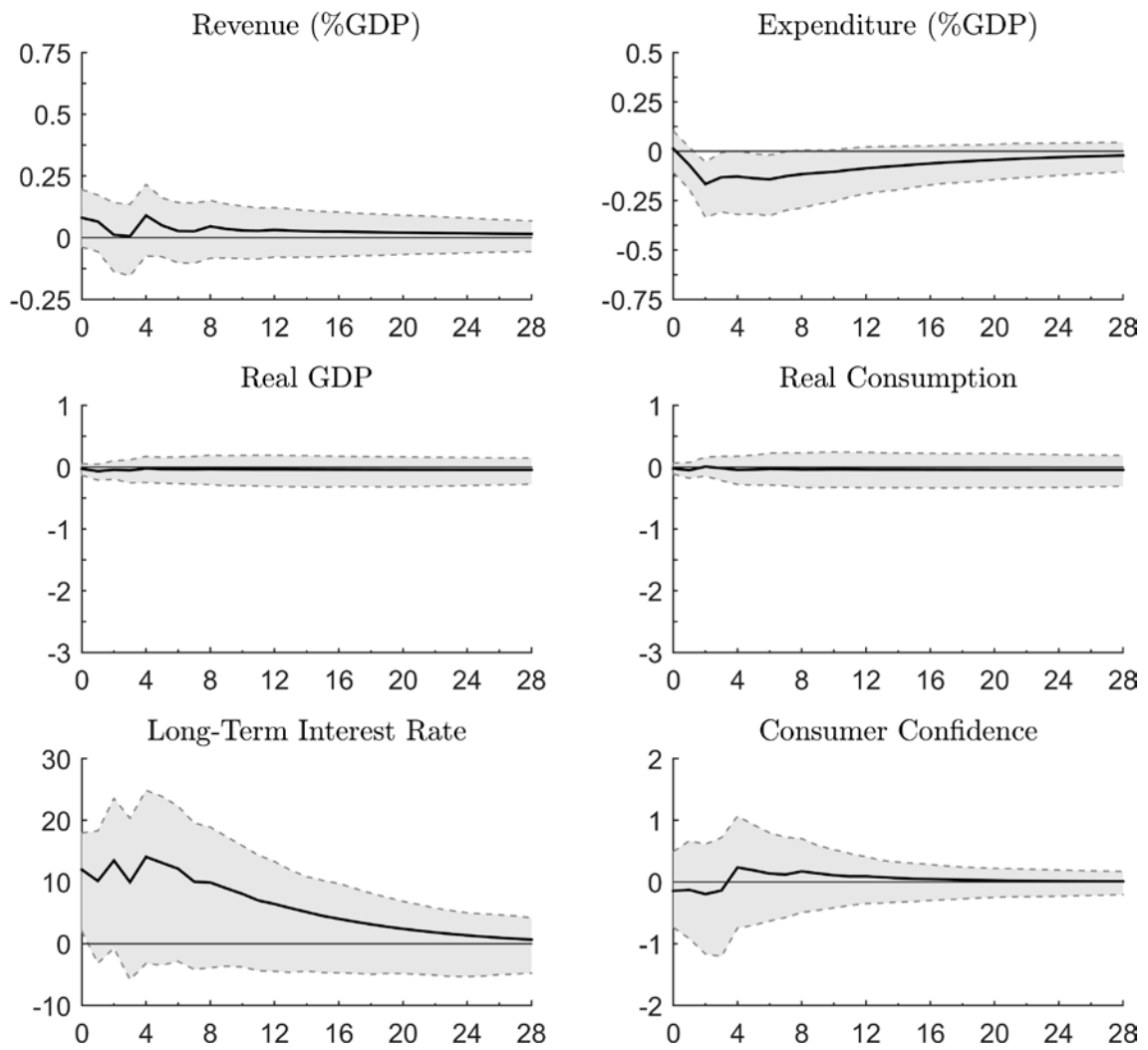
Notes: See Notes of Figure 1.

Figure D.5a: Baseline model with time fixed effects – revenue-based plans



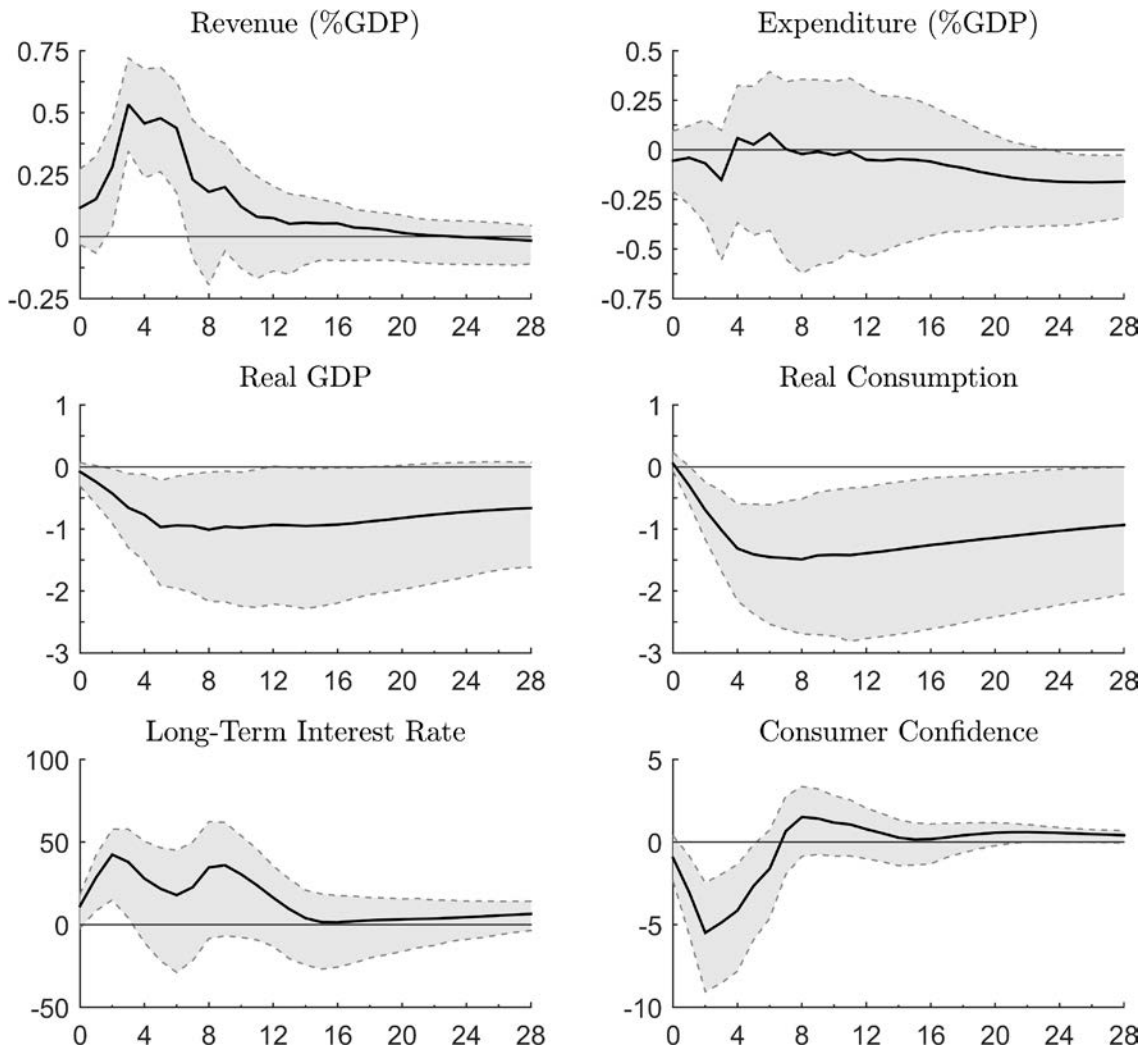
Notes: See Notes of Figure 1.

Figure D.5b: Baseline model with time fixed effects – spending-based plans



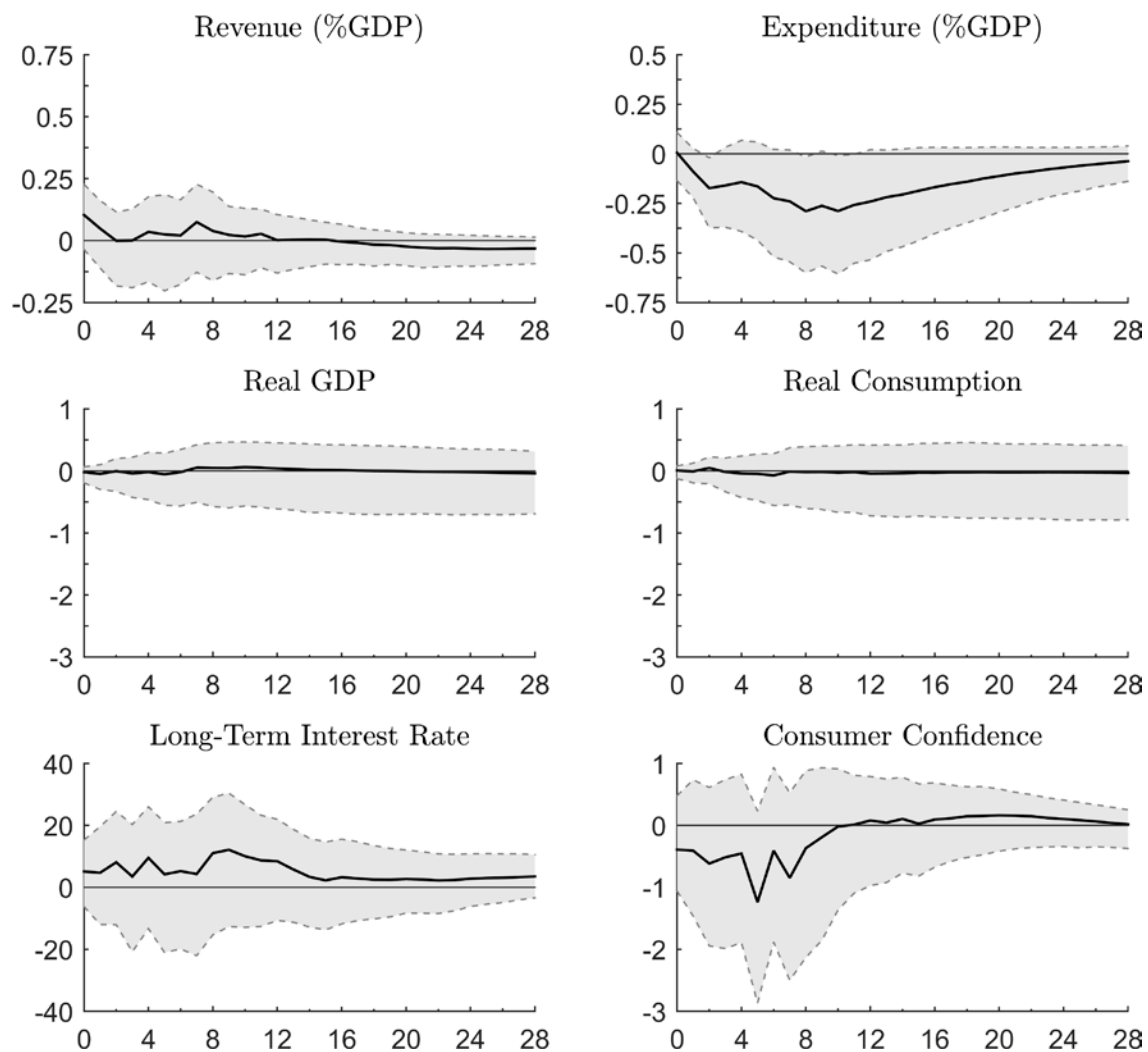
Notes: See Notes of Figure 1.

Figure D.6a: Baseline model with eight lags – revenue-based plans



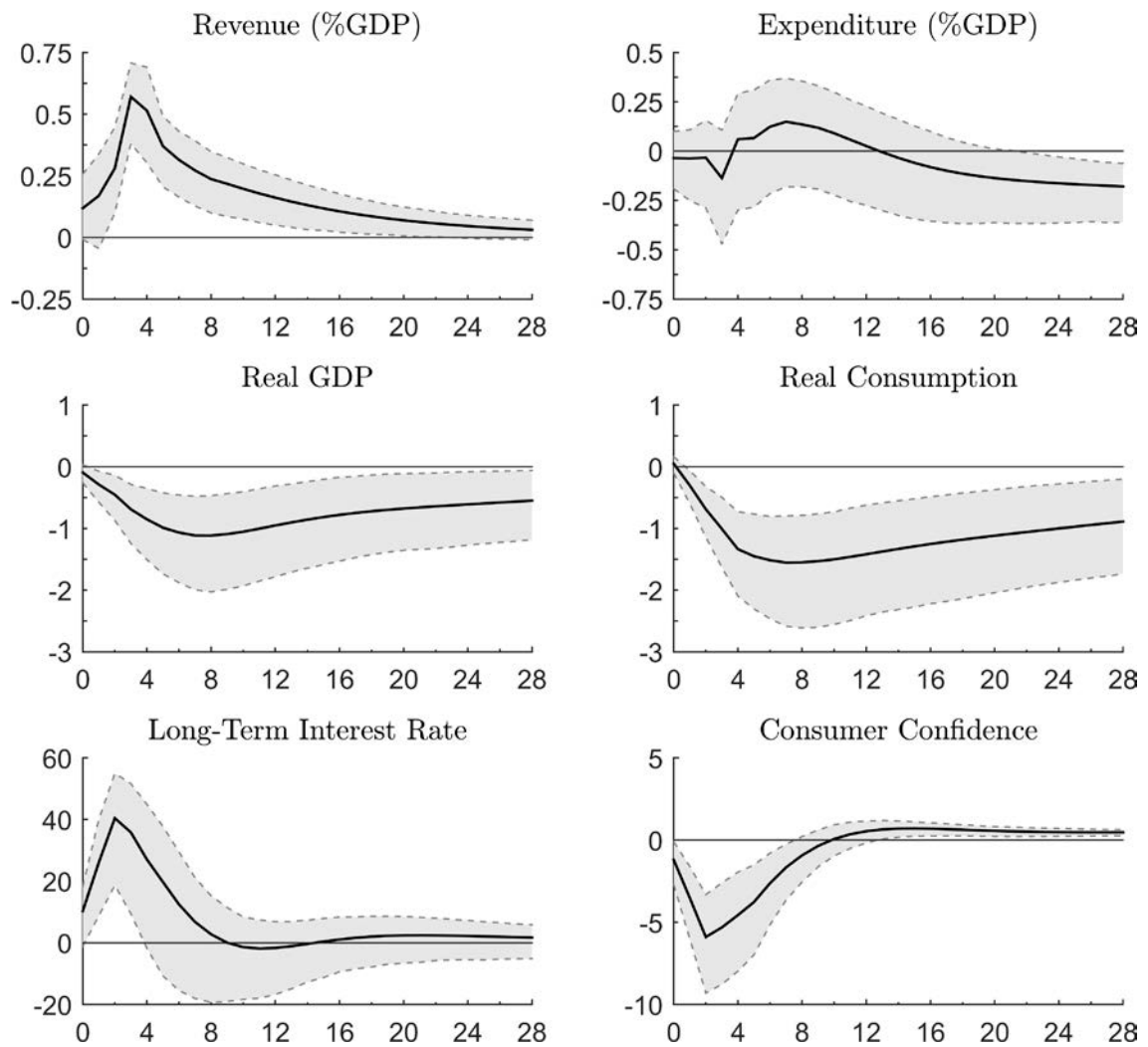
Notes: See Notes of Figure 1.

Figure D.6b: Baseline model with eight lags – spending-based plans



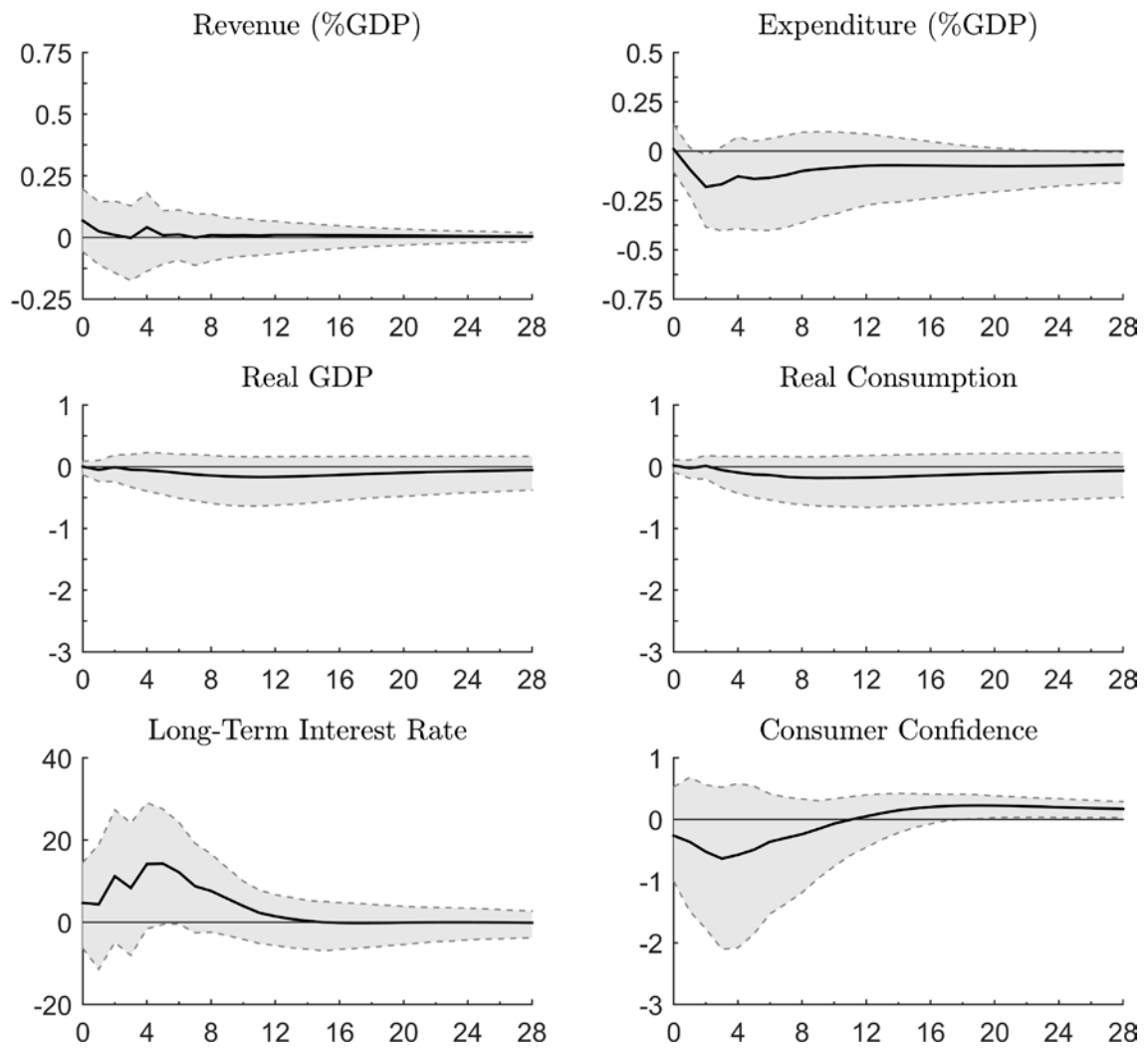
Notes: See Notes of Figure 1.

**Figure D.7a: Baseline model extended with lagged debt as exogenous variable
– revenue-based plans**



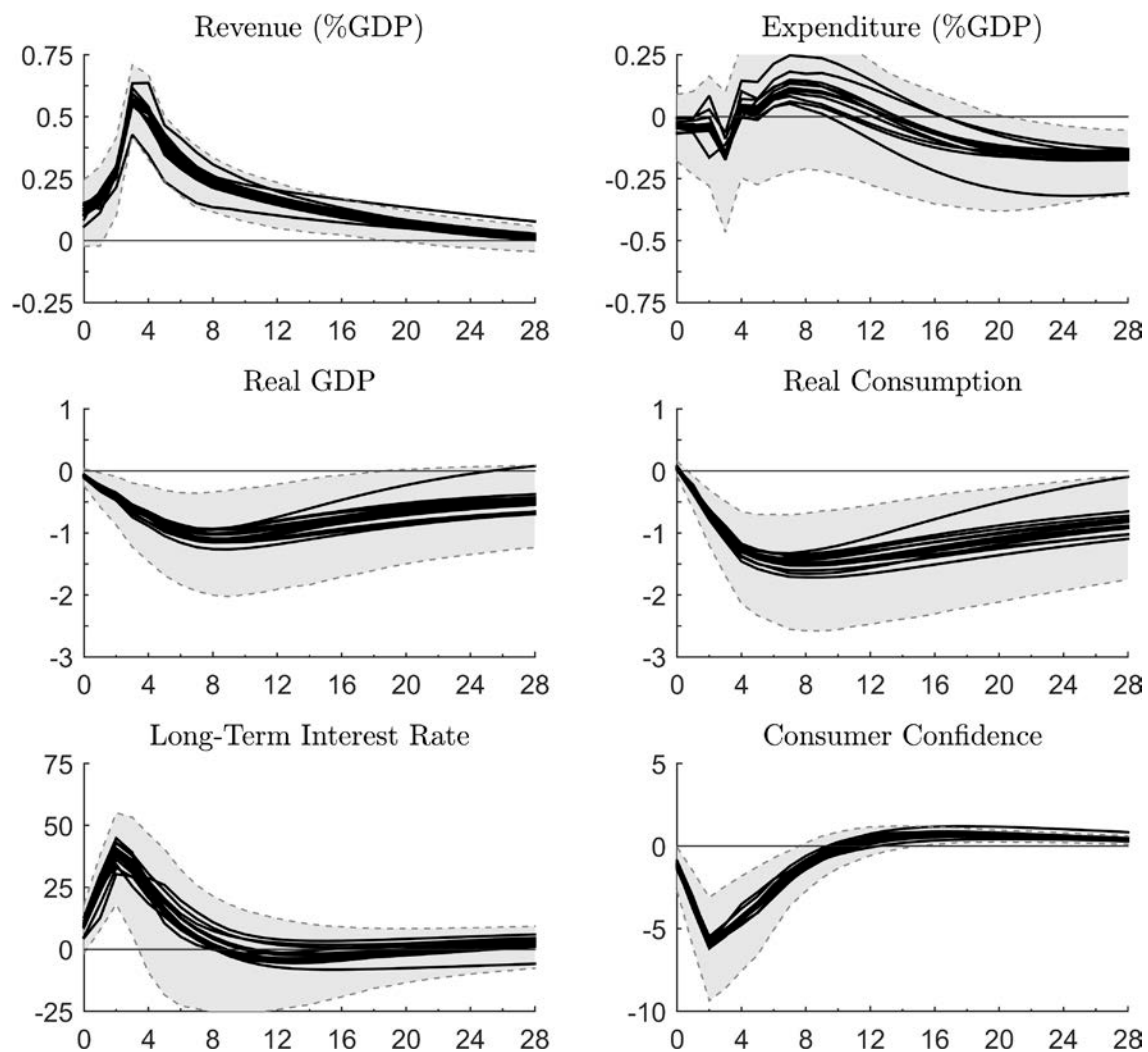
Notes: See Notes of Figure 1.

**Figure D.7b: Baseline model extended with lagged debt as exogenous variable
– spending-based plans**



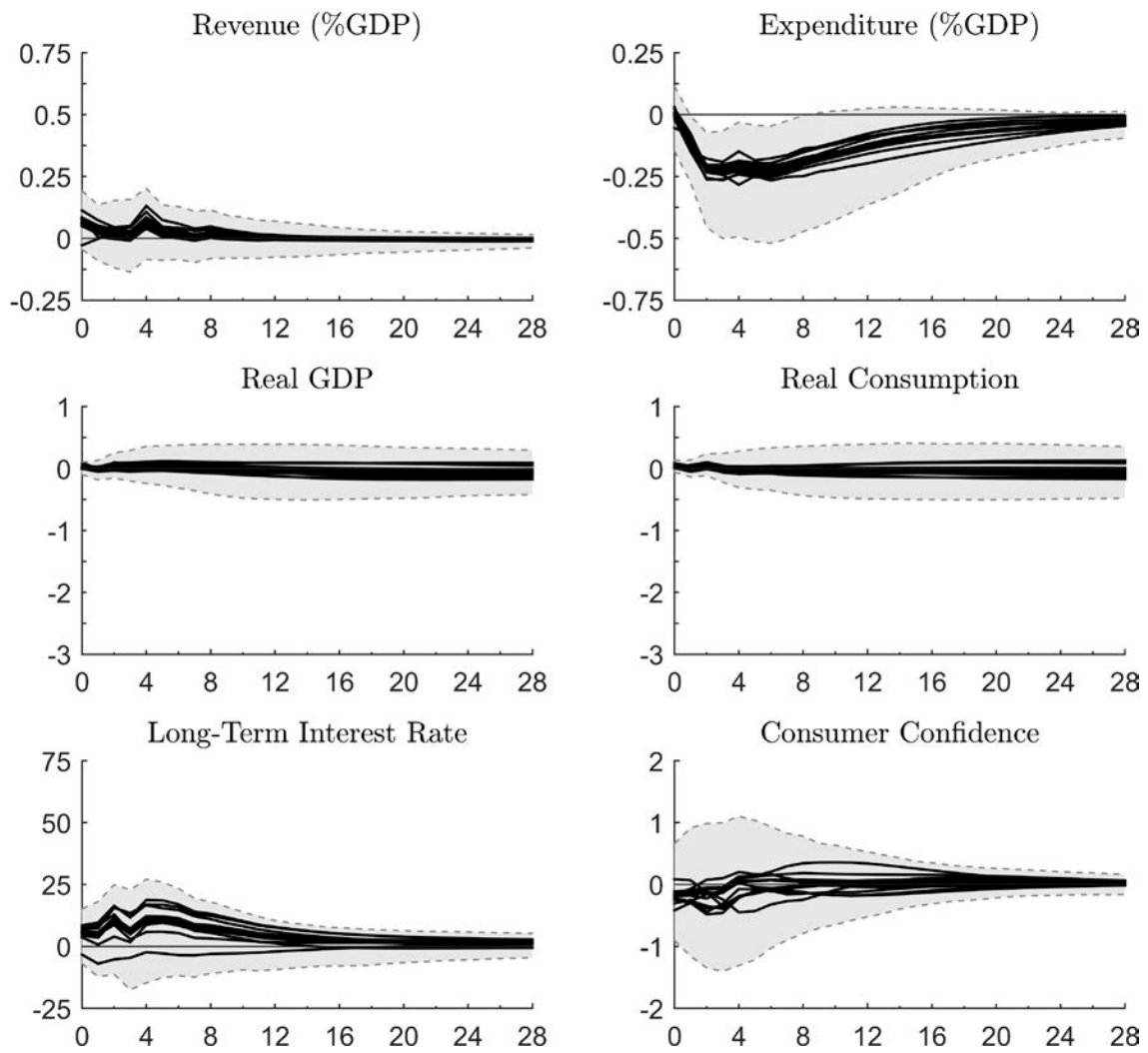
Notes: See Notes of Figure 1.

Figure D.8a: Baseline model excluding one country at a time – revenue-based plans



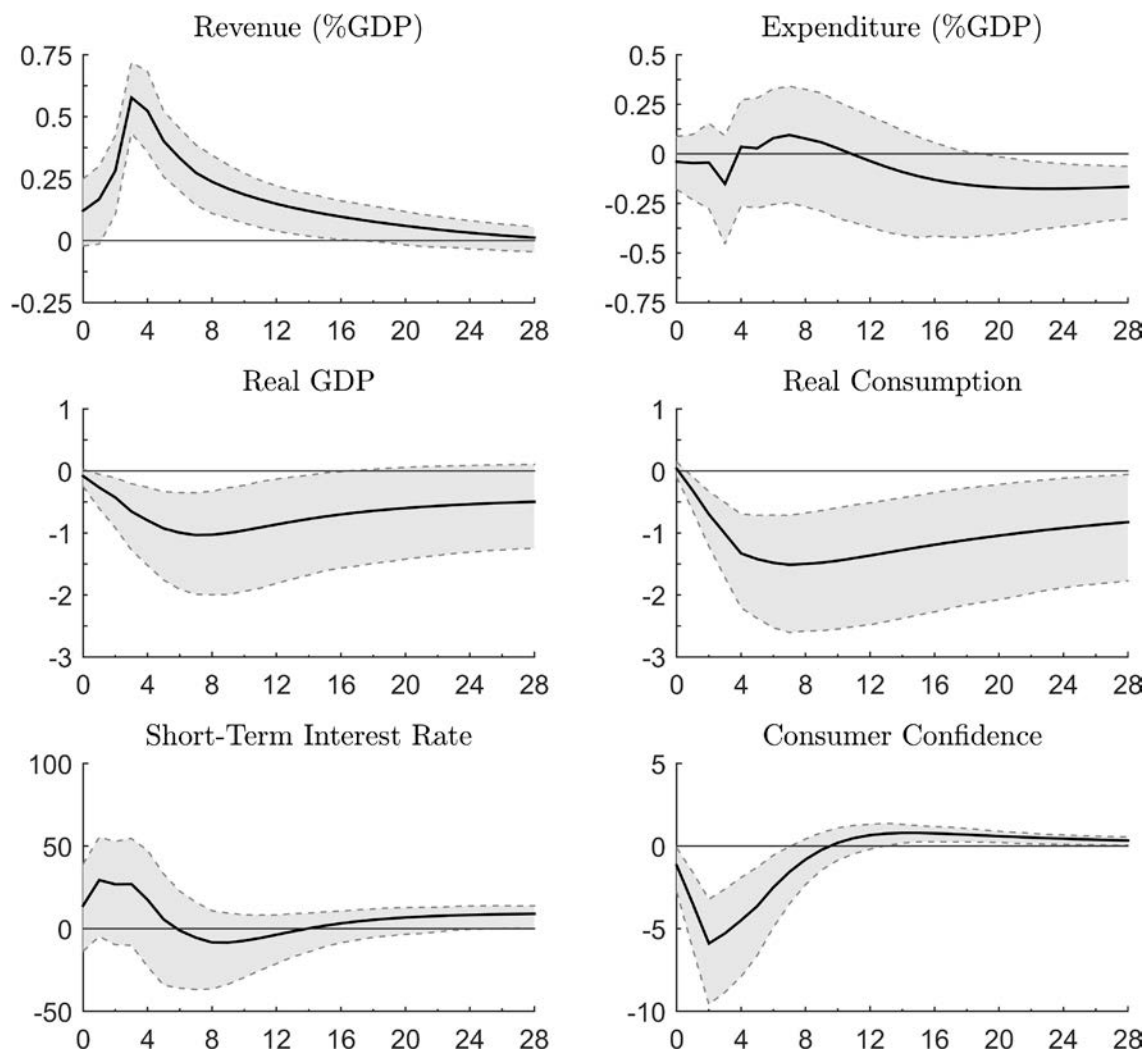
Notes: See Notes of Figure 1.

Figure D.8b: Baseline model excluding one country at a time – spending-based plans



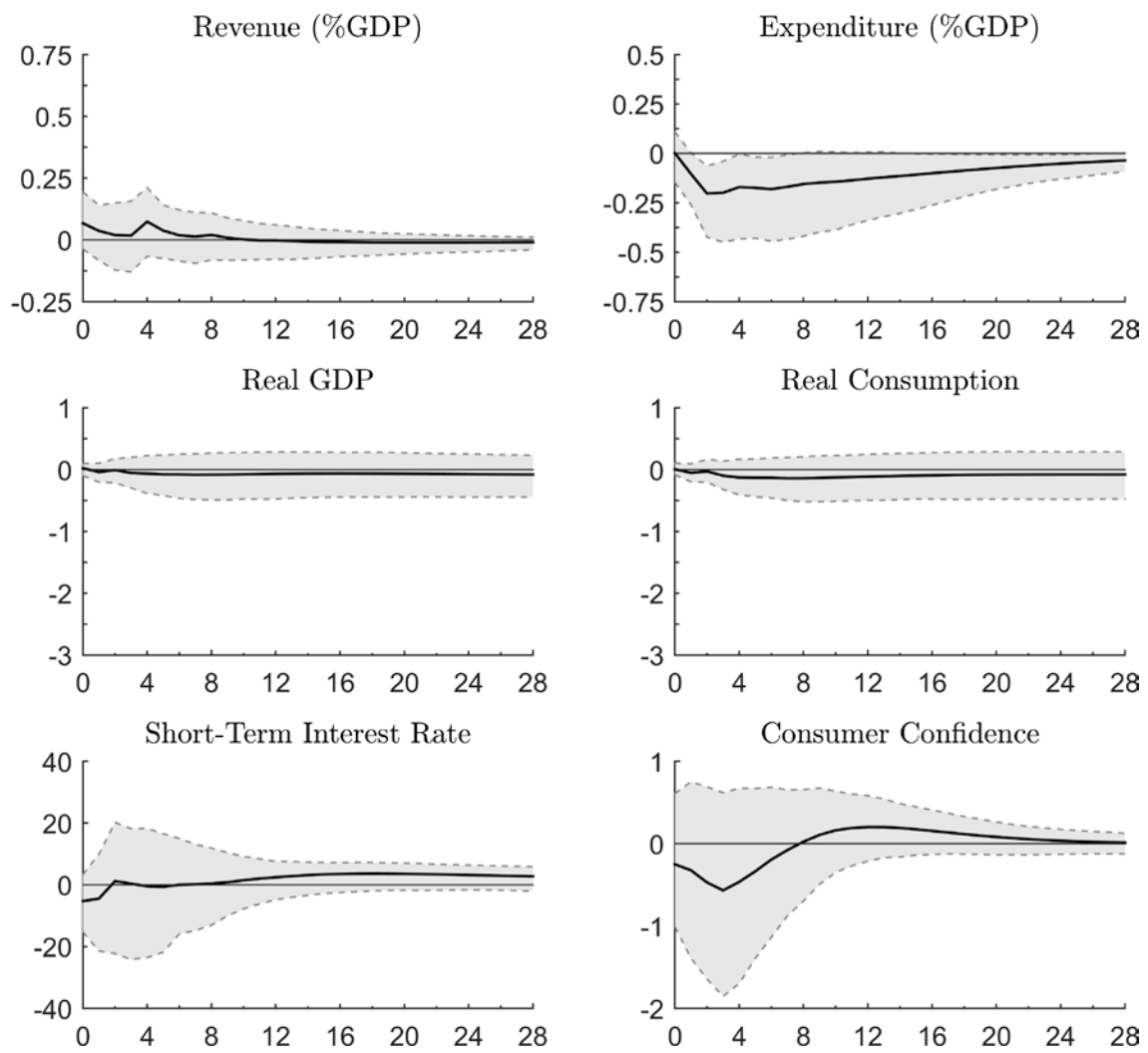
Notes: See Notes of Figure 1.

Figure D.9a: Baseline model with short-term interest rate – revenue-based plans



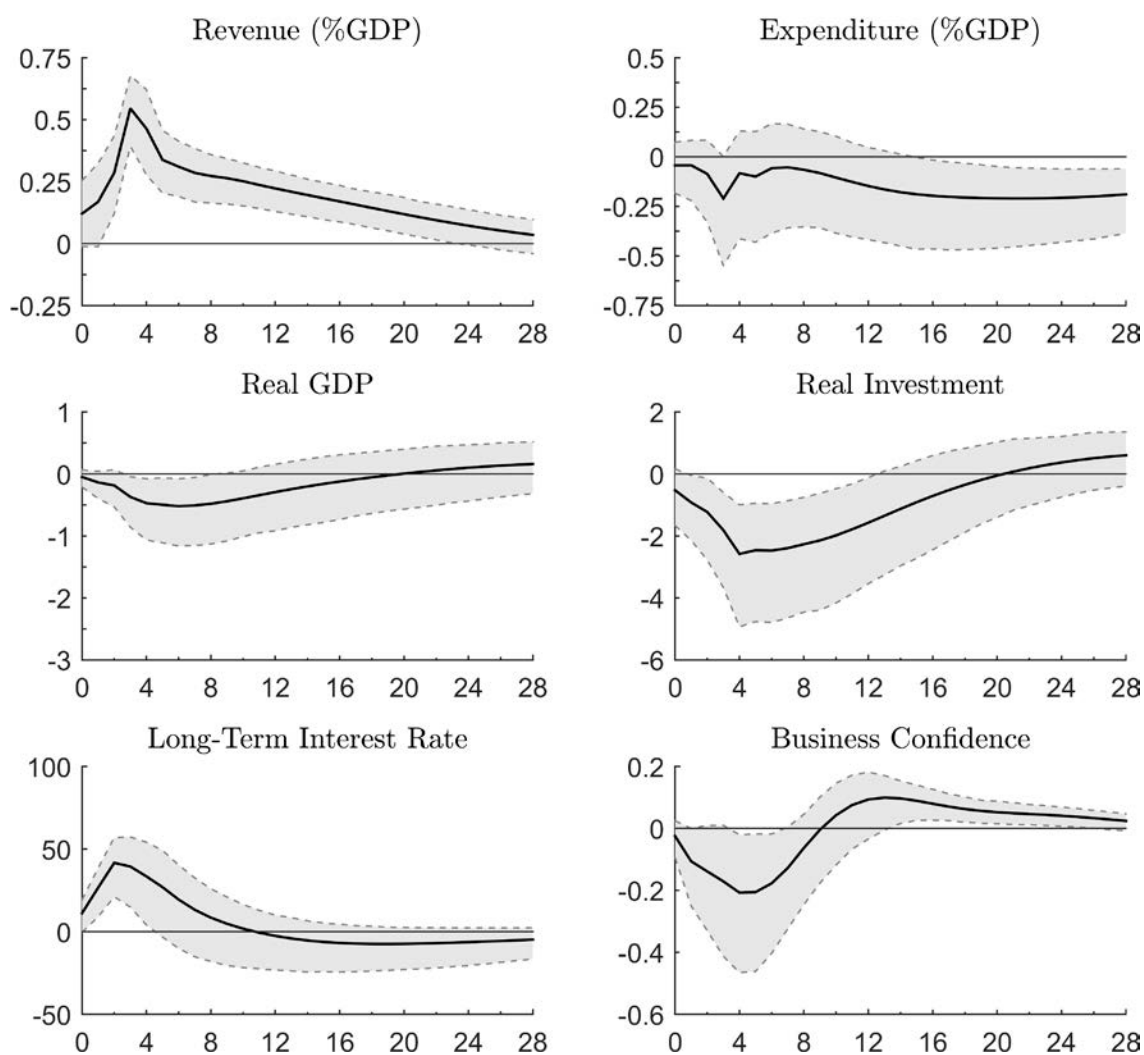
Notes: See *Notes* of Figure 1. The impulse response of the short-term interest rate is the deviation in basis points from its original value.

Figure D.9b: Baseline model with short-term interest rate – spending-based plans



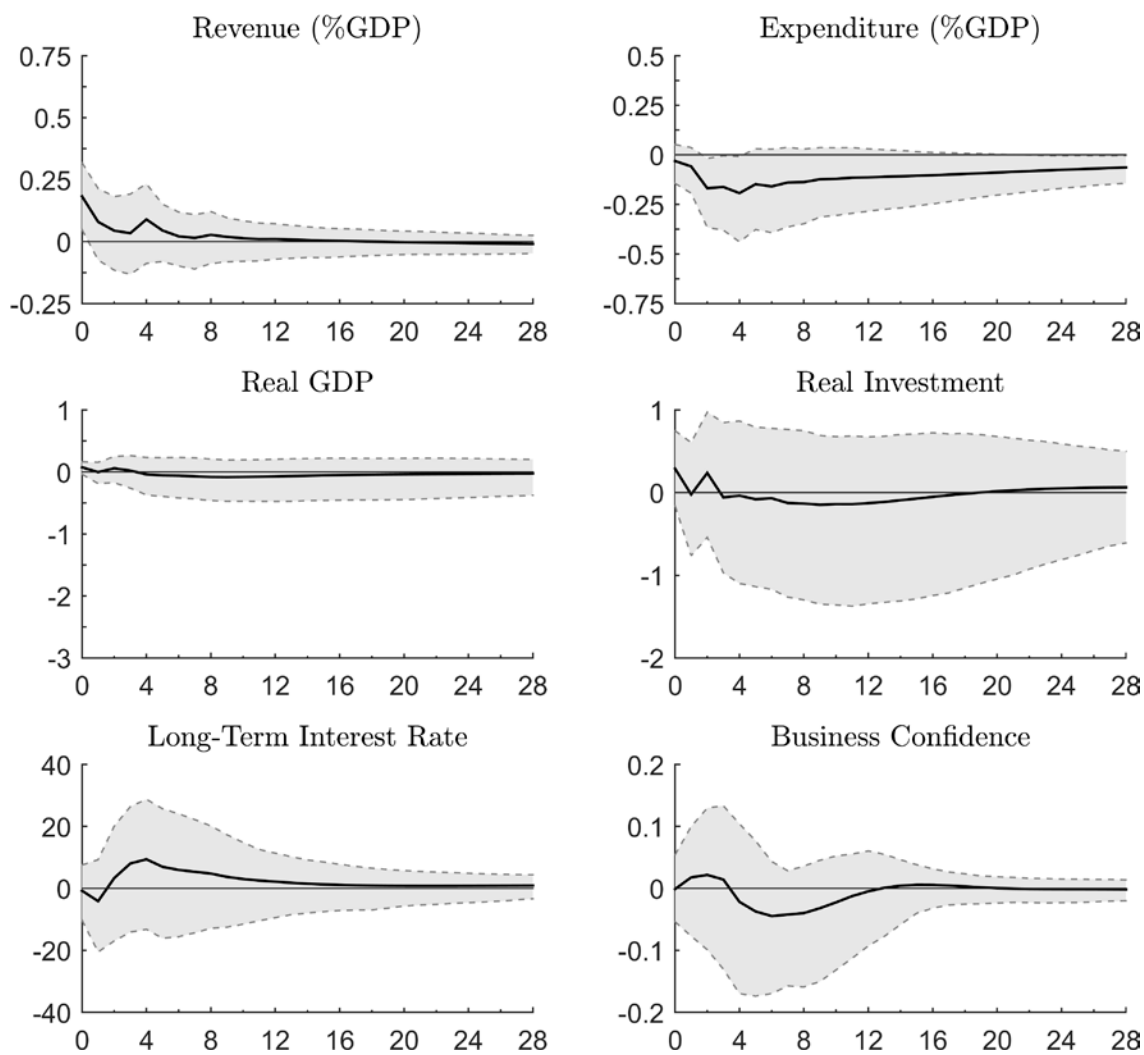
Notes: See *Notes* of Figure 1. The impulse response of the short-term interest rate is the deviation in basis points from its original value.

**Figure D.10a: Baseline model with business confidence and private investment
– revenue-based plans**



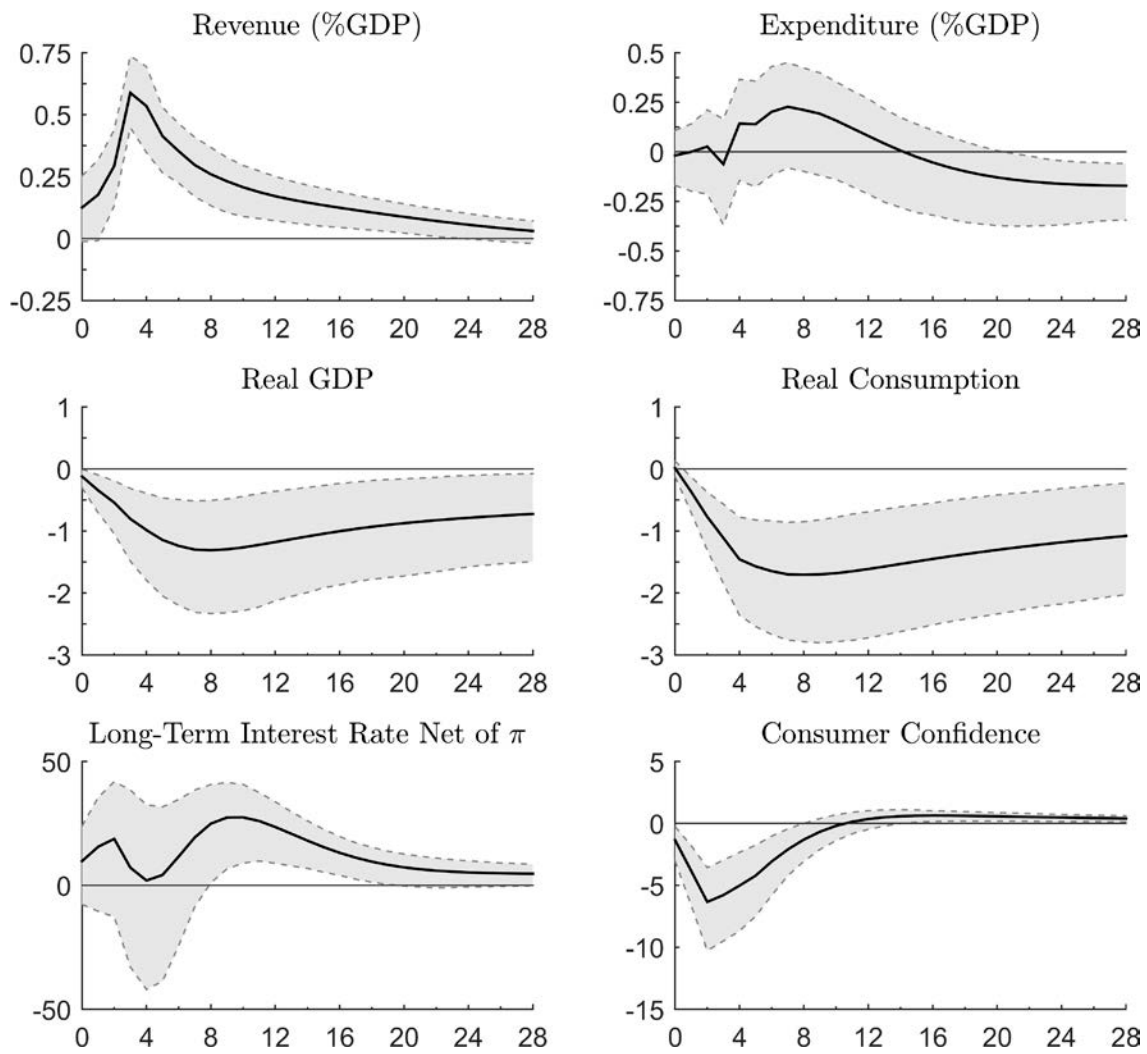
Notes: See *Notes* of Figure 1. The impulse response of business confidence is the deviation in percent from its original value.

**Figure D.10b: Baseline model with business confidence and private investment
– spending-based plans**



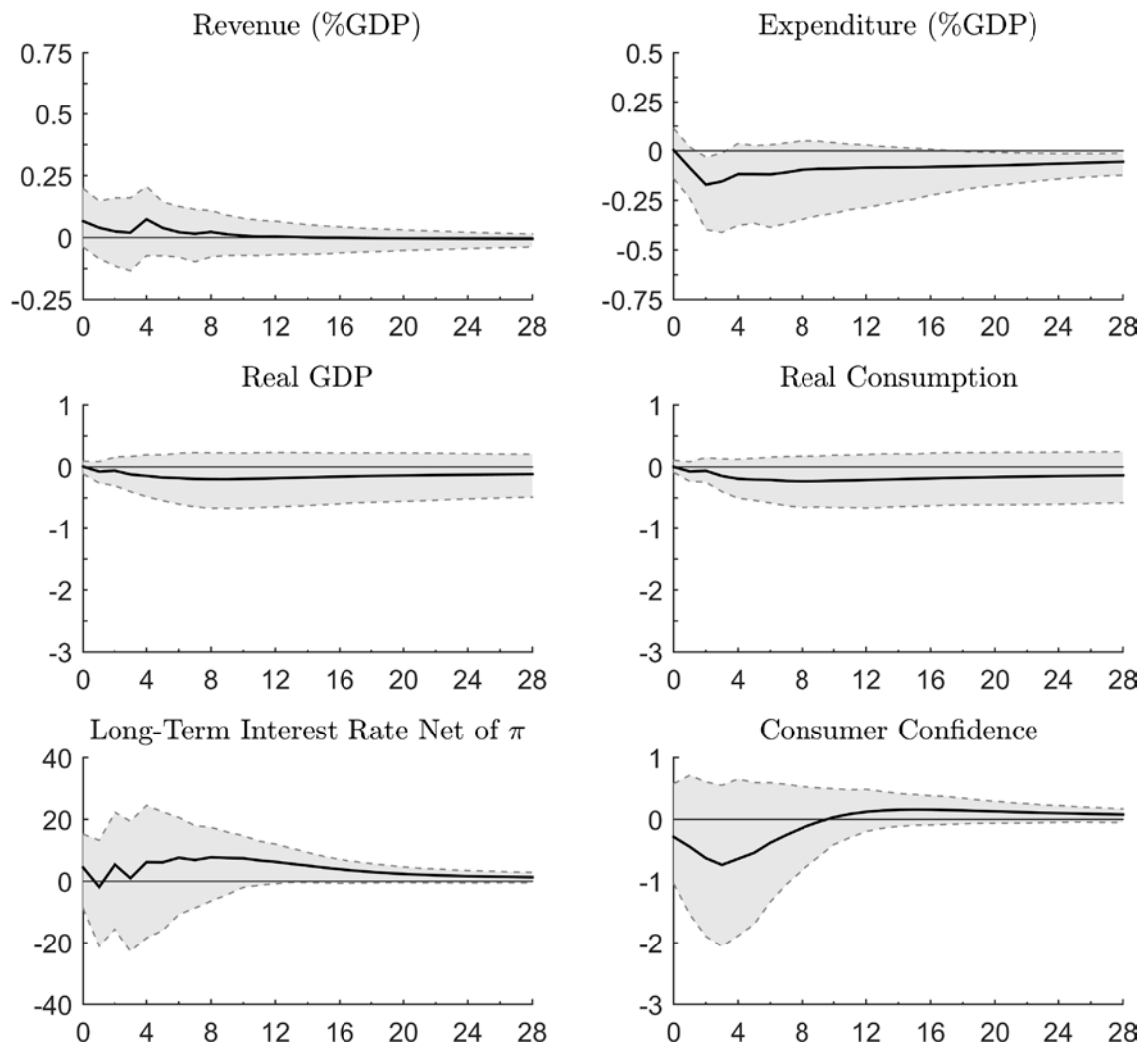
Notes: See Notes of Figure 1. The impulse response of business confidence is the deviation in percent from its original value.

Figure D.11a: Baseline model with ex-post long-term real interest rate – revenue-based plans



Notes: See *Notes* of Figure 1. The impulse response of the *ex-post* long-term interest rate is the deviation in basis points from its original value.

Figure D.11b: Baseline model with ex-post long-term real interest rate – spending-based plans



Notes: See *Notes* of Figure 1. The impulse response of the *ex-post* long-term interest rate is the deviation in basis points from its original value.

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