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REVENUE- VERSUS SPENDING-BASED CONSOLIDATION PLANS: THE ROLE OF FOLLOW-UP

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Abstract

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JEL Classification: E21, E62, H5

Keywords: fiscal consolidation, revenues, spending, Fiscal multipliers, follow-up, panel vector auto-regression, confidence

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Revenue- versus spending-based consolidation plans: the role of follow-up[1]

Roel Beetsma[2], Oana Furtuna [3], Massimo Giuliodori[4]

This version: July 5, 2017

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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? Over the 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 revenues-based consolidations are more harmful for output 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 more accommodative in the case of spending-based consolidations. Alesina *et al*. (2015a,b) propose an explanation based on the positive effect of spending-based consolidation on business confidence and private investment. More recently, Alesina *et al*. (2017) confirm the heterogeneous effects of spending- versus revenues-based consolidation plans, while controlling for monetary policy. In particular, based on 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.[5] The impact of persistent spending cuts is mitigated by wealth effects on aggregate demand.

In this paper we contribute to this literature in a number of ways. In the first part of the paper we provide supporting evidence for another channel why expenditure-based consolidations have more benign macroeconomic consequences than revenue-based consolidations. The channel may well complement other channels highlighted before. Through direct comparison with ex-post data, we show that the annual fiscal consolidation plans that are measured by using contemporaneous information tend to overestimate the actual size of the implemented consolidation measures. Most importantly, while these results are consistent with a large strand of the literature which documents large and systematic deviations of actually implemented from planned fiscal measures (e.g., Beetsma *et al.*, 2009, and Frankel and Schreger, 2013), we also show that this shortfall is on average much larger in the case of spending-based consolidation plans. Hence, a simple Keynesian framework can easily explain the differential effects on the economy as the result of differences in follow-up of the two types of consolidations. We offer two (potentially complementary) explanations supporting the larger shortfall for spending plans. The first is what we refer to as "passive" non-follow up and it results from over-optimistic output growth forecasts. For standard

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

estimates of elasticities and over-optimism in growth forecasts, we 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 bluntly not (or partially) implementing planned consolidation measures. We can rationalize the lower degree of follow-up for spending measures in a simple setting in which political resistance to planned measures is uncertain, but more likely to be prohibitive for spending than for revenue measures 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. This provides indirect support for the possibility that "active non-follow-up" explains part of the difference in follow-up.

The second part of the paper starts by presenting a newly-constructed quarterly narrative dataset of fiscal consolidation announcements for thirteen EU countries over the period 1978-2013. The dataset is based on determining as accurately as possible the moment when consolidation information becomes available. In particular, we assign an announcement shock to the month when such information becomes publicly available. We use the dataset to explore within a quarterly panel vector auto regression (VAR) the macroeconomic consequences of the two types of consolidation strategies.

This part of the paper has three objectives. First, it aims at confirming that spending-based consolidation announcements lead to less follow-up than revenues-based consolidation announcements, implying that the former have more benign macro-economic consequences than revenues-based consolidations. Second, it aims at doing so while properly accounting for potential private sector anticipation effects. Failing to do so may lead to wrong inference. In contrast to existing datasets our dataset allows us to pinpoint with a relatively high degree of precision the release of new consolidation information and thereby to account for anticipation effects that occur between the announcement and the actual implementation of the consolidation measures. In other words, we can model the response of the economy to real-time information on consolidation news. 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 constructed by 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, they do not identify the

moment of the consolidation announcement, which is critical to account for potential anticipation effects.

The third objective of this part of the paper is to take appropriate account of the confidence channel of fiscal consolidations. Awareness by the private sector of a potential lack of follow-up of consolidation announcements, and its consequences for the economy, may be reflected in the response of private sector confidence to consolidation announcements. Since confidence may react instantaneously to new information, with our dataset we are uniquely positioned to properly investigate the extent to which the asymmetry in follow-up is reflected in the credibility of fiscal announcements, as gauged by their effect on private sector confidence, and how this in turn affects the response of the economy following the announcements. We measure confidence through a consumer confidence indicator and the long-term interest rate.

Our panel VAR shows that a split into revenues- and spending-based consolidation announcements produces substantially different economic responses. Following a revenue-based announcement, GDP and consumption decline significantly and the long-term interest rate rises significantly. By contrast, after a spending-based consolidation announcement none of these variables reacts significantly. The estimates also confirm the difference in follow up: revenue-based consolidation announcements are on average followed by a strong and highly significant increase in revenues, while spending hardly moves following a spending-based consolidation announcement. The response of private sector confidence manifests itself in a significant increase in the long-term interest rate and a significant reduction in consumer confidence following a revenues-based consolidation announcement, while neither of these variables responds to a spending-based announcement. A counterfactual in which these variables are forced to remain unchanged following the consolidation announcement suggests a non-negligible role for the confidence channel in the transmission of revenue announcements. In other words, the presence of the confidence channel seems to strengthen the purely Keynesian effects of the differences in the follow-up.

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 of the plans with *ex-post* data on revenues and spending. Section 4 describes our newly-constructed quarterly dataset of announcements of fiscal consolidations. Section 5 presents the results of our panel VAR analysis. Finally, Section 6 concludes. The Appendix contains further information on the data, a simple framework able to rationalize the “active non-follow-up” and indirect evidence supporting the “active non-follow-up”. The Additional Appendix reports the figures

of our robustness tests. The Data Construction Appendix is not for publication, but will be made available via the authors' homepages.

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 shocks to revenues 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 the expenditure 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 economically less harmful than revenue-based consolidations. The literature offers several arguments 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 consolidation. A second argument is that, because they are politically more costly, resorting to spending-based consolidation provides a stronger signal by the government to the private sector that it intends to improve its financial situation (Ardagna, 2004).[6] 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.

[6] 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.

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 for EU or Eurozone countries are Beetsma *et al*. (2009), Von Hagen (2010), Pina and Neves (2011), Cimadomo (2012), Beetsma *et al*. (2013) and De Castro *et al*. (2013).[7] 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 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 the implementation during the first year since the presentation of the budget and 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 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 less for Eurozone countries that exhibit more ownership at the national level for fiscal discipline. 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 consolidation may either moderate or exacerbate the contractionary effect of the actual measures on the real economy. On the one hand, adherents of the "expansionary austerity" view claim that positive expectations effects can mitigate the contractionary effects of

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

fiscal consolidations: if private agents realize that 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) have convincingly shown 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 methodology produces different results from standard techniques. Our dataset of fiscal consolidation announcements is particularly suited to addressing such expectation 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 Brückner and Pappa, 2015). Thus, our work falls within the empirical literature on narratively identified fiscal VAR models where our external 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 the real-time fiscal consolidation measures identified by Devries *et al.* (2011) and expanded by Alesina *et al.* (2015a,b) compare to *ex-post* implemented actions. Therefore, we start by briefly describing the real-time data we use for the comparison. First, the annual fiscal consolidation measures in the dataset of Devries *et al.* (2011) are narratively selected such that their primary motivation is public finance sustainability and not a response to the business cycle. The magnitude assigned to each consolidation episode is the estimated budgetary impact of the fiscal austerity measures implemented in that given year.[8] In describing the data, the authors mention that the estimate of the budgetary impact is done contemporaneously with the release of the consolidation plans and that it reflects the "intentions and actions" of policymakers as described in the policy documents. 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 Programmes submitted by EU member

[8] The budgetary impact is measured as the change in budgetary savings accounted for by the measures included in the plan.

states; these documents contain both the forecast effects of the fiscal plans, as well as real-time estimates of the impact of the measures taken in the current or the preceding year. Therefore, in both the narrative dataset of Devries *et al.* (2011) and in its extension the observed magnitude of a fiscal consolidation represents a mixture of forecast and first-release data.

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

Concretely, we compare changes in public revenues and spending measured *ex-post* with planned consolidation measures narratively identified by the IMF using contemporaneous documents. The comparison is served best by matching as well as possible the IMF concepts of revenues and spending used in the narrative identification of the consolidation measures with the measures of revenues and spending from the OECD of which we calculate the changes *ex-post* and that we compare with the narratively identified consolidation measures. The revenue measures narratively identified by the IMF 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 transfers receipts". This is more narrow than (a subset of) our most comprehensive measure from the OECD "Total receipts, government". The spending measures narratively identified by the IMF 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 our most comprehensive measure from the OECD "Total disbursements, government". To demonstrate the robustness of our findings, we will compare the real-time narratively-identified consolidation measures with the *ex-post* changes in both our most comprehensive and narrower measures. The Appendix contains a full description of the annual budgetary data used in this section.

3.2. The narratively identified consolidations data

In this section we employ only the consolidation measures identified by Devries *et al.* (2011) and expanded by Alesina *et al*. (2015a,b) of the thirteen countries that we use in our panel VAR analysis later. For these countries, they identify a total of 211 consolidation plans. For 180 of them we were also able to establish the magnitude of their impact on the primary balance (see Table 1). 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 average horizon of the consolidation plans is between 1.3 and 2.3 years.

Table 1: 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
Total	180	1.37	0.85	0.51	1.7

Most consolidation plans combine measures on both the revenue and the expenditure side of the budget, which is why in Table 2 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 two 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 2 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 0.30% of GDP on the spending side and 0.96% 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 2: Plans according to their predominant instrument

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
Total	114	61	5	180

3.3. 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:

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

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 *f*inal 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*, 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 :

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

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 are more narrow 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).

3.4. Results of the comparison

Table 3 reports the results for revenues averaged per country over the consolidation years and taking the average over all (country, consolidation year) combinations. Table 4 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. 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 for sure 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 (0.85% of GDP) exceeds the average side of the revenues measures (0.51% 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 revenues measures. Based on the latter the average size of the spending shortfall should be 0.25% of GDP, half of the most favorable actual number.

Looking at the individual countries, where we average per country 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 revenues and spending measures we register a higher fractions of short-falls, but the spending measure is always characterized by weaker follow-up than the corresponding revenues measure.

Table 3: 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)
Average	-0.154 (108)	-0.146 (108)	-0.184 (108)	-0.173 (108)

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 of observations over which the overall average is calculated is determined by the maximum common availability over all measures. Hence, this number may differ from the sum of the numbers of observations across all the countries. (v) The number in brackets is the number of consolidation observations per country.

Table 4: 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)
Average	0.501 (111)	0.768 (111)	0.684 (111)	0.645 (111)

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 3.

3.5. Explanations for differences in follow-up

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

<u>"Passive" non-follow-up</u>

A first explanation are over-optimistic GDP growth forecasts 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 ($h = t-1$), 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 $\left(X_t^f / Y_t^f\right) - \left(X_t^{t-1} / Y_t^{t-1}\right)$, for $X = T, G$. In Beetsma *et al.* (2013) we show that this expression can be decomposed into three main components: a "base effect", a "growth effect" and a "denominator effect" according to the following formulation:[10]

$$\left(\frac{X_t^f}{Y_t^f} - \frac{X_t^{t-1}}{Y_t^{t-1}}\right) = \underbrace{\frac{1+x_t^{t-1}}{1+y_t^{t-1}}\left(\frac{X_{t-1}^f}{Y_{t-1}^f} - \frac{X_{t-1}^{t-1}}{Y_{t-1}^{t-1}}\right)}_{\text{base effect}} + \underbrace{\frac{X_{t-1}^f / Y_{t-1}^f}{\left(1+y_t^f\right)\left(1+y_t^{t-1}\right)}\left(x_t^f - x_t^{t-1}\right)}_{\text{growth effect}} - \underbrace{\frac{X_{t-1}^f / Y_{t-1}^f}{\left(1+y_t^f\right)\left(1+y_t^{t-1}\right)}\left(y_t^f - y_t^{t-1}\right)}_{\text{denominator effect}} \tag{3}$$

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

[10] In this version of the decomposition we ignore the residual effect which 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*. Under our assumption that now-cast estimates are equal to *ex-post* figures, the base effect in equation (3) is zero.[11] This simplification allows us to focus exclusively on the accounting effects of the over-optimism of GDP growth forecasts on the *ex-post* fiscal deviations. 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 $x_t^{t-1} = \varepsilon_X y_t^{t-1}$ (with $X = T, G$).

Frankel (2011) finds that the average output growth bias 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 and 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^{t-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)] ≈ -0.20 (growth effect) *minus* [0.39/((1+0.045)(1+0.050))](-0.5) ≈ -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)] ≈ 0.03 (growth effect) *minus* [0.39/((1+0.045)(1+0.050))](-0.5) ≈ -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 of spending reductions from plans than of revenues increases from plans, which can explain a non-negligible fraction of the empirically-observed average difference in the shortfalls.

[11] Beetsma *et al*. (2013) provide estimates of the base effect. Using these estimates, and the parameter values assumed below, we can back out a rough estimate of the component $\left(X_{t-1}^f / Y_{t-1}^f\right) - \left(X_{t-1}^{t-1} / Y_{t-1}^{t-1}\right)$ in (1) for *h*=*t*-1. We find that this component can account for about 0.02 – 0.03 percentage points of GDP of the difference in follow up between expenditures and revenues. The calculations are available upon request.

"Active non-follow-up"

Our second explanation concerns the "active non-follow-up", which refers to the possibility that announced consolidation measures are carried out only partially or are not carried out at all. The Appendix 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 formed, 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 evidence provided earlier in this section.

The Appendix 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 of 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. The Appendix 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 plan to raises 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 thirteen EU countries over the period 1978 – 2013. The countries in our sample are Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, the Netherlands, Portugal, Spain, Sweden and the United Kingdom. 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 a number of EU countries over this period. We in turn expand the data on narratively-identified consolidations further with Finland, the Netherlands and Sweden for the period 2009 – 2013, as for the period 1978-2008 these countries were already present in the Devries *et al*. (2011) dataset. 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 using the same methodology. The idea is that by carefully studying the motivation of each consolidation plan, one can single out the plans 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 plans 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, 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. The Appendix 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.[12] By aggregating the monthly announcements to the quarterly frequency, we ameliorate potential

[12] 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.

anticipation effects. To alleviate any potential anticipation effects further, we assign the announcement made in the first month of a quarter to the preceding quarter.[13]

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.[14] Hence, the assigned value to the announcement is only an estimate of the pure shock value of the consolidation plan. Nevertheless, reporting a value has a substantial advantage over merely reporting a simple dummy for a fiscal 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.

5. The panel vector auto regression (VAR) analysis

In Section 3 we have documented that follow-up is weaker for spending-based consolidation plans than for revenues-based consolidation plans. Employing a Keynesian setting, such follow-up differences can help to explain why the macro-economic consequences of spending-based consolidations are generally perceived to be more benign than those of revenue-based consolidations. This section investigates within a panel vector auto regression (VAR) the macroeconomic consequences of the two types of consolidation strategies. The purpose of this

[13] 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.

[14] 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.

section is three-fold. First, we want to confirm that also for our new dataset spending-based consolidation announcements lead to less follow-up than revenues-based consolidation announcements, and, hence, that spending-based consolidations have more benign macro-economic consequences than revenues-based consolidations. Second, we want to do this while taking 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 in anticipation of the actual execution of the plans, real variables, like private consumption, already adjust *ex ante*. Not taking account of such anticipatory behavior may lead to incorrect inference (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. In particular, and this brings us to the third objective of this section, our data enable us to capture the effects of announced plans on variables that can react instantaneously to new information on consolidation activity. This is specifically the case for confidence variables, which many commentators believe to play an important role in the transmission of fiscal consolidations. Awareness by the private sector of a potential lack of follow-up, and its consequences for the economy, may be reflected in the response of private sector confidence to consolidation announcements. Our data allow us to estimate the role of the confidence channel in the transmission following consolidation announcements.

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 lth lag. From the formula, we suppressed the exogenous explanatory variables, in particular the country-specific time trends, seasonal dummies and country fixed effects 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.[15] With the exception of the long-term interest rate, all series are expressed in logarithms and multiplied by 100 to facilitate the interpretation of the coefficients as percentage changes. In the case of interest rates, coefficients represent changes in basis points. 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, again the definitions of revenues and expenditures should correspond as closely as possible to the potential sets of measures included in the revenues respectively expenditure parts of fiscal consolidation. This implies in particular that $g_{i,t}$ will include transfers, hence $g_{i,t}$ 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 for 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

[15] The Appendix describes the quarterly budgetary and macroeconomic variables we use in this section.

invertible (Leeper *et al*., 2013), leading to imprecise inference. 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 according to the plan. Alesina *et al.* (2015a,b) try to distinguish between the implementation of anticipated and unanticipated measures. However, news about anticipated measures has been released earlier and 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 pinpointing austerity announcements to the moment of the 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.[16] We therefore estimate our panel VAR model using a Cholesky decomposition with the fiscal consolidation announcements ordered first, which allows 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.[17] The ordering of the other variables in the VAR is irrelevant.[18]

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. We

[16] 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.

[17] We have also tried ordering the announcement variable last in the VAR. The impulse response are similar to those reported below for the ordering 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.

[18] 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.

normalize the consolidation announcements to have an impact of 1% of GDP on the primary balance. The impulse responses can be interpreted as percentage changes in the endogenous variables in response to a consolidation announcement of this size. Finally, using standard bootstrapping with 1,000 replications we construct 90% confidence bands around the mean impulse responses. To economize on space, in our figures below we do not show the impulse responses of the consolidation announcement itself.

Figure 1 reports the baseline responses when all consolidation announcements are included. The shock, which takes place in period 1, is a consolidation announcement of 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 point to potential Keynesian effects on the economy. Indeed, we observe a maximum fall in GDP of around 0.2% that is close to 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. The responses of output, consumption and consumer confidence raise doubts about the "expansionary austerity" hypothesis.

Next, we split the announcements 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. 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 spending itself, none of the responses under spending-based announcements become significant.[19] 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 less than half the rise in revenues over GDP under a revenue-

[19] 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.

based consolidation announcement, even though in both cases the announcement is normalized to one percent of GDP. Both indicators also take some time to return to their long-run level, although they do return faster to their equilibrium than real GDP and consumption do.

5.4. Robustness of the baseline

We investigate the robustness of our baseline estimates in various ways. First, Figures AA.1a and AA.1b in the Additional Appendix report the responses for the revenues- and spending-based plans when the (relevant) variables are expressed as shares of potential output. The baseline results are both qualitatively and quantitatively essentially unchanged. Second, Figures AA.2a and AA.2b in the Additional Appendix 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. Third, this is also the case if we include a time dummy for each quarter in the sample – see Figures AA.3a and AA.3b in the Additional Appendix – or allow for eight instead of four lags in the panel VAR – see Figures AA.4a and AA.4b in the Additional Appendix. Fourth, 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 AA.5a and AA.5b in the Additional Appendix 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 AA.6a and AA.6b in the Additional Appendix 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 main our 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, the Bundesbank in the period before EMU 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 AA.7a and AA.7b in the Additional Appendix show that the impulse responses are qualitatively and quantitatively essentially unaffected, except that the response of the short-term interest rate in response to a revenue-based consolidation is insignificant and smaller than the baseline response of the long-term interest rate. It would be hard to argue that the observed change in the short interest rate following a revenue-based consolidation announcement is the reflection of a monetary policy response: if anything, we would expect monetary policy to become looser to avert the slow-down of the economy induced by the revenue-based consolidation announcement.

An alternative to the current baseline would have been a baseline with business confidence instead of consumer confidence. However, we have fewer observations on business confidence than on consumer confidence, hence we would lose precision in comparison to our current baseline. Including business confidence along with consumer confidence would require estimation of even more parameters, hence a further loss of precision. 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 our final 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 AA.8a and AA.8b in the Additional Appendix. 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.

5.5. An investigation into the role of the confidence channel

The literature, as well as many policymakers, have emphasized the role of confidence in the transmission of the effects consolidation announcements. In particular, as documented in Section 2, it is sometimes argued that the confidence effects of consolidations can be so beneficial for the economy that they may actually become expansionary. In contrast to other narrative fiscal datasets, our dataset of consolidation *announcements* offers a unique opportunity to properly explore the effects of (intended) consolidations 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 be missed or only be partially accounted for in existing datasets that fail to record the new information when it actually becomes available.

The above impulse response figures showed that the long-term interest rate, as an indicator of financial market confidence, and consumer confidence rise, respectively fall, in response to announcements of revenue increases, while they stay put in response to announcements of spending reductions. Hence, financial market and consumer confidence behave in a consistent manner *vis-à-vis* each other. Nevertheless, even though we interpret the long-term interest rate as a measure of confidence by the financial markets, a consolidation announcement may also affect the long-term interest rate through its 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. 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. We would like to convincingly rule out the possibility that the response of the long-run interest rate is driven by inflation. Hence, ideally one would purge the long-run interest rate of expected inflation (over the lifetime of the long-term debt). However, while we have survey measures on expected inflation, these measures only indicate whether private agents expect inflation to go up or down. Unfortunately, we do not have a proper quantification of expected inflation from the surveys and, hence, this route cannot be pursued. Hence, we redo the baseline regressions by replacing the long-run interest rate with the difference between the long-run interest rate and CPI inflation, i.e. the *ex-post* long-run real interest rate. The impulse responses are found in Figures AA.9a and AA.9b in the Additional Appendix. We observe that our baseline results are unaffected.

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 nor consumer confidence are affected, in line with fact that the state of the economy does not change, while in the case of revenue consolidations, both financial market and consumer confidence deteriorate in line with the anticipated deterioration of the economy. This suggests that the confidence channel actually strengthens the "traditional" Keynesian channel.

We investigate the strength of the confidence channel by conducting a counterfactual in which we shut the channel off by imposing both the long-term interest rate and consumer confidence to remain at their original values following the announcements – see Figures 3a and 3b. We observe that the deteriorations in real GDP and consumption following a revenues-based announcement are

substantially reduced, which suggests that the confidence channel plays a role in the transmission from the announcement shock to the real economy.

Overall, we can conclude that systematic differences in follow-up are necessary to account for the differences in the course of the economy following the two types of fiscal consolidation announcements. We also find that the presence of the confidence channel seems to strengthen the differences in macro-economic consequences resulting from the differences in the follow-up. Hence, our results are consistent with those of papers that find that confidence effects can amplify the negative effects of consolidations. This paper adds to this literature by demonstrating that confidence effects are likely to be important, but only when consolidation announcements are sufficiently credible.

6. Conclusions

Existing literature shows that narratively-identified spending-based consolidations have milder effects on the economy than revenue-based consolidations. This paper adds to this literature by providing evidence that this finding may at least be partly explained by weaker follow-up of plans to cut spending than of plans to raise revenues. A standard Keynesian framework would then predict that plans to cut spending are followed by more benign effects on the economy than plans to raise revenues. We have 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 follow-up. 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.

In the next step we constructed a narrative dataset on fiscal consolidation announcements in order to explore how the macro-economy reacts to announcements of fiscal consolidations. The advantage of this dataset is that we can model the response of the economy to real-time information on consolidation news, allowing us in particular to take account of the confidence channel of consolidations, which has recently received substantial attention from policymakers and academic researchers. Indeed, using a panel VAR we find for the full set of announcements robust evidence of a fall in GDP, private consumption, and a rise in the long-term interest rate following an announcement. A split into announcements of revenues- and spending-based consolation plans reveals that the aforementioned responses are concentrated among the revenue-based plans. The panel VAR also confirms that consistent with the stronger Keynesian responses of the economy to revenue announcements the follow-up of announced revenue increases is better than the follow-up

of announced spending reductions. In addition, the presence of the confidence channel seems to strengthen the differences in macro-economic consequences resulting from the purely Keynesian effects of the differences in the follow-up. In other words, this paper suggests that confidence effects are important, but only when consolidation announcements are sufficiently credible.

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Figures:

Figure 1: Impulse responses baseline model – all announcements

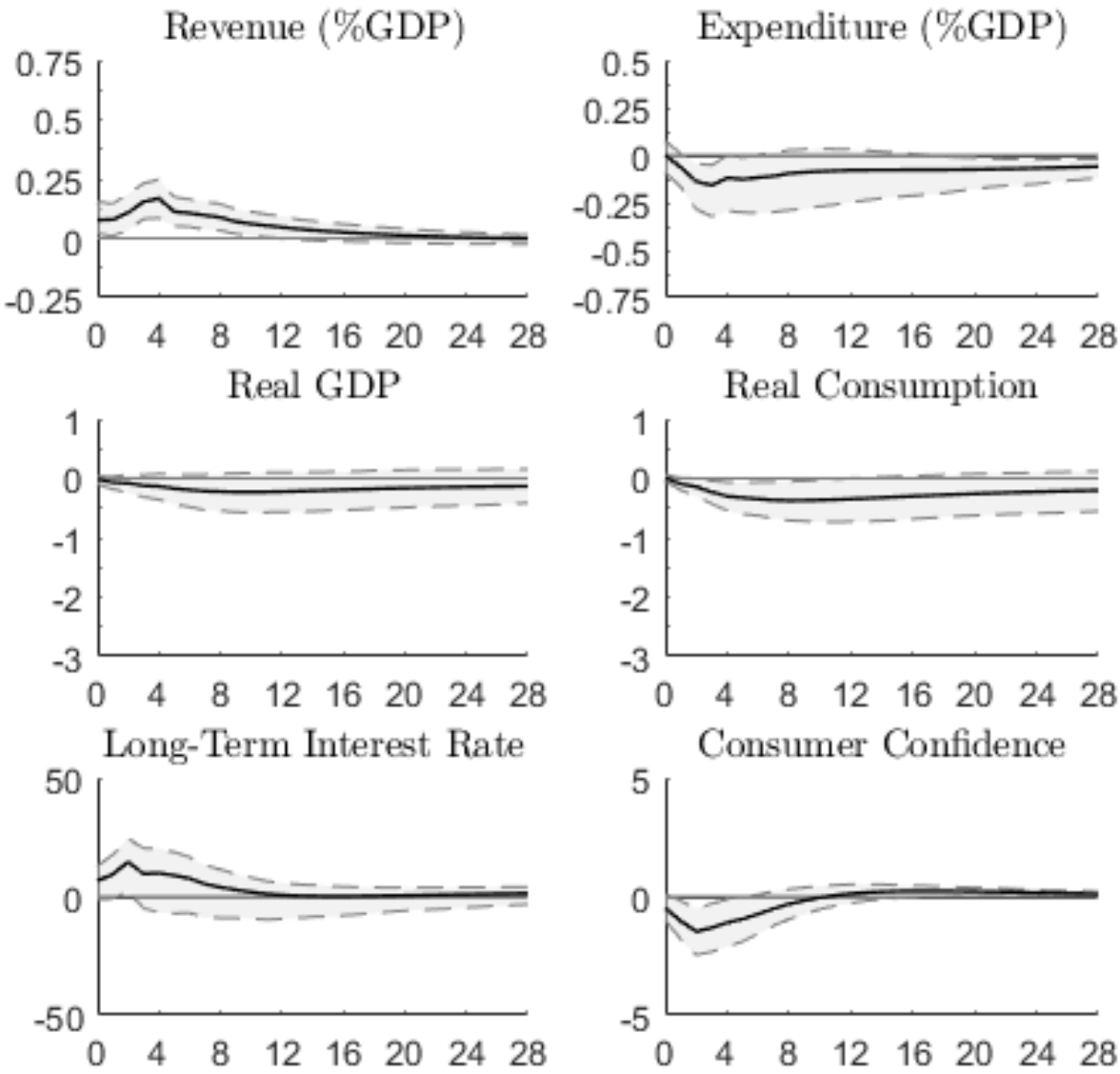


Figure 2a: Impulse responses baseline model – revenue-based plans

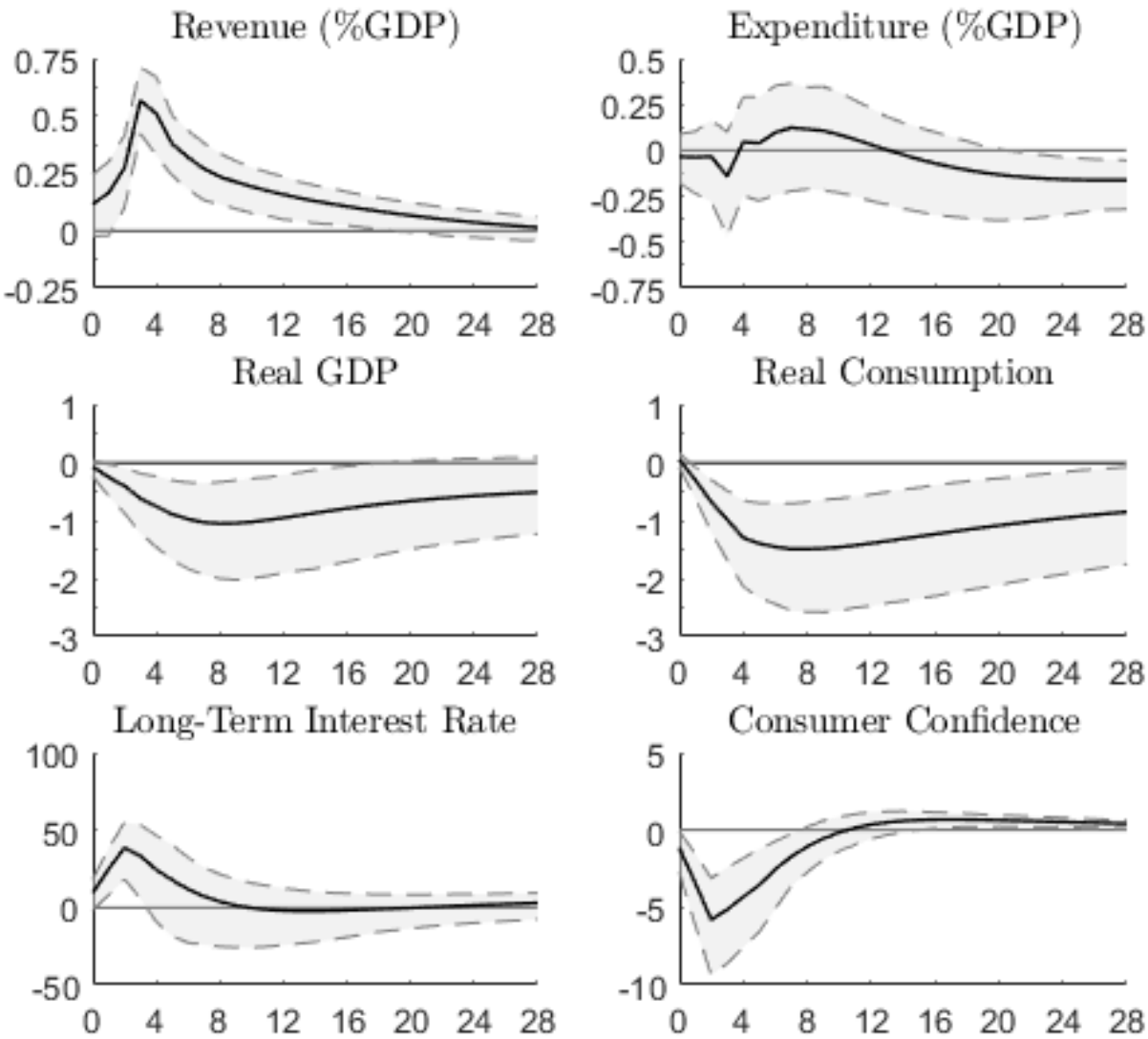


Figure 2b: Impulse responses baseline model – spending-based plans

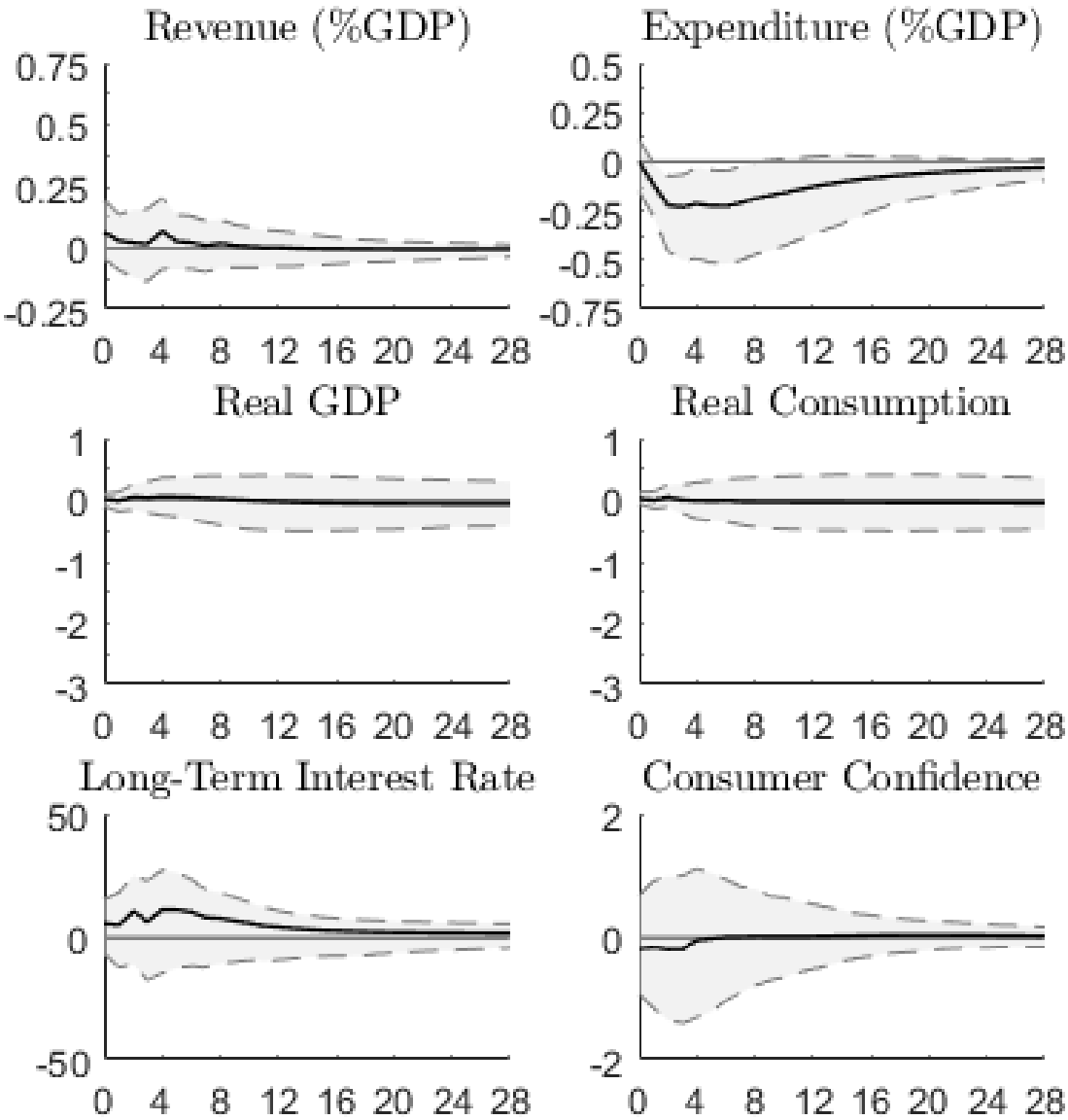


Figure 3a: Counterfactually shutting off the confidence channel – revenue-based plans

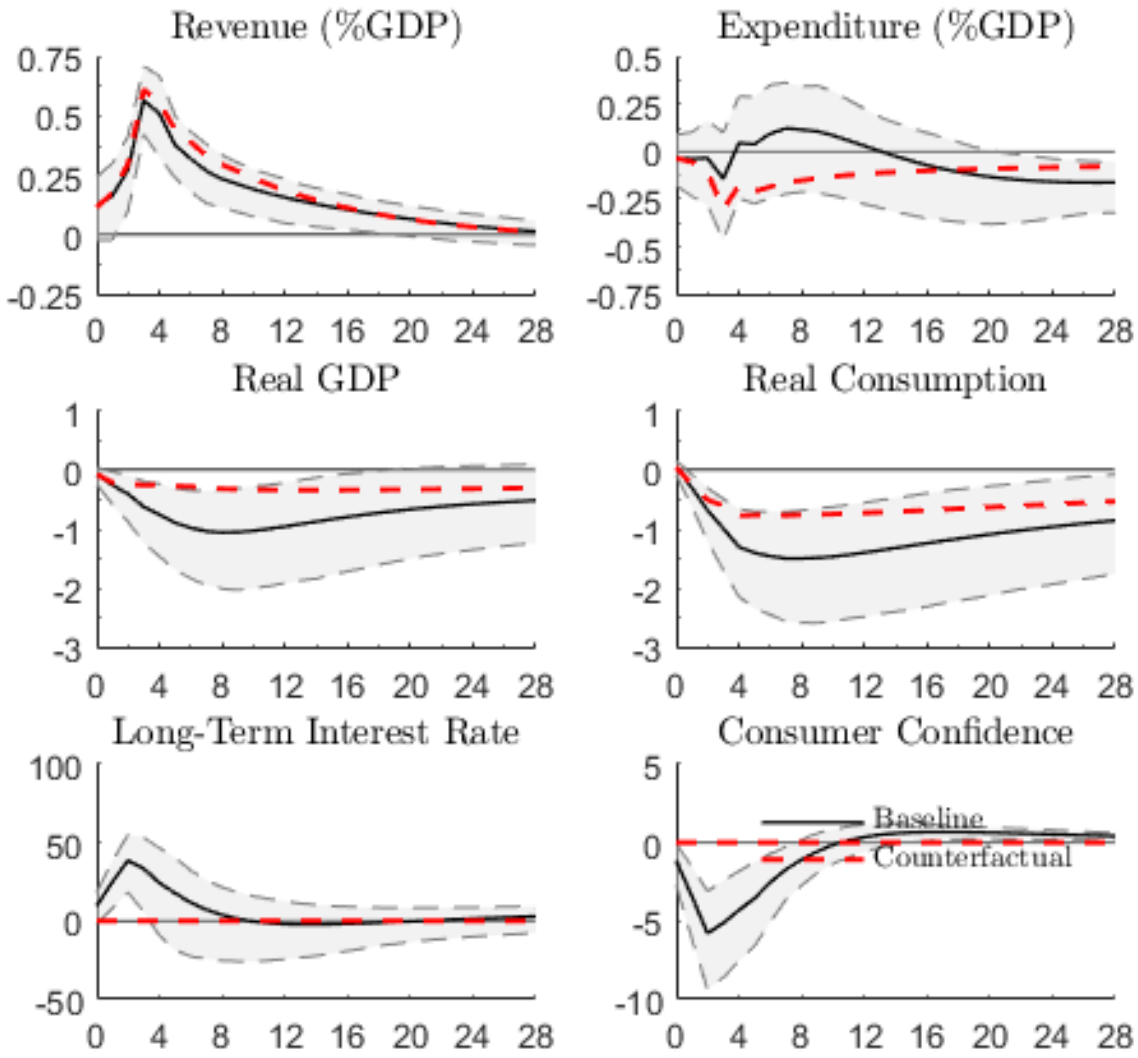
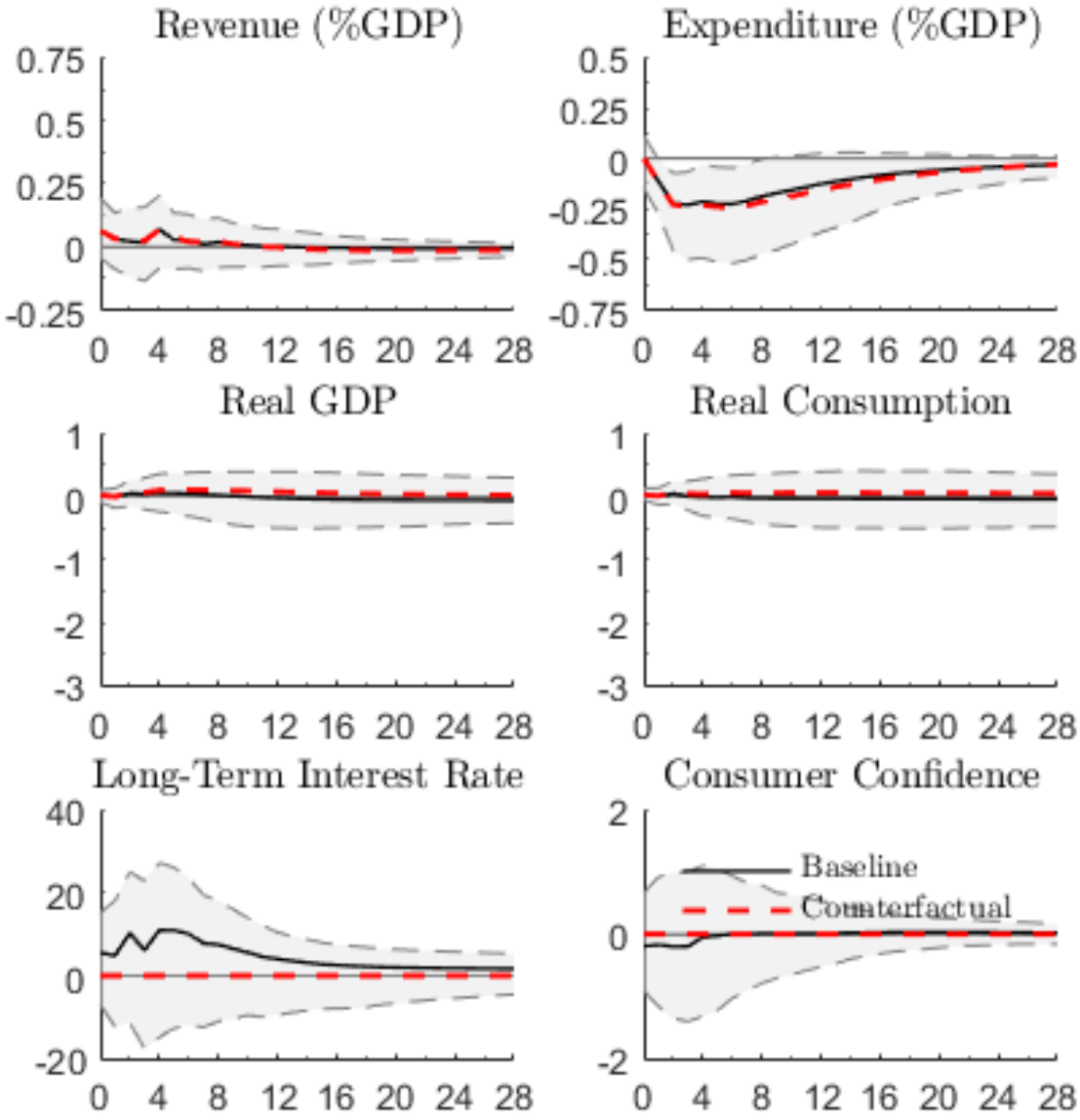


Figure 3b: Counterfactually shutting off the confidence channel – spending-based plans



Appendix

A: The 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:[20]

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

[20] 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.

<u>Construction of aggregate quarterly revenues and expenditure series:</u>

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.[21] 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:

[21] 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 HP 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).[22] 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,

[22] 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.[23]

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. "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 $d_0 > 0$

[23] 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).

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 $\left(g_1^a, \tau_1^a\right)$ 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 $\left(g_1, \tau_1\right)$ of:

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

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:

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

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 $\left(g_1^a, \tau_1^a\right)$. 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 $\left(g_1, \tau_1\right)$.

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 $\left(g_1^a,\tau_1^a\right)$ so as to minimize:

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

Some algebra yields the following outcomes for the consolidation announcements:

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

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

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

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

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

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 is quite clear whether the strike is associated with public spending cuts or tax increases. [24] 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 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.

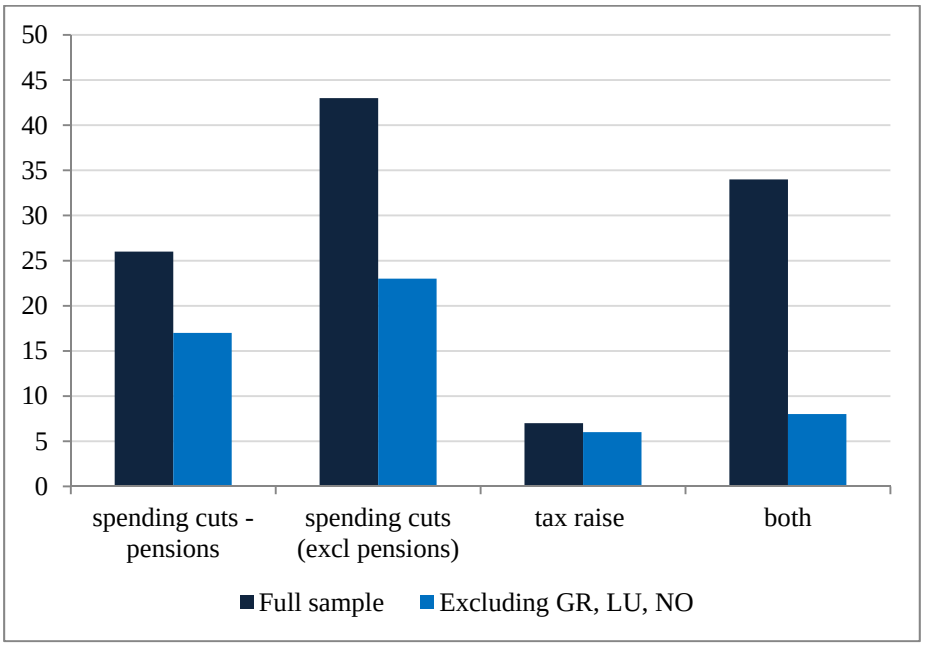
Out of the 159 disputes, we find that 69 are spending-cut motivated, 43 after excluding those where the issue in dispute is pensions, while the number of revenue-raise motivated strikes is only 7. Hence, disputes motivated by spending-cuts occur with a substantially higher frequency than

[24] 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.

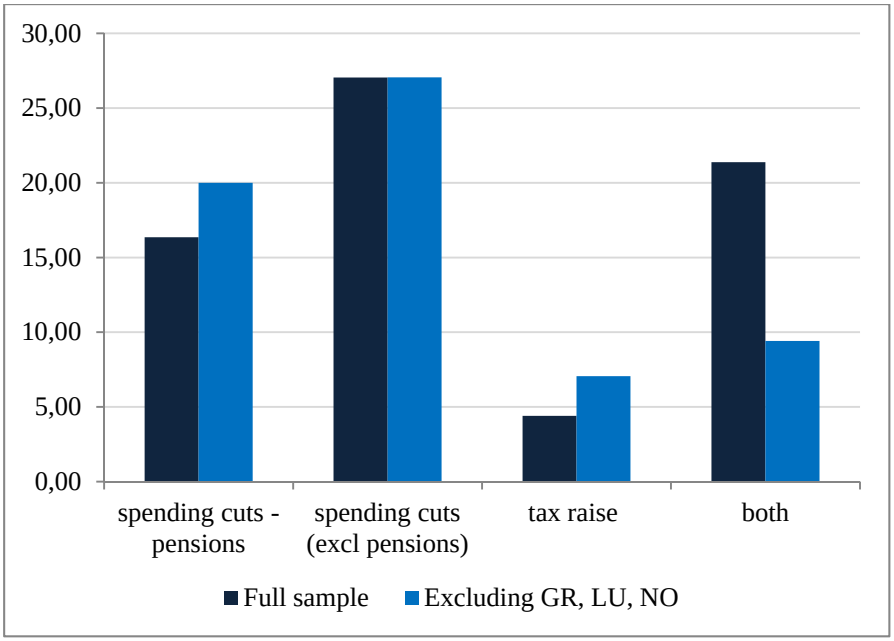
disputes motivated by revenue increases. We also observe 34 strikes against austerity in general, hence aimed at adjustments in both taxes and spending. Excluding Greece, Luxembourg and Norway, 85 strike episodes remain, of which 40 are motivated by spending cuts (23 upon exclusion of the pension-related 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 B.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 B.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 B.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".

C. Announcements and their dating

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:
  - "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).

Additional Appendix

Figure AA.1a: Baseline model in shares of potential GDP – revenue-based plans

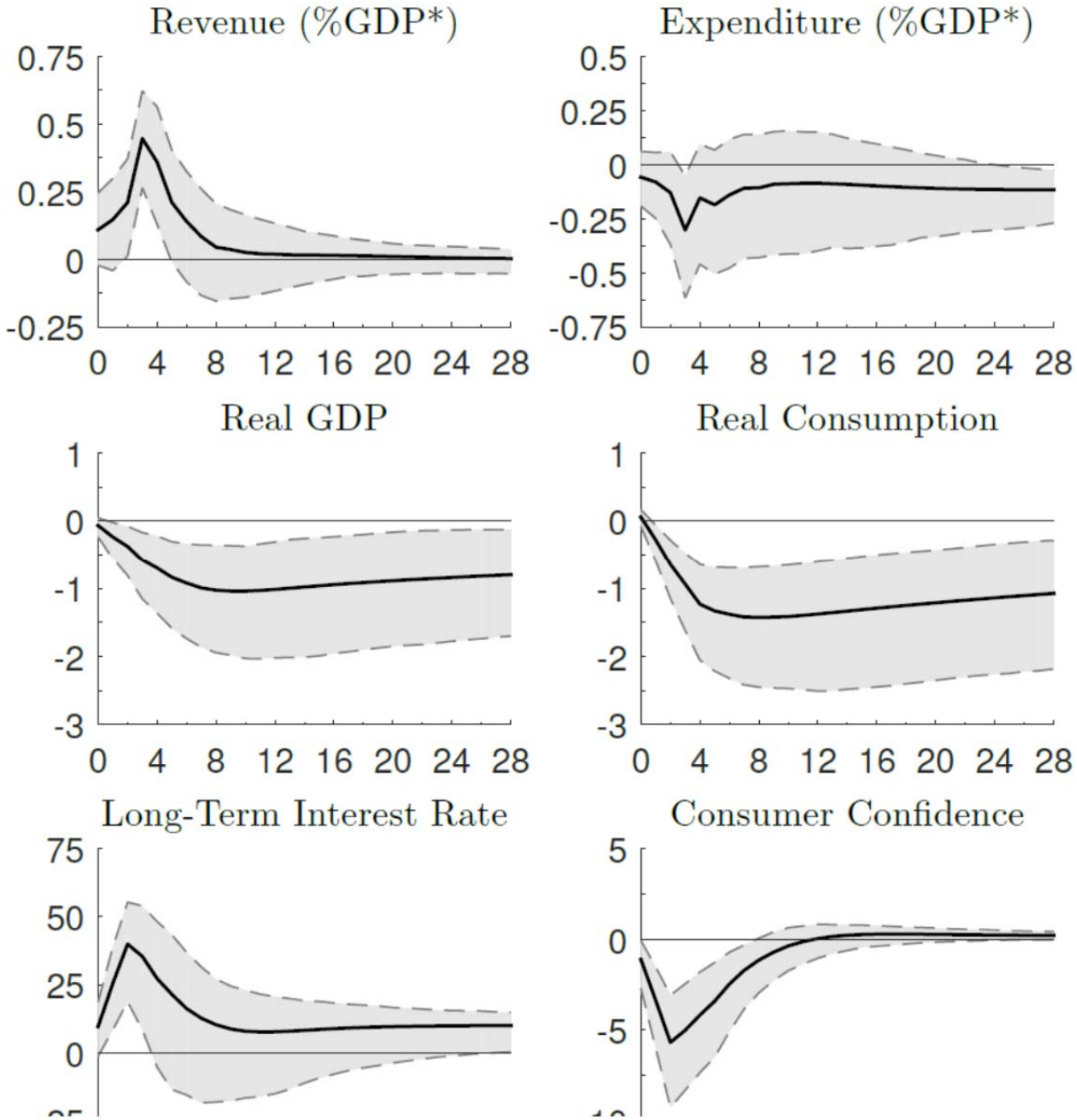


Figure AA.1b: Baseline model in shares of potential GDP – spending-based plans

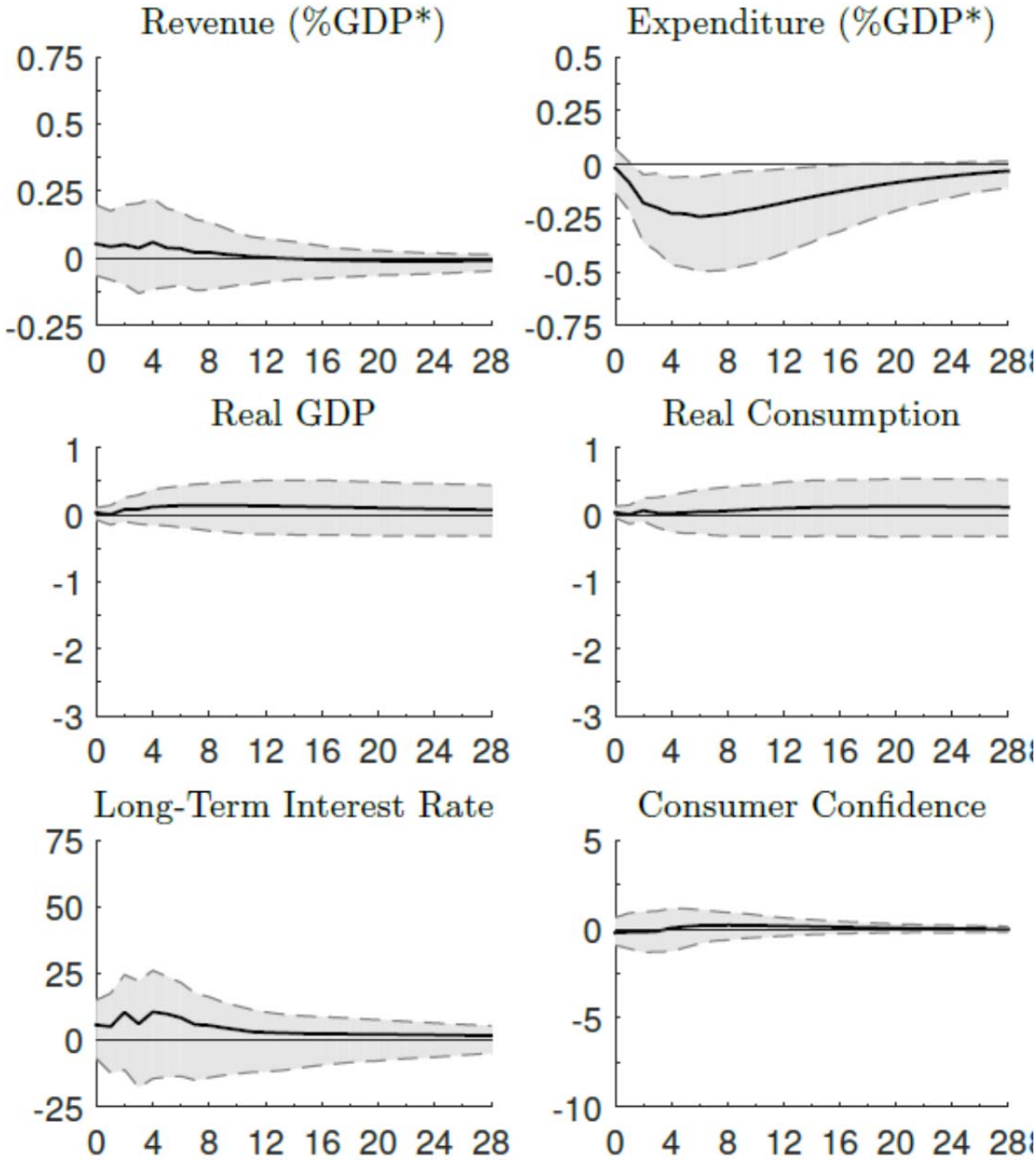


Figure AA.2a: Baseline model – revenue-based plans with at least 60% revenue measures

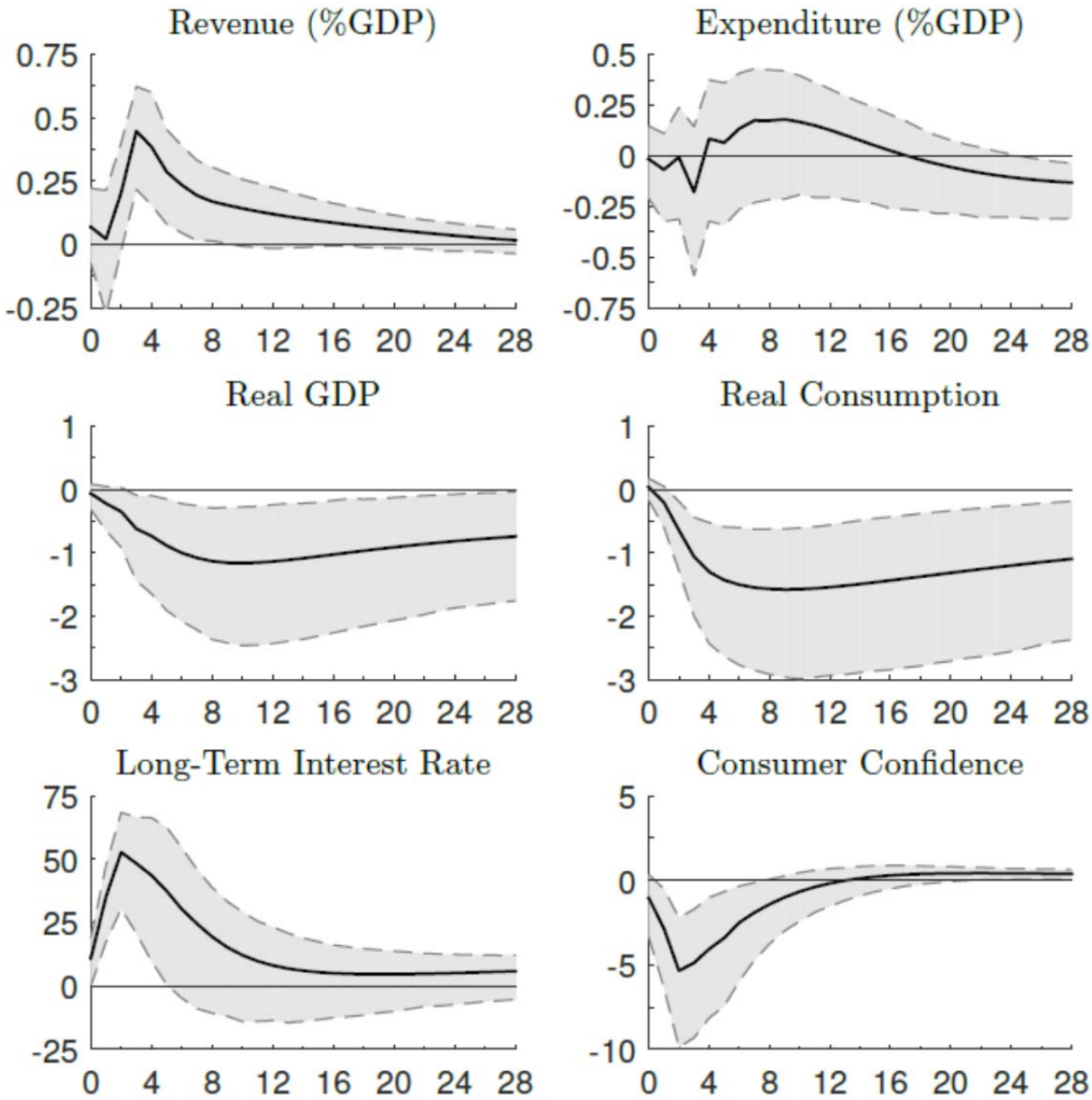


Figure AA.2b: Baseline model – spending-based plans with at least 60% spending measures

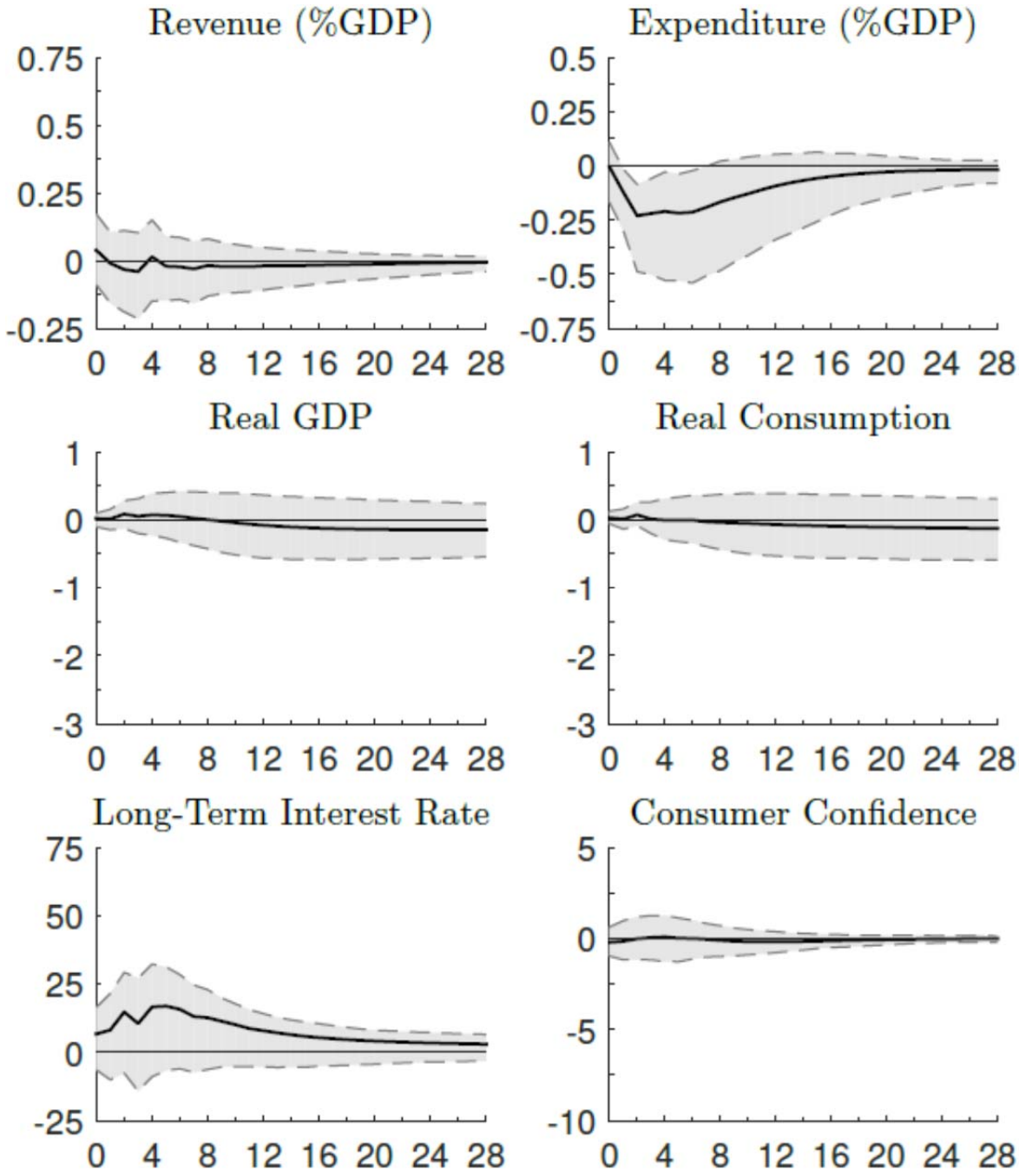


Figure AA.3a: Baseline model with time fixed effects – revenue-based plans

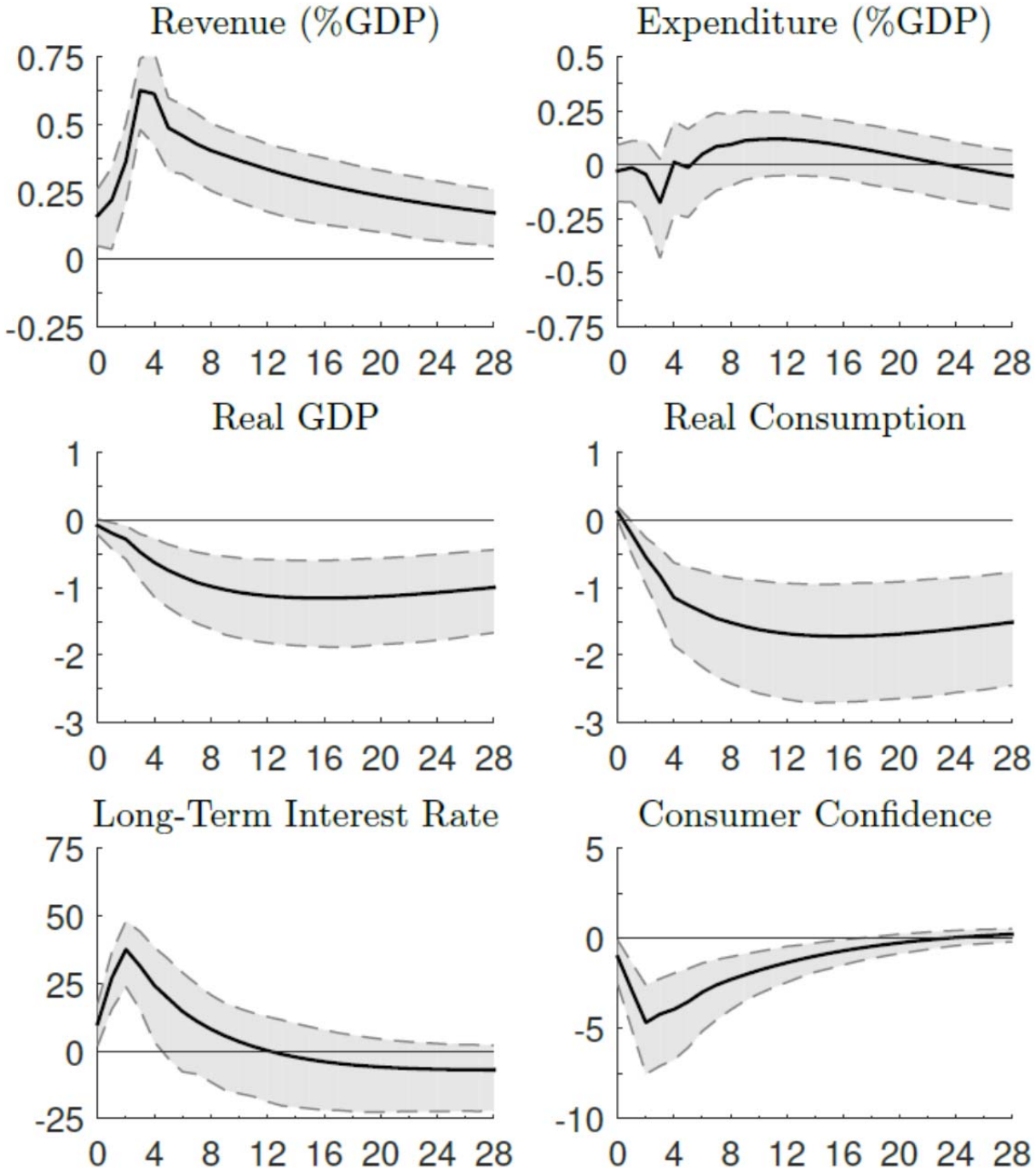


Figure AA.3b: Baseline model with time fixed effects – spending-based plans

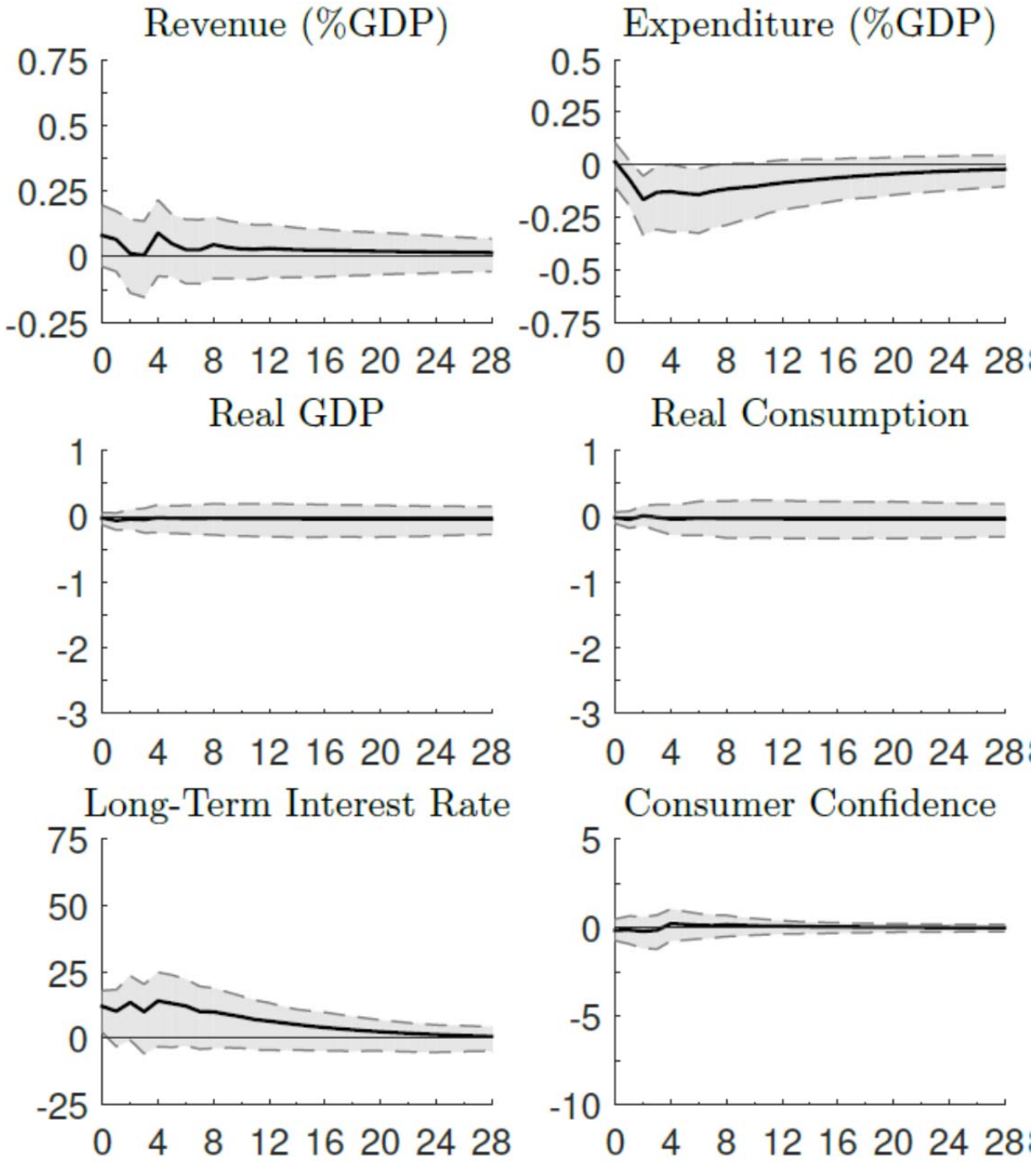


Figure AA.4a: Baseline model with eight lags – revenue-based plans

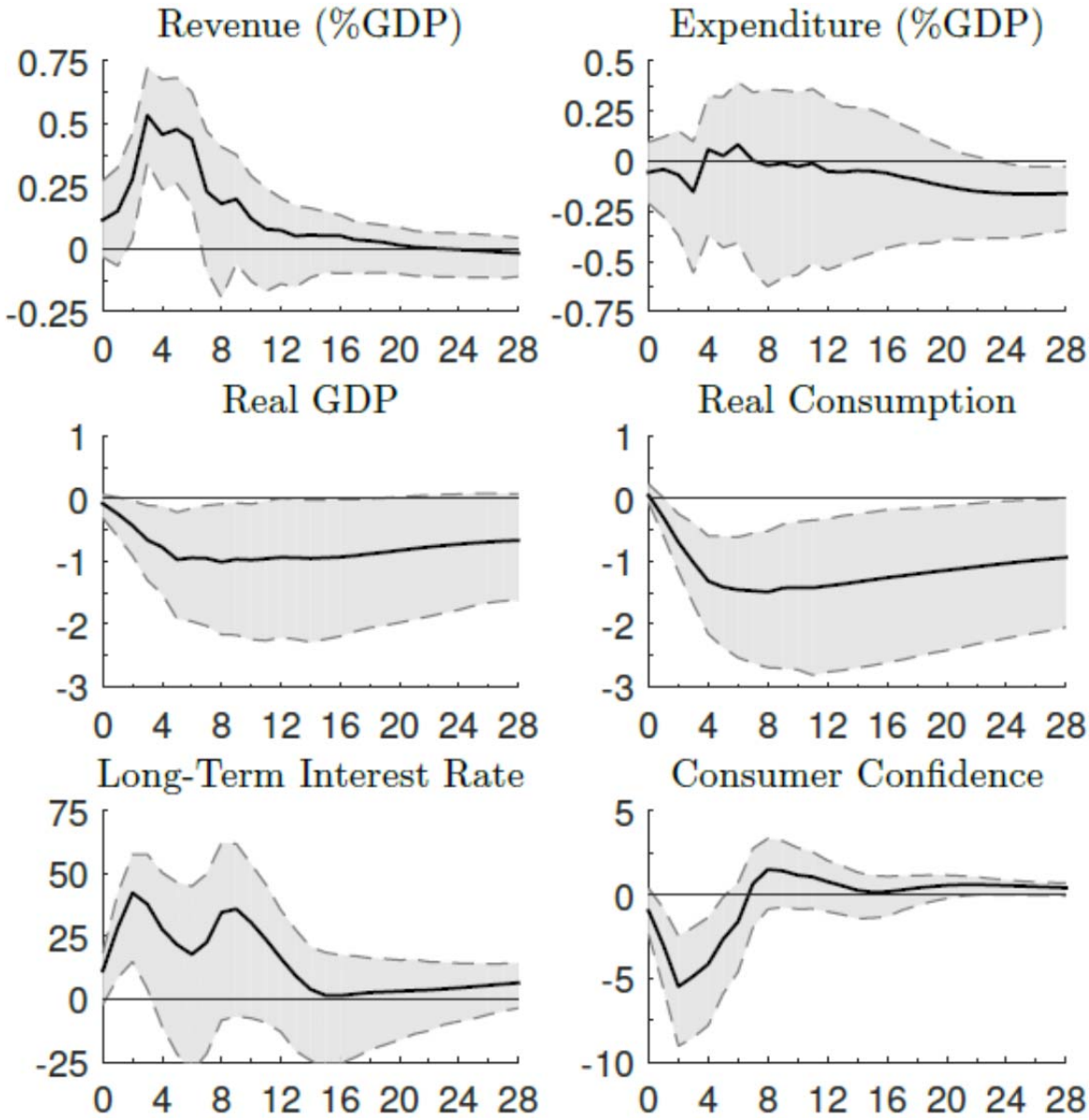


Figure AA.4b: Baseline model with eight lags – spending-based plans

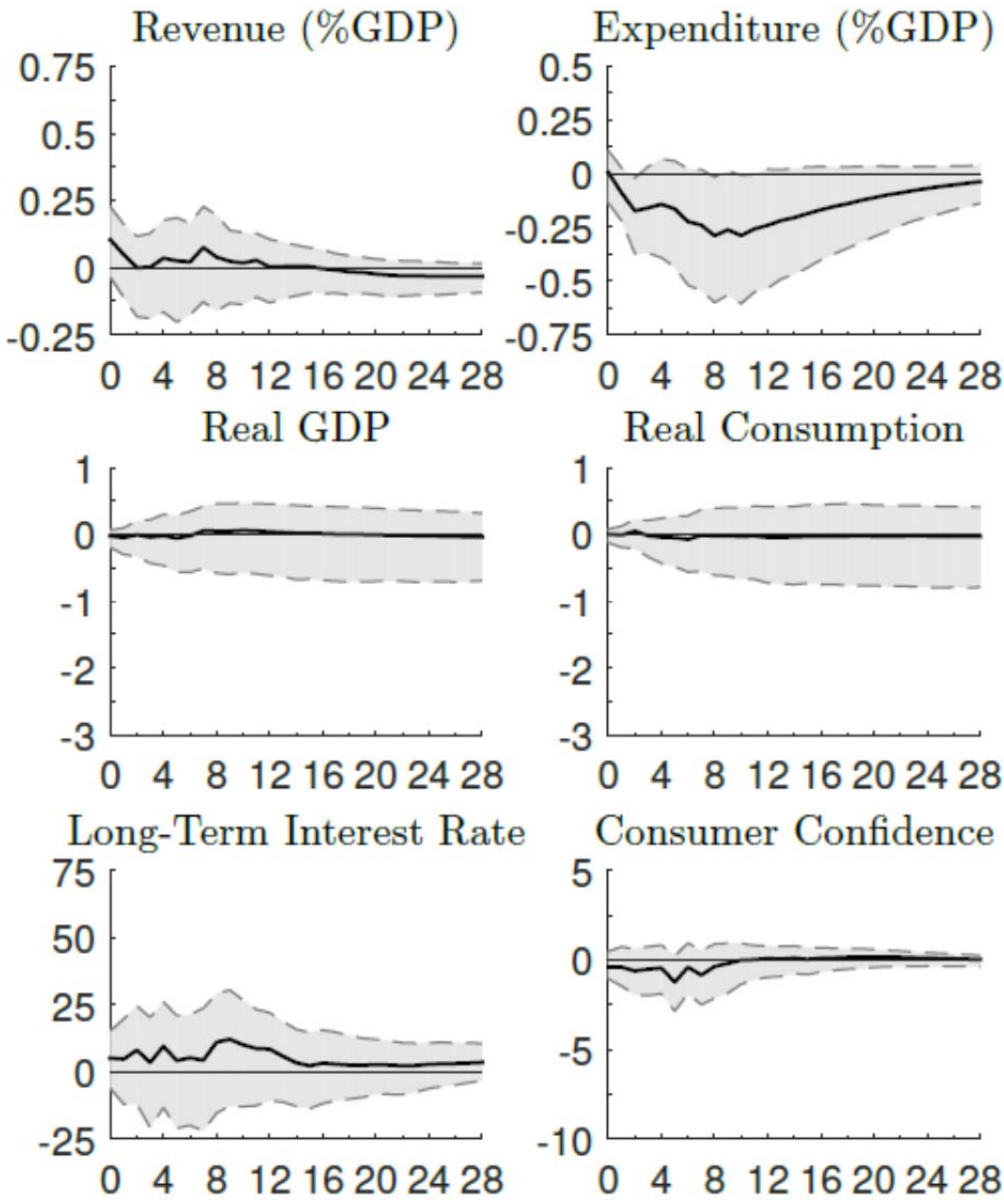


Figure AA.5a: Baseline model extended with lagged debt as exogenous variable – revenue-based plans

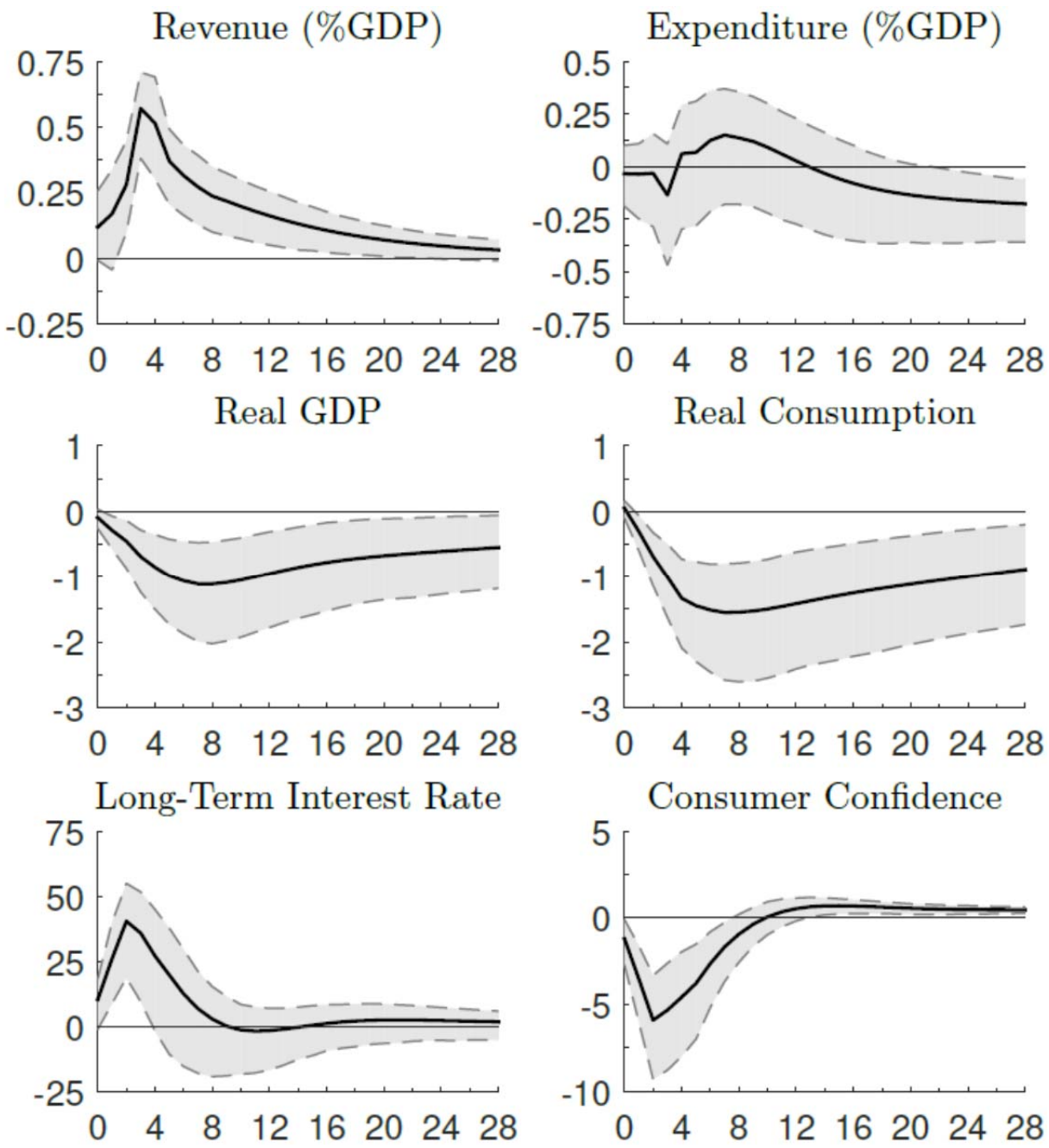


Figure AA.5b: Baseline model extended with lagged debt as exogenous variable – spending-based plans

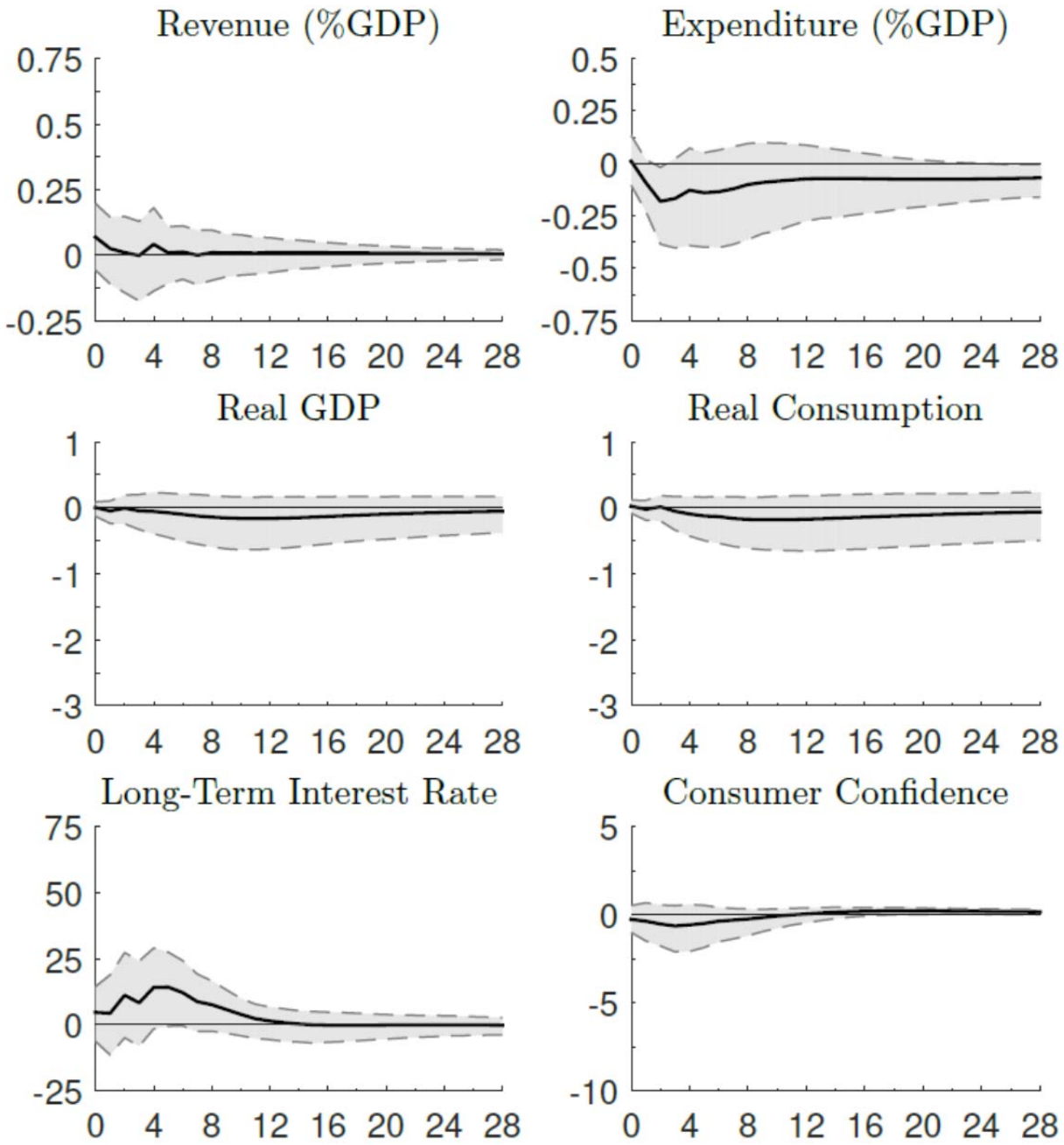


Figure AA.6a: Baseline model excluding one country at a time – revenues-based plans

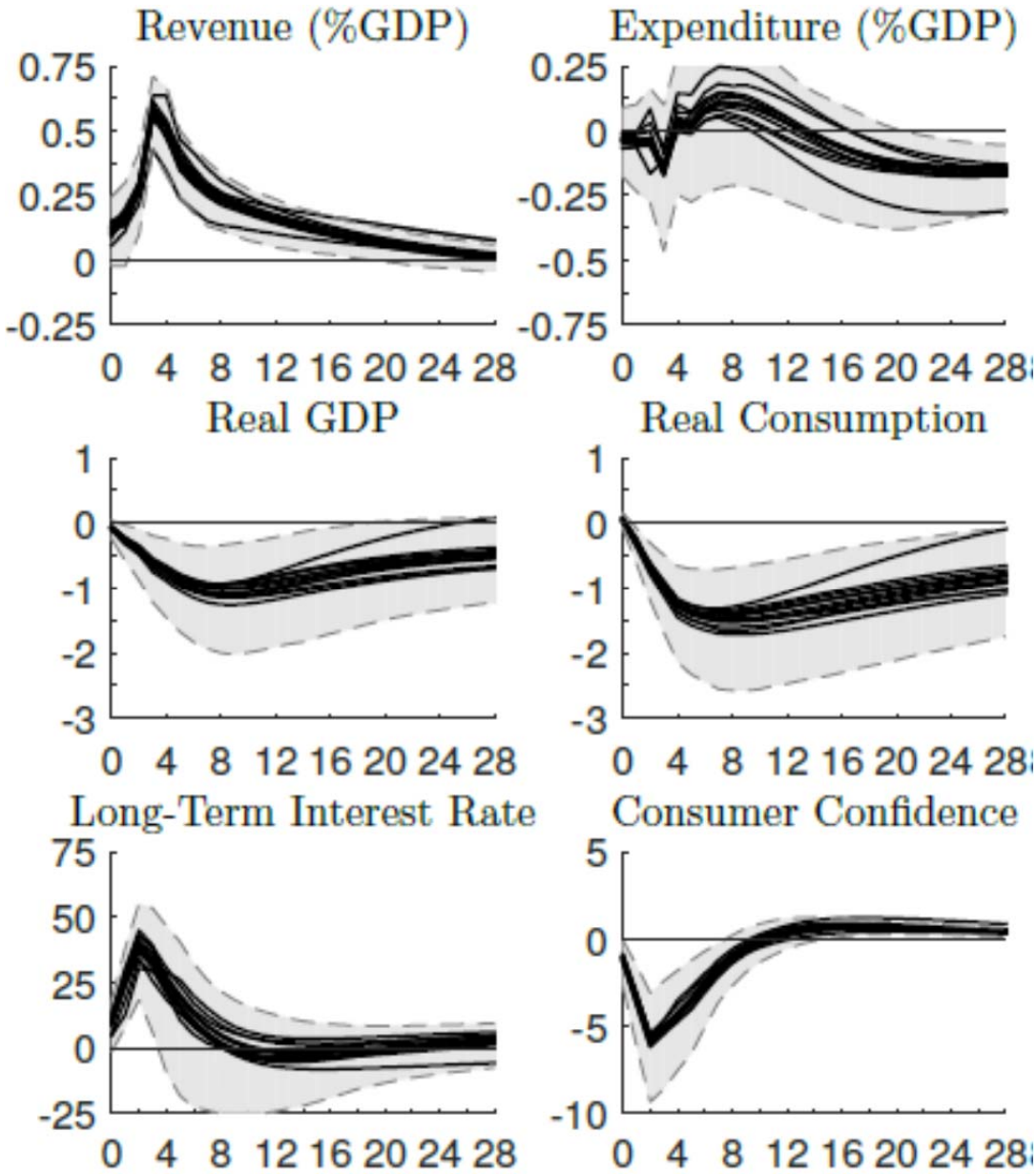


Figure AA.6b: Baseline model excluding one country at a time – spending-based plans

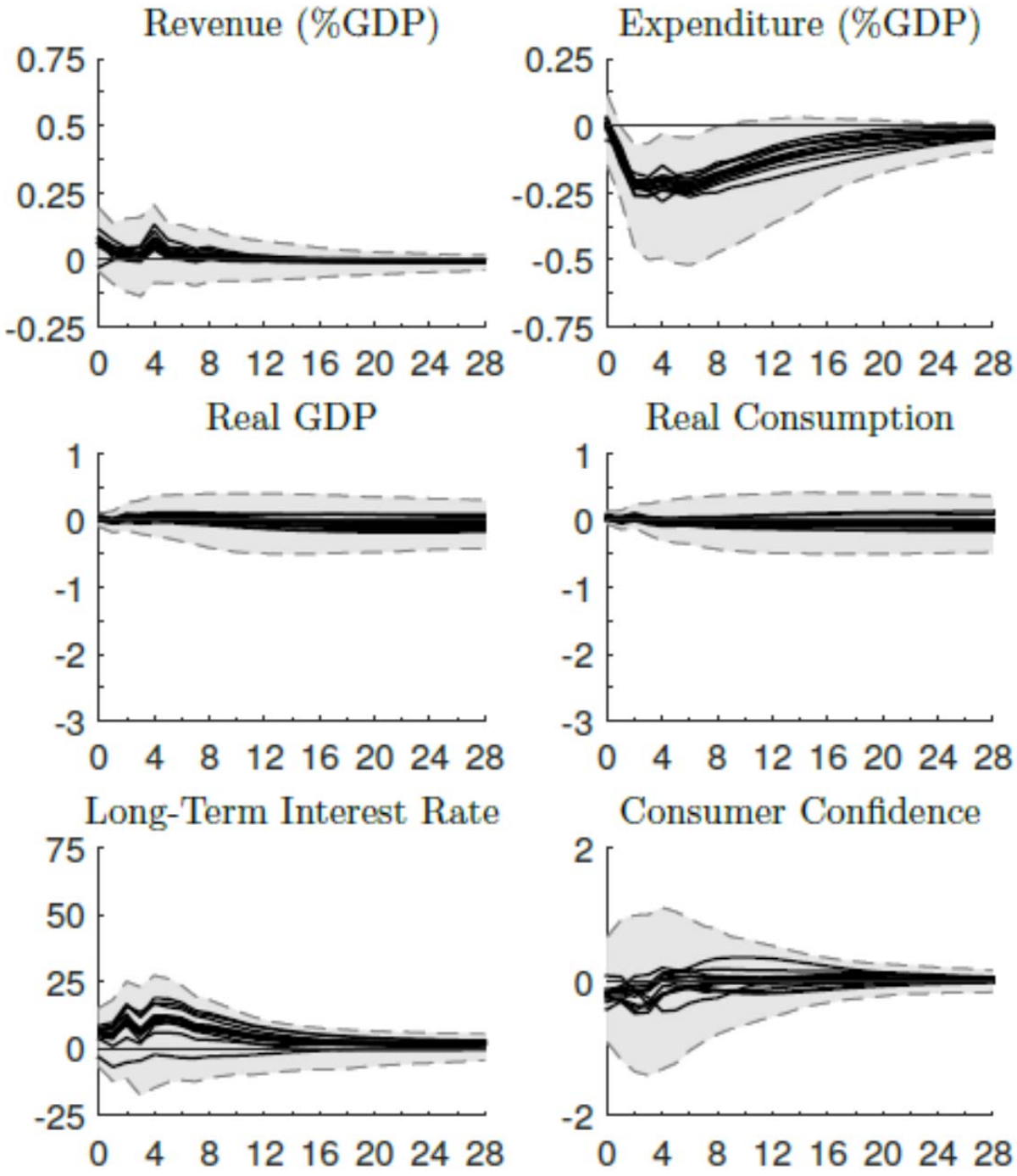


Figure AA.7a: Baseline model with short-term interest rate – revenue-based plans

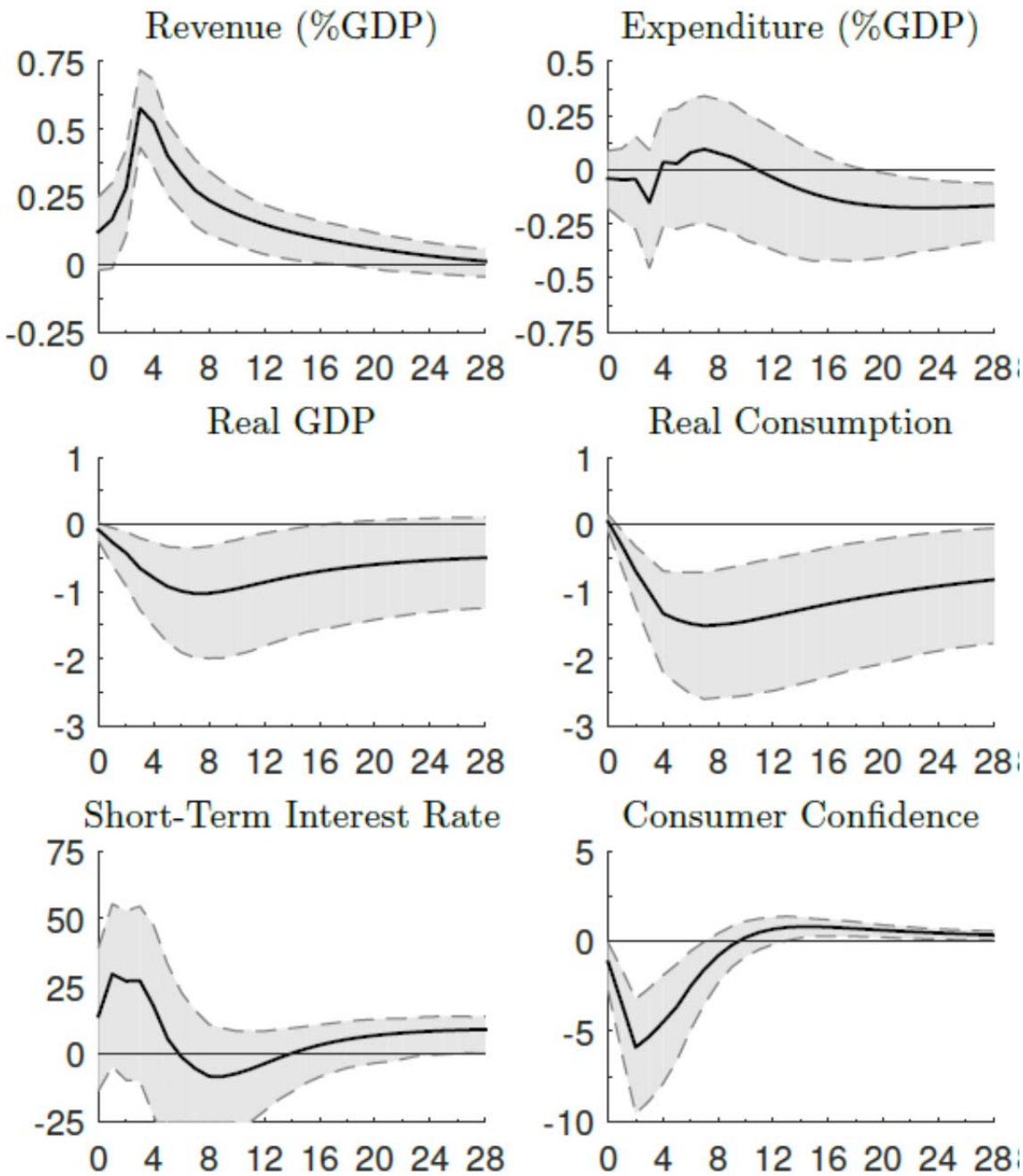


Figure AA.7b: Baseline model with short-term interest rate – spending-based plans

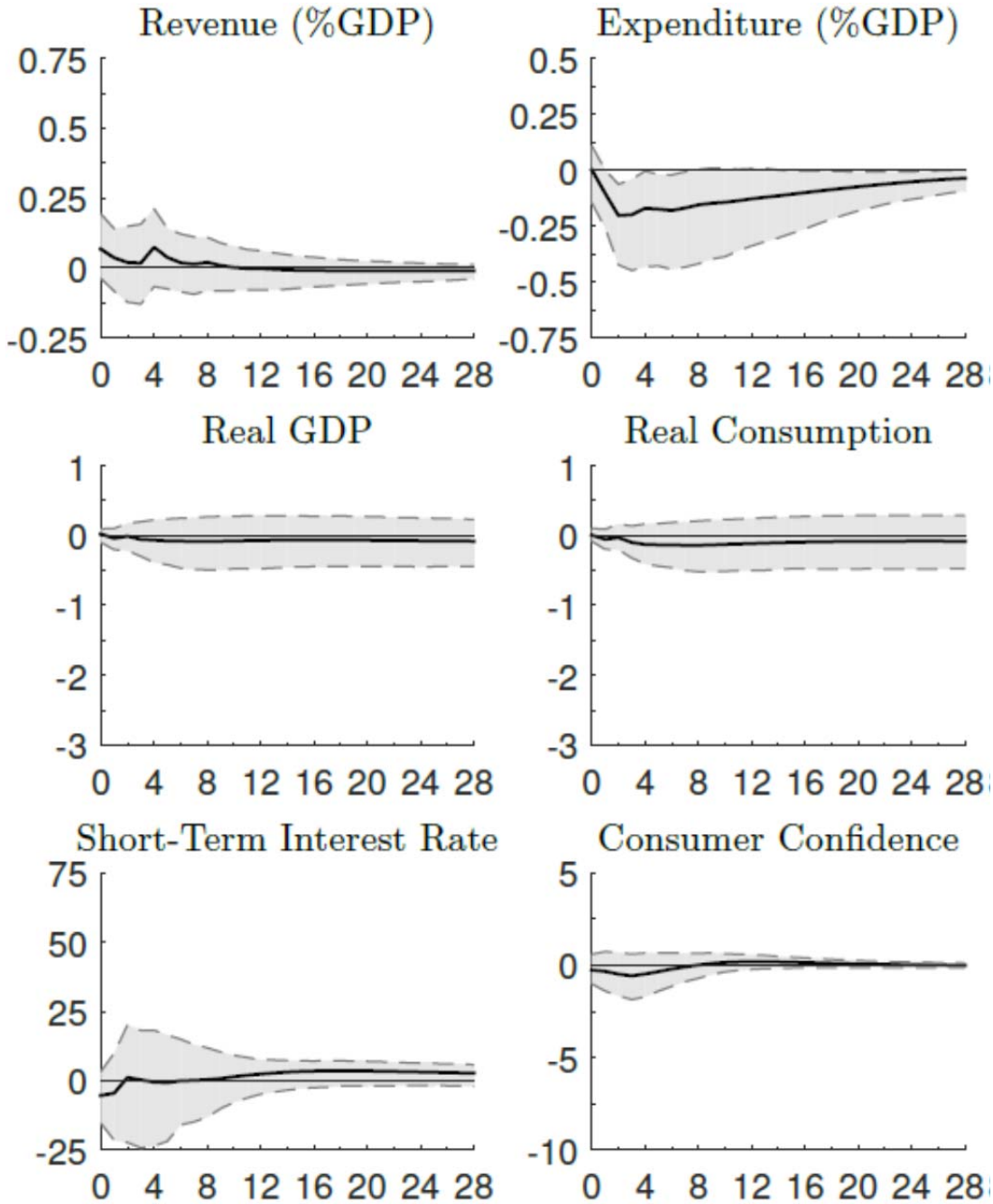


Figure AA.8a: Baseline model with business confidence and private investment – revenue-based plans

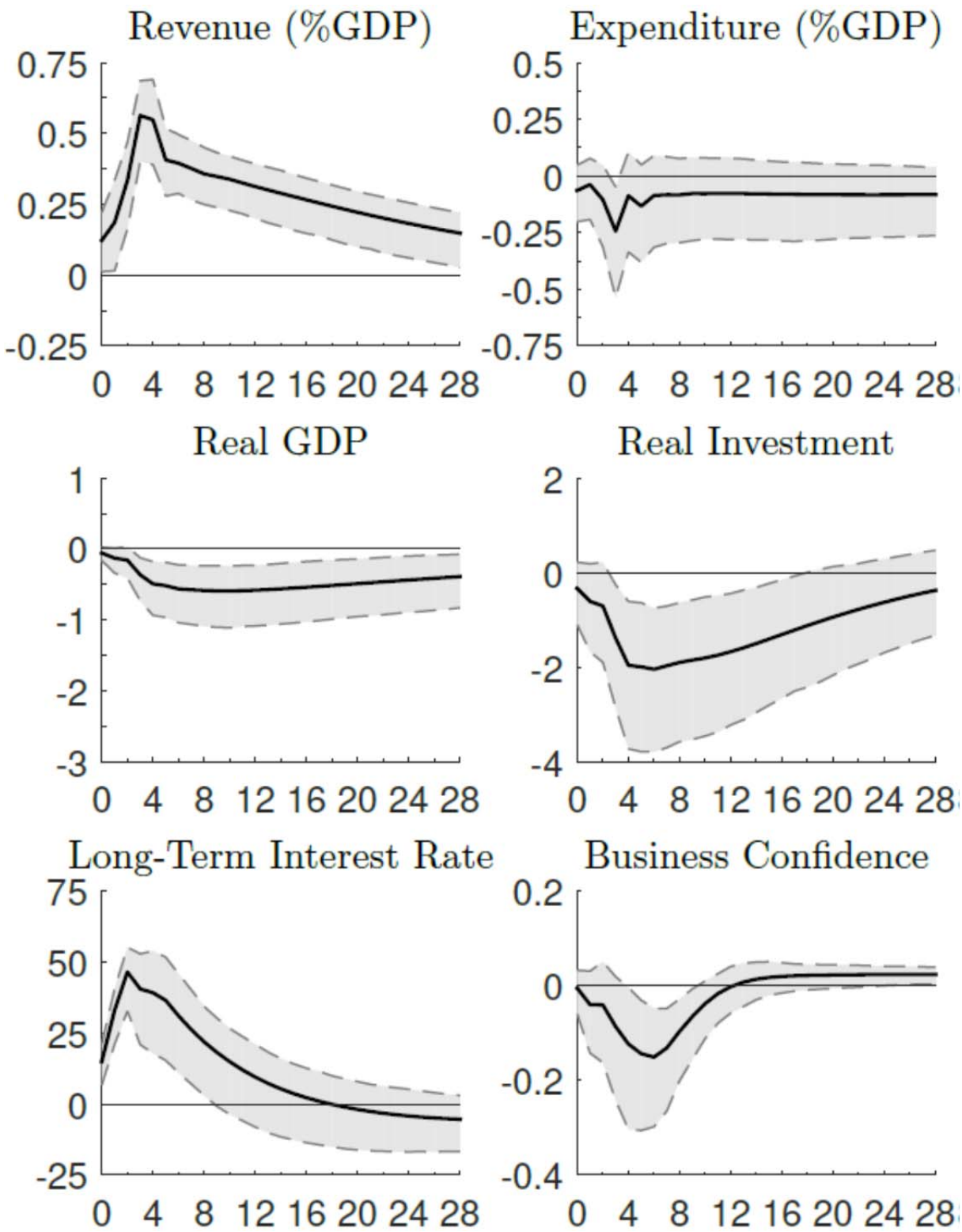


Figure AA.8b: Baseline model with business confidence and private investment – spending-based plans

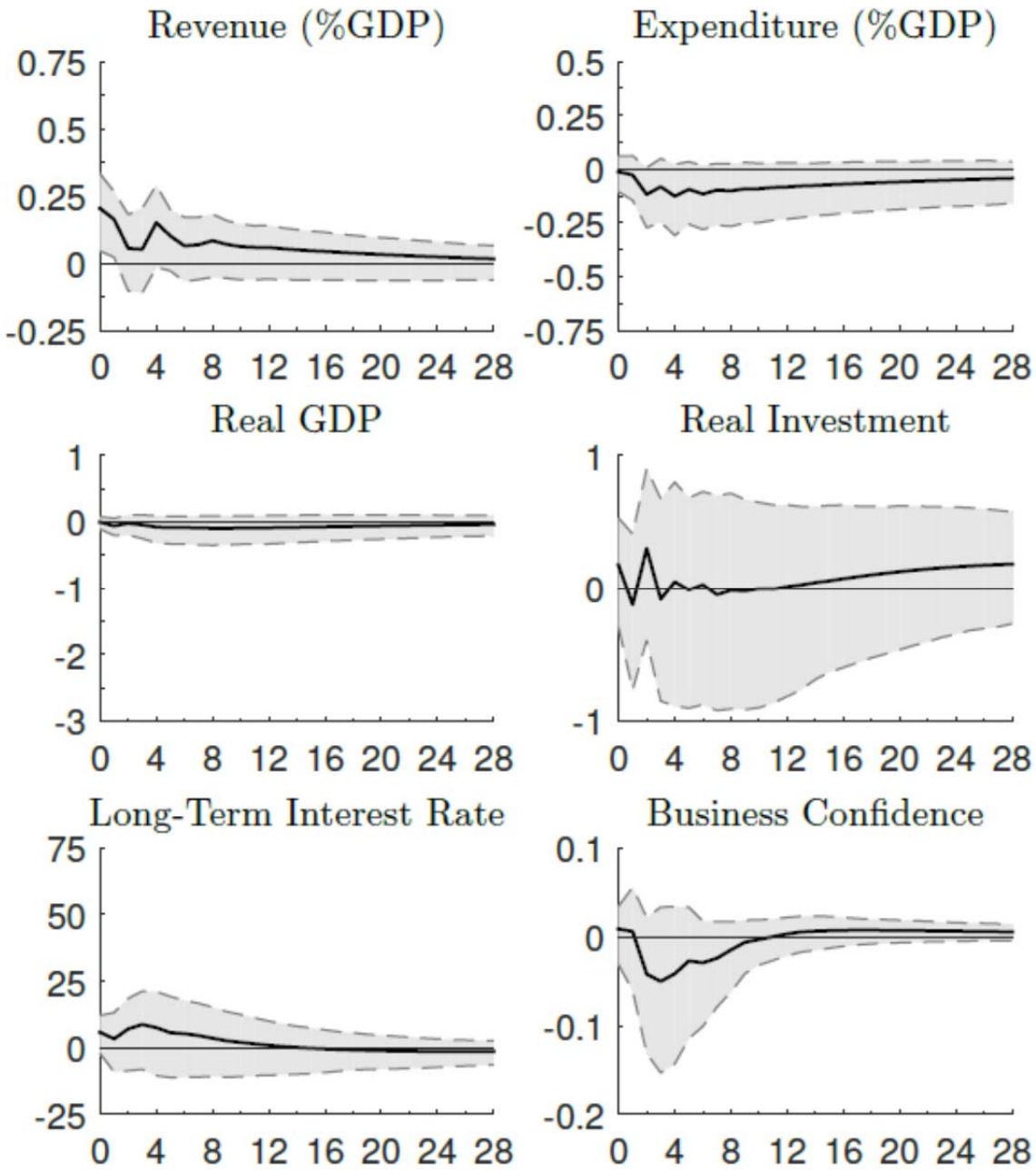


Figure AA.9a: Baseline model with ex-post long-run real interest rate – revenue-based plans

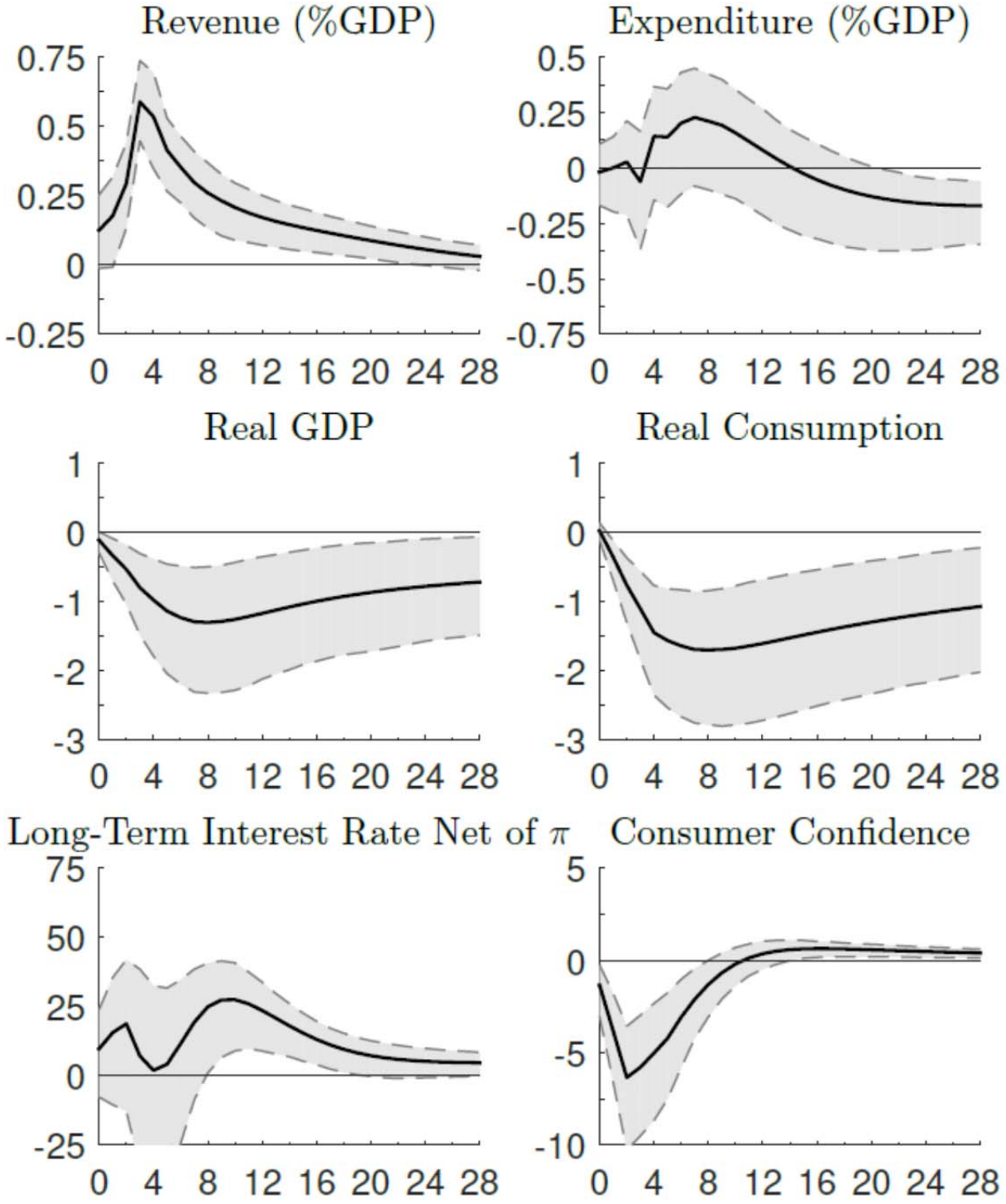
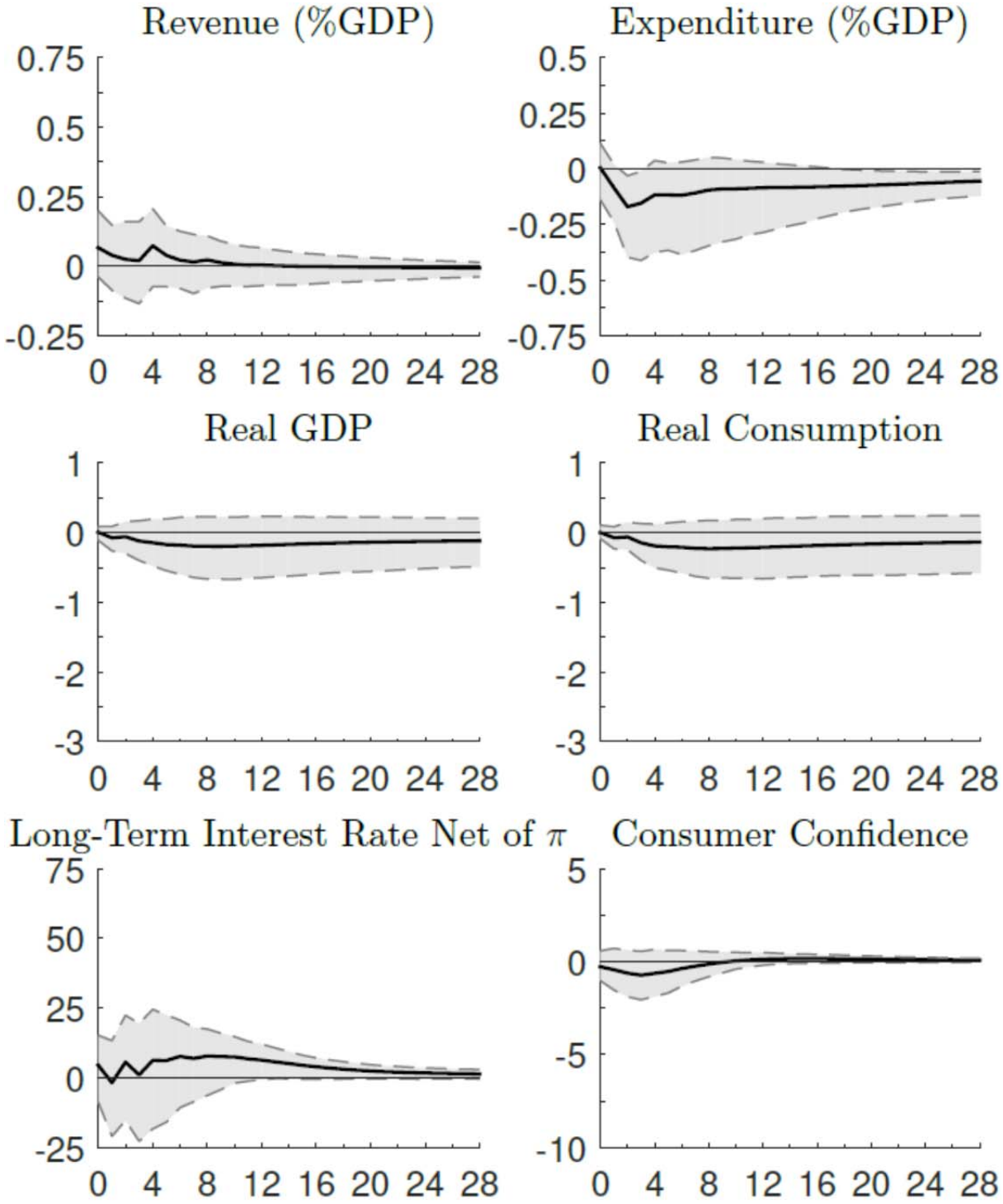



Figure AA.9b: Baseline model with ex-post long-run real interest rate – spending-based plans