DISCUSSION PAPER SERIES

DP12133
(v. 2)

REVENUE- VERSUS SPENDING-BASED FISCAL CONSOLIDATION ANNOUNCEMENTS: FOLLOW-UP, MULTIPLIERS AND CONFIDENCE

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INTERNATIONAL MACROECONOMICS AND FINANCE

MONETARY ECONOMICS AND FLUCTUATIONS

ISSN 0265-8003

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Discussion Paper DP12133
First Published 06 July 2017
This Revision 10 March 2020

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www.cepr.org

This Discussion Paper is issued under the auspices of the Centre's research programmes:

- International Macroeconomics and Finance
- Monetary Economics and Fluctuations

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Abstract

Based on a newly-constructed narrative dataset on fiscal consolidation announcements for thirteen EU countries over the period 1978-2013, a panel VAR analysis shows that revenue-based consolidation announcements affect economic activity more adversely than do spending-based announcements. The combination of larger actual follow-up and larger revenue multipliers helps to explain the difference. The unique nature of our dataset allows to control for fiscal anticipation and the confidence channel following the announcements. The confidence channel has an important role in explaining the difference in the multipliers following the two types of announcements.

JEL Classification: E21, E62, H5

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

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Acknowledgements
We thank for helpful comments on earlier versions Florence Huart, Eric Leeper, Davide Furceri, Enrique Moral Benito, Alexander Kriwoluzky, Javier Pérez, Gert Peersman, Malte Rieth, Christian Stoltenberg, participants of the Ghent Workshop in Empirical Macroeconomics, the workshop “Fiscal Sustainability, XXI Century” (Banco de España and Barcelona GSE), lunch seminars at the Tinbergen Institute and the University of Amsterdam, the Research on Monday seminar series at the Erasmus University, the Workshop on Fiscal Policy in Times of Crisis (DIW Berlin), the 7th UECE Conference on Economic and Financial Adjustments in Lisbon and the Université de Lille. We thank John Kelly for sharing the data on general strikes that he, Kerstin Hamann and Alison Johnston compiled.

Revenue- versus spending-based fiscal consolidation announcements: follow-up, multipliers and confidence[1]

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This version: February 2020

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

This paper explores differences in macroeconomic responses following *announcements* of revenue-based versus spending-based consolidations in the European Union (EU), highlighting the roles of differences in the degree to which these announcements are actually followed up, differences in revenue and spending multipliers and the mediating role of the confidence channel, captured by movements in private sector and financial market confidence indicators, which may react instantaneously following the announcement and before the execution of the consolidation plans actually starts. The macro-economy reacts in a markedly more negative way to a revenue-based than a spending-based announcement. This can be partly attributed to stronger follow-up in actual consolidation and a larger fiscal multiplier under the former. However, the confidence channel has a material role in this regard: muting this channel substantially shrinks the difference between revenue and spending multipliers. Our findings are complementary to the existing literature. However, they are based on a unique dataset identifying the precise moment of consolidation *announcements*, and as such they properly account for anticipation and confidence effects.

Our analysis proceeds in a number of steps. First, we provide evidence of differences in follow-up of revenue- versus spending-based consolidation plans by *directly* comparing *ex-post* actual fiscal data from the OECD with the *annual* narratively-identified consolidation plans of Devries *et al.* (2011) and

Alesina *et al.* (2015a,b). We do this by very carefully matching the narrative measures with the appropriate variables for the *ex-post* outcomes. We find a systematic shortfall of the latter relative to the narrative measures for spending-based measures. The average shortfall is substantially smaller for revenue-based measures. This finding motivates a deeper empirical analysis into the differential effects of spending-based versus revenue-based consolidation plans. To this end, we construct a new narrative dataset of fiscal consolidation *announcements* for thirteen EU countries over the period 1978-2013. The dataset is based on assigning consolidation information as accurately as possible to the *month* in which it first becomes publicly available.

We then enter the announcements as shocks into a quarterly (Bayesian) panel vector auto-regression (VAR). By using properly-timed announcements we can account for the potential private sector and financial market anticipation effects that may take place between the moment the plan becomes public information and its actual implementation. Moreover, we account for the response of confidence indicators to the real-time news on the planned fiscal consolidations and, hence, account for the confidence channel in the transmission from the announcement shocks to the economy. As confidence indicator for the non-financial private sector we use consumer confidence and as confidence indicator for the financial market we use the long-term interest rate.

Our panel VAR shows that announcements of revenue-based versus spending-based consolidations produce very different economic responses. The estimates confirm the difference in follow-up: revenue-based consolidation announcements lead on average to a significantly larger follow-up in terms of actual revenues than do spending-based consolidation announcements in terms of actual spending. We also find that following a revenue-based announcement, real GDP, real private consumption and consumer confidence decline significantly, while the long-term interest rate rises significantly. By contrast, after a spending-based consolidation announcement confidence indicators remain essentially unaffected, and real GDP and real private consumption do not react significantly. These findings are robust to a large number of alterations to the baseline specification.

We then investigate the sources of the differences in economic performance following the two announcement types. We decompose cumulative output responses into cumulative multipliers to the fiscal instrument and the cumulative follow-up of the instrument to the consolidation announcement. We find that the difference in the cumulative output responses between revenue- and spending-based announcements is explained by both a larger cumulative multiplier to enacted revenue measures and a larger follow-up of revenue measures to a revenue-based announcement. As mentioned above, the unique nature of our dataset enables us to control for the confidence channel following the announcements. Importantly, we find that for a given announcement shock

our confidence indicators are mostly driven by the announcement shock itself. A counterfactual scenario, in which we hold the responses of consumer confidence and the long-term interest rate at zero for a number of periods after the announcements, demonstrates the importance of the confidence channel. The drop in the revenue multiplier is so large that it eliminates a substantial part of the difference in the follow-up effect on GDP resulting from the two announcement types.

The remainder of this paper is structured as follows. Section 2 provides a brief review of the relevant strands in the literature. Section 3 investigates the realization of *annual* fiscal consolidation plans identified by existing datasets by directly comparing these plans with *ex-post* data on revenues and spending. Section 4 describes our newly-constructed dataset of fiscal consolidation announcements. Section 5 uses a panel VAR analysis to explore the roles of differences in follow-up, fiscal multipliers and the confidence channel for the asymmetric economic performance following the two types of plan announcements. Finally, Section 6 concludes. Appendices A and B provide further information on the macroeconomic data and the construction of the consolidation announcement data. Appendix C offers two complementary explanations for the differences in follow-up to announcements of revenue- and spending-based consolidations, while Appendix D provides some technical details on the Bayesian estimation of our panel VAR model. A detailed description of the construction of our dataset on consolidation announcements is found in an online Data Construction Appendix.

2. Literature review

This paper relates to several main strands of literature. First, it connects to the literature on the macroeconomic 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. Third, it connects to studies that emphasize the role of expectations in the transmission of policy changes and, finally, it relates to recent work linking fiscal consolidations and sovereign yields.

The Great Recession has motivated a large body of work estimating the sign and magnitude of fiscal multipliers. Empirical evidence generally shows that positive revenue shocks are contractionary (Blanchard and Perotti, 2002; Romer and Romer, 2010; Barro and Redlick, 2011), with output multipliers ranging between -0.5 and -5.[6] Reductions in public wage expenditures lower disposable

[6] Since the turn of the century a substantial amount of effort has been devoted to estimating fiscal multipliers. Examples of other contributions are Beetsma *et al.* (2008), Bénétrix and Lane (2009), Beetsma and Giuliodori (2011), Auerbach and Gorodnichenko (2012), Corsetti *et al.* (2012) and Ilzetzki *et al.* (2013).

income directly, while reductions in non-wage public spending on goods and services lower disposable income by depressing the demand for private sector output and, hence, income generated in the private sector. These results are confirmed for narratively-identified consolidation measures: for a panel of OECD countries Guajardo *et al.* (2014) find that both the revenues and spending measures are associated with reductions in private consumption and GDP. However, there is evidence (e.g. Guajardo *et al.*, 2014, and Alesina *et al.*, 2015a and 2015b) that spending-based consolidations are less harmful in terms of economic activity than are revenue-based consolidations. The literature offers several arguments for why this may be the case. One explanation, advanced by Guajardo *et al*. (2014), for example, is that monetary policy tends to be more accommodative in the case of spending-based consolidations. A second argument is that, because they are politically more costly, resorting to spending-based consolidations provides a stronger signal by the government to the private sector that it intends to improve its budgetary situation (Ardagna, 2004).[7] In a similar vein, Alesina *et al*. (2015a,b) argue that the less recessionary macroeconomic effects of spending-based consolidations are related to their positive effects on business confidence and private investment. 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.

A second strand of literature related to this paper consists of empirical studies that document sizable and systematic deviations of actual implementation from fiscal plans. Examples are Jonung and Larch (2006), Beetsma *et al*. (2009), Von Hagen (2010), Pina and Neves (2011), Cimadomo (2012), Beetsma *et al*. (2013), De Castro *et al*. (2013) and Debrun and Kinda (2017).[8] Using data from the EU's Stability and Convergence Programs, Beetsma *et al*. (2009) show that actual budgetary adjustment falls systematically short of planned adjustment, and that the shortfall increases with the projection horizon. Related analysis by Von Hagen (2010) indicates that the form of fiscal governance and the tightness of fiscal rules can help to explain these shortfalls. Pina and Neves (2011) employ EU Excessive Deficit Procedure reporting data to conclude that budget balance forecasting errors are responsive to fiscal institutions and opportunistic political motivations. A related conclusion is reached by Beetsma *et al.* (2013), who distinguish between systematic shortfalls in implementation during the first year after the presentation of the budget and any 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 –

[7] 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.

[8] 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.

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

A third strand of relevant literature is the growing body of work that explores the role of *news* for short-term economic dynamics. Here, the crucial assumption is that short-run output fluctuations can be driven by changes in the information set of agents. New information about future (economic) developments affects the expectations of private sector agents, who start to adjust their behavior in anticipation of the future state of the economy (Beaudry and Portier, 2014). Expectations of fiscal consolidations may either moderate or exacerbate the contractionary effect of the actual measures on the real economy. On the one hand, adherents to the “expansionary austerity" view claim that positive expectations effects can mitigate the contractionary effects of fiscal consolidations: if private agents realize that a 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 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 (2010, 2012) show that anticipation effects can play an important role in the identification of structural fiscal shocks and that the incorporation of narrative shocks in the empirical analysis produces results different from those based on standard techniques. Our dataset of fiscal consolidation announcements is particularly suited to addressing such expectations effects. From a methodological viewpoint, our work studies the link between news and short-term economic dynamics that uses explicitly-identified shocks (such as, for instance, in Brückner and Pappa, 2015). Thus, our work falls within the empirical literature on narratively-identified fiscal VAR models, while our exogenous instrument consists of announcements of future fiscal austerity measures.

Finally, the paper relates to recent work on the relationship between fiscal consolidations and sovereign yields. De Jong (2018) finds evidence that new information hinting at an improvement of the Dutch public budget reduces sovereign yield spreads against Germany. For a panel based on a broad sample of advanced and emerging countries, Born *et al.* (2019) 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. Similarly, for a panel of emerging economies experiencing high sovereign spreads or under an IMF program, David *et al.* (2019) report a significant drop in those spreads after news about the approval of austerity measures by the legislature.

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

This section explores the extent to which *ex-post* annual fiscal changes correspond to the annual fiscal consolidation measures narratively-identified by Devries *et al.* (2011) and Alesina *et al.* (2015a,b) in their expansion of the dataset of Devries *et al.* (2011). The dataset of Alesina *et al*. covers thirteen EU countries. For Austria, Belgium, Denmark, France, Germany, Ireland, Italy, Portugal, Spain and the United Kingdom the sample spans the period 1978 – 2013, whereas for Finland, Sweden and the Netherlands it covers the period 1978 – 2008. Evidence of a systematic difference between the follow-up of real-time revenue and spending measures helps to motivate the econometric analysis in Section 5, which will be based on our new dataset of fiscal consolidation announcements that we describe in Section 4, and in which we investigate the sources of the differences in output responses following revenue- and spending-based announcements. An important source will be differences in follow-up.

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

The annual fiscal consolidation measures in the dataset of Devries *et al.* (2011) are narratively selected from policy documents such that their primary motivation is sustainability of the public budget and not a response to the business cycle. The identified measures together with their estimated budgetary impact reflect the "intentions and actions" of policymakers as described in a wide range of contemporaneous policy documents.[9] 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

[9] The recorded budgetary impact is the estimated change in budgetary savings accounted for by all the measures implemented in a given year. The sources include Stability and Convergence Programs submitted by the governments to the European Commission, OECD Economic Surveys, IMF Staff Reports, IMF Recent Economic Development reports, official budget documents, *etc.*

used in particular by Alesina *et al.* (2015a,b) are the Stability and Convergence Programs submitted by EU member states. These documents contain both forecasts of the effects of fiscal plans for the coming years and real-time estimates of the impact of the measures taken in the current and preceding years. Therefore, in both the narrative dataset of Devries *et al.* (2011) and in its extension, the intended magnitude of a fiscal consolidation represents a mixture of forecasts and first-release data.

We compare changes in actual (i.e., *ex-post*) public revenues and spending with the estimated budgetary impact of the narratively-identified consolidation measures by Devries *et al.* (2011) and Alesina *et al.* (2015a,b) for each year. The comparison is served best by matching as well as possible the concepts of revenues and spending used in the narrative identification with *ex-post* actual revenues and spending. Because there is no obvious one-to-one correspondence between the revenue and spending consolidation plans and the respective *ex-post* measures, we will compare the narratively-identified consolidation measures with a variety of alternative (e.g. from the most comprehensive to more narrow definitions) *ex-post* budgetary measures. Appendix A contains a full description of the annual budgetary data used in this section.

3.2. A simple accounting framework

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

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

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 nominal 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 nominal GDP announced in year $h \leq t-1$, which is obtained from the narrative 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:

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

where superscript "*CA*" indicates the cyclically-adjusted component, which we obtain directly from the OECD Economic Outlook.

3.3. Results of the comparison

Table 1 reports the *ex-post* deviations of actual revenues from planned increases averaged per country over the consolidation years identified by Devries et al. (2011) and Alesina et al. (2015a,b) and averaged over all consolidation episodes in our EU dataset. Similarly, Table 2 reports the *ex-post* deviations of actual expenditures from planned reductions. As pointed out above, there is no unique one-to-one correspondence between the actual revenue and spending measures constructed from the OECD dataset and the composition of the consolidation plans identified by Devries *et al.* (2011) and Alesina *et al.* (2015a,b). However, the conclusions of the comparison of the shortfalls for revenues and spending are the same regardless of the alternative budgetary aggregates we use. For the most comprehensive measure of revenues, "Total receipts, excluding gross interest receipts" (which in all likelihood covers all the items contained in the consolidation plans), 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", its cyclically-adjusted version, and "Total revenues, narrow definition", the average shortfalls are slightly larger (up to a maximum of roughly 0.18% of GDP).

By contrast, the average shortfalls of spending are substantially larger than of revenues. For the most comprehensive measure, "Total disbursements, excluding gross interest payments", the average figure is 0.50% of GDP. For the other measures, i.e. "Current disbursements, excluding gross interest payments", its cyclically-adjusted version, and "Total expenditure, narrow definition", the average deviations are even larger. While the average amount of planned spending measures in a consolidation (0.85% of GDP) exceeds the average amount of planned revenues measures in a consolidation (0.51% of GDP), we find that the average shortfalls from planned spending reductions are proportionally larger than the average shortfalls from planned revenues measures. Based on the average shortfall of revenue measures, we would have expected an average shortfall of spending measures of (0.15/0.51)*0.85 = 0.25% of GDP, half of the actual figure of 0.50% of GDP.

Looking at the individual countries, where we average over the consolidation years, we observe that for the most comprehensive revenues measure, only 5 out of 13 countries exhibit a shortfall. This contrasts with the most comprehensive spending measure, for which we find that 10 out of 13 countries exhibit a shortfall. For the other revenue and spending measures we register higher

fractions of shortfalls, but the spending measure is always characterized by weaker follow-up than the corresponding revenues measure.

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

	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 shortfall of actual implementation from the plan. (ii) Averages are calculated over all consolidation years per country or over all (country, consolidation year) combinations. (iii) D_TREV = deviations based on "Total receipts, excluding gross interest receipts", D_CREV = deviations based on "Current receipts, excluding gross interest receipts", D_CACREV = deviations based on cyclically-adjusted "Current receipts, excluding gross interest receipts", and D_NREV = deviations based on "Total revenues, narrow definition". (iv) The number in brackets is the number of consolidation observations per country. (v) For definitions and construction of variables, see Appendix A.

Table 2: Average *ex-post* deviations for expenditures in percent of GDP

	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 shortfall of actual implementation from the plan. (ii) D_TEXP = deviations based on "Total disbursements, excluding gross interest payments", D_CEXP = deviations based on "Current disbursements, excluding gross interest payments", D_CACEXP = deviations based on cyclically-adjusted "Current disbursements, excluding gross interest payments", and D_NEXP = deviations based on "Total expenditure, narrow definition". (iii) For definitions and construction of variables, see Appendix A. (iv) Further, see the *Notes* to Table 1.

4. A new dataset of fiscal austerity announcements

Announcements of fiscal consolidations may affect private sector and financial market behavior and confidence before the actual consolidation measures are executed, whether in full or only partially. In particular, confidence indicators may respond as soon as the announcements become public and they may internalize the responses of the macro-economy to the announcements, which *a priori* we would expect to be affected by the follow-up, or lack thereof as demonstrated in the previous section. Confidence could potentially also act as a separate channel mediating the response of the macro-economy to the announcements. Accounting for anticipatory behavior as well as the responses of confidence indicators to announcements requires us to identify in the best possible way the precise moment of the announcements. In this section we discuss how we construct our novel dataset on *announcements* of fiscal austerity measures. The comparison in the previous section was by contrast based on the narratively-identified planned measures themselves.

Our dataset covers the thirteen EU countries mentioned earlier over the period 1978-2013. The announcements for the subsample period 1978-2008 are based on the narratively-identified consolidation measures documented at the annual frequency in Devries *et al*. (2011), while the announcements for the subsample period 2009-2013 are based on the consolidation measures narratively identified by Alesina *et al*. (2015a,b) for ten out of the aforementioned thirteen EU countries over this period. Using narrative identification from official contemporaneous government documents we expand their sample with Finland, the Netherlands and Sweden over the period 2009-2013.

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

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 in percent of GDP between now and up to six years ahead of all 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* onwards, while Measure 2 is expected to have a negative marginal effect on the primary balance of 0.2% of GDP from year *t+1* onwards. Then, the value of the announcement that we record for September of year *t-1* is 0.5 – 0.2 = 0.3% of GDP.[10]

The resulting set of announcements constructed at the *monthly* frequency is aggregated to the *quarterly* frequency. The main reason for this conversion is twofold. Firstly, macroeconomic and fiscal variables are (at best) only available at this frequency. Secondly, we aim at mitigating potential anticipation effects associated with 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. By aggregating the monthly announcements to the quarterly frequency, we reduce the problem of potential anticipation effects associated with fiscal news, because fiscal news that is leaked to the media and the official release of fiscal news are more likely to occur in the same period. To further address the potential anticipation effects associated with premature media leakages, we assign any announcement made in the first month of a quarter to the preceding quarter.[11]

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

[10] Since the different components of the announcement would materialize in different years, one might legitimately ask whether they should not be discounted. However, given that the time space between these components is at a maximum a few years and in most cases less, the effect of discounting would be very small and, hence, we abstain from discounting.

[11] 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, 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.

impact of the measures on the primary balance.[12] Hence, the assigned value to the announcement potentially measures the pure shock value of the consolidation plan with some error. Nevertheless, reporting a value has a substantial advantage over merely reporting a simple dummy for a fiscal 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 announcements of larger consolidations elicit stronger responses than announcements of 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. Finally, it allows us to assess the degree of ensuing follow-up in actual 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 assigns in most instances a value for the size of the announcement, instead of a simple dummy for the occurrence of the announcement as in Beetsma *et al.* (2015).

Table 3 reports the magnitudes of the announced plans. Note that the figures refer to the average annual size of the plans, while the plans themselves are dated to the quarter in which their announcement takes place, as described above. In total we have 211 fiscal consolidation announcements. For 180 of them we are also able to establish the magnitude of their impact on the primary balance. The cumulative annual impact of the measures on the primary balance ranges between 0% and 9.3% of GDP over a maximum period of 6 years, with an average value of 1.37% of GDP in our country sample.[13] The average spending-based content is 0.85% of GDP and the average revenue-based content is 0.51% of GDP. The cross-country average horizon of the consolidation plans ranges between 1.3 and 2.3 years.

Most consolidation plans combine measures on both the revenue and the expenditure side of the budget, which is why in Table 4, following the existing literature, we classify announcements 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-

[12] 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 an announced plan. 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.

[13] 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.

based". The five 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 announcements separately.

As Table 4 shows, the majority of the announcements in our sample are spending-based. Based on our data, though not reported in the tables, we find that the average spending-based plan 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. The average revenue-based plan announcement has a value of 1.26% of GDP, with an impact of 0.31% of GDP on the spending side and 0.95% of GDP on the revenue side.

Table 3: Summary statistics of fiscal announcement data

	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*

Table 4: Announcements according to the predominance of their instruments

	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*

5. Panel VAR analysis of follow-up, multipliers and confidence channel

In Section 3 we have documented that follow-up is weaker for spending-based than for revenue-based consolidation plans. That analysis provides a stepping stone to this section, in which we, using the new dataset constructed in the previous section, investigate the economy's responses to announcements of consolidation plans in a panel VAR, while accounting for anticipation effects. We show that revenue-based consolidation announcements have more adverse consequences for the economy than spending-based consolidation announcements. We try to disentangle the roles of differences in follow-up and in multipliers for this finding, while also exploring the mediating effect of the confidence channel.

5.1. The empirical specification

We estimate a quarterly Bayesian panel VAR model of the format:

$$Z_{i,t} = \alpha_i + \beta_i t + \gamma d_t + \sum_{l=1}^{L} A_l Z_{i,t-l} + u_{i,t}, \quad (3)$$

where i indicates the country and t the quarter, $Z_{i,t}$ is a vector of endogenous variables, α_i is a country-fixed effect, β_i is the coefficient of a country-specific linear time trend, d_t a vector of seasonal dummies with coefficient vector γ and $u_{i,t} \sim N(0, \Omega)$ a vector of zero-mean, stationary reduced-form disturbances. *L* represents the number of lags included in the panel VAR and A_l is the matrix of coefficients associated with the l^{th} lag of the endogenous variables. 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 in percent of GDP, $\tau_{i,t}$ and $g_{i,t}$ are nominal government revenues, respectively nominal government expenditures, in percent of nominal GDP, $y_{i,t}$ is the logarithm of real GDP, $c_{i,t}$ is the logarithm of real private consumption, $LTI_{i,t}$ is the long-term interest rate in basis points and $CCONF_{i,t}$ is consumer confidence in percent deviation from its average. Real macroeconomic variables are obtained using the GDP deflator.[14] The impulse responses should be interpreted as percent deviations from the original values, except in the case of government revenues and expenditures, where they are percentage point of GDP deviations from their original values, and in the case of interest rates, where they are basis point deviations from their original values. Importantly, because we are assessing the follow-up of consolidation announcements in terms of actual measures, the definitions of revenues and expenditures should correspond as closely as possible to the potential sets of measures included in the revenues

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

respectively expenditure components of the fiscal consolidation. This implies in particular that $g_{i,t}$ will include transfers and is, hence, more broadly defined than merely government purchases.

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 to be short on average. 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).[15] In addition, media coverage of a new policy measure sometimes predates its official announcement. By looking at military spending, Ramey (2011) finds that news reports about wars Granger-cause increases in defense spending, thus providing evidence of the anticipation of government spending shocks. If anticipated changes in revenues and public spending prompt economic agents to respond before the fiscal measures are actually implemented, the innovations identified in a structural VAR do not correspond to the true timing of the shocks. Formally, the moving-average representation of the VAR system is not invertible (Leeper *et al*., 2013), leading to biased estimates.

Existing datasets based on the narrative identification of consolidation plans do not fully account for the combined effect of legislative and implementation lags in fiscal policy. For example, the annual dataset of Devries *et al.* (2011) assigns consolidation measures to the year when they are supposed to be implemented, regardless of the year when they are announced. 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 unanticipated, if they had been announced in the preceding fall as part of a multiannual consolidation plan. However, the authors do not identify the exact moment of the consolidation announcement, which is critical to account for potential anticipation effects. Unlike these contributions, by timing austerity plans to the moment of their announcement in our newly-constructed data set, we can take explicit account of the anticipation effects during the plan's legislation and implementation phases. Hence, in contrast to Guajardo et al. (2014), for example, we allow macroeconomic variables to already respond to consolidation news before measures are actually implemented.

[15] However, in our dataset in many instances announcements coincide with the new budget for the next year, in which the implementation of the announced package starts.

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 identify the structural shocks by using a Cholesky decomposition of the residual covariance matrix:

$$\Omega = A_0 A_0'$$

where the contemporaneous impact matrix A_0 is lower triangular. Fiscal consolidation announcements are ordered first, allowing the austerity news to be predicted only by lags (of at least one quarter) of the economic and financial variables in the VAR. In doing so, the VAR equation corresponding to the fiscal consolidation announcements could be interpreted as a "policy announcement reaction function", with its residuals representing the discretionary fiscal consolidation news.[17] As demonstrated in Christiano *et al*. (1999), under a recursive identification the impulse responses of the endogenous variables in the block following the announcement shock are invariant to the ordering of these variables *vis-à-vis* each other.

We expect anticipation effects to work not only through real variables like output and private consumption, but also through private sector confidence, which may react immediately to announcements and which many commentators believe to play an important role in the transmission of fiscal consolidations. Our newly-constructed dataset on consolidation announcements allows us to incorporate this confidence channel as well, which we capture by including the consumer confidence indicator and the long-term interest rate into our panel VAR model. If it is indeed of relevance, then not accounting for the confidence channel could potentially lead to wrong inference.

We opt for a baseline specification containing four lags of the endogenous variables, hence amounting to a maximum lag length of one year. As shown below, the main results of the paper are robust to different choices of the lag structure and other configurations of the deterministic components. Note that the use of Bayesian estimation provides an efficient way to estimate error bands for impulse responses via Markov chain Monte-Carlo simulation.

[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 announcements can be predicted by past values of long-term interest rates.

[17] In Section 5.3 we show that the results are robust when the consolidation announcement shock is included as an exogenous variable.

5.2. Baseline estimates

Following Bańbura *et.al* (2012), a Normal-Inverse Wishart prior distribution is used for the VAR parameters with the prior hyper-parameters set to reflect a loose prior belief. The posterior distribution is approximated using a Gibbs sampling algorithm described in Appendix D. We conduct 25,000 replications with the last 10,000 draws used for inference. The inefficiency factors reported in Appendix D provide evidence in favour of convergence. As is standard in Bayesian VAR papers, the figures report 68% error bands, which are approximately one standard-deviation intervals for a normal distribution. We estimate the panel VAR model on our sample of 13 European Union countries over the period 1978:Q1-2013:Q4.

Figure 1 reports the baseline responses when all consolidation announcements are included. Here, and in the sequel, the shock, which takes place at time 0, is a consolidation announcement normalized to 1% of GDP. To save space and because they exhibit no persistence, we do not depict the impulse responses of the announcements themselves. We observe a highly significant rise in public revenues of more than 0.15% of GDP after a year and a peak fall in public expenditures of roughly the same magnitude after three quarters. Both responses depress disposable income, which may help to explain the fall in GDP, which reaches a maximum of around 0.2% after about two-and-a-half years. Private consumption exhibits a maximum deterioration of slightly less than 0.4% after about two years, while the long-term interest rate exhibits a positive jump on impact and peaks at 15 basis points after two quarters, after which it converges back to its steady state within about 4 years. Finally, consumer confidence jumps down on impact and declines by around 1.3 percent after half a year. Not surprisingly, because they are forward looking, the long-term interest rate and consumer confidence respond instantaneously to consolidation news and peak much earlier than the other variables.

Next, we split the news into announcements of revenue-based and spending-based plans (Figure 2). These are included simultaneously in the VAR model with the revenue announcements ordered first. As the two announcement series are virtually uncorrelated, this ordering is innocuous (as we will show in our robustness analysis below). For both types of consolidation plan announcements, the shock is normalized to 1% of GDP. Clear differences show up. The announcement of a revenue-based plan produces a systematic increase in revenues reaching a maximum of about 0.6% of GDP after three quarters, while GDP and private consumption exhibit reductions that reach maxima of around 1 and 1.5 percent, respectively, after about two years. Public spending stays put, while the long-term interest rate peaks at a maximum of about 40 basis points after half a year. Consumer confidence falls by a maximum of almost 6 percent after half a year.

Apart from public spending itself, with maximum declines of 0.20 – 0.25 percentage points of GDP, all the other responses under the spending-based announcements remain close to zero. In particular, GDP and private consumption remain almost perfectly flat. The long-term interest rate reaches a peak of slightly more than 10 basis points. The final column of Figure 2 shows that the confidence bands on the differences in the responses to the revenue- and spending-based plan announcements all deviate from zero. The difference in revenues between the plan types peaks at about 0.6 percent of GDP and that in expenditures at roughly 0.3 percent of GDP. The difference between the GDP responses reaches a maximum of about one percent and that between the consumption responses of almost 1.5 percent. Finally, the maximum difference in the responses of the long-term interest rate is almost 30 basis points and that of consumer confidence is around 5 percent. In the case of spending-based announcements the confidence variables appear to behave consistently with the subsequent trajectory of the economy: the lack of movement in consumer confidence is consistent with the fact that economic activity hardly changes in the ensuing period, while the very modest movement in the long-run interest rate is consistent with the health of the public finances effectively exhibiting only limited change.[18] In the case of revenue-based announcements, the deterioration of consumer confidence is in line with the ensuing deterioration of economic activity. The increase in the long-run interest rate is more difficult to interpret: more follow-up improves the public finances, while weaker activity would undermine it.[19] Hence, the net effect on the public financial health is unclear. Another possibility is that the financial market interprets the announcement of a revenue-based consolidation as a negative signal about the underlying health of the public finances.

5.3. Robustness of the baseline

We investigate the robustness of our baseline estimates in various ways. For each robustness variant, Figure 3 depicts the differences in revenue, expenditure and real GDP responses between a revenue- and expenditure-based consolidation announcement (i.e., each row in Figure 3 is the "transposed" analogue of the first three panels in the third column of Figure 2). First, it could be argued that by including the recent crisis period in our time sample we capture an atypical period, during which the responses of economies to announcement shocks could differ from those in other periods. However, the differences in impulse responses when we drop the period 2008–2013 confirm the differences in the baseline responses for the two types of consolidation. This is also the

[18] The confidence variables may also factor in the relatively weak follow-up of spending, resulting in relatively low credibility of spending-based consolidation announcements. Hence, confidence variables would also not move much for this reason.

[19] Notice that the higher degree of follow-up to revenue-based announcements may be partially explained by the slowdown of activity. This is what we refer to as "passive follow-up" in Appendix C.

case when the (relevant) nominal variables are expressed as shares of potential nominal GDP,[20] and when we replace the revenue and spending ratios of GDP by the logarithms of real revenues and real spending, although here the difference in revenue responses between the two plan types is a bit larger. Restricting ourselves to revenue plans that contain at least 60% revenues measures and spending plans that contain at least 60% spending measures yields again responses that are qualitatively and quantitatively very similar to those under the baseline. The same is the case if we include a time dummy for each quarter in the sample or allow for eight instead of four lags in the panel VAR. As our next robustness check, 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 response differences 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. The impulse responses are in all instances rather compactly clustered around the original responses, and in any case contained within the baseline confidence intervals, thus suggesting that no individual country drives our baseline estimates.

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 consolidation announcements, it is doubtful that these can be explained by differences in the monetary responses alone, because the ECB only responds to euro-area wide macroeconomic developments and not to those in individual countries. Likewise, in the period before EMU, the Bundesbank only responded to German developments and not to those in other countries pegging their exchange rate to the German mark. Nevertheless, to control for monetary policy, we replace the long-term interest rate with the short-term interest rate, which is closer to the central bank's policy instrument. However, again the impulse response differences are qualitatively and quantitatively essentially unaffected.[21]

[20] Potential nominal GDP is constructed by applying to the logarithm of nominal GDP a Hodrick-Prescott filter and taking the exponential of the resulting trend estimate.

[21] It seems implausible that the rise in the short-term interest rate is driven by a monetary tightening, because, if anything, we would expect monetary policy to become looser to avert the slowdown of the economy induced by a revenue-based consolidation announcement. In any case, a counterfactual in which we force the

An alternative to the current baseline would have been a specification with business confidence instead of consumer confidence. However, we have fewer observations on business confidence than on consumer confidence and, hence, we would lose degrees of freedom in comparison to our current baseline. Including business confidence alongside consumer confidence in the model would require the estimation of even more parameters, and thus cause a further loss of degrees of freedom. Moreover, the question arises which indicator of confidence would be more important. *A priori*, to the extent that confidence affects the real economy, we expect consumer confidence to affect private consumption and business confidence to affect private investment. Private consumption is a substantially larger fraction of GDP; hence, consumer confidence seems to be the more relevant variable to include in the baseline. Nevertheless, in this robustness check we replace the consumer confidence indicator with the business confidence indicator and private consumption with private investment. As under our baseline in the case of a revenue-based consolidation, real GDP again exhibits a fall, though this fall is smaller than under the baseline and, hence, the difference between the two types of consolidation announcements is also smaller. Private investment (not displayed) also exhibits a fall under a revenue-based consolidation announcement. Overall, the baseline specification seems to be more appropriate than the alternative with business investment and business confidence.

The long-term interest rate may be viewed as an indicator of financial market confidence. The baseline impulse responses show that, in response to announcements of revenue increases, the long-term interest rate rises and consumer confidence falls, while both variables (essentially) stay put in response to announcements of spending reductions. Hence, as indicators of financial market and consumer confidence the two variables behave consistently *vis-à-vis* each other. However, we would ideally like to rule out the possibility that the behavior of the long-term interest rate is driven by factors other than confidence. In particular, consolidation announcements may affect the long-term interest rate through their effect on inflation expectations. *A priori*, however, if a consolidation announcement is not expected to affect the economy, we would also expect it not to raise the long-term interest rate by pushing up expected inflation.[22] Hence, the fact that GDP falls following the revenue announcement with, if anything, potentially deflationary effects, suggests that the increase in the long-term interest rate is attributable to a decrease in confidence. To confirm that the results are not driven by movements in inflation expectations, we redo the baseline regressions by replacing the long-run interest rate with its difference with respect to realized CPI inflation, i.e. the *ex-post*

short-term interest rate to stay constant does not affect the impulse responses (results are available upon request).

[22] A potential exception concerns a consolidation that takes place largely through an increase in value-added taxes, which has a direct, though temporary, positive effect on inflation.

long-run real interest rate. Figure 3 shows that our baseline results are unaffected. Finally, in the penultimate robustness check we order spending announcement shocks before revenue announcement shocks, and in the final robustness check we include the announcement shocks as exogenous variables in the panel VAR. The results are very similar to those under the baseline.

5.4. Disentangling the sources of the differences in GDP responses

Exploiting our data on the announcements of consolidation plans, this subsection tries to disentangle the sources of the differences in GDP responses to the two types of announcements, focusing in particular on differences in follow-up and differences in multipliers, while exploring the mediating effect of the "confidence channel" captured by the responses of consumer confidence and the long-run interest rate.

The roles of differences in follow-up and differences in multipliers

Define the cumulative primary-balance multiplier of output following a one-percent of GDP announcement shock F as:

$$m_{F,h} \equiv \sum_{j=1}^{h} \hat{y}_{F,j} / \sum_{j=1}^{h} pb_{F,j}, \quad (4)$$

where h is the horizon (in quarters), $\hat{y}_{F,j}$ is the percent deviation of real output from its original value and $pb_{F,j}$ is the impulse response of the primary balance in percentage points of GDP following the announcement shock. In other words, the cumulative primary-balance multiplier $m_{F,h}$ of output is the cumulative percent change of real output, divided by the cumulative improvement in the primary balance in percent of GDP over a horizon of h periods. We denote $F = r$ in the case of a revenue-based announcement and $F = s$ in case of a spending-based announcement.

To explore the relative roles of differences in follow-up and differences in multipliers for the output dynamics, we write

$$\sum_{j=1}^{h} \hat{y}_{F,j} = m_{F,h} \sum_{j=1}^{h} pb_{F,j},$$

Hence, the cumulative change in output resulting from an announcement shock, i.e. the left-hand side of the above expression, is the product of the degree of follow-up of the announcement, i.e. the second factor on the right-hand side, and the cumulative multiplier, given by the first factor on the right-hand side overall.

Rows 1 and 2 of Table 5 report the figures for the cumulative multiplier $m_{F,h}$ under a revenue-based, respectively spending-based announcement. As variables are forced to return to their baseline, we

limit ourselves to a maximum horizon of 20 quarters. The cumulative primary-balance multiplier under revenue-based announcements is negative at all horizons and falling with the horizon. The effect on output per percentage-point improvement in the primary balance is substantial and reaches minus 3 percent after 5 years. The cumulative primary-balance multiplier under spending-based announcements is always close to zero. It starts out as positive and becomes mildly negative after some years. Row 3 of Table 5 reports the difference in the cumulative primary-balance multipliers.

Because most consolidation plans are mixtures of revenue and spending measures we also construct alternative decompositions of the cumulative output responses based on the fact that we can write:

$$\sum_{j=1}^{h}\hat{y}_{r,j} = \left(\sum_{j=1}^{h}\hat{y}_{r,j}/\sum_{j=1}^{h}\tau_{r,j}\right)\left(\sum_{j=1}^{h}\tau_{r,j}\right),$$

$$\sum_{j=1}^{h}\hat{y}_{s,j} = \left(\sum_{j=1}^{h}\hat{y}_{s,j}/\sum_{j=1}^{h}g_{s,j}\right)\left(\sum_{j=1}^{h}g_{s,j}\right),$$

where $\sum_{j=1}^{h}\tau_{r,j}$ is the cumulative change in revenues over GDP over the horizon h for a one percent of GDP revenue-based announcement and likewise $\sum_{j=1}^{h}g_{s,j}$ is the cumulative change in spending over GDP for a one percent of GDP spending-based announcement. Table 5, rows 4 and 5, shows that the cumulative multipliers $\sum_{j=1}^{h}\hat{y}_{r,j}/\sum_{j=1}^{h}\tau_{r,j}$ and $\sum_{j=1}^{h}\hat{y}_{r,j}/\sum_{j=1}^{h}\tau_{r,j}$ are quite similar to those calculated above, which reflects the dominance of revenue measures in revenue-based plans and spending measures in spending-based plans. Moreover, the confidence intervals on the differences reported in row 6 of Table 5 do not contain zero at any of the horizons reported.

Rows 1 and 2 of Table 5 also report the figures for the degree of follow-up $\sum_{j=1}^{h}pb_{F,j}$ under the two announcement types, while row 3 also reports the difference in follow-up. We observe that, in addition to the cumulative multiplier being larger (in absolute terms) for revenue-based than for spending-based announcements, also the degree of follow-up at the various horizons is larger under the former. In particular, follow-up is one-and-a-half to two times larger for revenue-based announcements. As seen from row 3, the confidence band is fully above zero after one year. Rows 4 and 5 report follow-up in terms of the "own" instrument, i.e. $\sum_{j=1}^{h}\tau_{r,j}$ for revenue-based and $\sum_{j=1}^{h}g_{s,j}$ for spending-based announcements. The numbers are rather close to the corresponding numbers in rows 1 and 2, thus indicating that follow-up in terms of the own instrument is larger for revenue- than for spending-based announcements. Now, the confidence band on the difference is uniformly above zero both at the four- and eight-quarter horizon (see row 6 of Table 2).

Table 5: Cumulative fiscal consolidation multipliers at various horizons

Row	Plan type	Description	Expression	*h*=4	*h*=8	*h*=12	*h*=20
1	Revenue-based	Cumulative multiplier primary budget balance	$\sum_{j=1}^{h}\hat{y}_{r,j} / \sum_{j=1}^{h} pb_{r,j}$	-0.98 (-2.0,-0.45)	-1.8 (-4,-0.85)	-2.5 (-6.5,-1.1)	-3.0 (-7.9,-1.1)
		Cumulative response primary budget balance	$\sum_{j=1}^{h} pb_{r,j}$	1.4 (0.9,1.9)	2.7 (1.5,3.9)	3.3 (1.5,5.2)	4.8 (2.0,7.5)
2	Spending-based	Cumulative multiplier primary budget balance	$\sum_{j=1}^{h}\hat{y}_{s,j} / \sum_{j=1}^{h} pb_{s,j}$	0.1 (-0.48,0.55)	0.09 (-0.63,0.57)	0.01 (-1.0,0.6)	-0.2 (-1.9,0.7)
		Cumulative response primary budget balance	$\sum_{j=1}^{h} pb_{s,j}$	0.7 (0.42,1.0)	1.8 (1.1,2.5)	2.5 (1.5,3.6)	3.3 (1.8,4.8)
3		Difference cumulative multipliers		-1.1 (-2.2,-0.29)	-1.9 (-4.1,-0.67)	-2.5 (-6.4,-0.61)	-2.7 (-7.7,-0.12)
		Difference cum. primary balance responses		0.7 (0.1,1.2)	0.9 (-0.4,2.3)	0.8 (-1.2,2.9)	1.5 (-1.5,4.5)
4	Revenue-based	Cumulative multiplier revenues	$\sum_{j=1}^{h}\hat{y}_{r,j} / \sum_{j=1}^{h} \tau_{r,j}$	-1.3 (-2.4,-0.66)	-1.9 (-3,-1.2)	-2.5 (-4.2,-1.5)	-3.5 (-6.2,-1.8)
		Cumulative response revenues	$\sum_{j=1}^{h} \tau_{r,j}$	1.1 (0.8,1.5)	2.6 (1.9,3.4)	3.5 (2.4,4.5)	4.4 (3.0,5.9)
5	Spending-based	Cumulative multiplier spending	$\sum_{j=1}^{h}\hat{y}_{s,j} / \sum_{j=1}^{h} g_{s,j}$	-0.41 (-1.3,0.47)	-0.50 (-1.3,0.50)	-0.45 (-1.5,0.85)	-0.25 (-1.7,1.9)
		Cumulative response spending	$-\sum_{j=1}^{h} g_{s,j}$	0.57 (0.4,0.8)	1.5 (0.9,2.1)	2.14 (1.2,3.1)	2.9 (1.5,4.3)
6		Difference cumulative multipliers		-0.90 (-2.0,-1.2)	-1.4 (-2.8,-0.46)	-2.1 (-4.2,-0.8)	-3.2 (-6.7,-1.3)
		Difference cum. primary balance responses		0.53 (0.1,1.0)	1.2 (0.2,2.1)	1.4 (-0.1,2.7)	1.6 (-0.5,3.5)

Notes: (i) The announcement shock always has a magnitude of 1 percent of GDP. (ii) horizon h is expressed in quarters. (iii) 68% error bands in parenthesis.

The cumulative fiscal consolidation multipliers and their respective differences are also depicted in the first two rows of Figure 4. The bottom row of this figure adds a policy experiment. The left-most panel of this row depicts the cumulative primary-balance multipliers of output for revenue-based announcements, while holding the actual spending response fixed at zero for the first K=8 quarters. This is achieved, as in Mountford and Uhlig (2009), by feeding the system with additional shocks, calculated via a non-linear equation solver, to ensure that the response of the relevant variable, in

this case actual spending, to the consolidation announcements is zero.[23] The middle panel of the bottom row reports the analogous multiplier under spending-based announcements, while holding the actual revenue response at zero for the first 8 quarters. The idea of the experiment is to show the "pure" cumulative multipliers by purging the response of spending (revenues) under the revenue-based (spending-based) plan announcements. The right-most panel of the bottom row depicts the difference. The figures are qualitatively and quantitatively similar to those in the previous rows, indicating that the role of spending changes for output in revenue-based plans is minor, and *vice versa*. Again, the confidence interval of the cumulative primary-balance multiplier for revenue-based announcements lies entirely below zero, and so does the difference.

Further exploration of follow-up and multipliers: counterfactual

To further explore the roles of differences in follow-up and differences in multipliers for the GDP responses under the two type of announcements, we turn to a counterfactual scenario. The idea behind the counterfactual is that we impose the same follow-up under the two types of announcements, so that the difference in output effects has to be due to differences in multipliers. Specifically, we can lift the degree of follow-up under a spending-based announcement to the actual follow-up under a revenues-based announcement, and *vice versa*.

We proceed as follows. We can calculate the counterfactual cumulative output effect in percent (indicated by a tilde) under revenue-based announcements for some counterfactual primary-balance path (indicated by subscript *c*) as:

$$\sum_{j=1}^{h} \widetilde{y_{r,J}} = m_{r,h} \sum_{j=1}^{h} pb_{c,j}$$

where $m_{r,h}$ is the multiplier calculated above. The implicit assumption is that the multiplier per unit of primary balance increase is independent of the size of the increase. Lagging this expression by one period, subtracting it from the original one, and rewriting, yields the counterfactual value of output growth after h quarters:

$$\widetilde{y_{r,h}} = m_{r,h} \sum_{j=1}^{h} pb_{c,j} - m_{r,h-1} \sum_{j=1}^{h-1} pb_{c,j}.$$

We can similarly calculate the counterfactual value of output growth after h quarters under a spending-based announcement:

[23] This experiment is not subject to the Lucas critique, because the VAR parameters are not modified. However, it is subject to the caveat that the additional shocks used to satisfy the constraints are not labeled from an economic perspective.

$$\widetilde{y_{s,h}} = m_{s,h}\sum_{j=1}^{h} pb_{c,j} - m_{s,h-1}\sum_{j=1}^{h-1} pb_{c,j}.$$

Hence, $\widetilde{y_{r,h}}$ and $\widetilde{y_{s,h}}$ represent the responses of output to revenue- and spending-based consolidation announcements imposing some common counterfactual follow-up trajectory for the primary balance.

We can now construct $\left(\hat{y}_{r,h} - \hat{y}_{s,h}\right) - \left(\widetilde{y_{r,h}} - \widetilde{y_{s,h}}\right)$, i.e. the difference in GDP responses under revenue- and spending-based announcements based on the *estimated actual* follow-up in both cases, *minus* the difference in GDP responses under some *identical counterfactual* follow-up scenario.

We consider two specific common counterfactual scenarios: in the first, we impose that the counterfactual primary balance follow-up is identical to the actual follow-up under the revenue-based announcement. Hence, $\widetilde{y_{r,h}} = \hat{y}_{r,h}$, so $\left(\hat{y}_{r,h} - \hat{y}_{s,h}\right) - \left(\widetilde{y_{r,h}} - \widetilde{y_{s,h}}\right)$ reduces to $-\left(\hat{y}_{s,h} - \widetilde{y_{s,h}}\right)$. This quantity captures for spending-based announcements the difference in output responses resulting from different follow-ups. As shown in the top panel of Figure 5, the confidence band around this difference always contains the horizontal axis except in the first quarter after the announcement. The size of the difference is always less than 0.1% of original GDP. By contrast, if we impose that the counterfactual primary balance follow-up is identical to the actual follow-up after the spending-based announcement, the aforementioned difference reduces to $\hat{y}_{r,h} - \widetilde{y_{r,h}}$, which is the difference in output responses from different follow-up under revenue-based announcements. This difference is negative, as expected, because the counterfactual follow-up is smaller than the actual follow-up. As of almost four years after the announcement the confidence band falls entirely below the horizontal axis (see bottom panel in Figure 5) and, in terms of magnitude, the difference is larger than that depicted in the top panel, reaching a maximum of approximately 0.5% of original GDP. Hence, if the follow-up under revenue-based announcements were to drop to the actual level under spending-based announcements, then the drop in GDP would be substantially smaller.

The combination of the two panels of Figure 5 suggests that the differences in the GDP responses following the two announcement types are the result of the *combination* of larger follow-up under revenue-based announcements *and* a larger multiplier following this type of announcement. This is confirmed by Figure 6, where the solid red lines in the first two panels depict the multipliers under the two types of announcements and in the third panel the difference between these multipliers.

The confidence channel

So far, we have shown that both differences in follow-up and in multipliers between different types of consolidation announcements matter for the ensuing GDP responses. Our dating of consolidation announcements provides us with the opportunity to explore whether the confidence channel, captured here by the responses of consumer confidence and the long-run interest rate, matters for differences in the multipliers.

As a preliminary step to exploring this question we follow the approach introduced by Kilian and Lewis (2011) and decompose the responses of our confidence variables conditional on the announcement shock into their driving sources. Figure 7 shows that during the first quarters it is *the announcement itself* that forces consumer confidence and the long-term interest rate to move, after which the respective variables become primarily driven by their own lags – a reflection of their persistence. Notably, the *actual* implementation of the revenue and expenditure measures has very little explanatory power. This suggests that it is the announcement of the specific plan type itself that is mostly driving private sector confidence and not the subsequent follow-up and trajectories of other economic variables. Given the absence of feedback of these latter variables onto our confidence variables, it is interesting to explore the responses of these variables when we hold our confidence variables fixed in response to the announcement shocks. This way we can explore the magnitude of the confidence channel.

Hence, we re-estimate the counterfactuals shown in Figure 5 (where we impose counterfactual follow-up to be equal to actual follow-up under either the revenues- or spending-based announcement) under the additional restriction that the confidence variables equal zero for a number of periods. Concretely, we compute again the above difference $\left(\hat{y}_{r,h} - \hat{y}_{s,h}\right) - \left(\widetilde{y_{r,h}} - \widetilde{y_{s,h}}\right)$, where we impose now that in all cases the long-term interest rate and consumer confidence are held at zero for the first two years ($K = 8$).[24] In other words, this restriction is imposed both for $\hat{y}_{r,h}$ and $\hat{y}_{s,h}$ (referred to as the "pseudo-actual" values below), and for the counterfactual values $\widetilde{y_{r,h}}$ and $\widetilde{y_{s,h}}$, which are again based on identical counterfactual follow-up trajectories. The dashed lines in Figure 5 depict the results of this experiment. In the top panel of Figure 5, these dashed lines show the new trajectory for $-\left(\hat{y}_{s,h} - \widetilde{y_{s,h}}\right)$, obtained by imposing equal primary balance follow-up under the counterfactual as under the "pseudo-actual" scenario following a revenue-based announcement. The dashed line in the bottom panel of Figure 5 depicts the new trajectory for $\left(\hat{y}_{r,h} - \widetilde{y_{r,h}}\right)$, based on equal follow-up under the counterfactual as under the "pseudo-actual"

[24] Results (which are available upon request) are robust if we impose a longer horizon of 4 years (K=16).

scenario following a spending-based announcement. We observe that over the entire trajectory the horizontal axis is contained in the confidence band on $-\left(\hat{y}_{s,h} - \widetilde{y_{s,h}}\right)$, while for $\hat{y}_{r,h} - \widetilde{y_{r,h}}$ it falls outside the confidence band about 3 years after the announcement. Indeed, Figure 6 suggests that suppressing the confidence channel produces a substantial reduction in the multiplier following a revenue-based consolidation announcement. We also observe from the third panel of Figure 6, that the difference in GDP responses between the two types of announcements is more than halved. Still, the confidence band on this difference does not contain the horizontal axis. Overall, our analysis suggests that the confidence channel plays a non-negligible role in the transmission of revenue-based consolidation announcements to the real economy.

6. Conclusions

Existing literature shows that narratively-identified spending-based consolidations have milder effects on the economy than revenue-based consolidations. In line with this, and using a newly-constructed narrative dataset on fiscal consolidation *announcements*, this paper demonstrates that announcements of revenue-based plans have more adverse effects on the economy than announcements of spending-based plans. We use a panel VAR analysis to show that the combination of larger actual follow-up and larger revenue multipliers helps to explain the different economic consequences of the two types of announcements. The unique nature of the dataset allows us to take proper account of fiscal anticipation effects and to also control for the confidence channel following the announcements. Indeed, we find that for a given announcement shock our confidence indicators, the long-term interest rate and consumer confidence, are mostly driven by the announcement shock itself. The confidence channel is important for the (different) trajectories of economic activity following the announcements: shutting off the confidence channel, the revenue multiplier in particular drops substantially, thereby to a large extent eliminating the effect on GDP of the difference in follow-up between revenue- and spending-based consolidation announcements.

Our analysis suggests a number of avenues for further research. First, it would be interesting to explore the extent to which the shortfalls from announcements of revenues- or spending-based consolidations originate from a lack of full implementation or miscalculation of the impact on GDP. Second, it may be possible that the effects of consolidation announcements depend on the state of the economy or the public finances and, hence, it could be worthwhile to investigate this. Finally, the composition of spending and revenues packages may matter. For example, the resistance to rolling back certain transfers may be larger than to increasing VAT, making announcements of the latter more credible than of the former. However, the latter two avenues for further work would require an expansion of the current dataset in order to draw reliable conclusions.

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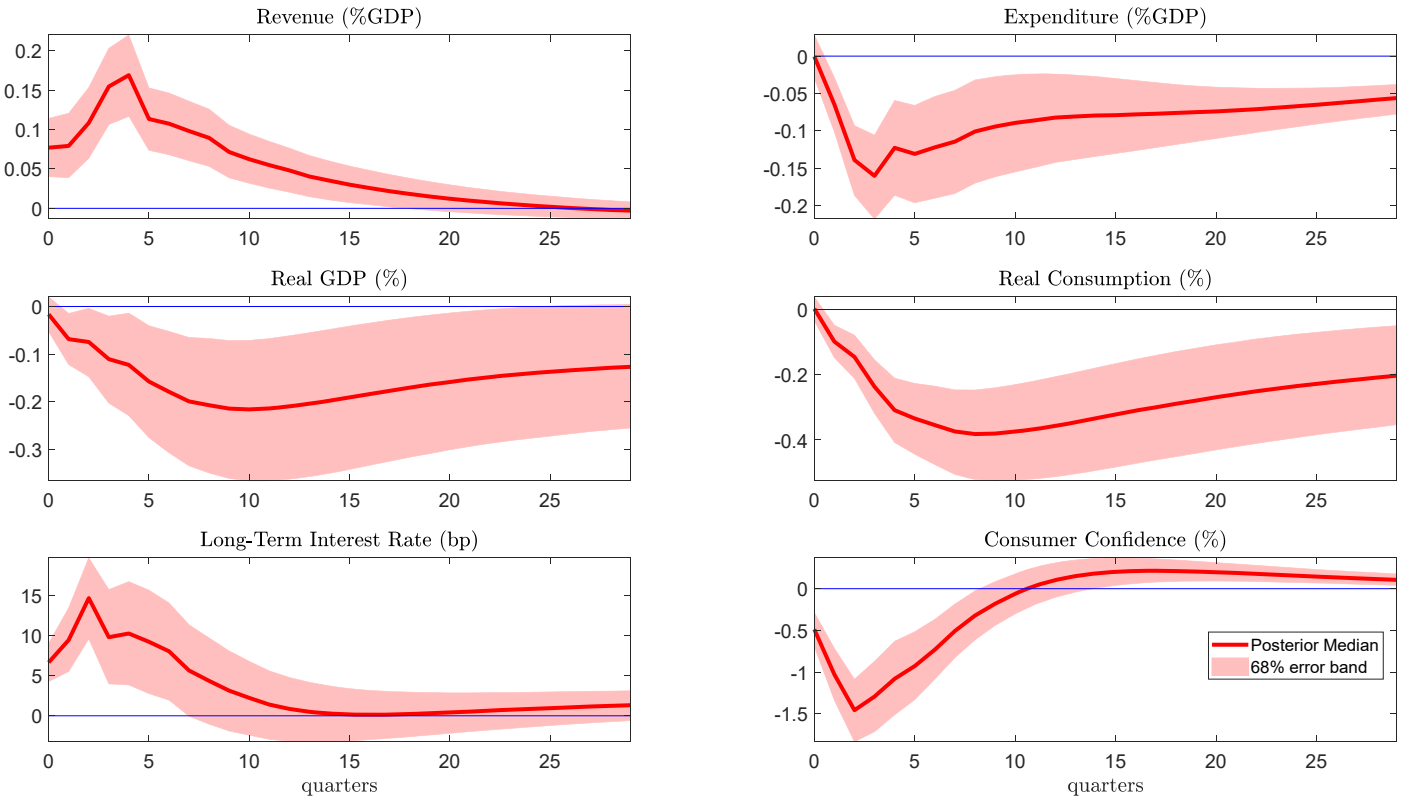
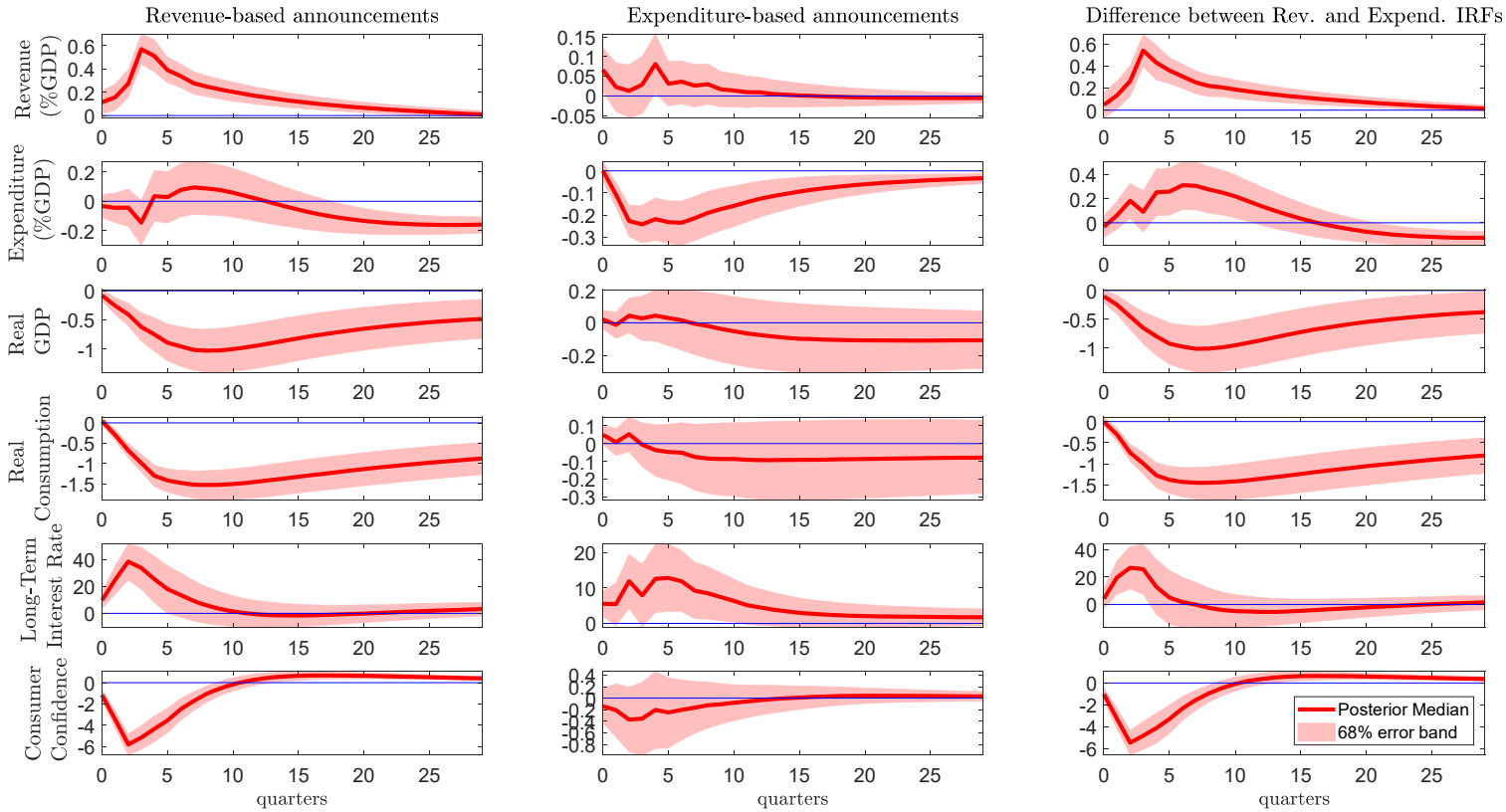
Figures:

Figure 1: Impulse responses baseline model – all consolidation plan announcements



Notes: (i) The consolidation announcement shock (not portrayed) always has a magnitude of 1 percent of GDP. (ii) The figure depicts median impulse responses and their 68% error band. (iii) The impulse responses for revenues and spending are deviations in percentage points of GDP from their original values; real GDP, real private consumption and consumer confidence are in percent deviations from their original values; and the long-term interest rate is the deviation in basis points from its original value.

Figure 2: Impulse responses baseline model – revenue- and spending-based plan announcements



Notes: See *Notes* to Figure 1. Further, the first column depicts the impulse responses to revenue-based plan announcements, while the second column exhibits the impulse responses to spending-based plan announcements. The third column shows the differences in the impulse responses subtracting the second from the first column.

Figure 3: Robustness analysis: differences revenue- and spending-based announcements

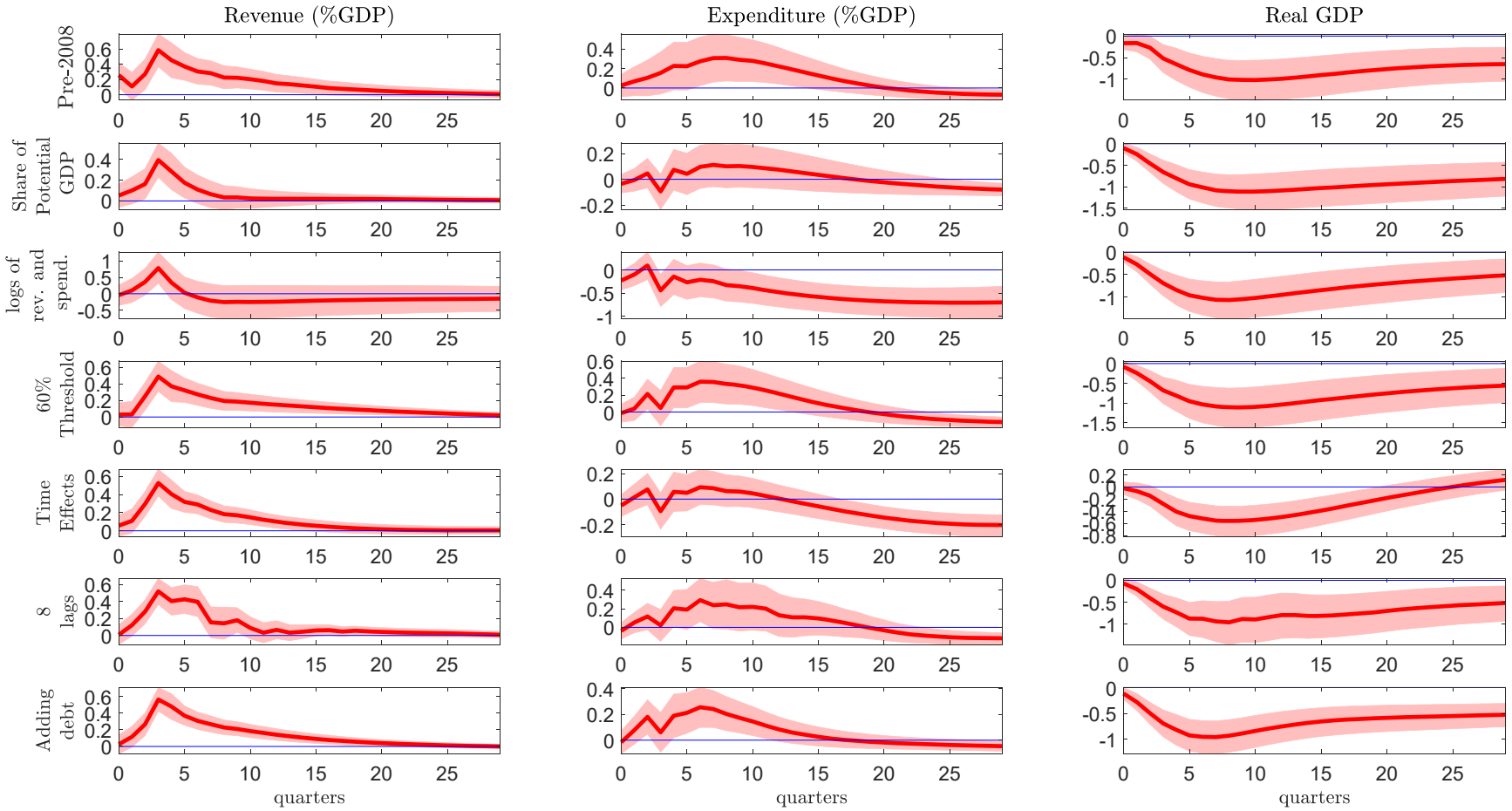
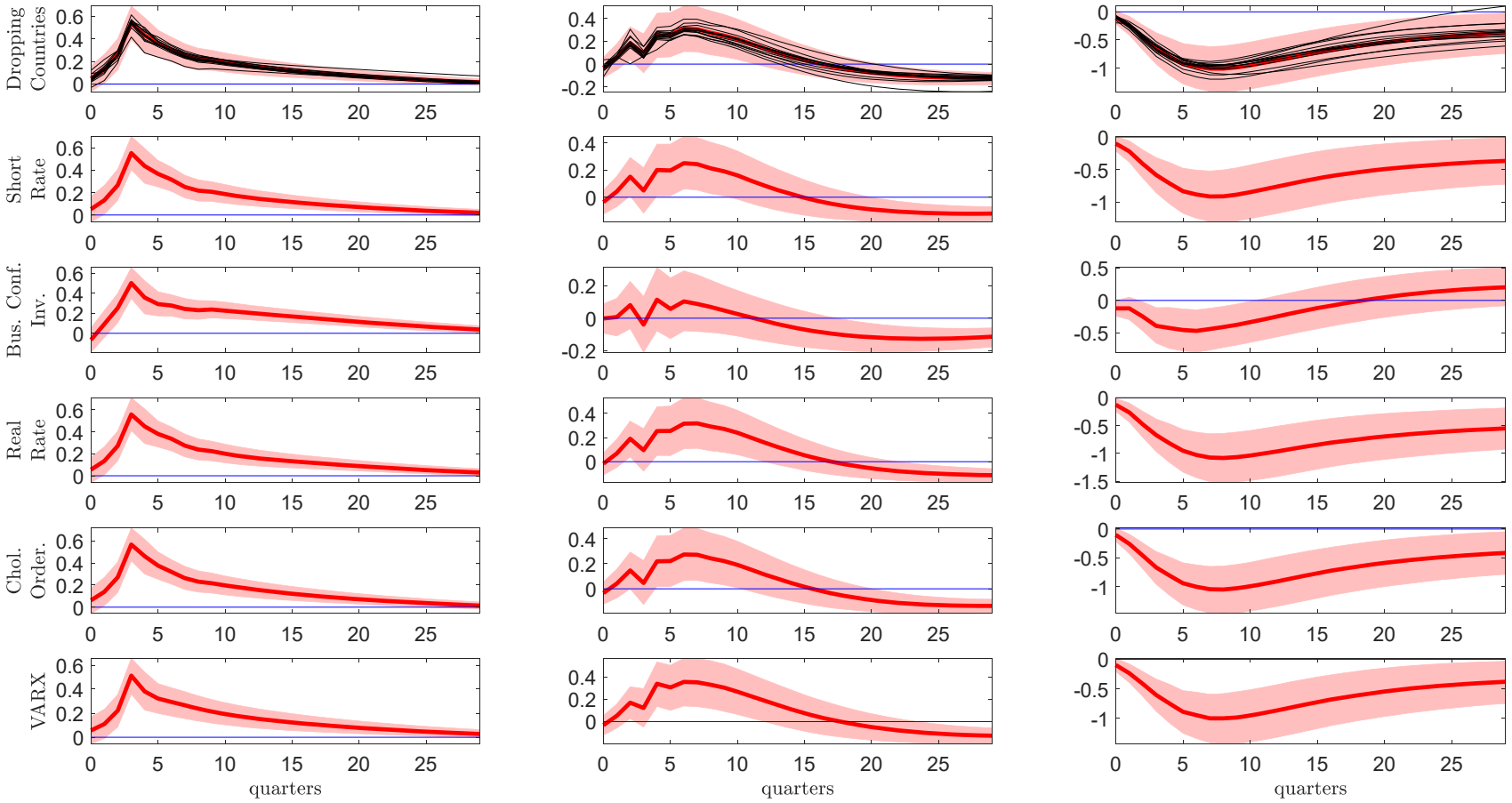
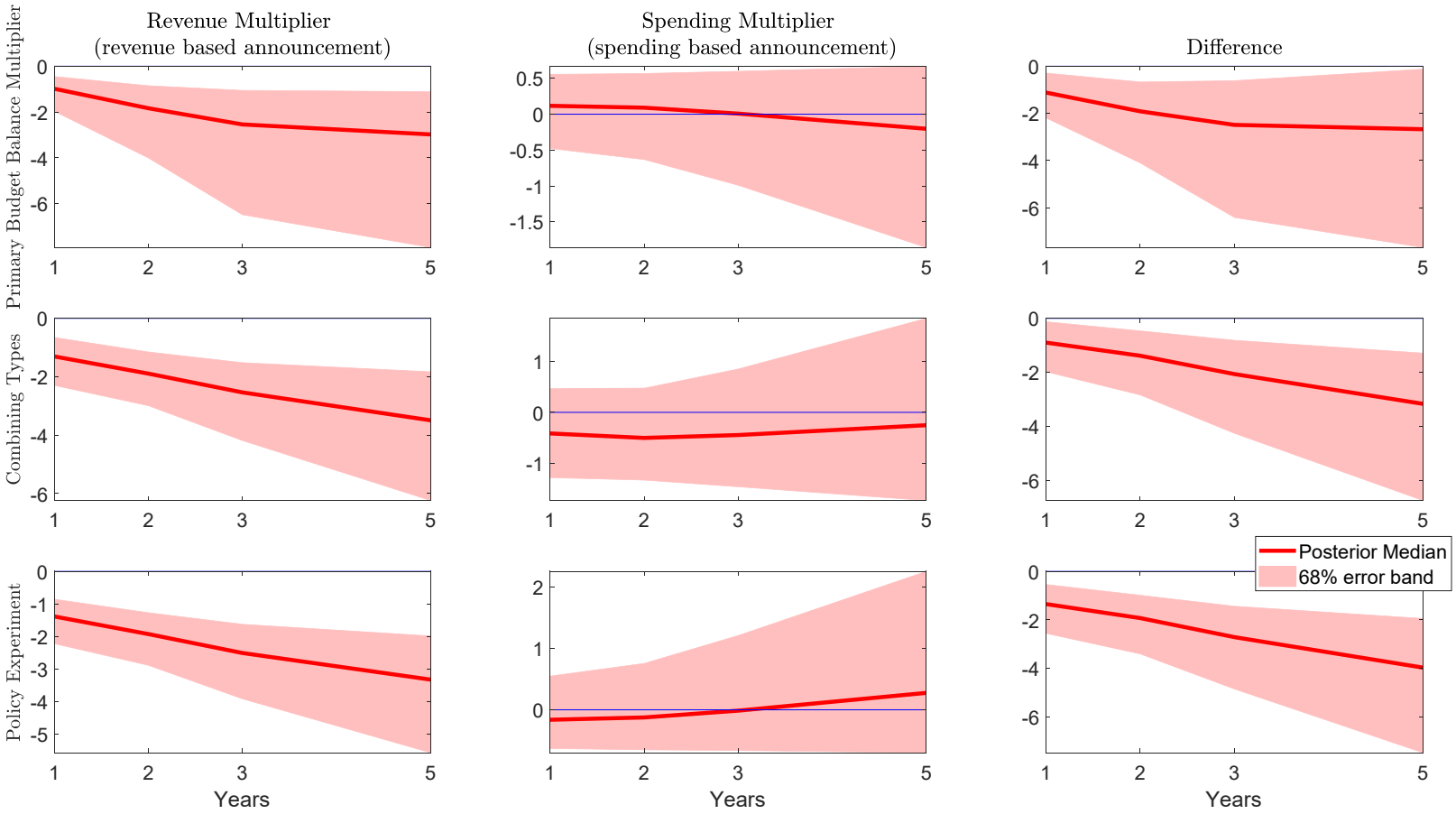


Figure 3: Robustness analysis: differences revenue- and spending-based announcements (continued)



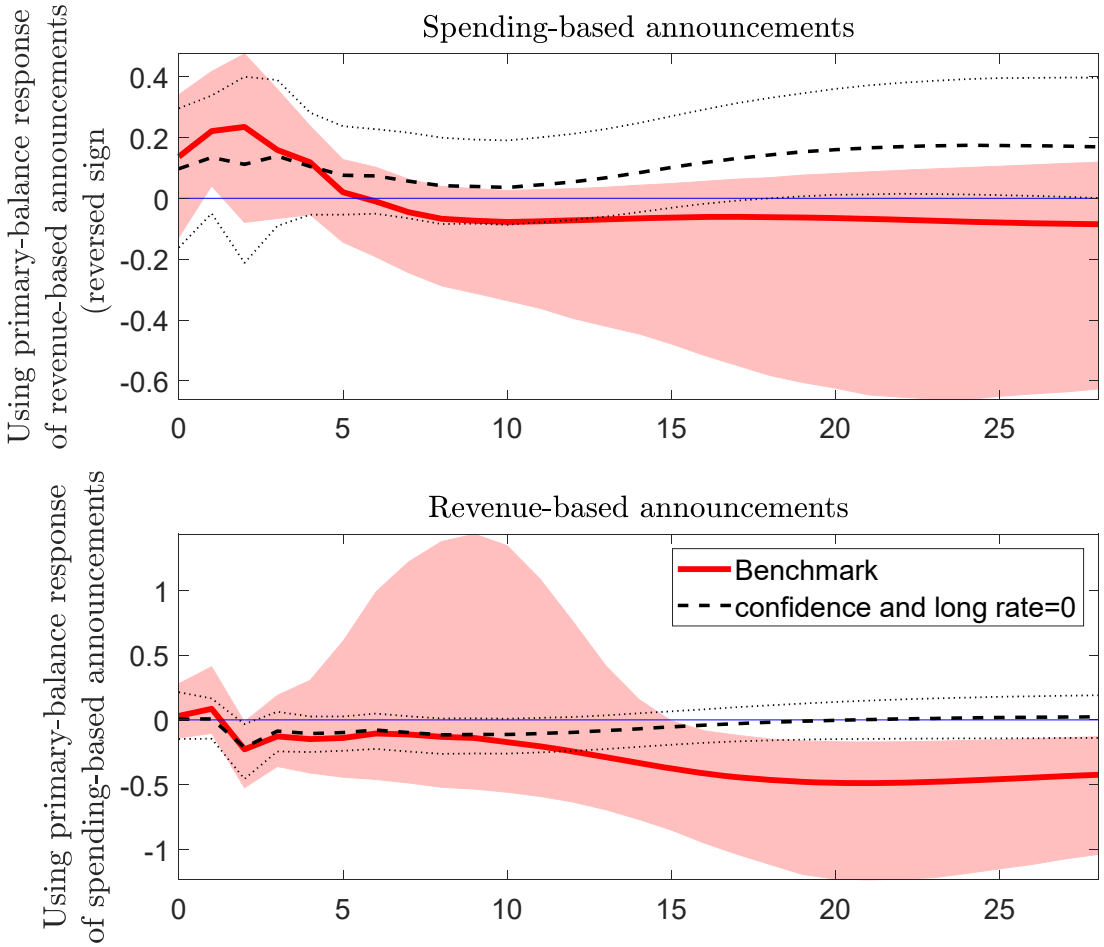
Notes: See *Notes* to Figure 1 and description in the main text. The figure shows differences between revenue- and spending-based announcements for actual revenue, actual spending and GDP. GDP response is in percent deviation from baseline value.

Figure 4: Cumulative multipliers



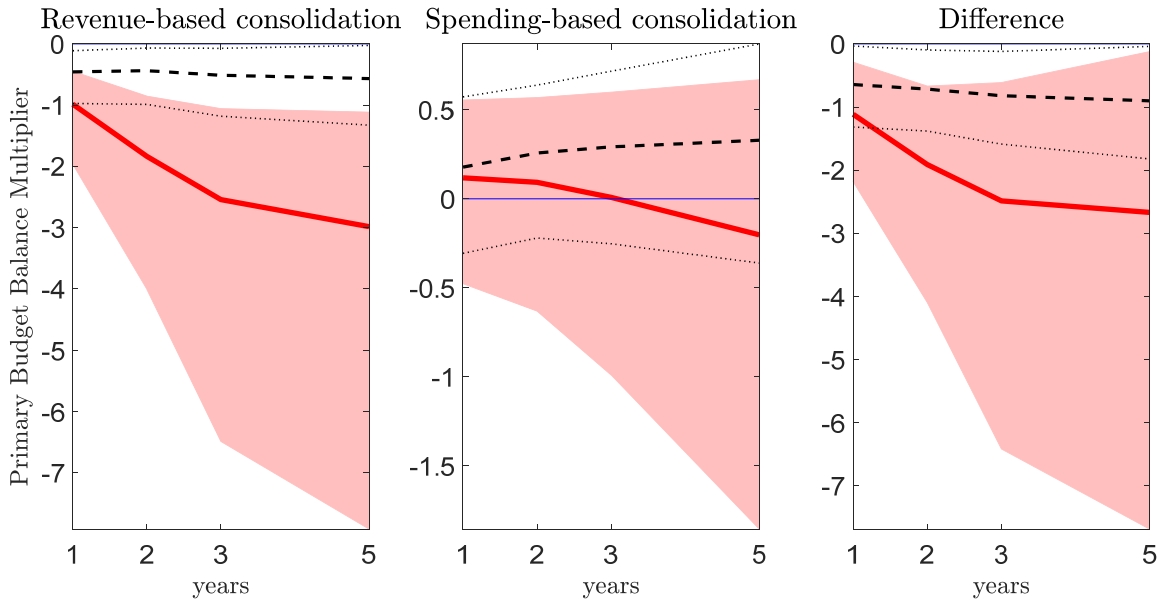
Notes: See *Notes* to Figure 1. Further, the top row shows the cumulative primary-balance multipliers of output under revenue-based, respectively spending-based program announcements calculated according to equation (4). The second row shows the cumulative multipliers of output divided by the cumulative change in revenues, respectively spending, under revenue-based, respectively spending-based announcements. The third row depicts the cumulative primary-balance multipliers of output for revenue-based (spending-based) announcements, while holding the actual spending (revenue) response fixed at zero for K=8 quarters. The third column shows the differences in the cumulative multipliers subtracting the second from the first column.

Figure 5: Differences (pseudo) actual and counterfactual



Notes: See *Notes* to Figure 1 and description in the main text. The vertical axis refers to percent deviations from original GDP. The top panel refers to spending-based announcements where the counterfactual is based on the actual primary-balance response of revenue-based announcements, while the bottom panel refers to revenue-based announcements where the counterfactual is based on the actual primary-balance response of spending-based announcements

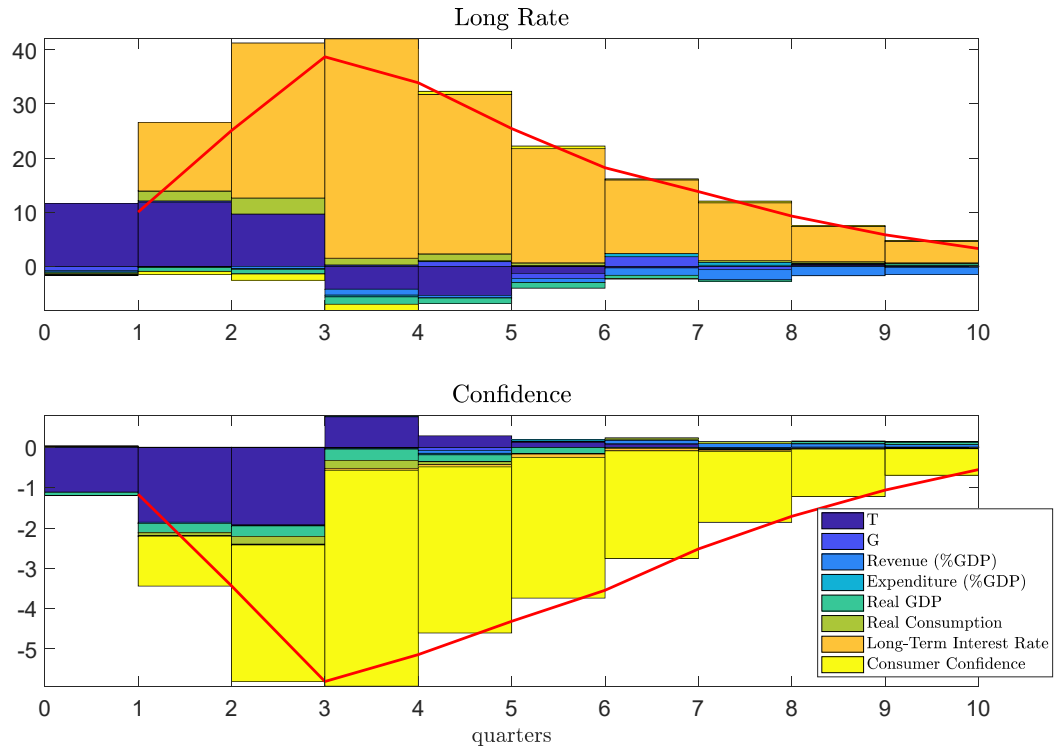
Figure 6: Comparison of multipliers with and without confidence channel



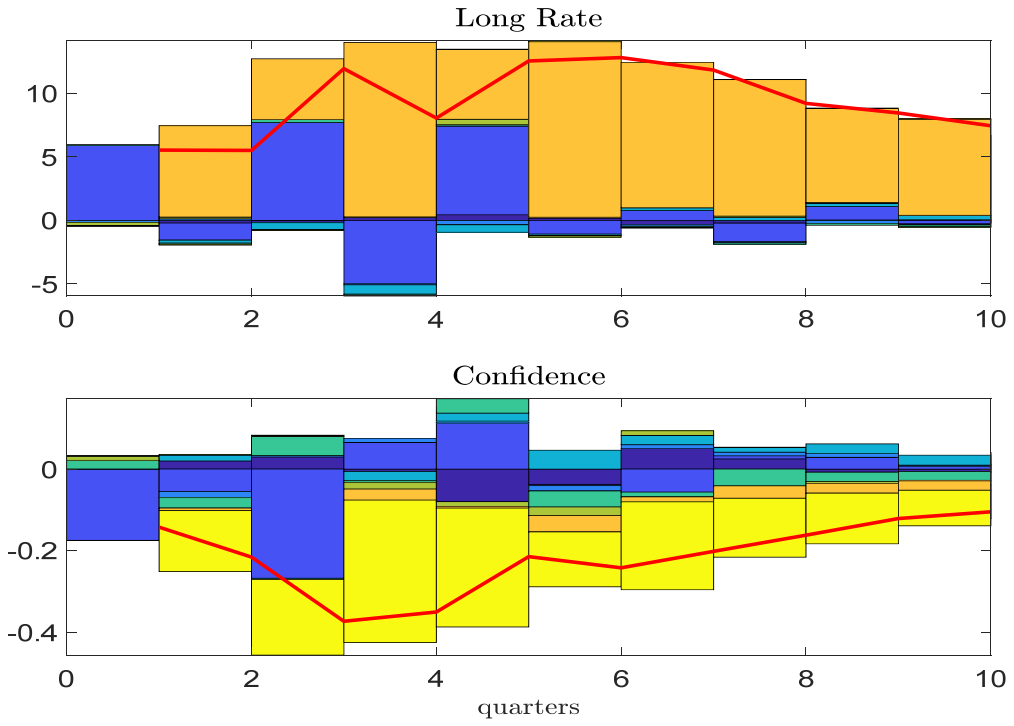
Notes: the solid red line refers to the original multipliers, so with the confidence channel active. The dashed black line is obtained by shutting off the confidence channel, i.e. holding the long-term interest rate and consumer confidence at zero for 8 quarters. The right-hand panel depicts the difference between the multipliers under revenue-based and spending-based consolidation announcements.

Figure 7: Decomposition of sources driving private sector confidence

Panel A – Responses to revenue-based announcement shocks



Panel B – Responses to spending-based announcement shocks



Notes: Legend in Panel A. Long-term interest rate is measured in basis points, consumer confidence in percent.

Appendices

Appendix A: Data sources and construction

In this paper we construct two main datasets. The first dataset is at the *annual* frequency and is used in Section 3 to calculate the e*x-post* deviations from the annual fiscal consolidation plans identified by Devries *et al.* (2011) for the period 1978-2008 and by Alesina *et al.* (2015a,b) for Austria, Belgium, Denmark, France, Germany, Ireland, Italy, Portugal, Spain and the United Kingdom and by ourselves for Finland, Sweden and the Netherlands for the period 2009-2013. The second dataset is at the *quarterly* frequency and constructed for the quarterly panel VAR analysis in Section 5. It contains the announcements of fiscal consolidation plans. Hence, both datasets cover thirteen EU countries, with the sample period spanning from 1978-2013.

A.1. Budgetary *annual* data for *ex-post* deviation analysis

The budgetary data are taken from the November 2015 edition of the OECD Economic Outlook. In order to make an *ex-post* comparison we need to match as well as possible the concepts of revenues and spending used in the narrative dataset identified by Devries *et al.* (2011) and Alesina *et al.* (2015a,b) with OECD data on actual revenues and spending. Because there is no obvious one-to-one correspondence between the two datasets, we use alternative *ex-post* budgetary measures (in parenthesis the original OECD Economic Outlook code).

For public revenues we construct the following four *ex-post* aggregates:

1. *Total receipts, excluding gross interest receipts* = Total direct taxes (TY) + Indirect taxes (TIND) + Social security contributions received by government (SSRG) + Other current receipts by government (TOCR) + Property income received by government (YPERG) + Capital tax and transfers receipts (TKTRG) - Gross government interest receipts (GGINTR).
2. *Current receipts, excluding gross interest receipts* = Total direct taxes (TY) + Indirect taxes (TIND) + Social security contributions received by government (SSRG) + Other current receipts by government (TOCR) + Property income received by government (YPERG) – Gross government interest receipts (GGINTR).
3. *Cyclically adjusted current receipts, excluding gross interest receipts*.
4. *Total revenues, narrow definition* = Total direct taxes (TY) + Indirect taxes (TIND) + Social security contributions received by government (SSRG).

For public spending we construct the following four *ex-post* aggregates:

1. *Total disbursements, excluding gross interest payments* (YPGTX) = Government final consumption expenditure, appropriation account (CGAA) + Property income paid by the government (YPEPG) + Social security benefits paid by the government (SSPG) + Other current outlays, government (YPOTG) + Government fixed capital formation, appropriation account (IGAA) + Capital transfers paid and other capital payments (TKPG) – Government consumption of fixed capital (CFKG) – Gross government interest payments (GGINTP).
2. *Current disbursements, excluding gross interest payments* = Government final consumption expenditure, appropriation account (CGAA) + Property income paid by the government (YPEPG) + Social security benefits paid by the government (SSPG) + Other current outlays, government (YPOTG) – Gross government interest payments (GGINTP).
3. *Cyclically adjusted current disbursements, excluding gross interest payments*.
4. *Total expenditure, narrow definition =* Government final consumption expenditure, appropriation account (CGAA) + Government fixed capital formation, appropriation account (IGAA) + Social security benefits paid by the government (SSPG).

In our analysis we express all budgetary aggregates in percent of gross domestic product, with the exception of the cyclically-adjusted measures which the OECD Economic Outlook publishes directly in percent of potential gross domestic product.

A.2. Budgetary, macroeconomic and confidence *quarterly* data for panel VAR analysis

Budgetary variables: We obtain budgetary variables at the quarterly frequency from Eurostat. All the data have been compiled under the European System of Accounts, 2010 edition (ESA2010). To construct the appropriate quarterly data series, we make sure that the series extracted from Eurostat and the OECD Economic Outlook 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 the annual frequency between the two sources. The correspondences between the codes from the two data sources are given in Table A.1:[25]

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

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

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 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 into 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 for each country over 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 data by scaling the interpolated observations with the ratio of the average (annualized) quarterly Eurostat observations over the annual OECD observation in 2002. We choose 2002 because in the case of Germany and Ireland the quarterly data is only available starting in 2002. Based on the above quarterly budgetary series we construct the following two variables, which correspond to the “narrow definition” of budgetary measures constructed in A.1 above:

Government revenues: Total direct taxes + Indirect taxes + Social security contributions received by government.

Government expenditure: Government final consumption expenditure, appropriation account + Government fixed capital formation, appropriation account (IGAA) + Social security benefits paid by the government (SSPG).

We use these two measures both in percentage of GDP (baseline specification) and potential GDP (robustness section).

Macroeconomic variables: Most of our quarterly macroeconomic variables are extracted from the OECD Economic Outlook. We retrieve the data (through Datastream) on private investment from the IMF International Financial Statistics database.[26] 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 the 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:

Nominal GDP = Gross Domestic Product (market prices), value, annual and quarterly. All series are from the November 2015 edition of the OECD Economic Outlook with some exceptions (due to data availability). Namely we use the November 2014 edition of the OECD Economic Outlook (Ireland after 2013), the May 2014 edition of the OECD Economic Outlook (Spain) and the December 2010 edition of the OECD Economic Outlook (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.

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

[26] 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).

Economic Outlook 96 and 88 series. 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. Because of this, we change the base year for the observations used from the Economic Outlook 96. To do so, for both series we calculate the year average of the quarterly values in the Economic Outlook 88 base year and construct a scaling factor calculated as the resulting number for Economic Outlook 88 divided by the number for Economic Outlook 96. Then all observations appended from Economic Outlook 96 are multiplied by this scaling factor. In an analogous way we adjust all observations for Germany from Economic Outlook 88, where the base year is 2000, to the base year 2010 used in Economic Outlook 96. Before taking this step, we reconstruct the German series from Economic Outlook 88, which is based on data from West-Germany in the first part of the sample and on data from unified Germany in the latter part of the sample. We take the data for unified Germany as of 1991 and, because for that year we also have data on West-Germany, we take the ratio of the unified German and West-German figures for 1991 and apply this scaling factor to all West-German data before 1991.

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).[27] 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);

[27] 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.

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, Spain), we opt for using the seasonally-adjusted series. Those series that are only available as non-seasonally adjusted, we seasonally adjust 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.[28]

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

[28] 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 effectively transform their base year to 2010).

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.

Appendix B. Construction details of the consolidation announcements dataset

This appendix is taken over with slight modification from Beetsma *et al.* (2015). Regarding *what* is considered the announcement of a new consolidation, we have taken the following specific decisions:

- If a newly-elected 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. Similarly, if a newly-elected government communicates a change to an existing plan, we consider this an announcement. However, in most of these cases no figure is available for the future budgetary implications and, hence, these announcements cannot included in the estimation.
- 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).

Appendix C: sources of imperfect follow-up

This appendix offers two (potentially complementary) explanations for the weaker follow-up of spending than of revenue plans. The first is what we refer to as "passive" non-follow-up and results from over-optimistic output growth forecasts. For standard estimates of elasticities and over-optimism in growth forecasts, this can explain one-third to almost one-half of the average difference in follow-up. The second is what we refer to as "active" non-follow-up. It is the result of only partially implemented planned consolidation measures. A potential explanation is that political resistance to consolidation plans is uncertain, but is more likely to be prohibitive for spending than for revenue plans when it comes to actual implementation. Data on general strikes in Western Europe do indeed suggest that announcements of spending cuts are more frequently followed by socio-political unrest than announcements of revenue increases.

C.1. "Passive" non-follow-up

A first explanation is based on over-optimistic GDP growth forecasts at the time when consolidation measures are devised. For lack of a better name, because governments may be deliberately over-optimistic, we refer to this phenomenon as "passive" non-follow-up. Using a back-of-the-envelope calculation, we show that systematically over-optimistic GDP growth forecasts account for a substantial fraction of the observed difference in follow-up between revenue- and spending-based consolidation plans. The starting point is equation (1). Because we merely want to provide an order-of-magnitude of the role of over-optimism in GDP forecasts in this regard, we keep our set-up as simple as possible, and focus on the case of one-year ahead consolidation plans (hence, $h = t - 1$), while assuming that for a generic variable Z, $Z_{t-1}^{t-1} = Z_{t-1}^{f}$, which implies that nowcast estimates (i.e. estimates for the current year) are equal to *ex-post* measures. Because forecasting inaccuracy increases with the horizon over which the forecast is made, the back-of-the-envelope numbers for the shortfalls that we calculate likely form a lower bound. In particular, there is evidence that budgetary forecasts in the EU are systematically more over-optimistic the further they reach into the future (e.g., Beetsma *et al.*, 2009). Under these assumptions, the difference between the *ex-post* and planned change (1) reduces to $\left(X_t^f/Y_t^f\right) - (X_t^{t-1}/Y_t^{t-1})$, for $X = T, G$. In Beetsma *et al.* (2013) we show that this expression can be decomposed into a "growth effect" and a "denominator effect" according to the following formulation:[29]

[29] There is also a so-called "base effect", which is zero, however, under our assumptions that nowcast estimates (i.e. estimates for the current year) are equal to *ex-post* measures, and there is a residual effect that we ignore, because it is of second-order importance – see Beetsma *et al.* (2013) for a discussion.

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

Here x_t^{t-1} is the planned growth in period $t-1$ of nominal revenues ($X=T$) or nominal expenditure ($X=G$) 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 and y_t^f is the period-t *ex-post* nominal income growth rate. 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 optimism bias in nominal output growth projections for EU countries is around 0.5%. Using the information in Table A.3 of Mourre *et al.* (2014), we are able to compute the average revenue and expenditure elasticities with respect to output of the thirteen EU countries in our sample. The resulting elasticities are $\varepsilon_T=1.11$ and $\varepsilon_G=-0.16$. Finally, based on the *ex-post* measures available in the OECD Economic Outlook (November 2015), we know that the ratios of total nominal revenues (narrow definition) and total nominal expenditure (narrow definition) over nominal GDP are $T_t^f/Y_t^f=0.39$ and $G_t^f/Y_t^f=0.39$, respectively. On the basis of this calibration, assuming an average *ex-post* nominal GDP growth y_t^f of 4.5% and (for consistency) an average nominal GDP growth forecast y_t^{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 as a result of the over-optimistic nominal GDP growth forecast. 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 systematic biases in the GDP growth forecasts lead to systematically larger shortfalls from plans for spending reductions than for revenue increases, which can explain a non-negligible fraction of the empirically-observed average difference between the shortfalls.

C.2. "Active" non-follow-up

Our second explanation concerns the "active" non-follow-up, which refers to the possibility that announced consolidation measures are only partially carried out. It is conceivable that at the

moment a consolidation plan is constructed, it is unclear how large the political costs associated with the plan are. As actual implementation approaches, it may be that spending reductions are more likely to be politically prohibitive and, hence, take place with a lower frequency than revenue increases – the latter tend to be less concentrated among specific groups than the former and, hence, feature a less favorable trade-off between the expected benefits and costs of any group in particular to engage in efforts to block the planned consolidation measures.

Here, we provide some indirect data support for the hypothesis that actually implementing spending cuts may be politically more prohibitive than actually increasing revenues. Hamann *et al.* (2013, 2016) document on 159 episodes of general strikes in fifteen countries in the European Union plus Norway over the period 1980-2006. The data contains information about the country where the strike occurs, the exact date of the strike, the main governing party, the issue in dispute and the outcome of the strike in terms of concessions. Examples of issues in dispute are "Labour law reform", "Austerity", "Pensions", "Economic policy" and "Public spending". In a number of instances the description of the issue in dispute makes it quite clear whether the strike is associated with public spending cuts or tax increases. [30] 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 that lead to strikes 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.

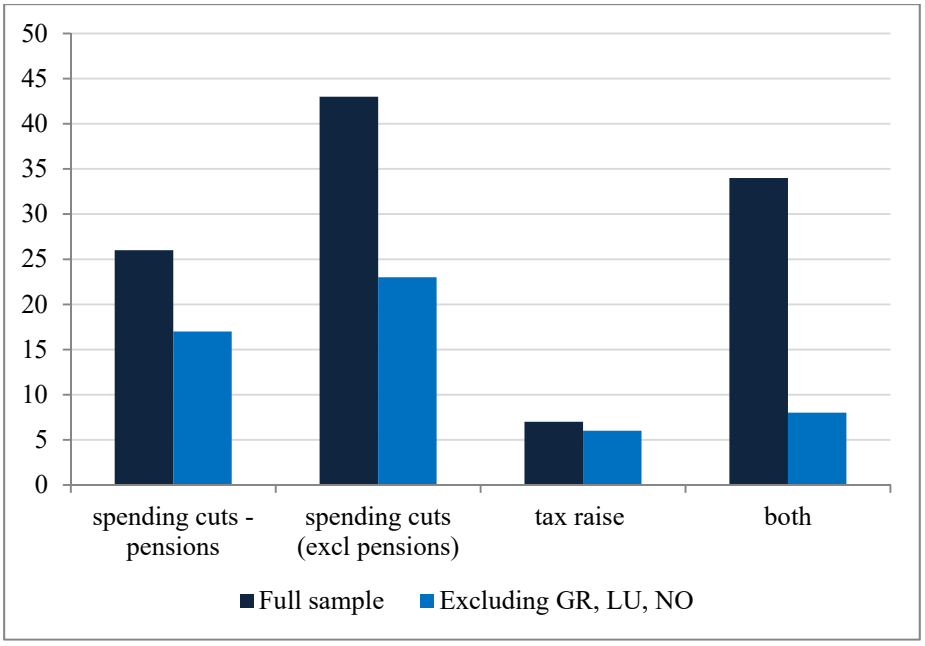
The dataset on strikes considers a slightly larger set of countries than our consolidation announcement data and it also includes an outlier in terms of the number of general strikes: out of the total of 159 episodes, 69 are registered in Greece. We start by analyzing the dataset in full and then restrict our attention to the country sample matching our 13 European OECD countries.

[30] 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.

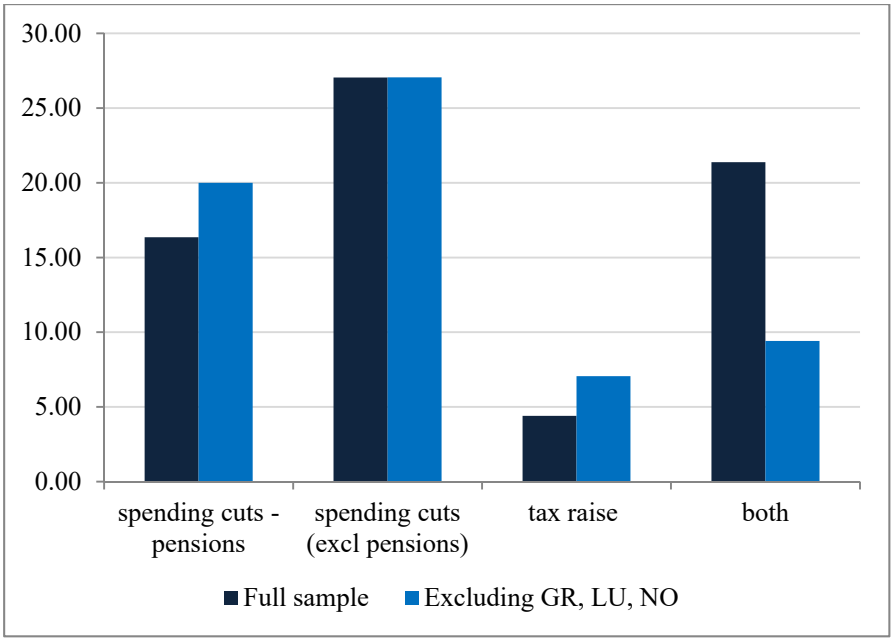
Out of the 159 episodes of general strikes, we find that 69 are spending-cut motivated, 43 after excluding those where the issue in dispute is pensions, while the number of revenue-raise motivated strikes is only 7. Hence, disputes motivated by spending-cuts occur with a substantially higher frequency than disputes motivated by revenue increases. We also observe 34 strikes against austerity in general, hence aimed at adjustments in both revenues 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.1(a) and C.1(b).

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

(a) Absolute numbers



(b) Percentages



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

Table C.1: Links between strikes and announcements in our dataset

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 revenue 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 we classify "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" 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".

Appendix D: Bayesian estimation of the panel VAR model

The panel VAR model is defined in equation (3).

Priors

The prior is implemented using T_d dummy observations Y_D and X_D where $Y_D = \begin{pmatrix} Y_{D1} \\ Y_{D2} \end{pmatrix}$ and $X_D = \begin{pmatrix} X_{D1} \\ X_{D2} \end{pmatrix}$. The prior on the VAR autoregressive coefficients and error covariance is implemented via the dummy observations

$$Y_{D1} = \begin{pmatrix} \frac{diag(\gamma\sigma_1, \dots, \gamma\sigma_N)}{\tau} \\ 0_{N(L-1)\times N} \\ \dots \\ diag(\sigma_1, \dots, \sigma_N) \\ \dots \\ 0_{1\times N} \end{pmatrix}$$

$$X_{D1} = \begin{pmatrix} \frac{diag(1,..,L) \otimes diag(\sigma_1, \dots, \sigma_N)}{\tau} & 0_{NL\times 1} \\ 0_{N\times NL} & 0_{N\times 1} \\ \dots & \dots \\ 0_{1\times NL} & c \times I_{EX} \end{pmatrix}$$

where γ denotes the mean of the prior, σ_i are scaling factors set using preliminary AR(1) regressions, τ is the tightness of prior for autoregressive coefficients, while c controls the prior for the exogenous regressors (i.e. fixed effects and coefficients on seasonal dummies, with the total number of coefficients on such regressors given by EX). In our application $\tau = 1$ and c=1/10000, implying a fairly loose prior. Following Bańbura *et al.* (2010), we also incorporate a prior belief for the sum of coefficients on the lagged dependent variables. This is implemented via the dummy observations:

$$Y_{D2} = \frac{diag(\gamma\mu_1, \dots, \gamma\mu_N)}{\lambda}$$

$$X_{D2} = \begin{pmatrix} \frac{(1_{1\times L}) \otimes diag(\gamma\mu_1, \dots, \gamma\mu_N)}{\lambda} & 0_{N\times 1} \end{pmatrix}$$

Where μ_i denotes the sample mean for the ith endogenous variable. As in Bańbura *et al.* (2010), the tightness parameters is set as $\lambda = 10\tau$.

Gibbs Algorithm

The posterior distribution is approximated via a standard Gibbs algorithm that samples from the following conditional posterior distributions (denote $B = vec([\alpha_i, \gamma, \beta_i, A_l])$ and let X denote all the regressors on the right-hand side of the VAR equations).

(1) $G(B|\Omega)$ This conditional posterior is a normal distribution:

$$\mathrm{N}(B^*, \Omega \otimes (X^{*\prime}X^*)^{-1})$$

where $B^* = (X^{*\prime}X^*)^{-1}(X^{*\prime}Z^*)$. Note that $Z^* = \begin{pmatrix} Z \\ Y_D \end{pmatrix}, X^* = \begin{pmatrix} X \\ X_D \end{pmatrix}$ represents the actual data stacked on top of the dummy observations.

(2) $G(\Omega|\mathrm{B})$ This conditional posterior is inverse Wishart:

$$IW(S^*, NT + T_D - K)$$

where $S^* = (Z^* - X^*B)'(Z^* - X^*B)$ and K denotes the number of regressors in each VAR equation. The algorithm uses 25000 iterations with a burn-in of 15000 iterations. The figure below shows that the inefficiency factors are low. This provides evidence in favour of convergence of the algorithm.