Fiscal austerity and risk sharing in advanced economies

Furtună, O.

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This thesis tackles the nuanced effects of fiscal policy in developed economies. It constructs a novel dataset of fiscal austerity announcements and uses it in empirical applications. First, it measures the effects of fiscal austerity on market expectations. Then, it studies the contrast between the announcements of fiscal policy measures and their implementation, considering the role of expectations in the transmission of policy shocks to the real sector. Finally, it tackles an additional question, namely the contribution of financial integration versus international financial assistance as channels for consumption risk sharing in the European Monetary Union (EMU).

Oana Furtună (1988) obtained an MSc in Economics from the University of Amsterdam, and then acquired experience as a research assistant in the policy environment. She holds an MPhil in Economics from the Tinbergen Institute. In 2014, upon graduation, she joined the University of Amsterdam as a PhD student, working under the supervision of Prof. Dr. Massimo Giuliodori and Prof. Dr. Roel Beetsma.
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Promotor: Prof. dr. M. Giuliodori Universiteit van Amsterdam
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Overige leden: Prof. dr. E.J. Bartelsman Vrije Universiteit Amsterdam
Prof. dr. C. van Ewijk Universiteit van Amsterdam
Prof. dr. F.J.G.M. Klaassen Universiteit van Amsterdam
Prof. dr. S.J.G. van Wijnbergen Universiteit van Amsterdam
Dr. J. Cimadomo European Central Bank
Dr. K. Mavromatis De Nederlandsche Bank

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Contents

Acknowledgements ................................................................. iv
List of Tables ................................................................. xiii
List of Figures ................................................................. xv

1 Introduction ................................................................. 1

2 A New Dataset of Fiscal Austerity Announcements .................. 7
   2.1 Introduction ............................................................ 7
   2.2 “Narrative” Data on Fiscal Consolidations ......................... 8
      2.2.1 The role of narrative data for shock identification ....... 8
      2.2.2 Narrative annual data on fiscal consolidations ........... 9
   2.3 Monthly Data on Fiscal Austerity Announcements ............... 11
      2.3.1 From implementation data to fiscal announcements ...... 11
      2.3.2 The magnitude of fiscal announcements ................. 12
      2.3.3 Sample differences ........................................... 13
   2.4 Broad Principles for Announcement Identification ............... 15

3 The Confidence Effects of Fiscal Consolidations ................. 19
   3.1 Introduction ............................................................ 19
   3.2 Literature Review .................................................... 23
      3.2.1 The effect of fiscal shocks on confidence ............... 23
      3.2.2 How confidence affects the real economy ............... 25
3.3 The Data ................................................................. 26
  3.3.1 Construction of annual consolidation variables ...................... 27
  3.3.2 The confidence indicators and the control variables ............... 28
  3.3.3 The monthly fiscal announcement variables ......................... 29
3.4 Annual Panel Regressions .............................................. 30
3.5 Event Study with Monthly Data ........................................ 36
  3.5.1 Consumer and business confidence ................................ 37
  3.5.2 Broadening the concept of confidence .............................. 44
3.6 Concluding Remarks ................................................... 46
Appendix 3.A Fiscal consolidation in the euro zone during the crisis ....... 61
Appendix 3.B Additional results for the baseline regression ................ 65
Appendix 3.C Coefficient equality tests .................................. 66
Appendix 3.D Can confidence predict consolidations? ...................... 67
Appendix 3.E Results of panel vector auto-regressions (PVAR) .......... 70
Appendix 3.F Business cycle confidence: split into expenditures and
  revenues ........................................................................ 73
Appendix 3.G Timing experiments ............................................ 75
  3.G.1 Mistiming ................................................................ 75
  3.G.2 Excluding the month with the most observations for each country.. 76
Appendix 3.H Results panel distributed-lag model .......................... 77
Appendix 3.I Plots of annual implemented consolidations ................. 78

4 Revenue-versus Spending-Based Consolidation Plans: The Role of Follow-Up 81
  4.1 Introduction ................................................................ 81
  4.2 Literature Review ........................................................ 85
  4.3 Ex-Post Deviations of Real-Time Fiscal Consolidation Measures .... 88
    4.3.1 Matching of ex-post data with the narrative consolidation data .... 88
    4.3.2 A simple accounting framework ................................ 89

4.3.3 Results of the comparison ................................................. 90
4.3.4 Explanations for differences in follow-up ............................. 91
4.4 Data on Fiscal Consolidation Announcements ........................... 95
4.5 The Panel Vector Autoregression (VAR) Analysis ......................... 97
  4.5.1 Follow-up versus differences in composition of consolidation plans ... 105
  4.5.2 The role of the confidence channel .................................. 108
4.6 Conclusion ........................................................................... 109
Appendix 4.A Data ................................................................. 119
  4.A.1 Budgetary variables ......................................................... 119
  4.A.2 Macroeconomic variables ................................................ 122
  4.A.3 Confidence .................................................................... 125
Appendix 4.B A framework and some indirect evidence for “active” non-follow-up ... 127
  4.B.1 Examples of assignment of issues in dispute in the strikes .......... 134
Appendix 4.C Robustness checks ................................................ 136

5 Private and Public Risk Sharing in the Euro Area ........................... 159
  5.1 Introduction .......................................................................... 159
  5.2 Literature Review ............................................................... 163
  5.3 Methodology and Data ......................................................... 166
    5.3.1 Baseline empirical setup ................................................. 166
  5.4 Introducing Financial and Fiscal Integration ............................ 169
    5.4.1 Construction of integration indices ................................. 169
    5.4.2 Regression based on extended model ............................... 171
  5.5 The Data ............................................................................. 172
  5.6 Empirical Results ............................................................... 174
    5.6.1 Simple risk sharing regression ....................................... 174
    5.6.2 The effects of financial and fiscal integration on risk sharing ... 176
    5.6.3 Risk sharing links between “Core” and “Periphery” ................ 179
List of Tables

2.1 Country and time coverage for narrative datasets on fiscal consolidations 18

3.1 Summary statistics of confidence variables 49
3.2 Baseline regressions using end-of-period CCI 50
3.3 Baseline regressions for subcomponents using end-of-period CCI 51
3.4 Baseline regressions using end-of-period BCI 52
3.5 Average deviation consumer confidence from level at announcement 53
3.6 Average deviation business confidence from level at announcement 54
3.7 Average deviation long-term interest rate from level at announcement 55
3.8 Average deviation stock price index from level at announcement 56
3.B.1 Additional variations and extensions on baseline in Table 3.2 65
3.C.1 Testing coefficient equality for consolidation measures 66
3.C.2 Testing coefficient equality for of expenditure- and revenue-based consolidations 66
3.D.1 Annual panel probit regressions 68
3.D.2 Monthly panel probit regressions 69
3.F.1 Baseline regressions for subcomponents using end-of-period BCI 73
3.F.2 Effect subcomponents of consolidations on business confidence 74
3.H.1 Results of the panel distributed lag model using values for announcements 77

4.1 Average of ex-post deviations for revenues 110
4.2 Average of ex-post deviations for spending 111
4.3 Cumulative multipliers at various horizons  .......... 112
4.4 Summary statistics of fiscal announcements  .......... 113
4.5 Plans according to their predominant instrument  .......... 113
4.A.1 Correspondence between OECD and Eurostat series  .......... 120
4.B.1 Links between strikes and announcements  .......... 133

5.1 Simple risk-sharing regression model  .......... 183
5.2 Risk sharing in the euro area: the role of financial integration and EFSF-ESM assistance  .......... 184
5.3 Risk sharing between “Core” and “Periphery”  .......... 185
5.C.1 Results with different estimation methods and including levels of interacted variables  .......... 196
List of Figures

3.1 Monthly frequency of fiscal announcements ................. 57
3.2 Histograms of confidence indices, pooled across countries .......... 58
3.3 Monthly distribution of changes in confidence ................. 58
3.4 Fiscal announcement effect on consumer confidence, full sample of consolidations 59
3.5 Spending and revenue-based consolidations ................. 59
3.A.1 Cumulated discretionary measures over the period 2009-2013 in the euro area and changes in the structural primary and actual balances .......... 64
3.A.2 Contribution of structural revenues and structural primary expenditure to the change in the structural primary balance over the period 2009-2013 .......... 64
3.E.1 Responses to a $CS^A$ shock ...................................... 70
3.E.2 Responses to a $CS^U$ shock ...................................... 71
3.E.3 Responses to a $CS^P$ shock ...................................... 72
4.1 Impulse responses baseline model – all announcements .......... 114
4.2 Impulse responses baseline model – revenue-based plans .......... 115
4.3 Impulse responses baseline model – spending-based plans .......... 116
4.4 Counterfactually shutting off the confidence channel – revenue-based plans .......... 117
4.5 Counterfactually shutting off the confidence channel – spending-based plans .......... 118
4.B.1 Strikes in Western Europe by issue in dispute .................. 132
4.C.1 Baseline model excluding the period 2008-2013 - revenue-based plans .......... 136
4.C.2 Baseline model excluding the period 2008-2013 - spending-based plans .......... 137
4.C.3 Baseline model in shares of potential GDP – revenue-based plans . . . . . . . . 138
4.C.4 Baseline model in shares of potential GDP – spending-based plans . . . . . . . . 139
4.C.5 Baseline model including logs of revenues and spending – revenue-based plans . 140
4.C.6 Baseline model including logs of revenues and spending – spending-based plans . 141
4.C.7 Baseline model – revenue-based plans with at least 60% revenue measures . . . . 142
4.C.8 Baseline model – spending-based plans with at least 60% spending measures . . 143
4.C.9 Baseline model with time fixed effects – revenue-based plans . . . . . . . . . . . 144
4.C.10 Baseline model with time fixed effects – spending-based plans . . . . . . . . . . . 145
4.C.11 Baseline model with eight lags – revenue-based plans . . . . . . . . . . . . . . . 146
4.C.12 Baseline model with eight lags – spending-based plans . . . . . . . . . . . . . . . 147
4.C.13 Baseline model extended with lagged debt as exogenous variable – revenue-based
    plans . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . 148
4.C.14 Baseline model extended with lagged debt as exogenous variable – spending-based
    plans . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . 149
4.C.15 Baseline model excluding one country at a time – revenue-based plans . . . . . 150
4.C.16 Baseline model excluding one country at a time – spending-based plans . . . . . 151
4.C.17 Baseline model with short-term interest rate – revenue-based plans . . . . . . . 152
4.C.18 Baseline model with short-term interest rate – spending-based plans . . . . . . . 153
4.C.19 Baseline model with ex-post long-run interest rate – revenue-based plans . . . . 154
4.C.20 Baseline model with ex-post long-run interest rate – spending-based plans . . . . 155
4.C.21 Baseline model with business confidence and private investment - revenue-based
    plans . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . 156
4.C.22 Baseline model with business confidence and private investment - spending-based
    plans . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . 157
5.1 Financial integration and EFSF-ESM assistance in the euro area . . . . . . . . . 186
5.2 Degree of shock absorption in the EMU . . . . . . . . . . . . . . . . . . . . . . 187
5.3 Contributions to changes in the degree of shock absorption . . . . . . . . . . 188
5.4 Evolution of risk sharing between “Core” and “Periphery” . . . . . . . . . . . . . 189
5.C.1 Baseline estimation excluding one country at the time . . . . . . . . . . . . . . . 197
Chapter 1

Introduction

In the aftermath of the recent economic and financial crisis, concerns about the sustainability of public finances exposed the extent to which escalating public debt ratios can affect sovereign bond yields. Increasing upward pressure on government bond yields prompted debt-laden European sovereigns to implement aggressive austerity measures in an attempt to swiftly consolidate their public balances. Motivated by these recent economic events, this dissertation first focuses on the reaction of private sector expectations to unexpected policy shocks. It then delves deeper into the transmission of fiscal policy announcements to the real economy and revisits a result previously found in the literature: the asymmetric output effects of revenue- and spending-based consolidation plans. Both analyses make use of a new dataset of fiscal consolidation announcements introduced and described in Chapter 2 of this thesis. The last chapter of this thesis considers consumption risk-sharing in the euro zone, carried out through both the private channel of financial integration and the public channel of centralized financial assistance. This analysis contributes to the policy discussion about deepening European integration by addressing the scope for a supra-national fiscal mechanism to improve consumption risk-sharing the euro zone.

The economic circumstances when fiscal austerity was required triggered a debate regarding its effects on the real sector and financial markets. On the one hand, fiscal consolidation was hailed as the right response to financial market concerns about public debt sustainability, and was considered
the solution to curb the steep increase in sovereign bond yields. However, the desirability of fiscal austerity during recessions was questioned, especially if undertaken in a context with monetary policy at the lower bound (and unable to stimulate the economy). Economic debates went over the existence, direction and magnitude of the potential ‘confidence’ effects that could be brought about by fiscal consolidations. For instance, some proponents of fiscal consolidations invoked the potential positive effect of government spending cuts on business confidence (Alesina and Ardagna, 2010; Alesina et al., 2015a). Conversely, Born et al. (2015) brought evidence that spending cuts increase sovereign yield spreads during times of fiscal stress. The debate surrounding the impact of fiscal consolidation on market expectations motivates the analysis in Chapter 3, where we zoom into the response of confidence proxies to austerity announcements.

As European sovereigns started planning the introduction of fiscal austerity, policy discussions ensued regarding the structure of the consolidation packages in terms of their component measures (i.e. tax hikes or public expenditure cuts). Empirical studies of consolidation episodes across the OECD contributed to the emergence of a dichotomy between the output effects of tax and spending instruments (see, for instance, Guajardo et al., 2014; Alesina et al., 2015a; Alesina et al., 2015b; Alesina et al., 2017). The emerging view from this literature is that when compared to spending-based austerity packages, revenue-based consolidation measures are associated with larger negative effects on output. The main proposed explanations for this asymmetry are the role of monetary policy and the reaction of business confidence and expectations. Chapter 4 revisits the consumption and output effects of tax and spending changes and proposes a new explanation for their asymmetry. More specifically, we suggest that the heterogeneity in the real effects of revenue increases and of expenditure cuts is partly motivated by the extent to which the announcement is followed-up in the subsequent implementation.

In an interconnected world of open economies, asymmetric shocks can be smoothed across countries by means of private markets (namely through cross-border financial asset holdings). Additionally, in federations such as the United States of America, risk-sharing across states can
be enhanced through a federal transfer scheme. In 2010, the deteriorating state of public finances across European states illustrated the importance of risk sharing mechanisms in a monetary union with asymmetric shocks, limited national fiscal instruments and no supra-national transfer scheme. Financial assistance was directed to distressed European sovereigns first through bilateral loans and then via the European Financial Stability Facility (EFSF). Finally, the European Stability Mechanism (ESM) was created as a backstop for governments that could no longer access markets for bond financing. These recent examples of centralized financial assistance in response to asymmetric shocks in the euro area provide evidence on the extent of and possibilities for fiscal risk-sharing in a monetary union. The analysis of these interventions contributes to the academic and policy debate on the role of a supra-national fiscal capacity in the smoothing of country-specific shocks. In Chapter 5 we use cross-border private and public financial flows to investigate the scope for a fiscal union in the Eurozone, from the perspective of its gains in terms of consumption risk-sharing.

In a nutshell, this dissertation introduces a new dataset on fiscal consolidation announcements, and collects three contributions that address fiscal policy questions of relevance for the European economy. The work in this thesis consists of applied econometric research, as well as a contribution to the literature on empirical fiscal policy in the form of a new ‘narrative’ dataset of fiscal policy announcements. The remainder of this chapter will provide an overview of the thesis and its constituent chapters.

**A New Dataset of Fiscal Austerity Announcements**

In Chapter 2 we develop a new monthly dataset of fiscal consolidation announcements. Motivated by the methodological challenge of shock identification in fiscal vector autoregressive models (VARs), the data contribution tackles the problem of anticipation in VARs in a manner similar to Romer and Romer (2010) and Ramey (2011). Starting from annual fiscal austerity plans implemented in the OECD between 1978 and 2009, we create a new dataset of fiscal consolidation announcements,
subsequently used in the analyses of Chapters 3 and 4. We find the dates and magnitudes of announcements by combining information from a range of written documents. The newly created data described and used in this thesis achieves the identification of policy announcements at monthly frequency, and thus captures more accurately the process of public information revelation in the case of fiscal policy. Thus, by using the precise moment of announcement we can attenuate the bias induced by shock anticipation in fiscal VAR models.

The Confidence Effects of Fiscal Consolidations

In Chapter 3 we explore the effects of fiscal consolidations on private sector confidence, a possible channel for the transmission of fiscal policy that has received particular attention recently - as a result of governments embarking on austerity trajectories in the aftermath of the financial crisis. Panel regressions based on the annual action-based datasets of Pescatori et al. (2011) and Alesina et al. (2015a) show that consolidations, and in particular their unanticipated components, affect confidence negatively. To obtain a more accurate picture of how consolidations affect confidence, we use a monthly dataset of consolidation announcements (described in Chapter 2) to investigate confidence effects in real time using an event study. The results suggest that consumer confidence falls around announcements of consolidation measures, an effect driven largely by revenue-based measures. Moreover, these effects are highly relevant for European countries with weak institutional arrangements, as measured by the tightness of fiscal rules or budgetary transparency. The effects on producer confidence are generally similar, but weaker than for consumer confidence. Long-term interest rates, as a measure of confidence in the sovereign, tend to fall around spending-based consolidation announcements. We have no evidence that the confidence effects of consolidation announcements are more pronounced in slumps than in booms. Generally, strengthening institutional arrangements may help in mitigating the adverse confidence effects of consolidations.
Revenue-versus Spending-Based Consolidation Plans: The Role of Follow-Up

The following chapter studies the asymmetry between the output effects of fiscal consolidation plans that consist predominantly of tax hikes and those characterized mostly by spending cuts. The literature on fiscal multipliers finds that spending-based fiscal consolidations tend to have more benign macro-economic consequences than revenue-based consolidations. By directly comparing ex-post data with consolidation plans, we present evidence of a systematically weaker follow-up of spending-based consolidation plans. In other words, the deviation of the implementation from the announced plan is lower for tax than for expenditure-based austerity measures. Next, we use the newly-developed dataset of consolidation announcements introduced in Chapter 2 in quarterly panel VAR regressions. Our estimations confirm the weaker follow-up of spending-based plans and their more benign macro-economic effects compared to those of revenue-based plans. We distinguish two factors that can explain the asymmetry in effects: (i) the difference in follow-up and (ii) the difference in the composition of revenue- and spending-based consolidations, working through the difference in revenue and spending multipliers. While the latter channel explains the largest fraction of the difference in economic trajectories, the difference in follow-up plays a non-negligible role.

Private and Public Risk-Sharing in the Euro Area

Chapter 5 investigates the contribution of private and public channels for consumption risk sharing in the European Monetary Union (EMU) over the period 1999-2015. “Private” risk-sharing refers to the scope of international financial portfolios to smooth consumption by generating income flows unrelated to idiosyncratic shocks in domestic output. “Public” risk-sharing indicates the smoothing effect of public policies implemented at the supra-national level. In particular, we explore the role of financial integration versus public official financial assistance for private consumption smoothing in 11 EMU countries. In addition, we present a time-varying test which allows estimating how risk sharing has evolved since the start of the EMU, and in particular during the recent crisis. Our results suggest that, whereas in the early years of the EMU only about 40% of country-specific output shocks were smoothed, in the aftermath of the euro zone sovereign debt crisis about 65% of these
shocks were absorbed, therefore reducing consumption growth differentials across countries. This progressive improvement of the shock absorption capacity is due to higher financial integration, but also to the activation of the European Financial Stability Facility (EFSF) and the European Stability Mechanism (ESM) channelling official loans to distressed euro zone economies. We also find that cross-border holdings of equities and debt seem to be more effective than cross-border bank loans in isolating households from country-specific shocks, therefore contributing to consumption smoothing.
Chapter 2

A New Dataset of Fiscal Austerity Announcements

2.1 Introduction

This chapter is devoted to the description of a newly constructed dataset comprising announcements of fiscal austerity plans. The data represent the basis for the analysis undertaken in Chapters 3 and 4. We first provide an overview of previous work and discuss the need for a dataset on fiscal austerity announcements, thus motivating our data collection exercise. Because the creation of the dataset was a gradual process that proceeded over an extended period of time, Chapter 4 uses an enriched version of the data relative to Chapter 3. We provide a detailed characterization based on the most complete dataset used in Chapter 4, and highlight the deviations relative to the earlier version used in Chapter 3.

The dataset introduced in this chapter contributes to the literature along two dimensions. First, it achieves a precise timing of fiscal announcements at monthly frequency, ensuring an accurate characterization of official communication regarding planned policy measures. Second, the complete database includes a quantification of each fiscal announcement according to its planned impact.
on the budget balance, enabling the use of this numerical measure in empirical analyses of fiscal policy.

2.2 “Narrative" Data on Fiscal Consolidations

2.2.1 The role of narrative data for shock identification

The Vector Autoregressive (VAR) model is the methodology of choice to estimate the impact of fiscal shocks on real variables. By orthogonalizing the innovations of the equations included in the estimated system, the VAR framework allows for shocks in each variable to be examined in isolation. Two main methodological approaches are used for the orthogonalization of VAR residuals. In the structural VAR, shock independence is achieved through short-run (Blanchard and Perotti, 2002) or long-run (Mountford and Uhlig, 2009) identification restrictions motivated by economic theory. Another method for the orthogonalization of VAR model innovations is the “narrative" approach. Within the narrative approach, the residuals are orthogonalized by augmenting the estimated system with direct measures of shocks, consisting of information about policy changes (as done for example in Romer and Romer, 2010). As will be described in the following, the “narrative" method is of particular relevance for the estimation of fiscal VAR models.

Recently, the debate around the estimation of fiscal policy effects focused on the particular case of austerity measures. Traditionally, the literature empirically identified fiscal consolidations from data on the cyclically-adjusted primary balance (CAPB). The CAPB is defined as the primary balance minus the component that is automatically, i.e., for given policies, driven by the business cycle. The argument is that public spending (revenues) is (are) negatively (positively) related to GDP due to built-in stabilizers like unemployment benefits (the progressive tax system). A contraction causes the deficit to rise, because it leads to an automatic increase in public spending and an automatic reduction in tax revenues.¹ With the automatic effect of the business cycle taken out, swings in the

¹There are various ways to operationalize the concept of the CAPB. For instance, Blanchard (1990b) argues that to purge the data of automatic effects, one needs to estimate spending and revenues had the unemployment rate remained
CAPB must be the result of discretionary policy changes. Hence, fiscal consolidations are defined as periods in which the CAPB increases substantially. Although intuitive, the use of the CAPB to identify fiscal consolidations (and discretionary fiscal shocks in general) has been criticized for various reasons. First, changes in the CAPB typically include measurement errors that are likely to be correlated with other economic developments (Guajardo et al., 2014). Second, changes in the CAPB can be the result of discretionary reactions of fiscal policy to cyclical conditions. Third, as Wolswijk (2007) argues, the conventional way of calculating the CAPB wrongly assumes that the automatic response of tax revenues is constant over time, while tax elasticities may change over time, thus leading to inaccurate estimates of the CAPB at any moment.

2.2.2 Narrative annual data on fiscal consolidations

To avoid these shortcomings, Pescatori et al. (2011) are the first to identify periods of fiscal austerity by using the ‘action-based’ or ‘narrative’ approach. By contrasting their approach with the CAPB-based identification method, the authors show that the latter creates a bias towards finding that austerity is expansionary. In contrast, when ‘action-based’ identification is used, fiscal consolidations turn out to be associated with economic slumps.

The ‘narrative’ method of Pescatori et al. (2011) characterizes fiscal consolidations as episodes of austerity primarily motivated by the intention to reduce budget deficits and not by a response to (prospective) economic conditions, for example a desire to restrain domestic demand. In order to establish the motivation behind each period of fiscal consolidation they examine historical sources, such as European Union (EU) Convergence and Stability Programs, International Monetary Fund (IMF) Reports, OECD Economic Surveys and country-specific Budget Reports, Budget Speeches, or Central Bank Reports. By looking closely at these documents, they are able to select the policy constant at the level of the previous year. The OECD method involves subtracting from the current primary deficit the value that would have prevailed, had expenditure in the previous year grown with potential GDP and revenues with actual GDP. The IMF uses the same calculation except that they use as a benchmark not the previous year, but a year in which potential output was close to actual output (see Alesina and Perotti, 1995).
measures meant for debt or deficit stabilization. The action-based dataset of Pescatori et al. (2011) records fiscal consolidation measures undertaken in 17 developed countries, for each year in the period 1978-2009. In addition to identifying the austerity episodes, the data documents the budgetary impact (as a percentage of GDP in the year of implementation) prompted by the measures introduced in a given year as part of a fiscal consolidation package. Because the dataset includes only fiscal austerity episodes undertaken to ensure the sustainability of public finances, we treat all reported policy measures as independent of contemporaneous business cycle dynamics.

Due to its focus on implementation, a caveat of the Pescatori et al. (2011) dataset is that it does not treat the measures as components of multi-annual plans. As illustrated in Ramey (2011), the distinction between expected and surprise changes in fiscal variables has a bearing on the estimation of output multipliers. To address this point, Alesina et al. (2015a) analyze (a large part of the country sample in) the Pescatori et al. (2011) dataset in the context of multi-year plans, by breaking each fiscal consolidation plan into anticipated and unanticipated measures. The total implementation in a given year (corresponding to the value recorded by Pescatori et al., 2011) is the sum of anticipated measures announced in previous years (and implemented in the current year) and unanticipated measures. If a measure is announced in the last quarter of the previous year, it is considered to be unanticipated in the current calendar year. Measures approved earlier that are supposed to have an effect on the current year are coded as anticipated. While the initial data collection effort of Alesina et al. (2015a) relies on the same time period as Pescatori et al. (2011),

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2 Their work uses the ‘narrative approach’ of Romer and Romer (2010), who investigate the macroeconomic effects of tax changes and select exogenous measures on the basis of their motivation. Specifically, based on the Economic Reports of the President, Presidential speeches and statements, Annual Reports of the Secretary of the Treasury on the State of Finances and the Budget of the United States Government, they select only those tax changes for which the motivation was not related to developments that could affect output.

3 The sample contains 13 European (Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, the Netherlands, Portugal, Spain, Sweden, the United Kingdom) and 4 non-European countries (Australia, Canada, Japan, the United States).

4 Some studies claim that the fiscal shocks identified by the IMF can be predicted by lags of output growth (Jordà and Taylor, 2016), as well as by their own lags, a consequence of the fact that fiscal policy is conducted through multi-annual plans. However, as long as these shocks are orthogonal to contemporaneous macroeconomic variables this does not invalidate exogeneity in a VAR framework.
Alesina et al. (2015b) subsequently focus on a subset of countries, while extending the dataset with information for the period 2009-2013.

The literature on news-driven business cycles (see, for instance Beaudry and Portier, 2014), emphasizes that the announcements of policy changes can produce real effects independent from their implementation. While these datasets provide valuable insights into the output effects of fiscal consolidation measures, the identification of announcement effects is not very accurate when undertaken at annual frequency. In reality, due to the budgetary process, some announcements were done a few months before the start of the year they were assigned to. Therefore, these were already anticipated in the year when they are classified as unanticipated. Such imprecisions in annual data are the main motivation behind the creation of a new dataset.

2.3 Monthly Data on Fiscal Austerity Announcements

2.3.1 From implementation data to fiscal announcements

We create a new monthly dataset comprising announcements of action-based fiscal consolidation measures; more precisely, we identify the dates when the intention to pursue a fiscal consolidation plan was first officially announced by the government. In other words, we start from the information on narratively-identified annual consolidation plans present in the data discussed in subsection 2.2. We look for additional detail on the fiscal measures therein, in order to map the consolidation plans into announcement moments. In doing so, we move from data on annual implementations of fiscal plans to a characterization of all the announcements made as part of a given austerity plan.

Our data collection exercise starts from the consolidation episodes identified by Pescatori et al. (2011) and the descriptions of their constituent measures. If the dataset does not indicate that a consolidation took place for a specific country-year combination, we undertake no further action in this specific case. However, if a country implements a consolidation in a particular year, we try
to establish the specific month when the consolidation measures were announced for that country. Often, the description of the consolidation contains some information regarding the specific month or occasion when it was announced. An example concerns Australia: “The August 1993 Budget (...) also announced a four-year consolidation plan to reduce the budget deficit (...) In FY 1994/95 fiscal consolidation amounted to 0.5 percent of GDP due to tax measures.” (Pescatori et al., 2011, p.9).

In fact, in this case we also avail of the size of the consolidation. We confirm these announcement dates with the information provided in the Calendar of Economic Events of the OECD Economic Surveys. For the example given above the OECD Economic Survey (Australia 1994, p. 40 and 127) states: “The bulk of the saving measures over the four years were planned on the revenue side.” Hence, this illustrates how the IMF and the OECD refer to the budget for the fiscal year 1993/94 announced in August 1993.

When the Pescatori et al. (2011) dataset contains no information about the announcement date, we consider the total implementation in a given year and find all the announcements behind this total – which may be the result of a number of measures communicated 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. In matching the implementation recorded in Pescatori et al. (2011) with announcement information from various sources, we start from information on the budgetary process of the country and use the description of the measures included in the consolidation. We initiate the matching by looking the Calendar of Economic Events of the OECD Economic Surveys. If Pescatori et al. (2011) or the OECD Calendar of Economic Events provide no information regarding the date of the announcement, we use official national sources, in some cases cross-checked with newspaper articles (available on the internet).

2.3.2 The magnitude of fiscal announcements

Further, we try to quantify the magnitude of each identified announcement. We do this by extracting, cross-checking and combining information from a variety of official documents, such as
the OECD Economic Surveys, OECD Reports on Public Finances, national budgets, EU Stability and Convergence Plans, as well as from newspaper articles. The documents generally 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 a given date as the sum of the budgetary effects of the various individual measures announced on that date. Concretely, the magnitude of the announcement on a given date is the sum of the marginal impacts on the primary balance between now and six years ahead of the various new measures announced on that date. To give an example 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 \( t \) on, while Measure 2 is expected to have a negative marginal effect on the primary balance of 0.2% of GDP from year \( (t + 1) \) on. Then, the value of the announcement that we record for September of year \( (t - 1) \) is \( 0.5 - 0.2 = 0.3\% \) of GDP.

### 2.3.3 Sample differences

We base our data collection on the dataset of Pescatori et al. (2011), which ends in 2009. However, immediately after the financial crisis of 2008-2009, the deterioration in public finances that followed prompted countries to shift towards austerity. This was particularly the case in the Eurozone, where countries are also subject to the restrictions imposed by the Stability and Growth Pact. Because the Pact acts as an exogenous motivating force, many European post-crisis austerity measures fulfill the eligibility criteria of Pescatori et al. (2011) for inclusion in the database. This consideration also motivated the work of Alesina et al. (2015b), who narratively identified the annual consolidation plans implemented in a sample of 10 EU countries over the period 2009-2013.

Because our analysis may hold lessons about the effects of austerity choices made by European authorities in the aftermath of the Great Recession, in Chapter 4 we focus on the European subsample of the Pescatori et al. (2011) dataset.\(^5\) Our European dataset covers thirteen countries, observed over

\(^5\) Additionally, the estimation of homogeneous effects may be more suitable in this subgroup than in the full set of OECD countries.
the period 1978-2013. As described in section 2.3, for part of the country sample the announcements build on the narratively-identified annual consolidation measures documented in Pescatori et al. (2011) and Alesina et al. (2015b). We broaden the data coverage by expanding the country sample with Finland, the Netherlands and Sweden for the period 2009-2013 (for the period 1978-2008 these countries were already present in the Pescatori et al., 2011 dataset although they were not covered in the subsequent extension by Alesina et al., 2015b). The consolidation measures are all identified from official contemporaneous government documents using the methodology described earlier in this chapter. Hence, the identified plans are in principle exogenous to the business cycle. Because data collection was a gradual process and the Pescatori et al. (2011) dataset that initially dictates our sample runs until 2009, the first conducted analysis (presented in Chapter 3) excludes consolidations carried out in the aftermath of the Great Recession. While the Alesina et al. (2015b) data update covers the years 2009-2013, it excludes 3 European countries relative to the extended version of our dataset used in Chapter 4. For an overview, Table 2.1 provides the sample comparison across all the discussed datasets.

It is worth mentioning that, owing to inaccuracies in the sources of narrative data, the actual value assigned to an announcement can be a mix of ex-ante forecasts and real-time estimates of the impact of the measures on the primary balance. Most of the time, our sources (mainly the OECD Economic Surveys) provide an estimated impact of a plan at the moment of its announcement. However, there are instances when we do not have information about the estimated impact of a plan upon its announcement. In those cases, we use the impact as recorded by the EU Stability and Convergence Programs, or in documents produced by the IMF or the OECD. Some of these documents may have been issued after the consolidation started, thereby potentially providing a real-time assessment of the impact of a plan. Hence, the assigned value to the announcement represents an estimate of the pure shock value of the consolidation plan. Nevertheless, reporting a value has a substantial advantage over merely using a simple dummy for a fiscal announcement. Despite potential concerns about measurement errors, using values implies that less information
is thrown away and it allows us to exploit the possibility that larger consolidations elicit stronger responses than smaller consolidations. Moreover, it helps in more accurately classifying plans according to the dominant class of constituent measures, into revenue- or expenditure-based; by using the announcement value we characterize the plans not only based on the narrative description of the measures, but using the relative estimated impact of the revenue versus the expenditure measures.

2.4 Broad Principles for Announcement Identification

This subsection summarizes the guidelines we have applied to ensure the consistent coding of the information from written documents. Regarding what is considered the announcement of a new consolidation, we have proceeded in the following manner:

- A year is only considered a consolidation year if it is designated as such by the Pescatori et al. (2011) dataset. We match the description of the measures provided by Pescatori et al. (2011) with the text of the OECD Economic Surveys and (in certain cases) with supplementary information from national documents. Any additional measures mentioned in these documents that are not mentioned in the original dataset will not be coded as announcements.

- 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.

- We do not treat EU convergence plans as independent consolidation announcements.

- 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 announcement date for 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.

- When we lack information about timing, those measures taken in the period between the presentation of the Budget by the Government and the vote in Parliament are considered additional announcements. Other measures taken outside this period are considered implementations of previously announced measures.

- As a rule of thumb, if a measure is first mentioned in January, we consider it has been announced in the budget for that given year. Unless specified otherwise, January is generally a month of implementation.
Regarding the measurement of the impact of a given announcement we use the following guidelines:

• Although the policy announcements are identified at monthly level, we record the yearly GDP effect of each measure over a horizon of up to 6 years from the year of the announcement. The choice is justified by the fact that in general, fiscal policy effects are difficult to assign to particular months.

• Based on the values associated to each announcement (as explained above), we classify each as either on the government spending or revenue side.

• We overlook any announcements of reforms that take place in the same period as the fiscal consolidation but are not part of the fiscal policy package. However, we do subtract the effect of measures mentioned by the IMF as having had a budgetary impact in the consolidation years.

• We have encountered cases when the impact of a certain fiscal package (as reported by Pesatori et al., 2011) was different from the one estimated at the time of the policy announcement. This difference stems from difficulties in implementation, expenditure overruns, etc. We compute the impact for each specific measure at the time of the announcement and in those cases our numbers differ from those of the IMF. This is done in order to quantify the importance of a given announcement in real-time.
Table 2.1: Country and time coverage for narrative datasets on fiscal consolidations

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Chapter 3

The Confidence Effects of Fiscal Consolidations

3.1 Introduction

During the recent economic and financial crisis public deficits and debt increased dramatically. As a result, concerns about the sustainability of the public finances have pushed many industrialized countries into implementing ambitious fiscal austerity measures. The consensus view among economists was always that such fiscal austerity has negative short-run effects on economic activity. While this standard view was challenged by Alesina and Ardagna (1998) and Alesina and Ardagna (2010), who claim that austerity measures can generate expansionary effects on the economy, Guajardo et al. (2014) show that the claimed expansionary effects of austerity may be the result of

1 Ardagna (2004) points to the so-called ‘expectations channel’. If the general public sees the need for a consolidation, then an increase in current taxes or a reduction in public spending would reduce the need for future taxes and this could stimulate the economy. However, for this mechanism to work in a context in which consumption depends only on lifetime net income, postponing a current contraction requires a more than proportional future contraction, so that the present value of resource for consumption falls. A possible channel is that the budgetary crisis resulting from the postponement produces a disproportionate increase in the interest paid on the public debt. Bertola and Drazen (1993) develop a model in which a cut in public spending leads to significantly lower expectations of future spending and taxes, thereby stimulating current consumption. In the overlapping generations model of Sutherland (1997) an increase in the public deficit when debt is already high may lead to a contraction in consumption, because a rise in taxes becomes more likely. Generally, in a non-Ricardian world in which consumption depends only on current income, e.g. because of the presence of credit constraints, expansionary contractions are implausible.
biases in the selection of fiscal consolidation episodes. Using the consolidations identified in the ‘action-based’ dataset constructed by Pescatori et al. (2011), Guajardo et al. (2014) find that the expansionary effects of fiscal consolidations tend to be exaggerated. Hence, while the expansionary consolidation hypothesis seems to have become untenable, the debate has shifted towards the role of the composition of the consolidation strategy in affecting the macroeconomy. For instance, Alesina and Ardagna (2013) show that spending-based adjustments cause less contractionary effects than revenue-based adjustments.

At present a lot is still unknown about the channels through which consolidations affect the economy and what accounts for the different effects associated with the composition of a consolidation. In this regard, commentators frequently point to the role of private sector confidence. In particular, they often argue that Europe’s difficulties in escaping from the crisis are to be blamed on a lack of demand resulting from weak confidence.

In this paper we study the effects of consolidations on private sector confidence. The topic is very timely, because many countries are now consolidating their public balances. While the key role of confidence in the fiscal transmission mechanism is often stressed- for example, Hemming et al. (2002) emphasize the dependence of consumption and investment on the attitudes of households and firms–the quantification of the effect of fiscal measures on confidence has attracted surprisingly little attention. An exception is Konstantinou and Tagkalakis (2011), who find evidence that expansionary fiscal policy can boost consumer and business confidence, which in turn stimulates private spending and economic activity. Other exceptions are Cimadomo et al. (2011), Alesina et al. (2015a) and Stracca and Kalbhenn (2015). The latter do not find much evidence that fiscal consolidations affect variables like confidence and trust.

To establish a link with the existing literature and as a stepping stone for the empirical analysis based on our new monthly consolidation announcements dataset, we first present annual panel regressions linking confidence to consolidations. The consolidation episodes are those that are
identified on an annual basis in the ‘action-based’ dataset constructed by Pescatori et al. (2011). Our analysis is thus based on their data for 17 OECD countries over the period 1978-2009. Generally, we find that consolidations affect consumer confidence negatively. We also explore separately the effects of the different components of consolidation plans, as identified in Alesina et al. (2015a), both through a split into revenues and expenditure measures and a split into anticipated currently implemented measures, unanticipated currently implemented measures and changes to planned future measures. The latter two play the largest role in affecting consumer confidence. This is consistent with a situation in which mostly new and credible information affects confidence. Further, the (negative) confidence effects running through the revenues component of consolidations are larger and more significant than those running through the spending component.

A more accurate and timely picture of how consolidations affect confidence can be obtained by reconstructing a more precise timing of the release of information of the consolidations. For example, the announcement of many of the unanticipated and currently implemented measures in Alesina et al. (2015a) can be traced back to the budget generally presented in the previous year. This way it becomes easier to exclude confounding events and to see in real time how such information affects confidence. Hence, based on the narrative account of the fiscal consolidation episodes identified by Pescatori et al. (2011) and on other institutional information and official documents, we enrich the available annual data further by identifying the specific month in which each consolidation measure is announced. The core of our empirical results comes from an event study based on the resulting monthly panel dataset - which covers the same 1978-2009 period and set of 17 OECD countries as in Pescatori et al. (2011) - and is aimed at exploring the real-time and higher-frequency (i.e., monthly) association between consolidation announcements and movements in confidence.

The main finding from the event study is that consolidation announcements are associated with a reduction in consumer confidence and that this fall is mostly driven by revenue - based announcements. This effect is especially strong for the European countries in our sample. European
countries with weak fiscal rules and low transparency are accompanied by a strong and negative association between consumer confidence and consolidation announcements, while for countries with strong fiscal rules and high transparency there is no evidence of a significant relationship. Dissecting the observations on the basis of the state of the economy, we observe that announcements in booms have a negative effect on consumer confidence, which is possibly the result of a signal that the underlying fundamentals of the economy are weaker than perceived. Our findings on the effects of announcements on business confidence are generally weaker, but consistent with those for consumer confidence. Also in this case, revenue-based measures tend to harm confidence, while no such thing is observed for spending-based measures. In a final step, we investigate how consolidation announcements affect confidence in the sovereign - as measured by the long-term interest rate on the public debt - and the stock market. Spending-based consolidations cause a significant reduction in the interest rate. Such a reduction is also observed when the output gap is negative. Finally, stock prices fall for announcements taking place when the output gap is positive, which may again point to a signal that the underlying economic fundamentals are weaker than originally perceived.

Our findings suggest some potentially useful policy implications. First, the confidence effects of spending-based consolidations appear to be less negative than those of revenue-based measures. Second, a slump period is not necessarily a worse moment to announce a consolidation than is a boom. Last, solid institutional arrangements in the form of tight fiscal rules and transparent budgets could help in mitigating any negative confidence effects of consolidations.

The remainder of this paper is structured as follows. Section 3.2 provides a brief literature review. Sections 3.2.1 and 3.3 discuss the identification of consolidation episodes and the dataset. Section 3.4 conducts the annual panel regression analysis investigating how consolidations affect confidence. Section 3.5 presents the event study, while Section 3.6 concludes the paper and offers some policy implications. Appendix 3.6 contains further details on the construction of the data, as
well as more details on the results. Finally, a separate Data Construction Appendix contains the case-by-case discussion of the construction of the monthly consolidation announcements.

3.2 Literature Review

Policy-makers and the media often stress the role of private sector confidence in the fiscal transmission mechanism. Confidence is important in this regard if fiscal policy decisions have a significant effect on confidence, which in turn can affect the real economy. We start by reviewing existing evidence on the first part of the transmission, which is the focus of this paper, and then we move on to discussing the second part of the chain.

3.2.1 The effect of fiscal shocks on confidence

Although many authors stress the key role of consumer confidence in the fiscal transmission mechanism, the quantification of the effect of fiscal measures on consumer confidence has attracted surprisingly little attention. Giavazzi and Pagano (1990) were among the first to highlight the importance of confidence in the transmission of fiscal policies. The argument was that a drastic fiscal adjustment—reflected in a sharp fall in long-term interest rates—tends to generate an increase in consumer and investor confidence. This is likely to compensate the depressive Keynesian effect of tax hikes and spending cuts, thus resulting in an overall economic expansion following an episode of fiscal consolidation. More specifically, the authors studied the experience of Denmark in the early eighties and Ireland at the end of the same decade and argued that these episodes represent cases of ‘expansionary fiscal adjustment’. While Giavazzi and Pagano (1990) and the ensuing literature on the ‘non-Keynesian effects’ of fiscal policy (see, e.g., Giavazzi and Pagano, 1996; Afonso, 2001) attributed an important role to confidence in the transmission of fiscal shocks, these papers did not provide direct econometric evidence on the effects of fiscal policies on measures of consumer and producer confidence.
More recently, some authors have tested directly the effects of fiscal policies on consumer and producer confidence indicators in advanced economies. Focusing on the United States and on the period 1981-2008, Cimadomo et al. (2011) test the different effects of positive government spending shocks that are subsequently reversed, and of spending shocks that are followed by further spending growth. It is found that consumer confidence reacts positively to fiscal shocks with reversal, suggesting that a temporary fiscal stimulus with future fiscal restraint is considered to be beneficial for overall economic conditions. Instead, fiscal shocks accompanied by further future spending growth have a muted effect on consumer confidence. Using quarterly data for nine OECD countries covering the period 1970-2007, Konstantinou and Tagkalakis (2011) show that cuts in direct taxes and increases in non-wage government consumption stimulate both consumer and business confidence. In contrast, higher government wage consumption and investment reduce confidence. Stracca and Kalbhenn (2015) analyse the impact of fiscal consolidations on four measures of public opinion in EU countries, namely (1) life satisfaction, (2) consumer confidence, (3) trust in national institutions (government and parliament) and (4) trust in Europe and European institutions. Based on a panel of 26 EU countries over the period 1973-2013, they find that, overall, fiscal consolidation episodes have little or no impact on these measures of public opinion.

The two papers that are closest to ours are Alesina et al. (2015a) and Alesina et al. (2015b). Based on a sample of 17 OECD countries over the period 1978-2009, Alesina et al. (2015a) find that both consumer and business confidence fall when a fiscal adjustment is started. The effects on consumer confidence are larger. Moreover, for both confidence measures, the effects of revenue-based consolidation are larger than for spending-based consolidation. Alesina et al. (2015b) expand the data constructed by Pescatori et al. (2011) up to 2013. Building on the approach in Alesina et al. (2015a), Alesina et al. (2015b) explore how fiscal austerity in the OECD over the years 2009-13 has affected output growth, again finding that spending reductions are less costly than revenue-based consolidation. The current paper extends in several ways the analysis of confidence effects in Alesina et al. (2015a), and other papers mentioned above. Our annual panel regression
links confidence to the various components of the consolidation plan, while also distinguishing between revenues- and spending-based consolidations. We further extend the existing data to a set of monthly consolidation announcements. Using an event analysis this allows us to explore in real time the anticipation and reaction of confidence to those announcements, also conditioning the confidence effects on economic and institutional variables.

3.2.2 How confidence affects the real economy

The literature discusses various channels through which private sector confidence may affect the economy. The so-called ‘animal spirits’ view, which recently regained attention (see, e.g., Akerlof and Shiller, 2009), suggests that surprise fluctuations in beliefs may have (temporary) effects on economic activity. For example, Blanchard (1993) regards exogenous movements in consumer confidence as a cause of business cycles and, more specifically, of the 1991-2 recession. In such an environment, fiscal announcements may improve sentiment if they show the policymakers’ commitment to macroeconomic stabilization. This, in turn, would stimulate demand. The ‘information’ or ‘news’ view suggests that innovations to confidence largely reflect news about future fundamentals (Beaudry and Portier, 2006). The ‘news view’ has been tested with mixed outcomes. Ludvigson (2004) finds that consumer confidence predicts a relatively modest amount of variation in future consumer spending. However, recent research points to more sizeable effects. In particular, Barsky and Sims (2012) show that, for the US economy, confidence innovations are associated with a modest immediate response of real activity but with sizeable and prolonged subsequent consumption and income growth. Others focus on the possibility that households fail to perfectly observe fundamentals, but use observables like aggregate output to form beliefs about their true values (see Lorenzoni, 2009). After a recession, beliefs about improving fundamentals may be slow to catch up, thereby slowing down the recovery. Fiscal (and monetary) authorities may implement expansive policies to signal that fundamentals have improved. This, in turn, boosts confidence and helps the recovery to take off.
For the US economy, a direct test of the role of confidence in the transmission of fiscal shocks is provided by Bachmann and Sims (2012). They allow fiscal policy to have a direct effect on the economy, i.e., through the traditional Keynesian multiplier channel, and an indirect effect, i.e., through confidence. In their VAR framework, they use a counterfactual experiment to isolate the importance of this latter channel. They find that the endogenous response of confidence explains almost all of the fiscal-driven output expansion in recessions, whereas its role in normal times is minor. The positive responses of output and productivity to fiscal stimulus in times of slack are mild on impact, but tend to rise in a gradual and prolonged way. This also provides support to the ‘news’ hypothesis. In this paper, we show that fiscal consolidations can have significant effects on confidence and that those effects depend on the economic and institutional situation. The findings described above suggest that these confidence effects of consolidations may have important consequences also for economic activity.

3.3 The Data

We make use of different datasets from various sources. We start from the action-based dataset of Pescatori et al. (2011), which dictates the sample. It spans 17 countries, covering the years 1978-2009. The countries included are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Netherlands, Portugal, Spain, Sweden, the United Kingdom and the United States. The dataset contains the budgetary impact (as a percentage of GDP in the year of implementation) of fiscal policy measures adopted in a given year as part of a fiscal consolidation package. It only includes fiscal consolidation episodes that are not undertaken with a view to stabilizing output. Hence, the policy measures identified in the dataset are taken to be independent of contemporaneous business cycle dynamics.
3.3.1 Construction of annual consolidation variables

The Pescatori et al. (2011) dataset has been augmented by Alesina et al. (2015a) by converting the set of consolidation measures into ‘fiscal plans’ that in each year consist of the combination of three elements: the anticipated measures CSA that were announced in previous years and that are implemented in the current year, the unanticipated measures CSU implemented in the current year, and the shifts in fiscal variables announced in the current year for implementation in the coming years. Our precise construction of the latter component deviates slightly from that in Alesina et al. (2015a) and is given by:

$$CS_{Pi,t} = \sum_{j=1}^{3} \frac{CS_{Ai,t,j} - CS_{Ai,t-1,j+1}}{(1 + IRS)^{j}} + \frac{CS_{Ai,t,3}}{(1 + IRS)^{3}}$$

It consists of the discounted sum of changes to previously planned measures for future years plus new measures announced for implementation in future years. For the discount factor we use the short-term interest rate IRS taken from the OECD. Further, $CS_{Ai,t,j}$ captures the measures planned in year $t$ for year $t+j$, so that $CS_{Ai,t,j} - CS_{Ai,t-1,j+1}$ is the change between what was planned in the previous year for year $t+j$ and what is planned this year for year $t+j$. In the data, planning is virtually always for a maximum of three years ahead. Hence, the final term in the expression for $CS_{Pi,t}$ does not enter as a difference (i.e., $CS_{Ai,t-1,4} = 0$). In other words, $CS_{Pi}$ is to be interpreted as the present discounted sum of all unanticipated extra (positive or negative) consolidation planned in year $t$ for the future, up to year $t+j$. Alesina et al. (2015a) also construct $REV_{i,t}^{A}$ and $EXP_{i,t}^{A}$ as the revenue and spending measures implemented in year $t$ that were anticipated from announcements in previous years, and $REV_{i,t}^{U}$ and $EXP_{i,t}^{U}$ as the unanticipated revenue and spending measures implemented in year $t$. Fully analogously to $CS_{Pi,t}$, we construct $REV_{i,t}^{P}$ and $EXP_{i,t}^{P}$ as the present discounted sum of all unanticipated additional consolidation through revenues (spending) planned in year $t$ for the future. All these variables are expressed in shares of current GDP. The current implementation of consolidation measures is defined as $CS_{i,t} = CS_{i,t}^{A} + CS_{i,t}^{U}$ where $CS_{i,t}^{A} = CS_{i,t,0}^{A}$. We also have
$CS_{i,t} = REV_{i,t} + EXP_{i,t}$, where $REV_{i,t} = REV^A_{i,t} + REV^U_{i,t}$ and $EXP_{i,t} = EXP^A_{i,t} + EXP^U_{i,t}$. Obviously, $CS_{j,t} = REV^j_{i,t} + EXP^j_{i,t}$ for $A$, $U$ and $P$.

3.3.2 The confidence indicators and the control variables

Consumer confidence indices are collected from the OECD, which in turn obtains them from national statistical institutes, government agencies, banks and private and other research institutes. Consumer confidence is based on questionnaires sent out to a random sample of the population and the computed indices have been standardized by the OECD to make them comparable across countries. Each of the questionnaires contains four, sometimes five, questions on the current and expected future personal and general economic situation. 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. For a detailed description of this variable, please consult Appendix 4.A.3.

We obtain our macroeconomic variables from the OECD Economic Outlook. These include the output gap defined as the deviation of actual from potential GDP in percent of potential GDP, public debt in percent of GDP, per-capita real-GDP growth, inflation, the long-term interest, the unemployment rate and OECD-wide per-capita real-GDP growth. For Germany, we link all series of Western Germany for the first part of the sample with those for Germany for the second part of the sample. Information that quantifies aspects of policy-making institutions comes from various sources. The index of the tightness of fiscal rules is obtained from the European Commission (2014) and it is described for example, in Debrun et al. (2008) and Beetsma et al. (2009). In short, the index combines the strength and coverage of all rules in force for the various government sectors (general, central, regional, local and social security). Strength is determined on the basis of the statutory or legal base of the rule, the nature of the bodies in charge of monitoring and enforcing the rule, the enforcement mechanism and the degree of media visibility. Tighter fiscal rules imply a higher value for the index. We also make use of the Bernoth and Wolff (2008) ‘Audit’
index for fiscal transparency. It is based on whether the fiscal book keeping of the governments is externally audited, the degree of independence of the auditing and the extent to which the obtained information is disseminated.

### 3.3.3 The monthly fiscal announcement variables

This section of analysis relies on a first version of the newly compiled monthly dataset of fiscal announcement variables, described in Chapter 2, section 2.3.

Our monthly dataset, which spans the same countries and time period as the annual data, contains 217 announcements of new consolidation measures. In most cases, we were able to establish whether announcements were spending-based and revenue-based. Of the total identified consolidation announcements, 53% were spending-based, 32.7% were revenue-based and 1.4% were balanced between spending and revenues. Based on available sources, it was not possible to classify the remaining 12.9% of the observations. Figure 3.1 shows the frequency distribution of announcements over the twelve months of the year. While each month features a number of announcements (the minimum being nine for the month April), a relatively large part of the announcements are made in the fall (namely in the months of September and October) with the introduction of the new budget, which is generally discussed and approved in these months. Finally, even though the budgetary adjustments we consider are not motivated by the state of the economy, a large fraction of 66% of the announcements is made in a year when the output gap is negative.

Table 3.1 reports summary statistics for the consumer confidence index (CCI) and the business confidence index (BCI) pooled over all observations. For both indices some observations are missing. There are 18 consolidation announcements (about 8% of the total) for which CCI is missing and there are 71 announcements (33% of the total) for which BCI is missing. The average

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2 The cases where this was not possible were instances when commitment to consolidation was announced, but no precise measures were mentioned (and there was no impact recorded for the measures either). Examples are “The Treasurer announced the intention of the newly elected Government to return the budget to underlying balance,” “The Government announced a budget consolidation package comprising also measures to promote economic growth,” or “A corrective plan was announced to reduce the deficit.”
value of the indices over all observations is roughly 100. All values of CCI are located within an interval of 4% on either side from the mean. The observations of BCI are spread over a slightly wider interval.

Figure 3.2 shows the frequency distributions of CCI and BCI over all observations. Both distributions show slight evidence of negative skewness, but clearly for both CCI and BCI the large majority of the observations are located in an interval of $\pm 2\%$ from the mean. Further, Figure 3.3 shows the average percent change in CCI and BCI (computed over all observations) in months with and without announcements. Despite the fact that the OECD has purged the confidence series of seasonality, we observe a slight amount of seasonality in the data without announcements. On average, over all months, the changes seem to be close to zero. Switching to the subsample with announcements, we see that variation across the months is substantially larger, while, moreover, most (nine) months with announcements feature on average a deterioration of CCI and seven months feature on average a deterioration in BCI. For this subsample the variation across months is most likely due to the announcements themselves, rather than seasonality in the confidence indicator.

### 3.4 Annual Panel Regressions

Using the consolidations narratively identified by Pescatori et al. (2011) and the fiscal plan decompositions by Alesina et al. (2015a), this section uses panel regressions to explore how consolidations affect confidence at the annual frequency. The analysis in this section will help to establish a link with the literature and serve as a stepping stone for the analysis based on our new monthly dataset in the next section. Fiscal consolidations can come in different formats. They can be (largely) based on revenue increases or on reducing expenditures. Hence, we will also investigate the role of the consolidation composition in this regard. The sample consists of our 17 OECD countries over the period 1978-2009. We use the following panel regression framework for consumer and business
confidence:

\[ xCI_{i,t} = \mu_i + \lambda_t + axCI_{i,t-1} + \beta_1 CS^A_{i,t} + \beta_2 CS^U_{i,t} + \beta_3 CS^P_{i,t} + \lambda_1 GROWTH_{i,t} + \lambda_2 \Delta u_{i,t} + \lambda_3 INFL_{i,t} + \lambda_4 INT_{i,t} + \lambda_5 \Delta STOCK_{i,t} + v_{i,t}, \quad (3.1) \]

where subscript \( i(t) \) refers to the country (year) and \( xCI_{i,t} \) is \( xCI = CCI, BCI \) now the natural log of the confidence index. Specification (4.1) includes country- and year-fixed effects, a lag of the dependent variable and, following Konstantinou and Tagkalakis (2011), a set of macro controls capturing current country-specific economic conditions: per-capita real GDP growth in percent \( (GROWTH_{i,t}) \), the change in the unemployment rate in percentage points \( (\Delta u_{i,t}) \) and the CPI inflation rate in percent \( (INFL_{i,t}) \). The disadvantage of these controls is that they ignore the possibility that confidence might depend more on their expected future values than on their current or past values. Therefore, we also include forward-looking variables as the long-term interest rate in percent \( (INT_{i,t}) \) and the (log) change in the stock price index in percent \( (\Delta STOCK_{i,t}) \). The interest rate controls for the reaction of financial markets to fiscal consolidations, which could have a separate effect on confidence. The stock price index serves as a general proxy for private sector expectations about future economic conditions. Finally, \( v_{i,t} \) is a mean-zero error term. Variables \( CS^A_{i,t}, CS^U_{i,t} \) and \( CS^P_{i,t} \) were defined earlier and are all in percent of GDP of year \( t \). We will also estimate variants in which we impose restrictions on the \( \beta \)-coefficients.

We estimate our model using ordinary least squares (OLS) with heteroskedasticity- and autocorrelation-robust standard errors.\(^4\) To avoid potential endogeneity biases resulting from a

\(^3\)A priori, it is not obvious whether consumer confidence should depend on the level rather than the change of some of our control variables. This is in particular the case for the inflation rate, the unemployment rate and the long term interest rate. We end up using the level of the inflation rate and the long-term interest rate and the change in the unemployment rate, because they have the best fit. However, our main results are robust to alternative transformations of the control variables.

\(^4\)As is well-known, OLS with fixed effects and a lagged dependent variable as regressor generally leads to the so-called Nickell-bias in the coefficient estimates. However, this bias is small when the number of observations in the time dimension is substantial, which is the case here. Using the least squares bias-correction based on Bruno (2005) indeed shows that the bias is marginal (results available upon request from the authors). Hence, we continue using OLS for our panel regressions.
feedback of confidence onto the right-hand side variables, we use the end-of-year (e.g. December) indicator for confidence. We found, however, that using the yearly average instead of the year-end value for confidence indicators hardly yields any differences.

Table 3.2 reports the estimation results. Following the more “traditional” approach, Column (1) uses the CAPB as the variable measuring the amount of discretionary fiscal changes. There is a fair amount of persistence: the first lag of confidence enters with a highly significant coefficient of about 0.5. Of the controls, only GDP growth and the change in stock prices are significant. Both have the expected positive sign. An increase in growth by one percentage point raises consumer confidence by about 0.18 percentage points, while a one-percentage point higher growth in stock prices raises confidence by 0.02 percentage points. However, the CAPB plays no role in explaining confidence. Column (2) repeats the regression in Column (1), but setting the CAPB to zero for country-year combinations that are not identified in the Pescatori et al. (2011) dataset as consolidation events. All coefficient estimates are essentially unchanged. Column (3) replaces the CAPB with CS, which is the sum of the anticipated and unanticipated consolidation measures implemented in the current period. This corresponds to the original IMF action-based consolidation variable. We see that a one percentage point of GDP additional implementation of consolidation measures results in a significant 0.12 percentage points reduction in consumer confidence.\(^5\)

Splitting current-period implementation into its anticipated and unanticipated components \(CS^A\) and \(CS^U\) (Column (4)) shows that the significance of the aggregate CS is driven by the unanticipated component. The component \(CS^A\) could potentially exert a significant effect, if its materialisation contains a surprise, because the original consolidation announcement was less than fully credible. However, only the coefficient on \(CS^U\) is significantly negative, in contrast to that on \(CS^A\), which,

\(^5\) We did the same regression replacing the nominal interest rate and inflation with the ex-post real interest rate. Both the magnitude and the significance of the other coefficients are unchanged – we report them in Appendix 3.B. Here, we also report the estimates for the case in which we limit the sample to those observations for which business confidence is also available. The negative effect of consolidations on consumer confidence is not driven by the sub-sample of observations for which business confidence is not available – in fact, the effect become even more strongly negative. Finally, in Appendix 3.B we report on the absence of possible spill-overs from consolidations abroad.
although negative, is insignificant and smaller in absolute magnitude. Indeed, to the extent that consolidations are credible and have confidence effects, the unanticipated component of the consolidation would affect confidence, but not the anticipated component. We also test formally whether the coefficients of $CS^U$ and $CS^A$ are equal. The results of this and other tests of the equality of coefficients are reported in Appendix 3.D. We cannot reject the hypothesis that the coefficients of $CS^U$ and $CS^A$ are equal. Column (5) adds to the specification in Column (4) the announced changes to the future consolidation measures ($CS^P$). The coefficient on $CS^P$ comes out significantly, again suggesting that it is new information on consolidations that is driving confidence. Moreover, the coefficient on $CS^P$ is substantially larger in absolute magnitude than the coefficients on $CS^U$ and $CS^A$. An announced increase in future consolidation by one percent of GDP reduces consumer confidence by almost 40 basis points. However, we cannot formally reject the equality of the coefficients for all the three variables $CS^A$, $CS^U$ and $CS^P$, or for any pair among them. Column (6) restricts the coefficients on the latter two components to be identical. This way we enter the present value of all unanticipated consolidation effort as an independent variable. As expected, only this coefficient is (highly) significant, while the coefficient on $CS^A$ is not. Again, a test for equality of the coefficients on $CS^A$ and on $CS^U + CS^P$ does not reject the equality hypothesis.

The main concern with model (1) is the correct identification of consolidation events that are exogenous to the other variables in the model as well as the correct dating of these events. The dating of the release of new information about consolidations is likely to be inaccurate. For example, what is classified as new information in year $t$ may have been announced in the context of the budget discussion in the fall of year $t - 1$. For this reason, we construct a monthly dataset of consolidation announcements, whose effects on confidence are studied in the next section. The IMF has been careful to narratively identify consolidation events that are not motivated by the state of the economy, but on the basis of “the objective of stabilizing or reducing the debt ratio.” Panel probit regressions reported in Appendix 3.D show that the only variables in our model able to predict consolidations are lagged debt, inflation and the long-term interest rate. Higher
debt and higher long-term interest rates negatively influence the long-run budgetary sustainability, making consolidation more likely, while higher inflation relaxes the government budget constraint, given the outstanding stock of public debt, and makes consolidation less likely. To control for the potential predictability of consolidations, and thereby for potential “allocation bias” (Jordà and Taylor, 2016), and feedback effects among variables, such as indirect effects of consolidations on confidence through stock prices or long term interest rates, we estimate separately annual panel vector auto-regressions (PVAR) with \([CS_j, CCI, GROWTH, INFL, ΔSTOCK, INT]′\), for \(j = A, U\) and \(P\), as the vector of endogenous variables. As in the PVAR of Guajardo et al. (2014), we apply a Cholesky, triangular identification scheme, with the consolidation measure ordered first. As such, the relative ordering of the other variables is irrelevant Christiano et al. (1999). Within this specification, we assume that anticipation effects of consolidations are absent and that there are no within-year feedback effects from these other variables onto the decision to consolidate. This assumption maybe tenuous, again providing a reason to consider consolidation announcements at the monthly frequency. We include country- and year-fixed effects and set the lag length to two. The detailed results are reported in Appendix 3.E. The impact effect of the anticipated component of a consolidation \(CS^A\) is insignificant, while those of a 1% of GDP unanticipated consolidation component \(CS^U\) and \(CS^P\) are significantly negative with impact effects of -0.23% and -0.44%, respectively. Hence, overall, the results on significance are in line with those reported in Table 3.2, while the magnitude of the effects with a PVAR tends to be larger, though of the same order.

Table 3.3 reports the estimates of (1) when we replace \(CS^j\) (\(j = A, U\) or \(P\)) with its components, that is \(EXP^j\) and \(REV^j\). Since the estimates of the coefficients of the control variables are very close to those in Table 3.2, we do not report these estimates. The same is the case for the coefficient on the first lag of consumer confidence, which is always close to 0.5. Column (1) includes the full current implementation of revenues and spending measures and shows that both enter with a negative coefficient, but that only that of revenues is (highly) significant. A one percent of GDP increase in consolidation effort through revenues reduces consumer confidence by 0.22 percentage points.
Statistically, the coefficients on revenues and spending measures do not differ, although the test is not far from significance at the 10% level. A split into anticipated and unanticipated measures in Column (2) shows that all four components enter with a negative sign, but that only the coefficient on unanticipated current revenues $REV^U$ is statistically significant, a result in line with those reported in Table 3.2 and in the previous column. The coefficient on $REV^U$ is also statistically different at the 10% level from that on $EXP^U$. In Column (3) we add the changes to the planned future measures $REV^P$ and $EXP^P$ to the specification in Column (2). Again, the coefficient on $REV^U$ is significantly negative and statistically different at the 10% level from that on $EXP^U$. Finally, Column (4) only distinguishes between anticipated current measures and the sum of unanticipated current measures plus planned future changes. While all coefficients are negative, only that on the latter variable enters significantly. We can summarise the main results from Tables 3.2 and 3.3 as follows. First, consolidations tend to affect consumer confidence negatively. Second, this negative effect is largely associated with the release of new information about the consolidations. Third, revenue-based measures exert a stronger negative effect on consumer confidence than spending-based measures. Table 3.4 is the analogue of Table 3.2, but for business confidence. The first lag is always positive, but insignificant. As in the case of consumer confidence, the only relevant controls are real GDP growth and the change in stock prices, both entering with highly significant positive coefficients. The CAPB and CAPB_IMF measures have no effect on business confidence. Neither has CS or its subcomponents $CS^A$ and $CS^U$. However, planned changes in future consolidation measures CSP do exert a significantly negative effect – see Column (5). This is in line with their effect on consumer confidence, also in terms of order of magnitude. Moreover, the coefficient on $CS^P$ is statistically different from that on $CS^A$ and $CS^U$ at, respectively, the 5% and the 10% level. In Column (6) we restrict the coefficients on $CS^U$ and $CS^P$ to be equal. The common coefficient is significantly negative at the 10% level and it is also significantly different at the 10% level from that on $CS^A$. Analogous to Table 3.3, Appendix 3.F reports how the revenues and spending components of consolidations affect business confidence. The results are not very clear-cut. We observe that
EXP\textsuperscript{A} enters with a significant positive coefficient in Columns (2) and (3), while the coefficient of EXP\textsuperscript{P} in Column (3) is significantly negative.

Overall, the negative confidence effects are weaker for business confidence than for consumer confidence. Possibly, the business sector has a more sanguine view of consolidations, because it realises better their necessity for the long-run health of the public finances. Most importantly, it is only the unanticipated components of consolidations that are driving both consumer and business confidence.

3.5 Event Study with Monthly Data

The core of our empirical results is obtained with our newly constructed monthly dataset of consolidation announcements. Compared to the annual dataset, our monthly dataset enables us to investigate in a more precise manner the role of consolidations for confidence, because we can gain accuracy in pinpointing the release of information about the consolidations and establish in real time how confidence reacts to this information. We perform our analysis on the full sample and a number of motivated sample splits. Concretely, we estimate the following regression:

\[ xCI_{i,t} - xCI_{i,0} = c_t + \varepsilon_{i,t}, \quad \text{where} \quad t = -6, -5, \ldots, 5, 6. \]  \hfill (3.2)

\( xCI_{i,t} \) is \((xCI = CCI, BCI)\) the natural logarithm of the confidence index in month \(t\) for country \(i\), \(c_t\) is the constant to be estimated and \(\varepsilon_{i,t}\) is an error term.\textsuperscript{6} We estimate equation (4.2) for each \(t\) relative to the month of the announcement.\textsuperscript{7} If \(t < 0(t > 0)\), a positive and significant value

\textsuperscript{6} Before estimating (4.2) we demean and de-trend the confidence indicator at country level. More specifically we obtain the confidence indicator from a panel regression that includes country-specific fixed effects and time trends. There is essentially no trend visible in any of the confidence variables, hence the de-trending has virtually no effect on the results. In all instances the standard errors are robust to heteroskedasticity.

\textsuperscript{7} A potential caveat of our approach is the existence of contamination from the presence of other announcements in the event window. To check how serious this limitation is in practice we do the following. When subsequent announcements happen in consecutive months, we treat them as a block and estimate (4.2) for \(t = -6, \ldots, -1\) relative to the first announcement and for \(t = 1, \ldots, 6\) relative to the last announcement. This approach leads to marginal differences to what we report in the main text. On top of this, below we also show that our results are robust to more general forms of contamination effects within a regression analysis.
of \( c_t \) indicates that confidence falls (rises) in the \( t \) months before (after) the announcement of a consolidation. Hence, we explore average movements of confidence in the half year before the announcement and the half year after.

### 3.5.1 Consumer and business confidence

Based on the regression in equation (4.2), Figure 3.4 depicts for the full sample the average movements of consumer confidence around announcement dates, i.e. the coefficient \( c_t \), plus an error band of \( \pm 1.645 \) standard deviations around the central line, so a 10% margin on either side of the confidence band. To read the figure, take as an example the value of 0.1 of the central line at \( t = -5 \), which says that five months before the consolidation announcement \(( t = 0)\), the confidence indicator is on average 0.1% higher than at the moment of the announcement. The figure reveals significant movement in the confidence index, both before and after the announcement, although the movement after the announcement is short-lived. The maximum overall movement within the window is on the order of \( 0.15\% \). This number seems to be rather small. However, the frequency distribution of consumer confidence in Figure 3.2 showed that the overwhelming majority of the observations is within \( \pm 2\% \) of the mean. Hence, a confidence movement of \( 0.15\% \) around a consolidation cannot be considered particularly small. Of particular relevance is the overall movement of confidence over the event window. On the one hand, a movement that fully reverses itself within a couple of months is likely to be less consequential for the economy than a more permanent movement. On the other hand, if we make the event window wider, the likelihood of other factors affecting the movement of confidence becomes larger. Hence, we consider as a reasonable compromise an event window of \( \pm 6 \) months. The final column of Table 3.5 reports the average movement of confidence over the entire window. The overall fall is \( 0.15\% \). The figure is close to significance at the 10% level.

Proper identification of consolidation announcements is again the most important issue. One concern is that the consolidations are not exogenous, because they are anticipated as the downward
movement in the index suggests. However, some anticipatory movements of confidence should not be too surprising if consolidations do indeed affect confidence. Many of the announcement months coincide with the presentation of next-year’s budget, while the budgetary process is closely followed by the media. In addition, there may be discussions in parliament, hints by politicians and leakages to the press. Such anticipation effects do not by themselves invalidate the assumption that the consolidation is exogenous. The exogeneity assumption would be invalid if confidence itself influenced the choice to consolidate, but that would mean that consolidation decisions are made within a couple of months after an initial movement in confidence. However, it is extremely implausible that confidence movements of the magnitude that we observe by themselves trigger consolidations and, moreover, that consolidation decisions take place so quickly after an initial movement in confidence. Nevertheless, following our annual investigation, we explore Appendix 3.D the predictability of consolidations at the monthly level. Because the macro variables are not available at such high frequency, we need to limit ourselves to confidence, stock prices and the interest rate. There is one instance in which the lagged change in confidence has some predictive power. However, as argued above, this is unlikely to be a causal effect. Changes in confidence over longer periods have no predictive power.

A second concern is our timing of the moment that new consolidation information is released. This timing is more accurate than for the annual dataset. Still, consolidation information may be released before the official announcement date. Indeed, we observe instances of announcement moments in the data after which confidence falls abruptly (e.g. Italy in September 2003) and instances with confidence (mainly) falling prior to the announcement (e.g. Austria in March 1996). However, we stick to the official announcements, because prior confidence movements may be partly caused by confounding factors and they may be the result of a gradual release of new information, which would make it impossible to assign one specific month in which all relevant consolidation information is released. If the mis-measurement of the exact timing of the consolidation information is random, then attenuation bias actually drives the estimated constants in equation (4.2) towards zero and thus works against finding significant effects of consolidations on confidence.
To explore the role of the timing of announcements further, we implement two experiments. In one we purposefully mis-time all announcements to one month earlier or later than our coding. Indeed, in the former (latter) case a stronger deterioration becomes visible after (before) the assumed announcement date (see Appendix 3.G). In the second experiment for each country we take out of the sample the month with the most observations. The deterioration of confidence around announcements tends to strengthen (see Appendix 3.G). This may not be too surprising: the month with most observations is the month in which the budget is most likely to be presented. Hence, what is left over are mostly extraordinary measures not taken in the context of the regular budgetary procedure. The need for consolidation in those instances may be particularly high.

Figure 3.5 depicts the outcomes of a split of the full sample of announcements into subsets of spending-based and revenue-based announcements. The former do not harm confidence. By contrast, announcements of revenue-based consolidations produce a significantly negative confidence effect. Relative to the overall sample, the maximum confidence deterioration over the full event window more than doubles to 0.4 percent - see also Table 3.5. Based on the estimation of (4.2) as a system for spending and revenues, the final column in Table 3.5 shows that the test that the full-window movements of confidence for spending and revenues are equal rejects at the 5% level. If confidence indeed plays a role in transmitting the effects of consolidations to economic activity, the combination of these findings is consistent with the findings of Alesina et al. (2015a) and related papers that spending-based consolidations have less adverse effects on the economy than revenue-based consolidations. We have repeated the exercise reported in Figure 3.4 and 3.5 by obtaining the confidence indicator from a panel regression that includes not only country-specific fixed effects and time trends, but also fixed effects for each month in the sample. The significance in Figure 3.4 and that for revenue-based consolidations even strengthens somewhat.

Two further concerns with our event study are (i) the presence of multiple announcements in the event window, so that confidence movements cannot be attributed to one specific announcement or the other, and (ii) that we give all announcements the same weight irrespective of the size
of the consolidation.\textsuperscript{8} To address these concerns, we estimate a panel distributed lag model of the log-difference in confidence, where on the right-hand side we include lags and leads of the announcement dummies to address concern (i), or values of the announcements to address concern (ii). In the latter case, because we cannot assign values to all announcements, we lose about one-third of the identified consolidations. Based on the announcement dummies, as in Table 3.5, we find no effect of a spending-based consolidation on consumer confidence, while a revenue-based consolidation leads to a significant decline in confidence with a magnitude of over 0.3 percent over the event window. As regards to the use of the values of the consolidations we find that the effect of a one-percent of GDP consolidation announcement on consumer confidence over the 12-month event window is close to zero. If we focus on spending- and revenues-based consolidations we obtain qualitatively the same results as for the event study. Over the full window a one-percent of GDP spending contraction raises confidence by about 0.3 percent, while an equally-sized revenue-based consolidation produces a fall in confidence of about 0.5 percent. The full set of results is provided in Appendix 3.H.

Table 3.5 also reports confidence movements based on a dissection of the observations into those associated with lower- and higher-than-average public debt over the sample period. For the low-debt observations confidence falls significantly in the period before the announcement, while for the high-debt observations there is a significant movement of confidence around the announcement date. However, the overall movement of confidence over the entire window is very similar and the equality test does not reject.

Recently a substantial number of contributions (e.g., Auerbach and Gorodnichenko, 2013; Owyang et al., 2013 and Jordà and Taylor, 2016) have investigated whether fiscal multipliers depend on the state of the economy. If confidence plays a role in the transmission of fiscal shocks to the economy, it is important to explore whether the effects of announced consolidations on confi-

\textsuperscript{8} A potential final concern is that the observed negative confidence effect of consolidations is actually the result of the severe contractions at the end of the sample and their effect on confidence. However, the plots in Appendix 3.I show that there is relatively little consolidation activity in the last two sample years 2008 and 2009, i.e. the crisis years. Hence, this possibility can be refuted.
dence differ depending on the state of the economy. We observe that that is indeed the case, where we split the sample according to whether the output gap is negative or positive in the year when the consolidation announcement takes place. When the output gap is negative an announcement does not have a significant effect on confidence. This contrasts with the case of a positive output gap, where the announcement of a consolidation on average leads to a highly significant reduction in confidence both before and after the announcement. The fall in confidence over the entire window is 0.75 percent, which is statistically different from the confidence movement when the output gap is negative. A possible explanation for our finding could be that the announcement of a consolidation during a boom signals that the underlying fundamentals of the economy are weaker than perceived thus far. Overall, the results suggest that potential worries that the confidence channel of a consolidation may negatively affect an already ailing economy may be misplaced for spending-based consolidations.

We can also dissect the sample into country sub-samples. The most obvious dissection is a split into European and non-European countries. For the non-European sub-sample, most likely due to the presence of too few observations, there is no significant movement in confidence and, hence, we do not report the results. This suggests that the effect of the announcements is largely confined to the European countries. For the European subsample we find indeed a 5% significant deterioration of confidence by 0.2 percent over the full event window. If we split further into revenue-based and spending-based consolidations for the European sub-sample, we do not observe significant confidence movements associated with spending-based consolidations, but we observe (highly) significant negative movements associated with revenue-based consolidations, both before and after the announcement. The deterioration over the full window is 0.6 percent and this is (highly) significantly different from the confidence movement in the case of spending consolidations.

---

9It is conceivable that, even in the absence of consolidation announcements, consumer confidence behaves systematically differently depending on the state of the economy. Therefore, we also explored a variant in which, before estimating (4.2) for the subsamples of negative and positive output gap observations, we orthogonalise the confidence indicator with respect to the country-specific real GDP growth rate in addition to demeaning and de-trending it at the country level. However, the pattern of confidence movements around announcements remains the same in terms of significance, though slightly smaller in magnitude.
Now we turn to the role of the institutional variables. We lose observations for four countries (Australia, Canada, Japan and the U.S.) because we have these data only for the European countries. We split the remaining 13 European countries into a group that on average over the years have a fiscal rules index higher than the average over all European countries and a group with an index lower than the average. The former group consists of Denmark, Finland, Germany, the Netherlands, Spain, Sweden and the United Kingdom, while the latter group includes Austria, Belgium, France, Ireland, Italy and Portugal. We see from Table 3.5 that countries with a relatively strong fiscal rules index exhibit no movement in confidence, while countries with relatively weak fiscal rules exhibit a highly significant decline in confidence both before and after the announcement. The difference in movement over the entire window for the two sub-samples is also significant. A potential explanation for the confidence behaviour in the group with weak fiscal rules is that private agents do not expect the consolidation to credibly address fiscal imbalances, but only to harm their own economic situation, for example by reducing their disposable income or by causing more unemployment.

Table 3.5 also reports the results for a similar split of the countries into groups featuring higher-than-average and lower-than-average fiscal transparency over the sample period. Transparency is based on the “Audit” index from Bernoth and Wolff (2008). The high-transparency group is Austria, Belgium, Denmark, Finland, Ireland, Netherlands and Sweden, while the low-transparency sample is France, Germany, Italy, Portugal, Spain and United Kingdom. The results are very similar to those for the split into countries with weak and strong fiscal rules. This is not surprising, since the country groups overlap to a substantial extent.\(^{10}\)

Finally, we also construct a country group with both strong rules and high transparency and a country group with both weak rules and low transparency. The first group consists of Denmark, Finland and Netherlands, while the second group consists of France, Italy and Portugal. Table 3.5 shows that the pattern of changes in confidence for the second group is similar to those for the countries with either weak rules or low transparency.

\(^{10}\) We also did a split based on the index constructed by Hallerberg et al. (2005). Here, the high transparency group was Finland, France, Germany, Ireland, Netherlands, Spain and United Kingdom, while the low transparency group was Austria, Belgium, Denmark, Italy and Portugal. The resulting figures look similar to those for the Bernoth-Wolff index.
Table 3.6 reports the results for business confidence around a consolidation announcement. Looking at the full sample, there appears to be no systematic behaviour in business confidence around consolidation announcements. However, if we split into spending- and revenue-based announcements, we obtain a pattern similar to that for consumer confidence: there is no systematic effect for spending-based consolidations, while revenue-based consolidations exhibit a fall in confidence before and after the announcements. The fall over the entire event window is more than 0.4 percent and it is highly significant. It is also significantly different from the full movement of confidence under spending-based consolidations.

Splitting the sample into high- and low-debt observations reveals little difference between the two groups. By contrast, when we split the sample into on the basis of the sign of the output gap in the year when the announcement takes place, there is a substantial difference with confidence not significantly moving when the output gap is negative and confidence exhibiting a significant deterioration both before and after the announcement when the output gap is positive. The full-window deterioration of confidence is about 0.6 percent. Focussing on European countries only, there is a significant deterioration of business confidence around announcement dates. Splitting the European sample further into spending- and revenue-based consolidations, we observe that this deterioration can be attributed entirely to the subsample of revenue-based announcements, which produce on average a full window decrease of confidence by about 0.65%. We also split the European sub-sample into countries with strong and weak fiscal rules and with high and low transparency. However, now there is not much action in confidence in any of the sub-samples.

Summarising, consolidation announcements are associated with a fall in confidence. The effect is primarily observed for revenue-based measures, when the output gap is positive and for European countries. For the latter group the negative announcement effect is stronger for countries with weak fiscal rules or low budgetary transparency. The effects on business confidence are quite similar to those on consumer confidence, though they are generally slightly weaker. They differ specifically for the institutional splits, which do not seem to matter for business confidence.
3.5.2 Broadening the concept of confidence

In this subsection we broaden the concept of confidence by analysing what are the consequences of consolidation announcements for financial markets. In particular, we focus on sovereign debt markets and stock markets.

More confidence in the sovereign implies lower borrowing costs, so a lower real debt burden and, hence, more resources for consumption. This form of confidence may also impact on economic activity. This became particularly clear during the recent European sovereign debt crisis, when for some vulnerable countries financial markets priced in a higher likelihood of default as reflected in higher sovereign yields. This eventually resulted in higher private sector lending rates and an overall credit contraction in this period (see Bofondi et al., 2013; Popov and van Horen, 2013).

Here, we explore the behaviour of the long-term public debt interest rate around announcements of consolidations. The impact of fiscal policies on sovereign yields and spreads has been investigated by a number of authors, but with mixed findings. For example, based on a dataset including 17 advanced economies over the 1989-2012 period, Dell’Erba and Sola (2016) find that fiscal consolidations tend to be associated with a decline in long-term interest rates. Moreover, after an increase in the public deficit, long-term interest rates increase more in countries characterized by macroeconomic or institutional weaknesses. Based on a panel of advanced and emerging countries for the period 1990-2013, Born et al. (2015) indeed find that cuts in government consumption tend to reduce spreads, but only during expansions. Instead, fiscal consolidations tend to trigger increases in public interest spreads during recessions and periods of fiscal stress.

We proceed as follows. First, we linearly de-trend the long-term interest rate. Then, we estimate equation (4.2) where the confidence indicators are replaced by the long-term rate. The results are reported in Table 3.7. For the full sample, we observe a significant fall in the long-term interest rate following a consolidation announcement. A split of the sample into spending- and revenue-based consolidations shows that there is no systematic movement of confidence around revenue-based announcements, while there is a highly significant and long-lasting fall in the interest rate after
the announcement of a spending-based consolidation. This suggests that financial markets are confident that a spending-based consolidation produces a fall in sovereign risk, while this is not the case for a revenue-based consolidation.

Splitting the sample into high- and low-debt observations yields very similar confidence dynamics for the two groups. By contrast, a split of the sample into observations with negative and positive output gaps reveals a significant fall in the long-term interest rate of almost 0.4 percent following a consolidation announcement in the former case, while no effect is detected in the latter case. This finding seems to deviate from Born et al. (2015), where spending cuts in the form of reduced government consumption exert a negative effect on spreads when the economy is expanding. A likely explanation for our finding is that in situations when the output gap is negative the financial position of the government is generally weak and, hence, a consolidation may inspire some confidence in investors that the financial problems will be tackled and that the default risk on their debt holdings becomes smaller.

Confining ourselves to the European countries we find again a significant decline in the long-term interest rate over the full window. Splitting the European observations into spending- and revenue-based observations, we confirm what we find for the full sample: spending-based consolidations produce a significant decline (of more than 0.3 percent) over the entire window, while revenue-based consolidations lead to hardly any movement in confidence over the window. The difference in overall movement for the two consolidation regimes is also significant. Finally, we split the European sample on the basis of the tightness of the fiscal rules and the degree of transparency. The picture that emerges is not very clear-cut. The decline in the long-term interest rate over the full window is significant in the case of weak fiscal rules and of high transparency. Probably, when fiscal rules are weak, confidence in the sovereign is low, implying substantial potential room for a reduction in the interest rate. If we consider only countries with both strong fiscal rules and high transparency and countries with both weak fiscal rules and low transparency, we observe for both groups a reduction in the long-term interest rate over the full window. However, these reductions are not statistically significant.
The format of our investigation of stock prices - whose results are reported in Table 3.8 - is the same as for confidence and long-term interest rates. For the full sample of consolidation announcements, there is no effect on stock prices, and neither is there for a split into spending- and revenue-based consolidations or a split into high and low-debt observations. A consolidation announcement when the output gap is positive produces a significant fall in the stock index. This seems consistent with the deterioration in consumer and business confidence and it may signal that the economy’s fundamentals are weaker than perceived before. Confining ourselves to the European countries, for a split into revenue- and spending based measures, we observe that the former are associated with a significant deterioration in confidence. Finally, splitting the sample on the basis of the quality of institutions does not yield specific differences.

Summarising, announcements of consolidations lower long-term interest rates on the public debt, in particular when the output gap is negative and when they are spending based. Stock prices exhibit a negative movement when the output gap is positive.

3.6 Concluding Remarks

This paper explores how fiscal consolidations affect consumer and business confidence. For this purpose, we expand the annual “action-based” consolidation dataset by Pescatori et al. (2011) into a dataset of monthly consolidation announcements. In our view, studying how fiscal consolidations affect confidence is important, because confidence may affect economic activity. It has been regularly argued that a lack of confidence hampers the Eurozone economies in escaping from their current stagnation.

As a stepping stone for the analysis based on our monthly data, we present the results from annual panel regressions, in which we link confidence to consolidation plans and their components, i.e., anticipated and currently implemented measures, the unanticipated and currently implemented measures, and the planned changes to future measures. We establish that with annual data the
largest role in affecting consumer confidence is reserved for the latter two components. This may not be surprising, as these components capture the release of new information.

The core of our empirical analysis is the study of the real-time reaction of confidence to consolidation announcements from our monthly dataset. Generally speaking, announcements are associated with a reduction in consumer confidence. Consistent with previous studies on the composition of consolidations, we find that the negative association of consumer confidence with consolidation announcements is driven by the announcements of revenue-based consolidations. This negative association is particularly strong for the European countries. Dissecting the European countries according to the tightness of their fiscal rules or the transparency of their budget reveals that weaker fiscal rules and lower transparency are driving the negative association between consumer confidence and consolidation announcements. Our findings for the association of announcements with business confidence are generally slightly weaker, but largely in line with those for consumer confidence, in that the more negative effect for revenue-based relative to spending-based consolidations is preserved. Finally, we explore how consolidation announcements affect confidence in the sovereign, as measured by the interest rate on long-term government securities, as well as stock price indices. Spending-based consolidations produce a significant reduction in the interest rate, especially for European countries.

Our findings point to a number of potentially useful policy insights on the “optimal” design of fiscal consolidations in terms of timing, composition and institutional factors. First, taking the need for consolidation as given, spending-based consolidations appear to have less harmful effects on (consumer) confidence than revenue-based measures. This may be a consideration when deciding about the design of a fiscal consolidation package. Second, since the confidence effects seem mainly driven by unanticipated measures, as our annual panel regressions suggested, a careful release of the information on prospective consolidations may be important. Third, while it is often asserted that periods of boom are more suitable for consolidation than slump periods, this is not borne out by our monthly sample. In fact, consolidations do not seem to harm private sector confidence in
slump periods, while they do seem to produce negative confidence effects when the output gap is positive, possibly the result of a signal that the underlying fundamentals of the economy are weaker than perceived before. Moreover, when the output gap is negative consolidation announcements push the long interest rate downward. These findings may rationalise the consolidation packages announced and adopted in many European countries during the recent crisis. In this context, consolidation announcements are likely to have signalled commitment by governments to restore financial markets’ trust in the long-term sustainability of the public finances, which could have triggered a reduction in the financing cost for these countries. Fourth, the quality of institutions may be important in mitigating negative confidence effects of consolidations. There seems to be a high correlation of the different dimensions of institutional quality, but tight fiscal rules in particular may be conducive in this regard. Since fiscal consolidations may be inevitable from time to time, for example because of adverse developments in financial markets, governments of countries with no or weak fiscal rules would do well to consider the adoption of tighter rules. When credible, these reduce the chances of a need for consolidation and, if this need emerges nevertheless, then the macroeconomic consequences via negative confidence effects are likely to be smaller.
Table 3.1: Summary statistics of confidence variables

(a) Monthly frequency, values

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(b) Annual frequency, end-of-the-year values

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Note: “st.dev” is “standard deviation”.
Table 3.2: Baseline regressions using end-of-period CCI

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Notes: Robust standard errors are reported in parentheses. Further, * = significance at the 10% level, ** = significance at the 5% level and *** = significance at the 1% level. The number of observations in Column (1) is one less than in the other columns, because one observation of CAPB is missing (specifically, Ireland 1979).
Table 3.3: Baseline regressions for subcomponents using end-of-period CCI

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Observations: 451 451 451 451
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Countries: 17 17 17 17
Country FE: YES YES YES YES
Year FE: YES YES YES YES

Notes: Robust standard errors are reported in parentheses. Further, * = significance at the 10% level, ** = significance at the 5% level and *** = significance at the 1% level.
Table 3.4: Baseline regressions using end-of-period BCI

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Notes: Robust standard errors are reported in parentheses. Further, * = significance at the 10% level, ** = significance at the 5% level and *** = significance at the 1% level.
Table 3.5: Average deviation consumer confidence from level at announcement

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<th>t=+1</th>
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<td>-0.041</td>
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<td>-.003</td>
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<td>0.033</td>
<td>0.012</td>
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<td>0.049</td>
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<td>.094**</td>
<td>.044***</td>
<td>-.043***</td>
<td>-.072*</td>
<td>-.08</td>
<td>-.199**</td>
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<td>-.261***</td>
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<td>-0.012</td>
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<td>.061**</td>
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<td>-.147***</td>
<td>-.188**</td>
<td>-.417***</td>
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</tr>
<tr>
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<td>0.076</td>
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<td>-.028</td>
<td>-.022</td>
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</tr>
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</tr>
<tr>
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</tr>
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Notes: The table reports the average deviation of the consumer confidence index in percent relative to the moment of the announcement of the consolidation. It does this for various moments around the announcement moment: “t = −6” denotes six months before, etc. The column under header “t = −6/+6” gives the total average percent movement over the entire event window from six months before to six months after the announcement. Robust standard errors are reported in parentheses. Further, * = significance at the 10% level, ** = significance at the 5% level and *** = significance at the 1% level. The column under “eq.” tests the difference in the total movement over the entire event window for the cases under consideration. It is always a chi-square test with two degrees of freedom. Finally, “rev” is “revenue”, “exp” is “expenditures”, “neg.” is negative, “pos.” is “positive”, “FR” is “fiscal rules” and “transp” is “transparency”.
Table 3.6: Average deviation business confidence from level at announcement

<table>
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<td>0.006</td>
<td>-0.03</td>
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Notes: See Notes to Table 1.5.
Table 3.7: Average deviation long-term interest rate from level at announcement

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<td>0.017</td>
<td>0.004</td>
<td>-0.058</td>
<td>-0.201</td>
<td>-0.325</td>
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</tr>
</tbody>
</table>

*Notes: See Notes to Table 1.5.*
Table 3.8: Average deviation stock price index from level at announcement

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<th>t=−1</th>
<th>t=+1</th>
<th>t=+3</th>
<th>t=+6</th>
<th>t=−6/+6</th>
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<tr>
<td>all</td>
<td>1.39</td>
<td>0.98</td>
<td>0.5</td>
<td>-0.74</td>
<td>-0.52</td>
<td>0.52</td>
<td>-0.87</td>
<td></td>
</tr>
<tr>
<td>exp</td>
<td>1.2</td>
<td>0.46</td>
<td>-0.04</td>
<td>-1.27</td>
<td>-0.46</td>
<td>2</td>
<td>0.8</td>
<td>0.89</td>
</tr>
<tr>
<td>rev</td>
<td>2.4</td>
<td>2.25</td>
<td>1.42**</td>
<td>-0.64</td>
<td>-0.78</td>
<td>-0.1</td>
<td>-2.5</td>
<td></td>
</tr>
<tr>
<td>high debt</td>
<td>-0.24</td>
<td>0.03</td>
<td>0.37</td>
<td>-0.82*</td>
<td>-0.16</td>
<td>0.95</td>
<td>1.19</td>
<td>2.37</td>
</tr>
<tr>
<td>low debt</td>
<td>3.93*</td>
<td>2.18*</td>
<td>0.79</td>
<td>0.04</td>
<td>-0.86</td>
<td>-0.15</td>
<td>-4.07</td>
<td></td>
</tr>
<tr>
<td>neg. gap</td>
<td>0.63</td>
<td>0.62</td>
<td>0.22</td>
<td>-0.15</td>
<td>0.76</td>
<td>2.85*</td>
<td>2.22</td>
<td>9.81***</td>
</tr>
<tr>
<td>pos. gap</td>
<td>2.85*</td>
<td>1.68</td>
<td>1.03**</td>
<td>-1.88**</td>
<td>-3.00**</td>
<td>-3.97**</td>
<td>-6.82***</td>
<td></td>
</tr>
<tr>
<td>Europe</td>
<td>2.41*</td>
<td>1.69*</td>
<td>0.74</td>
<td>-0.57</td>
<td>-0.79</td>
<td>0.5</td>
<td>-1.91</td>
<td></td>
</tr>
<tr>
<td>Europe, exp</td>
<td>1.73</td>
<td>1.05</td>
<td>0.25</td>
<td>-0.72</td>
<td>-0.38</td>
<td>3.03</td>
<td>1.29</td>
<td>2.89*</td>
</tr>
<tr>
<td>Europe, rev</td>
<td>4.43</td>
<td>3.66**</td>
<td>1.86**</td>
<td>-0.94</td>
<td>-1.69</td>
<td>-1.4</td>
<td>-5.83*</td>
<td></td>
</tr>
<tr>
<td>strong FR</td>
<td>1.5</td>
<td>1.03</td>
<td>0.64</td>
<td>0.06</td>
<td>-0.12</td>
<td>0.89</td>
<td>-0.61</td>
<td>0.51</td>
</tr>
<tr>
<td>weak FR</td>
<td>3.52</td>
<td>2.50*</td>
<td>0.87</td>
<td>-1.33*</td>
<td>-1.61</td>
<td>0.03</td>
<td>-3.5</td>
<td></td>
</tr>
<tr>
<td>high transp</td>
<td>3.66</td>
<td>2.12</td>
<td>0.9</td>
<td>-1.06</td>
<td>-1.15</td>
<td>0.23</td>
<td>-3.42</td>
<td>0.64</td>
</tr>
<tr>
<td>low transp</td>
<td>1.07</td>
<td>1.23</td>
<td>0.57</td>
<td>-0.04</td>
<td>-0.4</td>
<td>0.79</td>
<td>-0.28</td>
<td></td>
</tr>
<tr>
<td>strong FR, high transp</td>
<td>1.25</td>
<td>1.19</td>
<td>0.57</td>
<td>-0.57</td>
<td>-1.11</td>
<td>0.03</td>
<td>-1.21</td>
<td>0.01</td>
</tr>
<tr>
<td>weak FR, low transp</td>
<td>0.3</td>
<td>1.64</td>
<td>0.41</td>
<td>-0.94</td>
<td>-1.97</td>
<td>-0.42</td>
<td>-0.72</td>
<td></td>
</tr>
</tbody>
</table>

Notes: See Notes to Table 1.5.
Figure 3.1: Monthly frequency of fiscal announcements
Figure 3.2: Histograms of confidence indices, pooled across countries

![Consumer Confidence Index Histogram](image1)

![Business Confidence Index Histogram](image2)

Figure 3.3: Monthly distribution of changes in confidence

![Consumer Confidence Index Monthly Distribution](image3)

![Business Confidence Index Monthly Distribution](image4)

**Note**: Blue (red) dots (crosses) correspond to months with (without) announcements.
Figure 3.4: Fiscal announcement effect on consumer confidence, full sample of consolidations

Note: The figure depicts the average deviation in percent of consumer confidence relative to announcement date, plus an error band of ±1.645 standard deviations around the central line.

Figure 3.5: Spending and revenue-based consolidations

Note: See Note to Figure 4.
Appendix Chapter 3

3.A Fiscal consolidation in the euro zone during the crisis

The global economic and financial crisis, which erupted in September 2008 with the collapse of Lehman Brothers, was accompanied by a rapid deterioration of public finances in the euro zone and led to a “sovereign debt crisis” in some countries as of mid-2010. In response to the rapidly rising government deficit and debt-to-GDP ratios, in 2010 and 2011 most euro area governments announced ambitious multi-annual fiscal consolidations plans. The main motivation behind the approval of these adjustment plans was to address fiscal imbalances and market concerns regarding the sustainability of public finances. Therefore, as such, these adjustment plans could be considered to be exogenous with respect to the cycle, i.e., they were not implemented with a view of stabilizing cyclical fluctuations. Indeed, in most countries, fiscal consolidation packages were passed during a still depressed cyclical phase, with the output gap being in the negative territory for most countries during this period.

Figure 3.A.1 provides some summary statistics on the consolidation effort put in place by euro zone Member States between 2009 and 2013, together with the change in the overall government budget balance over the same period. The size of the fiscal consolidation effort is gauged in two ways. First, as typically done in the literature (see, e.g., Galí and Perotti, 2003), as the change in the government structural primary balance (SPB), i.e., the cyclically-adjusted government primary balance net of temporary and one-off measures. Secondly, in terms of cumulated discretionary fiscal measures approved by governments in the same period. The budgetary impact of these
measures has been recently evaluated by the European Commission based on a “bottom-up" or “narrative" approach (see European Commission, 2013). Under this approach, the discretionary fiscal effort is measured as the sum of the values that government authorities attributed to the measures in their budget at the time of adoption. Figure 3.A.1 suggests that the fiscal adjustment effort was very sizeable in many euro area countries, as reflected in both the improvement in the SPB (red bars) and the cumulated “narrative" discretionary measures (green bars) over the 2009-2013 period. For the euro area as a whole, the SPB improved by 3.3 percentage points of GDP, while the narrative discretionary measures indicate an improvement of 4.7 percentage points of GDP. The discretionary adjustment effort put in place in the euro zone countries generally led to an improvement in the overall government budget. For the aggregate euro area, the budget balance rose by 3.3 percentage points of GDP - i.e., from -6.3% of GDP to -3.0% of GDP - between 2009 and 2013. However, the developments in the overall balance were also driven by other factors. In particular, the economic cycle - through the operation of the automatic stabilisers - and one-off bank recapitalization operations played an important role. Therefore, for some countries, the sizeable fiscal consolidation effort was not fully reflected in an equivalent improvement in the headline deficit.  

An important dimension of the debate on the fiscal consolidation process in the euro zone was related to the composition of the fiscal adjustment. Indeed, the composition of the fiscal consolidation may have implications for the success of the consolidation process, in terms of both its sustainability and its macroeconomic effects (e.g. von Hagen and Strauch, 2001). In fact, past consolidation episodes often show that spending-based adjustments tend to cause milder and shorter contractions than revenue-based ones (Alesina et al., 2002, and Alesina and Ardagna, 2013): private investor confidence recovers faster if the consolidation relies more on expenditure cuts than on tax increases that tend to depress private investment and consumption. Against this background, Figure 3.A.2 sheds more light on the composition of the fiscal adjustment in the euro

11 In Slovenia, for instance, the government balance deteriorated from -4.0% of GDP in 2012 to -14.7% of GDP in 2013, but this was mainly the consequence of one-off sizeable bank recapitalization costs (of about 10% of GDP).
zone during the crisis. In particular, it shows the contribution of structural revenue and primary expenditure to the change in the SPB over the 2009-2013 period. It emerges that, for the euro area as a whole, the adjustment was rather balanced, although somewhat more tilted toward the revenue side: increases in structural revenues contributed by 1.8 percentage points of GDP of the total 3.3 percentage points of GDP improvement in the SPB, whereas cuts in expenditure contributed by 1.5 percentage points of GDP. Interestingly, for the most ‘vulnerable’ countries, which are also the ones that implemented the most sizeable consolidation effort, the adjustment was mostly expenditure-based (e.g., Greece, Spain, Portugal, Ireland, Cyprus), whereas for countries with a fiscal consolidation effort smaller than the euro zone average, the adjustment was predominantly revenue-based (e.g., France, Slovenia, Netherlands, Belgium, Austria). Hence, the design of the euro zone fiscal adjustments only partially reflected the principles put forward by the advocates of expenditure-based consolidations.
Figure 3.A.1: Cumulated discretionary measures over the period 2009-2013 in the euro area and changes in the structural primary and actual balances

Figure 3.A.2: Contribution of structural revenues and structural primary expenditure to the change in the structural primary balance over the period 2009-2013
3.B Additional results for the baseline regression

In Table 3.B.1, Column (1) repeats the estimates in Column (3) of Table 3.2, while in Column (2) the ex-post real interest rate ($RINT$) replaces in the same regression the nominal interest and inflation as independent variables. In Column (3) we limit the sample to those observations for which business confidence is also available. As the effect of consolidation on consumer confidence becomes even stronger, the negative effect of consolidations on confidence is not driven by the sub-sample of observations for which only consumer confidence is available.

Table 3.B.1: Additional variations and extensions on baseline in Table 3.2

<table>
<thead>
<tr>
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<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
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</thead>
<tbody>
<tr>
<td>$CCI(-1)$</td>
<td>0.495***</td>
<td>0.492***</td>
<td>0.392***</td>
<td>0.496***</td>
</tr>
<tr>
<td></td>
<td>(0.055)</td>
<td>(0.053)</td>
<td>(0.068)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>$CS$</td>
<td>-0.120*</td>
<td>-0.120**</td>
<td>-0.169**</td>
<td>-0.137**</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.056)</td>
<td>(0.067)</td>
<td>(0.063)</td>
</tr>
<tr>
<td>$CS^{RR}$</td>
<td></td>
<td></td>
<td>-0.03</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.027)</td>
<td></td>
</tr>
<tr>
<td>$GROWTH$</td>
<td>0.167***</td>
<td>0.168***</td>
<td>0.222***</td>
<td>0.168***</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.056)</td>
<td>(0.047)</td>
<td>(0.058)</td>
</tr>
<tr>
<td>$\Delta u$</td>
<td>-0.05</td>
<td>-0.052</td>
<td>-0.059</td>
<td>-0.038</td>
</tr>
<tr>
<td></td>
<td>(0.069)</td>
<td>(0.07)</td>
<td>(0.065)</td>
<td>(0.071)</td>
</tr>
<tr>
<td>$INFL$</td>
<td>-0.070*</td>
<td>-0.065</td>
<td>-0.067</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.047)</td>
<td>(0.042)</td>
<td></td>
</tr>
<tr>
<td>$INT$</td>
<td>0.054</td>
<td>0.053</td>
<td>0.054</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.053)</td>
<td>(0.062)</td>
<td>(0.055)</td>
<td></td>
</tr>
<tr>
<td>$RINT$</td>
<td></td>
<td></td>
<td>0.067</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.038)</td>
<td></td>
</tr>
<tr>
<td>$\Delta STOCK$</td>
<td>0.021***</td>
<td>0.021***</td>
<td>0.018***</td>
<td>0.021***</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.003)</td>
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<tr>
<td>Obs.</td>
<td>451</td>
<td>451</td>
<td>386</td>
<td>451</td>
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<tr>
<td>R-squared</td>
<td>0.707</td>
<td>0.707</td>
<td>0.711</td>
<td>0.708</td>
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<td>Countries</td>
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<td>16</td>
<td>17</td>
</tr>
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<td>Country FE</td>
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<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Year FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
</tbody>
</table>

Notes: Robust standard errors are reported in parentheses. Further, * = significance at the 10% level, ** = significance at the 5% level and *** = significance at the 1% level.

In Column (4) we explore whether there are confidence spill-overs from consolidations elsewhere. To this end, we define $CS^{RR}$ as the average value of $CS$ for the rest of the region (Europe if the country is European or non-Europe if the country is not European). We see that $CS^{RR}$ exerts a negative effect on confidence, but it is not significant.
3.C Coefficient equality tests

Table 3.C.1 reports the results of tests of the equality of coefficients in the annual regressions. The entries of the tables refer to the tables in the main paper. For example, “T2, C(4)” refers to Column (4) of Table 3.2.

Table 3.C.1: Testing coefficient equality for consolidation measures

<table>
<thead>
<tr>
<th>Restriction</th>
<th>Consumer confidence</th>
<th>Business confidence</th>
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<tbody>
<tr>
<td></td>
<td>T2, C(4)</td>
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</tr>
<tr>
<td></td>
<td>T2, C(5)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>T2, C(6)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>T4, C(4)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>T4, C(5)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>T4, C(6)</td>
<td></td>
</tr>
<tr>
<td>$CS^A = CS^U$</td>
<td>F(1,16)=0.18 p=0.678</td>
<td></td>
</tr>
<tr>
<td></td>
<td>p=0.510</td>
<td>F(1,15)=2.63 p=0.125</td>
</tr>
<tr>
<td></td>
<td>p=0.337</td>
<td></td>
</tr>
<tr>
<td>$CS^A = CS^U = CS^P$</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>F(2,16)=1.17</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>p=0.158</td>
<td></td>
</tr>
<tr>
<td>$CS^U = CS^P$</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>F(1,16)=1.97</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>p=0.179</td>
<td></td>
</tr>
<tr>
<td>$CS^A = CS^U + CS^P$</td>
<td>-</td>
<td>F(1,16)=0.40 p=0.534</td>
</tr>
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</tbody>
</table>

Table 3.C.2: Testing coefficient equality for of expenditure- and revenue-based consolidations

<table>
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<tr>
<th>Restriction</th>
<th>T3, C(1)</th>
<th>T3, C(2)</th>
<th>T3, C(3)</th>
<th>T3, C(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$REV = EXP$</td>
<td>F(1,16)=2.61 p=0.126</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>$REV^A = REV^U$</td>
<td>-</td>
<td>F(1,16)=1.12 p=0.304</td>
<td>F(1,16)=1.69 p=0.212</td>
<td>-</td>
</tr>
<tr>
<td>$EXP^A = EXP^U$</td>
<td>-</td>
<td>F(1,16)=0.14 p=0.709</td>
<td>F(1,16)=0.14 p=0.711</td>
<td>-</td>
</tr>
<tr>
<td>$EXP^A = REV^A$</td>
<td>-</td>
<td>F(1,16)=0.08 p=0.779</td>
<td>F(1,16)=0.13 p=0.719</td>
<td>F(1,16)=0.02 p=0.879</td>
</tr>
<tr>
<td>$EXP^U = REV^U$</td>
<td>-</td>
<td>F(1,16)=3.07 p=0.099</td>
<td>F(1,16)=3.41 p=0.083</td>
<td>-</td>
</tr>
<tr>
<td>$REV^A = EXP^P$</td>
<td>-</td>
<td>-</td>
<td>F(1,16)=1.99 p=0.177</td>
<td>-</td>
</tr>
<tr>
<td>$EXP^A = EXP^P$</td>
<td>-</td>
<td>-</td>
<td>F(1,16)=0.77 p=0.392</td>
<td>-</td>
</tr>
<tr>
<td>$REV^P = EXP^P$</td>
<td>-</td>
<td>-</td>
<td>F(1,16)=0.00 p=0.953</td>
<td>-</td>
</tr>
<tr>
<td>$EXP^A = EXP^U + EXP^P$</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>F(1,16)=0.03 p=0.865</td>
</tr>
<tr>
<td>$REV^A = REV^U + REV^P$</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>F(1,16)=1.51 p=0.237</td>
</tr>
<tr>
<td>$REV^U + REV^P = EXP^U + EXP^P$</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>F(1,16)=2.02 p=0.174</td>
</tr>
</tbody>
</table>
3.D Can confidence predict consolidations?

Table 3.D.1 below shows panel probit regressions for the prediction of a consolidation at the annual frequency. Standard errors are normally distributed and the variance-covariance matrix accounts for heteroskedasticity across countries. The variables are defined as follows. $ICS$ is an indicator that takes a value of 1 if a consolidation takes place in the current period and a value of 0 if not, $DEBT$ is the public debt-GDP ratio in percent, $\Delta y$ is per-capita real GDP growth in percent, $INFL$ is inflation in percent, $INT$ is the long-term interest rate in percent, $\Delta CCI(-1) = CCI(-1) - CCI(-2)$ is the (log) difference between consumer confidence at the end of years $(t-1)$ and $(t-2)$ times 100, $\Delta u(-1)$ is the difference in the unemployment rate in percentage points between years $(t-1)$ and $(t-2)$, $\Delta STOCK(-1)$ is the (log) change in the stock price index multiplied by 100, and $GR_{OECD}$ is GDP growth in the OECD in percent.

Finally, Table 3.D.2 shows the panel probit regressions for the prediction of consolidations at the monthly frequency. The set of explanatory variables is now more limited, because not all regressors are available at the monthly frequency. For the specifications in Columns (3) - (8) we use either averages or, in the case of confidence, the change over the past quarter, half year or full year. We denote by “(-3,-1)” the average over the past quarter, “(-6,-1)” the average over the half year and by “(-12,-1)” the average over the past year.
Table 3.D.1: Annual panel probit regressions

<table>
<thead>
<tr>
<th>VAR</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
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<tr>
<td></td>
<td>ICS</td>
<td>ICS</td>
<td>ICS</td>
<td>ICS</td>
<td>ICS</td>
<td>ICS</td>
<td>ICS</td>
<td>ICS</td>
<td>ICS</td>
</tr>
<tr>
<td>DEBT(-1)</td>
<td>0.036***</td>
<td>0.023***</td>
<td>0.005***</td>
<td>0.046***</td>
<td>0.037***</td>
<td>0.004*</td>
<td>0.029***</td>
<td>0.022***</td>
<td>0.008***</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.004)</td>
<td>(0.002)</td>
<td>(0.015)</td>
<td>(0.012)</td>
<td>(0.002)</td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>GROWTH(-1)</td>
<td>-0.105</td>
<td>-0.049</td>
<td>-0.043</td>
<td>-0.082</td>
<td>-0.07</td>
<td>-0.062</td>
<td>-0.143</td>
<td>-0.11</td>
<td>-0.087</td>
</tr>
<tr>
<td></td>
<td>0.135</td>
<td>0.13</td>
<td>0.101</td>
<td>0.12</td>
<td>0.118</td>
<td>0.074</td>
<td>0.147</td>
<td>0.139</td>
<td>0.111</td>
</tr>
<tr>
<td>INFL(-1)</td>
<td>-0.142</td>
<td>-0.136*</td>
<td>-0.116*</td>
<td>-0.162**</td>
<td>-0.147**</td>
<td>-0.112*</td>
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Notes: Robust standard errors reported in parentheses. Further, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. 69
3.E Results of panel vector auto-regressions (PVAR)

Figure D.1 shows the impulse response functions (to a consolidation shock) in an annual panel vector auto-regression (PVAR) with $[CS_j', CCI, GROWTH, INFL, ΔSTOCK, INT]'$, for $j = A, U$ and $P$. We include country- and year-fixed effects and set the lag length to two.

**Figure 3.E.1: Responses to a CS$^A$ shock**

*Notes: confidence intervals are constructed on Monte Carlo simulations based on 1000 replications. The red dotted lines correspond to two standard deviations around the mean impulse responses.*
Figure 3.E.2: Responses to a $CS^U$ shock

*Notes:* See notes to Figure 3.E.1.
Figure 3.E.3: Responses to a $CS^P$ shock

*Notes:* See notes to Figure 3.E.1.
3.F Business cycle confidence: split into expenditures and revenues

Analogous to Table 3.3 in the main text, we explore the effect of spending versus revenues-based consolidations on business confidence.

Table 3.F.1: Baseline regressions for subcomponents using end-of-period BCI

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<td>EXP&lt;sup&gt;P&lt;/sup&gt; + EXP&lt;sup&gt;U&lt;/sup&gt;</td>
<td></td>
<td>-0.095</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.082)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Observations   379  379  379  379
R-squared       0.722  0.723  0.729  0.725
Countries       16  16  16  16
Country FE      YES YES YES YES
Year FE         YES YES YES YES

Notes: Robust standard errors reported in parentheses. Further, *** p < 0.01, ** p < 0.05, * p < 0.1.
Table 3.F.2: Effect subcomponents of consolidations on business confidence

<table>
<thead>
<tr>
<th>Restriction</th>
<th>C(1)</th>
<th>C(2)</th>
<th>C(3)</th>
<th>C(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>( REV = EXP )</td>
<td>( F(1,15)=0.04 )</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>( p=0.835 )</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( REV^A = REV^U )</td>
<td>-</td>
<td>( F(1,15)=0.57 )</td>
<td>( F(1,15)=1.28 )</td>
<td>-</td>
</tr>
<tr>
<td>( p=0.462 )</td>
<td>( p=0.275 )</td>
<td>( p=0.136 )</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( EXP^A = EXP^U )</td>
<td>-</td>
<td>( F(1,15)=3.79 )</td>
<td>( F(1,15)=2.47 )</td>
<td>-</td>
</tr>
<tr>
<td>( p=0.070 )</td>
<td>( p=0.136 )</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( EXP^A = REV^A )</td>
<td>-</td>
<td>( F(1,15)=1.11 )</td>
<td>( F(1,15)=0.79 )</td>
<td>( F(1,15)=2.12 )</td>
</tr>
<tr>
<td>( p=0.309 )</td>
<td>( p=0.387 )</td>
<td>( p=0.136 )</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( EXP^U = REV^U )</td>
<td>-</td>
<td>( F(1,15)=0.08 )</td>
<td>( F(1,15)=0.33 )</td>
<td>-</td>
</tr>
<tr>
<td>( p=0.785 )</td>
<td>( p=0.573 )</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( REV^A = REV^P )</td>
<td>-</td>
<td>-</td>
<td>( F(1,15)=1.40 )</td>
<td>-</td>
</tr>
<tr>
<td>( p=0.255 )</td>
<td>( p=0.021 )</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( EXP^A = EXP^P )</td>
<td>-</td>
<td>-</td>
<td>( F(1,15)=6.57 )</td>
<td>-</td>
</tr>
<tr>
<td>( p=0.021 )</td>
<td>( p=0.230 )</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( REV^P = EXP^P )</td>
<td>-</td>
<td>-</td>
<td>( F(1,15)=1.56 )</td>
<td>-</td>
</tr>
<tr>
<td>( p=0.230 )</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( EXP^A = EXP^U + EXP^P )</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>( F(1,15)=5.89 )</td>
</tr>
<tr>
<td>( p=0.028 )</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( REV^A = REV^U + REV^P )</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>( F(1,15)=1.01 )</td>
</tr>
<tr>
<td>( p=0.331 )</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( REV^U + REV^P = EXP^U + EXP^P )</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>( F(1,15)=0.36 )</td>
</tr>
<tr>
<td>( p=0.557 )</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: Robust standard errors reported in parentheses. Further, *** \( p < 0.01 \), ** \( p < 0.05 \), * \( p < 0.1 \).
3.G  Timing experiments

3.G.1  Mistiming

Below we show three graphs. The first chart reports the baseline result of the consolidation announcements on consumer confidence (Figure 6 of the paper). The second shows the confidence effects as if the announcement is done one month after the actual announcement month. Finally, the third chart shows the confidence effect as if the announcement is done one month before the actual announcement month.
3.G.2 Excluding the month with the most observations for each country

Below we report the graph showing the confidence effects of consolidation announcements when the original dataset is reduced by removing, for each country, the month with the most consolidation announcements.
### 3.H Results panel distributed-lag model

Table 3.H.1 shows the results of a panel distributed lag model of the log-difference in consumer confidence, where on the right-hand side we include lags and leads of the values of the announcements. In the first variant we include country-fixed effects and country specific linear trends. In the second variant we also include month-fixed effects.

<table>
<thead>
<tr>
<th>Dummy for announcement, country FE and country-specific trends</th>
<th>t=-6</th>
<th>t=-3</th>
<th>t=-1</th>
<th>t=+1</th>
<th>t=+3</th>
<th>t=+6</th>
<th>t=-6/+6</th>
</tr>
</thead>
<tbody>
<tr>
<td>ann</td>
<td>-0.029</td>
<td>0.017</td>
<td>0.03**</td>
<td>-0.039***</td>
<td>-0.083</td>
<td>-0.092</td>
<td>-0.063</td>
</tr>
<tr>
<td>revenue</td>
<td>0.122</td>
<td>0.141*</td>
<td>0.088***</td>
<td>-0.089***</td>
<td>-0.133***</td>
<td>-0.214*</td>
<td>-0.337**</td>
</tr>
<tr>
<td>spend</td>
<td>-0.159</td>
<td>-0.062</td>
<td>-0.002</td>
<td>-0.005</td>
<td>-0.012</td>
<td>-0.007</td>
<td>0.153</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Dummy for announcement, country FE, country-specific trends and month TE</th>
<th>t=-6</th>
<th>t=-3</th>
<th>t=-1</th>
<th>t=+1</th>
<th>t=+3</th>
<th>t=+6</th>
<th>t=-6/+6</th>
</tr>
</thead>
<tbody>
<tr>
<td>ann</td>
<td>0.08</td>
<td>0.06*</td>
<td>0.041***</td>
<td>-0.047***</td>
<td>-0.1**</td>
<td>-0.114*</td>
<td>-0.194**</td>
</tr>
<tr>
<td>revenue</td>
<td>0.198</td>
<td>0.18***</td>
<td>0.105***</td>
<td>-0.099***</td>
<td>-0.145***</td>
<td>-0.214***</td>
<td>-0.412***</td>
</tr>
<tr>
<td>spend</td>
<td>-0.006</td>
<td>-0.003</td>
<td>0.012</td>
<td>-0.019</td>
<td>-0.036</td>
<td>-0.044</td>
<td>-0.038</td>
</tr>
</tbody>
</table>

**Notes:** In all cases the significance level is determined by the p-value of the Wald test with the hypothesis that the sum of the coefficients for that particular window equals zero.
3.I Plots of annual implemented consolidations

Implemented fiscal consolidations, Australia

Implemented fiscal consolidations, Austria

Implemented fiscal consolidations, Belgium

Implemented fiscal consolidations, Canada

Implemented fiscal consolidations, Denmark

Implemented fiscal consolidations, Finland
Chapter 4

Revenue- versus Spending-Based
Consolidation Plans: The Role of Follow-Up

4.1 Introduction

What are the macroeconomic effects of fiscal consolidation plans? Do revenue-based consolidations affect the economy in a different way than spending-based consolidations and, if so, why? Over the recent years a number of papers (Alesina et al., 2015a; Alesina et al., 2015b and Guajardo et al., 2014) have tried to address these questions starting from the annual narrative dataset constructed by Pescatori et al. (2011). A robust result in the literature based on this dataset is that revenues-based consolidations are more harmful for output than expenditure-based consolidations. Different explanations have been put forward to explain this finding. The explanation by Guajardo et al. (2014) is based on monetary policy being more accommodative in the case of spending-based consolidations. Alesina et al. (2015a,b) propose an explanation based on the positive effect of spending-based consolidation on business confidence and private investment. More recently, Alesina et al. (2017) confirm the heterogeneous effects of spending- versus revenues-based consolidation plans, while controlling for monetary policy. In particular, based on a richer version of the narrative data of Pescatori 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.\footnote{Their narrative dataset consists of permanent measures with a planning horizon roughly equal across revenues- and spending-based adjustments; governments usually front-load cuts in spending and implement revenues hikes by means of more gradual adjustments.} The impact of persistent spending cuts is mitigated by wealth effects on aggregate demand.\footnote{Their narrative dataset consists of permanent measures with a planning horizon roughly equal across revenue- and spending-based adjustments; governments usually front-load cuts in spending and implement revenues hikes by means of more gradual adjustments.}

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

In a first step we provide indicative evidence of differences in follow-up by directly comparing ex-post actual data from the OECD with the annual narratively-identified real-time consolidation measures of Pescatori et al. (2011) and Alesina et al. (2015a,b). We do this by very carefully matching the narrative measures with the appropriate variables for the ex-post outcomes. There is a systematic shortfall of the latter relative to the narrative measures. However, we also find that the shortfall is systematically larger for spending-based measures. We offer two (potentially complementary) explanations for the weaker follow-up of spending plans. The first is what we refer to as “passive” non-follow-up and results from over-optimistic output growth forecasts. For standard estimates of elasticities and over-optimism in growth forecasts, this can explain one-third to almost one-half of the difference in follow-up. The second is what we refer to as “active” non-follow-up. It is the result of partially implementing planned consolidation measures. We can rationalize the

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

2Their narrative dataset consists of permanent measures with a planning horizon roughly equal across revenue- and spending-based adjustments; governments usually front-load cuts in spending and implement revenues hikes by means of more gradual adjustments.
lower degree of active follow-up exhibited by spending plans in a simple setting where the political resistance to consolidation plans is uncertain, but more likely to be prohibitive for spending than for revenue plans when it comes to actual implementation. Data on general strikes in Western Europe do indeed suggest that announcements of spending cuts are more frequently followed by socio-political unrest than announcements of revenue increases.

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

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

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

The remainder of this paper is structured as follows. Section 4.2 provides a brief review of the relevant literature. Section 4.3 investigates the follow-up of the annual fiscal consolidation plans by direct comparison with ex-post data on revenues and spending, and discusses the passive and active follow present. Section 4.5 presents the results of our panel VAR analysis. Finally, Section 4.6 concludes. Chapter 2 and Appendix 4.A provide further information on the construction of the consolidation announcement data and the macroeconomic data. Appendix 4.B presents a simple framework rationalizing the “active” non-follow-up with some indirect evidence supporting it. Appendix 4.C reports the figures of our robustness tests.
4.2 Literature Review

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

The Great Recession has motivated a large body of work estimating the sign and magnitude of fiscal multipliers. Empirical evidence generally shows that positive shocks to revenues are contractionary (Blanchard and Perotti, 2002; Romer and Romer, 2010; Barro and Redlick, 2011; Favero and Giavazzi, 2012), with output multipliers ranging between -0.5 and -5. Reductions in public wage expenditures lower disposable income directly, while reductions in non-wage public spending on goods and services lower disposable income by depressing the demand for private sector output and, hence, income generated in the private sector. These results are confirmed for narratively-identified consolidation measures: for a panel of OECD countries Guajardo et al. (2014) find that both the revenues and the expenditure measures are associated with reductions in private consumption and GDP. However, there is evidence (e.g. Guajardo et al., 2014, and Alesina et al., 2015a,b) that spending-based consolidations are more effective in reducing the public debt and economically less harmful than revenue-based consolidations. The literature offers several arguments why this may be the case. One argument, advanced by Guajardo et al. (2014) for example, is that monetary policy tends to be more accommodative in the case of spending-based consolidations. A second argument is that, because they are politically more costly, resorting to spending-based consolidation provides a stronger signal by the government to the private sector that it intends to improve its financial situation (Ardagna, 2004).³ 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.

³The argument is related to Cukierman and Tommasi (1998) who argue that political decisions that are at odds with the preferences of the natural constituency of a party are most credible.
The second line of literature closely connected to this paper consists of empirical studies that document sizable and systematic deviations of actual implementation from fiscal plans. Examples for EU or Eurozone countries are Beetsma et al. (2009), von Hagen (2010), Pina and Venes (2011), Cimadomo (2012), Beetsma et al. (2013) and De Castro et al. (2013). Using data from the EU Stability and Convergence Programs, Beetsma et al. (2009) show that actual budgetary adjustment falls systematically short of planned adjustment, and that the shortfall increases with the projection horizon. Related analysis by von Hagen (2010) indicates that the form of fiscal governance and the tightness of fiscal rules can explain these shortfalls. Pina and Venes (2011) employ EU Excessive Deficit Procedure reporting data to conclude that budget balance forecasting errors are responsive to fiscal institutions and opportunistic political motivations. A related conclusion is reached by Beetsma et al. (2013), who distinguish between systematic shortfalls in the implementation during the first year since the presentation of the budget and potential further revision errors. They find that institutional quality - as measured by the tightness of national fiscal rules, the medium-term budgetary framework or budgetary transparency - improves budgetary reporting at both the planning stage and one year later. De Castro et al. (2013) go even further and carefully explore how data revisions gradually develop as the time distance to the original fiscal plan increases. In line with the literature, they find that preliminary deficit data releases are biased, with later data vintages exhibiting larger deficits. Countries try to systematically exploit the margins of acceptable reporting, but are subsequently corrected by Eurostat. Frankel and Schreger (2013) find that over-optimism in forecasting budgetary improvement is particularly strong when the deficit exceeds the 3% GDP limit at the moment that the forecast is constructed. However, the over-optimism is 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.

\footnote{Cimadomo (2012) shows that OECD countries often plan a counter-cyclical fiscal stance, while fiscal outcomes tend to point towards a-cyclical or pro-cyclicality.}
The third strand of relevant literature is the growing body of work that explores the role of news for short-term economic dynamics. Here, the crucial assumption is that short-run output fluctuations can be driven by changes in the information set of agents. New information about future (economic) developments affects the expectations of private sector agents, who start to adjust their behavior in anticipation of the future state of the economy (Beaudry and Portier, 2014).

Expectations of fiscal consolidation may either moderate or exacerbate the contractionary effect of the actual measures on the real economy. On the one hand, adherents of the “expansionary austerity” view claim that positive expectations effects can mitigate the contractionary effects of fiscal consolidations: if private agents realize that current fiscal consolidation prevents a future increase in taxation, the adjustment spurs optimism about the future path of public expenditure and tax burdens (Blanchard, 1990b, Giavazzi and Pagano, 1990, Alesina and Ardagna, 2010). On the other hand, Akerlof and Shiller (2009) posit the existence of a “confidence multiplier”, which may amplify the Keynesian effects of fiscal policy. This hypothesis is investigated in a recent study by Bachmann and Sims (2012), who find that during recessions in the United States the “confidence multiplier” reinforces the Keynesian effects of increases in government spending. Additionally, Ramey (2011) and Mertens and Ravn (2012) show that anticipation effects can play an important role in the identification of structural fiscal shocks and that the incorporation of narrative shocks in the empirical methodology produces different results from standard techniques. Our dataset of fiscal consolidation announcements is particularly suited to addressing such expectation effects. From a methodological viewpoint, our work is a study on the link between news and short-term economic dynamics that uses explicitly identified shocks (such as, for instance Brückner and Pappa, 2015). Thus, our work falls within the empirical literature on narratively identified fiscal VAR models where our external instrument consists of announcements of future fiscal austerity measures.
4.3 *Ex-Post* Deviations of Real-Time Fiscal Consolidation Measures

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

4.3.1 Matching of *ex-post* data with the narrative consolidation data

The annual fiscal consolidation measures in the dataset of Pescatori et al. (2011) are narratively selected from policy documents such that their primary motivation is public finance sustainability and not a response to the business cycle. The identified measures together with their estimated budgetary impact reflect the “intentions and actions” of policymakers as described in the policy documents.\(^5\) 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 Pescatori et al. (2011). An important source of information used in particular by Alesina et al. (2015a,b) are the Stability and Convergence Programmes submitted by EU member states; these documents contain both the forecast effects of the fiscal plans, as well as real-time estimates of the impact of the measures taken in the current or the preceding years. Therefore, in both the narrative dataset of Pescatori et al. (2011) and in its extension, the observed magnitude of a fiscal consolidation represents a mixture of forecast and first-release data.

\(^5\)The recorded budgetary impact is the estimated change in budgetary savings accounted for by all the measures implemented in a given year.
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 Pescatori 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 data obtained from the OECD on actual revenues and spending. The narratively-identified revenue measures include the following items found in the OECD data: “direct taxes”, “indirect taxes”, “social security contributions received by government”, “other current receipts by government” and “capital tax and transfers receipts”. This is more narrow than (a subset of) our most comprehensive measure from the OECD “Total receipts, government”. The spending measures narratively identified by the IMF include the following series from the OECD data: “Government final consumption expenditure, appropriation account”, “Government fixed capital formation, appropriation account”, “Social security benefits paid by the government”, “Capital transfers paid and other capital payments” and “Other current outlays, government”. This is more narrow than our most comprehensive measure from the OECD “Total disbursements, government”. To demonstrate the robustness of our findings, we will compare the real-time narratively-identified consolidation measures with the changes in both the most comprehensive and narrower actual series. Appendix 4.A contains a full description of the annual budgetary data used in this section.

### 4.3.2 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:

\[
\left( \frac{X^f_t}{Y^f_t} - \frac{X^f_{t-1}}{Y^f_{t-1}} \right) - \left( \frac{X^h_t}{Y^h_t} - \frac{X^h_{t-1}}{Y^h_{t-1}} \right), \quad \text{for } X = T, G \tag{4.1}
\]

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

$$
\begin{bmatrix}
X_t^f & X_{t-1}^f
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- \left(\begin{bmatrix}
X_t^h & X_{t-1}^h
\end{bmatrix}^{CA} - \left(\begin{bmatrix}
X_t^h & X_{t-1}^h
\end{bmatrix}^{CA}
- \left(\begin{bmatrix}
X_t^h & X_{t-1}^h
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X_t^h & X_{t-1}^h
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“Total revenues (narrow definition)”, the average short-falls are slightly larger (up to a maximum of roughly 0.18% of GDP). By contrast, the average shortfalls for spending are substantially larger than for revenues. For the most comprehensive measure, “Total disbursements, excluding gross interests payments, government”, the average figure is 0.50% of GDP. For the other measures, i.e. “Current disbursements, excluding gross interests payments, government”, its cyclically-adjusted version, and for “Total expenditure (narrow definition)”, the average deviations are even larger. While the average size of the spending measures (0.85% of GDP) exceeds the average side of the revenues measures (0.51% of GDP), we find that the average shortfalls for spending are proportionally substantially larger than justified by the average size of the spending measures relative to revenues measures. Based on the latter, the average size of the spending shortfall should be 0.25% of GDP, half of the most favorable actual number.

Looking at the individual countries, where we average over the consolidation years, we observe that for the most comprehensive measure of revenues, 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 revenues and spending measures we register a higher fractions of shortfalls, but the spending measure is always characterized by weaker follow-up than the corresponding revenues measure.

4.3.4 Explanations for differences in follow-up

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

“Passive” non follow-up

A first explanation are 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 enve-
lope calculation, we show that systematically over-optimistic GDP growth forecasts account for a substantial fraction of the observed difference in follow-up between revenues and spending consolidation plans. The starting point is equation (4.1). 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 \( Z \), \( Z_{t-1}^{t} = Z_{t-1}^{f} \) which implies that now-cast estimates (i.e. estimates done for the current year) are equal to ex-post measures. Because forecast inaccuracy increases with the horizon, the back-of-the-envelope numbers for the shortfalls that we calculate likely form a lower bound. Under these assumptions, the difference between the ex-post and planned change (4.1) reduces to 

\[
\left( \frac{X_{t}^{f}}{Y_{t}^{f}} - \frac{X_{t-1}^{f}}{Y_{t-1}^{f}} \right) \approx \left( \frac{x_{t}^{f} - x_{t-1}^{f}}{1 + x_{t}^{f}} \right) \left( \frac{X_{t}^{f}}{Y_{t}^{f}} \right) - \left( \frac{y_{t}^{f} - y_{t-1}^{f}}{1 + y_{t}^{f}} \right) \left( \frac{X_{t-1}^{f}}{Y_{t-1}^{f}} \right) \]  

(4.3)

Here \( x_{t}^{f} \) is the planned growth in period \( t - 1 \) of nominal revenues (if \( X = T \)) or nominal expenditure (if \( X = G \)) for period \( t \). Further, \( x_{t}^{f} \) is the corresponding ex-post growth rate over the same period. Finally, \( y_{t}^{f} \) is the projected nominal income growth rate in period \( t - 1 \) for period \( t \) and \( y_{t}^{f} \) is the period \( t \) nominal income growth rate as measured ex post. Assuming that the elasticities \( \varepsilon_T \) and \( \varepsilon_G \) of revenues, respectively expenditures with respect to output are constant, we have \( x_{t}^{f} = \varepsilon_X Y_{t}^{f} \) and \( x_{t-1}^{f} = \varepsilon_X Y_{t-1}^{f} \), with \( X = T, G \).

Frankel (2011) finds that the average output growth bias for EU countries is around 0.5%. Using the information in Table A.3 of Mourre et al. (2014), we are able to compute the average revenue and expenditure elasticities with respect to output of the thirteen EU countries in our sample.

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6 There is also a so-called “base effect” that is zero under our assumptions and a residual effect that we ignore because it is of second-order importance - see Beetsma et al. (2013) for a discussion.
The resulting elasticities are $\varepsilon_T = 1.11$ and $\varepsilon_G = -0.16$. Finally, based on the ex-post measures available in the OECD Economic Outlook (November 2015), we know that the ratios of total revenues (narrow definition) and total expenditure (narrow definition) over GDP are $T_i^f/Y_i^f = 0.39$ and $G_i^f/Y_i^f = 0.39$, respectively. On the basis of this calibration and assuming an average ex-post nominal GDP growth $y_i^f$ of 4.5% and (for consistency) an average nominal GDP growth forecast $y_{i-1}^f$ 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, 
\[
\left\{ \frac{0.39}{((1 + 0.045)(1 + 0.050))} \right\}[1.11(-0.5)] \approx -0.20 \text{ (growth effect)} - \frac{0.39}{((1+0.045)(1+0.050))}(-0.5) \approx -0.18 \text{ (denominator effect)},
\]

hence $-0.02$ percent of GDP. In other words, the ex-post revenue ratio of GDP is on average $0.02$ percent lower than planned. In the case of expenditure the shortfall is, in percent of GDP, 
\[
\left\{ \frac{0.39}{((1 + 0.045)(1 + 0.050))} \right\}(-0.16)(-0.5) \approx 0.03 \text{ (growth effect)} - \frac{0.39}{((1+0.045)(1+0.050))}(-0.5) \approx -0.18 \text{ (denominator effect)},
\]

hence $0.21$ percent of GDP. In other words, the ex-post spending ratio of GDP is on average $0.21$ percent of GDP higher than planned. The above back-of-the-envelope calculations show that biases in the GDP growth forecasts lead to systematically larger shortfalls from plans for spending reductions than for revenues increases, which can explain a non-negligible fraction of the empirically-observed average difference between the shortfalls.

"Active" non follow-up

Our second explanation concerns the "active non-follow-up", which refers to the possibility that announced consolidation measures only partially carried out. Appendix 4.B rationalizes this phenomenon in a very simple two-stage framework, in which in the first stage the government sets up a consolidation plan that is communicated to the private sector and in the second stage decides to what extent to actually carry out the plan. At the moment the consolidation plan is designed, the political costs of the measures are unclear, while closer to the actual implementation, there is a higher chance for spending reductions to be politically prohibitive than for revenues increases.
Under this assumption one can demonstrate that the average deviation of actual from planned consolidation measures is larger for spending than for revenues. This prediction is thus consistent with the empirical accounting evidence provided earlier in this section.

Appendix 4.B provides indirect evidence for this assumption. In particular, using data from Hamann et al. (2013, 2016) for fifteen countries from the European Union plus Norway, we show that strikes associated with public spending cuts occur much more frequently than political unrest associated with revenue increases. To the extent that strikes form a proxy for the socio-political unrest created by the austerity measures, this provides indirect support for the mechanism laid out above. We find that disputes motivated by spending cuts occur with a substantially higher frequency than those motivated by revenue increases: of the 159 disputes, 69 are spending-cut motivated (43 after excluding those in which the issue in dispute is pensions), while the number of strikes motivated by revenue increases is only 7. Excluding countries not present in our sample of consolidation plans, i.e. excluding Greece, Luxembourg and Norway, 85 strike episodes remain, of which 40 are motivated by spending cuts (23 upon exclusion of the pension-related disputes), 6 are motivated by revenue increases and 8 are motivated by both revenue increases and spending cuts.

Next, based on the narrative description of both the strike and the consolidation, we are able to match twenty strikes to the consolidation plans in our dataset (one in Finland, one in France, one in Spain, one in the Netherlands, two in Portugal, four in Belgium and ten in Italy). Of these strikes, three were undertaken in response to revenue-based announcements (namely in Italy in September 2011, December 2011 and October 2013) and seventeen in response to spending-based announcements. Of course, it is possible that the larger number of protests against spending cuts is the result of proposals to cut spending occurring more frequently than proposals to raise revenues. Our narrative data do show that spending-based consolidation plans occur relatively more frequently than revenue-based plans, but not to the extent that strikes against spending cuts dominate strikes against revenue increases. Appendix 4.B describes some further anecdotal evidence of protesters preferring revenue increases to spending cuts. Overall, our data suggest that plans to cut public
spending are more likely to encounter public opposition than plan to raises revenues, thus providing indirect support for a potential role of “active non-follow-up”.

4.4 Data on Fiscal Consolidation Announcements

The narrative fiscal data used in this section covers thirteen EU countries over the period 1978 - 2013. The news on fiscal austerity announcements consists of measures that are not proposed as a response to macroeconomic fluctuations, but with the main goal of reducing the deficit and/or the debt level. Hence, the identified measures are in principle exogenous to the business cycle. Details on data construction are provided in Chapter 2.

For the analysis undertaken in this section, the set of announcements constructed at the monthly frequency and described in Chapter 2 is aggregated to the quarterly frequency. The main reason for this conversion is that macro-economic and fiscal variables are (at best) only available at quarterly frequency. In addition, this approach mitigates potential anticipation effects because of information becoming available before the official consolidation announcement. It may be the case that a measure receives media attention before the first official announcement, for example, because information from discussions at the government level or in ministries is leaked to the press. However, pinpointing the first moments of media attention to such measures is virtually unfeasible given the coverage of the data in terms of countries and sample period. Moreover, initial discussions in the media generally provide only little information about the size and the composition of the measures. Nevertheless, the extensive investigation in Chapter 3 suggests that fiscal news recorded the way we do tends to be anticipated beforehand (Beetsma et al., 2015).7 By aggregating the monthly announcements to quarterly frequency, we ameliorate potential anticipation effects. To

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7 This was investigated by exploring the movements in consumer confidence around the official announcement dates. We observed that consumer confidence tends to move significantly already before the official announcement.
alleviate any potential anticipation effects further, we assign the announcement made in the first month of a quarter to the preceding quarter.\footnote{We find that our results are robust to assigning the announcement to the quarter in which it officially takes place (results are available upon request). Incidentally, note that Ramey (2011) also applies an adjustment to the quarterly timing of her 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.}

Table 4.4 reports the magnitudes of the announced plans. Note that the figures refer to the annual size of the plans, while the plans themselves are dated to the quarter in which their announcement takes place, as described above. In total we have 211 fiscal austerity announcements. For 180 of them we are also able to establish the magnitude of their impact on the primary balance. The cumulative annual impact of the measures on the primary balance ranges between 0% and 9.3% of GDP over a maximum period of 6 years, with an average value of 1.37% of GDP in our country sample. The horizon of the consolidation plans ranges between 1.3 and 2.3 years.

Most consolidation plans combine measures on both the revenue and the expenditure side of the budget, which is why in Table 4.5 we classify plans as predominantly revenue- or expenditure-based using a 50% threshold. That is, if more than 50% of the total announced budgetary impact comes from the expenditure side, the plan is classified as “spending-based”, while if more than 50% comes from the revenue side, it is classified as “revenue-based”. The 5 cases in which the division between spending and revenue measures is equal will be dropped from the sample, whenever we study the two subsamples of spending- and revenue-based plans separately. As Table 4.5 shows, the majority of the announcements in our sample are spending-based. In the group of expenditure-based announcements, the average announcement has a size of 1.42% of GDP, with an impact of 1.14% of GDP on the spending side and 0.28% on the revenue side. In the group of revenue-based announcements, the average announcement has a value of 1.26% of GDP, with an impact of 0.31% of GDP on the spending side and 0.95% of GDP on the revenue side.
4.5 The Panel Vector Autoregression (VAR) Analysis

In Section 4.3 we have documented that follow-up is weaker for spending-based consolidation plans than for revenues-based consolidation plans. This section follows up by further investigating the different macro-economic consequences of revenue-based and spending-based consolidation plans in a panel vector auto regression (VAR) where the announcements of the plans are introduced as shocks. Using this empirical model we will first confirm that more follow-up results from revenue-based consolidation announcements than from spending-based announcements. We also show that the former have much more adverse consequences for the economy than the latter. Most importantly, we disentangle the role of differences in follow-up and of differences in the composition of the revenue and spending plans for the divergence in the economic outcomes trajectories.

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

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

$$Z_{it} = \sum_{l=1}^{L} A_l Z_{it-l} + U_{it}$$

(4.4)

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

$$Z_{it} = [F_{it}, \tau_{it}, g_{it}, y_{it}, c_{it}, LTI_{it}, CCONF_{it}]'.$$

(4.5)

Here, $F_{it}$ is the fiscal consolidation announcement, $\tau_{it}$ is revenues as a share of GDP, $g_{it}$ is government expenditure as a share of GDP, $y_{it}$ is real GDP, $c_{it}$ is real private consumption, $LTI_{it}$ is the long-term interest rate and $CCONF_{it}$ is consumer confidence. All macroeconomic variables are expressed in real terms and deflated using the GDP deflator. With the exception of the long-term interest rate and the government revenue and expenditure ratios, all series are expressed in logarithms and multiplied by 100 to facilitate the interpretation of the coefficients as percentage changes. In the case of interest rates, coefficients represent changes in basis points. The deterministic components included in the baseline are seasonal dummies, country-fixed effects and country-specific linear trends. Importantly, because we are assessing the follow-up of consolidation announcements in terms of actual measures, the definitions of revenues and expenditures should correspond as closely as possible to the potential sets of measures included in the revenues respectively expenditure parts of fiscal consolidation. This implies in particular that $g_{it}$ will include transfers, hence $g_{it}$ is more broadly defined than merely government purchases.

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9 Appendix 4.A describes the quarterly budgetary and macroeconomic variables we use in this section.
We opt for a baseline specification containing four lags of the endogenous variables, hence amounting to a maximum lag length of one year. As shown below, the main results of the paper are robust to different choices for the lag structure and other configurations of the deterministic components.

**Identification and other estimation issues**

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

Existing datasets based on narrative identification, such as Pescatori et al. (2011), tend to assign the impact of austerity measures to the years of implementation in accordance with the plan. Alesina et al. (2015a,b) try to distinguish between the implementation of anticipated and unanticipated
measures. However, news about anticipated measures has generally been released earlier. The
same is usually the case for unanticipated measures, because these are mostly announced as part
of the new budget prepared in the year preceding the year for which the unanticipated measure is
reported. By timing austerity measures to the moment of their announcement, we take account
of the potential legislative and implementation lags. As mentioned earlier, we also try to handle
anticipation effects associated with earlier media coverage by assigning official announcements
made in the first month of a quarter to the preceding quarter.

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

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10 In the spirit of Hernández de Cos and Moral-Benito (2013) and Jordà and Taylor (2016), we analyze the pre-
dictability of the consolidation announcements by means of logistic regressions and find that the announcement shocks
are predicted by past debt dynamics and past values of long-term interest rates. The results are presented in Appendix
3.D.

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

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

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

Figure 4.1 reports the baseline responses when all consolidation announcements are included. Here, as well as in the following, the shock which takes place at moment 0 is a consolidation announcement normalized to 1% of GDP. We observe a highly significant rise in public revenues by around 0.15% of GDP after about a year and a fall in public expenditures of roughly the same magnitude that becomes significant after about half a year. Both responses point to potential “Keynesian-type” effects on the economy, in line with the consequences of the changes in public revenues and spending for disposable income. Indeed, we observe a maximum fall in GDP of around 0.2% that is close to significance. Private consumption exhibits a significant maximum deterioration of around 0.4%, while the long-term interest rate exhibits a positive jump on impact and reaches a significant maximum of around 15 basis points, after which it converges back to its steady state within a period of 3 years. The long-term interest rate thus reacts immediately to the news. Finally, consumer confidence shows a significant maximum fall of around 1.5 percent. The responses of output, consumption and consumer confidence raise doubts about the “expansionary austerity” hypothesis.

Next, we split the news into announcements of revenue-based plans (Figure 4.2) and spending-based plans (Figure 4.3). For both types of plans, the shock is normalized to 1% of GDP. Clear differences show up between the two types of announcements. The announcement of a revenue-based plan produces a highly significant increase in revenues reaching a maximum of almost 0.6% of GDP, while GDP and private consumption exhibit significant reductions that reach maxima of around 1 and 1.5 percent, respectively. Spending stays put, while the long-term interest rate rises by a maximum of about 40 basis points. Consumer confidence falls by a highly significant maximum of more than 5 percent. Note that both confidence indicators, the long-term interest rate and consumer
confident, even though they are forward-looking, reach their peak not immediately upon impact, but after half a year. By contrast, apart from spending itself, none of the responses under spending-based announcements become significant.\textsuperscript{13} In particular, GDP and private consumption remain almost perfectly flat. Spending falls by a maximum of 0.20 - 0.25 percent. The fall is significant, but in magnitude less than half the rise in revenues over GDP under a revenue-based consolidation announcement, even though in both cases the announcement is normalized to one percent of GDP. Both indicators also take some time to return to their long-run equilibrium level, although they do so faster than do real GDP and consumption.

**Robustness of the baseline**

We investigate the robustness of our baseline estimates in various ways. First, it could be argued that by including the recent crisis period in our time sample we capture an atypical period, during which the responses of economies to announcement shocks could differ from those in other periods. Therefore, Figures 4.C.1 and 4.C.1 in Appendix 4.6 report the impulse responses when we drop the period 2008-2013. However, these confirm the differences in the baseline responses for the two types of consolidation. Second, Figures 4.C.3 and 4.C.4 in Appendix 4.6 report the responses for the revenue- and spending-based plans when the (relevant) variables are expressed as shares of potential output. The baseline results are both qualitatively and quantitatively essentially unchanged. This is also the case when we replace the revenue and spending ratios of GDP by the logarithms of real revenues and real spending (Figures 4.C.5 and 4.C.6). Fourth, 4.C.7 and 4.C.8 in Appendix 4.6 report the responses when we restrict ourselves to revenues plans that contain at least 60% revenues measures and spending plans that contain at least 60% spending measures. Again, the responses are qualitatively and quantitatively very similar to those under the baseline. Fifth, this is also the case if we include a time dummy for each quarter in the sample - see Figures 4.C.9 and 4.C.10 in Appendix 4.C - or allow for eight instead of four lags in the panel VAR – Figures 4.C.11 and

\textsuperscript{13} For a panel based on a broad sample of advanced and emerging countries, Born et al. (2015) find that the effect of a cut in government consumption on the sovereign yield spread against a “riskless” reference country typically depends on the state of the economy.
Then, following Favero and Giavazzi (2012) we include the lagged public debt as an exogenous variable in the baseline specification. This way we control for the fact that past debt dynamics may help to predict the announcement shocks. Again, the resulting impulse responses reported in Figures 4.C.13 and 4.C.14 in Appendix 4.6 are qualitatively and quantitatively in line with those under the baseline. To check whether the baseline results are not driven by a specific country in our sample, our next robustness check drops one country at a time. Figures 4.C.15 and 4.C.16 in Appendix 4.6 show that the impulse responses are in all instances rather compactly clustered around the original responses, and in any case contained within the original 90% confidence intervals, thus suggesting that no individual country drives our main results.

Guajardo et al. (2014) suggest that the differences in impulse responses between revenue- and spending-based consolidations can be explained by monetary policy being more accommodative in the case of spending-based consolidations. However, over the largest part of the estimation period the majority of the countries in our sample had either a common currency or a stable exchange rate against the German mark. Hence, if we observe significant differences in the responses to revenue- and spending-based announcement consolidations, it is doubtful that these can be explained by differences in the monetary responses alone, because the ECB only responds to euro-area wide macroeconomic developments and not to those in individual countries. Likewise, in the period before EMU the Bundesbank only responded to German developments and not to those in other countries pegging their exchange rate to the German mark. Nevertheless, to control for monetary policy, we replace the long-term interest rate with the short-term interest rate, which is closer to the central bank’s policy instrument. Figures 4.C.17 and 4.C.18 confirm the findings in the baseline specification, except that the response of the short-term interest rate to a revenue-based consolidation is insignificant and smaller than the baseline response of the long-term interest rate.\footnote{The rise in the short-run interest rate following the announcement of a revenue-based consolidation is still not far from significant. However, it seems implausible that this rise is driven by a monetary tightening, because, if anything, we would expect monetary policy to become looser to avert the slow-down of the economy induced by the announcement. In any case, a counterfactual in which we force the short interest rate to stay constant does not affect the impulse responses.}
An alternative to the current baseline would have been a specification with business confidence instead of consumer confidence. However, we have fewer observations on business confidence than on consumer confidence, and hence we would lose precision in comparison to our current baseline. Including business confidence alongside consumer confidence would require estimation of even more parameters, and thus a further loss of degrees of freedom. Moreover, the question arises which indicator of confidence would be more important. A priori, to the extent that confidence affects the real economy, we expected consumer confidence to affect private consumption and business confidence to affect private investment. Private consumption is a substantially larger fraction of GDP; hence consumer confidence seems to be the more relevant variable to include in the baseline. Nevertheless, in our final robustness check we replace the consumer confidence indicator with the business confidence indicator and private consumption with private investment. The results are depicted in Figures 4.C.21 and 4.C.22 in the Appendix. Compared to our original baseline we observe that in the case of a revenue-based consolidation, real GDP again exhibits a significant fall. However, the fall is substantially smaller than under the baseline. This suggests that the confidence channel may be relevant and that consumer confidence is more important in this regard than business confidence.

The long-term interest rate may be viewed as an indicator of financial market confidence. The baseline impulse responses show that, in response to announcements of revenue increases, the long-term interest rate rises and consumer confidence falls, while both variables stay put in response to announcements of spending reductions. Hence, as indicators of financial market and consumer confidence the two variables behave consistently vis-à-vis each other. However, we would ideally like to rule out the possibility that the behavior of the long-term interest rate is driven by factors other than confidence. In particular, consolidation announcements may affect the long-term interest rate through their effect on inflation expectations. A priori, if a consolidation announcement is not expected to stimulate the economy, we would also not expect it to raise the long-term interest
rate by pushing up expected inflation.\textsuperscript{15} To confirm that the results are not driven by movements in inflation expectations, ideally one would include in the model the long-term interest rate purged of expected inflation (over the lifetime of the long-term debt). However, while we have survey measures on expected inflation, these only indicate whether private agents expect inflation to go up or down. Hence, we lack a quantification of expected inflation from the surveys, so this route cannot be pursued. Therefore, we redo the baseline regressions by replacing the long-run interest rate with its difference with respect to realized CPI inflation, i.e. the ex-post long-run real interest rate. The impulse responses are found in Figures 4.C.19 and 4.C.20 in Appendix 4.6. We observe that our baseline results are unaffected.

\textbf{4.5.1 Follow-up versus differences in composition of consolidation plans}

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

The responses are reported in Table 4.3, where the horizon \(h\) is expressed in quarters. As variables are forced to return to their baseline, we limit ourselves to a maximum horizon of 20 quarters. Follow-up after one year, as measured by the improvement in the primary balance, is more than 70 percent for revenue-based plans and only 25 percent for spending-based plans. Not surprisingly, for revenue-based plans, the largest fraction of the follow-up is an increase in revenues. Similarly, for spending-based plans the largest fraction of the follow-up is a reduction in spending. Besides differences in follow-up, we also observe differences in the cumulative primary balance

\textsuperscript{15} A potential exception concerns a consolidation that takes place largely through an increase in indirect taxes, which has a direct, though temporary, positive effect on inflation.
multiplier, which for horizon $h$ and revenue-based plans (henceforth indicated by subscript "r") we define as:

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

(4.6)

where $\hat{y}_{r,j}$ is the percent deviation of real output from its original value, hence $\sum_{j=1}^{h} \hat{y}_{r,j}$ is the cumulative percent deviation from the original value, $pb_{r,j}$ is the impulse response of the primary balance in percentage points of GDP for revenue-based plans after $j$ quarters. The cumulative primary balance multiplier for spending-based plans (henceforth indicated by subscript "s") at horizon $h$, $m_{h,p,s}$, is defined analogously. In other words, the cumulative primary balance multiplier is the cumulative percent change of real output, divided by the cumulative improvement in the primary balance in percent of GDP. The cumulative primary balance multiplier is substantially larger in absolute size for revenue-based than for spending-based consolidations, reaching a value of minus 3.6 after 5 years for the former, while for the latter it remains close to zero.

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

$$m_{h,\tau} \sum_{j=1}^{h} \tau_{r,j} + m_{h,\delta} \sum_{j=1}^{h} g_{r,j} = \sum_{j=1}^{h} \hat{y}_{r,j},$$

(4.7)

$$m_{h,\tau} \sum_{j=1}^{h} \tau_{s,j} + m_{h,\delta} \sum_{j=1}^{h} g_{s,j} = \sum_{j=1}^{h} \hat{y}_{s,j},$$

(4.8)
where \( \sum_{j=1}^{h} r_{r,j} \) and \( \sum_{j=1}^{h} g_{r,j} \) are the cumulative percentage point changes in revenue and spending over GDP. These are obtained directly from the impulse responses as reported in Table 4.3. The cumulative multiplier for revenues, also reported in Table 4.3, increases with the horizon \( h \) and reaches a maximum of 3.6 over the reported horizon, which is comparable in magnitude to the estimated effect of a 1 percent of GDP tax hike in Romer and Romer (2010). The cumulative multiplier for spending is actually negative\(^{16}\) An alternative explanation is based on the “expansionary austerity” view discussed in Section 4.2, suggesting that a spending reduction benefits output, although the effect is only rather small.

The most important question for the purpose of this paper concerns the effect of differences in follow-up between revenue- and spending-based plans. Table 4.3 therefore also reports the output effects of a revenue-based consolidation if we counterfactually impose the same primary balance effects of a spending-based consolidation. Exploiting expression (4.6), we calculate the counterfactual cumulative output effect in percent (indicated by a tilde) under revenue-based plans for the counterfactual primary balance path of spending-based plans as:

\[
\sum_{j=1}^{h} \tilde{y}_{r,j} = m_{h,p,r} \sum_{j=1}^{h} p b_{s,j}.
\] (4.9)

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

\[
\tilde{y}_{r,h} = m_{h,b,r} \sum_{j=1}^{h} p b_{s,j} - m_{h-1,b,r} \sum_{j=1}^{h-1} p b_{s,j}.
\] (4.10)

Not surprisingly, due to the counterfactual reduction in follow-up the fall in output shrinks. Output savings are particularly large shortly after the announcement shock has taken place. The fall in output after one year shrinks from 0.62 percent to 0.19 percent. The differences are rather small \(^{16}\)Corsetti et al. (2013) show that this may happen in the presence of default risk, when monetary policy is at the zero lower bound, hence unable to offset the impact of the default risk premium. However, for most of our sample monetary policy was not at the zero lower bound.

\(^{16}\)Corsetti et al. (2013)
after two and three years, but widen again at longer horizons. Overall, we observe that the largest
difference in the output trajectory between revenue- and spending-based plans is the result of the
different plan compositions. Even so, differences in follow-up do play a non-negligible role.

4.5.2 The role of the confidence channel

The impulse responses reported above show that, to the extent that movements in confidence are a
reflection of the anticipated course of the economy, they are consistent with the latter for both types
of consolidations: in the case of spending consolidations, neither financial market confidence as
captured by the long-run interest rate nor consumer confidence are affected, in line with fact that the
state of the economy does not change. In the case of revenue consolidations, both financial market
and consumer confidence deteriorate in line with the anticipated deterioration of the economy.

However, as documented in Section 4.2, it is sometimes argued that movements in confidence
themselves constitute a separate transmission channel of the effects of consolidations. In contrast
to other narrative fiscal datasets, our dataset of consolidation announcements offers a unique
opportunity to explore the effects of (intended) consolidations on confidence. The reason is that
our dataset allows us to detect the effect of unanticipated fiscal information on potential movements
in confidence that happen at the moment the information is released. These movements, and their
broader effect on the economy, would at most be partially accounted for in existing datasets that
fail to record the new information when it actually becomes available.

To obtain some indication of the potential importance of the confidence channel we conduct
a counterfactual in which we shut this channel off by fixing both the long-term interest rate and
consumer confidence at their original values following the announcements – Figures 4.4 and 4.5.
The deteriorations in real GDP and consumption following a revenue-based announcement are now
substantially reduced. However, in view of the Lucas critique, we take this finding only as indicative
of the potential role of confidence in the transmission from consolidation announcements to the real economy.

4.6 Conclusion

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

We then constructed a narrative dataset on fiscal consolidation announcements which were entered as shocks into a panel VAR. The goal of this exercise was to explore differences in the reaction of the macro-economy to the two types of consolidation announcements. In this way fiscal anticipation effects could be properly accounted for. The impulse responses confirmed the greater follow-up of revenue-based plans and showed that revenue-based consolidation announcements lead to a substantially larger reduction in economic activity than spending-based consolidation announcements. We then went on to disentangle the respective roles of the differences in follow-up and the differences in composition of the two plan types. We found that, while, the difference in the composition of the two plan types was the main contributor to the difference in economic performance (owing to substantially different multipliers for revenues and spending), differences in follow-up were able to explain a substantial fraction as well.
Table 4.1: Average of *ex-post* deviations for revenues

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

*Notes: (i) a negative number means a short-fall of actual implementation from the announcement. (ii) Averages are calculated over all consolidation years per country or over all (country, consolidation year) combinations. (iii) $D_{TREV}$ = deviations for “Total receipts, excluding gross interest receipts, government”, $D_{CREV}$ = deviations for “Current receipts, excluding gross interest receipts, government”, $D_{CACREV}$ = deviations for cyclically-adjusted “Current receipts, excluding gross interest receipts, government”, and $D_{NREV}$ = deviations for “Total revenues (narrow definition)”. (iv) The number in brackets is the number of consolidation observations per country.*
Table 4.2: Average of *ex-post* deviations for spending

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

*Notes:* (i) a positive number means a short-fall of actual implementation from the announcement. (ii) $D_{TEXP}$ = deviations for “Total disbursements, excluding gross interests payments, government”, $D_{CEXP}$ = deviations for “Current disbursements, excluding gross interest payments, government”. $D_{CACEXP}$ = deviations for cyclically-adjusted “Current disbursements, excluding gross interest payments, government”, and $D_{NEXP}$ = deviations for “Total expenditure (narrow definition)”. (iii) Further, see the Notes to Table 4.1.
<table>
<thead>
<tr>
<th>Plan type</th>
<th>Description</th>
<th>Expression</th>
<th>$h = 4$</th>
<th>$h = 8$</th>
<th>$h = 12$</th>
<th>$h = 20$</th>
<th>Max (revenue), Min (spending, output)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td><strong>Revenue-based</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Output</td>
<td>$\hat{y}_h$</td>
<td>-0.62</td>
<td>-1.04</td>
<td>-0.99</td>
<td>-0.69</td>
<td>-1.05</td>
<td></td>
</tr>
<tr>
<td>Primary budget balance</td>
<td>$pb_h = \tau_h - g_h$</td>
<td>0.71</td>
<td>0.15</td>
<td>0.11</td>
<td>0.2</td>
<td>0.71</td>
<td></td>
</tr>
<tr>
<td>Revenue ratio</td>
<td>$\tau_h$</td>
<td>0.57</td>
<td>0.27</td>
<td>0.17</td>
<td>0.08</td>
<td>0.57</td>
<td></td>
</tr>
<tr>
<td>Spending ratio</td>
<td>$g_h$</td>
<td>-0.14</td>
<td>0.12</td>
<td>0.06</td>
<td>-0.12</td>
<td>-0.14</td>
<td></td>
</tr>
<tr>
<td>Average cumulative revenue ratio</td>
<td>$\sum_{j=1}^{h} r_j / j$</td>
<td>0.28</td>
<td>0.33</td>
<td>0.28</td>
<td>0.22</td>
<td>0.34</td>
<td></td>
</tr>
<tr>
<td>Cumulative multiplier primary budget balance</td>
<td>$\sum_{j=1}^{h} \hat{y}<em>j / \sum</em>{j=1}^{h} pb_j$</td>
<td>-1</td>
<td>-1.98</td>
<td>-3.06</td>
<td>-3.63</td>
<td>-3.63</td>
<td></td>
</tr>
<tr>
<td>Output</td>
<td>$\hat{y}_h$</td>
<td>0.04</td>
<td>0.03</td>
<td>-0.02</td>
<td>-0.05</td>
<td>-0.05</td>
<td></td>
</tr>
<tr>
<td>Primary budget balance</td>
<td>$pb_h = \tau_h - g_h$</td>
<td>0.25</td>
<td>0.22</td>
<td>0.14</td>
<td>0.06</td>
<td>0.28</td>
<td></td>
</tr>
<tr>
<td>Revenue ratio</td>
<td>$\tau_h$</td>
<td>0.02</td>
<td>0.01</td>
<td>0</td>
<td>-0.01</td>
<td>0.07</td>
<td></td>
</tr>
<tr>
<td>Spending ratio</td>
<td>$g_h$</td>
<td>-0.23</td>
<td>-0.21</td>
<td>-0.14</td>
<td>-0.06</td>
<td>-0.23</td>
<td></td>
</tr>
<tr>
<td>Average cumulative spending ratio</td>
<td>$\sum_{j=1}^{h} s_j / j$</td>
<td>-0.14</td>
<td>-0.18</td>
<td>-0.17</td>
<td>-0.14</td>
<td>-0.18</td>
<td></td>
</tr>
<tr>
<td>Cumulative multiplier primary budget balance</td>
<td>$\sum_{j=1}^{h} \hat{y}<em>j / \sum</em>{j=1}^{h} s_j$</td>
<td>0.15</td>
<td>0.17</td>
<td>0.13</td>
<td>-0.01</td>
<td>0.18</td>
<td></td>
</tr>
<tr>
<td></td>
<td><strong>Spending-based</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Combining plan types</td>
<td>Cumulative multiplier revenues</td>
<td>$-1.33$</td>
<td>-1.92</td>
<td>-2.62</td>
<td>-3.64</td>
<td>-3.64</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Cumulative multiplier spending</td>
<td>$-0.5$</td>
<td>-0.55</td>
<td>-0.52</td>
<td>-0.35</td>
<td>-0.57</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Counterfactual output of revenue-based consolidation announcements with primary balances of spending-based announcements</td>
<td>$\tilde{y}_{r,h}$</td>
<td>-0.19</td>
<td>-0.9</td>
<td>-0.94</td>
<td>-0.19</td>
<td>-1.02</td>
</tr>
</tbody>
</table>

Notes: (i) The announcement shock always has a magnitude of 1 percent of GDP. (ii) horizon $h$ is expressed in quarters. (iii) The final column reports the maximum or the minimum over the maximum horizon of 20 quarters.
### Table 4.4: Summary statistics of fiscal announcements

<table>
<thead>
<tr>
<th>Country</th>
<th>Number of fiscal plans</th>
<th>Average annual size - all measures</th>
<th>Average annual size - spending measures</th>
<th>Average annual size - revenue measures</th>
<th>Average horizon of fiscal plans (years)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Austria</td>
<td>7</td>
<td>1.98</td>
<td>1.21</td>
<td>0.77</td>
<td>2.3</td>
</tr>
<tr>
<td>Belgium</td>
<td>18</td>
<td>1.14</td>
<td>0.68</td>
<td>0.46</td>
<td>1.5</td>
</tr>
<tr>
<td>Denmark</td>
<td>6</td>
<td>1.35</td>
<td>0.85</td>
<td>0.5</td>
<td>1.5</td>
</tr>
<tr>
<td>Finland</td>
<td>10</td>
<td>1.47</td>
<td>1.37</td>
<td>0.1</td>
<td>1.6</td>
</tr>
<tr>
<td>France</td>
<td>15</td>
<td>0.87</td>
<td>0.44</td>
<td>0.43</td>
<td>1.8</td>
</tr>
<tr>
<td>Germany</td>
<td>16</td>
<td>0.92</td>
<td>0.56</td>
<td>0.36</td>
<td>1.7</td>
</tr>
<tr>
<td>Ireland</td>
<td>15</td>
<td>2.05</td>
<td>1.1</td>
<td>0.95</td>
<td>1.3</td>
</tr>
<tr>
<td>Italy</td>
<td>25</td>
<td>1.31</td>
<td>0.74</td>
<td>0.57</td>
<td>2</td>
</tr>
<tr>
<td>Netherlands</td>
<td>22</td>
<td>1.17</td>
<td>0.99</td>
<td>0.18</td>
<td>1.3</td>
</tr>
<tr>
<td>Portugal</td>
<td>10</td>
<td>2.09</td>
<td>1.19</td>
<td>0.9</td>
<td>1.8</td>
</tr>
<tr>
<td>Spain</td>
<td>19</td>
<td>1.57</td>
<td>0.91</td>
<td>0.66</td>
<td>1.7</td>
</tr>
<tr>
<td>Sweden</td>
<td>5</td>
<td>2.38</td>
<td>1.57</td>
<td>0.8</td>
<td>2</td>
</tr>
<tr>
<td>United Kingdom</td>
<td>12</td>
<td>0.79</td>
<td>0.41</td>
<td>0.39</td>
<td>2.3</td>
</tr>
<tr>
<td>Total</td>
<td>180</td>
<td>1.37</td>
<td>0.85</td>
<td>0.51</td>
<td>1.7</td>
</tr>
</tbody>
</table>

### Table 4.5: Plans according to their predominant instrument

<table>
<thead>
<tr>
<th>Country</th>
<th>Spending-based</th>
<th>Revenue-based</th>
<th>Equal</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Austria</td>
<td>5</td>
<td>2</td>
<td>0</td>
<td>7</td>
</tr>
<tr>
<td>Belgium</td>
<td>8</td>
<td>8</td>
<td>2</td>
<td>18</td>
</tr>
<tr>
<td>Denmark</td>
<td>2</td>
<td>3</td>
<td>1</td>
<td>6</td>
</tr>
<tr>
<td>Finland</td>
<td>8</td>
<td>2</td>
<td>0</td>
<td>10</td>
</tr>
<tr>
<td>France</td>
<td>10</td>
<td>5</td>
<td>0</td>
<td>15</td>
</tr>
<tr>
<td>Germany</td>
<td>10</td>
<td>6</td>
<td>0</td>
<td>16</td>
</tr>
<tr>
<td>Ireland</td>
<td>8</td>
<td>6</td>
<td>1</td>
<td>15</td>
</tr>
<tr>
<td>Italy</td>
<td>15</td>
<td>9</td>
<td>1</td>
<td>25</td>
</tr>
<tr>
<td>Netherlands</td>
<td>19</td>
<td>3</td>
<td>0</td>
<td>22</td>
</tr>
<tr>
<td>Portugal</td>
<td>5</td>
<td>5</td>
<td>0</td>
<td>10</td>
</tr>
<tr>
<td>Spain</td>
<td>11</td>
<td>8</td>
<td>0</td>
<td>19</td>
</tr>
<tr>
<td>Sweden</td>
<td>5</td>
<td>0</td>
<td>0</td>
<td>5</td>
</tr>
<tr>
<td>United Kingdom</td>
<td>8</td>
<td>4</td>
<td>0</td>
<td>12</td>
</tr>
<tr>
<td>Total</td>
<td>114</td>
<td>61</td>
<td>5</td>
<td>180</td>
</tr>
</tbody>
</table>
Figure 4.1: Impulse responses baseline model – all announcements

Notes: (i) The announcement shock (not portrayed) always has a magnitude of 1 percent of GDP. (ii) The mean impulse responses and their 90% confidence intervals are constructed with standard bootstrapping techniques and are based on 1000 replications. (iii) The impulse responses for revenues and spending are deviations in percentage points of GDP from their original values; real GDP, consumption and consumer confidence are deviations in percent from their original values; and the long-term interest rate is the deviation in basis points from its original value.
Figure 4.2: Impulse responses baseline model – revenue-based plans

Revenue (%GDP)  Expenditure (%GDP)

Real GDP  Real Consumption

Long-Term Interest Rate  Consumer Confidence

Notes: See Notes of Figure 4.1.
Figure 4.3: Impulse responses baseline model – spending-based plans

Revenue (%GDP)  

Expenditure (%GDP)  

Real GDP  

Real Consumption  

Long-Term Interest Rate  

Consumer Confidence  

Notes: See Notes of Figure 4.1.
Figure 4.4: Counterfactually shutting off the confidence channel – revenue-based plans

Notes: See Notes of Figure 4.1.
Figure 4.5: Counterfactually shutting off the confidence channel – spending-based plans

Notes: See Notes of Figure 4.1.
Appendix Chapter 4

4.A Data

4.A.1 Budgetary variables

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

Correspondence Eurostat and EO series:

To construct appropriate quarterly data series, we have to make sure that the series extracted from Eurostat and the EO correspond to each other. To ensure maximum comparability of the OECD and Eurostat fiscal variables, we adopt the following procedure. First, we determine the correspondence between the budgetary components recorded at the annual frequency from the OECD with the annual data on the same components available from Eurostat. Based on the description of the data and the comparison of their numerical values, we are able to match perfectly a number of series observed at annual frequency between the two sources. The correspondences between codes from the two data sources are given in the follows:17

17There are other components of government revenues and expenditures available from both sources that cannot be matched.
Table 4.A.1: Correspondence between OECD and Eurostat series

<table>
<thead>
<tr>
<th>Code OECD</th>
<th>Code Eurostat</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>SSRG D61REC</td>
<td></td>
<td>Social security contributions received by government</td>
</tr>
<tr>
<td>TIND D2REC</td>
<td></td>
<td>Indirect taxes</td>
</tr>
<tr>
<td>TY D5REC</td>
<td></td>
<td>Total direct taxes</td>
</tr>
<tr>
<td>CGAA P3</td>
<td></td>
<td>Government final consumption expenditure, appropriation account</td>
</tr>
<tr>
<td>IGAA P51G</td>
<td></td>
<td>Government fixed capital formation, appropriation account</td>
</tr>
<tr>
<td>SSPG D62PAY</td>
<td></td>
<td>Social security benefits paid by the government</td>
</tr>
</tbody>
</table>

Collection and construction of the quarterly series:

Then we collect the quarterly data from Eurostat using the same variable definitions. Hence, the quarterly data match the annual data from both Eurostat and the OECD. All quarterly data are seasonally unadjusted and expressed in millions of euros or in local currency units. We multiply the series expressed in local currency units with the exchange rate against the euro and transform all the data in euros, after which we seasonally adjust the series using the X-11 procedure in EViews.

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

Construction of aggregate quarterly revenues and expenditure series:

We construct the following series of aggregate revenues and expenditures:

- **Total revenues (narrow definition)** = Total direct taxes + Indirect taxes + Social security contributions received by government;
- **Total expenditure (narrow definition)** = Government final consumption expenditure, appropriation account + Government fixed capital formation, appropriation account + Social security benefits paid by the government.

The relevant codes are found in Table 4.A.1. These series are constructed both at annual and quarterly frequency. At the annual frequency, we also construct other series. The most comprehensive annual measure of public revenues is:

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

Moreover, we collect:

\[
\text{Current receipts, excluding gross interest receipts, government} = \text{Total revenue (narrow definition)} + \text{Other current receipts by government (OECD code: TOCR)} + \text{Property income received by government (OECD code: YPERG)} - \text{Gross government interest receipts (OECD code: GGINTR)}.
\]

The most comprehensive measure of public spending that we use is: **Total disbursements, excluding gross interest payments, general government** (OECD code: YPGTX) = Current disbursements, government (OECD code: YPG) + Government fixed capital formation, appropriation account (OECD code: IGAA) + Capital transfers paid and other capital payments (OECD code: TKPG).
– Government consumption of fixed capital (OECD code: CFKG) \( \sim \) Gross government interest payments (OECD code: GGINTP).

Moreover we collect:

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

4.A.2 Macroeconomic variables

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

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

---

18 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).
**Potential real GDP**: we obtain this variable as the trend component resulting after first HP filtering the log of real GDP as defined above and then taking the exponential (inverse of the logarithm) of the resulting trend component of the series;

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

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

**Long-term interest rate** = Long-term interest rate on government bonds, quarterly (OECD Economic Outlook EO96). Missing observations are taken from EO88 (also quarterly): Germany before 1991 (Western Germany) and Ireland before 1990;
**Short-term interest rate** = Short-term interest rate, quarterly (OECD Economic Outlook 96).\(^{19}\) Missing observations are taken from EO88 (also quarterly): Germany before 1991 (Western Germany), Ireland between 1984 and 1990, and the UK between 1977 and 1978;

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

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

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

**Private investment** = Private gross fixed capital formation, volume. The International Monetary Fund (IMF) IFS database provides nominal, sometimes seasonally adjusted and sometimes non-seasonally-adjusted values in local currency units before 1999 and in euros after 1999. We use the IMF’s IFS because from the OECD the data are missing entirely for Austria, Italy, Portugal and Spain. The IFS data are processed further for two reasons. First, for Italy, before 1999 the series was in trillions of lira (we multiplied by 1000) and for Portugal it was in billions escudo (we multiplied by 1000). For Ireland the linked series was in millions of euros (we divided the entire linked series by 1000). To link two series before and after 1999, we multiply the data in local currency units by the official conversion rate to the euro prevailing in 1999. The conversion

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\(^{19}\) 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.
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 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.20

4.A.3 Confidence

Consumer confidence indices are collected from the OECD, which in turn obtains them from national statistical institutes, government agencies, banks and private and other research institutes. The indices have been standardized by the OECD to make them comparable across countries. The consumer confidence measures are based on questionnaires sent out to a random sample of the population. Each of the questionnaires contains four, sometimes five, questions on the current and expected future personal and general economic situation. For example, for the EU harmonized consumer confidence indicator the following information is collected:

- expected change in the financial situation of the household over the next 12 months;
- expected change in the general economic situation over the next 12 months;
- expected change in unemployment over the next 12 months;
- expected change in the savings of household over the next 12 months.

---

20 Both the GFCF deflator and the GDP deflator are discontinued for Germany in 1991. To link them, we take the ratio of prices for Germany (with base year 2010) and prices for Western Germany (with base year 1991), and average it over the quarters of 1991. We multiply by this factor all values for the price index with base 1991 (to transform their base to 2010).
Each of the questions has five possible answers: a lot better, a little better, the same, a little worse and a lot worse. The answers are balanced (positive over negative) and weighed to create an index.\textsuperscript{21} Although the specific questions may differ from country to country, the general format is the same. For business confidence, we use the OECD indicator based on business tendency surveys for manufacturing.\textsuperscript{22} The business confidence indicator is the arithmetic average of the balances (in percentage points) of the answers to the following questions:

- How do you expect your production to develop over the next three months? Possible answers are: it will increase, remain unchanged or decrease.

- Do you consider your current stock of finished products to be too large (above normal), adequate (normal for the season) or too small (below normal)?

- Do you consider your current overall order books to be sufficient (above normal), sufficient (normal for the season) or not sufficient (below normal)?

The OECD standardizes the series for consumer and producer confidence as follows. First, they are smoothed using the Hodrick-Prescott filter, where cycles shorter than six months are removed. Then, they are normalized by subtracting their mean and dividing this difference by their standard deviation. After the normalization, they are amplitude-adjusted to the de-trended indices of GDP, used as a proxy of the business cycle, and, finally, they are centred at around 100 (for further detail, see Organisation for Economic Co-operation and Development, 2006; Organisation for Economic Co-operation and Development, 2014a).

\textsuperscript{21} The EC assigns double weights to the extremes: a lot better/a lot worse get weight 1, a little better/a little worse get weight 0.5 and the same gets weight zero.

\textsuperscript{22} Other sectors (construction, retail trade and other services) were not included since data availability is scarce among non-European Union OECD member countries.
4.B A framework and some indirect evidence for “active" non-follow-up

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

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

In period 1, the government has the option to carry out the announced spending consolidation or stick to the spending level incurred in the previous period. Similarly, it has the option to carry out the announced increase in revenues or stick to the revenues level in the previous period. Carrying out consolidation measures is politically costly. However, ending period 1 with public debt is also costly. This will not be explicitly modeled, but it may be the result of politically-costly consolidation measures that are expected to be needed in the future. Hence, in period 1 the government features
a loss from carrying out the combination \((g_1, \tau_1)\) of:

\[
L = \frac{1}{2} \left[ \lambda (g_0 - g_1)^2 + \mu (\tau_0 - \tau_1)^2 + d_2^2 \right], \quad \lambda, \mu > 0, \quad (4.11)
\]

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

\[
\Delta g = \begin{cases} 
0, & \text{with probability } (1 - \pi_g) \\
\Delta, & \text{with probability } \pi_g
\end{cases} \quad \Delta \tau = \begin{cases} 
0, & \text{with probability } (1 - \pi_\tau) \\
\Delta, & \text{with probability } \pi_\tau
\end{cases}
\]

The constant \(\Delta\) is sufficiently large that the political cost of consolidating spending, respectively revenues, is prohibitively high, and the government is forced to stick to the spending, respectively revenues levels in period 0. Finally, \(\Delta g\) and \(\Delta \tau\) are assumed to be statistically independent.

The timing of events is as follows. In period 0, the government announces \((g_1^a, \tau_1^a)\). Then, the economy moves to period 1. At the start of period 1, the values of \(\Delta g\) and \(\Delta \tau\) become known. Finally, the government sets \((g_1, \tau_1)\). The following table indicates the probabilities and the possible outcomes for spending and revenues in period 1:

<table>
<thead>
<tr>
<th>(1 - \pi_g)</th>
<th>(\pi_g)</th>
<th>(\pi_\tau)</th>
</tr>
</thead>
<tbody>
<tr>
<td>((g_1, \tau_1) = (g_1^a, \tau_1^a))</td>
<td>((g_1, \tau_1) = (g_0, \tau_1))</td>
<td>((g_1, \tau_1) = (g_0, \tau_0))</td>
</tr>
</tbody>
</table>

Hence, exploiting the loss function \(L\), in period 0 the government sets \((g_1^a, \tau_1^a)\) so as to minimize:

\[
\frac{1}{2} (1 - \pi_g)(1 - \pi_\tau) \left[ \lambda (g_0 - g_1^a)^2 + \mu (\tau_0 - \tau_1^a)^2 + (d_1 + g_1^a - \tau_1^a)^2 \right] \\
+ \frac{1}{2} (1 - \pi_g)\pi_\tau \left[ \lambda (g_0 - g_1^a)^2 + (d_1 + g_1^a - \tau_0)^2 \right] \\
+ \frac{1}{2} \pi_g(1 - \pi_\tau) \left[ \mu (\tau_1^a - \tau_0)^2 + (d_1 + g_0 - \tau_1^a)^2 \right] + \frac{1}{2} \pi_g\pi_\tau(d_1 + g_0 - \tau_0)^2 \quad (4.12)
\]
Some algebra yields the following outcomes for the consolidation announcements:

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

(4.13)

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

(4.14)

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

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

(4.15)

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

(4.16)

It is easy to see that, not surprisingly, \( \mathbb{E}(g_1 - g_1^a) \) and \( \mathbb{E}(\tau_1^a - \tau_1) \) are increasing in the probabilities \( \pi_g \), respectively \( \pi_\tau \), that the announced consolidation measures will not be carried out. Most importantly, we find that \( \mathbb{E}(g_1 - g_1^a) > \mathbb{E}(\tau_1^a - \tau_1) \) if and only if \( \pi_g \mu > \pi_\tau \lambda \). If deviations in revenues and spending from their initial values feature equal weights in loss function, i.e. \( \mu = \lambda \), this condition reduces to \( \pi_g > \pi_\tau \). Hence, the average deviation of the actual from the announced spending reduction exceeds the average deviation of the actual from the announced revenues increase when the chance of not carrying out the spending consolidation exceeds that of not carrying out the revenues consolidation measures.

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

The dataset considers a slightly larger set of countries than our austerity announcement data and it also includes an outlier in terms of the number of general strikes: out of the total of 159 episodes, 69 are registered in Greece. We start by analyzing the dataset in full and then restrict our attention to the country sample matching our 13 European OECD countries. Out of the 159 disputes, we find that 69 are spending-cut motivated, 43 after excluding those where the issue in dispute is pensions, while the number of revenue-raise motivated strikes is only 7. Hence, disputes motivated

23 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.
by spending-cuts occur with a substantially higher frequency than disputes motivated by revenue increases. We also observe 34 strikes against austerity in general, hence aimed at adjustments in both taxes and spending. Excluding Greece, Luxembourg and Norway, 85 strike episodes remain, of which 40 are motivated by spending cuts (23 upon exclusion of the pension-related disputes), 6 are motivated by revenue increases and 8 are motivated by both revenue increases and spending cuts. The information is summarized in Figure 4.B.1.
Figure 4.B.1: Strikes in Western Europe by issue in dispute

(a) Absolute numbers

(b) Percentages
As a next step, we select only those strikes that took place before 2014 in our sample countries and we obtain data for general strikes in eight countries: Austria, Belgium, Finland, France, Italy, Netherlands, Portugal, Spain. After removing the strikes that cannot be assigned specifically to austerity measures, we are left with twenty strikes that can be matched with the consolidation announcements in our dataset on the basis of the narrative description of the strike and the consolidation (one in Finland, one in France, one in Spain, one in the Netherlands, two in Portugal, four in Belgium and ten in Italy). Out of the twenty strikes, three were undertaken in reaction to the same austerity announcement (in December 2011 in Italy). Three were undertaken in response to revenue-based announcements (namely in Italy in September 2011, December 2011 and October 2013) and 17 in response to spending-based announcements. Moreover, out of these 17 spending-based announcements eight have a revenue component of zero. From the table below 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>
<thead>
<tr>
<th>Strike following announcement</th>
<th>Average reduction in spending</th>
<th>Average increase in revenues</th>
<th>Total value of announcement</th>
</tr>
</thead>
<tbody>
<tr>
<td>no</td>
<td>0.82 (64.6%)</td>
<td>0.52 (35.4%)</td>
<td>1.34</td>
</tr>
<tr>
<td>yes</td>
<td>0.94 (72.7%)</td>
<td>0.29 (27.3%)</td>
<td>1.23</td>
</tr>
</tbody>
</table>

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

Interestingly, for some of the general strikes we consider, the newspaper articles and online sources that discuss them contain information regarding the preference of strikers 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 Becker, 2011, 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.” (March 26, 2004: The Guardian; Corriere della Sera).

4.B.1 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, the issue in dispute is “Labour law reform”. On the basis of additional information from Al Jazeera24: “Greece’s leading unions have launched a general strike that shut down several key sectors in protest over planned new pay cuts and taxes called for by international creditors”, we classify this as “both spending cut and revenue increase”.

Example 2: Belgium, 24 June 2016, the issue in dispute is “Austerity”. On the basis of additional information from Telesur25: “Workers are protesting against the government’s social and economic policies, which include 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 The BBC26: “Strikers are protesting against government cutbacks, including limits to benefits and overtime pay. The plans included cutting back holidays, reducing

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pensioners’ housing allowances, and reductions in employees’ overtime and Sunday pay.” we classify this as “spending cut motivated”.

4.C Robustness checks

Figure 4.C.1: Baseline model excluding the period 2008-2013 - revenue-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.2: Baseline model excluding the period 2008-2013 - spending-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.3: Baseline model in shares of potential GDP – revenue-based plans

**Revenue (%GDP*)**

**Expenditure (%GDP*)**

**Real GDP**

**Real Consumption**

**Long-Term Interest Rate**

**Consumer Confidence**

*Notes: See Notes of Figure 4.1.*
Figure 4.C.4: Baseline model in shares of potential GDP – spending-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.5: Baseline model including logs of revenues and spending – revenue-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.6: Baseline model including logs of revenues and spending – spending-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.7: Baseline model – revenue-based plans with at least 60% revenue measures

Revenue (%GDP)

Expenditure (%GDP)

Real GDP

Real Consumption

Long-Term Interest Rate

Consumer Confidence

Notes: See Notes of Figure 4.1.
Figure 4.C.8: Baseline model – spending-based plans with at least 60% spending measures

Notes: See Notes of Figure 4.1.
Figure 4.C.9: Baseline model with time fixed effects – revenue-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.10: Baseline model with time fixed effects – spending-based plans

Revenue (%GDP) vs. Real GDP

Expenditure (%GDP) vs. Real Consumption

Long-Term Interest Rate vs. Consumer Confidence

Notes: See Notes of Figure 4.1.
Figure 4.C.11: Baseline model with eight lags – revenue-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.12: Baseline model with eight lags – spending-based plans

Revenue (%GDP)  
Expenditure (%GDP)

Real GDP  
Real Consumption

Long-Term Interest Rate  
Consumer Confidence

Notes: See Notes of Figure 4.1.
Figure 4.C.13: Baseline model extended with lagged debt as exogenous variable – revenue-based plans

Revenue (%GDP) vs Expenditure (%GDP)

Real GDP vs Real Consumption

Long-Term Interest Rate vs Consumer Confidence

Notes: See Notes of Figure 4.1.
Figure 4.C.14: Baseline model extended with lagged debt as exogenous variable – spending-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.15: Baseline model excluding one country at a time – revenue-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.16: Baseline model excluding one country at a time – spending-based plans

Revenue (%GDP)

Expenditure (%GDP)

Real GDP

Real Consumption

Long-Term Interest Rate

Consumer Confidence

Notes: See Notes of Figure 4.1.
Figure 4.C.17: Baseline model with short-term interest rate – revenue-based plans

Revenue (%GDP) vs. Expenditure (%GDP), Real GDP vs. Real Consumption, Short-Term Interest Rate vs. Consumer Confidence

Notes: See Notes of Figure 4.1.
Figure 4.C.18: Baseline model with short-term interest rate – spending-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.19: Baseline model with ex-post long-run interest rate – revenue-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.20: Baseline model with ex-post long-run interest rate – spending-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.21: Baseline model with business confidence and private investment - revenue-based plans

Notes: See Notes of Figure 4.1.
Figure 4.C.22: Baseline model with business confidence and private investment - spending-based plans

Notes: See Notes of Figure 4.1.
Chapter 5

Private and Public Risk Sharing in the Euro Area

5.1 Introduction

The architecture and the functioning of the European Economic and Monetary Union (EMU) have been severely challenged in the context of the recent global financial crisis and in particular during the 2010-2012 euro zone sovereign debt crisis. Many commentators have argued that the lack of appropriate risk-sharing mechanisms at the euro area level may have contributed to aggravate the severity of the economic downturn in the euro zone periphery and may have delayed the recovery in the aftermath of the crisis (Allard, 2011). Against this background, the Five President Report highlights that euro area countries have to take steps, both individually and collectively, to compensate for the national adjustment tools they gave up on entry in the EMU.¹ First, when economic shocks occur, each country has to be able to respond effectively at the domestic level. Second, they may also smooth the impact of output shocks through risk sharing within the

EMU. Such risk-sharing mechanisms would facilitate consumption smoothing, thus decoupling consumption growth fluctuations from output growth fluctuations.²

Risk sharing can be achieved through integrated financial and capital markets, which is generally referred as “private risk sharing”. In addition, public policies at the supra-national level may also contribute to risk sharing across countries (Farhi and Werning, 2017). We refer to the latter as “public risk sharing”. Private risk sharing operates through two main channels. First, internationally diversified investment portfolios can generate income flows that are unrelated to fluctuations in the domestic economy. If the return on foreign assets is highly correlated with output growth in the issuer economy and weakly correlated with output growth in the domestic (holder) economy, cross-border portfolio investment contributes to consumption smoothing. Second, integrated credit markets could contribute to reinforce risk sharing: the supply of credit to the economy is expected to be less affected by country-specific shocks when international banks – which are in principle less exposed to the same shocks - operate in that economy. At the same time, more integration in the banking sector and financial markets may also amplify aggregate and idiosyncratic shocks, if the effects of such shocks would spill over more rapidly in an interconnected economic environment.

Risk sharing can also be supported via public channels at the EMU level. While a fully-fledged fiscal stabilisation mechanism for the euro area as a whole has been recently discussed, but not yet introduced, the EMU architecture has in recent years benefitted from the introduction of the European Financial Stability Facility (EFSF) and the European Stability Mechanism (ESM), which provide official financial assistance to EMU countries under stress. Therefore, such mechanisms might have also contributed to enhancing risk sharing within the EMU. The underlying intuition is that official assistance to distressed countries helped national governments in these countries maintain a certain level of public expenditure. For example, official assistance may have contributed to finance public salaries and pensions, which otherwise would have been cut even more severely²

² In general, perfect or full income risk-sharing – through both private and public channels - characterizes a situation where consumption growth rates are equalized across all countries (Mace, 1991).
(e.g., in case of a sovereign default). Therefore, our testable hypothesis is that public official assistance via the EFSF-ESM may have helped consumption smoothing in the euro zone periphery, on top of private channels.

This paper presents several contributions. First, based on a sample of 11 euro area countries for the period 1999-2015, we explore the role of financial integration and international financial assistance to distressed euro zone countries, i.e., official bilateral assistance via the EFSF-ESM, for consumption risk sharing. Second, we propose a time-varying framework which allows estimating how risk sharing, and the relative importance of the individual private and public risk-sharing channels, have evolved in the euro zone throughout this period, which includes the European sovereign debt crisis and its aftermath. Third, we analyse the degree and evolution of risk sharing by focussing on the link between “Core” and “Periphery” euro area countries.

Our paper also contributes to the existing literature by making use of a unique dataset of cross-border bank loans from the Bank for International Settlements (BIS). For the sample we are interested in, the confidential version of the BIS International Locational Banking Statistics (ILBS) reports the outstanding bilateral positions of banking sectors for 12 out of 19 euro zone countries against residents of the countries where they are located.\(^3\) We augment this information with data from the Coordinated Portfolio Investment Survey (CPIS) of the International Monetary Fund (IMF), recording bilateral cross-border holdings of portfolio investment securities, as well as their breakdown into debt and equity assets. We exploit the cross-sectional variation in bilateral exposures as well as the growing time coverage of the dataset. This is to our knowledge the first use of the CPIS database in a time series framework for the analysis of cross-country risk sharing.\(^4\)

We focus on the deviation of personal consumption growth with respect to output growth across EMU countries, as suggested by the reference literature in this field (see Asdrubali et al., 1996; \(^3\) Cyprus began reporting in 2008 and it thus not included in our sample. \(^4\) While previous studies have mainly focused on specific waves of the CPIS survey due to data availability Fratzscher and Imbs (2009), our analysis builds on different waves of this dataset. This expands considerably the number of observations used in the econometric analysis.
Sørensen and Yosha, 1998). More specifically, we follow Fratzscher and Imbs (2009) and, based on our still largely unexplored dataset of bilateral financial holdings and bilateral fiscal assistance, we estimate bilateral risk-sharing specifications which allow us to take full advantage of the time-series and cross-country information among euro area countries.

Our results suggest that, first, in the early years of the EMU only about 40% of country-specific (i.e., idiosyncratic) output shocks were smoothed. However, in the aftermath of the euro zone sovereign debt crisis, about 65% of these shocks were absorbed, therefore contributing to decrease consumption growth differentials across countries. The progressive improvement of the shock absorption capacity is due to higher financial integration, but also to the activation of the EFSF-ESM loans for Greece and other euro zone economies in 2010 (see also Milano, 2017). In addition, as regards private channels of risk sharing, cross-border holdings of equity and debt seem to be the most effective in smoothing consumption. This latter finding is particularly strong when we focus on the links between “Core” and “Periphery” EMU countries: holdings of debt and equity issued by Core countries and in the portfolio of agents in the “Periphery” (and vice versa) turn out to be effective in absorbing output shocks, thus allowing to better smooth consumption. However, our results indicate that cross-border bank loans tend to generate some shock amplification rather than shock absorption. This is likely to be explained by pro-cyclical borrowing and lending: countries have often borrowed from abroad in economic good times and repaid these loans in economic bad times, adding volatility to consumption in a pro-cyclical way.

The remainder of the paper is organised as follows. Section 5.2 presents a short review of the related literature on risk sharing. Sections 5.3 and 5.4 describe the basic and augmented empirical models, and Section 5.3 presents the dataset used in the analysis. Section 5.6 comments on the results and presents a number of robustness checks. Finally, Section 5.7 concludes.
5.2 Literature Review

The literature on income and consumption risk sharing has expanded considerably in the last three decades, reflecting stronger interest in the economic profession and among policy-makers on how countries (or states within a federation) may better isolate from idiosyncratic shocks hitting their economies (for a survey, see Ioannou and Schäfer, 2017).

Empirical studies of cross-country consumption risk sharing are motivated by a testable prediction of the international real business cycle model with complete markets. In a world with a single internationally-traded contingent bond, the Euler equations for the asset holdings indicate that the marginal rates of substitution between current and state-contingent future consumption should be equal across countries at each point in time. Consequently, consumption growth in any given country is only affected by global (and thus uninsurable) shocks. At the same time, in an equilibrium characterized by perfect risk sharing, the countries exhibit the same relative growth rate of consumption at each point in time irrespective of their relative output shocks (Mace, 1991; Canova and Ravn, 1996).5

The hypothesis of full international risk sharing has been largely rejected in the empirical literature. Contrary to the prediction of the model with complete markets, cross-country correlations in consumption growth are smaller in the data than correlations in income growth (Backus and Smith, 1993). Lewis (1996) investigates the role of financial markets and shows that capital market restrictions partly account for the lack of observed cross-country consumption risk sharing, indicating that financial market liberalization would improve consumption insurance. As mentioned in Canova and Ravn (1996), better consumption risk sharing can also result from the presence of institutions that improve insurance by means of transfer schemes, i.e. taxes and transfers, aid or lending.

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5 Farhi and Werning (2017) show that – even in presence of complete markets – some degree of public intervention allowing insurance against idiosyncratic shocks is welfare improving. In fact, private agents do not fully internalise the benefits from public risk-sharing channels when forming their decisions. Therefore, the authors make a strong theoretical case for fiscal insurance as a necessary complement to private risk sharing.
agreements. Therefore, we should observe higher levels of risk sharing in settings characterized by (supra)national transfer schemes, even in presence of unhampered financial markets.

One of the earlier and most influential contributions testing of the joint role of financial markets and transfer schemes for risk sharing is the seminal work of Asdrubali et al. (1996). The authors propose a framework based on a cross-sectional variance decomposition of shocks to GDP and quantify the amount of risk sharing among states in the United States over the period 1963-1990. They find that 39% of shocks to gross state product were smoothed by capital markets, 13% were smoothed by the federal government (via taxes, transfers, and grants to states), 23% percent were smoothed by credit markets while the remaining 25% were unsmoothed. Delrio et al. (2017) follow the Asdrubali et al. (1996) approach and explore the role of the current account, and in particular TARGET balances via the ECB, in influencing risk sharing in the EMU. Their findings point to a reduction of risk sharing during and after the crisis (see, also, European Central Bank (2017)) and identify the current account channel as the main driver of this this reduction. Milano (2017) updates and revisits the Asdrubali et al. (1996) approach to explore the role of European institutions (ESFS, ESM and the European Commission) for risk sharing in the euro area. She finds that shock absorption in the euro zone somewhat increased from 23% in the period 1999-2006, up to 31% in the period 2007-2014. Mélitz and Zumer (2002) examine the United States, France and the United Kingdom, and find that approximately 20% of regional income is stabilized through the central government budget, while Hepp and von Hagen (2013) suggest that this only 10% for Germany in the post-unification period (1995-2006). One recent paper exploring the role of fiscal transfers for cross-country consumption insurance within the euro area is Furceri and Zdzienicka (2015). On the basis of a counterfactual experiment introducing a fictitious supranational redistribution mechanism, the authors suggest there may be considerable insurance gains from setting up a fiscal stabilization mechanism in the euro area. All in all, what emerges from these studies it that a federal transfer scheme across regions seems to be able to smooth around 20% (or less) of local shocks.

6These results have been challenged by Del Negro (2002) who shows that – once measurement error in income and consumption is taken into account - the actual amount of risk sharing across U.S. states may be significantly lower than what suggested by Asdrubali et al. (1996).
More recently, a number of empirical studies have focused on financial variables and documented that greater financial globalization tends to increase risk sharing, at least among industrial countries. The underlying intuition is that more internationally diversified investment portfolios generate income changes that are unrelated to fluctuations in domestic income, therefore better isolating agents from idiosyncratic shocks that hit their economies (see Kose et al., 2007, Demyanyk et al., 2008, Pierucci and Ventura, 2010, Rangvid et al., 2016). Nevertheless, differences in regulation and accounting standards across countries may generate home bias, resulting in sub-optimal shares of foreign assets in domestic portfolios and lower than optimal international risk sharing. Indeed, Sørensen et al. (2007) show that international home bias in debt and equity holdings declined during the period 1993-2003 and this decline was accompanied by an increase in international risk sharing.

However, these findings generally refer to periods of financial upturn, while the effects of more financial market integration may be reversed during financial market downturns. In addition, if globalization leads to stronger co-movements between international stock markets, the benefits of cross-border holdings of financial assets might be limited (see, e.g., Beine et al., 2010). This is sometimes referred to as the “knife-edge” property of the financial markets: financial interconnections work as a shock absorber (i.e., leading to risk sharing) in certain states of the world. In others, interconnections tend to generate shock amplification, i.e., risk-spreading (see Tasca and Battiston, 2011, Balli et al., 2013).

Our paper connects, in particular, with Fratzscher and Imbs (2009) who extend the conventional tests of international consumption risk sharing introduced by Lewis (1996). By using bilateral asset holdings for 23 lending economies and 54 borrowing economies over the period 1961-2003, the authors explore the implications of transaction costs in influencing the degree of risk sharing via financial markets. Instead, we analyse the role of both public and private risk sharing channels in the euro zone, with a focus on the recent the European sovereign debt crisis and its aftermath. Moreover, we estimate bilateral consumption risk-sharing specifications which allow us to take full
advantage not only of the cross-sectional dimension as in Fratzscher and Imbs (2009), but also of
time-series information on bilateral holdings of financial assets among euro area countries. Within
our empirical framework we are also able to gauge the relative contribution of the different financial
and credit market (loans, equity and debt holdings) and the official lending (EFSF/ESM) channels
to the variation of consumption risk sharing from the early years of the EMU to the aftermath of
the euro zone’s sovereign debt crisis.

5.3 Methodology and Data

5.3.1 Baseline empirical setup

Most tests of consumption risk sharing are based on the difference between per capita consumption
growth in a country and the aggregate per capita consumption growth observed in the same currency
area, federation or in the rest of the world (depending on the relative importance of links between
countries in a certain area). Such tests are based on the following simple model,

$$\Delta \log C_{i,t} - \Delta \log C_t = \alpha + \beta(\Delta \log Y_{i,t} - \Delta \log Y_t) + \epsilon_{i,t} \quad (5.1)$$

where the log-growth of variable $X$ is denoted as $\Delta \log X_{i,t}$, $C_{i,t}$ denotes real per capita household
consumption and $Y_{i,t}$ stands for real per capita output in country $i$ at time $t$. $C_t$ and $Y_t$ denote
aggregate consumption and output in a certain reference area (e.g. the EMU).

Under the null hypothesis of perfect risk sharing, differences between the country-specific con-
sumption growth and the aggregate consumption growth (i.e., the country-specific or idiosyncratic
consumption growth), should be decoupled from the differences in output growth (i.e., idiosyn-
cratic output growth), thus yielding a risk-sharing coefficient $\beta$ equal to zero. Under the alternative
hypothesis, a coefficient $\beta$ statistically different from zero indicates imperfect risk sharing, and its
magnitude reflects the extent of the deviation from the theoretical benchmark.
We take this approach as a starting point but, contrary to most models in this literature, we fully exploit the information which is available in a three-dimensional panel of consumption growth and income growth differentials observed across country pairs over time, i.e., we test the relationship between consumption growth and output growth differentials between country $i$ and country $j$ at time $t$. In this bilateral setting, we define as ‘country-specific’ or ‘idiosyncratic’ a shock hitting country $i$ but not country $j$. It can be shown analytically that this corresponds to estimating the coefficient $\beta$ in the equation above. However, our setup allows us to exploit a much bigger dataset and therefore to increase the efficiency of our estimate. In our three-dimensional panel, the basic risk-sharing test then becomes:

\[
(\Delta \log C_{i,t} - \Delta \log C_{j,t}) = \alpha + \beta(\Delta \log Y_{i,t} - \Delta \log Y_{j,t}) + \gamma Z_{ij,t-1} + \eta_t + \mu_{ij} + \epsilon_{ij,t} \quad (5.2)
\]

The richest specification includes time-fixed effects $\eta_t$ to control for aggregate common shocks, country-pair fixed effects $\mu_{ij}$ to account for time-invariant bilateral characteristics and a set of control variables $Z_{ij,t}$ that vary across pairs $(ij)$ and over time $t$. In particular, the $Z$ matrix of controls includes the difference in the growth rate of statutory value added taxes $(\Delta VAT_{ij,t})$ and the difference in personal income taxes on distributed profit between countries $i$ and $j$ $(\Delta PIT_{ij,t})$. Indeed, Epstein et al. (2016) make a convincing case for the inclusion of tax measures in measurements of international risk sharing.\footnote{Epstein et al. (2016) account for the risk-sharing wedge generated by international differences in taxation. The authors augment a business cycle model with distortionary taxes and find that an increase in the relative consumption tax or capital income tax growth leads to lower relative consumption growth. They find that across country pairs, accounting for the distortionary effect of the capital tax wedge on the relative consumption growth rates contributes to revealing a positive link between insurance and financial integration.}

In addition to the tax-rate differentials, $Z$ also includes the inflation differential $(\Delta INF_{ij,t})$, the 10-year sovereign bond yield differential $(\Delta YIELD_{ij,t})$ and the domestic credit growth differential $(\Delta DCREDIT_{ij,t})$, defined as total credit by domestic banks to the private non-financial sector (see Appendix 5.B for a description of data and sources). The inclusion of the inflation differentials is theoretically justified by the link between the relative growth rates of consumption and the dynamics
of the real exchange rate (Backus and Smith, 1993; Galí and Monacelli, 2005). EMU countries are obviously characterized by invariant nominal exchange rates vis-à-vis other euro zone countries, therefore we account for real exchange rate differentials by including the relative dynamics of prices across countries. From a theoretical perspective, in a New Keynesian framework cross-country inflation differentials impact relative consumption growth rates (see, for example, Galí and Monacelli, 2005). We also include differentials in 10-year sovereign bond yields given that, for a large part of the sample period considered, the interest rate spreads in Europe were strongly affected by sovereign default risk. Based on a New Keynesian model featuring a ‘sovereign risk channel’, Corsetti et al. (2013) show that a larger default risk premium would translate into higher relative borrowing costs, thus exerting downward pressure on the relative growth rate of consumption. Finally, we also control for credit by domestic banks, given that this is a main source of financing for the domestic private sector, and therefore can substantially affect private consumption growth. We argue that controlling for domestic credit availability is of paramount importance for the period we analyse, given the documented increase in home bias during the sovereign debt crisis (Saka, 2017 and Ongena et al., 2016) and the unconventional monetary policy and liquidity provision measures taken by the European Central Bank in order to stimulate bank lending (asset purchase programmes, targeted longer-term refinancing operations).

In light of these considerations we condition the test of cross-country risk sharing on the chosen set of controls. In order to mitigate potential concerns about reverse causality in annual data, we use lagged values of all the covariates.\footnote{Consistently with their theoretical model, Epstein et al. (2016) introduce contemporaneous tax rates, rather than their lag. Whereas our estimates are robust to the use of contemporaneous tax measures, we prefer to report the results which in our opinion are less subject to potential endogeneity problems. It is if fact conceivable that the government sets the VAT and personal income tax rates also on the basis of the current state of the business cycle, which is driven by private consumption.}
5.4 Introducing Financial and Fiscal Integration

5.4.1 Construction of integration indices

In order to explore the role of financial integration and international official assistance as sources of time-varying heterogeneity in risk sharing within the EMU and following Fratzscher and Imbs (2009), we use interaction terms to model the dependence of the risk-sharing coefficient on measures of financial integration. Given our focus on changes in risk sharing since the inception of the EMU, we enrich the Fratzscher and Imbs (2009) model by allowing the interacting variables to vary not only across pairs, but also over time. Moreover, we extend their analysis by accounting also for the risk-sharing channel that operates through the EFSF-ESM financial assistance among the euro area countries.

To this end, we construct time-varying bilateral measures of financial integration and assistance through the EFSF-ESM. The financial integration measure is computed following Epstein et al. (2016) as the sum of claims of country $i$ over country $j$ and claims of country $j$ over country $i$, scaled by the sum of nominal GDP in country $i$ and country $j$:

$$ INT_{ij,t} = \frac{A_{i \rightarrow j,t} + A_{j \rightarrow i,t}}{Y_{i,t} + Y_{j,t}} $$

(5.3)

First, we compute a measure of ‘overall’ financial integration, where $A_{i \rightarrow j,t}$ is the sum of cross-border bilateral loans and cross-border portfolio investment $FIN_{ij,t}$. Then, we create two separate measures of integration for each of the two asset categories, namely LOAN$_{ij,t}$ and PORT$_{ij,t}$. Lastly, within the category of portfolio investment we further differentiate between debt and equity within the category of portfolio investment, and compute measures of integration for the corresponding assets (labelled respectively as DEBT$_{ij,t}$ and EQUITY$_{ij,t}$).

We further use formula (5.3) to compute a measure of EFSF-ESM bilateral assistance, which we label $EFSF_{ij,t}$ (for simplicity in notation, we label this variable only as $EFSF$, although it also
includes ESM loans). In this case, $A_{i\rightarrow j,t}$ represents the financial assistance provided by country $i$ to country $j$ and channelled via the EFSF-ESM facility at a given point in time. Figure 5.1 shows the extent of time variation exhibited by these integration measures, averaged across all country pairs. We notice that the cross-border bilateral debt holdings constitute the largest component. Cross-border bilateral loans and in particular equity holdings are quantitatively less than half of their debt counterpart. Cross-border holdings of debt instruments and cross-border loans show an upward trend up to the beginning of the financial crisis (loans) and the European sovereign debt crisis (debt), followed by a reduction which was particularly marked in the loan market. At the same time, cross-border holdings of equity are substantially stable over this period. The EFSF-ESM financial assistance variable is, by construction, zero up to 2009 given that the EFSF was activated only in 2010. As of 2010, it starts increasing, although it remains quantitatively smaller than the financial integration indices.

This approach also allows to compute integration indices at the country level, i.e., capturing the interlinkages of a single country vis-à-vis all other countries in the sample. The Figures in Appendix 5.A show these country-specific integration indices, where country $i$ is fixed (e.g., Austria), and the integration index is constructed by averaging the bi-lateral measure across the ten remaining $j$ countries.

First, we notice that Ireland and the Netherlands exhibit the highest levels of financial integration, driven in particular by their foreign debt holdings. On average, the least financially integrated countries appear to be Finland, Greece and Portugal. The majority of countries exhibit a higher level of integration though debt instruments than through equity. As expected, integration indices built on debt and equity holdings fall around the financial crisis of 2008-2009. Integration through equity markets recovers rather swiftly in the year(s) following the crisis, and in most cases exhibits an ascending trend after 2010. However, between 2009-2011, the period preceding the sovereign-debt crisis, debt integration drops in Ireland, Spain, Greece, Portugal (and to a lesser extent Italy).

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9 While all countries in our sample are contributors to the EFSF-ESM, we distinguish only 4 recipient countries (within our sample), namely Greece, Portugal, Spain and Ireland.
The sharp dynamics in the case of Greece reflect the moves in government debt markets during the sovereign debt crisis. We observe evidence of pro-cyclicality in financial integration through cross-border loans. Nearly all figures exhibit a hump-shaped pattern, with a peak in the business cycle upturn preceding the crisis (strongly visible, for instance, in the case of Belgium). Finally, the EFSF-ESM integration variable remains relatively small compared to the rest of the indicators. It reaches higher values for recipients of assistance, and a maximum of above one percent of GDP in the case of Greece.

5.4.2 Regression based on extended model

Allowing for the risk-sharing coefficient to depend linearly on financial and fiscal integration measures, the full model takes the form:

\[
(\Delta \log C_{i,t} - \Delta \log C_{j,t}) = \alpha + \beta_0(\Delta \log Y_{i,t} - \Delta \log Y_{j,t}) + \beta_1(\Delta \log Y_{i,t} - \Delta \log Y_{j,t})LOAN_{ij,t-1} \\
+ \beta_2(\Delta \log Y_{i,t} - \Delta \log Y_{j,t})EQUITY_{ij,t-1} + \beta_3(\Delta \log Y_{i,t} - \Delta \log Y_{j,t})DEBT_{ij,t-1} \\
+ \beta_4(\Delta \log Y_{i,t} - \Delta \log Y_{j,t})EFSF_{ij,t-1} + \gamma Z_{ij,t-1} + \eta_t + \mu_{ij} + \epsilon_{ij,t} (5.4)
\]

Formally, the coefficient capturing risk sharing between country \(i\) and \(j\) will be equal to the sum of the income growth differential coefficient (\(\beta_0\)) and the component that captures how risk sharing is related to K measures of financial/fiscal integration (\(\beta_1, \beta_2, \beta_3, \beta_4\)).

\[
\beta_t = \beta_0 + \sum_{k=1}^{K} \beta_k INT_{ij,t-1}^k (5.5)
\]

This measure of risk sharing is time-varying to the extent that the underlying measures of financial/fiscal integration (\(INT\)) change over time. In this model, the null hypothesis of perfect risk sharing amounts to testing whether the \(\beta_t\) coefficient in equation 5.5 is not statistically different from

---

10 Among the reasons for the sharp rise in the debt integration index between 2012 and 2013, there seems to be a sizeable increase of Italian and Spanish debt securities held by Greece.
zero. For positive values of $\beta_0$ and positive financial and fiscal integration indices, negative (positive) coefficients of $\beta_k$ indicate that higher values of integration improve (worsen) cross-country consumption risk sharing.\textsuperscript{11}

It is worth mentioning that correct econometric inference requires us to address the symmetry generated by constructing all variables as growth differentials. To avoid double counting we only keep one observation per country pair and thus for a sample of $N$ countries and $T$ time periods we use a total of $TN(N-1)/2$ observations. Moreover, the bilateral structure of our panel dataset induces a pattern of dyadic correlation in the errors: all pairs that have one country in common will be cross-sectionally correlated. According to Cameron and Miller (2014), the inclusion of country-pair fixed effects in panel models with paired data is insufficient to address the error-correlation structure and in general leads to underestimated standard errors. Therefore, we use the standard error correction proposed by Cameron and Miller (2014), which is specifically designed to address the particular correlation pattern of paired data.\textsuperscript{12}

### 5.5 The Data

Given the focus of our analysis, we restrict attention to a sample of 11 euro zone member countries: Austria, Belgium, Finland, France, Germany, Greece, Ireland, Italy, the Netherlands, Portugal and Spain. Our dataset is collected at a yearly frequency and covers the period 1999-2015. Although our analysis initially comprised the 12 euro zone countries for which cross-border bank loans from the Bank for International Settlements (BIS) are available, we exclude Luxembourg from our sample given its status as a financial hub and the observed cross-border exposures which indicate that this country is a clear outlier.

\textsuperscript{11} In the robustness section we extend equation (5.4) by including the level of the financial and fiscal assistance integration measures. Results show that the latter are generally statistically insignificant. Most importantly, our main estimates on the interaction terms remain almost identical both in size and statistical significance.

\textsuperscript{12} We find that our resulting errors are not significantly auto-correlated after applying the cross-sectional dyadic error correction, and thus do not necessitate further correction.
For our country and time sample, we construct a rich dataset combining information from multiple sources. As a first building block we use the confidential BIS International Locational Banking Statistics, which reports bilateral positions of the banking sector in country $i$ against each counterparty country $j$. The data is recorded using the residence principle, thus accurately reflecting cross-country exposures. To minimize the overlap with portfolio investment data, we restrict our attention to cross-border loans provided by a creditor banking system to the economy of a given debtor country. We combine the BIS information with bilateral data on portfolio investment from the IMF Coordinated Portfolio Investment Survey (CPIS). The CPIS consists of data on cross-border holdings of equity and debt securities, collected from holders by means of a survey and classified according to the residence of the issuer. In line with Lane and Milesi-Ferretti (2007), we use asset stocks as opposed to flows because they provide a better proxy for wealth.

Data on bilateral assistance provided by the EFSF and the ESM is retrieved from ECB sources and represents the stock amount flowing from each contributor to each recipient country in the euro area and channelled through the stability fund according to key capital contribution rates.\(^\text{13}\) The EFSF was an institutional entity created in 2010 with the purpose of providing financial assistance to distressed euro zone member states. Its activity was complemented by the bilateral loans channelled via the European Financial Stabilisation Mechanism (EFSM). In the period 2010-2013, the two facilities provided aid to Ireland, Portugal and Greece. In September 2012, their activity was taken on by the European Stability Mechanism (ESM), a similar institutional arrangement that provided assistance also to Cyprus and Spain.

In addition to financial and fiscal variables, we use standard macroeconomic variables available from Eurostat, namely final household consumption and gross domestic product at market prices. We deflate the series using by the harmonized index of consumer prices with reference year 2010. Finally, we divide them by total population. Therefore, consumption and output data are in real per capita terms.

\(^{13}\) For comparability with the financial data, we transform the EFSF and ESM assistance data to current U.S. dollars.
Following Epstein et al. (2016), we account for the role of relative tax differentials by including these as control variables in our estimation. We favour the use of statutory tax rates as opposed to measures of effective taxation derived from national accounts in order to alleviate the concerns about endogeneity to the dynamics of consumption and income, given that the effective consumption tax is a function of underlying household final consumption expenditure and the capital tax rate depends on production and imports. The tax differentials are constructed from data on tax rates. We use the statutory standard VAT rate and the overall (corporate plus personal) statutory tax rate on distributed profit, both available at annual frequency from the OECD Tax Database. As in Epstein et al. (2016), the consumption tax rate is used in differences and the capital tax rate is used in levels, following the structural equations of their model.

In order to capture bilateral differences in real interest rates, we further augment the set of control variables with the 10-year sovereign bond yield and consumer price index differentials from the OECD Main Economic Indicators.\textsuperscript{14} Finally, changes in country-level lending conditions by using data from the BIS total credit statistics. These data reflect the amount of credit supplied by domestic banks to private non-financial sectors.

\section*{5.6 Empirical Results}

\subsection*{5.6.1 Simple risk sharing regression}

In Table 5.1 we report the results of the simple bilateral risk-sharing regression as in equation (5.2), linking consumption growth differentials to output growth differentials (and a set of controls). In this first regression, we do not include measure of financial integration and official assistance. A coefficient on the output differential term \((\Delta \log Y_{it} - \Delta \log Y_{jt})\) equal to zero would signal perfect risk sharing, given that output growth differentials would not be reflected in consumption growth differentials. A coefficient equal to one would indicate no risk sharing. Table 5.1 shows the results

\footnote{More information on the sources, construction and coverage of the variables is reported in Appendix 5.B.}

174
from different specifications of the OLS estimation with standard errors clustered for dyadic data. In particular, column (1) shows the results with no controls added to the simple consumption-output regression, and with no fixed effects. Column (2) adds country-pair fixed effects, column (3) year fixed effects, and column (4) both types of fixed effects. The following part of the table shows the results when we include our set of controls, namely $\Delta VAT_{ij,t-1}$ and $\Delta PIT_{ij,t-1}$ (column 5), the domestic credit growth differential $\Delta DCREDIT_{ij,t-1}$ (column 6), the inflation differentials $\Delta INF_{ij,t-1}$ and the 10-year sovereign bond yield differentials $\Delta YIELD_{ij,t-1}$ (column 7). Finally, column (8) reports the regression results with all controls included.

Table 5.1 indicates that, across all specifications, the coefficient on the output differential is rather stable and in the interval 0.45-0.54. This indicates that, on average over the full sample, about 50% of idiosyncratic output shocks are smoothed in the euro zone, while the remaining 50% is unsmoothed.

The risk-sharing coefficient appears to be larger than other estimates in the literature. For example, using the Asdrubali et al. (1996) approach in a sample of EA11 countries, Alcidi and Thirion (2017) obtain risk-sharing estimates of 42 percent between 1998 and 2007, 55 percent between 2008 and 2009 and 16 percent between 2010-2013. With the same methodology of Asdrubali et al. (1996), Milano (2017) places the estimate around 23% for the period 1999-2006 and 31% for the period 2007-2014. The difference between our result and those obtained in other studies stems mainly from the definition of the consumption aggregate used in the estimation; while we opt for household final consumption expenditure (including the consumption of non-profit institutions serving households), other risk-sharing regressions rely on aggregate consumption data that also includes government final consumption expenditure.

The VAT and PIT rate differentials show a negative coefficient. Indeed, an increase in these tax rates in country $i$ is expected to lead to a decrease in consumption in that country relative to country $j$, which is reflected in a negative sign of the related coefficients. As regards the other controls, the
coefficient on the bond yield differentials is negative indicating a depressive effect of this variable on consumption, whereas domestic credit growth differentials are associated with diverging relative consumption growth rates. In other words, an increase in domestic credit rate in country \( i \) relative to the domestic credit rate in country \( j \) is associated with an increase in consumption of country \( i \) relative to country \( j \). Once we include all controls at once the tax differentials remain negative but lose statistical significance. Given that we want to use the most general specification, in the remainder we will use as baseline model the one with all controls (column (8)).

5.6.2 The effects of financial and fiscal integration on risk sharing

Table 5.2 reports the results from richer specifications, in which several interaction terms have been added to the baseline specification of Table 5.1 (column 8). In particular, as in equation 5.3, we interact the output growth differential with (i) international financial assistance, as represented by EFSF loans between any two euro zone countries as well as funds channelled through the ESM, and (ii) the terms representing financial integration (i.e., sum of bilateral bank loans and bilateral portfolio holdings). The underlying intuition behind this interacted variable regression is that, in this framework, idiosyncratic output shocks may affect consumption depending on the level of the interacted variables. For example, output shocks may have smaller (bigger) effects on consumption if financial integration is higher (weaker).

In addition to interacted terms, we deem appropriate to always include country-pairs fixed effects and year fixed effects to account for unobserved country pair characteristics (such as distance, or similarity in language or institutional and legal arrangements), as well as common euro area-wide factors (e.g., the European sovereign debt crisis, which started in 2010).

In column (1) we augment the simple specification of equation (5.2) only with the interaction term based on the international financial assistance \( EFSF_{ij,t-1} \). The results show that the coefficient on the EFSF-ESM loans is large, negative and highly significant, thus contributing to push the
coefficient on output growth differentials towards zero. This indicates that since 2010 (when the EFSF loans were activated), financial assistance has contributed in a statistically significant way to risk sharing in the euro zone. In column (2) we only include the interaction term based on the overall financial integration index \( FIN_{ij,t-1} \). The regression results suggest a statistically insignificant effect of financial integration in reducing consumption growth differentials across countries. This finding is also found when controlling simultaneously for the EFSF-ESM assistance (column (3)). Given that the true effects of financial integration might differ across the different financial instruments, we then add separately the interaction terms for bilateral bank loans in column (4), for bilateral portfolio holdings in column (5), and include loans and portfolio holdings jointly in column (6). Finally, in column (7) we account for differential effects of capital and credit markets by breaking the portfolio term into its equity and debt components and simultaneously add all three financial integration terms together with the EFSF-ESM fiscal assistance measure.

The results show that bilateral bank loans are either insignificant or tend to decrease consumption risk sharing, as reflected by the positive interaction coefficient in columns (6) and (7). This result may appear counter-intuitive, but it is consistent with a pro-cyclical behaviour of cross-border lending. More specifically, if a country pair is hit by asymmetric output shocks leading to an increase in the output growth differential, a high degree of integrated banks distributing cross-country lending in a pro-cyclical way will result in increase in the consumption growth differential.\(^{15}\) The latter effect is reflected in a higher \( \beta_t \) or lower consumption risk sharing. Table 5.2 also suggests that portfolio holdings have the expected negative sign, which would indicate their role in bringing about more risk sharing. This result is mostly driven by equity holdings. Our finding corroborates other studies in the literature that indicate the positive role of cross-border equity holdings in reducing consumption growth differentials (see, for example, Schmitz, 2010; Fratzscher and Imbs, 2009). The coefficients of the other control variables tend to preserve the sign shown in Table 5.1: PIT rate differentials and differentials in VAT changes are always negative (although the coefficient remains statistically insignificant). The contribution of bond yield differentials is also negative. If

\(^{15}\) see Albertazzi and Bottero, 2014 on the pro-cyclicality of lending from foreign banks.
the sovereign risk premium (as captured by the yield differential) increases in country $i$ relative to country $j$, then compared to country $j$ in country $i$ the cost of borrowing will increase. Ceteris paribus, this will exert downward pressure on the growth of consumption in country $i$ relative to $j$. At the same time, the differentials in domestic credit growth rates still enter the regression with a positive sign, indicating that consumption will grow faster in country $i$ relative to country $j$ when domestic credit in the former economy expands faster than in the latter.

We also evaluate the degree of shock absorption at specific values for the financial and fiscal integration indices. The latter is defined as $(1 - \beta_t)$ where $\beta_t$ is the overall risk-sharing coefficient included in equation (4) and based on the estimates in column (7) of Table 5.2. A value of one corresponds to full-risk sharing (perfect shock absorption) while a value of zero indicates no shock absorption. Indeed, an important main value added of our analysis is that it allows deriving a time-varying estimate for the degree of risk-sharing (or shock absorption) in the EMU. The figure indicates that the degree of shock absorption increased from around 40% (of country-specific output shocks) in the early 2000s, to around 65% the end of the sample. This reveals - perhaps surprisingly - that risk sharing has progressively improved in the EMU, and also during the recent crisis period.

Our framework also allows to pin down the relative contribution of each factor in explaining the time evolution in the degree of shock absorption. Indeed, Figure 5.3 reports the individual contribution of cross-border bank loans, cross-border holdings of equity and debt (i.e., portfolio) and official EFSF-ESM assistance to the time evolution of $(1 - \beta_t)$. It turns out that portfolio integration has been increasingly important as a shock absorber, until 2008. In 2009, at the beginning of the crisis, the coefficient on portfolio slightly increases, thus revealing a less positive contribution to risk sharing in that year. This might be possibly due to the fact that the 2009 recessionary shock hit all countries (and financial markets) simultaneously in the euro zone, therefore cross-border holdings of financial assets did not benefit households and consumers as the scope for risk sharing was reduced. Since 2010, the contribution of international portfolio holdings has been broadly stable. Financial assistance channelled through European institutions has been a very important
shock-absorber mechanism since 2010, when such loans were activated. Indeed, the EFSF and ESM assistance is the largest component, amounting in the last part of the sample to around 0.18 p.p. of the total change in shock absorption (around 0.25 p.p.). Finally, cross-border loans have contributed negatively, and in a rather stable way, to risk sharing. However, the impact of this factor seems to be rather small over the full sample (less than 5 p.p.) and has become less powerful since 2008, as reflected in progressive decline of the loan integration measure shown in Figure 5.1.

5.6.3 Risk sharing links between “Core" and “Periphery"

In this section, we zoom in into the risk-sharing links between “Periphery" and “Core" countries within the EMU. The first group includes the euro zone “vulnerable" countries, i.e., the one most hit by the recent crisis: Greece, Portugal, Ireland, Spain and Italy. The second group includes Austria, Belgium, Germany, Finland, France and the Netherlands. In fact, financial assistance has been mainly directed from the “Core" to the “Periphery", therefore exploring the links between these two groups of countries is in our view interesting. Column (2) of Table 5.3 presents the results for a panel model in which country pairs consist of one core and one periphery country. As a further robustness check, in column (3) we report the results in which Italy is assigned to the group of core countries instead of the peripheral countries. In both columns the coefficient of EFSF-ESM financial assistance remains negative and significant, although smaller in size relative to the baseline. The coefficient on bank loans integration maintains a positive and significant sign, while equity holdings contribute more significantly to risk sharing as compared to the all sample. Interestingly, the coefficient on debt holdings is now negative and statistically significant, suggesting that cross-border holding of debt may have led to stronger shock absorption that in the full sample, which includes country pairs of similar countries (i.e, Core-Core and Periphery-Periphery).

The differences in the coefficients are better reflected by the full effect estimated in the two country groups we consider, with the result presented in Figure 5.4. The shock absorption indicator \((1 - \beta_t)\) based on the full sample lies above the coefficient for the core-periphery group, indicating
a lower elasticity of relative consumption growth to relative income growth in more homogeneous country pairs. This indicates that, overall, risk sharing seems to have been more effective between similar group of countries (which are included in the full sample estimate), than between Core and Periphery countries. These differences in the degree of risk sharing are however minor, given that \((1 - \beta_t)\) is always included in the confidence bands of the full sample estimate.

### 5.6.4 Robustness checks

To test if alternative estimation methods would affect significantly these results, in Table 5.C.1 of Appendix 5.7 we replicate the last column of Table 5.2, using standard OLS with fixed effects (column 2), OLS with Driscoll-Kraay standard errors (column 3) and feasible GLS with panel-specific AR(1) autocorrelation in the error term (column 4). The Driscoll and Kraay (1998) standard error correction accounts for general forms of both cross-sectional and time correlation, whereas the feasible GLS estimation allows for panel-specific autocorrelation of order one in the errors. The results are consistent with the baseline estimates we find in Table 5.2 (replicated in column (1) of Table 5.C.1 for ease of comparison) in both sign and magnitude.

Columns (5) to (8) of Table 5.C.1 also show the results when we expand the nonlinear model with the level of the financial and fiscal assistance integration proxies. Results show that the latter are generally not statistically significant. Most importantly, our main estimates on the interaction terms remain almost identical both in size and statistical significance.

In Figure 5.C.1 of the Appendix we show the overall shock absorption indicator \((1 - \beta_t)\) when we exclude from the estimation one country at the time. Interestingly, we observe that although the exclusion of Greece does not induce a statistically significant shift in the relative elasticity estimate, it does lead to a flatter time path of the estimated coefficient across the full period. We can therefore claim that the presence of Greece in the euro area resulted in a slightly lower level of consumption insurance in the early years of the monetary union, and a higher one after 2011. This may be related
to the low level of financial integration observed in Greece throughout the period, which in part offset the positive effect through the participation of Greece in the EFSF-ESM assistance program.

We also observe that the exclusion of Ireland results in a lower shock absorption since the start of the financial crisis, although the effect is still within the confidence bands of the full-sample estimate (Figure 5.C.1). Although Ireland was a net receiver of EFSF and ESM assistance, its positive impact on the aggregate risk-sharing coefficient seems to be mainly driven by the fact that this country was highly financially integrated with the rest of euro zone countries, in particular through equity holdings.

5.7 Conclusion

Many commentators have argued that the effects of the financial crisis and the European sovereign debt crisis have been aggravated by the absence of appropriate risk-sharing mechanisms within the EMU. In this paper, we propose a novel approach aimed at gauging the extent of consumption risk sharing, and its main drivers, among member countries since the start of the EMU. In particular, based on a sample of 11 euro zone countries for the period 1999-2015, we explore the role of private channels (i.e., cross-border loans and holdings of financial assets) versus public channels (i.e., official financial assistance to distressed euro area countries) for consumption risk sharing.

Our results suggest that the shock-absorption capacity generated by international (private and public) channels has increased since the start of the EMU: in the early years of the EMU only about 40% of idiosyncratic output shocks were smoothed, while in the aftermath of the euro zone’s sovereign debt crisis around 65% of idiosyncratic output shocks were absorbed. Both financial integration and international official assistance play an important role in explaining this improvement. At the same time, our results show that while banking integration (via cross-border loans) tends to be ineffective or even to somewhat exacerbate country differences in consumption growth, cross-border holdings of equities and debt are powerful channels in isolating households
from country-specific shocks. This latter finding is particularly strong when we focus on the links between “Core” and “Periphery” EMU countries: holdings of debt and equity issued by Core countries and in the portfolio of agents in the “Periphery” (and vice versa) turn out to be effective in absorbing output shocks, thus allowing to better smooth consumption.

The finding that risk sharing has improved over time in the euro zone, also during the recent crisis, is to some extent surprising. Yet, this result does not imply that the severity of the crisis would have not been attenuated even further by a fully-fledged centralized fiscal capacity at the euro zone level.
Table 5.1: Simple risk-sharing regression model

<table>
<thead>
<tr>
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<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
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<th>(5)</th>
<th>(6)</th>
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</thead>
<tbody>
<tr>
<td>$\Delta \log Y_{ij} - \Delta \log Y_{ij}$</td>
<td>0.521***</td>
<td>0.537***</td>
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<td>0.515***</td>
<td>0.476***</td>
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<tr>
<td></td>
<td>(0.114)</td>
<td>(0.118)</td>
<td>(0.128)</td>
<td>(0.127)</td>
<td>(0.130)</td>
<td>(0.079)</td>
<td>(0.102)</td>
<td>(0.070)</td>
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<td>$\Delta VAT_{ij,t-1}$</td>
<td>-0.240*</td>
<td>-0.067</td>
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<td></td>
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<td>(0.16)</td>
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<tr>
<td>$\Delta PIT_{ij,t-1}$</td>
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<td>0.020</td>
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<td></td>
<td>(0.053)</td>
<td>(0.036)</td>
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<tr>
<td>$\Delta INF_{ij,t-1}$</td>
<td></td>
<td></td>
<td>0.131</td>
<td>-0.005</td>
<td></td>
<td></td>
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<tr>
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<tr>
<td>$\Delta DCREDIT_{ij,t-1}$</td>
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<td>0.134***</td>
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<td></td>
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<td>(0.028)</td>
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<td>Constant</td>
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<td>0.587**</td>
<td>0.538*</td>
<td>0.681**</td>
<td>-0.343</td>
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<td>(0.266)</td>
<td>(0.309)</td>
<td>(0.341)</td>
<td>(0.355)</td>
<td>(0.274)</td>
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Observations 870 870 870 870 870 815 815 760
Country pairs 55 55 55 55 55 55 55 55
Countries 11 11 11 11 11 11 11 11
Country pair FE NO YES NO YES YES YES YES
Year FE NO NO YES YES YES YES YES YES

Notes: OLS estimation with clustered standard errors for dyadic data (in parenthesis) of equation 5.2. ***, ** and * refer to the 1%, 5% and 10% statistical significance.
Table 5.2: Risk sharing in the euro area: the role of financial integration and EFSF-ESM assistance

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<th>(5)</th>
<th>(6)</th>
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<tr>
<td>$\Delta \log Y_{it} - \Delta \log Y_{jt}$</td>
<td>0.491***</td>
<td>0.557***</td>
<td>0.581***</td>
<td>0.478***</td>
<td>0.641***</td>
<td>0.635***</td>
<td>0.623***</td>
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<tr>
<td>($\Delta \log Y_{it} - \Delta \log Y_{jt})EFSF_{ij,t-1}$</td>
<td>-0.575***</td>
<td>-0.577***</td>
<td>-0.607***</td>
<td>-0.489**</td>
<td>-0.508***</td>
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<td>($\Delta \log Y_{it} - \Delta \log Y_{jt})PORT_{ij,t-1}$</td>
<td>-0.0251**</td>
<td>-0.0317***</td>
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<td>(0.012)</td>
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<td>$\Delta VAT_{ij,t-1}$</td>
<td>-0.0437</td>
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<td>-0.0566</td>
<td>-0.0603</td>
<td>-0.041</td>
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<td>(0.169)</td>
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<td>$\Delta INF_{ij,t-1}$</td>
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<td>0.0563</td>
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<td>0.0357</td>
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<td>$\Delta YIELD_{ij,t-1}$</td>
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<td>-0.00384</td>
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<td>-0.129**</td>
<td>-0.134**</td>
<td>-0.179***</td>
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<td>$\Delta DCREDIT_{ij,t-1}$</td>
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<td>0.118***</td>
<td>0.105***</td>
<td>0.111***</td>
<td>0.106***</td>
<td>0.0976***</td>
<td>0.0964***</td>
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<td>Constant</td>
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<td>0.337</td>
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<td>11</td>
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Notes: OLS estimation with clustered standard errors for dyadic data (in parenthesis) of equation 5.4. ***, ** and * refer to the 1%, 5% and 10% statistical significance.
Table 5.3: Risk sharing between “Core” and “Periphery”

<table>
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<tr>
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<th>Core-Periphery</th>
<th>Core-Periphery (IT in Core)</th>
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<tbody>
<tr>
<td>( \Delta \log Y_{i,t} - \Delta \log Y_{j,t} )</td>
<td>0.623***</td>
<td>0.687***</td>
<td>0.696***</td>
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<tr>
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<td>(0.087)</td>
<td>(0.083)</td>
<td>(0.079)</td>
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<td>( \Delta \log Y_{i,t} - \Delta \log Y_{j,t} ) (EFSF_{ij,t-1} )</td>
<td>-0.587***</td>
<td>-0.383***</td>
<td>-0.376***</td>
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<tr>
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<td>(0.211)</td>
<td>(0.123)</td>
<td>(0.122)</td>
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<tr>
<td>( \Delta \log Y_{i,t} - \Delta \log Y_{j,t} ) (LOAN_{ij,t-1} )</td>
<td>0.026**</td>
<td>0.036**</td>
<td>0.032**</td>
</tr>
<tr>
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<td>(0.011)</td>
<td>(0.015)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>( \Delta \log Y_{i,t} - \Delta \log Y_{j,t} ) (DEBT_{ij,t-1} )</td>
<td>-0.0292</td>
<td>-0.0479***</td>
<td>-0.0389*</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.017)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>( \Delta \log Y_{i,t} - \Delta \log Y_{j,t} ) (EQUITY_{ij,t-1} )</td>
<td>-0.0310**</td>
<td>-0.0434***</td>
<td>-0.0516***</td>
</tr>
<tr>
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<td>(0.0137)</td>
<td>(0.012)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>( \Delta VAT_{ij,t-1} )</td>
<td>-0.0219</td>
<td>-0.032</td>
<td>-0.0316</td>
</tr>
<tr>
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<td>(0.156)</td>
<td>(0.138)</td>
<td>(0.135)</td>
</tr>
<tr>
<td>( \Delta PIT_{ij,t-1} )</td>
<td>-0.0118</td>
<td>-0.0183</td>
<td>-0.0173</td>
</tr>
<tr>
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<td>(0.025)</td>
<td>(0.029)</td>
<td>(0.034)</td>
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<tr>
<td>( \Delta INF_{ij,t-1} )</td>
<td>0.0672</td>
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<tr>
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<td>(0.118)</td>
<td>(0.116)</td>
<td>(0.120)</td>
</tr>
<tr>
<td>( \Delta YIELD_{ij,t-1} )</td>
<td>-0.179***</td>
<td>-0.114*</td>
<td>-0.0759</td>
</tr>
<tr>
<td></td>
<td>(0.045)</td>
<td>(0.062)</td>
<td>(0.080)</td>
</tr>
<tr>
<td>( \Delta DCREDIT_{ij,t-1} )</td>
<td>0.0964***</td>
<td>0.101***</td>
<td>0.105***</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.031)</td>
<td>(0.033)</td>
</tr>
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</table>

Observations 715 394 361
Unique country pairs 55 30 28
Countries 11 11 11

Notes: OLS estimation with robust standard errors for dyadic data (in parenthesis) of equation (3). ***, ** and * refer to the 1%, 5% and 10% statistical significance. “All” refers to all pairs across EMU countries and “Core-Periphery” refers to all unique country pairs consisting of “Core” and “Periphery” countries. “Periphery” refers to vulnerable countries in the euro area (Greece, Spain, Italy, Portugal and Ireland) and “Core” refers to resilient countries in the euro area (Germany, the Netherlands, Belgium, Austria and Finland. In column (3) Italy is moved from the “Periphery” to the “Core” group. All regressions include country-pair and year fixed effects.
Notes: Annual country-pair averages in percentage points of GDP. “LOAN”, “EQUITY”, “DEBT” and “EFSF-ESM” are defined as the sum of the relevant bilateral exposure of country $i$ in country $j$ and the bilateral exposure of country $j$ in country $i$ over the sum of the GDP of countries $i$ and $j$. “LOAN” refers to data on cross-border bank lending from the Bank of International Settlements, “EQUITY” and “DEBT” mark the corresponding components of the IMF Coordinated Portfolio Investment Survey, and “EFSF-ESM” is official financial assistance through bilateral loans, as well as EFSF and ESM funds.
Figure 5.2: Degree of shock absorption in the EMU

Notes: The figure plots the degree of shock absorption defined as $(1 - \beta_t)$, where $\beta_t$ is the overall risk-sharing coefficient defined in equation (4) and based on the estimates in column (7) of Table 5.2. A value of one corresponds to full-risk sharing (full shock absorption of idiosyncratic output shocks), a value of zero indicates no shock absorption. The interaction terms are evaluated at their annual country-pair means (see Figure 1). Confidence bands correspond to the 90% level of statistical significance and are constructed using cluster-robust standard errors accounting for dyadic data.
Figure 5.3: Contributions to changes in the degree of shock absorption

Notes: The figure plots the contributions to the variation in shock absorption of the cross-border loans (LOAN), cross-border portfolio holdings (PORTFOLIO) and EFSF-ESM assistance (EFSF-ESM). These contributions are based on the estimates reported in column (7) of Table 2. “LOAN” is calculated as $-\beta_1 \Delta \text{LOAN}_{t-1}$; “PORTFOLIO” is calculated as $-\beta_2 \Delta \text{EQUITY}_{t-1} - \beta_3 \Delta \text{DEBT}_{t-1}$; “EFSF-ESM” is calculated as $-\beta_4 \Delta \text{EFSF}_{t-1}$. The interaction terms are evaluated at their annual country-pair means (see Figure 5.1).
Figure 5.4: Evolution of risk sharing between “Core” and “Periphery”

Notes: See Notes of Figure 5.2. We evaluate the non-linear effect in each subsample by using the relevant column of Table 5.3 and fixing the interacting variables to equal the annual averages of bilateral financial and fiscal integration computed in the corresponding sub-sample of country pairs.
Appendix Chapter 5

5.A Fiscal integration measures in EA11

We present country-level annual averages of financial integration indices across all partners, in percentage points of GDP. “LOAN”, “EQUITY”, “DEBT” and “EFSF-ESM” are defined as the sum of the relevant bilateral exposure of country $i$ in country $j$ and the bilateral exposure of country $j$ in country $i$ over the sum of the GDP of countries $i$ and $j$. “LOAN” refers to data on cross-border bank lending from the Bank of International Settlements, “EQUITY” and “DEBT” refer to the corresponding components of the IMF Coordinated Portfolio Investment Survey, and “EFSF-ESM” is official financial assistance through bilateral loans, EFSF and ESM funds.
## 5.B Data sources

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<tr>
<th>Variable</th>
<th>Source</th>
<th>Definition</th>
<th>Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bilateral portfolio equity and debt stocks</td>
<td>Coordinated Portfolio Survey (CPIS) – International Monetary Fund (IMF)</td>
<td>Cross-border holdings of equities and debt securities self-reported by holder economies and classified by the economy of residence of the issuer, in US dollars.</td>
<td>2001-2015</td>
</tr>
<tr>
<td>Bilateral EFSF and ESM</td>
<td>European Stability Mechanism (ESM) and European Central Bank (ECB)</td>
<td>EFSF and ESM loan disbursements and country contribution keys, in US dollars. To construct the bilateral flows, we multiply the amount withdrawn by each country with the capital keys of all contributors. When the year of payment into the fund is different from the year of withdrawal, we record the bilateral flow at the time when a given recipient (GR, ES) withdraws some funds. For the period before 2010 we set all values to zero.</td>
<td>1999-2015</td>
</tr>
<tr>
<td>Household consumption</td>
<td>ECB Statistical Data Warehouse (SDW)</td>
<td>Final consumption expenditure of households and non-profit institutions serving households. Current prices, million euro. Used only for Ireland.</td>
<td>1999-2015</td>
</tr>
<tr>
<td>Indicator</td>
<td>Source</td>
<td>Description</td>
<td>Time Period</td>
</tr>
<tr>
<td>-----------------------------------------------</td>
<td>---------------------------------------------</td>
<td>-----------------------------------------------------------------------------</td>
<td>-------------</td>
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<tr>
<td>Population</td>
<td>Eurostat</td>
<td>Total population national concept, Thousand persons, Seasonally and calendar adjusted data. Quarterly, aggregated to annual (as averages).</td>
<td>1999-2015</td>
</tr>
<tr>
<td>Value added tax</td>
<td>OECD Tax Database Table 2.A2.1</td>
<td>Standard Value Added Tax rate (General Sales Tax) - Annual.</td>
<td>2000-2015</td>
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<td>Statutory tax on dividend income (PIT)</td>
<td>OECD Tax Database Table II.4</td>
<td>Overall statutory tax rate on dividend income (Sum of the rate on distributed profit and the rate on grossed-up dividend). Annual.</td>
<td>2000-2015</td>
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<td>Domestic credit</td>
<td>Bank for International Settlements (Long series on total credit to the non-financial sectors)</td>
<td>Total credit by domestic banks to the private non-financial sector. The original series are market value, billion US Dollar, unadjusted (quarterly, aggregated to annual as averages).</td>
<td>1999– 2015</td>
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<td>Long-term (10Y) sovereign bond yield</td>
<td>OECD MEI (Main Economic Indicators)</td>
<td>Long-term (10Y) sovereign bond yield, not seasonally adjusted (quarterly, aggregated to annual as averages).</td>
<td>1999-2015</td>
</tr>
<tr>
<td>Consumer Price Index</td>
<td>OECD MEI (Main Economic Indicators)</td>
<td>Consumer Price Index, All items (Index 2010=100). Quarterly, aggregated to annual (as averages). We calculate inflation as the growth rate in the Consumer Price Index.</td>
<td>1999-2015</td>
</tr>
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<td>US/EUR exchange rate</td>
<td>ECB Statistical Data Warehouse (SDW)</td>
<td>ECB reference exchange rate, USD/EUR. Monthly, aggregated to annual (as averages). We use it to multiply GDP (in EUR) to match the currency of financial data (USD).</td>
<td>1999-2015</td>
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## 5.C Robustness checks on baseline model

Table 5.C.1: Results with different estimation methods and including levels of interacted variables

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<th>DK</th>
<th>GLS</th>
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<td>Δlog Y_i - Δlog Y_j</td>
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<td>(0.0894)</td>
<td>(0.0617)</td>
<td>(0.0768)</td>
<td>(0.0309)</td>
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<td>Δ(log Y_i - log Y_j)EFSF_{ij,t-1}</td>
<td>-0.587***</td>
<td>-0.612***</td>
<td>-0.625***</td>
<td>-0.619***</td>
<td>-0.611***</td>
<td>-0.651***</td>
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<td>(0.115)</td>
<td>(0.118)</td>
<td>(0.221)</td>
<td>(0.156)</td>
<td>(0.123)</td>
<td>(0.121)</td>
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<td>0.027***</td>
<td>0.026</td>
<td>0.028***</td>
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<td>0.026***</td>
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<td>-0.027***</td>
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<td>(0.0225)</td>
<td>(0.0103)</td>
<td>(0.0216)</td>
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<td>(0.0225)</td>
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<td>Δ(log Y_i - log Y_j)EQUITY_{ij,t-1}</td>
<td>-0.031**</td>
<td>-0.031**</td>
<td>-0.032***</td>
<td>-0.035***</td>
<td>-0.031**</td>
<td>-0.031**</td>
<td>-0.032***</td>
<td>-0.035***</td>
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<td>EFSF_{ij,t-1}</td>
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<td></td>
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<td></td>
<td>(0.0323)</td>
<td>(0.0758)</td>
<td>(0.0537)</td>
<td>(0.0711)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>DEBT_{ij,t-1}</td>
<td></td>
<td></td>
<td>-0.072</td>
<td>-0.111</td>
<td>-0.157</td>
<td>-0.179*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.0902)</td>
<td>-0.149</td>
<td>-0.13</td>
<td>-0.104</td>
<td></td>
<td></td>
</tr>
<tr>
<td>EQUITY_{ij,t-1}</td>
<td></td>
<td></td>
<td>0.046</td>
<td>0.034</td>
<td>0.058</td>
<td>0.063</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.0442)</td>
<td>(0.0774)</td>
<td>(0.0691)</td>
<td>(0.0763)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>ΔVAT_{ij,t-1}</td>
<td>-0.022</td>
<td>-0.024</td>
<td>-0.057</td>
<td>-0.008</td>
<td>-0.020</td>
<td>-0.023</td>
<td>-0.06</td>
<td>-0.005</td>
</tr>
<tr>
<td></td>
<td>(0.156)</td>
<td>(0.0681)</td>
<td>(0.12)</td>
<td>(0.0698)</td>
<td>(0.162)</td>
<td>(0.0699)</td>
<td>(0.12)</td>
<td>(0.0697)</td>
</tr>
<tr>
<td>ΔPIT_{ij,t-1}</td>
<td>-0.012</td>
<td>-0.010</td>
<td>-0.013</td>
<td>-0.002</td>
<td>-0.012</td>
<td>-0.011</td>
<td>-0.014</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.0246)</td>
<td>(0.013)</td>
<td>(0.0172)</td>
<td>(0.0145)</td>
<td>(0.0248)</td>
<td>(0.0127)</td>
<td>(0.0167)</td>
<td>(0.0145)</td>
</tr>
<tr>
<td>ΔNFL_{ij,t-1}</td>
<td>0.067</td>
<td>0.071</td>
<td>0.097</td>
<td>0.0537</td>
<td>0.068</td>
<td>0.073</td>
<td>0.101</td>
<td>0.058</td>
</tr>
<tr>
<td></td>
<td>(0.118)</td>
<td>(0.0631)</td>
<td>(0.114)</td>
<td>(0.0537)</td>
<td>(0.119)</td>
<td>(0.0665)</td>
<td>(0.104)</td>
<td>(0.054)</td>
</tr>
<tr>
<td>ΔYIELD_{ij,t-1}</td>
<td>-0.179***</td>
<td>-0.179***</td>
<td>-0.183**</td>
<td>-0.170***</td>
<td>-0.180***</td>
<td>-0.181***</td>
<td>-0.186**</td>
<td>-0.172***</td>
</tr>
<tr>
<td></td>
<td>(0.0448)</td>
<td>(0.0542)</td>
<td>(0.0801)</td>
<td>(0.0486)</td>
<td>(0.0493)</td>
<td>(0.0565)</td>
<td>(0.0824)</td>
<td>(0.0489)</td>
</tr>
<tr>
<td>ΔDCREDIT_{ij,t-1}</td>
<td>0.096***</td>
<td>0.096***</td>
<td>0.103***</td>
<td>0.105***</td>
<td>0.097***</td>
<td>0.097***</td>
<td>0.105***</td>
<td>0.106***</td>
</tr>
<tr>
<td></td>
<td>(0.0253)</td>
<td>(0.0128)</td>
<td>(0.0205)</td>
<td>(0.01)</td>
<td>(0.0261)</td>
<td>(0.0134)</td>
<td>(0.0201)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.202</td>
<td>0.0768</td>
<td>3.990**</td>
<td>0.179</td>
<td>0.168</td>
<td>0.0985</td>
<td>4.401**</td>
<td>0.116</td>
</tr>
<tr>
<td></td>
<td>(0.254)</td>
<td>(0.18)</td>
<td>(1.346)</td>
<td>(0.795)</td>
<td>(0.263)</td>
<td>(0.228)</td>
<td>(1.483)</td>
<td>(0.81)</td>
</tr>
</tbody>
</table>

Notes: The first four columns correspond to the estimation of the specification in Table 5.2 column (7) using different econometric techniques. The first column uses OLS estimation with heteroscedasticity-robust standard errors, the second corresponds to OLS estimation with Driscoll-Kraay corrected standard errors (for general cross-sectional correlation and autocorrelation up to lag 4), the third corresponds to GLS estimation with country pair-specific autocorrelation in the standard errors. The columns indexed with (8) refer to the same specifications, where we also include the levels of the interaction variables (coefficients generally not statistically significant and not reported).
Notes: See Notes of Figure 5.2. We evaluate the non-linear effect in each subsample by using the relevant estimates of equation (5.3) and fixing the interacting variables to equal the annual averages of bilateral financial and fiscal integration computed in the corresponding sub-sample of country pairs.
Chapter 6

Summary

This dissertation introduces a new dataset on fiscal austerity announcements and studies three questions related to fiscal policy in developed economies. In order to obtain an accurate measure of fiscal austerity shocks, we construct a monthly dataset of consolidation announcements, described in Chapter 2. We use this data in Chapter 3 to measure the effects of fiscal austerity on market expectations. We also use the data in Chapter 4, where we contrast the announcement of fiscal policy measures with their implementation, controlling for the role of expectations in the transmission of fiscal shocks to the real sector. In Chapter 5 we tackle an additional question, namely the contribution of financial integration versus international financial assistance as channels for consumption risk sharing in the European Monetary Union (EMU).

The work presented in Chapter 3 starts from an existing narrative dataset of fiscal consolidation plans. Panel regressions based on the annual action-based datasets of Pescatori et al. (2011) and Alesina et al. (2015a) show that consolidations affect confidence negatively. The main driver of changes in private sector expectations is the surprise component in the implementation of a consolidation plan. Then, we refine the analysis by using our monthly data on fiscal consolidation announcements. The results suggest that consumer confidence falls around austerity announcements, an effect likely driven by revenue-based measures. Long-term interest rates, as a measure of confidence in the sovereign, tend to fall around spending-based consolidation announcements.
Moreover, these effects are highly relevant for European countries with weak institutional arrangements, as measured by the tightness of fiscal rules or budgetary transparency.

Chapter 4 proposes a new explanation for the asymmetry between the output effects of fiscal consolidation plans that consist predominantly of tax hikes and those characterized mostly by spending cuts. We compare the *ex ante* announced impact of a fiscal consolidation with the *ex post* change in the revenue and expenditure components of the government budget, and conclude that follow-up is weaker for spending than for revenue-based austerity plans. We confirm this finding with both annual data on fiscal consolidation plans and and quarterly narrative data on austerity announcements. Both (i) the difference in follow-up and (ii) the difference in the combination of measures included in revenue- and spending-based consolidations play a role in accounting for the heterogeneous effects of the fiscal plans.

In Chapter 5 we investigate the contribution of financial integration versus international financial assistance as channels for consumption risk sharing in the European Monetary Union (EMU). Cross-border holdings of equities and debt seem to be more effective than cross-border bank loans in isolating households from country-specific shocks. Regarding public risk-sharing, we investigate fiscal integration in the form of channeling official loans to distressed euro zone economies. We conclude that in the aftermath of the euro zone sovereign debt crisis, the European Financial Stability Facility (EFSF) and the European Stability Mechanism (ESM) have contributed to the reduction in household consumption growth differentials across the EMU.

Based on the work reunited in this thesis we can conclude that fiscal austerity is on average more contractionary when it consists mostly of tax adjustments. This is partly due to the negative effects that revenue-based fiscal consolidation announcements exert on private sector confidence, and in part due to the fact that for tax hikes the gap between announcement and implementation is smaller than for public spending cuts. We also find that in a monetary union affected by asymmetric shocks,
a supranational mechanism that implements cross-border fiscal transfers can improve consumption risk-sharing.
Bibliography


Nederlandse Samenvatting

Dit proefschrift introduceert een nieuwe dataset over fiscale bezuinigingsaankondigingen en bestudeert drie vragen met betrekking tot fiscaal beleid in ontwikkelde economieën. Om een nauwkeurige meting van schokken in de fiscale bezuinigingen te verkrijgen, construeren we een dataset met maandelijkse data over consolidatieaankondigingen. Deze dataset is beschreven in hoofdstuk 1. In hoofdstuk 2 hanteren wij deze dataset om de effecten te testen van de aangekondigde fiscale bezuinigingen op het consumentenvertrouwen. Vervolgens, in hoofdstuk 3, maken we onderscheidt tussen aankondiging van fiscale beleidsmaatregelen en de uiteindelijke beleidsimplementatie, en analyseren wij of de verwachtingen van daadwerkelijke implementatie van dergelijke fiscale bezuiniging effect heeft op de reële sector. In hoofdstuk 4 behandelen we een aanvullende vraag, namelijk de bijdrage van financiële integratie in tegenstelling tot internationale financiële bijstand als kanalen voor het risicodelen van consumptie binnen de Europese Monetaire Unie (EMU).

Het werk gepresenteerd in hoofdstuk 3 gaat verder vanuit de geconstrueerde dataset over fiscale consolidatieplannen. Doormiddel panelregressie analyses gebaseerd op datasets met jaarlijkse data van Pescatori et al. (2011) en Alesina et al. (2015a) tonen we aan dat fiscale consolidaties het consumentenvertrouwen negatief beïnvloeden. De belangrijkste reden voor veranderingen in de verwachtingen van de desbetreffende sector is de verrassingscomponent bij de uitvoering van een consolidatieplan. Vervolgens verfijnen we de analyse door onze maandelijkse gegevens over aankondigingen van begrotingsconsolidatie te hanteren.
Onze resultaten tonen aan dat het consumentenvertrouwen daalt tijdens bezuinigingsaankondigingen, een effect dat waarschijnlijk wordt veroorzaakt door inkomsten gerelateerde maatregelen. De langetermijnrente, als maatstaf voor het vertrouwen in de overheid, heeft de neiging om te dalen tijdens uitgaven gebaseerde consolidatieaankondigingen. Bovendien zijn deze effecten van groot belang voor Europese landen met zwakke institutionele regelingen, gemeten aan de hand van de krappe speling binnen de begrotingsregels of budgettaire transparantie.

Hoofdstuk 4 laat nieuw licht schijnen op de asymmetrie tussen de effecten op de reële economie van begrotingsconsolidatieplannen die voornamelijk bestaan uit belastingverhogingen en die plannen die voornamelijk worden gekenmerkt door bezuinigingen. We vergelijken de *ex ante* aangekondigde impact van een begrotingsconsolidatie met de *ex post* veranderingen van de inkomsten- en uitgavencomponenten van de overheidsbegrotingen en concluderen dat de vervolg wijzigingen een zwakker effect hebben op uitgaven gerelateerde bezuinigingsplannen dan op inkomsten gerelateerde bezuinigingsplannen. We bevestigen deze bevinding met zowel jaarlijkse gegevens over begrotingsconsolidatieplannen als kwartaalgegevens over bezuinigingsaankondigingen. Zowel (i) het verschil in vervolgwijzigingen en (ii) het verschil in de combinatie van maatregelen die zijn opgenomen in de inkomsten en/of uitgaven gebaseerde consolidaties spelen een rol bij de verwerking van de heterogene effecten van de fiscale bezuinigingsplannen.

In hoofdstuk 5 onderzoeken wij de bijdrage van financiële integratie in tegenstelling tot internationale financiële bijstand als kanalen voor het risicodelen van consumptie binnen de Europese Monetaire Unie (EMU). Grensoverschrijdende posities in aandelen en schulden lijken effectiever te zijn dan grensoverschrijdende bankleningen bij het isoleren van huishoudens van land specifieke schokken. Wat de publieke risicodeling betreft, onderzoeken wij de fiscale integratie in de vorm van het kanaliseren van officiële leningen aan zwaar getroffen economieën in de eurozone. We concluderen dat in de nasleep van de staatsschuldencrisis in de eurozone, de Europese Faciliteit voor financiële stabiliteit (EFSF) en het Europees Stabiliteitsmechanisme (ESM) hebben bijgedra-
gen aan de vermindering van de verschillen in groei van de consumptieve bestedingen binnen de EMU.

Op basis van de analyses en resultaten die in dit proefschrift geleverd zijn, concluderen wij dat fiscale bezuinigingen gemiddeld meer krimpend van aard zijn indien deze voornamelijk uit belastin- gaanpassingen bestaat. Dit is deels te wijten aan de negatieve effecten die inkomsten-gebaseerde begrotingsconsolidatie aankondigingen hebben op het vertrouwen van de private sector, en deels omdat voor belastingstijgingen het verschil tussen de aangekondigde stijging en de daadwerkelijke stijging kleiner is dan die voor overheidsuitgaven besparingen. We zien ook dat een monetaire unie die wordt getroffen door asymmetrische schokken, een supranationaal mechanisme dat grensoverschrijdende fiscale overdrachten mogelijk maakt de risicodeling van de consumptie kan verbeteren.
The Tinbergen Institute is the Institute for Economic Research, which was founded in 1987 by the Faculties of Economics and Econometrics of the Erasmus University Rotterdam, University of Amsterdam and VU University Amsterdam. The Institute is named after the late Professor Jan Tinbergen, Dutch Nobel Prize laureate in economics in 1969. The Tinbergen Institute is located in Amsterdam and Rotterdam. The following books recently appeared in the Tinbergen Institute Research Series:

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