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Austerity and Private Debt*

Mathias Klein

DIW Berlin

October 13, 2016

ABSTRACT

This study provides empirical evidence that the costs of austerity crucially depend on the level of private indebtedness. In particular, fiscal consolidations lead to severe contractions when implemented in high private debt states. Contrary, fiscal consolidations have no significant effect on economic activity when private debt is low. These results are robust to alternative definitions of private debt overhang, the composition of fiscal consolidations and controlling for the state of the business cycle and government debt overhang. I show that deterioration in household balance sheets is important to understand private debt-dependent effects of austerity.

JEL Codes: C23, E32, E62.

Keywords: Fiscal consolidation, Private debt, Local projection.

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1 Introduction

This study shows that the effects of fiscal consolidations crucially depend on the level of private indebtedness. More specifically, I find that austerity leads to severe contractions in periods of private debt overhang.¹ In contrast, fiscal consolidations have no significant impact on economic activity when private debt is low.

The paper contributes to the literature as it tests for the validity of existing theoretical models which show that private indebtedness matters for the transmission of fiscal policy (for example Andrés, Boscá, and Ferri, 2015; Eggertsson and Krugman, 2012; Kaplan and Violante, 2014). In fact, I provide extensive empirical evidence that confirms predictions of theories pointing out the impact of fiscal policy interventions to be larger in periods of private debt overhang. My results help understanding the dismal growth performances in southern European countries, which implemented large-scale fiscal consolidation programs while confronted with high private debt levels. To the best of my knowledge, this is the first empirical study investigating private debt-dependent effects of fiscal consolidations.

Recent contributions have pointed to the important role of private debt for the propagation and amplification of shocks and policy interventions. In their influential work, Mian and Sufi (2011, 2012) show that those US counties which experienced the largest increase in housing leverage before the financial crisis, suffered from more pronounced economic slack in the postcrisis period. The authors present evidence that deterioration in household balance sheets can explain the large drop in private demand and employment. Jordà, Schularick, and Taylor (2016b) find that more mortgage-intensive credit expansions tend to be followed by deeper recessions and slower recoveries, while this effect is not present for non-mortgage credit booms. Moreover, Jordà, Schularick, and Taylor (2016a) empirically investigate the linkage between private borrowing, public debt burdens and financial instability and find that private credit booms, not excessive public borrowing or the level of public debt, are the main predictors of financial turmoil.

¹Private debt overhang describes periods when private debt-to-GDP ratios are above trend.

Concerning the interrelation between fiscal policy and private debt, Eggertsson and Krugman (2012), Kaplan and Violante (2014) and Andrés, Boscá, and Ferri (2015) demonstrate in theoretical models that the government spending multiplier increases with the level of private indebtedness. Within these models a significant share of households does not maximize lifetime utility due to borrowing constraints. Additionally, borrowing constrained households are characterized by a higher marginal propensity to consume out of income. Combined with price stickiness, Keynesian-type multipliers emerge if the share of these agents is large enough, which in turn depends on the level of private indebtedness.

Another strand of literature investigates state-dependent costs of fiscal consolidations (Born, Müller, and Pfeifer, 2015; Jordà and Taylor, 2016). None of these studies, however, allows the effects to differ according to the private debt level in the economy. This seems surprising given the above mentioned evidence which suggests that the responses to economic innovations are amplified by private debt overhang. Against this background, I provide empirical evidence that the economic consequences of austerity are significantly affected by the level of private indebtedness.

To investigate the effects of fiscal consolidations depending on the state of the economy, I estimate state-dependent impulse responses to exogenous changes in the government budget deficit using local projections as invented by Jordà (2005). The advantages compared to vector autoregressions (VARs) are that local projections are more robust to model misspecification and offer a very convenient way to account for state dependence.² Within the estimation approach, the state of the economy is allowed to vary according to the level of private debt overhang. High debt and low debt states are identified as periods when private debt-to-GDP ratios were respectively above and below trend. To identify fiscal consolidation periods, I use the narrative measure as proposed by Guajardo, Leigh, and Pescatori (2014). The baseline dataset of my analysis covers 12 OECD countries at an annual frequency for the period 1978-2008.

²A more detailed discussion of advantages and disadvantages of the local projection method is given in the next section.

The estimation results show that the responses to fiscal consolidations significantly differ according to the level of private indebtedness. Specifically, the results reveal a significant and severe decline in private consumption and GDP in high debt states. Contrary, in low debt states, private consumption and GDP show a marginal and insignificant reduction. A one percent of GDP fiscal consolidation translates into a 2 percent lower GDP after five years when implemented in a period of private debt overhang. The drop in private consumption is even larger, resulting in a cumulative decline of more than 3 percent. The respective values for fiscal consolidations in low private debt states are 0.7 percent for GDP and 1.1 percent for consumption.

Concerning other important variables, I find that imports and the employment rate significantly decrease in high private debt states, whereas these series do not show any significant effect when private leverage is low. Monetary policy reacts to fiscal consolidations by reducing the real interest rate by a similar magnitude irrespective of the private debt state. Interestingly, the sovereign default risk and the government debt-to-GDP ratio increase significantly after consolidations implemented in a high private debt environment. This finding contradicts to the usual intention of austerity programs which lies in reducing the risk of sovereign default and/or reducing the government debt burden.³

My findings are robust to alternative definitions of debt overhang, different ways of identifying exogenous fiscal consolidation periods and the composition of austerity programs. Moreover, I show that my baseline results prevail when extending the Guajardo, Leigh, and Pescatori (2014) narrative measure for the years 2010-2014. Thus, debt-dependent costs of fiscal consolidations are still present when explicitly taking into account the large-scale austerity programs implemented after the Global Financial Crises. In addition, the results prove to be robust when I condition on the state of the business cycle and government debt overhang.

Allowing the state of the business cycle to differ, I find that fiscal consolidations implemented in periods of high private debt induce economic activity to fall in recessions but also in booms. In expansions and recessions austerity has no significant effect on the

³Complementarily, Born, Müller, and Pfeifer (2015) show that austerity leads to an increase in the sovereign default premium in times of fiscal stress.

economy when private debt is below average. Similar results emerge when controlling for the government debt level. Independent of the government debt level, consolidations induce significant declines in economic activity when private leverage is high. In contrast, consolidations in low private debt states show insignificant effects irrespective of the public debt burden. To sum up, my findings suggest that the costs of austerity are mainly determined by the private debt level in the economy whereas the state of the business cycle and the level of public debt play only a minor role for the effectiveness of fiscal policy.

I highlight two additional results detecting changes in household balance sheets as a possible transmission channel through which my findings can be rationalized. First, by differentiating between household and corporate debt, I show that most of the results are driven by household leverage. While consolidations lead to a significant drop in GDP when households are highly indebted, GDP does not react significantly when corporate debt is above average. Therefore, private debt-dependent effects of fiscal policy seem to be caused by households' and not firms' borrowing decisions. Second, house prices significantly decline when fiscal consolidations are implemented in high private debt states, whereas they basically do not show any effect in low private debt states. Falling house prices typically reduce the value of home equity households can use as collateral to borrow against.⁴

The closest related work to this study is the paper by Bernardini and Peersman (2015). They find that the government spending multiplier is considerably larger in periods of private debt overhang. However, my paper departs from their study in two important dimensions. First, while Bernardini and Peersman (2015) focus on non-linear effects of government spending, I estimate private debt-dependent responses to fiscal consolidations which are a combination of tax-based and spending-based adjustments. It seems reasonable to assume that the effects of austerity measures differ from standard fiscal spending shocks, because fiscal consolidations are typically implemented under

⁴As shown by Mian, Rao, and Sufi (2013), highly leveraged households have a higher marginal propensity to consume out of housing wealth such that, *ceteris paribus*, the aggregate drop in private demand to falling house prices increases with the level of private debt overhang in the economy.

special circumstances or because they are particularly large (Born, Müller, and Pfeifer, 2015). Moreover, it is unclear whether the effects of equally-sized expansion and tightening of fiscal policy should be symmetric, especially in the face of borrowing constraints. This argument is supported by recent empirical evidence showing that the government spending multiplier significantly differs between fiscal consolidations and fiscal expansions (Barnichon and Matthes, 2015; Riera-Crichton, Vegh, and Vuletin, 2015). In addition, I make use of the narrative consolidation measure to detect exogenous changes in fiscal policy. Second, my analysis is based on a panel dataset, whereas Bernardini and Peersman (2015) focus on the US economy. Thus, I provide multi-country evidence for private debt-dependent responses to fiscal policy.

The structure of the paper is organized as follows. In Section 2, the econometric method, database and the identification of private debt states is described. Section 3 presents results of the benchmark estimation. In addition, it is shown that my results are robust to an alternative identification approach, different ways of separating trend from cycle in private leverage and when extending the narrative consolidation measure past 2009. In Section 4, I check whether the results depend on the composition of the fiscal consolidation. Moreover, I detect state-dependent effects of other relevant variables. In Section 5, I further control for two prominent state variables: the business cycle and government debt overhang. Section 6 presents evidence that indicates the importance of the household balance sheet for understanding private debt-dependent effects of fiscal consolidations. Finally, Section 7 concludes.

2 Econometric Method

To investigate the effects of fiscal consolidations depending on the state of the economy, I follow Auerbach and Gorodnichenko (2013), Ramey and Zubairy (2014) and Owyang, Ramey, and Zubairy (2013) in estimating state-dependent impulse responses to exogenous innovations in the government budget deficit using local projections as invented by Jordà (2005). Recently, this method has become a very popular tool to estimate

non-linear effects. The main advantages compared to VARs are that local projections are more robust to model misspecifications and do not impose the implicit dynamic restrictions involved in VARs. Moreover, local projections offer a very convenient way to account for state dependence. However, the Jordà method does not uniformly dominate the standard VAR approach for calculating impulse responses. In particular, because it does not impose any restrictions that link the impulse responses across different horizons, the estimates are often erratic because of the loss of efficiency. Moreover, it sometimes display oscillations at longer horizons. For a more detailed discussion, I refer to Ramey and Zubairy (2014).

Let $Y_{i,t+h} - Y_{i,t-1}$ denote the cumulative response of a particular variable of interest from time $t - 1$ to $t + h$ to an exogenous change in the government budget deficit at time t , where i indexes the countries in my sample. I estimate a set of regressions of $Y_{i,t+h} - Y_{i,t-1}$ on shocks to the government budget deficit $D_{i,t}$ measured by the narrative series as proposed by Guajardo, Leigh, and Pescatori (2014) and a set of control variables $X_{i,t}$:

$$\begin{aligned}
 Y_{i,t+h} - Y_{i,t-1} = & I_{i,t-1} [\psi_{A,h}(L)X_{i,t-1} + \beta_{A,h}D_{i,t}] \\
 & + (1 - I_{i,t-1}) [\psi_{B,h}(L)X_{i,t-1} + \beta_{B,h}D_{i,t}] + \alpha_{i,h} + \eta_{t,h} + \epsilon_{i,t+h}.
 \end{aligned} \tag{1}$$

Here, $\alpha_{i,h}$ are country-specific constants and $\eta_{t,h}$ captures time fixed effects to control for common macro shocks. $\epsilon_{i,t}$ denotes the error term which is assumed to have a zero mean and strictly positive variance. The dummy variable $I_{i,t}$ captures the state $\{A, B\}$ of the economy. $I_{i,t}$ takes the value of one when private debt is above a certain threshold and zero when it is below that threshold. Following the literature on state-dependent effects of fiscal policy (see for example Auerbach and Gorodnichenko, 2012; Ramey and Zubairy, 2014), I include a one-period lag of $I_{i,t}$ in the estimation to minimize the contemporaneous correlation between the shock series and changes in the indicator variable. L represents the lag operator. The collection of $\beta_{A,h}$ and $\beta_{B,h}$ coefficients directly provide the state-dependent responses of variable $Y_{i,t+h} - Y_{i,t-1}$ at time $t + h$ to the shock at time t . Given

my specification, $\beta_{A,h}$ indicates the response of $Y_{i,t+h} - Y_{i,t-1}$ to the consolidation shock in high private debt states whereas $\beta_{B,h}$ shows the effect in low private debt states.

Throughout the paper, all variables of interest are expressed in level log or level units. This stands in contrast to the approach used in Barro and Redlick (2011), Owyang, Ramey, and Zubairy (2013), and Ramey and Zubairy (2014) where the responses are scaled by GDP. However, given the facts that I use a much shorter sample compared to the aforementioned studies and that the consolidation shock $D_{i,t}$ is already scaled by GDP, it does not seem necessary to normalize the impulse responses by a measure of economic activity.

I prefer the specification of equation (1) to the propensity score matching method used in Jordà and Taylor (2016) because the former approach retains information about the size of fiscal consolidations, whereas the latter only allows the partition of fiscal consolidations into a binary dummy variable 0/1 indicating periods of fiscal consolidation and periods of no consolidation. By retaining information about the magnitude of fiscal consolidations, I am able to directly measure the size of fiscal consolidation across different private debt states. Indeed, in Section 3.2.1 I show that my results are robust when controlling for anticipation effects in the narrative measure.

The dataset of my analysis is of annual frequency over the period 1978-2008 for a balanced sample of 12 OECD countries.⁵ The sample size of the panel is limited by the availability of the credit data used. In my baseline specification, the control variables included in $X_{i,t}$ are the change in the cyclically adjusted primary balance relative to GDP (CAPB), the log difference of real GDP and the log difference of real personal consumption expenditures.⁶ This choice closely mimics the VAR specification used in Guajardo, Leigh, and Pescatori (2014). $L = 1$ in all estimations, although the results are robust to varying the lag length.

⁵The included countries are Australia, Canada, Germany, Denmark, Spain, France, United Kingdom, Italy, Japan, the Netherlands, Sweden and the United States. All data definitions and sources can be found in the appendix.

⁶The results are not affected when using *CAPB* in levels as control variable in the regressions. The appendix includes estimation results when controlling for *CAPB* instead of changes in the deficit variable.

To identify fiscal consolidation shocks, $D_{i,t}$, I use the narrative measure as proposed by Guajardo, Leigh, and Pescatori (2014). This measure is constructed by examining contemporaneous policy documents. The main advantage of identifying fiscal consolidations via the narrative measure compared to changes in the CAPB as suggested by Alesina and Ardagna (2010), is that the narrative measure is exogenous to current economic developments while changes in the CAPB are correlated to the business cycle. Guajardo, Leigh, and Pescatori (2014) show that there is a significant positive correlation between GDP forecast revisions and changes in the CAPB, whereas the null-hypothesis of no correlation between forecast revisions and the narrative measure cannot be rejected.

The definition of episodes of private debt overhang follows closely the approach by Bernardini and Peersman (2015). As an indicator for private debt, I use the private debt-to-GDP ratio, where data are taken from Schularick and Taylor (2012). Although the narrative consolidation measure is available for the period 1978-2009, Schularick and Taylor (2012) provide private debt data that just cover the years 1978-2008. To differentiate between high-debt and low-debt states, the debt-to-GDP ratios are filtered by country-specific smooth Hodrick-Prescott (HP) trends, where the smoothing parameter, λ , is set to 10,000. The relatively high smoothing parameter ensures that the filter removes even the lowest frequency variations in the private debt-to-GDP series. Indeed, the implementation of the Third Basel Accord (Basel III) involves the use of a similar credit gap indicator as used in my analysis (BIS, 2010). As shown by Borio (2014) and Drehmann, Borio, and Tsatsaronis (2012), the credit cycle is significantly longer and has a much greater amplitude than the standard business cycle. Therefore, Drehmann, Borio, and Tsatsaronis (2011) propose the use of an extremely smooth HP-trend to capture the low frequency of financial cycles. Given these considerations, applying an HP-filter with a smoothing parameter $\lambda = 10.000$ to construct the trending and cyclical component of private leverage seems appropriate for my analysis. High private debt states are defined as periods with positive deviations of the debt-to-GDP ratios from the trends, whereas low private debt states indicate periods when debt-to-GDP ratios were below its long-run trends. This procedure implies that out of the 372 periods included in the

sample, 215 or 58% are detected as low private debt periods, while the remaining 157 episodes or 42% indicate periods of private debt overhang. In a separate exercise it is shown that the results are robust to two alternative definitions of high/low private debt states.

3 Results

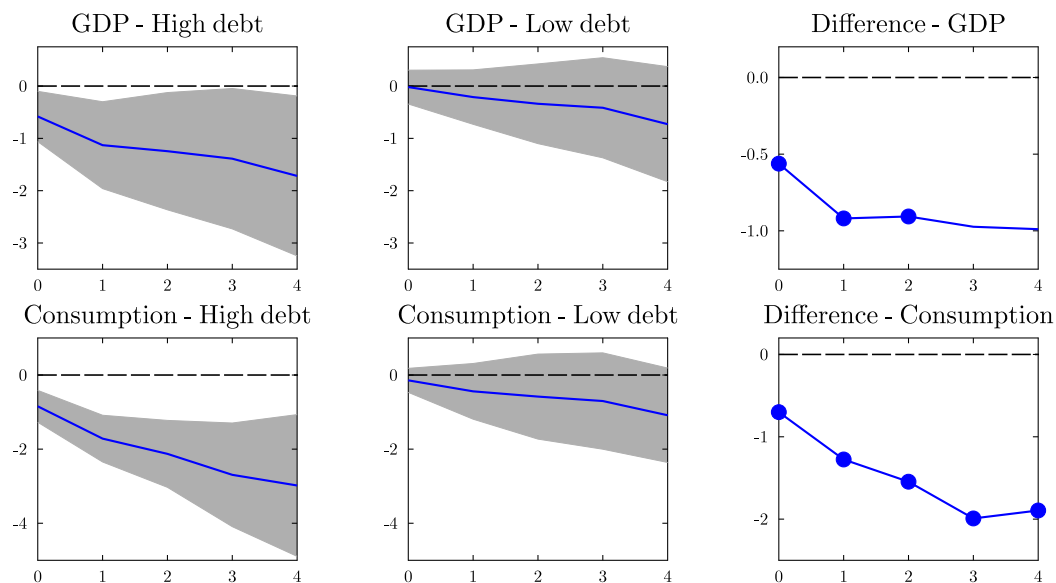
3.1 Baseline

The main variables of interest, $Y_{i,t+h} - Y_{i,t-1}$, are the cumulative change in the log of real GDP and the cumulative change in the log of real personal consumption expenditures. Therefore, $\beta_{A,h}$ and $\beta_{B,h}$ directly estimate the state-dependent cumulative percentage change in the variables of interest in response to a fiscal consolidation shock.

Figure 1 presents the results of my baseline specification. It shows the cumulative effects on GDP and private consumption (solid lines) from year 0 to year 4 in response to a fiscal consolidation shock, where 0 indicates the year in which the shock occurs. Shaded areas indicate 90% confidence bands based on robust standard errors clustered by country. The respective responses are normalized so that the CAPB rises by 1% of GDP in year 0. The left column shows the cumulative responses to a fiscal consolidation implemented in a high private debt state, while the second column shows the respective changes to a fiscal consolidation undertaken in a low private debt state.

When private debt is below average, GDP shows a mild and insignificant reduction which accumulates to less than 1% four years after the fiscal consolidation was implemented. Contrary, fiscal consolidations undertaken when private leverage is high lead to a significant decline in GDP which accumulates to almost 2% at the end of the forecast horizon. A similar pattern can be observed for the respective consumption responses. Private consumption expenditures do not show a significant change in a low debt state. However, in a high private debt state consumption falls significantly such that expenditures are 3% lower after five years. The results indicate that a fiscal consolidation implemented when private debt is low leads to a small but insignificant reduction in

Figure 1: Baseline Results



Note: The first two columns report cumulative changes (in per cent) in response to a shock of 1% of GDP to the cyclically-adjusted primary balance over $h = 0, 1, 2, 3, 4$ years. The shaded areas indicate 90% confidence bands based on robust standard errors clustered by country. The last column shows the estimated difference between high debt and low debt responses. Dots indicate statistically significant differences at the 90% level.

economic activity, while fiscal consolidations in high private debt states induce a severe contraction in the economy.

Similar long-lasting, but non-permanent negative effects of fiscal consolidations are found by Alesina and Ardagna (2010), Guajardo, Leigh, and Pescatori (2014), and Jordà and Taylor (2016). When estimating my baseline local projections for a longer horizon, all variables show a clear tendency to converge back to steady state values seven years after the fiscal consolidation was implemented. To rule out any instability concerns, I also estimated the model while including country-specific linear time trends. It turns out that the baseline results are not affected when controlling for a possible trending behavior in the endogenous variables. The results of both exercises can be found in the appendix.

Although the specific responses in Figure 1 give rise to different dynamics to fiscal consolidations in high and low private debt states, they do not imply whether these differences are significant, or in other words, whether the effects are significantly larger

in high private debt states than in low debt states. Therefore, I test for every variable of interest and at each year of the forecast horizon the following hypothesis:

$$H_0 : \beta_{A,h} - \beta_{B,h} \geq 0. \quad (2)$$

This hypothesis can be tested with a simple one-sided t-test. A similar approach is applied by Ramey and Zubairy (2014) to test whether government spending multipliers statistically differ during times of economic slack.

The right column of Figure 1 shows the respective differences $\beta_{A,h} - \beta_{B,h}$ for GDP and consumption at each period of the forecast horizon. Thus, a negative value indicates that the response in high debt states is lower than in low private debt states. The dots indicate statistical significance at the 90% level.

The response differences in GDP and private consumption are statistically significant for most of the periods. For GDP the differences are significant for 3 out of the 5 years, while they are significant for all 5 periods when inspecting the changes in private consumption. Complementarily to the first two columns of Figure 1, the latter findings indicate that the negative effects of austerity are significantly larger when the policy is implemented in a period of private debt overhang.⁷

As mentioned before, the Guajardo, Leigh, and Pescatori (2014) narrative consolidation measure covers the period 1978-2009. However, because the private debt data taken from Schularick and Taylor (2012) are just available for the period 1978-2008, the baseline sample includes the years 1978-2008. Nevertheless, I am confident that the finding of private debt-dependent effects of fiscal consolidations is not affected by leaving out the year 2009 for three reasons. First, for the sample used the narrative measure does not identify any exogenous fiscal consolidation shock for the year 2009. Therefore, I do not expect the point estimates of my local projections to change significantly when adding

⁷The results are robust to changes in the sample. In the appendix it is shown that the estimates prevail when leaving out the years of the Global Financial Crises. In addition, it presents results indicating that my findings are not driven by any key country in the sample. Moreover, the baseline results are not affected when accounting for serial correlation in the error terms. As shown in the appendix, my finding of private debt-dependent effects of fiscal consolidations prevails when using Driscoll and Kraay (1998) standard errors or when clustering the standard errors by time period.

observations of the final year 2009 to the sample. Second, I reestimate my baseline regressions using total credit data from the Bank of International Settlements (BIS). Contrary to the Schularick/Taylor series, they provide credit data for the year 2009. However, for my sample of interest the BIS-credit data only go back to 1980 so that I lose 12 observations compared to the (baseline) 1978-2008 sample. In the appendix it is shown that my findings prevail when using the BIS-credit data. Finally, as Section 3.2.3 shows, the result of private debt-dependent costs of fiscal consolidation is still present when using an extended version of the narrative measures such that the panel covers the years 1980-2014.

3.2 Robustness

3.2.1 Alternative Identification

Jordà and Taylor (2016) question the exogeneity of the narrative measure. They show that the Guajardo, Leigh, and Pescatori (2014) series has a predictable component. Therefore, my estimates could be biased when using the narrative measure as indicator for exogenous consolidation shocks.

To take account of possible anticipation effects, I combine the approach suggested by Jordà and Taylor (2016) with the forecast error-approach proposed by Auerbach and Gorodnichenko (2012).⁸ The procedure consists of two steps. First, I regress the narrative consolidation measure, $D_{i,t}$, on a set of control variables which possibly include information that help predict the outcome variable. The residuals of this regression measure the unpredictable component of fiscal consolidations. In a second step, the residuals are used as proxy for exogenous austerity innovations in the estimation of equation (1).

Motivated by the set of regressors chosen by Jordà and Taylor (2016), the vector of control variables in the first stage regression includes country and time fixed effects

⁸Auerbach and Gorodnichenko (2012) use the unpredictable component of government spending as proxy for exogenous variations in fiscal expenditures.

Table 1: Alternative Identification of Fiscal Shock (Effect in Year $t = 1$)

| Identification | GDP | | | Consumption | | |
|--|--------------------|-----------------|------------|--------------------|-----------------|------------|
| | High Debt | Low Debt | Difference | High Debt | Low Debt | Difference |
| Baseline | -1.13*** (0.50) | -0.21 (0.32) | -0.92*** | -1.72*** (0.38) | -0.44 (0.46) | -1.28*** |
| Unpredictable component of D_{it} | -2.14*** (1.06) | 0.05 (0.35) | -2.19*** | -3.06*** (0.79) | -0.09 (0.53) | -2.97*** |

Notes: The table reports point estimates and robust standard errors clustered by country in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; **significant at 10%; ***significant at 5%.

and a set of lagged macro variables (real GDP growth, real private consumption growth, change in government debt-to-GDP ratio, change in policy rate, CPI-inflation).

Table 1 presents the results when using the unpredictable component of $D_{i,t}$ as exogenous innovation and compares them to the benchmark estimation.⁹ As it turns out, my findings are robust to this alternative identification strategy. For both identification approaches, fiscal consolidations induce severe and significant reductions in GDP and private consumption when private debt is high, whereas in low private debt states both variables do not respond significantly. For both identifications, the GDP (consumption) response is estimated to be significantly lower when private leverage is high compared to low private leverage periods. This exercise shows that the finding of private debt-dependent effects of fiscal consolidation is robust to alternative ways of identifying fiscal consolidation episodes.

3.2.2 Alternative Debt State Definitions

One possible concern with my baseline estimation could be that the results depend on the underlying definition of low and high private debt states. For this reason, I make use of two alternative ways to differentiate between high and low private debt periods. On the one hand, I calculate high (low) private debt episodes as periods in which the private

⁹Following Guajardo, Leigh, and Pescatori (2014), all tables in the paper present the effects obtained two years after the fiscal consolidation was implemented (here, when $t = 1$). Moreover, the tables report whether the respective responses are statistically significant at the 5%, 10% and 16% level. The 16% level is chosen as lower threshold because of the relatively small sample size of the panel and because 16-84% confidence bands are widely used in the empirical macro literature (for example Bjørnland and Leitemo, 2009; Castelnuovo and Surico, 2010; Hofmann, Peersman, and Straub, 2012). For a general discussion on error bands for impulse responses see Sims and Zha (1999).

Table 2: Alternative Debt State Definitions (Effect in Year $t = 1$)

| Definition based on | GDP | | | Consumption | | |
|----------------------|--------------------|-----------------|------------|--------------------|-----------------|------------|
| | High Debt | Low Debt | Difference | High Debt | Low Debt | Difference |
| HP-filter (Baseline) | -1.13*** (0.50) | -0.21 (0.32) | -0.92*** | -1.72*** (0.38) | -0.44 (0.46) | -1.28*** |
| 15-year MA | -0.64*** (0.31) | 0.49 (0.71) | -1.14*** | -1.37*** (0.31) | -0.75 (0.67) | -0.62*** |
| Deviation from mean | -0.84** (0.47) | -0.31 (0.35) | -0.52* | -1.40*** (0.42) | -0.59 (0.43) | -0.80*** |

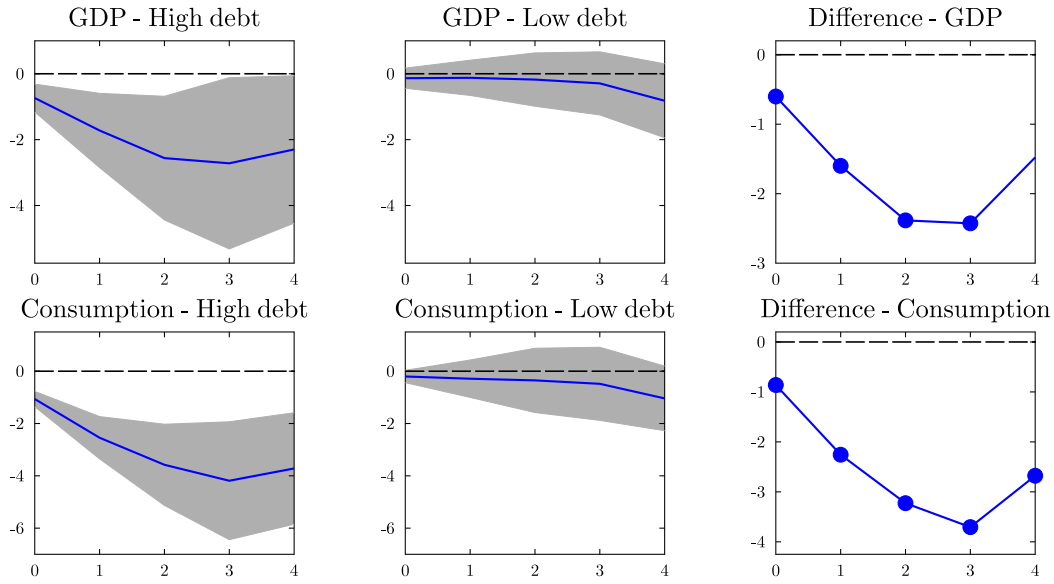
Notes: The table reports point estimates and robust standard errors clustered by country in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; **significant at 10%; ***significant at 5%.

debt-to-GDP ratio is above (below) its 15-year moving average. 15 years corresponds to the median length of financial cycles in industrialized countries (Borio, 2014). On the other hand, I follow Jordà, Schularick, and Taylor (2014) and define private debt states based on deviations from country-specific private leverage means. Whenever the change in the private debt-to-GDP ratio is above (below) its country-specific mean for two consecutive years, I define these episodes as high (low) private debt states.

As Table 2 shows, independent of the underlying debt state definition, I find strong evidence for private debt-dependent effects of fiscal consolidations. More precisely, GDP and private consumption decline significantly when private debt is high, whereas there is no significant response when private debt is low. Moreover, for all definitions, the respective GDP (consumption) response is estimated to be significantly lower when private debt is high compared to the corresponding low private debt one.

This exercise reveals that my findings do not rely on the specific way used to define low and high private debt states. The result of private debt-dependent costs of fiscal consolidations is robust to different definitions of private debt overhang. However, given the limited loss of observations compared to the other two definitions and its actual relevance in financial market policy (Basel III), in what follows I use the smooth HP-filter approach as the baseline method to separate trend from cyclical components in private leverage.

Figure 2: Extended Narrative Measure



Note: The first two columns report cumulative changes (in per cent) in response to a shock of 1% of GDP to the cyclically-adjusted primary balance over $h = 0, 1, 2, 3, 4$ years. The shaded areas indicate 90% confidence bands based on robust standard errors clustered by country. The last column shows the estimated difference between high debt and low debt responses. Dots indicate statistically significant differences at the 90% level.

3.2.3 Extending the Narrative Measure

The baseline dataset covers the period 1978-2008, so it does not include the large-scale consolidation programs implemented by several countries in response to the significant increase in public debt levels following the deep economic downturn after the Global Financial Crises. To test whether my result of private debt-dependent effects of fiscal consolidations prevails when taking these austerity measures into account, I extend the Guajardo, Leigh, and Pescatori (2014) narrative series for the years 2010-2014.

In extending the dataset, I follow closely Dell' Erba, Mattina, and Roitman (2015) and Agca and Igan (2013) who construct a series of the consolidation measure for the years 2010 and 2011. The extension of the dataset is based on the following three OECD reports: *Restoring Public Finances, 2011*, *Restoring Public Finances, 2012 Update*, and *The State of Public Finances, 2015*. These reports outline the economic situation, fiscal consolidation strategy and major consolidation measures for each of the OECD member countries. The country notes in each report lay out each government's rationale

for pursuing fiscal adjustment and are used to identify consolidation periods that were motivated by a desire for deficit reduction. Table 2 of the appendix lists the identified consolidation periods for the years 2010-2014.¹⁰

As the Schularick and Taylor (2012) loans series is just available until 2008, I make use of private credit data published by the Bank for International Settlements. To obtain private debt-to-GDP series, I divide the credit series by nominal GDP. Due to limited availability of the BIS credit data, the sample is now restricted to the period 1980-2014. As before, low/high private debt states are defined as deviations from a smooth HP-trend ($\lambda = 10.000$).

Figure 2 shows the impulse responses when using the extended narrative consolidation measure. Totally in line with the benchmark result, GDP and private consumption decrease significantly when private debt is high with slightly larger accumulated reductions compared to the baseline case. Contrary, GDP and private consumption do not respond significantly when private debt is below average. Additionally, the respective high debt responses are estimated to be significantly lower than the respective low debt ones for almost all periods. Thus, debt-dependent costs of fiscal consolidations are still present when explicitly taking into account the large-scale austerity programs implemented after the Global Financial Crises. Indeed, the results indicate that high private debt levels have amplified the negative effects of fiscal consolidations undertaken in the period 2010-2014.

4 Extensions

In this section, I test whether the result of debt-dependent costs of austerity vary with the composition of the consolidation measure. Additionally, I show that the responses

¹⁰I use the extension of the narrative measure as an additional robustness check and not as benchmark sample for two reasons. First, whereas the Guajardo et al. (2014) measure is constructed by examining contemporaneous policy documents of various sources (IMF Reports, OECD Economic Surveys, Central Bank Reports etc.), I rely mainly on the three OECD reports mentioned above. Second, it can be questioned to what extent the consolidations implemented between 2010 and 2014 can be treated as fully exogenous. Given the severity of the recession, the austerity programs undertaken in the aftermath of the Global Financial Crises could be related to the business cycle.

of other important macro variables also depend crucially on the private debt level when the consolidation is implemented.

4.1 Spending and Tax Based Consolidations

Guajardo, Leigh, and Pescatori (2014) and Alesina, Favero, and Giavazzi (2015) find that the costs of austerity differ with the composition of fiscal consolidations. Both studies show that tax-based consolidations lead to more severe contractions than spending-based adjustments. To allow the effects of consolidations to vary with its composition, I reestimate equation (1), where I make use of the composition definition stated by Guajardo, Leigh, and Pescatori (2014). The authors define fiscal policy changes as tax-based and spending-based if the budgetary impact of tax hikes and spending cuts, respectively, is greater than half the total impact.

Table 3 shows the estimates for spending-based and tax-based consolidations. Overall, the results coincide with the baseline estimation. Independent of the composition of the fiscal consolidation, GDP and private consumption do not change significantly when the austerity measure is implemented in a low private debt state. In contrast, GDP and private consumption are depressed significantly by tax-based and spending-based consolidations when private debt is high. In line with Alesina and Ardagna (2010) and Guajardo, Leigh, and Pescatori (2014), I find that tax-based consolidations have stronger effects on economic activity than spending-based adjustments. Nevertheless, my result of private debt-dependent costs of austerity is robust to the composition of fiscal consolidations.

4.2 Other Variables of Interest

So far, I have considered the private debt-dependent responses of GDP and consumption to fiscal consolidations. However, it seems worth studying whether other important macro variables also react differently to fiscal consolidations in high and low private debt periods. In the following, I check for divergent responses in other components of GDP: private investment, imports and exports. Moreover, I test whether the effects on

Table 3: Spending Based vs. Tax Based (Effect in Year $t = 1$)

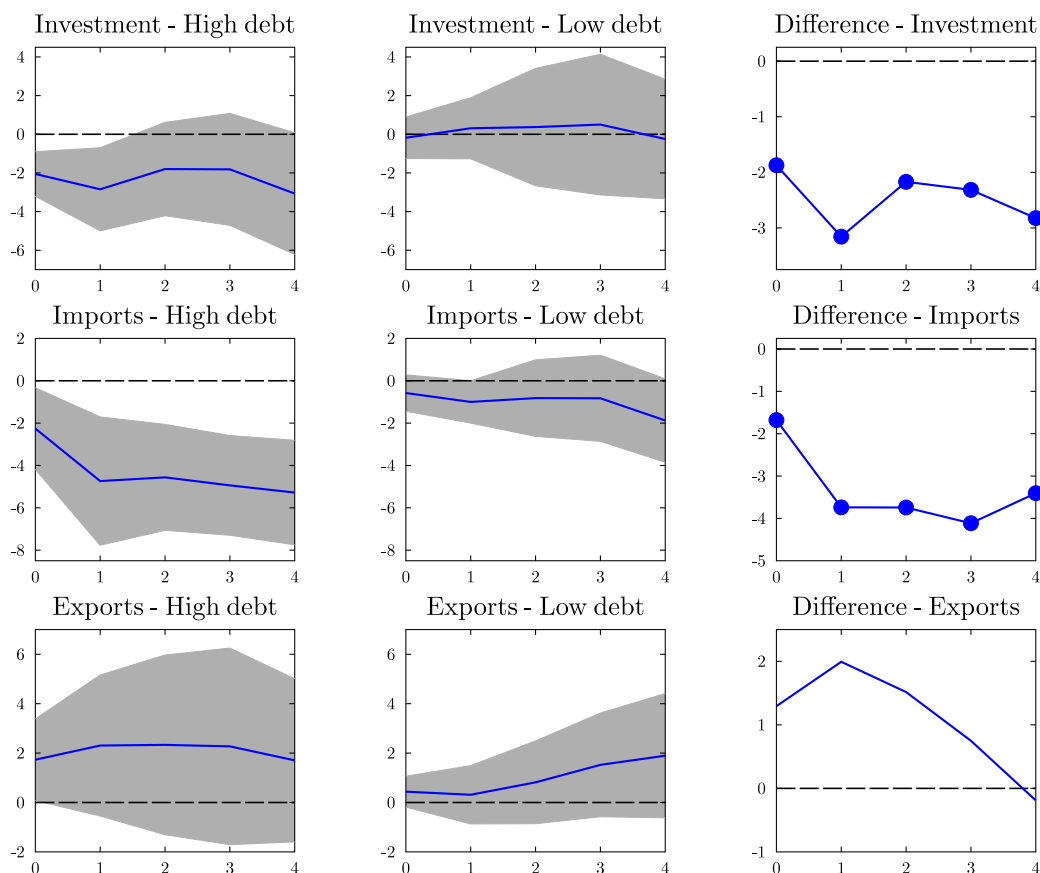
| Composition | GDP | | Consumption | |
|----------------|--------------------|-----------------|--------------------|-----------------|
| | High Debt | Low Debt | High Debt | Low Debt |
| Spending-based | -0.72** (0.43) | -0.21 (0.26) | -1.38*** (0.25) | -0.32 (0.39) |
| Tax-based | -5.20*** (1.82) | -0.59 (0.78) | -4.85*** (1.32) | -1.37 (1.07) |

Notes: The table reports point estimates and robust standard errors clustered by country in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; **significant at 10%; ***significant at 5%.

the labor market measured through the employment rate differ as well. It is shown that the central bank reduces its main policy rate by a similar magnitude, irrespective of the private debt state. Finally, I study how the sovereign default risk, indicated by the institutional investor ratings index (IIR) and the government debt-to-GDP ratio response to consolidations in both private debt states. At each horizon, I project these variables on fiscal consolidations and include their respective lags in the control vector $X_{i,t}$. While investment, imports and exports enter the estimation in log differences, the employment rate, interest rate, IIR and the government debt-to-GDP ratio are considered in level changes.

Figure 3 presents the responses of investment, imports and exports. Private investment increases slightly when the consolidation is undertaken in a period of low private debt. However, this increase is not statistically significant. In high private debt states, investment decreases significantly by more than 2% in the first two years. Afterwards, the effect becomes insignificant as well. The mostly insignificant investment response relates to the empirical evidence presented by Mian, Sufi, and Verner (2015). They show that rises in household debt are closely tied to consumption and less related to business investment. Additionally, it can be interpreted as a first indicator that households', not firms', borrowing decisions are mainly responsible for private debt-dependent effects of austerity. However, below I will elaborate in more detail on the household balance sheet as a possible transmission channel to rationalize my findings.

Figure 3: Investment, Imports, Exports



Note: The first two columns report cumulative changes (in per cent) in response to a shock of 1% of GDP to the cyclically-adjusted primary balance over $h = 0, 1, 2, 3, 4$ years. The shaded areas indicate 90% confidence bands based on robust standard errors clustered by country. The last column shows the estimated difference between high debt and low debt responses. Dots indicate statistically significant differences at the 90% level.

Divergent responses can also be observed for imports. Imports decrease slightly but insignificantly in low private debt states. In contrast, imports are more than 5% lower after 5 years when the consolidation is undertaken in a high private debt period. The difference in the respective import responses is significant for all periods.

In both debt states exports, increase substantially. However, the respective responses are not statistically different from zero for most of the periods. As exports react rather similar in low and high debt states, the response difference is not statistically significant.¹¹

¹¹Taking the effects on imports and exports together, in an additional exercise, I found that the current account significantly increases in high private debt states, while it stays almost unchanged when private debt is low.

Figure 4 shows the results for the employment rate, interest rate, IIR and government debt. The employment rate increases steadily when private debt is below average. Consolidations in high private debt states lead to a significant decline in the employment rate. The accumulated loss after four years is 1.5 percentage points. Additionally, as the right column shows, the employment rate response in high private debt states is significantly lower than the respective one in low private debt states. These findings indicate that the severe real costs of fiscal consolidations implemented when private debt is high also translate into a deterioration in the labor market. This relation is also captured by the theoretical set-up by Andrés, Boscá, and Ferri (2015). In their model, the improvement in the labor market to a government spending shock depends positively on the equilibrium level of household debt.

Private debt-dependent responses to fiscal consolidations could be explained by a different reaction of the monetary authority to austerity in low and high debt states. When the central bank reduces (increases) its interest rate by less (more) when austerity is realized in a high leverage period compared to a low debt state, then the more severe downturn could be caused by a debt-dependent interest rate change. Indeed, as the second row of Figure 4 demonstrates, this hypothesis is not supported by the data. The central bank reduces the interest rate by a similar magnitude irrespective of the private debt state. Overall, both interest rate responses are insignificant for almost all periods indicating a rather conservative expansionary monetary policy in reaction to fiscal consolidations. Not surprisingly, the response difference is statistically insignificant for all years of the forecast horizon.

The IIR is based on assessments of sovereign default risk by private sector analysts on a scale of zero to 100, with a rating of 100 assigned to the lowest perceived sovereign default probability. As the third row of Figure 4 shows, the index falls when consolidations are implemented in a high private debt state, implying a higher probability of sovereign default. Significant reductions in the IIR are visible up to three years after the consolidation. Interestingly, even in low debt states the IIR does not increase but mainly

stays unchanged 4 years after the implementation took place. In all of the five periods, the high debt IIR response is significantly lower than the low debt IIR response.

Finally, I look at how the government debt-to-GDP ratio is affected by fiscal consolidations in both private debt states. In high private debt states, the public debt burden shows a persistent and significant increase which accumulates to a rise of more than 4% at the end of the forecast horizon. In contrast, the government debt-to-GDP ratio does not respond significantly when private leverage is below average. In addition, the high debt response is estimated to be significantly larger than the respective low debt response in four out of the five periods considered. In contrast to reducing public debt burdens which is one of the main goals of fiscal consolidations, public debt burdens even increase when private debt is high. Together with the effects on the sovereign default probability, this finding indicates that austerity in high private debt states is not only associated with high costs for the private sector but also with a worsening of government finances.

To summarize, besides GDP and consumption, also imports, the employment rate, the sovereign default risk and the government debt-to-GDP ratio react differently to fiscal consolidations depending on the private debt level in the economy.

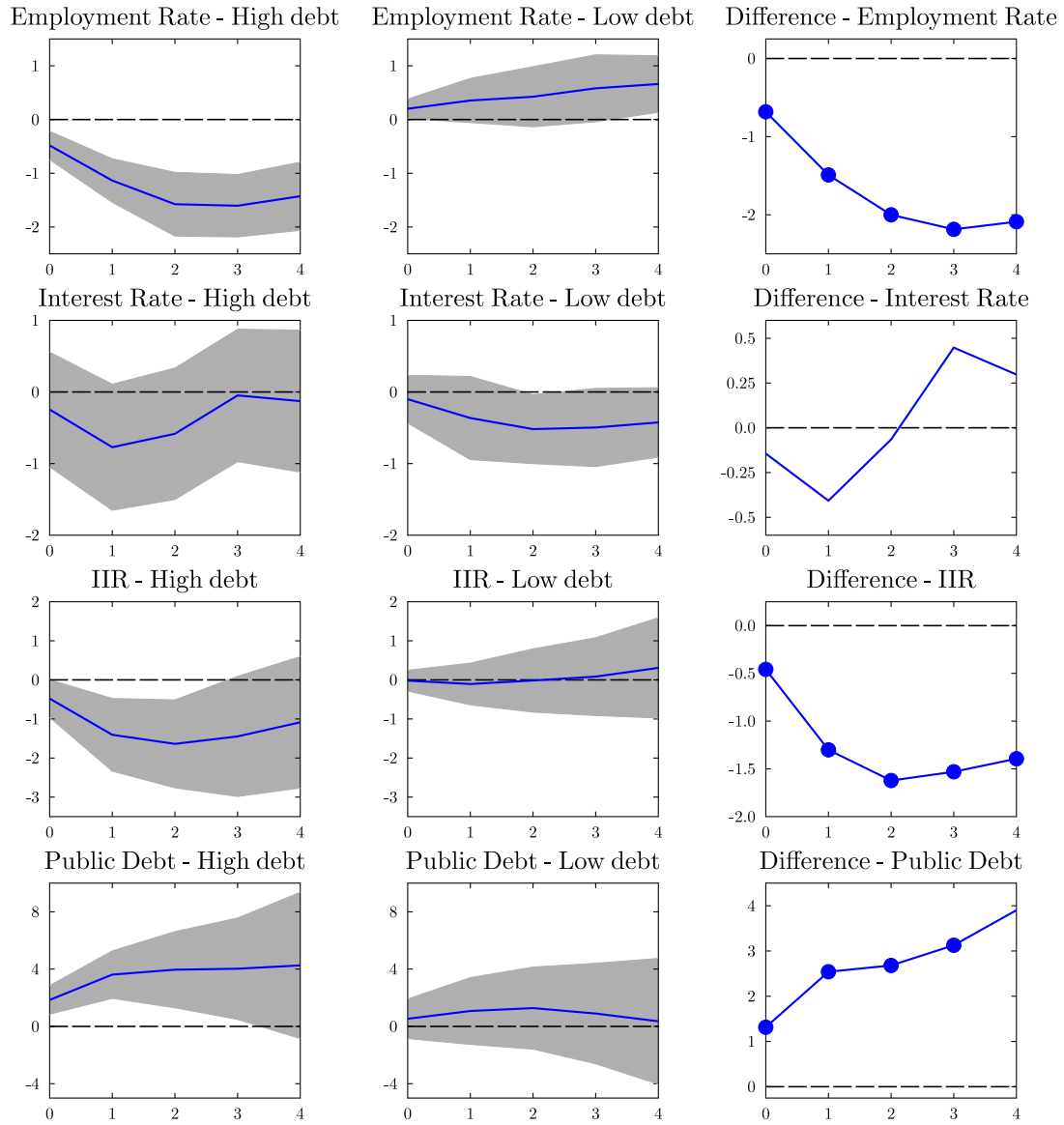
5 Additional State Variables

In this section it is demonstrated that the result of private debt-dependent effects of austerity still prevails when I further condition on two other prominent state variables: the state of the business cycle and government debt overhang.

5.1 Booms and Recessions

Jordà and Taylor (2016) show that the costs of fiscal consolidations differ according to the state of the business cycle. They find that austerity leads to a significant drop in economic activity when implemented in recessions while there is no significant effect when consolidations are undertaken in a boom. Additionally, Auerbach and Gorodnichenko

Figure 4: Employment, Interest Rate, Investors' Confidence, Public Debt



Note: The first two columns report cumulative changes (in percentage points) in response to a shock of 1% of GDP to the cyclically-adjusted primary balance over $h = 0, 1, 2, 3, 4$ years. The shaded areas indicate 90% confidence band based on robust standard errors clustered by country. The last column shows the estimated difference between high debt and low debt responses. Dots indicate statistically significant differences at the 90% level.

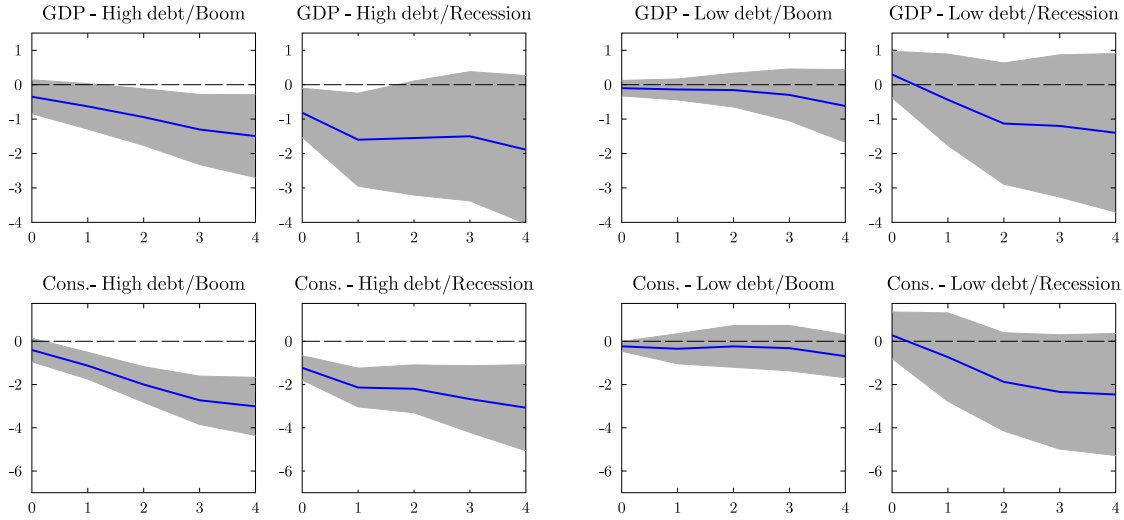
(2012) present empirical evidence that the government spending multiplier is larger in periods of economic slack. Contrary, Ramey and Zubairy (2014) do not find significant differences between spending multipliers in good and bad times. To check whether my findings are sensitive to the state of the business cycle, I further condition equation (1) on expansionary and recessionary states. Thereby, I make use of three common approaches to differentiate between expansionary and recessionary periods. As a benchmark case, I use the recession dates published by the OECD. Second, similar to Jordà and Taylor (2016), I calculate the cyclical component of GDP measured as deviations from (country-specific) HP trends with a smoothing parameter $\lambda = 6.25$ as suggested by Ravn and Uhlig (2002). Positive deviations from the trend are defined as booms and negative deviations as recessions. Third, following the approach proposed by Auerbach and Gorodnichenko (2012), I construct (country-specific) four-year moving averages of real GDP growth, and classify periods as expansions (recessions) whenever the actual growth rate is above (below) the moving average rate.

I reestimate equation (1) separately for low and high private debt states based on the following equation:

$$\begin{aligned}
Y_{i,t+h} - Y_{i,t-1} = & I_{C,i,t-1} [\psi_{C,h}(L)X_{i,t-1} + \beta_{C,h}D_{i,t}] \\
& + I_{D,i,t-1} [\psi_{D,h}(L)X_{i,t-1} + \beta_{D,h}D_{i,t}] \\
& + I_{E,i,t-1} [\psi_{E,h}(L)X_{i,t-1} + \beta_{E,h}D_{i,t}] + \alpha_{i,h} + \eta_{t,h} + \epsilon_{i,t+h}.
\end{aligned} \tag{3}$$

$I_{C,i,t}$ and $I_{D,i,t}$ now indicate the state of the business cycle of the respective private debt states. In the estimation for high private debt states, $I_{C,i,t}$ measures periods of high private debt that coincide with periods of economic contractions whereas $I_{D,i,t}$ indicates periods of high private debt that are also characterized by economic expansions. $I_{E,i,t}$ is then a dummy variable for being in the opposing private debt state (low private debt), irrespective of the state of the business cycle. Analogously, in the estimation for low private debt states, $I_{C,i,t}$ ($I_{D,i,t}$) measures periods of low private debt that coincide with periods of economic contractions (expansions) and $I_{E,i,t}$ indicates periods of high private

Figure 5: Controlling for State of the Business Cycle



Note: Cumulative changes (in per cent) in response to a shock of 1% of GDP to the cyclically-adjusted primary balance over $h = 0, 1, 2, 3, 4$ years. The shaded areas indicate 90% confidence bands based on robust standard errors clustered by country.

debt. $\beta_{C,h}$ and $\beta_{D,h}$ then provide the state-dependent responses for both debt states in recessions and booms, respectively.

Figure 5 shows the results based on the OECD business cycle classification, whereas Table 4 reports the effects for the two other classification strategies. Independent of the business cycle classification applied, when private debt is high GDP and consumption decline significantly in recessionary but also in expansionary periods. In contrast, in low private debt states the effects of fiscal consolidations are not significant neither in booms nor in recessions. Moreover, the size of the respective point estimates in both business cycle states is fairly similar indicating that business cycle-dependent effects of fiscal consolidations disappear when controlling for private leverage in the economy.

5.2 Government Debt

In addition to the state of the business cycle, previous literature found that the effects of fiscal policy vary with the level of public debt in the economy. Perotti (1999) shows that an increase in government consumption leads to higher private consumption expenditures when government debt is low, whereas consumption declines when public debt-to-GDP

Table 4: Alternative Business Cycle Classification (Effect in Year $t = 1$)

| Classification based on Detrended GDP | Boom | | Recession | |
|--|--------------------|-----------------|--------------------|-----------------|
| | High Debt | Low Debt | High Debt | Low Debt |
| GDP | -1.22* (0.79) | -0.07 (0.33) | -0.97** (0.49) | -0.42 (0.41) |
| Consumption | -1.75*** (0.70) | -0.28 (0.38) | -1.59*** (0.39) | -0.56 (0.61) |
| Classification based on MA GDP growth | Boom | | Recession | |
| | High Debt | Low Debt | High Debt | Low Debt |
| GDP | -1.08* (0.79) | -0.11 (0.21) | -0.98** (0.58) | -0.47 (0.65) |
| Consumption | -1.56*** (0.73) | -0.34 (0.43) | -1.60*** (0.52) | -0.55 (0.79) |

Notes: The table reports point estimates and robust standard errors clustered by country in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; **significant at 10%; ***significant at 5%.

levels are high. Similar, Ilzetzki, Mendoza, and Végh (2013) provide evidence that the government spending multiplier negatively depends on the public debt level.

To check whether the result of private debt-dependent costs of fiscal consolidations still holds when controlling for the public debt level, I reestimate equation (3) where $I_{C,i,t}$ and $I_{D,i,t}$ now indicate the respective government debt levels in the periods of both private debt states. In the estimation for high private debt states, $I_{C,i,t}$ measures periods of high private debt that coincide with periods of low government debt, whereas $I_{D,i,t}$ indicates periods of high private debt that are also characterized by high public debt levels. $I_{E,i,t}$ is then a dummy variable for being in the opposing private debt state (low private debt), irrespective of the government debt level.¹² Periods of high (low) public debt are defined as positive (negative) deviations of the government debt-to-GDP ratio from a country-specific smooth HP trend ($\lambda = 10,000$).

Figure 6 presents the cumulative responses for both private debt states when controlling for the public debt burden. GDP and private consumption decline significantly

¹²Analogously, in the estimation for low private debt states, $I_{C,i,t}$ ($I_{D,i,t}$) measures periods of low private debt that coincide with periods of low (high) public debt burdens and $I_{E,i,t}$ indicates periods of high private debt.

irrespective of the public debt level when private debt is high. In line with the findings by Perotti (1999) and Ilzetki, Mendoza, and Végh (2013), the effects are larger in periods of low public debt. When government debt is low, GDP (consumption) is 3.9% (4.8%) lower four years after the implementation. In high government debt states, the accumulated loss is 0.7% for GDP and 2.1% for consumption.

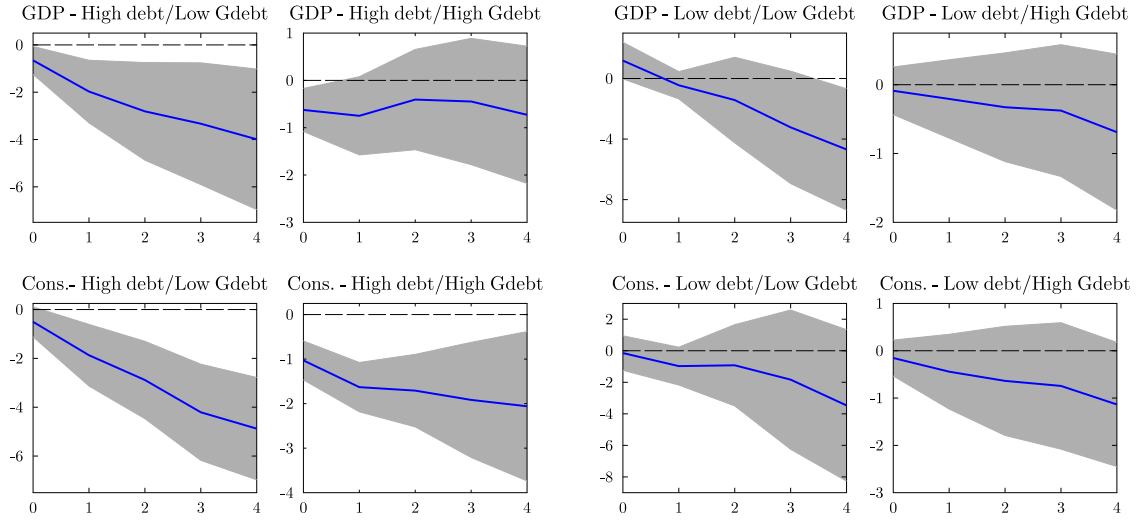
Turning to the low private debt responses, I find insignificant effects for periods with high public debt burdens. When government debt is low, GDP shows a significant response only in the last period of the forecast horizon, whereas consumption does not react significantly in all periods. In accordance to the respective high private debt responses, the point estimates for GDP and consumption are larger when the government debt level is low.

To sum up, the last two exercises demonstrate that fiscal consolidations implemented in high private debt states are always a drag on private economic activity, irrespective of the state of the business cycle or the government debt level. In contrast, austerity measures undertaken in low private debt periods do not have a significant effect on the economy in booms and recessions, when government debt is high or low. This result gives rise to the interpretation that effectiveness of fiscal policy does not vary with the business cycle or the public debt burden but rather with the level of private leverage. Whether this reasoning also contributes to the controversial debate of state-dependent government spending multipliers (see for example Auerbach and Gorodnichenko, 2012; Ramey and Zubairy, 2014) could be an interesting agenda of future research.

6 Household Balance Sheet

What is the underlying transmission channel through which my results can be rationalized? In the following, I present evidence indicating that deterioration in household balance sheets as proposed by Mian and Sufi (2011, 2012) is of central importance for understanding private debt-dependent responses to fiscal consolidations. They stress that the large drop in private demand during the Great Recession was mainly caused by a

Figure 6: Controlling for Government Debt Level



Note: Cumulative changes (in per cent) in response to a shock of 1% of GDP to the cyclically-adjusted primary balance over $h = 0, 1, 2, 3, 4$ years. The shaded areas indicate 90% confidence bands based on robust standard errors clustered by country.

worsening in housing net worth of highly leveraged households. Moreover, U.S. counties with a larger decline in housing net worth were found to experience a larger decline in employment. In a recent paper, Mian, Sufi, and Verner (2015) empirically show that an increase in private debt is associated with lower output growth in the future. This result only holds for increases in household debt, while for rises in corporate debt the authors do not find significant future output effects. In a theoretical framework, Andrés, Boscá, and Ferri (2015) show that the spending multiplier increases with the level of households' indebtedness. Their model economy is populated by two types of households, lenders and borrowers. Borrowing households face a collateral constraint which limits the maximum loans that an individual can get to a fraction of the liquidation value of the amount of housing held by the household, the loan-to-value ratio. By assuming that the collateral constraint holds with equality in equilibrium, it can be shown that borrowing households discount the future more heavily than lending households. This model feature is backed by the empirical finding that indebted households have a higher marginal propensity to consume out of housing wealth (Mian, Rao, and Sufi, 2013). In a simulation exercise, Andrés, Boscá, and Ferri (2015) show that the size of the spending multiplier positively

Table 5: Household Debt vs. Corporate Debt (Effect in Year $t = 1$)

| Private debt type | Household debt | | | Corporate Debt | | |
|-------------------|-------------------|-----------------|------------|-----------------|-----------------|------------|
| | High Debt | Low Debt | Difference | High Debt | Low Debt | Difference |
| GDP | -0.89** (0.53) | -0.01 (0.57) | -0.87** | -0.69 (0.56) | -0.45 (0.46) | -0.25 |

Notes: The table reports point estimates and robust standard errors clustered by country in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; **significant at 10%; ***significant at 5%.

depends on the share of borrowers in the economy and the loan-to-value ratio, which in turn depend on the level of indebtedness. Taken together, all these studies find that a high level of household indebtedness amplifies the effects to economic shocks.

The central determinant of housing net worth are real estate prices. Mian and Sufi (2011, 2012) demonstrate that changes in house prices crucially affect private consumption expenditures. Falling house prices led to a deterioration in households balance sheets which, through the housing net worth channel, resulted in the large reduction in economic activity observed during the Great Recession. Andrés, Boscá, and Ferri (2015) model house prices as one variable of the liquidation value households can use as collateral to borrow against.

Given these considerations, my results are tested in two additional dimensions. First, I split private debt into household debt and corporate debt and check whether my findings depend on the specific type of private leverage. Second, I show how house prices respond to fiscal consolidations in high and low private debt states.

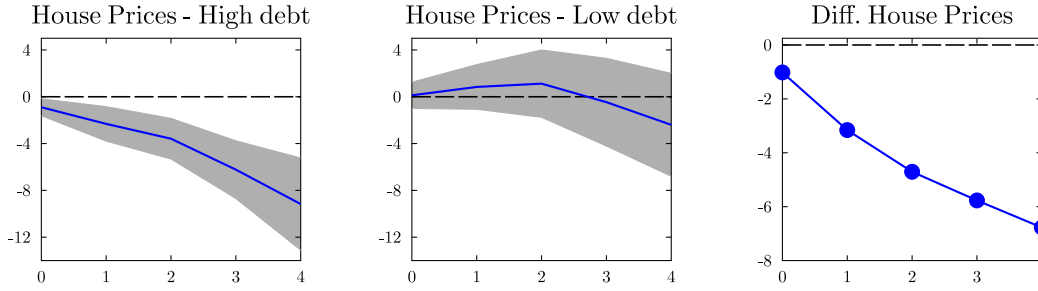
Table 5 presents the different GDP responses in low/high corporate debt and low/high household debt states. Equation (1) is separately estimated for both types of private debt. Series on corporate debt and household debt are taken from the Bank for International Settlements, where, due to data limitations, the panel is now restricted to the period 1980-2008 and the countries Australia, Canada, Germany, Spain, France, United Kingdom, Italy, Japan, Sweden and the United States. To obtain private debt-to-GDP series, I divide the respective debt series by nominal GDP. As before, low/high corporate debt and household debt periods are identified as deviations from a smooth trend (HP-filter with $\lambda = 10,000$).

It turns out that my major finding of private debt-dependent effects of fiscal consolidations is mainly driven by households' leveraging position and not corporate debt overhang. The fall in GDP in response to austerity is not significant when corporate debt is high. In contrast, GDP declines significantly when private households are highly leveraged. Although the effect in high corporate debt states is somewhat larger than in low corporate debt states, the difference between both responses is statistically insignificant. A different picture emerges for household debt. The response difference between high and low household debt states is estimated to be statistically significant. In line with the findings by Mian and Sufi (2011, 2012) and Andrés, Boscá, and Ferri (2015), the results in Table 5 point to the important role of household leveraging for the economic dynamics to fiscal interventions. Corporate debt levels do not seem to be responsible for understanding private debt-dependent effects of fiscal policy.

Given the prominent role of households' leveraging position for understanding my results, it seems natural to investigate how the central driver of housing wealth, house prices, react to fiscal consolidations in low/high private debt periods. As mentioned earlier, house prices are one key determinant of households' optimal consumption decision. Falling house prices reduce the home equity value that serves as collateral to borrow against, which ultimately results in lower consumption expenditures by constrained agents (Mian and Sufi, 2011, 2012). To test whether this transmission channel also applies to my findings, Figure 7 shows the response of house prices to fiscal consolidations implemented in low and high private debt states. House price data are taken from the Federal Reserve Bank of Dallas (Mack and Martínez-García, 2011). At each horizon, house prices are projected on fiscal consolidations and their respective lag is included in the vector of control variables $X_{i,t}$. House prices enter the estimation in log differences.

Figure 7 shows that the response of house prices crucially depends on the private debt level when the fiscal consolidation is undertaken. House prices do not react significantly when private leverage is low. However, in a high private debt state house prices significantly fall with an accumulated decline of almost 10% after five years. As the last column

Figure 7: House Prices



Note: Cumulative changes (in per cent) in response to a shock of 1% of GDP to the cyclically-adjusted primary balance over $h = 0, 1, 2, 3, 4$ years. The shaded areas indicate 90% confidence bands based on robust standard errors clustered by country.

of Figure 7 demonstrates the difference between the respective responses is statistically significant for all five periods.

Although causal interpretations should be taken cautiously, the evidence shown in this section indicates that private debt-dependent costs of fiscal consolidations can be rationalized through deterioration in household balance sheets. Theories should, therefore, elaborate on the housing net worth channel (Mian and Sufi, 2011, 2012) when studying the consequences of fiscal policy interventions.

7 Conclusion

Motivated by recent theoretical contributions that show the effects of fiscal policy to be larger in periods of high private leverage (see for example Andrés, Boscá, and Ferri, 2015; Eggertsson and Krugman, 2012; Kaplan and Violante, 2014), this paper shows that the level of private indebtedness significantly determines the costs of fiscal consolidations. Based on a panel of 12 OECD countries, I use local projection methods, which allow responses to differ between low debt and high debt states.

I find that austerity implemented in a low private debt state does not induce significant changes in GDP and private consumption. In contrast, fiscal consolidations lead to severe contractions in GDP and private consumption when private debt is high. This result is robust to different ways of identifying fiscal consolidations, alternative definitions

of low/high private debt states, the composition of fiscal consolidations, controlling for the state of the business cycle and government debt overhang. In addition, the finding of private debt-dependent costs of fiscal consolidations is still present when extending the sample such that it includes large-scale austerity programs implemented in the period 2010-2014. Imports and employment fall significantly when private leverage is high, while they do not show any significant effect when private debt is low. Moreover, in high private debt states, consolidations lead to a persistent increase in the government debt-to-GDP ratio which contradicts with one of the main goals of fiscal austerity that lies in reducing public debt burdens.

Two additional findings highlight the importance of the housing net worth channel (Mian and Sufi, 2011, 2012) for understanding my results. First, the private debt-dependent responses to fiscal consolidations are mainly driven by household debt and not corporate debt. Second, I show that house prices significantly decline when consolidations are implemented in a period of private debt overhang. Both of these latter observations indicate that deterioration in household balance sheets represents a possible channel through which my results can be explained.

My findings reveal important implications. They confirm predictions of theoretical models as the ones by Eggertsson and Krugman (2012), Kaplan and Violante (2014) and Andrés, Boscá, and Ferri (2015), which point out the impact of fiscal policy interventions to be larger in periods of private debt overhang. Moreover, high private debt levels in Southern European countries may have amplified the negative effects of large-scale fiscal consolidations. Contrary to its objective of improving public finances, austerity measures could have even increased solvency problems. More generally speaking, the level of private debt and especially of household debt seems to matter for the effects of fiscal policy.

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Appendix

This appendix includes all data definitions and sources and reports the results of additional estimation results and robustness checks mentioned in the text.

Table A1 presents all data definitions and sources.

Table A2 reports the identified narrative fiscal shocks for the period 2010-2014.

Table A3 shows that my results are not affected when using $CAPB_t$ in levels as control variable in the regressions. The baseline results still hold when controlling for $CAPB_t$ instead of $\Delta CAPB_t$.

Table A4 demonstrates that private debt-dependent effects of fiscal consolidations also emerge when using credit data from the Bank for International Settlements (BIS). The local projections using the BIS-credit data are based on the years 1980-2009.

Table A5 shows the effects when controlling for country-specific time trends in my baseline specification. It turns out, that my main finding is not affected when allowing for a possible trending behavior in the endogenous variables.

To rule out that my results are driven by the Global Financial Crises, Table A6 presents the results when considering the 1978-2006 sample. My estimates are robust to leaving out the Crises years.

To assess how important any individual country is for the results, I reestimate the local projections, while dropping one country at a time from the sample. As Table A7 indicates, the results are comparable to the baseline in each case.

Table A8 shows estimation results when accounting for serial correlation in the error terms. It presents results when using Driscoll and Kraay (1998) standard errors and when clustering the standard errors by time period. It turns out, that my main finding prevails when allowing the error terms to be correlated.

Figure A1 presents results when estimating the baseline regressions for a longer horizon. All variables show the tendency to converge back to steady state seven years after the consolidation was implemented. This gives rise to the interpretation that fiscal consolidations have long-lasting, but non-permanent negative effects. Together with Table A5 this finding indicates that my findings are not driven by unstable impulse responses.

The baseline sample covers the period 1978-2008 and the countries Australia, Canada, Germany, Denmark, Spain, France, United Kingdom, Italy, Japan, the Netherlands, Sweden and the United States.

Table A1: Data Definitions and Sources

| Variable | Definition | Source |
|--------------------------------------|---|---|
| GDP, real | Gross domestic product, constant prices, OECD base year | OECD |
| GDP, nominal | Gross domestic product, current prices, current PPPs, in US Dollar | OECD |
| Consumption | Final consumption expenditures, households and non-profit institutions serving households, constant prices, OECD base year | OECD |
| CAPB | Cyclically-adjusted primary balance | Alesina and Ardagna (2010) |
| Private debt to GDP | End-of-year amount of outstanding domestic currency lending by domestic banks to domestic households and nonfinancial corporations (excluding lending within the financial system) to GDP | Schularick and Taylor (2012) |
| Fiscal consolidation | Changes in fiscal policy motivated by a desire to reduce the budget deficit and not by responding to prospective economic conditions | Guajardo, Leigh, and Pescatori (2014) |
| Investment | Gross fixed capital formation, constant prices, OECD base year | OECD |
| Imports | Imports of goods and services, constant prices, OECD base year | OECD |
| Exports | Exports of goods and services, constant prices, OECD base year | OECD |
| Employment rate | Civilian employment as % population (15-64 years old) | OECD |
| Interest rate | Main central bank policy interest rate | Guajardo, Leigh, and Pescatori (2014) |
| Institutional Investors Rating Index | Assessments of sovereign risk by private sector analysts on a scale to 100, with a rating of 100 assigned to the lowest perceived sovereign default probability | Guajardo, Leigh, and Pescatori (2014) |
| Household debt | End-of-year credit to households and NPISHs from all sectors, market value, in US Dollar, adjusted for breaks | Bank for International Settlements; sample restricted to 1980-2008, no data for Denmark and Netherlands |
| Corporate debt | End-of-year credit to non-financial corporations from all sectors, market value, in US Dollar, adjusted for breaks | Bank for International Settlements; sample restricted to 1980-2008, no data for Denmark and Netherlands |
| Total credit to private sector | End-of-year credit to private non-financial sector from all sectors, market value, market value, in US Dollar, Adjusted for breaks | Bank for International Settlements; sample restricted to 1980-2014 |
| House prices | Real house prices index (four-quarter average) | Federal Reserve Bank of Dallas (Mack and Martínez-García, 2011) |
| Public debt to GDP | Face value of total general government debt outstanding to GDP | Jordà, Schularick, and Taylor (2016a) |
| OECD recession indicator | OECD based recession indicator from the peak through the trough | OECD |

In extending the narrative consolidation measure, I closely follow Dell’ Erba, Mattina, and Roitman (2015) and Agca and Igan (2013) who construct a series of the consolidation measure for the years 2010 and 2011. The extension of the dataset is based on the following three OECD reports: *Restoring Public Finances, 2011*, *Restoring Public Finances, 2012 Update*, and *The State of Public Finances, 2015*. These reports outline the economic situation, fiscal consolidation strategy and major consolidation measures for each of the OECD member countries. The country notes in each report lay out each government’s rationale for pursuing fiscal adjustment and are used to identify consolidation periods that were motivated by a desire for deficit reduction.

Table A2: Narrative Fiscal Shock, 2010-2014 (% GDP).

| Country | 2010 | 2011 | 2012 | 2013 | 2014 |
|----------------|------|------|------|-------|-------|
| Australia | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Canada | 0.00 | 0.10 | 0.10 | 0.30 | 0.20 |
| Germany | 0.00 | 0.50 | 1.50 | 0.60 | 0.30 |
| Denmark | 0.00 | 0.90 | 0.00 | 1.10 | -0.45 |
| Spain | 2.70 | 2.20 | 0.80 | 0.30 | 0.60 |
| France | 0.00 | 1.10 | 1.40 | 1.50 | 1.00 |
| United Kingdom | 0.60 | 1.20 | 1.00 | 1.00 | -0.10 |
| Italy | 0.00 | 0.90 | 3.40 | -0.49 | 0.47 |
| Japan | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Netherlands | 0.00 | 0.30 | 0.70 | 2.10 | 1.90 |
| Sweden | 0.00 | 0.40 | 0.00 | -0.60 | -0.90 |
| United States | 0.00 | 0.00 | 0.00 | 1.00 | 0.00 |

Table A3: Controlling for CAPB (Effect in Year $t = 1$)

| Specification | GDP | | Consumption | |
|------------------------------|--------------------|-----------------|--------------------|-----------------|
| | High Debt | Low Debt | High Debt | Low Debt |
| Baseline ($\Delta CAPB_t$) | -1.13*** (0.50) | -0.21 (0.32) | -1.72*** (0.38) | -0.44 (0.46) |
| $CAPB_t$ | -0.89** (0.52) | -0.64 (0.46) | -1.54*** (0.41) | -0.82 (0.74) |

Notes: The table reports point estimates and robust standard errors clustered by country in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; **significant at 10%; ***significant at 5%.

Table A4: Using BIS-Credit Data (Effect in Year $t = 1$)

| Definition based on | GDP | | | Consumption | | |
|-----------------------------------|--------------------|-----------------|------------|--------------------|-----------------|------------|
| | High Debt | Low Debt | Difference | High Debt | Low Debt | Difference |
| Baseline | -1.13*** (0.50) | -0.21 (0.32) | -0.92*** | -1.72*** (0.38) | -0.44 (0.46) | -1.28*** |
| BIS-credit data sample: 1980-2009 | -0.85** (0.50) | 0.01 (0.33) | -0.86*** | -1.34*** (0.38) | -0.23 (0.48) | -1.11*** |

Notes: The table reports point estimates and robust standard errors clustered by country in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; **significant at 10%; ***significant at 5%.

Table A5: Controlling for linear Time Trend (Effect in Year $t = 1$)

| Specification | GDP | | | Consumption | | |
|--------------------------------|--------------------|-----------------|------------|--------------------|-----------------|------------|
| | High Debt | Low Debt | Difference | High Debt | Low Debt | Difference |
| Baseline | -1.13*** (0.50) | -0.21 (0.32) | -0.92*** | -1.72*** (0.38) | -0.44 (0.46) | -1.28*** |
| Country-specific time trend | -0.90** (0.46) | -0.11 (0.33) | -0.79*** | -1.52*** (0.42) | -0.24 (0.53) | -1.28*** |

Notes: The table reports point estimates and robust standard errors clustered by country in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; **significant at 10%; ***significant at 5%.

Table A6: Leaving out Global Financial Crises (Effect in Year $t = 1$)

| Specification | GDP | | | Consumption | | |
|------------------|--------------------|-----------------|------------|--------------------|-----------------|------------|
| | High Debt | Low Debt | Difference | High Debt | Low Debt | Difference |
| Baseline | -1.13*** (0.50) | -0.21 (0.32) | -0.92*** | -1.72*** (0.38) | -0.44 (0.46) | -1.28*** |
| 1978-2006 sample | -0.94*** (0.42) | -0.20 (0.31) | -0.74*** | -1.58*** (0.38) | -0.39 (0.49) | -1.19*** |

Notes: The table reports point estimates and robust standard errors clustered by country in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; **significant at 10%; ***significant at 5%.

Table A7: Dropping one Country at a Time (Effect in Year $t = 1$)

| Country excluded | GDP | | | Consumption | | |
|------------------|--------------------|-----------------|------------|--------------------|------------------|------------|
| | High Debt | Low Debt | Difference | High Debt | Low Debt | Difference |
| Baseline | -1.13*** (0.50) | -0.21 (0.32) | -0.92*** | -1.72*** (0.38) | -0.44 (0.46) | -1.28*** |
| Australia | -0.95** (0.53) | -0.25 (0.32) | -0.71** | -1.54*** (0.39) | -0.25 (0.32) | -1.10*** |
| Canada | -0.93** (0.52) | -0.06 (0.28) | -0.87*** | -1.61*** (0.44) | -0.29 (0.43) | -1.32*** |
| Germany | -1.19*** (0.56) | -0.25 (0.33) | -0.94*** | -1.78*** (0.43) | -0.44 (0.47) | -1.35*** |
| Denmark | -1.09*** (0.51) | -0.36 (0.53) | -0.73** | -1.71*** (0.43) | -1.01* (0.65) | -0.70** |
| Spain | -1.04*** (0.46) | -0.07 (0.27) | -0.97*** | -1.66*** (0.34) | -0.28 (0.39) | -1.38*** |
| France | -1.16*** (0.55) | -0.22 (0.32) | -0.94*** | -1.74*** (0.41) | -0.39 (0.43) | -1.35*** |
| United Kingdom | -1.06** (0.55) | -0.27 (0.33) | -0.79** | -1.60*** (0.42) | -0.46 (0.48) | -1.14*** |
| Italy | -1.89*** (0.62) | -0.29 (0.40) | -1.60*** | -2.15*** (0.54) | -0.62 (0.58) | -1.53*** |
| Japan | -0.89*** (0.36) | -0.29 (0.35) | -0.60*** | -1.39*** (0.26) | -0.52 (0.51) | -0.87*** |
| Netherlands | -0.88* (0.56) | -0.01 (0.26) | -0.87** | -1.55*** (0.44) | -0.15 (0.32) | -1.40*** |
| Sweden | -1.22*** (0.59) | -0.36 (0.38) | -0.86** | -1.79*** (0.41) | -0.53 (0.55) | -1.26*** |
| United States | -0.99** (0.51) | -0.17 (0.31) | -0.82** | -1.52*** (0.37) | -0.37 (0.47) | -1.15*** |

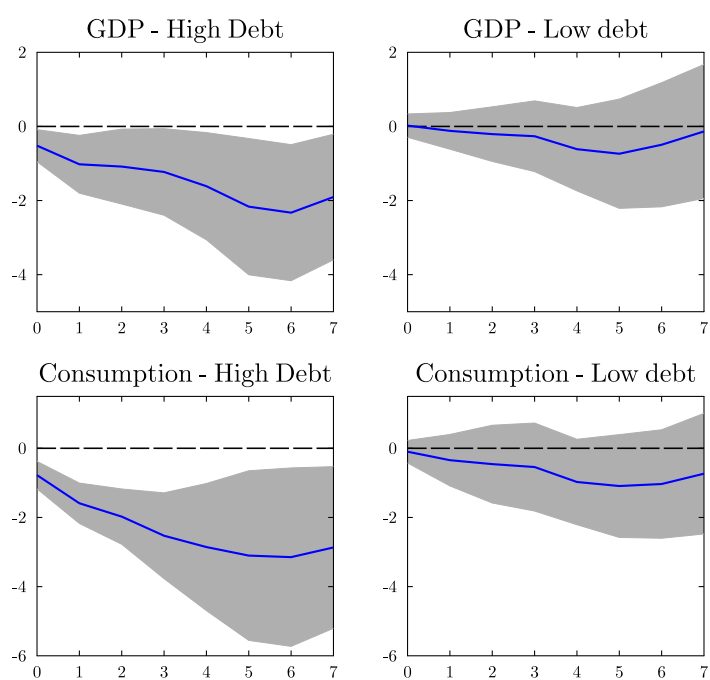
Notes: The table reports point estimates and robust standard errors clustered by country in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; ** significant at 10%; *** significant at 5%.

Table A8: Accounting for Serial Correlation (Effect in Year $t = 1$)

| Specification | GDP | | | Consumption | | |
|--------------------------------------|--------------------|-----------------|------------|--------------------|------------------|------------|
| | High Debt | Low Debt | Difference | High Debt | Low Debt | Difference |
| Baseline | -1.13*** (0.50) | -0.21 (0.32) | -0.92*** | -1.72*** (0.38) | -0.44 (0.46) | -1.28*** |
| Driscoll-Kraay standard errors | -1.13*** (0.34) | -0.21 (0.23) | -0.92*** | -1.72*** (0.33) | -0.44* (0.31) | -1.28*** |
| Clustered standard errors by time | -1.13*** (0.51) | -0.21 (0.34) | -0.92*** | -1.72*** (0.37) | -0.44 (0.49) | -1.28*** |

Notes: The table reports point estimates and standard errors in parentheses. In each case the shocks are normalized so that the CAPB rises by 1% of GDP in year $t = 0$. *Significant at 16%; **significant at 10%; ***significant at 5%.

Figure A1: Estimating for longer Horizon



Note: Cumulative changes (in per cent) in response to a shock of 1% of GDP to the cyclically-adjusted primary balance over $h = 0, 1, 2, 3, 4, 5, 6, 7$ years. The shaded areas indicate 90% confidence bands based on robust standard errors clustered by country.