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FINANCIAL CYCLES AND FISCAL MULTIPLIERS

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ABSTRACT

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Financial Cycles and Fiscal Multipliers*

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Abstract. We show that fiscal multiplier estimations may be biased by movements in asset and credit markets, as they facilitate spurious correlations of changes in cyclically adjusted revenues and spending with GDP growth via an identification bias and an omitted variable bias, thus overstating episodes of expansionary consolidations and downplaying contractionary consolidations. When controlling for asset and credit market movements in otherwise standard approaches to identification, we find multipliers to increase on average by 0.3 to 1 units. Fiscal consolidations are thus more likely to be contractionary and more harmful to growth than expected by some strands of the existing literature.

Keywords. multiplier effects; fiscal policy; asset markets; credit markets

JEL classification. C22, E62, H30

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1. Introduction

The large and growing literature on fiscal multipliers has produced a wide range of results ranging from negative multipliers (implying expansionary consolidations) to large positive multipliers (implying self-defeating consolidations). The lack of consensus seems to be partly due to different methods of identification of exogenous fiscal shocks and the inclusion or omission of important variables (Gechert 2015).

Several identification schemes have been applied to resolve the issue of endogeneity of budgetary components to business cycle fluctuations. One standard measure is using the cyclically adjusted primary balance (CAPB) in event studies (Alesina and Ardagna 2010). Another involves the cyclical adjustment of budget components by imposing budget sensitivities to GDP from external information onto structural vector-autoregressive models (SVARs) (Blanchard and Perotti 2002). However, the adjustment regarding business cycle movements may not be enough in the presence of pronounced financial market movements that influence the budget and GDP over and above what is generally recognised as business cycle swings (Guajardo et al. 2011; Perotti 2011; Bornhorst et al. 2011). Such influences tend to downward bias multiplier estimates.

The mechanism can be exemplified as follows: Consider an asset price boom that leads to higher revenues through capital gains and turnover taxation, unaccounted for by the usual budget elasticities and thus would falsely signal an improvement in the fiscal stance as measured by business cycle adjusted budget variables. If the asset price boom is followed by an increase in output, the positive correlation of the measure of the fiscal stance with output would be falsely deemed an episode of expansionary consolidations. The very same argument holds for downturns of asset price cycles where the cyclically adjusted balance and GDP are likely to exhibit a coincidental deterioration, which could be misinterpreted as a causality running from public deficits to decreasing GDP. Both situations would lead to underestimations of fiscal multipliers.

This paper contributes to the existing literature on fiscal multipliers by allowing for an impact of asset and credit market movements on the public budget and GDP in an otherwise standard VAR framework. In a first step, we show some descriptive evidence and lay out the channels through which financial cycles can affect budgetary components; in a second step, we quantify the possible bias on multiplier estimations by employing established identification schemes, namely the CAPB and the SVAR approach; we compare the resulting multiplier effects in the case of inclusion vs. exclusion of a private net-wealth proxy. For the CAPB identification, we use a recursive VAR and compare the results of a fiscal consolidation shock. For the SVAR, we test the potential bias regarding government spending and net-revenue impulses separately. Estimates are based on US quarterly data ranging from 1960:1 to 2015:4.

To the best of our knowledge, this is the first study to quantify the potential downward bias that has been claimed by Guajardo et al. (2011) and Perotti (2011) within a VAR approach. As opposed to Yang et al. (2013), who address only the usual identification bias in a single equation framework, we are thus able to allow for an additional omitted variable bias from movements in asset and credit markets on GDP, which could amplify the possible downward bias on multiplier estimations; second, with the structural VAR identification, we can disentangle the possible identification bias stemming from endogenous discretionary reactions of policymakers to the business cycle from the one that is central to the present paper – the endogeneity of cyclically-adjusted budget variables to movements in asset and credit markets. Third, we can coherently test the CAPB and Blanchard and Perotti (2002) approaches for similar biases, and disentangle the effects for spending and revenue side shocks. Fourth, in addition to asset market movements, we allow for an influence from credit markets as they may alter the net wealth position and interfere with the influence of asset swings on the budget. Fifth, we conduct a battery of robustness tests, capturing for example a structural break around 1980 and possible differences concerning the reaction of private consumption and investment. What is more, we present a formal framework to show both the identification bias and the omitted variable bias that can occur in the presence of asset and credit market movements.

Our results confirm the hypothesis of Guajardo et al. (2011). We find downward-biased multipliers from identifications based on prior information regarding business cycle endogeneity, namely the CAPB and standard structural VAR approaches, as they overlook the influence of asset and credit market movements on GDP and the budget. Multipliers are on average about 0.3 to 1 units higher when taking this influence into account. Effects are concentrated in the period after the 1980s when financial cycles arguably gained importance. The pure identification bias is relevant for revenue-side shocks, where contemporaneous correlations with financial cycles are much stronger. For spending shocks, the omitted variable bias that influences the dynamics of the GDP reaction seems to be more relevant. While the effects seem to run both through private consumption and investment, the latter even exhibit a qualitatively different reaction when accounting for financial cycles. As a general conclusion, consolidations are more likely to be contractionary and could be more harmful to growth than expected from the results of some of the existing literature.

The fiscal multiplier literature has discussed several omitted variable biases, such as the influences of international spillovers (Hebous and Zimmermann 2013), the monetary policy reaction (Woodford 2011), the exchange rate regime (Corsetti et al. 2012), public debt (Favero and Giavazzi 2007), and liquidity or credit constraints in recessions (Eggertsson and Krugman 2012) with empirical applications for example in Auerbach and Gorodnichenko (2012). Note that our approach is different from the latter, as we focus on a general downward bias that occurs both in the upswing and in the downswing of asset and credit markets.

The paper is structured as follows: Section 2 presents some descriptive evidence and lays out the channels through which asset and credit market variables affect the budget and GDP and their working through the identification and omitted variable bias. Section 3 outlines the empirical strategy and describes the dataset. In section 4, we explain the structure and the identification strategy used and discuss the properties of the identified fiscal shocks of the baseline and augmented models. Section 5 compares the effects of fiscal shocks in the baseline and augmented models, including several robustness checks. The final section concludes.

2. Descriptive Analysis and Theoretical Channels

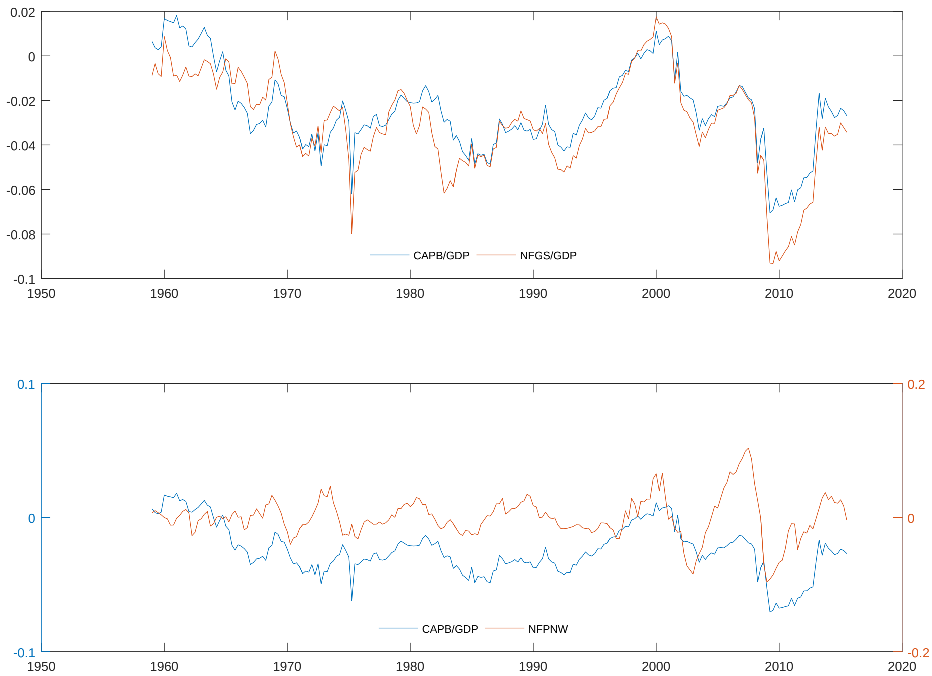
In this section, we discuss the relation between assets and liabilities, GDP and budgetary components. A brief look at the time series of budget variables and financial cycle proxies and the correlations between them is followed by a discussion of the channels of influence of financial swings on the budget in order to make sense of the observed patterns. A formal approach is presented in Appendix A: we distinguish a possible identification bias and an omitted variable bias when estimating fiscal multipliers in the presence of financial cycles.

A broad measure of private sector's financial involvements, namely the non-financial sector private net wealth (NFPNW), is chosen to account for a wide spectrum of possible channels, including both households and firms and both assets and liabilities. This net private wealth proxy is compared to four different budget variables; the cyclically adjusted primary balance (CAPB), the net federal government savings (NFGS), cyclically adjusted revenues (T) and outlays (G) of the federal budget as recorded by the Congressional Budget Office (CBO).

Figure 1 shows co-movement of the time series of CAPB, NFGS and NFPNW. The upper panel shows the difference a cyclical adjustment makes for the budget. Although the CAPB is less volatile than the unadjusted NFGS, many of the ups and downs are still similar. The lower panel of figure 1 shows that these ups and downs often co-move with the financial cycle proxy, in particular since the 1980s.

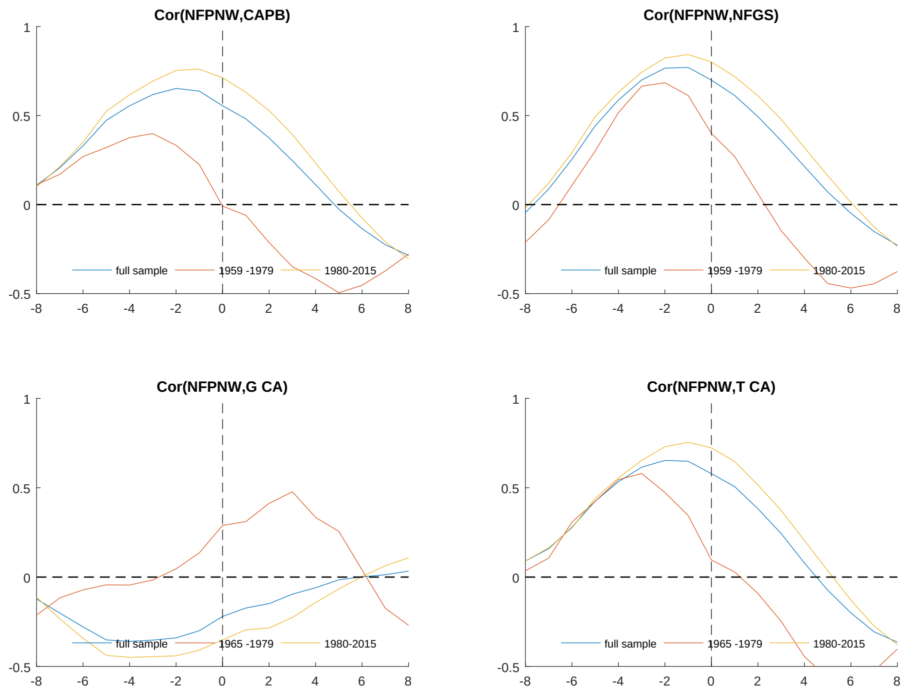
Figure 2 presents the correlation coefficients of leads and lags of the NFPNW with four fiscal variables. We take the CAPB-to-GDP ratio, the NFGS-to-GDP ratio and

Figure 1: CAPB, NFGS and NFPNW



(Source: CBO, FRB Flow of Funds and authors' own calculations)

Figure 2: Non-financial private sector net wealth Correlations



(Source: CBO, FRB Flow of Funds and authors' own calculations) the logs of NFPNW, T and G, and de-trend all of them by the HP-filter with $\lambda = 1600$. A full sample (1959-2015 for NFGS and CAPB, and 1965-2015 for T and G due to data limitations) and two subsamples are compared. The upper right panel gives the correlations between NFPNW and the unadjusted budget balance (NFGS). Contemporary correlations are extremely high for financial market movements and the budget. The peak of the correlations occurs for changes in NFPNW, which happen about one or two quarters before the change in the budget. Of course, since the NFGS is unadjusted for business cycle fluctuations, one would expect a strong correlation through GDP-related effects. However, turning to the upper left panel reveals that there is no significant decrease in the correlations when using the full sample and second subsample CAPB instead. Interestingly, contemporaneous correlation is close to zero for the earlier subsample when financial cycles were arguably weak. For the second subsample starting 1980, the cyclical adjustment of the budget seems incomplete with respect to financial market fluctuations.

From analysing the lower panels, the main driver of these correlations seems to be the cyclically adjusted tax revenues. But government spending appears as well to be entangled with the financial cycle. Significant differences can be observed for the two subperiods considered. The first period stretches from 1959 to 1979 and the second from 1980 to 2012. Financial cycles seem to be more relevant in more recent periods, which can be explained by the fact that they became more pronounced since the 1980s. In this period, when there is a net wealth upswing, G tends to be below trend and T tends to be above trend. These results hold if we exclude recent crisis years (not shown). The stronger correlations imply a more severe bias of more recent multiplier estimates. A further decomposition shows that there are no main differences between the correlations for business sector net wealth and household sector net wealth (not reported here).

The correlations can be explained by a set of channels. Starting with gross assets, there are considerable side effects through capital gains and turnover taxation that are not covered by the usual elasticities (Eschenbach and Schuknecht 2004; Tagkalakis 2011). Thus, asset price cycles are found to be a major factor of unexplained movements in cyclically adjusted budgets (Morris and Schuknecht 2007; Price and Dang 2011). Congressional Budget Office (2002) identifies changes in stock prices as an important source of fluctuations in US federal revenue via capital gains taxes, loss carry-forward and the effect of stock options on the income of highly taxed earners. Extraordinary capital gains have a direct effect on tax revenues that are not covered by ordinary elasticities. More indirectly, appreciations of stock options shift employees to higher income tax brackets as soon as these options are exercised, thus changing tax elasticities as well. Since taxes on capital gains are due only until the underlying asset is sold, this explains the peaks of the correlations in Figure 2 to be lagged by some quarters. Capital losses, on the other hand, can be used to offset capital gains and other forms of taxable income and can therefore also shift elasticities.[1]

The effects of asset cycles on government spending are less obvious. Jaeger and Schuknecht (2004) find evidence for exacerbated pro-cyclical behavior of government spending due to boom-bust phases in asset prices. A protracted increase in asset prices and the accompanying surge in tax receipts might tempt policy makers to increase spending during the boom. Either they really think that increased tax revenues are there to stay or they are tempted to spend the extra money anyway, maybe due to upcoming elections or other reasons that might please voters in the short-term. However, our results in the lower left panel of figure 2 for the period from 1980-2015 are more in line with Tagkalakis (2011), who estimates fiscal policy reaction functions and finds that increasing asset prices tend to lower government spending. Honohan and Klingebiel (2003) take a look at banking crisis and find considerable fiscal costs. Our data lend support to this finding, since when we exclude financial crisis years, the negative correlation largely disappears (results not shown).

[1] Current law puts a ceiling to the amount of capital losses used to offset other forms of taxable income, but unused capital losses can be carried forward (Auten 1999).

Turning to the impact of private liabilities, direct effects come via tax exemptions and interest deductions. Debt leveraging reduces companies' tax bills, for issuing debt enjoys tax privileges over issuing shares (de Mooij et al. 2013). Companies financing investment projects with bonds instead of shares can end up with a significantly lower tax burden (Miller 1977; Graham 2000). Moreover, the US allows for home mortgage interest deduction.[2] Thus, an increase in overall debt levels should lead to decreases in tax revenues. However, these effects are empirically difficult to observe. This is due to more indirect effects with opposing sign. Bénétrix and Lane (2011) investigate the impact of private credit market fluctuations on fiscal balances. They identify two main channels that explain positive correlations between debt and tax receipts, via asset prices and via inflation. Credit growth fuels asset prices and therefore leads to increased federal tax revenues as explained above. Credit growth could also fuel inflation, fostering the fiscal drag and thus raising tax elasticities. In general, these intertwined effects of assets and liabilities call for the use of a net wealth variable as a measure of the financial cycle, capturing all channels at once.

Even if the peaks are somewhat lagged, there is still considerable contemporaneous correlation between net wealth changes and cyclically adjusted budget components. Basically, effects of financial cycles on the budget that run through their impact on GDP should be covered by the usual budget elasticities. However, the substantial remaining correlation leads to unaccounted distortions of budget elasticities (Morris and Schuknecht 2007; Price and Dang 2011). This in turn implies biased identification of alleged structural fiscal shocks that are in fact driven by financial cycles. A financial cycle upswing would cause an increase in GDP (Drehmann et al. 2012) and a budget improvement. The latter would be deemed as a structural consolidation shock and would be viewed as the cause of the GDP hike if the financial cycle is not controlled for. The mirror image happens in a financial cycle bust. Such episodes lead to downward-biased multiplier

[2] 26 U.S.C. §163(h) of the internal revenue code.

estimates. In the following, this effect is called *identification bias*.

There is an additional potential *omitted variable bias* running through wealth effects. Basically, the impact of an increment in asset prices on the budget that is channeled through changes in consumer demand and thus sales and income taxes should be fully covered by the usual budget elasticities. However, when an expansionary fiscal shock leads to increases in asset prices, the latter can in turn provoke wealth effects that finally increase GDP as well. If this channel is switched off by omitting the financial cycle variable, the estimated GDP effect would be lower than the true one. In Appendix A, we present a formal model that captures the identification and omitted variable biases separately.

The various channels described above along with the simple empirical examination warrant a further investigation on how fiscal multiplier estimations might be affected by the financial cycle.

3. Empirical Strategy and Data

We follow a three-step approach. (i) In Section 4, we set up two baseline VAR models of standard identification approaches – either using the CAPB as a measure of exogenous fiscal shocks or employing the Blanchard and Perotti (2002) SVAR identification (BP henceforth).

The baseline for the BP model is a three-variate VAR of government spending net of transfers, GDP and tax revenues net of transfers akin to the standard model in Blanchard and Perotti (2002). The baseline CAPB specification is tested in a simple bivariate VAR, including the CAPB-to-GDP ratio and GDP with the structural shocks identified via recursive ordering. GDP effects of CAPB shocks have usually been tested in an OLS framework, defining episodes of fiscal consolidations, with the CAPB interpreted as the fiscal stance (Alesina and Ardagna 2010). We opt for a recursive VAR approach in order to provide a single coherent framework for all our tests; moreover, with the recursive VAR, we only impose contemporaneous exogeneity of the CAPB variable within the same quarter, exploiting recognition and implementation lags, while allowing for endogenous discretionary and automatic movements thereafter. With this strategy, we can disentangle the possible misidentification bias coming from endogenous discretionary reactions of policymakers to the business cycle from the one that is central to our study, namely the endogeneity of cyclically adjusted budget variables to movements in private wealth and debt.

(ii) We test as to whether the structural shocks derived from these two baseline models are orthogonal to lagged values of the financial cycle variable. If they are predictable, they cannot be deemed structural. For example, a positive coefficient of the financial cycle on both the CAPB and GDP would imply downward-biased multiplier estimates if financial cycles are not controlled for.

(iii) We augment our baseline VAR models with the financial cycle variable and assume a Choleski ordering for the additional variable, which is ordered last. Section 5 then compares the fiscal multipliers derived from these augmented models to their baseline counterparts. We would expect increased multipliers from the augmented models.

Estimations are based on US quarterly data from 1960 to 2015. Population, government budget series and GDP with its subcomponents stem from BEA tables. The CAPB series is taken from the Congressional Budget Office. Private sector wealth and debt data are provided by the Flow of Funds of the FRB. The GDP deflator, the effective federal funds rate, stock market and credit market data are taken from the FRED data base.

Nominal volumes are deflated by the GDP deflator and expressed in per capita terms, transformed to logs and multiplied by 100 to scale them in line with the variables in percentages. We thus have the log of real per capita government current spending net of transfers (g), the log of real per capita revenues net of transfers (τ), the log of real GDP per capita (y), and the log of non-financial private sector net wealth (f). CAPB is included as the CAPB-to-GDP ratio (s). Series are seasonally adjusted by the original sources or by the ARIMA X12 procedure.

All variables included have been tested for a unit root by the augmented Dickey-Fuller test and cannot be rejected to be I(1) at the 10 percent critical level. Johansen tests for cointegration rank (including a trend and four lags) reject a rank of zero for the primary specifications (results not reported). Cointegration makes it feasible to apply a classic VAR approach to non-stationary data as has been shown by Phillips and Durlauf (1986); West (1988); Fanchon and Wendel (1992) for example.

4. Structure and Identification

The terminology of the AB-model in Lütkepohl (2006: 364) is applied to specify the structural shocks. The structural form of the VAR model can be expressed as

$$\boldsymbol{A}X_t = \boldsymbol{A}\boldsymbol{\Gamma}(L)X_{t-1} + \boldsymbol{A}v + \boldsymbol{B}\varepsilon_t \tag{1}$$

$$u_t = \boldsymbol{A}^{-1}\boldsymbol{B}\varepsilon_t \tag{2}$$

$$\Sigma_u = \boldsymbol{A}^{-1}\boldsymbol{B}\Sigma_\varepsilon\boldsymbol{B}'(\boldsymbol{A}^{-1})' \tag{3}$$

with X_t being the K-dimensional vector of endogenous variables and v representing the vector of exogenous variables, namely a constant, a linear time trend and a dummy for 1975q2 covering an extreme temporary tax rebate that affects both CAPB and tax revenues. $\boldsymbol{\Gamma}(L)$ is a 4th-order lag polynomial of the $K \times K$ matrix $\boldsymbol{\Gamma}$, containing the coefficients of the endogenous variables and their lags.[3] ε_t is a K-dimensional vector of structural form disturbances (exogenous shocks). $\boldsymbol{A}$ and $\boldsymbol{B}$ are $K \times K$ factorization matrices and contain the contemporaneous dependencies among the endogenous variables and the structural shocks, respectively. A formal derivation of the identification of

[3] $\boldsymbol{\Gamma}(L)$ needs to be invertible for the VAR to be stable. That is, the coefficient matrices of $\boldsymbol{\Gamma}(L)$ must be absolutely summable. The coefficients of higher order of $\boldsymbol{\Gamma}(L)$ must converge to zero (Lütkepohl 2006: 27).

the structural model from the reduced-form VAR and of the impulse-response functions (IRF) can be found in Appendix B.

4.1. Baseline Models

In general, restrictions are set from prior economic information on elasticities, assumptions on institutional settings and recognition, implementation or response lags. To measure the effects of fiscal policy changes with the CAPB in our baseline setting, we set up a bivariate VAR with a lag order of four and the vector of endogenous variables

$$X_{CAPB}^{base} = \begin{bmatrix} s_t & y_t \end{bmatrix}'. \tag{4}$$

For identification of the CAPB-VAR we follow a simple Choleski decomposition. The CAPB-to-GDP ratio is ordered first since it is taken to represent structural changes in fiscal policy stripped of automatic endogenous reactions to y. Moreover, as argued in Fatás and Mihov (2001), due to recognition and implementation lags, discretionary fiscal policy should not respond to GDP within the same quarter and should thus be contemporaneously exogenous, i. e. ordered prior to GDP.

According to the recursive ordering, $\boldsymbol{A}$ becomes a lower triangular matrix with unit entries on the main diagonal. The $\boldsymbol{B}$ matrix collapses to a simple diagonal matrix.

Instead of relying on cyclically adjusted budget variables to identify exogenous changes in the fiscal stance, Blanchard and Perotti (2002) use the face value fiscal time series and impose prior information on budget sensitivities directly to the estimation of the structural VAR model. With such a model, one can evaluate fiscal multipliers of spending and revenue components separately. The baseline specification is a three-variate fourth-order structural VAR model with

$$X_{BP}^{base} = \begin{bmatrix} g_t & y_t & \tau_t \end{bmatrix}'. \tag{5}$$

For identification in line with (Blanchard and Perotti 2002), we restrict the factorization matrices of the baseline specification as follows.

$$\Sigma_\varepsilon = \boldsymbol{I} \tag{6}$$

$$\boldsymbol{A} = \begin{bmatrix} 1 & -\bar{\alpha}_{gy} & -\bar{\alpha}_{g\tau} \\ -\alpha_{yg} & 1 & -\alpha_{y\tau} \\ -\bar{\alpha}_{\tau g} & -\bar{\alpha}_{\tau y} & 1 \end{bmatrix} \qquad \boldsymbol{B} = \begin{bmatrix} \beta_{gg} & 0 & \bar{\beta}_{g\tau} \\ 0 & \beta_{yy} & 0 \\ \beta_{\tau g} & 0 & \beta_{\tau\tau} \end{bmatrix} \tag{7}$$

Parameters with $\bar{(\cdot)}$ indicate a restriction. The BP approach uses additional prior assumptions on budget elasticities of tax revenues and institutional settings for identification. Leaving $\beta_{\tau g}$ unrestricted and setting $\beta_{g\tau} = 0$ implies that in the process of setting up the budget, spending decisions are taken prior to revenue decisions, an assumption which has been shown to be robust for US data by BP. For reasons of comparison, we follow BP, who set the output elasticities of government spending and revenues for the full sample such that $\alpha_{\tau y} = 2.08, \alpha_{gy} = 0$; these assumptions are the most decisive ones for identifying spending and tax shocks and they come close the cyclical adjustment that is done to the CAPB. g is assumed to be inelastic to taxes within a quarter ($\alpha_{g\tau} = 0$) and also tax revenues are assumed not to be driven by government spending over and above $\beta_{\tau g}$ thus imposing $\alpha_{\tau g} = 0$. Imposing these restrictions gives a just-identified model and has the advantage that we can leave the contemporaneous reaction of GDP to changes in net taxes and public spending unrestricted and have them determined by the data.

4.2. Properties of the Baseline Structural Shocks

If the specification of the baseline models is correct, their structural shocks should be independent of other influences. However, our hypothesis is that private wealth and debt changes have an influence on the public budgetary position and on GDP over and above the usual business cycle fluctuations. We test this hypothesis for each of the three models against the null of no influence for the vector of shocks ε_t via the dynamic OLS model

$$\varepsilon_t^x = \alpha + \sum_{i=1}^{4} \varepsilon_{t-i}^x \beta_{t-i} + \sum_{i=1}^{4} fcc_{t-i} \gamma_{t-i} + e_t \tag{8}$$

with fcc being the cyclical component of the HP-filtered financial cycle variable. We use the HP filter ($\lambda = 1600$) in order to remove the trend of f as compared to the stationary ε_t^x series. Dynamic multipliers are reported in Figure 6 in Appendix D. In line with our theoretical reasoning, the structural shocks derived for the CAPB and GDP are predictable and show a significantly (at 95% CI) positive correlation with changes in the financial cycle. That is, an increase in private net wealth can predict an alleged exogenous improvement to the budgetary position and GDP. For the Blanchard and Perotti (2002) model, the government spending shock is negatively correlated and the GDP and tax shocks being positively correlated to the financial cycle. Again, these results correspond to the arguments developed in Section 2.

4.3. Augmented Models

In order to deal with the endogeneity of the structural shocks in the baseline models, they are augmented by the log of non-financial private sector net wealth (f). Since we do not want to rule out a contemporaneous dependency of the financial cycle on the other variables and because we expect that the channels of influence from private wealth and debt on the budget and GDP take some time to materialise, for both the augmented CAPB and the BP model, we order f last in the VAR, i.e. we assume a Choleski ordering against the other endogenous variables. Results are, however, robust to ordering f first, as shown in Appendix D. So for the augmented models we have

$$X_{CAPB}^{augm} = \begin{bmatrix} s_t & y_t & f_t \end{bmatrix}' \tag{9}$$

$$X_{BP}^{augm} = \begin{bmatrix} g_t & y_t & \tau_t & f_t \end{bmatrix}' \tag{10}$$

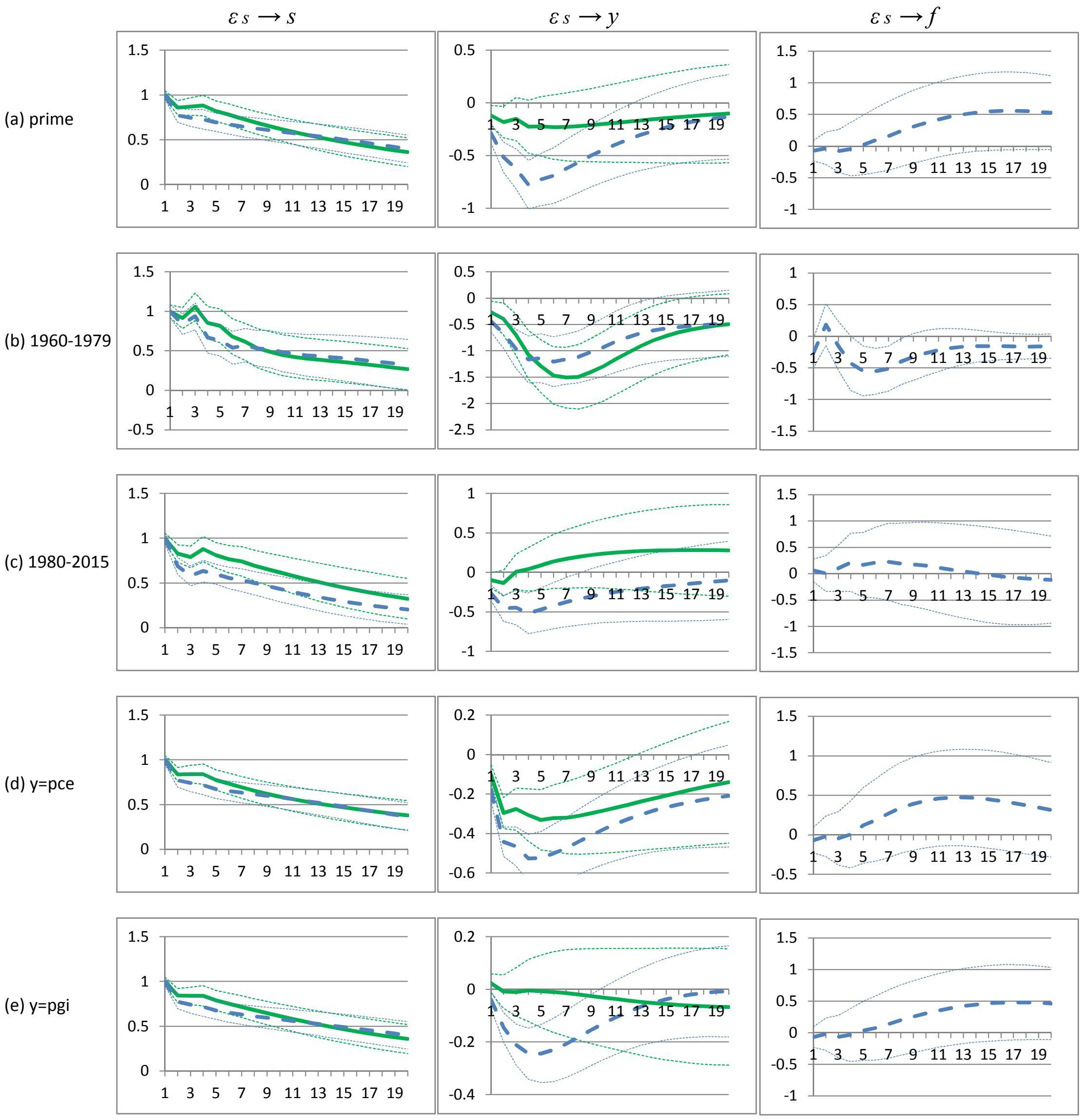
Note that while including the financial cycle variable does not make the CAPB a better estimate of the fiscal stance per se, it works as an additional filter, whereby the identified fiscal shocks are expected to be more likely to be exogenous. After solving the augmented models, we again retrieve the structural shocks and repeat the exercise of (8) to check whether the structural shocks are correlated with fcc, but find them orthogonal (results not reported).

5. Effects of Fiscal Policy Changes – Baseline vs. Augmented Models

The previous sections have shown that there are potential identification and omitted variable biases with respect to financial cycles in standard approaches to estimating fiscal multipliers. In order to quantify the impact of the bias, we now compare impulse-responses of shocks to budget variables in the baseline models to those of the augmented models both for the CAPB and the BP approach. For digits of multipliers of all models at selected horizons, refer to Table 1.

First, we simulate a 1% of GDP improvement in the CAPB-to-GDP ratio, i.e. a fiscal consolidation. Note that within our framework, the effects would be symmetric in case of a fiscal expansion. Figure 3 presents the IRFs with one-standard error bands for the baseline and augmented model, respectively. We focus on the reaction of aggregate demand (second column, y). Panel (a) covers our prime specification. Both models show a transitory contraction in GDP after the fiscal consolidation. The reaction is much more pronounced for the model that controls for the financial cycle; the response function of GDP remains significant for a much longer horizon. The impact multiplier

Figure 3: Impulse Responses to 1% Consolidation Shock in CAPB-to-GDP Ratio – Baseline (green solid), Augmented (blue dashed)



is twice and the peak multiplier three times as high for the augmented model with an absolute difference at the peak of 0.5 units. The cumulative multipliers are much higher for the augmented model (between two and three times as high), although reliability of the results lowers with an increasing horizon. In line with our hypothesis, controlling for financial cycle swings substantially increases the measured multiplier effects as the cyclical correlation that downward-biases the multiplier estimates is now controlled for.

Given that swings in the financial cycle have become much more pronounced since the 1980s, we split our sample to test the sensitivity of multiplier calculations. Panel (b) shows the effects for the period 1960-1979 when financial cycles arguably did not play a prominent role. In general, the level of multiplier estimates is higher for this subperiod with multipliers above 1 for both models. Augmenting the estimation with the financial cycle variable does not imply a significantly different GDP reaction, it is even a little lower for the augmented model. For the period 1980-2015 (panel (c)), marked by severe up- and downswings in private sector net wealth, the model ignoring these swings would estimate a very low impact multiplier, which even turns into an expansionary effect of the consolidation soon after. In contrast, the GDP reaction in the augmented model is much more in line with panel (a). Including the financial cycle seems to balance the GDP effects and gives much more robust results.

Panels (d) and (e) dig deeper into the aggregate demand reaction to the consolidation shock. In panel (d) private consumption expenditures (PCE) replace GDP in the model. The PCE reaction is scaled in % of GDP. Basically, the PCE reaction looks similar to GDP, even though the difference between the baseline and augmented models is less pronounced. In contrast, private gross fixed investment (PGI, also scaled in % of GDP), replacing GDP in panel (e), reacts markedly different for the two specifications. A model that ignores financial cycle swings would signal an essentially zero reaction of PGI to a consolidation shock, while controlling for private net wealth gives a PGI reaction very much in line with GDP. Summing up, it seems that both private consumption and investment channels are relevant for the impact of the financial cycle on multiplier estimates, but the investment channel marks a qualitative difference. For all specifications, the reaction of the financial cycle variable to the consolidation shock is essentially flat and insignificant.

For the baseline and augmented models following the BP approach, impulse responses of a government spending shock of 1% of GDP (ε_g) are presented in Figure 4. The responses are very much in line with those of the CAPB models, albeit on a different scale. For the full sample prime specification in panel (a), we find an impact multiplier of about 1 for both the baseline and augmented models; however, the dynamic GDP response is much more pronounced, with a peak multiplier above 2 for the augmented model, almost twice as high as for the baseline model. Both findings are quite plausible as we do not expect a severe identification bias for government spending shocks (i.e. a similar impact effect), but omitting f in the baseline model could downward bias the dynamics of the GDP response through the ignored wealth effect. In the augmented model, the g shock leads to a financial cycle boom that could explain the more persistent GDP reaction. Doing the sample split (panels (b) and (c)) again reveals that the inclusion of f is much more decisive for the more recent years. In panels (d) and (e) we consistently find that the baseline and augmented models differ in both private demand channels. Government spending crowds in PCE more strongly in the augmented model; PGI reacts slightly positive (though insignificant) for the augmented model but exhibits crowding out for baseline model. Taxes expectedly react very much in line with GDP, given that we impose a positive contemporaneous tax elasticity.

The BP model can also identify a tax shock, even though the literature shows that multiplier levels are quite sensitive to the identifying assumptions, in particular to the imposed tax elasticity $\alpha_{\tau y}$ (Caldara and Kamps 2012; Mertens and Ravn 2014). Therefore, we will focus on the difference between our baseline and augmented models when discussing Figure 5. For the full sample in panel (a) in the baseline model ignoring finan-

Table 1: Multipliers for Baseline and Augmented CAPB and BP Models

Specification		Model	Impact Quarter 1	Cumulative 10	20	Peak	(Quarter)
prime	s	base.	0.12	0.25	0.27	0.23	(6)
	s	augm.	0.29	0.81	0.68	0.78	(4)
	g	base.	1.02	0.84	0.66	1.31	(3)
	g	augm.	1.11	1.63	1.58	2.04	(7)
	t	base.	-0.03	0.10	0.51	0.36	(20)
	t	augm.	0.14	0.63	0.66	0.46	(4)
1960-1979	s	base.	0.26	1.47	1.69	1.51	(7)
	s	augm.	0.44	1.46	1.48	1.21	(6)
	g	base.	1.39	1.98	1.82	2.37	(6)
	g	augm.	1.41	1.87	1.72	2.10	(3)
	t	base.	0.41	1.19(5)[a]		1.12	(7)
	t	augm.	0.32	1.44	2.23	0.56	(4)
1980-2015	s	base.	0.10	-0.11	-0.30	0.14	(2)
	s	augm.	0.26	0.64	0.62	0.52	(4)
	g	base.	0.94	0.45	0.05	0.99	(3)
	g	augm.	1.06	1.62	1.64	1.99	(7)
	t	base.	0.01	0.44		0.45	(17)
	t	augm.	0.11	0.96		0.32	(4)
pce	s	base.	0.10	0.38	0.40	0.33	(5)
	s	augm.	0.18	0.62	0.60	0.53	(4)
	g	base.	0.36	0.40	0.35	0.51	(5)
	g	augm.	0.44	0.81	0.82	1.06	(8)
	t	base.	0.00	0.24	0.59	0.24	(18)
	t	augm.	0.09	0.57	0.91	0.32	(4)
pgi	s	base.	-0.02	0.01	0.05	0.07	(20)
	s	augm.	0.04	0.26	0.19	0.24	(4)
	g	base.	-0.01	-0.28	-0.44	-0.01	(1)
	g	augm.	0.03	0.07	-0.01	0.15	(9)
	t	base.	-0.10	-0.15	0.01	0.23	(20)
	t	augm.	-0.03	0.09	0.13	0.09	(3)

The equations for impact, cumulative and peak multipliers can be found in Appendix C.

[a] Calculated for quarter 5, since the IRF of τ turns negative thereafter.

Figure 4: Impulse Responses to 1% of GDP Shock in Government Spending Net of Transfers for BP VAR – Baseline (green solid), Augmented (blue dashed)

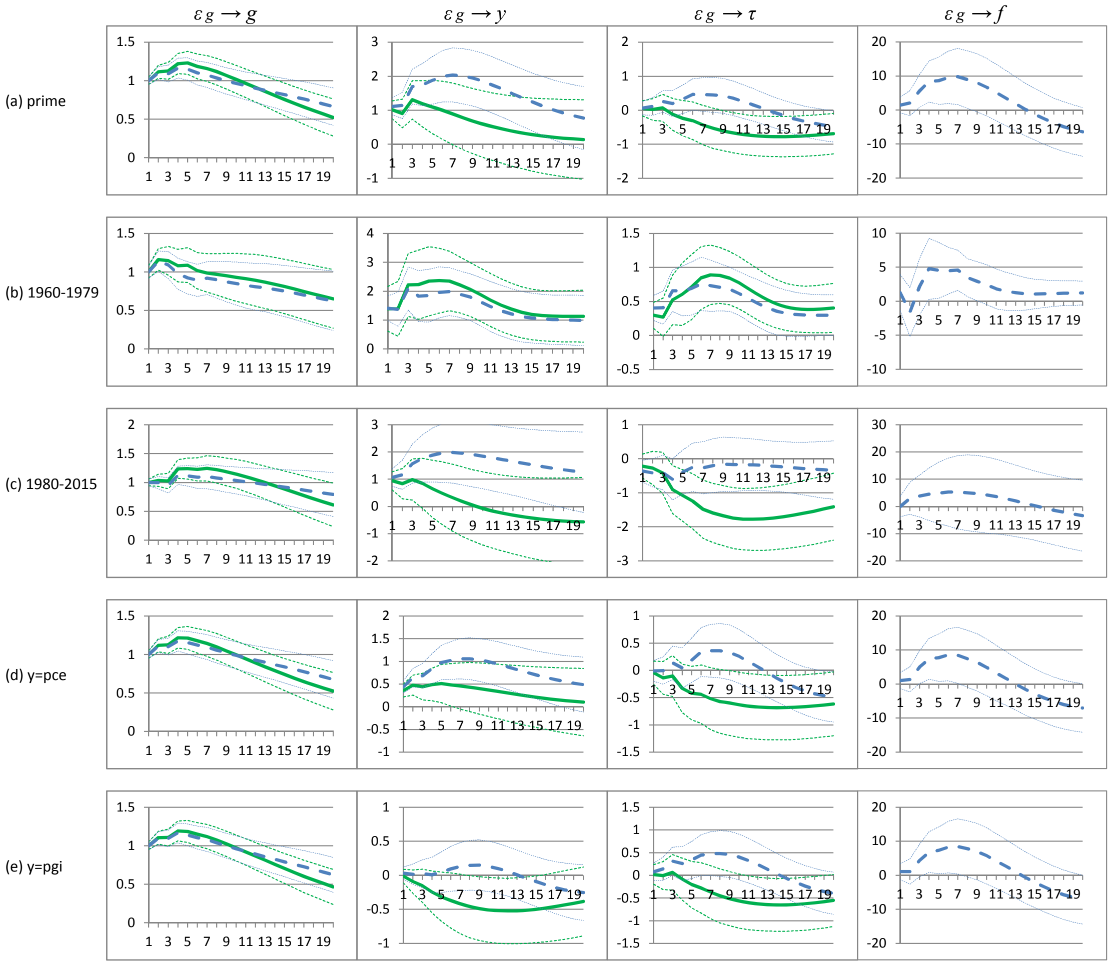
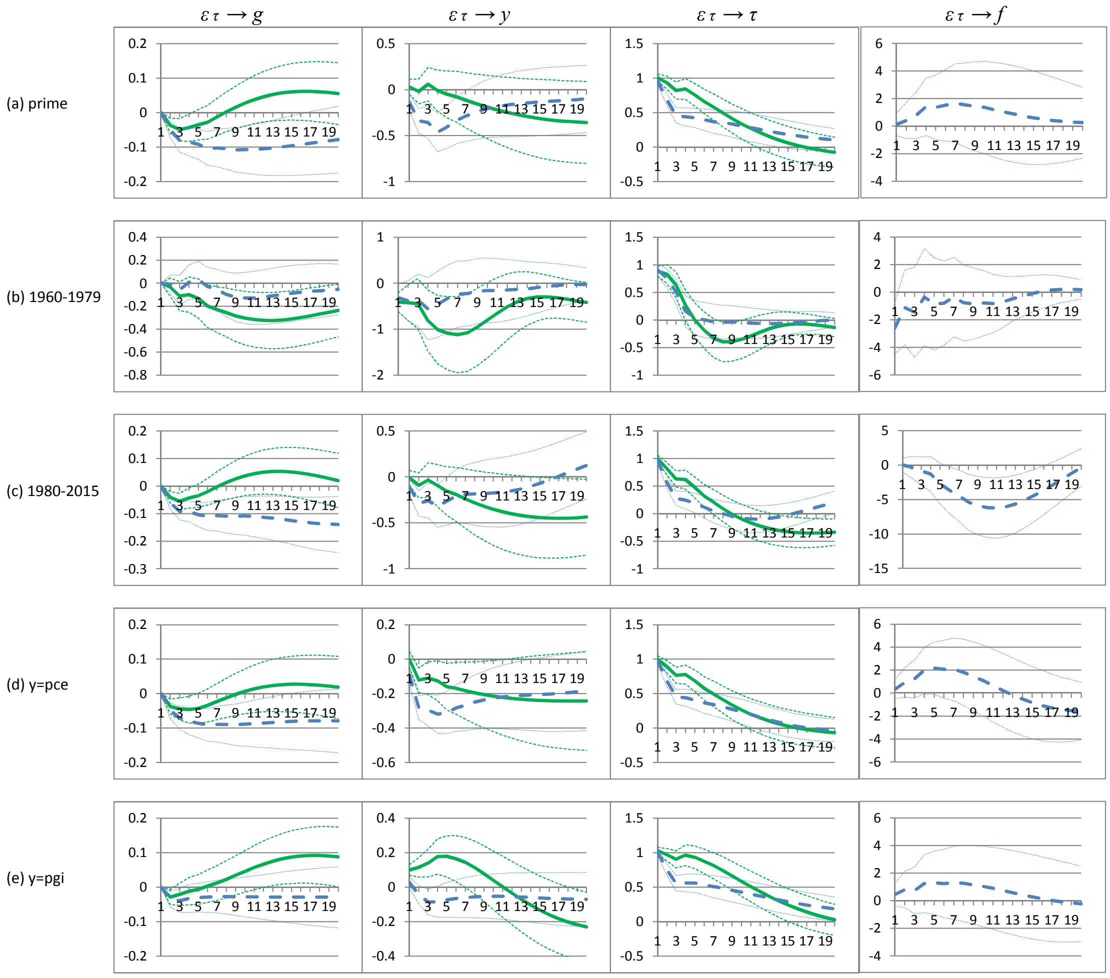


Figure 5: Impulse Responses to 1% of GDP Shock in Taxes Net of Transfers for BP VAR – Baseline (green solid), Augmented (blue dashed)



cial cycles, a tax hike of 1% of GDP in prospective net revenues leads to an insignificant close to zero reaction of GDP, which turns slightly negative over longer horizons. In contrast, the augmented model shows a plausible significantly negative reaction with a peak multiplier of around 0.5 that peters out after some quarters. Considering the sample split in panels (b) and (c) shows that including the financial cycle tames the GDP reaction with similar responses for the two subsamples, while the baseline model exhibits somewhat wild GDP reactions with close to zero impact multipliers for the more recent years. In panel (d), PCE reacts plausibly negative for both models, but the effect is expectedly stronger for the augmented model. In panel (e) the tax hike would lead to an implausible PGI increase in the baseline model, while PGI slightly falls after some quarters in the augmented model. Consistent with the spending and CAPB shock, the PGI reaction is qualitatively different and seems to be an important channel for the effect of financial cycles on multiplier estimates. Private net wealth reacts insignificantly for most specifications; only for some exceptions, the reaction is slightly significantly negative.

Generally, we find consistent empirical support for our hypothesis: Estimated multipliers are considerably larger when controlling for private wealth and debt in otherwise standard models. Moreover, concerning impact multipliers, the differences are stronger for CAPB shocks and tax shocks where the identification bias should play a more crucial role. For government spending shocks, the omitted variable bias in the baseline model, which should be more relevant for the dynamics than for the impact, seems to be more important. We present results of additional robustness checks in Appendix D.

6. Conclusions

We investigated whether movements in private wealth and debt imply both an identification bias and an omitted variable bias in standard multiplier estimation techniques that rely on prior information regarding endogeneity of fiscal time series with respect to the normal business cycle. In line with a growing literature (Guajardo et al. 2011; Perotti 2011; Yang et al. 2013), we argued that in the presence of movements in private net wealth standard approaches can lead to identifications that downward bias the estimated multiplier both in a financial cycle upswing and downswing.

To test this hypothesis, we set up a formal framework to pin down the impact of the omission of these channels on estimated multiplier values; the derivation showed that there should be a downward bias of estimated multipliers in the presence of movements in private net wealth in both directions. We then quantified the possible bias on multiplier estimations by employing empirical models of established identification schemes, namely the CAPB and Blanchard and Perotti (2002) method, and compared their resulting multipliers in the case of inclusion vs. exclusion of private debt and wealth proxies. For the CAPB identification, we used a recursive VAR and investigated the effects of a fiscal consolidation shock. The Blanchard and Perotti (2002) method enabled us to test the effects on government spending and tax multipliers separately.

Our results confirmed the hypothesis of Guajardo et al. (2011). We found downward-biased multipliers from identifications based on prior information regarding business cycle endogeneity, such as using the CAPB and standard structural VAR approaches, as they overlook the influence of financial cycles on budget components. Multipliers are on average about 0.3 to 1 units higher when taking these influences into account. These findings are robust to numerous alternative specifications. Fiscal consolidations thus are more likely to be contractionary and could be more harmful to growth than expected from the results of some of the previous literature.

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A. Appendix: Formal Model

To phrase the arguments in a more formal way, the simple static model in Perotti (2011) is extended in the following. Consider the true data-generating process to consist of three simplified equations, one for the change in the unadjusted primary balance as a share of GDP (s'), one for the change in the log of real GDP per capita (y) and one for the change in the log of nonfinancial private sector net wealth (f). The system reads

$$\boldsymbol{A}X_t = \boldsymbol{\varepsilon} \tag{11}$$

$$\Delta s' - \alpha_{sy}\Delta y - \alpha_{sf}\Delta f = \varepsilon_s \tag{12}$$

$$-\alpha_{ys}\Delta s' + \Delta y - \alpha_{yf}\Delta f = \varepsilon_y \tag{13}$$

$$-\alpha_{fs}\Delta s' - \alpha_{fy}\Delta y + \Delta f = \varepsilon_f \tag{14}$$

with $Cov(\varepsilon_i, \varepsilon_j) = 0 \quad if \quad i \neq j$.[4] The unadjusted primary budget surplus (s') depends on truly exogenous changes to the fiscal stance by the policymaker (ε_s), on y via automatic stabilizers α_{sy} , and on the financial cycle (f) via automatic α_{sf} reactions.

Equation (13) is a simplified GDP reaction function: Output reacts to changes in the fiscal stance, through α_{yf} to changes in the financial cycle and to orthogonal business cycle shocks ε_y that may capture all other changes. Unlike Perotti, who models his financial market variable as white noise positively correlated with economic activity, the financial cycle f is modelled via an own equation (14), to allow for the case of an omitted variable bias.

Next, the cyclically-adjusted primary balance stripped of automatic stabilizers is defined: $\Delta s = \Delta s' - \alpha_{sy}\Delta y$. Equation (12) thus shrinks to

$$\Delta s = \varepsilon_s + \alpha_{sf}\Delta f \tag{15}$$

which includes the truly exogenous shocks to the budget (ε_s), but also the disturbances channeled through f, which have not been filtered out by the business cycle adjustment. If $\alpha_{sf}\Delta f \neq 0$ then Δs is a biased identification of the fiscal stance ε_s.

Inverting the system to arrive at $X_t = \boldsymbol{A}^{-1}\boldsymbol{\varepsilon}$ shows directly the dependency of changes

[4] As compared to our SVAR AB model, here we use an A model for simplicity. Thus, we ignore all β terms that would for example include endogenous discretionary (countercyclical) reactions (β_{sy}). For quarterly data, the fiscal SVAR literature usually assumes the B matrix to be almost diagonal.

in the endogenous variables on the structural shocks:

$$\Delta s = \frac{1}{det(\boldsymbol{A})}((1-\alpha_{yf}\alpha_{fy})\varepsilon_s + \alpha_{sf}\alpha_{fy}\varepsilon_y + \alpha_{sf}\varepsilon_f) \tag{16}$$
$$\Delta y = \frac{1}{det(\boldsymbol{A})}((\alpha_{ys}+\alpha_{yf}\alpha_{fs})\varepsilon_s + (1-\alpha_{sf}\alpha_{fs})\varepsilon_y + (\alpha_{yf}+\alpha_{ys}\alpha_{sf})\varepsilon_f) \tag{17}$$
$$\Delta f = \frac{1}{det(\boldsymbol{A})}((\alpha_{fy}\alpha_{ys}+\alpha_{fs})\varepsilon_s + \alpha_{fy}\varepsilon_y + \varepsilon_f) \tag{18}$$
$$det(\boldsymbol{A}) = 1-\alpha_{yf}\alpha_{fy} - \alpha_{sf}\alpha_{fs} - \alpha_{sf}\alpha_{fy}\alpha_{ys} \tag{19}$$

As opposed to this extended model, consider the standard model applied in the literature:

$$\Delta s = \varepsilon_s \tag{20}$$
$$\Delta y = \alpha_{ys}\Delta s + \varepsilon_y \tag{21}$$
$$\tag{22}$$

where effectively it is assumed that $\alpha_{sf}, \alpha_{yf}, \alpha_{fs}, \alpha_{fy} = 0$. The arguments presented above, however, imply $\alpha_{sf}, \alpha_{yf}, \alpha_{fy} > 0$, while the sign of α_{fs} is not clear a priori. In the following, we consider the effects of a fiscal shock and isolate the biases caused by the identification problem when $\alpha_{sf}, \alpha_{yf} \neq 0$, and the omitted variable bias when $\alpha_{fs}, \alpha_{fy}, \alpha_{yf} \neq 0$.

Identification Bias

We isolate the identification bias (IB) by setting $\alpha_{fs}, \alpha_{fy} = 0$ and allowing for $\alpha_{sf}, \alpha_{yf} \neq 0$. The model shrinks to:

$$\Delta s = \varepsilon_s + \alpha_{sf}\varepsilon_f \tag{23}$$
$$\Delta y = \alpha_{ys}\varepsilon_s + \varepsilon_y + (\alpha_{yf}+\alpha_{ys}\alpha_{sf})\varepsilon_f \tag{24}$$
$$\Delta f = \varepsilon_f \tag{25}$$
$$det(\boldsymbol{A}) = 1 \tag{26}$$

The true multiplier in this model would come by truly exogenous fiscal shocks ε_s and would amount to $\gamma^{IB} = \Delta y/\varepsilon_s = \alpha_{ys}$. As positive changes in ε_s are consolidation efforts, in a Keynesian world we would expect $\alpha_{ys} < 0$. In the standard model, any change in s would be considered a fiscal shock, even if they are actually caused by ε_f. The measured multiplier $\hat{\gamma} = \Delta y/\Delta s$ would then be biased in relation to the true one by

$$\hat{\gamma} = \frac{\Delta y}{\Delta s} = \gamma^{IB} + \frac{\alpha_{yf}\varepsilon_f}{\varepsilon_s + \alpha_{sf}\varepsilon_f} \tag{27}$$

That is, in the presence of financial cycle shocks, one would measure a less negative impact of a fiscal consolidation on GDP under the plausible assumption that $\alpha_{sf}, \alpha_{yf} <$

0.

Omitted Variable Bias

An omitted variable bias in our case occurs when the indirect effects of a fiscal shock through induced financial cycles would be ignored by leaving out the financial cycle variable. These indirect effects might not occur instantaneously but dynamically. Our static model suffices to show the c.p. effect. Isolating the omitted variable bias (OVB) by setting $\alpha_{sf} = 0$ and allowing for $\alpha_{fs}, \alpha_{fy}, \alpha_{yf} \neq 0$ the true model shrinks to

$$\Delta s = \varepsilon_s \tag{28}$$

$$\Delta y = \frac{1}{1-\alpha_{yf}\alpha_{fy}}((\alpha_{ys} + \alpha_{yf}\alpha_{fs})\varepsilon_s + \varepsilon_y + \alpha_{yf}\varepsilon_f) \tag{29}$$

$$\Delta f = \frac{1}{1-\alpha_{yf}\alpha_{fy}}((\alpha_{fy}\alpha_{ys} + \alpha_{fs})\varepsilon_s + \alpha_{fy}\varepsilon_y + \varepsilon_f) \tag{30}$$

$$\tag{31}$$

The true multiplier in this case would be

$$\gamma^{OVB} = \frac{\Delta y}{\varepsilon_s} = \frac{\alpha_{ys} + \alpha_{yf}\alpha_{fs}}{1-\alpha_{yf}\alpha_{fy}} \tag{32}$$

while when ignoring the indirect effects we would end up with

$$\hat{\gamma} = \frac{\Delta y}{\Delta s} = \alpha_{ys} \tag{33}$$

where $\hat{\gamma} < \gamma^{OVB}$ if plausibly $\alpha_{yf}, \alpha_{fy} > 0$ and $\alpha_{fs} < 0$, where the latter would mean that a fiscal consolidation would lead to a net-wealth decline. Such a scenario might come into play, when Ricardian equivalence does not hold.

B. Appendix: Identification and IRFs

In order to solve the structural model and identify the structural shocks ε_t that are central for quantitative policy simulations, the VAR is estimated in reduced form

$$\boldsymbol{\Gamma}(L)X_t = v + \boldsymbol{A}^{-1}\boldsymbol{B}\varepsilon_t \tag{34}$$

$$= v + u_t \tag{35}$$

retrieving the K-dimensional vector of reduced form residuals u_t.

$$u_t = \boldsymbol{A}^{-1}\boldsymbol{B}\varepsilon_t \tag{36}$$

Equation (36) relates the reduced form shocks u_t and structural form shocks ε_t. Due to the multiple-way causation between the variables, the reduced form residuals u_t are almost certainly correlated with each other and therefore inappropriate to simulate exogenous policy changes. Thus, in a second step we solve for the structural shocks via

$$\varepsilon_t = \boldsymbol{B}^{-1}\boldsymbol{A}u_t. \tag{37}$$

This is done by taking the $K \times K$ variance-covariance matrix Σ_u of the reduced form residuals and by assuming ortho-normality of the structural shocks ($\varepsilon_t \sim (0, \Sigma_\varepsilon = I_K)$).[5] From (36) follows that

$$\Sigma_u = \boldsymbol{A}^{-1}\boldsymbol{B}\Sigma_\varepsilon\boldsymbol{B}'(\boldsymbol{A}^{-1})' = \boldsymbol{A}^{-1}\boldsymbol{B}\boldsymbol{B}'(\boldsymbol{A}^{-1})'. \tag{38}$$

Since (38) is over-parameterised, as it contains $2K^2$ unknowns and only $K(K+1)/2$ equations, we need to impose at least $2K^2 - K(K+1)/2$ restrictions from prior economic information on some parameters of $\boldsymbol{A}$ and $\boldsymbol{B}$ in order to calculate their remaining items. With just identified matrices $\boldsymbol{A}$ and $\boldsymbol{B}$, we are able to derive the structural shocks from (37). Afterwards, the *structural vector moving average representation* (SVMA) of the VAR can be determined:

$$X_t = \mu + \boldsymbol{\Theta}(L)\varepsilon_t = \mu + \sum_{h=0}^{p} \boldsymbol{\Theta}_i \varepsilon_{t-i} \tag{39}$$

with $\boldsymbol{\Theta}(L) = \boldsymbol{\Gamma}(L)^{-1}\boldsymbol{A}^{-1}\boldsymbol{B}$, $\mu = \boldsymbol{\Gamma}(L)^{-1}v$ and h being the respective horizon of interest. Note that $\boldsymbol{\Gamma}(L)$ must be invertible to allow for a MA representation.

Finally, the IRFs of the endogenous variables i to unit structural shocks to variable j at horizon h can be computed from the SVMA via

$$\boldsymbol{\Upsilon}_{i,j,h} = \frac{\partial x_{i,t+h}}{\partial \varepsilon_{j,t}}. \tag{40}$$

They show the deviation of variable i at horizon h from a steady state path of the model when the system is hit by an exogenous shock to variable j and can be interpreted as multipliers if they are scaled correctly.

C. Appendix: Definition of Multipliers

Multipliers are calculated either as the *impact* response of GDP divided by the initial fiscal impulse (FI)

$$k = \frac{\Delta y_t}{\Delta FI_t}, \tag{41}$$

[5] The assumption of ortho-normality is not restrictive. It ensures that the structural shocks are random and independent of one another and it pre-sizes their variance to easily interpret impulse responses later on. No information is lost, since the settings made here will be reflected in the coefficients of the $\boldsymbol{A}$ and $\boldsymbol{B}$ matrices.

or as the *cumulative* response function of GDP divided by the cumulative fiscal impulse function

$$k = \frac{\sum_h \Delta y_{t+h}}{\sum_n \Delta FI_{t+h}}, \tag{42}$$

or as the *peak* response of GDP with respect to the initial fiscal impulse

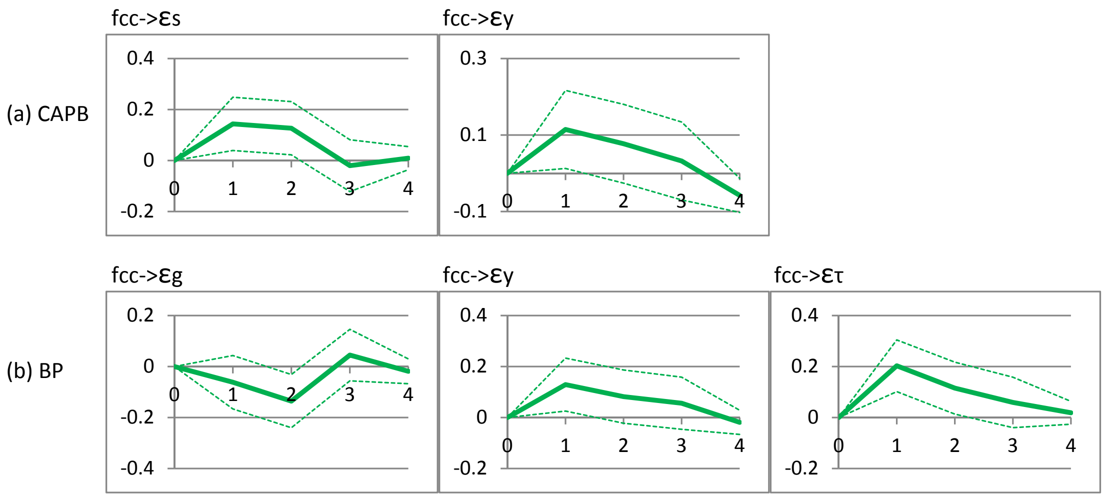
$$k = \frac{\max_h \Delta y_{t+h}}{\Delta FI_t}, \tag{43}$$

where $\Delta(\cdot)$ marks deviation from the steady state.

D. Appendix: Auxiliary results and robustness checks

Figure 6 shows the dynamic multipliers of the reactions of the identified structural shocks from the baseline model to an increment in the HP-filtered financial cycle variable stemming from a regression as in equation 8.

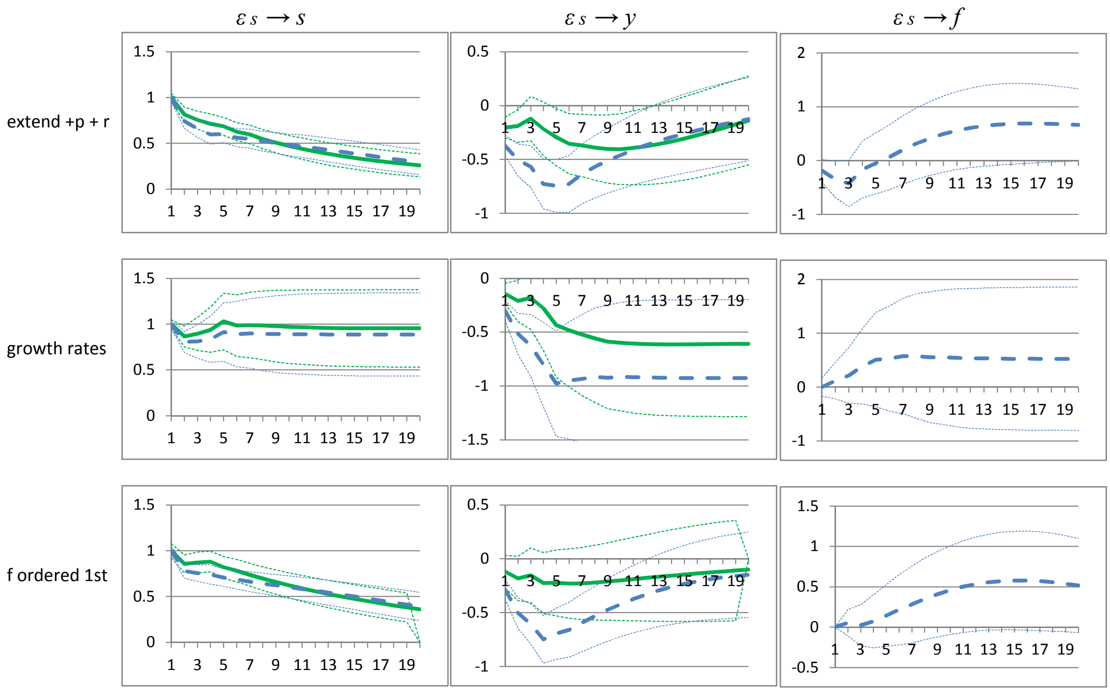
Figure 6: Predictability of structural shocks of baseline model from HP-filtered private net wealth



Figures 7 to 9 give the impulse responses for some additional robustness tests for the CAPB, spending and tax shocks. The upper panel of each figure uses an extended vector of endogenous variables, including the GDP deflator (p) and the real Fed Funds Rate (r) in line with other papers in the literature employing fiscal VARs (Perotti 2005; Favero and Giavazzi 2012). In terms of identification, r is ordered prior to the financial cycle but after the other variables as it is deemed not to provoke immediate changes in the other variables due to response lags, but could react to changes in other variables immediately. With regards to the two other variables, we follow the literature and order inflation after

GDP; however, results are robust to a reversed ordering of the two variables. In line with Perotti (2005), we assume an elasticity of government spending to changes in the price level of $\alpha_{gp} = -0.5$ and of taxes of $\alpha_{\tau p} = 1.25$. Generally, all findings remain intact for this extended specification.

Figure 7: Robustness: Impulse Responses to 1% Consolidation Shock in CAPB-to-GDP Ratio – Baseline (green solid), Augmented (blue dashed)



The middle panels estimate our models in log first differences (i.e. growth rates) instead of log levels and show the cumulative IRFs. A model in first differences would be preferable if the model in levels does not meet the co-integration assumptions. However, it eats away information on the medium to long-term dynamics. Therefore, the IRFs heavily depend on the effect on impact, which is why the GDP response to the government spending shock (Figure 8 middle panel) becomes quite similar for the baseline and augmented case, while the difference between baseline and augmented case is even reinforced for the CAPB and tax shocks.

The bottom panels test the impact of our ad-hoc assumption of ordering f last in the VAR. Even if we consider the other extreme, ordering f first, results change only very little. This is plausible given the close to zero response of f on impact for the augmented models.

Figure 8: Robustness: Impulse Responses to 1% of GDP shock to government spending net of transfers – Baseline (green solid), Augmented (blue dashed)

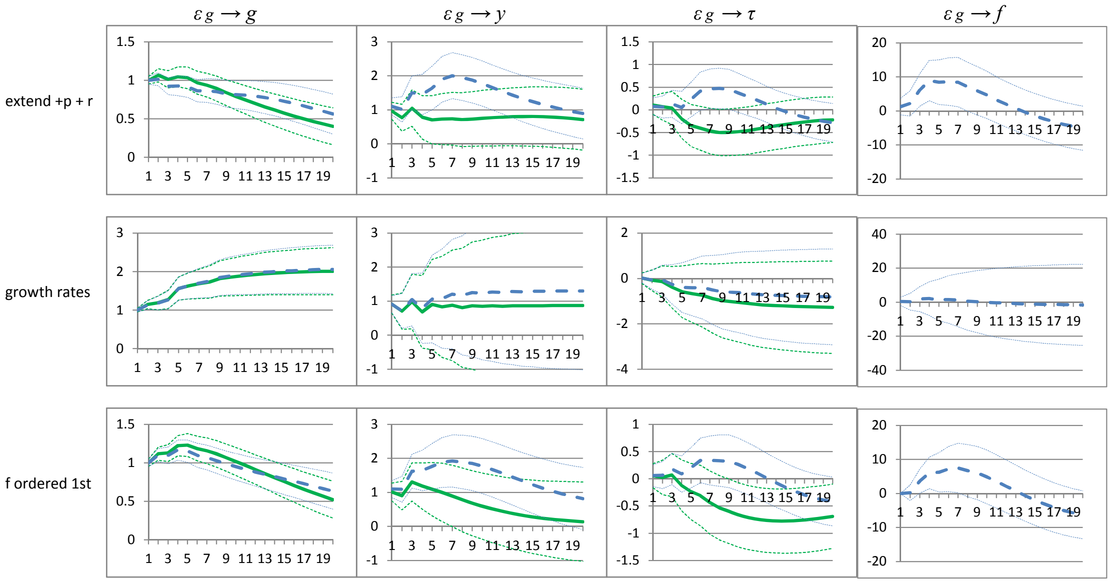
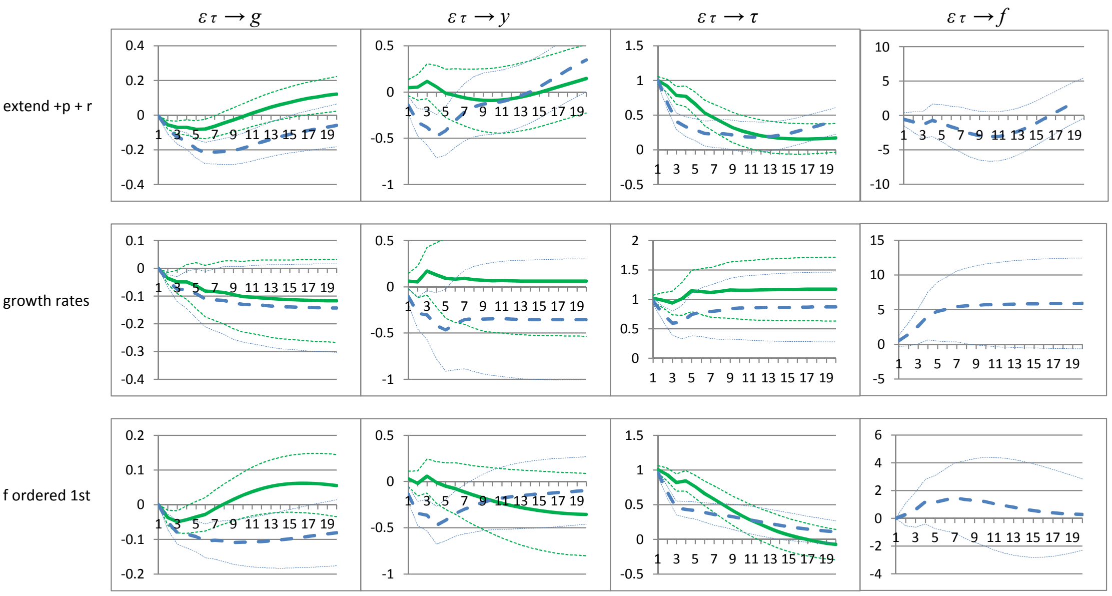


Figure 9: Robustness: Impulse Responses to 1% of GDP shock to taxes net of transfers – Baseline (green solid), Augmented (blue dashed)



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