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Recognizing the Bias: Financial Cycles and Fiscal Policy

by Nina Budina, Borja Gracia, Xingwei Hu and Sergejs Saksonovs

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Abstract

This paper argues that asset price cycles have significant effects on fiscal outcomes. In particular, there is evidence of debt bias—the tendency of debt to increase over the cycle—that is significantly larger for house price cycles than stand-alone business cycles. Automatic stabilizers and discretionary fiscal policy generally respond to output fluctuations, whereas revenue increases due to house price booms are largely treated as permanent. Thus, neglecting the direct and indirect impact of asset prices on fiscal accounts encourages pro-cyclical fiscal policies.

JEL Classification Numbers: E32, F34, G01, and H63

Keywords: housing cycles, public debt, private debt, debt bias.

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I. INTRODUCTION

Starting in the mid 70s, public debt in advanced economies has been rising steadily except for a period before the global financial crisis. Government debt fell significantly in advanced economies—from an average of 140 to 30 percent of GDP—after World War II until the mid 70s on the back of strong post war growth and financial repression (Abbas et al, 2011). Debt ratios began to rise again with the end of the Bretton Woods system of exchange rates and the two oil price shocks. The increase in debt was paused or even reversed in many advanced economies in the late 90s and early 2000s but has been significantly exacerbated by the recent financial and euro area crises.

Among the many explanations for this trend, surprisingly little attention has been paid to the role played by financial cycles. Observers have rightly pointed to expanding welfare states, moderating growth, and higher real interest rates as important factors behind the secular rise in sovereign debt levels across advanced economies. However, financial cycles have also become an increasingly important factor as financial deepening and increasing financial intermediation strengthened the interplay between credit and asset prices on the one hand and the public finances on the other.

Financial cycles involve a substantial and interrelated buildup of leverage across sectors of the economy exacerbating vulnerabilities. Leverage amplifies the business cycle and increases the likelihood of a financial crisis through heightened instability (Minsky, 1964). Once the bubble bursts, the downturn is often exacerbated by a downward spiral of falling asset prices and increasing real value of debt (Fisher, 1933). There are three main channels through which financial cycles are likely to affect fiscal policy: tax revenues (direct channel), automatic stabilizers (indirect output channel) and balance sheet transfer from the private sector (Eschenbach and Schuknecht, 2004). During a typical real estate and construction boom, fiscal balances tend to improve as revenues increase disproportionately benefiting from asset price inflation through a direct revenue channel and indirect output channel. When the cycle turns, private sector deleveraging leads to fiscal deterioration as asset prices decline and growth slows, especially if automatic stabilizers continue to function fully. In addition, the need to support banks can lead to sudden surges in sovereign debt via the bailout channel. Thus, financial cycles result in public debt bias when increases in debt during financial cycle downturns are larger than reductions during upturns.

While there is a clear link between financial cycles and public budgets, fiscal policy often does not account for financial fluctuations. Policy makers have learned to account for the ups and downs of ordinary business cycles when evaluating the strength of revenues or planning spending. However, as financial cycles are imperfectly correlated with business cycles (Poghosyan et al, 2014), focusing on business cycles exposes fiscal outcomes to financial sector dynamics above and beyond business cycle fluctuations. As a consequence, in many countries prior to the recent financial crisis, public debt did not fall despite strong credit-driven growth and windfall revenues but increased sharply in its aftermath. Political economy factors help explain why governments may spend most windfall revenues during financial sector booms, often magnifying the impact of house price shocks and leaving fiscal policy to absorb the impact of the shock in downturns (Benetrix and Lane, 2011).

In particular, in some euro area countries unsustainable real estate booms were treated as permanent and led to substantial fiscal imbalances. In the years prior to the 2009 financial crisis public sector debt fell moderately in percent of GDP in countries like Ireland and Spain, while private non-financial sector debt increased by 90 and 110 percent of GDP over 2002–09 respectively. Real estate booms boosted fiscal positions prior to the crisis but the subsequent bust triggered public debt increases of around 80 and 100 percent of GDP in Spain and Ireland, respectively.

This paper shows that financial cycles have a significant impact on fiscal outcomes and result in a larger debt bias—defined as a tendency of debt to increase over the financial cycle—than business cycles. First, we identify episodes of financial upturns and downturns using house prices, private non-financial debt and stock market prices. Second, we contrast these with episodes of output upturns and downturns. We estimate a panel VAR to quantify the debt bias allowing for dynamic interaction between house prices, real GDP growth and public debt while controlling for supply and demand shocks. The last section concludes and offers suggestions for enhancing fiscal policy design to better account for the impact of financial cycles.

The main findings are:

- Real estate cycles result in a public sector debt bias of around 5–6 percent of GDP on average over the cycle for a symmetric house price shock (10 percent increase in upturns and 10 percent decrease in downturns).
- This bias is stronger when debt—public, private or financial sector—is already high. On the other hand it is smaller for countries with low private sector debt.
- The risk of a significantly higher debt bias than the point estimates mentioned above is much larger for financial cycles than for stand-alone business cycles, especially for countries with high private or public debt.
- Automatic stabilizers and discretionary fiscal policy respond to output fluctuations more symmetrically across the cycle when they do not correlate to financial cycles. Thus, business cycles that don't coincide with real estate cycles result in a much smaller bias.

The paper is organized as follows. Section 2 discusses the identification of financial and business cycle episodes and introduces stylized facts. Section 3 presents VAR estimates of debt bias while section 4 analyzes in detail the factors driving debt bias. Section 5 provides some policy implications.

II. Stylized Facts of Financial and Output Gap Episodes

To assess stylized facts of fiscal policy and financial cycles, we identify episodes of high and low real house prices, real private sector debt, equity prices and output gap. The most common approach to identify 'financial cycles' is by using filtering techniques or turning point analysis. However, these approaches have difficulties identifying relevant episodes of rapid increase in private sector leverage as the sample gets dominated by the recent financial crisis.[1] This problem can be mitigated by using an episodic approach based on the behavior of financial variables relative to a benchmark. More precisely, a country specific downturn (upturn) is identified if the decline (increase) in a given variable (e.g. house prices) is more than one standard deviation below (above) the country-specific mean for at least three consecutive quarters. These events are then called low (high) episodes of a variable X. The dynamics of variables are then analyzed 10 quarters before and after the peak or trough of the corresponding variable. The data sample includes 30 countries from 1975Q1 to 2013Q3 although data availability varies by country and variable (see appendix).

House price growth episodes—both positive and negative—are more persistent than credit, equity or output growth episodes. We identify 59 (66) positive (negative) house price episodes (Table 1), 44 (45) private credit episodes, 55 (71) equity episodes and 55 (71) output gap episodes. While the growth of private sector debt, stock market index and output gap fully recovers to pre-peak and pre-trough levels within ten quarters, house price growth does not. The growth of house prices remains around 4.5 and 5.5 percentage points below its pre-tough and pre-peak levels, respectively, even after 10 quarters (Figure 1). In terms of volatility however, measured as the difference between peak and through, stock market episodes are the most volatile followed by house price and stock market episodes with output gap episodes being the least volatile.

[1] For example filtering techniques result in almost no cycles in the first part of the sample missing some relevant episodes of rapid increase in private sector leverage. Furthermore, identifying turning points in the series using the conventional BBQ algorithm (Classens et. al. (2011) and Hardy and Pagan (2002)) results in very few financial or GDP cycle downturns (the US would have had only 10 quarters of GDP downturn since 1975 and Poland zero since 1994). Standard censoring rules for BBQ algorithm consist, among other things, of each phase of at least 2 quarters and a cycle length of 5 quarters minimum.

Table 1. House Price Episodes 1/

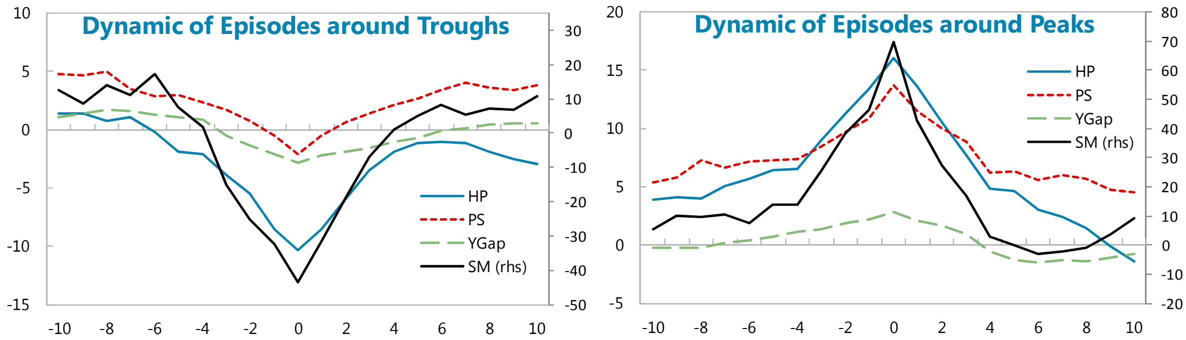
	High real HP growth episodes				Low real HP growth episodes			
	Country	Beginning	End	Maximum	Country	Beginning	End	Minimum
1	Spain	78Q1	78Q3	78Q2	Spain	79Q2	82Q3	82Q1
2	Spain	86Q4	90Q1	86Q4	Spain	11Q1	13Q2	11Q1
3	Spain	02Q2	04Q3	02Q2	Germany	82Q2	83Q1	82Q2
4	Germany	77Q3	79Q4	77Q3	Germany	83Q3	85Q1	83Q3
5	Germany	90Q1	91Q1	90Q1	Germany	97Q3	98Q2	97Q3
6	Germany	11Q3	12Q3	11Q3	Germany	02Q1	03Q1	02Q1
7	US	77Q4	79Q1	77Q4	Germany	04Q2	05Q4	04Q2
8	US	01Q4	02Q4	01Q4	US	80Q4	81Q4	80Q4
9	US	03Q2	06Q1	03Q2	US	07Q4	11Q4	07Q4
10	UK	79Q1	79Q3	79Q1	UK	76Q1	77Q3	76Q1
11	UK	87Q3	89Q2	87Q3	UK	81Q3	82Q2	81Q3
12	UK	99Q4	00Q4	99Q4	UK	90Q2	93Q1	90Q2
13	UK	02Q2	03Q2	02Q2	UK	08Q3	09Q3	08Q3
14	France	02Q3	06Q4	02Q3	France	81Q4	84Q4	81Q4
15	Italy	80Q4	81Q3	80Q4	France	92Q1	93Q1	92Q1
16	Italy	89Q1	90Q4	89Q1	France	95Q2	96Q2	95Q2
17	Korea	88Q2	90Q1	88Q2	France	08Q4	09Q4	08Q4
18	Korea	02Q1	03Q2	02Q1	Italy	83Q2	85Q4	83Q2
19	Korea	06Q4	07Q3	06Q4	Korea	91Q4	94Q4	91Q4
20	Sweden	88Q1	89Q3	88Q1	Korea	98Q1	99Q1	98Q1
21	Sweden	00Q1	01Q1	00Q1	Sweden	80Q2	84Q1	80Q2
22	Sweden	05Q4	06Q4	05Q4	Sweden	92Q1	93Q4	92Q1
23	Austria	89Q4	91Q2	89Q4	Austria	98Q2	99Q1	98Q2
24	Austria	91Q4	92Q2	91Q4	Belgium	80Q2	84Q2	80Q2
25	Belgium	76Q1	76Q4	76Q1	Belgium	84Q4	85Q3	84Q4
26	Belgium	77Q2	78Q1	77Q2	Estonia	08Q2	09Q4	08Q2
27	Belgium	89Q1	90Q1	89Q1	Finland	90Q2	93Q3	90Q2
28	Belgium	05Q1	06Q2	05Q1	Greece	10Q2	13Q2	10Q2
29	Cyprus	07Q1	08Q2	07Q1	Ireland	08Q3	12Q3	08Q3
30	Estonia	05Q3	06Q4	05Q3	Lux	08Q2	08Q4	08Q2
31	Finland	87Q4	89Q3	87Q4	Malta	08Q2	09Q2	08Q2
32	Finland	96Q4	97Q3	96Q4	Neth	79Q1	82Q4	79Q1
33	Greece	00Q4	02Q3	00Q4	Neth	12Q2	13Q2	12Q2
34	Ireland	78Q1	78Q4	78Q1	Portugal	93Q1	94Q2	93Q1
35	Ireland	90Q1	90Q3	90Q1	Portugal	12Q3	13Q2	12Q3
36	Ireland	98Q2	01Q1	98Q2	Slovakia	09Q2	09Q4	09Q2
37	Lux	06Q1	06Q4	06Q1	Slovenia	09Q1	09Q4	09Q1
38	Malta	04Q1	05Q1	04Q1	Slovenia	12Q1	13Q1	12Q1
39	Neth	76Q2	78Q1	76Q2	Czech	09Q2	10Q1	09Q2
40	Neth	99Q1	00Q3	99Q1	Denmark	80Q2	82Q4	80Q2
41	Portugal	89Q1	90Q2	89Q1	Denmark	87Q1	87Q4	87Q1
42	Portugal	91Q1	92Q2	91Q1	Denmark	89Q4	90Q4	89Q4
43	Portugal	99Q1	00Q2	99Q1	Denmark	08Q3	09Q3	08Q3
44	Slovakia	07Q2	08Q2	07Q2	Denmark	11Q4	12Q2	11Q4
45	Slovenia	03Q4	04Q2	03Q4	Hungary	09Q3	10Q2	09Q3
46	Slovenia	05Q3	07Q3	05Q3	Lithuania	95Q1	95Q4	95Q1
47	Czech	02Q1	03Q3	02Q1	Lithuania	08Q4	10Q1	08Q4
48	Czech	07Q1	07Q4	07Q1	Norway	89Q1	93Q2	89Q1
49	Denmark	83Q2	84Q1	83Q2	Norway	08Q3	09Q2	08Q3
50	Denmark	85Q3	86Q2	85Q3	Canada	82Q1	83Q1	82Q1
51	Denmark	05Q1	06Q4	05Q1	Canada	84Q1	84Q3	84Q1
52	Hungary	99Q1	00Q4	99Q1	Canada	90Q1	91Q1	90Q1
53	Lithuania	99Q1	99Q3	99Q1	Canada	95Q1	95Q4	95Q1
54	Lithuania	05Q2	06Q3	05Q2	Australia	82Q1	83Q2	82Q1
55	Norway	84Q4	86Q4	84Q4	Australia	90Q2	91Q1	90Q2
56	Norway	94Q1	94Q3	94Q1	Australia	11Q2	12Q1	11Q2
57	Norway	99Q4	00Q3	99Q4	Japan	76Q1	77Q1	76Q1
58	Norway	06Q2	07Q2	06Q2	Japan	92Q2	93Q3	92Q2
59	Canada	86Q2	89Q1	86Q2	Japan	03Q3	05Q2	03Q3
60	Canada	06Q2	08Q1	06Q2				
61	Australia	88Q3	89Q3	88Q3				
62	Australia	01Q3	04Q1	01Q3				
63	Australia	09Q4	10Q2	09Q4				
64	Japan	79Q2	82Q4	79Q2				
65	Japan	87Q1	88Q2	87Q1				
66	Japan	88Q4	91Q1	88Q4				

Shaded cells represent house price episodes for which there is avaialable public debt data and are, therefore, used to estimate public debt bias.

1/ Episodes are defined as at least three consecutive quarters above or below one standard deviation from the mean

Figure 1. Median Financial and Business Cycle Episode Dynamics

(In real growth rates) 1/



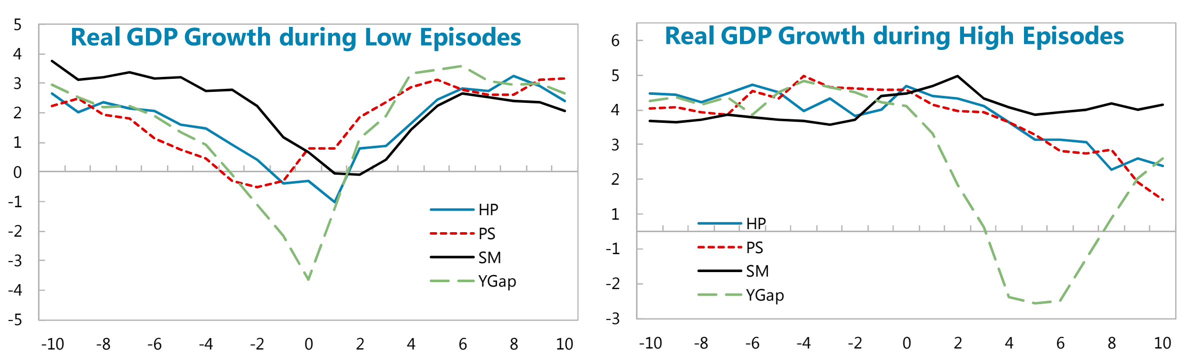
1/ HP, PS, YGap and SM stand for house prices, private sector credit, output gap and stock market episodes respectively.

Output and financial episodes are not fully synchronized. In particular, during downturns, output recovers at least two quarters earlier than private sector debt but lags house prices. As expected, GDP growth follows a standard v-shaped pattern during episodes of negative output gap, with growth slowing in the preceding 10 quarters and recovering in the subsequent 10 (left panel, Figure 2).

During financial sector downturns, except for stock market driven ones, GDP growth declines significantly but the recovery is symmetric and v-shaped. Real GDP growth declines around 3.5 percentage points during financial sector episodes in the 10 quarters up to the trough but it recovers to pre-through levels within two years. However, it remains positive throughout negative stock market episodes although post-trough growth is lower in this case (Figure 2, left panel and Table 2).

After post-financial sector booms, real GDP growth declines gradually but persistently over the next few years (Figure 2, right panel)**.** Notably, real GDP seems unchanged before and after episodes of high equity market growth. On the other hand GDP growth slows down significantly, although gradually, post house prices and private sector debt peaks.

Figure 2. Median Real GDP around Financial and Business Cycle Episodes



1/ HP, PS, SM and YGap stand for house prices, private sector credit, stock market and output gap episodes respectively.

Table 2: Financial and Business Cycle Episodes 1/

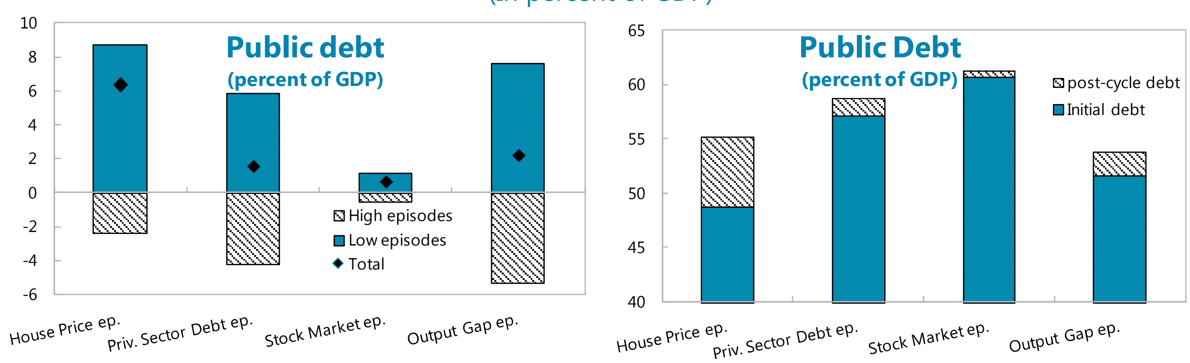
	Low Episodes		High Episodes		Net Effect		Number of episodes	
	Mean	Median	Mean	Median	Mean	Median	Low	High
			House Prices					
Public debt	10.2	8.7	-2.1	-2.4	8.1	6.3	33	35
Low public debt 2/	*6.9*	*8.5*	*-0.9*	*-1.2*	*6.0*	*7.3*	9	12
High public debt 3/	*13.5*	*14.3*	*0.9*	*2.6*	*14.4*	*16.9*	8	5
Low private debt 4/	*8.7*	*8.5*	*-0.3*	*-1.1*	*8.5*	*7.4*	15	13
High private debt 5/	*8.6*	*4.4*	*-5.2*	*-2.9*	*3.4*	*1.5*	8	8
High financial sector debt 6/	*9.2*	*9.3*	*-4.3*	*-3.4*	*4.9*	*5.9*	11	12
Extreme episodes	*15.6*	*14.9*	*-2.7*	*-3.5*	*12.9*	*11.4*	5	11
Sustained episodes	*14.4*	*12.8*	*-2.3*	*-2.4*	*12.1*	*10.4*	10	17
Overall Balance	-0.4	-0.7	1.2	0.5	0.8	-0.1	35	38
Real GDP growth	-3.8	-3.4	0.0	0.1	-3.7	-3.3	49	50
			Private Sector Debt					
Public debt	9.5	5.8	-3.6	-4.2	5.8	1.6	45	44
Low public debt 2/	*10.2*	*5.9*	*-1.3*	*-2.7*	*8.9*	*3.2*	9	13
High public debt 3/	*5.5*	*7.5*	*-2.5*	*-3.9*	*3.1*	*3.6*	13	10
Low private debt 4/	*5.7*	*5.4*	*-2.1*	*-2.5*	*3.6*	*3.0*	18	18
High private debt 5/	*12.1*	*8.0*	*-5.5*	*-5.5*	*6.5*	*2.5*	12	8
High financial sector debt 6/	*13.7*	*9.4*	*-5.0*	*-4.8*	*8.7*	*4.6*	15	13
Extreme episodes	*18.8*	*15.2*	*-2.6*	*0.0*	*16.2*	*15.2*	5	6
Sustained episodes	*17.2*	*11.9*	*-4.9*	*-5.4*	*12.3*	*6.5*	16	10
Overall Balance	-1.5	-0.7	1.6	1.6	0.1	0.9	43	44
Real GDP growth	-2.9	-2.3	0.6	0.9	-2.3	-1.5	45	44
			Stock Market					
Public debt	1.7	1.2	-1.3	-0.5	0.3	0.7	55	31
Low public debt 2/	*3.0*	*1.3*	*0.5*	*1.0*	*3.6*	*2.2*		
High public debt 3/	*6.7*	*5.7*	*-0.5*	*0.9*	*6.2*	*6.6*		
Low private debt 4/	*2.2*	*1.3*	*-1.7*	*-0.5*	*0.5*	*0.7*		
High private debt 5/	*1.8*	*1.0*	*1.4*	*0.0*	*3.3*	*1.0*		
High financial sector debt 6/	*2.1*	*0.7*	*-2.7*	*-2.8*	*-0.5*	*-2.1*		
Extreme episodes	*1.7*	*1.5*	*-7.5*	*-5.5*	*-5.8*	*-4.0*		
Sustained episodes	*9.8*	*9.4*	*-2.5*	*-3.1*	*7.3*	*6.4*		
Overall Balance	-0.7	-0.3	1.0	1.8	0.2	1.5	60	33
Real GDP growth	-4.1	-3.4	1.5	1.7	-2.6	-1.7	71	55
			Output gap					
Public debt	9.8	7.6	-4.4	-5.4	5.3	2.2	40	48
Low public debt 2/	*6.3*	*4.0*	*-3.3*	*-2.8*	*3.1*	*1.2*		
High public debt 3/	*15.7*	*11.1*	*-6.1*	*-5.1*	*9.6*	*6.0*		
Low private debt 4/	*10.1*	*6.9*	*-5.2*	*-5.6*	*4.9*	*1.3*		
High private debt 5/	*11.0*	*5.2*	*-5.0*	*-3.7*	*6.0*	*1.5*		
High financial sector debt 6/	*11.1*	*8.9*	*-6.2*	*-5.6*	*4.9*	*3.3*		
Extreme episodes	*10.4*	*7.5*	*-5.0*	*-5.6*	*5.5*	*1.9*		
Sustained episodes	*12.4*	*15.8*	*-4.4*	*-5.6*	*8.0*	*10.2*		
Overall Balance	-2.9	-2.8	0.8	0.9	-2.1	-1.9	45	50
Real GDP growth	-6.6	-5.9	0.0	0.0	-6.5	-5.9	53	59

1/ Episodes are defined as at least three consecutive quarters above or below one standard deviation from the mean
2/ Korea, Estonia, Luxembourg, Slovenia, Czech Republic, Lithuania, Norway, Australia
3/ France, Italy, Belgium, Greece, Canada, Japan
4/ Germany, Italy, Austria, Estonia, Greece, Slovakia, Slovenia, Czech Republic, Hungary, Lithuania, Poland, Canada, Australia
5/ US, Sweden, Ireland, Luxemburg, Netherlands, Denmark, Japan
6/ UK, France, Korea, Swede, Cyprus, Ireland, Luxemburg, Malta, Netherlands, Denmark, Japan

House price cycles result in a public sector debt bias that is three times bigger than the bias resulting from output cycles and other financial sector variables (Figure 3 and Table 2).[2] Of the 66 and 59 positive and negative house price episodes we identify, availability of public sector debt data reduces the sample to 33 and 35 positive and negative episodes respectively (shaded entries in Table 1). Of these, 15 out of 33 positive episodes reflect the run up of imbalances to the latest financial crisis while two thirds of the negative episodes reflect the impact of the crisis. The bias is the result of the asymmetric impact of upswings and downswings, i.e. the reduction in public debt during financial sector upturns is smaller than the increase during downturns. Whereas the increase in debt is similar in house price and output gap downturns, the larger debt bias associated with house prices is due to a much smaller (about a third) decline in public debt, 2 percent versus 6 percent of GDP.[3] The net increase in public debt obtained from episodes of house prices, private sector debt, and stock market cycles is 6, 2, and 1 percent of GDP, respectively (Figure 3, left panel). These estimates, however, are simple correlations and maybe the result of other factors such as demand and supply shocks that are accounted for in the next section.

Figure 3. Median Public Sector Debt Bias During Episodes

(In percent of GDP)



III. VAR ESTIMATES

We use state dependent panel VARs to quantify the asymmetric dynamic response of public debt to financial cycles, while accounting for supply and monetary factors. VARs allow for dynamic interactions between house price cycles, real GDP growth and public debt to change depending on the state of financial or output cycles. We introduce dummy variables to account for episodes of abnormal house price growth and output gap. This allows capturing the asymmetric impact of house prices and output gap fluctuations on public debt over the cycle. The dummies are based on the episodic approach described above. Finally,

[2] In the rest of the paper financial sector results will, therefore, focused on house price rather than private sector debt and stock market.

[3] This is consistent with the finding in the April 2015 Fiscal Monitor that fiscal stabilization policies are asymmetric through the cycle.

we use stochastic simulations to measure uncertainty around VAR estimates. See appendix for methodological details.

Formally, the baseline model with dummies can be written as:

$$\bar{Z}_{c,t} = \sum_{s=1}^{6} \theta_s \bar{Z}_{c,t-s} + \gamma d_{c,t} + \delta t + \varepsilon_{c,t}$$

where the vector $\bar{Z}_{c,t}$ represents de-meaned variables of interest at time t for country c; $d_{c,t}$ denotes a matrix of exogenous dummy variables and t is a common time trend.[4] The original variables $Z_{c,t}$ consists of house price growth (or output gap), real GDP growth to account for the output channel, inflation rate as a proxy for supply shocks, the change in policy rates to account for changes in monetary policy, and public debt in percent of GDP. The coefficient matrix θ_s, coefficient vector λ, and δ are estimated by least squares.[5]

We use state-dependent VARs to capture regime change when economic time series exhibit breaks. Similar to Ashenfelter and Krueger (1994) and Ashenfelter and Rouse (1998), we look at the difference between two models which differ only by the cyclical position of home prices or the output gap. In contrast to Ashenfelter and Krueger (1994), we use VARs to analyze the dynamic response of public debt to house price shocks; address endogeneity issues among variables; and deal with residual heterogeneity across different regimes.[6] Regime changes could be exogenous (due to events such as global or regional crises (Hamilton, 2005)), or endogenous (such as changes in policy (Sims and Zha, 2006)). Bayesian (Sims and Zha, 2006) and threshold (Caner and Hansen, 2001) approaches have also been used for endogenously determined state dummies. In this paper, we use pre-determined state dummies to capture regime changes across episodes of abnormal house price growth and output gaps. For simplicity we use pre-determined country-specific state dummies and ignore the heterogeneity of factors that cause the regime switching among these countries (e.g. domestic shocks, spillover from global shocks, policy changes, and political crisis).

Responses under the baseline VAR model (without dummies) are as expected. A real house price growth shock (one standard deviation or 3.9 percent) has a positive and

[4] We have also estimated the model with interactive dummies. Results are qualitative and quantitatively consistent with our baseline specification (see annex). In addition, results remain robust to several other specifications of this model including different ordering of variables and inclusion of different trade-weighted exchange rates.

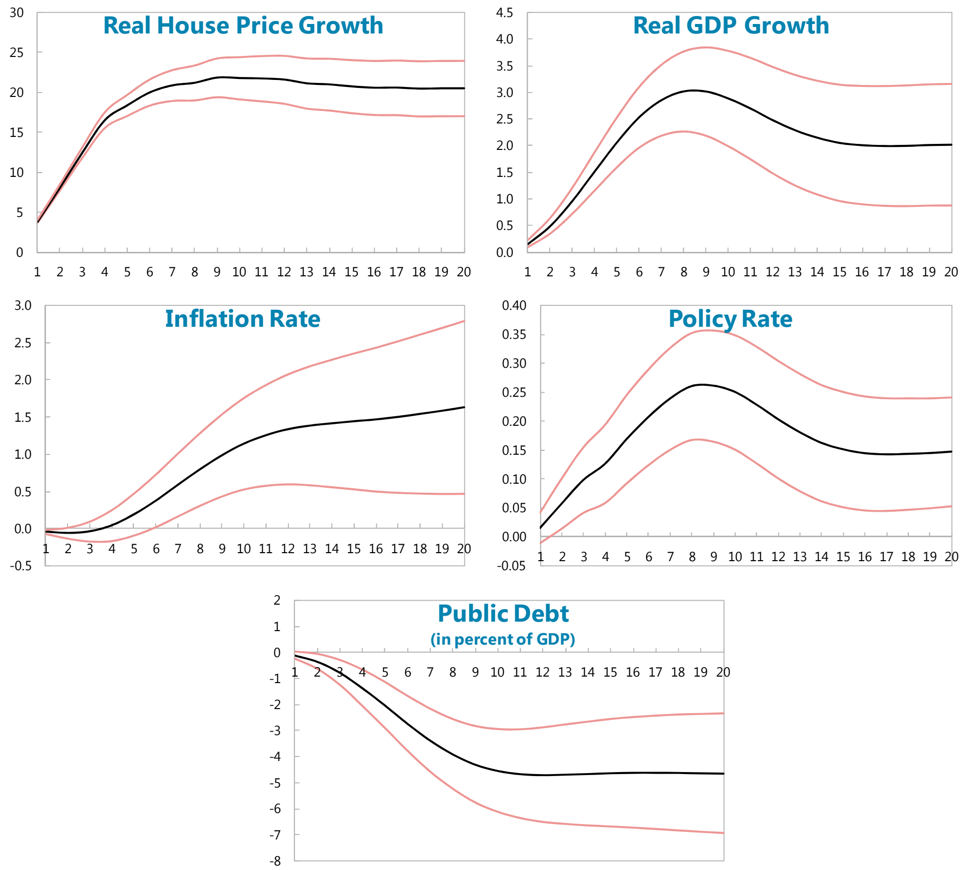
[5] Unit root tests indicate that all the endogenous variables in the VAR are stationary (Table A.5), while lag length tests indicate that six lags should be used (Tables A.7 and A.8).

[6] See Annex for Pairwise Granger causality tests and residual heteroskedasticity tests.

significant impact on real GDP growth, around 2 percent in the long-run, negative and significant impact on public debt, 4.7 p.p., and positive and significant impact on inflation, around 2 percent, and interest rates, 0.15 p.p. (Figure 4). These results, robust to different ordering of the endogenous variables, generally apply to individual country VARs.[7]

Figure 4. Response to a One Standard Deviation Shock in Real House Price Growth

(Confidence intervals of plus/minus two standard deviations)



House price induced debt bias remains sizeable and significant when accounting for monetary and supply factors and the dynamic interaction with real GDP growth or public debt. Negative house price shocks have a large and positive impact on public debt while positive house price shocks have a small, negative but barely significant impact (Figure 5). These results do not differ across the cyclical position of the business cycle. Moreover, the asymmetry in the response of public debt ratio to positive and negative house price shocks remains across all states of the output gap.

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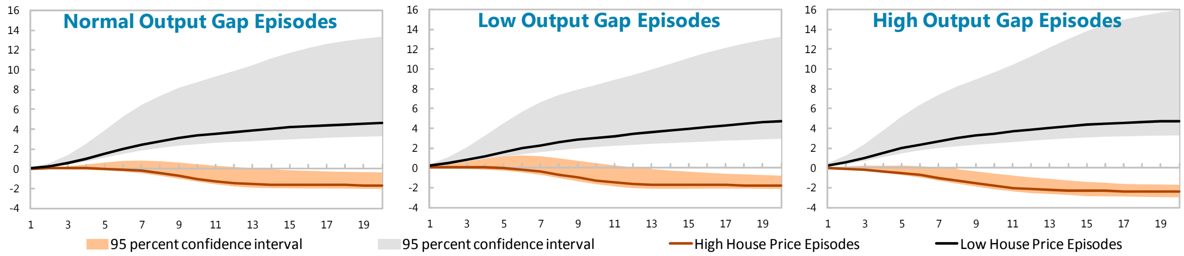
[7] Country specific results are broadly consistent with panel VAR estimates. In some cases though, VAR with dummies could not be estimated due to the presence of only one or none of the abnormal house price episodes.

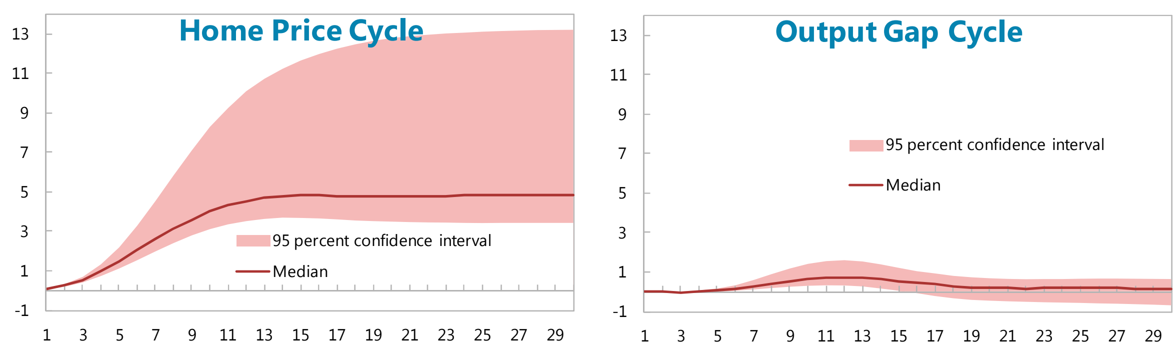
Figure 5. Median House Price Induced Public Sector Debt Bias from VAR Estimates

(In percent of GDP)



Quantitatively, the debt bias associated with house prices is sizeable while that associated with output gap is small and barely statistically significant. The estimated VAR debt bias for a 10 percent price fluctuation amounts to 4 percent of GDP over 10 quarters, 5 percent of GDP in the long-run, across abnormal house price episodes and is statistically significant (Figure 6).[8] The debt bias for output gap episodes, however, is small and barely significant, 0.7 percent of GDP over 10 quarters for a 10 percent shock and only significant in the medium-term (Figure 6).[9]

Figure 6. Public Sector Debt Bias 1/



1/ Net debt bias estimate obtained after aggregating the impact of high and low house price shocks (as seen in figure 5).

House-price induced debt bias could be much higher as the distribution of simulated impulse responses is significantly skewed upward (Figure 5 and 6). While the long-term debt bias of the 5th percentile is close to the median (3 versus 5 percent of GDP), the debt

[8] Figure 6 show VAR estimates of net debt bias (i.e. the debt bias from house prices is obtained as the increase in public debt during low episodes of house prices weighted over all states of the output gap, minus the reduction in debt during high episodes, similarly weighted) and associated 95 percent confidence intervals.

[9] While house price induced debt bias is quantitative and qualitatively robust to different specifications of the model, the point estimate of debt bias from output gap cycles becomes negative in specifications where state dummies interact with lagged endogenous variables. However, regardless of the specification the median debt bias estimate from output gap cycles is small and marginally significant, if not insignificant.

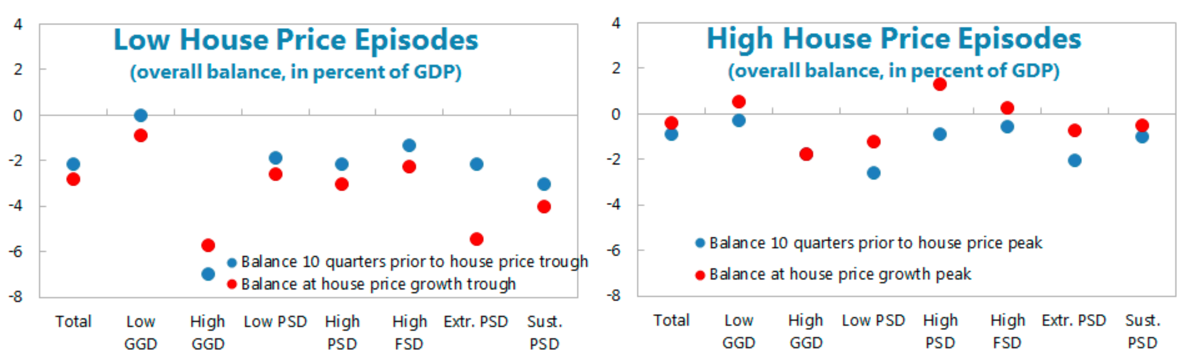
bias at the 95 percentile—at 13 percent of GDP—is almost three times the median value. Furthermore, there is only 2.5 percent chance that this estimate is below 3 percent of GDP.

The fiscal impact of house price cycles is much greater than that of business cycle fluctuations. The estimated increase in public debt when house prices fell is larger than what could be explained by output cycle fluctuations.[10] Revenue gains during house price upswings are largely taken as permanent with governments cutting taxes and increasing spending. As a result, many countries with limited policy options or constrained fiscal space are forced to adjust their fiscal positions pro-cyclically during financial cycle downturns.[11] Despite this, house price declines tend to result in higher public debt. However, while identified asymmetries induced by house-price shocks are significantly larger than those from standard business cycles, most of the fiscal policy discussion has been focused on the latter.

IV. Factors Driving Public Debt Bias

The reduction in debt during house price upturns is mostly due to GDP growth while fiscal deficits keep contributing to higher debt (figure 7, left panel). On average, fiscal balances improve in upturns a bit more than they deteriorate in downturns but debt bias is the result of continuing fiscal deficits even during upturns (figure 7). Thus, on average, observed fiscal improvement during upturns only slows down the accumulation of debt. By contrast, the overall balance responds more to output fluctuations, therefore leading to lower debt bias.

Figure 7. Median Overall Balance During House Price Episodes 1/



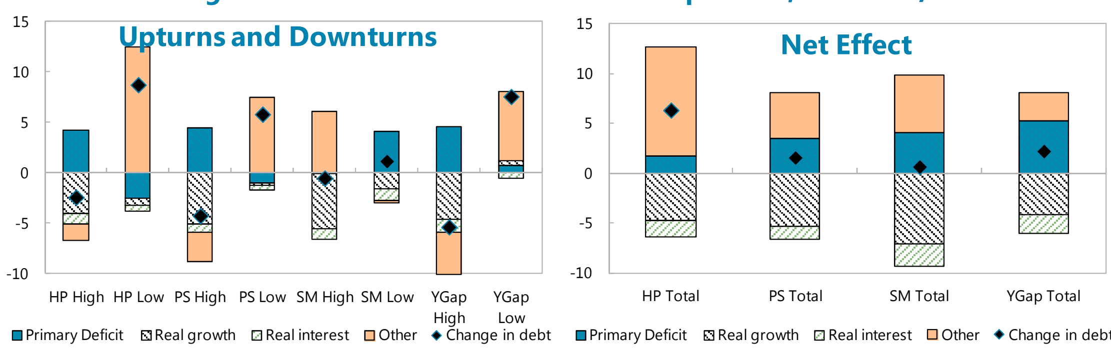
1/ Obtained from Financial and Output Gap Episodes

[10] The dummy for low house price episodes remains significant when including the output gap dummy.

[11] Balance sheet transfers in the form of public sector bail outs are an important factor in some cases.

Fiscal policy is pro-cyclical during financial cycles, except stock market driven ones, but moderately countercyclical during output downturns.[12] Debt dynamics deteriorate during downturns as the positive impact of growth vanishes and balance sheet transfers beyond fiscal balances increase such as financial sector bailouts, or valuation effects due to exchange rate movements (as captured in the residual component of debt accumulation in figure 8). However, fiscal policy is pro-cyclical during house price and, to a lesser extent, private sector debt driven downturns, as it tends to reduce debt at a time when growth is falling. The opposite is true for episodes of output gap downturns. Fiscal policy is also pro-cyclical during financial or output gap upturns (with the exception of stock market driven ones) as it is the only factor increasing public debt when growth is high.

Figure 8. Public Sector Debt Bias Decomposition, Median 1/



1/ Obtained from Financial and Output Episodes.

Debt bias, particularly from house price episodes, is stronger when public debt is high or the episodes are larger in magnitude or duration (figure 9). When public debt is above 100 percent of GDP on average over the sample, it increases by close to 17 percent of GDP over house price cycle episodes. VAR estimates suggest that although countries with high public debt have similar debt bias from output gap episodes as the whole sample, house price induced debt bias is twice as large (10 and 19 percent of GDP after 10 and 20 quarters respectively). Notably, countries with high public sector debt are unable to reduce it during house price upturns (figure 9). This is not the case for output cycle induced bias where the bias is marginally larger for countries with high public debt than the whole sample. In general, and as expected, the bias is also stronger for countries with extreme and sustained financial episodes, mostly due to the larger negative impact on growth.[13]

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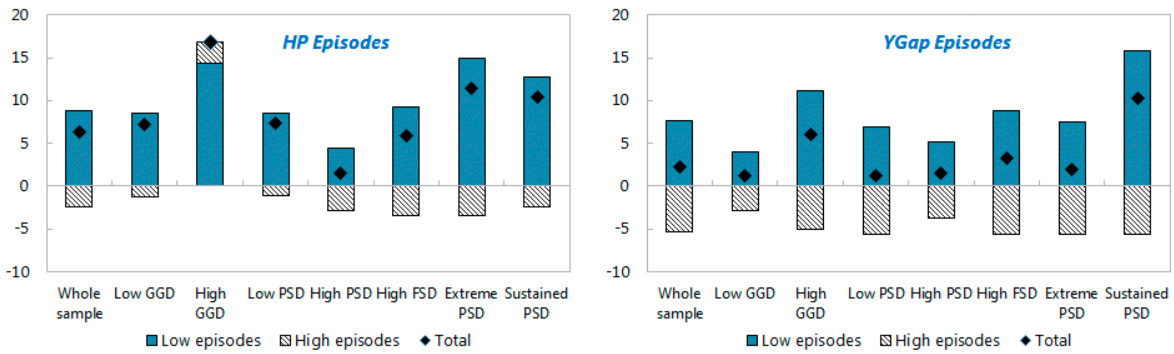
[12] Cyclicality of fiscal policy is defined liberally, not in reference to a business cycle adjusted output measure.

[13] Extreme (sustained) episodes are defined when variable X's growth is 1.5 (1) standard deviations above or below the country-specific mean for at least 3 (6) quarters.

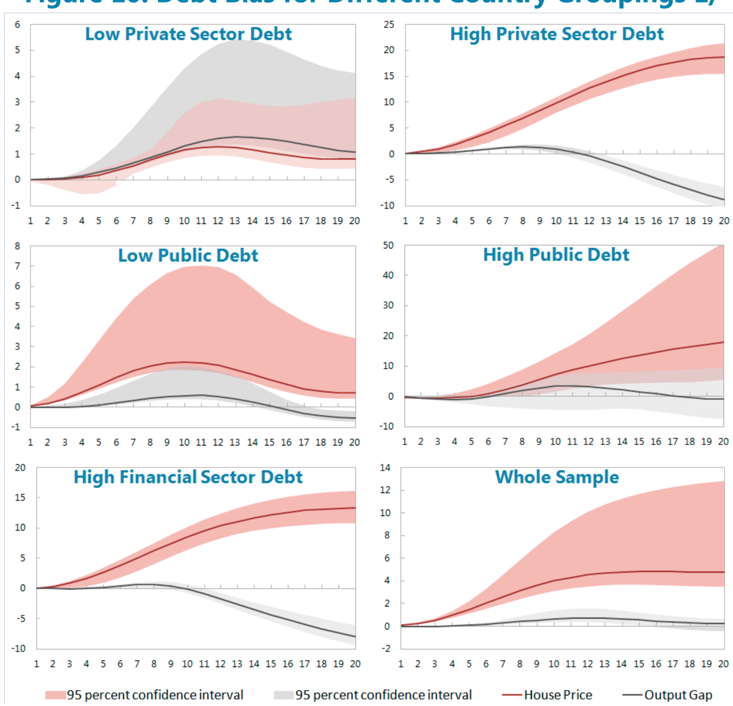
Figure 9. Median Debt Bias for Different Groups of Countries 1/



1/ Obtained from Financial and Output Episodes.

Debt bias is small for countries that have policy buffers. High financial or non-financial private sector debt results in the highest house price induced debt bias using VAR estimates, around 9 percent of GDP after 10 quarters (figure 10).[14] However, when private or public debt is low, debt bias is marginal. Low private debt, financial or non-financial, results in lower balance sheet transfer in house price busts and, therefore, in lower debt bias. Countries with low public debt save a larger fraction of the windfall during house price booms as manifested by the level and direction of public sector balance (figure 7).

Figure 10. Debt Bias for Different Country Groupings 1/



1/ Obtained from VAR estimates.

[14] Note that debt bias for countries with high private sector debt measured by house price episodes results in a small debt bias in contrast to VAR results.

V. Policy Implications

This paper finds evidence of debt bias—the tendency of debt to increase over the cycle—that is significantly larger for house price cycles than stand-alone business cycles. In particular, revenue gains during asset price upswings seem to be largely taken as permanent with governments cutting taxes and/or increasing spending. These decisions are difficult to reverse subsequently in part due to political economy considerations. As a result, countries with more policy constraints or limited fiscal space (high private, financial, and public sector debt) are forced to adjust their fiscal positions pro-cyclically during financial cycle downturns.

Addressing the asymmetric behavior of fiscal policy through financial cycles poses several policy challenges including diagnostics of the cycle, identification of its fiscal impact and the policy response:

The identification of financial cycles and their relationship with potential output is a precondition to quantify their fiscal impact. After the global financial crisis it became clear that the full impact of the unsustainable housing boom was not fully captured in potential output estimates. In particular, transitory movements in GDP driven by credit and house price movements should be identified and distinguished from changes in potential output (Berger et al., 2015).

Ignoring the impact of asset prices on fiscal accounts encourages pro-cyclical fiscal policies. Windfall revenues are taken as permanent and largely passed through into expenditure increases or tax cuts. At the same time, without reliable measures of the underlying financial cycle, policy makers will likely over-estimate the structural fiscal position leading to inadequate fiscal buffers when windfall revenues are gone. The direct impact of housing booms on fiscal accounts is relatively small (e.g. real estate, transaction and capital gains taxes) but should be closely followed as it could give an early indication of overheating. At the same time, complementary methodologies to the standard cyclical adjustment broadly applied to estimate the underlying "structural" fiscal balance, capturing financial sector dynamics should be routinely undertaken. Research in this area has intensified lately and focused on the effect of financial cycles on fiscal accounts through its impact on output (Girouard 2004, Borio 2012, 2013 and 2014, and Poghosyan 2014).

Once financial cycles are identified and their fiscal impact quantified, a policy response is needed to mitigate financial-cycle induced asymmetries identified in this paper. From a macroeconomic perspective, fiscal policy should at least be neutral with respect to financial cycles or at most help moderate macroeconomic volatility associated with them by adopting a counter-cyclical stance. From a fiscal perspective, fiscal policy should at least aim at eliminating the debt bias associated with financial cycles.

In particular,

- **The structural fiscal balance should be refined to account for movements in financial cycles**. The uncertainty surrounding estimates of the various direct and indirect channels through which financial cycles impact the fiscal stance suggests that ex ante structural fiscal targets should take this debt bias into account.

- **Strong fiscal institutions and robust fiscal rules could also help smooth the pro-cyclicality of fiscal policy by preventing a loose fiscal stance in good times**. Subsequently, rules enable continued access to financing in bad times by supporting a credible commitment to long-term sustainability (April 2015 Fiscal Monitor).

- **In the absence of debt sustainability concerns, automatic stabilizers need to be allowed to fully operate throughout the cycle**. This will require adhering to the correct measure of the structural balance accounting for financial as well as business cycles. Discretionary fiscal policy decisions in good times, particularly during house price booms, have largely prevented this from happening and resulted in large fiscal policy asymmetries along the cycle.

- **Fiscal buffers should internalize financial sector risks weighted by its likelihood**. Large increases in public debt associated with crisis-related bail outs have a disproportionate and long lasting effect on fiscal accounts. Furthermore, buffers should also reflect the uncertainty surrounding the identification of financial cycles and its impact on the underlying structural fiscal position.

- **Policy instruments, particularly tax policy, should not exacerbate debt bias associated with house price and financial sector fluctuations** (De Mooij, 2011). For example, many tax systems offer a tax advantage for corporate debt financing. At the same time, tax distortions (e.g. tax preferences for owner-occupied housing) and/or regulatory distortions (e.g. rent control, regulatory constraints on building, etc) tend to amplify housing booms. Addressing some of these underlying distortions might be needed to ameliorate debt bias. On the other hand, macroprudential policies are the first line of defense against financial sector booms and busts.

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APPENDIX: DATA

We use quarterly data for 30 advanced and emerging economies. Geographically most countries are from Europe but the sample also includes the US, Canada, Australia and Japan. The sample period is country specific and it depends on country availability. It expands from 1975Q1 to 2013Q2 in the case of the US, country with more data available. Data sources include the Bank for International Settlements, Bloomberg, Eurostat, Haver, national authorities, OECD House Price Index, IMF World Economic Outlook and IMF staff calculations. Data include data on indebtedness of households, non-financial corporate, financial corporate and government as well as fiscal variables (revenues, expenditures, interest payments). It also includes data on private credit as well as different financial sector indices (house price and stock market indices). A detail of the country coverage as well as some summary statistics is provided in the table below.

Table 1. Data Coverage

Variable	General Government Debt	Household and Non-Fin. Corp. Debt	Financial Corporate Debt	GDP growth	Stock Index	House Price Index
Austria	1999Q1 - 2013Q2	1995Q4 - 2013Q2	1995Q4 - 2013Q2	1988Q1 - 2013Q2	1969Q4 - 2013Q2	1990Q1 - 2013Q2
Belgium	1979Q4 - 2013Q2	1992Q4 - 2013Q2	1992Q4 - 2013Q2	1980Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q3
Cyprus	2000Q1 - 2013Q1	2004Q1 - 2013Q1	2004Q1 - 2013Q1	1995Q1 - 2013Q2	2004Q4 - 2013Q2	2006Q1 - 2011Q3
Estonia	1996Q1 - 2013Q1	2003Q4 - 2013Q2	1996Q1 - 2013Q1	1995Q1 - 2013Q2	1996Q3 - 2013Q2	2003Q3 - 2011Q4
Finland	1995Q1 - 2013Q2	1997Q4 - 2013Q2	1997Q4 - 2013Q2	1975Q1 - 2013Q2	1981Q4 - 2013Q2	1970Q1 - 2011Q4
France	1994Q4 - 2013Q2	1994Q4 - 2013Q2	1994Q4 - 2013Q2	1960Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q4
Germany	1991Q1 - 2013Q1	1999Q1 - 2013Q1	1999Q1 - 2013Q1	1990Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q3
Greece	1999Q1 - 2013Q2	1997Q4 - 2013Q2	1997Q4 - 2013Q2	2000Q1 - 2013Q2	1987Q4 - 2013Q2	1997Q1 - 2011Q4
Ireland	2002Q1 - 2013Q1	2002Q1 - 2013Q1	2002Q1 - 2013Q1	1997Q1 - 2013Q2	1987Q4 - 2013Q2	1970Q1 - 2011Q4
Italy	1995Q1 - 2013Q2	1995Q1 - 2013Q2	1995Q1 - 2013Q2	1970Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q3
Luxembourg	2005Q1 - 2013Q2	2005Q1 - 2013Q2	2005Q1 - 2013Q2	1995Q1 - 2013Q2	1999Q1 - 2013Q2	2005Q1 - 2011Q3
Malta	2003Q4 - 2013Q2	2004Q1 - 2013Q2	2003Q4 - 2013Q2	2000Q1 - 2013Q2	1995Q4 - 2013Q2	2000Q1 - 2011Q3
Netherlands	1998Q4 - 2013Q2	2005Q1 - 2013Q2	2005Q1 - 2013Q2	1977Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q4
Portugal	1999Q1 - 2013Q2	1997Q4 - 2013Q2	1997Q4 - 2013Q2	1986Q1 - 2013Q2	1969Q4 - 2013Q2	1988Q1 - 2011Q4
Slovak Republic	2004Q1 - 2013Q2	2004Q1 - 2013Q2	2004Q1 - 2013Q2	1995Q1 - 2013Q2	2000Q2 - 2013Q2	2005Q1 - 2011Q4
Slovenia	2004Q1 - 2013Q2	2004Q1 - 2013Q2	2004Q1 - 2013Q2	1995Q1 - 2013Q2	2003Q2 - 2013Q2	1995Q2 - 2011Q3
Spain	1980Q4 - 2013Q2	1980Q4 - 2013Q2	1980Q4 - 2013Q2	1980Q1 - 2013Q2	1969Q4 - 2013Q2	1971Q1 - 2011Q4
Czech Republic	2004Q1 - 2013Q2	2004Q1 - 2013Q2	2004Q1 - 2013Q2	1995Q1 - 2013Q2	1995Q3 - 2013Q2	1999Q1 - 2011Q1
Denmark	1990Q4 - 2013Q2	1998Q4 - 2013Q2	1998Q4 - 2013Q2	1990Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q3
Hungary	1989Q4 - 2013Q2	1989Q4 - 2013Q2	1989Q4 - 2013Q2	1995Q1 - 2013Q2	1995Q3 - 2013Q2	1998Q1 - 2011Q3
Latvia	2010Q4 - 2013Q2	2010Q4 - 2013Q2	2011Q4 - 2013Q1	1995Q1 - 2013Q2	2000Q1 - 2013Q2	2004Q1 - 2011Q4
Lithuania	2003Q4 - 2013Q4	2003Q4 - 2013Q4	2003Q4 - 2013Q4	1995Q1 - 2013Q2	2000Q1 - 2013Q2	1994Q1 - 2011Q4
Poland	2003Q4 - 2013Q4	2003Q4 - 2013Q4	2003Q4 - 2013Q4	1995Q1 - 2013Q2	1992Q4 - 2013Q2	2006Q3 - 2011Q4
United Kingdom	1987Q1 - 2013Q2	1987Q1 - 2013Q2	1987Q1 - 2013Q2	1960Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q4
Sweden	1996Q1 - 2013Q2	1996Q1 - 2013Q2	1996Q1 - 2013Q2	1980Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q4
Norway	1965Q4 - 2013Q2	1995Q4 - 2013Q2	1995Q4 - 2013Q2	1978Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q4
Canada	1990Q1 - 2013Q2	1990Q1 - 2013Q2	1990Q1 - 2013Q2	1961Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q4
United States	1960Q1 - 2013Q2	1960Q1 - 2013Q2	1960Q1 - 2013Q2	1960Q1 - 2013Q2	1980Q1 - 2013Q4	1970Q1 - 2011Q4
Australia	1988Q2 -2013Q2	1988Q2 - 2013Q2	1988Q2 - 2013Q2	1960Q1 - 2013Q2	1969Q4 - 2013Q2	1970Q1 - 2011Q4
Japan	1997Q4 - 2013Q2	1997Q4 - 2013Q2	1997Q4 - 2013Q2	1960Q1 - 2013Q2	1960Q1 - 2013Q2	1970Q1 - 2011Q3

Sources: Bank for International Settlements; Bloomberg; Eurostat; Haver; National Authorities; OECD House Price Index; IMF World Economic Outlook; and IMF staff calculations

TECHNICAL ANNEX

A. State Dependent Panel VAR: A Conceptual Framework

This appendix summarizes the conceptual framework used to estimate the dynamic interaction between house price cycles, real GDP growth and public debt, while accounting for supply shocks and changes in monetary policy. Specifically, the panel VAR includes the following variables for country *c* at year *t*: the change in public debt-to-GDP ratio $y_{c,t}$, the real house price growth $x_{c,t}$, and all other covariates $z_{c,t}^{(2,3,4)}$ of secondary interest (policy rate change, real GDP growth, and inflation rate change). Denote all these time series by the vector $Z_{c,t} = \begin{pmatrix} x_{c,t} \\ z_{c,t}^{(2,3,4)} \\ y_{c,t} \end{pmatrix}$.

Using dummies of output and house price episodes we estimate the asymmetric impact of business and financial cycles on public debt. The model with a dummy for high house price growth episodes, for example, produces the impulse response function for normal and low house price growth episodes. Thus, there are nine different states of the world reflecting output and house price episodes that have different (historical) likelihoods (Table A.1). The marginal effect of a certain episode is computed as the difference between the impulse response function of the VAR model with and without the dummy that represents the episode.[15] Thus, there are three marginal responses of public debt to a 10 percent positive house price shock in high house price episodes (for different states of the output gap: high, normal and low). The same is the case for negative house price shock. Therefore, for each state of the output gap there is an estimate of debt bias obtained as the difference between the increase in public debt in low house price growth episodes and the reduction in public debt during high episodes. An estimate of the total debt bias is obtained as a weighted average of the estimates for the three states of the output gap weighted by their likelihood (Table A.1).

Table A.1. Probability of House Price and Output Gap Episodes

		Output Gap			
		High	Normal	Low	*Total*
House Prices	High Growth	1.3	7.8	0.2	*9.3*
	Normal Growth	5.6	72.4	3.5	*81.5*
	Low Growth	0.5	7.0	1.8	*9.3*
	Total	*7.4*	*87.1*	*5.5*	*100*

15 We obtain Cholesky impulse response functions that are normalized for a house price shock of 10 percent—the medium cumulative growth observed during low house price episodes—ensuring comparability of impulse response functions across the different VARs for different dummy episodes.

Stochastic simulations are used to measure house price volatility and uncertainty surrounding the debt bias. Each simulation generates a random sample of 25 countries and 140 observations for each country. We simulate 5000 random sub samples and estimate the 16 VARs and the debt bias for each sub sample. From these 5000 simulated biases, we extract the median debt bias, the 95th percent confidence interval around the median, and other statistics.

Table A.2 shows strong evidence of heterogeneity across different episodes of house prices and output gap (we only report the standard errors from the equation of house price). Thus, state-dependent VARs will help mitigating heterogeneity.

Table A.2. Cross-regime Heteroskedasticity

	Ygap normal	Ygap low	Ygap high
HP normal	5.03	3.28	3.38
HP low	2.13	3.82	6.46
HP high	4.52	0.37	3.84

Measured by the standard error of the resiudal in the house price equation from VARs for different subsamples.

To segregate the entire data sample, we use the notation *(h,g)*, where *h* and *g* take values 0, 1 and 2 to indicate episodes of normal, high and low house price and output gap, respectively.

Pair-wise Granger Causality tests (Table A.3) show strong evidence of endogeneity from one variable to all other variables within six quarters (we have also performed block exogeneity test with similar results). To address this issue we apply a vector auto-regression model for any given pair *(h,g)*:

$$Z_{c,t}^{(h,g)} = \mu_c + \sum_{s=1}^{n} \theta_s Z_{c,t-s}^{(h,g)} + \delta t + \varepsilon_{c,t}$$

where μ_c is country c's fixed effect and the term δt would offset the potential quadratic trend in the data. As our primary interest is the dynamic change of $y_{c,t}$ with respect to $x_{c,t}$ in a generic country and episode setting, we remove the fixed effects by de-meaning all variables.

Table A.3. Pairwise Granger Causality Tests

Sample: 1975Q1 2013Q2
Lags: 6

Null Hypothesis:	Obs	F-Statistic	Prob.
Z2 does not Granger Cause Z1	2645	9.43975	3.00E-10
Z1 does not Granger Cause Z2		13.8012	2.00E-15
Z3 does not Granger Cause Z1	2302	1.68136	1.22E-01
Z1 does not Granger Cause Z3		8.44507	4.00E-09
Z4 does not Granger Cause Z1	2709	1.89244	7.84E-02
Z1 does not Granger Cause Z4		8.66203	2.00E-09
Z5 does not Granger Cause Z1	1709	2.1641	0.0438
Z1 does not Granger Cause Z5		8.55621	4.00E-09
Z3 does not Granger Cause Z2	2326	9.88715	9.00E-11
Z2 does not Granger Cause Z3		14.7634	1.00E-16
Z4 does not Granger Cause Z2	2476	12.4649	7.00E-14
Z2 does not Granger Cause Z4		15.6029	1.00E-17
Z5 does not Granger Cause Z2	1692	1.8464	0.0867
Z2 does not Granger Cause Z5		12.8817	3.00E-14
Z4 does not Granger Cause Z3	2379	7.55505	5.00E-08
Z3 does not Granger Cause Z4		9.29617	4.00E-10
Z5 does not Granger Cause Z3	1646	4.63305	1.00E-04
Z3 does not Granger Cause Z5		3.53296	0.0018
Z5 does not Granger Cause Z4	1685	14.1491	9.00E-16
Z4 does not Granger Cause Z5		4.13861	4.00E-04

Note: Z1,Z2,Z3,Z4,Z5 are real house price growth, real GDP growth, inflation rate change, policy rate change, and change in public debt-to-GDP ratio, respectively, all demeaned.

The VAR of demeaned variables is then

$$\overline{Z}_{c,t}^{(h,g)} = \sum_{s=1}^{n} \theta_s \overline{Z}_{c,t-s}^{(h,g)} + \delta t + \varepsilon_{c,t} \tag{10}$$

where $\overline{Z}_{c,t}^{(h,g)} = \begin{pmatrix} \overline{x}_{c,t}^{(h,g)} \\ \vdots \\ \overline{y}_{c,t}^{(h,g)} \end{pmatrix}$ is the de-meaned vector of $Z_{c,t}^{(h,g)}$. Table A.4 shows the homogeneity of shocks within each episode *(h,g)* in (10). Within each of the nine existing regimes, test results provide a validation of the least square estimation as well as support a regime-varying, rather than time-varying, impulse response analysis.

Table A.4. Within-regime Homoskedasticity

p-values for heteroskedasticity test for VAR in each eposide			
	Ygap normal	Ygap low	Ygap high
HP normal	0.000	0.057	0.000
HP low	0.000	0.000	0.000
HP high	0.000	0.000	0.002

Source: IMF staff estimates.
Note: The p-values are derived from the estimated VAR for the corresponding subsample.

The *k*-quarter-ahead dynamic effect of a house price shock on public debt ratio is quantified by the impulse response function IRF(g,h)[16] (Table A.5) and can be computed as,

[16] Cholesky decomposition is applied to address the interdependence among the contemporaneous shocks $\partial Z_t = (\partial x_t, \cdots, \partial y_t)'$. Variables are ordered by the reverse size of their endogeneity in the VAR model without any dummies. Our simulations also indicate that the ordering of the variables does not materially change our major findings when *k>5*.

$$\frac{\partial y_{t+k}^{(h,g)}}{\partial x_t^{(h,g)}} = \frac{\partial \overline{y}_{t+k}^{(h,g)}}{\partial \overline{x}_t^{(h,g)}} \tag{11}$$

or its cumulative value $\sum_{m=1}^{k} \frac{\partial y_{t+m}^{(h,g)}}{\partial x_t^{(h,g)}}$. Equality (11) is valid since $\overline{Z}_t$ and Z_t differ by a constant vector and the indices *c* and *t* can be dropped because of the homogeneity in the model.

Table A.5. Matrix of Impulse Responses

	Normal Y Gap (g=0)	High Y Gap (g=1)	Low Y Gap (g=2)
Normal House Price (h=0)	IRF(0,0)	IRF(1,0)	IRF(2,0)
High House Price (h=1)	IRF(0,1)	IRF(1,1)	IRF(2,1)
Low House Price (h=2)	IRF(0,2)	IRF(1,2)	IRF(2,2)

The debt bias for the specific *g* (e.g. high, low and normal output gap) is defined as the difference between the responses of the public debt to a negative house price shock in low house price episodes and a positive shock in high house price episodes:

$$B_g(k) \equiv \frac{\partial y_{t+k}^{(2,g)}}{-\partial x_t^{(2,g)}} - \frac{\partial y_{t+k}^{(1,g)}}{\partial x_t^{(1,g)}} = \frac{\partial \overline{y}_{t+k}^{(2,g)}}{-\partial \overline{x}_t^{(2,g)}} - \frac{\partial \overline{y}_{t+k}^{(1,g)}}{\partial \overline{x}_t^{(1,g)}}. \tag{12}$$

The negative sign in $-\partial \overline{x}_t^{(2,g)}$ indicates a negative house price shock.

The expected *k*-quarter-ahead debt bias across the three output gap episodes is then computed as follows, using historic likelihood of all of the *g* scenarios (high, low and normal):

$$B(k) \equiv \sum_{g=0}^{2} B_g(k) \times \Pr\text{ob(g)} = \sum_{g=0}^{2} \left(\frac{\partial \overline{y}_{t+k}^{(2,g)}}{-\partial \overline{x}_t^{(2,g)}} - \frac{\partial \overline{y}_{t+k}^{(1,g)}}{\partial \overline{x}_t^{(1,g)}} \right) \times \Pr\text{ob(g)}$$

where Prob(g) is the probability of occurrence of g = 0, 1 and 2 in the historical data (last row in table A.1).[17]

To estimate (12) we apply the least square procedure using all available data and all possible combinations of dummes for house price and output gap episodes as follows:

[17] Another possibility to aggregate the IRFs for all *(h,g)* pairs and to compute the debt bias across different output gap episodes is to apply *a priori* weights as in Shapley (1953) where the state dummies act as the players and the dynamic responses as the payoff function. This is particularly useful when the historical probabilities are not available or likely deviate from future prospects. Our simulations suggest that both weighting systems produce similar results.

$$\bar{Z}_{c,t} = \sum_{s=1}^{n} \theta_s \bar{Z}_{c,t-s} + b_g I_{\{g\}} + c_h I_{\{h\}} + \delta t + \varepsilon_{c,t}.$$

In the above formula, $I_{\{g\}}$ and $I_{\{h\}}$ are dummy variables for a given pair *(g,h)*. This modification allows for comparison of results estimated using the common data sample. For *h=1* and *2*, the modified VARs are

$$\bar{Z}_{c,t} = \sum_{s=1}^{n} \theta_s \bar{Z}_{c,t-s} + b_g I_{\{g\}} + c_1 I_{\{h=1\}} + \delta t + \varepsilon_{c,t}, \tag{13}$$

$$\bar{Z}_{c,t} = \sum_{s=1}^{n} \theta_s \bar{Z}_{c,t-s} + b_g I_{\{g\}} + c_2 I_{\{h=2\}} + \delta t + \varepsilon_{c,t}, \tag{14}$$

$$\bar{Z}_{c,t} = \sum_{s=1}^{n} \theta_s \bar{Z}_{c,t-s} + b_g I_{\{g\}} + c_1 I_{\{h=1\}} + c_2 I_{\{h=2\}} + \delta t + \varepsilon_{c,t}. \tag{15}$$

In an intuitive sense, the term $c_1 I_{\{h=1\}}$ in (13) accounts for high house price episodes; so the IRF, denoted *IRF(13)*, represents the impact of a house price shock in low and normal house price episodes, i.e., it combines rows 2 and 4 in table A.5. Similarly, the term $c_2 I_{\{h=2\}}$ in (14) accounts for low house price episodes and the associated IRF, *IRF(14)*, represents the impact of a house price shock in high and normal house price episodes. IRF in (15), denoted *IRF(15)*, represents the impact of a house price shock in normal house price episodes only (row 2 in table A.5). Therefore, the impact of a positive house price shock in high house price episodes is measured by the difference of IRF in (14) and IRF in (15): $IRF(14) - IRF(15)$.

By the same argument, the impact of a negative house price shock in low house price episodes is measured by the difference of IRF in (15) and IRF in (13): $-[IRF(13) - IRF(15)] = IRF(15) - IRF(13)$. The first negative sign in the above equation indicates a negative house price shock.

Finally, (12) is approximated by the doubled IRF of (15), net of the total IRF of (13) and (14): $[IRF(15) - IRF(13)] - [IRF(14) - IRF(15)] = 2 \times IRF(15) - [IRF(13) + IRF(14)]$.

B. Diagnostic Tests for the VAR Models

In this appendix, we provide a battery of diagnostic tests for the VAR models (10, 13-15).

Stationarity Tests

Popular panel unit root tests (e.g., Levin-Lin-Chu, Breitung, Im-Pesaran-Shin, Phillips-Perron, and Augmented Dickey-Fuller) assume either common stochastic trends or idiosyncratic stochastic trend for each country (but not both). All five panel time series may exhibit both common and idiosyncratic trends. Unit root tests indicate stationarity for all the variables.

Table A.6. p-values for Panel Unit Root Test

Cross-sections: 30
Sample: 1975Q1 2013Q2
Exogenous variables: None
Automatic lag length selection based on Asymptotic t-statistic (p=0.05): 1 to 13
Newey-West automatic bandwidth selection and Bartlet

Method	Z1	Z2	Z3	Z4	Z5
Null: Unit root (assumes common unit root process)					
Levin, Lin & Chu t *	-13.9	-11.5	-7.2	-29.2	-14.5
Null: Unit root (assumes individual unit root process)					
ADF - Fisher Chi-square *	296.4	238.6	205.1	1,188.4	345.6
PP - Fisher Chi-square *	312.0	453.2	678.9	3,740.6	250.2

* All results are significant at the 1% level

Cross dependence between the dummies and the endogenous variables

To account for the possible endogeneity of the dummies, models (13)-(15) could be complemented by adding cross dependence terms between the dummies and the variables of interest. A comparison between the two models—with and without cross dependence terms—reveals that while such an addition did not materially changes the estimates of (12), it increases model uncertainty measured by the Bayesian information criteria (BIC). BIC estimates a function of the posterior probability of a model being true, under certain Bayesian setting; a lower BIC means that a model has lower uncertainty. For all *(g,h)* in table A.7, VAR estimates without cross dependence have a lower BIC value than the corresponding VAR with cross dependence. Thus, we use the model without cross dependence.

Table A.7. BIC in the VAR models with or without Cross Dependence of Dummies 1/

Model Names	With Cross Dependence	Without Cross Dependence	Probability
VAR0000	17.2532	17.2532	0.7236
VAR0001	17.3144	17.2510	0.0351
VAR0010	17.3397	17.2466	0.0558
VAR0011	17.4038	17.2430	0.0909
VAR0100	17.3037	17.2507	0.0697
VAR0101	17.3736	17.2495	0.0177
VAR0110	17.3901	17.2437	0.0050
VAR0111	17.4644	17.2412	0.0227
VAR1000	17.3512	17.2428	0.0777
VAR1001	17.4079	17.2399	0.0022
VAR1010	17.4399	17.2362	0.0132
VAR1011	17.5001	17.2317	0.0154
VAR1100	17.3958	17.2374	0.1474
VAR1101	17.4630	17.2356	0.0199
VAR1110	17.4844	17.2302	0.0182
VAR1111	17.5563	17.2270	0.0381

Note : binary suffix in the model names indicates the appearance (1) of the dummies or not (0), in the order of HP High, HP Low, Ygap high, and Ygap Low.

1/ Optimal lag length following SIC determined 6 lags for all VAR models

Lag Length Selection

To determine an optimal lag for the VARs, we use the Bayesian information criteria to the VAR without dummies. Table A.8 selects six as the lag length for the VARs and Table A.9 shows that all the six lags are statistically significant.

Table A.8. VAR Lag Order Selection by the Bayesian Information Criteria

Endogenous variables: Z1 Z2 Z3 Z4 Z5
Sample: 1975Q1 2013Q2
Included observations: 1435

Lag	1	2	3	4	5	6	7	8
Exogenous variables: none								
LogL	-12,908	-12,693	-12,580	-12,432	-11,922	-11,804	-11,732	-11,693
BIC	18.12	17.94	17.91	17.83	17.25	**17.21**	17.24	17.31
Exogenous variables: trend								
LogL	-12,877	-12,664	-12,551	-12,405	-11,905	-11,787	-11,715	-11,673
BIC	18.10	17.93	17.90	17.82	17.25	**17.21**	17.24	17.31

Table A.9. VAR Lag Exclusion Wald Tests

Sample: 1975Q1 2013Q2
Included observations: 1495

Chi-squared test statistics for lag exclusion: Numbers in [] are p-values

	Z1	Z2	Z3	Z4	Z5	Joint
Lag 1	2017.2260	1627.8280	2719.7140	255.0955	1154.9060	7292.4860
	[0.000000]	[0.000000]	[0.000000]	[0.000000]	[0.000000]	[0.000000]
Lag 2	17.8770	30.5979	66.3491	48.3963	19.7005	164.7940
	[0.003105]	[1.12e-05]	[5.88e-13]	[2.95e-09]	[0.001422]	[0.000000]
Lag 3	26.2649	7.1410	9.9941	3.6916	1.7450	51.5834
	[7.93e-05]	[0.210364]	[0.075401]	[0.594614]	[0.883189]	[0.001353]
Lag 4	223.7293	257.7452	281.3290	10.8730	391.1252	1235.7710
	[0.000000]	[0.000000]	[0.000000]	[0.053957]	[0.000000]	[0.000000]
Lag 5	338.5626	161.7195	273.5293	27.5142	187.6127	946.5469
	[0.000000]	[0.000000]	[0.000000]	[4.53e-05]	[0.000000]	[0.000000]
Lag 6	125.9173	35.0774	67.5365	27.8596	19.1340	248.8075
	[0.000000]	[1.45e-06]	[3.33e-13]	[3.88e-05]	[0.001815]	[0.000000]
df	5	5	5	5	5	25

Cross-validation of the VAR models

The results estimated using (10) have been cross-validated by exploring four alternative models. For example, an alternative model for estimating (12) is to consider using the country-specific de-trended data, defined by

$$\tilde{Z}_{c,t} \equiv Z_{c,t} - \lambda_c - \delta_c t$$

where λ_c and δ_c are chosen to minimize $\sum_t \tilde{Z}_{c,t}^2$ for each country *i*. Modeling $\tilde{Z}_{c,t}$ by a VAR would, however, underestimate the size of the true shocks. In the IRF analysis and associated bias estimate for each episode, the constant covariance of the shocks comes from two parts: global and country-specific, i.e., within cross-section and between cross-section variances. By idiosyncratically de-trending the data, we decrease the within cross-section variance.

Other alternative models that have been considered include:

Model 1: VAR on $\bar{Z}_{c,t}$ without common deterministic trend,

$$\bar{Z}_{c,t} = \sum_{s=1}^{n} \theta_s \bar{Z}_{c,t-s} + \varepsilon_{c,t} .$$

Model 2: VAR on original data using common deterministic trend and common intercept,

$$Z_{c,t} = \lambda + \sum_{s=1}^{n} \theta_s Z_{c,t-s} + \delta t + \varepsilon_{c,t} .$$

Model 3: VAR on original data using cross-sectional dummies and common trend

$$Z_{c,t} = \sum_{s=1}^{n} \theta_s Z_{c,t-s} + \gamma_c I_{\{c\}} + \delta t + \varepsilon_{c,t} .$$

Our simulations show that all these models support the same conclusions as in (10). Models 1 and 2 have similar magnitude as (10) while Model 3 yields lower debt bias estimates. The reason is that Model 3 filters some country-specific (idiosyncratic) variance, as $\tilde{Z}_{c,t}$ does.