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Long-term effects of fiscal stimulus and austerity in Europe

by

Sebastian Gechert

(Macroeconomic Policy Institute (IMK), Düsseldorf)

Gustav Horn

(Macroeconomic Policy Institute (IMK), Düsseldorf)

and

Christoph Paetz

(Macroeconomic Policy Institute (IMK), Düsseldorf)

Long-term effects of fiscal stimulus and austerity in Europe

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We analyze whether there are negative (positive) long-term effects of austerity measures (stimulus measures) on potential output growth. Based on the approach of Blanchard and Leigh (2013) and Fatás and Summers (2016) and using a novel dataset of narratively identified fiscal policy shocks, we estimate the impact of these shocks on potential output. We robustly find a considerable underestimation of multiplier effects and their persistence for most European countries in the early years after the financial crisis and subsequent Euro Area crisis. We conclude that austerity was badly timed and thus not only deepened the crisis but may have caused evitable hysteresis effects.

Keywords: Fiscal Consolidation; Fiscal Multipliers; Forecast Errors; Hysteresis

JEL classification: E62, H68

[1] All: Macroeconomic Policy Institute (IMK), Düsseldorf. Corresponding author: Sebastian-Gechert@boeckler.de. We would like to thank Achim Truger, Antonio Fatas, Camelia Turcu, Karel Havik, Katja Heinisch, Oana Furtuna, Rudolf Zwiener and Wouter van der Wielen for helpful discussions and data access. All remaining errors are our own. Moreover, we thank the committee of the INFER Bucharest Workshop on Finance and Fiscal Policy for the honor of the Best Paper Award.

1. INTRODUCTION

Output in many European countries has long remained below pre-crisis potential. The recession took considerably longer and was much deeper compared to past downturns and the recovery was comparably weak. Forecasts by the European Commission (EC) or the International Monetary Fund (IMF) in the aftermath of the crisis assumed a quick recovery to previous trends, but had to be revised downwards several times. These revisions most strikingly concerned not only GDP but also potential GDP forecasts. Figures 1 and 2 show repeated over-optimism of GDP and potential output forecasts for the EU as a whole and Greece as an extreme example.[2]

[Figure 1 about here]

[Figure 2 about here]

The persistent and systematic forecast errors call into question the structure and assumptions of the forecasting models employed. Clearly, the financial crisis and the subsequent crisis of the Euro Area were extreme events, whose dynamics and channels of impact might be quite different from more tranquil times. A number of influential factors that unexpectedly drove the severity of the crisis have been discussed, among them the fragility of the financial system, private sector deleveraging, increased uncertainty of private agents, current account imbalances, monetary policy constraints, sustainability of public finances or the impact of discretionary fiscal policy.

In the present paper, we focus on fiscal policy, while we take into account the others. We ask whether the post-2009-shift towards fiscal consolidation had an unexpected substantial negative and persistent impact on GDP and potential output, in particular in the EU and the

[2] Apart from Germany in all other major European countries potential output growth rates decreased considerably and are now below pre-crisis figures. Potential output estimates were revised downwards both for forecasted and past values in most European countries, apart from Spain.

Euro Area, which could be a major explanatory factor for the second recessionary dip that followed in due course and the persistent gap to pre-crisis GDP trend and unemployment levels. This is equivalent to asking whether there was an underestimation of fiscal multipliers and, more importantly, their persistence.

Since the crisis, there has been an intense debate and a growing literature on short-run fiscal multiplier effects (Gechert 2015, Hebous 2011, Mineshima et al. 2014). Expansionary confidence effects of austerity have been discussed widely[3] (Alesina and Ardagna 2010) but have been found to be rather special cases (Perotti 2011). The general consensus among international institutions now seems to read that austerity reduces growth in the short run, can be particularly harmful during downturns and may even increase public debt-to-GDP ratios in the interim (Cottarelli and Jaramillo 2012, Furman 2016).

The long-term effects – although they are much more important in terms of welfare and sustainability of public finances – have attracted far less attention in the empirical literature and remain more controversial, except for the special case of public investment (Bom and Ligthart 2014). Certainly, robust inference is much harder to achieve for longer horizons, which might explain the lack of evidence. For the few exceptions, the dominant reading seems to be that while austerity brings short-run pain, it provides long-term gain in terms of reduced tax distortions and debt risk (Born et al. 2015, Rogoff 2012). DeLong and Summers (2012) on the other hand make the case for hysteresis effects where austerity in a deep slump would be self-defeating even in the long run.

The present paper builds on Blanchard and Leigh (2013) (BL hereafter) and Fatás and Summers (2016) (FS hereafter). BL exploit GDP growth forecast errors for European countries during the 2010-11 period to create a counterfactual of expected policy impact.

[3] Indeed, official statements by leading policy makers at the time seemed to assume a strong confidence effect of fiscal consolidation that would imply expansionary effects, i.e. negative multipliers. "My understanding is that an overwhelming majority of industrial countries are now in those uncharted waters, where confidence is potentially at stake. Consolidation is a must in such circumstances." (Trichet 2010) "All the eurozone governments need to demonstrate convincingly their own commitment to fiscal consolidation so as to restore the confidence of markets, not to speak of their own citizens." (Schäuble 2010)

They then regress these forecast errors on planned consolidation for the same sample in order to test whether the impact of consolidation was underestimated. They find a strong negative correlation between consolidation attempts and output revisions meaning that countries with bigger consolidation plans faced more severe growth disappointments – i.e. multipliers had been underestimated by forecasters. FS confirm the findings of BL with more recent data and extend their method by a second stage, where they regress longer-term potential output forecast errors on the GDP forecast errors that were arguably caused by the underestimation of multiplier effects. The coefficient of this second stage can be interpreted as a measure of persistence of these multiplier effects.

This paper provides two central innovations: (i) We argue that the measure of exogenous fiscal shocks employed by BL and FS, the change in the structural balance, may face endogeneity issues, as its calculation is based on potential output itself. We therefore opt for a narrative measure of the fiscal stance, the Discretionary Fiscal Effort (DFE), as provided by the AMECO database (EC 2013). (ii) We rigorously test the robustness of our findings and those of FS in terms of omitted variable biases, outliers, alternative estimation techniques, data sources and sample periods.

We find a significant underestimation of fiscal multipliers of about 0.8 units on average, which is strong, but still somewhat less pronounced than in BL and FS. This would translate into a multiplier effect of about 1.3, given that forecasters likely assumed a multiplier effect of 0.5 in their forecasts. These effects have a permanent impact as measured by five-year-ahead forecasts, making a strong case for hysteresis effects of fiscal consolidations and expansions during a deep recession. Our findings are robust to a large set of perturbations. Yet, as a plausible qualification, we find a weakening of the effects in later crisis years, in line with the slowdown of consolidation, learning effects or regime-dependent multiplier effects (Auerbach and Gorodnichenko 2012, Baum et al. 2012). Moreover, some Eastern European countries are influential outliers that weaken the relation to some extent. The effects seem to be stronger for

spending than for revenue shocks. We conclude that the European austerity measures were more harmful than expected even in the longer-term, while countries with a more expansionary fiscal stance fared better as this may have prevented hysteresis effects. Hence, austerity was badly timed which may have even had long-term negative consequences.

The remainder of the paper is organized as follows. Section 2 explains our approach and dataset. Section 3 presents the baseline results. Section 4 checks the robustness of these findings. The final section concludes.

2. METHOD AND DATA

First Stage: Underestimation of Fiscal Multipliers

In line with BL, we regress the forecast error (*fe*) of cumulated GDP growth for the years of 2010 (=*t*) and 2011 for country *i* on planned (*f*) fiscal consolidation for the very same period:

$$\Delta Y^{fe}_{i,t:t+1} = \alpha + \beta \Delta F^{f}_{i,t:t+1|t} (+X_i \theta) + \varepsilon_{i,t:t+1|t} \quad (1)$$

where

$$\Delta Y^{fe}_{i,t:t+1} \equiv \Delta Y_{i,t:t+1} - \Delta Y^{f}_{i,t:t+1|t} \quad (2)$$

is the forecast error of GDP as given by the difference between current-vintage figures of the cumulated growth rate of GDP over 2010 and 2011 and its forecast in the vintage of spring 2010. This figure is negative for most countries during this period. $\Delta F^{f}_{i,t:t+1|t}$ is a measure of planned fiscal consolidation as a percentage of GDP over the same two-year period. X_i marks a set of control variables that are likely alternative explanations for the forecast errors, besides consolidation. $\varepsilon_{i,t:t+1|t}$ is an *iid* error term. Two-year episodes are used to allow for lagged effects.

The rationale is the following: Using the forecast error of GDP exploits the deviation of the actual data from a counterfactual scenario given by the expectations of forecasters, based on

their information set, assumptions and model of the economy at the time, where channels work as expected by these experts. Regressing this forecast error on planned fiscal consolidation reveals, as to whether the impact of these consolidation plans was over- or underestimated. If the multiplier effect assumed in the forecasting model is correct, β should not deviate significantly from zero. The multiplier effect would be as expected.[4] A negative and significant β, however, would imply that countries with a more ambitious consolidation plan had bigger growth disappointments during that period, and vice versa. The multiplier effect would have been underestimated.

Second Stage: Persistence of Multiplier Effects

With respect to welfare and sustainability of public finances the long-term impact of the fiscal measures is key. In line with FS, we measure these long-term effects by five-year-horizon forecast errors of cumulated potential output growth. For inference, we build a Two-Stage Least Squares (TSLS) framework, where the exercise of BL is considered as the first stage, measuring the growth disappointments as caused by the stronger than expected impact of fiscal consolidation:

$$\Delta\hat{Y}^{fe}_{i,t:t+1} = \alpha + \beta\Delta F^{f}_{i,t:t+1|t} \tag{3}$$

The fitted values of the first stage – interpreted as the unexpected GDP change due to a stronger than expected impact of fiscal consolidation – then enter the second stage, where the forecast error of potential output is regressed on these fitted values:

$$\Delta \mathrm{Pot}Y^{fe}_{i,t:t+5} = \gamma + \delta\Delta\hat{Y}^{fe}_{i,t:t+1}\ (+X_i\pi) + \ \omega_{i,t:t+1|t} \tag{4}$$

[4] The IMF (2010, p. 94) WEO of October 2010 has estimates of fiscal multipliers of 0.5 on average, based on the IMF's GIMF model that is likely to inform forecasters. BL point to some further evidence in this direction. According to the European Commission (2012, p. 41) European Economic Forecast, multipliers from the EC's QUEST model, which likely informs forecasters, range between 0.2 and 0.8 depending on the specific measure and are about 0.4 on average. Also the meta-analysis of Gechert and Rannenberg (2018) finds average multipliers of about 0.5 in their sample of pre-crisis studies. Of course, such averages mask likely heterogeneity of the various fiscal measures, but they may suffice for the broadly defined change in the fiscal surplus that we employ here.

The relevant coefficient δ can be interpreted as a measure of persistence of changes in output that are caused by changes in the fiscal stance. If $\delta = 1$, the multiplier effect would be fully persistent and growth disappointments would carry on one-to-one to the long-run. For a fiscal consolidation shock in a standard New Keynesian model δ should be smaller than one and approach zero in the medium run, except for a cut in public investment that might drag down aggregate supply conditions. Of course, potential output figures usually follow persistent changes in GDP quite closely and might thus not be a perfect metric to investigate structural changes in output (Gechert et al. 2015).[5] However, a permanent effect on GDP after 5 years still runs counter to conventional assessments of the persistence of demand shocks and is much more in line with theories and evidence of hysteresis (DeLong and Summers 2012, Fatás 2000, Logeay and Tober 2006, Sturn 2014).

Identification of Consolidation Shocks

When estimating the impact of fiscal policy, identification of exogenous fiscal shocks is crucial. Three main concerns are usually discussed in the literature: (a) Since the budget is highly sensitive to business cycle fluctuations via automatic stabilizers, estimation based on headline budgetary figures would be prone to an endogeneity bias. (b) Even discretionary measures may be immediate reactions to macroeconomic circumstances (e.g. countercyclical policies) and thus reverse causality may apply. (c) Agents may anticipate fiscal policy measures due to early announcement and hence react prior to implementation (e.g. in the case of a tax hike), outside the information set of the econometrician.

(a) BL and FS rely on changes in the structural balance (*SB*) which is an established measure of the fiscal stance. It is derived from the actual budget balance by subtracting a cyclical component, based on assumptions of automatic stabilizers and the output gap, as well as one-

[5] For a critical review see the conference contributions at http://ec.europa.eu/economy_finance/events/2015/20150928_workshop/index_en.htm

off events. We argue that the structural balance still faces a likely endogeneity bias when it comes to measuring its impact on potential GDP forecast errors. This is because the structural balance depends on the assessment of potential output itself. To see this, consider the situation in 2010 where potential GDP was forecasted too optimistic in a phase of severe slack. At first, there would be a large measured output gap and forecasting models would estimate the output gap to close with high speed under such circumstances as they include a closing rule effective within the forecasting horizon (Havik et al. 2014). Any consolidation effort that improves the headline budget balance is then largely counted as cyclical, with only a smaller share left to be counted as structural consolidation. That is, if we consider two identical countries with the same true structural consolidation effort, and one country is hit by a stronger negative GDP shock than the other, the former would have a larger forecast error of potential GDP and a lower measured improvement in the SB. This will lead to inflated coefficients β and δ, measured with lower precision at that.[6]

In light of these issues, we opt for an alternative measure of the fiscal stance, namely the Discretionary Fiscal Effort (DFE) as published by the AMECO database. It is available for EU27 countries on an annual basis since 2010.[7] The DFE is essentially a mixed method for determining the discretionary fiscal stance. Changes on the revenue side are entirely based on a narrative account of fiscal shocks where the expected budgetary impact of factual law changes and other measures is recorded. On the expenditure side, where substantial discretionary changes happen at all levels of government and a full narrative record would be too costly, the DFE is calculated as the gap between public spending growth and a smooth trend output growth, while excluding changes in cyclical spending components (in particular

[6] When potential growth turns out lower than expected and is revised downward, so would the structural share of the consolidation effort need to be revised upward. However, SB enters the regression without such revision. Note that such revisions would be required due to pure technical dependence of the calculation of the structural balance on potential output figures, and must not be confused with revisions due to truly more ambitious consolidation efforts.

[7] AMECO publishes the DFE in nominal terms of national currency. For our econometric analysis below we express the discretionary changes in percentage of potential GDP just as the structural balance.

unemployment spending). The DFE thus avoids the dependence on estimated potential output figures and uncertain budget elasticities.

The DFE shock series has been argued to be more robust in estimating fiscal multipliers[8]. In the next section it will be shown that this is indeed the case for our exercise. In line with the arguments above, we find that the cumulated 2010-11 DFE is more positive on average than the respective change in SB ($\mu_{DFE} = 2.46\,pp,\ \mu_{SB} = .53\,pp$) and is moreover much more dispersed ($\sigma_{DFE} = 3.42,\ \sigma_{SB} = 1.68$), while the two are still highly correlated ($cor_{DFE,SB} = .74$). This could speak of an attenuation of the SB measure towards zero.

(b) Separating truly exogenous from endogenous legislations is an issue that is addressed by extensive country studies collecting data similar to the DFE, but also looking at the motivation of single law changes (Romer and Romer 2010, Cloyne 2013). We do not have enough information to make such a separation for the DFE. The narrative studies usually find that not controlling for endogenously motivated law changes tends to downward-bias the multiplier estimates. Thus, we regard our estimates as conservative in this regard. In any case, note that the SB approach does not address this issue either. Even detailed country-studies find it hard to give a clear judgment regarding the motivation of single law changes. Generally, existing measures of fiscal shocks, as ours, are only second best proxies, but a first best solution is yet to be discovered in the literature.

(c) Anticipation bias may arise when econometricians draw inference from ex-post realized data while agents may have had additional information from preannounced policies and reacted in advance. This should be less of a concern for our framework, since we create a counterfactual of realized data against expert forecasts that should be informed about policy announcements at least as well as the general public. In that sense anticipation may only be of

[8] See section 2 in Carnot and de Castro (2015) for an elaborate discussion on the DFE measure of identification.

concern if agents on average were better informed about policy actions than forecasters, which is rather unlikely.

Further Data

In our baseline, we stick to IMF World Economic Outlook (WEO) forecasts for GDP and potential output and use the vintage of spring 2016 vis-à-vis the spring 2010 forecast for the calculation of forecast errors.[9] Importantly, comparing data of different vintage years requires correction for changes of the base year, accounting rules and re-assessments of past potential output figures.[10] The second stage of our model uses *t+5* forecasts for potential output, as given by unpublished vintages of the IMF WEO.[11] In the baseline sample we focus on European countries, but due to missing data end up with 22 / 21 observations.[12] Due to this small sample, section 4 includes a battery of robustness checks for the baseline estimates. First, we include various alternative explanatory factors to control for omitted variable biases. Data for sovereign CDS spreads, pre-crisis household debt-to-GDP ratios and pre-crisis current-account-to-GDP ratios are obtained from the BL dataset.[13] Second, we also run our model using European Commission forecasts. The forecast vintages are obtained from a dataset by the FIRSTRUN project[14], which collects vintages of the AMECO dataset; moreover, we use unpublished *t+4* EC forecasts of potential output.[15] The EC data allows to extend the sample to the whole EU27 and thus some additional Eastern European countries that are absent from the IMF dataset. The third class of robustness checks extends the time horizon by applying a moving window and panel data analysis to increase the number of

[9] BL compare the IMF autumn 2012 forecast to the spring 2010 forecast.
[10] See Appendix A for a more detailed description of the computation of forecast errors for GDP and potential GDP.
[11] We are grateful to Antonio Fatás for providing us with the WEO data and files for replication of the FS results.
[12] For SB, the sample comprises Austria, Belgium, Czech Republic, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Malta, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Sweden, Switzerland and the United Kingdom. For DFE, we have Austria, Belgium, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Malta, Netherlands, Poland, Portugal, Slovak Republic, Slovenia, Spain, Sweden, United Kingdom.
[13] Further control variables are taken from the respective spring 2010 forecast, in line with BL.
[14] http://www.firstrun.eu/research/data/
[15] Courtesy of European Commission forecasting staff.

observations, where we use different spring vintage sets from the IMF and the EC data respectively and compare them to the vintage of spring 2016 to obtain our forecast errors.

3. ESTIMATION RESULTS

First Stage: Underestimation of Fiscal Multipliers

First, in Table 1(a), we replicate the BL results by using IMF WEO data and the change in the SB as our fiscal measure. In Table 1(b) we use the DFE instead.

[Table 1(a) about here]

[Table 1(b) about here]

Column (1) of Table 1(a) shows the result of the replication. The original finding of BL, a significant underestimation of fiscal multipliers by about 1.1, is even reinforced with $\beta \approx -1.3$. Is the latter effect driven by the assessment of spending or taxation? Such data are not directly available for structural balance components. In line with BL, in column (2) we split the structural balance into spending (G) and revenues (T), where $SB=T-G$. In terms of cyclical adjustment, we assume that government spending is insensitive and use its actual value G, while calculating cyclically adjusted revenues as the residual $T=SB+G$.[16] It turns out that the negative impact of government spending cuts was more strongly underestimated than the one from tax hikes. This is consistent with evidence from the meta regression of Gechert and Rannenberg (2018) who show that in particular spending multipliers increase during downturns.

A natural objection to the validity of the effects in columns (1) and (2) is the small sample size and the likely dependence on influential outliers. Using a quantile regression instead,

[16] This assumption is questionable since for example government spending on unemployment benefits and old age benefits is sensitive to the business cycle (Price et al. 2014). DFE measures are published separately for spending and revenues, so we don't need such an assumption there.

does only minimally alter the coefficient (column (6)). Likewise, in column (3) we exclude those countries in our sample that were under a bailout program (Greece, Ireland and Portugal). The effect is somewhat muted but still economically and statistically highly significant. Column (4) shows the results for a widened sample of advanced countries. Interestingly, β is not statistically significant any more. Arguably, many European countries were in a deeper crisis and bound to common currency and monetary policy at its zero lower bound during this phase. In line with this assessment, narrowing the sample to Euro Area countries in column (5) even slightly increases the effects.

In Table 1(b), using the DFE, the qualitative results are confirmed. However, the effect is somewhat smaller. This is in line with our reasoning above: the effects as measured by SB might be somewhat upward biased due to its possible endogeneity with growth forecast errors. Separating expenditures and revenues, which are directly available for the DFE, in column (2) gives consistent, though insignificant results; but the wide standard errors may not be trusted due to multicollinearity: The correlation of the series is extremely high ($cor_{DFEG,DFET} = .92$). Moreover, as shown in columns (3) and (4), including G and T, one at a time, strongly inflates the coefficients. Of course, the coefficients of (3) and (4) must not be trusted as they pick up the influence of the omitted counterpart of the budget, but they still show that the coefficients of column (2) could be significant if multicollinearity was absent.

In general, we can reconfirm the substantial underestimation of fiscal multipliers during the early stages of the Euro Area crisis as found by BL. Using a superior measure of the fiscal stance, the effect however, is more in a range of 0.8-0.9. Together with the well documented assumption that IMF forecasters implicitly used a multiplier effect of around 0.5, actual multipliers for the crisis period under investigation should lie in a range of 1.3 to 1.4. This is very much in line, but on the lower end of findings of ZLB effects in standard macroeconomic models (Christiano et al. 2011, Eggertsson 2011, Woodford 2011).

Persistence of fiscal multipliers

Investigating the persistence of multipliers, in Table 2(a) column (1), we first replicate the result of FS. The factor of persistence is close to one, which could be interpreted such that the GDP losses caused by fiscal consolidation became permanent, at least given the currently available information set on a five-year horizon.

[Table 2(a) about here]

[Table 2(b) about here]

Again, the results of the TSLS estimation are robust to the changes already discussed for the first stage in Table 1. Splitting the structural balance in spending and revenue components only minimally changes the estimated persistence (column (2)). So does a sample based on Euro Area countries (columns (4)). Down-weighting outliers by excluding program countries or using quantile regressions even reinforces the persistence (columns (3) and (5)). Interestingly, the results are not robust to using a direct regression like

$$\Delta \mathrm{Pot} Y^{fe}_{i,t:t+5} = \zeta + \eta\, \Delta F^{f}_{i,t:t+1|t} + \vartheta_{i,t:t+1|t} \qquad (5)$$

of the *t+5* potential output forecast error on the fiscal stance in column (6). The coefficient, which should be $\eta = \beta \cdot \delta$ has the expected sign and is large, yet is not statistically significant.

The results become more robust and persistence is even a little bit stronger when using the DFE measure of fiscal stance in Table 2(b). Moreover, the instrument seems quite strong judging from the first stage F statistics. In general, while the estimated multiplier effect is somewhat lower on impact when using the DFE, it is super-persistent and increases over the 5-year horizon by a factor of 1.25, or 1.05 per year. This time, the direct regression in column (6) is highly significant.

4. FURTHER ROBUSTNESS TESTS

Controlling for alternative explanations

As discussed in the introduction, there might be other factors at play that explain growth disappointments and that would lead to an omitted variable bias in our simple regressions. As a general note, it is vital to look at control variables that were already in the information set of forecasters to see if their impact was underestimated. Any later realizations of these variables that could have an influence on realized output growth would most likely be prone to reverse causality issues. For example, an increase in sovereign CDS spreads could cause lower growth but could as well be caused by growth disappointments (Cottarelli and Jaramillo 2012).

Most basically, since we exploit cross-sectional variation, our findings could be challenged by varying optimism and pessimism of forecasts for specific country-year observations that could explain the variation in forecast errors after 2010.[17] Some earlier literature points to politically motivated over-optimism in growth forecasts by national authorities (Jonung and Larch 2006, Frankel 2011). If this is the case for our sample, there should be a positive correlation of pre-crisis and within-crisis forecast errors. However, the correlation coefficient for the average IMF WEO April forecast errors of vintages of 1997-2006, with the 2010 vintage forecast error is close to zero ($\rho = 0.03$). So there is no indication that forecasts for countries with a large negative forecast error in the relevant period 2010-11 where generally too optimistic in pre-crisis years. The same holds for AMECO spring forecast vintages where we have data from 2000 onwards ($\rho = -0.04$).

Table 3(a) and (b) and Table 4(a) and (b) present regression results including various control variables using SB and DFE for the first stage and second stage regressions, respectively. Due

[17] We thank an anonymous referee for pointing us to this concern.

to low degrees of freedom, we include these controls one at a time. Column β in Tables 3 and δ in Tables 4 show our parameters of interest, the effects of multiplier underestimation and persistence; column θ and π give the coefficients of the control variables.

[Table 3(a) about here]

[Table 3(b) about here]

[Table 4(a) about here]

[Table 4(b) about here]

In row (1) we ask whether the *under-prediction of the 2008-09 recession* might in fact predict the 2010-11 forecast error. The rationale would be that the persistence of the crisis was underestimated and that the double dip was inevitable though not forecasted. The effect of fiscal consolidation, however, remains intact and the financial crisis forecast error is not significant. This holds true for both SB and DFE for first and second stage. In a similar fashion, in row (2) we control for the *size of the forecasted GDP growth* during the 2010-11 period itself. Maybe countries with strongly negative forecast errors simply had a comparably large GDP growth forecast from the outset that was unrealistic. However, including this variable does not affect the results qualitatively, even though the persistence parameter increases somewhat. The GDP forecast itself is negative and significant in the second stage. This is plausible, as higher expected GDP growth might have increased the potential output forecast and thus even made the potential output forecast error more negative.

Dovern and Jannsen (2017) show that findings of low forecast errors mask substantial differences for periods of recessions and expansions with strongly too optimistic forecasts for periods that turn out to be recessions ex post. Since our dataset includes observations with recessions and recoveries in 2010-11, the differential forecast errors might be driven by the

generally poor performance of forecasters in predicting recessions.[18] We control for this possibility in row (3) by including a recession dummy that equals 1 if a country had a negative realized growth rate in 2010 or 2011. The coefficient θ has the expected negative sign and is large, but remains insignificant. The coefficients of the DFE variable remain largely unaltered but in the second stage regression the SB turns insignificant. We also test a somewhat similar specification in row (4). There we use a dummy that equals 1 for an observation with a negative forecast error in 2010-11 as a control variable. Since such a variable should explain the big variation in forecast errors between countries, the fiscal policy variable can only take care of the more gradual differences between countries that are not due to underestimated recessions or recoveries. As expected, the coefficient of the dummy is negative, large, and highly significant. β is somewhat reduced and becomes insignificant for the SB case, but remains highly significant in the DFE case. Second stage results correspond to that.

Could it have been an underestimation of the sheer *size of consolidation* instead of the multiplier effect of consolidation that explains growth disappointments? In row (3) we add the forecast error of the change in the structural balance as an additional control. Again, the effects remain intact. Moreover, there seems to be no relevant underestimation of the consolidation effort during the 2010-11 years. The multiplier effect largely dominates the size effect in terms of forecast errors. What about the *consolidation effort of trading partners*, which could spill over to domestic growth? Adding in row (4) the trade-weighted consolidation effort of trading partners as measured by the change in their structural balance and scaled by the share of exports in GDP does not affect our coefficients of interest, even if the parameter itself becomes highly significant and large.

[18] We are grateful to an anonymous referee for pointing us to this issue.

Another perturbing candidate could be ignoring the impact of the *soundness of domestic public finances*. Maybe forecasts were too optimistic because public finances were in bad shape and their influence on growth might have been underestimated. We test this possibility in rows (5) to (7) where we use as a proxy either the initial sovereign debt-to-GDP level of 2009, the initial structural balance of 2009 or the spread of sovereign credit default swaps as an average during the first quarter of 2010, respectively. The parameters belonging to the DFE measure are qualitatively unaffected. When controlling for the initial structural balance in 2009, the persistence of multipliers is even reinforced. The coefficient of the initial structural balance itself becomes significantly negative in the second stage of the DFE estimation, meaning that for countries with higher structural deficits on the outset, potential growth forecasts were comparably too pessimistic. The stabilizing role of expansionary fiscal policy seems to have been underestimated. In the case of sovereign CDS spreads, first stage results do not change much, but the significance levels of the persistence parameter become lower in the DFE case and even insignificant when using the SB.

What about the private sector and its likely underestimated impact on growth through bank stress or private debt overhang? Controlling for the indicator of Laeven and Valencia (2012), which signals whether a country is in a *banking crisis* in a certain year, does not affect our parameters of interest. Using *pre-crisis household debt-to-GDP ratios* of 2007 as a proxy for the pressure to deleverage does not affect the first stage regressions, but lowers the significance level of the persistence parameter in the SB case. The DFE case again is much more robust. Finally, when controlling for the *pre-crisis current-account-to-GDP ratio* as a measure of external imbalances that might have stalled output growth more than expected, we again find our DFE estimation largely unaffected. For the SB case, the persistence coefficient turns smaller and insignificant.

Summing up, controlling for various alternative explanations does not affect our central findings at least when we rely on the narrative DFE measure, where also the *F* statistics still

signal strong instruments. For the coefficients of the SB measure results remain robust in most instances but the instruments become even weaker.

Using European Commission Data

Is the IMF forecast a special case? We test the European Commission's forecast as well, using the spring 2010 European Economic Forecast as well as *t+4* forecasts of potential output. The EC data include the whole EU27 and thus some additional Eastern European countries, that are absent from the IMF dataset. Repeating the previous regressions with EC data, most of the results are confirmed. Results are presented in an online Appendix B. Estimates using the structural balance are even more robust to the perturbations we tested for the IMF data. Concerning the DFE there are two interesting and plausible outliers: for the whole EU27, the coefficients of interest are somewhat weaker ($\beta = [.5; .7], \delta = [.9; 1.1]$). Most notably, the relation completely diminishes when excluding the program countries (Table B1(b), Column(3)), and the separate effects of spending and revenue shocks is turned upside down (Table B1(b), Column(2)). These findings are fully driven by the data of Latvia and Lithuania, countries that are absent in the IMF dataset and that witnessed a tremendous crash in 2009 with a cumulated GDP growth forecast error for the years 2008-09 as of the 2008 spring forecast of more than −20 pp each. It is not implausible that (potential) growth forecasts where more on the pessimistic side in the following years. Moreover, both countries are very small, very open economies that joined the EU only in 2004, which gave them a strong push to export growth. In such circumstances fiscal devaluation is considered less harmful (Perotti 2011). When we exclude these special cases, the previous results of the IMF sample are reestablished in full (Series (c) of Tables B1-4, online Appendix B).

Extending the Time Dimension

In our baseline we derive forecast errors from the vintage of spring 2016 vis-à-vis 2010 and are therefore restricted to only 21 / 22 observations in the IMF case and 27 with EC data. Fiscal consolidation in many European countries has, however, continued after 2011. Also, it might be interesting to check the short and long run impact for late crisis years. Therefore, we test for forecasts in subsequent years and extend the time dimension of the estimation in two ways. First, we assess different forecast vintages individually in form of a moving window and second, jointly in a panel structure. As we only have limited access to IMF vintages with *t+5* forecasts we concentrate in the main body of this paper on results with EC data for the moving window and panel model exercise. Online Appendix B presents limited samples with IMF data. Generally, the results for the first and second stage are robust to the exercise of extending the time dimension when using the DFE as fiscal shocks, while using SB produces rather inconsistent results. The model for the moving window is equivalent to section 2. The two-year fiscal shocks and growth forecast errors move along with the respective vintage year. Table 5(a) and (b) and Table 6(a) and (b) show moving window regression results for vintages between 2010 and 2014 using SB and DFE for the first and second stage, respectively.

[Table 5(a) about here]

[Table 5(b) about here]

[Table 6(a) about here]

[Table 6(b) about here]

In the first stage SB case, baseline results are not confirmed by other vintage years, β becomes economically and statistically insignificant. However, using DFE provides robust results for the main period of European consolidation, vintage years 2010-12 with a multiplier underestimation between -0.5 and -0.6. Afterwards the effect vanishes, which may be due to

the slowdown of consolidation in general, the fact that forecasters learned from their mistakes or be interpreted in line with findings of regime-dependent multiplier effects (Auerbach and Gorodnichenko 2012, Baum et al. 2012). Turning to the second stage provides a similar picture. The baseline persistence is qualitatively confirmed for DFE while SB only yields mixed results. For the years 2010-12, persistence estimated using DFE is on a somewhat higher level compared to baseline, δ increases from 0.965 for 2010 to 1.394 for 2012, afterwards fiscal shocks show no significant persistence effect. Hence, we observe a weakening of the effects in late crisis years. Contrary to baseline estimates, the results for later vintages do not elementarily differ when excluding Latvia and Lithuania.

In a next step we increase the number of observations by applying a panel structure with different sets of vintages, following BL in the case of short-term multipliers. For the sake of brevity, our panel regression results are discussed in detail in an online Appendix C. The panel results generally confirm the baseline estimates. Again, DFE proofs to be quite robust for alternative time dimensions, while SB remains ambiguous. The coefficient β stays within the range of 0.4 to 0.6. Including late crisis years somewhat lowers β but the structural underestimation does not vanish. Coefficient δ on the contrary increases with time, from 1.0 (for years 2010/11) to 1.2 (2010/14). Note that further specifications with different panel dimensions[19] after the crisis for both the SB and DFE case do not alter the general picture drawn so far – quite robust estimates with general weakening of the baseline effects in later crisis years, in line with the slowdown of consolidation, potential learning effects or the end of the downturn regime.

Exploring different time dimensions has shown that the severe underestimation of consolidation effects on output in the short-run and the subsequent persistence is restricted to the period of the European debt crisis where we observed significant contractions.

[19] The panel sample might start later or be shortened, e.g. 11/12-11/14, 12/13 etc.

Accordingly, the results could speak to the empirical literature on regime-dependent multipliers showing significantly increases of effect sizes in recessionary periods of the business cycle and on the lower end of findings under the ZLB in standard macroeconomic models (Christiano et al. 2011, Eggertsson 2011).

5. CONCLUDING REMARKS

By exploiting forecast errors of output and long-term potential output growth in the spirit of Blanchard and Leigh (2013) and Fatas and Summers (2016), but using a superior, narratively-identified measure of the fiscal stance, we have investigated as to whether the size and persistence of fiscal multipliers was underestimated for the austerity measures that were implemented in Europe after 2009. In line with these earlier papers, we find that multipliers were strongly underestimated by about 0.7 to 1.0 units. This would translate into a multiplier range of 1.2-1.5, given that forecasters of the IMF and the European Commission on average assumed a multiplier of 0.5, a claim, for which we presented some evidence. Most interestingly, fiscal policy seems to have had a permanent effect in the 2010-11 period and beyond. These results hold up to a battery of perturbations and particularly so when relying on our improved identification strategy. Interestingly, it turns out that the effects weaken for measures in late crisis years after 2013 and when including very small very open economies.

For our European sample, we find evidence for strong hysteresis effects as opposed to the short-run pain, long-term gain consensus that emerged after the early crisis years. That is, the turn to belt tightening was badly timed and therefore much more costly in terms of long-term output loss than a more gradual, backloaded consolidation.

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Appendix A – Computing forecast errors

For the calculation of real GDP and potential GDP growth forecast errors we follow the approach of FS. The main issue is to make the data for GDP or potential GDP comparable between the different vintages. The problem is caused by "data revisions, changes in base year and also changes in national accounting rules" (FS; p. 31). Real GDP growth forecast errors are defined as follows:

$$\Delta Y^{fe}_{i,t:t+1} \equiv \Delta Y_{i,t:t+1} - \Delta Y^{f}_{i,t:t+1|t} \,,$$

with $\Delta Y^{fe}_{i,t:t+1}$ growth forecast error of real GDP for the years t to t+1 for country *i*, *Y* actual GDP at the latest vintage and Y^f its respective forecasted value at vintage year *t*. Hence, the forecast error of GDP growth is given by the difference between current-vintage figures of the cumulated change in GDP over two years and its forecast in the spring vintage of year *t*. In all cases, current-vintage figures are taken from the spring 2016 publication of either the IMF WEO or the European Commission Economic Forecast. In order to account for base-year revisions or changes in national accounting rules that would bias our estimate of the forecast error, we rebase both real GDP level series at *t-1*, where *t* is the year of the earlier vintage. That is, we create two indices for real GDP *Y*, first for the 2010 vintage and second for the 2016 vintage, and use 2009 as base year (=100) for both series such that any technical level revisions are ruled out and we can simply compare the subsequent growth. Note that if we would analyze the 2011 vintage, our base year would be 2010 and so on. Afterwards, we simply derive the forecast error in our example with

$$\Delta Y^{fe,2010}_{i,2010:2011} = \frac{Y^{2016}_{i,2009:2011} - Y^{2010}_{i,2009:2011}}{Y^{2010}_{i,2009:2011}} \cdot 100.$$

Turning to potential output ($PotY$), given our interest in 5-year growth rate forecast errors for potential output we define them as follows:

$$\Delta PotY^{fe}_{i,t:t+5} \equiv \Delta PotY_{i,t:t+5} - \Delta PotY^{f}_{i,t:t+5|t}.$$

When computing forecast errors for potential output the values of the different vintages have to be adjusted in a slightly different way because as new (disappointing) GDP data come in, the assessment of past potential output values is revised (downwards) as well. Simply comparing cumulated potential growth rates of different vintages would therefore unduly downplay the forecast error. However, we still want to get rid of technical revisions due to changes in definitions or base years. We compute the *t+5* potential output growth forecast error of the 2010 vintage as

$$\Delta PotY_{i,2010:2015}^{fe,2010} = \frac{PotY_{i,2015}^{2016} - PotY_{i,2015}^{2010} \cdot k}{PotY_{i,2015}^{2010} * k} \cdot 100,$$

with k being an adjustment factor, given by

$$k = \frac{Y_{i,2009}^{2016}}{Y_{i,2009}^{2010}}.$$

That is, we adjust the potential output forecasts at vintage *t* by multiplying them with the ratio of the actual level GDP of *t-1* divided by the level of GDP of *t-1* at vintage *t*, thus correcting for any technical level revisions while acknowledging revisions of past potential output due to growth disappointments. Note that in the case of potential output we have *t+5* forecasts only for IMF data. The EC forecasts only incorporate figures up to *t+4*. This caveat has to be considered when comparing the results for the IMF and the EC case in section 4.

Online Appendix B

This appendix discloses further robustness tests for our regressions. First, we repeat the exercises of Tables 1-4, this time for European Commission Economic Forecast data of spring 2010 against spring 2016. Again, we use both the SB (this time from the European Commission) and the DFE measure. Tables B1-B4 display the findings.

[Table B1(a) about here]

[Table B1(b) about here]

[Table B1(c) about here]

[Table B2(a) about here]

[Table B2(b) about here]

[Table B2(c) about here]

[Table B3(a) about here]

[Table B3(b) about here]

[Table B3(c) about here]

[Table B4(a) about here]

[Table B4(b) about here]

[Table B4(c) about here]

In general, the results are confirmed. The effects based on SB (series (a)) are even more robust when using the EC forecasts. However, when including all 27 EU countries the effects weaken somewhat for the DFE measure (series (b)): they do not hold when excluding Greece, Portugal and Spain (Table B1(b), column (3)); moreover, the earlier finding that underestimation of multipliers was stronger for spending side-measures is turned upside down. These changes very much depend on the inclusion of Latvia and Lithuania. Series (c) of Tables B1-B4 gives the findings based on DFE for a sample excluding these two observations: all previous results are reconfirmed.

Table B5 presents moving window estimations with IMF data, showing a very similar picture for the two years of available vintages compared to the EC data case. DFE shocks indicate to have a weakening effect on the coefficients in the first and second stage, while first stage SB estimations are only significant in the 2010 baseline. The second stage for SB looks comparatively good, but cannot be relied upon given the opaque first stage results.

Table B6(a) and (b) include panel specifications with available IMF data, confirming previous results.

[Table B5 about here]

[Table B6(a) about here]

[Table B6(b) about here]

Online Appendix C

We increase the number of observations by applying a panel structure with different sets of vintages, following BL in the case of short-term multipliers. The estimation procedure is analogous to the TSLS estimation described in section two of the paper but features a time-fixed effect. The panel model has the following properties for the first (6) and the second stage (7)[20],

$$\Delta\hat{Y}^{fe}_{i,t:t+1} = \alpha + \rho_t + \beta\Delta F^{f}_{i,t:t+1|t} \quad (6)$$

$$\Delta \text{Pot}Y^{fe}_{i,t:t+5} = \gamma + \rho_t + \delta\Delta\hat{Y}^{fe}_{i,t:t+1} (+X_i\pi) + \omega_{i,t:t+1|t} \quad (7)$$

with ρ_t as a vector of time-fixed effects and t = 2010, … , 2013.

The panel results (Table C1-C5) generally confirm the baseline estimates. Again, DFE proofs to be quite robust for alternative time dimensions, while SB remains ambiguous. The coefficient β stays within the range of 0.4 to 0.6, see Table C1(b). Column 10/11 presents results for a panel estimation including vintage years 2010 and 2011, column 10/12 the years 2010, 2011 and 2012, and so on. Including late crisis years somewhat lowers β but the structural underestimation does not vanish. Coefficient δ (Table C2(b)) on the contrary increases with time, from 1.0 (10/11) to 1.2 (10/14). Even though δ shows a similar development for SB, results are not to be trusted given the insignificant first stage results. Nonetheless, panel samples using SB shocks starting already in 2009 deliver more robust estimates but on a somewhat lower level regarding the underestimation of short-run effects,

[20] For estimation we use the STATA command "ivreg2" by Baum et al. (2010) with robust standard errors.

see Table C3. Also note that further specifications with different panel dimensions[21] after the crisis for both the SB and DFE case do not alter the general picture drawn so far – quite robust estimates with general weakening of the baseline effects in later crisis years, in line with the slowdown of consolidation, potential learning effects or the end of the downturn regime.

[Table C1(a) about here]

[Table C1(b) about here]

[Table C2(a) about here]

[Table C2(b) about here]

[Table C3 about here]

[Table C4 about here]

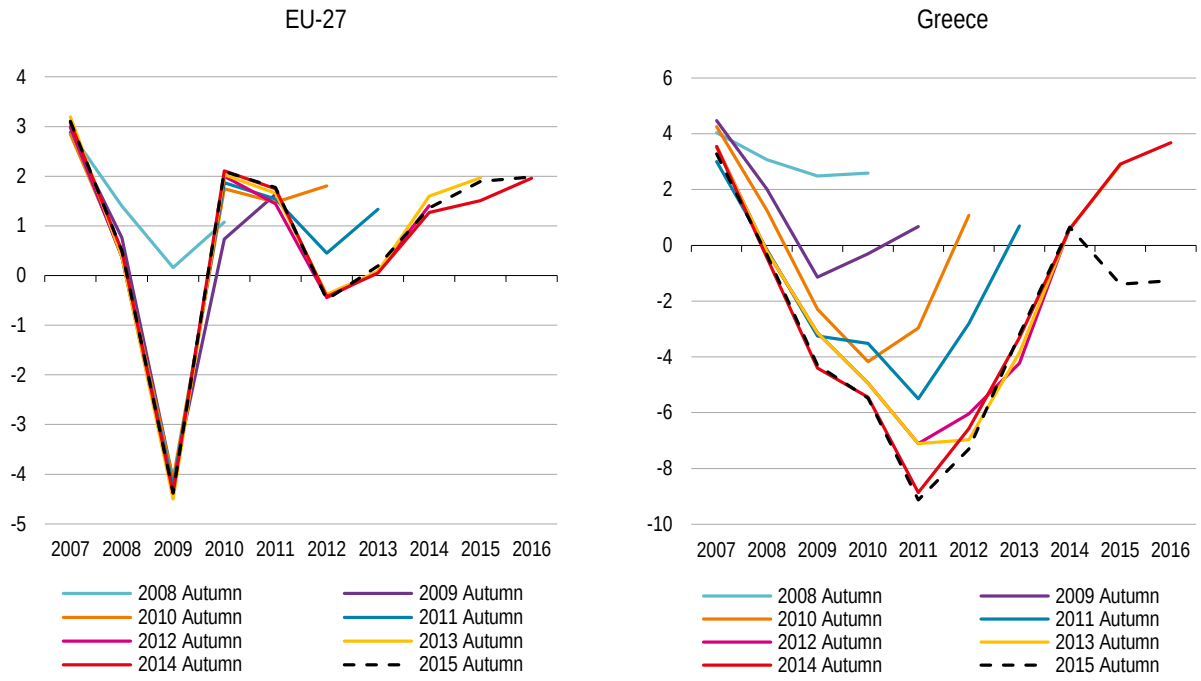
[Table C5 about here]

Lastly, we test how our panel results change when the control variables from above are included. Table C4 presents the underestimation of multiplier effects including all controls simultaneously. Estimates with DFE are very robust to this exercise. Findings for the second stage including control variables show similar characteristics, Table C5 – δ remains robust to the controls for DFE, while it does not for SB.

[21] The panel sample might start later or be shortened, e.g. 11/12-11/14, 12/13 etc.

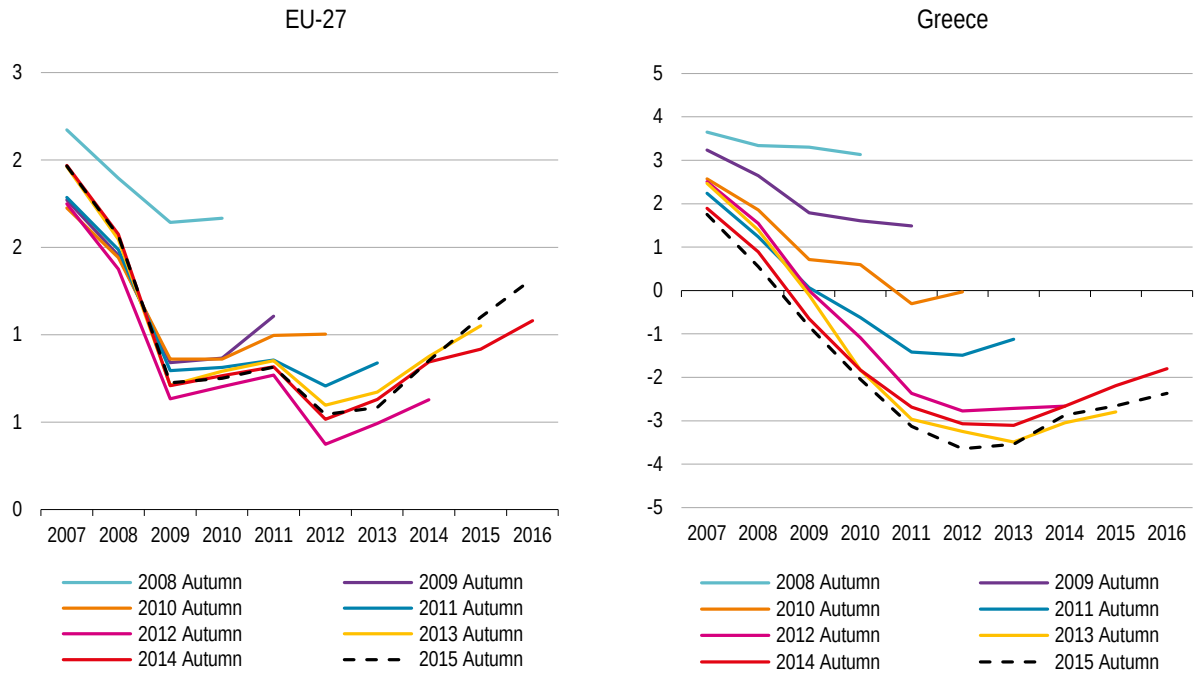
Figures and Tables

Figure 1: Vintages of GDP growth rate forecasts for the EU-27 and Greece, in %, 2007-2016



Source: Ameco, Firstrun database "A dataset of fiscal variables", own illustration.

Figure 2: Vintages of potential GDP growth rate forecasts for the EU-27 and Greece, in %, 2007-2016



Source: Ameco, Firstrun database "A dataset of fiscal variables", own illustration.

Table 1(a): First stage: Underestimation of multipliers with structural balance (SB)

Endogenous: forecast error of GDP 2010-11						
OLS	(1)SB	(2)GT	(3)NOPROG	(4)ADVA	(5)EURO	(6)QUANT
β	-1.341**		-0.942***	-0.632	-1.534**	-1.272***
	(0.53)		(0.243)	(0.614)	(0.578)	(0.306)
β^G		-1.699***				
		(0.477)				
β^T		-0.967**				
		(0.371)				
const	1.15***	1.223***	1.101***	0.919*	1.34***	0.856
	(0.402)	(0.36)	(0.374)	(0.493)	(0.393)	(0.62)
N	22	22	19	31	14	22
Adj. R^2	0.4755	0.6024	0.3307	0.0749	0.5763	

Table 1(b): First stage: Underestimation of multipliers with discretionary fiscal effort (DFE)

Endogenous: forecast error of GDP 2010-11							
OLS	(1)DFE	(2)GT	(3)G	(4)T	(5)NOPROG	(6)EURO	(7)QUANT
β	-0.861***				-0.934*	-0.875***	-0.874***
	(0.055)				(0.498)	(0.052)	(0.055)
β^G		-0.928	-1.906***				

		(0.761)	(0.209)				
β^T		-0.812		-1.462***			
		(0.566)		(0.122)			
const	2.75***	2.761***	2.84***	2.552***	2.7***	2.835***	2.573***
	(0.489)	(0.516)	(0.553)	(0.522)	(0.713)	(0.573)	(0.669)
N	21	21	21	21	18	16	21
Adj. R^2	0.6983	0.6816	0.6639	0.6721	0.1306	0.7508	

Table 2(a): Second Stage: Persistence of multiplier effects (SB)

Endogenous: forecast error of potential GDP 2010-15						
TSLS	(1)SB	(2)GT	(3)NOPROG	(4)EURO	(5)QUANT	(6)DIR
δ	1.005**	1.046***	1.296**	1.065***	1.401**	
	(0.402)	(0.289)	(0.544)	(0.387)	(0.647)	
η						-1.348
						(1.013)
const	-3.521**	-3.537***	-4.016**	-3.548***	-3.834**	-2.365**
	(0.869)	(0.819)	(0.861)	(1.114)	(1.356)	(1)
N	22	22	19	14	22	22
Adj. R^2	0.5813	0.5813	0.3346	0.6866		0.1218
1st stage F	6.3952	6.3621	15.036	7.0449		

Table 2(b): Second Stage: Persistence of multiplier effects (DFE)

Endogenous: forecast error of potential GDP 2010-15						
TSLS	(1)DFE	(2)GT	(3)NOPROG	(4)EURO	(5)QUANT	(6)DIR
δ	1.236***	1.234***	1.319*	1.28***	1.216***	
	(0.072)	(0.073)	(0.689)	(0.086)	(0.233)	
η						-1.065***
						(0.1)
Const	-3.914***	-3.912***	-4.459*	-3.303***	-4.304***	-0.515
	(0.745)	(0.747)	(1.523)	(0.872)	(0.704)	(1.257)
N	21	21	18	16	21	21
Adj. R^2	0.6807	0.6807	0.6109	0.7303		0.3203
1st stage F	244.36	115.39	3.5254	279.43		

Table 3(a): First stage with controls (SB)

additional control *X*	β	θ	const	n	Adj. R^2
(1) fe gdp0709	-1.316(0.516)**	0.043(0.143)	1.451(0.915)	22	0.451
(2) forec gdp1011	-1.174(0.442)**	0.242(0.238)	0.409(0.842)	22	0.473
(3) reces dummy	-1.018(0.507)*	-2.567(1.559)	1.437(0.456)***	22	0.524
(4) fe dummy	-0.972(0.632)	-2.323(1.091)**	1.895(0.604)***	22	0.550
(5) fe sb1011	-1.161(0.427)**	-0.51(0.365)	1.553(0.582)**	22	0.501
(6) trade part cons	-1.402(0.488)***	2.321(1.695)	0.964(0.419)**	22	0.509

(7)	sov'n debt09	-1.29(0.507)**	-0.008(0.018)	1.632(1.284)	22	0.451
(8)	sb09	-1.095(0.621)*	0.141(0.248)	1.689(1.131)	22	0.454
(9)	sov'n cds10q1	-1.199(0.587)*	-0.003(0.006)	1.408(0.639)**	22	0.457
(10)	bankcrisis	-1.324(0.515)**	-0.268(0.881)	1.262(0.481)**	22	0.450
(11)	private debt 07	-1.312(0.557)**	0(0.008)	1.107(0.955)	21	0.433
(12)	ca07	-1.301(0.685)*	0.013(0.087)	1.143(0.395)***	22	0.448

Table 3(b): First stage with controls (DFE)

additional control *X*	β	θ	const	n	Adj. R^2
(1) fe gdp0709	-0.861(0.06)***	-0.06(0.103)	2.186(1.038)**	21	0.693
(2) forec gdp1011	-0.864(0.139)***	-0.005(0.305)	2.77(1.195)**	21	0.682
(3) reces dummy	-0.794(0.111)***	-0.902(1.469)	2.733(0.495)***	21	0.686
(4) fe dummy	-0.702(0.081)***	-2.353(0.639)***	3.19(0.521)***	21	0.770
(5) fe sb1011	-0.906(0.11)***	0.263(0.326)	2.35(0.473)***	19	0.733
(6) trade part cons	-0.857(0.051)***	2.12(0.686)***	2.557(0.465)***	21	0.725
(7) sov'n debt09	-0.806(0.084)***	-0.015(0.018)	3.507(1.183)***	21	0.695
(8) sb09	-0.803(0.138)***	0.043(0.198)	2.563(0.812)***	19	0.725
(9) sov'n cds10q1	-0.966(0.336)**	0.007(0.018)	2.344(0.949)**	20	0.680
(10) bankcrisis	-0.85(0.053)***	-0.72(0.85)	3.068(0.686)***	21	0.694
(11) private debt 07	-0.876(0.061)***	-0.004(0.006)	3.17(1.004)***	20	0.703
(12) ca07	-0.924(0.125)***	-0.05(0.094)	2.787(0.525)***	21	0.690

Table 4(a): Second stage with controls (SB)

additional control *X*	δ	π	const	n	Adj. R^2	1st F
(1) fe gdp0709	0.845(0.406)**	0.374(0.263)	-0.715(1.792)	22	0.575	3.327
(2) forec gdp1011	1.382(0.403)***	-0.729(0.275)***	-1.72(1.102)	22	0.684	3.611
(3) reces dummy	0.93(0.753)	-0.807(4.141)	-3.344(1.616)**	22	0.539	5.003
(4) fe dummy	0.579(0.952)	-3.6(4.355)	-1.877(2.582)	22	0.472	9.202
(5) fe sb1011	1.043(0.508)**	0.145(0.636)	-3.679(1.209)***	22	0.571	4.029
(6) trade part cons	1.135(0.276)***	6.628(1.59)***	-4.203(0.598)***	22	0.708	5.004
(7) sov'n debt09	1.124(0.472)**	0.025(0.036)	-5.156(2.78)*	22	0.590	3.252
(8) sb09	1.567(0.699)**	-0.432(0.54)	-5.816(3.004)*	22	0.625	2.997
(9) sov'n cds10q1	0.962(0.605)	-0.001(0.01)	-3.364(1.713)**	22	0.550	3.328
(10) bankcrisis	1.083(0.37)***	1.625(1.41)	-4.291(1.199)***	22	0.595	3.620
(11) private debt 07	0.763(0.456)*	0.027(0.015)*	-7.345(1.682)***	21	0.659	2.783
(12) ca07	0.744(0.76)	0.111(0.166)	-3.286(0.986)***	22	0.505	3.541

Table 4(b): Second stage with controls (DFE)

additional control *X*	δ	π	const	n	Adj. R^2	1st F
(1) fe gdp0709	1.236(0.074)***	-0.023(0.121)	-4.131(1.44)***	21	0.663	103.67
(2) forec gdp1011	1.651(0.201)***	-0.859(0.369)**	-1.731(1.251)	21	0.771	115.65
(3) reces dummy	1.289(0.169)***	0.611(1.948)	-4.048(1.099)***	21	0.671	92.73
(4) fe dummy	0.974(0.202)***	-3.319(2.122)	-2.574(1.311)**	21	0.659	49.25

(5)	fe sb1011	1.312(0.189)***	0.381(0.537)	-4.582(0.805)***	19	0.635	88.94
(6)	trade part cons	1.22(0.092)***	6.582(1.605)***	-4.469(0.548)***	21	0.780	143.76
(7)	sov'n debt09	1.297(0.144)***	0.014(0.029)	-4.799(2.018)**	21	0.677	128.32
(8)	sb09	1.808(0.263)***	-0.727(0.297)**	-8.094(1.738)***	19	0.755	91.09
(9)	sov'n cds10q1	1.263(0.737)*	0.003(0.036)	-4.404(3.956)	20	0.660	97.64
(10)	bankcrisis	1.286(0.099)***	2.709(1.481)*	-5.249(1.1)***	21	0.732	148.98
(11)	private debt 07	1.12(0.083)***	0.019(0.016)	-6.542(1.552)***	20	0.742	122.66
(12)	ca07	1.331(0.13)***	-0.065(0.096)	-4.128(0.707)***	21	0.678	130.57

Table 5(a): Moving Window First Stage (SB)

Endogenous: forecast error of GDP t:t+1					
OLS	2010	2011	2012	2013	2014
β	-1.166**	0.065	-0.473	0.564	0.204
	(0.461)	(0.268)	(0.312)	(0.389)	(0.578)
const	0.633	-2.916***	-0.876	0.584	0.649
	(0.500)	(0.847)	(0.554)	(0.571)	(0.526)
n	27	27	27	27	27
Adj. R^2	0.431	0.001	0.094	0.065	0.009

Table 5(b): Moving Window First Stage (DFE)

Endogenous: forecast error of GDP t:t+1					
OLS	2010	2011	2012	2013	2014
β	-0.586**	-0.609**	-0.530***	0.248	0.343
	(0.245)	(0.252)	(0.116)	(0.172)	(0.286)
const	2.216***	-1.057	-0.029	0.474	0.417
	(0.552)	(0.851)	(0.460)	(0.570)	(0.516)
n	27	27	27	27	27
Adj. R^2	0.376	0.308	0.440	0.056	0.030

Table 6(a): Moving Window Second Stage (SB)

Endogenous: forecast error of potential GDP t:t+4					
TSLS	2010	2011	2012	2013	2014
δ	1.271***	-0.087	2.877***	1.379	1.445
	(0.222)	(5.688)	(1.095)	(1.279)	(2.223)
const	-3.909***	-3.359	3.921**	0.601	0.401
	(0.690)	(15.840)	(1.836)	(0.978)	(1.518)
n	27	27	27	27	27
Adj. R^2	0.543	-0.126	0.186	0.658	0.729
1st stage F	6.408	0.0594	2.290	2.101	0.124

Table 6(b): Moving Window Second Stage (DFE)

Endogenous: forecast error of potential GDP t:t+4					
TSLS	2010	2011	2012	2013	2014

δ	0.965***	0.983***	1.394***	0.893	-0.242
	(0.257)	(0.190)	(0.298)	(1.245)	(1.152)
const	-3.675***	-0.312	1.731*	1.031	1.449
	(0.690)	(0.913)	(0.965)	(1.159)	(1.342)
N	27	27	27	27	27
Adj. R^2	0.514	0.522	0.486	0.504	-0.305
1st stage F	5.708	5.826	20.75	2.077	1.439

Table B1(a): EC forecast, First stage (SB)

Endogenous: forecast error of GDP 2010-11					
OLS	(1)SB	(2)GT	(3)NOPROG	(4)EURO	(5)QUANT
β	-1.166**		-0.618*	-1.676***	-0.966***
	(0.461)		(0.327)	(0.441)	(0.266)
β^G		-1.719***			
		(0.397)			
β^T		-0.959***			
		(0.282)			
const	0.633	0.895**	1.134**	0.584	0.614
	(0.5)	(0.392)	(0.434)	(0.513)	(0.637)
N	27	23	24	17	27
Adj. R^2	0.4087	0.6636	0.1557	0.6771	

Table B1(b): EC forecast, First stage (DFE)

Endogenous: forecast error of GDP 2010-11					
OLS	(1)DFE	(2)GT	(3)NOPROG	(4)EURO	(5)QUANT
β	-0.586**		0.012	-0.863***	-0.861**
	(0.245)		(0.243)	(0.048)	(0.404)
β^G		0.405			
		(0.298)			
β^T		-1.567***			
		(0.267)			
const	2.216***	2.062***	1.325**	2.681***	2.501***
	(0.552)	(0.504)	(0.588)	(0.484)	(0.859)
N	27	27	24	17	27
Adj. R^2	0.3512	0.5185	-0.0453	0.7937	

Table B1(c): EC forecast, First stage (DFE), excl. Latvia, Lithuania

Endogenous: forecast error of GDP 2010-11					
OLS	(1)DFE	(2)GT	(3)NOPROG	(4)EURO	(5)QUANT
β	-0.844***		-1.077**	-0.863***	-0.852***
	(0.056)		(0.47)	(0.048)	(0.039)
β^G		-1.14**			
		(0.431)			
β^T		-0.616*			

		(0.315)			
const	2.273***	2.309***	2.497***	2.681***	2.35***
	(0.429)	(0.445)	(0.678)	(0.484)	(0.631)
N	25	25	22	17	25
Adj. R^2	0.6703	0.6590	0.2201	0.7937	

Table B2(a): EC forecast, Second stage (SB)

Endogenous: forecast error of potential GDP 2010-14						
TSLS	(1)SB	(2)GT	(3)NOPROG	(4)EURO	(5)QUANT	(6)DIR
δ	1.271***	0.758**	1.432**	0.947***	1.172***	
	(0.23)	(0.349)	(0.601)	(0.169)	(0.311)	
η						-1.482***
						(0.501)
const	-3.909***	-4.002**	-4.075**	-4.269***	-4.413***	-3.105***
	(0.717)	(0.835)	(1.18)	(1.025)	(0.889)	(0.923)
N	27	23	24	17	27	27
Adj. R^2	0.5435	0.4638	0.3713	0.5537		0.2228
1st stage F	6.408	9.3754	3.5681	14.467		

Table B2(b): EC forecast, Second stage (DFE)

Endogenous: forecast error of potential GDP 2010-14					
TSLS	(1)DFE	(2)GT	(3)EURO	(4)QUANT	(5)DIR
δ	0.965***	1.143***	1.038***	1.019	
	(0.267)	(0.12)	(0.096)	(0.773)	
η					-0.566
					(0.375)
const	-3.675***	-3.811***	-4.331***	-4.985***	-1.536
	(0.717)	(0.704)	(0.952)	(1.469)	(1.18)
N	27	27	17	27	27
Adj. R^2	0.5435	0.5435	0.5537		0.0922
1st stage F	5.7084	18.827	329.05		

Table B2(c): EC forecast, Second stage (DFE), excl. Latvia, Lithuania

Endogenous: forecast error of potential GDP 2010-14						
TSLS	(1)DFE	(2)GT	(3)NOPROG	(4)EURO	(5)QUANT	(6)DIR
δ	1.129***	1.116***	0.817	1.038***	1.03***	
	(0.106)	(0.111)	(0.623)	(0.096)	(0.167)	
η						-0.952***
						(0.105)
const	-3.981***	-3.974***	-3.51	-4.331***	-4.857***	-1.416
	(0.726)	(0.724)	(1.172)	(0.952)	(0.456)	(1.075)
N	25	25	22	17	25	25
Adj. R^2	0.5165	0.5165	0.3274	0.5537		0.2892

1st stage					
F	225.56	106.89	5.2524	329.05	

Table B3(a): EC forecast, First stage with controls (SB)

	additional control *X*	β	θ	const	n	Adj. R^2
(1)	fe gdp0809	-1.121(0.504)**	-0.078(0.106)	-0.135(1.07)	27	0.401
(2)	forec gdp1011	-1.102(0.351)***	0.462(0.214)**	-0.644(0.817)	27	0.499
(3)	fe sb1011	-1.199(0.369)***	-0.598(0.381)	1.507(0.584)**	27	0.461
(4)	trade part cons	-1.188(0.456)**	0.157(0.588)	0.418(0.546)	26	0.436
(5)	sov'n debt09	-0.919(0.381)**	-0.033(0.014)**	3.101(1.028)***	27	0.501
(6)	sb09	-0.938(0.445)**	0.149(0.102)	1.361(0.62)**	27	0.397
(7)	sov'n cds10q1	-1.106(0.462)**	-0.004(0.005)	1.209(0.604)*	26	0.382
(8)	bankcrisis	-1.255(0.401)***	-1.788(1.024)*	1.351(0.563)**	27	0.463
(9)	private debt 07	-1.477(0.414)***	-0.018(0.008)**	2.445(1.011)**	26	0.504
(10)	ca07	-1.192(0.514)**	-0.011(0.045)	0.576(0.533)	27	0.385

Table B3(b): EC forecast, First stage with controls (DFE)

	additional control *X*	β	θ	const	n	Adj. R^2
(1)	fe gdp0809	-0.704(0.158)***	-0.278(0.093)***	-0.265(1.027)	27	0.530
(2)	forec gdp1011	-0.566(0.258)**	0.051(0.25)	2.023(1.000)*	27	0.325
(3)	fe sb1011	-0.675(0.249)**	0.377(0.539)	1.882(0.621)***	27	0.346
(4)	trade part cons	-0.664(0.217)***	-0.061(0.639)	2.165(0.516)***	26	0.452
(5)	sov'n debt09	-0.446(0.218)*	-0.035(0.015)**	4.451(1.206)***	27	0.455
(6)	sb09	-0.402(0.307)	0.254(0.143)*	2.952(0.697)***	27	0.374
(7)	sov'n cds10q1	-0.772(0.287)**	0.008(0.01)	1.536(0.724)**	26	0.351
(8)	Bankcrisis	-0.579(0.246)**	-0.296(1.001)	2.319(0.74)***	27	0.326
(9)	private debt 07	-0.613(0.248)**	-0.006(0.006)	2.902(1.095)**	26	0.337
(10)	ca07	-0.604(0.267)**	-0.012(0.077)	2.195(0.533)***	27	0.325

Table B3(c): EC forecast, First stage with controls (DFE), excl. Latvia, Lithuania

	additional control *X*	β	θ	const	n	Adj. R^2
(1)	fe gdp0809	-0.844(0.059)***	-0.095(0.114)	1.415(1.101)	25	0.673
(2)	forec gdp1011	-0.781(0.122)***	0.167(0.223)	1.644(0.875)*	25	0.666
(3)	fe sb1011	-0.83(0.088)***	-0.081(0.397)	2.349(0.53)***	25	0.656
(4)	trade part cons	-0.848(0.056)***	-0.248(0.575)	2.327(0.443)***	25	0.657
(5)	sov'n debt09	-0.758(0.077)***	-0.015(0.013)	3.249(1.059)***	25	0.679
(6)	sb09	-0.72(0.083)***	0.154(0.122)	2.711(0.646)***	25	0.674
(7)	sov'n cds10q1	-0.633(0.091)***	-0.012(0.005)**	3.019(0.548)***	24	0.702
(8)	Bankcrisis	-0.839(0.064)***	-0.255(0.761)	2.365(0.547)***	25	0.657
(9)	private debt 07	-0.859(0.06)***	-0.004(0.006)	2.665(0.902)***	24	0.670
(10)	ca07	-0.79(0.085)***	0.049(0.06)	2.359(0.401)***	25	0.672

Table B4(a): EC forecast, Second stage with controls (SB)

	additional control *X*	δ	Π	const	n	Adj. R^2	1st F
(1)	fe gdp0809	1.234(0.227)***	-0.074(0.136)	-4.619(1.878)**	27	0.530	4.52
(2)	forec gdp1011	1.299(0.241)***	-0.234(0.282)	-3.281(1.066)***	27	0.534	6.17
(3)	fe sb1011	1.262(0.213)***	0.188(0.506)	-4.179(1.298)***	27	0.527	5.49
(4)	trade part cons	1.283(0.222)***	-1.551(1.431)	-3.633(0.837)***	26	0.524	3.41
(5)	sov'n debt09	1.43(0.331)***	0.025(0.024)	-5.861(1.795)***	27	0.540	5.46
(6)	sb09	1.19(0.575)**	0.061(0.36)	-3.559(2.379)	27	0.525	3.51
(7)	sov'n cds10q1	1.406(0.302)***	0.007(0.008)	-4.8(1.635)***	26	0.532	2.88
(8)	bankcrisis	1.258(0.218)***	0.307(1.272)	-4.024(1.128)***	27	0.525	5.14
(9)	private debt 07	1.308(0.223)***	0.001(0.008)	-4.525(1.015)***	26	0.642	6.71
(10)	ca07	1.424(0.304)***	-0.074(0.06)	-4.404(0.812)***	27	0.536	3.24

Table B4(b): EC forecast, Second stage with controls (DFE)

	additional control *X*	δ	π	const	n	Adj. R^2	1st F
(1)	fe gdp0809	1.04(0.161)***	-0.104(0.136)	-4.771(1.9)**	27	0.529	13.34
(2)	forec gdp1011	1.021(0.39)***	-0.079(0.347)	-3.499(1.262)***	27	0.531	2.74
(3)	fe sb1011	0.987(0.242)***	0.052(0.574)	-3.768(1.481)**	27	0.526	3.76
(4)	trade part cons	1.019(0.223)***	-1.473(1.442)	-3.488(0.813)***	26	0.523	4.76
(5)	sov'n debt09	0.973(0.425)**	0.001(0.03)	-3.767(2.376)	27	0.526	7.93
(6)	sb09	0.393(0.928)	0.461(0.435)	-1.07(3.067)	27	0.376	6.15
(7)	sov'n cds10q1	1.13(0.336)***	0.004(0.009)	-4.285(1.907)**	26	0.532	4.78
(8)	bankcrisis	0.967(0.256)***	0.032(1.239)	-3.689(1.171)***	27	0.525	2.78
(9)	private debt 07	0.896(0.287)***	0.002(0.007)	-4.331(0.953)***	26	0.642	3.27
(10)	ca07	1.056(0.263)***	-0.038(0.067)	-3.937(0.695)***	27	0.535	2.88

Table B4(c): EC forecast, Second stage with controls (DFE), excl. Latvia, Lithuania

	additional control *X*	δ	π	const	n	Adj. R^2	1st F
(1)	fe gdp0809	1.129(0.107)***	-0.017(0.192)	-4.133(2.26)*	25	0.495	103.33
(2)	forec gdp1011	1.14(0.243)***	-0.026(0.381)	-3.91(1.516)***	25	0.495	141.55
(3)	fe sb1011	1.072(0.157)***	-0.278(0.7)	-3.593(1.512)**	25	0.495	110.08
(4)	trade part cons	1.158(0.102)***	-1.558(1.395)	-3.709(0.802)***	25	0.519	114.00
(5)	sov'n debt09	1.236(0.218)***	0.016(0.024)	-5.259(1.761)***	25	0.509	116.29
(6)	sb09	0.87(0.231)***	0.272(0.248)	-2.617(1.525)*	25	0.493	121.91
(7)	sov'n cds10q1	1.035(0.486)**	-0.005(0.024)	-3.401(3.199)	24	0.481	120.53
(8)	bankcrisis	1.126(0.104)***	-0.122(1.308)	-3.931(1.237)***	25	0.495	127.82
(9)	private debt 07	1.09(0.101)***	0.004(0.007)	-4.888(0.943)***	24	0.625	107.41
(10)	ca07	1.153(0.146)***	-0.018(0.072)	-4.068(0.688)***	25	0.497	114.63

Table B5: Moving Window – First and Second Stage (IMF Data)

First Stage: Endogenous: forecast error of GDP t:t+1					
		SB		DFE	
OLS	2009	2010	2011	2010	2011
B	-0.792	-1.342**	-0.711	-0.861***	-0.737***
	(0.549)	(0.530)	(0.630)	(0.055)	(0.219)
Const	0.918	1.153***	-1.892**	2.750***	-0.696
	(0.974)	(0.402)	(0.734)	(0.489)	(0.914)
N	22	22	22	21	21
Adj. R^2	0.0605	0.476	0.0777	0.698	0.445
Second Stage: Endogenous: forecast error of potential GDP t:t+5					
TSLS					
Δ	1.641*	1.008***	1.465***	1.236***	1.169***
	(0.958)	(0.382)	(0.504)	(0.068)	(0.216)
Const	-6.304***	-3.522***	-0.129	-3.914***	-1.187
	(1.658)	(0.827)	(1.606)	(0.709)	(1.194)
N	22	22	22	21	21
Adj. R^2	0.734	0.538	0.680	0.663	0.567
1st stage F	2.081	6.403	1.272	244.4	11.32

Table B6(a): Panels First Stage (IMF Data)

Endogenous: forecast error of GDP t:t+1					
			SB	DFE	
Panel – OLS	09/11	10/11	09/11 All controls	10/11	10/11 All controls
β	-0.988***	-1.063**	-1.351***	-0.800***	-0.813***
	(0.356)	(0.427)	(0.459)	(0.109)	(0.255)
initial sov'n debt			-0.023		0.013
			(0.014)		(0.017)
fe gdp08			0.485***		0.529***
			(0.094)		(0.157)
fe sb			-0.507**		0.032
			(0.241)		(0.259)
init sb			-0.674***		-0.418*
			(0.194)		(0.209)
sov'n cds10q1			0.000		0.001
			(0.003)		(0.004)
Bankcrisis			0.234		-0.611
			(0.590)		(0.948)
forec gdp			-0.395**		-0.360*
			(0.177)		(0.183)
p gdp forec			0.519***		0.456
			(0.141)		(0.316)
trade part cons			0.635		1.127
			(0.971)		(0.685)
ca gdp			0.130**		0.154

			(0.057)		(0.115)
private debt 07			0.000		0.004
			(0.005)		(0.006)
Const	0.761	0.997**	0.902	2.617***	0.920
	(0.806)	(0.431)	(1.776)	(0.496)	(3.323)
N	66	44	63	42	36
Adj. R^2	0.402	0.431	0.643	0.660	0.806

Table B6(b): Panels Second Stage (IMF Data)

Endogenous: forecast error of potential GDP t:t+5

	SB			DFE	
TSLS	09/11	10/11	09/11 All controls	10/11	10/11 All controls
δ	1.253***	1.143***	1.447***	1.205***	1.875***
	(0.309)	(0.280)	(0.224)	(0.106)	(0.234)
initial sov'n debt			-0.000		0.002
			(0.016)		(0.016)
fe gdp08			0.090		-0.372**
			(0.132)		(0.156)
fe sb			0.672***		0.209
			(0.205)		(0.364)
init sb			-0.436***		-0.426**
			(0.133)		(0.175)
sov'n cds10q1			0.002		0.003
			(0.003)		(0.003)
bankcrisis			-0.124		0.697
			(0.576)		(0.810)
forec gdp			0.108		-0.072
			(0.224)		(0.186)
p gdp forec			-0.115		-0.286
			(0.141)		(0.197)
trade part cons			1.511*		0.875
			(0.876)		(1.112)
ca gdp			0.147**		0.046
			(0.074)		(0.133)
private debt 07			0.022***		0.008
			(0.005)		(0.005)
const	-5.702***	-3.576***	-7.731***	-3.886***	-9.549***
	(0.981)	(0.783)	(1.538)	(0.736)	(1.886)
n	66	44	63	42	36
Adj. R^2	0.644	0.611	0.839	0.626	0.800
1st stage F	7.705	6.198	8.656	53.94	10.20

Table C1(a): Panel First Stage (SB)

Endogenous: forecast error of GDP t:t+1				
Panel OLS	10/11	10/12	10/13	10/14
β	-0.574	-0.547**	-0.419*	-0.341
	(0.349)	(0.269)	(0.249)	(0.233)
const	0.702	0.705	0.719	0.728
	(0.544)	(0.541)	(0.560)	(0.573)
n	54	81	108	135
Adj. R^2	0.283	0.249	0.248	0.223

Table C1(b): Panel First Stage (DFE)

Endogenous: forecast error of GDP t:t+1				
Panel OLS	10/11	10/12	10/13	10/14
β	-0.596***	-0.577***	-0.456***	-0.419***
	(0.174)	(0.132)	(0.143)	(0.145)
const	2.240***	2.192***	1.894***	1.803***
	(0.516)	(0.481)	(0.494)	(0.495)
N	54	81	108	135
Adj. R^2	0.476	0.468	0.380	0.326

Table C2(a): Panel Second Stage (SB)

Endogenous: forecast error of potential GDP t:t+4				
Panel TSLS	10/11	10/12	10/13	10/14
δ	1.345***	1.695***	1.744***	1.766***
	(0.363)	(0.444)	(0.565)	(0.637)
const	-3.966***	-4.235***	-4.273***	-4.290***
	(0.735)	(0.864)	(0.934)	(0.977)
n	54	81	108	135
Adj. R^2	0.540	0.503	0.582	0.608
1st stage F	2.705	4.125	2.841	2.142

Table C2(b): Panel Second Stage (DFE)

Endogenous: forecast error of potential GDP t:t+4				
Panel TSLS	10/11	10/12	10/13	10/14
δ	0.973***	1.086***	1.101***	1.152***
	(0.162)	(0.139)	(0.175)	(0.189)
const	-3.681***	-3.768***	-3.779***	-3.818***
	(0.689)	(0.674)	(0.676)	(0.674)
n	54	81	108	135
Adj. R^2	0.519	0.531	0.586	0.618
1st stage F	11.72	19.19	10.13	8.396

Table C3: Further Panels First and Second Stage (SB)

First Stage: Endogenous: forecast error of GDP t:t+1					
OLS	09/10	09/11	09/12	09/13	09/14
β	-0.631**	-0.470**	-0.471**	-0.401**	-0.354**
	(0.253)	(0.214)	(0.188)	(0.179)	(0.173)
const	-0.228	0.007	0.007	0.109	0.178
	(0.741)	(0.719)	(0.700)	(0.697)	(0.696)
N	54	81	108	135	162
Adj. R^2	0.155	0.252	0.242	0.228	0.205
Second Stage: Endogenous: forecast error of potential GDP t:t+4					
TSLS	09/10	09/11	09/12	09/13	09/14
δ	1.158***	1.198***	1.445***	1.452***	1.452***
	(0.359)	(0.397)	(0.383)	(0.437)	(0.467)
const	-4.721***	-4.749***	-4.922***	-4.926***	-4.926***
	(0.845)	(0.848)	(0.805)	(0.824)	(0.835)
n	54	81	108	135	162
Adj. R^2	0.573	0.567	0.568	0.624	0.649
1st stage F	6.190	4.819	6.269	5.013	4.211

Table C4: Panel First Stage including all controls simultaneously (SB and DFE)

Endogenous: forecast error of GDP t:t+1				
	SB		DFE	
Panel OLS	10/11	10/12	10/11	10/12
β	-0.795	-0.800*	-0.877***	-0.734***
	(0.557)	(0.404)	(0.263)	(0.232)
initial sov'n debt	-0.025	-0.018*	-0.015	-0.004
	(0.016)	(0.009)	(0.014)	(0.009)
fe gdp08	-0.117	-0.111	-0.214*	-0.185**
	(0.125)	(0.080)	(0.106)	(0.074)
fe sb	-1.177***	-1.083***	-0.487*	-0.417**
	(0.294)	(0.232)	(0.250)	(0.176)
init sb	-0.257	-0.313**	-0.223*	-0.259**
	(0.199)	(0.140)	(0.131)	(0.105)
sov'n cds10q1	0.004	0.001**	0.007*	0.001***
	(0.004)	(0.000)	(0.004)	(0.000)
bankcrisis	0.488	0.957	-0.316	0.339
	(1.052)	(0.699)	(0.929)	(0.644)
forec gdp	0.253	0.161	0.010	-0.062
	(0.311)	(0.191)	(0.304)	(0.205)
p gdp forec	-0.039	0.037	-0.201*	-0.057
	(0.123)	(0.101)	(0.115)	(0.101)
trade part cons	-0.264		-0.313	
	(0.853)		(0.848)	
ca gdp	0.085	0.060	0.102*	0.044
	(0.067)	(0.039)	(0.060)	(0.035)

private debt 07	-0.004	-0.005	-0.003	-0.002
	(0.007)	(0.005)	(0.006)	(0.004)
const	1.544	0.434	3.076	0.917
	(2.515)	(1.583)	(2.333)	(1.636)
n	48	73	48	73
Adj. R^2	0.568	0.556	0.631	0.607

Table C5: Panel Second Stage including all controls simultaneously (SB and DFE)

Endogenous: forecast error of potential GDP t:t+4				
	SB		DFE	
Panel TSLS	10/11	10/12	10/11	10/12
δ	0.152	0.908*	0.739**	1.134***
	(0.728)	(0.488)	(0.345)	(0.367)
initial sov'n debt	0.009	0.031*	0.019	0.034**
	(0.022)	(0.018)	(0.019)	(0.017)
fe gdp08	-0.354*	-0.270***	-0.262***	-0.237**
	(0.194)	(0.102)	(0.092)	(0.092)
fe sb	-1.066	0.196	-0.538	0.364
	(0.733)	(0.459)	(0.393)	(0.377)
init sb	0.016	-0.211	0.001	-0.203
	(0.131)	(0.145)	(0.106)	(0.144)
sov'n cds10q1	0.015***	-0.000	0.013***	-0.001*
	(0.005)	(0.000)	(0.004)	(0.000)
bankcrisis	1.767	0.436	1.421	0.164
	(1.324)	(1.139)	(0.917)	(1.067)
forec gdp	0.687**	0.148	0.464*	0.081
	(0.348)	(0.213)	(0.241)	(0.183)
p gdp forec	0.253	0.302**	0.269**	0.286**
	(0.172)	(0.142)	(0.123)	(0.141)
trade part cons	1.512		1.157	
	(1.136)		(1.158)	
ca gdp	0.198*	0.098	0.137	0.082
	(0.119)	(0.067)	(0.089)	(0.068)
private debt 07	0.007	0.008	0.006	0.008
	(0.008)	(0.006)	(0.007)	(0.006)
Const	-13.049***	-14.128***	-13.306***	-13.992***
	(2.623)	(2.957)	(2.111)	(2.956)
N	48	73	48	73
Adj. R^2	0.457	0.591	0.685	0.615
1st stage F	2.035	3.927	11.16	10.05