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Abstract: This paper provides a comprehensive empirical assessment of the relation between the cyclicality of fiscal expenditure policy, output volatility, and economic growth, using a large cross-section of 88 countries over the period 1960 to 2004. Identification of the effects of (endogenous) cyclical expenditure policy is achieved by exploiting the exogeneity of countries' political and institutional characteristics, which we find to be relevant determinants of the cyclicality of expenditures. There are three main results: First, both pro- and countercyclical expenditure policy amplify output volatility, much in a way like pure fiscal shocks that are unrelated to the cycle. Second, output volatility, due to variations in cyclical and discretionary fiscal policy, is negatively associated with economic growth. Third, there is no direct effect of cyclicality on economic growth other than through output volatility. These findings advocate the introduction of fiscal rules that limit the use of (discretionary and) cyclical fiscal (expenditure) policy to improve growth performance by reducing volatility.

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I. Introduction

Does fiscal policy affect economic growth? This is clearly one of the most fundamental and policy relevant macroeconomic questions. Easterly (2005) argues that there is no robust evidence for a relation between macroeconomic policies (including fiscal policy) and economic growth, once institutions are controlled for. A similar point is made by Acemoglu et al. (2003). Yet this is not the predominant view. As Caballero (2008, p. 1) states more representatively: "Good macroeconomic policy helps growth ... I do not think this view is in any dispute in the applied and policy world."

Notwithstanding the wide agreement that macroeconomic policies can influence economic performance, it remains a challenge for both theory and empirics to identify the channels through which economic policy affects growth. The emergence of new endogenous growth theory, overcoming the traditional dichotomy between business cycle theory on the one hand and growth theory on the other hand, has laid the ground for such an analysis. It is hardly questioned that economic policy affects economic activity in the short run. But then, if business cycle volatility and economic growth are related as suggested by endogenous growth theory, economic policy can indirectly affect growth through its effect on the (volatility of the) business cycle. Such a finding would also lead to a reassessment of macroeconomic priorities: The welfare cost of volatility *per se* are widely regarded as negligible since Lucas (1987). But if volatility turns out to have a negative effect on economic growth, its costs – or equivalently, the gains from stabilization – will be substantial (Barlevy, 2004).

Regarding the role of fiscal policy, Fatas and Mihov (2003) suggest introducing fiscal rules as a means to reduce the use of discretionary fiscal policy, defined as fiscal policy unrelated to the business cycle, based on their finding for a large cross-section of 91 countries that aggressive use of discretionary policy lowers growth by increasing output volatility.

This paper highlights the role of another important element of fiscal policy, namely cyclical fiscal policy. So far, there are hardly studies investigating the effects of fiscal cyclicality on economic growth. One notable exception is Aghion and Marinescu (2008), who consider an (unbalanced) panel of annual data for 19 OECD countries from 1960 to 2007. Regressing growth on alternative cyclicality measures (and standard controls for economic growth regressions), they find a positive effect of the 'countercyclicality' of fiscal policy on economic growth. The policy relevance of such a finding is obvious. In fact, Aghion and Howitt (2006) argue that the lower degree of countercyclicality in European Monetary Union (EMU) countries is one of the reasons for their poor growth performance relative to the UK or the USA in the 1990s.

The main goal of the present paper is to shed more light on the role of cyclical fiscal policy and its transmission channels, considering both its effect on output volatility, and – in a second step – its effect on economic growth. We also test whether cyclicality affects growth 'directly' apart from its indirect effect through output volatility. Other than previous studies we consider a large cross-section of 88 countries covering the period 1960 to 2004, which is motivated by the use of (de facto) time-invariant variables on the countries' political and institutional characteristics to identify the causal effect of (endogenous) cyclicality on output volatility.

There is growing evidence that economic policy is shaped to a considerable extent by the characteristics of political and electoral systems (Person and Tabellini, 2000). The use of institutional variables as instruments for fiscal policy was first suggested by Fatas and Mihov (2003) in their study of the effects of discretionary fiscal policy, defined as fiscal policy unrelated to the business cycle, on volatility and economic growth. The present study is closely related to their approach but goes beyond the previous literature by considering the role of cyclical fiscal policy (as well as discretionary fiscal policy). We demonstrate that institutional variables (such as political of constraints and the average number of elections) provide considerable information on the variation in fiscal cyclicality across countries, and we use this exogenous variation to identify the causal effect of cyclicality on output volatility and economic growth. We also suggest a new instrument for output volatility, which is based on volatility spillovers from other countries and thus entirely unrelated to a particular country's institutions or policies, in order to explore the robustness of the link between volatility and economic growth.

We find that cyclical fiscal (expenditure) policy has a destabilizing effect on the economy, no matter whether it is pro- or countercyclical. In fact, it amplifies output volatility much the same way as discretionary fiscal policy. This adds to the widespread scepticism against the usefulness of fiscal policy as a fine-tuning instrument. We also find that output volatility, induced by variations in cyclical or fiscal policy, negatively affects economic growth. Taken together this has an important policy implication: Economic growth could be enhanced by introducing fiscal rules, designed to restrict both the use of discretionary fiscal policy (Fatas and Mihov, 2003) as well as the use of cyclical fiscal policy.

The remainder of the paper is organized as follows. Section II constructs measures of fiscal cyclicality and the aggressiveness of discretionary fiscal policy for a large cross-section of 88 countries. Section III motivates the identification strategy and provides evidence on the relation between cyclical (and discretionary) fiscal policy and output volatility. Section IV considers the effect of cyclicality on economic growth. Section V summarizes the results and concludes.

II. Constructing Measures of Cyclical and Discretionary Fiscal Policy

Changes in a government's fiscal stance can be decomposed into three components (Gali and Perotti, 2003): i) automatic fiscal responses under the set of existing fiscal rules and institutions, i.e., that part of fiscal policy driven by forces which are largely outside the control of fiscal authorities (at least in the short-run); ii) fiscal policy in response to the business cycle (henceforth 'cyclical fiscal policy'); and iii) fiscal policy unrelated to the business cycle (henceforth 'discretionary fiscal policy'). Although cyclical policy (ii) is also part of discretionary fiscal policy in a broader sense, we reserve the term 'discretionary' exclusively to denote policy unrelated to the cycle throughout this paper.

We use government consumption as indicator of fiscal policy. This choice is dictated by data availability, since there are no internationally comparable data for other measures of fiscal policy for our large cross-section of countries. On the one hand, this limits the generality of our results. On the other hand, an advantage is that government expenditures – compared with revenues – are less responsive to the cycle through stabilizers 'built-in' the fiscal system and can be changed with relative ease. As a consequence, expenditures are more indicative of a government's intentional cyclical policy than revenues, whose cyclical behaviour is driven by automatic stabilizers to a much larger extent. And while government consumption is only a subset of total expenditures, results of previous studies suggest that the cyclicality of government consumption reflects the cyclicality of overall government expenditures reasonably well.[1]

We follow the standard approach in the literature and estimate cyclicality parameters (χ) by regressing growth of real government consumption (G) on the growth of real GDP (Y), correcting for serial correlation in the error term:

$$\Delta \ln G_{i,t} = \alpha_i + \chi_i \Delta \ln Y_{i,t} + \eta_{i,t}\,, \tag{1a}$$

$$\eta_{i,t} = \rho_i \eta_{i,t-1} + \varepsilon_{i,t}\,. \tag{1b}$$

Equation (1) is estimated separately for each of the i = 1, …, 88 countries, which is the largest set of countries for which the key variables required in the present study are available. The time dimension t ranges from 1960 to 2004; for some countries, a slightly shorter time span had to be used. Appendix A1 provides a detailed description of the sample and data used.

Equation (1) should be regarded as reduced form equation for government consumption. As Lane (2003) points out there is no reason to control for simultaneous feedback from government spending to GDP. Hence, we estimate equation (1) by ordinary least squares. As

[1] In Lane (2003), for example, who studies the determinants of cyclical fiscal policy using a sample of 22 OECD countries, the correlation between the cyclicality of government consumption and that of total government expenditures is 0.71.

a result we obtain a decomposition of the growth of government consumption into a cyclical and a discretionary component. The time series of country *i*'s cyclical fiscal policy is given by $\hat{\chi}_i \Delta \ln Y_{i,t}$; and the estimate of the (structural) residual of equation (1), i.e., $\hat{\varepsilon}_{i,t}$, is interpreted as series of discretionary fiscal policy shocks. By least squares algebra these two series are orthogonal.

As will be outlined more in detail below, the approach pursued here is to identify the effects of cyclical fiscal policy on volatility and growth using the cross-country variation in the data and exploiting the exogeneity of (de facto) time-invariant measures of political and institutional characteristics. Hence, we require country-specific indicators of the average cyclicality of fiscal policy over the time period considered. That is exactly what the estimates of the parameter χ_i measure, a positive (negative) value being associated with procyclical (countercyclical) fiscal behaviour. Our results indicate substantial cross-country variation in the cyclicality parameters, whose estimates range from –0.835 to 2.698. Most of the countries show procyclical fiscal expenditure policy; only 11 of the 88 coefficients are negative. The country-specific cyclicality coefficients $\hat{\chi}_{i,t}$ are reported in Appendix A1.

Regarding discretionary fiscal policy, Fatas and Mihov (2003) interpret the volatility of the error term ($\varepsilon_{i,t}$) in an equation similar to (1) over a certain time period as an indicator of a government's aggressiveness of discretionary fiscal policy. In line with this reasoning, we define our empirical measure of discretionary fiscal policy (*DISCR*) as standard deviation of the residuals from equation (1), i.e., $DISCR_i = \text{sd}(\hat{\varepsilon}_i)$. The country-specific estimates of *DISCR* are reported in Appendix A1.

Regarding the relevance of cyclical versus discretionary fiscal policy, the R^2 in equation (1), averaged over all 88 countries, amounts to 0.201. This means that roughly one-fifth of the total variation in fiscal policy is due to cyclical fiscal policy. This is a non-negligible portion, bearing in mind that the overall variation in government consumption will also partly reflect measurement errors.

Our estimates are well in line with previous studies. Comparing our cyclicality coefficients $\hat{\chi}_i$ (for period 1960 to 2004) with those of Lane (2003) for 22 OECD countries (for 1960 to 1998), the correlation is 0.872. Our measure of discretionary fiscal policy is very close to that of Fatas and Mihov (for 1960 to 2000): The correlation of their levels (logs) is 0.887 (0.945). We conclude that the simple approach given by equation (1) yields reliable estimates of the cross-country variation in discretionary and cyclical fiscal policy, which are comparable with previous studies of fiscal policy.

III. Cyclicality of Fiscal Policy and Output Volatility

1. The Empirical Model

Our basic empirical framework is closely related to that of Fatas and Mihov (2003); the novel feature is that the cyclicality of fiscal policy is included as explanatory variable for output volatility:

$$\ln \sigma_i^y = \gamma_0 + \gamma_1 \ln CYC_i + \mathbf{x}_i \boldsymbol{\gamma} + u_i \, . \tag{2a}$$

The dependent variable is output volatility (σ^y), defined as standard deviation of the growth rate of (real) output per capita; *CYC* is our measure of the cyclicality of fiscal policy, which we construct from the estimates of equation (1) as will be outlined more in detail below; $\mathbf{x}_i$ is a vector of control variables, and u is a stochastic error term. The cross-section dimension (i) comprises 88 countries, the largest sample for which the required data are available. Unless mentioned otherwise, all data are averages over the period 1960 to 2004. (See Appendix A1 for a more detailed description of the sample and the data.)

As it is standard in skedastic regressions, we choose a logarithmic specification to avoid negative predicted values for the standard deviation of output growth. It is then natural to use the cyclicality measure in log form as well, such that the parameter of our main interest (γ_1) measures the relative change of output volatility with respect to relative changes in cyclicality. While the logarithmic specification yields a slightly better fit, it is not crucial for the results, as we show in the sensitivity analysis below.

We define cyclicality (*CYC*) as absolute value of $\hat{\chi}$ to allow for negative values of the cyclicality coefficients $\hat{\chi}_i$ in the logarithmic specification (2a). Obviously, the variable *CYC* $= |\hat{\chi}|$ then measures only the responsiveness of fiscal policy, but not its direction, i.e., whether it is pro- or countercyclical. This could be addressed by properly signing $\ln CYC$ for the respective observations. But this would impose the symmetry assumption that – if procyclical policy amplifies business cycles – countercyclical policy smoothes business cycles. This is an assumption we wish to test rather than impose right from the beginning in light of the widespread scepticism against the effectiveness of fiscal policy as fine-tuning instrument. Countercyclical fiscal policy might actually turn out destabilizing due to lags in (recognition, implementation, and) materialization, a point prominently made by Friedman (1953).

Consequently, we do not impose any assumption about the relation between the effects of pro- and countercyclical policy right from the beginning. Instead, we define CYC_i as absolute value of $\hat{\chi}_i$ ($CYC_i = |\hat{\chi}_i|$) and allow for different parameters of $\ln CYC_i$, depending on whether $\hat{\chi}_i$ is positive or negative for the respective observation i:

$$\ln \sigma_i^y = \gamma_0 + \gamma_1 \ln CYC_i + \gamma_1^{counter} D_i^{counter} \ln CYC_i + \mathbf{x}_i \boldsymbol{\gamma} + u_i , \quad (2b)$$

where $D^{counter}$ is a dummy variable taking a value of 1 for 'countercyclical observations', i.e., $D_i^{counter} = 1$ for all i where $\hat{\chi}_i < 0$ and 0 otherwise. The parameter $\gamma_1^{counter}$ then measures the difference between the effect of countercyclical fiscal policy and the effect of procyclical fiscal policy on output volatility (γ_1).

We start from a simple regression of output volatility on cyclicality (*CYC*) and then add three further explanatory variables: Government size (*GSIZE*) is included to account for the potentially stabilizing role of larger governments (Gali, 1994). Openness (*OPEN*), measured as imports plus exports as a share of GDP, is a standard explanatory control for output volatility and fiscal policy according to Rodrik (1998). Finally, the level of development, measured as (log of) real GDP per capita (*GDPPC*) accounts for the fact that poor countries typically have more volatile business cycles and controls for the quality of institutions and economic policy. Hence, the vector $\mathbf{x}_i = [GSIZE_i, OPEN_i, \ln GDPPC_i]$. Regarding government size, it has been argued that more volatile economies may have an incentive to set up larger governments as a means to reduce macroeconomic volatility (Rodrik, 1998). As a consequence, *GSIZE* might be endogenous in equation (2). In line with Fatas and Mihov (2003) we use the standard approach and instrument *GSIZE* by the (log of) population (*POP*), the urbanization rate (*URBAN*), and the dependency ratio (*DEP*).

In a final step, we will include the aggressiveness of discretionary fiscal policy as defined in section II (*DISCR*), yielding our most comprehensive model:

$$\ln \sigma_i^y = \gamma_0 + \gamma_1 \ln CYC_i + \gamma_2 \ln DISCR_i + \mathbf{x}_i \boldsymbol{\gamma} + u_i . \quad (3)$$

Before turning to the estimation results for models (2) and (3), two issues warrant discussion. First, our variable of main interest, the cyclicality of fiscal policy (*CYC*), is endogenous with respect to output volatility as a result of reverse causality. From a theoretical perspective, Talvi and Vegh (2005) show in a political economy model that lobbying for higher public spending during a boom generates a procyclical bias in fiscal policy. A feature of this model is that the larger the incipient primary surplus (the larger the boom) the higher the spending pressures and the resulting political distortions. As a consequence high output volatility tends to generate procyclical fiscal behaviour.[2] This would introduce an upward bias in the estimated effect of cyclicality on output volatility. In addition, it should be borne in mind that the cyclicality measure (*CYC*) is an estimate of its true value. As a result it might be subject to classical measurement error, causing an attenuation bias.

[2] Talvi and Vegh (2005) also provide evidence from a large cross-section of countries that the degree of procyclicality in government consumption is positively correlated with output volatility. Lane (2003) obtains a similar results for a sample of 22 OECD countries.

Another issue, related to the fact that our country-specific cyclicality measures (χ_i) are generated by model (1), is that the observations on our variable CYC_i are estimated with different precision. This is addressed by using a weighted (two stages) least squares procedure, using the inverse of the variance of $\hat{\chi}_i$ as weights.[3] This implies that observations, for which the variable *CYC* is measured more precisely, are assigned a higher weight in the regression. As we show below the weighting improves the fit but it is not crucial for the results.

2. Identification, First Stage Regressions, and Instrument Quality: Political and Institutional Characteristics as Determinants of Cyclicality

In order to identify the causal effect of cyclicality on output volatility, we use variables on political and institutional characteristics as instruments. This choice is motivated by the growing theoretical literature and empirical evidence that economic policy is as shaped to a considerable extent by the characteristics of political and electoral systems (Person and Tabellini, 2000). At the same time, institutions can be reasonably assumed to be exogenous with respect to output volatility. Fatas and Mihov (2003) were the first to suggest using institutional variables as instruments for (discretionary) fiscal policy.

We hypothesize and demonstrate that countries' political and institutional characteristics are not only relevant determinants of discretionary fiscal policy but also of cyclical fiscal policy. In particular, we consider four institutional variables: i) the average number of elections (*NELEC*) ii) a measure of political constraints (*POLCON*) by Henisz (2000), which captures the extent to which the executive faces political constraints to policy implementation;[4] iii) a dummy for majoritarian systems (*MAJ*), and iv) a dummy for presidential regimes (*PRES*).

Notice that the variable *CYC*, defined as absolute value of cyclicality ($CYC = |\hat{\chi}|$), in the first place measures the aggressiveness (but not the direction) of cyclical fiscal policy. As a consequence, part of the discussion by Fatas and Mihov (2003) motivating the use of the institutional variables (*NELEC*, *POLCON*, *MAJ*, *PRES*) as instruments for the aggressiveness of discretionary policy directly carries over to our measure of cyclical fiscal policy (*CYC*).

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[3] The choice of the weight is not affected by the logarithmic transformation. By the delta method, $\sigma^2_{\ln CYC} = 1/\mu_{CYC}\sigma^2_{CYC}$, i.e. the variances of the level and log of *CYC* are equal up to a rescaling by the mean of *CYC* (μ_{CYC}).

[4] This (0,1)-index counts the number of veto points in the political system and the distribution of preferences across and within the different branches of the government. Power is more dispersed, the greater the number of veto points and the greater the division of control across different political parties (see Henisz (2000) for more details).

The extent of political constraints (*POLCON*) is the instrument with the strongest theoretical motivation. According to the 'voracity hypothesis' (Tornell and Lane, 1998), power diffusion among more agents induces procyclicality, since fiscal competition by multiple power groups for fiscal revenues increases (decreases) in booms (recessions). On the other hand, governments less constrained in implementing their policy can respond more flexibly to the business cycle and will thus be better able to translate their 'cyclicality preferences' into actual policy. What we observe is only the net outcome; this bottom line effect of *POLCON* is ambiguous and remains to be determined empirically.

Regarding the electoral characteristics, there is a tradeoff between election-related and cyclical fiscal policy. The frequency and timing of elections (*NELEC*) and the induced electoral cycles will not be systematically related to the business cycle in general. As a consequence, the observed pattern of fiscal policy will show a smaller association with the (business) cycle, the larger the number of elections, i.e., the more the relation of fiscal policy to the business cycle is diluted by fiscal policy measures associated with the electoral cycle. A similar point can be made for *MAJ* in light of the argument by Persson and Tabellini (2001) that majoritarian systems will have more pronounced electoral cycles.

Regarding the dummy for presidential regimes (*PRES*), it is less clear whether one would expect a relationship with cyclicality. It could be the case that presidential regimes will not only be associated with a more aggressive use of discretionary fiscal policy as argued by Fatas and Mihov (2003), but also with a more active conduct of cyclical fiscal policy.

While our choice of institutional variables as instruments for cyclicality is well motivated theoretically, the ultimate question is whether the variables *NELEC*, *POLCON*, *MAJ*, and *PRES* are also relevant instruments in our empirical model, i.e., whether they are informative about the variation in fiscal cyclicality in our sample of countries. Table 1 reports the results of a regression of the log of *CYC* on the four institutional variables separately (columns (1a) to (1d)) and simultaneously (column (2a)). The number of elections (*NELEC*) and political constraints (*POLCON*) turn out to have the strongest effect; they are significant both in a simple regression (columns (1a) and (1b)) of Table 1 and in a multiple regression on all four political variables (column (2a)). The sign of the coefficient of *NELEC* is negative as expected. The variable *POLCON* also enters with a negative sign; this does not necessarily reject the voracity hypothesis but suggests that – among the various ways through which political constraints affect the cyclicality of fiscal (expenditure) policy – the voracity effect does not appear to be the most dominant force.[5] The variables *MAJ* and *PRES* are insignificant or only weakly significantly in a simple regression (columns (1c) and (1d)); in a multiple regression on all four institutional variables, both *MAJ* and *PRES* turn out

[5] Lane (2003) also finds little support for the voracity hypothesis in his study of the cyclicality of expenditures in a sample of 22 OECD countries; in particular, the effect of political constraints on cyclicality is often insignificant or shows the wrong sign.

insignificant with p-values of 0.854 and 0.789. Since the variables *MAJ* and *PRES* are uninformative about fiscal cyclicality, their inclusion in the first stage regression would only weaken the quality of our set of instruments (compare the F-statistic in columns (2a) and (2b)). Consequently, we will use only *NELEC* and *POLCON* as instruments for *CYC* in the two stages least squares regressions below.

< Table 1 >

Notice that column (2b) corresponds to the first stage regression for equation (2a) only in the most parsimonious specification without additional explanatory variables for output volatility. In the extended models, a more relevant issue is the partial correlation of *NELEC* and *POLCON* with *CYC*, controlling for the other explanatory variables (*GSIZE*, *OPEN*, and *GDPPC*). The first stage regression for the most comprehensive model including all controls – *GSIZE* (instrumented by *POP*, *URBAN*, and *DEP*), *OPEN*, and *GDPPC* – is given in column (3). An important result is that the variables *NELEC* and *POLCON* remain significant, both individually and jointly.

Column (4) shows the corresponding first stage regression for discretionary fiscal policy (*DISCR*). Results are in line with Fatas and Mihov (2003). Notice that – in contrast to the first stage regression for *CYC* – the two variables *MAJ* and *PRES* turn out significant at the five and one percent level. At least from an empirical perspective, this suggest that the variation in the variables *MAJ* and *PRES* can help to identify the (separate) effect of discretionary policy in model (3), where both *CYC* and *DISCR* are included simultaneously.

Overall, the results reveal interesting links between institutions and cyclicality. Exploring these links more in detail is beyond the scope of this paper. For the purpose of the present study, the most relevant message from the results in Table 1 is that the two variables *NELEC* and *POLCON* are relevant instruments for cyclicality (*CYC*); this is not the case for the variables *MAJ* and *PRES*, which are, however, strongly associated with the aggressiveness of discretionary fiscal policy (*DISCR*).

3. Estimation Results

We start from the most parsimonious specification of model (2), which includes only cyclicality (*CYC*) as explanatory variable.[6] Columns (1a) and (1b) show the weighted least squares estimates (WLS) of equation (2a), which allows the effects of pro- and countercyclical fiscal policy to differ by including an interaction between *CYC* and a dummy for countercyclical policy ($D^{counter}$). The estimated elasticity of output volatility with respect to procyclical fiscal policy is 0.184; the effect of countercyclical policy appears to be even larger

[6] The weighting accounts for the fact that *CYC* is a generated regressor, not for the presence of heteroskedasticity in the error term in models (2) or (3) (which is also confirmed by standard tests). Hence, we use robust standard errors for inference throughout.

(0.278), but the difference is insignificant with a p-value of 0.300. This conclusion holds up when the model is estimated by weighted two stages least squares (WTSLS), using the average number of elections (*NELEC*) and the index of political constraints (*POLCON*) as instruments for *CYC* (column (1b)). In that case the elasticities with respect to pro- and countercyclical fiscal policy are 0.595 and 0.501 respectively, but again the difference is insignificant (p-value: 0.618).[7]

< Table 2 >

In light of this result it appears to be justified to proceed with a restricted model, imposing parameter equality for pro- and countercyclical fiscal policy. Columns (2a) and (2b) show the WLS and WTSLS estimates of the simple regression of volatility on cyclicality. For comparison, columns (3a) and (3b) show the results of the unweighted LS and TSLS estimates. Notice first, that endogeneity of *CYC* is indeed pronounced: The difference between the (W)LS and (W)TSLS coefficients is sizeable. More formally, a Hausman test rejects that *CYC* is exogenous at the one percent level in all specifications. It is interesting to note that both the weighted and unweighted LS estimates of the effect of *CYC* on volatility show a strong attenuation bias towards zero. This suggests that measurement error is the dominant source of endogeneity rather than reverse causality (causing an upward bias). This view is also supported by a comparison of the weighted and unweighted estimates. In the weighted regressions, less precise estimates are assigned a lower weight, rendering the role of measurement error less relevant. As a consequence, the attenuation bias is less pronounced in the WLS regression, yielding coefficients that are closer to the WTSLS estimates.

While we postpone a more comprehensive sensitivity analysis to below, we emphasize that the weighting is not crucial for the results: A comparison of columns (2b) and (3b) shows that the weighted and unweighted TSLS estimates are virtually identical, pointing to an elasticity of output volatility with respect to cyclicality of around 0.6. The choice of the logarithmic form of *CYC* is not essential for the qualitative conclusions as well: The corresponding results for the specification in levels (columns (4a) and (4b)) are in line with the logarithmic specification (columns (1b) and (2b)).

A final observation is that the tests for overidentifying restrictions reject the null hypothesis of valid instruments in some specifications. This is not too surprising, given that several important variables have been omitted from the regression so far. Results for a more comprehensive model, including *GSIZE*, *OPEN* and *GDPPC* as controls are given in Table 3. As already discussed above, government size is likely to be endogenous with respect to

[7] We add that in a specification with two variables included, one for procyclical policy ($(1–D^{counter})CYC$) and one for countercyclical policy ($D^{counter}CYC$), both coefficients are individually significant at the one percent level as well. Of course, the implied coefficients and the test for parameter equality are identical to the specification considered in column (1b) in Table 2.

volatility, which is addressed by using population (*POP*), the urbanization rate (*URBAN*), and the dependency ratio (*DEP*) as instruments for *GSIZE*.

< Table 3 >

Columns (1a) and (1b) show the unweighted LS and TSLS estimates, whereas column (1c) gives the results of the WTSLS estimation. As expected the estimated elasticity of volatility with respect to cyclicality becomes smaller in magnitude when the control variables are added (around 0.3) but remains significant. Exogeneity of *CYC* is still clearly rejected in all models; we thus focus on the (W)TSLS results. The OID tests are insignificant in most specifications, suggesting that the institutional variables *NELEC* and *POLCON* (as well as *POP*, *DEP*, *URBAN*) are valid instruments. To reinforce the finding of our parsimonious specification, we repeat the test for parameter equality between pro- and countercyclical policy (see columns (2a) and (2b)). The conclusion is the same as before: There is no evidence for a stabilizing effect of countercyclical fiscal policy. In contrast, it adds to output volatility, in a way not significantly different from that of procyclical fiscal policy.

Of course, the results regarding the role of countercyclical policy should be interpreted with care. The number of countries which pursued countercyclical fiscal policy on average is rather small (11 out of the 88). While this might be too little variation to yield a significant difference in the estimated effect, it does not explain that the effect of countercyclical policy on volatility is always positive, a finding that is extremely robust. We also emphasize that our results should be interpreted as averages over countries and time. One cannot rule out that a highly effective government, which is aware of the relevant lag structures and able to respond very quickly, might be successful in its fiscal efforts to smooth business cycles. What our evidence suggest, however, is that such a constellation is rather the exception than the rule.

Finally, we consider the results for model (3), which includes both cyclicality (*CYC*) and discretionary fiscal policy (*DISCR*). It is a subtle question, whether discretionary fiscal policy should be controlled for. On the one hand, discretionary fiscal policy is certainly a relevant determinant of output volatility (Fatas and Mihov (2003), Badinger (2008)). Moreover, while the time series of discretionary and cyclical fiscal policy measures for a single country are orthogonal, this does not carry over the cross-country variation in cyclical and discretionary policy (averaged over time): Countries that are more responsive to the cycle might also more actively engage in discretionary fiscal policy.

Under these two assumptions – *DISCR* matters for volatility and is related to *CYC* – the estimated effect of *CYC* in model (2) will be upward biased due to the omission of *DISCR*. In that case, however, we would also expect the OID test to reject instrument validity, since the instruments used for *CYC* (i.e., *POLCON*, *NELEC*) are also related to discretionary policy (see section III, subsection 2). But this is not the case in any of the specifications, suggesting that these two elements of fiscal policy could be (close to) orthogonal in the cross-section.

One could still argue that the OID test has small power and *CYC* and *DISCR* should be regarded as related for theoretical reasons. Even in that case the question remains, whether discretionary policy should be controlled for: A main reason for a possible association between *DISCR* and *CYC* is that an active conduct of cyclical fiscal policy might partly result in (unintentional) discretionary policy (unrelated to the cycle) as a result of lags in implementation and materialization (again, on average over countries and time). Since these unintentional consequences of cyclical fiscal policy can hardly be ruled out by policy makers in practice, it might be reasonable to let the parameter of cyclicality in model (2) also capture its indirect effects on volatility through is relation to discretionary fiscal policy.

Notwithstanding these arguments that might favour model (2) over model (3), we now turn to the results when *DISCR* is included (see column (3a)). As expected the coefficient of *CYC* becomes smaller, pointing to an elasticity of volatility with respect to cyclicality of around 0.163, but it remains significant at the 10 percent level. The elasticity with respect to discretionary fiscal policy is 0.454 but insignificant with a p-value of 0.103. This is not too surprising; since both variables are instrumented using mainly the same set of institutional variables (only *MAJ* and *PRES* are added as additional instruments for *DISCR*), the predicted values for *CYC* and *DISCR* from the first stage regressions will be strongly correlated, causing a multicollinearity problem in the second stage regression. This is aggravated by the presence of a third endogenous variable (*GSIZE*).[8]

There are several ways to address this weak instruments problem: The most obvious would be to identify further instruments. Since it is difficult to think of variables affecting cyclicality (*CYC*) but not the aggressiveness of discretionary fiscal policy (*DISCR*) and vice versa, this approach does not appear to be very promising in the present context. Alternatively, an estimation technique more robust to weak instruments could be used. Stock and Yogo (2004), who consider the consequences of weak instruments and the performance of alternative estimators, find that limited information maximum likelihood (LIML) is far superior to TSLS estimation in the presence of weak instruments. Hence, we reestimate model (3) using LIML (see column (3b)): In that case *CYC* and *DISCR* turn both out significant with elasticities of 0.182 and 0.417 respectively.[9]

[8] If government size is treated exogenous and population is included as instrument, *CYC* and *DISCR* turn out significant at the five and one percent level respectively. However, since there are strong theoretical arguments to regard government size as endogenous, and since the theoretical motivation for using country size (population) as instrument for *CYC* and *DISCR* is weak (despite the fact that is highly significant in the first stage regression), we pursue the more conservative approach here and treat *GSIZE* as endogenous.

[9] Note that the superiority of LIML estimation in Stock and Yogo (2004) is obtained under homoscedasticity, which is also assumed in the LIML estimation here.

Another route would be to use a compound measure of discretionary and cyclical fiscal policy ($\ln CYC + \ln DISCR$), which could be justified in light of the fact that the hypothesis of parameter equality cannot be rejected (F-statistic: 0.704, p-value: 0.403). Results are given in column (4): In the restricted model, the compound measure of fiscal policy turns out highly significant with a coefficient of 0.238. The economic interpretation of this restricted model with equal parameters for cyclical and discretionary fiscal policy carries our finding regarding the irrelevance of the direction of cyclicality one step further: Not only has countercyclical policy the same effect on volatility as procyclical fiscal policy. It also implies that cyclical fiscal policy (*CYC*) has the same amplifying effect on output volatility as 'random' discretionary fiscal policy shocks, suggesting that the effects of intentional cyclical policy measures – due to poor timing and lags in implementation and materialization – spread over time in a way such that the implied average outcome is random with respect to the cycle.[10] Since the focus of the present paper is on cyclicality, however, we do not use a compound measure of fiscal policy in the following.

4. Robustness

We first demonstrate that the logarithmic transformation of *CYC* is not crucial for the results. Column (1a) in Table 4 uses the level of *CYC* and allows for parameter heterogeneity between pro- and countercyclical fiscal policy. As in the logarithmic specification, there is no evidence that countercyclical fiscal policy has a significantly different effect on volatility than procyclical fiscal policy. Column (1b) gives the results, when the parameters of pro- and countercyclical fiscal policy (in levels) are restricted to equality: *CYC* turns out significant at the 5 percent level with an average elasticity of 0.737. Judged by the standard error of estimation, the fit is worse than for the specification in log form.

< Table 4 >

We next consider subsample stability of the results for models (2) and (3) with respect to the country dimension. A visual inspection of a scatter plot of output volatility against cyclicality does not suggest that our results are driven by a few extreme observations (see Figure 1). We nevertheless reestimate models (2) and (3), excluding countries with 'large' output volatility or 'large' cyclicality from the sample.[11]

< Figure 1 >

[10] This argument was already made by Friedman in his informal essay on fiscal policy: "In fiscal as in monetary policy, all political considerations aside, we simply do not know enough to be able to use deliberate changes in taxation and or expenditures as a sensitive stabilization mechanism. In the process of trying to do so, we almost surely make matters worse… by introducing a largely random disturbance that is simply added to other disturbances." (Friedman, 1962, p. 78).

[11] For conservativeness, we focus on the TSLS results for model (3) here; the LIML coefficients are very similar and typically have smaller p-values.

Columns (2a) and (2b) give the results for models (2) and (3), excluding countries whose output volatility exceeds the sample average by more than one standard deviation. The same exercise is repeated in columns (3a) and (3b), excluding countries whose cyclicality coefficients exceed the sample average by more than one standard deviation. Overall, the results for the full sample in Table 2 hold up, confirming that our results are not driven by a few outlying observations.

In columns (4a) and (4b) we focus on a subsample of 28 'rich' countries, constituting the upper third of the income distribution of our sample in terms of GDP per capita. In both models (2) and (3) the variable *CYC* becomes insignificant with p-values of 0.245 and 0.193, respectively. However, if the level of development (*GDPPC*) is excluded, which appears to be justifiable for a group of countries with a similar level of development (in particular in model (3) where *GDPPC* is insignificant), *CYC* becomes significant again in models (2) and (3) at the 10 and 5 percent level respectively. Moreover, if the level rather than the log of *CYC* is used, the variable *CYC* is close to significance with a p-value of 0.116 in model (2) (even with *GDPPC* included). (The qualitative results are essentially the same for the OECD subsample.) It goes without saying that these estimates, relying on asymptotic properties, should not be overstressed due to the small number of observations. Overall, our reading of these results is that in the group of highly developed countries, cyclical fiscal policy appears to have a significant though somewhat less pronounced destabilizing effect.

In a final step we consider the results for the more recent period 1980 to 2004 to rule out that our estimates are driven by the comparably erratic times up the 1980s. We first note that our estimates of the cyclicality parameters (*CYC*) and the aggressiveness of discretionary fiscal policy (*DISCR*) for the full period from 1960 to 2004 and for the period of 1980 to 2004 are fairly similar (see Appendix A1). Many of the countries that pursued countercyclical fiscal policy from 1960 to 2004 on average did so as well in the period from 1980 to 2004. The correlation between *CYC* (*DISCR*) for the two time periods is 0.766 (0.935). This also favours the interpretation that (de facto) time invariant institutional features of countries have a strong impact on the cross-country variation in the conduct of fiscal policy.

Turning to the estimation results, a slight difference to the period 1960 to 2004 is that the effect of countercyclicality on volatility appears to be significantly different in magnitude from that of procyclical policy in the logarithmic specification (see column (5a)), though its effect on output volatility is still clearly positive. It is tempting to argue that countercyclical policy has become partly more effective (and thus overall less destabilizing). But this appears to be overstressing results a bit, given that there is no significant difference between the effects of pro- and countercyclical policy if the level rather than the log of *CYC* is used; column (5b) shows the (restricted) model using the level of *CYC*.

The estimates of model (3) for the period 1980 to 2004 (columns (6a) and (6b)), where *DISCR* is included along with *CYC*, are in line with the results for the full period 1960 to

2000, in particular when the model is estimated using limited information maximum likelihood (column (6b)). As before the hypothesis of parameter equality of *CYC* and *DISCR* cannot be rejected.

Finally, we add that the subsample stability with respect to the cross-country dimension for the period 1980 to 2004 (excluding countries with large volatility or large cyclicality, or considering rich countries only) is qualitatively very similar to that for the full period.

IV. Fiscal Cyclicality, Volatility, and Economic Growth

Having established a relationship between cyclicality and output volatility we now go on to assess the effect of cyclicality on economic growth through output volatility. From a theoretical perspective, the relation between output volatility and economic growth is ambiguous. A positive relation is conceivable as a result of a Schumpeterian cleansing effect of recessions (e.g., Caballero and Hammour, 1991) or due to the fact that the opportunity costs of productivity enhancing reorganizations are smaller during recessions (Hall, 1991). A negative relation might result from irreversibilities in investments or from credit market imperfections that constrain productivity enhancing investments in recession (Aghion et al. 2006). The relation between volatility and growth is even more intricate, since causality may also run from growth to volatility (Stiglitz, 1993). Empirically, Ramey and Ramey (1995) found a negative effect of output volatility on economic growth, and – though there is no consensus so far – the evidence that has emerged since then tends to support this finding.

While a number of studies have considered the effect of output volatility on growth, there is hardly evidence on the relation between the cyclicality of fiscal policy and growth. A noteable exception is Aghion and Marinescu (2008), who find a positive effect of countercyclicality measures in a growth regression, using an (unbalanced) panel of annual data for 19 OECD countries from 1960 to 2007. Moreover, no previous study has considered the relations between cyclicality, volatility and growth in a joint empirical framework.

We first consider the effects of cyclicality on growth directly, running a cross-section regression of average growth of real GDP per worker over ($\overline{\Delta \ln GDPPW}$) on the cyclicality of fiscal policy (*CYC*), again testing for differences in the effect of pro- and countercyclical policy:

$$\overline{\Delta \ln GDPPW_i} = \delta_0 + \delta_1 \ln CYC_i \; + (\delta_1^{counter} D_i^{counter} \ln CYC_i\;) + \mathbf{w}_i \boldsymbol{\delta} + \vartheta_i \tag{4}$$

The following standard controls ($\mathbf{w}_i$) are included in our cross-country growth regression: the (log of the) initial level of real GDP per worker ($GDPPW^{in}$), the average level of human capital in terms of educational attainment, i.e., the fraction of males above 25 with primary schooling (HC^{prim}) and secondary schooling (HC^{sec}). Model (4) refers to the time period from

1960 to 2004 again; the cross-section dimension is slightly smaller now with 80 rather than 88 countries due to missing human capital data.

As in the volatility regressions in section III, the possible endogeneity of *CYC* is addressed by using the institutional variables *NELEC* and *POLCON* as instruments; and the fact that *CYC* is calculated from fitted values of model (1) is accounted for by a weighted least squares approach, using the inverse of the variance of *CYC* as weight.

< Table 5 >

Columns (1a) and (1b) in Table 5 report the unweighted LS and TSLS estimates, allowing the effect of pro- and countercyclicality to differ. Columns (2a) and (2b) report the respective weighted estimates. In all specifications we find a significantly negative effect of cyclicality on economic growth. And as in section II, only the magnitude of cyclicality seems to matter: We find a negative effect of both pro-and countercyclicality on economic growth, and while the coefficient of countercyclical fiscal policy is smaller in magnitude, the difference in the coefficients is not significantly different from zero. This holds true for both the unweighted and the weighted estimates. According to the Hausman test there is no strong evidence for endogeneity of *CYC*, though the (W)LS estimates of the parameter of *CYC* are always smaller in magnitude than the (W)TSLS estimates.

The results in Table 5 suggest a negative relation between the cyclicality of fiscal policy and economic growth. In the corresponding model (4), which omits output volatility (and further controls), the parameter of *CYC* (δ_1) captures all effects of cyclicality on economic growth, both through its relation with output volatility (direct and indirect through *DISCR*), through its relation with other variables affecting growth, as well as ‘direct’ effects of cyclicality on economic growth (if any). From an economic policy perspective, this might in fact be the most relevant question.

Nevertheless, we would like to provide a more detailed picture of the relation between cyclicality and growth. In particular, we also wish to answer the question whether *CYC* affects growth only through output volatility or also directly. Such a direct link could be motivated through the model by Aghion et al. (2006). Their argument is that credit constrained firms have a borrowing capacity that depends on current earnings, which are reduced in recessions, such that firms are less able to borrow in order to maintain growth enhancing investments. This reasoning suggests that countercyclicality may foster productivity growth by reducing the magnitude of the output loss induced by market failures (as credit market imperfections) in a recession. One could argue that such an effect should also hold for a given output volatility (i.e., controlling for output volatility). Regressing growth on cyclicality alone is not informative about the relevance of two potentially offsetting effects of cyclical fiscal policy: a negative effect by increasing volatility (suggested by the results in section III) and a possibly positive effect through reducing market failures due to credit market imperfections.

The empirical framework employed to address this question is sketched by Figure 2, which illustrates the interrelationships between the key variables in our empirical models. Potential endogeneity is indicated by reversed arrows (though the source of endogeneity is not necessarily simultaneity); relations between variables of the same equation (such as *CYC* and *DISCR*) are omitted for simplicity here.

< Figure 2 >

To test for a direct effect of cyclicality on growth, we first consider a model relating growth to output volatility (and controls). In a next step we add cyclicality as additional regressor (again considering potentially different effects of pro- and countercyclical policy):

$$\overline{\Delta \ln GDPPW_i} = \varphi_0 + \varphi_1 \ln \sigma^y + \mathbf{w}_i \boldsymbol{\varphi} + (\delta_1 \ln CYC_i + \delta_1^{counter} D_i^{counter} \ln CYC_i) + \zeta_i \tag{5}$$

Column (1a) in Table 6 reports the least squares estimates of equation (5), column (1b) the TSLS estimates using the full set of institutional variables (*NELEC*, *POLCON*, *MAJ*, *PRES*) as instruments for volatility.[12] Notice that the exogeneity of output volatility (σ^y) is clearly rejected. Results of the TSLS estimates point to a negative effect of volatility on growth, consistent with our finding that cyclicality increases volatility (models (2) and (3)) and reduces growth (model (4)). The coefficient of output volatility (-2.968) suggests that an increase in volatility by one percent reduces average growth by some 0.3 percentage points. This is close to Fatas and Mihov (2003), who obtain a coefficient of -3.371 in a similar regression. We emphasize that results are very similar when *MAJ* and *PRES* are excluded from the set of instruments, or when *CYC* or *DISCR* are used as instruments directly. This is supportive of the finding in section III that the transmission mechanisms from political institutions to output volatility through cyclical and discretionary fiscal policy are very similar.

< Table 6 >

The OID tests, reported at the bottom of Table 6, suggest that there is nothing wrong with our instruments. However, one might still argue that the power of this test is low and that the instruments, which are based on the respective country's institutional characteristics might be somehow associated with other country-specific institutions or policies affecting economic growth. We thus propose a new instrument for output volatility (Z^{σ}), which is entirely unrelated to the respective country's characteristics. In particular, we suggest using, for each country *i*, the bilateral trade share weighted output volatility of all other countries *j* ($\neq i$) in the sample. In a world of highly interdependent economies, where local shocks are propagated through the international economic system through trade and financial flows, we expect

[12] Results for specifications without *CYC* are unweighted estimates.

‘volatility spillovers’ from foreign countries to be a relevant determinant of a country’s own output volatility. However, in order to ensure exogeneity two modifications are made: First, the actual trade shares are replaced by predicted values from a bilateral gravity model including geographical variables only, an approach inspired by Frankel and Romer (1999). Second, the actual output volatility is replaced by predicted values from a regression of output volatility on (a constant and) the four institutional variables (*NELEC*, *POLCON*, *MAJ*, *PRES*). Appendix A3 gives more details on the construction of this instrument, referred to as Z^{σ} henceforth. The validity of Z^{σ} relies on the assumption that country *i*’s geographical characteristics on the one hand and other countries’ political institutions on the other hand are exogenous with respect to country *i*’s economic growth in equation (4). In our view it is hard to think of convincing reasons why this assumption should be hurt.

The variable Z^{σ} also turns out to be a relevant instrument for output volatility besides the four institutional variables (*NELEC*, *POLCON*, *MAJ*, *PRES*). In the first stage regression for model (5), including all four institutional variables, Z^{σ} is significant at the one percent level. In fact, Z^{σ} turns out to be the strongest instrument for output volatility besides political constraints (*POLCON*). This is an important result, since it implies that Z^{σ} adds variation to identify the effect of output volatility in the growth regression (5) with *CYC* included as regressor, in addition to the identifying variation, which comes from the effects of institutions on volatility through on *DISCR* (which might be too closely associated with the effects of *CYC* on volatility) (see Figure 2).

Column (1c) shows the TSLS estimates of model (4), using only Z^{σ} as instrument; the estimated effect of volatility is even larger in magnitude, though the estimates are less precise. Nevertheless, output volatility remains significant with a p-value of 0.087. The fit of the model improves, when the four institutional variables are included as additional instruments (see column (1d)). Overall, the estimates of model (5) in columns (1b) to (1d), which differ only by the set of instruments used, are very similar. This is a reassuring result. In the following, we will use as instruments for volatility all four institutional variables (*NELEC*, *POLCON*, *MAJ*, *PRES*) as well as exogenous volatility spillovers (Z^{σ}) to provide additional identifying variation.

We next consider the robustness of the results with respect to controlling for institutional quality, which we measure using the government antidiversion policy (*GADP*) index by Hall and Jones (1999). Since institutional quality might be endogenous as a result of reverse causality – rich countries are generally better able to build up (costly) high quality institutions – we follow Hall and Jones (1999) and use the following proxy variables for Western influence as instruments for *GADP*: distance from equator, the fraction of a country’s population speaking English as mother tongue, and the fraction of a country’s population speaking one of the five European languages (English, French, German, Portuguese, Spanish) as mother tongue. The results in column (2) show that the coefficient of *GADP* is positive as expected but insignificant. This might be due to inherently poor measurement of institutional quality and a mismatch in the time span covered by our growth regression (1960 to 2004) and the *GADP* measure, which refers to 1986 to 1995. Moreover, the effect of institutional quality on per capita income is probably better estimated using a level rather than a growth rate approach (see Hall and Jones (1999), Alcalá and Ciccone (2004)). The most important result in the present context is that output volatility remains significant and negatively related to economic growth, even if institutional quality in terms of *GADP* is controlled for.[13]

We now test for a direct effect of cyclicality on growth. This means, that the variable *CYC* is included in the main equation (and instrumented by itself), whereas output volatility is instrumented by the institutional variables (*NELEC*, *POLCON*, *MAJ*, *PRES*) and exogenous volatility spillover (Z^{σ}). Columns (3a), (3b), and (3c) show the LS, TSLS, and WTSLS estimates of equation (5) including institutional quality as control.[14] (Results are very similar if *GADP* is omitted.) The estimates show no evidence for a direct effect. We reestimate model (3), allowing the effect of pro- and countercyclical policy to differ. Again no evidence for a direct effect of cyclicality on growth can be identified (see column (4)). This is a very robust result and also holds up for the unweighted regression or if the model is estimates least squares. We also explored the subsample stability with respect to the cross-country dimension (rich countries, excluding countries with large volatility or cyclicality coefficients) and the time dimension, considering the more recent time span 1980 to 2004. In none of the specifications, we could identify a direct effect of cyclicality besides output volatility, irrespective of whether we allow the effect of countercyclical policy to differ from that of procyclical policy or not.[15]

[13] We also considered the variables *OPEN* or *GSIZE* as controls, since they are theoretically motivated determinants of output volatility and could be related to growth. This does not affect the results, which is not too surprising in light of the fact that the empirical association between volatility and these two variables is very weak (see the volatility regressions in section III).

[14] Notice that, since *CYC* is included in the main model, the independent variation to identify the effect of volatility on growth comes from the instrument Z^{σ} as well as from the effect of institutions on volatility through discretionary fiscal policy (see Figure 2). The latter is also apparent from the fact, that in a regression with output volatility as dependent variable, the political variables (*MAJ*, *PRES*, *NELEC*, *POLCON*) are jointly significant determinants of output volatility when *CYC* is controlled for, but they become insignificant if *DISCR* is added to the regressions as well. This suggests an alternative approach, using *DISCR* as instrument for σ^{y} directly (instead of the instead of the institutional variables); results turned out almost identical and are not shown here for brevity.

[15] The robustness analysis for models (4) and (5) turns out very similar to that for output volatility in section II. Results for the effect of cyclicality on growth (Table 5) and volatility on growth (Tables 6) generally hold up but are less pronounced in the sample of rich countries, where p-values are slightly above 10 percent in some specifications.

Overall, the findings in sections III and section IV provide a consistent picture. Cyclical as well as discretionary fiscal policy amplify output volatility (Tables 2 to 4), which is in turn negatively related to economic growth (Table 6). This also shows up in direct estimates of growth on cyclicality (Table 5). The effects found for rich countries are of the same order of magnitude but estimated less precisely, rendering the effects insignificant in some specifications with p-values slightly above the 10 percent level.

V. Conclusions

Previous studies found that discretionary fiscal policy, defined as policy unrelated to the business cycle, lowers output growth by increasing output volatility. Using a large cross-section of 88 countries over the period 1960 to 2004, the present paper provides comprehensive empirical evidence that this is also true for the second important element of fiscal policy, i.e., cyclical fiscal policy.

Building on Fatas and Mihov (2003), we suggest using information on the countries political and institutional characteristics to identify the causal effect of (endogenous) cyclicality on output volatility. In a second step, we consider the relation between economic growth and (endogenous) output volatility, induced by cyclical and discretionary fiscal policy.

We estimate simple average measures of cyclicality of government consumption over the period 1960 to 2004 for each of the 88 countries of our sample. The choice of an expenditure-based measure is due to data availability on the one hand but also motivated by the fact that it is more indicative of intentional fiscal policy (and less driven by automatic stabilizers). We find that cyclical fiscal policy constitutes a non-negligible share of overall fiscal policy, accounting for roughly one fifth in the total variation of government consumption in our sample. We then demonstrate that institutional variables (such as political constraints on policy implementation and the average number of elections) contain considerable information about the cross-country variation of fiscal cyclicality.

Using this exogenous variation, we identify a destabilizing effect of cyclical fiscal policy on economic activity, irrespective of whether the policy is pro- or countercyclical. This not only confirms the scepticisms against the usefulness of countercyclical fiscal policy as fine tuning instrument to smooth business cycles. It also implies that countercyclical policy has the same amplifying effect on volatility as procyclical fiscal policy; in fact, we find some support for the hypothesis that cyclical fiscal policy affects volatility much in the same way as pure fiscal shocks (i.e., discretionary fiscal policy, unrelated to the cycle). Intentional cyclical policy measures – due to poor timing and lags in implementation and materialization (that will differ over alternative policy measures) – thus appear to spread over time in a way such that the implied average outcome is random with respect to (its effect on) the business cycle. According to this result, the way towards stabilization does not lead over more active countercyclical fiscal policy, but less cyclical fiscal policy at all.

The gains from this (passive) stabilization policy could be substantial in light of our finding that aggressive use of cyclical (as well as discretionary fiscal) policy has a negative effect on economic growth. This result is obtained both in a direct regression of growth on cyclicality and in a two stages least squares regression of growth on volatility, using the fiscal policy-related institutional variables as instrumental variables in the first stage regression. However, there is no evidence for a direct effect of cyclicality on economic growth, once output volatility is controlled for. These findings turn out robust across a large number of specifications and subsamples.

Overall, our results have an important policy implication: Economic growth could be enhanced by introducing fiscal rules, designed to limit the use of discretionary fiscal policy on the one hand (as already argued by Fatas and Mihov, 2003) but also the use cyclical fiscal expenditure policy on the other hand.

It should be added that – notwithstanding the robustness of our results with respect to subsample stability over the cross-country and time dimension – our cross-section estimates should be interpreted as averages over countries and time, not as economic laws that apply to every government at any time. Moreover, the use of government consumption as measure of fiscal policy– a choice required to obtain a relatively large sample of countries – limits the generality of the results. As a consequence, it would be interesting to investigate the effect of fiscal policy on volatility and growth for smaller groups of countries or single countries with more comprehensive, more detailed and higher frequency data on fiscal policy. Another question that remains to be addressed in future research is how existing fiscal rules affect the cyclical responsiveness of governments. Additional evidence on these issues would not only deepen our understanding of the channels, through which fiscal policy affects economic growth, but also help to answer the open question, how fiscal rules should be optimally designed in order to improve economic performance.

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Appendix

A1. Sample Description

The largest set of countries for which the required key variables are available comprises 88 countries. The list of countries is reported in Table A1, along with our estimates of the cyclicality parameter χ and the aggressiveness of discretionary fiscal policy (*DISCR*) from equation (1).

Table A1. *Sample and Data on Output Volatility, Cyclical and Discretionary Fiscal Policy*

		$\sigma^{\Delta \ln y}$		Cyclicality ($\hat{\chi}$)		*DISCR*	
country	time period (obs)	1960-2004	1980-2004	1960-2004	1980-2004	1960-2004	1980-2004
Argentina	1960-2004 (45)	5.753	6.378	1.540	2.190	29.741	37.765
Australia	1960-2004 (45)	1.840	1.807	-0.375	0.009	3.086	1.965
Austria	1960-2004 (45)	1.777	1.162	0.264	0.493	2.035	1.602
Burundi	1960-2004 (45)	5.798	4.399	1.070	0.875	16.535	17.037
Belgium	1960-2004 (45)	1.833	1.333	-0.080	-0.031	2.396	1.841
Benin	1960-2004 (45)	3.099	3.058	-0.034	0.038	8.456	9.478
Burkina Faso	1960-2004 (45)	3.101	3.338	0.740	1.211	13.896	11.118
Bangladesh	1960-2004 (45)	4.133	1.473	2.698	1.834	12.215	7.613
Bolivia	1970-2004 (35)	3.616	2.650	2.049	2.394	9.886	9.686
Brazil	1960-2004 (45)	3.877	3.453	0.480	0.596	8.490	9.000
Centr. Afr. Rep.	1960-2002 (43)	4.034	4.678	0.891	1.095	11.129	12.311
Canada	1965-2004 (40)	1.982	2.122	-0.464	-0.283	1.871	1.767
Switzerland	1960-2004 (45)	2.247	1.572	0.440	0.446	2.065	1.873
Chile	1960-2004 (45)	4.879	4.565	0.824	0.428	8.088	5.652
Cote d'Ivoire	1960-2004 (45)	5.214	4.104	1.270	1.059	9.886	10.647
Cameroon	1965-2004 (40)	5.818	5.306	0.988	1.004	8.558	10.020
Congo, Rep.	1960-2004 (45)	5.500	6.002	0.461	0.448	17.857	22.703
Colombia	1960-2004 (45)	2.084	2.095	0.686	0.693	9.275	9.074
Costa Rica	1960-2004 (45)	3.241	3.585	1.016	1.177	5.458	6.202
Germany	1960-2004 (45)	1.880	1.441	0.135	0.414	2.443	1.525
Denmark	1960-2004 (45)	2.151	1.624	0.215	-0.342	2.526	1.804
Dom. Republic	1960-2004 (45)	5.080	3.624	0.728	3.369	21.349	19.347
Algeria	1960-2004 (45)	7.147	2.591	0.931	0.551	9.207	7.538
Ecuador	1960-2004 (45)	3.448	3.162	1.365	0.826	11.162	10.418
Egypt	1960-2004 (45)	2.726	1.758	0.662	-0.202	9.407	6.722
Spain	1960-2004 (45)	2.326	1.574	0.656	0.430	2.659	2.223
Finland	1960-2004 (45)	2.838	2.859	-0.054	-0.074	2.958	2.857
Fiji	1960-2000 (41)	4.616	4.594	0.628	0.441	7.567	8.167
France	1960-2004 (45)	1.585	1.138	-0.281	-0.432	1.411	1.186
Gabon	1960-2000 (41)	9.397	5.968	0.616	0.683	20.247	17.257
United Kingdom	1960-2004 (45)	1.735	1.754	-0.262	-0.273	2.320	1.914
Ghana	1960-2004 (45)	4.592	3.603	1.776	3.716	14.358	14.671
Greece	1960-2004 (45)	3.736	2.321	0.437	0.771	5.451	5.355
Guatemala	1960-2004 (45)	2.458	2.178	1.121	2.170	8.607	9.144
Honduras	1960-2004 (45)	2.886	2.423	-0.110	0.596	7.057	7.434
Haiti	1967-2003 (37)	4.469	4.146	1.146	1.134	10.656	10.947
Indonesia	1960-2004 (45)	3.981	4.444	1.560	1.453	14.998	8.224
India	1960-2004 (45)	3.038	1.817	0.518	0.340	5.047	3.237
Ireland	1960-2004 (45)	2.595	2.930	0.602	0.610	3.561	3.373
Iceland	1960-2004 (45)	3.701	2.914	0.860	0.669	3.904	3.244

Table A1 (continued). *Sample and Data on Output Volatility, Cyclical and Discretionary Fiscal Policy*

		$\sigma^{\Delta \ln y}$ in percent		Cyclicality ($\hat{\chi}$)		*DISCR* in percent	
country	time period (obs)	1960-2004	1980-2004	1960-2004	1980-2004	1960-2004	1980-2004
Israel	1960-2004 (45)	3.331	2.118	1.181	1.334	9.928	6.283
Italy	1960-2004 (45)	2.148	1.275	0.059	0.293	2.848	2.578
Jamaica	1966-2004 (39)	4.596	3.167	1.150	1.427	9.612	10.284
Japan	1960-2004 (45)	3.453	1.791	0.128	0.123	2.205	0.879
Kenya	1960-2004 (45)	4.562	1.982	1.202	1.789	5.977	4.584
Korea, Rep.	1960-2004 (45)	3.441	3.782	-0.281	-0.121	6.844	3.137
Sri Lanka	1960-2004 (45)	1.901	1.722	0.319	1.089	8.646	9.695
Lesotho	1960-2004 (45)	6.383	3.844	0.239	-0.350	15.132	9.970
Morocco	1960-2004 (45)	4.449	4.796	0.700	0.549	7.568	4.719
Madagascar	1960-2004 (45)	4.266	4.782	1.240	1.220	9.628	10.979
Mexico	1960-2004 (45)	3.312	3.619	1.660	1.760	5.305	5.752
Mali	1967-2004 (38)	5.140	5.154	0.660	1.296	16.133	15.721
Mauritania	1960-2004 (45)	5.890	3.270	0.635	1.900	17.809	18.398
Mauritius	1980-2004 (25)	1.535	1.535	0.583	0.583	3.751	3.751
Malawi	1960-2004 (45)	5.423	5.721	-0.835	-2.066	15.870	16.817
Malaysia	1960-2004 (45)	3.385	3.971	0.229	0.548	8.451	8.308
Niger	1960-2004 (45)	6.253	5.542	0.762	1.270	11.605	11.165
Nigeria	1960-2004 (45)	7.186	5.100	0.429	0.814	19.854	22.460
Nicaragua	1960-2004 (45)	6.895	4.200	0.277	1.633	19.112	22.562
Netherlands	1960-2004 (45)	1.833	1.503	0.350	-0.027	2.177	1.770
Norway	1960-2004 (45)	1.554	1.618	0.631	0.023	3.675	3.982
New Zealand	1971-2004 (34)	2.835	1.946	0.360	0.159	4.133	2.658
Pakistan	1960-2004 (45)	2.187	1.793	0.975	1.308	8.104	7.276
Panama	1960-2004 (45)	4.197	4.832	1.168	1.044	6.571	6.125
Peru	1960-2004 (45)	5.038	6.201	1.248	1.381	9.718	7.948
Philippines	1960-2004 (45)	3.027	3.629	1.261	1.490	5.344	5.665
Pap. New Guinea	1961-1999 (39)	4.610	5.118	1.042	0.893	8.776	9.588
Portugal	1960-2004 (45)	3.295	2.521	0.750	1.261	3.549	2.571
Paraguay	1960-2004 (45)	3.793	3.768	0.469	0.988	9.713	10.104
Rwanda	1960-2004 (45)	11.997	14.623	1.171	1.303	16.300	16.653
Senegal	1960-2004 (45)	4.173	3.777	0.827	0.033	18.002	5.998
Singapore	1960-2004 (45)	4.175	3.832	0.043	-0.339	6.823	7.359
El Salvador	1960-2004 (45)	3.834	4.215	0.561	0.319	5.802	5.548
Sweden	1960-2004 (45)	1.925	1.837	0.032	0.050	2.252	1.828
Syr. Arab Rep.	1960-2004 (45)	7.809	5.298	0.569	-0.005	9.749	9.548
Chad	1960-2004 (45)	8.186	8.773	0.064	0.027	14.689	19.314
Togo	1960-2004 (45)	6.131	6.234	0.174	0.410	17.114	13.173
Thailand	1960-2004 (45)	3.635	4.436	0.519	0.446	4.761	3.667
Trin. and Tobago	1960-2004 (45)	4.749	5.666	1.195	1.269	11.929	13.900
Tunisia	1961-2004 (44)	3.343	2.557	0.443	-0.019	5.614	2.266
Turkey	1968-2004 (37)	4.147	4.637	0.629	0.602	7.737	8.357
Uruguay	1960-2004 (45)	4.744	5.812	0.989	0.974	10.328	5.996
United States	1960-2004 (45)	1.906	1.820	-0.048	-0.091	1.967	1.310
Venezuela	1960-2004 (45)	4.949	5.938	1.616	1.913	16.543	20.776
South Africa	1960-2004 (45)	2.525	2.658	0.892	0.664	4.724	3.700
Congo, Dem. Rep.	1960-2004 (45)	6.079	5.363	1.570	1.425	26.618	32.791
Zambia	1960-2004 (45)	4.703	3.919	1.485	1.928	21.520	24.135
Zimbabwe	1960-2004 (45)	5.828	5.811	0.335	0.339	12.693	15.324
Correlation		0.842		0.766		0.935	

A2. Variable Definitions and Data Sources

DEP dependency ratio, defined as ratio of people younger than 15 and older than 64 to working age population (people from 15 to 64). Source: World Development Indicators (WDI).

GC real general government consumption in national currency. Source: WDI.

GDPPC real GDP per capita in PPP$. Source: Penn World Tables (PWT) 6.2.

GDPPW real GDP per worker in PPP$. Source: PWT 6.2.

GSIZE ratio of government consumption to GDP.

HC^{prim} primary educational attainment, defined as fraction of males above 25 with primary schooling. Source: Barro and Lee (2002).

HC^{sec} secondary educational attainment, defined as fraction of males above 25 with secondary schooling. Source: Barro and Lee (2002).

MAJ zero-one dummy for electoral system (1 for majoritarian, 0 for proportional). Sources: Person and Tabellini (2001), Database of Political Institutions.

NELEC average number of elections. Sources: Database of Political Institutions.

OPEN ratio of imports plus exports to GDP. Source: PWT 6.2.

POLCON index of political constraints. Source: Henisz (2000).

POP population in million persons. Source: WDI.

PRES zero-one dummy for political regime (1 for presidential, 0 for parliamentary). Source: Person and Tabellini (2001), Database of Political Institutions.

URB ratio of urban population to total population. Source: WDI.

y real GDP in national currency per capita; $y = Y/POP$. Source: WDI.

Y real GDP in national currency. Source: WDI.

Data source for the geographical variables (such as bilateral distance, area, common border dummies) is the CEPII database (see Mayer and Zignago, 2006).

A3. Construction of Instrument for Output Volatility

The instrument for output volatility Z^{σ} takes the following value for country *i*:

$$Z_i^{\sigma} = \sum_{j=1, j\neq i}^{N} \hat{w}_{ij} \ln \hat{\sigma}_j^{y} ,$$

where $\hat{\sigma}_j^{y}$ is the 'institutionally induced' output volatility of country *j*, obtained by forming predicted values from a cross-country regression of $\ln \sigma^{y}$ on a constant and the four institutional variables *NELEC*, *POLCON*, *MAJ*, and *PRES*.

The weights $\hat{w}_{ij}$ are obtained as predicted values of the bilateral trade shares (imports plus exports as a share of GDP) from a gravity model including geographical variables only.[16] In line with Frankel and Romer (1999) the following explanatory variables are included in the geographical gravity model: the size of countries *i* and *j* (measured by area and population), distance between countries *i* and *j*, a common border dummy, a landlocked dummy, as well as interaction terms of all variables with the common border dummy. The gravity model is estimated for our sample of 7656 bilateral trade flows (on which 6393 nonzero observations are available), using average values from the period 1980 to 1990 (roughly corresponding to our sample midpoint).

Both models, the regression of volatility on the four institutional variables and the geographical gravity model, perform reasonably well with an R^2 (F-statistic) of 0.493 (20.191) and 0.347 (261.051) respectively. The correlation between Z^{σ} and the trade share weighted volatility based on actual rather than predicted values is 0.602. Finally, the correlation between Z^{σ} and output volatility is 0.336.

[16] This approach is inspired by Frankel and Romer (1999), who use the country-specific sum of predicted bilateral trade shares from a geographical gravity model – an aggregate measure of proximity – as an instrument in a cross-country regression of per capita income on (endogenous) trade and country size. The difference is that we use the bilateral predicted values as weights rather than summing them up to an aggregate proximity measure.

Table 1. *Political Determinants of Cyclicality and First Stage Regressions*

Dependent variable is:	(1a) $\ln CYC$	(1b) $\ln CYC$	(1c) $\ln CYC$	(1d) $\ln CYC$	(2a) $\ln CYC$	(2b) $\ln CYC$	(3) $\ln CYC$	(4) $\ln \sigma^{Discr}$
NELEC	-2.674***				-2.566**	-2.391***	-2.305***	-0.513
	(0.774)				(1.000)	(0.660)	(0.767)	(0.355)
POLCON		-1.905***			-1.704***	-1.822***	-1.489**	-0.470**
		(0.334)			(0.569)	(0.313)	(0.658)	(0.236)
MAJ			0.007		-0.036			-0.139**
			(0.233)		(0.197)			(0.068)
PRES				0.460*	0.112			0.408***
				(0.242)	(0.415)			(0.143)
$\ln POP$							-0.196***	-0.082**
							(0.064)	(0.024)
URBAN							-0.800	0.004
							(0.753)	(0.259)
DEP							3.003*	0.477
							(1.549)	(0.568)
OPEN							-0.577**	0.091
							(0.299)	(0.102)
$\ln GDPPC$							0.609*	-0.283**
							(0.359)	(0.122)
F-stat.[1)]	11.934***	32.589***	0.001	3.594*	12.312***	25.142***	6.587**	6.289***
F-stat.[2)]							8.472***	8.663***
R^2	0.122	0.275	0.000	0.040	0.372	0.372	0.502	0.872
	(0.007)	(0.146)	(0.017)	(0.018)	(0.091)	(0.089)	(0.13)	(0.674)
Observations	88	88	88	88	88	88	88	88

Notes: A constant is included in all models. *, **, *** denote significance at 10, 5, and 1 percent level respectively; standard errors in parenthesis. Table reports weighted least squares estimates, using the inverse of the variance of *CYC* as weight. R^2 refers to weighted model. [1)] F-test on excluding the institutional variables *(NELEC, POLCON, MAJ, PRES)* from first stage regression for $\ln CYC$ and $\ln \sigma^{Discr}$. [2)] F-Test on excluding all instruments (institutional variables, $\ln POP$, *URBAN*, *DEP*) from regression.

Table 2. *Cyclicality of Fiscal Policy and Output Volatility – Basic Model*

	(1a) WLS	(1b) WTSLS	(2a) WLS	(2b) WTSLS	(3a) LS	(3b) TSLS	(4a) WTSLS	(4b) WTSLS
$\ln CYC$	0.184**	0.595**	0.239***	0.600***	0.150***	0.742***		
	(0.082)	(0.232)	(0.060)	(0.207)	(0.044)	(0.185)		
$D^{counter} \ln CYC$	0.094	-0.094						
	(0.083)	(0.208)						
CYC							1.432***	1.762***
							(0.311)	(0.332)
$D^{counter}\ CYC$							-0.528	
							(0.517)	
Hausman 1) (p-val.)		(0.000)		(0.000)		(0.000)	(0.000)	(0.000)
OID 2) (p-val.)		(0.128)		(0.236)		(0.227)	(0.036)	(0.887)
R^2	0.128	0.096	0.108	0.108	0.108	0.108	0.105	0.099
SEE	0.448	0.606	0.454	0.626	0.431	0.731	0.768	0.915
Observations	88	88	88	88	88	88	88	88

Notes: Dependent variable is $\ln \sigma^y$. A constant is included in all models. *, **, *** denote significance at 10, 5, and 1 percent level respectively; robust standard errors in parenthesis. WLS denotes weighted least squares estimates, using the inverse of the variance of *CYC* as weight. (W)TSLS denotes (weighted) two stages least squares, using *NELEC* and *POLCON* as instruments for *CYC*. R^2 refers to unweighted model, calculated as squared correlation between actual and predicted values. 1) Heteroskedasticity-robust test for exogeneity; H_0: *CYC* is exogenous. 2) Heteroskedasticity-robust test of overidentifying restrictions; H_0: *NELEC*, *POLCON* are valid instruments.

Table 3. *Cyclicality of Fiscal Policy and Output Volatility – Extended Model*

	(1a) LS	(1b) TSLS	(1c) WTSLS	(2a) TSLS	(2b) WTSLS	(3a) WTSLS	(3b) LIML	(4) WTSLS
$\ln CYC$	0.079*	0.676**	0.292**	0.318*	0.324*	0.163*	0.182**	0.238***
	(0.040)	(0.270)	(0.119)	(0.166)	(0.188)	(0.090)	(0.083)	(0.068)
$D^{counter} \ln CYC$				-0.067	-0.084			
				(0.133)	(0.153)			
$\ln DISCR$						0.454	0.417**	restricted
						(0.276)	(0.204)	
GSIZE	-0.045	1.840	-3.406	0.635	-6.354	-2.709	-1.299	-3.431
	(0.620)	(4.894)	(3.903)	(0.260)	(4.654)	(2.854)	(0.994)	(2.724)
OPEN	0.176**	0.447*	-0.003	0.293*	0.077	-0.118	-0.108	-0.061
	(0.080)	(0.257)	(0.194)	(0.166)	(0.196)	(0.129)	(0.109)	(0.158)
$\ln GDPPC$	-0.239***	-0.096	-0.206	-0.189***	-0.166	-0.004	-0.013	-0.088
	(0.041)	(0.125)	(0.127)	(0.071)	(0.158)	(0.107)	(0.099)	(0.114)
Hausman [1)] (p-val.)		(0.000)	(0.000)	(0.179)	(0.000)	(0.016)		(0.000)
OID [2)] (p-val.)		(0.893)	(0.683)	(0.057)	(0.554)	(0.817)		(0.913)
R^2	0.404	0.159	0.279	0.292	0.220	0.479	0.534	0.376
SEE	0.358	0.658	0.507	0.418	0.583	0.469	0472	0.485
Observations	88	88	88	88	88	88	88	88

Notes: Dependent variable is $\ln \sigma^y$. A constant is included in all models. $^{*}, ^{**}, ^{***}$ denote significance at 10, 5, and 1 percent level respectively; robust standard errors in parenthesis. WLS denotes weighted least squares estimates, using the inverse of the variance of *CYC* as weight. (W)TSLS denotes (weighted) two stages least squares, using *NELEC* and *POLCON* as instruments for *CYC* and using *NELEC*, *POLCON*, *MAJ*, and *PRES* as instruments for *CYC* and *DISCR* in columns (3) and (4). [1)] Heteroskedasticity-robust Hausman test for exogeneity; H_0: $\ln CYC$, *GSIZE* (and $\ln DISCR$) are exogenous. [2)] Heteroskedasticity-robust test of overidentifying restrictions; H_0: *NELEC*, *POLCON* (*MAJ*, *PRES*) and $\ln POP$, *DEP*, *URBAN* are valid instruments.

Table 4. *Cyclicality of Fiscal Policy and Output Volatility – Robustness*

	1960-2004								1980-2004			
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)	(6a)	(6b)
	level of *CYC*		exl. large volatility		exl. large *CYC*		Rich		Level of *CYC*			
	equ. (2b)	equ. (2a)	equ. (2b)	equ. (3)	equ. (2a)	equ. (3)	equ. (2a)	equ. (3)	equ. (2b)	equ. (2a)	equ. (3)	equ. (3)
	WTSLS	WTSLS	WTSLS	WTSLS	WTSLS	WTSLS	WTSLS	WTSLS	WTSLS	WTSLS	WTSLS	LIML
$\ln CYC$			0.130	0.071*	0.282**	0.129**	0.131	0.063	0.231***		0.078	0.273*
			(0.087)	(0.042)	(0.116)	(0.059)	(0.110)	(0.047)	(0.066)		(0.056)	(0.153)
$D^{counter} \ln CYC$									-0.100**			
									(0.043)			
CYC	0.825***	1.034**								0.946**		
	(0.314)	(0.317)								(0.459)		
$D^{counter}\ CYC$	-0.026											
	(0.630)											
$\ln DISCR$				0.505**		0.554**		0.488**			0.405***	0.408**
				(0.170)		(0.212)		(0.235)			(0.072)	(0.164)
GSIZE	-4.899	-4.988	-2.389	-2.345	-2.310	-2.199	-2.122	-2.547	-1.369	-4.708	-3.606**	-6.977**
	(5.222)	(4.202)	(3.300)	(2.164)	(3.598)	(0.283)	(2.844)	(1.936)	(1.782)	(3.681)	(1.676)	(3.155)
OPEN	0.037	0.023	0.0063	-0.076	-0.029	-0.157	-0.021	-0.116	-0.039	0.208	-0.089	0.092
	(0.144)	(0.149)	(0.128)	(0.088)	(0.210)	(0.116)	(0.171)	(0.110)	(0.137)	(0.231)	(0.072)	(0.216)
$\ln GDPPC$	-0.145	-0.095	-0.187**	0.063	-0.237**	0.025	-0.502*	-0.172	-0.388***	-0.164	-0.105	0.065
	(0.150)	(0.145)	(0.093)	(0.090)	(0.114)	(0.090)	(0.277)	(0.314)	(0.065)	(0.194)	(0.098)	(0.186)
Hausman (p-val.)	(0.009)	(0.000)	(0.100)	(0.218)	(0.000)	(0.028)	(0.109)	(0.187)	(0.000)	(0.023)	(0.138)	
OID (p-val.)	(0.696)	(0.949)	(0.075)	(0.122)	(0.544)	(0.609)	(0.336)	(0.247)	(0.206)	(0.309)	(0.200)	
R^2	0.204	0.167	0.344	0.520	0.309	0.507	0.309	0.651	0.296	0.175	0.463	0.317
SEE	0.642	0.713	0.335	0.366	0.479	0.433	0.399	0.342	0.654	1.008	0.643	0.673
Observations	89	89	77	77	79	79	28	28	88	88	88	88

Notes: Dependent variable is $\ln \sigma^y$. A constant is included in all models. *, **, *** denote significance at 10, 5, and 1 percent level respectively; robust standard errors in parenthesis. See also Tables 2 and 3.

Table 5. *Cyclicality of Fiscal Policy and Economic Growth*

	(1a) LS	(1b) TSLS	(2a) WLS	(2b) WTSLS	(3a) LS	(3b) TSLS	(3c) WTSLS
$\ln CYC$	-0.362**	-0.767*	-0.341*	-0.606*	-0.297**	-1.149**	-0.439*
	(0.161)	(0.441)	(0.188)	(0.344)	(0.118)	(0.514)	(0.226)
$D^{counter} \ln CYC$	0.151	0.379	0.090	0.173			
	(0.157)	(0.331)	(0.152)	(0.215)			
$\ln GDPPW^{in}$	-0.748***	-0.798***	-1.046***	-1.016***	-0.748***	-0.895***	-1.087***
	(0.158)	(0.166)	(0.195)	(0.120)	(0.158)	(0.191)	(0.200)
$\ln HC^{prim}$	0.524**	0.568**	1.134***	1.180***	0.532	0.688**	1.163***
	(0.218)	(0.244)	(0.212)	(0.236)	(0.217)	(0.318)	(0.242)
$\ln HC^{sec}$	0.724***	0.646***	0.716***	0.567**	0.726	0.515**	0.663***
	(0.136)	(0.185)	(0.177)	(0.279)	(0.132)	(0.223)	(0.240)
Hausman [1] (p-val.)		(0.467)		(0.182)		(0.015)	(0.418)
OID [2] (p-val.)		(0.028)		(0.335)		(0.929)	(0.250)
R^2	0.400	0.363	0.370	0.358	0.395	0.268	0.365
SEE	1.051	1.100	1.203	1.121	1.048	1.332	1.107
Observations	80	80	80	80	80	80	80

Notes: Dependent variable is $\overline{\Delta \ln GDPPW}$. A constant is included in all models. *, **, *** denote significance at 10, 5, and 1 percent level respectively; robust standard errors in parenthesis. WLS denotes weighted least squares estimates, using the inverse of the variance of *CYC* as weight. (W)TSLS denotes (weighted) two stages least squares, using *NELEC* and *POLCON* as instruments for *CYC*. [1] Heteroskedasticity-robust Hausman test for exogeneity; H_0: $\ln CYC$ is exogenous. [2] Heteroskedasticity-robust test of overidentifying restrictions; H_0: *NELEC*, *POLCON* are valid instruments.

Table 6. *Cyclicality of Fiscal Policy, Output Volatility, and Economic Growth*

	(1a) LS	(1b) TSLS	(1c) TSLS	(1d) TSLS	(2) TSLS	(3a) LS	(3b) TSLS	(3c) WTSLS	(4) WTSLS
$\ln \sigma^y$	-0.994**	-2.968***	-3.689*	-2.993***	-2.174*	-0.662**	-2.101*	-1.173*	-1.416**
	(0.302)	(0.107)	(2.128)	(0.955)	(1.196)	(0.292)	(1.187)	(0.083)	(0.611)
$\ln CYC$						-0.100	-0.071	-0.194	-0.309
						(0.109)	(0.158)	(0.133)	(0.232)
$D^{counter} \ln CYC$									0.182
									(0.169)
$\ln GDPPW^{in}$	-0.821***	-1.066***	-1.155***	-1.069***	-1.065***	-0.971***	-1.058***	-1.412***	-1.364***
	(0.157)	(0.223)	(0.236)	(0.213)	(0.176)	(0.198)	(0.175)	(0.249)	(0.252)
$\ln HC^{prim}$	0.551**	0.697***	0.750***	0.699***	-0.569*	0.401***	0.587*	1.090***	1.127***
	(0.203)	(0.230)	(0.274)	(0.225)	(0.292)	(0.146)	(0.297)	(0.0186)	(0.190)
$\ln HC^{sec}$	0.580***	0.144**	-0.015**	0.138	0.446	0.598***	0.341	0.671***	0.061***
	(0.148)	(0.305)	(0.491)	(0.270)	(0.241)	(0.001)	(0.242)	(0.149)	(0.147)
$GADP$					1.007	2.067	0.861	-0.314	-0.786
					(1.562)	(0.650)	(1.642)	(1.610)	(1.463)
Hausman [1] (p-val.)		(0.023)	(0.026)	(0.011)	(0.251)		(0.302)	(0.187)	(0.041)
OID [2] (p-val.)		(0.760)	-	(0.771)	(0.644)		(0.607)	(0.692)	(0.673)
R^2	0.409	0.316	0.278	0.314	0.430	0.537	0.463	0.417	0.406
SEE	1.036	1.222	1.363	1.227	1.053	0.935	0.484	1.138	1.172
Observations	80	80	80	80	78	78	78	78	78

Notes: Dependent variable is $\overline{\Delta \ln GDPPW}$. A constant is included in all models. *, **, *** denote significance at 10, 5, and 1 percent level respectively; robust standard errors in parenthesis. WLS denotes weighted least squares estimates, using the inverse of the variance of *CYC* as weight. TSLS denotes (weighted) two stages least squares, using the following instruments for σ^y: *NELEC*, *POLCON*, *MAJ*, *PRES in* (1b), Z^σ in (1c), and both the institutional variables and Z^σ in all other columns. [1] Heteroskedasticity-robust Hausman test for exogeneity; H_0: $\ln CYC$, *GSIZE* (and $\ln DISCR$) are exogenous. [2]) Heteroskedasticity-robust test of overidentifying restrictions; H_0: *NELEC*, *POLCON* (*MAJ*, *PRES*) and $\ln POP$, *DEP*, *URBAN*.

Figure1. *Cyclicality of Fiscal Policy and Output Volatility*

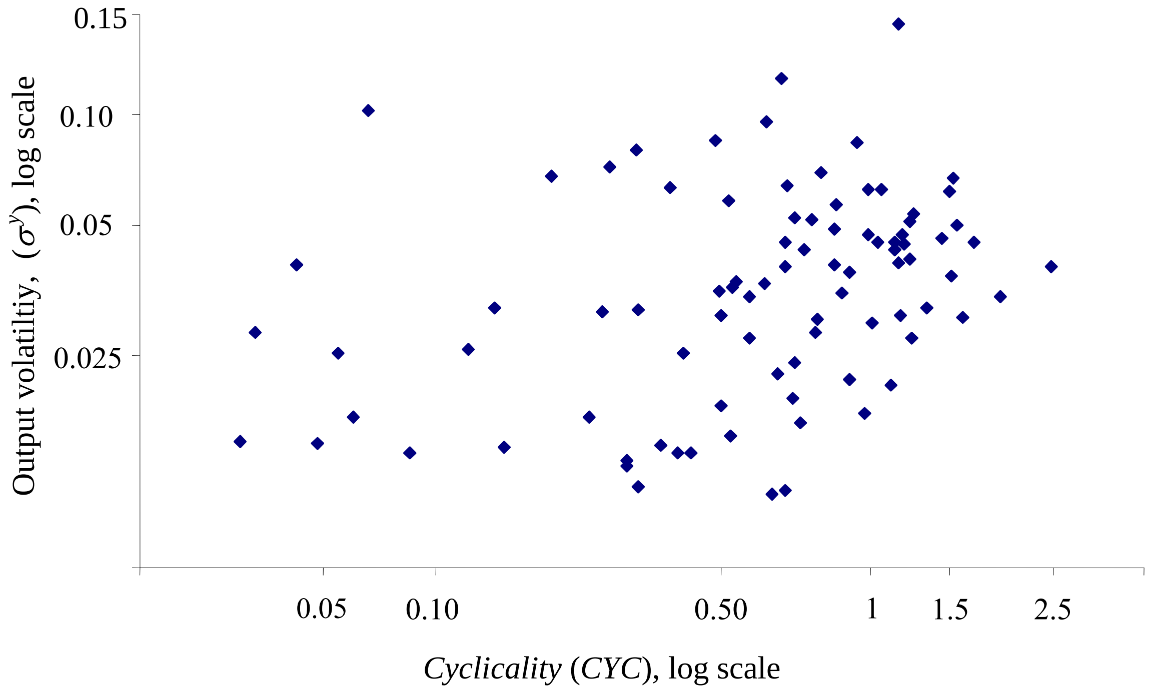

Figure 2. *Relationships between Key Variables in Empirical Models*