The Case for Restricting Fiscal Policy Discretion

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INSEAD and CEPR

March 2003

Abstract This paper studies the effects of discretionary fiscal policy on output volatility and economic growth. Using data for ninety-one countries we isolate three empirical regularities: (1) Governments that use fiscal policy aggressively induce significant macroeconomic instability; (2) The volatility of output caused by discretionary fiscal policy lowers economic growth by more than 0.8 percentage points for every percentage point increase in volatility; (3) Prudent use of fiscal policy is explained to a large extent by the presence of political constraints and other political and institutional variables. The evidence in the paper supports arguments for constraining discretion by imposing institutional restrictions on governments as a way to reduce output volatility and increase the rate of economic growth.

JEL Classification: E32, E62.

* We would like to thank Alberto Alesina, Jean Imbs, Eduardo Loyo, Roberto Perotti, Torsten Persson, two anonymous referees, participants at the CREI/CEPR Conference on Fiscal Policy, and at seminars in Columbia University, DELTA, University of Frankfurt, Humboldt University, the Institute for International Economic Studies in Stockholm, London Business School, the Federal Reserve Bank of New York, New York University, Bank of Spain, and INSEAD for helpful comments on the previous draft of the paper. Ilian Mihov gratefully acknowledges financial support from the Blue Bird Project administered by Wissenschaftskolleg zu Berlin and from the INSEAD research grant 2010-530R. Murlimanohar Ravi provided excellent research assistance. Earlier drafts of this paper were circulated under the title "The Case for Independent Fiscal Policy". Address for correspondence: Antonio Fatás, fatas@econ.insead.edu and Ilian Mihov, mihov@econ.insead.edu.

I. Introduction

Restricting the scope of discretion that policy makers can exercise comes periodically to the forefront of public debates. With respect to monetary policy the debate has led to the almost universally accepted proposition that monetary policy should be taken away from the government and given to an independent central bank. In the case of fiscal policy, the debate has gained momentum only recently and has not resulted yet in an agreement on mechanisms or institutional changes designed to improve policy outcomes. The proposed measures range from an extreme version of balanced budget constitutional amendments — as in the US — to the less restrictive limits on the budget deficit in EMU, or to discussions on how to build institutions that constrain discretion by the virtue of the political process. This paper focuses only on the last of these propositions. Namely, we study the political and institutional determinants of fiscal policy and their influence on macroeconomic performance.

A key argument for tying governments' hands by imposing various restrictions on fiscal policy is based on the assumption that discretion in fiscal policy can harm macroeconomic stability.[1] There is, however, some tension in this argument: While its potential of being destabilizing is obvious, it is also clear that fiscal policy can smooth out business cycle fluctuations by expansionary public spending or tax cuts in recessions or contractionary policy in expansions. Hence, there is an argument for leaving policy makers rather unrestricted. One of the main results in this paper is that imposing fiscal prudence by instituting political constraints on governments does indeed work. We show that in a large cross-section of countries aggressive use of fiscal policy generates undesirable volatility and leads to lower economic growth. Although we cannot show whether quantitative restrictions in the form of maximum deficits or balanced budget requirements are helpful, we do show that politically constrained policy makers generate lower volatility.

To make the case for restrictions on fiscal policy we address two questions from an empirical perspective:

(1) How harmful is discretionary fiscal policy for the economy?

(2) What are the political and institutional factors that shape fiscal policy?

[1] Of course, restrictions on fiscal policy can also be justified on the basis of stimulating fiscal responsibility on governments that, if left unrestricted, would run excessive deficits and accumulate too much debt.

We focus exclusively on changes in government spending, which are not related to the current state of the economy. Previous analysis based on spending shocks has provided many insights on the effects of fiscal policy. Blanchard and Perotti (2002) build a general framework for the study of fiscal policy shocks, Fatás and Mihov (2000) document the effects of spending shocks on consumption and employment, Alesina et al. (2002) estimate the effects of spending shocks on profits and investment, and Canzoneri et al. (2002) study the interaction between monetary and fiscal policy. In this paper, we take a slightly different approach. We start with a large set of countries and we exploit both time-series and cross-sectional information, as Alesina et al. (2002). The cross-sectional dimension in our data allows us to address in a different framework issues of endogeneity of policy responses and measurement errors. Succinctly, we use the cross-country variation in political and institutional characteristics to instrument for fiscal policy. A study based purely on time series data cannot use this information because country's political and institutional characteristics do not change very frequently. Furthermore, after extracting the policy shock we collapse the panel data into a cross section by studying only the volatility of fiscal policy shocks. We interpret this volatility as the aggressiveness with which policy-makers use fiscal policy. It is exactly this volatility that is harmful for growth.

The finding that macroeconomic volatility is linked to discretionary policy begs the question: Why do we observe cross-country dispersion in the use of fiscal policy? This question is related to the growing literature on the role of political institutions in shaping economic policy.[2] The literature on how political and electoral systems or, more generally, the *institutional environment* can moderate or magnify the incentives to use discretionary fiscal policy helps us explain the observed variation in fiscal policies. Based on the results in the paper we argue that institutional arrangements that constrain discretion via checks and balances allow nations to achieve higher rates of economic growth and reduce macroeconomic instability.

Our methodology is close in spirit to the literature on central bank independence. The well-known inflationary bias in monetary policy can be reduced or removed under certain institutional arrangements. Indeed, in the last fifteen years we have seen a clear trend towards tying governments' hands when it comes to monetary policy. This move was prompted by the theoretical

[2] See for a review Drazen (2000) or Persson (2001).

and empirical evidence that governments may use monetary policy in a rather opportunistic manner. In the case of fiscal policy there is no consensus on how to restrict discretion without hurting the potential of carefully targeted fiscal policy to smooth business cycles. This paper documents that empirically the institutional framework within which policy is conducted can be exploited to achieve a better outcome in this flexibility vs. discipline tradeoff.

The main results of the paper can be summarized as follows: (1) Highly volatile discretionary fiscal policy exerts strong destabilizing effect on the economy; (2) The volatility of output induced by discretionary fiscal policy lowers economic growth by more than 0.8 percentage points for every percentage point increase in volatility; (3) The use of fiscal policy is explained to a large extent by the lack of political constraints and by other political and institutional variables.

The next section starts with the construction of our measure of discretionary fiscal policy and proceeds with the estimation of the effects of fiscal policy on output volatility and economic growth. Section III explores the political and institutional determinants of discretionary fiscal policy. Several important issues concerning robustness and possible alternative explanations of our findings are taken up in Section IV. The last section provides discussion and concluding remarks.

II. Estimating the Effects of Discretionary Fiscal Policy

A. Constructing a Measure of Discretionary Fiscal Policy

We use the term discretionary fiscal policy to refer to changes in fiscal policy that do not represent reaction to economic conditions. In theory, it is useful to think about fiscal policy as consisting of three components: (a) automatic stabilizers, (b) discretionary fiscal policy that reacts to the state of the economy, and (c) discretionary policy that is implemented for reasons other than current macroeconomic conditions. We focus only on the last component of fiscal policy, i.e. changes in the cyclically-adjusted fiscal policy stance. It is necessary to state at the outset that there is no consensus in the literature on the appropriate

methodology for the construction of a cyclically-adjusted measure of fiscal policy.[3] The difficulty, of course, comes from the simultaneity in the determination of output and the budget. To reduce the bias introduced by this simultaneity we focus only on government spending as opposed to the budget deficit. Our choice is driven both by theoretical arguments that the political process in most countries does not allow for swift changes in discretionary spending, as well as by empirical estimates showing that spending does not react much to the cycle. The budget deficit, on the other hand, is largely affected by changes in macroeconomic conditions and therefore more vulnerable to endogeneity problems.[4]

We use annual data for ninety-one countries over the period 1960-2000 to estimate the following equation for each country:

$$\Delta G_{i,t} = \alpha_i + \beta_i \, \Delta Y_{i,t} + \gamma_i \Delta G_{i,t-1} + \ \delta_i \, \mathbf{W_{i,t}} + \epsilon_{i,t} \tag{1}$$

where G is the logarithm of real government spending, Y is the logarithm of real GDP.[5] We add to this regression various controls for government spending as well as deterministic components like time trends (W). We interpret the country-specific volatility of $\epsilon_{i,t}$ as a quantitative estimate of discretionary policy. We calculate the volatility as $\sqrt{Var_i(\epsilon_{i,t})}$ and we will denote it as σ_i^{ϵ}. This variable can be interpreted as the typical size of a discretionary change in fiscal policy for country i. The interpretation of $\epsilon_{i,t}$ as a discretionary spending shock is not new. Similar frameworks have been employed by Blanchard and Perotti (2002) using quarterly US data, and Alesina et al. (2002) with annual OECD data. The novelty of our approach is to focus *only* on the aggressiveness of discretionary policy, which we measure by the volatility of the spending shock.

In our baseline specification of equation (1) we include the contemporaneous

[3] See Alesina and Perotti (1996) or Blanchard (1993) for a discussion and criticism of alternative measures.

[4] The analysis of Chalk (2002) gives support to this hypothesis. The paper assesses the performance of alternative indicators of discretionary changes in fiscal policy by focusing on identifiable cases of changes in fiscal policy (qualitative analysis). Indicators that rely on government spending are more accurate matching the identified and observable changes in fiscal policy during the 90's in both Japan and Germany.

[5] The choice of our sample is dictated by data availability. We started the sample with 109 countries listed in Appendix C in Jones (2002). We had to drop eighteen countries from our sample either because fiscal data were not available or because the time span was too short for a meaningful estimation of equation (1). We kept ninety-one countries for which we had at least twenty-five years of data. The list of countries and data sources are described in a Data Appendix.

value of output growth and we use past values as instrumental variables to avoid the possibility of endogeneity bias. We instrument for current output growth with two lags of GDP growth, the index of oil prices, lagged inflation, and the lagged value of government spending growth. In the baseline regressions we estimate equation (1) for each country including as additional controls a time trend, inflation and inflation squared.[6]

Although the construction of this measure differs somewhat from methods used in other studies, there are also many similarities. One of the most common methods to measure discretionary changes in fiscal policy is based on Blanchard (1993). He proposes to estimate the discretionary change in policy as the difference between actual policy and the policy that would prevail under previous year's macroeconomic conditions. This measure was employed in Alesina and Perotti (1996) in a study of fiscal consolidations. Similarly, the more recent literature on the effects of fiscal policy that employs vector autoregressions deals explicitly with the identification issues and provides a more complete description of macroeconomic dynamics. The construction of the policy measure in this paper is not very different from the more general VAR approach and can also be given a straightforward interpretation in Blanchard's framework — we label as a fiscal policy shock the change in detrended government spending unexplained by past government spending or by current macroeconomic conditions. In fact, the application of Blanchard's proposal in Alesina and Perotti (1996) is equivalent to differencing the residuals of equation (1) when the latter is estimated in levels.[7] Since we are interested only in the average volatility of policy changes, it makes no difference whether we use the level of the residuals or their differences.

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[6] We include inflation to ensure that our results are not driven by high-inflation episodes in which the co-movement between real government spending and output might be due to monetary instability rather than fiscal policy. Inflation squared is included to control for possible non-linear relationship between inflation and spending. Previous versions of the paper have estimated equation (1) by various methods including: (a) using panel data techniques with fixed effects, (b) using the ratio of spending to GDP as a dependent variable, (c) using OLS rather than IV in the first stage, (d) explicit adjustment for autocorrelation, etc. The results from these variations in the first stage regressions lead to virtually identical qualitative conclusions and are available from the authors on request.

[7] See footnote 3 in Alesina and Perotti (1996) for the construction of their measure. A more recent variation of this approach is Alesina et al. (2002). For twenty OECD countries they use elasticities provided by the OECD to cyclically adjust fiscal variables. We have replicated their calculation for the twenty OECD countries in our sample and we have found that the results are very similar to those reported using our measure. Indeed the correlation between the two measures is 0.93.

B. The Effects of Fiscal Policy on Output Volatility

To study the link between discretionary fiscal policy and output volatility we exploit the cross-sectional variation in our data. Figure 1 shows the simple relationship between output volatility and the estimated variability in discretionary fiscal policy. The horizontal axis reports the logarithm of our measure of policy volatility obtained by estimating equation (1) for each country. Along the vertical axis we have plotted the logarithm of the standard deviation of output growth. The correlation between policy and output volatility is positive and highly significant.

[Insert Figure 1 here]

The formal regression analysis presented in Table 1 confirms the positive association between policy and output volatility. The general form of the regressions reported in Table 1 is as follows:

$$\log(\sigma_i^y) = \alpha + \beta \log(\sigma_i^\epsilon) + \gamma' \mathbf{X_i} + \nu_i \tag{2}$$

The dependent variable is the standard deviation of the annual growth rate of GDP per capita for each of the countries in our sample.[8] The key explanatory variable is the volatility of discretionary fiscal (σ^ϵ) policy constructed on the basis of equation (1). The additional control variables are Government Size (government consumption as % of GDP), Real GDP per Capita, and the ratio of imports and exports to GDP (Trade). Government size is included to control for the stabilizing role of fiscal policy as argued by Galí (1994) and Fatás and Mihov (2001). GDP per capita needs to be added because it is possible that poor countries have more volatile business cycle due to the lack of financial markets, for example, and at the same time poor countries may resort more often to discretionary policy. Finally, trade is included as a standard explanatory variable for output volatility and for fiscal policy as argued by Rodrik (1998).

The first two columns in Table 1 present the results from estimating equation (2) by OLS. The p-values reported in the parentheses below the point estimates reveal that both regressions indicate significance of fiscal policy at

[8] It is standard to use logs in scedastic regressions in order to ensure that the predicted standard deviation cannot be negative. Results using levels are practically identical.

Table 1. Output Volatility and Fiscal Policy

$$\log(\sigma_i^y) = \alpha + \beta \log(\sigma_i^\epsilon) + \gamma' \mathbf{X_i} + \nu_i$$

	OLS		LAD		IV		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Discretionary Fiscal Policy (σ^ϵ)	0.477 (0.000)	0.419 (0.000)	0.494 (0.000)	0.419 (0.000)	0.564 (0.000)	1.165 (0.007)	0.798 (0.000)
Government Size	-	0.285 (0.046)	-	0.280 (0.124)	-	0.698 (0.030)	0.176 (0.697)
GDP per Capita	-	-0.093 (0.039)	-	-0.111 (0.075)	-	0.257 (0.217)	0.123 (0.226)
Trade	-	0.122 (0.173)	-	0.101 (0.313)	-	-0.054 (0.752)	0.098 (0.552)
Adjusted R^2	0.482	0.535	0.306	0.365	-	-	-
Test of OID (p-value)	-	-	-	-	(0.115)	(0.636)	(0.387)
Number of countries	91	91	91	91	88	88	88

The p-values in the parentheses are based on heteroscedasticity-robust standard errors. In the IV estimation the OID test reports p-value from a test that the instruments are uncorrelated with the residuals. All regressions include an intercept. For LAD regressions pseudo-R^2 is reported.

better than the 1% level of significance. The next two columns report estimates from least absolute deviation (LAD) regressions. By estimating the median as a measure of central tendency the LAD estimator ensures that our results are not driven by outliers and thus confirm the conclusions from the first two columns. We check now that the results from the OLS and LAD estimation hold under weaker assumptions about the precision of our fiscal policy measure. First, our definition of aggressive fiscal policy is certainly a noisy one and classical measurement error problems could bias our results significantly. Second, although we do use instrumental variables to estimate equation (1) and we follow most of the literature in this area to construct the underlying policy indicator in a rather conventional manner, a skeptic might argue that some of the variation in σ_i^ϵ is still due to output volatility and not to discretionary policy.[9] To deal

[9] To justify this statement, one has to make a rather complicated argument. The reason is that

with both problems we estimate equation (2) by instrumental variables. In order to reduce the chance that the instruments themselves are driven by output volatility, we select variables that are linked to the institutional characteristics of the countries in our sample: the nature of the electoral system (majoritarian versus proportional), the nature of the political system (presidential versus parliamentary), the presence of political constraints (number of veto points in the government and the distribution of ideological preferences), and the number of elections for the executive and legislative branches.[10] In columns (5) and (6) we use these political and institutional variables to instrument for the volatility of discretionary policy. The results are quite eloquent: as one would expect the presence of a measurement error leads to attenuation bias in the OLS estimates so that our IV estimates of the effect of discretionary policy are about 15% higher than the OLS estimates in the univariate regressions (columns (1) and (5)) and the coefficient more than doubles when controls are used (columns (2) and (6)). Second, although one might expect *a priori* that these institutional characteristics explain only a small portion of the cross-sectional variation in discretionary fiscal policy, the estimates reveal that there is no deterioration in the statistical significance of the IV estimates. Finally, the last row shows that a test of the overidentifying restrictions does not reject the orthogonality of the instruments and the error terms. This is an important indicator of the appropriateness of our estimation and we will come back to this result in the section on robustness.

In the last column we modify our estimation to take into account that government size might be endogenous to output volatility. As Rodrik (1998) has argued, intrinsically more volatile economies have an incentive to set up larger governments as a way to reduce macroeconomic volatility. We deal with this endogeneity by using as instruments for government size the dependency ratio, urbanization rate, the logarithm of GDP per capita and the logarithm of total population.[11] The results are in column (7) of Table 1 and again we document that volatile discretionary policy generates significant macroeconomic instability

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even though we use lagged values as instruments in the first-stage regressions, the residuals are calculated by using the *actual* contemporaneous output value. Lagged instruments are only needed to estimate the slope parameters consistently. This means that under rather general conditions the residuals are "clean" both from projected and from unexpected output movements.

[10] We defer the discussion of these instrumental variables to the next section of the paper.

[11] The justification of these variables as instruments is standard. The reader is referred to Alesina and Wacziarg (1998), Fatás and Mihov (2001), and Rodrik (1998) for discussion.

(with a t-statistic of 4.10).

We can now interpret the coefficients from an economic point of view. Since both variables are in logs the coefficients report the elasticity of output volatility with respect to the volatility of discretionary policy. From the last two columns we can see that one percent reduction in policy volatility leads to between 0.8% to 1.2% decline in output volatility. If we take 0.8% as a conservative estimate we can illustrate these effects by a specific example: If Portugal brings down its policy volatility (3.9%) to that of Spain (2.6%), which is a 33% reduction in the standard deviation of the residual volatility, then it will see its output volatility go down by 26% (33% times 0.8) from 2.65% to slightly less than 2%. In the following section we investigate how a change in volatility of this magnitude can affect economic growth. But even if we consider only the effect of policy volatility on the second moments of output series, it is clear from these results that discretionary fiscal policy, shaped to a large extent by institutional and political factors, does induce significant fluctuations in economic activity.

C. The Effects of Policy Volatility on Economic Growth

The relationship between macroeconomic volatility and economic growth is a very intricate one. On the one hand, theories emphasizing irreversible investment imply that countries with higher volatility will have lower levels of investment and as a consequence will have lower growth. On the other hand, high growth economies might be based on risky technologies and therefore may experience sharp shifts in economic volatility. These arguments have been tested by Ramey and Ramey (1995) in their empirical study of the link between volatility and growth. The setup of their investigation is somewhat similar to ours — they use the squared innovation of the residuals from a government spending equation to fit a process for the volatility of the residual from a growth equation and then study the effects on average output growth. Although the question we approach here is similar, our main concern is whether political and institutional factors that shape discretionary fiscal policy can be responsible for business cycle volatility that is harmful to growth.

[Insert Figure 2 here]

In our sample there is negative association between average growth rates

($\overline{\Delta y_i}$) and output volatility (σ_i^y) as Figure 2 illustrates.[12] The correlation, however, seems to be sensitive to outliers like the Democratic Republic of Congo (ZAR). To investigate this relationship we run the following regression:

$$\overline{\Delta y_i} = \alpha + \lambda \log(\sigma_i^y) + \beta' \mathbf{X_i} + u_i \tag{3}$$

The first column shows that the simple correlation is almost significant at the 5% level, but the p-value doubles once Congo is removed from the sample.[13] When we introduce standard controls ($\mathbf{X_i}$) like the initial level of GDP per capita and the initial stock of human capital measured as the percentage of males over 25 with primary and secondary education — as in column 2 of Table 2 — we do not find significant relationship between growth and volatility.

Table 2. Growth and Fiscal Policy

$$\overline{\Delta y_i} = \alpha + \lambda \log(\sigma_i^y) + \beta' \mathbf{X_i} + u_i$$

	OLS		IV	3SLS
	(1)	(2)	(3)	(4)
Volatility (σ^y)	-0.984	-0.548	-3.671	-
	(0.051)	(0.351)	(0.002)	
Predicted Volatility ($\widehat{\sigma^y}$)	-	-	-	-4.054
				(0.003)
GDP per Capita	-	-0.671	-1.069	-1.121
		(0.046)	(0.011)	(0.001)
Primary Education	-	0.031	0.016	0.019
		(0.003)	(0.227)	(0.081)
Secondary Education	-	0.043	0.017	0.028
		(0.089)	(0.578)	(0.140)
Adjusted R^2	0.054	0.126	-	0.270
Number of observations	74	74	74	74

The p-values in the parentheses are based on heteroscedasticity-robust standard errors.
All regressions include an intercept.

[12] We had to drop seventeen countries from the sample for the estimation in this section because data were not available for one or more of the regressors. See the Data Appendix for details.

[13] This result is not reported in the table.

To evaluate the importance of discretionary fiscal policy we use our measure of policy volatility (σ_ϵ) as an instrument for output volatility. Column 3 reports the coefficient on output volatility (λ) when output volatility is instrumented by discretionary fiscal policy. The estimate is highly significant and negative and implies that the volatility in output induced by discretionary fiscal policy has negative consequences for growth. In addition to showing that aggressive fiscal policy is bad for economic growth, column 3 lends support to the claim that the measure of policy volatility is not driven by output volatility. If the association between policy and output volatility identified in the previous section was due to contamination of our measure by output volatility, then the use of our measure as an instrument in the growth will simply replicate the variation in output volatility and will not lead to changes in the coefficient. Instead the coefficient on output volatility changes sharply and significantly in the expected direction.

Column 4 of Table 2 presents yet another way of judging the importance of political variables in determining policy and output volatility and, through this channel, economic growth. We construct first a fitted value for our discretionary fiscal policy measure based on a political economy regression that includes the four politico-institutional variables. In other words, we are constructing a measure of fiscal policy volatility explained solely by these four variables. On the basis of this measure we construct the fitted value for output volatility from a simple cross-country regression of the standard deviation of output growth on the fitted policy volatility. This artificial construct — the portion of output volatility explained by the institutionally determined discretionary fiscal policy — is used as a regressor ($\widehat{\sigma^y}$) in a standard cross-sectional growth regression. The chain through which policy and institutions affect growth can be schematically represented as

Political and institutional setup
$\Downarrow$
Discretionary fiscal policy
$\Downarrow$
Output volatility
$\Downarrow$
Growth

The results in column 4 confirm our previous finding that output volatility caused by discretion in fiscal policy has a strong negative effect on economic

growth.[14] Figure 3 presents the link between policy-induced output volatility and economic growth and shows that the result is not driven by outliers.

[Insert Figure 3 here]

How does this compare to the findings of Ramey and Ramey (1995)? In their paper they use two samples — one with 92 countries and a smaller sample of the OECD economies. The reported coefficients for the 92 countries are never significant at the 5% level, but for the OECD economies they often are. More interestingly, despite the differences in methodology and sample definitions our results are quite close to theirs. Since we are using log volatility we have to divide the coefficients by the average output volatility in order to transform them into levels. The average output volatility in our sample is 4.42%, which implies that the adjusted coefficient in column 3 is -0.8. The range for λ in Ramey and Ramey (1995) is from -0.53 to -0.43, which is relatively close to our estimates. When we estimate the fitted-volatility regression then the magnitude of the coefficient changes to -0.9. Admittedly, in absolute magnitude this might be an upper bound on the effect of output volatility on growth.

III. Political and Institutional Determinants of Discretionary Fiscal Policy

The use of political and institutional characteristics as instruments in the previous regressions requires some elaboration. First, it is necessary to probe deeper in the determinants of discretionary policy to ensure that the use of these variables as instruments is theoretically justifiable and, second, the link between institutions and policy outcomes is of independent interest to economists. Indeed, a still-growing empirical and theoretical literature summarized by Drazen (2000), Persson (2001), and Persson and Tabellini (2001) explores how policy is shaped by the characteristics of the electoral and political systems.

In choosing the instruments we focus on how political institutions affect the motivation to use discretionary fiscal policy. There are four groups of theories

[14] The standard errors are adjusted in a sequential two-step procedure to account for the use of a generated regressor. See Murphy and Topel (1985).

that explain the use of discretionary fiscal policy: (i) the opportunistic electoral cycle (Nordhaus, 1975 and Rogoff and Sibert, 1988); (ii) the partisan electoral cycle (Hibbs, 1977 and Alesina, 1987); (iii) idiosyncratic changes, incompetence and greediness (Stokey, 2002); (iv) non-adjustment or delayed adjustment to shocks due to inability to build coalitions (Alesina and Drazen, 1991, and Milesi-Ferretti, Perotti and Rostagno, 2002).[15]

The motivation to resort to fiscal policy explained by these theories is affected by the political regime or the institutional environment in each country. Prime candidates for explaining the cross-country variation in the use of policy discretion are the political and electoral systems. Persson (2001) argues that majoritarian systems will have more pronounced electoral cycles because the career concerns of the incumbent become stronger as a result of higher individual accountability. Hence majoritarian regimes should be associated with more volatile discretionary policy and more pronounced electoral cycles. On the other hand, Alesina and Perotti (1994) argue that proportional systems lead to coalitions and fiscal deadlocks which delay stabilizations. This implies that proportional systems will be associated with larger *measured* volatility of policy due to non-adjustment to shocks. Electoral rules can also affect the likelihood and the shape of the partisan business cycle. We might expect that majoritarian systems, where single-party governments are more likely, will lead to more pronounced changes in ideology of the executive and therefore larger changes in fiscal policy — hence majoritarian regimes will be associated with more volatility due to partisan reasons. As one can see the overall effect of the electoral system on policy volatility is unclear — proportional systems delay stabilization and thus create more volatility in the unexplained portion of fiscal policy, while majoritarian systems are associated with more volatility either because of career concerns in an opportunistic model or because of the higher likelihood of single-party governments in a partisan model. To operationalize differences in electoral systems, we use a standard measure from Persson and Tabellini (2001) complemented with information from the Database of Political Institutions. The variable takes value 1 for majoritarian and 0 for proportional electoral systems.

The political system (presidential versus parliamentary) can also have an effect on fiscal policy. The discussion in Persson and Tabellini (2001) implies

[15] It is impossible to do justice to this literature here and we refer the reader to the excellent summaries in Alesina, Roubini and Cohen (1997), Drazen (2000), and Persson and Tabellini (2000).

that presidential systems must be associated with more volatile policy. In a parliamentary system the executive is supported by the parties in the parliament and therefore is constrained in the implementation of policy by the threat of a no-confidence vote. The president does not face the confidence requirement and hence can alter policy either for opportunistic or partisan reasons. Therefore, presidential regimes must be associated with more volatile discretionary policy. We use a dummy variable that determines whether the country has a presidential or a parliamentary system. The variable takes value 1 for presidential systems and 0 for parliamentary systems.

Another institutional characteristic is related to the constraints faced by governments in the process of policy implementation. Persson, Roland, and Tabellini (1997) show that separation of powers combined with appropriate agenda-setting rules can lead to significant improvement in equilibrium outcomes by reducing the rents extracted by politicians. In general, the theoretical literature in this area has focused on the composition and size of government spending, but it is safe to argue that countries with more constrained governments should also experience less volatility in discretionary policy. We use the indicator of political constraints constructed by Henisz (2000) that captures the extent to which the executive faces political constraints to implement his or her policy. The variable *Political Constraints* is based on a time-invariant or at least rarely changing coding of the number of veto points among various branches of government (executive, legislature, judiciary) and also on the ideological alignment across branches. Obviously the ideological alignment component can change with every election or every new appointment, but the main cross-sectional variation comes from the number of institutionally embedded veto powers.

Finally, the frequency of elections matters. On the one hand, elections open up the possibility of pre- or post-electoral policy manipulation and therefore more volatility due to the electoral cycle. On the other hand, elections fundamentally keep policy makers accountable (as in Ferejohn, 1986) and thus moderate the incentives to engage in idiosyncratic or opportunistic policy manipulation. We include the average number of legislative and executive elections for each country in our sample in order to control for the obvious possibility that discretionary policy is driven by the electoral cycle and also to determine the importance of accountability of politicians. Since the two factors work in opposite directions the net effect is ambiguous.

To explain the cross-section variation in the constructed measure of dis-

cretionary fiscal policy we use the four institutional variables in the following regression:

$$\log(\sigma_i^{\epsilon}) = \alpha + \beta' \mathbf{X_i} + u_i \tag{3}$$

where X_i are the four identified institutional determinants of discretionary fiscal policy. This is indeed the specification of the first-stage regression in the IV estimation from Section II.B.[16]

Table 3. Determinants of Fiscal Policy

$$\log(\sigma_i^{\epsilon}) = \alpha + \beta' \mathbf{X_i} + u_i$$

	(1)	(2)	(3)	(4)	(5)
Political constraints	-1.869	-	-	-	-1.377
	(0.000)				(0.000)
Majoritarian	-	-0.116	-	-	-0.004
		(0.569)			(0.969)
Presidential	-	-	0.942	-	0.415
			(0.000)		(0.030)
Number of elections	-	-	-	-1.065	-1.219
				(0.135)	(0.049)
Adjusted R^2	0.538	-0.006	0.349	0.012	0.550
Number of observations	90	88	90	90	88

The p-values in the parentheses are based on heteroscedasticity-robust standard errors. All regressions include an intercept.

Table 3 displays the results. Whether included separately or in a multivariate regression only two of the variables are significant — the nature of the political system and the degree of political constraints. The explanatory power of political constraints is particularly striking: Alone it can explain over 50% of the cross-sectional variation in the policy measure. Running the regression with all variables (column (5)) changes the results only slightly for *Political Constraints* and *Presidential*.

How do these results match up with the basic politico-economic theories? The nature of the electoral system — majoritarian or proportional — does

[16] There is no political data for Hong Kong and no data on the electoral system for Chad and Guinea-Bissau. This explains the differences in the number of observations across estimations.

not seem to affect discretionary fiscal policy significantly. The sign is always negative, which lends mild support to the claim that coalition governments that are normally observed in countries with proportional electoral rules adjust slowly to changes in the economic environment and thus create more volatility in discretionary policy. Again, the result should not be over-interpreted as it is not statistically significant from zero at conventional levels. Interestingly the number of elections has a negative and significant coefficient, which is consistent with the view that elections hold politicians accountable.[17] The coefficient estimates for the other two variables — political constraints and political systems — conform with our priors. Presidential systems are more volatile while countries with a large number of political constraints experience less volatility in discretionary fiscal policy.

IV. Robustness of The Results

A. How Appropriate Are the Instruments?

The use of institutional characteristics to instrument for policy volatility presumes that these characteristics do not affect output volatility directly. We have already tested this assumption in Section II.B. by using a test of overidentifying restrictions — had our instruments been important also in the determination of output volatility directly, then the test would have rejected the orthogonality of the errors and the instruments. Although we find that the test has never rejected our maintained assumption, we recognize that under certain conditions the test may have low power against plausible alternatives. An important case is when only one of the instruments is correlated with the instrumented variable. In this case, effectively we do not have overidentification and the errors will be orthogonal to this particular instrument by construction.

To address this issue we re-estimate equation (2) by removing one instrument at a time from the list of instruments used in Table 1 column (7) and including it as an exogenous variable in the determination of output volatility.[18] From

[17] One has to be careful interpreting this result because when we restrict our sample to those countries considered to be democracies, the effect if the number of elections becomes insignificant.

[18] Hall and Jones (1999) also use this approach to test the appropriateness of their instruments.

Table 4. Robustness Results

$$\log(\sigma_i^y) = \alpha + \beta \log(\sigma_i^\epsilon) + \lambda \text{ Added Variable} + \gamma' \mathbf{X_i} + \nu_i$$

	Added variable	β	λ	OID Test p-value
1.	Majoritarian	0.799	0.01	0.263
		(0.000)	(0.990)	
2.	Presidential	0.896	-0.347	0.895
		(0.003)	(0.171)	
3.	Political constraints	0.651	-0.591	0.469
		(0.000)	(0.128)	
4.	Number of elections	0.761	-0.637	0.546
		(0.001)	(0.172)	
5.	Political constraints	0.644	-0.498	0.499
		(0.000)	(0.178)	
	and number of elections		-0.508	
			(0.221)	

The sample includes 88 countries for the period 1960-2000. The p-values in the parentheses are based on heteroscedasticity-robust standard errors. All regressions include an intercept. The OID test reports p-value from an overidentification test that the instruments are uncorrelated with the residuals.

the list of our four instruments there are two that are of particular concern: (i) political constraints can affect output volatility through different channels like monetary policy; (ii) the number of elections can also be correlated with output fluctuations via the influence of elections on monetary policy. In Table 4 the first four specifications estimate the main regression of output volatility on discretionary fiscal policy by including one instrument at a time as an exogenous variable.[19] All results are consistent with our overidentification tests — the inclusion of the institutional variable does not affect the coefficient on policy volatility. Furthermore, none of the coefficients on the additional variable is significant at conventional levels. The last specification (5) demonstrates that leaving out both political constraints and the number of elections has no qualitative effect on our previous conclusions.

[19] The specification for these regressions is based on the one in column (7), Table 1. The results for the controls are suppressed to reduce clutter.

In summary, from the OID tests of Table 1 and the results of Table 4 we conclude that the political economy instrumental variables are correlated predominantly with discretionary fiscal policy — a statement, which is indeed plausible *a priori*. The use of these instruments can be justified with the possibility of a measurement error in the policy variable. This interpretation is consistent with the increase in the coefficient estimates in the IV estimation relative to OLS.

B. Sub-sample stability: Large Changes, OECD, Rich and Poor Countries.

Most of the variation in political constraints occurs between the rich and the poor countries. Both from a theoretical perspective and from a policy point of view it is important to determine whether the positive relationship between policy and output volatility holds also *within* each income group. To check this relationship we arrange in Table 5 estimates for the main regressions in three groups: OECD, rich, and poor countries. In our sample there are twenty-five OECD members. The split between rich and poor is at 9,000 US dollars GDP per capita in the 1990s, which selects approximately the top third of the income distribution. In addition we want to verify once more that our results are not driven by outliers. This issue is tackled in column (4) of Table 5, where we eliminate from the sample all countries that have reported for at least one change in the ratio of government consumption to GDP which is greater than five percentage points.[20]

We start the analysis with Panel A, where we report the estimates of the volatility equation using the same instruments and controls as in column 7 of Table 1. For the OECD sample and for the rich countries the coefficient on policy volatility has the same sign but it is smaller in magnitude compared to the full sample. The coefficient estimate for poor countries is close to the full-sample estimate. For these three sub-samples the coefficients on policy volatility are significant at the 5% level of significance. When we exclude those countries that have experienced large changes in government size — column 4 — we still find strong positive relationship between policy aggressiveness and macroeconomic instability.

[20] A less stringent test would be to eliminate only the specific observation while still retaining the rest of the series for the country. We have conducted this estimation and our results are even sharper than those reported in Tables 1 through 5 in the paper. These results are available from the authors upon request.

Table 5. Robustness: Large Changes, OECD, Rich and Poor countries.

	OECD	Rich	Poor	Excluding countries with large changes
	(1)	(2)	(3)	(4)
A. Volatility Regressions. $\log(\sigma_i^y) = \alpha + \beta \log(\sigma_i^\epsilon) + \gamma' \mathbf{X_i} + \nu_i$				
Discretionary Fiscal Policy (σ^ϵ)	0.490	0.323	0.741	0.836
	(0.026)	(0.017)	(0.003)	(0.001)
OID Test p-value	0.133	0.384	0.072	0.357
Number of Observations	25	28	60	65
B. Growth Regressions. $\overline{\Delta y_i} = \alpha + \lambda \log(\sigma_i^y) + \beta' \mathbf{X_i} + u_i$				
Volatility (σ^y)	-3.306	-1.793	-6.352	-3.738
	(0.332)	(0.000)	(0.013)	(0.020)
Number of observations	24	28	47	59
C. Political Determinants of Fiscal Policy $\sigma_i^\epsilon = \alpha + \beta' \mathbf{X_i} + u_i$				
Political Constraints	-1.711	-2.205	-0.627	-1.159
	(0.000)	(0.001)	(0.025)	(0.001)
Presidential	0.189	0.500	0.159	0.520
	(0.423)	(0.253)	(0.372)	(0.020)
Majoritarian	0.263	0.197	-0.203	0.060
	(0.169)	(0.411)	(0.066)	(0.556)
Number of elections	-1.315	-0.952	-1.241	-1.440
	(0.091)	(0.412)	(0.051)	(0.033)
Adjusted R^2	0.480	0.428	0.176	0.587
Number of observations	25	28	60	65

The p-values in parentheses are based on heteroscedasticity-robust standard errors. The regressions in Panels A and B are estimated by instrumental variables as in Table 1 (column 7) and Table 2 (column 3). The results for the controls are suppressed. See the Data Appendix for the list of countries.

In panel B we explore the sensitivity of the growth regressions when discretionary fiscal policy is used to instrument for output volatility. The magnitudes of the coefficients on output volatility are quite different in this case. This difference persists even after dividing the coefficients by the average output volatility: In the poor countries the coefficient implies that every percentage point reduction in policy-induced output volatility leads to 1.28% increase in the average growth rate, while in the rich countries the effect is only 0.51%. As column 4 documents, excluding countries with large jumps does not affect the main result reported for the full sample in Table 2.

The last panel in Table 5 looks at the political and institutional determinants of discretionary fiscal policy. Somewhat surprisingly political constraints matter mostly in the rich and OECD countries, and none of the other coefficients are significant in these sub-samples. It is worth noting the high explanatory power of political constraints for the rich countries and for the OECD group - the t-statistics are about 4 and this variable together with the other political variables explains over 40% of the variation in discretionary policy. Furthermore, when we limit the sample to countries with smoother changes in government consumption relative to GDP, we find even stronger confirmation of one of our key hypotheses — countries with more political constraints on the executive have enjoyed lower policy volatility. For this sub-sample the explanatory power of political variables for policy aggressiveness is an impressive 58%.

V. Conclusion

The key conclusion from this paper is that the aggressive use of discretionary fiscal policy amplifies business cycle fluctuations and harms economic growth. To avoid any endogeneity bias caused by the fact that the variable that captures discretionary fiscal policy might contain changes in fiscal policy in response to business cycle conditions and to deal with possible measurement errors, we use instrumental variables. Our instruments come from the political economy literature on the determinants of fiscal policy and, in doing so, we also explore the causes and mechanisms of discretionary fiscal policy. The use of these instruments together with a large battery of additional tests confirms the robustness of our result. In all cases, more aggressive discretionary fiscal policy is associated with more volatile business cycle and slower rates of economic growth.

A careful look at our use of instruments reveals interesting connections between political economy variables and fiscal policy. We present evidence that more political constraints lead to less frequent use of discretionary policy. This result is particularly strong in the group of the rich countries. To the extent that reduced volatility of the business cycle has negative welfare effects, we conclude that our results show the benefits of introducing restrictions on fiscal policy discretion.

There are still many open questions that are unanswered by our analysis. The most important one is how to design institutions that restrict fiscal policy without eliminating any of the automatic stabilizers. While a similar analysis in monetary policy led to the conclusion that monetary policy had to be made independent of governments, in the case of fiscal policy, its goals are much more complex and touch on many macroeconomic aspects that go beyond the stabilization of business cycles. For our recommendations to be implementable it is necessary to be able to separate between the stabilizing and the other roles of fiscal policy before restrictions can be discussed in a meaningful manner.

One possible criticism of the conclusions in the paper is that institutions are selected optimally to reflect differences in social preferences and macroeconomic fundamentals. Nations set up their institutions to maximize a welfare function that consists of various trade-offs. One trade-off, as in monetary policy, is between flexibility and discipline. Indeed volatility might be undesirable but the society might like to give the government more flexibility so that societal concerns about a sharp increase in inequality, for example, can be met immediately by a change

in fiscal policy. If institutions are too rigid, then it may take too long to induce an institutional change that will respond to the social demands for greater redistribution. In this logic, it is entirely plausible that fiscal policy is granted some discretion to respond in a timely manner to changes in social preferences.

This potential criticism although valid simply pushes the researcher to probe deeper — what is the social welfare function that can justify the institutional heterogeneity that we observe across countries? We believe that together with the new political economy literature the evidence in this paper is only the start of an investigation in the direction of understanding institutions and their role for macroeconomic performance.

VI. References

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VII. Data Appendix

We use annual data for ninety-one countries over the period 1960-2000. The choice of our sample is dictated by data availability. We started with a sample of 109 countries listed in Appendix C in Jones (2002). Eighteen countries in this sample had to be dropped either because fiscal data were not available or because the time span was too short for a meaningful estimation of time-series regressions in the paper. We keep the following ninety-one countries for which we have at least twenty-five years of data.

List of Countries

Algeria	Germany*	Niger
Argentina	Ghana	Nigeria
Australia*	Greece*	Norway*
Austria*	Guatemala	Pakistan
Bangladesh	Guinea-Bissau	Panama
Belgium*	Haiti	Papua New Guinea
Benin	Honduras	Paraguay
Bolivia	Hong Kong	Peru
Botswana	Iceland*	Philippines
Brazil	India	Portugal*
Burkina Faso	Indonesia	Rwanda
Burundi	Ireland*	Senegal
Cameroon	Israel	Singapore
Canada*	Italy*	South Africa
Central African Rep.	Jamaica	Spain*
Chad	Japan*	Sri Lanka
Chile	Kenya	Sweden*
Colombia	Korea, Rep.*	Switzerland*
Congo, Dem. Rep.	Lesotho	Syria
Congo, Rep.	Madagascar	Thailand
Costa Rica	Malawi	Togo
Cote d'Ivoire	Malaysia	Trinidad and Tobago
Denmark*	Mali	Tunisia
Dominican Rep.	Mauritania	Turkey*
Ecuador	Mauritius	United Kingdom*
Egypt	Mexico*	United States*
El Salvador	Morocco	Uruguay
Fiji	Netherlands*	Venezuela
Finland*	New Zealand*	Zambia
France*	Nicaragua	Zimbabwe
Gabon		

For Table 5 in the paper the following definitions apply: The countries with an asterisk are members of the OECD. The rich countries are those that have GDP per capita above 9000 US dollars (i.e. those in the top 30th percentile). These are the OECD members excluding Mexico and Turkey, and including Argentina, Hong Kong, Israel, Mauritius, Singapore, and Trinidad and Tobago. The countries with large jumps in the ratio of government spending to GDP that are excluded in the estimation of the regressions in column (4) are: Argentina, Burkina Faso, Bangladesh, Brazil, Central African Republic, Republic of Congo, Egypt, Gabon, Guinea-Bissau, Indonesia, Israel, Morocco, Mauritania, Malawi, Nigeria, Nicaragua, Papua New Guinea, Rwanda, Senegal, Togo, Trinidad and Tobago, Democratic Republic of Congo, Zambia, Zimbabwe.

Data series used in the country time-series regressions:

Growth rate of real government consumption — Calculated as the difference in the logarithm of general government final consumption expenditure (% of GDP) adjusted for the growth rate of real GDP (which is described below). General government final consumption expenditure includes all government current expenditures for purchases of goods and services (including compensation of employees). It also includes most expenditures on national defense and security, but excludes government military expenditures that are part of government capital formation. Data are from World Development Indicators, CD-ROM 2002. Series identifier in the original data set: *General government final consumption expenditure (% of GDP) (NE.CON.GOVT.ZS).*

Growth rate of real government consumption for Germany, Israel and Panama — These series are calculated using data from the International Financial Statistics CD-ROM applying the same methodology as above. We use IFS data for these three countries because of the following reasoning: (a) For Germany WDI does not report any data prior to 1972. The IFS series goes back to 1960 and in the period of the overlap (1972-2000) the values are very close to the WDI data. (b) For Panama we have the same reasoning with WDI availability starting in 1980. (c) For Israel the WDI series has a jump of seven percentage points in 1965, which is absent in the IFS series. These series are from International Financial Statistics, CD-ROM December 2002. Series identifiers in the original data set: *GOVERNMENT CONSUMPTION EXPEND.(...91F..ZF...)* and *GROSS DOMESTIC PRODUCT SA (...99B..ZF...).*

Growth rate of real GDP — Calculated as the difference in the logarithm of real GDP in constant local currency units from World Development Indicators, CD-ROM 2002. Series identifier in the original data set: *GDP (constant LCU) (NY.GDP.MKTP.KN).*

Growth rate of real GDP for Germany 1960-1972 — Calculated as the difference in the logarithm of GDP volume (index number) from International Financial Statistics, CD-ROM December 2002. Series identifier in the original data set: *GDP VOL. (1995=100) (13499BVRZF...).*

Inflation — Calculated as the difference in the logarithm of the GDP deflator from World Development Indicators, CD-ROM 2002. Series identifier in the original data set: *GDP deflator (NY.GDP.DEFL.ZS).*

Inflation for Germany 1960-1972 — Calculated as the difference in the logarithm of the GDP deflator. The GDP deflator is obtained by converting the GDP volume index described above to real GDP in local currency units and then dividing nominal GDP by this series. The GDP series are obtained from International Financial Statistics, CD-ROM December 2002. Series identifiers in the original data set: *GDP VOL. (1995=100) (13499BVRZF...)* and *GROSS DOMESTIC PRODUCT SA (13499B.CZF...).*

Index of oil prices — logarithm of Petroleum spot price from International Financial Statistics, CD-ROM December 2002. Series identifiers in the original data set: *Average crude price (US dollars per barrel, 00176AAZZF...).*

Data series used in the cross-sectional regressions:

Real GDP per capita — Real GDP per capita on Purchasing Power Parity basis from Penn World Tables (version 6.1). Series identifier in the original data set: *rgdpch.*

Government size — logarithm of the ratio of government consumption to GDP. The definition of the series are the same as those used in the construction of the growth rate of real government consumption described above.

Trade — logarithm of the sum of imports and exports as % of GDP from World Development Indicators, CD-ROM 2002. Series identifier in the original data set: *Exports of goods and services (% of GDP) (NE.EXP.GNFS.ZS)* and *Imports of goods and services (% of GDP) (NE.IMP.GNFS.ZS).*

Political system — dummy variable that takes a value of 1 for presidential

regimes and 0 for parliamentary regimes. Data for fifty-one countries are from Persson and Tabellini (2001). Series identifier in the original data set: *pres*. From the Database of Political Institutions for forty countries. The DPI variable *system* is recoded from the original 0, 1, 2 values to a dummy variable that takes a value of 1 for presidential regimes. Series identifier in the original data set: *System*.

Electoral system — dummy variable that takes a value of 1 for majoritarian systems and 0 for proportional systems. Data for fifty-one countries are from Persson and Tabellini (2001). Series identifier in the original data set: *maj*. From the Database of Political Institutions for forty countries. Series identifier in the original data set: *Pr*.

Number of elections — the average number of elections over the time period for which macroeconomic data are available. The series is constructed as the sum of legislative and executive elections from the Database of Political Institutions. Series identifiers in the original data set: *legelec* and *execelec*.

Political constraints — from Henisz (2000). Updated from author's web-site. Series identifier in the original data set: *POLCONV_2002*.

Dependency ratio — Age dependency ratio is the ratio of dependents–people younger than 15 and older than 64–to the working-age population–those ages 15-64. From World Development Indicators, CD-ROM 2002. Series identifier in the original data set: *Age dependency ratio (dependents to working-age population) (SP.POP.DPND)* .

Urbanization — Urban population as % of total population. From World Development Indicators, CD-ROM 2002. Series identifier in the original data set: *SP.URB.TOTL.IN.ZS—Urban population (% of total)*.

Population — From World Development Indicators, CD-ROM 2002. Series identifier in the original data set: *Population, total (SP.POP.TOTL)*.

Primary and secondary schooling of males over 25 — from Barro and Lee (2000). Downloaded from the NBER web-site.

Figure 1. Discretionary Fiscal Policy and Output Volatility

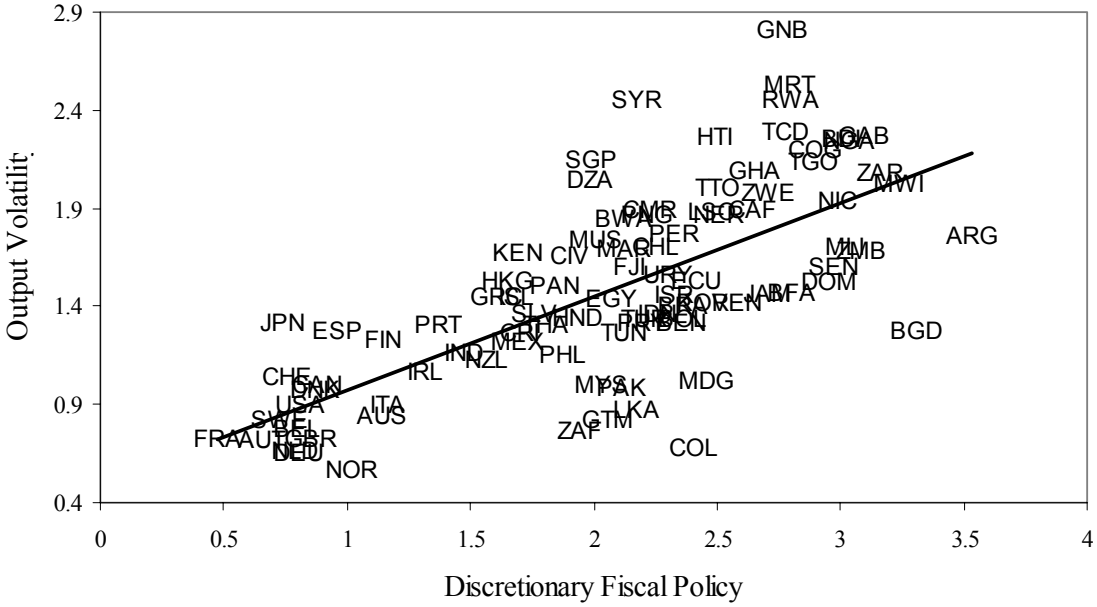

Figure 2. Volatility and Growth

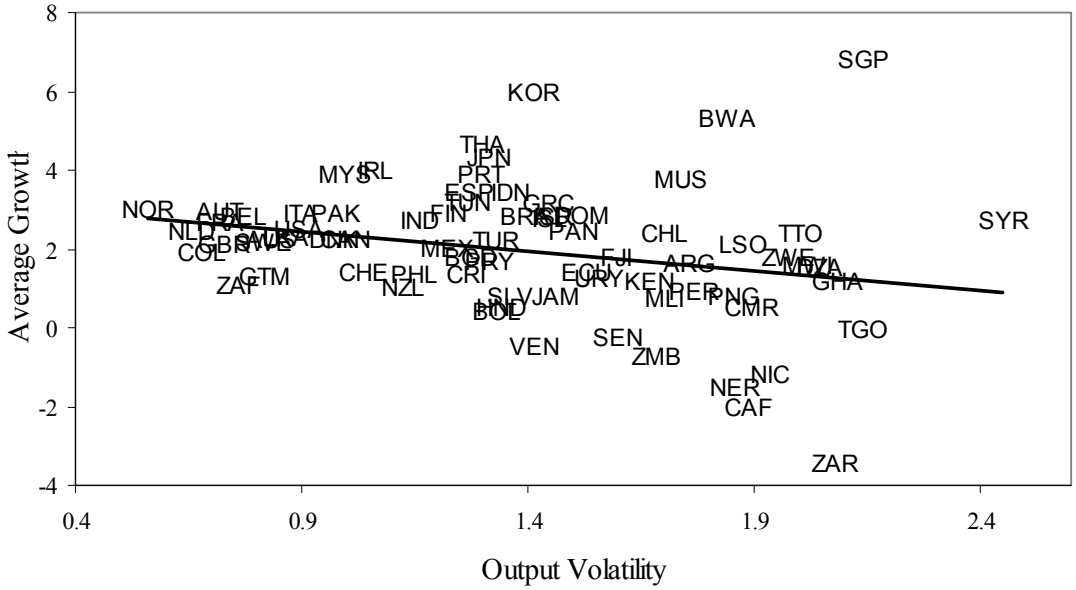

Figure 3. Policy-Induced Volatility and Growth
(Partial Correlation)