Policy Volatility, Institutions and Economic Growth

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Abstract: In this paper we present evidence that policy *volatility* exerts a strong and *direct* negative impact on growth. Using data for 93 countries, we construct measures of policy volatility based on the standard deviation of the residuals from country-specific regressions of government consumption on output. Undisciplined governments that implement frequent and large changes in government spending unrelated to the state of the business cycle generate lower economic growth. We employ both instrumental variables and panel estimation to address concerns of omitted variables and endogeneity. A one-standard-deviation increase in policy volatility reduces long-term economic growth by about 0.74% in the panel regressions, and by more than one percentage point in the cross-section.

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I. Introduction

The current economic crisis has generated a renewed interest in the role of macroeconomic policy as a stabilizing tool. In particular, fiscal policy is back in fashion after years when it was considered as too slow and ineffective. While fiscal and monetary policy can have beneficial short-term effects, there are also reasons to worry about potential adverse long-term effects of policies that are too aggressive. In this paper we focus on fiscal policy and look at one potential long-term cost: if used too often and at the wrong time, it can generate unnecessary volatility and lower growth.[1]

The growth literature has already looked at the role of fiscal policy for long-term economic growth. However, it generally considered fiscal policy in levels by looking at government size, tax rates, and the level of debt. Interestingly, in most cases the significance of these fiscal policy variables is very low and it disappears once the growth regression includes additional controls like the quality of political institutions. We show that if one considers the volatility instead of the level of fiscal policy, there is a robust negative relationship between discretionary changes in government spending and economic growth. This relationship is robust to the introduction of many control variables, including measures of institutional quality.

Our results are related to two different strands of literature on economic growth: one that looks at the link between volatility and growth and one that studies the determinants of growth by running a horse race among a large set of macroeconomic variables, some of which are related to economic policies.

The relationship between volatility and growth has been studied many times before but the debate is still open. There is evidence of a negative relationship between volatility and growth (Kormendi and Meguire (1985) and Ramey and Ramey (1995)) but the evidence is weak. One of the reasons why this relationship

[1] Some of the effects of fiscal policy on the economy have been documented in a large body of literature. For example, a stream of papers starting with Blanchard and Perotti (2002) have analyzed the dynamic effects of fiscal policy on the economy and have produced estimates of the so-called fiscal policy multipliers. Our approach here is different. We also study changes in fiscal policy that are unrelated to the state of the business cycle either because they are not timed properly or because they are motivated by factors other than the macroeconomic conditions in the country. However, the main goal of the paper is to show that *the volatility* of these policy changes exerts a strong negative effect on long-term macroeconomic performance.

might not be as strong as expected is that there could be reverse causality: countries that adopt riskier technologies are countries that grow faster (see Black (1987) for a first reference to this idea and Imbs (2007) for empirical validation of this hypothesis). One way to deal with the problem of reverse causality is to identify exogenous sources of volatility. By looking at the volatility induced by fiscal policy changes and by using instrumental variables estimation, we avoid the problem and our results show a much more robust relationship between volatility and growth than previous studies in this field. In fact, we show that in most of our specifications, volatility of output is not a significant explanatory variable, while policy-induced volatility is.

The view that policy volatility is key to long-term economic performance is certainly not new. In his Nobel laureate lecture, Friedman (1977) points out that while high inflation *per se* does not change the natural rate of unemployment, an increase in the variance of inflation can generate grave economic inefficiencies and affect the long-term performance of the country by raising its natural rate of unemployment. Thus long-term monetary neutrality holds when we consider the level of policy but not when we look at its volatility.

What is policy volatility? In this paper we construct a measure of policy volatility based on the variance of unforecastable changes in government consumption. We interpret this variance as the aggressiveness with which politicians use spending for reasons other than smoothing the business cycle. Since there are obviously many dimensions of macroeconomic policy, we need to justify our choice of policy volatility. We start with the idea that in order to estimate the variance of exogenous shifts in policy stance, we should be able to specify an equation that resembles a reaction function for every country in our sample. At first, it may seem easier to do this for monetary policy: Many recent studies have proposed reactions functions with the short-term interest rate as an instrument of monetary policy. This approach, however, leads to some fundamental difficulties: (1) Short-term interest rates are not available for many countries in our sample, especially for the period before 1984; (2) When data are available, the series do not have consistent definitions across countries; (3) The interest rate is properly labeled as a monetary policy instrument only in few countries, and only since 1984 at best (with the US being a notable exception). An alternative approach would be to use monetary aggregates, but again we face problems with the consistency of definitions. A third possibility would be to use the inflation rate. We decided against this because even a well-specified equation for inflation will inevitably contain shocks that are due

to other factors and not to monetary policy (e.g. oil price shocks, terms of trade shocks, etc.). As a result we focus on fiscal policy, and by using government consumption we can construct an imperfect and yet consistent measure of fiscal policy stance. Comparable series for government consumption exist for many countries and the time span is long enough to allow sensible time-series estimation of the reaction function.[2]

The second strand of literature that relates to this paper is the one that looks at the deep determinants of economic growth. This strand has recently questioned the role of macroeconomic policies in determining long-term growth rates. First, policy variables traditionally become insignificant in growth regressions where a large number of variables are tested as determinants of long-term performance. Second, while policies are very persistent over time, growth rates are not. And finally, there is evidence that some of the positive correlation that exists between good policies and growth is simply due to the fact that both are the result of good institutions, so once we control for the quality of institutions the correlation disappears.[3] In our analysis, we extensively test the robustness of our results by running specifications which are similar to those of Acemoglu et al. (2003) or Sala-i-Martin et al. (2004). In these specifications, many other institutional and economic variables are included to test the robustness of the relationship between fiscal policy volatility and growth. The difference between our specification and those of the previous literature is that while most previous empirical studies use policy *levels* as regressors to predict growth performance, we claim that a key policy characteristic that matters for the long-term country performance is the volatility of policy.[4] Thus we argue that it is not enough to attain low inflation and low budget deficits on average, but it is also necessary to have stable inflation and stable fiscal policy.

Using our measure of policy volatility, our findings strongly support the view that volatility in policy has a significant negative effect on long-term growth rates. In other words, the way that macroeconomic policy is conducted matters for

[2] An alternative measure of policy volatility is to look at the volatility of exchange rates. This is the approach of Aghion, Bacchetta, Ranciere and Rogoff (2006) and their results are very much consistent with the ones presented in our paper.

[3] One exception is Rodrik (2008a) where the level of the exchange rate is shown to be a significant determinant of growth.

[4] Woo (2009) also provides evidence that mismanagement in fiscal policy measured as procyclical and volatile fiscal policy can harm growth.

growth.

In our analysis, we acknowledge the role of institutions as we uncover a strong relationship between institutional settings (such as constraints on the executive) and fiscal policy outcomes. In this respect, political institutions that constrain the executive have a powerful effect on growth by shaping macroeconomic policy. Macroeconomic policy is then the mediating factor through which institutions affect growth. By understanding the specific channels through which low-quality institutions affect growth we can provide better recipes for institutional reform as argued by Rodrik (2008b).

But our results also suggest that even *within* similar institutional settings differences in policy volatility result in different growth rates. This finding is consistent with the view that policy volatility is determined both by the institutional environment and by shifts in political preferences or other idiosyncratic shocks.

The paper is organized in the following way. Section II describes our empirical strategy, the data set and presents the construction of our measure of policy volatility. Section III reports that in a large cross section of countries higher policy volatility affects economic growth negatively. In Section IV we construct a panel of 10-year averaged data in order to address issues of reverse causality and omitted fixed effects. The link of our results to the empirical and theoretical literature on growth is discussed in section V. We conclude the paper by summarizing our main findings and by raising some questions for future research.

II. Empirical strategy and data description

To test the hypothesis that policy volatility exerts a negative impact on long-term economic growth we have compiled annual data for 93 countries spanning the years from 1960 to 2007. We posit that the link between policy volatility and economic growth can be identified with the following modification of a standard growth regression introduced by Barro (1991):

$$\overline{\Delta y_i} = \alpha + \lambda \log(\sigma_i^{\epsilon}) + \beta' \mathbf{X_i} + \gamma' \mathbf{Z_i} + u_i \tag{1}$$

In this regression $\overline{\Delta y_i}$ is the average growth rate of output per capita (1970-2007) for country i. Our key regressor is the volatility of the exogenous shocks to government consumption (σ_i^{ϵ}). Throughout the paper we will refer to this variable as *policy volatility* with the obvious caveat that our measure relates only to one

instrument of macroeconomic policy. In equation (1), we also include a vector of variables (X) that have been identified as having significant explanatory power for the cross-country variation in growth. In order to verify that our key results are not due to omitted variables, we will also include other controls captured by the vector Z. In this section we will discuss the definitions of the main regressors in equation (1) and the justification for their inclusion. We start first with the measure of policy volatility.

A. Policy volatility

Our goal is to isolate a measure of policy stance that captures the portion of discretionary fiscal policy that is not explained by the state of the business cycle. Shifts of this kind may occur because of changes in the political preferences of the ruling party or because of the desire of the incumbents to generate a temporary boom before elections.[5]

In the introduction we explained our focus on fiscal policy by referring to the difficulties with the estimation of a monetary policy reaction function consistently across countries. In general, several variables can be used to characterize fiscal policy. We choose government consumption (as reported in the national income and product accounts) because this is the only series that is easily comparable across countries. Needless to say, this is not a perfect measure — it does not include important parts of government spending such as transfers, and it is not a comprehensive measure of fiscal policy because it omits the revenue side. However, the series for the more comprehensive measures like the budget balance, total expenditures and total revenues are unreliable as there are frequent breaks and changes of definitions.[6]

Equipped with our preferred measure of policy, government consumption, our next goal is to isolate movements in government consumption that can be considered as policy decisions exogenous to the state of the economy. The literature on fiscal policy uses several approaches to measuring policy volatility. One approach is to calculate raw standard deviations of policy variables (before or after detrending).

[5] The literature on the political economy of policy making is enormous. Drazen (2000) and Persson and Tabellini (2000) discuss various models within the political business cycle literature, in which politicians have incentives to change spending levels for reasons other than macroeconomic stabilization.

[6] The source for the data on government consumption is the World Development Indicators database of the World Bank. Sources for all the other variables are listed in an appendix.

Another technique is to use GARCH models to construct smooth (time-varying) measures of volatility, which can be used also in panel estimation (as in Henisz, 2004). Finally, one can use — as we do — regression analysis to isolate changes in fiscal policy that are exogenous to changes in economic conditions. It is straightforward to show that the first two methods do not extract the exogenous component of policy changes (unless policy is itself exogenous). Therefore, we adopt the third approach, which requires that for each country in our sample we run a regression of the following type:

$$\Delta \ln\left(G\right)_{i,t} = \alpha_i + \beta_{i,j}\, \Delta \ln\left(Y\right)_{i,t} + \epsilon_{i,t} \tag{2}$$

In these regressions we denote by G real government consumption spending, while Y is real GDP. One possible interpretation of this equation is that of a reaction function for the government. Following Alesina, Campante and Tabellini (2008), Lane (2003), Woo (2009), and Aghion, Hemous and Kharroubi (2009) who estimate similar policy reaction functions, we estiamte equation (2) by OLS. Of course, we are aware of the possible reverse causality from government spending to output and we have also estimated several versions of equation (2) using instrumental variables.[7]

We interpret the country-specific volatility of $\epsilon_{i,t}$ as an estimate of discretionary policy or as a measure of fiscal activism.[8] In the calculation of policy volatility we restrict the sample from 1970 to 2007 because we will use data from the 1960s as initial conditions in the growth regressions.[9]

[7] We have explored two sets of instruments. In a previous version of the paper, we estimated equation (2) in levels and we instrumented output with a time trend, logarithm of oil prices and a lag of the GDP deflator. In Appendix C on our web sites, we also report results where we use output in the rest of the world (current and lagged), as well as current and lagged oil prices as instruments. This is reminiscent of the instruments used by Gali and Perotti (2003) where they use foreign output gaps to instrument for domestic output gap. Appendix C also reports variations of equation 2 where inflation is included as a control variable. The appendix provides a correlation matrix between measures of policy volatility derived from various specifications. In all cases, the main results from these robustness checks are consistent with the OLS results presented in the paper and are available upon request.

[8] In a recent paper, Aghion and Marinescu (2006) use an alternative measure of budgetary activism based on the cyclicality of government debt. Their study is focused on understanding the growth effects of counter-cyclical fiscal policy.

[9] A recent paper by Koren and Tenreyro (2007) decomposes output volatility in 45 countries into volatility due to specialization in volatile sectors, volatility due to macroeconomic country-specific shocks, and volatility due to the covariation between sector specific and country-specific components. They interpret the macroeconomic component as resulting from volatile policies or

B. Policy volatility and institutions

Before we proceed to the growth regressions, we investigate whether the newly constructed measure is related in any way to the institutional structure of the country. Our main institutional variable is *Constraints on the executive*. We have chosen this variable because a version of this institutional characteristic is used in the previous literature (e.g. Acemoglu et al (2003)) and also because it shows how much freedom the executive has in changing policy stance. The particular variable that we use takes five values depending on how many checks on the executive exist. It is calculated as:

Constraints = Legislature + Upper chamber + Judiciary + Federal

Each of the variables on the right-hand side is a dummy variable that takes the value of 1 for countries that have the specific institutions: *Legislature* is equal to 1 for countries where the parliament is freely elected and independent of the executive; *Upper chamber* is 1 if the country has a bi-cameral legislature; *Judiciary* equals 1 for countries where the judiciary is separated from the executive branch; *Federal* equals 1 for countries with a federal structure whereby political power is shared between central and local governments. Thus the variable *Constraints* captures potential veto points on the decisions of the executive.[10] The data used to construct *Constraints on the executive* comes from Henisz (2000).

A variation of our measure of constraints is a variable constructed by Henisz (2000) called 'political constraints'. This variable differs from our measure in two ways: (1) the author adjusts for the ideological alignment across political institutions; and (2) he argues that each additional constraint has a diminishing marginal effect on policy outcomes and therefore the link between the overall measure and the veto points should be nonlinear.[11] We prefer to use the simple sum of constraints because it deals in part with the possibility of endogenous

political instability. In an earlier version of this paper, we used their data and we found that their measure of country-specific volatility had a correlation of over 0.75 with our policy volatility measure. This result supports their interpretation and also provides evidence that our measure is not capturing volatility due to the sectoral composition of country's output.

[10] The role of veto players in policy-making has been studied extensively in the political economy literature. See for example Tsebelis (2002) for an insightful discussion of the policy effects of veto players.

[11] We have run a nonlinear regression of policy volatility on our measure of constraints. The nonlinearity was expressed as an exponent of the constraints measure that was estimated by the model. Somewhat surprisingly the exponent is estimated to be very close to 1, i.e. there is no strong evidence of non-linearity in the effect of constraints on policy volatility.

response of electoral outcomes (and hence ideological alignment) to economic developments throughout the period.[12]

There are two other measures of the role of constraints used in the literature. Acemoglu et al. (2003) use *Constraints on the executive* from the Polity IV data set. In contrast to our measure which simply records the number of independent veto points, the Polity IV measure is based on interpretation of the effectiveness of the veto points. The Database of Political Institutions (DPI) provides a series for a variable called 'checks'. This variable, as it is the case for the 'political constraints' variable in Henisz (2000), captures not only institutional characteristics in the country but also political outcomes as its value is adjusted when, for example, the president and the legislature are members of the same party.[13]

[Insert Table 1 here]

Table 1 documents the institutional determinants of policy volatility. In the first column we report that our measure of constraints has a significant negative effect on policy volatility. Alone, this institutional characteristic explains 38% of the cross-country variation in policy instability. This is a very strong result which has a straightforward interpretation – countries with more checks and balances do not allow the executive to change policy for reasons unrelated to the state of the economy. Therefore in these countries overall policy volatility is lower.

Column 2 adds as controls three other political and institutional variables used in the literature as determinants of policy volatility: (1) political system (presidential vs. parliamentary); (2) electoral system (majoritarian vs. proportional); (3) number of elections.[14] These variables improve the fit of the regression by raising the R^2 to 58%. Given that these variables (with the possible exception of the last one) are exogenous to the current state of the economy, we use them later in our analysis as instruments for policy volatility.

In the next two columns we report similar regressions by restricting the sample to the set of the initially rich countries (column 3) and the set of initially poor

[12] The ideological alignment across agents occupying various political institutions can be an outcome of strategic voting, as Chari et al. (1997) argue.

[13] Although not reported in the tables below, we have replicated our results using the Polity IV index, the DPI measure of checks and balances, and the index constructed by Henisz. The results are available upon request.

[14] Persson (2005) discusses in detail why the nature of the political and electoral arrangements might matter for policy outcomes. We refer the reader to his analysis and also to our brief discussion of the related literature in Section V.

countries (column 4). The cutoff between rich and poor is set at \$6,000 GDP per capita (average in 1967-1969), which is close to the mean income in that year.[15] With this cutoff, 29 countries are classified as rich and the remaining ones as poor. The coefficient on constraints is highly significant for poor countries and marginally significant for the rich ones.[16]

C. Growth Data and Summary of Controls

In addition to constructing our measure of policy volatility for each country, we have also collected a set of controls that we will use in regression (1) to ensure that the link between policy volatility and growth is not due to an omitted variable. A detailed description of the series is provided in the Data Appendix. Here we offer only a brief overview of the data and the timing assumptions. In equation (1), the dependent variable ($\overline{\Delta y_i}$) is the average growth rate of output per capita (1970-2007) for country i. The period over which the growth rate is calculated corresponds to the period over which we construct our measure of policy volatility. As main controls in equation (1) (vector X) we include five variables that have been identified as having significant explanatory power for the cross-country variation in growth. Our choice of controls is based on a recent study by Sala-i-Martin et al. (2004), who use Bayesian averaging of classical regression estimates for 67 determinants of growth to identify 18 variables for which the posterior inclusion probability increases relative to the prior. Of these variables we select four that have clear economic interpretation: (1) Initial GDP per capita; (2) Initial level of human capital; (3) Initial investment price level and (4) Initial government consumption. In addition to these variables, we include in vector X average openness to control for the effect of trade on economic growth (see Frankel and Romer (1999)). All variables, except initial human capital, are calculated as the average over 1967-1969, i.e. the three years preceding the start of the growth sample. Initial level of human capital is measured as in Sala-i-Martin et al. (2004) as the percentage of population of relevant age enrolled in primary schools in 1960. The reason for the longer time lag is that those enrolled in primary school will contribute to growth as workers only after ten or more years.

We choose these five regressors as our main controls because behind each one of them there is a relatively well-accepted economic theory that explains why

[15] The income data is real GDP per capita (chained series) from the Penn World Tables v.6.3. Earlier version of the paper reports results based on PWT v.6.1

[16] This result is consistent with Aghion et al. (2005).

these variables should predict growth.[17] Thus one can explain why initial GDP per capita predicts growth by referring to the neoclassical growth model, but it is very difficult to provide a theory explaining why, for example, a dummy variable taking a value of 1 for East Asian countries should predict growth. Furthermore, if the East Asian dummy is indeed so successful in predicting growth, then many theories would suggest that this predictive power is only temporary — once the countries from the Asia-Pacific rim reach the technological frontier, their growth rate will slow down and the dummy variable will not be as successful as before. At the same time, GDP per capita will be a predictor (*ceteris paribus*) as long as there are countries away from the technological frontier.

While our baseline results include only five controls, we are sensitive to the criticism that the estimates might be driven by an omitted variable listed among the top predictors of growth in Sala-i-Martin et al. (2004). To investigate the sensitivity of our results to changes in the specification of equation (1), we include additional controls (vector Z). Within this set we will include sequentially *all* of the top 21 variables from Sala-i-Martin et al. (2004), as well as additional variables like output volatility, inflation volatility, institutions, etc.

III. Policy volatility and economic growth

A. Baseline Regression

We start by documenting in Figure 1 the correlation between policy volatility and long-term economic growth over the period 1970-2007. The most volatile fiscal policy is recorded in several African countries like Mozambique and Rwanda, while the most stable policies are those in the OECD economies. The unconditional raw correlation is negative and a regression of growth on policy volatility – reported in column 1 of Table 2 – yields a negative coefficient of -0.813, which is significant at the 1% level of significance. Taken at face value, this coefficient suggests that a country like Venezuela — with volatility of fiscal policy close to the mean of the sample — could raise its growth rate by about 0.5% per year if its fiscal policy were stabilized to the same level as Mexico.

[Insert Figure 1 here]

[Insert Table 2 here]

[17] For a textbook presentation of the relevant theories see Barro and Sala-i-Martin, 2003.

There are several reasons why we should interpret the scatter plot and the regression results with caution. First, it is possible that our measure of policy volatility is correlated with some other key determinant of economic growth and therefore in column 1 the coefficient on policy volatility is biased and inconsistent. Second, the result reported in column 1 could be driven by outliers and hold only for this specific sample. Third, it is possible that policy volatility does depend on recent growth performance and is therefore endogenous to long-term economic growth. The rest of Table 2 addresses in part the first two possibilities, while the reverse causality issue is taken up in Table 3.

To check for the possibility that a significant omitted variable is responsible for the documented link between policy and growth, we include in column 2 five key determinants of growth as we discussed in the previous section. We note that all of the variables enter with the expected sign: *Investment price 1960* and *Government size* are not significant, while *Initial GDP per capita*, *Primary enrolment*, and *Openness* are all significant at the 1% level. The coefficient on our key variable of interest – policy volatility – increases in absolute value and remains significant at the 1% level.

Next, we take up the possibility that the negative effect of policy instability on economic growth holds only for this particular sample. In columns 3 and 4 we split the sample into rich and poor countries.[18] In the poor countries policy volatility has a bigger impact on long-term growth than in the rich sub-sample. If we test in a nested model for the equality of the two coefficients, however, we find that the difference is not statistically significant.

A slightly different concern is that we might have misspecified our first stage regression (equation 2) when generating exogenous shocks by running a regression of government spending on output. It is conceivable that if we do not capture sufficiently well the reaction of fiscal policy to output growth, then a component of output fluctuations will enter the residuals. Hence, instead of measuring the effects of policy volatility, we might be documenting the effect of output volatility on economic growth. A straightforward way to test this claim is to include output volatility as a regressor. We report the results in column 5 where we also include the average inflation and its volatility as controls.[19] This modification has no effect

[18] As mentioned in the previous section, rich countries are defined as average GDP per capita in 1967-69 of at least $6,000.

[19] Monetary variables are included because they can potentially affect both long-term growth and fiscal policy and therefore our main estimates may suffer from omitted variables bias.

on the coefficient or significance of the policy volatility variable. This suggests that our measure of policy volatility is not simply a proxy for the volatility of output. The fact that the volatility of output is not in itself significant confirms previous results in the literature.[20]

In summary, our measure of fiscal policy volatility enters a standard growth regression with a negative and statistically significant sign. The effects are not negligible: A reduction in policy volatility corresponding to one standard deviation in our sample raises long-term economic growth by about 0.92 percentage points.

B. Treating policy volatility as endogenous: An IV approach

There are still two potential problems with the results reported in Table 2: omitted variables and endogeneity of policy volatility. It is indeed plausible to argue that an omitted variable may affect both growth and policy volatility, or that countries with low rates of growth resort more often to aggressive policy in order to boost demand in the economy (reverse causality). Indeed, this version of the reverse causality argument does generate a negative (conditional) correlation between output growth and policy volatility. In this section we address these concerns by using instrumental variables. Incidentally, instrumental variables will also help us deal with standard measurement error problems, which might be present because of the imprecision with which we have constructed the measure of policy volatility. The presence of measurement error creates an attenuation bias, i.e., it works against finding a significant relationship between policy and growth. If the instruments help us deal with the measurement error we should see an *increase* in the absolute value of the coefficient. If, on the other hand, endogeneity has an important impact on our OLS estimates, then we should see a *decrease* in the absolute value of the coefficient on policy volatility.[21]

From the analysis of the link between institutions and policy volatility in Table 1, we know that the constraints on the executive that were in place in 1969, i.e.

[20] As pointed out by Imbs (2007) and others, it is conceivable that there is a positive correlation between output volatility and growth. Countries that are willing to take more risks might grow faster and, as a result of investing in more innovative and risky technologies, they display higher output volatility. Similar discussion can be found in Ramey and Ramey (1995).

[21] Of course, the magnitude of the coefficient could increase in the IV estimation if in the original regression there is an omitted variable that has positive (negative) correlation with policy volatility and affects growth in a positive (negative) way. The measurement error is just an example of such influence.

the year before the start of our sample period, are very good predictors of policy volatility. In the next battery of tests we use the institutional characteristics of the countries in our sample as instruments for policy volatility.

[Insert Table 3 here]

The univariate regression reported in column 1 reveals again the strong negative impact of policy volatility on growth. In the next column we add our standard controls and the nature of our results does not change. Interestingly, the coefficient estimates for policy volatility in both cases jump relative to the OLS results reported in Table 2, which is consistent with the presence of measurement error in our policy variable. In columns 3 and 4 we split the sample again into rich and poor countries and still the coefficient for poor countries remain larger in absolute size.

Column 5 reports results from a perturbation of the baseline IV regression that also includes output volatility, inflation and its volatility as regressors. As before, the standard deviation of output growth is insignificant and the inclusion of monetary variables has no impact on the significance of policy volatility. This result shows again that one cannot attribute our key result to the role of general macroeconomic volatility for output growth. In both cases the results are consistent with those documented by Sala-i-Martin et al. (2004). Importantly, the coefficient on fiscal policy volatility is unaffected in terms of magnitude or statistical significance.[22]

C. Robustness

We return now to our selection of controls in the growth regression. The study by Sala-i-Martin et al. (2004) shows that many geographical fixed effects — like a dummy for the East Asian countries or the percentage of the country area with tropical climate — predict growth quite well. Although it is difficult to see why East Asian countries would have different policy volatility, i.e. why the omitted variable is correlated with our measure of policy, we explore later in this section how the most significant regressors from the Sala-i-Martin et al. (2004) study affect our findings.

[22] There are always concerns about the validity of instruments in growth regressions. One particular problem occurs when the same instrument has been used in different papers to instrument for different growth determinants. In Appendix D we discuss this problem and we use a method developed by Conley, Hansen, and Rossi (forthcoming) to provide a sensitivity analysis by relaxing instruments' exogeneity assumption.

Because our sample period and some of our data sources differ from those used by Sala-i-Martin et al. (2004), we start our robustness study by using exclusively data from their paper. In Table 4, the first column the dependent variable is growth from 1960 to 1996 and in addition to our measure of policy volatility, we include the controls as they are defined in their study. The fundamental difference is only that the initial period is 1960 for the controls and dependent variable is calculated over a different range. The estimation by OLS yields coefficients for the controls that are very close to the posterior means reported by Sala-i-Martin et al. (2004), while the coefficient on policy volatility remains close to the estimates from Table 2. Thus the change of the time period and in the exact definition of the controls has no effect on our results.

[Insert Table 4 here]

We now proceed by using the top six variables from Sala-i-Martin et al. (2004). We change the range for the dependent variable back to 1970-2007 as in our baseline regression. The top six variables from Sala-i-Martin et al. (2004) are: (1) East Asian dummy; (2) Primary enrolment 1960; (3) Investment price in the initial period; (4) Initial GDP per capita; (5) Fraction of tropical area; and (6) Coastal population density in the 1960s. Relative to our baseline regression from Table 2, we now drop government consumption and openness and we include three geographic and demographic characteristics. The OLS regression in column 2 shows that the effect of policy volatility on growth is slightly moderated, but it remains significant at the 5% level. The other variables are all significant with the exception of investment prices.

In the last column of Table 4 we replicate our baseline IV regression by instrumenting policy volatility with the same instruments as in the previous section while using the top six controls from Sala-i-Martin et al. (2004). The results remain largely unaffected with policy volatility still significant at 5%. An alternative approach to verify robustness of our results is to add sequentially all of the top eighteen variables from Sala-i-Martin et al. (2004) for which the posterior probability of inclusion is higher than the prior. We report the results from these regressions in Appendix Table B1. In all regressions policy volatility remains significant at better than the 5% level.

In summary, we have searched over the space of a large number of variables that have been found to determine long-term growth. Our conclusion is that policy volatility is robustly and significantly correlated with growth. We have not found a

single cross-sectional growth regression that challenges this conclusion. Of course, there might be some suspicion that the instruments are themselves determinants of growth and thus belong to the growth regression as regressors, or that endogeneity cannot be addressed in a satisfactory manner by using cross-sectional regressions. To address some of these concerns we report in the later part of the paper estimates from panel regressions designed to deal with the issue of reverse causality. In the next section we turn to the marginal explanatory power of our key institutional variable – constraints on the executive.

D. The marginal effects of policy volatility and institutions on growth.

The fact that constraints on the executive affect policy outcomes and thus policy volatility is both theoretically justifiable and intuitive. In his book on the role of veto players, Tsebelis (2002) discusses the role of veto points for policy stability and summarizes the evidence from a number of studies. The gist of the main argument is that countries with many veto players will have more stable and predictable policy because the process of negotiating new policy initiatives is more difficult and more costly. So far our results confirm this logic. It is, however, possible that in addition to shaping policy outcomes, constraints on the executive also exert a direct effect on growth. In this section we ask two related questions: (1) Do political constraints have any additional explanatory power for economic growth above the effect they have through policy volatility? (2) Within the same institutional setup, do we observe any effect of policy volatility on growth? In other words, is the link between volatility and growth fully explained by the way that policies are shaped by institutions?

[Insert Table 5 here]

Table 5 is devoted to answering the first question. The univariate regression of output growth rates from 1970 to 2007 on constraints on the executive in 1969 show significant positive correlation. A causal interpretation of this result suggests that countries with more constraints on the executive achieve faster economic growth. But what is the channel? As we have shown in our main tables, one explanation is that political constraints lead to more stable policy, which in turn creates a more favorable environment for growth. In column 2 we discover that these constraints have no marginal power in explaining growth above and beyond their effect on policy stability. Importantly, the coefficient on policy volatility is close to the estimates from Table 2, where political constraints are excluded. If the

institutional variable was a significant predictor of growth, then not only would we expect the coefficient on constraints to be significant, but we should also expect a significant change in the estimated effect of policy volatility on growth. Thus we can conclude that institutions do not affect growth directly and therefore they are good instruments for policy volatility. This conclusion is confirmed also by the results in column 3 where we include our standard controls.

Next we want to establish whether institutions have any marginal explanatory power for growth within the IV framework. In column 4 we include political constraints as a regressor in our baseline IV estimation. Policy is instrumented only with the political regime, electoral regime and number of elections. Constraints remain insignificant while the coefficient on volatility changes in magnitude but remains significant at the 1% level.

One potential criticism of this regression is that constraints themselves are endogenous. Indeed this is the argument that prompted Acemoglu et al. (2003) to use settlers' mortality rates as an instrument for institutional quality. Their main dependent variable is the level of GDP per capita and therefore in their case one can plausibly argue that the endogeneity of institutions to *past* levels of income per capita may create bias in the estimation if both institutions and income per capita are persistent processes. It is less clear how one can make the same argument when the dependent variable is the *subsequent* growth rate and initial income per capita is used as a control. Nevertheless, in column 5 we use as an instrument the logarithm of settlers' mortality and we instrument only political constraints. This estimation does not record a significant coefficient for constraints. In the last column – for completeness – we instrument both policy volatility and political constraints.

In all variations reported in Table 5 we find a statistically significant negative effect of policy instability on output growth. At the same time, constraints on the executive have little additional explanatory power. It is important to interpret this result correctly. It does not say that institutions do not matter. They do. However, their effect — as one might reasonably expect — is manifested through policy and there is very little (if any) additional impact of political constraints on growth.

Now we turn to the second question — whether within the same institutional setup policy volatility can make a difference. In the first two columns of Table 6 we report OLS regressions for countries where the number of veto points is less

than 2 (column 1) or more than 2 (column 2). Columns 3 and 4 replicate the same regressions by using our main instruments to instrument for policy volatility. In all cases except for column 2 the results are significant at 5% and indicate that even within similar institutional frameworks in terms of veto points, policy volatility matters.[23] In the last column we look at the effect of policy volatility within each institutional 'cell'. First we generate five dummy variables for each one of the institutional categories (from 0 to 4 constraints) and then interact these dummies with our policy volatility measure. The coefficients show that within each institutional structure the volatility of fiscal policy has a negative impact on growth. In all cases, except for the case with 4 constraints, the effect is statistically significant. It appears, however, that the impact of policy stability on growth is much more pronounced within the low levels of constraints (either dictatorships or one veto player systems) than for countries with more developed checks and balances.[24]

[Insert Table 6 here]

The results so far lead to potentially important policy recommendation: there is room for both institutional reform *and* good macroeconomic policies as recipes for growth. A simple way to illustrate this point is to look at Table 6. Both for the group of countries with low-quality institutions and for those with high-quality ones, the effect of policy volatility on economic growth is negative and significant. Thus, Table 6 provides some evidence that even when it is difficult to change institutions, growth performance can be improved by following stable macroeconomic policies.

IV. Panel estimation

Possibly the most interesting direction for further investigation is the study of the effects of policy changes on economic growth over time. Ideally, one would like to see how shifts in policy volatility affect growth *within* a country. One might want to use estimation of average treatment effects in order to control

[23] We have also estimated regressions for those countries with no constraints at all. We find that the coefficient on policy volatility remains highly significant and negative. This result not only confirms the importance of policy stability, but also indicates once again that our findings cannot be driven by the omission of constraints from the main regression.

[24] In the last cell, where we have countries with 4 constraints on the executive, we have only 5 observations. Thus, the large standard errors might be due to the insufficient number of observations.

for observed heterogeneity across countries and to evaluate whether differences in policy volatility in otherwise similar countries lead to differences in growth rates. Unfortunately, the estimation of treatment effects is very difficult to implement in our setting. The main reason is that we do not know exactly when treatment — in our case, the shift in policy volatility — has occurred. Despite the difficulties in addressing the time-variation in our data series, we have attempted to provide at least a partial view of the robustness of our results using within-country variation.

We start by creating a panel of 10-year averaged data. We have four non-overlapping periods: 1965-1974, 1975-1984, 1985-1994, and 1995-2004. For each decade of growth we use as initial conditions data on income per capita, primary education, investment price, government size and openness. These initial conditions are calculated as averages for the three years preceding the relevant decade. For example, when growth covers the 1985-1994 period, the initial conditions are calculated as the average from 1982 to 1984. All variables have the same definitions as before except primary education. In the cross-sectional regressions we used primary school enrolment in 1960 because we argued that enrolment in 1960 will determine to a large extent the educational level of the population in our growth sample period from 1970 to 2007. In the panel, however, primary school enrolment in 1982 to 1984 is clearly a poor predictor for the level of education of the labor force from 1985 to 1994. So instead of enrolment we use the percentage of population with primary education.

In Table 7 we present results from this panel by using first pooled OLS. Columns 1 and 2 start by reporting regressions where policy volatility is measured over the decade period. Focusing on the estimates in column 2, we notice that the coefficient on policy volatility is again negative, highly significant but it is somewhat smaller in magnitude compared to our cross-sectional estimate from Table 2. All of the controls are also highly significant and of the expected sign.

[Insert Table 7 here]

In columns 3 to 7 we report results where policy volatility is lagged one period (one decade). This is an important perturbation of our regression, because it deals with the criticism of reverse causality. A skeptic might argue that our results in the cross-section or in columns 1 and 2 are driven by the fact that in a low-growth environment, governments are tempted to try various spending programs to jump-start growth. In columns 3, 4 and 5 our baseline regression uses policy volatility from the previous decade to explain current growth for the

full sample, or from the two groups separated by their income per capita. The result is very encouraging for our argument: governments that use fiscal policy too aggressively and for reasons other than to smooth business cycles, have generated lower growth in subsequent years. Only for the group of rich countries does the effect become statistically insignificant, even though it is still negative. Column 6 confirms once again that this claim is not due to omitting the overall economic volatility from the equation.

Although the results are sufficiently robust to every modification of these panel equations, it is important to report one particular modification in which we include lagged growth rates as regressors. The reason for this inclusion is the following: Current growth rates cannot affect past policy volatility, and yet it is still possible that our results are biased if there is reverse causality within the period and at the same time innovations to growth are highly persistent. In this scenario a positive innovation to the growth rate may reduce contemporaneously policy volatility (if there is reverse causality within the period) and raise future growth rates (because of persistence). One way to address this concern is to include lagged growth in the estimation. We report the results in column 7. Indeed lagged growth enters with a positive sign and a statistically significant coefficient. Nevertheless, the effect of this modification on the coefficient on policy volatility is minimal. Relative to column 3, the current estimate is marginally lower but its significance is left virtually intact.

By including lagged growth in the regression we also control to a certain extent for fixed effects that explain cross-sectional differences in growth rates. In Table 8 we document the sensitivity of our key findings to the direct inclusion of country and time fixed effects. Because of the well-known bias in the estimation of panel regressions with fixed effects and lagged dependent variable, initially we drop lagged growth from the regression.The coefficient is somewhat lower in magnitude but remains significant when controls are added.

[Insert Table 8 here]

The following three columns include our baseline controls within the panel regression and estimate the model successively with country effects, with time effects and with both country and time fixed effects. The stability of the coefficient on policy volatility is quite remarkable. Among the controls the variable that exhibits the most consistent sign and significance is initial GDP per capita. Including output volatility as a separate control has no impact on the regression (column 5).

Note that the estimation in Table 8 has used lagged policy volatility as a regressor and therefore one can interpret the results as showing that neither omitted fixed effects, such as geography or weather, nor reverse causality can provide a reasonable alternative interpretation. What about reverse causality through persistence of growth innovations as an alternative interpretation? In columns 6 and 7 we estimate the panel regression by using the Arellano and Bond (1991) methodology, which allows us to include both fixed effects and lagged growth rates. All variables included in the regression are treated as predetermined, which implies that they might be correlated with lagged growth rate innovations but they are not correlated with future growth innovations. The estimation is a two-step GMM using all available lags of the dependent variable dated before the date of the included change in the lagged growth rate as instruments for the change in lagged growth. We include two lags of output growth since both lags seem to be highly significant. The results are quite telling — in this most demanding specification, policy volatility once again emerges as an important determinant of future *changes* in growth rate. The specification tests in the last rows indicate that the conditions required for this method to deliver consistent estimates are present – the test of overidentification indicates that there is no correlation between instruments and residuals, while the test for autocorrelation confirms the presence of a first-order correlation induced by differencing the data.

To sum up the panel estimation, we note that the negative impact of higher policy volatility on growth is confirmed in a wide variation of specifications. Even within countries, governments that pursue unstable fiscal policy create an environment that harms the subsequent growth performance of the country.

V. Discussion

The research agenda most closely linked to this paper focuses on the relationship between macroeconomic volatility (including policy volatility) and growth. One set of papers in this literature looks directly at the relationship between volatility and growth without focusing on a specific channel through which the effects take place. This group includes Ramey and Ramey (1995), Kormendi and Meguire (1985), Imbs (2007), Martin and Rogers (2000), Hnatkovska and Loayza (2005), and Aghion et al. (2005). All of these papers document a negative relationship between overall macroeconomic volatility and economic growth.[25]

[25] Koren and Tenreyro (2007) also document that the volatility of country-specific shocks,

When examining the relationship between volatility and growth there is concern about omitted variables and reverse causality as there may be factors that drive both the growth rate and the volatility of the country (as Imbs (2007) argues). To overcome these econometric issues one approach is to identify and isolate an exogenous source of volatility. For example, Alesina et al. (1996) and Dutt and Mitra (2008) study the effects of political instability on macroeconomic outcomes, including growth, while Judson and Orphanides (1999) analyze the effects of the volatility of inflation on growth. Ramey and Ramey (1995) also follow this route by using fiscal policy changes as an instrument for output volatility. In this respect, our paper builds on their approach by focusing on the volatility of discretionary fiscal policy as an exogenous source of volatility in the economy. By using instrumental variables and also checking a much larger set of controls we provide a more robust set of results.

Aghion et al. (2006) study a different dimension of policy volatility: the effect of exchange rate volatility on productivity growth and their results are consistent with the results presented in our paper. They find that volatility (of the exchange rate) has a negative effect on growth.

Finally, there is a set of papers that has looked at the effects of alternative dimensions of fiscal policy on growth. Aghion and Marinescu (2006) and Woo (2009) both study the link between the degree of countercyclicality of fiscal policy and growth. In Aghion and Marinescu (2006) less procyclical fiscal policy ("better fiscal policy") is associated with higher growth rates of productivity. Woo (2009) presents evidence suggesting that countries that run more procyclical fiscal policy have lower growth rates. Both of these papers are consistent with our results. Although their policy variables is not volatility, one could establish links between the cyclical stance of fiscal policy and its overall volatility. It is possible that both might be affected by the same budgetary processes or institutional variables that we use in our instrumental variables estimation; a higher degree of discretion in fiscal policy could be linked to more procyclical fiscal policy.

Although our paper is empirical, it is also interesting to understand the link to theoretical models. In endogenous growth models, and from a theoretical point of view, the relationship between volatility and growth is not an obvious one, as first

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which they interpret as policy shocks or political stability, is more important for the overall macroeconomic volatility in poor rather than in rich countries. Their study relates volatility to the stages of economic development and thus it is complementary to those studies that link volatility and growth.

documented in King, Plosser and Rebelo (1988). In a standard neoclassical model where agents (firms) are risk-neutral, investment might increase with uncertainty (at least in prices) because of the convexity of the profit function. The recent monograph by Aghion and Banerjee (2005) makes this theoretical point within the AK model. There are several ways of modifying the analysis so that volatility and uncertainty become detrimental to investment and long-term growth. The first is very mechanical and consists of thinking about fluctuations as asymmetric, as in Rodrik (1991). The link between volatility and growth could also be happening through uncertainty, as in Feeney (1999). Finally, an endogenous growth model can also introduce general equilibrium effects of uncertainty on growth through investment, consumer behavior and the labor supply, as in Aghion et al. (2005), Barlevy (2004), Jones et al. (2005) or de Hek and Roy (2001).

When we turn specifically to policy, we find fewer theoretical papers that establish a link between policy volatility and growth. Aizenman and Marion (1993) show that higher policy volatility (modeled as higher dispersion of tax rates) is detrimental for growth. Hopenhayn and Muniagurria (1996) discuss growth and welfare effects of policy volatility and persistence within a standard AK model of growth and find that an increase in the frequency of policy changes can lower growth, whereas a greater amplitude of policy changes is associated with higher growth rates. The role of policy volatility can also be detected in Barro (1990), who shows that growth is a concave function of government size and it is straightforward to demonstrate in his model that an increase in spending volatility will reduce growth. Chong and Gradstein (2006) emphasize a different and, in our view, a plausible mechanism: In countries where governments cannot commit to a stable tax rate, fewer firms enter into productive industries, which in turn lowers the aggregate growth rate. Using data from about 80 countries they document a negative effect of policy volatility on firms' growth rates.

In general, theoretical models emphasize the uncertainty related to the level of taxes and show that an increase in the variance of tax rates lowers growth.[26] Our empirical estimation suggests that volatility of government spending lowers growth. One way to link our finding to the theoretical literature on policy volatility is that an increase in spending volatility implies either concomitant increase in tax rate volatility or, more plausibly, raises the uncertainty about future tax rates, which in turn reduces investment and growth.

[26] The fact that higher variance of tax rates has detrimental welfare effects was first emphasized by Barro (1979).

Our paper also builds on several streams of research that link policy, economic growth and institutions. First, following Acemoglu et al. (2003) we explore the role of institutions and policies for economic development. Our main innovation is that we do not consider the level of policy variables (inflation, government consumption, or overvaluation of the exchange rate, i.e., standard macroeconomic policy variables) but instead argue that it is policy volatility that is detrimental to macroeconomic performance. Relative to Acemoglu et al. (2003), we also extend the analysis to a larger set of countries (not only former colonies). These two papers belong to a much broader and earlier literature on the effects of institutions on growth or volatility, which is surveyed in the paper by Acemoglu et al.(2004).

Another stream of literature that is related to our paper analyzes the effects of institutions on macroeconomic policy outcomes. This is a growing field with important recent contributions by Persson and Tabellini (2003, 2004) who study how constitutional rules shape fiscal policy outcomes. Within this literature several papers have specifically looked at the role of constraints in determining policies. The main hypothesis is that governments where power is more concentrated and which face fewer veto points are less constrained in the implementation of fiscal and monetary policy changes. In the case of fiscal policy, there is plenty of empirical evidence in favor of the idea that constraints matter. Roubini and Sachs (1989) present evidence for OECD economies that governments where power is more concentrated create an excessive fiscal policy response to economic shocks. Similar evidence exists for US states. Both Poterba (1994) and Alt and Lowry (1992) show that divided state governments display a less reactive fiscal policy to changing economic conditions. There is also plenty of evidence that veto points in budgetary processes affect fiscal policy outcomes (see Tsebelis (2002)). Talvi and Vegh (2000) present evidence of differences in fiscal policy behavior across countries and examine how these differences are associated with different political institutions or economic structures.

VI. Conclusions

Does macroeconomic policy volatility represent a significant determinant of economic growth? Our answer is 'Yes'. The results documented in this paper show that the volatility of fiscal policy has a first-order effect on long-term economic performance. Countries where governments use aggressively discretionary fiscal

policy for reasons not related to the state of the cycle experience a lower rate of economic growth.

This is an important result in light of the recent revival of fiscal policy as a tool to manage business cycles. While our results do not question the effectiveness of fiscal policy as a stabilizing tool, they raise some serious concerns about the consequences of its use. We show that exogenous changes in fiscal policy cause unnecessary output volatility and harm economic growth. Although by construction these changes are unrelated to the business cycle and, as such, one might think that they are unrelated to countercyclical fiscal policy, there can be a clear connection between the two. First, if the timing of countercyclical fiscal policy is not right, it will show up in the measure of fiscal policy volatility in our regressions. Second, if the asymmetric use of countercyclical fiscal policy leads to accumulation of debt, the government will be required to go through a fiscal consolidation which will also be captured by our measure of volatility. Of course, our measure of volatility also includes changes in fiscal policy which are unrelated to the business cycle, changes that are motivated by political decisions or changes in the agenda of the party in power.

The fact that macroeconomic policy is an important determinant of economic growth runs contrary to some of the recent results in the literature suggesting that economic policies are simply a proxy for poor institutions and do not have a significant role even as mediators in this relationship. By measuring macroeconomic policy in changes and in levels, as previous papers have done, we show that the result is robust to the inclusion of a long list of controls, including institutional variables. To be clear, we do not deny the role of institutions; indeed we document how institutions — and in particular constraints on the executive — shape policy outcomes. But at the same time we show that economic policies cannot simply be ignored. In this respect, while we agree that certain institutions create incentives for bad economic policy, we do not conclude that the *only* way forward is to improve institutions so that policy improves (and thus leads to higher growth); one can also envision improving policies without changing institutions. In fact, in our sample, policies do vary within the same set of institutions and this variation is robustly related to the subsequent growth of the country. A question for future research is: What drives good fiscal policy above and beyond good institutions?

Finally, it is important to frame our results regarding fiscal policy properly and within the limits of this analysis. Our findings warn of the potential costs of policy changes. It is, however, conceivable that in certain situations (e.g. fiscal

consolidations), a sharp and unexpected policy change might improve the long-term performance of the economy. Our results simply imply that policy changes should be implemented carefully, with an appropriate calculation of the long-term effects stemming from policy instability.

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Appendix A: Data description

We use annual data for 93 countries over the period 1960-2007. The choice of our sample is dictated by data availability on output and government consumption with the requirement that we have at least 20 years of data for each country.

List of Countries

Algeria	Gambia, The	Nicaragua
Australia*	Germany*	Niger
Austria*	Ghana	Norway*
Bangladesh	Greece*	Pakistan
Belgium*	Guatemala	Panama
Benin	Guinea	Papua New Guinea
Bolivia	Guinea-Bissau	Paraguay
Botswana	Honduras	Peru
Brazil	Hong Kong*, China	Philippines
Burkina Faso	Iceland*	Portugal*
Burundi	India	Rwanda
Cameroon	Indonesia	Senegal
Canada*	Ireland*	Seychelles
Cape Verde	Italy*	Singapore
Chad	Japan*	South Africa*
Chile*	Jordan	Spain*
Colombia	Kenya	Sweden*
Comoros	Korea, Rep.	Switzerland*
Congo, Rep.	Lesotho	Syrian Arab Republic
Costa Rica*	Madagascar	Thailand
Cote d'Ivoire	Malawi	Togo
Denmark*	Malaysia	Trinidad and Tobago*
Dominican Republic	Mali	Tunisia
Ecuador	Mauritania	Turkey
Egypt, Arab Rep.	Mauritius	Uganda
El Salvador	Mexico	United Kingdom*
Ethiopia	Morocco	United States*
Fiji	Mozambique	Uruguay*
Finland*	Namibia	Venezuela, RB*
France*	Netherlands*	Zambia
Gabon*	New Zealand*	Zimbabwe

The countries with an asterisk are in the group of rich countries with over $6,000 income per capita in 1967-69.

Data series used in the country time-series regressions:

Real government consumption — General government final consumption expenditure (constant LCU). 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. Series identifier in the original data set: *General government final consumption expenditure (constant LCU) (NE.CON.GOVT.KN).*

Real GDP — Real GDP in constant local currency units from World Development Indicators. Series identifier in the original data set: *GDP (constant LCU) (NY.GDP.MKTP.KN).*

Price level — GDP deflator from World Development Indicators. Series identifier in the original data set: *GDP deflator (NY.GDP.DEFL.ZS).*

Index of oil prices — logarithm of petroleum spot price from International Financial Statistics. 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 6.3). Series identifier in the original data set: *rgdpch.*

Investment price — Price level of investment on Purchasing Power Parity basis from Penn World Tables (version 6.3). Series identifier in the original data set: *pi.*

Government size — government share of real GDP per capita from Penn World Tables (version 6.3). Series identifier in the original data set: *cg.*

Openness — logarithm of the sum of imports and exports as percentage of GDP from World Development Indicators, CD-ROM 2002. Series identifier in the original data set: *Exports of goods and services (percent of GDP) (NE.EXP.GNFS.ZS)* and *Imports of goods and services (percent 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 51 countries are from Persson and Tabellini [2001]. Series identifier in the original data set: *pres.* From the Database of Political Institutions (Beck et al. [2000]), for 40 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 51 countries are from Persson and Tabellini [2001]. Series identifier in the original data set: *maj*. From the Database of Political Institutions for 40 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*.

Constraints on the executive — from Henisz [2000]. Updated from author's web-site. Calculated as the sum of four dummy variables indicating the existence of government branches independent of the executive. Series identifiers in the original data set: *L1, L2, J, F*.

Primary enrolment — Enrolment rate in primary education in 1960 originally constructed by Barro and Lee. Data are posted by Sala-i-Martin et al. (2004) on the AER web-site.

Log European settler mortality — Log of estimated mortality of European settlers during the early period of European colonization. See Acemoglu et al. (2001) for data description.

Data for Table 4, column 1 is from Sala-i-Martin et al. (2004).

Appendix B: Robustness Using Sala-i-Martin et al. (2004)

To check whether our results are robust to the inclusion of the most significant variables from Sala-i-Martin et al. (2004), we ran our baseline regression by including sequentially one of the top 21 variables from their paper. Since we already include 4 of these variables as our main controls, the list of new variables contains only 17 entries. In all regressions reported in Table B1, we include the main controls described in the text, policy volatility and one of the 17 variables. To conserve space we report the coefficients only for policy volatility and for the new variable. The estimation is by instrumental variables as described in Table 3.

[Insert Table B1 here]

Appendix C: The effect of endogeneity on the measure of policy volatility

We start with a simple univariate regression (we omit the subscript i). Assume that all variables are in deviations from the mean:

$$y_t = \beta x_t + u_t \tag{C.1}$$

Suppose that x_t is endogenous so that:

$$E[x_t u_t] = \gamma$$

The OLS estimate is biased and inconsistent:

$$plim\widehat{\beta_{ols}} = \beta + \frac{\gamma}{\sigma_x^2}$$

Thus the variance of the residuals will be:

$$\widehat{\sigma_u^2} = \frac{1}{N}\sum_{t=1}^{T}(y_t - \widehat{\beta_{ols}}x_t)^2 = \frac{1}{N}\sum_{t=1}^{T}(y_t - \beta x_t - \frac{\gamma}{\sigma_x^2}x_t)^2 \tag{C.2}$$

By definition:

$$\sigma_u^2 = plim\frac{1}{N}\sum_{t=1}^{T}(y_t - \beta x_t)^2 \tag{C.3}$$

After expanding C.3 and using the definition C.4, we obtain:

$$\widehat{\sigma_u^2} = \sigma_u^2 - \frac{\gamma^2}{\sigma_x^2} \tag{C.4}$$

Equation C.4 implies that if x is endogenous, then our estimate of volatility will be contaminated by a component which is related to the volatility of output growth (the regressor) and the degree of correlation between the errors in equation C1 and output growth.

In the paper we try to deal with this contamination in two ways: (1) by controlling for output volatility directly; and (2) by using instrumental variables

estimation in the growth regressions. If the political economy instruments affect output volatility only by influencing fiscal policy, then the growth regressions will not be severely impacted by variations in the fiscal policy reaction function. In other words, the misspecifications in the reaction function introduce a measurement error in the volatility estimates. Under standard assumptions, IV estimation in the growth regression will deal with the biases introduced by measurement errors and produce consistent and unbiased estimates of the coefficients of interest.

Appendix D: Instrument Validity. Sensitivity Analysis.

While we have provided a large number of robustness tests, there can always be questions about the validity of our instruments and the possibility that the channel through which they affect growth could be a different from the one that we stress. This is very much the criticism that Bazzi and Clemens (2009) raise for a variety of papers that use cross-country growth regressions. Solving the endogeneity problem using instrumental variables can only work if the instrument affects growth only through the channel that the paper proposes (volatility of fiscal policy in our paper). In that sense, using an instrument for more than one mediating channel is incorrect and the associated regressions will produce biased and inconsistent results. Bazzi and Clemens (2009) illustrate this point by using several empirical studies. The problem can fundamentally be seen as one of omitted variables: if there is an omitted endogenous variable, which can be instrumented with our instruments (Z), then the instruments will be correlated with the errors; hence the argument that researchers cannot use the same instrument in regressions with the same dependent variable instrumenting for different endogenous variables. One can reduce this problem to the following simple set-up with two endogenous variables and one instrument. Suppose that the true regression is:

$$Y = \beta X + \alpha W + \epsilon \qquad (D1)$$

Assume that both X and W are endogenous and Z is a valid instrument for both (i.e. orthogonal to the errors and highly correlated with X and W). Because we have one instrument for two endogenous variables, it is impossible to identify the effect of each of these variables on Y. Often this simple problem is overlooked when W is excluded from the regression, and the researcher uses Z as an instrument for X to estimate:

$$Y = \beta X + \nu \qquad (D2)$$

Since $\nu = \alpha W + \epsilon$ and $E[Z'W] \neq 0$, the IV regression is invalid.

There is a parallel between this problem and that of omitted variables in a standard OLS regression. Even if X and W are exogenous, omitting one of them could produce biased results in the estimation of the equation above (as long as they are not orthogonal). The literature on economic growth over the last decade

can be seen as an ongoing race to include an increasing number of controls in the regression to minimize the risk of an omitted variable bias.

In our setting, a skeptic might argue that there is an omitted endogenous variable, which is also correlated with our main instrument (political constraints). What do Bazzi and Clemens (2009) recommend in this case? In their concluding remarks, they make two recommendations relevant to our paper:

(1) *"Generalize the theoretical underpinnings of an instrument to account for other published results with the same instrument."* Although we do not provide a specific model, from a theoretical point of view the number of veto points can be easily linked to fiscal policy volatility. An extensive literature has looked at the connection between budget processes, institutions, and fiscal policy outcomes.[27] We believe that what we are presenting is a very specific channel through which a certain institutional set-up (number of veto points) affects growth. But what matters is whether there are other potential channels — different from policy volatility — through which veto points affect economic growth. Following Bazzi and Clemens's suggestion, we have completed a thorough search of the papers that cite Henisz's work (our measure of constraints is borrowed from his work). While his index has been used in many papers, we did not find a single paper where political constraints, as measured by Henisz, have been used as an instrument in a growth regression. This finding offers at least a minimum level of reassurance that we are not using an instrument, which had been used by others to explore the validity of another growth channel. In other words, there is no obvious omitted variable in the literature that could be a mediating channel in the regression we are running. Still, the fact that no one else has written a paper using this particular instrument does not rule out that such a variable exists. It is also possible that our instrument is correlated with another instrument that is in turn correlated with an omitted variable. To address these concerns we follow the second recommendation of Bazzi and Clemens (2009).

(2) *"Deploy the latest tools of probing validity."* In particular, they mention several recent papers which propose different methods for sensitivity analysis when using instrumental variables. We borrow the set-up from Conley, Hansen and Rossi

[27] One recent example is Alesina, Campante and Tabellini (2008). They provide a model to explain fiscal policy outcomes and make use of the political constraints variable that we use in one of their specifications. A second example, also close to our empirical specification, is Woo (2009). More generally, Persson and Tabellini (2002) offer a general discussion of the role of checks and balances, separation of powers, and appropriate budgetary procedures.

(forthcoming), henceforth referred to as CNR, to present our sensitivity analysis. Going back to equation ($D1$) above: Suppose that the projection of W onto Z is θZ. Then in regression ($D1$), we can substitute for W its projection and generate the following regression:

$$Y = \beta X + \gamma Z + \xi \qquad (D3)$$

This equation is listed (in matrix form) as equation (1) in CNR. In this regression, Z will be a valid instrument for X, in the sense that it will be uncorrelated with the error term. Of course, we cannot estimate this equation because Z is both a regressor and an instrument. The standard IV regression assumes that $\gamma = 0$. CNR suggest relaxing this assumption and exploring how the estimate of the main coefficient of interest, β, varies when γ deviates from zero.

CNR propose three different methods for sensitivity analysis. We use the so-called *Union of Confidence Intervals* method. In terms of coverage, as CNR point out, this is the most conservative interval estimate for the parameter of interest β. The method is based on the assumption that the researcher knows the support of γ, G. If the true value of γ is denoted by $\gamma_0 \in G$, then we can estimate β consistently by using Z as an instrument for X in the following regression:

$$(Y - \gamma_0 Z) = \beta X + \xi \qquad (D4)$$

The essence of this method is to obtain a $(1-\alpha)$ confidence interval for β conditional on a specific $\gamma_0 \in G$, and then to construct the confidence interval for β as the union of all confidence intervals for $\gamma_0 \in G$. This union of confidence intervals will be quite large but it will cover the true parameter value with a probability of at least $(1 - \alpha)$. The difficult question is to determine the support of γ. We follow one of the examples from CNR by selecting a range linked to the key coefficient of interest. We let $\gamma_0 \in [-\delta\beta, \delta\beta]$, where β is the coefficient on policy volatility. We take the regression from Table 3, column 2 as our baseline. For this regression the estimate of β is -2.90. We focus on our main instrument political constraints and, following CNR's example of demand for margarine, we allow the "indirect effect" of political constraints to be at most 30%. So, we vary δ from 0 to 0.3. This implies that the union of confidence intervals is constructed for $\gamma_0 \in [-0.87, 0.87]$. There is one issue that is not easy to resolve: the interpretation of this range. Is it wide enough? The problem is that without having a detailed perspective on what the additional channel is, the variance of the omitted variable or its correlation with the instrument, it is impossible to provide a straightforward interpretation of this

range. If we had a specific channel or an omitted variable in mind, this exercise could be even more meaningful. We still find this analysis quite useful because it shows that for some deviations from the assumption of pure exogeneity, the effect of policy volatility on growth remains negative and significant. The dashed lines in Figure 2 represent the lower and upper bounds for the union of confidence intervals, where the confidence interval for each specific γ_0 has coverage of 95%.

[Insert Figure 2 here]

It is clear that at some point the union of confidence intervals will include zero and the null of no effect will not be rejected. This observation, however, applies to any instrumental variables regression. But the main contribution of the sensitivity analysis is to show that mild relaxation of the excludability restriction does not invalidate our inference completely.

To sum up, Bazzi and Clemens (2009) argue that in order to produce more convincing results researchers should account for the findings in the literature, provide theoretical arguments to include both new and old results, and carry out sensitivity analysis. We think that these points can be linked together: If a variable has a clear theoretical justification, the range for the sensitivity analysis can be reduced. The link between veto points and fiscal policy volatility is quite strong and obvious; hence we think that the sensitivity analysis performed is informative. Of course, in this methodology one will never have definitive evidence that rules out the existence of other effects. The usefulness of our exploration is that it shows that the additional effect must be very strong in order to overturn our findings.

Table 1. Institutions and Policy Volatility

Dependent variable:Policy Volatility 1970-2007

	(1) Univariate	(2) Baseline	(3) Rich	(4) Poor
Constraints on the Executive	-0.365 (0.046)***	-0.234 (0.046)***	-0.143 (0.070)*	-0.191 (0.085)**
Presidential		0.759 (0.139)***	0.736 (0.194)***	0.294 (0.254)
Majoritarian		-0.122 (0.116)	0.081 (0.202)	-0.378 (0.155)**
Elections		-1.469 (0.439)***	-1.079 (0.635)	-1.182 (0.516)**
Constant	2.161 (0.084)***	2.036 (0.191)***	1.485 (0.246)***	2.471 (0.286)***
Observations	83	81	29	52
R-squared	0.38	0.58	0.40	0.28

Robust standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2. Average Growth and Policy Volatility

Dependent variable: Growth rate of output per capita 1970-2007

	(1) Univariate	(2) Baseline	(3) Rich	(4) Poor	(5) Additional Controls
Policy Volatility	-0.813 (0.153)***	-1.358 (0.356)***	-0.585 (0.274)**	-1.558 (0.459)***	-1.011 (0.362)***
Government Size		-0.001 (0.013)	-0.066 (0.019)***	0.007 (0.014)	-0.000 (0.013)
Investment Price		-0.003 (0.003)	-0.006 (-0.004)	-0.003 (0.005)	-0.003 (0.003)
Initial GDPpc		-1.195 (0.277)***	-0.719 (0.475)	-1.381 (0.370)***	-1.232 (0.269)***
Primary Enrolment		2.806 (0.732)***	-0.526 (4.268)	2.896 (0.800)***	3.153 (0.691)***
Openness		0.011 (0.004)***	0.006 (0.005)	0.01 (0.006)*	0.01 (0.004)**
Output Volatility					-0.427 (0.412)
Inflation					-0.02 (0.017)
Variance of Inflation					0.000 (0.000)
Constant	3.169 (0.268)***	11.599 (2.514)***	11.065 (6.426)*	13.233 (3.297)***	11.923 (2.512)***
Observations	93	87	30	57	87
R-squared	0.16	0.45	0.57	0.45	0.49

Robust standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%
All controls are calculated for the period 1967-1969 except Prim. Enrolment calculated for 1960-1964. Policy Volatility is calculated as the log of the standard deviations of the policy residuals from 1970 to 2007.

Table 3: Average growth and policy volatility: Instrumental Variables Estimation

Dependent variable: Growth rate of output per capita 1970-2007

	(1) Univariate	(2) Baseline	(3) Rich	(4) Poor	(5) Additional Controls
Policy Volatility	-1.094 (0.234)***	-2.902 (0.643)***	-1.791 (0.838)**	-2.871 (0.966)***	-3.378 (1.211)***
Government Size		0.005 (0.014)	-0.054 (0.038)	0.015 (0.015)	0.002 (0.015)
Investment Price		-0.002 (0.003)	-0.002 (0.003)	-0.006 (0.006)	-0.003 (0.003)
Initial GDPpc		-2.066 (0.393)***	-1.516 (0.708)**	-2.036 (0.455)***	-1.965 (0.418)***
Primary Enrolment		2.597 (0.885)***	1.351 (7.955)	2.606 (0.937)***	2.304 (1.217)*
Openness		0.016 (0.005)***	0.009 (0.009)	0.015 (0.005)***	0.014 (0.005)**
Output Volatility					1.079 (0.992)
Inflation					-0.012 (0.024)
Variance of Inflation					0.000 (0.000)
Constant	3.603 (0.426)***	21.159 (4.035)***	17.261 (9.954)*	21.028 (4.772)***	20.141 (4.365)***
Observations	81	79	29	50	79
OID Test	0.01	0.81	0.85	0.73	0.86

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. All controls are calculated for the period 1967-69 except Primary Enrolment which is for 1960. Policy Volatility is calculated as the log of the standard deviation of the policy residuals from 1970 to 2007. Instruments for policy volatility: Constraints in 1969, Presidential, Majoritarian and Elections. The last row reports the p-value from a test of overidentification.

Table 4: Robustness: Sala-i-Martin et al. (2004) Variables

Dependent variable: Growth rate of output per capita 1970-2007

	(1) Using Sala-i-Martin et al. Data	(2) Top 6 (OLS)	(3) Top 6 (IV)
Policy Volatility	-1.373	-0.835	-2.309
	(0.279)***	(0.365)**	(0.873)**
Investment Price	-0.004		
	-0.004		
Government Size	-5.88		
	(2.020)***		
Initial GDPpc	-1.294		
	(0.288)***		
Primary Enrolment	2.816		
	(0.651)***		
Openness	1.326		
	(0.443)***		
Investment Price		-0.002	0.000
		(0.002)	(0.003)
Initial GDPpc		-1.042	-1.705
		(0.249)***	(0.463)***
Primary Enrolment		2.426	2.324
		(0.656)***	(0.766)***
East Asian		1.103	0.627
		(0.554)**	(0.810)
Tropical Area		-0.917	-0.192
		(0.301)***	(0.591)
Population Coastal Area		0.001	0.001
		(0.000)***	(0.000)***
Constant	12.16	10.449	18.009
	(2.377)***	(2.407)***	(4.812)***
Observations	85	85	78
R-squared	0.6	0.57	
OID test			0.93

Robust standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.
In the first column we use data from Sala-i-Martin et al. (2004)
The dependent variable in this column is for the period 1960-69.
In columns (2) and (3) we use the same data as in Tables 2 and 3.
Initial GDPpc and Investment Price are calculated for the period
1967-69. Primary Enrolment is for 1960. The remaining controls
are as defined by Sala-i-Martin et al. (2004), see also Data Appendix.
Policy Volatility is claculated as the standard deviations of the policy
residuals from 1970 to 2007. Instruments for Policy Volatility:
Constraints in 1969, Presidential, Majoritarian and Elections.
The last row reports the p-value from a test of overidentification.

Table 5: Average Growth, Policy Volatility and Political Constraints

Dependent variable: Growth rate of output per capita 1970-2007

	(1) Univariate (OLS)	(2) Both Variables (OLS)	(3) Baseline (OLS)	(4) Volatility instrumented	(5) Constraints instrumented	(6) Both instrumented
Constraints	0.217	-0.190	-0.014	-0.107	0.478	0.027
	(0.105)**	(0.156)	(0.149)	(0.176)	(0.676)	(1.131)
Policy Volatility		-1.116	-1.502	-3.031	-1.163	-3.331
		(0.262)***	(0.373)***	(0.674)***	(0.483)**	(1.309)**
Government Size			0.004	0.007	-0.029	-0.016
			(0.013)	(0.016)	(0.025)	(0.041)
Investment Price			-0.006	-0.002	-0.005	-0.002
			(0.003)*	(0.003)	(0.003)	(0.004)
Intiial GDPpc			-1.275	-2.041	-1.721	-2.133
			(0.313)***	(0.415)***	(0.873)*	(1.158)*
Primary Enrolment			2.859	2.578	3.288	2.677
			(0.752)***	(0.912)***	(0.834)***	(1.241)**
Openness			0.012	0.016	0.012	0.014
			(0.004)***	(0.005)***	(0.006)*	(0.008)
Constant	1.475	3.881	12.545	21.259	15.059	22.943
	(0.250)***	(0.621)***	(2.725)***	(4.125)***	(6.231)**	(8.619)**
Observations	84	84	82	79	50	50
R-squared	0.03	0.21	0.49			
OID Test				0.23	0.02	0.58

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.
All controls are calculated for the period 1967-69 except Primary Enrolment which is for 1960.
Policy Volatility is calculatedas the log of the standard deviation of the policy residuals from 1970 to 2000.
Constraints are the constraints on the executive in 1969. Instruments for column (3): Presidential, Majoritarian and Elections. In columns (4) and (5) we also use the logarithm of settlers' mortality as calculated by Acemoglu et al. (2001). The last row reports the p-value from a test of overidentification.

Table 6: The Effect of Policy Volatility within Institutional Structure

Dependent variable: Growth rate of output per capita 1970-2007

	(1) Constr. < 2 (OLS)	(2) Constr. >= 2 (OLS)	(3) Constr. < 2 (IV)	(4) Constr. >= 2 (IV)	(5) with dummies (OLS)
Policy Volatility	-1.835	-0.387	-2.559	-1.713	
	(0.428)***	(0.318)	(1.087)**	(0.618)**	
Government Size	0.022	-0.054	0.019	-0.03	-0.004
	(0.014)	(0.012)***	(0.031)	(0.014)**	(0.016)
Investment Price	-0.009	0	-0.005	0.002	-0.009
	(0.004)**	(0.002)	(0.005)	(0.003)	(0.004)**
Initial GDPpc	-1.452	-0.574	-1.735	-1.475	-1.452
	(0.317)***	(0.469)	(0.507)***	(0.778)*	(0.322)***
Primary Enrolment	2.986	-1.558	2.716	-0.191	3.015
	(0.805)***	(2.228)	(0.935)***	(2.944)	(0.765)***
Openness	0.011	0.016	0.013	0.021	0.013
	(0.006)*	(0.007)**	(0.006)**	(0.010)**	(0.005)***
d0					14.194
					(2.990)***
d1					15.322
					(2.603)***
d2					13.504
					(3.021)***
d3					13.344
					(2.843)***
d4					13.517
					(4.184)***
d0*Policy Volatility					-1.48
					(0.510)***
d1*Policy Volatility					-2.466
					(0.361)***
d2*Policy Volatility					-0.816
					(0.401)**
d3*Policy Volatility					-0.736
					(0.366)**
d4*Policy Volatility					-0.309
					(3.519)
Constant	14.428	9.363	17.977	17.032	
	(2.764)***	(3.437)**	(5.859)***	(5.047)***	
Observations	50	31	48	31	81
R-squared	0.56	0.59			0.82
OID Test			0.05	0.71	

Robust standard errors in parentheses. All regressions include an intercept.
significant at 10%; ** significant at 5%; *** significant at 1%. All controls are calculated for the period 1967-69 except Primary Enrolment which is for 1960. Policy Volatility is calculated as the log of the standard deviation of the policy residuals from 1970 to 2007. Instruments for Policy Volatility: Constraints in 1969, Presidential, Majoritarian and Elections. The dummies d0-d4 in the last column are defined as: d0 is equal to 1 for countries with no constraints, d1 is equal to 1 for countries with 1 constraint, etc. The last row reports the p-value from a test of overidentification.

Table 7: Panel Estimation I, Pooled OLS

Dependent variable: Growth rate of output per capita by decades: (1965-74, 1975-1984, 1985-1994, 1995-2004)

	(1) Univariate	(2) Baseline	(3) With lagged volatility	(4) Rich	(5) Poor	(6) With output volatility	(7) With lagged growth
Policy Volatility	-0.678 (0.150)***	-0.918 (0.279)***					
Lagged Policy Volatility			-1.042 (0.274)***	-0.343 (0.453)	-1.294 (0.356)***	-0.976 (0.294)***	-0.957 (0.264)***
Initial GDPpc		-1.152 (0.286)***	-1.086 (0.306)***	-0.565 (0.808)	-1.073 (0.369)***	-1.110 (0.309)***	-1.207 (0.313)***
Primary Schooling		3.262 (0.817)***	3.064 (0.939)***	3.171 (2.216)	2.996 (1.040)***	3.075 (0.944)***	3.360 (0.945)***
Openness		0.011 (0.003)***	0.011 (0.003)***	0.009 (0.006)	0.011 (0.003)***	0.012 (0.003)***	0.01 (0.002)***
Government Size		-0.05 (0.025)**	-0.056 (0.022)**	-0.056 (0.045)	-0.052 (0.025)**	-0.054 (0.022)**	-0.044 (0.023)*
Investment Price		-0.006 (0.003)*	-0.001 (0.004)	-0.010 (0.007)	0.000 (0.005)	-0.001 (0.004)	0.000 (0.004)
Lagged Growth							0.167 (0.070)**
Lagged Output Volatility						-0.060 (0.116)	
Constant	2.821 (0.226)***	11.602 (2.460)***	10.856 (2.578)***	5.874 (7.131)	11.109 (3.039)***	11.084 (2.595)***	11.081 (2.609)***
Observations	351	292	221	86	135	221	221
R-squared	0.06	0.20	0.18	0.14	0.18	0.18	0.21

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable and policy volatility are calculated for the periods 1965-74, 1975-1984, 1985-1994 and 1995-2000. When lagged policy volatility is used it is the policy volatility for the previous decade. The controls are calculated for the three-year periods preceeding the decade for which the growth rate is calculated.

Table 8: Panel Estimation II, Controlling for Fixed Effects

Dependent variable: Growth rate of output per capita by decades: (1965-74, 1975-1984, 1985-1994, 1995-2004)

	(1) Univariate country effects	(2) Baseline country effects	(3) Baseline time effects	(4) With time and country effects	(5) Output Vol. time and country effects	(6) Arellano Bond estimator	(7) Arellano Bond estimator with time effects
Lagged Policy Volatility	-0.172 (0.323)	-0.921 (0.323)***	-1.032 (0.278)***	-0.857 (0.299)***	-0.867 (0.300)***	-1.033 (0.189)***	-0.730 (0.195)***
Initial GDPpc		-5.219 (0.707)***	-1.050 (0.306)***	-6.506 (0.710)***	-6.478 (0.713)***	-3.813 (0.227)***	-4.304 (0.360)***
Primary Schooling		9.418 (2.005)***	2.876 (0.921)***	1.308 (2.449)	1.213 (2.459)	2.447 (1.171)**	-0.453 (1.225)
Openness		0.018 (0.011)*	0.011 (0.003)***	0.015 (0.010)	0.015 (0.010)	0.014 (0.004)***	0.015 (0.004)***
Government Size		-0.016 (0.038)	-0.058 (0.022)***	-0.040 (0.037)	-0.042 (0.037)	-0.049 (0.019)***	-0.033 (0.016)**
Investment Price		0.008 (0.009)	-0.002 (0.004)	0.011 (0.008)	0.011 (0.008)	-0.005 (0.002)**	0.000 (0.003)
Lagged Output Volatility					0.036 (0.060)		
Lagged Growth						-0.297 (0.027)***	-0.256 (0.044)***
Lagged Growth (2nd)						-0.231 (0.034)***	-0.129 (0.040)***
Constant	1.647 (0.547)***	40.525 (5.762)***	11.069 (2.576)***	58.225 (6.481)***	57.985 (6.509)***	35.981 (2.234)***	41.471 (2.701)***
Country Effects	Yes	Yes	No	Yes	Yes	Yes	Yes
Time Effects	No	No	Yes	Yes	Yes	No	Yes
Observations	259	221	221	221	221	143	143
Number of Countries	93	78	78	78	78	77	77
R-squared	0.00	0.32	0.19	0.43	0.43		
OID Test						0.114	0.100
1st Order Serial Correlation						0.004	0.005

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.
The dependent variable and policy volatility are calculated for the periods 1965-74, 1975-1984, 1985-1994 and 1995-2000. When lagged policy volatility is used it is the policy volatility for the previous decade. The controls are calculated for the three-year periods preceeding the decade for which the growth rate is calculated.

Appendix Table B1: Average Growth and Policy Volatility (controlling for Sala-et-Martin et al. variables). IV estimation.

Dependent variable: Growth rate of output per capita 1970-2000

Included Control	(1) Coefficient on the marginal variable	(2) Coefficient on policy volatility	(3) R^2
East Asian dummy	-2.685	0.768	0.41
	(0.686)***	(0.723)	
Fraction of tropical area	-2.84	-0.244	0.37
	(0.792)***	(0.564)	
Population density coastal 1960s	-2.69	0.001	0.41
	(0.735)***	(0.001)	
Ethnolinguistic fractionalization	-2.994	0.461	0.33
	(0.672)***	(0.391)	
Real exchange rate distortions	-2.434	-0.001	0.47
	(0.753)***	(0.004)	
Population density 1960	-2.907	0.001	0.38
	(0.635)***	(0.000)***	
Fraction speaking foreign language	-3.063	-1.143	0.38
	(0.662)***	(0.694)	
Fraction Buddhist	-3.407	0.618	0.25
	(1.107)***	(0.764)	
Fraction Muslim	-2.821	1.012	0.39
	(0.580)***	(0.546)*	
Years open	-2.752	0.215	0.39
	(0.778)***	(1.083)	
Spanish colony	-2.849	-0.167	0.37
	(0.891)***	(0.367)	
Fraction GDP in mining	-2.964	3.058	0.37
	(0.642)***	(1.304)**	
Latin American dummy	-2.795	0.596	0.38
	(0.706)***	(0.664)	
African dummy	-2.994	-1.224	0.37
	(0.666)***	(0.363)***	
Fraction Confucian	-2.685	3.607	0.43
	(0.603)***	(0.833)***	
Life expectancy in 1960	-2.837	0.051	0.37
	(0.667)***	(0.028)*	
Malaria prevalence in 1960s	-3.311	-0.649	0.27
	(0.739)***	(0.584)	

Robust standard errors in parentheses. * significant at 10%;
** significant at 5%; *** significant at 1%.

Figure 1. Policy volatility and economic growth
(1970 - 2007)

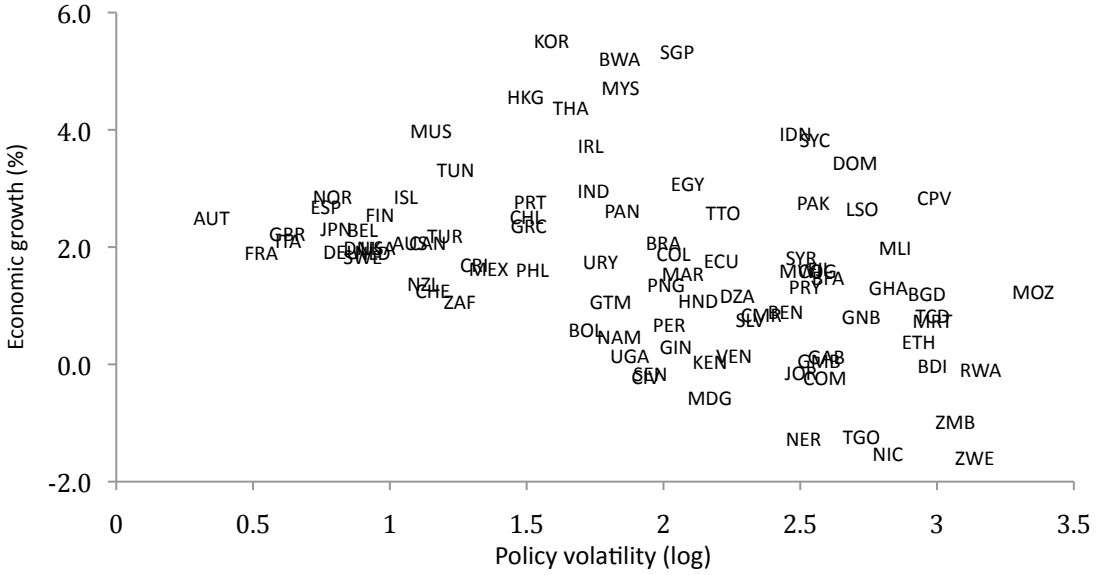

Figure 2: 95% Interval Estimates for λ (Appendix D)