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ORIGINAL ARTICLE

Political institutions and economic growth reconsidered

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Abstract The debate on the relationship between institutions and economic development is discussed, focusing on two illustrations, i.e., the impact of democracy and political instability on economic growth. Various pitfalls of existing research are identified, like sensitivity of the outcomes to model specification, sample heterogeneity, measurement of political variables, and the treatment of the time dimension.

Keywords Political institutions · Economic growth · Robustness

JEL Classification P43

1 Introduction

I will present my views on how to investigate the impact of political institutions on economic growth. North (1981) defines institutions as "a set of rules, compliance procedures, and moral and ethical behavioral norms designed to *constrain* the behavior of individuals in the interests of maximizing the wealth or utility of principals" (pp. 201–202). The key word here is constraints. As pointed out by Glaeser et al. (2004), there is another essential aspect of institutions: the constraints need to be reasonably permanent or durable.

I admit that this topic is not at all new.[1] In fact, it has been on the research agenda for quite a while. However, I am not very satisfied with the results so far, mainly because I feel that the methodology underlying most of the research may not be adequate to tackle this issue. I will use two examples: the impact of the political system on economic growth and

[1] For surveys see Cox and McCubbins (2001), Holcombe (2001), Kurrild-Klitgaard and Berggren (2004) and Gwartney et al. (2005).

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the impact of political instability on economic growth.[2] I will also provide some 'footnotes' on what I consider to be perverse research incentives in our profession.

2 Democracy and economic growth

Over time, views on the relationship between democracy and economic development have changed. In the 1960s and 1970s democracy and economic growth were often considered as competing concerns. As Bhagwati (1966, pp. 203–204) put it: "the political economy of development poses a cruel choice between rapid (self-sustained) expansion and democratic process". More recently, authors often came to more optimistic conclusions. Rodrik (2000, p. 22) states, for instance, that "Recent empirical studies based on samples of more than 100 countries suggest that there is little reason to believe democracy is conducive to lower growth over long time spans." Indeed, Sala-i-Martin (1997) and Sturm and De Haan (2005) reach the conclusion that respect for political rights and civil liberties is robustly related to economic growth.[3] Probably the best known paper in this field is Barro (1996). I will use this study to illustrate what goes wrong in this line of research.[4]

Barro has estimated a growth model applying a panel of roughly 100 countries observed from 1960 to 1990. The dependent variables are the growth rates of real per capita GDP over three periods: 1965–75, 1975–85, and 1985–90. The regressions include various control variables including measures of human capital in the form of schooling and health, the initial level of GDP, the fertility rate, government spending for consumption and education, the black-market premium on foreign exchange, an index of the maintenance of the rule of law, the ratio of gross investment to GDP, and the change in the terms of trade. If the democracy indicator, i.e., the so-called Gastil index, is entered in a quadratic form in this model the estimated coefficient of the linear term is positive whereas that of the squared term is negative. So these results suggest that, at low levels of democracy, more political freedom enhances growth. The growth rate reaches a peak at a middle level of democracy and then diminishes if democracy continues to rise.[5]

What is wrong with this model? I see at least four fundamental problems:

1. arbitrary model
2. possible sample heterogeneity
3. measurement of democracy

[2] The empirical evidence that I will show—by way of illustration—draws heavily on my collaboration with Jan-Egbert Sturm and Richard Jong-A-Pin.

[3] Doucouliagos and Ulubasoglu (2006) apply meta-regression analysis to 470 estimates derived from 81 papers on the democracy-growth association. They find that, once all the available evidence is considered, democracy has no direct effect on economic growth. On the other hand, it has robust and significant indirect effects on growth.

[4] This is not to say that Barro (1996) is representative of all studies in this line of research. See, for instance, Plümper and Martin (2003) for a much better attempt to model the relationship between democracy and growth.

[5] Barro (1996) also finds that democracy is positively related to income per capita, confirming the view generally associated with Lipset (1959), who argued that "only in a wealthy society in which relatively few citizens lived in real poverty could a situation exist in which the mass of the population could intelligently participate in politics and could develop the self-restraint necessary to avoid succumbing to the appeals of irresponsible demagogues" (p. 75). Recently, this view has been challenged by Acemoglu et al. (2005) who argue that including country fixed effects or employing instrumental-variables estimates removes the statistical association between income per capita and various measures of democracy.

4. treatment of time dimension

These problems are quite general: many empirical studies on the relationship between political institutions and growth suffer from one or more of these problems. First, from a theoretical perspective, the model as used by Barro (1996) is highly suspect: there is no theory that yields this particular specification.[6] Second, Barro does not check for the role of outliers and sample heterogeneity, while "It is well known in the statistics literature that the presence of a few influential outliers can either hide a relationship, or create the appearance of one where none exists . . ." (Temple 2000, p. 195). Third, Barro uses only the Gastil index as indicator of democracy while there exist at least 4 alternatives, as will be explained later on. All these variables are proxies for the latent variable "democracy" and may all suffer from measurement errors. Using just one particular indicator is therefore not the proper way to examine whether democracy matters. Unfortunately, this is what most studies on the relationship between political institutions and economic growth do. Finally, growth regressions are based on very strong assumptions about a single linear model being appropriate for all countries at all times, while very few countries have experienced consistently constant growth rates over periods of several decades (Hausmann et al. 2005). The more typical pattern is that countries experience phases of growth, stagnation, or decline of varying length. Even if a panel model is used—as in Barro (1996)—instead of a cross-country model there is no guarantee that differences in growth performances are picked up accurately. Most panel models use rather arbitrary periods as unit of observation so that it is unlikely that they will identify variations in growth rates over time.

I will discuss these shortcomings and outline the approach that I favor to deal with them.

3 Model specification

The model used by Barro (1996) is very arbitrary. Although Barro motivates the inclusion of his control variables, some of them are at least suspect, while other potentially relevant variables are not included. The basic problem here is that economic theory does not provide enough guidance to select the proper specification of an empirical growth model. In other words, growth theories are open-ended, i.e., the validity of one causal theory of growth does not imply the falsity of another. Sala-i-Martin (1997) identifies, for instance, around 60 variables that have been suggested to be correlated with economic growth. Levine and Renelt (1992) and Sala-i-Martin (1997) investigate the 'robustness' of regressions by checking how sensitive the estimated coefficient of each variable of interest is to the inclusion of additional explanatory variables. Although this so-called Extreme Bounds Analysis (EBA) has some severe limitations, to which I will return later, I think that it is a fairly neutral means to check robustness and compare the validity of conflicting findings in empirical research.[7]

The EBA, as first put forward by Leamer (1983), can be exemplified as follows. Equations of the following general form are estimated:

$$Y = \alpha M + \beta F + \gamma Z + u, \tag{1}$$

[6] Unfortunately, and here is my first 'footnote', the incentives in our profession are such that many similar models have since been published. Apparently, if a famous economist like Barro comes up with a model, there is no need to think further.

[7] Various recent studies apply the EBA. For instance, Sturm et al. (2005) use the EBA to examine to what extent variables are robust determinants of the likelihood that a country will receive IMF credit, while Baxter and Kouparitsas (2005) use it to analyze which variables affect business cycle synchronization.

where Y is the dependent variable; M is a vector of 'standard' explanatory variables; F is the variable of interest; Z is a vector of possible additional explanatory variables, which according to the literature may be related to the dependent variable; and u is an error term. The extreme bounds test for variable F says that if the lower extreme bound for $\beta-$ i.e., the lowest value for β minus two standard deviations—is negative, while the upper extreme bound for $\beta-$ i.e., the highest value for β plus two standard deviations—is positive, the variable F is not robustly related to Y.

Sala-i-Martin (1997) rightly argues that the test applied in the extreme bounds analysis is too strong for any variable to really pass it. If the distribution of the parameter of interest has some positive and some negative support, then one is bound to find one regression for which the estimated coefficient changes sign if enough regressions are run. Instead of analyzing the extreme bounds of the estimates of the coefficient of a particular variable, Sala-i-Martin (1997) suggests analyzing the entire distribution of the estimates of the parameter of interest. Broadly speaking, if the averaged 90% confidence interval of a regression coefficient does not include zero, Sala-i-Martin classifies the corresponding regressor as a variable that is strongly correlated with economic growth.

The EBA has a couple of limitations:

– the selection of variables to be included in the M vector is not obvious and as a consequence many of the models estimated may be misspecified;
– the number of variables in the M vector is arbitrarily often set at 3, following Levine and Renelt (1992);
– various variables in the Z vector may be proxies for the same (latent) variable and may cause multicollinearity problems.

Most of these concerns, however, can be addressed. As pointed out by Temple (2000), the selection of variables for inclusion in the M vector can be decided upon following a general-to-specific approach. In the BACE approach, which is yet another variant of the EBA as recently put forward by Sala-i-Martin et al. (2004), the number of variables to be included in the M vector is flexible.[8] The final problem can be dealt with by using factor analysis; I will come back to this when I address the measurement issue.

My conclusion is that authors should carefully check to what extent their results are affected by the inclusion of variables, be it using some variant of the EBA, or some other approach instead of relying on reporting their "favorite" regression. Unfortunately, even today many papers do not contain a solid sensitivity analysis.[9]

[8] The Bayesian Averaging of Classical Estimates (BACE) approach builds upon the approach as suggested by Sala-i-Martin (1997) in the sense that different specifications are estimated (by OLS) to check the sensitivity of the coefficient estimate of the variable of interest. The major innovation of BACE as compared to the Sala-i-Martin's approach is that there is no set of fixed variables included and the number of explanatory variables in the specifications is flexible. The biggest disadvantages of the BACE approach are the need of having a balanced dataset, i.e., an equal number of observations for all regressions (due to the chosen weighting scheme), the restriction of limiting the list of potential variables to be less than the number of observations and the computational burden.

[9] In my view editors of journals have a clear responsibility here as well, since the incentives in our profession are wrong in this respect. Suppose, an author has come up with an ingenious theoretical model that (s)he can test. If the outcome were that the empirical results would not support the model, the likelihood that this paper will be accepted for publication is close to zero. Therefore, the author will torture the data long enough so that (s)he can report at least some support for the model, preferably using a model that is akin to a model used by a famous economist, like Robert Barro. As a consequence, the literature is flooded by papers that yield conflicting conclusions.

4 Outliers and sample heterogeneity[10]

Following Barnett and Lewis (1994, p. 316), we define an outlier as an observation 'lying outside' the typical relationship between the dependent and explanatory variables revealed by the remaining data. For instance, point A in Fig. 1(a) is clearly an outlier. Outliers in the dependent variable—i.e., in the y-direction—often possess large positive or large negative residuals, which are easy to detect by plotting them. Observations may be outlying for several reasons. The most obvious one involves problems with the quality of the data. Outliers in the explanatory variables may be more problematic than outliers in the dependent variable. As Fig. 1(b) shows, an unusual observation in the x-direction (B) can actually tilt the OLS regression line. In such a case we call the outlier a (bad) leverage point. Note that looking at the OLS residuals cannot uncover bad leverage points. If a leverage point tilts the regression line, deleting the points with the largest OLS residuals implies that some 'good' points would be deleted instead of the 'bad' leverage point.

Basically, there are two ways to deal with outliers: regression diagnostics and robust estimation. Diagnostics are certain statistics mostly computed from the OLS regression estimates with the purpose of pinpointing outliers and leverage points. When there is only one unusual observation, some of these methods work quite well. However, single-case diagnostics are well known to be inadequate in the presence of multiple outliers or leverage points (Temple 2000).

Take, for instance, Fig. 1c. Deleting either of the two outliers will have little effect on the regression outcome and will therefore not be spotted by the single-case diagnostics. The potential effect of one outlying observation is clearly masked by the presence of the other. Testing for groups of observations to be influential might solve this masking effect problem but is extremely cumbersome. Therefore I prefer so-called robust regression techniques that employ estimators that are not strongly affected by (groups of) outliers.

Two closely related methods are the Least Median of Squares (LMS) and Least Trimmed Squares (LTS) introduced by Rousseeuw (1984). LMS minimizes the median of the squared residuals. LTS typically minimizes the sum of squares over half the observations, the chosen half being the combination which gives the smallest residual sum of squares. According to Temple (2000), LTS is generally thought preferable to LMS.

When we take a parameter heterogeneity perspective, it is clear that we can think about outliers in another way (Temple 2000). Some observations may be entirely correct, but drawn from a different regime. In most research, it is implicitly assumed that only one

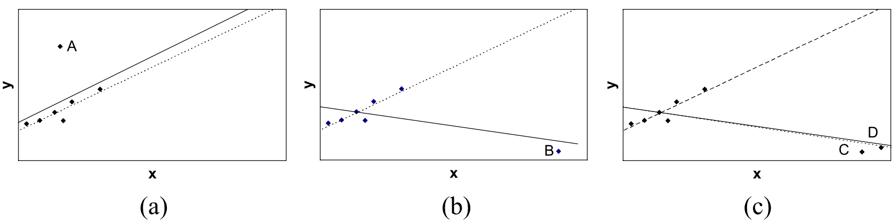

Fig. 1 Outlying observations and bad leverage points. The *solid lines* represent the OLS estimates including the unusual observation(s). The *dotted lines* represent the OLS estimates without the unusual observations A, B, or C. The *dashed line* represents the OLS estimate without observations C and D

[10]This part heavily draws on Sturm and De Haan (2005).

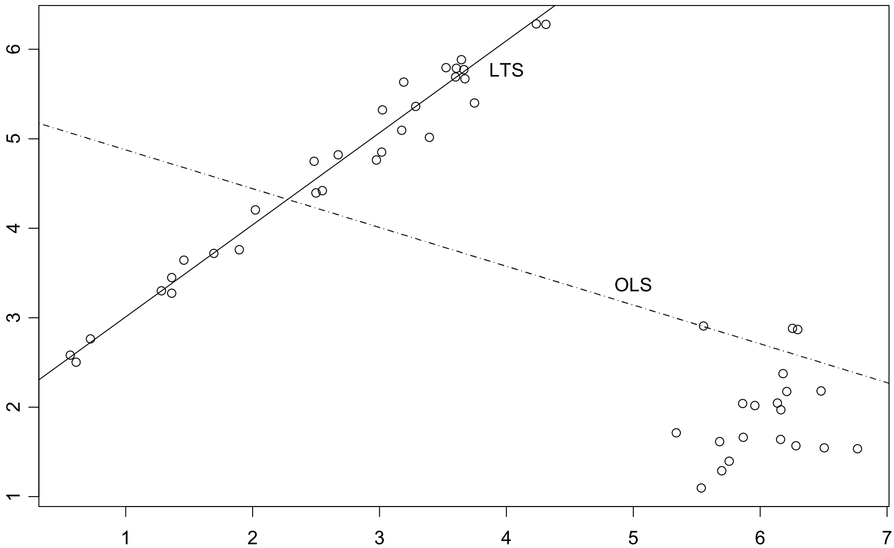

Fig. 2 Hypothetical example. Hypothetical dataset for series x and y of 50 observations. The first 30 observations have the following characteristics: series x is uniformly distributed between 0.5 and 4.5. Series y is distributed around the line $y = 2 + x + e$, where e is normally distributed with mean zero and standard deviation 0.2. The remaining 20 observations follow a Normal distribution: $x \sim N(6, 0.5)$ and $y \sim N(2, 0.5)$. The *solid line* represents the LTS estimate, whereas the *dotted line* represents the OLS estimate

regime generates the data, or that the parameters in the different regimes vary randomly. However, both assumptions may not be correct (see, e.g., Durlauf and Johnson 1995). Robust estimators can be thought of as trying to seek out the most coherent part of the data, the part best approximated by the model being estimated.

We can illustrate this by the following example taken from Sturm and De Haan (2005). Figure 2 shows a dataset for two variables (x and y) in which 40% of the (50) observations follow a different distribution than the rest of the observations. Assume a researcher would not know this and would simply estimate a linear relationship between x and y. As the OLS model assumes all observations are drawn from one single distribution, the OLS regression line estimated by this researcher as shown in Fig. 2 will not reveal any valuable information. In contrast, the LTS regression line looks for a linear relationship, which fits the majority of the data. As 60% of the dataset follows a linear relationship, LTS will reveal that relationship. The remaining 40% of the observation will have large negative residuals which are easily depicted by graphing the standardized residuals. As this example shows, that does not mean that those observations should be ignored and simply thrown away. Those observations reveal that the linear model is not adequate for the entire dataset as not all observations follow the same regime.

5 Testing for the robust impact of democracy using robust estimators

According to Temple (2000, p. 195), "any good approach to model uncertainty should ideally be robust to observations that are measured with error, or drawn from a different regime… I propose using a simple variant of EBA in which each regression is first estimated by robust methods". This is exactly what Jan-Egbert Sturm and I have done in a

Table 1 Democracy and economic growth, Extreme Bounds Analysis: OLS and LTS/RLS (using Gastil's civil liberty index)

(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variable:	Lower bound:	Upper bound:	Percentage of the regressions with significant β	β	Standard deviation of β	Unweighed CDF(0) test
	OLS outcomes					
Civil liberty	−0.0089	0.0039	33.50	−0.0025	0.0014	0.93
	LTS/RLS outcomes					
Civil liberty	−0.0137	0.0060	73.99	−0.0031**	0.0013	0.95

Note: Each row is based on 10,700 regressions. * and ** indicate significance at the 5 and 1 percent level, respectively

recent paper (Sturm and De Haan 2005). In work in progress, we also apply this method to examine the impact of democracy on economic growth.

Table 1 shows the outcomes of two EBAs: one estimated with OLS, the other one estimated by LTS/RLS.[11] In the first model, the coefficient of the civil liberty indicator (the proxy for democracy in this example) is not significantly different from zero, while in the other it is. I do not want to draw any firm conclusions from these regressions, but they illustrate that employing robust estimators may make a difference. The reason that I do not want to draw firm conclusions from this model has to with measurement issues, which is the third problem on my list of shortcomings of existing research on the relationship between political institutions and economic growth.

6 Measurement issues

Most studies—like Barro (1996) and, indeed, the model just shown—employ just one indicator for democracy and do not test whether conclusions are sensitive to the use of other indicators that have been suggested in the literature. There are various alternative indicators for the political system:

- The Gastil data and dummy variables based on it to take into account that the Gastil data are not cardinal variables. Gastil has created two measures of liberty: political liberty and civil liberty. Political (civil) rights are ranked from 1 (the highest degree of liberty) to 7 (the lowest). The political rights rankings are based on the degree to which individuals in a state have control over those who govern. The civil liberties rankings purport to measure the rights of the individual (e.g., independence of the judiciary, freedom of the press, freedom of assembly and demonstration, freedom of political organization, free trade unions, free religious institutions).

[11] The dependent variable is the average growth of per capita GDP. In the M vector the following variables are included: average equipment investment as share of GDP; the secondary school enrolment rate in 1960; and average population growth. We use the dataset of Sala-i-Martin (1997) that refers to 1960–92. A country has been included if we have observations for more than half of these years.

- The Polity IV indicators (Marshall and Jaggers 2004). The Polity Democracy Index ranges from 0 to 10 and is derived from coding the competitiveness of political participation, the openness and competitiveness of executive recruitment and constraints on the chief executive. The Polity Autocracy Index also ranges from 0 to 10 and is constructed in a similar way to the democracy score based on scoring countries according to competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment and constraints on the chief executive.
- Gasiorowski's (1993) dataset that measures how long a country has been a democracy or autocracy.[12]
- The democracy indicator developed by Vanhanen (2000), which is based on participation and competition. The first is measured by the smaller parties' share of the votes cast in parliamentary or presidential elections, or both. The second is measured by the percentage of the total population who actually voted in the election concerned.
- The dataset of Przeworski et al. (2000) in which they classify country-years on the basis of some electoral rules (chief executive must be elected, legislature must be elected, there must be more than one party).

All of these indicators are proxies for the latent variable "democracy" and they may all suffer from measurement errors. Their correlation is sometimes strikingly low.[13] The use of latent variable techniques, like factor analysis, is in my view called for under these circumstances.

Let me elaborate on this, using my second example, i.e., research on political instability and economic growth.[14] Since political instability in a country cannot be measured directly, empirical studies often rely on indicators like the number of *coups d'état* (Londregan and Poole 1990) or the number of political revolutions (Barro 1991). While these indicators probably capture some aspects of political instability, they are certainly not perfect. Some authors acknowledge the problem of measurement error and combine various indicators in a single index, while others predict the propensity of government change using binary choice models in which the occurrence of government transfers is related to various economic, political and institutional variables (e.g., Cukierman et al. 1992). These approaches have in common that the used indicators are assumed to be highly correlated with political instability and that political instability is a one-dimensional concept. The first assumption is generally validated on theoretical grounds, but it is never thoroughly tested, while the second one has been disputed (see, for instance, Hibbs 1973).

Jong-A-Pin (2006) has applied factor analysis to 26 political instability indicators, which have all been used in some previous study and which are available for 128 countries for the

[12]This variable focuses on the duration of a regime, which is, according to Clague et al. (1996), more relevant than the nature of the regime as such. These authors argue that the quality of economic policies and institutions depends partly on the incentives and constraints faced by policymakers, which vary from one autocracy to another and from one democracy to another. Clague et al. (1996) hypothesize that in autocracies it is the time horizon of the individual autocrat that is the main determinant of property and contract rights, whereas in democracies these rights depend upon whether the democratic system is durable. In a very similar way, Wintrobe (1998) distinguishes between two sorts of authoritarian regimes: totalitarians and tinpots. Whereas totalitarians derive utility from power as such and try to maximize it, tinpots choose the level of power that secures their remaining in office.

[13]Glaeser et al. (2004) discuss the measurement of some proxies for political institutions that have been used in recent research, asking if these measures of institutions reflect a) constraints on government and b) permanent or at least durable features of the environment. They argue that, in fact, these proxies—including one of the Polity IV indicators—reflect neither as they all measure outcomes, not some permanent characteristics that North refers to.

[14]The following part heavily draws on Jong-A-Pin (2006).

Table 2 Correlation matrix of the various dimensions of political instability

	Violence	Protest	Within instability	Regime instability
Violence	1.00			
Protest	0.28	1.00		
Within instability	0.01	−0.06	1.00	
Regime instability	0.37	0.40	−0.13	1.00

Source: Jong-A-Pin (2006)

period 1984–2003. The aim of the factor analysis model is to separate the information that is common to all indicators from the information that is unique to a single indicator. By assuming that the observed indicators are "generated" by a linear combination of unobserved factors and some individual error term, a simple model structure is imposed on the covariance matrix of the indicators. When a convenient and parsimonious model is specified, the factor analysis can be used to obtain unbiased predictions of the values of the unobserved latent variables. An additional advantage, which may especially be relevant in the context of the EBA, is that it reduces problems of multicollinearity.

Jong-A-Pin identifies four dimensions of political instability: (1) civil protest, (2) politically motivated violence, (3) instability *within* the political regime, and (4) instability *of* the political regime. Indicators that are associated with collective protest by the population are clearly the only variables that have high loadings for the first factor. The second factor has high loadings for the indicators associated with political violence and warfare, while the third factor corresponds to indicators reflecting changes within the political system, such as the changes in the chief executive and replacements of veto players in the political process. The indicators with high loadings on the fourth factor are the number of major constitutional changes, the number of *coups d'état* and the number of regime changes. As Table 2 shows, these dimensions are not highly correlated. So using just one indicator or one dimension is unlikely to fully capture the latent variable 'political instability'.

Jong-A-Pin (2006) also finds that the various dimensions of political instability that he has identified are differently correlated with economic growth. Using an augmented version of the model of Mankiw et al. (1992), he reports that the different dimensions of political instability do not have the same relationship with economic growth.

7 How to use the time dimension?

A single time trend does not adequately characterize the evolution of GDP per capita in many countries. In my view, the traditional cross-country and panel growth models are not well suited to use information provided by the time dimension. If panels are used, the time span should be at least 10 years; otherwise the model will not be able to distinguish properly between long-term growth and business cycles.[15] Unfortunately, the periods chosen in panel models are often justified only on the grounds that data were available at those frequencies

[15] Pritchett (2000) identifies four problems concerning the use of higher-frequency data in growth models, particularly with techniques that remove country-specific effects: lower power, greater measurement error, endogenicity, and dynamic misspecification.

or the researcher wanted to divide the whole period into equal chunks. These periods are unlikely to identify information provided by the development of variables over time. If, for instance, there is a high growth rate in the first five years and a low growth rate in the second half of the ten-year period, the period average will be rather uninformative. In recent research some alternatives have been suggested that, in my view, have the potential for using the information provided by the time dimension in a more promising way. Let me give you three examples.

Due to the fixation on long-run differences in growth, empirical growth research has underestimated the importance of instability and volatility in growth rates, especially in developing countries. Pritchett (2000) was one of the first studies to identify that instability in growth rates over time for a single country is great, relative to both the average level of growth and the variance across countries. Many countries have a break in their economic development. These shifts in growth rates lead to distinct growth patterns. While some countries have steady growth, others have rapid growth followed by stagnation, rapid growth followed by decline, continuous stagnation, or steady decline. Pritchett identifies the following patterns:

- *Steep hills*, i.e., countries that had growth rates higher than 3% in both periods.
- *Hills*, i.e., countries with growth rates higher than 1.5% in each period.
- *Plateaus*, i.e., countries that grew more than 1.5% before their structural break, but afterward growth fell to less than 1.5%, although it remained positive.
- *Mountains*, i.e., countries that had growth rates higher than 1.5% before their trend break, but negative rates afterward.[16]
- *Plains*, i.e., countries with growth rates less than 1.5% both before and after their structural break.[17]
- *Accelerators*, i.e., countries that did not have growth rates above 1.5% before their structural break, but did afterward.

I suggest using these country-specific patterns (or others) as units of observation in panel growth models instead of decade averages. This implies that the explanatory variables have to be constructed accordingly. This is a time-intensive approach, but worth the effort.

My second example is the study by Hausmann et al. (2005), who focus on turning points in growth performance, examining instances of rapid acceleration in economic growth. They identify more than 80 such episodes since the 1950s. They define a growth acceleration as an increase in per-capita growth of 2 percentage points or more. To qualify as an acceleration, the increase in growth has to be sustained for at least eight years and the post-acceleration growth rate has to be at least 3.5% per year. In addition, to rule out cases of pure recovery, they require that post-acceleration output exceed the pre-episode peak level of income. Countries can have more than one instance of growth acceleration as long as the dates are more than 5 years apart. Interestingly, these authors claim that the largest number of growth accelerations is actually in Africa, a continent that one hardly associates with economic growth. They also find that political-regime changes are statistically significant predictors of growth accelerations. A political regime change increases the probability of a growth acceleration by 5.3 percentage points. It is worthwhile to examine how robust the results of Hausmann et al. (2005) are.

[16] The mountains include some countries with cliffs, very sharp drops, usually resulting from war or civil unrest.

[17] Included among the plains countries are those with consistently negative growth rates that could be characterized as valleys.

My final example of how the time dimension can be taken into account is the study by Jones and Olken (2005), who examine whether national leaders, who change sharply and at potentially high frequency, have a causative effect on growth. As leadership transitions may be driven by underlying economic conditions, these authors focus on cases where the leader's rule ended at death due to either natural causes or an accident. They find evidence that leaders matter, notably in autocratic regimes. Their method is quite simple. First, they estimate the following regression:

$$g_{i,t} = \alpha_z PRE_z + \beta_z POST_z + v_i + v_t + \varepsilon_{i,t}, \quad (1)$$

where $g_{i,t}$ is the annual growth rate of real purchasing-power-parity GDP per capita, i indexes countries, t indexes time in years, and z indexes leader deaths. Country and time fixed effects are included through v_i and v_t, respectively. For each leader death, there is a separate set of dummies, denoted PRE_z and $POST_z$. PRE_z is a dummy equal to 1 in the T years prior to leader z's death in that leader's country, while $POST_z$ is a dummy equal to 1 in the T years after leader z's death in that leader's country. Jones and Olken estimate separate coefficients, α_z and β_z, for each leader death z. The model is estimated using all countries and all years of data, as countries without leader deaths can be used to help estimate time fixed effects. Jones and Olken employ parametric and non-parametric tests to examine whether there is any effect of leadership change on growth. This method can easily be applied to changes in political institutions, even though there is a problem of potential endogenicity. For instance, if there is a clear change in the political system, this method allows one to test whether economic growth differs before and after this change.

8 Conclusion

I have discussed research on the relationship between institutions and economic development, focusing on two illustrations, i.e., the impact of democracy and political instability on economic growth. Various pitfalls of existing research are identified, like sensitivity of the outcomes to model specification, sample heterogeneity, measurement of political variables, and the treatment of the time dimension. I have outlined some elements of what I consider to be a more promising approach of analyzing the relationship between political institutions and economic growth. I think that it will give us reliable answers to some questions that have been on our research agenda for so long.

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