Institutions and Economic Development. New tests and new doubts.

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September 30, 2010

Abstract

Is institutional quality a major driver of economic development? This paper tackles the question by focusing on the within-country variation of growth rates of GDP per capita. While previous attempts using this methodology have controlled for many of the standard determinants of the empirical growth literature, we argue that such approach is not adequate if good institutions are the main reason behind decisions to invest in human or physical capital accumulation or to engage in international trade. Our regressions exclude the proximate causes of growth in order to estimate the overall effect of institutional quality. Perhaps surprisingly, we find no support for the thesis that institutional quality improves economic growth. These results encourage a reconsideration of the evidence provided elsewhere in the literature.

Keywords: Institutions; Economic Development; Executive Constraints; Growth.

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1 Introduction

The last decade has seen a growing acceptance of the idea that institutions are the ultimate driver of economic development (see Acemoglu et al. 2005 for a review of the literature emphasizing this view). While discussions of the role of institutions in economic development can be traced all the way back to Adam Smith[1], the significant degree of consensus in today's debate is perhaps unprecedented. To a large extent, the current literature has used the work of Douglass C. North as the intellectual foundation on which the case of institutions as the ultimate driver of economic development could be built.

North describes institutions as "the humanly devised constraints that shape human interaction" (North 1990, p.3). The same essential idea can be found in later reformulations by other authors, for instance: "Economic institutions determine the incentives of and the constraints on economic actors, and shape economic outcomes" (Acemoglu et al. 2005). These are powerful metaphors for our ears, accustomed to think in terms of constrained maximization as the rule guiding personal actions. In a nutshell, North argues that if the "rules of the game" are such that agents can secure the returns of investing in physical capital, human capital and new ideas then these investments will take place and economic progress will follow. Otherwise, people would invest in rent seeking, political competition and violence; and economic stagnation or decline would be the consequence.

The empirical case for institutions as the main cause of long run economic development has been advanced and strengthened by Knack and Keefer (1995), Hall and Jones (1999), Acemoglu et al. (2001, 2002), Easterly and Levine (2003), Rodrik et al. (2004) and Feyrer and Sacerdote (2009) among

[1] See, for instance, Smith's discussion in Chapter 3, Book V of *The Wealth of Nations.* Most themes of the current literature on institutions, from secure property rights to the rule of law, had been pointed out by Smith more than two centuries ago.

others. The general outline that this section of the literature has followed was set up by Hall and Jones (1999). We start from the observation that the cross-sectional variation in measures of institutional quality is highly correlated with levels of GDP per capita. This cannot be interpreted as a causal relationship due to a likely endogeneity problem. To address this problem researchers instrument institutional quality with variables related to the country's climate, geography or history in the hope of uncovering a causal relationship.

The most influential work in this line of research has been Acemoglu et al. (2001, 2002). The authors instrument institutional quality with colonial-related variables such as the mortality of European settlers in colonial times or the population density of colonized areas; the argument being that these variables determined the degree of European settlement in the colonies which in turn dictated the initial institutional setting. They reach the conclusion that institutional quality has a large causal effect on economic development. Easterly and Levine (2003) and Rodrik et al. (2004) show that these results continue to hold when controlling for measures of geography and trade openness.

The above empirical literature can be criticized on two methodological aspects: it is cross-sectional and it uses levels (instead of growth rates) of GDP per capita[2].

Because cross-sectional regressions rely exclusively on the between-country variation in the data their results are always at risk of being biased by unobserved country characteristics. The issue is of particular importance in

[2] An additional criticism, raised most notably by Glaeser et al. (2004), is the difficulty to justify the use of historical variables as instruments for institutions. Instruments should not be correlated with the error term in the second-stage regressions, a condition that would be violated if the historical variables can influence development through channels other than institutional quality. Glaeser et al. (2004) put forward the fact that European colonizers brought their human capital in addition to any institutional structure.

this context since country-specific characteristics such as culture, religion or geography are the main alternatives to institutional quality as "deep" determinants of growth. The standard response to this problem in cross country regressions is to include control variables, but aspects such as culture appear difficult to measure and a comprehensive list of all relevant characteristics is at the very least elusive. Panel regressions, on the other hand, are able to significantly reduce this problem by the inclusion of fixed effects: country-specific intercepts that control for all possible time-invariant country characteristics.

A second justification for using panel regressions with fixed effects instead of cross-sections is that their results are more relevant for policy advice. At the moment the results of the literature are invoked to justify a focus on institutional reform as a pre-condition for growth in developing countries. This is a risky recommendation if based exclusively on the analysis of between-country differences since institutions may not be transferable across countries. Results based on within-country variation, on the other hand, would stem from actual institutional changes taking place inside a country.

Turning to the second point, the use of levels instead of growth rates of GDP per capita can also be called into question. The underlying theory that we are trying to test, North's views on how institutions affect economic development, is expressed in terms of growth rates. According to North, good institutions like secure property rights will result in more investment in physical capital, human capital and ideas; creating growth along the lines of standard growth theory from Solow (1956) to Romer (1990). Nobody would expect the Democratic Republic of Congo to suddenly become a rich country if it introduces good institutions this year, but that is what a level effect would imply. On the other hand, it would be natural to expect the DRC to grow faster; and to progressively become a rich country after a few decades if institutions remain adequate. Levels of GDP per capita are informative

because they reflect cumulated growth rates; but growth rates should be the natural first choice to test the theory[3].

A potential objection is that growth rates may reflect short term disturbances such as the state of the business cycle. This, however, is an objection that is easily dealt with by considering average growth rates over long enough periods of time. In this paper we carry out regressions with periods of 5, 10 and 20 years; long enough for business cycles and other short term disturbances to be smoothed out. Moreover, even if some observations are affected by temporary events we would expect positive and negative shocks to counteract each other if the number of observations is large enough.

Some papers in the literature have approached the relationship between institutions and economic development using a methodological approach different from the one described above. Glaeser et al. (2004) experiment with cross sectional regressions using growth rates of GDP per capita as the dependent variable. Lee and Kim (2009) consider panel regressions with fixed effects but use the level of GDP per capita as the dependent variable. Dollar and Kraay (2003) and Bhattacharyya (2009), finally, use panel regressions with fixed effects and growth rates of GDP per capita. This is also the approach that we follow here, consistent with our view that these methodological choices are the most adequate for the present question.

A problem with the results in Dollar and Kraay (2003) and Bhattacharyya (2009) is that they are derived after controlling for the effects of additional determinants of economic growth such as human capital, trade openness or private investment. This is of course in line with common practice in the em-

[3]Some may argue that if we are interested in the change over the very long run then it is equivalent to use current levels or growth rates since the initial level of GDP per capita was about the same around the globe before the Industrial Revolution. Even if we accept that argument, we should still relate the current level of GDP per capita to the long-run average of institutional quality - not to its value at the end of the period as it is commonly done in the literature.

pirical growth literature, but it poses a delicate question of how to interpret the results regarding the effects of institutions on growth. The theoretical case for the importance of institutions clearly indicates that these matter because they determine the costs and benefits of engaging in growth-enhancing activities such as physical and human capital accumulation or international trade. If that is the case, then all these factors are just intermediate channels through which institutions affect growth and we should not control for them if our aim is to estimate the overall effect of institutional quality. In other words, factors such as private investment, human capital and international trade are proximate determinants of economic growth; they are the outcome of deeper determinants such as institutional quality and their inclusion would pick up some of the growth effects that should ultimately be ascribed to institutions.

Our paper contributes to the literature by estimating the effects of institutions on economic development using panel regressions with fixed effects, growth rates of GDP per capita as the dependent variable, and eliminating control variables that are likely to be themselves the outcome of good institutions. In addition to these panel regressions, we also investigate the relationship between institutions and growth using country-by-country regressions, an approach that to the best of our knowledge has not been used in the literature. Finally, we extent the time coverage of previous studies by using data for the period 1940-2005, whereas Dollar and Kraay (2003) cover the period 1960-2000 and Bhattacharyya (2009) focuses on 1980-2004.

The rest of the paper is organized as follows. Section 2 discusses the data and section 3 presents our empirical methodology. Section 4 presents our results and the final section offers some concluding remarks.

2 Data

Three data sources have been widely used in the recent institutional literature: the International Country Risk Guide from Political Risk Services (ICRG), the Governance Matters dataset from the World Bank, and the Polity IV dataset. The first two rely on surveys of investors to construct their measures while the third one uses assessments from political scientists on diverse aspects of each country's institutions. Examples of variables from these three datasets are, respectively, the risk of expropriation measure used in Acemoglu et al. (2001, 2002), the index of the rule of law used in Rodrik et al. (2004) and the measure of constraints on the executive branch of government used in Acemoglu et al. (2001, 2002) and Glaeser et al. (2004). For all these measures there is an inevitable degree of subjectiveness as they are not based on quantifiable magnitudes.

Of these three datasets, only Polity IV is available over a long enough period of time to allow for a meaningful panel study using time periods of as much as 20 years. The variables from Polity IV start with each country's independence year and, for most European and American countries, go all the way back to 1820. This is in sharp contrast with the ICRG dataset, which only covers the 80s and 90s, and the Governance Matters dataset, which goes from 1996 to the present.

Our choice is thus the constraints on the executive variable from Polity IV. This variable is interesting for reasons other than its time coverage. First of all, it appears to be a very good match for North's notions of institutions. The variable "refers to the extent of institutionalized constraints on the decision-making powers of chief executives" (Marshall and Jaggers 2009). These constraints usually come in the form of a powerful parliament and an independent judiciary. Douglas North has emphasized the English Glorious Revolution of 1688 as a prime example of institutional progress, precisely because it set royal power firmly under the control of Parliament (North and

Weingast 1989, North 1990)[4]. Constraints on the executive power are thus central in the Northian view of good institutions.

Another advantage is that the Polity IV measures are not based on surveys - which may be biased by the image of the country on the international scene - but are computed by political experts following clear (albeit not easily quantifiable) guidelines. The variable ranges from 1 to 7 and full descriptions of what would constitute levels 1, 3, 5 and 7 are provided[5]. While critical of most institutional measures, Glaeser et al. (2004) identify the Polity IV variables as the most reliable of the lot and proceed to use them in their work.

Our data on real GDP per capita comes from Maddison (2009), who offers much longer time coverage than alternative sources. Although Maddison's series extend all the way back to 1820 for some countries, we will focus on the period 1940-2005 to limit the panel imbalance. Pre-1940 data will still be used as instruments of future GDP changes. With this data, we calculate average growth rates over non-overlapping periods of 5, 10 and 20 years using log-differences ($g_{i,t} = \frac{1}{T}(\log y_{i,t} - \log y_{i,t-T})$ with $T = 5, 10, 20$). Having a sample that starts in 1940 allows for up to three observations per country when using 20-year periods (1940-60, 1960-80, 1980-2000), six observations when using 10-year periods and thirteen observations when using 5-year periods.

Table 1 shows the distribution of executive constraints over the period 1940-2006, for all countries in our dataset and for five different regions. As

[4]From then onwards English Kings and Queens could not suspend laws, levy new taxes, interfere with the juditial system or maintain a standing army without the consent of Parliament.

[5]A value of 1, for instance, corresponds to the case of "Unlimited Authority" and would be assigned under conditions such as "There is no legislative assembly, or there is one but it is called and dismissed at the executive pleasure". A value of 7 corresponds to the case of "Executive Parity or Subordination" and its overall description is "Accountability groups have effective authority equal to or greater than the executive in most areas of activity". Values 2, 4 and 6 are just described as "intermediate category" (Marshall and Jaggers 2009).

could be expected, Western Europe and its offshoots fares best while Africa and the Middle East share the worst places. Note, however, that there is a lot of variation to identify growth effects: all 7 levels of institutional quality are present in all regions of the world.

3 Methodology

As discussed above, this paper tests the effect of institutions on economic development in panel regressions with fixed effects. Our empirical specification is thus:

$$g_{i,t} = \alpha_i + \lambda_t + \theta y_{i,t-T} + \beta I_{i,t/t-T} + \varepsilon_{i,t} \tag{1}$$

where $g_{i,t}$ is the average growth rate of GDP per capita in country i over the last T years (see previous section), $y_{i,t-T}$ is the log of GDP per capita in country i and year $t-T$ (i.e. the beggining-of-period level of development), $I_{i,t/t-T}$ is the average institutional quality in country i between years $t-T$ and t, and α_i and λ_t are country-specific fixed effects and time dummies.

While the presence of fixed effects controls for all time-invariant country characteristics such as geography, climate, religion and history, the question still remains of what other time-varying control variables should be included in (1). The list of variables used in the voluminous growth literature is long: investment rates, human capital, trade openness, government expenditures, inflation and many, many more have been tested with varying degrees of success[6]. Moreover, the fact that changes in the set of control variables typically imply large changes in the estimated results was noted early on in the literature (Levine and Renelt 1992).

As we discussed earlier, the issue of what control variable to include takes a special turn in the present case since most of them can be argued to be

[6]See Barro and Sala-i-Martin (2003, chapter 12) and, for a full list of variables used in the literature, Durlauf et al. (2005, Appendix 2).

the outcome of a good institutional setting. The solution that we propose is extremely simple: we do not include any additional control variable in equation (1). The easiest way to understand this choice is by noting that we are trying to estimate the overall effect of institutions on growth, which may work through a wealth of alternative channels such as investment in physical and human capital, good government policies and so on. Controlling for these aspects would simply remove some of the effects we are trying to capture.

Readers who are not satisfied with the above argument could complain that the absence of control variables would lead to a bias in the estimate of β. What is important to realize, however, is that such a bias would most likely be a positive one. Standard econometric theory (e.g. Wooldridge 2002, p.61-62) shows that if an omitted variable is correlated with one of our regressors, in this case with institutional quality, the estimated coefficient would have a probability limit given by:

$$plim\ \widehat{\beta} = \beta + \gamma \frac{Cov(I,q)}{Var(I)}$$

where I is our measure of institutions, q is the omitted variable and γ is the structural effect of the omitted variable on growth. Assume, without loss of generality, that $\gamma > 0$ (if the omitted variable is detrimental to growth redefine it with a minus sign in front). What is then the likely sign of the covariance between institutional quality and this growth-enhancing variable that has been omitted? Since I is a measure of good institutions, it is natural to expect this covariance to be positive or at least zero. After all, good institutions are supposed to promote, or at least not hinder, growth-enhancing policies such as low inflation and growth-enhancing behavior such as investment in human and physical capital. If I is negatively correlated with a large number of growth-enhancing factors we may as well doubt of its validity as a measure of good institutions.

If $\gamma > 0$ and $Cov(I, q) > 0$ the estimated β coefficient will be biased upwards. To put it differently, we are not claiming that all proximate growth determinants are caused by institutional quality and therefore that the estimated β converges to its true value in their absence. We are making the weaker claim that some proximate growth determinants are caused by institutional quality and others are not, but these last ones will tend to be positively correlated with institutional quality - biasing the estimated β in a predictable direction.

The consequence of this is that a regression without other controls will maximize the chances of finding a positive effect of institutions on economic development. This would mean that a positive coefficient does not ensure that institutions are growth enhancing, but a negative or zero coefficient does rule out a positive effect of institutions on growth.

A final point remains before continuing to our results. As is well-known, fixed effects regressions suffer by construction from endogeneity bias if they include a lagged dependent variable - as any growth regression does in order to control for convergence effects (Caselli et al. 1996, Durlauf et al. 2005). We deal with this by estimating (1) using the difference GMM estimation technique of Arellano and Bond (1991), where changes in endogenous variables are instrumented using lagged levels of those same variables. Initially we consider only lagged GDP per capita as endogenous, but the procedure is then expanded to take into account the potential reverse causality between institutions and growth.

4 Empirical results

A first look at the evidence is provided in figure 1. These scatter plots show the bivariate relationship between growth and institutional quality using period lengths of 5, 10 and 20 years. The first impression that the figure conveys

is that of no relationship between institutional quality and economic growth with the exception of some particularly dismal growth episodes at the lowest level of institutional quality. A simple pooled OLS regression of growth on institutional quality and initial GDP per capita confirms this, as reported in table 2. The coefficient on institutions is positive and statistically significant when we consider the whole sample but becomes smaller and not statistically significant if we restrict the sample to those observations with an average institutional score of 2 or more. Thus, these scatter plots and regressions would suggest that institutions do not enhance growth once we leave the first rung of the institutional ladder.

The pooled OLS results of table 2 are given for illustrative purposes only since they fail to control for country-specific time-invariant characteristics. This is of special importance in the present context since the variables that are in competition with institutions as "ultimate" determinants of economic development - geography, climate, historical legacy and so on - tend to be time invariant. In what follows we focus on fixed effects regressions as described in the previous section.

Our baseline results are presented in table 3. The top panel of this table estimates equation (1) using all available observations since 1940 and instrumenting only the initial level of GDP per capita. Results are not supportive of a role of institutional quality on economic growth. Whether we use 5, 10 or 20 year periods, we always obtain a small and statistically not significant coefficient on institutional quality whose value is either negative or null[7].

The remaining three panels of table 3 re-estimates our baseline regression in reduced samples to test for the possibility that the above results

[7]The standard procedure of using all available lags as instruments is used for 10 and 20 year periods. For 5 year periods we collapse the instrument set using the procedure described in Roodman (2006) in order to avoid the number of instruments growing too large.

are driven by the peculiarities of some group of countries. We exclude fuel-dependent countries, sub-Saharan African countries and Latin American countries[8]. Fuel-dependent countries are suspect because oil and gas riches may allow a country to grow fast despite having deficient institutions (although this hypothesis runs against the well-know "resource course" literature). Sub-Saharan Africa and Latin America are two regions that have underperformed the rest of the developing world over the last few decades and could be suspected of being in a poverty trap where institutional improvements are not helpful. Our results, however, continue to hold in all samples. In all cases the coefficient on institutional quality remains statistically not significant, taking at best small positive values when we exclude fuel-dependent and Latin American countries.

Table 3 also reports for each regression two tests for the suitability of the instruments: Sargan's test of overidentifying restrictions (where the null hypothesis is that the instrument set is exogenous) and Arellano and Bond's test for second order autocorrelation in the residuals in differences (where the null hypothesis is that no second order autocorrelation exists). For the GMM methodology to be valid these tests should not reject their null hypotheses. The p-values for the tests indicate that the null of no second order autocorrelation is not rejected in all cases while the null of an exogenous instrument set is not rejected in all but one case (all at the 5% level).

The regressions in table 3 do not address the potential reverse causality from growth to institutions which has been a mayor theme in the empirical literature. By so doing, they would actually tend to overestimate the effect of institutions on growth since the presumed reverse effect is also positive. We address the issue of reverse causality in table 4 by considering institutional

[8]Fuel-dependent countries are defined as those where fuel exports constitute more than 70% of total exports in the year 2000 (source: World Development Indicators 2009). We need to exclude countries instead of controlling with dummy variables due to the presence of country fixed effects.

quality as endogenous and instrumenting for it with its respective lags. As expected, this results in smaller estimates of the effects of institutions on growth: our baseline regression now reports small negative effects of institutions on growth which are even statistically significant when we use 10 and 20 year periods (top panel of table 4). These results continue to hold when we exclude fuel-dependent, sub-Saharan African and Latin American countries (not reported).

The middle and lower panels of table 4 modify equation (1) to take account of potential lags in the effect from institutions to growth. The justification for this approach is that the investments fostered by high quality institutions may need a certain time to produce their fruits. Investments in physical capital require a couple of years to build up plants or machinery, while investments in human capital could demand considerably longer.

With this in mind, the middle panel considers the value of institutional quality in the previous period as a determinant of growth in the current period. The lower panel consider institutional quality in the two preceding periods as determinants of growth in the current period. Since period length varies from 5 to 20 years, this procedure allows for a time lag of as much as 40 years before institutional quality has an effect on growth. The empirical specifications are:

$$g_{i,t} = \alpha_i + \lambda_t + \theta y_{i,t-T} + \beta I_{i,t-T/t-2T} + \varepsilon_{i,t} \tag{2}$$

and

$$g_{i,t} = \alpha_i + \lambda_t + \theta y_{i,t-T} + \beta_1 I_{i,t-T/t-2T} + \beta_2 I_{i,t-2T/t-3T} + \varepsilon_{i,t} \tag{3}$$

In both cases we instrument institutional quality with its own lags, which seriously constraints the number of available observations; particularly for 20 year periods.

None of the regressions with lagged values of institutional quality finds a statistically significant effect of institutions on growth. Whether we use equation (2) or (3), it appears that taking into account the dynamic nature of the relationship between institutions and growth does not change our conclusions. We also note that all regressions reported in table 4 satisfy the tests for exogeneity of the instrument set and no second order autocorrelation in the residuals in differences.

As an additional test of the role of institutions on growth, table 5 presents the results of country-by-country regressions. This procedure not only controls for all country-specific factors, like the fixed effects regressions employed above, but also allows for an effect of institutions on growth that is different in every country. The downside is that we require a large enough number of observations per country, which leads us to use yearly growth rates instead of the 5 to 20 year periods considered previously. While this leaves open the possibility of some yearly growth rates being affected by the state of the business cycle, business cycles should affect countries with low and high quality institutions alike - so there is no reason to fear a bias in the results.

Table 5 reports regression results for each country where some variation in institutional quality took place.[9] All regressions include as a control variable the initial level of GDP per capita but this is not reported for conciseness. Out of 118 countries for which a regression could be estimated a clear majority of 88 countries present no statistically significant relationship between institutional quality and economic growth. The remaining 30 countries with a statistically significant coefficient are quite evenly split between those where the relationship is statistically significant and positive (17 countries) and those where it is statistically significant and negative (13

[9] A certain number of countries do not show any variation in their level of institutional quality and cannot be included. The United States and Switzerland, for instance, are always assigned the top score of 7 while Saudi Arabia and Qatar are always assigned the lowest score of 1.

countries). In other words, there does not seem to be a tendency for higher institutional quality to be associated with higher growth when considering country-by-country regressions.

An additional observation is that several of the countries where a positive and statistically significant relationship is found would not be our first choice for a case study of how institutions benefit growth: Libya, Russia, Equatorial Guinea and Liberia are all in this group. It is thus difficult to come with a clear example of a country where institutional quality and growth are positively related. Table 5 tells us that the opposite relationship is just as likely to be found.

Overall, all the evidence presented in this section deny the existence of a positive and statistically significant effect of institutions on economic growth. Even the most supportive results presented, when the relationship was estimated using pooled OLS, suggest that any growth benefits disappear after a country passes from the lowest to the second-lowest level of institutional quality. Once we take into account country specificities by the use of fixed effects or country-by-country regressions, as we naturally should do, not even that result holds.

5 Concluding remarks

This paper tests the hypothesis that institutional quality, as measured by the constraints on executive power, are the ultimate driver of economic development. While the empirical literature on this subject has been voluminous, this paper studies the question from an angle that has not been taken previously. First, we use panel regressions with fixed effects to eliminate other "deep" determinants of economic development that are country-specific. Second, we consider growth rates instead of levels of GDP per capita as this accords better with the underlying theoretical mechanism. Third, we do not

include control variables such as private investment, human capital and international trade as these are likely to be the outcome of a good institutional framework and would therefore pick up part of the overall growth effect that should be assigned to institutions. Finally, we complement our panel regressions with country-by-country regressions which further relax the underlying assumptions by allowing different marginal effects across countries.

We do not find evidence to support the claim that countries characterized by higher institutional quality experience faster growth. Consistent with the outlook of the data in simple scatter plots, our econometric results find small and statistically not significant coefficients using several methodologies.

Could our results be the outcome of inadequate data? This is unlikely for GDP per capita, which is widely accepted as a measure of economic development and whose values over the period of analysis are not subject to major controversies. Measuring institutional quality, on the other hand, is a much more contentious issue. On that account, what we can say is that the measure of constraints on executive power is intended to capture a very important aspect of North's characterization of a good institutional environment, namely freedom from unilateral expropriation by the state. The actual measure may still fall short of its authors' intentions, of course, but if that is the case a large set of empirical results in the literature - most of which are supportive of the positive role of institutions - would need to be reconsidered as well.

The results of this paper present us with a puzzle. If institutional quality has a positive effect on long-term levels of development - as the cross-country evidence has strongly advocated - we would expect to observe countries growing faster when their institutions improve. We don't observe that, not even in the simplest of settings where any variable that could be picking up the effect from institutional quality has been eliminated. This clearly suggest that the results from the cross-country studies are either the outcome

of reverse causality (which could not be solved because of problems with the instruments) or that a common, unmeasured cause exists for institutions and economic development. Further research will most likely have more to say on this absorbing topic.

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