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Do economic institutions matter for growth episodes?

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Abstract

An increasingly large literature on the empirics of economic growth has viewed it as an 'episodic phenomenon'. In this paper, we re-evaluate the relationship between growth and economic institutions using an episodic framework, where the relevant units of empirical analysis are growth episodes. We use episodes identified in Kar et al. (2013) and quantify their success using a novel 'measure' termed as 'episode magnitude', adopted from Pritchett et al. (2016). In order to capture the multi-functionality of economic institutions, we use separate measures for property rights institutions, contractual institutions and state capacity. Using instrumental variable methods, we show that, together with human capital and level of development, higher institutional quality is also a significant factor that determines more successful growth episodes.

Keywords: Economic growth, institutions, episodes, state capacity, property rights

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I. Introduction

The literature on growth empirics has taken very significant strides over the past two decades, moving from proximate determinants like investment and exports to deeper determinants like globalisation, geography, human capital and, finally, institutions. There has been significant debate over which of these deep determinants has the most robust impact on growth and there seems to be some consensus that 'institutions rule'. A number of studies (Hall and Jones 1999, Acemoglu et al. 2001, Rodrik et al. 2004) have shown that good institutions are the best way to achieve higher growth rates over long periods of time.

Unfortunately, this literature focuses solely on long-term growth rates and its correlates, and ignores the fact that medium-term growth rates are highly volatile in most countries – particularly developing ones (see Pritchett 2000, Rodrik 1999, 2003, Hausmann et al. 2006, Aizenman and Spiegel 2010). Thus, the long-run average growth rates for these countries usually hide distinct medium-term episodes of successful growth and growth failures (Jones and Olken 2008). Clearly, analysing the growth of a developing country as a single long-term episode, when in reality it is the aggregation of these dissimilar medium-term episodes, simply fails to utilise a lot of information from the diversity of these medium-term episodes that is relevant for growth policy. Moreover, in order to maximise long-term growth rates, policymakers in these countries need to understand how to emulate the successful medium term growth episodes and how to avoid the episodes of growth failures (Berg et al. 2012). This realisation has led to increased interest in medium-term growth fluctuations and the evolution of the episode-based approach to analysing economic growth.

In this paper, we adopt an episodic framework to re-evaluate the relationship between economic institutions and growth. Much of the existing empirical literature on economic growth has attempted to prove that institutions are the single most important factor underlying long-run growth. In contrast, this paper focuses on the determinants of medium-term growth episodes. We investigate whether economic institutions (along with other factors) also play a role in achieving more successful growth episodes.

The episode-based analysis used in this paper redefines the problem of growth by posing three related questions:

1. For any country, how do we identify distinct growth episodes?
2. Given that different episodes may have different counterfactuals (i.e., what the growth rate *would have been* if the episode had not taken place) and different durations (i.e., how long the episode lasts), how do we define a measure of success of episodes (in terms of growth) that is comparable across episodes?
3. Having defined a measure, what are the underlying factors that determine more successful episodes based on such a measure?

Much of the 'growth episode' literature has focused on the first question – that is, the timing of the structural breaks in economic growth and the correlates of the onset of growth accelerations and decelerations (Rodrik 1999, Hausmann, Pritchett and Rodrik 2005, Hausmann, Rodriguez and Warner 2006, Arbache and Page 2007, Aizenmann and Spiegel 2010, Breuer and McDermott 2013, Berg et al. 2012). Kar et al. (2013) critique the methodological approaches in these papers and also provide a novel methodological alternative.

With regard to the second question, Pritchett et al. (2016) propose a method for measuring the success of an episode by calculating the magnitude of the change in per capita income as a result of the growth acceleration or deceleration in the episode. This measure is termed the *episode magnitude*, and is the only measure of the success of an episode available in the literature that incorporates the effect of both growth rates and the duration of episodes.

In this paper, we focus on the third question, i.e., identifying the factors – in particular economic institutions – underlying the success of episodes, using the concept of episode magnitude proposed by Pritchett et al. (2016).

This paper adds to the literature on growth episodes in two important ways. Firstly, it undertakes a regression-based analysis of the deep determinants of more successful growth episodes. The only other contribution to this literature is Berg et al. (2012), where the impact of an episode is measured only in terms of its 'duration'. In this paper, we examine the determinants of not only the duration of an episode, but also its average growth rate, as well as alternative counterfactual growth outcomes during the period.

The second contribution of this study is that it reevaluates the relationship between growth and economic institutions within the growth episode framework. We recognise the multifunctionality of economic institutions and hence use alternative measures of institutional quality in order to capture this aspect. Thus we focus on economic institutions not only in terms of property rights and contractual institutions – as is standard in the literature (see Rodrik et al. 2004, Acemoglu and Johnson 2005) – but also include state capacity – the institutional capability of the state to carry out various policies that deliver benefits and services to households and firms – an aspect of institutional quality that may be equally critical to the growth process of developing economies (Besley and Persson 2011, Savoia and Sen 2014). There are, of course, large areas of overlap between all three of these institutional aspects. Nevertheless, there are important differences as well. As pointed out in Acemoglu and Johnson (2005), while contractual institutions like the legal framework mostly enhance horizontal relationships between private citizens, property rights institutions are about vertical relationships, protecting the citizen from the power of the predatory state. It is plausible to argue that state capacity is also a vertical relationship since it deals with the state's ability to work for the private citizen. However, it is a 'facilitating' institution – an institution that mostly enhances private enterprise, rather than providing protection from the state – and hence it is distinct from property rights institutions, which are 'enforcing' institutions. Thus, each of these institutions may play an important role in the growth process, and in this paper, we attempt to understand whether they do, in fact, matter.

Our results show that higher institutional quality is a significant factor determining more successful growth episodes. They also indicate reverse feedback running from growth outcomes to institutional quality. Initial conditions like per capita income and human capital are also found to be significant for growth outcomes.

The literature that motivates this study is reviewed in the next section. Section 3 summarises the findings from two studies that provide very significant inputs to the current study. Here, we first discuss a mechanism for identifying growth episodes from Kar et al (2013), which is the source of the growth episodes in this study. Second, from Pritchett et al. (2016), we describe the concept of 'episode magnitude' that quantifies the success of a growth episode, as we use this concept extensively in this paper. Section 4 discusses data and empirical strategy. Section 5 presents the results. Section 6 concludes the paper.

2. Related literature

There are two different strands of the empirical growth literature that are relevant for this paper. The first deals with medium-term growth volatility, focusing mostly on attempts to identify growth episodes and partly on measures of the impact of an episode. This literature relates to the endogenous (left-hand side) variable in our regression analysis. The second strand deals with the deep determinants of growth and this literature relates to the explanatory (right-hand side) variables in our regressions.

The literature dealing with growth volatility focuses mainly on the first of the three questions posed earlier, i.e., how to identify a growth episode. The seminal paper in this area was Pritchett (2000), which showed that a single average growth rate fitted over a long time period gives very poor statistical fits in a large number of countries, particularly developing nations. A set of recent studies have followed this idea and attempted to identify breaks in growth rates of GDP per capita for countries with comparable income data. Two distinct approaches have been developed by this literature. The first is a 'filter-based' approach that identifies growth breaks on the basis of subjectively defined rules. Using this approach, Hausmann et al. (2005) study breaks that involve growth accelerations, Hausmann et al. (2006) study growth collapses, and Aizenman and Spiegel (2010) study takeoffs – periods of sustained high growth following periods of stagnation. The second approach is based on statistical structural break tests that use estimation and testing procedures to identify growth breaks in terms of statistically significant changes in (average) growth rates. The studies that have adopted the 'statistical' approach have used the Bai-Perron (BP) methodology (Bai and Perron 1998), which locates and tests for multiple growth breaks within a time-series framework.

Both approaches, however, have been shown to suffer from serious shortcomings. In the first approach, the use of filters pre-determined by the researcher is ad hoc, and leads to a lack of consistency in the identification of breaks across papers that use the filter-based approach. On the other hand, a significant shortcoming with the statistical approach is that it is limited by the low power of the Bai-Perron test, which leads to the rejection of true breaks that are suggested by the behaviour of the underlying GDP per capita series. Kar et al. (2013) suggest an alternative methodology that deals with both of these shortcomings. We will discuss this methodology in the next section.

The second of the three questions posed earlier – i.e., defining a measure that can compare between growth episodes – has largely been ignored in the literature. This is mainly because most of the contributions have used a 'before-and-after-the-break' framework that did not necessitate any such measure. Berg et al. (2012) is one of the few papers that attempts to deal with this issue by measuring the impact of an episode in terms of its 'duration'. However, this is only a partial answer to the problem, as this does not account for two things, namely: (i) the growth rate during the episode; and (ii) a consideration of an appropriate counterfactual growth rate during the episode. Pritchett et al. (2016) deal with all these issues and provide a measure of the success of a growth episode that is termed as 'episode magnitude'. The next section gives a detailed discussion of this concept and we use alternative measures of this variable in our regression exercises in the empirical analysis.

The literature that deals with deep determinants of growth has evolved as a critique of the large number of proximate determinants (investment ratio, exports ratio, fiscal policy, etc.) that were found to be 'causing' growth in the earlier cross-country literature. Four competing alternatives have been proposed as deep determinants of long-run growth. These are Geography, Globalisation, Institutional Quality and Initial Human Capital Endowment of Nations. Geography is an important determinant of climate, endowment of natural resources, disease burden, transport costs, and cost of diffusion of knowledge and technology from developed nations. All of these factors have important effects on productivity and growth (Diamond 1997, Gallup, Sachs, and Mellinger 1998, Sachs 2001). Globalisation is also argued to be an intrinsic factor underlying long-term growth, as it enables the transfer of capital as well as technology from developed to developing areas. Another important channel through which globalisation helps developing countries is by providing an external market for its products. Important contributions to the globalization-led growth literature include Frankel and Romer (1999) and Sachs and Warner (1995). The argument that institutions affect growth is an old one (North 1990). This is based on the premise that institutions, which are the rules of the game in a society, encourage desirable economic behaviour. This would lead to increases in both accumulation and productivity and hence to higher long-run growth. Hall and Jones (1999), Acemoglu et al. (2001, 2002, 2004) and Rodrik et al (2004) are important contributors to this literature. Glaeser et al. (2004), on the other hand, have argued that it is the initial endowments of Human Capital that lead to growth as well as better institutions.

An important limitation of the previous literature that has examined the institutional determinants of growth is that, while there is a strong connection between *levels* of prosperity and *levels* of the quality of 'institutions', the connection between the initial level of the quality of institutions and subsequent growth or between *economic* growth and *changes* in institutions is often weak (Pritchett and Werker 2012). Thus, while the literature has succeeded to a large extent to causally show the importance of institutional quality in determining long-run incomes, it remains an empirical question whether institutions are causally related to episodic growth, and whether institutions matter in determining successful growth episodes.

An emerging literature has also examined the determinants of the onsets of growth accelerations and decelerations. With respect to growth accelerations, Hausmann, Pritchett and Rodrik (2005) find that standard growth determinants, such as major changes in economic policies, institutional arrangements, political circumstances or external conditions, do a poor job of predicting the turning points. Pritchett (2000) suggests that slow-moving determinants of growth such as improvements in the quality of institutions, or time-constant factors such as geography (land-lockedness, distance from the equator), resource endowments (e.g. minerals), ethnic diversity, culture and colonial experience, are less likely to explain the frequent shifts from one growth regime to another that we observe in many developing countries and the wide variations in within-country economic growth. Jones and Olken (2008) show that growth accelerations are accompanied by increases in productivity and not investment, and with increases in trade, suggesting that reallocation of resources from less productive to more productive uses is an important part of growth accelerations. Growth declines, on the other hand, are associated with monetary instability and increases in inflation, along with higher frequency of military conflict, and trade does not play as important a role in growth declines as it does in growth accelerations. Jones and Olken also find that changes in institutions are not associated with either growth accelerations or declines, where institutional quality is measured by a lower level of corruption and the rule of law. Using a Markov switching model and calculating the transition probabilities of moving from one growth regime to another using historical GDP data, Jerzmanowski (2006) finds that better institutional quality improves the possibility that a country will remain in a growth acceleration episode and will be less likely to suffer a growth collapse.

However, none of these studies examines what determines the success (or lack of success) of a growth episode, *conditional* on the onset of a growth episode. For example, for two countries which have seen the initiation of a growth acceleration episode in a particular year, what determines which country will witness a higher magnitude of growth? For two countries which have witnessed a deceleration in growth in the same year, what determines which country is less likely to see a larger decline in incomes? In particular, does the quality of institutions play any significant role in determining the success of episodes? This paper attempts to address these questions.

3. Identifying growth episodes and estimating episode magnitudes

An episode-based analysis of growth is different from the Barro-type growth regressions or other standard regressions of long-run growth, in two different ways. The first difference is that in standard regressions, the period over which growth is measured is decided in an ad hoc manner (say, a decade), while episode-based approaches have to precisely define how to identify the length of an episode. The second difference is that while average growth rates are a suitable measure of the impact of growth in the standard regressions, they are not so in episode-based approaches, as the duration of episodes (which vary widely) is as important as the growth rate in this approach. Thus for our study, we need: (i) a set of identified growth episodes based on an acceptable methodology; and (ii) a measure of a successful growth episode that incorporates both growth rates and duration.

As discussed in earlier sections, there have been a couple of studies that have identified growth episodes for a large number of countries (Berg et al. 2012, Kerekes 2012, Kar et al. 2013). While there are a large number of growth episodes that are common to all of these studies, each of them is methodologically distinct, and hence for consistency, we have to choose our episodes from any one of them. The methodology from Kar et al. (2013) has been accepted by more than one peer-reviewed journal (Kar et. al 2013, Pritchett et al. 2016) and is able to identify a larger set of episodes compared to the other contributions. Consequently, we use the episodes from Kar et al. (2013) for the current study, both for methodological acceptability as well as larger sample size. For a measure of the success of a growth episode, we use the concept of 'episode magnitide' from Pritchett et al. (2016), as this is the only measure discussed in the literature that incorporates the effect of both growth rates and duration of episodes. In this section, we describe the methodological approaches from these two studies that we use extensively in this paper.

Identifying growth episodes

In any study of economic growth that is episode-based, we first need to identify episodes of growth accelerations and decelerations. The Kar et al (2013) technique for the identification of episodes is similar to the Bai Perron (BP hereafter) technique, in that, like the latter, it is also a two-step methodology. In the BP approach, the first step estimates up to a given number of breaks and the second step sequentially tests for the optimal number of statistically significant breaks. It may be noted that the poor power of the BP test can be attributed to the second step, where the statistical testing procedure rejects a large number of ‘true’ breaks. In order to provide an identification mechanism that is more broad-based and captures a larger number of ‘true’ breaks, Kar et al. (2013) propose an alternative two-step method, where the first step again uses the BP estimation technique to identify potential breaks and the second step uses an ‘economic filter’ instead of a ‘statistical procedure’ used in BP. The economic filter is as follows:

(i) In case of the first candidate break, since it is not known whether it follows an acceleration or deceleration, any change of more than 2 percent (up or down) is counted as a growth break.

After that, the threshold depends on the previous history:

(ii) If a candidate acceleration follows a previous deceleration or a candidate deceleration follows a previous acceleration, then to qualify as a genuine growth break, the absolute magnitude of the growth difference has to be 3 percent.
(iii) If, however, a candidate acceleration follows a previous acceleration or a candidate deceleration follows a previous deceleration, then a change of only 1 percent (in absolute value) qualifies as a genuine break.

To estimate potential breaks, the study assumed that a ígrowth regime’ lasts a minimum of eight years (as in Berg et al. 2012). The use of shorter periods (e.g. three or five years) risks conflation with ‘business cycle fluctuations’ or truly ‘short run’ shocks (e.g. droughts). Longer periods (e.g. 10 or 12 years) reduce the number of potential breaks.[1] Application of this procedure to the PWT7.1 data for 125 countries[2] for 1950-2010 identified 314 structural breaks in growth, with some countries having no breaks (e.g. USA, France, Australia) and others having four breaks (e.g. Argentina, Zambia). Appendix A in Kar et al. (2013) provides a list of all 314 breaks identified by country and year of break.

Estimating the episode magnitude of growth accelerations and decelerations

The calculation of episode magnitudes for growth episodes is discussed in detail in Pritchett et al. (2016). In this section we summarise this approach. We define the episode magnitude as the magnitude of the gain (or loss) in per capita income by the end of the episode, as a result of the growth in the episode. Equivalently, it is the product of (i) the additional growth during the episode; and (ii) the duration of the episode. The additional growth during the episode is the difference between the actual growth rate during the episode, and a predicted counter-factual growth rate of the economy, had it not transitioned to this particular episode.

How do we predict this counter-factual growth rate? One simple (although naive) prediction is that the growth rate would be what it was in the last episode (no change). This prediction however, ignores a very robust 'stylised fact' about medium-term growth rates, i.e., the tendency of these growth rates to 'regress to the mean'. Like other volatile variables such as returns on financial investments, medium-term growth rates have been shown to have very low persistence, and hence, for example, high growth in the current period increases the possibility of lower growth in the future (Easterly et al 1993, Pritchett and Summers 2014). In terms of growth episodes, this implies that a predicted counter-factual growth rate can do much better than a ‘no change’ assumption, by adopting some version of regression to mean.

There is another important reason why regression to mean needs to be incorporated in a definition of episode magnitudes. It should be noted that if there is a tendency of growth rates to regress to the mean, then it is a statistical phenomenon which is exhibited by many other variables. It is not causal, in the sense that the reversal of growth rates in any episode for any particular country due to this tendency is not attributable to changes in the determinants of growth during that episode. Since our interest in defining an episode magnitude is to subsequently relate it to the underlying determinants of growth, our definition of this variable needs to remove the part that is due to this statistical phenomenon, leaving only that part of the variation in the growth outcome that can be explained by underlying

[1]The length of the output data series that is available in the Penn World Tables varies from country to country. This implies that we need to specify a maximum number of candidate breaks for each country depending on the length of the data series available. We postulate that a country with: i) 40 years of data (only since 1970), can have a maximum of two breaks; ii) more than 40 years and up to 55 years (data since 1955), can have a maximum of three breaks; iii) more than 55 years (before 1955), can have a maximum of four breaks.

[2] From the PWT7.1 data, we eliminated all countries that had very small populations (less than 700,000 in 1980) and those that did not have data since 1970 (which eliminated many former Soviet sphere countries and some oil countries like Kuwait and Saudi Arabia).

factors. This implies that the measure of the success of a growth episode has to be 'over and above' its tendency to regress to the mean.

Based on these considerations, we propose three predicted 'counter-factual' growth rates, i.e., (a) the growth rate in the previous episode reflecting the idea of 'no regression to mean', (b) the world average growth rate during the episode reflecting the idea of 'complete regression to mean' and (c) a predicted growth rate based on the idea of 'partial regression to mean'. The 'partial regression to mean' growth rate uses a regression for each country/episode to allow 'predicted' growth to depend on a country's initial GDP per capita, the episode period specific world average growth and a flexibly specified regression to the mean.

Suppose we have a structural break in growth in year *t* that ends a previous growth episode. Also suppose the growth in the previous episode was g_{before} that lasted for N_b years and the growth in the current episode is $g_{ep}$and this episode lasts N_{ep} years. We define the episode magnitude of the current growth episode (where F denotes the episode) as the difference between the log of actual GDP per capita (GDPPC) in year t+ N_{ep}, and the log of its counter-factual level. If natural log of GDPPC is *y* then the equation is:

$$1)\ Episode\ Magnitude_F = y_{t+N_{ep}}^{Actual} - y_{t+N_{ep}}^{Counter\ factual}$$

By definition, the right-hand side of equation 1 is nothing but the product of the actual growth rate during the episode (relative to the counterfactual) and the duration of the episode. This definition of episode magnitude thus fulfils our criteria for a measure of the impact of a growth episode. Let us now formalise each of the three counter-factuals discussed above.

'No Regression to Mean': Counter-factual growth continues at pre-break levels. This assumes there is zero regression to the mean and the counter-factual for growth during the episode was the pre-break growth rate.[3] In this case, the magnitude of the total gain/loss from the episode is:

$$2)\ Episode\ Magnitude_F^{No\ Change} = (\ g_{ep} - g_{before}) * N_{ep}$$

'Complete regression to mean': Counter-factual growth during the episode is world average growth during the episode. Complete regression to the mean assumes the growth rate during the episode would have been the world average growth during the same period.[4]

$$3)\ Episode\ Magnitude_F^{World\ Average} = (\ g_{ep} - g_{World\ Average_{t,t+N_{ep}}}) * N_{ep}$$

[3] The 'no change' growth rate is the coefficient from an OLS regression of ln(GDPPC) on a time trend over the pre-break period.
[4] The world average growth rate is the average of the growth rates of all countries minus the country in question for the period of the growth episode.

'Partial regression to mean': Counter-factual growth during the episode is predicted from past growth. This counter-factual growth, denoted by g_{PRM} , is the prediction from a country/episode specific regression of growth for all countries j other than the country with the break on a constant plus initial GDP per capita plus previous growth. We use a spline to allow the coefficient on previous growth to be different whether the country's growth rate before the episode was higher or lower than the world average.

$$4)\ g_{PRM}^{j} = \alpha_j{}^{ep} + \beta_{below}^{ep} * c^j * \ (g_{before}^{j} - g_{before}^{world\ average}) + \beta_{above}^{ep} * d^j * \left(g_{before}^{j} - g_{before}^{world\ average}\right) + \gamma * y_t^j + \varepsilon^j$$

This functional form for the counter-factual growth allows for four things: (1) the constant α^{ep} allows the world average growth rate to vary over time and to be specific to the period of the episode to accommodate a global 'business cycle'; (2) regression to the mean is period specific; (3) regression to the mean depends on previous growth (as recoveries from negative/slow growth make have different dynamics that the slowing of accelerations), with the persistence coefficients, β_{below}^{ep} and β_{above}^{ep} capturing regression to the mean, if previous growth was below and above the previous world average growth rate respectively (with c^j =1 and d^j = 1 if the previous growth rate of the country in question was lower and higher than the previous world average growth rate respectively, 0 otherwise,); (4) growth to depend on the initial level of income, given by the coefficient γ (without conditioning variables this is *not* estimating "conditional convergence")[5]. The error term of the regression is given by ε^j.

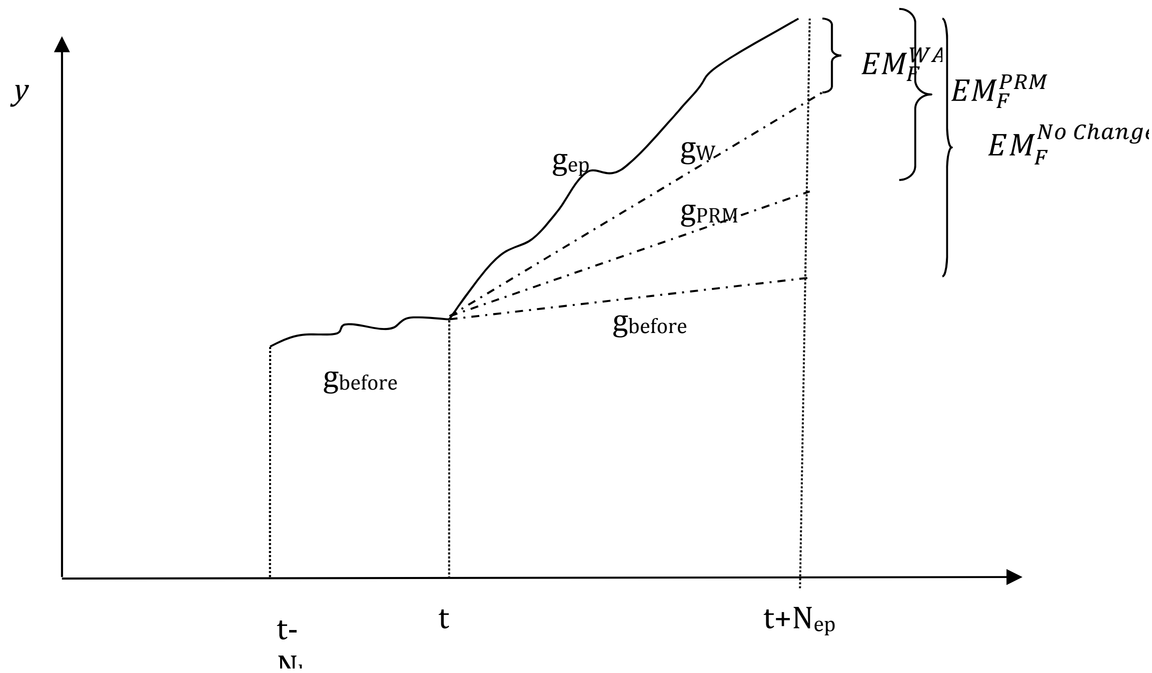
The episode magnitude of a growth episode, using the 'Partial regression to mean' as the counter-factual growth rate, is given by:

$$5)\ Episode\ Magnitude_F^{PRM} = (\ g_{ep} - g_{PRM}) * N_{ep}$$

Figure 1 illustrates the difference in the episode magnitude for the three counter-factuals for the case of an acceleration from low growth to high growth. In this (hypothetical) case the 'no regression to mean' counter-factual implies a very large magnitude, the 'complete regression to mean' counter-factual a small magnitude (as the post-acceleration growth is not much higher than the world average). The 'partial regression to mean' counter-factual will essentially be a regression determined weighted average of the two and hence will tend to be the two extremes. When using the 'Complete regression to mean' or 'Partial regression to mean' counter-factual, a growth acceleration could have a negative magnitude (or a growth deceleration a positive magnitude).

[5]For the period from the beginning of the data to the first growth break the g_{PRM} is just a regression of growth on the natural log level of initial output.

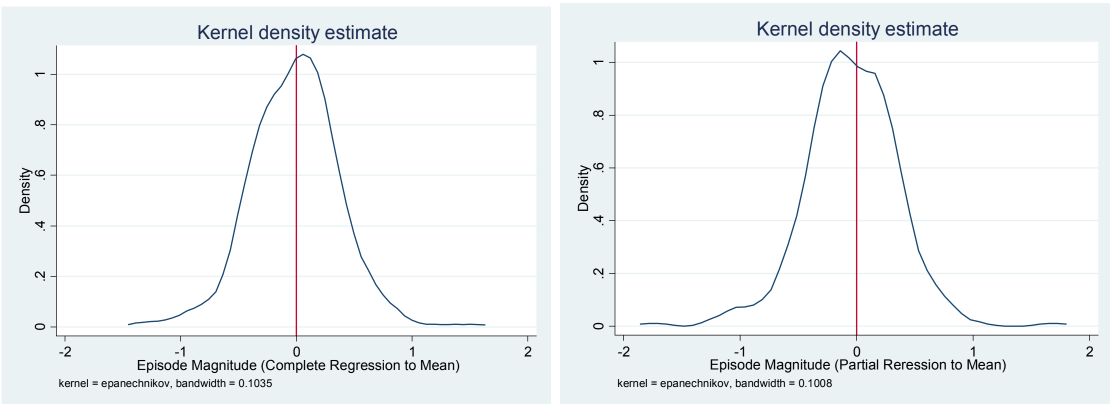
Figure 1: Episode magnitude of a growth episode based on three counter-factuals



Note: g_{PRM} is the 'partial regression to the mean' growth rate, and g_{WA} is the 'complete regression to the mean' or world average growth rate; g_{ep} is the actual growth rate during the episode, and g_{before} is the pre-break growth rate; $EM_F^{No\ Change}$, $EM_F^{WA}, and\ EM_F^{PRM}$ are episode magnitudes when the counter-factual growth rates are g_{before}, g_{WA} and g_{PRM} respectively; $N_{ep}$is the duration of the current episode and N_b is the duration of the previous episode; y is per capita income (in logs), and the break in growth occurs at time t.
Source: Our illustration.

Pritchett et al. (2016) provide the estimated episode magnitudes for 314 episodes based on the three counter-factual growth rates (see Table A1 in online appendix of the paper). For our empirical exercises, however, we will be using the two episode magnitudes based on the idea of regression to mean. Figure 2 gives a kernel density estimate of these two measures, representing the underlying statistical distribution for these variables. The figure on the left-hand side of the panel represents episode magnitudes where the counter-factual is the world average growth rate (Complete Regression to Mean). The figure on the right-hand side of the panel shows episode magnitudes for which the predicted counter-factual reflects Partial Regression to Mean. The two figures are significantly similar to each other, having a central tendency that is close to zero, and most of the density symmetrically distributed between -1 and 1. Here, episode magnitudes with positive values indicate a gain in per capita income due to the episode and those with negative values indicate a loss in per capita income due to the episode. As Table 1 shows, the arithmetic mean is very slightly negative for both measures of episode magnitude, indicating that the episodes with negative episode magnitude slightly outweigh episodes with positive magnitudes. Importantly, it may be noted that positive and negative episode magnitudes do not necessarily mean growth acceleration and decelerations, respectively, as the counterfactual growth rate plays an important role in the calculation episode magnitudes.

Figure 2: Distribution of episode magnitudes



Next, we look at the best and worst growth episodes according to these measures. The highest episode magnitude (Complete Regression to Mean) is 1.53 for Taiwan during the period 1962 to 1993, which is due to an additional growth (i.e., actual minus world average) of 0.047 (i.e., 4.7 percent) and a duration of 32 years. In comparison, the Chinese growth episode between 1977 and 1990 has higher additional growth of 0.064 (i.e., 6.4 percent), but a lower episode magnitude of 0.9, since its duration is less (19 years). The lowest episode magnitude (Complete Regression to Mean) is -1.34 for Democratic Republic of Congo during the period 1989 to 1999, which is due to an additional growth of -0.122 (i.e., -12.2 percent) and a duration of 11 years. The second variable, i.e., episode magnitude (Partial Regression to Mean) has the same Taiwanese episode as its best growth outcome, although the worst growth outcome in this case is an episode for Iran between 1976 and 1987, for which the episode magnitude is -1.74, which is due to an additional growth of -0.146 (i.e., -14.6 percent) and a duration of 12 years.

4. Data and empirical strategy

Data

The objective of this paper is to study the relationship between institutional quality and economic growth in a ‘growth-episode’ framework. To do this, we estimate and test the strength of the relationship between better institutions and more successful growth episodes. For this, we need data on these episodes corresponding to: (i) a measure of the success of these episodes; (ii) measures of institutional quality; (iii) control variables; and (iv) instrumental variables. We briefly discuss our approach corresponding to each of these points below.

As a measure of the success of a growth episode, we use the variable *episode magnitude* from Pritchett et al. (2016), as it is the only existing measure in the literature that captures both the growth rate (relative to counterfactuals) and the duration of the episode. We use the episode magnitude (partial regression to mean) as our preferred endogenous variable and use the episode magnitude (complete regression to mean) for robustness checks. The values of the two variables are taken from Table A1 in the Appendix of Pritchett et al. 2016.

The data for measures of institutional quality, which is the explanatory variable of interest, are taken from the International Country Risk Guide database (ICRG 2013), which is published by the Political Risk Services Group. In order to capture the multifunctionality of economic institutions, we use three alternative measures of institutional quality. In particular, the ICRG variable 'bureaucratic quality' is used as a measure of state capacity, while 'law and order' is used as a measure of contractual institutions, and 'contract viability' is a measure of property rights institutions. Bureaucratic quality is a good measure of state capacity, as it reflects both the inherent merit of the bureaucracy as well as its independence from the political system. For contractual institutions, the literature on institutions and growth has used measures of the legal framework (see Acemoglu and Johnson 2005). We adopt the same approach by using the ICRG variable, law and order. The ICRG data on law and order measures the rule of law by the strength and impartiality of the judicial system, while order is measured by the extent of popular observance of the law. Finally, we use contract viability as a measure of property rights, as it is a combination of two variables, i.e., risk of repudiation of contracts and expropriation risk.[6] It may be noted that while the ICRG provides data for bureaucratic quality and law and order from 1984 onwards, data on contract viability are available only since 2001. However, data on both repudiation of contract and expropriation risk are available for the earlier period and we have combined them using various weighing schemes in order to get alternative measures of contract viability for the whole period. Since all these alternative combinations give similar regression results, we choose to present the simplest one in this paper, which gives equal weights to both the variables.

There are three types of control variables that have been used in all empirical studies of growth. These are: (i) proximate determinants, like investment ratio or trade ratio; (ii) deep determinants that vary over time, like institutional quality or level of development (proxied by per capita income); and (iii) deep determinants that are *unchanged* over time, like geography or history (proxied by colonial origin, etc.). Econometrically, it is incorrect to include both proximate and deep determinants as control variables, since the former are themselves determined by the latter, making them 'bad controls' (i.e., not truly independent explanatory variables). Consequently for our study, we choose to identify only the deep determinants of successful episodes. However, the deep determinants used in the literature that are time-invariant cannot truly be 'determinants' of any measure of success of growth episodes. This is due to the fact that, for any particular country over time, there will be different episodes, with some of them more successful than others, but the deep determinants that are time-invariant will remain constant (for that country), thus being unable to explain this variation.

[6] See Hansson, 2006.

Thus, for our study we can include only deep determinants that vary over time as our control variables.

Our chosen set of control variables are those that are used in the most influential contributions to the empirical literature on growth and institutions (Rodrik et al. 2004, Glaeser et al. 2004) and also fulfil the considerations described above. They are: (i) globalisation (proxied by trade ratio); (ii) initial levels of human capital; and (iii) initial levels of per capita income. We do not include measures such as geography, colonial history or ethnic diversity, as they are time-invariant. The trade data are taken from the World Bank's World Development Indicators, 2013. The measure for human capital is Barro and Lee's average schooling years in the total population aged 15 and over, which is taken from the Quality of Government standard Dataset (Teorell et al. 2011). We have augmented this data for more countries and years using Caselli et al. 2010. The per capita income data are from the Penn World Tables 7.1 (Heston et al. 2012).

For our study, we choose instrumental variables that are widely accepted as reasonably good instruments of institutional quality in the empirical literature. These include the legal origin of a country and the geography of a country (absolute value of latitude). These are taken from the Quality of Government standard Dataset (Teorell et al. 2011). We also include a variable called 'break year from 1950' that is calculated by the authors, which measures how recently a specific episode has started. This variable is used as an instrument for institutional quality that varies with time across the episodes. A more detailed discussion of the logic of using this variable is given later on in this section.

It may be noted that in this study, we are constrained to use only those growth episodes from Kar et al. (2013) for which data is also available for all the variables described above. In particular, data on the institutional measures are only available from 1984 onwards. In order to keep our sample size as large as possible, we have also included those episodes for which we have at least some years of data. Since institutional variables are relatively stable over time, we have used the limited data to calculate the average value of the institutional variables for those episodes (in a subsequent section, we have tested for the robustness of the results by dropping these episodes that have partial data). Following this approach, the total number of episodes included in the regressions varies from 194 to 210. The summary statistics for these variables corresponding to these episodes are presented in Table 1.

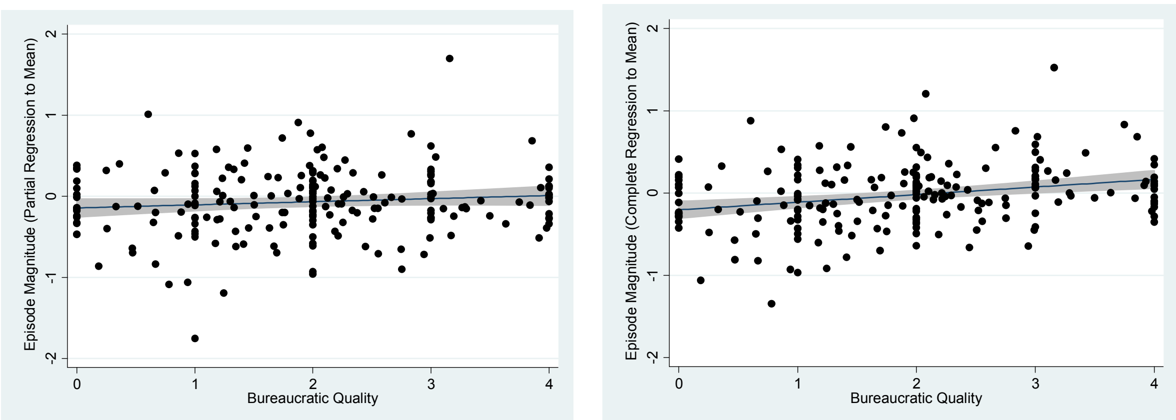
Before we undertake a rigorous econometric exercise, it is useful to study the bi-variate relationship between the endogenous variable (episode magnitude) and the variables measuring institutional quality. Figure 3 presents scatter-plots for these variables, with the panels on the left using episode magnitude (partial regression to mean) on the Y-axis and the panels on the right using episode magnitude (complete regression to mean) on the Y-axis.

Table 1: Summary statistics

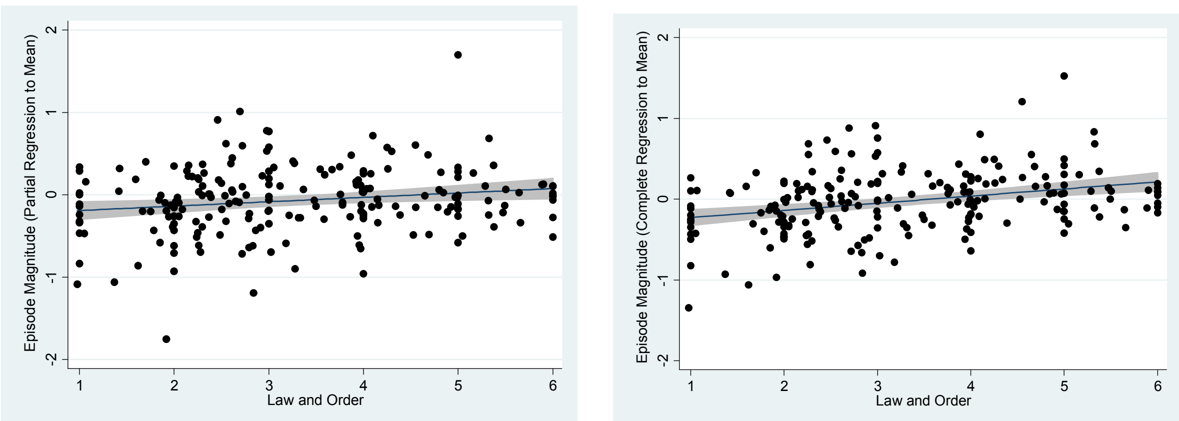
Variable name	Obs	Mean	Std. Dev.	Min	Max
Episode magnitude (complete regression to mean)	210	-0.026	0.390	-1.347	1.526
Episode magnitude (partial regression to mean)	210	-0.070	0.411	-1.755	1.699
Bureaucratic quality	210	1.933	1.106	0	4
Law and order	210	3.282	1.366	0.977	6
Contract viability	209	6.708	1.952	1.538	10
(log) Initial per capita income	198	8.149	1.246	5.116	10.515
Globalisation (trade ratio)	198	69.262	48.001	3.821	373.179
Initial human capital	198	4.733	2.588	0.261	10.837
Legal origin	195	1.908	0.880	1	5
Geography (absolute value of latitude)	195	0.250	0.174	0	0.7111
Break year from 1950	195	36.441	9.339	17	52

Figure 3: Relationship between episode magnitude and institutions

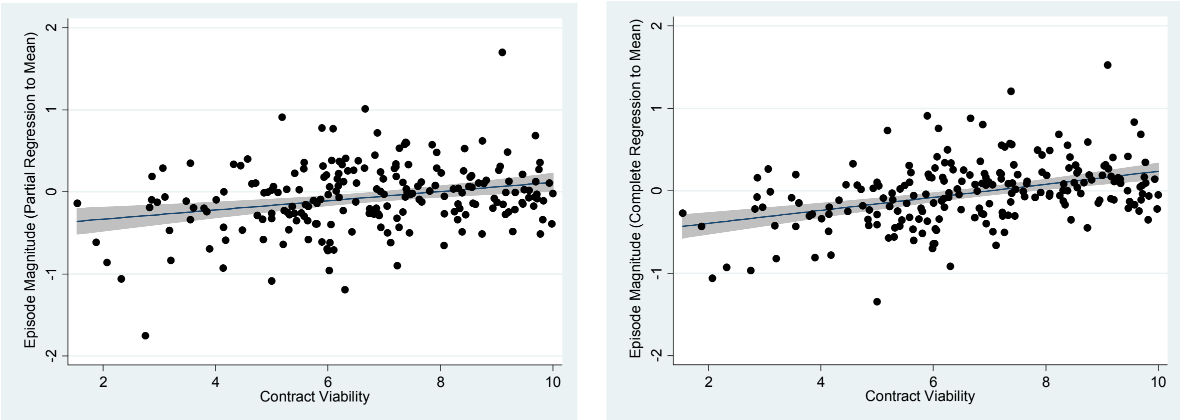
(a) Episode magnitude and bureaucratic quality



(b) Episode magnitude and law and order



(c) Episode magnitude and contract viability



The graphs indicate that there is a positive relationship between the measures of institutional quality and episode magnitudes. This is more pronounced for episode magnitude (complete regression to mean), but reasonably true for episode magnitude (partial regression to mean) as well. This seems to indicate a relationship between the variables, but clearly needs more rigorous empirical analysis that estimates and tests them robustly.

Empirical strategy

Our empirical exercise attempts to estimate the relationship between episode magnitude and institutional quality. We start with the episode magnitude (partial regression to mean) and regress it on one aspect of institutional quality at a time (i.e., either on bureaucratic quality or on law & order or on contract viability). For each of these three cases, there are three sets of regression equations. The first equation is an unconditional OLS regression between episode magnitude and institutional quality. This captures the relationship represented by the scatter-plots presented earlier. The second equation is a conditional OLS regression that controls for other deep determinants of growth identified in the literature. These include initial per capita income, initial human capital and trade ratio.

The empirical literature has underlined the possibility of a reverse feedback effect of growth on institutional quality rendering the OLS estimates between the two variables biased. In order to isolate the 'true' impact of institutional quality on episode magnitude, we estimate a third equation using two-stage least squares (TSLS) regression and appropriate instrumental variables. A comparison of the OLS and TSLS estimates can then be used to empirically validate the direction of causality between the two variables. The instruments used for the TSLS regressions are mostly widely accepted as reasonably good instruments of institutional quality. These include legal origin of countries and geography (absolute value of latitude). One shortcoming with these instruments is that they are country specific and hence do not vary over time. Nevertheless, as the empirical exercises show, they are reasonably good instruments. Furthermore, we have also included one instrument that varies over time. This variable is called 'break year from 1950' and gives the number of years between the year 1950 (the earliest year for which we have data) and the first year of the episode. Thus it measures how recently the episode has started (the higher the value of the variable, the more recent the beginning of the episode). The logic for using this variable as an instrument is that – arguably – the more recent the beginning of an episode, the higher the chances of that episode having better institutional quality. Our argument is that over time – and much more strongly in the recent decades – there has been an increasing focus on the important role of institutional quality in the overall development of countries – both by multilateral organisations like the World Bank and IMF, and by mainstream academia. This has resulted in an attempt by most countries to enhance their institutional quality in recent years. This implies that – on an average – a more recent episode has a higher chance of having a better institutional quality. At the same time, there is no reason to expect the episode magnitude of an episode which depends on both the growth rate and the duration of the episode to be higher for recent episodes, independently of the institutional quality. Empirically, we find that, together with the other instruments, the variable 'break year from 1950' satisfies the conditions that are necessary for a strong instrument.

The TSLS estimates based on the approach described above, represent our benchmark results. We next run another three sets of TSLS regressions to test for the robustness of the results obtained from the benchmark estimates. As mentioned earlier, in the benchmark estimate, we have included episodes for which we have data only for some years. The first robustness exercise tests whether this introduces a strong bias that leads to misleading results. We do this by repeating the TSLS regressions, but including only those episodes that start after 1980 – a period for which almost all data are available. Another source of bias in the benchmark estimates may arise due to the fact that a number of episodes are brought to an end in 2010, due to lack of data availability after that year. The second set of robustness exercises test whether this bias is significantly large, by including a dummy that has a value equal to one for all such truncated episodes and zero otherwise. Thus it controls for the possibility that the truncated episodes unduly influence the results in the benchmark exercise. Finally, the third set of robustness exercises repeat the TSLS estimation for an alternative measure of the endogenous variable, i.e., we use episode magnitude (complete regression to mean).

5. Results

Table 2 presents results of regressions relating episode magnitude to different aspects of institutional quality. The measure of episode magnitude used is that based on partial-regression-to-mean. For institutional quality, columns 2 to 4 use 'bureaucratic quality', columns 5 to 7 use 'law and order' and columns 8 to 10 use 'contract viability', respectively. For each of these three measures of institutional quality, the table presents three alternative regression estimates based on different functional specifications and alternative estimation techniques. The first column (say column 2) presents unconditional OLS estimates representing the bi-variate relationship between the episode magnitude and the institutional variable. The second column (say column 3) presents conditional OLS estimates which additionally control for initial per capita income, initial human capital and an average measure of globalisation that is proxied by the trade ratio. The final column (say column 4) presents TSLS estimates that also include the control variables in column 3. The results of the first stage regressions for the TSLS estimation are given in Table A1 in the appendix.

Table 2

Determinants of episode magnitude: Dependent variable is episode magnitude (partial regression to mean)

	OLS	OLS	TSLS	OLS	OLS	TSLS	OLS	OLS	TSLS
Bureaucratic quality	0.038*	0. 071**	0.238**						
	(1.61)	(2.47)	(2.09)						
Law and order				0.053***	0.062***	0.119**			
				(2.75)	(3.11)	(1.98)			
Contract viability							0.056***	0.074***	0.115**
							(3.73)	(4.26)	(2.28)
(log) Initial PCY		-0.178***	-0.249***		-0.168***	-0.186***		-0.182***	-0.201***
		(-4.06)	(-3.73)		(-4.27)	(-4.43)		(-4.99)	(-5.38)
Trade ratio		0.0001	-0.001		0.0001	-0.0001		-0.0001	-0.001
		(0.44)	(-0.91)		(0.34)	(-0.34)		(-0.40)	(-1.15)
Initial human capital		0 .062***	0.048**		0.058***	0.050***		0.050***	0.041**
		(3.71)	(2.52)		(3.60)	(2.72)		(3.39)	(2.29)
Cons	-0.143**	0 .934***	1.295***	-0.245***	0.803	0.823***	-0.445***	0.676	0.631
	(-2.51)	(3.38)	(3.39)	(-3.35)	(3.24)	(3.42)	(-4.08)	(3.03)	(2.67)
Number of obs	210	198	195	210	198	195	209	197	194
R-squared	0.0105	0.141		0.031	0.151		0.070	0.206	
Hansen J statistic			2.270			2.809			2.595
P-value			0.321			0.245			0.273
Underidentification test			16.878			25.323			16.632
P-value			0.001			0.000			0.001

Note: ***, ** and * denote significance at 1 percent, 5 percent and 10 percent level, respectively. Robust t-statistics are in parenthesis.

Column 2 shows that in a bivariate regression, the institutional variable 'bureaucratic quality' has a significant coefficient with the correct sign. Column 3 indicates that even after including control variables, the coefficient of bureaucratic quality remains strongly significant. Column 4 presents the results of the second stage regression using the two stage least square (TSLS) method. The instruments are: (i) legal origin of countries; (ii) geography (absolute value of latitude); and (iii) break year from 1950.

The results of column 4 show that the coefficient for bureaucratic quality has the correct sign and is statistically significant, although the t-statistic is far less strong compared to column 3. This indicates that the OLS estimates in column 3 were biased. Columns 3 and 4 also show that the initial levels of per capita income and the initial levels of human capital are both significantly related to episode magnitude with the correct signs, while globalisation (trade ratio) shows no such relationship.

Columns 5 to 7 present results of regressions relating episode magnitude to law and order. The TSLS estimation in column 7 again uses the same instruments, i.e.: (i) legal origin of countries; (ii) geography (absolute value of latitude); and (iii) break year from 1950. The results are very similar to those with bureaucratic quality. Thus, the OLS and TSLS estimates in columns 5, 6 and 7 all show that law and order has a positive and significant relationship with episode magnitude, although the results are much stronger for the OLS estimates. Columns 6 and 7 also show that the initial levels of per capita income and the initial levels of human capital are both significantly related to episode magnitude with the correct signs, while globalisation (trade ratio) shows no such relationship.

Columns 8 to 10 present results of regressions relating episode magnitude to contract viability. The TSLS estimation in column 10 uses the same instruments, i.e.: (i) legal origin of countries; (ii) geography (absolute value of latitude); and (iii) break year from 1950. The results are again very similar to those with bureaucratic quality and law and order. The OLS and TSLS estimates in columns 8, 9 and 10 all show that contract viability has a positive and significant relationship with episode magnitude, and here the results are even stronger for the OLS estimates. Columns 9 and 10 again show that the initial levels of per capita income and the initial levels of human capital are both significantly related to episode magnitude with the correct signs, while globalisation (trade ratio) shows no such relationship.

The use of instrumental variables in our regression exercises raises the issue of the validity of the instruments. From the first stage regressions presented in Table A1 in the appendix, we find that the instruments explain a large part of the variance of all three institutional variables, with the centred r-squares ranging between 42 and 47 percent. The under-identification test for weak instruments is presented in the bottom panel of Table 2. These show that, for all three measures of institutional quality, the TSLS estimation does not suffer from a weak instruments problem. Similarly, the instrument suitability (Hansen J statistic) tests are also presented in the bottom panel of Table 2. The instrument suitability is also not rejected in the case of any of the three institutional variables. These tests prove that our choice of instruments is statistically valid.

Robustness tests

In Table 2 presented above, we have already tested for the robustness of the results across alternative functional forms of the regression (unconditional and conditional OLS) and alternative estimation techniques (OLS and TSLS). In this section, we undertake more robustness checks. As described earlier, there are three sets of robustness checks that we have undertaken and these are presented in Table 3. The first robustness exercise tests whether the inclusion of episodes where there were some missing institutional data (and hence the measures of these institutional variables were the averages of only those years for which the data were available) introduces a bias in the benchmark TSLS results discussed earlier. We do this test by repeating the benchmark TSLS regressions, but including only those episodes that start after 1980 – a period for which almost all data are available. These TSLS regressions for bureaucratic quality, law and order, and contract viability, respectively, are presented in columns 2, 3 and 4 of Table 3. The second robustness exercise tests whether the truncated episodes (i.e., episodes which end in 2010 due to lack of data availability after that year) unduly bias the results in the benchmark exercise. In order to control for such a possibility, we repeat the benchmark TSLS estimation by including a dummy that has a value equal to one for all such truncated episodes and zero otherwise. The results for bureaucratic quality, law and order, and contract viability, respectively, are presented in columns 5, 6 and 7 of Table 3. Finally, the third set of robustness exercises repeat the benchmark TSLS estimations for an alternative measure of the endogenous variable, i.e., episode magnitude (complete regression to mean). Columns 8 to 10 present the results from these TSLS regressions for bureaucratic quality, law and order, and contract viability, respectively. The results from all three sets of robustness exercises confirm the results of the benchmark TSLS estimates in Table 2, underlining the robustness of these results.

Table 3. Alternative robustness tests

	Sample to include only breaks from 1980 Dep. Var.: episode magnitude (PRM)			Controlling for incomplete episodes Dep. Var.: episode magnitude (PRM)			Alternative measure of episode magnitude Dep. Var.: episode magnitude (CRM)		
Bureaucratic quality	0.353***			0.228**			0.225**		
	(2.68)			(2.25)			(2.23)		
Law and order		0.127**			0.117**			0.130**	
		(2.13)			(2.00)			(2.37)	
Contract viability			0.112***			0.167**			0.098**
			(2.65)			(2.26)			(1.99)
(log) Initial PCY	-0.252***	-0.143***	-0.172***	-0.241***	-0.181***	-0.232***	-0.186***	-0.132***	-0.134***
	(-2.93)	(-3.23)	(-3.93)	(-3.80)	(-4.19)	(-4.50)	(-3.11)	(-3.45)	(-3.85)
Trade ratio	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	0.001	0.001	0.001
	(-1.10)	(-0.21)	(-0.54)	(-1.13)	(-0.60)	(-1.36)	(0.86)	(1.29)	(0.59)
Initial human capital	0.025	0.034**	0.034**	0.046**	0.048***	0.035*	0.042**	0.041***	0.038**
	(1.12)	(1.98)	(2.09)	(2.49)	(2.63)	(1.83)	(2.53)	(2.63)	(2.24)
Dummy(final year)				0.056	0.053	-0.105			
				(0.97)	(0.95)	(-0.99)			
Cons	1.264***	0.546**	0.428**	1.231***	0.783***	0.635***	0.794**	0.352	0.189***
	(2.66)	(2.30)	(2.03)	(3.30)	(3.12)	(2.83)	(2.26)	(1.54)	(0.79)
Number of obs	142	142	142	195	195	194	195	195	194
Hansen J statistic	0.056	4.046	4.812	1.313	2.434	1.540	1.539	0.180	2.344
P-value	0.972	0.132	0.090	0.519	0.296	0.463	0.463	0.914	0.310
Under-identification test	13.187	24.488	19.497	20.888	26.278	9.927	16.878	25.323	16.632
P-value	0.004	0.000	0.000	0.000	0.000	0.019	0.001	0.000	0.001

Note: ***, ** and * denote significance at 1 percent, 5 percent and 10 percent level, respectively. Robust t-statistics are in parenthesis. Episode magnitude (PRM) is estimated with counterfactual growth based on partial-regression-to-mean and episode magnitude (CRM) is estimated with counterfactual growth based on complete-regression-to-mean.

6. Conclusions

In this paper, we contribute to the growth episode literature by carrying out a regression-based analysis of the determinants of more successful episodes. In particular, we reevaluate the relationship between growth and economic institutions within the growth episode framework. In contrast to the previous literature, which has focused mostly on the correlates of the timing of growth episodes, we examine the determinants of more successful growth episodes. To do this, we use a measure of the success of episodes called *episode magnitude*, which is the difference between the level of output at the end of the episode and the counter-factual of what the level of output would have been in the absence of the onset of the growth episode. We then use up to 210 growth episodes for which the relevant data are available, and examine the relationship between episode magnitudes and key explanatory variables, including different measures of institutional quality – property rights institutions, contractual institutions, and state capacity. Our results show that higher institutional quality, irrespective of the measure of institutions, is a significant determinant of more successful episodes. These results are robust to alternate specifications and to concerns of reverse causality.

Our results also indicate that there is reverse feedback running from episode magnitude to institutional quality. Initial conditions like per capita income and human capital are also found to be significant determinants of growth outcomes. The fact that the institutional variables have a significant relationship with episode magnitude, after controlling for initial conditions, also makes these results stronger, since it disproves the argument that institutions do not cause growth independently of initial endowments of human capital, etc. (Glaeser et al. 2004). The negative coefficient of initial per capita income also resonates with the idea of convergence of growth rates.

The previous literature on the determinants of long-run incomes across countries has established the robust causality from institutions to growth; however, the correlation between initial institutional quality or its change over time and economic growth has been observed to be weak. In this paper, we show that institutions matter in how successful a country will be in observing an increase in per capita incomes, with the onset of a growth acceleration episode. Conversely, higher institutional quality reduces the likelihood of a large decline in per capita incomes in a growth deceleration. Our findings, therefore, suggest that for many developing countries, strengthening institutional quality is a key ingredient in the achievement of successful growth episodes.

Appendix

TableA1: First-stage regression results for the benchmark model

Independent variables	fl_burqua	fl_lo	fl_contviab2
(log) Initial PCY	0.343***	0.201**	0.490***
	(4.86)	(2.12)	(3.22)
Trade ratio	0.004***	0.006***	0.009***
	(2.93)	(3.29)	(3.56)
Initial human capital	0.057	0.036	0.069
	(1.46)	(0.63)	(0.85)
Legal origin	-0.124	0.008	-0.040
	(-1.63)	(0.08)	(-0.29)
Latitude (absolute)	1.723***	3.428***	2.762***
	(4.09)	(5.81)	(3.12)
Break year from 1950	-0.007	0.005	0.041***
	(-0.99)	(0.48)	(3.02)
Cons	-1.348**	0.018	-0.345
	(-2.39)	(0.02)	(-0.28)
Number of obs	195	195	194
Centred R2	0.477	0.421	0.427
Uncentred R2	0.872	0.915	0.957

Note: ***, ** and * denote significance at 1 percent, 5 percent and 10 percent level, respectively.
Robust t-statistics are in parenthesis.

TableA2: First-stage regression results for robustness test

	Sample to include only breaks from 1980			Controlling for incomplete episodes			Alternative measure of episode magnitude		
	fl_burqua	fl_lo	fl_contviab2	fl_burqua	fl_lo	fl_contviab2	fl_burqua	fl_lo	fl_contviab2
(log) Initial PCY	0.417***	0.298***	0.708***	0.340***	0.197**	0.482***	0.342***	0.201**	0.490***
	(5.34)	(2.96)	(4.57)	(4.79)	(2.10)	(3.28)	(4.86)	(2.12)	(3.22)
Trade ratio	0.004**	0.008***	0.009***	0.003**	0.005***	0.007***	0.004***	0.006***	0.009***
	(2.28)	(3.76)	(3.21)	(2.46)	(2.68)	(2.72)	(2.93)	(3.29)	(3.56)
Initial human capital	0.015	-0.074	-0.070	0.058	0.038	0.073	0.057	0.036	0.069
	(0.34)	(-1.28)	(-0.84)	(1.54)	(0.70)	(1.00)	(1.46)	(0.63)	(0.85)
Legal origin	-0.202**	-0.0003	-0.068	-0.133*	-0.006	-0.075	-0.124	.008	-0.040
	(-2.02)	(-0.00)	(-0.43)	(-1.76)	(-0.06)	(-0.59)	(-1.63)	(0.08)	(-0.29)
Latitude (absolute)	1.715***	3.787***	2.843***	1.713***	3.414***	2.731***	1.723***	3.428***	2.762***
	(3.68)	(5.77)	(2.78)	(4.08)	(5.78)	(3.15)	(4.09)	(5.81)	(3.12)
Break year from 1950	0.017	0.037***	0.075***	-0.015**	-0.007	0.011	-0.007	0.005	0.041***
	(1.56)	(2.63)	(4.1)	(-2.30)	(-0.72)	(0.88)	(-0.99)	(0.48)	(3.02)
Dummy (final year)				0.309**	0.439***	1.114***			
				(2.49)	(2.60)	(5.57)			
Cons	-2.535***	-1.769**	-2.687**	-1.116**	0.348	0.485	-1.348**	0.0184	-0.345
	(-3.34)	(-2.01)	(-1.98)	(-2.04)	(0.46)	(0.42)	(-2.39)	(0.02)	(-0.28)
Number of obs	142	142	142	195	195	194	195	195	194
Centred R2	0.473	0.420	0.468	0.490	0.439	0.486	0.477	0.421	0.427
Uncentred R2	0.874	0.919	0.967	0.875	0.917	0.961	0.872	0.915	0.957

Note: ***, ** and * denote significance at 1 percent, 5 percent and 10 percent level, respectively. Robust t-statistics are in parenthesis.

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