

**INSTITUTIONS AND ECONOMIC GROWTH – REVISITING THE
EMPIRICAL EVIDENCE FROM A DEVELOPMENTAL
PERSPECTIVE**

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Abstract: We empirically analyse the interaction between institutions and growth in a sample of developing economies, using Rodrik’s taxonomy of “*market-creating*”, “*market-regulating*” “*market-stabilising*” and “*market-legitimising*” institutions. We therefore bring together economic institutions, political institutions and policy regimes into the same framework. We utilise a dynamic panel data model to revisit the issue. We attempt to test whether institutions have a direct partial impact on growth, after controlling for income level, time-invariant country-specific characteristics and time dimension. Using GMM estimators in a dynamic panel data model allows us to estimate the impact of institutions on economic growth without seeking “external” instruments, yet taking the problem of endogeneity into account. One innovation is to utilise ‘structural’ characteristics of a dual economy, following Lewis, since these may have implications as to how institutions are more or less effective. This integrates neoclassical growth considerations with structural models of development, which also has implications for public choice in a developing economy.

Keywords: Institutions, Economic Growth, Developing countries, Panel Data

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1. Introduction

To explore the interaction between institutions and economic development, the most direct analysis would be to study the growth impact of institutions. After all, one of the major means of improving the well-being of the society is through promoting economic growth, thereby also narrowing cross-country income differences and creating possibility of convergence. As Lucas claimed: “By the problem of economic development I mean simply the problem of accounting for the observed pattern, across countries and across time, in levels and rates of growth of per capita income” Lucas (1988).

The proposition that domestic institutions matter to economic growth is not new in the economic development literature. Arthur Lewis, often considered to be the ‘father’ of modern development economics, stated: “Differences between countries ... may be traced to three distinct causes, namely to differences in the valuation of material goods relatively to the effort required to get them, to differences in available opportunities, and to differences in the extent to which institutions encourage effort, either by removing obstacles in its way, or by ensuring to the individual the fruit of his effort. Many of the observed differences in effort are due to institutional defects, and social reformers who wish to promote economic growth are mainly concerned with seeking to bring about appropriate changes in institutions, whether by propaganda or by law”. Lewis (1955:23).

Nevertheless, there are competing views arguing that the role of institutions is too overplayed. Amongst others, critiques claim that geographical endowments, culture, religion, historical events are also key determinants. Other empirical works suggest that in fact human capital is an even more fundamental cause of economic growth. Exactly why institutions matter to economic growth is also very loosely defined? Others criticise that the *institution view* literature present evidence in a very long-run timeframe, assuming that institutions are persistent and without considering the effect of institutional change on current economic activity or outcome.

Although there is a vast amount of empirical evidence to support the institution view, the estimation strategies are being severely scrutinised. Critics rightly point out that much of the existing empirical literature is predominantly cross-sectional in nature, thereby ignoring the dynamic impact of institution on growth. Furthermore, they do not control for country-specific and time characteristics. In addition, both institutions and economic growth understandably can be endogenously determined. Even though instrumental variable (IV) estimations are widely used to tackle such problem, the validity of instruments has called for much doubt. Last but not least, data quality and definition of institution are also subject to criticisms.

In light of these limitations and comments, we propose to use a dynamic panel data model to revisit the issue. We attempt to test whether the institution has a direct partial impact on growth, after controlling for income level, time-invariant country-specific and time characteristics. Using GMM estimators in a dynamic panel data model allows us to estimate the impact of institution on economic growth without seeking “external” instruments, yet takes the problem of endogeneity into account.

In response to the question of exactly what institutions matter to growth, we adopt a unified framework developed by Rodrik (2005) for our empirical investigations. Under his taxonomy, institutions refer to those activities that sustain the market operations of the economy. He develops the taxonomy into four clusters, namely “*market-creating*” institution – measuring the security of property rights and contract enforcement, “*market-regulating*” institution – measuring the regulatory environment of the society, “*market-stabilising*” institution – measuring the fiscal and monetary institution to stabilise the market and finally “*market-legitimising*” institution – measuring the political regime which legitimises the operation of the market. As noted, this way of clustering brings three frequently, yet separately, tested, dimensions of institutions – economic institutions, political institutions and policy regimes – into synergy.

However, one glaring omission in the theoretical and empirical literature on institutions and economic growth is the absence of structural features which may impact on the transmission mechanism of the impact of institutions on economic development. Much of the augmented neoclassical literature uses an aggregative framework where the role of institutions is similar, independent of the structure of the economy. Yet, as Lewis showed, development is often based on different behaviour patterns of major macro sectors of the economy. In a dual economy framework, the different response function to policy, institutions and public choice may depend on the structural characteristics of the underdeveloped economy. Thus, the distinction between agriculture and industry, or rural and urban, or backward and forward sectors of an economy, or manufacturing and non-manufacturing, might be an important part of institutional change. We mimic these complex interactions by some simple proxies in the empirical model.

Against the background of these inadequacies of the existing empirical literature, we attempt to revisit the empirical evidence of institution and growth for three main objectives. First, we jointly test the impact of economic and political institutions on growth using dynamic panel data models. Such methodology helps partially solve the problem of finding “external” instruments for tackling the weak endogeneity problem. Second, it allows country-fixed effect and within-country change over time to be controlled for. Third, we use Rodrik (2005)’s taxonomy as the operational framework for our assessment, adopting a unified approach to investigate the relative importance and direct partial impact of each type of institutions on growth.

All in all, we essentially try to answer what institutions *do* and *what* type of institutions cause growth. The paper is organised as the following. We present a brief literature survey in Section 2. Our estimation technique and data are discussed in Section 3. The empirical results follow in Section 4. In particular Section 4.4 introduces the core developmental and structural features of a dual economy and replicates the empirical tests. We conclude in Section 5.

2. Literature Survey: Institution and Economic Growth

A vast amount of theoretical and empirical work on institutions and growth has been developed in the last two decades. In the following, we briefly review the results of these key studies, their critics and empirical shortcomings. Based on the literature survey, we summarise 6 key technical shortcomings of these empirics with an aim to provide justifications for the estimation we perform in the present paper.

2.1 Institution View: Economic vs. Political Institutions

Traditional growth theories emphasize the role of human capital (e.g. Lucas (1988)), technological diffusion (e.g. Barro and Sala-i-Martin (1997)), public infrastructure (e.g. Barro (1990)) or incentives to innovate (e.g. Romer (1990)). North and Thomas (1973) nonetheless argue that institutions are the sources of cross-country differences in growth. This view was further echoed by Acemoglu, *et al.* (2005) and IMF (2005), claiming that institution is a more fundamental cause of growth. The institution view essentially argues that institutions are the fundamental source of growth. Empirical studies have been developed into two separate strands, namely the effect of economic and political institutions. We highlight few core empirical studies below.

2.1.1 Economic Institutions and Growth

Acemoglu, Johnson, and Robinson (2001, 2002) suggest that economic institution were developed by colonial transplantation. Their key observation of “reversal of fortune” is that, among countries colonized by European powers during the past 500 years, those that were relatively rich in 1500 are now relatively poor. They find that economic prosperity in the past, measured by urbanization and population density, does not link to geographic factors. In contrast, they suggest that this reversal actually reflects changes in the institutions resulting from European colonialism. European colonialism led to the development of institutions of private property in previously poor areas,

while introducing extractive institutions or maintaining existing extractive institutions in previously prosperous places. The main reason for this is that relatively poor regions were sparsely populated, and this enabled or induced Europeans to settle in large numbers and develop institutions encouraging investment. In contrast, a large population and relative prosperity made extractive institutions more profitable for the colonizers. In their 2SLS regressions, they show that mortality rates faced by settlers, as proposed by Acemoglu, *et al.* (2001), are a good instrument for settlements of Europeans in the colonies, i.e. conditional on the other controls, the mortality rates of European settlers more than 100 years ago have no effect on GDP per capita today, other than their effects through institutional development that subsequently affects growth rates today.

La Porta, Lopez-de-Silanes, Shleifer, *et al.* (1997, 1998) examine specifically the importance of colonial rule in relation to financial development as a proxy for investor protection. They focus on how legal institutions were transplanted by the different colonial powers. Their analysis emphasises that differences between legal systems based on British common law versus French civil law provide different degrees of investor protection. In their 2SLS estimations, their results show that civil law economies, relative to those with common law countries, have less investor protection. In the second stage estimations, they show that countries with weaker investor protection have smaller debt and equity markets. As Nunn (2009) points out, nevertheless, a large amount of literature emerged which show that legal origin is also correlated with a host of other country characteristics, including labour market conditions (Botero *et al.* (2004)) and even economic growth (Mahoney (2001)). This inevitably calls into question the validity of using legal origins to be instruments in the IV estimation. Such problem is also mentioned by the authors themselves in La Porta, Lopez-De-Silanes, and Shleifer (2008).

Acemoglu and Johnson (2005) further attempt to ‘unbundle’ which institutions affect economic development. They define institutions into two forms – (i) “property rights institutions”, which protect citizens against expropriation by the government and powerful elites, and (ii) “contracting institutions”, which

enable private contracts between citizens. In practice, they are measured by “protection against risk of expropriation” and “constraints on executives” from the Polity IV dataset respectively. Using settler mortality rate as instruments, their cross-sectional IV estimations show that property rights institutions have a first-order effect on long-run economic growth, investment, and financial development. Contracting institutions appear to matter only for the form of financial intermediation, including credit and stock market capitalisation.

2.1.2 Political Institution and Growth

Empirical results on the positive impacts of economic institution on growth are not particularly controversial despite all the diversities of views aforementioned. However, the effect of political institutions on economic growth, especially political regime like democracy, is far from conclusive. Research interests have been actively revived since Olson (1993). According to his theory, the state can either be a ‘stationary bandit’ or ‘roving bandit’. The former possesses uncontested ‘ownership’ of the streams of income generated by the private agents in the economy. The stream of income becomes part of the endowment, enabling the state to prosper. A stationary bandit will invest and nurture the economy, which will increase the level of investment in the economy. On the contrary, a roving bandit possesses little incentive to conserve since the prosperity of the nation will be enjoyed by the next bandit. In short, the investment level, which determines economic growth, will depend on the incentive structure of the ‘state’ or the elite who control the state – as in public choice models (Mueller (2003)).

Subsequent research followed, by relating how authoritarian (or totalitarian) regime and democratic regime behave like stationary and roving bandit. Nevertheless, there has been no straightforward argument on political regimes and policy choices in the empirical literature.

Among others, Przeworski and Limongi (1993) claim that if property rights are secured, then it promotes investment and suppresses immediate consumption

sensibly. However, it is not clear why democracy secures property rights and provides this credible commitment. In their review of 21 empirical studies they found no consistent empirical pattern between democracy and growth. The form of democracy also matters for the adoption of structural policies that promote growth. By means of cross-section and panel data analysis, Persson (2005) finds that reforms in parliamentary (as opposed to presidential) and proportional (as opposed to majoritarian) and permanent (as opposed to temporary) democracy appear to produce the most growth-promoting policies. Persson (2005) investigates the effect of democratic transitions on income while Giavazzi and Tabellini (2005) study interactions between political and economic reforms. In overall terms, these papers suggest some positive correlation between democracy and growth.

However, Andreski (1969) argues that democracy contributes to stagnation rather than growth because of the lack of investment and capital, and to the necessity of choosing between investment for the future and immediate consumption. It may also result that resources are wasted by interest groups for lobbying (Becker (1983)). In short, whether democracy has a direct growth impact is yet empirically inconclusive.

2.2 Empirical Critics of Existing Institutions and Growth Literature

Although the claim that institutions matter to growth has been accepted at an analytical level, empirical evidence remain inconclusive. This indeed reveals several key technical problems. Even in those works that provide support to the institution view, the estimates obtained are arguably biased, primarily because of the endogeneity problem in these growth empirics. The instruments used are also subject to many critics and do not robustly show their validity. Also, despite all the positive empirical results, there is no consistent measurement of what institutions are referring to.

To discuss the various critics of the existing literature in proper perspective, we highlight 6 key shortcomings of the relevant literature below with an aim to provide justifications for the estimation strategy employed in this study. These

problems generally concern the data and methodology. The 6 issues are summarised below.

2.2.1 Data Problem: Availability, Credibility and Comparability

The lack of consensus of exactly what institutional variables should be used in empirics is partially caused by the availability of data. Firstly, the lack of an operational and consistent definition of institution hampers the comparability of the empirical studies. The scope of measurement also invites more questions than answers. In fact, most of the variables used in the empirical studies are not specifically developed or derived from theoretical models. In most of the cases, these variables are employed as different types of proxies of institutions to suit the models' particular needs. Therefore, "institutions" are fairly loosely defined. Harper (2003), for example, argues that an operational definition of, say, economic freedom is absent and the weights put on each sub-categories are very doubtful. Glaeser, *et al.* (2004), likewise, challenge the measures in the sense that they are not exactly measuring what "institutions" are supposed to mean, i.e. constraints of possibility choice sets.

Secondly, these variables, for example the predominantly used ICRG's quality of institution and Fraser Institute's Economic Freedom Index, are often composed of a combination of qualitative survey results and quantitative data. Inevitably, the use of subjective quantitative data is being heavily criticised. Indeed, most of the institutional data are subjective. Also, it is not uncommon to find that different qualitative measures are cross-referenced from similar sources. For example, Fraser Institute's Economic Freedom Index draws different measures of regulation from ICRG and World Bank's Ease of Doing Business Indicators to construct a composite regulation index. This thus presents particular difficulty to carry out robustness tests using alternative proxies.

Thirdly, the time span of data is often quite limited, in particular to those measuring economic institution. This also explains why most of the existing literature is cross-sectional in nature.

2.2.2 Reverse Causality: Economic Growth causes Institutional Change

The literature also suggests reverse causality between institutions and economic growth. Amongst others, Chong and Calderon (2000) argue that most of the studies concerning institution and economic growth have not paid enough attention to the possibility that economic growth may lead to better institution. Their hypothesis is that the poorer the country, and the longer the wait, the higher the influence of institutional quality on economic growth once growth kicks off.

Rosenberg and Birdzell (1986) also provide a hypothesis why income can cause institutional development. They explain that systems that protect property rights, such as the judiciary, first require the development of a volume of commerce large enough before actual mechanisms and regulations can be properly instituted. Eggertsson (1990) also suggests that higher income levels may lead to stronger institutions when property rights become more valuable, more is spent to protect them. Furthermore, Mauro (1995) also opines that good economic performance increases institutional efficiency.

Farr, Lord, and Wolfenbarger (1998) test the causality relationship formally. They use the dataset from Business Environmental Risk Intelligence (BERI) to measure institutions, which covers: (i) contract enforceability, (ii) nationalisation potential, (iii) infrastructure quality, and (iv) bureaucratic delays and test the causation on income growth rates. Using Granger causality, their empirical results surprisingly show that the BERI index does not contain useful information for predicting future economic growth. Nevertheless, economic growth appears to generate higher institutional quality. The causal relationship is statistically significant and contributes about 61% of the linear association between these two variables. Even using individual BERI components, the casual direction from economic growth to institutional quality remains higher than the one from institutions to growth.

2.2.3 Endogeneity and Validity of Instrumental Variables

The problem of endogeneity induces biased results. Briefly discussed before, IV estimation appears to be the most favourable strategy for this subject. The instruments used for IV estimation so far are principally derived from La Porta, *et al.* (1997) and La Porta, *et al.* (1998)'s legal origin, Acemoglu, *et al.* (2001)'s European settler's mortality rate and Alesina, Devlieeschauwer, Easterly, *et al.* (2003)'s religion, ethnic and languages fractionalization. Collectively, these instruments intend to introduce time-invarying factors, e.g. geography (distance from equator and predicted trade share, oil exporters, disease burden etc) and colonial origins into the analysis of current income. There are however very few justifiable variables to be used. As a result, the same instrumental variables are often naturally used in different studies for different indicators of institutions, regardless the latter is referring to expropriation risk, rule of law, or average quality of institution. Pande and Udry (2006) also wonder on the small amount of instruments developed so far despite the huge amount of empirical studies on the subject.

2.2.4 Institutional Persistence vs. Institutional Change

Most of the empirical works done so far are cross-sectional analyses, aiming at estimating the long-run impact of institutions on growth. They are designed in the way to investigate how the average scores of institutions over a long period of time can associate with the present GDP per capita. The use of time-invariant instruments implicitly assumes that institutions are quite persistent. Technically speaking, such design of the instruments also does not allow the researchers to identify the consequences of institutional *change* for growth. In other words, these cross-country studies cannot capture the effect of dynamic institutional change on growth, in particular, within country variation over time.

2.2.5 Omitted Variables: Country- and Time-Specific Effects

In terms of econometrics, the time-invariant instruments could not be used in a panel-setting, especially in a fixed-effect model, since all these time-invariant characteristics will be absorbed into the unobserved fixed-effects. Unfortunately, such country-specific and time-specific effects are too significant to be ignored in growth empirics. Acemoglu, *et al.* (2008), while showing that democracy and higher income may well be mutually reinforcing, suggest that the strong correlation between the two may be driven by variables that related to colonial heritage and early institutions. This also implies the importance of incorporating that country-specific effect in growth empirics. Only recently, there are very few studies on the subject using panel data analysis, although it has long been recognised that the dynamic nature of institutional change is very important (Acemoglu and Johnson (2005)).

2.2.6 Specification Problem: Growth Rates vs. Income Level

Sachs (2003) criticised the institution view claiming that empirical studies of institutions and growth suffer from specification problem, in particular to static versus dynamic growth models. Economic theory suggests that per capita income should be specified as a *dynamic process* (e.g. Barro and Sala-i-Martin, 1997), in which the growth of income during a time interval $[0,t]$ should be a function of the income level at the start of the period and some kind of average of the values of the regressors during the time interval $[0,t]$. In short, growth models typically specify cross-country growth in country i as

$$\dot{g} = \alpha + \beta \ln y_{iT} + \mathcal{I}_i + \delta \mathbf{X}_i + \varepsilon_i \quad (1)$$

where \dot{g} is the average growth rate of GDP per capita, \mathbf{y} , over a period, T indicates a particular chosen year, say 1960. $\ln y_{iT}$ intends to capture the initial level of development and β represents the rate of convergence. \mathbf{I} is a vector of institutional variables and \mathbf{X} is a vector of macroeconomic and social variables.

Sachs (2003) is of the view that it is much more likely the quality of

institutions in a given time period will affect the *growth rate* of the economy during that period (controlling for initial income), as opposed to the contemporaneous *level* of national income as in Hall and Jones (1999).

In the light of these limitations in the literature, we propose to use dynamic panel GMM estimation technique in the present study. In this framework, the use of panel data could incorporate country- and time-specific effects which help alleviate the problem of omitted variables. We also use GDP per capita growth rate instead of income level. The key advantage of this methodology is that we do not need to seek “external” instruments to deal with endogeneity. In the following section, we will discuss the estimator in greater details.

3. Empirical Strategy and Data for this paper

In this section, we will discuss the baseline specification of our model, the empirical strategy employed (i.e. dynamic panel GMM estimator) and data used.

3.1 Baseline Specification

We convert eq.(1) into a panel data setting and thus we can study within country variation as well as cross-country variations. The baseline specification model is modified as:

$$\ln y_{it} - \ln y_{it-1} = \theta_0 + \theta_1 \ln y_{it-1} + \theta_2 I_{it} + \theta_3 X_{it} + u_i + \delta_t + v_{it} \quad (2)$$

$\ln y_{it} - \ln y_{it-1}$ is the growth rate of GDP per capita of economy i for period t . I_{it} is a vector of institutional variables. X_{it} is a vector of macroeconomic and social variables. u_i is the country-specific effect absorbs time-invariant factors, such as culture, legal origin, historical determinants and other geographical endowments being the determinants of economic development as proposed in

earlier literature. δ_i is a period-specific dummy to capture structural changes that are common to all countries. v_{it} is an error term.

Our specification intends to be canonical in nature. Eq.(2) forms our baseline specification. To get rid of the short-term effect of cyclical dynamics, the dependent and independent variables are taken as a non-overlapping 5-year average. Data span from 1970 to 2004, with seven periods in total (i.e. $t = 1970-74, 1975-79, \dots, 2000-04$).

3.2 Estimation Strategy

Traditional growth regressions like the one in eq.(1) generally carry problems such as endogenous regressors, measurement errors and omitted variables (e.g. Acemoglu (2009)). Given these problems, least square estimations are biased since unobserved omitted variables may potentially correlate with one of the regressors.

The use of panel data helps alleviate the problem of omitted variables by taking into account of country-specific and time-specific effects. Islam (1995) is an early example of the panel data approach for growth regressions, thus allowing the unobserved time-invariant country-specific effect to be controlled for.

To tackle the problems of endogeneity and measurement error, the frequently used approach of 2SLS in the standard institution and growth literature aims at alleviating the problem of endogeneity by introducing innovative instruments, such as settlers' mortality rate, legal origins, ethnic or language fragmentation. However, researchers recently have cast doubt on the validity of these instruments in growth empirics. This reverts to the old problem of finding an "appropriate" instrument for growth regressions. Moreover, a dynamic growth model as specified as eq.(2) is also prone to "dynamic panel bias" (Nickell (1981)) since the lagged dependent variable is correlated with the disturbance terms.

Arising from this, Arellano and Bond (1991) propose the application of first-

differenced generalized method of moments (GMM) estimators to estimate a dynamic panel data model. Blundell and Bond (1998) separately develop a system GMM estimator by introducing an additional stationarity restriction to the “differenced GMM” estimator. Such approach is further used in growth regressions by Caselli, Esquivel, and Lefort (1996), Benhabib and Spiegel (2000) and Levine, Loayza, and Beck (2000) among others.

3.3 Advantages of Using Panel GMM Estimators for Growth Empirics

Bond, *et al.* (2001) recommends the use of the “system GMM” estimator for growth empirics. This is due to two main reasons. First, it is often the case that researchers take a period average of output growth to get rid of cyclical dynamics. Hence, the number of periods used in the standard growth literature is usually small. Second, their empirical results show that if the time series are persistent e.g. growth rate of GDP per capita and institution (see Coviello and Islam (2006)), the “differenced GMM” estimator can behave poorly because lagged levels of the series only provide weak instruments. They show that the “differenced GMM” estimates of the coefficient on the lagged dependent variable tend to lie below the corresponding within-group estimates in a fixed effect model, suggesting that the “differenced GMM” estimates are seriously biased. Hence, we deploy “system GMM” estimators for our empirical analysis.

To sum up, the GMM estimators for panel analysis allow arbitrarily distributed fixed individual and time effects. This provides an added advantage over cross-sectional regressions, which are particularly prevalent in existing institution and growth literature, where cross-country fixed effects are assumed away. Under a panel set-up, studying the variations over time could also be possible. Besides, the GMM estimators allow idiosyncratic, disturbance terms which are heteroskedastic and autocorrelated. These disturbance terms, however, are assumed to be uncorrelated across individuals. With regard to the regressors, they could be endogenous, predetermined and not strictly exogenous. The estimators are also applicable for a panel set-up with a large number of cross-sections (i.e. large N) and a small number of periods (i.e.

small T)¹. A particular key advantage in our context is that it does not assume the availability of “external” instruments. In other words, all possible instruments exist readily in the dataset. The use of these “internal” instruments – based on the lagged values of the dependent variables – implies that we allow the explanatory variables to be weakly exogenous.

3.4 Data

The dependent variable of eq.(2) is the PPP-adjusted real GDP per capita growth rate (*GDPPC_gr*) from the World Development Indicators (WDI). Our data sample spans from 1970 to 2004. The number of cross-sections (i.e. economies) in the estimations is around 99 countries in our main specification restricted by the country coverage of Fraser Institute’s Economic Freedom Index. In addition, we also control for human capital (*SCHOOLING*) in our baseline specification, as in Glaser et al (2004). As in standard growth empirics, it is proxied by the average year of schooling in the total population aged 25 or above, estimated by Barro and Lee (2001)².

Our measures of different institutional variables primarily come from Fraser Institute’s Economic Freedom of the World Report (Gwartney, Lawson, and Norton (2008)) and Polity IV project dataset (Marshall and Jaggers (2009)). As mentioned, the Fraser Institute’s Economic Freedom Index, to the best of our knowledge, is the only available source which covers some kinds of measures of economic institution dating back to 1970. A long time series is preferred in our case as we intend to undertake dynamic analysis. Furthermore, another advantage is that the various sub-components of the Fraser’s Index can be used to proxy the three facets of institutions as described above. In contrast, the commonly used alternative – ICRG’s rule of law index – is not used in our case. The major reason is that it covers a relatively shorter period of time, dating back to 1984 only. Nevertheless, we will use a composite index measuring the quality of government (*QOG*) from ICRG as robustness check

¹ The number of instruments used tends to explode with T in panel GMM estimations. This is one of the reasons why the GMM estimator is only applicable when T is small.

² Data can be retrieved from www.cid.harvard.edu/ciddata/ciddata.html.

for our results.

Specifically, in response to Rodrik (2000)'s 4-cluster taxonomy of market-sustaining institution, we use the Fraser Institute's legal structure and security of property rights index (**LEGAL**) to proxy the effectiveness of property rights security and contract enforcement for "*market-creating institutions*". For "*market-regulating institution*", we use the composite index of regulation (**REG**). The index covers the regulatory burden of the credit market regulation, labour market regulation and business regulation. While some components of both **LEGAL** and **REG** are primarily obtained from opinion survey results, they are designed in such a way to subjectively describe the general perception of the overall environment of security of property rights and regulatory environment. On **REG** in particular, this is an overall macro-level index and is not specific to the regulatory environment of particular markets, it thus sheds no particular light on whether there exists any mechanisms for correcting market failures. Nevertheless, we consider these two proxies are sufficient in a macroeconomic growth model as in our case.

For "*market-stabilising institutions*", we consider using the access to sound money index (**SM**) to proxy the effectiveness of monetary and fiscal institutions. The index covers money (specifically, M2) growth, standard deviation of inflation, average inflation rate and freedom to own foreign currency bank accounts, which can duly reflect the functioning of monetary institutions and management of macroeconomic policies. All Fraser's indices range from 0 to 10, with higher scores signifying better institutions.

For "*market-legitimising institutions*", we use the democracy index from the Polity IV project (**DEMOC**). It is a composite index, ranging from 0 to 10, measuring the competitiveness of political participation, openness and competitiveness of executive recruitment and constraints on the chief executive (**XCONST**). Higher score means the country is more democratic. In our robustness checks, we will also attempt to use the sub-component **XCONST** as a proxy of contract enforcement as in previous empirical studies like Acemoglu and Johnson (2005). In addition, we will also use Freedom House's indicators

on political rights and civil liberties (*IPOLITY2*) to proxy democratic governance instead of *DEMOC* to check sensitivity of our results. We will discuss these alternative measures when we present our robustness tests in Section 4.2.3.

The pairwise correlation matrix between key variables suggests that the institutional variables do not necessarily strongly correlate with each other. It thus appears that multicollinearity is not a major problem in our case.

4. Results and Discussion

4.1 Institution in a Growth Model: Preliminary Test

Sachs (2003) criticises the empirical studies of institution view that the specification is not dynamic in nature. In response to this, we first take a preliminary test of institutions in a dynamic growth model framework. We make reference to three growth models in the literature and re-examine them if institutions are associated with economic growth after controlling for all the time-invariant country-specific characteristics, say geography, as well as the initial level of economic development using dynamic panel data estimations.

The first one is Hall and Jones (1999)'s baseline specification, where output per worker solely depends on "social infrastructure". We modify their model in a panel setting in response to Sachs (2003)'s criticism as follows:

$$GDPPC_{-gr_{it}} = \alpha_0 + \alpha_1 \ln(GDPPC)_{it-1} + \alpha_2 I_{it} + u_i + \delta_t + v_{it} \quad (3)$$

We replace "social infrastructure" by the quality of government indicator (*QOG*), which is a composite indicator of corruption, law and order and bureaucratic quality consolidated by Teorell, Charron, Samanni, *et al.* (2009). The original data source is from ICRG, the one used in the literature as in Hall and Jones (1999). We use a composite indicator here instead of unbundling

different institutions is to allow comparability with the literature. This composite index we use here is scaled from 0 to 1, with higher values indicating higher quality of government. *QOG* is the average value during a 5-year period.

The second model is modified from Mankiw, Romer, and Weil (1992) (MRW) to test the effect of human capital in an augmented Solow growth model.

We can then re-assess the augmented Solow model in a dynamic panel data framework as in Bond, *et al.* (2001). The relevant growth equation is below:

$$\begin{aligned}
 GDPPC_gr_{it} = & \gamma_0 + \gamma_1 \ln(GDPPC)_{it-1} + \gamma_2 \ln(s_{it}) + \gamma_3 \ln(n_{it} + g + \eta) \\
 & + \gamma_4 SCHOOLING_{it} + u_i + \delta_t + v_{it}
 \end{aligned} \tag{4}$$

Eq.(4) includes the natural log of saving rate ($\ln(s_{it})$) and the natural log of population growth ($\ln(n_{it})$), plus 0.05, where 0.05 represents the sum of a common exogenous rate of technical change (g) and a common depreciation rate (η) as in MRW. Data of saving rate and population growth come from WDI. The regressors also include a measure of human capital, i.e. the average years of schooling (*SCHOOLING*). The unobserved country-specific effects can be interpreted as the differences in the initial level of efficiency, whilst the period-specific intercepts capture productivity changes that are common to all countries. As in MRW, the technical progress is assumed to be common for all countries.

The third model extends that of eq.(4). The augmented Solow growth model of Bond, *et al.* (2001) does not incorporate institution. We thus incorporate the effect of institution as in Penalosa and Teksoz (2006), in which the authors assume that institutions interact with the level of productivity but do not enter directly as inputs. With this assumption, the specification of our estimation can then written as (5) below.

$$GDPPC_gr_{it} = \gamma_0 + \gamma_1 \ln(GDPPC)_{it-1} + \gamma_2 \ln(s_{it}) + \gamma_3 \ln(n_{it} + g + \eta) \tag{5}$$

$$+ \gamma_4 \text{SCHOOLING}_{it} + \gamma_5 I_{it} + u_i + \delta_t + v_{it}$$

Estimation results of these three models are at Table 4.1-1 below. Vis-à-vis previous cross-sectional literature, estimating growth models in a dynamic panel data framework presents several interesting results. As shown in Model 2.(1), institution seems to have a significant and positive impact on economic growth. After controlling time-invariant determinants in a panel setting, institution still significantly explains cross-country economic performances. That said, our results provide support to the institution view, despite using modified specifications and estimation techniques.

**Table 4.1-1: Preliminary Tests of Dynamic Growth Models
(Two-Step System GMM Estimation)**

<u>Model</u>	<u>2.(1)</u>	<u>2.(2)</u>	<u>2.(3)</u>
<u>Specification</u>	<u>Hall and Jones (1999)</u>	<u>Mankiw et al (1992)</u>	<u>Penalosa and Teksoz (2006)</u>
<i>ln(GDPPC)_{t-1}</i>	-0.0020 (0.0014)	-0.0071 (0.0133)	-0.0165 (0.0125)
<i>ln(s)</i>		0.0193*** (0.0069)	0.0256*** (0.0063)
<i>ln(n+g+η)</i>		-0.0932* (0.0532)	-0.1038* (0.0617)
<i>SCHOOLING</i>		-0.0004 (0.0077)	0.0020 (0.0079)
<i>QOG</i>	0.0596*** (0.0096)		0.0386** (0.0172)
<i>Const</i>	0.0152 (0.0108)	-0.2338 (0.1733)	-0.2404 (0.1869)
<i>Observations</i>	760	644	424
<i>AR(1) p-value</i>	0.0774	0.0000	0.0000
<i>AR(2) p-value</i>	0.613	0.784	0.402
<i>Hansen Test Stat</i>	6.709	18.30	19.82
<i>Hansen Test p-value</i>	0.460	0.147	0.228

Dependent variable is *GDPPC_gr*. 5-year interval panel data is used for estimation. The lagged *ln(GDPPC)* is treated as pre-determined, while other regressors are endogenous. Period dummies are included but not reported. Standard errors are in parenthesis and Windmeijer-corrected, robust for heteroskedasticity and autocorrelation. ***, ** and * denote significance levels at 1%, 5% and 10% respectively. Instruments are transformed by orthogonal deviation and collapsed. Hansen test is used for overidentifying restrictions.

Secondly, the cross-sectional analysis of MRW shows that human capital plays a key role in economic growth. Their results suggest that, holding all other variables constant, a country with approximately 12% school enrolment should have income per capita of about 9 times that of a country. However, our Model 2.(2) shows that human capital does not appear to be statistically significant once we estimate the model in a dynamic panel data framework. In this Model, it even comes with a “wrong” sign.

Thirdly, Model 2.(3) shows that the impact of institution on economic growth remains statistically significant even after controlling for human capital. Human capital, though turns correctly positive to growth in the augmented

Solow model, stays statistically insignificant. One should also note that, comparing Models 2.(2) and 2.(3), all coefficients become slightly bigger once institution is included in the latter model. This may suggest that the channel through which institution affects growth may be via its effect on other factors of production.

To justify whether the two-stage system GMM estimator should be used, we see that all three models accept the AR(1) test but reject the AR(2) test, except that in Model 2.(1), it is only significant at 10% level. It therefore suggests that there is no serial correlation at level. The Hansen test also does not reject the null hypothesis. It implies that we cannot reject the over-identification restriction. All these results suggest that dynamic panel data models can be used.

4.2 Direct Partial Impact of Institution on Growth

4.2.1 Baseline Specification

We then proceed to estimate the direct partial impact of institution based on the taxonomy of institutions proposed earlier. Table 4.2-1 below presents the empirical results of our baseline specification as described in eq.(2).

**Table 4.2-1: Institution and Economic Growth – Baseline Specification
(Two-Step System GMM Estimation)**

Model	2.(4)	2.(5)	2.(6)	2.(7)	2.(8)
<i>DEMOC</i>	0.0011 (0.0009)				0.0015 (0.0015)
<i>REG</i>		0.0046 (0.0038)			0.0042 (0.0045)
<i>SM</i>			0.0020* (0.0010)		0.0031* (0.0018)
<i>LEGAL</i>				0.0066*** (0.0021)	0.0052*** (0.0020)
<i>SCHOOLING</i>	0.0102* (0.0059)	0.0091* (0.0053)	0.0147*** (0.0051)	0.0128** (0.0056)	0.0139** (0.0067)
<i>ln(GDPPC)_{t-1}</i>	-0.0060 (0.0108)	-0.0090 (0.0106)	-0.0130 (0.0089)	-0.0197** (0.0089)	-0.0304** (0.0118)
<i>Const</i>	-0.0042 (0.0556)	0.0030 (0.0565)	0.0112 (0.0469)	0.0553 (0.0430)	0.0843 (0.0575)
<i>Obs/No. of cty</i>	556/99	500/95	542/95	483/95	447/91
<i>AR(1) p-value</i>	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
<i>AR(2) p-value</i>	[0.364]	[0.855]	[0.605]	[0.540]	[0.279]
<u>Hansen Test</u>					
<i>Test Stat</i>	14.62	13.63	4.210	6.174	21.80
<i>p-value</i>	[0.404]	[0.191]	[0.838]	[0.628]	[0.241]

Dependent variable is *GDPPC_gr*. 5-year interval panel data is used for estimation. The lagged *ln(GDPPC)* is treated as pre-determined, while other regressors are endogenous. Period dummies are included but not reported. Standard errors are in parenthesis and Windmeijer-corrected, robust for heteroskedasticity and autocorrelation. ***, ** and * denote significance levels at 1%, 5% and 10% respectively. Maximum 4 lags are used for instruments. Instruments are transformed by orthogonal deviation and collapsed. Hansen test is used for over-identifying restrictions.

The presence of the lagged *ln(GDPPC)* in the model means that all the estimated coefficients of institutions represent their short-run effects. A significantly negative coefficient of *ln(GDPPC)* signifies convergence. In the baseline specification, we control for human capital as proxied by *SCHOOLING* as the only macroeconomic control variable. To recap, we use *DEMOC*, *REG*, *SM* and *LEGAL* to measure the four clusters of market-sustaining institutions, namely “*market-legitimising*”, “*market-regulating*”, “*market-stabilising*” and “*market-creating*” institutions respectively. We test individual variable in Models 2.(4) – 2.(7) and all variables jointly in Model 2.(8).

Our empirical results show that the signs of coefficients of *DEMOC* and *REG* are positive as expected. It implies that democracy and less regulation seem to associate with faster growth. However, their coefficients are not statistically significant. Similar to the results in the previous literature discussed in Section 2.1.2, democracy shows no significant linear effect on growth in our sample. With respect to regulation, Rodrik (2005) suggests that a more developed economy intends to have more regulation to rectify market failures, say externalities. However, the corruption literature (e.g. Djankov *et al.* (2002)) on the other hand argue that more regulations in developing economies is more likely lead to more corruption, which is socially wasteful. These two competing hypotheses probably explain the lack of linear direct effect of *REG* in our full sample.

LEGAL and *SM*, in contrast, show significant, positive and direct impacts on growth. In other words, security of property rights and “*market-stabilising*” institutions both impose direct impacts on economic growth, although *SM* is only statistically significant at 10% level.

In Model 2.(8), we estimate the baseline specification with all regressors being estimated jointly. The results suggest that a one sample standard deviation increase in the *LEGAL* and *SM* indices will increase growth rate by 1.0 and 0.69 percentage points respectively. In a dynamic panel data setting, our results generally agree with the key conclusion of the previous literature. In other words, “*market-creating*” and “*market-stabilising*” institutions have direct effect on growth, whereas “*market-regulating*” and “*market-legitimising*” institutions show no direct partial impact. While the previous literature aims at establishing a long-run direct impact of institutions, our estimations further relax the assumption of institutional persistence. We find that short-run institutional change has a direct impact on growth. More specifically, comparing to the results of Bhattacharyya (2009), our results show that *LEGAL* relatively plays a more significant role than *SM*.

Hansen test statistics show that the models satisfy the over-identification

restrictions and do not reject the joint validity of the instruments used. This suggests the lags used in the models are valid instruments. According to the Arellano and Bond autocorrelation test in levels in our results, the disturbances terms exhibit AR(1) process but not AR(2) in our models. A rejection of the second-order autocorrelation indicates that there is no autocorrelation of the disturbance term in level. In overall terms, the post-estimation tests indicate that using GMM estimators for our models are appropriate.

4.2.2 Choice of Estimators

To further verify whether the Blundell and Bond (1998)'s 2-step system GMM estimator is preferred in our case, we also run both the OLS and within group fixed-effect (FE) estimators for comparison. Although there is no formal test for justifying the use of the GMM estimator, Coviello and Islam (2006) suggest a possible rule of thumb. They argue that in a dynamic panel model, the OLS estimator is biased and inconsistent as the lag dependent variable is positively correlated with the country fixed effect and the error terms. On the other hand, the FE estimator, by estimating the within group difference, can eliminate this sort of inconsistency by transforming the equation to eliminate the country specific effect. However, for panels where T is small relative to N as in our case, the first difference transformation induces a “non-negligible” correlation between the transformed lagged dependent variable and the transformed error term. The FE estimator, therefore, is also potentially biased.

The nature of the bias of these two estimators could assist in identifying a satisfactory estimator. Nickell (1981) shows that when fixed effects are correlated with explanatory variables, then OLS overestimates the effect of the lagged dependent variable, fixed effect underestimates it and system GMM should be in between the two. Bond, *et al.* (2001) and Bond (2002) suggest that the OLS level estimator of the autoregressive coefficient could be regarded as the upper bound for the true estimates, whereas the FE estimator of the autoregressive coefficient can be treated as the lower bound of the true estimates. Therefore, we expect that the true estimator would lie between the

OLS and FE estimates. The GMM estimator, if appropriate in our case, should lie somewhere in between or at least not to be significantly higher/lower than the upper/lower bounds.

The relevant test results are presented in Table 4.2-2. Models 2.(9) and 2.(10) are the OLS and FE estimations respectively. Models 2.(11) and 2.(12) show the results of our system GMM estimations. The magnitudes of coefficients in the 1-step and 2-step GMM models are not quite significantly different. The coefficient on the lag dependent variable is around -0.03 in Models 2.(11) and 2.(12), which lies between that of the OLS (-0.006) and FE (-0.06) estimates. We hence conclude that the system GMM estimator could be used in our case.

Table 4.2-2: Institution and Economic Growth – Baseline Specification Using Different Estimation Techniques

Model Estimator	<u>2.(9)</u> OLS	<u>2.(10)</u> FE	<u>2.(11)</u> 2-Step GMM	<u>2.(12)</u> 1-Step GMM
<i>Lagged ln(GDPPC)</i>	-0.0055*** (0.0020)	-0.0629*** (0.0067)	-0.0304** (0.0118)	-0.0283** (0.0115)
<i>DEMOC</i>	0.0001 (0.0005)	0.0012* (0.0006)	0.0015 (0.0015)	0.0011 (0.0013)
<i>REG</i>	0.0003 (0.0017)	0.0042* (0.0023)	0.0042 (0.0045)	0.0061 (0.0046)
<i>SM</i>	0.0016** (0.0008)	0.0009 (0.0008)	0.0031* (0.0018)	0.0042*** (0.0015)
<i>LEGAL</i>	0.0043*** (0.0012)	0.0035*** (0.0012)	0.0052*** (0.0020)	0.0055*** (0.0019)
<i>SCHOOLING</i>	0.0017* (0.0009)	0.0013 (0.0024)	0.0139** (0.0067)	0.0123* (0.0065)
<i>Const</i>	0.0127 (0.0132)	0.4528*** (0.0543)	0.0843 (0.0575)	0.0566 (0.0593)
<i>Obs</i>	447	447	447	447
<i>No. of cty</i>	91	91	91	91
<i>Adj. R²</i>	0.130	0.0904		
<i>F-stat</i>	6.142	13.21		
<i>AR(1) p-value</i>			[0.0000]	[0.0000]
<i>AR(2) p-value</i>			[0.279]	[0.329]
<u>Hansen Test</u>				
<i>Test Statistics</i>			21.80	21.80
<i>p-value</i>			[0.241]	[0.241]

Dependent variable is *GDPPC_gr*. 5-year interval panel data is used for estimation. Period dummies are included in all models but not reported. ***, ** and * denote significance levels at 1%, 5% and 10% respectively. For Models 2.(9) and 2.(10), robust standard errors are in parenthesis. For Models 2.(11) and 2.(12), the lagged *ln(GDPPC)* is treated as pre-determined, while other regressors are endogenous. Standard errors are in parenthesis and Windmeijer-corrected, robust for heteroskedasticity and autocorrelation. Maximum 4 lags are used for instruments. Instruments are transformed by orthogonal deviation and collapsed. Hansen test is used for overidentifying restrictions.

4.2.3 Robustness Tests

4.2.3.1 With Additional Covariates

As robustness tests, we further run 8 regressions by adding extra covariates to the baseline specification. Test results are at Table 4.2-3. As in Barro and Sala-i-Martin (1997)'s growth empirics, we consider three additional macroeconomic control variables such as population growth rate (*pop_gr*), government consumption as percentage of GDP (*gcon_gdp*) and the natural log of investment share as percentage of GDP (*ln(invest_gdp)*) respectively in Models 2.(13) – 2.(15). Secondly, we also use a vector of openness measures, including FDI inflows as percentage of GDP (*fdi_gdp*), natural log of total trade as percentage of GDP (*ln(trade_gdp)*) as in Barro and Sala-i-Martin (1997) and Dollar and Kraay (2003). The results are shown in Models 2.(16) – 2.(17). Thirdly, we also consider covariates measuring financial openness and financial development on growth, following the literature arguing that financial development causes growth as extensively surveyed in Levine (2008). We use three recently developed indicators – (i) financial integration index from Lane and Milesi-Ferretti (2006) (*fin_open*); (ii) capital account openness index from Chinn and Ito (2006) (*ca_open*), and (iii) total liquid liabilities as percentage of GDP from Beck, Demirguc-Kunt, and Levine (2009) (*lliab_gdp*). The estimations are shown in Models 2.(18) – 2.(20). Finally, in Model 2.(21), we jointly test several covariates.

We find that even after having additional covariates in the baseline specifications, our results generally survive. Our empirical results remain suggesting that only *LEGAL* and *SM* have direct and statistically significant partial impact on economic growth amongst the four clusters of institutions.

The only exception is Model 2.(17), with *ln(trade_gdp)* incorporated in the baseline model. In this Model, the security of property rights (*LEGAL*) still imposes a direct impact on growth as in other specifications. However, the “market-stabilising” institution (measured by *SM*) no longer plays a significant

role. Moreover, *DEMOC* and *REG* now turn statistically significant at the 10% significance level. The results suggest that after controlling for the degree of openness, democracy and less regulatory burden tend to induce growth directly. As the market is more outward-orientated, institutions that are more growth-promoting are those that gear towards the interests of the foreign investors and those can sustain the linkage between the domestic market and the external environment. Very likely, foreign markets respond more positively to an economy with better security of property rights and less regulations. A democratic society, on the other hand, is likely to be more political stable, thereby becoming more favourable for foreign trade and investment. In contrast, monetary stability, e.g. inflation, may be less relevant. However, we note that such effect of openness cannot be found with regard to financial openness and capital account openness as shown in Models 2.(18) and 2.(19), but only to trade openness.

**Table 4.2-3: Institution and Economic Growth – Baseline Specification with Additional Covariates
(2-Step System GMM Estimations)**

Model	2.(13)	2.(14)	2.(15)	2.(16)	2.(17)	2.(18)	2.(19)	2.(20)	2.(21)
<i>Additional Covariates</i>	pop_gr	gcon_gdp	ln(invest_gdp)	fdi_gdp	ln(trade_gdp)	fin_open	ca_open	lliab_gdp	(13)+(14)+(15)+ (16)+(18)+(19)
<i>DEMOC</i>	-0.0014 (0.0016)	0.0010 (0.0012)	0.0013 (0.0014)	0.0015 (0.0012)	0.0020* (0.0011)	-0.0008 (0.0012)	0.0003 (0.0012)	0.0001 (0.0010)	-0.0009 (0.0011)
<i>REG</i>	0.0044 (0.0043)	0.0018 (0.0040)	0.0063 (0.0045)	0.0024 (0.0049)	0.0080* (0.0041)	0.0068 (0.0047)	0.0024 (0.0050)	0.0023 (0.0040)	0.0038 (0.0045)
<i>SM</i>	0.0044** (0.0019)	0.0040*** (0.0014)	0.0030* (0.0018)	0.0034* (0.0018)	0.0019 (0.0017)	0.0033** (0.0016)	0.0039** (0.0016)	0.0031* (0.0018)	0.0032** (0.0015)
<i>LEGAL</i>	0.0035 (0.0021)	0.0041** (0.0018)	0.0050** (0.0020)	0.0048*** (0.0018)	0.0052** (0.0021)	0.0041* (0.0022)	0.0048** (0.0019)	0.0042** (0.0019)	0.0037** (0.0018)
<i>SCHOOLING</i>	0.0110* (0.0061)	0.0051 (0.0053)	0.0126* (0.0068)	0.0083 (0.0055)	0.0032 (0.0047)	0.0097 (0.0081)	0.0138** (0.0062)	0.0088** (0.0042)	0.0006 (0.0041)
<i>Lagged ln(GDPPC)</i>	-0.0247** (0.0114)	-0.0087 (0.0091)	-0.0375** (0.0160)	-0.0183* (0.0100)	-0.0112 (0.0094)	-0.0169 (0.0156)	-0.0246** (0.0115)	-0.0173** (0.0078)	-0.0054 (0.0098)
<i>Const</i>	0.0915 (0.0662)	-0.0385 (0.0480)	0.0618 (0.0658)	0.0322 (0.0525)	-0.1262 (0.0887)	0.0173 (0.0797)	0.0533 (0.0608)	0.0344 (0.0456)	-0.0407 (0.0507)
<i>Obs/No. of cty</i>	447/91	446/91	446/91	444/91	437/91	436/87	442/91	409/86	429/87
<i>AR(1) p-value</i>	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
<i>AR(2) p-value</i>	[0.225]	[0.832]	[0.466]	[0.465]	[0.525]	[0.663]	[0.293]	[0.420]	[0.522]
<i>Hansen Test Stat</i>	23.72	32.83	13.41	23.95	22.74	22.99	22.07	23.39	34.35
<i>Hansen p-value</i>	[0.307]	[0.135]	[0.767]	[0.295]	[0.302]	[0.289]	[0.281]	[0.221]	[0.356]

Dependent variable is *GDPPC_gr*. 5-year interval panel data is used for estimation. The lagged *ln(GDPPC)* is treated as pre-determined, while other regressors are endogenous. Period dummies and additional covariates are included but not reported. Standard errors are in parenthesis and Windmeijer-corrected, robust for heteroskedasticity and autocorrelation. ***, ** and * denote significance levels at 1%, 5% and 10% respectively. Maximum 4 lags are used for instruments. Instruments are transformed by orthogonal deviation and collapsed. Hansen test is used for overidentifying restrictions.

4.2.3.2 With Alternative Measure of Institution

To test further the sensitivity of our results, we test the baseline specification with alternative measures of institutions in Table 4.2-4. Instead of *DEMOC*, we use the augmented Freedom House's polity index (*IPOLITY2*). Hadenius and Teorell (2005) suggests that averaging the Freedom House's political rights and civil liberties indices works better than simply using the individual one from the original source in terms of validity and reliability. Our Model 2.(22) in Table 4.2-4 shows that this alternative measure of democracy does not change our conclusion at all. Democracy still has no significant direct partial impact on economic growth.

We then substitute *LEGAL* by the composite measure of the quality of government index *QOG* (as defined in Section 4.1) to proxy the security of property rights and contract enforcement provided by the State in our estimation. Since this variable captures more dimensions of institutions, it is no wonder the variable dominates all the significant impact on growth as shown in Models 2.(23) and 2.(24). In these Models, only *QOG* remains statistically significant, possibly reflecting that the direct impact of *QOG* dominates that of other institutional variables.

To separately test “*market-creating*” institutions – property rights and contractual rights – as in previous literature (like in Acemoglu and Johnson (2005)), we also carry out similar tests in Models 2.(25) and 2.(26) using Polity IV's executive constraint (*XCONST*) as a measure of security of property rights. Higher values of *XCONST* mean that there are more constraints on the executives, implying that the risk of expropriation from the State is less likely. Our results shall suggest that “*market-creating*” institutions have a direct positive impact on economic growth.

**Table 4.2-4: Institution and Economic Growth – With Alternative Measures of Institutions
(2-Step System GMM Estimations)**

Model	<u>2.(22)</u>	<u>2.(23)</u>	<u>2.(24)</u>	<u>2.(25)</u>	<u>2.(26)</u>
<i>DEMOC</i>		-0.0002 (0.0015)			
<i>IPOLITY2</i>	0.0023 (0.0016)		0.0005 (0.0023)		
<i>QOG</i>		0.0651*** (0.0212)	0.0642** (0.0245)		
<i>LEGAL</i>	0.0052*** (0.0019)				0.0069*** (0.0022)
<i>XCONST</i>				0.0010*** (0.0002)	0.0007*** (0.0002)
<i>SM</i>	0.0032** (0.0016)	0.0014 (0.0017)	0.0014 (0.0017)		
<i>REG</i>	0.0040 (0.0044)	0.0047 (0.0045)	0.0041 (0.0047)		
<i>SCHOOLING</i>	0.0119** (0.0059)	0.0186*** (0.0050)	0.0178*** (0.0056)	0.0143* (0.0084)	0.0129** (0.0058)
<i>Lagged ln(GDPPC)</i>	-0.0246** (0.0106)	-0.0376*** (0.0100)	-0.0327*** (0.0095)	-0.0185 (0.0158)	-0.0225** (0.0094)
<i>Const</i>	0.0423 (0.0510)	0.1261** (0.0570)	0.0903* (0.0487)	0.0718 (0.0760)	0.0734* (0.0437)
<i>Obs/No. of cty</i>	463/94	391/84	402/86	559/99	469/92
<i>AR(1) p-value</i>	[0.0000]	[0.0000]	[0.0000]	[0.0443]	[0.0000]
<i>AR(2) p-value</i>	[0.298]	[0.132]	[0.235]	[0.283]	[0.376]
<i>Hansen Test Stat</i>	22.03	17.78	20.85	5.068	7.179
<i>Hansen p-value</i>	[0.231]	[0.471]	[0.287]	[0.750]	[0.708]

Dependent variable is *GDPPC_gr*. 5-year interval panel data is used for estimation. The lagged *ln(GDPPC)* is treated as pre-determined, while other regressors are endogenous. Period dummies are included but not reported. Standard errors are in parenthesis and Windmeijer-corrected, robust for heteroskedasticity and autocorrelation. ***, ** and * denote significance levels at 1%, 5% and 10% respectively. Maximum 4 lags are used for instruments. Instruments are transformed by orthogonal deviation and collapsed. Hansen test is used for overidentifying restrictions.

4.3 Developmental features

We now introduce the core structural feature that Lewis identified as central to developing countries – the structure of a dual economy. We use a readily available indicator of duality i.e. the share of manufacturing in aggregate GDP which affects total investment and therefore economic growth in a developing country. We assume that institutions perform better in an economy with higher

levels of manufacturing share since many of the modernisation impact of institutions work faster in the more ‘advanced sector’. Manufacturing often incorporates the latest technology, as well as FDI, and therefore advance growth under adequate regulation, human capital formation as well as macroeconomic stability. We use different sub-samples to test our baseline specification in Table 4.2-5, which includes developing country samples as well as those in sub-Saharan Africa. Since legal institutions are potentially important for the so-called ‘rule of law’, and following the literature on the effect of legal origins on institutions and economic outcomes³, we test the baseline model with non-English common law origin and non-French civil law origin countries in Models 2.(27) and 2.(28). In Models 2.(29) – 2.(32), we further test different sub-samples by income levels. Our general results are largely similar. These models, again, provide empirical evidence that “*market-creating*” institution, measured by the protection of property rights and contract enforcement (**LEGAL**), as well as “*market-stabilising*” institutions, measured by the stability of monetary and fiscal institution (**SM**), are directly conducive to economic growth. **DEMOC**, on the contrary, does not have any direct impact on growth.

One interesting finding is on **REG** – *market-regulating* institution. Using the full sample, we have seen that **REG** does not have any significant direct partial effect on growth, although the sign of the coefficients indicate that a less regulated environment tends to grow faster. However, we find a positive and statistically significant direct impact of **REG** on growth in the sub-sample of non-English common law origins (i.e. Model 2.(27)), non-industrialised countries (i.e. Model 2.(31)) and low income and lower middle income countries (i.e. Model 2.(32)). These results suggest that the negative impact of excess regulatory environment is more obvious in less developed economies. This also provides supporting evidence to the existing literature, like Djankov, *et al.* (2002) and Botero, *et al.* (2004), that there are generally more regulations

³ See La Porta, *et al.* (2008) for a survey on this strand of literature. In gist, this strand of view suggests that legal origin of an economy will directly affect its degree of security of property rights via judicial formalism, bureaucratic quality, the amount of red-tape and regulations, and henceforth, its economic performance. It is found that countries with English common law origin, vis-à-vis those with French civil law origin, generally perform better, with more developed financial markets as well as less corruption.

in less developed economies and they are more likely to be growth-impeding. This probably relates to the bureaucratic quality to execute these regulations. In particular, more red tape may also lead to more corruption which is growth-hampering. However, the case of Sub-Saharan African countries is quite peculiar. We do not find the negative effect of *REG* in this sub-sample. One, nevertheless, should note that the size of the sub-sample pertaining to Sub-Saharan African countries is significantly smaller than the other sub-samples.

The development indicator, share of manufacturing in GDP, also shows interesting impact effect depending on the samples selected.

**Table 4.2-5: Institution and Economic Growth – Sub-Samples Estimations
(2-Step System GMM Estimations)**

Model	<u>2.(11)</u>	<u>2.(27)</u>	<u>2.(28)</u>	<u>2.(29)</u>	<u>2.(30)</u>	<u>2.(31)</u>	<u>2.(32)</u>
Sample	Full	Non-English Common Law Countries[§]	Non-French Civil Law Countries[§]	Non Sub-Sahara African Countries	Sub-Sahara African Countries	Non Industrialised Countries[^]	Lower Middle Income & Low Income Countries[¶]
DEMOC	0.0015 (0.0015)	0.0006 (0.0013)	0.0002 (0.0021)	-0.0000 (0.0010)	0.0017 (0.0026)	-0.0006 (0.0013)	-0.0005 (0.0016)
REG	0.0042 (0.0045)	0.0089* (0.0050)	-0.0064 (0.0069)	0.0026 (0.0039)	0.0135 (0.0107)	0.0117* (0.0062)	0.0210** (0.0092)
SM	0.0031* (0.0018)	0.0030* (0.0018)	0.0065** (0.0027)	0.0023* (0.0013)	0.0035 (0.0045)	0.0030* (0.0017)	0.0035 (0.0035)
LEGAL	0.0052*** (0.0020)	0.0073*** (0.0024)	0.0061* (0.0032)	0.0050*** (0.0018)	0.0098** (0.0046)	0.0072** (0.0030)	0.0090** (0.0039)
SCHOOLING	0.0139* (0.0067)	0.0101* (0.0060)	0.0015 (0.0053)	0.0092** (0.0043)	-0.0064 (0.0107)	0.0131 (0.0088)	-0.0018 (0.0123)
Lagged ln(GDPPC)	-0.0304** (0.0118)	-0.0269*** (0.0096)	-0.0044 (0.0091)	-0.0218** (0.0094)	-0.0021 (0.0176)	-0.0293* (0.0162)	0.0030 (0.0323)
Share of manufacturing	0.0843 (0.0575)	0.0509 (0.0477)	-0.0047 (0.0433)	0.0744*** (0.0329)	-0.1039 (0.0691)	0.0427*** (0.0062)	-0.1700* (0.1166)
Obs/No. of cty	447/91	293/60	213/43	354/70	93/21	326/70	208/46
AR(1) p-value	[0.0000]	[0.0000]	[0.0004]	[0.0000]	[0.0190]	[0.0000]	[0.0000]
AR(2) p-value	[0.279]	[0.478]	[0.978]	[0.136]	[0.604]	[0.583]	[0.526]
Hansen Test Stat	21.80	17.12	18.02	25.92	9.322	18.00	15.55
Hansen Test p-value	[0.241]	[0.194]	[0.115]	[0.169]	[0.952]	[0.456]	[0.342]

Dependent variable is *GDPPC_gr*. 5-year interval panel data is used for estimation. The lagged *ln(GDPPC)* is treated as pre-determined, while other regressors are endogenous. Period dummies are included but not reported. Standard errors are in parenthesis and Windmeijer-corrected, robust for heteroskedasticity and autocorrelation. ***, ** and * denote significance levels at 1%, 5% and 10% respectively. Maximum 4 lags are used for instruments. Instruments are transformed by orthogonal deviation and collapsed. Hansen test is used for overidentifying restrictions.

[§] English common law and French civil law countries are classified based on La Porta, et al. (1997).

[^] Industrialised economies are classified based on Chinn and Ito (2006).

[¶] Classification of lower middle income and low income countries is based on World Bank classification.

5. Conclusion

In this paper, our primary interest is to revisit the empirical evidence of institution and growth in a dynamic panel data model and investigate *if* and *what* institutions matter to growth. The research motivation is driven by the technical limitations of the existing literature. After providing a wide literature survey of the subject, we point out that the existing literature is mostly cross-sectional in nature, which ignores the dynamic and short-run impact of institutions on growth. On the technicalities front, these studies assume that there is no direct growth effect of any institutional change. Estimation results are also subject to endogeneity without using proper and valid instruments. These results are also potentially biased due to omitted variables, like country-specific characteristics which may capture the effect of natural endowments, culture, historical developments and other time-invariant factors alike. Exactly what institutions matter to economic growth is not well defined in previous studies. Therefore, although the institution view suggests a convincing case that institutions are the fundamental sources of growth, empirical results are largely inconclusive.

We employ panel GMM estimator to carry out our empirical investigation. By so doing, we do not have to seek “external” instruments to tackle the weak version of endogeneity problem between institutions and economic growth. In addition, the methodology allows us to control for country-specific characteristics, time effect and initial income level. The results obtained help demonstrate the direct and short-run partial impact of institution on growth.

So after using our suggested methodology, which appears to be technically improved, do institutions matter to growth empirically? It all depends on what institutions we refer to. The general results are positive when we first take a preliminary test of an augmented Solow growth model with institutions incorporated in the specification. In such specification, we show that institutions, as measured by a composite index, do have a direct impact on growth. In contrast to, say human capital, the effect of institution is more statistically significant. We further attempt to test institutions impact on growth via taxonomic clusters. Based on Rodrik(2005)’s taxonomy, we cluster institutions into four dimensions, which essentially measures political institutions – democracy in particular, and economic institutions and policies – security of property rights, stability of monetary and fiscal policies and regulatory environment. This framework brings political institution, economic institution and policies into synergy and allows us to test institutions in a unified manner.

Our empirical results generally show that political institution, i.e. democracy in our case, does not have any direct effect on growth. This result holds even if we use alternative measures of democracy. Secondly, market-creating institutions, measured by the security of property rights as well as constraints on the executives, are directly associated with growth. This is in line with the existing literature. Thirdly, stability of monetary and fiscal policies, namely market-stabilising institutions, is also growth-promoting. Fourthly, market-regulating institutions, which measure the regulatory environment, show no direct growth impact either. It may suggest that whether regulations are growth-impeding is in relation to the stage of development of countries. In developed countries, regulations may help rectify externalities and restore market competitiveness. In the less developed economies, regulations may be growth-hampering due to the executive ability of bureaucrats and the likelihood of corruption thereby induced. Therefore, using a full sample, the impact of regulations is ambiguous. However, using a sub-sample, we find that fewer regulations directly carry a significant growth impact in less developed economies. This suggests that after controlling for the stage of development, the negative impact of regulations on growth is more pronounced. All these empirical results seem to survive after using different specifications with additional covariates and using different sub-samples.

In conclusion, the empirical evidence obtained under a coherent taxonomy demonstrates that economic institution and policies impose direct and partial growth impacts, but political institutions may not. We also provide additional empirical evidence to the institution view that institutions matter to growth, even after controlling for the unobserved time-invariant effects and the possible dynamics of institutional change. We should stress that we have no intention to overrule the endowment view or historical view of growth. In particular, our investigations do not take into account the impact of historical incidence, e.g. wars or civil conflicts on institutional change. Further, the impact of institutions may be non-linear. Our results mainly attempt to address the concerns and critiques of the existing institution view literature. We demonstrate that economic institutions and economic structures matter for economic growth.

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