Impact of Institutions and Policy on Economic Growth: Empirical Evidence[ⱡ]

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We reviewed the empirical evidence for the effect of institutions on per capita GDP growth. Although several of the literature suggested institutions are the primary cause of growth; empirical evidence is somewhat inconclusive. On one hand, it partly reflects that technical limitations - especially heterogeneity - have not been adequately addressed in these studies. On the other hand, there seems to be no consensus on which institutions specifically cause growth. We test the effect of four clusters of institutions (Using Rodrik-2005 Taxonomy) on growth using dynamic GMM panel estimation. The taxonomy provides a functional definition of institutions, allowing us to identify exactly the type of institutions that affect growth. Meantime, our methodology has been technically improved without the need to look for "external" tools to address the problem of homogeneity. More importantly, we can estimate the effects of institutional change (short-term effects of institutions) on growth, without assuming institutional stability.

Key Words: Economic Development, Institutions, Institutions Performance, GMM, Panel Data, Economic Growth,

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[ⱡ] This series is composed of five papers (The new updated version of the papers was created in 2009 -2010), one is an overview and four empirical studies, which investigate the effects of institutions on cross-country economic development from different perspectives drawn from my MPhil thesis of "Essay on Institutions, Policies, and Economic Development". The first paper, entitled "Institutions, Policies, and Economic Growth Overview", reviews the relationship between institutions and policy regulation with economic development from the perspective of economic literature. The second paper, entitled "Impact of Institutions and Policy on Economic Growth: Empirical Evidence", empirical analysis to explore the interaction between the institution and economic development. The third paper, entitled "Role of Political Institutions on Economic Growth: Empirical Evidence", is an empirical analysis to explore the effect of political institutions on economic development. The fourth paper, entitled "Impact of Natural Environment, Regional Integration, and Policies on FDI", explores the effects of three determinants of bilateral FDI, including natural barriers, the "at-the-border" barrier (regional trade agreement), and the "behind-the-border" barrier (domestic regulatory environment). The fifth paper, entitled "Cross Countries Economic Performances - SPF Approach", explores the differences in technical inefficiency (inefficient allocation of production inputs) and explains the diverse cross-country economic performances, using estimating a "global" stochastic production frontier (SPF) mod.

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1 Introduction

This is an empirical analysis to explore the interaction between the institution and economic development, the most direct analysis would be to study the growth impact of institutions. After all, one way of improving society's well-being is through promoting economic growth, thereby narrowing the cross-country income differences.

The proposition that domestic institutions matter to economic growth is not new in the economic development literature. Nevertheless, there are competing views arguing that the role of institutions is too overplayed. Amongst others, critics claim that geographical endowments, culture, religion, and historical events are also key determinants. Other empirical works suggest that human capital is an even more fundamental cause of economic growth. Exactly what institutions matter to economic growth is also very loosely defined. Others criticise that the institution view literature presents evidence in a very long-run timeframe, assuming that institutions are persistent and without considering the effect of institutional change on the economic outcome today.

Although there is a vast amount of empirical evidence to support the institution's view, the estimation strategies are being severely scrutinised. Critics rightly point out that the existing empirical literature is predominantly cross-sectional in nature, thereby ignoring the dynamic impact of the institution on growth. Furthermore, they do not control for the 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 problems, the validity of instruments has called for many doubts. Last but not least, data quality and the definition of an institution are also subject to many 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 the institution on economic growth without seeking "external" instruments, yet taking 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. To recap, institutions refer to those that sustain the market operations of the economy under his taxonomy. 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 market operation. As noted, this way of clustering brings three frequently, yet separately tested, dimensions of institutions – economic institution, political institution and policy – into synergy.

Against the background of all 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. That said, we do not assume institutions persist. Third, we use Rodrik's (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 institution on growth.

All in all, we essentially try to answer do and what institutions cause growth. Our estimation results reflect the direct and short-run impacts of institutions on economic growth. The questions of whether they affect other dimensions of economic development will be further investigated in the coming papers of this series, (entitled "Role of Political Institutions on Economic Growth: Empirical Evidence", which is an empirical analysis to explore the effect of political institutions on economic development).

This paper is organised as the following. We will present a brief literature survey in Section 2. Our estimation technique and data will be discussed in Section 3. The empirical results follow in Section 4. We conclude in Section 5.

2 A Literature Survey of the Institution and the 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 their empirical shortcomings. We will first compare three strands of views of cross-country economic development, namely the endowment view, institution view and a combination of the two. We then proceed to the empirical results of testing the institution view with specific reference to economic and political institutions. Based on the literature survey, we summarise 6 key technical shortcomings of these empirics intending to provide justifications for the estimation we perform in the present paper.

2.1 Glances View on the Economic Growth

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. However, this institutional view of growth invites a major debate with the endowment view. The latter essentially argues that geographical factors directly shape the output, income distribution or technology adoption in society. A third strand of the literature, hereafter developed, combines the essence of the institution and endowment views on growth. This strand opines that economic growth is not directly determined by natural factor endowment, but it will shape the policy and institutional choices of politicians and/or colonizers. These choices in turn have a positive and long-term effect on economic prosperity. This third strand largely dominates the empirical interests in the literature. We will discuss briefly these three competing and complementing views below.

2.1.1 The Direct Geography Impact

The endowment view claims that natural resources to the population determine the productivity and technology in production. Earlier works like Engerman and Sokoloff (1997) provide a historical account and qualitatively argue that the composition of the population, climate, soils and native populations in the United States and Canada significantly explained the relative distribution of wealth, human capital and the decentralized political power developed as compared to the development experiences of Latin America economies. Latin American economies, as they argue, enjoyed a climate and soil conditions that were extremely well suited for growing crops. Their populations then quickly generated vastly unequal distributions of wealth, human capital, and political power. The extensive native populations in the regions colonized by the Spanish were powerful factors leading to extreme inequality. In contrast, small, family-sized farms were the rule in the northern colonies of the North American mainland, where climatic conditions favoured a regime of mixed farming focused on grains and livestock that exhibited quite limited economies of scale in production and used few slaves. These regions do not appear to have been very attractive to Europeans during the first quarter of a millennium after they began to colonize the New World, since only a small fraction of the migrants to the New World opted to locate there. However, the circumstances fostered relatively homogeneous populations with relatively equal distributions of human capital and wealth.

Sachs and Warner (2001) postulate the "curse of natural resources", i.e. countries with a great wealth of natural resources tend to grow more slowly than resource-poor countries. Their ordinary least square (OLS) estimations suggest that the average GDP per capita growth rate during 1970-1990 is negatively associated with the natural resource abundance variables. They explain that this curse may be due to the fact that resource-abundant countries are high-price economies and, thus, they tend to miss out on the opportunities for export-led growth. Arezki and Ploeg (2007) provide more recent empirical evidence to support Sachs and Warner's hypothesis. Understandably, institutions, openness and income growth could be endogenously determined. Therefore, the authors use cross-country instrumental variable (IV) estimations to correct for the endogenous nature of institutions and openness[1], they find a significant negative direct impact of natural resources on income per capita, as well as an indirect effect of natural resources on institutions. In particular, the natural resource curse is particularly severe for economic performance in countries with a low degree of trade openness.

Other than natural resource endowment, the disease burden provides another explanation for poor economic performance. Diamond (1997) and Gallup, Sachs, and Mellinger (1998) share similar views that adverse geographical conditions hamper agricultural productivity and habitants' health. Gallup, et al. (1998), one of the earliest empirical works supporting the endowment view, investigates in a way how geography may matter directly for growth. Their OLS empirical results show that the location and climate of the economy have large direct effects on income levels and income growth through the effects on transport costs, disease burdens and agricultural productivity. The results survive even after controlling for the quality of public institutions[2].

They also find that poor regions are typically characterised by locations far from the coast, implying that they face large transport costs for international trade, as well as populations in tropical regions of high disease burden. Comparing different regions, Sachs (2000) similarly find that tropical areas have a higher infectious disease burden than temperate zones, leading to greater economic prosperity in the latter.

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[1] They incorporate the rule of law index from Kaufmann and Kraay (2002) to measure institution and de facto openness (i.e. total trade as a percentage of GDP) in their estimations which suffer from endogeneity bias. Hence, the instrument regresses with a combination of UK legal origin, log of settler mortality, fraction of population speaking English, and a bilateral gravity estimate of openness. Literature relating to the use of these instruments will be discussed further in the coming Section. The use of IV estimation helps tackle the problem of endogeneity. If ignored, the least square estimators are biased and inconsistent estimators.

[2] The quality of public institutions is based on an index created by Knack and Keefer (1995), which is an average of five indicators, including (a) the perceived efficiency of the government bureaucracy, (b) the extent of government corruption, (c) efficacy of the rule of law, (d) the presence or absence of expropriation risk, and (e) the perceived risk of repudiation of contracts by the government.

Bloom and Sachs (1998) suggest that the prevalence of malaria also partially explains the poor economic performances in Sub-Saharan countries.

Inevitably, the endowment view is not free from counter-arguments. Olson (1996) argues that natural resources do not account for diverse cross-country economic performances over time since most economic activities today are separated from raw materials and arable land. Technological improvement and sectoral change (a greater proportion of manufacturing and service industries) are no longer resource-dependent. Acemoglu, et al. (2005) on the other hand, by providing a real-life natural experiment – North and South Korea, strongly overrule the importance of geography. In terms of geographical features, both regions are extremely similar in almost all perspectives, not to mention their cultural heritage. The striking diverge economic performances of the two parts, as they argue, are due to political and economic institutions much more than any other possible aspects of economic development.

2.1.2 The Indirect Geography Impact via the Institutional Choice

Built on the endowment view, another strand of literature argues that geographical conditions do not directly affect growth, but endowments determine the institutional structure brought by colonizers and policies chosen by politicians. Hall and Jones (1999) is probably the earliest core empirical paper to consider the impact of "social infrastructure"[3] on cross-country output per capita. The authors explicitly link such understanding with North's criteria of institution – i.e. institutions that bring social returns of private actions as private returns. They find that differences in social infrastructure are largely associated with cross-country differences in capital accumulation, educational attainment and productivity. This in turn explains the cross-country difference in output per worker. While they acknowledge that social infrastructure and output per worker are endogenously determined, IV estimation is used. The authors propose to instrument social infrastructure by geographical and historical factors of "Western influence". The underlying hypothesis is that the importance of property rights and systems of checks and balances in government came from Western Europe. Western influences via earlier settlers' institutional transplantation thus determine the development of social infrastructure.

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[3] As they recognise, "social infrastructure" essentially refers to an environment that supports productive activities, capital accumulation, skill acquisition, invention and technology transfer. In practice, social infrastructure is estimated by two indices in their work: (1) the average score of (a) law and order; (b) bureaucratic quality; (c) risk of expropriation; (d) government repudiation of contracts, and (e) corruption from the International Country Risk Guide (ICRG) of Political Risk Service, and (2) openness to international trade based on Sachs and Warner (1995).

Western Europeans were also more likely to settle in areas further from the equator where the climate is similar to Western Europe. As instruments, therefore, the geographical distance from the equator and the fraction of the country's population speaking one of the five Western European languages are likely to be correlated with social infrastructure.

Referring to the effect of European diseases in the New World after 1492 and local disease environments on the colonization strategies of Europeans around the world from 1500 to 1900, Acemoglu, Johnson, and Robinson (2003) on the other hand claim that health conditions and disease environments do not have a first-order effect on income, but instead on institutions. Disease environments play an important role in shaping institutional development, thereby the path of economic development. The authors hypothesise that when two previously isolated populations come into contact, disease environments influence the balance of power between these populations and the type of institutions of the more powerful imposes the institutional structure on the less powerful.

Based on this institutional transplantation argument, Acemoglu, Johnson, and Robinson (2001) propose innovative "instruments" for the institution and growth empirics. Since IV estimation is so often used to tackle the problem of endogeneity between the institution and economic growth, these proposed "instruments" naturally are so frequently used in nearly all empirical works on the subject that come after. In essence, the authors claim that an unfavourable climate induced a higher rate of European settlers' mortality, which discouraged them to imbed good institutions (i.e. protecting against expropriation risk) in that colony. Under such circumstances, they were more likely to develop a predatory state without introducing good institutions for economic growth. These institutions persisted to the present, thereby affecting present economic performance. In other words, physical geography *per se* hardly explains the growth directly, but it affects the quality of the institution transplanted. In their 2SLS estimations, they regress current economic performance with the current institution, with the latter instrumented by settlers' mortality rate. They conclude that once the effect of institutions is controlled, countries in Africa or those closer to the equator do not have lower incomes due to poor institutions.

McArthur and Sachs (2001), however, use a wider sample of countries to refute the idea that geography determines institutional choice. They find that both geography and institutions matter for growth. Their major criticism is that it is hard to see why the determination of institutions strongly relates to physical geography. Glaeser, Porta, Lopez-de-Silanes, et al. (2004) also claim that one should not overestimate the effect of geography on economic development via institution. According to their empirical studies, a more basic source of growth may be simply human capital only, rather than institutional quality. They provide an alternative explanation to Acemoglu, et al.'s (2001)'s results by arguing that geographical conditions do not necessarily relate to the quality of institutions with which colonizers brought, but rather the conditions determined if the Europeans stayed in the colonies as a form of human capital which drove growth. More recently, Albouy (2008) also casts doubt on Acemoglu, et al.'s (2001) settlers' mortality rate data. He finds that 36 of the 64 countries in the sample are assigned mortality rates from other countries, typically based on mistaken or conflicting evidence. Incomparable mortality rates from populations of labourers, bishops, and soldiers are combined in a manner favouring the hypothesis. After controlling for these data issues, he realises that the relationship between mortality and expropriation risk lacks robustness, and IV estimates become unreliable.

More recently, Nunn (2009) suggests a historical view that the largest effects of geography on current economic development may work through its influence on past events (i.e. history) rather than through its direct effect on economic outcomes today. In his view, the institution is the channel through which history matters to economic development. He does not postulate that geography determines institutional choice. Instead, institutions together with culture, knowledge and technology determine the movements of the economy between multiple equilibria of the state of development. All these views together weaken the causal direct relationship between geography and institutions.

2.2 The Institution View: The Economic vs. the Political Institutions

The institution view essentially argues that the institution is the fundamental source of growth. Empirical studies have been developed into two separate strands, namely the effect of economic and political institutions. We highlight a few core empirical studies below.

2.2.1 The Growth and Economic Institutions

As we have discussed before, Acemoglu, Johnson, and Robinson (2002) suggest that economic institutions were developed by colonial transplantation and thus refute the endowment view completely. Their key observation of the "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 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 was 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 do not affect GDP per capita today, other than their effects through institutional development that subsequently affects the growth rates today.

La Porta, Lopez-de-Silanes, Shleifer, et al. (1997) and La Porta, Lopez-de-Silanes, Shleifer, et al. (1998) examine specifically the importance of colonial rule with financial development as a result of 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 shows that legal origin is also correlated with a host of other country characteristics, including the labour market (Botero, Djankov, La Porta, 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 a problem is also mentioned by the authors themselves in La Porta, Lopez-De-Silanes, and Shleifer (2008).

More recently, Acemoglu and Johnson (2005) attempted to unbundle which institutions affect economic development. They define institutions in 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 the risk of expropriation” and “constraints on executives” from the Polity IV dataset respectively. Using settler mortality rates 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.

2.2.2 Growth and Political Institution

Empirical results on the positive impacts of the economic institution on growth are not particularly controversial despite all the diversities of views aforementioned. However, the effect of political institutions on economic growth, and especially political regimes 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 a '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.

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

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. They propose a theoretical framework to suggest that there may exist an "optimal size of the State", in other words, either political regime implies it is more efficient than the others. An altruistic dictator may be better at mobilizing savings, while democracies are better at allocating investment. Democracy can potentially be positive for economic development because of the competition amongst interest groups.

The form of democracy also matters for the adoption of structural policies that promote growth. Employing 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. Roll and Talbott (2002) and Persson (2005) investigate 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 a 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 the necessity of choosing between investment for the future and immediate consumption. It may also result that resources being wasted for interest groups for lobbying (Becker (1983)). In short, whether democracy has a direct growth impact is yet conclusive.

Glaeser, et al. (2004) revisit the question of whether political institution causes economic growth, or whether growth and human capital accumulation lead to improvements in political institutions. They criticise that most of the political institution variables are not suitable for quantitative analysis. OLS and IV techniques are biased and mostly flawed. They discuss three sets of commonly used variables measuring political institutions. The first is the survey indicators of institutional quality from the International Country Risk Guide (ICRG). The second set is an aggregated index of survey assessments of government effectiveness from Kaufmann, Kraay and Mastruzzi (2005). The third is the Polity IV dataset which aims directly at measuring the limits of executive power. Glaeser, et al. (2004) criticise that the three datasets measure potential outcomes (e.g. bureaucratic quality or government effectiveness), but not some permanent characteristics of the political environment, i.e. the constraints imposed on the governments. From their point of view, the first two sets of political institutions variables are constructed in a way that dictators freely choose good policies to receive as high evaluations as governments are constrained to choose them. The Polity IV variables, in their view, are the only acceptable yet imperfect measure. The Polity IV variables are intended to focus on political constraints, but they too reflect political outcomes rather than durable constraints. In any case, their OLS results using the Polity IV indicators suggest that human capital is a more fundamental source of growth than political institutions are.

In tackling the problem of endogeneity, Rigobon and Rodrik (2004) suggest that the search for exogenous instrumental variables is truly difficult to justify. In fact, existing instruments for institutional quality are correlated strongly with geographical variables and with human capital. It thus raises interpretational questions about what is being identified. To estimate the interrelationships among economic institutions, political institutions, openness, and income levels, they deploy the strategy of identification through heteroskedasticity (IH) as developed in Rigobon (2003). The main merit of this approach is no need to find proper instruments. Instead, they exploit the difference in the structural variances in two sub-samples (including (i) colonies versus non-colonies; and (ii) continents aligned on an East-West versus those aligned on a North-South axis) to gain identification. The authors reassess the competing views of economic and political institutions and trade openness on economic growth. Their empirical evidence suggests that democracy and the rule of law are both good for economic performance, with the latter having a much stronger impact on incomes. Openness (measured by trade share of GDP) has a negative impact on income levels and democracy, but a positive effect on rule of law. Reciprocally, higher income produces greater openness and better institutions, but these effects are not very strong. Rule of law and democracy tend to be mutually reinforcing.

2.3 The Empirical Results of the Competing Views Testing

Against these competing hypothesises, Easterly and Levine (2003) systematically test three different views together – endowment view, institution view and effect of macroeconomic policy (called policy view for simplicity) – using cross-country data. Their OLS estimates reject the endowment view that settler mortality rates, latitude, whether the area is landlocked and crops/minerals productions, do not have any direct impact on the real GDP per capita in 1995 nor on other policies variables (measured as trade openness, inflation, and real exchange rate overvaluation) after controlling for institutions, legal origins, religions composition or ethnolinguistic diversity. Nevertheless, geography does associate with institutions. Using IV estimations, their empirical results show strong positive impacts of endowments on institutional development but not just on GDP per capita. Even more, macroeconomic policies do not help explain economic development after accounting for the impact of institutions.

Comparing similar competing views, Rodrik, Subramanian, and Trebbi (2004) use cross-sectional data to estimate the respective contributions of institutions, geography, and trade to cross-country income levels using IV estimations. Their key results suggest that the quality of institutions "trumps" everything else. They show that once institutions are controlled for, geography only has an indirect effect by influencing the quality of institutions. Similarly, trade is almost always insignificant. These results still stand in a series of robustness checks after using different measures of geographical variables and instruments. Sachs (2003), on the contrary, comments that IV estimates of such kind are biased. He insists that since the initial settler mortality rate has a direct and persistent effect on income today, the exclusion restriction of instruments – the geographical characteristics – is thus not satisfied. In his follow-up work, he proposes that malaria risk, which is strongly affected by ecological conditions, directly affects the level of per capita income after controlling for the quality of institutions. The quality of an institution, measured as the average risk of expropriation, however, shows no direct effect on income.

Inconclusive empirical evidence also arises from the use of different measures of geography and institutions. Presbitero (2006) argues that the institution's view is not as strong as it may appear. His empirical work shows that geographical factors related to the health and sanitary conditions and the accessibility to the sea of a country indeed play a more direct role in economic development. Kangur (2008) suggest that existing empirical results are not robust to alternative measures of institutional quality and/or respective instruments. In particular, the settler mortality rate proposed by Acemoglu et al. (2001) is not a dominant instrument for institutional quality, which is potentially prone to fail to satisfy the exclusion restriction. He concludes that, in comparison, human capital and geography appear to come out as winners amongst all these determinants of economic development.

2.4 Growth Literature and Critics of The Existing Institutions

Although the claim that institutions matter to growth has come to an academic conclusion, empirical evidence remains 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 intending to provide justifications for the estimation strategy employed in this study. These problems generally concern the data and methodology.

2.4.1 Data Problem: Availability, Credibility and Comparability

The lack of consensus on 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. Most of the variables used in the empirical studies are not specifically developed or derived from theoretical models. In most 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-category 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 possible 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 cross-referenced from similar sources significantly. 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 institutions. This also explains why most of the existing literature is cross-sectional in nature.

2.4.2 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 the institution and economic growth have not paid enough attention to the possibility that economic growth may lead to a better institution. They hypothesise that the poorer the country, and the longer they 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, and 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 of 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.

2.4.3 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-varying factors, e.g. geography (distance from the 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 of the latter referring to expropriation risk, rule of law, or average quality of the institution. Pande and Udry (2006) also wonder about the small number of instruments developed so far despite the huge amount of empirical studies on the subject.

2.4.4 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 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.

2.4.5 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 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)).

2.4.6 Specification Problem: Growth Rates vs. Income Level

Sachs (2003) criticised the institution view that empirical studies of institutions and growth suffer from specification problems, 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:

$$\dot{g} = \alpha + \beta \ln y_{\iota T}^{\cdot} + \gamma I_{\iota} + \delta X_{\iota} + \varepsilon_{\iota} \tag{1}$$

Where $\dot{g}$ is the average growth rate of GDP per capita, y, over a period, and 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. *I* is a vector of institutional variables and ***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).

Against all these limitations in the literature, we propose to use the 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 the 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 detail.

3 The Empirical Strategy and Data

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

3.1 The Baseline Specification

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

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

$\ln \dot{y}_{it}$ - $\ln y_{it-1}$ is the growth rate of GDP per capita of economy *i* for period *t*. I_{it} is a vector of institutional variables. X_{it} and social variables. u_i is the country-specific effect that 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_t is a period-specific dummy to capture structural changes that are common to all countries. v_{it} is an idiosyncratic error in the model.

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, …, 2000-04).

3.2 Estimation Strategy

Traditional growth regressions like the one in eq. (1) generally carry the problems like 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 country-specific and time-specific effects. Islam (1995) is among the first to adopt 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, and ethnic or language fragmentation. Nevertheless, as discussed earlier, researchers recently 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 the 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 an approach is further used in growth regressions by Caselli, Esquivel, and Lefort (1996), Benhabib and Spiegel (1997), Benhabib and Spiegel (2000), Easterly, Loayza, and Montiel (1997), Forbes (2000) and Levine, Loayza, and Beck (2000) among others.

3.1.1 An Estimator of The Linear GMM

In the following, we will briefly discuss the linear GMM estimator and its use in a dynamic panel data model. The discussion below relies heavily on Roodman (2009). The basic set up is defined as:

$$y = x' \beta + \varepsilon \tag{3}$$

where β is a column vector of coefficients to be estimated in the model. y and ε are random variables and x is a column vector of k regressors. Let's assume there exists a z column vector of j instruments, where x and z may share the same elements with $j > k$ and $corr($ z, x$) \neq 0$. Let X, Y, Z and E represent N observations for x, y, z and ε. $\hat{E}$ is the empirical residual given by an estimate $\hat{\beta}$. Since all the instruments are theoretically orthogonal to the error term (i.e. $\hat{E}[z\varepsilon] = 0)$), our aim is to obtain an estimate that $\frac{1}{N} Z' \hat{E}$ converges to zero or is minimized. Also, since given $j > k$, the system is over-identified. Let A be a matrix with positive semidefinite quadratic form and weights moments, such that a

$$\frac{1}{N} Z' \hat{E} \ \|_A = \ N \left(\frac{1}{N} Z' \hat{E} \right) A \left(\frac{1}{N} Z' \hat{E} \right) = \ \frac{1}{N} \ \hat{E}' ZAZ' \hat{E} \tag{4}$$

The consistent linear GMM estimator $\hat{\beta}_A$ solves the problem of argument $\hat{\beta}\, Z' \hat{E} \ \|_A$ This suggests that

$$\hat{\beta}_A = (X' Z A Z' X)^{-1} X' Z A Z' Y \tag{5}$$

In other words, the consistent GMM estimator hinges heavily on the assumption that the instruments are orthogonal to the errors. Infinite sample, nevertheless, the instruments are often at least slightly correlated with the endogenous components of the instrumented regressors. This suggests that this estimator is biased.

For efficiency, A must weigh moments in inverse proportion to their variances and covariances. This indicates a possible efficient GMM (EGMM) moment weighting matrix as:

$$A_{EGMM} = var[z\varepsilon]^{-1} = \underset{N\to\infty}{plim}\, N\, var \left[\frac{1}{N} Z' E\right]^{-1} \tag{6}$$

The EGMM estimator can then be expressed as,

$$\hat{\beta}_{EGMM} = (X' Z\, var[z\varepsilon]^{-1} Z' X)^{-1} X' Z\, var[z\varepsilon]^{-1} Z' Y \tag{7}$$

To turn the EGMM estimator feasible, we have to make some initial assumptions relating to $var[z\varepsilon]$, which can be expressed as

$$var[z\varepsilon] = \underset{N\to\infty}{plim}\, N\, var\left[\frac{1}{N} Z' E\right] = \underset{N\to\infty}{plim}\, N\, E\left[\frac{1}{N^2} Z' E E' Z\right]$$
$$= \underset{N\to\infty}{plim} \frac{1}{N} E\, [Z' \Omega Z] \tag{8}$$

Given eq.(8), A_{EGMM} could then be understood as $(\mathbf{Z'HZ})^{-1}$ as N increases, where **H** is an " estimate" of $\mathbf{\Omega}$.

This suggests in practical terms, we can first perform an initial GMM regression with a reasonable and arbitrary H, such as homoskedasticity (i.e. **H** = $\delta^2 I$), to yield $\widehat{\beta_1}$ (also known as a one-step GMM estimator). We can then obtain the residual's to construct a $\hat{\Omega}_{\widehat{\beta_1}}$ Rerun the GMM estimation by setting $A = (Z' \hat{\Omega}_{\widehat{\beta_1}} Z)^{-1}$ could bring the two-step GMM estimator , $\widehat{\beta}_{1_{EGMM}}$ which is the feasible and efficient GMM estimator. This two-step estimator, the one we will use for our estimation, is efficient and robust to any pattern of heteroskedasticity and autocorrelation.

Although the choice of $\boldsymbol{H}$, even if wrongly assumed, does not render inconsistent parameter estimates, the variance estimate for the parameters will be inconsistent as **Z′HZ** is not a consistent estimate of $var[z\varepsilon]$. Arellano and Bond (1991) point out that such linear GMM estimates can produce standard errors that are downward biased when j is large.

Henceforth, Windmeijer (2005) devises a small-sample correction for the two-step standard errors. He finds that two-step EGMM performs somewhat better than one-step in estimating coefficients, with lower bias and standard errors. With his correction, the two-step standard errors are quite accurate and it seems modestly superior to the cluster-robust one-step ones.

A critical post-GMM estimation test is the test of over-identification. If the model is exactly identified (i.e. $\boldsymbol{j} = \boldsymbol{k}$), the estimator will choose $\widehat{\beta}$ such that $Z' \widehat{E} = 0$ even if $\hat{E}[z\varepsilon] = 0$. The Sargan/Hansen test can be carried out to provide test statistics for the test of over-identification (i.e. a joint validity test of the moment conditions). Under the null hypothesis, the vector of $\left[\frac{1}{N} Z'\widehat{E}\right]$ is randomly distributed around zero. A Wald test renders the statistic for the null to hold is

$$\left[\frac{1}{N} Z'\widehat{E}\right] var[z\varepsilon]^{-1} \left[\frac{1}{N} Z'\widehat{E}\right] = \frac{1}{N} \left(Z'\widehat{E} \right) A_{EGMM}\, Z'\widehat{E} \quad \sim X^2 \; with\; d.f. = j - k$$

If $\boldsymbol{\Omega}$ is scale, then $A_{EGMM} = (Z' Z)^{-1}$

Hansen (1982)'s J-test coincides with the Sargan (1958) test. In contrast, if $\boldsymbol{\Omega}$ is not scalar. Sargan's statistics would be inconsistent. Since is unknown to us, we rely on the Hansen test for over-identification. If the null hypothesis cannot be rejected, it suggests the over-identification criterion is satisfied and the instruments used are valid.

3.1.2 Data Models of the Dynamic Panel

Let us reiterate the data-generating process of a dynamic panel model as

$$y_{it} = \alpha\, y_{it-1} + x'_{it}\,\beta + u_i + v_{it} \tag{9}$$

Where $E\,[\,u_i\,] = E\,[\,v_{it}\,] = E\,[\,u_i\, v_{it}] = 0\ and\ \varepsilon_{it} = u_i + v_{it}$. Alternatively, eq. (9) can be written as

$$\Delta\, y_{it} = (\,\alpha - 1\,)\, y_{it-1} + x'_{it}\,\beta + \varepsilon_{it} \tag{10}$$

We take the first difference of (10) to remove the fixed effects that give us

$$\Delta\, y_{it} = \alpha\,\Delta\, y_{it-1} + \Delta\, x'_{it}\,\beta + \Delta\, v_{it} \tag{11}$$

Nickell (1981) points out that eq. (11) is prone to "dynamic panel bias" since y_{it-1} is still correlated with the disturbance terms after transformation. Such transformation also poses two problems. First, the y_{it-1} term is correlated with. Any predetermined variables in x that are not strictly exogenous become potentially endogenous as they are correlated to $v_{it-1.}$ However, Arellano and Bond (1991) suggest the use of lagged levels dated (t – 2) of the dependent variable and earlier can be used as instruments for the equations in first differences.

GMM estimators control for endogeneity by using "internal instruments", that is, instruments based on lagged values of the explanatory variables. These models do not allow us to control for full endogeneity but are a weak version of it. Thus, it is assumed hereby that the explanatory variables are only "weakly exogenous", which means that they can be affected by current and past realisations of institutions but must be uncorrelated with future unanticipated shocks to institutions (the error term). This yields a consistent estimator of α as $\boldsymbol{N} \rightarrow \infty$ with $\boldsymbol{T}$ is fixed By assuming that the transient errors are serially uncorrelated, i.e. $E\,[\,v_{it}v_{is}\,] = 0$ where $\boldsymbol{s \neq t}$ then the initial condition $y_{i1}\ are$ predetermined, i. e $E\,[\,y_{i1}v_{is}\,] = 0$ and the explanatory variables x_{it} are weakly exogenous. These assumptions imply the moment restrictions to be

$$E\left[\,y_{i,t-s}\,\Delta\, v_{it}\right] = 0\ \ for\ \ t = 3, \dots\dots, T\ \ and\ \ s \geq 2$$

$$E\left[\,x_{i,t-s}\,\Delta\, v_{it}\right] = 0\ \ for\ \ t = 3, \dots\dots, T\ \ and\ \ s \geq 2 \tag{12}$$

Such estimates will no longer be biased by any omitted time-invariant variables. By instrumenting the regressors in the first-differenced equations using levels of the second lags or more, it allows parameters to be estimated consistently. Bond, Hoeffler, and Temple (2001) show that the instruments used in this setting potentially allow consistent estimation even in the presence of measurement error – a common critique of the data of GDP and institutions.

Nevertheless, Blundell and Bond (1998) show that this "differenced GMM" estimator may be subject to a large downward finite-sample bias, especially when the number of periods is small. They showed that when the explanatory variables are persistent over time (like institutions in this case), lagged levels of the dependent variable are weak instruments in the first differences. In these cases, severe problems of identification can lead to bias and could result in a poorly performing differenced estimator. To obtain a linear GMM estimator better suited to estimate autoregressive models with persistent panel data, they consider the additional stationarity assumption that

$$E\,[\,u_i\ \Delta\, y_{i2}] \;=\; 0\ \ and\ \ E\,[\,u_i\ \Delta\, x_{i2}] \;=\; 0\ \ for\ \ i = 1, \ldots., N \qquad (13)$$

This assumption requires a stationarity restriction on the initial conditions y_{i1}. This condition holds if the means of the y_{i1} series, whilst differing across individuals, are constant through periods 1, 2, …, *T* for each individual. This assumption yields *T – 2* further linear moment conditions

$$E\,[\,\varepsilon_{it}\ \Delta\, y_{it-1}] \;=\; 0\quad for\ \ i = 1, \ldots\ldots, N\ \ and\ \ t = 3, 4, \ldots., T \qquad (14)$$

Because

$$E\,[\,\varepsilon_{it}\ \Delta\, y_{it-1}] \;=\; E\,[\,\Delta\, y_{it-1}\ \mu_i] + \;E\,[\,\Delta\, y_{it-1} v_{it}] - \;E\,[\,\Delta\, y_{it-2} v_{it}]$$

These allow the use of lagged first differences of the series as instruments for equations in levels, as suggested by Arellano and Bover (1995). This estimator with an additional moment condition is commonly known as the "system GMM" estimator. In other words, Blundell and Bond (1998) instrument levels with differences whereas Arellano and Bond (1991) instrument differences with levels.

Secondly, eq. (11) magnifies gaps in unbalanced panels. Taking the first difference may result in some y_{it} being missing. As such, Arellano and Bover (1995) propose "orthogonal deviations". This suggests contemporaneous variables being subtracted from the average of all future available observations to minimise data loss. An added advantage of orthogonal deviation is that lagged observations in this transformation would be valid instruments[5]. As T increases, the number of instruments proliferates. The instrument for the transformed $y_{it-1}\ is\ \ y_{it-2^2}$ naturally[6].

[5] Orthogonal deviation, however, makes successive errors correlated (i.e. correlates with Δ). Roodman (2009) suggests that, in practice, it is not usual to assume homoskedasticity in applying these estimators. Therefore, this property does not seriously matter.

[6] It is only valid if the model does not have second-order autocorrelation. Otherwise, the third lags and longer should be used.

To improve efficiency, Holtz-Eakin, Newey, and Rosen (1988) suggests building a set of instruments from the second lag of the dependent variable for each time period and substituting zeros for missing observations. Alternatively, the instrument set can be collapsed, as in our estimation, into a single column like

$$\begin{vmatrix} 0 \\ y_{i1} \\ . \\ . \\ . \\ y_{iT-2} \end{vmatrix}$$

When estimating the dynamic panel model, Roodman (2009) recommends that lags 2 and up of the endogenous variables can be used as instruments in practice. One lag is valid for predetermined but not strictly exogenous variables. In addition, time dummies should be included to remove universal time-related shocks from the errors.

3.1.3 Test for The Post-estimation

The Sargan/Hansen test as discussed above for joint validity of the instruments can again be used. Additionally, Arellano and Bond (1991) develop an autocorrelation test for the idiosyncratic disturbance term v_{it} to test whether the lags are valid instruments. Since $\Delta\, v_{it}$ is mathematically related to $\Delta\, v_{it-1}$, negative first-order serial correlation is expected. To check for first-order serial correlation in levels, it is needed to look for whether the second-order correction indifference exists (i.e. the correlation between $\Delta\, v_{it}$ and v_{it-2} in $\Delta\, v_{it-2}$). A rejection of second-order autocorrelation indicates no autocorrelation of the disturbance term in level. In other words, to test the validity of our preferred specification, we are looking for an AR(1) test that rejects the null hypothesis but an AR(2) test that cannot reject the null hypothesis.

3.1.4 Advantages of Using Panel GMM Estimators for Growth Empirics

Bond, et al. (2001) recommend 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, and 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 that 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)[7]. 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.

3.2 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 as set out in Appendix Section A.2. 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)[8].

Our measures of different institutional variables primarily come from the Fraser Institute’s Economic Freedom of the World Report (Gwartney, Lawson, and Norton (2008)) and the Polity IV project dataset (Marshall and Jaggers (2009)). Detailed data description and descriptive statistics are in Appendix Sections A.1.

7 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.

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

As explained in paper 1 of this series "Role of Political Institutions on Economic Growth: Empirical Evidence", the Fraser Institute's Economic Freedom Index, to the best of our knowledge, is the only available source that covers some kinds of measures of an 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 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 a robustness check for our results.

Specifically, in response to Rodrik (2000)'s 4-cluster taxonomy of market-sustaining institutions, 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*" [9]. For "*market-regulating institution*", we use the composite index of the 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 exist any mechanisms for correcting market failures. Nevertheless, we consider these two proxies sufficient in a macroeconomic growth model as in our case. It goes beyond the purpose of our estimation here to test the impact of micro-level regulation on economic growth.

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, a 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

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9 Other than the empirical literature mentioned in Section 2.2, market-creating institutions are often tested in the context of economic liberties and economic freedom using indicators from Gwartney, et al. (2008) or Heritage Foundation (2009). Among others, Scully and Slottje (1991), De Vanssay and Spindler (1994), Gwartnet, Lawson, and Block (1996), Isham,

(***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***). A 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 the sensitivity of our results. We will discuss these alternative measures when we present our robustness tests in Section 2.3.

The pairwise correlation matrix between key variables, as shown in Appendix Section A.1, suggests that the institutional variables do not strongly correlate with each other. It thus appears that multicollinearity is not a major problem in our case.

4 Results and Discussion

4.1 The institution in The Growth Model: Preliminary Test

Sachs (2003) criticises the empirical studies of institutions' 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 Jone's (1999)'s baseline specification, where output per worker solely depends on "social infrastructure". We modify their model in a panel set in response to Sachs (2003)'s criticism as follows:

Kaufmann, and Pritchett (1997), Easton and Walker (1997) and Strum and Haan (2001) show that economic freedom (in general) is significantly related to a country's growth performance (conventionally measured by GDP per capita and/or GDP growth rate). Torstensson (1994) employs the sub-index rather than the overall score of economic freedom. He finds that, among all the 23 areas measured, the degree of state ownership does not seem to affect growth rates whereas property rights structure matters more. Moreover, some studies also attempt to interpret that it has an indirect effect. Ayal and Karras (1998) show that aggregate economic freedom enhances growth both via increasing total factor productivity and via enhancing capital accumulation. Adkins, Moomaw, and Savvides (2002) similarly find that institutions that promote greater economic freedom, in turn, promote efficiency.

$$GDPPC\_gr_{it} = \alpha_0 + \alpha_1\, ln(GDPPC)_{it-1} + \alpha_2\, I_{it} + u_i + \delta_t + v_{it} \tag{15}$$

We replace "social infrastructure" with 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 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 5 years.

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. Following Solow (1956), the production function for country i is postulated as a function of physical and human capital as well as technological progress as

$$Y_i(t) = K_i\,(t)^{\alpha}\,[\,A_i\,(t)\,H_i\,(t)^{1-\alpha} \tag{16}$$

For simplicity, we drop the subscript i. We further assume that human capital is expressed in terms of education E, say an average year of schooling of the population $H\,(t) = L(t)\,G\,(E)$. *G(E)* is assumed to be constant, where G(E) = $e^{\emptyset E}$. $L(t) = nL(t)$ *and* $A(t) = gA(t)$. Given the steady-state assumption, the steady-state physical capital per unit of effective labour services

$$K^* = \frac{K^*}{[AG(E)L\}} = \frac{s}{(n+g+\eta)^{1/(1-\alpha)}} \tag{17}$$

Where s is the saving rate, g is a common exogenous rate of technical change and η is the depreciation rate.

We further assume common technology advances for all countries, i.e.

$$A(t) = \bar{A}\exp(gt) \tag{18}$$

Hence, steady-state output per capita can be expressed as

$$\ln y = \frac{\alpha}{1-\alpha}\ln\frac{s}{(n+g+\eta)} + (1+\alpha)\,\emptyset E + (1+\alpha)\ln\bar{A} + (1-\alpha)gt \tag{19}$$

We could then re-assess the augmented Solow model in a dynamic panel data framework as in Bond, et al. (2001). Based on eq. (19), the revised specification is as eq. (20) below:

$$GDPPC\_gr = \gamma_0 + \gamma_1 \, ln\,(GDPPC)_{it-1} + \gamma_2 \ln(s_{it)} + \gamma_3 \ln(n_{it} + \mathrm{g} + \eta) + \gamma_4 (SCHOOLING)_{it} + u_i + \delta_t + v_{it} \qquad (20)$$

Eq. (20) 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 on 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, technical progress is assumed to be common for all countries.

The third model extends that of eq. (20). The augmented Solow growth model of Bond, et al. (2001) does not incorporate institutions. We thus incorporate the effect of the 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.

More specifically, the level of productivity is revised as a function of the institution as

$$A(t) = \bar{A} \exp(g\, t + \delta\, I) \qquad (21)$$

With this assumption, the specification of our estimation can then be written as (22) below.

$$GDPPC\_gr = \gamma_0 + \gamma_1 \, ln\,(GDPPC)_{it-1} + \gamma_2 \ln(s_{it)} + \gamma_3 \ln(n_{it} + \mathrm{g} + \eta) + \gamma_4 (SCHOOLING)_{it} + \gamma_0 \, I_{it} + u_i + \delta_t + v_{it} \qquad (22)$$

The estimation results of these three models are in Table 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), the institution seems to have a significant and positive impact on economic growth. After controlling time-invariant determinants in a panel setting, the institution still significantly explains cross-country economic performances. That said, our results provide support to the institution's view, despite using modified specifications and estimation techniques.

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

Model	2.(1)	2.(2)	2.(3)
Specification	Hall and Jones (1999)	Mankiw et al (1992)	Penalosa and Teksoz (2006)
$ln(GDPPC)_{t-1}$	-0.0020	-0.0071	-0.0165
	(0.0014)	(0.0133)	(0.0125)
ln(s)		0.0193***	0.0256***
		(0.0069)	(0.0063)
$ln(n+g+\eta)$		-0.0932*	-0.1038*
		(0.0532)	(0.0617)
SCHOOLING		-0.0004	0.0020
		(0.0077)	(0.0079)
QOG	0.0596***		0.0386**
	(0.0096)		(0.0172)
Const	0.0152	-0.2338	-0.2404
	(0.0108)	(0.1733)	(0.1869)
Obs/No. of cty	760/181	644/100	424/90
AR(1) p-value	0.0774	0.0000	0.0000
AR(2) p-value	0.613	0.784	0.402
Hansen Test Stat	6.709	18.30	19.82
Hansen Test p-value	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 an income per capita of about[9] times that of a country. However, our Model 2.(2) Shows that human capital9 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 the 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 the institution is included in the latter model. This may suggest that the channel through which an 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

[9] We also attempt to proxy human capital by secondary and tertiary enrolment ratios instead of average years of schooling. The statistically insignificant results remain.

models accept the AR(1) test but reject the AR(2) test, except that in Model 2.(1), it is only significant at a 10% level. It, therefore, suggests that there is no serial correlation at a 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 Institution Direct Partial Impact on Growth

4.2.1 Baseline Specification

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

Table 2: Institution and Economic Growth – Baseline Specification (Two-Step System GMM Estimation)

Model	2.(4)	2.(5)	2.(6)	2.(7)	2.(8)
DEMOC	0.0011				0.0015
	(0.0009)				(0.0015)
REG		0.0046			0.0042
		(0.0038)			(0.0045)
SM			0.0020*		0.0031*
			(0.0010)		(0.0018)
LEGAL				0.0066***	0.0052***
				(0.0021)	(0.0020)
SCHOOLING	0.0102*	0.0091*	0.0147***	0.0128**	0.0139**
	(0.0059)	(0.0053)	(0.0051)	(0.0056)	(0.0067)
ln(GDPPC)$_{t-1}$	-0.0060	-0.0090	-0.0130	-0.0197**	-0.0304**
	(0.0108)	(0.0106)	(0.0089)	(0.0089)	(0.0118)
Const	-0.0042	0.0030	0.0112	0.0553	0.0843
	(0.0556)	(0.0565)	(0.0469)	(0.0430)	(0.0575)
Obs/No. of cty	556/99	500/95	542/95	483/95	447/91
AR(1) p-value	*[0.0000]*	*[0.0000]*	*[0.0000]*	*[0.0000]*	*[0.0000]*
AR(2) p-value	*[0.364]*	*[0.855]*	*[0.605]*	*[0.540]*	*[0.279]*
Hansen Test					
Test Stat	14.62	13.63	4.210	6.174	21.80
p-value	*[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", and "market-stabilising" and "market-creating" institutions respectively. We test individual variables 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 2.2, 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. Mauro (1995) and Djankov, Porta, Lopez-de-Silanes, et al. (2002)) on the other hand argue that more regulations in developing economies are more likely to 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 a 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 the growth rate by 1.0 and 0.69 percentage points[10] respectively. In a dynamic panel data set, our results generally agree with the key conclusion of the previous literature. In other words, "market-creating" and "market-stabilising" institutions have a 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, compared to the results of Bhattacharyya (2009), our results show that LEGAL relatively plays a more significant role than SM[11].

[10] The quantitative impacts are obtained from 0.0052*1.9251≈1.0% and 0.0031*2.2143≈0.69% respectively.

[11] By way of comparison, his results show that the respective parameters have growth effects of 0.75 and 0.75 percentage points respectively.

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 is appropriate.

4.2.2 Choice of Estimators

To further verify whether Blundell and Bond's (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 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 be significantly higher/lower than the upper/lower bounds.

The relevant test results are presented in Table 3. 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 3: Institution and Economic Growth – Baseline Specification Using Different Estimation Techniques

Model Estimator	2.(9) OLS	2.(10) FE	2.(11) 2-Step GMM	2.(12) 1-Step GMM
Lagged ln(GDPPC)	-0.0055***	-0.0629***	-0.0304**	-0.0283**
	(0.0020)	(0.0067)	(0.0118)	(0.0115)
DEMOC	0.0001	0.0012*	0.0015	0.0011
	(0.0005)	(0.0006)	(0.0015)	(0.0013)
REG	0.0003	0.0042*	0.0042	0.0061
	(0.0017)	(0.0023)	(0.0045)	(0.0046)
SM	0.0016**	0.0009	0.0031*	0.0042***
	(0.0008)	(0.0008)	(0.0018)	(0.0015)
LEGAL	0.0043***	0.0035***	0.0052***	0.0055***
	(0.0012)	(0.0012)	(0.0020)	(0.0019)
SCHOOLING	0.0017*	0.0013	0.0139**	0.0123*
	(0.0009)	(0.0024)	(0.0067)	(0.0065)
Const	0.0127	0.4528***	0.0843	0.0566
	(0.0132)	(0.0543)	(0.0575)	(0.0593)
Obs	447	447	447	447
No. of cty		91	91	91
Adj. R^2	0.130	0.0904		
F-stat	6.142	13.21		
AR(1) p-value			[0.0000]	[0.0000]
AR(2) p-value			[0.279]	[0.329]
Hansen Test				
Test Statistics			21.80	21.80
p-value			[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 in Table 4. 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 a percentage of GDP (gcon_gdp) and the natural log of investment share as a 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 a percentage of GDP (fdi_gdp), natural log of total trade as a 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 in Greenwood and Jovanovic (1990), King and Levine (1993), Levine (1997), Harrison, Sussman, and Zeira (1999), Beck, Demirguc-Kunt, and Levine (2001), Beck, Demirguc-Kunt, and Levine (2003a), Rajan and Zingales (2003) and Beck and Levine (2004). We use three recently developed indicators – (i) the financial integration index from Lane and Milesi-Ferretti (2006) (fin_open); (ii) the capital account openness index from Chinn and Ito (2006) (ca_open), and (iii) total liquid liabilities as a 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 to suggest that only LEGAL and SM have a 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 toward the interests of the foreign investors and those that 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 fewer regulations. A democratic society, on the other hand, is likely to be more politically 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 an 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: 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)**
Additional Covariates	**pop_gr**	**gcon_gdp**	**ln(invest_gdp)**	**fdi_gdp**	**ln(trade_gdp)**	**fin_open**	**ca_open**	**lliab_gdp**	**(13)+(14)+(15)+(16)+(18)+(19)**
DEMOC	-0.0014	0.0010	0.0013	0.0015	0.0020*	-0.0008	0.0003	0.0001	-0.0009
	(0.0016)	(0.0012)	(0.0014)	(0.0012)	(0.0011)	(0.0012)	(0.0012)	(0.0010)	(0.0011)
REG	0.0044	0.0018	0.0063	0.0024	0.0080*	0.0068	0.0024	0.0023	0.0038
	(0.0043)	(0.0040)	(0.0045)	(0.0049)	(0.0041)	(0.0047)	(0.0050)	(0.0040)	(0.0045)
SM	0.0044**	0.0040***	0.0030*	0.0034*	0.0019	0.0033**	0.0039**	0.0031*	0.0032**
	(0.0019)	(0.0014)	(0.0018)	(0.0018)	(0.0017)	(0.0016)	(0.0016)	(0.0018)	(0.0015)
LEGAL	0.0035	0.0041**	0.0050**	0.0048***	0.0052**	0.0041*	0.0048**	0.0042**	0.0037**
	(0.0021)	(0.0018)	(0.0020)	(0.0018)	(0.0021)	(0.0022)	(0.0019)	(0.0019)	(0.0018)
SCHOOLING	0.0110*	0.0051	0.0126*	0.0083	0.0032	0.0097	0.0138**	0.0088**	0.0006
	(0.0061)	(0.0053)	(0.0068)	(0.0055)	(0.0047)	(0.0081)	(0.0062)	(0.0042)	(0.0041)
Lagged ln(GDPPC)	-0.0247**	-0.0087	-0.0375**	-0.0183*	-0.0112	-0.0169	-0.0246**	-0.0173**	-0.0054
	(0.0114)	(0.0091)	(0.0160)	(0.0100)	(0.0094)	(0.0156)	(0.0115)	(0.0078)	(0.0098)
Const	0.0915	-0.0385	0.0618	0.0322	-0.1262	0.0173	0.0533	0.0344	-0.0407
	(0.0662)	(0.0480)	(0.0658)	(0.0525)	(0.0887)	(0.0797)	(0.0608)	(0.0456)	(0.0507)
Obs/No. of cty	447/91	446/91	446/91	444/91	437/91	436/87	442/91	409/86	429/87
AR(1) p-value	*[0.0000]*	*[0.0000]*	*[0.0000]*	*[0.0000]*	*[0.0000]*	*[0.0000]*	*[0.0000]*	*[0.0000]*	*[0.0000]*
AR(2) p-value	*[0.225]*	*[0.832]*	*[0.466]*	*[0.465]*	*[0.525]*	*[0.663]*	*[0.293]*	*[0.420]*	*[0.522]*
Hansen Test Stat	23.72	32.83	13.41	23.95	22.74	22.99	22.07	23.39	34.35
Hansen p-value	*[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 5. Instead of DEMOC, we use the augmented Freedom House's polity index (IPOLITY2). Hadenius and Teorell (2005) suggest 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 5 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 impacts 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[12]. 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.

[12] DEMOC has not to be incorporated simultaneously to avoid multicollinearity since XCONST is a component of DEMOC.

Table 5: Institution and Economic Growth – With Alternative Measures of Institutions
(2-Step System GMM Estimations)

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

4.2.3.3 With Different Sub-samples

Finally, we use different sub-samples to test our baseline specification in Table 6. Following the literature on the effect of legal origins on institutions and economic outcomes[13], 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" institutions, 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 institutions (SM), are directly conducive to direct impact on growth.

[13] See La Porta, et al. (2008) for a recent survey on this strand of literature. In gist, this strand of view suggests that the legal origin of an economy will directly affect its degree of security contrary does not have any

economic growth. DEMOC, the 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 indicates 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 the 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 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, the more red tape may also lead to more corruption which is growth-hampering. However, the case of Sub-Sahara 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-Sahara African countries is significantly smaller than the other sub-samples.

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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.

Table 6: Institution and Economic Growth – Sub-Samples Estimations
(2-Step System GMM Estimations)

Model	2.(11)	2.(27)	2.(28)	2.(29)	2.(30)	2.(31)	2.(32)
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	0.0006	0.0002	-0.0000	0.0017	-0.0006	-0.0005
	(0.0015)	(0.0013)	(0.0021)	(0.0010)	(0.0026)	(0.0013)	(0.0016)
REG	0.0042	0.0089*	-0.0064	0.0026	0.0135	0.0117*	0.0210**
	(0.0045)	(0.0050)	(0.0069)	(0.0039)	(0.0107)	(0.0062)	(0.0092)
SM	0.0031*	0.0030*	0.0065**	0.0023*	0.0035	0.0030*	0.0035
	(0.0018)	(0.0018)	(0.0027)	(0.0013)	(0.0045)	(0.0017)	(0.0035)
LEGAL	0.0052***	0.0073***	0.0061*	0.0050***	0.0098**	0.0072**	0.0090**
	(0.0020)	(0.0024)	(0.0032)	(0.0018)	(0.0046)	(0.0030)	(0.0039)
SCHOOLING	0.0139*	0.0101*	0.0015	0.0092**	-0.0064	0.0131	-0.0018
	(0.0067)	(0.0060)	(0.0053)	(0.0043)	(0.0107)	(0.0088)	(0.0123)
Lagged ln(GDPPC)	-0.0304**	-0.0269***	-0.0044	-0.0218**	-0.0021	-0.0293*	0.0030
	(0.0118)	(0.0096)	(0.0091)	(0.0094)	(0.0176)	(0.0162)	(0.0323)
Const	0.0843	0.0509	-0.0047	0.0744	-0.1039	0.0427	-0.1700
	(0.0575)	(0.0477)	(0.0433)	(0.0529)	(0.0891)	(0.0962)	(0.1866)
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 institutions 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 brief 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 a 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 the 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 the 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, have a direct impact on growth. In contrast to, say human capital, the effect of the institution is more statistically significant.

We further attempt to test institutions into clusters. Based on Rodrik (2005)'s taxonomy described in first paper of this series "Role of Political Institutions on Economic Growth: Empirical Evidence", we cluster institutions into four dimensions, which essentially measure 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 institutions, economic institutions 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, the 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 institutions and policies impose direct and partial growth impacts, but political institutions do 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 incidences, e.g. wars, on the institutional changes. Our results merely attempt to address the concerns on and critiques of the existing institution view literature. Moreover, the observations show that institutions that do not have a direct growth impact do not mean that they play no particular role in the development process. Also, their impacts may be non-linear. With this in mind, we will try to explore other channels through which these defined clusters may affect economic development in the following papers of this series.

APPENDICES

A.1 Descriptive Statistics and Correlation Matrices

Descriptive Statistics of Institutional Variables

Variable	Obs	Mean	Std. Dev.	Min	Max
Key Variables					
GDPPC_gr	1101	0.0163	0.0458	-0.4288	0.3237
DEMOC	986	3.9782	4.1208	0	10
LEGAL	664	5.3639	1.9251	1.1500	9.3340
IPOLITY2	1189	5.3734	3.4537	0	10
QOG	625	0.5488	0.2351	0.0556	1.0000
XCONST	1006	0.1601	14.2511	-88.0000	7.0000
REG	699	5.4414	1.1096	2.4700	8.7600
SM	800	6.5251	2.2143	0.0000	9.8633
SCHOOLING	715	4.7890	2.9261	0.0420	12.2470
Control Variables					
fdi_gdp	956	0.0303	0.1276	-0.0528	3.5772
pop_gr	1375	0.0181	0.0165	-0.1605	0.1773
fin_open	865	1.7679	6.5469	0.1195	179.2779
ca_open	1037	-0.0657	1.4480	-1.8081	2.5408
lliab_gdp	840	0.4439	0.3376	0.0084	3.0226
gcon_gdp	1143	23.5662	11.3795	2.5525	79.5660
ln(trade_gdp)	1075	4.1823	0.6104	0.8215	5.9644
ln(invest_gdp)	1143	2.4376	0.6801	-0.0657	4.5148

Correlation Matrix of Institutional Variables

	GDPPC_gr	*DEMOC*	*LEGAL*	*IPOLITY2*	*QOG*	*XCONST*	*REG*	*SM*	*SCHOOLING*	*Lagged ln(GDPPC)*
GDPPC_gr	1									
DEMOC	0.0627	1								
LEGAL	0.2585	0.5181	1							
IPOLITY2	0.0713	0.9717	0.5463	1						
QOG	0.2014	0.5742	0.8768	0.5767	1					
XCONST	0.1616	0.2917	0.3219	0.2609	0.367	1				
REG	0.2321	0.3741	0.4496	0.4165	0.4159	0.1684	1			
SM	0.1725	0.2457	0.4578	0.2541	0.4351	0.1707	0.4611	1		
SCHOOLING	0.1479	0.6487	0.7006	0.6767	0.7633	0.2917	0.4132	0.3771	1	
Lagged ln(GDPPC)	0.0507	0.5626	0.6907	0.5373	0.7403	0.2505	0.436	0.4232	0.8356	1

A.2 List of Economies

Country Coverage of the Fraser Institute's Economic Freedom of the World

Albania	Denmark	Latvia	Romania
Algeria	Dominican Rep.	Lesotho	Russia
Angola	Ecuador	Lithuania	Rwanda
Argentina	Egypt	Luxembourg	Senegal
Armenia	El Salvador	Macedonia	Serbia
Australia	Estonia	Madagascar	Sierra Leone
Austria	Ethiopia	Malawi	Singapore
Azerbaijan	Fiji	Malaysia	Slovak Rep
Bahamas	Finland	Mali	Slovenia
Bahrain	France	Malta	South Africa
Bangladesh	Gabon	Mauritania	South Korea
Barbados	Georgia	Mauritius	Spain
Belgium	Germany	Mexico	Sri Lanka
Belize	Ghana	Moldova	Sweden
Benin	Greece	Mongolia	Switzerland
Bolivia	Guatemala	Montenegro	Syria
Bosnia and Herzegovina	Guinea-Bissau	Morocco	Taiwan
Botswana	Guyana	Mozambique	Tanzania
Brazil	Haiti	Myanmar	Thailand
Bulgaria	Honduras	Namibia	Togo
Burkina Faso	Hong Kong	Nepal	Trinidad & Tobago
Burundi	Hungary	Netherlands	Tunisia
Cameroon	Iceland	New Zealand	Turkey
Canada	India	Nicaragua	Uganda
Central Afr. Rep.	Indonesia	Niger	Ukraine
Chad	Iran	Nigeria	Unit. Arab Em.
Chile	Ireland	Norway	United Kingdom
China	Israel	Oman	United States
Colombia	Italy	Pakistan	Uruguay
Congo, Dem. R.	Jamaica	Panama	Venezuela
Congo, Rep. Of	Japan	Pap. New Guinea	Vietnam
Costa Rica	Jordan	Paraguay	Zambia
Cote d'Ivoire	Kazakhstan	Peru	Zimbabwe
Croatia	Kenya	Philippines	
Cyprus	Kuwait	Poland	
Czech Rep.	Kyrgyz Republic	Portugal	

Notes:

Economies are classified as all-time democracy (autocracy) if they continuously score 5 or above (less than 5) in ***DEMOC*** during the sample period.

Economies are classified as more democratic (autocratic) if they switched from autocracy (democracy) to democracy (autocracy) during the sample period, i.e. from scores of less than 5

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