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Heterogeneity Happens: How Rights Matter in Economic Development

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ABSTRACT:

The paper examines how much governance matters for long run economic development in poor countries.

Answering this question confronts four methodological challenges. First, since growth equations are likely to be underspecified, unobserved effects are likely to render standard estimators biased and inconsistent. Second, measurement error, time-varying unobserved effects, and possible feedback from growth into governance will violate the exogeneity assumptions of many estimators. Bias and inconsistency is again the consequence. Third, use of cross-country evidence opens the possibility that the interaction between governance and growth shows considerable country-specificity. In the presence of heterogeneity, estimation under a specification that imposes homogeneity on the governance – growth link, will again result in bias and inconsistency. Finally, an assumption of linearity in the growth-governance relationship across large ranges of per capita income and differences in institutional structure is at least questionable. The paper addresses these problems by contrasting results obtained under pooled OLS, fixed effects, GMM, country specific time series and PMG estimators. Results confirm that all four methodological concerns are valid. Taking account of the problems renders estimates of the impact of governance more robust, and serves to increase its impact.

Our best estimate of the impact of improving rights on the level of real per capita output is that this differs between countries with good, mid-range and poor rights. The implied elasticities of a benevolent impact on the level of real per capita output of improving rights ranging from 0.28 to 0.22 for countries with the worst rights, and 0.07-0.02 for countries with the best rights. Countries with midrange rights have a perverse association between improving rights and output, though the estimated elasticity range is relatively weak over the 0.03-0.02 range. Improving rights have an indirect impact on output as well a direct one. Improving rights serve to increase the productivity of investment, with the

elasticity of output with respect to investment rising from 0.29 to 0.45 between countries with the worst rights, and those with mid-range rights (thus improving its productivity by a factor of 1.6). The impact of economic policy on output similarly improves under improved governance – though more weakly than the productivity of investment – with both the openness and the anti-inflationary policy stance of the economy proving to be significant. Investment in human capital by contrast has an impact on output that is invariant to the level of rights. The estimated elasticity of education on output reaches 0.44 at an average of eleven years of schooling, and unitary elasticity at an average of 25 years of schooling.

These results find a significant echo, but also some nuance in equations specified in terms of economic growth. Substantively, our estimation results confirm an increasing productivity of investment for growth purposes under rising governance, and they are consistent with rising levels of investment under improving governance. Further evidence in support of the impact of good governance comes from the fact that physical capital, human capital, openness of the economy, foreign direct investment and anti-inflationary economic policy all further spur growth – often dramatically so. By contrast, for mid-range rights countries only investment in physical capital raises economic growth (though strongly so), while for poor rights countries empirical results prove mixed at best. The implications are twofold. Under good governance policy makers have the gamut of standard policy handles at their disposal in promoting growth. Their task is correspondingly easier. And on a methodological note, the finding of strong heterogeneity across countries serves to offer an explanation of why the literature may have struggled to isolate particularly robust results under cross sectional and insufficiently sophisticated panel estimators.

JEL: O10, D99.

KEYWORDS: Governance, economic growth, rights.

1.0 Introduction

Does governance matter for long term economic development?

This question has been posed by recent important developments in economic policy making.¹ In February 2003, President George W. Bush sent Congress a bill to increase foreign aid by 50 percent over the next three years by creating a Millennium Challenge Account (MCA). The MCA allocates an additional US\$5 billion per year to a select group of poor countries, allocated on the basis of sound policies and honest governments. By early 2004, the Bush administration had identified 63 countries eligible to compete for MCA funds because their per capita income was below \$1,415 and they were not deemed sponsors of terrorism. To qualify for MCA funds, countries are rated on 16 indicators of performance from a variety of sources.² To receive MCA funds, a poor country has to score above the median on the anti-corruption indicator and above the median in half the indicators in each of three domains of performance broadly related to governance.³ In both scale and design, the MCA has been called the first major foreign aid initiative in more than 40 years.⁴ Its underlying logic is that aid can help countries with good governance, but will make little difference

¹ It is also arguably as old as economics itself. Adam Smith's *Wealth of Nations* (1776) played close attention to the impact of economic institutions on development; David Landes' *The Wealth and Poverty of Nations* (1998) does likewise. The modern resurgence of interest in institutions and their impact on development is significantly due to the contributions of North (1980, 1990) and North and Thomas (1970, 1973). For a detailed examination of the argument in Landes (1998) see Fedderke (1999). For a review of the literature in the first half of the 1990's, see Fedderke (1997). Acemoglu et al (2004) present an attempt at both theoretical and empirical investigations into why governance might differ across time and space.

² The following 16 indicators (with sources) were, "chosen because of the relative quality and objectivity of their data, country coverage, public availability, and correlation with growth and poverty reduction, will be used to assess national performance relative to governing justly, investing in people, and encouraging economic freedom":

1. Governing Justly: Civil Liberties (Freedom House); Political Rights (Freedom House); Voice and Accountability (World Bank Institute); Government Effectiveness (World Bank Institute); Rule of Law (World Bank Institute); Control of Corruption (World Bank Institute).
2. Investing in People: Public Primary Education Spending as Percent of GDP (World Bank/national sources); Primary Education Completion Rate (World Bank/national sources); Public Expenditures on Health as Percent of GDP (World Bank/national sources); Immunization Rates: DPT and Measles (World Bank/UN/national sources).
3. Promoting Economic Freedom: Country Credit Rating (Institutional Investor Magazine); Inflation (IMF); 3-Year Budget Deficit (IMF/national sources); Trade Policy (Heritage Foundation); Regulatory Quality (World Bank Institute); Days to Start a Business (World Bank).
<http://www.whitehouse.gov/infocus/developingnations/millennium.html>

³ Exceptions will be allowed by recommendation of the MCA Board of Directors to the President. Once chosen, recipient countries will sign three-year contracts with the United States, and the effectiveness of their efforts will be judged by the results.

⁴ See Radelet (2003a, 2003b).

in countries with bad governance.⁵ Certainly this is a lesson that has been taken to heart by major aid funders.⁶

It would seem that around the world we are rediscovering the importance to economic development of property rights, the rule of law, and the enforcement of contracts. They are receiving increasing attention in development policymaking and in academic work. One consequence of this new emphasis is a concomitant de-emphasis of what might be called institution-free economics. If “institutions matter” (to quote one recent book) or “culture matters” (the title of another), then “we” must “go beyond” the simple constructs of, say, elementary macroeconomics. We must examine things beyond investment rates, money supply, taxes and tariffs and marginal propensities to consume. “Go beyond” has several possible senses. Institutions and governance may shape these economic variables. And policy choices may affect the institutions. Thus, both practitioners and theorists are asking anew, “What productive choices can be made relevant to such things as property rights, the rule of law, and the enforcement of contracts? What are the costs as well as the putative benefits of those choices? How might the answers depend on the context?” How on earth might one do research about such questions? What do “institutions” and “governance” mean?

When we delve into areas such as property rights, the rule of law, and the enforcement of contracts, clearly norms and legal systems are important. But not just the norms in the scriptures and the fairy tales, nor just the laws on the books. As John Noonan once wrote about bribery, in any society one can talk about norms at four levels:

Often a society has at least four definitions of a bribe—that of the more advanced moralists; that of the law as written; that of the law as in any degree enforced; that of common practice. If one is to say that an act of bribery has been committed, one should know which standard one is using (Noonan, 1984:xii).

So, we should also consider the institutions that transmit and enforce these norms and laws, such as courts and legislatures and police forces and land registries. We should go beyond the formal structures of these institutions, since the performance of these institutions depends on who is running them and how.

⁵ This is a lesson drawn by Dollar and Pritchett (1998) — and see also Burnside and Dollar (2000).

⁶ For instance, USAID states that: “Only if governance becomes more democratic and accountable will development occur in the poorly performing countries. And only with a comprehensive, consistent, ‘tough love’ from the international community is political will for governance reform likely to emerge and be sustained.” (USAID 2002: 51)

So, we should also examine the policy choices being made about and within institutions, and the quality of these institutions' management. Finally, the results in terms of better property rights, effective but not oppressive enforcement of laws, and workable contracts also depend on what Noonan called "common practice" — on citizens' and firms' and officials' behavior, which presumably is related to their values and their incentives, to their levels of income, to the information available to them and their ability to process and act upon that information.

When we examine the effects on economic development of better property rights and the rule of law, we may encounter important determinants at all four of Noonan's levels. Figure 1 depicts various factors that influence the quality of property rights and the rule of law. How can one describe and study the four groups of factors on the left? Obviously, each group is complicated and, at least to an economist, poorly theorized. In the absence of agreed-upon theories and terms, words like "institutions" and "governance" have been applied to each of the four groups, and to all of them together. So, an "institution" can refer to something like the Central Bank, or it can refer to the reality of norms and cultures on the ground. "Governance" is also a flexible term (to put it charitably). Sometimes governance refers to a constitutional scheme or even a theory of government. Sometimes it descends to the level of the quality of public management in a particular agency.

INSERT FIGURE 1 ABOUT HERE

Faced with such linguistic variety, one has two choices. One can choose a particular definition, a kind of conceptual specification. Or, one can proceed empirically, examining a variety of descriptors that seem intuitively to match more or less the concept(s) in question and see how, in fact, these descriptors go together.

In our work, we choose the latter approach. In earlier research, we examined hundreds of social and political indicators and their relationships with each other and with economic variables (Klitgaard and Fedderke 1995; Fedderke and Klitgaard 1998). In the research reported here, we narrowed our scope to more than 40 variables thought by different experts to capture institutional or governance aspects of property rights, rule of law, and sanctity of contracts. These variables ranged from measures of rights and liberties, through structural variables about the nature of a government system, through variables describing

the performance of specific agencies at a particular time, to variables that reflect the attitudes and behaviors of citizens and firms toward their governments. Two of the measures are constructs created by Raymond Gastil, “political rights” and “civil liberties”.⁷ These two measures are available for a large number of countries for a longer time period than most of the rest of the 40-odd measures. Fortunately from a statistical point of view, these two correlate highly with most of those 40-plus measures of governance. Conceptually, one can certainly distinguish these two measures of rights and liberties from various other measures of institutions, governance, and citizen behavior. In practice, in a statistical sense these two “rights variables” pick up much of the variability across countries of the world in all the other measures. So, in this paper, we use these two variables as a proxy for “governance” and (with somewhat more trepidation) for institutions relevant to property rights, rule of law, and democracy.

But do we in fact know that governance really matters for economic development? And how confident are we, especially given the very different conditions facing developing countries around the world?

The ubiquitous tool of analysis employed during the course of the 1990’s in exploring the development – governance nexus has been cross country evidence – and regression analysis in particular. Yet the statistical evidence advanced in support of the link between governance and economic development has been notable for its lack of robustness.⁸

At least four estimation issues are raised by attempts to isolate the impact of governance on economic growth from cross country evidence. Few papers that we have encountered take full cognizance either of the existence of the methodological questions involved, or of appropriate means of resolving them.

⁷ The Freedom House political rights variable assesses to what extent the political system offers voters the opportunity to choose freely from among candidates and to what extent the candidates are chosen independently of the state. Formal electoral procedures are not the only factors that determine the real distribution of power. In many countries, the military retains a significant political role, while in others, the king maintains considerable power over the elected politicians. Civil liberties measures the extent to which countries maintain a set of basic rights for their citizens, including freedom of expression, assembly, association, education, religion, and are governed by an established and generally equitable system of rule of law. The measure does not equate constitutional guarantees of human rights with the on-the-ground fulfillment of these rights.

⁸ To cite but one example, Easterly *et al.* (2004) question the robustness of the Dollar and Pritchett (1998) statistical results. See also the reply by Burnside and Dollar (2004).

Incorporating a measure of governance into growth regressions faces a first hurdle in the form of having to choose a measure of governance. Measures of the social and political dimensions of societies have proliferated during the course of the 1990's. But very few such measures are available for the comprehensive width of geographic coverage, for a range of countries over different levels of development and cultural spaces, and over sufficiently long time runs to inspire confidence in their usefulness in being able to address the long run and dynamic developmental questions raised by linking growth and governance.

There are three dimensions to the data heterogeneity problem.

First, Freedom House's two measures of political rights and civil liberties are amongst the few that present themselves as suitable for inclusion in panel data sets that have any aspiration to any depth in the time dimension.⁹ The question we face here is whether we are justified in employing a single or even a few measure(s) of governance where there might exist many.¹⁰ Are we missing something by including only a few of the measures of governance in estimation, rather than the various dimensions that have emerged in the literature? For instance the governance measures suggested by Kaufman et al (1999a, 2002) isolate six dimensions of governance are both distinct, and argued by the authors to be separately related to economic growth: voice and accountability, political stability, government effectiveness, regulatory quality, rule of law, and control of corruption. An important methodological task that confronts studies interested in the long run impact of governance must be the measurement question of whether the governance measures available are adequate to the task.

The second dimension is in a sense the converse of the first: should we choose to rely on a range of measures of governance (perhaps even more than the Kaufman 6?), not only does the range of countries covered by the measures come to be circumscribed, but the length of time covered by the measures is similarly truncated (the Kaufman 6 cover only 1997/8 and 2000/1 for instance). How confident are we of drawing general conclusions concerning developmental trajectories that typically are concerned with very long run characteristics of economies on the basis of such data? How would we know that we are justified in so concluding, since we have no means of observing the long run in this

⁹ By contrast, the Kaufman et al (1999a, 2002) measures cover only about half a decade at best.

¹⁰ Skepticism such as that of Geertz (1983) concerning the feasibility of measuring the full diversity, locally specific, and protean aspects of "governance" would seem to emphasize the importance of measuring as many different aspects of governance as possible. Desirability is easy to concede. Necessity does not follow quite so immediately.

context? Specifically, how are we to conclude anything about long term *dynamic* processes and interactions?

The third dimension is specifically an estimation problem. Fedderke and Klitgaard (1998) showed that when considering a set of approximately 67 indicators of social and political dimensions of society, there exists a diverse, and often strong web of association between these social and political variables (or at least clusters of variables), and between many of these variables and measures of economic activity such as output and investment. The problem here is that estimation proceeds in the presence of unobserved effects that are correlated not only with the dependent variables in estimation, but also with explanatory regressors that are included in estimation.¹¹ Isolating the true impact of the governance measure employed is more difficult here, since a crucial source of heterogeneity across the countries included in the panel, which matters both for final outcomes and impacts on governance itself, has not been accounted for.

One solution to this first problem is estimation under fixed effects. Always presuming that measurement error is not prevalent in measuring governance. Klitgaard and Fedderke (1995) in examining evidence on social integration and disintegration noted concerns about both the validity and the reliability of the social indicators that are now routinely employed in growth regressions.¹² Where measurement error is present in governance, bias and inconsistency follows for fixed effects estimation also, since explanatory regressors once again correlate with the error term in estimation. A similar consequence follows from the now oft presented observation that reverse causality may exist from output to governance – see for instance Barro (1997), Fedderke and Klitgaard (1998), Hall and Jones (1999), Chong and Calderon (2000) and Acemoglu et al (2004).¹³ Finally, there is no reason to suppose that unobserved effects are time invariant – as presumed by standard fixed effects estimation. Political instability has long been noted as having a possible impact on output and growth – see for instance what one might cite as the original cross country growth regression, Barro (1991)¹⁴ - while a link between political instability and governance is almost

¹¹ In the microeconomic literature this estimation issue has been addressed for some time –for instance in isolating so-called average treatment effects. Health outcomes may depend on the level and quality of medical care – but both the impact of the medical care and the health outcome may also depend on some unobservable (genetic, say) characteristics of patients.

¹² Bollen (1991) and Inkeles (1991) provide detailed reflections on these questions from the perspective of political scientists. Fedderke, et al (2001a) provides a further detailed discussion of issues arising from the measurement of dimensions of governance in the context of long time series evidence.

¹³ For a theoretical model examining possible mechanisms for the reverse causality, see Gradstein (2004).

¹⁴ For an application to South African growth, using time series evidence, see Fedderke et al (2001b). For a demonstration of a link from political instability to investment, see Fedderke (2004).

tautological.¹⁵ Since in all three instances the strict exogeneity assumption of fixed effects estimation is violated, once again not accounting for the impact of relevant heterogeneity across countries, a further solution such as instrumentation under a generalized methods of moments estimator is required.

Introduction of an impact of governance on long run economic development must raise the question of appropriate time horizons for the estimation. The impact of governance, if it is present, is likely to be both pervasive, but may also take considerable time to fully manifest itself. Given the contractual, even constitutional nature of the mechanisms of governance, this of necessity must take time. Rodrick (2003) points to a distinction between short run growth stimuli, and the more serious challenge of raising growth in a manner that is sustainable in the long run. He also points to the extent to which governance is not only central to the latter, but is highly specific to individual countries and their particular histories. Clinical understanding of individual countries' experience and evolution, on this argument, are crucial in being able to isolate the impact of governance on growth, and how it was exercised.¹⁶ The merit of this argument is that it has some traction on resolving how many countries that appeared to violate principles of sound policy managed to grow,¹⁷ while others who did appear to adopt sound policy, have not realized growth pay-offs.¹⁸ But it also points to the importance of long run data, of covering a wide range of different experiences, of isolating general lessons from seemingly diverse trajectories.

If true, this argument again carries serious estimation challenges. First, dynamic estimators are likely to be relevant. Perhaps more importantly, if countries are institutionally strongly differentiated, there is little reason to suppose that the reaction of growth to any specific measure of governance would be homogenous across widely divergent types of countries, and countries at very different levels of development.¹⁹ The imposition of such homogeneity where it is not justified,

¹⁵ Though the correlation between measures of instability and governance is not always strong.

¹⁶ The point for Rodrick is that the general prescription that incentives matter, that good governance matters, leaves considerable scope for particular solutions as to how this might be attained in any instance.

¹⁷ The oft cited examples in the literature from East Asia. Rodrick's (2003) point is that they adhered to the important general principles (security of property rights, properly aligned incentives), while finding particular solutions appropriate to their history and cultural patterns.

¹⁸ Read here particularly Latin America, which adopted policies to emerge from the Washington Consensus (first and second generation), but without much improvement in growth.

¹⁹ Fedderke (2001) in considering time series evidence on a country-by-country basis, demonstrated that the structure of the link between governance and economic performance differs between countries, while the strength of the association varies even amongst countries that share a common structure.

would once again carry the cost of both bias and inconsistency for estimation.²⁰ Finally, it becomes at least an open issue whether the standard linearity assumption of most estimators is justified across all countries. If path dependence matters and history matters, are we justified in assuming the link between governance and growth to be not only homogeneous across countries, but across time for any specific country? Two relevant extensions here are the use of country specific time series estimation, as well as the deployment of modern dynamic heterogeneous panel estimation techniques.

The present paper is an attempt to make headway with these methodological problems, while addressing the substantive question of whether governance matters for long term economic development. In presenting results, we strive to present evidence so as to give an indication of the severity of each of the methodological problems we have identified, by comparing and contrasting the results obtained under alternative estimation methodologies. The data we employ draws on a wide range of measures of governance. In terms of estimation approaches, we employ both panel data estimation methodology drawing on a range of alternative estimators, as well as modern time series estimation approaches.

We focus on the impact of rights on long run economic development. In doing so we address both the impact of rights on the level of per capita output, and the impact of rights on economic growth. While the methodological concerns outlined above, and fully explained in section 2 of the paper apply to both contexts, we illustrate them fully only with respect to the levels specification, while in the case of transitional dynamics we present only final results. As a result, the findings of the paper are relevant not only in detailing the dynamics of income over time, but also in assessing to what extent rights impact on the long run steady state characteristics of the economy. This reflects our conviction that any concern with economic development must certainly consider the trajectory of development, but also its ultimate target. Alone the possibility that the growth path of an economy may be non-monotone is enough to establish the importance of steady state as well as time path characteristics.

In section 2 of the paper we outline in greater detail both the methodological problems and their consequences for estimation in our context. Section 3 presents the data we employ. Section 4 examines the evidence under alternative

²⁰ See Pesaran, Shin and Smith (1999).

estimation methodologies designed to address our methodological concerns. Section 5 concludes.

2.0 Methodological Issues

Our core question is whether governance matters for long run economic welfare.

Consider the general specification:

$$Y_{it} = Y(K_{it}, R_{it}) + \varepsilon_{it} \quad (2.1)$$

where Y denotes real output, K a vector of independent variables (we might think of these as capital stocks of various sorts, physical, human, financial, as well as labour factor services – and whatever else might be thought relevant), R the measure or measures of governance we employ,²¹ and ε an unobserved error term. We note explicitly that while in principle the discussion of the present section can allow R to be a vector of governance measures, we are specifically concerned with the instance where estimation has to proceed under a non-complete specification of the governance dimension. Nothing is lost in generality therefore by considering the case in which we have only a single measure of governance. Indeed, given the general unavailability of governance measures with a substantial time dimension, much is gained in practical relevance to panel estimation of growth equations incorporating governance. For this reason, we couch the discussion in terms of a single generic growth measure.

Our question is first whether $\partial Y/\partial R \neq 0$, and $\partial Y/\partial R > 0$ in particular, and second the strength (if any) of the $\partial Y/\partial R \neq 0$ effect..

Addressing the question empirically faces three methodological estimation challenges, and one related to the nature of the data series that we confront. Table 1, and the associated Figures 2 through 5 provide a summary account of the discussion, noting the source of the estimation problem, a diagrammatic illustration of its consequence, the implication for estimation and the proposed solution.

INSERT TABLE 1 AND THE ASSOCIATED FIGURES 2 – 5 ABOUT HERE.

2.1 Heterogeneity I: unobserved effects

²¹ Note that in the discussion we will employ the terms “governance” and “rights” interchangeably.

The most immediate concern of any specification such as (2.1) is that the true specification is in fact:

$$Y_{it} = Y(K_{it}, R_{it}, C_{it}) + e_{it} \quad (2.1')$$

where C denotes an unobserved random variable, a vector of characteristics that also impact on output. Our first concern is that estimation in the context such as ours, which introduces a relatively under-theorized dimension (governance) into estimation, must constantly stand under the suspicion of being subject to unobserved effects.

With potentially serious consequences. Provided that $cov(C, K_{it}) = cov(C, R_{it}) = 0$, C represents another unobservable not systematically related to K, R . But violation of either covariance zero restriction renders standard estimation subject to bias and inconsistency.²²

Appropriate responses to the presence of unobserved effects depend both on the nature of the unobserved random variables, as well as on any regressors with whom they may covary. Where the unobserved effect is time invariant, the availability of panel data²³ allows for recourse to either random effects (RE: appropriate where $cov(C, K_{it}) = cov(C, R_{it}) = 0$),²⁴ or fixed effects (FE: appropriate where $cov(C, K_{it}) \neq 0$, and/or $cov(C, R_{it}) \neq 0$).

Consistency of RE requires the exogeneity assumption that explanatory variables in each time period are uncorrelated with the errors in each time period, $cov(R_{is}, e_{it}) = 0 = cov(K_{is}, e_{it})$, $s, t = 1, \dots, T$. In the context of attempting to isolate an impact of governance on output there is justification for the presumption that $cov(C, R_{it}) \neq 0$, since the literature has identified a number of variables that have been linked to both output and to governance. For instance, the extent of ethnolinguistic fractionalization, the colonial history, and the degree of political instability have all been linked to both the governance structure, as well as the growth performance of countries.²⁵ The general point here is that if governance matters for economic performance, assuming governance to be exogenous to the

²² For a general discussion of the issues raised in this subsection, see Wooldridge (2002: ch's 10, 11 and 15).

²³ Cross sectional data would require the use of proxies for C in direct estimation of (2.1'), or recourse to some instrumental variables approach – presuming appropriate instruments are available.

²⁴ An alternative to RE would be to use pooled OLS (POLS), provided that the introduction of serial correlation due to the presence of the omitted variable in the error term is corrected for through the use of robust variance matrix estimator, and robust standard errors. Indeed, the presence of serial correlation in errors is one proposed means of testing for the presence of unobserved effects.

²⁵ See for instance Barro (1991) on political instability, and on colonial background see Véliz (1994), North, Summerhill and Weingast (2000) and Wiarda (2001). See also the discussion in Acemoglu *et al* (2004).

socio-institutional context and history of a country is implausible. RE estimation is therefore unlikely to suffice.

FE estimation is more promising, since under this estimator the case of $cov(C, K_{it}) \neq 0$, and/or $cov(C, R_{it}) \neq 0$ is explicitly embraced.²⁶ Consistency of the estimator continues to require the same exogeneity condition as under random effects, i.e. that explanatory variables in each time period are uncorrelated with the errors in each time period, $cov(R_{is}, e_{it}) = 0 = cov(K_{is}, e_{it}), s, t=1, \dots, T$.²⁷

For our purposes, allowing for covariance between unobserved effects and regressors appears desirable. While it is possible that political instability, fractionalization and history may not correlate with the governance structure of a country, it seems at least questionable to rule out the possibility. Once again, assuming exogeneity of governance to the socio-institutional context and history of a country seems like a very long shot. An additional problem that has not yet been addressed is the possibility that feedback effects from the dependent variable to *future* values of the explanatory variables may also be present. For instance, shocks to output may well carry implications for the stability of political dispensations, and hence the level and quality of governance. Yet the exogeneity assumption required for the consistency of the fixed effects estimator prohibits precisely this possibility.

One solution here is given by the generalized methods of moments (GMM) class of estimators that allow for sequential moment restrictions. In particular, the requirement is that $cov(R_{ij}, e_{it}) = 0, cov(K_{ij}, e_{it}) = 0, t=1, \dots, T, j=t-1, \dots, 1$, such that the explanatory variables are sequentially exogenous conditional on the unobserved effect. Using the first difference²⁸ of (2.1') under the sequential exogeneity assumption allows the use of lagged values of the explanatory variables as instruments.²⁹ Employing an AR(1) specification, Anderson and Hsiao (1982)

²⁶ Note that the FE assumption of time invariant unobserved effects excludes the possibility of employing time-invariant regressors in estimation. One generalization which relaxes the assumption of time invariant unobserved effects is given by random trend models, in which each group in the panel has an individual-specific trend in addition to its specific unobserved effect. Consistent estimation can then proceed by means of fixed effects on second differences, as long as the exogeneity assumption is satisfied, since the first difference eliminates the time invariant unobserved effect, the second difference the individual time trend. See Heckman and Hotz (1989).

²⁷ We restrict ourselves to the within estimator, since the between estimator loses the time variation within each cross section. Note that efficiency would also impose the requirement of homoscedastic variance and an absence of serial correlation. Serial correlation may be a concern particularly where the time dimension is large, and requires a robust asymptotic variance estimator – see for instance Arellano (1987).

²⁸ Which eliminates the time-invariant unobserved effect.

²⁹ A set of overidentifying restrictions allows the assumption of sequential exogeneity to be tested.

proposed the use of a limited set of instruments; Arellano and Bond (1991) suggested using the entire set of instruments.³⁰

Where we have a specification with dependent, truly exogenous, and sequentially exogenous variables, additional considerations for the choice of appropriate instruments arise where there exists the possibility of *contemporaneous* correlation between sequentially exogenous variables and the error term. Such correlation may be due to *time-varying* unobserved effects,³¹ simultaneity between the dependent variable and one or more of the sequentially exogenous variables,³² or due to measurement error in one or more of the explanatory variables. Again these are considerations likely to be of relevance to any attempt to specify the impact of governance on output. Certainly the possibility of feedback from output to governance has been repeatedly suggested in the literature.³³ Concerns about the accuracy and reliability of measures of governance are similarly pertinent – though such concerns while noted, receive less analytical attention than they deserve.³⁴ Finally, it only requires that political instability has time varying intensity for its impact on the output equation incorporating governance to be that of a non-constant unobserved effect.

Under the feedback and time varying unobserved effects cases the crucial consideration concerns the sequentially exogenous variables of the specification to be estimated. In the first difference specification of GMM, all strictly exogenous variables are valid instruments, but lagged values of the dependent variable only if there is no serial correlation, and use of lagged values of the sequentially exogenous variable only if they have only contemporaneous impacts on the dependent variable. For our application, this is a strong assumption to make, since it implies that governance has only a contemporaneous effect on output. Yet governance might be expected to have persistent effects, since it may impact on long term irreversible investment decisions, long term contracts, and

³⁰ Practical considerations may preclude this option. Large numbers of instruments can render estimation not only difficult (due to the column dimension of instruments), but large numbers of overidentifying restrictions have poor finite sample properties – see for instance Tauchen (1986), Altonji and Segal (1996), and Ziliak (1997). Note also that under an AR(1) structure where the autocorrelation coefficient approaches unity, the proposed instruments tend to be weak – though Blundell and Bond (1998) propose a solution, based on starting estimation from a steady state solution.

³¹ An example of an application here is Levine, Gustafson and Velenchik (1997), in examining the effect of smoking on wages. An omitted variable is income, which depends on the wage, but may also have an income effect on cigarette consumption.

³² See the analysis in Levitt (1996) of prison population on crime rates.

³³ Classically by Lipset (1959), but see also the discussion in Fedderke and Klitgaard (1998).

³⁴ See for instance the discussion in Bollen (1991) and Inkeles (1991). Klitgaard and Fedderke (1995) presents additional evidence on some of the socio-political measures often employed.

risk perceptions of country contexts that may take time to alter. Assumptions of valid sequential exogeneity may thus be difficult to justify. Under these circumstances the only available recourse may be to the adoption of truly exogenous instruments that satisfy the standard requirement that they do not appear in the structural relation to be estimated, but are correlated with the dependent variable.³⁵

In the event of the third source of contemporaneous correlation between sequentially exogenous variables and the error term, measurement error, the GMM estimator is consistent provided that the error occurs in strictly exogenous regressors, and that the measurement error is uncorrelated with both the accurately measured exogenous variables and true values of the inaccurately measured exogenous variables of the specification. Where in addition the measurement error is serially uncorrelated, lagged values of the variables measured with error become available as instruments.³⁶

One final issue requires noting before we move on to specific estimation issues that arise from the introduction of dynamics. Many of the measures of governance that are available are not continuous, some are binary. For instance, the Freedom House Political Rights and Civil Liberties measures are scaled from 1 (good rights) to 7 (poor rights), with countries being assigned integer scores over this range. Other measures take binary form, such as those which relate to the colonial history of the country, the form of the executive, type of ideological orientation of the polity, amongst others. The use of non-continuous measures makes relevant the well-established microeconomic average treatment literature. This raises the relevance of the class of random coefficient models, in which the “treatment” variable (here governance) takes on more than two values.³⁷ Unobserved effects remain present. The most straightforward suggestion for a consistent estimator of the marginal effect of “treatment” suggested by Heckman and Vytlacil (1998), for our application, is to estimate:

$$R_{it} = R(K_{it}, Z_{it}) + \eta_{it} \quad (2.2)$$

³⁵ It might appear as if an appropriate alternative would be to employ the exogenous variables of the structural relationship. Unfortunately this is unlikely to suffice, since their exogeneity is likely to render their correlation with the first difference of the variables being instrumented for low, so that they provide poor instruments.

³⁶ See the full discussion in Wooldridge (2002:311f) on these points.

³⁷ See for instance Angrist and Imbens (1995), Heckman (1997), and Heckman and Vytlacil (1998). Wooldridge (2002:638f) serves as overview. For the binary case, Persson and Tabellini (2004) provides an application, as well as further relevant references.

under linearity, with η an unobserved error term, and Z_{it} a vector of instrumental variables. Resultant fitted values are then employed in a second stage regression of (2.1).

What emerges from the discussion is that estimation in the context of attempting to isolate the impact of governance on growth faces non-trivial (though not necessarily fatal) issues of instrumentation, both in contexts of GMM estimation, and in the context of average treatment effects. Given the significantly under-theorized context of governance and growth, guidance on appropriate resolutions to these estimation questions is limited.

Our treatment of a number of crucial estimation issues raised in this subsection has been synoptic, and perforce brief. Table 1 provides the summary account of the impact of unobserved effects as heterogeneity Type Ia, and illustrates by means of Figure 2. The impact of violating exogeneity assumptions is summarized in Table 1 as heterogeneity Type Ib.

We conclude here merely by noting that the empirical literature exploring the impact of governance on growth has for the most part ignored the implications of the methodological issues here raised.

2.2 Heterogeneity II: dynamics

Sources of heterogeneity in estimation are not restricted to violations of conditional independence.³⁸ The *second* methodological trap that awaits is the imposition of homogeneity in regression coefficients, where the coefficients in fact vary across groups, in the context of dynamic panel estimation.

It does not appear reasonable to assume that either changes in governance, or the impact that such changes in governance would have on economic performance would occur instantaneously. Transitional dynamics under conditions where governance demonstrates persistent rather than instantaneous effects on output are of immediate interest. Moreover, transitional dynamics are likely to be pervasive provided only that adjustment costs in governance and the response of economic activity to changes in governance are non-trivial. Neither is implausible. Under such conditions, dynamic specifications of (2.1) are more reasonable than static formulations.

³⁸ See the introductory discussion to these issues in Baltagi (1995).

A number of estimation issues arise in this context. Suppose that (2.1) took the static form:

$$\begin{aligned} Y_{it} &= \beta_i K_{it} + \varepsilon_{it}, \quad i=1,2 \quad t=1,\dots,T \\ K_{it} &= \rho K_{i,t-1} + v_{it}, \quad |\rho| < 1 \quad v_i \square iid(0, \sigma_i^2) \end{aligned} \quad (2.3)$$

such that we suppress the impact of governance only for the sake of illustration. Where the model is estimated in dynamic specification as:

$$Y_{it} = \lambda Y_{i,t-1} + \beta K_{it} + \omega_{it}, \quad i=1,2 \quad (2.4)$$

Robertson and Symons (1992) demonstrate that under OLS, $plim \hat{\lambda} > \lambda$, $plim \hat{\beta} < \beta$.

Under the limiting case of $\rho=1$, the perverse result $plim \hat{\lambda}=1$, $plim \hat{\beta}=0$ emerges.

Monte Carlo experiments indicate the dynamics to be misleading even for $T=40$.³⁹

In the extension particularly relevant to our question, Pesaran and Smith (1995) consider estimation of a dynamic panel data model where parameters are individually heterogeneous. In our example:

$$\begin{aligned} Y_{it} &= \lambda_i Y_{i,t-1} + \beta_i K_{it} + \mu_{it}, \quad i=1,\dots,N \quad t=1,\dots,T \\ \lambda_i &\square IID(\lambda, \sigma_\lambda^2), \quad \beta_i \square IID(\beta, \sigma_\beta^2) \end{aligned} \quad (2.4')$$

with λ_i, β_i , independent of $Y_{is}, K_{is}, \omega_{is}, \forall s$. Attempting to estimate (2.4') by means of (2.4), provides:

$$\omega_{it} = \mu_{it} + (\lambda_i - \lambda) y_{i,t-1} + (\beta_i - \beta) K_{it} \quad (2.5)$$

By continuous substitution of lagged values of $y_{i,t-s}$, it follows that ω_{it} is correlated with all present and past values of $Y_{i,t-1-s}, K_{it-s}$, rendering the OLS inconsistent regardless of whether estimation proceeds by (i) aggregate time-series regressions of group averages; (ii) cross-section regressions of averages over time; (iii) pooled regressions allowing for fixed or random intercepts; or (iv) separate regressions for each group, averaging coefficients across groups. Since the error is correlated with lagged values of the regressors, lags are ruled out as valid instruments.⁴⁰

The question is how such parameter heterogeneity can be appropriately corrected for in estimation. We outline three possible alternatives. For the sake of

³⁹ Use of the Anderson and Hsiao (1982) estimator of dynamic models in first differencing to eliminate individual effects, using lagged first differences as instruments, as suggested by Arellano (1989) for stationary data, does not offer a solution. Instruments are invalid under false homogeneity restrictions on the β_i , unless K_{it} is white noise or follows a random walk. The second alternative is the economically most relevant, and for this Robertson and Symons demonstrate that if K_{it} is a random walk, the instrument is rendered orthogonal to the instrumented variable, and the estimator has infinite asymptotic variance. Finally, severe biases occur where K_{it} is stationary and autocorrelated.

⁴⁰ Thus precluding GMM estimation.

greater generality, consider the unrestricted error correction ARDL (p, q) representation:

$$\Delta y_{it} = \phi_i y_{i,t-1} + \beta_i' x_{i,t-1} + \sum_{j=1}^{p-1} \lambda_{ij} \Delta y_{i,t-j} + \sum_{j=0}^{q-1} \delta_{ij}' \Delta x_{i,t-j} + \mu_i + \varepsilon_{it}, \quad i=1, \dots, N \quad t=1, \dots, T \quad (2.6)$$

Here y_{it} is a scalar dependent variable, x_{it} ($k \times 1$) is the vector of (weakly exogenous) regressors for group i , while μ_i represents fixed effects. Allow $\varepsilon_{it} \sim iid(0, \sigma_i^2)$, and assume $\phi_i < 0 \quad \forall i$. Then there exists a long-run relationship between y_{it} and x_{it} :

$$y_{it} = \theta_i' x_{it} + \eta_{it}, \quad i=1, 2, \dots, N, \quad t=1, 2, \dots, T, \quad (2.7)$$

where $\theta_i = -\beta_i' / \phi_i$ is the $k \times 1$ vector of the long-run coefficients, with η_{it} 's stationary with possibly non-zero means (including fixed effects). Then (2.6) can be written as:

$$\Delta y_{it} = \phi_i \eta_{i,t-1} + \sum_{j=1}^{p-1} \lambda_{ij} \Delta y_{i,t-j} + \sum_{j=0}^{q-1} \delta_{ij}' \Delta x_{i,t-j} + \mu_i + \varepsilon_{it}, \quad (2.8)$$

where $\eta_{i,t-1}$ is the error correction term given by (2.7), and thus ϕ_i is the error correction coefficient measuring the speed of adjustment towards the long-run equilibrium.

Under this general framework we consider three approaches: *First*, the dynamic fixed effects (DFE) model which imposes homogeneity on all parameters except for fixed effects. Fixed effects estimates of all short-run parameters are obtained by pooling and denoted $\hat{\phi}_{DFE}, \hat{\beta}_{DFE}, \hat{\lambda}_{jDFE}, \hat{\delta}_{jDFE}$, and σ_{DFE}^2 . The estimate of the long-run coefficient is obtained by $\hat{\theta}_{DFE} = -(\hat{\beta}_{DFE} / \hat{\phi}_{DFE})$

Second, the mean group (MG) estimator proposed by Pesaran and Smith (1995) allows for heterogeneity of all the parameters in (2.6) and gives MG estimates of

short-run and long-run parameters $\hat{\phi}_{MG} = \frac{\sum_{i=1}^N \hat{\phi}_i}{N}$, $\hat{\beta}_{MG} = \frac{\sum_{i=1}^N \hat{\beta}_i}{N}$, $\hat{\lambda}_{jMG} = \frac{\sum_{i=1}^N \hat{\lambda}_{ij}}{N}$, $\hat{\delta}_{jMG} = \frac{\sum_{i=1}^N \hat{\delta}_{ij}}{N}$, where $\hat{\phi}_i, \hat{\beta}_i, \hat{\lambda}_{ij}$ and $\hat{\delta}_{ij}$ are the OLS estimates obtained individually

from (2.6), and $\hat{\theta}_{MG} = N^{-1} \sum_{i=1}^N -(\hat{\beta}_i / \hat{\phi}_i)$

Finally, the Pooled Mean Group (PMG) estimator of Pesaran, Shin and Smith (1999), provides an intermediate case between the above two limiting cases. This estimator allows the intercepts, short-run coefficients and error variances to differ freely across groups, but the long-run coefficients are constrained to be

homogeneous, $\theta_i = \theta, i = 1, 2, \dots, N$. The common long-run coefficients and the group-specific short-run coefficients are computed by pooled maximum likelihood (PML) estimation, denoted by $\tilde{\phi}_i, \tilde{\beta}_i, \tilde{\lambda}_{ij}, \tilde{\delta}_{ij}$ and θ . The PMG estimators

$$\text{as follow as } \hat{\phi}_{PMG} = \frac{\sum_{i=1}^N \tilde{\phi}_i}{N}, \hat{\beta}_{PMG} = \frac{\sum_{i=1}^N \tilde{\beta}_i}{N}, \hat{\lambda}_{jPMG} = \frac{\sum_{i=1}^N \tilde{\lambda}_{ij}}{N}, \hat{\delta}_{jPMG} = \frac{\sum_{i=1}^N \tilde{\delta}_{ij}}{N},$$

$\hat{\theta}_{PMG} = \tilde{\theta}$. This highlights both the pooling implied by the homogeneity restrictions on the long-run coefficients and the averaging across groups used to obtain means of the estimated error-correction coefficients and other short-run parameters.

Choice between DFE, MG and PMG estimators rests on the extent of heterogeneity between groups. Tests of homogeneity of error variances and/or short- or long-run slope coefficients are available as log-likelihood ratio tests, since the PMG and DFE estimators are restricted versions of (possibly heterogeneous) individual group equations. However the finite sample performance of such tests is generally unknown and thus unreliable. An alternative is provided by Hausman (1978) type tests. The MG estimator provides consistent estimates of the mean of the long-run coefficients, though these will be inefficient if slope homogeneity holds, while under long-run slope homogeneity the PMG estimator is consistent and efficient. Therefore, the effect of both long-run and short-run heterogeneity on the means of the coefficients can be determined by the Hausman test (hereafter *h* test) applied to the difference between MG and PMG or DFE estimators.

Table 1 summarizes the discussion of this section as heterogeneity Type II, and illustrates the MG and PMG estimators in Figures 3 and 4.⁴¹

2.3 Heterogeneity III: non-linearity

A final assumption of standard estimation approaches which we address in this paper is that of linearity across countries. Generally the justification given for the imposition of linearity is that of local approximation. One challenge to linearity in the context of identifying the determinants of aggregate output might be inferred from the fact that a standard sample of countries drawn from across the world would cover a range of real per capita GDP from US\$105 to US\$42,246.⁴²

⁴¹ The DFE is readily represented as under the PMGE (Figure 4), but with identical dynamics around the long run equilibrium relationship across the two groups.

⁴² See also the discussion in Persson and Tabellini (2004) on this point.

Under such circumstances the local approximation justification of linearity no longer appears credible.

But in the current context our concern is more specific. Assumptions of linearity are readily rendered inappropriate also due to the nature of the impact of explanatory regressors. Consider again the general framework provided by (2.1), such that the level of output depends on a standard factor of production such as capital. It also depends on the level of rights.

Suppose further that technology has the standard feature that $Y_K > 0$, $Y_{KK} < 0$, $Y_R > 0$, $Y_{RR} < 0$. It follows that:

$$dY = Y_K dK + Y_R dR \quad (2.9)$$

such that output growth depends on governance – or improvements in governance. But estimation of (2.9) may suffer from at least two potential complications. *First*, accumulation of capital may itself depend on governance. The simplest justification of this proposition is given by the potential impact of governance on uncertainty, hence investment.⁴³ The proposal then is simply that governance impacts on the uncertainty of investment prospects. There need be no presumption here that the impact of governance on uncertainty, hence investment, be either positive or negative⁴⁴ - though empirical evidence appears to favour a positive impact.⁴⁵ Hence:

$$K = K(R) \quad (2.10)$$

such that:

$$dY = Y_K dK + (Y_R + Y_K K_R) dR \quad (2.9')$$

The impact of any change in governance on output will be both direct ($Y_R dR$), and indirect by altering the **level** of investment ($K_R dR$), and hence output ($Y_K K_R dR$). Given only standard concavity of output in capital, and capital in governance (i.e. $Y_{KK} < 0$, $K_{RR} < 0$), it follows that the impact of changes in governance will be both nonlinear in the level of governance and the level of capital intensity of production. Specifically, at high levels of governance, and at

⁴³ The context here is the theory of investment under uncertainty given irreversibility due to Dixit and Pindyck (1994).

⁴⁴ Indeed argument has been presented in either direction. For instance, autocratic regimes have been argued to represent high degrees of discretionary power on the part of policy makers, rendering the expected rate of return uncertain for investors – see for instance Borner, Brunetti and Weder (1995). As an alternative, democratic regimes may be subject to high levels of populist pressure, rendering policy consistency uncertain, while autocratic regimes if stable may provide both the high savings rates and the stability of policy direction conducive to long run accumulation – see the discussion in Barro (1994).

⁴⁵ See for instance Fielding (1997, 2000) and Fedderke (2004) on investment in physical capital stock for manufacturing, Fedderke and Liu (2002) on capital flows, and Fedderke, De Kadt and Luiz (2001b) on aggregate investment.

high levels of capital accumulation improvements in governance will generate less of an impact on output than where governance or capital stock are low (i.e. $\partial Y/\partial R$ will be greater at low levels of R , than where R is high).

Second, suppose instead that not the level of capital stock, but its marginal product is contingent on the quality of governance - a unit of capital under good governance may contribute more to output than under bad. This is essentially the argument proposed by Dollar and Pritchett (1998) and Burnside and Dollar (2000) with reference to aid. Then we have $Y_K(R)$ such that:

$$dY = (Y_{KR}dR + Y_K)dK + Y_RdR \quad (2.9'')$$

Again the impact of any change in governance will be both direct and indirect – direct through Y_RdR , indirect by changing the **impact** any investment has on output, via $(Y_{KR}dR + Y_K)dK$. Non-linearity again follows, in this instance across levels of governance. The impact of investment in physical capital on output rises with the level of governance, provided that $Y_{KR} > 0$, though at a declining rate, supposing $Y_{KRR} < 0$.

In the presence of either secondary impact of governance (level of investment, or marginal product of investment), (2.9') and (2.9'') demonstrate that estimation of the fundamental growth relation given by (2.9) in linear specification across a wide range of either per capita output or governance levels, would return biased coefficients. Table 2 summarizes the implications of (2.9' and 2.9'').

INSERT TABLE 2 ABOUT HERE.

Estimation strategies should at least control for the possibility, and in the empirical section of this paper we consider explicitly estimations designed to test for the possible impact of the identified non-linearities. In Table 1 we summarize the implications of heterogeneity Type III, while Figure 5 illustrates one possibility that may emerge in practical estimation, presuming three ranges of the rights dimension with distinct impacts on output.

3.0 The Data

This study employs data on governance, on real per capita output, on investment, on human capital and on the quality of economic policy. All variables and their descriptions are reported in Appendix 1.

Estimation is on two distinct sets of data. The first is an almost balanced panel of 66 countries for the 1971-2000 time period,⁴⁶ chosen since they have the longest available time runs in three crucial dimensions; real output, real investment and Freedom House's two rights measures, political rights and civil liberties. Where the requirement is no longer for balance in a panel, but simply time series evidence on a country by country basis, data on more countries becomes available. Here we assembled data for 162 countries – though it should be noted that for many of these data is available only for some years in the period of 1971 - 2000.

The five indicators of human capital employed are standard to the growth literature⁴⁷ and are obtained from the Barro-Lee (1994) data set, the percentage of the population without any education, the percentage with completed primary, secondary and high schooling, and the average years of schooling in the population. Finally, we also employed three proxies of the quality of economic policy making, the proportion of GDP that is exported, the ratio of foreign direct investment to GDP, and the inflation rate.

For this study we have assembled a wide variety of publicly available measures of governance across countries. The first thing we note is that the various sources of these measures have different geographical coverage. Some include over a hundred countries; others, many fewer countries. Many measures have already been statistically massaged - for example, normalized. Others are in raw state.

3.1 Descriptive Statistics of the Data

We briefly report on the nature and quality of the data employed in the estimations reported below.

Univariate statistics reveal highly non-normal distributions for most variables. The distributions of most variables are skewed to the right, and several have major outliers. Univariate statistics for several key variables are presented in Table 3.

INSERT TABLE 3 ABOUT HERE.

⁴⁶ For a few countries one or two year's observations at the start of the T dimension are missing.

⁴⁷ See for instance Persson and Tabellini (2004).

The distribution of real per capita GDP (RGDP) and its log transform (LNRGDP) are, not surprisingly, both right tailed. The median of RGDP rises very slowly over time, but the range of the distribution nearly triples between 1972 and 2000. The increase in spread is driven by a few rightward outliers – noticeably Singapore, where RGDP rises from \$7K to \$28K over the time period studied. The growth variable (one-year change in log-transformed per capita GDP) has a more symmetric, two-tailed distribution. However, both the median and skewness tend to fluctuate over time, and the spread contracts after 1990. While average worldwide income increases relatively steadily (average growth among all countries fell below zero only in 1982 and 1983), there is considerable per-country variation. Not only do countries vary enormously in income and average growth rates (from Nicaragua's -2.3% to China's 6.8%), but also in the variability of growth – there is a factor of ten between the lowest and highest per-country standard deviations in growth rates over the time period studied.

The linear combination of the two Gastil measures (RIGHTS) ranges from 2 (most free) to 14 (virtually no freedom).⁴⁸ The distribution of the variable across all years and countries is roughly bimodal, with peaks at the 4-5 range and the 10-12 range. This encourages us to divide the countries into multiple governance strata (see the discussion below). While average world governance improves over the time period studied (see Figure 6), this masks considerable heterogeneity at the country and regional level, with the strongest improvements in rights for our 66 country panel occurring in North West, Central & Southern Europe, the worst rights on average occurring in the Middle East & North Africa, Sub-Saharan Africa, and Asia, while South America, Central America and the Caribbean and Oceania on average show movement from poor to mid-level rights (see Figure 7). Some nations (eg. Benin, Greece, Malawi) experience sudden and lasting transitions from low to high levels of freedom; others (eg. Ghana, Nigeria, Thailand) experience sharp shifts in both directions; while still others remain relatively unchanged or show no clear pattern over time. Shifts between strata of governance are discussed below.

INSERT FIGURE 6 ABOUT HERE.

INSERT FIGURE 7 ABOUT HERE.

⁴⁸ This study has at its disposal a number of measures of governance. One question therefore is why we do not have recourse to factor or principal components analysis. Two sets of reasons motivate our choice. First, given our interest in long development, our analysis requires long time runs of data in the governance dimensions, and many of the governance indicators do not have this available. Secondly, given the many strong correlations between the rights measures for the time periods in which many indicators are present, suggests that the informational content gain from the principal component is not necessarily strong.

The general improvement in governance has tended to be concentrated at discrete time points, with 1979, 1988 and 1996 showing noticeable improvements in rights. We divided the country-years in our sample into three strata of the RIGHTS variable: GOOD (2 to 7), MID-RANGE (8 to 10), and POOR (11 to 14). This differs slightly from Freedom House's own country status assignment rule. In 1972, almost half the sample (45%) was POOR and close to one third (20 countries) were considered GOOD. By 2000, this distribution shifted significantly, with over half (56%) in GOOD and only a quarter (17 countries) in POOR. The advancement of GOOD's share resembles a step function, making leaps in 1979, 1988, and 1996, and remaining relatively stable in between. This global transition is shown graphically in Figure 8.

INSERT FIGURE 8 ABOUT HERE.

For many individual countries, however, governance shifts often and without clear trends. On the country level, most countries fluctuated by one or two points every other year, if not annually. Nineteen countries fluctuated so wildly in RIGHTS that they made many stratum moves in both directions and exhibited no trend.⁴⁹ Twelve countries exhibited some indecisive trend (mostly positive), and many have not yet settled within a single stratum.⁵⁰ Thirteen countries exhibited a strong trend and/or have remained in a single stratum over the last decade studied.⁵¹ These are overwhelmingly in GOOD. Ten countries have left their long-time stratum once or twice.⁵² Finally, twelve countries have persisted in the same stratum through the entire period.⁵³ Most of these are African countries categorized as POOR.

For the measure of investment (INVEST), the median and spread oscillate over time, while skewness increases to the right. The worldwide average of INVEST fluctuates in the 1970's, rises from the mid-1980's to the late-1990's, and falls thereafter. Foreign direct investment (FDI) follows a similar pattern. Inflation (CPI) is the most skewed variable (again, to the right), with extreme outliers in

⁴⁹ Bangladesh, Burkina Faso, Chile, Ghana, Guatemala, Haiti, Indonesia, Ivory Coast, Lesotho, Madagascar, Mexico, Nicaragua, Niger, Nigeria, Pakistan, Peru, Thailand, Zambia, and Zimbabwe.

⁵⁰ Benin, Gabon, Gambia, Kenya, Malawi, Mali, Nepal, Paraguay, Senegal, South Africa, South Korea, and Swaziland.

⁵¹ Argentina, Bolivia, Brazil, Cameroon, Ecuador, El Salvador, Greece, Hungary, Malaysia, Philippines, Portugal, Spain, and Uruguay.

⁵² Algeria, Colombia, Egypt, Guyana, India, Morocco, Sri Lanka, Togo, Tunisia, and Venezuela.

⁵³ Burundi, China, Congo-Kinshasa, Dominican Republic, Israel, Jamaica, Mauritania, Mauritius, Singapore, Syria, and Trinidad.

the countries of South America and in Congo-Kinshasa. The education variables (percentage of population without education denoted LU, percentage of population with completed primary, secondary, high school education denoted LPC, LSC, LHC respectively, and average years of education TYR) have relatively stable distributions over time, and with the exception of TYR, are also skewed noticeably to the right.

Country ranks tend to remain stable over time. Rank correlations were computed to examine how greatly each country's ranking along a particular variable changed over time. Country rankings are especially stable for the income variables and for the percentage of population without schooling (LU) (rank correlations of approximately 0.99) but also exceed 0.9 for exports, investment, and the remaining education variables. Governance rank correlations fluctuate over time between approximately 0.7 and 0.9. The growth variable has the lowest rank correlations, ranging from -0.11 to 0.33.

Some of the heterogeneity between countries is noticeably clustered by region. Fluctuations in regional rankings are usually small, except for the growth variable. Noticeably, Asia makes an advance in all variables except RIGHTS and FDI, and individual Asian countries have the sharpest investment and export increases. Africa either keeps its low rank or moves down, and performs especially poorly among the education variables. Not surprisingly, Europe either keeps the top ranking or moves upward to it along most variables. Other regions have mixed movements.

Log-transformed income provides the strongest correlation with governance (compared to RGDP and GROWTH). Correlations for the key variables are presented in Table 4. The human capital variables have, as a group, the most consistently high correlations with income. Among them, TYR provides highest correlations with LNREGDP (around 0.76). Investment is almost perfectly correlated with income, but not with growth. RIGHTS has a high and benevolent correlation with RGDP that increases in time up to -0.60 (decreases in RIGHTS represent improvements). FDI and EXPORTS have weaker but consistently positive correlations with income, while the relationship between income and inflation is inconsistent in direction and very weak.

INSERT TABLE 4 ABOUT HERE.

The correlations between income and other variables changes little with lags.

The correlation between income and rights does weaken as the lag between the two variables increases from zero to ten years (from -0.52 to -0.47 using LNRGDP), as it does for inflation and investment. The correlations between income and two variables – exports and FDI – actually increase slightly as the lag increases. For the most part, however, lags do not change the correlations significantly.

Governance is associated with output to a higher extent as countries move away (in either direction) from the middle of the governance scale. We next examined the impact of underlying governance on the relationship between RIGHTS and income, using both rolling categories (i.e., RIGHTS from 2 to 4, from 3 to 5, and so on) and the three major governance classifications above. Using both measures, there is a benevolent correlation between RIGHTS and LNRGDP at both ends of the governance scale (of about -0.4) with ambiguous or slightly perverse correlations in the middle of the scale. This relationship, which exists at both early and late years in the study period, suggests that marginal political improvements have positive impacts on income only if the country is in an “extreme” state of governance – either solidly good or solidly bad. Note that these results parallel those of the higher-order analysis in section 4.3.3. Looking at other variables, the low but positive correlations between growth and the education variables increase slightly (by about $+0.2$) as the rights category gets worse. Finally, the correlation between LNRGDP and EXPORTS increases significantly (from -0.4 to $+0.4$), again as we move to worse categories of governance.

Limiting the data to periods of large and rapid changes in governance produces different results. For country-years with rapid improvements (decreases) in RIGHTS, there is a high positive correlation between Δ RIGHTS and same-year GROWTH. Since improvements are negative, this means that larger improvements in RIGHTS are associated with poorer growth performance. This may reflect a negative shock to growth associated with political liberalization. Conversely, it may represent reverse causality – the greater the economic problems in an unfree country, the larger the liberalization that is subsequently triggered. This correlation reverses with a one-year lag, suggesting that large liberalizations are associated with higher levels of next-year growth. After one year, there is no clear trend. For country-years with rapid deteriorations (increases) of RIGHTS, the correlation between Δ RIGHTS and current-year GROWTH is also positive, then strongly negative in the next two years. Since these changes are positive, however, the interpretation is the reverse

of the above: large deteriorations in rights are associated with high growth in the same year, but lower growth in following years.

Volatility of RIGHTS seems to have some positive relation with output. However, the volatility of RGDP and GROWTH does not seem to be related to volatility of RIGHTS. The volatility of CPI inflation impacts GROWTH negatively, with a correlation close to -0.4 .

While no general statement can be made on the association of changes in RIGHTS with output, a number of patterns are present. Thirteen countries seem to show a benevolent effect of governance on either income, growth, or the volatility of growth,⁵⁴ and an additional six seem to do so after initially negative shocks.⁵⁵ Central & Southern African is heavily represented in the former group, and thus seems more sensitive to RIGHTS (in the benevolent direction). In five countries, governance seems to have a perverse relationship with output.⁵⁶ For example, Colombia's RGDP improved rather persistently as RIGHTS continued to worsen. Indeed, South American countries seem to be represented heavily in this group. The remaining countries show patterns generally too complex or ambiguous to draw out an association between governance and output. Asian countries, in particular, seem to have relationships which are less easy to categorize. This difficulty may arise from Asia's relatively mid-range governance values, where (see above) the income-governance correlations are more ambiguous. Figure 9 shows by example the heterogeneity of income-governance relationships. In El Salvador, the levels of both rights and growth seem to track each other fairly closely over the time period studied. In Benin, the sharp improvement in rights in 1991 noticeably reduces the volatility in growth over the next eight years. Yet Peru seems to yield opposite conclusions – both the largest swings in growth rate and the worst growth performance occur during the “best” political period in the country.

INSERT FIGURE 9 ABOUT HERE.

3.2 Using Measures of Governance in the Context of Dynamic Analysis

There are thus many aspects of governance that may matter for economic development, and that are sometimes measured. Ideally we would like to control

⁵⁴ Benin, Bolivia, Burkina Faso, Burundi, Chile, El Salvador, Guyana, Mauritania, Mauritius, Mexico, Senegal, S Africa, Zimbabwe.

⁵⁵ Argentina, Greece, Hungary, Mali, Philippines, and Uruguay.

⁵⁶ Cameroon, Colombia, Malaysia, Peru, and Venezuela.

for all possible dimensions (subject to the availability of sufficient degrees of freedom), precisely in order to avoid unobserved effects difficulties. The difficulty is that data coverage prevents this from being feasible.

The first question we therefore face is whether this is terminal to any attempt to explore the link between governance and economic development? One condition under which we might proceed with analysis is where there are governance measures that have long time runs, and wide country coverage, and that are highly correlated with other dimensions of governance. Such measures might usefully proxy for the full set of governance variables relevant to development, provided that we bear in mind the importance of unobserved effects (heterogeneity Type I).

When trying to reduce a large number of indicators to a smaller number, there are broadly two strategies available. The first one builds on theory.⁵⁷ The choice of variables may also be driven by how well some combination of governance indicators predicts some other outcome of interest. For example, Jeffrey Sachs' "growth competitiveness index" selects those governance measures that most effectively predicted growth over a previous time period.⁵⁸

A second approach is empirical. It takes large batches of information and asks about the regularities and patterns that seem to be there. The driving idea in this second approach is to use statistical techniques to develop summary scores, factors, and dimensions. This paper follows the second approach.

After many statistical explorations, including considerable attention to probability distributions, outliers, and bimodality, we find that the most governance variables are not symmetric around the means, i.e., skewed to the right or to the left. In general, the probability distributions are relatively stable over time for most variables and skewness seems not severe for some key governance variables. Univariate descriptive statistics for several key governance indicators are presented in Table 5.

INSERT TABLE 5 ABOUT HERE.

⁵⁷ For example, someone may have a theory of governance that gives great weight to participation and democracy, but little to stability and bureaucratic efficiency. Another may simply focus on the prevalence of corruption.

⁵⁸ Sachs chapter in World Economic Forum volume, Feb. 2002. It is "theoretical" in that it includes data that "seem to be" related to governance—or that someone else has thought it so related. It does not include data about health care or education, for example.

The distributions of the World Bank's six composite indicators – voice and accountability (VA), political stability (PS), government effectiveness (GE), rule of law (RL), regulatory quality (RQ), and controlling corruption (CONCOR) – are less deviated from a normal distribution. Nevertheless, they also tend to slightly skew to the right or to the left, excluding voice and accountability variable.

Rank correlations reveal fairly stable country ranks over time for most governance variables, excluding some economic policy-related indicators. For example, country rankings are especially stable for rights-related variables (polity, political rights, and civil liberties) and the World Bank's composite indicators. For these variables rank correlations exceed 0.95. However, country rankings for Heritage Foundations governance indicators tend to highly fluctuate over time, excluding composite economic freedom and property rights indices. For these two variables rank correlations fluctuate around 0.9.

INSERT TABLE 6 ABOUT HERE.

We also examined the correlations among different measures of governance. Correlations for the key governance variables are presented in Table 6. This reveals that the many measures of governance are correlated in the 0.6 to 0.9 range, with the exception of some variables that include so-called second-generation governance measures. For example, using data from 2002, the bivariate correlations of the Heritage Foundations composite economic freedom index with the Freedom House's civil liberties indicator and the World Bank's regulatory quality composite indicator are 0.72 and 0.83, respectively. We also explored the correlations among the World Bank's six composite governance indicators derived in the best data reduction exercise to date (Kaufmann et al. 1999a and 1999b). Using data from 2002, the bivariate correlations among six composite variables ranged from 0.73 to 0.97. For example, "government effectiveness" and "rule of law" are correlated 0.97; "voice and accountability" and "controlling corruption" are correlated 0.80. Our analyses also reveal that the bivariate correlations among many governance measures tend to increase over time. For example, based on data from 1996 and 1998, one may conclude that there are no or weak correlations between the Freedom House's rights variables and the World Bank's corruption composite indicator. However, based on data 2000 and 2002, one also may conclude that these measures of governance are highly correlated. How we could explain these? We presume, these can be explained by improvements in data collection and decreasing measurement error in the data. Probably, these variables have strong correlations in nature;

however, excessive measurement error in parts of earlier data might lead to the observed weak correlations between them.

For another example, the correlation between two rival indices of country competitiveness in the World Economic Forum's annual Global Competitiveness Index pioneered by Jeffrey Sachs and John MacArthur and the Business Competitiveness Index pioneered by Michael Porter – is above 0.9.⁵⁹ Based on data through early 1990s, Klitgaard and Fedderke (1995) found correlations exceeding 0.8 between measures of democracy and measures of corruption.

How might we interpret these correlations? These are imprecise measures. Each suffers from (unknown) measurement error. For normally distributed data the observed correlation between two variables is equal to the “true” correlation between such variables if perfectly measured times the square root of the product of the reliability coefficients for each variable.⁶⁰ Suppose two variables are each measured with reliability of 0.8, and we observe a correlation of 0.6 between them. Our best guess of the “true” correlation is the observed correlation divided by the square root of the product of the reliability coefficients, or $0.6/0.8=0.75$. For many social data, reliability is not above 0.8 to 0.9. So observed correlation coefficients of 0.6 to 0.8 are high, given the unreliability of measurement. Putting it another way: we would be hard-pressed to say that these highly correlated variables are measuring very different things.

Two of these variables are available for many countries over a long time period (1972 to today): the measure of political rights and of civil liberties developed by Raymond Gastil and now continued by Freedom House. They correlate between 0.55 and 0.91 with the World Bank's six composite measures. The canonical correlation between the six World Bank indicators and the two Freedom House indicators is about 0.9 and relatively stable over time (see Figure 10).

INSERT FIGURE 10 ABOUT HERE.

In our analysis of the broader set of governance measures, we transformed many of the variables to prepare them for factor analysis. In various samples of countries and combinations of variables, we factor analyzed the governance measures, excluding a few of the bizarrely performing second-generation measures. In these analyses, a single factor consistently explained “most” of the

⁵⁹ World Economic Forum (2004). The correlation is from the 2001-2002 report.

⁶⁰ A reliability coefficient of 1.0 would indicate perfect agreement and no measurement error, 0 would indicate pure measurement error and no agreement.

variances, and the two Freedom House measures consistently “correlated highly” with this factor.

Because the Freedom House measures are available for more countries and more years – and because these measures are highly correlated with other imperfect measures of “governance” – we select the two Freedom House variables as our indicators of good governance. The general characteristics of the values of these two variables are presented in Table 7 and Table 8, respectively. We gained statistical power, but we sacrificed information. As a result, it is possible that using just the two variables we can be misled about certain countries.

INSERT TABLE 7 ABOUT HERE.

INSERT TABLE 8 ABOUT HERE.

We leave this section with several findings. First, when we have many possible indicators of performance, we may want to reduce their dimensions through statistical analysis. Are all the indicators highly correlated, or are several constellations of measures involved? Correlations need to be assessed in light of the reliability of each measure. When each performance indicator is measured with error, the correlation between variables has a much lower maximum than the theoretical 1.0. Second, most of our governance measures turn out to be “highly correlated,” especially when we take into account the measurement error in each of them. Third, we choose two of the measures, civil liberties and political rights, not for their methodological excellence or theoretical attractiveness, but because of their statistical properties and wide coverage. Each of them correlates “highly” with most other governance measures and with the first factor from a factor analysis of all governance measures, and both these measures are available for many years and many countries. Fourth, even so, using these two measures alone misses what is happening in certain countries, such as the Andean countries (challenged as they are by drugs and ethnic inequalities) and others such as Singapore. We should compare our results with other analyses based on more limited data but a greater range of variables.

Rewording these results for other performance indicators: First, especially when we don’t have a strong theory to define “performance,” we should carefully examine many possible measures. In the process, we should take account of outliers, long-tailed distributions, bimodality, measurement error, and other troubling features of the data. Second, factor analysis and other multivariate techniques can be useful to see which measures agree how well, and to explore whether “performance” appears to be multidimensional. Third, when one factor

captures most of the variance, then we may wish to select a few measures that correlate highly with that factor and are widely available, so as to open up more countries, and longer time runs to analysis. Finally, if we do choose just a few measures, we sacrifice information. We should examine how some performers deviate from the rest of the population along certain dimensions. And we should compare our results with using a broader set of measures.

4.0 Estimation and Results

Estimation of (2.1) immediately raises an additional empirical constraint. International data series abound in providing measures of output – the same cannot be said for the real physical capital stock. Instead, data is available only in real investment format. It follows immediately that inference based on the implicit production function of (2.1) is compromised. Two options that preserve a role for capital present themselves: abandoning the production function justification and treating the expression as a reduced form of output determination in investment and governance, or moving explicitly to a growth equation.⁶¹ A third option would be to estimate the relationship, but to drop the physical capital stock dimension from estimation. While we allow for all possibilities, we note that the interpretations of the resultant coefficients vary.⁶²

In section 2 we highlighted a number of methodological concerns. We address each of the methodological concerns, while focusing on the substantive questions of whether governance matters for economic activity, how it so matters, and with what strength. In section 4.1 we consider results from pooled OLS estimation, test for the presence of unobserved effects, and correct where appropriate by means of FE estimation. Section 4.2 extends the analysis by considering the impact of endogeneity, by means of GMM estimation. In section 4.3 we begin the analysis of possible cross-country heterogeneity by means of clinical country-by-country estimations employing time series techniques of analysis. Section 4.4 extends the possibility of cross-country heterogeneity by considering DFE, MG and PMG estimator results, and considers the possibility of non-linearity in greater detail. Finally, section 4.5 considers the evidence on economic growth as opposed to the level of economic development, utilizing the methodological lessons learnt from the former

⁶¹ Where the latter route is adopted, note that the implicit question is then whether it is the *level* of governance that is relevant to growth (as would be implied by the production function formulation (2.1)), or whether it is changes in governance that drive output growth.

⁶² Justification for the agnosticism is that theory provides little guidance here.

4.1 Addressing Heterogeneity Type I: the impact of unobserved effects

We begin by estimating equation (2.1) in linear form, such that we have:

$$Y_{it} = \beta_0 + \beta_1 \left(\frac{dK_{it}}{dt} \right) + \beta_2 R_{it} + \sum_{j=1}^k \gamma_j X_{j,it} + \varepsilon_{it} \quad (4.1)$$

where notation follows the convention thus far, and the X_j denote k additional explanatory variables thought relevant to output determination.⁶³

We present two separate sets of results.⁶⁴ In the first, we estimate under the exclusion of the $X_{j,it}$ -vector (impose $\gamma_j=0 \forall j$), but employ both the two Freedom House rights measures separately (POLRIGHT, CIVLIB), and the linear combination of the two (RIGHTS). Second we lift the restrictions on the γ_j , but repeat estimations both with and without the investment variable (restricting $\beta_1=0$), and with the composite rights variable and the two separate rights variables.

Our X_j vector includes indicator variables for geographical location (we employ dummies for sub-Saharan Africa, denoted C&SAfrica, the Middle East and North Africa, MENA, Central America and the Caribbean, denoted CAmer&Carib, South America, denoted SAmer, and Asia, denoted Asia),⁶⁵ British colonial origin (Britain), and most recent date of state formation (StateForm2). We further include four measures from the standard Barro-Lee (1994) human capital data set on the level of and investment in human capital, the percentage of the population without schooling (denoted LU), the percentage of the population that has completed primary schooling (denoted LPC), the percentage of the population that has completed secondary schooling (denoted LSC), the percentage of the population that has completed high school (denoted LHC), and the average years of education in the population (denoted TYR).⁶⁶ Finally, we incorporate three proxies of the quality of the economic policy environment, given by a measure of openness as measured by exports as a percentage of GDP (denoted EXP),⁶⁷ a measure of the investor-friendliness of economies as indicated by the level of foreign direct investment measured as a percentage of GDP (denoted FDI), and

⁶³ Estimation in Section 4.1 is by means of PCGIVE.

⁶⁴ In a third variant we repeated the two sets of specifications reported, but under a $\beta_1=0$ restriction. Since the results do not differ materially from those reported, we do not discuss them here for the sake of parsimony. Full results are available from the authors.

⁶⁵ The control group is thus North America, North, West and Central Europe.

⁶⁶ The Barro-Lee data set contains data point only at 5 yearly intervals. Since changes in the human capital dimensions being measured over half decades are gradual for all but a small number of countries, we interpolated linearly.

⁶⁷ On the importance of openness to growth see Sachs and Warner (1995).

the quality of macroeconomic stabilization policy as measured by the inflation rate (denoted CPI).⁶⁸

Given that results employing the two Freedom House rights measures separately provide symmetrical results to those obtained under estimations utilizing the composite rights measure, in the interest of parsimony we report only results for the latter. Table 5 demonstrates that there exists a close association between the two rights measures in our panel of countries. The association between the two rights measures is not only statistically significant, but is approximately proportional. Thus a one unit improvement on the Freedom House scale in civil liberties is associated with an approximately one unit improvement in the political rights variable. This holds regardless of whether we consider simple pooled OLS estimation, correct for fixed effects (FE) and serial correlation (FGLS) (see the results of columns 1, 2 and 3 of Table 9),⁶⁹ or allow for dynamic association between the two rights dimensions by means of PMG estimation (see column 6 of Table 9).⁷⁰

One caveat does bear emphasis in the results. The association between the two rights measures is weakest under poor governance (a composite rights score above 11 on the Freedom House scale – see column 9 of Table 9), strongest in the mid range of governance (a composite rights score above 7 but below 11 on the Freedom House scale– see column 8 of Table 9), and closest to a proportional association for countries with good governance (a composite rights score below 7 on the Freedom House scale– see column 7 of Table 9). The finding is significant since it suggests that the association even between two variables as closely associated as political rights and civil liberties is more attenuated in countries with poor rights, than elsewhere. As we will see in what follows, the distinction between countries with poor rights and others will consistently be of significance in understanding the impact of rights on economic development also.

INSERT TABLE 9 ABOUT HERE.

⁶⁸ Note that in estimation the policy variables enter in logarithmic transform, in order to minimize the impact of outliers and strong right tailed distributions.

⁶⁹ Given the presence of significant autocorrelation, we report only robust standard errors – as throughout this study.

⁷⁰ Note, however that the difference between FE and FDOLS results gives an indication of the unsurprising possibility of significant simultaneity between the two rights measures. Since our concern is merely to establish close association between the measures here, not the establishment of the precise form of the association, we note with interest, but due not pursue the question further at this point. Individual country time series evidence below does contain further information on the association between different measures of governance.

Table 10 reports results for the log of real output. Column (1a) presents results under the $\gamma_j=0 \forall j$, restriction. It confirms a statistically significant positive impact of investment on real per capita GDP, and a statistically significant benevolent impact of rights (readers should bear in mind that high is bad on the Freedom House scale). In the case of investment the impact is also economically significant, with a 1% increase in real investment generating a 0.79% increase in real per capita GDP.⁷¹ The semi-elasticity of rights is variable, with a 1% increase in rights having a positive impact on output of 0.56% at the worst possible rights rating (Freedom House score of 14), which declines to an elasticity of 0.08% at the best possible rights rating (Freedom House score of 2).⁷² Figure 11 illustrates the variable elasticity over the rights range as *beta-high*.

INSERT TABLE 10 ABOUT HERE.

INSERT FIGURE 11 ABOUT HERE.

First results thus confirm a benevolent association between rights and economic development – though the strength of the association diminishes as the quality of governance continues to increase. Governance does thus appear to suffer from diminishing returns in terms of its impact on real per capita output.

But are the results merely an artifact of omitted dimensions we know to be important for long run economic development? In order to test this possibility, we introduce three sets of additional regressors often advanced in the literature: geographical location and colonial heritage, education and human capital measures, and three measures of economic policy. Columns (1b) through (1e) of Table 6 include the three sets of additional regressors both singly, and jointly. The investment elasticity is not statistically significantly affected by the inclusion of the additional explanatory variables. While the impact of rights on real output remains statistically significant, it approximately halves in economic significance, suggesting the possibility of some interaction between governance, education and policy – a possibility that we return to later in our analysis. Nevertheless, the impact of rights on output remains non-negligible, with an elasticity which declines from 0.28% to 0.04% over the rights range of 14 to 2.⁷³ Figure 11

⁷¹ We explored a number of possible specifications, including ones in which investment is not subjected to the log transform. While providing variable elasticities none of the results reported are materially affected. Full results are available from the authors on request.

⁷² Estimation under a constant elasticity specification returns a coefficient of 0.29, consistent with our variable elasticity results.

⁷³ A constant elasticity specification returns a coefficient of 0.17.

illustrates the revised rights elasticity over the full range of rights, providing a new lower bound (*beta-low*) under the pooled OLS estimators.

Few of the additional regressors prove to be statistically significant, though the evidence suggests that later state formation is associated with lower economic development, that countries Asia have lower per capita GDP than can be accounted for by investment, governance, education and economic policy variables, and average years of schooling appear to be positively correlated with real per capital GDP. Policy variables prove to either insignificant (exports as a percentage of GDP), or have a perverse though weak association with per capita GDP in the case of inflation.⁷⁴ Foreign direct investment is negatively associated with real per capita GDP in at least one specification, and with a relatively strong elasticity of -0.47. Two possible interpretations of this finding are either that FDI comes to crowd out domestic investment, or that returns on FDI decline in per capita GDP, making less developed countries a more attractive target for FDI flows than more developed.⁷⁵

Of greater concern is the presence of serial correlation at least in the output levels estimations – the test for unobserved effects bias and inconsistency suggested by Wooldridge (2002:264).⁷⁶ Consideration of the AR(1) test statistic for (1a) through (1e) of Table 10, suggests the possibility of unobserved effects being present.

We therefore proceed with FE estimation under the Within estimator, in order to correct for potential unobserved effects bias and inconsistency under pooled OLS. Results are reported in column (2) of Table 10 for simple FE, while column (3) reports results under correction for the presence of serial correlation

⁷⁴ There is some weak evidence of a concave relationship between inflation and real per capita GDP, which we explored in some depth. While theoretically appealing (by implying an optimal inflation rate), the result is unfortunately not robust to simple sensitivity tests, and we therefore abandoned the possibility. Full results available from the authors on request.

⁷⁵ The literature has suggested both possibilities. The declining marginal return on capital follows immediately from any standard Solow-Swan like growth model, though note that a number of authors have suggested endogenous growth models that reverse the declining returns on factors of production in the context of FDI flows. See for instance De Mello(1997) and Ramirez(2000). The same authors raise the possibility of crowd-out, though generally empirical findings have suggested positive technology spillovers and an absence of crowd out. See also Blomstrom, Lipsey, and Zejan (1994) and Borensztein, De Gregorio, and Lee (1998).

⁷⁶ Suppose:

$$Y_{it} = \beta_0 + \beta_1 \left(\frac{dK_{it}}{dt} \right) + \beta_2 R_{it} + \sum_{j=1}^k \gamma_j X_{j,it} + u_{it} \quad (4.1')$$

where the presence of a time invariant unobserved effect renders $u_{it} = \varepsilon_{it} + c_i$, where c_i denotes the unobserved effect. AR(1) follows immediately.

(FEGLS).⁷⁷ Estimation under FE significantly alters coefficients both in statistical and in economic terms. The elasticity of output with respect to investment halves in magnitude under FE estimation relative to pooled OLS, with a 1% increase in investment being associated with a 0.3% increase in real per capita GDP (under pooled OLS the coefficient was 0.7-0.8). While investment remains at least statistically significant, rights lose both statistical as well as economic significance (the variable elasticity ranges from 0.01 to 0.002 – see *beta-multi* of Figure 11 to place the magnitude in relation to those obtained under pooled OLS), though it maintains its benevolent sign.

Under FEGLS estimation, correcting for serial correlation, Asia and sub-Saharan Africa are statistically significantly poorer in real per capita GDP terms than the rest of the world, four of the education variables are statistically significantly and positively related to real output (primary school, secondary school and high school completion, as well as average years of education in the population), while our openness proxy (exports as a percentage of GDP) is positively related to output, and the negative elasticity (elasticity of 0.11), while the negative impact of FDI approximately halves from that obtained under pooled OLS (from an elasticity of -0.47 to -0.18).

Consideration of the diagnostic statistics provided by AR test statistics confirm the appropriateness of correcting for unobserved effects. The results from FE estimation suggest that concerns about possible bias and inconsistency of estimation under pooled OLS are justified. The impacts of governance as well as investment both change significantly in both statistical and economic terms when FE estimation is applied.

Yet the FE correction does not yet suffice for our purposes. Recall that our methodological discussion (section 2.1) noted that where the error term is correlated with any of the explanatory variables over any two time periods, FE would cease to be unbiased and consistent.⁷⁸ One test for the presence of such correlation between error term and explanatory variables is suggested by Wooldridge (2002:284f). The FE and the FD⁷⁹ estimators generally have the sample probability limits, except where the error term and explanatory

⁷⁷ While we report only the specification controlling for all additional possible regressors, in all instances we also estimated under the full $\gamma_j=0 \forall j$ restrictions, as well as the partial versions discussed under the pooled OLS results of Table 10. Results are symmetrical to those discussed in the text, but full results are available from the authors.

⁷⁸ Reasons include time varying unobserved effects, measurement error, and endogeneity.

⁷⁹ OLS first difference.

regressors covary.⁸⁰ Under these conditions both the FD and the FE estimators are inconsistent, and they have different *plims*.

Table 10 column (4) reports the result of applying the FD estimator to the output levels equation. Parameter estimates vary strongly from FE estimates – compare the results of column (3) and (4) of Table 10. The elasticity of investment falls further to approximately a third of that estimated by FEGLS in Table 10, (0.12 as opposed to 0.31), though it maintains its statistical significance, while the significance of the rights variable reappears at the 10% level, though it remains at the economically insignificant levels obtained under FE in the multivariate specification.⁸¹

Comparison of the results obtained under the FE and FD estimators thus carries the implication that not only do we face unobserved effects (heterogeneity Type Ia of the Table 1 classification), but that the exogenous variables may be contemporaneously correlated with the error term (heterogeneity Type Ib of the Table 1 classification). Under this diagnosis GMM estimation is potentially appropriate, since the divergence between FD and FE estimators does not allow us to dismiss the violation of the exogeneity assumptions the FE estimator invokes.

For this reason it is as yet difficult to draw substantive conclusions concerning the link between governance and economic development. Nevertheless, accounting for heterogeneity in unobserved effects appears to dismiss the importance of governance for economic development – in both statistical and economic terms, and against the initial promise of a relatively strong effect offered by standard pooled OLS estimation.

4.2 Addressing Heterogeneity Type Ib: violation of exogeneity

Introduction of governance into a production function context suggests that a violation of the assumption of strict exogeneity, viz. that explanatory variables in each time period are uncorrelated with the errors in each time period, $cov(R_{is}, e_{it}) = 0 = cov(K_{is}, e_{it}), s, t=1, \dots, T$, is possible, even probable. Measurement error in the governance dimension, feedback from economic development to governance (and other dimensions), and time varying unobserved effects are all mechanisms by which strict exogeneity might come to be violated. The divergence between

⁸⁰ Presuming that they do not vary due to sampling error.

⁸¹ While we do not report the results, use of the second difference estimator of Heckman and Hotz (1989) confirms the conclusions from the FD estimator. Full results are available from the authors.

FD and FE estimators above suggests that such a violation finds corroborating evidence in the current application.

We address the issue by means by employing the GMM estimator.⁸² We employ the 2 step estimation procedure for small sample correction.⁸³ Table 11 reports results. Estimation employs individual effects, while time dummies are included to control for exogenous shocks - Wald test statistics confirm the inclusion of both forms of dummy variables. While we estimated under both the first difference (FD) and orthogonal deviation (Orth)⁸⁴ transformation of individual effects, with one exception we report the results based on orthogonal deviation.⁸⁵ Wald test statistics further confirm the joint significance of the regressors, while crucially the Sargan test statistics does not reject the null hypothesis of valid instrumentation.⁸⁶

INSERT TABLE 11 ABOUT HERE.

In columns (1) and (2) of Table 11 we report results employing lagged values of all variables (including real output) as instruments (though excluding the dummy variables on the RHS), under the FD and orthogonal transformations.⁸⁷ The elasticity of output with respect to investment retains statistical significance, and lies somewhat above that obtained under FE estimation (the elasticity ranges from 0.37 to 0.46 as opposed to the elasticity of approximately 0.30 under FE). As for FE, the rights variable is both statistically and economically insignificant.

A notable difference to FE and FEGLS results is that particularly the geography and policy, but also the human capital variables play a stronger role. Both

⁸² Estimation in Section 4.2 is by means of PCGIVE. Again for the sake of parsimony, we report only results for the composite RIGHTS measure – given the consistent comparability of the results for the two individual and the composite rights measures. Full results remain available from authors on request.

⁸³ See Windmeijer (2000).

⁸⁴ Orthogonal deviations express each observation as the deviation from the average of future observations in the sample for the same country, and weight each deviation in order to standardize the variance. See Arellano and Bover (1995).

⁸⁵ This reflects both the broad consistency of the results obtained from the Orth-transform, and the marginally better stability of the Orth-based results.

⁸⁶ Note that we employ both levels and the standard FD GMM instruments. This is valid where lagged levels of the instruments are uncorrelated with the individual effects, but correlated with the dependent variable. The Sargan test statistic validates the choice. The Sargan test is asymptotically chi-square distributed, with degrees of freedom given by the number of overidentifying restrictions under the null of valid instrumentation. The Sargan test is heteroscedasticity-consistent only under the two-step GMM estimator.

⁸⁷ Here too we examined evidence from more parsimonious specifications, excluding the geography, policy and human capital variables. No results reported above are materially affected by such exclusions. As always full results are available from the authors.

exports and FDI report strong positive elasticities (in the 0.3-0.4 and 0.5-0.7 ranges respectively). A clear direct impact of policy on real output thus emerges, and under GMM estimation the somewhat discordant negative coefficient on FDI is reversed. Sub-Saharan Africa, MENA, Central America and the Caribbean countries all have lower levels of economic development than predicted given their levels of investment, quality of policy and human capital investment. South America has a higher level of real output, while the Asian countries return ambiguous results depending on the transformation employed.

Recall from our methodological discussion that the use of lagged values of the dependent variable is valid only in the absence of serial correlation, and of sequentially exogenous variables only where there is no contemporaneous correlation with the error term. Evidence of AR(1) remains under both columns (1) and (2) of Table 11, while our methodological discussion has already noted that the impact of governance may be persistent and pervasive, such that the assumption of no contemporaneous correlation between rights and the error term may well come to be violated. Similar argument might be extended to investment expenditure. For this reason, columns (3), (4) and (5) of Table 11 cumulatively exclude the log of real output, rights, and the log of real investment from the list of instruments. Column (6) excludes rights and investment only – since the test for autocorrelation under GMM is based on the first differences of the residuals. An absence of serial correlation thus corresponds to significant negative first order, but no second order serial correlation. This is what we observe in columns (1) and (2).

Modification of the set of instruments maintains the significance of investment, with an elasticity of 0.35-0.40, while the statistical significance of the rights variable is restored. Moreover, the strength of its impact on real per capita output is restored, from an elasticity of 0.42 at a Freedom House right index value of 14, declining to 0.06 at an index value of 2. Figure 11 illustrates that this (beta GMM) lies between the upper and lower bound values obtained under pooled OLS estimation.

Geographic effects remain present and primary school education continues to be positively associated with real output.⁸⁸ What is noteworthy is that all three policy variables now prove to be significant, and of benevolent sign with respect to output. Exports and FDI are positively associated (elasticities are

⁸⁸ Note incidentally that the British colonial heritage is by no means robustly benevolent – we readily obtain a negative and statistically significant impact of a British colonial past.

approximately 0.30 and 0.50 respectively), inflation negatively so (elasticity of approximately 0.06).

What emerges from this section is that accounting for possible measurement error, for time varying unobserved effects and possible simultaneity, carries substantial implications for estimated results. The impact of investment stabilizes, and the significance of governance which appeared to dissipate under FE estimation returns, while its economic significance is also reestablished. In addition, the policy variables included in the present study all come to be statistically significant, of the anticipated benevolent sign, and in the case of openness and FDI with relatively strong elasticity values.

4.3 Addressing Heterogeneity Type II: introducing dynamics; the time series evidence

Our approach to heterogeneity type II is by two distinct steps. We begin by considering country-specific time series evidence, to establish the plausibility of the heterogeneity type II hypothesis. The present subsection is devoted to this evidence. Once we have considered the implications of the time series evidence, we modify our panel estimators appropriately in section 4.4 of the paper.

Advantages of standard time series techniques of analysis are threefold. First, consideration of the univariate time series characteristics of the data gives an initial indication of feasible forms of associations between the variables. This gives a first indication of the possibility of heterogeneity of type II. Second, estimation by means of standard VECM techniques provides further evidence on the exogeneity of explanatory regressors. Finally, VECM estimation is explicitly dynamic, and offers the means of choosing optimal lag structures in estimation.

We also take the opportunity to explore the possibility of diverse forms of association between the political rights and civil liberties Freedom House measures. For the time series section only we therefore employ the two measures separately rather than in the composite linear combination form, unless otherwise indicated.

For the analysis of section 4.3 alone we have extended our sample from 66 countries, to 162 countries, without loss of generality, since the requirement of a

balanced panel was no longer binding.⁸⁹ Note that the nature of the sample is thereby considerably altered, incorporating many developed countries. Use only of the 66 countries included in our panel leaves the general conclusions of the time series analysis intact –though on many fewer data points.

4.3.1 The univariate time series structure of the data

We begin with a consideration of the stationarity properties of the variables included in this study, on a country by country basis. To do so we report augmented Dickey Fuller (ADF) test statistics.⁹⁰ Table 12 provides a summary representation of our findings, with the order of integration of the variables employed for this study rated across the stationarity $I(0)$, stationarity in first difference $I(1)$, and stationarity under higher orders of integration $I(>1)$ possibilities.⁹¹

INSERT TABLE 12 ABOUT HERE

The implication of the results represented in Table 12 is that heterogeneity type II *must* be present in the sample of countries considered for this study. First, countries manifest different combinations of orders of integration across the two economic, and the two Freedom House rights measures employed for this study. Three sets of countries constitute the majority of cases. Group IV countries in Table 12 have stationary governance variables, but first difference stationary economic variables. For Group V countries both governance and economic variables are first difference stationary. In Group VIII governance variables are first difference stationary, but economic variables that are integrated under higher orders. While these three groups account for most countries we have considered in this study, we also note that there are at least six other feasible combinations of stationarity conditions governing the governance and economic variables – and every combination has at least one country that falls into these categories.⁹²

⁸⁹ Estimation in Section 4.3 is by means of MICROFIT. Where relevant, we provide remarks on the sensitivity of results to consideration of the 66 country sample employed in our panel, rather than the full set of 162 countries.

⁹⁰ Full results are reported in Appendix 2. Note that where ADF test statistics were ambiguous, testing was supplemented by Phillips-Perron, autocorrelation function, and spectral density statistics.

⁹¹ Note that some countries are classified in more than one cell of Table 8. This is because per capita output, investment, and the two Freedom House rights variables are all individually characterized in terms of stationarity, such that in both the economic and governance dimensions a country can fall into two order of integration.

⁹² Note that here we have followed the data diagnostics strictly, and have not allowed theoretical considerations to override the empirical evidence. Full clinical investigation that pays complete attention to

It follows immediately that the form of association between governance as measured by the Freedom House indicators, and economic performance must differ between the various groupings of countries. For instance, in Group IV countries, governance with its stationary mean and variance simply *cannot* account for the level of output and investment in these countries, given their first difference stationary mean and variance. Instead, governance can only be associated with growth in output and investment. Group VIII countries are symmetrical, except that in this case growth in governance can at best be associated with the rate of change of the economic dimensions. Group V countries are those in which the level of rights is plausibly associated with the level of output and investment. Symmetrically for other country groupings.

Heterogeneity type II follows of necessity: since the form of association between governance and economic performance is shown to be different across countries, estimation of the association in any given specification of the variables must face different coefficient values across the countries included in the sample.⁹³

In what remains of section 4.3, we take the univariate time series evidence seriously. Our focus is on the three groupings that capture the majority of the countries considered, Groups IV, V and VIII, though we conclude with some brief additional remarks about the remaining groupings. The purpose of the further investigation is to examine what more can be said, if anything, about the association between governance and economic performance in these groupings.

4.3.2 Group IV: stationary governance, difference stationary economic dimensions

Two features strike about this 30 country grouping. First, note that the group contains a strong clustering of developed democracies, with high levels of rights in both the political rights and civil liberties dimensions.⁹⁴ In general, the

country specific evidence, may override some of the diagnostics on a priori considerations, and in the light of special circumstances that apply to the context.

⁹³ There is a second sense in which heterogeneity type II is assured. As Table 4 illustrates, some countries fall into more than one grouping – either because the two alternative rights measures may be integrated of different orders, or because the output and investment variables are integrated of different orders. Again, the implication is that heterogeneity type II is present, but in this instance not only between, but even possibly within countries.

⁹⁴ The mean of RIGHTS in this group of countries is 5.45, lower than the average of 8.5 in our panel of 66 countries. When Algeria, Burundi, Oman, Rwanda and the UAE are omitted from Group IV, the mean of RIGHTS falls further to 4.16. Of the Group IV countries, 12 average a Freedom House political rights score of 1 over the 30 year period of our sample.

developed countries in this grouping show either very little, or no variation at all over time in the governance dimension. However, the group is not confined to developed democracies, and also contains North African and Middle Eastern, South Asian and Latin American countries. A number of these are not only less developed, but have a much lower level of mean rights than the developed democracies, though all countries show very low variability in rights. Independently of the quality of governance be in these countries, therefore, governance appears to be stable over time.

The second is that given the divergent orders of integration across governance and economic performance measures, the level of governance cannot be associated with the level of output and/or investment. But perhaps the growth performance of countries in this grouping differs, depending on the level of rights?⁹⁵ To test this proposition we regress mean output growth in our 30 countries on mean rights, singly in the political rights or civil liberties dimensions, and against the mean composite rights measure.⁹⁶ We report the results in Table 13.

INSERT TABLE 13 ABOUT HERE

Columns (1) through (3) of Table 13 report the results for all 30 countries in Group IV. Within our 30 country sample, countries with better rights statistically have a higher average growth performance. Moreover the association is economically significant also. Moving from the country with the worst average level of rights, Burundi with a mean score of 6.76, to countries with the best rights, the OECD countries with mean scores of 1 on the rights scale, would increase the average growth rate in real per capita GDP by approximately 1.73% per annum. The impact is invariant to the political rights or the civil liberties measure. Use of the composite rights measure strengthens the impact of full democratization (moving from 14 to 2 on the rights scale) marginally to 2.04%.

One possibility is that the result is merely due to the presence of outliers.⁹⁷ Of our 30 countries, the UAE has an exceptionally low growth rate. Perhaps the

⁹⁵ This would render all regressors stationary, allowing standard estimation techniques to be employed in principle. However, the virtually complete absence of variation in the governance dimension, precludes the use of standard estimators.

⁹⁶ In this sample of countries CIVLIB and POLRIGHT are closely related. A regression of POLRIGHT on CIVLIB returns a partial correlation coefficient of 1.07, with standard error 0.05. Use of both measures jointly augurs problems of multicollinearity.

⁹⁷ We also tested for the possibility that the volatility of rights might eliminate the rights –growth association by including a measure of the standard deviation of rights for each country in estimation.

benevolent association between governance and growth is due to its influence? We repeated our regressions without UAE in the sample – results are reported in columns (4) through (6) of Table 13. The benevolent and significant association between mean output growth and mean levels of rights proves robust to UAE's exclusion, though it is slightly weaker both statistically and economically. The move from Burundi to Switzerland now raises real per capita growth by approximately 1.2% per annum on the single rights measures, and 1.32% on the composite rights measure.

We conclude by noting that Group IV countries are undoubtedly different from Group V countries, say. But within Group IV, countries with better rights do grow more rapidly (within the 1.2 - 2% per annum range), than countries with poor rights.

4.3.3 Group V: difference stationary governance, output and investment

Group V countries are those that lend themselves to estimation by standard time series techniques, since all variables are first difference stationary. Johansen⁹⁸ techniques of estimation are now standard, so that discussion of estimation methodology here can be brief. We employ a vector error-correction (VECM) framework, for which in the case of a set of k variables, we may have cointegrating relationships denoted r , such that $0 \leq r \leq k - 1$. This gives us a k -dimensional VAR:

$$z_t = A_1 z_{t-1} + \dots + A_m z_{t-m} + \mu + \delta_t \quad (4.2)$$

where m denotes lag length, μ a set of deterministic components and δ a Gaussian error term. While in general z_t may contain $I(0)$ elements, given our bivariate association, as long as non-stationary variables are present we are exclusively restricted to $I(1)$ elements. Reparametrization provides the VECM specification:

$$\Delta z_t = \sum_{i=1}^{k-1} \Gamma_i \Delta z_{t-i} + \Pi z_{t-k+1} + \mu + \delta_t \quad (4.3)$$

The existence of r cointegrating relationships amounts to the hypothesis that:

$$H_1(r) : \Pi = \alpha\beta' \quad (4.4)$$

where Π is $p \times p$, and α, β are $p \times r$ matrices of full rank. $H_1(r)$ is thus the hypothesis of reduced rank of Π . Where $r > 1$, issues of identification arise.⁹⁹

However, the association between rights and growth remains statistically significant, the economic significance virtually unchanged, while the variability of rights has no statistical impact on mean growth.

⁹⁸ See Johansen (1991) and Johansen and Juselius (1990).

⁹⁹ See Wickens (1996), Johansen and Juselius (1990, 1992), Pesaran and Shin (1995a, 1995b), Pesaran, Shin and Smith (1996).

While our chief concern here is with the existence of cointegration, hence the possibility of long-run equilibrium relationships contained in $\beta'z_t$ we are also interested in the possibility of heterogeneity between countries in terms of the *nature* of the link between economic activity and rights.

In all instances we test for the presence of a link by means of the standard reduced rank Johansen procedure, and verify the presence of the relevant link(s) between rights and output by means of both overidentifying restrictions, and in terms of tests for weak exogeneity.

INSERT TABLE 14 ABOUT HERE

Table 14 provides the summary results, specifying the estimated elasticities between output and the rights measures, at the minimum level of rights reported by the country, the maximum rights as well as mean rights.¹⁰⁰

We begin estimation by considering the possibility of association between the two individual rights measures (POLRIGHT, CIVLIB). The test examines the direction of association between the rights measures, if any.¹⁰¹ Very few countries in Group V report any systematic relationship between the two rights measures. Those for whom such a relationship is present are Benin, Bhutan, Cambodia, Guinea Bissau, India, Iran, Jamaica, Jordan, Malaysia, Mozambique, Nicaragua, Niger, Portugal, South Africa, Spain, Sudan, and Venezuela. These countries required a separate approach to estimation only where more than one cointegrating vector is present when both individual rights measures are included in the (4.1) specification. Where this is not the case, we continue to use the composite rights measure in estimating (4.1) as a VECM structure, and no identification issues as in (4.5) arise. However, where more than one vector is present, for the Π matrix of (4.4) we estimate:

$$\begin{bmatrix} \ln Y \\ iR \end{bmatrix} = \begin{bmatrix} \alpha_{11} & \alpha_{12} \\ \alpha_{21} & \alpha_{22} \end{bmatrix} \begin{bmatrix} \beta_{11} & \beta_{12} & \beta_{13} & \beta_{14} \\ \beta_{21} & \beta_{22} & \beta_{23} & \beta_{24} \end{bmatrix} \begin{bmatrix} \ln Y \\ \ln I \\ PR \\ CL \end{bmatrix} \quad (4.5)$$

¹⁰⁰ Appendix 3 provides the full results of estimation. Note that in a few instances the presence of a bivariate specification, or very clear indication of a single cointegrating relationship, we were able to estimate the relationship by means of an ARDL cointegrating framework. We indicate where this approach is pursued in Appendix 3. See Pesaran, Shin and Smith (2001) for details on the estimation framework.

¹⁰¹ See Pesaran, Shin and Smith (2001) for details of the test.

where PR denotes $POLRIGHT$, CL $CIVLIB$, and iR denotes the rights index that is indicated as the outcome variable by the F-test. Overidentifying restrictions are $\beta_{11}=1=\beta_{23}$, $\beta_{14}=0=\beta_{21}=\beta_{22}$ for $iR=PR$,¹⁰² and $\beta_{11}=1=\beta_{24}$, $\beta_{13}=0=\beta_{21}=\beta_{22}$ for $iR=CL$.¹⁰³ The impact of rights on output for these countries thus takes a very specific channel – either civil liberties or political rights, while the specific rights variable driving output in turn is determined by the other rights dimension.

Note immediately therefore, that even within the Group V countries for all of whom both governance and economic dimensions are difference stationary, the *structure* of association between the governance measures and output differs between countries. In most instances it is feasible to examine a link between composite rights and output – but for at least some countries the association is only with specific forms of rights, political or civil depending on the country. The presence of heterogeneity between countries already noted under sections 4.3.1 and 4.3.2, viz. that the nature of the association between rights and output is not uniform in structure across countries deepens here.

Since detailed country-specific results are reported in Appendix 3, note only that for the majority of countries a valid cointegrating relationship from governance to real output is confirmed by trace, maximal eigenvalue, and weak exogeneity tests based on the loading matrix (note the valid ECM coefficients) is confirmed.

Here we focus on the implied elasticities that emerged from the estimated relationships. Estimated specifications specify output and investment in log transform, and the rights measures in levels. Hence the rights impact on output is that of a variable elasticity, and in Table 14 we note the estimated elasticity at the minimum (best rights), mean, and highest (worst rights) value of the relevant rights index for each country in Group V.

The immediate implication is that the elasticities are variable not only across the range of the rights indexes that is encountered *within* countries, but also that both the strength of the elasticity and the extent of its variation across the range of feasible rights differs *across* countries. Thus even where the structure of the association between governance and output is the same across countries (in the sense that difference stationary governance is associated with difference stationary economic performance), the *strength* of the impact of changes in governance varies across countries. This provides a first intimation of the

¹⁰² This is the case for Benin, Bhutan, Nicaragua.

¹⁰³ This is the case for Guinea Bissau, Jordan, Malaysia, Mozambique, Niger, Portugal, South Africa, Sudan.

possibility of heterogeneity type III within the Group V country sample – that the strength of the impact of changes in governance varies between countries.

The real question then must be whether the heterogeneity reveals any systematic patterns? Are the estimated elasticities simply randomly distributed across the range of rights, or can we find any general patterns? In order to investigate this issue we consider the relationship between the estimated elasticities reported in Table 14, for mean values of rights per country, and the range of mean rights in various linear and non-linear specifications.¹⁰⁴ We report the results in Table 15. While the goodness of fit measure indicates the high degree of dispersion of elasticities about the fitted relationships, estimation confirms the presence of a polynomial between the elasticity of output with respect to governance found for a country, and the mean level of rights maintained by the country.

INSERT TABLE 15 ABOUT HERE

INSERT FIGURE 12 ABOUT HERE

Figure 12 plots the line of best fit implied by the specification estimated by column (6) of Table 15. The relationship suggests the presence of an inverted U-shape.¹⁰⁵ Figure 12 suggests that over the range of rights from 2 through approximately 7 on the Freedom House 2-14 range, and for rights above approximately 10.7, the association between governance and output is benevolent. In the case of countries with very poor rights (above 12, say), the relationship becomes particularly strong, implying up to a unit elasticity of output with respect to rights. While democratic regimes experience a less dramatic pay-off to improvements in rights, nevertheless the relationship remains benevolent at least on average for the 78 Group V countries. The real difficulty emerges for countries in the mid-level rights range from 7 to 10.7 on the Freedom House scale – here improvements in rights have the perverse effect of *lowering* growth, though the effect remains moderate (the conditional mean elasticity does not exceed 0.2).

¹⁰⁴ Note that for the purposes of the regressions, as well as the subsequent discussion of this subsection, countries whose elasticity was estimated on a rights measure on the 1-7 scale, were rescaled to the 2-14 scale. Note also that we exclude the two outlier cases given by Sudan and Oman, with their strong elasticities.

¹⁰⁵ An earlier indication of such a possibility was advanced by Barro (1997: ch 2), which postulated an inverted U-shaped relation between governance and growth in output. Barro found that countries in mid-range levels of governance experienced the highest levels of growth in output, while both extreme autocracies and extreme democracies experience somewhat lower growth. Our finding is also parabolic, but it reverses the association implied by Barro's (1997) finding.

It thus appears as if democratic transitions while at first being supported by potentially quite dramatic growth accelerations, as rights increase from a very low base, may face a barrier at mid range rights. Here growth costs of further democratization may hinder any continued improvement in governance, despite the possibility of further growth gains at even higher levels of governance.

The same implication emerges from a consideration of the evidence reported in Table 16, in which we report the average output-rights elasticity, average income, investment, output growth, investment growth, changes in rights, dispersion of elasticity values,¹⁰⁶ and productivity of investment¹⁰⁷ to emerge for Group V countries.¹⁰⁸

Table 16 confirms the presence of the strongest benevolent impact of rights improvements on output changes at the two extremes of the rights spectrum – with the strongest impact reserved for countries with poor rights. What also emerges is that the within-country variability of the output-rights elasticity is the strongest at the two extremes of the rights scale (compare the computation of elasticity dispersion of greater than 0.3 at the two extremes, as opposed to 0.05 in mid-range rights). The summary information of Table 16 further shows both the levels and the growth in income and investment increasing with mean levels of rights for Group V countries (both growth in real per capita GDP and real investment is three times as high for countries with good governance, as they are for poor governance countries). Importantly, the productivity of investment as measured by the ratio of output to investment growth also increases dramatically in governance. Under good governance the ratio of output to investment growth approximates 0.7 – under poor governance it struggles to attain 0.30, less than half the ratio under good governance. The better the mean level of rights of countries, the higher the productivity of investment expenditure.

INSERT TABLE 16 ABOUT HERE

The import of the present subsection is thus threefold. First the evidence for Group V countries not only confirms that they differ from countries in Group IV. There is a directly estimable relationship between governance and output in a sense that was not feasible for Group IV. Second, Group V countries are

¹⁰⁶ Measured as the difference between the variable elasticity computed at the highest (worst rights) level on the rights scale, and the elasticity computed at the lowest (best rights) level on the rights scale. It thus reports the extent to which countries face variation in their output-governance elasticity.

¹⁰⁷ Measured as the ratio of output growth to investment growth.

¹⁰⁸ The two outliers given by Sudan and Oman continue to be excluded.

themselves not homogeneous. They differ strongly in the strength of the elasticity we find between output and governance. This confirms the presence of our type II heterogeneity, even within Group V countries. Third, the *nature* of the difference between countries is such that it serves to suggest that heterogeneity between countries may be due to the presence of non-linearities in the association between governance and real output. Country-specific estimated elasticities suggest an inverted U-shape over the rights range, and we find evidence of differential investment productivity over the different ranges of rights within the sample.

The time series evidence from Group V alone is enough to suggest that both Type II and Type III heterogeneity (as classified in Table 1) is present in this class of countries. While time series evidence provides some indication of differences between countries, it can only be suggestive of the existence of the general structure of interaction between governance and economic development suggested by Figure 12. It remains to be seen whether type II and type III heterogeneity is exploitable in panel estimation, in order to provide stronger statistical verification of the proposed general structure to the association between rights and output. This question is left to section 4.4.

4.3.4 Group VIII: difference stationary governance, output integrated of order above 1

One substantial grouping of countries remains in the time series evidence. Group VIII countries have first difference stationary rights, but output and/or investment integrated of at least order 2. As for Group IV countries, it follows immediately that governance and output cannot be associated with one another in levels.

What is distinctive about the countries that feature in Group VIII, however, is that many of them have experienced not only dramatic changes in economic performance (hence the high order of integration), but that they have experienced strong changes in their rights structure also. This is typified by the appearance of East European states in Group VIII. Indeed, for many of the countries in Group VIII even beyond Eastern Europe the change in rights structure occurred immediately after the fall of the Berlin Wall in 1989. The second feature of Group VIII is that for a number of the countries undergoing a strong change in rights, the change came off the worst possible rights score – 14 on the composite Freedom House scale. Third, the countries in Group VIII show three distinct patterns in the association between growth and changes in rights.

The largest grouping, shows a collapse in output following the liberalization of rights – and for a number of countries the rights liberalization subsequently comes to be reversed. An example of this class of countries is Albania – see Figure 13. The second largest grouping liberalizes rights, but with positive consequences for economic growth – see for example El Salvador in Figure 14. The third grouping, shows no systematic pattern at all.

To explore these plausibility of these patterns, we considered the relationship between output growth and changes in the rights structure, by estimating:

$$d \ln Y = \beta_0 + \beta_1 dR + \sum_{i=1}^k \gamma_i D_i + \varepsilon \quad (4.6)$$

where the D_i denote a series of deterministic variables, by means of the ARDL approach to cointegration.¹⁰⁹ Strictly, the specification of course remains unbalanced since ADF test statistics suggest that $d \ln Y \sim I(1)$ at least, while $dR \sim I(0)$. However, many of the instances of $d \ln Y \sim I(1)$ occur due to structural breaks in the output series, which can be corrected for by the inclusion of appropriate deterministic elements into estimation. Estimation proceeds on a country by country basis.

INSERT FIGURE 13 ABOUT HERE

INSERT FIGURE 14 ABOUT HERE

INSERT TABLE 14 ABOUT HERE

In Table 14 we report the results of these country estimations. Results confirm the existence of the three classes of countries noted above.

For a number of countries, improvements in rights (such that $dRights < 0$ on the Freedom House scale) results in a negative growth impact. This is the case specifically for Albania, Angola, Guinea, and Lebanon, in a number of which the improvements in rights did not prove to be sustainable. It is also true of Bulgaria, Mongolia, Rumania and Sao Tome & Principe, though in these cases the rights improvement was sustained, and in a number of which output growth appeared to be recovering in the latter half of the 1990's after their rights transition. For almost all countries in this category, the rights transition occurred post 1989.

In the second group of countries, improvements in rights are associated with an increase in output growth. Djibouti, El Salvador, Kuwait after 1992, Guyana, Nigeria (w.r.t. to investment, not output), Poland and Tanzania all fall into this

¹⁰⁹ See Pesaran (1997) and Pesaran, Shin and Smith (2001) for details on the estimation framework.

group. For El Salvador and Guyana the pattern is one of strongly worsening rights off mid-range rights, combined with a collapse of economic performance, and subsequent reform of rights providing a growth stimulus.

The remaining countries, Equatorial Guinea,¹¹⁰ Paraguay, Sierra Leone, Tajikistan, Thailand, Trinidad, Vietnam and Yemen appear to show no systematic association between growth and changes in rights at all.

The implication of this evidence appears to be that while transitions from autocratic regime status, to higher levels of democracy is risky since it involves a possible negative shock to growth,¹¹¹ such a negative shock is not necessary (some countries experience growth acceleration), and even where it does occur, may come to be reversed. Unfortunately the evidence suggests that the risk factors are likely to outweigh the potential positive pay-offs. Figures 15 and 16 plot the estimated impacts of changes in rights on growth, against the mean rights change and the mean growth rate for each country in Group VIII. What emerges is that the negative growth shocks of rights transitions easily dominate any benevolent growth acceleration (positive semi elasticities readily dominate the negative in absolute terms). Moreover, this appears to be the case regardless of the strength of the change in rights structure (i.e. regardless of the magnitude of the mean change in rights).

INSERT FIGURE 15 ABOUT HERE

INSERT FIGURE 16 ABOUT HERE

The import of the evidence is that Group VIII thus appears to identify transition countries – but transition countries that face a substantial barrier to improvements in their rights structure. Improvements in rights generate a substantial collapse in output. This echoes the negative growth impact of moving below a rights level of 11 on the Freedom house scale for Group V countries. But for Group VIII countries the collapse in output issues from improvements in rights even from the worst possible level of 14 on the Freedom House scale. For Group V countries, the 14 – 11 rights range generated growth acceleration under conditions of improving rights. Moreover, the growth collapse appears far more dramatic than any impact isolated for Group V countries, since for Group VIII real output actually falls, often to a dramatic extent – consider Figure 13 and 14. While there are countries that experience a growth acceleration after rights

¹¹⁰ Here oil discoveries led to strong increases in output – but rights remained essentially unchanged.

¹¹¹ Note that this finding is consistent with the non-linearity we reported for Group V countries.

liberalization in Group VIII, such acceleration is relatively mild when compared to the apparently sizeable downside risk presented by rights transitions off a low level of rights.

Group VIII countries thus appear to face a substantial growth trap in any attempt to reform their governance.¹¹²

4.3.5 The Rest

Only a few countries do not fall into Groups IV, V and VIII, and we comment only briefly on a few features of the remaining cases. All the more so since many of the countries fall into the remaining groupings only due to one series being subject to a structural break, and also since many of the countries listed in various groupings have already been examined in detail under Groups IV, V, and VIII. Thus here we leave aside entirely Group VI, for instance. Nevertheless, a number of cases remain to be examined.

First, there is a grouping of countries that are largely dominated by countries to have emerged from the former Soviet Union, for whom time series data are not available for long time runs, or even at all, and leave ADF (and other) test statistics ambiguous.¹¹³ For these countries it is too early to draw firm statistical conclusions.

Second, for a number of the other groupings little evidence of an association between governance and economic development emerges. Under Group III, though Uzbekistan has some (though still limited) data availability, it shows little evidence of association between rights and real output. Of Group VII countries, only Ireland, Turkmenistan and the Ukraine remain unexamined. For Ireland, rights fluctuate between 2 and 3 on the Freedom House composite rights scale, while economic growth has been accelerating. It is not feasible to find a relationship between rights and growth under these circumstances. Turkmenistan shows virtually no variation in rights (it simply moves from 11 to 14 on the Freedom House scale at the outset and remains there), making statistical inference difficult. The Ukraine experiences an output collapse much as

¹¹² A corollary here is that attempts to link aid to governance may not generate much effect for countries at very low levels of governance. Leaders in such countries may not know whether they face a Group VIII or a Group V scenario – and the risk of facing an output melt-down during a rights transition may be prohibitive. Performance intensity of aid here might have little impact.

¹¹³ These include Armenia, Azerbaijan, Belarus, Bosnia, Croatia, Cuba, Georgia, Iraq, Kazakhstan, North Korea, Kyrgyzstan, Latvia, Liberia, Libya, Lithuania, Macedonia, Moldova, Myanmar, Qatar, Russia, Slovakia, Slovenia, Somalia, and Taiwan.

do Group VIII countries – though the stationary rights in this instance make it difficult to attribute the collapse to governance. Finally, for Group IX, Eritrea simply has too little time series data for estimation purposes. For Estonia we were unable to find a statistically significant association between rights and output (the limited time run of data may account for this). Estonia does share some characteristics with the transition countries of Group VIII, since its rights improvements coincided with a fall in output. However, it experienced a relatively rapid a sustained recovery in output after the rights transition.

Third, where a relationship between governance and development does emerge, it is generally weak. For instance, amongst Group I countries, only Ecuador is a pure case, and given the stationarity of both rights and real output, OLS estimation is legitimate in this instance. Regression of the natural log of real output on the natural log of real investment and the composite rights measure, returns a coefficient of -0.017 on rights, significant at the 1% level, and with an adjusted R^2 of 0.78. Thus rights are associated benevolently with real output in Ecuador, with the variable elasticity assuming the value of -0.1054 at the mean level of rights of 6.2 (i.e. a 1% increase in rights generates a 0.11% increase in real GDP). The implied impact is thus benevolent, but not particularly strong. In Group II, only the Bahamas has not been dealt with under another classification. Given its univariate time series structure, we examined a regression of the natural log of real GDP on the change in the composite Freedom House rights index. This returned a coefficient of only 0.045, though it is significant at the 5% level. Again therefore, while the result does suggest an association between governance and development, the strength of the association is weak.

4.4 From Heterogeneity Type II to Type III: back to the panel evidence

The time series evidence has shown that heterogeneity Type II in the association between rights and output is not only possible, but probable. For Group V countries, this appears to be supplemented by the presence of Type III heterogeneity also (recall the classification proposed in Table 1 above). The time series evidence does suffer from two limitations. The country-specific estimation has low statistical power – particularly in the face of the data hunger of modern time series estimation methods. We now have thirty years data on a range of governance measures – but thirty years still constitutes a short time run, especially when considering the impact of governance on economic development, and the likely lags that are involved in such an association. Time series evidence also does not fully exploit cross-country variation to test the validity of features such as the non-linearities postulated under our examination

of Group V countries above. In short, while the evidence summarized by Figure 12 is suggestive of a general form of association between rights and economic development, the time series evidence provides little means of testing its validity across both time and country.

In this section we return to panel evidence, but take seriously the possibility of both heterogeneity across the countries included in the panel, and the possibility of non-linearities that may be present in the association between rights and output.¹¹⁴ To do so, we begin with estimation under the DFEE, MGE and PMGE methodologies introduced in section 2.2, before incorporating the modifications to specification suggested by section 2.3. The PMGE exploits the improved power characteristics of a panel by imposing a homogeneous long run equilibrium relationship across all countries constituting the panel, while allowing for heterogeneity in the dynamics of the specification, as well as fixed effects. Note that that the solution to the implied difference equation for each country would in general imply quite distinct steady states. The advantage of the PMG estimator is that it has greater efficiency than estimators that allow for greater heterogeneity in the panel (eg the MGE). Estimators that impose excessive homogeneity on the panel (say by imposing homogeneity in both the long run and the dynamics, or by allowing only for fixed effects, such as the DFE estimator), risk introducing bias and inconsistency in estimation. We test for the presence of long run homogeneity accordingly by means of a Hausman test. In estimation, we impose a maximum lag length of 3 (on annual data), and choose the lag length for each individual country in the panel by means of an information criterion.¹¹⁵

In the estimations of the current section we generally include only a single human capital measure – though in the preliminary work there are some exceptions. This is explained by the strength of the association amongst the various human capital measures. In order to avoid both problems of multicollinearity and of excessive loss of degrees of freedom under data hungry

¹¹⁴ Estimation for section 4.4 was by means of GAUSS. Relevant code was kindly contributed by Yongcheol Shin.

¹¹⁵ Given the use of annual data, a lag length of 3 may appear more than adequate. However, since we are here investigating the impact of institutions and governance on long run economic development, we would have liked to explore higher order lags. Unfortunately, since we also control for human capital, and economic policy in a number of dimensions, degrees of freedom limitations prevented us from employing more liberal lag lengths. Exploration of optimal lag lengths in estimating the impact of governance remains a potentially fruitful topic for further research once additional data becomes available.

dynamic estimation techniques, we henceforth reply only on the average years of schooling measure.¹¹⁶

4.4.1 The Base Case Evidence

In Columns (1a) through (1d) of Table 18 we report results from estimation of (4.1) under static fixed effects, DFE, MG and PMG estimators.¹¹⁷ We note immediately that use of the PMG estimator appears justified. The Hausman h-test does not reject long run homogeneity, while the ϕ -term estimating the speed of adjustment to long run equilibrium confirms the presence of an equilibrium relationship, with 11% of any error eliminated in the succeeding time period.

INSERT TABLE 18 ABOUT HERE.

Estimation with the PMGE revises the results found under either static FE estimation, or under the alternative dynamic estimators considered here. The elasticity of output with respect to investment is revised upward (from 0.27 under static fixed effects to 0.35), while the impact of governance approximately doubles (from a 0.14-0.02 to a 0.28-0.04 elasticity range). Introduction of the human capital measure and the three policy variables does lower the estimated impact of both investment and governance (see columns (2) and (3) of Table 18, with investment elasticity of approximately 0.30, and the 0.14-0.02 elasticity range for governance). Human capital is statistically significantly and positively associated with output, with a variable elasticity rising from 0.09 at an average schooling of one year, reaching a unitary elasticity at approximately 11 years of schooling. The impact of policy on output is again confirmed, with a positive

¹¹⁶ The evidence presented in Appendix 4 provides a fuller explanation. Regression of the average years of schooling in the population on the other human capital measures (the percentage of the population without schooling, with completed primary, secondary and high schooling), reported in column (1) of the Table in Appendix 4, reveals a strong statistical association among the various human capital measures. The weakest impact attaches to the percentage of the population that has completed primary schooling, with a 10% increase in the population with primary schooling leading to an increase in the average years of schooling of 0.2. By contrast, the equivalent increase in completed high schooling, leads to an increase of 1.4 years in average schooling. The reason is straightforward: the rising coefficients over primary, secondary and high schooling reflects the cumulative nature of education. There is thus some measure of double counting in our specifications –nevertheless all variables remain statistically significant.

¹¹⁷ Readers should note that the country-specific estimation evidence of section 4.3 provides much the same evidence as the MGE does – though the MGE provides the summary information of the mean coefficients across countries. For the sake of parsimony, we report only specifications which include the composite rights variable, rather than both political rights and civil liberties. Results and implications are not affected by the choice of governance indicator. Full results for the individual rights indicators are available from the authors upon request.

impact from openness and declining inflation, while the crowd-out interpretation of foreign direct investment reappears.

4.4.2 The Challenge from Modernization Theory

In a recent paper Glaeser *et al* (2004) raise a number of concerns of relevance to the interpretation of these results. They present results that provide support to the Lipset (1959) modernization hypothesis, in which it is economic development which generates institutional development, rather than the reverse. In support of their argument they present evidence suggesting that it is human capital development that is more important for economic growth than institutions, and that it is human capital that leads to institutional development. While the econometric methodology employed in Glaeser *et al* (2004) is not sophisticated, the point raised in the paper is important for at least two reasons.¹¹⁸ It raises significant questions about the appropriate interpretation of the findings that we have reported on the association between governance and economic development. Particularly whether it is appropriate to interpret the benevolent association as leading from institutional development to economic development, or whether the finding merely reflects the reverse causality. For estimation purposes, it raises the prospect of significant bias and inconsistency where simultaneity has not been adequately addressed.

The concern is valid. But does the evidence support it? At first sight it appears as if the range of problems Glaeser *et al* identify with their institutional measures (which differ from those used in the present study) may replicate here. For instance, their concern with the “depth” of institutional as opposed to human capital measures reproduces here, though in lesser degree. The issue here is that institutional measures show reversion to the mean that is too great for the institutional measures to serve as good proxies for the stable “rules of the game” that new institutional economics requires.¹¹⁹ Human capital, by contrast proves far more stable. In Figure 16 we provide the cross plot of fitted against actual values of our rights and human capital variables, from an AR(5) specification. It is clear that persistence on the average years of schooling measure is far greater than for the institutional rights measure we employ – much as in the Glaeser *et al* study. On the other hand, despite of greater evidence of reversion to the mean for rights than for human capital, persistence in rights is also not insubstantial –

¹¹⁸ Independently of their specific evidence, similar evidence has been presented elsewhere, for instance Alvarez *et al* (2000) and Barro (1999).

¹¹⁹ Of the North (1990) sort, say. It is a moot point whether this is an appropriate concern in a period generally recognized as containing substantial pressure toward democratization.

as shown in Figure 17. And if governance is indeed to provide the policy handle for positive change that is posited by the new institutionalists, then surely it must have at least some capacity to change, especially in an era that has often been characterized as given to successive waves of democratization?¹²⁰

INSERT FIGURE 17 ABOUT HERE.

But what of the evidence of a link between human capital and institutions, given that Glaeser et al (2004) suggest that governance may be the *outcome* of human capital development? In Table 19 we present PMG estimations between our rights measure and the five human capital measures we employed for the present study from Barro and Lee (1994). What emerges is that there is indeed a link between human capital and institutions (rights) – but it is by no means one that provides unambiguous support to the Glaeser-suggestion. From column (1) of Table 19, while there does indeed exist a benevolent association between average total years of schooling (the measure of human capital also employed by Glaeser *et al*) and rights, this does not extend to all the human capital measures. Thus lowering the percentage of the population with no schooling, and increasing the percentage of the population that has completed primary, secondary and high schooling all serves to improve rights – directly contradicting the prior of the modernization hypothesis. What is more, the benevolent link between average total years of education and rights is not robust. We separate the sample of countries in our panel into three groupings, those countries whose average rights index rating falls below 7 on the 2-14 Freedom House scale, those with an average rights index between 7 and 11, and those with an average index above 11. Reestimating the association between rights and human capital demonstrates that the benevolent association between rights and human capital attaches only to countries with average rights below 7 on the Freedom House scale. In all other instances it implies worsening rights with increasing average years of schooling, and the heterogeneity extends to the other human capital measures also.

INSERT TABLE 19 ABOUT HERE.

Indeed, our evidence suggests that the crucial distinction to make when considering the impact of institutions on economic development is the underlying heterogeneity between different classes of countries. This

¹²⁰ See the evidence of Figure 8 of this paper, famously the arguments in Fukuyama (1992) and Huntington (1992).

heterogeneity was already suggested by the evidence that emerged from our time series evidence.¹²¹ We have just seen from Table 19 that the interaction of human capital and rights differs between three classes of countries, and challenges the simple association suggested by Glaeser et al (2004). But the point proves to be general. Table 19 shows that the interaction amongst human capital variables differs across the three country groupings also – with the pay-off to low rights countries in raising average years of schooling in their populations resting on increasing the proportion of their population that has any schooling at all, and primary schooling, while for countries with good rights the highest returns lie in increasing secondary and high school completion rates. This may well be an expression of the fact that under good rights, universal primary education is more likely with very low variation in primary schooling across countries, such that any returns from additional education must lie in secondary education.

Nor is policy different. Table 20 shows that not only do rights appear to impact on our policy variables, but that again the three classes of country already introduced show significant signs of difference. For openness, improved rights raise the percentage of GDP exported only for countries with good rights – over the mid range of rights there is no statistically significant impact at all, and for countries with poor rights improved governance appears to have a deleterious effect on export performance. Inflationary pressure is diminished with improved rights only for countries with mid-range and good rights, with the latter having the stronger impact. Countries with poor rights again have a perverse increase in inflation under improving rights. Only for FDI does the benevolent pay-off from improving rights appear to attach to mid-range and poor rights countries. For countries with already good rights, further improvement is associated with lower FDI flows instead.

INSERT TABLE 20 ABOUT HERE.

The upshot of this evidence is that what is plausible about the Glaeser et al (2004) argument, is that social, institutional and economic dimensions interact. But it is not plausible that the line of causation run clearly from economic development and human capital stocks to the institutions of a society. It is neither plausible that the interactions are quite so narrow – at least policy appears to matter also – nor are the interactions as homogeneous as Glaeser et al posit. Countries differ, even though general patterns in the heterogeneity can be explored. In what

¹²¹ Recall that for what we termed Group V countries, the elasticity of output with respect to rights was strong and benevolent for countries with very good or very bad governance – it was weaker and perverse for countries with mid-range rights.

follows we therefore conduct our analysis not only in terms of the different classes of countries our time series evidence has suggested – but we explicitly allow for interactions between human capital and policy variables with the institutional rights dimension also. As we will see in the analysis that follows, Glaeser et al (2004) are correct to point to the significance of the human capital (and we wish to add policy) dimensions, though their significance comes to be exercised in their *interaction* with institutions, not their determination of them.

This still leaves the direction of causality question raised by Glaeser et al (2004) unanswered, however. Even though the great preponderance of our time series evidence favoured output rather than the rights variable as the dependent variable (see the evidence from the weak exogeneity tests that the loading matrix allows),¹²² we therefore briefly consider some additional evidence. In Table 21 we consider results for a specification in which RIGHT is the dependent variable, and in which we control for two distinct indicators of economic development – the log of real per capita GDP, and the contemporaneous real investment flow – in order to provide a direct test of the modernization hypothesis.¹²³ Columns (1), (3), (5) and (7) report. Evidence is mixed. For countries with good rights (<7 on the Freedom House scale), increases in income are indeed associated with improvements in governance. But the relationship is reversed for all countries with an average rights rating above 7 on the Freedom House composite index, and statistically significantly so. The same is true for the panel as a whole. Nor does investment prove to have any more consistency in confirming or falsifying the modernization hypothesis. As a further test we also estimated our (4.1) specification with lagged values of the rights measure, reported as columns (2), (4), (6) and (8) of Table 21. While statistically insignificant changes in the rights impact do appear (in general the rights impact becomes marginally stronger), results remain generally unaffected.

INSERT TABLE 21 ABOUT HERE.

Evidence in favour of the modernization hypothesis in our sample of countries is thus weak. Instead, the evidence favours a direction of association *from* governance *to* economic development, at least in the levels specification of real

¹²² Where the loading matrix indicates that a dependent variable is adjusting into the long run equilibrium relationship provided by the cointegrating vector (which requires that the element of the loading matrix α_i is $-2 < \alpha_i < 0$), this is consistent with the long run equilibrium relationship being a determinant of the dependent variable. Where adjustment into the long run equilibrium relationship is absent (i.e. $-2 > \alpha_i > 0$), the dependent variable is *not* determined by the long run relationship given by the cointegrating vector, and hence can be considered to be weakly exogenous to the system.

¹²³ We also considered both less and more parsimonious specifications. Our conclusions remain unaffected.

per capita GDP. Readers may recall that while for the level of real output, simple correlations between real output and lagged values of governance did not show markedly different patterns from those for contemporaneous values of governance. The evidence presented above, is thus consistent with that to have emerged from the simple correlations we reported in section 3.1 of this paper.

For these reasons, the remainder of the discussion of the steady state specification of real output will treat the governance dimension as at least weakly exogenous.

4.4.3 Testing for the Possibility of Type III Heterogeneity – do we find evidence of the non-linearity the time series evidence suggested?

Given the very mixed support for the modernization hypothesis which emerges, and given the lack of dramatic change in our results on the introduction of lagged rights as the regressors, we therefore continue to rely on the (4.1) specification.

What we do explore further is the possibility of significant differences between the three classes of country defined by their rights classification. Table 22 reports results from the reestimation of (4.1) for the three classes of countries, again with the human capital and policy dimensions. Results are reported in columns (1a) through (1c) of Table 22.

INSERT TABLE 22 ABOUT HERE

Salient differences between the countries emerge. The investment elasticity increases dramatically as we move from the class of countries with an average rights index greater than 11, to countries with mid range rights, from 0.29 to 0.45. However, continued improvement in the governance dimension does not further increase the impact of investment on output (for countries with an average rights index below 7 the elasticity is 0.42). The impact of rights on output is statistically significant only for countries with very poor (index >11) or very good rights (index <7), while for countries with mid-range rights the impact is insignificant once the impact of policy is controlled for in estimation. In economic terms, the variable rights elasticity over the 14-2 Freedom House composite rights index range declines from 0.28-0.04 to 0.14-0.02, between the poor and good rights country grouping. For countries with mid range rights, the impact of improving rights is perverse, with improving rights lowering output, though the effect is not strong economically, and is insignificant statistically.

It is reassuring that the finding from the dynamic panel estimator thus accords with our time series findings for the Group V countries – with the association between rights and output following an inverted U-shape, with the strongest impact of governance improvements reserved for circumstances with the worst rights, and a perverse association for mid-range rights countries.

The impact of human capital is consistent in both its statistical significance and its economic magnitude across the three country groupings. The estimated coefficient implies an elasticity rising from 0.04 at one year of schooling on average in a population, to 0.44 at 11 average years of schooling, and reaches unitary elasticity once the average years of schooling reaches 25.

Of the policy variables we employ, FDI proves to be statistically insignificant for all three country groupings.¹²⁴ Lower inflation is associated with higher real per capita output, but only for countries with mid-range or good rights index scores, and more strongly so for mid-range rights countries (-0.06 vs. -0.02 elasticities). Improved export performance as proxy for increased openness, consistently is positively associated with real output, with the strongest elasticities emerging for countries with either good or poor rights (0.20 for poor rights, 0.06 for mid-range rights, and 0.23 for good rights countries).

However, even on splitting our sample of countries into the three groupings, on introduction of the policy variables into estimation, joint homogeneity tests for the long run specification fail, and we are able to confirm homogeneity only on the individual coefficients (see the reported h-tests).¹²⁵ The suggestion is that we may not yet have captured all relevant heterogeneity in our sample of countries with respect to policy. One response would be to explore the possibility of distinct groupings of countries beyond the three that we have employed thus far.¹²⁶ Since our attempts at doing so did not provide substantively different results, we instead estimated results for our existing three country groups, but allowing for interaction between investment and rights, human capital and rights, as well as policy and rights. This thus represents a generalization of the

¹²⁴ This suggests that the unstable sign on the FDI variable that we have encountered thus far, may well be a result of attempting to fit a linear specification across the three distinct country groupings.

¹²⁵ Thus the test for homogeneity conducted across groups on individual coefficients did not reject the null of homogeneity, but the joint test of homogeneity of all coefficients across all groups did not consistently avoid rejecting the null.

¹²⁶ In estimation we did in fact explore this possibility – but the substantive points to emerge from the presented results did not alter.

concern of Glaeser et al (2004) of possible interactions between governance and human capital. Table 23 reports.¹²⁷

INSERT TABLE 23 ABOUT HERE.

The interaction terms introduce yet further complexity into the analysis. Our findings on investment do not change, such that the productivity of investment increases markedly as we move from the low rights countries to mid-range and good rights countries. The only nuance to emerge is that where we control for foreign direct investment, the elasticity of investment continues to increase as we move from poor, to mid-range and good rights. Since foreign direct investment interacts with aggregate investment (by definition, but also since it may affect both the level and productivity of domestic investment),¹²⁸ finding that investment behaves differently once controlling for FDI is not surprising. However, the general finding that investment is more productive in countries with mid-range and good rights remains unaffected.

The impact of governance becomes more nuanced in the presence of interaction effects. Four core lessons emerge from the evidence.

First, the benevolent impact of rights on the level of real per capita output appears to be indirect, rather than direct. Columns (1a) through (1c) of Table 23 show that once the possibility of an interaction between rights and investment is controlled for, the direct impact of rights on output is rendered insignificant. The impact of improving rights renders investment more efficient under mid-range and good rights, while improving rights render investment less efficient for poor rights countries. In the case of good rights countries, the investment elasticity increases from 0.41 to 0.45 as rights improve from 7 to 2 on the Freedom House index, while for mid-range rights countries the improvement is from 0.48 to 0.51 as rights move from 11 to 7. By contrast, as rights change from 14 to 11 on the Freedom House scale, the investment elasticity of poor rights countries falls from 0.23 to 0.21.

¹²⁷ Note that as interaction terms proliferate, so the sensitivity of results to differing lag structures increases markedly. This stands in marked contrast to the results reported in other sections of this paper. Our interpretation is that the degree of heterogeneity between countries may well increase once we control for interactions. More research into possibly more differentiated country classes may well be a fruitful line of further research.

¹²⁸ See De Mello(1997) and Ramirez(2000) on the positive externalities associated with FDI, while Blomstrom, Lipsey, and Zejan (1994) and Borensztein, De Gregorio, and Lee (1998) explore the growth impacts.

The indirect impact of rights on output is confirmed under each of the sets of estimations reported in Table 23, that control for human capital and our policy proxies. The impact of the indirect effect is always such as to benevolently increase the impact of human capital, and of the policy proxy. Moreover, the indirect impact of rights on the effectiveness particularly of policy proves to be strong in economic terms. For the maximum feasible rights changes under good, mid-range and poor rights countries, the elasticity of output with respect to exports rises from 0.42 to 0.67 (good rights case), 0.71 to 1.03 (mid-range rights case), and 1.39 to 1.63 (poor rights case) respectively. For foreign direct investment, the symmetrical changes are 0.84 to 1.24 (good rights case), 1.23 to 1.71 (mid-range rights case), and 3.68 to 4.85 (poor rights case). In the case of inflation, the impact of policy and the indirect impact of rights changes are significant only for mid-range and good rights countries, and the symmetrical changes are -0.06 to -0.11 (good rights case) and -0.17 to -0.21 (mid-range rights case).

The indirect effect of rights is also evident for our human capital variable, though here it emerges only for mid-range rights countries. Here the estimated impact of one additional year of schooling increases from 0.17 to 0.23 for an improvement in rights from 11 to 7. This translates into an increase of the elasticity of output with respect to schooling from 0.67 to 0.90 at the mean years of schooling (3.92 years) for the group of mid-range rights countries.

Second lesson of the evidence is that the direct effect of rights on output is more mixed where we control for interaction effects. We have already noted that the direct effect disappears where the interaction between rights and investment is controlled for – see columns (1a) through (1c) of Table 23. Where we control for both the rights-investment and the human capital-rights interaction, the non-linear inverted U direct association already noted in connection with Group V countries, and in the discussion surrounding Table 22 immediately preceding. The evidence is reported in columns (2a) through (2c) of Table 23. Thus a benevolent direct association between rights and output is present for countries with either good or poor rights, while the association is perverse for mid-range rights countries. What is more the implied elasticities are economically strong. For countries with good rights the benevolent variable elasticity range is 0.49 to 0.14 over the 7 to 2 Freedom House index range, for poor rights countries 0.98 to 0.78 over the 14 to 11 Freedom House index range. In the case of the mid-range rights countries, with the perverse association between rights and output, the elasticity range is 0.99 to 0.63 over the 11 to 7 Freedom House index range. Similar evidence emerges for the estimations that control for inflation – columns

(5a) through (5c) of Table 23. In this case, however, the direct rights impact proves statistically significant (and benevolent) only for countries with good rights, with an associated variable elasticity of 0.28 to 0.08 over the 7 to 2 Freedom House rights index range.

In the case of the estimations controlling for exports and foreign direct investment (columns (3a) through (3c) and (4a) through (4c) respectively), the direct impact of rights on output becomes perverse (in the sense that increasing rights are associated with declines in output) for all country groupings – more strongly so the worse the rights. We note both that these two cases are the only instances in which we have found such a perverse impact of rights on output, and that the implied elasticities are of a magnitude that raise doubts concerning their validity. Particularly for poor rights countries controlling for foreign direct investment, the implied elasticity range of 13.3 to 10.45 seems implausibly large – but even the smallest elasticity range for this set of estimations, for good rights countries controlling for exports, seems inordinately large, ranging from 1.26 to 0.36.

Therefore, while we note the countervailing evidence to emerge on the direct impact of rights on output, the more important lesson we draw from the estimations controlling for indirect impact of rights on output, is a methodological one. What follows from the evidence of Table 23 is that results are sensitive to controlling for the presence of indirect effects. Exploration of the theoretical and empirical possibilities opened up by the presence of interactions is a matter of priority – in this we concur with the spirit of the Glaeser (2004 *et al*) argument. In general the evidence suggests that the positive impact of governance on economic development remains present – but it is possible, even likely, that the impact may prove to be indirect, exercised through the productivity of investment, of human capital, and the effectiveness of policy.

The third implication of the evidence is that the impact of policy remains present, even where we control for the possibility of indirect effects of governance on output. Indeed, the findings of the section suggest that the impact of policy is strengthened under conditions of improving governance, while the impact of improving rights remains benevolent throughout. Moreover, in the case of the indirect effects operating through improving efficiency of investment, and the impact of inflation, the improving efficiency effect proves to be strongest under good rights or mid-range rights, rather than poor rights.

The fourth and most general finding of the present section remains that the existence of the general non-linear structure of association between rights and real output that we first encountered in our time series evidence on Group V countries, does find statistical corroboration from the panel evidence. We have both Type II and Type III heterogeneity present in our panel of countries, but the panel data estimation techniques here employed offer a way forward.

The findings of the section suggest that despite the appearance that countries at different levels of governance behave quite differently under conditions of improving governance, there remain present general patterns of association that serve to provide a unitary structure to the apparently diverse behaviour we observe across countries. An analogy from the natural sciences might refer to the apparently diverse behaviour of objects with differential mass, density, shape and size – but which are governed by common natural laws.

4.5 So what has happened about growth?

In the introduction to this paper we promised an examination of economic development's interaction with governance both in terms of steady state and in terms of issues surrounding transitional dynamics. Thus far, steady state questions have received considerable attention - transitional dynamics relatively little.

We have not forgotten about growth, and we return to it now.

Fortunately the methodological lessons so painstakingly developed above, replicate. Once again, unobserved effects, both time invariant and time variant, measurement error and endogeneity matter, as does heterogeneity across countries as evidenced by time series evidence, and the use of dynamic panel estimators. We therefore economize on exposition, and move directly to the presentation of the dynamic panel results.

The discussion builds cumulatively, beginning with the base case in which we explore the full sample impact of investment, rights, human capital and policy on growth in real per capita GDP. We then move on to examine whether the non-linearities that we noted for the level of real per capita GDP, are also evident for the dynamics of output.

4.5.1 Growth in Real Per Capita GDP – the full sample evidence

We begin straightforwardly with a base case in which we examine the impact of investment, improvements in governance, human capital as proxied by average years of schooling, and our three proxies for the quality of economic policy making (openness, the extent to which the policy environment is investor friendly, inflation), on economic growth. In effect, we therefore return to an estimation of equation (4.1), while replacing the level of output with output growth, and the level of the rights variable with its first difference.

For the full sample in general our results for economic growth mirror those that we obtained for the level of real per capita output. Column 1 of Table 24 reports.

Thus we find that investment has a positive and statistically significant elasticity with respect to output growth. The estimated coefficient implies an elasticity of growth with respect to investment of 0.79. Thus a 1% increase in per capita investment is associated with a 0.79% increase in growth (not a 0.79 percentage point increase in growth). Since growth elasticities are difficult to interpret, we note that the mean per capita investment expenditure in our sample is US\$26.58 in real terms. The implied associated growth performance is 3.28% per annum (the actual mean is 1.27%). Increasing the real per capita investment expenditure to US\$30, US\$40 and US\$60, would raise growth to 3.40%, 3.69% and 4.09% respectively. While growth performance can be enhanced by investment in physical capital, therefore, the required increase in investment is fairly substantial in order for the impact to be strong. But perhaps the more important implication is that the 66 countries in our sample are not realizing the growth performance they should given their investment expenditure (countries are only realizing 1.27%, rather than 3.28%). Other things are clearly not equal, and it remains to be seen what detracts from the 66 country growth performance.

INSERT TABLE 24 ABOUT HERE.

The impact of an improvement in rights remains statistically and economically significant for growth. The estimated coefficient for the full sample suggests that an improvement in rights from 14 to 2 on the Freedom House index scale would increase growth by 2.40 percentage points, while an improvement in rights from the sample mean (8.02 on the Freedom House scale) to the best possible level of rights (2 on the Freedom House scale), would raise growth by 1.20 percentage points. Thus the first answer to the question of what has decreased the growth performance of our sample of countries below that which would have been predicted by their real per capita investment levels, is that their governance has been relatively poor, and this has negatively affected their growth performance.

The second explanation for the poor growth performance of sample countries relative to that predicted by their real per capita investment expenditure, comes from the policy variables we control for. All three policy variables prove to have a benevolent and statistically significant impact on economic growth. In the case of openness, the impact is relatively weak in economic terms. We control for the openness of the economy by means of the percentage of GDP traded (exported and imported). The sample mean value of the variable is 26.79% (median of 24.09), such that the estimated coefficient suggests that raising the openness measure from the sample mean to 35%, 40% and 50% of GDP, would serve to raise real per capita output growth by 0.3, 0.4 and 0.7 percentage points per annum respectively.

By contrast, the impact particularly of both foreign direct investment, but also of inflation proves to have a strong economic impact on economic growth. FDI is measured as a percentage of GDP, with an in sample mean of 1.33% (median of 0.69%). Raising FDI from the sample mean (median) to 2%, 5% and 10% of GDP, would serve to raise real per capita output growth by 2.08 (5.43), 6.75 (10.10) and 10.29 (13.64) percentage points per annum respectively. The strength of the impact suggests that FDI is either capturing the impact of some excluded dimension (security of property rights, credibility of policy, stability of the macroeconomic environment), or that interaction between FDI and aggregate investment is distorting the FDI impact. While caution is thus advised in interpreting the FDI coefficient, nevertheless the implication is that poor policy environments, which have been unsuccessful in attracting FDI, may also explain why aggregate private sector investment has realized poor growth effects.

A similar, though less dramatic picture emerges with respect to inflation. The sample mean inflation is 92.29% per annum, such that a reduction of inflation to 10% per annum (close to the sample median of approximately 11.64% p.a.), would raise real per capita growth by 1.78 percentage points per annum.

There is only one exception to our findings of benevolent impacts of the standard policy handles associated with improved economic growth. Raising investment in human capital appears to carry negative consequences for economic growth. Moreover, the predicted impact is not only statistically significant, but strong in economic terms. In particular, the estimated coefficient of column (1) of Table 24 suggests that raising the mean years of schooling from the sample mean of 3.99 (the median is 3.86), to 6, 8, 10 and 12 years, lowers economic growth by 1, 2, 3 and 4 percentage points respectively. The finding reverses the generally

benevolent association between human capital and the level of real per capita output reported in preceding sections. On the other, it does accord with numerous other studies on the impact of human capital on economic growth. The real cause for concern here is the sheer size of the negative impact of human capital on growth – sufficiently large to raise concerns about the validity of the finding.

Two general conclusions follow from the evidence for the full sample of countries. The first is the point that has already been noted, viz. that in addition to poor governance, poor policy performance serves to explain why countries have performed worse in growth terms than would have been predicted by their real per capita investment expenditure.

The second general point emerges from the fact that the strong divergence between sample mean and median for FDI and inflation, suggests that some countries in particular are paying a particularly high growth price due to poor economic policy. Since the divergence between mean and median is far less extreme for rights (8.02 against 8.00 – though there are also countries with mean rights above 13 on the Freedom House index), the implication is that while governance is indeed of significance in attempts to improve growth, improvements in particularly egregious violations of sound economic management may well be the place to start if the objective is to raise growth rates.

Unless good governance interacts with sound policy management.

It is this question that the following section also serves to address.

4.5.2 Growth in Real Per Capita GDP – exploring the possibility of non-linearities

In our explorations of determinants of real per capita output in the long run, we noted the plausibility of the Glaeser et al (2004) argument that social, institutional and economic dimensions interact. We also noted that such interactions have to take cognizance of the possibility that the line of causation does not run from economic development and human capital stocks to the institutions of a society. It is neither plausible that the interactions are quite so narrow – at least policy appears to matter also – nor are the interactions as homogeneous as Glaeser et al posit. Countries differ, even though general patterns in the heterogeneity can be explored.

In what follows we therefore revisit the possibility that the different classes of countries our time series evidence has suggested matter for economic growth as well as the level of real per capita output. In the process we explicitly allow for interactions between human capital and policy variables with the institutional rights dimension also.

4.5.2.1 Revisiting the possibility of modernization processes

This again leaves the question of the direction of causality between economic development and governance unanswered, however. As for our examination of the level of real per capita output, therefore, we briefly consider additional evidence, presented in Table 25. We consider results for a specification in which the change in rights is the dependent variable, ΔR , and in which we control for the two indicators of economic development, real per capita investment expenditure and growth in real per capita output. Once again therefore, this represents a test of the modernization hypothesis.

INSERT TABLE 25 ABOUT HERE.

Columns (1a), (2a) and (3a) report. Evidence in favour of modernization is mixed at best, and at worst absent altogether. Coefficients favouring improvements in rights with economic development (investment, growth) emerge only for mid-range rights countries, and countries with poor rights. By contrast, for countries with good rights (below 7 on the Freedom House scale) the association between economic development and governance reverses – suggesting worsening rights with improved rates of economic development. What is more, all coefficients on economic development, for all country groupings, prove to be statistically insignificant, regardless of the sign on the coefficient. Finally, for countries with good rights, the Hausman test statistic provides some evidence suggesting heterogeneity across the countries included in the good rights country sample.

As an additional test we also compared results from specifications that included rights as a lagged variable, and specifications in which a contemporaneous specification of rights appears. Columns (1b), (1c), (2b), (2c), (3b) and (3c) of Table 25 report the results, for our three classes of countries. Statistically insignificant changes in the rights impact on growth do appear between the two sets of specification – but only for countries with poor rights (above 11 on the Freedom House scale). But in general the impact of rights remains unaffected by the use of lagged values of rights. Lagged specifications of governance thus does

little to unseat the implication to have emerged from the individual country time series evidence, as well as the evidence of the levels specification of real per capita GDP, that the direction of association runs *from* rights *to* economic development, rather than the other way around.

The econometric evidence presented in Table 25 sheds additional light on the initial correlation evidence reported in section 3.1 of the paper. Recall that in the correlation evidence we found that consideration of lag structure by means of correlations did present some evidence that lagged improvements of rights did impact negatively on economic growth one period later. While the correlation evidence is not consistent with the modernization hypothesis (the direction of association runs the wrong way), note that the econometric evidence of Table 25 does not suggest that the contemporaneous and the lagged impact of rights is statistically significantly different. Indeed, only for countries with poor rights is there any evidence of a different impact at all, and the impact is weakened.

As was the case for the level of real per capita output, therefore, we conclude that the evidence in favour of the modernization hypothesis is weak, and dominated by the alternative that the direction of association runs from governance to economic growth rather than *vice versa*.

Instead, the suggestion we take further in our analysis, is the possibility that the three classes of countries consistently identified in this study as being distinct in both the time series evidence and the levels specifications, continue to be important to economic growth also. In doing so, we explicitly also recognize the possibility of interactions between governance and physical and human capital, and governance and the quality of economic policy making.

4.5.2.2 A first simple examination of the possibility of non-linearities

In investigating the impact of governance on growth we employ the specifications provided by section 2.4, and begin by estimating the long run relationship given by:

$$d \ln Y_{it} = \delta_{it} + \delta_1 \ln INV_{it} + \delta_2 dR_{it} + \varepsilon_{it} \quad (4.7)$$

where INV denotes investment, and recognizing that $\delta_1 = Y_{K_g} dg + Y_K$, and $\delta_2 = Y_g + Y_K K_g$ incorporate the impact of governance on the marginal product of capital and the level of investment respectively. Where improving governance increases the *level* of investment, as identified in section 2.4 of the paper the estimated parameter value for δ_2 should be biased upward from the marginal direct impact of

governance, Y_g , by $Y_K K_g$. Similarly, where improving governance improves the *productivity* of investment, section 2.4 of the paper showed that the estimated parameter value, δ_1 , should be biased upward from the marginal direct impact of investment, Y_K , by $Y_{K_g} dg$.

The first check we perform of these two possibilities is to estimate (4.7) over subgroupings of countries within our 66 country sample. In particular we estimate (4.7) for country groupings defined by a mean score of Rights < 7, 7 < Rights < 11, and Rights > 11 on the composite 2-14 Freedom House rights index. In columns (2a), (3a) and (4a) of Table 24 we present initial evidence for the estimation of equation (4.7). Both the Hausman h-test, which confirms the homogeneity of the long run relationship being estimated, and the ϕ -coefficient testing for adjustment to long run equilibrium confirm the soundness of the PMGE methodology.

Once again, where improving governance increases the marginal product of capital, $Y_{K_g} > 0$ (as we assume feasible), the prior to be tested is that δ_1 increases as we move from countries with very poor rights, to countries with better rights, i.e. from Rights > 11 to 7 < Rights < 11 and Rights < 7 successively.

For δ_1 our prior is weakly confirmed. For countries with very poor rights (>11), the impact of a one percent increase in real per capita investment expenditure is a 1.6 percentage point increase in growth. This rises to a 2.7 percentage point increase for mid-range rights countries, before returning to a 1.9 percentage point increase for countries with good rights.

The first implication is therefore that the evidence is broadly consistent with improved efficiency of investment under good governance. Only two limitations constrain the inference. First, since the interaction term contained in δ_1 has not yet been separated (recall that $\delta_1 = Y_{K_g} dg + Y_K$) we cannot yet be sure that the increase in δ_1 is due to the productivity improvement of physical capital due to improvements in governance, $Y_{K_g} dg$, rather than variations in the level of Y_K across the three groups of countries. Second, the δ_1 coefficient under good rights (0.019) is not statistically significantly greater than under poor rights (0.016), though that for mid-range rights countries (0.027) is. Nevertheless, the conclusion that moving away from poor rights environments improves the growth pay-off to investment in physical capital does appear to stand.

No robust test of the impact of governance on the level of investment is available from an estimation of (4.7). Recall that $\delta_2 = Y_g + Y_K K_g$. As long as investment

increases in the level of governance, $K_g > 0$ (as we assume), and provided that Y_g, Y_K , remain relatively constant, δ_2 should increase in absolute terms, given the inverted rights scale over the specified rights groups (Rights < 7, 7 < Rights < 11, and Rights > 11 on the composite 2-14 Freedom House rights index). However, since we have no priors on the relative rate of change of the marginal impact of governance on output, Y_g , and the impact of governance on capital stock, K_g , no unambiguous prediction can be made concerning δ_2 over the rights groupings.¹²⁹

Our estimates of δ_2 do not confirm an increase in absolute terms in the coefficient as rights improve. On the contrary, the estimated absolute impact of changes in governance is strongest under conditions of poorest rights (>11), with an impact three times as strong as under good governance (<7), and no statistically significant impact for mid-range rights countries. While our findings therefore cannot confirm rising *levels* of investment under improved governance, since we have no information concerning changes in the marginal impact of governance on capital and output, the evidence is also not a conclusive falsification. Indeed, one additional possible reason for the unanticipated pattern of change in δ_2 may be due to the impact of a rising marginal product of capital under improving governance, which might serve to lower the absolute size of δ_2 given our postulate of $\delta_2 = Y_g + Y_K K_g$, and given our findings for δ_1 .

These initial findings remain unaffected where we control for human capital and economic policy variables. Columns (2b), (3b) and (4c) of Table 24 report the results. Given the statistical insignificance of investment in the estimation for poor rights countries, the evidence in favour of an increasing δ_1 parameter is strengthened, though the strongest impact of investment on growth continues to emerge for mid-rights countries. For the δ_2 parameter the evidence continues to point to a decline in absolute terms in the parameter as rights improve, again failing to provide evidence consistent with rising levels of investment. Human capital continues to have a negative impact on economic growth, though the statistical significance of this finding is restricted to mid-range rights countries. Openness of economies proves to have positive growth impacts under good and poor rights, while foreign direct investment exercises a positive impact on growth only in mid-range rights countries. Finally, inflation lowers growth only for mid-range and good rights countries, and is statistically insignificant for poor rights countries.

¹²⁹ Note that more complex forms of non-linearity cannot be excluded – while the impact of Y_K , cannot be explicitly controlled for in the absence of physical capital stock data. Again more complex non-linearity in K_{gg} , as well as Y_{gg} , and the impact of Y_K may intervene in preventing the isolation of the postulated non-linearity in governance.

Since we address the impact of policy in detail in the discussion which follows, we conclude the initial evidence in favour of non-linearity merely by noting that the results not only support the presence of non-linearities, but that the evidence is robust to the inclusion of additional human capital and economic policy variables.

4.5.2.3 Further evidence on non-linearity

To provide additional evidence on the presence of non-linearities, we reestimated (4.7) under explicit separation of the interaction effect:

$$d \ln Y_{it} = \delta_{0,it} + \delta_3 \ln INV_{it} + \delta_2 dg_{it} + \delta_4 \ln INV_{it} dg_{it} + \varepsilon_{it} \quad (4.7')$$

where $\delta_3=Y_K$, δ_2 remains defined as before, and $\delta_4=Y_{Kg}$. Thus the previously composite effect under δ_1 has now been explicitly separated into the separate δ_3 and δ_4 effects. We again estimate over the three distinct country groupings. Changes in δ_4 now provide more precise information concerning the nature of Y_{Kgg} over the rights groupings, while δ_3 provides direct evidence concerning Y_K . While the ambiguity of δ_2 is not unresolved, since direct evidence on both Y_{Kg} and Y_K are available from δ_4 and δ_3 respectively, more information concerning Y_g does emerge.

Estimation results for equation (4.7') are reported in Table 26.

INSERT TABLE 26 ABOUT HERE.

Four general lessons emerge from the evidence of Table 26.

First, the evidence provides support both for improvements in the productivity of investment under improving governance, as well as rising levels of investment in physical capital stock under better rights. The estimates of δ_3 confirm the results obtained under equation (4.7) for the marginal product of capital. Comparison of the results of columns (1a), (2a) and (3a) suggests that with rising governance, the estimate of the marginal product of capital increases – with the impact of investment under sound governance being roughly one and a half that which holds under poor governance. The important qualifier to the finding is that the improved efficiency of investment is primarily realized on moving from countries with poor rights, to those with mid-range rights. Further improvements in governance do not lead to comparable increases in investment efficiency, and instead show some evidence of eroding investment efficiency. The

economic significance of the estimated impact in columns (1a), (2a) and (3a) remains largely unchanged from that obtained under (4.7), and reported in Table 24. Moreover, the finding of rising efficiency of investment under improved governance is also generally true for the remainder of the results reported in Table 26 (the only potential exceptions are for those estimations (1e), (2e) and (3e) that control for inflation). The finding of improved efficiency of investment under improved governance thus appears robust to more explicit treatment of potential interaction effects.

This finding is further corroborated by a consideration of the direct estimates of the impact of the marginal impact of governance on the marginal product of capital provided by δ_4 . Consideration of the evidence of columns (1a), (2a) and (3a) suggests that the estimated marginal impact is benevolent (improvements in rights increase the marginal product of capital). What is more, the strongest marginal impact obtains under the worst rights (>11), with an impact roughly six times as strong as under mid-range levels of governance ($7 < \text{Rights} < 11$) – note the coefficients of -0.024 and -0.004 respectively. For countries with good rights, the statistical insignificance of the δ_4 suggests that further improvements in the efficiency of physical capital can no longer be realized. The findings thus confirm that allocative efficiency of physical capital improves with governance – and the strongest increase appears to be reserved for moving out of the worst possible rights category, since the δ_4 coefficients across rights groups strongly imply $Y_{Kgg} < 0$.

The impact of improved governance on the level of investment is clarified under the estimation of equation (4.7'). The estimates of δ_2 presented in columns (1a), (2a) and (3a) now show an increase in absolute terms under improving governance. Recall that $\delta_2 = Y_g + Y_K K_g$, which made the identification of the levels impact on investment of improving governance difficult. But we now know from our estimates of δ_3 that the marginal product of capital does indeed increase with improved governance (thereby lowering the absolute value of δ_2). Further, our estimates of δ_4 also confirm that $Y_{Kgg} < 0$. Thus the increasing absolute value of the δ_2 parameter can only be due to an increasing marginal product of governance (Y_g), or to an increased incentive to accumulate physical capital under good governance (K_g).

The very controversy surrounding the impact of governance on economic development renders it unlikely that increasing returns to governance, if positive, are strong. The more plausible interpretation is thus that the level of physical capital accumulation increases in governance, instead.

The evidence thus suggests that both of the potential sources of non-linearity in the association between rights and output which we postulated at the outset, may well be present. Both the productivity and the level of investment rises in increasing governance.

We conclude the first general lesson we draw from this evidence by noting an important qualification. The evidence on the impact of improving governance on the level of investment is best read by contrasting the coefficient on rights for countries with the worst rights (statistically = 0), with that on rights for countries with good rights (statistically significant, and = 0.012, suggesting that an increase in rights by one unit on the Freedom House scale raises per capita output growth by 1.2 percentage points). While the coefficient for countries with mid-range rights lies between these two in absolute terms (0.009), it does so with a reversal in sign, such that governance improvements lead to a decline in output growth. Increases in the level of investment in physical capital are thus best viewed as occurring between worst and best rights countries. The evidence on mid-range countries by contrast can be thought of as being consistent with the finding of an inverted U-shaped association between economic development and governance, with a perverse association for mid-range rights countries, which this study has repeatedly encountered in the time series evidence and in connection with the level of real per capita output.

The three remaining general lessons to emerge from the evidence of Table 26 relate to the growth patterns that attach to the three country classes.

For countries under good governance (Rights<7), conditions for economic growth are the most propitious of any of the three country groupings. This is true in the sense that consideration of the evidence of columns (2b) through (2e) suggests that growth for these countries responds positively to all of the possible determinants of growth considered by this study. Investment in physical capital is consistently a positive and statistically significant driver of real output growth, with a one percent increase in real per capita investment raising growth by 0.7 – 2 percentage points, depending on specification. Investment in human capital similarly raises economic growth, with one year of additional schooling for the population on average raising growth by 0.4 percentage points. Finally, sound economic policy similarly matters for economic growth. Increasing the openness of the well-governed economies from the mean of 29.25% of GDP traded (the median is 25.91%) to 35%, 40% and 50%, would serve to raise real per capita output growth by 0.38, 0.66 and 1.13 percentage points respectively. Raising

foreign direct investment from the mean of 1.69% of GDP (the median is 0.92% of GDP) to 2%, 5% and 10% would raise output growth by 1.75, 11.28 and 18.49 percentage points respectively.¹³⁰ Finally, dropping inflation from the sample mean of 82.02% per annum (median of 13.31%) to 10%, would raise output growth by 1.89 percentage points.

Thus for countries under good rights, the full range of possible growth supporting mechanisms (physical and human capital, economic policy) are not only found to be statistically significant, but they are found to have a benevolent impact on economic growth, often with strong economic significance.

At the other end of the complexity spectrum are countries with mid-range rights. For these countries it is really only investment in physical capital that proves to be statistically significant, with a one percent increase in real per capita investment expenditure leading to an increase in real per capita output growth of between 2.3 and 2.8 percentage points, depending on specification. Human capital investment, openness of the economy, foreign direct investment, – none of these matter statistically significantly for real per capita output growth, and nor do any interaction effects between rights and the policy variables. The only positive policy impact comes from anti-inflationary economic policy, where dropping inflation from the sample mean of 60.82% per annum (median of 9.96%) to 10%, would raise output growth by 2.89 percentage points.

While some clear general growth policy lessons appear to emerge for good and mid-range rights countries, the case that is most difficult to interpret is that of countries with poor rights. The estimated impact of investment in physical capital is erratic at best (alternating between statistical significance and insignificance, and where significant between relatively strong and relatively weak impacts), human capital has a perverse impact on economic growth, and anti-inflationary economic policy proves to be statistically insignificant. While both openness of the economy, and foreign direct investment prove to have a benevolent impact on growth, note also that the interaction between rights and FDI suggests falling FDI efficiency under improving governance.

The most appealing conclusion appears to be that at least for purposes of economic growth, leaving the poor rights classification for mid-range rights or good rights is desirable. At least investment in physical capital comes to be

¹³⁰ Once again we note that the FDI results appear to be implausibly strong. Possible interaction effects between FDI and aggregate investment may introduce bias into the estimated coefficient – a point already noted in the preceding discussion.

productive, and under good rights the full range of policy instruments is unambiguously available for benevolent intervention.

In closing the discussion of this section, we note only that the strong differences between countries that we have identified, may well come to account for the absence of stable and robust results across empirical studies. Countries do differ. Not without system, but nevertheless significantly so, rendering standard estimation approaches limited in their applicability.

5.0 Conclusions and Evaluations

Rights matter for long term economic development.

This paper has shown that in order to arrive at the central substantive message, some important methodological issues have to be addressed. We have identified four that were of concern.

Returning to our classification of potential estimation issues that we summarized in Table 1, this paper has argued that heterogeneity Types Ia, Ib, II and III are all relevant to addressing the question of this paper: how rights matter. We briefly reconstruct the sequence of results obtained under estimation, in order to fix the question of how relevant awareness of heterogeneity Type I – III in estimation is if we wish to identify the relative magnitudes of the impact of rights on long run economic development.

Estimation in a large cross-country context, and over long time runs, must take cognizance of the fact that a phenomenon as complex as economic development will have many determinants. Not all of these can always be measured over the geographical and time sample which concerns one. Many of the unobserved dimensions may covary with explanatory regressors. Estimation without accounting for the impact of unobserved effects will render biased and inconsistent estimation. Presuming that we therefore do face heterogeneity Type Ia (see Table 1), accounting for the impact of unobserved effects, reduced the estimated elasticity range of improvements in governance from 0.56-0.08 to 0.01-0.002 (for the full multivariate setting, incorporating human capital and policy measures), thus rendering it apparently negligible for purposes of economic development. The comparison is between *beta high* and *beta multi* of Figure 11. But this is only the first step in the analysis.

The evidence pointed to the likelihood that the exogeneity assumptions of standard estimators are violated in the relationship between output and rights.

Measurement error, feedback loops, time varying unobserved effects would all explain the violation of the assumptions – and all are either likely or at least feasible in the present context. Employing GMM estimators to account for the violation (or heterogeneity Type Ib of Table 1), reversed the decrease in the impact of rights, suggesting an elasticity range of 0.42-0.06 – see *beta-GMM* of Figure 11. This estimated impact lies only marginally below that obtained under the most simple pooled OLS estimation not accounting for any unobserved effects, even in the presence of human capital and policy measures, and substantially above the FE and FEGLS impacts.

In the course of presenting extended time series evidence from individual country estimations, three core insights emerged. The evidence suggests that countries do differ in terms of their link between economic development and governance. Univariate time series evidence indicated that the *structure* of the relationship between rights and output must differ between countries. At least nine distinct groupings of countries were identified, though most countries fell into three groups –termed Group IV, Group V and Group VIII countries in our discussion. Extensive use of VECM estimation for Group V countries demonstrated that the *strength* of association between rights and output differs amongst countries that shared a common structure of association. There was also some evidence to support the possibility of a *growth trap*. This could take two forms. For Group V countries we found a growth pay-off for improving rights from a very low base. But at mid-level rights (11-7 on the Freedom House scale), this pay-off reverses sign – countries attempting to improve rights, on average appeared to face lower output growth. Only once rights improved even further (below 7 on the Freedom House scale), did countries again come to experience a growth stimulus from further democratization. In Group VIII countries the nature of the growth trap is somewhat different. In this grouping of countries, most of which had very poor rights in sample, attempts at democratization (often off the very worst possible level of rights) led to growth disasters: real output falling, often by substantial amounts. For these countries, improvements in governance held substantial risks – and many quickly reversed any attempts at reform.

Importantly, the detailed country specific time series evidence served to show that heterogeneity across countries is not only possible, but probable. In effect, the evidence suggests that we face heterogeneity Types II and III in attempting to isolate the impact of governance on economic development. But time series estimation faces two limitations. Country specific estimation has low statistical power – particularly in the face of the data hunger of modern time series

techniques. It also does not exploit cross country variation to arrive at insights that extend beyond the individual country.

While particularly the evidence from the Group V countries suggested that the heterogeneity across countries while real, might nevertheless conform to an underlying general structure governing the way in which the interaction between rights and output changes across rights categories, country specific time series evidence does not facilitate testing the validity of the association.

For this reason our empirical investigation concluded with the deployment of dynamic heterogeneous panel estimation techniques – and PMG estimation in particular. In the process, we also took account of the presence of possible non-linearities in the association between governance and economic output. The elasticity range of rights under dynamic panel estimation was found to be 0.14-0.04 over the full 14-2 Freedom House index range, approximately 10 times as high as that found under FE and FEGLS estimation, but $\frac{1}{3}$ of the strength that obtained under GMM (see *beta-low* of Figure 11). Neglecting dynamics and possible heterogeneity across countries matters, and use of the PMG estimator allows us to exploit both the dynamics, as well as the power of the panel, while allowing for considerable heterogeneity across countries in estimation.

Importantly, the importance of distinguishing between countries with very poor rights, and countries with mid range rights and good rights is confirmed by the panel evidence. The elasticity range of 0.14-0.04 hides the existence of a benevolent association ranging over 0.28-0.22 over the 14-11 Freedom House rights range (the worst rights category), a benevolent 0.07-0.02 over the 7-2 rights range (the best rights), and a *perverse* elasticity range of 0.03-0.024 over the mid range rights range of 10-8 on the Freedom House scale (see *beta-variable* of Figure 11).

These findings confirm the prior intimation of a nonlinear association between rights and output suggested by Group V countries in the time series evidence. They also demonstrate that the use of the PMG estimator has allowed evidence providing confirmation of the existence of a general underlying structure governing the changes in the interaction between rights and output across rights categories.

Further important findings of this study suggest that the impact of governance may well be exercised not directly on output – but that the impact may come indirectly through the efficiency of investment expenditure, of human capital

and of policy effectiveness. Our findings suggest that the efficiency of capital accumulation is up to three times as high under mid-range to good rights than it is under poor rights. Moreover, the effectiveness of human capital investment, of improving export, FDI and inflation performance all improve with improving governance.

We note also that the lessons derived from real per capita output, of heterogeneity Types I through III apply to growth also. Substantively, our estimation results confirm an increasing productivity of investment under rising governance, and they are consistent with rising levels of investment under improving governance. Further evidence in support of the impact of good governance comes from the fact that physical capital, human capital, openness of the economy, foreign direct investment and anti-inflationary economic policy all further spur growth – often dramatically so. By contrast, for mid-range rights countries only investment in physical capital raises economic growth (though strongly so), while for poor rights countries empirical results prove mixed at best. The implications are twofold. Under good governance policy makers have the gamut of standard policy handles at their disposal in promoting growth. Their task is correspondingly easier. And on a methodological note, the finding of strong heterogeneity across countries serves to offer an explanation of why the literature may have struggled to isolate particularly robust results under cross sectional and insufficiently sophisticated panel estimators.

We conclude with some brief reflections on the implications of the evidence for policy. First, it would appear as if interventions such as those that impose performance intensity on aid dispensation does receive backing from the data. Countries with better governance do appear to experience higher growth than countries with poor governance. Strictly, the real growth pay-off to governance sets in under good governance (Freedom House index <7), rather than in the mid-range rights range in which the median level of governance in our sample lies (at approximately 9 on the composite rights scale). At least in the sense that under good governance investment in physical and human capital, and the pursuit of sound economic management all pay off to an extent that they do not under lower levels of governance. But it also remains true that it is mid-range rights countries that realize the highest growth returns on investment expenditure of any class of countries. Our evidence thus certainly suggests that poor governance is to be avoided at all costs if the objective is to raise economic growth performance.

Second, the pay-off to the performance intensity of aid may not only be higher growth in countries that respond to the incentive to improve governance. The allocative efficiency of investment may also improve under increased governance. We saw from our evidence that allowed for non-linear impacts of governance on output growth that the marginal productivity of capital appears to increase under higher levels of governance. Plausibly, therefore, aid resources stand to be employed more effectively under better governance.

But there is also evidence that provides significant nuance to this finding. We saw from the time series evidence that countries differ. Some countries may respond positively to the incentive to improve governance. In some instances, this may be due to the significant risk that an improvement in governance may lead to a collapse of output during the process of reform. For political classes in developing countries, this spells risk, and the attempt to provide positive incentives for improved governance through performance intensity of aid may not be universally successful. Different mechanisms of aid distribution, with different incentives to improve performance may be necessary in these instances.

Paying attention to methodology here matters in isolating the impact of governance on economic development. The substantive point remains, however.

Rights matter in economic development. Big time.

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Appendix 1: Definitions of Governance Variables

Polity score (POLITY) – The polity score ranges from –10 to 10 (higher is more democratic). The University of Maryland developed this indicator. It measures the following concepts of governance: competitiveness of chief executive recruitment; openness of chief executive recruitment; constraints on chief executive; regulation and competitiveness of participation; regulation of executive recruitment. The in-house expert opinion is used in constructing this indicator.

Political rights (POL_RIGHT) – Raymond Gastil developed this indicator, now administered by Freedom House. It measures free and fair elections for offices with real power; freedom of political organization; significance of opposition; freedom from domination by power groups; and political inclusion of minority groups. Expert opinion is the source of this indicator. The political rights measure ranges from 1 to 7 (lower is more free).

Civil Liberties (CIV_LIB) – Raymond Gastil developed this indicator, now administered by Freedom House. It measures freedom of expression and belief; freedom of association and organizational rights; rule of law and human rights; personal autonomy; and economic rights. Expert opinion is the source of this indicator. The political rights measure ranges from 1 to 7 (lower is more free).

Free Press (PRESS) - Freedom House developed this governance indicator. It measures the following two concepts of governance: media objectivity and freedom of expression. Expert opinion is the source of this indicator. The press freedom measure ranges from 0 to 100 (lower is more free).

Political Process (POL_PROC), Civil Society (CIV_SOC), Independent Media (IND_MED), Governance and Public Administration (GOV_PA), Constitutional, Legislative, and Judicial Framework (LAW) and Corruption (COR_FH) - Freedom House developed these governance indicators for former socialist countries in cooperation with leading scholars from transition countries, Western Europe, and United States. The survey methodology is used to construct these indicators that covers 28 transition countries. They rate countries on a comparative basis in reforming the political and economic process. These measures of governance range from 1 to 7 (lower score represents more liberal political and economic process).

Political Process (POL_PROC) examines national executive and legislative elections, the development of multiparty systems, and popular participation in the political process. The average of Political Process score is 3.85 and the median is 4.00.

Civil Society (CIV_SOC) evaluates the development of NGOs and free trade unions, their organizational capacity, financial sustainability and participation in policy process, and the legal and political environment in which they function. The average of Civil Society score is 3.60 and the median is 3.75.

Independent Media (IND_MED) assesses the legal framework for and present state of press freedom. The average of this score is 3.99 and the median is 4.25.

Governance and Public Administration (GOV_PA) addresses the authority of legislative bodies, decentralization of power and legislative and executive transparency. Its average is 4.22 and the median is 4.50.

Constitutional, Legislative, and Judicial Framework (LAW) addresses constitutional reform, human rights protection, criminal code reform, the judiciary, and judicial independence. This indicator's average is 4.11 and the median is 4.25.

Corruption (COR_FH) measures the perceptions of corruption in the civil service, the business interests of top policy makers, laws on financial disclosure and conflict of interest, and anticorruption initiatives. Its mean is 4.90 and the median is 5.25.

Economic Opacity (OPAV), Regulation Opacity (REG_PWC), and Corruption (COR_PWC) – the PriceWaterhouseCoopers developed these governance indicators for 35 countries around the world using survey methodology. These indicators address opacity (transparency) component of governance. The larger scores reflect more opacity, while smaller scores reflect more transparency.

Economic Opacity (OPAC) addresses the predictability of government policy as reflected in fiscal, monetary and foreign exchange policies.

Regulation Opacity (REG_PWC) assesses the presence or absence of clearly established rules for changing and/or consistently applying regulatory rules and procedures.

Corruption (COR_PWC) considers the effects of corruption on the cost of capital through crowding out of capital markets because of widely used politically connected and corrupt lending practices.

Regulation (HWJ), Government Intervention, Wages and Prices, Trade, Foreign Investment, Banking and Finance, Black Market, and Property Rights – The Heritage Foundation in cooperation with the Wall Street *Journal* developed these indicators as factors of the Economic Freedom Index. These indicators of governance range from 1 to 5. Lower scores indicate better governance.

Regulation (REG_HF) measures how easy or difficult it is to open and operate business. It addresses the following components: licensing requirements to operate a business, ease of obtaining a business license, corruption within the bureaucracy, labor regulations, and other regulations that impose burden on business.

Government Intervention (GOV_INT) addresses government's direct use of scarce resources for its own purposes and government's control over resources through ownership. It comprises both government consumption and government production.

Wages and Prices (WP) measures the extent to which a government allows the market to set wages and prices. It looks at which products have prices set by the government, and whether government has a minimum wage policy or otherwise influences wages.

Trade (TRADE) addresses the degree to which government hinders the free flow of foreign trade. The score is given based on a country's average tariff rate and other barriers to trade like import quotas, licensing requirements and mandates.

Foreign Investment (FOR_INV) considers countries regulations toward foreign investment. It examines the investment laws and procedures.

Banking and Finance (BANKING) indicator measures the relative openness of banking and financial system in a country. It is scored by assessing how difficult it is to open domestic banks and other financial services firms, how heavily regulated the financial system is, and whether foreign banks and financial services firms are able to operate freely.

Black Market (BLK_MKT) indicator is scored relying on Transparency International's Corruption Perceptions Index and the specific procedures that consider the extent to which black market activities occur. It measures black market activities in the production, distribution, or consumption of goods and services.

Property Rights (PROPERTY) measures the degree to which private property rights are protected and the degree to which the government enforces laws that protect private property. It also considers the independence of the judiciary and the ability of individuals and businesses to enforce contracts.

Contract Intensive Money (CIM) measures the proportion of the money supply that is held in the bank accounts and as other financial assets. The percentage of contract intensive money indicates in part how much faith investors have in the government's ability and willingness to enforce financial contracts, and to refrain from expropriating financial assets. It is a measure of trust in banks and in the government. Contract intensive money is calculated as one minus the ratio of currency outside of banks to the money supply.

Trade Tax (TRTAX) indicator estimates the share of trade taxes as a percentage of total government revenues. It reflects government's administrative capacity and trade policy.

Trade Tax Controlling for Population (trtax_p) is adjusted for country size by calculating the deviation of trade tax revenues from the value that would be expected based on country size. Data on trade tax revenues included in the IMF's Government Finance Statistics.

Business Start-up Procedures-Number of Procedures (BSP_NOP), Business Start-up Procedures-Time (BSP_T) and Business Start-up Procedures-Cost (BSP_C) – These indicators address procedures, time, and costs of starting new businesses across countries. Data are collected from government publications, reports of development agencies, and local law firms. The focus is a “standardized firm” that operates in the largest city, performing general industrial or commercial activities.

Business Start-up Procedures-Number of Procedures (BSP_NOP) assesses the number of procedures that are officially required to obtain all necessary permits and completing all of the required notifications for the company to operate legally.

Business Start-up Procedures-Time (BSP_T) estimates the minimum number of business days required to complete the business start-up process assuming no delays by government officials.

Business Start-up Procedures-Cost (BSP_C) evaluates the cost of business start up in fees, photocopies, notary charges, etc.

Budget Volatility (BUD_VOL) assesses the coherence and predictability of government policy for business. It assumes that to the extent that policy decisions are captured in the budget, then stable policy should be reflected in stable budget allocations, and vice versa. This indicator is estimated as the median of the year-to-year changes in each of the 14 functional budget classifications over the preceding 4 years, where budget changes are defined as the difference in expenditure shares for each functional classification from year n to year $n+1$, calculated as a proportion of the year n figure. Primary data that are used to construct budget volatility indicator is available from the IMF's Government Financial Statistics.

Regulation of Dispute Resolution Index-Eviction of Tenant (EVICT), Regulation of Dispute Resolution Index-Check Collection (CHK_COL) – By surveying members of

the largest international association of law firms, data have been collected on the number of “independent procedural actions” required to file a complaint and to obtain and enforce a judgment in each of two areas of dispute: eviction of non-paying tenants, and collection of overdue debt. These indices estimate the duration in days from initiation to completion of the process, for debt collection and for rent eviction. They reflect only procedures for simple cases, where facts are not in dispute, and the amount of the claim is relatively small.

Policy Unpredictability (POL_UNP) indicator addresses businesspersons’ perceptions of policy unpredictability as an obstacle to conducting business. It is obtained from World Development Report’s (1997) Private Sector Survey. The respondents are asked, “Do you regularly have to cope with unexpected changes in rules, laws, or policies which materially affect your business?” on a scale of 1 to 6 (1 is completely predictable, 6 is completely unpredictable). The score for this indicator is the average answer from all businesspersons surveyed in the country.

Quality of Service (QUAL_SER) indicator assesses perceptions of quality of government service. It is obtained from World Development Report’s (1997) Private Sector Survey. The respondents are asked to “rate the efficiency of government in delivering services” on a scale of 1 to 6 (1 indicates very efficient and 6 indicates very inefficient). The score for this indicator is the average answer from the country’s respondents.

Growth Competitiveness Index (GCOMP) – The GCI measures the capacity of the national economy to achieve sustained economic growth over the medium term, controlling for current level of development. It is published by the World Economic Forum.

Microeconomic Competitiveness Index (CCOMP) – The MCI is the weighted average of the two indices that measure the quality of the national business environment and the sophistication of company operations and strategy. This index is published by the World Economic Forum. In the 2004 *World Competitiveness Report*, it is renamed the Business Competitiveness Index.

Corruption Perceptions Index (COR_TI) - Transparency International’s Corruption Perceptions Index ranks countries in terms of the degree to which corruption is perceived to exist among public officials and politicians. CPI relates to perceptions of the degree of corruption as seen by business people, academics, and risk analysts. It ranges between 10 (very clean) and 0 (very corrupt).

The next six indicators of governance are from the World Bank’s *Governance Indicators Report*, perhaps the most sophisticated analysis of governance measures. In fact, this report includes proprietary World Bank and other data not available to the public. All six indicators vary between –2.5 (lowest quality of governance) and 2.5 (high quality).

Voice and Accountability (VA) includes in it a number of indicators measuring various aspects of political process, civil liberties and political rights.

Political Stability (PS) combines several indicators that measure perceptions of the likelihood that the government in power will be destabilized or overthrown by possible unconstitutional means.

Government Effectiveness (GE), which combines perceptions of the quality and competence of civil service, the independence of public service from political pressure, and credibility of government into a single group.

Regulatory Quality (RQ) measures of the incidence of market-unfriendly policies as well as perception of the burdens imposed by excessive regulations.

The Rules of Law (RL) indicator includes several indicators, which measure the extent to which agents have confidence in and abide by the rules of society.

Control of Corruption (CONCOR) is sixth cluster. It measures perceptions of corruption, conventionally defined as the exercise of public service for private gain.

The following indicators have been obtained from Political Systems of the World of J Denis Derbyshire and Ian Derbyshire (1996)

Nation States or sovereign states defined following Lane and Ersson (1994) as “a state that recognizes no higher decision-making power outside itself”. The nation-state, or ‘stato’, of the Weberian type, is characterized by the monopoly of the legitimate use of force within a specified territory and the concentration of power in an impersonal administrative organization.

1st Year of State Formation indicates first date at which nation state was established

2nd Year of State Formation indicates second date at which nation state was established

Regions classifies the geographic locations of the countries concerned and have been chosen by Derbyshire and Derbyshire (1996) as the most manageable way to demonstrate the link between the history; geography and social development of these countries. The nine regions are: Asia; Central America and the Caribbean; Central, Eastern, and Southern Europe; Central and Southern Africa (essentially Sub-Saharan Africa); the Middle East and North Africa; North America; Northern and Western Europe; Oceania; South America.

Colonization - Colonizing powers listed are Australia; Belgium; Britain; China; France; Italy; Netherlands; Portugal; Spain; South Africa; Russia (then USSR); USA. Various countries have emerged from a prior state of administration by another . These variables indicate the last external power (if any) to have administered a specific country.

Year of decolonization or transfer of sovereignty indicates the year in which the country was granted independence.

Date of establishment of control indicates the year in which the last colonizing power established power in that country.

An **Ideological base** is defined by a specific body of ideas which reflects the beliefs and values of a nation and its political system. Political regimes are identified as follows:

Liberal Democracy (Lib-dem) is a product of two concepts: the right to representative government (Liberal) and the right to enjoy individual freedom (democracy).

Emergent Democracy (Em-dem) is attributed to states with instability in their political systems but which do bear many characteristics of Liberal democracy.

Communism (Commun) is characterized by four distinguishing features:

1. Marxism-Leninism (in the case of China, Maoism-Dengism) has been adopted as the official ideology, source of legitimacy, and vocabulary of the political affairs.
2. The bulk of economic activity is under state ownership and subject to administrative (central) planning.
3. One party, the Communist Party, dominates the political scene and is tightly controlled from above in accordance with the Leninist precept of “democratic centralism”.
4. The influence of the Communist Party, constitutionally ascribed a leading role” in the nation’s affairs, is all-pervasive, controlling state organs, trade unions, the media, the judiciary, and the industrial and agricultural enterprises through both supervision and direct membership.

National Socialism (Nat-soc) categorizes states in which many of the attributes of a communist state are present but in a less developed and structured form.

Authoritarian Nationalism (Auth-nat) indicates an extreme kind of nationalism that rests on the belief that a specific race or creed is so unique that they have the right to be regarded a nation and to exclude others. Features of a state that subscribes to this ideology are:

1. Restrictions on the activities of all political parties, or a limitation to one which gives undivided and uncritical support to the state.
2. An authoritarian charismatic personal or collective executive.
3. Either the absence of an assembly to balance the power of the executive or the presence of an assembly which is essentially the servant of the executive.

Military Authoritarianism is a form of authoritarian nationalism whereby military leaders take it upon themselves to impose a government on the people by overthrowing civilian administrations.

Islamic Nationalism is attributed to those states in which Islamic fundamentalism fulfills a political function.

Absolutism is attributed to states with an absolute monarch as the legitimate government.

Political executives are defined as the permanent executive consisting of the salaried civil service which normally remains in office regardless of the political party in power. Derbyshire and Derbyshire (1996) have divided political executives into seven types:

Parliamentary executive often referred to as the “Westminster model” and displays three essential features:

1. The role of head of state is separate from that of the head of government and is distant from party politics, serving mainly as the patriotic and ceremonial focus of the nation.
2. The executive is drawn from the assembly and directly responsible to it, and its security of tenure is dependent on the support of the assembly, or parliament.

3. The leader of the party, or coalition of parties, commanding the support of parliament is called upon by the head of state, monarch or president, to become prime minister and form a government.

An example is the United Kingdom.

Limited presidential executive is characterized by four key features:

1. Presidents are elected for a fixed term to perform the dual role of head of state and head of government
2. Presidents' tenure are secure unless they commit a grave unconstitutional act.
3. Presidents govern with an advisory cabinet of nonelected departmental secretaries, whom they choose and appoint and who are fully responsible to them.
4. Presidential powers are limited by the need for the approval of the assembly for certain executive actions.

An example is the USA.

The **Dual executive** provides for a president with considerable power, including appointment of the senior executive, the prime minister and cabinet are expected to wield ultimate power, while the president remains aloof from day to day politics. An example is France.

The **Communist executive**: the Communist Party determines policy objectives and it is the state apparatus which implements them. The constitution is subservient to the needs of the state, as interpreted by the party and is often changed to meet party requirements.

The (unlimited) **Presidential executive** is found in one-party, non-communist states

The **Military**: the military acts as executive.

The **Absolute executive** applies to monarchies and not imposed following a coup.

Lower house electoral system – voting systems

Indirect: **Lower House members elected by another representative or institutional body**

Simple Plurality (SP): “winner-takes-all ‘first-past-the-post’” method.

Elected / Appointed (mixed E/A)

Proportional representation – party list (PR-PL): proportional representation under political party lists.

Proportional representation – additional member system (PR-AMS): makes use of party lists but also allows the elector two votes – one for the candidate and one for the party. Half the assembly is then elected on a SB or SP basis and the other half using the party lists.

Proportional representation - single transferable vote (PR-STV): proportional representation with voters providing rankings of candidates.

Second ballot (SB): A simple majority election is held and if no candidate gets more than 50% of the total vote, the candidate with the least votes is eliminated and a second election is held.

Appointed (A): Lower House members appointed, rather than voted for.

Transitional (TRANS): states where established political order has disintegrated, without replacement by another.

Political Parties: can be described as an association of people who hold similar views about what should be a community's social and economic priorities and come together to establish these priorities by gaining control of the machinery of government. The modern party displays three essential features: a permanent structure and organization; an authority to represent people, whether or not they are members of the party, based on open elections; and, an intention to form a government or participate in government.

Number of parties regularly operating (does not include all registered parties and is an approximation in some cases because the emergence and disappearance of minor groupings is often a notable feature of some political systems)

Number of parties with >10% of assembly seats:

Appendix 2: Augmented Dickey-Fuller Test Statistics

Country	RGDP		Investment		Political Rights		Civil Liberties		Composite Rights	
	$\sim I(0)$	$\sim I(1)$	$\sim I(0)$	$\sim I(1)$	$\sim I(0)$	$\sim I(1)$	$\sim I(0)$	$\sim I(1)$	$\sim I(0)$	$\sim I(1)$
Afghanistan	-	-	-	-	-5.2327* (-2.9850)	-7.2111* (-2.9907)	-4.6444* (-2.9850)	-4.8296* (-2.9907)	-5.0782* (-2.9850)	-6.1559* (-2.9907)
Albania	-1.8274 (-3.0660) †	-2.5399 (-3.0819) †	-	-	-1.1954 (-2.9850)	-3.2106* (-2.9907)	-1.2104 (-2.9850)	-3.4574* (-2.9907)	-1.85918 (-2.9850)	-3.0614* (-2.9907)
Algeria	-2.1848 (-2.9850) †	-3.2250* (-2.9907) †	-1.8739 (-2.9850) †	-3.8527* (-2.9907) †	-3.4256* (-2.9850)	-4.7956* (-2.9907)	-3.6064* (-2.9850)	-3.8216* (-2.9907)	-3.3607* (-2.9850)	-4.5703* (-2.9907)
Angola	-1.5893 (-3.0660) †	-2.5039 (-3.0819) †	-	-	-1.35725 (-3.0039)	-7.1151* (-3.0115)	-3.0990* (-3.0039)	-4.9338* (-3.0115)	-2.4328 (-3.0039)	-5.0618* (-3.0115)
Argentina	-1.6947 (-2.9850) †	-4.3221* (-2.9907) †	-2.0982 (-2.9850) †	-4.2426* (-2.9907) †	-1.6894 (-2.9850)	-3.8546* (-2.9907)	-2.7356 (-2.9850)	-4.4222* (-2.9907)	-1.6413 (-2.9850)	-4.0625* (-2.9907)
Armenia!!	-1.3131 (-3.5512) †	-4.4648* (-3.7449) †	-	-	-2.7296 (-3.5512)	-1.7372 (-3.7449)	-9.4868* (-3.2197)	-20.3333* (-3.2698)	-2.2361 (-3.5512)	-2.9459 (-3.7449)
Australia	.58862 (-2.9850) †	-4.5137* (-2.9907) †	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Austria	-1.1908 (-2.9850) †	-5.2776* (-2.9907)	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Azerbaijan!!	-2.3059 (-3.2197) †	-1.3890 (-3.2698) †	-	-	No Var.	No Var.	-2.7386 (-3.7449)	-.0000 (-4.0703)	-.45515 (-3.5512)	-9.4491* (-3.7449)
Bahamas	-3.6819* (-2.9850) †	-4.1617* (-2.9907) †	-	-	-1.4460 (-2.9850)	-4.6904* (-2.9907)	-.97128 (-2.9850)	-4.7569* (-2.9907)	-1.0247 (-2.9850)	-4.7186* (-2.9907)
Bahrain	-.58931 (-3.0819) †	-3.7223* (-3.1004) †	-	-	-1.3641 (-2.9850)	-4.0337* (-2.9907)	-1.7720 (-2.9850)	-3.8627* (-2.9907)	-1.3205 (-2.9850)	-4.2555* (-2.9907)
Bangladesh	1.7243 (-2.9850) †	-6.4509* (-2.9907) †	-.90114 (-2.9850)	-4.0826* (-2.9907)	-2.6778 (-2.9850)	-4.9744* (-2.9907)	-2.5475 (-2.9850)	-5.7446* (-2.9907)	-2.4115 (-2.9850)	-4.4828* (-2.9907)
Barbados	-1.0976 (-2.9850) †	-3.2225* (-2.9907)	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Belarus!!	-3.3136* (-3.2698) †	-1.0251 (-3.3353) †	-	-	-1.3295 (-3.2698)	-3.8730* (-3.3353)	-.78881 (-3.2698)	-4.2500* (-3.3353)	-.74846 (-3.5512)	.14907 (-3.7449)
Belgium	-.38034 (-2.9850) †	-5.0419* (-2.9907) †	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Belize	-.50073 (-2.9850) †	-3.1399* (-2.9907) †	-	-	-4.2426* (-3.0401)	-10.0995* (-3.0522)	-1.9008 (-3.0660)	-3.6056* (-3.0819)	-.68313 (-3.0660)	-6.4531* (-3.0819)
Benin	-1.4585 (-2.9850) †	-4.2255* (-2.9907) †	-1.8243 (-2.9850) †	-5.7178* (-2.9907) †	-.88440 (-2.9850)	-3.6747* (-2.9907)	-.80541 (-2.9850)	-4.4167* (-2.9907)	-1.1272 (-2.9850)	-3.2734* (-2.9907)
Bhutan	-.93796 (-3.0660) †	-3.7775* (-3.0819) †	-	-	-1.0693 (-2.9798)	-5.5012* (-2.9850)	-1.4695 (-2.9798)	-3.8397* (-2.9850)	-1.4098 (-2.9850)	-4.2588* (-2.9907)
Bolivia	-2.9693 (-2.9850) †	-3.0642* (-2.9907) †	-.67254 (-2.9850) †	-4.0097* (-2.9907) †	-1.5872 (-2.9850)	-6.7698* (-2.9907)	-3.3675* (-2.9850)	-4.7619* (-2.9907)	-1.8056 (-2.9850)	-6.7402* (-2.9907)
Bosnia!!	-2.3245 (-4.0703) †	-6.6525* (-4.7059) †	-	-	-1.3693 (-3.3353)	-2.6458 (-3.4243)	-.44721 (-3.7449)	-2.0000 (-4.0703)	-.44721 (-3.7449)	-2.0000 (-4.0703)
Botswana	-.83385 (-2.9850) †	-3.1210* (-2.9907) †	-3.8730* (-3.1004)	-3.2173* (-3.1223)	-2.0052 (-2.9850)	-4.6904* (-2.9907)	-1.6891 (-2.9850)	-4.7749* (-2.9907)	-2.1233 (-2.9850)	-4.3886* (-2.9907)
Brazil	-2.0383 (-2.9850) †	-4.2210* (-2.9907) †	-.98375 (-2.9850) †	-4.2379* (-2.9907) †	-1.8626 (-2.9850)	-5.8054* (-2.9907)	-2.4735 (-2.9850)	-5.4498* (-2.9907)	-2.3967 (-2.9850)	-5.5255* (-2.9907)

Brunei	-1.1783 (-3.0115) ‡	-3.6920* (-3.0199) ‡	-	-	-.74297 (-2.9850)	-4.8990* (-2.9907)	-1.4220 (-2.9850)	-6.3246* (-2.9907)	-1.0090 (-2.9850)	-5.7411* (-2.9907)
Bulgaria	-2.2520 (-3.0660) ‡	-2.5974 (-3.0819) ‡	-	-	-.38308 (-2.9850)	-3.8739* (-2.9907)	-.91754 (-2.9850)	-3.6332* (-2.9907)	-.49032 (-2.9850)	-3.7953* (-2.9907)
Burkina Faso	.41549 (-2.9850) ‡	-6.2937* (-2.9907) ‡	-.28356 (-2.9850) ‡	-4.8723* (-2.9907) ‡	-1.9894 (-2.9850)	-3.8334* (-2.9907)	-1.7664 (-2.9850)	-4.4780* (-2.9907)	-1.7829 (-2.9850)	-4.7587* (-2.9907)
Burundi	-.81952 (-2.9850) ‡	-3.5951* (-2.9907) ‡	-2.9147 (-2.9850) ‡	-6.0579* (-2.9907) ‡	-3.1897* (-2.9850)	-6.3460* (-2.9907)	-3.3912* (-2.9850)	-6.9570* (-2.9907)	-3.9932* (-2.9850)	-7.4421* (-2.9907)
Cambodia	-1.0073 (-3.2698) ‡	-5.0254* (-3.3353) ‡	-	-	-2.0337 (-2.9850)	-5.1596* (-2.9907)	-.74297 (-2.9850)	-4.2378* (-2.9907)	-1.5169 (-2.9850)	-4.8629* (-2.9907)
Cameroon	-2.2196 (-2.9850) ‡	-3.0905* (-2.9907) ‡	-2.7288 (-2.9850) ‡	-3.4019* (-2.9907) ‡	-2.3852 (-2.9850)	-4.6904* (-2.9907)	-2.0414 (-2.9850)	-4.7300* (-2.9907)	-2.5708 (-2.9850)	-4.7368* (-2.9907)
Canada	-.58940 (-2.9850) ‡	-3.3655* (-2.9907) ‡	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Cape Verde	1.3335 (-3.0819) ‡	-3.1129* (-3.1004) ‡	-	-	-.90481 (-3.0039)	-4.3474* (-3.0115)	-.55096 (-3.0039)	-3.6673* (-3.0115)	-.98255 (-3.0039)	-3.2236* (-3.0115)
Cen. Afr. Rep.	-1.2019 (-2.9850) ‡	-5.6016* (-2.9907) ‡	-	-	-1.2132 (-2.9798)	-4.0804* (-2.9850)	-1.6626 (-2.9798)	-4.5007* (-2.9850)	-1.3388 (-2.9850)	-4.0077* (-2.9907)
Chad	-2.2921 (-2.9850) ‡	-4.8695* (-2.9907) ‡	-	-	-2.2866 (-2.9850)	-5.8180* (-2.9907)	-.74975 (-2.9850)	-7.4947* (-2.9907)	-.65911 (-2.9850)	-7.3724* (-2.9907)
Chile	.17726 (-2.9850) ‡	-3.8351* (-2.9907) ‡	-.66717 (-2.9850) ‡	-5.2443* (-2.9907) ‡	-1.0124 (-2.9850)	-4.1558* (-2.9907)	-1.2614 (-2.9850)	-2.2752 (-2.9907)	-1.0742 (-2.9850)	-3.1977* (-2.9907)
China	.51003 (-2.9850) ‡	-4.6353* (-2.9907) ‡	.42410 (-2.9850) ‡	-4.2851* (-2.9907) ‡	-1.4154 (-2.9850)	-6.7759* (-2.9907)	-2.2551 (-2.9850)	-6.4398* (-2.9907)	-1.9245 (-2.9850)	-6.4398* (-2.9907)
Colombia	-1.4840 (-2.9850) ‡	-3.4204* (-2.9907) ‡	-1.9041 (-2.9850) ‡	-4.2269* (-2.9907) ‡	-1.0644 (-2.9907)	-4.3877* (-2.9907)	-.91667 (-2.9907)	-4.7958* (-2.9907)	-1.9328 (-2.9850)	-.87513 (-2.9907)
Congo Brazz.	-.26057 (-2.9970) ‡	-2.2050 (-3.0039) ‡	-	-	-1.9494 (-2.9850)	-4.9982* (-2.9907)	-1.6444 (-2.9850)	-3.8840* (-2.9907)	-1.8557 (-2.9850)	-3.8186* (-2.9907)
Costa Rica	-.72213 (-2.9850) ‡	-3.0654* (-2.9907) ‡	-	-	No Var.	No Var.	-.65908 (-2.9798)	-5.0000* (-2.9850)	-.67823 (-2.9850)	-4.8990* (-2.9907)
Cote d Ivoire	-1.0332 (-2.9850) ‡	-3.7005* (-2.9907) ‡	-1.4817 (-2.9850) ‡	-4.5207* (-2.9907) ‡	-2.0972 (-2.9850)	-3.4933* (-2.9907)	-2.7335 (-2.9850)	-4.4880* (-2.9907)	-2.7276 (-2.9850)	-4.9259* (-2.9907)
Croatia!!	-3.9081* (-3.5512) ‡	-1.1328 (-3.7449) ‡	-	-	-.40825 (-3.5512)	-2.2361 (-3.7449)	1.6330 (-3.5512)	-.44721 (-3.7449)	.27217 (-3.5512)	-1.6398 (-3.7449)
Cuba	-	-	-	-	-1.4460 (-2.9850)	-4.6904* (-2.9907)	-.95917 (-2.9850)	-4.8990* (-2.9907)	-1.4248 (-2.9850)	-3.3517* (-2.9907)
Cyprus	-1.1110 (-3.0115) ‡	-5.2903* (-3.0199) ‡	-	-	-2.1448 (-2.9850)	-4.8990* (-2.9907)	-1.9577 (-2.9850)	-3.6795* (-2.9907)	-2.0898 (-2.9850)	-3.8431* (-2.9907)
Czech Rep.	-1.8893 (-3.5512)	-3.6663* (-3.7449)	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Denmark	-.32814 (-2.9850) ‡	-4.9457* (-2.9907) ‡	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Djibouti	-1.5159 (-3.2698) ‡	-.42064 (-3.3353) ‡	-	-	-2.4937 (-3.0199)	-4.2605* (-3.0294)	-3.2898* (-3.0199)	-4.8033* (-3.0294)	-3.2400* (-3.0199)	-6.1789* (-3.0294)
Dom. Rep.	1.6335 (-2.9850) ‡	-3.5956* (-2.9907) ‡	-.45226 (-2.9850) ‡	-4.4964* (-2.9907) ‡	-2.2036 (-2.9850)	-4.3084* (-2.9907)	-2.7242 (-2.9850)	-5.4668* (-2.9907)	-1.3984 (-3.0199)	-4.7125* (-3.0294)
Ecuador	-3.5635* (-2.9850) ‡	-5.2970* (-2.9907) ‡	-3.6991* (-2.9850) ‡	-7.4437* (-2.9907) ‡	-3.4256* (-2.9850)	-4.1210* (-2.9907)	-3.2422* (-2.9850)	-4.3084* (-2.9907)	-3.5619* (-2.9850)	-3.1962* (-2.9907)
Egypt	-2.7672 (-2.9850) ‡	-4.2028* (-2.9907) ‡	-1.1769 (-2.9850) ‡	-3.4273* (-2.9907) ‡	-1.2190 (-2.9850)	-4.5907* (-2.9907)	-1.7150 (-2.9850)	-5.1649* (-2.9907)	-1.1910 (-2.9850)	-4.6379* (-2.9907)
El Salvador	-1.5960 (-2.9850) ‡	-2.8600 (-2.9907) ‡	-.72334 (-2.9850)	-3.8056* (-2.9907)	-1.7744 (-2.9850)	-3.7559* (-2.9907)	-1.9778 (-2.9850)	-6.0553* (-2.9907)	-1.2729 (-2.9850)	-4.9556* (-2.9907)

Equat. Guin.!!	1.4468 (-3.1803) ‡	-1.9520 (-3.2197) ‡	-	-	-5.2327* (-2.9850)	-6.5638* (-2.9907)	-2.4573 (-2.9850)	-5.4040* (-2.9907)	-2.5730 (-2.9850)	-5.1732* (-2.9907)
Eritrea!!	-.78647 (-4.0703) ‡	1.6179 (-4.7059) ‡	-	-	-1.00000 (-4.0703)	-1.7321 (-4.7059)	.57735 (-4.0703)	-.94868 (-3.5512)	-.44721 (-4.0703)	-4.0415 (-4.7059)
Estonia!!	-2.0673 (-3.0819) ‡	-1.7085 (-3.0819) ‡	-	-	-1.9506 (-3.3353)	-2.4495 (-3.4243)	-2.5298 (-3.2197)	-3.0000 (-3.2698)	-1.7876 (-3.3353)	-1.6408 (-3.4243)
Ethiopia	-1.4660 (-3.0819) ‡	-3.7602* (-3.1004) ‡	-	-	-1.1993 (-2.9850)	-4.7913* (-2.9907)	-1.1843 (-2.9850)	-5.0628* (-2.9907)	-.92861 (-2.9850)	-5.3225* (-2.9907)
Fiji	-2.0820 (-2.9850) ‡	-6.4423* (-2.9907) ‡	-	-	-1.4258 (-2.9850)	-7.7476* (-2.9907)	-2.1744 (-2.9850)	-3.9988* (-2.9907)	-2.1855 (-2.9850)	-6.9181* (-2.9907)
Finland	-.14072 (-2.9850) ‡	-3.1253* (-2.9907) ‡	-	-	-1.1349 (-2.9850)	-4.8990* (-2.9907)	-.95917 (-2.9850)	-4.8990* (-2.9907)	-1.2198 (-2.9850)	-5.1381* (-2.9907)
France	-1.2451 (-2.9850) ‡	-3.2128* (-2.9907) ‡	-	-	No Var.	No Var.	-5.1439* (-2.9850)	-6.3762* (-2.9907)	-5.1439* (-2.9850)	-6.3762* (-2.9907)
Gabon	-2.4426 (-2.9850) ‡	-7.2820* (-2.9907) ‡	-2.8212 (-2.9850) ‡	-5.5214* (-2.9907) ‡	-1.6745 (-2.9850)	-4.0347* (-2.9907)	-.92154 (-2.9850)	-3.7607* (-2.9907)	-.90375 (-2.9850)	-4.6884* (-2.9907)
Gambia	-2.4622 (-2.9850) ‡	-3.5348* (-2.9907) ‡	-5.7005* (-2.9850) ‡	-3.2925* (-2.9907) ‡	-1.3990 (-2.9850)	-3.8419* (-2.9907)	-1.7189 (-2.9850)	-5.3841* (-2.9907)	-1.4784 (-2.9850)	-4.4777* (-2.9907)
Georgia!!	-1.6059 (-3.5512) ‡	-1.4270 (-3.7449) ‡	-	-	1.6353 (-3.5512)	-1.1471 (-3.7449)	-1.1386 (-3.2698)	-2.8284 (-3.3353)	-2.2361 (-3.5512)	-1.4536 (-3.7449)
Germany	-1.3973 (-2.9907) ‡	-3.0002* (-2.9970) ‡	-	-	No Var.	No Var.	-3.5164* (-2.9798)	-6.9222* (-2.9850)	-3.5164* (-2.9798)	-6.9222* (-2.9850)
Ghana	-1.1012 (-2.9850) ‡	-3.3247* (-2.9907) ‡	-1.7971 (-2.9850) ‡	-6.3571* (-2.9907) ‡	-1.4104 (-2.9850)	-4.1218* (-2.9907)	-1.3027 (-2.9850)	-4.7569* (-2.9907)	-1.1896 (-2.9850)	-4.0150* (-2.9907)
Greece	.032765 (-2.9850) ‡	-3.0198* (-2.9907) ‡	-1.1782 (-2.9907) ‡	-5.5143* (-2.9970) ‡	-2.0755 (-2.9850)	-4.7569* (-2.9907)	-.67823 (-2.9850)	-4.8990* (-2.9907)	-2.2760 (-2.9850)	-4.6904* (-2.9907)
Guatemala	-4.4399* (-2.9850) ‡	-3.1635* (-2.9907) ‡	-1.1247 (-2.9850) ‡	-4.6426* (-2.9907) ‡	-2.6617 (-2.9850)	-3.8582* (-2.9907)	-4.4158* (-2.9850)	-3.7938* (-2.9907)	-3.6167* (-2.9850)	-3.2274* (-2.9907)
Guinea	.38944 (-3.2197) ‡	-3.0390 (-3.2698) ‡	-	-	-.88230 (-2.9850)	-4.8990* (-2.9907)	-1.0355 (-2.9850)	-4.8990* (-2.9907)	-.73312 (-2.9850)	-4.7549* (-2.9907)
Guinea-Bissau	-2.6587 (-2.9850) ‡	-5.8907* (-2.9907) ‡	-	-	-1.1820 (-2.9907)	-4.9441* (-3.0039)	-1.2082 (-2.9907)	-3.6795* (-3.0039)	-1.0548 (-2.9907)	-4.5539* (-3.0039)
Guyana	-1.6536 (-2.9850) ‡	-2.5560 (-2.9907) ‡	-2.4568 (-2.9850) ‡	-4.1846* (-2.9907) ‡	-1.0887 (-2.9850)	-3.5123* (-2.9907)	-.57894 (-2.9850)	-3.8566* (-2.9907)	-.87232 (-2.9907)	-3.3303* (-3.0039)
Haiti	.038384 (-2.9850) ‡	-4.4675* (-2.9907) ‡	-2.0531 (-2.9850) ‡	-5.2495* (-2.9907) ‡	-2.5389 (-2.9850)	-4.8512* (-2.9907)	-2.9764 (-2.9850)	-6.1329* (-2.9907)	-2.7955 (-2.9850)	-4.6914* (-2.9907)
Honduras	-4.5995* (-2.9850) ‡	-3.6878* (-2.9907) ‡	-.50250 (-2.9850) ‡	-4.2398* (-2.9907) ‡	-2.2529 (-2.9907)	-4.3268* (-2.9907)	No Var.	No Var.	-2.2529 (-2.9907)	-4.3268* (-2.9907)
Hungary	-1.3158 (-2.9850) ‡	-3.0871* (-2.9907) ‡	-1.1451 (-2.9850) ‡	-3.1230* (-2.9907) ‡	-.46409 (-2.9850)	-3.9879* (-2.9907)	-1.3919 (-2.9850)	-4.5583* (-2.9907)	-.67023 (-2.9907)	-3.7296* (-2.9907)
Iceland	-2.0513 (-2.9850) ‡	-3.1089* (-2.9907) ‡	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
India	1.1741 (-2.9850) ‡	-5.2014* (-2.9907) ‡	1.5513 (-2.9850) ‡	-6.0617* (-2.9907) ‡	-1.8630 (-2.9850)	-4.6904* (-2.9907)	-4.0958* (-2.9850)	-6.6769* (-2.9907)	-2.1319 (-2.9850)	-3.8722* (-2.9907)
Indonesia	-1.6944 (-2.9850) ‡	-3.5094* (-2.9907) ‡	-2.4979 (-2.9850) ‡	-3.0490* (-2.9798) ‡	-2.8933 (-2.9850)	-2.7464 (-2.9907)	-2.5294 (-2.9850)	-3.8481* (-2.9907)	-2.8927 (-2.9850)	-3.1177* (-2.9907)
Iran	-2.7321 (-3.0039) ‡	-3.3453* (-3.0115) ‡	-	-	-2.3860 (-2.9850)	-4.6904* (-2.9907)	-2.2658 (-2.9850)	-5.7094* (-2.9907)	-1.9618 (-2.9850)	-5.4819* (-2.9907)
Iraq	-	-	-	-	-2.0052 (-2.9850)	-4.6904* (-2.9907)	-5.2327* (-2.9850)	-5.6125* (-2.9907)	-3.3578* (-2.9850)	-8.1240* (-2.9907)
Ireland	2.6857 (-2.9850) ‡	-2.5410 (-2.9907) ‡	-	-	No Var.	No Var.	-3.4101* (-2.9850)	-5.1956* (-2.9907)	-3.4101* (-2.9850)	-5.1956* (-2.9907)

Israel	1.0023 (-2.9850) ‡	-4.3500* (-2.9907) ‡	-.64709 (-2.9850) ‡	-4.2930* (-2.9907) ‡	-.63328 (-2.9907)	-4.7958* (-2.9970)	-2.2532 (-2.9907)	-5.5105* (-2.9970)	-2.9155 (-3.0039)	-6.5003* (-2.9970)
Italy	-2.6541 (-2.9850) ‡	-3.9504* (-2.9907) ‡	-	-	-2.5377 (-2.9850)	-6.2436* (-2.9907)	-2.2551 (-2.9850)	-4.8905* (-2.9907)	-1.9752 (-2.9907)	-4.4721* (-2.9970)
Jamaica	-2.4838 (-2.9850) ‡	-3.2001* (-2.9907) ‡	-.98079 (-2.9850) ‡	-4.1148* (-2.9907) ‡	-3.2581* (-2.9750)	-5.0990* (-2.9798)	-1.6729 (-2.9850)	-4.7569* (-2.9907)	-2.6695 (-2.9850)	-5.4540* (-2.9907)
Japan	-1.4392 (-2.9850) ‡	-3.1672* (-2.9798) ‡	-	-	-2.6173 (-2.9850)	-4.7154* (-2.9907)	-.81064 (-2.9850)	-4.8990* (-2.9907)	-1.6204 (-2.9850)	-4.6904* (-2.9907)
Jordan	-2.6395 (-3.0115) ‡	-3.0707* (-3.0199) ‡	-	-	-1.6818 (-2.9850)	-3.9390* (-2.9907)	-1.7756 (-2.9850)	-4.8737* (-2.9907)	-1.6490 (-2.9850)	-4.7625* (-2.9907)
Kasakhstan!!	-1.6816 (-3.4243) ‡	-1.3037 (-3.5512) ‡	-	-	-2.5298 (-3.2197)	-3.0000 (-3.2698)	-4.3028* (-3.2197)	-9.7011* (-3.2698)	-4.1952* (-3.2197)	-4.0734* (-3.2698)
Kenya	-3.9575* (-2.9850) ‡	-3.2292* (-2.9907) ‡	-2.6904 (-2.9850) ‡	-5.0592* (-2.9907) ‡	-2.4303 (-2.9850)	-4.3477* (-2.9907)	-1.9017 (-2.9850)	-5.5498* (-2.9907)	-2.0833 (-2.9850)	-5.1719* (-2.9907)
Korea N	-	-	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Korea S	-.84245 (-2.9850) ‡	-4.3703* (-2.9907) ‡	-1.8669 (-2.9850) ‡	-4.2119* (-2.9907) ‡	-1.1615 (-2.9850)	-5.7900* (-2.9907)	-1.0222 (-2.9850)	-4.5879* (-2.9907)	-1.0150 (-2.9850)	-4.6855* (-2.9907)
Kuwait	-1.2052 (-3.1004) ‡€ -2.1806 (-4.0703) ‡€	-2.6144 (-3.1223) ‡€ -2.5081 (-4.7059) ‡€	-	-	-1.8737 (-2.9850)	-4.7569* (-2.9907)	-2.0761 (-2.9850)	-6.6923* (-2.9907)	-2.9276 (-2.9850)	-6.6735* (-2.9907)
Kyrgyzstan!!	-4.1306* (-3.2197) ‡	-1.5543 (-3.2698) ‡	-	-	-1.4142 (-3.5512)	-3.8730* (-3.7449)	-1.0000 (-3.5512)	-2.1381 (-3.7449)	-.042576 (-3.5512)	-3.2733 (-3.7449)
Laos	-.66330 (-3.1485) ‡	-4.7998* (-3.1803) ‡	-	-	-2.0052 (-2.9850)	-4.6904* (-2.9907)	-1.6891 (-2.9850)	-6.7759* (-2.9907)	-2.1080 (-2.9850)	-7.2664* (-2.9907)
Latvia!!	-2.3059 (-2.9850) ‡	-2.5930 (-2.9907) ‡	-	-	-2.1213 (-3.55120)	-1.53452 (-3.7449)	-2.3333 (-3.2698)	-2.8284 (-3.3353)	-1.00000 (-3.55120)	-.25820 (-3.7449)
Lebanon	-5.0513* (-3.3353) ‡	-.45597 (-3.4243) ‡	-	-	-1.4739 (-2.9850)	-6.4226* (-2.9907)	-1.7591 (-2.9850)	-4.7154* (-2.9907)	-1.4415 (-2.9850)	-5.3423* (-2.9907)
Lesotho	-2.4288 (-2.9850) ‡	-4.8732* (-2.9907) ‡	-2.5176 (-2.9850) ‡	-5.4701* (-2.9907) ‡	-2.0787 (-2.9850)	-6.2374* (-2.9907)	-1.5846 (-2.9850)	-4.6904* (-2.9907)	-1.3815 (-2.9850)	-5.7646* (-2.9907)
Liberia	-	-	-	-	-3.2747* (-2.9850)	-3.6760* (-2.9907)	-2.7072 (-2.9850)	-5.2842* (-2.9907)	-2.4341 (-2.9850)	-3.8922* (-2.9907)
Libya	-	-	-	-	-1.4254 (-2.9850)	-4.6904* (-2.9907)	-1.8134 (-2.9850)	-6.7759* (-2.9907)	-1.2373 (-2.9850)	-5.4540* (-2.9907)
Lithuania!!	-7.2539* (-3.2698) ‡	-2.8542 (-3.3353) ‡	-	-	-2.5298 (-3.2197)	-3.0000 (-3.2698)	-1.2649 (-3.2197)	-3.0000 (-3.2698)	-1.9243 (-3.2197)	-3.5496* (-3.2698)
Luxembourg	1.4072 (-2.9850) ‡	-3.9403* (-2.9907) ‡	-	-	-3.4069* (-2.9706)	-5.2915* (-2.9706)	No Var.	No Var.	-3.4069* (-2.9706)	-5.2915* (-2.9706)
Macedonia!!	-.29110 (-3.5512) ‡	-1.8044 (-3.7449) ‡	-	-	-2.2361 (-3.7449)	-2.1106 (-4.0703)	-3.0000 (-3.2698)	-2.1213 (-3.3353)	-2.4597 (-3.7449)	-.66667 (-4.0703)
Madagascar	-1.8750 (-2.9850) ‡	-3.9702* (-2.9907) ‡	-2.2737 (-2.9850)	-5.8512* (-2.9907)	-.86932 (-2.9850)	-5.2849* (-2.9907)	-1.2233 (-2.9850)	-5.8054* (-2.9907)	-.74280 (-2.9850)	-4.1713* (-2.9907)
Malawi	-2.0652 (-2.9850) ‡	-7.0543* (-2.9907) ‡	-.97768 (-2.98500)	-7.1725* (-2.9907)	-1.3688 (-2.9850)	-4.6601* (-2.9907)	-.40870 (-2.98500)	-4.0204* (-2.9907)	-1.1290 (-2.9850)	-3.6556* (-2.9907)
Malaysia	-.92231 (-2.9850) ‡	-3.7035* (-2.9907) ‡	-1.4913 (-2.9850)	-3.8538* (-2.9907)	-1.3341 (-2.9850)	-3.7607* (-2.9907)	-2.0185 (-2.9850)	-5.8054* (-2.9907)	-.74409 (-2.9850)	-5.0763* (-2.9907)
Maldives	-.71434 (-3.1485) ‡	-3.4737* (-3.1803) ‡	-	-	-2.2171 (-2.9850)	-5.1381* (-2.9907)	-3.0164* (-2.9850)	-4.7569* (-2.9907)	-2.7623 (-2.9850)	-3.2869* (-2.9798)
Mali	-1.4852 (-2.9850) ‡	-5.1243* (-2.9907) ‡	-2.0791 (-2.9850)	-4.5986* (-2.9907)	-.65290 (-2.9850)	-4.9709* (-2.9907)	-.99167 (-2.98500)	-4.6495* (-2.9907)	-.60102 (-2.9850)	-4.6040* (-2.9907)

Malta	-.15979 (-2.9850) ‡	-4.4531* (-2.9907) ‡	-	-	-1.4460 (-2.9850)	-6.7759* (-2.9907)	-1.0152 (-2.9850)	-4.7186* (-2.9907)	-.99253 (-2.9850)	-4.9400* (-2.9907)
Mauritania	-1.4453 (-2.9850) ‡	-6.2984* (-2.9907) ‡	-.48213 (-2.9850) ‡	-3.5635* (-2.9907) ‡	-1.3235 (-2.9850)	-4.2128* (-2.9907)	-2.9289 (-2.9850)	-6.7759* (-2.9907)	-.87557 (-2.9850)	-5.6592* (-2.9907)
Mauritius	-.45416 (-2.9850) ‡	-4.3071* (-2.9907) ‡	-.61401 (-2.9850) ‡	-5.7237* (-2.9907) ‡	-.39381 (-2.9850)	-5.6840* (-2.9907)	-2.4919 (-2.9850)	-3.9641* (-2.9907)	-1.4623 (-2.9850)	-4.3239* (-2.9907)
Mexico	-1.4330 (-2.9850) ‡	-3.5050* (-2.9907) ‡	-1.2341 (-2.9850) ‡	-4.6605* (-2.9907) ‡	-1.4126 (-2.9850)	-5.7446* (-2.9907)	-4.1442* (-2.9850)	-6.1675* (-2.9907)	-1.8465 (-2.9850)	-4.3743* (-2.9907)
Moldova!!	-.32531 (-3.0660) ‡	-3.4204* (-3.0819) ‡	-	-	-2.5981 (-3.5512)	-3.0984* (-3.7449)	-3.2998* (-3.2698)	-4.7958* (-3.3353)	-2.5981 (-3.5512)	-3.0984 (-3.7449)
Mongolia	-2.7491 (-3.0819) ‡	-2.1821 (-3.1004) ‡	-	-	-.58547 (-2.9850)	-4.0726* (-2.9907)	-.72323 (-2.9850)	-3.8063* (-2.9907)	-.98593 (-2.9850)	-3.2496* (-2.9907)
Morocco	-2.3862 (-2.9850) ‡	-9.7724* (-2.9907) ‡	-2.1945 (-2.9850) ‡	-5.8883* (-2.9907) ‡	-2.3452 (-2.9850)	-4.9070* (-2.9907)	-3.2333* (-2.9850)	-7.3181* (-2.9907)	-2.9165 (-2.9850)	-4.9850* (-2.9907)
Mozambique	-.13907 (-3.0660) ‡	-3.5406* (-3.0819) ‡	-	-	-.60719 (-2.9850)	-5.0383* (-2.9907)	-.88050 (-2.9850)	-4.7594* (-2.9907)	-.26823 (-2.9850)	-4.6082* (-2.9907)
Myanmar	-	-	-	-	-3.7533* (-2.9750)	-10.5889* (-2.9798)	-2.2109 (-2.9850)	-6.7759* (-2.9907)	-3.4606* (-2.9850)	-7.2326* (-2.9907)
Namibia	-.28808 (-3.0660) ‡	-3.6327* (-3.0819) ‡	-	-	No var.	No var.	-3.3166* (-3.1803)	-4.8990* (-3.2197)	-3.3166* (-3.1803)	-4.8990* (-3.2197)
Nepal	1.6902 (-2.9850) ‡	-5.7046* (-2.9907) ‡	-.16015 (-2.9970) ‡	-7.0041* (-3.0039) ‡	-2.4742 (-2.9850)	-4.8156* (-2.9907)	-3.0975* (-2.9850)	-5.0416* (-2.9907)	-2.4832 (-2.9850)	-5.4381* (-2.9907)
Netherlands	1.3829 (-2.9850) ‡	-3.0005* (-2.9907) ‡	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
New Zealand	-.45897 (-2.9850) ‡	-3.3156* (-2.9907) ‡	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Nicaragua	-1.7505 (-2.9850) ‡	-3.8777* (-2.9907) ‡	-2.7286 (-2.9850) ‡	-7.0937* (-2.9907) ‡	-1.2389 (-2.9850)	-5.2842* (-2.9907)	-1.8101 (-2.9850)	-5.4929* (-2.9907)	-1.2407 (-2.9850)	-5.0819* (-2.9907)
Niger	-.66594 (-2.9850) ‡	-4.1174* (-2.9907) ‡	-2.2349 (-2.9850) ‡	-5.8740* (-2.9907) ‡	-1.3637 (-2.9850)	-4.9936* (-2.9907)	-.79682 (-2.9850)	-4.8990* (-2.9907)	-1.2112 (-2.9850)	-6.0591* (-2.9907)
Nigeria	-2.8944 (-2.9850) ‡	-3.3346* (-2.9907) ‡	-1.4224 (-2.9850) ‡	-2.9001 (-2.9907) ‡	-1.9574 (-2.9850)	-4.6441* (-2.9907)	-2.4526 (-2.9850)	-3.8938* (-2.9907)	-2.2576 (-2.9850)	-4.0450* (-2.9907)
Norway	-1.0180 (-2.9850) ‡	-4.0897* (-2.9907) ‡	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Oman	-.78296 (-3.0199) ‡	-4.0898* (-3.0294) ‡	-	-	-4.8113* (-2.9750)	-5.0990* (-2.9798)	-2.5036 (-2.9850)	-6.7759* (-2.9907)	-2.5036 (-2.9850)	-6.7759* (-2.9907)
Pakistan	-1.9616 (-2.9850) ‡	-4.0557* (-2.9907) ‡	-2.8403 (-2.9850) ‡	-4.8060* (-2.9907) ‡	-1.7473 (-2.9850)	-5.6336* (-2.9907)	-3.3166* (-2.9850)	-5.2885* (-2.9907)	-1.6464 (-2.9850)	-4.6320* (-2.9907)
Panama	-1.7848 (-2.9850) ‡	-4.0236* (-2.9907) ‡	-1.0662 (-3.0660) ‡	-3.7522* (-3.0819) ‡	-.53847 (-2.9850)	-5.5971* (-2.9907)	-2.6057 (-2.9850)	-4.1842* (-2.9907)	-1.8263 (-2.9850)	-5.1362* (-2.9907)
Papua New Guinea	-2.1403 (-2.9850) ‡	-3.4327* (-2.9907) ‡	-	-	No Var.	No Var.	-1.5287 (-3.0039)	-6.7666* (-3.0115)	-1.0435 (-2.9850)	-7.2664* (-2.9907)
Paraguay	-3.8280* (-2.9850) ‡	-1.9115 (-2.9907) ‡	-3.2263* (-2.9850) ‡	-2.8763 (-2.9907) ‡	-1.8423 (-2.9850)	-6.3460* (-2.9907)	-1.5640 (-2.9850)	-4.8590* (-2.9907)	-1.5683 (-2.9850)	-5.7040* (-2.9907)
Peru	-1.6295 (-2.9850) ‡	-3.1095* (-2.9907) ‡	-2.7367 (-2.9850) ‡	-4.6158* (-2.9907) ‡	-1.8261 (-2.9798)	-4.6221* (-2.9850)	-1.9381 (-2.9798)	-3.0005* (-2.9850)	-1.7776 (-2.9850)	-4.1790* (-2.9907)
Philippines	-3.2846* (-2.9850) ‡	-3.5417* (-2.9907) ‡	-2.2838 (-2.9850) ‡	-4.1248* (-2.9907) ‡	-1.1499 (-2.9850)	-4.9639* (-2.9907)	-1.7470 (-2.9850)	-4.7913* (-2.9907)	-1.2534 (-2.9850)	-4.7027* (-2.9907)
Poland!!	-4.4536* (-3.5512) ‡	-.85075 (-3.7449) ‡	-	-	-.47460 (-2.9850)	-4.7150* (-2.9907)	-1.4450 (-2.9850)	-4.0196* (-2.9907)	-.82137 (-2.9850)	-3.8081* (-2.9907)

Portugal	- .53203 (-2.9850) ‡	-3.5940* (-2.9907) ‡	- .32948 (-2.9907) ‡	-3.6692* (-2.9970) ‡	-1.8698 (-2.9850)	-4.8990* (-2.9907)	- .81064 (-2.9850)	-4.8990* (-2.9907)	-1.2293 (-2.9850)	-5.1381* (-2.9907)
Qatar	-	-	-	-	-1.3544 (-2.9850)	-4.7300* (-2.9907)	- .74297 (-2.9850)	-4.8990* (-2.9907)	-1.0283 (-2.9850)	-4.8264* (-2.9907)
Rumania	-1.7333 (-3.0115) ‡	-2.2926 (-3.0199) ‡	-	-	.19402 (-2.9850)	-3.9879* (-2.9907)	.0000 (-2.9850)	-5.1381* (-2.9907)	.39721 (-2.9850)	-3.8071* (-2.9907)
Russia!!	-3.2371* (-2.9850) ‡	-1.7622 (-2.9907) ‡	-	-	.57735 (-3.7449)	- .84017 (-3.5512)	- .77460 (-3.7449)	-2.0000 (-4.0703)	-1.2247 (-3.7449)	-2.0000 (-4.0703)
Rwanda	-1.7863 (-2.9850) ‡	-5.5257* (-2.9907) ‡	-2.6366 (-2.9850) ‡	-6.6573* (-2.9907) ‡	-1.4786 (-2.9850)	-4.6904* (-2.9907)	-3.8773* (-2.9850)	-5.7332* (-2.9907)	-2.7480 (-2.9850)	-5.4591* (-2.9907)
Sao Tome & Principe	-1.5436 (-3.2197) ‡	.55953 (-3.2698) ‡	-	-	- .42597 (-3.0039)	-3.7351* (-3.0115)	-1.3501 (-3.0039)	-4.7345* (-3.0115)	- .88061 (-2.9850)	-4.0336* (-2.9907)
Saudi Arabia	-1.9770 (-3.2197) ‡	.67670 (-3.2698) ‡	-	-	- .42597 (-3.0039)	-3.7351* (-3.0115)	-1.3501 (-3.0039)	-4.7345* (-3.0115)	- .88061 (-2.9850)	-4.0336* (-2.9907)
Senegal	-2.4126 (-2.9850) ‡	-5.6517* (-2.9907) ‡	- .36982 (-2.9850) ‡	-4.9255* (-2.9907) ‡	-3.7949* (-2.9850)	-4.3867* (-2.9907)	-4.0217* (-2.9850)	-4.0797* (-2.9907)	-4.2578* (-2.9850)	-4.8167* (-2.9907)
Sierra Leone	1.8770 (-2.9850) ‡	-2.3181 (-2.9907) ‡	-	-	-2.3100 (-2.9850)	-9.1836* (-2.9907)	-1.6583 (-2.9850)	-8.1240* (-2.9907)	-2.7832 (-2.9850)	-9.1337* (-2.9907)
Singapore	-1.1939 (-2.9850) ‡	-4.2515* (-2.9907) ‡	- .91286 (-2.9850) ‡	-3.6742* (-2.9907) ‡	-2.0951 (-2.9850)	-6.0553* (-2.9907)	-2.3182 (-2.9850)	-4.4721* (-2.9907)	-1.8337 (-2.9850)	-4.6904* (-2.9907)
Slovakia!!	-2.5013 (-3.1485) ‡	-1.4199 (-3.1803) ‡	-	-	-1.00000 (-4.0703)	-1.7321 (-4.7059)	-1.0030 (-3.5512)	-1.7322 (-3.7449)	.34384 (-3.5512)	-1.7321 (-4.7059)
Slovenia!!	2.6459 (-3.5512) ‡	-2.4340 (-3.7449) ‡	-	-	-2.5298 (-3.2197)	-3.0000 (-3.2698)	No Var.	No Var.	-7.5895* (-3.2197)	-2.3333 (-3.2698)
Solomon Islands	-2.7921 (-2.9850) ‡	-4.3936* (-2.9907) ‡	-	-	- .84256 (-3.0294)	-4.0920* (-3.0401)	- .95929 (-3.0294)	-4.1160* (-3.0401)	- .75645 (-3.0294)	-4.1160* (-3.0401)
Somalia	-	-	-	-	- .20000 (-2.9850)	-4.8990* (-2.9907)	No Var.	No Var.	- .20000 (-2.9850)	-4.8990* (-2.9907)
South Africa	-1.4872 (-2.9850) ‡	-3.4779* (-2.9907) ‡	-2.2743 (-2.9850) ‡	-4.6345* (-2.9907) ‡	- .34336 (-2.9850)	-4.3970* (-2.9907)	- .41848 (-2.9850)	-4.2310* (-2.9907)	.035839 (-2.9850)	-4.1793* (-2.9907)
Spain	.58676 (-2.9850) ‡	-3.1413* (-2.9750) ‡	- .55582 (-2.9907) ‡	-2.9975* (-2.9798) ‡	-14.8720* (-2.9850)	-14.9222* (-2.9907)	-3.0454* (-2.9850)	-5.8480* (-2.9907)	-5.4551* (-2.9850)	-10.6630* (-2.9907)
Sri Lanka	.75009 (-2.9850) ‡	-3.6961* (-2.9907) ‡	-2.6767 (-2.9850) ‡	-4.2416* (-2.9907) ‡	-1.5676 (-2.9850)	-4.7569* (-2.9907)	-1.6058 (-2.9850)	-6.7105* (-2.9907)	-1.4821 (-2.9850)	-5.0558* (-2.9907)
Sudan	- .72331 (-2.9850) ‡	-3.7181* (-2.9907) ‡	-	-	-1.6790 (-2.9850)	-4.3717* (-2.9907)	-1.5246 (-2.9850)	-5.6701* (-2.9907)	-1.5225 (-2.9850)	-4.3304* (-2.9907)
Suriname	-1.8877 (-2.9850) ‡	-3.9673* (-2.9907) ‡	-	-	-1.6753 (-3.0039)	-6.7782* (-3.0115)	-2.1461 (-3.0039)	-5.0630* (-3.0115)	-1.6296 (-3.0039)	-6.0346* (-3.0115)
Swaziland	- .47990 (-2.9850) ‡	-4.5182* (-2.9907) ‡	-2.2105 (-2.9850) ‡	-5.3549* (-2.9907) ‡	-1.4460 (-2.9850)	-4.6904* (-2.9907)	-2.7242 (-2.9850)	-5.8054* (-2.9907)	-2.8885 (-2.9850)	-7.2664* (-2.9907)
Sweden	.22755 (-2.9850) ‡	-3.2400* (-2.9907) ‡	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Switzerland	-1.3958 (-2.9850) ‡	-4.2367* (-2.9907) ‡	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Syria	-1.3409 (-2.9850) ‡	-5.9231* (-2.9907) ‡	-2.9060 (-3.0115) ‡	-3.5831* (-3.0199) ‡	- .86304 (-2.9850)	-6.1299* (-2.9907)	-1.8698 (-2.9850)	-4.8990* (-2.9907)	-1.5658 (-2.9850)	-4.6921* (-2.9907)
Taiwan	-	-	-	-	.85068 (-2.9850)	-10.1840* (-2.9907)	- .88757 (-2.9850)	-7.1662* (-2.9907)	.12544 (-2.9850)	-9.4989* (-2.9907)
Tajikistan!!	-2.4955 (-3.1803) ‡	-1.9405 (-3.2197) ‡	-	-	-1.6330 (-3.5512)	-2.2361 (-3.7449)	-1.6330 (-3.5512)	-2.2361 (-3.7449)	-1.6330 (-3.5512)	-2.2361 (-3.7449)

Tanzania	-.47612 (-3.3353) ‡	-1.3308 (-3.4243) ‡	-	-	2.4495 (-2.9850)	-5.1381* (-2.9907)	-.73603 (-2.9850)	-4.1278* (-2.9907)	1.8524 (-2.9850)	-5.9111* (-2.9907)
Thailand	-1.1575 (-2.9850) ‡	-2.6155 (-2.9907) ‡	-1.5982 (-2.9850) ‡	-3.3627* (-2.9907) ‡	-2.5947 (-2.9850)	-6.6865* (-2.9907)	-3.6231* (-2.9850)	-4.5713* (-2.9907)	-2.6082 (-2.9850)	-6.4341* (-2.9907)
Togo	-1.3680 (-2.9850) ‡	-5.1212* (-2.9907) ‡	-2.3086 (-2.9850) ‡	-5.6077* (-2.9907) ‡	-1.2088 (-2.9850)	-6.4226* (-2.9907)	-.81064 (-2.9850)	-4.8990* (-2.9907)	.75794 (-2.9850)	-5.7966* (-2.9907)
Trinidad	-2.8656 (-2.9850) ‡	-1.7288 (-2.9907) ‡	-2.4861 (-2.9850) ‡	-5.0342* (-2.9907) ‡	-1.1258 (-2.9850)	-2.1153 (-2.9907)	-.90995 (-2.9850)	-3.8566* (-2.9907)	-.50023 (-2.9850)	-1.7943 (-2.9907)
Tunisia	.20068 (-2.9850) ‡	-5.3374* (-2.9907) ‡	-.96037 (-2.9850) ‡	-4.1575* (-2.9907) ‡	-1.7104 (-2.9850)	-8.1240* (-2.9907)	-4.0825* (-2.9850)	-5.6125* (-2.9907)	-3.2133* (-2.9850)	-4.6940* (-2.9907)
Turkey	-.49385 (-2.9850) ‡	-5.6850* (-2.9907) ‡	-2.4473 (-3.2698) ‡	-3.6363* (-3.3353) ‡	-2.2551 (-2.9850)	-4.4973* (-2.9907)	-2.2306 (-2.9850)	-5.4498* (-2.9907)	-2.0401 (-2.9850)	-4.1742* (-2.9907)
Turkmenistan	-2.0685 (-3.2698) ‡	-.83666 (-3.3353) ‡	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Uganda	-.0080794 (-3.1004) ‡	-3.5263* (-3.1223) ‡	-	-	-2.6216 (-2.9850)	-3.7559* (-2.9907)	-2.5941 (-2.9850)	-5.2842* (-2.9907)	-2.7618 (-2.9850)	-3.1818* (-2.9907)
Ukraine!!	-2.4159 (-3.2698) ‡	-.43961 (-3.3353) ‡	-	-	-.40825 (-3.5512)	-2.2361 (-3.7449)	No Var.	No Var.	-.40825 (-3.5512)	-2.2361 (-3.7449)
UAE	-1.2062 (-3.0115) ‡	-4.1467* (-3.0199) ‡	-	-	-1.5513 (-2.9850)	-6.7759* (-2.9907)	No Var.	No Var.	-1.5513 (-2.9850)	-6.7759* (-2.9907)
UK	.15045 (-2.9850) ‡	-3.5302* (-2.9907) ‡	-	-	No Var.	No Var.	-.85163 (-2.9798)	-5.0000* (-2.9850)	-.85163 (-2.9798)	-5.0000* (-2.9850)
USA	-.48123 (-2.9850) ‡	-3.7420* (-2.9907) ‡	-	-	No Var.	No Var.	No Var.	No Var.	No Var.	No Var.
Uruguay	-1.4579 (-2.9850) ‡	-3.1152* (-2.9907) ‡	-2.6228 (-2.9850) ‡	-3.0913* (-3.0660) ‡	-1.3935 (-2.9850)	-5.1184* (-2.9907)	-1.4083 (-2.9850)	-5.5966* (-2.9907)	-1.3994 (-2.9850)	-5.3984* (-2.9907)
Uzbekistan	-17.7721* (-3.2698) ‡	-13.5214* (-3.3353) ‡	-	-	-2.5298 (-3.2197)	-3.0000 (-3.2698)	-2.6295 (-3.2197)	-2.2863 (-3.2698)	-3.1782 (-3.2197)	-2.0402 (-3.2698)
Venezuela	-1.3289 (-2.9850) ‡	-4.5339* (-2.9907) ‡	-2.6992 (-2.9850) ‡	-5.0643* (-2.9907) ‡	-1.4669 (-2.9850)	-5.8681* (-2.9907)	.96563 (-2.9850)	-3.6795* (-2.9907)	-.19462 (-2.9850)	-5.0545* (-2.9907)
Vietnam	-1.7931 (-3.1485) ‡	-1.3267 (-3.1803) ‡	-	-	-3.3343* (-3.0115)	-5.0498* (-3.0199)	-1.8185 (-3.0115)	-4.3152* (-3.0199)	-3.2409* (-3.0115)	-4.6599* (-3.0199)
Yemen	-3.5294 (-3.5512) ‡	-2.6140 (-3.7449) ‡	-	-	-2.7689 (-2.9850)	-5.8457* (-2.9907)	-1.4064 (-2.9850)	-4.6883* (-2.9907)	-2.3270 (-2.9850)	-4.8734* (-2.9907)
Congo Kinshasa	-1.8063 (-2.9850) ‡	-2.5910 (-2.9907) ‡	-1.6359 (-3.0660) ‡	-5.0797* (-3.082) ‡	-2.7209 (-2.9850)	-5.1698* (-2.9907)	-1.8127 (-2.9850)	-4.6904* (-2.9907)	-2.4917 (-2.9850)	-4.2504* (-2.9907)
Zambia	-1.1214 (-2.9850) ‡	-4.7647* (-2.9907) ‡	-3.9213* (-2.9850) ‡	-7.9479* (-2.9907) ‡	-2.2835 (-2.9850)	-4.6904* (-2.9907)	-1.5156 (-2.9850)	-4.3974* (-2.9907)	-2.0477 (-2.9850)	-4.6946* (-2.9907)
Zimbabwe	-4.0468* (-2.9850) ‡	-3.7034* (-2.9907) ‡	-2.8373 (-2.9850) ‡	-4.6301* (-2.9907) ‡	-2.4910 (-2.9850)	-3.1623* (-2.9907)	-2.2536 (-2.9850)	-3.3619* (-2.9907)	-2.7707 (-2.9850)	-3.2538* (-2.9907)

Figures in round parentheses denote critical values of the relevant ADF statistic.

!! denotes few data points. ADF unreliable.

No Var. denotes no variation in the relevant variable.

* denotes statistically significant rejection of the null of non-stationarity.

‡ denotes the use of the natural logarithmic scale for the income and investment variables.

† denotes the use of the normal scale for the income and investment variables.

€ denotes the use of 1971-89, 1993-2002 subsamples.

Φ denotes the use of 1971-91 subsample

Appendix 3: Johansen VECM Estimation Results

Country	CL←PR	PR←CL	lnY	Int.	lnI	CivLib	Polright	Rights	lnY	ECM1	ECM2	λ_{\max}			λ_{trace}		
												r=0	r≤1	r≤2	r=0	r≤1	r≤2
Argentina	2.22	.76	lnY ARDL (1, 2, 2)	4.28* (.27)	.43* (.03)			-.004* (.002)		-.60* (.13)							
Bahrain	.64	1.95	lnY ARDL (2, 0)	8.28* (.19)	-			.08* (.017)		-.57* (.18)							
Bangladesh	2.11	2.01	lnY ARDL (3, 5, 5)	2.94* (.32)	.37* (.03)			-.04* (.012)		-.40* (.11)							
Benin	5.49**	1.03	lnY VAR=4		.38* {24.09}	.02* {39.30}				-.32* (.11)	-.02* (.01)	38.92*	32.36*	14.00	85.28*	46.36*	14.00
			Civlib VAR=4				.77* {37.82}			1.48 (2.65)	-.82* (.31)						
Bhutan	6.73*	.60	lnY VAR=4			.32* {13.36}				-.17** (.09)	-.05 (.04)	29.41*	13.86**	.14	43.40*	14.00	.14
			Civlib VAR=4				.78* {7.29}			-1.22 (2.27)	-						
										1.76* (.86)							
Bolivia	CL~I(0)	PR~I(1)	lnY VAR=3		.24* {18.50}		.04* {18.73}			-.32** (.16)		31.58*	12.83	.1E-3	44.41*	12.83	.1E-3
Botswana	.88	2.83	lnY VAR=3		lnI~I(0) lnY~I(1)			.31 {1.84}		-.06* (.01)		15.40*	6.79		22.19*	6.79	
Brunei	3.79	1.94	lnY ARDL (2, 2)	12.73* (1.26)				-.24* (.11)		-.27* (.16)							
Burkina Faso	1.50	5.08*	lnY VAR=4		.37* {18.77}			-.04* {19.23}		-.38* (.11)		24.52*	12.85	5.00	42.37*	17.85	5.00
Cambodia	5.51**	8.20*	lnY VAR=3					-.03* {16.87}		-.16* (.02)		28.65*	5.33		33.98*	5.33	
Cameroon	4.76	2.84	lnY VAR=5		.49* {7.88}			-.11 {6.66}		-.68* (.14)		30.42*	15.32*		51.46*	21.04*	
Cape Verde	.86	.68	lnY ARDL (1, 2)	7.46* (.08)				-.02* (.006)		-.18* (.06)							
Cent. African Rep.	.24	1.69	lnY ARDL (1, 4, 4)	5.10* (.15)		.21* (.05)	-.06 (.03)			-.52** (.27)							
Chad	2.47	3.78										10.26	.18		10.44	.17	
Chile	.46	2.05	lnY VAR=4		.52* {13.73}			.02* {6.54}		-.51* (.21)		28.05*	14.19	.22	42.47*	14.42	.22
China trend=04 1979-2000	2.36	1.23	lnY VAR=1		.42* {19.41}			-.01* {20.09}		-1.06* (.21)		25.62*	6.15	3.54	35.32	9.70	3.54
Colombia	.24	.62	lnY VAR=4		.29* { 6.38}			.03** { 3.77}		.16 (.19)		21.50*	13.63	1.50	36.62*	15.13	1.50
Costa Rica	CL~I(1)	PR~I(0)	lnY ARDL (2, 0)	7.76* (.08)		.22* (.07)				-.23* (.10)							
Cote d Ivoire	2.93	1.74	lnY VAR=4		.33* {20.93}			.34* {36.58}		-.36* (.15)		36.84*	4.45	.23	41.53*	4.68	.23
Cyprus	4.89	2.01	RIGHTS VAR=5						-1.32* {10.24}	-.46* (.19)		15.36*	2.84		18.20*	2.84	
Dominican Republic	2.28	2.52	lnY VAR=4		.45* {30.99}			-.05* {28.76}		-.68* (.04)		35.55*	7.05	4.56	47.17	11.61	4.56
Egypt	4.69	1.33	lnY VAR=5		.60* {8.05}			.30* {8.57}				21.73*	14.75	12.21*	48.70*	26.96*	12.21*

El Salvador trend= -.01	2.85	3.46	lnY VAR=3		.38* {12.06}			-.04* {25.99}		-.41* (.20)		32.80*	13.50	6.18	52.49*	19.68	6.17
Fiji trend= .10	.90	4.47	lnY VAR=5					-.03* {15.85}		-.36* (.15)		21.03*	4.88		25.92	4.88	
Finland trend= .03	CL=PR	CL=PR	lnY VAR=4					.10* {17.93}		-.72* (.14)		24.30*	6.29		30.59	6.29	
Gabon trend= -.02	CL=PR	CL=PR	lnY VAR=2		.98* {8.48}			-.19 { 6.57}		-.01** (.004)		30.84*	17.24	6.39	54.47*	23.63	6.39
Gambia	2.99	.74	lnY ARDL(5,0)	5.95* (.03)				-.01* (.004)		-.44* (.20)							
Ghana	.93	1.21	lnY VAR=3		.37 {14.98}			-.01 {13.82}		-.72* (.16)		29.23*	19.73*	14.02*	62.99*	33.75*	14.02*
Greece trend = .01	1.92	3.75	lnY VAR=4		.31* {17.66}			-.02* {6.90}		-.32* (.11)		33.83*	15.79	7.67	57.29*	23.46	7.67
Guatemala trend = -.01	CL~I(0)	PR~I(1)	lnY VAR=4		.35* {7.83}			-.05* {11.33}		-.50** (.24)		23.44	15.62	9.47	48.52*	25.09	9.47
Guinea Bissau	1.04	9.29*	lnY VAR=4 trend= .01		.10** {3.33}					-.71* (.20)	-.02 (.04)	37.19*	28.89*	12.27	78.36*	41.16*	12.27
			Polright VAR=4 trend= -.06			.61** {3.33}				-1.96 (1.18)	-.67* (.22)						
Guyana	1.14	2.82	lnY VAR=1		.14* {5.61}			-.05* {17.56}		-.42* (.09)		27.50*	7.90	2.73	38.13*	10.63	2.73
Haiti	2.64	1.22	lnY VAR=2		.35* {6.58}			.12* {18.24}		-.01* (.003)		27.15*	8.87	.02	36.04*	8.90	.02
Honduras	CL~I(0)	PR~I(1)	lnY VAR=2		.05* {8.87}		.01* {7.72}			-.70* (.10)		34.65*	11.21	1.78	47.64*	12.99	1.78
Hungary trend = .04	.71	.91	lnY VAR=4		1.15* {38.43}			.14* {22.32}		-.24* (.06)		56.61*	17.96	5.93	80.50*	23.89	5.93
India	6.13*	1.79	lnY VAR=4		.55* {16.41}			-.005* {24.19}		-.98* (.28)		28.26*	4.19	4.03	36.49*	8.22	4.03
Indonesia	CL~I(1)	PR~I(>1)	lnY VAR=4	2.05	.44* {21.88}			-.04* {21.78}		-.15** (.09)		26.65*	11.81	7.48	45.94*	19.29	7.48
Iran	8.04*	1.17	lnY ARDL(2,0)	6.17* (.52)				.09* (.04)		-.30* (.07)							
Israel	2.50	.24	lnY VAR=4	6.00	.44** {3.36}			-.12* {15.41}		-.08* (.03)		21.40	7.81	5.73	34.95*	13.55	5.73
Italy trend = .01	2.36	3.41	lnY VAR=4					-.03* {17.38}		-.52* (.10)		32.72*	3.20		35.92*	3.20	
Jamaica trend = -.02	5.29**	6.00*	lnY VAR=4		.41* {39.46}			-.01* {35.52}		-.01** (.003)		49.71*	15.13	10.05	74.89*	25.18	10.05
Japan	.59	2.21	lnY VAR=4	14.11				-.93* {6.29}		-.01* (.004)		15.21	5.37		20.59*	5.37	
Jordan	7.16*	11.59*	lnY VAR=4 trend= -.03				-1.26* {11.30}			-.03* (.01)	-.13* (.03)	28.97*	23.51*	12.22	64.70*	35.73*	12.22
			Polright VAR=4 trend= -.06			.93* {21.67}				-.23* (.10)	-.48* (.23)						
Kenya	.98	1.36	lnY		.66*			.13*		-.08*		31.87*	11.42	2.75	46.03*	14.17	2.75

			VAR=1 trend = -.02		{13.50}			{16.36}		(.02)							
												5.14	1.74		6.88	1.74	
Korea South	.74	1.43	lnY VAR=4		.10 {.25}			-.10* {20.07}		-.21** (.11)		23.20*	8.53	.48	32.22*	9.01	.48
Laos trend = .04	2.55	5.57	lnY VAR=4					-.22 {26.64}		-.33* (.10)		84.30*	24.93*		109.24*	24.93*	
Lesotho trend = .02	.78	3.16	lnY VAR=4		.13 {.36}			-.03* {19.06}		-.90* (.31)		20.37	17.67	5.51	43.56*	23.18	5.51
Madagascar trend = -.02	.44	.78	lnY VAR=5		.22* {27.19}			-.002* {27.08}		-.004* (.002)		34.93*	10.78	4.61	50.33*	15.40	4.61
Malawi	.63E-6	2.97	lnY VAR=1		.16* {16.03}			-.02* {25.25}		-.88* (.19)		25.98*	4.82	.72	31.52*	5.54	.72
Malaysia	.91	9.51*	lnY VAR=4		.40 (.01)		.04** {3.07}			-.49** (.27)	.04 (.02)	71.83*	23.73*	4.19 .80	100.55*	28.72*	4.99 .80
			Polright VAR=4	lnY: 1.41 (.67)		.72* {.46}				.78 (1.33)	-.84* (.29)						
Maldives	3.26	.51										13.45	.66		14.12	.66	
Mali	.59	2.02	lnY VAR=4	4.72	.11 {.26}			.02** {3.79}		-.27** (.16)		27.21*	15.84	3.03	46.08*	18.88	3.03
Malta	2.91	2.18	lnY VAR=4					-.19* {15.54}		-.07* (.01)		16.41*	.60		17.01*	.60	
Mauritania	3.38	.96	lnY ARDL(1,0,2)		-.01 (.02)			-.05* (.02)		-.57* (.20)		22.25	8.02	.88	31.15	8.90	.88
Mauritius	1.66	1.33	lnY VAR=2		.45 {9.19}			-.20 {22.54}		-.33* (.09)		34.15*	10.39	.11	44.65*	10.50	.10
Mexico	CL~I(0)	PR~I(1)	lnY VAR=1	5.15	.32* {14.27}			-.09* {13.19}		-.24* (.09)		39.17*	11.28	1.76	52.22*	13.04	1.76
Morocco	4.81	.91	lnY VAR=4		1.46* {25.60}			.01 {.27}		-.004* (.002)		42.41*	13.41	5.09	60.91	18.50*	5.09
Mozambique	.98	6.44*	lnY VAR=4				-.10 {38.33}			-.97* (.27)	.05 (.02)	42.91*	25.21*	4.46	72.59*	29.68	4.46
			Polright VAR=4			1.03* {20.28}				-.13 (2.69)	-.99 (.23)						
Nicaragua	7.90*	.40	lnY VAR=4 trend = -.05		.34E-3* {5.43}			-.13* {19.37}		-	.004** (.002)	34.27*	8.14	5.36	47.78*	13.50	5.36
			Civlib ARDL(1,1)	.88* (.56)				.79* (.12)		-.88* (.21)							
Niger	.96	18.83*	lnY VAR=4 trend = -.01		.33* {14.96}			-.02* {16.59}		-.51* (.21)		30.50*	15.44	6.95	52.89*	22.39	6.95
			Polright ARDL(1,1)	-2.28* (.43)		1.53* (.07)				-1.90* (.34)							
Nigeria	1.59	4.05	lnY VAR=4 trend = -.01		.18* {5.95}			.02* {25.23}		-1.01* (.26)		33.47*	8.87	3.41	45.74*	12.27	3.41
Oman	CL~I(1)	PR~I(0)	lnY VAR=2					-3.07* {15.28}		-.10* (.03)		18.21*	2.01		20.22*	2.01	
Pakistan	CL~I(0)	PR~I(1)	lnY VAR=4		.56* {11.70}			-.01* {15.73}		-.49* (.28)		23.55*	8.22	2.30	34.08*	10.52	2.30
Panama	1.53	.55	lnY VAR=4		.22* {21.93}			.02* {7.18}		-.81* (.31)		45.78*	14.52	5.44	65.75*	19.96*	5.44
Papua New Guinea	CL~I(1)	PR~I(0)	lnY VAR=4					.13* {7.00}		-1.16* (.30)		23.04*	9.64		32.68*	9.64	

			trend = .01														
Peru	2.28	4.23										17.42	7.80	2.16	27.39	9.96	2.16
Philippines	1.93	.38	lnY ARDL (2, 1, 0)	5.12* (.88)	.18** (.09)		.01 (.01)		-.19** (.11)								
			lnI VAR=4				.17* {11.82}		-.46 (.19)		19.81*	7.22		27.03	7.22		
Portugal	1.20	9.61*	lnY VAR=2		.50* {15.74}		-.17* {34.41}		-.11* (.05)		54.77*	16.90*	.40	72.08*	17.30	.40	
			Polright ARDL (1, 1)			.63* {15.13}			-.71* (.10)		31.62*	1.50		33.12*	1.50		
Rwanda	1.03	1.58	lnY VAR=4		.34* {16.30}		-.39* {24.55}		-.08** (.04) t-3		26.13*	9.48	1.19	36.79*	10.66	1.19	
Singapore	2.48	2.47	lnY VAR=4		.97* {21.70}		.08* {34.27}		-.08* (.01)		42.62*	8.35	3.80	54.77	12.15	3.80	
South Africa	2.01	8.47*	lnY VAR=5 trend = -.003		.40* {30.60}		.02* {30.79}		-.48* (.14)		36.44*	16.22	4.83	57.48*	21.05	4.83	
			Polright ARDL (1, 1)	-1.51* (1.49)		1.18* (.30)			-.26** (.15)								
Spain	22.96*	5.77*	lnY VAR=2 trend = .02		.24* {11.02}		.01* {26.21}		-.20* (.07)		37.16*	14.89	9.28	61.33*	24.17	9.28	
Sri Lanka	4.28	.84	lnY VAR=2		.62* {25.01}		.04* {22.42}		-.03** (.01)		26.03*	6.17	.44	32.63*	6.61	.44	
Sudan	2.07	6.95*	lnY VAR=5				2.05* {17.22}		-.04* (.01)		19.12*	1.89		21.00*	1.89		
			Polright ARDL (1, 0)	-1.24* (.76)		1.18* (.12)			-.80* (.11)								
Suriname	2.84	1.20	lnY VAR=4				-.05** {3.60}		-.42* (.14)		11.28	6.61		17.89*	6.61		
Swaziland	3.59	2.91	lnY VAR=4		.54* {24.04}		.16* {20.08}		-.38* (.16)		42.57*	13.04	2.49	58.10*	15.53	2.49	
Syria	3.82	1.61	lnY VAR=2 trend = .02		.23* {10.45}		-.09* {14.25}		-1.06* (.19)		26.41*	9.05	3.20	38.65*	12.24	3.20	
Thailand	2.92	.49	lnY VAR=5 trend = .04		.22* {7.75}		.03* {23.99}		-.61** (.33)		31.12*	7.44	2.66	41.22*	10.10	2.66	
Togo	1.89	.22	lnY VAR=4 trend = .01		.07 {1.42}		.17* {15.95}		-1.42* (.37)		24.37*	16.86	8.41	49.64*	25.27	8.41	
Trinidad	1.07	1.44	lnY VAR=1 trend = 7.10		.15 {1.66}		.10** {3.33}		-.26* (.04)		23.84*	6.23	2.54	32.61*	8.77	2.54	
Tunisia	2.58	1.70	lnY VAR=4		.77* {28.68}		-.31* {28.74}		-1.02 (.70)		30.01*	16.34	1.22	47.58*	17.57	1.22	
Turkey	2.51	3.47									10.28	.001		10.28	.001		
UAE	CL~I(0)	PR~I(1)									13.84	4.93		18.76	4.93		
UK	CL~I(1)	PR~I(0)									15.48	3.93		19.41	3.93		
Uruguay	1.05	1.46	lnY VAR=5		.38* {23.84}		-.02 {23.53}		-.37** (.21)		34.11*	13.78	.10	47.99*	13.88	.10	
Venezuela	.49	9.57*	lnY VAR=5	1.92	.66* {11.72}		-.02* {18.08}		-.60 (.30)		27.21*	15.43	9.04	51.68*	24.47	9.04	
Zambia	.69	2.39	lnY VAR=1		.19* {11.08}		-.002* {11.45}		-.32* (.13)		22.49*	11.13	5.95	39.56*	17.08	5.95	
Zimbabwe	3.62	1.83	lnI				-.10		-.41*		14.82	4.40		19.23*	4.40		

			VAR=4					{ .48 }		(.11)						
--	--	--	-------	--	--	--	--	---------	--	---------	--	--	--	--	--	--

λ_{\max} , λ_{trace} , denote the maximal eigenvalue and trace test statistics for the number of cointegrating vectors respectively. r^2 denotes the null of the test statistic. * denotes significance of the test statistic at the 5% level.

lnY denotes the log of real output.

lnI denotes the log of real investment.

CivLib, Polright, Rights denote the civil liberties, political rights and composite rights indicators respectively.

ECMi denotes the error correction term.

Figures in round parentheses denote standard errors.

Figures in curly parentheses denote test statistics for significance distributed chi-square.

Significance of coefficients is denoted * at the 5%, ** at the 10% level.

CL \leftarrow PR, PR \leftarrow CL, denote two F-tests for any potential direction of association between the two individual rights measures.¹³¹

Significance of the test rejects the null of no association.

¹³¹ See Pesaran, Shin and Smith (2001) for details of the test.

Appendix 4: Association between Human Capital Variables

	(1)	(2)	(3)	(4)
Estimator:	PMGE	PMGE	PMGE	PMGE
Sample:	Full Sample	Rights <7	7<Rights<11	Rights>11
Info. Criterion:	ARDL(3)	ARDL(3)	ARDL(3)	ARDL(3)
Dep. Variable:	Avg Years Schooling	Avg Years Schooling	Avg Years Schooling	Avg Years Schooling
% Pop. No School	-0.043* (0.001)	-0.014* (0.002)	-0.028* (0.002)	-0.087* (0.000)
% Pop. Prim. Compl.	0.018* (0.001)	0.047* (0.003)	0.051* (0.005)	-0.122* (0.002)
% Pop. Second. Compl.	0.123* (0.002)	0.211* (0.007)	0.141* (0.005)	0.034* (0.001)
% Pop. High Sch. Compl.	0.143* (0.002)	0.243* (0.005)	0.218* (0.010)	0.080* (0.001)
ϕ	-0.06*** (0.04)	-0.08* (0.05)	-0.017* (0.009)	-0.295* (0.194)
h-test	2.15 [0.14] 2.70 [0.10] 0.63 [0.43] 2.34 [0.13]	1.80 [0.18] 0.46 [0.50] 1.89 [0.17] 0.08 [0.78]	2.07 [0.15] 0.70 [0.40] 0.80 [0.37] 0.65 [0.42]	3.22 [0.07] 2.41 [0.12] 8.34 [0.00] 0.21 [0.64]
LR	1852.31 [0.00]	751.66 [0.00]	1059.23 [0.00]	274.93 [0.00]
RLL	5680.70	1982.74	3880.45	1069.07
ULL	6606.85	2358.57	4410.07	1206.54

Table: Dynamic Heterogeneous Panel Estimation: PMGE denotes pooled mean group estimators; * denotes significance at the 5%, ** at the 10% levels; ϕ denotes the adjustment to the long run equilibrium term; h-test denotes the Hausman test statistic under the null of long-run homogeneity, RLL and ULL denotes the restricted an unrestricted log likelihood.

Figures and Tables

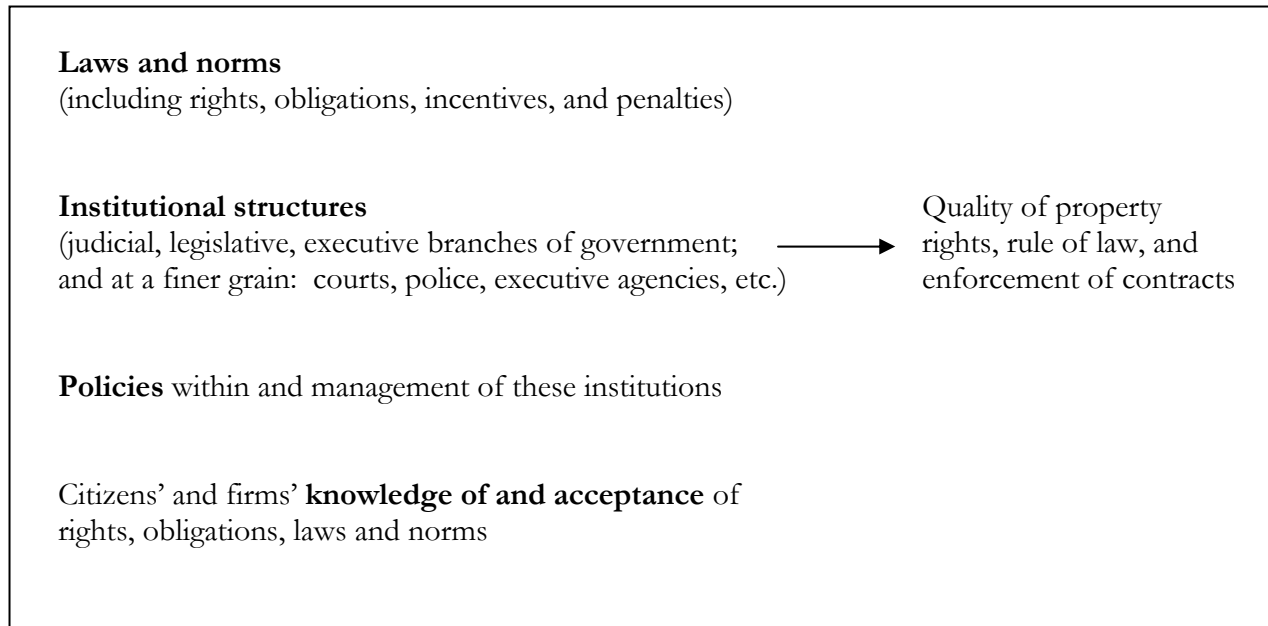


Figure 1: Factors Influencing Property Rights and Rule of Law

Heterogeneity Type	Problem	Illustration	Consequence under Standard Estimator & Cure
Type Ia	$Y_{it} = \alpha + \beta X_{it} + \gamma C_i + \varepsilon_{it}$ $Cov(C_i, Y_{it}) \neq 0, Cov(C_i, X_{it}) \neq 0$ $C_i \text{ unobserved}$	Figure 2	Bias & Inconsistency; Fixed Effects (FE)
Type Ib	$Y_{it} = \alpha + \beta X_{it} + \gamma C_i + \varepsilon_{it}$ $Cov(X_{it}, \varepsilon_{it}) \neq 0$		Bias & Inconsistency; GMM
Type II	$\Delta Y_{it} = \phi_i (Y_{t-1} - \beta X_{t-1}) + \sum_{j=1}^k a_{ij} \Delta Y_{it-j} + \sum_{j=1}^k b_{ij} \Delta X_{it-j} + \mu_i + \varepsilon_{it}$ <p>Heterogeneity in a_i, b_i, across groups. Possibility 1: β not homogeneous across all groups (MGE): distinct steady state across groups Possibility 2: β homogenous across all groups (PMGE): distinct steady state across groups</p>	Figure 3: MGE Figure 4: PMGE	Bias & Inconsistency; MGE or PMGE
Type III	$\Delta Y_{it} = \phi_i (Y_{t-1} - \beta_X X_{t-1}) + \sum_{j=1}^k a_{ij} \Delta Y_{it-j} + \sum_{j=1}^k b_{ij} \Delta X_{it-j} + \mu_i + \varepsilon_{it}$ <p>Heterogeneity in a_i, b_i, across groups. Non-linearity: β homogenous across groups for ranges of X, but not across whole domain of X.</p>	Figure 5	Bias MGE or PMGE

Table 1: Summary of Distinct Heterogeneity Types, Illustrations of the Impact of the Heterogeneity, and Proposed Solutions.

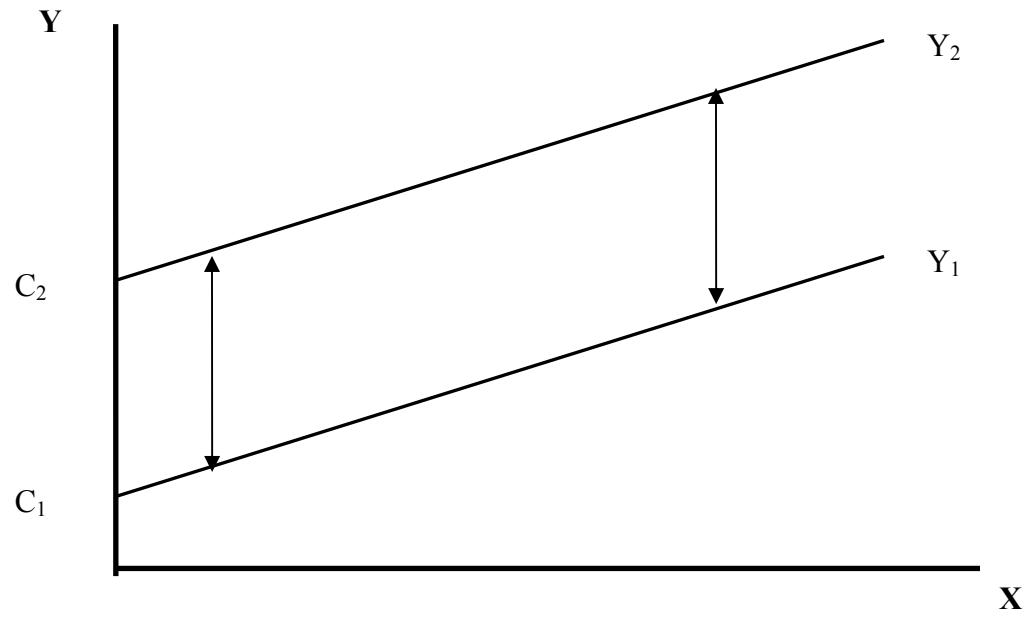


Figure 2: Impact of Time Invariant Unobserved Effects

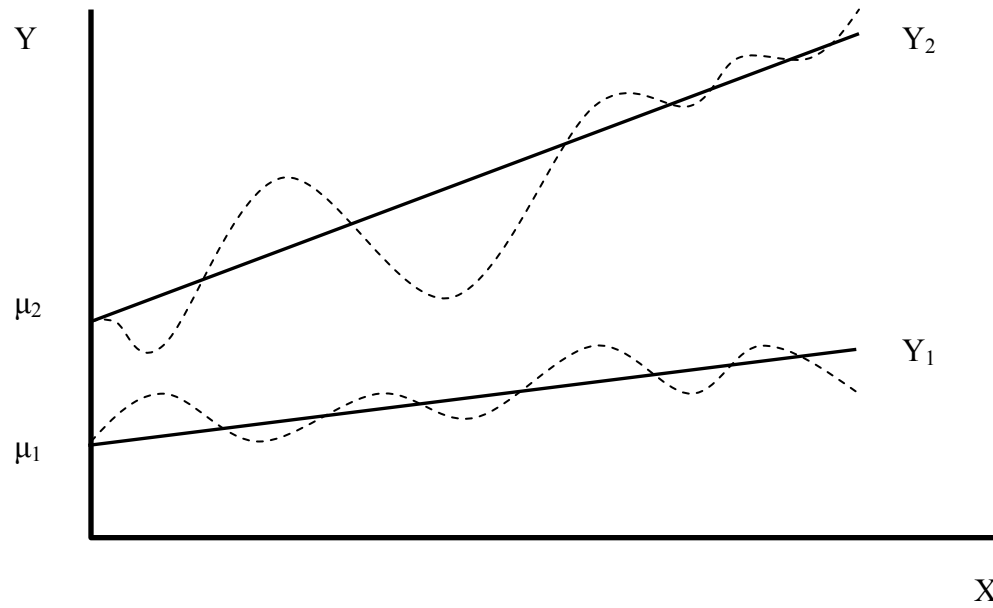


Figure 3: Impact of Heterogeneous Long Run Slope Coefficients, Heterogeneous Short Run Dynamics, and Fixed Effects. Solid lines represent long run equilibrium relationships, dashed the short run dynamics about the long run.

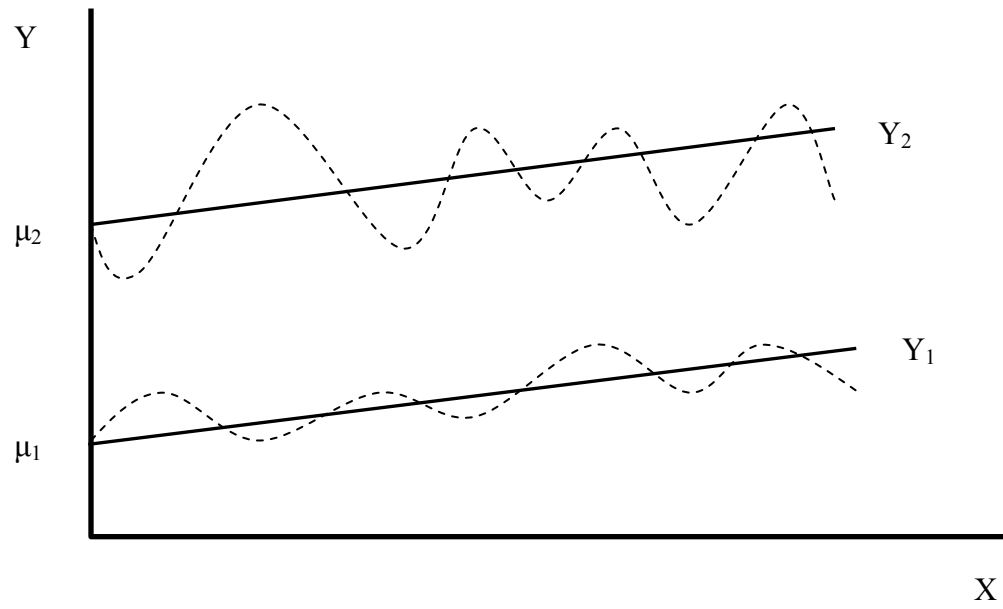


Figure 4: Impact of Homogeneous Long Run Slope Coefficients, Heterogeneous Short Run Dynamics, and Fixed Effects. Solid lines represent long run equilibrium relationships, dashed the short run dynamics about the long run.

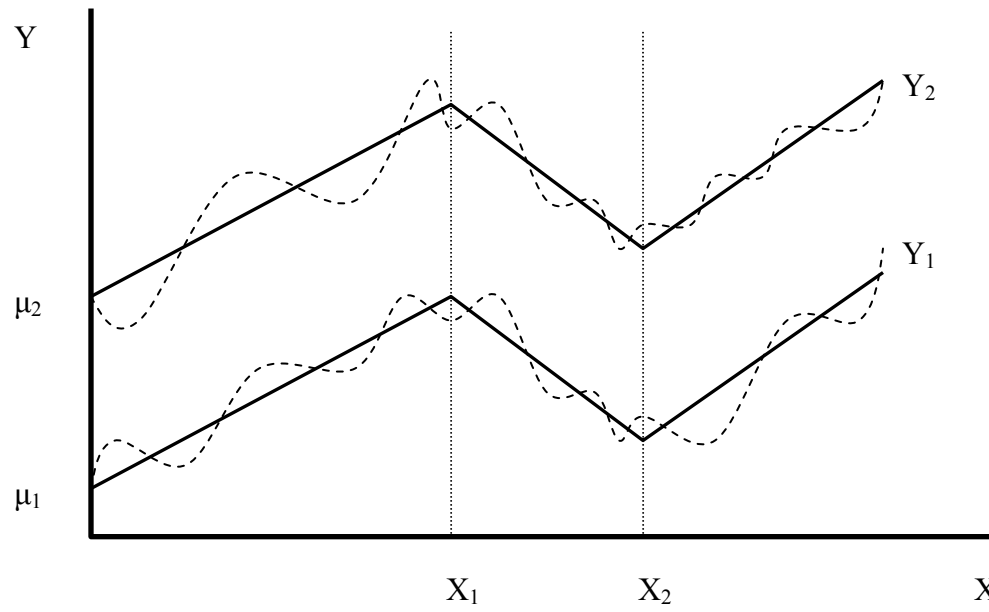


Figure 5: Impact of Non-linearity. Solid lines represent long run equilibrium relationships, dashed the short run dynamics about the long run.

	Low/Poor Rights	High/Good Rights
dY/dK	Low	High
dY/dR	High	Low

Table 2: Summary of the differential impact of capital and governance at different levels of rights.

	<i>Mean</i>	<i>St Dev</i>	<i>Median</i>	<i>IQR</i>	<i>Skewness</i>	<i>Kurtosis</i>
<i>RGDP</i>	2,357	3,464	987	2,297	2.9	13.5
<i>GROWTH</i>	1.26%	5.17%	1.80%	5.30%	-1.0	12.4
<i>RIGHTS</i>	8.5	3.3	9.0	6.0	-0.2	1.8
<i>INVEST</i>	59.0	101.4	23.7	54.3	4.0	24.5
<i>EXPORTS</i>	28.0	19.8	24.0	17.9	3.2	20.2
<i>FDI</i>	1.5	2.9	0.7	1.6	4.1	45.9
<i>CPI</i>	79	766	11.1	14.2	22	587
<i>LU</i>	39.6	26.2	35.1	42.1	0.4	2.0
<i>LPC</i>	13.0	9.7	10.6	9.9	1.7	6.9
<i>LSC</i>	6.3	5.8	4.9	7.1	1.8	7.8
<i>LHC</i>	3.4	3.4	2.2	4.3	1.3	4.3
<i>TYR</i>	4.0	2.1	3.9	3.0	0.4	2.6

Table 3: Univariate Statistics for Key Variables

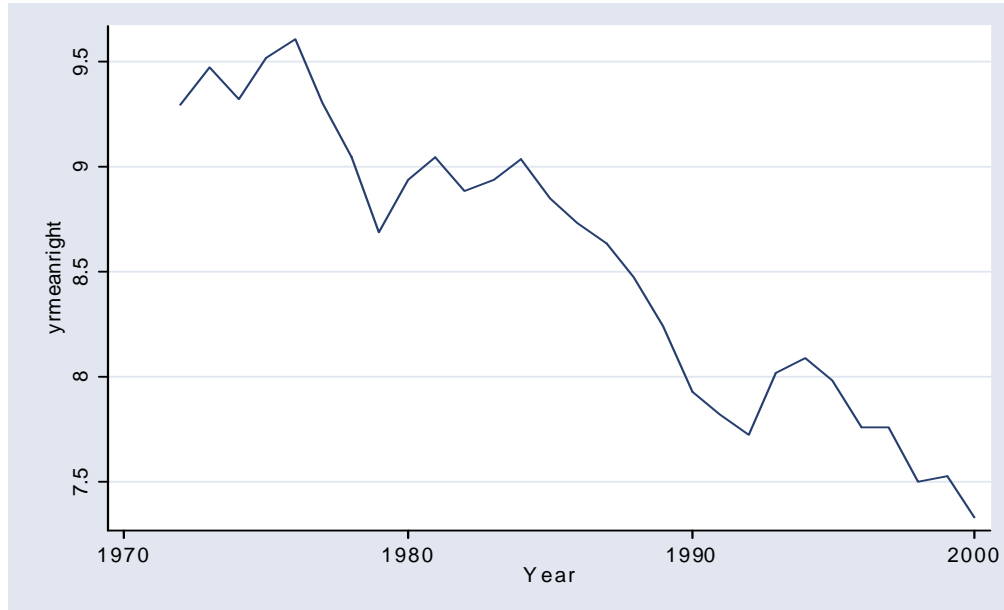


Figure 6: Entire Sample – Annual Average Rights

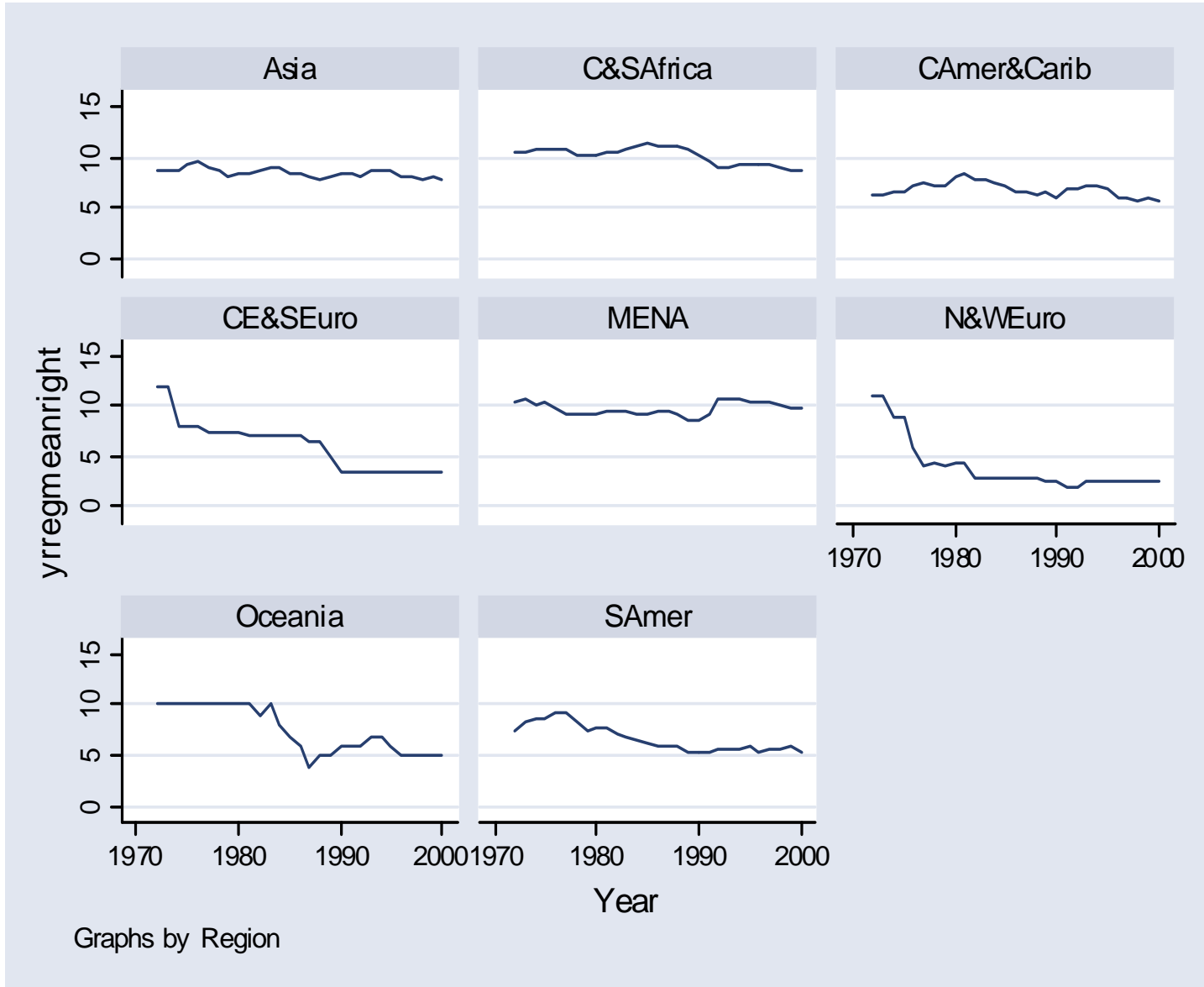


Figure 7: By Region – Annual Average Rights



Figure 8: The Worldwide Improvement in Governance.

	<i>RGDP</i>	<i>LNRGDP</i>	<i>GROWTH</i>	<i>RIGHTS</i>	<i>INVEST</i>	<i>EXPORTS</i>	<i>FDI</i>	<i>CPI</i>	<i>LU</i>	<i>LPC</i>	<i>LSC</i>	<i>LHC</i>	<i>TYR</i>
<i>RGDP</i>	1.00												
<i>LNRGDP</i>	0.83	1.00											
<i>GROWTH</i>	0.09	0.10	1.00										
<i>RIGHTS</i>	-0.42	-0.49	-0.05	1.00									
<i>INVEST</i>	0.95	0.74	0.13	-0.32	1.00								
<i>EXPORTS</i>	0.32	0.26	0.09	-0.05	0.45	1.00							
<i>FDI</i>	0.18	0.17	0.13	-0.10	0.25	0.48	1.00						
<i>CPI</i>	-0.02	-0.00	-0.14	0.01	-0.03	-0.05	-0.05	1.00					
<i>LU</i>	-0.53	-0.75	-0.07	0.47	-0.44	-0.20	-0.19	-0.01	1.00				
<i>LPC</i>	0.51	0.64	0.09	-0.31	0.43	0.10	0.06	-0.02	-0.71	1.00			
<i>LSC</i>	0.52	0.60	0.16	-0.36	0.50	0.26	0.14	-0.02	-0.62	0.39	1.00		
<i>LHC</i>	0.48	0.60	0.04	-0.37	0.42	0.04	0.12	0.02	-0.59	0.39	0.61	1.00	
<i>TYR</i>	0.62	0.76	0.12	-0.43	0.55	0.26	0.23	-0.00	-0.89	0.65	0.80	0.78	1.00

Table 4: Correlations for Key Variables

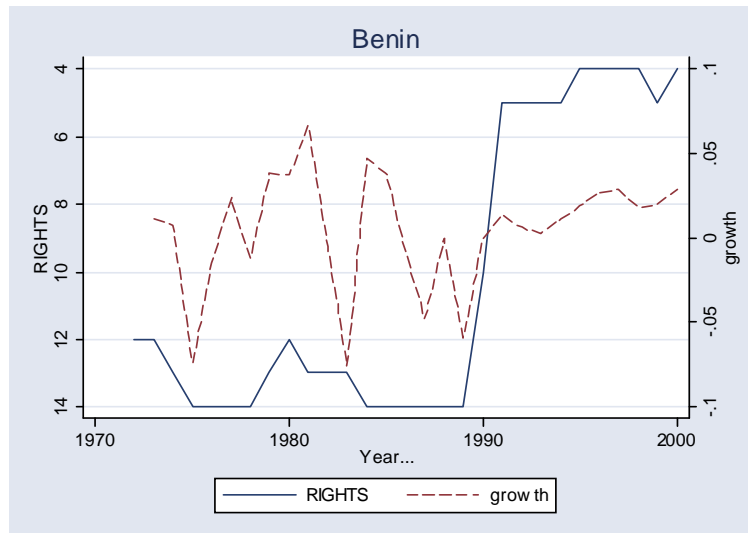
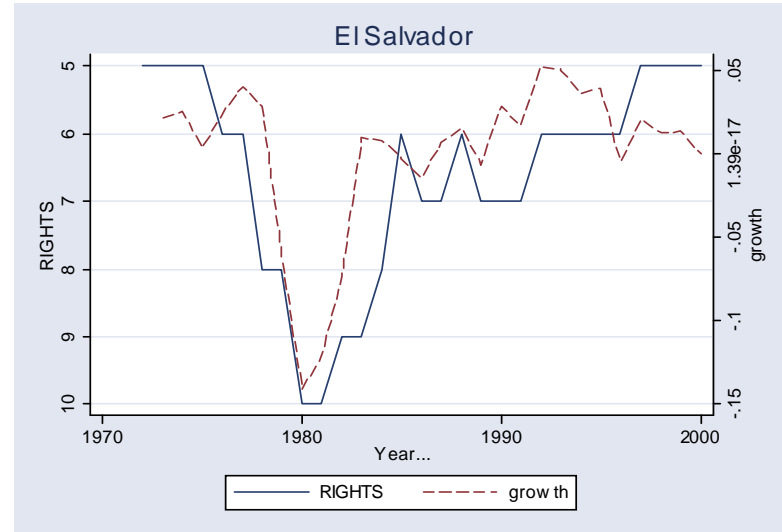
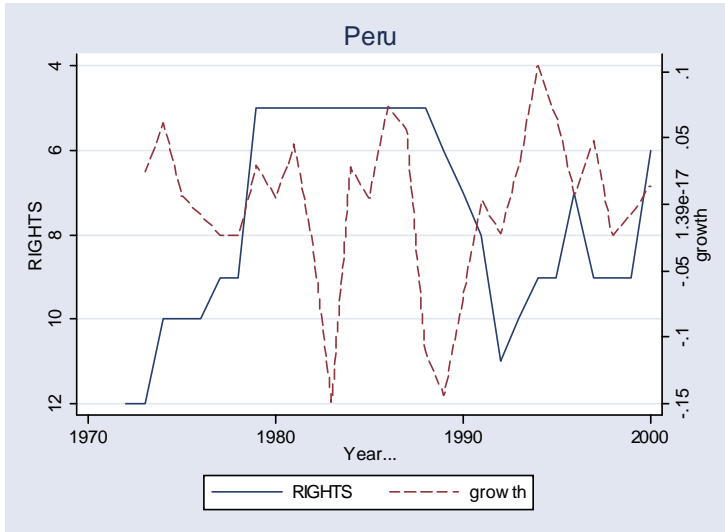


Figure 9: Rights vs. Growth in Three Selected Countries

	Mean	Median	SD	IQR	Skewness	Kurtosis
Polity	2.95	6	6.69	13	-0.52	1.72
Pol_right	3.56	3	2.20	5	0.26	1.57
Civ_lib	3.70	4	1.80	3	0.15	1.98
VA	-0.07	-0.16	0.98	1.63	0.03	1.99
PS	-0.01	0.05	0.96	1.34	-0.46	2.70
GE	-0.01	-0.20	0.98	1.32	0.55	2.76
RL	0.01	-0.24	1.00	1.47	0.56	2.40
RQ	0.01	0.05	0.97	1.25	-0.44	3.34
CONCOR	0.00	-0.29	1.02	1.33	0.87	2.90
EFS_HF	3.20	3.19	0.79	1.13	0.14	2.41
PROPERTY	2.88	3	1.19	2	-0.11	2.14

Table 5. Descriptive Statistics for Key Governance Variables (Sample includes 162 countries)

	POLITY	POL_RIGHT	CIV_LIB	VA	PS	GE	RL	RQ	CONCOR	EFS	PROPERTY
POLITY	1.00										
POL_RIGHT	0.83	1.00									
CIV_LIB	0.81	0.92	1.00								
VA	0.82	0.88	0.91	1.00							
PS	0.36	0.55	0.60	0.71	1.00						
GE	0.48	0.64	0.70	0.84	0.77	1.00					
RL	0.48	0.66	0.71	0.85	0.81	0.97	1.00				
RQ	0.60	0.73	0.79	0.90	0.75	0.94	0.94	1.00			
CONCOR	0.41	0.61	0.66	0.80	0.75	0.95	0.96	0.90	1.00		
EFS	0.57	0.65	0.72	0.82	0.59	0.83	0.83	0.91	0.79	1.00	
PROPERTY	0.50	0.62	0.69	0.78	0.64	0.84	0.86	0.87	0.82	0.87	1.00

Table 6. Correlations for Key Governance Variables (for 162 countries, 2002)

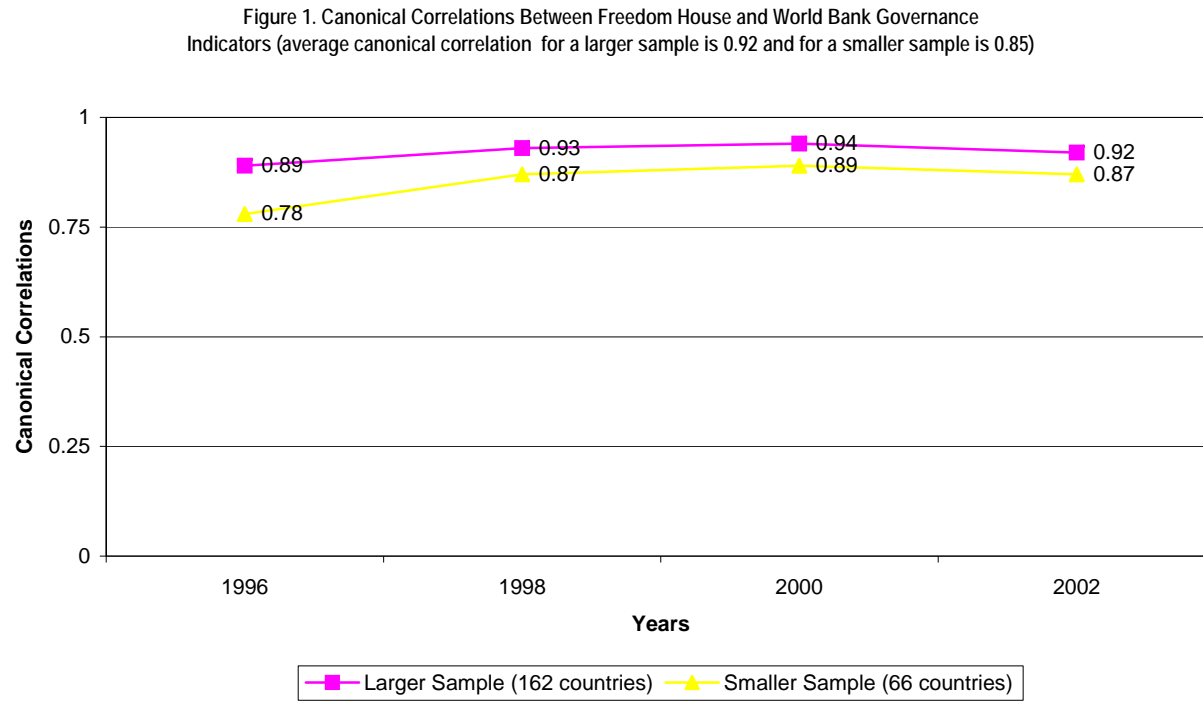


Figure 10: Canonical Correlations Between Freedom House and World Bank Governance Indicators (average canonical correlation for a larger sample is 0.92 and for a smaller sample is 0.85).

Score	Description	Examples of Countries*
1	Country has a fully competitive electoral process with free and fair elections and competitive political parties; and opposition has actual power and plays an important role	Barbados, Belize, Bulgaria, Cape Verde, Costa Rica, Grenada, Hungary, Micronesia, Panama
2	As ranking 1, however, such factors as political corruption, political discrimination against minorities, and foreign and military influence on political process may be present and weaken the quality of political freedom	Bolivia, Botswana, Brazil, Chile, Croatia, El Salvador, Ghana, Guyana, India, Jamaica, Lesotho, Mali, Mexico, Mongolia, Namibia, Peru, Philippines, Senegal
3	Less effective enforcement of competitive election process than 1 and 2.	Albania, Argentina, Benin, Ecuador, Honduras, Indonesia, Macedonia
4	Ditto 3, but government may have been selected outside the public view by various faction leaders	Bangladesh, Bosnia-Herzegovina, Burkina-Faso, Colombia, Djibouti, Georgia, Guatemala
5	No effective electoral process in place, however struggle for consensus among a variety of political, ethnic and other groups in society	Bahrain, Central African Republic, Comoros, Ethiopia, Malaysia, Russia
6	No competitive electoral processes are allowed and a country is ruled by one party dictatorships, religious hierarchies, military juntas, or autocrats, however, leaders may respond to certain popular (cultural, religious and ethnic) desire	Afghanistan, Algeria, Bhutan, Burundi, Cambodia, Chad, Congo, Cote d'Ivoire, Iran, Jordan, Lebanon, Liberia, Pakistan, Qatar, Somalia, Tajikistan
7	Political rights are absent or virtually nonexistent and power is controlled by political despots only	Burma, China, Cuba, Eritrea, Iraq, North Korea, Libya, Rwanda, Sudan

* Examples of countries are drawn from a sample of developing and transition countries based on the Freedom House's 2003 ratings

Table 7. General Characteristics of Political Rights Ratings

Score	Description	Examples of Countries*
1	Country provides full freedom of expression, assembly, association, education, and religion and distinguished by an established and generally equitable rule of law	Barbados, Chile, Kiribati, Marshal Islands, Slovenia, Uruguay
2	As ranking 1, however there are deficiencies in the implementation of some aspects of civil liberties	Belize, Benin, Bulgaria, Cape Verde, Costa Rica, Guyana, Latvia, South Africa
3	There are some elements of censorship in the press and some restrictions with respect to assembly, association, and religion	Albania, Argentina, Bolivia, Brazil, Ecuador, Fiji, Ghana, India, Lesotho, Mali, Namibia, Tanzania
4	The press is strongly censored, free speech and other civil liberties are limited and torture may be existent	Bangladesh, Burkina Faso, Colombia, Gabon, Indonesia, Kenya, Niger
5	Little or no free press, legal authorities have apparently extensive control over social order, and political prisoners are in place	Algeria, Angola, Chad, Djibouti, Ethiopia, Guinea, Kazakhstan, Lebanon, Morocco, Pakistan
6	Severely restricted rights of expression and association, few partial civil liberties, such as some religious and social freedoms, and some highly restricted private business activity	Belarus, Cameron, China, Haiti, Iran, Laos, Liberia, Qatar, Uzbekistan, Vietnam, Zimbabwe
7	Virtually no civil liberties, an overwhelming and justified fear of repression based on politics and ethnicity	Burma, Cuba, Iraq, North Korea, Libya, Saudi Arabia, Somalia, Syria, Turkmenistan

* Examples of countries are drawn from a sample of developing and transition countries based on the Freedom House's 2003 ratings

Table 8. General Characteristics of Civil Liberties Ratings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
	Pooled OLS	FE	FEGLS	FD OLS	SD OLS	PMGE	PMGE	PMGE	PMGE	
Sample:	Full Sample	Full Sample	Full Sample	Full Sample	Full Sample	Full Sample	Rights <7	7<Rights<11	Rights>11	
Dep. Variable:	POLRIGHT	POLRIGHT	POLRIGHT	POLRIGHT	POLRIGHT	POLRIGHT	ARDL (3,3)	ARDL (3,3)	HQ(3)	ARDL(3,3)
CIVLIB	1.127* (0.027)	0.945* (0.051)	0.996* (0.019)	0.525* (0.063)	0.677* (0.065)	1.235* (0.029)	1.163* (0.035)	1.339* (0.043)	0.549* (0.137)	
Intercept	-0.567* (0.120)		-0.004 (0.099)	-0.028* (0.008)	1.512e-019 (0.002)					
ϕ						-0.324* (0.035)	-0.345* (0.065)	-0.339* (0.049)	-0.342* (0.115)	
h-test						3.38 [0.07]	1.10 [0.29]	4.84* [0.03]	1.54 [0.21]	
LR						200.61 [0.00]	53.00 [0.00]	101.28 [0.00]	44.00 [0.23]	
RLL						-809.38	-195.81	-497.38	-115.03	
ULL						-709.08	-169.31	-446.73	-93.03	
Adj-R ²	0.74	0.52	0.58	0.47	0.27					
Wald (joint)	1735* [0.00]	338.4* [0.00]	2643* [0.00]	69.76* [0.00]	107.0* [0.00]					
Wald (dummy)	22.47* [0.00]	n/a	0.002 [0.968]	13.50* [0.00]	2.75e-033 [1.00]					
AR(1) test:	7.18* [0.00]	6.53* [0.00]	45.32* [0.00]	-3.27* [0.00]	-6.31* [0.00]					
AR(2) test:	6.65* [0.00]	5.57* [0.00]	28.61* [0.00]	-2.44* [0.02]	1.12 [0.26]					

Table 9: Relationship between the Rights Measures. FE denotes static fixed effects, FEGLS generalized least squares fixed effects, FDOLS first difference OLS estimation, SDOLS second difference OLS estimation, and PMGE pooled mean group estimators. * denotes significance at the 5%, ** at the 10% levels. Numbers in round parentheses denote robust standard errors, in square parentheses probability levels. ϕ denotes speed of adjustment to equilibrium under PMGE estimation, h-test the test for homogeneity of long run coefficients under PMGE, and RLL and ULL restricted and unrestricted log likelihood values.

	(1a)	(1b)	(1c)	(1d)	(1e)	(2)	(3)	(4)
Estimator:	Pooled OLS	Pooled OLS	Pooled OLS	Pooled OLS	Pooled OLS	FE Within	FEGLS Within	FD OLS
Dep. Variable:	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP
Constant	4.82* (0.13)	5.32* (0.38)	4.97* (0.32)	4.73* (0.54)	6.65* (1.03)		8.47* (0.64)	0.01* (0.002)
Ln(Investment)	0.79* (0.03)	0.76* (0.04)	0.73* (0.06)	0.78* (0.03)	0.68* (0.05)	0.29* (0.03)	0.31* (0.08)	0.12* (0.01)
Rights	-0.035* (0.01)	-0.024* (0.011)	-0.022 (0.01)	-0.032* (0.010)	-0.021*** (0.01)	-0.0003 (0.004)	-0.001 (0.002)	-0.002*** (0.001)
StateForm		-0.0002 (0.0002)			-0.001** (0.0002)	0.000	-0.001 (0.0004)	0.00
Asia		-0.36* (0.15)			-0.34** (0.16)	0.000	-0.87* (0.20)	0.00
C&S Africa		-0.24 (0.15)			-0.12 (0.21)	0.000	-0.92* (0.21)	0.00
MENA		-0.27 (0.18)			-0.13 (0.21)	0.000	-0.37 (0.23)	0.00
CAmer & Carib		-0.04 (0.14)			0.10 (0.18)	0.000	-0.33 (0.20)	0.00
SAmer		-0.04 (0.12)			-0.01 (0.18)	0.000	-0.18 (0.19)	0.00
Britain		-0.12 (0.09)			-0.17 (0.12)	0.000	0.09 (0.11)	0.00
% Pop. No School			-0.004 (0.003)		0.003 (0.004)	0.001 (0.003)	0.001 (0.001)	0.001 (0.002)
% Pop. Prim. Compl.			0.005 (0.004)		-0.0004 (0.01)	0.002 (0.003)	0.003** (0.001)	0.002 (0.002)
% Pop. Second. Compl.			0.004 (0.01)		0.01 (0.01)	0.01** (0.007)	0.01* (0.003)	0.01*** (0.01)
% Pop. High Sch. Compl.			0.03 (0.02)		-0.02 (0.02)	0.01 (0.01)	0.01** (0.004)	0.02 (0.01)
Avg Years Schooling			-0.02 (0.06)		0.11*** (0.06)	0.05 (0.05)	0.06* (0.02)	0.01 (0.03)
Ln(Exports)				0.01 (0.05)	0.02 (0.07)	0.11* (0.03)	0.11* (0.01)	0.01 (0.01)
Ln(Inflation)				0.19** (0.08)	0.13 (0.08)	0.002 (0.01)	0.005 (0.005)	-0.01* (0.003)
Ln(FDI)				-0.20 (0.12)	-0.47** (0.22)	-0.17 (0.11)	-0.18* (0.04)	0.02 (0.02)
Adj-R ²	0.88	0.90	0.89	0.89	0.92	0.69	0.60	0.27
Wald (joint)	615.5* [0.000]	1622* [0.000]	823.3* [0.000]	839.7* [0.000]	1898* [0.000]	356.6* [0.000]	1766.* [0.000]	148.1* [0.000]
Wald (dummy)	1354* [0.000]	197.5* [0.000]	244.8* [0.000]	77.10* [0.000]	41.70* [0.000]	n/a	183.7* [0.000]	8.77* [0.000]
AR(1) test:	3.81* [0.000]	3.69* [0.000]	3.19* [0.000]	4.61* [0.000]	3.68* [0.000]	4.80* [0.000]	78.90* [0.000]	2.39* [0.000]
AR(2) test:	3.64* [0.000]	3.53* [0.000]	3.08* [0.000]	4.39* [0.000]	3.54* [0.000]	4.63* [0.000]	62.49* [0.000]	3.12* [0.000]

Table 10: Static Estimation - Log of real GDP: FE denotes static fixed effects, FEGLS generalized least squares fixed effects, FDOLS first difference OLS estimation, and SDOLS second difference OLS estimation. * denotes significance at the 1%, ** at the 5% and *** at the 10% levels. Numbers in round parentheses denote robust standard errors, in square parentheses probability levels.

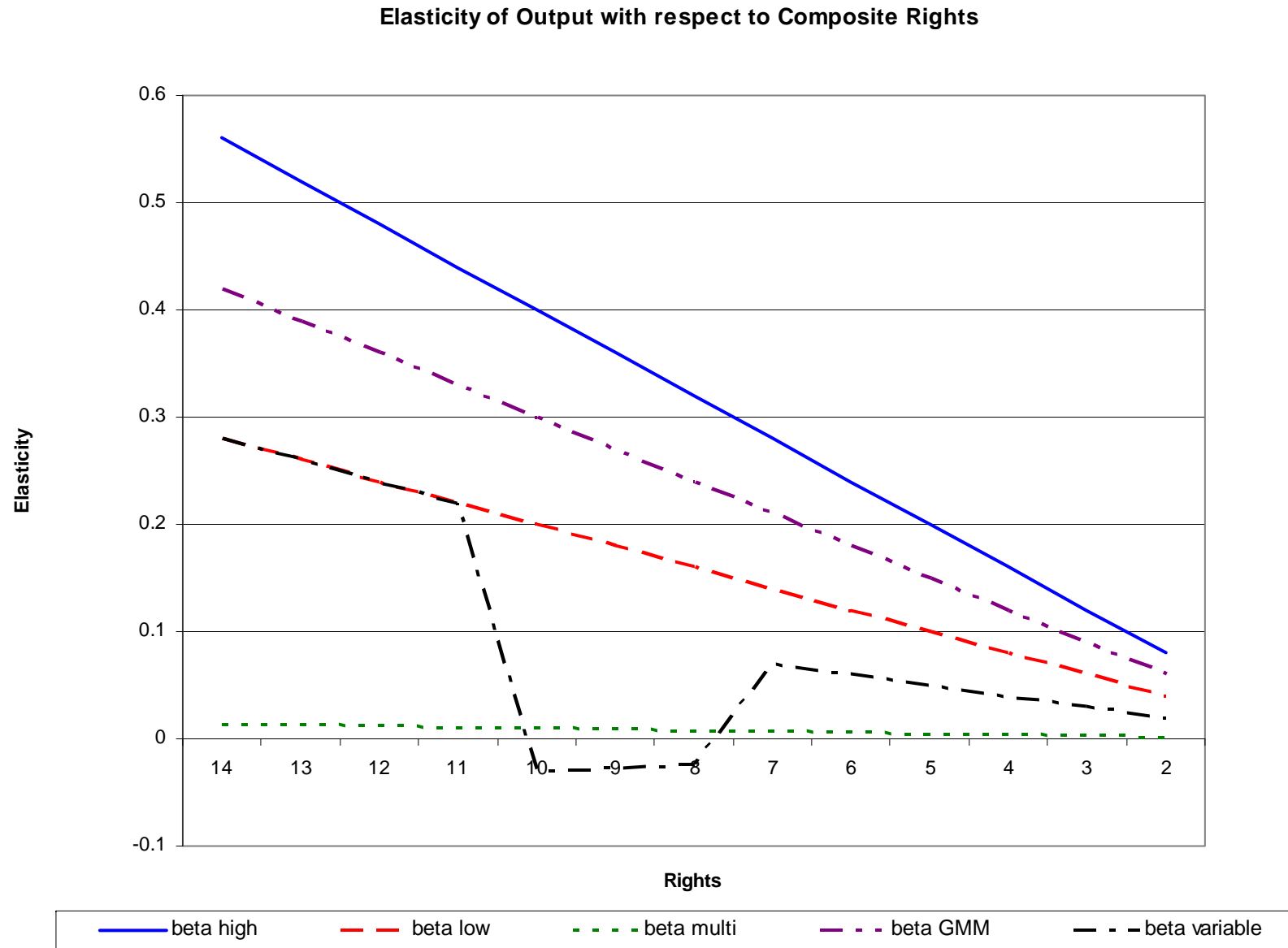


Figure 11: Elasticity of Output with respect to Composite Rights: upper and lower bounds on Pooled OLS

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator:	GMM 2 Step FD	GMM 2 Step Orth.	GMM 2 Step Orth.	GMM 2 Step Orth.	GMM 2 Step Orth.	GMM 2 Step Orth.
Dep. Variable:	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP
Constant	0.32* (0.04)	0.30* (0.04)	0.30* (0.04)	0.10* (0.02)	0.07* (0.02)	0.09* (0.02)
Ln(Investment)	0.46* (0.06)	0.37* (0.06)	0.40* (0.07)	0.39* (0.06)	0.39* (0.06)	0.35* (0.07)
Rights	0.01 (0.02)	-0.008 (0.010)	-0.018*** (0.010)	-0.027** (0.014)	-0.031** (0.02)	-0.034** (0.01)
StateForm	0.002* (0.0004)	0.002* (0.0004)	0.002* (0.0004)	0.002* (0.0003)	0.002* (0.0003)	0.001* (0.0004)
Asia	0.39* (0.10)	-0.37* (0.10)	-0.54* (0.12)	0.15 (0.13)	0.001 (0.13)	0.05 (0.12)
C&S Africa	-0.69* (0.13)	-0.42* (0.15)	-0.38* (0.14)	-0.15*** (0.08)	-0.15 (0.09)	-0.30* (0.10)
MENA	-0.16*** (0.09)	-0.48* (0.14)	-0.33* (0.11)	0.03 (0.04)	0.03 (0.04)	-0.05 (0.07)
CAmer & Carib	-0.46* (0.15)	-0.24*** (0.17)	-0.21 (0.16)	0.13** (0.06)	0.13* (0.05)	0.14** (0.06)
SAmer	0.76* (0.19)	0.53* (0.11)	0.41* (0.10)	-0.06 (0.08)	-0.02 (0.07)	0.03 (0.10)
Britain	-0.78* (0.12)	-0.65* (0.16)	-0.83* (0.14)	-0.30* (0.09)	-0.18*** (0.10)	-0.36* (0.07)
% Pop. No School	-0.02** (0.01)	-0.01 (0.01)	-0.002 (0.01)	-0.001 (0.007)	0.001 (0.01)	0.004 (0.01)
% Pop. Prim. Compl.	0.01 (0.01)	0.01*** (0.01)	0.01* (0.006)	0.03* (0.01)	0.03* (0.01)	0.02* (0.004)
% Pop. Second. Compl.	0.03 (0.02)	0.03 (0.02)	0.03 (0.02)	-0.01 (0.01)	-0.01 (0.01)	-0.02*** (0.01)
% Pop. High Sch. Compl.	0.05 (0.04)	0.01 (0.04)	-0.01 (0.04)	0.03 (0.04)	0.03 (0.03)	0.004 (0.04)
Avg Years Schooling	-0.31 (0.24)	-0.05 (0.18)	0.01 (0.06)	0.12 (0.14)	0.13 (0.13)	0.23 (0.16)
Ln(Exports)	0.41* (0.13)	0.27*** (0.14)	0.28** (0.14)	0.30* (0.11)	0.30** (0.15)	0.33* (0.12)
Ln(Inflation)	-0.01 (0.03)	-0.04 (0.03)	-0.02 (0.03)	-0.07* (0.02)	-0.05* (0.02)	-0.06* (0.02)
Ln(FDI)	0.48* (0.08)	0.72* (0.14)	0.63* (0.12)	0.48* (0.12)	0.42* (0.11)	0.48* (0.13)
Indiv Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes
GMM Dummies	Trans.Eq. All Vars 1,2 Level Eq. All Vars 2 Lev	Trans.Eq. All Vars 1,2 Level Eq. All Vars 2 Lev	Trans.Eq. All RHS Vars 1,2 Dep Excl Level Eq. All RHS Vars 2 Lev Deo Excl.	Trans.Eq. All RHS Vars 1,2 Dep, RIGHT Excl Level Eq. All RHS Vars 2 Lev Dep. RIGHT Excl.	Trans.Eq. All RHS Vars 1,2 Dep, RIGHT, INV Excl Level Eq. All RHS Vars 2 Lev Dep. RIGHT, INV Excl.	Trans.Eq. All RHS Vars 1,2 RIGHT, INV Excl Level Eq. All RHS Vars 2 Lev RIGHT, INV Excl.
Wald (joint)	8.65e+006* [0.000]	2.51e+006* [0.000]	2.77e+006* [0.000]	7.61e+005* [0.000]	1.34e+006* [0.000]	4.09e+005* [0.000]
Wald (dummy)	3.52e+004* [0.000]	3919* [0.000]	5421* [0.000]	5.68e+004* [0.000]	1.89e+004* [0.000]	3371* [0.000]
Wald (time)	216.6* [0.00]	254.5* [0.00]	207.7* [0.00]	213.3* [0.00]	271.6* [0.00]	550.1* [0.00]
Sargan	110.3 [1.00]	47.08 [1.00]	55.41 [1.00]	86.65 [1.00]	88.25 [1.00]	71.32 [1.00]
AR(1) test:	-2.77* [0.006]	-2.78* [0.005]	-2.73* [0.006]	-1.03 [0.30]	-0.88 [0.380]	-1.06 [0.285]
AR(2) test:	-0.69 [0.488]	-0.39 [0.694]	-0.88 [0.379]	-0.63 [0.528]	-0.69 [0.488]	-1.22 [0.222]

Table 11: GMM Estimation. Estimations reported are 2-step small sample corrections, under either first difference (FD) or orthogonal (Orth.) transformations.¹³² * denotes significance at the 1%, ** at the 5% and *** at the 10% levels. Numbers in round parentheses denote robust standard errors, in square parentheses probability levels.

¹³² The first difference and orthogonal deviations transformations eliminate individual effects from the transformed error term, without at the same time introducing all lagged values of the disturbances into the transformed error term. See Arellano and Bover (1995).

		Rights		
		$\sim I(0)$	$\sim I(1)$	$\sim I(>1)$
Output; Investment	$\sim I(0)$	GROUP I Ecuador, Guatemala ⁹ , Honduras ¹¹	GROUP II Bahamas, Botswana ² , Gambia ⁷ , Guatemala ⁹ , Honduras ¹¹ , Kenya ¹⁴ , Paraguay ²¹ , Philippines ²² , Poland ²³ , Zambia ³⁰ , Zimbabwe ³¹	GROUP III Uzbekistan
	$\sim I(1)$	GROUP IV Algeria, Australia, Austria, Barbados, Bolivia ¹ , Burundi, Canada, Costa Rica ⁴ , Czech Rep., France, Germany, Guatemala ⁹ , Honduras ¹¹ , Iceland, India ¹² , Luxemb., Mexico ¹⁵ , Morocco ¹⁶ , Namibia, Netherlds., New Zeal., Oman ¹⁸ , Pakis. ¹⁹ , Pap N Guin ²⁰ , Rwanda ²⁴ , Senegal, Sweden, Switzerland, UAE ²⁷ , UK ²⁸	GROUP V Argentina, Bahrain, Bangladesh, Benin, Bhutan Bolivia ¹ , Botswana ² , Brunei, Burkina Faso, Cambodia, Cameroon, Cape Verde, Cen. Afr. Rep., Chad, Chile ³ , China, Colombia, Costa Rica ⁴ , Cote d Ivoire, Cyprus, Dom. Rep., Egypt, El Salvador ⁶ , Fiji, Finland, Gabon, Gambia ⁸ , Ghana, Greece, Guatemala ⁹ , Guin.-Bissau, Guyana ¹⁰ , Haiti, Honduras ¹¹ , Hungary, India ¹² , Indonesia ¹³ , Iran, Israel, Italy, Jamaica, Japan, Jordan, Kenya ¹⁴ , Korea South, Laos, Lesotho, Madagascar, Malawi, Malaysia, Maldives, Mali, Malta, Mauritania, Mauritius, Mexico ¹⁵ , Morocco ¹⁶ , Mozambique, Nicaragua, Niger, Nigeria ¹⁷ , Oman ¹⁸ , Pakistan ¹⁹ , Panama, Pap N Guin ²⁰ , Peru, Philippines ²² , Portugal, Rwanda ²⁴ , Singapore, South Africa, Spain, Sri Lanka Sudan, Suriname, Swaziland, Syria, Thailand ²⁵ , Togo, Trinidad ²⁶ , Tunisia, Turkey, UAE ²⁷ , UK ²⁸ , Uruguay, Venezuela, Zambia ³⁰ , Zimbabwe ³¹	GROUP VI Chile ³ Indonesia ¹³
	$\sim I(>1)$	GROUP VII Djibouti ⁵ , Equat. Guin. ⁷ , Ireland, Turkmenistan, Ukraine, Vietnam ²⁹	GROUP VIII Albania, Angola, Bulgaria, Congo Brazz., Djibouti ⁵ , El Salvador ⁶ , Equat. Guin. ⁷ , Guinea, Guyana ¹⁰ , Kuwait, Lebanon, Mongolia, Nigeria ¹⁷ , Paraguay ²¹ , Poland ²³ , Rumania, Sao Tome & Principe, Sierra Leone, Tajikistan, Tanzania, Thailand ²⁵ , Trinidad ²⁶ , Vietnam ²⁹ , Yemen, Congo Kinsh.	GROUP IX Eritrea Estonia

Table 12: ADF Results. Superscripts provide cross-references for multiple classifications.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Mean Growth	Mean Growth	Mean Growth	Mean Growth	Mean Growth	Mean Growth
Constant	0.021* (0.004)	0.022* (0.004)	0.021* (0.004)	0.020* (0.003)	0.020* (0.003)	0.020* (0.003)
Mean Polright	-0.0033* (0.001)			-0.0020** (0.001)		
Mean Civlib		-0.0033** (0.001)			-0.0022** (0.001)	
Mean Rights			-0.0017** (0.0006)			-0.0011* (0.0004)
Adj R ²	0.23	0.19	0.22	0.18	0.17	0.18
N	30	30	30	29	29	29

Table 13: Linking Growth and Governance in Group IV Countries

Country	Implied $\varepsilon_{v,R}$						
	Rights Index	ε	R_{\min}	ε	R_{μ}	ε	R_{\max}
Argentina	R	-0.01	3	-0.02	6.2	-0.04	12
Bahrain	R	0.60	8	0.81	10.83	0.97	13
Bangladesh	R	-0.21	5	-0.33	7.9	-0.51	12
Benin	CL	0.04	2	0.10	4.87	0.15	7
Bhutan	CL	1.27	4	1.66	5.23	2.22	7
Bolivia	PR	0.24	1	0.74	3.03	1.70	7
Botswana	R	0	3	0	4.57	0	7
Brunei	R	-2.66	11	-2.79	11.53	-3.14	13
Burkina Faso	R	-0.18	5	-0.35	9.73	-0.47	13
Cambodia	R	-0.29	8	-0.41	12.8	-0.45	14
Cameroon	R	-1.10	10	-1.29	11.7	-1.43	13
Cape Verde	R	-0.06	3	-0.16	8.22	-0.26	13
Cent. African Rep.	CL	0.84	4	1.19	5.68	1.47	7
Chile	R	0.60	3	1.59	7.93	2.40	12
China	R	-0.11	11	-0.13	13.03	-0.14	14
Colombia	R	0.12	4	0.17	5.79	0.24	8
Costa Rica	CL	0.22	1	0.28	1.29	0.44	2
Cote d' Ivoire	R	3.06	9	3.67	10.8	4.08	12
Cyprus	R	Y \leftrightarrow R		Y \leftrightarrow R		Y \leftrightarrow R	
Dominican Republic	R	-0.15	3	-0.24	4.80	-0.35	7
Egypt	R	2.4	8	3.12	10.4	3.6	12
El Salvador	R	-2	5	-2.61	6.53	-4	10
Fiji	R	-1.20	4	-1.80	6	-3.30	11
Finland	R	0.20	2	0.31	3.07	0.40	4
Gabon	R	-1.33	7	-2.02	10.63	-2.28	12
Gambia	R	-0.03	3	-0.07	6.83	-0.13	13
Ghana	R	-0.05	5	-0.10	9.7	-0.13	13
Greece	R	-0.06	3	-0.09	4.27	-0.24	12
Guatemala	R	-0.10	2	-0.18	3.67	-0.30	6
Guinea Bissau	PR	0.30	3	0.53	5.25	0.70	7
Guyana	R	-0.20	4	-0.35	7	-0.50	10
Haiti	R	0.96	8	1.40	11.67	1.68	14
Honduras	PR	0.02	5	0.03	6.48	0.07	10
Hungary	R	0.42	3	1.11	7.90	1.68	12
India	R	-0.02	4	-0.03	5.53	-0.04	8
Indonesia	R	-0.28	7	-0.42	10.40	-0.52	13
Iran	R	0.90	10	1.05	11.63	1.17	13
Israel	R	-0.36	3	-0.50	4.17	-0.60	5
Italy	R	-0.24	2	-0.34	2.83	-0.48	4
Jamaica	R	-0.03	3	-0.04	4.37	-0.05	5
Japan	R	-1.86	2	-2.51	2.70	-3.72	4
Jordan	PR	-6.39	3	-3.78	5.07	-7.56	6
Kenya	R	1.17	9	1.39	10.70	1.69	13
Korea South	R	-0.40	4	-0.74	7.43	-1.10	11
Laos	R	-2.20	10	-2.87	13.03	-3.08	14
Lesotho	R	-0.21	7	-0.28	9.33	-0.36	12
Madagascar	R	-0.01	6	-0.02	8.77	-0.02	12
Malawi	R	-0.22	5	-0.10	10.90	-0.26	13
Malaysia	PR	0.08	2	0.15	3.67	0.20	5

Mali	R	0.08	4	0.21	10.33	0.28	14
Malta	R	-0.38	2	-0.60	3.17	-1.14	6
Mauritania	R	-0.60	10	-0.61	12.27	-0.70	14
Mauritius	R	-0.60	3	-0.80	4	-1.20	6
Mexico	R	-0.45	5	-0.65	7.23	-0.72	8
Morocco	R	0	7	0	9.13	0	11
Mozambique	PR	-0.30	3	-0.56	5.57	-0.70	7
Nicaragua	CL	-0.39	3	-0.55	4.20	-0.78	6
Niger	PR	-0.06	3	-0.12	6.10	-0.14	7
Nigeria	R	0.10	5	0.19	9.60	0.28	14
Oman	R	-33.77	11	-36.93	12.03	-39.91	13
Pakistan	R	-0.06	6	-0.10	9.53	-0.12	12
Panama	R	-0.03	3	-0.08	8.30	-0.13	13
Papua New Guinea	R	0.52	4	0.61	4.70	0.78	6
Peru	R	∅CV		∅CV		∅CV	
Phillipines	R	0	4	0	7.40	0	10
Portugal	PR	-0.17	1	-0.29	1.73	-0.85	5
Rwanda	R	-4.29	11	-4.79	12.27	-5.46	14
Singapore	R	0.64	8	0.76	9.47	0.80	10
South Africa	PR	0.02	1	0.08	3.87	0.12	6
Spain	R	0.02	2	0.04	4.30	0.11	11
Sri Lanka	R	0.16	4	0.27	6.77	0.36	9
Sudan	PR	8.20	4	12.36	6.03	14.35	7
Suriname	R	-0.15	3	-0.37	7.33	-0.65	13
Swaziland	R	0.96	6	1.68	10.47	1.76	11
Syria	R	-0.99	11	-1.18	13.10	-1.26	14
Thailand	R	0.15	5	0.22	7.27	0.36	12
Togo	R	1.70	10	2.02	11.90	2.21	13
Trinidad	R	0.20	2	0.33	3.27	0.60	6
Tunisia	R	-2.48	8	-3.29	10.60	-3.41	11
Turkey	R	∅CV		∅CV		∅CV	
UAE	R	∅CV		∅CV		∅CV	
UK	R	∅CV		∅CV		∅CV	
Uruguay	R	-0.04	2	-0.13	6.27	-0.24	12
Venezuela	R	-0.06	3	-0.09	4.33	-0.16	8
Zambia	R	-0.01	5	-0.02	9.37	-0.02	11
Zimbabwe	R	∅CV		∅CV		∅CV	

Table 14: Estimated Elasticities of Output w.r.t. Governance; ε denotes the implied elasticity; R_{\min} denotes the minimum, R_{μ} the mean, and R_{\max} the maximum value of the relevant rights index; CL denotes the CIVLIB rights index, POLRIGHT the political rights index, and R the composite rights index. ∅CV denotes the absence of a cointegrating relationship. $Y \rightarrow R$ that weak exogeneity tests suggest that the direction of association is from the economic to the governance dimension.

	(1)	(2)	(3)	(4)	(5)	(6)
	$\epsilon_{Y,R}$	$\epsilon_{Y,R}$	$\epsilon_{Y,R}$	$\epsilon_{Y,R}$	$\epsilon_{Y,R}$	$\epsilon_{Y,R}$
Const.	-0.059 (0.44)	-1.59 (1.02)	0.68 (2.48)	-0.72 (0.47)	-0.14 (0.84)	-0.47 (0.33)
R_{μ}	-0.013** (0.050)	0.44 (0.28)	-0.636 (1.12)			
R_{μ}^2		-0.029 (0.018)	0.121 (0.151)	0.036*** (0.021)	-0.040 (0.09)	
R_{μ}^3			-0.006 (0.006)	-0.003*** (0.002)	0.010 (0.02)	0.0033*** (0.002)
R_{μ}^4					-0.001 (0.001)	-0.0003*** (0.0001)
Adj-R ²	0.0009	0.036	0.049	0.045	0.053	0.051
N	78	78	78	78	78	78

Table 15: Relationship between elasticity of output with respect to governance to the range of mean rights over the 2-14 scale. ** denotes significance at the 5%, * at the 10% level.**

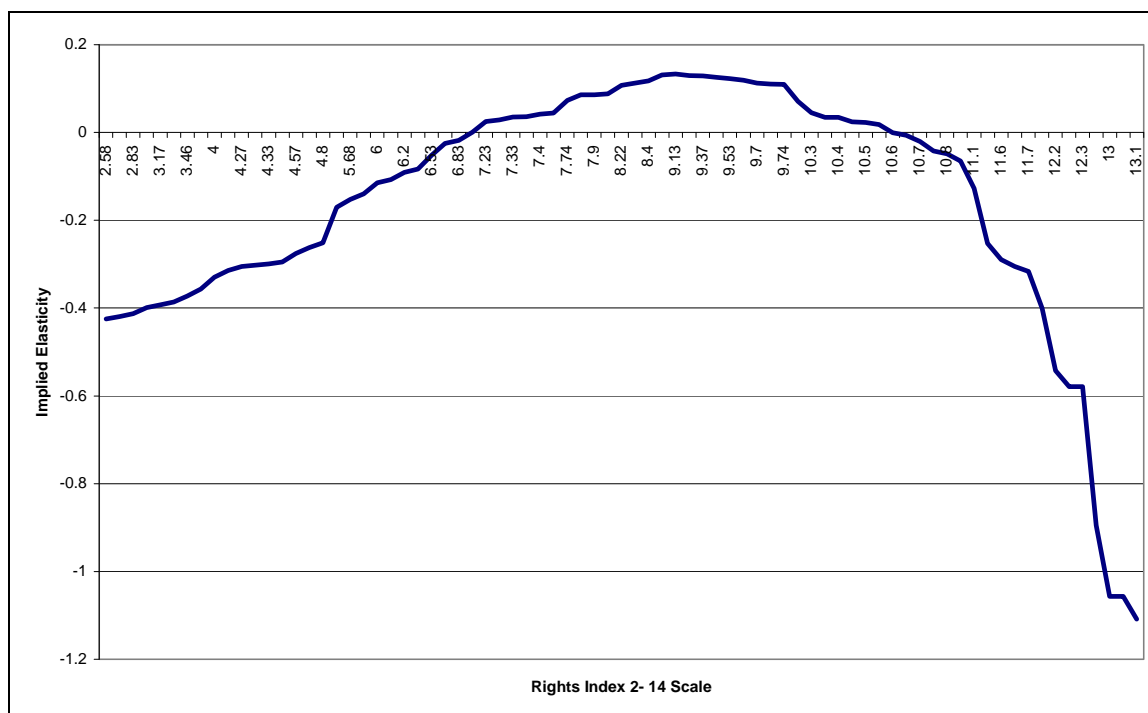


Figure 12: Line of Best Fit From Specification (6) of Table 9.

R	mean $\epsilon_{Y,R}$	ln(RGDP)	ln(Invest)	Growth dlnRGDP	Invest Growth dlnInvest	Change in Rights	Elasticity Disp. $\epsilon_{\max} - \epsilon_{\min}$	Invest. Prod. dlnY/dlnI
Classification 1								
2-4	-0.38	8.91	8.00	0.03	0.04	-0.03	-0.37	0.72
4-8	-0.09	7.61	8.52	0.02	0.04	-0.06	-0.05	0.46
8-12	0.08	6.64	7.17	0.01	0.03	-0.06	0.05	0.29
12-14	-1.26	5.92	6.97	0.01	0.05	-0.04	-0.33	0.30
Classification 2								
2-5	-0.24	8.51	8.40	0.02	0.03	-0.05	-0.23	0.69
5-8	-0.08	7.38	8.51	0.02	0.04	-0.06	-0.03	0.41
8-11	0.11	6.60	7.32	0.01	0.03	-0.07	0.05	0.35
11-14	-0.73	6.31	6.68	0.01	0.04	-0.03	-0.17	0.19

Table 16: Summary characteristics of Group V countries, over the range of the Rights Indexes. $\epsilon_{Y,R}$ denotes the elasticity of output with respect to rights; R denotes the relevant rights range

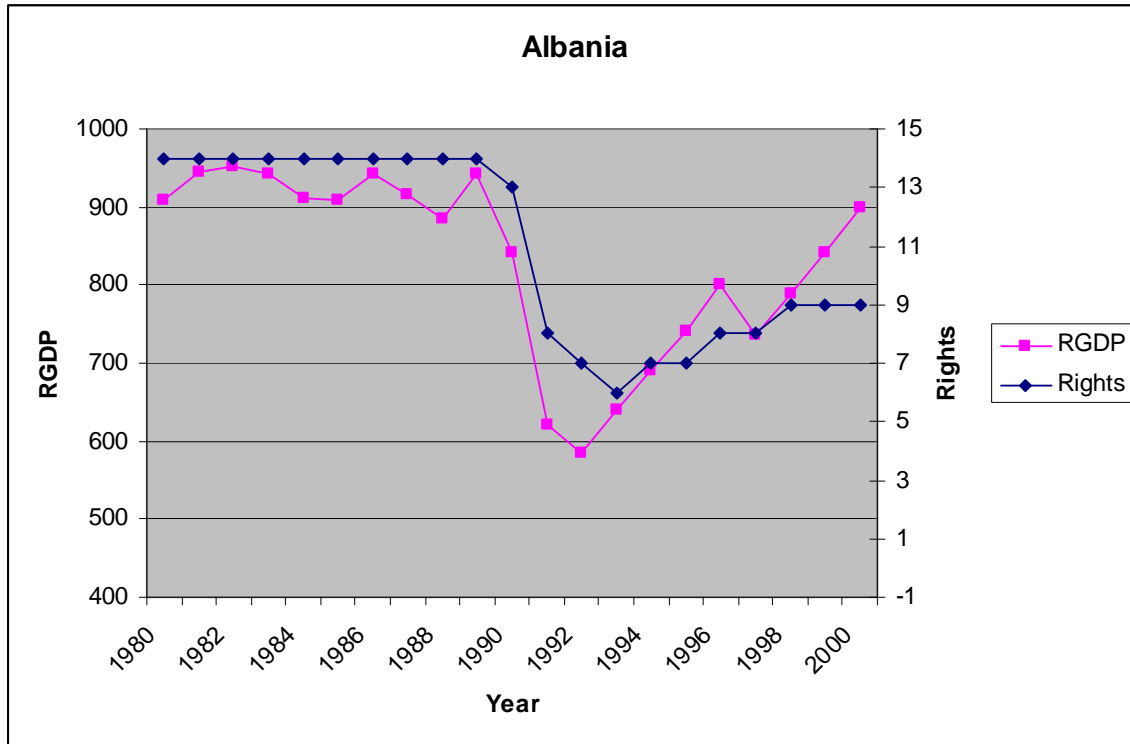


Figure 13: Albania

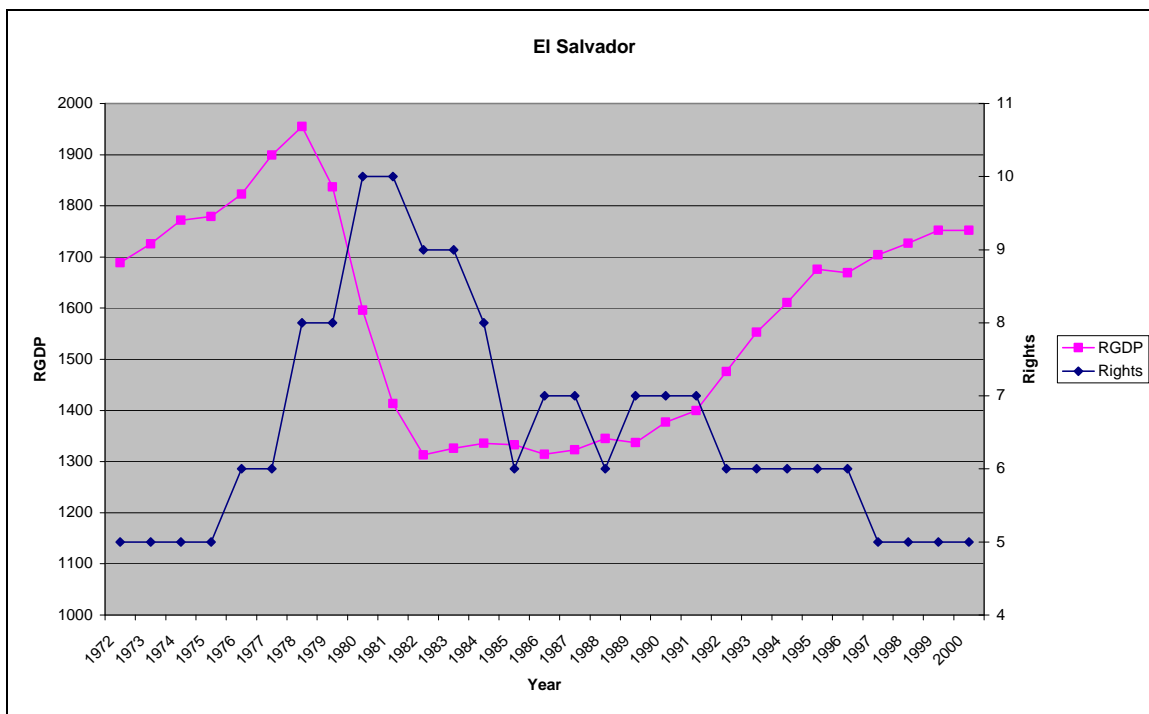


Figure 14: El Salvador

	Dependent Variable:	dRights	ECM	Mean dRights	Mean Growth (dlnRGDP)
Albania	dlnY ARDL(1,1)	.061563* (.014325)	-1.04* (.31)	-0.24138	-0.00061
Angola	dlnY ARDL(4,2) AIC	.11638*** (.053733)	-.63* (.18)	0.00000	-0.01608
Bulgaria	dlnY ARDL(1,1)	.034586*** (.016319)	-.75** (.28)	-0.34483	0.00615
Congo Brazzaville	dlnY ARDL(1,1)	.041351** (.016982)	-.52* (.16)	-0.17241	-0.04363
Djibouti	dlnY ARDL(1,3)	-.099282*** (.043317)	-.74 (.37)	0.20833	-0.04766
El Salvador	dlnY ARDL(1,3)	-.095481*** (.046392)	-.35*** (.17)	0.00000	0.00234
Equatorial Guinea	n/a				
Guinea	dlnY ARDL(1,1)	.024064*** (.012929)	-.74** (.23)	-0.10345	0.01295
Guyana	dlnY ARDL(3,4)	-.060716* (.012121)	-1.35* (.32)	0.00000	0.00629
Kuwait Pre-1990	dlnY ARDL(0,0)	.031527 (.034174)	n/a	0.03448	-0.03717
Kuwait Post-1992	dlnY ARDL(0,1)	-.19647** (.049378)	n/a	0.03448	-0.03717
Lebanon	dlnY ARDL(2,2)	.16414** (.041374)	-.70* (.08)	0.24138	0.01339
Mongolia	dlnY ARDL(1,3)	.048617* (.0041784)	-1.25* (.30)	-0.31034	0.00516
Nigeria	dlnY ARDL(1,2)	-.10226*** (.049904)	-.68* (.21)	-0.03448	-0.00504
Paraguay	dlnY ARDL(3,0)	-.010104 (.018880)	-.35 (.21)	-0.10714	0.01517
Poland	dlnY ARDL(1,0)	-.024101*** (.011851)	-.77* (.07)	-0.31034	0.03453
Rumania	dlnY ARDL(4,1)	.041516* (.012521)	-1.03* (.31)	-0.31034	0.00474
Sao Tome & Principe	dlnY ARDL(0,3)	.0058540*** (.0025732)	n/a	-0.26923	-0.00773
Sierra Leone	dlnY ARDL(1,1)	.017776 (.020051)	-.82* (.21)	0.00000	-0.02331
Tajikistan	dlnY ARDL(1,0)	-.061158 (.093286)	-.37 (.27)	0.60000	-0.07892
Tanzania	dlnY ARDL(2,2)	-.035263** (.0090646)	-1.48** (.40)	-0.13793	0.00404
Thailand	dlnY ARDL(1,0)	-.5898E+4 (.011753)	-.49** (.18)	-0.24138	0.04476
Trinidad	dlnY ARDL(2,1)	.14878 (.096587)	-.37*** (.18)	0.03448	0.02241
Vietnam	dlnY ARDL(2,1)	-.053695 (.032474)	-.55** (.21)	-0.04000	0.04262
Yemen	dlnY ARDL(1,1)	.020398 (.019344)	-.93** (.28)	0.13793	0.01436
Congo Kinshasa	dlnY ARDL(1,3)	-.17008 (.12686)	-.40** (.18)	0.00000	0.00702

Table 17: Impact of Changing Rights on Output Growth; * denotes significance at the 1%, ** at the 5%, and * at the 10% level; ECM denotes the error correction term.**

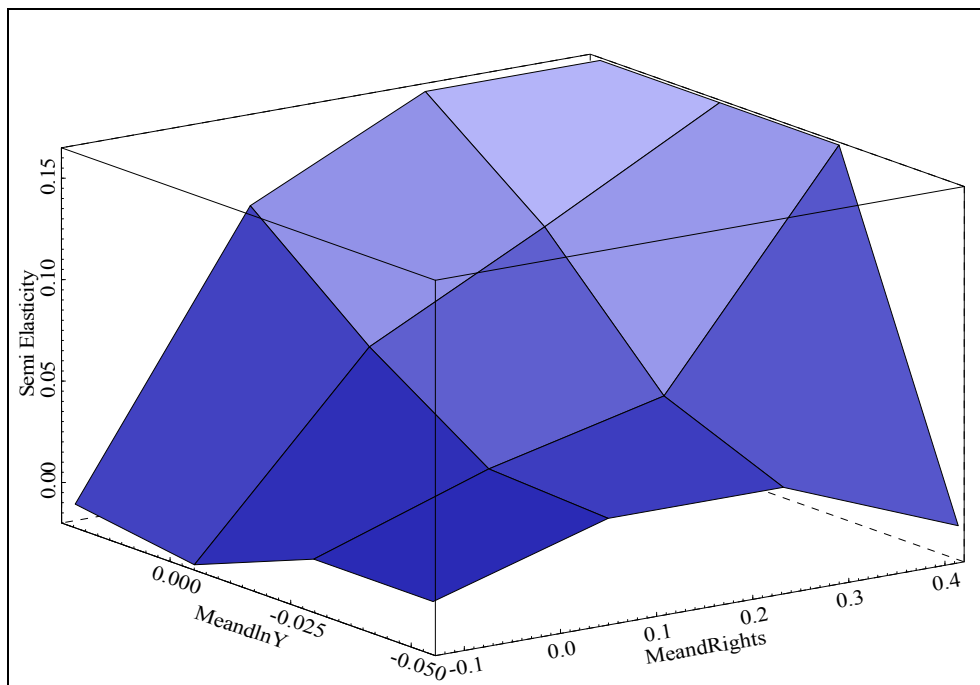


Figure 15: Estimated Semi-elasticities against mean Rights Change, Mean Output Growth

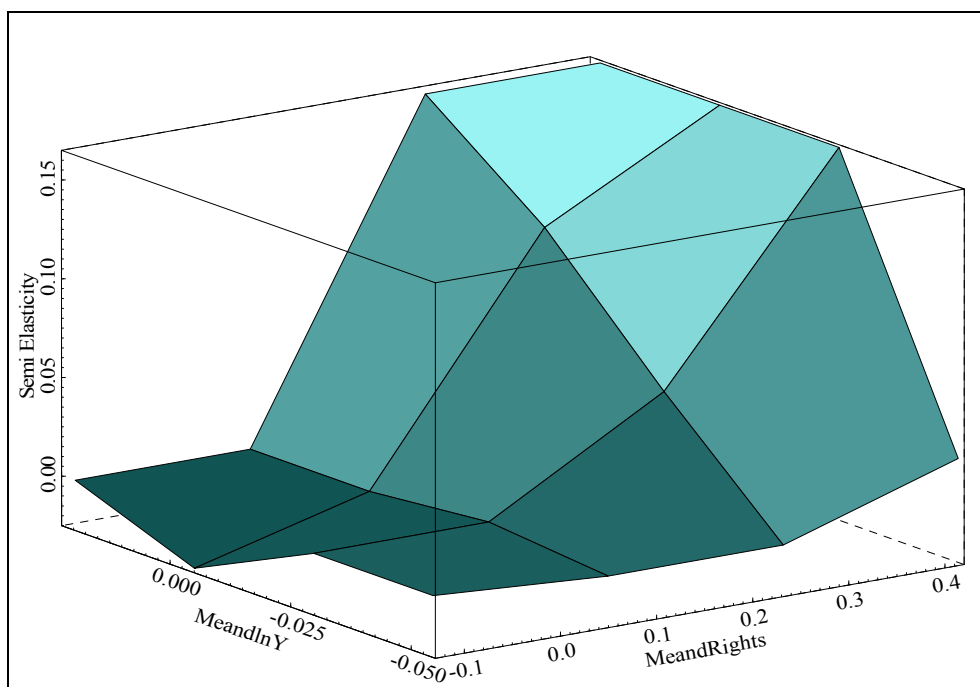


Figure 16: Estimated Semi-elasticities against mean Rights Change, Mean Output Growth; statistically insignificant coefficients zero-restricted

Full Sample	(1a)	(1b)	(1c)	(1d)	(2)	(3)
Estimator:	SFE	DFE	MGE	PMGE	PMGE	PMGE
Dep Variable:	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP
Info. Crit.:	n/a	ARDL (3,3,3)	AIC(3)	AIC(3)	ARDL (1,3,3,3)	ARDL (2,2,2,2,1,1,1)
Ln(Investment)	0.27* (0.07)	0.37* (0.08)	0.27* (0.13)	0.35* (0.01)	0.31* (0.004)	0.27* (0.01)
Rights	-0.01* (0.005)	-0.03* (0.01)	0.05 (0.06)	-0.02* (0.002)	-0.01* (0.001)	-0.01* (0.002)
Avg. Years Schooling					0.09* (0.003)	0.09* (0.01)
Ln(Exports)						0.25* (0.03)
Ln(FDI)						-0.02* (0.01)
Ln(CPI)						-0.05* (0.01)
ϕ	n/a	-0.05 (0.01)	-0.22* (0.03)	-0.11* (0.02)	-0.16* (0.03)	-0.12* (0.03)
h-test	n/a	n/a	n/a	1.01 [0.60]	5.44 [0.14]	0.57 [0.45] 1.00 [0.32] 2.29 [0.13] 4.16 [0.04] 0.09 [0.76] 0.59 [0.44]
LR	n/a	n/a	n/a	622.74 [0.00]	845.14 [0.00]	1369.80 [0.00]
RLL				4058.88	3698.63	3002.70
ULL				4370.25	4121.20	3687.60
Constant	n/a		1.27* (1.28)	0.67* (0.14)	0.92* (0.17)	0.66* (0.14)

Table 18: Dynamic Heterogeneous Panel Estimation: PMGE denotes pooled mean group estimators; * denotes significance at the 5%, ** at the 10% levels; ϕ denotes the adjustment to the long run equilibrium term; h-test denotes the Hausman test statistic under the null of long-run homogeneity, RLL and ULL denotes the restricted and unrestricted log likelihood.

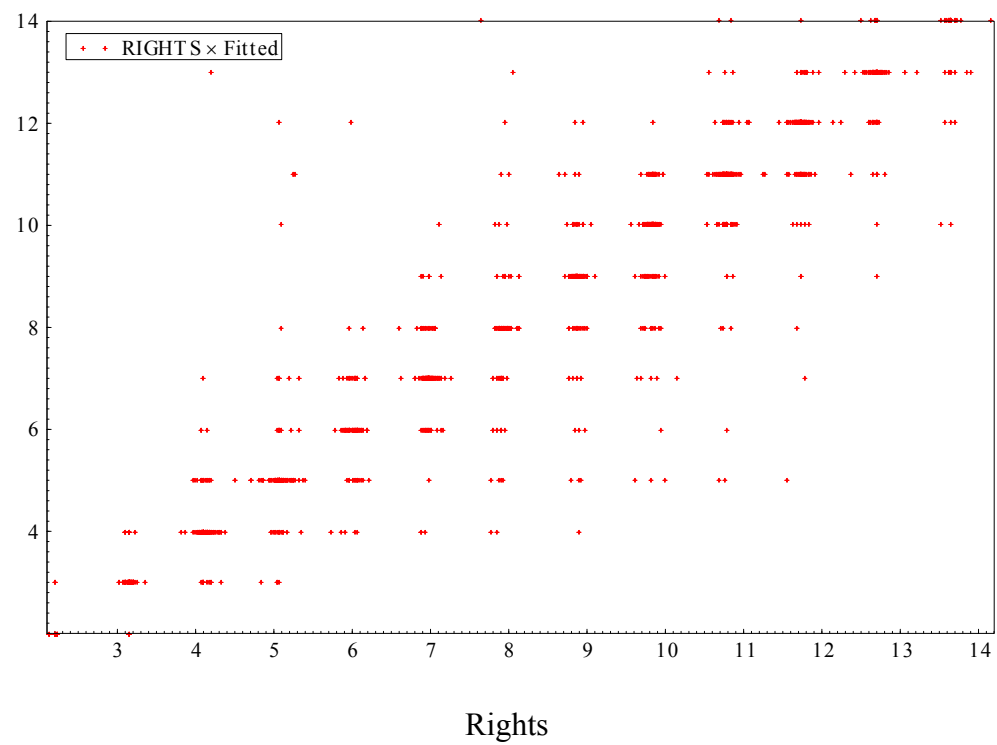
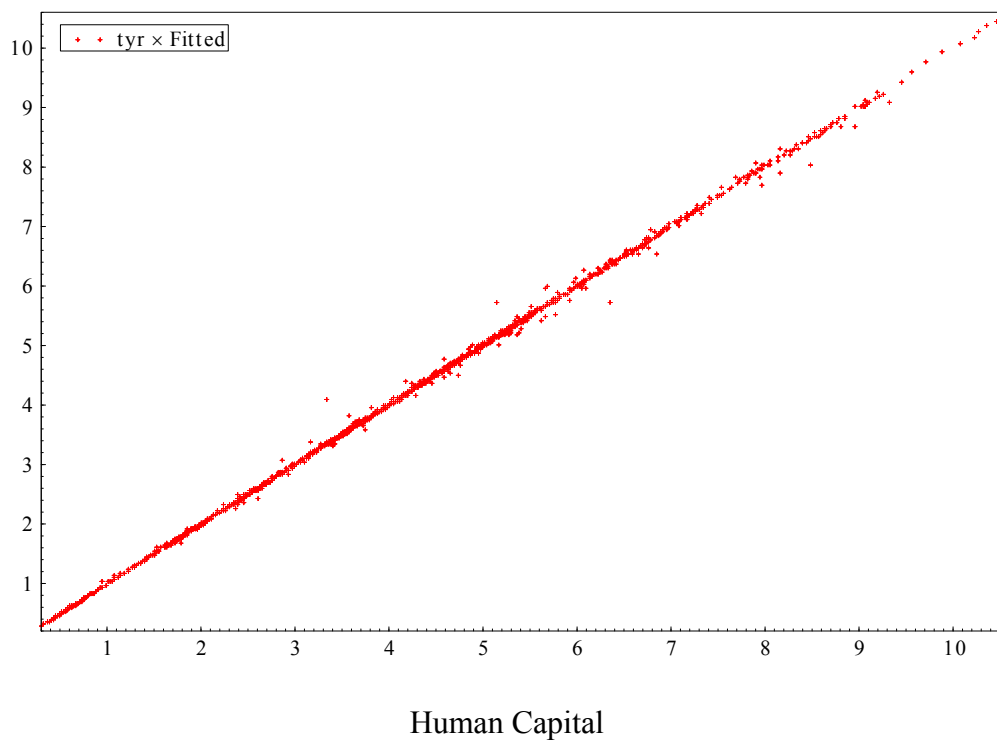


Figure 17: Depth of Human Capital and Rights

	(1)	(2)	(3)	(4)
Estimator:	PMGE	PMGE	PMGE	PMGE
Sample:	Full Sample	Rights <7	7<Rights<11	Rights>11
Dep. Variable:	RIGHT	RIGHT	RIGHT	RIGHT
Info. Criterion:	ARDL(2)	ARDL(2)	ARDL(2)	ARDL(2)
LUS	-0.217* (0.023)	-0.244* (0.025)	0.079* (0.013)	0.356* (0.129)
LPC	0.116* (0.022)	0.138* (0.028)	-0.014* (0.031)	0.000 (0.235)
LSC	0.140* (0.065)	0.366* (0.088)	-0.784* (0.057)	-0.508*** (0.298)
LHC	0.688* (0.094)	0.987* (0.113)	-0.739* (0.085)	-1.595* (0.689)
TYR	-3.447* (0.445)	-5.060* (0.550)	3.517* (0.195)	8.521* (2.917)
ϕ	-0.507* (0.064)	-0.629* (0.155)	-0.751* (0.132)	-0.513* (0.153)
h-test	5.78 [0.33]	3.53 [0.62]	5.47 [0.36]	46.59 [0.00]
LR	1459.72 [0.00]	751.66 [0.00]	734.62 [0.00]	193.18 [0.00]
RLL	-1186.38	1982.74	-538.91	-187.89
ULL	-456.52	2358.57	-171.60	-91.30

Table 19: Link between RIGHT and human capital measures. Dynamic Heterogeneous Panel Estimation: PMGE denotes pooled mean group estimators; * denotes significance at the 5%, ** at the 10% levels; ϕ denotes the adjustment to the long run equilibrium term; h-test denotes the Hausman test statistic under the null of long-run homogeneity, RLL and ULL denotes the restricted an unrestricted log likelihood.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Estimator:	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE
Sample:	Full Sample	Rights <7	7<Rights<11	Rights>11	Full Sample	Rights <7	7<Rights<11	Rights>11	Full Sample	Rights <7	7<Rights<11	Rights>11
Dep Var.	Ln(Exports)	Ln(Exports)	Ln(Exports)	Ln(Exports)	Ln(FDI)	Ln(FDI)	Ln(FDI)	Ln(FDI)	Ln(CPI)	Ln(CPI)	Ln(CPI)	Ln(CPI)
Info. Criterion:	ARDL(3,3)	ARDL(3,3)	ARDL(3,3)	ARDL(3,3)	ARDL(3,3)	HQ(3)	HQ(3)	HQ(3)	ARDL(3,3)	HQ(3)	HQ(3)	ARDL(3,3)
Rights	-0.012* (0.005)	-0.026* (0.007)	-0.004 (0.006)	0.081* (0.021)	-0.000 (0.000)	0.008* (0.002)	-0.008* (0.001)	-0.003* (0.001)	0.031* (0.003)	0.043* (0.012)	0.031* (0.004)	-0.119* (0.052)
ϕ	-0.262* (0.030)	-0.396* (0.081)	-0.210* (0.044)	-0.298* (0.073)	-0.078 (0.232)	-0.213* (0.080)	-0.414* (0.089)	-0.729* (0.102)	-0.412* (0.045)	-0.297* (0.091)	-0.574* (0.067)	-0.421* (0.051)
h-test	0.69 [0.41]	0.13 [0.72]	0.36 [0.55]	0.15 [0.69]	0.13 [0.71]	1.18 [0.28]	0.78 [0.38]	1.07 [0.30]	1.10 [0.29]	1.63 [0.20]	0.23 [0.63]	0.66 [0.42]
LR	169.88 [0.00]	46.01 [0.00]	86.96 [0.00]	25.53 [0.00]	228.72 [0.00]	59.01 [0.00]	141.38 [0.00]	28.42 [0.00]	112.30 [0.00]	43.46 [0.00]	79.08 [0.00]	10.52 [0.23]
RLL	1307.74	398.22	704.49	156.30	3201.07	1059.22	1962.66	597.73	385.64	53.29	158.21	-31.10
ULL	1392.68	421.22	747.97	169.06	3315.43	1088.72	2033.35	611.95	441.79	75.03	197.75	-25.84

Table 20: Policy and Rights. Dynamic Heterogeneous Panel Estimation: PMGE denotes pooled mean group estimators; * denotes significance at the 5%, ** at the 10% levels; ϕ denotes the adjustment to the long run equilibrium term; h-test denotes the Hausman test statistic under the null of long-run homogeneity, RLL and ULL denotes the restricted an unrestricted log likelihood.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimator:	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE
Sample:	Full Sample	Full Sample	Rights <7	Rights <7	7<Rights<11	7<Rights<11	Rights>11	Rights>11
Dep. Variable	RIGHT	lnRGDP	RIGHT	lnRGDP	RIGHT	lnRGDP	RIGHT	lnRGDP
Sel. Crit.	HQ(3)	HQ(3)	HQ(3)	HQ(3)	HQ(3)	HQ(3)	HQ(3)	HQ(3)
Ln(Invest)	-0.236 * (0.100)	0.375* (0.012)	0.762 * (0.210)	0.335* (0.018)	-0.526* (0.247)	0.440* (0.021)	-0.124 (0.114)	0.278* (0.021)
lnY	1.045* (0.243)		-2.662 * (0.322)		1.305* (0.365)		1.218* (0.362)	
Right(-1)		-0.025* (0.002)		-0.026* (0.002)		-0.022* (0.003)		0.001 (0.008)
ϕ	-0.296* (0.034)	-0.106* (0.021)	-0.287* (0.053)	-0.07*** (0.04)	-0.280* (0.045)	-0.119* (0.026)	-0.420* (0.110)	-0.170* (0.058)
h-test	14.78* [0.00]	2.18 [0.34]	4.57 [0.10]	1.70 [0.43]	9.10* [0.01]	0.73 [0.69]	3.73 [0.16]	2.99 [0.22]
LR	457.70 [0.00]	589.07 [0.00]	96.88 [0.00]	162.71 [0.00]	246.87 [0.00]	314.10 [0.00]	61.72 [0.00]	94.63 [0.00]
RLL	-2031.72	3945.19	-576.48	1316.18	-1104.12	2046.58	-325.00	591.31
ULL	-1802.87	4239.72	-528.04	1397.53	-980.68	2203.63	-294.14	638.62

Table 21: Testing for Direction of Causation. Dynamic Heterogeneous Panel Estimation: PMGE denotes pooled mean group estimators; * denotes significance at the 5%, ** at the 10% levels; ϕ denotes the adjustment to the long run equilibrium term; h-test denotes the Hausman test statistic under the null of long-run homogeneity, RLL and ULL denotes the restricted an unrestricted log likelihood.

	(1a)	(1b)	(1c)
Sample:	Rights<7	7<Rights<11	Rights>11
Estimator:	PMGE	PMGE	PMGE
Dep. Variable	lnRGDP	lnRGDP	lnRGDP
Info. Crit.:	HQ(1)	ARDL (1,0,0,0,0,0)	ARDL (1,0,0,0,0)
Ln(Investment)	0.42* (0.02)	0.45* (0.02)	0.29* (0.03)
Rights	-0.004* (0.002)	0.003 (0.003)	-0.02*** (0.01)
Avg. Years Schooling	0.04* (0.01)	0.04* (0.01)	0.04* (0.01)
Ln(Exports)	0.23* (0.02)	0.06* (0.02)	0.20* (0.04)
Ln(FDI)	-0.12 (0.08)	-0.13 (0.09)	0.07 (0.18)
Ln(CPI)	-0.02* (0.01)	-0.06* (0.02)	0.02 (0.02)
ϕ	-0.38* (0.10)	-0.20* (0.03)	-0.31* (0.07)
h-test	0.93[0.33] 1.23[0.27] 0.45[0.50] 0.10[0.75] 0.81[0.37] 0.35[0.55]	1.41[0.24] 1.48[0.22] 0.00[0.99] 0.09[0.76] 2.63[0.10] 1.39[0.24]	1.13[0.29] 1.04[0.31] 1.04[0.31] 1.24[0.27] 0.17[0.68] 1.04[0.31]
LR	636.03 [0.00]	416.01 [0.00]	162.64 [0.00]
RLL	1043.98	1149.51	383.23
ULL	1361.99	1357.51	464.54
Constant	2.11* (0.55)	1.22* (0.18)	1.48* (0.29)

Table 22: Taking Fuller Account of Heterogeneity - Dynamic Heterogeneous Panel Estimation: PMGE denotes pooled mean group estimators; * denotes significance at the 5%, ** at the 10% levels; ϕ denotes the adjustment to the long run equilibrium term; h-test denotes the Hausman test statistic under the null of long-run homogeneity, RLL and ULL denotes the restricted and unrestricted log likelihood.

	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)	(4a)	(4b)	(4c)	(5a)	(5b)	(5c)
Estimator:	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE
Sample:	Rights<7	7<Rights<11	Rights>11	Rights<7	7<Rights<11	Rights>11	Rights<7	7<Rights<11	Rights>11	Rights<7	7<Rights<11	Rights>11	Rights<7	7<Rights<11	Rights>11
Dep. Variable:	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)	Ln(RGDP)
Info. Crit.:	ARDL(1,2,1,1)	HQ(2)	SBC(2)	AIC(2)	ARDL(2,2,3,3,1,1)	ARDL(3,0,3,1,1,1)	SBC(2)	ARDL(2,2,2,1,1,1)	AIC(2)	AIC(2)	ARDL(2,2,2,1,0,2)	ARDL(1)	AIC(1)	AIC(2)	AIC(1)
Ln(INVEST)	0.413* (0.019)	0.481* (0.031)	0.247* (0.065)	0.344* (0.020)	0.420* (0.033)	0.164* (0.069)	0.312* (0.021)	0.478* (0.035)	0.206*** (0.109)	0.384* (0.018)	0.342* (0.037)	0.234* (0.069)	0.357* (0.024)	0.421* (0.026)	0.305* (0.053)
RIGHTS	0.005 (0.012)	0.004 (0.009)	-0.012 (0.010)	-0.066* (0.009)	0.066* (0.011)	-0.090* (0.024)	0.175* (0.023)	0.219* (0.034)	0.223* (0.059)	0.232* (0.081)	0.405* (0.157)	0.946* (0.467)	-0.044* (0.016)	-0.038 (0.023)	0.017 (0.042)
RIGHTS * Ln(INVEST)	-0.007* (0.003)	-0.007* (0.003)	0.006 (0.005)	0.014* (0.003)	-0.013* (0.003)	0.022* (0.006)	-0.008* (0.003)	0.001 (0.004)	0.001 (0.009)	-0.009* (0.002)	-0.003 (0.003)	0.006 (0.006)	-0.005* (0.003)	-0.005* (0.003)	-0.003 (0.004)
Avg. Years of Schooling				0.062* (0.010)	0.172* (0.017)	0.036 (0.077)									
RIGHTS * Avg. Years of Schooling				-0.001 (0.002)	-0.014* (0.002)	0.002 (0.007)									
Ln(EXPORT)							0.420* (0.037)	0.714* (0.120)	1.390* (0.278)						
RIGHTS* LN(EXPORT)							-0.046* (0.006)	-0.076* (0.014)	-0.081* (0.021)						
Ln(FDI)										0.837* (0.197)	1.226* (0.404)	3.681* (1.775)			
RIGHTS* LN(FDI)										-0.068* (0.026)	-0.124* (0.049)	-0.298* (0.144)			
Ln(CPI)													-0.059* (0.013)	-0.169* (0.056)	0.124 (0.157)
RIGHTS* LN(CPI)													0.010* (0.003)	0.011* (0.005)	-0.006 (0.013)
ϕ	-0.102*** (0.058)	-0.124* (0.030)	-0.376* (0.118)	-0.426* (0.104)	-0.166* (0.056)	-0.205* (0.073)	-0.150* (0.061)	-0.078* (0.028)	-0.307* (0.099)	-0.199* (0.086)	-0.107* (0.029)	-0.144* (0.045)	-0.109* (0.060)	-0.147* (0.047)	-0.192* (0.068)
h-test	3.15 [0.37]	2.48 [0.48]	5.02 [0.17]	1.41[0.23] 1.52[0.22] 0.41[0.52] 0.73[0.39] 0.84[0.36]	0.84[0.36] 1.06[0.30] 1.10[0.29] 1.88[0.17] 1.03[0.31]	1.93[0.17] 0.00[0.99] 1.95[0.16] 0.04[0.84] 0.01[0.91]	0.37[0.54] 0.99[0.32] 0.53[0.47] 0.43[0.51] 0.26[0.61]	7.12 [0.21]	8.51 [0.13]	8.77 [0.12]	10.26 [0.07]	8.24 [0.14]	0.01[0.91] 0.15[0.70] 0.07[0.79] 1.01[0.31] 1.18[0.28]	9.57 [0.09]	6.27 [0.28]
LR	172.44 [0.00]	270.87 [0.00]	337.46 [0.00]	1083.96 [0.00]	666.20 [0.00]	176.43 [0.00]	466.87 [0.00]	444.75 [0.00]	371.49 [0.00]	500.80 [0.00]	446.64 [0.00]	205.63 [0.00]	327.79 [0.00]	535.42 [0.00]	135.18 [0.00]
RLL	1349.68	2115.58	507.68	1147.08	1880.28	520.10	1365.93	2139.76	591.44	1262.66	2107.45	563.89	1228.16	1747.20	452.37
ULL	1435.90	2251.02	676.41	1689.06	2213.38	608.32	1599.37	2362.14	777.19	1513.06	2330.77	666.71	1392.05	2014.91	519.96

Table 23: Exploring Interactions 1: Dynamic Heterogeneous Panel Estimation: PMGE denotes pooled mean group estimators; * denotes significance at the 5%, ** at the 10% levels; ϕ denotes the adjustment to the long run equilibrium term; h-test denotes the Hausman test statistic under the null of long-run homogeneity, RLL and ULL denotes the restricted an unrestricted log likelihood.

	(1)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Estimator	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE
Sample:	Full Sample	Rights<7	Rights<7	7<Rights<11	7<Rights<11	Rights>11	Rights>11
Dep. Var.:	Growth	Growth	Growth	Growth	Growth	Growth	Growth
Info Crit:	ARDL (1,1,1,3,1,1,1)	ARDL (3,0,0)	ARDL (1,2,1,3,2,1,2)	ARDL (3,0,3)	ARDL (2,0,0,0,0,0,0)	ARDL (3,0,3)	ARDL (2,0,1,0,0,0,2)
Ln(Investment)	0.010* (0.002)	0.019* (0.005)	0.007* (0.003)	0.027* (0.003)	0.031* (0.004)	0.016* (0.006)	0.012 (0.011)
dRights	-0.002* (0.001)	-0.005* (0.002)	-0.002*** (0.001)	0.002 (0.002)	-0.001 (0.001)	-0.015* (0.007)	-0.011* (0.005)
Avg. Years of Schooling	-0.005* (0.001)		-0.003 (0.002)		-0.007* (0.002)		-0.003 (0.005)
Ln(EXPORT)	0.011* (0.003)		0.027* (0.007)		-0.004 (0.005)		0.045* (0.011)
Ln(FDI)	0.051* (0.016)		0.016 (0.024)		0.063* (0.028)		0.280* (0.122)
Ln(CPI)	-0.008* (0.001)		-0.006* (0.002)		-0.015* (0.002)		0.002 (0.009)
N	45	20	17	34	22	12	8
ϕ	-0.914* (0.043)	-0.826* (0.078)	-0.923* (0.094)	-0.988* (0.080)	-1.074* (0.102)	-0.753* (0.095)	-0.987* (0.120)
h-test	0.11 [0.74] 3.62 [0.06] 1.21 [0.27] 0.13 [0.72] 2.81 [0.09] 1.14 [0.29]	0.45 [0.80]	0.04 [0.84] 0.02 [0.89] 0.66 [0.42] 3.68 [0.06] 0.80 [0.37] 0.41 [0.52]	1.88 [0.17] 0.22 [0.64]	2.73 [0.10] 0.00 [0.98] 2.03 [0.15] 4.28 [0.04] 0.98 [0.32] 2.51 [0.11]	1.93 [0.38]	1.01 [0.32] 2.64 [0.10] 3.78 [0.05] 0.00 [0.97] 1.59 [0.21] 3.49 [0.06]
Constant	-0.171* (0.010)	-0.051* (0.007)	-0.113* (0.015)	-0.067* (0.007)	-0.206* (0.019)	-0.023* (0.006)	-0.954* (0.106)

Table 24: PMGE Examination of non-linearities in economic growth. * denotes significance at the 5%, ** at the 10% levels. ϕ denotes the adjustment to the long run equilibrium term; h-test denotes the Hausman test statistic under the null of long-run homogeneity.

	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)
Estimator	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE
Sample:	Rights<7	Rights<7	Rights<7	7<Rights<11	7<Rights<11	7<Rights<11	Rights>11	Rights>11	Rights>11
Dep. Var.:	dRights	Growth	Growth	dRights	Growth	Growth	dRights	Growth	Growth
Info Crit:	AIC(2)	HQ(3)	SBC(3)	SBC(3)	AIC(3)	AIC(3)	HQ(3)	HQ(2)	HQ(2)
Ln(Investment)	0.042 (0.078)	0.002 (0.003)	0.007* (0.003)	-0.093 (0.059)	0.008* (0.002)	0.008* (0.002)	-0.073 (0.060)	0.007*** (0.004)	0.007*** (0.004)
Growth	0.145 (0.954)			-0.667 (0.536)			-0.474 (0.621)		
dRights		-0.002* (0.001)			0.001 (0.001)			-0.006* (0.002)	
dRights(-1)			-0.002* (0.001)			0.001 (0.001)			-0.004* (0.002)
ϕ	-1.063* (0.059)	-0.955* (0.061)	-0.970* (0.057)	-1.105* (0.065)	-1.078* (0.073)	-1.021* (0.072)	-1.345* (0.198)	-1.046* (0.071)	-1.004* (0.086)
h-test	7.06* [0.03]	1.00 [0.61]	1.18 [0.56]	4.12 [0.13]	3.63 [0.06] 3.05 [0.08]	4.63 [0.10]	1.20 [0.55]	0.04 [0.84] 3.97 [0.05]	1.06 [0.30] 0.06 [0.81]
Constant	-0.254* (0.048)	0.005 (0.003)	-0.014* (0.003)	0.241* (0.042)	-0.013* (0.002)	-0.010* (0.002)	0.159* (0.070)	-0.011* (0.004)	-0.009* (0.004)

Table 25: Testing for Direction of Causation. * denotes significance at the 5%, ** at the 10% levels. ϕ denotes the adjustment to the long run equilibrium term; h-test denotes the Hausman test statistic under the null of long-run homogeneity.

	(1a)	(1b)	(1c)	(1d)	(1e)	(2a)	(2b)	(2c)	(2d)	(2e)	(3a)	(3b)	(3c)	(3d)	(3e)
Estimator	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE
Sample:	Rights<7	Rights<7	Rights<7	Rights<7	Rights<7	7<Rights<11	7<Rights<11	7<Rights<11	7<Rights<11	7<Rights<11	Rights>11	Rights>11	Rights>11	Rights>11	Rights>11
Dep. Var.:	Growth	Growth	Growth	Growth	Growth	Growth	Growth	Growth	Growth	Growth	Growth	Growth	Growth	Growth	Growth
Info Crit:	ARDL (3,0,0,1)	HQ(3)	ARDL (2,0,1,1,1)	ARDL (3,0,2,2,1)	ARDL (3,0,1,1,0)	ARDL (3,0,3,3)	ARDL (3,0,1,0,0)	ARDL (2,0,1,1,1)	ARDL (2,0,1,1,1)	ARDL (2,0,1,0,0)	ARDL (3,0,2,3)	ARDL (3,0,3,1,1)	ARDL (3,0,3,2,3)	ARDL (3,0,1,0,1)	ARDL (3,0,2,3)
Ln(Investment)	0.023* (0.005)	0.007* (0.003)	0.020* (0.006)	0.012* (0.005)	0.020* (0.005)	0.023* (0.003)	0.028* (0.003)	0.022* (0.003)	0.023* (0.002)	0.027* (0.003)	0.015* (0.007)	0.003 (0.011)	0.007 (0.006)	0.016* (0.006)	0.022* (0.009)
dRights	-0.012* (0.005)	-0.010* (0.003)	0.019 (0.011)	0.037 (0.104)	-0.014* (0.007)	0.009* (0.004)	0.001 (0.002)	0.003 (0.004)	0.008 (0.075)	0.001 (0.006)	0.019 (0.015)	-0.088* (0.031)	0.020 (0.038)	-0.98*** (0.55)	0.066 (0.057)
RIGHTS * Ln(INVEST)	0.002 (0.002)					-0.004* (0.002)					-0.024* (0.010)				
Avg. Years of Schooling		0.004* (0.001)					-0.001 (0.001)					-0.034* (0.011)			
RIGHTS* Avg. Years of Schooling		0.001* (0.001)					0.000 (0.001)					0.029 (0.012)			
Ln(EXPORT)			0.021* (0.008)					0.000 (0.004)					0.026* (0.010)		
RIGHTS* LN(EXPORT)			-0.008* (0.004)					-0.001 (0.002)					-0.013 (0.012)		
Ln(FDI)				0.104* (0.033)					0.020 (0.025)					0.244* (0.071)	
RIGHTS* LN(FDI)				-0.012 (0.031)					-0.002 (0.023)					0.297*** (0.168)	
Ln(CPI)					-0.009* (0.003)					-0.016* (0.002)					-0.002 (0.009)
RIGHTS* LN(CPI)					0.003 (0.002)					-0.001 (0.002)					-0.025 (0.017)
N	19	19	18	18	16	34	28	33	33	28	12	10	12	12	9
φ	-0.823* (0.086)	-1.001* (0.099)	-0.731* (0.089)	-0.826* (0.086)	-0.840* (0.099)	-0.886* (0.071)	-1.071* (0.083)	-0.983* (0.070)	-1.009* (0.073)	-1.011* (0.081)	-0.677* (0.102)	-0.609* (0.131)	-0.770* (0.106)	-0.765* (0.111)	-0.651* (0.138)
h-test	0.04 [0.85] 1.29 [0.26] 1.07 [0.30]	2.13 [0.14] 1.00 [0.32] 1.66 [0.20] 0.98 [0.32]	0.73 [0.39] 0.20 [0.65] 0.43 [0.51] 0.13 [0.72]	6.77 [0.15]	1.09 [0.30] 0.14 [0.71] 1.02 [0.31] 0.15 [0.70]	4.91 [0.18]	0.01 [0.93] 0.11 [0.74] 2.37 [0.12] 0.61 [0.44]	1.10 [0.29] 0.91 [0.34] 2.11 [0.15] 0.89 [0.35]	1.00 [0.32] 3.77 [0.05] 0.93 [0.33] 3.79 [0.05]	1.47 [0.22] 1.14 [0.28] 0.07 [0.79] 1.21 [0.27]	2.18 [0.14] 3.20 [0.07] 0.12 [0.73]	3.38 [0.50]	1.35 [0.25] 0.01 [0.92] 0.00 [0.97] 0.00 [0.99]	7.76 [0.10]	1.60 [0.21] 1.11 [0.29] 1.39 [0.24] 0.45 [0.50]
Constant	-0.062* (0.009)	-0.036* (0.005)	-0.095* (0.012)	-0.314* (0.033)	-0.025* (0.007)	-0.050* (0.006)	-0.070* (0.007)	-0.053* (0.005)	-0.123* (0.008)	-0.017* (0.006)	-0.019* (0.005)	0.043* (0.13)	-0.072* (0.012)	-0.639* (0.094)	-0.024* (0.008)

Table 26: PMGE Examination of non-linearities in economic growth: interaction effects. * denotes significance at the 5%, ** at the 10% levels. φ denotes the adjustment to the long run equilibrium term; h-test denotes the Hausman test statistic under the null of long-run homogeneity.