ELSEVIER
www.elsevier.com/locate/worlddev

World Development Vol. 51, pp. 187–206, 2013

0305-750X/$ - see front matter

http://dx.doi.org/10.1016/j.worlddev.2013.05.009

How Much Do Rights Matter?

JOHANNES FEDDERKE
Pennsylvania State University, USA
Economic Research Southern Africa, South Africa
University of the Witwatersrand, South Africa

and

ROBERT KLITGAARD *
Claremont Graduate University, USA

Summary. — Do better political rights yield more economic development? By addressing the econometric challenges plaguing this question, we find support for a positive impact of rights on development. For a significant grouping of countries the association is nonlinear: the positive impact of rights is particularly strong at low rights levels; it is either absent or negative in an intermediate rights range; and returns to a moderate positive impact at high levels of rights. There is also evidence to suggest that transitions from autocratic political dispensations are associated with significant negative output shocks.

Key words — governance, economic growth, rights

1. INTRODUCTION

The connections between democracy and development have spawned a vast literature ranging from philosophy through political science to economics. A fact lies at the heart of this research: over time, measures of democratic rights and liberties and measures of per capita income have both increased across countries. But what causes what? If a given country reforms its rights and liberties, can it expect per capita income to rise even faster than the trend? Or is it that economic development brings about, as a by-product, improved institutions? The simple correlation between measures of democracy and per capita income does not tell us.

Theory is also ambiguous.

A substantial body of literature asserts that it is institutions that drive development. At its most general the argument is Coasian: sound institutions lower transactions costs, thereby accelerating the rate at which exchange transactions among agents can grow. This proposition has been advanced narrowly with respect to property rights,[1] broadly to the incentives that are associated with the rules of interaction imposed by institutional dispensations,[2] with respect to neoliberal political dispensations favorable to economic development,[3] in relation to the formal legal structures adopted by societies,[4] and with respect to the informal social capital that generates trust.[5] Empirically, the link has been defended as being both strong,[6] and as more robust and important than competitor explanations.[7]

But the reverse direction of causation has also been asserted. Modernization theory advances the proposition that economic development brings with it the requirement for institutional evolution—and that if development is to be sustained, institutional development will itself be inevitable.[8]

So, in theory, causality can run both ways.

The empirical estimation game reflects the theoretical uncertainty. In the influential paper by Acemoglu *et al.* (2001), the potential endogeneity of measures of institutional quality (in their instance: expropriation risk) is dealt with by instrumenting on Settler mortality in the 19th century (shown to be strongly correlated with expropriation risk at the close of the 20th century, and presumably exogenous to economic performance in 2000). This strategy, while widely emulated, has been questioned on the grounds of the reliability of the measures of Settler mortality. Albouy (2012) argues that a significant proportion of the sample of 64 countries in the Acemoglu *et al.* (2001) data set is inferred from data outside of current national borders, that Settler mortality is inferred from military sources, and that results are sensitive to implied data corrections—though see the detailed response by Acemoglu *et al.* (2012). The general concern with the strength and validity of instruments,[9] has certainly found repeated echoes in relation to growth regressions.[10]

In terms of the reverse direction of causality, the evidence is no less contested. Acemoglu *et al.* (2008) find no evidence of an independent, or causal, relationship between a country's per capita income and various measures of democracy. While the simple correlation is strong, and over time there is a tendency toward more democracy and more income, they nonetheless do not support a causal effect of income on democracy, and instead interpret the evidence as the result of societies embarking on divergent development paths at critical junctures (Acemoglu *et al.*, 2008, p. 813). But Gundlach and Paldam (2008) criticize Acemoglu *et al.*'s methods. They argue that a small change in the estimation process immediately reveals the strong effect of income on democracy. And a newer paper by Heid *et al.* (2011) does find evidence of a statistically significant positive relation between income and democracy, argued to be robust across different specifications and choices of instrumental variables.

How does one make progress with empirical evidence under these circumstances?

* The authors would like to thank Kamil Akramov for research assistance on data compilation, and the generation of descriptive statistics for this study. Fedderke acknowledges the financial and research support of Economic Research Southern Africa. Final revision accepted: May 25, 2013.

In this paper we proceed as follows. We examine the link between institutions and growth at its most general, by considering the impact of political rights and civil liberties on per capita real output. Our choice of rights is motivated both theoretically and practically. In terms of theory, securely anchored democratic rights on average are likely to render more specific freedoms such as property rights more credible, and hence effective as drivers for growth—see Leblang (1996). There is also evidence to suggest that they foster a higher rate of technological innovation—see Aghion *et al.* (2006). And some authors argue that higher levels of social capital, hence lower transactions costs emerge under democratic institutions—see Paxton (2002) who report an interdependence. Our focus is on whether rights matter for economic performance rather than the reverse.

We then show transparently the impact of estimating the relationship under a range of alternative estimators, being explicit about the assumptions that require satisfaction under the estimators, and about whether they are likely satisfied in empirical application. In doing so, we are able to respond explicitly to concerns regarding omitted variables in estimation, and to potential endogeneity of regressors. Above all, the reader is able to establish how much difference the use of alternative estimation methodologies makes to inference—in short whether the debates regarding appropriate estimation approaches carries practical significance.

Our methodological finding is that choice of estimation approach matters a great deal. Of strongest concern is endogeneity bias, and bias that results from pooling countries that are heterogeneous.

Substantively, we find that improvements in rights do lead to increases in national income, though recognizing country heterogeneity and choice of appropriate estimation technique is crucial in isolating the effect. For a significant grouping of countries the positive impact of improvements in rights on output is strongest at low levels of rights, at intermediate levels of rights improvements in rights can lead to lower levels of income, while the positive impact of rights on output reemerges at higher rights levels. [11]

These findings carry important implications for any conceptualization of the interaction of rights and economic development. Under both very poor rights dispensations, and under the best rights, the results suggest that there are significant efficiency gains to be realized from improvements in rights, without the generation of negative externalities significant enough to offset the positive gains in economic performance. However, here exists some intermediate range of the rights measures, over which the positive efficiency gains either disappear, or perhaps are reversed. This may be due to the generation of significant levels of uncertainty that inhibit investment and other future discounting behavior, until a new stable institutional dispensation has been reached. While this posited mechanism is conjectural, the evidence certainly suggests the existence of a significant qualitative differentiation between "low-level" and "high-level" rights, and that the transition from one to the other is costly.

(a) *The question of this paper and core associated empirical methodological challenges*

In our introduction we have pointed to the active theoretical discussion of the link between rights and economic development. Yet despite both its history and the burgeoning nature of this literature, there is no theoretical agreement about measures or causal models. Under such conditions, how might we proceed with scientific rigor? Our approach is not only to heed theory insofar as it provides guidance—but also to allow feedback from empirical findings to theoretical reflection. [12]

In this spirit, consider a general specification:

$$Y_{it} = Y(K_{it}, R_{it}) + \varepsilon_{it} \tag{1}$$

where Y_{it} denotes real output of country i in period t, K a vector of independent variables (we might think of these as capital stocks of various sorts, physical, human, financial, and labor factor services–and whatever else might be thought relevant), R the measure or measures of rights we employ, and ε an unobserved error term. Our question is first whether $\partial Y/\partial R \neq 0$, $\partial Y/\partial R > 0$ in particular, and the strength (if any) of the $\partial Y/\partial R \neq 0$ effect.

Estimation in such a context introduces a relatively undertheorized dimension (rights) into estimation, and thereby stands under the suspicion of being subject to unobserved effects, [13] such that the true specification of (1) is in fact:

$$Y_{it} = Y(K_{it}, R_{it}, C_{it}) + \varepsilon_{it} \tag{2}$$

where C_{it} denotes an unobserved random variable, a vector of characteristics that also impact on output. Under time-invariant unobserved effects, $C_{it} = C_i$, given that it is consistent even where $cov(C_i,K_{it}) \neq 0$, and/or $cov(C_i,R_{it}) \neq 0$, the Fixed Effects (FE) estimator is the obvious choice. In the present study we employ the within version of the estimator. [14]

The direct advantage of the FE estimator is thus that it directly corrects for omitted variables bias. In the present study we further check for the potential impact of omitted variables by controlling for a range of additional potential determinants of output.

However, the FE estimator imposes an exogeneity assumption, such that explanatory variables in each time period are uncorrelated with the errors in each time period, [15] in order for the estimators to be consistent. In our context, feedback effects from the dependent variable at least to future values of the explanatory variables are plausible. For instance, shocks to output may well carry implications for the stability of political dispensations, and hence the level and quality of rights. Empirical research on the impact of governance on development has spent considerable effort on dealing with this endogeneity problem. In accordance with the classic statistical prescription, researchers have sought instruments that are uncorrelated with errors in each period. For instance, Acemoglu *et al.* (2001) rely on 19th century Settler mortality. [16]

Unfortunately, Bazzi and Clemens (2013) show how fundamentally fraught all the instrumentation strategies proposed in the literature are. The issue is that precisely when instruments are *strong* (highly correlated with the endogenous variable), they are likely to be *invalid* (correlated with variables that materially affect growth, other than the variable being instrumented for). Thus any instrument Z_{it}, which is strong (for instance $corr(R_{it},Z_{it}) \neq 0$), but for which there is an association with another channel which affects growth not controlled for in estimation, $cov(C_{it},Z_{it}) \neq 0$, since the error term in estimating (2) is given by $\upsilon_{it} = C_{it} + \varepsilon_{it}$, it follows that $cov(Z_{it},\upsilon_{it}) \neq 0$, such that the the instrument is invalid, leaving parameter estimates biased to an unknown degree and in an unknown direction. The solution might appear to be the inclusion of C_{it} in the estimation of (2). But this comes at the cost of requiring unique instruments for each of the endogenous RHS variables, each of which is valid and strong. Thus for the two instruments Z_{it}, $\tilde{Z}_{it}$, we require $cov(Z_{it},\upsilon_{it}) = cov(\tilde{Z}_{it},\upsilon_{it}) = 0$, and $corr(R_{it},Z_{it}|\tilde{Z}_{it}) \neq 0$, $corr(C_{it},\tilde{Z}_{it}|Z_{it}) \neq 0$. In short, ensuring instrument validity, may render the instruments weak.

Fundamentally the issue here is that reliance on the range of instruments proposed in the growth literature suffers from theory open-endedness. Given the range of theories of

economic growth, which are generally not mutually exclusive, it is difficult to exclude the possibility that many proposed instruments themselves have a direct impact on economic performance, or that they are correlated with omitted growth determinants, and this renders them invalid.[17] Ironically, the extensive use in the growth literature of the same instruments for a wide range of possible determinants of growth, has served to demonstrate the invalidity of the instruments being deployed.

These estimation problems might appear terminal. But an alternative estimation approach that allows for valid instruments is given by the Generalized Methods of Moments (GMM) class of estimators. Given the difficulties with instrumentation outlined above, the estimator has received increased attention in the growth literature—see Caselli *et al.* (1996), Levine *et al.* (2000) and Bond *et al.* (2001) for first applications to growth contexts. GMM formulates a set of moment restrictions using lagged values of variables in the model, to find parameter estimates that come as close as possible to achieving the orthogonality properties with the error structure of the model. Difference GMM employs the first difference transform of (2) under the moment conditions $E(Y_{i,t-j},\Delta\varepsilon_{it})=0$, $E(K_{i,t-j},\Delta\varepsilon_{it})=0$, $E(R_{i,t-j},\Delta\varepsilon_{it})=0$; while asymptotically the moment conditions would be set under the full set of lags,[18] the consequence is that in small samples the instrument set becomes overidentified, the instruments are rendered weak, and the result is a downward bias in the direction of the within group estimator particularly in the presence of persistent series.[19] The systems GMM estimator, which augments the difference equation with a levels equation, and which exploits the additional set of moment conditions $E(\omega_{it},\Delta Y_{i,t-1})=0$, $E(\omega_{it},\Delta K_{i,t-j})=0$, $E(\omega_{it},\Delta R_{i,t-j})=0$, $\omega_{it}=C_i+\varepsilon_{it}$, for $t=3,\ldots,T$.[20] Note explicitly that the $K_{i,t}$, $R_{i,t}$, are assumed endogenous, so that $cov(K_{it},\varepsilon_{it})\neq 0$ and $cov(R_{it},\varepsilon_{it})\neq 0$, here are *not* terminal (in contrast to the FE above)—in effect the estimator is designed to deal with precisely this difficulty. It is feasible to test for the validity of instrumentation by means of a Sargan test[21] of whether the parameter estimates are consistent with the orthogonality conditions. Weakness of instruments under systems GMM has received less attention than validity. Unfortunately the recent examination of this issue in Bazzi and Clemens (2013) provides Monte Carlo evidence of the fact that given that the systems GMM estimator relies on lagged levels of regressors explaining the variance of current differences, and such associations are inherently weak. the systems GMM may itself suffer from the weak instruments that it was trying to correct for in difference GMM, and that the impact on precision may be nonnegligible.

There is a further potential concern with results that we might derive from GMM. Suppose that Eqn. (1) took the form (omitting the impact of capital for the sake of illustration):

$$Y_{it}=\beta_i R_{it}+\varepsilon_{it},\ i=1,2,\quad t=1,\ldots,T \tag{3}$$

$$R_{it}=\rho R_{i,t-1}+\upsilon_{it},\ |\rho|<1,\ \upsilon_i\sim iid(0,\sigma^2) \tag{4}$$

A dynamic specification of (1) is more reasonable than static formulations provided only that changes in rights do not have an instantaneous impact on economic performance. Where the model is estimated in dynamic specification as:

$$Y_{it}=\lambda Y_{i,t-1}+\beta R_{it}+\omega_{it},\quad i=1,2 \tag{5}$$

Robertson and Symons (1992) demonstrate that under OLS, the estimators of both λ and β are biased, with Monte Carlo experiments indicating the dynamics to be misleading even for $T=40$. To deal with this problem Pesaran and Smith (1995) consider estimation of a dynamic panel data model allowing parameters to be individually heterogeneous, such that:

$$Y_{it}=\lambda_i Y_{i,t-1}+\beta_i R_{it}+\omega_{it},\ i=1,\ldots,N$$
$$t=1,\ldots,T,\quad \lambda_i\sim iid\left(\lambda,\sigma_\lambda^2\right),\quad \beta_i\sim iid\left(\beta,\sigma_\beta^2\right) \tag{6}$$

with λ_i, β_i, independent of Y_{is}, R_{is}, ω_{is}, $\forall s$. Since this renders ω_{it} correlated with all present and past values of Y_{it-1-s}, R_{it-1-s}, least squares estimation will be inconsistent regardless of whether estimation proceeds by (a) aggregate time-series regressions of group averages; (b) cross-section regressions of averages over time; (c) pooled regressions allowing for fixed or random intercepts; or (d) separate regressions for each group, averaging coefficients across groups. Moreover, since the error is correlated with lagged values of the regressors, lags are ruled out as valid instruments, thereby precluding GMM estimation.

A response to these concerns is to employ the Pooled Mean Group (PMG) estimator of Pesaran *et al.* (1999). Consider the unrestricted error correction ARDL(p,q) representation:

$$\Delta y_{it}=\phi_i y_{i,t-1}+\boldsymbol{\beta}_i'\mathbf{x}_{i,t-1}+\sum_{j=1}^{p-1}\lambda_{ij}\Delta y_{i,t-j}+\sum_{j=0}^{q-1}\boldsymbol{\delta}_{ij}'\Delta\mathbf{x}_{i,t-j}+\mu_i+\varepsilon_{it},$$
$$i=1,\ldots,N,\quad t=1,\ldots,T \tag{7}$$

where μ_i represents fixed effects. Provided the disturbances $\varepsilon_{it}\sim\left(0,\sigma_i^2\right)$ are independently distributed across i and t, and $\phi_i<0$ for all i, there exists a long-run relationship between y_{it} and $\mathbf{x}_{it}$:

$$y_{it}=\boldsymbol{\theta}_i'\mathbf{x}_{it}+\eta_{it},\quad i=1,2,\ldots,N,\quad t=1,2,\ldots,T, \tag{8}$$

where $\boldsymbol{\theta}_i=-\boldsymbol{\beta}_i'/\phi_i$ is the vector of the long-run coefficients, and η_{it}'s are stationary with possibly nonzero means (including fixed effects).[22] Under these assumptions the PMG estimator allows the intercepts, short-run coefficients and error variances to differ freely across groups, but the long-run coefficients to be homogenous; that is, $\boldsymbol{\theta}_i=\boldsymbol{\theta}$, $\forall i$. Group-specific short-run coefficients and the common long-run coefficients are computed by pooled maximum likelihood estimation. Denoting these estimators by $\tilde{\phi}_i,\tilde{\boldsymbol{\beta}}_i,\tilde{\lambda}_{ij},\tilde{\boldsymbol{\delta}}_{ij}$ and $\tilde{\boldsymbol{\theta}}$, we obtain the PMG estimators by $\hat{\phi}_{PMG}=\frac{\sum_{i=1}^{N}\tilde{\phi}_i}{N}$, $\hat{\boldsymbol{\beta}}_{PMG}=\frac{\sum_{i=1}^{N}\tilde{\boldsymbol{\beta}}_i}{N}$, $\hat{\lambda}_{jPMG}=\frac{\sum_{i=1}^{N}\tilde{\lambda}_{ij}}{N}$, $j=1,\ldots,p-1$, and $\hat{\boldsymbol{\delta}}_{jPMG}=\frac{\sum_{i=1}^{N}\tilde{\boldsymbol{\delta}}_{ij}}{N}$, $j=0,\ldots,q-1$, $\hat{\boldsymbol{\theta}}_{PMG}=\tilde{\boldsymbol{\theta}}$.

Advantage of PMG estimation is that it exploits the statistical power offered by the panel through long-run homogeneity, while still admitting short-run heterogeneity. In the context of growth studies, since the mechanisms being identified are those that determine long run economic performance at the most fundamental of levels, there is a presumption of generality. Thus long-run homogeneity should be discernible-though whether this is justified is of course an empirical question. The crucial question is whether the assumption of long-run homogeneity is justified empirically, given the threat of inefficiency and inconsistency.[23] Also, provided we have a valid long-run relationship between variables ($-2<\phi<0$, $\varepsilon_{it}\sim I(0)$), concern that omitted time-varying explanatory variables would violate the $cov(\mathbf{x}_{i,t-1},\varepsilon_{it})=0$ exogeneity condition is attenuated, provided that the variables in levels are stationary in first differences.

We have suggested that the problem of possible reverse causality between economic development and rights can potentially be dealt with by means of GMM estimation—but also

that a fuller consideration of dynamics may come to preclude this option, and necessitate reliance on estimators such as the PMGE. It is therefore worth noting that where we have access to clinical country-specific data with a sufficiently large time dimension, use of cointegration techniques in estimation carry a further means of resolving the reverse causality problem. Reverse causality has its destructive bias and inconsistency impacts on estimators because the violation of the $cov(R_t,\varepsilon_t) = 0$ assumption does not allow signal and noise to be clearly differentiated in estimation. Under cointegration, under full asymptotics since the order of integration of the explanatory variables is greater than that of the error term (typically $R_t \sim I(1)$, $\varepsilon_t \sim I(0)$), the Granger superconsistency property demonstrates that signal and noise can be separated. Estimation in practical contexts still allows the structure of association between variables to be tested for (Section 3(c)(ii) elaborates). In our analysis, we will consider results from GMM, PMGE, as well as country specific time series estimation, and reflect in what relation they stand to one another.

2. THE DATA

Given the range of theoretical propositions that have emerged in the literature,[24] in principle R in Eqn. (1) is appropriately specified as a vector of governance measures which might impact development. However, moving from underlying theoretical concepts to empirically operationalizeable measures to data has been problematic, since closely matching empirical measures to theoretically proposed concepts has proved elusive.[25] Specifically, in practical estimation contexts governance measures proposed by theory are either not available at all (for instance a narrow measure of property rights), while those measures that are available are subject to incomplete time and geographical coverage. The consequence is that the empirical literature exploring the impact of governance on development has employed a wide array of measures.[26]

This study considered a wide range of data on governance, on real per capita output, on investment, on human capital, and on the quality of economic policy. A total of 71 institutional and governance variables were considered—though in estimation we employed primarily the Freedom House civil liberties and political rights measures, due to the length of time and breadth of country coverage they offer. A full description of all variables, their sources, and some principal characteristics are reported in the working paper version of the paper, Fedderke *et al.* (2011). The various governance measures have different geographical coverage: some include over a hundred countries, others many fewer. Importantly for our purposes, the time coverage of the alternative governance measures was also strongly differentiated. For very few governance measures are observations available over time runs that allow the impacts of changes in governance on economic development to be reliably isolated. Nonetheless, over the time periods for which we do have data, the measures of governance are highly correlated among one another. Typical correlations range over 0.6–0.8, so that considering the impact of measurement error the "true" correlation range may be 0.7–1.

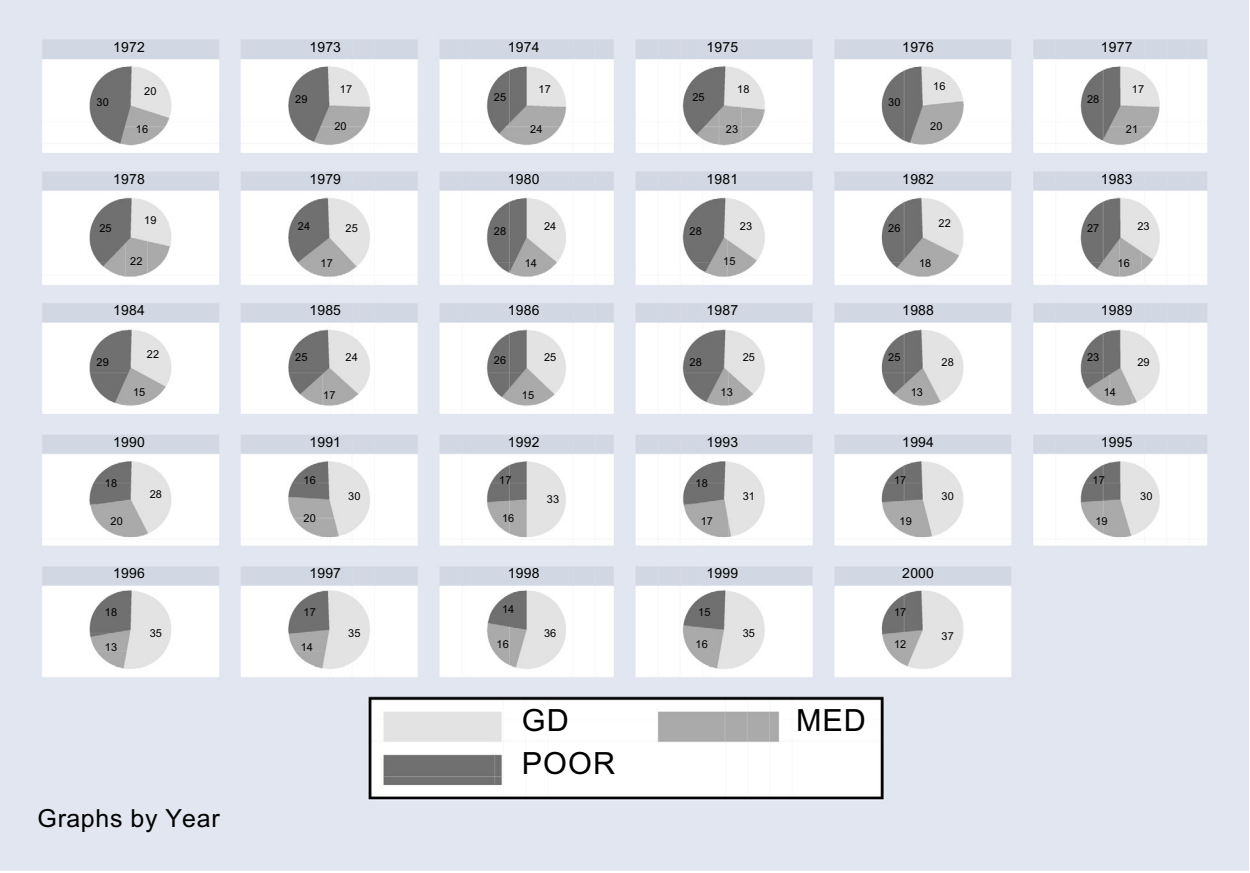

Figure 1. *The world-wide improvement in rights.*

Table 1. *ADF results. Superscripts provide cross-references for multiple classifications*

Output; Investment	Rights		
	$\sim I(0)$	$\sim I(1)$	$\sim I(>1)$
$\sim I(0)$	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

In an earlier study we emphasized the potential problems these "webs of association" between social indicators create for the identification and interpretation of robust estimation results—see Fedderke and Klitgaard (1998). Data concerns do not rest here. Additional concerns arise from the subjective nature of many governance measures used in the literature, from the use of discrete categorical scales and their interpretability, as well as the sometimes unusual distributional characteristics of the data. A more extensive discussion of these and other concerns is contained in Klitgaard and Fedderke (1995), and the working paper version of this paper (Fedderke *et al.*, 2011), in which the wider literature on the topic is also considered. The topic is vast, reflecting its importance, and here we restrict ourselves to narrower estimation concerns.

For this reason estimation is on two distinct data sets. The first is an almost balanced panel of 66 countries for the 1971–2000 time period,[27] 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. The second is provided by time series data on a country by country basis. Here we assembled data for 162 countries–though many of these data are available only for some years in the period of 1971–2000. In both, our analysis places emphasis on two of the governance measures, civil liberties (CIV_LIB) and political rights (POL_RIGHT) generated by Freedom House. Each of them correlates "highly" with most other governance measures (more consistently so than any other measure of governance) and with the first factor from a factor analysis of all governance measures. But critically, what renders these measures of particular interest is that both are available for many years and many countries, allowing the dynamics of development and growth to be explored. In estimation we employ a composite measure of rights (denoted RIGHTS), given by a linear combination of the CIV_LIB and POL_RIGHT.[28]

Five human capital measures standard to the growth literature are obtained from the Barro and Lee (1993) 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. In addition we 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.

A number of features emerge from a descriptive consideration of the data used for this study.

Average world rights improve over the time period studied in this paper—see Figure 1—though this hides considerable heterogeneity at the country and regional levels. The general improvement in rights has tended to be concentrated at discrete time points, with 1979, 1996, and to a lesser extent 1988 showing noticeable improvements. For many individual countries, however, rights change often and without clear trends. The strongest improvements in rights for our 66 country panel occurred in North West, Central & Southern Europe, while South America, Central America and the Caribbean, and Oceania on average show movement from poor to mid-level rights. Some countries (e.g., Benin, Greece, and Malawi) experience sudden and lasting transitions from low to high levels of freedom; others (e.g., Ghana, Nigeria, and Thailand) experience sharp shifts in both directions; while still others remain relatively unchanged or show no clear pattern over time. The worst rights on average were found in the Middle East & North Africa, Sub-Saharan Africa, and Asia.

Consideration of the time series structure of the data for individual countries, carries one immediate fundamental implication for any empirical exploration of the association between rights and economic performance. The stationarity properties of the core variables included in this study,[29] are summarized in Table 1, 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, for both the institutional (Rights) and economic (output, investment) dimensions. Countries manifest different combinations of orders of integration across the two economic, and the two Freedom House rights measures employed for this study.[30]

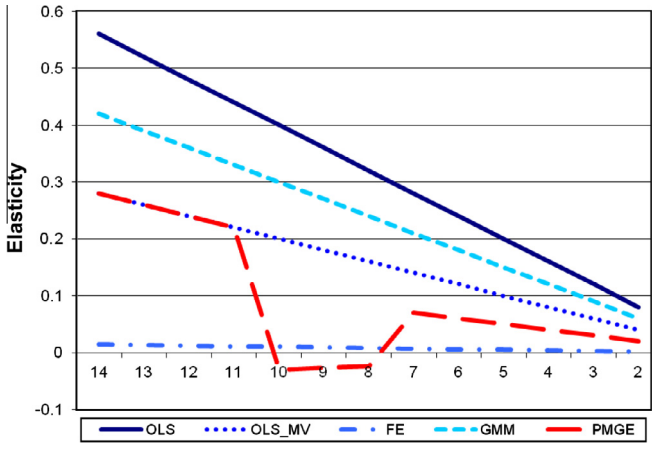

Figure 2. *Elasticity of output with respect to composite rights.*

Three sets of countries constitute the majority of cases. Group IV countries in Table 1 have stationary rights variables, but first difference stationary economic variables. For Group V countries both rights and economic variables are first difference stationary. In Group VIII rights 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 rights and economic variables–and every combination has at least one country that falls into these categories.

It follows immediately that the form of association between rights as measured by the Freedom House indicators, and economic performance must differ between the various groupings of countries. For instance, in Group IV countries, rights with their 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, rights can only be associated with growth in output and investment. Group VIII countries are symmetrical, except that in this case growth in rights 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. It follows, therefore, that the association between rights and economic performance across countries, must be heterogeneous.

The inference that we draw from this section is that care must be taken to recognize both the relevance of dynamics, and the existence of possible heterogeneity in the form of association between rights measures and economic outcomes across countries. In the estimation results reported below, this will be a consistent thematic concern.

3. ESTIMATION RESULTS

Given the restricted data availability across geography and time, in our empirical estimation we consider the impact of a composite rights measure comprized of civil liberties and political rights on economic development.

In Section 3(a) we consider results from pooled OLS estimation, test for the presence of unobserved effects, and correct where appropriate by means of FE estimation. Section 3(b) extends the analysis by considering the impact of endogeneity, by means of GMM estimation. In Section 3(c) we consider possible cross-country heterogeneity (as well as endogeneity) by means of both clinical country-by-country estimations employing time series techniques of analysis and appropriate panel data estimators.

Throughout, one of our explicit concerns is to provide an indication of *how much* impact the alternative empirical approaches have on estimates of the impact of rights on economic development. We keep track of these in a common graphical representation, provided by Figure 2. Throughout, note that the estimated elasticities of output with respect to rights are variable across the range of feasible rights scores.

(a) *Baseline results*

We begin by estimating Eqn. (1) in linear form, such that we have:

$$Y_{it} = \beta_0 + \beta_1\left(\frac{dK_{it}}{dt}\right) + \beta_2 R_{it} + \sum_{j=1}^{k}\gamma_j X_{j,it} + \varepsilon_{it} \tag{9}$$

where Y_{it} denotes the natural log of real per capita GDP, dK_{it}/dt the natural log of real investment in physical capital stock, R_{it} our composite rights measure, and the $X_{j,it}$ denote k additional explanatory variables potentially relevant to output determination.

The X_j vector includes indicator variables for geographical location (we employ dummies for sub-Saharan Africa, denoted C&S Africa, the Middle East and North Africa, denoted MENA, Central America, and the Caribbean, denoted CAmer & Carib, South America, denoted SAmer, and Asia, denoted Asia),[31] British colonial origin (Britain), and most recent date of state formation (StateForm). We further include five measures from the standard Barro and Lee (1993) human capital data set on the level of and investment in human capital, the percentage of the population without schooling (denoted % Pop No School), the percentage of the population that has completed primary schooling (denoted % Pop Prim. Compl.), the percentage of the population that has completed secondary schooling (denoted % Pop Sec. Compl.), the percentage of the population that has completed high school (denoted % Pop High Sch. Compl.), and the average years of education in the population (denoted Avg. Years Schooling).[32] 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 Exports), 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 the quality of macroeconomic stabilization policy as measured by the inflation rate (denoted Inflation).[33]

Table 2 reports estimation results. Column (1a) presents results under a $\gamma_j = 0$, $\forall j$ restriction in Eqn. (9). It confirms a statistically significant positive impact of investment on real per capita GDP, and a statistically significant benevolent impact of rights (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.[34]

The impact 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 (RIGHTS score of 14), which declines to 0.08% at the best possible rights rating (RIGHTS score of 2). Figure 2 illustrates the variable elasticity under pooled OLS estimation over the rights range as *OLS*.

Table 2. *Static estimation—log of real GDP*

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

Notes: FE denotes static fixed effects, FEGLS generalized least squares fixed effects, FDOLS first difference OLS estimation. Numbers in round parentheses denote robust standard errors, in square parentheses probability levels.
* Significance at the 1% level.
** Significance at the 5% level.
*** Significance at the 10% level.

First results thus confirm a benevolent association between rights and economic development—though the strength of the association diminishes as the quality of rights continues to increase, suggesting diminishing returns to improvements in governance in terms of its impact on real per capita output.

In order to test whether this result is merely an artifact of omitted dimensions we know to be important for long run economic development, 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 2 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. However, while the impact of RIGHTS on real output remains statistically

Table 3. *GMM estimation*

Estimator:	(1) GMM 2 Step FD	(2) GMM 2 Step Orth.	(3) GMM 2 Step Orth.	(4) GMM 2 Step Orth.	(5) GMM 2 Step Orth.	(6) GMM 2 Step Orth.
Dependent Variable:	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP
Constant	0.32*	0.30*	0.30*	0.10*	0.07*	0.09*
	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)
Ln(Investment)	0.46*	0.37*	0.40*	0.39*	0.39*	0.35*
	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)	(0.07)
Rights	0.01	−0.008	−0.018***	−0.027**	−0.031**	−0.034**
	(0.02)	(0.010)	(0.010)	(0.014)	(0.02)	(0.01)
StateForm	0.002*	0.002*	0.002*	0.002*	0.002*	0.001*
	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0004)
Asia	0.39*	−0.37*	−0.54*	0.15	0.001	0.05
	(0.10)	(0.10)	(0.12)	(0.13)	(0.13)	(0.12)
C&S Africa	−0.69*	−0.42*	−0.38*	−0.15***	−0.15	−0.30*
	(0.13)	(0.15)	(0.14)	(0.08)	(0.09)	(0.10)
MENA	−0.16***	−0.48*	−0.33*	0.03	0.03	−0.05
	(0.09)	(0.14)	(0.11)	(0.04)	(0.04)	(0.07)
CAmer & Carib	−0.46*	−0.24***	−0.21	0.13**	0.13*	0.14**
	(0.15)	(0.17)	(0.16)	(0.06)	(0.05)	(0.06)
SAmer	0.76*	0.53*	0.41*	−0.06	−0.02	0.03
	(0.19)	(0.11)	(0.10)	(0.08)	(0.07)	(0.10)
Britain	−0.78*	−0.65*	−0.83*	−0.30*	−0.18***	−0.36*
	(0.12)	(0.16)	(0.14)	(0.09)	(0.10)	(0.07)
% Pop. No School	−0.02**	−0.01	−0.002	−0.001	0.001	0.004
	(0.01)	(0.01)	(0.01)	(0.007)	(0.01)	(0.01)
% Pop. Prim. Compl.	0.01	0.01***	0.01*	0.03*	0.03*	0.02*
	(0.01)	(0.01)	(0.006)	(0.01)	(0.01)	(0.004)
% Pop. Second. Compl.	0.03	0.03	0.03	−0.01	−0.01	−0.02***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
% Pop. High Sch. Compl.	0.05	0.01	−0.01	0.03	0.03	0.004
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)
Avg. Years Schooling	−0.31	−0.05	0.01	0.12	0.13	0.23
	(0.24)	(0.18)	(0.06)	(0.14)	(0.13)	(0.16)
Ln(Exports)	0.41*	0.27***	0.28**	0.30*	0.30**	0.33*
	(0.13)	(0.14)	(0.14)	(0.11)	(0.15)	(0.12)
Ln(Inflation)	−0.01	−0.04	−0.02	−0.07*	−0.05*	−0.06*
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
Ln(FDI)	0.48*	0.72*	0.63*	0.48*	0.42*	0.48*
	(0.08)	(0.14)	(0.12)	(0.12)	(0.11)	(0.13)
Indiv Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes
GMM	Trans.Eq.	Trans.Eq.	Trans.Eq.	Trans.Eq.	Trans.Eq.	Trans.Eq.
	All Vars 1,2	All Vars 1,2	All RHS Vars 1,2	All RHS Vars 1,2	All RHS Vars 1,2	All RHS Vars 1,2
Dummies	Level Eq.	Level Eq.	Dep Excl	Dep, RIGHT Excl	Dep, RIGHT, INV Excl	RIGHT, INV Excl
	All Vars 2 Lev	All Vars 2 Lev				
			Level Eq.	Level Eq.	Level Eq.	Level Eq.
			All RHS Vars	All RHS Vars	All RHS Vars	All RHS Vars
			2 Lev	2 Lev	2 Lev	2 Lev
			Deo Excl.	Dep. RIGHT Excl.	Dep. RIGHT, INV Excl.	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]

Notes: Estimations reported are 2-step small sample corrections, under either first difference (FD) or orthogonal (Orth.) transformations. 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). Numbers in round parentheses denote robust standard errors, in square parentheses probability levels.

* Significance at the 1% level.

** Significance at the 5% level.

*** Significance at the 10% level.

significant, it approximately halves in economic significance, suggesting the possibility of some interaction between governance, education, and policy.[35] Nevertheless, the impact of RIGHTS on output remains nonnegligible, with a variable elasticity which declines from 0.28% to 0.04% over the RIGHTS range of 14–2. Figure 2 illustrates the revised rights elasticity over the full range of RIGHTS, providing a new lower bound (*OLS_MV*).

The presence of serial correlation in the output levels estimations (the AR(1) test statistic for (1a) through (1e) of Table 2) suggests the possible presence of unobserved effects, with the result of inconsistency in pooled OLS results.[36] We therefore proceed with Fixed Effects (FE) estimation under the within estimator. FE results are reported in column (2) of Table 2, while column (3) reports results under correction for the presence of serial correlation (FEGLS). 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). Moreover, RIGHTS loses both statistical as well as economic significance. The variable elasticity of RIGHTS ranges from 0.01 to 0.002 over the 14–2 (low to good) rights measure range, effectively appearing to eliminate its impact on output from any serious consideration—see *FE* of Figure 2—even though it maintains its benevolent sign.

The diagnostic statistics provided by AR test statistics confirm the appropriateness of correcting for unobserved effects. Moreover, the results from FE estimation suggest that concerns about possible bias and inconsistency of estimation under pooled OLS are justified. The impacts of rights 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. Where the error term is correlated with any of the explanatory variables over any two time periods,[37] FE ceases to be unbiased and consistent. The FE and the First Difference (FD) estimators generally have the same sample probability limits, except where the error term and explanatory regressors covary. Under these conditions both the FD and the FE estimators are inconsistent, and they have different *plims*. Hence, a test for the presence of correlation between error term and explanatory variables is provided by a direct comparison of the FD and FE estimators—deviation in the results obtained from the two estimators provides evidence of correlation between the error term and explanatory regressors.[38]

Table 2 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 2. The elasticity of investment falls further to approximately a third of that estimated by FEGLS in Table 2, (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.[39]

Comparison of the results obtained under the FE and FD estimators thus carries the implication that not only do we face unobserved effects, but that the exogenous variables may be contemporaneously correlated with the error term. 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.

(b) *The impact of endogeneity*

Introduction of rights into a production function context suggests that a violation of the assumption of strict exogeneity, is probable. Measurement error in the rights dimension, feedback from economic development to rights (and other dimensions), and time varying unobserved effects are all mechanisms by which strict exogeneity might come to be violated. The divergence between 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. Table 3 reports results. Estimation employs individual effects, while time dummies are included to control for exogenous shocks—Wald test statistics do not reject the inclusion of both forms of dummy variables. Wald test statistics further confirm the joint significance of the regressors, while crucially the Sargan test statistics do not reject the null hypothesis of valid instrumentation.[40]

In columns (1) and (2) of Table 3 we report results employing lagged values of all variables (including real output) as instruments (though excluding the dummy variables on the RHS), under the first difference and orthogonal transformations.[41] 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.

Under GMM estimation, 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 3, 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 3 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.

This 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 rights index value of 14, declining to 0.06 at an index value of 2. Figure 2 illustrates that these elasticity values (denoted *GMM*) lie between the upper and lower bound values obtained under pooled OLS estimation.

What emerges from this section is that accounting for possible simultaneity, carries substantial implications for estimated results. The impact of investment stabilizes, and the significance of rights which appeared to dissipate under FE estimation returns, while its economic significance is also reestablished.

The remaining concern with the GMM results is that possible country heterogeneity and the full impact of dynamics has not been explicitly addressed. Consequently we turn to time series and PMG estimation results.

(c) *Dynamics and nonlinearity*

The question of dynamics and country heterogeneity is broached in a sequence of steps.

We begin with an examination of clinical evidence that we derive from a large number of individual countries, in order to explore the plausibility and nature of the association between rights and economic performance measures. Recall the evidence about heterogeneity across countries in Table 1. Group IV (30 of the 162 countries in one of our two data sets) display first-difference stationary economic performance and stationary rights. Group V (89 countries) have first-difference stationary economic performance and rights. Group VIII 25 countries) show greater-than-first-difference stationary economic performance and first-difference stationary rights. [42]

First, we examine the three groups separately. Second, we look at our panel of 66 countries to explore the extent to which the individual country time series results generalize.

(i) *Group IV: Stationary rights, difference stationary economic dimensions*

Group IV contains many developed democracies, with high levels of rights throughout the time period. [43] The group also contains some countries from North Africa and the Middle East, South Asia, and Latin America. Whatever the level of rights in Group IV countries, rights are relatively stable over our time frame.

Given the divergent orders of integration across rights and economic performance measures, the level of rights cannot be associated with the level of output and/or investment. Instead, the feasible association is between the growth performance of countries and the level of rights. This would render all regressors stationary, allowing standard estimation techniques to be employed in principle. However, the virtually complete absence of variation in the rights dimension, precludes the use of standard estimators. To proceed for the Group IV countries we therefore 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. We report the results in Table 4.

Columns (1) through (3) of Table 4 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. Omitting the UAE as a possible outlier reduced the coefficients (see Table 4, columns 4 through 6), but they remain significant and important. [44]

We conclude by noting that within Group IV, countries with better rights do grow more rapidly (by a margin of 1.2–2% per annum), than countries with poor rights.

(ii) *Group V: Difference stationary rights, output, and investment*

Group V countries lend themselves to estimation by standard time series techniques, since all variables are first difference stationary. We employ the vector error-correction (VECM) framework. [45]

Estimation is conducted on an individual-country basis. Our concern is with the existence of cointegration between the economic performance and rights measures, hence the possibility of long-run equilibrium relationships between these variables. We test for the presence of cointegration by means of the 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.

For the majority of countries a valid cointegrating relationship *from* rights *to* real output is confirmed by trace, maximal eigenvalue, and weak exogeneity tests. [46] 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.

Table 5 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. [47]

An 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 rights and output is the same across countries (in the sense that difference stationary rights are associated with difference stationary economic performance), the strength of the impact of changes in rights varies across countries.

Moreover, the estimated elasticities are not randomly distributed across the range of rights. Consideration of the

Table 4. *Linking growth and governance in Group IV countries*

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

Notes: Numbers in round parentheses denote robust standard errors.
* Significance at the 1% level.
** Significance at the 5% level.
*** Significance at the 10% level.

Table 5. *Estimated elasticities of output w.r.t. governance in Group V countries*

Country	Implied $\varepsilon_{y,R}$							Country	Implied $\varepsilon_{y,R}$						
	Rights Index	ε_{min}	R_{min}	ε_{μ}	R_{μ}	ε_{max}	R_{max}		Rights Index	ε_{min}	R_{min}	ε_{μ}	R_{μ}	ε_{max}	R_{max}
Argentina	R	−0.01	3	−0.02	6.2	−0.04	12	Korea South	R	−0.40	4	−0.74	7.43	−1.10	11
Bahrain	R	0.60	8	0.81	10.83	0.97	13	Laos	R	−2.20	10	−2.87	13.03	−3.08	14
Bangladesh	R	−0.21	5	−0.33	7.9	−0.51	12	Lesotho	R	−0.21	7	−0.28	9.33	−0.36	12
Benin	CL	0.04	2	0.10	4.87	0.15	7	Madagascar	R	−0.01	6	−0.02	8.77	−0.02	12
Bhutan	CL	1.27	4	1.66	5.23	2.22	7	Malawi	R	−0.22	5	−0.10	10.90	−0.26	13
Bolivia	PR	0.24	1	0.74	3.03	1.70	7	Malaysia	PR	0.08	2	0.15	3.67	0.20	5
Botswana	R	0	3	0	4.57	0	7	Mali	R	0.08	4	0.21	10.33	0.28	14
Brunei	R	−2.66	11	−2.79	11.53	−3.14	13	Malta	R	−0.38	2	−0.60	3.17	−1.14	6
Burkina Faso	R	−0.18	5	−0.35	9.73	−0.47	13	Mauritania	R	−0.60	10	−0.61	12.27	−0.70	14
Cambodia	R	−0.29	8	−0.41	12.8	−0.45	14	Mauritius	R	−0.60	3	−0.80	4	−1.20	6
Cameroon	R	−1.10	10	−1.29	11.7	−1.43	13	Mexico	R	−0.45	5	−0.65	7.23	−0.72	8
Cape Verde	R	−0.06	3	−0.16	8.22	−0.26	13	Morocco	R	0	7	0	9.13	0	11
Cent. African Rep.	CL	0.84	4	1.19	5.68	1.47	7	Mozambique	PR	−0.30	3	−0.56	5.57	−0.70	7
Chile	R	0.60	3	1.59	7.93	2.40	12	Nicaragua	CL	−0.39	3	−0.55	4.20	−0.78	6
China	R	−0.11	11	−0.13	13.03	−0.14	14	Niger	PR	−0.06	3	−0.12	6.10	−0.14	7
Colombia	R	0.12	4	0.17	5.79	0.24	8	Nigeria	R	0.10	5	0.19	9.60	0.28	14
Costa Rica	CL	0.22	1	0.28	1.29	0.44	2	Oman	R	−33.77	11	−36.93	12.03	−39.91	13
Cote d' Ivoire	R	3.06	9	3.67	10.8	4.08	12	Pakistan	R	−0.06	6	−0.10	9.53	−0.12	12
Cyprus	R	Y↦R		Y↦R		Y↦R		Panama	R	−0.03	3	−0.08	8.30	−0.13	13
Dominican Republic	R	−0.15	3	−0.24	4.80	−0.35	7	Papua New Guinea	R	0.52	4	0.61	4.70	0.78	6
Egypt	R	2.4	8	3.12	10.4	3.6	12	Peru	R	∄CV		∄CV		∄CV	
El Salvador	R	−2	5	−2.61	6.53	−4	10	Phillipines	R	0	4	0	7.40	0	10
Fiji	R	−1.20	4	−1.80	6	−3.30	11	Portugal	PR	−0.17	1	−0.29	1.73	−0.85	5
Finland	R	0.20	2	0.31	3.07	0.40	4	Rwanda	R	−4.29	11	−4.79	12.27	−5.46	14
Gabon	R	−1.33	7	−2.02	10.63	−2.28	12	Singapore	R	0.64	8	0.76	9.47	0.80	10
Gambia	R	−0.03	3	−0.07	6.83	−0.13	13	South Africa	PR	0.02	1	0.08	3.87	0.12	6
Ghana	R	−0.05	5	−0.10	9.7	−0.13	13	Spain	R	0.02	2	0.04	4.30	0.11	11
Greece	R	−0.06	3	−0.09	4.27	−0.24	12	Sri Lanka	R	0.16	4	0.27	6.77	0.36	9
Guatemala	R	−0.10	2	−0.18	3.67	−0.30	6	Sudan	PR	8.20	4	12.36	6.03	14.35	7
Guinea Bissau	PR	0.30	3	0.53	5.25	0.70	7	Suriname	R	−0.15	3	−0.37	7.33	−0.65	13
Guyana	R	−0.20	4	−0.35	7	−0.50	10	Swaziland	R	0.96	6	1.68	10.47	1.76	11
Haiti	R	0.96	8	1.40	11.67	1.68	14	Syria	R	−0.99	11	−1.18	13.10	−1.26	14
Honduras	PR	0.02	5	0.03	6.48	0.07	10	Thailand	R	0.15	5	0.22	7.27	0.36	12
Hungary	R	0.42	3	1.11	7.90	1.68	12	Togo	R	1.70	10	2.02	11.90	2.21	13
India	R	−0.02	4	−0.03	5.53	−0.04	8	Trinidad	R	0.20	2	0.33	3.27	0.60	6
Indonesia	R	−0.28	7	−0.42	10.40	−0.52	13	Tunisia	R	−2.48	8	−3.29	10.60	−3.41	11
Iran	R	0.90	10	1.05	11.63	1.17	13	Turkey	R	∄CV		∄CV		∄CV	
Israel	R	−0.36	3	−0.50	4.17	−0.60	5	UAE	R	∄CV		∄CV		∄CV	
Italy	R	−0.24	2	−0.34	2.83	−0.48	4	UK	R	∄CV		∄CV		∄CV	
Jamaica	R	−0.03	3	−0.04	4.37	−0.05	5	Uruguay	R	−0.04	2	−0.13	6.27	−0.24	12
Japan	R	−1.86	2	−2.51	2.70	−3.72	4	Venezuela	R	−0.06	3	−0.09	4.33	−0.16	8
Jordan	PR	−6.39	3	−3.78	5.07	−7.56	6	Zambia	R	−0.01	5	−0.02	9.37	−0.02	11
Kenya	R	1.17	9	1.39	10.70	1.69	13	Zimbabwe	R	∄CV		∄CV		∄CV	

Notes: 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↦R that weak exogeneity tests suggest that the direction of association is from the economic to the governance dimension.

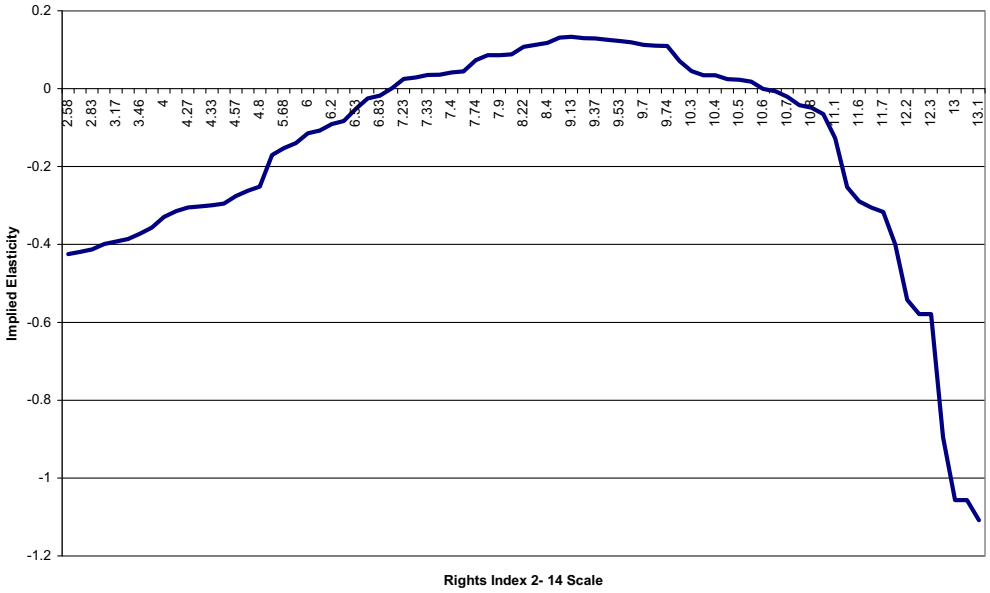

Figure 3. *Line of best fit from specification (6) of Table 7.*

relationship between the estimated elasticities reported in Table 5 for mean values of rights per country, and the range of mean rights in nonlinear specification,[48] generates the line of best fit illustrated in Figure 3. The relationship suggests the presence of an inverted U-shape.[49] Figure 3 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, approximately), 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 for which we have elasticity estimates. By contrast, for countries in the mid-level rights range from 7 to 10.7 on the Freedom House scale, improvements in rights have the perverse effect of lowering output, though the effect remains moderate (the conditional mean elasticity does not exceed 0.2).

Democratic transitions at first thus generate potentially quite dramatic growth accelerations as rights increase from a very low base. However, at mid-range rights further democratization may generate negative impacts on output, though the positive association between rights and output reemerges at even higher levels of governance. The implication of this finding is that improvements in rights, while accelerating economic development under very autocratic dispensations, eventually leads to a point where the continuation of rights reforms leads to such fundamental change, that the resultant uncertainty acts as a deterrent to economic activity. However, provided that political reform continues, once a new institutional dispensation is settled upon, the positive impact on economic output reemerges.

The import of the analysis of Group V countries is threefold. First the evidence for Group V countries confirms that they differ from countries in Group IV. There is a directly estimable relationship between rights and output in a sense that was not feasible for Group IV. Second, Group V countries are themselves not homogeneous. They differ strongly in the strength of the elasticity we find between output and rights. 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 nonlinearities in the association between rights and real output. Country-specific estimated elasticities suggest an inverted U-shape over the rights range.

(iii) *Group VIII: Difference stationary rights, output integrated of higher order*

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 rights and output cannot be associated with one another in levels.

What is distinctive about the countries that feature in Group VIII is that many of them have experienced dramatic changes in both economic performance and in their rights structure. This is typified by the appearance of East European states in Group VIII. 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, *viz.* 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. The second largest grouping liberalizes rights, but with positive consequences for real output per capita—for example El Salvador. The third grouping, shows no systematic pattern at all.

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

$$d\ln Y_t = \beta_0 + \beta_1 dR_t + \sum_{i=1}^{k} \gamma_i D_i + \varepsilon_t \tag{10}$$

where the D_i denote a series of deterministic variables, by means of the ARDL approach to cointegration.[50] Strictly, the specification remains unbalanced (since $d\ln Y_t$ and dR_t have

Table 6. *Impact of changing rights on output growth in Group VIII countries*

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

ECM denotes the error correction term.
* Significance at the 1% level.
** Significance at the 5% level.
*** Significance at the 10% level.

different orders of integration) 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. [51] Estimation proceeds on a country by country basis. In Table 6 we report the estimation results.

Results confirm the existence of the three classes of countries noted above.

For a number of countries, improvements in rights (such that $dR < 0$ on the Freedom House scale) results in a negative growth impact. [52] In the second group of countries, improvements in rights are associated with an increase in output growth. [53] The remaining countries, [54] appear to show no

systematic association between growth and changes in rights at all.

The implication of this evidence appears to be that transitions from autocratic regime status to improved levels of democracy may be associated with negative shocks to output.[55] Such a negative shock does not necessarily emerge (some countries do experience growth accelerations), and even where it does occur, it may come to be reversed in the sense that output levels recover. However, unfortunately the evidence suggests that the risk factors are likely to outweigh the potential positive pay-offs from rights liberalizations. The evidence suggests that negative growth shocks of rights transitions easily dominate any benevolent growth acceleration (recall that the rights scale is inverted—and among estimated elasticities the positive semi elasticities readily dominate the negative in absolute terms, suggesting a perverse association between rights and output). 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).

Group VIII countries thus appear to face the possibility of substantial negative impacts on output in any attempt to reform their governance—at least in the first instance—even if reforming from very poor rights (in the 11–14 range).[56] This is in contrast to Group V countries, for which the developmental impact of reform was particularly strong under very poor rights, and any negative impact on per capita output emerged only at mid-level rights.

(iv) *Generalizing to panel data*

The time series evidence we have considered suffers 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 30 years data on a range of rights measures—but 30 years still constitutes a short time run, especially when considering the impact of rights 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 nonlinearities postulated under our examination of Group V countries above. In short, while the evidence summarized by Figure 3 is suggestive of a general form of association between rights and economic development, the time series evidence derived on a country-by-country basis 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

Table 7. *Pooled mean group estimations*

	(1)	(2)	(3)	(4a)	(4b)	(4c)
	Full Sample	Full Sample	Full Sample	Rights < 7	7 < Rights < 11	Rights > 11
Estimator:	PMGE	PMGE	PMGE	PMGE	PMGE	PMGE
Dep Variable:	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP	lnRGDP
Info. Crit.:	AIC(3)	ARDL	ARDL	HQ(1)	ARDL	ARDL
		(1,3,3,3)	(2,2,2,2,1,1,1)		(1,0,0,0,0,0,0)	(1,0,0,0,0)
Ln(Investment)	0.35*	0.31*	0.27*	0.42*	0.45*	0.29*
	(0.01)	(0.004)	(0.01)	(0.02)	(0.02)	(0.03)
Rights	−0.02*	−0.01*	−0.01*	−0.004*	0.003	−0.02***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)	(0.01)
Avg. Years		0.09*	0.09*	0.04*	0.04*	0.04*
Schooling		(0.003)	(0.01)	(0.01)	(0.01)	(0.01)
Ln(Exports)			0.25*	0.23*	0.06*	0.20*
			(0.03)	(0.02)	(0.02)	(0.04)
Ln(FDI)			−0.02*	−0.12	−0.13	0.07
			(0.01)	(0.08)	(0.09)	(0.18)
Ln(Inflation)			−0.05*	−0.02*	−0.06*	0.02
			(0.01)	(0.01)	(0.02)	(0.02)
ϕ	−0.11*	−0.16*	−0.12*	−0.38*	−0.20*	−0.31*
	(0.02)	(0.03)	(0.03)	(0.10)	(0.03)	(0.07)
h-test	1.01	5.44	0.57[0.45]	0.93[0.33]	1.41[0.24]	1.13[0.29]
	[0.60]	[0.14]	1.00[0.32]	1.23[0.27]	1.48[0.22]	1.04[0.31]
			2.29[0.13]	0.45[0.50]	0.00[0.99]	1.04[0.31]
			4.16[0.04]	0.10[0.75]	0.09[0.76]	1.24[0.27]
			0.09[0.76]	0.81[0.37]	2.63[0.10]	0.17[0.68]
			0.59[0.44]	0.35[0.55]	1.39[0.24]	1.04[0.31]
LR	622.74	845.14	1369.80	636.03	416.01	162.64
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
RLL	4058.88	3698.63	3002.70	1043.98	1149.51	383.23
ULL	4370.25	4121.20	3687.60	1361.99	1357.51	464.54
Constant	0.67*	0.92*	0.66*	2.11*	1.22*	1.48*
	(0.14)	(0.17)	(0.14)	(0.55)	(0.18)	(0.29)

PMGE denotes pooled mean group estimators; 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. AIC and HQ denote the Akaike and Hannan–Quinn information criteria respectively.

* Significance at the 1% level.

** Significance at the 10% level.

*** Significance at the 10% level.

included in the panel, and the possibility of nonlinearities that may be present in the association between rights and output. We are thereby able to also control for a greater set of dimensions than is feasible in the country time series estimations.

Estimation is by means of the PMG estimator.[57] 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. The solution to the difference equation for each country would in general imply quite distinct steady states. We test for the presence of long run homogeneity by means of a Hausman test. In estimation, we impose a maximum lag length of 3 (on annual data),[58] and choose the lag length for each individual country in the panel by means of an information criterion.

We estimate (9) under the inclusion of a single human capital measure, given by average years of schooling,[59] and three measures that proxy for the quality of the policy environment, given by Foreign Direct Investment (FDI), exports, and inflationary pressure (as measured by the consumer price index).

Results are reported in Table 7.

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.

The elasticity of output with respect to investment is reported at approximately 0.35, while the impact of rights is found to lie in the 0.28–0.04 elasticity range-see column (1) of Table 7. Introduction of the human capital measure and the three policy variables does lower the estimated impact of both investment and rights (see columns (2) and (3) of Table 7, with investment elasticity of approximately 0.30, and an 0.14–0.02 elasticity range for rights).

Given the time series evidence of Section 3(c)(ii), in favor of a potential nonlinearity in the association between rights and output, we also estimated (9) for three distinct country groupings. These groups are given by countries in our panel which experienced poor rights (RIGHT $>$ 11) in our sample period, countries with mid-level rights (7 $<$ RIGHT $<$ 11), and countries with good rights (RIGHT $<$ 7). The values of the RIGHTS which distinguish between the three country classes are thus those identified empirically under Section 3(c)(ii). Results are reported in columns (4a), (4b), and (4c) of Table 7.

Salient differences between the different country groupings 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 rights 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 0.28–0.22 to 0.03–0.01, between the poor and good rights country grouping (specified only over the rights range that applies to the two groups of countries). 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. The implied elasticity ranges are reported in Figure 2 as *PMGE*.

These findings are thus consistent with those obtained under the time series methodologies employed for Group V countries. The association between rights and output follows an inverted U-shape, with the strongest impact of rights improvements reserved for circumstances with the worst rights, and a perverse association for mid-range rights countries.

4. CONCLUSIONS AND EVALUATION

Political rights and civil liberties affect long-term economic development in a complicated way. In this conclusion, we try to summarize some lessons for theory, for empirical work, and for policy.

Theory seeks generality. But one of our findings is that countries differ in terms of the impact of differences in political rights and civil liberties. It is not just that some countries are more free and some less. Rather, the point is that improvements along an (admittedly ordinal) scale of rights have differing effects on growth. Theorists are invited to speculate why an improvement from a very low to a low level of rights is associated with a larger increase in GDP per capita than an improvement from a middling level of rights to a pretty good level, and why the impact of rights at mid-levels may perhaps generate a negative impact on output. We believe the same point may hold for empirical analyses of the interactions among institutions, political regimes, social settings, and economic outcomes.

In empirical work, assuming homogeneity across countries and grinding out a regression coefficient for improvements in rights overlooks this variation and is therefore at risk for error. Fortunately, as we have seen, alternative techniques help us take heterogeneity into account. Beyond heterogeneity, we have seen how other features of our problem help us adjust to reality. We have seen how four other methodological challenges may affect our results: unobserved variables apparently are having an effect; exogeneity assumptions are not satisfied; relationships among variables are nonlinear; dynamic effects matter.

What our results demonstrate concretely is that the modeling choices matter profoundly for the purpose of inference. Estimation which ignores the impact of unobserved variables substantially biases upward the estimated impact of rights on output. Controlling for some omitted variables in estimation reduces the estimated impact (it approximately halves). Estimation under panel techniques that allow for time invariant unobserved variables, but which do not allow for the possibility that rights may be endogenous effectively find no impact of rights on output. Two estimation techniques that do allow for endogeneity (GMM, PMGE) restore the impact of rights in an intermediate range and with the implication that across the full range of observed rights values the association with output may be nonlinear. Since econometric theory implies that estimation without accounting for unobserved effects may result in upward bias in estimation, that by contrast accounting for fixed effects without correcting for endogeneity generates downward bias, our reported findings accord with prior expectations.

The range of elasticities found for the impact of rights on GDP per capita was found to be 0.28–0.22 for countries with poor rights (below 11 on the Freedom House scale), declining to 0.03–0.01 for countries with good rights (below 7 on the Freedom House scale)—and there may be a perverse (negative) though economically small impact of rights improvements at mid-range rights (11–7 on the Freedom House scale). These estimates confirm the importance of distinguishing between countries with very poor rights, and countries

with mid-range rights and good rights. 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 supports the existence of an underlying structure governing the changes in the interaction between rights and output across rights categories. The implication of this finding is that improvements in rights, while accelerating economic development under very autocratic dispensations, eventually leads to a point where the continuation of rights reforms leads to such fundamental change, that the resultant uncertainty acts as a deterrent to economic activity. However, provided that political reform continues, once a new institutional dispensation is settled upon, the positive impact on economic output reemerges.

A significant implication of this paper is thus that estimation methodology really matters, and that it is useful in interpreting results to be fully transparent about the sensitivity of results to the assumptions being made concerning error structure. Statistical techniques enable us to identify the challenges we specified and to devise responses to them. In our case, we showed how dynamic heterogeneous panel estimation techniques (and PMG estimation in particular) enable us to put countries together in a way that allows short-term heterogeneity but still allows us to estimate any presumed long-term, homogeneous relationships between rights and growth. In the process, we also took account of the presence of possible nonlinearities, and to be sensitive to the possibility of endogeneity of regressors.

The findings also reemphasize the continued importance of advances in theory to improve the accuracy of specifications employed in empirical work. In the search for valid instruments, as well as the long run relationships to be embedded in PMGE-type specifications, it continues to be critical.

And these results are also worth exploring in policy circles. Many empirical studies of development document regularities and stop there. In fact, figuring out exactly what is happening "empirically" and what it means "theoretically" may be advanced via discussions among politicians, business people, civil society organizations, and international agencies. "In your experience in your country, have changes in political rights and civil liberties gone along with more effective and potent investment? With improvements in human capital, exports, FDI, and inflation? Can you give examples? How does it happen, exactly? What policy decisions have led to the changes in political rights and civil liberties? Given where we are now, how might things be improved?"

We find that rights matter for GDP, and differentially across groups of countries. Our results suggest some ways that better rights may change other things that matter. The results of cross-county analyses like ours, seemingly far away from ground-level decisions in a particular country at a particular time, may in fact provide a valuable incitement for locals to think even harder about what to do.

NOTES

1. See for instance North and Thomas (1973).

2. See for instance North (1981, 1990, 1991, 2005). It is this more general view of institutions that has gained currency in economics—see the discussion in Acemoglu *et al.* (2005) for instance.

3. See Fukuyama (1992, 1995).

4. See La Porta *et al.* (1998, 1999).

5. See Putnam (1993).

6. See for instance Hall and Jones (1999), in which institutions account for a ratio of 1:25 out of the total disparity of 1:32 in income differentials between the richest and the poorest country in their sample.

7. See for instance Rodrick, Subramanian, and Trebbi, 2004, in which institutions are found to be more important than either openness or geography. The significance of openness has been argued for both empirically (see Sachs & Warner, 1995), as well as in terms of an endogenous growth framework (see Aghion & Howitt, 2009; Aghion, Fedderke, Howitt, & Viegi, 2008). The impact and significance of geography have been examined by Bloom and Sachs (1998) and Gallup, Sachs, and Mellinger, 1999.

8. The seminal contribution is Lipset (1959).

9. See Murray (2006).

10. See Brock and Durlauf (2001), Dollar and Kraay (2003), Glaeser *et al.* (2004), Durlauf *et al.* (2005), Kraay (2008), Hauk and Wacziarg (2009), and Bazzi and Clemens (2013).

11. Acemoglu *et al.* (2008, pp. 836–837) hint at this possibility when they call for more research on possible interaction effects that make income and democracy have different effects in different places.

12. In this we are guided by exemplars from the natural sciences, and physics in particular. Examples here include the development of superconductors from "shake-and-bake" chemistry, rather than theoretical first principles. From physics, a relevant example is Lamb and Retherford's (1947) Shelter Island conference presentation on the fine structure of the hydrogen spectrum, which stood in disagreement with the 1928 Dirac Equation. The result was the Hans Bethe theoretical and computational response to account for the Lamb–Retherford empirical results, which was instrumental to Feynman, Schwinger, and Tonomaga (and Dyson) in formulating the modern theory of quantum electro dynamics more completely. Lamb and Retherford were awarded the 1958 Nobel in Physics, Feynman, Schwinger and Tonomaga that of 1965, and Bethe that of 1967. Our point is that scientific insight is not simply the product of testing theory against data. Data also can and should speak to theory, and lead theorists to look in new directions. In the natural sciences the requirement is careful experiment. In the social sciences, absent controlled experimentation, the recourse is careful attention to the estimation technique.

13. In Fedderke and Klitgaard (1998) we noted that when considering a set of 67 measures of social dimensions of countries, these measures stood in strong and multiple forms of statistical association with one another. This suggests both that multiple social dimensions matter for development, and that any partial specification is difficult to interpret both in terms of causality and in terms of precision.

14. Random Effects (RE) is an alternative provided that $cov(C_i,K_{it}) = cov(C_i,R_{it}) = 0$. If this condition is met, RE would be more efficient than FE; but where the condition is violated, RE would provide biased and inconsistent results. The literature has identified a

number of variables that have been linked to both output and to governance. The extent of ethnolinguistic fractionalization, 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. See for instance Barro (1991) on political instability, and on colonial background see La Porta *et al.* (1998, 1999), Véliz (1994), North *et al.* (2000) and Wiarda (2001). See also the discussion in Acemoglu *et al.* (2005). Thus $cov(C_i,R_{it}) \neq 0$ is indicated, rendering RE inappropriate.

15. Such that $cov(R_{is},\varepsilon_{it}) = 0 = cov(K_{is},\varepsilon_{it})$, $s,t = 1, \ldots T$.

16. The discussion in Durlauf *et al.* (2005, p. 637) provides a range of additional references and instrumentation strategies.

17. See also Durlauf *et al.* (2005, 635).

18. Arellano and Bond (1991) suggested using the entire set of instruments. Practical considerations may preclude this option. Large numbers of instruments 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).

19. See Alvarez and Arellano (2003) and Blundell and Bond (1998).

20. Note that there is no necessity for a lagged dependent variable in the specification-in which case instrumentation is indicated over lags defined over $t = 2, \ldots, T$.

21. See Sargan (1958) and Hansen (1982).

22. This allows (7) to be written in error correction form, with ϕ_i providing the error correction coefficient measuring the speed of adjustment toward the long-run equilibrium.

23. A Hausman (1978) test (hereafter h test) on the difference between Mean Group and PMG estimates of long-run coefficients to test for long-run heterogeneity provides an empirical test for such homogeneity.

24. For instance, the relevance of rights for development has been advanced at least narrowly with respect to property rights (see North and Thomas, 1973), broadly to the incentives that are associated with the rules of interaction imposed by institutional dispensations (see North, 1990, 1991, 2005), with respect to neo-liberal political dispensations favorable to economic development (see Fukuyama, 1992, 1995), in relation to the formal legal structures adopted by societies (see La Porta *et al.*, 1998, 1999), and with respect to the informal social capital that generates trust (see Putnam, 1993).

25. See for instance the discusion in Holmberg *et al.* (2009), which suggests that no single set of variables has been agreed upon as complete and adequate as the determinants of growth. In addition, the measures used in empirical work have been criticized on the grounds that they effectively measure the *outcomes* that theory suggests follow from institutional dispensations (such as rights), rather than the inputs specified by theory. As such, much of the empirical evidence advanced in support of the impact of institutions (rights), is deemed misspecified. See particularly Glaeser *et al.* (2004).

26. A nonexhaustive list includes the POLITY measures of political institutions, the Freedom House measures of political rights and civil liberties, freedom from expropriation, freedom of the print, and/or broadcast media, freedom of movement, freedom of property, freedom from the military draft, freedom of the foreign exchange regime, freedom of information, freedom of internal, and/or foreign travel, freedom of peaceful assembly, freedom from work permits, freedom from search without a warrant, and freedom from arbitrary seizure of property. See for instance Scully (1988) and Acemoglu *et al.* (2005).

27. For a few countries 1 or 2 years' observations at the start of the T dimension are missing.

28. Examination of the CIV_LIB and POL_RIGHT rights measures, suggests the existence of a close statistical association between them. Estimation results for our 66 country panel suggests that 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—regardless of whether we estimate under pooled OLS, correct for country Fixed Effects (FE) and serial correlation (FEGLS), or allow for dynamics by means of PMG estimation. However, there is some evidence of a nonlinearity in the association, since the association between the two rights measures is weakest under poor governance (a rights score under a linear combination of the two rights measures above 11), strongest in the mid range of governance (a rights score above 7 but below 11), and closest to a proportional association for countries with good governance (a rights score under a linear combination of the two rights measures below 7).

29. We tested for stationarity on a variable by variable, country by country basis, by means of standard Augmented Dickey Fuller (ADF) test statistics. Use of any of the large number of alternative statistics for data stationarity, does not affect these results. Ideally this might be repeated for all governance variables—but given the limited time runs over which the majority of measures are available, this is constrained by data unavailability. Further research that exploits the univariate time series character of the data is a fruitful line of further research, as data availability grows.

30. Note that some countries are classified in more than one cell of Table 2. 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 orders of integration.

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

32. The Barro-Lee data set contains data points 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.

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

34. 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.

35. In earlier work we reported the pervasiveness and strength of "webs of association" between social characteristics of countries—see Fedderke and Klitgaard (1998). A related literature has pointed to the impact of the quality of institutions on the efficacy of economic policy making—see for instance Burnside and Dollar (2000, 2004) and Easterly *et al.* (2004). We explored this in our data throughout the specifications reported in this paper, and found some evidence that supported the benevolent link between policy effectiveness and governance. Our core findings remain unaffected.

36. Suppose:

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

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. See Wooldridge (2002, p. 264).

37. Reasons for such a correlation would include time varying unobserved effects, measurement error, and endogeneity.

38. See the discussion in Wooldridge (2002, p. 284f).

39. 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.

40. We employ the 2 step estimation procedure for small sample correction. See Windmeijer (2000). Note that we employ both levels and the standard first difference 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 χ^2-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. While we estimated under both the First Difference (FD) and orthogonal deviation (Orth) transformation of individual effects, with one exception we report the results based on orthogonal deviation. This reflects both the broad consistency of the results obtained from the Orth-transform, and the marginally better stability of the Orth-based results. Orthogonal deviations express each observation as the deviation from the average of future observations in the sample for the same country, and weigh each deviation in order to standardize the variance. See Arellano and Bover (1995).

41. Here too we examined evidence from more parsimonious specifications, excluding the geography, policy, and human capital variables. No reported results are materially affected by such exclusions.

42. Only a few countries do not fall into Groups IV, V, and VIII, and we comment only briefly on these. 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. 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. Second, for a number of the other groupings little evidence of an association between governance and economic development emerges. Third, where a relationship between governance and development does emerge, it is generally weak.

43. 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.

44. 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%. 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. 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.

45. See Johansen (1988), Johansen and Juselius (1990, 1992), Pesaran and Shin (1995a, 1995b), Pesaran *et al.* (1996), and Wickens (1996).

46. We test for weak exogeneity by means of the methodology proposed by Pesaran *et al.* (1996, 2001). In one instance (Cyprus) weak exogeneity tests suggest that causality runs from output to rights, and in five instances there is no cointegration (Peru, Turkey, UAE, UK, Zimbabwe).

47. 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 5 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. In a few instances the presence of a bivariate specification, or clear indication of a single cointegrating relationship, we were able to estimate the relationship by means of an ARDL cointegrating framework—see Pesaran *et al.* (2001). Full estimation results are available in the long working paper version of the paper—see Fedderke *et al.* (2011).

48. We experimented with polynomials to the fourth order. While goodness of fit measures indicates high degrees 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. Figure 3 illustrates the fourth order polynomial that generates the best fit. Note that we excluded two outlier cases given by Sudan and Oman, with their strong elasticities.

49. An earlier indication of such a possibility was advanced by Barro (1997, chap. 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. In one of the few other studies to report nonlinearities in governance variables, Eicher *et al.* (2009) generate a nonlinearity between education and corruption. The suggestion is that improvements in education generate enough rents to incentivize corruption, but insufficient monitoring.

50. See Pesaran *et al.* (2001).

51. Moreover, the ARDL approach to cointegration estimation has reduced sensitivity to mismatched orders of integration across variables.

52. 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 be recovering in the latter half of the 1990s after their rights transition. For almost all countries in this category, the rights transition occurred post 1989.

53. Djibouti, El Salvador, Kuwait after 1992, Guyana, Nigeria (w.r.t. to investment, not output), Poland and Tanzania all fall into this 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.

54. Equatorial Guinea (where oil discoveries led to strong increases in output—but rights remained essentially unchanged), Paraguay, Sierra Leone, Tajikistan, Thailand, Trinidad, Vietnam, and Yemen.

55. Note that this finding is consistent with the nonlinearity we reported for Group V countries.

56. It is not clear why this should be the case. The fact that many of the countries reformed from socialist economic systems suggests the possibility that it is difficult to restructure incentive systems from those of planned economies to market oriented incentive structures. But here we can only speculate, and leave this issue to future work.

57. See Pesaran *et al.* (1999) and the exposition in Section 1(a).

58. 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.

59. The reason for the restriction to a single measure of human capital is given by the strength of the association among the various human capital measures. In order to avoid both problems of multicollinearity and of excessive loss of degrees of freedom under data hungry dynamic estimation techniques, we henceforth reply only on the average years of schooling measure.

REFERENCES

Acemoglu, D., Johnson, S., & Robinson, J. (2001). The colonial origins of comparative development: An empirical investigation. *American Economic Review, 91*(5), 1369–1401.

Acemoglu, D., Johnson, S., & Robinson, J. (2005). Institutions as a fundamental cause of long-run growth. In P. Aghion, & S. N. Durlauf (Eds.), *Handbook of economic growth* (pp. 385–472). North-Holland.

Acemoglu, D., Johnson, S., & Robinson, J. (2012). The colonial origins of comparative development: An empirical investigation: Reply. *American Economic Review, 102*(6), 3077–3110.

Acemoglu, D., Johnson, S., Robinson, J. A., & Yared, P. (2008). Income and democracy. *American Economic Review, 98*(3), 808–842.

Aghion, P., Alesina, A., & Trebbi, F. (2006). Democracy, technology and growth. NBER Working Paper No. 13180.

Aghion, P., & Howitt, P. (2009). *The economics of growth*. Cambridge: MIT Press.

Aghion, P., Fedderke, J. W., Howitt, P., & Viegi, N. (2008). Testing creative destruction in an opening economy: the case of the south African manufacturing industries. Economic Research Southern Africa Working Paper No. 93, forthcoming Economics of Transition.

Albouy, D. Y. (2012). The colonial origins of comparative development: An empirical investigation: Comment. *American Economic Review, 106*(6), 3059–3076.

Altonji, J. G., & Segal, L. M. (1996). Small-sample bias in GMM estimation of covariance structures. *Journal of Business and Economic Statistics, 14*, 353–366.

Alvarez, J., & Arellano, M. (2003). The time series and cross section asymptotics of dynamic panel data estimators. *Econometrica, 71*, 1121–1159.

Arellano, M., & Bond, S. R. (1991). Some specification tests for panel data: Monte Carlo evidence and an application to employment equations. *Review of Economic Studies, 58*, 277–298.

Arellano, M., & Bover, O. (1995). Another look at the instrumental variances estimation of error-components models. *Journal of Econometrics, 68*, 29–51.

Barro, R. J. (1991). Economic growth in a cross section of countries. *Quarterly Journal of Economics, 106*, 407–443.

Barro, R. J. (1997). *Determinants of economic growth: A cross-country empirical study*. Cambridge, MA: MIT Press.

Barro, R. J., & Lee, J. -W. (1993). International comparisons of educational attainment. *Journal of Monetary Economics, 32*, 363–394.

Bazzi, S., & Clemens, M. A. (2013). Blunt instruments: Avoiding common pitfalls in identifying the causes of economic growth. *American Economic Journal: Macroeconomics, 5*(2), 152–186.

Bloom, D. E., & Sachs, J. D. (1998). Geography, demography and economic growth in Africa. *Brookings Papers on Economic Activity, 2*, 207–295.

Blundell, R., & Bond, S. R. (1998). Initial conditions and moment restrictions in dynamic panel data models. *Journal of Econometrics, 87*, 115–144.

Bond, S., Hoeffler, A., & Temple, J. (2001). GMM estimation of empirical growth models. Centre for Economic Policy Research Discussion Paper No. 3048.

Brock, W. A., & Durlauf, S. N. (2001). Growth empirics and reality. *World Bank Economic Review, 15*(2), 229–272.

Burnside, C., & Dollar, D. (2000). Aid, policies and growth. *American Economic Review, 90*(4), 847–868.

Burnside, C., & Dollar, D. (2004). Aid, policies and growth: Reply. *American Economic Review, 94*(3), 781–784.

Caselli, F., Esquivel, G., & Lefort, F. (1996). Reopening the convergence debate: A new look at cross-country growth empirics. *Journal of Economic Growth, 1*(3), 363–389.

Dollar, D., & Kraay, A. (2003). Institutions, trade and growth. *Journal of Monetary Economics, 50*(1), 133–162.

Durlauf, S. N., Johnson, P. A., & Temple, J. R. W. (2005). Growth econometrics. In P. Aghion, & S. N. Durlauf (Eds.), *Handbook of economic growth* (pp. 555–678). North-Holland.

Easterly, W., Levine, R., & Roodman, D. (2004). Aid, policies, and growth: Comment. *American Economic Review, 94*(3), 774–780.

Eicher, T., Garcia-Penalosa, C., & Van Ypersele, T. (2009). Education, corruption and the distribution of income. *Journal of Economic Growth, 14*, 205–231.

Fedderke, J. W., & Klitgaard, R. E. (1998). Growth and social indicators: An exploratory analysis. *Economic Development and Cultural Change, 46*(3), 455–490.

Fedderke, J. W., Klitgaard, R. E., & Akramov, K. (2011). Heterogeneity happens: How rights matter in economic development. Economic Research Southern Africa Working Paper No. 220.

Fukuyama, F. (1992). *The end of history and the last man*. New York: Free Press.

Fukuyama, F. (1995). *Trust: The social virtues and the creation of prosperity*. London: Hamish Hamilton.

Gallup, J. L., Sachs, J. D., & Mellinger, A. D. (1999). Geography and economic development. *International Regional Science Review, 22*(2), 179–232.

Glaeser, E. L., La Porta, R., Lopez-de-Silanes, F., & Shleifer, A. (2004). Do institutions cause growth?. *Journal of Economic Growth, 9*(3), 271–303.

Gundlach, E., & Paldam, M. (2008). Income and democracy: A comment on Acemoglu, Johnson, Robinson, and Yared (2008). Kiel Working Paper No. 1458, October 2008.

Hall, R. E., & Jones, C. I. (1999). Why do some countries produce so much more output per worker than others?. *Quarterly Journal of Economics, 114*(1), 83–116.

Hansen, L. P. (1982). Large sample properties of generalized method of moments estimators. *Econometrica, 50*, 1029–1054.

Hauk, W. R., Jr., & Wacziarg, R. (2009). A Monte Carlo study of growth regressions. *Journal of Economic Growth, 14*(2), 103–147.

Hausman, J. A. (1978). Specification tests in economics. *Econometrica, 46*, 1251–1270.

Heckman, J. J., & Hotz, V. J. (1989). Choosing among alternative nonexperimental methods for estimating the impact of social programs: The case of manpower training. *Journal of the American Statistical Association, 84*, 862–875.

Heid, B., Langer, J., & Larch, M. (2011). Income and democracy: Evidence from system GMM estimates, Ifo Working Paper No. 118, December 2011.

Holmberg, S., Rothstein, B., & Nasiritousi, N. (2009). Quality of government: What you get. *Annual Review of Political Science, 12*, 135–161.

Johansen, S. (1988). Statistical analysis of cointegrating vectors. *Journal of Economic Dynamics and Control, 12*, 551–570.
Johansen, S., & Juselius, K. (1990). Maximum likelihood estimation and inference on cointegration—With applications to the demand for money. *Oxford Bulletin of Economics and Statistics, 52*(2), 169–210.
Johansen, S., & Juselius, K. (1992). Testing structural hypotheses in a multivariate cointegrating analysis of the PPP and the UIP for UK. *Journal of Econometrics, 53*, 211–244.
Klitgaard, R., & Fedderke, J. W. (1995). Social integration and disintegration: An exploratory analysis of cross-country data. *World Development, 23*(3), 357–369.
Kraay, A. (2008). Instrumental variables regressions with honestly uncertain exclusion restrictions. Policy Research Working Paper Series 4632, World Bank.
Lamb, W. E., & Retherford, R. C. (1947). Fine structure of the hydrogen atom by a microwave method. *Physical Review, 72*(3), 241–243.
La Porta, R., Lopez de Silanes, F., Shleifer, A., & Vishny, R. (1998). Law and finance. *Journal of Political Economy, 106*(6), 1113–1155.
La Porta, R., Lopez de Silanes, F., Shleifer, A., & Vishny, R. (1999). The quality of government. *Journal of Law, Economics and Organization, 15*(1), 222–279.
Leblang, D. A. (1996). Property rights, democracy and economic growth. *Political Research Quarterly, 49*(1), 5–26.
Levine, R., Loayza, N., & Beck, T. (2000). Financial intermediation and growth: Causality and causes. *Journal of Monetary Economics, 46*(1), 31–77.
Lipset, S. M. (1959). Some social requisites of democracy: Economic development and political legitimacy. *American Political Science Review, 53*, 69–105.
Murray, M. P. (2006). Avoiding invalid instruments and coping with weak instruments. *Journal of Economic Perspectives, 20*(4), 111–132.
North, D. C. (1981). *Structure and change in economic history*. New York: Norton.
North, D. C. (1990). *Institutions, institutional change and economic performance*. New York: Cambridge University Press.
North, D. C. (2005). *Understanding the process of economic change*. Princeton: University Press.
North, D. C. (1991). Institutions. *Journal of Economic Perspectives, 5*, 97–112.
North, D. C., Summerhill, W., & Weingast, B. R. (2000). Order, disorder, and economic change: Latin America versus North America. In B. Bueno de Mesquita, & H. L. Root (Eds.), *Governing for prosperity*. New Haven: Yale University Press.
North, D., & Thomas, R. (1973). *The rise of the western world: A new economic history*. New York: Cambridge University Press.
Paxton, P. (2002). Social capital and democracy: An interdependent relationship. *American Sociological Review, 67*(2), 254–277.
Pesaran, M. H., & Shin, Y. (1995a). Long run structural modeling. Unpublished manuscript, University of Cambridge.
Pesaran, M. H., & Shin, Y. (1995b). An autoregressive distributed lag modeling approach to cointegration analysis. DAE Working Paper No. 9514, Department of Applied Economics, University of Cambridge.
Pesaran, M. H., Shin, Y., & Smith, R. J. (1996). Testing for the existence of a long run relationship. DAE Working Paper No. 9622, Department of Applied Economics, University of Cambridge.
Pesaran, M. H., Shin, Y., & Smith, R. P. (1999). Mean group estimation of dynamic heterogenous panels. *Journal of the American Statistical Association, 94*, 621–634.
Pesaran, M. H., Shin, Y., & Smith, R. (2001). Bounds testing approaches to the analysis to the testing of level relationships. *Journal of Applied Econometrics, 16*, 289–326.
Pesaran, M. H., & Smith, R. P. (1995). Estimating long-run relationships from dynamic heterogeneous panels. *Journal of Econometrics, 68*, 79–113.
Putnam, R. D. (1993). *Making democracy work: Civic institutions in modern Italy*. Princeton: University Press.
Robertson, D., & Symons, J. (1992). Some strange properties of panel data estimators. *Journal of Applied Econometrics, 7*, 175–189.
Rodrick, D., Subramanian, A., & Trebbi, F. (2004). Institutions rule: The primacy of institutions over geography and integration in economic development. *Journal of Economic Growth, 9*, 131–165.
Sachs, J. D., & Warner, A. (1995). Economic reform and the process of global integration. Brookings Papers on Economic Activity, The Brookings Institution.
Sargan, J. D. (1958). The estimation of economic relationships using instrumental variables. *Econometrica, 26*, 393–415.
Scully, G. W. (1988). *Constitutional environments and economic growth*. Princeton: University Press.
Tauchen, G. (1986). Statistical properties of generalized method-of-moments estimators of structural parameters obtained from financial market data. *Journal of Business and Economic Statistics, 4*, 397–416.
Véliz, C. (1994). *The new world and the gothic fox: Culture and economy in English and Spanish America*. Berkeley: University of California Press.
Wiarda, H. J. (2001). *The soul of Latin America: The cultural and political tradition*. New Haven: Yale University Press.
Wickens, M. R. (1996). Interpreting cointegrating vectors and common stochastic trends. *Journal of Econometrics, 74*, 255–271.
Windmeijer, F. (2000). A finite sample correction for the variance of linear two-step GMM estimators. IFS Working Paper W00/19. London: The Institute for Fiscal Studies. Available from: http://www.ifs.org.uk.
Wooldridge, J. M. (2002). *Econometric analysis of cross section and panel data*. Cambridge, MA: MIT Press.
Ziliak, J. P. (1997). Efficient estimation with panel data when instruments are predetermined: An empirical comparison of moment-condition estimators. *Journal of Business and Economic Statistics, 15*, 419–431.