Institutional Foundations of Inequality and Growth

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Lewis Davis
Union College
davisl@union.edu

and

Mark Hopkins
Gettysburg College
mhopkins@gettysburg.edu

Abstract:

We investigate the impact of political and economic institutions on income inequality, and find strong evidence that property rights lead to lower levels of inequality. In light of this new evidence, we call into question previous claims regarding the effect of inequality on growth that have treated inequality as exogenous, failing to account for the central role of institutional variables. We find that the omission of property rights explains the negative relationship between growth and inequality commonly found in cross-country growth regressions. Turning to evidence based on fixed effects estimates in panel data, we show that the inclusion of economic institutions allows for unbiased estimation using a more efficient GLS estimator. While we confirm previous evidence of a positive short-run relationship between inequality and economic growth within countries, the random effects model does not support the position that the short run and long run relationships are the same, suggesting that the short run relationship may be driven by business cycle dynamics.

Key Words: Growth, Inequality, Property Rights, Democracy, Institutions

Section 1: Introduction

After more than a decade of empirical work, there is little consensus about the impact of income inequality on economic growth. Early investigations by Alesina and Rodrik (1994) and Persson and Tabellini (1994) using cross-country growth regressions concluded that initial income inequality is associated negatively with future growth. Later researchers argued that these estimates may be subject to bias from omitted variables. Li and Zou (1998) and Forbes (2000) use panel data techniques to control for omitted variables, and find that income inequality is good for growth. While correcting for the influence of omitted country-specific variables, they do nothing to identify the variables that may be responsible for the negative relationship between growth and inequality observed in the cross-country data. Moreover, since most of the variation in inequality is across countries rather than across time, these estimates are inefficient, using very little of the available information in the cross-country data.

In this paper we present evidence that reconciles the puzzle presented by the empirical literature on inequality and growth by noting that, being derived from moments of the same, evolving distribution of individual incomes, it makes sense to think of growth and inequality as being generated by a common set of processes. From this perspective, the question that has motivated much of the research in this area – Persson and Tabellini's query "Is inequality harmful to growth" – is itself misleading. We believe a more revealing question is that pursued in this paper: "What are the determinants of inequality and how are they related to economic growth?"

Economic institutions that grant property and contractual rights have been shown to be fundamental determinants of growth.[1] We believe they are also likely to be key determinants of inequality, since indices that have commonly and perhaps too vaguely been referred to as measuring institutional *quality* might, in our view, be better viewed as measuring institutional *equality*. As we argue in the next section, our emphasis on the distribution of property rights is fully consistent with the view of institutional quality presented in the most influential papers on

[1] Important papers in the literature on institutions and growth include Knack and Keefer (1995), Mauro (1995), Rodrik (2000), Acemoglu, Johnson and Robinson (2001) and Easterly (2001).

institutions and growth, Acemoglu, Johnson and Robinson (2001, 2002) and Engermann and Sokoloff's (1997, 2002). It is also consistent with the existing empirical work on the determinants of income inequality. Section 3 presents empirical support for this idea, showing that economic institutions play a fundamental role in determining the level of income inequality.

Sections 4 re-examines the cross-country evidence on inequality and growth in light of this finding. The evidence supports our contention that the omission of institutional quality is responsible for the negative relationship between growth and inequality reported in the literature. When both institutions and inequality are included in a growth regression, institutions appear to affect growth but inequality does not. This result is robust to changes in the list of regressors, the use of asset rather than income inequality, and the use of alternative empirical models.

In section 5 we address the panel data evidence on inequality and growth. We begin by showing that institutional quality is highly correlated with the country-specific intercepts generated by a canonical fixed effects regression, suggesting again that institutional quality is an important source of the omitted variable bias motivating the use of fixed effects estimators. We use this information to construct an efficient GLS estimator that allows us to identify separately the (long-run) between- and (short-run) within-country effects of inequality on growth. With the inclusion of institutional quality, a Hausman specification test fails to reject the assumption that the included regressors are uncorrelated with the country-specific errors, a problem cited by Forbes (2000) and others as necessitating the use of the less efficient fixed effects estimator. Consistent with earlier evidence from fixed effects models, we find that short-run changes in inequality over time are positively associated with growth within countries. The variation in long-run inequality levels across countries is not a significant determinant of growth, however.

Section 2: Institutional Foundations of Inequality

Over the last decade, a number of authors have presented empirical evidence that a country's economic performance is strongly correlated with the "quality" of its institutions, a term that suggests a ranking along some vertical hierarchy. Both civil rights and the protection of private property and contractual rights – the primary dimensions of institutional quality emphasized in the literature – implicitly include some notion of equality, however. Where private property rights are not well protected through the courts and other public institutions, for instance, agents will use private resources to protect their property. In this case, there will be unequal protection of property reflecting the inequality of wealth and political power which agents have at their disposal.[2] Therefore, we view "weak" property rights as those leading to unequal protection.

A link between institutions and inequality may be found in the work of Acemoglu, Johnson and Robinson (2001, 2002), henceforth denoted as AJR. In their 2001 paper, AJR find that in colonies where European mortality rates were high, settlers adopted "extractive institutions" that tended to retard development. Despite the wide influence of these papers, little emphasis has been placed on the explicitly distributional dimension of AJR's understanding of extractive institutions, "which concentrate power in the hands of a small elite and create a high risk of expropriation for the majority of the population, [and thus] are likely to discourage investment and economic development." (2002, p. 1235) To AJR, it is not sufficient that *some*

[2] For example, Robert Mugabe and his supporters enjoy highly protected property rights in contemporary Zimbabwe; what is important for both development and for economic equality, however, is that this level of institutional protection be broadly shared among the population.

have secure property rights. A reasonable degree of security must be *shared by the wider population*.[3]

Section 3: Evidence on Institutions and Inequality

Previous empirical work on the determinants of income inequality across countries is scarce and little consensus exists regarding proper model specification. To examine the impact of institutional variables we adopt as a benchmark model the specification used by both Li, Squire and Zou (1998) and Lundberg and Squire (2003). The authors regress inequality on four variables measuring financial development, education, land inequality and civil liberties, intended as a test of theories that emphasize capital market imperfections and political protections.[4] Each of these four variables can also be interpreted as measuring some dimension of economic or social inequality, however: unequal access to education, to land, to capital, and to political power. As emphasized by Engermann and Sokoloff (1997), these proximate determinants of income inequality can be viewed as the expression of deeper institutional structures that manifest themselves through their influence on land tenure and settlement, the provision of public education, the regulation of financial organizations, and restrictions on political participation.

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[3] The link between institutions and income inequality also emerges in a series of papers by Engermann and Sokoloff (1997, 2000, 2002) addressing the comparative development of North and South America. The authors suggest that where there was an abundant supply of indigenous labor and conditions were appropriate for large scale plantations and mining operations, immigration was limited and European colonizers adopted institutions that tended to concentrate economic and political power in their hands. From this perspective, high levels of inequality are not simply an unintended consequence of weak institutions and poorly protected property rights, but rather a deliberate consequence.

[4] Galor and Zeira (1993), Piketty (1997) and Aghion and Bolton (1997) stress the role of asset inequality and imperfect capital markets in determining income inequality, while Benabou (2000), Bourguignon and Verdier (2000) and Acemoglu and Robinson (2000) give a central role to educational variables and the redistributive effects of democratic politics.

The evidence we present in this section is broadly consistent with this interpretation. When a measure of property rights protection from the state is introduced to our benchmark specification, to capture these deeper economic institutional characteristics, a strong association is observed with inequality while many of the variables emphasized previously in the existing literature are no longer significant. This result proves to be robust to the inclusion of additional control variables and of instruments to address the potential endogeneity of property rights.

Data

In keeping with previous literature we measure income inequality using a cross-country panel of data on Gini coefficients, compiled from an array of country surveys conducted over the post-war period by a number of researchers, starting with Deininger and Squire (1996). These surveys employ different methodologies, however, and in some cases multiple Gini estimates are available for a country in a given year, while no data is available for other years. To facilitate transparency, we use those observations suggested by Milanovic (2006) and then adjust Gini coefficients for remaining differences in survey sources and methodologies using a hedonic regression on survey type. From the resulting unbalanced panel of annual data we construct a panel of observations over 6 five year periods, from 1961 through 2000, which we employ throughout the paper.[5] Our cross-sectional regressions employ each country's average level of the Gini coefficient *in the panel*, which allows consistency across the different type of regressions employed in the paper. Because inequality levels are so highly stable over time, the use of an average, rather than initial or final level of inequality is not important to our results, but

[5] Adjustments were made using point estimates from a regression with country fixed effects. Our panel was constructed using data from the last available year of each period. Where this required going back more than three years, interpolations of Ginis from the nearest past and future years were used if available. A full description of the methodology is detailed in a Data Appendix available on request from the authors.

it does help reduce measurement error introduced by variation in survey methodology and offer a larger and more representative sample of countries.

In choosing the regressors for our benchmark regression we follow Li, Squire and Zou (1998) by including measures of human capital, land inequality, financial development and democratic political institutions. We use slightly different measures for these variables, however, owing to increased data availability or because it allows a better match between theoretical concept and empirical proxy. The differences in variable definitions are not essential to our results, however.[6]

We use two variables to measure the quality of institutions. First, to measure the openness and competitiveness of political markets and the equality of political power, we use the Polity IV database described in Marshall and Jaggers (2006).[7] We adopt a common approach of computing a "polity score" of political freedoms ranging from -10 to +10 by subtracting the autocracy score from the democracy score. Our cross-country regressions use a polity score from 1970 to measure the quality of political institutions.[8]

To measure the quality of *economic* institutions we use the 10 point "freedom from expropriation" index reported by the ICRG surveys. This variable has been used as a measure of property rights protection in a number of studies, including Keefer and Knack (1995) and Acemoglu, Johnson and Robinson (2001). These scores are only available for the second half

[6] Following Sokoloff and Engermann (2000), we use primary rather than secondary school enrollment rates as a measure of access to human capital. For land inequality, we use a more recent set of estimates compiled by Frankema (2005). Similarly, we use the ratio of private credit to GDP from Beck, Demirgüç-Kunt, and Levine (2000) to measure financial development rather than the ratio of M2 to GDP.

[7] We feel that the Polity IV database offers a better measure of political rights than the Freedom House data used by Li, Squire and Zou, as it provides more detail, more transparency in its construction and a longer time series.

[8] Although earlier data is available this allows nation states that emerged or were granted independence in the 1960s to be included in our sample, using a consistent year of measurement for all countries.

our sample period, so in both cross-sectional and panel regressions we use a single observation for each country representing the average expropriation risk in the country over all years.

Results

Column (1) of Table 1 shows that our baseline regression alone explains over 40 percent of the cross-country variation in inequality. The variables all have the expected sign, and two of the four (land inequality and political freedom) are statistically significant at the 5% level using White heteroskedasticity-corrected standard errors. In the second column we modify this baseline specification by including freedom from state expropriation, our proxy for the quality of economic institutions. The inclusion of this variable significantly increases the fit of the regression and the coefficient on freedom from expropriation is large and statistically significant at the 1% level. The partial correlation, shown in Figure 1, implies that a one-standard deviation increase in freedom from expropriation (1.8 on a 10 point scale) corresponds to a 7 point increase in the average level of the Gini coefficient, or roughly ¾ of a standard deviation. Moreover, with the exception of land inequality, none of the variables in the canonical specification continues to have a significant association with income inequality after controlling for equality of property rights.

Because of collinearity among the independent variables, columns (3) through (6) pair freedom from expropriation risk with each variable individually – in none of these regressions are the results qualitatively different from those reported in column (2), however. In each column freedom from expropriation is significant at the 1% level and, of the other regressors, only the coefficient on land inequality is statistically significant. For this reason, and because theory suggests that asset inequality should have the clearest, most direct impact on income inequality, we retain only land inequality in further robustness checks of the role of economic institutions.

In columns (7) – (8), we test whether the importance of institutional quality is robust to the inclusion of a number of additional control variables. The first are the log of per capita income in 1970 and its square to control for the presence of a Kuznets-curve, a pattern found in cross-sectional data by a number of authors including Li, Squire and Zou (1998) and Barro (2000). Neither income variable is significant in this specification, however, while expropriation risk remains strongly so.

A number of papers, including Rosser, Rosser and Ahmed (2000) and Davis (2007), have suggested a link exists between the share of the informal sector of the economy and income inequality, and informality has been linked to various measures of institutional quality in work by de Soto (1989), Djankov et al. (2002), and Friedman, Johnson, Kaufmann, and Zoido-Lobatón (2000). To test whether our estimates are biased by the omission of informality in column (8) we introduce a measure of the informal sector's share of output, constructed by Friedman et al. (2000). The inclusion of informality results in a lower estimate for the magnitude of the coefficient on freedom from expropriation, ostensibly because part of the reason expropriation risk increases inequality is because it discourages participation in the formal sector.[9] The coefficient on expropriation risk retains its statistical significance, however, suggesting that it may play a fundamental role in shaping the income distribution beyond simply creating a barrier to formality.

Omitted geographic characteristics may also create a bias in our estimates. Geography has been linked to institutional quality by Hall and Jones (1999), and is plausibly correlated with

[9] Although the resulting sample size also decreases by 14 observations, the coefficient on expropriation risk is unaffected by the change in sample composition (it remains roughly -3.4 in the smaller sample).

inequality as well, for instance by generating a natural resource curse. Similarly, inequality and institutions seem to be correlated with regional location. As shown in columns (9) and (10), the inclusion of geographic and regional variable reduces the magnitude of the coefficient on freedom from expropriation by nearly one-half, but its significance does not fall below the 5% level.[10] These results suggest that freedom from expropriation is not simply a proxy for omitted regional or geographic variables.

Overall, the results presented in Table 1 are consistent with the hypothesis that the quality of economic institutions plays a key role in determining the level of income inequality across countries. With the exception of land inequality, many of the other variables highlighted by recent empirical work appear to be much less important in that they are not robust to the inclusion of our measure of the quality of economic institutions.

Addressing the Endogeneity of Institutional Quality

The results reported above are subject to three criticisms related to the treatment of institutional measures as exogenous regressors. The first, articulated well by Acemoglu et al. (2001), is that the institutional variables are derived from expert opinion and survey data, and thus potentially subject to systematic measurement error. This will occur, for example, if experts tend to "see" better institutions in countries that experience higher growth rates or less income inequality. The second is reverse causation. A number of papers argue that income inequality reduces property rights protection, e.g. Glaeser, Scheinkman and Shleifer (2003) and Keefer and

[10] We include regional variables for East Asia Pacific, South Asia, Latin America and Caribbean, Sub-Saharan Africa and Middle East North Africa. We omit variables for North America, Western Europe. Eastern and Central Asia is omitted as it only has a single observation.

Knack (2002).[11] Finally, these regressions are intentionally parsimonious, and the omission of variables that are simultaneously correlated with institutions and inequality may bias our coefficient estimates.

We address these issues by instrumenting for our key institutional variable, freedom from expropriation, to avoid bias introduced by the potential correlation between it and the unobservable components of income inequality. In doing so, we rely on instruments for contemporary institutions that have been previously used in the empirical growth literature. This literature relies heavily on arguments that historic colonization patterns represented an exogenous shock that determined the path of institutional formation that gave rise to the institutional qualities observed in the present. We believe that the arguments made for why these instruments in growth regressions would be uncorrelated with omitted determinants of current per-capita incomes apply equally well to the omitted determinants of the income distribution more broadly.

AJR (2001, 2002) contend that European settler mortality rates affected colonization strategy, with low mortality regions becoming settlement colonies with good institutions and high mortality rate regions becoming extractive colonies with poor institutions. The importance of pre-colonial population density is stressed both by AJR and by Engermann and Sokoloff (1997, 2000), who suggest that the availability of an indigenous workforce limited European immigration and led to the adoption of institutions designed to preserve the privileges of an elite European minority. Acemoglu et al. (2001) also find that a history of democratic politics is an

[11] While we know of no data on property rights or inequality with a sufficiently long time series to allow a serious test of causality, we do not believe that our results are due solely to reverse causality. Replicating Table 1 but *controlling for initial inequality around 1970*, inequality in 1990 and in 2000 are both lower in countries enjoying stronger property rights. Unfortunately, sample sizes in such an exercise are small, varying from 9 to 33 countries. Thus we believe the data support our hypothesis that institutions determine inequality, but we cannot rule out the possibility that institutions are also endogenous, the issue we address in Table 2.

important determinant of current institutional quality. Their preferred variable, democracy in 1900, constrains the ability to include younger nations into a cross-country sample and vastly constrains sample size, however, so we instead use democracy in 1960.

Geography and language have also been suggested as instruments for institutional characteristics by Hall and Jones (1999), who argue that latitude serves as a proxy for institutional transfer during colonization.[12] Hall and Jones also suggest the fractions of a country's population that speaks English or a European language as a first language might serve as instruments for institutional quality.

A country's legal heritage has been linked to both the efficiency of its courts (Djankov et al., 2003a) and the regulation of entry and labor markets (Djankov et al., 2002, and Botero, et al. 2004). Djankov et al. (2003b) argue that French civil law system was designed to enhance the power of the state relative to private actors, with obvious implications for the protection of private property rights. Thus we include dummy variables indicating English, French, German, Scandinavian and Socialist legal heritage among the instruments we consider for institutional quality.

This large and overidentifying set of plausible instruments can generate a good fit for the first-stage regression, but it increases the likelihood of introducing bias from weak instruments. We therefore use several subsets of these instruments to generate different IV estimates, following three criteria. The first is parsimony, for example, by using only a single language variable. The second is selecting instruments that allow us to retain an adequate sample size.[13] The last is that the instruments be valid; plausibly uncorrelated with unobserved determinants of

[12] Our geographical instruments include absolute value of latitude and its square, a non-linearity suggested by the first-stage regressions.

[13] For this reason, we rely on the instruments identified by Acemoglu et al. (2001, 2002) mainly to test the robustness of our results.

inequality and not just growth. While this is required as an *a priori* assumption, we use the overidentifying restrictions to check the validity of the exogeneity assumption, and address potential criticisms of our maintained exogeneity assumptions in robustness checks.

Table 2 presents two-stage least squares estimates that confirm our earlier finding that the protection of property rights has a strong, statistically significant and robust effect on income inequality. For each specification, the second-stage regression of average inequality is reported in the first column (A), with results from the first stage instrumenting regression for freedom from expropriation displayed in the second column (B). Using a variety of instruments and controls in columns (1) through (5), we consistently find that freedom from expropriation is significant at the 1% level with a coefficient of similar magnitude as that reported in Table 1.

Below each specification we report the p-value from Hansen's J statistic and in each case a test of overidentifying restrictions (OIR) fails to reject the joint null hypothesis that our instruments are correctly excluded.[14] The high R-squared and significant coefficients in the first-stage regressions give them impression of strong instruments, but we also test for excessive bias arising from weak instruments using the reported Cragg-Donald F-statistic and the critical values calculated by Stock and Yogo (2005), which vary by number of instruments. In every regression we are able to reject the null hypothesis that the bias of the IV estimator exceeds the bias of the OLS estimator by more than 10 percent,. Thus we believe that our instruments are valid and are not particularly subject to bias from weak instruments.

The data alone cannot guarantee the validity of the instruments, of course, and a potential criticism of the model estimated in column (1) is that French legal heritage is not properly

[14] The OIR test examines whether the excluded instruments are able to explain differences in inequality beyond their ability to account for differences in the endogenous regressor, freedom from expropriation. Hansen's J statistic is used in place of Sargan's test of over-identifying restrictions to allow for heteroskedasticity in the residual terms. Under the assumption of homoskedastic errors, the two tests are equivalent.

excluded from the inequality regression equation. Beck, Demirgüç-Kunt and Levine (2004) have argued, for example, that legal heritage influences inequality through financial sector regulation and development. To control for this, in specification (2) we include the ratio of private credit to GDP to proxy for financial development. A similar criticism is that the degree to which a population speaks a European language might be directly related to income distribution through channels related to international trade and capital flows.[15] In our column (3) we test for this effect by including a measure of openness in the second stage of the regression. In both cases the coefficient on freedom from expropriation remains significant while neither private credit nor trade is significant as a determinant of either inequality or expropriation risk.

To control for other potential omitted factors, regional dummy variables are introduced in the specification in column (4) indicating whether a country is in either Latin America and the Caribbean or in sub-Saharan Africa, both of which have a statistically significant association with both inequality and expropriation risk. Adding these controls reduces the economic significance of freedom from expropriation on inequality, but the coefficient remains statistically significant at the 1% level. These results suggest that the correlation between inequality and expropriation risk is somewhat stronger across than within regions, but omitted regional variables cannot fully account for the influence of institutions on inequality.

In the fifth column we include European settler mortality rate as an additional instrument. While use of this instrument lowers sample size significantly, AJR present strong arguments for the relevance and exogeneity of this instrument in instrumenting for expropriation risk so we consider it useful in assessing the validity of our other instruments. With the expanded instrument set, the coefficient on freedom from expropriation is slightly reduced in magnitude,

[15] While neoclassical trade theory predicts the direction of these effects will differ across countries depending on relative factor abundance and the pattern of factor ownership, it is possible that either positive or negative effects are more common within our sample.

but it remains significant at the 1% level.[16] The tests statistics for instrument validity and weakness are both reduced, but they remain above their critical values. Thus we believe our choice of excluded instruments is reasonable and, more importantly, that our conclusions regarding the importance of economic institutions for determining inequality is not sensitive to a specific set of instruments.

The results reported in this section suggest that institutions, specifically property rights protections, play a central role in reducing income inequality. Across a wide range of specifications expropriation risk is consistently significant at the 1% level and by itself explains close to half the observed variation in income inequality. Instrumental variable techniques suggest that these results are not driven by reverse causation or subjectivity bias in the measurement of institutions.

We believe these results are important to the ongoing debate over the role of income inequality in economic growth. If stronger property rights protection is associated both with equality of incomes and growth in incomes there is reason to believe that the negative relationship between income inequality and economic growth found in many cross-country growth regressions may be due to omitted institutional variables rather than evidence of a direct causal link suggested by models of credit market imperfections or politically-driven redistribution. The next section considers this argument in greater detail.

[16] The change in the coefficient on expropriation results mostly from the inclusion of the new instrument, not the change in sample composition. The decline in R^2 (from 0.56 to 0.35) in the second stage regression comes entirely from the smaller sample, however, and not the inclusion of the settler mortality instrument.

Section 4: Re-evaluating the Cross-Country Evidence on Growth and Inequality

The fact that stronger property rights may lower inequality has important implications for the empirical literature on inequality and growth, given that property rights have also been shown to be an important determinant of economic growth. Thus, regressions that include inequality but not institutional variables will generate negatively biased estimates of the influence of inequality on growth.

To test for this we begin by replicating what we consider to be a canonical growth-inequality regression, based upon the specifications commonly used in the first-wave of the growth and inequality literature (e.g. Alesina and Rodrik, 1994, Persson and Tabellini, 1994). The dependent variable is the average growth rate of real per capita income (PPP) from 1970 to 1995, a period that maximized sample size. This is regressed on average income inequality and several control variables, including initial income, to capture conditional convergence effects, and the primary enrollment rate in 1970, a measure of human capital investment. To avoid potential endogeneity problems, we do not include the average investment rate as a growth regressor but use instead two variables that influence investment decisions, the domestic price of investment goods, averaged from 1970 to 1990, and private credit relative to national income, a measure of financial development.

The results from our benchmark regression, reported in column (1) of Table 3, confirm the central finding of the first wave of empirical work on growth and inequality: the coefficient on inequality is large, negative and highly significant, in this case at the 1% level. With the exception of the price of investment goods, all of our control variables have the expected sign and are significant as well. In column (2), we introduce our political and economic institutional variables to the regression.[17] Freedom from expropriation has a positive effect on growth that is both statistically and economically significant, with the estimated coefficient suggesting that an increase in freedom from expropriation of one standard deviation (1.83 out of 10 points, or roughly the difference between Panama at 5.66 and Chile at 7.5) is associated with a 1.43 percentage point increase in the average annual growth rate. In constrast, we find that political freedoms are negatively – although not strongly – associated with growth..

Introducing institutions dramatically reduces the measured effect of inequality, however, and its coefficient does not remain statistically significant. These results suggest that the omission of institutional variables introduced bias into earlier cross-country regression estimates which led to mistaken inference regarding the role of income inequality on growth.

Our results could also be subject to omitted variable bias, of course. Institutions, development and inequality all vary systematically with geographic location, with countries in the tropics tending to suffer both from lower levels of property rights protection and from several potentially important determinants of economic growth, including disease burden, fertility of the land and average temperature. To consider this possibility, column (3) adds two geographic control variables, a dummy variable indicating landlocked countries and the absolute distance from the equator. The inclusion of these geographic variables reduces the coefficient on freedom from expropriation somewhat, but our conclusion is otherwise the same..

To ensure that our results are not driven purely by regional differences we introduce four regional dummy variables in column (4) but, again, find that economic institutions remain significant at the 1% level while income inequality is insignificant. Thus, the first four columns of Table 3 tell a consistent story: good economic institutions have a robust positive relationship

[17] Given its discussion in previous literature, we allow for the presence of a non-linear relationship between growth and democracy.

with economic growth while, a country's average level of inequality does not appear to be directly related to its long-run rate of growth.

Empirical work by Birdsall and Londono (1997) and Deininger and Olinto (2000) has found that asset inequality, and land inequality in particular, is more robustly related to growth than is income inequality, as would be expected by theories that emphasize the importance of capital market imperfections. Land inequality is highly correlated with freedom from expropriation, however, raising the question of whether the omission of economic institutions has led these researchers to misinterpret the role that asset inequality plays in economic growth.

In columns (5) through (8) of Table 3, we run the same regressions from columns (1) through (4) while substituting land inequality for income inequality. The results are similar, reinforcing our earlier conclusions. In column (5), we find that land inequality is negatively related to economic growth, though only at the 10% level. In column (6), where we include our measures of economic and political institutions, we find that freedom from expropriation is significant at the 1% level, while land inequality is insignificant. In Columns (7) and (8) we find this result is robust to the inclusion of geographic and regional variables. Our evidence suggests, therefore, it may be the omission of institutional variables that was responsible for earlier claims that land inequality was a significant determinant of economic growth.

Simultaneous Estimation of Growth and Inequality

The fact that the same institutional measures appear to affect both inequality and per-capita incomes should not be surprising given that both measures are derived from moments of the same income distribution. This raises the possibility, however, that other omitted determinants of growth may be correlated with inequality, creating an endogeneity problem that could bias the coefficients in Table 3. We address this issue in Table 4 by estimating a system of equations in which both growth and inequality are treated as endogenous, with each regression again using the canonical specifications employed of Tables 1 and 3.[18]

Our empirical model is recursive, with average growth rates depending on average inequality and a set of controls similar to those used earlier and inequality identified through land inequality and political institutions, which are excluded from the growth equation. Initial income and the price of investment goods are excluded from the inequality equation. We do not assume that long-run growth rates contemporaneously affect the level of inequality due to the absence of both such a link in the theoretical literature and evidence of a link in our data.

The coefficient estimates reported in column (1)A of Table 4 all have the expected sign, and mirror our earlier results from Table 3. Broadly speaking, these results are consistent with the conclusions of the first-wave literature that inequality appears to negatively affect growth in regressions in which economic institutions are not included. Estimates from the inequality regression in column (1)B closely follow those from the first column of Table 1. In the second column of Table 4 we include our proxies for economic and political institutions in both equations and the impact of this change is again consistent with our previous results. The inclusion of institutional variables significantly improves the fit of each equation. In addition, the protection of property rights is statistically significant in both regressions: better property rights protection simultaneously increases the rate of growth and decreases income inequality. With the inclusion of our institutional variables, the coefficient on inequality is no longer significant in the growth equation, an outcome that is retained across all of the remaining specifications. Our proxy for political institutions is not significant in either regression.

[18] The simultaneous equations approach is also taken by Lundberg and Squire (2003) and Barro (2000), although unlike this paper, these authors do not focus on the role of institutions in jointly determining growth and inequality.

We proceed a step further in the third column in Table 4 by treating freedom from expropriation and democracy in 1970 as endogenous as well, using political rights in 1960 and several of the instruments discussed previously, including French legal heritage, the share of the population speaking a European language, distance from the equator and its square. The results reported in column (3) confirm qualitatively our earlier conclusions: inequality is not significant in the growth equation, while freedom from expropriation is.[19] In addition, instrumented democratic rights now are significantly and negatively related to growth.

Most of our results are robust to the inclusion of regional dummies, however one key finding is not. In column four, we include regional dummy variables for sub-Saharan Africa and Latin America and the Caribbean, and find that freedom from expropriation is no longer significant in the growth regression.[20] This suggests that the coefficient estimate in column (3) reflects inter-regional variations in institutions more than intra-regional variations.

In column (5), we include European settler mortality rates as an instrument and drop the population share speaking a European language. While this change of instruments used also significantly reduces both the sample size and the fit of the regression, our measure of economic institutions remains highly significant in both equations. In addition, the magnitude of the coefficient in the inequality regression is much higher. A potential explanation for this finding is that the link between economic institutions and inequality is stronger among former colonies.

Overall, these regressions confirm our hypothesis that economic institutions matter for both growth and inequality, but inequality itself does not have an independent effect on

[19] Instrumenting for institutions actually increases the magnitude of the coefficient on expropriation risk in each equation. As suggested by Acemoglu, Johnson and Robinson (2001), this result is consistent with the idea that measurement error – particularly the tendency to see good institutions in countries with good outcomes – may have biased downward the institutional coefficients reported in column (2).

[20] Given our small sample size, we do not include extraneous regional dummy variables. An F-test for the exclusion of the regions East Asia and Pacific, South Asia and Middle East and North Africa in both equations has a P-value of 0.62, suggesting we cannot reject the hypothesis that the coefficients of these variables are zero in each equation.

economic growth. Interestingly, the role of financial development on income inequality appears to be somewhat different than is portrayed in the existing literature, which argues that financial development is important for extending access to capital to the poor. Inequality is positively associated with the ratio of private credit to GDP in all four regressions that control for the level of economic institutions, and it is significant in two of them.

Section 5: Institutions and Fixed Effects Estimators

We are not the first to claim that the estimates from the first-wave of growth and inequality regressions might suffer from omitted variable bias. Previous authors have addressed this issue by exploiting the time variation in inequality and growth available in panel data, using fixed effects estimators to control for unobservable country-specific, time invariant characteristics. This approach suffers from a number of drawbacks, however. First, parameter estimation using a fixed effects estimator is inefficient, exploiting only the information resulting from time-series variation within countries. Since variation in both inequality levels and growth rates is typically much higher across countries than within them the precision of the resulting estimates is reduced. Second, the effect of any relatively time-invariant variables – including many institutional measures – cannot be identified within a fixed effects specification, which limits the usefulness of these regressions as the basis for further research in this area.

More importantly, the use of fixed effects estimates to control for omitted variable bias requires *changing the question* from the effect of inequality on long-run growth in aggregate supply to a much shorter-run correlation between inequality and growth, typically over a series of five-year periods. Estimates based on this higher frequency variation are likely to be highly influenced by business cycle dynamics. This point seems particularly relevant given the political economy mechanism involving variations in the intensity of redistribution, which may vary with the political business cycles. As a result, coefficient estimates based on fixed effects estimators may be of limited relevance for understanding the long run relationship between growth and inequality.

An alternative method of controlling for intrinsic country-specific heterogeneity is through estimation of random effects, or country-specific residuals, and this provides a solution to both of these problems. Coefficients estimated using random effects are, by construction, a weighted average of the within-group fixed effects estimates and estimates generated using group averages. This allows both the identification of time-invariant covariates and maximal precision, as the random effects estimator is also the GLS estimator. The problem for Forbes (2000) and others is that a Hausman specification test rejects the maintained assumption that the omitted time-invariant variables are uncorrelated with the included variables, suggesting that coefficients estimated using random effects would be biased.

Rather than resorting to a fixed effects estimator, we address this dilemma directly by identifying the time-invariant omitted variables that *cause* the random-effects estimator to be rejected in a Hausman specification test. Unsurprisingly, we find that institutional quality is the most important of these omitted variables. Having identified the variables leading to the omitted variable bias, we include them in a specification employing random effects, the efficient GLS estimator. This approach permits us to take advantage of both cross-country variation in inequality and the ability to identify the effect of additional country-specific characteristics.

Our estimates suggest that controlling for institutional quality, the long-run effect of inequality on growth is not significantly different from zero. Faster growth and lower inequality in the long run data are both products of the income dynamics generated by stronger property rights protection. The positive relationship between growth and inequality identified by earlier researchers employing panel data techniques exists only in the higher frequency data.

Institutions and Country Intercepts

The country fixed effects estimated by Forbes (2000) and Li and Zou (1998) to control for omitted variable bias reflect the joint impact of all of the time-invariant variables that influence a country's growth rate. The relative stability of institutions over time suggests they may constitute a key element of this permanent component of a country's growth rate. Indeed, the persistence of institutions is one of the central themes of the new institutional economics.[21] To investigate this possibility, we employ a fixed effects regression using the same specification as Forbes (2000), one of the more influential papers from the second-wave literature, to estimate country-specific intercepts. Using an unbalanced panel of data covering eight 5-year periods starting in 1961-65 and ending in 1996-2000, growth in each period is regressed on the log of real per-capita income and average years of secondary schooling taken from the final year of the previous period, an income Gini coefficient calculated around the final year of the previous period, and the average price of investment (in *PPP* terms) over the previous period.

[21] Sokoloff and Engermann (2000) argue that the persistence of colonial institutions exerts a continuing effect on Latin American development. Acemoglu, Johnson and Robinson (2001) provide empirical support for a high degree of institutional persistence and suggest a number of mechanisms that could lead to institutional persistence, including sunk costs, complementarities with existing investments, and the continuity of local elites. North (1990) attributes institutional stability to increasing returns, broadly construed, which generates multiple stable institutional equilibria. Complementarities between formal institutions and highly persistent informal institutions may also contribute to institutional stability.

The results presented in the first column of Table 5 are similar to those reported by Forbes (2000), with the coefficient on inequality positive and significant at the 1% level.[22] Forbes estimates the effect of male and female schooling separately, but we cannot reject the hypothesis that the coefficients on the two education variables are equal. Thus, parsimony motivates the regression reported in the second column of Table 5, using average years of secondary education for the combined population.[23] If this regression is correctly specified, then the country-specific intercepts (fixed effects) capture the portion of a country's growth that is explained by a country's time-invariant characteristics. The range in this permanent component of the annual growth rate is surprisingly large, with a one standard deviation equaling 4.3 percentage points.

In Table 6 we report estimates of a cross-sectional regression of the country intercepts on our measures of economic and political institutions. As shown in the first two columns, these two institutional variables explain 65 percent of the variation in the country intercepts, but almost all of the explanatory power of these variables comes from the quality of economic institutions: political rights are not significant and their inclusion only slightly reduces the adjusted R-squared. We interpret this regression as supporting the hypothesis that economic institutions are a key determinant of the permanent component of a country's growth rate.[24]

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[22] The growth and inequality data used to compute the fixed effects are both updated and from slightly different sources from that used by Forbes. Forbes also presents results using the Arellano-Bond instrumental variables estimator to control for potential bias from the lagged income term in the growth regression. For simplicity, and to avoid the weak instrument problems associated with the Arellano-Bond estimator, we report results using the standard fixed effects estimators alone.

[23] *A priori* reasoning also suggests that estimates suggesting a differential impact of male and female schooling found by Forbes may not reflect intrinsic differences in human capital productivity but rather the implicit effect of institutions in a country that both lower growth and generate gender disparities in education – something we want to explore directly.

[24] Because our institutional variables reflect quantitative measures of qualitative concepts, we allowed for the possibility of non-linearities, but our results in the first two columns are robust to the inclusion of quadratic terms.

Mauro (1995) has argued that ethnic fractionalization is associated with low quality institutions. To ensure its exclusion is not biasing our results we include a measure of ethnic fractionalization and find, as reported in column (3), that ethnic fractionalization is indeed statistically significant but the coefficient on expropriation risk remains highly significant and is only slightly reduced in magnitude. In column (4), we show that the importance of expropriation risk is robust to the inclusion of these regional and geographic variables as well.

Random Effects Estimation

The evidence above that institutions account for a large part of a country's permanent growth rate suggests they may be a primary source of the omitted variable bias generating conflicting results between the first and second waves of growth and inequality regressions. To test whether this is in fact the case we employ a series of panel regressions reported in Table 7. Column (1) reproduces the Forbes specification from Table 6, column (2) used to estimate fixed effects. Column (2) presents the same estimates derived using a random effects estimator. If the unobserved country-specific effects are uncorrelated with the regressors, both estimates will be consistent estimators of the same quantities but the random effects estimator – the GLS estimator – will be more efficient. Comparing the coefficient estimates, a Hausman specification test strongly rejects this maintained hypothesis, however, suggesting that the random effects estimator is inconsistent.

The estimates generated by the random effects estimator are, by construction, a weighted average of the fixed effects estimates derived from period-to-period variations within countries and a "between" estimator using cross-country averages. Column (3) helps to highlight the problem inherent in Column (2) of conflating these "short-run" and "long-run" effects by running the same random effects regression but decomposing the income Gini into two components: one the average inequality for each country over the sample, and the other period-to-period deviations from this average. This reveals that the coefficient on inequality reported in column (2) was not significant because it is averaging a short-run positive relationship within countries and a long-run negative relationship across countries. As expected, the coefficient estimate on within-country inequality in column (3) is quite similar to that reported using fixed effects in column (1), while the between-country estimate is roughly similar to that reported in column (1) of Table 3.

A Hausman test still rejects the exogeneity of random effects across all the estimates in column (3), but this fact changes after controlling for institutional quality. The specification in column (4) includes expropriation risk alone while that in column (5) includes political rights as well, although these two institutional variables are now treated somewhat differently. Due to data limitations we continue to use a single time-invariant average score for expropriation risk, but the long time series in the Polity IV database allows us to use a time-varying measure of political rights. Because the uncertainty associated with changes in political institutions in any direction may have a negative impact on growth, we control for this separately with a dummy indicating periods involving a significant political transition (during which the Polity IV database provides no scores).

The results from columns (4) and (5) confirm our earlier conclusions: expropriation risk is strongly associated with growth while the extent of political rights matters much less. Political transitions do clearly reduce growth in the subsequent period, as might be expected. The Hausman test statistic falls drastically with the inclusion of expropriation risk although not sufficiently far to reject null of exogenous country effects by itself. We see in the remaining columns, however, that expropriation risk is the key to eliminating the omitted variable bias.

In columns (6) and (7) we include our landlocked dummy and the ethnic fractionalization score, while columns (8) and (9) include six regional dummy variables. In each case, the first column reports the effect of the controls together with expropriation risk and the second column without it. The results show clearly that when economic institutions are used in conjunction with either of these additional sets of controls, the consistency of the random effects estimator cannot be rejected. Using these controls by themselves without expropriation risk, however, results in the Hausman test rejecting the random effects specification. This supports our contention that, more than other potential excluded controls, the omission of economic institutions led to omitted variable bias in the first-wave and inefficient estimators in the second-wave of the growth and inequality literature.

The most noteworthy aspect of Table 7 is that it nests and ultimately reconciles the results from the first and second wave of the growth and inequality regressions within a single unified framework. In column (3), we see that the average (or "long-run") level of inequality in a country has a negative and significant effect on growth rates, while period-to-period deviations from this average are associated with a deviation in growth rates the following period in the same direction. In columns (4) through (9) we see clearly that the apparent long-run association is, in fact, spurious: it is an artifact of the joint association of growth and inequality with omitted property rights. This leads us to conclude that the final answer to the long-pondered question of whether inequality affects long-run growth is *no*.

While a full exploration of the short-run dynamics of inequality and growth within countries lies beyond the scope of this paper, our results also suggest caution regarding how we interpret the positive relationship reported by Forbes (2000) and Li and Zou (1998). Although we too find evidence of a positive short-run relationship within countries, the random effects model does not support the position that the short run and long run relationships are the same. Over 5 year periods, the mechanism generating this correlation could well be more closely associated with business cycles than with long run economic growth if, for instance, inequality is correlated with unemployment.

Nevertheless, the broader theme in this paper, that inequality may be endogneous, may also apply at these higher frequencies. For example, Davis (2007) suggests that positive fixed effects coefficient estimates could result from the omission of time-varying variables that reduce inequality and growth together, such as the intensity of redistribution. Banerjee and Duflo (2003) investigate the panel data evidence closely, and argue that it is the absolute value, not the direction of a change in inequality which affects growth rates, a result they motivate using a simple political-economy model.

Section 6: Conclusion

This paper has argued that a country's institutions are a critical determinant of the level of income inequality. We examine the role of subjective indices of the quality of political and economic institutions, a democracy index measuring political rights and an index of expropriation risk measuring property rights. The relationship between property rights and income inequality is particularly strong, and is robust to the addition of controls and the use of several instruments to control for the endogeneity of our institutional variables. The evidence presented suggests that risk of state expropriation creates an institutional climate more costly to the poor and disenfranchised than for economic and social elites. We believe this evidence supports the view that institutions have an explicit distributional dimension to them and that, as a result, what is often referred to as the quality of institutions might be better understood and analyzed as the equality of institutions or property rights protection.

Our results do not support the commonly held view that democratic political institutions are an important determinant of income inequality. Although the evidence we present on this is far from conclusive, it appears that the correlation between political rights and inequality in the data may be an indirect one; most significantly, by improving the protection of property rights. We believe this raises interesting questions for future research regarding the mechanisms linking democracy and inequality.

After establishing the link between institutions and inequality, we reexamine results from the empirical growth literature and find that the negative relationship between inequality and growth reported in the first wave of growth-inequality regressions does not survive the inclusion of our institutional variables. Our results show that the protection of property rights has a strong positive relationship with economic growth, while inequality is not significantly related to growth. This outcome is robust to the use of alternative definitions of inequality, ancillary variables, estimators, and instrumental variables used to control for the endogeneity of institutions.

Finally, we reexamine the positive relationship between income inequality and economic growth reported by researchers in a second wave of growth-inequality regressions using panel data estimation techniques. To avoid the bias introduced by the omission of time-invariant, country specific factors, authors have typically employed inefficient fixed effects estimators. In a contrasting approach, we identify institutional quality is the primary source of omitted variable bias, allowing us to employ a more efficient random effects estimator. While we confirm the earlier finding of a positive short run relationship between inequality and economic growth, our results do not suggest that the short run and long run relationships are the same. This suggests to us that the short run relationship may have more to do with the dynamics of business cycles than with the determinants of long run economic growth.

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Figure 1

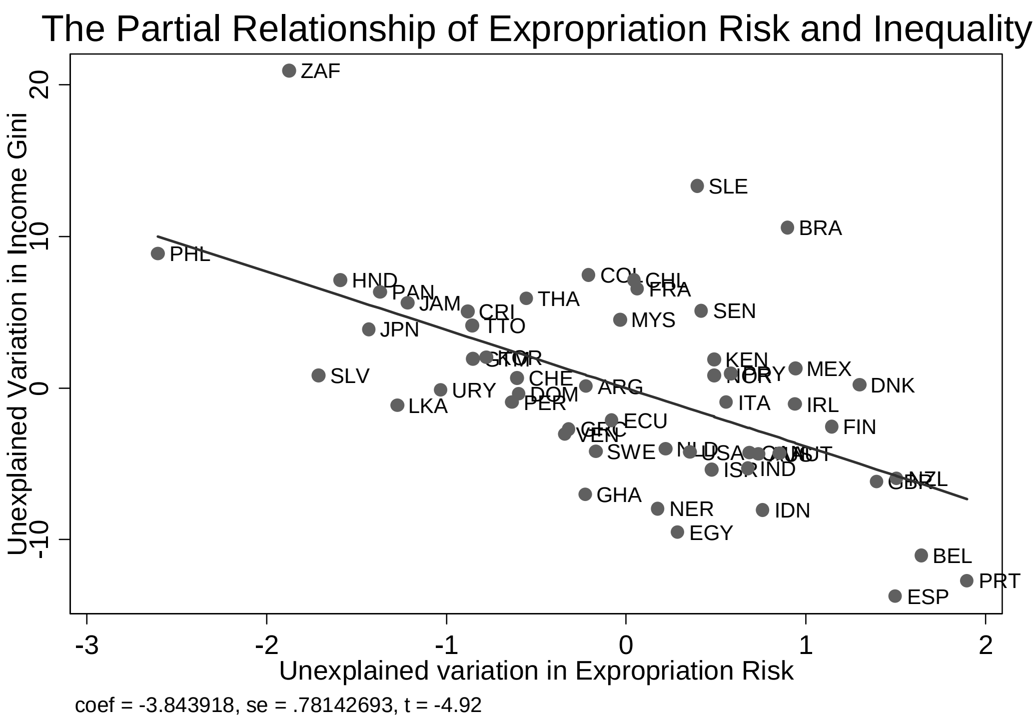

Table 1: Institutions and Inequality

(Dependent Variable is Average Income Gini, 1960-2000)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Primary	-0.0729	-0.0471	0.0289							
Enrollment in 1970	*(0.061)*	*(0.052)*	*(0.042)*							
Private Credit/GDP	-6.832	5.579		1.705						
	(4.17)	*(3.95)*		*(3.98)*						
Land Gini	17.17**	18.70***			10.86*		11.73*	12.22**	5.728	8.442*
	(8.33)	*(4.47)*			*(5.63)*		*(6.03)*	*(6.03)*	*(4.66)*	*(4.96)*
Political Rights	-0.395**	-0.125				-0.0906				
(Polity in 1970)	*(0.16)*	*(0.15)*				*(0.14)*				
Property Rights		-3.844***	-3.801***	-4.378***	-3.456***	-3.674***	-3.095***	-2.585***	-1.623***	-1.516**
(expropriation risk)		*(0.75)*	*(0.37)*	*(0.64)*	*(0.42)*	*(0.49)*	*(0.77)*	*(0.76)*	*(0.53)*	*(0.74)*
Log income							2.854			
							(27.1)			
Log income squared							-0.220			
							(1.60)			
Informal Share								0.134		
								(0.089)		
Tropics Dummy									5.896***	
									(1.92)	
Latitude									-0.106**	
									(0.043)	
Landlock Dummy									2.291	
									(2.17)	
Regions	N	N	N	N	N	N	N	N	N	Y
Constant	43.69***	63.22***	69.68***	76.08***	62.97***	72.08***	51.45	51.31***	51.47***	44.46***
	(6.40)	*(6.99)*	*(4.26)*	*(3.84)*	*(6.24)*	*(3.61)*	*(114)*	*(9.70)*	*(5.68)*	*(7.05)*
Observations	59	53	70	58	70	74	70	56	69	70
R-squared	0.43	0.67	0.48	0.60	0.48	0.50	0.48	0.54	0.64	0.71

Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Regions include East Asia Pacific, South Asia, Latin America and Caribbean, sub-Saharan Africa, and Middle East and North Africa.

Table 2: Institutions and Inequality using Two-Stage Least Squares

	(1)		(2)		(3)		(4)		(5)	
	A	B	A	B	A	B	A	B	A	B
Dependent Variable:	avgini	exprop	avgini	exprop	avgini	exprop	avgini	exprop	avgini	exprop
Expropriation Risk	-3.695***		-4.584***		-3.616***		-2.324***		-3.171***	
	(0.41)		(0.65)		(0.42)		(0.48)		(0.75)	
Land Gini	14.53***	-0.269	15.07**	1.245	15.90***	-0.145	10.49*	0.879	17.11***	-1.507
	(5.10)	(1.28)	(6.20)	(0.80)	(4.78)	(1.17)	(5.32)	(1.23)	(6.01)	(1.13)
Fraction European Language		0.862**		0.290		0.885**		1.346***		0.906**
		(0.33)		(0.29)		(0.34)		(0.37)		(0.41)
Distance from Equator		-0.0123		0.0160		-0.00186		-0.0509		-0.0983*
		(0.033)		(0.027)		(0.029)		(0.031)		(0.051)
Squared Distance from Equator		0.00129**		0.000704		0.00115**		0.00155***		0.00245**
		(0.00054)		(0.00042)		(0.00050)		(0.00049)		(0.0012)
Dummy for French Legislative Origin		-0.988***		-0.618**		-0.971***		-0.618**		-0.711**
		(0.28)		(0.24)		(0.28)		(0.26)		(0.27)
Private Credit / GDP			5.296	1.843***						
			(3.70)	(0.44)						
Log Settler Mortality										-0.608***
										(0.15)
Constant	62.11***	6.821***	66.36***	4.973***	59.36***	6.379***	51.32***	7.178***	57.53***	10.96***
	(5.78)	(0.98)	(7.15)	(0.56)	(6.01)	(0.91)	(5.62)	(0.95)	(6.68)	(0.91)
Sub-Saharan Africa							8.923***	-1.158***		
							(3.35)	(0.41)		
Latin America & Caribbean							6.226***	-1.809***		
							(2.10)	(0.42)		
Openness (1970-90 avg.)					0.0242	0.00381				
					(0.023)	(0.0063)				
Observations	64	64	55	55	64	64	64	64	44	44
R-squared	0.56	0.66	0.64	0.79	0.57	0.66	0.68	0.75	0.35	0.64
P-value of Hansen's J	0.269		0.615		0.391		0.891		0.550	
Cragg-Donald F-stat	26.34		16.03		26.08		17.56		12.94	
Critical-value for 10% maximal relative bias	10.27		10.27		10.27		10.27		10.83	

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. B-columns report first-stage regression results.

Table 3: The role of institutions and inequality in cross-country growth regressions

Dependent variable:	average per-capita income growth, 1970-1995							
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
initial log income	-1.322***	-1.695***	-1.859***	-1.835***	-1.172***	-1.691***	-1.908***	-1.809***
	(0.38)	(0.31)	(0.33)	(0.36)	(0.33)	(0.31)	(0.32)	(0.36)
private credit	2.669***	1.259**	1.405**	1.002	2.606***	1.029*	1.331*	0.938
	(0.72)	(0.58)	(0.58)	(0.66)	(0.82)	(0.61)	(0.66)	(0.64)
investment price	-0.0149	-0.00186	-0.00244	-0.00423	-0.00644	0.000650	-0.000979	-0.00160
	(0.0090)	(0.0071)	(0.0072)	(0.0056)	(0.0095)	(0.0078)	(0.0079)	(0.0050)
primary enrollment	0.0406***	0.0398***	0.0406***	0.0220*	0.0501***	0.0439***	0.0413***	0.0220*
	(0.0093)	(0.0095)	(0.0099)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
Income Gini	-0.0737***	-0.0394	-0.0326	0.00784				
	(0.024)	(0.024)	(0.026)	(0.031)				
Property Rights		0.784***	0.689***	0.786***		0.902***	0.796***	0.832***
		(0.19)	(0.22)	(0.28)		(0.18)	(0.22)	(0.28)
Political Rights		-0.0706*	-0.0658	-0.0290		-0.0664*	-0.0595	-0.0446
		(0.038)	(0.039)	(0.040)		(0.035)	(0.037)	(0.039)
Political Rights squared		-0.0121*	-0.0114	-0.00784		-0.0108	-0.0102	-0.00852
		(0.0069)	(0.0069)	(0.0064)		(0.0071)	(0.0071)	(0.0064)
Land Gini					-2.508	-1.088	-0.252	-0.802
					(1.51)	(1.17)	(1.36)	(1.21)
Observations	63	55	55	55	59	53	53	53
R-squared	0.50	0.70	0.71	0.79	0.42	0.68	0.70	0.80
Adjusted R-squared	0.45	0.65	0.65	0.72	0.37	0.63	0.63	0.73

Table Notes: Robust standard errors appear in parentheses below coefficient estimates. Significance noted as *** p<0.01, ** p<0.05, * p<0.1
Columns (3) and (7) include two geography controls, a dummy variable for being landlocked and distance from the equator. Columns (4) and (8) include regional dummy variables as controls

Table 4: Cross-Sectional Simultaneous Equations Regressions using Three-Stage Least Squares

	(1)		(2)		(3)		(4)		(5)	
Dependent:	**growth**	**inequality**	**growth**	**inequality**	**growth**	**inequality**	**growth**	**inequality**	**growth**	**inequality**
Covariate										
Initial log income	-1.656***		-1.848***		-2.123***		-1.850***		-2.252***	
	(0.34)		(0.26)		(0.35)		(0.35)		(0.42)	
Primary enrollment in 1970	0.0443***	-0.0732	0.0410***	-0.0471	0.0380***	-0.0345	0.0251**	0.0454	0.0411***	-0.0438
	(0.0083)	(0.051)	(0.0076)	(0.046)	(0.0083)	(0.055)	(0.011)	(0.056)	(0.012)	(0.075)
Investment Price	-0.00843		-0.00412		-0.00333		-0.00494		-0.00463	
	(0.0074)		(0.0070)		(0.0081)		(0.0096)		(0.0099)	
Private Credit	2.490***	-6.833*	1.757***	5.579	1.063	6.759	2.028**	7.812**	1.439	18.16*
	(0.71)	(3.58)	(0.64)	(3.42)	(0.86)	(4.34)	(0.83)	(3.67)	(2.05)	(9.65)
Income Inequality	-0.101**		-0.0590		-0.0276		-0.115		-0.0000875	
	(0.043)		(0.054)		(0.061)		(0.092)		(0.072)	
Land Inequality		17.21**		18.70***		18.31***		12.19*		24.57**
		(7.20)		(6.09)		(6.80)		(7.00)		(10.2)
Political Rights		-0.394***	-0.0374	-0.125	-0.0693*	0.175	-0.0383	0.145	-0.199**	0.327
		(0.14)	(0.024)	(0.13)	(0.038)	(0.20)	(0.034)	(0.18)	(0.078)	(0.40)
Property Rights			0.543**	-3.844***	1.040**	-4.912***	0.375	-4.257***	1.713**	-7.120**
			(0.27)	(0.74)	(0.48)	(1.27)	(0.37)	(1.36)	(0.79)	(2.82)
SS Africa dummy							-1.191	10.85***		
							(1.39)	(3.51)		
Latin America dummy							0.590	4.666		
							(1.26)	(3.26)		
Constant	15.62***	43.68***	11.68***	63.22***	9.525*	68.88***	16.78***	57.18***	4.662	76.75***
	(4.01)	(4.86)	(4.20)	(5.49)	(4.89)	(7.79)	(6.16)	(8.40)	(6.69)	(15.7)
Observations	59	59	53	53	52	52	52	52	35	35
R-squared	0.55	0.43	0.66	0.67	0.63	0.62	0.64	0.70	0.41	0.20
Excluded Instruments	None		None		Polity1960, French, European Language, distequat, dist2		Polity1960, French, European Language, distequat, dist2		Polity1960, French, mortality, distequat, dist2	
Endogenous Variables	Growth and inequality		Growth and inequality		Growth, inequality, polity1970, exprop		Growth, inequality, polity1970, exprop		Growth, inequality, polity1970, exprop	

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: A Fixed Effects Growth Regression with Country-Specific Intercepts using the Forbes (2000) specification

Dependent variable: per-capita GDP growth over subsequent 5-year period

Covariate	(1)	(2)
Initial log income	-3.718*** (0.502)	-3.718*** (0.493)
Female Years Secondary Schooling	0.419 (0.623)	--
Male Years of Secondary Schooling	0.550 (0.630)	--
Total Years of Secondary Schooling	--	0.976*** (0.279)
Price of Investment	-0.008* (0.004)	-0.008* (0.004)
Income Gini	0.085*** (0.029)	0.086*** (0.029)
N	450	450
Country Groups	88	88
R-squared	0.18	0.18

Note: *** represents significance at the 1% level; ** significance at the 5% level, and * significance at the 10% level in a two tailed test.

Table 6: Institutions and Country-Specific Intercepts

COEFFICIENT	(1)	(2)	(3)	(4)
Expropriation Risk	2.016***	2.101***	1.864***	1.408***
	(0.18)	(0.22)	(0.20)	(0.26)
Political Rights in 1970		-0.0391	-0.0423	0.00251
		(0.051)	(0.044)	(0.040)
Ethnic Fractionalization			-0.0457***	
			(0.010)	
Landlocked Dummy				-0.853
				(0.67)
Distance from Equator				0.0381
				(0.024)
Region Dummies				*Included*
Observations	72	68	68	67
R-squared	0.65	0.65	0.73	0.86
Adjusted R2	0.64	0.64	0.72	0.84

(Note: *** implies significance at the 1% level, ** at the 5%, * at the 1% in two-tailed tests)

Table 7: Institutions, Inequality and Growth using a GLS Random-Effects Estimator

Dependent variable is per-capita income growth over following 5-year period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log initial income	-3.718***	-0.489*	-0.751***	-1.402***	-1.399***	-1.874***	-1.275***	-2.267***	-1.508***
	(0.50)	(0.28)	(0.28)	(0.33)	(0.34)	(0.34)	(0.31)	(0.36)	(0.33)
Average years of	0.976***	0.274	0.330	0.264	0.263	0.408*	0.417*	0.515**	0.416*
secondary schooling	(0.28)	(0.22)	(0.21)	(0.22)	(0.22)	(0.21)	(0.22)	(0.21)	(0.22)
Price of investment	-0.00751*	-0.0131***	-0.0113***	-0.0251***	-0.0246***	-0.0251***	-0.0103**	-0.0216***	-0.00951**
	(0.0040)	(0.0039)	(0.0038)	(0.0054)	(0.0054)	(0.0053)	(0.0044)	(0.0053)	(0.0038)
Income Gini	0.0857***	-0.0285							
	(0.029)	(0.019)							
Income Gini (within)			0.0904***	0.0663**	0.0670**	0.0633**	0.0788***	0.0690**	0.0863***
			(0.030)	(0.031)	(0.030)	(0.030)	(0.030)	(0.030)	(0.029)
Income Gini (average)			-0.0917***	-0.0273	-0.0307	-0.0253	-0.0947***	0.0188	-0.0351
			(0.022)	(0.028)	(0.028)	(0.028)	(0.024)	(0.035)	(0.032)
Expropriation Risk				0.990***	0.969***	0.938***		1.202***	
				(0.19)	(0.19)	(0.19)		(0.21)	
Polity (time varying)					-0.00133				
					(0.024)				
Political Transition					-1.612**				
Dummy					(0.81)				
Landlocked Dummy						-1.454**	-1.474**		
						(0.62)	(0.60)		
Avg. Ethnic						-0.0245***	-0.0216***		
Fractionalization						(0.0074)	(0.0078)		
Region Dummies (6)								Included	Included
								F=43.4	*F=33.4*
Country Effects	Fixed	Random	Random	Random	Random	Random	Random	Random	Random

Observations	450	450	450	402	398	395	427	402	450
Country Groups	88	88	88	72	72	71	83	72	88
R-squared (within)	0.183	0.029	0.095	0.176	0.184	0.196	0.130	0.201	0.150
R-squared (between)	0.076	0.043	0.071	0.209	0.209	0.327	0.107	0.428	0.191
R-squared (overall)	0.011	0.042	0.089	0.199	0.209	0.270	0.149	0.356	0.206
Hausman chi-square (k=5)		92.12	63.1	19.48	22.4	4.02	50.09	0.85	40.53
Hausman test p-value		0.000	0.000	0.002	.0004	0.55	0.000	0.973	0.000