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Does Economic Inequality Promote Economic Growth? A Cross-National Time-Series Analysis

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Abstract

Over the past two decades, the belief that economic performance is best enhanced by freeing markets of government interference has become widely accepted. A powerful theoretical case had been made, and had persuaded politicians and planners to adopt public policy measures that increased the extent of inequality in society. But the argument that inequality stimulates economic growth was virtually devoid of empirical support. In fact, the research literature is divided on this point. Evidence from cross-national regression analysis suggests that inequality reduces growth, but the theoretical logic underlying this finding has been disputed. More recent research utilizing panel regression and better quality data has concluded that inequality increases growth. This suggests that negative effects found in cross-sections are spurious.

This paper uses annual time-series data on a sample of thirty-nine countries to investigate the impact of inequality on growth over the 1950-1998 period. Our inequality measure is the property-income share of GDP, selected because it is both the means and the motive for investment, the proximate cause of growth in most theories. Cross-national time-series regression analysis of the pooled data finds only limited evidence that inequality increases subsequent growth, and only in a few countries. There is no evidence that this effect can be generalized beyond these few nations. The argument that inequality promotes economic growth remains largely unsupported.

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I. Introduction

The last two decades of the twentieth century have witnessed the global expansion of neo-liberal economic policy, most notably drastic cutbacks in government regulation of economic activity. The belief that economic performance is best enhanced by freeing markets of government interference has become widely accepted among policy planners and politicians on both the left and right (for a classic statement of this position, see Friedman 1982).

The most visible examples of purportedly harmful government interference in market processes are social welfare and other re-distributive programs. These are widely regarded as both wasteful and harmful, diverting societal resources from more productive use and discouraging initiative and effort. Program outcomes may include a more equitable distribution of income, but at the cost of decreased economic performance. Neo-liberals "justify the economic inequalities associated with markets in terms of efficiency: By providing a greater incentive for individual hard work and initiative, freer markets are supposed to lead to more competitive national economies and a bigger economic pie for everybody" (Carruthers and Babb 2000, p. 102).

The neo-liberal argument has impressive academic credentials, a persuasive voice in public policy debates, and two decades of world-wide political successes. But it lacks empirical support. Simply put, there is very little evidence that increasing inequality is associated with improved economic performance. In this paper, we examine the empirical research linking inequality to economic growth, then provide new evidence from a cross-national time-series analysis.

II. Theory

There are strong theoretical grounds for believing that inequality affects subsequent growth and development. Lenski (1966, p.296) argues that "The surplus of any society is not determined solely by the means of production available to its members. It also depends on the nature of the distributive system. Given the same productive system and identical environments, several societies could have economic surpluses of very different size depending upon the skill and ruthlessness of the dominant classes". But whether inequality increases or decreases societal wealth depends on other aspects of social organization. In the absence of effective fertility control, for example, a more equitable distribution of surplus would likely be consumed by rapid population growth, thus not available for investment in future production. On the other hand, elites may " find themselves in the paradoxical situation in which they can maximize their net input of rewards by responding to pressures from below and making certain concessions. By granting the lower classes some share in the economic surplus, they can reduce worker hostility and the accompanying losses..." (P. 314). A more equitable distribution of surplus may reduce the rate of economic growth (by promoting the inefficient use of social resources), or may increase the rate of economic growth (by promoting social cooperation).

A trade-off between equality and growth is firmly embedded in neoclassical economic theory. In market economies, incomes are the incentives that allocate resources among alternative activities. Under standard neo-classical assumptions, all inputs are paid the value of their marginal product, so any redistribution of income will result in a less than optimal allocation of resources (Friedman 1982). Furthermore, because the rich save at higher rates than the poor,[1] any redistribution of income from rich to poor will deplete the funds available for investment. Because investment is funded from private wealth and is motivated by the expectation of private profit, a more equitable distribution will reduce both the capacity and the incentive to invest in

[1] In the United States, for example, those at the top of the wealth distribution are responsible for the bulk of national savings (Avery and Kennickell 1991).

future growth. Keynesian macro-economic theory, in contrast, suggests that shifting income from the rich to the poor increases aggregate demand, thereby increasing income growth if output is below its full capacity level. But if there is no excess capacity, increasing aggregate demand will only result in inflation (Samuelson and Nordhaus 1985).

An impact of equality on growth is also posited by Marxist economics. In both 'profit squeeze' and 'realization failure' theories of crisis in capitalist societies, distribution of income between classes is an important determinant of profit rates, thus also of investment in future production (Wright 1978; Weisskopf 1979). But whether increased equality between classes squeezes profits, thereby hindering economic growth (as in neoclassical theory) or boosts aggregate demand, thereby fostering growth (as in Keynesian theory) is contingent on the level of aggregate demand and the utilization of productive capacity (Sarkar 1993; Dutt 1987; Blecker 1989). This is also the case in Marxist business cycle theory (Sherman 1991). An increase in labor's share of income (decreasing inequality between classes) squeezes profits near business cycle peaks, when capacity is fully utilized, but boosts aggregate demand near business cycle troughs when there is excess capacity.

More recently, Alesina and Rodrik (1991) and Persson and Tabellini (1992; 1994) have developed political growth models in which tax policy is determined by the wealth of the median voter. The more unequal the distribution of wealth, the lower the wealth of the median voter and the higher the tax rate. Because incentives for investment depend critically on the private appropriation of returns from investment, "inequality is harmful for growth, because it leads to policies that do not protect property rights and do not allow full private appropriation of returns from investment" (Persson and Tabellini 1994, p. 617). Inequality is predicted to decrease growth rates, but only in democracies.

III. Prior Research

Research from the "Basic Needs" perspective on economic development[2], while not explicitly addressing effects of income inequality on growth, does suggest that directing substantial social resources towards the poor may increase rates of aggregate income growth in the industrializing societies (Streeten, et al. 1981; Moon and Dixon 1992; Firebaugh and Beck 1994). Findings for the advanced industrial countries are similar. Friedland and Sanders (1985) pool cross-sectional and longitudinal data on twelve advanced industrial nations between 1962 and 1983. They find that, as a percent of GDP, income transfers to households increase short-run GDP growth rates, while income transfers to firms do not. They suggest that, consistent with Keynesian theory, equalizing income transfers did promote aggregate income growth by increasing aggregate demand.

The most recent flurry of research on inequality and growth was initiated by Alesina and Rodrik (1991) and Persson and Tabellini (1992; 1994). Alesina and Rodrick (1991) use cross-national regression to test their political growth model in which inequality affects growth through the electoral process. Although they model the relationship between wealth inequality and growth, they use quintile income shares as the key independent variable because measures of wealth distribution are "unavailable for a sufficiently large sample of countries" (1991, p. 25). Their sample consists of 64 countries (24 democracies, 43 non-democracies) with data on per-capita GDP in 1960 and 1985, the primary-school enrollment ratio in 1960, and quintile income shares "dated reasonably close to 1960" (p. 25). Their dependent variable is the average percent growth in per-capita GDP , 1960-1985. They find, as predicted, that inequality reduces growth rates in the sample of democracies, but has no effect in the sample of non-democracies.

Persson and Tabellini (1992) use cross-national regression to test a similar model. They

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[2] This perspective argues that there is no necessary trade-off between equality and growth in the developing nations. Rather, strategies to alleviate poverty -- including the provision of adequate nutrition, housing, education, and medical services -- are also human-capital investments that increase labor productivity and promote long-run growth (Streeten, et al. 1981).

add measures of both income equality (the income share of the middle quintile) and wealth inequality (the Gini coefficient for land ownership) to the variables used by Alesina and Rodrik (1991), and control for the percent of the labor force in agriculture. The inclusion of these additional variables reduces their sample to 36 countries. Their results are similar, however. Income equality increases, and land inequality decreases, the 1960-1985 GDP growth rate, but only in democracies.

Persson and Tabellini (1994) test their model with a historical sample (9 industrial countries, 1830-1985) and a post-war sample (56 countries, 1960-1985). For the historical sample, the 1830-1985 period was divided into 20-year subperiods. The per-capita GDP growth rate was calculated for each sub-period, and the independent variables were measured at the start of each subperiod. Independent variables include income inequality (the personal-income share of the top quintile), political participation, a schooling index, and a measure of development (the gap between per-capita GDP and the highest per-capita GDP in the sample during the same sub-period). The post-war sample is essentially the same as that used in their previous (1992) research. Regression analysis of the pooled historical data and the cross-national post-war data produced essentially the same result: Income inequality reduces growth rates (political participation had no effect in the pooled historical data) .

Extensive reviews of the research on inequality and growth have recently appeared in the literature (Benabou 1996; Aghion, Caroli, and Garcia-Penalosa 1999). Benabou (1996) observes that recent cross-country growth regressions “run over a variety of data sets and periods with many different measures of income distribution, deliver a consistent message: initial inequality is detrimental to long-run growth” (p. 13). However, he argues that the evidence does not support the political explanation for this finding. The empirical finding that effects differ between democracies and non-democracies have not been replicated, and the income share of the 3rd (or the sum of the 3rd and 4th) quintile has no consistent impact in growth regressions.[3] Furthermore, the hypothesis underlying the political explanation, that inequality reduces growth because inequality increases redistribution, is not supported: measures of redistribution are consistently positive in growth regressions (p. 25).

Aghion, Caroli, and Garcia-Penalosa (1999) observe that in the textbook case there is a positive impact of inequality on economic growth. They note, however, that recent cross-national analyses find the opposite – inequality has a negative impact on growth (see also Temple's (1999) review of growth theory and evidence). This literature is "impressively unambiguous, since they all suggest that greater inequality reduces the rate of growth. Such a result comes as a surprise both with regard to traditional theories in the field and to the channels through which inequality might affect the growth process.... The underlying theory is that wealth inequality determines investment in physical or human capital, which in turn affects the long-run growth rate" (p. 1617). Redistribution, by lowering inequality, should thereby lower growth, thus also lower incentives to invest, further lowering growth. The empirical evidence is not consistent with these arguments, however. In the standard political explanation of the unexpected negative impact of inequality on growth, this effect is attributed to redistributive government policies. However, they note that redistribution has been found to increase, not decrease growth rates. Aghion, Caroli, and Garcia-Penalosa (1999) suggest that these unexpected findings are best explained by imperfections in capital markets: in imperfect capital markets, investment is proportional to wealth, so redistribution creates investment opportunities for those unable to borrow sufficient capital.

Forbes (2000) disputes the findings that inequality reduces growth. She argues that this research is deficient in several respects. First, all utilize cross-national regression. The problems that plague cross-national growth regression have recently received a great deal of attention

[3] Benabou notes "the lack of almost any data on the distribution of wealth ... even though in most theories it is this distribution rather than that of income which is determinant" (p. 18, fn. 5). Aghion, Caroli, and Garcia-Penalosa (1999, p. 1617) make note of this, also.

(Temple 1999). Of particular concern in this context is omitted-variable bias. Because of the paucity of relevant data, omitted variables are likely to be correlated with both inequality and growth. Forbes (2000, p. 3) observes that in prior research the inclusion of regional dummy variables eliminates any effect of inequality on growth: "... since the regional variables render the coefficient on inequality insignificant, this suggests that the coefficient on inequality may actually capture the effect of these omitted variables on growth" (p. 5).

Second, this research relies on unreliable inequality data: "Coverage is generally uneven, and there is a lack of consistency in the definition of income and the unit of account. As a whole, while most studies acknowledge that inequality statistics are plagued with measurement error, they also admit that since no good instrument for inequality exists, it is difficult to correct for this problem" (pp. 4-5). Third, the prior research was unable to model the impact of inequality on growth within countries.

Forbes (2000) addresses these issues by using the "high-quality" inequality data compiled by Deiniger and Squire (1996). These data are more consistent than those used in prior research, and cover a wider range of countries and years. In addition to improved comparability, these data permit the use of panel estimation techniques that can partially mitigate the confounding impact of omitted variables on the inequality-growth relationship by controlling for country-specific effects. Her data cover the period 1966-1995, and means are calculated for five-year intervals. This produces a six-wave, 45 country panel dataset with measures of economic growth (first-difference of the log of per-capita GNP), initial income (log of per-capita GNP), income inequality (Gini coefficient), male and female schooling (mean years of secondary schooling), and a proxy for market distortions. Fixed-effects and GMM estimation of the models used in the prior research find a positive and significant impact of inequality on growth. Forbes (2000) then re-estimates her models with the low-quality data and/or OLS regression to locate the sources of discrepancies between her findings and the prior research. She finds that both data quality and panel estimation techniques (i.e. controlling for country-specific effects) are important.

Barro (1999) also estimates panel regressions (3SLS) with the Deininger and Squire (1996) “high-quality” data, but adds an additional 48 observations he considers acceptable (p. 17). His findings generally agree with Forbes (2000). He does not include individual country effects, but controlling for the total fertility rate changes the inequality effect from negative to zero, and there are significant differences between high-income and low-income countries: the inequality effect is negative for low-income countries, positive for high-income countries. He concludes that the negative inequality effect reported in prior research is spurious (pp. 20-21), and that “inequality retards growth in poor countries but encourages growth in richer places”(p. 32).

Raffalovich, Wallace and Leicht (1995) use quarterly time series data to investigate the relationship between income inequality and income growth in the United States. Their inequality measure is the profit-share of value added, because they are interested in “the power of a propertied elite to appropriate the benefits of economic growth and control the economic prospects of other members of society” (p. 2). With vector autoregression techniques, they find a large positive impact of income growth on the profit share and also find a small positive impact of the profit share on subsequent income growth. With respect to the latter, they conclude that although inequality does increase subsequent growth, the small magnitude of this effect suggests that there are other, more important mechanisms for generating growth. “Elite assertions regarding the importance of increasing profitability for sustained economic growth are clearly overstated” (p. 18).

In summary, prior research using cross-national growth regression has consistently found a negative impact of inequality on growth. Serious questions have been raised about the validity of these findings. Panel regressions with better-quality data and controls for country-specific effects eliminate the negative effect of inequality or change the sign of the coefficient. This suggests that these prior research findings may be spurious. Research that estimates the within-country impact of inequality on growth consistently finds that inequality increases growth. It is therefore crucial to control for unmeasured sources of between-country differences in growth rates, and to estimate within-country effects if data permit.

IV. The Current Research

We use annual time-series data on a sample of thirty-nine countries to investigate the impact of inequality on growth over the past half-century. Growth is measured as annual percent change in real GDP. Inequality is measured as the property-income share of GDP. This measure was selected because investment is funded from wealth and is motivated by the expectation of future profit from investment. Property income therefore provides both the means and the motive for investment in future growth. Unlike the inequality measures used in most prior research, the property-income share directly links inequality to investment, the proximate causal mechanism generating economic growth in most theories.

The property-income share, measured as the ratio of operating surplus to gross domestic product, is from the United Nations' National Accounts Statistics (United Nations 1998).[4] Real economic growth, measured as the annual percent change in real gross domestic product, is from the International Monetary Fund's International Financial Statistics (International Monetary Fund 2000). The countries included in this research are all those with a minimum of 25 consecutive annual observations on both the property-income share and the GDP growth rate.

In these analyses, we estimate only reduced-form (total) effects of inequality on growth. The analytic strategy is as follows: First, for each of the countries in the sample, we conduct Granger causality tests between the property-share and growth time series. We then estimate fixed-effects cross-national time-series regression models for the pooled data. Preliminary analyses with these data found that the GDP growth rate is stationary autoregressive (AR1) within countries, but that the property-income share is non-stationary. Our analyses therefore use

[4] In the United Nations' system of national accounts, operating surplus is “defined as the return to providers of all forms of capital” (Carson and Honsa 1990, p. 26). Because the property-income share is the ratio of operating surplus to GDP, we need not be concerned with exchange rates.

the first-difference (i.e., annual change) of the property-share (see Raffalovich 1994). Thus, we model the impact of annual change in property-share on the annual percent change in real GDP

"Granger causality" between two variables, X and Y, means that past values of X are useful (in the sense of reducing prediction error) for predicting future values of Y. Because the past behavior of any variable is typically a good predictor of its future behavior, the test controls for past values of Y. The "Granger causality" test between X and Y estimates two equations, one for each variable. The explanatory variables in each equation are identical: lagged values of both variables. 'X causes Y' means that net of the lagged values of Y, lagged values of X significantly improve prediction of current Y; and 'Y causes X' means that net of the lagged values of X, lagged values of Y significantly improve prediction of current X. Tests of these causal hypotheses therefore require estimation of unrestricted and restricted models, then calculating an F-statistic or likelihood ratio chi-square statistic (Mills 1990). The null hypothesis of no causal relationship is rejected if the associated p-values are less than a specified Type-I error rate.

With pooled data, we then estimate cross-national time-series regression models of the form:

$$Growth_{it} = \alpha_i + \beta_i Growth_{i,t-1} + \sum_{k=1}^{4} \gamma_{i,k} \, \Delta Property\text{-}share_{i,t-k} + \epsilon_{it} \qquad (1)$$

where $Growth_{it}$ is the real GDP growth rate for country ***i*** in year ***t*,** $Growth_{\mathbf{i,t-1}}$ is the prior year's growth rate for country *i*, ΔProperty-share$_{\mathbf{i,t-k}}$ is the first-difference (annual change) in property-income shares for country *i* in year ***t-k*,** $\boldsymbol{\alpha}_{\mathbf{i}}$ is the intercept for country ***i*,** $\boldsymbol{\gamma}_{\mathbf{i,k}}$ is the effect of the property-share for country *i* lagged *k* years on the GDP growth rate for country *i*, and $\boldsymbol{\epsilon}_{\mathbf{it}}$ is the stochastic term. We include the prior year's growth rate to model the AR(1) behavior of the dependent variable, noted above.

We use this model to test a variety of specifications and to estimate parameter values. Two types of specifications are tested: lag length and equality restrictions. These tests are designed to answer two types of questions: how long does it take for inequality to affect growth (if at all), and does the relationship between inequality and growth vary among countries? Only if the relationship is invariant across countries can cross-sectional results be believed (Firebaugh 1980). We estimate models with lags of from 1 to 4 years. We do not include a zero-lag (i.e. an immediate effect of property-share on growth) because the processes involved, whether through investment or some other mechanisms, take time. Our model assumes that this length of time does not exceed four years. For each lag length, we impose equality constraints on the effects of property-shares; specifically, we constrain these effects to be the same for all countries. Both types of specification tests impose restrictions on sets of parameters, and these restrictions are evaluated with the likelihood-ratio test.

Because there are many candidate models, and multiple tests are conducted, model selection procedures must be able to account for Type-I error rates that exceed nominal significance levels (Lovell 1983). Raftery's (1995) Bayesian Information Criterion (BIC) is therefore used in addition to the standard likelihood-ratio test to direct model selection. In contrast to standard statistical testing procedures which evaluates evidence against the null hypothesis, Raftery's (1995) BIC evaluates evidence in favor of the alternative hypothesis. BIC < 0 means that the evidence favors the alternative hypothesis, whereas BIC > 0 means that the evidence favors the null.

Application of the BIC criterion requires that we first select a baseline or null model to which all other models can be compared. The BIC statistic then assesses model fit relative to this null model. The BIC statistic for the null model is zero, by definition. One model is preferred to another only if the associated BIC statistic is smaller, so the null model is preferred to any model with BIC>0. Because of its obvious importance in assessing model fit, the baseline model should captures the minimum prior information we have about the distribution of growth rates over countries and time. Our null model contains country-specific intercepts, and country specific effects of the lagged dependent variable. This is the minimum information we think necessary to model cross-national and time-series variation in growth rates.

V. Findings

Results from the Granger causality tests are presented in Table1. Although the tests are bi-directional, we present only results relevant to the hypothesis that inequality causes growth.[5] The null hypothesis is the absence of causation: thus the null hypothesis is that inequality does not cause growth. In Table 1, the dependent variable is the real GDP growth rate. The independent variables are lagged values of both growth and property share. Tests were conducted for lags of up to 4 years (with a lag length of 4 years, there are eight independent variables not counting the intercept: 4 lags each of inequality and growth).

Table 1 about here

The entries in Table 1 are p-values, and a ‘*’ flags p<.01. Thus, a ‘*’ signifies rejection (at p<.01) of the null hypothesis of no causation. For Australia, for example, we reject the null hypothesis that inequality does not cause growth with a lag of 1 year (i.e. we conclude that growth does cause inequality with a lag of one year). For Australia, we would also conclude that inequality causes growth with a lag of two years or less (column 2), with a lag of three years or less (column 3), and with a lag of four years or less (column 4). Importantly, this does not imply that the causal effect operates at a four year lag, only that the combined effect of the four lagged variables significantly reduce prediction error in GDP growth. Australia is an exception in these data, together with Canada and Greece. For the vast majority of countries, we cannot reject the hypothesis of no causation from inequality to growth. However, each test is based on a single, relatively short time series. Perhaps a larger sample, thus a more powerful test, would reach a different conclusion. One way of increasing the sample size is by pooling the information for all countries and all years. The pooled data contain 1249 observations with valid values of growth

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[5] Prior research has shown that, at least for the industrialized countries, growth does increase the property-income share (Raffalovich 1999; Raffalovich and Vesselinov 1999).

and property-shares, including one-year lags on both variables (longer lags decrease the sample size by 1 observation per country per year). The data are unbalanced (i.e. not all countries have the same number of observations, and not all countries have valid observations for the same years). The maximum number of valid observations for each country are presented in the final column of Table 1.

Model specification and summary statistics for the fixed-effect time-series regression models are presented in Table 2, and model comparison results are presented in table 3. Because we compare models with different lag structures, the number of valid cases will vary from one model to another, and because different countries have valid data for different years we may confound differences in model specification with differences in sample size and sample composition. To avoid this, we conduct these tests on those countries and years with valid data for all variables, including up to 4-year lags. This reduces our sample to 663 observations: 39 countries and 17 years (1975-1991).

Table 2 about here

Table 2 presents the baseline null and eight alternative models. As noted above, all models contain country-specific intercepts to control for unmeasured between-country differences in growth rates. In addition, preliminary analyses with these data found that growth is autoregressive within many countries, so all models include a one-period lag on the dependent variable. The effects of the lagged dependent variable are modeled as country-specific to capture country-specific growth dynamics. With this specification, Ljung-Box Q tests (Mills 1990) on model residuals do not reject the hypothesis of no autocorrelation for any country.

Model 1 expands this specification by including country-specific effects of lagged change in property-shares. Model 2 expands on Model 1 by including country-specific effects of two-year lags of change in property-shares. Model 3 and Model 4 further expand on Model 2 by including country-specific effects of three-year and four-year lags, respectively.

Model 5 is the same as Model 1, but restricts the effects of lagged property-share to be constant across countries. Likewise, Model 6, Model 7, and Model 8 are the same as Model 2, Model 3, and Model 4, respectively, with the imposition of these cross-country equality constraints on the effects of property-shares. The log-likelihood statistics reported in Table 2 will be used for model comparisons in Table 3, below. We note now, however, that the BIC statistics reported in Table 2 indicate that the null model is preferred to all others. That is, by the BIC criterion, there is no effect of inequality on growth at any lag.

Specific model comparisons are presented in table 3. There are three panels in this Table: Panel A tests lag-length with no cross-country parameter restrictions. Panel B tests lag-length with cross-country equality constraints on lagged effects. Finally, panel C tests cross-country equality constraints for models with one-year lagged effects and for models with one- through four-year lagged effects.

Table 3 about here

Looking at Panel A, the first comparison (Model 1 vs. the baseline Model 0) tests the null hypothesis of no impact of property-share on growth against the alternative of a one-year lagged effect that differs among countries. The likelihood-ratio χ^2 tests the null hypothesis that <u>all</u> 39 parameters are zero (no effect for any country) against the alternative that <u>one or more</u> of these 39 parameters are not zero (an effect for <u>some</u> country). We can reject the hypothesis of no effect for any country at p=.002 (the probability of Type I error – of wrongly rejecting this null hypothesis – is 2/1,000). But it has long been known that in the context of multiple hypothesis tests – there are seven tests in Panel A – the calculated p-value can greatly understate the true Type I error rate (Lovell 1983). In contrast to this standard statistical testing procedure which provides evidence <u>against</u> the null hypothesis, Raftery's (1995) BIC provides evidence <u>in favor of</u>

the alternative hypothesis. BIC < 0 means that the evidence favors the alternative hypothesis, whereas BIC > 0 means that the evidence favors the null. In the comparison of Model 1 vs the null Model 0, BIC = +184.8 is evidence <u>for</u> the null hypothesis of no effect for any country, compared to the alternative hypothesis of country-specific effects of property-share on growth at a one-year lag Thus, the evidence does not support the hypothesis of 39 different effects of property-share on growth. We can conclude that there is no one-year lagged effect of property-share on growth, or that the hypothesis is poorly specified: perhaps all effects are the same, or effects are different for different groups of countries, or there are effects for some countries but not for others. Because BIC evaluates evidence in favor of a specific alternative hypothesis, the calculation imposes a penalty for extraneous parameters: all else the same, parsimonious hypotheses are favored.

The likelihood-ratio test rejects the null of no effect of property-share against the alternative of country-specific one-year lagged effects (Model 1 vs. Model 0, p=.002) and also against the alternative of country-specific effects at lags of one through four years (Model 4 vs. Model 0, p=.001). The BIC rejects both of these alternatives (BIC = +184.8 and +796.7, respectively). But Model 1 contains 39 additional parameters compared to Model 0 (one for each country), and Model 4 contains 156 extra parameters (four for each country). A more parsimonious specification of the alternative hypotheses is that the effects of property-share on growth are the same for all countries. These alternative specifications are tested in Panel B of Table 3. The first comparison of Model 5 vs. the baseline Model 0 tests the same null hypothesis of no effect against the alternative of a one-year lagged effect constant across countries. The likelihood-ratio test does not reject this null at the .01 significance level (p=.036), and the BIC likewise favors the null. But note that, according to BIC, the evidence for the null model compared to this alternative Model 5 is much weaker than the evidence for the null model compared to Model 1. Model 5 is more parsimonious than Model 1.

Likelihood-ratio test results in Panel A reject the null hypothesis of no effect against the alternative of country-specific one-year lagged effects (p=.002) and against the alternative of country-specific effects at lags of one through four years (p=.001). BIC favors the null model in both instances. In Panel B, neither criterion favors the alternative hypothesis of lagged effects. Panel C of Table 3 compares the more parsimonious with the less parsimonious specifications at lags of one-year and at lags of one through four years. The first comparison in Panel C (Model 5 vs. Model 1) tests the hypothesis of no differences among countries in one-year lagged effects of property-share on growth. The likelihood-ratio test rejects this hypothesis (p=.005), suggesting that there are some country differences. BIC, on the other had, finds the evidence strongly favors this hypothesis, suggesting that Model 1 (all country differences) is over-parameterized. Similar results are found for the comparison of Model 8 (country-specific effects at one- through four-year lags) vs. Model 4 (these lagged effects do not differ among countries).

The evidence in Table 3 suggests that there may be effects of property-share on growth, but that the estimated models may be either over-parameterized (Models 1 through 4) or too parsimonious (Models 5 through 8). To cast additional light on these issues, we present Maximum-likelihood estimates of the Model 1 parameters in Table 4, and of Model 5 parameters in Table 5. For this estimation, we used all available data (N=1249 cases).

Table 4 about here

Table 4 presents the Maximum-likelihood estimates of all 117 parameters in Model 1 (country-specific intercepts, country-specific effects of one-year lagged growth rates, country specific effects of one-year lagged property-shares). Parameter estimates are presented along with associated t-ratios, and t-ratios with p<.01 are flagged. There is substantial evidence here of the autoregressive behavior of the real growth rate noted earlier from preliminary analyses. Almost all autoregressive parameters are positive (as would be expected), and a third are significant at the .01 level. Findings for the impact of property-share are much weaker. Of the 39 estimated effects (one for each country) only 3 are significant at the .01 level. Of these, two are positive (Australia and UK) and one is negative (Greece).[6]

Model 5 results are presented in Table 5. In contrast to Table 4, the equality constraint imposed by Model 5 means that there is one pooled estimate of the impact of property-shares

Table 5 about here

on growth instead of 39 country-specific effects. Here, property-shares have no appreciable impact of growth.[7] The estimated effect is positive, but statistically indistinguishable from zero at the .01 significance level.

VI. Discussion

A variety of economic and sociological theories predict that inequality has an impact on economic growth, but have different predictions regarding the direction of this effect. The prior research literature is also divided on this point. Evidence from cross-national regression analysis suggests that inequality reduces growth, but the theoretical logic underlying this finding has been disputed. More recent research utilizing panel regression and better quality data has concluded that inequality increases growth rates. This suggests that negative effects found in cross-sections are spurious, and that it is crucial to control for unmeasured sources of between-country differences in growth rates. Finally, research on the impact of inequality on growth within individual nations has found a positive effect.

The inequality measures used in this prior research have almost exclusively been calculated from data on total individual or household income. The theories linking inequality to

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[6] At the .05 significance level, two additional effects are significant: Mauritius (negative) and South Africa (positive).

[7] The t-ratio of 1.72 corresponds to 2-tailed p=.085.

growth, however, have focused almost exclusively on investment as the proximate causal mechanism. Although it is widely recognized that data on the distribution of wealth is more relevant to these theories, insufficient wealth data were available for analysis. In addition, because of the paucity of data on the distribution of total income, the prior research that did study the longitudinal impact of inequality on growth had to aggregate measures of inequality and growth over five- or ten-year periods. These panel datasets therefore had only a few observations for each country.

The research reported here overcomes some of these limitations. First, we use an inequality measure, the property-income share, that directly links inequality to investment, the proximate causal mechanism generating economic growth in most theories. Second, we use annual time-series for each country over a period of several decades. With more adequate measures and considerably more data on within-country variation, we find no support for the argument that inequality reduces growth, at least among the 39 nations in this sample. This finding is consistent with all other research that examined the within-country effect of inequality on growth. Although we do find very limited evidence that inequality increases subsequent growth in a few countries, there is no evidence that this effect can be generalized beyond these few nations.

Differences between likelihood-ratio and BIC evaluation of the evidence for and against specific models suggests that while our models of country-specific effects are clearly over-parameterized, our more parsimonious models of no between-country differences may be under-parameterized. There may well be effects of inequality on growth that differ among countries, as suggested by the likelihood-ratio tests, but BIC suggests that each country is not unique. Rather, we need to direct attention to patterned ways in which countries differ from one another that affect the inequality-growth relationship. Raffalovich and Vesselinov (1999), for example, found that measures of institutional structure, such as corporatism, affect the impact of growth on inequality, and others have argued that the effect of inequality on growth differs between democratic and authoritarian political systems (Alesina and Rodrik 1991; Persson and Tabellini 1992; 1994). Recently, Henisz (2000) presented evidence that political stability and structural policy constraints affect rates of economic growth. These are likely avenues for future research.

This paper set out to examine the empirical evidence linking inequality to growth. A powerful theoretical case had been made, and had persuaded politicians and planners to adopt public policy measures that increased the extent of inequality in society. But the neo-liberal argument that inequality stimulates economic growth was virtually devoid of empirical support. The results reported in this paper contribute to that conclusion. The evidence that inequality has an important role in promoting economic growth is extremely tenuous.

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