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Does Inequality Harm Growth Only in Democracies?

A Replication and Extension

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Late 18th-century and 19th-century political theorists viewed democracy as incompatible with the security of private property necessary for investment and economic growth to occur. Their intuition can easily be phrased in terms familiar to modern-day theorists of political economy. With a rightward-skewed distribution of income and wealth, the median income in society will fall short of the mean income. Under majority-rule voting on redistributive issues, the median-income voter will have an incentive to ally with the poorer half of voters to transfer income and wealth from the richer half.

In Federalist #10, Madison argued that "...the most common and durable source of factions has been the various and unequal distribution of property. Those who hold and those who are without property have ever formed distinct interests in society." In the absence of sufficient checks and balances on popular majorities, "...democracies...have ever been found incompatible with personal security or the rights of property; and have in general been as short in their lives as they have been violent in their deaths." In a letter to Jefferson, John Adams predicted that "Democracy will envy all, contend with all, endeavor to pull down all..." Similarly, universal suffrage was deemed to be incompatible with property rights by David Ricardo, J.S. Mill, Thomas Macaulay, Karl Marx, and Daniel Webster, among others.

Modern-day theorists have formalized such thinking in models of political redistribution in democracies. For example, Meltzer and Richard (1981) develop a model in which the income tax rate chosen by the median voter is higher when income inequality is higher.

Persson and Tabellini (1994) add this political redistribution mechanism to an overlapping generations model with an intertemporal externality permitting endogenous growth, generating the prediction that investment and growth are decreasing functions of inequality in democracies. For nondemocracies, they argue, the predicted relationship between inequality on the one hand, and investment and growth in the other, is ambiguous. The median voter is not decisive in those polities, as decisions are not made through majority-rule voting. The redistributive policy favored by the decisive individual or group, they claim, may have very little to do with the distribution of income and wealth in nondemocracies.

Persson and Tabellini test their hypotheses using cross-national data. They find, as predicted, a negative relationship between government transfers as a proportion of GDP (averaged over 1960-81) and the share of national income accruing to the middle quintile of income earners (as measured circa 1960). With only 13 observations, this relationship is not statistically significant, however.¹ They similarly find a negative but insignificant effect of transfers on growth.

Given this weak evidence on the direct links between inequality and transfers, and between transfers and growth, Persson and Tabellini turn to indirect tests examining the impact of inequality on economic performance. Using time-series cross-sectional data for nine developed nations going back to 1830, they find inequality is significantly associated with lower growth rates. They report, however, that the strength of inequality's dampening effect on growth does not increase with the enfranchised proportion of the electorate -- as one would expect if democracy impairs growth through politically-driven redistribution. Additionally, they report that when time dummies are added, inequality no longer is significantly associated with growth. In other words, at a given point in time, there is little cross-country correlation between the two variables.

Empirical evidence in favor of the Persson and Tabellini model thus rests crucially on their analysis of a postwar cross-section of 49 nations, in which they conclude that a significant and negative correlation exists between inequality and growth only within their subsample of 29 democracies. If inequality influenced growth through channels other than the political distribution mechanisms they posit, one would expect a similar correlation between inequality and growth in their subsample of 20 nondemocracies. They claim to show there is no such correlation within these nondemocracies. The fact that "this relation is only present in

¹They use only data on OECD nations, on the grounds that data on transfers are poor for other countries. If larger samples are used, egalitarian income distributions are generally positively associated with transfers, counter to the Persson and Tabellini thesis (Keefer and Knack, 1995).

democracies" is cited by Persson and Tabellini as the key piece of empirical evidence in favor of their theory.

Table 1, equations 1 and 2 replicate these key results from Persson and Tabellini (p. 612). The dependent variable is average annual growth in per capita incomes over the 1960-85 period (GR6085). Independent variables, standard in the growth literature² include per capita GDP levels in 1960 (GDP60), as a measure of catch-up growth opportunities, and the percentage of the relevant age group enrolled in primary school in 1960, as a measure of human capital (PRIM60). The income distribution variable (MIDDLE) is the percentage of national income received by the middle quintile of the population. The higher this value, the more egalitarian is the distribution of income.

My initial attempts at replication failed, although results were not dramatically different. I obtained from Persson a printout of values for all of his variables except GDP60. His values for MIDDLE perfectly matched those contained in Paukert (1973), the source cited by Persson and Tabellini. Values for PRIM60 for nine countries differed from those contained in the dataset used (and widely distributed) by Barro (1991), and which I used in my initial replication attempt. Values in Persson's printout turned out to correspond exactly with those printed in the 1984 *World Development Report*, cited by Persson and Tabellini as their source for PRIM60.³ I was then able to perfectly replicate the results for the democracy sample.

Among the nondemocracies, Persson's value for GR6085 for Brazil of 4.79% differed substantially from Barro's 3.52%, despite being derived from a common source (Summers and Heston, 1988); all other values for GR6085 matched.⁴ Minor differences in results remained

²See, for example, Barro (1991).

³The 1984 *WDR*, in turn, cited various years of the UNESCO Yearbook as its source for enrollment data. Barro cites UNESCO Yearbook and an International Labor Organization publication.

⁴Summers and Heston periodically revise their estimates of per capita GDP. Brazil's annual growth rate for 1960-85 as calculated using a recent update (version 5.5 of their dataset) is only 3.24%. Neither the original Persson-Tabellini results nor my extensions here are sensitive to correcting the (apparently) mistaken values for GDP60 and GR6085 they use for Brazil.

after substituting Persson's GR6085 value for Barro's. By process of elimination, I concluded these differences were attributable to discrepancies in one or more values of GDP60 -- which were not reported in Persson's printout.

Summers and Heston report only per capita GDP levels; researchers compute their own growth rates from these levels. A possible source, then, of the discrepancy in GR6085 values for Brazil was different values for GDP60. By using the value for 1985 per capita GDP contained in Barro, and Persson's value for GR6085, I computed an implied value for GDP60 for Brazil which differed from that contained in Barro by several hundred dollars. After substituting this implied value of GDP60 for Brazil in place of Barro's, I was able to replicate the Persson and Tabellini nondemocracy findings sufficiently closely that the trivial remaining differences could be attributed to rounding.⁵

Two simple checks on these Persson and Tabellini results strongly suggest that the differential impact of inequality on growth in democracies and nondemocracies is entirely an artifact of measurement error. I conclude that the hypothesis that inequality harms growth as much in nondemocracies as in democracies cannot be rejected.

The first correction for measurement error deletes the seven observations -- all autocracies -- on inequality that Paukert (1973, p. 125), the sole source of inequality data for Persson and Tabellini, warns are of "rather doubtful value."⁶ If autocracies tend to be poor, and poverty and closed political systems generate less reliable data, we are less likely to detect a "true" relationship between inequality and growth among nondemocracies than among democracies. The fact the seven observations Paukert questions are all autocracies is consistent with this conjecture.

⁵Growth rates in Barro are reported out to two fewer decimal places than in Persson's printout. Given the error with which income levels across countries are surely measured, little if anything is lost by rounding to two decimal places.

For easier interpretation of coefficient values, I use GDP60 in thousands of dollars and PRIM60 as a proportion; Persson and Tabellini used dollars and percentages, respectively.

⁶The seven countries are Burma, Chad, Morocco, Niger, Nigeria, Sudan and Tunisia.

Deleting these seven suspect observations in fact increases the coefficient on MIDDLE somewhat for the autocracies (Table 1, equation 3). Given the resulting small sample size, a more informative test employs the combined sample of democracies and autocracies, in which inequality is interacted with a democracy dummy. The coefficient on this interaction term drops substantially upon deletion of these seven autocracies (Table 2, compare coefficients on MIDDLE*DEMOC in equations 1 and 2), and the difference in MIDDLE's impact across the two regime types is no longer statistically significant.

The second correction concerns the highly dubious regime type classifications of Persson and Tabellini, who include El Salvador, South Korea, Madagascar, Mexico, Panama, the Philippines, and Senegal among their democracies. Gurr's (1990) *Polity II* dataset indicates whether the chief executive of countries is elected competitively, and whether the legislature is an effective check on the executive. Of these seven dubious democracies, only the Philippines ever had an effective legislature (for 12 years, out of 26) in the 1960-85 period in question. Only El Salvador, Korea, the Philippines, and Senegal ever chose their chief executive through competitive elections during the period, and only for 10, 10, 12, and 2 years, respectively. Many nations classified as autocracies by Persson and Tabellini actually have more effective legislatures and electorates than these "democracies." Argentina (12 years), Chile (13), Ecuador (12), and Sierra Leone (12) all chose their chief executives through competitive elections during about half of the period. Argentina (3 years), Brazil (3), Chile (12), Nigeria (4), and Peru (5) sometimes had an effective legislature.⁷

Re-classifying these seven authoritarian regimes as nondemocracies (while retaining the seven poor-data-quality observations identified above) substantially alters the coefficients on

⁷Persson and Tabellini cite Banks (1987) and Taylor and Jodice (1983) as their sources for classification of regime types, with no further details. The nearest Taylor and Jodice come to such a classification is a table listing Gastil's ratings of civil liberties and political freedoms for the 1970s. These ratings conform much more closely to my Gurr-based classification than to Persson and Tabellini's, however. Banks does not classify regime types. The recent histories of various countries described therein do not appear to support the Persson-Tabellini classifications -- South Korea's entry, for instance, is a long narrative of authoritarianism.

MIDDLE for the two subsamples (compare equations 4 and 5 in Table 1 to equations 1 and 2 respectively). In the combined sample, the coefficient on MIDDLE*DEMOC drops by nearly one-half, and again is no longer significantly different from zero (Table 2, compare equation 3 to equation 1).

Differences between democracies and nondemocracies in inequality's growth effects disappear entirely when the two corrections are made together, i.e. when regimes are re-classified and the seven suspect observations on MIDDLE are dropped. The coefficients and t-statistics on MIDDLE are nearly identical for the two samples (equations 4 and 6 in Table 1). The coefficient of MIDDLE*DEMOC drops precipitously in the interaction specification (Table 2, equation 4).

Any dichotomous classification of regime type is admittedly too simple. As an alternative to the Persson-Tabellini and Gurr-based dichotomies, I use an index based on Gastil in columns 5 and 6 of Table 2. Beginning in 1973, Gastil (1987) assigned a rating of 1 to 7 for each of two variables, political freedoms and civil liberties, with higher scores indicating less freedoms. I averaged these variables over the period 1973-85, summed the two averages, and finally reversed the scores so that higher values indicate greater democracy.⁸ Including all 49 countries, the coefficient on MIDDLE*DEMOC is smaller than in column 1, and is only marginally significant. When the seven most suspect inequality observations are deleted, the coefficient...

Employing a common method of correcting for possible measurement error, Persson and Tabellini report results of two-stage least-squares estimates, using as instruments for MIDDLE the share of the labor force in agriculture, male life expectancy, and secondary school enrollment (all measured in 1960). The coefficient for MIDDLE remains much higher in their democracy

⁸Numerous other studies (e.g., Burkhart and Lewis-Beck, 1994) have used an additive index of Gastil ratings as a measure of democracy.

sample than in their autocracy sample. This result proves to be highly dependent on their use of secondary enrollment -- perhaps the most inappropriate possible instrument. Secondary enrollment is strongly correlated with error term of the growth equation, and is in fact a standard regressor in growth equations (e.g., Barro, 1991). The most widely-cited theory of education's impact on growth does not posit inequality as a channel through which secondary education influences growth (Nelson and Phelps, 1966). In some theoretical models (e.g., Saint-Paul and Verdier, 1993), education is even endogenous to inequality.

Despite the elegance of median voter theories, the finding presented here -- that inequality's impact on growth does not differ significantly by regime type -- is likely to surprise few observers of world politics. Even where autocrats or bureaucrats, rather than elected representatives, choose tax policies, their choices can be influenced by the need to maintain popular support, or at least acquiescence. Additionally, even where leaders' choice of taxation and redistribution policies are not highly sensitive to inequality, growth may be harmed by inequality through increasing political violence. Theories on possible links between inequality and political violence date to Aristotle, and numerous recent empirical studies have addressed these links, albeit not conclusively.⁹ Barro (1991) and others have shown that political violence is detrimental to growth. The types of political violence most strongly related to growth -- revolutions and coups -- are far more common in autocracies than in democracies.

⁹See Wang et al. (1993) for a sample of this research.

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