Is Democracy Good for the Poor?

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Comments welcome

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Abstract

Many scholars claim that democracy tends to improve the material well-being of the poor. I argue that previous tests of this claim were flawed, particularly by sample bias. Once these flaws are addressed, there is no evidence that poor people have benefitted from living under democratic governments. This disturbing result suggests that democracies have not functioned well for their poorest citizens. I speculate about the reasons for this finding, and suggest avenues for further research.
Introduction

For centuries, political leaders have made claims about alleviating poverty. Yet social scientists know surprisingly little about what types of governments have actually produced better outcomes for the poor.

The urgency of this issue is self-evident. In 2001, over half of the world’s population – some 2.73 billion people – lived on less than $2 a day. While the fraction of the world’s population living in poverty has fallen over the last two decades – due mostly to China’s rapid growth – the absolute number of people who live in poverty has risen [World Bank 2004].

There is a well-developed literature in political science on poverty, redistribution, and welfare in the advanced democracies [see, for example, Alesina, Glaeser, and Sacerdote 2001; Iverson and Soskice 2002]. Yet compared to Africa and Asia, poverty levels in these countries are trivial. Where poverty is truly severe – in the developing world – our understanding of government’s role is much weaker.

Social scientists claim to know one important thing about the politics of poverty in the world at large: that democracy is good for the poor [Sen 1981, 1999; Moon and Dixon 1985; Dasgupta 1993; Boone 1996; Przeworski et al. 2000; Zweifel and Navia 2000; Bueno de Mesquita and Root, 2000; McGuire 2001; Siegle, Weinstein, and Halperin 2004]. This paper challenges this common finding, suggesting that previous studies are wrong: between 1970 and 2000, poor people fared no better under democratic governments than they did under nondemocratic ones.

Proponents argue that democracy helps the poor in three ways: democracies empower poor people through elections, and hence force governments to attend to their needs; democracies facilitate the free flow of information, and hence give governments better information about the condition of the poor; and democratic governments provide their citizens with more public goods.
Each of these mechanisms was illustrated by India’s May 2004 election, in which the heavily-favored incumbent Bharitiya Janata Party was defeated – despite a booming economy – partly due to opposition from low-income voters. Less than two months later, the new Congress Party government presented a budget that included additional financing for sanitation, schools, health care, and basic infrastructure for impoverished rural areas [Rai 2004]. All three “pro-poor” mechanisms appeared to be functioning: poor people had used their electoral influence to punish a neglectful government; the elections revealed hidden information about lagging incomes in impoverished, rural areas; and the new government responded by providing the poor with more public goods.

Yet this example is less illuminating than it first appears: even if India’s democratic government is spending more money on poverty alleviation, we do not know if it is producing better outcomes for the poor, such as longer, healthier, or more productive lives. Perhaps the new government’s spending will be ineffectual: recent studies have found that the quality of government schools and health clinics in rural India – and other low-income democracies – is “abysmal” [Kremer 2004; Banerjee, Deaton, and Duflo 2003]. Maybe the benefits from these programs will be diverted to, or captured by, middle and upper-income groups. Or perhaps these and other pro-poor programs will undermine the fiscal, labor, and trade policies that the country needs to generate higher growth rates, thus offsetting any gains for those at the bottom of the income distribution.

If democracy produces better outcomes those in the bottom deciles, then countries that transit to democracy should subsequently report improvements in key social indicators, like infant and child mortality rates. In general, they do not. Consider Eastern Europe, where 13 states became democratic in the early 1990s. As Figure One suggests, in the ten years following this transition (1991-2001), the natural log of infant mortality rates in these 13 states fell notably, by
about 12.6 percent. But in the final ten years before the transition, for which complete data exist (1976-1986), infant mortality rates fell even faster, by 13.4 percent.¹

Was Eastern Europe an anomaly? Figure Two displays changes in the infant mortality rates of all 44 states that made a single, unambiguous transition to democracy between 1970 and 1999.² While the infant mortality rates of these states collectively fell by 7.4 percent during the first five years after their transitions, they fell by 10.7 percent during the five years before their transitions.

These figures may help explain why people in newly-democratized countries often become disillusioned with democracy, and vote for candidates and parties associated with former dictators. A recent United Nations survey found a startling level of dissatisfaction with democracy in Latin America: 54.7 percent of the respondents said they would prefer a dictatorship to a democracy, if it would help “resolve” their economic problems [UNDP 2004]. Is their disappointment misplaced?

This paper suggests that past studies of the democracy-poverty relationship – which overwhelmingly found that democracy helps the poor – have erred in three ways: they failed to account for country-specific effects; they neglected exogenous factors, including global health trends; and most importantly, they have been confounded by the problem of missing data, and inadvertently excluded many high-performing authoritarian states from their datasets. I show that once the first two problems have been addressed, democracy’s impact on the poor becomes very small; once the missing data problem is addressed, using multiple imputation, the relationship between democracy and poverty alleviation vanishes altogether. I also show that

¹ I use the natural log of infant and child mortality rates here and elsewhere to make intertemporal, and cross-national, comparisons. Without the logarithmic transformation these comparisons would be harder to make, since infant and child mortality are more costly to reduce as their numbers decline, and they cannot be reduced below zero.
² I have excluded the 20 states that made multiple transitions to democracy during this period. I use the updated coding of Przeworski et al. (2000) here and elsewhere to determine democratic transitions.
there is, at best, only weak evidence that democracy reduces the likelihood of economic
catastrophe for those in poverty.

While the livelihoods of the poor are influenced by their nation’s economic performance,
its disease environment, and the diffusion of medical interventions, a government’s regime type
seems to make little difference. This disturbing result suggests that democracies have not
functioned well for their poorest citizens. It also suggests we have even less general knowledge
than we imagine about the political economy of poverty alleviation.

I begin by explaining my choice of measures – infant and child mortality rates – for the
condition of the poor. I then describe previous studies of the impact of democracy on poverty,
and point out some of their shortcomings. In section three I describe my hypotheses, variables
and model, and how I cope with the missing data problem. I report my test results in section
four. In section five I explore whether democracy influences the variance in outcomes for the
poor. In the conclusion I summarize my findings and speculate about why democracies do not
perform as well as earlier theories suggest.

I. Measuring the Condition of the Poor

Some cross-national studies examine relative poverty, that is, the condition of those in the
bottom quintile or decile of a given population [e.g., Dollar and Kraay 2002]. My focus is on
absolute poverty, the condition of those who struggle to meet their basic subsistence needs.
While those who dwell in relative poverty are – by definition – evenly distributed among states,
people who suffer from absolute poverty are overwhelmingly concentrated in Sub-Saharan
Africa, South Asia, and East Asia.3

Although poverty is often defined by income, income is an unreliable way to compare
poverty rates over time and across countries. The incomes of the very poor are difficult to

3 On the definition of poverty, and the debate over absolute versus relative poverty, see Sen
[1983]; Lipton and Ravallion [1995]; and Kanbur and Squire [2001].
measure, and it is hard to be confident that the reported measures can be used for cross-national and intertemporal comparisons [Deaton 2001]. Moreover, income alone is not always a good measure of poverty: some families with very low incomes have assets they can use during periods of duress.

The most important drawback of focusing on income, for the purposes of this study, is that it only measures access to private goods, while the welfare of the poor is determined by their access to both private and public goods. Governments can help the poor by providing public goods like better access to health care and schooling, or smoother food prices – all without affecting their income. A simple measurement of the private income of the poor, similar to the “poverty headcount” employed by the World Bank, will not capture the ways that governments can help the poor.

Instead of using income, I rely on two measures of well-being that are closely correlated with the condition of the poor: the infant mortality rate, which describes the number of live-born infants who fail to reach the age of one; and the child mortality rate, which describes the number of live-born infants who fail to reach the age of five. Infant and child mortality rates have long been recognized by international agencies as good indicators of the condition of the poor, as they reflect a wide array of factors that characterize extreme poverty: lack of access to clean water and sanitation; indoor air pollution; crowding; low education and literacy rates among mothers; inadequate prenatal and neonatal health services; diets that have insufficient caloric intake and are deficient in essential micronutrients; greater vulnerability to disease; and of course, low income [Victoria et al. 2003; Lipton and Ravallion 1995; Sen 1998].

As indicators of the living standards of the poor, these measures have several advantages. They directly measure desirable outcomes – a child’s survival – rather than factors like income, school enrollment, or caloric intake that may or may not lead to desirable outcomes. And there is good evidence that infant and child mortality are concentrated in the lowest income deciles, even
in low-income countries, which makes them good measures of the condition of the poor [Kanbur and Squire 2001].

Other commonly-used measures of well-being – school enrollment and access to primary health care – are inferior measures of the welfare of people at subsistence levels. Since they do not measure outcomes, they are only indirect measures of how well the needs of the poor are being met. Moreover, in many low-income countries, some of these services are provided by religious or private institutions, not the government, giving us less insight into which types of governments are better at assisting the poor. Finally, recent studies have raised questions about the validity of common measures of educational attainment and health care, possibly due to unmeasured variations in the quality of these services [Filmer, Hammer, and Pritchett 2000; Pritchett 2001].

Data on infant and child mortality are independently compiled by several international bodies, most prominently the World Bank, the World Health Organization (WHO), and UNICEF. Each uses a combination of data from government registries – corrected for errors, and for alternative definitions of infant and child mortality – and independent demographic and health surveys. All three sources are used by scholars, and both UNICEF and WHO have published explanations of their estimation procedures [Hill et al. 1999; Ahmad et al. 2000]. There is a high level of agreement among the three child mortality datasets, and the two infant mortality datasets, although they vary somewhat in their coverage over time and across countries.

Child mortality rates are often considered more reliable than infant mortality rates, although much depends on the method used to aggregate the data [Hill et al. 1999; Ahmad, Lopez, and Inoue 2000]. For the study of government’s impact, child mortality rates may be slightly more informative than infant mortality rates, since the longer treatment period (five years

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4 While the World Bank and UNICEF each publish data on both infant and child mortality, WHO only publishes data on child mortality. These three organizations hence produce five datasets.  
5 The World Bank uses the same method as UNICEF to estimate infant and child mortality, although they sometimes assign different weights to certain observations and are somewhat less transparent.
instead of one year) gives the state more opportunities to influence outcomes. Still, I treat each of
the five datasets as containing useful information, and test my models with each of them.

All five data sets show that infant and child mortality rates have dropped substantially
over the last three decades, from 122 per thousand in 1970 to 69 per thousand in 1999. Observers
generally agree that the drop in child mortality reflects, in part, rising incomes, and the dispersion
of low-cost health interventions such as childhood immunization and oral rehydration therapy
[Hill and Amouzou 2004; Hill and Pande 1997].

II. Previous Research

There is a large body of research on the determinants of poverty in general, and infant and child
mortality in particular. Many studies pay little attention to politics; among those that link politics
to poverty, the most prominent claim is that democracy is good for the poor.

The best-known proponent of this argument is Sen [1981, 1999], who offers two reasons
why democracies seem to have fewer famines than nondemocracies. The first is that
democracies, through the electoral process, allow the poor to penalize governments that allow
famines to occur; and political leaders, acting strategically, must therefore try to avert famines:

Famines kill millions of people in different countries in the world, but they don’t kill the
rulers…if there are no elections, no opposition parties, no scope for uncensored public
criticism, then those in authority don’t have to suffer the political consequences of their
failure to prevent famines. Democracy, on the other hand, would spread the penalty of
famines to the ruling groups and political leaders as well [Sen 1999, 180].

Sen’s second argument is that democracies are better than nondemocracies at transmitting
information from poor and remote areas to the central government, thanks to freedom of the
press:

The most elementary source of basic information from distant areas about a threatening
famine are enterprising news media, especially when there are incentives – provided by a
democratic system – for bringing out facts that may be embarrassing to the government
(facts that an authoritarian government would tend to censor out) [Sen 1999, 181].
Hence even when democratic and nondemocratic leaders are equally devoted to stopping famine, democracies are more likely to be successful. While Sen’s arguments are specifically about famines, they can be – and have been – broadly applied to the problems of the poor in general.

More recently, other scholars have developed a third reason why democracies are good for the poor: because they tend to provide more public goods than nondemocracies [Lake and Baum 2004; Ghorbarah, Huth, and Russett 2004; Boix 2001; Bueno de Mesquita et al. 2000]. Since the poor are more likely than the rich to rely on public goods – such as subsidized health care, childhood immunization programs, publicly-funded water and sanitation projects, and public schools – this implies that democracies are especially beneficial for the poor.

This arguments are consistent with Meltzer and Richards’ [1981] model of the distributional impact of democracy: it suggests that when political power is evenly distributed but wealth is not – such that a country’s median income is below its mean income – voters will act to redistribute wealth from the rich to the poor. In an unequal society, democracy should improve the welfare of low-income voters.

Virtually all large-N studies in the past two decades suggest that democracy helps the poor. Tests run by Moon and Dixon [1985] show that democracy in general, and leftist democratic governments in particular, lead to positive welfare outcomes. Dasgupta [1993] finds a simple correlation between measures of political and civil rights and improvements in living standards in 51 poor countries between 1970-80. Boone [1996] shows that political rights are correlated with lower infant mortality, as do Zweifel and Navia [2000]. Przeworski et al. [2000] report that, after controlling for selection effects, democracy substantially reduces infant mortality. According to Siegle, Weinstein, and Halperin [2004, 60], democracies have significantly outperformed nondemocracies on a wide array of social indicators; they report that “poor democracies also suffer 20 percent fewer infant deaths than poor autocracies.”

Qualitative case studies have come to similar conclusions. McGuire’s [2001] four country study, for example, finds that “vigorous electoral competition” led to the expansion of
welfare programs to the poor in Costa Rica and Chile, even though their economies grew less quickly than South Korea and Taiwan.

Still, not all scholars agree with these claims. A 1975 study by Jackman found no simple correlation between democracy and social welfare. Moore and White [2003] are skeptical about the democracy-poverty alleviation claim, and Kohli [2003, 5] suggests that democracy’s contribution to helping the poor remains “unclear.”

These studies of democracy and the poor have three important limitations. First, none account for country-specific effects – even though other cross-national studies of infant and child mortality have found large country-specific effects [Pritchett and Summers 1996; Jamison, Sandbu, and Wang 2004]. These effects should not be surprising: nation-states are heterogeneous units, and their welfare levels are probably influenced by factors like geography, colonial legacy, leadership, and historical idiosyncrasies, that are difficult to measure. Ignoring these country-specific factors may bias our estimates of how the remaining variables, including democracy, influence outcomes. Indeed, Pande [2003] shows that when fixed effects are included in the Zweifel-Navia estimations, the influence of regime type drops sharply in size and statistical significance.

The second problem is the neglect of exogenous factors, including global health trends. Between 1970 and 2000, the mean infant mortality rate among nations fell by almost half. Unless this trend is accounted for, the reduction in mortality due to this global trend may be wrongly attributed to other variables that have also trended over time – such as democracy, which grew more prevalent at the same time that infant and child mortality rates were falling.

The third and most important problem is sample bias. In 2000, the world had 169 sovereign states with populations over 200,000. Yet these studies – like almost all ‘global’ studies – are based on a much smaller set of states. Dasgupta [1993] looks at 51 states; Boone [1996] at 97 states; Lake and Baum [2001] at 92 states; and Przeworski et al. [2000] at 135 states. While some of these samples appear to be large, missing data (along with the use of listwise
deletion) invariably reduces the size of the actual sample in the regressions. Przeworski et al., for example, can only employ 1,417 out of 4,126 possible observations in their infant mortality estimations: about two-thirds of their data are missing.⁶

These reduced samples would not be a problem if the missing states were randomly distributed by regime type, income, and other characteristics, but they are not: undemocratic countries are much more likely to be missing than democratic ones. Figure Three shows a scatter plot of countries, with regime type on the X axis, and a variable on the Y axis that counts the number of available observations between 1970 and 2000 on four commonly-used variables: population, income, infant mortality, and child mortality.⁷ In a typical regression, list-wise deletion will lead to the omission of countries below some cut point on the Y axis. Note that almost all countries that reside below a certain cut point – around 75 observations – were mostly authoritarian between 1970 and 2000.

This might not bias the results if the authoritarian states with fewer observations were similar to those with more observations. Yet they are not: as Table One shows, authoritarian states with fewer than 75 observations had higher incomes and lower child mortality rates than those with more than 75 observations. Even though the low-observation states grew more slowly between 1970 and 2000, they also produced faster drops in child mortality than the high-observation states. In other words, poor people were better off initially, and enjoyed more improvements, in the low-observation authoritarian states (e.g., Cuba, Lithuania, Poland, Oman, Libya, and Saudi Arabia) than the high-observation authoritarian states (e.g., Zambia, Niger, Mauritania).

This implies that missing data problems tend to cause large-N studies to omit many high-performing authoritarian states, while including low-performing authoritarian states. This, in

⁶ Przeworski et al. also exclude seven wealthy authoritarian states from their dataset, due to their large oil revenues. This may also bias their findings.
⁷ For this variable, I took data on income from Heston, Summers, and Aten [2002], and data on population, infant mortality, and child mortality from World Bank [2002].
It is not surprising that authoritarian states report less data than democratic states. But why should high-performing authoritarian states report fewer data than low-performing ones? One reason might be that the low-observation states are simply distorting their data, and hence falsely report lower child mortality rates. This is unlikely for two reasons. First, the international agencies that calculate infant and child mortality rates for all countries (UNICEF, the World Bank, and WHO) cross-check government statistics against independent social and demographic surveys, which helps limit the effect of any falsified data. More importantly, even if states are reporting inaccurate data on their levels of child mortality, as long as these inaccuracies are consistent within states over time, they should not bias data on changes in child mortality once country-specific effects are accounted for. In fact, since the ability of international agencies to measure child mortality has improved over time, any states that falsely reported low child mortality rates in 1970 would turn out to show exceptionally slow improvements between 1970 and 2000. Yet, as noted above, the low-observation states showed exceptionally fast improvements between 1970 and 2000.

An alternative explanation entails a brief theory about why states produce economic and social data. Perhaps democracies tend to collect and report this data, but authoritarian states do not unless they are compelled to by an outside agency; and outside agencies (like the World Bank, the International Monetary Fund, or the United Nations Development Programme) only make these demands of countries that approach them for economic and development assistance. Authoritarian countries with higher incomes, and more successful poverty-alleviation programs – like Cuba, Saudi Arabia, and Libya – may thus have fewer incentives to report their annual statistics to international agencies, and can remain opaque. Authoritarian countries with lower incomes, on the other hand, may have strong incentives to report because they are more likely to approach outside agencies for economic and development assistance.

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8 The World Bank, for example, offers its clients programs for “statistical capacity building,” alongside programs to promote education and build rural infrastructure.
incomes that need more assistance – like Zambia, Mauritania, and Niger – must report more data and become less opaque. Hence fewer data would be available for high-performing authoritarian states than low-performing ones.

III. Data, Model, and Variables

To estimate the effect of regime type on infant and child mortality, I use a dataset that includes all states that were sovereign between 1970 and 2000, and had populations over 200,000. To minimize the problem of sample bias, it is important to include states regardless of their turbulence during this 31 year period (1970-2000). For states that became sovereign between 1970 and 2000, I treated them as sovereign for the entire period if I could obtain political, economic, and demographic data for them going back to 1970: hence I treat the new states that emerged from the breakup of the Soviet Union, Yugoslavia, and Czechoslovakia as if they had been sovereign states since 1970, provided I could obtain data on their pre-independence income, population, and infant and child mortality. During their periods as nonsovereign states under communist rule, I coded them as being under communist governments; during their subsequent periods of independence I coded them according to the same rules I used for other sovereign states.⁹

I treated states that merged between 1970 and 2000 (North and South Vietnam, North and South Yemen, East and West Germany) as though they were merged states for the entire period. When the states were of similar size (North and South Vietnam) I used population-weighted averages to estimate their pre-merger economic conditions; when one state was significantly

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⁹ To ensure that my results were not unduly influenced by the Soviet-era data, I tested a dummy variable for the former Soviet states. I also tested a dummy variable for states with populations below one million, to make sure my findings were not biased by the inclusion of small states in the sample. Although these two variables reached statistical significant in some specifications, they never altered the significance levels of the other variables, and they had little substantive effect.
larger than the other (North and South Yemen, West and East Germany), I used the pre-merger values of the larger state.

The resulting dataset has 169 states, and covers the years 1970-2000. Since observations of infant and child mortality are only available from UNICEF, WHO, and the World Bank at five-year intervals, I collapse these data into five-year panels.

Unfortunately, the dataset still contains a large amount of missing data; as noted above, the missing observations are almost certainly nonrandom. To mitigate this problem I employ multiple imputation, using statistical inference to estimate the missing values.

I use a program called Amelia [Honaker et al. 2001] for the imputation. Amelia uses the known values for certain variables for each state to generate estimates for the missing values for other variables. Rather than produce a single estimation for the missing observations, it generates five datasets, each of which has unique values for the missing observations. The variance in the imputed values, across the five datasets, reflects Amelia’s uncertainty about the observation’s true value. I then run my estimations with each of the five datasets and combine the results, using a procedure designed to reflect the appropriate uncertainty levels for each of the missing values.\footnote{The procedure I use to combine the results across the imputed datasets was developed, and generously shared, by Kenneth Scheve.}

Since I use this imputation process for some of my estimates, I am working, in effect, with two datasets: the original one with the missing observations (which I refer to as the “biased dataset”) and a second dataset in which the missing observations are replaced with imputed values (which I refer to as the “completed dataset”). I report my estimations using both the biased and the completed datasets.

Model

The most appropriate model for estimations with pooled time-series cross-sectional data with unit-specific effects is a fixed-effects model. To control for exogenous global health trends (and
any other contemporaneous shocks), I include a dummy variable for each of the five-year periods in my dataset; to mitigate unit heteroscedasticity, I use robust standard errors. To reduce the possibility of endogeneity, I lag each of the right-hand side variables by a single five-year period. Since my OLS estimates generally suffer from serial correlation – that is, the residuals are significantly correlated with their lagged values – I employ Prais-Winsten regressions.

One drawback of the fixed-effects model is that it requires a lot of data from each country: seven observations on each variable, one for each time period. Since many countries only produce data intermittently, the resulting regressions have large numbers of missing observations. To reduce the number of missing observations, I also use a first-differences model; it may be written as

\[ Y_{i,t} - Y_{i,t-1} = \beta_0 (X_{i,t} - X_{i,t-1}) + \mu_{i,t} \]

where \( i \) is the country, \( t \) is time, \( X \) is a vector of variables, and \( \mu \) is the error term. The first-differences model has some helpful properties. Like the fixed-effects model, it solves the problem of unit-specific effects: since they do not change over time, they are “netted out” of the equation.\(^{11}\) Since the first-differences model only covers a single period, I do not have to worry about serial correlation or contemporaneous shocks. Most importantly, the first-differences model facilitates the inclusion of data-scarce countries in the estimations, since it only requires two observations (in 1970 and 2000) on most variables.

**Dependent Variables**

I have two dependent variables: the log of the infant mortality rate, and the log of the child mortality rate. Infant mortality rates are compiled independently by the World Bank [2004] and UNICEF [2004], while child mortality rates are gathered and calculated by the World Bank, UNICEF, and the WHO [Ahmad, Lopez, and Inoue 2000]. Although the measures are highly

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\(^{11}\) To see this, imagine that the country-specific effect was one of the \( X \)’s; since \( X \) will have the same value as \( X_{t,i} \), its net impact will be zero.
correlated, their coverage varies slightly; I hence run tests using each of these five datasets. To keep the number of tables at a manageable level I only list the complete results of tests run with the child mortality data from UNICEF, while summarizing the results of the other tests.\footnote{The infant and child mortality figures for the former Soviet states raise special concerns about bias and accuracy. While there is evidence of deliberate data falsification in some regions after 1970, the Soviet data mostly suffer from the same types of errors as non-Soviet mortality data [Velkoff and Miller 1995; Anderson and Silver 1997]. The only unusual problem with the Soviet-era data stems from the government’s idiosyncratic definition of infant mortality: infants were excluded from government figures if they were shorter than 28 centimeters, weighed less than one kilogram, and died within a week of their birth – producing a sharp, downward bias in the data. After the fall of the Soviet Union, western demographers devised adjustments for each of the Soviet republics, which are now reflected in each of these datasets.}

**Independent Variables**

I test two hypotheses about the influence of government on the poor, using multiple indicators when appropriate.

First, I test the hypothesis that democracy is good for the poor. I use three alternative measures of regime type. One is based on the Polity IV dataset, which contains separate 0-10 measures of democracy and authoritarianism for each country-year; following standard practice, I combine these two measures to produce a 21-point scale, which I call POLITY. A second \textit{(DEMOCRACY)} is the Alvarez, Cheibub, Limongi, and Przeworski measure, which is dichotomous, simply indicating whether or not each country, for each year, was a democracy.

The third measure allows for a country’s history of democratic rule to influence its infant and child mortality rates. Several recent studies suggest that new democracies perform less well than established ones [Keefer and Vlaicu 2005; McGuire 2004]. A state with 50 years of democratic experience, for example, might reduce infant mortality more quickly (or slowly) than a country with just one year of democratic experience. To test this possibility, I use a variable
(DEMYRS) that counts the total number of years of democracy a country has experienced, beginning in 1900.\textsuperscript{14}

My second hypothesis is that communist governments tend to improve the condition of the poor. While the regime-type indicators described above differentiate between democracies and autocracies, none of them distinguish between communist and non-communist autocracies – a distinction that might matter for people at the subsistence level. I test communism with three different measures: one is a dummy variable (COM) that indicates whether a country was under communist rule in a given year; the second (COMYRS) is the number of years of communist rule since 1900; and the third is the natural log of COMYRS.

Control Variables
The model has five control variables; in addition, it includes dummy variables for each country and for each period. I selected the controls so that I may capture the total effect of government on the poor, not simply the partial effect. Governments may influence infant mortality through a variety of causal mechanisms – for example, by improving education, sanitation, and rural health care. Yet if these intervening mechanisms matter, and I include them in the model, they may reduce the substantive and statistical significance of my “regime type” variables, and consequently, underestimate the role that governments play. Hence even though I control for variables that are largely immune to government influence – like the country’s disease environment, and its population density – I purposely avoid controlling for factors that may reflect government interventions, like education, sanitation, fertility rates, income inequality, or

\textsuperscript{14} I use the ACLP measure for democracy from 1950 to 2000, and follow a simple coding rule (drawing on both the ACLP standard and the Polity IV dataset) for 1900 to 1950. See appendix for details on the coding rules.

Bollen [1993] and Bollen and Paxton [2000] show that subjective measures of regime type (like Polity IV) suffer from systematic measurement error. The two measures based on the ACLP indicator – DEMOCRACY and DEMYRS – are less subjective and should help mitigate this problem.
the number of doctors per capita. By omitting these intervening variables, I am increasing the likelihood that democracy will be significantly correlated with infant and child mortality.

The first control variable is the log of income per capita (LOGINCOME); I take my data from the chain series index of the Penn World Tables 6.1 [Heston, Summers, and Aten 2002]. Virtually every cross-national study of infant and child mortality finds that income per capita has a strong effect.

My second control variable is the log of population density (LOGPOPDEN). I hypothesize that governments find it harder to provide health care, education, sanitation, and other public goods to the poor when the poor are widely-scattered in rural areas. Data on population density are from the World Bank [2002].

The third control variable is regime change. The true, long-term effect of democracy on poverty might be masked by the short-term disruptions caused by the democratic transition itself. For example, Figure Two shows that most countries saw slower improvements in infant mortality in the five years after they democratized than the five years before. But maybe this was caused by the institutional and economic chaos that often surrounds the democratization process itself: if we could observe infant mortality rates ten or twenty years after the transition, perhaps we would see much faster improvements. I hence include a dummy variable, REGCHANGE, that is coded one for years with a regime change, and zero otherwise. Since the data are organized into five year panels, and the control variables are lagged for a single five-year period, the model allows a single regime change to influence child mortality rates for five to nine years.

The fourth control variable is economic growth. There is strong evidence that growth is good for the poor [Dollar and Kraay 2002; Ravallion and Chen 1997; Firebaugh and Beck 1994]. If a country’s regime type influenced its growth rate, and growth helps reduce infant and child mortality, then placing growth in the model would bias my estimates of the true effect of governance on infant and child mortality. There is no strong evidence that regime type is linked
to growth, although the topic is still under debate.¹⁵ Still, for this reason I run models both with and without growth as a control.¹⁶

Finally, the fifth variable controls for the impact of HIV/AIDS on the poor. My model should not control for the influence of all diseases: many are more of a consequence of poverty – and of government failures – than a cause, since they tend to flourish where nutrition is poor, and access to clean water, sanitation, and primary health care facilities is inadequate. But two major diseases directly harm the poor, and are exceptionally difficult for governments to control: HIV and malaria. I control for HIV but not malaria, because high-quality data on the latter is unavailable.

For reasons that remain poorly-understood, HIV is highly specific to certain regions of the world. The twenty countries with the highest HIV rates are all in Sub-Saharan Africa. The top seven countries are all in southern Africa. Some fraction of this tragedy is probably attributable to government failures in education and health care access; moreover, the scope for government intervention has grown sharply in the last few years, as medical treatments have become more affordable and available. Still, a large fraction of the epidemic’s impact in Africa in the 1980s and 1990s was caused by non-governmental factors: the absence of affordable medical treatments; demographic patterns that increased transmission rates; the long latency period of the disease, which masks the contagion effects; and perhaps, other environmental or genetic factors not yet identified. Botswana’s government was one of the most effective and least

¹⁵ After controlling for selection effects, Przeworski et al. [2000] find no indication that democracies and autocracies grow at different rates. Tavares and Wacziarg [2001] explore a number of channels linking regime type and growth, and conclude that democracy’s impact on growth is moderately negative. Baum and Lake [2003] find that democracy has no direct effect on growth, while Krieckhaus [2004] shows that the regime type-growth relationship is sensitive to both the choice of time period and the selection of control variables.

¹⁶ Datasets on growth have a large number of missing data points that are almost certainly nonrandom: many developing states, and authoritarian states, are more likely to report growth rates in good years than bad years. This is more of a problem with the fixed-effects model, which requires annual growth data, than then first-differences model, which requires only data on GDP per capita in 1970 and 2000.
corrupt in the developing world, and between 1970 and 1999 generated consistently high economic growth; yet its HIV infection rate is the world’s highest.

I therefore control for the log of the HIV prevalence rate \((\text{LOGHIV})\), using data from UNAIDS [2003], with missing values taken from the Central Intelligence Agency [2003]. Unfortunately, these data are for 2001 only and there are no reliable estimates for earlier years for most countries – although we can safely assume that the 1970 rate was close to zero. Hence I interpolate, assuming that HIV prevalence rates in 1995 were two-thirds of their 2001 levels; 1990 rates were one-third of their 2001 levels; 1985 rates were one-sixth of their 2001 levels; and rates for 1980 and before were zero. Since these interpolations are crude, the LOGHIV variable is more likely to fit the data in the first-differences model (where no measures between 1970 and 2000 are needed, and hence, the interpolations are not employed) than the fixed-effects model (which employs the interpolations).\(^{17}\)

**IV Results**

Before turning to the results, it may be useful to look at the core model with only the control variables, using the first-differences and fixed-effects models with the biased data, and the fixed effects model with the completed data.

\(^{17}\) Besides these five control variables, I explored four others that turned out to have no measurable effect on my dependent variables. One measured the incidence of violent conflict – both civil and international wars – weighted by the number of battle-related deaths. A second indicator tried to capture a country’s susceptibility to malaria by measuring the fraction of a country’s population living in tropical zones. The third variable measured the fraction of the population that was either migratory or indigenous; several studies have suggested that these populations have exceptionally high infant mortality rates [Hentschel and Waters 2002; Yang, Knöbel, and Chen 1996]. A fourth variable measured ethno-linguistic fractionalization, which according to several scholars tends to reduce the quality of governance [Easterly and Levine 1997, Easterly 2001, Keefer and Khemani 2003]. None of these variables was significantly associated with my dependent variables; nor were there sufficiently compelling theoretical reasons to keep them in the model. For all but the conflict variable, however, the lack of statistical significance may be partly because they are fixed, and both the fixed-effects and first-differences models have trouble estimating the impact of fixed variables.
Table Two presents the results from the first-differences model with the biased data, which employs 150 of the 169 states – about 89 percent coverage. Column One suggests that high initial income, low HIV prevalence, high population density, and high economic growth are all strongly correlated with lower child mortality rates. The variable for regime change is not statistically significant, but since it is highly correlated with democracy I drop it from the subsequent first-differences regressions to avoid diluting the effect of the democracy variables.\^18

Table Three (column one) displays the results of the fixed-effects regressions with the biased panel data; it finds that both INCOME and LOGHIV are highly significant. The period dummies are also highly significant, although I do not include them in the tables. An F test strongly rejects the null hypothesis that the country dummies are not significant (Prob>f=0.0000). Neither population density, nor economic growth, nor regime change, are significantly correlated with child mortality, although these results may be due to the problem of nonrandom missing data: the regression uses just 491 of 1,183 possible observations, about 42 percent coverage.

Table Four (column one) shows the results of the same fixed effects model after the missing observations have been replaced with imputed values. Both INCOME and LOGHIV remain statistically significant, and population density and REGCHANGE now achieve statistical significance at the .10 level but not the .05 level. Economic growth is not statistically significant, although it becomes so in other specifications. All of the period dummies remain significant at the p=.001 level.

\textit{Democracy}

\^18 Since the democracy variables are already first differences – democracy in 2000 minus democracy in 1970 – change in regime is, in a sense, already part of this model. If I include REGCHANGE in the remaining first-differences estimations, the democracy variables lose all statistical significance.
When democracy’s effect on child mortality is tested with the biased datasets, it sometimes achieves statistical significance, but its substantive effect is very small. When the tests are run with the completed dataset, the correlation between democracy and child mortality disappears.

Table Two reports the results of the first-differences tests with the biased data. Both DEMOCRACY and DEMYRS reach statistical significance at the .10 level, and POLITY achieves significance at the .05 level.\(^\text{19}\) The greater statistical significance of POLITY may an artifact of a smaller sample of states: the POLITY specification incorporates 127 states, while the DEMOCRACY and DEMYRS tests include 150 states. When DEMYRS is tested with the same 127 states as POLITY, it too achieves significance at the .05 level.

The fixed-effects tests with the biased data (Table Three) are somewhat similar: this time, DEMOCRACY is significant at the .05 level and DEMYRS is close (\(p=.053\)), while POLITY falls short. Although I drop the population density and growth variables for these estimations – since they are not significant and further reduce the number of observations – their inclusion weakens the effect of the democracy variables.

Even if the tests with these biased data are right, the effect of democracy on child mortality would be very small. Using the biased samples, the first-differences tests imply that when a state’s Polity score is improved by a single standard deviation (about 6.9 on the 21 point scale), its child mortality rate should drop by a little less than one death per thousand. The fixed-effects tests suggest an effect of a similar magnitude: for two states that are identical except for regime type, the democracy’s child mortality rate should be lower by slightly less than one death in a thousand.

Any reduction in child mortality is praiseworthy, but these changes are not large. Between 1970 and 2000, mean child mortality rates fell by about two deaths in a thousand per

\(^{19}\) An alternative measure of democracy, the log of DEMYRS, never approaches statistical significance under any specification.
year, due to economic and health-related advances. Democracy would cause the equivalent of a single six-month boost in this trend.

The magnitude of this effect—which I emphasize is only visible in the biased dataset—is much smaller than others have claimed. Przeworski et al. [2000] find a gap of 10.3 infant deaths per thousand between dictatorships and democracies; Navia and Zweifel [2003] suggest the gap is 4.6 infant deaths per thousand; and Siegle, Weinstein, and Halperin [2004] claim the gap is “twenty percent.”

Yet even this modest improvement probably overstates the benefits of a democratic transition. If the estimates from the imputed data are correct, a regime change itself has a harmful effect on child mortality; moreover, this negative effect is about the same size as democracy’s beneficial effect. This implies that for poor people in an authoritarian state, any improvements that come from being democratic would likely be offset by the costs of becoming democratic, at least for the first five to nine years.

The issue of democracy’s substantive effect on the poor, however, may be moot. When I employ the completed dataset, any link between democracy and child mortality vanishes: none of the three democracy indicators approach statistical significance (Table Four). Interestingly, in two of the three tests, REGCHANGE is now significantly associated with heightened child mortality. This is consistent with Figures One and Two, which show that democratic transitions in Eastern Europe, and all states collectively, have been followed by a slowdown in the decline of infant mortality rates.

Communism

Did the communist states do a better job improving the condition of the poor? These tests suggest the answer is “no.” Using the biased dataset, the fixed-effects tests find no link between

20 These studies both use Heckman selection models to control for unobserved factors that might be influencing both infant mortality and regime type. Note that if I used this procedure it would further reduce the size of any substantive effect that democracy might have on child mortality.
communist rule and child mortality (Table Five). The first-differences tests show a significant but perverse correlation between communist rule (COMYRS) and child mortality: states that experienced more years of communist rule between 1970 and 2000 were likely to have higher child mortality rates. The substantive impact of communist rule, once again, is small: having 30 years of communist governments during this interval would produce 1.3 additional child deaths per thousand.

Once the missing observations have been replaced with imputed values, however, even this small effect disappears. As Table Five column 3 suggests, in the completed dataset communist rule appears to have no impact on child mortality. Several alternative measures of communist rule – measuring the total number of communist years since 1900, or taking the log value of communist years – are also uncorrelated with child mortality. Even a dummy variable for the former Soviet states is not linked to child mortality outcomes. This should not be too surprising: between 1970 and 2000 there was enormous variation in the economic and social performance of states under communist rule, ranging from astonishing (China) to catastrophic (North Korea). In recent decades, communist rule itself tells us little about the fate of the poor.

Using Alternative Measures of the Dependent Variable

When tests are run with different measures of infant and child mortality, the results are much like those described above. Tables Six, Seven, and Eight summarize these tests by noting which models find a statistically significant correlation (at the p=.05 level) between some measure of democracy and some measure of child or infant mortality.\(^{21}\) Tables Six and Seven describe the first-difference and fixed effects tests that employ the biased data: in about half (14 of 30) of the tests, a measure of democracy is significantly correlated with a measure of infant or child mortality. But once the missing observations have been filled in (Table Eight), only one out of 15 tests shows a statistical correlation, which is about what might be expected by chance.

\(^{21}\) Full test results are available from the author.
When assessing the effect of communist rule, the alternative datasets again produce a similar story (Table Nine). When the data are biased (columns one and two), the COMYRS variable is significantly linked to higher infant and child mortality in six of the ten tests; when the missing observations are filled in (column three), a significant correlation appears in just one of the five tests.

V. Does Democracy Reduce the Risk of Catastrophe?

Even if democracy does not produce higher living standards, is it a safer form of government for poor people? People who live at subsistence levels must by necessity be risk-averse, since they dwell so close to starvation. I have so far interpreted Sen’s argument about democracy and famines to mean that democracy improves the absolute standard of living for people at the bottom of the income distribution. But an alternative reading of Sen suggests that democracy’s value comes from warding off economic disasters, such as famines. Between 1970 and 2000, did democracy achieve this result?

If we measure disasters by using economic growth, the answer is probably “yes.” Przeworski et al. [2000] show that democracy tends to reduce the variance in economic growth, lowering the chance of both disasters and “miracles.” But measuring economic growth is almost certainly the wrong way to test this claim, since growth tells us little about outcomes, especially for low-income families. Moreover, some states cushion the poor against economic contractions with effective safety nets; figures on economic growth tell us little about the presence, and effectiveness, of these programs.

Moreover, observing GDP growth (or other economic data) for one-year periods may be misleading, since the relationship between economic growth and the welfare of the poor may be non-linear over time. A single year of falling GDP may have small welfare consequences, as families fall back on their savings; yet two or three successive years of falling GDP could have disproportionately severe effects, as these savings are exhausted.
Hence to measure catastrophic outcomes, I turn once again to data on child mortality rates. Figure Four is a scatterplot of 169 states, showing their mean democracy score (1970-1999) on the Y axis, and the difference between their 1999 and 1970 child mortality rates (log values) on the X axis; lower scores indicate larger drops in child mortality. While the democracies are clustered together at the top, the authoritarian states are more widely scattered, indicating both better and worse results. If these states are divided into two groups – those with more than 15 years of democracy between 1970 and 2000, and those with fewer – the standard deviation in the performance of the latter group is almost twice as large.

Yet this picture might be deceptive, because we cannot be confident that democracy is causing the lower variance: perhaps it was brought about by some other factor, such as higher incomes. The more salient question is whether democracy has a significant partial effect on the variance in outcomes, after income and other factors are accounted for.

Figure Five is a scatterplot similar to Figure Four: democracy scores are still on the Y axis, but the X axis now plots the residuals from the first-difference OLS regression in Table Two, column two. While Figure Four showed how much each country had reduced its child mortality rate, Figure Five shows how much it reduced its child mortality rate after accounting for initial income, growth, HIV prevalence rate, and population density.\(^{22}\)

Once these other factors are taken into account, democracy is no longer correlated with lower variance in child mortality outcomes. While in Figure Four the more democratic states are closely bunched, in Figure Five they are more dispersed. An OLS regression finds no correlation between any measure of democracy and the size of these residuals.

Another way to analyze these data is to divide the residuals into four groups: states that were always democratic, mostly democratic, mostly authoritarian, and always authoritarian. Table 10 summarizes the statistics for these four groups: while the “always democratic” states

\(^{22}\)Since in both cases I am measuring changes in child mortality over time, country-specific effects are also accounted for.
had less variance than the “always authoritarian” states, the “mostly democratic” states had more variance than the “mostly authoritarian” ones. There is no obvious pattern.

Note, however, that Table 10 uses real data only, and hence covers just 149 out of 169 states. Would the inclusion of these 20 omitted cases alter the results – for example, by increasing the variance among the authoritarian states? The answer is not obvious. The missing states generally performed about as well as the nonmissing states in each of the four categories. The main exception is North Korea, which according to the WHO child mortality data had the worst performance of any state between 1970 and 1999.

The available data suggests that between 1970 and 2000, democracy was not associated with reduced hazards for the poor; yet the missing data problem makes it difficult to reject this hypothesis outright.

**Conclusion**

Between 1970 and 2000, the global condition of the poor – measured by infant and child mortality rates – improved dramatically, as the mean child mortality rates for 169 states dropped by about half. The gains were widespread: child mortality rates dropped in 163 countries and rose in just six. These improvements were overwhelmingly caused by economic growth, medical innovations, and the diffusion of low-cost health interventions.

Over the same three decades, there was also a dramatic rise in the prevalence of democracy; yet I find no evidence that the rise of democracy helped cause the fall in infant and child mortality rates. Democracy produces non-economic benefits for people in poverty, offering them important political rights. But for those in the bottom deciles, these political rights did not lead to improvements in their material well-being. This troubling result contradicts the claims made, and the tests run, by a generation of scholars.

It also raises two questions for future research.
First, why does democracy fail to improve the condition of the poor? Scholars have offered plausible reasons why democracy should help lift living standards in the bottom tiers of society – because it enfranchises the poor, produces better information flows, and encourages governments to provide public goods. Why don’t these mechanisms produce the results they should?

Recent studies suggest at least two answers. First, there is evidence that government services in developing states often fail to reach the poor [Kremer 2004; Banerjee, Deaton, and Duflo 2003]. In fact, there is disturbingly little correlation between public spending on health and education, and the health and education of targeted populations [Musgrove 1996; Desai and Alva 1998; Pritchett 2001; Filmer and Pritchett 1999; Filmer, Hammer, and Pritchett 2000]. Sometimes this may be due to gross inefficiencies; other times, because social programs that are nominally targeted at low-income groups are captured by the rich [Deolalikar 1995; Castro-Leal et al. 1999; Barat et al. 2003]. Either way, breakdowns in the delivery of services may be depriving the poor of the fruits of democratic government.

Alternatively, empowering the poor may have perverse economic effects: maybe the pro-poor policies of democratic governments are offset by lower growth rates, resulting in no net reductions in poverty. This has apparently happened in Indonesia, which transited to democracy beginning in 1998. Under the dictator Suharto, the government’s economic policies were tightly guided by a small group of technocrats, who helped produce three decades of rapid growth and a dramatic fall in poverty rates. After moving to democracy, Indonesia’s governments have faced strong populist pressures to increase benefits for the poor through direct transfers, new minimum wage regulations, and protection for certain industries. Yet these policies have produced few if any gains for the poor, and may have undermined the fiscal, trade, and labor policies that the country needs to generate high growth rates [Timmer 2004].

The second puzzle is identifying political factors that distinguish the high-performance states from the low-performance states. It is clear that economic growth, and global health trends,
have dramatic effects on the welfare of the bottom deciles; but even after these are accounted for, there is still substantial variation around the world in poverty outcomes. Indeed, there are often remarkable variations in poverty within countries, which are clearly not caused by variations in regime type: in India, child mortality rates range from 18.8 in Kerala to 137.6 in Uttar Pradesh. Are there any dimensions of governance that explain these variations? If democracy does not matter, what does?
References


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Iverson, Torben, and David Soskice. 2002. Electoral Systems and the politics of coalitions: Why some democracies redistribute more than others.


Kremer, Michael. 2004. Randomized Evaluations of Educational Programs in Developing Countries: Some Lessons.


Figure 3: Regime Type and Missing Observations
Figure 4: Democracy and Child Mortality 1970-99

Figure 5: Democracy and Regression Residuals
<table>
<thead>
<tr>
<th>Table 1: High Observation and Low Observation Authoritarian States</th>
<th>More than 75 Obs</th>
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Table 2: First-Difference Estimations with Biased Data Set
Dependent Variable is Log of Child Mortality

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Standard errors are robust, and listed in parentheses below the coefficients.

Table 3: Fixed-Effects Estimations with Biased Data Set
Dependent Variable is Log of Child Mortality

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Prais-Winsten regressions. Each regression also includes a dummy variable for each period. All
of the independent variables are lagged for one period. Standard errors are robust, and listed in
parentheses below the coefficients.
Table 4: Fixed Effects Estimations with Completed Data Set
Dependent Variable is Log of Child Mortality

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<td>.222***</td>
<td>.221***</td>
<td>.218***</td>
</tr>
<tr>
<td></td>
<td>(.022)</td>
<td>(.022)</td>
<td>(.022)</td>
<td>(.022)</td>
</tr>
<tr>
<td>LOGPOPDEN</td>
<td>-.029</td>
<td>-.029</td>
<td>-.028</td>
<td>-.031</td>
</tr>
<tr>
<td></td>
<td>(.015)</td>
<td>(.015)</td>
<td>(.015)</td>
<td>(.015)</td>
</tr>
<tr>
<td>GROWTH</td>
<td>-.005</td>
<td>-.005</td>
<td>-.005</td>
<td>-.005</td>
</tr>
<tr>
<td></td>
<td>(.003)</td>
<td>(.003)</td>
<td>(.003)</td>
<td>(.003)</td>
</tr>
<tr>
<td>REGCHANGE</td>
<td>.059</td>
<td>.064*</td>
<td>.065*</td>
<td>.056</td>
</tr>
<tr>
<td></td>
<td>(.031)</td>
<td>(.031)</td>
<td>(.031)</td>
<td>(.031)</td>
</tr>
<tr>
<td>POLITY</td>
<td>-</td>
<td>-.004</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>(.003)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>DEMOCRACY</td>
<td>-</td>
<td>-</td>
<td>-.062</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(.040)</td>
<td></td>
</tr>
<tr>
<td>DEMYRS</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-.002</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(.001)</td>
</tr>
<tr>
<td>Observations</td>
<td>1183</td>
<td>1183</td>
<td>1183</td>
<td>1183</td>
</tr>
</tbody>
</table>

Each regression also includes a dummy variable for each period. All of the independent variables are lagged for one period. Standard errors are robust, and listed in parentheses below the coefficients. Tests are run with Stata 8.0.

Table 5: The Effect of Communist Rule
Dependent Variable is Log of Child Mortality

<table>
<thead>
<tr>
<th></th>
<th>FD Test</th>
<th>FE Test</th>
<th>MIEST</th>
</tr>
</thead>
<tbody>
<tr>
<td>LOGINCOME</td>
<td>-.231***</td>
<td>-.223***</td>
<td>-.198***</td>
</tr>
<tr>
<td></td>
<td>(.032)</td>
<td>(.050)</td>
<td>(.033)</td>
</tr>
<tr>
<td>LOGHIV</td>
<td>.130***</td>
<td>.311***</td>
<td>.235***</td>
</tr>
<tr>
<td></td>
<td>(.018)</td>
<td>(.030)</td>
<td>(.024)</td>
</tr>
<tr>
<td>LOGPOPDEN</td>
<td>-.305**</td>
<td>-</td>
<td>-.027</td>
</tr>
<tr>
<td></td>
<td>(.112)</td>
<td></td>
<td>(.016)</td>
</tr>
<tr>
<td>GROWTH</td>
<td>-.435***</td>
<td>-</td>
<td>-.004</td>
</tr>
<tr>
<td></td>
<td>(.066)</td>
<td></td>
<td>(.003)</td>
</tr>
<tr>
<td>COMYRS</td>
<td>.009*</td>
<td>.006</td>
<td>.004</td>
</tr>
<tr>
<td></td>
<td>(.004)</td>
<td>(.009)</td>
<td>(.002)</td>
</tr>
<tr>
<td>REGCHANGE</td>
<td>-</td>
<td>.025</td>
<td>.056</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(.019)</td>
<td>(.032)</td>
</tr>
<tr>
<td>Observations</td>
<td>149</td>
<td>513</td>
<td>1183</td>
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<tr>
<td>R-squared</td>
<td>0.66</td>
<td>0.99</td>
<td>-</td>
</tr>
</tbody>
</table>

Standard errors are robust and listed in parentheses below the coefficients. Tests are run with Stata 8.0.
* significant at the 0.05 level
** significant at the 0.01 level
*** significant at the 0.001 level
Table 6: Summary of First Differences Tests (Biased Data) with Alternate Measures of Infant and Child Mortality

<table>
<thead>
<tr>
<th>Dependent Variable and Source</th>
<th>POLITY</th>
<th>DEMOCRACY</th>
<th>DEMYRS</th>
</tr>
</thead>
<tbody>
<tr>
<td>CMR World Bank</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>CMR UNICEF</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>CMR WHO</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>IMR World Bank</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>IMR UNICEF</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
</tbody>
</table>

“Yes” indicates the variable statistically significant (p=.05) when the

Table 7: Summary of Fixed Effects (Prais-Winsten) Tests (Biased Data) with Alternate Measures of Infant and Child Mortality

<table>
<thead>
<tr>
<th>Dependent Variable and Source</th>
<th>POLITY</th>
<th>DEMOCRACY</th>
<th>DEMYRS</th>
</tr>
</thead>
<tbody>
<tr>
<td>CMR World Bank</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>CMR UNICEF</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>CMR WHO</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>IMR World Bank</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>IMR UNICEF</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Table 8: Summary of Fixed Effects Tests (Completed Data) with Alternate Measures of Infant and Child Mortality

<table>
<thead>
<tr>
<th>Dependent Variable and Source</th>
<th>POLITY</th>
<th>DEMOCRACY</th>
<th>DEMYRS</th>
</tr>
</thead>
<tbody>
<tr>
<td>CMR World Bank</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>CMR UNICEF</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>CMR WHO</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>IMR World Bank</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>IMR UNICEF</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
</tbody>
</table>

Table 9: Summary of Tests of Communist Rule with Alternate Measures of Infant and Child Mortality

<table>
<thead>
<tr>
<th>Dependent Variable and Source</th>
<th>FD Test</th>
<th>FE Test</th>
<th>Full Data</th>
</tr>
</thead>
<tbody>
<tr>
<td>CMR World Bank</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
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<tr>
<td>CMR UNICEF</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>CMR WHO</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>IMR World Bank</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>IMR UNICEF</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
</tr>
</tbody>
</table>
Table 10: Standard Deviations from Regression Residuals, by Regime Type

<table>
<thead>
<tr>
<th>Regime Type</th>
<th>Standard Deviation</th>
<th>Number</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>Always Democratic</td>
<td>.308</td>
<td>30</td>
<td>-.803</td>
<td>.522</td>
</tr>
<tr>
<td>Mostly Democratic</td>
<td>.448</td>
<td>19</td>
<td>-1.08</td>
<td>.682</td>
</tr>
<tr>
<td>Mostly Authoritarian</td>
<td>.297</td>
<td>43</td>
<td>-.797</td>
<td>.783</td>
</tr>
<tr>
<td>Always Authoritarian</td>
<td>.361</td>
<td>57</td>
<td>-.852</td>
<td>1.04</td>
</tr>
<tr>
<td>All States</td>
<td>.353</td>
<td>149</td>
<td>-1.08</td>
<td>1.04</td>
</tr>
</tbody>
</table>