Is Democracy Good for the Poor?

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Many scholars claim that democracy improves the welfare of the poor. This article uses data on infant and child mortality to challenge this claim. Cross-national studies tend to exclude from their samples nondemocratic states that have performed well; this leads to the mistaken inference that nondemocracies have worse records than democracies. Once these and other flaws are corrected, democracy has little or no effect on infant and child mortality rates. Democracies spend more money on education and health than nondemocracies, but these benefits seem to accrue to middle- and upper-income groups.

Many studies claim to show that democracies do a better job than nondemocracies of improving the welfare of the poor (Boone 1996; Bueno de Mesquita et al. 2003; Dasgupta 1993; Franco, Alvarez-Dardet, and Ruiz 2004; Lake and Baum 2001; McGuire 2001; Moon and Dixon 1985; Przeworski et al. 2000; Sen 1981, 1999; Siegle, Weinstein, and Halperin 2004; Zweifel and Navia 2000). These claims are consistent with leading political economy models, which suggest that democracies produce more public goods, and more income redistribution, than nondemocracies (Acemoglu and Robinson 2005; Boix 2003; Bueno de Mesquita et al. 2003; Ghorbarah, Huth, and Russett 2004; McGuire and Olson 1996; Meltzer and Richard 1981; Niskanen 1997).

There is good evidence that democracies fund public services at a higher level than nondemocracies (Avelino, Brown, and Hunter 2005; Brown and Hunter 2004; Gerring, Thacker, and Alfaro 2005; Kaufman and Segura-Ubiergo 2001; McGuire 2006; Stasavage 2005a; Tavares and Wacziarg 2001). But it is not obvious that these infusions of money actually reach the poor; nor is it obvious that they produce better social *outcomes,* such as longer, healthier, or more productive lives. If democracy produces better outcomes for low-income families, then countries that transit from autocratic to democratic rule should see improvements in their infant and child mortality rates. In general, they do not. Figure 1 displays changes in the log of infant mortality rates of all 44 states that made a single, unambiguous transition to democracy between 1970 and 1999.[1] While the infant mortality rates of these states collectively fell by 7.4% during the first five years after their transitions, they fell by 10.7% during the five years before their transitions.[2]

Perhaps this helps explain why people in newly democratized countries often vote for candidates and parties associated with former dictators. A recent United Nations survey found that 54.7% of respondents in Latin America would prefer a dictatorship to a democracy, if it would help "resolve" their economic problems (UNDP 2004).

This article suggests that past studies of democracy and the poor have been flawed by a surprising form of selection bias: most cross-national studies omit from their samples nondemocratic states with good economic and social records, which creates the false impression that democracies have outperformed nondemocracies. Most also fail to control for country-specific fixed effects and global health trends. Once these flaws are corrected, democracy has little or no effect on infant and child mortality.

If democracies spend more money on public services than nondemocracies, why do they fail to achieve better results? Perhaps democracies subsidize the budgets of middle- and upper-income groups who can afford to buy food and health services privately, but not the poor, who

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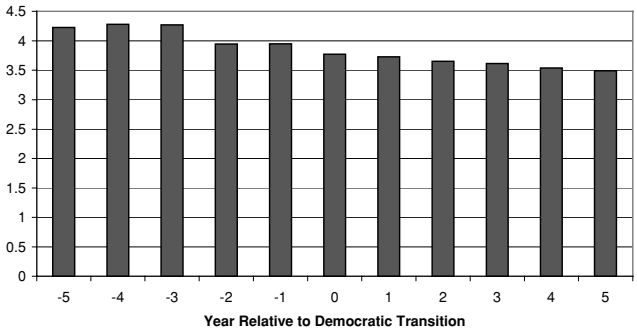
FIGURE 1 Log of Infant Mortality Rate Before and After Democratic Transitions, for 44 States



find food and health services unaffordable. Under these conditions, an increase in government health spending would allow middle- and upper-income groups to switch from private to public services, while leaving low-income groups unaffected. Since these subsidies would produce no net rise in the use of critical goods or services, infant and child mortality would be unchanged. We show that this explanation is consistent with a more nuanced interpretation of leading political economy models.

For the cross-national study of poverty, infant and child mortality data are exceptionally useful. Infant mortality is typically concentrated in the lowest income quintile (Gwatkin 2004). It is also a sensitive measure of many other conditions—including access to clean water and sanitation, indoor air quality, female education and literacy, prenatal and neonatal health services, caloric intake, disease, and of course, income—that are hard to measure among the very poor (Lipton and Ravallion 1995; Sen 1999; Victoria et al. 2003). Other commonly used measures of well-being, such as poverty rates, school enrollment rates, and access to primary health care, tend to be less reliable (and less comparable) since their definitions vary from country to country and over time; the death of an infant, however, has the same lamentable qualities everywhere. Finally, data on infant and child mortality are available for virtually all countries since 1970.

The article begins by reviewing theories of regime type and redistribution. The second section summarizes cross-national studies of democracy and infant and child mortality and shows they are marked by selection bias and other problems. The third section tests models that measure democracy's influence on infant and child mortality, both before and after correcting for selection bias, and the fourth revisits theories of regime types and redistribution and suggests one way they can be adjusted to account for both higher social spending, and unchanged infant mortality, in democracies. A brief conclusion summarizes the article's findings and discusses its implications.

Theories of Regime Type and Poverty

Three common theories imply that democracy will raise living standards in the lowest income quintiles. Two are associated with Sen (1981, 1999), whose work on the causes of famine is often extended to cover the causes of poverty more generally. His first argument is that democracies, through the electoral process, allow the poor to penalize governments that allow famines to occur; and political leaders, acting strategically, will 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)

[1]We 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. We employ the updated coding of Przeworski et al. (2000) here and elsewhere to determine democratic transitions.

[2]We explore below the possibility that democracy takes longer than five years to affect infant mortality rates. One of the most careful studies of this issue, however, argues that democracy has its greatest impact on social welfare after just three years (Lake and Baum 2001).

Hence even when democratic and nondemocratic leaders are equally devoted to stopping famine, democracies are more likely to know when action is needed.

The third theory suggests that democracies tend to help the poor by producing more public goods, and more income redistribution, than nondemocracies. According to some scholars, democracies produce more public goods because they are forced by the electoral process to spend their revenues on government services, while autocratic governments face no such constraint (Deacon 2003; Lake and Baum 2001; McGuire and Olson 1996; Niskanen 1997). A second set of scholars—including Bueno de Mesquita et al. (2003) and Ghorbarah, Huth, and Russett (2004)—suggest it is because democratic governments have a wider range of supporters to appease, which induces them to produce public goods instead of private ones.

Perhaps the most influential version of this argument comes from Meltzer and Richard (1981), who developed a seminal model on the distributional effects of democracy. In the model, democratization occurs when political rights are extended from a wealthy elite to the rest of the citizenry. As suffrage expands, the position of the median voter—whose preferences determine government policy—shifts down in the income distribution. Under universal suffrage, the median voter will earn the median income; when income is unequally distributed, however, the median income is less than the mean income. Since the decisive voter now earns a below-average income, she favors a higher tax rate (since it will fall most heavily on the wealthy) and more economic redistribution. In short, democracy brings more people with below-average incomes to the polls, and they collectively force the government to redistribute income downwards.

The Meltzer-Richard model has influenced much subsequent work on democracy and redistribution.[3] Boix (2003) builds on the Meltzer-Richard framework, adding in the effects of capital mobility, and exploring the strategic interactions of an elite, who control the state under authoritarian rule, and the masses, who accrue power under democratic rule. Acemoglu and Robinson (2005) also build on the Meltzer-Richard framework and explore the conditions under which states transit from authoritarian to democratic rule; like both Meltzer and Richard and Boix, they suggest that authoritarian governments favor the interests of the elite and no redistribution to the masses, while democracies favor a broader range of interests and support some redistribution.

There is good evidence that these theories are at least narrowly correct: democracies seem to fund social services at higher levels than nondemocracies. Historical studies show a partial correlation between the extension of the franchise and the size of government both among U.S. states (Gouveia and Masia 1998), and among western and Latin American countries more broadly (Kristov, Lindert, and McClelland 1992; Lindert 1994). An analysis of 44 African states by Stasavage (2005a) finds strong evidence that democracy has increased government spending on education, and a series of studies of Latin America finds that democracy is robustly linked to higher spending on health, education, and social security (Avelino, Brown, and Hunter 2005; Brown and Hunter 2004; Kaufman and Segura-Ubiergo 2001). Each of these studies controls for both country-fixed effects and exogenous time trends, and the Africa and Latin America studies use relatively complete data.[4]

Even if democracies increase social funding, however, they do not improve infant and child mortality rates—which may be the most accurate and comprehensive indicator of social welfare among the poor (see below).

Empirical Studies

Virtually all large-N studies on this topic in the past two decades suggest that democracy leads to lower infant and child mortality rates and better welfare outcomes more generally. Tests run by Moon and Dixon (1985) show that democracy in general, and leftist democratic governments in particular, produce better 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 and 1980. 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. Lake and Baum (2001) find that a move from complete autocracy to complete democracy should produce a drop in infant mortality of five deaths per thousand. According to Siegle, Weinstein, and Halperin, democracies have significantly outperformed nondemocracies on a wide array of social indicators; they report that

[3] For a summary, see Mueller (2003, 512–19). The underlying logic of this claim is similar to Sen's first argument. Both were anticipated by Tocqueville, who argued that "In democracies, where the sovereign power belongs to the needy, only an increase of its prosperity will win that master's goodwill; almost never can this be done without money" ([1840] 1969, 211).

[4] Also see Tavares and Wacziarg (2001), Ghobarah, Huth, and Russett (2004), Gerring, Thacker, and Alfaro (2005), and McGuire (2006), which use global data and arrive at the same conclusion. For a dissenting view, see Mulligan, Gil, and Sala-i-Martin (2004).

"poor democracies also suffer 20% fewer infant deaths than poor autocracies" (2004, 60). Bueno de Mesquita et al. find that "Infants have a vastly better prospect of surviving and going on to live a long, prosperous life if they are born in a democratic, large-coalition society than if they are born anywhere else" (2003, 194).

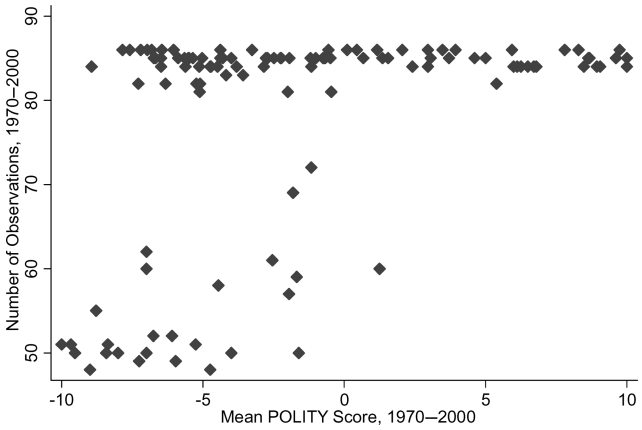
Qualitative case studies have come to similar conclusions. McGuire's (2001) four-country study, for example, finds that "vigorous electoral competition" led to the expansion of welfare programs to the poor in Costa Rica and Chile, even though their economies grew less quickly than South Korea and Taiwan. Still, not all scholars agree: a study by Jackman (1975) found no simple correlation between democracy and social welfare, while both Moore and White (2003) and Kohli (2003) are skeptical about the democracy-poverty alleviation claim.

Past quantitative studies of democracy and the poor have three important problems. First, none account for country-specific effects—even though other cross-national studies of infant and child mortality have found large country-specific effects (Jamison, Sandbu, and Wang 2004; Pritchett and Summers 1996). These effects should not be surprising: nation-states are heterogeneous units, and their welfare levels are probably influenced by factors like geography, culture, colonial legacy, past leadership, and historical idiosyncrasies that are difficult to measure. If any of these country-specific factors influence both a state's regime type and its poverty levels, their absence from the model will lead to an omitted variable bias. Indeed, Pande (2003) shows that when fixed effects are included in the Zweifel-Navia estimations, the influence of regime type on infant mortality drops sharply in size and statistical significance.

The second problem is the neglect of exogenous global health trends. Between 1970 and 2000, the mean infant mortality rate among nations fell by almost half, due largely to the spread of low-cost health interventions. Unless this trend is accounted for, the reduction in mortality due to health trends 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 problem is sample bias. In 2000, the world had 168 sovereign states with populations over 200,000. Yet these studies—like almost all "global" studies—are based on the smaller set of states that produce easily available data. Dasgupta (1993) gathers data on 51 states; Boone (1996) on 97 states; Lake and Baum (2001) on 92 states; and Przeworski et al. (2000) on 135 states.[5] While some of these samples appear to be large, missing data (along with the use of listwise deletion) reduce the size of the actual sample in the regressions. Lake and Baum's analysis of democracy and infant mortality, for example, is based on data from just 78 countries. Przeworski et al. employ 1,417 out of 4,126 possible observations in their infant mortality estimations: about two-thirds of their data are missing.[6]

FIGURE 2 Missing Observations and Regime Type



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 2 shows a scatterplot 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, listwise deletion will lead to the omission of countries below some cut point on the Y axis. Almost all of the countries that reside below a certain cut point—around 75 observations—were mostly authoritarian between 1970 and 2000.

[5]Others, like Bueno de Mesquita et al. (2003), do not report the number of countries in their sample.

[6]Przeworski et al. also exclude six wealthy authoritarian states from their dataset, due to their large oil revenues; this creates another source of selection bias. Between 1970 and 2000, infant mortality rates in the six excluded autocracies dropped by an average of 82%; in all other autocracies, it dropped by 43%. This leads Przeworski et al. to further underestimate the success of authoritarian states in alleviating poverty.

[7]For this variable, We took data on income from Heston, Summers, and Aten (2002) and data on population, infant mortality, and child mortality from World Bank (2004). We excluded from the sample the states of the former Soviet Union and Yugoslavia, which only became independent—and hence only began producing data—after 1990. Including them, however, does not alter the results.

Table 1 High Observation and Low Observation Authoritarian States

	More Than 75 Obs (n = 54)	Fewer Than 75 Obs (n = 26)
GDP per capita 2000	3198	5984
Child Mortality Rate 1970	206	161
Child Mortality Rate 2000	125	85
CMR Change 1970–2000	−39%	−47%

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 1 shows, authoritarian states with fewer than 75 observations had higher incomes and lower child mortality rates than those with more than 75 observations. 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, Poland, Oman, Libya, and Saudi Arabia) than the high-observation authoritarian states (e.g., Zambia, Niger, and Mauritania). Excluding low-observation states in general will hence produce a sample in which authoritarian states have worse records than they do in the full population.

The omission of these states still might not have a *large* effect if past studies had controlled for fixed effects, since cross-national differences play little or no role in fixed-effects models. But with no control for fixed effects, past studies have been strongly influenced by cross-national differences, which make this type of sample bias especially worrisome.

It is not surprising that authoritarian states report fewer data than democratic states (Rosendorff and Vreeland 2004). And it should not be surprising that low-performing democracies—perhaps suffering from economic crisis or civil war—sometimes produce fewer data than high-performing democracies, as occurred in the Lake and Baum study. But why should high-performing authoritarian states report fewer data than low-performing ones? One possible explanation is that authoritarian states are more likely to distort their data and that the "high performers" are falsely reporting lower infant mortality rates. This is unlikely to be a problem for two reasons. First, the international agencies that calculate infant and child mortality rates for all countries (UNICEF, the World Bank, and the WHO) cross-check government statistics against independent social and demographic surveys, which helps limit the effect of any falsified data. Moreover, even if states are reporting inaccurate data, as long as these inaccuracies are consistent within states over time, they should not bias data on changes in infant 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 show exceptionally *slow* drops in infant mortality between 1970 and 2000. Yet, as noted above, the low-observation states showed exceptionally fast improvements between 1970 and 2000.

An alternative explanation requires a brief theory about why states produce economic and social data. Perhaps democracies tend to collect and report these data because they are compelled to by their citizens, but authoritarian states do not because they are isolated from popular pressures. But authoritarian states may nonetheless produce these data if they are compelled to by an outside agency (i.e., the World Bank, the International Monetary Fund [IMF], or the United Nations Development Programme), and outside agencies only make these demands when countries approach them for assistance.[8] Authoritarian countries with higher incomes and more successful poverty-alleviation programs— such as 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 that need more assistance— like Zambia, Mauritania, and Niger—must report more data and become more transparent. Hence fewer data would be available for high-performing authoritarian states than low-performing ones.

To show that this theory is plausible, several regressions using a cross-section of 168 states were carried out. The dependent variable is the same measure used in Figure 2: the number of nonmissing observations between 1970 and 2000 for the population, income, infant mortality, and child mortality of each state whose population exceeds 200,000. There are two explanatory variables: DEMOCRACY 1970–2000 is the number of years from 1970 to 2000 that a country was democratic; IMF 1970–2000 is the number of years from 1970 to 2000 that a country was under a conditioned IMF arrangement.[9]

Table 2 displays the regression results, using ordinary least-squares and robust standard errors. In column one,

[8]The World Bank, for example, offers its clients programs for "statistical capacity building," alongside programs to promote education and build rural infrastructure.

[9]We are grateful to James Vreeland for sharing his data on IMF arrangements.

Table 2 Nonmissing Observations in Democracies and Nondemocracies

	1 All States	2 Democracies Only	3 Nondemocracies Only	4 Nondemocracies Only
IMF 1970–2000	.72***	.63*	.9***	.83***
	(.14)	(.27)	(.17)	(.16)
INCOME 1970	−3.58**	4.12	−5.1***	−3.54**
	(1.22)	(3.01)	(1.3)	(1.13)
DEMOCRACY 1970–2000	.62***	–	–	–
	(.094)	–	–	–
Former Soviet Union	–	–	–	−11.55***
	–	–	–	(1.93)
Former Yugoslavia	–	–	–	−15.83**
	–	–	–	(6.03)
Observations	168	50	118	118
R-squared	.34	.13	.37	.46

Standard errors are listed in parentheses below the coefficients.
*Significant at .05 level.
**Significant at .01 level.
***Significant at .001 level.

both DEMOCRACY and IMF are statistically significant and in the expected directions: countries produce more data when their governments are democratic and when they are bound by IMF agreements. The control variable, the log of income per capita in 1970 (INCOME 1970), is also statistically significant. In column two we restrict the sample to states that were democratic for at least half of the time between 1970 and 2000. IMF supervision still predicts the amount of data that states produce, but the income variable has switched signs and is no longer statistically significant. Column three looks at the nondemocracies. Once again IMF supervision is associated with the production of more data, and this effect is much stronger than among democracies. Moreover, the income variable is now negative and highly significant: rich authoritarian states produce fewer observations than poor ones. Column four shows that these results are unchanged when we add dummy variables to control for states that were born from the collapse of the Soviet Union and Yugoslavia and which have therefore produced fewer data than longer-lived countries.

The results suggest we should expect countries to produce fewer data when they are authoritarian, and among the authoritarian states, when they have higher incomes and less contact with the IMF. This implies that cross-national datasets tend to exclude authoritarian states, particularly when they are wealthy and high performing. This may lead to the false inference that democracies outperform nondemocracies.

Data, Model, and Variables

To estimate the effect of regime type on infant and child mortality, we use a dataset that includes all 168 states that were sovereign between 1970 and 2000 and had populations over 200,000.[10] Since observations of infant and child mortality are only available from UNICEF, WHO, and the World Bank at five-year intervals, we collapse the data into five-year panels.

Unfortunately, the dataset still contains a large amount of missing data, which is almost certainly nonrandom. To mitigate this problem we employ multiple imputation, using statistical inference to estimate the missing values.

The imputation is carried out with a program called Amelia (Honaker et al. 2001). Amelia uses the values from states where data are not missing to generate estimates for states where they are missing. Rather than produce a single estimation for each missing observation, Amelia generates multiple datasets (in this case, five), each with unique

[10]To ensure that the results were not unduly influenced by the Soviet-era data, separate tests were carried out using a dummy variable for the former Soviet states. Separate estimations were also run using a dummy variable for states with populations below one million, to make sure the findings were not biased by the inclusion of small states in the sample. Although these two variables reached statistical significance in some specifications, they never altered the significance levels of the other variables and had little substantive effect.

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. The estimations are then run with each of the five datasets, and the results are combined by using a procedure designed to reflect the appropriate uncertainty levels for each of the missing values.[11] We report our estimations using both the original dataset with the missing observations (referred to as the "original dataset") and the dataset in which the missing observations are replaced with imputed values (referred to as the "filled-in dataset").

Model

There are two widely used estimation methods for pooled time-series cross-sectional data with unit-specific effects: a fixed-effects model, and Beck and Katz's (1995) OLS with "panel-corrected standard errors" (PCSE) and a lagged dependent variable. Both were used here. The fixed-effects model does a better job of controlling for country-specific effects, but it also has drawbacks: it tends to reduce or eliminate the significance of variables that change slowly or not at all and can exacerbate measurement error among the right-hand side variables. The PCSE model with the lagged dependent variable does not control for country-specific factors as well as the fixed-effects model, but it does a better job of estimating the influence of fixed or slowly changing variables. We control for exogenous global health trends (and other contemporaneous shocks) with dummy variables for five of the six five-year periods in the dataset. Each of the right-hand side variables is lagged by a single five-year period.

Dependent Variables

There are 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). Each institution bases its estimates on 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. Although the measures produced by the three agencies are highly correlated, their coverage varies slightly; we hence carry out tests with each of these five datasets. To keep the number of tables at a manageable level, only the complete results of tests run with the child mortality data from UNICEF—the most complete and transparent dataset— are listed here, while the results of the other tests are summarized in Table 5.[12]

Independent Variables

We use two 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, we combine these two measures to produce a 21-point scale, which we call *POLITY*.

The other 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 2006). 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.[13] To explore this possibility, we use a variable that is based on the total number of years that each country has been a democracy, beginning in 1900; we take the natural log of this figure (*DEMOCRATIC YEARS*) to capture the intuition that the marginal benefits of democracy will diminish over time.[14] One advantage

[11]The procedure that combines the results across the imputed datasets was developed and generously shared by Kenneth Scheve. It produces a point estimate for each variable that is the mean of the point estimates produced by each analysis in each of the five datasets; it also produces a variance for this point estimate that is based on the average variance in each of the five analyses and the sample variance in the point estimates across the datasets.

[12]The pre-independence infant and child mortality data for the states of the former Soviet Union raise special concerns about bias and accuracy. While there is evidence of deliberate data falsification in some regions, the Soviet-era data mostly suffer from the same types of errors as non-Soviet mortality data (Anderson and Silver 1997; Velkoff and Miller 1995).

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 these datasets. Although these adjustments are imperfect, the fixed-effects model and the use of a dummy variable for the Soviet-era communist states should minimize any remaining bias from these data.

[13]Thanks to both David Laitin and Jeff Lewis for pointing this out.

[14]We also tried two other democracy measures. One was the number of democratic years since 1900 but without the logarithmic transformation; it never approached statistical significance. The other was the Alvarez, Cheibub, Limongi, and Przeworski dichotomous measure of democracy. It produced the same results as the Polity measure.

of this latter variable is that it varies over time for each country in the dataset: while POLITY retains the same value every year for countries whose level of democracy did not change, DEMOCRATIC YEARS always changes from one year to the next. This helps ensure that the effects of regime type, for states that have been continuously democratic or continuously authoritarian, are accounted for in the fixed-effects models.

Control Variables

The model includes four control variables plus dummy variables for each period. The controls were selected to 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 they are included in the model, they may reduce the substantive and statistical significance of the "regime type" variables, and consequently, underestimate the role that governments play. Hence even though the model controls for variables that are largely immune to government influence—such as the country's disease environment and its population density—it does not control for factors that may reflect government interventions, like education, sanitation, fertility rates, income inequality, or the number of doctors per capita. Omitting these intervening variables will *increase* the likelihood that democracy will be significantly correlated with infant and child mortality.

The first control variable is the log of income per capita (*INCOME*), drawn from the chain series index of the Penn World Tables 6.1 (Heston, Summers, and Aten 2002). Virtually every cross-national study finds that income per capita has a strong effect on infant and child mortality.

The second control variable is the log of population density (*POP DENSITY*). Perhaps governments find it harder to provide health care, education, sanitation, and other public goods to the poor when they are widely scattered in rural areas. Data on population density are taken from the World Bank (2004).

The third control variable is economic growth. There is strong evidence that growth is good for the poor (Dollar and Kraay 2002; Firebaugh and Beck 1994; Ravallion and Chen 1997). If a country's regime type influences its growth rate, and growth helps reduce infant and child mortality, then placing growth in the model would bias the 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 we run models both with and without growth as a control.

Finally, the fourth variable controls for the impact of HIV/AIDS on the poor. The 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. The model controls for HIV but not malaria, because high-quality data on the latter is unavailable.

While the scope for government intervention has grown as low-cost HIV prevention and treatment efforts have spread, a large fraction of the epidemic's impact in Africa in the 1980s and 1990s was caused by nongovernmental 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.

The model therefore controls for the log of the HIV prevalence rate (*HIV*), 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 the 1980 rate was probably close to zero. Hence we extrapolate backwards, assuming that HIV prevalence rates doubled from 1985 to 1990, and again from 1990 to 1995, then increased by 50% from 1995 to 2000—reflecting the maturing of the epidemic and the beginning of HIV-control efforts.[16]

[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 an indirect effect, but 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] Besides these four control variables, we explored five others that turned out to have no measurable effect on the 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. A 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 2001; Easterly and Levine 1997;

Results

Table 3 displays a series of regressions using the original dataset. Column one shows a PCSE regression with the lagged dependent variable and the five control variables, but no period dummies; all of the variables are statistically significant. When the *POLITY* variable is added in column two, it achieves statistical significance and causes *INCOME* to lose significance. In column three, the period dummies are added to the model; *POLITY* retains significance, and its coefficient grows slightly. When fixed effects are introduced in column four, however, *POLITY* (along with three of the control variables) loses significance. Columns five through seven display the same models as two through four, but with *DEMOCRATIC YEARS* replacing *POLITY; DEMOCRATIC YEARS* never reaches statistical significance. For the fixed-effects models (columns four and seven), an F test rejects the null hypothesis that the country dummies are not significant (Prob $>$ f $=$ 0.0000).

Table 4 displays the same models as Table 3, but with the filled-in dataset, in which all missing values—representing 18% of the data—have been replaced by imputed values. The estimations now employ data from all 168 countries, instead of just 150 countries, and contain almost four times the number of observations. While most of the other variables perform as well as they did with the original data, neither *POLITY* nor *DEMOCRATIC YEARS* reaches statistical significance. The results are unchanged if *GROWTH*—which might be collinear with the democracy measures—is dropped.

In sum, the democracy measures fail to reach significance in the original dataset when fixed effects and period dummies are included and in the filled-in dataset even when fixed effects and period dummies are not used.

When these tests in Table 4 are run with the alternative measures of infant and child mortality, the results are much like those described above. Table 5 summarizes these results: in the 15 tests, *POLITY* reaches statistical significance at the .05 level once—which is about what we would expect from chance alone—and the .10 level twice.

Even if the one test in which *POLITY* gains statistical significance were correct, democracy's impact on child mortality would not be large. If a state moved from complete authoritarian rule to complete democracy—changing its Polity score from one extreme to the other (−10 to 10)—its child mortality rate would only drop by about 0.2 death per thousand per year. To put this in perspective, between 1970 and 2000, mean child mortality rates fell by about 10 times that rate—a full two deaths per thousand per year—due to economic and health-related advances alone. Democracy would cause the equivalent of a single five-week boost in the existing trend.

The magnitude of this effect is much smaller than others have claimed. Przeworski et al. (2000), for example, 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.[17] The tests above suggest the true infant mortality gap between dictatorships and democracies is close to zero.

Theories of Democracy and Redistribution Revisited

If democracies spend more money on social services generally, and health care particularly, why does this have so little effect on infant and child mortality rates? A simple model suggests that governments can only lower infant mortality rates when they target low-income households. If we drop a single, implausible assumption in the Meltzer-Richard model, there is no reason to expect democracies to favor these households.

Consider a model in which households seek to maximize their utility, subject to various constraints, including their income and the price of goods and services they seek. Among these goods and services are those that minimize the probability of infant and child mortality, such as food, clean water, pre- and postnatal care, immunization, and other medical services. At the household level,

Keefer and Khemani 2003). A fifth variable looked at the impact of communism. None of these variables was robustly associated with the dependent variables; nor were there sufficiently compelling theoretical reasons to keep them in the model. Some of them may, however, help account for the large country fixed effects found in this and other studies.

Finally, we also tested a dummy variable for 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 1 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 10 or 20 years after the transition, perhaps we would see much faster improvements. The Regime Change variable was often statistically significant, but it never affected the significance levels of the other variables. Because it may be collinear with the democracy variables and slightly reduces their impact, it is omitted from the estimations in Tables 4 and 5.

[17] 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 this procedure were used, it would almost certainly reduce the size of any substantive effect that democracy might have on child mortality.

TABLE 3 Original Dataset (Dependent Variable Is Log of Child Mortality)

	1 LDV Only	2 LDV Only	3 LDV & Period Dummies	4 FE & Period Dummies	5 LDV Only	6 LDV & Period Dummies	7 FE & Period Dummies
INCOME	−.027*	−.029*	−.031**	−.24***	−.029*	−.022	−.25***
	(.01)	(.014)	(.011)	(.061)	(.013)	(.013)	(.059)
HIV	.1***	.11***	.11***	.31***	.1***	.11***	.31***
	(.0066)	(.007)	(.0063)	(.033)	(.0066)	(.0074)	(.033)
POP DENSITY	−.015***	−.015***	−.015***	.078	−.015***	−.015***	.053
	(.0014)	(.001)	(.0012)	(.13)	(.001)	(.001)	(.13)
GROWTH	−.0079***	−.0077***	−.0071***	−.00084	−.0074***	−.0074***	−.00058
	(.00088)	(.00085)	(.00088)	(.0019)	(.00088)	(.00063)	(.0019)
POLITY	–	−.0011***	−.001**	−.0021	–	–	–
		(.00033)	(.00035)	(.002)			
DEMOCRATIC	–	–	–	–	−.0043	−.0032	−.025
YEARS	–			–	(.0029)	(.0031)	(.019)
Observations	282	282	282	490	282	282	491
Countries	150	150	150	149	150	150	149
R-squared	.99	.99	.99	.99	.99	.99	.99

All of the independent variables are lagged for one period. Standard errors are listed in parentheses below the coefficients. Constants, lagged dependent variables, period dummies, and country dummies are not reported.
*Significant at .05 level.
**Significant at .01 level.
***Significant at .001 level.

TABLE 4 Filled-In Dataset (Dependent Variable Is Log of Child Mortality)

	1 LDV Only	2 LDV Only	3 LDV & Period Dummies	4 FE & Period Dummies	5 LDV Only	6 LDV & Period Dummies	7 FE & Period Dummies
INCOME	−.13***	−.13***	−.15***	−.18***	−.13***	−.15***	−.18***
	(.028)	(.025)	(.024)	(.038)	(.028)	(.027)	(.041)
HIV	.035	.044***	.1***	.22***	.035	.095***	.21***
	(.024)	(.016)	(.011)	(.033)	(.023)	(.017)	(.031)
POP DENSITY	−.023***	−.022**	−.02***	−.021	−.023***	−.021**	−.026
	(.006)	(.0052)	(.0051)	(.016)	(.0059)	(.006)	(.014)
GROWTH	−.0046	−.0048	−.0071*	−.0033	−.0046	−.007*	−.0038
	(.0026)	(.0023)	(.0023)	(.0035)	(.0026)	(.0024)	(.0029)
POLITY	–	−.0015	−.0025	−.00096	–	–	–
		(.0011)	(.0012)	(.003)			
DEMOCRATIC	–	–	–	–	.0006	−.0068	−.012
YEARS					(.0046)	(.0052)	(.013)
Observations	1176	1122	1122	1122	1176	1176	1176
Countries	168	168	168	168	168	168	168

All of the independent variables are lagged for one period. Standard errors are listed in parentheses below the coefficients. Constants, lagged dependent variables, period dummies, and country dummies are not reported.
*Significant at .05 level.
**Significant at .01 level.
***Significant at .001 level.

Table 5 Summary of Results with Alternate Measures of Infant and Child Mortality, Using Filled-In Data

	LDV Only	LDV & Period Dummies	FE & Period Dummies
CMR World Bank	No	No*	No
CMR UNICEF	No	No*	No
CMR WHO	No	Yes	No
IMR World Bank	No	No	No
IMR UNICEF	No	No	No

"Yes" indicates POLITY reached statistical significance at the .05 level.
*Indicates POLITY reached statistical significance at the .10 level.

the demand for these goods and services is relatively price inelastic: households will spend whatever they can to keep their children alive.

Now consider the role of the state. By providing public goods and transfers, governments can lower the price of these mortality-averting goods and services. But recall that demand for them is relatively inelastic: as long as households are not income constrained (or credit constrained), they will buy them anyway.

This implies that government subsidies will not affect national infant and child mortality rates, as long as those subsidies go to middle- and upper-income households, who would purchase these mortality-averting goods and services anyway. Public goods and transfers can only reduce infant and child mortality rates when they are received by households that are income constrained and would not otherwise be able to afford the goods and services that ensure child survival.[18] This implies that the government's impact on infant and child mortality rates is largely a function of the assistance it gives to low-income households.

Democratic regimes will hence only affect infant and child mortality rates if they deliver more benefits to low-income households than nondemocratic regimes. If we loosen an implausible assumption in the Meltzer-Richard model, however, there is no reason to expect they would.

The common interpretation of the Meltzer-Richards model is that the downward redistribution of income will penalize the rich and help the lower and middle classes. But there is no reason to assume that in helping the middle class, democracies will also help the poor. In the Meltzer-Richards model, the policies of a democratic government are designed to benefit the median voter, who lies in the middle income quintile; those in the bottom income quintiles will enjoy the benefits of any downward redistribution only if the government is constrained to provide flat-rate benefits to all of its citizens. This is implausible: governments are adept at channeling benefits to the constituencies they wish to favor. If we allow the government to distribute goods and services more selectively, there is no longer any simple median voter result. To predict who the government will target, we must consider additional variables: the specific design of democratic institutions and the class coalitions they produce (Iversen and Soskice 2006); the collective action capacities of the lower quintiles; or perhaps the tendency of the poor in developing states to vote along clan, ethnic, or religious lines, instead of class lines (Varshney 2000).

This does not mean that the additional public goods produced by poor democracies benefit no one, only that they do not benefit the poor. Health, education, and public infrastructure projects may provide jobs, patronage, and health and education subsidies to those in the middle and upper quintiles.[19] But if they deliver, at the margin, mortality-averting goods and services to households that are not income constrained, these subsidies should have little or no net effect on aggregate infant and mortality levels.

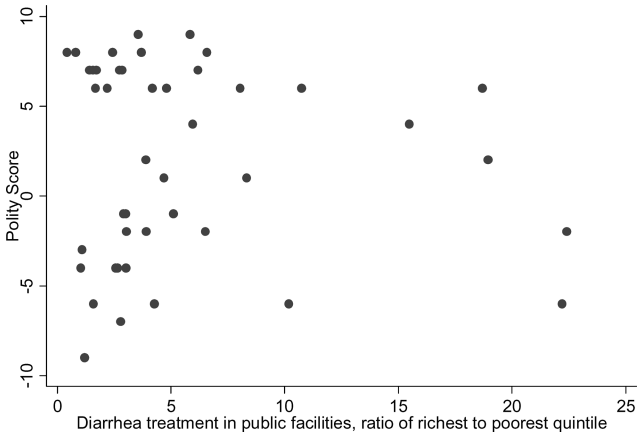
This model is consistent with several recent studies of health and public policy. Filmer and Pritchett (1999) find that public spending has virtually no impact on child and infant mortality. According to Bidani and Ravallion (1997), public spending is only welfare improving when the recipients are poor. This implies that public spending will have no impact on infant and child mortality unless it delivers benefits to low-income households.

The model is also consistent with data gathered by the World Bank on the income quintile of households that used government health services in 45 developing countries (Gwatkin et al. 2004). The study compared the treatment rate in public health facilities of the bottom and top income quintiles for the delivery of infants and for two types of easily treated childhood illnesses—diarrhea and

[18] Strictly speaking, if public goods can be directed towards a specific group, they are actually quasi-public goods.

[19] For examples, see Deolalikar (1995); Castro-Leal et al. (1999); Barat et al. (2003); and Reinikka and Svensson (2004). Another possibility is that poor democracies deliberately produce public goods that are socially inefficient. Robinson and Torvik (2005) suggest that governments in low-income democracies will purposely fund socially inefficient "white elephant" infrastructure projects when they are unable to make credible commitments to their supporters. A model developed by Mani and Mukand (2002) suggests that as governments move from authoritarian to partially democratic, they will tend to favor high-visibility public projects over essential public goods. Both models imply that democracies will produce more social spending but few additional social benefits.

FIGURE 3 Child Health Treatments and Democracy in 42 Developing Countries



acute respiratory infections. Since the upper quintiles can afford private care, we might expect that treatment rates in *public* health facilities would be higher in the lower quintiles. Yet the treatment rate for diarrhea was higher in the richest quintile in 38 of the 42 countries with data; it was more than twice as high in 12 of them. For acute respiratory infections, the treatment rate was higher in 28 of the 40 countries with data. For deliveries, it was higher in 43 of the 45 countries. In other words, public health services generally helped the rich more than the poor.

Did the democracies in this group produce more benefits for the poor than the nondemocracies? Figure 3 is a scatterplot that shows each country's democracy score (using the Polity scale) and the rich-poor treatment ratio for childhood diarrhea; there is no obvious pattern. In Table 6, we regress the rich-poor treatment ratio for deliveries, acute respiratory infections, and diarrhea on each state's Polity score. We try these estimations both with and without income per capita as a control variable. There is no relationship, among these 45 states, between a country's regime type and the distribution of government health services between rich and poor. In other words, poor children did not receive a larger share of public health benefits in democracies than they did in nondemocracies.

Conclusion

Social scientists know surprisingly little about what types of governments tend to improve the welfare of the poor. The urgency of this issue is self-evident: in 2001, almost half of the world's population—some 2.73 billion people—lived on less than $2 a day. About four million newborn babies die each year; three-quarters of them could be saved by low-cost interventions.

While there is a well-developed literature on the politics of income, welfare, and redistribution in advanced industrialized democracies, poverty levels in these countries are trivial (Alesina, Glaeser, and Sacerdote 2001; Iverson and Soskice 2006). Where poverty is truly severe—in the developing world—our understanding of government's role is much weaker.

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 168 states dropped by about half. The gains were widespread: child mortality rates dropped in 163 countries and rose in just six. 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, antibiotics for neonatal infections, and oral rehydration therapy (Cutler, Deaton,

TABLE 6 Democracy and Public Health Inequalities in 45 States

	Deliveries	Deliveries	ARI Treatment	ARI Treatment	Diarrhea Treatment	Diarrhea Treatment
Polity 1999	−.076	−.0032	−.3	−.24	.0089	.017
	(.16)	(.16)	(.23)	(.19)	(.02)	(.019)
GDP per capita 1999 (PPP)	–	−.0009**	–	−.00062	–	−.000076
		(.00033)		(.00044)		(.000058)
Observations	45	45	40	40	42	42
R-squared	.005	.086	.04	.05	.03	.03

Standard errors are listed in parentheses below the coefficients.
**Significant at the .01 level.

and Lleras-Muney n.d.; Hill and Amouzou 2004; Hill and Pande 1997).

Over the same three decades, there was also a dramatic rise in the prevalence of democracy; yet we find little evidence that the rise of democracy contributed to the fall in infant and child mortality rates. Democracy unquestionably produces noneconomic benefits for people in poverty, endowing them with political rights and liberties. But for those in the bottom quintiles, these political rights produced few if any improvements in their material well-being. This troubling finding contradicts the claims made by a generation of scholars.

This finding highlights the importance of understanding why democracies perform so badly for their poorest citizens and what can be done to improve their record. Keefer and Khemani (2005) argue that the failure of democracies to help the poor is largely caused by the incomplete information of voters, the difficulty that politicians have in making credible promises, and social polarization. Stasavage's (2005) study of Uganda found that its democratic transition had a strong impact on the government's decision to support universal primary education, but that this outcome was contingent on the availability of information about government policy and social polarization. Habyarimana et al. (2006) suggest that ethnic polarization in Africa tends to inhibit the provision of public services.

It also raises questions about whether other, unmeasured political factors can distinguish states with good infant mortality records from states with bad ones. It is clear that economic growth and global health trends matter; 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 cannot be caused by 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 much, what political factors do?

Finally, this study points out a type of selection bias that is typically overlooked. It shows that countries tend to produce fewer data on key variables when they are less democratic and are unconstrained by IMF agreements. This implies that authoritarian states with good economic records are likely to be undercounted in cross-national studies—which can lead unsuspecting scholars to underestimate the collective performance of authoritarian regimes, and hence overestimate the benefits of democracy. Correcting this bias could alter some widely held beliefs about the merits of democratic government.

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