Democracy and Human Development

ABSTRACT

Does democracy improve the quality of life for its citizens? Scholars have long assumed that it does, but recent research has called this orthodoxy into question. This paper reviews this body of work, develops a series of causal pathways through which democracy might improve social welfare, and tests two hypotheses: a) that a country's level of democracy in a given year affects its level of human development, and b) that its stock of democracy over the past century affects its level of human development. Using infant mortality rates as a core measure of human development, we conduct a series of time-series cross-national statistical tests of these two hypotheses. We find only slight evidence for the first proposition, but substantial support for the second. Thus, we argue that the best way to think about the relationship between democracy and development is as a time-dependent, historical phenomenon.

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From classical Greece to the present era, writers have usually assumed that the institutions of democracy propel a political dynamic favorable to the needs and interests of the less advantaged citizens in a society. While there has always been controversy over the question of whether democracy enhances economic growth (Kurzman, Werum, Burkhart 2002; Przeworski et al. 2000), the consensus view has been that democracy enhances human development (e.g., Boix 2001; Brown & Mobarak 2004; Brown & Hunter 2004; Dreze & Sen 1989; Ghobarah, Huth & Russett 2004; Lake & Baum 2001; Lenski 1966; Lipset 1959; Meltzer & Richards 1981; Muller 1988). The logic of this argument rests largely on the idea that popular participation in government empowers ordinary citizens—including the very poor—and should, as a result, lead governments to be more accountable to their interests. For Aristotle, Madison, and most latter-day political economists, it is nearly axiomatic that democracy serves as a mechanism for redistribution.

Recently, this consensus opinion has been strongly challenged. Several studies argue that there is no positive correlation between regime type and various measures of human development, or that these relationships are not especially robust (Gauri & Khaleghian 2002; McGuire 2004; Ross 2006; Shandra et al. 2004). These large-sample crossnational studies are bolstered by a good deal of qualitative evidence. Some of the most dramatic improvements in human development over the course of the twentieth century have occurred under the auspices of authoritarian rule (e.g., in the East Asian NICs and in communist countries), while many democratic societies in the developing world have been characterized by persistent disparities in wealth and high levels of poverty (e.g., India, sub-Saharan Africa, and many Latin American countries). Moreover, some of the causal pathways by which democracy was previously thought to influence the welfare of the poor seem rather dubious in light of recent empirical analysis. While conventional wisdom assumed (largely on the basis of the experience of OECD countries) that democracy would lead to higher social spending and this, in turn, would enhance the welfare of the poor, it turns out that there is little or no correlation between public spending and human development outside the OECD (Filmer & Pritchett 1999; McGuire 2004). The stipulated mechanisms of the welfare state do not lead—at least not in any consistent fashion—to an improvement of social welfare, as measured by mortality, literacy, and other human development outcomes. Thus, even if one brackets the question of economic growth (thereby assuming that regime type is growth-neutral), the case for democracy as a welfare-enhancing mechanism appears shaky. An age-old assumption faces serious challenge.

Thus far, the debate between proponents and skeptics has centered largely on democracy's contemporaneous relationship to human development.[1] Empirical work tests the relationship between democracy today and human development in the following year or decade (depending upon the time lag of the model). Theoretical discussions follow suit. In this paper, we introduce the possibility that democracy's developmental effects might be longer-term, characterized by a distal rather than proximal causal relationship.

We begin by revisiting the traditional theoretical arguments in an attempt to show what a difference history might make. We proceed to test two hypotheses in a series of crossnational regression tests with the infant mortality rate (IMR) as our primary measure of human development. The first hypothesis replicates the traditional causal model, linking IMR to a country's *level* of democracy in the previous year. The second hypothesis measures democracy with a *stock* index that captures a country's regime history from 1900 to the observation year. (In both instances, we measure regime type with a continuous, rather than a dichotomous, measure. We deem this essential to capturing the variegated quality of regimes throughout the world.) Our findings vindicate the latter hypothesis. A country's contemporary level of democracy has only a weak association with improved human development. By contrast, a country's historical experience with democracy has a strong and robust influence on its current level of human development. We conclude that democracy advances human development, but only when considered as a historical phenomenon.

THINKING HISTORICALLY ABOUT DEMOCRACY AND DEVELOPMENT

For the most part, the two sides of the longstanding debate between those who are optimistic about democracy's effect on human development and those who are more skeptical share one fundamental (and scarcely noticed) presupposition. Democracy is presumed to have a proximal relationship to developmental outcomes. Yet, new democracies and old democracies are not the same. While new democracies are prone to a host of problems associated with regime transition, old democracies generally enjoy higher-quality governance. It would be surprising, indeed, if the human development performance of countries moving from authoritarian to democratic rule were substantially improved over the course of the first decade or so. We surmise, however, that if a

[1] Brief tests of historical measures of democracy are included in McGuire (2004) and Ross (2006), as discussed below.

democratic form of government is maintained over a longer period of time the net effect of that regime type will be positive for the welfare of its citizens.

Regimes do not begin again, de novo, with each calendar year. Where one is today depends critically upon where one has been. Historical work suggests that democracy and authoritarianism construct deep legacies, extending back several decades, perhaps even centuries (Collier & Collier 1991; Hite & Cesarini 2004; Linz & Stepan 1996; Mahoney 2002). It follows that we should concern ourselves with the accumulated effect of these historical legacies, not merely their contemporary status. We contend, therefore, that the effects of political institutions are likely to unfold over time—sometimes a great deal of time—and that these temporal effects are cumulative. Let us consider four of the numerous possible causal pathways linking democracy and human development, taking into account the possible time-dependent nature of this relationship.[2]

First, competition among elites for voters' favor should produce a situation in which elites are accountable to the citizenry—at the very least, to a plurality of the voting electorate. Since widespread human misery is unpopular, democratically elected leaders may be more likely to concern themselves with issues of human development than leaders who maintain their positions through other means. To be sure, the latter sort of leaders might also be concerned with the potentially destabilizing effects of widespread poverty. However, they may be more likely to weather this kind of bad news than their democratic counterparts because they typically face a much smaller "selectorate" (Bueno de Mesquita et al. 2003). As long as the authoritarian regime's core constituency (e.g., the military, ruling party, and economic elites) is well compensated, it is unlikely that the sufferings of the masses will threaten their control over the state. A striking example of this can be found during the China's Great Leap Forward (Kane 1989; Riskin 1995). The massive starvation that ensued in the wake of Mao's disastrous reforms, which may qualify as the largest number of government-induced deaths in recorded history, did not threaten Mao's leadership or the leadership of the Chinese Communist Party. It is difficult to imagine such an event occurring in a democratic setting without serious negative consequences for those in power.

This argument is quite plausible when applied to disastrous policy outcomes such as famine, and the empirical results are strong. To date, no large-scale famine has occurred in a full-fledged democracy (Dreze & Sen 1989). Yet, for more complex developmental policies, where the failures

[2] This literature review draws on previous reviews of the literature by McGuire (2004) and Ross (2006).

are less obvious, less dramatic, and less easily tied to the current government, the principal-agent logic of democratic accountability attenuates. There is no obvious reason why a democratically elected government would benefit from incurring present costs for the sake of future gains unless the time-horizons of those elites have shifted to a longer-term perspective. This shift, in turn, is unlikely to occur in the early years of a recently democratized polity, where institutions are in flux, parties are nascent, and voter affiliations ephemeral. Faced with political uncertainty and instability, politicians in this context may face incentives to pursue short-run goals at the expense of long-term development (Haggard 1991). In a longstanding democracy, by contrast, it seems plausible that leaders might find it in their interest to pursue policies with benefits that lie far in the future. Since these sorts of policies are commonly recognized to be the more effective in solving human development problems than short-term ("populist") policies, we surmise that old democracies may do a better job of improving the welfare of the less advantaged. Consider, also, that the success of such policies rests on their maintenance over the long haul. An education policy may reap few immediate benefits, for example. Thus, even if elected elites in a new democracy undertake long-term policies, the fruits of these policies will be harvested only at a later date, as the polity matures.

Second, the institutions of democracy tend to foster a well-developed civil society. This is because political rights and civil rights are highly correlated,[3] and the existence of civil rights usually leads, over time, to a dense network of voluntary associations, which may be religious or secular, national or international, issue-specific or broadly pitched (Boone & Batsell 2001; Parker 1994; Webb 2004). In turn, these voluntary associations are often instrumental in providing services for the poor, perhaps in conjunction with official state bodies and/or international actors. They may also be instrumental in lobbying for legislation that addresses the needs of the poor and improves the quality of public administration (Sondhi 2000). Non-governmental organizations (NGOs) appear to have played a critical role in child vaccination campaigns, in campaigns for the treatment of HIV/AIDS, in education and health care, and in many other policies that directly affect the general welfare (Gauri & Lieberman 2004; Gauri & Khaleghian 2002; Lake & Baum 2001). The evolution of civil society is a long-term process. Voluntary associations and NGOs do not spring

[3] Democracy is not a necessary condition for the presence of a strong civil society, as can be seen in cases like Zimbabwe, a less than democratic state with a strong network of AIDS-related NGOs (Batsell 2005). (We are grateful to Even Lieberman on this point.) But it is likely that, all else being equal, civil society networks will be stronger in a democratic system than a non-democratic one.

forth overnight. Thus, insofar as strong civil societies encourage better governance and greater attention to the needs of the less advantaged citizens in a society, we can expect these causal mechanisms to kick in only over several decades. Again, the age and historic strength of democracy would seem to matter when considering human development outcomes.

Third, democracy may serve to inaugurate a culture of equality that empowers oppressed groups. In the process of granting formal citizenship rights to out-groups—lower castes and classes, peasants, racial, ethnic, and religious minorities—democracy may foster a political dynamic in which these groups conceptualize their interests as a matter of rights and take a correspondingly aggressive approach to satisfying those rights in the political, social, and economic spheres (Alvarez et al. 1998; Rubin 1997). This political dynamic, once initiated, may have important repercussions for societal human development insofar as it leads to an extension and improvement of government services and increased utilization of those services. Again, it seems reasonable to suppose that this process of norm-led change would occur only over a period of decades, if not centuries (e.g., the American civil rights movement).

Finally, we expect that older democracies will benefit from greater institutionalization in the political sphere. Although political institutionalization is difficult to define, there seems to be general consensus that procedures in a well-institutionalized polity are functionally differentiated, regularized (and hence predictable), professionalized (including meritocratic methods of recruitment and promotion), rationalized (explicable, rule based, and nonarbitrary), and infused with value (Huntington 1968; Levitsky 1998; Polsby 1968).[4] Virtually all long-standing democracies fit this description. They feature highly developed, highly differentiated systems of governance, involving both formal bureaucracies and extraconstitutional organizations such as interest groups, political parties, and other nongovernmental organizations. Thus, the length of time a democracy has been in existence serves as a rough indicator of its degree of institutionalization. By contrast, the length of time an authoritarian regime has been in existence may have little or no bearing on its level of institutionalization. Indeed, institutional reversals are common, as in the latter days of the Soviet Union or in Iraq under Saddam Hussein.[5]

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[4] The concept of institutionalization has deep intellectual roots and may be traced back to work by Henry Sumner Maine, Ferdinand Tonnies, Max Weber, Emile Durkheim, and Talcott Parsons, among others (Polsby 1968: 145).

[5] Institutional decay can occur under democratic rule too, as the cases of Venezuela and Peru have

We suspect that the reasons for this stem directly from their systems of rule. Where power is personalized, as it is in so many authoritarian settings, the development of legal-bureaucratic authority is virtually impossible. In particular, leadership succession is difficult to contain within regularized procedures and promises a period of transition fraught with uncertainties. Thus, even if a monarch or dictator adheres to consistent policy objectives during his or her rule, there may be little continuity between that regime and its successor (we employ the term "regime" here in its broader sense). The hallmark of a long-standing democracy, by contrast, is its ability to resolve the problem of leadership succession without turmoil and without extraordinary discontinuities in policy and in political organization. The framework remains intact, and this means that the process of institutionalization may continue, despite the occasional bump in the road.

More importantly, we suspect that the institutionalization of power leads to greater gains within a democratic setting than in an authoritarian setting. Institutionalization matters more under democracy. Consider the problem of establishing social order and stability in a polity and resolving problems of coordination (Hardin 1999). Noninstitutionalized polities are unstable and inefficient, almost by definition, for there are no regularized procedures for reaching decisions. However, in an authoritarian setting, a Hobbesian order may be established simply and efficiently by fiat and force. Rule by coercion, insofar as it is successful, can be imposed without loss of time and without negotiation; the threat of force is immediate. Consequently, there is less need for highly institutionalized procedures for reconciling differences and establishing the force of law. The sovereign may rule directly.

In a democratic setting, by contrast, resolving conflict is complicated and generally takes a good deal of time. Somehow, everyone must agree upon (or at least agree to respect) the imposition of society-wide policy solutions that involve uneven costs and benefits. In order to handle these quintessentially political problems, a democratic polity has little choice but to institutionalize procedures for negotiation among rival constituencies and organizations. Once these procedures are established (a process that takes time), we expect them to be more effective in resolving differences and finding optimal solutions than would be fiats imposed from above. Indeed, whatever centripetal tendencies are inherent in democracy are more likely to be in evidence when those democratic arrangements have been in operation for some time. For this reason, the thesis of

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demonstrated in recent years. Our argument is probabilistic: old democracies are more likely than new democracies and authoritarian regimes to have strong institutions.

democratic overload is much more compelling when applied to new democracies than when applied to old. Democratization is often a boisterous, obstreperous affair. Established democracies, by contrast, tend to be more restrained. In particular, the norm of incremental change is more likely to be accepted.

Thus, given sufficient time, we expect that democracies will provide greater stability and more efficient public policies. Arguably, the problem of overload arises not from institutional sclerosis (Olson 1982) but rather from insufficient institutionalization (Huntington 1968). This provides yet another reason to suppose that long-term democracies may adopt smarter social policies and may implement them with greater vigilance and flexibility. If democracy survives its often tumultuous youth, indicators of human development should demonstrate marked improvements—even if no immediate improvement was registered in the initial transition from authoritarian rule. Democracy, we conclude, is best considered as a *stock*, rather than *level*, concept. Two dimensions of democracy, history and degree, must be gauged together in order to explain a country's human development capacity.

EMPIRICS

The focus of most work on international development is economic growth. The progress of nations—"development"—is understood primarily as economic progress, and the generally recognized marker of that progress is per capita gross domestic product (GDP). The growth of GDP is probably the most-studied statistic in the social sciences and the most closely watched in the world of practical politics. Yet, this ubiquitous indicator suffers from at least one obvious flaw. GDP per capita registers the mean (average) income in countries with diverse, and changing, income distributions. Consequently, the status of the poor may be quite different in countries with the same per capita GDP. Similarly, there is no necessary monotonic relationship between growth and poverty, even within a single country, since the distribution of income in that country may change over time. For this reason, a country's per capita GDP does not provide a good depiction of human welfare at all levels of society. This simple point is merely definitional. The relevant question is whether we can do better.

One alternative is provided by income measures of poverty. Unfortunately, while income measures are useful for making within-nation comparisons they are often problematic when employed for cross-country comparisons. First, many workers in the developing world labor in the informal economy and thus have little formal "income" per se. Consumption-based measures of

income attempt to correct for this fact, but they also rest on dubious assumptions. (What is an ear of corn worth in a barter economy?) Second, when examining incomes across societies one faces problems of currency comparability. The use of purchasing power parity indices is an attempt to overcome this problem, though it involves another series of assumptions, none of which is unassailable. Third, income-based comparisons usually rely upon poverty thresholds, which are inevitably arbitrary and inevitably controversial. What income threshold should define a person who is poor—$1 a day? $2 a day? Fourth, surveys of income-poverty are conducted infrequently and do not include many countries or historical data for those countries where data are given. Thus, income-based measures of poverty tend to suffer from poor data quality and coverage. Fifth, census- and survey-based studies often reach very different conclusions about the income of the poor, results that are difficult to reconcile. Sixth, income poverty is not a direct measure of human capabilities since, as Michael Ross (2006) points out, poor people sometimes have assets they can use to smooth income flows over time. Finally, income poverty measures access to private goods, not public goods. Poor people may benefit from government- or NGO-provided assets, but income poverty measures do not reflect them. For all these reasons, income-based poverty measures are problematic.[6]

A better alternative is to be found in mortality statistics, which are widely available, crossnationally comparable (a death is a death is a death), and reasonably accurate (Sen 1998). They also sidestep the agonizing threshold problem. Mortality statistics, finally, are sensitive to the life conditions of the least advantaged. Much of the variation in mortality that one observes across populations is a product of the status of less-advantaged groups within those populations.[7]

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[6] These difficulties are discussed in Deaton (2003b), Dreze and Sen (1989), UNDP (2003: 42), Moon (1991), Moon and Dixon (1985), Morris (1979), Nissan (1993), Reddy and Pogge (2003), SahnfFoote and Stifel (2000).

[7] There is some disagreement over the degree to which IMR varies across income groups. Kanbur and Squire (1999) find a high differential mortality rate among rich and poor, while more recent work by Gauri (2005) finds somewhat smaller differences (an average IMR rate of 91 for the bottom quintile and 51 for the top quintile among 45 developing countries observed). On balance, however, calculations of IMR by quintile show that poorer groups typically experience substantially higher mortality rates than richer groups (Gwatkin et al. 2005). For example, the lowest quintile in a sample of the fifty-four countries in the 2005 *Human Development Report* for which sufficient data are available had, on average, infant mortality rates twice as

Among mortality statistics, the most useful for our purposes is the infant mortality rate (IMR), defined as the number of children who perish during the first year of life, per one thousand live births. This statistic is widely available, generally reliable, and characterized by high variance, thus providing sufficient leverage for empirical analysis. From a moral perspective, we may grant it priority over other mortality indicators since the loss of an infant's life represents the loss of a whole life, while mortality experienced later in the life cycle represents the loss of a smaller portion of a life.

To be sure, deaths after the age of one also matter, and insofar as one is interested in a complete picture of life and death one might prefer to employ life expectancy as a measure of human wellbeing around the world. Unfortunately, this statistic is often based on data that are actually a series of "extrapolations using child mortality and assumptions about countries' characteristic life tables (e.g., 'North' or 'South' models)" (Filmer & Pritchett 1999: 1312). Consequently, statistics on life expectancy are rather unreliable for most countries prior to the mid-1990s (Murray 2004). Empirically, IMR also forms a more useful measure because its variance is considerably greater. This, of course, is a product of the greater vulnerability of human beings during the first year of life, where moderate differences in environment (health care, nutrition, shelter, and so forth) may translate into great differences in mortality rates. In any case, the two statistics are highly correlated (see Table 1), as is to be expected, since IMR is a principal component of estimated life expectancy.

Child mortality, defined as the number of children (per 1000) who die in the first five years of life, offers an option whose vices and virtues lie somewhere in between infant mortality and life expectancy. Relative to IMR, the historical data are not as plentiful and the variance not so great. For these reasons, and because IMR and under-five mortality are so highly correlated (Pearson's $r = 0.992$), we employ only IMR in the following tests.

Another option that one might consider begins with life expectancy but adjusts that statistic to account for the wellness of a person's life. The intuition is that since our ultimate concern is with wellbeing, we must consider not simply the quantity of life but also its quality. John Broome (2004: 261) suggests a concept that he calls "wellbeing-adjusted life years," calculated by multiplying a person's total life (or life expectancy) by the average quality of her life across that span of time. This seems simple enough until one contemplates the problem of how to arrive at such a measurement of wellbeing throughout the life course. One such approach reduces the complex issue of wellbeing to

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high as those found in the richest quintile.

the relatively simple dimension of health, hence the concept of "quality-adjusted" or "disability-adjusted" life-years (DALYs) (Broome 2004: 261; Murray 1996). Even so, the health of populations is exceedingly difficult to measure, requiring many epidemiological assumptions (e.g., with respect to the prevalence of diseases across poorly studied populations). These assumptions are multiplied if the base concept is life expectancy, for the reasons noted above. Scholars have managed to produce estimates of DALYs for a wide range of countries in recent years. However, there is little prospect of extending this data back in time, thus precluding time-series analysis (Mathers et al. 2001).

We consider one final option for measuring wellbeing here. This concerns various composite indicators, such as the Human Development Index (HDI), which combines indicators of mortality, education, and income (per capita GDP), or the Physical Quality of Life Index (PQLI), which combines indicators of mortality and education.[8] At first glance, the composite approach to wellbeing is appealing, precisely because the various components are measurable and the resulting index is multidimensional. The problem with a composite view of human wellbeing is the aggregation problem encountered by all composite indices. Which components shall we choose and how shall we weight them? It turns out that the three components of the HDI are only reasonably well correlated with one another (Pearson's r ranges from 0.5 to 0.8). The HDI also weights each component equally, though, in our view, they are not equally valuable. (We presume that life is more important than education and income.) Moreover, as discussed above, per capita GDP reflects aggregate economic output averaged across society, and does not directly reflect the wellbeing of the least advantaged citizens in a society. Thus, the HDI, like most aggregate indices, is problematic if regarded as a single-shot indicator of human wellbeing (Hicks & Streeten 1979; Sagar & Najam 1998; Silber 1983). If one prefers a basket of indicators, it might be advisable to choose a set of variables that are more highly correlated, such as the PQLI. But in the end, that approach offers no real advantage over IMR, precisely because these other human development indicators are so strongly associated with one another.

We have now reviewed the normative and empirical properties of six categories of indicators that might be employed to measure the ineffable concept of human development: GDP per capita, income-based poverty measures, mortality-based measures (infant mortality and life expectancy),

[8] On the HDI, see the *Human Development Report* (various years), Ul Haq (1995), Streeten (1995), Streeten et al. (1981). The PQLI combines infant mortality, life expectancy, and literacy into a single index (Morris 1979; see also Moon 1991; Moon and Dixon 1985; Nissan 1993).

quality-adjusted mortality indices (e.g., DALYs), and composite indicators (e.g., the HDI). Table 1 provides descriptive statistics, a correlation matrix, and a factor analysis for most of these variables (those for which sufficient data are available).

We argue that of all these indicators infant mortality, or the highly correlated child mortality, provides the best summary indicator of the life-conditions of the world's least advantaged citizens. To be sure, IMR is certainly not the only plausible indicator, and we do not wish to dismiss other efforts based on multiple indicators. But insofar as one must choose, for any given project (no collection of indicators can ever be truly comprehensive), IMR is an excellent summary measure, focusing on a dimension of human wellbeing that is both essential and broadly reflective of the welfare of the least advantaged within each nation-state. Due to its normative importance, conceptual validity, and the availability of fairly reliable data, IMR offers the best single measure of human development currently available.

MEASURING INFANT MORTALITY

To clarify, we understand the concept of "human development" to refer to the aggregate welfare of societies, with particular attention to the welfare of the least advantaged citizens within those societies. We employ IMR as a summary indicator of this underlying concept.

Scholars have compiled two important global IMR datasets in recent years, one sponsored by UNICEF (Hill et al. 1999) and the other by the World Bank (2003) (see review in Ross 2006). Reassuringly, these two measurements of IMR are highly correlated (r=0.996). Not surprisingly, results using either variable are quite similar. We show results only for the latter (drawn from the *World Development Indicators* dataset [World Bank 2003]) because it has broader country coverage, and for that reason is less vulnerable to sample biases. (The substantive interpretation of the results is the same for both.)

We should note that because we employ a fixed-effects format in most of our statistical tests, the principal methodological issue is less the crossnational comparability of the data than the within-country longitudinal comparability. For example, if surveys of IMR in Sri Lanka employ a somewhat different methodology than surveys of IMR in India this is less problematic than if surveys within Sri Lanka or India change their methodology, without making subsequent corrections in previously collected data. (Some of our analyses also introduce year-specific controls, which should compensate for any global changes in methodology.)

In analyzing the causes of infant mortality it is important to make some correction for the

bounded, uneven nature of this phenomenon. Although IMR measures increments of one (per 1,000 live births), we cannot interpret these increments as representing equal policy achievements. (Presumably, it is easier to lower IMR from high levels than from low levels.) The "distance" from 100 to 101 is not equivalent to the distance from five to six if we are interested in the question of what actually causes IMR. A country with a very high IMR may experience a noticeable improvement with only a marginal investment of funds, while at low rates an improvement in the rate of infant mortality is likely to cost a great deal more. It is no simple matter for Japan to improve on its current rate of 6 deaths per 1,000 live births. This is a product of the statistic itself, which cannot go below 0 and which consequently does not vary freely at its lower bound. Thus, as a dependent variable IMR is usually measured as an elasticity, i.e., as a percentage change in the outcome. We achieve this by transforming infant mortality rates into logarithmic form (by taking the natural log of IMR).

Since IMR data are not available on an annual basis for many countries, we adopt two complementary strategies to avoid the potential bias associated with non-random missing data (see King et al. 2001). (We do not want some countries to receive a great deal more weight in the analysis than others simply because they happen to have more complete data.) First, we interpolate missing data to create a more complete time-series for each country. This increases the potential sample from 4213 (as drawn from World Bank 2003) to 7418, a sizeable augmentation. However, in no case do we extrapolate missing data beyond the first and last data points for a country. And in no case are there more than two or three consecutive years of missing data. Note that IMR data are highly regular; in the short run, temporal changes tend to follow well-defined paths for each country. In this situation, the technique of interpolation is relatively unproblematic. (By contrast, one would hesitate to interpolate missing data for growth or inflation, since these factors vary irregularly from year to year within a country.) Given the highly "predictable" nature of IMR from year to year, it makes sense to work with a complete annual dataset that is close to what we suspect is the actual trajectory for each country, rather than a dataset that is much smaller, irregularly spaced and likely biased by the systematic omission of data from the poorest countries.[9]

[9] Another alternative would be to interpolate missing data and then employ data at less frequent intervals. This reduces the time periods available for analysis and can complicate the error-correction procedure. In other respects, it is likely to lead to results that are similar to those reported here. Table 3 reports the results for such an analysis on data measured at three-year intervals, with very similar findings.

Second, in some analyses we employ the technique of multiple imputation developed by Gary King and his colleagues (King et al. 2001). This procedure uses AMELIA software (Honaker et al. 2001) on the original (not interpolated) data for IMR and all other variables used in the analysis to impute missing values for all variables. It generates a series of imputed datasets with full coverage, and the resulting analysis takes into account the variation across the different estimates of the imputed data as a measure of the estimates' reliability. This increases the number of observations to 6233.

Note that most of the following analyses are conducted with interpolated IMR, but not additional imputed data. A few analyses are conducted without any data additions (i.e., with the raw IMR data as provided by the World Development Indicators 2003 dataset). Results for key variables are stable across these differently constructed samples.

MEASURING DEMOCRACY

There is no fully satisfactory measure of regime type (Munck & Verkuilen 2002), and the options are considerably reduced when one requires a measure that provides a large sample of countries over a long period of historical time. We consider it desirable to measure the quality of regimes in as differentiated a fashion is possible, thus precluding dichotomous measures of democracy (e.g., Przeworski et al. 2000). Among the continuous measures, the only measure with broad historical coverage is the Polity2 variable, drawn from the Polity IV dataset (Marshall & Jaggers 2000). This variable measures the extent to which democratic or authoritarian "authority patterns" are institutionalized in a given country. It takes into account how the executive is selected, the degree of checks on executive power, and the form of political competition.

The Polity2 variable is, in principle, highly sensitive (coding ranges across a 21-point scale). It also offers extensive country (all sovereign polities except micro-states) and historical coverage. Moreover, it allows us to consider both the degree of democracy in any given country-year and its duration over a long period of time (the dataset begins in 1800). The Polity dataset, however, imposes two notable costs. First, the rules used to create the key variable, Polity2, are dizzyingly complex. The Polity User's Manual makes a valiant effort to explicate coding procedures, but the methods remain rather difficult to unpack. Second, there are serious questions regarding measurement error in the index (Bollen & Paxton 2000; Bowman et al. 2004; Munck & Verkuilen 2002; Treier & Jackman 2003). To be sure, questions might be raised with respect to all extant, and all conceivable, democracy indices. Polity2 is likely no worse than the rest, and probably better than

most. It is, indeed, the industry standard, owing largely to the strengths noted above. Reassuringly, it correlates highly with other existing measures of democracy.[10] We do not suspect systematic errors in this index that might affect the substantive findings of this study.

To correct for Polity2's exclusion of micro-states, an exclusion that might bias our sample, we impute democracy scores for these excluded cases using other democracy indices that are conceptually and empirically close to the Polity2 measure: a) the Freedom House Political Rights indicator,[11] b) Ken Bollen's Liberal Democracy variable (Bollen 1993), c) Tatu Vanhanen's Competition variable (Vanhanen 1990), d) Arthur Banks's Legislative Effectiveness variables (I and II), and e) Banks's Party Legitimacy variable (Banks 1994). These measures of democracy take into account the degree to which citizens can participate freely in the political process, the extent of suffrage, the competitiveness of national-level elections, the degree of party competitiveness, and the degree to which the legislature affects public policy. With the additional imputed data the original Polity2 variable gains about 1500 observations, constituting roughly twenty percent of the final available sample. (Reassuringly, these additional data points do not have an appreciable effect on the results reported in subsequent tables.)

What we have referred to as a *level* measure of democracy is simply the score a country receives on the Polity2 index (scored from -10 to +10) in a given year. To create a *stock* measure of democracy we sum each country's score from 1900 to the present year, applying a one percent annual depreciation rate. This means that a country's regime stock stretches back over the course of the twentieth century, but that more distant years receive less weight than recent ones. Our expectation is that the causal effect of democracy, like other capital stocks, depreciates over time. We choose the year 1900 as a threshold period that ushered in a period in which mass democracy becomes a world-historical phenomenon (no longer restricted to the US and a few European states). We choose a one percent depreciation rate because it seems a reasonable estimation of how a long-run historical effect might play out. (For further discussion of variable depreciation rates, see below.)

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[10] Correlations between Polity2 and other democracy indices (introduced below in the text) are as follows: "Political Rights" (Freedom House) = -0.85; "Liberal Democracy" (Bollen) =0.92; "Democracy index" (Vanhanen) = 0.85.

[11] Freedom in the World, survey methodology, on the Freedom House web site: www.freedomhouse.org/research/freeworld/2000/methodology.htm

Because the historical component of this index weighs heavily on our understanding of the concept and because the Polity dataset ignores non-sovereign states in its coding procedures, we supplement the Polity2 coding with our own coding of several nation-states that were previously part of contiguous empires. The procedure is as follows. For each year that a nation-state belonged to a contiguous imperial power it receives the same Polity2 score as its imperial ruler; e.g., Estonia receives the same score as the Soviet Union from 1941 through 1990. We use this procedure only for nation-states contiguous with the empire to which they previously belonged. We assume that contiguous colonies are likely to be governed in the same manner as the imperial power itself, a dynamic less likely to be true for overseas colonies.[12]

METHOD OF ANALYSIS

The empirical tests consist of a series of crossnational estimations in which we regress the natural log of IMR against democracy, along with various controls. The resulting samples include all countries for which relevant data are available during the 1960 to 2000 time period. This allows for the construction of time-series cross-sectional (TSCS) samples that approach nearly complete global coverage of sovereign nations and of the world's population. (Even estimations with incomplete country coverage include the vast majority of the world's population, as smaller countries are more likely than large ones to be excluded from these analyses, as they are more likely to suffer data omissions.) The largest samples include 192 countries and over 6,500 observations. The smallest

[12] This re-coding affects the following countries: Albania (1900-1912, Ottoman Empire), Andorra (1900-present, France), Armenia (1900-1990, Russia/USSR), Azerbaijan (Russia/USSR 1900-1990), Belarus (Russia/USSR, 1900-1990), Bosnia-Herzegovina (1908-1917, Austria-Hungary; Yugoslavia 1929-1991), Croatia (1900-1917, Austria-Hungary; Yugoslavia 1929-1991), Czech Republic (1900-1917, Austria-Hungary), Slovakia (1900-1917, Austria-Hungary), Estonia (1900-1916 and 1941-1990, Russia/USSR), Finland (1900-1916, Russia), Georgia (1900-1990, Russia/USSR), Iraq (1900-1917, Ottoman Empire), Israel (1900-1917, Ottoman Empire), Kazakhstan (1900-1990, Russia/USSR), Kyrgyzstan (1900-1990, Russia/USSR), Latvia (1900-1917 and 1941-1990, Russia/USSR), Lithuania (1900-1917 and 1941-1990, Russia/USSR), Macedonia (1922-1990, Yugoslavia), Moldova (1900-1945, Romania; 1946-1990, USSR), Mongolia (1900-1920, China), Bangladesh (1947-1971, Pakistan), Slovenia (1900-1917, Austria-Hungary; Yugoslavia 1929-1991), Syria (1900-1917, Ottoman Empire), Tajikistan (Russia/USSR, 1900-1990), Turkmenistan (1900-1990, Russia/USSR), Ukraine (1900-1917 and 1920-1990, Russia/USSR), Uzbekistan (1900-1990, Russia/USSR), and East Timor (1976-1999, Indonesia).

"full" samples include 149 countries and over 4,200 observations (see Tables 2 and 3). Even when all interpolated data for the dependent variable are dropped, the sample still includes 159 countries (in the reduced-form model) and over 2600 observations (see Table 4, columns 1 and 2). Multiple imputation results include 199 countries and more than 6,200 observations (Table 3, columns 1-3). We include all countries for which data are available, including both developed and developing nations, in order to maximize variation on the dependent variable and to capture both the successes and failures in human development around the world. (But see Table 4 for an analysis excluding the OECD and other groups of countries.) Most of our analyses incorporate Newey-West standard errors, with a one-period (AR1) correction for autocorrelation (exceptions are indicated in the tables).

In all analyses, we lag the independent variables one time-period. This separates the dependent variable from the predictors, offering some protection against X:Y endogeneity. Tests with longer (ten year) lags and tests that instrument for current values of all variables with previous values (in an Arellano-Bond format) provide further reassurance that endogeneity issues do not greatly affect the results.

Most analyses incorporate annual data (the unit of analysis is a country-year). Since data are available only at 3-year intervals for some countries we interpolate missing data, as discussed above. In separate analyses, we conduct tests a) with data observed at 3-year intervals and b) with samples that exclude all interpolated data (see Tables 3 and 4).

Most analyses include country fixed effects. This technique removes many of the specification problems that typically plague crossnational studies, imposing a unique intercept for each country. This makes it less likely that the results suffer from omitted variable bias. The only exception would be a situation in which an unmeasured factor drives both a) the change in the independent variable and b) the change in IMR. In separate tests, we employ a series of spatial controls as a substitute for country fixed effects (see Table 3). However, because of the extreme uncertainty in model specification we have greater confidence in the fixed-effect format.

While fixed-effect regressions mitigate specification problems, they do not obviate them. In our search for dynamic (non-static) controls we try to identify factors that are measurable across countries, are important influences on IMR, and are—at least to some extent—exogenous relative to the dependent variable (IMR) and our theoretical variable of interest (democracy). This search culminated in the choice of four control variables: *GDP per capita* (logged [World Bank 2003]),

urbanization (World Bank 2003), *female illiteracy* (logit, World Bank 2003),[13] and *instability* (including assassinations, general strikes, guerilla warfare, government crises, purges, riots, revolutions, and anti-government demonstrations).[14] We anticipate a negative relationship for IMR with GDP per capita and urbanization (Pritchett & Summers 1996), and a positive one with female illiteracy and instability. Democracy, or democratic history (captured by our "stock" variable), may affect each of these controls. We assume that these effects are relatively minor and, more importantly, that any such effects should bias the direction of the regression results against our hypotheses. That is, if democracy does influence these factors it will probably have a salutary effect (Gerring et al. 2005 on the effect of democracy on economic growth). Therefore, including them in the regression models may underestimate the true effects of democracy on IMR. We also include a time trend variable that takes on a value of one in 1960 and increases by one in each subsequent year. Since trends in infant mortality rates are consistently sloped downwards, this variable controls for the possibly spurious correlation between that variable and any heavily trended independent variables.

When we remove country fixed effects, the search for relevant controls becomes more onerous, extending to any factor—spatial or temporal—that might affect the rate at which infants die in a country. In addition to the foregoing (dynamic) variables, we include the following static variables in the non-fixed effects estimations: *population* in 1960 (ln, World Bank 2003),[15] *ethnic*

[13] The World Bank (2003) does not report illiteracy statistics for certain countries that have near 100% literacy rates. For these cases, we hand-code a 0.5% female illiteracy rate. Illiteracy data for other countries are also somewhat limited. We impute missing data using the following technique. First, we fill in missing years between observed years of total adult illiteracy rates using linear interpolation. Second, we use annual regional averages to fill in missing data in the new interpolated total adult illiteracy variable. Third, we linearly interpolate missing years between observed years for female adult illiteracy. Fourth, we use the interpolated data for total illiteracy to impute values for interpolated female adult illiteracy. Fifth, because the data are bounded between 0 and 100, we take the logit of the imputed female illiteracy variable [logit=ln(x/(1-x))].

We also used multiple imputation to replace missing data in the entire dataset (see discussion in text), a process that employed the original World Bank variable.

[14] We standardize and add these variables, drawn from the Banks (1994) dataset, together to form a composite index.

[15] We treat population as a static variable, measured in the first year of the analysis, in order to minimize endogeneity problems. Treating it as a dynamic variable would introduce endogeneity with the

fractionalization (the likelihood that two persons randomly chosen from a population belong to different ethnic groups [Alesina et al. 2002]), *latitude* (absolute value of distance from the equator, ln, LaPorta et al. 1999), Muslim (percent Muslims; CIA *World Factbook* [on-line]), *Africa* (dummy), *Asia* (dummy), *Latin America* (dummy), and *Socialism* (La Porta et al. 1999). We anticipate positive signs for population, ethnic fractionalization, Muslim, Africa, and Latin America, and negative signs for latitude, Asia and Socialism. We have no doubt that other factors could be proposed, and some of those chosen here might be questioned, either on theoretical or empirical grounds. However, we doubt that a different selection of controls would alter the substance of the results reported here. In any event, we regard these tests as robustness checks for the fixed-effect models.

The Appendix provides descriptive statistics for all variables used in the following analyses.

RESULTS

To reiterate, our twin research questions concern the possible causal effects of contemporary democracy (a level variable) and democratic history (a stock variable) on annual (or three-year) variations in infant mortality rates. We hypothesize that a country's stock of democracy, but not its current regime status, will be associated with a lower rate of infant mortality in the following period, all other things being equal. In the following tests, we present these two sets of results side by side. In each case, the fit of the models is quite good, with F-tests significant at the 0.0001 level and high R^2 values.[16]

Table 2 displays a series of fixed-effect regressions with different model specifications. Models 1 and 2 include a reduced-form model including only the democracy level and stock variables, respectively, and a time trend control. Models 3 and 4 introduce two additional control variables (GDP per capita and urbanization) to capture the effect of economic development. Models 5, 6 and 7 add two additional controls: female illiteracy and a variable intended to measure political conditions in a country that might have strong effects on human development: the level of

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outcome since lower IMR is strongly correlated with lower population growth, a bi-directional relationship that is difficult to model.

[16] This R^2 is harvested from the first "phase" of Newey-West regressions, before the error correction process. Note that the use of fixed effects and the various time control variables inflates the R^2 values obtained here. We report them as a measure of fit for the interested reader, without placing much substantive emphasis on them.

political instability. We regard columns 5, 6 and 7 as "benchmark" models, since they include a full selection of plausible controls in a fixed effects format with a time trend and correction for autocorrelation. Model 7 introduces democracy level and stock together in the same estimation.

Models 8 and 9 substitute *T-1* annual year dummies for the trend variable to provide another means of modeling change over time. Models 10 and 11 provide yet another approach to modeling time effects, this time with a lagged dependent variable (and fixed effects, resulting in a Least Squares Dummy Variable, or LSDV, estimator; see Beck & Katz 2004). The coefficient for the lagged dependent variable is quite high (0.97-0.98), which is not surprising given the strongly trended nature of IMR data. The inclusion of the lagged dependent variable means that the coefficients for the remaining variables capture only their short-term effects. To calculate their long-term effects, we divide the coefficient by one minus the coefficient of the lagged dependent variable (Beck & Katz 2004). This generates a long-run coefficient of 0.028 for the level variable and 0.003 for the stock variable. Models 12 and 13 revert to the benchmark equation (employed in models 5 and 6), but lag the two democracy variables by ten years (instead of the usual single-year lag). This addresses the possibility that the causal relationship between democracy and human development may have a long time lag.[17]

Table 3 provides further specification tests. Models 1-3 employ multiple imputation to address concerns over the possible biases introduced by missing data (see Ross 2006).[18] Smaller, poorer countries tend to have less data available, and the exclusion of those non-randomly missing cases could result in a biased sample that might affect results (King et al. 2001). Results for the fully imputed dataset are quite similar to those for our narrower sample, giving us greater confidence in both samples. Models 4 and 5 test the benchmark equation on data drawn from 3-year (rather than annual) intervals. This accords with the infrequent nature of data collection in many developing countries. Reassuringly, results are quite similar to those generated from our annual samples.

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[17] It also may help alleviate concerns about endogeneity, or reverse causality, between infant mortality and democracy, though we find this possibility much less likely. We see no reason to suppose that decreases in IMR might cause increases in the quality of democracy in a country.

[18] As recommended by King et al. (2001), the multiple imputation process included all the variables used in this study, plus a series of other variables plausibly related to the variables used here: trade/GDP (World Bank 2003), King and Zeng's (2001) measure of deviation from the global IMR mean, the UNICEF infant mortality measure (Hill et al. 1999), and total illiteracy (World Bank 2003).

Models 6 and 7 revert to annual data but include a series of static controls in place of country fixed effects. Models 8 and 9 replicate 6 and 7 except that they include annual dummy variables instead of the trend variable. Finally, models 10 and 11 test the benchmark equation using the Arellano-Bond technique rather than the Newey-West or LSDV procedures. This method combines first differencing with a series of lags, equivalent to the total number of prior observations in the dataset, for each variable in the model (Arellano, Bond 1991).

Table 4 imposes a series of restrictions on the full sample. Models 1 and 2 exclude all data that were interpolated for the dependent variable (as described in a previous section); it thus represents the data as drawn from the WDI dataset, without further additions. Models 3 and 4 exclude the decile of most autocratic countries in the full sample, while models 5 and 6 exclude the most democratic decile, thus eliminating the potential impact of extreme cases on either end of the democracy continuum. Subsequent models exclude various regions and groups of countries around the world: Asia (models 7 and 8), Latin America (9 and 10), Africa (11 and 12), Middle East (13 and 14), and the OECD (15 and 16).

With a few exceptions, most of the control variables perform as expected. Higher levels of economic development, lower rates of female illiteracy and less political instability are generally associated with lower rates of infant mortality. For the key theoretical variables (in bold), the patterns displayed in Tables 2-4 are striking.

Arguably, the most important test is contained in Table 2, model 7, which tests the level and stock variables together in the same benchmark model. Here, the inclusion of a contemporary measure of democracy leaves the coefficient and standard error for the stock variable unaffected. However, the democracy level variable shows a *positive* (though not significant) relationship to IMR. (These results are stable when we lag the stock variable by two years so as to better disentangle its effects from the democracy level variable, which has a one-year lag.) This inclines us to conclude that long-term democratic stock, not the status of the current regime, causes human development to occur.

In other tests, as well, the democracy level variable performs inconsistently. In some specifications it is associated with lower numbers of infant deaths, but the relationship reaches conventional levels of statistical significance in the expected direction only occasionally. The inconsistency of the findings for democracy level suggests that the true relationship between democracy and IMR is likely an historically mediated one.

The results obtained for the democracy stock variable are much more robust, as shown in Tables 2-4. We also note that this way of measuring democracy is less subject to certain identification problems. Both democracy (at *t-1*) and IMR (at *t*) may be caused by some underlying factor that is unmeasured. If so, these models are misidentified. However, it is less likely that a stock measure of democracy will be subject to this sort of problem, since year-to-year changes in this variable are a product of its (very long) history. An additional year of high-quality democracy matters more for Uganda (a new democracy) than for the United States (an old democracy). This means that the slope of the democracy stock variable is quite different from the slope of the democracy level variable for any given country. Granted, a country's accumulated stock of democracy is also the product of underlying causes; it is not an unmoved mover. Yet, insofar as these underlying causes comprise static features of a country—e.g., its prior history, demography, geography, or culture—they are captured in the country fixed effect and do not affect those results. Our purpose here is to test the effect of democratic stock on human development. The prior question of what causes democratic stock lies beyond the scope of this inquiry.

The democracy stock variable is robust in each and every specification (twenty in total), at the 0.01 level of significance or better in all cases but one, where it attained the 0.10 level ($p=0.059$). Beyond the specification tests presented in Tables 2, 3 and 4, we also ran an additional battery of tests (not reported) to assess the robustness of these findings. First, we used a dependent variable that measures countries' deviations from the global infant mortality mean in a particular year (King & Zeng 2001). This is a de-trending exercise, intended to focus attention on "best" and "worst" performers relative to the global trend. Employing this dependent variable (obtained from Gary King) in the models of this paper, we obtain similar results.

Second, we included a control for a country's real rate of per capita economic growth at time *t* (as opposed to *t-1*, the lag used with other independent and control variables). This should control for the state of the economy in a given year, and, to the extent that economic patterns within a given country reflect broader tendencies, broader global trends that we might not otherwise capture. The inclusion of the growth control had no effect on the substantive findings reported here.

Third, we employed an interaction term between democratic stock and GDP per capita, to further test for the possibility that democratic history matters differently for countries at varying levels of development. (Plausibly, stock matters more for poorer countries than for richer ones.) Results for the interaction term are not significant, and its inclusion does not affect the performance of the stock variable.

Fourth, as noted previously, there is a variety of ways in which one might calculate, and test, the concept of democratic stock. The one percent depreciation rate employed in this study is intended to capture the possibility that democratic stock does not accumulate linearly over time; it is not intended, however, to serve as a definitive measurement of that concept. Regrettably, because of the highly trended nature of democracy (countries that are highly democratic today are likely to have maintained this pattern in previous years) it is not possible to employ a distributed lag model; the multiple lags would be so highly correlated as to introduce prohibitive problems of collinearity. However, we did experiment with alternative measurements of democratic stock, including both a quadratic and square root measure of this key variable. In both cases, results are substantively the same as those presented here. We also tested a five percent depreciation measure of democratic stock, with the same substantive results. We postpone further exploration of the stock concept for future work.

Finally, it is important to note that the relationship between democratic stock and infant mortality observed here is not merely statistically significant; it is also substantively important. Because the dependent variable is logged, any independent variable's coefficient measures the effect of a one-unit change in that variable as a percentage change in the dependent variable (Wooldridge 2002). Thus, for illustrative purposes only, using the coefficient for democratic stock of -0.0013 from Table 2, models 6 and 7, we find that a one standard deviation increase in a country's democratic stock lowers its infant mortality rate by approximately thirty-two percent. By way of comparison, the similar figure for GDP per capita is roughly thirty-six percent, while that for urbanization is about fourteen percent. Thus, democratic history appears to exert a causal impact on this measure of human development on par with the most widely accepted previously studied causal factors.

DISCUSSION

Contrary to much recent work, this paper argues that there is no strong relationship between a country's current regime type and its subsequent human development, as measured by infant mortality rates. In this respect, we agree with recent critiques of the received view (Gauri & Khaleghian 2002; McGuire 2004; Ross 2006; Shandra et al. 2004). However, we argue that a robust causal relationship does appear if democracy is considered as a long-run, diachronic phenomenon. Regression tests show that a stock measure of democracy is associated with improved human development. Because of the manner of construction of the variable, it may also be less vulnerable

to some of the potential identification problems that affect regression tests using a "level" measure of democracy.

This is a new angle on an old question. Indeed, we find only one published paper that purports to test the relationship between democratic stock and infant mortality.[19] That study, by Michael Ross (2006), concludes that there is no demonstrable causal relationship between the two factors, a finding directly contradictory to our own. We suspect that these divergent findings are accounted for by one very consequential choice in research design. Ross measures level democracy with a continuous measure (the same Polity2 that is employed in the present study). However, in measuring democratic stock he dichotomizes the concept so that all country-years must be coded as either democratic or autocratic. As noted in our previous discussion, this approach to measurement presumes that there is no difference in causal impact between hard autocracies (e.g., North Korea) and soft authoritarian states (e.g., Mexico under the Institutional Revolutionary Party) or between semi-democratic polities (e.g., Malaysia since 1969) and strongly democratic polities (e.g., Mauritius). All cases are lumped into one of two piles. While this sort of dichotomous coding may be theoretically defensible for certain purposes, it is inconsistent with theoretical arguments about democracy and outcomes like infant mortality. Here, the presumed intermediary factors (in Ross's account and our own) are matters-of-degree, rather than either/or. Dichotomous coding also introduces potentially large coding errors, for misclassifications have extreme effects where there are only two possible coding categories. But the more important point is that a more differentiated measure of regime type is necessary in order to test more precisely a causal argument about human development outcomes in what is, after all, a relatively small-sample analysis.

Let us now return to the theoretical underpinnings of the argument. Looking back at the proposed causal pathways—competition-induced accountability, a vigorous civil society, a culture of equality, and institutionalization—one can appreciate that such factors are unlikely to take effect immediately. Indeed, it may be unreasonable to expect any relationship between regime type and distal policy outcomes such as infant mortality to materialize in the space of a year, or even in five- or ten-year periods, as stipulated by extant studies. It is a country's regime history, more than its present status, that determines whether, and to what extent, that society is likely to achieve significant improvements in the lives and livelihoods of its citizens. This, we argue, is likely because

[19] Ross (2006) draws the idea from an earlier unpublished paper of ours (Gerring, Thacker & Alfaro 2005), a precursor to the present paper.

long-term democracies benefit from more political competition leading to greater accountability, stronger civil societies pushing for and promoting human development, the development of norms that support greater demands for equality, and higher levels of institutionalization, relative to authoritarian regimes or new democracies.

If the logic of our argument is correct it may also apply to other measures of development, a matter that we are currently exploring in a companion study (Gerring & Thacker [in process]). Arguably, most, and perhaps all political-institutional variables are time-dependent, which is to say that their effects today are a product, in part, of their histories. These histories may be quite long, and quite consequential. Figuring out the ways in which present outcomes depend upon past choices is a critical task for every empirical analysis, not simply a matter to be reserved for case-study or historical-institutionalist researchers, though we surely have much to learn from work in these historically-oriented genres (Collier & Collier 1991; Mahoney & Rueschemeyer 2003; Pierson 2004).

The practical implications of this argument introduce grounds for both optimism and caution with respect to the ability of developing countries to improve their levels of human development. Realistically, countries should not expect large immediate dividends in human development to result from democratic transitions. On the other hand, given sufficient time, democracy should begin to yield important, tangible benefits to the underprivileged in society. In a world characterized by chronically short time horizons, the substantial political challenge is to allow democratic institutions the time necessary to realize these strong but distal benefits.

Table 1:
Human Development Indicators Compared

DESCRIPTIVE STATISTICS	Obs	Mean	Std Dev	Min	Max
Infant mortality rate (0-1)	7424	66.6	52.5	2.4	293.0
Child mortality rate (0-5)	6781	104.0	89.1	3.0	517.0
Life expectancy	7319	61.0	11.9	31.2	81.1
Illiteracy	4320	31.0	25.7	.2	94.3
Income poverty (% below nat'l poverty line)	229	34.4	16.6	4.6	73.3
GDP per capita	6400	5248.5	8219.7	.0	56382.0

CORRELATION MATRIX	1.	2.	3.	4.	5.
1. Infant mortality rate (0-1)	1.00				
2. Child mortality rate (0-5)	.991	1.00			
3. Life expectancy	-.946	-.947	1.00		
4. Illiteracy	.827	.832	-.804	1.00	
5. Income poverty (% below nat'l poverty line)	.656	.655	-.641	.296	1.00
6. GDP per capita	-.542	-.506	.581	-.382	-.542

FACTOR ANALYSIS	Loadings:			Unique
	1	2	3	-ness
Infant mortality rate (0-1)	.9906	.0085	.0643	.0143
Child mortality rate (0-5)	.9925	.0703	.0246	.0095
Life expectancy	-.9481	.0837	.0514	.0915
Illiteracy	.8529	.1693	-.0442	.2420
GDP per capita	-.5205	.2751	.0033	.6534

Note: Income poverty is excluded from the factor analysis due to its smaller number of observations. All data are drawn from World Bank (2003). See text for notes on the handling of missing data.

Table 2: **Fixed-Effect Specification Tests**

	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**
Temporal correction	lag(1)	lag(1)	lag(1)	lag(1)	lag(1)	lag(1)	lag(1)	lag(1)	lag(1)	LSDV	LSDV	lag(1)	lag(1)
Democracy level	**-0.0004**		**-0.0026****		**-0.0001**		**0.0002**	**-0.0012**		**-0.0006*****			
	(0.001)		(0.001)		(0.001)		(0.001)	(0.001)		(0.0002)			
Democracy stock		**-0.0013*****		**-0.0012*****		**-0.0013*****	**-0.0013*****		**-0.0013*****		**-0.0001***		
		(0.0001)		(0.0001)		(0.0001)	(0.0001)		(0.0001)		(0.00004)		
Democracy level (t-10)												-0.0085***	
												(0.001)	
Democracy stock (t-10)													-0.0010***
													(0.0001)
GDPpc (ln)			-0.3322***	-0.2591***	-0.2988***	-0.2373***	-0.2370***	-0.2800***	-0.2210***	-0.0118***	-0.0089**	-0.3202***	-0.2539***
			(0.020)	(0.020)	(0.022)	(0.021)	(0.021)	(0.022)	(0.021)	(0.004)	(0.004)	(0.023)	(0.023)
Urbanization			-0.0031**	-0.0075***	-0.0019	-0.0057***	-0.0057***	-0.0019	-0.0058***	-0.0006***	-0.0010***	-0.0020	-0.0061***
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0002)	(0.0003)	(0.001)	(0.001)
Female illiteracy (logit)					-0.0058	-0.0025	-0.0024	0.0322***	0.0249***	-0.0051***	-0.0047***	-0.000006	0.0047
					(0.006)	(0.005)	(0.005)	(0.007)	(0.007)	(0.001)	(0.001)	(0.005)	(0.005)
Instability					0.0014*	0.0008	0.0008	0.0012	0.0006	0.000002	-0.00002	0.0009	0.0011
					(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0001)	(0.0001)	(0.001)	(0.001)
Lagged dep var										0.9771***	0.9731***		
										(0.006)	(0.007)		
Trend	-0.0304***	-0.0310***	-0.0251***	-0.0238***	-0.0294***	-0.0268***	-0.0268***					-0.0293***	-0.0281***
	(0.0005)	(0.0004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)					(0.001)	(0.001)
Year dummies								YES	YES				
Constant	4.4689***	4.4310***	6.9632***	6.5808***	6.7488***	6.4162***	6.4150***	6.6954***	6.3436***	0.1658***	0.1764***	6.9087***	6.5917***
	(0.012)	(0.011)	(0.152)	(0.151)	(0.173)	(0.166)	(0.167)	(0.175)	(0.169)	(0.046)	(0.052)	(0.182)	(0.178)
Observations	*6569*	*6562*	*5547*	*5545*	*4495*	*4492*	*4492*	*4495*	*4492*	*4492*	*4489*	*4296*	*4291*
Countries	*192*	*192*	*178*	*178*	*158*	*158*	*158*	*158*	*158*	*158*	*158*	*157*	*157*
Sample Period	*1960-00*	*1960-00*	*1960-00*	*1960-00*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*
R-square (within)	*0.707*	*0.737*	*0.782*	*0.805*	*0.813*	*0.834*	*0.834*	*0.820*	*0.840*	*0.980*	*0.980*	*0.827*	*0.838*
Prob>F	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*

Dependent variable: IMR (ln) Unit of analysis: country-year. All independent variables are lagged one time-period. Newey-West standard errors in parentheses.
Country fixed effects are included in each model. Models 5-7 are regarded as the benchmark model.

Table 3: **Further Specification Tests**

	1	2	3	4	5	6	7	8	9	10	11
Temporal correction	lag(1)	lag(1)	lag(1)	lag(1)	lag(1)	lag(1)	lag(1)	lag(1)	lag(1)	A-Bond	A-Bond
Data interval	Annual	Annual	Annual	3-years	3-years	Annual	Annual	Annual	Annual	Annual	Annual
Democracy level	**-0.0021***		**-0.0004**	**-0.0005**		**-0.0095*****		**-0.0098*****		**0.0006**	
	(0.001)		(0.001)	(0.002)		(0.001)		(0.001)		(0.0005)	
Democracy stock		**-0.0010*****	**-0.0010*****		**-0.0012*****		**-0.0006*****		**-0.0006*****		**-0.0004*****
		(0.0001)	(0.0001)		(0.0002)		(0.00004)		(0.00004)		(0.00005)
GDPpc (ln)	-0.1721***	-0.1372***	-0.1376***	-0.3072***	-0.2474***	-0.3823***	-0.3496***	-0.3611***	-0.3357***	-0.0552***	-0.0451***
	(0.020)	(0.021)	(0.021)	(0.036)	(0.035)	(0.012)	(0.012)	(0.013)	(0.013)	(0.009)	(0.009)
Urbanization	-0.0013	-0.0035***	-0.0035***	-0.0018	-0.0055**	-0.0043***	-0.0043***	-0.0044***	-0.0043***	0.0009*	-0.0005
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female illiteracy (logit)	0.0127**	0.0118**	0.0115**	-0.0152	-0.0094	0.0486***	0.0392***	0.0826***	0.0660***	-0.0010	-0.0025
	(0.005)	(0.005)	(0.005)	(0.010)	(0.010)	(0.006)	(0.007)	(0.008)	(0.008)	(0.002)	(0.002)
Instability	0.0022**	0.0017**	0.0017**	0.0031*	0.0018	0.0087***	0.0077***	0.0087***	0.0078***	0.0003	0.0003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0003)	(0.0003)
Population (ln, 1960)						-0.0082	-0.0009	-0.0091*	-0.0021		
						(0.005)	(0.005)	(0.005)	(0.005)		
Ethnic fractionalization						0.2771***	0.2800***	0.2712***	0.2757***		
						(0.034)	(0.033)	(0.034)	(0.033)		
Latitude (ln)						0.0336***	0.0382***	0.0344***	0.0379***		
						(0.012)	(0.011)	(0.012)	(0.011)		
Muslim						0.0026***	0.0022***	0.0021***	0.0018***		
						(0.0003)	(0.0003)	(0.0003)	(0.0003)		
Africa						0.2306***	0.2866***	0.2044***	0.2653***		
						(0.033)	(0.032)	(0.032)	(0.032)		
Asia						-0.1667***	-0.1609***	-0.1829***	-0.1737***		
						(0.032)	(0.033)	(0.032)	(0.032)		
Latin America						0.3516***	0.2890***	0.3416***	0.2850***		
						(0.027)	(0.026)	(0.026)	(0.025)		
Socialism						-0.1869***	-0.3632***	-0.1412***	-0.3146***		
						(0.038)	(0.043)	(0.038)	(0.044)		
Lagged dep var										0.8644***	0.8186***
										(0.009)	(0.010)
Trend	-0.0278***	-0.0278***	-0.0278***	-0.0285***	-0.0259***	-0.0239***	-0.0255***				
	(0.001)	(0.001)	(0.0009)	(0.001)	(0.001)	(0.001)	(0.001)				
Multiple imputation	YES	YES	YES								
Year dummies								YES	YES		
Country fixed effects	YES	YES	YES	YES	YES					YES	YES
Constant	5.7715***	5.5923***	5.5939***	6.7762***	6.4515***	7.3107***	7.0463***	7.3441***	7.0117***	-0.0043***	-0.0048***
	(0.159)	(0.169)	(0.169)	(0.282)	(0.274)	(0.107)	(0.104)	(0.112)	(0.108)	(0.0004)	(0.0004)
Observations	*6233*	*6233*	*6233*	*1640*	*1639*	*4371*	*4368*	*4371*	*4368*	*4328*	*4325*
Countries	*199*	*199*	*199*	*158*	*158*	*149*	*149*	*149*	*149*	*157*	*157*
Sample Period	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*
R-square (within)	*0.732*	*0.749*	*0.749*	*0.813*	*0.833*	*0.882*	*0.891*	*0.886*	*0.893*		
Prob>F	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*		
Sargan test (prob)										*0.000*	*0.000*

Dependent variable: IMR (ln) All independent variables are lagged one time-period. Newey-West standard errors in parentheses and AR1 correction for serial autocorrelation, except in the final two models, where the analysis is Arellano-Bond.

Table 4: **Sample Restrictions**

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Excluding	Interpolated data		Most autocratic		Most democratic		Asia		Latin America		Africa		Middle East		OECD	
Democracy	**-0.0015**		**-0.0005**		**-0.0007**		**-0.0028****		**-0.0028***		**-0.0010**		**-0.0032****		**0.0040*****	
level	(0.001)		(0.001)		(0.001)		(0.001)		(0.002)		(0.002)		(0.001)		(0.001)	
Democracy		**-0.0013*****		**-0.0012*****		**-0.0011*****		**-0.0013*****		**-0.0013*****		**-0.0005*****		**-0.0016*****		**-0.0008*****
Stock		(0.0001)		(0.0001)		(0.0001)		(0.0001)		(0.0001)		(0.0001)		(0.0001)		(0.0001)
GDPpc (ln)	-0.3343***	-0.2686***	-0.3225***	-0.3393***	-0.2674***	-0.2705***	-0.3335***	-0.2303***	-0.3552***	-0.2711***	-0.2105***	-0.2011***	-0.3826***	-0.2994***	-0.2098***	-0.1989***
	(0.027)	(0.025)	(0.027)	(0.030)	(0.028)	(0.028)	(0.027)	(0.025)	(0.031)	(0.030)	(0.032)	(0.031)	(0.028)	(0.027)	(0.026)	(0.026)
Urbanization	-0.0011	-0.0053***	0.0024	-0.0031*	-0.0015	-0.0063***	0.0009	-0.0038**	-0.0006	-0.0059***	-0.0082***	-0.0095***	0.0024	-0.0030*	-0.0004	-0.0016
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Female	-0.0098	-0.0094	-0.0124	-0.0054	-0.0095	-0.0115*	-0.0405***	-0.0307***	-0.0193*	-0.0165	0.0285***	0.0253***	-0.0286***	-0.0327***	-0.0104	-0.0093
Illiteracy	(0.008)	(0.007)	(0.009)	(0.008)	(0.007)	(0.007)	(0.009)	(0.009)	(0.011)	(0.011)	(0.007)	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)
Instability	0.0017*	0.0009	0.0022**	0.0006	-0.0006	0.0015	0.0021**	0.0014	0.0026**	0.0013	0.0013	0.0011	0.0019*	0.0007	-0.0002	-0.0003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Trend	-0.0317***	-0.0283***	-0.0335***	-0.0281***	-0.0278***	-0.0271***	-0.0331***	-0.0296***	-0.0311***	-0.0275***	-0.0350***	-0.0332***	-0.0315***	-0.0270***	-0.0295***	-0.0281***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	6.8436***	6.5566***	6.5524***	7.0506***	6.5248***	6.6090***	6.7179***	6.1910***	6.9191***	6.5549***	6.2356***	6.2398***	6.9744***	6.6223***	6.1426***	6.0239***
	(0.225)	(0.209)	(0.235)	(0.244)	(0.207)	(0.205)	(0.239)	(0.214)	(0.258)	(0.237)	(0.274)	(0.263)	(0.246)	(0.222)	(0.190)	(0.190)
Observations	*2603*	*2602*	*2426*	*2400*	*1715*	*1832*	*2290*	*2289*	*2247*	*2246*	*2084*	*2084*	*2390*	*2389*	*1607*	*1606*
Countries	*159*	*159*	*153*	*146*	*134*	*141*	*139*	*139*	*138*	*138*	*113*	*113*	*142*	*142*	*128*	*128*
Sample Period	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*	*1960-99*
R-square (within)	*0.850*	*0.866*	*0.856*	*0.873*	*0.771*	*0.801*	*0.855*	*0.871*	*0.849*	*0.866*	*0.901*	*0.903*	*0.859*	*0.881*	*0.753*	*0.761*
Prob>F	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*

Dependent variable: IMR (ln) Unit of analysis: country-year (with fixed effects). All independent variables are lagged one time-period. Newey-West standard errors in parentheses.
AR1 correction for serial autocorrelation. Exclusions from the full sample are explained in the text.

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Appendix: **Descriptive Statistics**

Variable	*Observations*	*Mean*	*Std. Dev.*	*Minimum*	*Maximum*
IMR (interpolated, ln)	6569	3.785	0.992	0.88	5.66
IMR (ln)	3688	3.482	1.049	0.88	5.63
Democracy level	6569	-0.283	7.536	-10	11
Democracy stock	6561	-39.603	253.635	-534.7	637.6
GDPpc (ln)	5588	7.428	1.544	3.898	10.937
Urbanization	6524	46.341	24.123	2.23	100
Female illiteracy (logit)	5467	-1.501	2.158	-6.213	4.385
Instability	5703	-0.073	4.308	-2.214	68.495
Population (ln, 1960)	6462	8.141	1.858	2.708	13.411
Ethnic fractionalization	6308	0.449	0.262	0.000	0.930
Latitude (ln)	6436	-1.589	0.966	-4.500	-0.341
Muslim (percent)	6522	23.739	35.894	0.00	99.90
Africa (dummy)	6523	0.265	0.441	0	1
Asia (dummy)	6523	0.128	0.334	0	1
Latin America (dummy)	6482	0.120	0.325	0	1
Socialism (dummy)	6522	0.189	0.392	0	1