

DEMOCRACY AND ECONOMIC GROWTH

A Historical Perspective

By JOHN GERRING, PHILIP BOND, WILLIAM T. BARNDT,
and CAROLA MORENO*

DOES regime type affect economic performance? The predominant view is that democracy has either a negative effect on GDP growth or no overall effect. Countries with authoritarian political systems are thus predicted to grow as rapidly as democracies, perhaps even faster. To be sure, democracy may have some positive *indirect* effects—for example, greater stability or more extensive property rights. The econometric evidence suggests, however, that these positives are balanced by negatives such that the *net* effect of democracy on growth performance cross-nationally over the last five decades is negative or null.¹ For the most part, case study approaches to this question confirm the results of

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¹See, e.g., Robert J. Barro, “Democracy and Growth,” *Journal of Economic Growth* 1 (March 1996); Yi Feng, “Democracy, Political Stability and Economic Growth,” *British Journal of Political Science* 27 (July 1997); idem, *Democracy, Governance, and Economic Performance: Theory and Evidence* (Cambridge: MIT Press, 2003); Jonathan Krieckhaus, “The Regime Debate Revisited: A Sensitivity Analysis of Democracy’s Economic Effect,” *British Journal of Political Science* (forthcoming); Charles K. Kurzman, Regina W. Werum, and Ross E. Burkhart, “Democracy’s Effect on Economic Growth: A Pooled Time-Series Analysis, 1951–1980,” *Studies in Comparative International Development* 37 (January 2002); Adam Przeworski and Fernando Limongi, “Political Regimes and Economic Growth,” *Journal of Economic Perspectives* 7 (Summer 1993); idem, “Modernization: Theories and Facts,” *World Politics* 49 (January 1997); Adam Przeworski, Michael Alvarez, Jose Antonio Cheibub, and Fernando Limongi, *Democracy and Development: Political Institutions and Material Well-Being in the World, 1950–1990* (Cambridge: Cambridge University Press, 2002). Elias Papaioannou and Gregorios Siourounis and Dani Rodrik and Romain Wacziarg find a relationship between episodes of democratization (if sustained) and growth rates in subsequent years, though they make no claims about the effect of democracy per se on growth performance; see Papaioannou and Siourounis, “Democratization and Growth,” Working Paper (London Business School, October 2004); and Rodrik and Wacziarg, “Do Democratic Transitions Produce Bad Economic Outcomes?” (Manuscript, December 2004). Barro finds that “growth is increasing in democracy at low levels of democracy, but the relation turns negative once a moderate amount of political freedom is attained”; Barro, *Determinants of Economic Growth* (Cambridge: MIT Press, 1997). A few recent studies find a positive overall relationship between democracy and growth, but this is not the standard finding. These recent studies include Surjit S.

cross-national growth empirics.² Thus, although most of the rich countries in the world are democratic, the direction of causality is unclear. One must keep in mind that many rich countries have become rich under authoritarian auspices. If this conventional conclusion is correct, one might be justified in concluding that democracy is a luxury to be enjoyed only by countries rich enough to afford it. (Indeed, this is a common argument among authoritarian leaders in the developing world.)³

In this article, we show that the skeptical view of democracy and development rests on a questionable assumption in which democracy is treated as a more or less immediate cause. This year's level of democracy is thought to influence growth performance in the following period (usually a decade or two). We argue that this is an unrealistic scenario. If democracy matters for growth today, it is reasonable to assume that this effect stems from a country's regime history as well as its current status. The distant past may have contemporary effects. Democracy is thus best considered as a *stock*, rather than *level*, variable.

We begin by constructing a *prima facie* case for a historical understanding of democracy and its relationship to economic growth. Second, we discuss the definition and measurement of the key concept, democracy. Third, we discuss the method that will be employed to test our hypothesis. Fourth, we discuss the results of these regression tests. Fifth, we discuss the "democratic growth effect"—its magnitude, its policy significance, and directions for future research. We conclude with a discussion of the possible merits of a historical approach to the quantitative analysis of political-institutional variables.

WHY HISTORY MIGHT MATTER

Work on democracy and growth has usually focused on the possible causal effect of a country's contemporary level of democracy on its subsequent growth performance. The subsequent period at issue might be a single year or some multiyear period (for example, a decade); in any case, scholars have conceptualized the problem as the effect of regime

Bhalla, "Freedom and Economic Growth: A Virtuous Cycle?" in Axel Hadenius, ed., *Democracy's Victory and Crisis* (Cambridge: Cambridge University Press, 1997); and David A. Leblang, "Political Democracy and Economic Growth: Pooled Cross-Sectional and Time-Series Evidence," *British Journal of Political Science* 27 (July 1997).

² See, e.g., Sylvia Chan, *Liberalism, Democracy and Development* (Cambridge: Cambridge University Press, 2002); Meredith Woo-Cumings, ed., *The Developmental State* (Ithaca, N.Y.: Cornell University Press, 1999).

³ See Lee Kuan Yew, quoted in *The Economist*, August 27, 1994, 15.

type on growth rates at time T plus some specified period. These studies look forward in time, but not backward.⁴

We propose to consider regimes as historically informed phenomena rather than as contemporary variables. This means looking both backward and forward in time (via lagged predictors). In particular, it means measuring a country's accumulated *stock* of democracy rather than its *level* of democracy at a particular moment in time. The core insight is that institutional effects unfold over time, sometimes a great deal of time, and that these temporal effects are cumulative.

Nowhere does this seem more likely than when one is considering the causal effects of regime type. Regimes do not begin again *de novo* each fiscal year. Where one is today depends critically upon where one has been. Democracy and authoritarianism are commonly thought to construct deep legacies, extending back several decades, perhaps even centuries.⁵ It is the accumulated effect of these historical legacies, in addition to contemporary regime status, that ought to be of central concern if we wish to understand the causal effect of a regime type on a variety of current outcomes—social, cultural, political, or economic.

Our interest here of course is in one specific outcome—economic performance, as measured by GDP per capita growth. Thus, we are impelled to think carefully about the causal mechanisms that might link a country's regime history with its current growth performance. Our explanation rests on the key concept of *capital*.

Capital, in common usage, implies a fungible resource that may accumulate over time (creating a "stock") and promising increased returns in the future. An investment today should bring a higher yield at some later date. If a democratic regime endures, we argue that it is likely to foster four types of capital: *physical*, *human*, *social*, and *political*. Extant studies indicate that all four types of capital have positive impacts on growth performance. Thus, we anticipate that the longer a country remains democratic, the greater will be its physical, human, social, and political capital—and the better its growth performance. A schematic diagram of these multiple causal pathways is reproduced in Figure 1.

⁴Even studies by economic historians that look at the relationship between democracy and growth in early periods of development tend to conceptualize this relationship as contemporaneous (simultaneous). See, e.g., Peter H. Lindert, "Voice and Growth: Was Churchill Right?" *Journal of Economic History* 63 (June 2003).

⁵Ruth Berins Collier and David Collier, *Shaping the Political Arena: Critical Junctures, the Labor Movement, and Regime Dynamics in Latin America* (Princeton: Princeton University Press, 1991); Katherine Hite and Paola Cesarini, eds., *Authoritarian Legacies and Democracy in Latin America and Southern Europe* (Notre Dame, Ind.: University of Notre Dame Press, 2004); James Mahoney, *The Legacies of Liberalism: Path Dependence and Political Regimes in Central America* (Baltimore: Johns Hopkins University Press, 2002).

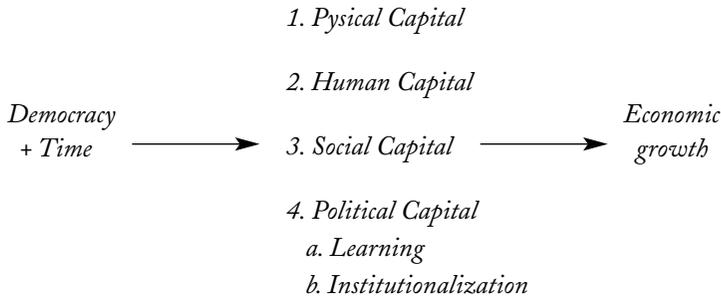


FIGURE 1

FROM DEMOCRACY TO ECONOMIC GROWTH, A STYLIZED CAUSAL DIAGRAM^a^aMultiple feedback loops have not been included in this stylized diagram.

Since political capital is the newest concept in this panoply of capitalisms, we focus primarily on this causal pathway.

Needless to say, the causal factors at work in the democracy/growth relationship are much more complex than can be illustrated in a simple diagram; they include, for example, multiple feedback loops, which we have omitted. A degree of ambiguity in the argument is unavoidable, given that the key concepts are abstract, virtually impossible to measure, and tend to overlap. We are acutely aware that an arbitrary choice of terms among near synonyms imposes some confusion on a subject. Nonetheless, there is no escaping the need to impose theoretical order, however provisional, on the causal argument. Our preference, it should be noted, is for a fairly elaborate theoretical argument, as opposed to a parsimonious (but necessarily partial) explanation.

PHYSICAL, HUMAN, AND SOCIAL CAPITAL

The extent to which physical capital is equally distributed throughout a society is now commonly regarded as an important component of long-run economic growth.⁶ Insofar as democracy is successful in redistributing wealth—through progressive taxation, social policies, land

⁶Timothy Besley and Robin Burgess, "Land Reform, Poverty Reduction and Growth: Evidence from India," *Quarterly Journal of Economics* 115 (May 2000); Kenneth L. Sokoloff and Stanley L. Engerman, "Institutions, Factor Endowments, and Paths of Development in the New World," *Journal of Economic Perspectives* 14 (Summer 2000). But see Kristin Forbes, "A Reassessment of the Relationship between Inequality and Growth," *American Economic Review* 90 (September 2000).

redistribution, or simply opening up markets and institutions in civil society to previously excluded groups⁷—it seems reasonable to expect that the longer this regime type is in existence, the greater will be its aggregate effect on the achievement of social equality and hence on growth. Indeed, there is some evidence for a “political Kuznets curve,” in which the immediate effect of democracy is to exacerbate inequality while the long-run effect is to diminish it.⁸ Thus, although the short-term effects of democracy are equivocal, theory and research suggest that a prolonged experience with democratic rule should have positive repercussions on the distribution of wealth and income in a society.⁹

Consider, as well, the role of human capital, which endogenous (“new growth”) theory identifies as a strong influence on growth performance.¹⁰ It seems plausible to regard democracy as an important institutional factor in the development of human capital, as measured by declining fertility rates and improvements in education, public health, and life expectancy. Political elites in a democracy have electoral incentives to avoid famine and improve the quality of life for the least advantaged, incentives that are not present or are present to a much smaller degree in authoritarian systems. Many studies have shown that democratic rule translates into improvements in a society’s human capital resources.¹¹ Because of the (literally) long-lived nature of human capital, the longer a democracy is in place, the more pronounced we can expect its impact on the level of human capital to be.¹²

Finally, many commentators have argued that social capital also contributes to a society’s economic productivity. For example, in a recent

⁷ Gerhard Lenski, *Power and Privilege: A Theory of Social Stratification* (Chapel Hill: University of North Carolina Press, 1966); Seymour Martin Lipset, “Some Social Requisites of Democracy: Economic Development and Political Legitimacy,” *American Political Science Review* 53 (March 1959); Allan H. Meltzer and Scott F. Richard, “A Rational Theory of the Size of Government,” *Journal of Political Economy* 89 (1981); Edward N. Muller, “Democracy, Economic Development, and Income Inequality,” *American Sociological Review* 53 (February 1988).

⁸ Alberto Chong, “Inequality, Democracy, and Persistence: Is There a Political Kuznets Curve?” *Economics and Politics* 16 (July 2004).

⁹ Mark Gradstein and Branko Milanovic, “Does Liberte=egalite? A Survey of the Empirical Links between Democracy and Inequality with Some Evidence on the Transition Economies,” Working Paper Series no. 261 (CESifo, 2000); Jose Tavares and Romain Wacziarg, “How Democracy Affects Growth,” *European Economic Review* 45 (August 2001).

¹⁰ Barro (fn. 1, 1997); Xavier X. Sala-i-Martin, “Fifteen Years of New Growth Economics: What Have We Learnt?” (Manuscript, Department of Economics, Columbia University, 2002).

¹¹ Matthew A. Baum and David A. Lake, “The Political Economy of Growth: Democracy and Human Capital,” *American Journal of Political Science* 47 (April 2003); Jean Dreze and Amartya Sen, *Hunger and Public Action* (Oxford: Clarendon Press, 1989); Feng (fn. 1, 2003).

¹² John Gerring, Strom Thacker, and Rodrigo Alfaro, “Democracy and Human Development” (Paper presented at the annual meeting of the American Political Science Association, Washington, D.C., September 2005).

careful study, Guiso, Sapienza, and Zingales present evidence that social capital enhances financial development (which is, in turn, generally regarded as growth enhancing).¹³ If democracy contributes to the development of social capital, it stands to reason that long-term democracy would have strong effects on the development of social capital.¹⁴ Again, these observations suggest that democracy's effect on growth may become more pronounced over time, as a country's stock of social capital matures.

POLITICAL CAPITAL

In addition to physical, human, and social capital, we argue that established democracies create a species of capital that is explicitly *political*. Just as deferred consumption generates physical capital, which in turn contributes toward output, so should a country's political experience today affect tomorrow's political capital and, in turn, its economic output.

Before introducing a new capital-ism to the lexicon, it is important to acknowledge the intrinsic ambiguity of the core concept. "Capital," by virtue of its theoretical abstraction, poses severe problems of conceptualization and measurement. It is, after all, a *capability* (a potentiality). As such, it cannot be directly measured. Even so, the ambiguities of capital-based theories may be redeemed by the theoretical leverage that they offer. In particular, they allow us to think about the role of political institutions over time. This, we believe, is a significant theoretical advantage and is not well captured by other terms (for example, "governance," "good institutions," "property rights").

Politics, too, may be capital-ized. Recall that physical capital is usually measured in monetary terms (for example, accumulated investments), human capital by the spread of education and public health, and social capital by patterns of social interaction. We propose that political capital may be operationalized by various measures of the relative health of a polity—for example, bureaucratic capacity, low levels of corruption, political consensus, stability, legitimacy, trust, the wisdom and farsightedness of political leaders, and so forth. These resources, like

¹³Luigi Guiso, Paola Sapienza, and Luigi Zingales, "The Role of Social Capital in Financial Development," *American Economic Review* 94, no. 3 (2004). See also Stephen Knack and Philip Keefer, "Does Social Capital Have an Economic Payoff? A Cross-Country Investigation," *Quarterly Journal of Economics* 112 (1997); Michael Woolcock, "Social Capital and Economic Development: Toward a Theoretical Synthesis and Policy Framework," *Theory and Society* 27 (April 1998).

¹⁴Robert D. Putnam, ed., *Democracies in Flux* (Oxford: Oxford University Press, 2002); Mark E. Warren, *Democracy and Trust* (Cambridge: Cambridge University Press, 1999).

other sorts of capital, accumulate through time and may be drawn upon for a range of purposes.

In some respects, our use of the term political capital is an extension of colloquial usage, where it is usually understood as a resource attached to a particular figure—someone who gains or loses political capital. The parallel is not exact. An individual's political capital may be nothing more than a quid pro quo, a favor done for a friend that is banked for some period of time. However, an individual's political capital may also refer to something more complex: a pattern of behavior that establishes a reputation for fidelity and competence that translates, in turn, into a relationship of generalized trust. It is at this point that a representative becomes a trustee rather than simply a delegate and the transactional costs usual to politics are lowered (since explicit quid pro quo agreements between actors are no longer required). Plausibly, the reputational quality of political capital operates for institutions as well as for persons.

How, then, might democratic political arrangements foster political capital in a country and, ultimately, enhance economic growth? Let us begin with a brief listing of political outputs that are assumed to have strong and direct impacts on economic performance. These include market-augmenting economic policies,¹⁵ political stability (understood as a reduction of uncertainty),¹⁶ rule of law,¹⁷ and efficient public bureaucracies.¹⁸ We shall assume that these political factors contribute to economic growth over the long term.

¹⁵ For purposes of discussion, we shall assume that large budget deficits, high inflation, high tariff barriers, and heavy regulatory burdens usually have negative consequences over the long term. This does not mean that the state should withdraw from the marketplace; it means, rather, that economic policies should be "market augmenting." Omar Azfar and Charles Cadwell, eds., *Market-Augmenting Government: The Institutional Foundations for Prosperity* (Ann Arbor: University of Michigan Press, 2002). The security of property rights depends upon the intelligent engagement of the state. Douglass C. North, *Institutions, Institutional Change and Economic Performance* (Cambridge: Cambridge University Press, 1990); idem, "The Historical Evolution of Politics," Working Paper (Department of Economics, Washington University, St. Louis, Mo., 1994).

¹⁶ Ahmed Mushfiq Mobarak, "Democracy, Volatility and Development," *Review of Economics and Statistics* 87 (May 2005).

¹⁷ Adam Przeworski and Jose Maria Maravall, eds., *Democracy and the Rule of Law* (Cambridge: Cambridge University Press, 2003); Roberto Rigobon and Dani Rodrik, "Rule of Law, Democracy, Openness, and Income: Estimating the Interrelationships" NBER Working Paper 10750 (September 2004).

¹⁸ Alberto Ades and Rafael Di Tella, "The New Economics of Corruption: A Survey and Some New Results," in Paul Heywood, ed., *Political Corruption* (Oxford: Basil Blackwell, 1997); Philip Keefer and Stephen Knack, "Why Don't Poor Countries Catch Up? A Cross-National Test of an Institutional Explanation," *Economic Inquiry* 35 (1997); Paolo Mauro, "Corruption and Growth," *Quarterly Journal of Economics* 110 (August 1995).

How, then, might political capital be generated within a democratic setting? Why, in particular, might we expect better results from a long-term democracy than from an authoritarian regime or a country that has recently become democratic? All organizations, and certainly all polities, are subject to the “liability of newness.”¹⁹ However, there are reasons to believe that these liabilities are greater in the case of democracies than in the case of autocracies. Experience matters more in a democratic setting. In explaining the political importance of time, we focus on two intertwined processes—*learning* and *institutionalization*. The first concerns the behavior of individual actors (citizens and elites); the second concerns the behavior of political institutions (with secondary effects on the perceptions and actions of individuals).

LEARNING

Learning refers here to cognitive developments that occur through encounters with some external reality that provide periodic “corrections” and have important consequences for the actors. In this process, interests and values are understood to hold constant, though policy preferences may of course change. We expect that learning plays a particularly important role in economic policy, a policy arena that is complex and often unintuitive.

Consider that policy-making in authoritarian regimes is generally monopolized by a small number of elite actors and has few mechanisms of accountability. There may be a very small “selectorate” or there may be none at all; in any case, power is likely to be concentrated in a single leader and his or her coterie. Naturally, the longer these elites (and their progeny) are in power, the greater their opportunities for gaining experience in the diverse tasks of governance. However, since the political environment is highly constrained, the only potential learners are the small set of elite actors who run the government.

In democratic regimes, by contrast, the policy-making process generally involves many more players. It is a group process, rather than a leader-centered process. This, by itself, may enhance the quality of decision making, as suggested by recent research in social psychology.²⁰ As yet, there have been few attempts to test “the wisdom of crowds” in ex-

¹⁹ Arthur L. Stinchcombe, “Social Structure and Organizations,” in James March, ed., *Handbook of Organizations* (Chicago: Rand McNally, 1965).

²⁰ James Surowiecki, *The Wisdom of Crowds: Why the Many Are Smarter Than the Few and How Collective Wisdom Shapes Business, Economies, Societies, and Nations* (New York: Doubleday, 2004).

licitly political settings.²¹ However, there is good reason to suspect that, consistent with our argument, this sort of learning is more likely to take place in seasoned democracies than in newly democratized polities.

Policy-making in a democracy may be described as a continual back and forth between those occupying formal positions of power—often, a large number of individuals ensconced within relatively autonomous organizations—and groups and individuals within civil society. Each of these players must learn to anticipate the goals, interests, and special sensitivities of the other players in this far-flung policy-making system. Hence, the process of defining “good policy” is likely to take considerable time; “lessons” are learned only after many miscues. Not only must governing politicians learn what constitutes good policy; voters must also learn to recognize good policy. There may even be a third stage, during which politicians learn that voters have learned to distinguish good policies from bad. In Sartori’s words: “Elected officials seeking reelection (in a competitive setting) are conditioned, in their deciding, by the anticipation (expectation) of how electorates will react to what they decide. The rule of anticipated reactions thus provides the linkage between input and output, between the procedure (as stated by Schumpeter) and its consequences.” Sartori refers to this as a “feedback theory of democracy.”²²

The interaction between elites and masses is particularly evident in the arena of economic policy for the simple reason that the fate of the economy is such an important influence in voting decisions. Here, mechanisms of electoral accountability are propitious. The most important lesson to be learned by democratically elected elites is that growth performance matters for their political future. In new democracies, politicians frequently adopt short-term policies intended to pay off political supporters and stimulate the economy during election seasons.²³ However, once elites and voters have experienced a series of electoral and economic cycles, longer time horizons may prevail. Voters who have directly experienced the effects of populist economic policies are likely to be skeptical of claims that soaking the rich, inflating

²¹ Clare Lombardelli, James Proudman, James Talbot, “Committees versus Individuals: An Experimental Analysis of Monetary Policy Decision Making,” *International Journal of Central Banking* (May 2005); Alan S. Blinder and John Morgan, “Are Two Heads Better Than One? Monetary Policy by Committee,” *Journal of Money, Credit and Banking* 37, no. 5 (2005).

²² Giovanni Sartori, *The Theory of Democracy Revisited* (Chatham, U.K.: Chatham House 1987), 152.

²³ Rudiger Dornbusch and Sebastian Edwards, eds., *The Macroeconomics of Populism in Latin America* (Chicago: University of Chicago Press, 1991).

the economy, abrogating debt agreements, or resorting to massive expropriation of property will enhance their livelihoods.²⁴ Indeed, Duch finds that it takes time for voters in a newly democratized country to begin to link their votes to the country's economic performance.²⁵ Economic voting appears only as the electorate develops trust in new institutions and begins to treat elected politicians as guardians of the economy.²⁶ Consequently, leaders in established democracies may be willing to impose sacrifices over the short term to facilitate stronger growth performance over the course of their administration.²⁷

Thus, as democratic experience accumulates, we expect a slow transition away from a populist style of politics and policy-making. As a result, countries with extensive democratic histories are expected to institute better policies than transitional democracies or authoritarian regimes. These policies, in turn, should have strong effects on aggregate growth performance. Of course, the learning process may affect other policies and the general structure of political institutions (for example, the level of corruption in government); our discussion focuses on economic policy because it offers the most plausible case for a learning theory premised on mechanisms of democratic accountability.

INSTITUTIONALIZATION

"The major role of institutions in a society is to reduce uncertainty by establishing a stable . . . structure to human interaction," writes Douglass North.²⁸ In this minimal sense, institutions foster economic growth via their positive impact on certainty. 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 (legitimate).²⁹

²⁴ Karen L. Remmer, "The Political Impact of Economic Crisis in Latin America in the 1980s," *American Political Science Review* 85 (September 1991); Kurt Weyland, *The Politics of Market Reform in Fragile Democracies: Argentina, Brazil, Peru, and Venezuela* (Princeton: Princeton University Press, 2002).

²⁵ Raymond M. Duch, "A Developmental Model of Heterogeneous Economic Voting in New Democracies," *American Political Science Review* 95 (December 2001).

²⁶ Michael Lewis-Beck and Mary Stegmaier, "Economic Determinants of Electoral Outcomes," *Annual Review of Political Science* 3 (June 2000).

²⁷ Susan Carol Stokes, *Markets, Mandates, and Democracy: Neoliberalism by Surprise in Latin America* (Cambridge: Cambridge University Press, 2001); Stokes, ed., *Public Support for Market Reforms in New Democracies* (Cambridge: Cambridge University Press, 2002).

²⁸ North (fn. 15, 1990) 6.

²⁹ See, e.g., Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968); Nelson Polsby, "The Institutionalization of the U.S. House of Representatives," *American Political Science Review* 62 (March 1968). Note that the concept of institutionalization has deep intel-

With political institutionalization, as with learning, we anticipate that there will be gains over time and that these gains will be greater in democratic than in authoritarian contexts.

While it is beyond the scope of this article to examine the comparative institutionalization of democratic and authoritarian regimes over time, we suppose that relatively few authoritarian regimes in the modern era are well institutionalized. Ethiopia, for example, has enjoyed sovereignty for centuries but has yet to develop a well-articulated set of governing institutions: as in most authoritarian states, power remains highly personalized and informal.³⁰ In contrast, virtually all long-standing democracies have 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, while the length of time an authoritarian regime has been in existence may have little or no bearing on its level of institutionalization. Reversals are common, as in the latter days of the Soviet Union or in Iraq under Saddam Hussein.

We suspect that the reasons for this stem directly from their systems of rule. Where power is personalized, as it is in most 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 that is 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 (“regime” is employed 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 is likely to continue.

More importantly, we suspect that the institutionalization of power leads to greater gains in political capital within a democratic setting than in an authoritarian setting. Institutionalization *matters more*. Consider the matter of establishing social order and stability in a polity and

lectual roots and may be traced back to work by Henry Sumner Maine, Ferdinand Tonnies, Max Weber, Emile Durkheim, and Talcott Parsons. See Polsby, 145.

³⁰Harold G. Marcus, *A History of Ethiopia*, 2nd ed. (Berkeley: University of California Press, 2002).

resolving problems of coordination (for example, between different interests, different points of view, and different identities).³¹ 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. 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, 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 societywide 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, we expect them to be more effective in resolving differences and finding optimal solutions than would be fiats imposed from above.

The argument is not self-evident. Indeed, a large literature on democratic overload posits that democracy engenders costly and destabilizing power struggles among subgroups.³² And the literature on democratization is replete with examples of the difficulties encountered by newly democratizing countries—particularly when those countries are poor or ethnically divided or where the question of nationality is open to question.³³ Such countries are burdened with a surfeit of expectations, accumulated over many years. Citizens have been told to expect great achievements from self-government, and they generally expect these goods to materialize in a hurry. It is the fashion of political leaders during the long and dangerous struggle for democracy to overpromise, and transitions offer little preparation for the humdrum nature of everyday politics. Thus, when the transition finally occurs, it

³¹Russell Hardin, *Liberalism, Constitutionalism, and Democracy* (Oxford: Oxford University Press, 1999).

³²See, e.g., Michel J. Crozier, Samuel P. Huntington, and Joji Watanuki, *The Crisis of Democracy* (New York: New York University Press, 1975)

³³Amy Chua, *World on Fire: How Exporting Free Market Democracy Breeds Ethnic Hatred and Global Instability* (New York: Random House, 2003); Helen Fein, "More Murder in the Middle: Life-Integrity Violations and Democracy in the World, 1987," *Human Rights Quarterly* 17, no. 1 (1995); Demet Yalcin Mousseau, "Democratizing with Ethnic Divisions: A Source of Conflict?" *Journal of Peace Research* 38 (September 2001); Papaioannou and Siourounis (fn. 1); Jack L. Snyder, *From Voting to Violence: Democratization and Nationalist Conflict* (New York: W. W. Norton, 2000).

may be greeted with extravagant expectations. Almost inevitably, democracy experienced is never quite the same as democracy envisioned (see, for example, O'Donnell and Schmitter on *desencanto*).³⁴ The process of give-and-take among competing priorities may seem to barter away what had initially been gained, a corruption of the pure democratic ideal into brokerage politics. Needless to say, such disillusionment does not augur well for political stability. In addition, democratization frequently stimulates a surge of demands on the part of previously quiescent and perhaps even actively repressed groups. These might be lower classes, excluded ethnic or racial groups, or some other category of out-group.³⁵ While beneficial in the long run, the short-run effects of such mobilizations from below may be destabilizing and may have adverse effects on the investment climate.³⁶

If democracy survives its tumultuous youth, however, we anticipate that the extreme nature of political conflict will moderate over time. A democratic political system is, by definition, open to the inclusion of all sizable social groups and interests. Once granted a taste of political power, elites at the head of radical social movements may find it in their interest to join an existing party or coalition. Moreover, the relatively open nature of deliberation in an established democracy may diminish the appeal of conspiracy theories, which tend to flourish in the fog of authoritarian rule.³⁷ 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 these reasons, the thesis of democratic overload is much more compelling when applied to new democracies than when applied to old ones. New democracies tend to be boisterous, obstreperous affairs. 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 and given a sufficient degree of political institutionalization, we expect that democracies will provide greater stability and more efficient public poli-

³⁴ Guillermo O'Donnell and Philippe Schmitter, *Transitions from Authoritarian Rule: Tentative Conclusions about Uncertain Democracies* (Baltimore: Johns Hopkins University Press, 1986).

³⁵ Susan Eckstein, ed., *Power and Popular Protest: Latin American Social Movements* (Durham: University of North Carolina Press, 1989); Arturo Escobar and Sonia Alvarez, eds., *The Making of Social Movements in Latin America* (Boulder, Colo.: Westview Press, 1992); Alfred P. Stepan, *Democratizing Brazil: Problems of Transition and Consolidation* (Oxford: Oxford University Press, 1989); Sidney Tarrow, *Power in Movement: Social Movements and Contentious Politics* (Cambridge: Cambridge University Press, 1998).

³⁶ Stephen Haggard and Robert R. Kaufman, *The Political Economy of Democratic Transitions* (Princeton: Princeton University Press, 1995), 184–86.

³⁷ Charles Tilly, "Parliamentarization of Popular Contention in Great Britain, 1758–1834," *Theory and Society* 26 (1997).

cies. Arguably, the problem of overload arises not from institutional sclerosis³⁸ but rather from *insufficient* institutionalization.³⁹

A final product of successful political institutionalization under democratic auspices is that nebulous state of grace known as the *rule of law*. In a state governed by the rule of law

1) laws must be general; 2) laws have to be promulgated (publicity of the law); 3) retroactivity is to be avoided, except when necessary for the correction of the legal system; 4) laws have to be clear and understandable; 5) the legal system must be free of contradictions; 6) laws cannot demand the impossible; 7) the law must be constant through time; and 8) congruence must be maintained between official action and declared rules.⁴⁰

The rule of law is generally acknowledged to be a key ingredient in the establishment of secure property rights and in the achievement of credible commitment to those policies, which underpin growth in a market economy.

While a limited rule of law has been successfully established in some authoritarian states, it is usually difficult to maintain and can never—by definition—bind the ultimate decision makers. With respect to the legislature, the judiciary, and other arms of government, authoritarian states usually find it difficult to depersonalize political authority, a key requisite of the rule of law. In no autocracy is it possible for present-day rulers to effectively constrain future decisions, particularly those taken by their successors. This means that long-term credible commitment is impossible in an authoritarian setting.

By contrast, the institutionalization of power in a democratic regime is closely linked to the establishment of rule of law. The same forces that rationalize channels of power also tacitly endorse the rule of law—so much so that a fully institutionalized democracy (as described above) is impossible to imagine in the absence of rule of law. While we have granted causal precedence to “institutionalization,” it will be seen that these two processes are so closely aligned that they are difficult to disentangle empirically. In any case, the key point is that it takes a great deal of time to establish a formal framework to create and administer the law in a new regime, to ensure compliance, and to allow for the

³⁸ Mancur Olson, *The Rise and Decline of Nations* (New Haven: Yale University Press, 1982).

³⁹ Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968).

⁴⁰ Ignacio Sanchez-Cuenca, “Power, Rules, and Compliance,” in Jose Maria Maravall and Adam Przeworski, eds., *Democracy and the Rule of Law* (Cambridge: Cambridge University Press, 2003), 68. Alternative definitions tend to converge on key points, though terminology differs, for which, too, see Maravall and Przeworski.

slow diffusion of norms sanctioning this delicate arrangement. Thus, it may be argued that there are two necessary conditions for the firm establishment of the rule of law: democracy and a well-institutionalized public sphere.⁴¹

SUMMARY

In this lengthy theoretical preamble we have stipulated that democracy, if maintained over time, influences economic performance through four main channels, each of which can be conceptualized as a variety of capital—*physical*, *human*, *social*, and *political*. Thus, we believe that the argument for a “democratic growth effect” is quite plausible if regime type is considered through a historical lens: democracy + time = economic development.

Since three of these pathways are well rehearsed, our discussion focused primarily on the last—*political capital*. (This should not be understood as an implicit weighting scheme; indeed, we believe that all four of these causal pathways are critical.) We argued that this key concept may be useful as a general term for a variety of political developments that show cumulative causal effects over time and that result from processes of political learning and political institutionalization.

It is worth reflecting upon the fact that while the concepts of physical capital and human capital are well known among economists and the concept of social capital is increasingly common in all the social sciences, we lack an equivalent concept pertaining to the political realm. The concept of capital captures the time-dependent qualities of political institutions and is, on this ground, more satisfying than other terms that have entered the contemporary jargon, for example, “good institutions,” “governance,” and so forth.

To be sure, our notion of political capital overlaps somewhat with that of human and social capital. Learning about policy-making is in a sense simply a special form of human capital appreciation, while the emergence of consensus is reminiscent of some conceptions of social capital. What we have in mind, however, is knowledge and consensus that can be acquired only through political exposure. It is the nature of the “investment” required to generate political capital that most clearly distinguishes it from human and social capital.

⁴¹This conclusion echoes and dissents from Christopher Clague, Philip Keefer, Stephen Knack, and Mancur Olson, “Property and Contract Rights in Autocracies and Democracies,” *Journal of Economic Growth* 1, no. 2 (1996).

DEMOCRACY: CONCEPT AND MEASUREMENT

In order to transform our theoretical expectations into a testable hypothesis, we must address the difficult question of how to conceptualize and measure the key concept, democracy. It is a broad concept, encompassing a wealth of possible attributes circling around a core meaning—rule by the people. A useful definition must strive to capture the essentials of whatever theoretical argument the concept is intended to test. Thus, our definition, while resonating with ordinary usage, is not intended to capture all nuances of “democracy” and should *not* be viewed as a general definition of that term.

Since our theoretical question is whether democracy has a positive effect on economic performance, our definition of this difficult concept must attend to those aspects of democracy that seem most likely to affect economic performance. So viewed, democracy is perhaps best understood, following Joseph Schumpeter, as “that institutional arrangement for arriving at political decisions in which individuals acquire the power to decide by means of a competitive struggle for the people’s vote.”⁴² This feature, rather than the closeness of the vote,⁴³ turnout, or actual turnover among ruling elites,⁴⁴ seems mostly likely to affect economic performance. The key question is whether the current in-group is at all likely to become an out-group in the near future. Competition may remain a potentiality; it need not be actualized over the short term. Indeed, members of a polity may freely choose to reappoint a ruling group over many years, and the differentials between winning and losing parties may be quite large. Botswana, where the Botswana Democratic Party (BDP) has ruled since independence, exhibits both of these features but is still, according to our reckoning, a country in which the ruling elites face potential rejection at each election. Thus, we shy away from simple, mechanistic indicators of democracy.

Instead, the concept of potential political competition compels us to consider a raft of matters that are sometimes difficult to measure; they are, indeed, analogous to the equally fuzzy notion of an “open” market. (How does one know for sure that a market is unconstrained?) With respect to political markets, the following issues seem critical but are by

⁴² Joseph A. Schumpeter, *Capitalism, Socialism and Democracy* (New York: Harper and Bros., 1942/1950), 269.

⁴³ Tatu Vanhanen, *The Process of Democratization: A Comparative Study of 147 States, 1980–88* (New York: Crane Russak, 1990).

⁴⁴ Przeworski et al. (fn. 1).

no means comprehensive. Elected officials should be sovereign; non-elective bodies such as a military junta, monarchy, or caste must not exercise real power behind the scenes. There should be regular elections, and these elections must be open to all citizens (both as candidates and as electors), subject to only minor restrictions having to do with age or residency. Suffrage should be broad, though not necessarily universal. Political liberties should be extensive, particularly as pertains to the task of political organization and campaigning. Election resources, including access to money, the media, and voters, should be fairly allocated. Each of these is perhaps best understood as a matter of degree. (Can one envision a perfectly fair allocation of election resources?) This inclines us toward a continuous, rather than a dichotomous, measure of democracy.⁴⁵

Our central hypothesis concerns the temporally dependent role of democracy, its long-term potential to foster economic growth. We wish to capture not only differences in *degree* of democracy-autocracy but also differences of *duration*. We propose therefore to measure democracy as a stock, rather than level, variable; that is, to measure the accumulation of democratic experience.⁴⁶

We employ the Polity2 variable drawn from the Polity IV data set as our principal measure of democracy.⁴⁷ This variable is constructed by measuring the extent to which democratic or autocratic “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. This indicator is highly sensitive (it employs a twenty-one-point scale) and offers extensive country coverage (all sovereign polities except microstates) and good historical coverage. Moreover, it allows us to consider both the degree and the duration of democracy in any given country-year. The Polity data set,

⁴⁵ On conceptualizing and measuring democracy, see Kenneth A. Bollen and Pamela Paxton, “Subjective Measures of Liberal Democracy,” *Comparative Political Studies* 33 (February 2001); David Collier and Robert Adcock, “Democracy and Dichotomies: A Pragmatic Approach to Choices about Concepts,” *Annual Review of Political Science* 2 (June 1999); Scott Mainwaring, Daniel Brinks, and Anibal Perez-Linan, “Classifying Political Regimes in Latin America, 1945–1999,” *Studies in Comparative and International Development* 36 (October 2001); Gerardo L. Munck and Jay Verkuilen, “Conceptualizing and Measuring Democracy: Alternative Indices,” *Comparative Political Studies* 35 (February 2002); Przeworski et al. (fn. 1).

⁴⁶ Among extant studies of democracy and growth we have found only a few that approach the concept of democracy over time, for example, Erich Weede, “Legitimacy, Democracy, and Comparative Economic Growth Reconsidered,” *European Sociological Review* 12 (December 1996); and we have found none that stretches back to encompass the entire twentieth century.

⁴⁷ Monty G. Marshall and Keith Jagers “Polity IV Dataset Project: Political Regime Characteristics and Transitions, 1800–1999” (2000). Page numbers cited refer to Dataset User’s Manual.

however, imposes two serious 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.⁴⁸ Granted, questions might be raised with respect to all extant, and all conceivable, democracy indices (see previous citations). Polity2 is no worse than the rest and probably better than the average. It is, indeed, the industry standard, owing to the strengths noted above. Reassuringly, the Polity2 variable correlates highly with other existing measures of democracy.⁴⁹ There is no reason to suspect systematic errors in this index that might affect the substantive findings of this study.

To correct for Polity2's exclusion of microstates, 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: (1) the Freedom House Political Rights indicator,⁵⁰ (2) Ken Bollen's Liberal Democracy variable, (3) Tatu Vanhanen's Competition variable, (4) Arthur Banks's Legislative Effectiveness variables (I and II), and (5) Banks's Party Legitimacy variable.⁵¹ These various 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. This imputation process adds about five hundred observations (less than 10 percent of the original sample) to the original Polity2 variable.

To create a *stock* measurement of democracy from this variable we add up each country's Polity2 score from 1900 to the present year, with a 1 percent annual depreciation rate. This means that a country's

⁴⁸Bollen and Paxton (fn. 45); Kirk Bowman, Fabrice Lehoucq, and James Mahoney, "Measuring Political Democracy: Case Expertise, Data Adequacy, and Central America" *Comparative Political Studies* 38 (October 2005); Munck and Verkuilen (fn. 45); Shawn Treier and Simon Jackman, "Democracy as a Latent Variable" (Paper presented at the Political Methodology meetings, University of Minnesota, Minneapolis-St. Paul, July 2003).

⁴⁹Correlations between Polity2 and other democracy indices (introduced below in the text) are as follows: "Political Rights" (Freedom House) = -.85; "Liberal Democracy" (Bollen) = .92; "Democracy index" (Vanhanen) = .85.

⁵⁰Freedom in the World, survey methodology, on the Freedom House Web site: www.freedomhouse.org/research/freeworld/2000/methodology.htm.

⁵¹Kenneth A. Bollen, "Liberal Democracy: Validity and Method Factors in Cross-National Measures," *American Journal of Political Science* 37 (November 1993); Vanhanen (fn. 43); Arthur S. Banks, "Cross-National Time-Series Data Archive" (Binghamton, N.Y.: Center for Social Analysis, State University of New York at Binghamton, 1994).

regime stock stretches back over the course of a century. The year 1900 is chosen as a threshold year ushering in a period (1) in which mass democracy becomes a world-historical phenomenon (no longer restricted to the U.S. and a few European states), (2) in which it is not unreasonable to assume a causal relationship between democracy and growth, and (3) in which the data exist to test such a relationship.⁵²

Because the historical component of this index weighs heavily on our understanding of the concept and because the Polity data set ignores nonsovereign 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 an imperial power, it receives the same Polity2 score as its imperial ruler; for example, Estonia receives the same score as the Soviet Union from 1941 through 1990. We use this procedure only for nation-states that were contiguous with the empire to which they 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.⁵³

For noncontiguous colonies we assign a Polity2 score of 0 for all preindependence years. While this procedure is admittedly somewhat arbitrary, it has relatively little effect on an analysis that focuses only on postindependence years in a fixed-effect format (for obvious reasons, there are no growth data prior to a country's achievement of formal sovereignty). Note that the preindependence years constitute a largely static component of a country's score for any given (postindependence) observation; thus, it is captured in that country's unique intercept. This means that any inaccuracies introduced by our arbitrary scoring of preindependence years will have relatively little effect on empirical results reported

⁵²We see no reason to suppose that a longer period of measurement—which might, in principle, stretch back to 1850—would alter any of the findings presented here.

⁵³This recoding affects the following countries: Albania (1900–1912, Ottoman Empire), Andorra (1900–present, France), Armenia (1900–1990, Russia/USSR), Azerbaijan (1900–1990, Russia/USSR), Belarus (1900–1990, Russia/USSR), Bosnia-Herzegovina (1908–17, Austria-Hungary; Yugoslavia 1929–91), Croatia (1900–1917, Austria-Hungary; Yugoslavia, 1929–91), Czech Republic (1900–1917, Austria-Hungary), Slovakia (1900–1917, Austria-Hungary), Estonia (1900–1916 and 1941–90, Russia/USSR), Finland (1900–1916, Russia), Georgia (1900–1990, Russia/USSR), Iraq (1900–1917, Ottoman Empire), Palestine/Israel (1900–1917, Ottoman Empire), Kazakhstan (1900–1990, Russia/USSR), Kyrgyzstan (1900–1990, Russia/USSR), Latvia (1900–1917 and 1941–90, Russia/USSR), Lithuania (1900–1917 and 1941–90, Russia/USSR), Macedonia (1922–90, Yugoslavia), Moldova (1900–1945, Romania; 1946–90, USSR), Mongolia (1900–1920, China), Bangladesh (1947–71, Pakistan), Slovenia (1900–1917, Austria-Hungary; 1929–91, Yugoslavia), Syria (1900–1917, Ottoman Empire), Tajikistan (1900–1990, Russia/USSR), Turkmenistan (1900–1990, Russia/USSR), Ukraine (1900–1917 and 1920–90, Russia/USSR), Uzbekistan (1900–1990, Russia/USSR), and East Timor (1976–99, Indonesia).

in subsequent data tables—most of which employ a fixed-effect format. Thus, although we are conscious of the arbitrary quality of this coding procedure, we are confident that it does not jeopardize the main results of the article.⁵⁴ (For the *nonfixed* effect regression tests displayed in the appendix, the arbitrary scoring of these preindependence years matters much more. This constitutes yet another reason for preferring a regression specification with country fixed effects.)

In order to clarify how this coding procedure translates into democratic stock for the countries in our sample, we include a graph with scores for four countries that illustrate diverse regime trajectories. Figure 2 depicts democratic stock for the United States (democratic throughout the century), China (authoritarian throughout the period), Chile (intermittently democratic), and Botswana (democratic since independence, in 1966). We include scores for the entire century even though our empirical tests cover only the postwar era (1950–2000). Note that the slope of the curve moderates as a country accumulates more democratic experience, as in the case of the United States toward the end of the twentieth century.

ANALYSIS

The relationship between democracy and growth may be tested in many ways. We take as our point of departure the cross-country regression format. Yet, this scarcely limits the methodological field since any of the multitudinous approaches employed in current growth regressions might also be applied to this particular question.⁵⁵ The researcher faces choices about which time intervals to consider, how to correct for serial and spatial autocorrelation, and how to resolve issues of specification, simultaneity, and endogeneity, among other matters.

Fortunately, there is general agreement about how to measure the dependent variable, economic growth, which is usually understood as the percentage change in GDP per capita. We employ the World Devel-

⁵⁴In an alternative coding of this variable we assign a score of -10 (the lowest score on the Polity2 variable) to missing data from preindependence years (rather than the usual default score of 0). The historical variable constructed from this alternative coding procedure shows virtually identical results to those displayed in Table 1. Thus, in fixed-effect tests it does not matter what sort of coding procedure is used to fill in missing data from years prior to a country's independence. These scores, the reader will recall, matter only for the construction of the stock variable; they are not observations in the data analysis (no growth data is available for periods prior to independence).

⁵⁵For a recent review, see Jonathan Temple, "The New Growth Evidence," *Journal of Economic Literature* 37 (March 1999).

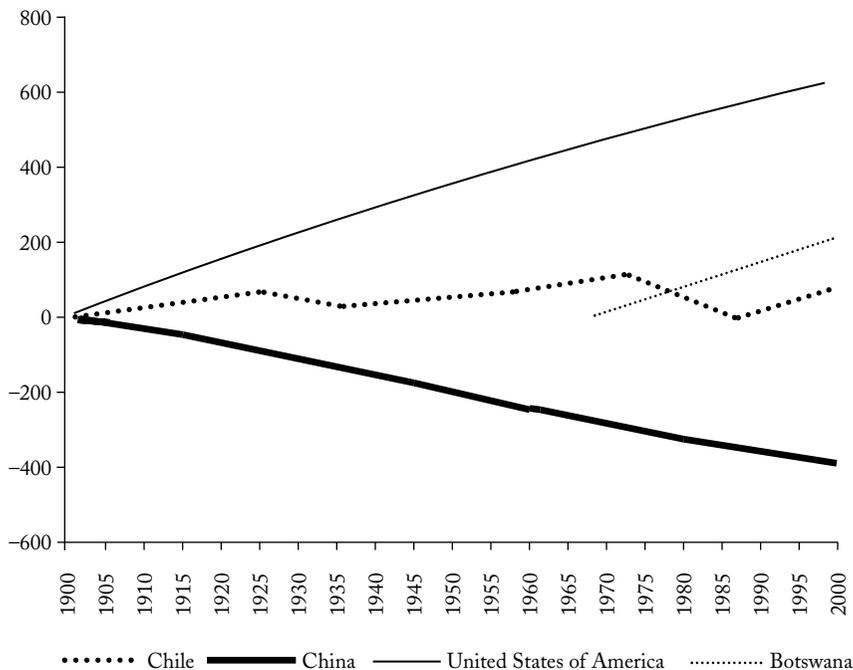


FIGURE 2
DEMOCRATIC STOCK: AN ILLUSTRATION^a

^aDemocratic stock (based on 1 percent annual depreciation rate) for four countries, 1900–2000. (Subsequent regression tests are limited to the years 1950–2000.)

opment Indicators growth variable, measured in constant dollars.⁵⁶ Additional data for the 1950s is imputed using the Penn World Tables (PWT) 6.1 data set (Chain index, constant dollars).⁵⁷ Our choice of the WDI data set as the primary data source for indicators of country growth is motivated by two concerns. First, WDI country coverage is considerably larger than offered by the PWT data set. Second, for various reasons explored by Nuxoll and Temple, the WDI indicator is probably the best measure of growth performance.⁵⁸

⁵⁶World Bank, *World Development Indicators* (data file) (Washington, D.C.: World Bank, 2003).

⁵⁷Robert Summers and Alan Heston, "The Penn World Table (Mark 5): An Expanded Set of International Comparisons, 1950–1988," *Quarterly Journal of Economics* 106 (May 1991).

⁵⁸See Daniel A. Nuxoll, "Differences in Relative Prices and International Differences in Growth Rates," *American Economic Review* 84 (December 1994); and Temple (fn. 56), 118–19.

Since data comprising the dependent and independent variables of interest are compiled at annual intervals, and since significant changes in both right-hand and left-hand variables occur from year to year, it makes sense to employ annual data in this analysis. Not to do so constitutes a waste of information and is prone to the aggregation problem (how long intervals of aggregated data should be). So long as patterns of temporal autocorrelation and period-specific effects can be corrected, we see no justification for aggregating data over five- or ten-year periods (or longer), as is sometimes done. The unit of analysis in all the following data tables is therefore the *country-year*. (In the appendix we offer a series of additional tests that employ data aggregated over five-year periods.)⁵⁹

Serial autocorrelation is a serious concern, as it is in most time-series formats. Our approach assumes a one-period autocorrelation in the residual. An alternative approach, with much to recommend it in certain situations, employs a lagged dependent variable in order to model the temporal dynamics of the dependent variable.⁶⁰ We find little difference in substantive results when this alternative method is employed (see the appendix). Since the interpretation of results is considerably less intuitive when a lagged dependent variable is introduced on the right side, we prefer the more straightforward AR1 correction in the residual.

The most serious challenge to cross-country analyses of distal causal relationships is the problem of simultaneity. Suppose, for instance, that a growth regression identifies factor X as contributing to growth. Almost invariably the skeptic can plausibly argue that this finding is a consequence of the existence of some unmeasured factor Z that has an impact on both growth rates and factor X. In short, other (unmeasured) factors that are correlated with democracy may account for superior growth performance, rendering our results spurious. Indeed, it is plausible to suppose that countries that are able to maintain a high level of democracy over a long period of time are also blessed with other advantages—good institutions, good resources, and so forth. Nevertheless, it is difficult, perhaps even impossible, to measure all such country-specific (unit) effects. (This supposition is borne out by vari-

⁵⁹For further discussion of the debate between annual and aggregated temporal data, see Orazio P. Attanasio, Lucio Picci, and Antonello E. Scorcu, "Saving, Growth and Investment: A Macroeconomic Analysis Using a Panel of Countries," *Review of Economics and Statistics* 82, no. 2 (2000); and Papaioannou and Siourounis (fn. 1). The annual format appears to be considerably more common in recent studies.

⁶⁰Nathaniel Beck and Jonathan Katz, "What to Do (and Not to Do) with Time-Series Cross-Section Data," *American Political Science Review* 89 (September 1995).

ous tests of spatial autocorrelation comparing the results of fixed and random effects regressions, for example, Breusch and Pagan Lagrangian multiplier tests and Hausman tests.)

To control for possibly severe unit effects (spatial autocorrelation), we employ a fixed-effect format in all analyses (additional analyses shown in the appendix break substitute a set of static controls for country fixed effects). Although this precludes examining variation across countries, a loss of explanatory leverage, it effectively removes many of the specification problems that plague cross-country studies. Note that the fixed-effect format imposes a unique intercept for each country. Factors that are for the most part constant across the time period of interest (1950–2000), such as geography, culture, and ethnicity, are eliminated by this research design. Thus, our results will suffer from omitted variable bias only if the *change* in growth rate and the *change* in democracy stock are both driven by some other (unmeasured) factor. We attempt to control for such factors in Table 1.

Endogeneity between economic growth and democracy stock is less worrisome than it may at first appear. Previous studies have shown a causal relationship between levels of economic development (as measured by per capita GDP) and democracy.⁶¹ However, it is debatable whether a country's growth rate has any effect on its level of democracy.⁶² And it seems even less likely that a country's growth performance in time T would have any effect whatsoever on its democracy stock at T-1 (stock being a measure that extends back over many decades). As a further precaution, we lag democracy stock an additional nine years (Table 2, model 4) and nineteen years (Table 2, model 5).

Specification problems pervade all cross-country growth regressions.⁶³ While the fixed-effect format handles the problem of invariant controls, it does nothing to control for factors that might vary over time. To control for convergence effects we include GDP/capita (natural logarithm) as part of the benchmark model.⁶⁴ (Thus, we measure the

⁶¹ See, e.g., Przeworski et al. (fn. 1).

⁶² Ross E. Burkhardt and Michael Lewis-Beck, "Comparative Democracy: The Economic Development Thesis," *American Political Science Review* 88 (December 1994); John B. Londregan and Keith T. Poole, "Does High Income Promote Democracy?" *World Politics* 49 (October 1996); John Helliwell, "Empirical Linkages Between Democracy and Economic Growth," *British Journal of Political Science* 24 (1994), 233–34, cited in Baum and Lake (fn. 11), 340.

⁶³ Ross Levine and David Renelt, "A Sensitivity Analysis of Cross-Country Growth Regressions," *American Economic Review* 82 (September 1992); Xavier X. Sala-i-Martin, "I Just Ran Two Million Regressions," *American Economic Review* 87 (May 1997); Temple (fn. 55).

⁶⁴ A recent reevaluation of cross-country growth empirics concludes that the log of GDP per capita is the *only* variable that is robust across all models; Michael Bleaney and Akira Nishiyama, "Explaining Growth: A Contest between Models," *Journal of Economic Growth* 7 (March 2002), 45.

TABLE 1
SPECIFICATION TESTS^a

| | 1 | 2 | 3 | 4 | 5 |
|--------------------------------|----------------------|----------------------|---------------------|----------------------|----------------------|
| Democracy stock (1900–) | | 0.006*** | 0.002* | 0.009*** | 0.007*** |
| | | (0.001) | (0.001) | (0.002) | (0.002) |
| Democracy level | -0.028 (0.021) | | | | |
| GDP pc (ln) | -2.597*** (0.452) | -2.961*** (0.488) | | -4.805*** (0.519) | -4.655*** (0.668) |
| Inflation (ln) | | | | -0.450*** (0.082) | -0.389*** (0.091) |
| Investment (PWT) | | | | 0.023 (0.023) | 0.002 (0.024) |
| Instability (Banks) | | | | -0.099*** (0.021) | -0.113*** (0.023) |
| Trade openness (PWT) | | | | 0.041*** (0.007) | 0.047*** (0.008) |
| Life expectancy (WDI) | | | | 0.134*** (0.037) | 0.194*** (0.058) |
| Oil shock (dummy) | | | | -1.338*** (0.318) | 0.124 (17.053) |
| Growth pc (trade-weighted) | | | | | 0.468*** (0.117) |
| Population growth (WDI) | | | | | 13.472 (17.259) |
| Years independent | | | | | -0.166 (0.280) |
| Regime durability (Polity IV) | | | | | 0.003 (0.010) |
| Social conflict (Marshall) | | | | | -0.503 (0.645) |
| Government consumption (PWT) | | | | | 0.001 (0.025) |
| Illiteracy (ln) | | | | | 0.640 (0.707) |
| Trend | | | | | 0.136 (0.281) |
| Annual dummies | | | | | YES |
| Constant | 21.145*** (3.387) | 23.885*** (3.670) | 1.771*** (0.085) | 28.654*** (3.576) | 31.125 (0.000) |
| <i>Observations</i> | 6264 | 6264 | 6430 | 3721 | 3296 |
| <i>Countries</i> | 180 | 180 | 187 | 149 | 129 |
| <i>Sample Period</i> | 1950–00 | 1950–00 | 1950–00 | 1961–99 | 1961–98 |
| <i>R squared (within)</i> | 0.03 | 0.03 | 0.00 | 0.08 | 0.13 |
| <i>Prob > F</i> | 0.0000 | 0.0000 | 0.0673 | 0.0000 | 0.0000 |

*p < .10; **p < .05; ***p < .01

^aFixed effect regressions with AR(1) disturbance. Units of analysis: country-year. Dependent variable: annual per capita growth rate. All predictors are lagged one year. Newey-West standard errors in parentheses (two-tailed tests). Variables and procedures are defined in the text.

TABLE 2
DEMOCRACY STOCK, VARIOUSLY OPERATIONALIZED^a

| | 1 | 2 _a | 3 _b | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|-----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| Stock (1900-) | 0.006*** (0.001) | 0.007*** (0.002) | 0.006** (0.003) | | | | | | 0.006*** (0.002) | 0.005** (0.002) |
| Stock (1900-), t - 10 | | | | 0.004** (0.001) | | | | | | |
| Stock (1900-), t - 20 | | | | | 0.004** (0.001) | | | | | |
| Dichotomous: | | | | | | | | | | |
| Continuous stock | | | | | | 0.042** (0.017) | | -0.042 (0.026) | -0.001 (0.025) | |
| Cumulative stock | | | | | | | 0.051*** (0.018) | 0.090*** (0.033) | | 0.017 (0.028) |
| GDP pc (ln) (WDI) | -2.961*** (0.488) | -2.854*** (0.524) | -3.195*** (0.887) | -2.851*** (0.561) | -2.797*** (0.637) | -3.122*** (0.617) | -3.286*** (0.654) | -3.300*** (0.655) | -2.954*** (0.636) | -3.108*** (0.689) |
| Constant | 23.885*** (3.670) | 22.985*** (3.926) | 27.513*** (7.163) | 23.241*** (4.271) | 23.193*** (4.919) | 24.537*** (4.439) | 25.424*** (4.620) | 25.384*** (4.616) | 23.843*** (4.514) | 24.696*** (4.759) |
| Observations | 6264 | 5567 | 2884 | 5574 | 4737 | 6264 | 6264 | 6264 | 6264 | 6264 |
| Countries | 180 | 156 | 78 | 178 | 176 | 180 | 180 | 180 | 180 | 180 |
| Sample Period | 1950-00 | 1950-00 | 1950-00 | 1950-00 | 1950-00 | 1950-00 | 1950-00 | 1950-00 | 1950-00 | 1950-00 |
| R squared (within) | 0.03 | 0.03 | 0.04 | 0.03 | 0.03 | 0.03 | 0.03 | 0.03 | 0.03 | 0.03 |
| Prob > F | 0.0000 | 0.0000 | 0.0004 | 0.0000 | 0.0001 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 |

*p < .10; **p < .05; ***p < .01 (two-tailed tests)

^aFixed effect regressions with AR(1) disturbance. Units of analysis: country-year. Dependent variable: annual per capita growth rate. All predictors lagged one year. Newey-West standard errors in parentheses. Variables and procedures defined in the text. a: Includes no imputed scores for the democracy stock variable, which is drawn directly from the Polity 2 variable in the Polity IV dataset, without adjustments. b: Includes only countries for which complete information is available for the democracy stock variable over the course of the entire twentieth century. Continuous measures of democracy stock are based on the 21-point Polity2 index (-10 to +10), as elsewhere. Dichotomous measures of democracy stock follow a coding system that assigns a score of 1 (democratic) to a year if a country achieves a score greater than 4 on the Polity2 scale; 0 otherwise.

effect of democracy on a country's growth rate given its current level of economic wealth.) This variable is drawn from the WDI data set,⁶⁵ with a small number of missing cases from the 1950s imputed from the PWT 6.1 data set.⁶⁶

Other controls are less obvious by virtue of their possibly endogenous relationship to democracy, their lack of robustness, or their theoretical status. At the same time, it is vital that we test as comprehensive a set of alternative controls as possible. These controls must encompass not only those identified by the prodigious literature on economic growth but also those factors that might affect the simultaneity problem discussed above. These control variables are introduced seriatim and then in a series of "full" regressions, so as to test their individual and collective effects on the variable of interest. (Descriptive statistics for all variables are included in the appendix, in Table 4.)

To summarize, our main variable of interest is the depreciated democracy stock of country i in year t , defined by:

$$democracy_stock_{i,t} = \sum_{s=1900}^t 0.99^{t-s} democracy_level_{i,s} \quad (1)$$

As discussed, the democracy level is measured using Polity2 scores; missing values are either imputed or assigned to the intermediate value of 0 in the case of countries that have gained independence from non-contiguous ruling counties since 1990.

Our main estimation model is

$$g_{i,t} = \alpha_i + \beta democracy_stock_{i,t-1} + \gamma Z_{i,t-1} + \varepsilon_{i,t} \quad (2)$$

where $g_{i,t}$ is the growth rate in country i in year t , α_i is a country-specific intercept term, $Z_{i,t}$ is a vector of control variables for country i in year t , and $\varepsilon_{i,t}$ is the error term. As discussed, we use an AR1 specification, so that $\varepsilon_{i,t} = \rho \varepsilon_{i,t-1} + v_{i,t}$ for some constant ρ , with $v_{i,t}$ independently and identically distributed over both countries i and dates t . The main object of interest is the parameter β , which measures the relationship between the stock of democracy and the growth rate.

Most of the following regression tests employ Newey-West standard errors, which assume a heteroskedastic error distribution and apply a TSCS equivalent of Huber/White/sandwich, or "robust," standard er-

⁶⁵World Bank (fn. 56).

⁶⁶Summers and Heston (fn. 57).

rors.⁶⁷ (Substantive results are virtually indistinguishable in other formats, for example, with a Prais-Winsten feasible generalized least squares [FGLS] approach with panel corrected standard errors.) A one-period correction for serial autocorrelation is assumed in all models, with the exception of several regressions in the appendix where a lagged dependent variable is included or where five-year panels are substituted for annual data.

RESULTS

Table 1 presents a series of specification tests probing the performance of the main independent variable of interest, democracy. We begin with a *level* indicator of democracy, as measured by the Polity2 variable drawn from the Polity IV data set (discussed above). We include only one all-purpose control in this reduced-form model, GDP per capita (logged). The results confirm the standard finding: democracy has no statistically significant effect on economic growth. This nonrelationship is robust across a wide range of democracy indicators and model specifications (not shown). It matters not how one measures the level of democracy in a given year; it still has no effect on subsequent economic performance.

In the remaining models we investigate democracy as a *stock* variable. Model 2 presents what we regard as our benchmark model, with only one control variable (replicating the specification of model 1). Model 3 drops that control variable, demonstrating that democracy stock enhances growth performance even when convergence effects are ignored.

Model 4 includes a small set of control variables chosen by virtue of their theoretical significance (in the growth literature) and/or their robust performance in this specification. These include: *Inflation*, understood as annual percent change in consumer prices (natural logarithm);⁶⁸ *Investment*, understood as the share of real GDP comprised by investment (PWT 6.1); *Instability*, including assassinations, general strikes, guerrilla warfare, government crises, purges, riots, revolutions, and antigovernment demonstrations;⁶⁹ *Trade openness*, understood as imports and exports as a share of GDP (PWT 6.1); *Life expectancy*,⁷⁰ and *Oil shock* (dummy: 1950–73 = 0, 1974–2000 = 1).

⁶⁷ Whitney K. Newey and Kenneth D. West, "A Simple, Positive Semi-Definite, Heteroskedasticity and Autocorrelation Consistent Covariance Matrix," *Econometrica* 55 (May 1987).

⁶⁸ World Bank (fn. 56).

⁶⁹ These variables, drawn from the Banks (fn. 51) data set, are added together to form a composite index (construction of index by the authors).

⁷⁰ World Bank (fn. 56).

Model 5, which may be regarded as a “full” (or “packed”) specification, adds another set of control variables that are available for a large sample of countries over the postwar period and are suggested by (or might be intuited from) the literature. These include: *Growth per capita, trade-weighted* (each country is assigned the mean value of the growth rate of all other countries in the world in that year, weighted by their bilateral trade with the country in question); *Population growth*; ⁷¹ *Years independent* (the number of years a country has been sovereign; coded by the authors); *Regime durability*, understood as the number of years since the last three-point change in the composite Polity2 score (Polity IV); *Social conflict*, including civil violence, civil war, ethnic violence and ethnic war; ⁷² *Government consumption*, understood as the government share of real GDP per capita (PWT 6.1); *Illiteracy*, as a logged function (World Bank 2003); ⁷³ a trend variable (1950 = 1, 1960 = 2, . . .); and a series of annual dummy variables to provide a further control for time-specific effects. ⁷⁴

Although these various specification tests affect the coefficient and standard error of democracy stock, as one might expect, the variable remains highly statistically significant. (The one exception is model 3, in which no controls are included at all. Even here, the coefficient on democracy stock is positive at a 10 percent significance level.) When measured as a stock variable, democracy appears to have a strong positive relationship to growth performance regardless of the specification of the growth equation. In additional analyses (not shown), we introduce these control variables seriatim into the benchmark equation to make sure that their individual effects do not impair the performance of the key variable of interest, democracy stock. (They do not.) For the most part, these control variables perform as expected.

However, for a variety of reasons, we do not maintain these controls in most of the tests that follow. First, there is a substantial loss in degrees of freedom when the equation is expanded to include the full set of controls. Second, there is serious question about the theoretical justification (not to mention the empirical robustness) of all of these con-

⁷¹ Ibid.

⁷² These variables are added together to form a composite index (construction by the authors). Monty Marshall, “Major Armed Conflicts and Conflict Regions, 1946–1997,” data set from CIDCM, University of Maryland (1999), obtained via the State Failure Task Force data set, <http://gking.harvard.edu/data.shtml> (accessed April 25, 2005).

⁷³ World Bank (fn. 56).

⁷⁴ We interpolate missing values for illiteracy, life expectancy, and population growth in order to maintain a consistent sample (failing to do so would have significantly reduced the sample size and perhaps biased our results).

trols. Third, there is the danger of overspecifying causal relationships: note that democracy stock may affect any and all of these control variables. Indeed, the coefficient for democracy stock *increases* in the full models, a result that seems dubious if one's principal objective is to measure the independent effect of democracy stock. For all these reasons, it seems safer to work with a smaller benchmark equation, including only GDP per capita, as in model 2. This is the only control that can claim some degree of theoretical consensus, is empirically robust, and is—with respect to the causal question at hand—exogenous.

Having tested a series of control variables we turn now to alternative conceptualizations of our key variable, democracy stock, and other samples. Table 2 begins with the benchmark equation (model 1), which replicates model 2 in Table 1. This will provide the basis for easy comparisons across alternative measures and samples. Model 2 excludes all cases for which we added or imputed additional data for the democracy stock variable, and thus represents the stock variable drawn directly from the Polity IV data set (Polity2). Not surprisingly—since we have lost only 697 observations—the coefficients and standard errors are fairly stable.

It is possible, however, that democracy stock is measuring something other than democracy per se. One possibility is that this variable is acting as a proxy for the length of time a country has been autonomous, that is, the duration of a nation-state. It could be, in other words, that a high score on democracy stock is indicative of a long historical experience of sovereignty, rather than (or in addition to) a long experience with democratic elections. We offered an initial test of this hypothesis in Table 1 (see model 5), where we entered a control variable measuring the duration of sovereignty (the number of years a country has been independent). As an alternative test, in model 3 we recalculate the benchmark regression including only those countries for which complete data are available *for the entire twentieth century* (including scores based on the Polity2 values of empires of which a country was, for a time, encapsulated within, as explained above). Although the number of countries in the data set, as well as the corresponding number of observations, drops to less than one-third, the coefficient and standard errors are fairly stable, suggesting that our results are indeed driven by differences in democracy and not by sovereignty or a lack thereof or by arbitrary coding decisions with respect to missing data.

Models 4 and 5 introduce longer lags than are employed in other models. Recall that all regressions reported in Table 1 and elsewhere in this article systematically lag independent variables by one time period,

that is, one year. Model 4 shows a ten-year lag on democracy stock and model 5 shows a twenty-year lag. Remarkably, the value of democracy stock's coefficients diminishes only slightly, while the standard errors are stable (relative to the benchmark equation), confirming that the relationship between democracy stock and growth is not an instance of reverse causality.

Models 6–10 test the effects of democracy stock on growth when the concept of democracy is understood as dichotomous (democracy/autocracy), rather than continuous.⁷⁵ A country-year is coded as democratic if it falls above 4 on the Polity2 scale (from –10 to +10) and authoritarian otherwise. Our first variable, Continuous Stock, is created by adding up all years of continuous democracy experienced by a country in a given year. In 2000 Chile had experienced only twelve years of continuous democratic rule and thus receives a score of 12. The United States, by contrast, had experienced one hundred years of democracy since 1900 and therefore receives a score of 100. Results are depicted in model 6.

Democracy might also be considered as a *cumulative* phenomenon such that a country's economic performance depends upon the total number of democratic years since 1900—disregarding any authoritarian breaks. In this rendition, Chile receives credit for its previous democratic history, prior to the Pinochet coup. Results for this variable are depicted in model 7.

Model 8 includes both dichotomous measures of democracy—Continuous Stock and Cumulative Stock—together. Models 9 and 10 reprise models 6 and 7 with the addition of our preferred measure of democracy stock, where democracy is understood as a scalar concept (on the twenty-one-point Polity2 scale).

Two important conclusions may be drawn from these five regressions (6–10). First, the relationship to growth is stronger when democracy is considered as a cumulative concept, rather than a continuous concept. Prior democratic experience seems to matter, regardless of whether or not such experience was interrupted by an authoritarian interlude. Second, the relationship of democracy to growth is much stronger when democracy is measured as a scalar, rather than dichotomous, concept. Our original variable, based on the twenty-one-point Polity index, swamps the effects of both dichotomous measures. Apparently, both the *degree* and the *duration* of democratic experience matter when one considers the effect of democracy on growth.

⁷⁵ See Papaioannou and Siourounis (fn. 1); Przeworski et al. (fn. 1).

TABLE 3
SPLIT-SAMPLE TESTS^a

| | <i>Excluding</i> | | | | |
|---------------------------|--|---------------------------|----------------------------|--|----------------------------|
| | <i>1</i> <i>Middle</i> <i>East</i> | <i>2</i> <i>Africa</i> | <i>3</i> <i>Asia</i> | <i>4</i> <i>Latin Am/</i> <i>Caribbean</i> | <i>5</i> <i>OECD</i> |
| Democracy stock (1900–) | 0.006*** (0.002) | 0.004** (0.002) | 0.011*** (0.002) | 0.008*** (0.002) | 0.006*** (0.002) |
| GDP pc (ln) | -2.552*** (0.525) | -2.487*** (0.367) | -4.467*** (0.675) | -2.849*** (0.541) | -3.234*** (0.657) |
| Constant | 20.615*** (3.912) | 21.801*** (2.910) | 35.154*** (5.107) | 22.938*** (4.047) | 24.302*** (4.608) |
| <i>Observations</i> | 5596 | 4534 | 5488 | 4958 | 4852 |
| <i>Countries</i> | 161 | 131 | 157 | 145 | 147 |
| <i>Sample Period</i> | 1950–00 | 1950–00 | 1950–00 | 1950–00 | 1950–00 |
| <i>R-squared (within)</i> | 0.02 | 0.03 | 0.05 | 0.03 | 0.03 |
| <i>Prob > F</i> | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 |

*p < .10; ** p < .05; *** p < .01 (two-tailed test)

^aFixed effect regressions with AR(1) disturbance. Units of analysis: country-year. Dependent variable: annual per capita growth rate. All predictors are lagged one year. Newey-West standard errors are in parentheses.

In Table 3, we offer a series of split-sample tests of our major hypothesis. Evidently, the results shown here are unlikely to be affected by the results of any single country. With a sample of 180 countries across the world—essentially the entire universe of independent nation-states—it is virtually inconceivable that an individual country would constitute an influential case. However, it is possible that particular *regions* of the world might affect the results reported here. In this series of regressions, we exclude regional or socioeconomic blocs that might serve as an influential set of cases in the benchmark equation: the Middle East (model 1), sub-Saharan Africa (model 2), Asia (model 3), Latin America and the Caribbean (model 4), and the OECD (model 5). None of these split-sample tests threatens the statistical significance of the democracy stock variable, though there are modest changes in the coefficient and standard error.

Note that the final model restricts the sample to the developing world. This offers a test of the possibility that the effects measured in our aggregated results might be driven by peculiarities of the—mostly Anglo-European—early democratizers. Model 5 indicates that the connection between democracy and growth captured in the democracy stock variable is as strong among late democratizers as it is among early democratizers. Thus, it seems safe to conclude that our results are not

the product of idiosyncratic regional effects. This is not to say that the relationship between democracy and growth is *identical* in all regions; most assuredly, it is not. It is merely to point out that the aggregate results are not being driven by regional peculiarities.⁷⁶

THE DEMOCRATIC GROWTH EFFECT

We have shown thus far that the relationship between democratic stock and growth is robust in a variety of plausible specifications and operationalizations. (Further tests are conducted in the appendix.) We turn now to the question of its *practical* significance. Is the democratic growth effect significant in real (policy) terms?

Let us consider the results of model 5 in Table 1, in which we control for a range of other possible causal factors. We regard this model as offering a conservative estimate of causal effects since many of the variables introduced as controls in this model may be endogenous to democracy, and hence might be suppressing democracy's true causal effect on growth. (Note that the coefficient for democracy is virtually unchanged from the reduced-form equation in model 2, so it hardly matters which model one chooses to base this estimate on.)

Within the parameters of this model, a country with no existing stock of democratic capital (for example, Botswana in 1966) experiences the following democratic growth effect: for each full decade of high-quality democracy ($\text{Polity2} = 10$), democracy stock increases by approximately 100 points. To estimate the predicted effect of this change on growth, we simply multiply this change by the coefficient on democracy stock, 0.007. So from model 5 in Table 1, the predicted growth impact of a decade of high-quality democracy is approximately 0.7 percent. Given the well-known cumulative effects of small increases in the growth rate, these changes are significant. For instance, an increase in the annual growth rate from 2 percent to 2.7 percent reduces the time needed to achieve a doubling of incomes from thirty-five to twenty-six years; an increase to 3.4 percent further reduces the doubling period to 20.7 years.⁷⁷

⁷⁶ An additional test—not shown—divides the sample into (1) observations where a country registers a “positive” democratic stock (>0) and (2) observations where a country registers a “negative” democratic stock (<0). This tests the possibility that our aggregate results are being driven by influential groups of long-term democracies *or* long-term autocracies. Reassuringly, the coefficients and standard errors for our key variable, democracy stock, are virtually constant across these two subsamples, suggesting that there is a relatively consistent democratic growth effect for deep autocracies and deep democracies.

⁷⁷ The number of years that a country growing at rate g takes to double its income is given by $\ln 2 / \ln(1+g)$.

One concrete implication of this approach concerns the effect of institutional reforms, reforms that currently occupy the attention of academics and policymakers in the developed and developing world. If democratic institutions contribute to a community's political capital and thereby to secular-historical changes in policy outcomes, then it is inappropriate to judge the results of such reforms on the basis of immediate policy gains. Although this renders the testing of hypotheses more complicated—since we must await long-run results—it may temper the impatience of those who jump to premature conclusions about the success or failure of regime change, neoliberal reform, electoral reform, and other reforms of basic institutions. It is unrealistic to expect that the sweeping changes brought about by such reforms would show instantaneous results. Indeed, while immediate effects of institutional change are often negative—since such change introduces uncertainties and information costs in the short run—positive changes are likely to take longer to materialize, since they depend upon the establishment of a new equilibrium. The concept of political capital may help policymakers to conceptualize, and thereby accurately account for, the true causal effects of institutional reform.

Many important questions remain to be considered, two of which merit special mention. First, we have adopted a 1 percent depreciation rate for the calculation of a country's regime history. This choice is based primarily on trial and error: we found that alternative measures of democracy stock calculated using depreciation rates of 5 percent and 10 percent had less predictive power for a country's growth rate. In principle, the appropriate depreciation rate could also be estimated using nonlinear estimation techniques. For example, a country's regime type ten years ago might be more important for growth today than its regime type twenty years ago or its regime type five years ago. These issues lie outside the scope of the current article, whose purpose was to introduce the concept of democratic stock, not to test a panoply of measures for that concept. Second, in our theoretical discussion we suggested several mechanisms by which such a causal relationship might be realized (for example, better economic policies and greater political stability). However, we did not attempt to assign a causal weight to any of these causal pathways or to test them directly, a task that we must also defer to future work.

INSTITUTIONS IN TIME

This article has demonstrated that the effect of regime type on growth is mediated by a country's secular-historical experience of democracy and authoritarianism. This claim stands in sharp contrast to the con-

ventional wisdom that there is no consistent relationship between growth and democracy—or, perhaps, a negative relationship. Our somewhat surprising findings stem from a conceptual insight that regime effects materialize from a country's democratic stock rather than its contemporaneous regime type. Thus, while a country's level of democracy in a single year has no measurable impact on its growth rate in the subsequent year, its democratic experience over the course of the twentieth century is positively associated with growth in subsequent years. Long-term democracy leads to stronger economic performance.

This finding, while important in its own right, may also prompt us to reconceptualize the causal role of other institutional variables in the social sciences. Let us begin by contrasting institutional explanations with explanations that are rooted in opinions, interests, or coercion (which may be considered a special variety of interest-based argument). In the latter, we expect that the relationship between a cause and its putative effect is generally fairly immediate and generally takes a linear form. A person's interests (or opinions or coercive incentives) today should be acted upon today and will remain the same (or will increase/decrease in a linear fashion) tomorrow, unless some other factor changes the nature of that interest (or value).⁷⁸

With *institutional* explanations, however, the relationship between cause and effect is often quite different. The most important effects of a change in electoral laws, in the structure of the executive (parliamentary/presidential), in sovereignty, or in property rights are distal, not proximal. Thus, these causal relationships are properly conceptualized over a period of time, perhaps quite a long period of time. They are "long memoried." Moreover, these slowly developing causal relationships may alter in fundamental ways over this period. Thus, we commonly speak of threshold effects, increasing returns, reactive sequences, and a wide variety of nonlinear relationships.⁷⁹ Arguably, the most important and well-known institutional causes have the fewest immediate effects and the greatest long-term (and perhaps nonlinear) effects.

These observations are commonplace among historical institutionalists, where time is reckoned in decades, centuries, or millennia and

⁷⁸ Temporal delays and nonlinear causal relationships are possible only if one moves beyond the realm of interests, opinions, and coercion *tout court*. For example, if a person does not fully realize his or her true interests then some delay between cause and effect might be anticipated.

⁷⁹ Paul Pierson, *Politics in Time: History, Institutions, and Social Analysis* (Princeton: Princeton University Press, 2004).

where nonlinear causal relationships are more or less assumed.⁸⁰ However, a complex approach to time is only beginning to gain adherents among scholars who work with large-N data sets.⁸¹ Here, explanatory factors are usually considered as contemporaneous events or as lagged (but still recent and generally linear) events. To be sure, cross-country regression analyses have begun to introduce secular-historical variables into the mix. It is now common to employ variables that measure a country's colonial history, climate, and geography.⁸² By usual construction, these are static variables. But it is not self-evident that their effects are static through time; that is, it may be important how long a country was colonized or how long ago. While this is probably not a meaningful question in the context of geographic variables, it is certainly a meaningful question with respect to most humanly created institutions.

In this article, we have proposed a particular way of thinking about the causal effect of social institutions. The key insight is that institutions sometimes have cumulative effects, effects that are noticeable only if an institution's history is brought into view. Our analysis of the relationship between democracy and growth suggests that the significant feature of this relationship is the accumulated history of the variable in question. In this initial study we have employed a simple depreciated stock measure to capture a country's democratic history. More nuanced views of the ways in which political experience aggregates over time clearly exist and warrant investigation. Nonetheless, we hope our results are sufficient to establish the viability of a cumulative conceptualization of institutional causes. Since institutions are by definition enduring and since the effects of institutions are, almost by definition, constitutional—in the sense of altering the perceptions and behavior of many actors at a systemic level—a wide array of causal factors might be reconceptualized in this fashion.

⁸⁰James Mahoney and Dietrich Rueschemeyer, eds., *Comparative Historical Analysis in the Social Science* (Cambridge: Cambridge University Press, 2003); Sven Steinmo, Kathleen Thelen, and Frank Longstreth, eds., *Structuring Politics: Historical Institutionalism in Comparative Analysis* (Cambridge: Cambridge University Press, 1992); David Waldner, *State Building and Late Development* (Ithaca, N.Y.: Cornell University Press, 1999).

⁸¹Pierson (fn. 79), 97–99.

⁸²Daron Acemoglu, Simon Johnson, and James A. Robinson, "Colonial Origins of Comparative Development: An Empirical Investigation," *American Economic Review* 91 (December 2001); John Luke Gallup and Jeffrey D. Sachs, "Geography and Economic Development," CID Working Paper no. 1 (March 1999); Rafael La Porta, Florencio Lopez-de-Silanes, Andrei Shleifer, and Robert W. Vishny, "The Quality of Government," *Journal of Economics, Law and Organization* 15 (March 1999).

APPENDIX: REVIEW OF ALTERNATIVE APPROACHES

In the body of the article we defend a particular method for testing the democracy/growth relationship in a time-series cross-national format. The approach relies on annual data, fixed effects, and an AR1 correction for serial autocorrelation. (Descriptive statistics for all variables used in the foregoing regression tests are found in Table 4.) We show in the appendix that our results are robust even when various elements of this methodology are altered: fixed effects versus random effects, annual versus five-year increments of data, the possible influence of OECD cases (tested this time in a random-effects format), a lagged-dependent-variable approach to modeling serial autocorrelation, the possible peculiarities of the democracy stock variable, and a wide variety of static (time-invariant) control variables, as displayed in Table 5.

For each model with annual data we have followed our usual approach of measuring all independent variables in the year prior to the dependent variable. For each model with five-year increments we have maintained the same approach, this time measuring the independent variables in the first year of the period under study. The dependent variable in this case is a five-year average of growth performance during the subsequent period. So in both cases the dependent variable is forward-lagged one time period.

The standard method of correcting for autocorrelation is employed where annual data are used (AR1 error correction), but not when five-year increments are used. (By virtue of the fact that we are dealing with five-year increments, it should be less of a problem.) No correction for serial autocorrelation is usually necessary when a lagged dependent variable is included, as in models 8 and 9, and none is employed.

Model 1 is a fixed-effects model with one control (GDPpc) and growth data aggregated across five-year periods. Model 2 is a random-effects model with annual data and the same all-purpose control. Model 3 is a non-fixed-effects model with annual data that includes all large-N controls employed previously (see Table 2), plus some additional static controls, intended to model spatial heterogeneity. These include English legal origin (dummy),⁸³ Muslims (as percentage of the population), ethnic fractionalization (the likelihood that two persons chosen randomly from a population will share the same ethnicity),⁸⁴

⁸³ La Porta et al. (fn. 82).

⁸⁴ Alberto Alesina, Arnaud Devleeschauwer, William Easterly, Sergio Kurlat, and Romain Wacziarg, "Fractionalization," *Journal of Economic Growth* 8, no. 2 (2003).

East Asia (dummy), Middle East (dummy), Latin America (dummy), and latitude (logarithm of the absolute value of the distance of a country's capital city from the equator; calculated by authors).

Model 4 is a non-fixed-effects model with five-year data increments and the same set of controls. Model 5 is a fixed-effects model with five-year increments and all relevant (varying) controls. Models 6 and 7 replicate models 3 and 4, this time excluding OECD cases. Models 8 and 9 test the lagged-dependent variable approach to TSCS analysis, discussed previously. Models 10 and 11 test another version of the political capital thesis. Instead of measuring democracy stock since 1900, we instead employ a moving sum variable that adds up democracy scores (from the same PolityIV data set) over twenty-year and fifty-year intervals. In model 12 we test the benchmark specification in an Arellano-Bond format. This procedure combines first-differencing with a series of lags—equivalent to the total number of prior observations in the data set—for each variable in the model.⁸⁵ (Simple first-difference models, without lagged instruments, show results similar to those in model 12.)

In each of these various tests we find that the democracy stock variable retains statistical significance, usually at the .01 level (two-tailed tests).

⁸⁵ Manuel Arellano and Stephen Bond, "Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations," *Review of Economic Studies* 58 (April 1991).

TABLE 4
DESCRIPTIVE STATISTICS AND CORRELATION TABLE FOR KEY VARIABLES

| Variable | <i>Descriptive Statistics</i> | | | | | | |
|----------------------------|-------------------------------|-----------|-------|--------|-----------|---------|--------|
| | Sample Period | Countries | Obs | Mean | Std. Dev. | Min | Max |
| Democracy level | 1950/2000 | 202 | 8521 | -0.53 | 7.47 | -10.00 | 10.00 |
| Democracy stock (1900-) | 1950/2000 | 202 | 8521 | -48.90 | 246.39 | -604.64 | 637.63 |
| Growth pc | 1951/2000 | 194 | 6872 | 1.82 | 6.60 | -52.10 | 138.90 |
| Growth pc (trade-weighted) | 1951/2000 | 189 | 7052 | 2.19 | 1.75 | -10.56 | 10.05 |
| GDP pc (ln) | 1950/2000 | 189 | 6880 | 7.45 | 1.52 | 3.88 | 10.94 |
| Inflation (ln) | 1961/2000 | 161 | 4629 | 1.94 | 1.44 | -5.21 | 10.08 |
| Investment | 1950/2000 | 170 | 5837 | 16.74 | 9.83 | -3.60 | 60.07 |
| Instability (Banks) | 1950/1999 | 194 | 6734 | -0.08 | 4.25 | -2.21 | 68.50 |
| Trade openness | 1950/2000 | 170 | 5840 | 65.69 | 45.39 | 3.15 | 473.86 |
| Life Expectancy | 1960/2000 | 195 | 7130 | 60.92 | 11.88 | 31.22 | 81.07 |
| Oil shock | 1950/2000 | 224 | 11424 | 0.53 | 0.50 | 0.00 | 1.00 |
| Population growth | 1951/2000 | 200 | 8341 | 0.02 | 0.02 | -0.44 | 0.22 |
| Years independent | 1950/2000 | 221 | 10973 | 46.32 | 66.68 | 0.00 | 453.00 |
| Regime durability | 1950/2000 | 164 | 6493 | 20.95 | 23.41 | 0.00 | 100.00 |
| Social conflict | 1955/1998 | 187 | 6193 | 2.09 | 0.22 | 2.00 | 3.45 |
| Government consumption | 1950/2000 | 170 | 5837 | 18.99 | 12.14 | 1.29 | 116.89 |
| Illiteracy (ln) | 1950/2000 | 176 | 7076 | 2.59 | 1.71 | -1.61 | 4.61 |

Correlation Table

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 |
|-------------------------------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|------|------|
| 1. Democracy level | 1.00 | | | | | | | | | | | | | | | |
| 2. Democracy stock (1900-) | 0.70 | 1.00 | | | | | | | | | | | | | | |
| 3. Growth pc | 0.05 | 0.09 | 1.00 | | | | | | | | | | | | | |
| 4. Growth pc (trade-weighted) | -0.03 | 0.09 | 0.23 | 1.00 | | | | | | | | | | | | |
| 5. GDP pc (ln) | 0.52 | 0.57 | 0.10 | -0.02 | 1.00 | | | | | | | | | | | |
| 6. Inflation (ln) | -0.05 | -0.17 | -0.19 | -0.24 | -0.19 | 1.00 | | | | | | | | | | |
| 7. Investment | 0.36 | 0.37 | 0.22 | 0.03 | 0.62 | -0.13 | 1.00 | | | | | | | | | |
| 8. Instability (Banks) | 0.04 | -0.01 | -0.09 | 0.01 | -0.03 | 0.16 | -0.01 | 1.00 | | | | | | | | |
| 9. Trade openness | 0.16 | 0.09 | 0.11 | -0.06 | 0.26 | -0.14 | 0.22 | -0.24 | 1.00 | | | | | | | |
| 10. Life Expectancy | 0.47 | 0.31 | 0.07 | -0.13 | 0.80 | -0.09 | 0.57 | 0.00 | 0.28 | 1.00 | | | | | | |
| 11. Oil shock | 0.13 | -0.01 | -0.11 | -0.30 | 0.09 | 0.25 | -0.10 | -0.08 | 0.26 | 0.29 | 1.00 | | | | | |
| 12. Population growth | -0.29 | -0.17 | -0.06 | 0.10 | -0.27 | 0.00 | -0.21 | -0.02 | -0.14 | -0.34 | -0.13 | 1.00 | | | | |
| 13. Years independent | 0.32 | 0.28 | 0.02 | 0.01 | 0.33 | 0.05 | 0.25 | 0.20 | -0.31 | 0.30 | 0.13 | -0.15 | 1.00 | | | |
| 14. Regime durability | 0.30 | 0.53 | 0.05 | 0.02 | 0.56 | -0.22 | 0.34 | -0.12 | 0.12 | 0.46 | 0.11 | -0.17 | 0.28 | 1.00 | | |
| 15. Social conflict | -0.05 | -0.07 | -0.10 | 0.00 | -0.22 | 0.19 | -0.12 | 0.35 | -0.20 | -0.13 | 0.11 | 0.04 | 0.04 | -0.09 | 1.00 | |
| 16. Government consumption | -0.13 | -0.20 | -0.08 | -0.08 | -0.22 | 0.16 | -0.24 | -0.05 | 0.20 | -0.12 | 0.30 | 0.04 | -0.22 | -0.19 | 0.08 | 1.00 |
| 17. Illiteracy (ln) | -0.48 | -0.41 | -0.04 | 0.16 | -0.74 | 0.05 | -0.49 | 0.05 | -0.19 | -0.75 | -0.17 | 0.49 | -0.25 | -0.52 | 0.17 | 0.22 |

TABLE 5
ALTERNATIVE ESTIMATORS AND MODELS^a

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 |
|--------------------------------|----------------------------|----------------------------|---------------------------|---------------------------|----------------------------|----------------------------|---------------------------|----------------------------|----------------------------|---------------------------|---------------------------|----------------------------|
| <i>AR correction</i> | <i>AR(0)</i> | <i>AR(1)</i> | <i>AR(1)</i> | <i>AR(0)</i> | <i>AR(0)</i> | <i>AR(1)</i> | <i>AR(1)</i> | <i>AR(0)</i> | <i>AR(0)</i> | <i>AR(1)</i> | <i>AR(1)</i> | <i>AR(1)</i> |
| <i>Frequency</i> | 5 years | 1 year | 1 year | 5 years | 1 year | 1 year | 5 years | 1 year | 1 year | 1 year | 1 year | 1 year |
| <i>Sample:</i> | All | All | All | All | nonOECD | nonOECD | nonOECD | All | All | All | All | All |
| <i>Country fixed effects:</i> | Yes | No | No | No | Yes | No | No | Yes | Yes | Yes | Yes | No |
| Democracy stock (1900–) | 0.009*** (0.002) | 0.002*** (0.000) | 0.002** (0.001) | 0.002** (0.001) | 0.012*** (0.002) | 0.003*** (0.001) | 0.003** (0.001) | 0.006*** (0.001) | 0.008*** (0.002) | 0.005** (0.002) | 0.005** (0.002) | 0.034*** (0.002) |
| Democracy stock (20-year ave.) | | | | | | | | | | | | |
| Democracy stock (50-year ave.) | | | | | | | | | | | | |
| Growth pc (lagged dep var) | | | | | | | | | | | | |
| GDP pc (ln) (WDI) | -4.581*** (0.864) | 0.096 (0.077) | -0.544** (0.212) | -0.632*** (0.230) | -4.592*** (0.662) | -0.556** (0.241) | -0.690*** (0.266) | -3.053*** (0.423) | 0.151*** (0.031) | -2.885*** (0.591) | -3.096*** (0.900) | -20.960*** (0.421) |
| Inflation (ln) (WDI) | | | -0.250*** (0.085) | 0.006 (0.080) | -0.185* (0.101) | -0.230** (0.099) | -0.001 (0.088) | | | | | |
| Investment (PWT) | | | 0.004 (0.015) | -0.013 (0.015) | -0.008 (0.023) | 0.001 (0.018) | -0.006 (0.019) | | | | | |
| Govt consumption (PWT) | | | -0.037*** (0.013) | -0.030** (0.014) | -0.006 (0.024) | -0.035** (0.015) | -0.029* (0.016) | | | | | |
| Trade openness (PWT) | | | 0.010*** (0.003) | 0.010*** (0.003) | 0.021** (0.009) | 0.009*** (0.004) | 0.007* (0.004) | | | | | |
| Population growth (WDI) | | | -24.731 (15.804) | -46.596*** (15.445) | -33.207* (18.896) | -20.794 (17.533) | -44.483** (18.894) | 15.296 (16.500) | | | | |
| Instability (Banks) | | | -0.078*** (0.021) | -0.001 (0.021) | -0.007 (0.023) | -0.101*** (0.027) | -0.007 (0.026) | | | | | |

| | | | | | | |
|---------------------------------|-----------|-----------|----------|----------|----------|-----------|
| Social conflict (Marshall) | 0.045 | 0.217 | -0.177 | 0.042 | 0.047 | -0.520 |
| | (0.435) | (0.464) | (0.607) | (0.499) | (0.541) | (0.607) |
| Years independent | 0.000 | -0.000 | 0.103 | 0.001 | 0.001 | -0.071 |
| | (0.002) | (0.002) | (0.097) | (0.003) | (0.003) | (0.229) |
| Regime durability (Polity IV) | 0.013** | -0.013** | -0.013 | -0.016* | -0.019** | 0.004 |
| | (0.006) | (0.006) | (0.010) | (0.009) | (0.009) | (0.010) |
| Life expectancy (WDI) | 0.092*** | 0.093*** | 0.145*** | 0.074*** | 0.075** | 0.167*** |
| | (0.024) | (0.025) | (0.051) | (0.029) | (0.030) | (0.053) |
| Illiteracy (ln) (WDI) | 0.312** | 0.091 | 0.545 | 0.177 | -0.174 | 0.634 |
| | (0.146) | (0.150) | (0.531) | (0.221) | (0.218) | (0.604) |
| Trend | -0.016 | -0.026 | -0.117 | -0.010 | -0.026 | 0.062 |
| | (0.016) | (0.019) | (0.102) | (0.021) | (0.025) | (0.230) |
| Oil shock (dummy) | -1.387*** | -1.171*** | -0.464 | -1.186** | -0.995* | -0.973*** |
| | (0.370) | (0.432) | (0.388) | (0.493) | (0.598) | (0.350) |
| English legal origin (La Porta) | 0.161 | 0.131 | | 0.143 | 0.025 | |
| | (0.230) | (0.265) | | (0.317) | (0.359) | |
| Muslims (% pop) | -0.014*** | -0.009 | | -0.014** | -0.007 | |
| | (0.005) | (0.006) | | (0.006) | (0.007) | |
| Ethnic fractionaliz. (Alesina) | -0.761 | -0.547 | | -0.546 | -0.661 | |
| | (0.499) | (0.562) | | (0.705) | (0.785) | |
| East Asia (dummy) | 2.624*** | 2.919*** | | 2.862*** | 3.111*** | |
| | (0.409) | (0.477) | | (0.600) | (0.688) | |
| Middle East (dummy) | 1.305*** | 1.181** | | 1.653*** | 1.395** | |
| | (0.488) | (0.565) | | (0.565) | (0.659) | |
| Latin America (dummy) | -0.409 | -0.565 | | -0.330 | -0.783 | |
| | (0.333) | (0.362) | | (0.493) | (0.518) | |
| Latitude (ln) | 0.248 | 0.143 | | 0.211 | 0.073 | |
| | (0.168) | (0.195) | | (0.191) | (0.234) | |

TABLE 5 (cont.)

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 |
|--------------------|----------------------|-------------------|------------------|------------------|----------------------|------------------|--------------------|----------------------|----------------------|----------------------|----------------------|---------------------|
| Constant | 35.797*** (6.457) | 1.073* (0.583) | 2.854 (1.937) | 3.522 (2.157) | 21.313*** (8.061) | 3.697 (2.654) | 6.038** (2.921) | 24.082*** (3.171) | 27.400** (13.951) | 23.784*** (5.332) | 26.555*** (7.253) | 0.298*** (0.010) |
| Observations | 1082 | 6264 | 3231 | 601 | 615 | 2335 | 435 | 6136 | 3302 | 4657 | 2851 | 5954 |
| Countries | 179 | 180 | 128 | 119 | 121 | 101 | 92 | 180 | 130 | 176 | 95 | 178 |
| Sample Period | 1960–95 | 1950–00 | 1961–98 | 1965–95 | 1965–95 | 1961–98 | 1965–95 | 1951–00 | 1961–98 | 1950–00 | 1950–00 | 1950–00 |
| R squared (within) | 0.18 | 0.01 | 0.11 | 0.28 | 0.24 | 0.09 | 0.25 | 0.11 | 0.11 | 0.03 | 0.03 | |
| Prob > F | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0001 | 0.0027 | 0.0000 |
| Sargan test (prob) | | | | | | | | | | | | |

*** p < .01 ** p < .05 * p < .10 (two-tailed tests)

^aAll regressions are OLS (Newey–West standard errors in parentheses) except for model 12, which is estimated using the Arellano–Bond estimator. Corrections for autocorrelation in the residual and inclusion of country fixed effects are included as noted. Dependent variable: growth rates, either annually or in five-year increments (mean). All predictors lagged one year. Variables and procedures defined in the text.