Size and Democracy, Revisited

John Gerring
<jgerring@bu.edu>
(corresponding author)

Dominic Zarecki
<dominicz@bu.edu>

Boston University
Department of Political Science
232 Bay State Road
Boston MA 02215

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The smaller the society, the fewer probably will be the distinct parties and interests composing it; the fewer the distinct parties and interests, the more frequently will a majority be found of the same party; and the smaller the number of individuals composing a majority, and the smaller the compass within which they are placed, the more easily will they concert and execute their plans of oppression. Extend the sphere, and you take in a greater variety of parties and interests; you make it less probable that a majority of the whole will have a common motive to invade the rights of other citizens; or if such a common motive exists, it will be more difficult for all who feel it to discover their own strength, and to act in unison with each other. Besides other impediments, it may be remarked that, where there is a consciousness of unjust or dishonorable purposes, communication is always checked by distrust in proportion to the number whose concurrence is necessary.

- James Madison, *Federalist #10*

The consensus view among most academics and lay commentators is that (*pace* Madison) size is inversely correlated with democracy. Polities are more likely to maintain a democratic form of rule insofar as they are small. We shall refer to this as the classical view since it has an ancient pedigree and is also well-grounded in contemporary social science.[1]

Exemplars for the classical perspective are found in the city-states of ancient Greece and Renaissance Italy, on the one hand, and empires from time immemorial on the other. The first set of polities is small and highly democratic (at least by the standards of the day), while the second set is large and makes no pretension to democracy. This seems to offer strong confirmation of the thesis that, when it comes to democracy, small is beautiful. Likewise, the transition from republic to empire, understood as an enlargement of territory and incorporation of diverse peoples, is usually understood as a loss of popular control over the apparatus of governance. The Roman example remains at the forefront of modern thinking.

By a similar logic, the decentralization of power within a polity is usually thought to enhance prospects for democracy (Diamond, Tsalik 1999). Indeed, some believe that a polity in which the center is constrained and localities enjoy a large measure of self-government is inherently more democratic than one where authority is highly centralized. Switzerland (perhaps the extreme example of decentralization in the contemporary era) is more democratic than France or Britain (two nation-states with a high level of centralization through most of the 20th century). Hence, the familiar equation of democracy with power that is diffused to the grass-roots, where it can be exercised by local communities. Colomer (2007: 64) summarizes: "While dictatorship affirms itself by means of concentrating power in the center, resistance to authoritarian rule always involves more transparent and accountable local self-government."

Arguably, community identity and consensus are easier to construct, and social order easier to maintain, when communities are small (Dahl, Tufte 1973; Ott 2000: 98). The larger the polity the greater the potential number of conflicts, each of which poses coordination problems and, in the extreme, disrupts the peace. This does not bode well for democracy (Colomer 2007: 58). Likewise, it could be that smaller polities offer greater opportunities for exit, an option that may constrain the rapacity of elites (Rogowski 1998; Tiebout 1956). Small polities are presumably more transparent (Farrugia 1993), a factor often considered integral to democracy.

These are the various strands of the classical argument, and they are certainly correct in some respects. Over the broad sweep of history, population appears to be inversely correlated with democracy (Colomer 2007: ch 8), and especially with the participatory component of democracy (Alesina, La Ferrara 2000; Anckar 2004; Costa, Kahn 2003; Oliver 2000; Remmer 2010; Weldon 2006).

In the modern era, however, we shall argue that larger populations foster greater electoral democracy, understood across a number of empirical dimensions. Before continuing, several clarifications are in order.

Historically, one can expect that there was a strong correlation between the land area of a polity and its population size, for ancient modes of production did not allow for dense concentrations of human settlement. Thus, when Aristotle or Plato spoke of large and small states it was not necessary to clarify whether they were talking about area or population. In the modern era, however, these concepts have become highly differentiated;

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[1] The intellectual history, stretching back to Plato, Aristotle, Montesquieu, and Rousseau, is reviewed in Dahl, Tufte (1973: ch 1). See also Alesina, La Ferrara (2000), Almond (1956), Anckar (2002, 2004), Anckar and Anckar (1995), Colomer (2007), Dahl (1971: 109-11), Diamond, Tsalik (1999), Hadenius (1992: 61-2, 122-7), Lijphart (1977: 65), Ott (2000), Stasavage (2010), and work reviewed in Table 1 (below).

indeed, the correlation between the two is exceedingly modest (Pearson's r=.54). Our argument is exclusively about population, not land area, so work focused on the causes or effects of small/large territories (e.g., Green 2010; Knack, Azfar 2003; Olsson, Hansson 2010) is only peripherally relevant.

Democracy means many things, so it would not be surprising if the relationship between size and democracy has varying effects on different aspects of this multivalent concept. Our theory focuses on the *electoral* (aka competitive, contestation, minimal, Schumpeterian) dimension of democracy, that is, the degree of electoral competition found in a polity. It does not embrace other dimensions – egalitarian, participatory, majoritarian, liberal, deliberative, and so forth (Coppedge, Gerring 2011).

A final clarification concerns the scope-conditions of the argument. We are primarily interested in the modern era of human history – roughly, the past two centuries, sometimes labeled the democratic era. Prior to that, limited communication and transportation networks (Stasavage 2010), as well as limited experience with the representative principle, meant that the relationship between size and democracy was probably quite different.

The paper proceeds as follows. In the first section, we explore the theory, including a discussion of several illustrative regions of the world and possible causal mechanisms. In the second section, we interrogate the issue empirically, beginning with crossnational evidence and continuing with evidence drawn from a single polity that exemplifies a good deal of within-case variation: the United States. The final section summarizes the conclusions and discusses the strengths and weaknesses of the evidence.

Theory

Of the world's twelve most populous countries – China, India, United States, Indonesia, Brazil, Pakistan, Nigeria, Bangladesh, Russia, Japan, Mexico, Philippines – all but one now feature multi-party elections and most have a healthy level of political competition. By contrast, among the world's twelve smallest non-island polities – San Marino, Monaco, Liechtenstein, Suriname, Equatorial Guinea, Luxembourg, Bhutan, Guyana, Bahrain, Djibouti, East Timor, Swaziland – the situation is mixed. (We address the question of island states below.) Most are strongly democratic, but authoritarian rulers in Equatorial Guinea, Bahrain, Djibouti, and Swaziland have resisted the move to openly contested multi-party elections. A quick glance at the extremes of the size spectrum in the early twenty-first century thus provides some fodder for the argument.

We might also consider the matter from the perspective of the outcome variable, regime-type. While the list of highly democratic countries is too long to easily peruse, the list of extremely autocratic countries, as identified by the Polity IV dataset (Polity2 < -7), is short. As of 2003, it included Afghanistan, Bhutan, Iraq, North Korea, Oman, Qatar, Saudi Arabia, Swaziland, Turkmenistan, United Arab Emirates, Uzbekistan, and Vatican City. While Afghanistan, Iraq and North Korea are mid-sized, the rest are quite small by international standards, offering further corroboration for our theory.

Of course, this represents but one snapshot in time, and there are many potential confounders. We turn now to several historical examples that will illustrate the nature of our argument and the sort of variation (real and counterfactual) we are interested in explaining.

In South Asia, India is by far the largest of the region's post-1947 states, and also one of the most democratic. We conjecture that this is not accidental. We conjecture, further, that there would be an even higher overall level of democracy in the South Asian region if more of the territories within British India had remained within a single political unit after 1947, rather than fragmenting into the current states of Bangladesh, Bhutan, India, Myanmar (Burma), Nepal, Pakistan, and Sri Lanka. Likewise, we anticipate that there would be a lower level of democracy within the current borders of India if that country were to have fragmented further – e.g., if each of the Indian states had become a fully sovereign state, or if the current federal state were to have devolved into a much looser confederation (a la the European Union).

Similar arguments can be made with respect to the colonial and post-colonial history of the New World. The United States has been more democratic than Latin American polities over most of the past two centuries, a fact that may be related to its greater numbers.[2] Likewise, we conjecture that if the Union had failed, and the colonies had developed into separate states – e.g., fifty independent nation-states or one Northern and one

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[2] Of course, many additional factors served to bolster democracy in the North and authoritarian rule in the South (Sokoloff, Engerman 2000).

Southern nation-state – or if the US had developed along the highly decentralized lines set forth in the original Articles of Confederation, the result would have been a lower level of democracy overall. Similarly, had the Union government managed to impose greater centralized control – a continuation of Reconstruction – the end-result would have been a higher level of democracy than was in fact enjoyed in the post-Civil War era.

In Central and North Asia, our argument appears to founder, as most states have been autocratic over the past several centuries, led by Russia/USSR. However, the eventual break-up of the Soviet Union confirms a central tenet of our argument: large polities are at pains to hold together unless they are democratically constructed. Note also that in the post-Soviet era, Russia – by far the largest of the remaining states – is more democratic than most of its smaller neighbors (excepting those within the orbit of the European Union). Likewise, we speculate that the overall level of democracy in the region would be higher today if more countries had elected to stay within the re-founded nation of Russia, and the area within Russia today would be less democratic overall if more regions had defected (or been allowed to defect).

East and Southeast Asia might also appear to form an exception to our general argument, given that the largest country (by far) is persistently autocratic. Even so, most of the leading democratizers are sizeable in population (e.g., Indonesia, Japan, Malaysia, Philippines, South Korea, Taiwan, Thailand), while many of the least democratic countries are relatively small (e.g., Brunei, Cambodia, Laos, North Korea, Singapore). Our counterfactual hypothesis is that greater regional integration under the auspices of China would have led to a more democratic China, for there is no other mechanism that we can imagine that would induce other states to participate in such a hegemonic union. Such a scenario, if correct, would have led to a higher level of democracy overall in the region. Of course, another counterfactual is conceivable. The regional hegemon – in this case, China – might conquer adjacent countries. Our presumption, however, is that in the modern era conquest over foreign territories with diverse peoples (relative to the core) is difficult to achieve and even more difficult to maintain *unless* democratic freedoms are granted. Consider that any scenario in which Taiwan re-joins the mainland is likely to be accompanied by democratic guarantees for the former (as in Hong Kong) and/or a thorough democratization of the latter. Thus, we view the "authoritarian over-rule" scenario as unlikely.

This review of cases has been superficial, to say the least, and we have said nothing about sub-Saharan Africa. Our purpose, however, is not to present empirical evidence for the theory (postponed until the second part of the paper) but rather to clarify what the theory means, in concrete terms (including relevant causal counterfactuals), and to make a prima facie case for its plausibility. We proceed now to a discussion of causal mechanisms.

Mechanisms

Why might population size foster electoral democracy in the modern era? The short answer is that greater size makes it more difficult for a single faction to successfully monopolize power, as Madison perceived (see also Horowitz 1985: 37-8; Reilly 2000). A more long-winded answer, to which we now turn, encompasses four interconnected causal mechanisms: (a) democratic infrastructure, (b) institutionalization of power, (c) dispersal of power, and (d) conflict mitigation.[3]

Democratic Infrastructure.   Democracy, even in our minimal definition of the term, requires a wealth of supporting institutions, within the state apparatus and beyond. These include a bureaucracy and a cadre of civil servants, a court system and a cadre of professional lawyers, a media system and a cadre of reporters and editors, as well as nongovernmental organizations (NGOs) representing diverse interests and objectives. A large country, even though impoverished, is generally more capable of developing and sustaining this infrastructure than a small country. For example, in a very small country one is lucky to have a single well-staffed media outlet or NGO. By contrast, in a large country one is likely to find numerous media outlets and NGOs, some headquartered in the capital city and others spread across the country. This bodes well for democracy.

Likewise, in a large country there will be a large elite class, i.e., many people with the education and experience that would qualify them to fill leadership positions in government, in administration, and in civil society. Moreover, we can anticipate that many of these elites will find public service in their country of origin to be a rewarding activity. Note that a political career generally offers greater rewards – greater power, status, and salary – in a large polity than in a small polity. It is more rewarding to serve as head of a large country than a small country; more rewarding to serve as a bureaucrat in a large state than a small state; and so forth. Would-be public servants

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[3] Some of these points are touched upon by Srebrnik (2004: 334-9).

thus face different incentives, depending upon the size of the polity. It seems probable, therefore, that high-caliber people are more likely to choose a life of public service in a polity where their talents will be amply rewarded than in one where they will be meagerly remunerated and will receive little acclaim from the outside world. In small polities, talented elites may prefer to pursue careers in the private sector or in international organizations (e.g., the World Bank or the United Nations), where their horizons are not bounded by national borders. Opportunity costs thus suggest a significant political brain drain in the political life of small communities relative to large communities (Farrugia 1993: 225; Wood 1967). For elites in a small country, the world is a stage; for elites in a populous country, the country provides a stage that is sufficiently large and important to demonstrate their prowess.

Consequently, a large country is likely to attract many well-qualified applicants for top political jobs. By contrast, in small polities there may exist only a small number of viable contestants for top offices. Insofar as greater competition leads to higher quality, we can expect higher-quality leaders in governmental and nongovernmental positions. And insofar as the advantages of incumbency are counterbalanced by high-quality challengers (Mann, Wolfinger 1980) we can expect greater turnover in polities where there are plenty of high-quality challengers.

Institutionalization of Power. The size of a polity affects the likelihood that institutionalized procedures of rule and an overall ethic of legal-bureaucratic rationality will develop. Indeed, the larger the state the harder it will be to run political affairs in a personalized, ad hoc fashion.

In many small countries Farrugia (1993: 221) observes a "social ecology composed of a closely knit, integrated but open community with highly personalized relationships," where each member of the political elite is compelled to take on numerous and diverse tasks and where decisionmakers find it difficult to resist pressures from those they are intimately connected to. Along these lines, Benedict (1967: 47-9, quoted in Ott 2000: 39) comments:

> It is commonplace in anthropological studies of small communities that economic, political, religious and kinship systems are very often coincident or nearly so. This has important implications for economic and social development for it means that decisions and choices of individuals are influenced by their relations in many contexts with other individuals. It becomes difficult to remove an inefficient employee on grounds of inefficiency alone because he is attached to his employer by kinship and political ties. Impersonal standards of efficiency, performance, and integrity are modified by the myriad relationships connecting the individuals concerned.[4]

For this reason, a certain type of corruption is common in small communities. Arthur Lewis (1965: 9; see also Parsons 1951: 508) comments,

> In a small island of 50,000 or 100,000 people, dominated by a single political party, it is very difficult to prevent political abuse. Everybody depends on the government for something, however small, so most are reluctant to offend it. The civil servants live in fear; the police avoid unpleasantness, the trade unions are tied to the party, the newspaper depends on government advertisements, and so on.

Arguably, personal ties among members of a close-knit political community encourage a clientelistic style of politics and policymaking, one ill-suited to the development of electoral democracy.[5]

By contrast, in a large polity one expects to find greater differentiation among institutions, greater professionalism among their staff, and the spread of impersonal (universal) norms across elite groups. This is partly a response to the sheer size of the elite, in which individuals come to play highly specialized roles. But it is also, perhaps more importantly, a response to the task of maintaining authority over a large population. Note that in small polities credible commitments across key players can be secured through informal arrangements. If elite families representing different sectors of a society inter-marry this may obviate the need for formal, written constitutions. By contrast, in a large polity intermarriage and other informal methods are unlikely to be sufficient to achieve and maintain bargains across sectors, groups, and classes. They will need to be supplemented by a constitutionalized structure of power, sufficient to secure credible commitment.

Likewise, the coordination tasks implicit in any polity (Hardin 1999) become more difficult to handle effectively by authoritarian fiat as the number of players and perspectives increases. Coordination problems are

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[4] See also Peters (1992: 100).

[5] Olsson, Hansson (2010) find a negative contemporaneous correlation between the land area of countries and the World Bank measure of rule of law; however, the relationship between population and rule of law is not significant when land area is included in the model. In any case, there are reasons to be suspicious of this particular measure of rule of law, which are tilted toward polls with business respondents and may say more about the overall level of economic performance than about rule of law per se. Similarly, Knack, Azfar (2003) show that there is no statistically significant relationship between corruption and population size.

relatively simple in a game with few actors and a narrow range of perspectives. Here, one can imagine resolving political conflict by formula, fiat, or elite-level agreement. Deliberation may be informal, a "sense of the community." Just as families and small organizations may dispense with formal democratic procedures as redundant so may small nation-states. By contrast, in a game with many players and very diverse interests, coordination is more difficult to achieve and less stable, even when achieved. In large – and presumably diverse – polities, institutionalized procedures are difficult to dispense with.

As a general rule, institutionalization is more likely, and more necessary (from a functional perspective), the larger the group of people whose activities and interests must be coordinated. This is especially evident in the development of political parties. While small polities can be governed informally without the development of political parties, larger polities will require more formal procedures for the aggregation of interests and the selection of candidates (Anckar, Anckar 2000). Historically, the growth of parties is linked to the expansion of the electorate in countries with restrictive suffrage laws, reinforcing the notion that mass parties are dependent upon the existence of large constituencies (Cox 1987). There may not be an iron law of oligarchy, but there is almost certainly an iron law of institutionalization. And insofar as the institutionalization of power – aka the rule of law – favors democratic mechanisms of leadership selection this serves as another causal mechanism in our story.

Dispersal of Power. Large polities tend to disperse power among a larger number of institutions, whether functionally or territorially defined. Small polities tend toward a greater concentration of power in a single actor or institution. This tendency is most visible in formal constitutional structures like *federalism*, which seeks to decentralize power between national and subnational units. Not surprisingly, federalism is robustly associated with population size (logarithm) in a global sample of countries.

We do not claim that this, by itself, makes large polities more democratic (as it might seem from the perspective of the liberal model of democracy). Indeed, the relevant counterfactual comparison is between (a) a large, decentralized polity (e.g., India or the United States) and (b) a series of smaller, more centralized polities (e.g., the states of India or the US reconstituted as sovereign political bodies). The latter arrangement is surely more decentralized and thus qualifies as more democratic according to one plausible construal of the liberal model of democracy. Our claim, however, is that the former is more likely to foster electoral democracy, for under circumstances of federalism it is more difficult for a single actor or institution to monopolize power and more difficult to resist societal pressures to adopt electoral institutions.

Why is it that larger polities tend to be run in a more decentralized manner? Partly, it is a matter of establishing *legitimacy.* The greater the size of a polity the harder it will be for a small clique to claim exclusive leadership over that country (Lewis 1965: 14; Peters 1992). Consider that a single faction may claim to represent the people of Bhutan. But it will be harder for any such faction to claim to represent the extremely numerous and diverse citizens of India. This fact augurs for a democratic form of government in the modern era.

Likewise, at mass levels a large country offers numerous platforms upon which opposition can form. Whether based on business, professional, labor, religious, civil, ethnic, caste, or regional ties, the more social groups there are, and the greater the dispersion of (conflicting) interests and values, the harder it will be for an authoritarian ruler to coopt the opposition, and the easier it will be for opposition leaders to find potential bases of support. This, too, fosters a dispersal of power.

Containing Conflict. A large country presumably contains many cleavages, providing fodder for numerous conflicts. These conflicts may be damaging to the prospect of democratic transition and, a fortiori, to the chances of democratic consolidation. Yet, we argue, following Madison, that conflicts in a large country may be less damaging to the polity than they would in a smaller polity.

Farrugia (1993: 223) observes,

> Small communities often develop strong social cohesion. Once the social unity is ruptured, however, the divisions that ensue run deep and take many years to heal. Minor issues which are easily absorbed in larger states, assume national dimensions in small states. Minor divisions that are allowed to spread eventually pervade other sectors of the population. In communities where practically everyone knows everyone else, where individuals' utterances and actions soon become public knowledge, it often happens that once a position is taken people find it difficult to retract. This is especially so in communities where the close personal interactions belie long-held suspicions or age-long feuds that exist between different groups, or among family clans.

The point is that social conflicts are destabilizing only where they engage a substantial portion of the citizenry. Where the population is large (and presumably diverse) cleavages are likely to be cross-cutting and individual

conflicts more likely to be local rather than national in scope. As such, they are more easily contained.

India's history offers a case in point. Rent by violent conflict between Hindus and Muslims, along with a Maoist insurrection in the countryside, India's democracy has held steady over the past six decades. At no point have these conflicts threatened to derail elections at a national level – though they have certainly upset the rule of law and electoral processes in certain states. This fulfills Madison's premise that the violence of faction might be calmed not by suppressing conflict but rather by enlarging the scope of the polity so that multiple conflicts, and multiple lines of cleavage, are embraced. It is also in line with a well-established theory of social conflict, harking back to Georg Simmel (1955) and Lewis Coser (1956), namely, that "the frequency of intra-group conflict is positively related to the size of a group while the *intensity* of conflict is negatively related to the size of the group" (Black 1974: 1245).

Empirics

Recent empirical work on our theoretical question consists mostly of crossnational regression analyses (panel or cross-sectional), summarized in Table 1. Outcome measures include various indicators of democracy (understood as binary, multichotomous, or continuous); binary outcomes may be bi-directional or unidirectional (i.e., democratic survival or democratic breakdown); and a variety of estimators and specifications have been employed. The reader will see that these studies generally confirm the classical narrative. Indeed, no extant study shows a positive and statistically significant relationship between population and democracy.[6]

Even so, there are reasons to be skeptical of this apparent consensus. Note that while the effect of size on economic outcomes has received extensive attention from scholars (e.g., Alesina, Spolaore 2003) its effect on electoral democracy has received very little attention.[7] Of the six studies reviewed in Table 1 only one is focused on our topic of interest (Ott 2000). Here, the empirical evidence is limited to cross-tabulations.

The remaining five studies are focused on other theoretical questions, as signaled by the final column in Table 1. This means that measures of population serve as controls and are not subjected to sustained inquiry. The chosen specifications and estimators, while perhaps appropriate for the author's purpose, may not be appropriate for testing our hypothesis.

A few issues are worth flagging at the outset. First, in specifying a model it is important to exclude covariates that may be endogenous to population. Indeed, many of the covariates usual to democratization studies threaten to introduce problems of endogeneity (back-door paths from Y to X). Second, it is important to employ a large sample of countries and observations, multiple operationalizations of the key variables, and a variety of estimators. No single approach can resolve these knotty questions, given the many threats to inference. Finally, in addition to the usual staple of crossnational data, within-country evidence should be exploited. Neither by itself is likely to be very convincing.

Of course, all empirical tests are likely to be flawed insofar as the treatment of interest is not randomly assigned.[8] Even so, observational evidence may be revealing – and is, in any case, probably our only means of exploring this empirical question since one can neither create polities de novo nor reassign persons across existing polities.

Our first set of analyses adopts the traditional crossnational regression format. The second exploits within-country comparisons across districts within the United States, in regression and matching formats. Problems of causal inference are addressed in each section and again in the conclusion.

Crossnational Tests

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[6] One study of communities within the United States shows that larger political units are less likely to approve referendums that discriminate against minority groups (Donovan, Bowler 1998). This is not included in Table 1 as it seems peripheral to the main theoretical question, and is limited to a single country.

[7] One study trains its focus on ethnic heterogeneity (Fish, Brooks 2004). However, the treatment is still rather brief and tests are limited to a cross-sectional analysis of a single year. (Findings are equivocal.) No recent extant study that we are aware of focuses on the role of population.

[8] One can imagine small-scale field experiments, but the external validity of such experiments remains open to question.

The reader will recall that our theory concerns a normative model of democracy known variously as electoral, competitive, contestation, minimal, or Schumpeterian. As an aggregate, this model of democracy is captured in two widely used crossnational indicators – the *Polity2* variable from the Polity IV dataset (Marshall, Jaggers 2007) and the *Political Rights* variable from the Freedom House (FH) dataset (www.freedomhouse.org). Since Polity2 has excellent historical coverage and also offers the most differentiated scale (-10 to +10), we employ this in our benchmark model. Missing data for micro-states is imputed from other sources.

Additional tests are conducted with the Political Rights variable from Freedom House. This variable is coded by integers from 1-7 and thus offers a more limited range of variation, as well as a somewhat shorter time-span (beginning in 1971). The variable is re-scaled (by multiplying by -1) so that it conforms to the direction of other outcome variables; that is, a higher score indicates a higher level of democracy.

Several categorical variables are constructed from the Polity2 variable by imposing breakpoints in the 21-point scale. Binary variables impose cutoffs at 5 (for most tests) and 0 (for a few tests). A trichotomous ordinal scale adopts cutoffs at -5 and 5.

Given the perils of highly aggregated measures of democracy (Coppedge, Gerring 2011) it seems wise to supplement composite indices with more specific indicators that attempt to capture some of the underlying components of electoral democracy. We include two such indicators.

Competition is a measure of electoral contestation, calculated as 100 minus the percent votes won by the largest party in presidential or parliamentary elections (or both, averaged) (Vanhanen 1990, 2000). Competitiveness (aka contestation) is central to the electoral model of democracy. Indeed, a lack of competition is often taken as a sign of a lack of democracy (Key 1949). Moreover, there is a strong empirical correlation in our crossnational dataset between this measure competition and our general measure of electoral democracy (Pearson's r = .81), evidence of convergent validity.

Turnover marks a change of top leaders of a country under regular circumstances, thus differentiating between irregular changes (e.g., by coup or revolution) and those that conform to constitutional law. Data is drawn from the Archigos dataset (Goemans et al. 2009). The simple fact of turnover in leadership is a feature that we expect to find in electoral democracies and which we generally expect to be more common in democracies than in autocracies. Indeed, some have argued that turnover in power is a definitional characteristic of democracy (Alvarez et al. 1996; Przeworski 2000).

Tests

Population is one of the few pieces of social-science data about which there is little disagreement over conceptualization. Of course, one can quibble about who counts as a member of a country (official citizens, residents, …). But this rarely has a large impact on aggregate statistics. Multiple sources of data are combined in order to provide reasonably full coverage of sovereign and semisovereign countries since 1800 (see Table A1).

Our initial sample includes all sovereign and semi-sovereign countries from 1820 to the present. Of course, sample size varies from model to model by reason of data availability; some tests are of necessity restricted to the postwar era.

Variation in the factors of interest (both on the left- and right-side of the equation) is primarily latitudinal rather than longitudinal, as population changes slowly and a country's democracy score may remain constant for long periods. Consequently, the following tests focus primarily on latitudinal rather than longitudinal comparisons, while utilizing all available historical evidence.

The benchmark model, Model 1 in Table 2, adopts an OLS estimator with Newey-West standard errors and a one-period lag structure (to capture autocorrelation).[9] Controls are minimal, including only variables which are exogenous relative to the factor of theoretical interest, for which a strong theoretical rationale can be offered,[10] and for which sufficient data is available throughout the time-period. These include GDP per capita (+), Island (+), Protestant (+), Muslim (-), a diffusion variable (+), and decade dummies. The hypothesized direction of causal effects is indicated in parentheses next to each variable. Definitions and sources are listed in Table A1.

One might wonder about the wisdom of Island as a covariate in an empirical test of population. A large majority of the world's microstates are islands, and there is a modest correlation between island and the logarithm

of population (Pearson's r=.36). Moreover, island states have a higher than average level of democracy, a fact that is sometimes interpreted as a sign that smallness favors democratic systems of rule. However, the relationship between island status and democratic rule may be the product of other features. Many island states are Christian – and more specifically Protestant – in religious orientation (Anckar 2008). Many island states were colonized by the British and remain within the Commonwealth. Many experienced an extensive tutelary relationship with a European power, culminating in many years experience with electoral politics and semi-autonomous governance prior to independence. Many island states have few natural resources, and thus do not suffer from the "resource curse" (Ross 2001). Most islands depend upon international trade or tourism for a large share of their national income, which may predispose them to democratic rule. Living on an island may foster a greater sense of national community and citizenship than one finds in (otherwise equivalent) land-based states (Anckar, Anckar 1995: 213, 220-2). It has also been argued that island-states are less militarist, because their sovereignty is more secure than land-based states and because expansionist policies are more difficult to pursue (Clague et al. 2001: 22-3; Faris 1999). In short, quite a number of factors might explain the association between islands and democracy, other than population size (Anckar and Anckar 1995; Anckar 2002; Clague et al. 2001: 22-3; Dahl, Tufte 1973; Dommen 1980; Hadenius 1992: 125-7; Lockhart, Drakakis-Smith, Schembri 1993; Srebrnik 2004). Consequently, we regard Island as a plausible – and, indeed, important – control in the benchmark model.

Model 2 is regarded as a maximal specification. It adds several additional controls to the benchmark: Landlock (-), Latitude (+), English legal origin (+), European language (+), Linguistic fractionalization (+), Religious fractionalization (-), Catholic (-), Land area (-), Urban population (+), Population density (+), Oil production per capita (-), number of years since State formation (+), Trade openness (+), and regional dummies (West Europe, Latin America, Middle East, Africa). The sample is limited to contemporary decades (1960-) by reason of missing data for a number of these covariates.

Model 3 is parsimonious, including only the diffusion variable and decade dummies. Model 4 returns to the benchmark specification, this time substituting year dummies for the usual trend variable. Model 5 recalibrates Polity2 as standard deviations from the mean (z scores) for each year, another method of de-trending. Model 6 subjects the benchmark specification to a Prais-Winsten regression with AR(1) autocorrelation and panel correct standard errors (PCSE) (Beck, Katz 1995; Hoechle 2007). Model 7 substitutes a lagged dependent variable for the usual decade dummies, along with robust standard errors (Beck, Katz 1995). Model 8 is identical, but with errors clustered by country.

Model 9 re-codes population as a series of dummies in order to test for non-linearities in the relationship with democracy (ones that might not be captured by the logarithmic transformation). Dummy variables include Micro (<1,000,000 [the excluded category]), Small (between 1,000,000 and 10,000,000), Large (between 10,000,000 and 100,000,000), and Huge (>100,000,000).

Model 10 adopts the *Political Rights* variable from Freedom House as an alternative outcome measure of democracy. Model 11 employs a much narrower measure of electoral democracy focused on electoral contestation. *Competition* measures the percent votes not won by the largest party in presidential or parliamentary elections (Vanhanen 1990, 2000).

Results of the eleven models tested in Table 2 indicate a strong positive relationship between population and electoral democracy as tested by a variety of specifications, estimators, and outcome measures. Of course, coefficients change when the specification is altered – from the benchmark model (Model 1) to the maximal model (2) and finally the minimal model (3). Generally, the relationship of theoretical interest is stronger when more covariates are included. However, it remains statistically significant even in a sparse model with only a diffusion variable and decade dummies. Model 9, which divides the variable into 4 dummies (the excluded category is the smallest), suggests that there are no significant non-linearities.

[Comment on impact.]

Table 3 adopts the benchmark specification (for the most part), focusing this time on categorical measures of democracy, as described briefly above. Model 1 transforms the Polity2 variable into a trichotomous outcome, which is then analyzed with ordered logit in the benchmark model. Model 2 regresses this trichotomous dependent variable against the same covariates with a random effects ordered probit estimator (following Epstein et al. 2006). Model 3 employs a dichotomous measure of democracy (described above) in a logit regression with year dummies (following Beck, Katz, Tucker 1998). Model 4 repeats this format with a lagged dependent variable (substituting for the year dummies). Model 5 adopts the *Turnover* variable collected in the Archigos dataset (Goemans et al. 2009) – coded as 1 whenever a change of top leaders occurs under regular circumstances.

Models 6 and 7 employ the Polity2 binary outcome variable to conduct event-history tests. Here, the question is not the likelihood of being either democratic or autocratic, but rather the likelihood (or, more specifically, the rate) of transitioning from autocracy to democracy, i.e., democratization (Model 6) or from democracy to autocracy (Model 7). Both Cox and Weibull models are plausible and both have been employed in the literature (Bernhard et al. 2001; Kapstein, Converse 2009). We prefer the Weibull model because it explicitly models changes in the likelihood of democratization over time – a likelihood that presumably increases substantially over the period of observation (as noted by Kapstein, Converse 2009). Coefficients indicate hazard ratios, i.e., the estimated percentage change in the baseline hazard rate resulting from 1-unit change in the independent variable. A positive relationship indicates that a transition is hastened; a negative relationship indicates that a transition is postponed. Note that we do not include proximate causes such as economic growth, instability, or trade in our model; nor do we include a measure of democratic consolidation (e.g., number of previous transitions). This is because all of these factors are quite likely endogenous to the factors of theoretical interest and would therefore serve as confounders.

Results from tests in Table 3 are fairly consistent. Population is positively associated with democracy, and generally statistically significant. The two transition models suggest that this effect is primarily a product of population's effect on democratization, rather than on democratic consolidation.

Since we are examining an extraordinarily long period of time one might wonder about variation in the key relationships across the 1820-2002 period. Figure 1 presents rolling regressions of the benchmark model – Model 1, Table 2 – for every twenty-year period from 1820 to the present. Each point on the line represents the coefficient for population (ln), along with a 90% confidence interval, for a specific 20-year period. Not surprisingly, there is variation over time, with great instability in the nineteenth century – at which time there are many fewer cases in the sample and much greater instability in regime-type on the part of these sovereign states. One can see a notable dip in the mid-nineteenth century as well as in the early-twentieth century. By contrast, the postwar period is quite stable, with a small (but statistically significant) coefficient for population. Thus, short-term variation is considerable, and begs explanation (which we are still wrestling with). The only apparent long-term pattern in the relationship between population and democracy is the stability of this relationship in the postwar era – partly no doubt a product of the larger sample of sovereign and semisovereign countries on our sample and partly, perhaps, the solidification of a causal relationship that was, until that point, in flux. Note that many of the causal mechanisms articulated in this paper are more likely to be true in the present era than in previous historical eras.

Threats to Causal Inference

In drawing inferences from the data one must be cognizant of the circumstances that might cause a polity to be more or less populous. They include: (a) the changing borders (e.g., when the polity is formed or re-formed), (b) in- and out-migration, and (c) fertility and mortality rates.

Most of the work attempting to explain the size of nations is focused on exogenous factors that are presumed to affect all countries equally and permanently (e.g., economies of scale, burdens of heterogeneity, necessity of self-defense) or all countries equally at particular points in time (e.g., the prominence of international trade; technological developments affecting transport, communications, warfare, and administration; and threats to sovereignty) (Alesina, Spaolore 2003; Hiscox, Lake 2001; Wittman 1991, 2000). Insofar as these rather abstract arguments are correct they explain variation through time but not across countries.

It will be seen that violations to SUTVA (Rubin 2005) are inherent in the chosen research design. Units contaminate each other; the treatment of one unit may affect the treatment of another. That said, we do not anticipate that this contamination is very large. During the observed periods of analysis, borders remained mostly constant, migration was fairly minimal, and fertility and mortality patterns had slow-moving effects. Our predominantly cross-sectional analyses should not be greatly affected by potential confounders occurring within the observation period.

A more serious potential threat to inference is posed by endogeneity between the outcome and the factors of theoretical interest. This is most likely to manifest itself prior to the observation period, i.e., prior to a country's international recognition as sovereign. Hiscox and Lake (2001) speculate that regime-type affects territory size. Specifically, democracies may be more likely to have a smaller circumference than autocracies.[11] If this is indeed a

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[11] Alesina, Spaolore (2003) view democracy as an exogenous circumstance, with changes in regime-type affecting likely changes in the size of nations. However, their conclusions might also apply to variation across nation-states at a particular point in time.

causal relationship, and if it carries over to population size (larger territories having more people), then there is some circularity in our research design. However, the conjectured bias runs contrary to our hypothesis. Thus, this selection effect, insofar as it exists, should strengthen the credibility of the crossnational evidence presented here.

Democracy may also affect population size through its impact on fertility and mortality. There is debate over whether democracies have lower mortality rates (Gerring et al. 2010; Ross 2006); but the aggregate effect on population is in any case not very large. There is strong evidence that democracies have lower fertility rates (Beer 2009; Przeworski et al. 2000) which over time would impact on population size. However, the direction of confounding runs against the grain of the hypothesis (democracies shrinking and autocracies expanding), lending credence to the evidence.

Let us now suppose that in the long-term process of state formation (most of which occurs prior to the period of observation) large countries often fragment into smaller ones. Here is a situation where the causal factors of interest affect the selection criterion. As an example, let us imagine that British Empire in South Asia fragmented into Bangladesh, Bhutan, India, Myanmar (Burma), Nepal, Pakistan, and Sri Lanka because the original colonial polity was too large and/or too diverse to contain these heterogeneous elements (a plausible conjecture). This biases the crossnational analysis only if the resulting polities (those that we see today) have been more/less likely to democratize for reasons *other than* their size, and if these reasons are not effectively measured and controlled in the analysis.

Let us entertain another conjecture. Suppose that, over time, "successful" polities become larger (because they are wealthier and more powerful) and unsuccessful polities fragment or disappear. This is a problem if the factors driving success also drive democratization, and if these factors are not controlled in our analyses. But it is not clear that powerful polities are more or less likely to become, and remain, democratic. Although richer countries are more likely to be democratic, this factor is controlled in our analysis. And wealth, in any case, is orthogonal to population (Pearson's r=.07).

Finally, one might wonder about variations in the size of states due to varying geography. Tam (2004) finds that countries with natural, protected borders (e.g., mountains, sea) have larger populations. He argues that this is not simply a feature of the benefits of ocean access but also a feature of the protection that natural borders provide. In any case, we note that Landlock is included as a control in Model 2, Table 2, and that its inclusion in the model has minimal impact on the relationship of theoretical interest.

Interpreting observational data is always fraught, and perhaps nowhere moreso than when relationships are distal and there are no discrete interventions (allowing for clearly delineated pre-and post-tests). However, historical knowledge and causal intuition do not suggest anything to mitigate the plausibility of the causal effects between population and electoral democracy. There is no reason to suppose that large polities are more likely to realize electoral democracy than small units – other than by the fact of their size.

Within-Country Tests

In addition to crossnational data, we examine data drawn from different electoral districts within the United States. Here, one finds much less variation in the quality of electoral democracy but also less background heterogeneity, easing problems of causal inference. Before turning to the data analysis we review several US-focused studies that bear upon our theoretical question.

Studies examining Senate and House elections have often focused on the question of why the former are generally more competitive (Gronke 2000; Krasno 1994). Explanations for this pattern center on a series of stylized contrasts: Senate races occur in districts with greater social heterogeneity; they occur in districts with greater partisan balance; they feature higher-quality challengers; they attract more money and consequently more spending; they have greater media efficiency due to the overlap between Senate districts and media markets; and they are higher in salience and turnout. All of these factors probably owe something to the greater population size of Senate districts, and are in this sense concordant with the argument pursued here. One study of constituent relationships among Senators finds that, "In large population states, senators and constituents simply are unable to have the level of personal contact that exists in small population states. Large state senators must resort to more impersonal levels of contact – mass mailings and electronic media. In turn, constituents feel more distant from their senators in these states" (Oppenheimer 1996: 1297). This, too, may help explain the vulnerability of senators – who, as a class, have

larger districts than their House counterparts.

A number of studies have looked explicitly at the relationship between population and competitiveness (understood as closeness of the vote or incumbent defeat). Hibbing and Brandes (1983) compare Senate elections. Aistrup (2004) examines country-level elections. Lascher (2005) looks at county board of supervisor elections in California. Most extant studies seem to support the notion that a larger district encourages greater competition. Lascher (2005) does not; however, he includes several covariates in his model that are quite plausibly endogenous to population (e.g., number of challengers, quality of challengers, and partisanship).

No extant study of electoral competitiveness in the US has encompassed a wide range of different electoral offices. Here, we present results from presidential, Senate, House, gubernatorial, upper state house, and lower state house elections – a total of 74,287 elections, observed across several decades.

Two measures of electoral democracy are adopted. *Competition* is defined here as 100 minus the margin of victory between top two vote-getters. This is virtually identical to the measure (drawn from Vanhanen 1990) that we employed in one of our crossnational tests (see Table 2). *Turnover* is understood as a change of seat from one party to another (including independents), and is broadly analogous to the measure of turnover in leadership (Goemans et al. 2009) that we applied at national levels (see Table 3).

Evidently, these two measures do not fully encompass the concept of electoral democracy. However, they do measure important aspects of electoral democracy, as argued in the previous section. Consequently, we view evidence drawn from electoral contests within the United States as providing important corroborating evidence for the findings contained in Tables 2 and 3.

Electoral data used in coding these variables is drawn from a variety of sources: (a) for presidential and senatorial elections, the Office of the Clerk Election Statistics[12]; (b) for House electoral data, Lublin (1997), (c) for state upper and lower house electoral data, Carsey et. al. (2007), and (d) for gubernatorial data, Parker (2010). Population data is drawn from decennial US Census reports,[13] with values imputed in order to cover every year in the dataset. In subsequent empirical tests this variable is transformed by the natural logarithm, as in previous crossnational tests.

The distribution of available data across key variables, regions, and time-periods is depicted in Table 4. In comparing different types of offices, their varying populations, and electoral outcomes, one finds that larger districts are generally associated with greater competitiveness and more frequent turnover. Of course, this relationship may be spurious; hence, the need for a more formal analysis.

Subsequent analyses employ a variety of covariates judged to be important and exogenous influences on competition and/or turnover. This includes ethnic fractionalization, the percentage share of different social groups (White, Black, Non-English, and Other), Urban (percent living in urban areas), Income per capita (natural logarithm), High school (percent above age 25 with a high school degree), and College (percent above age 25 completing college). Some of these covariates are treated as constants since they change little over the observed period; these are taken from the 2000 US Census records. Historical data (at decadal intervals) is available for House districts from Lublin (1997) and for the entire United States from the US Census[14] and the Bureau of Economic Analysis.[15]

Redistricting poses a challenge for this analysis since it alters the composition of House and state house districts – rendering the Turnover variable meaningless and also creating problems of missing-ness in covariate data for districts for which there is little historical data (since contemporary data cannot be projected backward). This challenge is treated in two ways. First, a dummy variable is generated to mark the first election after redistricting. Tests reveal that this variable has little impact on the variables of theoretical interest, and is in any case proxied by year dummies in many of the subsequent analyses. Thus, this variable is omitted in results reported below. Second, we omit earlier observations for state house elections whenever there is a substantial (>2%) change in the number of districts within a state after redistricting (signifying a significant change in the composition of that state's districts).

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[12] http://clerk.house.gov/member_info/electionInfo/index.html
[13] E.g. http://www.census.gov/main/www/cen2000.html
[14] Ethnicity data from www.census.gov/population/www/documentation/twps0029/tab08.html, education data from www.census.gov/hhes/socdemo/education/data/census/half-century/tables.html (Tables 5 and 6, both sexes), and other data from selected US Census documents.
[15] For income data, see www.bea.gov/national/nipaweb/SelectTable.asp?Selected=N, Table 2.1.

Regression Tests

Table 5 provides a variety of regression tests for the main hypotheses. Models 1-10 adopt *Competition* as a measure of electoral democracy, analyzed with ordinary least squares. Model 1 tests the benchmark model with a full sample of election districts, all available covariates (as listed above), along with state and year dummies. Model 2 restricts the model to population and state dummies. Subsequent models repeat this contrast between maximal and minimal specifications with samples drawn from Senate elections (3&4), gubernatorial elections (5&6), state upper house elections (7&8), and state lower house elections (9&10).

Models 11&12 adopt *Turnover* as the outcome of interest, explored with a logit estimator in maximal and minimal samples. As a binary outcome (and a relatively rare one, at that), turnover offers less variation and generally shows weaker results – which do not always hold up in smaller, more focused samples.

The covariates *White* and *Income* behave as expected in most models, showing a positive relationship to both competition and turnover. Other covariates are less consistent. Results for the variable of theoretical interest are positive and statistically significant in all models, though the coefficient varies considerably across different samples as one might expect. There is some variation, in particular, across maximal and minimal models. However, the fact that the relationship is robust in both models suggests that the finding is not contingent upon arbitrary choices in specification.
Discuss estimated causal impacts.

Matching Tests

The final within-country analyses, summarized in Table 6, explore several quasi-experimental tests, using Competition as our measure of electoral democracy. Here, elections held coterminously within the same state are regarded as similar in all respects except the treatment of theoretical interest, district size. Thus, we compare the competitiveness of coterminous Senate and House elections (within the same state), the competitiveness of gubernatorial and state upper house elections (within the same state), and the competitiveness of state upper and lower house elections (within the same state). The first-named district is regarded as the treatment (being the larger of the two) and the second as the control.

Analysis is by nearest-neighbor matching, with replacement, with a minimum of a single match, and with coefficients understood as sample average treatment effects (Abadie et al. 2001). (Population average treatment effects are almost identical. Likewise, when the number of minimum matches is increased there is only a slight change in estimated coefficients and standard errors.)

Two specifications are provided for each treatment/control comparison. The first selects a minimal set of covariates, including only state (a categorical variable on which exact matching is performed) and year (understood as a continuous variable). The second offers a fuller set of covariates including many of the same factors included in the previous regression tests (Table 5): Ethnic fractionalization, White, Black, Non-English, Other, Urban, Income per capita, High school, and College. Note that White is included as a covariate in Model 5 because it eases the computational burden of the analysis (which otherwise produces so many equivalent matches that it has difficulty converging).

Minimal and maximal specifications select different, but also eminently plausible, matches for the treatment group. In the minimal model, the contrast includes all other elections occurring in the same state at the same time. (Exact matching is on State and inexact matching on Year.) Thus, in Model 1 we contrast the competition score for a Senate election with the average competition score of all House elections in that state and year. (This comparison is iterated for each state/year in the sample.) However, in Model 2 we are comparing the competitiveness of a Senate election with that House election(s) in the same state and year that matches most closely on other covariates – White, Black, Non-English, Other, Urban, Income per capita, High school, and College.

Models 3 and 4 are identical, insofar as a single state-wide electoral unit (a gubernatorial election) is compared with multiple within-state electoral units (state upper house elections.

In Model 5, we contrast the average competition score for all state upper house elections with the average competition score of all state lower house elections in that state and year, and this comparison is iterated for each state/year in the sample. Likewise, in Model 6 we contrast the average competition score for all state upper house elections with that state lower house election(s) in the same state and year which matches most closely on other covariates – White, Black, Non-English, Other, Urban, Income per capita, High school, and College.

These are both plausible counterfactuals. The minimal model defines a composite of all other elections in

the state at the lower electoral unit as the control group. The maximal model defines a subset of those elections which most closely fit the demographics of the treatment group.

An even more precise comparison is possible for models 5 and 6. One might compare the state upper house district with those lower house districts that fit within it (as defined by centroids of the latter). We are currently collecting data that should allow us to do this, at least for elections held over the past several election cycles. (Leads would be greatly appreciated.)

As it happens, results vary only slightly across the minimal and maximal models. Since the minimal model provides a better match on the covariate of greatest importance – State – we regard the odd-numbered models as somewhat more trustworthy than the even-numbered models.[16]

Results across the tests displayed in Table 6 conform to theoretical predictions. The larger group, defined as the treatment group, shows greater competitiveness in every comparison and in every specification. Moreover, the size of these causal effects varies across treatments as one might expect. Where the ratio of the size of the district treatment group to the size of the control treatment group is greatest, one finds the strongest treatment effect. The population ratio of the average gubernatorial district (i.e., state) to the average state upper house districts is roughly 31. The size ratio of Senate (i.e., state) districts to House districts is approximately 10. And the size ratio of state upper house to lower house districts is roughly 2. This is reflected in the coefficients reported in Table 6.

Unfortunately, we cannot interrogate these coefficients in order to obtain a precise estimate of causal effects. The reason for this should already be apparent: the treatments contained in these natural experiments (which contrast different district types) are not isomorphic with the treatment of theoretical interest (population). Although treatment groups are always at least as large in population as corresponding control groups, there is considerable variation across states and treatments in the size of this differential. This is especially evident in the first analysis, comparing Senate and House elections. Consequently, although we can state fairly confidently that there is a treatment effect we cannot use the matching analyses to provide an estimate of how much an increase in constituency size might impact the competitiveness of an election, as we did in the regression-based analyses (see Table 5, bottom).

This observation suggests another approach to estimation. One might gain some traction on our problem by comparing the *competitiveness differential* between Senate and House elections in differently-sized states. Note that Senate elections impose a single electoral unit on each state, regardless of size, whereas House elections (since Baker v. Carr) are roughly equal in size. This means that some states have a single House district while others have dozens. Our theory suggests that there should be a greater differential between the competitiveness of Senate and House elections in large states, and a minimal differential in small states. Indeed, the minimal differential found in states with only one House district (i.e., in Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont, and Wyoming) represents the background noise that one would, in a perfect field experiment, hope to neutralize (e.g., those effects on electoral behavior that have nothing to do with the size of constituencies per se). The only problem with this interpretation is contamination across units; the behavior of elites and voters in single-district states is probably not independent of behavior in other states. In any case, the results of this analysis confirm expectations. When Competitiveness differential (Senate Competition minus mean House Competition in that same state and year) is regressed against Population (unadjusted), the result is a positive and statistically significant relationship. To be sure, it explains only a small percentage of the variation ($R2=.06$), perhaps because of the contamination problem discussed above.

Let us now return to the main results, posted in Table 6, to discuss some of the implicit research-design issues. Note that in a classic experiment individuals are assigned to treatment and control groups. Here, the same individuals (more or less) are subject to both conditions: by virtue of casting a ballot they are given the option of voting for both large- and small-district offices. Voting participation is not relevant for our analysis so long as we can assume either (a) that nonvoters would have behaved similarly to voters (had they voted) *or* (b) that the behavior of American voters reflects the behavior of electorates elsewhere (note: no electorate experiences 100% turnout). Likewise, voter fatigue (individuals who choose to vote for one office but not another) poses a problem of noncompliance if the result of interest lies at the individual level. However, this lack of equivalence does not threaten the causal effects of theoretical interest, which are measured at aggregate (district) levels. It is the behavior of a district's voters, not individual voters, that we are primarily concerned with. Naturally, it might be helpful to

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[16] It is worth mentioning that results displayed in Table 6 are also replicable in OLS models that mirror each of the matching analyses (available from authors). Indeed, coefficients obtained from these replication models are extremely close to the estimates contained in Table 6.

know about turnout, about individual vote choices, and about ballot fatigue, for this could shed light on causal mechanisms. But since our theoretical interest is in aggregate-level behavior we are primarily concerned with the *district* as a unit of analysis.

Fortunately, we have many groups at our disposal so sample size is not an issue. All districts (of the specified types) are included in the analysis, so problems of sampling within the United States arise only with respect to the chosen time-period. We must leave open the possibility that the size effect might have been different in previous eras of American history. Likewise, we can only speculate about the representativeness of these results across other countries. Hopefully, the crossnational analyses presented in the previous section mitigate this concern.

A potential problem with the first two settings (Models 1-4) is that the smaller district (the control condition) is subject to political manipulation every ten years as a product of redistricting, while the larger district (the treatment condition), being a state, is fixed. This introduces systematic bias into the comparison if redistricting is carried out with the primary goal of incumbency protection. In order to test this proposition, we regress Competition against a dummy variable representing the first post-redistricting election. This is tested with House elections, state upper house elections, and state lower house elections, and in a variety of specifications. In all tests, the dummy shows a positive relationship to Competition, suggesting that the purpose of redistricting is primarily to gain partisan advantage rather than simply to protect incumbents (Gelman, King 1994). That is, lines are redrawn with the purpose of removing "safe" districts controlled by the majority party and creating a small number of extremely safe districts for the minority party. Thus, we see no positive bias in the analysis. (This control is not maintained in the matching analysis because it is collinear with Year dummies.)

Although confounders may be unlikely, these analyses are subject to problems of interpretation. Specifically, it is clearly the case that the treatments differ from the controls in more than district size, an issue we have already touched upon. Senate offices – and hence Senate elections – are different from House offices/elections. Likewise for gubernatorial and state upper house offices/elections. Most important, these offices are different in the degree of power they wield. It is possible that power, not sheer size of constituency, is what drives the differences picked up in the matching analyses. Two things may be said about this. First, this problem of interpretation does not mar the regression analyses reported in models 3-10 of Table 6, where differently sized districts for the same office are compared. Second, this problem of interpretation is considerably less significant in the final matching analysis in Table 5. Although state senators are more powerful than state representatives, it is a matter of slight degrees, and one that few constituents are likely to be aware of. Finally, the power associated with an office is to some extent integral to the theory. Recall that one of the causal mechanisms set forth in the previous section is about the power of offices in large and small polities. Large polities offer more powerful positions, ceteris paribus; the prime minister of India occupies a more powerful post than the prime minister of Sri Lanka. This, by itself, may motivate behavior – of masses and elites alike. So, the fact that the natural experiments enlisted in this final analysis are dissimilar in size and in power is not entirely problematic. In some respects, it replicates a reality on the ground that is directly relevant to the theory being tested.

Discussion

Before concluding, it is worth reemphasizing that most of the evidence presented here falls far from the experimental ideal. The treatment is not randomized, units are not independent over the observed time-period, many potential confounders can be identified, and no general theory can be relied upon to solve specification problems. Under the circumstances, results must be regarded as suggestive rather than conclusive. (This is true, of course, for most observational data analyses, and especially for cross-country analyses.[17]) Nevertheless, where stronger research designs are impossible we must satisfy ourselves with the evidence that is available (Gerring 2011).

In any case, the purpose of this empirical exercise is not to estimate precise causal effects but rather to indicate the general direction and plausibility of a causal effect. Primary attention is therefore paid to robustness tests (designed to test various assumptions) rather than to point estimates.

With these caveats and clarifications, we conclude that population enhances the prospects of democracy, understood according to the precepts of the electoral model. This relationship holds in crossnational tests with a

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[17] Freedman (1991), Gigerenzer (2004), Kittel (2006), Seawright (2010), Summers (1991).

wide variety of specifications, estimators, and outcome measures as well as within-country tests, including several quasi-experimental research designs. We have also identified ample causal mechanisms to support this empirical story. Although we cannot specify precisely why – i.e., which concatenation of mechanisms connect population to democracy – we have given plausible reasons to support the conjecture. It is hoped that the combination of crossnational and within-country tests is informative. Note that the former offers a full range of variation in the outcome while the latter offers a set of tests that minimize background assumptions. Insofar as results are complementary, the validity and generalizability of the findings are upheld.

If the findings are true, our research suggests a somewhat less optimistic view of current trends signaling the breakup of large states into smaller ones (Lake, O'Mahoney 2004) and the devolution of power within nation-states to subnational units (Rodden 2004). While these developments bring power closer to the people – enhancing the participatory dimension of democracy – they may also compromise the quality of *electoral* democracy.

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Table 1:
Extant Crossnational Studies

Study	Outcomes	Research design	Period	Finding	Focus
Acemoglu et al 2008	PR	TSCS: OLS w/ fixed effects, GMM	1960-2000	0	No
Barro 1999	PR, CL	TSCS: SUR w/ lagged Y at 5-year intervals	1972-1995	0	No
Gassebner et al. 2009	Survival (DD)	TSCS: Extreme bounds	Varying	0	No
Gassebner et al. 2009	Democratiz (DD)	TSCS: Extreme bounds	Varying	0	No
Hadenius 1992	Hadenius index	TSCS: OLS	1988	-	No
Ott 2000	PR, CL, and others	CS: Cross-tabs	1973-1995	-	Yes
Rose 2006	PR, CL	Various	1960-2000	0	No

Units of analysis = countries or country-years. *Outcomes* = measures of democracy. *Finding* = negative (-), positive (+), or null (0). *Focus* = the main focus of the study. *CS* = cross-section. *TSCS* = time-series cross-section. *SUR* = seemingly unrelated regression. *GMM* = generalized method of moments. *DD* = (Alvarez et al. 1996; Cheibub et al. 2010). *PR* = Political Rights (Freedom House). *CL* = Civil Liberties (Freedom House).

Table 2:
Continuous-Outcome Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Y at t+1	Polity2	Polity2	Polity2	Polity2	Polity2(z)	Polity2	Polity2	Polity2	Polity2	Pol Rights†	Competition
Estimator	OLS, NW SE	OLS, NW SE	OLS, NW SE	OLS, NW SE	OLS, NW SE	PW, PCSE	OLS, Robust SE	OLS, Cluster SE	OLS, NW SE	OLS, NW SE	OLS, NW SE
Population (ln)	0.428*** [0.053]	0.396*** [0.108]	0.126*** [0.043]	0.425*** [0.053]	0.059*** [0.008]	0.313*** [0.100]	0.025** [0.010]	0.025** [0.011]		-0.077*** [0.020]	1.344*** [0.191]
Population (small)									0.439 [0.290]		
Population (large)									0.966*** [0.307]		
Population (huge)									3.127*** [0.525]		
GDP pc (ln)	1.479*** [0.059]	1.409*** [0.160]		1.491*** [0.059]	0.170*** [0.009]	0.190** [0.075]	0.063*** [0.013]	0.063*** [0.012]	1.483*** [0.060]	-0.566*** [0.020]	6.054*** [0.220]
Island	3.049*** [0.238]	2.778*** [0.399]		3.059*** [0.240]	0.341*** [0.034]	3.688*** [0.519]	0.109** [0.046]	0.109** [0.053]	2.633*** [0.248]	-0.757*** [0.090]	5.557*** [0.870]
Protestant	0.047*** [0.003]	0.006 [0.005]		0.047*** [0.003]	0.010*** [0.000]	0.060*** [0.007]	0.002*** [0.001]	0.002*** [0.000]	0.046*** [0.003]	-0.010*** [0.001]	0.146*** [0.009]
Muslim	-0.049*** [0.003]	-0.033*** [0.007]		-0.049*** [0.003]	-0.009*** [0.000]	-0.059*** [0.008]	-0.003*** [0.001]	-0.003*** [0.001]	-0.049*** [0.003]	-0.016*** [0.001]	-0.136*** [0.010]
Diffusion	6.789*** [0.500]	1.898*** [0.352]	11.176*** [0.597]	6.577*** [0.501]	0.447*** [0.050]	2.719*** [0.439]	0.469*** [0.075]	0.469*** [0.120]	6.644*** [0.500]	-1.790*** [0.139]	24.323*** [1.898]
Latitude (ln)		0.223 [0.158]									
Landlock		0.691** [0.329]									
English legal or.		2.821*** [0.313]									
European lang.		2.635*** [0.419]									
Linguistic fract		2.686*** [0.548]									
Religious fract		-4.298*** [0.634]									
Catholic		-0.029*** [0.006]									
Land area		-0.000** [0.000]									
Urban pop		0.044*** [0.008]									
Pop density		0.003 [0.002]									

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Oil production	-3.874*** [0.939]										
State formation	-0.008*** [0.002]										
Trade	-0.005 [0.004]										
West Europe	3.954*** [0.570]										
Latin America	1.353** [0.597]										
Middle East	-2.236*** [0.563]										
Africa	-1.914*** [0.406]										
Decade dummies	X	X	X			X			X	X	X
Year dummies				X							
Y (lagged)							X	X			
Constant	X	X	X	X	X	X	X	X	X	X	X
Observations	9755	4570	14322	9755	9755	9755	9687	9687	9755	5010	8787
Countries	183	143	220	183	183	183	183	183	183	182	178
Years	1820-2002	1961-2000	1815-2002	1820-2002	1820-2002	1820-2002	1820-2002	1820-2002	1820-2002	1972-2003	1820-2000
R2	0.4309	0.5793	0.1755	0.4346	0.3599	0.0441	0.9462	0.9462	0.4281	0.4769	0.4086
Impact	$	$								$	$

Y = outcome (forward-lagged). †: Reverse scale. *X* = covariates included. *** p<0.01, ** p<0.05, * p<0.1 (two-tailed tests). *OLS* = ordinary least squares. *NW* = Newey-West standard errors. *RE* = random effects. *SE* = standard errors. *Cluster* = clustered by country. *PW, PCSE* = Prais-Winsten regression with AR(1) autocorrelation and panel correct standard errors. *Impact* = estimated increase in electoral democracy/competition when population increases by 100,000 from the sample mean.

Table 3:
Categorical-Outcome Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Y at t+1	*Polity2*	*Polity2*	*Polity2*	*Polity2*	*Turnover*	*Polity2*	*Polity2*
Construction	*Trich-otomous*	*Trich-otomous*	*Dich-otomous*	*Dich-otomous*	*Dich-Otomous*	*Democratic transition*	*Autocratic transition*
Estimator	*Ordered logit*	*Ordered probit*	*Logit (MLE)*	*Logit (MLE)*	*Logit (MLE)*	*Weibull Hazard, Cluster SE*	*Weibull Hazard, Cluster SE*
Population (ln)	0.126***	0.078***	0.156***	0.084**	0.078***	0.082**	-0.022
	[0.014]	[0.008]	[0.016]	[0.042]	[0.021]	[0.035]	[0.030]
GDP per cap (ln)	0.473***	0.280***	0.577***	0.269***	0.310***	0.212***	-0.173***
	[0.018]	[0.010]	[0.022]	[0.055]	[0.027]	[0.039]	[0.027]
Island	1.041***	0.596***	1.142***	0.407**	-0.046	0.311**	-0.727***
	[0.066]	[0.038]	[0.074]	[0.199]	[0.091]	[0.128]	[0.216]
Protestant	0.016***	0.010***	0.016***	0.008***	-0.004***	0.006***	-0.007***
	[0.001]	[0.001]	[0.001]	[0.003]	[0.001]	[0.001]	[0.002]
Muslim	-0.015***	-0.008***	-0.018***	-0.011***	-0.012***	-0.014***	0.003***
	[0.001]	[0.000]	[0.001]	[0.003]	[0.002]	[0.004]	[0.001]
Diffusion	4.835***	2.400***	3.726***	2.469***	0.901***	0.362***	-1.069***
	[0.232]	[0.109]	[0.259]	[0.403]	[0.177]	[0.120]	[0.253]
Decade dummies	X	X				X	X
Year dummies			X				
Lag Y				X			
Constant	X	X	X	X	X	X	X
Observations	9755	9755	9747	9687	8498	10860	10860
Failures						4109	5646
Countries	146	146	146	146	146	191	191
Years	1820-2002	1820-2002	1823-2001	1820-2002	1870-2002	1820-2002	1820-2002
Pseudo-R2	0.2469	0.2413	0.3204	0.8576	0.1023	.	.
Log pseudolikelihood						18098	26000
Impact	$		$			$	

Y = outcome (forward-lagged). Trichotomous = cutoffs at -5 and 5 on the Polity2 scale. Dichotomous = cutoff at 5 on the Polity2 scale. *X* = covariates included. *** $p<0.01$, ** $p<0.05$, * $p<0.1$ (two-tailed tests). *Impact:* estimated increase in electoral democracy/competition when the population increases by 100,000 from the sample mean.

Population Coefficient for Benchmark Model
20 Year Periods
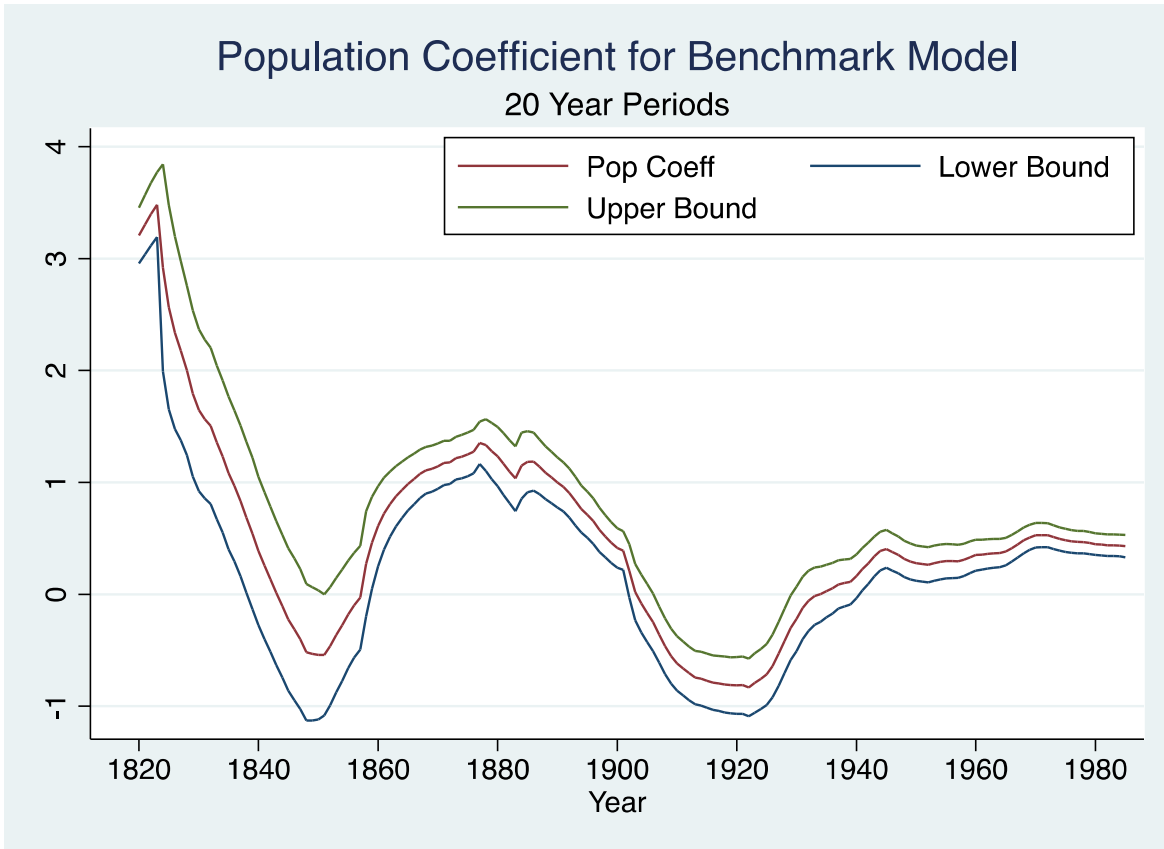
Coefficients (solid line) and 90% confidence intervals (two-tailed) for Population (ln) in Model 1, Table 2 (the benchmark model) in rolling regressions restricted to 20-year sample periods beginning in 1820 and ending in 1996. Each point is graphed at the mid-point of a 20-year period. The coefficient is positive and significant in 64% of the models, negative and significant in 14% of the models, insignificant in 22% of the models.

Data Description (US)

	Population (1000s)			Competition			Turnover	Coverage		
	min	max	mean	min	max	mean	mean	years	units	elections
Offices										
Lower state house	8	423	66	0	100	52.5	0.133	1968-2003	4161	52999
Upper state house	13	847	152	0	100	55.7	0.162	1968-2003	1805	16882
House of Rep	302	799	499	0	100	60.8	0.093	1972-1992	435	3699
Senate	402	33900	5155	0	100	78.5	0.184	1980-2000	100	376
Governor	382	33100	4718	35	100	83.4	0.341	1977-2000	50	317
President	*157	*291	*223	77	100	90.8	0.571	1948-2000	1	14
Regions										
Northeast				0	100	59.1	0.133			16490
Midwest				0	100	57.6	0.137			21372
West				0	100	60.1	0.157			15594
South				0	100	41.3	0.132			20817
Total				0	100	53.9	0.139			74273
Eras										
1948-69				0	100	65.2	--			2241
1970-79				0	100	59.6	0.149			16802
1980-89				0	100	52.1	0.131			21363
1990-2003				0	100	51.5	0.139			33881
Total	8	*291	194	0	100	53.9	0.139		41	6552	74287

Regions = summarizes data for all but presidential elections by region. *Empty cells* = data not relevant. * = population in millions.

Table 5:
District-level Regression Analyses (US)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Estimator	OLS, robust SE										Logit	
Sample	Full		Senate		Governor		State upper house		State lower house		Full	
Population (ln)	3.882*** [0.231]	5.065*** [0.157]	2.422*** [0.926]	3.036*** [0.786]	3.528*** [1.108]	1.506** [0.755]	17.417** [8.717]	37.002*** [8.831]	27.020*** [3.264]	40.678*** [3.353]	0.045* [0.024]	0.060*** [0.016]
Ethnic fract	19.411*** [1.534]		39.626 [35.365]		-1.483 [22.582]		15.404*** [3.668]		18.926*** [1.755]		0.782*** [0.174]	
White	8.120*** [2.970]		43.843 [29.451]		-20.526 [20.430]		13.446** [6.250]		7.983** [3.597]		0.220 [0.273]	
Black	-32.520*** [2.902]		-24.643 [32.515]		-23.885 [20.903]		-22.548*** [6.009]		-32.259*** [3.543]		-1.407*** [0.265]	
Non-English	-17.637*** [2.117]		-52.308 [37.930]		13.420 [28.862]		1.207 [4.777]		-20.867*** [2.616]		-0.596*** [0.229]	
Other	-22.945*** [3.326]		128.657* [69.743]		8.604 [56.984]		-8.550 [9.041]		-26.241*** [4.580]		-1.321*** [0.383]	
Urban	3.189*** [0.582]		-22.428* [11.995]		-19.003* [10.903]		2.387 [1.463]		5.241*** [0.707]		-0.043 [0.051]	
Income per cap	1.699*** [0.558]		56.509*** [12.894]		-2.727 [12.455]		8.654*** [2.544]		3.892*** [1.203]		0.038 [0.055]	
High school	12.323*** [1.526]		17.491 [35.053]		48.190 [29.701]		23.450 [14.869]		2.282 [5.542]		0.041 [0.177]	
College	8.650*** [2.505]		-106.743*** [37.539]		-56.440 [48.084]		61.655*** [12.277]		-17.876** [7.103]		0.296 [0.268]	
State dummies	X	X					X	X	X	X	X	X
Year dummies	X	X	X	X	X	X	X	X	X	X	X	X
Constant	X	X	X	X	X	X	X	X	X	X	X	X
Years	1948-2003	1948-2003	1980-2000	1980-2000	1977-2000	1977-2000	1968-2003	1968-2003	1968-2003	1968-2003	1948-2003	1948-2003
Obs (elections)	74,425	74,425	376	376	317	317	16,882	16,882	52,999	52,999	69,155	69,155
R2 (pseudo)	0.172	0.147	0.198	0.085	0.125	0.070	0.177	0.155	0.175	0.151	0.0619	0.0569
Impact	$		$		$		$		$		$	

Y = competitiveness, forward-lagged. *X* = covariates included in model. *Full* = includes elections for all offices listed in Table 4. *** p<0.01, ** p<0.05, * p<0.1 (two-tailed tests). *Impact* = estimated increase in competition or increased probability of turnover when population of a district increases by 100,000 from sample mean.

District-level Matching Analyses (US)

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	Senate		Gubernatorial		Upper state house	
Control	House		Upper state house		Lower state house	
SATE	20.045*** [2.497]	19.045*** [2.597]	28.167*** [1.978]	28.110*** [2.155]	2.724*** [.427]	2.665*** [.923]
Matching covariates						
Year	X	X	X	X	X	X
White		X		X	X	X
Black		X		X		X
Non-English		X		X		X
Other		X		X		X
Urban		X		X		X
Income per cap		X		X		X
High school		X		X		X
College		X		X		X
Exact matching covariate						
State	X	X	X	X	X	X
Exact matches	98%	78%	95%	76%	95%	93%
Years	1980-2000	1980-2000	1977-2000	1977-2000	1968-2003	1968-2003
N (elections)	4075	4075	17199	17199	69881	69881

Y = competition. *Unit of analysis* = district/election. Nearest-neighbor matching (Abadie et al. 2001), where number of matches ≥ 1. Coefficient and standard error for the treatment variable, estimating the sample average treatment effect (SATE). *** $p<0.01$, ** $p<0.05$, * $p<0.1$ (two-tailed tests).

APPENDIX

Table A1:
Crossnational Variables

Variable	Definition, source
Africa	Dummy (coding by authors).
Catholic	Percent Catholic (CIA WorldFactbook on-line).
Competition (Vanhanen)	100 - votes won by the largest party in presidential or parliamentary elections (or both, averaged) as % of total votes cast (Vanhanen 1990: 17). *Competition_Vanhanen*
Diffusion	Weighted average of the democracy scores of all the other countries. Weights are the inverse of the distance to each country.
English legal origin	English legal origin (La Porta et al 1999).
European language	Percent speaking a European language (CIA WorldFactbook on-line).
Executive constraints	Measures the degree of constraint on the chief executive (XCONST) with three codings: "(1) Unlimited authority, (2) Intermediate category, and (3) Slight to moderate limitations" (Mashall, Jaggers 2007: 15).
GDP per cap (ln)	GDP per capita (Maddison 2008), with missing data imputed from World Bank (2007). Transformed by natural logarithm.
Island	Dummy (coding by authors).
Land area	Land area, square kilometers (World Bank 2007), extended backward through time. TBD: recalculate using CLIO data.
Landlock	1 if country is landlocked, 0 otherwise (Acemoglu, Johnson, Robinson).
Latin America	Dummy (coding by authors). *LatinAm*
Latitude (ln)	Latitude (ln).
Linguistic fract	Linguistic fractionalization: 1 - summation of the square of each linguistic group's proportion (Alesina et al 2003). *Linguistic_fractionaliz_Alesina*
Middle East	Dummy. Coding by authors.
Muslim	Percent Muslim (CIA WorldFactbook on-line).
Oil production per cap	Oil production (Humphreys 2005) per capita (calculated by authors).
Political rights	Political Rights Index (Freedom House).
Polity2	Polity2 variable (Polity IV), with additional data imputed as described in the text.
Polity2, dichotomous	0 if Polity2 <5, 1 if Polity2 >=5, based on imputed Polity2 variable. *Polity4_imp_dich5*
Polity2, trichotomous	0 if Polity2 <-5, 1 if Polity2 >=-5 & <5, 2 if Polity2 >=5, based on imputed Polity2 variable. *Polity4_imp_trich*
Population density	Population density, persons per square kilometer (Banks 1994).
Population (ln)	Population (World Bank 2007), prior to 1960 supplemented by other sources. Transformed by the natural logarithm. TBD: recalculate using CLIO data.
Protestant	Percent Protestant (CIA WorldFactbook on-line).
Religious fract	Religious fractionalization: 1 - summation of the square of each religious group's proportion (Alesina et al. 2003).
State formation	Number of years since state formation, with 1648 as the earliest possible date (coding by authors). *Years_Late_1648*
Trade/GDP	Imports + exports as a share of GDP (World Bank 2007).
Trend	Annual count variable (coding by authors). *Year*
Turnover (Archigos)	Top political leaders enter and exit in a prescribed manner (Goemans et al. 2009). *Regular_Archigos*
Urban population	Urban population, percent of total (World Bank 2007).
West Europe	Dummy (coding by authors).

Table A2:
Descriptive Statistics: Crossnational Data

Variable	Obs	Mean	SD	Min	Max
Africa	60987	0.20	0.40	0.00	1.00
Catholic	44082	32.12	35.88	0.00	99.10
Competition (AP)	3308	0.49	0.38	0.00	1.00
Competition (Vanhanen)	13840	22.45	24.97	0.00	70.00
English legal origin	43255	0.33	0.47	0.00	1.00
European language	46146	0.30	0.42	0.00	1.06
GDP per cap (Maddison)	11956	7.27	1.52	-2.25	10.89
Island	44496	0.29	0.46	0.00	1.00
Land area (ln)	42944	10.95	2.92	0.67	16.61
Landlock	44496	0.17	0.38	0.00	1.00
Latin America	44289	0.10	0.30	0.00	1.00
Latitude (ln)	43050	-1.58	0.93	-4.50	-0.22
Linguistic fract	41393	0.39	0.27	0.00	.92
Middle East	44082	0.09	0.29	0.00	1.00
Muslim	44082	21.89	35.32	0.00	99.90
Oil production per cap	7973	0.05	0.32	0.00	5.83
Political rights (FH)	5919	3.91	2.25	1.00	7.00
Polity2	22085	-2.49	6.99	-10.00	10.66
Polity2, binary	22085	0.23	0.42	0.00	1.00
Polity2, trichotomous	22085	0.72	.79	0	2
Diffusion	44268	-0.06	0.26	-1.43	2.33
Population density (Banks)	14385	2790	14927	5	330000
Population (huge)	19081	0.04	0.19	0.00	1.00
Population (large)	19081	0.26	0.44	0.00	1.00
Population (small)	19081	0.46	0.50	0.00	1.00
Population (ln)	19081	15.07	2.01	7.17	20.99
Protestant (%)	44082	14.05	24.73	0.00	98.00
Religious fract	43876	0.44	0.23	0.00	0.86
State formation	6309	74.00	44.86	1.53	456.09
Trade/GDP	6309	74.00	44.86	1.53	456.09
Trend	62158	1904	60.22	1800	2010
Turnover (Archigos)	13084	0.12	0.32	0.00	1.00
Urban population	9480	47.75	25.23	2.00	100.00
West Europe	44496	0.11	0.31	0.00	1.00

Variable Definitions and Descriptive Statistics: United States

Variable	Definition *variablename*
Black	Black population as percent of total. *black_perc*
College	Percent of population above 25 with bachelor's degree. *education_college*
Competition	100 - margin of victory between top two vote-getters. Calculated by authors. *competition*
Ethnic fract	1 - summation of the square of each ethnic group's proportion. Calculated by authors. *ethnic_hetero_alesina*
High school	Percent of population above 25 with high school degree. *education_highschool*
Income per cap (ln)	Personal income per capita, natural logarithm. *incomepc_ln*
Non-English	Proportion who speak another language and speak English less than very well. *nonenglish_perc*
Other	Other race as percent of total. *other_perc*
Population (ln)	Population, natural logarithm. *population_ln*
Turnover	1 if incumbent party lost, 0 otherwise. Coded by authors. *turnover_party*
Urban	Urban population as percent of total. *urban_perc*
White	White population as percent of total. *white_perc*

Note: Sources described in the text.

Variable	Obs	Mean	SD	Min	Max
Black	74425	0.11	0.18	0.0003	0.98
College	74425	0.18	0.10	0.02	0.64
Competition	74425	53.92	35.92	0.00	100
Ethnic fract	74425	0.26	0.18	0.006	0.87
High school	74425	0.29	0.13	0.03	0.92
Income per cap (ln)	74425	9.89	0.32	7.24	11.49
Non-English	74425	0.11	0.13	0.001	0.86
Other	74425	0.04	0.07	-2.98E-08	0.75
Population	74425	11.08	1.08	8.97	19.49
Turnover	69164	0.14	0.35	0.00	1.00
Urban	74425	0.73	0.29	0.00	1.00
White	74425	0.80	0.21	0.008	1.00