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Democratization

The State of the Art

Second revised edition

Barbara Budrich Publishers
Opladen & Farmington Hills 2007

strengths of small-N methods (the generation of rich data, the sensitivity to the unfolding of processes over time, the focus on causal mechanisms) and of large-N methods (the emphasis on systematic cross-case and over-time comparison, the concern with generalizability, the formulation of precise estimates of causal effect and statistical significance). There are good reasons, however, why such a multi-method approach is hard to use in practice. Thus, the need for large-N data sets on key, processual variables, and for statistical analysis based on stronger research designs, was discussed. And some suggestions concerning small-N research projects that are most likely to yield important benefits were presented. Finally, the hope that a multi-track approach would give way to a genuine multi-method approach was expressed.

In sum, the field of democracy has made significant strides but still faces important challenges. In this sense, it constitutes an exciting research agenda. Researchers on democracy have opened up and continue to open up new substantive agendas and have generated some important findings. Moreover, the issues addressed by this field of study put it in dialogue with some of the main debates about theory and methods in comparative politics. Students of democracy focus consistently on core issues of modern politics, the conflict over how the power of the state is accessed and used. In turn, the study of democracy is a site of important methodological innovations and a substantive field where a range of methodological issues have come into sharp focus. In short, the study of democracy is a vibrant research program.

4. Determinants of Democratization: Taking Stock of the Large-*N* Evidence

Jan Teorell and Axel Hadenius

4.1. Introduction

Since the third wave of democracy peaked some 10 years ago, large-*n* studies of the determinants of democratization have expanded across space and time, covering more countries and longer time periods. In terms of the theories tested and variables employed, however, most analyses have been highly specialized, focussing on the effects of but one or a few major explanatory factors. A large number of studies have assessed the effect on democracy of economic development and socioeconomic modernization (Burkhart & Lewis-Beck 1994; Helliwell 1994; Londregan and Poole 1996; Barro 1999; Przeworski et al. 2000; Boix & Stokes 2003; Epstein et al. 2006), whereas others have largely focused on the impact of economic crises (Gasiorowski 1995; Bernard et al. 2001, 2003), resource wealth (Ross 2001), colonial heritage (Bernard et al. 2004), or international factors such as globalization (Li and Reuveny 2003; Rudra 2005) and diffusion effects (O'Loughlin et al. 1998; Starr and Lindborg 2003; Brinks & Coppedge 2006).

Without denying the merits of specialization, we shall argue that this large-*n* literature has serious limitations. To begin with, the results pertaining to each determinant of democratization may be incorrect if the assessment is not performed in the context of all relevant controls. In other words, specialization may lead to erroneous conclusions even with respect to the one or few explanatory variables under study.

Second, and equally important, these previous studies do not address the question of how far all hypothesized determinants together can take us in explaining movements to and from democracy. The latest most comprehensive large-*n* study even reached the conclusion that, whereas democratic survival "is quite easily predictable", transitions to democracy appear to be explained by chance factors (Przeworski et al. 2000, 137). If that really proves to be the case, it would lend support to the anti-structural, actor-oriented, "no preconditions"-approach to democratization proffered in particular by Rustow (1970) and O'Donnell and Schmitter (1986), an approach which played a key part in the "transition paradigm" recently proclaimed dead (Carothers 2002). Apart from the distinction between transitions toward

or away from democracy, however, the question of overall explanatory performance could critically hinge upon the time perspective applied. What may appear unpredictable and erratic in the short-run sometimes turn out to be stable and predictable in the long-run. As a matter of fact, O'Donnell and Schmitter were themselves well aware of this in that they did not deny "the long-run causal impact of 'structural' (including macroeconomic, world systemic, and social class) factors". Their assertions regarding the non-structural determinants of democratic transitions only concerned short-term dynamics (1986, 4–5).

In this chapter we propose to remedy the problem of specialization in the large-*n* literature on democratization by drawing on a considerably expanded range of available cross-sectional time-series data. Using a combination of two predominant democracy indices, we purport to explain variations in democracy over time across 142 countries over the period 1972-2000. These analyses break new ground on several accounts. First, in terms of the range of explanatory variables entered into our models, we outperform most, if not all, earlier studies in the field. Second, we present some novel findings pertaining to factors hitherto not tested on a global scale. This particularly concerns the democratizing effects of mass protest, a posited determinant of democratization which hitherto has attracted limited attention in large-*n* studies. Third, although we deploy a graded measure of democracy, we make an effort to test whether different determinants affect movements in different directions along the democracy scale. In other words, we endeavour to separate the effects on movements towards as well as reversals from the democratic end of the graded scale. Fourth, we systematically explore the effects as well as the overall predictive performance of these determinants in both the short-run and long-run perspective. To the best of our knowledge, the third and fourth endeavors have never before been systematically undertaken.

Our results indicate that the most important determinants of democratization or the lack thereof are the share of Muslims in the population, the degree of religious fractionalization, country size, the level of socioeconomic development, natural resource abundance in terms of oil, trade dependence, short-term economic performance, democratic diffusion among neighboring states, membership in democratic regional organizations, and the frequency of peaceful anti-government demonstrations. Taken together, however, these determinants display a strikingly poor explanatory performance in the short-run; this particularly concerns models of reversals toward authoritarianism, but applies for movements toward democracy as well. Yet in the long-run perspective the explanatory performance can be deemed fairly satisfactory. Thus, what were considered to be well-established structural predictors of democracy, these do not take us very far in understanding short-term changes. They do however help explain the long-run equilibrium levels of democracy towards which countries gravitate.

The chapter is organized as follows. We start by reviewing the previous research on explaining democratization. We then present our research design, followed by the results. We conclude by summing up and discussing implications for future work in the field.

4.2. Explaining Democratization

Theories purporting to explain why some countries develop and sustain democratic regimes whereas other remain or become authoritarian have not been cast in a single mold (see also chapter 1 above) .At least four theoretical approaches may be distinguished in the literature. The first, which we shall focus on here, is the structural perspective, seeking to locate the most significant triggers of democratic advancement outside the immediate reach of human agency: in the economy, in society at large, or in the international environment. Lipset (1959) stands out as the most important forerunner of this tradition. A second approach, which we have already touched upon, is the strategic approach, also dubbed the "transition paradigm" (Carothers 2002). According to this view, the installation of a democratic regime is largely explained through a process of strategic elite interaction, where the indeterminate process of democratization itself in large parts explains its outcome (see, e.g., Rustow 1970; O'Donnell and Schmitter 1986; Casper and Taylor 1996). A third approach, emanating from the work of Moore (1966), is the "social forces" tradition (Bellin 2000). By locating the origins of democracy in organized interests and collective action in society, this approach blends structural with actor-centric perspectives (see, e.g., Rueschemeyer et al. 1994; Collier 1999). Fourth, in recent years a new approach to explaining democratization has appeared. Deploying theoretical tools common in economics, most notably by anchoring macro-level predictions in game theoretical models of self-interested economic micro behavior, this emerging literature has begun to cast new light on the determinants of democratization (see, in particular, Boix 2003; Acemoglu and Robinson 2006).

We should make it clear from the outset that this review does not purport to pay equal attention to all these theoretical traditions, nor all hypothesized determinants of democratization. Our focus is limited to explanatory factors that have been put forward within the structural tradition, and that are amenable to testing in a cross-sectional time-series setting. We distinguish among three types of determinants of democratization to be reviewed below: domestic economic, domestic social, and international factors.[1]

1 We do not cover institutional determinants, neither democratic institutions such as electoral systems or forms of government, nor authoritarian institutions such as types of dictatorship. The reason is that these institutions are endogenous features of the two systems we want to explain shifts to and from. In a recent paper, however, we find that some types of authori-

We are thus not interested in testing or developing a particular theory of democratization, but in assessing a broad range of theories or empirical regularities that emerge from previous large-*n* studies on the topic. Having said this, we will in a way pay attention to the non-structural perspectives, too. The strategic approach will indirectly be assessed in terms of the residual variance in democratization that our long list of structural determinants does not explain. The social forces tradition will be assessed in terms of one of its key predictions: that democratization occurs as the response to large-scale popular mobilization. The economic approach, finally, will in the concluding section serve to illustrate the need for a more integrated theory of democratization to be developed in the future.

4.2.1. Domestic Economic Determinants

Since the seminal article by Lipset (1959), there have been countless studies confirming that one of the most stable determinants of democracy across the globe is the level of socioeconomic modernization. For the most part, this empirical support has been based on measures of modernization in terms of economic development, such as energy consumption and GDP per capita. This pertains both to earlier cross-sectional studies (for an overview, see Diamond 1992) and to the more recent tests based on pooled time-series data (Burkhart & Lewis-Beck 1994; Helliwell 1994; Londregan and Poole 1996; Gasiorowski and Power 1998; Barro 1999; Przeworski et al. 2000; Boix & Stokes 2003; Epstein et al. 2006). In Lipset's (1959) original account, as well as in the early studies following in its wake (Cutright 1963; Neubauer 1967; Olsen 1968; Winham 1970), however, a much wider range of indicators of socioeconomic development was employed. Apart from national income they included industrialization, education, urbanization and communications. According to modernization theorists these developmental processes should be viewed as parts of one underlying syndrome, socioeconomic modernization, which eventually enhances democratic development (Lerner 1958; Deutsch 1961). This broader theoretical underpinning for the Lipset hypothesis has received surprisingly little attention by the more recent comparative democratization literature. In this chapter we try to remedy this situation by treating socioeconomic modernization as a coherent syndrome with multiple observable indicators.[2]

tarian institutions, most notably limited multi-party systems, appear to enhance the prospects for democratization (Hadenius and Teorell 2007).

2 A particular version of the modernization hypothesis is Inglehart and Welzel's (2005) cultural theory of democratization. Apart from not being amenable to testing on time-series cross-section data, we question the tenacity of this theory elsewhere (Hadenius and Teorell 2005b; Teorell and Hadenius 2006).

We also attempt to reassess the widely cited finding by Przeworski et al. (2000) that socioeconomic modernization does not trigger transitions to democracy, but instead helps to sustain democracies once installed. This finding has been amply criticized on empirical grounds (Boix and Stokes 2003; Epstein et al. 2006), but rarely without clinging to a discrete measure of dictatorships and democracies.[3] By separating the effects on upturns from downturns, as discussed below, we perform a systematic test of this finding using a graded democracy scale.

A theoretical argument that has developed alongside the modernization hypothesis has been concerned with the impact of economic performance (Haggard and Kaufman 1995, 1997). The large-*n* empirical support for this contention has mostly been based on yearly growth rates as the measure of performance, and on dichotomous conceptions of the dependent variable, basically indicating whether regimes are authoritarian or democratic. Two findings have been predominant. On the one hand, that growth is negatively related to transitions from authoritarian to democratic rule, or, inversely, that authoritarian regimes fall under the pressure of economic crisis (Gasiorowski 1995; Remmer 1996; Przeworski et al. 2000). On the other hand, growth has been shown to positively affect democratic survival, implying that democracies too are vulnerable to economic crises (Przeworski et al. 2000; Bernard et al. 2001, 2003). These results do not translate easily into contexts where graded measures of democracy are being used. They could however imply that the coefficients for economic performance should be differently signed depending on the direction of change in the democracy scale, which might explain why the few studies that have tested them on graded measures have produced weak and inconsistent results.[4]

A more robust finding appears to be the anti-democratic effect of natural resource abundance. In a set of regressions predicting the development of democracy over time, Ross (2001) found that both the abundance of oil and of other non-fuel minerals as the primary sources of national exports had a markedly negative effect on the prospects for democratization. Earlier studies purporting to show the negative impact of oil had only made cursory remarks on the ill-performance of democratic governance in a few oil producing countries on the Arabian Peninsula (Helliwell 1994; Barro 1997, 1999). Ross (2001), by contrast, was able to show that the effect occurred on a global scale, and pertained to other sources of strongly profitable materials. Ac-

3 The one exception we are aware of is Acemoglu et al. (2005), who (in Table 12) make use of the same technique as we in order to separate the effects of transition toward and away from democracy.

4 Using the same democracy index (Polity), but different controls, Londregan and Poole (1996) found a negative but small short-term impact of growth on democratization, whereas Li and Reuveny (2003) found no effect of growth but a positive effect of inflation that decreased over time.

cording to Ross the relationship is due to the development of a "rentier state" in countries rich in natural resource wealth. Regimes that are predominantly reliant on such vast resources are capable of using both the carrot (tax cuts and patronage) and the stick (repression) to hold contestation at bay.

4.2.2. Domestic Social Determinants

Apart from the economic factors, a large number of other domestic determinants have been suggested in the literature. One is the sheer size of a country's population. There is an old school of thought arguing that democracy should be more likely to prosper in smaller countries. Another well-established presumption is that democracy's prospects are dimmed by social heterogeneity. Religiously or ethnically diverse societies, the argument goes, are more prone to intercommunal conflict and hence are less likely to democratize (see, e.g., Hadenius 1992, 112-4, 122-5; Fish and Brooks 2004).

A longstanding debate concerns the effects of colonialism on a country's prospects for democracy. This literature has pointed to the fact that colonialism has been associated with underdevelopment and high levels of social fractionalization, which in turn impede democratic development. Usually the effect of colonialism is not assumed constant across different colonial powers. Most importantly, a British colonial legacy has been assumed more conducive to democracy than the effect of other colonizers. On most accounts the Britons supposedly were better at nurturing self-government and a more independent civil society in their colonies (Bernard et al. 2004, 227-32).[5]

Yet another non-economic determinant of democracy is religious tradition. Various scholars have asserted that Catholicism, Orthodox Christianity, Islam and Confucianism should be expected to negatively impact on the prospects for democracy, whereas Protestantism should be positively linked with democracy. According to Lipset (1993, 5), "These differences have been explained by (1) the much greater emphasis on individualism in Protestantism and (2) the traditionally close links between religion and the state in the other four religions".

In a recent study Barro (1999) tested the effects on democratization of these non-economic determinants, once the level of socioeconomic development was controlled for. He found only a marginally significant negative effect of ethnolinguistic fractionalization, and no effect of country size or

5 In two much-cited articles Acemoglu et al. (2001; 2002) argue that colonial origins determine a country's institutional quality and long-run levels of growth. There are two reasons why we do not address this theory. First, Acemoglu et al. only purport to explain variations among former colonies, whereas our assessment includes non-colonies as well. Second, Acemoglu et al. do not discuss different legacies of the colonizing countries, which is what the democratization literature on colonialism has been concerned with.

colonial history. The only significant predictor in this set of variables turned out to be the size of the Muslim population, which had a markedly negative impact. A negative impact of the size of the Muslim population was also found by Ross (2001), but vanished once he introduced a dummy for countries residing in the Middle East and Africa. Stepan and Robertson (2003, 2004) also urge us to rethink the seemingly negative impact of Muslim majority countries in terms of a contextual effect peculiar to the Arab world.

Before leaving the domestic scene, we shall take note of a possibly more proximate trigger of democratic transitions operating at the societal level: popular mobilization. In the founding texts of the transition literature, mainly derived from the experience of democratization in Southern Europe and Latin America, the analytical focus was almost entirely directed at the elite level. Democracy in these countries appeared to have been brought about in the context of demobilized masses (O'Donnell and Schmitter 1986). Although this view has been challenged empirically in more recent accounts of the same region (Bermeo 1997; Collier 1999), the contrast still seems sharp in relation to the subsequent collapse of authoritarian regimes in Eastern Europe and Sub-Saharan Africa. In these instances, collective action on behalf of the mass public appears to have been a widely occurring phenomenon, with allegedly democracy enhancing effects (Bratton and van de Walle 1997, 83f.; Geddes 1999, 120; McFaul 2002, 222f.; Bunce 2003, 171-8). Anecdotal evidence also suggests that democratization in both Western Europe and Latin America in the early 20th century followed in the wake of social unrest and popular mass action (Acemoglu and Robinson 2006, 67-8, 71-3).

From a theoretical perspective, this is what we should expect if the "social forces" approach to explaining democratization should prove to be correct. Strike activity should thus be one form of popular mobilization predicted to impact on democratization, particularly within the strand of this tradition that emphasizes the importance of organized labor (Rueschemeyer et al. 1994; Collier 1999). But an effect of more general forms of protest activity undertaken by other groups in society, including both violent clashes and peaceful demonstrations, could also be conjectured (Foweraker and Landman 1997; Gill 2000; Wood 2001). Although less attention has been paid to the subject lately, there also seems to be a growing awareness of an older tradition claiming that popular mobilization may not be unreservedly beneficial for democracy (Bermeo 2003; Armony 2004).

In light of these observations there are surprisingly few large-*n* studies of the possible effect that popular mobilization may exert on democratization. To our knowledge only two other global studies relate to the effect of popular mobilization (Lipset et al. 1993; Przeworski et al. 2000), but neither of them makes this assessment in dynamic equations explaining regime change. We thus concur with Coppedge's verdict that "[t]he true impact of political mobilization ... remains an open question" (2003, 125).

4.2.5. International Determinants

There is a large and growing literature on factors impeding or enhancing democratization at the international level. An old school of thought in this regard are the so-called dependency theorists (for an overview, see Hadenius 1992, 91-3). They claimed that international capitalist exchange involving trade and investments favored wealthy international "centers" at the expense of the poor "periphery", which was exploited. In order to maintain such relations democratic rule in peripheral countries is stifled, according to dependency theorists, since authoritarian leaders supposedly are more receptive to the interests of the international economic centers.

However, most of the early cross-sectional tests of the dependency predictions produced weak or inconsistent support. In a recent account—although couched in the language of "globalization", presently more in vogue—Li and Reuveny (2003) tested some of the old predictions in a cross-sectional time-series setting. Interestingly, their results by and large confirm dependency theory. According to their findings, both trade openness and portfolio investments inflows negatively affect democratization. And while foreign direct investment inflows—their third indicator of globalization—had a positive impact, it has weakened over time. They concluded by stating that "the economic aspects of integration into the world economy are beginning to cause a decline in national democratic governance" (2003, 53).

Li and Reuveny (2003), however, found a positive effect of another facet of international dependence: the spread of democratic ideas across countries, or what is usually referred to as democratic diffusion. To systematically assess such external diffusion or demonstration effects with large-*n* data is a fairly novel enterprise. Yet hitherto the evidence has by and large been confirming expectations. Diffusion has been showed to affect democratization both at the most proximate level of neighbour states, at the level of world regions, and at the global level (Starr 1991; O'Loughlin et al. 1998; Kopstein and Reilly 2000; Brinks and Coppedge 2006; Starr and Lindborg 2003; Gleditsch and Ward 2006).

In a recent book, Pevehouse (2005) suggests another potent non-domestic determinant of democratization: regional international organizations. With a mixture of case-study and statistical evidence, Pevehouse demonstrates that homogenously democratic regional organizations can pressure authoritarian member states to undertake democratic reforms, socialize military and economic elites into accepting democratic procedures, and bind newly elected elites in fledgling democracies to these reforms once committed. In this way, membership in democratic regional organizations, according to Pevehouse (2005), both precipitates movements toward democracy and enhances democratic survival.

Most of these studies of international determinants have, however, not assessed the impact of globalization, diffusion and regional organizations net of all other domestic influences of democratization. As should be evident, what appears to be a diffusion linkage between two countries could disappear once possible confounding factors simultaneously affecting democracy in both countries are taken into account. Basically the same goes for economic dependence and shared membership in regional international organizations. In this chapter we try to remedy this by assessing international effects in the context of more fully specified models.

In sum, there is a large literature specialized in different global determinants of democratization. What has gone missing along the road to specialized knowledge is a test of what effects remain in the presence of the full possible set of controls, and an overall assessment of how well all determinants, when taken as a whole, predict movements to and from democracy. Moreover, few studies have tried to separate direct from indirect effects, and short-run from long-run performance.[6] This is exactly the kind of assessment we purport to make in this chapter.

4.3. Data and Research Design

The dependent variable in our study is based on two well-established graded measures of democracy: the average scores of political rights and civil liberties reported by Freedom House (2003), and the revised combined autocracy and democracy scores derived from the Polity IV data (Marshall and Jaggers 2002). A previous study has shown that despite their high inter-correlation the democracy indices reported by Freedom House and Polity may produce different results (Casper and Tufis 2003). Whereas Munck and Verkuilen (2002) limit their discussion to the methodological strengths and weaknesses of these indices (see also chapter 2), we show in a recent paper that both are actually subject to systematic measurement error. Polity tends to underestimate the limits of political freedoms, whereas Freedom House underestimates the freedom and fairness of elections. To mitigate these tendencies, it makes sense to combine the two indices (Hadenius & Teorell 2005a). Hence, we first transform the Freedom House and Polity to vary between 0 ("least democratic") and 10 ("most democratic"), and then average them.

Using this combined democracy index implies both space and time limitations. In terms of time, the Freedom House data only pertain to the period

6 Partial exceptions are Barro (1999) and Londregan and Poole (1996). Barro computes the long-run forecasts of democracy for each country, but without reporting the long-run parameters. Londregan and Poole (1996), by contrast, report the long-run effect parameters but without computing the equilibrium democracy levels. None of them assess the long-run predictive performance.

from 1972 onwards. In terms of space, the Polity scores only cover countries with a minimum population of 500,000 in 2002. After taking missing data in the explanatory variables into account, this leaves us with a data set of 2628 annual observations in 142 countries of the world from 1972-2000.[7]

The results reported below are based on regression analysis, using yearly changes in the combined democracy index as dependent variable, and a series of measures of potential determinants of democratization as independent variables (see Appendix A for a detailed description of the variables). Although we give a more detailed account of the statistical model used in Appendix B, we would like to highlight some critical features of this model here.

First, we make important use of the temporal dimension of the data. To begin with, for all years we include our measures of the independent variables from the year before the dependent variable is measured. This is done in order to mitigate the problem of "reversed causation", that is, that the explanatory variables at least in part are also being caused by the dependent variable. We also include measures from previous years of the dependent variable itself in the model. There are both theoretical and methodological reasons for this. Substantially it makes sense to include this control for the past experience of democracy in a country, since democracy is a fairly sticky phenomenon: neither democracy nor autocracy is invented anew each year in every country. There is a lingering presence of the past, or "path dependence": having democracy (or not) today positively impacts on the incidence of having democracy (or not) tomorrow. Methodologically, the presence of this control most importantly "proxies" for a host of other potential determinants of democratization that cannot be measured but still might have affected a country's level of democracy at earlier time points.

The inclusion of previous levels of democracy in the model is also the key to our distinction between short-term and long-term effects and explanatory performance. Since the level of democracy in the previous year represents all changes in the dependent variable up until that time point, our direct estimates of the effects of all other explanatory variables only pertain to the change in democracy over this last year. This is our definition of a short-term effect. Democracy being a sticky phenomenon, however, the effect of a change in one independent variable also makes itself felt in more years to come. If the system of government in a country is perturbed by a shock in a given year, say a deep recession, then the effect of this shock will slowly dissipate, being strongest at the outset and then slowly loosing its strength over the years. The sum of all these yearly effects of a hypothetical change in a given independent variable is our definition of a long-term effect. How long

7 With respect to countries that have merged or split during the period of observation, we treat Germany as a continuation of West Germany, and Ethiopia as a continuation of itself before the secession of Eritrea.

it takes for such an effect to reach its limit depends on the degree of stickiness in the dependent variable, and is thus an empirical question. According to our estimates in the analyses that follow, it takes approximately 40 years for the full long-run effects in our models to occur.

In order to compare the explanatory performance in the short-run and the long-run, we make use of this same distinction in time horizons. In order to assess short-term performance, we simply compute a standard measure of model fit, such as the explained variance. This measure compares the predictions our model yields over a one year period to the actual yearly change in the level of democracy. In order to assess long-term performance, we must instead compare the actual level of democracy in a given year with the level of democracy that would ensue if all variables were allowed to experience their full long-run effects. We may think of this later state as the long-run equilibrium to which a country is attracted. The question of long-run explanatory performance then pertains to how far from this long-run equilibrium the level of democracy is in each country.

Finally, we take a simple approach to comparing the effects on movements toward and reversals away from the democratic end of the scale. Since the yearly change in level of democracy may be either positive (upturns) or negative (downturns), we simply run the same analysis after having set all downturns to zero in order to estimate the effect on upturns, and by setting all the upturns to zero in order to estimate the effects on downturns. By comparing these results together with the result when both upturns and downturns are considered jointly, we draw conclusions as to whether a particular determinant exerts most of its influence in either or both directions.

4.4. Results

In order to save space and avoid too many technicalities, we will in this chapter not present any numerical coefficients or other statistical quantities of interest (these are available from the authors upon request). Instead we summarize our main findings in Figure 5.

4.4.1. Social and Demographic Determinants

Turning to our first determinant of democratization, religious denomination, our results show—contrary to the earlier literature summarized by Lipset (1993), but in line with Barro's (1999) findings—that there are no significant effects on democratization of different forms of Christianity. In other words, Protestant countries have no democratizing advantage. By contrast, societies dominated by Muslims have an evident anti-democratic propensity. This ef-

fect is fairly substantial. If we compare two hypothetical countries, one with 100 and one with 0 percent Muslims, the Muslim dominated country will have an estimated democratization rate of .311 less in the short-run, and a long-run equilibrium level of 3.47 less on the 0-10 democracy scale. In terms of the direction of change, it appears that Muslim societies are both significantly less likely to make upturns towards democracy, and significantly more likely to make downturns towards authoritarianism. Even more importantly, the negative impact of Islam holds even as we control for regions, that is, taking into account the difference between Middle Eastern and North African countries and the rest of the world. Thus, *pace* Ross (2001) and Stepan and Robertson (2003, 2004), the Muslim gap according to our estimates is not merely an "Arab" gap.[8]

Figure 5: Summary of robust statistical findings.

	TRIGGERS	IMPEDIMENTS (to)
Upturns	Neigbour diffusion Regional organizations Peaceful demonstrations	Muslim population Religious fractionalization Size Oil Trade dependence
Downturn	Muslim population Religious fractionalization Economic crisis	socioeconomic modernization

Having said this, we are the first to admit that we do not know why the Muslim effect appears. Fish's (2002) suggestion that the anti-democratic effect of Muslim countries would be due to female subordination finds no support in our data (cf. Donno and Russett 2004).[9] Moreover, cultural interpretations falter when checked against individual-level data, mostly showing that Muslims, if anything, are relatively *more* supportive of democracy than other people (Tessler 2002; Norris and Inglehart 2004; Hofmann 2004). While statistically robust, then, the Muslim effect currently lacks an intelligible explanation.

8 We get the same finding as Barro (1999) with respect to the fraction of *non*-religious people in the population: although this factor at first seems to exert a significantly negative impact on democratization, the result vanishes once China, an extremely influential outlier, is excluded from the analysis. We also find that the proportion of orthodox Christians has a positive effect when only downturns are considered, but this effect is also due to the influence of one single outlying country: Cyprus.

9 We tested this by including the ratio of secondary school enrollment among women over men. This factor, if added to our range of determinants, is miniscule and not statistically significant in itself, and does not reduce the Muslim effect.

Yet religion also impinges on democratization in another way: the degree of religious *fractionalization* has a clearly significant and negative impact. Net of other influences, the more the population of a country is split among different religious denominations, the weaker are its chances to democratize and the larger is the risk that democracy will falter.[10] The impact of religious fractionalization in model (2) means that if a country would make the hypothetical move from having a population of a single religious denomination (perfect homogeneity) to one where each individual has his or her own denomination (perfect heterogeneity), the yearly democratization rate would decrease by −.194, whereas the long-run equilibrium level of democracy would be shifted downward by 2.17 on the 0-10 democracy scale. Countries with a population that is heterogeneous in terms of its ethnolinguistic composition, by contrast, are not significantly more prone to move in either direction on the democracy scale.[11]

Why are religiously heterogeneous societies less prone to democratize? Fearon and Laitin (2003) find that neither religious nor ethnolinguistic fractionalization increases the risk of civil war. Thus, the hypothesized link between fractionalization and resistance to democratization running through inter-communal conflict does not hold water. What the mechanism then might be is again an area worthy of further study.

With respect to colonial heritage we find, in line with Barro (1999), that democracy has not fared significantly better in former British colonies than in countries of other colonial origin. Nor do we find that there is any general negative democratic legacy of being a former Western overseas colony. Colonial heritage simply does no add to our understanding of third wave democratization.[12]

10 Although the general effect of religious fractionalization is statistically significant, its effect on upturns is only marginally significant (p=.077) and its effect on downturns is insignificant (p=.129). The effect magnitude is however similar for its effect on both upturns and downturns (around −.10 in both cases). We interpret this as evidence that religious fractionalization has a general effect driven both by upturns and downturns, but that this effect is underestimated when these two directions of change are considered separately.

11 Ethnolinguistic fractionalization (ELF) has a somewhat ambiguous effect when upturns and downturns are assessed separately: the effect is positive (!) on upturns, but negative on downturns; both these effects are however only marginally significant (p=.073 and .089, respectively).

12 There is one minor exception: former Spanish colonies have had significantly larger downturns in their democracy scores. It turns out, however, that if we enter the effects of colonial legacy without controlling for religion, the effect of being a former Spanish colony is miniscule and no longer significantly different from zero. Being confined to Latin America, the religious composition of the former Spanish colonies is of course almost exclusively Catholic, undisputedly a heritage from their colonial past. This situation is markedly different in the former French and British colonies, where the religious denomination of the colonizers (Catholicism and Protestantism) has left a relatively small imprint on society. In sheer numbers, the mean fraction of the population being Catholic is .91 in the Spanish colonies of our estimation sample, whereas the corresponding figure is .15 in the French colonies, and only

As opposed to Barro (1999), however, we find that small size—measured as the log of population—has a significant and positive, although not very substantial, impact on democratization. What drives this result turns out to be that fact that smaller and medium sized countries have had somewhat larger upturns in their democracy scores compared to larger countries. There is however no net association between size and downturns.

4.4.2. Modernization, Resource Wealth, and Economic Performance

In terms of economic determinants, we first replicate the finding from some 50 years of comparative research on the positive relationship between socio-economic modernization and democratization. It should be noted, however, that our result is based on a composite measure of the entire process of modernization, not only one of its macroeconomic sub-components. A standard deviation change in the modernization index—which is approximately equivalent to a move from the level of Somalia (at the very bottom) to the level of Namibia, or from El Salvador (at the mean) to the level of Ireland—results in an expected increase of .082 in the level of democracy the following year. The same shift amounts to about a unit increase in the long-run equilibrium level of democracy.

Interestingly, moreover, the effect of modernization according to our estimates is *not* propelled by a tendency among modernizing countries to advance towards democracy. Rather it is the tendency among less modernized countries to revert towards authoritarianism that drives the result. In other words, whereas we find a significant impact on (the absence of) downturns, we find no such relationship with respect to upturns. This pattern bears a striking resemblance to the finding by Przeworski et al (2000) that socio-economic modernization does not effect transitions to democracy, but hinders reversals to authoritarianism.

When looking at short-term macroeconomic performance, we find no effect of inflationary crisis on regime change. Recessionary crisis (measured as the yearly growth rate), however, basically performs according to expectations. Whereas growth recessions have a positive impact on upturns toward democracy, they also trigger downturns. The former effect (on upturns) is

.12 for Protestants in the British colonies. By way of comparison, the mean fraction of Muslims is 0 in the Spanish colonies, .35 in the British and .53 in the French colonies. As a result, there appears to be no net effect of Spanish colonialism after all. Although the Spanish colonial power left conditions in its wake that—net of all other influences—have negatively impacted on democratization (presumably unfavorable social and institutional conditions), they also left a religious composition that has enhanced democratic development to such extent that this negative impact is leveled out by now.

however weaker and only marginally significant. These results thus most closely confirm Gasiorowski's (1995) findings, that recessionary crises more strongly affect democratic breakdown than transition to democracy.

Despite the fact that we control for a much larger set of determinants, our results confirm Ross' (2001) findings on the anti-democratic effect of oil. According to our estimates the discovery of an oil find increasing the export share of oil from 0 to 100 percent of GDP would lead to an expected decrease of .323 in the level of democracy the following year, and to a downward shift of 3.61 in the long-run equilibrium level of democracy. This effect is primarily caused by a much larger share of upturns in the level of democracy among countries not dependent on oil relative to oil-rich countries. Probably due to the fact that so few oil-rich countries have reached higher levels of democracy, the effect of oil as a trigger of downturns is weak and insignificant.

Whereas we thus confirm Ross' primary finding with respect to the effect of natural resource wealth, there are two qualifications. The first is that we do not find any significant negative impact of non-fuel metals and ores. The second is that the oil effect is in our data more restricted to the Middle Eastern region. Although a substantial negative effect of oil on democratization remains after controlling for world regions, the effect is weakened (the short-term coefficient being -.278) and, more importantly, only marginally significant (p=.074).

4.4.3. International Determinants

Turning to international determinants of democratization, our results partly confirm the finding by Li and Reuveny (2003) that openness to trade impedes on democratization. We find no general effect of trade, however, but only when upturns in the level of democracy are being assessed.[13] This effect is primarily due to a relatively large share of democratic upturns among less or intermediately trading countries. This pattern would thus at face value seem to confirm the old prediction by dependency theory that largely trade dependent countries are hindered from democratizing. However, the assumption key to this theory—that the negative impact on democracy is due to trade with the international "centers" of the world system—does not hold water in our data.[14] In other words, this is yet another finding in want of theoretical explanation.

13 Although there is a significantly positive effect of trade when only downturns are being considered, this effect is solely due to two extremely influential outliers: Ghana in 1981 and Turkey in 1980 (that is, two relatively trade independent countries facing a military coup).

14 We tested this hypothesis by controlling for bilateral trade share, as a fraction of GDP, with the US, the UK, France, China and Soviet Union/Russia (based on data from Gleditsch

Moreover, larger integration into the World economy in terms of gross capital flows does not impact on democratization. Apart from being inconsistent with dependency theory, this contradicts Li and Reuveny's (2003) findings for both foreign direct investment and portfolio flows. Since both these forms of dependency measures are lumped together in our variable for gross capital flows, it would be worth further study to try to disentangle their effects with a more fully specified model and on a fuller sample of countries.

Our next set of international determinants aim at capturing diffusion effects—the spread of democracy or autocracy from one country to another. Of the three spatial levels included, only the most geographically proximate appears to have an effect. If the mean level of democracy among neighbouring countries is shifted upward one unit between time t–2 and t–1, the net expected change in democracy at time t is .139. The long-run equilibrium level of democracy, moreover, is increased by 1.55. This implies a fairly tight long-run adjustment of the levels of democracy among neighbouring states. At the regional and global level, however, there seem to be no diffusion effects at work net of other influences. In this regard our results differ from the existing literature on diffusion effects (see, e.g., O'Loughlin et al. 1998; Starr and Lindborg 2003), the probable reason being our more fully specified explanatory models.

Interestingly, our results confirm one key prediction of Pevehouse's (2005) argument on the importance of regional organizations: membership in relatively democratic regional organizations precipitates upturns in the level of democracy of a country. This is a noteworthy finding in light of the fuller set of determinants of democratization taken into account by our model. We find no support, however, for the flip side of Pevehouse's argument: that regional organizations also help democracies survive. In terms of our empirical strategy for assessing this, we find no effect of regional organizations on downturns (neither do we find a general effect when both upturns and downturns are being assessed jointly).

4.4.4. Popular Mobilization

Turning to the last group of determinants entered in our model, we are able to present some novel insights into the role played by popular mobilization. Confirming expectations, large numbers of peaceful anti-government demonstrations facilitate upturns toward democracy. It should be kept in mind that this variable, much as all the other time-varying determinants tested, is lagged one year. What we observe is thus *not* an upsurge of popular protest

2002). We found no significant effect of any of these five trading variables, and no change in the general trading variable once these sources of trade were controlled for.

that is an integral part of the democratization process. What is captured is instead the impact of popular mobilization in one year on the propensity to democratize the *following year*, all else being equal, which lends support to a causal interpretation of its impact. This estimated short-run increase in the rate of democratization is .030 per demonstration, whereas the long-run equilibrium level of democracy is increased by .340 per demonstration. This confirms, on systematic evidence, the observation referred to above by numerous observers of democratic transition processes in Eastern Europe, Sub-Saharan Africa, and even Latin America that popular mobilization played a more influential role for the outcome than "transition paradigm" theorists initially acknowledged (as they believed the process was mainly elite driven).

However, we do not observe homogeneous effects of all forms of popular mobilization. Neither riots (i.e., violent clashes involving the use of physical force) nor strikes aimed at national government policies or authority exert any impact on democratization. Thus, although the effect of demonstrations is consistent with the more general "social forces" approach to explaining democratization, we find no systematic evidence in favour of a special role played by labour through the organization of strike activity (Foweraker and Landman 1997; Collier 1999). Moreover, *pace* Bermeo's (2003) insightful analysis of the Latin American experience of the 1970s, no form of popular mobilization appears to work as triggers of downturns towards autocracy.

4.4.5. Explanatory Performance

We now turn to the question of how well these determinants, when taken together, explain the incidence of democratization. It is easily verified that the predictive performance of the short-run model is fairly modest. The R-squared reaches some 11 percent explained variance at its best. Interestingly, there is a large difference in explanatory performance of upturns versus downturns. Whereas the explained variance in upturns reaches some 13 percent at best, the corresponding figure for downturns is only 6 percent. In other words, although our model fairs rather poorly in both instances, it does a better job at explaining short-term movements upward on the democracy scale than at explaining movements downwards. This result clearly contradicts the pattern found by Przeworski et al. (2000) that transitions toward democracy are more or less a random process, whereas reversals to autocracy may be more easily predicted. The most likely reason for this difference is that we include a much broader array of explanatory factors that mainly affect the upturns, such as oil, regional organizations and popular mobilization.

More importantly, by looking at explanatory power in the long run the picture radically changes. In the full model the R-squared reaches its long-run

maximum of 64.5 percent explained variance.[15] This means that the actual level of democracy on average comes fairly close to the long-run equilibrium level determined by the explanatory variables. In sum, whereas our models explain little of the short-term dynamics, they fare considerably better in explaining regime change in the long-term.

4.5. Conclusions and Discussion

To sum up, neither economic, societal nor international determinants of democratization trump each other unequivocally in terms of explanatory performance. What we find is that a mixture of these different types of explanatory factors is needed in order to explain democratization. The most important impediments to democratization appear to be a large Muslim population, a high degree of religious fractionalization, natural resource abundance in terms of oil, and heavy dependence on trade. Although the evidence is somewhat weaker, we also find larger countries to have a smaller likelihood of moving towards democracy. Democratization is instead enhanced by democratic diffusion among neighboring states, membership in democratic regional organizations, and popular mobilization in terms of peaceful demonstrations. Socioeconomic modernization primarily works as an impediment to downturns, implying that more modernized countries are more likely to uphold the level of democracy already achieved. Short-term economic crisis also mostly works as a trigger of downturns toward autocracy. Finally, factors appearing to have no imprint on the incidence of democratization include: colonial origin, the form of non-Muslim religion, natural resource abundance in terms of minerals, gross capital flows, democratic diffusion at the regional and global level, inflation and popular mobilization in terms of riots and strikes.

Overall, these determinants perform rather poorly in explaining short-term democratization. This result is well in line with the uncertainty and unpredictability so much stressed by O'Donnell and Schmitter (1986) and other adherents of the "no preconditions" paradigm. It also seems to support the chance argument as far as transitions go proffered by Przeworski et al (2000), although our results indicate that, albeit still at a low level, upturns are more easily explained than downturns.

15 We reach the same conclusion by looking at another measure of fit, the standard error of regression. In the short-run models this index is .643, which certainly is not a far cry from the standard deviation of .699 in the yearly democratization rate itself. This only amounts to an increase in predictive performance of (.699–.643) /.699 ≈ 8.0 percent. In the long-run model assessment, however, the standard error of regression is 2.0, which compared to the standard deviation of 3.38 in the democracy index itself amounts to an increase in predictive performance of (3.38–2.0)/3.38 ≈ 41 percent.

By contrast, our models do a fairly good job when trying to predict the long-term equilibrium levels of democracy. In other words, when projected against a longer time horizon democratic development is not as unpredictable as adherents of the voluntaristic or chance-oriented views have asserted. On the contrary, in this time perspective the structural approach to democratization performs relatively well. This might explain why, as noted by Carothers (2000), over the long haul the actor-oriented approach associated with the "transition paradigm" has not been a very helpful theoretical lens through which to understand democratization.

In light of these findings, we recommend future studies on determinants of democratization to pay particular attention to the following two observations. The first concerns the need for a theoretical synthesis of the empirical regularities uncovered. We have already commented upon the lack of a viable causal mechanism accounting for the negative impact of the percentage of Muslims, religious fractionalization and trade dependence (after taking trading partners into account). We now turn to the want for a broader model that could fit the pieces together into a coherent theory of democratization. What is it about these factors, together with size, modernization, economic crises, resource abundance, democratic diffusion, regional organizations and peaceful mobilization that make them foster or hinder democratic development? And why do structural factors mostly exert their influence on a long-term basis, whereas the short-term dynamics appear more erratic?

In our view, the most promising approach to such theoretical integration to date is the work of Boix (2003). Firmly based in the tradition of formal economic theory, Boix assumes that people only care about their income, and hence evaluate their preferences for democracy or autocracy in terms of this. By implication, the fundamental struggle over democracy occurs between the rich and the poor. Since under democracy the poor set the tax rate in order to redistribute income, the poor generally prefer democracy whereas the rich prefer autocracy. Two fundamental parameters may, however, alter this scheme of things. The first is income inequality: the more equally distributed the level of income is to begin with, the less the rich have to fear from conceding democracy to the poor. The second is capital mobility, or asset specificity: the less productive an asset is at home relative to abroad, the lower will be the tax rate in order to avoid capital flight. This means that the burden of democracy to the rich decreases as asset specificity decreases.[16] From these simple assumptions, Boix develops a simple game theoretic model in which the rich may choose to repress (sustain autocracy) at a certain cost or not repress (allow democracy), and the poor may choose to revolt

16 Interestingly these two fundamental parameters parallel Bellin's (2000) discussion of two factors that determine the stance toward democracy among the capital class: on the one hand fear (of redistribution) and state dependence (the latter, among other things, leading to low capital mobility).

(mobilize against the regime) or acquiesce. A key part of this setup is played by an informational asymmetry: the poor are uncertain about the likelihood that the rich will use repression.

A surprisingly large number of predictions ensuing from Boix's model concur with our findings. Popular mobilization, quite evidently, should increase the likelihood of democratization by increasing the repression costs of the rich (ibid., 44–46). The estimated probability on behalf of the poor that the rich will employ repression, moreover, may help explain two of our findings. The first concerns the apparently erratic nature of short-run changes in the level of democracy as compared to the more predictable long-run equilibria. By prompting citizens to update their beliefs on the likelihood that different courses of actions will have different consequences, short-term political events have more unpredictable consequences than the more slow-moving forces that shape the income distribution and the degree of asset specificity. The second concerns diffusion effects: the presence of information uncertainty helps explain why events in neighbouring countries, for example, may lead domestic actors to re-estimate the chances of achieving their goals in light of the recent experience of similar actors abroad (ibid., 29).

Although previous studies of the importance of income inequality for democratization have tended to produce mixed results (see, e.g., Bollen and Jackman 1985; Muller 1995; Burkhart 1997),[17] several others of our findings support Boix's conjectured importance of asset specificity. Socioeconomic modernization, to begin with, and even more notably the large spectrum of societal processes included in this phenomenon taken into account by our broader measurement strategy, should be expected to go hand in hand with decreased asset specificity. As countries industrialize and develop from rural to urban economies, and as people become more educated and informed through the mass media, productive capital may not be as easily taxed without the risk of moving abroad. Natural resource abundance, by contrast, is a fixed asset that may not be productively moved abroad. Country size, moreover, should be negatively linked to asset specificity since it increases the physical distance that has to be traversed in order to reach another country (ibid. 41-44). Hence, in line with Boix's predictions, socioeconomic modernization should increase, whereas oil wealth and country size should decrease, the chances of democracy.

17 We have not included income inequality among our determinants of democratization in this chapter due to the poor coverage of existing time-series cross-sectional data. If we replicate Boix's (2003, 76) measurement strategy, that is, by taking 5 year moving averages of Deininger and Squire's (1996) "high quality" observations, we get 1014 country year observations from 93 countries. The effect of income distribution (lagged one year) in this dataset is however insignificant (either with or without including all other determinants as controls).

Obviously, Boix's model cannot explain all our findings, nor are all of them consistent with his predictions. This is also not the place to make a full assessment of the strengths and drawbacks of Boix's theory of democratization. The purpose of the preceding discussion is merely to highlight the fruitful potential involved in trying to apply more integrated theories of democratization to future empirical assessments. As to date, the formal models based on the economic approach are the most suitable candidates for such an exercise.

A final observation concerns the methodological future of the field. We have in this chapter tried to show that the actor-centric and structure-centric approaches to democratization need not be incompatible; they simply speak to different factors operating at different time horizons. This has been done, however, on the basis of results produced completely within a statistical and large-*n* framework. The next generation of democratization studies ought to take this as cue for integrating small-*n* and large-*n* analysis into the same research program. Only in this way may the gap dividing the two approaches to democratization be bridged not only in theory but also in practice.

Appendix A: Definition of Variables

Democracy: The graded measure from Freedom House (2003) is computed by taking the average of their 1-7 ratings of *Political Rights* and *Civil Liberties*, and then inverting and transforming this scale to run from 0 to 10. The graded measure from Polity is the *Revised Combined Polity Score* (Marshall and Jaggers 2002, 15-16) transformed to run from 0 to 10. These two graded measures are then averaged into a combined index running from 0 to 10. We have imputed missing values by regressing the average FH/Polity index on the FH scores, which have better country coverage than Polity.

Religious denomination: The data on religious denominations have been collected from Barrett et al. (2001). The data are estimates of the fraction of the population as of 1970 being Catholics, Protestants, Orthodox, Other Christians, Muslims, Hindus, Buddhists, Other denomination (including miscellaneous East-Asian religions and Jews), and Nonreligious.

Fractionalization: We employ data on *ethnolinguistic* and *religious fractionalization* collected by Alesina et al. (2003), both reflecting the probability that two randomly selected individuals from a population belong to different groups. The figures on ethnolinguistic fractionalization are based on 650 distinct ethnic groups (ethnicity being defined in either racial or linguistic terms), those on religious fractionalization on 294 different religions. Although the underlying data only pertain to one year for any given country (in most instances from the 1990s or around 2000), we treat these figures as constants over the entire time period 1972-2000. Although this of course might distort real world developments and cause problems of endogeneity, we concur with Alesina at al.'s claim that treating these figures as constants "seems a reasonable assumption at the 30 year horizon" (2001, 160).

Colonial heritage: We include five dummies for countries being a former Western overseas colony: British (including 39 states in the estimation sample), French (20 states), Spanish (17 states), Portuguese (4 states), and finally a collapsed residual category consisting of the former Dutch, Belgian and Italian colonies (including 6 states in the estimation sample). We thus follow the practice of Bernard et al. (2004) in exclusively focusing on a particular form of colonial legacy (Western overseas colonialism), and by excluding the "settler colonies" from the group of British colonies (including the US, Canada, Australia, New Zealand and Israel). We coded as a colony each country that has been colonized since 1700. In cases of several colonial powers, the last one is counted, if it lasted for 10 years or longer. Source: *Encyclopedia Britannica* and *Atlas till Världshistorien* (Stockholm: Svenska bokförlaget, 1963).

Population: In order to measure country *size*, we use the logged population figures from WDI (2004).

Socioeconomic modernization: The indicators combined into this index are: (1) *industrialization*, measured as the net output of the *non*-agricultural sector expressed as a percentage of GDP; (2) *education*, measured as the gross secondary school enrollment ratio; (3) *urbanization*, measured as the urban percentage of the total population; (4) *life expectancy* at birth (in years); (5) the inverse of *infant mortality* (per 1000 live births); (6) the number of *radios* per capita; (7) the number of *Television* sets per capita; and (8) *newspaper* circulation per capita. The source of indicators (1)-(5) is WDI (2004), of indicators (6)-(8) Banks (2002). We used linear intrapolation, country by country, to fill in missing years for secondary school enrollment, life expectancy and infant mortality. We used the secondary school enrollment ratio since it has the strongest correlation with the Barro and Lee (2000) indicator "average years of primary schooling in the total population", although with more extensive country coverage. Our final indicator is (9) *GDP per capita*. In order to maximize country coverage, we used WDI (2004) data expressed in constant 1995 US dollars (thus *not* corrected for PPP), completed with WDI (2004) data expressed in current USD for Libya and Somalia.

The principal components factors loadings for these 9 indicators are (n=2965):

Industrialization	.83
Education	.91
Urbanization	.89
Life expectancy	.91
Inverse infant mortality	.90
Radios	.83
TVs	.88
Newspapers	.81
GDP/capita	.93

The eigenvalue of this first dimension is 6.94, explaining 77.1 percent of the variation in the indicators across time and space. The eigenvalue of the second component is .67, strongly supporting unidimensionality. The factor loadings are extremely similar if computed at any given year instead of pooled across all years. The index of socioeconomic modernization is computed by taking the factor scores of the above pooled solution, and then using imputation on the regression line with all 9 indicators as regressors.

Apart from the theoretical argument proffered in the text, there are two more technical reasons why we base our results on this summary measure instead of any or some of its constituent parts, which has been the dominant approach in the field. First, since our index is based on multiple indicators it should have a reliability edge over any of its sub-components. Second, all of the indicators used have a theoretical underpinning in the modernization literature. Yet were we to introduce them separately into a regression equation we would introduce huge amounts of multicollinearity. We avoid this by only including the summary index.

Economic performance: Following Gasiorowski (1995), we employ two measures of short-term economic performance (both based on WDI 2004): recessionary crises, measured as the annual *growth* rate of GDP per capita in fractions, and inflationary crises, measured as the annual *inflation* rate (based on the GDP deflator), also in fractions. This measure of inflation correlates at .98 with the one based on consumer price index, but has much larger country coverage.

Oil and Minerals: Following Ross (2001), *oil* is the export value of mineral-based fuels (petroleum, natural gas, and coal), *minerals* is the export value of nonfuel ores and metals, both expressed as fractions of GDP, based on data from WDI (2004). Also following Ross (2001, 358), we replaced the values for Singapore and Trinidad & Tobago by .001. We filled in missing values from Ross' original data set, which he generously made available to us, and by yearly linear imputation (country by country).

Trade openness: Defined as "the sum of exports and imports of goods and services measured as a share of gross domestic product", expressed as a fraction of GDP. Source: WDI (2004).

Gross capital flows: Defined as "the sum of the absolute values of direct, portfolio, and other investment inflows and outflows recorded in the balance of payments financial account, excluding changes in the assets and liabilities of monetary authorities and general government", calculated as a fraction of GDP. This measure captures both of the two capital exchange variables tested by Li and Reuveny (2003)—i.e., foreign direct investment and portfolio investments—but with considerably improved country coverage. Source: WDI (2004).

Diffusion effects: We employ three proxies for diffusion effects. They are composed of mean scores of the combined democracy index computed at three different spatial levels. The most proximate level is that of neighbouring countries. Neighbours are defined as countries separated by a land or river border, or by 400 miles of water or less, using Stinnett et al.'s (2002) direct contiguity data. The rationale behind the water contiguity distance is that 400 miles is the maximum distance at which two 200-mile exclusive economic zones can intersect (ibid., 62). This criterion creates the maximum number of contiguous states in the world system, only leaving New Zealand (among the ones for which we have data on the dependent variable) without any defined neighbouring countries at any time. Beyond contiguous neighbours, we also test whether diffusion effects may operate at the regional (regions being defined below) and global level. Both these measures are computed as yearly means.

Regional organizations: Following Pevehouse (2005), we compute the average degree of democracy among the countries belonging to the same regional organization as a

country. For countries belonging to more than one regional organization, only the score for the most democratic regional organization is included. Countries not belonging to any regional organization a particular year are scored zero; instead, a dummy variable is entered scored 1 for these countries, zero for all others. Data on membership in regional organizations are provided by Pevehouse et al. (2004). Again following Pevehouse (2005, 49-50, 67-70), we have only included political, economic and/or military intra-regional organizations, thus excluding inter-regional organizations and international financial institutions, as well as cultural, technical and environmental organizations. To the list of regional organizations existing up until 1992 provided by Pevehouse, we have added a small number of organizations formed afterwards.

Popular mobilization: We used Banks' (2002) data on the yearly number of *demonstrations*, defined as "any peaceful gathering of at least 100 people for the primary purpose of displaying or voicing their opposition to government policies or authority, excluding demonstrations of a distinctly anti-foreign nature"; *riots*, defined as "any violent demonstration or clash of more than 100 citizens involving the use of physical force"; and *strikes*, defined as "any strike of 1000 or more industrial or service workers that involves more than one employer and that is aimed at national government policies or authority".

According to Banks (2002), all these figures are "derived from the daily files of The New York Times". This could be a source of bias, since press coverage of protest events are known to overestimate events in their geographical proximity, and underestimate events of minor intensity (see, e.g., Mueller 1997). In our case, however, we believe the potential geographical bias makes our tests of the mobilization variables conservative, since there is smaller variation in the dependent variable in the West. Moreover, the fact that minor protests are underreported might seem less of a problem from a theoretical point of view, since one could argue that only large-scale events should stand any chance to affect regime change.

Regional effects: As a check on the robustness of our findings, we include dummy variables for six world "regions": Eastern Europe & Central Asia, Latin America & the Caribbean, Middle East & North Africa, Asia & the Pacific, the West, and Sub-Saharan Africa.

Appendix B: Statistical Model

Let $D_{i,t}$ be the democracy index of country i at time t. We then model $(D_{i,t} - D_{i,t-1})$, or ΔD_i for short, as a function of $\mathbf{x}$, a vector of explanatory variables. Most of these variables vary over time, in which case we have lagged them one year as a partial check on endogeneity bias. Moreover, we control for previous levels of democracy, $D_{i,t-1...p}$, that is, $D_{i,t}$ lagged up to a maximum of p years.

There are numerous reasons to include lagged values of the dependent variable. First, as argued in the text above, a lagged dependent variable may work as an explanatory factor in itself. Second, including lagged values of the dependent variable in the model helps to control for the possibility of endogeneity bias, that is, causality

running in the direction from democracy to the explanatory variables instead of vice versa. Lagging the explanatory variables is only a first step towards this control. By also including lagged values of D_i on the right-hand side of the equation the model assures that any effects of D_i on $\mathbf{x}_i$ occurring previous to t-p is controlled for. For example, the inclusion of $D_{i,t-1}$ rules out any effects due to the path $D_{i,t} \leftarrow D_{i,t-1} \leftarrow D_{i,t-2} \rightarrow \mathbf{x}_{i,t-1}$ (see, e.g., Finkel 1995, 24-31). More generally, by controlling for the history of democracy in each country, including the lagged dependent variable works as a proxy control for other potential determinants not included in the model. Third, lagging the dependent variable helps to control for serial correlation in the error term (Beck and Katz 1996).

In sum, this yields the model

$$(D_{i,t} - D_{i,t-1}) = \sum_{n=1}^{p} \phi_n D_{i,t-n} + \mathbf{x}_{i,t-1}\beta + \varepsilon_{i,t}, \quad (1)$$

where ϕ_n and the β-vector contain the short-run effect parameters to be estimated, and $\varepsilon_{i,t}$ is the error term. It turns out that no less than three lagged values of the dependent variable are in our data required to purge the residuals from serial autocorrelation.[18] In other words, p=3 in the analyses reported here.

Although OLS should yield consistent estimates of β, there are a number of statistical problems involved in estimating its standard errors on time-series cross-section data: serial and spatial autocorrelation as well as panel heteroskedasticity (Beck and Katz 1995). We control for the first of these problems, as already noted, by controlling for lagged values of the dependent variable, and for the second and the third through panel corrected standard errors, as recommended by Beck and Katz (1995, 1996). Since we also model spatial dependence directly by controlling for diffusion effects, our reported estimates should err on the conservative side.

In order to gauge the long-run performance of our models, we make use of the fact that the lagged values of the dependent variable also affect the way our x-variables affect democracy over time. If we assume a sustained one unit increase at time t in one of these explanatory variables, say x_j, the immediate impact occurring over the following year t +1 is of course this variable's corresponding short-term β-coefficient, that is β_j. Due to the first lagged value of the dependent variable, however, an effect of the magnitude $(1+\phi_1)\times\beta_j$ will then be induced over the year t+2. In the following year (i.e., at t+3) up to two lagged values of the dependent variable will influence the effect, which now amounts to $[(1+\phi_1)^2 + \phi_2] \times \beta_j$ and so on. According to the estimates of our model, about 40 percent of the adjustment back to equilibrium occurs over a 5-year period, some 70 percent over a 10-year period, and around 90 percent over 20 years. Only after 40 years the adjustment reaches 99 percent. We may

18 The estimated value of ρ from the Lagrange multiplier test recommended by Beck and Katz (1996, 9) is .203 and .423 (p=.000), respectively, when one or two lags are added. When the third lag is entered ρ=-.329 (p=.143).

thus conclude that it takes approximately 40 years for the full long-run effects in our models to occur.[19]

In order to obtain the long-term estimates for each determinant we project the behavior of the short-run coefficients according to this logic as t goes to infinity. As long as the usual stationarity conditions are satisfied (i.e., that the roots of the characteristic equation for $\phi_{1...n}$ lie outside the unit root circle; see, e.g., Green 1997, 829), we may compute the long-run impact multipliers according to the formula

$$\theta = -\beta / \phi^*, \quad (2)$$

where $\phi^* = \sum_{n=1}^{p} \phi_n$, and β and $\phi_{1...n}$ are estimated through equation (1). Since θ is a ratio of coefficients there is no general formula for its exact variance. Following Bårdsen (1989) and Londregan and Poole (1996, 17)), a large sample approximation formula can however be obtained by

$$\text{var}(\theta) \cong (\phi^*)^{-2} [\text{var}(\beta) + \theta^2 \text{var}(\phi^*) + 2\theta \text{ cov}(\beta, \phi^*)], \quad (3)$$

where the variances and covariances in our case are panel corrected.[20]

In order to assess long-term predictive performance, we proceed as follows. The long-term projections discussed above are based on the notion of a static equilibrium, determined by the x-vector of explanatory variables, towards which each system is attracted. We may think of such an equilibrium in terms of a state where any inherent tendency to change has ceased, that is, as the estimated level of democracy that would arise in the long run if all explanatory variables were held fixed at their current values. In the present context we can compute this long-run equilibrium level of democracy for each country and year as

$$D^*_{i,t} = x_{i,t}\theta \quad (4)$$

We then simply regress the actual level of democracy for each country and year on this projected long-run equilibrium level, and assess model fit.

We compute two fit indices in order to guide this assessment. The first (and the only reported in the main body of the text) is the ordinary explained variance, adjusted for the degrees of freedom. It should be noted that since we have put the first difference of the democracy index on the left-hand side of our equations, we avoid inflating the R-squared by the static variance common to both $D_{i,t}$ and its lagged values (cf. Li and Reuveny 2003, for example, who report levels of explained variance well above 90 percent for this simple reason). The second estimate is the standard error of the regression (also known as the root mean squared error). It is simply the standard deviation of the residuals, that is, the unexplained part of the dependent variable. Being expressed in the same measurement units as the dependent variable, its minimum value is zero, indicating perfect fit, but unlike the R-squared, it lacks a normed upper bound. In order to ease its interpretation, we compare it to the actual variance in the dependent variable. In the estimation sample of 2628 observations the standard deviation of $\Delta D_{i,t}$ is .694, and of $D_{i,t}$ 3.38.

Finally, there is the issue of how to separate effects on movements toward versus away from democracy. With a dichotomous measure of democracy, this is straightforward. By limiting the sample of cases to countries that are authoritarian at time t-1, the results that ensue pertain to effects on transitions toward democracy. Similarly, when the results are based on countries that are democratic at time t-1, the estimates pertain to transitions toward autocracy, that is, on democratic survival. With a graded measure of democracy, however, things are not quite as simple since change may now both start and end at various levels of democracy, whereas stability might imply that either high, low or intermediate levels of democracy are being sustained. In this chapter we take a simple approach to this problem by simply distinguishing cases of $\Delta D_i > 0$ (upturns) from cases of $\Delta D_i < 0$ (downturns). When the former are being modeled, we simply set all cases of downturns to zero, and vice versa.

19 These calculations were made by plugging in the coefficients for the lags of the dependent variable in our estimated model into a purely autoregressive equation, and then simulate the projected response to a one-unit change at time t=0 as t goes from 0 to 50.

20 The estimates reported in this chapter have been computed by the xtpcse and nlcom commands of Stata 9.2.