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Diffusion Is No Illusion

Neighbor Emulation in the Third Wave of Democracy

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This article develops and tests a specific model of the role of diffusion as a determinant of the magnitude and direction of regime change, using a database covering the world from 1972 to 1996. The authors find that countries tend to change their regimes to match the average degree of democracy or non-democracy found among their contiguous neighbors and that countries in the U.S. sphere of influence tended to become more democratic in the period examined. They also confirm that countries tend to follow the direction in which the majority of other countries in the world are moving. Their model builds on several findings in the diffusion literature but adds methodological improvements and includes more extensive controls for other variables that have been found to affect regime change—including levels of development, presidentialism, and regional differences—offering further support for some and challenging other findings of the regime change literature.

Keywords: democracy; diffusion; democratization; regime; income

Kant (1963) suggested 225 years ago that some of the causes of democracy lie beyond a country's borders, and Rustow (1970) and Whitehead (1986) echoed this argument. Yet the first large-*N* studies of democratization focused exclusively on domestic factors, ignoring the possible influence of neighboring states on regime change. The notion that countries are self-contained units, isolated from external forces, was challenged again in Huntingon's (1991) *The Third Wave*. Since then, some researchers, with varying degrees of success, have entertained and attempted to test the idea

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that democracy diffuses—most recently, Starr and Lindborg (2003), who find significant effects for global-, regional-, and neighbor-induced processes of diffusion. But a theoretically sound and empirically well-elaborated test of diffusion is still a rare phenomenon in the literature on democratization. Diffusion processes are notoriously difficult to pin down because it is hard to distinguish true diffusion from illusions of diffusion created by global trends, correlated disturbances, or the regional clustering of domestic factors.

In this article, we take an additional step toward separating the illusion from the reality. We focus on one type of democratic diffusion that could be called "neighbor emulation": a tendency for neighboring countries to converge toward a shared level of democracy or nondemocracy. To test whether such a tendency exists, we create a diffusion variable that measures the theoretically expected impact of regimes in neighboring countries on a target country. This variable operationalizes the expected magnitude and direction of the pressures for or against regime liberalization exerted by neighbors on each other for every country in the world from 1973 to 1996. Our diffusion variable enables us to test more rigorously than any other study to date whether international pressures from neighbors play any role in regime change. We also construct superpower influence variables that measure the expected impact of the United States and the Soviet Union on countries within their respective spheres of influence, and we construct a measure of global trends. This trio of variables allows us to test for the influence of neighbors, superpowers, and worldwide trends on the decision to become more or less democratic, controlling for domestic factors.

We find strong support for a pattern of diffusion in which countries tend to become more like their immediate geographic neighbors over time. We further find that countries within the U.S. sphere of influence during the third wave improved their democratic performance relative to their peers and that global trends have a strong impact on regime change. These results are highly robust: They persist even when controlling or correcting for the major specification, measurement, selection, and serial and spatial autocorrelation problems that could masquerade as diffusion. Democratic diffusion is no illusion. The strong influence of international factors on changes in levels of democracy highlights the importance of taking international influences into account in large-*N* analyses of democratization.

Diffusion in the Democratization Literature

Many researchers neglect diffusion by implicitly assuming that the critical variables determining a political system are domestic ones. Most scholars who include aspects of transnational causation in their theories do so only in the simplest ways. Some include a dummy variable for British colonial influence (e.g., Bollen & Jackman, 1995; Lipset, Seong, & Torres, 1993, pp. 168-170; Muller, 1995; Rustow, 1970, p. 348), whereas others include dummies for world-system position (see, e.g., Bollen & Jackman, 1995; Burkhart & Lewis-Beck, 1994; Gonick & Rosh, 1988; Lipset et al., 1993; Muller, 1988). Starr (1991) tests whether regime transitions take place closer together in time than mere chance would predict as well as whether countries whose contiguous or regional neighbors have experienced transitions in the 3 prior years are more likely to undergo a regime transition themselves. Hannan & Carroll (1981) and Helliwell (1994) report that countries in certain geographical regions are systematically either more democratic or less democratic than purely domestic models of democratization would predict. Every one of these studies is consistent with transnational influence, but the way they operationalize diffusion makes it difficult to distinguish between true diffusion and regional clustering of domestic factors.

A few studies introduce more precise measures of diffusion and controls for important domestic variables. Przeworski, Alvarez, Cheibub, and Limongi (2000) find that democracy is more likely to survive in a country that is in a more democratic region and becomes more likely to survive as the total number of democracies in the world rises, even controlling for important domestic variables. O'Loughlin et al. (1998) find that "even controlling for GDP per capita, there remains strong clustering of the political democracy scores" (p. 557). Finally, Pevehouse (2002) finds that membership in certain regional organizations increases the probability of a regime change, even controlling for many domestic factors. Most recent, Starr and Lindborg (2003) carry out sequential tests for global, regional, and neighbor effects, but do not incorporate all three into the same model or control for other geographically clustered and possibly confounding variables such as economic development and presidentialism.

Our study builds on these and makes several improvements. First, our diffusion variable takes into consideration levels of democracy in both sending and receiving countries and predicts a stronger effect when the difference in levels is greater. It therefore yields more precise predictions that are easier to distinguish from the simple regional clustering of domestic determinants. Second, we model a partial adjustment process. If our model is accurate, the aggregate effect of partial adjustment would be waves of democratization (or breakdown) that spread from country to neighboring country. Such waves arise where the greatest gaps between democratic and nondemocratic countries are found, sending ripples in all directions through their previously similar neighborhoods. This partial adjustment model is not only consistent with the general literature on diffusion but also mirrors the wavelike historical patterns of democratization described by Huntington (1991) and others.

Third, we use a two-stage model to test separately the likelihood that a given country will adopt a new form of government and whether, if it does change, that government will be more democratic or less so and by how much. This two-stage model allows us to correct for selection bias, using a modified Heckman selection model (Breen, 1982). And fourth, we introduce more controls for domestic, regional, and global influences and for period effects than any previous study. As a result, we can be more certain that we have isolated the effect of diffusion from the effect of geographically and temporally clustered third variables. If, as all the prior research and our own results suggest, diffusion is an important phenomenon, then researchers who ignore diffusion risk exaggerating the impact of domestic determinants. In short, our study builds on important earlier work on diffusion, in particular Gleditsch and Ward (1997), Most and Starr (1990), and Starr and Lindborg (2003), but adds more comprehensive controls for domestic variables, more controls for potentially confounding technical and substantive factors, more sensitive measures of democracy, and different methodologies, thus, adding significantly to the weight of the evidence and the precision of estimates of the impact of diffusion on democratization.

Our Model of Diffusion

Although democratic diffusion could take many forms, we model and test only one—neighbor emulation. This is a species of the selective (decisional) models of diffusion described in Most and Starr (1990; i.e., a model in which actors in one state make a change that is similar in nature to a change occurring in other states). As a beginning step in our investigation of diffusion, in this article we limit the impact to contiguous neighbors. The core assumption of this model is that countries are rewarded when their regimes are similar to those of their neighbors. The rewards could be of many different kinds: peace, mutual security, trade, investment, ease of communication, and so forth. For example, the democratic peace literature suggests that democracies rarely if ever go to war against other democracies. But the nature of the reward for having similar regimes is less important than that there be a reward of some kind or more accurately, that key actors *believe* that such rewards exist.

Given our research design, we do not take a position on who these actors might be. Some could be international. For example, when Fujimori shut down democratic institutions in Peru, the neighboring—democratic—governments in the Organization of American States brought pressure to bear for a return to democracy. Conversely, neighboring governments could pressure other governments to crack down on domestic opposition—especially if there is a shared view that some kinds of opponents (Communists, Islamist radicals, transnational nationalities) are dangerous and could have a domino effect. For these and other reasons, domestic actors in one country sometimes urge their governments to demand better human rights protections in another, to intervene militarily, to fund insurgents, to greenlight a coup, or to apply other pressures to neighboring states.

Even purely domestic actors can be influenced by events in neighboring countries. Influential members of civil and political society can use their neighbors as good or bad examples. Something like this appears to have happened in the 1970s when one democratic regime after another in Latin America fell to military coups that championed the same national security ideology. The same dynamic partially accounts for movements toward democracy in the former Warsaw Pact countries—for example, when the Hungarian borders to the West opened, threatening an exodus from East Germany and creating dramatic popular pressure for liberalization. Arguments for emulation may be backed by strong evidence or they may be motivated by simple mimicry; whatever the motive or justification, what matters is that some influential actors champion regime convergence.

The data at our disposal cannot tease out these specific mechanisms. However, we can use this model to derive a testable prediction: The greater the gap in the level of democracy between a country and its neighbors, the greater the pressure will be for convergence. When two countries are equally democratic or nondemocratic, there is no pressure; emulation is a nonissue, as between the United States and Canada or Syria and Iraq. When there is a small difference, there is small pressure; and when there is a great difference, there is strong pressure: It becomes a salient issue, as between the United States and Cuba, North Korea and South Korea, East Germany and West Germany, or Francoist Spain and the rest of Western Europe.[1] We could also predict that when a country has several neighbors with different regimes, there are cross-pressures. Each country has its own influence on its neighbors. A partially democratic country could have some actors who press for emulation of a more democratic neighbor and other actors who prefer to crack down more harshly like a less democratic neighbor. Our model predicts a net effect that would correspond to the mean of all these cross-pressures.

1. As we discuss below, we find that these pressures alone are not enough to trigger a regime change. A country must be ready to move before these pressures can be felt. But our model of convergence suggests that if there is a regime change in Cuba, for example, it will be toward greater democracy and not in the opposite direction.

This model of diffusion conforms to the basic parameters of diffusion as understood in the literature. We match, for example, the description of selective diffusion in Most and Starr (1990), and we specify the elements of diffusion identified by sociologist Everett Rogers (1995). Rogers defines diffusion as "the process by which [1] an *innovation* is [2] *communicated* through certain *channels* [3] over *time* among the members of [4] a *social system*" (p. 10). Our model specifies each of these four elements. First, the innovation with which we are concerned is the adoption of a more or less liberal democratic style of government, as measured by a Freedom House–based score for political rights and civil liberties (Freedom House, n.d.). In keeping with the notion of diffusion, which concerns the adoption of a *new* idea or technique, we look for *changes* in levels of democracy in the target country. Our dependent variable, therefore, is neither a static measure of levels of democracy nor the longevity of democracy but instead, an indicator of change in the degree of democracy from year to year: Δd.

The data we use cover all the countries of the world from 1972 (or the date they came into existence) to 1996. The annual change in democracy levels is calculated from an index based on each country's Freedom House annual rating for political rights and civil liberties.[2] Although it is technically an indicator of "freedom," this rating correlates at upwards of .85 with accepted measures of democracy such as Bollen's indices for 1960 and 1965, Gurr's Polity III measure of democracy-autocracy, and the Coppedge-Reinicke Polyarchy Scale (Coppedge & Reinicke, 1990, p. 61; Przeworski, Alvarez, Cheibub, & Limongi, 1996, p. 52; Vanhanen, 1990). Despite some criticisms (e.g., Mainwaring, Brinks, & Pérez-Liñán, 2001), Freedom House data are used to measure democracy in several respected studies (Burkhart & Lewis-Beck, 1994; Helliwell, 1994) and together with the Polity IV series, are the only global annual democracy indicators available and, therefore, the only ones suited to a study of democratic diffusion.[3]

We use the numerical scores, rather than the tripartite ordinal classification derived from these scores (unlike Starr & Lindborg, 2003) to create more sensitive measures of change and influence. One payoff is the finding, as we discuss below, that increases in GDP are associated with movement toward greater freedom at lower levels of democracy, contrary to the finding

2. The index is 16 – (political rights + civil liberties), which produces a 13-point index ranging from 2 *(least freedom)* to 14 *(greatest freedom)*.

3. We have not yet tried to confirm that our results hold when using Polity data because the best comparison of these indicators finds that the Freedom House (n.d.) data are more valid and more reliable (Bollen, 1993) and because the calculation of a Polity-based diffusion variable would be extremely time-consuming. However, most studies that use both variables tend to find similar results with either one.

by Przeworski and Limongi (1997). The debate about which democracy indicator to use will not be decided here (see, e.g., Bollen, 1991, 1993; Bollen & Jackman, 1989; Bollen & Paxton, 2000; Mainwaring et al., 2001). However, for our purposes, there is clearly an advantage to a 13-point index that preserves the full range of information available and produces more sensitive measures of change and influence.

The second element is the channel of communication. As noted, we do not test directly for particular diffusion channels. We do, however, specify which behavior in other countries sends the message down the channel, and we test a variety of hypotheses—whether diffusion is best modeled as a simple demonstration effect by neighboring countries, a global demonstration effect, or a destabilizing influence in which change in the social network breeds change. Our primary hypothesis is that the greater the difference between the network's average levels of democracy and the potential adopter's level at time t, the greater the resulting change in the adopter at $t + 1$.[4] This allows us to be more specific in our predictions and more rigorous in our testing than Starr and Lindborg (2003), who dichotomize networks of neighbors into those in which 50% or more are (or are not) democratic, as measured by the free, partly free, and not free Freedom House categories.

We model the impact of worldwide trends toward or away from democracy with a variable that represents the average of all changes in the world (except the target country's) for the current year. And we test for the destabilizing impact of the absolute value of change in all the countries in the network in the previous year.[5] Again, the use of the actual Freedom House scores permits greater sensitivity in measuring levels of instability in the region than earlier approaches (Starr, 1991; Starr & Lindborg, 2003), which

4. In averaging Freedom House (n.d.) scores, we are, in effect, treating these scores as if they were ratio-level data, although they are more likely only ordinal and at best perform as interval-level data. In doing so, we run the risk of introducing noise into our calculations. However, using the tripartite classification instead, as Most and Starr (1990) do, would only further increase the measurement error. In practice, averages of a 13-point index are quite meaningful and are certainly more sensitive to variation than, for example, dichotomies that indicate whether more or less than half of a country's neighbors are democratic. It is fortunate that the more the Freedom House scores differ from an interval-level scale, the less significant our estimate of the diffusion effect would be: To the extent that the variable conflates neighborhoods that show high and low scores with those that show uniformly average scores, it should simply make it harder to find an effect of diffusion. Therefore our estimates of the effect of diffusion are, if anything, conservative.

5. We also include in the regression a series of dummy variables identifying countries that belong to the same region. These variables serve not to model regional diffusion but to control for the domestic variables that cluster by region or for omitted international impacts on an entire region.

rely on a count of transitions rather than measuring how great the changes are in each individual neighbor relative to the number of neighbors.

The third element relates to timing. Diffusion theory suggests that the individual characteristics of potential adopters will affect the rate of acceptance of the innovation (Rogers, 1995; p. 220). In a similar manner, the literature on regime transitions suggests that countries may need particular triggers—such as state breakdown (Skocpol, 1973), economic crisis (Przeworski et al., 1996), rapid economic growth (Huntington, 1968), a split in the authoritarian elite (O'Donnell & Schmitter, 1986), or the removal or death of the executive (Londregan & Poole, 1996)—to break the inertia of the existing regime and adopt a new structure. In other words, we might expect that regardless of diffusion and other pressures for adoption of a particular regime type, the potential adopter must first be primed for change.

This assumption matters methodologically as well as theoretically. If we use standard regression techniques on the entire sample to estimate the effect of our variables on the extent of regime change, their true impact may be obscured by the years of stasis that precede a movement. But if we select for inclusion in the sample only those observations that actually show some change, we may be overestimating the importance of these factors by ignoring many cases in which the hypothesized variables produce no observable change. We control for both of these dangers by employing a selection model that estimates the likelihood that country i at time t will be selected for change and uses the results of that analysis to estimate the impact of the independent variables on the direction and extent of change, *given the probability of change*.

The fourth element that Rogers (1995) identifies is the social system, which in this context is a network of countries. Networks need not be based on proximity; they may link dispersed countries that share colonial, cultural, economic, or political ties. Indeed, as Most and Starr (1990) argue, we might expect countries that interact more frequently to have a greater influence on each other, and neighbors do not always have a great deal of interaction—North Korea and South Korea are one striking example. Still, although many networks are possible and promising, for this project we select three types of networks. Simple contiguity is our main criterion for inclusion, a beginning step for the study of the spread of democracy. It is theoretically the most simple but in execution the most complex, as each country has a different set of neighbors and is, therefore, in a unique network that overlaps with other networks.[6] In addition, we use one variable that assumes a single global network

6. Specific coding rules for considering countries' neighbors are available from the authors. Physical contiguity is the main criterion, but certain island nations are also considered to have

and variables that assume politically defined networks of superpower influence, grouping countries into those aligned with the Soviet Union or the United States.

In summary, our primary network is the network of contiguous neighbors that surround a target country. As noted, because most countries have more than one neighbor, we average the size of the gap over all of the target country's neighbors. Therefore the variable that operationalizes our model of diffusion among neighbors is calculated as follows: Diffusion effect on country i, at time $t + 1 = \frac{1}{k}\sum_{k=1}^{k}(d_{k,t} - d_{i,t})$, where k is the number of countries in the network of contiguous neighbors, $d_{k,t}$ is the democracy score of country k at time t, and $d_{i,t}$ is the democracy score of country i at time t. This variable simultaneously operationalizes three predictions from our theoretical model. One is that there are interconnections among *neighbors*. Another is that the relationship is positive: Countries in more democratic neighborhoods tend to become more democratic and vice versa. The third prediction is that the size of changes tends to be proportional to the size of the gap in scores. We are not, therefore, simply searching for just any kind of spatial correlation or temporal clustering of democracy scores; rather, we are testing for a very specific pattern of democratic diffusion.

One assumption in this model is that all neighbors are alike and influence each other equally. However, we believe it is more likely that some countries are more influential than others. To test this we run separate models weighting the difference in d between k and i by the ratio in i and k $[(d_k - d_i)*(\text{weight}_i/\text{weight}_k)]$ of a series of readily available weighting factors, such as GDP, per capita GDP, territorial extension, population size, and several permutations of these weights. Our results with these easily available weights are either nonsignificant or not clearly better than the unweighted effects, so we do not report them here. According to these tests, the impact of a neighbor is not clearly conditional on its wealth or size. The same may not be true, of course, in networks defined according to less strictly geographic terms. Our results with superpower diffusion, for example, suggest that the United States, at least, has more influence on its allies than its allies have on it. In a similar manner, it is entirely possible that the impact of a trading partner is conditioned on its wealth or the relative size of trade flows. We leave all these inquiries to future research.

The global diffusion variable is calculated as the average of all values of Δd_t, excluding the target country's. For the networks of superpower influ-

neighbors, as in the Caribbean. We do not consider noncontiguous countries neighbors, even if they have significant interaction, leaving these noncontiguous networks for further research.

Table 1
Results From Diffusion Regression Models

Variable	Model 1	Model 2	Model 3
Lagged diffusion variable	0.172**** (9.99)	0.166**** (9.77)	0.180**** (10.78)
Global trend variable		0.585**** (4.98)	0.544**** (4.75)
U.S. effect variable			0.104* (1.86)
Soviet effect variable			0.002 (0.03)
N	1,089	1,089	1,089
R^2	.084	.105	.152
Prob > *F*	0.0000	0.0000	0.0000

Note: Parentheses contain *t* statistics.
*$p < .06$. ****$p < .001$.

ence, we code each country in each year as belonging to the U.S. sphere of influence, the Soviet sphere, or neither. Then we calculate the difference between the democracy score of each country and the score of the relevant superpower, if any, to test whether countries tended to converge with "their" superpower.[7] The result is a variable that scores 0 if the country is unaligned, and $d_s - d_i$ otherwise, where d_s is the democracy score of the relevant superpower. The superpower influence variables are lagged 1 year.

Testing for the Presence of Diffusion

Simple regressions using these variables singly or in combination on all the observations of change in Freedom House scores strongly support the presence of mild but highly significant diffusion processes, as seen in Table 1.[8] The U.S. effect approaches but does not quite reach conventional levels of significance in this model, whereas the Soviet effect here is nonsignificant.[9]

However, we can trust these results only if we assume we did not omit any variables that are correlated with the diffusion variables, that there are no abnormalities in the error structure that might affect OLS estimation, that there are no idiosyncrasies in the instrument we use to measure democracy, and that limiting the sample to years in which target countries actually change creates no selection bias. The remainder of this article is largely de-

7. The codings of superpower spheres of influence are available from the authors on request.

8. Running the same models on a full sample produces the same levels of significance, although the coefficients are substantially smaller because of the large number of static cases.

9. Including all the diffusion variables in the same model, rather than running separate tests for each type of network, as Starr and Lindborg (2003) do, permits us to tease out the separate impact of neighbors from regional, global, and superpower networks.

voted to ruling out all those issues that might lead to a spurious finding of significance—the illusion of diffusion.

Controlling for Domestic Determinants

The first step is to rule out domestic variables that might co-vary with our diffusion variable. In a more complete model, we include variables that have been shown to affect levels of democracy in previous studies, such as level of development, institutional configuration, and colonial history. Because these variables cluster geographically, as advanced industrial development and parliamentarism do in Western Europe and presidentialism does in Latin America, they could create a diffusion illusion. Any variable that favors countries being, becoming, or remaining democratic would, if regionally concentrated, lead to a region that appears to be more likely than average to be, become, or remain democratic; and this regional tendency could appear to be the product of democratic diffusion within the region. In fact, the two processes would be indistinguishable in a static cross-sectional analysis. Previous studies suggesting the presence of diffusion do not include these controls and, thus, could confuse diffusion with the clustering of domestic variables. Our cross-national time-series analysis helps to distinguish the two processes, and we include regional dummies to capture any omitted domestic variables that might also cluster geographically, such as language, culture, or religion. And as before, we include a measure of global trends to control for special moments that might affect the entire world, such as the fall of the Soviet Union.

Juan Linz (1994; Linz & Stepan, 1978) argues that presidential democracies are less stable than parliamentary regimes. Some empirical research supports Linz's thesis (Stepan & Skach, 1993), some qualifies it (Mainwaring, 1993; Mainwaring & Shugart, 1997), and some challenges it (Gasiorowski & Power, 1997). To test this hypothesis, we employ a dummy variable for presidential democracies (presidential systems that score at least 8 on our Freedom House Scale).[10] We limit the effect to relatively high-scoring democracies because the destabilizing effect of presidentialism is theorized to occur only once alternation in office is a real possibility and is not theorized to vary with levels of democracy. To distinguish the effect of presidentialism from the effect of simply being in a group with high Freedom House scores, we also include a dummy for all countries that score at least 8. The combination of dummies allows us to compare presidential countries that meet a certain democratic threshold to their nonpresidential democratic peers, and both groups to countries that score less than 8 on our Freedom House Scale. If

10. A list of the countries and years coded as presidential is available from the authors.

Linz's hypothesis is correct, the presidentialism dummy should have a negative coefficient.

In Table 2, as in Table 1, the coefficients for neighbor emulation and global trends are strongly significant. The expected U.S and Soviet impacts also achieve significance. The results at this stage of the analysis suggest that the diffusion impacts from Table 1 are not an illusion caused by regional concentrations of wealth, presidentialism, British colonial heritage, or fixed regional characteristics. In this analysis, the domestic variables do not achieve conventional levels of significance. At this stage, we find no support for Linz's (1994; Linz & Stepan, 1978) presidentialism hypothesis, the colonial experience hypothesis, or even an impact of wealth. So far, this is consistent with the finding by Przeworski et al. (2000) that development contributes to the stability of democracy but not necessarily to its advent. The regional dummies similarly fail to reach significance. Although the lack of significance of these variables might initially appear at odds with the findings of much of the democratization literature, the dependent variable in our regression is Δd, the magnitude of change in levels of democracy, which is more difficult to predict than static levels of democracy. The key point at this stage is that the effect of diffusion remains, even with standard controls.

Correcting for Selection Bias

The model in Table 2, however, assumes that all the observations are randomly drawn and independent of each other, with the exception of the diffusion relationships we model explicitly. To relax this assumption and rule out spurious effects due to selection bias, the structure of the data, and possible anomalies in the error structure, we introduce a series of methodological improvements.

The initial difficulty in doing a global analysis of changes in democracy for a series of years lies in sample selection. Most countries simply do not change in most years. Of the 3,979 country-years in our data set, 73.5% are cases of no change. When OLS is applied to these data, the concentration of cases at an outcome of 0 tends to flatten the slope of any independent variable, making it less likely to be statistically distinguishable from 0. On the other hand, including only the observations with a change—as in Models 1 to 4—raises obvious sample selection issues. To the extent that the causes of change are positively correlated with size and direction of change, the first approach risks underestimating and the latter overestimating the impact of the explanatory variables.

To address both of these concerns, we model a two-stage causal process. The first stage "selects" the countries that are most likely to change. The sec-

Table 2
Model 4: Diffusion With Domestic and Regional Controls

Dependent Variable: Δd	Coefficient	T	$p > T$
Diffusion variables			
Lagged diffusion variable	0.147	6.88	0.000
Mean global Δd	0.532	4.69	0.000
U.S. effect	0.403	3.98	0.000
Soviet effect	–0.388	–6.86	0.000
Domestic variables			
Log of per capita GDP	0.096	1.19	0.233
Presidential democ (t – 1)	–0.138	–0.61	0.540
Democ (t – 1) dummy	–0.737	–3.5	0.000
Former Anglo colony	–0.071	–0.45	0.653
Regional dummies			
Northern Africa	–0.044	–0.09	0.927
Middle East	–0.453	–0.91	0.363
Central America	–0.638	–1.5	0.133
South America	–0.467	–1.13	0.260
Former Soviet Union	0.290	0.65	0.519
Pacific State	0.330	0.65	0.518
Gulf State	–0.070	–0.14	0.892
South Asia	0.244	0.47	0.636
Sub-Saharan Africa	0.298	0.64	0.520
South East Asia	0.050	0.11	0.914
East Asia	0.339	0.72	0.470
Southern Europe	0.287	0.69	0.490
Southern Africa	0.418	0.78	0.438
Eastern Europe	0.773	1.66	0.097
Caribbean	–0.168	–0.4	0.691
Constant	–0.634	–0.77	0.439
Model characteristics/OLS regression			
N = 1,085			
$F(23, 1061) = 11.78$			
Prob > F = 0			
$R^2 = .2034$			
Adjusted $R^2 = .1861$			
Root mean square error = 1.7304			

ond-stage variables operate only on the countries selected in the first stage, determining how far up or down they change, and correct estimates by taking into account each country's predicted probability of change. Although some of the findings are substantively interesting, this stage is discussed only in the appendix because our primary concern in modeling the first stage is not the study of regime breakdown per se but correcting for selection bias.

Correcting for Correlated Disturbances

One issue that is usually not addressed in published studies of democratization is the danger that results from spatial correlations among the disturbances.[11] In a discussion of techniques for the analysis of cross-sectional time-series data, Beck (2001, pp. 280-282) warns that one should expect results for one country to be affected by events in neighboring or otherwise influential countries, such as trading partners. He notes that geographers are aware of the problem and generally treat these spatial correlations as a nuisance, attempting to correct for them. Beck emphasizes, however, that they can be treated as an omitted variable problem and recommends modeling these potential influences as relationships of interest (see also, Franzese & Hays, 2004).

This is precisely what we do in this article. Our diffusion variable estimates the impact of neighbors on one another and, thus, specifies a potentially important omitted variable. So long as the errors do not show a temporal correlation, a diffusion variable such as ours should overcome the difficulties attributable to spatial correlations in the error term (Beck, 2001, p. 282); so long as we avoid the simultaneity problem and incorporate the main sources of common shocks to the countries at issue (as we do by lagging the diffusion variable and including the most important domestic variables), our estimates should be relatively unbiased and consistent (Franzese & Hays, 2004). As discussed below, our own investigations support this conclusion. The question then becomes whether we have properly addressed the other common complications found in pooled cross-sectional time-series data: autocorrelation within countries, temporal correlations due to global processes that affect all the countries in the sample in a given year, and cross-panel heteroscedasticity.[12]

These complications are attenuated in the reduced sample we use for the second-stage regression. When only instances of change are selected, there are a few countries with single observations, many noncontemporaneous observations across countries, and many interruptions in the time series within countries. Nevertheless, we address these issues using panel-corrected standard errors for the regression estimation, with a control for

11. O'Loughlin et al. (1998) and Gleditsch and Ward (1997) are exceptions, but they use few of the other controls and corrections that we use.

12. A hazard analysis would be an alternative approach to the low incidence of change and some of the case selection issues encountered here. Indeed, some of the most important and interesting studies of diffusion use this method (Most & Starr, 1990; Starr & Lindborg, 2003). We chose the two-stage model partly to produce separate models for the likelihood of change and the magnitude and direction of change, which we find substantively and theoretically interesting, and partly to address more effectively many of the other issues we discuss above.

first-order autocorrelation. To control for time-specific global effects, we test a series of annual dummies, retaining all those that are significant. The results of this analysis (Model 6) are presented in Table 3. It is also possible, as with any cross-sectional time-series data, that our results simply reflect country-specific differences (Beck, 2001, pp. 284-285) or that variations across countries obscure the true effect of our variables on variation within countries. To rule out these possibilities, we add country dummies to Model 6. The results of this fixed-effects model (Model 7) are also presented in Table 3.

We also take steps to avoid potential problems associated with the use of our diffusion variable. Because we construct our diffusion variable from the democracy scores of neighboring countries—which are hypothesized to exert a mutual influence on each other—we run the risk of introducing a simultaneity bias into the model. We avoid this possible simultaneity bias primarily by lagging the diffusion variables: Country k at t has an effect on country i at $t + 1$ whereas, of course, i at $t + 1$ does not have an anachronistic effect on k at t. This formulation avoids simultaneity but introduces a new issue. Our model is equivalent to the mutual adjustment model described in Hanushek and Jackson (1977, pp. 169-170), which is known to be equivalent to a model with a lagged dependent variable. It is therefore consistent in large samples and prone only to small-sample bias. A mathematical derivation of our model (available on request) demonstrates this point.

To assess the severity of this small-sample bias, we ran Monte Carlo simulations, varying assumptions about the nature of a possible correlation in the error terms among neighboring countries and different variations on the structure of the data.[13] These simulations show that any small-sample bias that might be present would not be large enough to materially affect the results of our analysis under conditions similar to those we find in our data. Even in a small data set (two countries over 24 years) in which the bias should be greatest, the small-sample bias would not inflate the coefficient enough to produce spurious significance unless the errors in neighboring countries were negatively correlated at very high levels. Given the messiness of the real world, and given further that we actually model the impact countries have on each other and include all the standard controls for domestic and regional variables, it is inconceivable that we would consistently overpredict change in one country and simultaneously underpredict change in its neighbors with such mathematical precision.

As a final check, we removed all the potentially problematic sequential observations from the data set and reran the analysis. By removing sequential

13. We are very thankful to Mitch Sanders for his assistance in programming the Monte Carlo simulations and for his comments as we worked through these issues.

observations, we deleted any cases in which a lagged variable on the left side of the equation might also directly influence a variable on the right side because of first-order autocorrelation. Our results, despite the loss of a number of cases, did not vary significantly from the full-sample findings. Further details about any of these calculations are available from the authors.

Adjusting for Truncated Measurement of the Dependent Variable

There is one other nonsubstantive control that must be introduced. As shown in Table 2, Model 4 suggests that countries that are already high democratic achievers tend to decay—they are less likely than others to become more democratic. In reality, however, this may simply be an artifact of using a measure of democracy that is bounded at both ends: Countries at the top of the scale have no place to go but down, whereas countries at the bottom can only improve their scores, causing a regression to the mean. The same characteristic of the scale might cause difficulties for the first-stage probit analysis. If the scale does not register the likely variations above or below the bounds, then countries at either extreme will register less variation in scores than countries in the middle, even if they are changing just as much.

To control for this idiosyncrasy of the scale, we include an appropriate correction factor in each analysis. The probit stage includes a bell-curve adjustment that takes on values of 0 at the extremes of 2 and 14 and a maximum of .25 in the middle.[14] The adjustment factor in the second-stage regression equation is equal to $(8 - FH_{t-1})/6$, producing a linear function descending from +1 if a country was at score 2 in the previous year to –1 if the country was at score 14. We allow the regression to determine how much weight to give these adjustments; if we are wrong about the consequences of using a truncated scale or misjudge the shape of the adjustment function, these controls will simply turn out nonsignificant.[15]

In summary, in deciding on the most appropriate estimator, we do not simply make assumptions about the error structure or ignore possible sample selection biases but instead, test different approaches to rule out the possi-

14. This adjustment is based on the sine function. As FH increases from 2 to 14, $.5*\{\text{sine}[15*(FH - 2) - 90] + 1\}$ follows a sine curve rising from 0 to 1. The adjustment used is this function times (one minus this function).

15. One possible substantive interpretation of the bell-shaped control in the first stage is that regimes that are neither fully democratic nor fully authoritarian are less stable than those at either end of the scale. This is consistent with Starr and Lindborg's (2003) finding that partly free countries are less stable than either not free or free countries. Although there is likely some truth to this observation, it is impossible at this point to know whether our results are evidence of this empirical pattern or an artifact of the Freedom House (n.d.) index.

bility that anomalies in the disturbance matrix are driving our results. The final model, for these reasons, is a two-stage model, using probit with robust standard errors for the first stage, and a Prais-Winsten regression (panel-corrected standard errors and a correction for first-order autocorrelation) for the second stage, first with regional fixed effects and then with country fixed effects. The final model includes not only the substantive controls included in Model 4 but also corrections for autocorrelation, cross-panel heteroscedasticity, a bounded scale, and selection bias. The results for our diffusion variables are robust to all these controls and methodological variations whether we introduce them singly, in various combinations, or omit them entirely.

Findings

The appendix presents the results from the first-stage model (Model 5 in Table 4). Here we present and discuss only the results of the second-stage model. Models 6 and 7 demonstrate that once we estimate robust standard errors and include controls for selection, autocorrelation, global effects, and various other considerations, some of our earlier estimates from Model 4 are shown to be overconfident, whereas others reach significance for the first time. However, none of the diffusion variables is seriously affected.

As we see in Table 3, neighbor emulation remains significant even when we correct for heteroscedasticity across countries, whether we use regional or country dummies. Global trends are highly significant and of the expected sign. The U.S. and Soviet effects are nonsignificant in the country-level fixed-effects model. This is not surprising, because there is so little variation in these variables within any given country that they are hard to distinguish from fixed effects. In Model 6, however, the U.S. effect is significant. This confirms that during the past 20 years, countries commonly understood to be aligned with the United States have taken greater steps in a democratic direction than other similarly situated countries, although the policies of particular administrations do not appear to have affected that movement to any significant degree.[16]

16. In preliminary models, we estimate a different coefficient for each superpower administration to test for the impact of different foreign policies of different U.S. administrations and the difference between the foreign policy and influence of the Soviet Union as compared to post-1989 Russia. It is perhaps surprising that we find all U.S. administrations had more or less the same impact, with nonsignificant differences, whereas the impact of the Soviet Union was consistently nonsignificant, even well before its collapse. Our final models, therefore, estimate only the impact of each superpower.

Table 3
Models 6 and 7:
Full Models With Substantive and Methodological Controls

	Model 6: XTPCSE, AR1		Model 7: XTPCSE, AR1, FE	
Dependent Variable: Δd	Coefficient	*t* Score	Coefficient	*t* Score
Diffusion variables				
Diffusion variable (t – 1)	0.065***	(2.49)	0.229****	(4.46)
Global trend in Δd	0.592****	(5.02)	0.531****	(4.67)
U.S. effect	0.099***	(3.08)	0.040	(0.91)
Soviet effect	–0.010	(–0.13)	0.038	(0.41)
Technical correction variables				
Correct for democ (t – 1)	1.231****	(4.52)	2.046****	(5.17)
Lambda	0.230	(0.78)	0.725**	(2.44)
Domestic variables				
Per capita GDP	0.195**	(2.35)	0.584**	(2.12)
Former Anglo colony	0.126	(0.74)	3.731***	(3.06)
Presidential democ (t – 1)	–0.344	(–1.35)	0.353	(1.02)
Democ (t – 1) dummy	0.396	(1.31)	0.233	(0.70)
Regional variables[a]				
Northern Africa	–1.167***	(–2.92)		
Middle East	–1.538****	(–4.10)		
Central America	–1.128***	(–3.16)		
South America	–0.931**	(–2.46)		
Former Soviet Union	–1.139***	(–3.08)		
Pacific State	–0.248	(–0.50)		
Gulf State	–1.578***	(–3.48)		
South Asia	–0.940**	(–2.14)		
Sub-Saharan Africa	–0.798**	(–2.11)		
South East Asia	–1.261***	(–3.05)		
East Asia	–0.668	(–1.66)		
Southern Europe	–0.250	(–0.77)		
Southern Africa	–0.731	(–1.47)		
Eastern Europe	–0.063	(–0.17)		
Caribbean	–0.768**	(–2.26)		
Constant	–0.978	(–1.25)		
Model statistics				
R^2	.181		.378	
Wald χ^2	192.42		682923.1	
Prob > χ^2	0.0000		0.0000	
Rho	0.110		–0.001	
N = 1,026				
N of groups = 184				
Observations per group				
Minimum = 1				
Average = 5.6				
Maximum = 16				

Note: XTPCSE, AR1 = panel-corrected Prais-Winsten estimates; XTPCSE, AR1, FE = panel-corrected Prais-Winsten estimates with fixed effects (XTPCSE is a command in STATA).
a. Regional dummies excluded from Model 7 in favor of country dummies; country dummy results not reported for clarity of exposition.
$^{**}p < .05$. $^{***}p < .01$. $^{****}p < .001$.

Once we control for correlated disturbances, most of the regional variables become significant. Lambda, the correction for sample selection, is significant and positive in the fixed-effects model (Model 7) but not in Model 6.[17] These results for lambda suggest that although countries are more or less randomly dispersed in their reaction to this stimulus, when we look at within-country variations in readiness to change, we find that the more ready a country is to change in any given year, the further it goes when it does change. The insignificance of lambda in the more general model that attempts to average these effects across the entire world is entirely consistent with the transitions literature, which places the focus on short-term political variables that cannot be modeled in a large-*N* study.[18]

Contrary to Przeworski and Limongi (1997), we find that greater wealth is associated with more movement in a democratic direction. If we use panel-corrected standard errors, we find levels of GDP are positive and significant in both models (at the .01 and .05 levels, respectively). The discrepancy between our findings and theirs, however, may simply be because of their use of a dichotomous dependent variable. The relationship we find between income and democratization is a logarithmic one, so that income must increase tenfold for each quarter point of additional change in democracy level. As a result, almost all of the impact of per capita GDP on change is registered at GDP levels below US$6,000. These low incomes tend to correspond to low scores on the democracy scale. The improvements we observe, therefore, occur at levels that would be considered subdemocratic in the Przeworski and Limongi dichotomy and so, would pass unnoticed. But the mean GDP value in our sample is approximately US$4,600, and nearly 75% of all cases fall below the mean. As a result, our estimates are highly relevant to the vast majority of countries that are struggling with low levels of wealth and low levels of freedom at the same time. This result underscores the value of using more continuous measures of democracy (Elkins, 2000).

As before, the presidential democracy dummy is nonsignificant. And once we control for previous levels of democracy, we find that the high-performing democracies identified by the democracy dummy behave, ceteris paribus, like all other countries. On the other hand, when we include controls for prior levels of democracy and use panel-corrected standard errors, many

17. To rule out the possibility that results for the diffusion variables are a result of the inadequacy of our correction for sample selection, we run the model on the full data set. The results for diffusion remain significant.

18. The pseudo R^2 of Model 5 is only .13 and its predictive power is modest: It predicts nearly 90% of the nontransitions, but only about 30% of the transitions, for an overall success rate of just more than 70%. Improvements in the first-stage model may yield a variable that predicts both sets well.

of the *regions* are shown to be consistent democratic underperformers. In response to similar conditions—international influences, wealth, and so forth—countries in Northern Africa, the Middle East, Central America, South America, the former Soviet Union, Pacific States, the Persian Gulf, South Asia, Southeast Asia, and the Caribbean did not move as far in a democratic direction as their counterparts in Eastern Europe, Southern Africa, Southern Europe, East Asia, sub-Saharan Africa, and the control group in Western Europe.[19]

Using the results from Model 7, we can see that the substantive impact of neighborhood effects on Δd is quite substantial. When the average difference between the target state and its neighbors is 1 point, we would expect an annual change of almost .25 points on the Freedom House Scale in the target state, or a change of 1 point every 4 years. But in our model, neighbors affect each other with time, and these effects spread to others in adjacent networks. To demonstrate the effects of diffusion in a world system of interrelated networks of states, we simulate the effect of the difference between two states on each other and then on the adjacent network of countries. For this simulation, we model 16 states, each contiguous with 2 neighbors on the left and 2 on the right (except for the end-most states). At time t, 3 states are high-performing democracies, scoring 14 on our scale, whereas the others are in the depths of authoritarianism, all scoring 2. We hold all other variables constant and run the model forward several years.

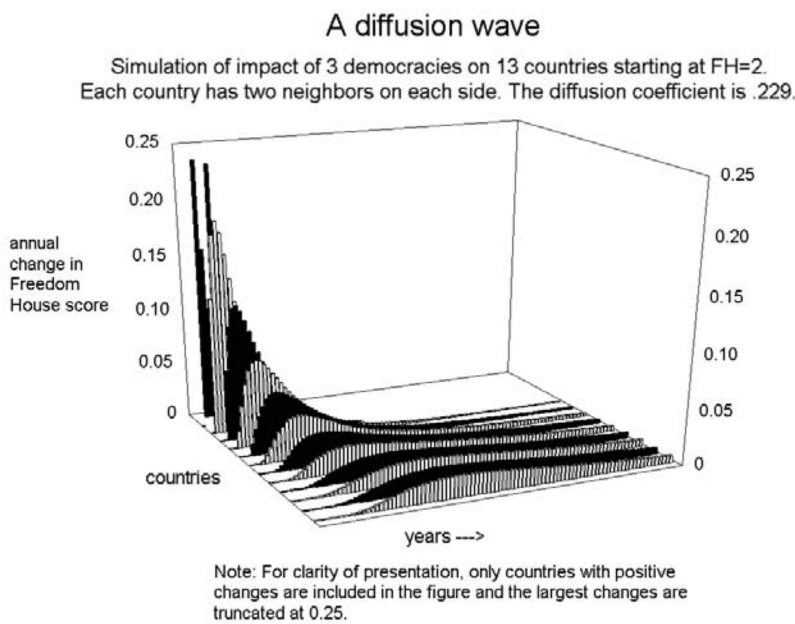
Figure 1 plots the values of Δd with time and across space, showing the impact of the more democratic neighbors on the authoritarian regimes and how that impact spreads across the authoritarian neighborhoods until they all stabilize at an average level of democracy. The large difference between the democratic states and their contiguous authoritarian neighbors produces a large positive change in the latter in Year 1. (The negative impact on the former is omitted for clarity.) The mutual effect decreases the gap between the two, so that in following years there are ever-smaller impacts. The diffusion effect also ripples across the remaining states, as neighbors are "infected" by neighbors. The impact of the global trend is even more substantial. For every additional point in the average change worldwide, we expect a change of half a point in the target country.

Conclusion

Our analysis shows that regimes linked in some networks exert a measurable force on each other. Any model exploring the determinants of democra-

19. Regional variables, being redundant fixed effects, are dropped from the fixed-effects model.

Figure 1
A Diffusion Wave



Note: Simulation of impact of three democracies on 13 countries starting at FH = 2. Each country has 2 neighbors on each side. The diffusion coefficient is .229. For clarity of presentation, only countries with positive changes are included in the figure and the largest changes are truncated at 0.25.

tization that does not account for these spatial relationships is underspecified. To the extent that the omitted spatial variable correlates with variables included in the model, the consequence is not just a loss of efficiency but also a bias affecting any substantive conclusions we might draw. We urge researchers, therefore, to account for diffusion effects in methodologically sound and theoretically interesting ways. Our data are available to any researchers who wish to explore other ways of doing this.

It is certainly possible to model democratic diffusion in different ways. Rather than making changes in the level of democracy the dependent variable, one could seek to explain the levels themselves—a democracy/nondemocracy dichotomy, some other kind of regime change, or the duration of any kind of regime. In a similar manner, the stimulus from the sending state need not be the democracy gap between sending and receiving states; it could be an average democracy level, a dummy for regime change in any sending state, a count of the number of changes during periods of time, and so on. Our study focuses on two geographically defined networks—contiguous neighbors and a single global network—and a political network—superpower spheres of influence. However, researchers could also test for other politically defined networks (such as international organizations), economically defined networks (trading partners), culturally defined networks (common languages, colonial histories), or religious networks.

In this project we assume that all countries matter equally, but it may be that large, powerful, populous, or prosperous countries matter more than others. We find no evidence for this here; in fact, as we note, our preliminary analyses—not reported here—suggest that neighbors tend to have equal influence. But this seems to be an obvious avenue for further investigation. Future researchers could also choose whether to follow our lead in modeling a selection or priming stage. The nature of our testing precludes any empirical examination of the nature of the causal mechanisms; the best we can offer in that regard is a sketch of a theory that makes neighbor emulation plausible. However, this is an interesting question that a different research design could address.

Appendix
Results of First-Stage Probit Analysis

To control for sample selection issues in our regression analysis, we use a modified version of STATA's Heckman estimator.[20] The results are presented in two stages. The first stage is a probit analysis of the probability that a country will change in a given year based on the variables that we hypothesize to be related to regime stability. The second-stage regression models the direction and magnitude of change in those countries that do change, including a variable (λ) that is derived from the probit analysis and corrects for the possible effects of selection bias. The results of the first-stage model, Model 5, are presented in Table 4.

The first-stage probit is also applied to a cross-sectional time-series data set, so we use robust standard errors clustered on countries in this analysis. In addition, following Beck, Katz, and Tucker (1998), we include time dummies in a preliminary model,

20. Sample selection models make it possible to obtain better estimates of the influence of the second-stage factors if one is able to model the first stage fairly well (Breen, 1982). However, STATA's maximum likelihood algorithm is inappropriate for our problem of, in effect, censoring the outcomes in the center of the distribution, as it is written for the more typical problem of censoring one tail of a distribution, whereas our data present what Moses (1968) refers to as "inner truncation" (p. 196). Fortunately, Kajal Mukhopadhyay derived a modified estimator that uses both tails, minus the zero cases, to correct the error distribution. Lambda is given by $\lambda = [\phi(a - Zg) - \phi(-a - Zg)]/[1 + \Phi(-a - Zg) - \Phi(a - Zg)]$, where ϕ is the standard normal density func-

marking the number of years since the last event, to see if our results are an artifact of the pooled time-series structure of the data. The dummies are not significant, singly or as a group, and our results are not otherwise affected, so we drop them. A simple counter marking the number of years since the last transition, on the other hand, is significant and negative, so that the more time has elapsed since the last transition, the less likely another change becomes. This produces estimates equivalent to those of a proportional hazards model.

The average amount of instability in the network in the preceding year, measured as the mean of absolute values of Δd for contiguous countries, is significant but of the opposite sign than we expected, so that instability in neighboring countries results in less change in the target country. It may be that instability in the network acts as a signal to the regime that it should clamp down on change rather than as a signal to the opposition that change is possible—at least in the short term.[21] The average difference between the target country and each of its neighbors, weighted by the difference in per capita GDP, is almost but not quite significant ($p > .07$).

The more a country within the U.S. sphere of influence differs from the United States, the more likely it is to experience a change. It is unlikely, however, that this can be attributed to U.S. foreign policy, because the policies of various administrations during the period covered in this study differ greatly, whereas the effect is essentially constant for the period. We initially ran the model with separate variables for each U.S. administration but collapsed them when we found that all the variables had nearly identical impacts on the probability of change. Countries within the Soviet sphere of influence, on the other hand, are neither more nor less likely to change during this time than similarly situated unaligned countries, once we introduce all the other controls.

Although it is of the expected sign, and although it comes close to significance ($p <$ .07), per capita GDP does not have a statistically significant impact on the probability that a country will change. This result contradicts the finding of Przeworski et al. (2000) that more affluent countries are less likely to change regardless of the regime. Only when we run the model without the regional dummies do we find that GDP becomes highly significant and negative. Because this is not the focus of this article, we leave open the question of whether the propensity to change is better explained by regional disparities of wealth or other characteristics. We do find support for Linz's

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tion, Φ is the cumulative standard normal distribution function, the Zg are the probit predictions of the probability of a case being selected into the sample of nonzero observations, and a is an arbitrary distance from the zero center of the distribution of Zg. In this application, we set a to .616, which is the Zg value that corresponds to the observed frequency of change. We are extremely grateful to Mukhopadhyay for deriving this estimator and to Vince Wiggins of the STATA Corporation for explaining how the STATA algorithm works.

21. Because this result is contrary to our expectations, and to rule out a spurious result, we tested a number of similar network variables, including average Δd, sum of Δd, sum of absolute values of Δd, average number of transitions, sum of transitions, and others, all with a 1st-year lag. None of these variables performed as well, although some approached or achieved significance, and all had a negative tendency. In longer lags, all lost significance.

Table 4
Results of Model 5 (First-Stage Probit Analysis)

Probability of Change in Levels of Democracy	Coefficient	*z* Score
Diffusion variables		
Absolute value of Δ*d* in network (*t* – 1)	–0.119**	–2.43
Years since last episode of change	–0.025****	–3.65
GDP-weighted neighbor diffusion effect	–0.003	–1.81
Soviet effect	–0.011	–0.31
U.S. effect	0.060****	4.19
Domestic variables		
Per capita GDP	–0.055	–1.84
Presidential democracy (*t* – 1)	0.179**	1.98
Democracy dummy (*t* – 1)	–0.159**	–2.06
Correction for lagged democracy level	3.043****	9.51
Regional control variables		
Northern Africa	0.574****	3.63
Middle East	0.407**	2.26
Central America	0.392**	2.31
South America	0.282	1.79
Former Soviet Union	0.947****	5.09
Pacific State	0.019	0.12
Gulf State	0.232	1.44
South Asia	0.537***	2.62
Sub-Saharan Africa	0.614****	4.07
South East Asia	0.467***	2.89
East Asia	0.318	1.83
Southern Europe	0.583****	4.09
Southern Africa	0.170	0.78
Eastern Europe	0.546****	3.55
Caribbean	0.495****	3.51
Year dummies		
1979	0.263**	2.52
1989	0.842****	7.75
1990	0.456****	4.09
1991	0.353***	3.17
1992	0.422****	4.10
1993	0.856****	8.01
Constant	–0.995****	–3.46
N = 3,842		
Wald χ^2 = 553.29		
Log likelihood = –1,934.29		
Pseudo R^2 = .1324		

**$p < .05$. ***$p < .01$. ****$p < .001$.

(1994; Linz & Stepan, 1978) theory that presidential regimes are less stable. Although the results are weak and not very robust to different specifications, presidential regimes seem more likely to experience a regime change than their democratic peers.

Regional variables and the control for previous levels of democracy are significant. Even with the extensive controls we introduce in the model, countries in most regions experience more unexplained change than the control group, the advanced industrial democracies. As expected, the bell-shaped control for prior levels of democracy is highly significant, highlighting the importance of controlling for the bounded nature of the democracy scale in looking for the determinants of regime stability. Finally, the year dummies show a superabundance of transitions in 1979 and a more prolonged wave of regime changes going from 1989, the fall of the Soviet Union, to 1993.

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