Patterns of Diffusion in the Third Wave of Democracy

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I.   <u>**Introduction**</u>

Until very recently comparativists were confident that economic development helps cause countries to become more democratic (Lipset 1959; Jackman 1973; Bollen and Jackman 1985; Brunk, Caldeira, and Lewis-Beck 1987; Gonick and Rosh 1988; Diamond 1992; Rueschemeyer 1991; Hadenius 1992; Helliwell 1994; Muller 1995a & 1995b; Coppedge 1997).[1] This belief was recently discredited by Przeworski and Limongi (1997) and Przeworski, Limongi, Alvarez, and Cheibub (1996), who produced extensive evidence that economic development certainly helps democracies remain democratic, but does not explain why they became democratic to begin with.

This finding brings empirical research back to the question that transitions theorists asked 15 years ago: what causes the dynamics of democratization, as opposed to the stability of democratic regimes? Most of the theory and empirical research that has issued from that line of questioning has been focused on strategic actors interacting on timescales of months, weeks, or days, and therefore cannot be tested using the existing large comparative datasets in which the units of analysis are countries observed, at best, once a year (Coppedge 1999). However, some of the transitions literature also suggests one structural hypothesis that can be tested: the idea that democracy diffuses. The claim is that democratization is partly driven by forces originating outside a country's borders, rather than being a self-contained domestic process. This is an old idea, originally attributed to Kant, but sustained more recently by Rustow (1970), Whitehead (1986), and Huntington (1991), and other scholars cited below. But mainstream political scientists have by and large been rather vague about the concept of diffusion, rarely going beyond the notion that, in some poorly understood way, regional, global or other transborder processes may affect the timing or the direction of regime change in a given country.

Here we define various patterns of diffusion, predict hypothesized consequences, and report preliminary results of our tests for a few of the many possible patterns of diffusion using global data from the Third Wave and after (1973-1996). We find strong support for a pattern of diffusion in which countries tend to become more like their immediate geographic neighbors over time. Contrary to expectation, however, we do not find that countries with large populations, economies, or land areas or high standards of living have more influence over their neighbors than others; rather, countries seems to have approximately equal weight. We identify certain trends that are statistically associated with certain US administrations: Reagan's first and second administration in particular seems to have witnessed more movements in democracy among US allies at the lower end of the Freedom House scale, and those movements tended to be in a positive direction. We also find that economic development plays a rather paradoxical role in democratization. On the one hand, the wealthier a country is, the less likely it is to change its level of democracy in any direction. But on the other hand, if a country is at the lower end of the wealth spectrum, or if other conditions combine to force a wealthy country to change, then the wealthier a country is, the larger and more positive its change tends to be.

The paradoxical influence of prosperity illustrates a significant difference between our study and similar ones. After exploring the differences between explanations of the stability of democratic or authoritarian regimes and the variables associated with the selection of the resulting regime, we propose a new stage in the process of democratization. Scholars conventionally distinguish between the stages of "transition" and "consolidation." Some go further and discuss a stage of breakdown of the authoritarian regime (liberalization), democratic

deepening, and other possible stages. Our review of the data and the literature has persuaded us that it is necessary to theorize about a stage of "selection," or becoming primed for change, that takes place before any transition and is independent of the direction of any subsequent change. We argue that this prior process should be taken into account in order to understand the process of transition.

II.   Discussion

In the first section of the discussion, we first explore findings and arguments from the general democratization literature that are relevant to the study of diffusion, then define and describe our conceptualization of the diffusion processes at issue, and finally evaluate the potential advantages of our research design over others that have been used. In the second section, we describe our methodology, explaining and justifying the estimation techniques we use, the way we operationalize the diffusion variables, and the control variables we include in the model. In the final section we explore the results of our tests, and the answers they provide to two different questions: Why do countries change in a given year, and not in others? And, why do countries choose to become more (or less) democratic, once they begin the process of change?

A. Patterns of Diffusion

1. Diffusion in the Democratization Literature

Many democratic theorists have neglected diffusion, implicitly assuming that the critical variables determining a political system are domestic ones.[2] But other scholars have included aspects of transnational causation in their theories. Some, for example, have noted that certain

countries can trace their institutional framework to an earlier colonial or occupying power (*e.g.,* Rustow 1970, 348; Dahl 1971; Stepan 1986; Lipset, Seong, and Torres 1993, 168-70). A more modern, and more negative, influence of the great powers was integral to dependency theory: this theory argues that countries in the periphery and semiperiphery of the world economic system are *less* likely to become democratic than those in the core, because elites outside the core repress their populations in order to maintain an exploitative alliance with elites in the core (Wallerstein 1974, Cardoso & Faletto 1979, O'Donnell 1979). Transnational influences have also been cited to explain the wavelike pattern of democratic transitions and breakdowns – whether the vehicle for such influence is a worldwide normative shift for or against democracy, the end of Soviet hegemony in Central and Eastern Europe, the political pressures accompanying economic integration, or a reorientation of the Catholic Church (Huntington 1991, Diamond 1996). Most recently, scholars in both international relations and comparative politics have welcomed research on the impact of transnational activist networks on domestic politics, including some research on human rights and democracy (Sikkink 1993, Risse-Kappen 1995).

But diffusion gets more attention in theory than it does in empirical research. Much of the empirical research on democratization simply ignores the possibility that democracy may diffuse across national borders. The bulk of the research has dealt with cross-national variation only (Lipset 1959; Cutright 1963; Olsen 1968; Jackman 1973; Powell 1982; Brunk, Caldeira, & Lewis-Beck 1987; Seligson 1987; Arat 1988; Mainwaring 1993; Stepan & Skach 1993; Berg-Schlosser & De Meur 1994; Linz 1994). Another type of research has dealt with changes in levels of democracy within one country at a time (Moore 1966, Skocpol 1973, Linz & Stepan

1978, O'Donnell and Schmitter 1986, Putnam 1993). Yet other research has combined cross-sectional and time-series analysis, but still without close attention to transnational influences (Cutright & Wiley 1969, Gasiorowski & Power 1997). And of course some scholars have theorized about diffusion without testing their hypotheses systematically with a large sample of countries (Rustow 1970, Dahl 1971, Cardoso & Faletto 1979, O'Donnell 1981, Huntington 1991, Kasza 1996, Sikkink 1993).[3]

However, all the empirical research that has addressed democratic diffusion has confirmed the significance of transnational influences of one sort or another. First, Starr (1991) found that regime transitions take place closer together in time than mere chance would predict, and that countries whose contiguous or regional neighbors have experienced transitions in the three prior years are more likely to undergo a regime transition themselves. Second, Przeworski *et al.* (1996, 43) report that the more democratic neighbors a country has, and the more democratic countries there are in the world, the more likely an existing democracy is to survive. In fact, Przeworski *et al.* state that "international conditions predict regime survival better than does the level of development" (1996, 43). The significance of this geopolitical dimension is consistent with a second result, that countries in certain geographical regions are systematically either more democratic or less democratic than purely domestic models of democratization would predict (Hannan & Carroll 1981, Helliwell 1994).[4] In fact, this regional variable is the closest that much of the mainstream literature comes to accounting for foreign influences, and in many cases it is more of an attempt to control for regional similarities than to address issues of diffusion.

Third, many studies confirm that former British colonies are more likely to be democratic,

even when controlling for domestic determinants of democracy (Bollen & Jackman 1985; Lipset, Seong, & Torres 1993; Bollen & Jackman 1995; Muller 1995a; Muller 1995b). This implies that the past diffusion of democratic institutions or a pro-democratic population or culture continues to have an impact on current political practices. Finally, many studies report that countries in the periphery or semiperiphery of the world system are less democratic than those in the core, even when controlling for the level of economic development and other variables (Bollen 1983; Bollen & Jackman 1985; Gonick & Rosh 1988; Muller 1988; Lipset, Seong, & Torres 1993; Burkhart & Lewis-Beck 1994; Bollen & Jackman 1995).[5]

Finally, a recent article by O'Loughlin, Ward and others bears significant similarities to ours in that it is a global analysis explicitly aimed at uncovering diffusion effects, using a continuous measure of democracy over a long series of years (O'Loughlin, *et al.* 1998). They use the Polity III data set to measure levels of democracy, and "map and graph changes in the number and nature of political regimes to explore the spatial and temporal regularities and oddities of the process of democratic diffusion" (1998: 545). They generate a "contiguity matrix" for the whole world, and use measures of geographic clustering to explore the diffusion of democracy. They find that, "even controlling for GDP per capita, there remains strong clustering of the political democracy scores" (1998: 557). The relatively modest conclusion of this sophisticated analysis captures what is probably the state of the art in the diffusion of democracy literature: they find a "high degree of regularity and evenness across time and space … [but] are not yet able to state the exact nature of the diffusion process."

2. Our Model of Democratic Diffusion

Everett Rogers defines diffusion as "the process by which an *innovation* is *communicated* through certain *channels* over *time* among the members of a *social system*" (Rogers 1995:10, emphasis his). It is useful, then, to begin by specifying what each of these four elements looks like in our study. Most of the political science literature has focused on the diffusion of democracy, variously defined, though there is one study of the diffusion of *coups d'etat* (Li & Thompson 1975). In our study we do not assume that only democracy has the ability to cross borders, but rather test simultaneously for the influence of both more and less democratic countries on the potential adopter's movements up or down on a Freedom House-based scale of democracy. The innovation with which we are concerned, therefore, is a (relatively) continuous measure of regime type that ranges from very authoritarian regimes to more democratic ones, a slightly broader concept than democracy *per se*.

In addition, in keeping with the notion of diffusion, which after all concerns the adoption of a new idea or technique, we look for <u>changes</u> in levels of democracy in the target country. Our dependent variable, therefore, is not a static measure of levels of democracy, or the expected longevity of democracy, but an indicator of change in the nature of the regime over the previous year – evidence that, indeed, that country has adopted a new idea over the previous year's (though not necessarily one that it had never tried before).

The second element is the channel of communication. In this study we do not test directly for particular vectors of the message. Our research design, covering all the countries of

the world for a fairly lengthy period, makes it impossible to gather specific information about how ideas about regime change might be carried from one country to another. We gather some information indirectly that might suggest that one or another channel is more likely, by using different predicted patterns of diffusion, but it is not direct evidence of the way in which new ideas enter into and are adopted in any given country. We do not know, therefore, if ideas about regimes are being carried by graduate students studying abroad, news reports, high level meetings of foreign ministers, or pressure by international financial institutions. What we do test for is the effectiveness of the communication among members of a particular network, by estimating as nearly as possible the impact of diffusion on the potential adopter. In fact, we believe we have employed the most appropriate statistical estimation techniques and gathered the most extensive data set, in order to obtain the most precise measure to date of the extent of the impact of diffusion on an innovation adopter.

Moreover, the communication also must be triggered by some behavior in the country of origin – that is, we had to specify what behavior in the other countries in the system actually sends the message down the channel of communication. It could be a simple demonstration effect of the benefits of an alternative regime type. In this case, the independent variable would be the other countries' level of democracy or, in a dichotomous sense, whether they are democratic or not. Alternatively, change could be prompted by viewing transitions in other countries as a lesson that current powers and institutions can be overcome, so that change breeds change (or, to put it another way, instability in the neighborhood is destabilizing), without necessarily directing the type of regime that results from the change. Or transitions to

democracy could be prompted by other countries' transitions to democracy, but not by transitions in the opposite direction.

The independent variable in a study of diffusion, therefore, could be the timing of change in other countries' level of democracy, or the direction of change; or the size (and implicitly, the direction) of change; or the gap between the other countries' level of democracy and the existing level in the target country. Each of these possible messages implies a distinct theory of how democratic diffusion works, and can yield quite different theoretical predictions. Here we theorize and test primarily what might be called a convergence theory of diffusion: the notion that countries will tend to become more like each other, and the greater the distance between the network's average levels of democracy and the potential adopter's level, the greater the pressure for change. We also test for the impact of a global demonstration effect, measured as the average of all changes in the world for the current year. And we test for the impact of superpower foreign policies on the countries that are most unlike the superpower with which they are associated.

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 1995:220). Similarly, the literature on regime transitions suggests that countries may need particular triggers – triggers that are quite distinct from any variables associated with democracy per se – in order to break the inertia of the existing regime and adopt a new structure. Thus, some have theorized that a breakdown in state mechanisms of control must precede any revolutionary change (Skocpol 1973), or that economic crises might trigger the breakdown (at

different rates) of both authoritarian and democratic regimes (Przeworski et al.1996), or conversely, that rapid economic growth might create pressures for the breakdown of democratic regimes (Huntington 1968), or that a split in the ruling authoritarian regime must precede any transition toward democracy (O'Donnell & Schmitter 1986).

In order 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. Thus we use an estimation technique that accounts for the fact that the impact of the variables that affect which regime a particular country would adopt in any given year may be obscured by the presence of other variables that determine whether that country can change from its present regime. This is a significant change from, and, we think, improvement on other large-N studies of regime change.

A simple example may clarify this concept. All the relevant actors in country x may have gradually come to be strongly in favor of adopting a parliamentary democratic structure in that country, and all the structural conditions necessary to put in place and sustain such a regime may have presented themselves at time t_{-5}. But any movement away from the present regime may be at least temporarily foreclosed by the presence of an autocratic ruler who commands the personal loyalty of the leaders of the armed forces. In such circumstances it may well take the death of the ruler at time t to free up the forces that will move the country toward greater democracy at t_{+1}. None of the variables that determine the regime type that will ultimately be adopted will have changed from t to t_{+1} – indeed, in our example they appeared at t_{-5} – but their impact will only become observable at t_{+1}. On the other hand, we cannot know, especially in a large sample

such as the one we use, whether regime change is impeded by the presence of some blocking factor, or because the variables of interest are simply not effective at producing the outcome we are testing for.

Thus, if we use standard regression techniques to estimate the effect of these variables, their true impact may be obscured by the instances of no change at t_{-5} through t, which are actually attributable to other factors altogether. But if we only look at instances of transition (that is, if we select for inclusion in the sample only those observations for countries and years that actually show some change) we may be ignoring many countries in which the hypothesized democratization variables are present but produce no change even in the absence of any particular forces preventing a transition. This would lead to an overestimation of the impact of these variables. We attempt to control for both of these dangers by explicitly adopting an estimation technique that tests for the likelihood that country i at time t will be selected for change, and uses the results of that analysis to give us the impact of the independent variables on the direction and extent of change, given the probability of change.[6]

The fourth element identified by Rogers is the social system, which in this context is a network of countries. The most obvious criterion for inclusion in a given network is geographical proximity, which can be subdivided into networks of neighbors, various regional networks, and a single global network. But networks need not be based on a criterion of proximity; they may link together geographically dispersed countries that share colonial or other historical ties; cultural ties such as a common religion, languages, ethnicity, or even exposure to the same mass media; economic ties such as trade, investment, or travel; or political ties such as

alliances and membership in international organizations. There are at least a dozen different ways of specifying which countries are linked together in a diffusion network. We have selected three of them. Simple contiguity is our main criterion for inclusion and, we think, a beginning step for the study of the spread of democracy. It is theoretically the most simplistic but in execution the most complex, as each country has a different set of neighbors, and is in effect in a unique network that overlaps with other networks.[7] In addition, we use one variable that is derived from behavior in a single global network, and one that assumes politically defined networks of superpower influence, grouping countries into those aligned with the Soviet Union, the US, or neither.

Moreover, each member of a social system or diffusion network may not be equal to all the others. In fact, diffusion research suggests that each system will have certain opinion leaders or change agents that are afforded particular credibility, and will have greater influence over the other members of the network. Most of the studies of diffusion implicitly – and counter-intuitively – assume that all countries are created equal, giving each country equal weight in the model. Our study explores various factors that may determine which countries have more influence than others, i.e., what the set of *weights* is. We test, for example, whether countries with a high standard of living have more influence than poor countries, or whether countries have greater weight if they have large populations, take up a lot of space on a map, or produce lots of goods and services.[8]

As noted, all of these elements can be combined in a great variety of ways, to produce a large number of possible diffusion patterns. For example, one could propose a population-

weighted impact of levels of democracy on levels of democracy in linguistic networks, or a wealth-weighted impact of direction of change on magnitude of change among neighbors; or an un-weighted impact of gaps on timing of change among trading partners; and so on. In some instances, we have simply chosen which parameters to use based on the theory, the feasibility and the current stage of our research, while in others we actually test various arrangements in order to evaluate competing hypotheses about what matters.

To summarize our model: we include only each country's immediate neighbors in the diffusion network that produces our primary diffusion variables. We also include a global measure of diffusion based on average worldwide democracy scores, and we test for the influence of the two main superpowers. We test a variety of different weights, to see if countries that are larger, more populous, produce greater wealth for its citizens (as measured by per capita GDP), or have a greater economic production serve as opinion leaders within each network. We theorize that differences in democratic practices are the message that motivates change, and that the greater the gaps between other actors' levels of democracy and the target's the greater the changes will be. Finally, we test to see if these variables affect (1) the probability that a country will change in a given year (in any direction), (2) the direction in which that country will change if it does change, and (3) the magnitude of any resulting changes. Figure 1 summarizes the patterns of diffusion that we tested for this paper.

Figure 1: Tested Diffusion Patterns

Network	Trigger	Weights	Effect
neighbors	gap	equal weights	probability of change
			direction of change
			size of change
		land area	probability of change
			direction of change
			size of change
		population	probability of change
			direction of change
			size of change
		GDP	probability of change
			direction of change
			size of change
		per cap GDP	probability of change
			direction of change
			size of change
		log(land area)	probability of change
			direction of change
			size of change
		log(GDP)	probability of change
			direction of change
			size of change
		log(per cap GDP)	probability of change
			direction of change
			size of change
neighbors	instability	equal weights	probability of change
single global	direction and size of change	equal weights	direction of change
			size of change
superpower influence	gap	equal weights	probability of change
			direction of change
			size of change

3. Advantages of Our Research Design

Our study of diffusion is different from, and arguably superior to, other studies in five respects. First, it is the only study that attempts to explain the probability of change, the direction of change, and the degree of change. Most studies examine only one process at a time; Przeworski and Limongi (1997), the sole exception, compared the probability of becoming democratic and the probability of surviving as a democracy. We do both, with the twist of using nearly continuous data rather than a democratic vs. nondemocratic dichotomy. The use of continuous data allows us to be more sensitive to probability of changes within these two broad categories and more sensitive to the magnitude of changes observed. We believe this sensitivity translates into greater confidence in our estimates (see Elkins 2000 for a compelling case for the use of more continuous variables in the study of democratization).

Second, ours is the only study of the impact of democracy *gaps* on democratization. The smaller the gap in democracy scores between two countries, the less influence one country should have on the other, and the greater the gap, the greater the expected influence. We think this is more logical than the simple assumption that the level of democracy, or simply the number of democratic neighbors, has an equal impact on democratic and nondemocratic countries alike (Przeworski et al., 1996). And we feel it is a more direct way of testing the adoption of new ideas than a measure of the degree to which static levels of democracy cluster geographically (O'Loughlin, 1998).

Third, ours is the only study that considers the possibility that countries have different

weights in diffusion. Not all countries are created equal: small, weak countries may be affected more by diffusion than are large, powerful countries, whose political evolution is more internally driven. Large, rich, and powerful countries should be expected to exert great influence over other countries, while small, poor, and weak ones should affect other countries very little and be affected by them much more. The most important and readily available potential indicators of the weight of countries in the diffusion networks are their population, gross domestic product, and land area. Land area should be a significant weight if the actors affecting political change use an implicit map of states' political boundaries to gauge the influence they should accord to pressures from other countries: the larger the state looks on a(n equal-area projection) map, the more influence actors allow it to have over their actions in the process of political change. Population should be a significant weight if these actors instead use numbers of inhabitants rather than land area to gauge appropriate influence. A significant GDP weight would suggest that source countries with large economies "buy" influence, or that target-country actors tend to respect the wishes of the source countries, anticipating economic rewards for their actions. A significant per capita GDP weight, on the other hand, would support the idea that countries tend to emulate their most economically successful neighbors.

Fourth, our study controls for domestic influences on democratization at least as well as any other study. Some domestic determinants of democratization could easily be mistaken for diffusion if they are geographically clustered. This is a real danger with respect to economic development, presidentialism, and various ethno-religious regional or other regional factors. These and other correlates must be controlled for in order to get estimates that do not exaggerate

the impact of diffusion. Here we control directly for per capita GDP and presidential institutions, and indirectly for other unmeasurable factors that are associated with certain world regions and historical periods. We also make vigorous efforts to minimize distorted estimates due to the rarity of change, the use of a democracy scale with fixed endpoints, and spatial and temporal autocorrelation.

One respect in which our study may be inferior to published analyses is the sample size. O'Loughlin et al. (1998), using the Polity III dataset, cover every country from 1820 to the present. As our dependent variables are derived from the Freedom House data, we are limited to the period 1973-1996 and are unable to distinguish trends unique to this period from more universal tendencies. However, we do include every country in the world during those years, and the more recent time period improves our ability to control for domestic determinants simply because the necessary data exist or can be estimated reasonably well. This is probably the most extensive test of diffusion hypotheses yet, but it is clearly just a first step in a very complex investigation.

B. The Analysis

1. The Estimation Method

There are at least two key difficulties in doing a global analysis of changes in democracy for a series of years. The first difficulty arises because we are analyzing changes in democracy levels, and most countries simply do not change in most years. To be more precise, even using our more sensitive dependent variable – change in Freedom House-based scores from one year to

the next – out of the 3,979 country-years in the dataset, 2,925 (73.5 percent) are cases of zero change. When OLS is applied to these data, the concentration of cases at an outcome of zero tends to flatten the slope of any independent variable, making it smaller and less likely to be statistically distinguishable from zero. Of course, it is always possible that this result is the truth: that the explanatory factors in such models really do not explain very much about change in levels of democracy, and that any factors that might do a better job of accounting for this peculiar pattern of variation remain to be discovered.

We disagree. Instead, as explained in the earlier theoretical section, we propose a different model: that there is a two-stage causal process leading to the observed changes in democracy levels. The first stage "selects" some countries that are "ready" to change, and prevents the other countries from changing. The second stage then operates only on the countries "selected" in the first stage, determining how far up or down they change. To model this process, we use a modified version of a STATA maximum likelihood estimator, which controls for the impact of the hypothesized selection variables in estimating the impact of the hypothesized democratization variables.[9] The results of this analysis are presented in two stages. The first stage shows the results of a probit analysis that calculates the probability that a country will change in a given year, based on the variables that we hypothesize are related to regime stability. The second stage (a Prais Winsten regression) includes the variables which we hypothesize influence the direction and magnitude of change in those countries that do change, as well as a variable that is derived from the probit analysis and corrects for the possible effects of selection bias.

The second difficulty is due to the peculiarities of pooled cross-sectional time series data: autocorrelations within countries, temporal correlations due to global processes that affect all the countries in the sample in a given year, and across-panel heteroscedasticity.[10] We address this problem using the STATA XTPCSE procedure for the regression estimation;[11] and using robust standard errors clustered on countries in our first stage probit analysis.[12]

2. Operationalizing the Variables of Interest

The data we use cover all the countries of the world, from 1972 (or the date they came into existence) to 1996. Our sample includes even microstates such as Andorra, Liechtenstein, and Monaco, as size is potentially an important weighting factor in diffusion. The dependent variable in the regression model, dFH (for "delta Freedom House"), reflects changes in levels of democracy within each country, calculated from an index based on each country's Freedom House annual rating for political rights and civil liberties. The index is 16 – (political rights + civil liberties), which produces a 13-point index ranging from 2 (least freedom) to 14 (greatest freedom).[13] 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 and Reinicke 1990, 61; Vanhanen 1990; Przeworski *et al.* 1996, 52). Freedom House data have been used to measure democracy in several respected studies (Helliwell 1992, Burkhart & Lewis-Beck 1994) and, together with the Polity III series, are the only annual time-series democracy indicators available, and therefore the only ones suited to a study of democratic

diffusion.[14] We therefore refer to this scale as a democracy scale. The dataset containing this Freedom House-based index for all independent countries for the period 1972-1996 allows us to examine the entire period of Huntington's Third Wave, and beyond, for all regions of the world. The dependent variable in the probit stage of the model, SELECT, is a dummy variable that equals one for years in which a country changes its Freedom House score (i.e., observations that have nonzero values on dFH) and zero for the static cases.

The key independent variables of interest, of course, are the expected diffusion effects. As noted, we focus on the neighbor networks in this first round. Some studies of diffusion have inferred diffusion indirectly from patterns in the data that are not explained by domestic factors (Starr 1991, Gonick and Rosh 1988, Bollen and Jackman 1985, Przeworski et al. 1996). We take a more direct approach. We derived a theoretical model that predicts what patterns we would *expect* to find, given certain assumptions about various diffusion patterns. This is a more cautious approach, because we give diffusion credit only for the patterns that our theory suggested, rather than for just any transnational or regional pattern in the data.

The reasoning behind the indicators is as follows. The message carrying new ideas is assumed to be the gap between freedom scores of neighbor countries. If k is more democratic than i, then the expected impact of the gap is positive (pro-democratic pressure); if k is less democratic than i, then the expected impact is negative. But because most countries have more than one neighbor, we look for the expected impact of the average neighbor. We do this by averaging the size of the gap over all of the target country's neighbors, that is, by summing up

their separate impacts, some of which could be positive and others negative, and then dividing by

the number of neighbors *(k)*. Our basic formula for the average gap is therefore

$$D_{i,t+1} = \frac{1}{k} \sum_{k=1}^{k} (F_{kt} - F_{it})$$

where $D_{i,t+1}$ is the expected impact of diffusion on target country i at time t+1, k is the number of

countries in the network, and F_{kt} and F_{it} are the democracy scores of countries k and i,

respectively, at time t. This is the correct formula if all the countries involved have equal weight.

If some neighbors have greater influence than others, then the correct formula should be a

weighted average of the gaps, or

$$D_{i,t+1} = \frac{1}{k} \sum_{k=1}^{k} \frac{w_{kt}}{w_{it}} (F_{kt} - F_{it})$$

where w_{kt} is the weight of the kth country in the network at time t and w_{it} is the weight of country

i at time t. The greater the neighbor's weight relative to the target country's weight, and the

greater that neighbor's weight relative to those of the other neighbors, the more influence it will

have. The weighted impacts should therefore help us decide which countries have the most

weight, and may suggest why.[15]

   We calculated weighted average gaps for the neighbor network, for each country-year,

using the following weights: the relative population, per capita GDP, log of per capita GDP,

overall GDP, log of overall GDP, total land area of the target country and each of its neighbors,

and log of land area. The GDP and land area diffusion effects are calculated by weighting the

difference between the democracy score of the target country and that of each of its neighbors by the ratio of the weights, and then averaging those weighted differences over the whole set of neighbors. These expected diffusion effects were calculated with a series of large spreadsheets and then copied into the master database.

One of the most difficult tasks in constructing these weighted indices is obtaining the data needed for the weights. Due to the way in which we chose to operationalize diffusion, missing data would create serious bias, because calculating the expected diffusion effect requires information on all of each country's neighbors. If one country is dropped from the sample, the average diffusion effects for all of its neighbors will be wrong. Furthermore, each country is likely to be included in several networks, and the results are often lagged, which extends the error further. Because this web of diffusion relationships may perpetuate or magnify errors, missing data is unacceptable. For this reason, we estimated, as described in the Appendix, GDP and per capita GDP for the countries and years for which comparable data were not available. The estimates are not perfect, but they seem to be close, so we believe that the drawbacks of using estimated data are less serious than the drawbacks of dropping cases (King 1999).

The population-weighted average gap is calculated using a slightly different formula. This formula uses a ratio of the populations in each pair of countries to weight the scores before calculating the difference in weighted scores and averaging these differences for the whole neighborhood. The formula is

$$D_{i,t+1} = \frac{1}{k} \sum_{k=1}^{k} \left(1 - \frac{a_{it}^2}{(a_{it} + b_{kt})^2 - b_{kt}^2}\right)(F_{kt} - F_{it})$$

where a_{it} is the population of the target country at time t and b_{kt} is the population of the kth country in the network at time t.[16] If target country a is far larger than neighbor b, then this special weight approaches zero; if the neighbor country is far larger than the target country, it approaches one.

Table 1 displays some summary statistics and bivariate correlations for these indicators. Diffusion weighted by the population formula, log(GDP), and log(land area) yields expected impacts very similar to those of unweighted diffusion and all seem promising as explanations for change. But diffusion variables weighted by per capita GDP, GDP, and land area lead to quite different expectations and seem less promising as explanations.

Table 1: Summary Statistics and Correlations for Variables of Interest										
				Correlation with variable number:						
Dependent variable:	Mean (Std.Dev.)	Min	Max	2	3	4	5	6	7	8
1. dFH	0.18 (1.91)	-9	8	.29	.28	.27	.28	.16	-.00	.01
diffusion with weights:										
2. unweighted	-0.22 (3.27)	-10	10	1.00	.99	.94	.96	.47	.16	.05
3. log(GDP)	-0.16 (3.52)	-12	12		1.00	.96	.97	.50	.22	.06
4. population	-0.12 (2.48)	-10	13			1.00	.96	.45	.21	.20
5. log(land area)	0.34 (3.87)	-14	19				1.00	.46	.26	.22
6. per capita GDP	0.78 (12.25)	-199	113					1.00	.10	.01
7. GDP	83.88 (956.71)	-3627	20469						1.00	.26
8. land area	322.35 (8800.70)	-8705	275050							1.00

dFH is the change in 16-(Freedom House's political rights + civil liberties).
"Population" is the diffusion variable weighted by the population formula described in the text.

In addition to testing the weighted neighbor effects on the magnitude and direction of change, we test for superpower influence on regime stability – the diffusion effect of the U.S.

and the Soviet Union/Russia, disaggregated by the period associated with each leader of the superpowers in order to account for different foreign policies under different leaders. In order to calculate expected diffusion effects for each leader, we first coded each country, for each year, as falling within the sphere of influence of the US, the Soviet Union (and later Russia), or neither.[17] Then we computed the "superpower gap"– the difference between the Freedom House score of the superpower and the target country – to identify countries that might be subject to pressure to conform to the superpower model. Finally, we created a variable for each leader that scored 0 if the country was not within that leader's sphere of influence, or if the leader was not in power that year, and equaled the "superpower gap" otherwise. These variables will test for the possibility that the stability of countries within a superpower's sphere of influence may have been affected by the foreign policies of the various superpower leaders. If they are significant and positive, they will indicate that, the more a country differs from its superpower, the more it is induced to change (in any direction). Including these variables in the second stage will tell us whether, if they do change, they tend to change up or down the Freedom House scale.

In the first stage selection model, we also include a variable that measures the average amount of instability in the country network. It represents the absolute value of regime change for the average country in the network in the preceding year (i.e. the sum of square roots of the squared dfh values of all countries in the network, over the total number of countries in the network). We use this value to test whether instability in a regime's neighbors might lead to a greater probability of regime change in the following year.

Our regression model also includes the average annual <u>change</u> in democracy scores worldwide (TREND). This could be taken as an indicator of diffusion effects that are not specific to any region or set of neighbors, but it more likely serves as a variable that very indirectly controls for any worldwide tendency not directly specified in the model that could cause large numbers of countries to move in the same direction at the same time. These tendencies could be related to the breakup of the Soviet Union and the end of the Cold War, worldwide economic trends, or many other forces. Obviously it would be preferable to measure each possible cause separately in order to distinguish their impacts. But until that becomes possible, controlling for the worldwide trend at least helps disentangle the specifically neighbor-generated diffusion pressure from larger processes that may be taking place.

3. The Control Variables

Both the selection model and the regression model include several control variables. These are included not simply to improve the overall fit of the model, but more importantly, to minimize the risk of exaggerating the impact of diffusion. Including these controls actually makes it harder to detect a significant impact of diffusion and therefore increases confidence in the estimate of its impact.

The Freedom House scale is capped at each end. Countries at the highest score, 14, have nowhere to go but down, while those at the lowest score, 2, have nowhere to go but up. We therefore expect that changes will be more often positive at the low end of the scale and more often negative at the high end. To control for this idiosyncrasy of the scale, we include an

adjustment factor equal to (8-FH)/6 and lag it one year. The adjustment factor for the current year is a linear function descending from +1 if a country was at score 2 in the previous year to 0 if it was at score 8, to -1 if the country was at score 14.[18] But note that the scale may be a truncated measuring device that does not reflect all existing levels of democracy. It is likely, in fact, that the Freedom House scale does not register levels of democracy higher than 14 or lower than 2 even though some countries would fall in these extended ranges if the indicator were not bounded at both ends. Consequently, the scale is unable to register movement that takes place at higher and lower levels than 14 or 2, and as a result it is less likely to register changes at the extremes than in the middle. The probit stage thus requires a different sort of adjustment: we add an inverted-U adjustment that takes on values of zero at the extremes of 2 and 14 and a maximum of .25 in the middle.[19] We allow the regression to determine how much weight to give these adjustments; if we are wrong about the nature of the scale, the adjustments will simply turn out non-significant.

Another hypothesis is that countries that have changed in the past are more likely to change in the future, for reasons that we cannot model directly, but that probably have to do with the fact that large-scale processes of regime change typically extend over several years, and may be registered as a series of consecutive changes in Freedom House scores. We control for this possibility by including the absolute value of the change in the previous year as an explanatory variable in the selection equation. We use the absolute value because the direction of the previous year's change is immaterial; we are simply modeling the probability of change in any direction.[20]

Some of the other control variables are more substantively interesting. Chief among them is per capita GDP, or wealth. There is some controversy about whether wealth helps cause countries to become more democratic (Rueschemeyer 1991, Diamond 1992), or merely helps democracies survive after the transition, or perhaps helps any kind of regime survive (Przeworski and Limongi 1997). We test both hypotheses. Following Przeworski and Limongi, we propose that wealthy countries are less likely to change their level of democracy in either direction. The selection model addresses this part of the question. Then the regression model tests the idea that wealthy countries are more likely to become more democratic, i.e., to change in a positive direction. The variable is the same estimate of per capita GDP described in the appendix.

Another debate that our control variables address concerns presidentialism. Long ago, Hermens (1941) argued that parliamentary democracies were prone to breakdown. More recently, Juan Linz and other have argued that presidential democracies are more unstable (Linz 1978, 1994). Some empirical research supports Linz's thesis (Stepan and Skach 1994), some qualifies it (Mainwaring 1993, Mainwaring and Shugart 1997), and some challenges it (Gasiorowski and Power 1998). We employ a dummy variable PREZDEM for presidential democracies (presidential systems that score at least 9 on our Freedom House scale), to test the hypothesis that presidential systems are more unstable than other democracies.[21] In order to tease out the effect of presidentialism from the effect of being in a group with high Freedom House scores, we also include a dummy DEM for countries that score at least 9. The combination of dummies allows us to compare presidential democracies to non-presidential ones, and both to non-democracies (conceptualized, for this purpose, dichotomously). In the regression stage, we

include the same variables, but this time lagged to indicate whether a country that was a presidential democracy before it changed saw an increase or decrease in its democracy score.

We include still other variables that control indirectly for interesting tendencies that we cannot measure directly. In addition to the global trend mentioned above, we include in the selection model dummy variables for selected years in which all countries were significantly more likely to change. We also include some regional dummies because there are some regions in which countries are more likely to change, or less so, or in which countries tend to move in a more positive or a more negative direction when they do change. We can speculate but do not really know why these tendencies exist: common culture, similar social structure, religion, similar institutions, and parallel economic changes are all possibilities. Comparing the significant dummies to the non-significant ones can suggest some reasons for these tendencies. But the immediate reason for including the regional dummies is, again, to improve the degree of control and to strip from the neighbor effects we are testing any variance that might simply be attributable to these regional processes. This helps us err on the side of caution, making it harder to turn up spurious associations and increasing confidence in our estimates of the impact of diffusion.

C. The Results of the Analysis

1. Why Do Countries Change or Not?

Table 2 presents the probit estimates of our selection model. It must first be admitted that the fit is not impressive. Nevertheless, to the extent that omitted factors would not be correlated

with those included, some tendencies emerge very strongly.

Table 2: Probit Estimates of the Probability
of Changing vs. Not Changing

Dependent variable change/ no change	Coef.	Robust Std. Err.	z	P>|z|
Diffusion Variables				
Neighbor gap weighted by per capita gdp	-0.003	0.002	-1.550	0.120
Sum of absolute values of change in previous year	-0.114	0.050	-2.290	0.022
Member soviet bloc	-0.090	0.094	-0.970	0.334
Member US bloc	-0.324	0.107	-3.030	0.002
Ford gap effect	0.087	0.024	3.560	0.000
Nixon gap effect	0.072	0.028	2.550	0.011
Carter gap effect	0.090	0.022	4.030	0.000
Reagan 1 gap effect	0.083	0.025	3.290	0.001
Reagan 2 gap effect	0.127	0.034	3.770	0.000
Bush gap effect	0.056	0.029	1.920	0.055
Clinton gap effect	0.100	0.032	3.100	0.002
Andropov gap effect	-0.034	0.164	-0.210	0.834
Brezhnev gap effect	-0.192	0.094	-2.030	0.042
Gorbachev gap effect	0.021	0.040	0.530	0.595
Yeltsin gap effect	-0.007	0.047	-0.150	0.878
Control variables of interest				
log(per capita GDP)	-0.098	0.031	-3.190	0.001
Presidential democracies	0.211	0.080	2.630	0.009
All democracies	-0.007	0.087	-0.080	0.933
Adjustment for prior level	2.597	0.358	7.260	0.000
Absolute value of change last year	0.055	0.027	2.020	0.044
Dummy variables				
Western Europe	-0.372	0.133	-2.800	0.005
Former Soviet Union	0.590	0.142	4.170	0.000
Pacific States	-0.530	0.143	-3.690	0.000
1976	0.307	0.127	2.410	0.016
1977	0.261	0.125	2.090	0.036
1978	0.325	0.123	2.640	0.008
1979	0.364	0.112	3.250	0.001
1980	0.185	0.117	1.580	0.114
1984	0.338	0.109	3.090	0.002
1988	0.182	0.119	1.530	0.126
1989	0.870	0.113	7.690	0.000
1990	0.484	0.113	4.290	0.000
1991	0.463	0.113	4.090	0.000
1992	0.524	0.110	4.780	0.000
1993	0.954	0.112	8.500	0.000
Constant	-0.339	0.233	-1.450	0.146

N=3842 Wald chi2(35)=494.03 Prob>chi2=0.0000 Pseudo R2=0.1337
 Log likelihood= -1931.5115 (standard errors adjusted for clustering on country)

a) The control variables:

The control variables suggest the following, roughly in order of interest:

1. There have been two extended waves of change: one from 1976 to 1980 and another from 1988 to 1993. The latter wave may be related to the collapse of the Soviet Union, as it peaks in 1989, based on the magnitude of the coefficients, but as we shall see, this effect is independent of the effect of the superpower influence variables, and is not, in principle, limited to countries associated with former Soviet Union. Moreover, it is not clear what the mechanism or mechanisms are – the end of Soviet military or economic assistance, the rise of ethnic violence, economic crisis in former communist countries, the triumph of Western political norms, or some other change. The earlier wave is less pronounced, judging from the coefficients, and harder to pin down, although potential triggers are the Portuguese Revolution, or the death of Franco, and the subsequent changes that swept Latin America and other regions.

2. Presidential democracies (i.e. presidential countries that score 9 or better) are slightly more likely to change than the average case, and significantly more likely to change than similar non-presidential democracies. Whether they are changing in a positive or negative direction, when they do change, is another question, one that will be answered by the second stage regression analysis.

3. In line with the finding of Przeworski and Limongi (1997), wealthy countries are less likely to change than poor ones. See Figure 2. This is true regardless of the degree of democracy the country practices. This result is consistent across many similar models, and the

confidence intervals suggest a reliable estimate. The effect is logarithmic, so the effects are felt mostly at lower levels of wealth, though the line continues to have a significant slope well past the mean GDP for the sample (around $4,600).

Figure 2: the effect of wealth on stability
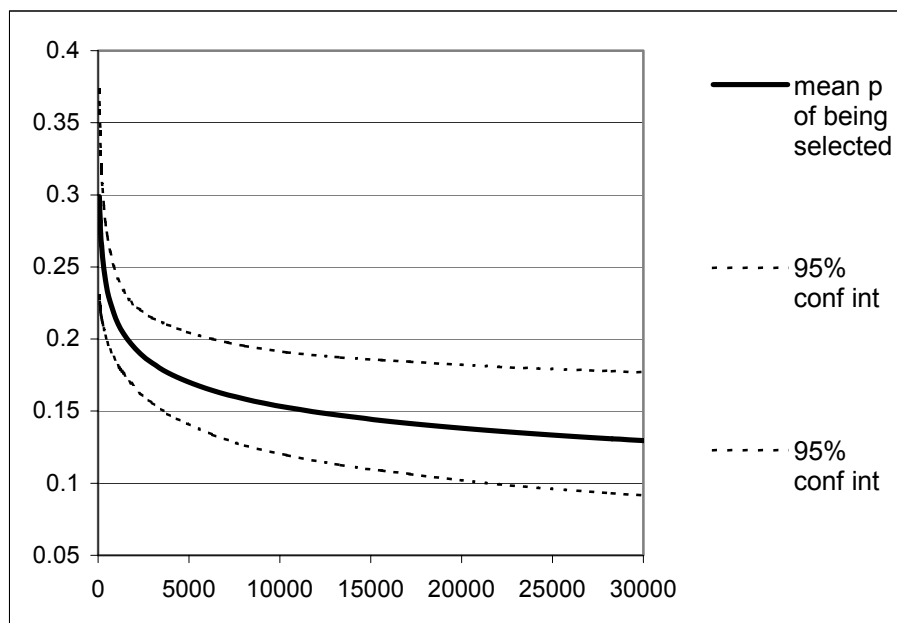
4. As expected, countries at intermediate levels on the democracy indicator are far more likely to change than those at the non-democratic or more democratic extremes. This is among the clearest findings in the model.

5. Change is extremely rare in Western Europe and the Pacific (including Australia and New Zealand) and much more common in the former Soviet Union. This is hardly surprising, but it becomes more meaningful when compared with the regions in which change is not significantly more likely or less so – North Africa, the Middle East, Central America, South America, the gulf states, South Asia, Sub-Saharan Africa, Southeast Asia, East Asia, North

America, Southern Europe, Southern Africa, Eastern Europe, and the Caribbean. (Their dummies have been trimmed from the model.)

6. Countries that changed last year are a bit more likely to change this year, although the tendency is not a strong one.

In short, the control variables in the probit stage suggest some findings that are of interest for the diffusion literature, but also some that inform broader debates in comparative politics about presidentialism, the impact of development on democratization, and even the nature of the Freedom House scale.

b) The diffusion variables:

1. We tested for the possibility that countries with more democratic neighbors are more likely to change. One might expect that they would, because if these gaps do incline them to make a bigger change when they do make a change, they might also be putting pressure on them to change. However, this estimate refutes that hypothesis. This variable does not achieve conventional levels of significance.[22]

2. The level of instability in a network in the previous year appears to have a counter-intuitive effect. The coefficient is moderately significant but negative – the opposite sign than expected. This suggests that, at least in the short-term, leaders take network instability as a signal that they should clamp down on change. Since we only test for the first year lag, it may well be that this process reverses itself after a longer period of instability, and thus our results do not necessarily disconfirm the findings reported by Starr (1991).

3. Countries that belong in the US bloc generally are much less likely to change, even

controlling for their prior level of democracy, than similar countries outside the US bloc. But

countries that are within the US bloc and score less than the US all showed significantly

higher probabilities of change than similar cases. Figure 3 illustrates this dynamic. The

value of the coefficient is highest – suggesting greater probabilities of change – for the

second Reagan administration, followed in descending order by Clinton, Carter, Reagan 1,

Ford and finally Nixon. Again, until we analyze the results of the second stage, we cannot

know if these are movements in a positive or negative direction.

Figure 3: Superpower effects on the probability of changing
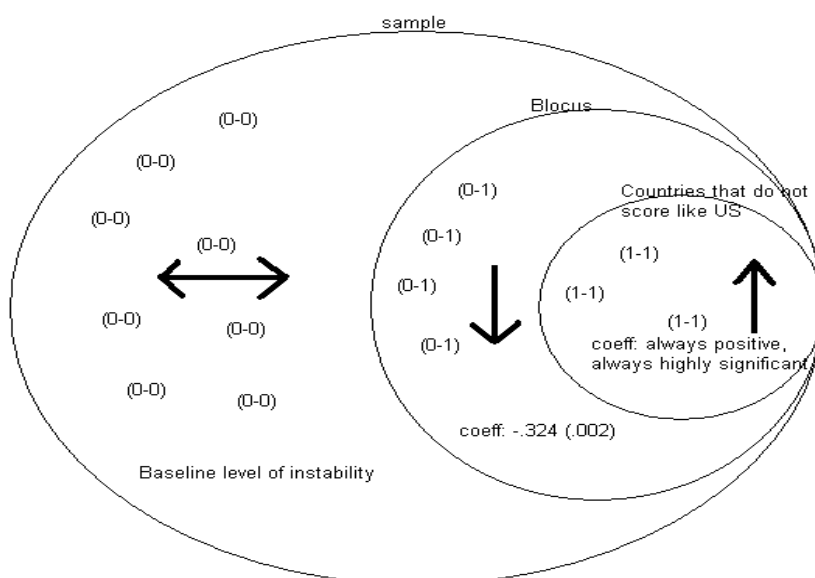
The first number in each pair indicates the score on the blocus dummy, the second the score on the superpower diffusion variable (all 1s stand for non-zero values, since the variable is a continuous one)

4. The dynamic for the Soviet bloc countries is quite different: the general soviet bloc countries, for all years, are not distinguishable from the general population, except that countries that score differently than the Soviet Union are less likely to change during the Brezhnev administration than countries with similar characteristics, while none of the other Soviet

leaders show a significant effect on regime stability. The estimate is only moderately significant, but if this is correct, then Brezhnev was the last soviet leader to keep a tight lid on the countries in the periphery of the Soviet bloc.

Therefore, our tentative conclusion is that a country's propensity to change its level of democracy in any given year is driven by a process that is mostly independent of democracy gaps among neighbors. Relationships with superpowers seem to matter, depending upon the administration, but beyond that it seems to be a domestic process dominated by the level of economic development and pre-existing levels of democracy. However, it would be wise to remember how little variance is explained by this model. There is plenty of variance left for leadership, party systems, social structure, and other factors to explain.

2. Why do Countries Become More (or Less) Democratic?

Our main goal, however, was not to explain which countries change regimes, but why they choose the resulting regime. In order to do that we must model the impact of our variables on the direction and magnitude of change – i.e., we must test whether the choice of regime, after a change, is affected by the nature of the regimes around them, superpower pressures, and other variables of interest. Table 3 reports the second-stage regression estimates of explanations for the magnitude and direction of change, given the probability of changing at all (the variable lambda is the correction factor from the first stage). The fit is not as good as that typically reported in pooled cross-sectional analyses of democracy, but this is because our dependent variable is not the level of democracy, which is very static and therefore easy to "explain" with a lagged dependent variable, but change in the cases that are changing, which is more variable and

harder to explain. Considering this difference, the fit is respectable, and the results for our

variables of interest are illuminating.

Table 3: Model of the Magnitude and Direction of Change in Freedom House Scores, with correction for selection bias

Prais-Winsten regression, heteroscedastic panels corrected standard errors	Coef.	Het-corrected Std. Err.	z	P>|z|
Diffusion Variables				
Lagged unweighted average neighbor gap	0.095	0.024	3.960	0.000
Member US bloc	0.834	0.299	2.790	0.005
Ford gap effect	-0.052	0.060	-0.870	0.387
Nixon gap effect	0.075	0.073	1.030	0.303
Carter gap effect	-0.044	0.061	-0.710	0.476
Reagan 1 gap effect	0.075	0.065	1.150	0.251
Reagan 2 gap effect	-0.049	0.074	-0.660	0.510
Bush gap effect	-0.045	0.078	-0.580	0.564
Clinton gap effect	0.051	0.086	0.590	0.554
Member soviet bloc	-0.139	0.218	-0.640	0.524
Andropov gap effect	0.325	0.420	0.770	0.440
Brezhnev gap effect	0.183	0.263	0.690	0.488
Gorbachev gap effect	0.038	0.141	0.270	0.787
Yeltsin gap effect	-0.171	0.112	-1.530	0.126
Control Variables				
Prezdlag (presidential and fhlag>=9)	-0.516	0.240	-2.150	0.032
Demlag (fhlag>=9)	0.503	0.296	1.700	0.090
Lambda (adjustment from probit stage)	1.162	0.315	3.690	0.000
Adjustment for prior FH score	1.241	0.276	4.500	0.000
Log(per capita GDP)	0.182	0.065	2.790	0.005
Global trend in FH changes	0.934	0.122	7.670	0.000
Former Soviet Union Countries	-0.520	0.253	-2.050	0.040
South American countries	-0.388	0.285	-1.360	0.172
Central American countries	-0.693	0.251	-2.760	0.006
Constant	-1.575	0.509	-3.090	0.002

rho (autocorrelation coefficient) = 0.071

R-squared = 0.2045   N = 1026
Prob > chi2 = 0.0000
Wald chi2(19) = 217.06

Group variable: code	Number of groups = 184
Panels: heteroscedastic (unbalanced)	Obs per group: min = 1 max = 16 avg = 5.58
Time variable: year	Autocorrelation: common AR(1)

a) The control variables:

The control variables yield some interesting information. The adjustment for the previous level of democracy is strongly positive, confirming our expectation that – given that they are changing at all – the least democratic countries are more likely to rise and the most democratic countries are more likely to fall. Lambda, the variable that controls for the probability of being selected into the sample based on the first stage selection model, is strongly significant, suggesting that the variables we included at the first stage do play an important and significantly different role in priming the countries for change than the variables in the second stage. This validates the choice of methods, suggesting that if we don't take the unobserved variation into account, we are likely misstating the true effect of the variables on the magnitude and direction of change in Freedom House scores (indeed, running the model without lambda has erratic effects on the variables, dropping at least one of the control variables into insignificance, and raising others into conventional levels of significance). And there appears to be a significant impact of global trends: average positive change worldwide is associated with positive changes, and negative with negative, even controlling for other suspected causes of change.

All other things being equal, the Central American and former Soviet Union countries tend to change toward less democracy, while the South American countries do not show an appreciable trend. This may be surprising, given the striking political liberalization in all of these regions during the Third Wave. However, this estimate reflects the fact that the FSU countries enter the sample only after gaining their independence from the old Soviet Union, when they had already liberalized a great deal, and then suffer some retrenchment. The Central and South American countries scores follow a similar inverted N-shaped curve, dropping low in

the early 70s, then rising, then dropping again in the early 90s (though never back to their earlier lows). The estimate therefore reflects a trend that is confirmed on closer analysis of these countries: a period of non-democracy, strong early liberalization and then a backlash toward less democracy in the years after independence – at least as reflected in Freedom House scores. Moreover, we are controlling for a number of other variables that might explain the positive changes in a slightly more precise way, such as the neighbor effects, and the impact of wealth, leaving only the negative variation to be explained by the general regional variables.

When we look at the first and second stage results together, we find confirmation for Juan Linz's thesis that presidential democracies are more prone to breakdown than parliamentary ones. As explained earlier, we included a dummy in the regression that distinguishes all countries that score 9 or better, and a second dummy that identifies those countries that score 9 or better and also have presidential regimes (the variables are lagged, as the effect of instability at t-1 is the change in Freedom House score that appears at time t). The standard for comparison are all those countries that are non-democratic. The results suggest that, when they change, democracies tend to change in a positive direction, even after controlling for previous levels of democracy (though the effect is not terribly significant). But presidential democracies are (a) more likely to change, as we saw from the probit results, and (b) when they do change, they are more likely (than other democracies) to change in a negative direction. The similar magnitude and opposing sign of the coefficients for the two dummies suggests that presidential democracies are roughly as likely to change in a negative direction as non-democratic countries.

Another interesting finding is the significantly positive impact of per capita GDP on the

direction of change. Contrary to Przeworski and Limongi (1997), we find that wealthier

countries *do* have more positive transitions. Our results in this respect are highly significant and

robust across all the specifications of the model and the various estimating techniques we have

tried. The discrepancy between our findings and theirs may be reconciled by examining a plot of

the impact.

Figure 4: the effect of wealth on democratization[23]
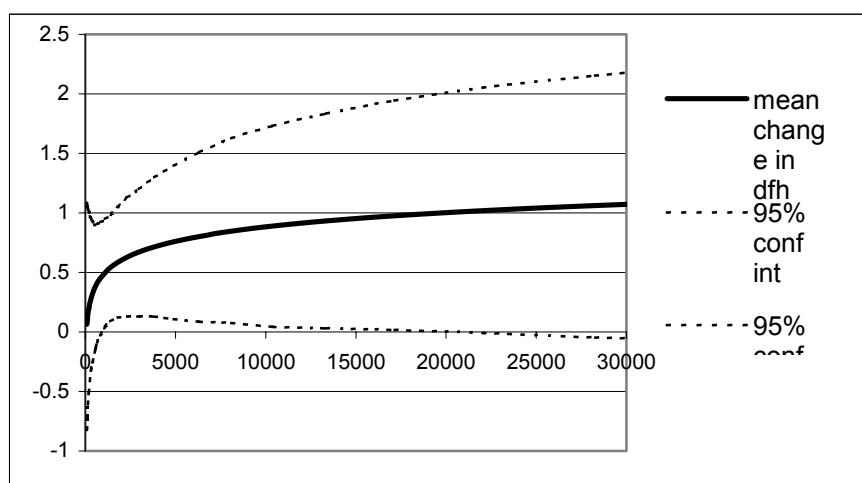
Figure 5: the effect of wealth on democratization at low levels of GDP
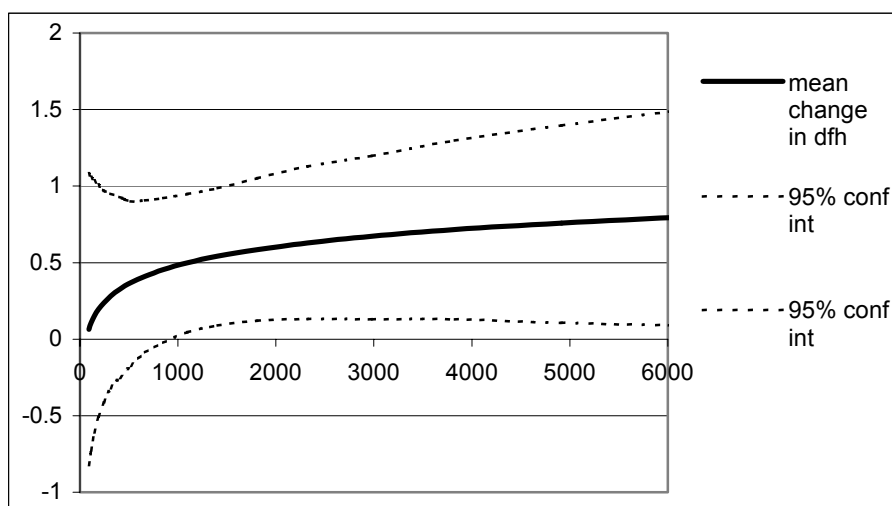
As Figures 4 and 5 show, because it is a logarithmic relationship, income must increase tenfold

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 $6,000, as seen in Figure

5 (the middle line in the graph is the trend and the outside lines reflect 95% confidence

intervals). These low incomes tend to correspond to low scores on the democracy scale. The

changes that we observe, therefore, occur at levels that would be considered sub-democratic in

the Przeworski and Limongi dichotomy, and would pass unnoticed in their analysis. At the same

time, the mean GDP value in our sample is around $4,600, and nearly 75% of all cases fall below

the mean. As a result, our estimates are relevant to the vast majority of countries that are

struggling with low levels of wealth and freedom at the same time.

Taken together with the results of the selection model, the role of economic development

in democratization is contradictory. On the one hand, wealth makes countries less likely to

change; but on the other hand, when they are moved to change for other reasons, wealth makes

changes larger and more positive. This contradictory relationship is consistent with the longstanding view that economic development promotes democracy, because it both makes countries more democratic and helps democracies survive; *and* consistent with a failure to detect a relationship between wealth and change in a research design that overlooks change at low levels.
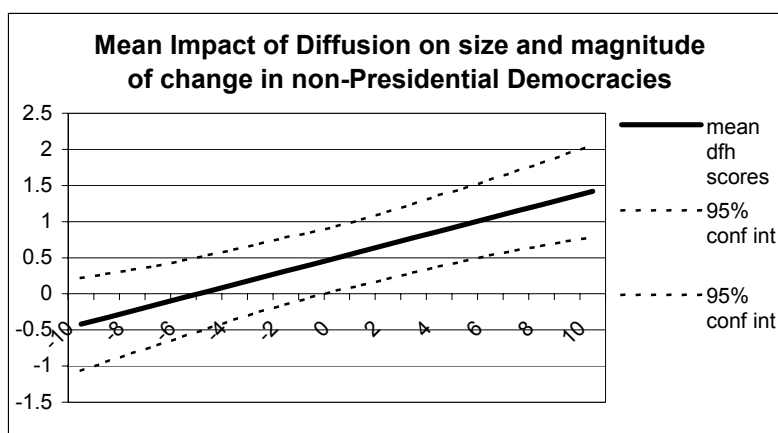
b) The diffusion variables:

The interpretation of the superpower variables in the two stages of the regression is interesting. As noted earlier, all the countries that were within the US sphere of influence, and scored less than 14 on the Freedom House scale were more likely to change their score, during all US administrations. The second stage confirms that, on the whole, this change was for the better – *i.e.*, all else being equal, falling within the US bloc made it more likely that any regime change would be in the direction of more democracy. Moreover, the gap-driven variables are non-significant, so that, *ceteris paribus*, the gap between target-country scores and the US score adds nothing to the explanation. Once we control for the effect of other variables, all the countries within the US bloc moved upward, between 1972 and 1996, at about the same rate. The Soviet bloc countries, on the other hand, do not act in a way that is distinguishable from similar cases outside the sphere of Soviet influence.

Finally, we are able to confirm the expected impact of democracy gaps among neighbors. Figure 6 displays predictions for unweighted neighbor effects. The impact is clearly positive. It is also a tendency about which one can be fairly sure, judging from the 95 percent confidence intervals. The fact that gaps still have a positive impact after extensive controls for

presidentialism, economic development, global trends, regional tendencies, starting point, and selection bias adds to our confidence in the estimate. Our estimate is that a country that changes tends to experience a change equal to approximately ten percent of the gap in democracy scores between itself and its average neighbor. Using 95 percent confidence intervals, this means that we would expect to see a change of one point in 2.4 to 8 years if the gap is (and remains) one point, 1.8 to 4.9 years if the gap is two points, 1.5 to 3.7 years if the gap is three points, or 1.2 to 3.0 years if the gap is four points.

Figure 6: the impact of neighbor gaps on democratization

What is surprising about our diffusion variable is that the unweighted pattern performs as well as any of the weighted patterns. In fact, judging from the impact on the overall model, the z values and significance levels, it performs better than any other diffusion indicator, though some of the differences among the first three variables are probably too small to be significant. Table 4 reports the differences among the differently-weighted diffusion indicators.[24] Gaps weighted by log(land area), log(GDP), and the population function are probably indistinguishable from the unweighted version, but all four are clearly superior to gaps weighted by per capita GDP or land

area or untransformed GDP. This is related to the fact that the statistically significant indicators are influenced more by the diffusion component, while the nonsignificant indicators are influenced more by the weights used to derive them. As Table 1, earlier in this paper, shows, the first four indicators have much smaller variances and are highly intercorrelated.

Table 5: Comparing the performance of differently weighted diffusion variables

	Basic Model	1	2	3	4	5	6
Weighting factor	None	log(land area)	log(GDP)	ratio of populations	per capita GDP	Land area	Over all GDP
coefficient	0.946	0.069	0.079	0.099	0.010	0.000004	-0.00005
std err.	0.024	0.018	0.021	0.027	0.005	0.000002	0.00003
z	3.960	3.850	3.750	3.640	2.020	1.860	-1.440
sig. >	0.001	0.001	0.001	0.001	0.043	0.063	
R-squared	0.2045	0.202	0.202	0.200	0.194	0.191	0.191
Waldchi2	217.06	214.68	214.62	212.65	207.96	203.61	207.04
Prob >chi2	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Within contiguous networks, then, our results contradict one of the longstanding findings of the sociological diffusion literature. The diffusion of regime types is not assisted by the stratification of the country network into more or less important figures, at least as measured by the most obvious indicators of importance for countries.

III. <u>Conclusion</u>

The picture of diffusion that emerges is fairly complex. The various US administrations seem to be associated with both instability within the US sphere of influence and with a general tendency in all westward looking countries toward greater democratization. Similarly, democracy among a country's neighbors tends to influence those countries that are experiencing a transition to choose democracy over more authoritarian regimes, and authoritarian regimes have a similar influence in the opposite direction. But neighbors tend to have relatively equal

influence over the magnitude and direction of regime change whenever a country is primed for change. Democratic diffusion among neighbors, therefore, does not appear to be a function of the imposition of ideas by stronger (neighborhood) actors on weaker ones, or even the result of emulating more economically successful neighbors.[25]

If diffusion were driven by coercion or the promise of economic success, the unlogged wealth, GDP, land area, and population weights should not only have been significant, but more significant than the indicators that are most similar to the unweighted pattern. On the contrary, the fact that the unweighted pattern yields the clearest results suggests that countries do not evaluate the appropriateness of the regime they are to choose on the basis of how well other countries with that regime perform on non-political measures. In more colloquial terms, they are not accepting the idea of a new regime based on the curriculum vitae of the proponent. This is somewhat speculative, of course, but our results, coupled with the overall positive trend, are consistent with the idea that the spread of democracy in the last 25 years is in part the result of an ideological convergence that is independent of the perceived non-political benefits of democratic rule. If this is true, countries choose democracy after seeing their neighbors model it, not because they think it leads to greater economic benefits, because they want to be more like their more powerful neighbors, or because the bully on the block is making them, but rather on its own merits.

Appendix: Estimating GDP and GDP per capita

The best available dataset with cross-nationally comparable data on GDP and GDP per capita is in the Penn World Tables 5.6 (PWT). The PWT data, however, (a) only run to 1992, (b) do not include most of the countries of the former Soviet Union and other formerly communist states with non-market economies, and (c) fail to include many developing countries. In order to complete the sample we estimated the missing GDP data wherever possible using various estimation techniques. First, of course, we included all the PWT data, using their GDPCHG variable, which transforms national figures into inflation-adjusted US dollars. Then we obtained per capita GDP-Purchasing Power Parity data from1982 through 1996 from the World Bank's World Development Indicators database, for all countries, with a significantly greater cross-national coverage. We calculated annual growth rates for each economy using the World Bank data, and where we had at least one PWT data point for a country, we applied the annual World Bank-based growth rates to that value to estimate missing years.

Where we did not have even one year of PWT data, we used data for all the years and all the countries in which the two time series overlapped to estimate a regression equation that would allow us to calculate a GDP value ("GDPCH") for every World Bank GDP-PPP value, and we filled in the blanks as much as possible with the results of the regression equation. The fit for this regression was better than .95. The result was a much more complete data set, which does not exclude the formerly communist countries and runs through 1995. Where there were gaps of less than three years in this set, we interpolated, monotonically, to fill in even further.

At this point, there still remained a number of country-years missing,[26] so we used the United Nations' Statistical Yearbook to collect annual GDP figures for most of the missing

years. We derived growth rates from this data, and again applied these growth rates to extend

forward or backward the combination of GDPCH and GDP-PPP data we had already calculated,

in order to fill in longer missing series in the data. That left a number of mostly very small

countries still without any data,[27] and for these we simply used the UN Yearbook data,

unadjusted, interpolating for missing years. The final result was a data set missing only the

Turkish side of Cyprus and North Vietnam, which is in our data set for only three years. We

acknowledge that the estimated values are likely to contain errors (even, rarely, errors as great as

one third of the equivalent GDPCH value, as we can see when we compare the results of our

calculations to actual data for the years in which PWT and World Bank data overlap). This

dataset is certainly not accurate enough to calculate within-country economic growth rates, and

we do not use it for that purpose. At the same time, we believe this is still a less troubling way to

deal with the missing cases than simply omitting them altogether (see, e.g., King 1999).

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Notes:

1. The contrary hypothesis, that economic development makes a country more likely to suffer authoritarian setbacks, now survives only as the notion of an "N-curve" (a dip in the road to democracy) (Huntington 1991; Lipset, Seong, and Torres 1993) or as a phenomenon limited to the Southern Cone of South America in the 1960s and 1970s (Przeworski et al. 1997, 41).

2. The research on diffusion began with issues other than democracy. Policy (Walker 1969), social security (Collier & Messick 1975), coups d'etat (Li & Thompson 1975), and especially war (Most & Starr 1980, Russett & Starr 1989, Rousseau *et al.* 1996) are found to spread across national borders.

3. Rustow (1970), Kasza (1996), and Sikkink (1993) state explicitly that they are generating theory rather than testing it. Dahl (1971) argued that data were insufficient to test his hypotheses. Cardoso & Faletto (1979) developed theory that was virtually immune to falsification. O'Donnell (1979) tested his theory systematically for a small set of cases in one region. Huntington (1991) uses a large number of cases, but selectively.

4. Lipset (1959) in effect controlled for world region (Europe or Latin America) without testing to see whether there was a significant difference. Coppedge (1997) reports that most apparent regional effects are really threshold effects, i.e., spurious associations created by the fact that the countries in each region tend to cluster around certain levels of development. However, this same study does find some significant regional effects, particularly for the Western countries and the Middle East and North Africa, even when controlling for threshold effects.

5. Hannan & Carroll (1981) stated that export-partner dependence would be one of their explanatory variables, but reported no results for it. Hadenius (1992, 10) found that democracy in Third World countries was positively associated with trade with the United States. Muller (1995a&b) is the only study that finds no significant association between democracy (in this case, democratic stability) and world-system position, but it does report that former British colonies survive longer as democracies.

6. Sociological studies of the diffusion of ideas have consistently supported a finding that ideas follow an S-curved rate of adoption, as a few early innovators slowly pick up on a new idea, then the mass of the population rapidly joins in, leaving the few late adopters to join in at the tail end of the curve. As we explain later on, our measure of the pressure to adopt a new idea makes it difficult to test for this peculiar pattern, as we cannot distinguish between early and late adopters. We are exploring various ways of accounting for this phenomenon, but for the present are modeling a linear relationship between the various diffusion variables we test and the dependent variable.

7. Countries on continents are counted as neighbors if they share a border. All countries must have at least one neighbor. Australia is counted as an island rather than a continent. If they are islands and close to a continent, then their neighbor is the closest neighbor on that continent and any island nations in between. Island countries that are about equally close to 2 continents (such as Indonesia) or to 2 countries on the same continent (Cuba) have as neighbors both mainland countries plus any islands in between. Island countries that are not close to any continent, such as those in the South Pacific, have as neighbors all islands within 150 percent of the nearest neighbor. If the island is large, as with Australia, the distances are measured between capitals or major cities. The following exceptions were made to these basic rules: 1. All of the Lesser Antilles are counted as neighbors of one another; 2. Sweden and Denmark are counted as neighbors; 3. Finland and Estonia are counted as neighbors.

8. Note that specifying the weight to be assigned to the influence of a given country is different from using a similar criterion to determine whether the country belongs to the same social system. Thus we may define a proximity-based system, but use economic weights to test whether how much influence each country has over the other countries in the network depends on its economic success.

9. Statistical procedures called sample selection models have been developed that 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). The best-known method is called the Heckman selection model, which combines a first-stage probit model of the probability of a case being selected into the sample with a second-stage regression model of the outcome of interest. An improved version is a maximum likelihood estimator that combines the two stages in order to model the outcome given the probability of being selected into the sample. Both procedures can be run easily in STATA. However, STATA's algorithm is inappropriate for our problem of, in effect, censoring the outcomes in the center of the distribution, as it was written for the more typical problem of censoring part of one tail of a distribution. When STATA's maximum-likelihood variant of the HECKMAN procedure is run on our data, it uses only one tail of the probit errors to correct the distribution of the regression errors, and therefore overcorrects for selection bias. Fortunately, Kajal Mukhopadhyay has derived a modified estimator that uses both tails, minus the zero cases, to correct the error distribution. This is the one on which we rely. We are extremely grateful to Kajal Mukhopadhyay of Notre Dame's Laboratory for Social Research for deriving this estimator and patiently explaining what it does and why it is necessary; and to Vince Wiggins of the STATA Corporation for explaining how his algorithm works. Lambda is given by $\lambda = [\varphi(a\text{-}\mathbf{Zg})\text{-}\varphi(\text{-}a\text{-}\mathbf{Zg})]/[1+\Phi(\text{-}a\text{-}\mathbf{Zg})\text{-}\Phi(a\text{-}\mathbf{Zg})]$, where φ is the standard normal density function, Φ is the cumulative standard normal distribution function, the $\mathbf{Zg}$ 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 $\mathbf{Zg}$. In this application, we set a to .616.

10. Heteroscedasticity in this context refers to regularities in the error terms that arise from the fact that values on particular variables for each country are likely to be consistently at different places on the spectrum. For example, even if the values of GDP vary an average of 10% annually for all countries, the errors in prediction are going to be of radically different magnitude for the very rich countries than for the very poor ones. This distortion violates one of the basic assumptions of regression analysis.

11. This procedure allows for certain choices to deal in different ways with different peculiarities of the data set in question. We use the c(a) and hetonly options. The first of these tells STATA what kind of autocorrelation, if any, to assume. The STATA autocorrelation default for this procedure is to assume that the within-panel observations are not autocorrelated, but at first blush this seems wrong for our data. The c(p) option tells STATA that the observations within each country are first-order autocorrelated and that each country has its own autocorrelation coefficient. This seems reasonable for our data, but the problem is that many of the panels do not have sufficient observations or have too many interruptions in the time series to calculate rho, the panel-specific autocorrelation coefficient (remember that we are using only those cases that have an observation for lambda and dfh, i.e. only those cases that experienced regime change). If it cannot calculate rho, STATA assumes 0 autocorrelation. As a result, with c(p), STATA is calculating the panel level autocorrelation coefficient mostly with insufficient data, and assuming it to be 0 for a significant portion of our panels. The solution is to use c(a), which assumes a similar pattern of autocorrelation that obtains for all panels, and calculate one global coefficient, which is then used to correct the estimates. In the end, the results are robust regardless of which assumption we make, so we can be confident that the results are not due to errors in estimating within-panel autocorrelation.

In addition, we used the hetonly option. Hetonly assumes that the disturbances are heteroscedastic across panels (i.e. each panel has its own variance), and that there is no assumed contemporaneous correlation across panels. The latter might seem problematic since we acknowledge the likelihood of global trends which affect all countries, but the panels, especially once purged of the 0 dfh observations, do not share a time period, which is required in order to calculate the covariance across all panels for each year. As a result, STATA cannot run the procedure if we ask it to control for global trends on its own. In addition, we include TREND, a variable that denotes global effects which we wish to test. Including this variable accounts for our theorized period effect, and thus should perform a similar function as the automatic control for temporal autocorrelation.

12. Following Beck Katz & Tucker (1998), we included time dummies in our probit model to see if our results were an artifact of the pooled time series structure of the data. Our results were not affected, so we dropped the dummies, as they suggest.

13. Ross Burkhart and Michael Lewis-Beck shared with us their dataset containing their periphery/ semiperiphery coding for 131 countries from 1972 to 1989, and other variables. We also gratefully acknowledge Freedom House's assistance in sharing its data on all countries for the years 1972 to 1997.

14. The Polity II and Polity III data have the tremendous advantage of extending back to the early 19th century, and have been widely used. One recent critique, however, indicates that only one component of this indicator, most likely "decisional constraints on the executive," validly measures democracy; its other components may simply add noise (Gleditsch and Ward 1997).

15. In averaging Freedom House scores, we are, in effect, treating Freedom House scores as if they were ratio level data, though they are more likely only ordinal level, and at best perform as interval level data. In doing so we run the risk of introducing noise into our calculations, since, if the intervals between each score are radically different, and the scores are not measured from true zero, averaging different groups of numbers may produce similar values for what are in reality very different overall levels of democracy. While this is, of course, less than ideal, in reality it does not greatly affect confidence in our estimates. The more the Freedom House scores differ from an interval level scale, the more noise we would find in our estimate of the diffusion effect, and thus the less significance. Therefore, our estimates of the effect of diffusion are, if anything, conservative.

16. The formula for population weighted effects produces a curvilinear weighting variable: we began with the intuition that the effect of population differentials cannot increase indefinitely at the same rate, but must smooth out at some level, and sought to theorize the sort of curvilinear relationship there might be between population levels and the strength of diffusion. Ultimately, we reasoned, if a country is completely autarchic its citizens are influenced only by one another, while if diffusion is complete, a country's citizens are influenced as much by the residents of the neighboring country as by one another. To test for the importance of population-weighted diffusion from a neighboring state, then, we first assume that every citizen in both countries can potentially have *some* influence over every other citizen, as if no border existed. (The coefficient for the diffusion effect will estimate the actual extent of that influence.) We then calculate the number of possible pairs of citizens in the combined populations of the two countries. We theorize that the ratio of possible *foreign* population-related influence over the total population-related influence in the dyad of countries is equal to the ratio of the total possible *cross-border* pairs that could exist between the population of the target country and the neighbor, over the total possible number of pairs in the combined population (minus the pairs that do not include any citizens of the target country). Holding the value of the target state's population constant and plotting the value of this ratio as the population of the neighbor grows gives a curve similar in shape but more gradual than what the ratio of the base 10 logs of the two populations would give over the same population range. This population weighting formula worked better than the ratio of populations or of logged populations, which we then abandoned.

17. Our coding was intended to identify countries that would be "aligned" with either the US or the Soviet Union, in the sense that they might look to one or the other as a political and economic model – without letting measures of the dependent variable creep into the coding. As a result we tried, whenever possible to use objective information, gathered from a standard Encyclopedia, to identify ties between the target country and one of the superpowers, and consulted with area experts in doubtful cases.
A country is deemed aligned with the US if it meets one or more of the following conditions:
- NATO member,
- OAS member, except if it is under a socialist government that is clearly aligned with the soviet union (Cuba to present and Nicaragua '79-'90),
- Other pro-Western country: if a country has looked for membership in NATO or engaged in some similar objective sign of looking to the West for its political and economic model (as judged by area experts), especially but not necessarily after the demise of the Soviet Union.
A country is deemed aligned with the Soviet Union (and later, Russia) if it meets any of the following conditions:
- Warsaw Pact member,

- Other pro-Soviet country: if a country is ruled by a communist or socialist regime that has close ties with the Soviet Union/Russia, and/or derives significant military or economic support therefrom.

18. In an earlier version of this paper, for which we used different estimating techniques, we found that changes were better predicted at the high end than at the low, and therefore weighted the regression by this adjustment +2 (to make all the weights positive). We had to abandon this weighting value in the current analysis, in order to use an estimating technique that is more appropriate to the pooled cross-sectional time series nature of the data, and still control for serial autocorrelation. Turning off the autocorrelation control allows the use of the weighting variable, and the results do not differ with or without the weighting device. We are satisfied, therefore, that we have adequately accounted for the structure of the data.

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

20. We set this variable to zero for the first year in the series, 1973. The estimate is virtually identical if it is reset to the global average, .265, for that year.

21. A total of 87 countries are coded as presidential systems, including Albania, Argentina, Armenia, Belarus, Benin, Bolivia, Brazil, Bulgaria, Burkina Faso, Central African Republic, Cape Verde, Chile, Colombia, Comoros, Congo, Costa Rica, Croatia, Dominican Republic, Ecuador, Egypt, El Salvador, Estonia, Georgia, Guatemala, Guinea-Bissau, Guyana, Honduras, Hungary, Kenya, North and South Korea, Kyrgyzstan, Lesotho, Liberia, Macedonia, Madagascar, Malawi, Maldives, Mali, Marshall Islands, Mauritania, Mexico, Micronesia, Moldova, Mozambique, Namibia, Nicaragua, Niger, Nigeria, Panama, Paraguay, Peru, Philippines, Russia, São Tome and Principe, Senegal, Seychelles, Sierra Leone, Slovakia, Slovenia, Somalia, Taiwan, Tajikistan, Togo, Tunisia, Turkmenistan, Uganda, Ukraine, United States, Uruguay, Uzbekistan, Venezuela, the former Yugoslavia, and Zaire. This list includes some systems that have mixed presidential-parliamentary constitutions but have strong presidents in practice.

22. The per capita GDP weight is reported here because it is the diffusion pattern that comes closest to conventional significance in similar probit models.

23. Figure 1 is derived from the probit estimates. Figures 2 and 3 derive from the second stage estimates, and the graphing program has not yet been adapted to STATA's XTPCSE procedure. As a result, we used the predictions of an ordinary regression analysis on the same model, which, at the scale of these graphs, is indistinguishable.

24. The full results for each model are available upon request, but they were too cumbersome to include in one table, so for clarity of presentation, results for variables that we are not directly comparing are not reported. The results for the other variables are not, in any event, remarkably different from those produced by the basic model which is reported earlier in this paper.

25. Note that, except for the superpower variables, we have not tested for the effect of ties to or dependency upon international financial institutions or other powerful actors, so we simply cannot say whether this other sort of global bullying is taking place. Our results simply suggest that, among immediate neighbors, size and strength don't matter.

26. The countries still missing some data at this point (usually only a few years but in some cases the entire series) are Afghanistan, Albania, Andorra, Antigua & Barbuda, Bahamas, Bhutan, Bosnia & Herzegovina, Brunei, Bulgaria, Burma (Myanmar), Cambodia, Croatia, Cuba, Cyprus (Turkish side), Djibouti, Dominica, Eritrea, Equatorial Guinea, Grenada, Iraq, Kiribati, N.Korea, Kuwait, Laos, Lebanon, Libya, Liechtenstein, Macedonia, Marshall Islands, Micronesia, Monaco, Nauru, Palau, Qatar, San Marino, Sao Tome & Principe, Seychelles, Somalia, Sudan, Suriname, Tanzania, Tonga, Tuvalu, United Arab Emirates, N & S Vietnam (before consolidation), Vietnam (after consolidation), N & S Yemen (before consolidation), and Yugoslavia (after the breakup in 1991).

27. The remaining countries are Afghanistan, Albania, Andorra, Brunei, Bosnia-Herzegovina, Cambodia, Croatia, Cuba, Kiribati, N. Korea, Libya, Liechtenstein, Macedonia, Marshall Islands, Micronesia, Monaco, Nauru, San Marino, Sao Tomé, Tonga, Tuvalu, North Vietam, and North and South Yemen.