Openness, Closeness, and Regime Quality: Evidence from the Second and Third Waves

Kishore Gawande*
Texas A&M University, College Station, TX

Alejandro Islas-Camargo
ITAM, Mexico City, Mexico

Sukrit Narula
Lutheran HS, Orange County CA

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Abstract

Has globalization, by integrating world economies, been a force for better political governance? This papers sheds new light on this important question. For our dependent variable we use newly constructed data by Pemstein et al. (2008) that unify political regime ratings collected by a number of raters, including Polity, Freedom House and Przeworski et al. (2000). The unified scores are robust to error in the measurement of democracy and autocracy, both of which are theoretically well-defined concepts but hard to measure precisely. Methodologically, we provide perhaps the most thorough analysis of the implication of globalization for regime quality. First, our analysis exploits only within-country variation in the data, in contrast to the existing literature which relies largely on cross-country variation to make inferences about the determinants of regimes Second we make causal inference about the impact of globalization on regimes by accounting for the endogeneity in our globalization measure using a sturdy and robust instrument. Third, we find strong support for the spatial association between a country's regime score and those of its neighbors, a new finding in the literature. Our surprising answer to the question posed above is, unfortunately, not. The accumulating evidence that globalization has increased inequality within countries makes our findings consistent with the theory advanced in Acemoglu and Robinson (2005) and Boix (2003) .

Keywords: Regime quality; Openness; Spatial; Fixed Effects; Endogeneity.

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*Corresponding Author. Bush School of Government Texas A & M University, College Station, TX 77843-4220. Email: kgawande@tamu.edu.

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1. Introduction

Two simultaneously evolving developments distinguish the post-WWII era from previous eras. The first is the widespread integration of the world economy, in the 1950s as a liberalized trading order took shape and institutionally expanded beyond the developed world, and then in the 1990s as openness was forced upon countries as technology decimated trade costs. The second development are the era's innumerable regime change experiences. The period 1950-2000 is marked by great regime volatility, moving countries both towards and away from democracy. These moves were first documented in Huntington (1991) and separated into two distinct experiences – the Second Wave of democratizations and reversals, between 1950 and 1973, and the Third Wave of democratizations, which started in 1974, and is now coming to a close. The great variety in the experiences of countries as they dealt simultaneously with openness and regime volatility provides the political laboratory for our analysis.

Because they have the institutional capability, consolidated democracies like those in Western Europe and the United States have dealt with shocks that openness dealt to them – employment loss, the collapse of manufacturing sectors, sudden changes in the composition of their GDPs, rising inequality – and used the greater global openness to their long-run advantage. But what of new democracies with nascent distributive institutions? Were they able to withstand the social impact of such shocks, and did the quality of their political regimes worsen and shift away from democracy? Were there differences in the second and third wave experiences? Did reversals become more common or less? It is these important questions of our time that motivate this paper.

The primary objective of the paper is to examine whether trade openness is a force behind changes in the quality of the political regimes. Empirically establishing that openness *causes* regimes is no easy task. One may correctly imagine that totalitarian regimes forbid interaction of their citizens with foreigners because such contact, via a variety of mechanisms, erodes the legitimacy of the regime paving the way for effective revolt. That is, the regime determines the extent of openness, not the other way around. That regime type impact international trade has been established, among others, in Mansfield, Milner and Rosendroff (2000, 2002), Milner and Kubota (2005) and Dutt and Mitra (2002, 2005).

We believe that causality runs strongly from globalization to regime quality for three reasons. First, openness has increased inequality. In their excellent survey of the voluminous literature on globalization and wages, Goldberg and Pavcnik (2007) reach that conclusion.[1] This evidence is surprising to believers in traditional trade theories – after all, the Stolper- Samuelson theorem predicts a reduction in inequality as exports of labor-intensive goods benefit labor in labor abundant countries.[2] One of the motivations for new trade theories – based on the firm, or on the global fragmentation of production chains – is to show that technological and trade-cost saving innovations that have dramatically integrated the world have also fundamentally changed the nature of trade in a way that makes traditional trade theory less relevant. The finding that trade has increased inequality around the world is more in line with the new theories than traditional ones. On the political regime side, Acemoglu and Robinson (2005) and Boix (2003) describe mechanisms showing that inequality, when exacerbated, is likely to move a regime away from democracy. With greater inequality, the provision of public goods imposes greater taxes on the rich and elite, which they will use their power – or increase their hold over power – to resist.[3]. We will argue that our findings are consistent with the fact that trade has exacerbated inequality and, via the Acemoglu-Robinson-Boix mechanism, lowered regime quality in democracies and even reversed the move towards democracy.

Second, formal evidence has accumulated showing that political leaders of countries manage openness to suit them. The "resource curse" literature (see e.g. Ross 1999; Robinson, Torvik and Verdier 2006) indicates how commodity booms induced by globalization has helped keep less democratic political leaders of natural-resource-rich states in power. The better terms of trade on offer from the world has incentivized these leaders to nationalize natural resources, and then use their encompassing power to redistribute enormous wealth to themselves. They can then provide just enough public goods to their population to prevent revolt. Thus, the "resource curse" remains long-lived *because* globalization has increased the vast appetite of industrializing countries for natural resources. Third, sometimes there is no choice but to accept openness. Disruptive computing and telecommunications technology over the last thirty years have forced countries to accept openness even before their institutions were prepared to harness the advantages of doing so. We will argue below that not having the institutions to deal with these shocks then lowered the quality of their democracies. Regardless of why countries became open – and we are confident that choice was sometimes endogenous to their regimes – we believe the force exerted by openness on regime quality was strong.

A second objective of the study is to pay greater attention to a phenomenon observed by Hunting-

[1]It is their broad conclusion from the existing literature, but as they emphasize, the existing literature is itself limited by the unavailability of more micro-level data so necessary to reach a firm conclusion on this issue.

[2]This is true in the textbook 2-good 2-factor model, but not necessarily so with more factors.

[3]The Acemoglu-Robinson-Boix mechanism is described in detail in Section 3 below

ton, but never seriously considered in empirical work on the determinants of regimes. Specifically, the waves of democratization that Huntington studied had strong spatial relationships. The ebbs and flows of democracy seemed to be geographically similar. Many Latin American countries, for example, experienced a collective movement to democracy in the Second Wave, and Eastern European countries in the Third Wave. Reversals from democracy also appeared to be geographically clustered. Is there is a spatial component to changes in regime? We not only present strong evidence of the spatial component, but find it to be a robust feature of both waves. Our findings suggest that ignoring this spatial aspect, as previous studies have done, introduces a serious missing variables problem and gravely afflicts inference about regime quality or regime change.

Our results about openness and democracy run counter to almost all that has been published on this subject. The literature's sanguine conclusion that globalization is good for democracy is unconvincing. A major reason is that the empirical investigations on which this conclusion is based suffer from a serious missing variables problem – they do not control for country-fixed effects. Since their results are driven mainly by cross-sectional variation in the regime quality and openness data, those inferences are afflicted by a severe country-heterogeneity problem. Equally seriously, the strategy used in the literature to identify the effect of trade on regime quality is quite incomplete, since it is not clear that when instruments are used they are weakly exogenous.

Methodologically, we use perhaps the most thorough analysis to data on determinants of regimes. First, we use a dynamic panel model that exploits only within-country variation in the data, in contrast to the existing literature. Second we establish that trade openness causes regime quality by using a sturdy and robust instrument to aid our identification. Third, we solve a spatial correlation problem, and in the process expose the literature to another missing variables problem that has remained underappreciated.

The paper proceeds as follows. In Section 2 we discuss the theoretical and empirical literature relevant to our study. In particular we lay out the link from trade openness to the quality of political regimes. Section 3 describes our dependent variable in detail, and also our regressors. We use four democracy measures, one a new encompassing measure made up from ten native measures – unified democracy scores – and three popularly used measures but now standardized so the results are comparable across them. Section 4 lays out the full econometric model, and the instrument we use to aid identification. Section 5 is the main contribution of this paper. It discusses the results from a variety of models and data. Unified democracy scores as well as scores of individual raters like Freedom House and Polity are used; 'globalization' is alternately measured as trade openness, as trade surges, and as liberal trading regimes; openness is interacted with education and other variables to check for nonlinearities; two types of econometric methods are used for each model we estimate. In short, we are confident of producing a most robust picture of this relationship. Section 6 provides concluding observations.

2. Theory and Empirical Literature

Theory

Our primary view of the source of regime change is through the theoretical lens of Acemoglu and Robinson (2004) and Boix (2003). In their theories, inequality plays a fundamental and critical role. Their logic of the emergence of democracy proceeds as follows. In democracies, the poor are able to impose higher taxes on the rich, and therefore redistribute to themselves a higher share of GDP than they can in non-democracies. The poor are therefore naturally pro-democracy, while the rich and elite have strong incentives to oppose it. While the poor in non-democracies are excluded from political power, they pose a revolutionary threat. To prevent this threat from becoming reality, the elite redistribute just sufficiently to prevent revolution. But such prevention is temporary, since redistribution is not guaranteed into the future. To prevent the threat from materializing more permanently, the elite must make a credible current commitment to sufficient future redistributions. This is accomplished via a regime change in which, for example, voting rights are granted. The birth of democracy does not make the regime change permanent, since the elite have power and also the incentive to take power by force, for example by coup d'etat. The poor understand that the way to lower the incentives for the elite to revert to autocracy is to agree to a low level of future taxation. However, this commitment is not necessarily credible about future levels of taxation, and when taxes become high coups become likely. This is where inequality makes its presence felt. Theoretical models – either where government maximizes welfare (Sandmo 1976) or where the median voter is the key actor (Meltzer and Richard 1981) – predict that optimal taxes are higher in more unequal societies. Therefore unequal societies fluctuate in and out of democracy.

If greater integration through trade makes a country more equal, then greater openness can only help its regime move towards greater democracy. However, if globalization increases inequality within the country, then openness erodes its regime quality. What makes recent globalizations different from their pre-WWII counterparts is that they are the result of rapid declines in trade and transport costs, combines with great technological advances. The trade-in-tasks model of Grossman and Rossi-Hansberg (2008) offers an insight into how trade expansion due to lowering of trade costs can exacerbate inequality by reducing the wages of low-skilled workers. The United States is itself an example of the tensions accompanying such trade. The lowering of transport costs has opened up the fragmentation of the production chain, with tasks previously performed inside a country, now sourced out to low-wage countries. The lowering of prices of those tasks immediately have an adverse terms of trade effect on the less skilled workers. Further, the outsourcing of tasks suited to unskilled workers lowers their wages. Grossman and Rossi-Hansberg (2006) show that the real wages of low-skilled blue-collar workers in the United States have stayed much below what they could have earned in the absence of these effects. The silver lining is provided by the upside in wages due to a technological effect from outsourcing, which increases productivity generally. The same mechanism works in poorer countries in reverse. They receive the tasks that are outsourced by firms in rich countries, but these tasks reward those who are in the upper part of the skill distribution in those countries (English speaking, averagely well educated), exacerbating inequality by producing a new elite class, which does not include the median voter. The point is that trade openness has unleashed unexpected sources of shocks to the economy, the primary one being inequality.

Literature

An emergent literature has attempted to formally understand the impact of factors such as inequality (Boix 2003), education (Castill-Climent 2008; Bobba and Coviello 2007; Glaeser, Ponzetto and Shleifer 2005; and Acemoglu, Johnson, Robinson and Yared 2005), income (Lipset 1959; Acemoglu, Johnson, Robinson and Yared 2008) and financial liberalization (Li and Reuveny 2003; Quinn 1998) on the stability and consolidation of democracy. Of direct relevance to our paper are the examination of the impact of trade openness on democracy in Eichengreen and Leblang (2008), Giavazzi and Tabellini (2005), López-Córdova and Meissner (2008), Papaioannou and Siourounis (2008), Rigobon and Rodrik (2005), and Rudra (2006). The main issue around which we discuss these contributions concerns missing variables and identification.

Missing variables that might control for the enormous country-heterogeneity endemic to any cross-country study the main motivation for using country fixed effects in a panel. For example, Acemoglu et al (2005, 2008) worry that ignoring fixed effects produces spurious results, and the robustness of their *non*-result about the effect of education on democracy or of income on democracy is in large part attributable to the inclusion of country-fixed effects. Random effects are used, for example, in Eichengreen and Leblang (2008), but we suspect their results about the impact of globalization on democracy would disappear once fixed effects are included. Nowhere is this more clearly demonstrated than in López-Córdova and Meissner (2007), who, find a significant positive association between openness and democracy from cross-sections. But when they account for country fixed-effects, they get non-results (and, to their credit, report it). In general, most empirical explorations of determinants of political regimes in the political science literature are devoid of fixed effects, impairing their credibility. Not including fixed effects opens the door to theoretically appealing variables that are time-invariant, but the country-heterogeneity problem severely limits those inferences.

Since regime quality and trade openness are jointly determined, identifying the impact of openness on regime quality is critical. While we use instrumental variables, others have used different strategies – Giavazzi and Tabellini (2005) compare a focus group that experienced transitions to openness with a control group that did not, while Rigobon and Rodrik (2005) use heteroskedasticity to aid their identification. Our solution to the endogeneity problem is described in Section 4.

3: Data

Dependent Variables

The second wave of democratizations began during he second world war, and the reversal of that wave came about in the late 1950s (Huntington 1991). The third wave started in the mid 1970s, and experienced reversals in the 1990s. Our data set begins in 1950 and end in 2000, capturing the second and third wave experiences. To be sure, the third wave continues beyond 2000, and many nations remain at indecisive political junctures today as their counterparts did in the late 1960s.

Our dependent variables are democracy scores. Th chief source for the democracy scores we use in the paper are from the Unified Democracy Scores (UDS) project undertaken by Pemstein, Meserve and Melton's (2008, henceforth PMM). PMM construct one set of standardized democracy scores from ten oft-used scores of regime-types. The ten measures of democracy include the popularly used Marshall, Jaggers and Gurrs (2006) Polity scores, the Freedom House Index (2007), and the binary democracy-authoritarian measure due to Przeworski, Alvarez, Cheibub and Limongi (2000) (PACL). They also include Arat (1991), Bowman, Lehoucq and Mahoney (2005) (BLM), Bollen (2001), Hadenius (1992), Coppedge and Reinickes (1991) Polyarchy scale, Gasiorowskis (1996) Political Regime Change measure (PRC), and Vanhanen (2003). No two of these measures are alike, and diverge significantly for some country-years. They also agree a lot, especially when a regime is strongly authoritarian.

The measurement of democracy is subjective in two ways. First, the choice of which dimension(s) of democracy to emphasize is rater-specific. The measurement of democracy on each of those dimensions is itself based on an ordinal scale that categorizes a country in a given year (a country-year) into a finite set of democracy measures. The number of ordinal categories varies from score to score. Freedom House, for example has three categories that are used to make a "Political Rights" index and four others to make a "Civil Liberties" index.[4] The focus on individual rights is a distinctive feature of the Freedom House scores. Polity scores are based on the idea that polities have both democratic and autocratic features, and quantifies both on a ten point scale. The difference between the two is the basis for the Polity scores.[5] The PACL measure is a binary categorization distinguishing democracies from non-democracies. The separation is based on the concept of contested democracy, and hinges on the assessment on a case-by-case basis, of whether the political leader in the country-year risked being overthrown in an election (democracy) or did not (non-democracy). Due to their differing emphasis, none of these measures is perfect, and sometimes differ from each other. For example, the Committee on Evaluation of USAID Democracy Assistance Programs (2008, p. 80) calculated that the correlation between the Freedom House and Polity scores for autocratic countries over the 1972 - 2002 period was 0.274. A safe conclusion is that the democracy scores are each prone to measurement error.

A simple measurement error model serves to highlight the advantage of combining the democracy measures. The unit of observation i in the model is a country-year. Suppose the true (latent) democracy score for country-year i is denoted z_i. Then rater j's measure, or rating, t_{ij} of z_i is modeled by PMM as

[4] The Political Rights categorization is based on: A. Electoral Process (e.g. freeness and fairness of elections), B. Political Pluralism and Participation (right to organize, existence of opposition), and C. Functioning of Government (elected representatives making policy, corruption, accountability of government). The Civil Liberties categorization is based on D. Freedom of Expression and Belief (e.g. free media, free speech), E. Associational and Organizational Rights (freedom of assembly and organization), F. Rule of Law (independent judiciary, police accountability), G. Personal Autonomy and Individual Rights (Personal autonomy, Freedom of travel, equality of opportunity). Each of those has further sub-questions that are rated on a 0-4 point scale. These are added together, and the totals used to partition country-years into seven categories of the extent of political rights and seven categories of the extent of civil liberties present in that country-year.

[5] The dimensions used to assess the extent of democracy and autocracy are: A. Institutionalized procedures for transferring executive power (e.g. forceful seizures of power, hereditary succession or in competitive elections), B. Competitiveness (Selection, hereditary succession, election), C. Open Recruitment (chief executive determined by hereditary succession, or competitive election or intermediate), D. Constraints on executive (unlimited authority, moderate, substantial), E. Participation (political participation is fluid, sectarian, Stable and enduring political groups regularly compete), and F. Competitiveness of participation (repressed, suppressed, factional, competitive).

$$t_{ij} = z_i + e_{ij},\ \ e_{ij} \sim N(0, \sigma_j^2), \tag{1}$$

where the measurement error e_{ij} for rater j is independent across observations i, and has a mean zero. The measurement error variance σ_j^2 is rater-specific and constant for each rater. The parsimonious assumption of constant variance is justified since the source of the efficiency gains arise from averaging across the ten measures. The composite measure has variance that is lower than any individual measure.

PMM's main objective of constructing a composite (continuous) democracy measure from the ten (discrete) scores is reliably accomplished using a Bayesian method. Here we intuitively explain PMM's method, leaving the details in the appendix. In the Bayesian view, data are not available in repeated samples. The analysis is conditioned on the available data sample and produces exact confidence intervals. The parameters are viewed as random variables, and their 'posterior' distributions are the object of interest.[6] If the posterior distribution is analytically tractable, sufficient statistics may be computed to estimates the expected value of the parameter or functions of the parameter. Usually, the posterior distribution is not analytically tractable, sampling methods may be used to reliably estimate parameters of interest.

PMM provide this simple example. Suppose the rater perceptions t_{ij} were available to us and we knew the error variances σ_j^2 for every rater. Bayesian analysis proceeds by declaring a prior distribution for z_i. Ignorance about z_i is adequately modeled with a normal distribution for z_i with mean 0 and a very large variance σ_0^2. The large variance indicates that the prior contains little, if any, information about z_i. The posterior distribution for z_i in this simple case is analytically tractable. It is a normal distribution with mean

[6] Obtaining the posterior distribution in a Bayesian econometric framework is conceptually simple. Consider the simple univariate regression model with parameters regression coefficient β and (homoskedastic) error variance σ^2. The Bayesian analysts views the parameters as random. This is the most controversial aspect and separates frequentists from Bayesians. Bayesians are simply presuming that unknowns (like $\{\beta, \sigma^2\}$) are best treated as random variables. Once that is accepted, all the Bayesian logic and computations that follow are incontrovertible. A 'prior' (marginal) distribution for $\{\beta, \sigma^2\}$ is first specified. For example, a normal distribution for β may be multiplied with a gamma distribution for σ^2 (a 'normal-gamma' prior) to yield a prior distribution $f_0(\beta, \sigma^2)$, where the parameters of the prior distribution may be set to values that reflect prior information or prior ignorance. The product of the prior with the data likelihood $L(\mathbf{y}|\beta, \sigma^2)$ yields the joint distribution of the data $\mathbf{y}$ and parameters. Applying Bayes theorem yields the posterior distribution of the parameters conditional on the data $f(\beta, \sigma^2|\mathbf{y})$. If the prior contains little or no information, the posterior is determined by the data, not the prior. The posterior distribution is the main object of Bayesian analysis.

$$\hat{z}_i = \frac{\sum_{j=1}^{10} \frac{t_{ij}}{\sigma_j^2}}{\frac{1}{\sigma_0^2} + \sum_{j=1}^{10} \frac{1}{\sigma_j^2}}, \tag{2}$$

and variance

$$\hat{\sigma^2} = \frac{1}{\frac{1}{\sigma_0^2} + \sum_{j=1}^{10} \frac{1}{\sigma_j^2}}. \tag{3}$$

An important Bayesian message is that the end result is a distribution. The prior distribution of z_i (reflecting ignorance) is now updated to the posterior distribution of z_i given the data. The updated distribution is more informative about z_i, as summarized in (2) and (3). Since the prior precision (inverse of variance) $1/\sigma_0^2$ is very small, the posterior mean $\hat{z}_i$ is determined largely by the data. Specifically, it is a weighted average of the t_{ij}'s with rater j's precision as weight. The posterior variance $\hat{\sigma^2}$ decreases as the number of raters increase, making it clear that the more scores that are available, the smaller the variance of a score that combines them.[7]

The full problem is more complex. The error variances σ_j^2 are not known. Nor are the rater perceptions t_{ij} measured precisely. The native democracy scores available from each rater place each country in an ordinal category, and they must be standardized in some way to make them comparable across raters, like the t_{ij}'s in (1). The 'multiple rater' methodology for ordinal ratings proposed by Johnson (1996) is used by PMM to solve these problems and (i) estimate unified democracy scores z_i and (ii) estimate the t_{ij}'s that ensures comparability for each observation i across the j raters. The technical details are presented in the appendix. What is important is to understand that Johnson's method used by PMM allows the analysis of the posterior distribution of a large number of parameters:

$$\Big(\{z_i\}, \{\sigma_j^2\}, \{t_{ij}\}, \quad i = 1, \ldots, n, \quad j = 1, \ldots, 10\Big). \tag{4}$$

The complexity of the model creates two separate problems, both of which are solved using the Monte Carlo Markov Chain (MCMC) simulation method. The first is that the posterior distribution

[7] We note that not all observations have ratings from all ten sources. The BLM measures, for example, are only available for a small set of Latin American countries. Being specialized, they are evidently more carefully measured and prone to less error, but their scope is limited relative the sweeping coverage of Freedom House or Polity scores.

is not analytically tractable anymore, and no simple expression for the posterior means and variances are possible. Using the MCMC method, samples from the posterior distribution are possible even if the posterior distribution is analytically intractable, so long as the conditional distributions whose product makes up the posterior distribution are tractable. In a surprisingly large number of cases it has been shown that while the posterior distribution is analytically difficult to handle, it may be broken down into tractable conditionals. Standard ergodic theorems then show that the chain of samples that sequentially circulates across the conditionals, where sampling from a conditional distribution uses the previous sample as its conditioning information, eventually yields samples from the actual posterior distribution.

The second problem is that there are too many parameters in the full model (see Appendix), more than the available data. For example, the posterior estimates of the t_{ij}'s – each rater's (latent) perceptions of democracy – are accomplished by treating them as parameters. The parameters are as numerous as data points (the native scores). This is not a particularly difficult problem in the MCMC setting. Asymptotics in the MCMC setting mean that any number of samples from the posterior distribution can be drawn from the posterior distribution of parameters in order to estimate functions of parameters – however numerous the parameters they may be. The number of replications are well within the researcher's control.[8]

Our first dependent variable is PMM's unified democracy score (UDS) for each country-year. The UDS score for each country-year i unifies the ten rater perceptions in the manner of (2) in order to measure the latent variable (or parameter) z_i. Since z_i is a parameters whose values are drawn from a posterior distribution of the parameters in (4), PMM provide files with 1000 draws for each country-year from this posterior distribution.[9] We primarily use the mean across these 1000 samples as our dependent variable. The draws themselves are useful to examine the robustness of the primary results, as we explain while discussing our results.

Our second set of dependent variables is PMM's standardized score for the individual raters, specifically Freedom House (augmented by Bollen before 1973), Polity, and PACL. The literature has focused on the use of these specific measures, since the UDS scores are only recently available. PMM also provide individual scores, which are standardized so their magnitudes are comparable. For each individual rater, PMM provide 1000 draws (from the posterior distribution) measuring

[8]This is distinct from conventionally used (frequentist) asymptotics based on large data samples, which are notional and out of a researcher's control.

[9]These are available at `http://www.clinecenter.uiuc.edu/research/affiliatedresearch/UDS/uds.html`

rater perceptions t_{ij}, for each rater j and country-year i that have non-missing native scores.[10]

The third set of dependent variables we use are binary indicators of democracies and non-democracies. In order to do this we need information on the ordinal categories into which the native scores lie. In (4), for brevity, we did not include the cutoff values $\{\lambda_{j,1}, \lambda_{j,2}, \ldots, \lambda_{j,c_j}, \;\; j = 1, \ldots, 10\}$ that define the (ordinal) intervals $[\lambda_{j,1}, \lambda_{j,2}), [\lambda_{j,2}, \lambda_{j,3}), \ldots, [\lambda_{j,c_{j-1}}, \lambda_{j,c_j})$, into which each country-year observation was categorized by rater j. In the PMM model, these λ's are also treated as parameters whose values are simulated from the posterior distribution. Using the (mean of the simulated) cutoff values, we are able to map the UDS, Freedom House, Polity, and PACL measures into a 0-1 democracy/non-democracy measure that is scale-consistent across the scores (we elaborate below). In sum we have a rich set of dependent variables, whose measures are benchmarked to the UDS scores, and hence comparable in magnitude. The econometric estimated of parameters of interest may therefore be compared across measures, allowing for much more robustness than in any other study of political regimes.

The regime scores estimated by PMM are available for each year for which the native scores are available. This means that the Polity scores on the UDS scale are available from 1946-2000, the Freedom House scores from 1973-2000, and the PACL scores from 1946-2000. In our econometric analysis we will use 5-yearly occurrences of the data starting in 1950. Regimes are sticky on a year-to-year basis, and changes in regimes are best observed over a length of time. We augment the Freedom House scores by the Bollen scores for 1950, 55, 60, 65, and 70. Thus, for each of the fours scores – UDS, Freedom House/Bollen, Polity PACL – we have a maximum possible sample size of eleven observations for any country. The data comprise an unbalanced panel covering over 120 countries.

Table 1 provides descriptive panel statistics for our dependent variables. The standardized UDS scorfes range from -1.774 to 2.177 over the sample of 837 country-years (taken 5-yearly). The within-variation in the UDS scores is substantial – the within standard deviation is 0.455 relative to an overall standard deviation of 0.982. The individual scores have similar within-variation, similar range since they are standardized to the UDS scores. We note again that the data themselves are means from 1000 draws from their posterior distributions, and the individual draws may have more or less variation. We perform a sensitivity analysis of the parameter estimates below using each individual draw as data.

[10]These are available at `http://www.clinecenter.uiuc.edu/research/affiliatedresearch/UDS/other.html`

Regressors

Our focus in this paper is on two influences on regime quality – trade openness and proximity to other regimes. Trade openness (OPENNESS) is measured the trade (=imports+exports) -to- GDP ratio, both measured in constant prices. These are taken from the Penn World Tables (Summers and Heston 1991, updated). Regime proximity (CLOSESTREGIMES) is measured as the distance-weighted average of the UDS score (or the specific rater score when that is the dependent variable), where the average is taken over the ten geographically closest countries. Capital-to-capital distances are used as weights. The UDS score is lagged one period to minimize endogeneity concerns. Table 1 indicates quite a bit of within variation in OPENNESS (within standard deviation = 0.22). CLOSESTREGIMES for using UDS scores is shown in Table 1 to have less within-variation than the UDS scores themselves due to averaging.

We argue below that income is a theoretically and econometrically appropriate and adequate instrument. Table 1 shows that there is a fair amount of within-variation in income (PerCapitaGDP, also taken from the Penn Tables), making it a good candidate for explaining the within-variation in OPENNESS.

We will also investigate nonlinearities in how OPENNESS may affect regime quality by interacting OPENNESS with quartile splines of four variables. These are the average years spent in primary school by the population below age 25 (PrimaryYears), the average years spent in high school by the same population (HSYears), percentage of the population living in urban areas (%Urban), and a measure of inequality in education (GINIEducation). The education variables are updated values of the original data in Barro and Lee (2001)[11] Urbanization data are from the World Development Indicators (2007). Using Barro and Lee's data on total years of education for the below-25 population, we computed GINIEducation using the method proposed by Castellóand Doménech (2002 Eq.(3)). Table 1 indicates a good amount of within-variation in the data in these variables. For example, the 2-standard deviation interval within which the values of these variables lie for the average country are: 2.5 years to 4.4 years of primary schooling, zero to 0.52 years of high school, 34% to 60% urbanized, and 0.24 to 0.52 for GINIEducation.

4. Models and Methods

[11] Updated Barro-Lee data are downloaded from `http://www.cid.harvard.edu/ciddata/ciddata.html`

Econometric Specifications

We follow Acemolgu et al. (2005, 2008) and employ two dynamic specifications that remove fixed effects are ordinary least squares with fixed effects (2SLS), and the Arellano-Bond (1991) (henceforth AB) dynamic panel model which differences out the fixed effects. In the 2SLS model we include a lagged dependent variable to capture dynamics, bringing it up to par with the AB model. The AB method has two further advantages over the dynamic 2SLS specification. The first is that is well suited to panels that have a large cross-sectional dimension but a small time dimension, as is the case with out 5-years panel of over 120 countries. AB does not lose valuable degrees of freedom in estimating a large number of fixed effects. Second, the first-differencing in AB models usually accounts for autocorrelation in the error term. Autocorrelation is a serious problem in the study of regime changes, since shocks to regime quality might persist over time. For example, a shock that moves a country towards an authoritarian regime may continue to move it in that direction even in 5 years (positive autocorrelation). Of course, a shock in this time period could also move the country in the opposite direction 5 years later (negative autocorrelation), and our results will indicate what the direction of the autocorrelation, if any, is. The greatest advantage of 2SLS is that the endogenous regressor is instrumented in a manner that makes the underlying theoretical argument for the main instrument(s) transparent. If the errors are not autocorrelated, then it is best to use dynamic 2SLS. Use of the AB model risks over-instrumenting – a caveat that is not often followed by users of this specification. A thumb rule is that the number of instruments should be much smaller than the number of units in the cross section. It is why the model is especially suited to "large-N, small-T" data sets. Otherwise tests of exogeneity and overidentification to assess instrument quality are not valid.

An important but unexamined aspect of panel data on regime quality is that there may be much spatial correlation in the data. Huntington (1991) observes that many countries and their neighbors have experienced regime changes in consonance. Thus, the data may have a strong spatial correlation component, especially during the period of the third wave. We confirm that to be true in the data using tests of spatial correlation (Table A1 in the appendix shows those results). If spatial correlation is exclusively of the kind documented in Huntington, then a solution to the problem is to include a variable that is 'spatially lagged' (e.g. Anselin 1988). The variable CLOSESTREGIMES in fact serves this purpose well. Tests of spatial correlation (e.g. Moran's I test) indicate that including this variable eliminates spatial correlation in the errors. Thus, CLOSESTREGIMES serve two valuable functions – testing Huntington's implication of co-movements in regime quality of countries that are geographically close, and controlling for spatial correlation which would otherwise weaken any inference from the regime score data.

Finally, although both regime scores and openness have an important dynamic component, they are not spuriously correlated. Dickey-Fuller tests disconfirm the hypothesis that either series follows a random walk, implying they are not cointegrated. The 2SLS and AB models are therefore on firm ground.

We begin by estimating 2SLS and AB models without instrumenting for OPENNESS. The dynamic 2SLS specification of this baseline model is:

$$\begin{aligned} \mathrm{UDS}_{i,t} &= \alpha \mathrm{UDS}_{i,t-1} + \beta_1 \mathrm{OPENNESS}_{i,t} + \beta_2 \mathrm{CLOSESTREGIMES}_{i,t} + u_i + v_t + e_{i,t}, \\ & \quad i = 1, \dots, n, \quad t = 1, \dots, 11. \end{aligned} \tag{5}$$

In (5) e_i refers to country-fixed-effects (FE). v_t indicate time-fixed-effects, and we test for the use of a simple trend versus time-FE. A problem with using trend or time-FE is that the cross-sectional average of the regime scores sharply trended upwards as the Second and Third Waves began (Figures 1 and 2). If trade liberalizations were *the* distinguishing feature of the second half (last 25 years) of our sample period, then including time effects unfairly takes away from what should be ascribed to openness. It could even overturn the results. Rather than enter into an irresolute debate over how much of the time effects should really be ascribed to openness, we presume that there were many shocks other than openness occurring in the world economy, including technological change, oil price shocks, financial innovations, more than one realignment of the world order, all of which influenced regime quality of countries, and need to be accounted for via a trend or time FE. Having included a spatial lag term, a lagged dependent variable, country-FE and time-FE, makes it reasonable to proceed on the assumption that the error term $e_{i,t}$ has mean zero, constant variance, and is independent of other error terms.

Even though our data are spaced at 5-year intervals and are less sticky than annual regime quality data, we still suspect error terms to be sequentially dependent. Time-differencing (5) leads to the AB specification which has better serial correlation properties:

$$\begin{aligned} \Delta\mathrm{UDS}_{i,t} &= \alpha \Delta\mathrm{UDS}_{i,t-1} + \beta_1 \Delta\mathrm{OPENNESS}_{i,t} + \beta_2 \Delta\mathrm{CLOSESTREGIMES}_{i,t} + \Delta v_t + e_{i,t}, \\ & \quad i = 1, \dots, n, \quad t = 1, \dots, 11. \end{aligned} \tag{6}$$

The endogeneity of the lagged dependent variable is instrumented using its second lag in the 2SLS specification, and multiple instruments in the AB specification – the AB model is designed expressly to solve that endogeneity problem. We do not attempt to instrument for CLOSESTREGIME. It would be hopeless to attempt to causally interpret the influence of neighboring regimes upon the source country's regime. We are content to observe any relationship as an association, in the spirit in which Huntington observed those co-movements. The biggest challenge that confronts us in interpreting β_1 as the causal force of openness behind regime quality change is finding appropriate and strong instruments for OPENNESS.

Endogeneity

Removing any doubt about endogeneity and identification of the causal impact requires the use of an instrument for OPENNESS. The instrumental variable (IV) should first and foremost itself be exogenous - shocks to the dependent variable should be uncorrelated with the IV. Second, the IV should be strongly correlated with openness. Third, the IV itself should clearly not belong in the regression, that is, the IV should not theoretically or otherwise be capable of explaining regime change (conditional on the other variables).

Finding instruments that satisfy all three requirements is a daunting proposition. In our view, previous attempts in the literature have simply not succeeded in solving the endogeneity problem. López-Córdova and Meissner (2008) fashion an instrument using the method of Frankel and Roemer (1999). They use dyadic (that is, country-pair) data on trade intensity, which they predict using variables motivated by the gravity model. They then aggregate the predictions for each importing country across all its partners to produce an instrumented openness variable, which is then used to explain the variation in their country-year panel of democracy scores. However, in their first-stage regressions on the dyad data, they do not include importer fixed-effects. The gravity instruments therefore appear to have great explanatory power. The problem with their gravity instruments is that, except for population, they are time-invariant.[12] The authors do report a number of second-stage explorations (we laud their transparency). Unsurprisingly, when country fixed-effects are accounted, they find no impact of openness on democracy scores. The explanatory power of their gravity variables appears to be largely due to the cross-sectional variation. Similarly, the

[12]It is not obvious that the statistical significance of population would remain if fixed effects were present. We think not. Population changes rather slowly and where population experiences sudden changes – due to policy, civil wars, seccesions, or unions – these events would be picked up by the fixed effects.

explorations of Bussmann and Schneider (2007), Eichengreen and Leblang (2008), Papaioannou and Siourounis (2008), and are subject to the same criticism – either their instruments, or their models, or both, do not include fixed effects. Two exception are Giavazzi and Tabellini (2005) and Rudra (2005). Rudra finding is a conditional one – trade (and financial) openness enables democratization in developing countries only if social spending (as a percentage of government spending) increases. While the openness measures are not instrumented for endogeneity, the presence of fixed effects is reassuring. Giavazzi and Tabellini (2005) do not find any impact of openness on democracy, but their indentification strategy is based not on instrumenting but a difference in differences. Their results crucially depend on whether averaging over the control group makes it similar to the treatment group.

We believe we have an instrument for openness that satisfies all three conditions. The careful exploration by Acemoglu, Johnson, Robinson and Yared (2008, henceforth AJRY) into the question of whether income causes democracy provides us with an instrument that potentially meets all the three requirements, namely income. Two of their findings are especially relevant. Using 5-yearly observations for a panel of 150 countries over the 1960-2000 period, they find, first, that changes in income do not cause changes in regime quality. Once country fixed effects are included, the estimated impact of a 100% increase in per capita income on the change in the Polity measure of democracy is a mere -0.006 with a standard error of 0.039. The impact of a similar income change on the Freedom House measure is 0.01 with a standard error of 0.035. The large standard errors together with estimates that are close to zero clearly show that income plays virtually no role in explaining regime scores, and hence satisfies the important condition of excludability of income from the democracy equation.

This finding does not imply, however, that there is no correlation between income change and regime change. This may arise if shocks to democracy scores (i.e. regimes) are correlated with income. AJRY's second finding is that such a correlation, as has been found in a number of previous studies in economics and political science, is an artifact of not having controlled for factors that *cause* regime change and are also correlated with income. That is, once these influences are controlled for, there is little, even by way of correlation, that connects change in income with change in regime. AJR show that over the longer 1900-2000 period, a panel of 37 countries produces similar results. In sum,

(i) Shocks to regime change are uncorrelated with income, that is, the error term in (5) is uncorrelated with income, *once we control for factors that fundamentally cause regime change, and are* *also correlated with income.* This is why country-fixed effects are crucial in our study, in order to validly use any instrument A(including income). In fact, fixed effects are crucial to any study that purports to understand what may cause regime change, since the multitude of missing influences that these fixed effects pick up would, in their absence, be mistakenly attributed to other variables. Income has been attributed a greater influence in previous studies (see studies cited in AJR) of the determinants of regimes than it should have simply because those studies ignored fixed effects.

(ii) Unless the period under consideration includes an event of such significance that it alters the future *path* of development, we should expect change in income to be quite uncorrelated with change in regime. Perhaps extraordinary events such as the end the era of colonization that originated the third wave, or the fragmentation of a country such as the 1989 break-up of the Soviet Union, or the start or end of long civil wars qualify as critical junctures. But AJR do not find such evidence in the post-war period or even in the 20th century. Critical junctures are rather more extreme events that critically change the path of development. For example, the countries that were prosperous before WWII continued to be prosperous after it.

Thus, income satisfies the second condition for admissibility of an instrument, namely that it is uncorrelated with shocks to regimes scores, our dependent variable.

The third property of a good instrument is that it should be well-correlated with the endogenous regressor, *conditional* on fixed effects and other control variables. Is income a potentially good instrument for trade openness? The gravity trade model originally due to Leamer and Stern (1970) and Anderson (1979) and empirically tested by Bergstrand (1985) theoretically and empirically established income to be among the principally important forces that determine the volume of bilateral trade among nations. We expect income to therefore serve as fitting instrument for OPENNESS. Weak-instrument diagnostics provide a tight check on whether income is in fact a strong enough instrument (see e.g. Stock and Yogo 2004) for identifying the causal relationship between openness and regime change.

5: Results

5.1 Baseline Models of Openness and Closeness

(i) *Unified Democracy Scores (UDS): Model Selection and Diagnostics:*

All our dependent variables are the mean scores taken over 1000 samples from the posterior distribution provided by PMM. Table 2 presents a panel view of the within-country variation in the dependent variables we will use in the analysis. The top panel pertains to UDS scores, which have been broke down into eight intervals. The table report transition probabilities, that is the empirical probability for a country (scaled by 100) of transitioning from one interval to another. The diagonal elements indicated the probability of not transitioning, once a country is in that interval. The right half of the diagonal increases in magnitude, indicating that once countries find themselves in positive intervals, they are more likely to stay there. If they achieve a UDS score of 1.5 (11.6% of the country-year observations fall in this category), they are very likely (an empirical probability of 0.83) of not experiencing a deterioration in regime. This message is affirmed by the other three panels containing empirical transition probabilities for the BFH, Polity and PACL scores. The main message from these tables is that there is quite a bit of variation in the transition experiences of the more than 120 countries in our sample.

We begin the econometric analysis with the estimates from a set of models explaining the within-variation in the UDS score. Every specification we estimate contains the instrumented 2SLS – or two-stage least squares (2SLS) – and Arellano-Bond (AB) pair of models. All models account for country-fixed effects. All are dynamic panel models that include a lagged dependent variable. One of our objectives here is to narrow down our specification search to the pair that performs best. The six models in the left half of the table experiment with specifications with and without time-effects. The first pair of models have neither trend nor time-FE; the second pair include a time trend; the third pair include time-FE. The coefficient of interest is on the variable OPENNESS. In every 2SLS model we estimate, the two variables OPENNESS and Lagged UDS (or rater-specific score) is instrumented using PerCapitaGDP and the second-period lag of UDS (rater-specific score). The AB models are estimated in differences as in (6), not in levels. The AB models use as instruments a set of variables generated from lag-differences of the dependent variable (see e.g. Roodman, 2008). We restrict the number of excluded instruments to 16 in all models (lag-differences of the dependent variables plus those of exogenous variables, which instrument themselves).

The 2SLS models reject the "No Trend" model, which has a low within-R squared of 0.046 in favor of the "Trend" model. The 2SLS model with time-FE has a highest R squared of 0.317. This set of models fails the important test of no spatial correlation in any of the cross-sections of the panel.[13]

[13]We tested the residuals from the 2SLS model for spatial correlation using Moran's I test (see e.g. Anselin 1988). The test is straightforward for cross-sections, but not for panel data. We tested each of the eleven cross-sections in our data and rejected the hypothesis of no spatial correlation in any cross-section. The cross-sections in the later years are specifically vulnerable to spatial correlation in the errors. The results of these tests are reported in the appendix.

The right half of Table 3 includes CLOSESTREGIME. It is a variable that is not only of intrinsic interest because it tests Huntington's idea of "contagion", but also has the potential to solve the spatial correlation problem.[14] Moran's I tests on each cross-sections cannot reject the hypothesis of no spatial correlation. It thus serves a methodological purpose – conditioning the errors on the (lagged) distance-weighted UDS scores of the ten closest neighbors removes any spatial correlation in the errors.

Including CLOSESTREGIME considerably improves the fit of the 2SLS models over their previous counterparts. CLOSESTREGIME is strikingly significant, both statistically and politically. As noted, we do not instrument CLOSESTREGIME – we consider establishing its causality a futile exercise. Thus, any relationship inferred from its coefficient is an association. It is possible for endogeneity in CLOSESTREGIME to contaminate causal inference about OPENNESS through correlations between the two variables. Their partial-correlation conditional on fixed effects and other variables in the model is statistically no different from zero, which alleviates concern about contamination. In any case, we choose the minor contamination that comes from a small correlation over the problem of spatial correlation in the errors. Dropping CLOSESTREGIME would also impact our inferences about OPENNESS, which, in view the significance of the former, presents a serious missing variables problem.

We base our quantitative inferences on the model with the trend, which conserves valuable degrees of freedom and has a higher adjusted R-squared (0.3821) that the 2SLS models with time-FE (0.3818). Figures 1 and 3 in the appendix indicate why this is so: the movements to democracy are *the* defining events of the 2975-2000 period.[15]

[14] Consider the general spatial autoregression model $y = \rho W y + X b + u$, where b is the vector of coefficients to be estimated and $W : N \times N$ is an appropriately defined spatial contiguity matrix (see e.g. Anselin 1988). ρ is the spatial autoregressive coefficient on the spatially lagged dependent variable Wy. In this model, the assumption of independence of the error term u may be maintained, but is violated if the spatial lag Wy is excluded. While 2SLS estimates are still unbiased the standard errors are incorrect. A simple method suggested by Can and Megbolugbe (1997) for constructing Wy may be adapted here. Define the (i, j)th element of W, w_{ij}, to be inversely proportional to distance d_{ij} between country i and country j, and normalize them to sum to 1. Then the spatially lagged vector Wy – which is our measure CLOSESTREGIME – is given as:

$$\text{CLOSESTREGIME}_i = \sum_{j=1}^{n} w_{ij} R_j = \sum_{j=1}^{n} \left[\frac{\frac{1}{d_{ij}}}{\sum_{j=1}^{n} \frac{1}{d_{ij}}} \right] R_j,$$

where R_j's are regime scores of countries neighboring country i. To minimize endogeneity problems, W lagged regime scores of neighbors are used in the equation. Our measure includes the closest ten countries (d_{ij} is measured between capital cities) that is, $n = 10$.

[15] Since the time series in the graphs are convex, we experimented with adding a squared time trend. It adds no explanatory power and is statistically insignificant.

The time trend captures most of the time variation in the experiences of individual countries, leaving little else for time dummies to explain. Including the time trend raises an issue with assessing the impact of OPENNESS. If movements to democracy were *the* defining events of an era, including a time trend to remove time-effects greatly diminishes the ability of other variables that also were defining events of the same era, like globalization, to explain movements in democracy. This tension is brought to fore in Table 3 when comparing the 2SLS estimates with and without the time trend. The models labeled "No Trend" has a statistically significant *positive* coefficient on OPENNESS, in agreement with the well known emergence of globalization at about the same time as the third wave of democratizations. However, once the time trend is included, OPENNESS is consigned to statistical insignificance. While we base our inferences on a model that includes the trend because otherwise OPENNESS would pick up the effect of other variables that moved with the trend (for example, oil price).

While the 2SLS and AB models do not estimate the same coefficient with conflicting signs, they do not necessarily agree on their statistical significance. The AB model, which has been designed to solve the endogeneity of the lagged dependent variable, produces a strong positive coefficient on the lagged dependent variable while the 2SLS model estimates it with a statistically insignificant (positive) coefficient. The AB model produces a strong *negative* coefficient on OPENNESS while the 2SLS model estimates it to be statistically insignificant. The differences in estimates may be due to a number of reasons. the AB model is estimated in differences while the 2SLS model in levels. The AB model economically accounts for fixed effects, while the 2SLS model uses up about 120 degrees of freedom to do so. Even though the AB model has more excluded instruments (16) than the 2SLS models (2), we have restricted their number to a level where over-instrumentation is not a problem (the cross-section greatly outnumbers the instruments).[16] Both models perform well when assessed on important diagnostics. Consider the "weak instrument" problem that the excluded instruments are only weakly related to the endogenous variables OPENNESS and lagged UDS scores, given the included regressors (country-fixed effects, time trend, CLOSESTREGIMES). A consequence of weak instruments is that the 2SLS estimates are biased. The two instruments used in the 2SLS models do not suffer from the "weak instrument" problem. The Kleibergen-Paap F statistic of 16.97 indicates that the two endogenous variables are well-predicted by the two instruments. More formally, comparing this F statistic with the weak-instrument diagnostics provided by Stock and Yogo (2004, Table 1) indicate that the small-sample bias in the 2SLS estimates is relative to the

[16] We restrict the number of instruments in AB by allowing only the second-period lag of UDS (and its transformations) to be used as an instrument for the lagged UDS score. The AR2 test for the "Trend" model in Table 3 rejects (at the 1% level) second-degree autocorrelation in the differenced error. This means that there is no first order autocorrelation in error levels, enabling the use of the 2-period lag of UDS (and its transformations) as a valid instrument for the 1-period lagged UDS score in the model (Roodman 2000).

bias of OLS estimates is less than 5%.[17] That is, instrumenting virtually eliminate 2SLS bias. The Anderson-Rubin χ^2 test of the joint hypothesis that the instruments are significant and that overidentification restrictions are valid cannot be rejected at the 5% level. The Sargan and Hansen overidentification tests affirm that the overidentification restrictions are valid in the AB models as well. In sum, both models are capable of producing methodologically reliable and credible inferences.

(ii) *Unified Democracy Scores (UDS): Results*

We interpret the coefficients as long-term impacts, $\beta_{LR} = \beta/(1-\alpha)$, where β is the coefficient of interest and α is the coefficient on the lagged dependent variable. In the 2SLSL model, since the estimate on α is statistically no different from zero, the coefficient estimates are themselves long-run impacts. The coefficient on the first variable of interest to us, OPENNESS, is statistically no different from zero. The AB model, however, indicates that in the long-run, a 0.222 (one within-standard deviation) increase in OPENNESS *reduces* the UDS score by 0.622 (*more* than one within-standard deviation). This is a strong result. Neither model supports openness as a force for democracy, and the AB model is emphatic that openness causes countries to move in the opposite direction.

Our results stand in sharp contrast with the findings of Eichegreen and Leblang (2008) and López-Córdova and Meissner (2008). They both find a positive relationship between openness and democracy scores. We think this is mainly because their methods end up using cross-sectional variation, not within variation, to make this inference. The enormous heterogeneity in country cross-sections casts serious doubt on their findings, something that was recognized in the large growth literature in the 1990s. When López-Córdova and Meissner's do account for fixed effects (their Table 11) they find no relationship between openness and Polity scores. Both papers space their data annually, not 5-yearly like we do, with the result that their regime scores are more sticky and the within-variation in the regressors is small (another reason why their results are driven by cross-sectional variation). Finally both specifications include per capita income, which is strongly correlated with openness, but as AJRY show, it does not belong in the regression.

There is less theoretical support for a positive relationship between trade openness and democracy than believed. The mechanism by which openness influences democracy works through inequality

[17] The "collective" bias of the 2SLS coefficients relative to their OLS bias is defined in Stock and Yogo (2004, eq. (3.1)).

in the models of Acemoglu and Robinson (2005) and Boix (2003), as explained in Section 2. How openness affects inequality is therefore determines regime quality in these models. The Stolper-Samuleson theorem, one of the main results of the Heckscher-Ohlin theorem in a 2-good 2-factor world in which every country uses the same technology, provides the strongest argument that trade decreases inequality. Opening trade benefits the factor that is used more intensively in the production of the export good. Thus, for example, the opening up of a labor-abundant developing country to trade enriches labor, thereby reducing the pre-trade income inequality between owners of capital and owners of labor.

The relevance of the Heckscher-ohlin model on which this idea is founded has been questioned in a series of studies beginning with Leontief's paradox. The last word on the subject by Davis and Weinstein (2001) indicates that the basic Heckscher-Ohlin model is rejected by the data.[18] Davis and Mishra (2007) present a compelling argument for doubting the relevance of Stolper-Samuelson in today's world. Once of their strongest arguments applies especially to the case of developing or emerging countries (in which regime changes occur most frequently). Imports of advanced industrial equipment and even sophisticated consumer goods by developing countries do not compete equally with what is produced domestically. Davis and Mishra, in fact, terms them "non-competing" goods. the Stolper-Samuelson theorem simply fails to apply in this context, since it requires imports to shrink the production of the import-competing industry and create the conditions for expanding the exporting sector, where, presumably, the bulk of unskilled labor will move. With non-competing intermediates goods, they show that liberalization of intermediate goods trade benefits capital owners and makes labor worse off. the Stolper-Samuelson theorem applies only when two countries produce the same set of goods, so that trade involves the exchange of competing goods. An oft-observed fact in developing countries is that the sectors that receive protection *are* the ones that employ the bulk of unskilled labor (e.g. Goldberg and Pavcnik). This is consistent with the view that these sectors compete with local competitors or producers from other developing countries. Trade liberalization then makes unskilled workers worse off, widening inequality.

Our finding that openness did not help to enhance the quality of regimes in the third wave is on firm theoretical ground. Trade increased inequality, and via the Acemoglu-Robinson-Boix mechanism

[18] Davis and Weinstein (2001) also find that the model's main prediction about the factor content of trade is rejected even when adjustments are made to factor data to make them suit the model's assumptions about equal factor productivity in the world. Once the presence of nontradables are accounted for, and the fact that techniques of production may differ across countries, the model tests better. But without factor price equalization, it is doubtful the Stolper-Samuelson theorem continues to apply.

encourage movements away from democracy. Below, we discuss other mechanisms by which trade increases, not lowers, inequality

A less theoretical but equally compelling support for our finding is this. While a raft of electoral democracies took birth in the third wave, the wave had a weak undertow. In the words of Diamond (1999): "This growing contradiction - continued expansion of the form of electoral democracy (and even more widely, of multiparty elections), while levels of actual freedom within such regimes diminish signals the ideological hegemony of democracy in the postCold War world system but also the superficial nature of that hegemony." If democracy is defined as *liberal* democracy – well beyond the definition of electoral democracy – where institutions that guarantee civil liberties of individuals and association rule, where elected legislatures (nor unelected elites) have power over public policy, where power is decentralized to satisfy local needs, then in Diamond's view, the third wave was effectively over by 2000. Diamond's insight is relevant to our findings. To the extent that the third wave democracies were illiberal democracies with weak redistributive institutions and concentrations of power among unelected elites, even had the Stolper-Samuelson theorem worked as expected to lower inequality, the way for elites (landowners, capitalists) to prevent redistribution away from them would be to keep their democracies hollow (lowering regime scores) or force a reversal into an authoritarian regime. While this explanation for our does not have a formal theoretical foundation, it is supported by Diamond's analysis and evidence. Perhaps this view will engender theory. Such theory would need to diverge from the median voter model and therefore be an alternative to the one proposed by Acemoglu and Robinson. A special interest model of the kind proposed by Shleifer and Vishny's (2002) "grabbing hand" model of government might be a viable alternative, but we leave its development to others.

The second finding in Table 3, new to the literature, is the strong positive association of a country's regime scores with that of its neighbors. The 2SLS model (with trend) indicates that a one-unit increase in CLOSESTREGIMES is associated with a 0.525-unit increase in the host country UDS score. The corresponding long-run estimate from the AB model is even larger at 0.741 ($= \frac{0.361}{1-0.513}$) This is a large and politically significant association. Huntington's narrative explains this finding as a contagion. The contagion itself played out in stages during the third wave. The earliest democratization in this wave personal dictatorship in Portugal was replaced (by an opposition), followed by the replacement of the military dictatorship in Greece, both influencing the transformation – with elites in power taking the lead – of the personal dictatorship in Spain. The Spanish transition to democracy instigated the replacement of the Argentinian military dictatorship, a country that was culturally, if not geographically, close. This , in turn initiated transformations of the personal dictatorship Chile and the military dictatorship in Brazil, spread to Bolivia. These nascent transitions to democracy in Peru and Bolivia were not stamped out by their militaries. In East Asia, the replacement of the personal dictatorship in 1986 started Philippines on the path to democracy, which,in turn, led the military dictatorship in Korea to expand political participation and competition. While the contagion spread to Burma, and China, they were controlled by their states, but the Taiwanese transformation may be traced to the Philippine democratization. The East European contagion began as they emerged from one-party rule after the break-up of the Soviet Union in 1989, and led to the formation of a number of young democracies. Our association is the first formal evidence of this contagion. The same effect could work in the reverse, but is unlikely. Diamond's (1996) worry about the unraveling of third wave "pseudo-democratizations" has so far failed to materialize. The interesting future questions are not about reversals of the third wave, but whether those movements will consolidate into liberal democracies with wider participation and competition or vacillate around the middle.

(iii) *Polity (POL), Bollen/Freedom House (BFH), and Przeworski et al. (PACL) Scores: Results*

Since much of the literature on regime transitions has used regime scores from specific raters, it is natural to ask whether the results hold up in when rater-specific scores are used instead of UDS scores. The answer is provided in Table 4, where the spatial model with time trend is estimated using data over the 1950-2000 period specific to three raters – Bollen/Freedom House (BFH), Polity (POL) and Przeworski et al. (PACL). The BFH scores do not produce any evidence that openness helps or hurts regime quality. The POL scores indicate just the opposite. The evidence from both 2SLS and AB models is unequivocal: openness hurts regime quality. Using PACL scores produces estimates that are quantitatively and qualitatively closest to those from using the UDS scores. The evidence is not as unequivocal, but neither 2SLS nor AB finds that openness facilitates democracy. On closeness, all measures uphold the finding from the UDS scores – there is a strong positive association between a country's regime score and those of it's neighbors. the coefficients on CLOSESTREGIMES is about the same magnitude as those from the UDS scores.[19]

Three robustness checks on the results in Table 4 are worth mentioning here. First, the same models in Table 4 were estimated using native Freedom House (augmented using Bollen's scores for 1955-1970) and Polity scores over the 1950-2000 period. Second, there is no weak-instrument problem here, no serial correlations that invalidate the 2SLS or AB results, and that the overidentification restrictions in the AB models are valid. The results qualitatively mirror those in Table

[19]The difference in the cross-sections is not the reason for the differing results. Virtually the same results hold if BFH and PACL are restricted to the POL cross-section.

4. For brevity, we do not report these diagnostics in the tables below, but their robustness carries over to all the models we report below. Third and most important, the extra advantage from using the PMM's data come from being able to examine the robustness of the scores to errors in the measurement of regime quality. We estimate the models 1000 times, each time with a sample drawn from the posterior distribution of a rater-specific regime score. In bold are indicated coefficients for which 95% of estimates have the same sign, and are statistically significant. These are resistant to errors in the measurement of democracy. Coefficients which were statistically significant are also seen to be resistant to measurement error. This should be reassuring to those who use these scores in their analysis – these three raters that have the smallest measurement error among the ten raters considered by PMM. We suspect the same is not true of other measures with greater measurement error, since sample drawn from their posterior distribution would exhibit much greater variation. We leave this question open for others to demonstrate.

To the question of whether openness enhances regime quality, the answer provided in Table 4 is unambiguously not. To the question of whether openness has any influence on regime quality, the answer is that it depends on the score that one thinks is most appropriate and the method of estimation that is believed to be most appropriate. We present all results for transparency and with a view to letting the reader make up their minds. We personally favor the result from the UDS scores (similar to the PACL inferences) from the AB model, since it economizes on precious degrees of freedom and yet account for fixed effects. We do believe that with thrusting openness on young democracies adds another dimension to the balancing act their governments must perform, and for which they have not yet developed the institutions to control. If openness causes inequality, then institutions of redistribution must exist for the population to collectively and individually experience the welfare gains. But sounds taxation systems and equitable social spending are the purview of consolidated liberal democracies, not emerging electoral democracies. Rodrik's (1997, 1998) voices these concerns and indicates evidence that managing openness requires established redistributive institutions .

The dependent variables were measured continuously thus far. Do the same results hold for binary measures of democracy (regime score=1) and autocracy (regime score=0)? We used the cutoffs estimated by PMM in order to distinguish democratic from authoritarian regimes. Since native PACL scores are themselves binary, their cutoff serves to identify the democracy-autocracy break for other scores. PMM's indicate that the standardized PACL score of 0.18 separates democracies from non-democracies. PMM's mapping of this PACL score into the Polity and BFH cutoff intervals enable us to set the binary demarcations for those scores. Table 5 presents the results from the instrumented linear probit models. The signs and significance of the coefficients have many similarities with their counterparts in previous tables, but there are important differences. With UDS scores, OPENNESS is now statistically significant in the 2SLS model but not the AB model, no longer significant in the 2SLS model with Polity scores, and significant in both models with PACL scores. Since the designers of PACL were concerned only with this binary result, theirs is perhaps best source of inference about regime *change*.[20] The results from the PACL scores are unequivocal and firm – openness makes it more likely for countries to become less democratic. The 2SLS estimates indicate that a one (within-)standard deviation increase in OPENNESS of 0.222 makes it 8.3% more likely to change from a democracy and become more authoritarian. The AB model estimate is more pessimistic. The same change in OPENNESS makes an anti-democratic regime change 28.4% more likely over the long run.

Rather that looking at this result as providing grist for an anti-globalization mill, recall from our first lessons in trade theory that openness *can* raise welfare in a Pareto sense without making anyone worse off, and therefore there is no reason for openness to trade to be incompatible with democracy. One reason why openness does not appear to support democracy might just be because the gains from trade are not being equally distributed, and the Pareto outcome remains a theoretical ideal instead of a real-world outcome. A view espouse by Rodrik and others is to manage globalization so that the ideal comes close to being realized. If the key is for countries to bring about the ideal outcome within their systems and constraints, then managing globalization has not been easy. The PACL results simply affirm that countries which have been unable to manage it are likely to turn away from democratization. The overwhelming majority of transitions are from weak democracies to weak autocracies,[21] precisely those countries that may not have had the time to build institutions that can distribute the gains from globalization.

Another view is that the ideal is not possible, at least within the life of a political regime, because the gains from trade accrue disproportionately to skilled workers and redistributing these gains from a part of the population that has made the right investments in skills to earn these gains from trade is not just politically infeasible, but may be distortionary and inefficient. Rather, the solution is a wider investment in skills which is expensive and may not be possible to do within the lifetime of a political regime. Prolonging their political life may just require populist distortions, which

[20]The binary results caution us about making inferences about regimes 'changes' – a stronger term than changes in regime quality – from scores designed to distinguish fine-grained movements in regimes than extreme democracy-autocracy measures. BFH and Polity scores are conceptually designed for more continuous movements in regime quality, and are less suitable to understanding regime change.

[21]In the PACL sample the empirical probability for a country to transition from a non-democracy to a democracy is 13.03%, and from a democracy to a non-democracy is 7.43%.

keep a regime in place but one that is underinvested in the right institutions. In their excellent survey, Goldberg and Pavcnik (2007) document the compelling evidence that has accumulated in the large literature on the subject of trade and inequality. Among their most powerful conclusions is that trade exacerbated inequality. The reason is that imports into developing countries were technologically superior intermediate goods compared with those manufactured domestically, and complemented skilled workers. This evidence is richly comparative, covers a range of data – from aggregate country and sectoral studies, to firms data and to individual data from longitudinal surveys. Goldberg and Pavcnik document the problems with measuring inequality in this variety of settings, but point out how these different measures are very credible in the context of their inquiries. It is perhaps the most powerful evidence linking what we find – that openness corrodes regime quality – to the Acemoglu-Robinson-Boix theories about inequality and democracy that predict such a result. Quite simply, if trade increases inequality, it will cause a deterioration of regimes away from democracy.

5.2 Extended Models: Nonlinearities, Trade Surges, Liberalization Episodes

In Table 6 we explore the possibility that openness may benefit different countries differently. The interaction of three variables with openness are considered: years of primary education (PrimaryYears), years of high-school education (HSYears), the proportion of the population organized (%URBAN), and education inequality (GINIEducation). We explore three interactions of openness with each of these variables. For the variable PrimaryYears, for example, three dummy variables are defined , one indicating observations falling in the bottom quartile of PrimaryYears ($I_{PRIMARY1}$), another indicating observations in the middle two quartiles ($I_{PRIMARY2}$), and a third indicating observations in the top quartile ($I_{PRIMARY3}$). We construct instruments for the three interacted variables as follows. OPENNESS is regressed on PerCapitaGDP, fixed effects, and a time trend to predict the variable $\widehat{\text{OPENNESS}}$). The interactions of the three dummies with $\widehat{\text{OPENNESS}}$) are used as instruments for the interactions of the dummies with OPENNESS in both the 2SLS and AB models. They do not suffer from the weak instrument problem in any of the regressions we report. In order to prevent collinearity, we estimate a separate interaction model for each of the four variables. Table 6 shows the results with UDS as the dependent variable. The surprise here is that none of the interactions are statistically significantly different from zero, with the exception of high schooling. The 2SLS estimates model with interactions of HSYears quartiles indicate that if country-years fall within the second and third quartiles of HSYears, then OPENNESS has a *positive* impact on regime quality. Moving from the bottom quartile to this quartile increase the regime score by $\frac{.240}{1-.275}$=0.331, roughly three-quarters of a (within) standard deviation. Moving from the bottom quartile to the top quartile also the same effect.[22] This result is very much in line with the Goldberg-Pavcnik conclusion about trade producing inequality between skilled and unskilled workers. Observations in which the large majority of the population has accomplished a threshold level of education – proxied here by being in the second and higher quartile of years of high-schooling – were well-placed to deliver the gains from trade to that majority, since trade complemented skills. The same result is weakly supported by the AB model, where the impact of being in I_{HS2} is measured less precisely. The same result is also found in the 2SLS estimates using BFH and PACL scores but not with Polity scores.

Is it possible that countries that experienced long periods of openness did better those that experienced it only temporarily or not at all? Rodrik (1999) indicates that countries that experienced long periods of investment surges grew durably and fast. We adopt his way of measuring openness surges as the dummy I^{SURGE} from annual data on OPENNESS for every country in the sample as follows. I_{it}^{SURGE} takes the value one if country i experienced a positive transition in OPENNESS during year t. The beginning of surge is identified as the year T at which the 5-year lagged moving average of OPENNESS is exceeded by each of the five 3-year moving averages beginning in $T+1$. If T is identified as the beginning of an OPENNESS surge then $I_{it}^{\text{SURGE}} = 1$ for $t = T, T+1, \ldots, T+5$. Otherwise $I_{it}^{\text{SURGE}} = 0$. A country i may experience breaks between surges, or no surges at all. While the surge dummy is defined every year, the analysis is conducted at the same 5-year intervals beginning 1950.

Table 7 compares the effect of trade surges in the post-third wave period (during and after 1974) with the period preceding it. The coefficient on interaction $I^{\text{SURGE}} \times I_{\text{PRE75}}$ is unequivocally negative regardless which regime quality measure is used, and estimated with precision when using UDS and PACL scores. It also appears that the second (pre-1974) wave of democratization and reversals was more adversely affected by openness surges than was the third (post-1974) wave. However, formal nonlinear tests cannot reject the hypothesis in any model that the effects were the same. We conclude that openness surges did not necessarily help regime quality either.[23]

[22] The base coefficient is OPENNESS, to which the impact of being in I_{HS2} is incremented by the coefficient on OPENNESS$\times I_{HS2}$. Since OPENNESS is statistically no different from zero, the coefficient on OPENNESS$\times I_{HS2}$ is interpreted as the impact of OPENNESS for a country-year in I_{HS2} relative to a country-year in the bottom quartile.

[23] Since the surge dummy is difficult to instrument, it may be endogenous. The reverse causality, however, would be in the positive direction, going by the evidence in Milner and Kubota and others who have investigated that effect, which mitigates endogeneity concerns.

6. Conclusion

During the Second and Third waves of democratizations and reversals that occurred in the second half of the twentieth century, many countries vacillated between weak democracies and weak dictatorships. The same period was accompanied by innovations that greatly reduced the costs of international trade, bringing countries closer together. Our study investigates the degree to which the waxing and waning of regimes was caused by their openness experiences, and whether openness aided the moves democracy or disadvantaged them.

Globally aggregated data shows a positive correlation between the number of democratizations and the volume of trade in the 1950-2000 period. It appears therefore that that the increased openness of countries pushed them towards better governance and greater self-determination. We believe that correlation has strongly pre-informed researchers and readers in this area. Our closer examination using individual country experiences over time finds this to be untrue about any but the most consolidated democracies.

Our study is a methodologically complete analysis of openness as a force behind the quality of political regimes. We use a dynamic panel model that exploits only within-country variation in the data, in contrast to much of the existing literature. Our identification strategy uses a sturdy and robust instrument – income – based on the finding of Acemoglu et al (2008) that income is uncorrelated with democracy. Finally, we find the importance of spatial contagion, which also helps to solve spatial correlation in the errors. Ignoring this variable leads to a serious missing variables problem. We find that openness does not help democracy, and creates a move way from it. The result is endorsed by the findings in the literature that trade has caused inequality in the post WWII era (Goldberg and Pavcnik 2008). Our findings are therefore consistent with the Acemoglu-Robinson-Boix theory that inequality undermines the stability of regimes, which move between weak democracies and weak autocracies.

What do our findings imply for policy? Many countries in which democracy took root during the Second and Third Waves failed to develop redistributive institutions which are the foundational strength of the industrialized democracies of Western Europe and the United States. Latin American countries, for example, have weak tax systems to this day. Harnessing the advantages of globalization today requires deep-rooted institutions, without which there are no guarantees about what openness may do to the quality of a country's political regime. This is the primary conclusion of our study, and echos the concern raised by Rodrik (1997, 1998) that exposure to trade poses social risks which is only balanced by government spending. Yes, trade can help, but only in the presence of institutional shock-absorbers capable of mitigating inequality .

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Figure 1: Cross-section Regime Score Means

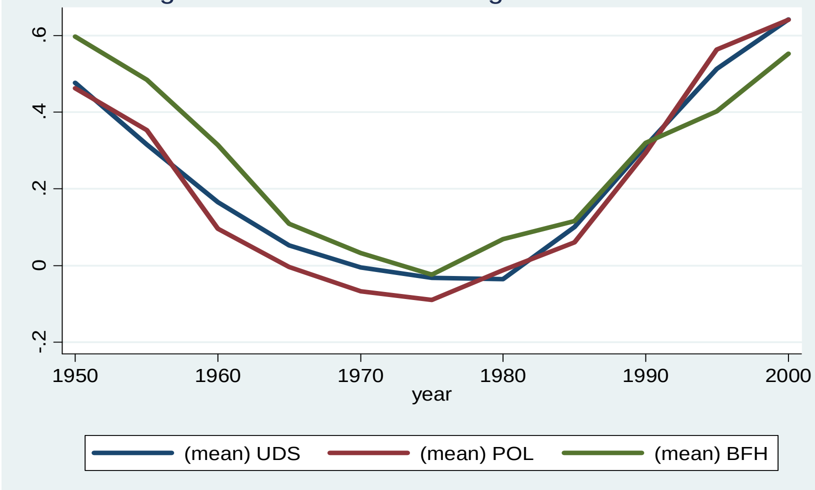

Figure 3: Cross-section Means (Fixed-effects removed)

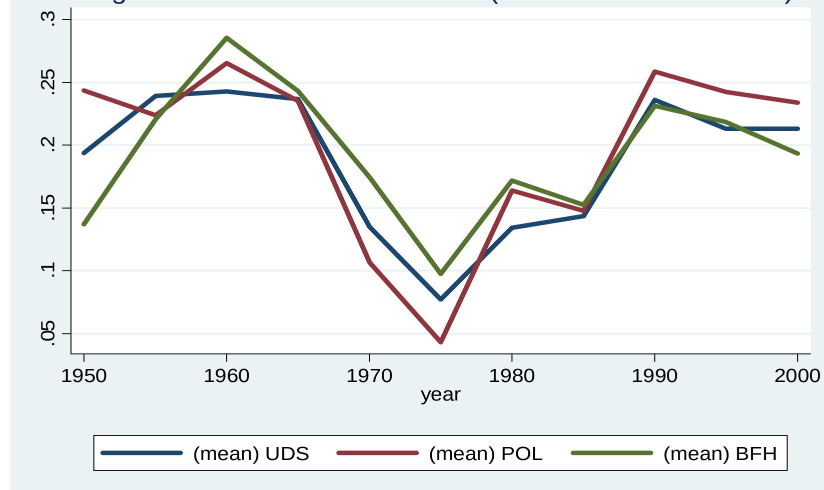

Figure 2: Cross-section Regime Score Standard Deviations

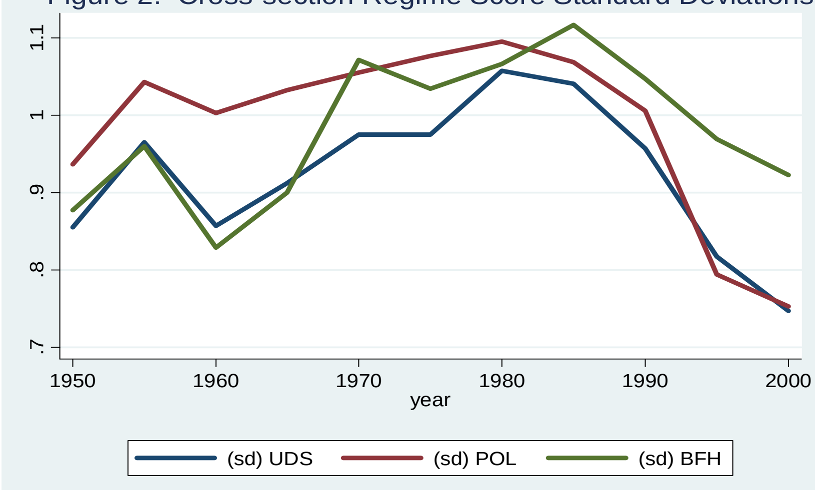

Figure 4: Cross-section Std. Deviations (Fixed-effects removed)

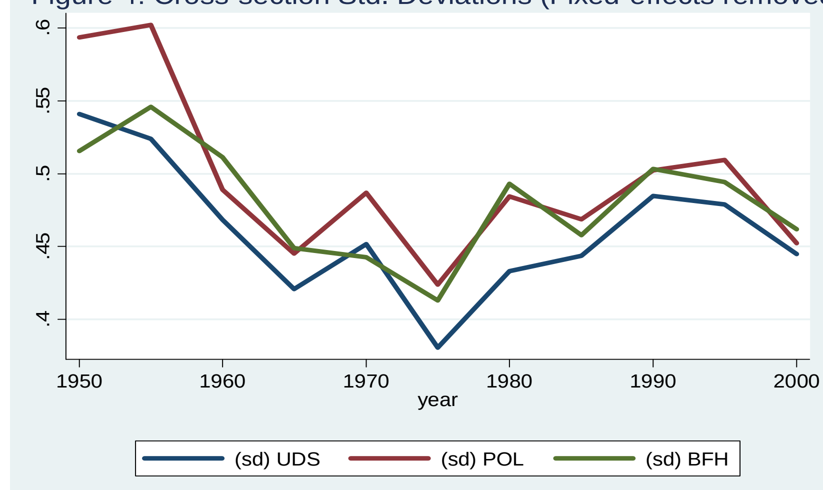

Table 1: Descriptive Statistics

	Variable	Description	type	mean	sd	min	max	*N*
Dependent variables	UDS Score	Unified Democracy Score (standardized)	overall	0.261	0.982	-1.774	2.177	837
		Source: Pemstein, Meserve, Melton (2008)	within		0.455			
	BFH Score	Bollen./Freedom House Score (standardized)	overall	0.272	1.047	-1.890	2.256	814
		Source: Pemstein, Meserve, Melton (2008)	within		0.465			
	POLITY Score	Polity House Score (standardized)	overall	0.250	1.035	-1.867	2.190	748
		Source: Pemstein, Meserve, Melton (2008)	within		0.481			
	PACL Score	Przeworski-Alvarez-Cheibub-Limongi Scores (standardized)	overall	0.265	0.992	-1.776	2.186	835
		Source: Pemstein, Meserve, Melton (2008)	within		0.471			
Regressors/Conditioning variables	OPENNESS	Trade intensity =(x+m)/gdp, constant prices	overall	0.646	0.446	0.027	3.410	837
		Source: Penn World Tables	within		0.222			
	CLOSESTREGIMES	Distance-weighted average of nearest 10 country UDS scores	overall	0.231	0.744	-1.138	1.823	837
		(using latitude and longitudes of capital cities)	within		0.288			
	PerCapitaGDP	Real per capita income (PPP $ per annum)	overall	6704	6681	322	43989	837
		Source: RGDPCH in Penn World Tables	within		2801			
	PrimaryYears	Years in Primary School	overall	3.451	1.717	0.023	7.667	684
		Source: Barro and Lee, 2001	within		0.488			
	HSYears	Years of High School	overall	0.231	0.244	0.000	1.608	690
		Source: Barro and Lee, 2001	within		0.144			
	%URBAN	Proportion Urbanized	overall	0.481	0.234	0.032	1.000	827
		Source: World Development Indicators, 2007	within		0.061			
	GINIEducation	Education Gini	overall	0.384	0.240	0.062	0.996	684
		Source: Barro and Lee, 2001	within		0.070			

Notes:

1. Data are 5-yearly country panels from 1950-2000. The largest coverage (UDS) is 124 countries.
2. The Regime scores are from the Unified Democracy Score website: http://www.clinecenter.uiuc.edu/research/affiliatedresearch/UDS/ based on Pemstein, Meserve, Melton (2008, PMM). These statistics are based on the means taken over a sample of 1000 scores generated from the posterior distribution of the true (latent) scores. See Section 3 and the Appendix.

Table 2: Within-Transition Probabilities (%)

UDS Scores

	[min, -1.5)	[-1.5, -1)	[-1, -0.5)	[-0.5, 0)	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, max]		*N*
[min, -1.5)	-	-	50.0	50.0	-	-	-	-	100	2
[-1.5, -1)	1.0	**43.7**	36.9	9.7	4.9	3.9	-	-	100	103
[-1, -0.5)	0.4	17.8	**50.4**	16.5	10.0	3.9	0.9	-	100	230
[-0.5, 0)	-	3.0	24.6	**49.3**	15.7	7.5	-	-	100	134
[0, 0.5)	-	5.7	10.5	13.7	**50.8**	18.6	0.8	-	100	124
[0.5, 1)	-	0.7	3.5	0.7	9.7	**66.2**	17.9	1.4	100	145
[1, 1.5)	-	-	-	-	-	11.4	**68.9**	19.7	100	132
[1.5, max]	-	-	-	-	-	-	16.7	**83.3**	100	102
Total	0.2	10.1	21.2	13.7	13.0	16.2	14.1	11.6	100	972

Bollen-Freedom House (BFH) Scores

	[min, -1.5)	[-1.5, -1)	[-1, -0.5)	[-0.5, 0)	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, max]		*N*
[min, -1.5)	**43.6**	23.1	18.0	12.8	2.6	-	-	-	100	39
[-1.5, -1)	12.7	**38.1**	38.1	1.6	7.9	1.6	-	-	100	63
[-1, -0.5)	7.1	15.4	**43.2**	16.0	11.2	7.1	-	-	100	169
[-0.5, 0)	1.8	2.9	28.7	**40.4**	19.3	5.3	1.8	-	100	171
[0, 0.5)	-	0.8	5.4	25.4	**43.1**	23.9	1.5	-	100	130
[0.5, 1)	-	1.5	1.5	6.0	19.6	**54.1**	14.3	3.0	100	133
[1, 1.5)	-	-	1.0	-	1.0	11.7	**65.1**	21.4	100	103
[1.5, max]	-	-	-	-	-	-	12.3	**87.7**	100	138
Total	4.2	7.1	17.2	15.1	14.9	14.5	11.4	15.5	100	946

POLITY Scores

	[min, -1.5)	[-1.5, -1)	[-1, -0.5)	[-0.5, 0)	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, max]		*N*
[min, -1.5)	**55.6**	33.3	-	11.1	-	-	-	-	100	9
[-1.5, -1)	1.6	**71.3**	8.5	9.3	6.2	3.1	-	-	100	129
[-1, -0.5)	-	4.4	**65.6**	16.9	6.3	6.3	0.6	-	100	160
[-0.5, 0)	-	3.1	12.4	**61.2**	13.2	10.1	-	-	100	129
[0, 0.5)	-	3.6	11.8	8.2	**60.9**	15.5	-	-	100	110
[0.5, 1)	-	0.8	3.8	2.3	6.1	**80.3**	4.6	2.3	100	132
[1, 1.5)	-	-	-	-	2.1	2.1	**69.2**	26.6	100	94
[1.5, max]	-	-	-	-	-	-	12.7	**87.3**	100	118
Total	0.8	12.6	17.0	14.9	12.7	17.3	9.9	14.9	100	881

PACL Scores

	[min, -1.5)	[-1.5, -1)	[-1, -0.5)	[-0.5, 0)	[0, 0.5)	[0.5, 1)	[1, 1.5)	[1.5, max]		*N*
[min, -1.5)	**-**	-	50.0	50.0	-	-	-	-	100	2
[-1.5, -1)	1.0	**43.1**	37.3	9.8	2.9	5.9	-	-	100	102
[-1, -0.5)	0.4	17.4	**52.1**	16.5	6.8	5.9	0.9	-	100	236
[-0.5, 0)	-	2.6	23.9	**53.6**	9.7	10.3	-	-	100	155
[0, 0.5)	-	6.9	8.3	19.4	**41.7**	22.2	1.4	-	100	72
[0.5, 1)	-	1.8	4.8	2.4	4.8	**68.5**	16.1	1.8	100	168
[1, 1.5)	-	-	-	-	-	12.9	**68.2**	18.9	100	132
[1.5, max]	-	-	-	-	-	-	16.5	**83.5**	100	103
Total	0.2	10.0	22.0	15.6	7.4	19.0	14.1	11.8	100	970

Notes:

1. Empirical probability that a country transitions from a row category to a column category
2. "-" indicates a value of zero (no observations).
3. *N* indiactes number of observations in the data.

Table 3: Baseline (Dynamic) Panel Models -- OLS and Arellano-Bond (AB) specifications

Dependent Variable: Unified Democracy Scores (UDS) Scores

	Models -- Baseline						**Models -- Spatial**					
	No Trend		Trend		Year FE		No Trend		Trend		Year FE	
	2SLS	AB	2SLS	AB	2SLS	AB	2SLS	AB	2SLS	AB	2SLS	AB
Lagged UDS Score	0.235	0.702***	0.270**	0.579***	0.218	0.529***	0.164	0.579***	0.198	0.513***	0.207	0.479***
	[0.148]	[0.126]	[0.133]	[0.149]	[0.146]	[0.155]	[0.129]	[0.133]	[0.126]	[0.146]	[0.135]	[0.146]
OPENNESS	0.989***	−0.594*	−0.673***	−1.589***	−0.516**	−1.799**	0.388**	−0.685**	−0.185	−1.364***	−0.172	−1.616**
	[0.260]	[0.322]	[0.215]	[0.508]	[0.216]	[0.749]	[0.165]	[0.316]	[0.220]	[0.492]	[0.223]	[0.722]
CLOSESTREGIMES	-	-	-	-	-	-	0.706***	0.435***	0.525***	0.361***	0.485***	0.322***
							[0.0911]	[0.0946]	[0.0971]	[0.109]	[0.100]	[0.121]
Trend	No	No	Yes	Yes	na	na	No	No	Yes	Yes	na	na
Year Fixed Effects	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
N	835	837	835	837	835	837	835	837	835	837	835	837
R -squared	0.046		0.271		0.317		0.341		0.385		0.390	
Adj. *R* -squared	0.0437		0.2684		0.3087		0.3386		0.3821		0.3818	
#countries	122	124	122	124	122	124	122	124	122	124	122	124
k	2		3		10		3		4		11	
Instrument Quality:		-		-		-		-		-		-
Kleibergen-Paap *F*	20.59	-	21.81	-	20.01	-	25.32	-	16.97	-	16.78	-
AR2	-	1.707	-	1.908	-	1.89	-	1.941	-	2.02	-	1.961
AR2 *p* -value	-	0.088	-	0.056	-	0.059	-	0.052	-	0.043	-	0.05
Sargan statistic	-	24.94	-	18.32	-	17.92	-	19.32	-	15.66	-	15.03
#excl. instruments	2	16	2	16	2	16	2	16	2	16	2	16
Sargan *p* -value	-	0.0709	-	0.305	-	0.329	-	0.252	-	0.477	-	0.523
Hansen	-	34.01	-	30.42	-	27.11	-	29.37	-	26.9	-	24.06
Hansen *p* -value	-	0.005	-	0.016	-	0.04	-	0.021	-	0.043	-	0.088

Notes:

1. Data are 5-yearly (1 period=5 years) panels across countries. Std. errors in parentheses. ***, **, * indicate statistical significance at 1%, 5%, and 10% resp.
2. All models include a one-period lagged dependent variable (to incorporate dynamics). The second period lag is used to mainly instrument this variable..
   The long-run impacts equal $\beta/(1-\lambda)$, where β is the coefficient of interest.and λ is the coefficient on the lagged dependent variable.
3. In the OLS models real per capita GDP and 2-period lagged UDS scores is to instrument the 2 endogenous variables OPENNESS and lagged UDS scores.
4. Quality of instruments is assessed using two statistics: Anderson-Rubin $\chi 2$ (2 df) tests the (joint) null hypothesis that the instruments are significant and that overidentification restrictions are valid. Kleibergen-Paap *F* is used to assess whether instruments for the two endogenous variables are weak (Section 5).
4. The statistic "AR(2)" tests the null of no AR(1) in the Arellano-Bond model errors. Sargan and Hansen test the null that overidentiying restrictions are valid.
5. *N*= #observations; *k* =#regressors; "#excl. instruments" indicates the number of excluded exogenous variables used to instrument the endogenous variables.

Table 4: Spatial Models -- Popularly Used Regime Scores

Dependent Variable →	Bollen/Freedom House (BFH)		Polity (POL)		Przeworski et al. (PACL)	
	2SLS	AB	2SLS	AB	2SLS	AB
Lagged Dependent Var.	0.148	**0.704*** **	**0.421*** **	**0.576*** **	0.199	**0.479*** **
	[0.134]	[0.108]	[0.102]	[0.175]	[0.141]	[0.151]
OPENNESS	0.288	−0.761	**−0.451** **	**−1.471*** **	−0.238	**−1.267*** **
	[0.265]	[0.467]	[0.214]	[0.554]	[0.232]	[0.481]
CLOSESTREGIMES	**0.365*** **	**0.326*** **	**0.407*** **	**0.403*** **	**0.506*** **	**0.355*** **
	[0.0931]	[0.0979]	[0.0827]	[0.102]	[0.0979]	[0.106]
Trend	yes	yes	yes	yes	yes	yes
N	810	812	728	730	833	835
(Within) *R*- squared.	0.229	-	0.434	-	0.365	-
#countries	119	121	110	112	122	124
k	4	4	4	4	4	4
Instrument Quality:						
Kleibergen-Paap *F*	16.27	-	19.17	-	17.19	-
AR2	-	2.025	-	1.493	-	2.035
AR2 *p* -value	-	0.043	-	0.135	-	0.042
Sargan statistic	-	19.01	-	18.05	-	16.85
#excl. instruments	2	16	2	16	2	16
Sargan *p* -value	-	0.269	-	0.321	-	0.395
Hansen	-	23.39	-	29.83	-	24.89
Hansen *p* -value	-	0.104	-	0.0189	-	0.0717

Notes:

1. See Notes to Table 2. Specification chosen based on UDS score results in Table 2.
2. Coefficients in bold indicate that their 95% posterior confidence intervals do not contain zero. These CI's are exact Cis, based on 1000 replications.

	Bollen/Freedom House (BFH)		Polity (POL)		Przeworski et al. (PACL)	
	2SLS	AB	2SLS	AB	2SLS	AB
Lagged Dependent Var.	(-0.118, 0.418)	(0.477, 0.923)	(0.219, 0.621)	(0.246, 0.910)	(-0.084, 0.497)	(0.185, 0.783)
OPENNESS	(-0.204, 0.800)	(-1.655, 0.176)	(-0.863, -0.047)	(-2.580, -0.475)	(-0.683, 0.217)	(-2.181, -0.348)
CLOSESTREGIMES	(0.182, 0.540)	(0.135, 0.520)	(0.252, 0.565)	(0.198, 0.604)	(0.312, 0.699)	(0.150, 0.545)

Table 5: Democracy versus Autocracy: Binary dependent variables

	UDS		**B-FH**		**POLITY**		**PACL**	
	2SLS	AB	2SLS	AB	2SLS	AB	2SLS	AB
Lagged Score	0.0472	0.532***	0.0469	0.517***	0.330**	0.490***	0.2	0.450***
	[0.198]	[0.0994]	[0.192]	[0.0718]	[0.128]	[0.112]	[0.179]	[0.125]
OPENNESS	-0.433**	-0.536	-0.253	0.389	-0.251	-0.567**	-0.374**	-0.575**
	[0.172]	[0.391]	[0.179]	[0.346]	[0.155]	[0.277]	[0.160]	[0.275]
CLOSESTREGIMES	0.294***	0.252***	0.1	0.145**	0.209***	0.230***	0.265***	0.232***
	[0.0665]	[0.0664]	[0.0644]	[0.0699]	[0.0530]	[0.0586]	[0.0613]	[0.0622]
N	835	837	808	810	728	730	833	835
(Within) *R*- sq.	0.087		0.059		0.267		0.171	
#countries	122	124	119	121	110	112	122	124
k	4		4		4		4	
Model-F	24.255		10.911		32.17		31.176	
#Excl. Instrs.		16		16		16		16

Notes:

1. Transformation from UDS scale to binary scale based on the PACL cutoff in Figure 4 of Pemstein et al. (2008).
   PACL=0.18 (on the UDS scale) is taken to be the binary demarcation between democray and autocracy.
   The B&FH and POLITY cutoffs corresponding to this PACL score cutoff serve as their 0-1 demarcations.
   These occur at the top of FH interval 7 (score=0.151) and bottom of POLITY interval 15 (score=0.204).
2. Linear probabilty models.
3. Trend included.

Table 6: Nonlinearities: Interactions of OPENNESS with Education and Urbanization Quantiles

	Dependent Variable: UDS Scores							
	2SLS	AB	2SLS	AB	2SLS	AB	2SLS	AB
Lagged UDS Score	0.253**	0.481***	0.275**	0.431***	0.211*	0.499***	0.241*	0.465***
	[0.121]	[0.162]	[0.122]	[0.164]	[0.128]	[0.148]	[0.124]	[0.169]
OPENNESS	−0.0123	−0.803**	−0.0894	−0.940**	−0.451	−1.515***	−0.122	−0.866*
	[0.226]	[0.380]	[0.223]	[0.399]	[0.285]	[0.471]	[0.308]	[0.486]
OPENNESS×$I_{PRIMARY2}$	−0.19	−0.133						
	[0.148]	[0.212]						
OPENNESS×$I_{PRIMARY3}$	−0.138	−0.159						
	[0.168]	[0.221]						
OPENNESS×I_{HS2}			0.240**	0.166				
			[0.107]	[0.105]				
OPENNESS×I_{HS3}			0.239*	0.0487				
			[0.138]	[0.156]				
OPENNESS×I_{URBAN2}					−0.0173	0.0569		
					[0.137]	[0.153]		
OPENNESS×I_{URBAN3}					0.233	0.264		
					[0.185]	[0.255]		
OPENNESS×$I_{EDUCGINI2}$							−0.0135	0.00541
							[0.128]	[0.137]
OPENNESS×$I_{EDUCGINI3}$							0.152	−0.0352
							[0.200]	[0.215]
CLOSESTREGIMES	0.513***	0.410***	0.513***	0.427***	0.521***	0.348***	0.516***	0.426***
	[0.106]	[0.105]	[0.105]	[0.107]	[0.0990]	[0.118]	[0.108]	[0.111]
Time Trend	yes	yes	yes	yes	yes	yes	yes	yes
N	681	636	687	646	826	783	681	636
(Within) R−sq.	0.392		0.4		0.36		0.393	
#Countries	94	95	94	95	120	121	94	95

Notes:

1. I_{VAR1}, I_{VAR1}, and I_{VAR3} are binary indicators that the values of the variable VAR are, respectively, in the lowest quartile, in the middle two quartiles, and in the top quartile (taken over the full sample).

Table 7: Openness Surges in the Second and Third Waves

	UDS		BFH		POLITY		PACL	
VARIABLES	2SLS	AB	2SLS	AB	2SLS	AB	2SLS	AB
Lagged Score	0.215*	0.486***	0.149	0.670***	0.420***	0.532***	0.221	0.458***
	[0.126]	[0.140]	[0.129]	[0.110]	[0.105]	[0.173]	[0.141]	[0.142]
OPENNESS	-0.337	-1.578***	0.207	-1.035**	-0.568**	-1.797**	-0.396	-1.434**
	[0.241]	[0.587]	[0.285]	[0.494]	[0.250]	[0.706]	[0.261]	[0.570]
$I^{SURGE} \times I_{PRE74}$	-0.149**	-0.185***	-0.0539	-0.131	-0.115	-0.169**	-0.148**	-0.200**
	[0.0682]	[0.0705]	[0.0767]	[0.0807]	[0.0725]	[0.0677]	[0.0740]	[0.0777]
$I^{SURGE} \times I_{POST74}$	-0.0594	-0.107*	-0.0301	-0.0934*	-0.0512	-0.112	-0.0595	-0.0991*
	[0.0385]	[0.0556]	[0.0437]	[0.0529]	[0.0450]	[0.0735]	[0.0413]	[0.0547]
CLOSESTREGIMES	0.553***	0.390***	0.368***	0.319***	0.429***	0.433***	0.528***	0.382***
	[0.0968]	[0.110]	[0.0917]	[0.0994]	[0.0858]	[0.106]	[0.0969]	[0.106]
Trend	yes	yes	yes	yes	yes	yes	yes	yes
N	838	839	812	813	730	731	836	837
(Within) *R*- sq.	0.383		0.246		0.416		0.361	
#Countries	123	124	120	121	111	112	123	124

Notes:

1. I^{SURGE} is a dummy variable for long-run trade surges, defined as in Section 4.
2. I_{PRE74}=1 for years before 1974 (pre-third wave) and zero otherwise. 1 - I_{POST74} = 1 - $I_{PRE74.}$

Table 8: Effect of Trade Liberalizations in Second and Third Waves

	UDS		Bollen/FH		POLITY		PACL	
VARIABLES	2SLS	AB	2SLS	AB	2SLS	AB	2SLS	AB
Lagged Score	0.163	0.665***	0.15	0.816***	0.393***	0.755***	0.152	0.620***
	[0.124]	[0.132]	[0.129]	[0.104]	[0.101]	[0.127]	[0.138]	[0.144]
$I^{LIB} \times I_{PRE74}$	-0.158**	-0.058	-0.159*	-0.0148	0.00915	0.0489	-0.178**	-0.0469
	[0.0744]	[0.0895]	[0.0881]	[0.119]	[0.0614]	[0.0912]	[0.0818]	[0.0919]
$I^{LIB} \times I_{POST74}$	0.0203	0.0466	-0.00651	-0.0478	0.0498	0.0803	0.0171	0.0604
	[0.0404]	[0.0447]	[0.0467]	[0.0560]	[0.0412]	[0.0489]	[0.0433]	[0.0480]
CLOSESTREGIMES	0.577***	0.380***	0.358***	0.354***	0.434***	0.378***	0.569***	0.369***
	[0.0926]	[0.0963]	[0.0862]	[0.0966]	[0.0815]	[0.0956]	[0.0947]	[0.0978]
Trend	yes	yes	yes	yes	yes	yes	yes	yes
N	837	839	812	814	728	730	835	837
(Within) *R*- sq.	0.389		0.258		0.466		0.371	
#Countries	123	125	120	122	110	112	123	125
Difference	-0.178***	-0.105	-0.153**	0.033	-0.041	-0.031	-0.195***	-0.107

Notes:

1. I^{LIB} is a dummy variable indicating liberalized trading regime (= 1 for years following liberalization unless reversal). Data from Hasumann et al. (2005) and Wacziarg and Welch (2008).
2. I_{PRE74}=1 for years before 1974 (pre-third wave) and zero otherwise. 1 - I_{POST74} = 1 - I_{PRE74}.

Tables A.1: Spatial Diagnostics, Moran's *I* on yearly cross-sections

year		UDS	w/o spatial lag residual 1	w/spatial lag residual 2
1955	Moran's I	0.366***	0.057	-0.014
	z-value	7.35	1.527	0.179
1960	Moran's I	0.179***	0.143***	0.041
	z-value	4.494	3.72	1.362
1965	Moran's I	0.212***	-0.035	-0.062
	z-value	8.679	-0.895	-1.955
1970	Moran's I	0.203***	-0.019	-0.044
	z-value	9.776	-0.385	-1.555
1975	Moran's I	0.256***	-0.027	-0.044
	z-value	13.27	-0.89	-1.734
1980	Moran's I	0.235***	0.021	-0.014
	z-value	13.396	1.665	-0.268
1985	Moran's I	0.301***	0.105***	0.028
	z-value	14.715	5.475	1.736
1990	Moran's I	0.28***	0.04**	0.008
	z-value	13.762	2.351	0.791
1995	Moran's I	0.224***	0.03	-0.017
	z-value	11.238	1.886	-0.449
2000	Moran's I	0.252***	0.047**	-0.001
	z-value	11.579	2.513	0.333

Notes:

1. OLS-FE models with lagged UDS score, OPENNESS (instrumented), and trend.
2. *** and ** indicate statistical significance at 1% and 5%, respectively, implying reject the hypothesis of no spatial correlation.