

Globalization and Democracy, 1870-2000

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Abstract

We study whether international trade fosters democracy. The likely endogeneity between democracy and trade is addressed via the gravity model of trade, allowing us to obtain a measure of natural openness. This serves as our instrumental variable for actual trade openness *à la* Frankel and Romer (1999). We use this powerful instrument to obtain estimates of the causal impact of openness on democratization. A positive impact of openness on democracy is apparent from about 1895 onwards. Late nineteenth century globalization may have helped generate the “first wave” of democratization. Between 1920 and 1938 countries more exposed to international trade were less likely to become authoritarian. Finally, our post-World War II results suggest that a one standard deviation increase in trade with other countries could bring countries like Indonesia, Russia or Venezuela to be as democratic as the US, Great Britain or France. We also see some variation in the impact of openness by region and note that commodity exporters and petroleum producers do not seem to become more democratic by exporting more of such items.

1 Introduction

Does democracy go hand in hand with international trade? Or, on the contrary, does integration stem incipient democracy? The questions go beyond mere academic interest. From an economist’s perspective, answering those questions is relevant on at least three grounds. First, as argued by Amartya Sen (1999), a democratic political system is central to the process of development and is essential for understanding and fulfilling economic needs. Other authors have shown that democracy can have a positive impact on economic growth, on reducing economic volatility, on the ability to handle adverse shocks, and on achieving an equitable distribution

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of income and assets.¹ Promoting closer economic integration in the world might also help achieve economic development by strengthening efforts toward building better institutions [Rodrik (2000)]. Second, the perceived positive causal link between trade openness and democratic governance among policy makers has been an important rationale for deepening trade liberalization, both at the global and regional levels. Therefore, such beliefs, warranted or not, have important implications for the conduct of international trade policy and for the shape that global economic integration takes in the future. To the extent that trade fosters democracy, if at all, the implications are crucial for global trade talks and for the negotiation of regional integration arrangements. Third, from a long-run perspective, there may be a case that today's most economically advanced democracies achieved such status partially because of their engagement with international trade.

Judging from remarks that often circulate in policy circles, both in the United States as well as in other advanced democracies, trade is a major promoter of enhanced political rights. In the United States such a belief cuts across party lines. Vivid illustration of that are remarks by former US President William J. Clinton arguing that “[j]ust as democracy helps make the world safe for commerce, commerce helps make the world safe for democracy. It’s a two-way street;”² and by his successor, George W. Bush, who claims that “when we promote open trade, we are promoting political freedom. Societies that open to commerce across their borders will open to democracy within their borders, not always immediately, and not always smoothly, but in good time.”³ The perceived causal link between increased trade flows and democratization is often brandished by policy makers in support for engaging in trade liberalization talks and the establishment of regional integration arrangements.⁴ For example, the July 2000 presidential election of Mexican opposition candidate Vicente Fox, after 71 years of one-party rule, led some observers to establish a link between the country’s membership in the North American Free Trade Agreement (NAFTA) and its democratic transition.⁵ More recently, China’s accession to the World Trade Organization has been defended in part because increased commercial engagement with that country is expected to facilitate political freedom.⁶

Moves toward regional economic integration across the world in recent years often make

explicit the link between free trade and democracy. In April 2001, the heads of the 34 democratically-elected governments of the Western Hemisphere, meeting in Quebec City, adopted a “democracy clause” declaring that “any unconstitutional alteration or interruption of the democratic order in a state of the Hemisphere constitutes an insurmountable obstacle to the participation of that state’s government in the Summit of the Americas process.”⁷ In essence, countries falling in that situation would be excluded from participating in the negotiation of a *Free Trade Area of the Americas*. Democracy clauses also exist at the sub-regional level in the Americas. Following political turmoil in Paraguay in the mid 1990s, which threatened to de-rail the latter’s incipient democracy, the Mercosur members—Argentina, Brazil, Paraguay, and Uruguay—made political democracy a prerequisite for membership in the South American customs union. Similarly, and perhaps not coincidentally, the transition to democracy in Southern Europe—Greece, Portugal, and Spain—occurred almost simultaneously with an expansion of the European Economic Community. Today, countries seeking accession to the European Union must meet the Copenhagen Political Criteria, which require that candidate countries have achieved “stability of institutions guaranteeing democracy, the rule of law, human rights, and respect for and protection of minorities.”⁸ Last, free trade agreements between the European Union and other countries include a standard democratic clause.⁹

However appealing the arguments linking globalization and democracy might be, they remain to be substantiated by a careful evaluation of the historical record. Given that these claims have important implications for economic welfare, for the conduct of international trade policy and for the shape that global economic integration takes in the future, we believe it is important to ask whether there is any empirical support for such claims. Accordingly, in this paper we investigate whether there is any systematic evidence suggesting that globalization could be responsible for the growing preponderance of democracy. We look at three time periods spanning 130 years of world history: the first era of globalization, immediately preceding World War I (1870 to 1910); the inter-war years (1917-1939), which saw a collapse in international trade; and the post-World War II period (1960-2000), which toward the end has witnessed a second era of globalization. We analyze whether trade openness —what we sometimes call globalization—

causally explains democracy. We define the latter not simply as a political system in which the chief executive is chosen through free, fair, and competitive elections, but in which there also exist institutional constraints on the power granted to the executive and in which political participation and the respect of civil liberties is guaranteed.

Figures 1, 2, and 3 show the evolution of the global average value of “openness” to trade and the average of our preferred measure of democracy for the three historical periods we consider in this paper. The figures suggest that, over the long run, there seems to be a positive relationship between trade and democracy. To date, there has been relatively little quantitative empirical research on the relationship after 1950, and there is no research that we are aware of for the years prior to 1950. Moreover, in the literature that studies the post-World War II period, there is no consensus on whether the apparent positive link holds under a more careful empirical analysis. Margit Bussmann (2001) explores the issue using simultaneous equation techniques and instrumental variables (different from those we propose) and finds that there is little evidence that trade openness is associated with democracy in the late twentieth century. Quan Li and Rafael Reuveny (2002) report a negative relationship between trade openness and democracy after 1970, but do not consider a number of important econometric specification problems. More recently, in a broader study looking at the interplay between democracy, rule of law, openness, and growth, Roberto Rigobón and Rodrik (2004) find an inverse relationship between trade openness and democracy. Our view, spelled out in the next section, is that the existing empirical studies suffer from potentially misleading shortcomings that call for continued research on the issue.

We contribute to the emerging empirical literature by refining the methodological approach to the problem, by providing a long-term historical perspective on the issue, and by presenting some initial thoughts on the channels through which globalization may affect democracy. In particular, we propose a new, powerful instrumental variable to better sort out the causal impact of trade on democracy. Our instrument uses exogenous geographic information to predict trade flows similar to Jeffrey A. Frankel and David Romer (1999). We use this in two-stage least squares regressions for data from the period 1870 to 2000. Contrary to the

empirical work we are familiar with, we find some evidence of a substantive and statistically significant positive impact of openness on the level of democracy. Most of our evidence relies on cross-sectional variation in the data to identify the relationship of interest and our conclusion that there is a positive relationship hinges crucially on this. We believe our instrument, which measures proximity to other nations in a complex way, gives us a good impression of the reduced form association between openness and democracy. But there is some evidence that other time-invariant national characteristics which are correlated with proximity are creating more democracy. However, since it is not immediately apparent which factors these are and our instrument seems to incorporate truly exogenous information that is nonetheless time invariant, we conclude that there is more evidence for a positive relationship than otherwise.

In what follows, we begin by reviewing some theoretical arguments of why we might expect to find a positive link between trade openness and democracy, as well as the existing empirical literature on the subject. We outline our research design in Section 3 and present our econometric results in Section 4. We conclude by putting these in greater context while also suggesting further lines of investigation.

2 Trade and democracy: Theory and empirical evidence

The view, both in academic and popular discourse, that globalization helps build democracy typically rests on two arguments: (i) that globalization is conducive to economic development and (ii) that economic development fosters democracy. The first argument is the subject of a fierce academic debate in economics. Frankel and Romer (1999) and Francisco Alcalá and Antonio Ciccone (2004), among others, argue that trade openness is causally correlated with economic growth. Francisco Rodríguez and Rodrik (1999) offered an (early) skeptical assessment of that literature. Moreover, Rodrik, Arvind Subramanian and Francesco Trebbi (2002) underscore the importance of institutions as the main driver of economic prosperity and find that trade has an insignificant impact on growth and often appears to have a negative effect.¹⁰ Regarding the second argument, the proposition that economic development fosters democracy has been advanced by Seymour Martin Lipset (1959). Barro (1999) and, more recently, Carles

Boix and Susan C. Stokes (2002) find econometric evidence in support of Lipset's proposition.

According to Jagdish Bhagwati (2004), Lipset's views have usually been interpreted as implying that economic development is accompanied by the emergence of a middle class demanding expanded political rights, eventually giving rise to democracy. In accordance with this view, Justin Yifu Lin and Jeffrey B. Nugent (1995, p. 2336) state that

“...a dictatorship or authoritarian regime may not be compatible with long-run economic growth. The more successful is such a state in achieving economic development, the more likely it is that the state will face a legitimacy crisis. This is because both a financially independent middle class and the integration of the domestic economy with the world economy are at the same time both necessary conditions for and natural effects of economic success in the modern world. As a result, democratic ideology of DCs [developed countries] may penetrate the middle class and undermine the legitimacy of the regime. These pressures may also force the state to cut its own power of intervention in order to make credible its commitment to such reforms. Thus, authoritarian states may gradually be transformed into democratic states, as seems to be happening in Korea, Taiwan, and Chile.”

The preeminence given to the emergence of a middle class is reminiscent of recent work by Acemoglu and Robinson (2001) which puts the distribution of income and assets at the center of a model of political transitions. When the distribution of assets is skewed, economic elites fear the adoption of redistributive policies by a democratic government and, as consequence, support authoritarian regimes. In this view, egalitarian societies are more likely to be democratic as there would be less propensity and opposition to redistribution. Acemoglu and Robinson (2001) suggest that policies that result in a better distribution of assets, such as improved access to education by the poor, to the extent that they ameliorate the appeal of redistribution, would prop efforts to democratize. Extending this line of argument, Acemoglu and Robinson (2003) argue that globalization should improve income distribution —through Stolper-Samuelson effects for example— and, in turn, should reduce political conflict and favor democratization.

The evidence on whether globalization results in a better distribution of income is mixed. On the one hand, there is ample evidence suggesting that trade openness and FDI flows may worsen wage disparities.¹¹ That would occur for example if the demand for skilled workers in less-developed countries rises as foreign producers bring with them advanced technologies or as exporters attempt to upgrade their products to access foreign markets. Acemoglu and Robinson (2003) present a model in which even when the gap between skilled and unskilled wages deepens, democracy may ensue, arguing that one could think of skilled workers as representing the middle class. On the other hand, focusing on income distribution in general, and not simply on wage disparities, recent studies come to a more benign assessment of how trade openness affects inequality. Computable-general equilibrium modelling of how trade liberalization affects product and factor prices, coupled with household level information, suggests that trade liberalization within Mercosur in Argentina [Guido G. Porto (2003)], or that continued tariff elimination in Mexico [Elena Ianchovichina, Alessandro Nicita, and Isidro Soloaga (2001)] improve income distribution. Other studies indicate that regions within China and Mexico that are more globalized have more equal income distribution [Shang-Jin Wei and Yi Wu (2002); Fernando Borraz and Ernesto López-Córdova (2004)].

There is of course an extensive non-technical literature in political science that offers alternative explanations and theoretical predictions of how globalization affects democracy. Li and Reuveny (2003) provide an extensive survey, pointing that authors reach opposite conclusions on the subject. A review of that literature is beyond the scope of the present paper. We will simply note in passing that several of the purported reasons why globalization would affect the level of democracy are difficult, if not impossible, to operationalize in an econometric exercise—e.g., the argument that globalization "degrades the concept of citizenship" or that "it deepens ethnic and class cleavages and diminishes the national-cultural basis of democracy".¹²

In sum, theoretical arguments in the economics and political science disciplines do not come to an unambiguous prediction about how globalization affects democracy. Moreover, however persuasive economic models on the subject appear to be, empirical evidence in support of each of the building blocs in those models is inconclusive. Therefore, asking whether democracy goes

hand in hand with globalization is, ultimately, an empirical question.

To the best of our knowledge, the only papers that set out to analyze whether trade openness causally explains the level of democracy, in addition to ours, are those by Li and Reuveny (2003), Bussmann (2001), and Rigobón and Rodrik (2004). Like our paper, both use the same source for defining democracy (see Section 3.2), although Rigobón and Rodrik (2004) use a slightly different definition. Contrary to our paper, all three sets of authors focus only on the post-World War II period. The main issue that authors must deal with in analyzing the causal link between globalization and democracy is the endogenous relationship between the two variables, and the three papers main difference lies in this crucial aspect.

Li and Reuveny (2003) estimate pooled OLS regressions that measure the degree of globalization in terms of openness to trade, FDI, and portfolio capital flows, as well as on the number of democracies in the region to which a country belongs in order to capture the diffusion of democratic ideas across borders. In addition, they include lagged values of democracy as a regressor, as well as information on inflation, and income per capita (in logarithms) and its growth rate. Endogeneity is dealt with by including lagged values of the globalization variables. The authors find a negative relationship between trade openness and democracy. However, in our view, there are several problems with the econometric specification used by the authors. First, if there is serial correlation in the error terms of the regression, as is likely, including lags of the globalization variables does not solve the endogeneity problem. Second, with serial correlation, lagged democracy would be correlated with the error term and ordinary least squares yields biased coefficient estimates; applying dynamic panel techniques would be more appropriate in that case, although they pose additional challenges. In addition, they use fixed effects and a lagged dependent variable in the same estimating models. This is a mis-specification of the econometric model. In this case the regressors are correlated with the error term by definition, and this leads to an inconsistent estimate of the parameters of interest. Third, including all four globalization variables simultaneously might give rise to multicollinearity problems. Fourth, inflation, income per capita, and GDP growth are also likely to be endogenous. Finally, since one channel through which globalization or trade may affect democracy is through its impact

on economic development, coefficient estimates on the globalization variables when including income per capita as a regressor do not take account of that potential channel.

Bussmann (2001) sets up a simultaneous equation framework to understand the interplay between trade openness, democracy, economic growth and peace, although her focus is on the latter variable. The author does not find a significant impact of globalization on democracy. She uses two-stage least square techniques to account for the endogenous relationship between the four variables. The author chooses instrumental variables that do not appear to be valid, from our perspective. In the regressions in which democracy is the dependent variable, she instruments openness, economic growth and her measure of peace with the log of population, GDP per capita, investment, and government consumption among others. With the possible exception of population, all of these are arguably affected by democracy and are, therefore, not valid instruments. For example, we have already argued that several authors found a causal impact of democracy on growth. Bussmann also includes GDP per capita as a regressor and therefore our previous criticism on this point applies to her work.

Rigobón and Rodrik (2004) look at the interrelationship between democracy, rule of law, openness, and growth, and find that trade openness has a negative impact on democracy. They rely on identification through heteroskedasticity (see Rigobón (2003)) to sort out the two-way causal relationship between the four variables. Although we feel that Rigobón and Rodrik's paper has better methodological foundations than the two previous papers, we also believe their approach should be taken with caution. First, there are conceivably other variables that should have been incorporated in the analysis; their omission might affect the estimated results. For example, in our earlier review of theoretical literature, we highlighted the potential role played by income and asset distribution as a determinant of democracy. We argued that openness likely affects income distribution; the converse is probably also true. Moreover, inequality is deemed to have a pernicious effect on economic growth. In a two-stage least squares framework like the one we use in this paper and describe in the next section in more detail, as long as the instruments are not correlated with other variables, such as income inequality, that might affect the regressand, omitting those variables from the regression should not be a cause for concern.

It is not clear that the identification through heteroskedasticity methodology is immune to such omissions. A second concern has to do with the robustness of the results. For example, Rigobón and Rodrik (2004) find that openness also has a negative impact on income levels. However, Ha Yan Lee, Luca Antonio Ricci, and Rigobón (2004), who also apply the identification through heteroskedasticity methodology, come to the opposite conclusion. A last cause of concern is that Rigobón and Rodrik (2004), given the methodology they used, must assume that the estimated impact of openness on democracy is the same for all countries, which is a common assumption in cross-country studies. The results we present below, however, show that openness may have a differential impact on democracy which varies by region, for example. In view of the above concerns, we find Rigobón and Rodrik's finding of a negative impact of openness on democracy provocative, but deserving of an in-depth look.

Before describing our econometric strategy in the next section, we should perhaps emphasize two other aspects that distinguish our work from the existing empirical studies. First, by looking at three different historical periods, our paper exploits data on a number of experiences and variation in both the level of international integration and of democracy. For instance, the years between 1870 and 1913 witnessed rapid integration in the world economy stimulated in part by liberal trade policies, falling shipping costs, economic growth, improved communication infrastructure and decreasing transaction costs of international trade associated with the spread of the gold standard.¹³ It is natural to wonder whether these factors had anything to do with the fact that at roughly the same time many western countries extended the franchise to larger segments of society. Also, the interwar years saw attempts to re-build the international order of the late nineteenth century and a burst of interest in democratization associated with the creation of new countries after the War. Eventually the momentum fizzled and even reversed. The global trading system and capital markets imploded. Fascists and Bolsheviks stormed to power. Liberal democracy lost ground. Because of this, the period is an excellent testing ground for our econometric model. One might ask if the countries with the most extensive commercial links with other countries avoided or delayed the downward spiral into authoritarianism beginning in the mid-1920s. While theory and observation suggests there should be a link between

democracy and integration, our statistical techniques complement these approaches and work to find the reduced form association.

A second innovation in this paper is that we investigate the channels through which open trade might affect democracy. Are the impacts coming from strengthening procedural democracy, i.e., by guaranteeing that the executive is elected through free and fair elections? Are constraints on executive power more stringent as globalization proceeds? Are political participation and civil rights strengthened? We believe that such discussion is an important aspect that deserves consideration and ours is a first attempt in that direction.

3 Empirical strategy

The challenge for an empirical test of the above hypotheses is that there is, in all likelihood, a two-way causal relationship present. Mansfield, Milner and Rosendorff (2000) argue that democracies are more likely to enact free trade policies. In the other direction, international trade, as argued above, may lead to greater political freedoms. The sign of the endogeneity bias in a regression of democracy on openness would be ambiguous *a priori*. Also, in a democracy, interest groups may also be better able to obtain protection from foreign competition and voters may even agree to impose import restrictions in order to help less-privileged social groups.

If there is in fact a simultaneous determination of political and trade outcomes, one possible approach in the absence of a plausible structural model is to find variables that determine trade flows but are not correlated with any other factors influencing the level of democracy. These could then be used as instrumental variables to isolate the impact of greater international trade flows on political outcomes. Such a technique avoids the simultaneity bias which would plague any regression analysis that attempts to explain democracy in terms of trade flows.

As appropriate instruments, we take an approach inspired by the Frankel and Romer (1999) study of the impact of trade openness on output per capita. This is a problem similarly affected by endogeneity. The methodology has been subsequently used by a number of papers including Frankel and Rose (2002), who study how currency unions, through their impact on trade, affect growth; Wei (2000), who examines the impact of openness on corruption and the quality of

governance; and ?, who look at the effect of trade on a country’s environment.

Frankel and Romer used geographic variables—distance from other nations, land area, and information on waterway access—to estimate a country’s “natural openness” through the *gravity* equation of trade. That is, these variables are used as regressors in an OLS regression where the dependent variable to be explained is exports and imports (relative to GDP) between any two countries. The predicted level of openness then serves as an instrument for the actual level of openness. The authors then estimated an instrumental variables regression with income per capita as the dependent variable and openness as an endogenous regressor.

In a similar fashion, we use a gravity equation, augmented with other variables not used by Frankel and Romer, to create an instrument for trade openness. Then we regress a country’s democracy measure on trade openness using our predicted openness measure as an instrumental variable. Of course, other variables besides openness may influence a country’s political status. Nevertheless, pinpointing the correct set of those other variables poses an important challenge in itself, and many would themselves be subject to endogeneity problems.¹⁴ As Rodrik (2000) has noted, democracy is a “meta-institution” which likely defines the effectiveness of other institutions—e.g., the rule of law— that shape economic outcomes. While this is an important line of research, as long as the geographic information we use to predict openness is not correlated with these omitted variables the relationship we estimate will not be biased because of these exclusions.

3.1 Econometric Model

Given the above, democracy in country i during period t might be a function of the following variables: the level of exports plus imports divided by GDP during the period (Openness_{it}); a $(1 \times K_1)$ vector, x , consisting of time-varying country-specific characteristics; and a $(1 \times K_2)$ vector, z , containing time-invariant country-specific characteristics.

Possibilities for time-invariant (or very slowly changing) characteristics that do not suffer from an endogeneity problem are colonial legacy, land area, ethnic or linguistic diversity or simple country indicator variables to control for unobservable characteristics such as culture. Time varying variables might include lagged values of the democracy measure and population.¹⁵

We also posit a possibly heteroscedastic and within country autocorrelated error term u_{it} . The following equation summarizes the basic econometric model of interest:

$$(1) \quad Democracy_{it} = \beta_0 + \delta Openness_{it} + x_{it}\Psi + z_i\Gamma + u_{it}$$

where β_0, δ, Ψ , and Γ are vectors of parameters to be estimated. Model (1) may be subject to endogeneity or omitted variable bias, as discussed before, since openness could be correlated with the error term u_{it} or since variables that belong to z_i might be omitted. To circumvent these problems, we need appropriate excluded instrumental variables for openness that are not correlated with any other possible omitted determinants of democracy not included in equation (1) but which are strongly correlated with trade openness. We use a two-step procedure to obtain such instruments.

First we estimate a gravity equation. The gravity equation is a powerful reduced form model of bilateral trade flows with a long theoretical and empirical history. An early example of a theoretical derivation of the equation we estimate is in Anderson (1979). Modern practical and theoretical applications and variations can be found in Anderson and van Wincoop (2003), Frankel (1998) and Frankel and Rose (2002). The augmented gravity equation we use has the following form

$$(2) \quad \ln\left(\frac{Exports_{ijt} + Imports_{ijt}}{GDP_{it}}\right) = G_{it}\lambda_t + \omega_{it} = \lambda_0 + \lambda_{1t}(\ln(\text{population}_{it})) + \lambda_{2t}(\ln(\text{population}_{jt})) \\ + \lambda_{3t}\ln(\text{area}_{it}) + \lambda_{4t}\ln(\text{area}_{jt}) + \lambda_{5t}(\ln(\text{distance}_{ijt})) \\ + \lambda_{6t}(\text{border}_{ij}) + \lambda_{7t}(\text{landlocked}_{ij}) \\ + \lambda_{8t}(\text{island}_{ij}) + \lambda_{9t}(\text{language}_{ij}) + \omega_{ijt}$$

where G_{it} is a (1×9) vector of explanatory variables, distance is the great-circle distance between two countries' geographic centers measured in kilometers and is a proxy for the size of transportation costs, border_{ij} equals one if country i and country j share a border and is also a proxy for transportation and information costs, landlocked takes on the values 0, 1 or

2 depending on whether neither, one or both countries are on a major body of water, island (defined similar to the landlocked variable) indicates whether the countries are islands, language is 1 if there is a dominant or official language shared by both countries and 0 otherwise, the other variables are naturally interpreted and ω_{it} is a homoscedastic white noise error term.¹⁶

We estimate the vector of coefficients, λ_t , separately for each year. When we pool the data this allows our instrument to vary somewhat over time both because some of the included variables can change over time and because the parameters collected in λ vary over time. The latter might happen for instance if the way in which distance affects trade varies over time say due to technological innovations in transportation and communication.

We then predict the logarithm of bilateral openness for each country pair at each date for which we have all explanatory variables. For each country we then arrive at the level of predicted or “natural” trade openness, our instrument for actual trade openness, by taking the exponential of the predictions and adding up over all other trading partners:

$$(3) \quad \text{predicted openness} = \hat{T}_{it} = \sum_{j \neq i} \exp \left[G' \hat{\lambda}_t \right]$$

where $\hat{\lambda}_t$ is the OLS estimate of the vector of coefficients in equation (2). Note that we can actually predict trade even for countries for which we have geographical data but not trade data. As usual \hat{T}_{it} is uncorrelated with the residuals from the regression and hence incorporates only the geographic and technological determinants of trade.¹⁷ These combinations of variables would seem to be uncorrelated with other possible determinants of democracy.

3.2 Data

We refer the reader to the data appendix for a full description of our sources and methodology for implementing the different econometric specifications. Here we outline the basic characteristics of the data.

To estimate our gravity equation we used trade data from López Córdova and Meissner (2003) and Barbieri (1996) prior to 1913; Barbieri (1996) and Ritschl and Wolf (2004) for the interwar period; and Rose and Glick (2001) for post-1960.¹⁸ We also supplemented the Rose and

Glick trade data with the IMF's *Direction of Trade Statistics* and missing GDP and population data from the World Bank's *World Development Indicators* data base. Where possible we use PPP-adjusted output. Post-1960 the PPP adjustment factors come from the World Bank's *World Development Indicators*; the interwar period uses Maddison's PPP-adjusted GDP. Trade is expressed in US dollars as in the original sources and converted to real values for our two latter samples. Prior to 1910 we divide real trade values by real GDP.

To measure democracy, we use the *polity* score from the Polity IV data set (described by Marshall and Jaggers (2000)). The polity score summarizes different indicators of political authority patterns in order to measure three aspects of a country's political system:

1. *Competitiveness and openness in the process of executive recruitment*, reflecting, for example, whether the head of government is selected through hereditary succession, or by designation of the ruling political elite; or through free and fair elections among candidates from different political parties and in which all citizens have the opportunity to become the executive.
2. *Constraints on the chief executive*, or the extent to which the head of government must take into consideration other points of view during the decision making process, such as those of a legislature.
3. *Competitiveness and regulation of political participation*, measuring the degree to which political competition is institutionalized (e.g., through major political parties that are stable and enduring) and is free from government control (e.g., free and fair elections, respect for human rights and civil liberties).

A weighted sum of the component variables is taken, and two new variables, one measuring democracy and the other autocracy, are created. The democracy variables take the values 0, 1, 2,...,10. The autocracy variables take the values -10, -9, -8,...,0. The Polity IV score is the sum of the eleven point measure of democracy and the eleven point measure of autocracy and, thus, it ranges from -10 to 10. Countries are only included in the Polity IV data set if they achieved

independence by 1998 and had a population of 500,000 or more in 1998. Dependencies at any point in time are not included.

We use the Polity IV measure because it appears to be the best available data set that measures deep political and institutional change over a long time and for a large number of nations. As a check we carry out a similar analysis using the binary measure of democracy created by Alvarez, Cheibub, Limongi and Przeworski (2000). Other measures such as Polyarchy measure only participation rates in elections, while Freedom House measures information relevant to economic and political liberties at the personal level and is only available from 1973. Nevertheless we are aware that any attempt to measure the level of democracy is fraught with conceptual and mechanical problems or inaccuracies.¹⁹

4 Results

Below we show that there is ample, but not incontrovertible, evidence for a sizeable positive relationship between openness and democracy. For the interwar and post-1960 period, our econometric evidence suggests that switching from complete economic autarchy to the median level of (PPP-adjusted) openness could, in the long run, raise a country's polity score by as much as three or four points. In other words, in the year 2000, such opening up to trade could have transformed Russia, Venezuela or Indonesia into a full-fledged democracy with a polity score equivalent to that in the US, France or Great Britain.²⁰ The relationship is also statistically significant and positive but less precisely estimated prior to 1913. Still, the point estimates suggest that a change in openness seems to have a similar impact over all three historical periods.

Though our results are fairly robust, some specifications suggest that we have found a spurious correlation. We use various panel techniques and also year-by-year cross-sectional regressions to identify the coefficient on openness. Our regressions are forced to rely on cross-sectional variance for identification and hence the data reveal more about the long-run equilibrium relationship of interest than the dynamic impact of opening up on democratization.

In a panel setup that allows for general country fixed factors or country fixed factors and

persistence in regime choice, the relationship disappears in statistical and substantive terms. We believe this is because our variables of interest, including our instrument, do not sufficiently vary over time within countries. This impedes us from disentangling the impact of opening up to trade from the influence of unknown fixed factors or country specific unobservables. We do however make an attempt to control for certain identifiable fixed factors and still find some evidence of a positive relationship.

4.1 Instrument Validity

As described above, we use the estimated gravity model separately for each year of our sample to predict dyadic trade relative to output as in equation (3). The results from this “first stage” regression are reported in Tables 1 and 2. The gravity model itself fits the data very well, the signs and sizes of the coefficients make economic sense and are mostly comparable to other studies. One difference between the earlier periods and the latter periods is that land areas are either positively correlated or statistically unassociated with trade.

As instruments, the predicted values of openness seem extremely powerful. Indeed Tables 3 and 4 show that predicted openness is always extremely highly statistically significant in a regression of actual openness on this variable. In fact, the t-statistic on the key excluded instrument–predicted openness–is always larger than 2.7. The F-statistic is often an order of magnitude larger. In the year-by-year regressions, it is also usually much larger than this. It is often on the order of six or larger in the 1960-2000 period, and in the pooled data it is never smaller than 16 and as large as 42. The predictive power and the correlation between our instrument and openness is relatively weaker but still quite strong when including controls for country size as column (3) from Table 3 shows.

We also tested the exclusion restriction that the geographic information incorporated in the gravity equation was uncorrelated with the error term with Hansen’s J-statistic (a test for overidentifying restrictions that is robust to heteroscedasticity). This is actually a delicate matter because the equation of interest is just identified if natural openness is the only excluded instrument.

To resolve this we extracted the key regressors used for the gravity equation from the dyadic

trade data set. For those variables that are defined at the dyad level, we generated simple averages or sums. For example, for distance we created the logarithm of the average distance to all trading partners. For the border and common language indicators, we generated the total number of borders and the number of countries with which the home country shared an official language. We have done the summation over the dyads with non-missing trade values. Our list of instruments that could potentially be correlated in the error term or have a direct effect on the level of democracy is then average distance to all trading partners, the logarithm of land area, the logarithm of population, the number of borders, the number of countries with a common language, and island indicator, and an indicator if the country is landlocked.

The findings are overall encouraging. First, for each period, in a pooled panel model without fixed effects and when we include all of these variables as excluded instruments we soundly reject the exclusion restrictions. Specification checks found that when we include population, land area, number of borders and the landlocked indicator directly and use only the average distance to countries and the island indicator as excluded instruments we can never reject the null hypothesis of exogeneity. It would appear that the variables besides distance and being an island matter directly for the determination of the level of democracy. We believe this could very well be because of the same logic that dictates we should include land area and population in the second stage regressions. We would also argue that inclusion of these variables will lead to consistent estimates as they seem exogenous in terms of the level of democracy. The logic for including these variables would be that countries with few borders and those that are not landlocked are also larger than the average country. Hence they substitute internal trade and commerce for international trade. If it is commercial interactions that matter then it could be important to specify the model in this way.

A few other observations are worth making here. In year-by-year cross-sectional regressions the list of variables in question (except population and land area) usually appear to be exogenous.²¹ This is opposite to what happens when we pool the data. However, this is not the case for a period spanning roughly 1965 to the mid-1980s where the overidentification tests tell us the same as in the pooled setup.²² Another more formidable problem with extracting these

underlying factors and using them as the excluded instruments (or even using only the subset that appear exogenous as the excluded variables) is that a) we lose the important information incorporated in the vector of parameters estimated in the gravity equation and b) the gravity relation breaks down. The latter happens because in the first stage we are predicting the *level* of openness with an atheoretical list of variables in a log-linear specification. Both of these factors combine so as to make this list of variables extremely weak in the year by year regressions. To counter all of these problems, the approach we take is to continue using predicted openness as the only excluded instrumental variable and to control directly for the factors that seem suspect according to the tests of overidentifying restrictions. In this way, identification of the effect of openness on democracy will come from that part of natural openness which is uncorrelated with the included exogenous variables.

We have chosen to present estimates of the panel models and then to present regressions for single selected years for comparison. For the post-1960 period and from 1870 to 1913 we provide results every five years. For the interwar period, more data was available for certain years, and so we present results from those years.²³

Our key results use various panel data techniques which provide different ways to look at the data. Our main results use three common specifications: country “random effects”, country “fixed effects” and a partial adjustment model which allows for a lagged dependent variable. We also ran GMM and instrumental variables regressions on the first differenced data to control for fixed effects and to allow for the lagged dependent variable. A word about each type of model and our justification for using it is in the econometric appendix.

4.2 Pooled Data Results

With these considerations in mind, we estimate equation (1) both controlling and not controlling for endogeneity. Tables 5 and 6 present various specifications. First observe that a univariate OLS regression of polity on openness shows a statistically significant and positive relationship in all three time periods.²⁴ Comparing instrumental variables estimates from column (2) to OLS estimates in column (1) for all three sub-tables suggests the OLS coefficient is biased slightly downward. This could happen for example if openness had measurement error and only roughly

proxied democracy-enhancing interactions. Alternatively it could simply be sampling error as Frankel and Romer suggested.

To check the robustness of the univariate results, we included various measures of a nation's size and also the troublesome landlocked, borders and shared language variables. Columns (3) and (4) show these results. In the 1960-2000 period and the interwar period, our coefficient is positive and highly statistically significant. In the pre-World War I period, the coefficient is significant at the 93 percent level. Overall however, the qualitative results are quite similar to the simple specifications in column (2), so in further specifications below rely on this pared down model as a baseline. Using the coefficients from column (3), the magnitudes of the partial effects given a one standard deviation change in openness in the pre-World War I period, the Interwar period and the post-World War II period are four, five and three polity points respectively. This long-run comparability is interesting in its own right and could be suggestive of a structural relationship between the two variables.²⁵

In contrast to these results, columns (4) through (6) which control for fixed country effects provide no evidence for any link between openness and democracy.²⁶ The coefficient on openness is uniformly much smaller (even becoming negative prior to World War II) than in the random effects specifications, and they are not statistically significant at any conventional level. For skeptics of a positive relationship, the interpretation of columns (4) through (6) is that correlation between openness and even natural openness and unobservable or omitted variables or factors are responsible for generating the already discussed positive coefficients on openness from column (3) or (4). However, one should not immediately accept the idea that the positive relationship found in the random effects models is spurious.

The obvious reason that the fixed effects specification does not work is that for many countries our instrument for openness does not vary too much over time. Dickey-Fuller tests for predicted openness (unreported) show that for the vast majority of the countries we could not reject the notion that the levels were stationary. Most of the variation appears to be across countries rather than within countries. In this case it would not be wise to expect good identification from a fixed effects regression that relies on within country variation in the data.

The key excluded instrument seems highly collinear with the included fixed factors. But it is itself based on factors which are time-invariant or are slowly changing. Moreover, it is a prediction of actual openness which also changes fairly slowly over time. At the same time, it does not seem sensible to say that our measure of proximity and geography are correlated in any substantive way to other unobserved or omitted factors. So a plausible conclusion is that there is a mechanical problem here. A reasonable approach in the absence of other instruments then is to rely on the estimates that come from cross-sectional identification either in a pooled sample or in the year-by-year regressions.

Nevertheless in columns (1) and (2) of Table 7 we drop the country dummies and include a number of time-invariant controls that the fixed effects could be capturing. These include the absolute difference between a country's latitude and zero, a measure of ethnolinguistic fractionalization (from Roeder 2001) as measured in the year 1985, indicators for petroleum and commodity exporting countries and regional indicators. We focus on the 1960-2000 period as such data is only readily available for these years. In column (1) we see that the coefficient on openness is much smaller than in Column (3) of Table 5, but it is still positive and highly significant.²⁷ However, when we control for country size, the coefficient on openness diminishes in size and statistical significance so that it is statistically indistinguishable from zero. This is likely to be for the same reasons that plague the fixed effects regressions of Tables 5 and 6. Moreover we would expect proximity to be negatively correlated if at all correlated with size. But instead the coefficient appears to be upward biased when omitting size controls.

Other notable results on these new control variables suggest that countries further from the equator are more democratic while exporters of commodities and possibly petroleum may be less democratic after controlling for other factors. Also the data confirm that, conditional on a number of variables, the Middle East and Sub-Saharan regions are the least democratic in the world compared to the US and Canada while the Latin American/Caribbean countries, East Asia and Western Europe are somewhat less so relative to these two North American countries. Because it is not obvious why our instrument would be correlated with these fixed factors, except for the fact that these are slowly moving over time, and because these factors explain almost

60 percent of the variation in our instrument, Table 7 makes us more comfortable with the idea that trade openness has a role in determining political outcomes rather than unobservable factors or other obvious underlying fixed characteristics of countries.²⁸

We also present two other types of dynamic specifications in Tables 5 and 6. The “partial adjustment” model of columns (7) through (9) shows that the long-run impact of a change in openness is roughly approximated by the simple non-dynamic specification. For instance column (9) shows for the post-1960 period that the long-run coefficient is comparable to the estimate of column (3). It is calculated as $0.1 (= \frac{0.004}{1-0.96})$. For the interwar period, the long-run marginal impact appears roughly similar to what column (3) implies. Prior to World War I, the magnitude of the coefficient on openness varies between -0.002 and 0.006 and does not appear statistically significant. For the latter two periods however, these specifications suggest that there is a dynamic process underway and that adjustment of the polity level to changes in openness is cumulative rather than immediate as one might expect.

The Arellano-Bond/GMM and Anderson-Hsiao/IV estimates are in columns (10) and (11) of Tables 5 and 6. We find very mixed results when we eliminate fixed factors in this way. Most of the time the coefficient is statistically indistinguishable from zero which echoes the results from the other fixed effects specifications. In the GMM specification, we could not reject the null hypothesis of no second order autocorrelation in the transformed error terms, and so we rule out the possibility that this problem has caused our estimator to be inconsistent. The reason we find a coefficient statistically indistinguishable from zero is again unclear. It is known that the Arellano-Bond procedure can produce weak estimates in a panel context with high persistence in the dependent variable. The problems of invariance in the instrument cited above also creep in here, so it is unclear how worthwhile these specifications are given the data structure.

In columns (3) through (6) of Table 7 we also control for the possibility of heterogeneous coefficients by region during the 1960-2000 period. It is possible that cross-regional differences in the relationship could bias our aggregated estimates. We include separate slope coefficients on openness for a number of geographical regions. This is under the assumption that certain unobserved cultural or geographical variables could influence the connection between openness

to trade and democracy in a similar way throughout the region. Our regions, as seen in Table 7, are South East and East Asia, Eastern Europe and Central Asian states, Middle East and Northern Africa, South Asia, Western Europe, Sub-Saharan Africa, Latin America and the Caribbean, and North America (consisting only of the United States and Canada).²⁹

There appears to be a particularly significant positive relationship in Western Europe, North America, Latin America, South Asia and post-1989 Eastern Europe.³⁰ The same cannot be said for the Middle East/North Africa nor for Sub-Saharan Africa. In both regions, the estimated coefficient is usually negative and statistically different from zero. If we include lagged values of the polity score, the coefficient signs stay the same, but we can only reject the null hypothesis of no relationship for Latin America, Western Europe and the US and Canada.³¹

Besides showing some regional differences in the relation, another lesson from Table 7 is that primary commodity and petroleum exporters do not benefit from the pressures to democratize arising from international trade.³² It would therefore tend to suggest that each dollar of international commerce is not equally beneficial for the process of democratization. This evidence would be compatible with the idea that increased competition in goods markets or Stolper-Samuelson effects in a two-factor model could force more competition in the political sphere.

4.3 Robustness: Year By Year Results

Results: 1960-2000

We present simple specifications similar to those above for various years so that the reader can see if the panel results are masking underlying changes in the relationship over time.³³ For various baseline years, Table 8 presents OLS regressions and shows that there is a positive relationship between openness and democracy which is statistically significant at conventional levels between 1960 and 1975. Simple OLS suggests the coefficient decreases over time from 0.23 in 1960 to a low of about 0.05 in 1995.³⁴

Table 9 reports our year by year instrumental variables regressions. The relationship between international trade and democracy is positive but the coefficient tends to become less statistically significant over time. Up to 1980 it is significant at greater than the 90 percent

level. The coefficient also changes magnitude somewhat from year to year and shows a tendency to be trending downwards (as in the OLS results) over time.

In all years, the relationship is fairly substantive. In 1960, the estimated coefficient is at its maximum of 0.28. This seems substantive. A one standard deviation increase in openness, or a move from autarchy to the median level of openness of 12 percent, would imply an increase in the polity score of three and a half points. This is equal to almost one half of the polity score's standard deviation. According to the polity rankings in 1960, this would have been equivalent to seeing the likes of Brazil or Turkey become as democratic as the US, the UK or the Netherlands.

In the year 2000, when the coefficient is at its estimated low, the marginal effect of opening up from autarchy to the median level of openness (i.e., 18 percentage points) would have been to increase the polity score by a much smaller but still substantive 0.72 points. This implies a beta coefficient of about 0.13. In that year this would have been similar to moving from a polity score of 8, as in Mexico or Senegal, to one with a polity score of 9 as in France, South Africa or India.³⁵

Robustness

We included size variables in Table 10. The difference between the results here and the previous table is largest in 1960 and 1965. Controlling for country size also increases the statistical significance of the coefficient on openness in most years. Besides eliminating the already discussed bias, it appears to alleviate the excessive influence Singapore exerts on the results.

Ordered probit specifications for selected years appear in Tables 11 and 12. As mentioned above, this specification of the dependent variable is technically more correct, but whether the parameterization makes a difference in practice to the substantive results is the question. We follow techniques adapted from Rivers and Vuong (1988) to control for endogeneity. Our results are qualitatively similar to the two-stage least squares results. The coefficient is larger in earlier years and openness is associated with more democracy. In contrast to the linear regressions, the coefficient is also statistically significant at greater than the 95 percent level in all years

except 1970.³⁶

Finally, our regressions use “generated instruments” as instruments rather than the geographic information itself. As in Wooldridge (2002 p. 117) the usual two-stage least squares standard errors are asymptotically correct under the assumption the parameters from the gravity equation are consistent and the correlation between the geographic information and the error term is zero. Nevertheless, we simulated the change in the standard error of the openness coefficient for small changes in the underlying geographic data.³⁷ This allows us to gauge how much sampling error could be affecting the standard errors. To save on computation time, we chose to carry out such a simulation for 1960 and 1995. Doing so barely altered the standard errors on the coefficient on openness. For example, our simulations yielded an increase of 0.001 in the standard error of the openness coefficient in both years.

Results: 1920-1938

For the 1920s and 1930s, our regressions support the idea that naturally open countries or places where trade was more important were less likely to fall to authoritarianism. Table 8 shows that during the interwar period there is a highly statistically significant and positive relationship between openness and democracy when not controlling for endogeneity. The results here show that the coefficient on openness is much larger than that in the pre- and post-War eras. In many years the coefficient is between 0.3 and 0.5. The marginal effect of a one standard deviation change in openness is measured as roughly four which is similar to that in the early post-1960 period.

Table 9 reports the instrumental variables regressions. The relationship between international trade and democracy is positive, highly statistically significant and larger than nearly all the year by year estimates after 1960. The estimated coefficient of 0.53 in 1928 implies a move from the median level of PPP-adjusted openness of 21 percent to 10 percent (a one standard deviation decrease) would imply a decrease in the polity score of roughly six points. In Finland for example openness fell from 31 percent in 1928 to 19 percent in 1931. The polity score fell from ten to four between these years. Of course other countries experienced falls in measured openness during the worldwide depression, but the econometrics suggest the countries with the

largest falls in trade would be expected to succumb to increasing authoritarianism.

For this sub-period we also tried other specifications of these baseline regressions. We included controls for size in Table 10 and obtained larger more statistically significant coefficients on openness than those in Table 9. Here there is almost a one-for-one relationship between openness and the polity score. Ordered probit results produce positive coefficients (and increases in the polity score for increases in openness) that are also highly statistically significant in all years of the interwar period.

Results: 1870-1913

For the relatively limited nineteenth century sample, Table 8 shows that in many of our benchmark years there is a statistically significant and positive relationship between openness and democracy.³⁸ Simple OLS suggests the coefficient on openness equals roughly 0.1.³⁹ Statistical significance is higher in the latter years of this period suggesting that this first wave of globalization (which dates from at least the mid-nineteenth century) took time to provoke the initial stimulus to democracy's first major advance.

Table 9 reports instrumental variables regressions. The relationship between international trade and democracy is positive and statistically significant at better than the 90 percent level in four out of the nine years presented. In most years, the OLS coefficient seems to be biased downward compared to the instrumental variables regressions. The estimated coefficient of 0.1 implies that a move from autarky to the median level of openness of 38 percent would yield an increase in the polity score of roughly four points. According to the polity rankings in 1910, this would have been equivalent to seeing countries like Spain, Portugal or Denmark gain a polity score equal to the US or Canada.

We tried other specifications of these baseline regressions to test their sensitivity. Ordered probit results in Table 12 find some evidence that there is a positive relationship between democracy and openness. While the coefficient is positive and the marginal effects are too, in many years these are only statistically significant at about the 85 or 90 percent confidence level. One might attribute these imprecise results to the fact that the number of degrees of freedom are so small. Indeed, given the theoretical arguments and the other empirical evidence, we feel

cautiously inclined to trust the point estimates and to downplay the large standard errors here.

4.4 Channels through which globalization affects democracy

As in the existing literature, we have concentrated on looking at how trade openness affects an aggregate measure of democracy. Although we find some evidence that the latter is causally explained by the globalization of goods markets, the results presented thus far do not tell us through which channels the causal impact might take place. The Polity IV data set allows us to remedy such a shortcoming and to undertake an initial exploration of those channels.

As explained in section 3 (3.2), our measure of democracy, the *polity score*, summarizes three aspects of a country's political system: (i) competitiveness and openness of executive recruitment; (ii) constraints on the chief executive; and (iii) competitiveness and regulation of political participation. We take a look at each of these aspects in turn and ask whether trade openness has a causal impact on them using the same econometric strategy that we used for the aggregate polity score. We concentrate on the 1960-2000 period.

Instrumental-variable regression estimates with pooled data (not shown) are qualitatively similar to those presented in Table 6 for the aggregate polity score. Each component of the polity score is significantly affected, in statistical terms, by trade openness, although significance disappears when we account for country size. Likewise, year-by-year results are, in general, in line with the results presented in Tables 8 and 9, suggesting that openness affects all of the components of the polity score but that statistical significance drops in recent years. Interestingly, however, our previous results using the aggregate polity score appear to be driven, in certain years, by the behavior of one of its particular components but not by all of them. For example, Table 13, which extends the results in Table 9, indicates that openness affects democracy in the year 2000 solely through its impact on the competitiveness and regulation of political participation; in contrast, in 1970 and 1985 openness only had an impact on executive recruitment and not on the other two components of the polity score.

In sum, while it is difficult to extract any unambiguous lessons from the above exercises as to the channels through which globalization affects democracy, our results suggest that the relative importance of those channels may be changing over time. This possibility may merit a

further look in future studies.

5 Conclusions

In this study we attempt to gauge the causal impact of trade openness on the level of democracy. To do so we construct a measure of “natural” openness and use it to see if countries that are more naturally open are more democratic. Indeed we find that open countries have consistently been more strongly democratic between 1895 and 2000.

Our methods suggest that an autonomous move from autarky to the average level of openness could raise a country’s democracy measure by somewhere between three and five points over the long-run. This is a substantial increase. To put this in perspective, this would have been comparable to seeing countries like Argentina, South Korea, Brazil or Romania (all with polity scores of 8) achieve a polity score similar to the US, the UK or France in the year 2000. There is no way to tell using our methodology if this effect would be similar if there was an actual policy change such as the ending of inward looking trade policies or the signing of trade agreements to lower tariffs. Our reduced form model suggests on average that it would be, but it is difficult to imagine such an experiment without other factors changing concurrently.

Interestingly there is evidence that the strength of the relationship is fairly constant over the long run, and changes in openness also seem to have a long-run impact on the level of the polity score rather than an immediate short-run effect. Further evidence has shown that there may be variation by region in the impact. Unfortunately, we cannot totally rule out that a set of as yet un-identified fixed or slowly changing country-level variables is responsible for the move to democratization.

Since our definition of democracy is based on the Polity IV data set it may not capture all aspects of a democratic system. For the cautious reader, our results might be interpreted as saying that openness raises competition in the recruitment of the executive, more open participation in choosing the executive and more checks and balances against an executive. It would be interesting to find out through which channels openness is affecting the polity score.

Given the components that the polity score measures, this study may shed light on the kind

of theoretical model one would need to explain this process. The results may be in line with a story that argues that openness and more fierce competition in goods markets creates similar pressures in the political system. This is surely a promising avenue for further research both from a theoretical and an historical perspective.

Overall, the long-run patterns suggest a similar story for the period between 1870 and 1913, again between 1920 and 1939 and finally between 1960 and 2000. We believe there is adequate information available to argue that international trade or, at the very least, fundamental factors that drive openness to trade can help increase the process of building and consolidating democracy.

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Notes

¹See Dani Rodrik (1997, 2000) for an overview. On economic growth, see John F. Helliwell (1992), Robert J. Barro (1996), Rodrik (1999) and Peter H. Lindert (2003). On income and asset distribution, see Daron Acemoglu and James A. Robinson (2000) and Lindert (2003).

²Quotes from *Between Hope and History*, by Bill Clinton, p. 36, Jan 1, 1996, as cited in http://www.issues2000.org/Celeb/Bill_Clinton_Free_Trade.htm (downloaded on 27 November 2004).

³Quotation taken from Office of the Press Secretary, The White House, “Remarks by the President to the Council of the Americas” (May 7, 2001). Downloaded on 8 May 2001 from <http://www.whitehouse.gov/news>.

⁴There is also a perception among certain quarters that globalization in general, and not just the expansion of international trade, is good for democracy. For example, some people have argued that the presence of transnational corporations in developing countries at present-times instills democratic values in the population of the host countries. Consider the following passage taken from *Civilization*, the magazine of the U.S. Library of Congress:

“In developing countries, most of them with strong authoritarian traditions and rigid hierarchical structures, American companies often provide workers, at least those who are reasonably educated and somewhat skilled, their first experience of civil society—their first chance to cultivate the skills needed to make most of democracy, free markets, and the rule of law. Companies do so not because they can help it. They build the skills of civil society because of the way they work.” [Bernard Avishai, “America’s invisible export,” *Civilization*, April-May 2000, p. 84.]

⁵For example, Thomas Carothers, of the Carnegie Endowment for International Peace, a Washington, DC-based think tank, wrote at the time that “[b]ringing Mexico into the North American Free Trade Agreement created a broader context of integration that reinforced pressure on the Mexican political elite to move ahead with liberalization”. (*The Washington Post*, 29 January 2001). Similar remarks can be found in a US Trade Representative’s report on NAFTA [USTR (1999, chapter VII)] stating that:

In the decade since the end of the Cold War, the United States has continued to lead in development of the world’s trade agenda, and to lead in advancing the democratic values embodied in free markets, consumer choice, respect for basic rights of workers and the environment, and transparency of process. The NAFTA exemplifies this leadership.

⁶Indeed, the statements by Presidents Clinton and Bush that we cite above were made in the context of US-China trade relations.

⁷*Declaration of Quebec City*, 22 April 2001. Venezuela reserved its position regarding the Declaration's aim at strengthening representative democracy in the Americas.

⁸Downloaded from the European Commission's enlargement website on 27 November 2004 (<http://europa.eu.int/comm/enlargement/intro/criteria.htm>).

⁹For example, Article 1 of the *Economic Partnership, Political Coordination and Cooperation Agreement* between Mexico and the European Union states that the "[r]espect for democratic principles and fundamental human rights, proclaimed by the Universal Declaration of Human Rights, underpins the domestic and external policies of both Parties and constitutes an essential element of this Agreement."

¹⁰For a reply to this view, see Jeffrey D. Sachs (2003), who argues in favor of the role of geography in explaining income per capita. Edward L. Glaeser, Rafael La Porta, Florencio Lopez-de-Silanes and Andrei Schleifer (2004) also downplay the importance of institutions in explaining growth.

¹¹See Pinelopi Koujianou Goldberg and Nina Pavcnik (2004) for a recent review of the literature.

¹²See Li and Reuveny (2003), Table 2.

¹³See for example O'Rourke and Williamson (1999), Estevadeordal, Frantz, and Taylor (2003), López-Córdova and Meissner (2003) and David S. Jacks (2004).

¹⁴For example, it is compelling to think that the level of democracy is positively related to schooling levels. However, schooling itself is likely to be influenced by the political regime.

¹⁵An important point that Frankel and Romer made is that controls for size were necessary. Smaller countries are naturally more proximate to other countries and also engage in more international and less intra-national trade. If trade and exchange in general (i.e., the overall size of the economy), rather than international trade affected the amount of democracy, our instrument could be negatively correlated with the error term if we omit such controls and therefore our point estimate could possibly be biased downward. Therefore, key controls may be population and land area.

¹⁶We make land area and distance a time-varying variable. In principle, if the area of a country changes, then the geographic center of a country may also change. It turns out that we do not have too many observations with full data that change bilateral distance or area within our samples.

¹⁷We assume a homoscedastic error process for equation (2). If we do not then \hat{T}_{it} will be multiplied by a pair-specific residual, and this would affect our parameter of interest. Assuming homoscedasticity makes it so that a constant pre-multiplies all of the instruments and hence will not affect our instrumental variables regressions.

¹⁸The latter is available on line at <http://faculty.haas.berkeley.edu/arose/RecRes.htm>.

¹⁹Treier and Jackman (2003) provide a recent analysis of some important issues and provide measures of the latent value of democracy. These variables could be of interest as a dependent

variable in further study.

²⁰We are aware that this comparative static is only an approximation. One problem with this is that categorical coding of the dependent variable makes it generally technically incorrect to suppose that incremental changes are uniform across the interval. Ordered probit analysis we undertake below helps us to be more precise about this.

²¹These results are available upon request.

²²One possibility is that the nature of the process of democratization has changed over time. This is plausible. Huntington's "third wave" does not really begin until the late 1970s. It could also be the case that if these factors are nearly time invariant or highly persistent (as most are) they end up being naturally correlated with other unobservables.

²³We have in fact carried out these specifications every five years between 1870 and 1910 (the only years for which we have data) and for all years between 1920 and 1939 and 1960 and 2000. Reporting only benchmark years does not change the qualitative results reported below.

²⁴Our results differ from Bussman (2001) because she uses different instrumental variables than we do. Her results appear to rely mainly on population growth rates to instrument for openness. Contrary to Li and Reuveny (2003) we find a positive coefficient. This is possibly because of their use of fixed effects with a lagged dependent variable which is a mis-specification given the structure of the data. They also include other variables likely to be endogenous but do not correct for this problem either.

²⁵Singapore is an extreme outlier for the 1960-2000 sample. In each year it massively influences the results compared to its relative importance on the global scene. Taking Singapore out changes our results drastically: in the specification of column (3) the coefficient is estimated at 0.21 and has a standard error of 0.01 while the other coefficients are qualitatively similar.

²⁶We do not run a panel "fixed effects" regression with the lagged value of polity as in Li and Reuveny (2003). The presence in equation (1) of fixed effects indicators coupled with a lagged dependent variable would present an econometric problem even if there were no autocorrelation in the error term. To see why, one uses the notion that the fixed effects panel estimator can be written as a model where all the regressors are in terms of deviations from individual mean values. One immediately sees that the error term in this specification is a function of all periods' error terms, and because of this, the lagged dependent variable is correlated with this error term.

²⁷In a regression of our instrument on the fixed factors included in Table 7 column (2) and year indicators, the R-Squared is roughly 0.6. This is another way to say that the instrument is fairly fixed over time and the root of our multicollinearity problems.

²⁸We also included the average polity score of each country's Politically Relevant International Environment (PRIE) and output per capita. We instrumented the latter with distance from the equator. We define the PRIE as the US, the UK, France, and the USSR (or Russia after 1990). We include China if the country is in Asia. In the PRIE score we also include the polity score of all countries that are less than 601 miles from the home country. In the year-by-year specifications with this variable we ran, we found that openness is positively related and statistically significant at better than the 90 percent level up to 1980. Output per capita is

always positive and significant as is the PRIE polity score. In a panel specification, openness is negative and significant or negative and insignificant depending on the particular specification. Still, the lack of convincing instruments for output per person and the econometric identification problem associated with neighborhood effects as outlined in William A. Brock and Steven N. Durlauf (2003) persuaded us not to report these specifications. While we are aware that the non-linear ordered probit model may identify the regional effects, we are cautious because of the remaining “selection” issues and because the instrument for openness does not seem related to the regional average choices.

²⁹Prior to 1990 the countries for which we have data in the Eastern Europe region are Poland, Hungary, Romania and Turkey.

³⁰If we leave out Singapore, East Asia also has a large, statistically significant and positive relationship.

³¹We also allowed for different slopes by region both for openness and lagged polity. The results are qualitatively similar to those of Table 7 where we allow for regional variation in the coefficient on openness.

³²When we control for whether a country is a primary commodity exporter or a petroleum exporter by interacting these variables with openness in the baseline two-stage least squares regression, results show that these types of countries have significantly lower polity scores.

³³Below we present ordered probit results for selected years. This type of model, strictly speaking, may be more appropriate for the structure of our dependent variable. We are also comfortable using continuous dependent variable techniques because, as the number of categories becomes large, ordered probit and linear models produce very similar results and because the predictions we get from our model are very nearly within the bounds of the actual outcomes.

³⁴Again, Singapore is an extreme outlier. In each year it overly influences the results compared to its relative importance on the global scene. Taking Singapore out changes our results slightly up to 1980 and significantly after 1980. When we take it out, the coefficient on openness is highly statistically significant up to 2000 and does not show a secular decline.

³⁵We also ran instrumental variables regressions with a quadratic term for openness including Singapore adding the square of predicted openness to our instrument list. Results are qualitatively very similar to (un-reported) results which left Singapore out. The total partial effect (evaluated at the mean of openness) ranges from a high of 0.50 in 1970 to a low of 0.14 in 2000. Nevertheless, the sum of the coefficients in the polynomial are statistically significant only from 1970 onwards which suggests a more linear relationship prior to the 1970s when global trade had yet to fully take off.

³⁶As mentioned above, we used the binary data from Alvarez et. al. in an instrumental variables probit model including simply openness but also controls for country size and GDP per capita. Their data is available from 1960 to 1990. The results show a highly significant and positive relationship between democracy and openness. It would appear that the general positive correlation is not too sensitive to the way in which we code the democracy variable.

³⁷Essentially we allow each variable in the gravity equation to increase by 0.001 and generate

a new value of predicted trade. We then redo the instrumental variables regression to see how the standard error of our coefficient changes. See Frankel and Romer (1999) for a precise explanation.

³⁸The countries in our 1910 sample are Great Britain, France, USA, Belgium, Switzerland, Italy, Germany, Denmark, Norway, Sweden, Austria, Russia, Spain, Portugal, Greece, Australia, Canada, Japan, Brazil, Mexico, Chile, Argentina and New Zealand.

³⁹There are also a few outliers in this period with openness greater than 110 percent. Excluding the Netherlands from 1880, Switzerland in 1880 and Chile from 1880 to 1890 changes the simple linear model results drastically. The coefficients on openness are larger and more significant after 1890. However, including a quadratic for openness yields OLS results very similar to those found excluding these outliers.

Econometrics Appendix

Our random effects models assume that one part of the error term is idiosyncratic to the country, and, most importantly, that the country error is uncorrelated with the instrument, and other included or excluded regressors. Our fixed effects specifications allow us to relax this strong assumption and model any unobserved heterogeneity with a country specific intercept. The other benefit of this type of model is that it uses the variance over time in the data *within* a country to estimate the average relation between changes in openness to changes in democracy. The drawback is that if the regressors are uncorrelated with the country specific error component the estimates are inefficient and would tend to over-reject the null hypothesis of no correlation.

The partial adjustment model pools the data but allows for the influence of past outcomes by including the lagged value of the polity score. One natural interpretation of the partial adjustment model is that a country has a conditional target or optimal level of democracy, but that it is costly or difficult to adjust towards this level. The model hence allows for estimation of a short run-impact and a long-run impact on the level of democracy.⁴⁰ For all of these specifications we make sure to use heteroscedasticity and autocorrelation robust standard errors.

We also present a generalized method of moments (GMM) estimation technique for dynamic panels with fixed effects made operational by Arellano and Bond (1991). This type of technique is precisely for panel data models that have the structure of equation (1) but which might include lagged values of democracy to control for the importance of historical regime choice. Note that if previous levels of democracy matter, a fixed effects panel regression is not valid in this case because of the correlation between the error term and the lagged values induced by the within transformation.

To obtain unbiased estimates of the coefficients, one first writes equation (1) in first differences as follows

$$(4) \quad \Delta Democracy_{it} = Democracy_{it} - Democracy_{it-1} = \tilde{\beta}_0 + \tilde{\delta}(\Delta Openness_{it}) + \sum_{k=1}^T \tilde{\gamma}_k (\Delta Democracy_{it-k}) + \Delta u_{it}$$

where the “ Δ ” is the difference operator, and we use the “ $\tilde{\cdot}$ ” to distinguish the coefficients estimated in this model from those of equation (1). Notice that the variables that are constant over time (e.g., the country fixed effects) fall out of the equation, and so we control for these factors by taking the first differences. The lagged dependent variable terms are correlated with the new error term since the lagged error term appears in the differenced error term. To get around this, one uses plausible instruments for the lagged values of the dependent variable. Assuming we restrict the lag structure to include only the first-lagged difference of the democracy measure, then any previous lag from period $t - 2$ and before would be viable. These values are highly correlated with lagged democracy but are uncorrelated with the error term.⁴¹

The GMM estimates gain efficiency by using a large number of “moment” restrictions and solve for the parameters on the basis of those restrictions. In particular the moment restrictions here are that the expectations of the cross products of the lagged values used as instruments and the error terms are zero. We use up to the fifth lag of the dependent variable as an instrument, the level and first difference of predicted openness and the second lag of actual openness in the instrument set. Our justification for including the second lag of actual openness is that this variable can be considered predetermined and hence not correlated with the error term.⁴² We use *Stata* and also reproduced our results in *Ox*. The details on estimation methods are given in the *Stata User's Manual*.

Anderson and Hsiao (1982) performed a similar regression using only the second lag as an instrument for the lagged difference. Arellano and Bond (1991) pointed out that using many more lags as instruments could increase the efficiency of the estimation. We present Anderson Hsiao type estimates for comparison.

Data Appendix

Democracy

1870-2000: We use the polity measure from the Polity IV data set. We classify this variable as missing if there is an interruption, interregnum, or transition. See the discussion in the text and the Polity IV manual for insight into how this variable is constructed.

Openness

1870-1913: Exports and imports were taken from Barbieri (1996), López Córdova and Meissner (2003) and Mitchell (1992, 1993 and 1995). GDP come from the data set underlying López Córdova and Meissner (2003). Some additional nominal GDP and total trade values were taken from the data set used in Obstfeld and Taylor (2003). Nominal imports and exports were divided by the nominal value of GDP to arrive at an openness measure.

1917-1940: Exports and imports in dollars were taken from Barbieri (1996) and Mitchell (op. cit.). Nominal GDP data come from Eichengreen and Irwin (1995), Mitchell (op. cit.), Obstfeld and Taylor (2003), and Ritschl and Wolf (2003). Real PPP-adjusted GDP is available from Maddison (2001). For real-PPP adjusted openness we inflated current values of exports and imports to 1990 values using the US CPI from www.freelunch.com. We then divided these values by Maddison's GDP values.

1960-2000: Total trade in dollars comes from Rose (2003) supplemented by data from the IMF *International Financial Statistics* and the World Bank's *World Development Indicators*. We reflatd trade using a US CPI available from www.freelunch.com. We divided these real values of trade by GDP values from the *Penn World Tables* supplemented by the *World Development Indicators*. To make the PPP adjustment, we used the GDP price level factor from the *World Development Indicators* to account for local deviations of the price levels.

Gravity Regression Components

Bilateral Trade

1870-1913: Bilateral trade comes from López Córdova and Meissner (2003). We divided current dollar value bilateral trade by nominal current dollar values of GDP. Using PPP-adjustments made the regressions of interest infeasible.

1917-1940: Bilateral trade comes from Barbieri (1996) supplemented by Ritschl and Wolf (2003). We converted trade to real 1990 values using a CPI for the US and divided these values by Maddison's PPP-adjusted GDP.

1960-2000: Bilateral trade comes from Rose (2003) supplemented by the IMF's *Direction of Trade Statistics*. We use the average of exports and imports as reported by both countries in a country pair rather than the average of all four possible values as in Rose (2003). We converted trade to real values as described above and divided by PPP-Adjusted GDP.

GDP

See previous notes on openness.

Population

1870-1913: Data come from López Córdova and Meissner (2003).

1917-1940: Populations come from Mitchell (op. cit.) supplemented by data from Eichengreen and Irwin (1995).

1960-2000: Population comes from the World Bank's *World Development Indicators*.

Land Area This is measured as the logarithm of square kilometers.

1870-1913: López Córdova and Meissner (2003)

1917-1940: Stinnett, Tir, Schafer, Diehl, and Gochman (2002)

1960-2000: Rose (2003)

Bilateral Distance

1870-1913: López Córdova and Meissner (2003)

1917-1940: Eichengreen and Irwin (1995)

1960-2000: Rose (2003)

Shared Border Indicators

1870-1913: López Córdova and Meissner (2003)

1917-1940: Eichengreen and Irwin (1995)

1960-2000: Rose (2003)

Landlocked Indicators

1870-1913: López Córdova and Meissner (2003).

1917-1940: Eichengreen and Irwin (1995)

1960-2000: Rose (2003)

Island Indicator

All years come from Rose (2003)

Common Language

1870-1913 and 1917-1940: López Córdova and Meissner (2003)

1960-2000: Rose (2003)

Table 1: Gravity Equation Results, 1870-2000

<i>Regressors</i>	<i>Year</i>									
	1960	1965	1970	1975	1980	1985	1990	1995	2000	
distance	-0.65 (0.03)	-0.72 (0.03)	-0.94 (0.03)	-1.10 (0.03)	-1.14 (0.03)	-1.24 (0.04)	-1.30 (0.04)	-1.17 (0.03)	-1.27 (0.03)	
ln (area) country i	-0.10 (0.02)	-0.07 (0.01)	-0.17 (0.02)	-0.11 (0.02)	-0.14 (0.02)	-0.15 (0.02)	-0.13 (0.02)	-0.14 (0.02)	-0.07 (0.02)	
ln (area) country j	-0.12 (0.02)	-0.12 (0.02)	-0.19 (0.02)	-0.12 (0.02)	-0.14 (0.02)	-0.21 (0.02)	-0.23 (0.02)	-0.25 (0.02)	-0.12 (0.02)	
ln (population) country i	-0.34 (0.02)	-0.35 (0.02)	-0.08 (0.02)	-0.11 (0.02)	-0.08 (0.02)	-0.05 (0.02)	0.00 (0.02)	0.01 (0.02)	-0.14 (0.02)	
ln (population) country j	0.74 (0.02)	0.75 (0.02)	0.92 (0.02)	0.83 (0.02)	0.88 (0.02)	1.00 (0.02)	1.08 (0.02)	1.10 (0.02)	0.95 (0.02)	
landlocked	-0.19 (0.06)	-0.19 (0.05)	-0.43 (0.06)	-0.50 (0.06)	-0.65 (0.06)	-0.74 (0.06)	-0.94 (0.06)	-1.13 (0.05)	-0.91 (0.04)	
island	0.38 (0.05)	0.42 (0.05)	0.27 (0.06)	0.19 (0.06)	0.10 (0.05)	0.29 (0.06)	0.14 (0.06)	0.20 (0.05)	0.46 (0.05)	
border	0.15 (0.13)	0.14 (0.12)	0.42 (0.13)	-0.04 (0.14)	0.16 (0.14)	0.12 (0.14)	0.28 (0.14)	0.79 (0.13)	0.80 (0.11)	
common language	0.41 (0.06)	0.45 (0.05)	0.45 (0.06)	0.44 (0.06)	0.33 (0.06)	0.34 (0.07)	0.30 (0.07)	0.52 (0.06)	0.55 (0.05)	
constant	-6.33 (0.46)	-6.25 (0.42)	-10.11 (0.44)	-8.22 (0.43)	-8.25 (0.40)	-9.70 (0.45)	-11.34 (0.47)	-12.52 (0.39)	-9.27 (0.32)	
Number of obs	4477	5699	8040	8817	9657	10051	10842	13005	11942	
R-squared	0.44	0.43	0.35	0.33	0.34	0.31	0.33	0.36	0.46	

Notes: Dependent variable in the "Gravity Equation" is the log of total bilateral trade divided by GDP. See the text for precise definitions.

Table 2: Gravity Equation Results, 1870-1939

Regressors	Year					
	1920	1925	1928	1932	1935	1938
distance	-0.61 (0.10)	-0.34 (0.07)	-0.51 (0.08)	-0.51 (0.07)	-0.42 (0.06)	-0.48 (0.06)
ln (area) country i	0.22 (0.05)	0.09 (0.04)	-0.01 (0.04)	-0.02 (0.04)	-0.00 (0.03)	0.02 (0.03)
ln (area) country j	0.07 (0.06)	-0.06 (0.04)	-0.04 (0.05)	-0.06 (0.04)	-0.04 (0.03)	-0.02 (0.04)
ln (population) country i	-0.52 (0.07)	-0.49 (0.05)	-0.35 (0.06)	-0.35 (0.05)	-0.48 (0.04)	-0.51 (0.05)
ln (population) country j	0.49 (0.08)	0.54 (0.06)	0.66 (0.06)	0.65 (0.05)	0.51 (0.04)	0.52 (0.05)
landlocked	-0.23 (0.19)	-0.27 (0.12)	-0.22 (0.14)	-0.16 (0.12)	-0.18 (0.09)	-0.32 (0.12)
island	0.70 (0.16)	0.41 (0.12)	0.00 (0.13)	0.04 (0.12)	0.48 (0.09)	0.50 (0.10)
border	0.37 (0.30)	0.61 (0.19)	0.10 (0.22)	0.03 (0.19)	0.21 (0.15)	-0.11 (0.19)
common language	0.99 (0.25)	0.90 (0.18)	1.21 (0.22)	1.24 (0.20)	1.14 (0.16)	1.01 (0.18)
constant	-2.18 (1.23)	-3.77 (0.88)	-4.89 (0.99)	-5.29 (0.86)	-3.00 (0.70)	-2.67 (0.80)
Number of obs	396	488	640	716	693	698
R-squared	0.38	0.43	0.36	0.38	0.48	0.42

Notes: Dependent variable in the "Gravity Equation" is the log of total bilateral trade divided by GDP. See the text for precise definitions.

Regressors	Year								
	1870	1875	1880	1885	1890	1895	1900	1905	1910
distance	-0.46 (0.18)	-0.50 (0.14)	-1.04 (0.15)	-1.00 (0.16)	-1.02 (0.14)	-0.93 (0.12)	-0.78 (0.12)	-0.67 (0.12)	-0.71 (0.10)
ln (area) country i	0.06 (0.08)	0.01 (0.07)	0.16 (0.08)	0.14 (0.08)	0.11 (0.07)	0.09 (0.06)	0.05 (0.07)	-0.09 (0.06)	-0.02 (0.05)
ln (area) country j	0.03 (0.09)	0.05 (0.07)	0.10 (0.08)	0.07 (0.09)	0.09 (0.08)	0.12 (0.07)	-0.07 (0.07)	-0.13 (0.06)	-0.03 (0.05)
ln (population) country i	-0.43 (0.13)	-0.32 (0.10)	-0.24 (0.12)	-0.27 (0.12)	-0.33 (0.11)	-0.24 (0.09)	-0.21 (0.10)	-0.11 (0.08)	-0.17 (0.07)
ln (population) country j	0.45 (0.10)	0.47 (0.09)	0.53 (0.11)	0.48 (0.11)	0.48 (0.09)	0.53 (0.08)	0.74 (0.08)	0.77 (0.07)	0.73 (0.06)
landlocked	-0.97 (0.45)	-1.33 (0.34)	-1.41 (0.43)	-1.05 (0.42)	-0.77 (0.34)	-0.66 (0.26)	-0.65 (0.25)	-0.84 (0.26)	-0.41 (0.22)
island	1.43 (0.27)	1.25 (0.23)	0.17 (0.29)	-0.21 (0.29)	0.12 (0.25)	0.41 (0.23)	-0.18 (0.24)	0.07 (0.19)	0.40 (0.16)
border	1.29 (0.40)	1.91 (0.32)	0.73 (0.42)	0.45 (0.42)	0.48 (0.37)	0.59 (0.33)	0.68 (0.35)	0.89 (0.36)	0.53 (0.30)
common language	1.36 (0.44)	0.80 (0.32)	1.19 (0.43)	0.82 (0.44)	0.76 (0.38)	0.79 (0.33)	0.53 (0.34)	0.65 (0.32)	1.01 (0.28)
constant	-3.33 (1.13)	-3.13 (0.93)	-1.64 (1.17)	-0.87 (1.21)	-0.57 (1.07)	-1.93 (0.91)	-0.80 (0.95)	0.36 (0.89)	-1.05 (0.78)
Number of obs	173	245	290	293	330	386	416	481	451
R-squared	0.50	0.52	0.37	0.34	0.40	0.42	0.41	0.39	0.49

Notes: Dependent variable in the "Gravity Equation" is the log of total bilateral trade divided by GDP. See the text for precise definitions.

Table 3: First Stage Regression Results, 1870-2000

Regressors	Year								
	1960	1965	1970	1975	1980	1985	1990	1995	2000
Predicted Openness	2.28 (0.43)	2.35 (0.46)	3.73 (0.76)	4.6 (1.20)	5.06 (1.52)	3.88 (1.24)	3.93 (1.51)	3.49 (1.54)	3.51 (0.79)
constant	4.56 (1.84)	5.06 (1.94)	3.7 (2.70)	2.18 (5.91)	-0.29 (9.19)	3.1 (5.25)	4.3 (7.18)	6.58 (8.99)	0.9 (5.29)
Number of obs	76	90	97	103	104	105	105	117	115
R-squared	0.58	0.48	0.47	0.4	0.44	0.42	0.29	0.26	0.4

NOTES: Dependent variable is the real value of total exports and imports divided by PPP-adjusted GDP. Predicted openness comes from the predicted values of the gravity equation presented in Table 1. Heteroscedasticity consistent errors are reported in parentheses.

Regressors	Year					
	1920	1925	1928	1932	1935	1938
Predicted Openness	0.83 (0.22)	1.01 (0.12)	0.84 (0.28)	1.08 (0.25)	1.06 (0.12)	0.95 (0.25)
constant	15.20 (4.88)	7.92 (2.65)	15.06 (3.46)	5.77 (1.92)	5.25 (1.60)	8.10 (2.51)
Number of obs	22	26	27	29	29	29
R-squared	0.37	0.61	0.22	0.36	0.66	0.57

NOTES: Dependent variable is real value of total exports and imports divided by PPP-adjusted GDP. Predicted openness comes from the predicted values of the gravity equation presented in Table 2. Heteroscedasticity consistent errors are reported in parentheses.

Regressors	Year								
	1870	1875	1880	1885	1890	1895	1900	1905	1910
Predicted Openness	0.95 (0.39)	0.98 (0.37)	0.86 (0.23)	1.16 (0.34)	1.15 (0.30)	1.52 (0.29)	1.36 (0.29)	1.16 (0.25)	1.18 (0.28)
constant	13.40 (8.42)	15.35 (10.12)	20.11 (7.76)	17.15 (7.71)	17.41 (8.25)	11.78 (7.01)	12.93 (5.62)	17.62 (5.31)	9.77 (6.49)
Number of obs	18	19	17	19	19	21	21	22	23
R-squared	0.34	0.32	0.24	0.20	0.28	0.35	0.47	0.48	0.62

NOTES: Dependent variable is total exports and imports divided by GDP. Predicted openness comes from the predicted values of the gravity equation presented in Table 1. Heteroscedasticity consistent errors are reported in parentheses.

Table 4: First Stage Regression Results for Pooled Data, 1870-2000

Variable	Random Effects	Partial Adjustment	Random Effects Omitted Variables	Fixed Effects
	(1)	(2)	(3)	(4)
Predicted Openness	3.65 (0.10)	3.53 (0.11)	3.57 (0.13)	1.68 (0.13)
ln(population)	---	---	-3.68 (0.35)	---
ln (land area)	---	---	1.01 (0.36)	---
Polity <i>t-1</i>	---	0.34 (0.06)	---	---
Constant	-2.26 (3.08)	-2.29 (3.04)	43.45 (5.55)	-6.4 (13.28)
Number of obs	4184	4084	4184	4184
R-Squared	0.40	0.41	0.43	0.83

Notes: Sample is 1960 to 2000. Dependent variable in all columns is real openness. Time dummies are not reported. Auto-correlation and heteroscedasticity consistent standard errors are reported in parentheses.

Variable	Random Effects	Partial Adjustment	Random Effects Omitted Variables	Fixed Effects
	(1)	(2)	(3)	(4)
Predicted Openness	0.88 [0.06]	0.68 [0.06]	0.58 [0.08]	0.23 [0.04]
ln(population)	---	---	0.54 [0.30]	---
ln (land area)	---	---	-3.73 [0.61]	---
Polity <i>t-1</i>	---	0.66 [0.07]	---	---
Constant	13.97 [2.69]	15.53 [2.41]	74.56 [13.31]	14.35 [1.45]
Number of obs	522	518	522	522
R-Squared	0.50	0.62	0.57	0.9

Notes: Sample is 1920 to 1940. Dependent variable in all columns is real openness. Time dummies are not reported. Auto-correlation and heteroscedasticity consistent standard errors are reported in parentheses.

Variable	Random Effects	Partial Adjustment	Random Effects Omitted Variables	Fixed Effects
	(1)	(2)	(3)	(4)
Predicted Openness	1.76 (0.24)	1.35 (0.36)	1.9 (0.28)	0.35 (0.30)
ln(population)	---	---	-2.32 (3.23)	---
ln (land area)	---	---	-6.26 (4.99)	---
Polity <i>t-1</i>	---	0.001 (0.56)	---	---
Constant	-8.91 (10.07)	138.89 (94.63)	-8.97 (12.56)	32.58 (10.59)
Number of obs	190	170	190	190
R-Squared	0.31	0.32	0.33	0.79

Notes: Sample is 1870 to 1910 with country year observations every five years. Dependent variable in all columns is real openness. Time dummies are not reported. Auto-correlation and heteroscedasticity consistent standard errors are reported in parentheses.

Table 5: Openness and Democracy: Panel Estimates for Three Periods

Variable	<i>Post World War II Sample: 1960-2000</i>											
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) 2SLS	(8) OLS	(9) 2SLS	(10) 2SLS	(11) GMM	(12) 2SLS
Openness	0.08 (0.01)	0.13 (0.02)	0.13 (0.02)	0.13 (0.02)	-0.02 (0.006)	0.02 (0.03)	0.02 (0.03)	0.0003 (0.001)	0.004 (0.002)	0.005 (0.003)	-0.005 (0.006)	-0.14 (0.10)
ln(population)	---	---	1.07 (0.16)	1.27 (0.16)	---	---	0.24 (1.12)	---	---	0.06 (0.03)	---	---
ln (land area)	---	---	-0.32 (0.15)	0.16 (0.14)	---	---	---	---	---	0.02 (0.02)	---	---
landlocked	---	---	---	-0.35 (0.41)	---	---	---	---	---	0.006 (0.09)	---	---
number of borders	---	---	---	-0.95 (0.07)	---	---	---	---	---	-0.05 (0.02)	---	---
countries with same language	---	---	---	-0.02 (0.009)	---	---	---	---	---	-0.003 (0.001)	---	---
Polity <i>t</i> -1	---	---	---	---	---	---	0.96 (0.003)	0.96 (0.004)	0.96 (0.004)	0.95 (0.004)	0.94 (0.02)	0.61 (0.11)
Constant	-0.07 (0.84)	-0.93 (0.85)	-13.85 (2.31)	-19.7 (2.4)	-11.97 (0.58)	-3.38 (5.01)	-12.58 (22.78)	-0.11 (0.22)	-0.03 (0.10)	-1.03 (0.55)	0.13 (0.12)	-0.55 (0.48)
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Dummies	No	No	No	No	Yes	Yes	Yes	No	No	No	---	---
Number of obs	4184	4184	4184	4184	4170	4170	4170	4084	4084	4084	3941	3622

Notes: Dependent variable in columns (1)-(9) is the Polity score. The dependent variable in columns (10) and (11) is the first difference of the Polity score. Auto-correlation and heteroscedasticity consistent standard errors are reported in parentheses in columns (1)-(9). Heteroscedasticity consistent standard errors are reported in parentheses in columns (10) and (11). Time dummies are not reported.

Variable	<i>Interwar Sample: 1920-1940</i>											
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) 2SLS	(8) OLS	(9) 2SLS	(10) 2SLS	(11) GMM	(12) 2SLS
Openness	0.38 (0.03)	0.40 (0.04)	0.58 (0.09)	0.57 (0.09)	0.01 (0.02)	-0.12 (0.08)	-0.15 (0.09)	0.02 (0.01)	0.02 (0.01)	0.05 (0.03)	0.03 (0.02)	0.48 (1.44)
ln(population)	---	---	1.96 (0.78)	2.00 (0.67)	---	---	-6.17 (6.04)	---	---	0.24 (0.16)	---	---
ln (land area)	---	---	-0.20 (0.2)	-0.08 (0.21)	---	---	---	---	---	-0.001 (0.04)	---	---
landlocked	---	---	---	3.32 (0.93)	---	---	---	---	---	0.46 (0.29)	---	---
number of borders	---	---	---	-0.11 (0.25)	---	---	---	---	---	-0.06 (0.07)	---	---
countries with same language	---	---	---	0.06 (0.1)	---	---	---	---	---	0.02 (0.03)	---	---
Polity <i>t</i> -1	---	---	---	---	---	---	0.98 (0.01)	0.97 (0.02)	0.97 (0.02)	0.95 (0.02)	0.77 (0.08)	0.02 (1.17)
Constant	-5.46 (1.42)	-6.25 (1.82)	-43.09 (14.82)	-45.46 (13.80)	10.08 (0.47)	13.24 (1.55)	128.05 (112.1)	0.16 (0.45)	0.09 (.41)	-4.86 (3.55)	-0.03 (0.03)	5.35 (15.8)
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Dummies	No	No	No	No	Yes	Yes	Yes	No	No	No	---	---
Number of obs	522	522	522	522	522	518	522	518	518	518	481	466

Notes: Dependent variable in columns (1)-(9) is the Polity score. The dependent variable in columns (10) and (11) is the first difference of the Polity score. Auto-correlation and heteroscedasticity consistent standard errors are reported in parentheses in columns (1)-(9). Heteroscedasticity consistent standard errors are reported in parentheses in columns (10) and (11). Time dummies are not reported.

Table 6: Openness and Democracy: Panel Estimates for Three Periods (cont.)

Variable	Pre-World War I Sample: 1870-1910											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS	GMM	2SLS
Openness	0.02 (0.01)	0.07 (0.03)	0.10 (0.06)	0.16 (0.07)	0.0005 (0.01)	-0.14 (0.15)	-0.17 (0.16)	-0.002 (0.003)	0.006 (0.006)	0.00 (0.007)	0.02 (0.02)	0.13 (0.08)
ln(population)	---	---	0.48 (1.22)	3.1 (1.22)	---	---	-4.15 (6.29)	---	---	-0.03 (0.22)	---	---
ln (land area)	---	---	0.44 (0.77)	0.81 (0.77)	---	---	---	---	---	-0.11 (0.13)	---	---
landlocked	---	---	---	3.45 (2.57)	---	---	---	---	---	-0.53 (0.27)	---	---
number of borders	---	---	---	-1.53 (0.3)	---	---	---	---	---	-0.02 (0.07)	---	---
countries with same language	---	---	---	0.07 (0.23)	---	---	---	---	---	-0.01 (0.03)	---	---
Polity $t-5$	---	---	---	---	---	---	---	0.96 (0.02)	0.95 (0.02)	0.96 (0.02)	0.66 (0.06)	0.62 (0.4)
Constant	0.37 (0.89)	-3.12 (1.79)	-18.49 (27.77)	-64.9 (28.68)	3.96 (0.86)	9.27 (5.84)	81.99 (106.92)	0.67 (0.19)	0.29 (0.30)	2.54 (4.11)	0.2 (0.1)	0.29 (0.26)
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Dummies	No	No	No	No	Yes	Yes	Yes	No	No	No	---	---
Number of Obs	190	190	190	190	190	190	190	170	170	170	147	108

Notes: Dependent variable in columns (1)-(9) is the Polity score. The dependent variable in columns (10) and (11) is the first difference of the Polity score which for this sample is recorded each five years. Auto-correlation and heteroscedasticity consistent standard errors are reported in parentheses in columns (1)-(9). Heteroscedasticity consistent standard errors are reported in parentheses in columns (10) and (11). Time dummies are not reported. For this Pre-World War I sample, the observations are for country years every five years beginning in 1870.

Table 7: Instrumental Variables Estimations with Omitted Variables and Regional Coefficients, 1960-2000

Variable			Variable	Regional Variation in Coefficients			
	(1)	(2)		(3)	(4)	(5)	(6)
Openness	0.06 (0.01)	-0.01 (0.01)	East Asia Pacific x Openness	0.04 (0.01)	0.01 (0.01)	0.001 (0.002)	0.0006 (0.002)
ln (population)	---	-0.19 (0.13)	Eastern Europe/CIS x Openness	0.13 (0.03)	0.07 (0.04)	0.01 (0.01)	0.01 (0.01)
ln (land area)	---	-0.75 (0.12)	Middle East/North Africa x Openness	-0.13 (0.02)	-0.17 (0.03)	-0.01 (0.01)	-0.01 (0.01)
Distance from Equator	0.15 (0.02)	0.18 (0.02)	South Asia x Openness	0.4 (0.10)	0.2 (0.11)	-0.004 (0.02)	-0.01 (0.03)
Ethnolinguistic Fragmentation	3.76 (0.58)	5.5 (0.61)	Western Europe x Openness	0.19 (0.01)	0.17 (0.01)	0.01 (0.002)	0.01 (0.003)
Primary Commodity Exporter Dummy	-2.04 (0.31)	-1.81 (0.32)	Sub-Saharan Africa x Openness	-0.12 (0.02)	-0.16 (0.03)	-0.01 (0.01)	-0.01 (0.01)
Petroleum Exporter Dummy	-1.08 (0.50)	0.36 (0.53)	Latin America/Caribbean x Openness	0.14 (0.02)	0.09 (0.02)	0.01 (0.004)	0.01 (0.01)
East Asia Pacific	-3.71 (0.96)	-5.21 (0.95)	USA/Canada x Openness	0.34 (0.03)	0.35 (0.03)	0.02 (0.008)	0.02 (0.01)
Eastern Europe/CIS States	-7.99 (0.98)	-11.46 (1.06)	ln(population)	---	0.37 (0.16)	---	0.03 (0.04)
Middle East/North Africa	-12.43 (0.97)	-15.22 (1.02)	ln (land area)	---	-0.52 (0.12)	---	-0.02 (0.03)
South Asia	-3 (1.06)	-6.25 (1.14)	Polity t-1	---	---	0.94 (0.01)	0.94 (0.01)
Western Europe	-2.12 (0.93)	-4.13 (0.93)	Constant	-0.23 (0.99)	1.12 (3.21)	0.02 (0.21)	-0.14 (0.66)
Sub-Saharan Africa	-7.75 (0.99)	-11.22 (1.09)					
Latin America/Caribbean	-2.17 (0.97)	-5 (1.03)					
Constant	-0.36 (1.46)	14.12 (3.08)					
Number of obs	4184	4184	Number of obs	4184	4184	4084	4084

NOTES: Dependent variable is the Polity score. Estimations assume a "random effects" error term. Auto-correlation and heteroscedasticity consistent standard errors are reported in parentheses in columns. Year indicators are not reported. Omitted category in the regional dummies is "North America" which includes Canada and the US.

Table 8: Democracy and Openness, OLS Regressions, 1870-2000

Regressors	Year								
	1960	1965	1970	1975	1980	1985	1990	1995	2000
Openness	0.23 (0.06)	0.21 (0.05)	0.17 (0.08)	0.08 (0.04)	0.05 (0.04)	0.08 (0.06)	0.07 (0.05)	0.04 (0.03)	0.05 (0.03)
constant	-2.5 (1.28)	-3.56 (1.21)	-3.49 (1.45)	-2.99 (1.21)	-2.13 (1.35)	-1.56 (1.31)	0.4 (1.31)	3.2 (1.08)	3.37 (1.00)
Number of obs	76	90	97	103	104	105	105	117	115
R-squared	0.15	0.13	0.11	0.05	0.05	0.05	0.06	0.04	0.08

NOTES: Dependent variable is the Polity score as described in text. GDP in the openness variable is adjusted for purchasing power parity. Heteroscedasticity consistent errors are reported in parentheses.

Regressors	Year					
	1920	1925	1928	1932	1935	1938
Openness	0.14 (0.04)	0.36 (0.08)	0.39 (0.08)	0.58 (0.16)	0.51 (0.11)	0.46 (0.09)
constant	2.24 (1.81)	-4.37 (2.52)	-5.41 (2.58)	-3.88 (2.77)	-6.89 (2.89)	-6.28 (2.67)
Number of obs	22	26	27	29	29	29
R-squared	0.37	0.5	0.51	0.31	0.35	0.39

NOTES: Dependent variable is the Polity score as described in text. GDP in the openness variable is adjusted for purchasing power parity. Heteroscedasticity consistent errors are reported in parentheses.

Regressors	Year								
	1870	1875	1880	1885	1890	1895	1900	1905	1910
Openness	0.11 (0.06)	0.07 (0.07)	0.08 (0.07)	0.09 (0.05)	0.08 (0.05)	0.12 (0.06)	0.15 (0.06)	0.12 (0.05)	0.10 (0.06)
constant	-5.11 (2.75)	-3.03 (3.11)	-2.77 (3.28)	-2.59 (2.81)	-1.77 (2.74)	-3.36 (2.96)	-3.82 (3.10)	-1.74 (2.90)	-0.06 (2.84)
Number of obs	18	19	17	19	19	21	21	22	23
R-squared	0.19	0.08	0.07	0.13	0.12	0.17	0.23	0.16	0.16

NOTES: Dependent variable is the Polity score as described in text. GDP in the openness variable is adjusted for purchasing power parity. Heteroscedasticity consistent errors are reported in parentheses.

Table 9: Democracy and Openness, Instrumental Variables Regressions, 1870-2000

Regressors	Year								
	1960	1965	1970	1975	1980	1985	1990	1995	2000
Openness	0.28 (0.08)	0.24 (0.08)	0.25 (0.14)	0.18 (0.09)	0.11 (0.06)	0.15 (0.10)	0.13 (0.08)	0.06 (0.06)	0.04 (0.03)
constant	-3.25 (1.39)	-3.97 (1.55)	-4.87 (2.49)	-5.9 (2.32)	-4.05 (1.99)	-3.01 (2.17)	-1.44 (2.22)	2.45 (1.68)	3.61 (1.03)
Number of obs	76	90	97	103	104	105	105	117	115
R-squared	0.14	0.13	0.09		0	0.02		0.02	0.08

NOTES: Dependent variable is the polity score as described in text. Instrumental variable for openness is the value of predicted openness as described in the text. Heteroscedasticity consistent errors are reported in parentheses.

Regressors	Year					
	1920	1925	1928	1932	1935	1938
Openness	0.23 (0.07)	0.36 (0.11)	0.53 (0.17)	0.79 (0.23)	0.51 (0.14)	0.43 (0.10)
constant	-0.54 (2.71)	-4.21 (3.13)	-8.99 (4.72)	-6.61 (3.99)	-6.85 (3.30)	-5.85 (2.84)
Number of obs	22	26	27	29	29	29
R-squared	0.23	0.50	0.45	0.27	0.35	0.39

NOTES: Dependent variable is the polity score as described in text. Instrumental variable for openness is the value of predicted openness as described in the text. Heteroscedasticity consistent errors are reported in parentheses.

Regressors	Year								
	1870	1875	1880	1885	1890	1895	1900	1905	1910
Openness	0.22 (0.11)	0.15 (0.13)	0.17 (0.13)	0.12 (0.12)	0.12 (0.10)	0.18 (0.10)	0.16 (0.09)	0.11 (0.07)	0.14 (0.07)
constant	-9.7 (4.52)	-6.2 (5.56)	-5.93 (5.97)	-3.9 (5.64)	-3.07 (4.83)	-5.81 (4.58)	-4.19 (4.26)	-1.67 (3.71)	-1.39 (3.23)
Number of obs	18	19	17	19	19	21	21	22	23
R-squared	---	---	---	0.11	0.11	0.12	0.23	0.16	0.14

NOTES: Dependent variable is the polity score as described in text. Instrumental variable for openness is the value of predicted openness as described in the text. Heteroscedasticity consistent errors are reported in parentheses.

Table 10: Democracy and Openness, Instrumental Variables Regressions Controlling for Country Size, 1870-2000

Variable	Year								
	1960	1965	1970	1975	1980	1985	1990	1995	2000
Openness	0.52 (0.11)	0.43 (0.11)	0.23 (0.16)	0.23 (0.10)	0.12 (0.05)	0.15 (0.09)	0.09 (0.06)	0.07 (0.05)	0.06 (0.03)
Ln (population)	2.16 (0.70)	2.91 (0.68)	1.4 (0.84)	1.51 (0.91)	1.08 (0.77)	0.9 (0.76)	1.15 (0.70)	0.2 (0.60)	0.3 (0.60)
ln (land area)	0.54 (0.82)	-0.91 (0.49)	-0.64 (0.61)	0.01 (0.85)	-0.21 (0.79)	-0.16 (0.70)	-0.64 (0.73)	0.08 (0.54)	0.1 (0.54)
Constant	-48.07 (13.13)	-41.94 (11.84)	-18.96 (14.35)	-31.51 (13.46)	-19.07 (9.51)	-15.71 (9.89)	-11.06 (8.91)	-1.82 (7.90)	-2.98 (7.31)
Number of obs	76	90	97	103	104	105	105	117	115
R-squared	0.18	0.21	0.15	---	0.02	0.04	0.08	0.02	0.09

NOTES: Dependent variable is the Polity score as described in text. Instrument for Openness is the value of predicted openness as described in the text. Heteroscedasticity consistent standard errors are in parentheses.

Regressors	Year					
	1920	1925	1928	1932	1935	1938
Openness	0.69 (0.52)	0.63 (0.19)	1.3 (0.87)	0.91 (0.33)	0.81 (0.28)	0.87 (0.20)
Ln (population)	5.72 (6.28)	2.85 (1.71)	7.74 (7.38)	1.18 (2.05)	2.71 (2.74)	3.52 (2.50)
ln (land area)	-0.62 (0.94)	-0.41 (0.58)	-1.08 (1.15)	-0.27 (0.83)	-0.14 (0.72)	0.78 (0.68)
constant	-101.15 (112.41)	-52.52 (31.83)	-14.145 (135.05)	-24.24 (37.97)	-54.97 (49.06)	-80.78 (42.62)
Number of obs	22	26	27	29	29	29
R-squared	---	0.46	---	0.23	0.35	0.4

NOTES: Dependent variable is the polity score as described in text. Instrumental variable for openness is the value of predicted openness as described in the text. Heteroscedasticity consistent errors are reported in parentheses.

Regressors	Year								
	1870	1875	1880	1885	1890	1895	1900	1905	1910
Openness	0.48 (0.26)	0.44 (0.51)	0.16 (0.35)	-0.01 (0.18)	0.07 (0.18)	0.17 (0.17)	0.05 (0.09)	0.01 (0.06)	0.07 (0.07)
Ln (population)	2.95 (2.92)	3.06 (3.83)	2.37 (6.07)	-0.68 (4.34)	0.32 (3.57)	1.85 (2.80)	-0.2 (2.64)	-1.52 (2.38)	-0.91 (2.18)
ln (land area)	2.82 (2.03)	2.72 (3.97)	0.22 (2.45)	-0.67 (1.63)	0.39 (2.30)	1.08 (2.35)	-0.28 (1.84)	-0.12 (1.49)	0.78 (1.49)
constant	-104.57 (73.72)	-103.49 (127.39)	-49.7 (139.81)	21.17 (92.17)	-12.23 (88.15)	-51.56 (69.04)	6.78 (59.64)	29.22 (48.80)	5.49 (47.31)
Number of obs	18	19	20	21	21	22	22	23	24
R-squared	---	---	0.02	---	---	---	0.04	---	---

NOTES: Dependent variable is the polity score as described in text. Instrumental variable for openness is the value of predicted openness as described in the text. Heteroscedasticity consistent errors are reported in parentheses.

Table 11: Ordered Probit "Instrumental Variables" Regressions with Additional Controls, 1960-2000, 1920-1938

Variable	Post World War II Sample: 1960-2000								
	1960	1965	1970	1975	1980	1985	1990	1995	2000
Openness	0.06	0.05	0.04	0.04	0.03	0.03	0.02	0.02	0.02
	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Openness Residuals	-0.03	-0.02	-0.02	-0.04	-0.02	-0.03	-0.01	-0.01	-0.004
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
In (Population)	0.34	0.41	0.2	0.15	0.12	0.12	0.1	-0.03	0.002
	(0.11)	(0.11)	(0.12)	(0.09)	(0.09)	(0.09)	(0.11)	(0.12)	(0.13)
In (land area)	0.05	-0.15	-0.06	0.04	0.01	0.02	-0.02	0.06	0.08
	(0.11)	(0.08)	(0.10)	(0.11)	(0.11)	(0.11)	(0.11)	(0.09)	(0.11)
Cutpoint 1	5.04	3.13	0.61	1.86	0.41	1.44	-0.11	-1.63	-0.35
Cutpoint 2	5.84	4.53	1.99	2.68	1.59	1.83	0.30	-0.86	0.00
Cutpoint 3	6.08	4.61	2.15	2.94	1.87	2.60	1.25	-0.48	0.12
Cutpoint 4	6.50	5.15	2.86	3.70	2.48	2.73	1.41	-0.34	0.37
Cutpoint 5	6.63	5.25	2.89	3.80	2.61	2.78	1.49	-0.14	0.53
Cutpoint 6	6.75	5.39	2.95	3.83	2.69	2.81	1.52	-0.01	0.70
Cutpoint 7	6.79	5.42	2.98	3.89	2.74	2.83	1.55	0.11	0.81
Cutpoint 8	6.87	5.45	3.01	3.97	2.77	2.88	1.60	0.21	0.84
Cutpoint 9	7.03	5.59	3.06	4.00	2.87	2.96	1.63	0.29	0.87
Cutpoint 10	7.08	5.69	3.12	4.05	2.92	2.98	1.68	0.31	0.93
Cutpoint 11	7.16	5.76	3.20	4.08	2.95	3.01	1.70	0.38	0.98
Cutpoint 12	7.37	5.80	3.32	4.13	2.98	3.03	1.75	0.41	1.06
Cutpoint 13	7.51	5.90	3.38	4.22	3.00	3.08	1.80	0.57	1.15
Cutpoint 14	7.60	6.01	3.44	4.25	3.06	3.13	1.95	0.79	1.37
Cutpoint 15	7.78	6.12	3.50	4.28	3.08	3.18	2.08	0.99	1.69
Cutpoint 16	7.96	6.27	3.53	4.50	3.17	3.29	2.13	1.23	2.00
Cutpoint 17	8.01	6.44	3.59	4.68	3.23	3.46	2.38	1.56	2.29
Cutpoint 18	---	6.48	3.80	---	3.41	3.66	2.59	---	---
Cutpoint 19	---	---	4.00	---	3.65	---	---	---	---
Number of obs	76	90	97	103	104	105	105	117	115
Pseudo R-squared	0.05	0.05	0.03	0.02	0.02	0.02	0.03	0.02	0.04

NOTES: Dependent variable is the Polity score as described in text. Openness Residuals are the residuals of a regression of actual openness on predicted openness, population and land area. Heteroscedasticity consistent errors are reported in parentheses.

Variable	Interwar Sample: 1920-1938					
	1920	1925	1928	1932	1935	1938
Openness	0.15	0.43	0.27	0.42	0.32	0.12
	(0.06)	(0.12)	(0.09)	(0.23)	(0.13)	(0.05)
Openness Residuals	-0.11	-0.31	-0.16	-0.33	-0.23	0
	(0.06)	(0.13)	(0.11)	(0.26)	(0.16)	(0.05)
In (Population)	0.75	2.12	0.85	1.38	1.53	0.35
	(0.46)	(0.87)	(0.64)	(1.22)	(0.86)	(0.49)
In (land area)	-0.28	0.06	0.2	-0.02	0.13	0.18
	(0.21)	(0.21)	(0.22)	(0.16)	(0.14)	(0.14)
Cutpoint 1	10.59	43.16	19.53	26.92	30.48	7.56
Cutpoint 2	11.43	43.81	20.25	27.28	31.53	8.46
Cutpoint 3	11.72	44.02	20.49	27.52	31.66	8.63
Cutpoint 4	11.98	44.26	21.07	28.01	31.90	8.93
Cutpoint 5	12.51	44.51	21.26	28.15	32.14	9.31
Cutpoint 6	12.81	44.80	21.47	28.41	32.26	9.44
Cutpoint 7	13.31	45.06	21.72	28.55	32.38	9.58
Cutpoint 8	13.86	45.30	21.96	28.69	32.52	9.73
Cutpoint 9	---	45.58	22.15	28.83	32.66	9.87
Cutpoint 10	---	46.09	22.34	28.95	32.94	10.01
Cutpoint 11	---	---	22.53	29.21	33.12	10.16
Cutpoint 12	---	---	22.90	29.34	---	10.32
Number of obs	22	26	27	29	29	29
Pseudo R-squared	0.22	0.29	0.22	0.12	0.16	0.14

NOTES: Dependent variable is the Polity score as described in text. Openness Residuals are the residuals of a regression of actual openness on predicted openness, population and land area. Heteroscedasticity consistent errors are reported in parentheses.

Table 12: Ordered Probit "Instrumental Variables" Regressions with Additional Controls, 1870-1910

Variable	Pre World War I Sample: 1870-1910								
	1870	1875	1880	1885	1890	1895	1900	1905	1910
Openness	0.06 (0.02)	0.04 (0.02)	0.02 (0.02)	0.001 (0.03)	0.02 (0.03)	0.06 (0.04)	0.01 (0.03)	0.01 (0.02)	0.05 (0.02)
Openness Residuals	-0.04 (0.01)	-0.02 (0.02)	-0.01 (0.02)	0.004 (0.03)	-0.01 (0.03)	-0.06 (0.04)	-0.02 (0.03)	-0.01 (0.02)	-0.05 (0.02)
ln (Population)	0.67 (0.43)	0.34 (0.48)	0.14 (0.47)	-0.17 (0.62)	0.04 (0.69)	0.53 (0.63)	-0.1 (0.53)	-0.31 (0.43)	0.1 (0.47)
ln (land area)	0.17 (0.23)	0.12 (0.33)	-0.16 (0.31)	-0.21 (0.34)	0.1 (0.42)	0.35 (0.42)	-0.003 (0.38)	0.1 (0.32)	0.52 (0.29)
Cutpoint 1	14.31	7.09	-0.89	-7.55	1.08	13.95	-2.96	-5.33	8.66
Cutpoint 2	15.05	7.51	-0.42	-7.08	2.11	14.33	-2.53	-4.97	9.02
Cutpoint 3	15.30	7.81	-0.11	-6.79	2.41	15.13	-2.05	-4.51	9.27
Cutpoint 4	15.53	8.80	0.81	-6.06	2.66	15.42	-1.60	-4.20	9.46
Cutpoint 5	16.16	9.10	1.08	-5.80	3.02	15.67	-1.47	-4.07	9.63
Cutpoint 6	16.56	9.24	1.21	-5.68	3.28	16.02	-1.36	-3.94	9.80
Cutpoint 7	16.74	9.39	1.34	-5.45	3.41	16.15	-1.01	-3.58	10.24
Cutpoint 8	16.95	9.55	1.62	-5.33	3.72	16.30	-0.89	-3.46	10.38
Cutpoint 9	17.16	9.73	1.78	-5.07	3.91	16.44	-0.64	-3.35	10.51
Cutpoint 10	17.37	9.92	2.12	-4.92	---	16.76	-0.51	-3.12	10.77
Cutpoint 11	17.58	10.13	---	-4.57	---	16.96	-0.36	-2.86	11.00
Cutpoint 12	17.85	10.36	---	---	---	---	-0.19	-2.71	11.26
Cutpoint 13	18.46	---	---	---	---	---	---	---	11.41
Number of obs	18	19	20	21	21	22	22	23	24
Pseudo R-squared	0.1	0.05	0.03	0.02	0.01	0.03	0.02	0.02	0.05

NOTES: Dependent variable is the Polity score as described in text. Openness Residuals are the residuals of a regression of actual openness on predicted openness, population and land area. Heteroscedasticity consistent errors are reported in parentheses.

Table 13: Openness and the Components of Democracy, 1960-2000

	1960	1965	1970	1975	1980	1985	1990	1995	2000
<i>Dependent variable: Competitiveness and openness of executive recruitment</i>									
Openness	0.1406 (0.0387)***	0.1067 (0.0373)***	0.0646 (0.0378)*	0.0575 (0.0246)**	0.0280 (0.0138)**	0.0351 (0.0208)*	0.0164 (0.0147)	0.0064 (0.0126)	0.0098 (0.0106)
ln (population)	0.5978 (0.2199)***	0.8059 (0.2111)***	0.4428 (0.2166)**	0.4478 (0.2328)*	0.3485 (0.2067)*	0.2436 (0.1933)	0.3358 (0.1798)*	0.0786 (0.1515)	0.2175 (0.1540)
ln (land area)	0.1682 (0.2370)	-0.2900 (0.1755)	-0.2138 (0.1819)	-0.0007 (0.2232)	-0.1371 (0.2123)	-0.1485 (0.1858)	-0.3533 (0.1814)*	-0.1765 (0.1625)	-0.1174 (0.1456)
Constant	-12.9336 (4.5739)***	-10.2951 (4.0740)**	-5.0369 (3.7981)	-8.4278 (3.9254)**	-4.3296 (2.8781)	-2.1922 (2.6423)	-0.3859 (2.3857)	2.4562 (2.3332)	-0.6153 (2.0632)
Observations	74	87	97	101	99	103	103	113	114
R-squared	0.1611	0.1748	0.1400	---	0.0515	0.0403	0.1091	0.0516	0.0797
<i>Dependent variable: Constraint on chief executive</i>									
Openness	0.1994 (0.0390)***	0.1789 (0.0409)***	0.0996 (0.0656)	0.0791 (0.0363)**	0.0386 (0.0202)*	0.0599 (0.0362)	0.0370 (0.0237)	0.0256 (0.0216)	0.0190 (0.0139)
ln (population)	0.7867 (0.2718)***	1.1057 (0.2506)***	0.4895 (0.3340)	0.4911 (0.3402)	0.3943 (0.2953)	0.3513 (0.2863)	0.3768 (0.2740)	0.0903 (0.2330)	0.1397 (0.2386)
ln (land area)	0.2195 (0.2918)	-0.2533 (0.1914)	-0.1248 (0.2286)	0.0198 (0.3136)	-0.0967 (0.3095)	0.0161 (0.2631)	-0.1213 (0.2857)	0.0463 (0.2395)	0.0242 (0.2293)
Constant	-17.5783 (4.6815)***	-16.9293 (4.3520)***	-7.8776 (5.6263)	-10.2602 (5.0665)**	-6.2591 (3.7193)*	-6.7708 (3.7718)*	-4.4676 (3.5722)	-0.9355 (3.5140)	-1.0988 (2.9962)
Observations	74	87	97	101	99	103	103	113	114
R-squared	0.1788	0.2008	0.1281	---	0.0366	0.0412	0.0631	0.0122	0.0811
<i>Dependent variable: Competitiveness and regulation of political participation</i>									
Openness	0.1810 (0.0414)***	0.1782 (0.0468)***	0.0695 (0.0623)	0.0877 (0.0383)**	0.0516 (0.0230)**	0.0615 (0.0394)	0.0436 (0.0240)*	0.0271 (0.0206)	0.0247 (0.0116)**
ln (population)	0.7151 (0.2697)***	1.0930 (0.2670)***	0.4702 (0.3272)	0.5544 (0.3667)	0.4251 (0.3297)	0.3047 (0.3018)	0.3078 (0.2895)	-0.0191 (0.2389)	-0.0619 (0.2381)
ln (land area)	0.1790 (0.3443)	-0.3734 (0.2146)*	-0.2969 (0.2489)	0.0089 (0.3393)	-0.0045 (0.3410)	-0.0111 (0.2740)	-0.0608 (0.3053)	0.1776 (0.2259)	0.1718 (0.2102)
Constant	-16.9854 (5.0138)***	-16.5186 (4.6598)***	-6.0456 (5.6167)	-12.6961 (5.1918)**	-9.6547 (3.8864)**	-6.9149 (3.9270)*	-5.4351 (3.4039)	-1.8097 (3.2374)	-0.6783 (2.8069)
Observations	74	87	97	101	99	103	103	113	114
R-squared	0.1614	0.1286	0.1178	---	---	0.0307	0.0417	---	0.0438
<i>Dependent variable: Aggregate Polity Score (from Table 9)</i>									
Openness	0.5189 (0.1104)***	0.4329 (0.1084)***	0.2338 (0.1612)	0.2263 (0.0969)**	0.1155 (0.0540)**	0.1546 (0.0928)*	0.0924 (0.0601)	0.0669 (0.0498)	0.0569 (0.0321)*
ln (population)	2.1643 (0.7006)***	2.9141 (0.6792)***	1.4025 (0.8449)	1.5147 (0.9111)*	1.0818 (0.7733)	0.9029 (0.7566)	1.1505 (0.6964)	0.1990 (0.6046)	0.3047 (0.5991)
ln (land area)	0.5403 (0.8243)	-0.9078 (0.4920)*	-0.6355 (0.6142)	0.0102 (0.8473)	-0.2067 (0.7888)	-0.1564 (0.6988)	-0.6402 (0.7339)	0.0761 (0.5427)	0.1017 (0.5439)
Constant	-48.0680 (13.1334)***	-41.9400 (11.8439)***	-18.9600 (14.3517)	-31.5138 (13.4647)**	-19.0725 (9.5113)**	-15.7134 (9.8893)	-11.0555 (8.9135)	-1.8247 (7.8978)	-2.9754 (7.3093)
Observations	76	90	97	103	104	105	105	117	115
R-squared	0.1821	0.2105	0.1458	---	0.0202	0.0393	0.0810	0.0229	0.0908

Notes:

Figures reported come from a regression of each component of the polity score on openness.

Instrumental variable for openness is the value of predicted openness as described in the text.

Robust standard errors, clustered by country, are reported.

* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 1: Global Un-Weighted Averages of Democracy and Openness, 1870-1910

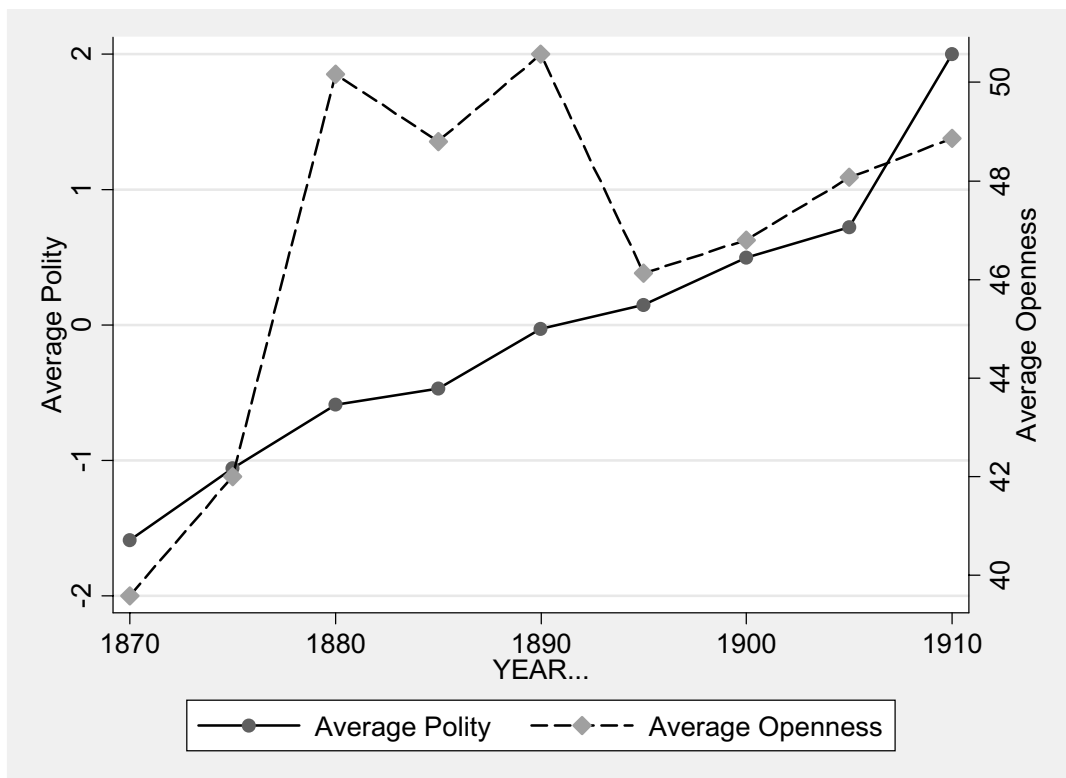


Figure 2: Global Un-Weighted Averages of Democracy and Openness, 1917-1939

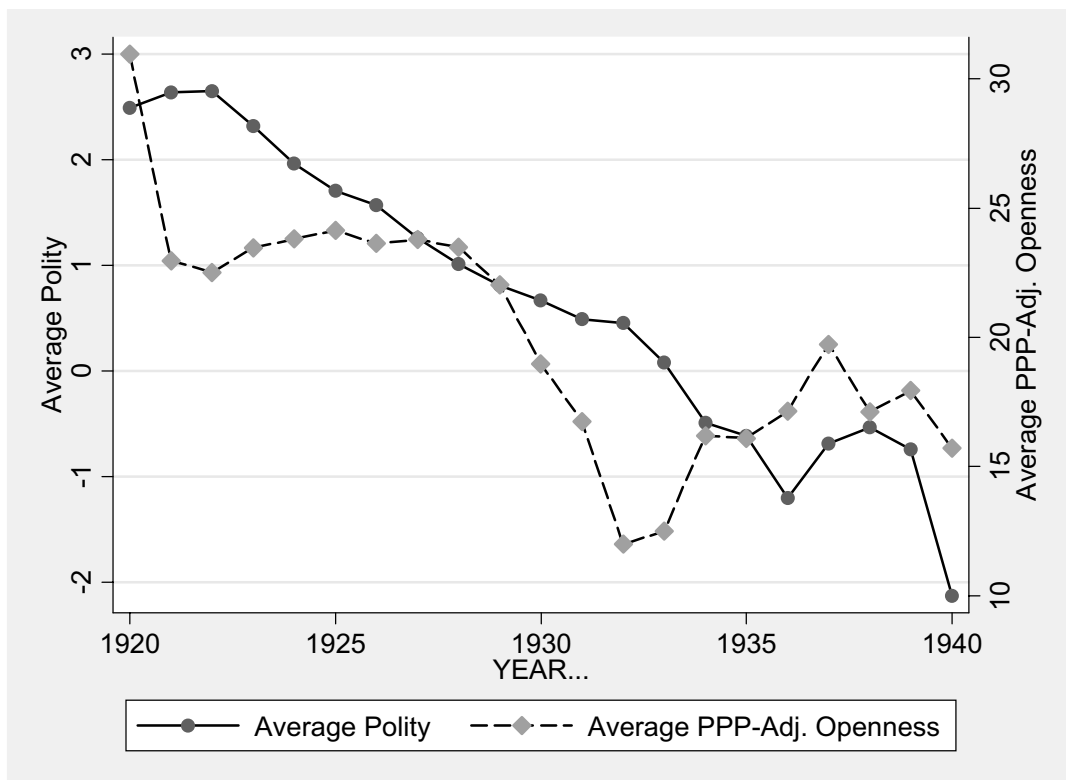


Figure 3: Global Averages of Democracy and Openness, 1960-2000

