Draft of chapter 4, "International Factors"

For a second V-Dem volume, Michael Coppedge and Staffan I. Lindberg, eds., "What Do We Know About Democratization After 230 Years?" In progress.

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The other chapters of this book, like the majority of quantitative analyses of democratization, examine domestic determinants: geography, economic factors, institutions, and civil society. In this chapter we develop and test hypotheses about possible causes that lie outside national borders. There are many good reasons to expect that domestic factors are not the sole determinants. We lay out a theoretical framework that systematically catalogues most of the possible international hypotheses. Although some of the hypotheses have been tested repeatedly, very few of the tests have employed the best available methods from spatial econometrics. We use the best currently available methods to test a handful of hypotheses about international sources of democratization and erosion. We show that international war and global economic expansions affect levels of electoral democracy, changes in democracy, and upturns, but not downturns. We also show that all of the four networks that we test – contiguity, alliances, current colonial ties, and former colonial ties – channel contagion for at least one of the four outcomes, and contiguity matters for all of them.

The international relationships that we report here are significant but small – so small that it appears to be unnecessary for researchers who are interested in short-term domestic effects to take any international influences other than exogenous shocks into account. However, this conclusion is misleading for two reasons. First, the effects that appear to be irrelevant are averages over a very large sample in which nearly all the country-to-country effects are zero. These averages hide many local effects that are larger. Second, the small effects are just the most proximate and immediate effects; they grow stronger as they reverberate through the international system and accumulate over many years. These larger consequences are barely detectable in models that impose a short-term perspective on us, but the same models imply important and fascinating long-term local processes of international influence.

There are many other hypotheses about international factors that remain to be tested. These findings are therefore preliminary and provisional even though we consider them an advance beyond the previous literature.

Rationale

There are at least three good reasons to expect international influences to matter. First, it should be obvious that democracy did not evolve independently in each country. Many ideas rooted in principles of self-government, political equality, and inclusion sprouted in different regions of the world at different times – ancient Athens, the Roman republic, Italian city-states,

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ecclesiastical institutions, Western European feudalism, Enlightenment norms, resistance to absolute monarchy, the French Revolution, independence struggles, mass conscription, socialist writings, civil rights movements, feminism, and technological innovations – and gradually but incompletely blended together into several varieties of democracy that are practiced in about half of the countries in the world today (Held 2006, Coppedge et al. 2011). In addition, we know that countries are bound together in many ways: through trade and investment, migration, shared news and entertainment, international organizations, transnational NGOs, diplomacy and treaties, military alliances. Furthermore, these ties tend to follow homophily with respect to language, religion, linked histories, etc. It is likely that these networks convey norms about desirable political regimes whether the networks were constructed to shape political regimes or not. Moreover, many democratic countries make active efforts to spread democratic practices to other countries in their international networks. Some non-democracies attempt to prevent or undermine democracy abroad by encouraging non-democratic forms of government, too.

The second reason to expect international influence is the empirical evidence that is consistent with such influence. Spatial patterns suggest that geographic proximity matters. A map of degrees of democracy in any decade since 1900 makes it evident that the most democratic countries tend to be found persistently in the northwest quadrant of the world and in Oceania (O'Loughlin et al. 1998) (Figure 1). This clustering is statistically significant.[2] Moreover, when democracy becomes more common, it tends to arise next in countries adjacent to these regions, such as Latin America and Eastern Europe, with a few exceptions such as Japan, India, South Africa, and Mongolia. Second, historical changes in the proportion of countries that are democratic tend to occur in waves and reverse waves (Huntington 1991, Diamond 1999). As is well known, there was a long, slow process of gradual movement toward electoral democracy among sovereign countries in the 19th century, with perhaps a small sudden improvement around 1848; then some dramatic improvement after the First World War; then the well-known setbacks of the Interwar Period; then a new wave of democratization after the Second World War; then another reverse wave starting around 1960; then the celebrated Third Wave starting in the late 1970s, with some acceleration from 1989 until 1993; and there are some indications that we are now at the beginning of a new reverse wave (Lührmann and Lindberg 2019). There are always countries that swim against these tides, but the average trends are clear. It is extremely unlikely that these spatial and temporal patterns could be purely the products of domestic forces working independently within each country, separate from domestic processes in any other country. There must be some kind of coordination mechanism.

Figure 1 about here

Third, several scholars have developed arguments about the channels and mechanisms through which countries affect democracy, or components of democracy, in other countries. Although these works tend not to offer general tests, they have documented the workings of these mechanisms in qualitative case studies. Keck and Sikkink (1998), for example, show how transnational issue networks helped abolish slavery, win women's suffrage, and protect women's

[2] Following a regression of V-Dem's polyarchy index for 2017 (v8) on an intercept, Moran's test of spatial independence decisively rejects the null hypothesis of no spatial autocorrelation based on latitude and longitude ($Chi\text{-}sq_{1, 169} = 188.16$, $p<.00005$).

rights. Whitehead (2001), although he takes a sober, cautionary view of international influences, distills lessons from a collection of regional surveys and case studies, distinguishes among three processes: contagion among neighbors (Li and Thompson 1975), control (or imposition or occupation) by a single foreign power, and consent (the interaction of internal and external conditions favoring democracy). Huntington (1991) argues that the Catholic Church's shift in favor of democracy and human rights helped drive the third wave of democratization. It is hard to find a study arguing that international influences do not matter, although it is possible that researchers bury null findings in the proverbial file drawer.

Theoretical Framework[3]

Theories about how and why external factors matter for democratization are usually not well developed. Perhaps it is not surprising that the theories are vague, given that these are claims about the impact of distant, diffuse causes on a complex macro-phenomenon, democracy, in many diverse global and historical contexts. It is a challenge to reduce the process to just one causal mechanism, much less to specify who the actors are, what motivates them, what resources they command, and why they are successful or not. The most common response to this daunting challenge has been to settle for documenting empirical generalizations that are compatible with multiple causal mechanisms, while leaving theoretical precision for later (Brinks and Coppedge 2006). There are several articles that develop more specific hypotheses about international influences. Among the better examples are Mainwaring and Pérez-Liñán's discussion of the ideas shared in the international Left in Latin America, Woodberry's arguments about how mission schools indirectly promoted democracy, and Pevehouse's reasoning about belonging to a regional organization might influence regime change (Mainwaring and Pérez-Liñan 2013, 93-123; Woodberry 2012; Pevehouse 2002). While these are important contributions, they do not claim to provide a comprehensive inventory of other pathways for international influence. In principle, it is always prudent to avoid omitted variable bias, but in practice it is especially necessary to avoid it when studying international influences because so many of these potential pathways are correlated and believed to produce similar outcomes. Otherwise, the effect of being trading partners or military allies, for example, could easily be mistaken as an effect of merely being neighbors or sharing a language or religion or a level of income. In this section we make an inventory of practically all the possible kinds of international influence as a sobering reminder of how much remains to be tested.

In this chapter, "international influences" are hypothesized causes of democracy in a country that originate outside that country's borders. This definition excludes hypotheses about domestic causes even if they cluster in certain regions and therefore contribute to regional clusters of democracy such as those in figure 1. These exclusions make a long list comprising most of the hypotheses that the rest of this book and, indeed, the bulk of the democratization literature, takes most seriously: hypotheses about income, inequality, education, social cleavages, state strength, culture, institutions, latitude, climate, and so on. How domestic causes, such as high income, strong states, and relative social equality, came to be concentrated in certain regions is an interesting question, but it is not the question we pose here. Spatially dependent domestic causes and truly international causes are easily confused because they often have indistinguishable

[3] Many of the elements of this framework were originally laid out by Starr (1991).

effects. Distinguishing between them is therefore an essential goal of our analysis (Houle et al. 2016).

We make further distinctions between kinds of international influence. At the most fundamental level, we distinguish between unidirectional and multidirectional relationships. In unidirectional relationships, influence flows only from a source to a target country or countries, never – even indirectly – from targets to sources. Unidirectional relationships are therefore safely treated as exogenous; for this reason, some of them are called "exogenous shocks." Within the category of unidirectional, exogenous relationships, we make further distinctions depending on whether the source of influence is the international system, a single country, or a group of countries. Good examples of "shocks" from the international system are global economic crises and world wars. Country-to-country exogenous relationships (not tested here) would include democracy promotion programs, sanctions, occupation, and colonial domination (if assumed to be unidirectional). If groups of countries, such as the World War II allies, carry out these activities, there may be exogenous influences flowing from multiple countries.

In multidirectional relationships, countries are both sources and targets, and their outcomes are therefore endogenous. The simplest example is when country A affects country B and country B affects country A. However, the relationship is also endogenous when the feedback is indirect, as when A affects B, which affects C, which affects A. Endogeneity requires us to think about, and model, these two kinds of international relationships differently. All endogenous relationships can be conceptualized and modeled as networks linking countries. As with exogenous relationships, we can distinguish networks by the structure of the linkages: which countries, and how many countries, are linked together. Some networks consist of mutually exclusive groups of countries, all of which affect each other. Examples include international organizations, alliances, and geographic regions. NATO members presumably affect one another, as did members of the Warsaw Pact before the breakup of the Soviet Union. A group could be as small as two countries, such as North and South Korea, which illustrates the fact that groups need not correspond to a formal organization; rather, which groups matter is a theoretical supposition that we must test. Perhaps countries adopting Huawei's 5G network will be connected in political relevant ways. The European Union probably matters more for democratization than the Organization of American States does, but perhaps less than the group consisting of Australia and New Zealand.

Some networks, by contrast, are not mutually exclusive: countries that are linked to one group are also linked to other groups. These are networks of overlapping networks in which every country that is connected to any other country is indirectly linked to every other country in the network. A good example is the network of contiguous neighbors, which is complex because each country is directly linked to only a handful of other countries, but because every country's direct network is slightly different from its neighbors', every country is indirectly linked to all other countries (assuming that we define neighbors linking islands to a mainland and continents to each other, as we do). Other examples are colonial relationships (if multidirectional), trading partners, and linkages formed by investment flows. Some networks reach the extreme of being global, linking every country to every other one, such as a network defined by the distance between every possible pair of countries.

Beyond the direction and structure of the linkages between sources and targets, a framework for classifying types of international influence must consider the nature of the stimulus that sources send to targets. For exogenous shocks, what matters is usually the magnitude of the shock that each country experiences: in war, lives lost, territory surrendered, resources spent; in an economic crisis, the unemployment rate, shrinkage of GDP, loss of access to capital markets, and so on. For international influences channeled by networks, we think it is useful to use the outcome in the source country – here, levels, changes, upturns, and downturns in electoral democracy – as the stimuli. Therefore, we use our networks to model "contagion," a common term for outcomes here affecting outcomes there (Li and Thompson 1975). If we modeled independent variables in one country affecting both outcomes in that country and outcomes in other countries, a different term, such as "spillovers," would apply.

However, whether the stimulus is an exogenous shock or a democracy outcome, there is still an undertheorized linkage between the stimulus and the response, which is a predicted level of democracy in another country. Researchers must supply additional theory to flesh out causal mechanisms. One way to get closer to mechanisms is to specify which countries carry more weight, and why. Is a country's international influence proportional to population? Income? Economic growth? Military capabilities? Population flows? Volume of investment? Media production? Internet presence? Assigning weights to countries based on such variables can narrow down the kinds of mechanisms to a few possibilities. For example, the hypothesis that countries emulate the political regimes of economically successful countries could be modeled by weighting influence by per capita GDP or economic growth rates (which are two different implications of this idea). By contrast, a more coercive kind of diffusion would be better modeled with networks among countries whose weights are proportional to military power. A softer, more ideational kind of diffusion could be represented by networks linking countries speaking the same language, which encourages flows of information, news, entertainment, and social media. This kind of diffusion could be compatible with an equal weighting of countries, which would permit the examples set by smaller, less important countries to matter as much as the experiences of large, important countries. [All analyses in this draft are unweighted, but we may test weighted versions before the final draft.]

This theoretical framework is fairly comprehensive in scope. In principle, one could translate any hypothesis found in the literature on international influences on democracy into some combination of (1) either an exogenous impact or a network (2) in one or multiple directions (3) with a certain stimulus and (4) an appropriate weighting scheme.[4] The earliest hypotheses proposed exogenous impacts of belonging to one international group or another. For some it was the group of former colonies, or of former colonies of Great Britain or another great power (Bernhard et al. 2004, Bollen and Jackman 1985, Gassebner et al. 2009, Gunitsky 2014, Miller 2012, Woodberry 2012). For others (Bollen 1983, Burkhart and Lewis-Beck 1994), it was the group of countries in the periphery or semi-periphery of the world economy. For Pevehouse (2002) it is membership in regional organizations. Similarly, some scholars have controlled for the average democracy level or the proportion of democracies in a country's geographic region

[4] It may be necessary to add a time parameter to specify lag lengths and cumulative exposure to the stimulus. In addition, the dependent variable could be categorical, a continuous magnitude of change, upturns or downturns, the probability of change, or the cumulative hazard of change.

or the globe, which is an exogenous impact of membership in an informal geographic group (Gunitsky 2014, Starr and Lindborg 2003). Miller (2016) weights mean democracy (Polity) at the regional and global levels by economic growth, concluding that democracy is more likely to spread when democracies are experiencing economic growth. This model, too, is treated as an exogenous group effect.

Others have proposed that democracy promotion efforts have had bilateral, unidirectional exogenous impacts (Finkel et al. 2007; Mainwaring and Pérez-Liñan 2013, 93-123; Miller 2012). Woodberry (2012) argued that prolonged early exposure to Protestant missions aided later democratization, a thesis that translates into an exogenous impact of a linkage to colonizers weighted by mission exposure.

A common strategy has been to construct networks derived from geography, whether based on contiguity (Brinks and Coppedge 2006, Leeson and Dean 2009, O'Loughlin et al. 1998, Starr 2001) or proximity (Gleditsch and Ward 2006).[5] Miller (2016) also tests his growth-weighted model for contiguous countries, but finds no significant relationship at that level. As we argue below, there is a conflict between the assumption of multidirectional linkages in geographic networks and the tests these studies report, which treat these relationships as exogenous. A well executed application of spatial econometrics to democratization using distance is Cook et al. (2018). The authors find that results depend crucially on model specification, and in their preferred model, there is no significant country-to-country influence. This approach is a latecomer to political science, but it is increasingly used in democratization research (Leeson and Dean 2009, Zhukov and Stewart 2013, Goodliffe and Hawkins 2015, Goldring and Greitens 2019).

Levitsky and Way (2006)'s argument that transitions from competitive authoritarian regimes are determined by linkage and leverage also fits in our framework. Which countries are linked together is a matter of being included in the western or eastern network. Linking is the strength of the economic and technological ties, and leverage is the degree of asymmetry of those ties. Both linkage and leverage can be expressed as weights for each direction of a dyadic relationship: linkage by the absolute size of each country's weight, leverage by the ratio of their weights.

Gunitsky (2014, 2017) proposes that several international conditions work in combination. He argues that regime transitions are more likely when there are "hegemonic shocks": "abrupt shifts in the distribution of power among leading states"; but that other factors – British colonial legacies, the proportion of democratic neighbors in the region and globally and recent neighbor transitions – also have exogenous impacts, alone and in interaction with hegemonic shocks. Furthermore, he finds that average trade with the United States in the previous 5 years also matters. Trade is, in effect, a weight in each country's bilateral relationship with the United States. Finally, Gunitsky allows for some countries to influence others, sometimes in the

[5] O'Loughlin et al. (1998) were early adopters of some of the spatial econometric techniques we use, but used them only in a descriptive way, to test for significant spatial clustering of democracy.

aftermath of a hegemonic shock. Gunitsky treats all of these linkages as unidirectional and therefore exogenous.

Teorell (2010, 77-99) tested the greatest variety of international influences: total trade volume; democratization among neighbors; level of democracy among neighbors, in the region, and in the world; democratization and prior level of democracy in regional organization; flows of portfolio and foreign direct investment; economic sanctions; and military interventions. Furthermore, Teorell estimated the impact of each of these factors on the short-term level of democracy, upturns and downturns, and the long-run level of democracy, and complemented the quantitative analysis with several case studies. He specified all of these factors as unidirectional, and therefore exogenous, relationships.

One of the best-known typologies of international influence is Elkins and Simmons's set of distinctions between hard coercion, soft coercion, competitive advantage-seeking, learning, and emulation (2005). These are better seen as broad categories of explanation rather than specific explanations that have empirically distinguishable observable implications. Nevertheless, each type is compatible with one or more combinations of possibilities in our theoretical framework. Most of the emphasis in their typology is on the nature of the stimuli and the weights, without precisely specifying the structure of the network, although in most cases country-to-country relationships are implied. Soft coercion could take the form of aid conditionality, economic sanctions, withdrawing an ambassador, sponsoring a censure vote in an international organization, or secret diplomacy. "Learning" does not specify who learns what from whom. Is it the executive, party leaders, military officers, jurists, journalists, NGOs, or the mass public that learns? Do they learn to emulate positive examples abroad (which raises the question of how learning differs from emulation) or to avoid mistakes? This chapter will not answer these questions, but neither does any other large-sample research project. Some clues can be found in small-sample qualitative studies (Elkins 2013, Madrid 2003, González Ocantos 2016), but more general answers will have to wait.

This list is not exhaustive, but it serves to support the point that all the major hypotheses readily translate into our theoretical framework. It should be clear that there is a world of hypotheses about international influences on democratization. It would be impossible to test them all in a single chapter. Below we develop the hypotheses we have chosen to test here. But first, in the next section we explain the methodological problems with previous research designs and how we seek to get better estimates.

Methodological Considerations

As long as a variable in the model is exogenous, no special handling is needed: it becomes a right-hand-side variable and its coefficient has a straightforward interpretation, exactly like the coefficients of domestic variables. War, international economic shocks, invasions and occupations, and sanctions fall under this heading. If a variable represents a spatially endogenous explanatory factor, however, it requires a much more elaborate procedure to obtain accurate estimates and meaningful interpretations. In fact, processes with spatial endogeneity necessarily violate Rubin's SUTVA, the Stable Unit Treatment Value Assumption, because (a) units

interfere with each other – this is the point! – and (b) potential outcomes are not uniquely defined.[6] We expect the impact of the erosion of democracy in Hungary to be different for Ukraine than it is for Austria, because it makes a difference that Austria's other neighbors are Slovakia, Czechia, Germany, Italy, and Slovenia, while Ukraine's other neighbors are Poland, Slovakia, Romania, Moldova, Russia, and Belarus. From a potential outcomes perspective, it is doubtful whether one can make any meaningful causal inference in such situations.[7] Yet we believe that spatial dependence exists and must be understood if we are to achieve unbiased estimates of the effects of domestic conditions. Therefore, rather than give up, we use the best alternative observational methods from spatial econometrics.

We must use such methods for all of the variables that use democracy in $country_j$ to explain democracy in $country_i$. They include terms for the effects of contiguity or distance, trade and investment flows, membership in alliances and other international organizations, regional location, and status in the world economy. It also includes the effects of colonial rule, invasion, or occupation if there is reason to believe that these actions spur a backlash on the colonizer, invader, or occupying power. These variables are endogenous because every country in the network is both a source and a target.

In a South American neighbor network, for example, we cannot take democracy in Argentina at face value and use it to explain democracy in its neighbor, Brazil, because democracy in Brazil presumably affects democracy in Argentina. Our estimate of democracy in Argentina must be purged of Brazil's influence (and the influence of Chile, Bolivia, Paraguay, and Uruguay) before we use it to explain democracy in Brazil. We must also purge Brazil's democracy of the influence of its ten neighbors before testing for its influence on Argentina. The solution is to use instrumental variables to approximate what the level of democracy would have been in each country if it had no neighbors and then use those instruments to try to explain democracy in the other countries. The same precaution is necessary when the explanatory variable is the regional or global mean or the mean score for members of an alliance or international organization, or any network in which influence directly or indirectly flows in both directions.

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[6] The problem is not utterly hopeless: Aronow and Samii (2013), among others, have offered advice on how to design experiments that achieve causal identification when interference among units is likely. However, research applications in this area have so far been confined to controlled experiments on individuals or small communities. No one has attempted to apply such an approach to the study of countries over more than a century of history, for which it is impossible to randomize any sort of treatment. Natural or quasi-experiments may be found, but they would exclude most of the cases, many of which must be included in any valid study of international influence.

[7] However, even Imbens and Rubin (2015, p. 10 of advance typescript) acknowledge that "(. . .) SUTVA is only one candidate exclusion restriction for modelling the potentially complex interactions between units and the entire set of treatment levels in a particular experiment," while adding that "In many settings, however, it appears that SUTVA is the leading choice."

As far as we know, no other published work has yet used instrumental variables to correct for endogeneity when analyzing international influences on democracy.[8] At best, others have used a temporal lag of other countries' scores to lessen the threat of endogeneity, but given the very strong serial autocorrelation in democracy scores it is far from clear that lagging an independent democracy variable is an adequate solution. Appendix A explains models of spatial dependence in greater detail.

Selected Hypotheses

Exogenous Shocks

Exogenous shocks originate outside a target country's borders, but not from any specific other country. They are emergent properties of the international system.[9] We consider shocks produced by either violent conflict or by the international economy. Both of these can have a negative manifestation – war or economic contraction – or a positive manifestation – peace or

[8] There are two additional limitations of existing research. First, much of the quantitative research considers only the long-term effects of international factors. This is implied by, for example, by Woodberry's analysis of the consequences of Protestant missions (Woodberry 2012). His analysis uses missions data from 1900-1923 to explain mean levels of democracy in 1955-1994, a cross-section observed more than three decades later. Long-term effects are also implied when a panel analysis uses dummy variables to represent an experience in a country's distant past, such as colonial experience (Barro 1999, Bollen and Jackman 1985, Burkhart 1997, Gassebner et al. 2009, Lipset et al. 1993, Muller 1995). Is this a problem? Certainly there can be long-term consequences of past experiences, and they need to be studied. However, we are too easily impressed by empirical relationships that persist over long spans of time. Cross-sectional differences can be persistent even if they are spurious. Cross-sectional regression assumes that differences between countries are equivalent to changes over time, which would be more valid evidence of causation. These are not sufficiently rigorous tests; they cannot distinguish well between the past experience one is interested in and all the other experiences the country had in the distant past. If differences rooted in the past are persistent, they are all relatively fixed effects for these countries that would all appear to have the same consequences. Short-term effects would be much less confounded. Second, a subset of studies use a binary dependent variable. For example, some researchers model the difference between democracies and dictatorships (Londregan and Poole 1996), or the probability of a transition or breakdown (Przeworski et al. 2000 Przeworski et al. 2000, Bernhard et al. 2004), or of a coup (Li and Thompson 1975). These are valid, interesting questions. However, they are dramatic, rare, low-probability events that are hard to model. Only 565 of the 24,751 country-years in v9 of the V-Dem dataset, or 2.3 percent, registered an absolute change of at least 0.1 on the 0-1 polyarchy index. Regime changes as measured by a binary indicator are much rarer than this. Models of continuous outcomes, or changes in continuous outcomes, would be more sensitive to the modest effects that international factors are likely to have.

[9] Climate change may eventually be the best example of an exogenous shock, but we think it is probably too recent to be an important determinant of political change in most of the world. Pandemics (AIDS, SARS, the Spanish Flu of 1918) could be historical examples.

economic expansion. Each of these four scenarios could affect democratization in a different way, for different reasons.

Wartime threatens democracy in several ways. Most obviously, some countries can be conquered and occupied by others, and typically the conqueror governs the occupied territory undemocratically. Even if a more-democratic country occupies a less-democratic one, it will not immediately allow the occupied country to govern itself by electing leaders with real power. However, occupations are the most extreme form of country-to-country coercive diffusion. War can also act as an exogenous shock that can weaken democracy in more subtle ways. The sense of foreign threats, the heightened nationalism, the need for mobilization, and the imperative of national security and secrecy encourages and empowers domestic actors who are inclined to suppress dissent, muzzle the media, or curtail the rights of groups suspected of being disloyal. The degree of damage probably ranges widely, from barely perceptible changes to genocide, but we expect the average impact to be harmful to democracy.

The aftermath of war, however, sometimes has the opposite effect, even beyond the recovery of any rights that were suppressed during wartime (Gunitsky 2017). It is no accident that the most dramatic expansions of the suffrage took place soon after World War I. Mass conscription gave millions of veterans the authority to demand the right to vote, and the economic contributions of women during the war helped secure their calls for suffrage (Dahl 1989). Similar consequences have been attributed to the end of the Napoleonic Wars, World War II, the Vietnam War, and other conflicts. Wars tend to have a leveling effect that is realized only after the peace is won.

The economic expansions and contractions that we treat as exogenous shocks are not the routine, year-to-year fluctuations of the economy captured by national economic growth rates, which often follow different trends in different countries and are therefore better treated as domestic variables. Rather, the exogenous economic shocks are the major shifts that affect many countries at the same time, for an extended period, such as the Great Depression, the long post-World-War-II expansion, and the financial crisis of 2007. These shocks, we believe, are more consequential for political regimes than the routine ups and downs of individual national economies. Deep, lasting economic decline prompts politicians and publics to search for fundamental flaws in the economy and the political system. When this self-questioning takes place in the most democratic and economically advanced countries in the world, the legitimacy of the democratic ideal (as it is understood at the time) is tarnished. The advocates of democracy are put on the defensive and advocates of alternatives such as communism, fascism, technocratic authoritarianism, Islamist fundamentalism, and populism are taken more seriously. However, this works both ways: the economic decline of non-democratic rule, such as the collapse of communism, can undermine that form of rule and enhance the legitimacy of democrats. The impact of the economic shock is conditional on the regime where it hits: a good example of what Franzese and Hays (2008a) called “context-conditional exogenous shocks.” We expect that exogenous economic expansions are also context-conditional. Politicians and publics tend to interpret sustained prosperity as a vindication of their political system, whether it is democratic or not. Prosperity can therefore encourage both the improvement of democracy and the entrenchment of non-democratic regimes (Haggard and Kaufman 1995).

Endogenous Influences

In this paper we focus on three kinds of networks: neighbors, military alliances, and colonial networks. Our expectations about how and why they matter are different for each kind of network. Neighbor networks are the most frequently tested kind, but are also least informative. The proximity of neighbors is probably a proxy for many more specific relationships that would tell us more about how diffusion works. Neighbors are more likely to share languages and religions, to experience migration, to trade and invest, to go to war, and (paradoxically) to be allies. Some of what appears to be diffusion among neighbors is probably actually diffusion within more meaningful networks that happen to be regionally clustered. Ideally the estimated diffusion due to proximity would disappear if we could fully specify all these other networks. Until then, we interpret neighbor effects as a residual category of unspecified mechanisms that follow proximity.

Our models control for contagion through three other networks as well: military alliances, current colonial ties, and former colonial ties. Appendix B develops the theoretical expectations about these networks, as we use the neighbor networks throughout this chapter to illustrate network dependence.

Operationalizing the Hypotheses

The Dependent Variable and the Sample

Our dependent variable is the V-Dem Electoral Democracy Index, also known as polyarchy (*v2x_polyarchy*).[10] As in the other chapters, we model levels of polyarchy and three transformations of it: change, upturns, and downturns. The full v9 dataset consists of 26,834 country-year observations, divided into 202 countries over 8 to 230 years, or on average 132.8 years. This full dataset includes many colonies prior to independence. However, we use data only from 1900 to 2018 due to missing data on key variables such as literacy. This cuts the baseline number of countries to 181. Furthermore, because Stata cannot handle matrices larger than 11,000 rows and columns, we aggregated all observations into two-year periods (except for 1900, which is one year). Our **W** matrices therefore have 181*60 = 10,860 rows and columns. Missing values for some analysis variables further reduces the sample size to 6,755-6,853, which covers a range equivalent to more than 13,000 country-years.

[10] In previous work, some of us proposed that the kind of stimulus that flows through networks is a democracy *gap* rather than a level (Brinks and Coppedge 2006, Coppedge et al. 2016), Furthermore, our dependent variable was the change in democracy rather than the level. The notion of a gap had the advantage of allowing the effect to depend on the relative positions of the source and target countries: a more democratic source country exerts a positive influence (assuming a positive coefficient) on a less democratic target country, and a less democratic source country exerts a negative influence on a more democratic target country. In this paper we have abandoned gaps in favor of levels because (a) calculating gaps builds ineradicable simultaneity into the model, as democracy in the target country is on both sides of the equation; (b) adding democracy in the mean source country to the right-hand side of the equation is nearly the same as subtracting it from the left-hand side; and (c) allowing the lagged value of democracy in the target country to have a coefficient is less restrictive than calculating change, which forces that coefficient to be 1.

Exogenous Shocks

To measure international are, we use a dummy that captures cases of interstate war with at least 1000 battle deaths, recoded from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002, Petterson et al. 2019, Petterson 2019) and the Correlates of War project (Sarkees 2010).

To measure exogenous global economic expansions and contractions, we averaged the annual growth rates of per capita GDP for all available countries in each year, using the "GDP growth" variable *e_migdpgro*.[11] This variable does not cover all countries, especially before 1950, but we deem this only a small problem because the fluctuations we need to capture are those that affect a very large number of countries in the same way. We can therefore assume that trends that affect the available countries affected the countries with missing data as well. If in reality some national economies expanded and others contracted, this indicator should produce mean growth or decline rates closer to zero. The only problem would be if growth rates were correlated with missingness, which we think is unlikely. Because annual average global growth is still quite volatile, we smoothed these values with a three-year centered moving average and then calculated the first difference of the moving average.[12] The result is a series with a mean of less than 0.0001 and a standard deviation of .009 and that has its largest declines (-.032 to -0.025) in 1930-31 and 2008 and its largest increases (.025 to .045) in 1945-47 and 1933-34. Because we assume that expansions and contractions do not necessarily have opposite effects, we split this into two series: one for negative values and one for positive values, with values of the opposite sign set to zero, as shown in Figure A1. The economic shocks are lagged one year.

Endogenous Hypotheses

We test four networks that link countries in different ways: contiguous neighbors, military allies, current colonies and their colonizers, and former colonies and their colonizers. Neighbor "weights" may be a misleading term because they are binary: either a country is a neighbor (weight=1) or it is not (weight=0).[13] However, during the analysis this matrix is row-standardized so that the influence on each country is an average of the influence coming from all immediate neighbors. To define neighbors we use the criteria of contiguity used in Brinks and Coppedge (2006), with a few amendments. Countries on continents are neighbors if they share a border; Australia is counted as an island, rather than a continent. If an island is close to a continent, its neighbors are the closest neighbor on that continent and any island nations in between. If an island is about equally close to any continent, or to multiple countries on the same continent, it has as neighbors all nearly equally close mainland countries and any islands in between. If an island is not close to any continent it has as neighbors islands within 150 percent of the nearest neighbor. For example, we classify Cuba and the Dominican Republic as

[11] V-Dem estimates this variable using the "GDP per capita" variable from the Maddison Project (2013).

[12] This formula simplifies to $(growth_{t+1} - growth_{t-2})/3$, where *growth* is the global average by year.

[13] We plan to retest relationships using inverse distances instead of a classification of neighbors.

neighbors of the United States. Neighbor dyads are bidirectional. That is, if country A is a neighbor of country B, then country B is also a neighbor of country A.[14]

To test our hypotheses about contagion through military alliance networks, we constructed a matrix that specifies every pair of countries that are members of a shared alliance. Similar to the neighbor network weights above, alliance network weights are binary but row-standardized. If two countries are in multiple military alliances, the network weight remains one. There is no additional weight for multiple alliance memberships. We also do not distinguish between bilateral alliances and multilateral alliances, coding all members of multilateral alliances as in bilateral alliances with every other member of the alliance. To construct our alliance network variables, we use the formal alliances data set from the Correlates of War project (Gibler 2009). In both datasets, countries and their allies are coded on a yearly basis and for every year they code a dyad as being in an alliance, the dyad is given a one in the alliance network variable. When the alliance ends, the dyad reverts back to 0.

Importantly, we only code membership in what Gibler calls defensive alliances and what Leeds et al (2002) call offensive or defensive alliances. These are military alliances that obligate the members of the alliance to aid in the defense of their alliance members if attacked militarily, and sometimes also compel alliance members to aid an alliance member with offensive military operations. These are distinct from non-aggression pacts, which are coded separately, and do not meet the full definition of military alliances.[15] Non-aggression pacts do not operate in the same manner as military alliances and as such we do not expect them to impact the diffusion of democracy.[16]

Our approach is unlike any other in several respects beyond the gap-driven mutual adjustment model. First, we V-Dem electoral democracy data (version 9). V-Dem data does not just provide extensive geographic and historical coverage; it is the only dataset that measures electoral democracy (and other types of democracy) for most colonies before independence, which is crucial for this analysis. Second, the Electoral Democracy Index (also referred to as "polyarchy" here because it operationalizes Dahl's concept of polyarchy) we use is constructed

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[14] We have yet to exploit the potential of weighting neighbors by population, GDP, military capabilities, etc., to learn more about why neighbors matter. However, Brinks and Coppedge (2006) found that various weighting schemes performed no better than unweighted neighbor networks.

[15] Leeds et al (2002) define alliances as "written agreements, signed by official representatives of at least two independent states, that include promises to aid a partner in the event of military conflict, to remain neutral in the event of conflict, to refrain from military conflict with one another, or to consult/cooperate in the event of international crises that create a potential for military conflict."

[16] This draft excludes tests that weight military alliances by capabilities and explore the timing of diffusion through alliances. However, elsewhere we have reported that (a) much of the convergence on levels of democracy takes place in the lead-up to alliance membership, (b) countries with strong military capabilities exercise more influence in diffusion within alliances, and (c) convergence is much more rapid among neighboring allies than among distant ones (Denison and Coppedge 2017).

from variables measured on a true interval scale, unlike most democracy measures, which are ordinal. Interval-level measurement is especially important for calculating democracy gaps between countries, as it is meaningful to subtract equal-interval values but not ordinal ranks—an advantage that ordinal Freedom House data did not afford to Brinks and Coppedge (2006). Third, we operationalize diffusion paths separately before and after independence.[17] Therefore, we use a "former colonies" **W** matrix for linkages between former colonizers and former colonies after independence and a "current colonies" **W** matrix for linkages between colonizers and colonies before independence. Both colonial matrices have a radial structure in which colonies are linked to their colonizer but not to one another. All nine colonial networks – Belgian, British, Dutch, French, German, Italian, Portuguese, Spanish, and U.S. – are included in each matrix. We therefore estimate spatial dependence as a weighted average across all the colonizers and colonies.

We used information in the V-Dem Country Coding Units document to define colonies (Coppedge et al. 2014). We coded these networks for the period from 1900 to 2016. A network weight of 1 represents the existence of a relationship for the two corresponding countries in a given year, and 0 represents the absence of such relationship.

Controls

We include several domestic determinants in the analysis that serve as control variables for the domestic part of the analysis (effects on $country_i$) and instrumental variables in the international part of the analysis (effects of $country_j$ on $country_i$). Election year is a dummy for a presidential, legislative, or constituent assembly election taking place in a given year. We include this because we consistently find that democracy scores tend to change more at the time of these high-profile events. For more severe shocks, we use measures of economic shocks. Following the work of Miller (2012) and Teorell (2010), we use a dummy variable for incidents of hyperinflation. We also use a dummy that captures cases of internal war which, like international war, we recoded from the UCDP/PRIO Armed Conflict Dataset and the Correlates of War project (Sarkees 2010). In both cases, the dummy variables have some values of 0.5 that we created when we aggregated all the data to averages for two-year periods. Unfortunately, listwise deletion in analyses with these measures reduces our sample to fewer than 8,327 country-years.

We use two controls for economic development: estimates of per capita GDP and the adult literacy rate.[18] Many have argued that literacy has a relationship with economic development, as

[17] In earlier versions of this analysis, we tested more than 50 colonial hypotheses that yielded separate estimates not only for current and former colonies but also for each of nine colonizers, three types of colony (occupation, settlement and forced settlement) for four of the colonizers (Britain, France, Spain, and Portugal), and two directions of influence: from periphery to center and from center to periphery. For now estimating so many relationships with spatial econometrics tools is not feasible unless we treat them all as unidirectional, exogenous influences. However, we plan to incorporate some of these distinctions in our spatial models eventually.

[18] In preliminary baseline models (not shown), we used the natural log of per capita gross domestic product, which is an interpolation and imputation of data from Maddison (2001) using GDP per capita PPP in constant 2005 international dollars from the World Bank (2013).

increased levels of literacy and schooling produce higher levels of human capital inside a country (Blaug 1966, Barro 1991, Benhabib and Spiegel 1994). The country can then convert human capital into tangible economic growth. Our measure of literacy is the adult literacy rate, which measures the percentage of the population age 15 or older who are literate. We use the percent literate variable from Vanhanen (2003) and merge it with the World Bank's (2016) adult literacy variable for country-years not covered by Vanhanen. Both variables measure the adult literacy rate in the same percentage format. Many colonizers kept records of the literacy rate and education in their colonies, which gives greater data coverage for the literacy variable. Since both data sources have gaps in their coverage of the literacy rate, however, we interpolated the data after combining them into one measure. After interpolation, the literacy variable has 3577 more observations than the most comprehensive GDP per capita measure and covers almost all of the colonial cases we are interested in. Finally, we used multiple imputation to fill in the 1441 remaining missing values (most of which are for nonexistent country-periods that are not used in the analysis).[19] We also use, a measure of GDP per capita in all the models. It comes from a dynamic latent trait model that combines information from six different sources to produce the most comprehensive time series measure of this concept (Farris et al. 2017).[20]

Because literacy and income are both usually proxies for the same concept – either economic development or some version of modernization – it may seem odd to include both as domestic predictors. However, empirically these two indicators are correlated at only .70 in this sample, probably not strongly enough to risk multicollinearity. More importantly, they are not the variables of interest here. Even if they are collinear, it is not a problem because their role in the model is to serve as two of the nine instruments for polyarchy. If either one explains some additional variance in polyarchy, it is all to the good.

Finally, our models incorporate two variables that correct for selection into the set of colonizers and colonies. These corrections are included to prevent biased estimates for the two colonial networks. Because we give little attention to the colonial networks in this chapter, we discuss these correction factors in Appendix C.

Estimation

We estimate the above relationships with a spatio-temporal autoregressive (STAR) model, which is described in detail in Appendix D. The essential features are that it models democracy

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However, the GDP per capita measure reduces our sample by over 7000 country-years and does not include colonial territories prior to 1960, so we are better off using literacy as a proxy.

[19] Although it would be better to generate many imputed values, run our model many times to generate many parameter estimates, and then report their medians and confidence intervals, it is not feasible to combine this amount of computation with the hours it takes to run each model once on Notre Dame's high-performance computing machines.

[20] This time series is not yet publicly available. Members of the V-Dem team have been allowed to use it internally, but we cannot share the data and will not publish with it unless its creators release it publicly. If it remains proprietary, we will either delete the GDP per capita measure from the model or use an alternative, such as the data from the Maddison Project.

in each country as a function of exogenous shocks (international war and global economic expansions and contractions), a set of nine domestic determinants, and democracy in other countries that are linked via the four networks. To deal with the complication that democracy in every country is potentially endogenous to democracy in every other country, we use the domestic determinants as instruments for democracy. This reassures us that we have a good idea of what democracy in each country would have been if there were no such international influences, and therefore enables us to estimate influence through networks with less bias. We also correct for the possibility that countries that appear to affect one another may be merely similar with respect to some omitted domestic determinants. We estimate this model four times, once of each of the dependent variables: level of polyarchy, change, upturns, and downturns.

Figure 2 makes the structure of the model more concrete with a simplified depiction of these relationships in a hypothetical world with a target country, i, and source countries j, which are linked to each other via our four networks. (The four W matrices appear twice to prevent arrows from crossing in the figure, but the two sets of matrices are the same in the actual model.) Every country is $country_i$ and every country is a $country_j$ with respect to some other countries, but Figure 2 focuses attention on one dyad. Each country has a polyarchy score, $Polyarchy_{it}$ or $Polyarchy_{jt}$, which the model attempts to explain. The model specifies several exogenous drivers of the process: a lagged dependent variable, a set of instrumental variables, and exogenous shocks. The dependent variable is lagged two periods so that it precedes the one-period lag of literacy.[21] It also has Heckman correction factors for the probability of being "selected" into the group of colonizers, the group of colonies, or neither.

The model also structures the way in which polyarchy in one country depends on polyarchy in other countries. $Country_i$ influences $country_j$ and vice versa. Arrows drawn through the four **W** networks indicate that countries influence each other only if they are connected by the network in question. However, polyarchy in one country does not directly influence polyarchy in the other. Rather, the instrument of $Polyarchy_{it}$, $Polyarchy_{it}$*, influences $Polyarchy_{jt}$ while the instrument of $Polyarchy_{ijt}$, $Polyarchy_{jt}$*, influences $Polyarchy_{it}$. The wide arrows represent the first-stage estimation of the instruments. Overall it is a partially circular process driven by the exogenous shocks and domestic determinants.

Figure 2 about here

Results

Tables A3 to A6 in the appendix present all the estimates from our STAR regressions; Table 1 compiles the coefficients for these and compares them with estimates from models without networks. We modeled the same four dependent variables that the other chapters of this book analyze, with the probably unimportant difference that our observations are aggregated into two-year averages. Models 1 and 2 explain the level of polyarchy, which is most relevant for

[21] Recall that all variables are aggregated into two-year periods after 1900. Therefore, t_0 is the mean of year 1 and year 2, t_{-1} is the mean of $year_{t-1}$ and $year_{t-2}$, and t_{-2} is the mean of year t_{-3} and $year_{t-4}$.

understanding long-term trends in democracy and cross-national differences. Models 3-8 analyze change, upturns, and downturns, which reveal more about short-term, within-country dynamics.

Table 1 about here

The odd-numbered models are simple panel regressions with exogenous shocks but without contagion through networks; the even-numbered STAR models specify the four contagion networks.[22] This comparison is important because one of the concerns about ignoring spatial relationships has been that we may obtain exaggerated estimates of the effect of domestic determinants. Our comparisons strongly suggest that there is no basis for this concern in this application. Although each of these four networks is statistically significant in at least one STAR model, they appear to make no difference in the impact of the other factors. There is little reason, for example, to discount the estimates in the other chapters in this book on the grounds that they usually do not take interference among units into account. We cannot generalize this conclusion to all large-N quantitative analyses of democratization; strictly speaking, it is limited to explaining these four variables, with these measures, using this set of independent variables, and every other specification decision that undergirds this analysis. Nevertheless, it is at least possible that Galton's Problem is less problematic for democratization analysis than many (including us!) have warned.

Not much needs to be said about the domestic covariates because they are not the variables of interest. Lagged polyarchy is always the strongest variable in each model, although more for levels than for the more dynamic dependent variables. Election years help every aspect of democratization except downturns. Episodes of hyperinflation, strangely, are associated with higher levels of democracy and more positive changes and upturns. Could it be that the causal arrow is reversed, with democracies being a bit more prone to hyperinflation? We will leave this oddity for others to resolve. Internal war has negative effects on change and downturns, but not levels or upturns. But the coefficient for upturns barely misses significance, and the change coefficient is approximately the sum of the upturns and downturns coefficients. Controls for latitude, port distance, and being a likely colonizer are all insignificant. Not surprisingly, being a likely colony is associated with lower levels of polyarchy on average. It is interesting that literacy and income have positive effects on level, but oppositely-signed coefficients for the dynamic variables. If there is a substantive reason for this pattern, it may be that income affects polyarchy with diminishing returns but literacy does not. It is also possible that these two variables are collinear, making their coefficients unstable. However, as noted previously, it does not matter because these domestic covariates serve as instruments for domestic-driven polyarchy, not as variables of interest.

In six of the eight models, two of the exogenous shocks affect polyarchy in expected ways. First, international war is harmful: it reduces the level of polyarchy, it shifts changes in

[22]The simple panel regressions use random effects. They do not use fixed effects or clustered standard errors in order to maximize comparability with the STAR models.

polyarchy in a negative direction, and it makes upturns smaller, shifting them toward zero.[23] Global economic expansion has the opposite effects. First, it raises the level of democracy. Although the coefficient is large, the substantive effect is modest because the rate of global expansion is .037. Multiplied by the coefficient, it would raise the level by .036 on average. Expansion makes the average change in polyarchy 0.021 points less negative or more positive, on the 0-1 scale. It also makes the average upturn in polyarchy about 0.018 points more positive. Neither war nor economic expansion has a significant effect on downturns. However, global economic contractions do not hurt, on average. We should model economic growth and decline at the domestic level as well, as Knutsen and Dahlum do in chapter 5.

The four networks channel different contagion effects for different dependent variables. The neighbor network is significant for all four outcomes, which may indicate either that we have not yet adequately specified all the meaningful linkages that are correlated with geography or that space just intrinsically matters and no specific reason can be given for this phenomenon.

The other networks matter only for some of the dynamic dependent variables. The alliances network channels polyarchy contagion significantly only for upturns and downturns. This tendency suggests that the causal mechanisms that alliances employ – diplomacy, bargaining, carrots in the form of inducements and sticks in the form of sanctions and the threat of expulsion – operate primarily in the short term. All networks have significant effects on upturns. In the case of the former colonial network, upturns receive its only significant impact. Current colonial ties (the "current" signifies the pre-independence period, when colonial rule was still ongoing) affect both upturns and change, the two most positive dynamic variables.[24]

It is a paradox that there are multiple significant network effects, yet they do not alter the estimated effects of domestic determinants or exogenous shocks. The most obvious resolution of the paradox would be that the network effects are small (which is true), and they are significant only because the sample is very large. However, there is a less obvious reason, too: the full effects are larger than the coefficients indicate. In order to understand how this is possible, it is important to know how to simulate the magnitudes of these effects.

The network effects reported in Table 1 are only the immediate first-order effects: that is, the effect of the source country's polyarchy on the closest countries' polyarchy in the next period. However, these are not the full effects. In time-series analysis, it is customary to report a long-run effect, which is the limit of the sum of the effects of all previous years. There is a spatial analogue to this: the "steady-state" effect, which is the limit of the sum of all the country-to-country effects. The effects of each country on itself are largest, then they decay exponentially for each successive network lag. In the neighbor network, for example, neighbors have stronger

[23] We do not see evidence here for the dramatic spells of democratization in the aftermath of the two world wars. However, our dummy variable covers only war years; to test for positive postwar effects, we should model the consequences of wars ending, with several year lags.
[24] Note that the two colonial networks do not discriminate between CP and PC directions of change, unlike in earlier drafts. This may obscure an important distinction. We mean to weight colonizers more in the future.

effects than the neighbors of neighbors, and so on.[25] In models with both spatial and temporal lags, combined long-run steady-state (LRSS) effects are most relevant.

Again, however, the LRSS effects vary a great deal over time and geography, so the global average LRSS effect can obscure more than it reveals. For this reason, the most informative interpretation of these network models comes from the disaggregated, country-year to country-year effects, called "marginal response paths," or MRPs. These statistics are simulated estimates of how much of an effect polyarchy in a country in a given year has on polyarchy in another country in subsequent years. Figure 3 maps the predicted effect of a unit increase in polyarchy in Brazil in 1900 on polyarchy in most other Latin American countries in 1925-26. Most South American countries are immediate neighbors of Brazil, so they are predicted to experience the strongest effects. (There is some slight variation among them, for example smaller effects on Paraguay and Uruguay, because the MRPs were estimated by taking into account not only contiguity, but also the other networks, the exogenous shocks, and all the covariates in the model.) There are noticeably smaller effects on the second-order neighbors Chile, Ecuador, and Panama; and by the time the simulated pulse from Brazil reaches the third-order neighbors in Central America, the expected effect has decayed to virtually zero. Obviously, the neighbor effect of Brazil on countries outside of Latin America would be nil as well. These effects are highly localized.

Figure 3 about here

What this map cannot show is how dynamic these relationships are. We can explore all of these relationships with a line graph that tracks the influence of polyarchy in Brazil in 1900 on polyarchy in selected other countries in the region in all subsequent years. Figure 4 does this for two immediate neighbors (Peru and Uruguay), two second-order neighbors (Ecuador and Chile), and the third-order neighbor Costa Rica. As expected, the model predicts the stronger effects on the closer neighbors. But what is most striking is the long-lasting, dynamic effect of Brazil's degree of polyarchy in 1900. It increases in each period until peaking 20-40 years later and then beginning a gradual decline. Our model suggests some effects lingering even a century later.

Figure 4 about here

Moreover, if Brazil's 1900 degree of polyarchy has an expected effect decades later, then its polyarchy in 1901-02 also has long-term effects, and so do all subsequent years. Therefore, the

[25] A common way to present the steady-state effects in a spatial analysis is to distinguish between a "direct" effect, which is the average steady-state effect of each country on itself, and an "indirect" effect, which is the average effect of every other country on each country (LeSage and Pace 2009). These are meaningful summary statistics for compact networks, i.e., ones with relatively few higher-order linkages, such as a regional organization that indirectly links every member to every other member. Indirect effects are less meaningful for the highly dispersed neighbor network, in which every country is linked to every other country but the vast majority of the linkages are high-order linkages that are very close to zero. The indirect effects in such networks will vary over a wide range that makes the average much less meaningful, and the average will tend to be very small.

effect of Brazil on another country's polyarchy in any given year is the sum of Brazil's effects originating in all prior years. This cumulative effect, which we can call the "cumulative MRP," of Brazil on the other country is much larger than the MRP for this country pair. These effects are plotted in Figure 5. Here, the magnitudes of the cumulative MRPs far exceed any reasonable prediction because they are based on the assumption of an impossible unit change in polyarchy in Brazil in each two-year period. A unit change would be a jump from the polyarchy index's theoretical minimum of zero to its theoretical maximum of 1 in just two years. No country has ever reached either theoretical extreme, so a jump of that magnitude is impossible in one period, much less in every period for many decades.

Figure 5 about here

A more realistic depiction of Brazil's influence would interact the MRPs on much smaller jumps, such as its actual changes from year to year, and then cumulate them. Figure 6 simulates these effects on the United States, Cuba, Colombia, and Venezuela. There is no significant effect of Brazil on Cuba or the US (which lie directly on the zero line): they are too far away. However, there are quite dramatic and statistically significant expected effects on the two immediate neighbors, Colombia and Venezuela. The effects are small, less than .02 on a 0-1 scale, but significant even though their overlapping confidence intervals tell us that they are indistinguishable from each other. Both countries receive negative (anti-democratic) influence from 1930 to 1943, then positive (pro-democratic) influence in 1947-63 and after 1983.

Figure 6 about here

These figures barely begin to explore the effects implied by our network models. The examples shown here focus only on the neighbor network, ignoring allies and colonies. The figures deal only with the effects on levels of polyarchy, as we have not yet begun to simulate effects on change, upturns, and downturns. Furthermore, these simulated effects could be aggregated in other interesting ways. We have shown a set of "one-to-one" cumulative effects: the effect of only one country on only one other country, cumulated over prior years. We could also consider "many-to-one" cumulative effects: the impact of all other countries in the network, cumulated over prior years, on a given country in a given year. This would be a summary measure of the given country's expected susceptibility to influence through this network. Figure 7 displays this many-to-one effect for twelve countries in South America from 1900 to 2018. Each country's degree of susceptibility varies somewhat independently, but they all roughly track a weighted average of the level of polyarchy in the region, because it influences all of them through the instruments for polyarchy in their average immediate neighbors. We could also consider the opposite: a "one to many" cumulative effect, which would indicate how influential each country has been relative to other countries in its network.

Figure 7 about here

Most importantly, we have estimated less than a thousand marginal response paths among a few countries in the Western Hemisphere. The number of response paths that one could potentially explore is the number of country-periods (about ten thousand), squared, multiplied by

the number of networks and by the number of dependent variables. This amounts to tens or hundreds of millions of biennial bilateral relationships.

Conclusions

There are significant international influences on democratization no matter whether we represent the outcomes as levels, changes, upturns, or downturns. Exogenous shocks in the form of international war and global economic expansion matter for three of these outcomes. Contagion through various international networks matters for various outcomes as well. Controlling for contagion, at least as modeled here, may not lead to any different conclusions about the impact of domestic determinants or external shocks, but contagion processes are interesting in themselves. The short-term, global average effects estimated by our panel regressions are so small and orthogonal to the other relationships that their relevance is difficult to discern. It is only when we simulate their expected long-term, local effects that they reach their fullest expression.[26] In this sense, these contagion processes reside in a hidden dimension of democratization, only recently discovered, that invites deeper exploration.

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[26] We must exercise some caution when drawing conclusions from individual simulated effects, which are expected to be true only on average in large samples. For that reason, it is prudent to plot the confidence intervals around the simulated effect of a counterfactual shock from one country on another.

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Figures and tables for chapter 4, International Influences

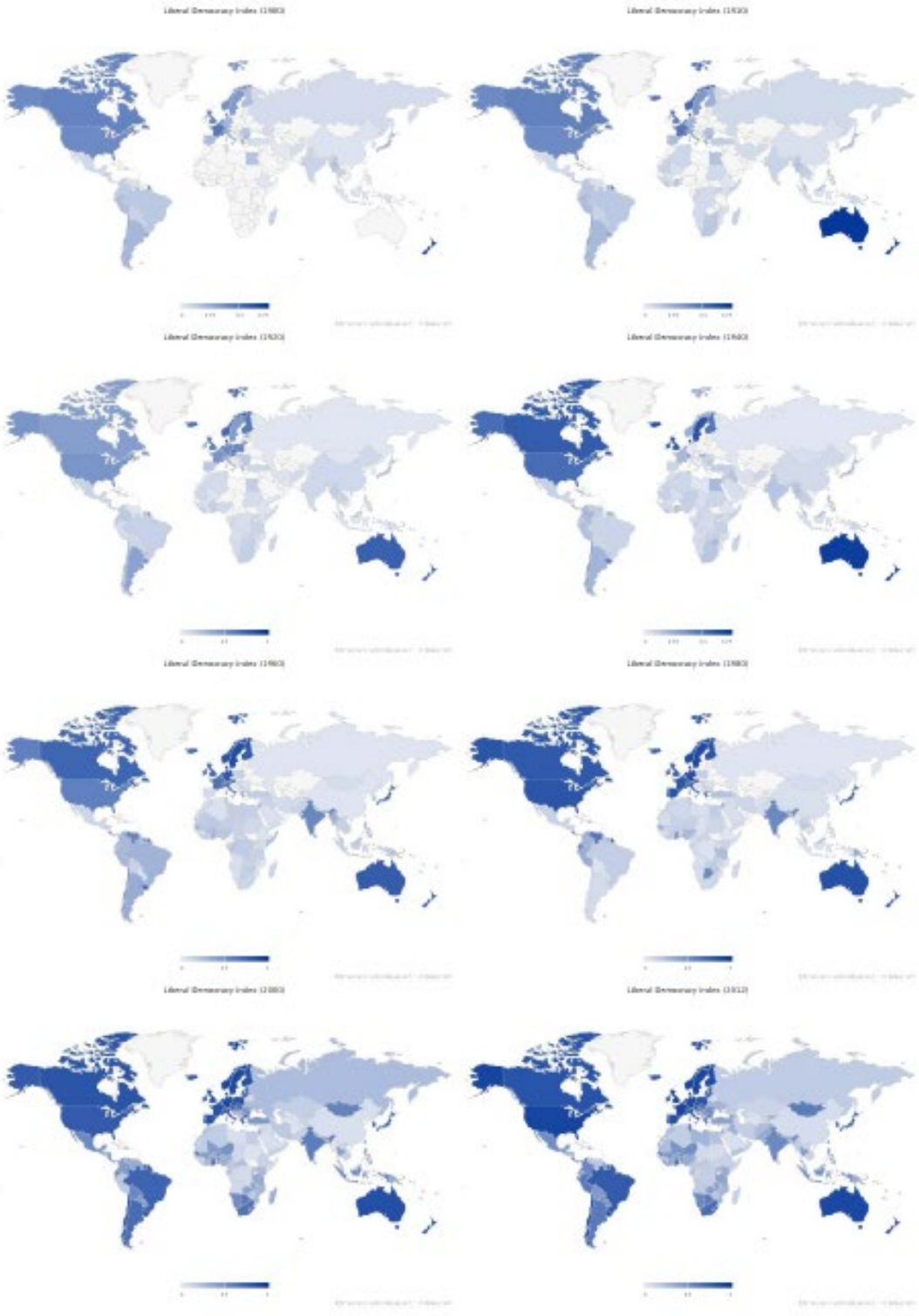


Figure 1: Clustering and Spread of Liberal Democracy, 1900-2012 [To be updated with v9 data]

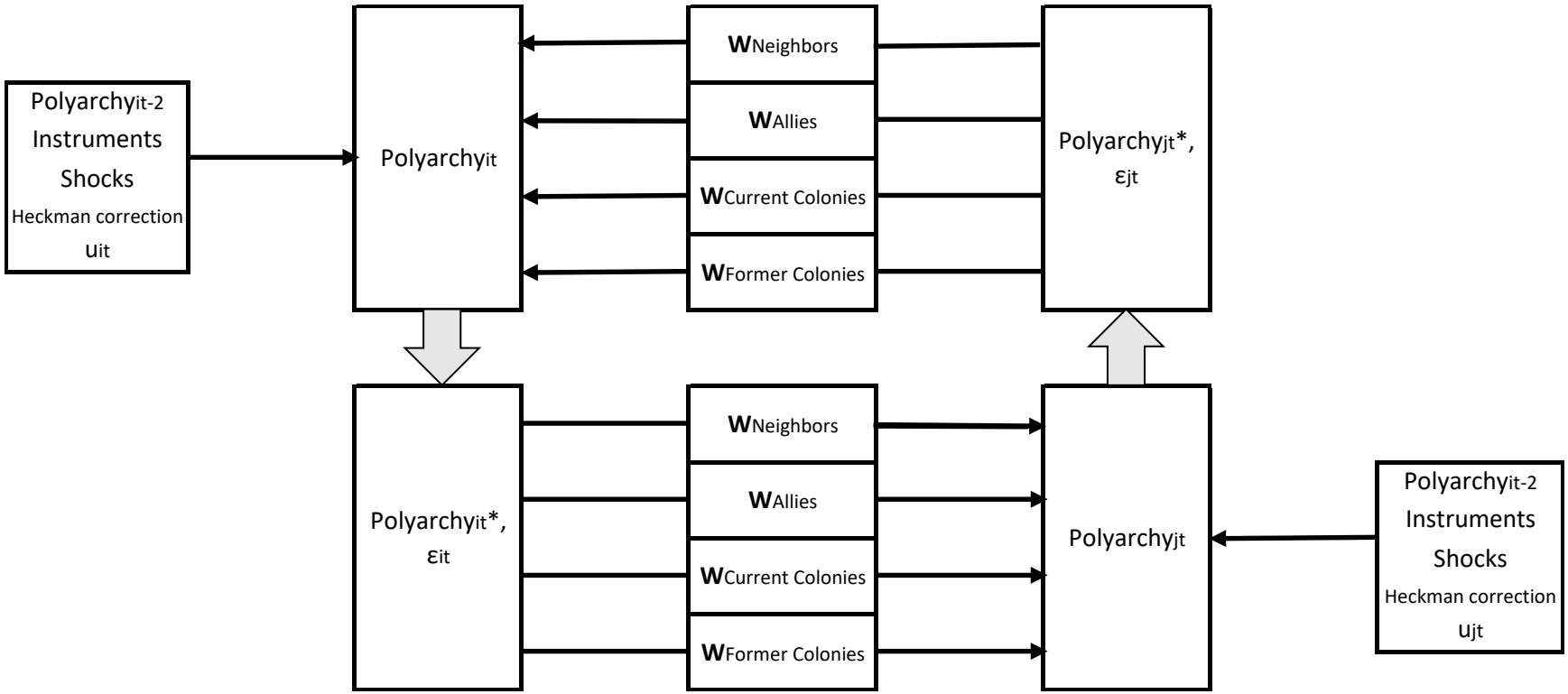
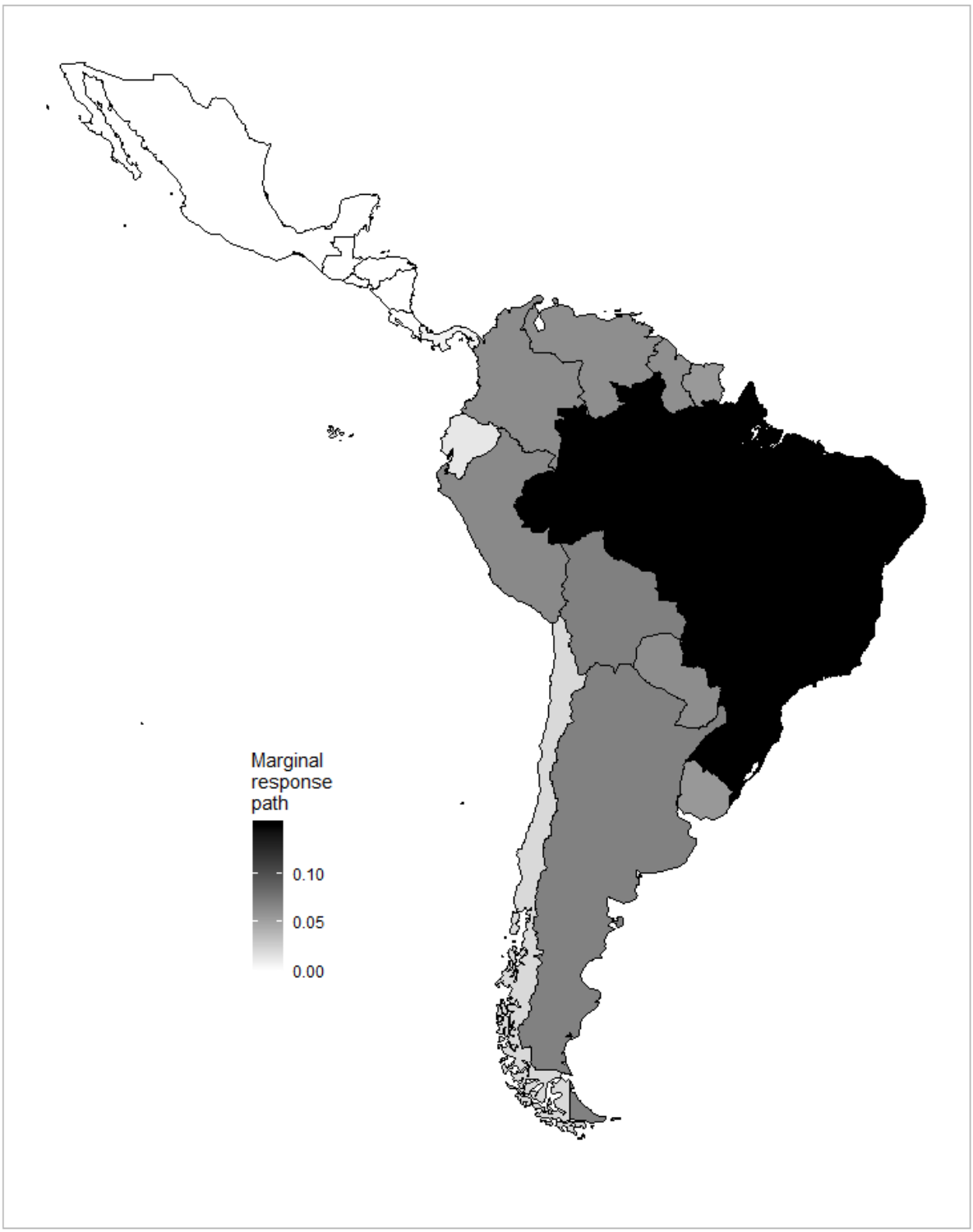


Figure 2: The Full STAR Model

Wide arrows represent the first-stage estimation of the instruments $Polyarchy_{it}$* and $Polyarchy_{jt}$*.
The four W matrices appear twice to prevent arrows from crossing, but the two sets of matrices are the same.
Arrows drawn through the matrices indicate interactions between the matrices, on the one hand, and on the other, the errors and the instruments for polyarchy, producing estimates of spatial autocorrelation of polyarchy and spatial error correlation.

Effect of Brazil (1900) on regional countries in 1925-26



Brazil's shade is set to black because it is off the scale.

Figure 3

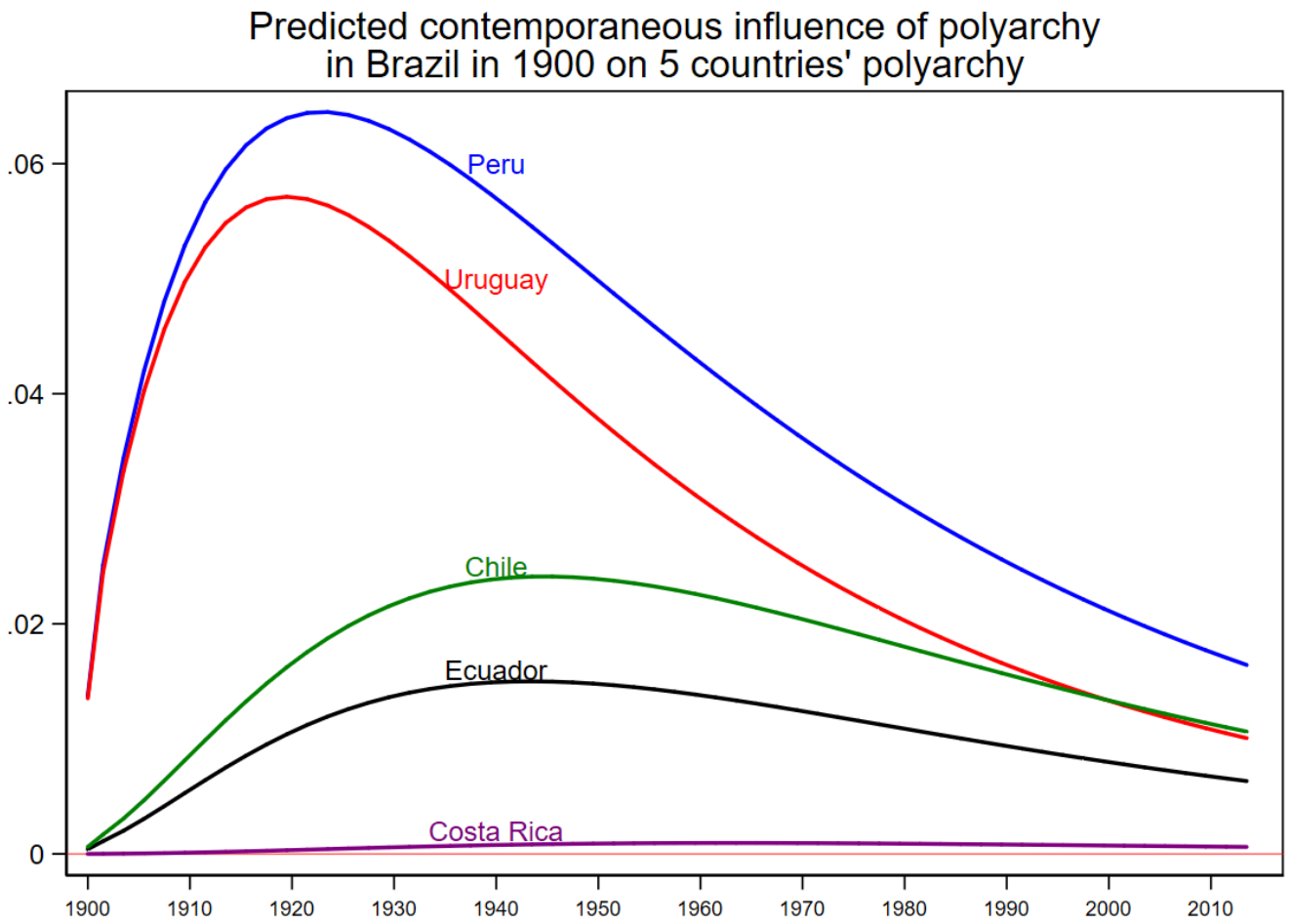

Predicted contemporaneous influence of polyarchy
in Brazil in 1900 on 5 countries' polyarchy
.06
.04
.02
0
Peru
Uruguay
Chile
Ecuador
Costa Rica
1900
1910
1920
1930
1940
1950
1960
1970
1980
1990
2000
2010

Figure 4

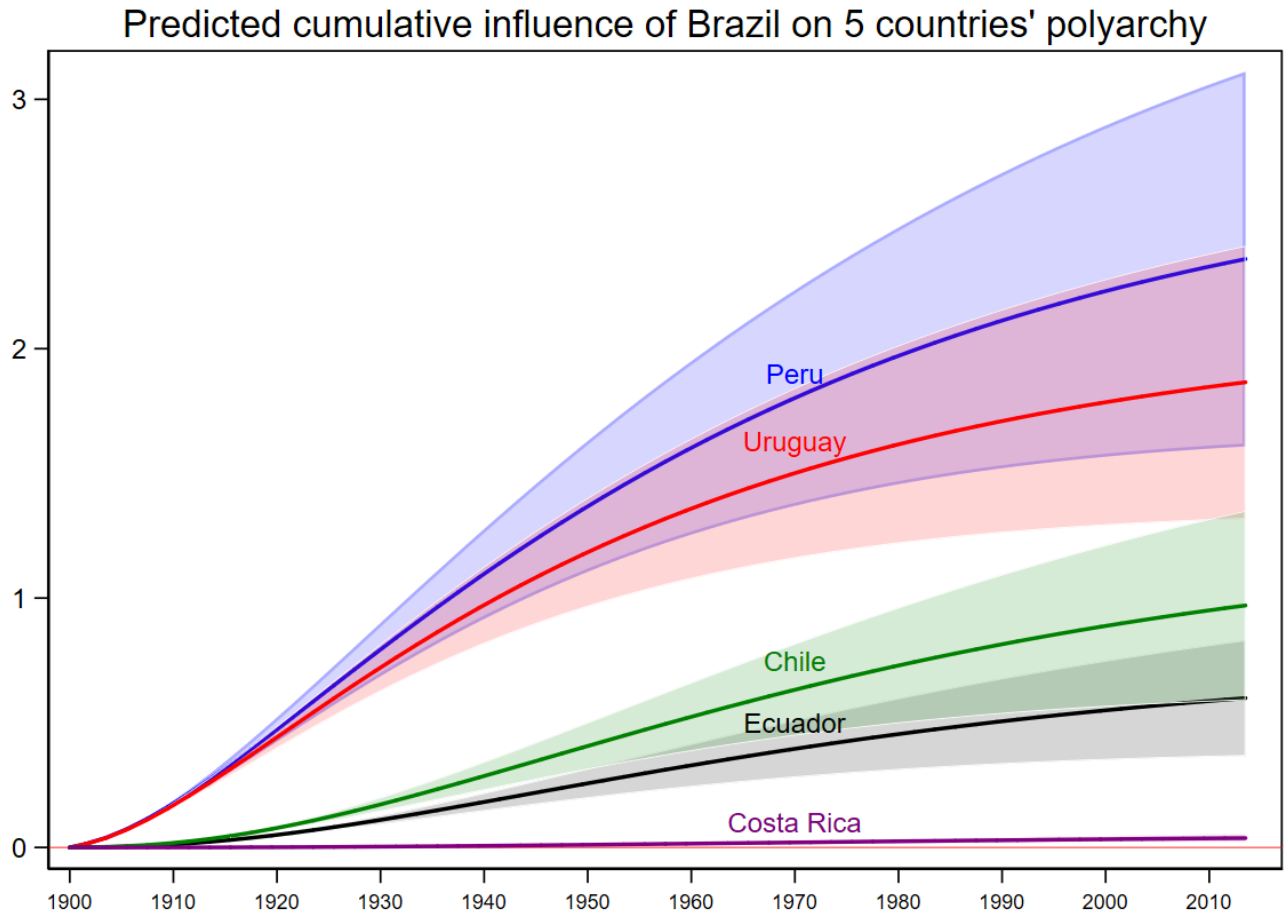


Figure 5

Note: These predicted effects are not on a realistic scale because they are based on the assumption of an impossible unit change in polyarchy in Brazil in each two-year period.

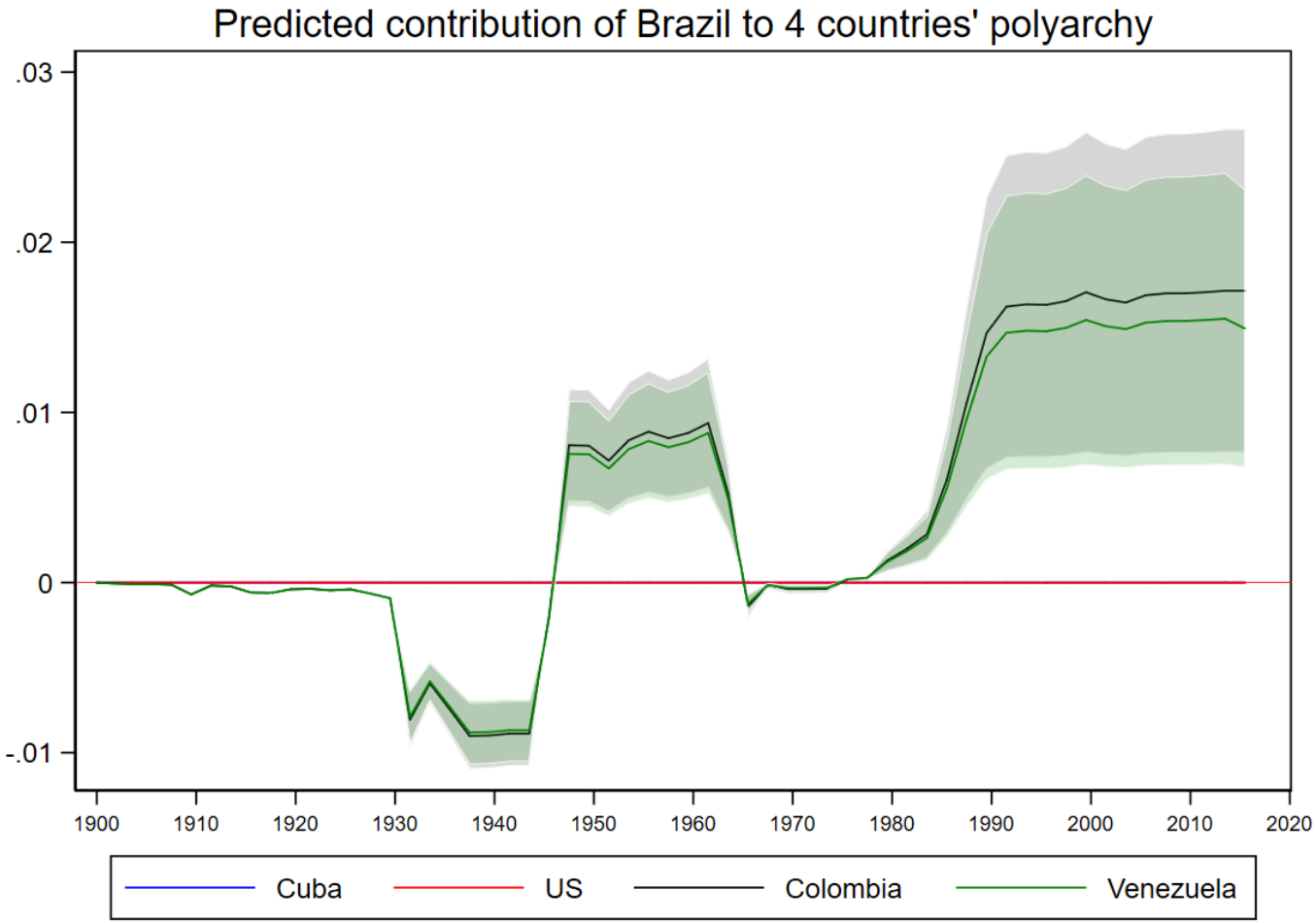

Predicted contribution of Brazil to 4 countries' polyarchy
.03
.02
.01
0
-.01
1900
1910
1920
1930
1940
1950
1960
1970
1980
1990
2000
2010
2020
Cuba
US
Colombia
Venezuela

Figure 6

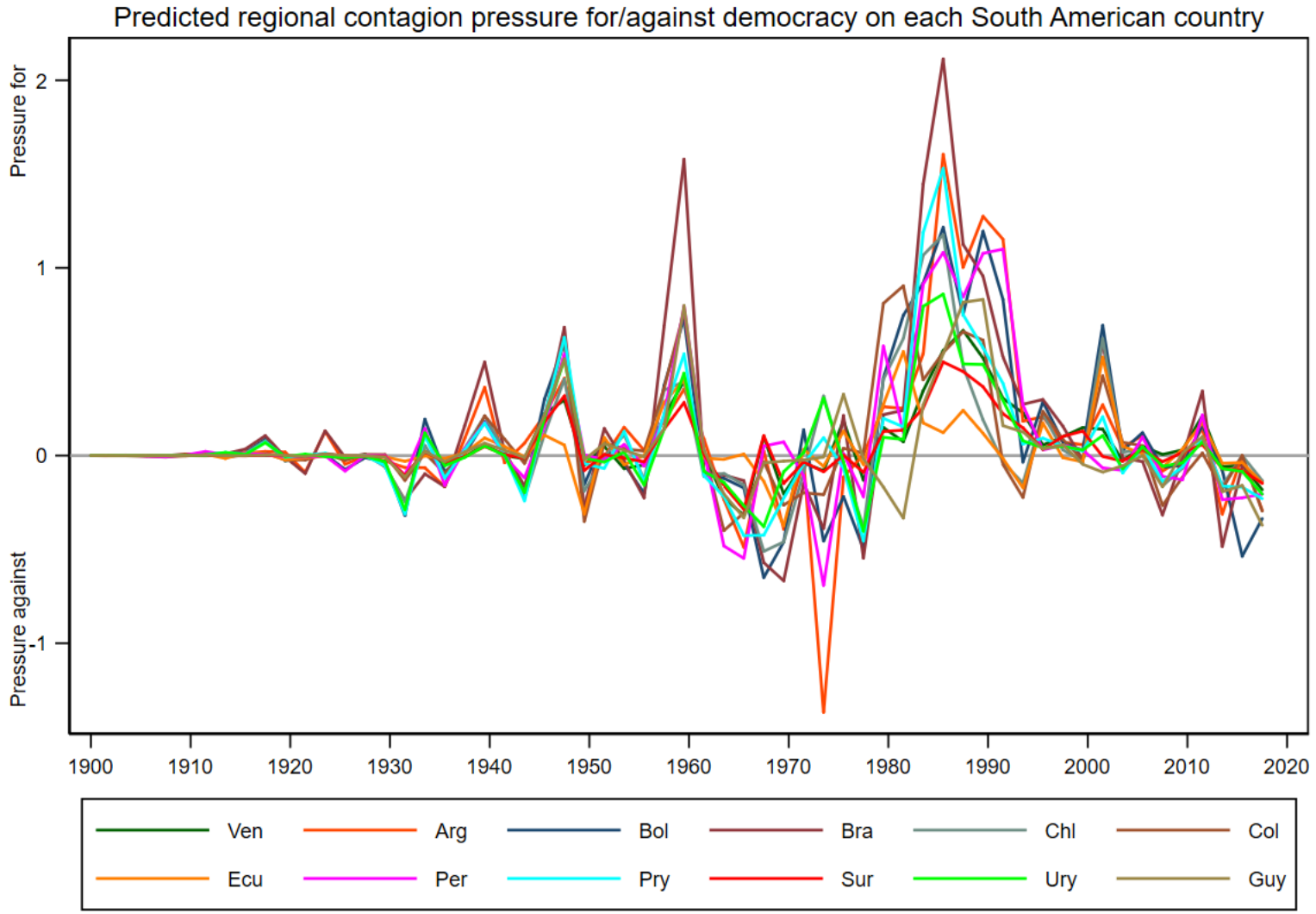

Predicted regional contagion pressure for/against democracy on each South American country
Pressure for
Pressure against
2
1
0
-1
1900
1910
1920
1930
1940
1950
1960
1970
1980
1990
2000
2010
2020
Ven
Arg
Bol
Bra
Chl
Col
Ecu
Per
Pry
Sur
Ury
Guy

Figure 7

Table 1: Comparison of Models with and without Network Relationships

	Level		Change		Upturns		Downturns	
Model	1	2	3	4	5	6	7	8
	without networks	with networks	without networks	with networks	without networks	with networks	without networks	with networks
Domestic covariates								
Lagged DV	**0.898**	**0.896**	**0.200**	**0.199**	**0.250**	**0.240**	**0.208**	**0.200**
Election year	**0.034**	**0.035**	**0.016**	**0.015**	**0.014**	**0.014**	0.001	0.001
GDP per capita	**0.005**	**0.005**	**-0.002**	-0.002	**-0.003**	**-0.003**	**0.002**	**0.002**
Literacy/1000	**0.520**	**0.526**	**0.068**	**0.065**	**0.138**	**0.131**	**-0.080**	**-0.076**
Hyperinflation	**0.021**	**0.021**	**0.014**	**0.014**	**0.015**	**0.014**	-0.002	-0.002
Internal war	-0.005	-0.005	**-0.007**	**-0.007**	-0.003	-0.003	**-0.004**	**-0.004**
Colonizer selection/1000	-0.630	-0.542	-0.108	-0.076	0.084	0.121	-0.212	-0.211
Colony selection/1000	**-4.227**	**-3.710**	-1.050	-1.065	-0.539	-0.450	-0.487	-0.476
Latitude	0.000	-0.006	-0.005	-0.004	-0.008	-0.007	0.004	0.004
Port distance/1000	-0.002	-0.002	0.000	0.000	-0.002	-0.002	0.002	0.001
Exogenous shocks								
International war	**-0.019**	**-0.020**	**-0.009**	**-0.009**	**-0.007**	**-0.006**	-0.002	-0.002
Global GDP expansion	**0.980**	**0.978**	**0.580**	**0.575**	**0.507**	**0.478**	0.055	0.062
Global GDP contraction	0.077	0.072	0.044	0.056	-0.092	-0.057	0.136	0.137
Constant	**-0.024**	**-0.024**	**0.015**	0.012	**0.029**	**0.022**	**-0.016**	**-0.013**
Current colony network								
Lagged instrument		0.007		**0.078**		**0.075**		0.015
Network error term		-0.054		-0.038		-0.063		-0.023
Former colony network								
Lagged instrument		0.009		-0.020		**0.054**		-0.007
Network error term		-0.009		-0.011		-0.058		0.012
Neighbor network								
Lagged instrument		**0.015**		**0.198**		**0.350**		**0.335**
Network error term		-0.012		**-0.224**		**-0.325**		**-0.373**
Alliance network								
Lagged instrument		0.006		0.081		**0.192**		**0.139**
Network error term		**-0.101**		-0.157		**-0.253**		**-0.168**
N	6853		6784		6755		6756	

Appendix A: Modeling Spatial Dependence

In spatial econometrics, the standard way to model network relationships is to construct a **W** matrix, which is simply a square matrix with one column and one row for each observation in the dataset (Neumayer and Plümper 2016). To analyze a panel dataset with N countries and T time periods, the W matrix has dimensions NTxNT. In a spatial regression, all spatially dependent terms in the equation are interacted with **W** in order to estimate spatial relationships correctly. The cells (denoted w_{ij}) of the matrix contain zeroes for countries (or country-time periods) that are not linked, and non-zero weights to indicate the weight of each column country relative to each row country. For unweighted relationships, the cells contain only zeroes and ones. The weights indicate only direct (first-order) linkages, such as a country's own contiguous neighbors, which constitute the first spatial lag. However, they also imply indirect (second- and higher-order) linkages, or longer spatial lags, such as neighbors of neighbors, neighbors of neighbors of neighbors, and so on. Spatial lags are different from temporal lags because a country tends to have more than one country in its first spatial lag, while a temporal lag always refers to the same country. Weirdly, every country is one of its own second-order spatial lags, as it is always a neighbor of its neighbors (unless the linkage is unidirectional). This is another difference with temporal lags, as time flows in only one direction and therefore does not circle back on itself. In spatially lagged relationships, impacts reverberate among countries at a diminishing rate until they die out.

The spatial lag coefficient, rho, captures only the direct impact of a spatially lagged variable. The full coefficient, usually called the "steady-state" effect, is the sum of all of the exponentially diminishing spatial lags. This is somewhat analogous to the long-run effect, phi, of a temporal lag. In both cases, one divides the reported coefficient by one minus the lag coefficient to obtain long-run or steady-state (or in the presence of both temporal and spatial lags, long-run steady-state) coefficient. If rho is large, the spatial dependence diminishes slowly and the cumulative effect is large. If rho is small, the spatial dependence may die out almost immediately. If so, it may not be useful to model more than the first lag, as Leeson and Dean (2009) did despite using a **W** matrix. Of course, it is always possible that a spatial lag is not significant at all.

In non-spatial regression, we interpret coefficients as the average effect of a unit change in the independent variable, other things being equal. In spatial regression, the effects depend on multiple source countries, and each target country is often linked to a different set of source countries. For this reason, the "effect" is not a single number, but an NTxNT matrix of observation-specific effects. In the Delta Method, one interprets effects as the propagation of a unit-change shock through the network. The effect is the sum of the diminishing spatial lag coefficients multiplied by a unit change in the spatially lagged variable for every linked observation.

A final difference between our approach and those used in the existing literature is that we test multiple **W** networks head-to-head. It is important to do this because network memberships can overlap: countries tend to ally with neighbors, trade with allies, share a language with their former colonial power, be contiguous with other former colonies of the same power, and so on. When modeled separately, each network is likely to claim some of the credit that properly belongs to overlapping networks. Modeling them together helps sort out which networks matter the most. We also report a total effect of all networks working together as a way of assessing the

relative importance of domestic and international variables. Conveniently, the average indirect effect for multiple networks and a temporally lagged dependent variable is rho divided by one minus each of the first-order spatial and temporal lag coefficients (Franzese and Hays 2008b, 24):

$$1 - \rho_1 \boldsymbol{W_1} - \rho_2 \boldsymbol{W_2} - \ldots - \rho_k \boldsymbol{W_k} - \varphi \quad (1)$$

Appendix B: Other Networks

We expect that military alliances matter in a few distinct ways. First, the prospect of enhancing security is a powerful incentive for joining a defensive alliance. Some important alliances, formally or informally, make a certain type of regime a prerequisite for membership, so countries may evolve toward that kind of regime before joining. For example, NATO expects prospective member countries to move toward democracy (although Portugal, Greece, and Turkey have not always conformed to this expectation). This expectation turned into a formal requirement following the Cold War (Schimmelfennig 2007). In the Warsaw Pact, it was the opposite, as both the eastern alliance structure and the non-democratic regimes were imposed by the Soviet Union. In both cases, influential groups were comfortable with these international alignments and would have resisted promising to defend a country with a very different regime (Siverson and Emmons 1991). Second, once in the alliance, alliance members often exert pressure on other member countries to bring their political regimes into conformity with their own, whether democratic or nondemocratic, through means ranging from quiet diplomacy, to public admonishments, to sanctions, to invasion (as in East Germany, Hungary, and Czechoslovakia). In general, alliance networks can exert pressure ranging from subtle, informational linkages that encourage convergence to forceful, coercive convergence. These theoretical expectations are in line with recent findings casting doubts on the democratic peace literature, specifically that democracies do not make war against other democracies precisely because they tend to be allies and under a powerful ally's protection (McDonald 2015).

Several different causal mechanisms could be relevant in colonial networks. If there is convergence between colonizers and (former) settlement colonies, then the settlers themselves and their ties to the mother country presumably play the main role of transmitting ideas, institutions, and norms, as well as a common language and sometimes religion to the colony. Even beyond the colonies of settlement, however, colonizers and colonies are often linked by language and religion and with them, easy access to literature, news, and entertainment. These cultural ties could also encourage trade and investment. Colonial elites have often been educated in the colonizing country. In some empires, institutions such as courts, elections, and legislatures were transplanted to the colonies before independence. The most dramatic forms of influence have been economic sanctions and military intervention in former colonies. Such actions reached their peak before independence, with the British in India, the Portuguese in Africa, and the French in North Africa; but France continues to intervene militarily in its former West African colonies, most recently in Mali.

There are, however, other possible mechanisms that would lead colonizers and colonies to diverge in their levels of democracy. Much of the literature on colonialism emphasizes the exploitative nature of these relationships (Wallerstein 1974, Cardoso and Faletto 1979, Acemoglu et al. 2001, Lange et al. 2006, Mahoney 2010). The motivation for colonization was not to spread democracy, but to bring economic benefits to the colonial powers. They, or private firms chartered by them, extracted immense mineral wealth from some colonies and purchased agricultural products from others at artificially low prices. In order to maintain control over colonial territories and populations, colonizers appointed governors who ruled in authoritarian and sometimes violently brutal ways. In the colonies of occupation, colonizers ruled indirectly through local elites, who thereby became less accountable to their own communities. Although France and Portugal considered their colonies overseas territories in a unified empire and even granted them representation in the national parliament (when there was an elected parliament), both states created a second-class "indigenous" citizenship for colonial peoples who were not descended from settlers (Owolabi 2010 and 2012). In the most extreme instances, colonizers imported enslaved Africans to provide a workforce for the most difficult and dangerous labor. In sum, at a time when Europe was moving slowly and with fits and starts from absolute monarchy toward proto-democratic systems, its colonial populations were being subjected to profound economic, social, and political inequalities. There are good reasons to expect that the net impact of colonial rule may have caused political development in Europe and its colonies to diverge.

Appendix C:

Identification Strategy

[In progress for new model; previous procedure is described in this section.[1]]

One of the formidable challenges of making causal inferences about the impact of diffusion networks on democratization is the fact that network membership was not randomly assigned to countries. Take colonial rule as an example.[2] There were systematic differences not only between colonizers and colonies, but also between advanced countries that built empires and those that did not, and between developing regions that were selected for colonial rule and those that maintained their sovereignty. If these differences are correlated with subsequent democratization, then inferences about the relationship between colonial rule and democratization will be biased. However, if we can correct for this selection bias and show that the actual and potential colonies and colonizers are balanced with respect to relevant covariates, then we can consider assignment to colonial rule an "as if random" treatment and make comparisons across these groups that come closer to causal inferences.

The set of 53 non-colonial reference countries is large and diverse enough to contain countries such as Russia, Finland, Denmark that have much in common with actual colonizers and countries such as Liberia, Thailand, Iran, and Afghanistan that are in many ways similar to

[1] For the estimates in progress for this version, we simply carried forward the v7.2 Heckman correction factors, as they are constant over time.

[2] We have developed corrections for selection bias only for the colonial networks so far. We intend to develop them for other diffusion networks eventually.

actual colonies.[3] In fact, under different definitions of what a "colony" is, some of them could be considered colonizers or colonies.[4] The selection bias therefore lies not so much between colonizers and colonies as it does between actual or potential colonizers, on the one hand, and actual or potential colonies, on the other. This is the kind of selection bias we take pains to correct.

To do so, we use Heckman models to estimate separately the probabilities of either being a colonizer or being a colony.[5] The process of colonization was not randomly assigned to territories, and thus the distribution of colonizers and colonies throughout the world could have been determined by various characteristics that either allowed for colonization or inhibited colonization. Controlling for this form of selection bias helps create more conservative estimates of the diffusion effects, as it takes into account this non-random assignment of colonization. To correct for this bias, we follow the two-step Heckman selection procedure (Heckman 1979). First, we produce two first-stage probit models that predict the probability of being either a

---

[3] The reference countries are Afghanistan, Albania, Armenia, Austria, Azerbaijan, Belarus, Bosnia and Herzegovina, Bulgaria, China, Croatia, Czech Republic, Denmark, Estonia, Ethiopia, Finland, Georgia, German Democratic Republic, Greece, Hungary, Iceland, Iran, Ireland, Kazakhstan, North Korea, South Korea, Kosovo, Kyrgyzstan, Latvia, Liberia, Lithuania, Macedonia, Moldova, Mongolia, Montenegro, Nepal, Norway, Poland, Romania, Russia, Saudi Arabia, Serbia, Slovakia, Slovenia, Sweden, Switzerland, Taiwan, Tajikistan, Thailand, Turkey, Turkmenistan, Ukraine, Uzbekistan, and Yemen.

[4] There are two reasons for not considering other empires, such as Imperial Russia and the Soviet Union, the Ottoman Empire, and the Austro-Hungarian Empire, as colonial networks. First, we see a distinction between governing distant, non-contiguous territories, which we consider colonies, and dominating territories that are incorporated into, or continguous to, the homeland, although the latter are certainly hierarchical relationships that also have the potential to affect subsequent democratization. The second reason is that the V-Dem project does not provide separate ratings for the subunits of the Russian, Austro-Hungarian, or Ottoman Empires, so we would not be able to test these relationships anyway. However, it would be possible to examine the network linking the Soviet Union to the Warsaw Pact countries of Eastern Europe, which have been coded by V-Dem. We do this, in effect, both in our test of neighbor networks (which are the strongest networks we have yet found) and in a separate analysis of the diffusion of democracy through military alliances. The latter confirms that this type of network also helps explain democratization and erosion. We consider the Japanese domination of East and Southeast Asia before and during the Second World War to have been a series of military occupations rather than a colonial empire. In preliminary models, we tested for military occupation, including the Japanese cases, and could never confirm a significant independent effect on democratization.

[5] Unlike Brinks and Coppedge (2006), this first-stage probit model does not correct for selection bias that could be due to factors that inhibit countries from changing. We have estimated such a model (adapted to our continuous dependent variable), and it yields some interesting conclusions about the conditions under which countries change their level of electoral democracy more than an insignificant amount. However, this correction does not significantly or substantively alter estimates in the main model, probably because we use a continuous rather than ordinal variable and because the differenced dependent variable controls for stasis very well.

colonizer or a colony (see Tables A1 and A2 and Figure A2). For the colonizer selection probit, we estimate the probability of a country being a colonizer based on distance from the equator, whether the country is an island, the average elevation of the country (Gallup et al. 1999), the year the country enters the dataset, and whether the country had been a great power at any time since 1815 (Levy 1981).[6] For the colony selection probit, we similarly estimate the probability of a country having been a colony or a part of a colonial territory based on distance from the equator, percentage of the country in the tropics (Gallup et al. 1999), the average elevation of the country, whether the country is an island, whether the country neighbors a colony, and whether the country was a great power at any time since 1815. Both models fit our data well and for the most part accurately predict whether a country is more or less likely to be a colony and whether a country is more or less likely to be a colonizer. After producing the probit model predictions, we calculate the inverse Mills ratios for both the colonizer and colony selection models and produce two selection variables to help control for selection bias in the model.[7] Figure 4 displays the probabilities of being a colonizer or a colony for all the reference countries. We incorporate this information into our multilevel model of polyarchy change to correct for selection bias.

This correction works well enough to justify treating the reference countries as comparable to the imperial countries. The groups can be considered comparable when they are not significantly different, on average, with respect to any exogenous covariates that might be correlated with both colonial rule and democratization. Table 1 shows that we have met this important criterion quite well for a broad selection of potentially relevant economic, social, and state capacity covariates. Economic development has long been known to be associated with democracy levels. It is obvious that only wealthy powers could be colonizers and poorer regions tended to become colonies. We measure this likely confounder with ln(per capita GDP) and average height, as an unobtrusive proxy for several aspects of development such as income, nutrition, and healthcare. Another economic factor is reliance on mineral exports, which are widely believed to be a hindrance to democracy (Ross 2001). In many cases, access to gold, silver, other metals, and later, oil, was an important motivation for colonial expansion and resistance to independence. To minimize missing data, we check only oil reserves and oil reserves per land area. Social covariates matter for the same reasons as economic development in general, although the mechanisms are probably slightly different. Here we look at population, urbanization, literacy, education and access to education, life expectancy, and infant mortality. All of these covariates could affect both the probability of being a colony or a colonizer and success at democratization. Finally, we also examine two indicators of civil war on the grounds

[6] Since the traditional measures of state material power does not cover the countries and years of our dataset, we use a dummy variable for great power status since 1815 to capture the dynamic of state power since 1815 in predicting colonization by a country, assuming a certain level of material power is necessary to colonize foreign lands. The first year in the dataset variable helps determine whether the country was in existence during the colonial period. It helps control for the entrance of new countries into the dataset following the dissolution of Yugoslavia, the Soviet Union, and other secessionist events, which are distinct from the decolonization process.

[7] The formula for calculating the inverse Mills ratios is as follows: $\lambda_i(-\mathbf{Z_i\gamma}) = \frac{\boldsymbol{\varphi}(\mathbf{Z_i\gamma})}{\boldsymbol{\Phi}(\mathbf{Z_i\gamma})}$ where φ =the standard normal probability density function and Φ =the standard normal cumulative distribution function.

that any kind of domestic political violence would make it harder to sustain democracy and harder to establish or hold onto a colony.

Taken together, these covariates cover a wide variety of potentially confounding conditions that could bias estimates of the impact of colonial rule on democratization. Table 1 compares the means for actual colonizers, actual colonies, and their respective reference groups after correcting for selection bias.[8] Only one of the 26 differences in means is statistically significant. Some of the differences in group means appear large, but the standard errors are also large, rendering the differences insignificant. The one exception, oil reserves, merits further investigation, such as controlling for this variable in the final model. But in all other respects, it seems to be safe to treat the treatment groups (actual colonizers and colonies) and control groups (potential colonizers and colonies) as balanced. With this correction, we can treat the "assignment" of countries to colonizer or colony status as though it were random.

Appendix D: Estimation

The spatio-temporal autoregressive (STAR) model is expressed as:

$$\mathbf{y} = \rho\mathbf{W}\mathbf{y} + \varphi\mathbf{M}\mathbf{y} + \boldsymbol{\beta}\mathbf{x} + \varepsilon \qquad (2)$$

(Franzese and Hays 2008a; Lesage and Pace 2009, 27). In equation (3), **W** is a matrix of spatial lags and **M** is a matrix of temporal lags. Therefore, ρ is the coefficient of spatial dependence and φ the coefficient of temporal dependence. **x** is a vector of domestic independent variables, **β** is a vector of their coefficients, and ε is the error term. Specifically, because we use instrumental variables to address spatial endogeneity, we specifically use generalized spatial two-stage least squares (GS2SLS), which is very similar to feasible generalized least squares and the spatial generalized method of moments (Franzese and Hays 2008a, Land and Deane 1992). As instruments we use the spatial lags of the non-spatial regressors **x**, which are the "ideal instruments" in many situations because the domestic determinants of democracy in source countries is not likely to directly affect democracy in the target country except through democracy in the source countries.[9] At the specific-country level, when these instruments substitute for polyarchy scores in source countries, equation (3) can be rewritten as

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[8] To compare means while controlling for selection bias, we regressed each exogenous covariate on dummies for the actual colonizers, the actual colonies, the potential colonizers, and the potential colonies in a multilevel model. The second level modeled the country-clustered errors as a function of the selection-correction variables (inverse Mills ratios for the probabilities of either being a colonizer or being a colony). This corrected the group means for selection bias. The group means reported in Table 1 are the intercept for the fixed effects equation plus the corresponding group dummy. The p-values come from t-tests of the significance of the difference between the coefficient for the actual group and the coefficient for the potential group. We tested, for example, whether the difference in mean literacy between actual and potential colonizers is statistically distinguishable from zero.

[9] Spatial lags of the non-spatial regressors would be problematic instruments if democracy in the target country affected domestic determinants of democracy in source countries (for example if

$$y_{it} = \frac{\rho\beta}{1-\rho^2} x_{jt} + \frac{\beta}{1-\rho^2} x_{it} + \frac{\varphi}{1-\rho^2} y_{it-1} + \varepsilon_{it} + \rho\varepsilon_{jt}, \quad (3)$$

for $i \neq j$. Thus if there is spatial dependence on polyarchy, then democracy at home is a function of both covariates at home and those same covariates abroad, and all coefficients must be adjusted by multipliers.

This setup also makes it possible, if desired, to model spatial dependence in the errors ($\rho\varepsilon_{jt}$). We choose to include this term in order to correct for spatially dependent bias in omitted variables. For example, many domestic conditions help create and preserve high levels of democracy in Western Europe. The only one that we control for is literacy, an imperfect proxy for multifaceted economic and social development. Others are certainly omitted, and we do not wish to falsely attribute their influence to spatial dependence: European democracies propping one another up. The spatial autocorrelation term helps isolate the effects of such omitted domestic variables so that we get a better estimate of the international processes that interest us.

One of our innovations is to test multiple networks of spatial dependence: neighbors, allies, current colonies, and former colonies. Each additional network requires adding another matrix multiplication to the equation. This is computationally costly, but it has a simple impact on the multiplier, which becomes, for the whole sample and k networks (Franzese and Hays 2008b),

$$[\mathbf{I} - \rho_1\mathbf{W_1} - \rho_2\mathbf{W_2} - \ldots - \rho_k\mathbf{W_k} - \varphi\mathbf{Ly}]^{-1}. \quad (4)$$

(**L** is a matrix of temporal lags.)

There is room for improvement in this model. It assumes, for instance, that past polyarchy in country j is exogenous: it influences present polyarchy in country i only through the instrument for present polyarchy in country j. We treat it, in other words, as an instrument, which may or may not be reasonable. Latitude and distance to ports are excellent instruments, as they are obviously exogenous; literacy seems to be a good choice as well, although there is a possibility of reverse causation. But should the time-invariant variables latitude, distance to ports, and the two correction factors be treated as instruments? We did not use them as second-level regressors simply because we do not know how to combine a mixed model with GS2SLS. One would also question whether the time-varying but country-invariant *expand* and *contract* economic shock measures belong in the list of instruments. There are also all the specifications we have not been able to implement yet -- weighting, more disaggregated networks, interactions with crisis, etc. These models simply mark some significant progress on an ongoing journey.

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democratic neighbors raised literacy in a source country but non-democratic neighbors did not) or if there is reverse causality in the target country combined with spatial dependence in democracy.

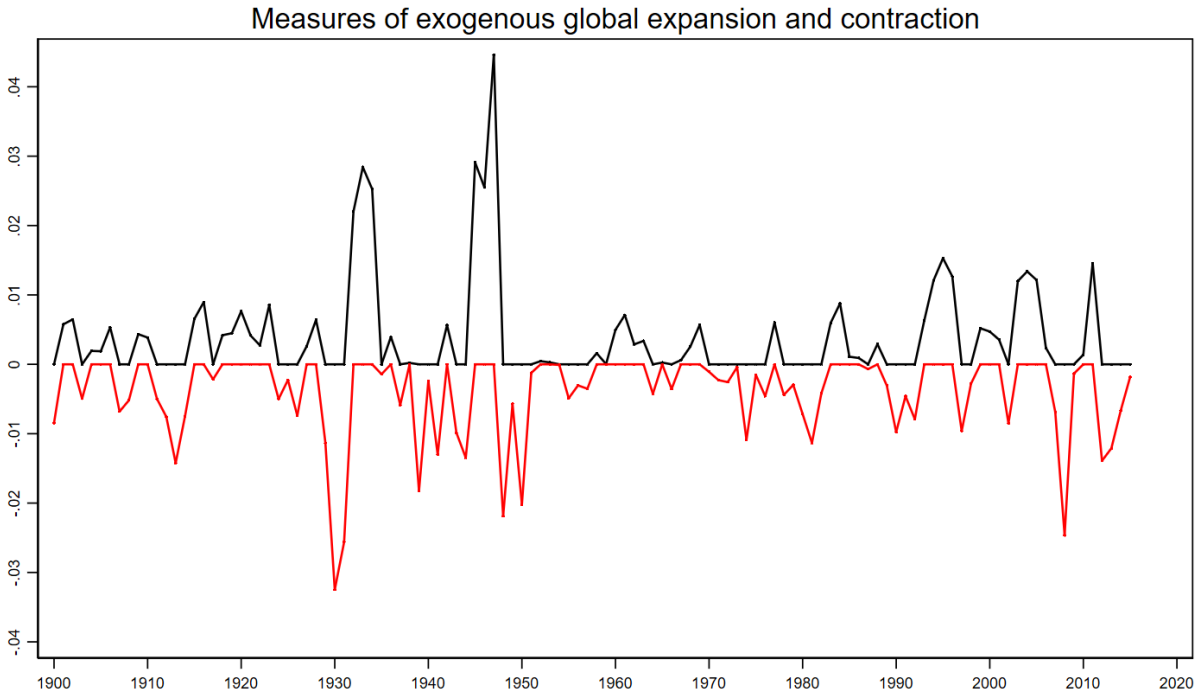

Figure A1: Measures of exogenous global expansion and contraction

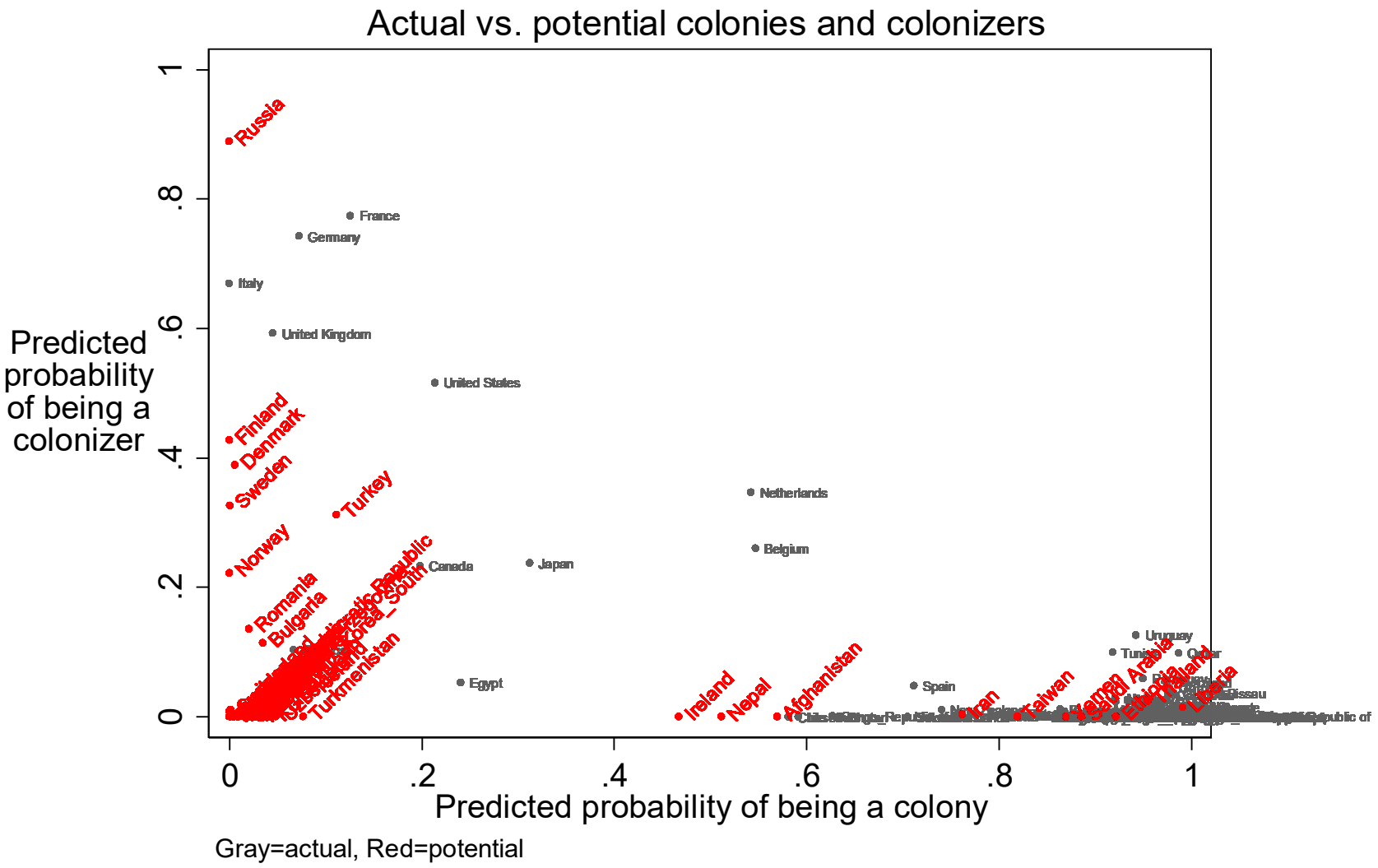
Actual vs. potential colonies and colonizers
Predicted probability of being a colonizer
Predicted probability of being a colony
0
.2
.4
.6
.8
1
Russia
France
Germany
Italy
United Kingdom
United States
Finland
Denmark
Sweden
Turkey
Norway
Netherlands
Belgium
Canada
Japan
Romania
Bulgaria
Egypt
Ireland
Nepal
Afghanistan
Spain
Iran
Taiwan
Uruguay
Qatar
Saudi Arabia
Ethiopia
Liberia
Gray=actual, Red=potential

Figure A2

Table A1: Comparison of Treatment and Control Groups on Potentially Relevant Covariates

		colonizers				colonies			
Covariate	N	treated	control	difference	p	treated	control	difference	p
Economic									
Oil reserves per million square kilometers	9,908	0.92	1.04	-0.12	0.996	7.3	8.86	-1.56	0.884
Mean height (cm)	10,442	173.3	173.3	0.1	0.976	167.7	167.5	0.1	0.904
GDP per capita (natural log)	10,444	9.00	8.32	0.68	0.242	7.41	7.58	-0.17	0.499
Oil reserves	9,649	3.2	16.4	-13.2	0.074	2.2	11.3	-9.0	0.004
Social									
Population (millions)	17,545	50.1	44.5	5.6	0.966	15.9	14.4	1.5	0.937
Educational inequality	11,488	18.8	20.4	-1.6	0.919	56.1	59.7	-3.6	0.631
Urbanization	17,545	0.644	0.450	0.194	0.135	0.286	0.275	0.012	0.841
Average education (years)	14,377	8.31	6.92	1.39	0.379	3.59	3.08	0.50	0.455
Literacy	16,426	93.6	79.9	13.8	0.450	42.8	28.8	14.0	0.069
Life expectancy (years)	14,944	67.0	58.5	8.5	0.133	51.1	51.6	-0.5	0.835
Infant mortality	12,067	47.9	76.5	-28.6	0.261	87.9	100.4	-12.6	0.247
State capacity									
Civil war	10,149	-0.005	0.109	-0.114	0.268	0.088	0.076	0.012	0.788
Armed conflict, internal	12,706	0.051	0.232	-0.180	0.051	0.116	0.077	0.039	0.325

"Treatment" groups are actual colonizers or colonies. "Control" groups are countries that were never colonizers or colonies but had at least a 40 percent probability of being in one of these categories, based on latitude, Great Power status, being an island, mean elevation, first year of existence, percent of territory in the tropics, and having a neighbor that is a colony. Using a 30, 20, or 10 percent threshold for the probability of being a colonizer (in order to include a larger set of reference countries) does not change this conclusion. Means for each group were calculated after hierarchically correcting for selection bias using country-level inverse Mills ratios calculated from this model.

Table A2: First Stage Selection Probit Models		
	Colonizer Selection	Colony Selection
Intercept	250.72***	1.91
	68.19	1.20
Latitude	2.98**	-7.10**
	0.89	2.32
Great Power Since 1815	1.80**	-1.49*
	0.68	0.65
Island Country	-1.05	2.28***
	0.62	0.46
Elevation	-0.0013	-0.0008**
	0.0007	0.0003
First Year	-0.13***	--
	0.04	
Percent in Tropics	--	-1.17
		0.67
Neighbors Colony	--	2.36***
		0.46
N	19772	19772
N-countries	172	172
Prob>χ^2	0.000	0.000
Pseudo R^2	.5360	.6571
* p<0.05, ** p<0.01, *** p<0.001		

Table A3: Model of Levels of Polyarchy

Polyarchy level	Coef.	s.e.	z	p
Domestic covariates				
Lagged level	**0.896**	0.006	152.72	0.000
Election year	**0.035**	0.004	8.77	0.000
GDP per capita	**0.005**	0.002	3.05	0.002
Literacy/1000	**0.526**	0.053	10.01	0.000
Hyperinflation	**0.021**	0.007	3.03	0.002
Internal war	-0.005	0.004	-1.17	0.244
Colonizer selection/1000	-0.542	0.448	-1.21	0.227
Colony selection/1000	**-3.710**	1.599	-2.32	0.020
Latitude	-0.006	0.010	-0.53	0.594
Port distance/1000	-0.002	0.004	-0.63	0.528
Exogenous shocks				
International war	**-0.020**	0.006	-3.30	0.001
Global GDP expansion	**0.978**	0.196	4.99	0.000
Global GDP contraction	0.072	0.239	0.30	0.763
Constant	**-0.024**	0.011	-2.15	0.032
Current colony network				
Lagged level*	0.007	0.007	0.94	0.350
Network error term	-0.054	0.024	-2.31	0.021
Former colony network				
Lagged level*	0.009	0.006	1.59	0.111
Network error term	-0.009	0.018	-0.51	0.612
Neighbor network				
Lagged level*	**0.015**	0.006	2.50	0.012
Network error term	-0.012	0.020	-0.62	0.532
Alliance network				
Lagged level*	0.006	0.008	0.76	0.446
Network error term	**-0.101**	0.046	-2.21	0.027

Wald chi-sq test of spatial terms: 23.59, df=8, p=0.0027
N=6853, Wald=62801.92, p=0, Pseudo R-sq=0.900
*Instrumented

Table A4: Model of Change in Polyarchy

Change in polyarchy	Coef.	s.e.	z	p
Domestic covariates				
Lagged change	**0.199**	0.012	16.99	0.000
Election year	**0.015**	0.003	6.06	0.000
GDP per capita	-0.002	0.001	-1.71	0.086
Literacy/1000	**0.065**	0.031	2.10	0.036
Hyperinflation	**0.014**	0.004	3.16	0.002
Internal war	**-0.007**	0.003	-2.69	0.007
Colonizer selection/1000	-0.076	0.285	-0.27	0.790
Colony selection/1000	-1.065	1.009	-1.06	0.291
Latitude	-0.004	0.006	-0.69	0.487
Port distance/1000	0.000	0.002	-0.11	0.915
Exogenous shocks				
International war	**-0.009**	0.004	-2.29	0.022
Global GDP expansion	**0.575**	0.126	4.57	0.000
Global GDP contraction	0.056	0.153	0.37	0.714
Constant	0.012	0.007	1.80	0.072
Current colony network				
Lagged change*	**0.078**	0.035	2.21	0.027
Network error term	-0.038	0.057	-0.66	0.511
Former colony network				
Lagged change*	-0.020	0.022	-0.90	0.368
Network error term	-0.011	0.031	-0.36	0.720
Neighbor network				
Lagged change*	**0.198**	0.042	4.71	0.000
Network error term	**-0.224**	0.049	-4.61	0.000
Alliance network				
Lagged change*	0.081	0.066	1.22	0.224
Network error term	-0.157	0.090	-1.74	0.083

Wald chi-sq test of spatial terms: 41.36, df=8, p=0
N=6784, Wald=456.72, p=0, Pseudo R-sq=0.058
*Instrumented

Table A5: Model of Upturns in Polyarchy

Upturn in polyarchy	Coef.	s.e.	z	p
Domestic covariates				
Lagged upturn	**0.240**	0.011	21.11	0.000
Election year	**0.014**	0.002	7.08	0.000
GDP per capita	**-0.003**	0.001	-4.18	0.000
Literacy/1000	**0.131**	0.023	5.68	0.000
Hyperinflation	**0.014**	0.003	4.31	0.000
Internal war	-0.003	0.002	-1.44	0.149
Colonizer selection/1000	0.121	0.213	0.57	0.569
Colony selection/1000	-0.450	0.751	-0.60	0.549
Latitude	-0.007	0.005	-1.54	0.124
Port distance/1000	-0.002	0.002	-0.94	0.347
Exogenous shocks				
International war	**-0.006**	0.003	-2.12	0.034
Global GDP expansion	**0.478**	0.095	5.01	0.000
Global GDP contraction	-0.057	0.116	-0.49	0.623
Constant	**0.022**	0.005	4.18	0.000
Current colony network				
Lagged upturn*	**0.075**	0.030	2.51	0.012
Network error term	-0.063	0.052	-1.20	0.229
Former colony network				
Lagged upturn*	**0.054**	0.023	2.35	0.019
Network error term	-0.058	0.037	-1.58	0.114
Neighbor network				
Lagged upturn*	**0.350**	0.036	9.86	0.000
Network error term	**-0.325**	0.044	-7.44	0.000
Alliance network				
Lagged upturn*	**0.192**	0.053	3.62	0.000
Network error term	**-0.253**	0.084	-3.01	0.003

Wald chi-sq test of spatial terms: 152.52, df=8, p=0
N=6755, Wald=935.32, p=0, Pseudo R-sq=0.095
*Instrumented

Table A6: Model of Downturns in Polyarchy

Downturn in polyarchy	Coef.	s.e.	z	p
Domestic covariates				
Lagged downturn	**0.200**	0.012	16.9	0.000
Election year	0.001	0.001	0.74	0.460
GDP per capita	**0.002**	0.001	2.98	0.003
Literacy/1000	**-0.076**	0.017	-4.55	0.000
Hyperinflation	-0.002	0.002	-0.75	0.454
Internal war	**-0.004**	0.001	-3.05	0.002
Colonizer selection/1000	-0.211	0.152	-1.38	0.167
Colony selection/1000	-0.476	0.536	-0.89	0.374
Latitude	0.004	0.003	1.16	0.245
Port distance/1000	0.001	0.001	1.08	0.279
Exogenous shocks				
International war	-0.002	0.002	-0.97	0.334
Global GDP expansion	0.062	0.068	0.91	0.363
Global GDP contraction	0.137	0.083	1.66	0.097
Constant	**-0.013**	0.004	-3.41	0.001
Current colony network				
Lagged downturn*	0.015	0.043	0.34	0.733
Network error term	-0.023	0.070	-0.33	0.744
Former colony network				
Lagged downturn*	-0.007	0.016	-0.47	0.637
Network error term	0.012	0.025	0.50	0.618
Neighbor network				
Lagged downturn*	**0.335**	0.036	9.37	0.000
Network error term	**-0.373**	0.043	-8.77	0.000
Alliance network				
Lagged downturn*	**0.139**	0.049	2.87	0.004
Network error term	**-0.168**	0.072	-2.33	0.020

Wald chi-sq test of spatial terms: 107.93, df=8, p=0

N=6756, Wald=511.94, p=0, Pseudo R-sq=0.0526

*Instrumented