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Michael Coppedge, Benjamin Denison,
Paul Friesen, Lucía Tiscornia, Yang Xu

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Please address comments and/or queries for information to:

V-Dem Institute

Department of Political Science

University of Gothenburg

Sprängkullsgatan 19, Box 711

405 30 Gothenburg

Sweden

E-mail: contact@v-dem.net

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"International Influence: The Hidden Dimension"[1]

Michael Coppedge
Benjamin Denison
Paul Friesen
Lucía Tiscornia
Yang Xu[2]

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[2] Coppedge and Friesen: Department of Political Science, University of Notre Dame; Denison, Notre Dame International Security Center; Tiscornia, Centro de Investigación y Docencia Económicas (CIDE): Yang Xu, Center for Social Science Research, University of Notre Dame. We are deeply indebted to Robert Franzese for his ICPSR workshop on "Spatial Econometrics," including permission to adapt a post-estimation Stata do-file he wrote with Jude Hays and Scott Cook; and to Nick Ottone, Jack Stump, and Jacob Turner for research assistance. We are also grateful to the Kellogg Institute's Research Cluster on Democratization Theory for many discussions and to Vanderbilt University's Center for the Study of Democratic Institutions for support during Coppedge's research leave in spring 2021, in particular to Vanderbilt colleagues Joshua Clinton, John Sides, Kristin Michelitch, Noam Lupu, and David Lewis.

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 influences that lie outside national borders. A major reason for studying international influences is Galton's Problem, which cautions that domestic determinants may receive too much credit for democratization if we ignore international determinants. For example, Brinks and Coppedge argued that "If, as all the prior research and our own results suggest, diffusion is an important phenomenon, then researchers who ignore diffusion risk exaggerating the impact of domestic determinants" (Brinks and Coppedge 2006, 466). 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. We then test selected hypotheses about exogenous shocks and contagion – the spread of democracy outcomes from country to country through various international networks. Surprisingly, contagion at first appears to be real but so small that it could be ignored when studying domestic influences. However, for some kinds of contagion our analysis implies that the long-run effects grow quite large and must be taken into account if we want to understand how democracies develop and decline. In fact, our estimates suggest that the average West European/North American country was about 0.20 points more democratic over the 1900-2010 period than it would have been without other democracies as neighbors. This paradox leads us to conclude that international influences are a hidden dimension of democratization.

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. 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. We show that two kinds of exogenous shock matter: International war undermines and global economic expansions enhance levels of electoral democracy and upturns. However, neither has an effect on downturns. We also show that all of the four networks that we test – contiguity, military alliances, current colonial ties, and former colonial ties – channel contagion in the short term for at least one of the three outcomes, and contiguity matters for all of them.

Some of the contagion effects that we report here are large in the long term. However, their importance is difficult to discern from the regression results, for two reasons. First, the regressions report only short-term effects, which may be small because they are averages over a very large sample in which nearly all the country-to-country effects are zero. These averages can hide short-term local effects that can be larger. For example, below we show that contagion among neighbors is stronger and more certain in some regions than it is in others, and the implications of these relationships are quite different for different countries even in the same region.

Second, the small effects reported in regression tables are just the most proximate and immediate effects; they grow stronger as they reverberate through the international system and accumulate over several years. The long-term effects become evident only when we simulate the predictions of our regressions. Paradoxically, as long as researchers limit their attention to global averages of short-term relationships, it is probably unnecessary to take any international influences other than exogenous shocks into account: estimates of domestic determinants are not likely to be biased in such models. However, a fuller understanding of democratization requires taking the long-term and local relationships into account.

4.1 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, 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 shared languages, 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). This clustering is statistically significant.[3] 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. In addition, 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.This process was followed by some dramatic improvement after the First World War; then the well-known setbacks of the Interwar Period and a new wave of democratization after the Second World War; then another reverse wave starting around 1960, followed by the celebrated Third Wave starting in the late 1970s, with great acceleration from 1989 until 1993. 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 linkage among countries.

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 rights. Whitehead (2001), although he takes a sober, cautionary view of international influences, distills lessons from a collection of regional surveys and case studies, and 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).

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[3] 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 ($\text{Chi-sq}_{1, 169} = 188.16$, $p<.00005$).

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.

4.2 Theoretical Framework[4]

Theories about how and why external factors matter for democratization – the causal mechanisms – are rarely 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 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

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

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 clustered domestic causes and strictly international causes are easily confused because they often have very similar effects. Distinguishing between them is therefore an essential goal of our analysis (Houle et al. 2016).

We make further distinctions between exogenous shocks, endogenous networks, and exogenous networks. Exogenous shocks are international influences that are typically not attributable to a specific source country; rather, they are emergent properties of the international system.[5] This kind of influence leads countries to act in similar ways around the same time, but not because they are influencing one another; they are all simply reacting to the same external stimulus. Good examples of shocks from the international system are global economic crises, world wars, and pandemics. These are unidirectional relationships in which influence flows only from a diffuse international source to a group of countries, never – even indirectly – from targets to sources. This is why they are safely treated as exogenous.

Networks are sets of ties linking countries. Normally they are multidirectional relationships in which countries are both sources and targets, so 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, exogenous and endogenous 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

[5] "Exogenous shock" is a term of art in econometrics for a stimulus that is assumed to originate outside the unit that receives it and is therefore not caused by characteristics of the unit itself. That assumption may not be perfectly true. In a world war, each combatant may have played a role in initiating or responding to attacks; in a global depression, each country's economic decline contributes to the shrinking of the world economy; in a pandemic, each country plays a role in spreading or containing the sickness. However, when such outbreaks of war, depression, or illness involve many countries and develop quickly, each country's contribution to the process is small, and in a century-long panel study the outbreaks happen relatively suddenly, in one or two years. For these reasons we think it a reasonable simplification to treat such shocks as exogenous.

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.[6] Perhaps countries adopting Huawei's 5G network will be connected in politically relevant ways. The Organization of African Unity probably matters more for democratization than Association of Southeast Asian Nations (ASEAN) does, but probably less than the European Union.

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 multidirectional colonial relationships, 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.

However, some networks are exogenous. This is possible when a dominant country in a network affects all the other countries but they do not affect either the dominant country or one another: it is a single hierarchy. Exogenous networks, which are not tested here, would include democracy promotion programs, sanctions, occupation, and unilateral domination of colonies by a colonizer.

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,

[6] In this regard, our approach differs fundamentally from the fast-growing inferential network analysis approach, which uses exponential random graph models (ERGMs) primarily to explain network structures (Cranmer et al. 2021). We take the networks as theoretically given and focus on estimating their effects on outcomes.

the unemployment rate, shrinkage of GDP, and loss of access to capital markets; in pandemics, numbers of infections, hospitalizations, and deaths. For international influences channeled by networks, we think it is useful to use the outcome in the source countries – here, levels, upturns, and downturns in electoral democracy – as the stimuli. Therefore, we use our networks to model "contagion," a common term for outcomes "there" affecting outcomes "here" (Li and Thompson 1975). If we modeled independent variables in one country affecting 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. In this chapter the stimulus is an increment to the democracy level or upturn or downturn 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. Although we consider network weighting a promising avenue for future research, all analyses in this chapter are unweighted.[7]

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)

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[7] Technically, our spatial weights are row-weighted, meaning that each source country's weight is divided by the number of countries directly linked with the target country so that the sum of the weights of source countries is 1. Row weighting is not the only or necessarily the best weighting scheme, but because of its simplicity it is the most common one.

with a certain stimulus and (4) an appropriate weighting scheme.[8] 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 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).[9] 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. Spatial econometrics is a recent adoption in 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).

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[8] 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.

[9] 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.

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 aftermath of a hegemonic shock. However, 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. However, he specified all of these factors as unidirectional, and therefore exogenous, relationships. Sanctions and occupations probably are but others probably involve some endogeneity.

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.

4.3 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 and international economic shocks, and if we included them here, invasions, 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.[10] As we will see, the same model implies different effect sizes for different countries, depending on how they are linked to other cases. 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

[10] 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.

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.[11] 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 "there" (in country$_j$) to explain democracy "here" (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. Our 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.

As far as we know, no other published work has yet used instrumental variables to correct for endogeneity when analyzing international influences on democracy. 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.

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[11] 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."

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, 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 relatively fixed effects for these countries that would appear to have the same consequences. Short-term effects would be much less confounded. We therefore estimate short-term effects and simulate their long-term consequences, which decay as time passes.

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 also 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, are more sensitive to the modest effects that international factors are likely to have.

We cannot completely eliminate all the competing explanations. One, as we have noted, is Galton's Problem: the difficulty of distinguishing truly international causes from domestic causes that happen to be geographically clustered. We address this threat to inference in two ways. First, we control for selected domestic predictors whether they are spatially correlated or not. Second, our models also correct for network-correlated errors, which helps mop up variance that is due to omitted variables that are geographically clustered. For example, why are so many Western

European countries democratic today? Neighbor influence may play a role, but it is important to distinguish it from the fact that these countries tend to be wealthy, highly educated, Christian or secular, and so on. Countries like these may have been democratic even if they were surrounded by authoritarian regimes. Our models control explicitly for literacy, income, Protestant population, and/or European population, but we cannot control for everything Western European countries have in common. An error term that is designed to be correlated with contiguity helps correct for the influence of the variables that happen to be spatially clustered but are not included in the model. These two strategies are uninformative about the nature of any spillovers from domestic variables "there" to democracy outcomes "here." However, if there are such spillovers, our models help prevent them from biasing our estimates of contagion.

Another competing explanation is that our networks are effects rather than causes of the other variables in the models, a phenomenon called "endogenous network formation." Techniques exist to model network formation (Hays et al. 2010 in spatial econometrics; Cranmer et al. 2021 in inferential network analysis) but we have not availed ourselves of them. However, we think it safe to treat neighbors as an exogenous network. No modern political science variable placed France next to Germany or Burma far from Bolivia. The exploitability of natural and human resources certainly did influence which territories European powers chose to colonize, but colonizers in the 18th and 19th centuries probably did not weigh democracy heavily in these choices given that only proto-democracy existed in Europe at the time. Even so, we include Heckman corrections for membership in the two colonial networks in the upturns and downturns models. However, endogenous network formation is a real concern that could bias our estimates of the effects of alliance membership. Homophily, including similar levels of democracy and shared values, is a known determinant of alliance membership. We therefore interpret effects of alliances cautiously.

4.4 Our Hypotheses

4.4.1 Exogenous Shocks

Exogenous shocks originate outside a target country's borders, but not from any specific country. They are emergent properties of the international system.[12] 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 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 threat, 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.[13]

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,

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[12] 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 and Covid-19 is the obvious current example.

[13] We plan to estimate the impact of the aftermath of war in future research.

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." 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). We expect that exogenous economic expansions are also context-conditional, although we do not model them here.

4.4.2 Endogenous Influences

In this paper we focus on four kinds of networks: neighbors, military alliances, and current and former 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 contagion 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 contagion among neighbors is probably actually contagion within more meaningful networks that happen to be regionally clustered. Ideally the estimated contagion 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 contiguity. Appendix B develops the theoretical expectations about military alliances, current colonial ties, and former colonial ties. We emphasize the neighbor networks throughout this chapter to illustrate endogenous network dependence.

4.5 Operationalizing the Hypotheses

4.5.1 The Dependent Variable and the Sample

Our outcome variables are is the V-Dem Electoral Democracy Index, also known as polyarchy (*v2x_polyarchy*), and two transformations of it: upturns and downturns.[14] Upturns are the positive period-to-period changes in v2x_polyarchy; negative changes are recoded to zero. Downturns are the negative changes, with positive changes recoded to zero. 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 60 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 6,755-7,170, which still covers a range equivalent to more than 13,000 country-years.

4.5.2 Exogenous Shocks

To measure international war, 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*.[15] 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

[14] Unlike some of the other chapters in this book, we omit the change outcome because coefficients of the variables explaining it are almost always very close to the sum of the coefficients of the variables explaining upturns and downturns.

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

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.[16] 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. The economic shocks are lagged one period.

4.5.3 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).[17] 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.[18] 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.[19]

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

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[16] This formula simplifies to ($growth_{t+1} - growth_{t-2}$)/3, where *growth* is the global average by year.
[17] We plan to retest relationships using inverse distances in addition to this classification of neighbors.
[18] 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 neighbors of the United States.
[19] 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.

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.[20] 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.[21]

Our approach is unlike any other in several respects beyond the gap-driven mutual adjustment model. First, we use 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 (or "polyarchy") we use is constructed 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.[22] Therefore, we use a

[20] 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."

[21] 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).

[22] 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,

"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 (before row standardization) represents the existence of a relationship for the two corresponding countries in a given year, and 0 represents the absence of such relationship.

4.5.4 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). The model of levels of polyarchy employs a different selection of variables than the models of upturns and downturns because the latter are much more dynamic outcomes; levels are better explained by less dynamic variables. For example, the model of levels of polyarchy includes measures of the Protestant and European percentages of the population, which regard as proxies for many possible explanations associated with ties to Europe. See chapter 3 for sources.

We use two controls for economic development: estimates of per capita GDP (for upturns and downturns only) and the adult literacy rate.[23] Many have argued that literacy has a relationship with economic development, as 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

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.

[23] 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.

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).[24] We also use a measure of GDP per capita in the upturns and downturns models, based heavily on Farris et al. (2017). See chapter 5 for details.

For the upturns and downturns models, we include Election year: 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. We also include latitude and distance from natural harbors. See chapter 3 for sources. 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-periods. Finally, our models incorporate one or 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.

[24] 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 multiply this amount of computation by the hours it takes to run each model once on Notre Dame's high-performance computing machines.

4.6 Estimation

We estimate the above relationships with a spatio-temporal autoregressive (STAR) model, which is described in detail in Appendix C. It models democracy outcomes in each country as a function of exogenous shocks (international war and global economic expansions and contractions), a set of domestic determinants, and democracy outcomes 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 three times, once for each of the dependent variables: level of polyarchy, upturns, and downturns.

Figure 4.1 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 for simplicity 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 outcome variable, a set of instrumental variables, and exogenous shocks. All predictors of endogenous variables are lagged one period.[25] It also has Heckman correction factors for the probability of being "selected" into the group of colonizers, the group of colonies, or neither.[26]

The model 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

[25] 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$_{t-4}$.

[26] Details about how the Heckman corrections for selection into membership into colonizers and colonies are available on request.

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 4.1 about here]

4.7 Results

Table 4.1 compiles the coefficients for all the STAR regressions and compares them with estimates from models without networks. We modeled three of the four dependent variables that most of 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 understanding long-term trends in democracy and cross-national differences. Models 3-6 analyze upturns, and downturns, which reveal more about short-term, within-country dynamics.

[Table 4.1 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.[27] This comparison is important because one of the concerns about models that ignore spatial relationships has been that they may yield exaggerated estimates of the effect of domestic determinants. Our comparisons suggest that there is little basis for this concern, at least in this application. Although each of these four networks is statistically significant in at least one STAR model, they appear to make very little 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 three variables, with these measures, using this set of independent variables, and every other

[27] 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.

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 outcomes are always the strongest variables in each model, although more for levels than for the more dynamic dependent variables. Election years and, strangely, episodes of hyperinflation, are associated with more positive upturns.[28] Internal war tends to increase the magnitude of downturns, but not levels or upturns. 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 two 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 four of the six models, exogenous shocks affect polyarchy in expected ways. First, international war makes upturns smaller, shifting them toward zero. 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 average rate of global expansion is .037. Multiplied by the coefficient, it would raise the level by .036 on average. Expansion makes the average upturn in polyarchy about 0.018 points more positive on the 0-1 scale. Neither war nor economic expansion has a significant effect on downturns. However, global economic contractions do not hurt, on average.

The four networks channel different contagion effects for different dependent variables. The neighbor network is significant for all three 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 for upturns and downturns. This tendency suggests that the causal mechanisms that alliances employ – diplomacy, bargaining, carrots in the form of inducements and

[28] One possibility is that hyperinflation is correlated with economic crisis, making the input of big players more important when inflation does occur. For example, developing countries that need a bailout from the IMF or World Bank and are subject to conditionality to respect the rule of law and democracy. Aid comes with strings attached.

sticks in the form of sanctions and the threat of expulsion – operate primarily in the short term. All networks have significant effects on upturns. The former colonial network has a significant positive impact on upturns. The same is true for current colonial ties (the "current" signifies the pre-independence period, when colonial rule was still ongoing). These estimates suggest that an upturn in one country tends to add a small increment to upturns in other countries with which it is linked by contiguity, military alliance, or colonial ties.

We present simulations below, but first there is one more model to discuss. As we noted, one possible reason for the small contagion coefficients is that they are the average effect for the entire globe over more than a century. It seems likely that we would find some variation in these effects if we obtained estimates for specific regions or historical periods; or for specific alliances rather than treating all alliances as interchangeable. In this spirit, in Table 4.2 we report estimates for six regional networks consisting of neighbors in Sub-Saharan Africa, Eastern Europe, Latin America, Asia and the Pacific, Western Europe and North America (and Australia and New Zealand) or "WENA," and the Middle East and North Africa ("MENA"). This table confirms our intuitions. First, all of the regional neighbor networks are significant except for the Middle East and North Africa. They are significant even though the model also shows that there is significant spatial clustering of omitted variables in Africa, Eastern Europe, and WENA. Moreover, the magnitude of the network coefficients ranges from 0.016 in Asia to twice that, 0.032, in Sub-Saharan Africa. The effects of neighbors on contagion are not homogeneous across regions.

However, how large any of these effects are, how certain they are, and how long they persist must be explored through simulations that go beyond the immediate, first-order effects represented by regression coefficients.

4.8 Interpretation

It is a paradox that Table 4.1 shows multiple significant network effects, yet the coefficients of the domestic determinants and exogenous shocks are practically the same whether our models include networks or not. 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. Another possible reason is that our instruments for outcomes "there" are the same domestic variables that are specified in the non-spatial models (albeit for different countries). A third possible reason is that because the domestic instruments take on values in countries "there," their contributions are practically uncorrelated with the values of these domestic variables "here."

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 first band of contiguous 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 first- and higher-order spatial 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 effects than the neighbors of neighbors, and so on.[29] In models with both spatial and temporal lags, combined long-run steady-state (LRSS) effects are most relevant. Appendix C contains further explanation.

Because it is difficult to understand what the model says simply by inspecting the network coefficients, simulations are required to interpret STAR model estimates properly.[30] We perform three kinds of simulation: (1) Long-run steady-state (LRSS) estimates of the full effect of a hypothetical shock emanating from one country; (2) Dynamically evolving effects of shocks from

[29] 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.

[30] Strictly speaking, the contributions of each network included in the same model are not separable, as they interact with one another to produce the combined effect. However, we do separate simulations of the effects of each of our four networks even when we estimated them in the same model because the total effect becomes uncertain when some of the networks are not statistically significant. The "noise" of the non-significant networks overwhelms the effects of the significant ones. These simulations therefore model the *marginal* effects for a hypothetical world in which all countries were members of only the one network that is the focus of the simulation: for example, a world in which countries were neighbors but none were allies or ever colonies. Simulated effects would be somewhat different if they were based on a model of only one network at a time. For example, neighbor coefficients for all three outcomes modeled separately would be within 27% (sometimes larger, sometimes smaller) of the coefficients from a model that included all four networks. See Table E1 for all the comparisons. Given that the significant coefficients are usually fairly similar in magnitude whether they are estimated separately or together, we use the pooled-model coefficient estimates in our simulations. In our view, the benefits of controlling rigorously for other networks outweighs the drawbacks of simulating a world that is marginally less realistic.

every country on neighbors in each period; and (3) a snapshot map of the "front lines" of polyarchy contagion among regional neighbors all over the world in a single period.[31]

4.8.1 LRSS Estimates

LRSS estimates show the full effect of contagion after a single hypothetical shock has propagated through the network and decayed over time. The steady-state part is the total of all the first-order (immediate neighbors), second-order (neighbors of neighbors), third-order (neighbors of neighbors of neighbors) effects, and beyond to infinity. With each remove from the first shock, the effect decays exponentially. Because the first-order effects tend to be small to begin with, they decay fast. The spatial propagation is assumed to happen instantaneously in the first period. The long-run part of the LRSS effect is a similar process, but playing forward in time rather than space. However, because the temporal lag coefficient is much larger (around 0.90) in models of levels of polyarchy, this process decays more slowly and therefore cumulates much more. The long-run steady-state impact of an initial shock can be much larger than the coefficient reported by the regression. The notion of a "half-life," borrowed from the physics of radioactive decay, gives us a useful way to understand the long-run impact of an initial shock. The half-life of an effect is the time it takes for half of the long-run effect to be realized. The half-life of the effects of contagion on levels of polyarchy in Table 4.1 is more than 20 years.[32] Because there is tremendous inertia in levels of polyarchy, the influence of contagion is analogous to the pull of a tugboat on an oil tanker: when a country is very undemocratic, it takes a long time to get things moving and make noticeable progress; but once a country gains momentum toward democracy, it also takes a long time for contagion to slow it down and reverse direction. (Other determinants can have larger and more immediate effects.) The temporal lag coefficients in upturns and downturns models are much smaller (0.25 and 0.20) and therefore the temporal effects decay very quickly: half of the effect is realized within a year, 90 percent of it within about three years. These more dynamic processes ramp up and die down quickly.

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[31] The simulation with six regional networks is done properly. Countries with neighbors from one region show the effect of that region; countries with neighbors in more than one region show the effects of all the regions they are linked to.

[32] A half-life of a temporal lag can be calculated as ln(1-proportion of effect remaining)/ln(temporal lag coefficient). In the levels model, this works out to ln(1-.5)/ln(.938) = 10.87 two-year periods, or 21.74 years. It takes more than 70 years for 90 percent of the effect to be realized.

This calculation of the LRSS effect of a hypothetical shock yields nonsensical predictions if the shocks are unrealistic or out-of-sample.[33] Conventional interpretations of the network coefficient based on the notion of a "unit change" are out of the question, as the theoretical range of the polyarchy level variable is 0-1, the real-life range is a bit less, and most actual values are much smaller than one: 0.314 on average over the whole sample. In our simulations, therefore, we use a typical value of the polyarchy level, upturn, or downturn, depending on the model, for each category of network membership.

However, the average outcomes that we can observe for neighbors, allies, and current or former colonies are post-treatment: they already include the effects of network membership that we are trying to estimate. Therefore, we compare the observed outcomes to a simulation of what the outcomes would have been without the network contagion.[34] The differences between the simulated pre-contagion outcome and the actual outcome with contagion show how much contagion matters.

Figures 4.2-4.5 show that in the long run (decades), contagion matters quite a bit for neighbors, less for alliances, and not significantly for colonial ties. The LRSS effects are significant whenever the observed values are outside the range of estimated averages excluding contagion. Specifically,

- Figure 4.2: The neighbor network is the only significant one in the long run for explaining mean levels of polyarchy.
- Figure 4.3: Contagion in the neighbor network nearly doubles the size of upturns. The alliance network has a significant positive effect as well, but not as large. Contrary to the short-term coefficients in Table 4.1, the current and former colonial networks have no significant impact on upturns in the long run.
- Figure 4.4: Both alliances and neighbors increase the size of the average downturn, this time making them larger negative changes. In the neighbor network, contagion once again doubles the effect. Consistent with Table 4.1, neither colonial network significantly augments these changes.

[33] This is because the LRSS multiplier is $1/(1 - \rho\mathbf{W} - \varphi)$, where ρ is the spatial lag coefficient, φ is the temporal lag coefficient, and **W** is the spatial weights matrix. With row-standardization we can replace **W** with 1 for a simple calculation, which makes it easy to see that the multiplier becomes extremely large as $\rho + \varphi$ approaches 1.
[34] We can conceive of the observed level of polyarchy as the level before contagion plus the LRSS effect due to contagion. Therefore, we can calculate the counterfactual shock before contagion as ρ*observed shock/$[1 + (1 - \rho\mathbf{W} - \varphi)]$. We bootstrap these estimates with 5000 draws from the distributions of ρ and φ to get 95% confidence intervals for the outcomes excluding contagion.

- Figure 4.5: In the long run, contagion among neighbors significantly raises levels of democracy in all six regions, including the Middle East and North Africa despite its non-significant short-run coefficient in Table 4.2
- The size of the effect increases nonlinearly as the mean regional democracy level increases. For the least-democratic regions, MENA and Asia, contagion raises the mean democracy level by less than 0.10; for the most-democratic region (Western Europe, Canada, the U.S., Australia, and New Zealand), the increase is nearly 0.22.

[Figures 4.2-4.5 about here]

The significant LRSS effects are much larger than the immediate pre-spatial effects shown in Tables 4.1 and 4.2. Propagation of a shock through space and time augments the effect of network membership. The LRSS effect increases nonlinearly with higher mean outcome values. The more democratic network members are on average, the more of a boost they get. This tendency helps explain the persistent geographic clustering of both democracy and non-democracy. Undemocratic countries are more likely to be stuck at a low level of democracy when they are surrounded by other undemocratic countries that cannot give them a boost. But democratic countries in a democratic neighborhood owe a surprising proportion of their democracy level to the support they receive from their neighbors.

However, it also matters which network is operating. Not just any linkages among countries will produce significant effects: it matters which countries are connected. It also matters which region you look at, as the effects are not uniform. This is mostly a function of the average level of democracy in each region, but it also depends on the neighbor network coefficient for the region. Modeling multiple networks reveals more differentiated and sensitive understanding that simply modeling one big global neighbor network. Similarly, it would probably help to disaggregate military alliances: NATO probably has a different effect than the Warsaw Pact; the OAS and OAU may matter while ASEAN may not; British colonization probably had different effects than Spanish or Portuguese or French.

Contiguity is by far the strongest channel of influence. Again, contiguity is best considered a proxy for many channels of influence that are correlated with distance. Alliances help explain why contiguity matters, but there are probably many other kinds of distance-related ties that matter, yet are omitted from these models: communication of ideas and norms through literature, news, television, movies, and social media; trade; migration and other population flows; etc. We need more research on these specific proximity-based channels of influence.

4.8.2 Dynamic Trends

Figures 4.7-4.11 illustrate how shocks from neighbors are predicted to affect each country's level of polyarchy over time. The shocks we used in this simulation are the expected levels of polyarchy in each neighbor, in each period, as predicted by domestic variables alone. Unlike Figures 4.2-4.5, which simulate the effect of single shock, Figures 4.7-4.11 show what the model expects to happen when each country contributes a new shock – i.e., a domestically predicted level of polyarchy – in each two-year period. Because the shocks are predictions of domestic variables only, they are roughly the instrumental shocks used in the model to estimate contagion among neighbors, and therefore pre-contagion shocks. The effects shown in the graphs are the steady-state expected consequences of these shocks as they develop in each country period by period.

These facet plots use the distinct regional neighbor network coefficients from Table 4.2. Clearly, we are no longer dealing with a model whose predicted effects are a simple function of one variable. These effects are functions of the regional network coefficient, the set of neighbors each country has, how many neighbors it has, how democratic the neighbors are, and how much the temporal lags carry over the previous period's increment into the current period. The confidence bounds around each predicted effect reflect the variances and covariances of several of these conditions. The effects and their certainty therefore vary a great deal across regional networks and by country within them:

- The predicted effects are much more certain in some regions (Sub-Saharan Africa in Figure 4.7, Eastern Europe in Figure 4.8) than in others (Northwest Europe in Figure 4.9).
- The effects are much larger and more certain in some countries (Iran, Morocco, Algeria, Libya) than others (Lebanon, Israel, Jordan, Saudi Arabia) within the same region (Figure 4.11). Note: Because the scale maximums of these figures range from .012 for MENA to .03 for Europe and the Americas, readers must exercise some caution when making comparisons across figures.
- The largest effects are on less-democratic countries bordered by more-democratic ones (Botswana, Zimbabwe, and Mozambique next to South Africa in the 1990s in Figure 4.7). The smallest effects are on non-democratic countries surrounded by other non-democratic countries (countries on the Arabian Peninsula in Figure 4.11). The least certain effects are on

democratic countries surrounded by other democratic countries (Canada in Figure 4.8; Northern Europe in Figure 4.9).

[Figures 4.7-4.11 about here]

4.8.3 Map of Front-Line Contagion Zones

Ultimately, the best way to understand what our model says about spatial relationships is to see the simulated effects on a map such as Figure 4.12. It is important to bear in mind that this is *not* a map of levels of electoral democracy like those many of us have encountered elsewhere; rather, this map shows the neighbor network effect estimates. It does so for just one period, 2009-2010. It cannot show the confidence intervals around the estimates, but it does a good job of showing the geographic locations of the predicted high and low effects. The dark blue zones are regions where pro-democracy influence from neighbors is absent because most of the countries in that zone are highly undemocratic. The most yellow zones are those where neighbors are estimated to exert the strongest influence in favor of electoral democracy. These zones do not necessarily contain the most democratic countries on the map; instead, they are zones receiving the most democratic influence. In this sense, we can call them the front-line zones for contagion.

[Figure 4.12 about here]

Two patterns are most striking about Figure 4.12. First is the contrast between the yellow front-line zones and the dark zones lacking democratic influence. In 2009-2010 (and beginning in the late 1990s) the front-line zones were located in Latin America, Eastern Europe, and Southern and West Africa. The Arabian Peninsula was a kind of impenetrable bastion of non-democracy. The other striking patterns are the bands of supranational color gradients separating the light and dark zones. The positive influences received by Eastern Europe and West and Southern Africa fade dramatically as one moves toward the darkness of the Arabian Peninsula. The degree of international influence transcends the levels of democracy found in individual countries, which would be more variegated. This map reveals that our STAR model does a good job of extracting an elegant spatial pattern from a mass of noisy data.

Furthermore, although Figure 4.12 is a snapshot that cannot show it, the spatial pattern revealed by the model evolves. Few countries were very democratic in 1900 by V-Dem's criteria, so the whole world was dark blue or purple at that time. In the Inter-War Years, light brown zones took shape in the Nordic countries and Canada. After the Second World War, fronts gradually intensified in Western Europe, especially Sweden; then Canada; and briefly in South America. By the mid-1980s, Spain was on the front line as well. In the 1990s, first Latin America, then Eastern Europe, then Southern and West Africa became the leading front-line zones, and the dark blue areas that had covered most of Asia and Africa from the beginning shrank down to the Arabian Peninsula, flanked by purple bands. These macro-historical and geographic trends could become a useful component of explanations for democratization.

Nevertheless, it is important to remember that these simulated effects do not show what actually happened; rather, they show the implications of our models: what would have happened in each case if the model were correct.[35] The simulated effects are analogous to predicted values from a regression, albeit more complex regressions than we usually see. However, we think they give readers a better sense of the kinds of relationships that our models are designed to estimate.

4.9 Conclusions

This chapter produces extensive evidence showing that international influences matter for democracy outcomes such as levels of polyarchy and its upturns and downturns. Our estimates suggest that over many decades, contagion from neighbors has boosted the average level of polyarchy in the most democratic region of the world by an amount equivalent to 20 percent of the entire range of the polyarchy scale. The ties among neighbors and allies have tended to double or magnify by 50 percent the sizes of upturns and downturns, respectively, in the first few years after a shock. Colonial networks may also transmit small increments of upturns from colonizers to colonies and back in the first year; however, their long-run impact is uncertain. Exogenous shocks matter, too. Involvement in international wars tends to lower the level of polyarchy and reduce the size of upturns. Global economic growth, by contrast, tends to increase the size of upturns by a small amount.

These conclusions are based on complex analyses of extensive data V-Dem data, including colonies before independence. We are aware of the risks of large-N quantitative analysis, which

[35] This is why the map shows simulated effects on the Soviet Socialist Republics and South Sudan even before they became independent: the simulated effects depend only on their neighbors' values. Predictions for these cases should not be taken very seriously, but they are the model's attempt to show what the effects of contagion on them would have been if they had been independent. Note: Because the model did not generate this kind of counterfactual prediction for Kyrgyzstan before 1991, we assigned it the same values as Kazakhstan's for those years.

concern all the other chapters in this book and are discussed in the introductory and concluding chapters. In this chapter we attempt to avoid the worst problems by using the latest tools of spatial econometric modeling. First, we interact our variable of interest – democracy outcomes in other countries – with **W** networks, which are the best way to model dependence among units. They are unusually large, in fact global, networks for this type of analysis. Second, rather than testing these networks in simple cross-sections as much of the literature has, we use a STAR model that can also handle panel data, in this case covering more than 100 years of history for the whole world. Third, this is one of very few analyses of democratization that tests multiple networks against one another in order to learn which kinds of ties most powerfully channel international influence. Fourth, our use of a network-correlated error term helps us distinguish true contagion from domestic influences that happen to be spatially clustered. Fifth, we use domestic variables as instruments for democracy outcomes "there" to prevent the endogeneity inherent in our multidirectional networks from confounding our estimates of the truly international effects. Our use of instrumental variables does a much better job of dealing with this inferential threat than the usual practice of trusting temporal lags (which we also use) to do the job. Finally, rather than limiting our interpretations to tables of regression coefficients, we employ computational simulations of long-run steady-state effects of counterfactual shocks. These simulated effects are necessary for understanding the substantive implications of the models. No analysis of observational data can completely eliminate confounded estimates, but we have improved upon the methods used by other researchers addressing this topic.

Paradoxically, controlling for the endogenous network effects does not substantially modify estimates of the impacts of domestic variables. The evidence so far reassures us that models of democracy outcomes that test domestic variables exclusively, like those in the other chapters of this book, do not yield substantially biased estimates. However, there are so many hypotheses about international influences that have not yet been tested that this conclusion is subject to change.

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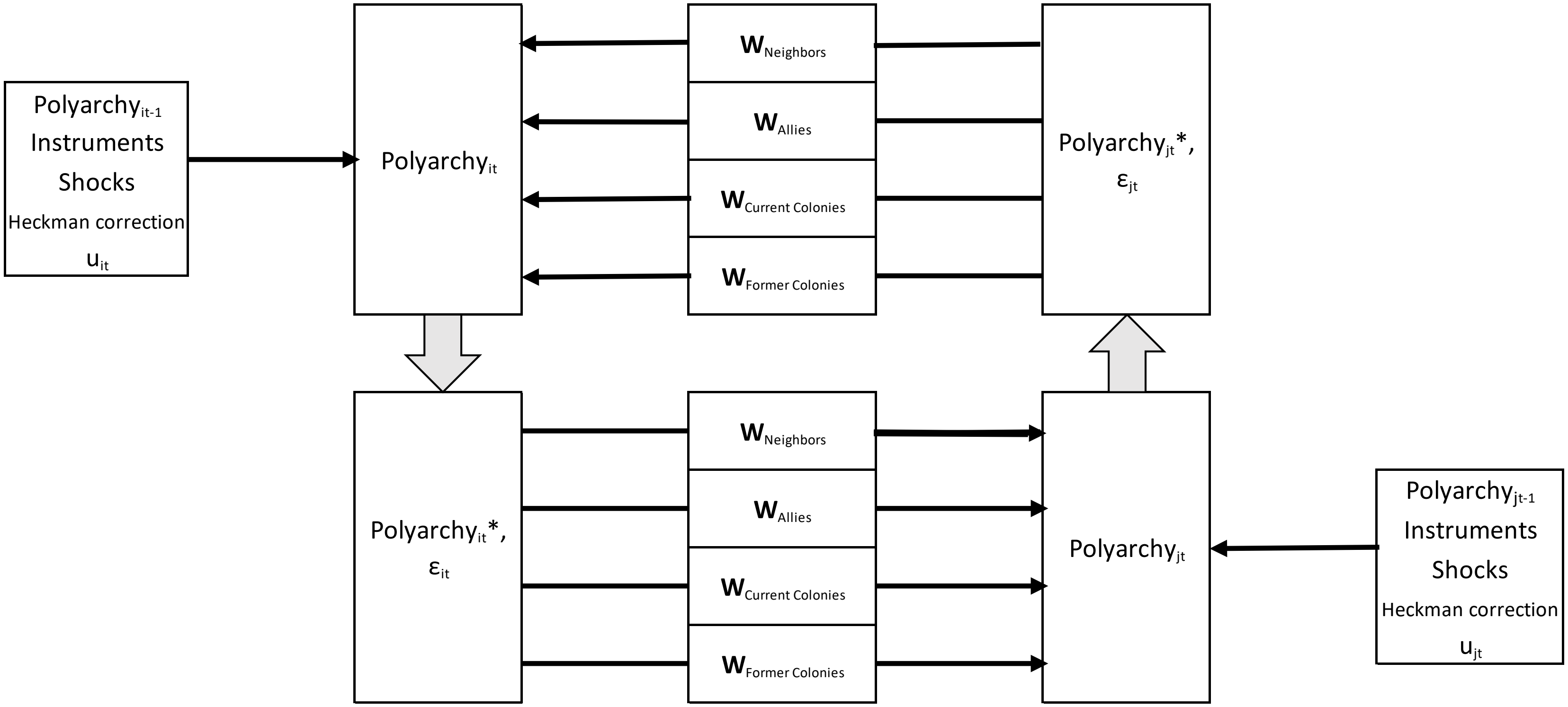


Figure 4.1: The Full STAR Model

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.

Figure 4.2

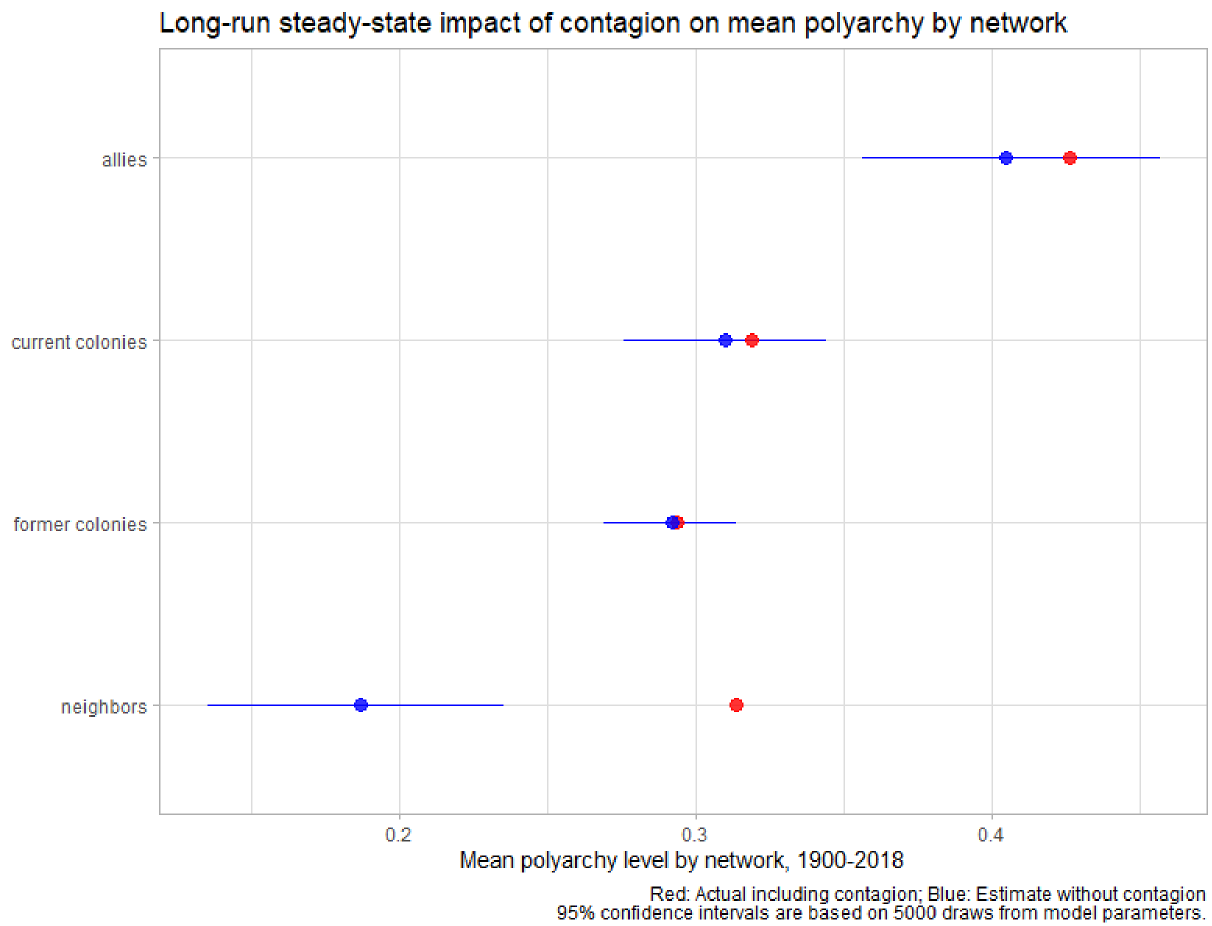

Long-run steady-state impact of contagion on mean polyarchy by network
allies
current colonies
former colonies
neighbors
0.2
0.3
0.4
Mean polyarchy level by network, 1900-2018
Red: Actual including contagion; Blue: Estimate without contagion
95% confidence intervals are based on 5000 draws from model parameters.

Figure 4.3

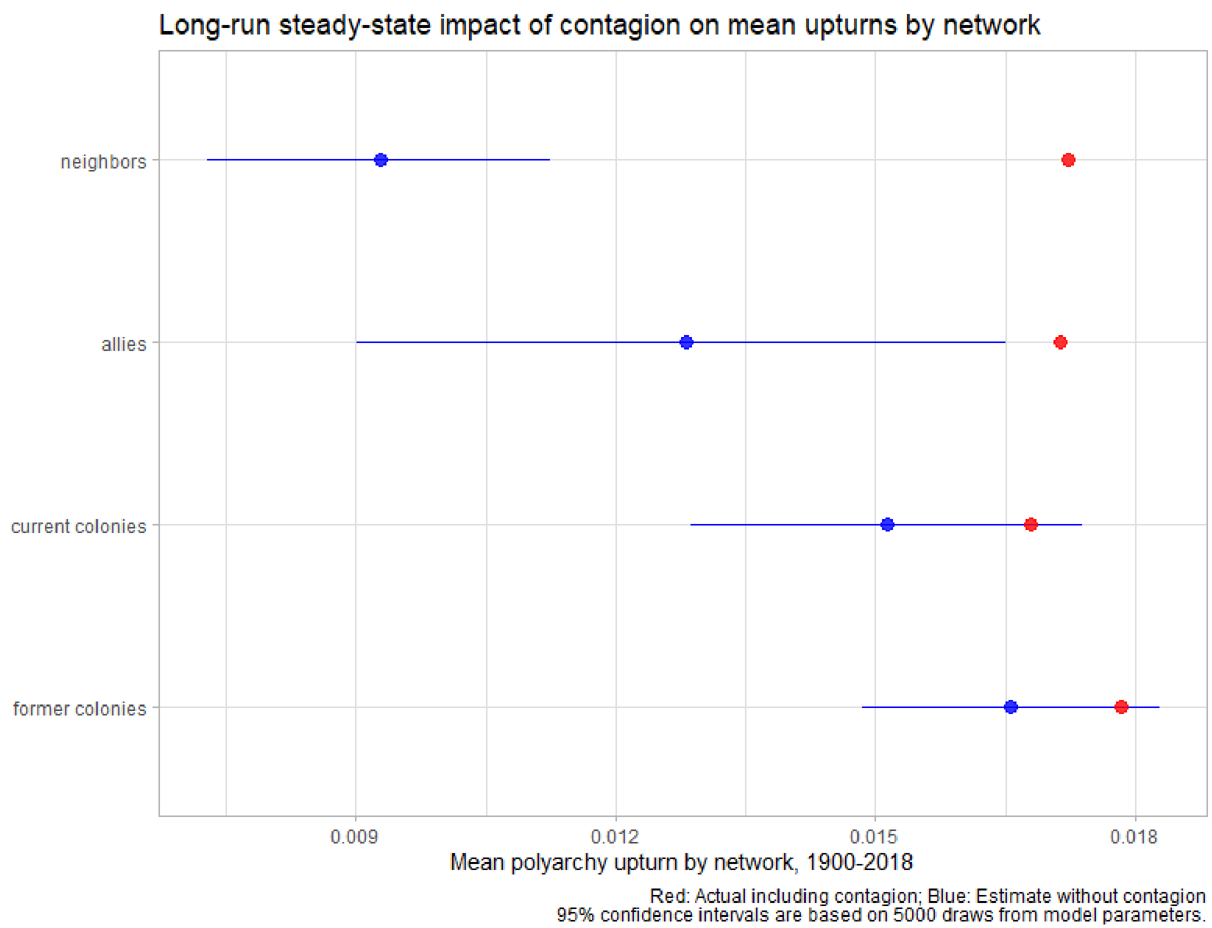

Long-run steady-state impact of contagion on mean upturns by network
neighbors
allies
current colonies
former colonies
0.009
0.012
0.015
0.018
Mean polyarchy upturn by network, 1900-2018
Red: Actual including contagion; Blue: Estimate without contagion
95% confidence intervals are based on 5000 draws from model parameters.

Figure 4.4

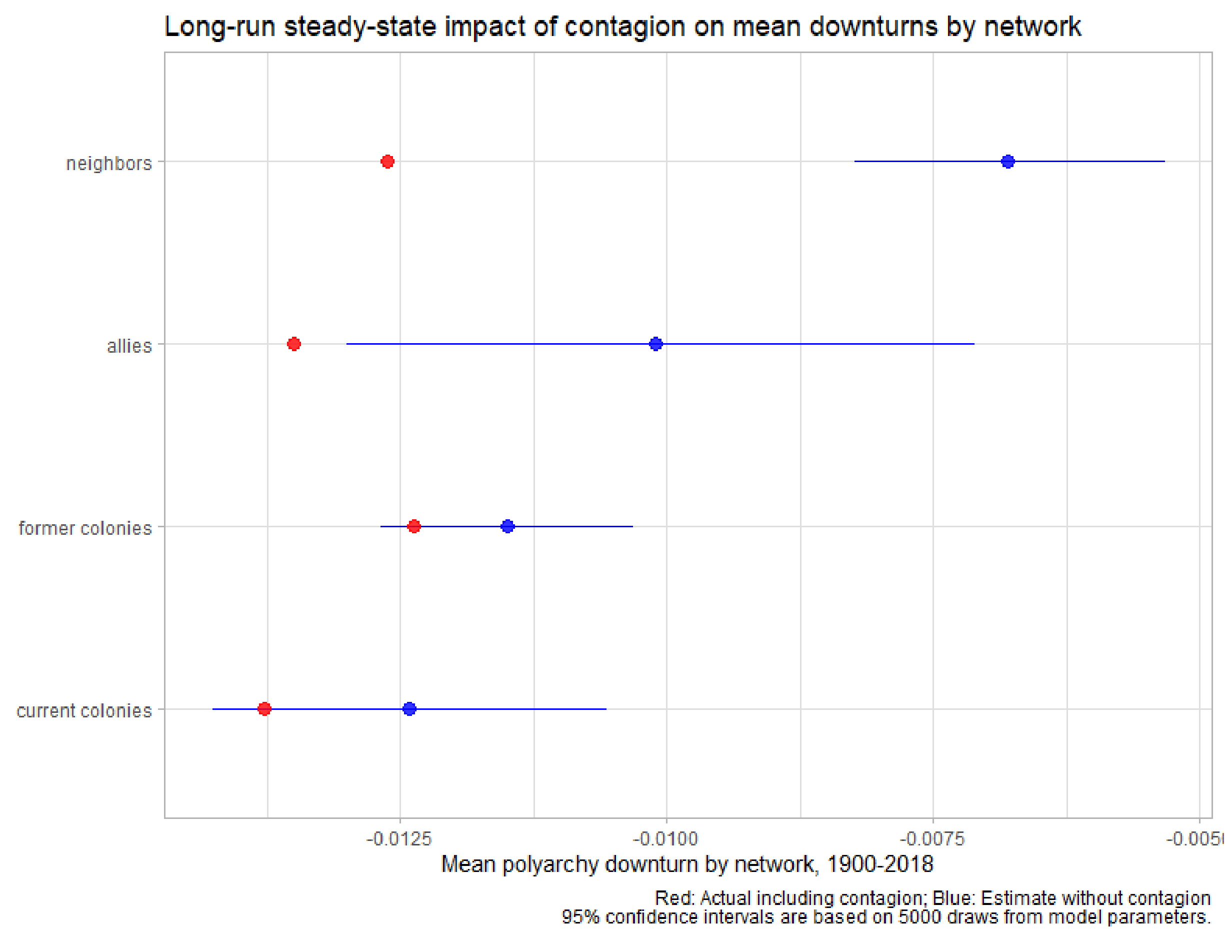
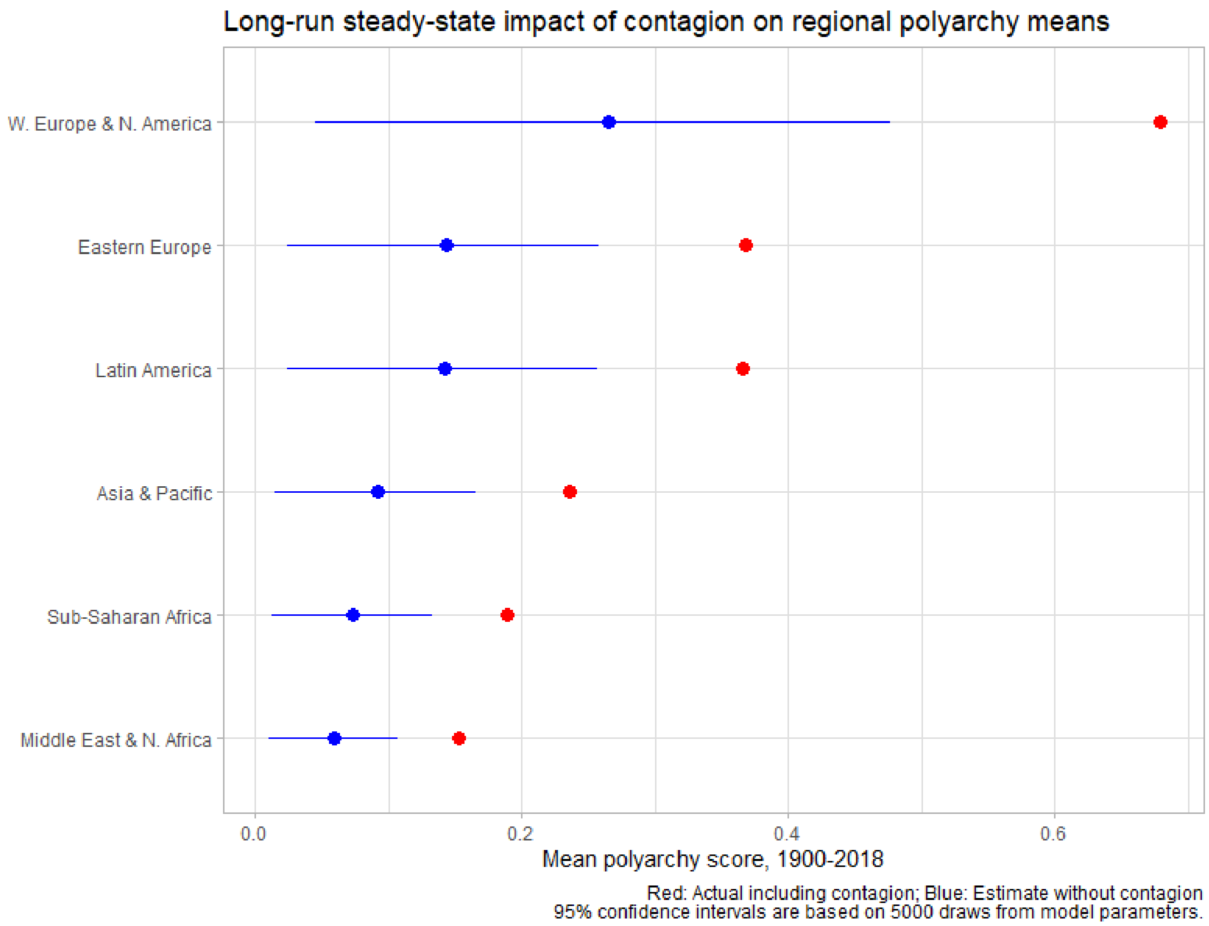

Long-run steady-state impact of contagion on mean downturns by network
neighbors
allies
former colonies
current colonies
-0.0125
-0.0100
-0.0075
-0.005
Mean polyarchy downturn by network, 1900-2018
Red: Actual including contagion; Blue: Estimate without contagion
95% confidence intervals are based on 5000 draws from model parameters.

Figure 4.5


Long-run steady-state impact of contagion on regional polyarchy means
W. Europe & N. America
Eastern Europe
Latin America
Asia & Pacific
Sub-Saharan Africa
Middle East & N. Africa
0.0
0.2
0.4
0.6
Mean polyarchy score, 1900-2018
Red: Actual including contagion; Blue: Estimate without contagion
95% confidence intervals are based on 5000 draws from model parameters.

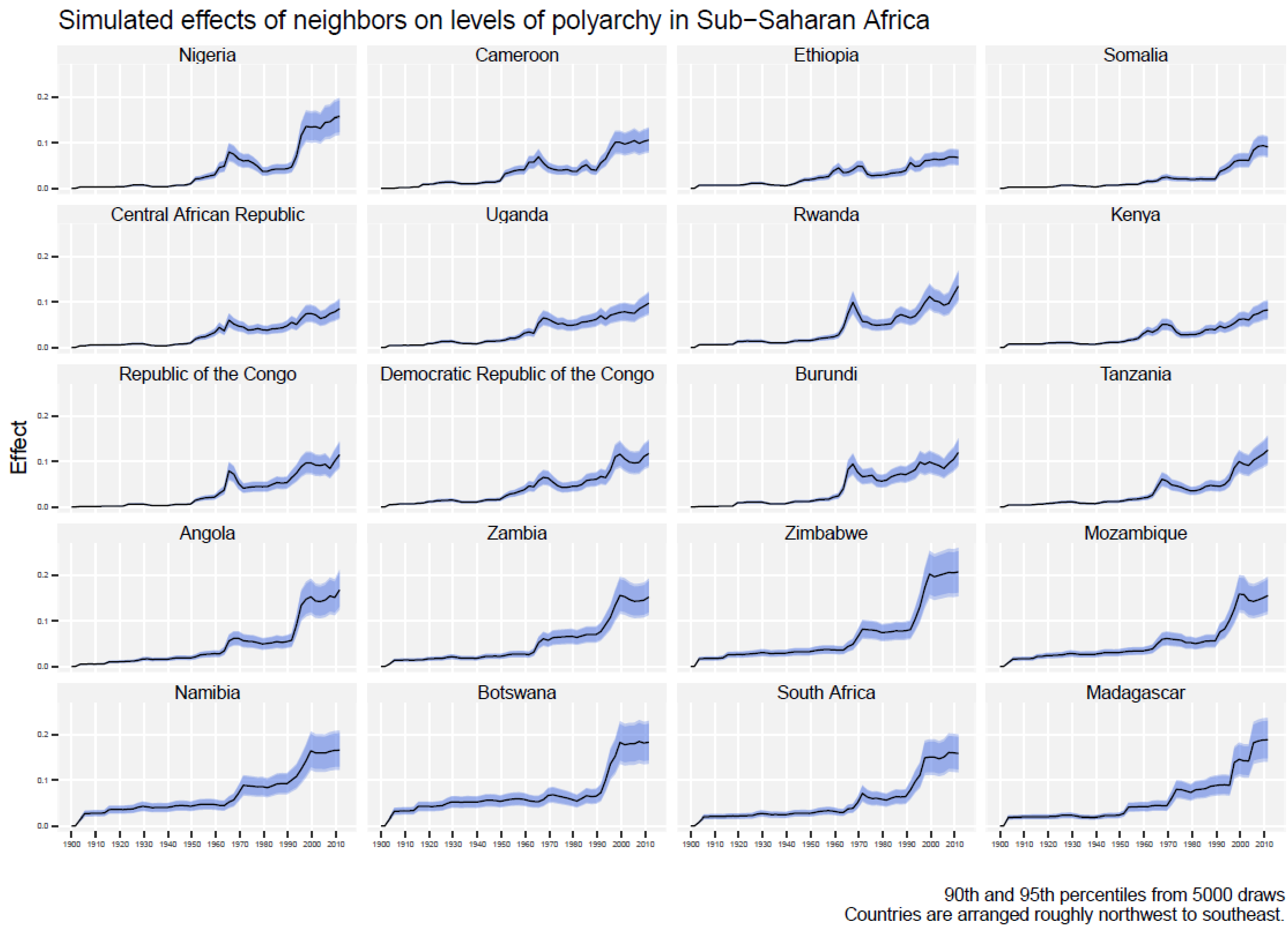

Simulated effects of neighbors on levels of polyarchy in Sub−Saharan Africa
Nigeria
Cameroon
Ethiopia
Somalia
Central African Republic
Uganda
Rwanda
Kenya
Republic of the Congo
Democratic Republic of the Congo
Burundi
Tanzania
Angola
Zambia
Zimbabwe
Mozambique
Namibia
Botswana
South Africa
Madagascar
Effect
0.0
0.1
0.2
1900
1910
1920
1930
1940
1950
1960
1970
1980
1990
2000
2010
90th and 95th percentiles from 5000 draws
Countries are arranged roughly northwest to southeast.

Figure 4.6

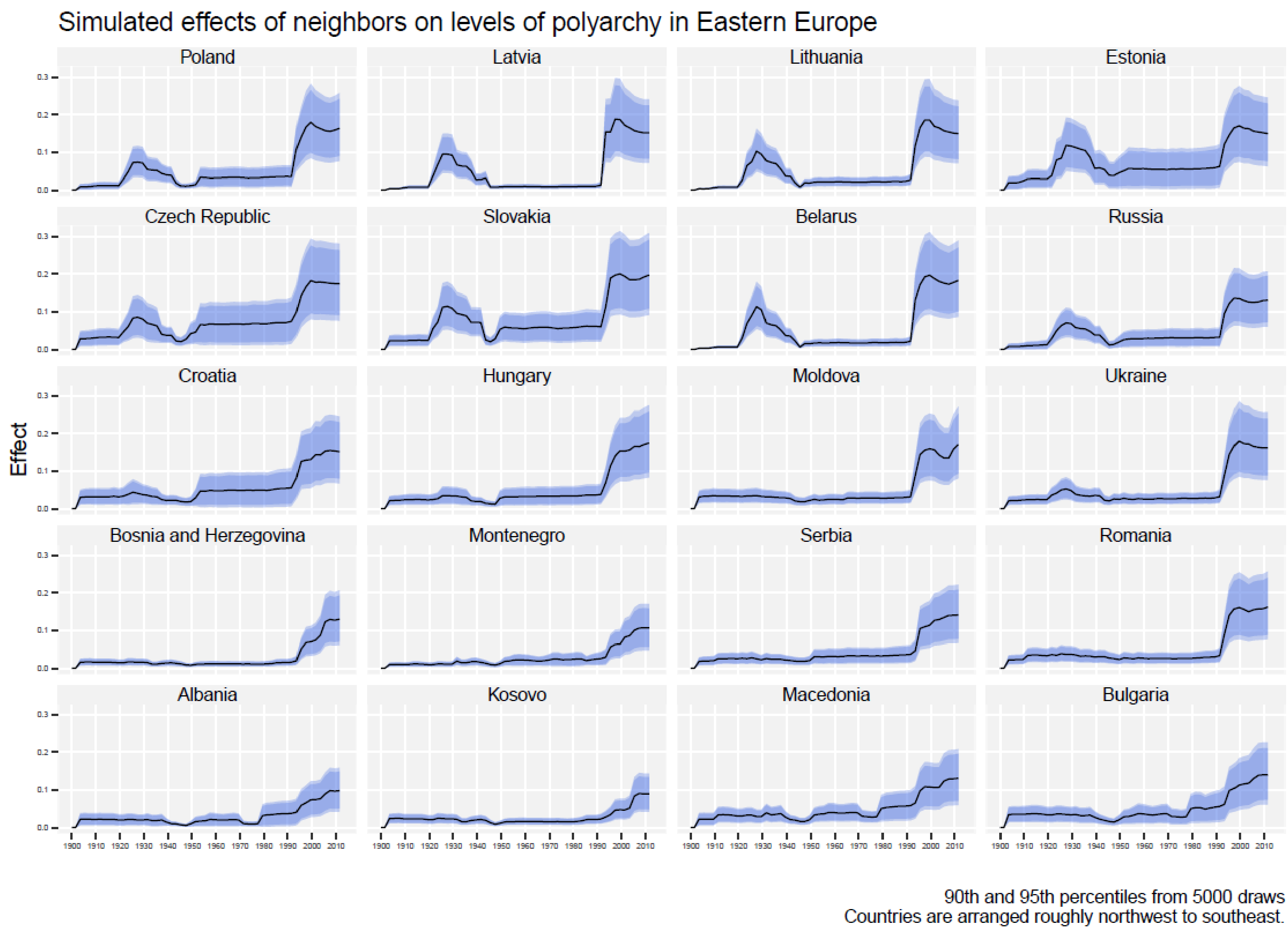

Simulated effects of neighbors on levels of polyarchy in Eastern Europe
Poland
Latvia
Lithuania
Estonia
Czech Republic
Slovakia
Belarus
Russia
Croatia
Hungary
Moldova
Ukraine
Bosnia and Herzegovina
Montenegro
Serbia
Romania
Albania
Kosovo
Macedonia
Bulgaria
Effect
90th and 95th percentiles from 5000 draws
Countries are arranged roughly northwest to southeast.

Figure 4.7

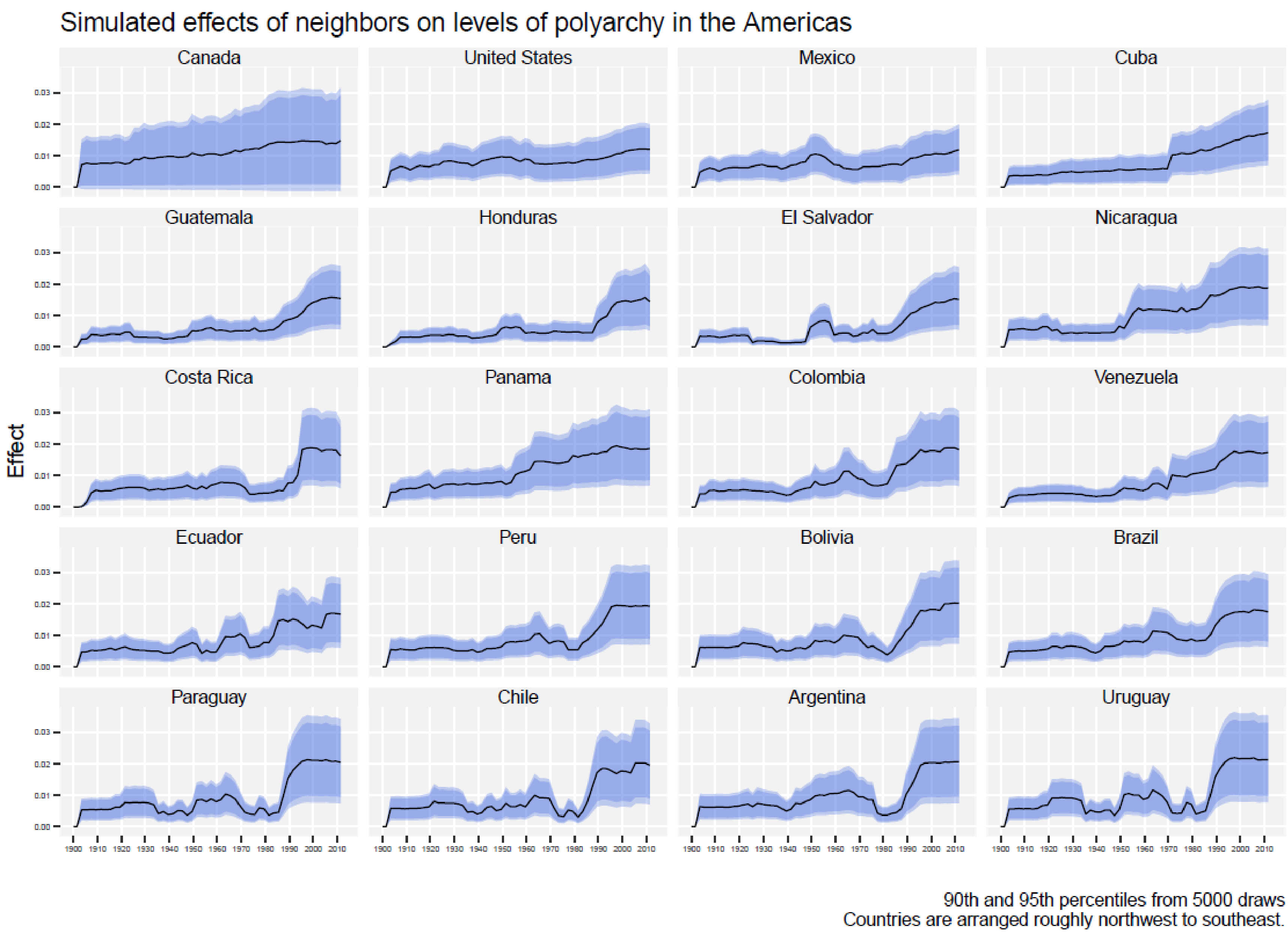

Simulated effects of neighbors on levels of polyarchy in the Americas
Canada
United States
Mexico
Cuba
Guatemala
Honduras
El Salvador
Nicaragua
Costa Rica
Panama
Colombia
Venezuela
Ecuador
Peru
Bolivia
Brazil
Paraguay
Chile
Argentina
Uruguay
Effect
0.00
0.01
0.02
0.03
1900
1910
1920
1930
1940
1950
1960
1970
1980
1990
2000
2010
90th and 95th percentiles from 5000 draws
Countries are arranged roughly northwest to southeast.

Figure 4.8

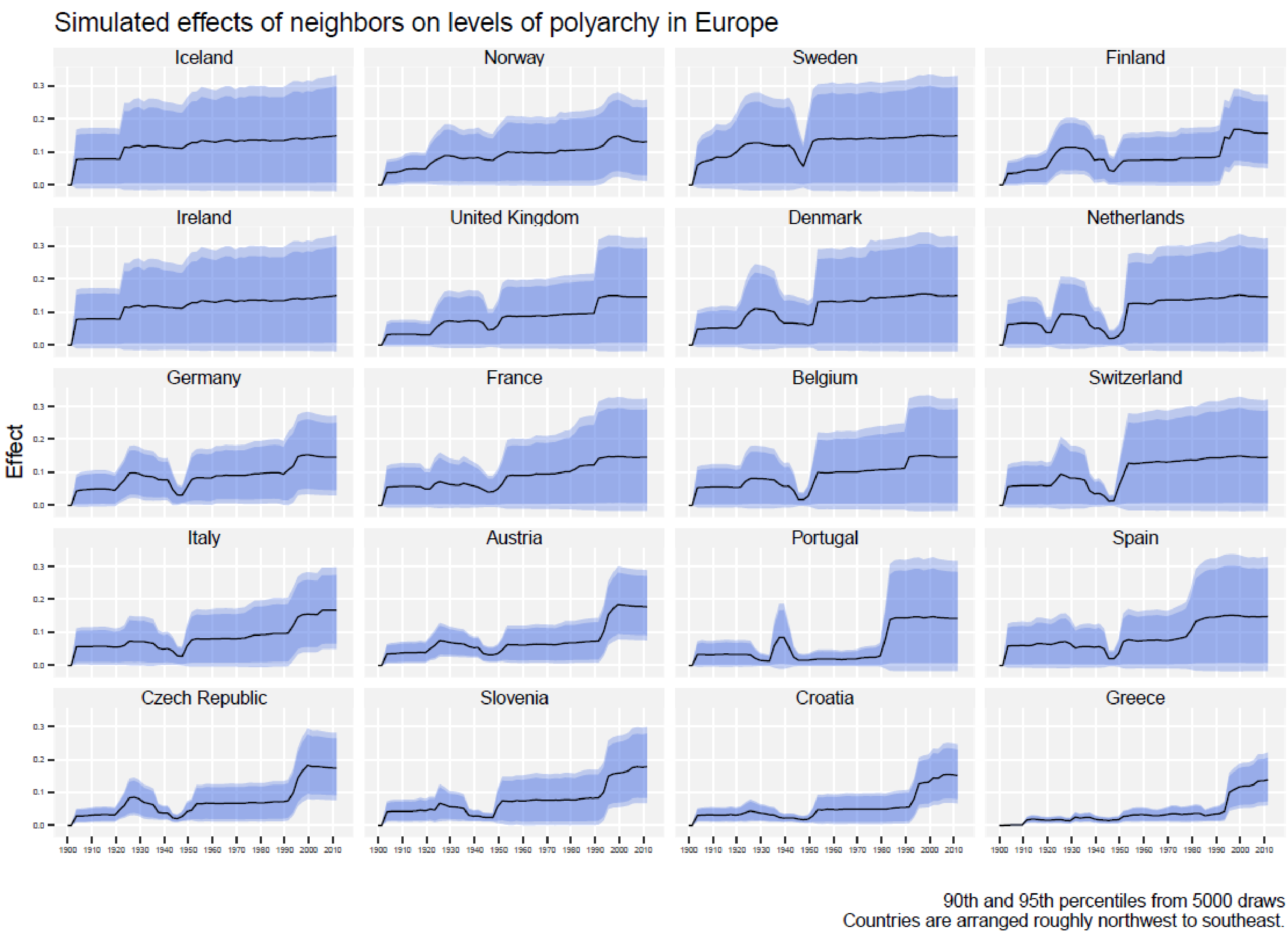

Simulated effects of neighbors on levels of polyarchy in Europe
Iceland
Norway
Sweden
Finland
Ireland
United Kingdom
Denmark
Netherlands
Germany
France
Belgium
Switzerland
Italy
Austria
Portugal
Spain
Czech Republic
Slovenia
Croatia
Greece
Effect
90th and 95th percentiles from 5000 draws
Countries are arranged roughly northwest to southeast.

Figure 4.9

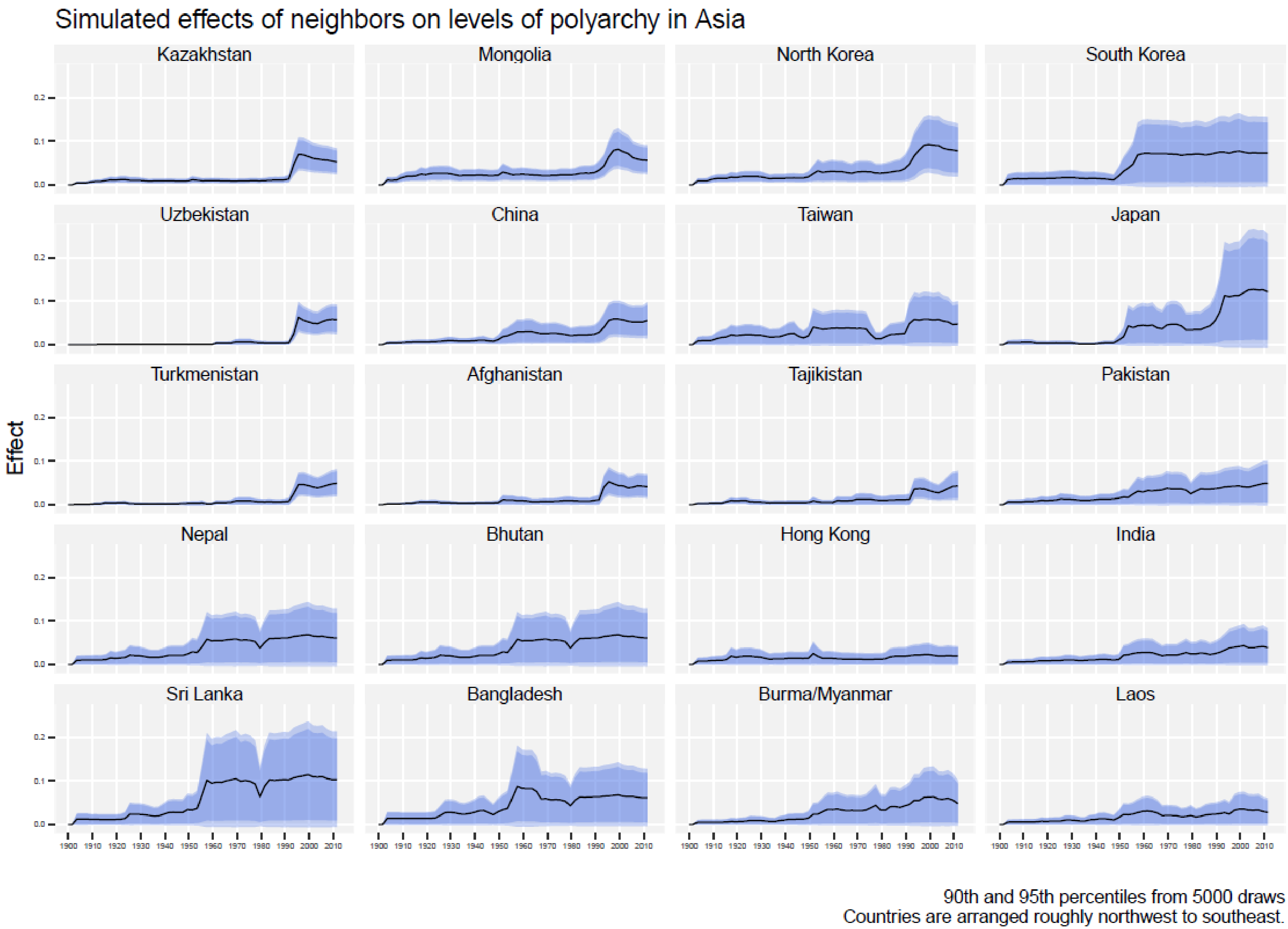
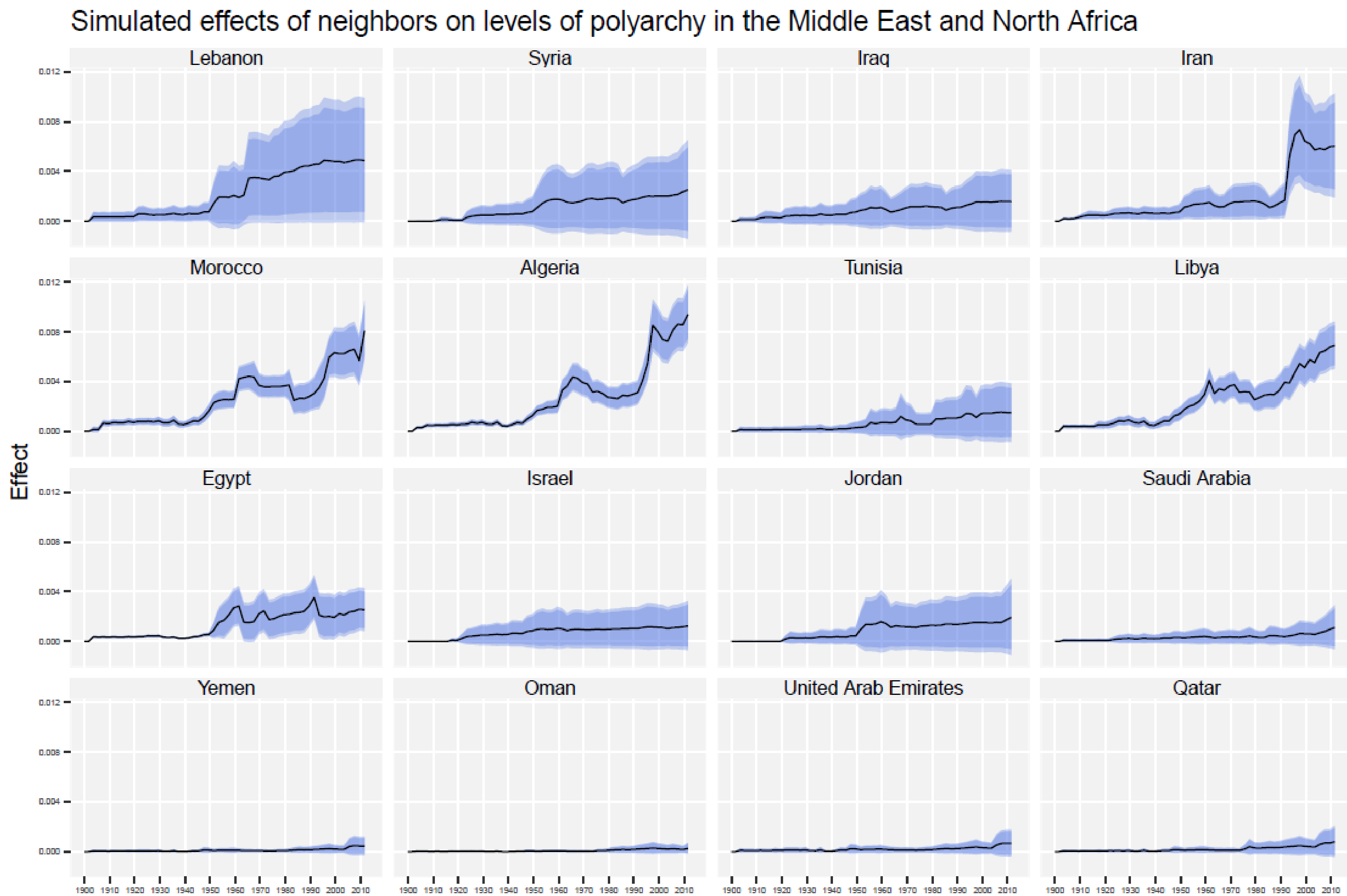

Simulated effects of neighbors on levels of polyarchy in Asia
Kazakhstan
Mongolia
North Korea
South Korea
Uzbekistan
China
Taiwan
Japan
Turkmenistan
Afghanistan
Tajikistan
Pakistan
Nepal
Bhutan
Hong Kong
India
Sri Lanka
Bangladesh
Burma/Myanmar
Laos
Effect
0.0
0.1
0.2
1900
1910
1920
1930
1940
1950
1960
1970
1980
1990
2000
2010
90th and 95th percentiles from 5000 draws
Countries are arranged roughly northwest to southeast.

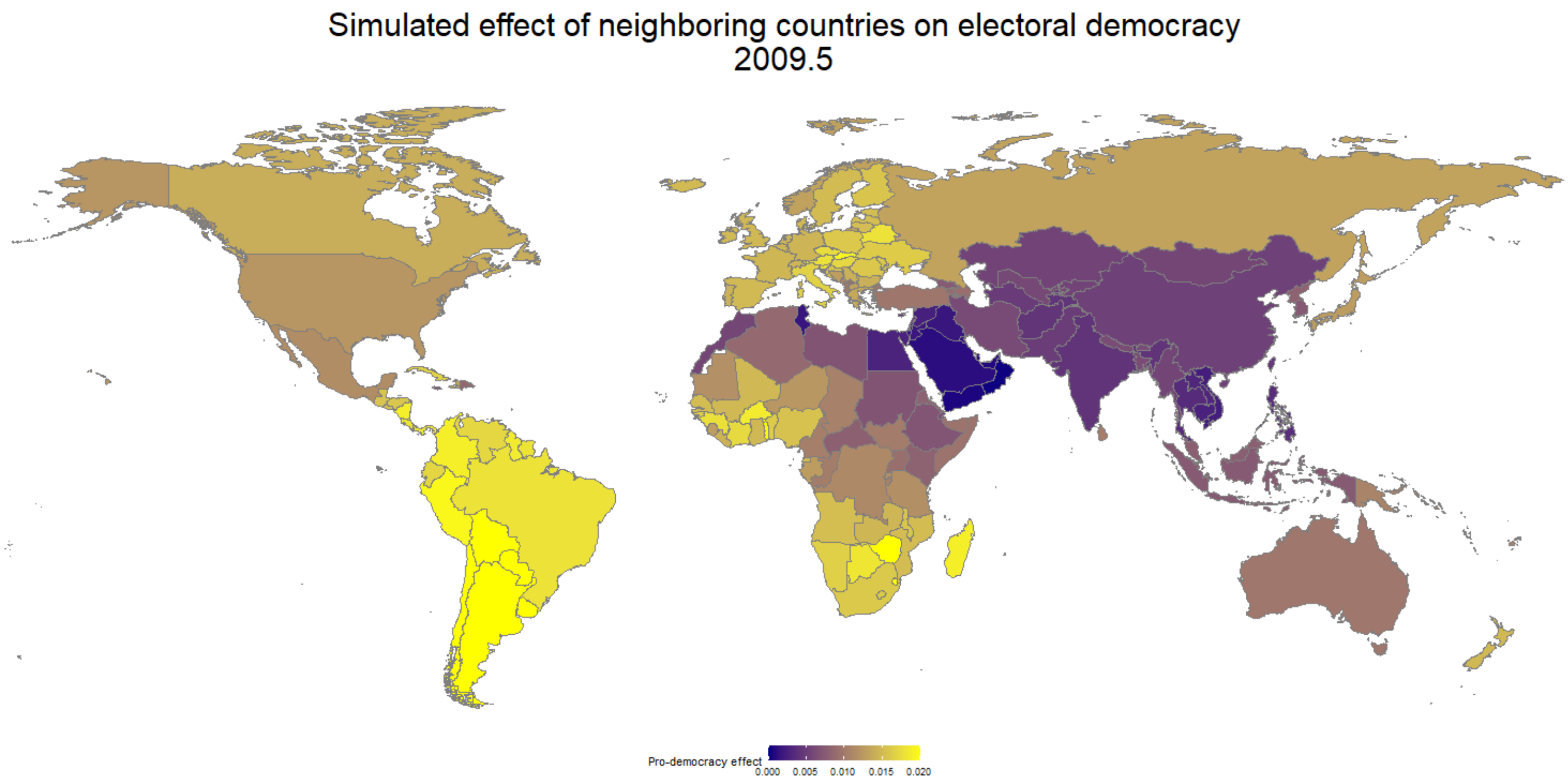
Figure 4.10



90th and 95th percentiles from 5000 draws
Countries are arranged roughly northwest to southeast.

Figure 4.11

Figure 4.12: Front lines of polyarchy level contagion in 2009-2010



Higher values (lighter shades) do not reflect levels of electoral democracy. They indicate the front line of contagion: zones of influence favoring electoral democracy emanating from contiguous countries. Dark blue indicates zones in which influence favoring electoral democracy is absent.

Table 4.1: Comparison of Models with and without Network Relationships

	Level		Upturns		Downturns	
	without networks	with networks	without networks	with networks	without networks	with networks
Domestic covariates						
Lagged DV	**0.9518**	**0.9382**	**0.2504**	**0.240**	**0.208**	**0.200**
Literacy/1000	**0.1731**	**0.2015**	**0.1382**	**0.131**	**-0.080**	**-0.076**
Colony selection/1000	**-4.2266**	**-3.7104**	-0.5387	-0.450	-0.487	-0.476
Protestant population	**0.0001**	**0.0001**				
European population	**0.0001**	**0.0001**				
Election year			**0.0144**	**0.014**	0.001	0.001
GDP per capita			**-0.0034**	**-0.003**	**0.002**	**0.002**
Hyperinflation			**0.0151**	**0.014**	-0.002	-0.002
Internal war			-0.0026	-0.003	**-0.004**	**-0.004**
Colonizer selection/1000			0.0837	0.121	-0.212	-0.211
Latitude			-0.0080	-0.007	0.004	0.004
Port distance/1000			-0.0018	-0.002	0.002	0.001
Exogenous shocks						
International war			**-0.0068**	**-0.006**	-0.002	-0.002
Global GDP expansion	**0.9802**	**0.6542**	**0.5075**	**0.478**	0.055	0.062
Global GDP contraction			-0.0921	-0.057	0.136	0.137
Decade dummies	yes (not shown)		no		no	
Constant	**0.0106**	**0.0031**	**0.0287**	**0.022**	**-0.016**	**-0.013**
Endogenous networks and spatially correlated errors						
Current colony network						
Lagged instrument		0.0017		**0.075**		0.015
Network error term		0.0182		-0.063		-0.023
Former colony network						
Lagged instrument		0.0003		**0.054**		-0.008
Network error term		-0.0021		-0.058		0.012
Neighbor network						
Lagged instrument		**0.0250**		**0.350**		**0.335**
Network error term		**0.1473**		**-0.325**		**-0.373**
Alliance network						
Lagged instrument		0.0032		**0.192**		**0.140**
Network error term		**0.2177**		**-0.253**		**-0.168**
N	7170		6755		6756	
Wald chi^2 for spatial terms:		295.82		152.52		107.93

Coefficients in bold are significant at the 95% level or better.

Table 4.2: STAR Estimates for a Model of Polyarchy Levels with Six Regional Neighbor Networks

region	contagion	se	z	p	spatial error	se	z	p
Africa	**0.0332**	**0.0087**	**3.793**	**0.0001**	**0.1548**	**0.0411**	**3.765**	**0.0002**
E. Europe	**0.0271**	**0.0083**	**3.273**	**0.0011**	**0.3480**	**0.0302**	**11.533**	**0.0000**
Latin America	**0.0263**	**0.0057**	**4.588**	**0.0000**	0.0469	0.0249	1.883	0.0598
WENA	**0.0173**	**0.0051**	**3.389**	**0.0007**	**0.1984**	**0.0256**	**7.743**	**0.0000**
Asia	**0.0161**	**0.0072**	**2.222**	**0.0263**	0.0219	0.0459	0.476	0.6338
MENA	0.0071	0.0098	0.725	0.4686	-0.1069	0.0771	-1.387	0.1656

Figures in bold are statistically significant at the 95% level or better.
Temporal lag = 0.9456, se = 0.0042.
Covariates (not shown) are ten decade dummies, Protestant population, European population, adult literacy, and global economic expansion.
N = 7,170. Wald test of spatial terms: $chi^2(12) = 357.32$, $p > chi^2 = 0.0000$.

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, ρ, captures only the direct impact of a spatially lagged variable. The full spatial effect, 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, φ, of a temporal lag. In both cases, one divides the reported coefficient by one minus the lag coefficient (times **W**, for spatial lags) to obtain long-run or steady-state (or in the presence of both temporal and spatial lags, long-run steady-state) coefficient. If ρ is large, the spatial dependence diminishes slowly and the cumulative effect is large. If ρ 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 hypothetical 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: what is known as a multiplex model. 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. Although we do not report this, it is also possible to calculate 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):

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

In the section on Interpretation, we defend basing our long-run steady-state simulations on rhos estimated in a multiplex model rather than in separate models for each network. The main defense is that our focus on the marginal effects of one network, holding other networks constant, pertains to a hypothetical scenario in with the controlled networks did not exist. But a secondary defense is that the empirical estimates do not differ very much. We offer evidence of that here. Table A.1 compares the rhos for the neighbors, alliances, current colonies, and former colonies networks in models of polyarchy levels, upturns, and downturns, depending on whether the estimates come from multiplex or separate models.

Table A.1: Comparison of multiplex and separate estimates of ρ

outcome	model	neighbors	alliances	current colonies	former colonies
level	multiplex	0.02496***	0.00316	0.00175	0.00035
	separate	0.01886***	0.00188	0.00284	0.00262
upturns	multiplex	0.35027***	0.19186***	0.07465*	0.05365*
	separate	0.43848***	0.22795***	0.00156	0.03969
downturns	multiplex	0.33475***	0.13947**	0.01451	-0.00747
	separate	0.24356***	0.28891***	-0.61593	-0.12012**

* p < .05, ** p < .01, *** p < .001

There are some differences, but where they exist, we prefer the multiplex estimates because they appear to do a better job of apportioning credit among networks that overlap to some extent. In models of levels, it makes sense that the one significant network, neighbors, has a larger effect when all networks are included. In models of upturns, it makes sense that neighbors have a smaller effect when three other significant networks are in the same model. And in models of downturns, it makes no sense at all that the former colonial network has a significant negative coefficient when it is estimated separately. That would mean that a downturn in one country leads to upturns in other former colonies. In the multiplex estimate, former colonies have no significant effect.

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: 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 (2), $\mathbf{W}$ is a matrix of spatial lags and $\mathbf{M}$ is a matrix of temporal lags. Therefore, ρ is the coefficient of spatial dependence and φ the coefficient of temporal dependence. $\mathbf{X}$ is a matrix of domestic independent variables, $\boldsymbol{\beta}$ 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.[36] 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.

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. We control only for literacy, an imperfect proxy for multifaceted economic and social development, and a few other variables. Still 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),

$$[\mathbf{I} - \rho_1\mathbf{W_1} - \rho_2\mathbf{W_2} - \ldots - \rho_k\mathbf{W_k} - \varphi\mathbf{My}]^{-1}. \qquad (3)$$

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.

[36] 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 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.