

Superpower Interventions and their Consequences for Democracy: An Empirical Inquiry

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Abstract

Do superpower interventions to install and prop up political leaders in other countries subsequently result in more or less democracy, and does this effect vary depending on whether the intervening superpower is democratic or authoritarian? While democracy may be expected to decline contemporaneously with superpower interference, the effect on democracy after a few years is far from obvious. The absence of reliable information on covert interventions has hitherto served as an obstacle to seriously addressing these questions. The recent declassification of Cold War CIA and KGB documents now makes it possible to systematically address these questions in the Cold War context. We thus develop a new panel dataset of superpower interventions during the Cold War. We find that superpower interventions are followed by significant declines in democracy, and that the substantive effects are large. Perhaps surprisingly, once endogeneity is addressed, US and Soviet interventions have equally detrimental effects on the subsequent level of democracy. Our findings thus suggest that one should not expect significant differences in the adverse institutional consequences of superpower interventions based on whether the intervening superpower is a democracy or a dictatorship.

1) Introduction-

Do superpower interventions to install and prop up political leaders in other countries subsequently result in more or less democracy, and does this effect vary depending on whether the intervening superpower is democratic or authoritarian? While democracy may be expected to decline contemporaneously with superpower interference, the effect on democracy after a few years is far from obvious. The absence of reliable information on covert interventions has hitherto served as an obstacle to seriously addressing these questions. The recent declassification of Cold War CIA and KGB documents now makes it possible to systematically address these questions in the Cold War context, which is what we do. Aside from being of considerable historical importance in its own right, a study of Cold War interventions is of great interest because it speaks directly to whether and how outside forces may alter the path of institutional evolution, in contrast to the usual focus on domestic actors. This, of course, is an issue of special policy relevance to our time.

As implied in the questions above, our interest is not in the contemporaneous effect of superpower interventions but rather on the effects after the passage of a few years.¹ Our ex ante theoretical expectations on this issue are far from clear. From one theoretical perspective one may reasonably expect the effects of superpower interventions to differ depending on whether the intervening superpower is a democracy or not. For instance, superpower governments might have a preference for spreading their own form of government. If the benefits of establishing democracies overseas exceed the costs for a

¹ Unsurprisingly democracy does decline contemporaneously with superpower interventions.

democratic but not for an authoritarian superpower, interventions by a democratic superpower could subsequently result in more democratic environments than interventions by a non-democratic superpower. However, one may also reasonably adopt the theoretical perspective that interventions by democratic and authoritarian superpowers alike are primarily driven by considerations of repressing opponents overseas. Crony authoritarian regimes overseas offer less checks and balances against repression of opponents, which may cause democratic and non-democratic superpowers alike to favor the presence of crony dictators overseas. If so we may expect interventions by a democratic and an authoritarian superpower to have similar adverse consequences for democracy in intervened countries. The goal of this paper is to check which of these perspectives finds greater support in the Cold War data.

In order to answer this question we develop a new panel dataset of superpower interventions during the Cold War. Our goal is to examine the effects of a superpower intervention in a given five year period on the average level of democracy in the following five year period. We find that superpower interventions are followed by significant declines in democracy and that the substantive effects are large. Perhaps surprisingly US and Soviet interventions have equally detrimental effects on democracy. Our findings thus indicate that one should not expect significant differences in the adverse institutional consequences of superpower interventions based on whether the intervening superpower is a democracy or a dictatorship.

This paper relates to literatures in both political science and economics. In political science, our paper builds on recent work by Bueno de Mesquita and Downs (2006) on the effects of interventions in wars and other militarized disputes. Our paper extends this work in three ways. First, we do not focus exclusively on military interventions, but extend our examination to behind the scenes efforts by the secret services of the superpowers to install and prop up leaders in office. Second, we do not limit ourselves to studying countries which are experiencing violent conflict, but also consider interventions with limited and no casualties. This difference is significant because wars and militarized disputes are relatively rare events. Third, we offer instruments for addressing questions of endogeneity, which is a serious concern because interventions may be driven by the anticipated state of the institutional environment.

As far as economics is concerned, a large literature on the effect of institutions on economic development has begun to explore the first-stage determinants of institutions, including variables such as inequality, ethnic fractionalization, and colonizing strategies. Our paper differs from these in focusing on a relatively time varying determinant of institutions.

In the next section of the paper we offer a brief review of the previous literature on interventions. In Section 3 we describe our hypotheses relating interventions to institutions. In Section 4 we describe our new panel data set of interventions and outline the rationale and sources for our control variables. Section 5 describes how we address issues of identification. In Section 6 we present our results, while Section 7 concludes.

2) Literature review-

The literature that specifically focuses on the institutional effects of interventions is concentrated in political science. Bueno de Mesquita and Downs (2006) have recently provided an excellent survey of this literature and we summarize some of the highlights below. One group of studies focuses on military interventions and finds that interventions by democratic countries have a positive effect on democratic reform in the short term, but generate political instability in the long term. (Kegley and Hermann, 1997 and Gleditsch et al., 2004). Another group of political scientists focuses more narrowly on cases of interventions by the US. Some of the case study literature finds that US interventions are not associated with democratization and attributes this to US military and economic interests (Karl, 1990 and Rueschmeyer et al., 1992). Other studies focus on the difficulty of imposing democracy from above. (Herman and Broadhead, 1984, O'Donnell et al., 1986, Whitehead, 1991.) Yet another group of scholars finds that US interventions have a positive effect on democracy under some limited circumstances. (Meernik, 1996, Wantchekon and Nickerson, 1999, Enterline and Greig, 2003.)

Bueno de Mesquita and Downs' paper is the latest contribution to the interventions literature. Their paper focuses on military interventions in wars and militarized interstate disputes and finds that this genre of interventions "lead to little if any improvement, and all too often erosion in the trajectory of democratic development."² They argue that this is because democratic and authoritarian leaders alike share a common interest in

preferring to deal with non-democratic target states. In the case of authoritarian regimes, which they argue are primarily concerned with providing private goods to government insiders, non-democratic target regimes facilitate the transfer of resources to insiders. In the case of democratic regimes, which they argue are primarily concerned with providing public goods to their country's citizens, non-democratic regimes in the target state facilitate policy concessions that benefit the intervener's citizens.

This paper advances the above literature in two ways. First, the widespread availability of datasets on war and military interventions has led previous econometric studies to focus almost exclusively on interventions in the context of wars, to the neglect of behind the scenes interventions that were conducted by the secret services of the superpowers. Second, while undeniably insightful, the literature on interventions has hitherto not seriously addressed issues of reverse causality. This paper seeks to advance our understanding of interventions by filling these gaps in the literature.

This paper also attempts to advance the literature on institutions in economics. This literature is growing rapidly, partly of interest in its own right and partly as a side effect of searching for instruments for institutions to use in empirical exercises assessing the effects of institutions on other variables. Engerman and Sokoloff (1997) in a seminal paper stress inequality as a key (negative) factor in the evolution of institutions in North and South America; Easterly (2007), Easterly and Levine (2003), and Satyanath and Subramanian (2007) confirm this for the worldwide sample. The famous work by Acemoglu et al. (2001, 2005) stresses colonizing strategies as a historical determinant of

²Bueno de Mesquita and Downs 2006, 627.

institutions, using settler health conditions as a determinant of whether the colonizer followed a settler colony strategy or an exploitative strategy. Mauro (1995), Easterly and Levine (1997, 2003), and Alesina et al. (2003) focus on ethnic fractionalization as an adverse factor inhibiting institutional development; Alesina et al. (2007) suggest that colonial partitioning of ethnic groups and other forms of artificial boundary drawing made things even worse. Acemoglu and Johnson (2005) also stress long run factors like inequality, share of agriculture in production, and natural resource endowments in their magisterial work on determinants of democracy. Our paper extends this literature by focusing on a recent set of political shocks that influenced democracy – Cold War interventions by the superpowers.

Our paper has a much more distant relation to the extensive branch of the literature which looks at the success of foreign “peacekeeping” forces in ending wars (Doyle and Sambanis, 2000, Gilligan and Stedman 2004, Weinstein 2005), which is in turn related to the literature on the determinants of civil war (Collier and Hoeffler, 1998, 2001, and 2002, World Bank 2003, Miguel, Satyanath, and Sergenti, 2004) and the literature on what to do about “failed states” (Collier 2007, Birdsall et al (2006), Weinstein et al. (2004), Council on Foreign Relations (2005), and Brainard (2007)). Although we share the interest in foreign intervention, our paper deals with a different subject than these papers do -- we are looking at foreign interventions that are explicitly concerned with leadership selection and maintenance rather than “peacekeeping” or fixing “failed states”. Our results still may have some indirect relevance for this very different debate, however, since both focus on the effects of foreign intervention.

3) Hypothesis on the relationship between superpower interventions and democracy-

This paper attempts to sort out which of two theoretical perspectives finds greater support in the Cold War data. To take the first, one may expect superpower governments to have a preference for spreading their own form of government. The benefits of establishing democracies overseas may then exceed the costs for a democratic but not for an authoritarian superpower. From such a perspective interventions by a democratic superpower could result in more democratic (less authoritarian) environments than interventions by a non-democratic superpower.

However, it is also plausible that an inclination favoring the spread of democracy on the part of a democratic superpower may be swamped by other, realpolitik related, considerations. Interventions by both superpowers may be primarily aimed at repressing threats to its security overseas (communist opponents in the case of the US, and non-communist opponents in the case of the Soviet Union). Under such circumstances, the survival in office of a superpower-installed chief executive plausibly hinges on providing the superpower that installed him what it wants by way of repressive policies.

Repression of communists/anti-communists is plausibly easiest for a chief executive to implement if he is unconstrained from seeking the approval of a large number of domestic political actors prior to implementing the repressive measures. It is thus in the interest of a superpower-installed chief executive to attempt to reduce the domestic checks and balances on his power. It is not in the interest of any superpower (democratic

or undemocratic) which has placed a priority on repressing opponents overseas to oppose such attempts at centralization of power, since checks and balances are likely to make repression more difficult. Thus, from this perspective we may plausibly expect interventions by democratic and authoritarian superpowers to lead to similar declines in democracy.

Which of these perspectives finds more support in the data? The rest of this paper is devoted to answering this question.

4) Data

The focus of this paper is on superpower interventions to install and prop up the leaders of other countries during the Cold War. Recently declassified documents reveal that most such Cold War interventions took the form of operations organized by the covert services of the superpowers, namely the CIA/KGB. There is no pre-existing dataset that comprehensively captures such operations. Recent contributions to the Cold War history literature (much of which is based on recently declassified documents) make it possible to identify which countries were subject to such interventions. We have thus relied on this literature, supplemented by our own archival research, to construct a dataset of superpower interventions to install and prop up leaders of other countries that includes covert interventions. We offer citations to accompany every case that we have identified as an intervention.

The variables in the dataset are as follows. Each variable has one variant which captures a US intervention and one which captures a Soviet intervention:

- 1) ONSET- This variable is coded as one in the period in which a leader is installed in office with the support of the covert services of a superpower. This may be either via a coup or by providing slush funds for elections.
- 2) FAILED- Coded as one for all periods in which a superpower initiates efforts to install a leader in office, but is unsuccessful in its efforts.
- 3) COUNTER-Coded as one for periods in which a superpower organizes counterinsurgency operations for a leader who was not installed in office by the superpower. (One may think of this as an exclusively counterinsurgency based intervention.)
- 4) OFFSET-Coded as one for the first point of time after an onset in which a superpower explicitly relinquishes its ability to select the leader of a country. This may be by force (for example in the case of Iran in 1979) or voluntary (as in the case of Gorbachev and Eastern European satellites in the late 1980s).
- 5) INTERMED-This variable captures superpower meddling in periods other than onset and offset periods. It is coded as one for all periods between ONSET and OFFSET as well as for periods coded as one for COUNTER.

6) INVASION-Coded as one for all periods in which a superpower army invades a country and successfully installs a leader in office. (Covert onsets and invasions are not necessarily mutually exclusive, since invasions and covert operations may be used simultaneously to install a leader.)

7) INTERVENTION-This is our omnibus measure of successful superpower meddling in the domestic affairs of other countries. (Success here refers to successfully installing a leader via covert service operations, successfully organizing counterinsurgency operations, successfully installing a leader after invading a country, or successfully propping up a leader in office.) All country periods coded as one on either ONSET, COUNTER, INVASION, or INTERMED, plus periods not covered by these variables when the literature indicates a leader is depending heavily on superpower support for survival in office (prop ups) are coded as ones. Note that offset periods are coded as prop up periods (and thus as intervention periods) if the offset does not occur the very beginning of a period, which is generally the case. This is so because the portion of the offset period prior to the actual occurrence of the offset is logically a time when the superpower has not relinquished control over leadership selection.

Our core analysis covers 127 countries, of which 24 were subject to leaders installed with CIA involvement and 16 to leaders installed with KBG/MGB involvement. While the sample is very large we note that we are unable to handle a few partitioned/reunified

countries such as Pakistan and Yemen due to difficulties in gathering clean data on controls for different segments of a national unit.

We are aware of the fact that we may be missing some “secret” interventions but we see no reason why these omissions should be systematically biased in a way that helps our results. If anything, the incentives for the superpowers to reveal an intervention should be greatest if the outcome is positive from an institutional standpoint so, if anything, we are likely to be misclassifying some interventions that had adverse outcomes as non-interventions. Furthermore, a secret intervention is most likely to be ultimately revealed in environments that ended up having strong transparent institutional environments. This, of course, would only make it harder to find support for our hypotheses.

We conduct our analysis in a panel setting with five year periods. This serves two purposes. First, it is much easier to precisely pin down the five year period in which an intervention occurred, as opposed to the year. Second, this strategy provides time for the institutional effects of interventions to work themselves out. The goal of our analysis is to see if an intervention in one five year period is associated with an improvement or deterioration in democracy in the succeeding five year period. The Cold War may most plausibly be claimed to have begun in 1947 with the announcement of the Truman doctrine, and have ended in 1989 with the fall of the Berlin Wall. However, if we use this entire period we are left with periods of uneven length. To ensure that all periods are of equal length the first period for which we code interventions is 1950-54. The last

period for which we code interventions is 1985-89. In order to ensure that we do not lose interventions in the 1947-49 period we classify these as having occurred in Period 1.³

In conducting this analysis we are careful to ensure that we are not simply capturing a correlation that results from interventions being aimed primarily at authoritarian regimes, or by an automatic contemporaneous adjustment of the democracy score in response to an external intervention. We rule out that we are capturing such effects by controlling for the level of democracy in the period of the intervention, and observing the effect of the intervention on democracy in the subsequent five year period. (See a description of our GMM strategy below.) Aside from GMM instruments, we also use instruments with explicit causal stories to address the endogeneity of interventions to anticipated levels of democracy. (Our precise strategy to address endogeneity is described in the next section.)

With regard to our dependent variable, we use the two measures of democracy that are most widely used in political science. The first, called REG, is from Przeworski and colleagues and is a dummy variable which takes a value of 1 in the presence of an authoritarian regime (Alvarez et al. 2000). The measure is behavioral in the sense that a country is only classified as being democratic if it displays turnover in office following an election. This procedure acts as a safeguard against counting rigged elections which

³ Note that not all interventions began with or ended with the Cold War. There are a small number of cases of states where there was already superpower influence well before the Cold War began (American influence in Liberia, Ethiopia, and gulf oil states), or where it continued after the Cold War ended (continuing American support to Soeharto in Indonesia and Habre in Chad). In the former cases, we do not show an ONSET with the beginning of the Cold War, and in the latter cases we do not show an OFFSET with the end of the Cold War. We require hard evidence of superpower withdrawal to code an OFFSET during or at the end of the Cold War.

perpetuate the tenure of the incumbent as democratic elections. The second measure of democracy that we use is the continuous measure from Polity.⁴ This is a subjective measure that takes values from -10 to 10. Unlike the REG measure this measure is increasing in the degree of democracy. Since we are conducting a panel analysis using five year periods, we use five year averages of these measures. (The REG measure thus takes values between 0 and 1 depending on the proportion of years spent as an authoritarian regime in a five year period.)

We use REG as our core measure of democracy and only use Polity for conducting robustness checks. We do so because we believe that it is especially important to be wary of subjective measures of democracy in the context of this project. The subjectivity of Polity is a special concern for this paper given that Polity estimates are subjective assessments from the perspective of the western bloc, which was of course one of the participants in the Cold War. There is thus cause for concern that Polity may overstate the degree of democracy in countries in which the United States intervened. There is no such concern with respect to REG because it is behaviorally based, hinging on observed turnover in government following an election, rather than based on subjective assessments.

We control for the main variables that have been identified in the political science literature as influencing democracy. As Przeworski and his colleagues have found, democracy is significantly influenced by per capita GDP (Alvarez et al. 2000). We thus include log GDP per capita as a control variable. The widest available coverage of GDP

⁴The data is available at <http://www.cidcm.umd.edu/polity/>.

comes from the dataset created by Angus Maddison (2003). This is of special interest to us because Maddison takes great pains to generate GDP estimates for countries that were part of the Soviet bloc. Missing data from other conventional sources is heavily skewed towards countries that experienced Soviet interventions, which is obviously a serious problem for the subject of this paper. One statistic which should reassure readers that our results are not driven by quirks in Maddison's approach to assessing GDP is that GDP values in his data set are highly correlated to those of the widely used GDNGD (Growth Development Network Growth Data) data set. (The correlation is 98%.) We also conduct robustness checks using GDP data from GDNGD for reassurance.

While per capita GDP is the primary variable associated with democracy, Haggard and Kaufmann have argued that bad economic times in general influence the propensity for democracy (Haggard and Kaufman 1995). We thus also include controls for GDP growth from Maddison and GDNGD. We additionally include control variables for legal origin (from La Porta et al. 1999), oil exporter (from Bueno de Mesquita et al. 2004), ethnic fractionalization (from Alesina et al. 2003), and religion ((from Bueno de Mesquita et al. 2004), as well as a variable capturing the length of time since the last transition from democracy/dictatorship as a means of addressing duration dependence. Continental dummies are always included, given the propensity of some regions such as the Middle East for having authoritarian regimes. In our instrumental variables regressions (described in the next section) we use data for distance of each country's capital from Moscow from Gleditsch and Ward (2001). All regressions include dummies for each time period. Summary statistics are presented in Appendix Table 1.

5) Identification strategy-

Identification is a serious issue for the question at hand. Interventions may be driven by the institutional environment or by the same factors that contribute to worse institutions. We adopt two strategies for addressing endogeneity. One captures between country variation in the effects of interventions, while the second captures within country variation. We describe these strategies below.

One strategy, which captures between country variation, relies on using instruments that hinge on an explicit causal logic. As the instrument for US interventions we use the distance of a country's capital from Moscow. (We log this value to alleviate the effects of outliers.) The reasoning behind this first stage regression is straightforward. Efforts by the US to interfere in the politics of countries that are in the proximity of Moscow are relatively likely to be perceived by the Soviet leadership as threatening to its security. The US is thus most likely to anticipate stiff resistance from Moscow to interventions in such countries, and US interventions are more likely to be located elsewhere. It follows that the smaller the distance of a country's capital from Moscow, the lower the likelihood of a US intervention.⁵

⁵The above logic implies that an intervention by the US close to Moscow would be perceived as more threatening than one that is close to Vladivostok. While this is plausible another potential instrument may be provided by the distance from the nearest Soviet border. Unfortunately there is no reliable measure of distance from the closest border at this point in time. We ran regressions with a preliminary (as yet unfinalised) version of a dataset that includes such data and got very similar results.

While it is clear that distance from Moscow is not subject to reverse causality we must also consider other ways in which this distance may affect democracy, and control for them in order to satisfy the exclusion restriction. For instance, countries that are extremely distant from Moscow may receive less investment from and trade less with the Soviet Union, resulting in slower growth. Slower growth may, in turn, be associated with the collapse of political regimes. We thus control for economic growth and per capita GDP in all our specifications.

The analogous instrument to use for Soviet interventions would be distance from Washington D.C. However, geography and history conspire to make this a poor indicator of which potential Soviet interventions would be perceived as threatening to US security. Specifically, ever since the Monroe doctrine of 1823 the United States has clearly communicated that it considers any interventions in the Americas by other powers to be unacceptable. South America happens to be an exceptionally long continent, which means that many countries that are far from Washington D.C. are also ruled out as viable targets for Soviet interventions thanks to the Monroe doctrine. The consequence is that it is not possible to get a strong first stage for Soviet interventions using distance from Washington D.C. as an instrument. We thus use an alternative instrumentation strategy for Soviet interventions.

We use the US security perimeter as of the fall of Nazi Berlin (May 1945) as our instrument for Soviet interventions. This perimeter is the boundary generated by the Monroe Doctrine of 1823 (the Americas) and the relatively rapid collapse of the Nazi

eastern front (which subsequently limited the US sphere of influence in Europe), namely the Americas and Europe west of Germany. We believe this historically generated perimeter is not the consequence of reverse causality for the following reasons. This perimeter clearly emerged as a consequence of factors long in the historical past (the Monroe doctrine) and short term military considerations (anticipated Axis resistance on the Western vs. Eastern fronts in World War II), and not as a consequence of the quality of institutions in the countries included within the perimeter. Historians are in broad agreement that the extent of the Soviet western advance in World War II was agreed upon between Stalin and Roosevelt in Teheran (1943) and Yalta (early 1945) on the basis of considerations of the most efficient means to topple the Nazis. Furthermore, the perimeter as of May 1945 clearly did not emerge as a consequence of anticipated Soviet expansionism. As the Cold War historian John Lewis Gaddis puts it:

Roosevelt had built his whole strategy upon the expectation that the wartime alliance would survive the end of the war...Although concerned during the last months of his life about the increasing frequency of misunderstandings with the Russians, he at no point sought to contest the substantial expansion of Soviet influence in Europe and Asia that the end of the war would bring...Despite obvious differences in personality and style, Harry S. Truman continued Roosevelt's policy upon coming into office... During his first months in office he firmly rejected proposals from Winston Churchill and some of his own advisors that would have denied the Russians previously agreed upon occupation zones in Central Europe and North East Asia. "I was having as much difficulty with Prime Minister Churchill as I was having with Stalin," the new president noted in May of 1945. As late as the fall of that year, both Truman and his new secretary of state, James P. Byrnes, were still relying upon the establishment of a personal relationship with Stalin as the best way to overcome the difficulties that had already begun to emerge in the Soviet-American relationship.⁶

⁶Gaddis 1987, 26.

For our perimeter instrument we code all countries in the Americas and all Western European countries excluding Greece and Finland as ones. Greece is coded as a zero because it was only included as part of the perimeter with the announcement of the Truman doctrine in 1947. Finland's status, of course, remained ambiguous for years after the end of World War II and was never entirely resolved until the end of the Cold War. We code Asian countries as being outside the US security perimeter as of May 1945. This is because there was considerable uncertainty about where the US considered its perimeter to lie in Asia at this time. (Recall that the Japanese surrender still lay ahead by several months. Also, a great deal of uncertainty existed even as late as in 1950, forcing Acheson to make his fateful "clarifying" speech at the National Press Club on January 12.)

The causal logic for the perimeter instrument is also straightforward. We should expect the Soviets to anticipate relatively high resistance from the United States for intervening in a country that is inside the US's historically established security perimeter. This would make Soviet interventions outside the perimeter more likely, causing us to expect a negative relationship between being within the US perimeter and Soviet meddling. Once again we are careful to control for growth and per capita GDP since being within the US perimeter may be associated with more trade and investment and therefore higher growth and development (as the Marshall plan suggests), which in turn can help the quality of institutions.

Do we additionally expect a negative effect of distance from Moscow on Soviet interventions and a positive effect of the US security perimeter on US interventions? We are less confident about these relationships for two reasons. A potential positive relationship between Soviet interventions and proximity to Moscow may be washed out by Soviet interventions in countries that are far from Moscow and also outside the US perimeter. Likewise, a positive relationship between the US perimeter and US interventions may be washed out by US interventions in countries outside the perimeter that are far from Moscow. While not ruling out these alternative causal paths, our identification strategy thus hinges on the former two proposed causal links rather than on the latter two links.

The instruments described above are obviously invariant over time, which rules out the use of country fixed effects. We address concerns about omitted variables bias in this part of our analysis by including a comprehensive set of control variables. Note that by conducting our analysis in a panel setting we are also improving considerably on conventional cross country analysis by explicitly controlling for the effects of economic shocks which, as mentioned earlier, have been found to affect the level of democracy. (We are effectively allowing the predicted values of interventions to respond to changes in the values of time varying control variables as well as to common shocks to the global economy.) We conclude our description of this instrumentation strategy by noting that our instruments offer the added strength of not being correlated with the condition of institutions at the end of World War II since very few countries in the world were democracies in May 1945.

Our second strategy to address endogeneity is aimed at capturing within country variation. We instrument for interventions using the GMM estimator developed by Blundell and Bond (1998). The estimator incorporates the lagged value of the dependent variable on the right hand side. The estimator uses equations in first differences (instrumented by extended lags of levels) as well as equations in levels (instrumented by first differences). The Blundell and Bond estimator is thus referred to as the system GMM estimator. The identification assumptions of this estimator are that transient errors are serially uncorrelated, that initial conditions are predetermined, and that country specific effects are uncorrelated with the differenced values of the right hand side variables. We are careful to conduct tests of AR(2) in differences which must be surmounted for the identification strategy to be valid. In addition we test for violations of the exclusion restriction by using the Hansen overidentification test.

6) Results-

We begin in Table 1 by addressing endogeneity using the distance from Moscow and the U.S security perimeter as of May 1945 as instruments for US and Soviet interventions. Since we only have two instruments we can only include two endogenous variables for interventions in this section of our analysis. The regressions in Table 1 thus capture the effects of our omnibus measures of successful interventions, US INTERVENTION and SOVIET INTERVENTION. In Table 2, where we use the multiple instruments offered

by the GMM technique, we assess the independent effects of the numerous intervention variables listed in Section 4.

We begin Table 1 with the results for a fixed effects OLS specification in which we place our core measure of democracy (REG) on the left hand side (column 1). We then proceed to our core instrumental variables results. Our interest is in seeing how an intervention in a given five year period affects democracy in the subsequent five year period, and to see if this effect differs depending on whether the intervening superpower is a democracy (the US) or a dictatorship (the Soviet Union).

Column 1 of Table 1 shows that US interventions are associated with a significantly lower level of democracy in the next five year period in an OLS setting. (Recall that the REG measure is decreasing in democracy.) Soviet interventions display the same sign and the point estimates for US and Soviet interventions are statistically indistinguishable from each other ($p=.31$). It is conceivable that the above results are contaminated by endogeneity; the Soviets may have intervened in inferior institutional environments. We thus now proceed to address endogeneity with the two instruments listed above.

Column 2 of Table 1 displays our core instrumental variables result. US and Soviet interventions are instrumented by distance from Moscow and the US security perimeter in May 1945. Corresponding first stage results are presented in columns 1 and 2 of Appendix Table 2. The first stage results are in accordance with our theoretical expectations. Distance from Moscow is positively associated with US interventions. The

US security perimeter is negatively associated with Soviet interventions. The Cragg-Donald F-statistic is 15 (vs. a benchmark of 10), which suggests that the specification does not suffer from weak instrument problems. The second stage results reveal that both US and Soviet interventions are negatively associated with democracy in the following five year period (column 2). Both interventions are significant at the 5% level and the point estimates are statistically indistinguishable from each other. (The p value for the test for the equality of coefficients is .72). We now proceed to conduct various robustness checks.

In column 3 we include failed interventions in our definition of interventions, thereby capturing the effects of all meddling (successful as well as unsuccessful). The new intervention variables with this addition are called US INTERVENTION2 and SOVIET INTERVENTION2. Column 3 shows that both US and Soviet interventions are still associated with significant declines in democracy and the point estimates are once again indistinguishable from each other ($p=.73$). (The first stage results are very similar to those observed for the column 2 specification and are displayed in columns 3 and 4 of Appendix Table 2.)

The regressions presented so far use GDP and GDP growth data from Maddison. As mentioned, this is the appropriate source for this paper because it is the only one which offers adequate coverage of countries that lie within the Soviet bloc. (If we were to rely on one of the other widely disseminated sources most of these countries would be dropped from the analysis when controlling for the most significant alternative

determinants of democracy, which would leave us unable to accurately estimate the effects of Soviet interventions.) While using alternative income and growth controls effectively rules out comprehensively estimating the effects of Soviet interventions, it is still useful to see if the core results for US interventions hold when we use GDP data from another source. We conduct this robustness check using GDP and GDP growth data from the Growth Development Network Growth Database (GDNGD). The consequence of switching from Maddison to this source is that the sample size drops to 564. The smaller sample size here is largely a consequence of missing observations for several countries with Soviet interventions such as Czechoslovakia, Romania, Hungary, North Korea, Sudan, North Korea, Bulgaria, Poland, Somalia, and Afghanistan. In line with this pattern of missing observations, we should expect the result for US interventions alone to be robustly significant in GDNGD based specifications.⁷

Our results are in accord with this expectation. Columns 4 and 5 in Table 1 present second stage results for specifications that are the same as those in columns 2 and 3, with the exception that GDNGD controls replace Maddison. The first stages for US interventions remain strong in these specifications. (See Appendix Table 2, columns 5-8.) Unsurprisingly, given the pattern of missing data, the first stages for Soviet interventions are much weaker with the GDNGD controls. As column 4 in Table 1 shows, US interventions have a significant negative relationship with democracy in the next five year period at the 1% level. Soviet interventions are significant at the 10% level. The point estimates are once again statistically indistinguishable from each other. In column

⁷ We are entirely unable to estimate the effects of Soviet military interventions and counterinsurgencies in the GDNGD specifications on account of missing data for Soviet bloc countries.

5 we include failed interventions in our definition of interventions and the second stage results are substantially unchanged.

In columns 6-9 of Table 1 we conduct the same exercise as in columns 2-5, but using the Polity measure of democracy. (The column 1 OLS result is unchanged when replacing REG with Polity and is available upon request.) As mentioned in Section 4, we do not use Polity as our core measure of democracy because this is a subjective measure (unlike REG which is behavioral). Additionally the subjective assessments are entirely made from one side of the East-West divide, which generates the possibility of bias which could be problematic when comparing the effects of Soviet and US interventions. We have special reasons for concern about the subjectivity of the Polity measure here because we have several cases where Polity codes countries subjectively as democracies following a period of US intervention, while REG codes these same countries behaviorally as dictatorships in the same period.⁸ In contrast to 22 discrepancies of this type, there are only two instances of such discrepancies following Soviet interventions. Other discrepancies between the two variables are limited in scope (there are three cases of REG coding a democracy and Polity a dictatorship in the wake of a US intervention score of one, and two instances of the same in the wake of a Soviet intervention score of one). Overall, we should thus expect the negative results for US interventions to be weaker for the Polity measure than with the REG measure.

⁸ The countries are Guatemala, Honduras, El Salvador, Ecuador, Paraguay, Peru, Bolivia, Chile, South Africa, Syria, Taiwan, South Korea, Laos, and the Philippines.

Columns 6 and 7 use the Maddison controls, and use the INTERVENTION and INTERVENTION2 variables respectively. The strong first stages are displayed in Appendix Table 3, columns 1-4. The second stage estimate in Column 6 shows that Soviet interventions are negatively associated with democracy. (Recall that unlike the REG measure, the Polity measure is increasing in democracy.) US interventions fall short of significance ($p=.14$), but the point estimate is statistically indistinguishable from that for Soviet interventions ($p=.38$). Column 7 shows that including failed interventions in the definition of interventions leaves the results essentially unchanged.

In columns 8 and 9 we repeat the specifications from columns 6 and 7 with the exception that we replace the Maddison controls with GDNGD. The first stages for US interventions are once again strong. The first stages for Soviet interventions are weaker, which is to be expected given the pattern of missing observations in GDNGD. (See Appendix Table 3, columns 5-8.) The difference between REG and Polity codings in countries with US interventions should cause us to expect a less robust result for US interventions. On the other hand the lack of coverage of Soviet bloc countries by GDNGD could also weaken the results for Soviet interventions. As columns 8 and 9 show Soviet interventions emerge as robustly significant while US interventions are on the borderline of significance, but the point estimates are still statistically indistinguishable from each other.

In Table 2 we check if our core instrumental variables result, which was presented in column 2 of Table 1, is robust to addressing endogeneity in a different way, namely via

the Blundell and Bond GMM technique. Note that the sample sizes here are larger because we are no longer constrained by limitations in the coverage of data on time invariant controls. In column 1 of Table 2 we present the GMM analogue of our core instrumental variables result. The AR(2) and Hansen tests do not indicate problems with identification; the null hypothesis of no AR(2) cannot be rejected ($p=.64$), and the same is the case for the fulfillment of the exclusion restriction (Hansen test p value of 1.) Similar to what we observed in our core IV regression, both US and Soviet interventions emerge as having significant negative effects on democracy.

Since the GMM technique is based on using multiple lags of levels and differences as instruments, we are not limited to only including two endogenous intervention variables in our GMM analysis. In GMM we can thus separately examine the independent effects of onsets, offsets, counterinsurgency support, and military invasions. We now thus focus on estimating the independent effects of these variables.

Column 2 shows that both US and Soviet onsets are negatively associated with democracy (at the 5% and 10% levels respectively). Substantively, these amount to decreases of 20-25% in the level of democracy. US and Soviet offsets are positively associated with democracy (also at the 5% and 10% level respectively). Failed interventions and counterinsurgency support have no effect on democracy. US military

interventions are positively associated with democracy here, but this latter result turns out not be robust.⁹

In column 3 we address the question of whether the negative effects of onsets and the positive effects of offsets are robust to controlling for the periods between onsets and offsets in addition to counterinsurgencies. The variables USA INTERMED and SOVIET INTERMED capture the effects of these periods. The results for US and Soviet onsets and offsets remain robust to the addition of the INTERMED variables as controls. The point estimates for onsets are again very similar to those seen earlier. The substantive effect of the US installing a leader is to reduce the level of democracy in the next five year period by 18%. The corresponding effect of a Soviet onset is 23%. We are unable to reject the null hypothesis that the two point estimates are equal to each other ($p=.70$). We consider these to be our core GMM results.

As far as the INTERMED variables themselves are concerned, most of the changes in democracy scores in the case of US interventions appear to be occurring in the immediate wake of onsets and offsets. When it comes to Soviet interventions the relative sizes of the point estimates also indicate that onsets and offsets generate larger marginal effects on democracy than an intermed year, although the SOVIET INTERMED variable does achieve significance (unlike USA INTERMED).

⁹ This result is based on a very small sample of invasions (Dominican Republic, Grenada, and Panama). As we will see later, this latter result also does not survive our robustness check using the Polity measure of democracy.

In column 4 we check if the results for our most expansive specification (the one with the INTERMED controls) are robust to replacing the REG measure with the Polity measure of democracy on the left hand side. A decrease in the Polity measure indicates a decline in democracy. We note that identification here is less robust than with REG. (The null hypothesis of no AR(2) cannot be rejected at the 95% level, but is on the border of rejection at the 90% level.) The results for US and Soviet onsets are robust. From a quantitative perspective a USA onset is associated with a 19% decline in democracy, while a Soviet onset is associated with a 22% decline in democracy, which is very similar to what we observed with the REG measure. These point estimates are also statistically indistinguishable from each other. (The p for the test for the equality of coefficients is .84). (The earlier result for US military invasions does not survive this robustness check and the sign is in fact reversed.)

The regressions presented so far in Table 2 use data for GDP and GDP growth from Maddison. As mentioned, this is the appropriate source for this paper because it is the only one which offers adequate coverage of countries within the Soviet bloc. Just as we did in Table 1 we now check for the consequences of using GDP and GDP growth data from the Growth Development Network Growth Database (GDNGD) in place of Maddison. Columns 5-8 are analogous to columns 1-4, with the only difference being that GDNGD controls replace Maddison controls. Column 5 shows that our omnibus measure of US interventions is negatively associated with democracy (as per the REG measure). Soviet interventions are not significantly associated with democracy, but this

is plausibly attributable to the poor coverage of the Soviet bloc by GDNGD. The point estimates, however, continue to be statistically indistinguishable from each other.

Column 6 shows that when we consider separately consider onsets, offsets, military invasions, and counterinsurgency support, US onsets are negatively associated with democracy. Soviet onsets are insignificant but display the same sign as US onsets, while US offsets, Soviet offsets, and US military invasions are significantly associated with more democracy. (Recall the very small number of US military invasions, which should cause us to treat this latter result with caution.) Once again we are unable to reject the null hypothesis of equality between the US and Soviet onset point estimates ($p=.74$). In column 7 we use the INTERMED control variable (which includes periods between onsets and offsets in addition to including counterinsurgencies) and find that the results are similar. In column 8 we replace the REG measure with the Polity measure of democracy. While the negative effect of US onsets remains significant, Soviet onsets also emerge as significant in this specification and the point estimates are again statistically indistinguishable from one another.

Overall, we thus find that US and Soviet interventions during the Cold War had significant adverse consequences for democracy. We also find that the interventions of the two superpowers were similar in their effects irrespective of the fact that one was a democracy and the other was not.

7) Conclusion

In this paper we have demonstrated that superpower interventions in the domestic politics of other countries during the Cold War had substantial adverse consequences for democracy in the intervened countries. How far can one extrapolate from the history of the Cold War to interventions justified by the war on terror? We have no rigorous empirical basis on which to calibrate the relevance of the past to the present, but to the extent that commonalities exist some qualified conclusions can be made. To the extent that US leaders continue to share the Cold War world view that repressing anti-US groups overseas enhances security, the US government has incentives to install leaders who have powerful incentives to repress anti-US groups in their countries. It is clear that under these conditions our findings offer a negative prognosis for democracy in intervened countries.

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Table 1: Instrumental Variables Results

	REG					POLITY			
	MADDISON CONTROLS			GDGND CONTROLS		MADDISON CONTROLS		GDGND CONTROLS	
	OLS FE	Int	Int2	Int	Int2	Int	Int2	Int	Int2
US Intervention	0.137** (0.0572)	0.624** (0.292)	0.623** (0.300)	0.707*** (0.224)	0.687*** (0.228)	-6.29 (4.23)	-6.03 (4.60)	-6.13* (3.30)	-5.49 (3.70)
Soviet Intervention	0.0452 (0.0860)	0.500** (0.215)	0.497** (0.222)	0.466* (0.260)	0.489* (0.259)	-10.8*** (2.18)	-10.9*** (2.27)	-11.2*** (2.39)	-11.6*** (2.59)
Time Since Transition	0.0586*** (0.0176)	0.0285 (0.0181)	0.0273 (0.0193)	0.0273 (0.0182)	0.0253 (0.0188)	-0.0367 (0.219)	-0.0150 (0.234)	-0.0536 (0.186)	-0.0232 (0.201)
Log (GDP) per capita	-0.112 (0.0727)	-0.144** (0.0668)	-0.141** (0.0697)	-0.0946 (0.0621)	-0.0973 (0.0639)	3.17*** (0.922)	3.16*** (1.00)	2.82*** (0.858)	2.95*** (0.964)
GDP Growth	0.605* (0.347)	1.01 (0.676)	1.20* (0.703)	0.0177*** (0.00692)	0.0180*** (0.00693)	-13.7 (10.7)	-16.8 (10.9)	-0.204* (0.120)	-0.212* (0.119)
French Law		-0.0686 (0.117)	-0.0755 (0.126)	-0.0703 (0.0887)	-0.0611 (0.0904)	-1.92 (1.85)	-1.97 (2.03)	-2.23 (1.57)	-2.51 (1.66)
Ethnic Fractionalization		0.116 (0.128)	0.117 (0.130)	0.160 (0.134)	0.148 (0.132)	-1.33 (1.82)	-1.37 (1.89)	-0.468 (1.80)	-0.178 (1.86)
Oil Exporter		0.176* (0.102)	0.157 (0.106)	0.149 (0.111)	0.157 (0.110)	-3.03** (1.26)	-2.77** (1.32)	-2.64** (1.29)	-2.79** (1.29)
Percent Moslem		0.00160 (0.00133)	0.00169 (0.00140)	0.00264** (0.00124)	0.00278** (0.00129)	-0.000245 (0.0186)	-0.00240 (0.0196)	-0.0123 (0.0190)	-0.0151 (0.0197)
Percent Protestant		-0.000598 (0.00140)	-0.000901 (0.00149)	-0.000167 (0.00140)	-0.000131 (0.00142)	-0.00336 (0.0215)	-0.000271 (0.0237)	-0.00613 (0.0190)	-0.00700 (0.0201)
Africa		0.442*** (0.148)	0.439*** (0.150)	0.617*** (0.133)	0.601*** (0.134)	-4.35** (2.18)	-4.19* (2.36)	-6.38*** (2.12)	-5.93** (2.37)
Asia		0.0912 (0.109)	0.0738 (0.112)	0.195* (0.106)	0.192* (0.104)	-1.27 (1.63)	-1.06 (1.71)	-1.84 (1.28)	-1.74 (1.29)
Middle East		0.267* (0.151)	0.244 (0.159)	0.344** (0.163)	0.319* (0.171)	-5.62** (2.27)	-5.05** (2.40)	-6.14*** (2.34)	-5.48** (2.54)
N	728	728	728	564	564	728	728	564	564
CD F-stat		15.4	14.3	24.1	17.3	15.5	14.4	23.1	18.0
P(US-RUS)=0	0.309	0.718	0.728	0.502	0.598	0.379	0.393	0.320	0.280

Robust Standard Errors Clustered by Country in Parentheses.
Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence.

Table 2: GMM Results

	MADDISON CONTROLS				GDNGD CONTROLS			
	REG		POLITY		REG		POLITY	
US interv.	.0862***				.114***			
	(.0289)				(.0422)			
Soviet interv.	.112**				.0189			
	(.0530)				(.0717)			
		0.114**	0.122**	-1.74*		0.185***	0.195***	-2.64**
USA onset		(0.0563)	(0.0568)	(0.985)		(0.0694)	(0.0707)	(1.15)
Soviet onset		0.146*	0.156**	-2.00*		0.264	0.259	-4.61*
		(0.0763)	(0.0766)	(1.08)		(0.219)	(0.211)	(2.72)
		-0.106**	-0.100*	0.475		-0.133**	-0.138**	1.27
USA offset		(0.0540)	(0.0533)	(0.915)		(0.0649)	(0.0592)	(0.872)
Soviet offset		-0.204*	-0.194*	3.43***		-0.256*	-0.243*	4.66
		(0.105)	(0.101)	(1.25)		(0.148)	(0.139)	(1.32)
		0.0481	0.0587	-1.55*		0.0916	0.116	-1.73
USA failed		(0.0544)	(0.0537)	(0.855)		(0.154)	(0.152)	(1.78)
Soviet failed		-0.113	-0.108	1.07*		-0.181	-0.156	2.24***
		(0.0761)	(0.0793)	(0.639)		(0.117)	(0.117)	(0.821)
USA invasion		-0.359***	-0.351***	3.94		-0.357***	-0.324***	3.12
		(0.0550)	(0.0487)	(2.81)		(0.0694)	(0.0447)	(2.02)
Soviet invasion		0.0137	-0.00288	0.0432		Dropped	Dropped	Dropped
		(0.0579)	(0.0527)	(0.721)				
USA counter		0.0421				0.111		
		(0.0563)				(0.0930)		
Soviet counter		0.00391				Dropped		
		(0.0391)						
USA Intermed.			0.0297	-0.223			0.0668	-1.39***
			(0.0287)	(0.343)			(0.0463)	(0.532)
Soviet Intermed.			0.101***	-1.58***			0.136	-2.46***
			(0.0334)	(0.378)			(0.0849)	(0.613)
Lagged democracy	0.691***	0.762***	0.732***	0.794***	.580***	0.678***	0.654***	0.697***
	(0.0569)	(0.0332)	(0.0370)	(0.0321)	(0.0683)	(0.0492)	(0.0506)	(0.0430)
Time since Transition	0.0227***	0.0193***	0.0177***	0.0720	0.0294***	0.0157**	0.0139*	0.0277
	(0.00680)	(0.00592)	(0.00610)	(0.0638)	(0.00884)	(0.00732)	(0.00729)	(0.0787)
Log (GDP) per capita	-0.0633***	-0.0495***	-0.0489***	0.707***	-.104***	-	-0.0573**	0.973***
	(0.0210)	(0.0164)	(0.0174)	(0.248)	(0.0318)	(0.0219)	(0.0226)	(0.343)
GDP growth	-0.111	-0.213	-0.237	4.79	.00481	0.00156	0.000970	-0.0375
	(0.216)	(0.199)	(0.204)	(3.28)	(0.0334)	(0.00287)	(0.00283)	(0.0490)
Africa	0.135***	0.0862***	0.108***	-0.952**	0.195***	0.145***	0.174***	-1.85***
	(0.0441)	(0.0312)	(0.0345)	(0.446)	(0.0672)	(0.0369)	(0.0424)	(0.582)
	0.0438	0.0354	0.0434	-0.373	0.0541	0.0490	0.0621	-0.380
Asia	(0.0348)	(0.0271)	(0.0299)	(0.348)	(0.0564)	(0.0373)	(0.0402)	(0.449)
Middle East	0.154***	0.127***	0.147***	-2.16***	0.190***	0.160***	0.184***	-2.87***
	(0.0373)	(0.0296)	(0.0338)	(0.433)	(0.0582)	(0.0416)	(0.0479)	(0.699)
N	878	878	878	878	596	596	596	596
AR(2) p	0.637	0.553	0.604	0.098	0.232	0.135	0.14	0.118
Hansen p	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
US-RUS=0	0.64	0.71	0.7	0.84	0.21	0.74	0.77	0.51

Robust Standard Errors in Parentheses.

Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence.

Appendix Table1: Summary Statistics

	Observations	Mean	St. Dev.	Minimum	Maximum
USA onset	878	0.0251	0.156	0	1
Soviet onset	878	0.0182	0.134	0	1
USA offset	878	0.0216	0.146	0	1
Soviet offset	878	0.0182	0.134	0	1
USA failed	878	0.0182	0.134	0	1
Soviet failed	878	0.0103	0.101	0	1
USA invasion	878	0.00228	0.0477	0	1
Soviet invasion	878	0.00797	0.0890	0	1
USA counter	878	0.0205	0.142	0	1
Soviet counter	878	0.00456	0.0674	0	1
USA inter	878	0.0763	0.266	0	1
Soviet inter	878	0.0581	0.234	0	1
USA influence	878	0.261	0.439	0	1
Soviet influence	878	0.110	0.314	0	1
USA influence2	878	0.273	0.446	0	1
Soviet influence2	878	0.121	0.326	0	1
REG	878	0.658	0.455	0	1
Time since REG transition	878	2.60	2.12	0	7
Polity2	878	-1.11	7.39	-10	10
Time since Polity2 Transition	878	2.55	2.13	0	7
Log (GDP) per capita MADDISON	878	7.82	1.01	5.86	10.6
GDP growth MADDISON	878	0.0176	0.0341	-0.161	0.218
Log (GDP) per capita GDNGD	596	8.11	1.01	6.11	10.1
GDP growth GDNGD	596	2.01	3.15	-8.29	16.4
Log distance to Moscow	878	8.55	0.672	6.84	9.71
USA perimeter	878	0.314	0.465	0	1
Ethnic Fractionalization	728	0.454	0.271	0	0.930
French Legal Origin	728	0.577	0.494	0	1
Percent Protestant	728	15.5	23.3	0	90.6
Percent Moslem	728	21.5	35.2	0	99.8
Oil Exporter	728	0.115	0.320	0	1

Appendix Table 2: First Stage Results – REG Specifications

	Table 1 Column 2		Table 1 Column 3		Table 1 Column 4		Table 1 Column 5	
	USA	SOVIET	USA	SOVIET	USA	SOVIET	USA	SOVIET
Log Distance to Moscow	0.169*** (0.0578)	-0.0441 (0.0329)	0.164*** (0.0584)	-0.0370 (0.0345)	0.214*** (0.0533)	-0.0210 (0.0238)	0.212*** (0.0532)	-0.00811 (0.0281)
USA Security Perimeter	-0.140 (0.0872)	-0.359** (0.166)	-0.145 (0.0887)	-0.357** (0.167)	-0.182** (0.0728)	-0.270 (0.178)	-0.179*** (0.0734)	-0.269 (0.180)
Time Since Transition	0.0262 (0.0188)	0.00326 (0.0101)	0.0266 (0.0192)	0.00527 (0.0103)	0.0139 (0.0189)	0.00592 (0.00771)	0.0163 (0.0190)	0.00703 (0.00814)
Log (GDP) per capita	-0.130*** (0.0408)	-0.0487** (0.0238)	-0.136*** (0.0415)	-0.0496** (0.0241)	-0.133*** (0.0374)	-0.0487*** (0.0180)	-0.137*** (0.0377)	-0.0399** (0.0199)
GDP Growth	1.21* (0.647)	-1.18*** (0.432)	1.01 (0.654)	-1.32*** (0.428)	0.00630 (0.00749)	-0.00490 (0.00421)	0.00640 (0.00748)	-0.00512 (0.00364)
French Law	0.239*** (0.0729)	-0.0731 (0.0529)	0.257*** (0.0733)	-0.0818 (0.0534)	0.234*** (0.0764)	-0.0247 (0.0383)	0.237*** (0.0763)	-0.0364 (0.0393)
Ethnic Fractionalization	-0.0934 (0.147)	-0.0103 (0.0487)	-0.0883 (0.147)	-0.0196 (0.0530)	-0.127 (0.140)	-0.0153 (0.0400)	-0.126 (0.141)	0.00179 (0.0480)
Oil Exporter	-0.0230 (0.102)	0.00790 (0.0347)	-0.000998 (0.104)	0.0198 (0.0374)	-0.109 (0.0791)	-0.000444 (0.0244)	-0.111 (0.0789)	-0.0182 (0.0291)
Percent Moslem	-0.000942 (0.00110)	-0.0000901 (0.000691)	-0.000911 (0.00114)	-0.000330 (0.000690)	-0.000721 (0.00105)	-0.000463 (0.000376)	-0.000740 (0.00105)	-0.000726* (0.000375)
Percent Protestant	0.00230 (0.00148)	-0.00144 (0.00126)	0.00267* (0.00150)	-0.00130 (0.00131)	0.00170 (0.00128)	-0.000409 (0.000853)	0.00168 (0.00127)	-0.000371 (0.000920)
Africa	-0.453*** (0.125)	-0.411** (0.168)	-0.460*** (0.128)	-0.400** (0.170)	-0.596*** (0.116)	-0.320* (0.181)	-0.592*** (0.116)	-0.301* (0.184)
Asia	-0.117 (0.144)	-0.419** (0.169)	-0.101 (0.144)	-0.408** (0.170)	-0.290** (0.138)	-0.296 (0.188)	-0.288** (0.139)	-0.290 (0.190)
Middle East	-0.0773 (0.128)	-0.389** (0.176)	-0.0911 (0.132)	-0.328* (0.176)	-0.294** (0.125)	-0.234 (0.192)	-0.296** (0.125)	-0.181 (0.193)

Robust Standard Errors Clustered by country in Parentheses
Significantly different from zero at 90% (*), 95% (**), 99% (***)

Appendix Table 3: First Stage Results – POLITY Specifications

	Table 1 Column 6		Table 1 Column 7		Table 1 Column 8		Table 1 Column 9	
	USA	SOVIET	USA	SOVIET	USA	SOVIET	USA	SOVIET
Log Distance to Moscow	0.167*** (0.0571)	-0.0345 (0.0327)	0.162*** (0.0577)	-0.0272 (0.0343)	0.211*** (0.0518)	-0.0097 (0.0249)	0.208*** (0.0517)	0.00329 (0.0295)
USA Security Perimeter	-0.151 (0.0931)	-0.367** (0.160)	-0.156 (0.0947)	-0.366** (0.1600)	-0.189** (0.0789)	-0.289* (0.173)	-0.189** (0.0800)	-0.289 (0.175)
Time Since Transition	0.0217 (0.0177)	0.0193* (0.0101)	0.0222 (0.0181)	0.0214** (0.0107)	0.0093 (0.0182)	0.0203** (0.00852)	0.0116 (0.0183)	0.0215** (0.00956)
Log (GDP) per capita	-0.126*** (0.0399)	-0.0533** (0.0241)	-0.132*** (0.0405)	-0.0539** (0.0244)	-0.130*** (0.0366)	-0.0560*** (0.0198)	-0.134*** (0.0368)	-0.0472** (0.0214)
GDP Growth	1.22* (0.651)	-1.16*** (0.430)	1.03 (0.657)	-1.29*** (0.430)	0.00620 (0.00750)	-0.00453 (0.00392)	0.00631 (0.00748)	-0.00474 (0.00346)
French Law	0.246*** (0.0726)	-0.0744 (0.0520)	0.264*** (0.0732)	-0.0826 (0.0521)	0.237*** (0.0763)	-0.0261 (0.0370)	0.240*** (0.0762)	-0.0377 (0.0377)
Ethnic Fractionalization	-0.0890 (0.149)	-0.00917 (0.0474)	-0.0838 (0.149)	-0.0181 (0.0523)	-0.127 (0.140)	-0.0100 (0.0404)	-0.124 (0.141)	0.00727 (0.0486)
Oil Exporter	-0.0274 (0.102)	0.00768 (0.0351)	-0.00546 (0.104)	0.0192 (0.0371)	-0.110 (0.0786)	-0.002301 (0.0247)	-0.112 (0.0783)	-0.0202 (0.0289)
Percent Moslem	-0.000956 (0.00110)	-0.00000113 (0.000687)	-0.000924 (0.00114)	-0.000240 (0.000685)	-0.000738 (0.001050)	-0.000376 (0.000366)	-0.000756 (0.00105)	-0.000637* (0.000363)
Percent Protestant	0.00236 (0.00151)	-0.00147 (0.00127)	0.00273* (0.00153)	-0.00133 (0.00130)	0.00171 (0.00129)	-0.000386 (0.000858)	0.00170 (0.00128)	-0.000346 (0.000914)
Africa	-0.459*** (0.130)	-0.411** (0.162)	-0.466*** (0.133)	-0.401** (0.164)	-0.603*** (0.121)	-0.338* (0.177)	-0.601*** (0.122)	-0.320* (0.179)
Asia	-0.122 (0.147)	-0.437*** (0.163)	-0.107 (0.148)	-0.426** (0.165)	-0.295** (0.144)	-0.324* (0.185)	-0.296** (0.146)	-0.319* (0.187)
Middle East	-0.0799 (0.132)	-0.402** (0.169)	-0.0939 (0.136)	-0.341** (0.169)	-0.297** (0.129)	-0.255 (0.186)	-0.301** (0.130)	-0.203 (0.187)

Robust Standard Errors Clustered by country in Parentheses
Significantly different from zero at 90% (*), 95% (**), 99% (***)

Data Appendix A: Countries Included in Analysis

Afghanistan#	Djibouti #	Korea, South	Portugal
Albania	Dominican Republic	Kuwait#	Qatar#
Algeria	Ecuador	Laos	Romania
Angola	Egypt	Lebanon#	Rwanda
Argentina	El Salvador	Lesotho	Saudi Arabia
Australia	Equatorial Guinea	Liberia#	Senegal
Austria	Ethiopia	Libya#	Sierra Leone
Bahrain	Fiji*	Luxembourg*	Singapore
Bangladesh	Finland	Madagascar	Somalia#
Belgium	France	Malawi	South Africa
Benin	Gabon	Malaysia	Spain
Bhutan*	Gambia	Mali	Sri Lanka
Bolivia	Ghana	Mauritania	Sudan
Botswana	Greece	Mauritius	Swaziland
Brazil	Guatemala	Mexico	Sweden
Bulgaria	Guinea	Mongolia	Switzerland
Burkina Faso	Guinea-Bissau	Morocco	Syria
Burundi	Guyana*	Mozambique	Taiwan
Cambodia#	Haiti	Myanmar#	Tanzania
Cameroon	Honduras	Nepal	Thailand
Canada	Hungary	Netherlands	Togo
Central African Republic	Iceland*	New Zealand	Trinidad and Tobago
Chad	India	Nicaragua	Tunisia
Chile	Indonesia	Niger	Turkey
China	Iran	Nigeria	Uganda
Colombia	Iraq#	Norway	United Arab Emirates#
Comoros	Ireland	Oman	United Kingdom
Congo	Israel	Pakistan	Uruguay
Costa Rica	Italy	Panama	Venezuela
Cote d'Ivoire	Jamaica	Papua New Guinea*	Yugoslavia#
Cuba#	Japan	Paraguay	Zaire
Cyprus*	Jordan	Peru	Zambia
Czechoslovakia#	Kenya	Philippines	
Denmark	Korea, North	Poland	

Denotes countries not appearing in the GDNGD sample. (Note GDNGD is also missing several periods of GDP data for Eastern European countries in the sample).

* Denotes countries only appearing in the GDNGD sample.