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DEMOCRATIZATION AND CIVIL WAR

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Abstract

This paper examines the impact of civil war on democratization, particularly focussing on whether civil war provides an opportunity for institutional reform. We investigate the impact of war termination in general, along with prolonged violence, rebel victory, and international intervention on democratization. Using an unbalanced panel data set of 96 countries covering a 34-year period, our analysis suggests that civil war lowers democratization in the succeeding period. Our findings also suggest that United Nations intervention increases democratization, as do wars ending in stalemates. However, wars ending in rebel victories seem to reduce democratization. These findings appear robust to conditioning, different instrument sets, modelling techniques, and the measurement of democracy.

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1. INTRODUCTION

With the withdrawal of U.S. forces from Iraq at the end of 2011, the reduction of international forces from Afghanistan, and the ongoing civil war in Syria, uncertainty remains whether democracy emerges post-conflict. The fragile nature of democracy in Iraq, the inability to foster democratic governance in Afghanistan, and calls for democratization in Syria, bring into question the efforts of the United States and its partners to build democratic systems in the aftermath of civil conflict. The literature on democratization suggests that the prospect of conflict encourages the emergence of democratic institutions (Acemoglu and Robinson 2005). However, after a civil war, the likelihood of future conflict and political decay undoubtedly increases. Still, while "turnarounds" in failing states are rare, they are more likely to occur in a post-war environment (Chauvet and Collier 2009). Our paper seeks to answer the question of whether democratic turn-arounds are more or less likely in a postwar environment. That is, does the post-war environment set states on a more democratic trajectory than their non-warring counterparts?

This paper examines the influence of civil war on democratization. If civil war or the characteristics of its termination incentivize the emergence of democracy, then international institutions and parties to civil war are likely to have a keen interest in these incentives. Enhancing or attenuating these incentives may increase the likelihood of democratization. On the other hand, if civil war does not alter the likelihood of democratization, then this implies that the literature does not clearly portray the mechanisms of democratization. This finding would also suggest that

much of the current effort to foster democracy in conflict-prone regions is prone to failure.

The remainder of this paper is structured as follows. The next section briefly reviews the literature on democratization and develops several testable hypotheses. The third section describes the data and discusses the estimation methodology. The fourth section of the paper presents and considers the results. The last section concludes and discusses opportunities for future research.

2.   REVIEW OF THE LITERATURE

Civil war appears to be a development trap (Collier 2008; Collier and Hoeffler 2004).   However, as such, theory suggests that democracy may arise as a compromise to prevent and settle wars. That it, the economic costs of conflict may encourage parties to democratize (Rosendorff 2001). There are good reasons that this might be an attractive political outcome: democracies may be attractive given their economic performance relative to autocracies over time (Gleditsch 2002). Democratic governance, while imperfect, is relatively efficient and effective when compared to other forms of governance (Sen 2000) and appears to be growth enhancing (Perotti 1996).

 Empirical literature examining post-war democratization has not clearly established the implications of war on democracy. Fortna and Huang (2012) point out that post-war countries sometimes democratize and sometimes do not, but that countries that democratize post-war are much like those that would without a war.

This leaves open an important question: controlling for drivers of democracy, does civil war make a difference in democratization?

Our first empirical question is whether the termination of a civil war affects democratization in the succeeding period? From the theoretical literature, we would hypothesize that, as in Chauvette and Collier's (2009) work on failing states, war offers a unique opportunity to build new institutions that others may not possess. The theoretical literature postulates that civil war occurs because a democratic solution is untenable to the conflicting parties and that they are willing to bear the cost of war. War essentially substitutes for peaceful democratic competition (Acemoglu and Robinson 2005). The termination of war suggests that the cost of war has grown too significant (resource exhaustion) or that one party triumphed over the other (military victory). The conclusion of civil war should create opportunities for reform.

Beyond the question of whether civil war termination affects democratization, we desire to explore whether the conditions of termination affect democratization in the succeeding period. The growing body of literature that analyzes pre-war v. post-war outcomes considers many of these factors (e.g. Gurses and Mason 2008, Fortna and Huang 2012, Joshi 2010, DeRouen et al. 2010). Using non-warring countries as a control to measure whether wars that end in certain ways make countries more democratic than their non-warring counterparts, we believe complements this line of research.

Our second empirical test is whether civil war duration affects democratization in the succeeeding period. Thirdly we test whether a clear military

victory impacts democratization. Here we should observe two effects: that the extended duration of a civil war positively affects democratization in the succeeding period; and that a civil war ending with no clear victor positively affects democratization in the succeeding period. Protracted conflicts may result in the realization that neither side is likely to prevail and that the benefits of peace outweigh the benefits of continued conflict (Jensen and Wantchekon 2004; Wantchekon 2004). Democratic governance is a potential method for sharing the benefits of ending a conflict; otherwise there would be an incentive for one or more of the parties to continue warfare. Wantchekon (2004) goes on to explain that "democracy will become the natural outcome of civil wars, provided that there is military stalemate and the factions are economically dependent on citizens' productive investments." A counterpoint to this idea comes from DeRouen and Bercovitch (2008) and DeRouen et al (2010): military victories appear to provide a more enduring peace and a result of negotiated settlements may be that suffer from enduring rivalries that promote continued conflict (DeRouen and Bercovitch 2008).

Alternatively, wars may end in government or rebel victories, and one might want to believe that rebels, fighting for freedom from government oppression are more likely to democratize; however, it seems more likely that if either side wins a war it has incentives to consolidate power. Empirical studies of post war samples have suggested that if a conflict ends with military victory, democratization is less likely to occur (Gurses and Mason 2008) A rebel victory may lead to internal violence and oppression as rebels settle 'scores' and, often, are unprepared for the

tasks of governance. A victory by the government also reduces the incentive to liberalize society.

Our last empirical question is whether UN invervention in a civil war affects post-war democratization? When peacekeeping is part of ending a civil war, there is a possibility for greater state capacity and as a result a pathway to democratization. As with a stalemate, peacekeeping forces negotiations that may lead both sides to democratization. There is a sizeable literature on the role of UN intervention and it produces mixed conclusions on its effectiveness at either building state capacity or creating lasting peace. McBride, Milante, and Skaperdas (2011) suggest that outsiders can play a critical role in encouraging power sharing through investments in state capacity. Put another way, external intervention may influence democratization by allowing for the formation of credible commitments (Fortna 2004; Walter 2001). UN intervention appears to generate stable peace and democracy in "non-identity wars"(Doyle and Sambanis 2000). UN intervention may also accelerate the occurrence of democratic elections, although early elections may provide an incentive for the emergence of individuals and parties who favor a return to conflict (eg. Brancati and Snyder 2011; Hoddie and Hartzell 2010; Joshi 2010). This question is far from settled, however, as other studies have found that UN intervention has had no statistically significant impact on democratization (Fortna and Huang 2012) and that intervention can prolong war (Cronin 2010). Outsiders, either unwittingly, or because of conflicting objectives, also may undermine state capacity (McBride, Milante, and Skaperdas 2011).

In summary, there are arguments in the literature for the positive influence of civil war termination, duration, and the absence of a clear victor on democratization in the succeeding period. Rebel victory, on the other hand, is more likely to reduce democratization. There is also a lack of consensus on how UN intervention impacts democratization. It should not be surprising that the literature has not answered these questions, even though statements of policymakers would suggest that there is strong evidence for democratization following the conclusion of civil wars.

3.   EMPIRICAL ANALYSIS

The choice of democracy index and empirical methodology may significantly influence the conclusions with respect to the emergence (or lack thereof) of democracy. Most papers employ either the Freedom House measures of civil liberties and political rights or the Polity IV democracy score, each of which employs a different strategy for measuring democracy and, as a result, contain measurement error. Some researchers use Ordinary Least Squares (OLS) or Tobit estimators to argue that a variety of factors including resource rents and war characteristics, significantly influence democratization (Epstein et al. 2006; Fortna and Huang 2012; Ross 2001). These findings are subject to suspicion, however, due to the presence of country-specific effects and the persistence of democracy. When using a difference Generalized Method of Moments (GMM) estimator to control for these country-specific effects and the persistence of the democracy (and other) variables, factors such as education and income no longer appear to have a statistically significant influence on democratization (Acemoglu et al. 2005; Acemoglu et al. 2008)

More recently, an argument has emerged from the literature that, in small samples, the system GMM estimator is not only consistent with, but also relatively efficient compared to the difference GMM estimator when empirically investigating the determinants of democratization (see: Aslaksen 2010; Castelló-Climent 2008; Csordás and Ludwig 2011; Heid, Langer, and Larch 2012). These studies have found limited evidence for a statistically significant relationship between resource rents, education, economic growth, and democratization. We seek to build upon this empirical literature to examine the influence of civil war termination on democratization.

3.1 Data and Model Specification

One common problem in cross-country studies of democratization is how to properly measure democracy and transitions between regime types. Democratization is a complex process involving many public and private institutions and we readily acknowledge that any measure is likely to be imperfect. Ideally, we would construct a panel data set of civil and political institutions to effectively quantify the democratically oriented activities of society. This would demand not only significant knowledge about formal institutions but also informal institutions. Constructing such a panel data set would require information not only on the political, administrative, and fiscal operation of the central government but also on subnational governments. Unfortunately, we cannot readily address these issues with the available data. We are left with the standard, albeit imperfect, measures of democracy.

Several measures of democracy, not surprisingly, are available. The Freedom House, for example, constructs measures of civil and political rights, which many authors use to construct a composite measure of democracy. More recently, the World Bank has created a composite measure of "voice and accountability" that uses the Freedom House measure, among others. The Polity IV measure quantifies democratic and autocratic characteristics of governing institutions and subtracts the autocracy score from the democracy score for its composite index. Both of these measures, however, include violent conflict in their scores (it reduces democracy in both cases), which biases the measurement of democracy downward during conflict and upwards post-conflict. Unfortunately, the components of the Freedom House measures, and by extension the World Bank measure, are not readily available and we are unable to decompose these measures net of conflict. We can, however, examine the components of the Polity IV measures of democracy and autocracy. Two components of the Polity score contain conflict as a criterion (Vreeland 2008). We follow Vreeland, and subtract the *Regulation of Participation* and *Competitiveness of Participation* components of the democracy score as these measures include aspects of conflict. While Vreeland's revised polity score is our preferred measure of democracy, to examine the robustness of our results to alternative specifications, we compare our revised measure of democracy to the Freedom House measure.

With regards to the nature of the democracy data, we follow Treisman (2011) in arguing that democracy is 'mostly continuous.' The Polity IV and Freedom House data attempt to measure democracy across a range of possible outcomes, from the complete lack of democracy (freedom) to a completely democratic (free) society.

These measures are imperfect, however, we treat them as continuous variables for the sake of analysis. Still, in section 3.7 we relax this assumption and use the Wooldridge (2005) method to treat these as count variables because of their integer values.

To build our final data set, we include control variables from a variety of sources. We use data on Gross Domestic Product (GDP) and economic openness (the sum of imports and exports as a share of GDP from the Penn World Tables (Heston, Summers, and Aten 2011). We obtain population and other socio-economic data from the World Development Indicators (2012). For consistency with the literature, we also include a measure of human capital using Barro and Lee's education measure that takes the average number of years of schooling of the population over the age of 25. Finally, we construct a measure of natural resource rents as a share of GDP.[1]

Data on conflict come largely from the Correlates of War dataset (Sarkees and Wayman 2010). The Correlates of War defines a civil war as between a government and one or more armed internal non-state groups and requires 1,000 battle related deaths per year to qualify for inclusion. They use these criteria to assign a date to conflict termination. Using this data, we are able to include the duration of a war, and the type of war termination experienced (Stalemate or Rebel Victory). We also build on Sambanis and Doyle's (Doyle and Sambanis 2000) data set for UN intervention and add observations from the UN (Sarkees and Wayman 2010). Table 1 defines the variables used in the empirical model and their sources. Tables 2 and 3

present descriptive statistics of these variables, the sample countries and time periods, respectively.

For each country in the sample, we have potentially one observation for each of the sub-periods (1970-1974, 1975-1979, 1980-1984, 1985-1989, 1990-1994, 1995-1999, 2000-2004). We use five-year periods because the annual data are noisy and we are concerned that using annual data may result in spurious correlations. Second, we seek to avoid short-term fluctuations and focus on changes in the variables across longer swaths of time (Fortna and Huang 2012). We also investigate whether the results are robust to alternative measures of democracy, estimators, control variables, and instrument count (see: Jensen and Wantchekon 2004; Roodman 2008; Wantchekon 2004). Combining the Polity data with data extracted from other sources results in a dataset of 620 observations. The final panel data set is unbalanced and covers 96 countries from 1970 to 2004.

-Tables 1-3 here-

We define the dependent variable, *Democracy*, as the Polity IV score for democracy net of the *Regulation of Participation* and *Competitiveness of Participation* components of the democracy score. For robustness we define *Democracy-Alternate* as the Freedom House measure of democracy. Following the Correlates of War database (Sarkees and Wayman 2010), we define war as a dummy variable that is equal to one if a war starts or is ongoing in a period, 0 otherwise. The end of conflict (*War End*) is also a dummy variable, coded 1 if a war ends in the period and 0 otherwise. Likewise, we create dummy variables to capture a rebel victory (*Rebel Win)*, United Nations military intervention (*U.N. Intervention*), and a count variable

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to capture the duration of the conflict in years at its conclusion (*Duration*). A matrix *X* of control variables includes population, population density, GDP per capita, natural resource endowments, and openness to international trade. (Gleditsch 2002; Levine and Renelt 1992).

We employ the following estimation strategy to estimate the impact of civil war on democratization.

$$\text{Democracy}_{i,t} = \alpha_0 + \beta\, \text{Democracy}_{i,t-1} + \tau\, w_{i,t-k} + \gamma X_{i,t-k} + c_i + \lambda_t + u_{i,t} \qquad (1)$$

where c_i and λ_t denote the unobserved country and time effects. The subscripts i, k, and t denote country, lags, and time period, respectively. The binary indicator, w, indicates whether a war has ended. The coefficient τ captures the treament effect of interest. We assume that the error term, $u_{i,t}$, follows a random walk. The error components' specification accounts for time-invariant characteristics that may influence the development of democracy, to include colonial hertitage, geographic location, and cultural characteristics, among others. The specification also accounts for unobservable global trends that may also influence the development of democracy.

3.2   Econometric Issues

We began by examining whether the variables of interest exhibit a unit root process as the presence of a unit root, unless *N* and *T* grow large, is likely to induce inconsistent and biased estimates (Baltagi 2008). We employ the Fisher test to examine the null hypothesis that all the series are stationary versus the alternative that at least one series is stationary (Maddala and Wu 1999). We reject the null

hypothesis of non-stationarity for our measure of democracy and the macroeconomic independent variables at the 1% level of significance.[2]

We first present results from pooled Ordinary Least Squares (OLS) and two-way error components estimators. We note that the pooled OLS model explicitly assumes the country-specific effects are equal to zero and, in the presence of persistent effects, is inconsistent. If one suspects country or time specific effects impact the dependent variable, that is, country or time period characteristics impact democracy, then one can take these effects into account using error component estimators. We examine whether to employ a fixed or random effects error components estimator using a Hausman test and reject the null hypothesis of the exogeneity of the components and the regressors at the 1% level of significance.[3] We thus conclude that the random effects GLS estimator is inconsistent and employ the less efficient, but consistent (under specific assumptions about the exogeneity of the policy instruments) Within or fixed effects estimator. In addition, using the fixed effects estimator, we reject the null hypothesis of homoscedasticity at the 1% level of significance.[4] We reject the null hypothesis of no serial correlation at the 1% level (Drukker 2003; Wooldridge 2001).[5] Finally, we reject the null hypothesis that the individual and time-specific effects are jointly equal to zero at the 1% level of significance and thus employ the two-way Within estimator throughout the remainder of the paper.

Within estimators, however, preclude the use of several time-invariant variables used in previous literature (Gassebner, Lamla, and Vreeland 2013). In addition, we must make caveats to employ the Within estimator. First, the policy

indicator (w) must be strictly exogenous to the u_{it}, else the Within estimator is inconsistent. If the policy assignment changes in reaction to past outcomes on y_{it}, then it violates strict exogeneity. In cases where $w_{it} = 1$ whenever $w_{ir} = 1$ for $r < t$, strict exogeneity is usually a reasonable assumption; however, this implies that once a war begins, it does not end or, conversely, that when there is no war at time r, there is no war at time t. Our interest lies in those cases where war in time r is succeeded by an end to conflict at time t, that is, the cases of $w_{it} = 0$ that are preceded by $w_{ir} = 1$. We are thus concerned that this treatment effect violates the strict exogeneity assumption, rendering the Within estimator inconsistent. We also note that the Within estimator may be biased and inconsistent in samples with large N and small T and the presence of a lagged dependent variable is mechanically correlated with the error term, violating its strict exogeneity (Perotti 1996).

Accordingly, we are immediately confronted with significant econometric issues that, if left uncorrected, are likely to result in inconsistent and biased estimates. As democracy may slowly change over time, it is also probable that the current level of democracy is dependent upon the level of democracy in the previous period. While there are significant variations in the level of democracy across countries, democracy is relatively stable within countries. Of the 96 countries in the sample, 26 experienced no change in the level of democracy throughout their sample period. The individual effects, characterizing the heterogeneity among countries, are a second source of persistence over time. Finally, we are concerned that some of the traditional determinants of democracy, including GDP per capita, are endogenous. Previous explorations of the determinants of democracy that do not

take these potential econometric issues into account are likely to be suspect, due to the inconsistent nature of their estimators.

Several instrument variable approaches are available to address systematic endogeneity, including using lags of the dependent variable as an explanatory variable. The Anderson-Hsiao Instrumental Variables (IV) estimator takes the first difference of all variables, then instruments for the first difference of the lagged dependent variable with the second lagged level of the dependent variable (Anderson and Hsiao 1982). This IV estimator is consistent but relatively inefficient in the presence of a lagged dependent variable and significant individual effects. The difference GMM estimator, on the other hand, is consistent, relatively more efficient than the IV estimator, and employs all available lagged levels of the dependent variable, beginning with the second lag, as instruments for the lagged difference of the dependent variable (Arellano and Bond 1991). We can also use the difference GMM estimator to treat regressors such as GDP per capita as endogenous, using second and deeper lagged levels of GDP as instruments for its first difference.

The persistence in the levels of education, natural resources, and democracy may account for the insignificant relationships in much of the literature employing fixed effects and various difference estimators (Aslaksen 2010). The difference GMM estimator, however, may also be inefficient because levels may not be good instruments for differences. Differences may be a superior instrument for the levels (Roodman 2006). Therefore, in addition to the difference GMM estimator, we employ a system-GMM estimator that uses all available lagged differences as instruments for the lagged levels (Arellano and Bover 1995; Blundell and Bond

1998). The short *T* and persistent series appear to support the extra moment conditions of the system GMM vice the difference GMM (Baltagi 2008). The system GMM estimator should thus produce dramatic efficiency gains over the difference GMM as the persistence effect of the dependent variable grows (Blundell and Bond 1998). Finally, regardless of the GMM estimator's form, GMM estimators offer standard errors that are robust to heteroskedasticity and serial correlation.

Researchers have several options available to them when using GMM estimators that incur important trade-offs. We report the results of several specifications, per Roodman's (2008) advice, to ensure robustness to specification choices. Specifically, we can execute GMM using a one or two step process. The one-step estimator provides estimated standard errors robust to heteroskedasticity and serial correlation. The two-step process is generally more efficient and naturally resilient to heteroskedasticity but tends to downward bias standard errors enough to make inference impossible when instrument counts are large (Arellano and Bond 1991). The two-step process with Windmeijer (Windmeijer 2000) corrected standard errors may ameliorate the problems with standard errors; we report this as well.

We also explore the sensitivity of our results to changes in the set of instruments. Instrument proliferation can overfit endogenous variables and weaken Hansen tests.[6] We collapse the instrument matrix and limit the number of lags to control for instrument proliferation (Roodman 2008). In some specifications, we employ forward orthogonal deviations which can preserve the size of a dataset with gaps, a problem encountered with our data, by using the future differences to instrument for past differences (Arellano and Bover 1995). For the purposes of this

paper, we present the unrestricted, one-step GMM estimates, the one-step estimates with collapsed instruments and a lag-limit of three, and the two-step estimates with collapsed, forward orthogonal instruments and a lag-limit of three.[7]

This approach allows us to compare and contrast the unrestricted GMM estimates with the restricted GMM estimates. The unrestricted model, by using all available lag lengths, allows for the possibility of deep lags influencing the current level of democracy. The cost of using deep lags, however, is that number of instruments proliferates and an overfit of the endogenous variable(s). We employ the Sargan test (for both the difference and system GMM estimators) and the Differences-in-Hansen test (for the system GMM estimator) to determine the appropriate restrictions, to include lag length, collapsing the instrument matrix, and using forward orthogonal deviations.

Finally, we report the results of several standard tests employed to validate GMM estimates. We test the hypothesis that the error term is serially correlated in the first order and not serially correlated in the second order. We test the validity of the moment conditions by using the Sargan test and robustness of additional moment conditions with the Hansen difference test. We recognize that any one estimator may have flaws, however, by examining the hypotheses of interest across different estimators, we argue that our results are appropriately conditioned to these flaws.

3.3   AR(1) specifications of democracy

We first regress democracy on its first period lag. While the OLS and Within Groups estimators are inconsistent, these estimators are likely to be biased in opposite directions and thus provide upper (OLS) and lower (Within Groups) bounds for the IV and GMM estimators (Bond 2002). If the estimated coefficient for the AR(1) model falls within these bounds, then we may proceed, with empirical evidence that the model is well specified. On the other hand, were the estimated coefficient on the supposedly consistent estimator to fall dramatically outside these bounds, one would suspect severe finite sample bias or inconsistency. We would, in this case, need to more rigorously test our underlying assumptions about the viability of the GMM estimators. Table 4 presents the AR(1) estimations of the democracy measure.

We estimate a first-order autoregressive model with year-specific effects to account for common cross-country shocks to the democracy variable. The first two columns present estimates of ρ with heteroskedasticity consistent standard errors. We reject the null hypothesis of no serial correlation at the 1% level for the OLS and Within Groups estimators. The third column is the just identified, consistent, Anderson-Hsiao IV estimator. As expected, the OLS estimate forms a lower bound while the Within Groups estimate forms an upper bound. The IV estimate lies within these bounds.

With regards to the GMM estimates, the unrestricted, one-step difference GMM estimate is within the expected bounds. We reject the null hypothesis of the Sargan test, however, suggesting that the model is over identified. When we restrict the lag-length on the instruments and collapse the instrument matrix, the estimate is

marginally above the OLS estimate of ρ. Using forward orthogonal deviations in conjunction with lag-length limits, a collapsed instrument matrix, and the Windmeijer correction produces a higher estimate of ρ. We suspect finite sample bias may make the difference GMM estimator inefficient.

Turning to the system GMM estimator, the estimated coefficients for ρ are positive, lie within the established bounds, and are statistically significant at the 1% level. While we reject the null hypothesis of the Sargan test with the unrestricted, one-step estimator, suggesting the model is overidentified, constraining the instrument matrix appears to be an appropriate correction. We reject the null of exogeneity using the difference-in-Hansen test for the two-step system GMM estimator with a lag-limit of three and collapsed and orthogonal instruments.

We thus have evidence to support our argument that the *AR(1)* model is well specified for the democracy series and the ranking of the OLS, Within Groups, and IV estimators is consistent with our *a priori* expectations. While the difference GMM estimator may be subject to finite sample bias, the system GMM estimator lies within the established bounds, regardless of restrictions on lag-length or the composition of the instrument matrix. These results suggest the system GMM estimator is most appropriate to test our hypotheses of interest; we will, however, continue to present the results of the other estimators for comparative purposes.

- Table 4 here -

3.4   Democracy and Termination of Civil Wars

A priori, we would believe, on the basis of the literature and political statements, that the termination of a civil war is an opportunity for democracy to 'flourish.' We would expect a positive and statistically significant relationship between the termination of a civil war and democracy in the succeeding period. As before, we employ the OLS and Within Groups estimators to establish the bounds for the IV and GMM estimators (Table 5).

We find that the end of a civil war *negatively* impacts democracy in the succeeding five-year period. This relationship is consistent and statistically significant across the IV, difference GMM, and system GMM estimators. The relatively inefficient IV estimator suggests that the termination of a civil war in time *t-1* leads to a decline in the democracy score by 3.35 in time *t*. The affect is similar in the difference GMM models, except in the most restrictive model (15). We fail to reject the null hypotheses of over-identification in the one-step model with collapsed instruments (14) and the fully restricted model (15). The estimated coefficient from the difference GMM estimators, particularly model (15), with a lag-limit of 3 and a collapsed, orthogonal instrument matrix, are however, well outside the upper bound established by the OLS estimator, suggesting again that the difference GMM estimator may not be appropriate to test the hypotheses in question.

With regards to the system GMM estimator, we find that the termination of a civil war leads to a decline in the democracy score. The democracy score declines by approximately 2 in the unrestricted system GMM model (16) to approximately 1.32 in the two-step estimator with three lags, collapsed instruments based on forward orthogonal deviations (18). The estimated coefficients for the system GMM model

are statistically significant at the 1% level, except for the two-step estimator, which is at 10%, most likely due to the downward biased standard errors. We fail to reject the null hypotheses that the moment conditions are valid and that instruments are exogenous in the one-step model with collapsed instruments (17) and the fully restricted model (18), suggesting that the unrestricted model (16) is overidentified.

We thus conclude that the termination of civil war negatively impacts democracy in the succeeding period. Our results for this bivariate estimation are consistent whether we limit the instruments, collapse the instrument matrix, employ forward orthogonal deviations as instruments, or use the Windmeijer correction. These results suggest that there is a lack of empirical evidence for democracy flourishing after the conclusion of civil wars, and that, in practice, war termination often leads to a consolidation of power away from democracy.

-Table 5 here-

3.5   Robustness Checks

We now turn to the question of whether the estimated coefficient for the termination of civil war is robust to the inclusion of additional explanatory variables and changes in the set of instruments. Our set of conditioning variables includes per-capita GDP, openness to international trade, population, natural resource rents as a and share of GDP, and average education.

We find, as with the bivariate regressions, that the termination of a civil war negatively affects democratization in the succeeding five-year period (Table 6). For the difference GMM estimators, we fail to reject the null hypothesis that the moment conditions are valid for each of the models. The estimated coefficient for lagged

democracy is within expected bounds and the coefficient for civil war termination is statistically significant at 1% in the unrestricted model and 5% in the restricted models. The marginal effect of civil war termination for the difference GMM models with controls is slightly larger than the bivariate difference GMM models, with the termination of a civil war leading to reduction of approximately 3 points in the unrestricted model (21) to approximately 4.83 points for the fully restricted model (23).

For the system GMM estimator, we fail to reject the null hypotheses that the moment conditions are valid and that the instruments are exogenous for the one-step (25) and two-step restricted models (26). We do reject the null hypothesis that the moment conditions are valid for the unrestricted model (24) and thus conclude that the unrestricted model is overidentified. The estimated coefficient for lagged democracy is within expected bounds and the coefficient for civil war termination is statistically significant at 1% for the restricted system GMM models. War termination leads to a decline in democracy by approximately 2 points in the succeeding period.

We also note that, in our preferred system GMM estimates, education appears to positively influence democratization, a result previously found in the literature. Education is significant at the 5% level (25) and at the 1% level (24, 26). The estimates for education, however, appear sensitive to the choice of estimator, as the estimated coefficients are insignificant for the IV and difference GMM estimators.

Finally, we find scant evidence to corroborate significant impacts from the control variables suggested in the literature. Our results cast doubt on the suggestion

that countries experiencing civil war democratize for the same reasons as those unaffected by civil war (Fortna and Huang 2012). Despite any impacts war may have on development, or that development may have on war, countries that have experienced war have lower subsequent levels of democratization.

-Table 6 here-

3.6   Characteristics of Civil War

The evidence to this point strongly suggests that the termination of a civil war leads to a decline in democracy in the succeeding period, although the magnitude of this effect depends on the choice of estimator and restrictions on instruments. Controlling for the end of civil war, we now explore whether the characteristics of its termination impact democratization in the succeeding period. We examine whether the duration of a civil war, whether the war ends with a stalemate, whether the rebels win the civil war, and whether the UN intervenes at the termination of the civil war have a significant impact on democratization.

We present the estimates from the difference and system GMM estimators in Table 7. We continue to use the control variables presented in the previous section.[8] For the restricted difference GMM models (28, 29), the estimated coefficient for the end of a civil war is no longer statistically significant. We do find, however, that UN intervention is positive and statistically significant at the 1% (27, 28) and 5%(29) level. While duration appears to affect democratization, the result is fragile and becomes statistically insignificant in the fully restricted difference GMM model. Likewise, rebel victory appears to lower democratization in the succeeding period, although the estimated coefficient becomes insignificant in the most restrictive

model. The unrestricted difference GMM estimator (27) appears overidentified as we reject the null hypothesis that the moment conditions are appropriate. We again caution that the difference GMM estimator is likely to be less efficient than the system GMM estimator and may suffer from finite sample bias.

Turning to the preferred system GMM estimators, the estimated coefficient for the termination of civil war is negative and, unlike the difference GMM estimator, statistically significant at the 5% level for all models. UN intervention is positive and statistically significant while rebel victory is negative and statistically significant at the 1% level in all models. Duration, on the other hand, appears to have a negative impact on democratization in the succeeding period although this result is fragile to restrictions on the instruments. We once again reject the null hypotheses of proper moment conditions and exogeneity of the instruments for the unrestricted model (30). For the restricted models (31, 32), we fail to reject these nulls, making these our preferred estimates.

These results suggest that the conditions under which a civil war ends are important indicators of a country's subsequent political development. The positive impact of UN intervention suggests that an outside party can serve to ameliorate the adverse effect of war termination on democratization. Rebel victories, however, appear to have a negative affect on democratization, suggesting that rebel movements are either unprepared for democratic governance or are using claims of fighting for democracy as a form of propaganda.

3.7   Alternative measures of democracy

Next, we turn to the question of whether our measure of democracy influences the results above. We construct two alternative measures of democracy that range from 1979 to 2004. The first measure is the adjusted Polity IV democracy score. We derive the second measure from the Freedom House's measures of civil liberties and political rights, and normalize both measures of democracy for comparability with 0 being a complete lack of democracy and 1 being completely democratic. We also constrain the data to the same samples for this test. We continue to caution that the Freedom House measure of democracy is biased by the inclusion of freedom from war as one of its criteria, and we are unable to ascertain the extent to which war affects the measures of civil liberties and political rights. This bias should lead to a positive bias in the post-war coefficient. For the discussion in this section, we only present the system GMM estimates given our previous concerns about the difference GMM estimator.

We first compare results of the bivariate estimations for the Normalized Polity and Freedom House scores. The estimated coefficients for the termination of civil war are negative and statistically significant in both models, although the coefficient for the Freedom House measure of democracy is biased upward, as expected. For the system GMM estimator with collapsed, orthogonal instruments and Windmeijer corrected standard errors, we reject the null hypothesis of the exogeneity of instruments for the Normalized Polity model but we fail to reject for the Freedom House model.

For the fully specified models, we reject the null of proper moment conditions for the unrestricted models (35, 36) and proceed to estimate the most restricted models (37, 38). We fail to reject the null of proper moment conditions and instrument exogeneity for the restricted models.

The estimated coefficient for the end of a civil war is negative and statistically significant at the 5% level in all models, continuing to support the previous results of a negative impact of civil war termination on democratization. UN intervention is positive and statistically significant at the 5% level in all models except the restricted Freedom House model. Rebel victory is insignificant in both restricted models, although it is negative and significant in both unrestricted models. The negative coefficients for the Freedom House model appear to be upward biased relative to the Normalized Polity model.

3.8   Alternate dynamic specification

Considerable debate has arisen in the literature as to the appropriate way to measure Democracy. Bollen (1990), Przeworski et. al. (2000), Elkins (2000), Munck and Verkuilen (2002), among many others, argue whether democracy is discrete or continuous. More recent discussion is whether democracy should be treated as dichotomous, ordinal, or continuous. In order to address possible concerns, here we treat Democracy as discrete, and model it as a count variable. We feel a dynamic model is the most appropriate of these kinds of models because the level of democracy is slow to change. Thus, we have converted the POLITY data to non-negative integers and have followed Wooldridge's (2005) technique for estimating a dynamic count model. In addition to allowing integer values, Wooldridge's model

controls for fixed effects by modelling them on initial conditions of independent variables.

Table 9 shows the results using this model, which generally support the same conclusions as our other models. Specifically, lagged democracy is an important determinant of present democracy. War termination significantly and negatively effects subsequent democracy. UN intervention and stalemates both have significant positive impacts on subsequent democracy. These results are all significant at the 1% level.

4.   DISCUSSION AND CONCLUSION

The continuing discussion of intervention in Syria is couched in the language of freedom, democracy, and civil liberties. President Obama, in a meeting with the Emir of Qatar, stated that the two nations are seeking to remove Syrian President Bashar al-Assad and "strengthen an opposition that can bring about a democratic Syria that represents all people and respects their rights (Talev 2013)." UN Secretary-General Ban Ki-moon noted recently that, "The prospects may seem dim, but I remain convinced that a political solution is possible. This is the only way to end the bloodshed and bring about a new and democratic Syria" (Yan 2013).

We find scant evidence to support these policy statements. Instead, we find empirically robust evidence that the termination of a civil war negatively impacts democracy in the succeeding period. This evidence appears to be robust and statistically significant across a number of specifications, instrument sets, and measures of democracy. While many hope that the end of internal conflict will

promote the emergence of a democratic society, our findings suggest that the post-conflict environment leads to more authoritarian regimes. Moreover, we find that it is unlikely that rebel victories will assure democratic transitions. Evidence suggests that rebels are more likely to undermine existing democratic institutions than to implement reforms. Supporting stalemates, not rebels, appears to be a better policy solution for promoting democratization.

We do find evidence to suggest that external intervention, through the United Nations, may increase democratization in the succeeding period. This finding appears relatively robust. We argue that this appears to support the argument that outside intervention can promote democratization after a period of internal conflict, a situation where democratization is not otherwise likely. The parties may require an independent arbiter not only to separate them, but also to moderate discussion and the emergence of democracy.

The findings in this paper suggest that further research is needed into the impact of civil war on institutions and into government capacity more broadly. Measures of institutional development, such as corruption and the rule of law may also be affected by civil war and may be equally important to democratic representation as long-run indicators of political well being. Moreover, our future research aims include investigating how post-war countries allocate expenditures in order to understand under what circumstances countries break out of the development trap that is civil war.

A more difficult question is whether or not the termination of civil war is endogenous with respect to democratization. If lower levels of democracy lead to

civil war, then does the onset of civil war lead to lower levels of democracy and longer war duration (or a lack of termination)? As the onset and termination of civil wars are discrete events, this creates the question of how to approach a dynamic estimation of the influence of civil war termination on democratization. Our initial evidence suggests that this question will be of increased interest in the future.

Taken together, this paper's findings suggest caution: merely negotiating a conclusion to civil war is insufficient to promote democracy. We find that successful rebellions are unlikely to lead to democracy despite their rhetoric. External intervention, and potentially stalemates at the end of conflict, appears to support the movement towards a more democratic and representative society.

Endnotes

[1] We obtained similar results examining rents as a share of population and excluding all but oil rents. These results are available upon request.

[2] We run the Fisher test without and with a trend variable for *democracy*, *log of GDP, openness to international trade*, and *population*, among others. Detailed test statistics are available upon request.

[3] Comparing a two-way random effects GLS estimator and a two-way Within estimator, we reject the null hypothesis that the differences in the two sets of estimated coefficients are not systematic with a Chi-squared test with 11 degrees of freedom and a resultant test statistic of 23.07.

[4] We employ a Breusch-Pagan test and reject the null hypothesis of homoskedasticity with a Chi-squared test with 1 degree of freedom and resultant test statistics of 13.95 and 56.89 for the Within estimator without and with a lagged dependent variable, respectively.

[5] We employ the Wooldridge test for autocorrelation in the panel data and reject the null hypothesis of no first-order autocorrelation with a $F(1,87)$ test statistic of 39.802 and 92.771 for the Within estimator without and with a lagged dependent variable, respectively.

[6] GMM estimators with too many moment conditions can be subject to overfitting biases in small samples (Bond 2002). We thus compare the unrestricted and restricted estimates and the loss of information from deep lags is thought to be minimal.

[7] We also estimate the one-step GMM estimator with the lag-limits set to three or greater; the one-step GMM estimator with the lag-limits set to three, and the one-step GMM estimator with collapsed, forward orthogonal instruments. These estimates are available upon request.

[8] The full results, including estimated coefficients and standard errors for the control variables, are available upon request.

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Table 1
Variables

Variable	Definition	Units	Source
Democracy (Polity)	The degree of openness of democratic institutions.	-6 – 7	Polity IV with Vreeland (2008) modification.
Population	Natural log of population at start of period.		Penn World Tables 7.0 Heston, Summers, Aten (2011)
GDP Per Capita	Natural log of purchasing power parity adjusted GDP per capita at the start of the period.		Penn World Tables 7.0 Heston, Summers, Aten (2011)
Openness to International Trade	Measured as the sum of exports and imports as a share of GDP		Penn World Tables 7.0 Heston, Summers, Aten (2011)
Education	Measures the average number of years of schooling of the population over the age of 25.		www.barrolee.com
Rents to GDP	Measures the difference between the value of production of natural resources and total costs of production. This is a cumulative measure of oil, natural gas, mineral, coal, and forest rents.		World Development Indicators http://databank.worldbank.org
War End	Takes the value of 1 if a civil war ended during the period.	0,1	Correlates of War Sarkees and Wayman (2010)
Stalemate	Takes the value of 1 if a civil war ended in a stalemate during the period.	0,1	Correlates of War Sarkees and Wayman (2010)
Rebel Victory	Takes the value of 1 if a civil war ended in a rebel victory during the period.	0,1	Correlates of War Sarkees and Wayman (2010)
Duration	Evaluated in the period the conflict ends; it takes on the number of years a conflict was ongoing.		Correlates of War Sarkees and Wayman (2010)
U.N. Intervention	This variable takes the value of 1 if a war ended and there was UN intervention during the period.	0,1	Doyle and Sambanis (2000) and http://www.un.org/en/peacekeeping/operations

Table 2
Descriptive Statistics

Series	N	Mean	Standard Deviation	Minimum	Maximum
Democracy (Polity)	620	1.91	4.56	-6	7
Population	620	45,397	141,628	455.15	1,300,000
GDP Per Capita	620	7,760	9,080	345.97	44,813
Openness to International Trade	620	64.74	44.33	5.31	412.16
Rents to GDP	620	7.03	10.17	0	74.68
Average Years School	510	5.26	3.027	0.23	13
War End	620	0.08	0.27	0	1
Duration	620	0.28	1.56	0	20
U.N. Intervention	620	0.01	0.11	0	1
Democracy (Freedom House)	533	0.55	0.32	0	1
Democracy (Normalized)	620	0.61	0.35	0	1

Table 3
Sample Countries

Country	Years	Country	Years
Algeria	1970-2004	Japan	1970-2004
Argentina	1970-2004	Kenya*	1970-2004
Australia*	1970-2004	Lesotho	1970-1999, 2005-2004
Austria*	1970-2004	Madagascar	1970-2004
Bangladesh	1975-2004	Malawi	1970-2004
Belgium*	1970-2004	Malaysia	1970-2004
Benin	1975-1989, 1999-2004	Mali	1970-2004
Bolivia	1970-2004	Mauritania*	1970-2004
Botswana	1970-2004	Mexico	1970-2004
Brazil	1970-2004	Morocco	1970-2004
Burkina Faso	1970-2004	Namibia*	1990-2004
Burundi	1970-1994, 2000-2004	Nepal	1970-2004
Cameroon	1970-2004	Netherlands*	1970-2004
Canada*	1970-2004	Nicaragua	1970-1979, 1985-2004
Central African Rep.	1970-2004	Niger	1970-2004
Chad	1970-1979, 1985-2004	Nigeria	1970-2004
Chile	1970-2004	Norway*	1970-2004
China	1970-2004	Pakistan	1975-2004
Colombia	1970-2004	Papua New Guinea*	1975-2004
Congo	1970-2004	Paraguay	1970-2004
Costa Rica*	1970-2004	Peru	1970-1999
Cote d'Ivoire	1970-2004	Philippines	1970-2004
Cuba*	1970-2004	Portugal	1970-1974, 1980-2004
Dem. Rep. of the Congo*	1970-1994	Qatar	1975-2004
Denmark*	1970-2004	Romania	1970-2004
Dominican Rep.	1970-2004	Rwanda	1970-2004
Ecuador	1970-2004	Senegal	1970-2004
Egypt*	1970-2004	Sierra Leone	1970-1999
El Salvador	1970-1979, 1985-2004	Singapore*	1970-2004
Fiji	1970-1999	South Africa*	1970-2004
Finland*	1970-2004	South Korea	1970-2004
France	1970-2004	Spain	1970-1974, 1980-2004
Gabon	1974-1989, 1999-2004	Sri Lanka	1970-2004
Gambia	1970-1989, 1995-2004	Sudan	1975-1984, 1990-2004
Germany*	1994-2004	Swaziland	1970-2004
Ghana	1970-2004	Sweden*	1970-2004
Greece	1970-2004	Syria	1970-2004
Guatemala	1970-1984, 1990-2004	Thailand	1970-2004
Guyana	1970-2004	Togo	1970-2004
Haiti	1970-2004	Trinidad and Tobago*	1970-2004
Honduras	1970-1979, 1985-2004	Tunisia	1970-2004
Hungary	1970-2004	Turkey	1970-2004
India	1970-2004	Uganda	1970-1984, 1990-2004
Indonesia	1970-2004	United Kingdom*	1970-2004
Iran	1970-1979, 1985-2004	United States*	1970-2004
Ireland*	1970-2004	Uruguay	1970-2004
Israel*	1970-2004	Venezuela	1970-2004
Italy*	1970-2004	Zambia	1970-2004

*Indicates no variation in X-POLITY score for the duration

Table 4
Estimates of the *AR(1)* specifications for Democracy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	Fixed Effects	IV	Difference GMM	Difference GMM	Difference GMM	System GMM	System GMM	System GMM
Democracy$_{t-5}$	0.820**	0.318**	0.660**	0.579**	0.833**	0.898**	0.650**	0.754**	0.787**
	(0.0234)	(0.0719)	(0.202)	(0.153)	(0.159)	(0.145)	(0.092)	(0.163)	(0.127)
Constant	0.321+	0.251	-0.389	---	---	---	1.504**	1.144*	0.994**
	(0.194)	(0.248)	(0.354)				(0.421)	(0.511)	(0.325)
Observations	514	514	411	411	411	411	514	514	514
Adjusted R²	0.698	0.283	---	---	---	---	---	---	---
M1	-2.608**	73.169**	-4.732**	-4.602**	-2.762**	-2.920**	-4.544**	-2.493**	-2.964**
M2	0.244	---	1.568	1.490	1.426	1.462	1.557	1.325	1.448
Lag Limits	---	---	---	All	3	3	All	3	3
Number of Instruments	---	---	---	20	7	7	26	9	9
Collapsed / Orthogonal	---	---	---	---	Collapsed	Collapsed Orthogonal	---	Collapsed	Collapsed Orthogonal
One step or Two	---	---	---	One	One	Two	One	One	Two
Sargan test	---	---	---	38.217**	2.250	1.957	47.188**	2.912	3.635
Diff. in Hansen test	---	---	---	---	---	---	3.307	1.654	3.061+

Notes: Year dummies included in all models. *m1* and *m2* are tests for first-order and second-order serial correlation, asymptotically N(0,1). *m1* test for Within estimator is the Wooldridge F-test. The just-identified 2SLS estimator is for the equation in first differences, using democracy$_{t-10}$ as the instrumenting variable. GMM results are one-step estimates with heteroskedastically-consistent standard errors and test statistics. *Orthogonal* is the forward orthogonal deviations transform instead of first differencing. Sargan test is a test of overidentifying restrictions for the GMM estimators. The difference-in-Hansen test is a test of the exogeneity of the instruments for the lagged democracy variable with the null hypothesis that the instruments are exogenous. **,*,+ denote significance at the 1%,5%, and 10% level respectively.

Table 4A
(NOT FOR PUBLICATION)
Additional Alternative estimates of the *AR(1)* specifications for Democracy

	Difference GMM	Difference GMM	Difference GMM	System GMM	System GMM	System GMM
Democracy$_{t-5}$	0.669**	0.704**	0.942**	0.716**	0.602**	0.806**
	(0.141)	(0.143)	(0.146)	(0.103)	(0.111)	(0.151)
Constant		---	---	1.272**	1.671**	0.964*
				(0.397)	(0.435)	(0.451)
Observations	411	411	411	514	514	514
M1	-3.177**	-2.762**	-2.800**	-2.844**	-4.764**	-2.545**
M2	1.580	1.426	1.438	1.408	1.521	1.345
Lag Limits	3 and greater	3	3	3 and greater	3	3
Number of Instruments	15	12	7	20	12	9
Collapsed / Orthogonal	---	---	Collapsed Orthogonal	---	---	Collapsed Orthogonal
One or Two Step	One	One	One	One	One	One
Sargan test	25.747**	15.074**	1.957	28.494**	2.912	3.635
Diff. in Hansen test	---		---	2.331	1.654	3.061+

Notes: See Table 4.

Table 5
Democracy and War Termination

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	OLS	Fixed Effects	IV	Difference GMM	DifferenceGMM	Difference GMM	System GMM	System GMM	System GMM
Democracy$_{t-5}$	0.819**	0.344**	0.769**	0.654**	0.830**	0.939**	0.666**	0.614**	0.748**
	(0.023)	(0.065)	(0.215)	(0.127)	(0.164)	(0.159)	(0.077)	(0.130)	(0.111)
War End $_{t-5}$	-2.307**	-1.631**	-3.523*	-3.351**	-3.831**	-4.795*	-2.022**	-2.025**	-1.319+
	(0.574)	(0.588)	(1.284)	(1.137)	(1.378)	(1.865)	(0.625)	(0.633)	(0.771)
Constant	0.480**	0.371	-0.295	---	---	---	1.568**	1.747**	1.244**
	(0.199)	(0.255)	(0.372)				(0.356)	(0.431)	(0.331)
Observations	514	514	411	411	411	411	514	514	514
Adjusted R²	0.710	0.300	---	---	---	---	---	---	---
M1	-2.223*	92.804**	-4.763**	-4.800**	-2.964	-3.273	-4.518**	-2.982**	-3.232**
M2	-0.132	---	1.269	0.444	0.554	0.373	0.945	0.752	1.165
Lag Limits	---	---	---	All	3	3	All	3	3
Number of Instruments	---	---	1	21	8	8	33	16	16
Collapsed / Orthogonal	---	---	---	---	Collapsed	Collapsed Orthogonal	---	Collapsed	Collapsed Orthogonal
One or Two Step	---	---	---	One	One	Two	One	One	Two
Sargan test	---	---	---	35.561**	0.903	0.258	52.669**	9.181	9.989
Diff. in Hansen test	---	---	---	---	---	---	3.271	0.433	1.880

Notes: See Table 4. **,*,+ denote significance at the 1%,5%, and 10% level respectively.

Table 5a
NOT FOR PUBLICATION
Additional Estimates:
Democracy and War Termination

	Difference GMM	Difference GMM	Difference GMM	System GMM	System GMM	System GMM
Democracy$_{t-5}$	0.674**	0.706**	0.968**	0.654**	0.630**	0.736**
	(0.146)	(0.148)	(0.170)	(0.095)	(0.095)	(0.110)
War End $_{t-5}$	-3.370**	-3.424**	-5.104*	-2.022**	-2.024**	-2.036**
	(1.211)	(1.229)	(2.048)	(0.627)	(0.630)	(0.625)
Constant	---	---	---	1.608**	1.693**	1.325**
				(0.369)	(0.373)	(0.344)
Observations	411	411	411	514	514	514
M1	-3.293**	-3.366**	-3.049	-3.178**	-3.156	-2.997**
M2	0.463	0.509	0.333	0.856	0.817	0.916
Lag Limits	3 and greater	3	3	3 and greater	3	3
Number of Instruments	16	13	8	27	24	16
Collapsed / Orthogonal	---	---	Collapsed Orthogonal	---	---	Collapsed Orthogonal
One or Two Step	One	One	One	One	One	One
Sargan test	26.376**	15.924**	0.258	38.602**	34.457**	9.989
Diff. in Hansen test	---	---	---	4.906	4.366	1.880

Notes: See Table 4. **,*,+ denote significance at the 1%,5%, and 10% level respectively.

Table 6
Democracy, War Termination, and Controls

	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)
	OLS	Fixed Effects	Difference GMM	Difference GMM	Difference GMM	System GMM	System GMM	System GMM
Democracy$_{t-5}$	0.717**	0.542**	0.558**	0.651**	0.703**	0.611**	0.580**	0.704**
	(0.0387)	(0.135)	(0.168)	(0.182)	(0.171)	(0.0869)	(0.146)	(0.109)
War End$_{t-5}$	-1.737**	-2.626**	-3.270**	-3.552*	-4.823*	-1.839**	-2.018**	-2.082**
	(0.623)	(0.969)	(1.220)	(1.430)	(2.085)	(0.666)	(0.704)	(0.777)
GDP Per Capita$_{t-5}$	0.212	-1.562	-1.396	-1.125	-2.525	-0.254	-0.604	-1.183**
	(0.156)	(1.439)	(2.366)	(4.246)	(3.479)	(0.301)	(0.597)	(0.423)
Population$_{t-5}$	-0.0897	1.544	3.040	3.111	0.447	-0.0906	-0.0259	0.247
	(0.110)	(2.229)	(3.030)	(3.484)	(2.688)	(0.202)	(0.345)	(0.310)
Openness$_{t-5}$	-0.662*	-0.479	0.141	1.278	1.410	-0.701	-0.534	0.296
	(0.322)	(1.274)	(2.042)	(2.719)	(1.953)	(0.602)	(1.082)	(0.893)
Rents Share GDP$_{t-5}$	-0.114*	0.507	2.371*	2.217	2.033	-0.170	-0.0500	-0.107
	(0.0577)	(0.533)	(1.049)	(1.551)	(1.277)	(0.133)	(0.154)	(0.144)
Log Education$_{t-5}$	0.552*	0.521	-1.011	-1.409	-0.256	1.401**	1.964*	2.000**
	(0.254)	(0.866)	(1.169)	(1.453)	(.990)	(0.487)	(0.873)	(0.600)
Constant	1.178					5.377	6.147	4.505
	(2.321)					(4.756)	(7.239)	(6.733)
Observations	492	394	394	394	394	492	492	492
Adjusted R^2	0.742							
M1	-1.036	-3.753**	-2.687**	-2.693**	-2.92**	-4.149**	-2.514*	-3.014**
M2	-0.171	0.401	0.530	0.436	0.16	0.534	0.415	0.527
Lag Limits			All	3	3	All	3	3
Number of Instruments		68	36	16		95	27	27
Collapsed/ Orthogonal				Collapsed	Collapsed/ Orthogonal		Collapsed	Collapsed/ Orthogonal
One or Two Step			One	One	Two	One	One	Two
Sargan test		71.919	23.888	3.236	0.54	98.897+	12.177	11.682
Diff. in Hansen test						10.206	0.652	1.377

Notes: See Table 4. **,*,+ denote significance at the 1%,5%, and 10% level respectively.

Table 6A -NOT FOR PUBLICATION
ADDITIONAL ESTIMATIONS - Democracy, War Termination, and Controls

	Difference GMM	Difference GMM	Difference GMM	System GMM	System GMM	System GMM
Democracy$_{t-5}$	0.558** (0.168)	0.751** (0.186)	0.703** (0.171)	0.613** (0.106)	0.610** (0.0996)	0.682** (0.118)
War End $_{t-5}$	-3.270** (1.220)	-5.276* (2.143)	-4.823* (2.085)	-1.863** (0.668)	-1.816** (0.656)	-2.163** (0.718)
GDP Per Capita $_{t-5}$	-1.396 (2.366)	-2.359 (3.878)	-2.525 (3.479)	-0.485 (0.366)	-0.387 (0.398)	-0.775 (0.592)
Population $_{t-5}$	3.040 (3.030)	0.479 (2.957)	0.447 (2.688)	0.0298 (0.234)	0.0943 (0.229)	0.0820 (0.337)
Openness$_{t-5}$	0.141 (2.042)	1.154 (2.029)	1.410 (1.953)	-0.269 (0.689)	-0.0505 (0.685)	-0.135 (1.077)
Rents Share GDP $_{t-5}$	2.371* (1.049)	2.093+ (1.220)	2.033 (1.277)	-0.208 (0.129)	-0.141 (0.133)	-0.0660 (0.148)
Log Education $_{t-5}$	-1.011 (1.169)	-0.140 (0.971)	-0.256 (0.990)	1.589** (0.445)	1.496** (0.451)	1.731* (0.756)
Constant				4.120 (5.007)	1.903 (5.126)	4.978 (7.089)
Observations	394	394	394	492	492	492
Adjusted R^2						
M1	-2.687**	-2.695**	-2.925**	-2.869**	-2.914**	-2.694**
M2	0.530	0.133	0.156	0.454	0.485	0.509
Lag Limits	3 and greater	3	3	3 and greater	3	3
Number of Instruments	36	16	16	71	59	27
Collapsed / Orthogonal	---	---	Collapsed Orthogonal	---	---	Collapsed Orthogonal
One or Two Step	One	One	One	One	One	One
Sargan test	23.888	0.544	0.544	80.002*	69.524*	11.682
Diff. in Hansen test				22.621	14.625	1.377

Notes: See Table 4. **,*,+ denote significance at the 1%,5%, and 10% level respectively.

Table 7

Democracy, War Termination, and War Characterstics

	(27)	(28)	(29)	(30)	(31)	(32)
	Difference GMM	Difference GMM	Difference GMM	System GMM	System GMM	System GMM
Democracy$_{t-5}$	0.554**	0.705*	0.530*	0.627**	0.574**	0.711**
	(0.140)	(0.305)	(0.237)	(0.0863)	(0.143)	(0.123)
War End $_{t-5}$	-1.179	-2.266	-2.015	-1.798*	-1.922*	-1.965*
	(0.753)	(1.839)	(3.149)	(0.879)	(0.899)	(0.936)
UN Intervention $_{t-5}$	4.319**	4.768**	3.219*	2.705*	2.519*	3.057**
	(1.152)	(1.075)	(1.442)	(1.091)	(1.240)	(0.830)
Duration $_{t-5}$	-0.243*	-0.218+	-0.245	-0.134+	-0.132+	-0.123
	(0.107)	(0.113)	(0.455)	(0.0708)	(0.0755)	(0.0783)
Stalemate $_{t-5}$	1.115	1.811	-1.645	3.003**	2.853*	1.610+
	(1.304)	(1.990)	(9.165)	(1.007)	(1.119)	(0.853)
Rebel Victory $_{t-5}$	-6.907+	-6.226+	-6.168	-4.681**	-4.774**	-4.264**
	(3.548)	(3.291)	(5.968)	(1.375)	(1.433)	(1.475)
Constant				4.433	6.609	5.967
				(5.033)	(7.688)	(7.721)
Observations	317	394	394	492	492	492
M1	-2.906**	-2.103*	-1.811*	-3.962**	-2.475**	-2.947**
M2	-1.184	0.160	-0.234	0.336	0.162	0.370
Lag Limits	All	3	3	All	3	3
Number of Instruments	41	18	18	83	32	32
Collapsed / Orthogonal	---	Collapsed	Collapsed Orthogonal	---	Collapsed	Collapsed Orthogonal
One or Two Step	One	One	Two	One	One	Two
Sargan test	38.251+	4.957	3.363	89.826*	14.67	14.25
Difference in Hansen test	---	---	---	24.353+	0.171	1.112

Notes: See Table 4. Controls include the log of GDP, population, openness to international trade, rents to GDP, and education. The full and additional estimations are available upon request. **,*,+ denote significance at the 1%,5%, and 10% level respectively.

Table 8
Alternative Measures of Democracy

	(33)	(34)	(35)	(36)	(37)	(38)
	Normalized Polity	Freedom House	Normalized Polity	Freedom House	Normalized Polity	Freedom House
Normalized Democracy$_{t-5}$	0.651**	---	0.507**	---	0.403*	---
	(0.138)		(0.130)		(0.190)	
Freedom$_{t-5}$	---	0.837**	---	0.789**	---	0.519**
		(0.159)		(0.126)		(0.143)
War End$_{t-5}$	-0.188**	-0.102*	-0.205*	-0.148*	-0.232*	-0.173*
	(0.056)	(0.046)	(0.0859)	(0.0663)	(0.0936)	(0.0705)
UN Intervention$_{t-5}$	---	---	0.198*	0.142*	0.187*	0.0809
			(0.0976)	(0.0678)	(0.0870)	(0.104)
Duration$_{t-5}$	---	---	-0.00665	0.00533	-0.00309	0.00173
			(0.00798)	(0.00468)	(0.0106)	(0.00902)
Stalemate$_{t-5}$	---	---	0.277*	0.0620	0.275+	0.175
			(0.107)	(0.0730)	(0.148)	(0.133)
Rebel Victory$_{t-5}$	---	---	-0.269+	-0.124*	-0.188	-0.105
			(0.147)	(0.0608)	(0.138)	(0.0843)
Constant	0.287**	0.138	0.596	0.413	1.172	0.838
	(0.095)	(0.093)	(0.490)	(0.353)	(0.837)	(0.648)
Observations	429	429	411	411	411	411
M1	-2.883**	-3.074**	-2.807**	-3.121**	-1.846*	-2.835**
M2	-0.998	-0.080	-1.390	-1.309	-1.352	-1.132
Lag Limits	3	3	3	3	3	3
Number of Instruments	14	14	45	45	30	30
Collapsed / Orthogonal	Collapsed Orthogonal	Collapsed Orthogonal	--	--	Collapsed Orthogonal	Collapsed Orthogonal
Controls	No	No	Yes	Yes	Yes	Yes
One or Two Step	Two	Two	One	One	Two	Two
Sargan test	11.28	5.919	64.988**	44.074*	13.656	13.185
Difference in Hansen test	6.862**	2.217	8.377	12.726	3.914	1.013

Notes: See Table 4. Controls include the log of GDP, population, openness to international trade, rents to GDP, and education. All estimations employ the System GMM estimator. The full and additional estimations are available upon request. **,*,+ denote significance at the 1%,5%, and 10% level respectively.

Table 9
Alternative Estimation

	(39)	(40)	(41)	(42)
	Polity Count	Polity Count	Polity Count	Polity Count
Polity Count$_{t-5}$	0.11**	0.11**	0.09**	0.10**
	(0.01)	(0.01)	(0.01)	(0.01)
War End$_{t-5}$	--	-0.19**	-0.20**	-0.33**
		(0.07)	(0.08)	(0.12)
UN Intervention$_{t-5}$	--	--	--	0.47**
				(0.11)
Stalemate$_{t-5}$	--	--	--	0.33**
				(0.10)
Duration$_{t-5}$	--	--	--	0.005
				(0.009)
Constant	1.16**	1.18**	1.51**	1.54**
	(0.05)	(0.05)	(0.24)	(0.24)
Observations	709	709	559	559
Number of Instruments	1	2	8	11
Controls	No	No	Yes	Yes

Notes: See Table 4. Controls include the log of GDP, population, openness to international trade, rents to GDP, and average years of schooling. The full and additional estimations are available upon request. **,*,+ denote significance at the 1%,5%, and 10% level respectively.