Article

Journal of Conflict Resolution
2017, Vol. 61(1) 56-83

DOI: 10.1177/0022002715569773
journals.sagepub.com/home/jcr
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Beyond Zeroes and Ones: The Intensity and Dynamics of Civil Conflict

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Abstract

There is a tremendous amount of variation in conflict intensity both across and within civil conflicts. Some conflicts result in huge numbers of battle deaths, while others do not. Conflict intensity is also dynamic. Conflict intensity escalates, de-escalates, and persists. What explains this variation? We take one of the most prominent explanations for the onset and occurrence of civil conflict—variation in economic conditions—and apply it to the intensity and dynamics of civil conflict. Using an instrumental variables strategy and a rich set of empirical models, we find that the intensity of conflict is negatively related to per capita income. We also find that economic conditions affect conflict dynamics, as poorer countries are likely to experience longer and more intense spells of fighting after the onset of conflict.

Keywords

civil wars, political economy, conflict, trade interdependence

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Over the last decade, a significant amount of research has sought to explain the *extensive* margin of civil conflict, that is, the causes of civil war onset and occurrence. Much less attention has been paid to variation at the *intensive* margin, that is, how many combatants lose their lives during battle. The amount of variation in the intensity of civil conflict is tremendous and multifaceted. Over the last half century, the number of combat deaths during a year of civil conflict has ranged from less than 100 to over 100,000. The intensity of civil conflict is also dynamic. Within conflict spells, the intensity of fighting can rise and fall sharply at some times and remain steady at others. Some conflicts are persistent, with fighting simmering at consistent levels over longer periods of time, while other conflicts become more volatile. We take one of the most prominent explanations for the onset and occurrence of civil conflict, the level of per capita income, and ask two questions: does variation in economic conditions affect the intensity of civil conflict and the dynamics of civil conflict? We find an affirmative answer to both.

Using cross-national data on the number of battle deaths during civil conflicts from 1960 to 2008, we find that the effect of per capita income on the number of battle deaths is both statistically and substantively meaningful. The best estimate from a Blundell and Bond (1998) model of the effect of income on battle deaths is that a unit change in the logarithm of per capita income leads to a reduction of 321 battle deaths in the current year and 720 deaths overall, after accounting for the full dynamic effect. The magnitude of these estimates is approximately twice as large as the analogous estimate that would be derived from analysis of only the extensive margin of conflict.

The second and the most important set of results provides estimates of conflict dynamics. Analyzing conflict severity allows us to estimate rich models of how conflicts evolve and persist over time. We initially describe the overall level of persistence of conflict intensity. To our knowledge, this is the first analysis to document the degree to which past conflict intensity affects future conflict intensity.[1] We find that conflict intensity is mean-reverting but persistent. In dynamic $AR(1)$ models describing the degree to which conflict intensity in period t affects intensity in period $t+1$, we find an average $AR(1)$ coefficient that is between 0.55 and 0.78. These estimates indicate that conflict intensity is persistent across time but does not tend to be explosive. The estimated $AR(1)$ coefficient governing the extensive margin of civil conflict is much larger than the estimated $AR(1)$ coefficient governing the intensity of conflict, suggesting that conflicts smolder, with low levels of fighting, but conflicts, in expectation, do not erupt in response to past fighting.

We then examine which factors can change the persistence of conflict intensity. We find that a country's income level has a significant effect on conflict dynamics. To make these results more tangible, we show how income affects the "half-life" of conflict, that is, the amount of time it takes a conflict to return to "normal" levels after a spike in intensity. For observed conflicts, in country-years in the top 5 percent of the income distribution, it takes less than one year for the deaths from a conflict shock to decline to half the level of the shock. In stark contrast, for country-years in the bottom 5 percent of the income distribution, it takes over nine years for the deaths from a conflict shock to decline to half the level of the shock.

Establishing causality is difficult with observational data. Throughout the analysis, we take seriously threats to establishing a relationship between income and conflict severity, such as the endogeneity of economic performance, spillovers across countries, and unobserved heterogeneity. For a variety of reasons, a country's level of civil conflict can influence its economic performance, and unobservable factors potentially influence both economics and civil conflict. To account for this endogeneity problem, we use an instrumental variables strategy where the economic performance of a country's export partners is an instrument for per capita income. This identification strategy is similar to Acemoglu et al. (2008), which studies the relationship between income and democracy. To establish causality, a valid instrument must satisfy the exclusion restriction, that is, that the instrument affects the explanatory variable of interest (per capita income) but be uncorrelated with the error term. The exclusion restriction here is plausible, requiring that economic fluctuations in a country's distant export destinations are related to civil conflict only through their effect on income. To make the exclusion restriction more plausible, we modify the Acemoglu et al. (2008) instrument by removing adjacent countries when calculating the per capita income of export partners, further reducing the potential for geographic spillovers and spatially correlated shocks that may violate the exclusion restriction. The instrument must also be strong for explaining the endogenous regressors. We show how our instrument is a sufficiently strong enough explanator for income to meet this criteria.

We also take seriously the possibility of measurement error. It is well known that precisely measuring the number of deaths from civil conflict is difficult. Measurement error in the dependent variable is only a potential problem because the dynamic models include lags of regressors that contain measurement error, and serially correlated measurement error may bias estimates when using dynamic models and panel-style instruments. We take a number of steps to assess the sensitivity of the estimates to this potential problem, all of which yield the conclusion that the dynamics of civil conflict are essential for our understanding of the conflict process.

This research represents an important addition to understanding conflict dynamics. Deaths from combat are one of the most immediate and direct consequences of civil conflict, so understanding variation in conflict intensity is of inherent importance. Furthermore, many of the most pressing policy questions regarding civil conflict also deal with dynamics. For example, once a conflict has broken out, understanding the conditions under which conflicts escalate or de-escalate should inform decisions over the appropriateness of outside actions, be they military or economic.

Although we extensively analyze the relationship between economic variation and conflict dynamics, our findings suggest that much remains to be learned from deeper inquiry into the evolution and dynamics of civil wars. We find that, among four other factors identified in the prior literature as correlates of civil war, ethnic fractionalization is most associated with prolonged conflict persistence. Oil-producing countries have conflicts that die out relatively quickly, possibly because oil-producing countries tend to be relatively wealthy. Countries with high religious fractionalization and mountainous terrain do not differ from other countries in terms of conflict persistence. Since we do not have valid instruments for these variables, the claims we can make regarding them are more limited than those regarding income. A deeper understanding of the micro- and macro-level relationships between these variables and the intensity and dynamics of civil conflict is a warranted next step.

Theory

There are several excellent survey articles that review recent advances in the study of civil war, so we only make a few relevant observations that motivate the study of the intensity and dynamics of civil conflicts (Blattman and Miguel 2010). Most importantly, the extensive margin of civil conflict and the number battle of deaths, although related, are distinct phenomena. The *extensive margin* is akin to well-known variables coding the onset or occurrence of civil war in a particular country-year observation. Although this is an important source of variation, there is also tremendous variation at the *intensive margin*, that is, how intense is conflict for a particular country-year. Figure 1 plots the distribution of the logged number of battle deaths for country-years with positive battle deaths, showing the magnitude of this variation. The number of battle deaths from civil conflict ranges from 0 to 115,000. The standard deviation for the number of battle deaths is over seven times as large as the sample mean.

One aspect of this variation is obvious—conflict intensity varies across conflicts; some conflicts are much costlier in terms of human lives than others. However, not all of this variation can be attributed to across-conflict differences. Civil conflicts are also dynamic phenomena, with conflict intensity rising and falling over time. In its decade long civil war, Angola experienced years with as few as 25 battle deaths from civil conflict and years with as many as 20,000 deaths.

Economic Conditions and Conflict Intensity

What factors might affect the intensity and dynamics of civil conflict? One of the most commonly studied explanations for the extensive margin of civil conflict—variation in economic conditions—is also plausibly related to the intensive margin of conflict. At least two mechanisms motivate the link between income and civil conflict: opportunity costs and state capacity. In opportunity cost theories, low per capita income increases the likelihood of civil conflict through the relative cost of rebellion. For an individual choosing between lawful participation in the economy and insurgency, economic downturns may increase the attractiveness of fighting

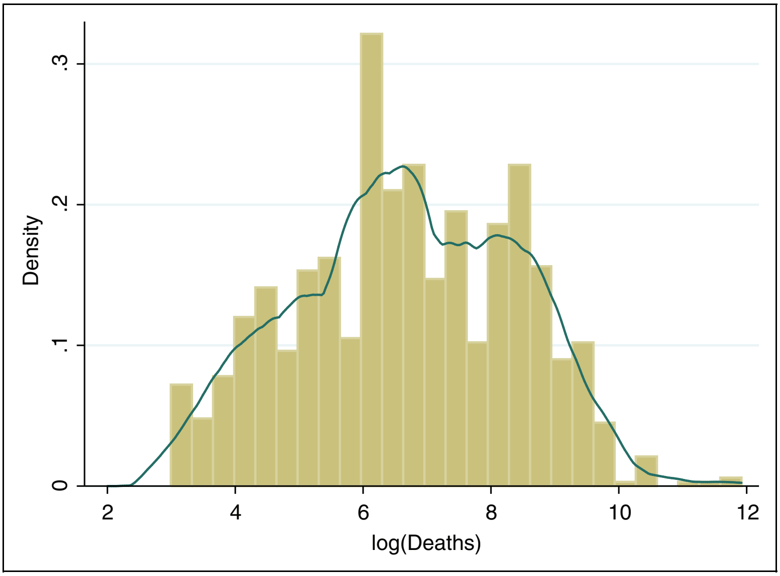

Figure 1. Distribution of log battle deaths in conflict years. Kernel density plot and histogram of log number of battle deaths for conflicts during years with positive numbers of battle deaths. The distribution is truncated at approximately three because the battle deaths data only contain years with at least twenty-five deaths.

relative to employment (Collier and Hoeffler 1998). The second mechanism describes the possibility that poor states may be unable to buy off or effectively suppress rebellious groups' capacity (Fearon and Laitin 2003). Poor economic conditions hinder the state's ability to provide public goods or placate a large enough subset of the population to avert armed rebellion.

The empirical work linking income conditions with the extensive margin of civil conflict has produced varying results. Among the most recent, Miguel, Satyanath, and Sergenti (2004) and Brückner and Ciccone (2010) find that economic growth, instrumented by a country's rainfall and export prices, respectively, decreases the probability of civil war in sub-Saharan Africa from approximately 1979 to 1999. Djankov and Reynal-Querol (2010) find no relationship between poverty and the probability of civil war using a broader sample and different estimators, though without instrumenting for income. Bazzi and Blattman (2014) find weak/inconsistent evidence linking commodity price variation and civil war across a broad array of specifications.

The theoretical mechanisms relating economic downturns and the extensive margin of civil war apply equally well to the intensive margin of conflict. For opportunity costs mechanisms, poor economic conditions may make rebellion relatively more attractive for each individual citizen, which increases the number of combatants at risk of dying in combat. For state capacity mechanisms, decreased ability to buy off or suppress rebellion may also increase the number of individuals fighting and therefore the number at risk of dying.

Despite the possible theoretical links between economic conditions and conflict intensity, we are aware of few empirical studies of this relationship. To the best of our knowledge, only Lacina (2006), Bazzi and Blattman (2014), and Esteban, Mayoral, and Ray (2012) study the severity of civil conflicts cross-nationally.[2] Lacina (2006) and Bazzi and Blattman (2014) find limited effects of economic changes on conflict severity, while Esteban, Mayoral, and Ray (2012) explain variation in the intensity of civil conflict using several different indices of the distribution of ethnic types within a country. Both Lacina (2006) and Bazzi and Blattman (2014) select the sample based on cases where conflict is occurring; their goal is to study whether economic fluctuations matter conditional on conflict. We take a different approach because characterizing the dynamics of conflict requires use of the years without conflict as well.

Economic Conditions and Conflict Persistence

Existing work recognizes that the extensive margin of civil conflicts tends to be persistent over time. For a variety of reasons, countries can become mired in conflict traps, where a civil conflict in year t increases the likelihood of a civil conflict in year $t + 1$ (Collier et al. 2003). In dynamic models of the extensive margin of civil war, Elbadawi and Sambanis (2002) and Ciccone (2011) find that conflict in year t has a large, positive, and significant effect on the probability of war in year $t + 1$.

The persistence of conflict intensity, however, has not received attention. Existing theoretical work suggests that, as with the extensive margin of civil conflict, conflict intensity should also be persistent, with the intensity of conflict in time t positively associated with conflict intensity in time $t + 1$. Both of the theoretical mechanisms linking economic downturns with the extent of civil conflict suggest that conflict intensity should be state dependent, with past shocks affecting the future trajectory of conflict. For a combatant who is comparing the costs and benefits of rebellion versus lawful employment, choosing rebellion entails significant sunk costs. Once associated with rebellious groups, a combatant cannot always easily return to lawful employment, even if improving economic conditions make fighting sufficiently unattractive. Choosing to become a rebel, especially if the incumbent government retains power, may entail significant risk of being labeled a traitor, resulting in future prosecution or execution. Similarly, state capacity is likely to be persistent. The ability of states to provide adequate public goods and suppress rebellions is slow moving. Weak states are likely to stay weak, even when transitory economic improvements make them stronger temporarily.

It is also possible that conflict intensity is an explosive process, where an increase in intensity during year t results in an even greater increase during year $t + 1$. If adverse economic conditions increased the intensity of conflict in year t, the resulting deaths from combat could create conditions for increased conflict severity in year $t + 1$. In the usual opportunity costs models, if a particularly intense conflict in year t further depressed the expected utility of legal participation in the economy, then this could drive even more individuals into combat. It is possible that a country's susceptibility to this type of feedback loop depends on their overall level of income. A better economy may be more resistant to this type of cascade effect than a poorer one.

Although our article focuses most heavily on economics, several other factors that have been proposed in the literature as correlates of civil war could also affect the time paths of civil conflicts. For example, ethnolinguistic or religious fractionalization have been linked to conflict occurrence (Montalvo and Reynal-Querol 2005). Fractionalization might similarly make conflicts more persistent. Once ethnic or religious tensions boil over to violent conflict, this may make divisions between groups more salient, making negotiated settlement more difficult. The presence of natural resources, such as oil, has also been linked to the occurrence of civil conflict (Ross 2004). The theoretical link between natural resources and conflict dynamics is less clear. The presence of a consistent flow of rents from natural resources might make conflicts more persistent. On the other hand, one group capturing a valuable, resource-rich area might be able to translate that wealth into increased military capacity, which they could use potentially to win and end a particular conflict. Finally, terrain has also been linked to the occurrence of conflict, with mountainous terrain favoring insurgency (Fearon and Laitin 2003). This theoretical mechanism could also affect conflict persistence. If terrain affords insurgents the ability to mount persistent guerilla attacks, while limiting the state's ability to conduct counterinsurgency operations, then we would expect mountainous terrain to be associated with persistent, simmering conflicts.

The empirical models that follow shed light on both the static and dynamic relationship between per capita income and the costliness of civil conflict. The opportunity cost theory and the state-capacity theory provide the same qualitative predictions and are tested jointly against a null hypothesis that there is no relationship between economic measures and civil conflict. This null hypothesis has gained prominence in the literature and is rejected when employing data on conflict intensity in the first part of this article.[3]

The second part of this article then tracks the evolution of conflict intensity. We provide overall estimates of the persistence of conflict intensity and then examine whether particular country characteristics—economic conditions, fractionalization, oil exports, and terrain—are associated with increased persistence of conflict intensity.

Data

Dependent Variable: Battle Deaths

The dependent variable in our analysis is BattleDeaths_{it}, which describes the number of battle deaths resulting from civil conflict in country i during year t. The battle deaths data are from the Uppsala Conflict Data Program/International Peace Research Institute Armed Conflict Dataset and accompanying Battle Deaths Dataset, which collects data on civil conflicts defined as "internal armed conflict [occurring] between the government of a state and one or more internal opposition group(s)" (Gleditsch et al. 2002, 9). Battle deaths are "deaths resulting directly from violence inflicted through the use of armed force by a party to an armed conflict during contested combat" (Lacina and Gleditsch 2005, 3). The Armed Conflict Dataset distinguishes between civil conflicts with and without outside intervention from a foreign state. We focus on civil conflicts without outside intervention. The definition of battle deaths excludes deaths not related to combat. The battle deaths data cover civil conflicts in 196 countries from 1960 to 2008.[4]

Table 1 provides summary statistics for each measure of civil conflict for different regional breakdowns: the full sample, a sample restricted to sub-Saharan Africa, the full sample excluding Western Democracies and Japan, and the full sample excluding sub-Saharan Africa. In all breakdowns, conflict intensity varies greatly. Standard deviations of battle deaths are approximately six to eight times the means, emphasizing the variation in conflict intensity.

Battle deaths data are difficult to collect and are susceptible to measurement error. Measurement error in the dependent variable does not affect the consistency of the parameter estimates. However, measurement error also occurs on the right-hand side of the estimating equations through the lagged dependent variable. In the classical errors in variables problem, if the right-hand side x variable is measured with error (in this case, the lagged dependent variable), it is possible to use an instrumental variable, z, to consistently estimate the parameter of interest so long as any measurement error in z is independent of the measurement error in x. Panel instruments based on lags of the data may not solve the consistency problem because the measurement error may be autocorrelated. For example, if data are interpolated, the interpolation procedure will introduce correlated measurement error.

We use a number of approaches, including the use of instruments that are more or less susceptible to serially correlated measurement error, to assess the sensitivity of results. One approach is to use relatively coarse functions of the lagged data as instruments for the lagged dependent variable. These coarse functions do not capture as much information as the original lagged dependent variable, but they are less likely to be measured with error that is correlated with the measurement error in the lagged dependent variable. While the intensity of fighting in any given year may be measured with error, the start dates and end dates of conflict are subject to less measurement error than data on the timing of battle deaths. Because of this, we construct an instrument defined as lags of a conflict indicator times conflict duration, $\mathbb{I}(Battle\ Deaths_{it} \geq 25) \times (t - last\ Year\ Of\ Peace_{it})$. Error in this measure, if there is any, is likely to have very little correlation with measurement error in $Y_{i,t-1}$.[5] While our analysis uses the "best" estimate in the Armed Conflict Dataset, we also reestimate the main models using the "high" and "low"

Table 1. Summary Statistics.

	Observations	Mean	SD	*Min*	*Max*
Panel A: Full sample					
Battle deaths	8,142	335	2,473	0	115,000
Binary war indicator (>25 deaths)	8,142	0.119	0.324	0	1
Log income per capita (1967 dollars)	8,142	6.171	1.46	2.701	9.946
Panel B: Sub-Saharan Africa					
Battle deaths	1,184	528	3418	0	115,000
Binary war indicator (>25 deaths)	1,184	0.158	0.365	0	1
Log income per capita (1967 dollars)	1,184	4.808	0.85	2.701	7.851
Panel C: Full sample excluding Western democracies					
Battle deaths	7,182	379	2630	0	115,000
Binary war indicator (>25 deaths)	7,182	0.131	0.338	0	1
Log income per capita (1967 dollars)	7,182	5.909	1.335	2.701	9.946
Panel D: Full sample excluding sub-Saharan Africa					
Battle deaths	6,258	277	2104	0	100,500
Binary war indicator (>25 deaths)	6,258	0.108	0.31	0	1
Log income per capita (1967 dollars)	6,258	6.582	1.352	2.803	9.946
Panel E: Top half of ethnic fractionalization					
Battle deaths	4,737	422	2509	0	115,000
Binary war indicator (>25 deaths)	4,737	0.136	0.343	0	1
Log income per capita (1967 dollars)	4,737	5.826	1.392	2.706	9.946
Panel F: Top half of religious fractionalization					
Battle deaths	4,499	329	2996	0	115,000
Binary war indicator (>25 deaths)	4,499	0.081	0.273	0	1
Log income per capita (1967 dollars)	4,499	6.02	1.44	2.701	9.946
Panel G: Top half of mountainous countries					
Battle deaths	4,318	457	2753	0	115,000
Binary war indicator (>25 deaths)	4,318	0.138	0.344	0	1
Log income per-capita (1967 dollars)	4,318	6.066	1.366	2.701	9.946
Panel H: Oil producing countries					
Battle deaths	1,371	609	4698	0	115,000
Binary war indicator (>25 deaths)	1,371	0.163	0.369	0	1
Log income per capita (1967 dollars)	1,371	6.275	1.139	3.688	9.477

Note: Summary statistics for the estimation samples presented in later tables. See the text for variable definitions. DV = Dependent variable; AB = Arellano-Bond; AR = Autoregressive.

estimates, as well as a categorical dependent variable indicating 0–24, 25–999, and $\geq$1,000 deaths. The results are similar to those here and are discussed in greater detail in the appendix.

Excluded Instruments

Endogeneity concerns are well established in the literature linking economic factors with civil war. Because civil wars and more intense conflicts are likely to be associated with decreased income, we use an instrumental variables approach to identify the effect of per capita income. An instrumental variable (a) affects the explanatory variable of interest, income and (b) does not have a direct effect on the dependent variable, conflict intensity.[6] The first statement describes the strength of the instrument, something we test directly subsequently. The second statement, often called the exclusion restriction, is an untestable assumption. Subsequently, we describe steps taken to make this assumption more plausible.

The instrument used here is similar to that of Acemoglu et al. (2008) in their study of income and democratization. The instrument measures export-weighted variation in the gross domestic product (GDP) of a country's trading partners. The instrument theoretically affects income because business cycles are transmitted from one country to another via international trade. As one country's economic fortunes rise or fall, this can affect the economies of its trading partners (Acemoglu et al. 2008, 824). Our construction of the instrument leverages the fact that some economies affect each other more than others. A country is most affected by economic windfalls or recessions in partner countries that receive a higher share of their exports.

The first step is to construct a set of time-invariant weights, w_{ij}, that measure the degree of connectivity between country i and country j through exports from i to j, as a percentage of country i's GDP. To ameliorate the possibility that conflict in one country may have a direct effect on the economy of geographically proximate trading partners, the instrument construction sets geographically connected countries' weights to zero. That is, to help alleviate geospatial spillovers that may violate the exclusion restriction, when constructing the weights for country i, all countries that are contiguous with i are excluded.[7] Also, the Acemoglu et al. (2008) instrument uses total trade—imports and exports—to construct their weights. Here, the weights are distinctly based only on exports. This change is made because the effect of an economic fluctuation to an import partner is likely to have a different effect on income than a fluctuation in an export partner.[8]

The weight for dyad ij, w_{ij}, is constructed by:

$$w_{ij} = \frac{\mathbb{I}\left(Non - Contiguous_{ij}\right)}{\Upsilon_{ij}} \sum_{s=1980}^{1989} \frac{X_{ijs}}{\mathrm{GDP}_{is}}, \tag{1}$$

where Υ_{ij} is the number of years for which bilateral trade data are available for dyad i, j between 1980 and 1989.[9] X_{ijs} is the value of exports from country i to country j in year s in 1967 US dollars.[10] GDP_{is} measures the total GDP of country i in year s in 1967 US dollars.[11]

The instrument, Z_{it}, is constructed by:

$$Z_{it} = \sum_{j=1, j \neq i}^{N} w_{ij} \mathbb{I}_{jt} \log(\text{GDP}_{jt}) \left(\frac{\sum\limits_{j=1, j \neq i}^{N} w_{ij}}{\sum\limits_{j=1, j \neq i}^{N} I_{jt} w_{ij}} \right), \tag{2}$$

where $\mathbb{I}_{jt}$ is an indicator for whether data for $\log(\text{GDP}_{jt})$ are available. The final term, in parentheses, corrects for the unbalanced nature of the panel by adjusting the weights to ensure that the sum of the weights is the same for country i across time. In a balanced panel, this term equals one. The total GDP of country j in year t is measured the same as in equation (1).

Explanatory Variable and First Stage Results

The main explanatory variable of interest is logged per capita GDP of country i in year t in 1967 US dollars.[12] Because panel Generalized Method of Moments (GMM) estimators are used later, the relevant first stage regression to assess instrument strength is as follows:

$$\Delta \log(\text{GDP}_{it}/Population_{it}) = \beta \Delta Z_{it} + \delta_t + \mu_{it}, \tag{3}$$

where δ_t is a year fixed effect. Some specifications are estimated with country-specific time trends, making the model $\Delta \log(\text{GDP}_{it}/Population_{it}) = \beta \Delta Z_{it} + \delta_t + \alpha_i + \mu_{it}$ where α_i is a country fixed effect.

Table 2 shows results from the first stage for the income instrument.[13] The model is estimated on four samples: all countries with available data, sub-Saharan African countries, all countries except Western democracies, and all countries except sub-Saharan Africa. Each specification in panel A corresponds to parameter estimates from equation (3). In each subsample, the relationship between the instrument and logged per capita GDP is positive and significant. The instrument is comparably strong in this sample as in the sample used by Acemoglu et al. (2008). In addition, the F-statistic is larger than ten in each of these four samples, meeting the often-used standard for instrument strength. Panel B of Table 2 adds country fixed effects to equation three, which corresponds to country-specific time trends in levels. The instrument retains its strength, although the F-statistic falls slightly below ten in the some of the regional subsamples.

Table 2. First Stage Regressions.

	(1) Full Sample	(2) Sub-Saharan Africa	(3) Excluding Western Democracies	(4) Excluding Sub-Saharan Africa
Panel A: Dependent variable is first differenced per capita income				
Lag of first differenced exports instrument	0.195*** (0.0621)	0.995*** (0.322)	0.206*** (0.0631)	0.144** (0.0605)
Observations	8,055	1,862	7,095	6,193
R^2	.157	.197	.148	.158
F-Statistic	39.70	12.75	33.46	31.13
Panel B: Same as Panel A with country fixed effects (for country-specific time trends in level equation)				
Lag of first differenced exports instrument	0.159** (0.0653)	1.005*** (0.364)	0.161** (0.0660)	0.126** (0.0640)
Observations	8,055	1,862	7,095	6,193
R^2	0.185	0.216	0.175	0.181
F-Statistic	10.69	8.837	9.893	9.614

Note: Robust standard errors reported in parentheses. Table presents first differenced estimates of the first stage regression of log gross domestic product per capita on the export-weighted income of trading partners in nonadjacent countries. Adjacent countries are defined by the Correlates of War dataset. Adjacent countries share a land or river border or are separated by less than 400 miles of water. All models contain year fixed effects. Panel B adds country fixed effects to accommodate country-specific time trends. Numbers of observations differ between this and later tables because of differences between first differenced and orthogonal deviations transformations and use of moment conditions in levels.
*$p < 0.05$. **$p < 0.01$. ***$p < 0.001$.

Economic Fluctuations, Intensity, and Average Dynamics

The three questions we ask are as follows: (1) how do economic fluctuations affect the intensity of civil conflict? (2) How persistent is conflict intensity? and (3) What explains the persistence of conflict intensity? In this section, we focus on the first two questions. We discuss a "restricted" model that recovers the average effect of income variation on the intensity of civil conflict and the average $AR(1)$ parameter governing the persistence of conflicts. We call this the restricted model because the autoregressive coefficient is constrained to be common across all countries. In the following section, we focus on the third question and discuss an "unrestricted" model, where the autoregressive coefficient is allowed to vary based on country characteristics, like income level or degree of fractionalization. For each of the two models (restricted and unrestricted), we discuss the model used, discuss interpretation of the relevant parameters, and then discuss the results.

Restricted Model

The model is based on the dynamic panel data model proposed in Blundell and Bond (1998). The Blundell–Bond estimator accommodates unobserved heterogeneity in a country's intensity of civil conflict, serial correlation in the civil conflict process, and endogenous realizations of income variation. The model in levels is as follows:

$$y_{it} = \alpha_i + \gamma y_{i,t-1} + \beta \log(Income_{it}/Population_{it}) + \delta_t + \varepsilon_{it}, \tag{4}$$

where y_{it} is the dependent variable of interest, γ measures the persistence of the process, β is the effect of a unit change in log per capita income on y_{it}, α_i is a country fixed effect, and δ_t is a year fixed effect.

The Blundell–Bond estimator allows for instruments outside of the system, and the export-weighted income measure is employed as an instrument for $\log(\mathrm{GDP}_{it}/Population_{it})$. The estimator used is a "system" GMM estimator as opposed to a "difference" GMM estimator. We use the system estimator because of the poor performance of the difference estimator when elements of the history of the process in levels $y_{i,t-2}, \ldots, y_{i,1}$ are weak instruments for lagged differences $(y_{i,t-1} - y_{i,t-2})$. This insight about the weakness of instruments was originally developed by Blundell and Bond in part to accommodate the case where the process $\{y_{it}\}$ is close to a unit root; in such settings, lagged levels of the process will have little predictive power for future differences. In this setting, because many adjacent years of the process have zero battle deaths, levels are poor instruments for future differences for the same reason.[14]

In the difference equation, the instruments for $(y_{i,t-1} - y_{i,t-2})$ are adjusted based on the results of autocorrelation tests. We dynamically adjust the instrument matrix; if s is the order of autocorrelation detected at the 10 percent level, then the instruments for $(y_{i,t-1} - y_{i,t-2})$ will consist of $y_{i,t-s-2}$, $y_{i,t-s-3}$, and $y_{i,t-s-4}$ (assuming data availability; otherwise, suitable lags will be used subject to the serial correlation tests). The instruments for $y_{i,t-1}$ in the level equation are the corresponding instruments in lagged differences. The instrument for $\log(Income_{it}/Population_{it})$ is only the contemporaneous trade-weighted measure. The forward orthogonal deviations transformation is used to preserve available observations (Arellano and Bover 1995) and statistical inference is based on panel robust standard errors.[15]

Restricted Model: Parameter Interpretation

The two parameters of interest in the restricted model are β and γ. In the restricted model, γ is the autoregressive coefficient that describes the degree of persistence in conflict intensity. Its interpretation is familiar to many time-series applications. We are primarily interested in whether it exceeds one, since this would suggest explosive conflict dynamics. We are also interested in the rate at which conflicts return to "normal" levels after spikes or lulls in conflict intensity. We can calculate the half-life of conflict intensity as $\log(0.5)/\log(\gamma)$.

The parameter β in the restricted model, equation (4), is the combined intensive and extensive marginal effect of log income on battle deaths. To understand what this means, some background on the traditional tobit model may help with intuition.

In ordinary least square (OLS), with censored data, the slope parameter (in this case β) is biased toward zero because of the mass of data censored at the origin. If there is a corner solution—that is, zero is the actual choice agents make rather than the result of censoring—then the slope parameter from OLS captures the marginal effect from crossing into the uncensored portion of the data and the slope once moving into the uncensored portion. This is the combined (overall) empirical marginal effect.[16]

Restricted Model: Results

We now present the results on the relationship between civil conflict severity and per capita income. Panel A of Table 3 shows parameter estimates of equation (4). Column 1 contains estimates of the parameters for all countries in the sample. The estimated marginal effect of a unit increase in the logarithm of per capita income is -321 battle deaths per year. In addition to this contemporaneous effect of income on the intensity of civil war, the results strongly show that these battle deaths will propagate into additional deaths in the future. The coefficient on $\text{BattleDeaths}_{i,t-1}, \hat{\gamma}$, is 0.55. Using the coefficient on income and lagged battle deaths, the total decrease in expected number of deaths from a one-unit increase in log income is approximately $\frac{\hat{\beta}}{1-\hat{\gamma}} = \frac{-321}{1-0.55} \approx -720$.[17] The next specifications in Panel A provide results for the regional subsamples. In all specifications, log income is negatively and significantly associated with battle deaths.

The lagged battle deaths variable is also positive and statistically significant across the specifications. The degree of persistence exhibits some heterogeneity across the specifications, ranging from 0.71 in the sub-Saharan Africa sample to 0.42 in the sample that excludes sub-Saharan Africa. The point estimates for the reduction in the expected number of long-run battle deaths range from 676 to 998 across the samples.[18] The specification also allows us to estimate the expected half-life of conflict deaths. The expected half-life of battle deaths is 1.2 years for the entire sample and is largest, 2 years, when we restrict the analyses to sub-Saharan nations.

Panel B of Table 3 repeats the analysis in panel A with the alternative instruments for lagged battle deaths, $\mathbb{I}(war_{it}) \times (t - \textit{last Year Of Peace}_{it})$. The possibility of correlated measurement error in battle deaths (one potential ramification of interpolation in the battle deaths data) motivates the need to check the sensitivity to alternative instruments for lagged battle deaths.[19] The use of the interaction of lagged binary war indicators and conflict duration as instruments instead of lagged battle deaths in panel B alleviates some potential concern. As in panel A, variation in the trading partners' GDP is also included as an instrument. The results largely corroborate the findings in Panel A. In all samples, the coefficient estimate on log per capita GDP is statistically significant, ranging from -225.7 to -127.6. The magnitude of the autoregressive parameter is even greater than in panel A. The estimates of the long-run decrease in expected number of battle deaths from a one-unit increase in

Table 3. Estimates from Blundell–Bond Dynamic Panel Data Models of the Battle Deaths Process.

	(1) Full Sample	(2) Sub-Saharan Africa	(3) Excluding Western Democracies	(4) Excluding Sub-Saharan Africa
Panel A: Excluded instruments are shocks to export partners and lags of battle deaths				
β: Parameter estimate on log income/capita	−321.3*** (118.2)	−289.4** (124.8)	−295.4*** (112.6)	−482.6*** (133.6)
γ: Parameter estimate on battle deaths $t-1$	0.554*** (0.114)	0.710*** (0.0734)	0.563*** (0.115)	0.422*** (0.104)
$\beta/(1-\gamma)$	−720	−998	−676	−835
Half-life	1.2	2	1.2	0.8
Observations	8,142	1,884	7,182	6,258
Number of countries	203	43	183	160
Overidentifying restrictions p value	0.215	>0.99	0.599	0.938
AB test of AR 1 p value	0.0373	0.209	0.0368	0.0959
AB test of AR 2 p value	0.667	0.428	0.679	0.619
Panel B: Excluded instruments are shocks to trading partners and lags of war indicators times conflict duration				
β: Parameter estimate on log income/capita	−129.5** (50.41)	−182.6* (95.34)	−127.6*** (49.02)	−225.7*** (71.99)
γ: Parameter estimate on battle deaths $t-1$	0.759*** (0.0737)	0.828*** (0.0412)	0.763*** (0.0706)	0.584*** (0.101)
$\beta/(1-\gamma)$	−537	−1,062	−538	−543
Half-life	2.5	3.7	2.6	1.3
Overidentifying restrictions p value	0.269	>0.99	0.587	0.949
AB test of AR 1 p value	0.0340	0.212	0.0344	0.0997
AB test of AR 2 p value	0.894	0.449	0.897	0.877

Panel C: Panel A including country-specific time trends

β: Parameter estimate on log income/capita	−887.4* (455.8)	−1,074*** (413.7)	−796.7* (479.8)	−797.0 (518.6)
γ: Parameter estimate on battle deaths $t-1$	0.459*** (0.0993)	0.628*** (0.0696)	0.468*** (0.101)	0.396*** (0.0989)
β/(1 − γ)	−1,640	−2,887	−1,498	−1,320
Half-life	0.9	1.5	0.9	0.7
Overidentifying Restrictions p value	<0.01	>0.99	<0.01	<0.01
AB test of AR 1 p value	0.0457	0.209	0.0438	0.102
AB test of AR 2 p value	0.500	0.424	0.519	0.562

Panel D: Panel B including country-specific time trends

β: Parameter estimate on log income/capita	−402.3** (188.4)	−885.4** (370.6)	−377.9* (205.4)	−131.5 (90.43)
γ: Parameter estimate on battle deaths $t-1$	0.704*** (0.0768)	0.915*** (0.0839)	0.712*** (0.0777)	0.568*** (0.102)
β/(1 − γ)	−1,359	−10,416	−1,312	−218
Half-life	2	7.8	2	1.2
Overidentifying restrictions p value	>0.99	>0.99	>0.99	>0.99
AB test of AR 1 p value	0.0343	0.227	0.0334	0.0962
AB test of AR 2 p value	0.848	0.465	0.857	0.846

Note: Robust standard errors in parentheses. Table reports Blundell–Bond estimates of the battle deaths model as described in the text. All models include the export-weighted log per capita gross domestic product of trading partners as instruments. Panel-style instruments are described in the panel headings. A maximum of three lags of the panel-style instruments is used, and the beginning and ending lags are dynamically adjusted based on the results of AB tests of autocorrelation as described in the text. The p value of Hansen's test of overidentifying restrictions is also reported. Observation counts are the same in all panels. The AR language refers to: Autoregressive 1, Autoregressive 2 and Autoregressive 3. DV = Dependent variable; AB = Arellano-Bond; AR = Autoregressive.

*$p < 0.05$. **$p < 0.01$. ***$p < 0.001$.

log per capita GDP range from -538 to $-1{,}062$. The estimated half-life of battle deaths are slightly higher in these specifications than in the results in panel A. The estimated half-life for the entire sample of nations is 2.5 years and once again the largest estimate is found in the sub-Saharan African sample.

The specifications reported in panels A and B include year fixed effects. Panels C and D add country-specific time trends to allow the conflict process to evolve idiosyncratically across countries. Again, battle deaths decrease in response to increases in log per capita GDP, across all specifications. These results are statistically significant in all samples with the exception of the sample that excludes Western democracies. The magnitude of the long-run decrease in battle deaths from a unit increase in log per capita GDP is $-1{,}640$ in the specification with lagged battle deaths as instruments and $-1{,}359$ in the specification with the lagged interaction of the binary war indicator and conflict duration. These magnitudes are even larger than the results in panels A and B. Unlike in panels A and B, where we fail to reject the validity of the instruments in all specifications, an overidentification test rejects the lags of battle deaths used as instruments in some of the specifications employed in panel C. None of the models using the interaction of lagged war indicators and duration as instruments (Panel D) are rejected. The estimated half-lives are generally similar to the results in panels A and B. The estimates range across sample regions from 0.7 to 1.5 years in the panel C specifications and 1.2 to 7 years in the panel D specifications. Again, the sub-Saharan Africa sample has the highest estimated half-life.

Model fit and average dynamics. The prior results suggest that conflicts do not exhibit explosive dynamics, on average. The extensive margin of conflict appears substantially more persistent than the severity of conflict. The autocorrelation coefficient governing the extensive margin of civil conflict is much larger than the autocorrelation coefficient governing the severity of conflicts, suggesting that conflicts do not escalate in intensity solely because of past fighting, but conflicts are likely to smolder after they have started.[20]

Data visualization confirms that the autoregressive parameter estimates in the previous section fit the data well. Figure 2 plots log battle deaths at time t against log battle deaths in $t-1$ in the restricted sample that *only includes* conflict years. Using a locally weighted regression, the figure displays a semiparametric model governing the relationship between log battle deaths and lagged log battle deaths. A similar model is then fit using OLS. The locally weighted model and OLS both fit the data well, and inspection suggests that the linear fit does not differ significantly from the locally weighted fit. The estimated slope of the linear fit is around 0.8, but it is important to note that this estimate is not comparable to $\hat{\gamma}$ from the dynamic panel data models because observations with zero battle deaths are not included here.

This provides compelling evidence that an estimate of $\gamma < 1$ is reasonable. During spells of conflict with at least twenty-five battle deaths, the probability of escalation

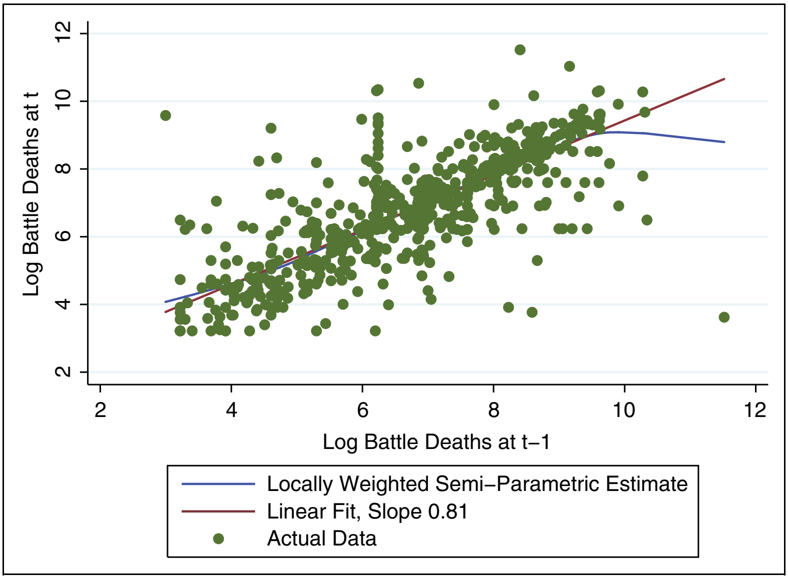

Figure 2. Log battle deaths in year t versus log deaths in year $t-1$. Scatterplot shows log battle deaths in year $t-1$ on the horizontal axis versus log deaths in year t on the vertical axis for consecutive years with strictly positive battle deaths. The red line is the predicted values from a regression of log deaths in year t on log deaths in $t-1$. The green line is from a locally weighted semiparametric model.

to a higher number of battle deaths in the next year is 0.327. Again, this raw statistic suggests that conflict severity isn't explosive in expectation.

Heterogeneous Dynamics

We now turn to the question of whether economic factors and other explanations for civil war also affect the persistence of conflict. In this section, we use an unrestricted model in which the persistence of conflict can vary by a country's income level. We also examine whether persistence varies by other factors such as a country's degree of fractionalization, amount of mountainous terrain, or oil wealth.

Unrestricted Model

We estimate the following model, in which dynamics can vary by income level:

$$y_{it} = \alpha_i + \gamma_1 y_{i,t-1} + \gamma_2 y_{i,t-1} \times \log\left(Income_{i,t-1}/Population_{i,t-1}\right) + \\ \beta \log(Income_{it-1}/Population_{it-1}) + \delta_t + \varepsilon_{it}x.$$

In estimating the unrestricted model, the coarsened instrument interacted with the income instrument, $Z_{it} \times \mathbb{I}(Battle\ Deaths_{it} \geq 25) \times (t - last\ Year\ Of\ Peace_{it})$, is included. The model uses lagged income rather than concurrent income to ease interpretation of the interaction of lagged income and lagged battle deaths.

Unrestricted Model: Parameter Interpretation

The parameters of interest in the unrestricted model are γ_1, γ_2, and β. The interpretation of β is the same as in the previous section. γ_2 and γ_1 are difficult to interpret individually. It is easier to describe the overall intertemporal spillover of fighting across years. If $\gamma_{i,t-1} > 0$, then the overall intertemporal spillover is as follows:

$$\tilde{\gamma}_{i,t-1} = \hat{\gamma}_1 + \hat{\gamma}_2 y_{i,t-1} \times \log\left(Income_{i,t-1}/Population_{i,t-1}\right). \tag{6}$$

As with the restricted model, we can calculate the half-life of the battle deaths process. In the unrestricted model, this quantity is calculated as $\log(0.5)/\log\left(\tilde{\gamma}_{i,t-1}\right)$.

Unrestricted Model: Results

Table 4 presents the results from the unrestricted model. For a comparison with previous estimates, estimates of the model with γ_2 constrained to zero are presented in columns 1 and 3.[21] Estimates of summary measures of the distribution of $\tilde{\gamma}_{i,t-1}$ are presented in the bottom portion of Table 4.

Overall, conflict persistence does appear to be heterogeneous depending on income, as past fighting is most likely to spill over into future fighting for poor countries. In column 4, we cannot reject that the dynamics of conflict at the extensive margin vary based on lagged income. For the poorest country-years at war in the sample, the estimated $\tilde{\gamma}_{i,t-1}$ is greater than one. The mean is around 0.74 in our preferred specification (column 4) with a standard deviation of about 0.14. Countries in war years in the top 5 percent of the distribution of $\tilde{\gamma}$ have estimated persistence that is 7.9 times the bottom 5 percent of persistence in column 2 and over 10 times the level of persistence in column 4.

This substantial amount of heterogeneity highlights the very different evolution of civil conflicts in poor versus wealthy countries. Wealth mediates the persistence of conflict over time. In 98 percent of the country-years with positive battle deaths (936 of the 959 observations), $\tilde{\gamma}$ is less than 0.95.

Another possibility is that persistence depends on distinct characteristics that are largely time invariant, such as ethnic or religious fractionalization, mountainous terrain, and oil wealth, which have all been linked to the incidence of civil war. To investigate whether the dynamic evolution of conflict varies across countries with and without these characteristics, the models in Table 3 are estimated on samples restricted to countries that (a) are in the top half in terms of ethnic and religious

Table 4. Estimates Including Heterogeneous Dynamics.

	(1)	(2)	(3)	(4)
Specification	DV: Battle deaths; IVs: Lags of deaths, exports	DV: Battle deaths; IVs: Lags of deaths, exports, exports × Lags of war × Duration	DV: Battle deaths; IVs: Lags of war × Duration, exports	DV: Battle deaths; IVs: Lags of war × Duration, exports, exports × Lags of war × Duration
β: Parameter estimate on lag of log income/capita	−279.1** (112.4)	−144.6** (57.78)	−124.4** (49.75)	−73.20** (36.96)
γ_1: Parameter estimate on battle deaths $t - 1$	0.557*** (0.115)	1.345** (0.557)	0.762*** (0.0714)	1.348*** (0.212)
γ_2: Parameter estimate on lag of log income/Capita × Battle deaths $t - 1$		−0.156 (0.116)		−0.114** (0.0466)
Observations	8,062	8,062	8,062	8,062
Number of countries	203	203	203	203
Overidentifying restrictions *p* value	.220	.195	.269	.301
AB test of AR 1 *p* value	.0373	.0541	.0345	.0412
AB test of AR 2 *p* value	.690	.465	.943	.876

Summary measures of persistence for years with $\text{deaths}_{t-1} > 0$
Persistence calculated as $\gamma_1 \times \text{Deaths}_{t-1} + \gamma_2 \times \text{Deaths}_{t-1} \times \text{Log income}_{t-1}/\text{Deaths}_{t-1}$

	(1)	(2)	(3)	(4)
Mean		0.512	0.741	
SD		0.189	0.138	
5th Percentile		0.131	0.463	
10th Percentile		0.244	0.545	
50th Percentile		0.531	0.755	

(continued)

Table 4. (continued)

Specification	(1) DV: Battle deaths; IVs: Lags of deaths, exports	(2) DV: Battle deaths; IVs: Lags of deaths, exports, exports × Lags of war × Duration	(3) DV: Battle deaths; IVs: Lags of war × Duration, exports	(4) DV: Battle deaths; IVs: Lags of war × Duration, exports, exports × Lags of war × Duration
90th Percentile		0.747	0.912	
95th Percentile		0.773	0.931	

Note: Robust standard errors in parentheses. Table reports Blundell–Bond estimates of the battle deaths model with heterogeneous persistence as described in the text. All models include the export-weighted log per capita gross domestic product of trading partners as instruments. Columns 1 and 2 use lags of battle deaths as panel-style instruments and lags of battle deaths interacted with the exports measure as an IV style instrument. Columns 3 and 4 use the war indicator times conflict duration as panel-style instruments and lags of the war indicator times duration interacted with the exports measure as an IV style instrument. A maximum of three lags of the panel-style instruments is used, and the beginning and ending lags are dynamically adjusted based on the results of AB tests of autocorrelation as described in the text. The AR language refers to: Autoregressive 1, Autoregressive 2 and Autoregressive 3. DV = Dependent variable; AB = Arellano-Bond; AR = Autoregressive.
*$p < 0.05$. **$p < 0.01$. ***$p < 0.001$.

fractionalization and mountainous terrain and (b) are oil exporters. The results are presented in Table 5.

As mentioned previously, the parameter estimate on log per capita income is negative in all samples and statistically significant in all but the religious fractionalization sample. The effect of per capita income was highest in mountainous countries, where the long-run effect of a unit increase in income is approximately 1,260 fewer battle deaths. This magnitude is greater than the full sample estimate of 720 and is consistent with the Fearon and Laitin finding that mountainous countries may be more likely to experience war. However, mountainous countries do not seem to be more prone to sustained fighting in response to past conflict. The estimated long-run magnitudes are smaller than the full sample for the top quartile of religious fractionalization and oil-exporting countries while it is slightly larger for the top quartile of ethnic fractionalization countries.

The persistence parameter estimate is positive and statistically significant across all samples. In virtually all subsamples, the hypothesis that $\gamma = 1$ in equation (4) is rejected.[22] In general, conflicts are most persistent in ethnically fragmented countries. For the most ethnically fractionalized countries, the persistence of conflict was approximately twice as large as the next highest category. For ethnically fractionalized countries, the half-life of conflict ranged from 1.5 to 7.9, depending on the specification. The half-lives for the other subsamples were generally smaller and estimated to be in

Table 5. Estimates of Average Persistence on Samples Split by Country Characteristics.

	(1) Top Half of Ethnic Fractionalization	(2) Top Half of Religious Fractionalization	(3) Top Half of Mountainous	(4) Oil Producers
Panel A: Excluded instruments are shocks to export partners and lags of battle deaths				
β: Parameter estimate on log income/ Capita	−242.8*** (74.29)	−314.0** (148.4)	−562.9** (224.7)	−228.0** (102.0)
γ: Parameter estimate on battle deaths $t-1$	0.719*** (0.0787)	0.502*** (0.135)	0.553*** (0.147)	0.326*** (0.0907)
$\beta/(1-\gamma)$	−864	−631	−1,259	−338
Half-life	2.1	1	1.2	0.6
Observations	4,737	4,499	4,318	1,371
Number of countries	125	120	115	32
Overidentifying restrictions p value	>0.99	>0.99	>0.99	>0.99
AB test of AR 1 p value	0.162	0.0661	0.0468	0.0936
AB test of AR 2 p value	0.157	0.389	0.781	0.201
Panel B: Excluded instruments are shocks to trading partners and lags of war indicators times conflict duration				
β: Parameter estimate on log income/ Capita	−97.42* (53.94)	−217.4** (107.7)	−155.5* (93.99)	−139.7* (77.51)
γ: Parameter estimate on battle deaths $t-1$	0.871*** (0.0273)	0.592*** (0.120)	0.775*** (0.116)	0.470*** (0.0768)
$\beta/(1-\gamma)$	−755	−533	−691	−264
Half-life	5	1.3	2.7	0.9
Overidentifying restrictions p value	>0.99	>0.99	>0.99	>0.99
AB test of AR 1 p value	0.170	0.0433	0.0692	0.101
AB test of AR 2 p value	0.159	0.533	0.973	0.158

(continued)

Table 5. (continued)

	(1) Top Half of Ethnic Fractionalization	(2) Top Half of Religious Fractionalization	(3) Top Half of Mountainous	(4) Oil Producers
	Panel C: Panel A including country-specific time trends			
β: Parameter estimate on log income/ Capita	−961.1*** (371.4)	−1,030 (817.6)	−1,583*** (582.2)	−370.0 (497.9)
γ: Parameter estimate on battle deaths $t-1$	0.626*** (0.0690)	0.385*** (0.115)	0.448*** (0.151)	0.327** (0.135)
$\beta/(1-\gamma)$	−2,570	−1,675	−2,868	−550
Half-life	1.5	0.7	0.9	0.6
Overidentifying restrictions p value	<0.01	<0.01	<0.01	>0.99
AB test of AR 1 p value	.158	.0817	.0340	.0868
AB test of AR 2 p value	.166	.261	.590	.268
	Panel D: Panel B including country-specific time trends			
β: Parameter estimate on log income/ Capita	−526.5** (232.5)	−659.5 (712.2)	−497.1** (251.8)	−337.3 (242.0)
γ: Parameter estimate on battle deaths $t-1$	0.916*** (0.0741)	0.507*** (0.124)	0.799*** (0.135)	0.454*** (0.130)
$\beta/(1-\gamma)$	−6,268	−1,338	−2,473	−618
Half-life	7.9	1	3.1	0.9
Overidentifying restrictions p value	>0.99	>0.99	>0.99	>0.99
AB test of AR 1 p value	0.177	0.0400	0.0770	0.107
AB test of AR 2 p value	0.157	0.512	0.981	0.237

Note: Robust standard errors in parentheses. For details, see Table 3. The AR language refers to: Autoregressive 1, Autoregressive 2 and Autoregressive 3. DV = Dependent variable; AB = Arellano-Bond; AR = Autoregressive.
*$p < 0.05$. **$p < 0.01$. ***$p < 0.001$.

Table 6. Estimates from Blundell–Bond Dynamic Panel Data Models of the Binary War Process.

	(1) Full Sample	(2) Sub-Saharan Africa	(3) Excluding Western Democracies	(4) Excluding Sub-Saharan Africa
Panel A: DV is war indicator. Excluded instruments are shocks to export partners and lags of war indicator				
β: Parameter estimate on log income/Capita	−0.0122***	−0.0276***	−0.0119***	−0.00960*
	(0.00471)	(0.0102)	(0.00413)	(0.00516)
γ: Parameter estimate on war $t-1$	0.896***	0.868***	0.898***	0.911***
	(0.0215)	(0.0332)	(0.0201)	(0.0194)
β/(1 − γ)	−0.117	−0.209	−0.117	−0.108
Mean battle deaths in sample war years	2,809	3,347	2,889	2,571
Observations	8,142	1,884	7,182	6,258
Number of countries	203	43	183	160
Overidentifying restrictions p value	0.928	>0.99	0.856	1.000
AB test of AR 1 p value	<0.01	<0.01	<0.01	<0.01
AB test of AR 2 p value	0.00165	0.0255	0.00116	0.0225
AB test of AR 3 p value	0.480	0.227	0.461	0.531
Panel B: DV is war indicator from Fearon Laitin (warFL). Excluded instruments are shocks to export partners and lags of warFL				
β: Parameter estimate on log income/Capita	−0.00454	−0.0322***	−0.0120**	−0.00429
	(0.00705)	(0.0111)	(0.00584)	(0.00537)
γ: Parameter estimate on war $t-1$	0.919***	0.875***	0.908***	0.927***
	(0.0150)	(0.0247)	(0.0134)	(0.0139)
β/(1 − γ)	−0.056	−0.258	−0.13	−0.059
Mean battle deaths in sample war years	2,577	2,857	2,676	2,441
Observations	5,055	1,489	4,275	3,566
Number of countries	156	43	136	113
Overidentifying restrictions p value	0.170	>0.99	0.974	0.998
AB test of AR 1 p value	9.33e-0	0.000531	1.31e-0	2.77e-0
AB test of AR 2 p value	0.970	0.300	0.998	0.341

Note: For notes, see Table 3. The AR language refers to: Autoregressive 1, Autoregressive 2 and Autoregressive 3. DV = Dependent variable; AB = Arellano-Bond; AR = Autoregressive.
*p < 0.05. **p < 0.01. ***p < 0.001.

narrower ranges. For religiously fractionalized countries, the half- life estimates ranged from 0.7 to 1.3. For mountainous countries, the estimates ranged from 0.9 to 3.1. Oil exporters had the least persistent conflicts, with half-lives ranging from 0.6 to 0.9.

Without instruments for variables like fractionalization, we cannot make claims that are as strong as our claims about the effects of income. It is beyond the scope of this article to instrument for each of these variables and assess their impact on severity and dynamics in the same way that we assessed the effect of income. The relationships in Table 5, however, are suggestive that these other variables, like fractionalization, are associated with increased conflict persistence. We view these as valuable areas for future research. Table 6 shows estimates that are analogous to Table 3, only using the binary civil war occurrence indicator variable from Fearon and Laitin (2003). The estimates suggest that a one unit change in income leads to a 1.2% decrease in the likelihood of a civil war. Since the average number of battle deaths during civil war years in the Fearon and Laitin data is approximately 2,568, this effect implies a decrease in contemporary battle deaths of 257. This is much smaller than the analogous estimate found using the battle deaths dependent variable, which was a decrease of 321 battle deaths. On the other hand, the estimated autoregressive coefficient, 0.896, is much larger, suggesting that the presence of civil conflict is much more persistent than the intensity of conflict.

Conclusion

Civil wars are more than just discrete events. They are phenomena that vary in intensity, with some conflicts much more severe than others. Using an instrumental variables strategy, we find that economic downturns, which are often associated with the onset or occurrence of civil war, significantly increase conflict intensity.

More importantly, civil conflicts are dynamic phenomena that can escalate or deescalate, potentially in response to past fighting. Conflicts, on average, are persistent but not explosive. Conflicts appear only to be explosive for the poorest countries. The persistence of conflict also varies with income, with poorer countries having a much slower rate of mean reversion. The persistence of conflict also varies according to other country characteristics, with highly ethnically fractionalized countries suffering from the most persistent conflicts.

Our study compliments recent research that has emphasized the dynamics of how conflicts transition between periods of peace and fighting. This study also points toward a potentially fruitful area of future research. Cross-national work on the onset and occurrence of civil war has triggered a rich body of within-country and micro-level work on the mechanisms of conflict. This study points to how similar research might contribute to our understanding of the dynamics of conflict intensity.

Authors' Note

Replication materials are available at https://thedata.harvard.edu/dvn/faces.

Acknowledgments

We thank Luke Condra, Raymond Hicks, Michael Horowitz, Sarah Hummel, Paul Poast, Burcu Savun, Scott Schaefer, Jacob Shapiro, Kathryn Shaw, and John Vasquez for their helpful discussions on this project.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

Notes

1. Previous studies have analyzed the persistence of conflict incidence (Ciccone 2011; Fearon and Laitin 2003; Elbadawi and Sambanis 2002).
2. In studies specific to Colombia, Angrist and Kugler (2008) and Dube and Vargas (2013) find that commodity price fluctuations affect subnational variation in violence.
3. In the appendix, we show how static models of the relationship between economic fluctuations and civil conflict may yield biased estimates in the presence of conflict dynamics.
4. A conflict must have at least twenty-five battle deaths to enter the Armed Conflict Data set. The Battle Deaths Data set records a "low," "high," and "best" estimate for the number of battle deaths. We use the "best" estimate.
5. Results are similar if the data are winsorized, suggesting that outliers due to erroneous data are not driving the estimates. Results also do not depend on whether interpolation is used to replace missing values.
6. Existing literature uses a variety of instruments for income like rainfall, (Miguel et al. 2004, Hidalgo et al. 2010), or commodity prices (Brückner and Ciccone 2010; Bazzi and Blattman 2014). We chose our instrument because it afforded broader geographic coverage than the rainfall instruments and because the relationship between commodity futures prices (which possibly anticipate civil conflict) and spot prices could violate the exclusion restriction.
7. Contiguity is defined by the Correlates of War project. Contiguous countries share a land or river border or are separated by less than 400 miles of water.
8. Results using weights constructed with total trade are similar, but the instrument is not as strong.
9. Trade data are from the International Monetary Fund's Direction of Trade Statistics. We used the years 1980–1989 to maximize coverage, but for countries without trade data for the 1980s, we constructed weights using trade data from the 1970s, and 1990s when data for the 1970s and 1980s were unavailable. $X_{iss} = 0$ by construction.
10. Nominal data are deflated to US 1967 dollars using the International Monetary Fund's (IMF's) World Development Indicators (WDI) inflation data.
11. Gross domestic product data are constructed using the IMF's WDI data and data from Goldstein, Rivers, and Tomz (2007).
12. Data for $Population_{it}$, the population of country i in year t are from the Penn World Tables.

13. For the first stage for the lagged variables used in the Blundell–Bond models, see the appendix.
14. The level panel instrument fails weak instrument tests in the difference GMM equation. Adding the system component helps to alleviate concern about the strength of the panel instruments. Adding the levels equation, of course, relies on additional assumptions about growth rates of the process being stationary. Year fixed effects remove any aggregate failures of the stationarity assumption. Models are additionally estimated with country-specific time trends to remove differential growth rates across countries.
15. We also estimated linear models in levels and first differences, with and without country fixed effects. The results are largely consistent with those presented here. See the appendix for details.
16. We experimented with a semiparametric version of the panel data tobit model, but the estimator requires substantially more nonzero observations than were present.
17. In terms of elasticities, the most intuitive measure is the short-run version elasticity: $\hat{\beta}/\overline{\text{deaths}} \approx -321/335 = -0.96$.
18. These results do not appear to be driven by outliers—estimates are very similar when we limit the sample to conflict year pairs (current and lagged conflict years) with fewer than 50,000 battle deaths or when we winsorize the conflict data.
19. Another possibility is to exclude observations with interpolated values of the dependent variable from the sample. This analysis is in the appendix and the results are qualitatively similar.
20. The estimated half-life of conflict from the estimates using war_{it} as the dependent variable is around six years.
21. Results using $\log\left(\frac{Income_{i,t-1}}{Population_{i,t-1}}\right)$ or $\log\left(\frac{Income_{it}}{Population_{it}}\right)$ appear similar.
22. The only exception is column 1 of panel D.

Supplementary Materials

The online appendices are available at http://journals.sagepub.com/doi/suppl/10.1177/0022002715569773.

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