NBER WORKING PAPER SERIES

BEYOND ZEROES AND ONES:
THE INTENSITY AND DYNAMICS OF CIVIL CONFLICT

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Working Paper 20258
http://www.nber.org/papers/w20258

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2014

We thank Luke Condra, Raymond Hicks, Michael Horowitz, Sarah Hummel, Paul Poast, Burcu Savun, Scott Schaefer, Jacob Shapiro, and Kathryn Shaw. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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June 2014
JEL No. C23,D74,N40

ABSTRACT

There is tremendous variation in conflict intensity both across and within civil conflict spells. Using an instrumental variables approach and a rich set of dynamic, empirical models, we find that the intensity of conflict is negatively related to per-capita income. Economic conditions also affect conflict dynamics, as higher per-capita income reduces the persistence of past conflict intensity.

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Over the last decade, a significant amount of research has sought to explain the *extensive* margin of civil conflict, i.e. the causes of civil war onset and occurrence in a particular country and year. However, much less attention has been paid to variation in the *intensive* margin of civil conflicts, i.e. 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 particular conflict spells, the intensity of fighting can rise and fall sharply at some times and remain steady at others. Approximately 20% of the low intensity conflicts that occurred between 1993 and 2004 escalated to large-scale civil wars (Melander, Möller and Öberg, 2009). Some conflicts are persistent, with fighting simmering at consistent levels over longer periods of time, while other conflicts become more volatile.

This paper puts the dynamics and intensity of civil conflict front and center. Specifically, 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 related 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 questions.

Using cross-national data on the number of battle deaths resulting from combat between governments and rebel groups from 1960 to 2008, we find that the effect of per-capita income on the number of battle deaths in conflict 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. By taking into account variation at the intensive margin of conflict, we find that 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 most important set of results provides estimates of conflict dynamics. Analyzing the severity of civil conflicts 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. We find that conflict intensity is mean-reverting but persistent. In dynamic $AR(1)$ models, we find an average $AR(1)$ coefficient that is between 0.55 and 0.78. The AR(1) coefficient describes the degree to which the level of conflict intensity in period t affects conflict intensity in period $t + 1$. Our estimates are positive and less than one, which indicates that conflict intensity is persistent across time, but does not tend to be explosive. Here, too, we find that modeling the intensive margin of conflict yields a richer understanding of conflict dynamics than analysis that only considers the extensive margin. 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 also affects the "half-life" of conflict, i.e. 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 1 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 9 years for the deaths from a conflict shock to decline to half the level of the shock.

Throughout the analysis, we take seriously 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 the economic performance of a country's export partners as an instrument for per-capita income. This identification strategy is similar to Acemoglu et al. (2008) in their study of the relationship between income and democracy. A valid instrument must satisfy the exclusion restriction, i.e. 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.

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. Our research contributes to a growing body of work that emphasizes the interdependence of conflict decisions over time (Findley, 2013) and the effects of outside influences, like mediation, on limiting conflict escalation (Greig, 2013).

While 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.[1] 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. 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.

The paper proceeds as follows. Section 2 lays out the theoretical link between economic variation, conflict intensity, and conflict dynamics. Section 3 describes the main dependent variables, the instrumental variable, and the first stage relationship between the instrument and variation in national income. Section 4 describes results from models of the effect of economic fluctuations on conflict intensity and average conflict persistence. Section 5 describes results from models in which conflict persistence can vary according to a country's income level or other factors. The final section concludes.

1 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.[2] Most importantly, the extensive margin of civil conflict and the number battle deaths, although correlated with one another, 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. While this is an important source of variation, there is also tremendous variation at the *intensive margin*, i.e. 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. In our sample, 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. In its decades 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. This is an example of variation in conflict dynamics, with conflict intensity rising and falling over time.

[Figure 1 About Here]

Economic Conditions and Conflict Intensity

What factors might affect the intensity and dynamics of civil conflict? Here we establish that 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 prior studies on 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 relative to employment.[3] The second mechanism describes the possibility that poor states may be unable to buy-off or effectively suppress rebellious groups' capacity.[4] 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 papers, Miguel, Satyanath and Sergenti (2004) find that economic growth, instrumented by a country's rainfall, has a negative effect on the probability of civil war in sub-Saharan Africa from 1979-1999. Brückner and Ciccone (2010) also find that decreases in the price of a country's exports increase the probability of civil war in a similar sample. Djankov and Reynal-Querol (2010) find no relationship between poverty and the probability a country is engaged in civil war using a broader sample and different estimators. Bazzi and Blattman (2011) 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. More combatants could directly increase conflict intensity. Some micro-level evidence supports this possibility. Using data on recruitment during the Sierra Leone civil war, Humphreys and Weinstein (2006) find that individual-level poverty is associated with an increased probability of joining both the rebellion and the government counter-rebellion. On the other hand, Berman et al. (2011) find that higher unemployment levels were not associated with higher rates of insurgent attacks against government forces in Afghanistan, Iraq, and the Philippines.

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 (2011) and Esteban, Mayoral and Ray (2012) study the severity of civil conflicts cross-nationally. Lacina (2006) and Bazzi and Blattman (2011) 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 (2011) select the sample based on cases where conflict is occurring; their goal is to study whether economic fluctuations matter conditional on conflict. As will become clear later, we take a different approach because characterizing the dynamics of conflict requires use of the years without conflict as well. Lacina (2006) regresses the number of battle deaths in 114 country-year observations between 1946 and 2002, for which there were over 900 battle deaths, on the log of the country's GDP, lagged one year, and a set of control variables. She finds no effect for logged GDP on the number of battle deaths. Bazzi and Blattman (2011) regress battle deaths from civil conflict on variation in the prices of a country's commodity exports, a count of the number of years of conflict, and an indicator for civil war onset. They find a negative effect for commodity prices; increases in the prices of a country's commodity exports decrease the number of battle deaths experienced by a country-year in some of their specifications. In related studies specific to Colombia, Angrist and Kugler (2008) and Dube and Vargas (2013) study the effects of commodity prices on the intensity of ongoing civil conflicts in particular regions, which vary in their sensitivity to variation of the particular commodity price. Angrist and Kugler (2008) find that increases in coca prices and production increased violence in coca cultivating regions. Dube and Vargas (2013) find that commodity prices have differential impacts on violence, depending on location and the labor intensity of the good in question.

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) find that conflict in year t has a large, positive, and significant effect on the probability of war in year $t+1$.[5]

The persistence of conflict intensity, however, has not received much 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 in conflicts where the incumbent government retains power, may entail the significant risk of being labeled a traitor, resulting in future prosecution or execution. Similarly, the competence or inadequacy of 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 conflict intensity during year t results in an even greater increase in conflict intensity 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 of rebellion, an individual chooses to rebel if the expected utility of legal participation in the economy is lower than that of choosing to join in armed conflict. If 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 snowball or cascade effect than a poorer one.

While our paper focuses most heavily on economics, several other factors which have been proposed in the literature as correlates of civil war could also affect the time-paths of civil conflicts. For example, ethno-linguistic or religious fractionalization or polarization have been linked to the occurrence of civil conflict (Montalvo and Reynal-Querol, 2005). Fractionalization might also make conflicts more persistent, in addition to making conflict more likely. Once ethnic or religious tensions boil over to violent conflict, this may make divisions between groups more salient or more precisely defined, making a 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 to escalate or win (and potentially end) a particular conflict. Finally, the terrain of a country has also been linked to the occurrence of conflict, with mountainous terrain favoring insurgency.[6] 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 counter-insurgency 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 the paper.[7]

The second part of the paper then tracks the evolution of the severity of civil conflict. We provide overall estimates of the persistence of conflict intensity, and then examine whether particular country characteristics -fractionalization, oil exports, and mountainous terrain- are associated with increased persistence of conflict intensity.

2 Data

2.1 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 UCDP/PRIO 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, p. 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, p. 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, e.g. violence solely against civilians or execution of prisoners of war. [8] The battle deaths data cover civil conflicts in 196 countries from 1960-2008.

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 6-8 times the means, emphasizing the variation in conflict intensity.

[Table 1 About Here]

Before proceeding, it is well known that 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 instrumental variables 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, $\mathbb{I}\left(BattleDeaths_{it} \geq 25\right) \times \left(t - lastYearOfPeace_{it}\right)$. Measurement error in this measure, if there is any, is likely to have very little correlation with measurement error in $y_{i,t-1}$.[9]

In addition to the steps taken in this paper, we would emphasize that there are advantages and disadvantages to using the number of battle deaths as compared to using binary data about the onset or occurrence of civil war. The main advantage of the battle deaths data is that it allows us to say something about the intensity and dynamics of civil conflict. These data also avoid the difficult issue of defining what constitutes civil war. The existing literature does not have a consensus on what constitutes a civil war and uses (at least) 11 different datasets. According to Sambanis (2004), the correlation between pairs of datasets concerning civil war onset is often low, sometimes even as low as 0.42, and the average correlation is only 0.68. In some instances, the choice of threshold for civil war classification can double the number of country-years considered to be at war. These definitional discrepancies are non-trivial. Classification error with a binary dependent variable results in inconsistent parameter estimates. In finite samples, the biases that emerge from misclassification can be severe. In a series of Monte Carlo simulations, Hausman, Abrevaya and Scott-Morton (1998) show that even classification error of 2 percent yields parameter estimates that are biased by 15 to 25 percent of the true value. Sambanis (2004) and Hegre and Sambanis (2006) demonstrate that choices regarding the definition of civil wars can indeed change empirical conclusions. They estimate the effect of economic growth on the onset and occurrence of civil war using a set of commonly used datasets and find that the sign of economic growth is positive in approximately half the regressions and negative in the other half. Bazzi and Blattman (2011) find similar inconsistencies in their analysis of the effects of commodity prices. Sambanis (2004) speculates that "one way around these problems is to stop trying to ... analyze civil wars as a distinct phenomenon and, instead, to code levels of violence along a continuum" (p. 819). Analyzing the intensity of civil conflict does exactly this, avoiding definitional problems by focusing on the level of violence in a particular country-year rather than focusing on whether or not to call that country-year a civil war. Our point is not that battle deaths data are a panacea for these problems. Rather, it is important to recognize that there are strengths and weaknesses to the binary and continuous approaches.

2.2 Excluded Instruments

Endogeneity concerns are well established in the literature linking economic factors with civil war. Because civil wars and more intense civil 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 is a variable which (a) affects the explanatory variable of interest income and (b) does not have a direct effect on the dependent variable, conflict intensity.[10] The first statement describes the strength of the instrument, something we test directly below. The second statement, often called the exclusion restriction, is an untestable assumption. Below, we describe steps taken to make this assumption more plausible.

The instrument used here is similar to that described in Acemoglu et al. (2008) of income and democratization. The instrument measures export-weighted variation in the 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, p. 824). Our construction of the instrument leverages the fact that some economies affect each other more than others. A country is most affected by conomic windfalls or recessions in countries which 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. It is possible that civil conflict in one country could have a direct effect on the economy of geographically proximate trading partners, because fighting or refugees may spill across borders. This would violate the exclusion restriction. To ameliorate this possibility, the instrument construction sets geographically connected countries' weights to 0. That is, to help alleviate geo-spatial spillovers that may violate the exclusion restriction, when constructing the weights for country i, all countries that are contiguous with i are excluded.[11] 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.[12]

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

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

where Υ_{ij} is the number of years for which bilateral trade data are available for dyad i, j between 1980 and 1989.[13] X_{ijs} is the value of exports from country i to country j in year s in 1967 U.S. dollars.[14] GDP_{is} measures the total GDP of country i in year s in 1967 U.S. dollars.[15]

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

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

where $\mathbb{I}_{jt}$ is an indicator for whether data for $log(GDP_{jt})$ 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.

2.3 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 U.S. dollars.[16] Because panel GMM estimators are used later, the relevant first stage regression to assess instrument strength is:

$$\Delta \log \left(GDP_{it} / Population_{it} \right) = \beta \Delta Z_{it} + \delta_t + u_{it} \tag{3}$$

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

Table 2 shows results from the first stage. 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 sub-sample, 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).[17] In addition, the F-statistic is larger than

10 in each of these four samples, meeting the often-used standard for instrument strength. Panel B of Table 2 adds country fixed effects to 3, which corresponds to country-specific time trends in levels. The instrument retains its strength, although the F-statistic falls slightly below 10 in the some of the regional sub-samples.

[Table 2 About Here]

3 Economic Fluctuations, Intensity, and Average Dynamics

The three questions asked in this paper are: (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, then discuss interpretation of the relevant parameters, and then discuss the results.

3.1 Restricted Model

The model is based on the dynamic panel data model proposed in Blundell and Bond (1998). The Blundell-Bond estimator can accommodate 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:

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

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 $\log \left(GDP_{it} / Population_{it} \right)$. 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 $y_{i,t-2}, ..., y_{i,1}$ 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.[18]

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 $\log \left(Income_{it} / Population_{it} \right)$ 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.

3.2 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 1, 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 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. If, on the other hand, the mass is due to censoring, the OLS parameter estimate does not have this interpretation–and the parameter β is neither the extensive, intensive, or overall marginal effect.

While there is a point-mass of battle deaths at 0, we do not correct for this in the sense of a tobit model, as 0 is the theoretical minimum number of possible battle deaths in a year. Therefore, β, the marginal effect of income variation is the combined marginal effects on the intensive and extensive margin. The marginal effect on the extensive margin can be recovered easily using the extensive margin indicator as the dependent variable. However, the marginal effect on the intensive margin is much more difficult to recover because it requires an explicit hurdle crossing model (like tobit).[19]

3.3 Restricted Model: Results

Having described the empirical strategy, we now turn to the presentation of the results on the relationship between civil conflict severity and per-capita income. Panel A of Table 3 shows parameter estimates of Equation 4 using the number of battle deaths from civil conflict as the dependent variable and suitable lags of battle deaths as instruments for battle deaths$_{i,t-1}$. 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 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 $\frac{\hat{\beta}}{1-\hat{\gamma}} = \frac{-321}{1-0.55} \approx -720$.[20] 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.[21] 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.

[Table 3 About Here]

Panel B of Table 3 repeats the analysis in Panel A with the alternative instruments for lagged battle deaths, $\mathbb{I}(war_{it}) \times (t - lastYearOfPeace_{it})$. 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.[22] 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 with these alternative instruments 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 log per capita GDP range from -538 to -1062. 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 -1640 in the specification with lagged battle deaths as instruments and -1359 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.

The qualitative consistency of results across specifications and across sub-samples suggests a negative relationship between civil conflict severity and income per-capita. This is consistent with existing theories of civil conflicts. The differing estimates of $\hat{\gamma}$ across specifications suggests that the choice of instruments matter. It is not surprising that $\hat{\gamma}$ is largest using the set of instruments least prone to serially correlated measurement error.

3.3.1 Model Fit and Average Dynamics

The prior results suggests 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.[23]

Data visualization confirms that the autoregressive parameter estimates in the previous section fit the conflict intensity 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.[24] Using a locally weighted regression, the figure displays a semi-parametric 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 (a return to peace) are not included in the sample. The goal here is not to estimate the γ parameter corresponding to the previous models but to assess whether modeling γ as uniform in response to past fighting is a reasonable assumption.

[Figure 2 About Here]

This provides compelling evidence that an estimate of $\gamma < 1$ is reasonable. During spells of conflict with at least 25 battle deaths, the probability of escalation 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.

4 Heterogenous 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.

4.1 Unrestricted Model

The model used in the section is very similar to the one from the previous section. The main difference is that this model allows potential heterogeneity in conflict dynamics. [25] We call this the unrestricted model, since the autoregressive coefficient is allowed to vary across countries. We estimate the following mode, in which dynamics can vary by income level:

$$\begin{aligned} y_{it} &= \alpha_i + \gamma_1 y_{i,t-1} + \gamma_2 y_{i,t-1} \times \log\left(Income_{i,t-1}/Population_{i,t-1}\right) + \\ &\quad \beta \log\left(Income_{it-1}/Population_{it-1}\right) + \delta_t + \varepsilon_{it}. \end{aligned} \tag{5}$$

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

4.2 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 $y_{i,t-1} > 0$, then the overall intertemporal spillover is:

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

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 $\log\left(0.5\right) / \log\left(\tilde{\gamma}_{i,t-1}\right)$.

4.3 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 0 are presented in Columns 1 and 3.[26] 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. This allows a more nuanced tests of whether conflicts are explosive, in expectation, by allowing dynamics to be heterogeneous. 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 1. The mean is around 0.74 in our preferred specification (Column 4) with a standard deviation of about 0.14.

[Table 4 About Here]

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 959 observations), $\tilde{\gamma}$ is less than 0.95, where $\tilde{\gamma}$ is an estimate of persistence that is allowed to depend on lagged-income.

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

As above, 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 sub-samples, the hypothesis that $\gamma = 1$ in Equation 4 is rejected.[27] 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 sub-samples were generally smaller and estimated to be in 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.

[Table 5 About Here]

5 Conclusion

Civil wars are more than just discrete events. They are phenomena which 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 which can escalate or de-escalate, 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 (Findley, 2013). This study also points towards 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.

Notes

[1]Montalvo and Reynal-Querol (2005); Fearon and Laitin (2003); Ross (2004).

[2]See: Blattman and Miguel (2010); Collier and Hoeffler (2007); Hegre and Sambanis (2006).

[3]Collier and Hoeffler (1998); Besley and Persson (2011); Dal Bó and Dal Bó (2011); Dube and Vargas (2013).

[4]Fearon and Laitin (2003).

[5]Some models of civil war onset in year t include a variable indicating whether there was a distinct civil war in year $t-1$, but these estimates don't describe the persistence of civil war since onset is coded distinctly from occurrence. Typically, the coefficient for the effect of a lagged occurrence variable on onset is negative.

[6]Fearon and Laitin (2003).

[7]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.

[8]We use version 4 of the Armed Conflict Dataset and version 3.0 of the Battle Deaths Dataset- the most recent version of each. Note that a civil conflict must have at least 25 battle deaths to enter the Armed Conflict Dataset. The Battle Deaths Dataset records a "low," "high," and "best" estimate for the number of battle deaths. We use the "best" estimate.

[9]Results are similar if the data are winsorized, suggesting that outliers due to erroneous data are not driving the estimates. When alternative values of the series employing the lowest estimated battle deaths total are used, the estimates of persistence are larger while the effect of income on deaths is smaller. The latter finding is consistent with the fact that the mean number of battle deaths in the low series is less than half the mean of the "best" series. Results also do not depend on whether interpolation is used to replace missing values. The combined results suggest that the estimates are not sensitive to outliers, severely mis-measured dependent variables, or serially correlated measurement error on the right hand side.

[10]Existing literature uses a variety of instruments for income. Miguel, Satyanath and Sergenti (2004) use a function of rainfall and Brückner and Ciccone (2010) use export weighted commodity prices. Hidalgo et al. (2010) also employ rainfall as an instrument for income in their study of Brazilian land invasions and occupations. Bazzi and Blattman

(2011) use commodity prices. We chose our instrument because it afforded broader geographic coverage than the rainfall instruments. We also did not use commodity prices as an instrument because it is plausible that they violate the exclusion restriction. Commodities have futures markets. The prospect of civil conflict may affect the futures price of a commodity, and it is well-established that futures prices can affect spot prices (Fama and French, 1987).

[11]Contiguity is defined by the Correlates of War project. Contiguous countries are those that share a land or river border or are separated by less than 400 miles of water.

[12]Results using weights constructed with total trade are similar, but the instrument is not as strong.

[13]Trade data are from the International Monetary Fund's Direction of Trade Statistics (DOTS). 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. $X_{iis} = 0$ by construction.

[14]Nominal data are deflated to U.S. 1967 dollars using the IMF's World Development Indicators (WDI) inflation data.

[15]GDP data are constructed using the IMF's WDI data and data from Goldstein, Rivers and Tomz (2007).

[16]Data for $Population_{it}$, the population of country i in year t are from the Penn World Tables.

[17]Acemoglu et al. (2008) estimate a coefficient ranging from 0.402 to 0.529, using a lagged instrument, five-year observations in the panel, and some additional covariates.

[18]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.

[19]We experimented with using a semi-parametric version of the panel data tobit model to accomplish this goal (the traditional tobit model is inconsistent with fixed effects), but the estimator requires substantially more uncensored data than were present. Therefore, the best that we can do is recover combined marginal effects and elasticities.

[20]In terms of elasticities, the most intuitive measure is the short-run version elasticity: $\hat{\beta}/\overline{\text{deaths}} \approx -321/335 = -0.96$.

[21]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.

[22]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. This approach is not preferred, however, because the data show that missing year-to-year coverage of conflict intensity is associated with much more severe conflicts. Difficulty in measurement is likely increasing with conflict severity, and discarding observations for which interpolation is necessary potentially biases downward estimates of civil conflict severity.

[23]In fact, the estimated half-life of conflict from the estimates using war_{it} as the dependent variable is around 6 years.

[24]The choice of logs is to aid in presentation by minimizing the appearance of outliers, but the substantive conclusions appear similar if the analysis is conducted in levels.

[25]An alternative interpretation is that this more flexible model captures the intensive margin of the effect of income on civil conflict by conditioning the effect of income on lagged battle deaths.

[26]Results using $\log\left(Income_{i,t-1}/Population_{i,t-1}\right)$ or $\log\left(Income_{it}/Population_{it}\right)$ appear similar.

[27]The only exception is Column 1 of Panel D.

Appendix

Robustness

When deaths data are unavailable for particular years, the Uppsala/PRIO dataset does not report a “best estimate". Interpolation using adjacent years of data is used to fill in missing observations in these cases. Sub-saharan Africa is the region with the most missing data. There are 193 conflict-years that include a “best estimate" in the Uppsala/PRIO dataset for sub-saharan Africa, but there are 121 observations missing when conflicts are occurring in the same country in adjacent years. Interpolation thus provides an additional 121 country-years of data for sub-saharan Africa. For other regions, the discrepancy is much smaller. There are 511 conflict-years outside of sub-saharan Africa with an available “best estimate" in Uppsala/PRIO, and interpolation fills in another 182 conflict-years. Appendix Table 1 shows the results that exclude these observations and only uses observations for which distinct, yearly deaths data were available. Estimates differ, especially in sub-Saharan Africa, for two reasons: first, the number of observations with data on battle deaths falls–reducing statistical power; second, the conflicts that remain are, on average, less severe than the excluded conflicts that require interpolation.

As a further robustness check regarding whether interpolation affects the results on dynamics, Figure A1 re-produces the results from Figure 2 without using the interpolated measures of battle deaths. The results suggest that interpolation does not substantively change the interpretation or estimates of γ.

Finally, as an additional assessment of the importance of measurement error, the models in Tables 3 and 5 were re-estimated using the "low" battle deaths series. The mean number of battle deaths in a country-year using the low series is 121compared to 335 deaths in the series used in Tables 3 and 5. The "low" estimate is populated in all country years; in country years where both the "low" series is populated and the "best" estimate is populated, the low series has a mean of 89 battle deaths and the best estimate has a mean of 173 battle deaths. Given these differences in means, it is not surprising that the marginal effect of income is smaller when using the "low" data. The estimated $AR(1)$ parameter is also larger in these models, suggesting that prior estimates of serial dependence are conservative.

Readers who are interested in comparisons with the extensive margin should exercise caution when combining results with the "low" series and estimates of the extensive margin from the text. Calculations were conducted using the moments of the battle deaths data; because the first and second moments of the "low" series and the "best" series differ, the results are not comparable when using the "low" series.

Bias in Static Models

The estimates from the dynamic panel data models presented in the main paper suggest that the conflict process is dependent. Many prior papers use static models, but the parameter estimates of any parameter of interest from static models are likely to be inconsistent even with an instrument. This is easiest to see using first differences, but the same logic applies to the within-transformed IV estimator because the justification for the most prevalent instruments used in the literature–rainfall shocks and the price of commodity exports–is that the instruments and the error are orthogonal conditional on the unobserved fixed effects. However, these instruments are not likely to be valid without the fixed effects–meaning that the instrument is correlated with the country effects. For example, a country's time-invariant mix of commodity exports or a country's long-run average weather patterns may influence the probability of civil war–but the within-country, time-varying instruments would likely satisfy the exclusion restriction after accounting for the fixed effects if the conflict process were static. If the process is dynamic, the fixed effects cannot be differenced out, so the instrument is correlated with the error, violating the exclusion restriction.

The bias can be signed in the case of the first-differenced IV estimator. Ignoring time fixed effects for exposition, let the true model generating the data be given by $y_{it} = \gamma y_{i,t-1} + x_{it}\beta +$

$\alpha_i + \varepsilon_{it}$, with $E\left(x'_{it}\varepsilon_{it}\right) \neq 0$, $E\left(z'_{it}\varepsilon_{it}\right) = 0$, $E\left(\varepsilon'_{it}\varepsilon_{is}\right) = 0$ for $s \neq t$, and $E\left(x'_{it}z_{it}\right) \neq 0$. Suppose it is erroneously assumed that $\gamma = 0$, and estimation is via first-differenced instrumental variables. The estimated parameter is $\hat{\beta} = \left(\Delta z'_{it}\Delta x_{it}\right)^{-1} \Delta z'_{it}\Delta y_{it}$ and the bias is

$$E\left(\hat{\beta} - \beta\right) = E\left(\left(\Delta z'_{it}\Delta x_{it}\right)^{-1} \Delta z'_{it}\Delta y_{i,t-1}\right)\gamma. \tag{7}$$

To sign the bias analytically, further assume that the time series relationship for the instrument is $z_{it} = \gamma_z z_{i,t-1} + u_{it}$.[28] The bias is

$$E\left(\left(\Delta z'_{it}\Delta x_{it}\right)^{-1} \Delta z'_{it}\Delta y_{i,t-1}\right)\gamma = E\left(\left(\Delta z'_{it}\Delta x_{it}\right)^{-1} \left[\left(\gamma_z - 1\right) z_{it-1} + u_{it}\right]' \Delta y_{i,t-1}\right)\gamma.$$

The first stage implies that $E\ \left(\Delta z'_{it}\Delta x_{it}\right) > 0$ and γ is expected to be positive, so with these restrictions, the term $E\left(\left[\left(\gamma_z - 1\right) z_{it-1} + u_{it}\right]' \Delta y_{i,t-1}\right)$ determines the sign of the bias. After substituting in $z_{it-1} = \gamma_z z_{i,t-2} + u_{it-1}$, the relevant term becomes

$$E\left(\left[\gamma_z z_{it-1} + u_{it} - z_{it-1}\right] y_{it-1} - \left[\underbrace{\gamma_z^2 z_{it-2} + \gamma_z u_{it-1} + u_{it}}_{z_{it}} - \underbrace{\gamma_z z_{it-2} - u_{it-1}}_{-z_{it-1}}\right] y_{it-2}\right).$$

Assuming that $E\left(u_{it}y_{it-s}\right) = 0$ for $s > 0$ and taking expectations, the sign of the bias is determined by

$$E\left([\gamma_z - 1]z_{it-1}y_{it-1} - [\gamma_z - 1]\gamma_z z_{it-2}y_{it-2}\right)$$

Suppose that the reduced form relationship $E\left(y_{it}z_{it}\right) < 0$ is constant for all t. If z_{it} is stationary, then $\gamma_z < 1$, which implies $\left(\gamma_z - 1\right) - \left(\gamma_z - 1\right)\gamma_z < 0$ so that $E\left(y_{it}z_{it}\right)\left[\left(\gamma_z - 1\right) - \left(\gamma_z - 1\right)\gamma_z\right] > 0$. Combined with $\gamma > 0$ and $E\left(\Delta z'_{it}\Delta x_{it}\right) > 0$, parameter estimates from static models are biased upward.

Presumably having an excluded instrument will alleviate some concern about the potential bias from a static model. However, this intuition is only true if the instrument z_{it} is orthogonal to both country fixed effects, α_i, and the error, ε_{it}. Otherwise, the instrument is only valid conditional on the procedure to remove α_i ; these procedures will suffer from Nickell (1981) bias in the case of the within-transformation or the bias derived previously in the case of the first-difference transformation.

To test whether the instrument is orthogonal to α_i, the null hypothesis is that the pooled OLS IV estimator and the within-transformed IV estimator have the same probability limit.[29] It is possible to construct over-identified estimators from moment conditions that impose $E\left(z_{it}\left[\alpha_i + \varepsilon_{it}\right]\right) = 0$ or only $E\left(z_{it}\varepsilon_{it}\right) = 0$. Using 2 sets of moment conditions, the first of which corresponds to pooled OLS IV and the second of which corresponds to within-transformed IV, equality of the estimates is rejected at the 5 percent level using Hansen's J-test. The results of this test confirm that the variation used to estimate the effect of income in static models is valid only conditional on fixed effects. However, if the true data generating process is dynamic, static estimates are biased.

How large is the bias? The empirical estimate of the bias term for the first-differenced IV estimator, $\overline{\left(\Delta z_{it}' \Delta x_{it}\right)^{-1} \Delta z_{it}' \Delta y_{i,t-1}}$, is $2,381$. This suggests that static models may be biased badly, and the bias is likely to be increasing in the degree of persistence.

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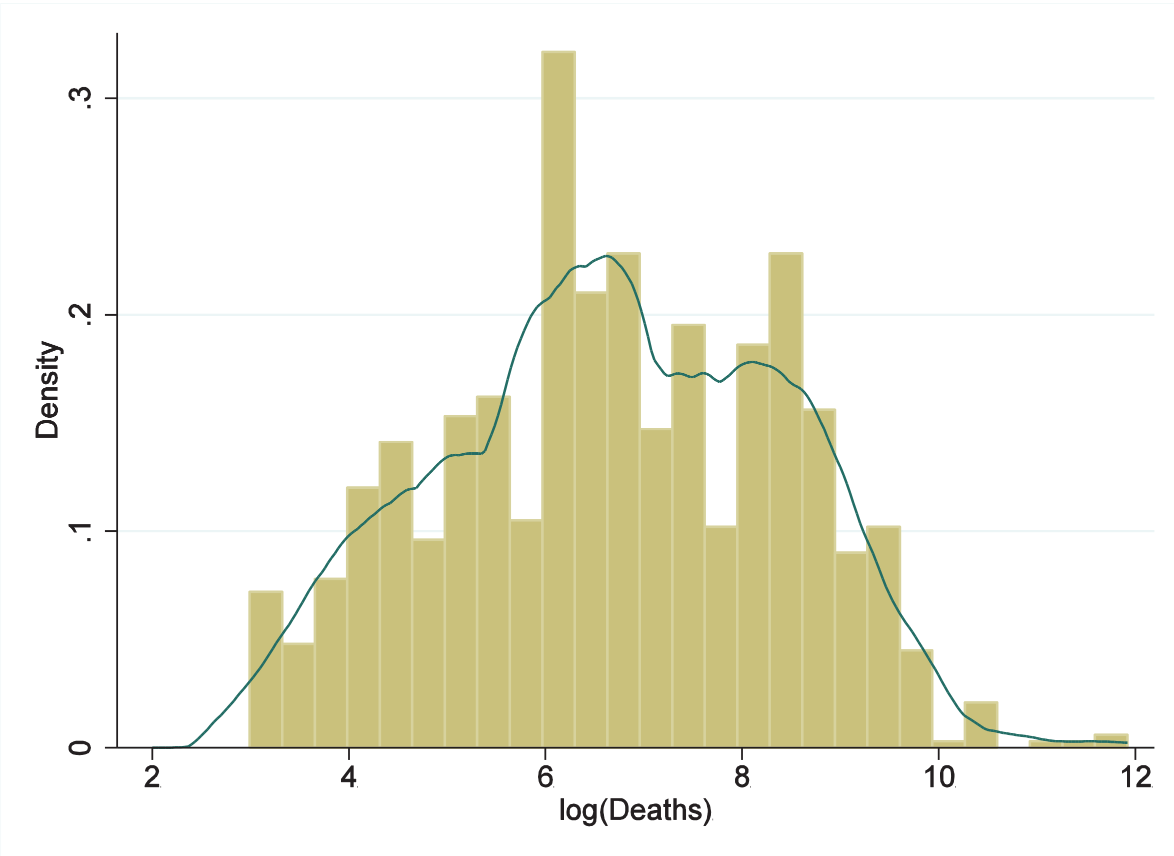
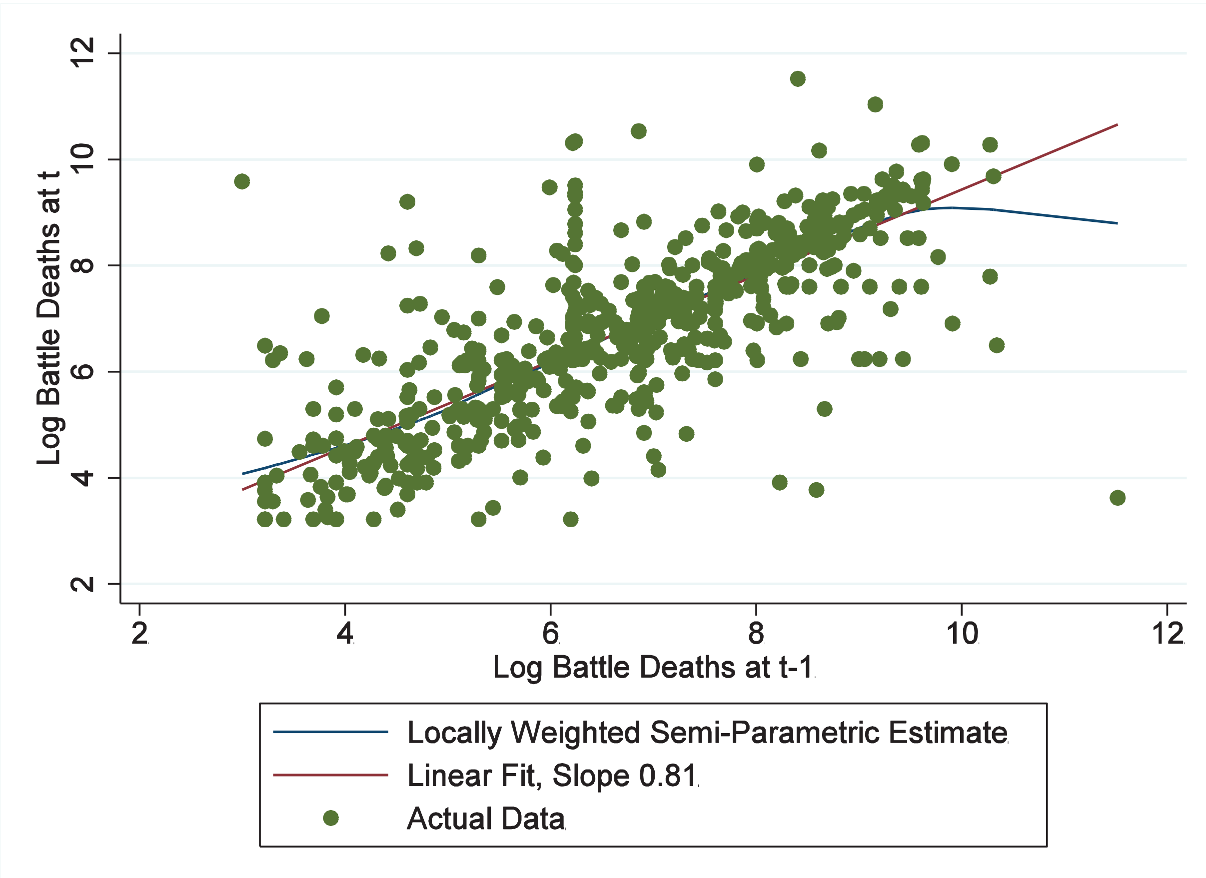
Figure 1: Distribution of Log Battle Deaths in Conflict Years



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 3 because the battle deaths data only contain years with at least 25 deaths.

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 semi-parametric model.

Table 1: Summary Statistics

	Observations	Mean	Std. Dev.	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	115000
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	115000
Binary War Indicator (>25 Deaths)	4,499	0.081	0.273	0	1
Log Income Per-Capita (1967 Dollars)	4,499	6.025	1.44	2.701	9.946
Panel G: Top Half of Mountainous Countries					
Battle Deaths	4,318	457	2753	0	115000
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	115000
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

Notes: Summary statistics for the estimation samples presented in later tables. See the text for variable definitions.

Table 2: First Stage Regressions

	(1)	(2)	(3)	(4)
	Full Sample	Sub-Saharan Africa	Excluding Western Democracies	Excluding Sub-Saharan Africa
Panel A: Dependent Variable is First Differenced Per-Capita Income				
Lag of First Differenced Exports Instrument	0.195***	0.995***	0.206***	0.144**
	(0.0621)	(0.322)	(0.0631)	(0.0605)
Observations	8,055	1,862	7,095	6,193
R-Squared	0.157	0.197	0.148	0.158
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**	1.005***	0.161**	0.126**
	(0.0653)	(0.364)	(0.0660)	(0.0640)
Observations	8,055	1,862	7,095	6,193
R-Squared	0.185	0.216	0.175	0.181
F-Statistic	10.69	8.837	9.893	9.614

Notes: Robust standard errors reported in parentheses. Table presents first differenced estimates of the first stage regression of log gdp per-capita on the export-weighted income of trading partners in non-adjacent 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 accomodate 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.

Table 3: Estimates from Blundell-Bond Dynamic Panel Data Models of the Battle Deaths Process

	(1)	(2)	(3)	(4)
	Full Sample	Sub-Saharan Africa	Excluding Western Democracies	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***	-289.4**	-295.4***	-482.6***
	(118.2)	(124.8)	(112.6)	(133.6)
γ: Parameter Estimate on Battle Deaths t-1	0.554***	0.710***	0.563***	0.422***
	(0.114)	(0.0734)	(0.115)	(0.104)
β / (1-γ)	-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	1	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**	-182.6*	-127.6***	-225.7***
	(50.41)	(95.34)	(49.02)	(71.99)
γ: Parameter Estimate on Battle Deaths t-1	0.759***	0.828***	0.763***	0.584***
	(0.0737)	(0.0412)	(0.0706)	(0.101)
β / (1-γ)	-537	-1062	-538	-543
Half-Life	2.5	3.7	2.6	1.3
Overidentifying Restrictions p-value	0.269	1	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*	-1,074***	-796.7*	-797.0
	(455.8)	(413.7)	(479.8)	(518.6)
γ: Parameter Estimate on Battle Deaths t-1	0.459***	0.628***	0.468***	0.396***
	(0.0993)	(0.0696)	(0.101)	(0.0989)
β / (1-γ)	-1640	-2887	-1498	-1320
Half-Life	0.9	1.5	0.9	0.7
Overidentifying Restrictions p-value	0	1	0	0
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**	-885.4**	-377.9*	-131.5
	(188.4)	(370.6)	(205.4)	(90.43)
γ: Parameter Estimate on Battle Deaths t-1	0.704***	0.915***	0.712***	0.568***
	(0.0768)	(0.0839)	(0.0777)	(0.102)
β / (1-γ)	-1359	-10416	-1312	-218
Half-Life	2	7.8	2	1.2
Overidentifying Restrictions p-value	1	1	1	1
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

Notes: 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 gdp of trading partners as instruments. Panel-style instruments are described in the panel headings. A maximum of 3 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.

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**	-144.6**	-124.4**	-73.20**
	(112.4)	(57.78)	(49.75)	(36.96)
γ_1: Parameter Estimate on Battle Deaths t-1	0.557***	1.345**	0.762***	1.348***
	(0.115)	(0.557)	(0.0714)	(0.212)
γ_2: Parameter Estimate on Lag of Log Income/Capita x		-0.156		-0.114**
Battle Deaths t-1		(0.116)		(0.0466)
Observations	8,062	8,062	8,062	8,062
Number of Countries	203	203	203	203
Overidentifying Restrictions p-value	0.220	0.195	0.269	0.301
AB Test of AR 1 p-value	0.0373	0.0541	0.0345	0.0412
AB Test of AR 2 p-value	0.690	0.465	0.943	0.876
Summary Measures of Persistence For Years With $\text{Deaths}_{t-1} > 0$: Persistence Calculated as $\gamma_1 * \text{Deaths}_{t-1} + \gamma_2 * \text{Deaths}_{t-1} \text{ x Log Income}_{t-1} / \text{Deaths}_{t-1}$				
Mean		0.512		0.741
Std. Dev		0.189		0.138
5th Percentile		0.131		0.463
10th Percentile		0.244		0.545
50th Percentile		0.531		0.755
90th Percentile		0.747		0.912
95th Percentile		0.773		0.931

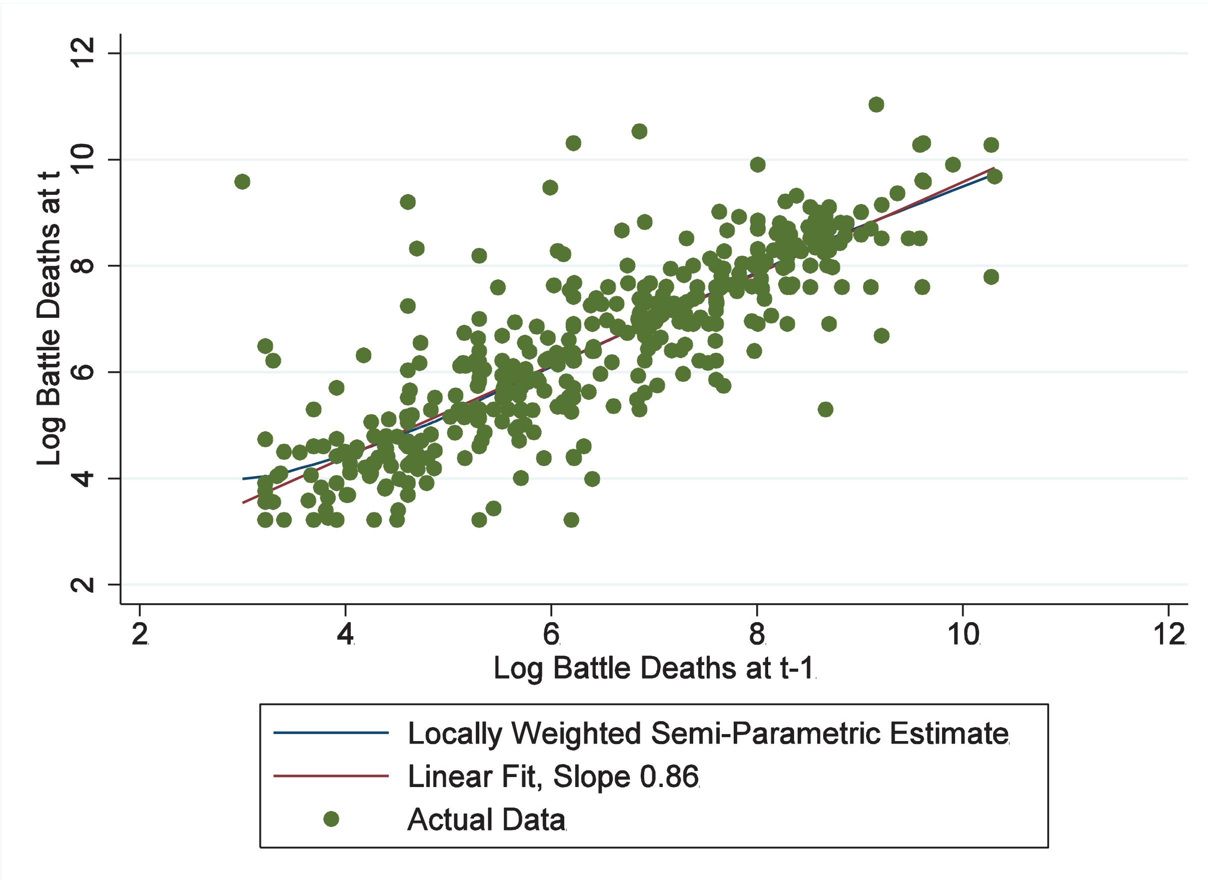
Notes: Robust standard errors in parentheses. Table reports Blundell-Bond estimates of the battle deaths model with heterogeneous persistence as described in the text. All models include the export-weighted log per capita gdp of trading partners as instruments. Columns 1 and 2 use lags of battle deaths 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 3 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.

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

	(1)	(2)	(3)	(4)
	Top Half of Ethnic Fractionalization	Top Half of Religious Fractionalization	Top Half of Mountainous	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***	-314.0**	-562.9**	-228.0**
	(74.29)	(148.4)	(224.7)	(102.0)
γ: Parameter Estimate on Battle Deaths t-1	0.719***	0.502***	0.553***	0.326***
	(0.0787)	(0.135)	(0.147)	(0.0907)
β / (1-γ)	-864	-631	-1259	-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	1.000	1.000	1	1
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*	-217.4**	-155.5*	-139.7*
	(53.94)	(107.7)	(93.99)	(77.51)
γ: Parameter Estimate on Battle Deaths t-1	0.871***	0.592***	0.775***	0.470***
	(0.0273)	(0.120)	(0.116)	(0.0768)
β / (1-γ)	-755	-533	-691	-264
Half-Life	5	1.3	2.7	0.9
Overidentifying Restrictions p-value	1.000	1.000	1.000	1
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
Panel C: Panel A including country-specific time trends				
β: Parameter Estimate on Log Income/Capita	-961.1***	-1,030	-1,583***	-370.0
	(371.4)	(817.6)	(582.2)	(497.9)
γ: Parameter Estimate on Battle Deaths t-1	0.626***	0.385***	0.448***	0.327**
	(0.0690)	(0.115)	(0.151)	(0.135)
β / (1-γ)	-2570	-1675	-2868	-550
Half-Life	1.5	0.7	0.9	0.6
Overidentifying Restrictions p-value	0	0	0	1
AB Test of AR 1 p-value	0.158	0.0817	0.0340	0.0868
AB Test of AR 2 p-value	0.166	0.261	0.590	0.268
Panel D: Panel B including country-specific time trends				
β: Parameter Estimate on Log Income/Capita	-526.5**	-659.5	-497.1**	-337.3
	(232.5)	(712.2)	(251.8)	(242.0)
γ: Parameter Estimate on Battle Deaths t-1	0.916***	0.507***	0.799***	0.454***
	(0.0741)	(0.124)	(0.135)	(0.130)
β / (1-γ)	-6268	-1338	-2473	-618
Half-Life	7.9	1	3.1	0.9
Overidentifying Restrictions p-value	1	1	1	1
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

Notes: Robust standard errors in parentheses. For details, see Table 3.

Figure A1: Non-Interpolated Log Battle Deaths in Year *t* Versus Log Deaths in Year *t-1*



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 3 because the battle deaths data only contain years with at least 25 deaths. The data and estimates exclude all observations based on interpolated battle deaths.

Appendix Table 1: Estimates from Blundell-Bond Models Without Interpolated Battle Deaths

	(1)	(2)	(3)	(4)
	Full Sample	Sub-Saharan Africa	Excluding Western Democracies	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	-221.9***	-58.12	-224.8***	-428.5***
	(75.87)	(93.71)	(75.80)	(102.3)
γ: Parameter Estimate on Battle Deaths t-1	0.406***	0.828***	0.410***	0.247***
	(0.126)	(0.109)	(0.126)	(0.0932)
β / (1-γ)	-374	-338	-381	-569
Observations	7,788	1,744	6,828	6,044
Number of ccode	203	43	183	160
Overidentifying Restrictions p-value	0.226	1	0.614	0.985
AB Test of AR 1 p-value	0.0383	0.0423	0.0380	0.0746
AB Test of AR 2 p-value	0.747	0.179	0.761	0.531
Panel B: Excluded instruments are shocks to trading partners and lags of war indicators times conflict duration				
β: Parameter Estimate on Log Income/Capita	-79.99***	-80.06	-80.44**	-169.5***
	(31.04)	(61.21)	(34.86)	(57.12)
γ: Parameter Estimate on Battle Deaths t-1	0.698***	0.788***	0.701***	0.528***
	(0.117)	(0.105)	(0.114)	(0.154)
β / (1-γ)	-265	-378	-269	-359
Observations	7,788	1,744	6,828	6,044
Number of ccode	203	43	183	160
Overidentifying Restrictions p-value	0.221	1	0.586	0.942
AB Test of AR 1 p-value	0.0504	0.0632	0.0518	0.0487
AB Test of AR 2 p-value	0.720	0.197	0.716	0.592
Panel C: Panel A including country-specific time trends				
β: Parameter Estimate on Log Income/Capita	-559.5**	-388.6	-619.1**	-463.1
	(258.6)	(262.6)	(275.6)	(283.1)
γ: Parameter Estimate on Battle Deaths t-1	0.364***	0.807***	0.365***	0.206**
	(0.123)	(0.0965)	(0.124)	(0.0865)
β / (1-γ)	-880	-2013	-975	-583
Observations	7,788	1,744	6,828	6,044
Number of ccode	203	43	183	160
Overidentifying Restrictions p-value	0	1	0	0
AB Test of AR 1 p-value	0.0421	0.0424	0.0415	0.0845
AB Test of AR 2 p-value	0.624	0.184	0.628	0.416
Panel D: Panel B including country-specific time trends				
β: Parameter Estimate on Log Income/Capita	-319.2**	-598.6*	-328.1**	-285.7*
	(129.0)	(341.4)	(146.6)	(164.1)
γ: Parameter Estimate on Battle Deaths t-1	0.640***	0.753***	0.641***	0.458***
	(0.125)	(0.101)	(0.122)	(0.139)
β / (1-γ)	-887	-2423	-914	-527
Observations	7,788	1,744	6,828	6,044
Number of ccode	203	43	183	160
Overidentifying Restrictions p-value	1	1	1	1
AB Test of AR 1 p-value	0.0484	0.0613	0.0494	0.0573
AB Test of AR 2 p-value	0.771	0.204	0.767	0.742

Notes: See Notes for Table 3. Sample sizes differ because observations with interpolated data on battle deaths are excluded.

Appendix Table 2: Estimates from Blundell-Bond Dynamic Panel Data Models using the "Low" Battle Deaths Series

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample	Sub-Saharan Africa	Excluding Western Democracies	Excluding Sub-Saharan Africa	Top Half of Ethnic Fractionalization	Top Half of Religious Fractionalization	Top Half of Mountainous	Oil Producers
Panel A: Excluded instruments are shocks to export partners and lags of battle deaths								
β: Parameter Estimate on Log Income/Capita	-52.22**	-60.01	-58.48**	-182.3***	-53.35**	-70.58***	-51.74	-20.65
	(23.02)	(42.83)	(25.50)	(47.16)	(21.96)	(27.11)	(32.85)	(14.81)
γ: Parameter Estimate on Battle Deaths t-1	0.758***	0.813***	0.755***	0.330**	0.825***	0.819***	0.852***	0.555***
	(0.0969)	(0.0680)	(0.0941)	(0.161)	(0.0695)	(0.0686)	(0.0360)	(0.0168)
β / (1-γ)	-216	-321	-239	-272	-305	-390	-350	-46
Half-Life	2.5	3.3	2.5	0.6	3.6	3.5	4.3	1.2
Observations	8,142	1,884	7,182	6,258	4,737	4,499	4,318	1,371
Number of Countries	203	43	183	160	125	120	115	32
Overidentifying Restrictions p-value	0.176	1	0.515	0.947	1.000	1.000	1.000	1
AB Test of AR 1 p-value	0.0377	0.142	0.0370	0.0381	0.0546	0.0983	0.0448	0.166
AB Test of AR 2 p-value	0.586	0.217	0.584	0.118	0.253	0.264	0.951	0.179
Panel B: Excluded instruments are shocks to trading partners and lags of war indicators times conflict duration								
β: Parameter Estimate on Log Income/Capita	-34.60**	-83.40*	-39.79**	-115.6***	-54.59	-50.18	-26.37	-19.79
	(14.66)	(47.47)	(15.98)	(41.24)	(38.87)	(32.33)	(26.14)	(13.77)
γ: Parameter Estimate on Battle Deaths t-1	0.861***	0.879***	0.864***	0.482**	0.905***	0.875***	0.916***	0.631***
	(0.0582)	(0.0158)	(0.0538)	(0.208)	(0.0158)	(0.0152)	(0.0371)	(0.0592)
β / (1-γ)	-249	-689	-293	-223	-575	-401	-314	-54
Half-Life	4.6	5.4	4.7	0.9	6.9	5.2	7.9	1.5
Overidentifying Restrictions p-value	0.197	1	0.555	0.987	1.000	1.000	1.000	1
AB Test of AR 1 p-value	0.0360	0.141	0.0367	0.0294	0.0560	0.0985	0.0464	0.170
AB Test of AR 2 p-value	0.572	0.224	0.571	0.0690	0.256	0.279	0.944	0.200
Panel C: Panel A including country-specific time trends								
β: Parameter Estimate on Log Income/Capita	-256.6**	-274.8	-257.3**	-309.5*	-245.7	-357.0	-313.2*	-139.3**
	(100.4)	(214.4)	(107.4)	(161.2)	(163.9)	(218.9)	(173.4)	(68.04)
γ: Parameter Estimate on Battle Deaths t-1	0.634***	0.736***	0.636***	0.305*	0.777***	0.766***	0.828***	0.568***
	(0.110)	(0.0865)	(0.104)	(0.161)	(0.0984)	(0.113)	(0.0716)	(0.0394)
β / (1-γ)	-701	-1041	-707	-445	-1102	-1526	-1821	-322
Half-Life	1.5	2.3	1.5	0.6	2.7	2.6	3.7	1.2
Overidentifying Restrictions p-value	0	1	0	0	0	0	1	1
AB Test of AR 1 p-value	0.0264	0.151	0.0266	0.0368	0.0583	0.106	0.0459	0.197
AB Test of AR 2 p-value	0.598	0.210	0.596	0.109	0.257	0.255	0.976	0.175
Panel D: Panel B including country-specific time trends								
β: Parameter Estimate on Log Income/Capita	-149.4	-598.9**	-169.3	-102.8*	-245.6	-430.1	-249.1*	-63.43**
	(92.77)	(295.1)	(110.6)	(60.19)	(180.9)	(301.3)	(146.9)	(29.09)
γ: Parameter Estimate on Battle Deaths t-1	0.862***	1.010***	0.878***	0.456**	1.011***	1.068***	1.017***	0.661***
	(0.104)	(0.0753)	(0.108)	(0.205)	(0.112)	(0.149)	(0.102)	(0.0725)
β / (1-γ)	-1083	N/A	-1388	-189	N/A	N/A	N/A	-187
Half-Life	4.7	N/A	5.3	0.9	N/A	N/A	N/A	1.7
Overidentifying Restrictions p-value	1	1	1	1	1	1	1	1
AB Test of AR 1 p-value	0.0343	0.227	0.0334	0.0962	0.0614	0.112	0.0503	0.208
AB Test of AR 2 p-value	0.848	0.465	0.857	0.846	0.241	0.254	0.940	0.196

Notes: 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 gdp of trading partners as instruments. Panel-style instruments are described in the panel headings. A maximum of 3 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.