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Beyond Zeroes and Ones: The Severity and Evolution of Civil Conflict*

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Abstract

We assess risk factors affecting the severity and dynamics of civil wars, departing from analyses focused primarily on static models of the effect of income on the extensive margin of conflict. Civil conflicts are shown to be persistent, but rarely do they become more severe in response to past fighting. Substantial heterogeneity in the speed of mean reversion is documented: severe fighting lasts longest in poor countries and ethnically fractionalized countries.

JEL Codes: D74, N40, C23

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1 Introduction

Civil conflicts vary greatly in their intensity. 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. This variation is present both across countries and within conflict spells. Deaths from combat are the most direct consequence of these conflicts, making the tremendous amount of variation in conflict intensity inherently important.

This paper makes two contributions. First, we conduct the most extensive analysis to date of the relationship between per-capita income and the intensity of civil conflict. Using cross-national data on the number of battle deaths resulting from combat between governments and rebel groups from 1960 to 2008, the effect of income on the battle deaths in conflict is both statistically and economically 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 log-income leads to a reduction of 321 battle deaths in the current year and 720 deaths overall after accounting for the impulse response of a perturbation.[1]

The second and most important set of results provides estimates of 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 and then assess heterogeneity in persistence. Several results related to the dynamics of conflict are of interest:

1) Conflict intensity is mean-reverting but persistent, with an average $AR(1)$ coefficient between 0.55 and 0.78. Restricting analysis to conflict-spells in adjacent years, data visualization suggests that an $AR(1)$ model fits the time series of conflicts well.

2) Past fighting does not cause escalation of conflict except in rare cases. While the average behavior of the conflict time series makes clear that conflicts mean revert, is it possible that some conflicts escalate in response to past fighting? In its most crude form, the inference that escalation is uncommon comes from comparing estimates of the persistence of conflict intensity with estimates of the persistence of conflict on the extensive margin. The estimated $AR(1)$ coefficient governing the extensive margin of civil conflict is much larger

[1] When using the extensive margin of civil war as the dependent variable, a one unit increase in log-income reduces the probability of civil conflict by 1.2 percentage points in the current period. Since the average year of civil conflict has $2,809$ deaths, the average effect of a one unit change in log-income on battle deaths is about 34 deaths in the current year and 330 total deaths after accounting for the impulse response of a current period perturbation.

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.

In richer models that allow the persistence of conflict to differ based on a country's income, conflicts do not escalate in response to past fighting except for the poorest countries in the sample. Conflicts are, however, more persistent in poorer countries, and for extraordinarily poor countries it is not possible to reject that the conflict process is explosive. That is, for 1-2 percent of the conflict-years in our sample, when heterogeneity in the autocorrelation-coefficient governing battle deaths is permitted, the $AR(1)$ coefficient implied by the data is greater than 1.

3) A related interpretation is that the rate of mean reversion, and thus the half-life of the battle deaths time series, varies substantially with income. 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 contrst, 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.

4) Among four other factors identified in the prior literature as correlates of civil war, ethnic fractionalization is most associated with prolonged conflict persistence.[2] Oil producing countries have conflicts that die out relatively quickly, primarily because these 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.

Our findings regarding the dynamics of civil conflict have implications for the broader literature estimating the effect of income and other factors on civil conflict. Existing findings for the effect of income on civil war are varied. We isolate one potential reason for these varied results: static models and the IV strategies that they use are biased when the exclusion restriction is a "conditional restriction" and the underlying process is dynamic. That is, when the instrument is only valid conditional on a fixed effect, IV estimates from static models will be biased when the empirical process is state-dependent. We characterize this bias and suggest that it is likely to be large.

Throughout the analysis, we take seriously the endogeneity of economic performance, spillovers across countries, and unobserved heterogeneity. To account for endogeneity, the economic performance of a country's export partners is used as an instrument for per-capita income. This identification strategy is similar to Acemoglu et al. (2008) in their study of

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

the relationship between income and democracy. The approach in this paper modifies the Acemoglu et al. (2008) instrument; by removing adjacent countries when calculating the per-capita income of export partners, the potential for geographic spillovers and spatially-correlated shocks that may violate the exclusion restriction is reduced. The exclusion restriction is plausible, requiring that economic fluctuations in a country's distant export destinations are related to civil conflict only through their effect on income.

One word of caution is necessary about the set of results regarding conflict dynamics. The empirical work in this area relies on data at the country level which may be measured with error. A number of steps are taken to assess the sensitivity of the estimates, all of which yield the conclusion that the dynamics of civil conflict are essential for our understanding of the conflict process. While measurement error in the conflict intensity data is not a problem for the dependent variable, serially correlated measurement error may bias estimates when using dynamic models and panel-style instruments. The results are robust to using instruments for the dynamic panel model that are more or less prone to serially correlated measurement error.

Going forward, this research should be augmented with more data at the micro-level within conflicts. Cross-national analysis of explanations for the onset and occurrence of civil wars led to a rich body of theoretical and empirical work examining the micro-mechanisms behind these findings. These initial findings suggest an agenda for new inquiry on the evolution and dynamics of civil wars.

The rest of the paper is organized as follows: Section 2 briefly surveys existing literature and motivates the study of conflict severity and dynamics. Section 3 defines measurements of conflict severity. Section 4 describes the instrument and empirical strategy. Section 5 reports the baseline results for the effect of income shocks on the intensity of conflict. Section 6 analyzes conflict dynamics in detail. Section 7 discusses the potential bias of static models, compares our estimates with existing literature and concludes.

2 Background

The extensive margin of civil conflict and the number battle deaths, although correlated with one another, are distinct phenomena. Figure 1 plots the distribution of the logged number of battle deaths for country-years with positive battle deaths, showing there is tremendous variation in the number of deaths in particular country-years. In addition, when battle deaths are positive, they tend to be bunched together over time. This suggests either that whatever factors are responsible for civil conflict are long lived or that conflicts are state dependent.

[Figure 1 About Here]

The existing literature focuses on risk factors explaining the outbreak of civil conflict, with the most recent focus on the effect of income on civil conflict.[3] The empirical work linking income shocks 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 shocks to 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 evidence linking commodity price shocks and civil war across a broad array of specifications.[4]

At least two mechanisms motivate the prior studies on the link between income and civil conflict: opportunity costs and state capacity. In opportunity cost models, income shocks increase the likelihood of civil conflict by decreasing the relative cost of rebellion. For an individual choosing between lawful participation in the economy and insurgency, negative economic shocks may increase the attractiveness of fighting relative to employment (Collier

[3] For a recent survey of this literature, see Blattman and Miguel (2010).

[4] 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 shocks 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 explanatory variables measuring theoretically important quantities like the country's regime type, population, religious and ethnic polarization, etc. She finds no effect for logged GDP on the number of battle deaths. Bazzi and Blattman (2011) regress battle deaths from civil conflict on price shocks to 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 price shocks; 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 (Forthcoming) study the effects of commodity price shocks on the intensity of ongoing civil conflicts in particular regions, which vary in their sensitivity to the particular commodity price shock.

and Hoeffler, 1998; Besley and Persson, 2011; Dal Bó and Dal Bó, 2011; Dube and Vargas, Forthcoming). The second mechanism describes the possibility that negative economic shocks decrease the state's ability to buy-off or effectively suppress rebellious groups' capacity (Fearon and Laitin, 2003).[5]

This paper pushes the literature forward by simultaneously analyzing the time-path that civil wars follow as well as risk factors, like income shocks, that explain the outbreak of civil war. Both theoretical mechanisms linking economic measures with the extent of civil conflict suggest that civil conflict 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.

Several other factors have been proposed in the literature as correlates of civil war that 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). They 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

[5] The theoretical mechanisms that motivate empirical analyses of the extensive margin of civil war would seem to apply equally well to the intensive margin. For opportunity costs mechanisms, income shocks may make rebellion relatively more attractive for each individual citizen, which increases the number of combatants at risk of dying in combat. 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. 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.

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 (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 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.

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.

3 Data

The dependent variable in subsequent models 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).[6] The battle deaths data cover civil conflicts in 196 countries from

[6]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. 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

1960-2008.[7]

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.[8] 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, as displayed in Figure 1.

[Table 1 About Here]

Before proceeding, it is well known that battle deaths data are difficult to collect and are susceptible to measurement error. There are at least two reasons why their use is preferable to a coarse function of the underlying data.

First, measurement error in the dependent variable is irrelevant for the consistency of the parameter estimates. As discussed later, both finite sample concerns and measurement error that is correlated with income are unlikely to affect inference. One concern, however, is that the lagged dependent variable in the dynamic model may enter with 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.[9]

Second, analyzing the intensity of civil conflict avoids another well-known measurement

battle deaths. We use the "best" estimate.

[7] For those interested with comparisons to the existing literature, two binary measures of the extensive margin of civil wars are used to estimate models for comparison purposes. The first is a binary indicator based on a 25 death country-year threshold: the variable war_{it} equals one if country i experienced at least 25 battle deaths in year t. The second is an indicator of civil war based on spells of conflict, warFL_{it}, which uses the Fearon and Laitin measurement of the occurrence of civil war. The Fearon and Laitin dataset covers 156 countries from 1960-1999, and codes the beginning and end of conflict spells that accrued at least 1,000 cumulative battle deaths.

[8] Region codings are from Fearon and Laitin (2003). The larger sample sizes for the Uppsala PRIO-based measurements are because of their greater temporal coverage than the Fearon and Laitin dataset. The means for Fearon and Laitin's binary indicator are higher than the binary indicator based on the battle deaths data because Fearon and Laitin code some country years as civil war even if there is a year-long lull in an ongoing war.

[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 compined 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.

issue in the study of civil wars: defining what constitutes a civil war. In the vast majority of cross-national studies of civil war, the dependent variable is binary, taking on a value of one if a particular country-year observation is associated with the onset or occurrence of a civil war. What constitutes civil war depends on a variety of definitional criteria. Most datasets choose one of two approaches. The first approach is to pick a particular threshold for the number of battle deaths that must occur in a particular country-year observation for that observation to be considered a civil war. A related approach treats civil wars as spells of tension with a beginning and end, and in which there may be intermittent years without any battle deaths. Codings of the second type generally choose a total number of battle deaths or a yearly-average number of battle deaths (often 1,000 total deaths or an average of 1,000 yearly deaths) that a conflict spell must exceed in order to be considered a civil war as opposed to lower level civil unrest. Datasets vary in the thresholds chosen and in defining the beginning or end of particular conflict spells. 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.[10]

[10]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 price shocks. In fact, 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 this problems. Rather, it is important to recognize that there are strengths and weaknesses to the binary and continuous approaches.

4 Empirical Strategy

4.1 Excluded Instruments

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.[11] The instrument is similar to that described in (Acemoglu et al., 2008) of income and democratization. The instrument measures export-weighted shocks to the GDP of a country's trading partners, and is designed to capture "the transmission of business cycles from one country to another through trade" (Acemoglu et al., 2008, p. 824).

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. Because it is possible that civil conflict has a direct effect on geographically proximate trade partners' GDP, the instrument construction sets geographically connected countries' weights to 0. That is, to help alleviate spatial spillovers that may violate the exlusion restriction, when constructing the weights for country i, all countries that are contiguous with i are excluded.[12] 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 income shock to an import partner is likely to have a different effect on income than a shock to an export partner.[13]

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.[14] X_{ijs} is the value of exports from country i to country j in year s

[11] Other papers use 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 price shocks.

[12] 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.

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

[14] 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.

in 1967 U.S. dollars.[15] GDP_{is} measures the total GDP of country i in year s in 1967 U.S. dollars.[16]

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

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

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.

The main explanatory variable of interest outside of the battle deaths time series is logged per capita GDP of country i in year t in 1967 U.S. dollars.[17] 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).[18] In addition, the F-statistic is larger than 10 in each of these four samples, the rule-of-thumb for instrument strength. Panel B of Table 2 adds country fixed effects to 3, which

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

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

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

[18] 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.

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]

4.2 Empirical Model I: Restricted Model

We first discuss a baseline model that recovers the average effect of economic shocks on the intensity of civil conflict and the average $AR(1)$ parameter governing the persistence of conflicts. 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 the economic shock. The model in levels is

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

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. We call this the restricted model because the autoregressive coefficient is constrained to be common across all countries.

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.[19]

[19]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.

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.

Before continuing, it is worth addressing the implications of measurement error. In the classical errors in variables problem, if the right hand side x variable is measured with error, it is possible to use an instrumental variables regression using, z, an instrument that also contains measurement error, to consistently estimate the parameter of interest so long as the measurement error in x and z is independent. 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.

To overcome this difficulty, it is possible 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 wtih 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, an alternative instrument defined as lags of a conflict indicator times conflict duration, $\mathbb{I}(BattleDeaths_{it} \geq 25) \times (t - lastYearOfPeace_{it})$, is constructed. Measurement error in this measure, if there is any, is likely to have very little correlation with measurement error in $y_{i,t-1}$.

4.3 Empirical Model II: Unrestricted Model

We also use the estimator to study how conflict evolves over time while allowing the persistence of conflict to vary with a country's income level.[20] The following model is estimated

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

to allow potential heterogeneity in conflict dynamics based on income:

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

We call this the unrestricted model because the autoregressive coefficient is allowed to vary according to income. 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.

4.4 Parameter Interpretation

Some discussion of the interpretation of parameters is necessary before proceeding to results. The parameters of interest are γ_1, γ_2, and β, but it is difficult to interpret γ_2 and γ_1 in the unrestricted model. It is instead easiest to interpret the rate of intertemporal spillover in fighting for each conflict-year, defined as

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

A second summary measure that is useful is the half-life of the battle deaths process, calculated as $\log(0.5) / \log(\tilde{\gamma}_{i,t-1})$ in the unrestricted model, Equation 5, and $\log(0.5) / \log(\gamma)$ in the restricted model (4).

The parameter β in the restricted model, Equation 4, is the combined intensive and extensive marginal effect of log income changes on battle deaths. To understand what this means, some background on the traditional tobit model may help with intuition. In OLS, 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, the marginal effects recovered are 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).[21]

5 Results for Restricted Model

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 $\text{deaths}_{i,t-1}$. 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 $\text{BattleDeaths}_{i,t-1}$, $\hat{\gamma}$, 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 $\frac{\hat{\beta}}{1-\hat{\gamma}} = \frac{-321}{1-0.55} \approx -720$.[22]

The next specifications in Panel A provide results for the regional sub-samples. 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.[23] The specification also allows us to estimate the expected half-life of conflict deaths. The expected half-life of battle deaths

[21]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.

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

[23]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.

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, $\mathbb{I}\left(war_{it}\right) \times \left(t - lastYearOfPeace_{it}\right)$. 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.[24] 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, the shock to 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 are estimated to 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, overidentification test rejects the

[24] 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.

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.

6 Results for Unrestricted Model: Dynamics and the Evolution of Conflicts

Given the importance of dynamics for the parameter estimates, a natural question is: what can be learned from the estimates regarding the evolution of civil conflicts? This section addresses that question using the unrestricted model described above. Several novel results are of interest. First, the evolution of conflict does not appear, in itself, to vary with the past level of fighting. That is, an $AR(1)$ model appears to fit the data well overall.

Second, it appears that conflicts are most persistent in poor countries. When allowing for heterogeneity in conflict persistence, 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. For the very poorest countries-years, however, some estimates of $\tilde{\gamma}$ are greater than 1, suggesting that *if* conflicts do escalate in response to past fighting, this effect is likely to be magnified in the poorest countries.

This section proceeds to analyze the dynamics of conflict as follows. We first consider whether an $AR(1)$ model fits the data well and assess whether conflicts exhibit explosive dynamics, on average. After this evidence about average conflict behavior, richer models that allow for heterogeneous conflict dynamics are presented. We then characterize the bias that could result from static models.

6.1 Model Fit and Average Dynamics

Are conflicts likely to exhibit explosive dynamics? Preliminary evidence from the prior results sugggests the answer is no, 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.[25]

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.[26] 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.

6.2 Heterogeneous Conflict Dynamics

Does conflict persistence appear to differ based on observable characteristics? To answer this question, we first estimate a model allowing heterogeneous conflict dynamics based on

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

[26] 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.

income. Other sources of heterogeneity are then considered by splitting the sample based on geographic, demographic, and economic features of the countries.

Table 4 presents the results. For a comparison with previous estimates, estimates of the model with γ_2 constrained to 0 are presented in Columns 1 and 3.[27] 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.

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.

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

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.[28] 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]

7 Discussion and Conclusion

7.1 Bias in Static Models

The estimates from the dynamic panel data models presented in the preceding section 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

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

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 $z_{it} = \gamma_z z_{i,t-1} + u_{it}$.[29] 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

[29] Dickey-Fuller style tests reject the null that $\gamma_z = 1$ in favor of an alternative that z_{it} is a trend-stationary process.

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.[30] 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.

7.2 Comparison to Estimates in the Literature

Given the voluminous literature examining the effects of changes in macroeconomic performance on the probability that countries are afflicted with civil war, how do the estimates above compare to those implied by previous research using the extensive margin of conflict? Panel A of Table 6 estimates the same models as Table 3 using war$_{it}$, the binary indicator if the country experienced greater than 25 battle deaths. Panel B repeats the analysis with warFL$_{it}$, the Fearon and Laitin binary indicator of civil war, as the dependent variable. For each panel, the columns report specifications estimated on each of the subsamples described previously. We focus on the results in Panel A, as these come from a sample comparable to the estimates in Table 3 of the main text. Comparing the results using battle deaths to the same model using the two binary measures of civil war further highlights the importance of the intensive margin.

The static number of battle deaths associated with a unit change in log-income can be calculated as $E\left(deaths|deaths>0\right) \times \frac{\partial \Pr(deaths>0)}{\partial \log(Income/Capita)} = E\left(deaths|deaths>0\right) \times \beta$; dividing by $(1-\hat{\gamma})$ gives the total number of deaths. The mean number of battle deaths given non-zero deaths is $2,809$ for the full sample. With $\hat{\beta} = -0.0122$ from the full sample in Column 1, the static number of battle deaths reduced by a unit-change in log per-capita gdp

[30]The within-transformed moment conditions are used rather than the first-difference IV moment conditions to preserve the same number of observations across specifications.

is about 34. The long-run total reduction is 330 deaths. This is less than half the estimate when using the battle deaths data. If using the data on battle deaths as a benchmark for the true estimate of the marginal effect of log-income on conflict severity, using only variation at the extensive margin significantly underestimates the effect of income changes on the costliness of civil conflict.

[Table 6 About Here]

A comparison to the reported estimates in several other studies is also possible. In the specification closest to ours, Djankov and Reynal-Querol (2010) find no statistically significant relationship between changes in log per capita GDP and the incidence of civil war. In their fixed effects specifications, the point estimates range from -0.09 to 0.12 and are never statistically significant. Their point estimate of -0.06 implies that a unit increase in GDP decreases expected battle deaths by approximately 169 casualties.[31]

The primary independent variable of interest in Miguel, Satyanath and Sergenti (2004) is economic growth instead of logged GDP per capita. Comparable estimates can be generated by basing the expected battle deaths calculation on a one standard deviation change in economic growth in their sample and comparing it to a calculation from a one standard deviation change in logged GDP per capita in our sample.[32] The coefficient estimates on their contemporaneous growth variables range from -1.13 to -1.48.[33] The decrease in expected battle deaths from a one-standard deviation increase in growth ranges from 225.4 to 295.2 based on the Miguel, Satyanath and Sergenti (2004) specification.[34] Again, these magnitudes are quite a bit smaller than our finding that a standard deviation increase in logged GDP per capita is associated with a decrease of 465 expected battle deaths in the global sample.[35]

In addition to this body of research on the incidence of civil war, there are also two recent

[31] In our sample, the average number of battle deaths in a year with civil war is 2,809. Multiplying this figure by the coefficient estimate on GDP per capita in a particular study generates estimates of the expected battle deaths from a given change in logged GDP per capita.

[32] In their sample, the standard deviation of economic growth is 0.071 and in our sample, the standard deviation of logged GDP per capita is 1.345.

[33] Miguel, Satyanath and Sergenti (2004) also include lagged values of GDP growth as independent variables. The magnitudes of these estimates are generally larger than the contemporaneous values in their sample, but we cannot directly compare these lagged results to our specifications.

[34] In our sample, the average number of battle deaths in a year of civil conflict in the sub-Saharan Africa sample is 3,347. We could also compare expected battles deaths using this figure to reach estimates ranging from 268.5 to 351.7, which also confirm the substantive conclusion that restricting attention to the extensive margin underestimates the cost of civil war.

[35] The differences are further magnified when we include the future expected battle deaths from $\hat{\gamma}$.

papers (Lacina, 2006; Bazzi and Blattman, 2011) that study how economic shocks affect the intensity of civil war. In contrast to the approach here, both of these papers restrict the sample to country-years with strictly positive battle deaths. In a specification with logged battle deaths as the dependent variable and an entire civil war as the unit of analysis,[36] Lacina (2006) finds a coefficient estimate of -0.19 on lagged log GDP per-capita, but this estimate is insignificant. While it is difficult to compare expected battle deaths directly due to the differences in the specifications, one reasonable interpretation is that a unit increase in log GDP per-capita is associated with a reduction of 2,311.8 expected battle deaths over the full course of a civil war in Lacina (2006)'s model.[37] The magnitude of this estimate is quite a bit larger than our dynamic estimate of reductions in expected battle deaths. In their specifications with battle deaths as the dependent variable, Bazzi and Blattman (2011)'s coefficient estimates on contemporaneous commodity price changes range from -803.5 to -430.3.[38] Again, these expected decreases in battle deaths from a positive standard deviation sized commodity shock are substantively larger in magnitude than our analogous estimate from the full sample. Restricting the sample to observations with strictly positive deaths seems to overstate the effects of macroeconomic performance on battle deaths from civil conflict because the sample has no variation at the extensive margin.

7.3 Concluding Remarks

Accounting for the severity of civil wars is important to understanding the effects of income on intrastate conflicts. Moving beyond the extent of civil wars provides two main conclusions. We first found that economic shocks have a strong effect on the intensity of civil conflict. This effect was especially strong compared with analogous results that focus only on the extensive margin of conflict.

More importantly, our analysis of conflict severity afforded us the ability to analyze the dynamics of civil conflict. We found that conflicts, on average, are persistent but not explosive. Conflicts appear only to be explosive for the poorest countries. The persistence of conflict also varied with income, with poorer countries having a much slower rate of mean-reversion. The persistence of conflict also varied according to other country characteristics,

[36] This contrasts with our approach where we use a country-year as the unit of analysis.

[37] More precisely, the estimated constant term in the log-log regression is 9.5. When all of the independent variables are set to 0 the expected number of battle deaths is approximately 13,359.7. A one unit increase in the GDP variable reduces expected battle deaths to 11,047.9, which gives a decrease of 2,311.8 expected battle deaths.

[38] Bazzi and Blattman (2011) standardize their commodity price shocks to have mean 0 and variance 1.

with highly ethnically fractionalized countries suffering from the most persistent conflicts.

Apart from these results, this study gave one potential explanation for the wide variation in findings regarding economic shocks and the extent of civil war. If the civil war process is dynamic, as we found to be the case, then static models are biased. This bias could contribute to the discrepancies in existing literature. This study also points towards a potentially fruitful area of future research. Cross-national work on the onset and occurrence of civil war 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.

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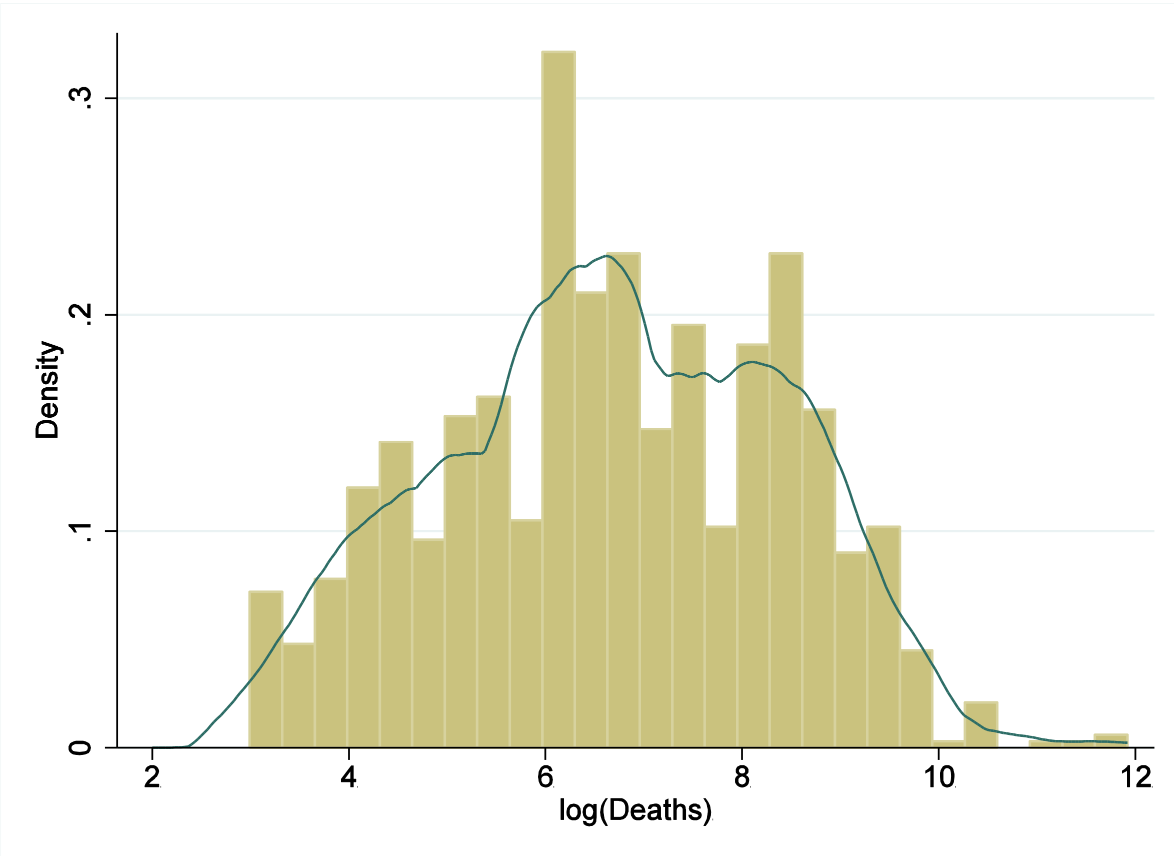
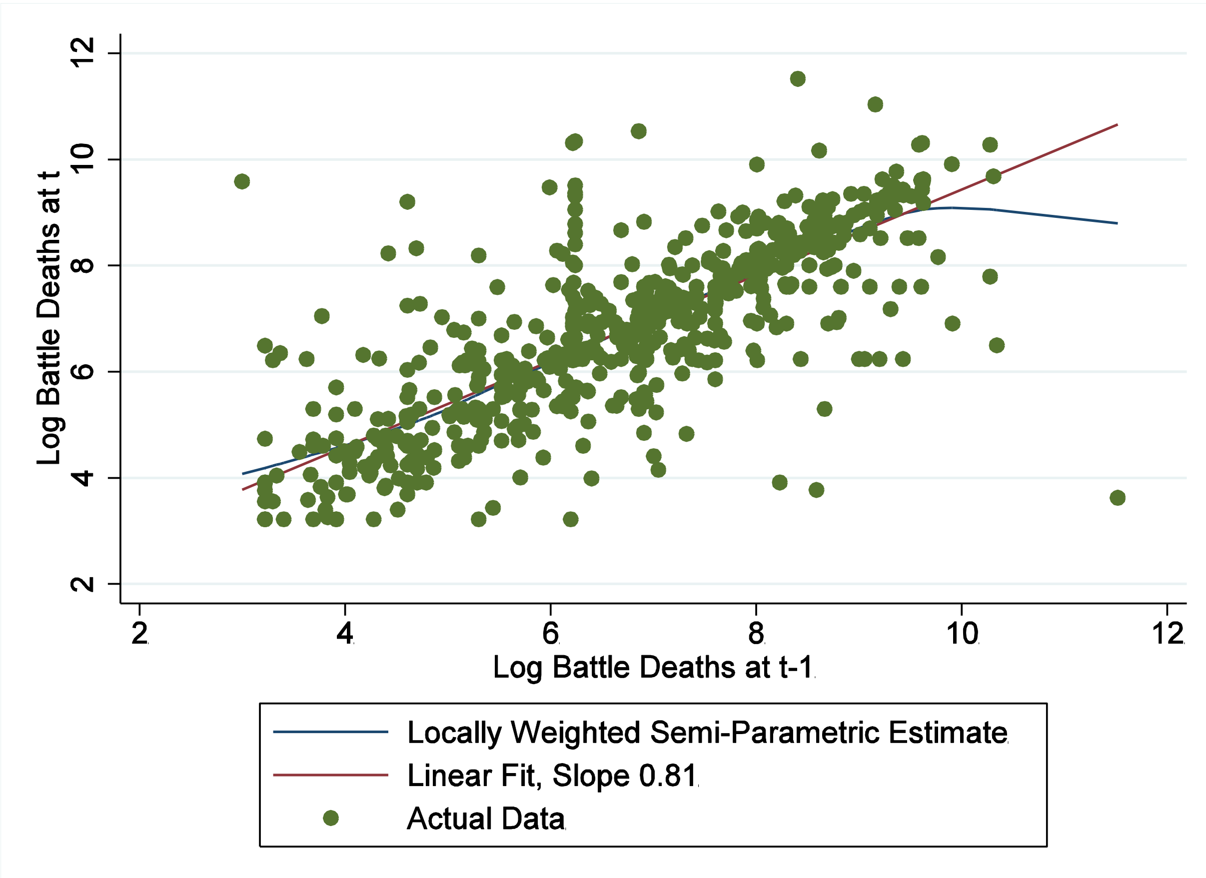
Figure 1: Distribution of Log Battle Deaths in Conflict Years



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

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
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 $Deaths_{t-1} > 0$:
Persistence Calculated as $\gamma_1 * Deaths_{t-1} + \gamma_2 * Deaths_{t-1} \times Log\ Income_{t-1} / Deaths_{t-1}$

	(1)	(2)	(3)	(4)
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.

Appendix

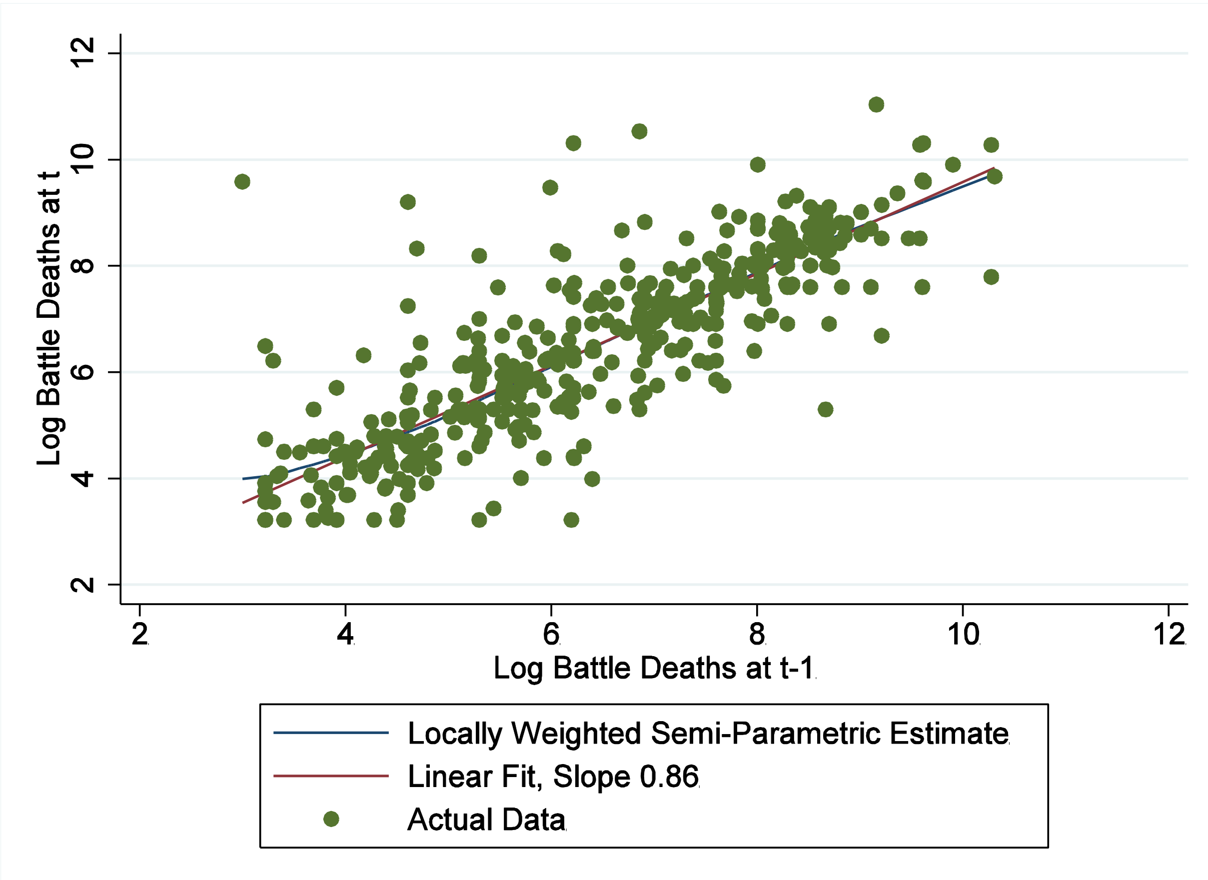
When deaths data are unavailable for particular years, the Uppsala/PRIO dataset does not report a "best estimate". Interpolation using adjacent years of data is used to fill in missing observations in these cases. Sub-saharan Africa is the region with the most missing data. There are 193 conflict-years 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.

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.