Economic Shocks and Civil Conflict: An Instrumental Variables Approach

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Abstract: Estimating the impact of economic conditions on the likelihood of civil conflict is difficult because of omitted variable bias and endogeneity. We use exogenous rainfall variation as an instrumental variable for economic growth in 41 Sub-Saharan African countries during 1981-1999, and use a new and comprehensive dataset of civil conflict. Economic growth is strongly negatively related to the incidence of civil conflict: a negative growth shock of five percentage points increases the likelihood of conflict by one-half in the following year. We attempt to rule out other channels through which rainfall might affect conflict. Surprisingly, the impact of income shocks on civil conflict is *not* significantly different in richer, more democratic, more ethnically diverse, or more mountainous African countries, or across a range of country political institutional characteristics.

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1. Introduction

Civil wars have gained increasing attention from academics and policymakers alike in recent years.[1] This concern is understandable since civil conflict is the source of immense human suffering: it is estimated that civil wars have resulted in three times as many deaths as wars between states since World War II.[2] A major locus for civil wars in recent years has been Sub-Saharan Africa, where twenty-nine of forty-three countries suffered from civil conflict during the 1980s and 1990s. In the median Sub-Saharan African country, hundreds of thousands of people were displaced from their homes as a consequence of civil war during this period.[3]

There is a growing body of research that highlights the association between economic conditions and civil conflict (see Sambanis 2001 for a review). However, the existing literature does not adequately address the endogeneity of economic variables to civil war, and thus does not convincingly establish a causal relationship. In addition to endogeneity, omitted variables – for example, government institutional quality – may drive both economic outcomes and conflict, producing misleading cross-country estimates.

In this paper we use exogenous variation in rainfall as an instrumental variable for income growth in order to estimate the impact of economic growth on civil conflict.[4] Weather shocks are plausible instruments for GDP growth in economies that largely rely on rain-fed agriculture, i.e., neither have extensive irrigation systems nor are heavily industrialized. The instrumental variable method makes it credible to assert that the association between economic conditions and civil war is causal relationship, rather than simply a correlation. As such this paper relates to the empirical approaches recently taken by Acemoglu, Johnson, and Robinson (2001), and especially Brunner (2002), who also employ an instrumental variable approach familiar from applied microeconomics in the context of cross-country empirical growth research. Note that the nature of our econometric identification strategy allows us to

[1] World Bank (2003).
[2] Fearon and Latin (2003).
[3] Sambanis (2001).
[4] Microeconomic studies that use weather as an instrumental variable for income include Paxson (1992) and Miguel (2003), among many others.

focus on short-term economic fluctuations that "trigger" conflicts, but is not as well-suited for understanding conflict duration.[5]

Sub-Saharan Africa is the ideal region for this identification strategy: the World Development Indicator database indicates that only one percent of crop land is irrigated in the median African country, and the agricultural sector remains large. We find that weather shocks are in fact closely related to income growth in Sub-Saharan Africa (in the first stage regression). However, we find that our identification strategy is inappropriate for other regions of the world, since weather is not sufficiently closely linked to income growth.[6] Although our analysis is not global, it is likely to be of exceptional interest from both the research and policy perspectives, since the incidence of civil wars in Africa is high and has increased in the past two decades.

A further strength of our empirical strategy is that it allows us to address the problem of measurement error in African national income figures, which are widely thought to be unreliable (Seers 1983, Heston 1994, Behrman and Rosenzweig 1994). An instrumental variable approach addresses the attenuation bias that may result from mismeasured explanatory variables, which, if not addressed, would bias coefficient estimates on these terms toward zero.[7]

Our main empirical findings are as follows. Using the comprehensive new database of conflicts developed by the International Peace Research Institute of Oslo, Norway and the University of Uppsala, Sweden, we find that GDP growth is significantly negatively related to the incidence of civil conflict in Sub-Saharan Africa during the period 1981-1999 across a range of regression specifications, including some with country fixed effects. The relationship between GDP growth and the incidence of civil wars is extremely strong: a five percentage point drop in annual economic growth increases the likelihood of a civil conflict (at least 25 deaths per year) in the following year by over 12 percentage points – which

[5] Coleman (1990) discusses theoretical debates regarding deeper social causes of political violence and revolution.
[6] For instance, using rainfall measures from both U.S. NASA and U.N. Food and Agriculture Organization databases (described below), we find that rainfall variation is not robustly related to income growth in the industrialized countries, Eastern Europe, Latin America, Asia, or the Middle East/North Africa region (results not shown).
[7] Krueger and Lindahl (2000) also use an instrumental variable method to address attenuation bias in cross-country estimates of the returns to education.

amounts to an increase of more than one-half in the likelihood of civil war. However, other variables that have gained prominence in the recent literature – per capita GDP level, democracy, ethnic diversity, and oil exporter status – do not display a similarly robust relationship with the incidence of civil wars in Sub-Saharan Africa. In our second main result, we find – perhaps surprisingly – that the impact of income shocks on civil conflict is *not* significantly different in richer, more democratic, more ethnically diverse, or more mountainous African countries, or in countries with a range of different political institutional characteristics.

These results resonate with previous findings by Collier and Hoeffler (1998, 2001, 2002) and Fearon and Laitin (2003) that economic variables are often more important determinants of civil war than measures of objective political "grievances." Collier and Hoeffler stress the gap between the returns from taking up arms relative to those from conventional economic activities, such as farming, as the causal mechanism linking low income to the incidence of civil war. Fearon and Laitin, however, argue that individual opportunity costs matter less than state military strength and road coverage. They argue that low national income leads to weaker militaries and worse infrastructure, and thus makes it difficult for poor governments to repress insurgencies. Our results are consistent with both explanations, and we view the opportunity cost and repressive state capacity arguments as complements rather than competing explanations: the weak repressive capabilities of African states (Herbst 2000) constitute the background conditions under which poor young men choose between fighting and conventional economic activities. Negative growth shocks make it easier for armed militia groups – which are often major combatants in Africa's civil wars – to recruit fighters from an expanding pool of underemployed youths.[8]

Admittedly, there are several alternative causal paths, aside from the labor market mechanism, through which poor economic performance could also cause civil conflict. For instance, negative economic growth may produce greater income inequality which could heighten resentment and generate tensions across social classes, or with the state. However, like previous contributors to this literature, we

[8] An excellent set of summary case studies, from the voluminous literature on African civil wars, may be found in Mekenkamp et al (1999). We briefly discuss two African case studies in Appendix 1.

are severely hampered by the absence of reliable data on income inequality, rural poverty rates, hunger, and urban unemployment in Africa and are unable to rigorously estimate the importance of these intermediate causal channels. In the end, our principal measure of current economic conditions in this paper is the annual growth rate of per capita income, largely because of its near universal availability rather than overarching theoretical considerations. Yet note that the limited data that is available does not suggest that income inequality is systematically correlated with civil conflict.[9] We also attempt to rule out other non-economic channels through which rainfall might affect conflict.

In the next section we provide an overview of the literature on the determinants of civil war. In Section 3, we describe our data, and in Section 4 discuss the estimation strategy. Section 5 contains the empirical results and further discussion of econometric identification, and the final section concludes.

2. Existing Literature

Sambanis (2001) has already provided a detailed review of the cross-country empirical literature on civil war, so we do not attempt to be comprehensive, and instead summarize main findings of recent studies.

As we mention above, economic growth may affect civil conflict through several channels. First, as in Collier and Hoeffler (1998, 2001, 2002), young men are thought to be more likely to take up arms when income opportunities are worse for them in agriculture or in the formal labor market, relative to their expected income as a fighter. Collier and Hoeffler argue that civil wars are fundamentally driven by such economic opportunities rather than by political grievances – for instance, repression against particular social groups – finding that slow income growth, low per capita income, natural resource dependence (proxied by primary commodity exports), lower male secondary education enrollment, rebel military advantages (proxied by dispersed population), and total population are all significantly positively associated with the onset of civil conflict. They also find that democracy does not reduce the probability of civil war onset, which they take as further support for the view that civil wars are not driven by political grievances. Finally, they find that conflicts in Africa have the same determinants as elsewhere.

[9] See both Collier and Hoeffler (2001) and Fearon and Laitin (2003).

Elbadawi and Sambanis (2002) study the incidence of civil war, defined as "the probability of observing either a new war onset or the continuation of an ongoing war or both" (p. 307). They confirm most of Collier and Hoeffler's findings on the role of economic factors, but find that ethnic fractionalization does have a statistically significant quadratic relationship with the incidence of civil war, with the highest probability of civil conflict at intermediate levels of diversity. In another departure from Collier and Hoeffler, they find that democracy reduces the incidence of civil war, including in Africa.

Like the above scholars, Fearon and Laitin (2003) find that lower per capita GDP is significantly associated with the onset of a civil war – this appears to be the most robust finding of the previous literature. As mentioned above, Fearon and Laitin argue that the key channels linking poverty and civil war are low repressive capabilities resulting from weak militaries and poor roads. Using novel geographic data, they also emphasize the role of rough terrain – captured by percentage of the country that is mountainous – in sustaining insurgencies. Fearon and Laitin concur with Collier and Hoeffler on the weak link between democracy and conflict, and also find that ethnic diversity does not contribute to conflict onset.[10]

These authors are aware of the potential endogeneity problems in estimating the relationship between civil war and economic outcomes, and attempt to address this by using lagged values of per capita GDP growth and/or levels as explanatory variables.[11] However, this approach implicitly assumes that economic actors do not anticipate the incidence of civil war and adjust economic activity (e.g., investment) accordingly. Since this is a very strong assumption, simply lagging economic variables is not a convincing solution to the endogeneity problem. Others, including Elbadawi and Sambanis (2002), use an instrumental variables approach, but, in our view they do not provide a sufficiently transparent discussion of why the instruments they choose are plausible.[12] The existing analyses may also be prone to omitted variable bias: fast-growing countries may differ from slow-growing countries along many

[10] Easterly and Levine (1997) also find that ethnic diversity is not significantly related to conflict across countries.
[11] Refer to Collier and Hoeffler (2002), Fearon and Laitin (2003).
[12] For instance, Sambanis (2000) and Elbadawi and Sambanis (2002) appear to employ lagged endogenous variables as instrumental variables, although this is not entirely clear from the papers.

institutional dimensions, some of which are hard to measure, and thus it becomes difficult to pinpoint the true underlying causes of conflict.

3. Data and Measurement

3.1 Data on Civil Conflict

Most contributors to the existing literature on civil conflict have worked with, or built on, the Correlates of War (COW) database. However, the lack of transparency and inconsistencies of the COW database are well known, and have been the subject of a detailed evaluation by Sambanis (2002).[13] Furthermore, the arbitrary 1000 death threshold the COW database, and virtually every other database, uses to identify a civil war has the danger of excluding conflicts that may be major for smaller countries, including many African countries.

We instead use the new Armed Conflict Data database developed by the International Peace Research Institute of Oslo, Norway and the University of Uppsala, Sweden (we refer to it as PRIO/Uppsala). The PRIO/Uppsala database is more transparent in its construction than COW, and also, uniquely, records all conflicts with a threshold of 25 battle deaths per year, in addition to classifying conflicts by the standard 1000 death threshold, thus including more small conflicts in the analysis. An armed conflict is defined in the PRIO/Uppsala database as follows: "a contested incompatibility which concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths."[14] The database is careful to only focus on politically motivated violence. Note that, like other cross country civil war data sets, PRIO/Uppsala unfortunately does not include conflict information at the sub-national level, or by month

[13] For instance, it is unclear if the Correlates of War database uses 1000 cumulative deaths, or 1000 per year, when identifying a civil war. Refer to Sarkees et al. (2003) for another recent discussion of the COW database.
[14] Refer to the PRIO website (www.prio.no/cwp/ArmedConflict) or the University of Uppsala website (www.pcr.uu.se). A more detailed description of the criteria used to code civil wars is provided in Gleditsch et al. (2002) and in Appendix 1 of this paper.

within each year, nor does it provide the exact number of conflict deaths, and this by necessity limits certain aspects of the empirical analysis.

Our empirical analysis has other limitations. First, the above definition of conflict means that we do not capture many important types of organized violence in Sub-Saharan Africa that do not directly involve the state – for instance, clashes among pastoralist groups in northern Kenya, or crime related to the drugs trade in Lagos, Nigeria – that are of considerable research interest in their own right. Second, we do not focus separately on ethnic violence (refer to Fearon and Laitin 2003 for a recent discussion), although we do examine the effects of ethnic diversity in our econometric analysis below. Finally, while the PRIO/Uppsala database also includes detailed information on conflicts between countries, we focus exclusively on civil wars – the PRIO/Uppsala conflict Categories 3 and 4, which cover civil conflict with and without interference from other countries, respectively.[15]

The civil conflict indicator variable for country i in year t is denoted $CONFLICT_{it}$, and all country-year observations with a civil conflict in progress with at least 25 battle deaths per year (or 1000 battle deaths, in some specifications) are coded as ones, while other observations are zeros. Civil conflict was remarkably widespread in Sub-Saharan Africa during the period 1981-1999: fully 27 percent of all country-year observations suffered from conflict according to the PRIO/Uppsala 25 annual battle deaths definition, 17 percent according to the PRIO/Uppsala 1000 deaths definition, and 24 percent under the Fearon and Laitin (2003) definition, using a 1000 death threshold (Table 1, Panel A).[16] In addition to conflict incidence, we also examine onset, where $ONSET_{it}$ is an indicator variable such that $1(CONFLICT_{it} = 1 \mid CONFLICT_{i,t-1} = 0)$. Thirty-eight separate conflicts began during the sample period of 1981-1999 – not including conflicts that were ongoing in 1981 – and 27 ended, at least temporarily.

3.2 Rainfall Data

[15] We leave an empirical analysis of the causes of conflicts between countries for future research.
[16] Appendix 2 Table A1 contains a list of all countries in our sample, and the number of years for which they are coded as having a conflict in the PRIO/Uppsala dataset.

We use the Global Precipitation Climatology Project (GPCP) database of monthly rainfall estimates, which stretches back to 1979, as a source of exogenous weather variation.[17] The GPCP data relies on a combination of actual weather station rainfall gauge measures, as well as satellite information on the density of cold cloud cover (which is closely related to actual precipitation) to derive rainfall estimates, at 2.5 latitude and longitude degree intervals. We focus on the GPCP dataset over two other possible global weather sources – the National Centers for Environment Prediction (NCEP) database and the U.N. Food and Agricultural Organization Climatic (FAOCLIM) database – because it is the only one of these three datasets that at the same time: includes both gauge and satellite data; rejects gauge measures thought to be unreliable; and corrects for systematic errors in gauge measures (Rudolf 2000).[18] In any case, the correlation among these three datasets is high (over 0.8), and we find similar empirical results with the alternative datasets, as discussed below.[19]

As far as the mechanics of the rainfall data are concerned, we have rainfall estimates for each point at which latitude and longitude degree lines cross, at the 2.5 degree intervals.[20] Using this dataset, Kenya, a medium-sized African country, contains eight rainfall data "nodes", while the largest country, Sudan, contains thirty-four nodes. The GPCP rainfall measure at latitude-longitude degree node point p in country i, during month m of year t is denoted R_{ipmt}, and we denote the average rainfall across all points p and months m for that year, R_{it}. Our principal measure of a rainfall shock is the proportional change in rainfall from the previous year, $(R_{it} - R_{i,t-1}) / R_{i,t-1}$, denoted, $?R_{it}$. We examined various alternative measures of rainfall variation – including the sum of squared rainfall deviations across all nodes in a given year, absolute rainfall deviations (from average levels), and absolute rainfall deviations greater than

[17] The GPCP data is publicly available on the web at http://orbit-net.nesdis.noaa.gov/arad/gpcp/.
[18] FAOCLIM uses only gauge data, which considerably limits its coverage, while NCEP does not reject unreliable data sources or correct for systematic gauge errors.
[19] In a previous version of this paper, we also employed Normalized Difference Vegetation Index (NDVI) data, which captures the density of plant life – and is closely related to rainfall in Africa, at correlation 0.9 – as an alternative measure of weather variation, but we no longer focus on this measure since vegetation levels may be a function of crop choices made in response to civil conflict, and could thus potentially be endogenous.
[20] No degree grid node fell within the national boundaries for five small African countries – Burundi, Djibouti, Gambia, Guinea-Bissau, and Rwanda – so in these cases we assigned them rainfall measures from the nearest node to their borders.

certain threshold levels – but these measures are not as strongly correlated with income growth in the first stage regressions (results not shown). Descriptive statistics indicate that there is considerable variation in rainfall in the sample (Table 1, Panel B), and this holds both across countries and through time for the same country.

3.3 Other Country Characteristics

The remaining data is drawn mainly from Fearon and Laitin (2003) and from World Bank databases;[21] we do not describe these well-known variables in detail here, and instead refer the reader to the excellent data description in Fearon and Laitin (2003). The main country control variables include: ethnolinguistic fractionalization (drawn from the Soviet ethnographic index *Atlas Marodov Mira*), and religious fractionalization (based on the CIA Factbook); measures of democracy (from the Polity IV dataset); the log of per capita income (from the Penn World Tables and the World Bank); the proportion of a country that is mountainous according to the geographer A.J. Gerard (from Fearon and Laitin 2003); log of total country population (based on World Bank data); oil exporters, measured by an indicator for countries where oil constitutes more than one-third of export revenues, based on World Bank data (Table 1, Panels C and D). We do not include measures of income inequality, poverty, or unemployment rates as additional explanatory variables, due to the large number of missing or unreliable African observations in existing macroeconomic series.

4. Estimation Framework

We focus principally on the incidence of civil war in country i, year t ($CONFLICT_{it}$) according to the PRIO/Uppsala database, but also present results using the onset of conflict, since the impact of income shocks on conflict may theoretically differ depending on whether the country is already experiencing conflict. We use weather variation, as captured in current and lagged rainfall growth ($?R_{it}$, $?R_{i,t-1}$) as instrumental variables for per capita economic growth ($GROWTH_{it}$) in the first stage, controlling for other

[21] We thank for Jim Fearon and David Laitin for their generosity in sharing this data.

country characteristics (X_{it}); results are broadly similar if current and lagged deviations from the average country rainfall level are used as instrumental variables for growth instead (results not shown). We include country fixed effects (a_i) in some specifications to capture time-invariant country-characteristics that may be related to civil conflict and also include country-specific time trends in most specifications to capture additional variation:

$$(1) \qquad GROWTH_{it} = a_{1i} + X'_{it}b_1 + c_{1,0}R_{it} + c_{1,1}R_{i,t-1} + d_{1i}YEAR_t + e_{1it}$$

(The "1" subscript here denotes the equation number here.) e is a disturbance term, and these disturbances are allowed to be correlated across years for the same country in all regressions.

Note that the first-stage relationship between rainfall and income growth is strongly positive: current and lagged rainfall growth are both significantly related to income growth at over 95 percent confidence (Table 2, regression 1), and this relationship is robust to the inclusion of country controls (regression 2) and fixed effects (regression 3). Positive rainfall growth typically leads to better agricultural production since most of Sub-Saharan Africa lies within the semi-arid tropics and is prone to drought. The rainfall instruments are reasonably strong (F-statistic=4.5 and p-value=0.01 in regression 3), suggesting that bias due to weak instruments is unlikely to apply in our IV-2SLS estimates (Bound et al 1995). As an identification check, we also estimated a "false experiment" specification in which future rainfall growth – which should be orthogonal to current economic growth – is included as an additional explanatory variable, and find that it is indeed not statistically significant (coefficient estimate 0.001, standard error 0.019, regression 4).[22] The change in the country's terms of trade (which are driven by commodity price movements) is not significantly related to economic growth (regression 5).

The positive and approximately linear first-stage relationship is presented graphically in Figure 1, using a non-parametric Fan local regression method with an Epanechnikov kernel (Deaton 1997). Higher order polynomial rainfall growth terms are not statistically significantly related to economic growth (results not shown). We experimented with a variety of other instrumental variables – including further

[22] We thank Guido Imbens for this suggestion.

lags of rainfall growth; the interaction of current and lagged rainfall growth; current and lagged rainfall levels; the interaction of rainfall growth with the share of agriculture sector value added in national GDP – but the first-stage results in those cases are weaker than the specifications presented in Table 2 (results not shown), so we opt for the above specification.

Higher levels of rainfall are associated with significantly *less* conflict in the reduced-form regression, both for all civil conflicts (Table 3, regression 1), with a point estimate of –0.122 (standard error 0.052) on lagged rainfall growth, as well as for major conflicts (regression 3), in which case coefficient estimates on both current and lagged growth are statistically significant at 95 percent confidence. This is our first indication that better rainfall makes civil conflict less likely in Africa. The false experiment specifications indicate that future rainfall growth is not statistically significantly related to either measure of current conflict (regressions 2 and 4). The negative and nearly linear non-parametric relationship between lagged rainfall growth and conflict is presented in Figure 2.

The second stage equation estimates the impact of income growth on the incidence of violence:

$$(2) \qquad CONFLICT_{it} = \alpha_{2i} + X_{it}'\beta_2 + \beta_{2,0}GROWTH_{it} + \beta_{2,1}GROWTH_{i,t-1} + \gamma_{2i}YEAR_t + \varepsilon_{2it}$$

We performed both Instrumental Variable Two-Stage Least Squares (IV-2SLS) estimation, as well as a non-linear two-stage procedure following Achen (1986) to correct standard errors in the presence of a dichotomous dependent variable in the second stage. The IV-2SLS method is typically preferred even in cases where the dependent variable is dichotomous (see Angrist and Kreuger 2001 and Wooldridge 2002) since strong specification assumptions are required to justify the Achen (1986) and related Rivers and Vuong (1988) methods, and we thus focus on the IV-2SLS specification below. Note that we find similar results with both specifications, although statistical significance falls somewhat when we employ the non-linear second stage specification and bootstrap the standard errors (results not shown).

5. Main Empirical Results

Contemporaneous and lagged economic growth rates are negatively, though not statistically significantly, correlated with the incidence of civil conflict in probit (Table 4, regression 1) and OLS specifications with country controls (regression 2), and contemporaneous growth is negatively associated with conflict in OLS specifications with and without country fixed effects (regressions 3 and 4). The results using probit and linear specifications are nearly identical, and from now on we restrict our attention to the latter. Note that of the other variables prominently cited in the existing literature, only the measure of mountainous terrain has statistically significant predictive power in these specifications, and national population is also marginally positively associated with conflict in one specification. We also concur with Fearon and Laitin (2003) and others that ethnic diversity is not significantly associated with civil conflict in Sub-Saharan Africa.

An instrumental variable estimate including country controls yields point estimates of –2.14 (standard error 1.03) on lagged growth, which is significant at 95 percent confidence, and–0.38 (standard error 1.38) on current growth (Table 4, regression 5). The two growth terms are jointly significant at over 90 percent confidence (p-value=0.09). The IV-2SLS fixed effects estimate on lagged growth is similarly large, negative, and significant at –2.53 (standard error 1.10, regression 6). Note that we cannot reject the hypothesis in either specification that current and lagged economic growth have the same impact on civil conflict. Since we have instrumented for economic growth, we make the causal assertion that the incidence of civil wars in Sub-Saharan Africa is influenced by economic shocks, while a range of other political, social, and geographic variables have at best a tenuous impact.[23] Using the IV-2SLS method we discover that economic shocks have an even more dramatic impact on civil war incidence than has been previously recognized. (Of course, this finding does not imply that the reverse relationship – civil conflict may also reduce economic growth – is not present as well, but the identification strategy does not allow us to shed light on that issue.)

[23] We also find that economic growth shocks do not cause changes in the measured extent of democracy in this sample (results not shown).

The size of the estimated impact of lagged economic growth on conflict is huge: focusing on the IV-2SLS fixed effects specification as our benchmark, the point estimate indicates that a one percentage point decline in GDP increases the likelihood of civil conflict by over two percentage points, and thus a 5 percentage point decline in lagged growth – which is somewhat less than one standard deviation in annual per capita growth (Table 1) – leads to a greater than 12 percentage point increase in the incidence of civil war, an increase of nearly one-half of the average likelihood of conflict. This IV-2SLS estimate is in fact much more negative than the analogous OLS estimates, which suggests that bias due to measurement error in the per capita income growth measures is likely to be larger in magnitude than the endogeneity bias, which is presumably negative. Note that we are left with the unexpected finding of positive (though insignificant) point estimates on lagged growth in certain OLS specifications (Table 4, regressions 3 and 4), which casts some doubt on this attenuation bias explanation for the difference between the OLS and IV estimates. Unfortunately, we are aware of no work that quantifies the extent of measurement error in African national income data, or determines whether measurement errors are classical (i.e., white noise) at all, although the claim is often made that these errors are likely to be large (Seers 1983, Heston 1994, Behrman and Rosenzweig 1994).

In our preferred IV-2SLS fixed effects framework, the impact of current economic growth on *major* civil conflicts – those with at least 1000 deaths, according to PRIO/Uppsala – is large and significant at 90 percent confidence, with a point estimate of –1.48 (standard error 0.82, Table 4, regression 7), suggesting that the causal link between economic conditions and civil conflict holds for both major and minor conflicts. Given that the mean incidence of major wars is only 0.17, a negative contemporaneous economic growth shock of 5 percentage points again increases the likelihood of major civil war by nearly one-half. The impact of lagged growth is negative but not statistically significant in this case (estimate –0.76, standard error 0.70). We are left with the finding that lagged growth is more important than current growth in generating conflicts at the 25 deaths threshold, while current growth has a larger estimated effect on the incidence of conflicts at the 1000 deaths threshold. Note that this is not

driven by differences in the timing of conflict onset, as shown below in Table 5. However, note that we cannot reject the hypothesis that effects are in fact the same for current and lagged growth in any regression specification, and thus we do not emphasize these differences.

We next perform additional robustness checks and explore a range of different dependent variables. The IV-2SLS fixed effects results for the 25 death threshold are robust to dropping one country at a time, with coefficients on lagged economic growth ranging from –2.9 to –1.9, and remaining significant at 95 percent confidence levels in all regressions. There is no statistically significant differential impact of positive versus negative economic growth shocks on civil conflict (regression not shown), as suggested by Figure 2. Including only lagged economic growth as an explanatory variable (instrumented only with lagged rainfall growth) rather than both lagged and current growth yields similar results (coefficient estimate -2.85, standard error 1.29 – regression not shown). Results are similarly large and statistically significant when we use an ordered probit specification with the categories no conflict (fewer than 25 battle deaths per year), minor conflict (between 25 and 1000 deaths), and major conflicts (more than 1000 deaths) – results not shown. The impact of lagged growth on civil conflict is large and negative for alternative measures of rainfall (Appendix 2, Table A2), both for the NCEP (estimate –2.25, standard error 1.35) and FAOCLIM (-1.31, standard error 0.67) datasets. The results are also similar if we conduct our main analysis on the databases used by Collier and Hoffler (Appendix 2, Table A3, regression 3), Doyle and Sambanis (regression 4), and Fearon and Laitin (regression 5), although statistical significance tends to be somewhat lower with the 1000 death threshold used in these datasets than with the 25 death PRIO/Uppsala threshold. Still, all ten coefficient estimates on economic growth in Table A3 are negative with t-statistics greater than one, and several are significantly different than zero at high levels of confidence (i.e., using the Fearon and Laitin dataset).

In our second main result, we find that the impact of economic growth shocks on the incidence of major conflicts is remarkably – and perhaps surprisingly – similar for African countries with a wide range of institutional, political, social, and economic characteristics. There are compelling theoretical reasons

to expect to find such effects; for instance, given an adverse economic growth shock, countries with stronger democratic institutions (and similarly, wealthier countries) may be better able to negotiate compromises among social groups to avert unrest, while such negotiations may more often break down in ethnically or religiously fragmented societies (Benhabib and Rustichini 1996; Easterly and Levine 1997). However, we find that the interactions between economic growth (current and lagged) and a measure of democracy (Table 5, regression 1), and between growth and per capita income levels in 1979 (regression 2), are not significantly related to civil conflict, nor are the two interaction terms jointly significant in either case.[24] In other words, the democracy interaction results indicate that relatively non-democratic African countries hit by negative income shocks are just as prone to civil conflict as relatively democratic countries, suggesting that even democratic states in Africa typically lack the institutional capability to adequately respond to negative economic shocks and avert conflict (Van de Walle 2002). Coefficient estimates on the interaction terms in both of these regressions are reasonably precisely estimated, and thus we have the statistical power to rule out moderate-sized effects. We find similarly weak results for interactions with non-linear measures of democracy (not shown).

We also find that there is no differential impact of economic growth shocks in more or less ethnically diverse countries (Table 5, regression 3 – although in this case the coefficient estimates on the interaction terms are imprecisely estimated); in oil-producing countries (regression 4); or in mountainous countries (regression 5). There is similarly no significant difference in the effect of economic growth on conflict across former British colonies, French colonies and other countries; by African Sub-region (Central, East, Southern, and West Africa); for countries with Socialist political regimes at the start of the sample period (from Barro 1991); by religious fractionalization; population density; or a range of measures of democracy, political competition, regulation of political participation, and constraints on executive power (from the Polity IV dataset); other political institutional measures, including the degree

[24] These results also hold if conflict at the higher 1000 death threshold is the dependent variable, or in a reduced-form specification in which country characteristics are interacted with rainfall shocks (results not shown).

of federalism, and government checks and balances (from the World Bank's Database of Political Institutions); and political and civil freedom (using Freedom House data – results not shown).

The simplest reading of these findings is that economic factors trump all others in determining civil conflict incidence, and in particular, that institutional and social characteristics have minimal impact in mitigating the effects of economic shocks. However, it is important to note that the relatively limited variation in many of these characteristics across African countries during the sample period – most were poor, ethnically diverse, and undemocratic, with similar colonial histories – means that this finding may not generalize to other regions of the world, with their broadly different constellation of economic, social and political characteristics. Moreover, despite our attempts to examine the broadest possible range of country political and social characteristics, it remains possible that some other characteristics not adequately captured in existing datasets – perhaps along the lines of the "shadow state" institutions described by Reno (1998) in West Africa – do mitigate the adverse effects of negative economic shocks, but that we are unable to examine them here.

Finally, we explore how economic growth affects the onset of conflict, and to do so we restrict attention to country-year observations in which there was no civil conflict during the previous year. Using both PRIO/Uppsala definitions, 25 and 1000 battle deaths, conflicts are significantly less likely to start as economic growth increases (Table 6, regressions 1 and 2), and once again we cannot reject the hypothesis that effects are the same for current and lagged economic growth. The results are robust to the inclusion of country controls rather than fixed effects, and there is no significant difference in the impact of growth shocks on the onset of conflict for countries with different political and social characteristics (results not shown). The results on the ending, or "offset", of conflict are also consistent with the incidence findings in Table 4, with mainly positive point estimates on economic growth (regressions not shown), although in neither case are estimates significantly different than zero at traditional confidence

levels; the sharp drop in sample size in these offset regressions partially accounts for this lack of statistical precision.[25]

5.1 Potential Violations of the Exclusion Restriction

While it is intuitively plausible that our rainfall instruments are exogenous, they must also satisfy the exclusion restriction: weather shocks should only affect civil conflict through economic growth. In the introduction above, we acknowledge the possibility that economic channels other than per capita economic growth *per se* (i.e., income inequality, or rural poverty rates) may be key underlying causes of civil conflict in the aftermath of adverse rainfall shocks, but unfortunately do not have reliable cross-country data on these other intermediate channels. We do, however, have central government budget figures for approximately half of our sample period from the World Bank, and find that rainfall growth is not significantly associated with tax revenues (neither total revenues, nor revenue as a proportion of national income – results not shown), indicating that changes in governments' fiscal positions are unlikely to be driving our findings.

A more serious violation of the exclusion restriction is the possibility that high levels of rainfall might directly affect civil conflict independently of economic conditions. For instance, floods may destroy the road network and thus make it more costly for government troops to contain rebel groups. Note that this first possibility is not a serious threat to our estimation strategy, since higher levels of rainfall are empirically associated with significantly *less* conflict in the reduced-form regressions, and thus to the extent that this bias exists, our estimates would be lower bounds on the true impact of economic growth.

Another possibility, however, is that rainfall may make it difficult for both government and rebel forces to engage each other in combat, and to achieve the threshold number of deaths that constitute a

[25] In specifications where lagged conflict is included as an explanatory variable, and is also interacted with the economic growth shocks, in our preferred fixed effects IV specification, these interaction terms are not statistically significant but the coefficient estimate on lagged growth remains similar (point estimate -1.7, standard error 1.3, regression not shown), suggesting that not much additional insight is gained with this alternative specification. However, note that including lagged dependent variables as explanatory variables may lead to bias in fixed effects estimation, and hence we do not emphasize these results here.

conflict, due to more difficult transportation conditions. To explore this possibility, we estimated the impact of rainfall shocks on the extent of the useable road network (using World Bank data), and did not find a statistically significant relationship; in fact, the point estimates on current and lagged rainfall are both positive (e.g., the coefficient on current rainfall growth is 192, standard error 1023 – regression not shown), which argues against the above theory. Another potential violation of the exclusion restriction could occur if low rainfall is associated with heat waves that raise tempers and spontaneously provoke conflict. However, we showed above that the incidence of conflict using the 25 death threshold is most responsive to economic growth (and rainfall) lagged by one year, which would presumably leave ample time for "cooler" heads to prevail and avert such conflicts. It should further be noted that we have not found references to either of these two potential violations of the exclusion restriction in our survey of the case study literature. Nonetheless, we acknowledge that we are unable to definitively rule out the possibility that rainfall could have some independent impact on the incidence of civil conflict beyond its effect working through economic growth, though we believe these other effects are likely to be minor.

6. Conclusion

We address a major methodological problem that lies at the core of the cross-country literature on civil wars, the potential endogeneity of the economic factors often used as explanatory variables, by using rainfall shocks as instrumental variables for economic growth. We find that economic growth shocks have a dramatic causal impact on the likelihood of civil war: a five percentage negative growth shock increases the likelihood of a civil war the following year by nearly one-half. In our sample of African countries, the impact of economic shocks is also approximately the same across countries with a range of different economic, social and political institutional characteristics, suggesting that economic conditions are the most critical determinants in triggering civil conflict in Africa.

The implications of this research are potentially important from a public policy perspective: if a short-term drop in the opportunity cost of being a rebel (or government) soldier significantly increases the

incidence of civil conflict, it may be possible to reduce the incidence of conflict through the design of better income insurance for unemployed young men during hard economic times. One example would be public works projects funded by international donors during recessions (formal unemployment insurance programs, similar to those found in wealthy countries, are unlikely to succeed in poor rural African settings due to limited institutional capacity). If such public work schemes focused on building roads – facilitating the transport of foodstuffs – and irrigation or water systems, they could also serve to reduce local vulnerability to future rainfall shocks.

Despite the progress we feel has been made with the approach taken in this paper, given the inherent limitations of cross-country analysis, we believe that further micro-empirical analysis and careful case studies are urgently needed to illuminate the precise causal channels linking economic conditions to civil conflict, and to allow researchers to draw more credible policy prescriptions for Sub-Saharan Africa and other regions suffering from conflict.[26]

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[26] Refer to MacCulloch (2003) for new micro-empirical work along these lines, based on internationally comparable survey data. Keen (1998) presents insightful case studies regarding the underlying economic motivations of participants in several recent African civil conflicts. Of course, the difficulty in collecting reliable micro-economic data on economic conditions and individual decisions in wartime situations will likely complicate this endeavor.

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9. Tables and Figures

Table 1: Descriptive Statistics

	Mean	Std dev.	Obs.
Panel A: Civil conflict measures (1981-1999)			
Civil conflict with ? 25 deaths, PRIO/Uppsala	0.27	0.44	743
Onset	0.07	0.25	555
Offset	0.15	0.36	188
Civil conflict with ? 1000 deaths, PRIO/Uppsala	0.17	0.37	743
Onset	0.04	0.19	625
Offset	0.15	0.36	118
Civil conflict with ? 1000 deaths, Collier and Hoeffler (CH)	0.17	0.38	743
Civil conflict with ? 1000 deaths, Doyle and Sambanis (DS)	0.22	0.41	724
Civil conflict with ? 1000 deaths, Fearon and Laitin (FL)	0.24	0.43	743
Panel B: Rainfall measures (1981-1999)			
Annual rainfall (mm), GPCP measure	1001.6	501.7	743
Annual growth in rainfall, time t	0.018	0.209	743
Annual growth in rainfall, time t-1	0.011	0.207	743
Panel C: Economic growth			
Annual economic growth rate, time t	-0.005	0.071	743
Annual economic growth rate, time t-1	-0.006	0.072	743
Panel D: Country characteristics			
Log(GDP per capita), 1979	1.16	0.90	743
Democracy level (Polity IV score, -10 to 10), time t-1	-3.6	5.6	743
Democracy indicator (Polity IV score > 5), time t-1	0.15	0.36	743
Ethno-linguistic fractionalization (source: Atlas Marodov Mira)	0.65	0.24	743
Religious fractionalization (source: CIA Factbook)	0.49	0.19	743
Oil exporting country (source: World Development Indicators, WDI)	0.12	0.32	743
Log(mountainous) (source: Fearon and Laitin)	1.6	1.4	743
Log (national population), time t-1 (source: WDI)	8.7	1.2	743
Growth in terms of trade, time t (source: WDI)	-0.01	0.16	661

Table 1 Notes: The source of most characteristics in Panel D is the World Bank's World Development Indicators. Initial log per capita income for Namibia is for 1990, its first year in the sample (after independence).

Table 2: Rainfall and Economic Growth (First-Stage)

	Dependent variable: Economic growth rate, t				
Explanatory variable	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)
Growth in rainfall, t	0.055***	0.053***	0.048***	0.049***	0.053***
	(0.016)	(0.017)	(0.017)	(0.018)	(0.018)
Growth in rainfall, t-1	0.034**	0.032**	0.028**	0.028**	0.037**
	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)
Growth in rainfall, t+1				0.001	
				(0.019)	
Growth in terms of trade, t					-0.002
					(0.023)
Log(GDP per capita), 1979		-0.010*			
		(0.006)			
Democracy (Polity IV), t-1		0.0001			
		(0.0007)			
Ethno-linguistic fractionalization		0.009			
		(0.045)			
Religious fractionalization		0.023			
		(0.043)			
Oil exporting country		0.002			
		(0.020)			
Log(mountainous)		0.001			
		(0.005)			
Log (national population), t-1		-0.002			
		(0.009)			
Country fixed effects	No	No	Yes	Yes	Yes
Country-specific time trends	No	Yes	Yes	Yes	Yes
R^2	0.02	0.07	0.13	0.13	0.16
Root MSE	0.07	0.07	0.07	0.07	0.06
Number of observations	743	743	743	743	661

Table 2 Notes: Ordinary least squares (OLS) specifications. Huber robust standard errors in parentheses. Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Regression disturbance terms are clustered at the country level. A country-specific year time trend is included in all specifications (coefficient estimates not reported).

Table 3: Rainfall and Civil Conflict (Reduced-Form)

	Dependent variable:			
	Civil conflict ? 25 deaths		Civil conflict ? 1000 deaths	
	OLS	OLS	OLS	OLS
Explanatory variable	(1)	(2)	(3)	(4)
Growth in rainfall, t	-0.024	-0.022	-0.063**	-0.040
	(0.043)	(0.062)	(0.030)	(0.047)
Growth in rainfall, t-1	-0.122**	-0.120**	-0.068**	-0.054
	(0.052)	(0.061)	(0.032)	(0.043)
Growth in rainfall, t+1		0.003		0.039
		(0.056)		(0.047)
Country fixed effects	Yes	Yes	Yes	Yes
Country-specific time trends	Yes	Yes	Yes	Yes
R^2	0.70	0.70	0.70	0.70
Root MSE	0.26	0.26	0.22	0.22
Number of observations	743	743	743	743

Table 3 Notes: Ordinary least squares (OLS) specifications. Huber robust standard errors in parentheses. Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Regression disturbance terms are clustered at the country level. A country-specific year time trend is included in all specifications (coefficient estimates not reported).

Table 4: Economic Growth and Civil Conflict

Explanatory variable	Dependent variable: Civil conflict ? 25 deaths: Probit (1)	OLS (2)	OLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)	Civil conflict ? 1000 deaths: IV-2SLS (7)
Economic growth rate, t	-0.37	-0.33	-0.15	-0.18	-0.38	-1.13	-1.48*
	(0.26)	(0.26)	(0.19)	(0.16)	(1.38)	(1.40)	(0.82)
Economic growth rate, t-1	-0.14	-0.08	0.07	0.09	-2.14**	-2.53**	-0.76
	(0.23)	(0.24)	(0.19)	(0.16)	(1.03)	(1.10)	(0.70)
Log(GDP per capita), 1979	-0.067	-0.041	0.093		0.063		
	(0.061)	(0.050)	(0.072)		(0.080)		
Democracy (Polity IV), t-1	0.001	0.001	0.004		0.005		
	(0.005)	(0.005)	(0.006)		(0.006)		
Ethno-linguistic fractionalization	0.24	0.23	0.53		0.53		
	(0.26)	(0.27)	(0.41)		(0.41)		
Religious fractionalization	-0.29	-0.24	-0.08		-0.01		
	(0.26)	(0.24)	(0.41)		(0.43)		
Oil exporting country	0.02	0.05	-0.19		-0.15		
	(0.21)	(0.21)	(0.21)		(0.23)		
Log(mountainous)	0.077**	0.076*	0.06		0.06		
	(0.041)	(0.039)	(0.06)		(0.06)		
Log (national population), t-1	0.080	0.068	0.232***		0.226**		
	(0.051)	(0.051)	(0.081)		(0.087)		
Country fixed effects	No	No	No	Yes	No	Yes	Yes
Country-specific time trends	No	No	Yes	Yes	Yes	Yes	Yes
R^2	-	0.13	0.52	0.70	-	-	-
Root MSE	-	0.42	0.32	0.26	0.36	0.32	0.24
Number of observations	743	743	743	743	743	743	743

Table 4 Notes: Huber robust standard errors in parentheses. Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Regression disturbance terms are clustered at the country level. Regression (1) presents marginal probit effects, evaluated at explanatory variable mean values. The instrumental variables for economic growth in (5)-(7) are (Growth in rainfall, t) and (Growth in rainfall, t-1). A country-specific year time trend is included in all specifications (coefficient estimates not reported), except for (1) and (2), where a linear time trend is included.

Table 5: Interactions between Economic Growth and Country Characteristics

	Dependent variable: Civil conflict ? 25 deaths				
Explanatory variable	IV-2SLS (1)	IV-2SLS (2)	IV-2SLS (3)	IV-2SLS (4)	IV-2SLS (5)
Economic growth rate, t	0.59	2.55	7.62	0.80	-0.53
	(1.78)	(2.67)	(35.7)	(2.16)	(1.92)
Economic growth rate, t-1	-2.15	-2.29	1.15	-1.85	-1.34
	(1.45)	(1.92)	(5.43)	(1.33)	(1.70)
Economic growth rate, t *	-0.13				
Democracy (Polity IV), t-1	(0.29)				
Economic growth rate, t-1 *	-0.09				
Democracy (Polity IV), t-1	(0.19)				
Economic growth rate, t *		-1.35			
Log (Per capita income, 1979)		(0.97)			
Economic growth rate, t-1 *		0.31			
Log (Per capita income, 1979)		(0.77)			
Economic growth rate, t *			-9.99		
Ethno-linguistic fractionalization			(47.6)		
Economic growth rate, t-1 *			-4.11		
Ethno-linguistic fractionalization			(8.38)		
Economic growth rate, t *				0.7	
Oil exporting country				(11.2)	
Economic growth rate, t-1 *				-5.8	
Oil exporting country				(5.5)	
Economic growth rate, t *					1.28
Log(mountainous)					(1.87)
Economic growth rate, t-1 *					-0.63
Log(mountainous)					(1.88)
Country controls	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	No	No	No
Country-specific time trends	Yes	Yes	Yes	Yes	Yes
R^2	-	-	-	-	-
Root MSE	0.36	0.37	0.41	0.40	0.43
Number of observations	743	743	743	743	743

Table 5 Notes: Huber robust standard errors in parentheses. Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Regression disturbance terms are clustered at the country level. The instrumental variables are (Growth in rainfall, t) and (Growth in rainfall, t-1), and these two terms interacted with the appropriate explanatory variable. A country-specific year time trend is included in all specifications (coefficient estimates not reported). The country controls are those included in Tables 2 and 3 above: initial log per capita income; Democracy (Polity IV), time t-1; Ethno-linguistic fractionalization; Religious fractionalization; Oil producing country; Log(mountainous); Log (national population), time t-1. Similar interaction patterns hold when (Civil conflict ? 1000 deaths) is the dependent variable, and in most OLS specifications (results not shown).

Table 6: Economic Growth and the Onset of Conflict

	Dependent variable:	
	Onset, Civil conflict ? 25 deaths	Onset, Civil conflict ? 1000 deaths
	IV-2SLS	IV-2SLS
Explanatory variable	(1)	(2)
Economic growth rate, t	-3.32*	-2.70*
	(1.90)	(1.36)
Economic growth rate, t-1	-1.93	-0.76
	(1.51)	(1.20)
Country fixed effects	Yes	Yes
Country-specific time trends	Yes	Yes
R^2	-	-
Root MSE	0.29	0.24
Number of observations	555	625

Table 6 Notes: Huber robust standard errors in parentheses. Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Regression disturbance terms are clustered at the country level. The instrumental variables for economic growth are (Growth in rainfall, t) and (Growth in rainfall, t-1). A country-specific year time trend is included in all specifications (coefficient estimates not reported).

Figure 1: Non-Parametric Fan Regression, Economic Growth Rate on Rainfall Growth (conditional on lagged rainfall, country fixed effects, country-specific time trends)

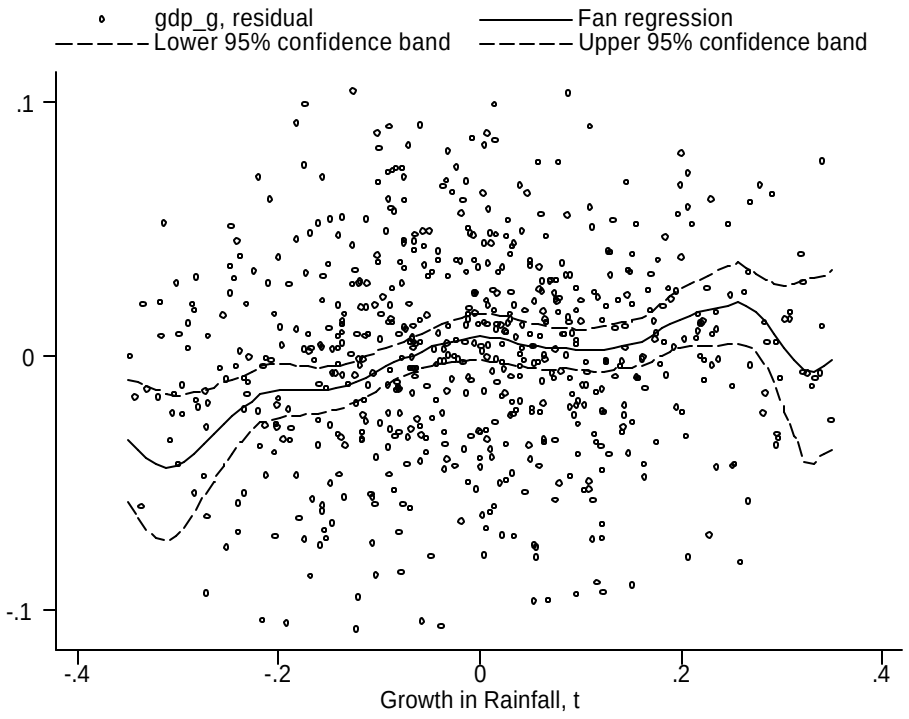
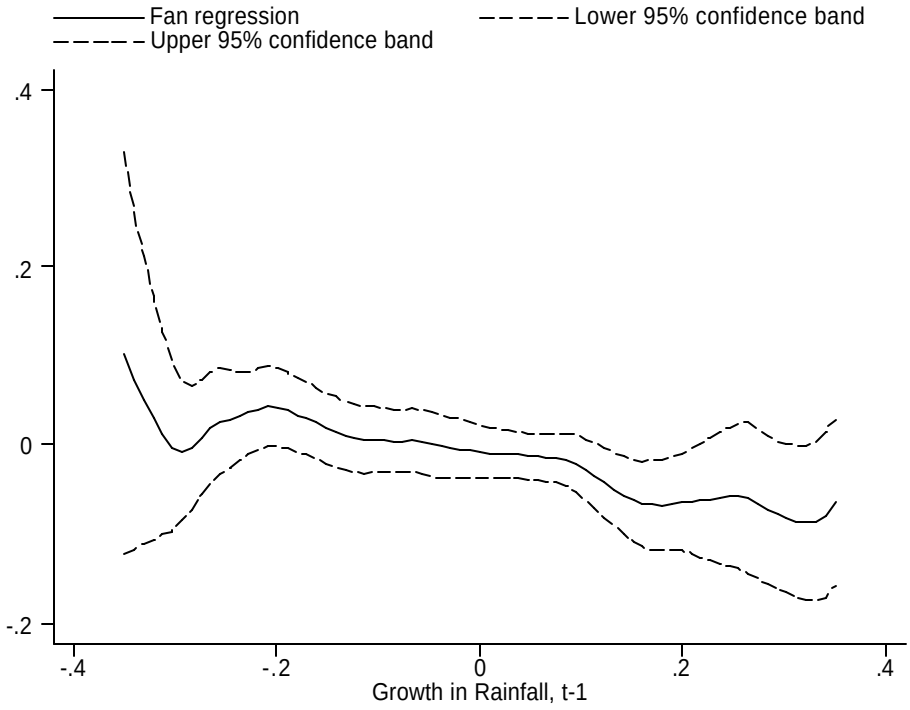


Figure 2: Non-Parametric Fan Regression, Likelihood of Civil Conflict (? 25 battle deaths) on Lagged Rainfall Growth (conditional on current rainfall, country fixed effects and country-specific time trends)



Appendix

Appendix 1: Two Illustrative African Cases

It is impossible to adequately summarize the entire literature on African civil war here, but two brief country examples – Sierra Leone and Niger – illustrate the cross-country findings presented above.

The civil war in Sierra Leone was initiated by rebels in 1991 with the help of troops from neighboring Liberia, in the wake of an adverse rainfall shock (a drop of negative ten percent) in 1990. The case literature indicates that the Sierra Leone war may well have petered out in 1993 after the withdrawal of Liberian troops, but for the rebel Revolutionary United Front's remarkable subsequent success in recruiting local "semi-literate village school drop-outs"[27] to fight. This research indicates that the main attraction of joining the RUF was the freedom to engage in looting to bolster income.[28] These arguments dovetail nicely with the above empirical analysis, since the GPCP data shows that there were five successive years of adverse rainfall shocks in Sierra Leone from 1993 onwards, which made agricultural labor relatively less attractive than joining armed militias for many rural youths.[29]

Similarly, the recent civil war in Niger has commonly been attributed to the increasing poverty of the pastoral Tuaregs, who were hard-hit by droughts in the 1970s and 1980s, which killed many of the cattle that are their livelihood.[30] The possibility of armed conflict during that period was largely defused by the mass emigration of Tuaregs to Algeria and Libya in search of better living conditions; however, the repatriation of thousands of Tuaregs to Niger in the late 1980s, and their dissatisfaction with the government's "Aid to the Repatriated" income compensation scheme – compounded by severe negative rainfall shocks in both 1989 and 1990 – triggered a new round of conflict. Five of seven years after 1990 were also years of falling rainfall, and the civil conflict between the Tuaregs and the state continued to simmer during this period. The conflict finally ended in 1998; note that Niger experienced increasing rainfall in three out of four years between 1998 and 2001, the period when peace finally stuck.[31]

[27] Quoted in Bangura (1997), p. 126.
[28] Ibid. For more on the motivations of fighters, see also Keen (1998) and Abdullah (1997).
[29] Refer to Mamdani (2001) and Newbury (1995) for related accounts on the role of falling agricultural income in driving militia recruitment during the civil war in Rwanda during the early 1990's.
[30] Mekenkamp et al (1998), p. 326.
[31] Note that the relatively small-scale conflict in Niger is not captured in the Correlates of War (COW) database, but is recorded as a conflict in PRIO/Uppsala under the 25 deaths per year definition (see Appendix 2, Table A1).

Appendix 2: Definition of Civil War

We use the new Armed Conflict Database, developed by the International Peace Research Institute of Oslo, Norway and the University of Uppsala, Sweden. In this database an armed conflict is defined as follows. "An *armed conflict* is a contested incompatibility which concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths." The elements of the definition are operationalized as follows:

(1) *Use of armed force*: use of arms in order to promote the parties' general position in the conflict, resulting in deaths.

(1.1) *Arms*: any material means, e.g. manufactured weapons but also sticks, stones, fire, water, etc.

(2) *25 (1000) deaths*: a minimum of 25 (1000) battle-related deaths per year and per incompatibility.

(3) *Party*: a government of a state or any opposition organization or alliance of opposition organizations.
(3.1) *Government*: the party controlling the capital of the state.
(3.2) *Opposition organization*: any non-governmental group of people having announced a name for their group and using armed force.

(4) *State*: a state is
(4.1) an internationally recognized sovereign government controlling a specified territory, *or*
(4.2) an internationally unrecognized government controlling a specified territory whose sovereignty is not disputed by another internationally recognized sovereign government previously controlling the same territory.

(5) *Incompatibility concerning government and/or territory*: the incompatibility, as stated by the parties, must concern government and/or territory.
(5.1) *Incompatibility*: the stated generally incompatible positions.
(5.2) *Incompatibility concerning government*: incompatibility concerning type of political system, the replacement of the central government or the change of its composition.
(5.3) *Incompatibility concerning territory*: incompatibility concerning the status of a territory, e.g. the change of the state in control of a certain territory (interstate conflict), secession or autonomy (intrastate conflict)."

This database includes data on interstate, intrastate, and extrastate conflicts, all in a single list of conflicts. However, we focus exclusively on the categories of conflict which can be classified as being civil wars (Categories 3 and 4, which cover intrastate conflict). The precise number of deaths per conflict-year are not publicly available, unfortunately.

Appendix 3: Additional Tables (*Not intended for publication)

Table A1: List of countries in the sample

Country	Total Years	Years of Civil conflict ? 25 deaths, PRIO/Uppsala	Years of Civil conflict ? 1000 deaths, PRIO/Uppsala
1. Angola	19	19	17
2. Benin	19	0	0
3. Botswana	19	0	0
4. Burkina Faso	19	3	1
5. Burundi	19	8	1
6. Cameroon	19	1	0
7. Central African Republic	19	0	0
8. Chad	19	17	11
9. Republic of Congo (Brazzaville)	19	3	3
10. Democratic Republic of Congo (Kinshasa)	18	12	11
11. Cote d'Ivoire	19	0	0
12. Djibouti	11	1	0
13. Ethiopia	19	15	11
14. Gabon	19	0	0
15. Gambia	19	1	0
16. Ghana	19	2	0
17. Guinea	19	2	1
18. Guinea-Bissau	19	2	1
19. Kenya	19	1	0
20. Lesotho	19	1	0
21. Liberia	11	3	1
22. Madagascar	19	0	0
23. Malawi	19	0	0
24. Mali	19	2	0
25. Mauritania	19	0	0
26. Mozambique	19	12	12
27. Namibia	9	2	2
28. Niger	19	6	0
29. Nigeria	19	0	0
30. Rwanda	19	9	5
31. Senegal	19	7	1
32. Sierra Leone	19	9	2
33. Somalia	11	11	3
34. South Africa	19	13	13
35. Sudan	18	16	14
36. Swaziland	19	0	0
37. Tanzania	19	0	0
38. Togo	19	2	0
39. Uganda	19	17	12
40. Zambia	19	0	0
41. Zimbabwe	19	2	2
TOTAL	743	199	124

Table A1 Notes: The nineteen sample years are 1981-1999. Eritrea and Equatorial Guinea were dropped from the analysis due to missing data. For Djibouti, Liberia, and Somalia, GDP data are missing since 1992. For Sudan and the Democratic Republic of Congo, GDP data are missing for 1999. Namibia became independent in 1990.

Table A2: Results using Other Measures of Weather Variation

	Dependent variable: Civil conflict ? 25 deaths		
	IV-2SLS (IV: GPCP data)	IV-2SLS (IV: NCEP data)	IV-2SLS (IV: FAOCLIM data)
Explanatory variable	(1)	(2)	(3)
Economic growth rate, t	-1.13	-0.00	0.31
	(1.40)	(1.81)	(0.67)
Economic growth rate, t-1	-2.53**	-2.25	-1.31*
	(1.10)	(1.35)	(0.67)
Country fixed effects	Yes	Yes	Yes
Country-specific time trends	Yes	Yes	Yes
R^2	-	-	-
Root MSE	0.32	0.31	0.27
Number of observations	743	743	607

Table A2 Notes: Huber robust standard errors in parentheses. Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Regression disturbance terms are clustered at the country level. Current and lagged growth in rainfall are the instrumental variables for economic growth. A country-specific year time trend is included in all specifications (coefficient estimates not reported). Regression 1 reproduces the result from Table 3, regression 6.

Table A3: Results using Other Measures of Civil Conflict

	Dependent variable:				
	Civil conflict ? 25 deaths (Source: PRIO/Uppsala)	Civil conflict ? 1000 deaths (Source: PRIO/Uppsala)	Civil conflict ? 1000 deaths (Source: CH)	Civil conflict ? 1000 deaths (Source: DS)	Civil conflict ? 1000 deaths (Source: FL)
	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS
Explanatory variable	(1)	(2)	(3)	(4)	(5)
Economic growth rate, t	-1.13	-1.48*	-0.96	-1.62	-0.84
	(1.40)	(0.82)	(0.77)	(1.07)	(0.78)
Economic growth rate, t-1	-2.53**	-0.76	-0.64	-0.95	-0.83***
	(1.10)	(0.70)	(0.56)	(0.68)	(0.30)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Country-specific time trends	Yes	Yes	Yes	Yes	Yes
R^2	-	-	-	-	-
Root MSE	0.32	0.24	0.18	0.24	0.23
Number of observations	743	743	743	724	743

Table A3 Notes: Huber robust standard errors in parentheses. Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. Regression disturbance terms are clustered at the country level. The instrumental variables for economic growth are (Growth in rainfall, t) and (Growth in rainfall, t-1) using the GPCP measures. A country-specific year time trend is included in all specifications (coefficient estimates not reported). The dataset labels correspond to: PRIO/Uppsala = International Peace Research Institute of Oslo and University of Uppsala, Sweden, CH=Collier and Hoeffler, DS=Doyle and Sambanis, FL=Fearon and Laitin. Regression 1 reproduces the result from Table 3, regression 6, and regression 2 reproduces the result from Table 3, regression 7.