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. We find that 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. 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 recently taken by Acemoglu, Johnson, and Robinson (2001), 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

[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.

econometric identification strategy allows us to 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 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 amounts to an

[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.

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]

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 many 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 Peace Research Institute of Oslo, Norway and the University of Uppsala, Sweden (we refer to it as PRIO-UU). The PRIO-UU 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-UU 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-UU unfortunately does not include conflict information at the sub-national level, or by month within each year, nor does it provide the exact number of conflict deaths, and this by necessity limits certain aspects of the empirical analysis.

[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.

[14] Refer to the PRIO website (www.prio.no) or the University of Uppsala website (www.peace.uu.se). A more detailed description of the criteria used to code civil wars is provided in Appendix 1 of this paper.

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-UU database also includes detailed information on conflicts between countries, we focus exclusively on civil wars – the PRIO-UU 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-UU 25 annual battle deaths definition, 17 percent according to the PRIO-UU 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 twenty-seven ended, at least temporarily.

3.2 Rainfall Data

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

[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-UU dataset.
[17] The GPCP data is publicly available on the web at http://orbit-net.nesdis.noaa.gov/arad/gpcp/.

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, ΔR_{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 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

[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.

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-UU 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 (ΔR_{it}, $\Delta R_{i,t-1}$) as instrumental variables for per capita economic growth ($GROWTH_{it}$) in the first stage, controlling for other 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

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

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 in this sample: 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). 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). We experimented with a variety of other instrumental variables – including further 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; as well as measures of terms of trade shocks (which are driven by commodity price movements) – 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 2, regression 4), with a point estimate of –0.126 (standard error 0.051) on lagged rainfall growth, as well as for major conflicts (regression 5), 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 negative 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:

$$(2) \qquad CONFLICT_{it} = \beta_{2i} + X_{it}\beta_2 + \beta_{2,0}GROWTH_{it} + \beta_{2,1}GROWTH_{i,t-1} + \beta_{2i}YEAR_t + \beta_{2it}$$

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 3, 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 3, 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.[22] Using the IV-2SLS method we discover that negative 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.)

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 3, regressions 3 and 4), which casts some doubt on this attenuation bias explanation for the difference between the OLS and

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

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-UU – is large and significant at 90 percent confidence, with a point estimate of –1.48 (standard error 0.82, Table 3, 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 (results 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-UU 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 4, 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.[23] 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.

[23] 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).

We also find that there is no differential impact of economic growth shocks in more or less ethnically diverse countries (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 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-UU definitions, 25 and 1000 battle deaths, conflicts are significantly less likely to start

as economic growth increases (Table 5, 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 3, 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.[24]

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

[24] 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.

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 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).

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.[25]

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[25] 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-UU	0.27	0.44	743
Onset	0.07	0.25	555
Offset	0.15	0.36	188
Civil conflict with ≥ 1000 deaths, PRIO-UU	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

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).