Economic Shocks and Civil Conflict: An Instrumental Variables Approach

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Estimating the impact of economic conditions on the likelihood of civil conflict is difficult because of endogeneity and omitted variable bias. We use rainfall variation as an instrumental variable for economic growth in 41 African countries during 1981–99. Growth is strongly negatively related to civil conflict: a negative growth shock of five percentage points increases the likelihood of conflict by one-half the following year. We attempt to rule out other channels through which rainfall may affect conflict. Surprisingly, the impact of growth shocks on conflict is *not* significantly different in richer, more democratic, or more ethnically diverse countries.

I. Introduction

Civil wars have gained increasing attention from academics and policy makers alike in recent years (see, e.g., World Bank 2003). 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

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many deaths as wars between states since World War II (Fearon and Laitin 2003). A major locus for civil wars in recent years has been sub-Saharan Africa, where 29 of 43 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 (Sambanis 2001). The long-term burden of disease and disability caused by war likely far outweighs the number of deaths during fighting (Ghobarah, Huth, and Russett 2003).

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.[1] Weather shocks are plausible instruments for growth in gross domestic product in economies that largely rely on rain-fed agriculture, that is, 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 a 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 focus on short-term economic fluctuations that "trigger" conflicts, but it is not as well suited for understanding conflict duration.

Sub-Saharan Africa is the ideal region for this identification strategy: the World Development Indicator database indicates that only 1 percent of cropland 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, our identification strategy is inappropriate for other regions of the world, since weather is not sufficiently closely linked

[1] Microeconomic studies that use weather as an instrumental variable for income include Paxson (1992) and Miguel (2003), among many others.

to income growth.[2] Although the 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 (Behrman and Rosenzweig 1994; Heston 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.[3]

The 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 1981–99 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 increase of more than one-half in the likelihood of civil war. 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 the 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 in-

[2] For instance, 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).

[3] Krueger and Lindahl (2000) also use an instrumental variable method to address attenuation bias in cross-country estimates of the returns to education.

come 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.[4]

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 are severely hampered by the absence of reliable panel 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 are available do not suggest that income inequality is systematically correlated with civil conflict (see Collier and Hoeffler 2001; Fearon and Laitin 2003). We also attempt to rule out other noneconomic channels through which rainfall may affect conflict.

In Section II, we provide an overview of the literature on the determinants of civil war. In Section III, we describe our data, and in Section IV, we discuss the estimation strategy. Section V contains the empirical results and further discussion of econometric identification, and Section VI presents a conclusion.

II. 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 the main findings of recent studies.

As we mention above, economic growth may affect civil conflict

[4] An excellent set of summary case studies, from the voluminous literature on African civil wars, may be found in Mekenkamp, van Tongeren, and van de Veen (1999). We briefly discuss two African case studies in App. A.

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[5]—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 enrollment in secondary education, 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 the onset of civil war, 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.

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), and confirm most of Collier and Hoeffler's findings on the role of economic factors. They also find that ethnic fractionalization has a statistically significant quadratic relationship with the incidence of civil war and that democracy reduces the incidence of civil war.[6]

Like the scholars mentioned above, 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 the percentage of the country that is mountainous—in sustaining insurgencies. Fearon and Laitin also find that ethnic diversity does not contribute to the onset of conflict.[7]

These authors are aware of the potential endogeneity problems in estimating the relationship between civil war and economic outcomes, and they attempt to address this by using as explanatory variables lagged

[5] A recent journalistic account of a Liberian warlord resonates with this view: "Essentially Conneh [Sekou Conneh, leader of the main Liberian rebel movement] is a businessman, not a soldier or a politician. … Occasionally he seems to remember that he should say something politically relevant and will make a short impromptu speech about the struggle for democracy and the freedom of the Liberian people" (Brabazon 2003, p. 12).

[6] Hegre et al. (2001) find that regimes with intermediate levels of democracy are most prone to civil conflict.

[7] Easterly and Levine (1997) also find that ethnic diversity is not significantly related to conflict across countries.

values of per capita GDP growth or levels (see Collier and Hoeffler 2002; Fearon and Laitin 2003). 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.[8] The existing analyses may also be prone to omitted variable bias: fast-growing countries may differ from slow-growing countries along many institutional dimensions, some of which are hard to measure, and thus it becomes difficult to pinpoint the true underlying causes of conflict.

III. Data and Measurement

A. *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).[9] Furthermore, the arbitrary 1,000-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.

This paper instead uses the Armed Conflict Data database recently developed by the International Peace Research Institute of Oslo, Norway, and the University of Uppsala, Sweden (referred to 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 1,000-death threshold, thus including more small conflicts in the analysis. An armed conflict is defined in the PRIO/Uppsala database as "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

[8] 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 discussion in the papers.

[9] For instance, it is unclear whether the COW database uses 1,000 cumulative deaths or 1,000 per year when identifying a civil war. Refer to Sarkees, Wayman, and Singer (2003) for another recent discussion of the COW database.

deaths."[10] The database is careful to focus only on politically motivated violence. Note that, like other cross-country civil war data sets, PRIO/Uppsala unfortunately does not include conflict information at the subnational level or by month within each year; nor does it provide the exact number of conflict deaths, which by necessity limits certain aspects of the empirical analysis.

The 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 drug trade in Lagos, Nigeria—which 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 the main 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. (We leave an empirical analysis of the causes of conflicts between countries for future research.)

The civil conflict indicator variable for country i in year t is denoted conflict_{it}. All country-year observations with a civil conflict in progress with at least 25 battle deaths per year (or 1,000 battle deaths in some specifications) are coded as ones, and other observations are coded as zeros. Civil conflict was remarkably widespread in sub-Saharan Africa during the period 1981–99: there was civil conflict in fully 27 percent of all country-year observations according to the PRIO/Uppsala definition of 25 annual battle deaths, 17 percent according to the PRIO/Uppsala 1,000-death definition, and 24 percent under the Fearon and Laitin (2003) definition, using a 1,000-death threshold (panel A of table 1).[11] In addition to conflict incidence, we also examine onset, where onset_{it} is an indicator variable such that $1(\text{conflict}_{it} = 1|\text{conflict}_{i,t-1} = 0)$. Thirty-eight separate conflicts began during the sample period of 1981–99—not including conflicts that were ongoing in 1981—and 27 ended, at least temporarily.

[10] Refer to the PRIO website (http://www.prio.no/cwp/ArmedConflict) or the University of Uppsala website (http://www.pcr.uu.se). A detailed description of the criteria used to code civil wars is provided in Gleditsch et al. (2002) and in App. B of this paper.

[11] Appendix table C1 contains a list of all the countries in our sample and the number of years for which they are coded as having a conflict in the PRIO/Uppsala data set.

TABLE 1
DESCRIPTIVE STATISTICS

	Mean	Standard Deviation	Observations
	A. Civil Conflict Measures (1981–99)		
Civil conflict with ≥25 deaths: (PRIO/Uppsala)	.27	.44	743
Onset	.07	.25	555
Offset	.15	.36	188
Civil conflict with ≥1,000 deaths:			
PRIO/Uppsala	.17	.37	743
Onset	.04	.19	625
Offset	.15	.36	118
Collier and Hoeffler (2002)	.17	.38	743
Doyle and Sambanis (2000)	.22	.41	724
Fearon and Laitin (2003)	.24	.43	743
	B. Rainfall Measures (1981–99)		
Annual rainfall (mm), GPCP measure	1,001.6	501.7	743
Annual growth in rainfall, time t	.018	.209	743
Annual growth in rainfall, time $t-1$	.011	.207	743
	C. Economic Growth		
Annual economic growth rate, time t	−.005	.071	743
Annual economic growth rate, time $t-1$	−.006	.072	743
	D. Country Characteristics		
Log(GDP per capita), 1979	1.16	.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 $t-1$	.15	.36	743
Ethnolinguistic fractionalization (source: *Atlas Marodov Mira*)	.65	.24	743
Religious fractionalization (source: *CIA Factbook*)	.49	.19	743
Oil-exporting country (source: WDI)	.12	.32	743
Log(mountainous) (source: Fearon and Laitin 2003)	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)	−.01	.16	661

NOTE.—The source of most characteristics in panel D is the World Bank's World Development Indicators (WDI). Initial log per capita income for Namibia pertains to 1990, its first year in the sample (after independence).

B. 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.[12] The GPCP data rely on a combination

[12] The GPCP data are publicly available on the Web at http://precip.gsfc.nasa.gov/.

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 data set 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 data sets that at the same time includes both gauge and satellite data, corrects for systematic errors in gauge measures, and rejects gauge measures thought to be unreliable (Rudolf 2000).[13] In any case, the correlation among these three series is high (over .8), and we find similar empirical results with the alternative data sets, as discussed below.[14]

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 2.5-degree intervals.[15] In this data set, Kenya, a medium-sized African country, contains eight rainfall data "nodes," whereas the largest country, Sudan, contains 34 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 as R_{it}. The 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}. Various alternative measures of rainfall variation were examined—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 rainfall in the sample (panel B of table 1), and this holds both across countries and through time for the same country; note that variability in rainfall is greater in sub-Saharan Africa than in tropical regions of Asia or the Americas (Bloom and Sachs 1998, p. 222).

[13] The FAOCLIM uses only gauge data, which considerably limits its coverage; the NCEP does not reject unreliable data sources or correct for systematic gauge errors. The FAOCLIM weather gauge coverage also becomes increasingly limited after 1990, and especially after 1996, leading to missing observations.

[14] In an earlier version of this paper, we also employed data from the Normalized Difference Vegetation Index, which captures the density of plant life—and is closely related to rainfall in Africa, at a correlation of .9—as an alternative measure of weather variation. 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.

[15] 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 node nearest to their borders.

C. Other Country Characteristics

The remaining data are drawn mainly from Fearon and Laitin[16] (2003) and from World Bank databases; 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's article. 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 data set); 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); and oil exporters, measured by an indicator for countries in which oil constitutes more than one-third of export revenues, based on World Bank data (panels C and D of table 1). Measures of income inequality, poverty, or unemployment rates are not included as additional explanatory variables because of the large number of missing or unreliable African observations in existing macroeconomic series.

IV. Estimation Framework

We focus principally on the incidence of civil war in country i in year t (conflict_{it}) according to the PRIO/Uppsala database, but we 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. Weather variation, as captured in current and lagged rainfall growth (ΔR_{it} and $\Delta R_{i,t-1}$), is used to instrument for per capita economic growth (growth_{it}) in the first stage, with other country characteristics (X_{it}) controlled for. Results are broadly similar—although somewhat weaker—if current and lagged deviations from the average country rainfall level, or current and lagged rainfall levels, are used as instrumental variables for growth instead (results not shown). Country fixed effects (a_i) are included in some specifications to capture time-invariant country characteristics that may be related to civil conflict, and we also include country-specific time trends in most specifications to capture additional variation:

$$\text{growth}_{it} = a_{1i} + X'_{it}b_1 + c_{1,0}\Delta R_{it} + c_{1,1}\Delta R_{i,t-1} + d_{1i}\text{year}_t + e_{1it}. \quad (1)$$

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

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

TABLE 2
RAINFALL AND ECONOMIC GROWTH (First-Stage)
Dependent Variable: Economic Growth Rate, t

Explanatory Variable	Ordinary Least Squares				
	(1)	(2)	(3)	(4)	(5)
Growth in rainfall, t	.055***	.053***	.049***	.049***	.053***
	(.016)	(.017)	(.017)	(.018)	(.018)
Growth in rainfall, $t-1$	.034**	.032**	.028**	.028*	.037**
	(.013)	(.014)	(.014)	(.014)	(.015)
Growth in rainfall, $t+1$				.001	
				(.019)	
Growth in terms of trade, t					−.002
					(.023)
Log(GDP per capita), 1979		−.011			
		(.007)			
Democracy (Polity IV), $t-1$		.0000			
		(.0007)			
Ethnolinguistic fractionalization		.006			
		(.044)			
Religious fractionalization		.045			
		(.044)			
Oil-exporting country		.007			
		(.019)			
Log(mountainous)		.001			
		(.005)			
Log(national population), $t-1$		−.009			
		(.009)			
Country fixed effects	no	no	yes	yes	yes
Country-specific time trends	no	yes	yes	yes	yes
R^2	.02	.08	.13	.13	.16
Root mean square error	.07	.07	.07	.07	.06
Observations	743	743	743	743	661

NOTE.—Huber robust standard errors are in parentheses. Regression disturbance terms are clustered at the country level. A country-specific year time trend is included in all specifications (coefficient estimates not reported).
* Significantly different from zero at 90 percent confidence.
** Significantly different from zero at 95 percent confidence.
*** Significantly different from zero at 99 percent confidence.

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 (regression 1 in table 2), 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 semiarid tropics and is prone to drought. The rainfall instruments are somewhat weak (the F-statistic is 4.5 in regression 3), suggesting that the instrumental variable two-stage least squares (IV-2SLS) estimates may be somewhat biased toward ordinary least squares (OLS) estimates (Bound, Jaeger, and Baker 1995;

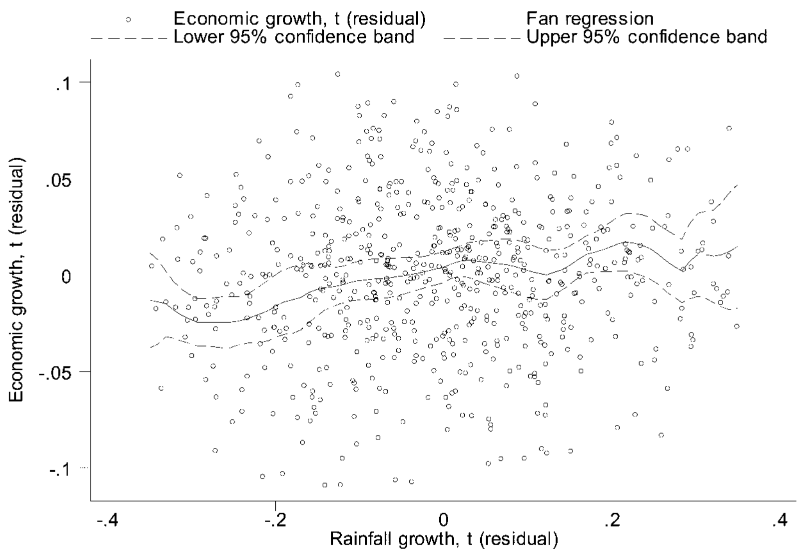

FIG. 1.—Current economic growth rate on current rainfall growth. Nonparametric Fan regression, conditional on country fixed effects and country-specific time trends.

Staiger and Stock 1997). As an identification check, we estimate a "false experiment" specification in which future rainfall growth—which should be orthogonal to current economic growth, conditional on country-specific time trends—is included as an additional explanatory variable, and we find that the coefficient estimate is indeed near zero (coefficient estimate 0.001, standard error 0.019, in regression 4).[17] Changes in country terms of trade, which are largely driven by commodity price movements, are not significantly related to economic growth (regression 5).

The positive and approximately linear first-stage relationship is presented graphically in figure 1, using a nonparametric Fan local regression method with an Epanechnikov kernel. 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 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 agricultural sector value added in national GDP, and the interaction of rainfall growth with the rural share of the national population. In the latter two cases, the coefficient estimates on the interaction terms are positive as expected and sometimes marginally statistically significant (regressions not shown). However, the first-stage results in these cases

[17] We thank Guido Imbens for this suggestion.

TABLE 3
RAINFALL AND CIVIL CONFLICT (Reduced-Form)

	DEPENDENT VARIABLE	
EXPLANATORY VARIABLE	Civil Conflict ≥25 Deaths (OLS) (1)	Civil Conflict ≥1,000 Deaths (OLS) (2)
Growth in rainfall, t	−.024	−.062**
	(.043)	(.030)
Growth in rainfall, $t-1$	−.122**	−.069**
	(.052)	(.032)
Country fixed effects	yes	yes
Country-specific time trends	yes	yes
R^2	.71	.70
Root mean square error	.25	.22
Observations	743	743

NOTE.—Huber robust standard errors are in parentheses. Regression disturbance terms are clustered at the country level. A country-specific year time trend is included in all specifications (coefficient estimates not reported).
* Significantly different from zero at 90 percent confidence.
** Significantly different from zero at 95 percent confidence.
*** Significantly different from zero at 99 percent confidence.

are weaker than the specifications presented in table 2 (results not shown), so we opt for the more parsimonious specification above.

Higher levels of rainfall are associated with significantly *less* conflict in the reduced-form regression, for all civil conflicts (regression 1 in table 3), with a point estimate of −0.122 (standard error 0.052) on lagged rainfall growth. In the case of major conflicts, those involving more than 1,000 deaths per year (regression 2), coefficient estimates on both current and lagged rainfall growth are statistically significant at 95 percent confidence. This is the first indication that better rainfall makes civil conflict less likely in Africa.[18] The nonparametric relationship between lagged rainfall growth and conflict is negative and roughly linear (fig. 2).

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

$$\text{conflict}_{it} = \alpha_{2i} + X'_{it}\beta_2 + \gamma_{2,0}\text{growth}_{it} + \gamma_{2,1}\text{growth}_{i,t-1} + \delta_{2i}\text{year}_t + \epsilon_{2it}. \quad (2)$$

We performed both IV-2SLS estimation and a nonlinear two-stage procedure following Achen (1986) to correct standard errors in the

[18] The false experiment specifications again indicate that future rainfall is not statistically significantly related to either measure of current conflict: for the 25–battle death threshold, the coefficient estimate on rainfall in period $t+1$ is near zero, at 0.000, with a standard error of 0.055 (regression not shown).

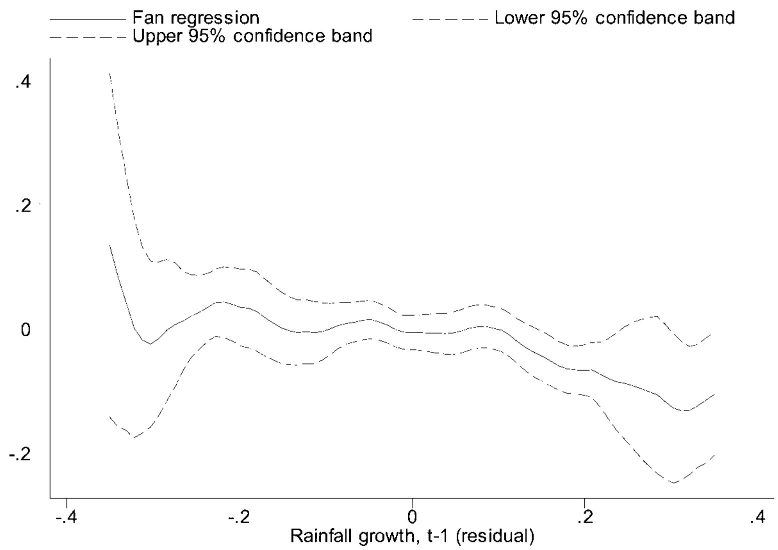

FIG. 2.—Current likelihood of civil conflict (≥25 battle deaths) on lagged rainfall growth. Nonparametric Fan regression, conditional on current rainfall growth, country fixed effects, and country-specific time trends.

presence of a dichotomous dependent variable in the second stage. The IV-2SLS method is typically preferred even in cases in which the dependent variable is dichotomous (see Angrist and Kreuger 2001; Wooldridge 2002) since strong specification assumptions are required to justify the Achen and related Rivers and Vuong (1988) methods. We thus focus on the IV-2SLS specification below. Note that results are similar with both specifications, although statistical significance falls somewhat in the nonlinear second-stage specification with bootstrapped standard errors (results not shown).

V. Main Empirical Results

Contemporaneous and lagged economic growth rates are negatively, though not statistically significantly, correlated with the incidence of civil conflict in probit (regression 1 in table 4) 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 linear specifications. Note that among the other variables prominently cited in the existing literature, only the measure of mountainous terrain has statistically significant predictive power in

TABLE 4
Economic Growth and Civil Conflict

	Dependent Variable: Civil Conflict ≥25 Deaths						Dependent Variable: Civil Conflict ≥1,000 Deaths
Explanatory Variable	Probit (1)	OLS (2)	OLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)	IV-2SLS (7)
Economic growth rate, t	−.37 (.26)	−.33 (.26)	−.21 (.20)	−.21 (.16)	−.41 (1.48)	−1.13 (1.40)	−1.48* (.82)
Economic growth rate, $t-1$	−.14 (.23)	−.08 (.24)	.01 (.20)	.07 (.16)	−2.25** (1.07)	−2.55** (1.10)	−.77 (.70)
Log(GDP per capita), 1979	−.067 (.061)	−.041 (.050)	.085 (.084)		.053 (.098)		
Democracy (Polity IV), $t-1$	.001 (.005)	.001 (.005)	.003 (.006)		.004 (.006)		
Ethnolinguistic fractionalization	.24 (.26)	.23 (.27)	.51 (.40)		.51 (.39)		
Religious fractionalization	−.29 (.26)	−.24 (.24)	.10 (.42)		.22 (.44)		
Oil-exporting country	.02 (.21)	.05 (.21)	−.16 (.20)		−.10 (.22)		
Log(mountainous)	.077** (.041)	.076* (.039)	.057 (.060)		.060 (.058)		
Log(national population), $t-1$	.080 (.051)	.068 (.051)	.182* (.086)		.159* (.093)		
Country fixed effects	no	no	no	yes	no	yes	yes
Country-specific time trends	no	no	yes	yes	yes	yes	yes
R^2	…	.13	.53	.71	…	…	…
Root mean square error	…	.42	.31	.25	.36	.32	.24
Observations	743	743	743	743	743	743	743

Note.—Huber robust standard errors are in parentheses. 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 regressions 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 regressions 1 and 2, where a single linear time trend is included.
* Significantly different from zero at 90 percent confidence.
** Significantly different from zero at 95 percent confidence.
*** Significantly different from zero at 99 percent confidence.

these specifications, and national population is also marginally positively associated with conflict in one specification. These results confirm Fearon and Laitin's (2003) finding 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 −2.25 (standard error 1.07) on lagged growth, which is significant at 95 percent confidence, and −0.41 (standard error 1.48) on current growth (regression 5 of table 4). The two growth terms are jointly significant at nearly 90 percent confidence (p-value .12). The IV-2SLS fixed-effects estimate on lagged growth is similarly large, negative, and significant at −2.55 (standard error 1.10 in 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.[19] 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, whereas a range of other political, social, and geographic variables have, at best, a tenuous impact.[20] Using the IV-2SLS method, we discover that economic shocks have an even more dramatic impact on the incidence of civil war than has been previously recognized.[21]

The size of the estimated impact of lagged economic growth on conflict is huge: when we focus 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. Thus a five-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 (regressions 3 and 4 of table 4), which casts some doubt on this attenuation bias explanation for the difference between the OLS and instrumental variable 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 (Behrman and Rosenzweig 1994; Heston 1994).[22]

[19] Coefficient estimates on current and lagged economic growth are slightly stronger in a specification with country fixed effects but not country-specific time trends, at -1.35 (standard error 1.36) and -2.55 (standard error 0.96), respectively (regression not shown). If only lagged economic growth is included as the endogenous variable—instrumented with lagged rainfall growth—the coefficient estimate is -2.73 (standard error 1.38; regression not shown). Coefficient estimates remain negative, but are not always statistically significant, when rainfall levels—rather than rainfall growth—are used as instruments for income growth (regressions not shown).

[20] Economic growth shocks do not cause changes in the extent of democracy in this sample (results not shown).

[21] Of course, this finding does not imply that the reverse relationship—i.e., civil conflict may also reduce economic growth—is not present as well. The identification strategy, however, does not allow us to shed light on this issue.

[22] An alternative explanation for the divergence between OLS and instrumental variable estimates is that the first stage systematically underestimates the impact of rainfall shocks on economic growth—because of extensive nonclassical measurement error in national

In the preferred IV-2SLS fixed-effects framework, the impact of current economic growth on *major* civil conflicts—those with at least 1,000 deaths, according to PRIO/Uppsala—is large and significant at 90 percent confidence, with a point estimate of −1.48 (standard error 0.82 in regression 7 of table 4), 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 five 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 −0.77, standard error 0.70). Thus lagged growth appears more important than current growth in generating conflicts at the 25-death threshold, whereas current growth has a larger estimated effect on the incidence of conflicts at the 1,000-death threshold. Note that this is not driven by differences in the timing of the onset of conflict, as shown below in table 6. 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.[23]

We next perform additional robustness checks and explore 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 94 percent confidence levels in all regressions. Negative economic growth shocks do not have a statistically significant differential impact on civil conflict than positive shocks (regression not shown). Results are similarly large and statistically significant in an ordered probit specification, with the categories no conflict (fewer than 25 battle deaths per year), minor conflicts (between 25 and 1,000 deaths), and major conflicts (more than 1,000 deaths) (results not shown).

The impact of lagged growth on civil conflict is large and negative for alternative measures of rainfall (App. table C2), both for the NCEP (estimate −2.26, standard error 1.36) and FAOCLIM (−1.35, standard error 0.75) data sets. The results are also similar to those with the databases used by Collier and Hoeffler (regression 3 in App. table C3), Doyle and Sambanis (regression 4), and Fearon and Laitin (regression

accounts data—leading instrumental variable estimates to exaggerate the impact of economic growth on conflict. We thank Guido Imbens for this point.

[23] The results are similar in a specification that estimates the impact of *income levels* on the incidence of conflict in a fixed-effects framework, using *rainfall levels* as instrumental variables. In the case of the 1,000-death threshold, the IV-2SLS estimate on contemporaneous income is −2.23 (standard error 1.17; regression not shown); point estimates remain large and negative, though not statistically significant at traditional confidence levels, when an indicator for at least 25 battle deaths per year is the dependent variable (not shown).

5), although statistical significance tends to be somewhat lower with the 1,000-death threshold used in these data sets than with the 25-death PRIO/Uppsala threshold. Still, all 10 coefficient estimates on economic growth in table C3 are negative with *t*-statistics greater than one, and several are significantly different from zero at high levels of confidence.

In the 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 strong 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, whereas such negotiations may more often break down in ethnically or religiously fragmented societies (Benhabib and Rustichini 1996; Easterly and Levine 1997). However, the interactions between economic growth (current and lagged) and a measure of democracy (regression 1 of table 5) 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 nondemocratic 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 there is generally sufficient statistical power to rule out moderate-sized effects. There are similarly weak results for interactions with various alternative democracy measures, including measures that classify countries as democracies if they attain certain Polity IV democracy score thresholds (results not shown).

Economic growth shocks do not have a differential impact in more ethnically diverse countries (regression 3 of table 5, although in this case the coefficient estimates on the interaction terms are imprecisely estimated, complicating inference), 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 former colonies of other countries; by African subregion (Central, East, Southern, and West

[24] These results also hold if conflict at the higher 1,000-death threshold is the dependent variable or in a reduced-form specification in which country characteristics are interacted with rainfall shocks (results not shown).

TABLE 5
INTERACTIONS BETWEEN ECONOMIC GROWTH AND COUNTRY CHARACTERISTICS
Dependent Variable: Civil Conflict ≥25 Deaths

	IV-2SLS				
EXPLANATORY VARIABLE	(1)	(2)	(3)	(4)	(5)
Economic growth rate, t	−1.20	.92	−9.9	−.99	−1.85
	(1.43)	(2.62)	(22.9)	(1.26)	(1.81)
Economic growth rate, $t-1$	−2.86*	−3.01*	−6.4	−2.37**	−2.97**
	(1.46)	(1.70)	(6.1)	(1.04)	(1.39)
Economic growth rate, $t\times$ democracy (Polity IV), $t-1$	.01				
	(.21)				
Economic growth rate, $t-1\times$ democracy (Polity IV), $t-1$	−.10				
	(.16)				
Economic growth rate, $t\times$ log(per capita income, 1979)		−1.98			
		(2.70)			
Economic growth rate, $t-1\times$ log(per capita income, 1979)		.58			
		(1.09)			
Economic growth rate, $t\times$ ethnolinguistic fractionalization			12.1		
			(30.1)		
Economic growth rate, $t-1\times$ ethnolinguistic fractionalization			5.1		
			(8.1)		
Economic growth rate, $t\times$ oil-exporting country				−2.8	
				(6.9)	
Economic growth rate, $t-1\times$ oil-exporting country				3.2	
				(3.1)	
Economic growth rate, $t\times$ log(mountainous)					.39
					(.83)
Economic growth rate, $t-1\times$ log(mountainous)					.23
					(.62)
Country fixed effects	yes	yes	yes	yes	yes
Country-specific time trends	yes	yes	yes	yes	yes
Root mean square error	.33	.34	.41	.32	.32
Observations	743	743	743	743	743

NOTE.—Huber robust standard errors are in parentheses. 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). Similar interaction patterns hold when civil conflict ≥1,000 deaths is the dependent variable and in most OLS specifications (results not shown).

* Significantly different from zero at 90 percent confidence.
** Significantly different from zero at 95 percent confidence.
*** Significantly different from zero at 99 percent confidence.

Africa); for countries with socialist political regimes at the start of the sample period (from Barro [1991]); by religious fractionalization, or any of the social fractionalization measures from Alesina et al. (2003); by population density; across a range of measures of democracy, political competition, regulation of political participation, and constitutional constraints on executive power (from the Polity IV data set); for other political institutional measures, including the degree of federalism, and government checks and balances (from the World Bank Database of Political Institutions); and for political and civil freedom (from Freedom House; results not shown).

The simplest reading of these findings is that economic factors trump

TABLE 6
Economic Growth and Conflict Onset

	Dependent Variable	
Explanatory Variable	Onset, Civil Conflict ≥25 Deaths (IV-2SLS) (1)	Onset, Civil Conflict ≥1,000 Deaths (IV-2SLS) (2)
Economic growth rate, t	−3.15*	−2.85*
	(1.87)	(1.45)
Economic growth rate, $t-1$	−1.84	−.80
	(1.48)	(1.25)
Country fixed effects	yes	yes
Country-specific time trends	yes	yes
Root mean square error	.28	.24
Observations	555	625

Note.—Huber robust standard errors are in parentheses. 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).
* Significantly different from zero at 90 percent confidence.
** Significantly different from zero at 95 percent confidence.
*** Significantly different from zero at 99 percent confidence.

all others in determining the incidence of civil conflict and, in particular, that institutional and social characteristics have minimal impact in mitigating the effect 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 legacies—means that this finding may not generalize to other regions of the world. Despite attempts to examine the broadest possible range of country political and social characteristics, it also remains possible that some other characteristics not adequately captured in existing data sets—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 we are unable to examine them here. Moreover, problems of measurement for the institutional and social characteristics exacerbate these concerns and may bias coefficient estimates on the interaction terms toward zero.

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. When either PRIO/Uppsala definition is used, 25 or 1,000 battle deaths, conflicts are significantly less likely to start as economic growth increases (regressions 1 and 2 of table 6), 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 once again 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 estimates are not significantly different from zero at traditional confidence levels. The sharp drop in sample size in these offset regressions is likely to partially account for this lack of statistical precision.[25]

A. *Potential Violations of the Exclusion Restriction*

While it is intuitively plausible that the rainfall instruments are exogenous, they must also satisfy the exclusion restriction: weather shocks should affect civil conflict only 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; unfortunately we do not have reliable cross-country data on these other intermediate channels. There are, however, central government budget figures for approximately half of our sample period from the World Bank, and we 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. Thus to the extent that the hypothesized bias exists, our estimates would be lower bounds on the true impact of economic growth on civil conflict.

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,

[25] In fixed-effects instrumental variable specifications in which lagged conflict is included as an explanatory variable and is also interacted with the economic growth shocks, these interaction terms are not statistically significant, but the coefficient estimate on lagged growth remains similar (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 bias fixed-effects estimates, and hence we do not emphasize these results.

because of more difficult transportation conditions. To explore this possibility, we estimated the impact of rainfall shocks on the extent of the usable road network using World Bank data, and we 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 1,025; regression not shown), which argues against the theory above. 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 impact working through economic growth, though we believe that these other effects are likely to be minor.

VI. Conclusion

This paper addresses a major methodological problem that lies at the core of the cross-country empirical literature on civil wars, the potential endogeneity of economic factors used as explanatory variables. Using rainfall shocks as instrumental variables for economic growth, we find that growth shocks have a dramatic causal impact on the likelihood of civil war: a five-percentage-point negative growth shock increases the likelihood of a civil war the following year by nearly one-half. In the 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 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 because of limited government institutional capacity). If such public work schemes focused on building roads—facilitating the transport of food—as well as on irrigation and other water projects, they could also serve to somewhat reduce local vulnerability to future rainfall shocks. We offer these prescriptions with the cautionary note that—given the inherent limitations of cross-country research—further micro-empirical analysis and careful case studies are urgently needed to illuminate precise causal channels and to design more effective policy responses.[26]

Appendix A

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—help make the cross-country findings more concrete.

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 10 percent drop) 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" (Bangura 1997, p. 126) to fight. This research indicates that the main attraction of joining the front was the freedom to engage in looting to bolster income.[27] These arguments dovetail nicely with the empirical analysis above since the GPCP data show that there were five successive years of adverse rainfall realizations in Sierra Leone from 1993 onward, which presumably made agricultural labor relatively less attractive than joining armed militias for many rural youths.[28]

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 (Mekenkamp et al. 1999, p. 326). 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 in which peace finally stuck. (Note that the relatively small-scale conflict in Niger

[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 microeconomic data on economic conditions and individual decisions in wartime situations complicates this endeavor.

[27] For more on the motivations of fighters, see also Abdullah (1997) and Keen (1998).

[28] Refer to Newbury (1995) and Mamdani (2001) for related accounts on the role of falling agricultural income in driving militia recruitment during the civil war in Rwanda during the early 1990s.

is not captured in the COW database but is recorded as a conflict in PRIO/Uppsala under the definition of 25 deaths per year; see App. table C1.)

Appendix B

Definition of Civil Conflict

The Armed Conflict Database was 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 nongovernmental 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 that can be classified as being civil wars (categories 3 and 4, which cover intrastate conflict). The precise number of deaths per conflict-year is not publicly available, unfortunately.

Appendix C

Additional Tables

TABLE C1
List of Countries in the Sample

Country	Total Years	Years of Civil Conflict ≥25 Deaths (PRIO/Uppsala)	Years of Civil Conflict ≥1,000 Deaths (PRIO/Uppsala)
Angola	19	19	17
Benin	19	0	0
Botswana	19	0	0
Burkina Faso	19	3	1
Burundi	19	8	1
Cameroon	19	1	0
Central African Republic	19	0	0
Chad	19	17	11
Republic of Congo (Brazzaville)	19	3	3
Democratic Republic of Congo (Kinshasa)	18	12	11
Côte d'Ivoire	19	0	0
Djibouti	11	1	0
Ethiopia	19	15	11
Gabon	19	0	0
Gambia	19	1	0
Ghana	19	2	0
Guinea	19	2	1
Guinea-Bissau	19	2	1
Kenya	19	1	0
Lesotho	19	1	0
Liberia	11	3	1
Madagascar	19	0	0
Malawi	19	0	0
Mali	19	2	0
Mauritania	19	0	0
Mozambique	19	12	12
Namibia	9	2	2
Niger	19	6	0
Nigeria	19	0	0
Rwanda	19	9	5
Senegal	19	7	1
Sierra Leone	19	9	2
Somalia	11	11	3
South Africa	19	13	13
Sudan	18	16	14
Swaziland	19	0	0
Tanzania	19	0	0
Togo	19	2	0
Uganda	19	17	12
Zambia	19	0	0
Zimbabwe	19	2	2
Total	743	199	124

Note.—The 19 sample years are 1981–99. Eritrea and Equatorial Guinea were dropped from the analysis because of 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 C2
RESULTS USING OTHER RAINFALL MEASURES
Dependent Variable: Civil Conflict ≥25 Deaths

Explanatory Variable	IV-2SLS: IV: GPCP Data (1)	IV-2SLS: IV: NCEP Data (2)	IV-2SLS: IV: FAOCLIM Data (3)
Economic growth rate, t	−1.13	.02	.45
	(1.40)	(1.82)	(.68)
Economic growth rate, $t-1$	−2.55**	−2.26	−1.35*
	(1.10)	(1.36)	(.75)
Country fixed effects	yes	yes	yes
Country-specific time trends	yes	yes	yes
R^2	…	…	…
Root mean square error	.32	.31	.27
Observations	743	743	607

NOTE.—Huber robust standard errors are in parentheses. Regression disturbance terms are clustered at the country level. The instrumental variables for economic growth are growth in rainfall, t and growth in rainfall, $t-1$. 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 regression 6 of table 4.
* Significantly different from zero at 90 percent confidence.
** Significantly different from zero at 95 percent confidence.
*** Significantly different from zero at 99 percent confidence.

TABLE C3
RESULTS USING OTHER CIVIL CONFLICT MEASURES: IV-2SLS

Explanatory Variable	Dependent Variable: Civil Conflict ≥25 Deaths (1)	Dependent Variable: Civil Conflict ≥ 1,000 Deaths (2)	(3)	(4)	(5)
Economic growth rate, t	−1.13	−1.48*	−.96	−1.62	−.84
	(1.40)	(.82)	(.77)	(1.07)	(.78)
Economic growth rate, $t-1$	−2.55**	−.77	−.65	−.96	−.84***
	(1.10)	(.70)	(.56)	(.68)	(.30)
Country fixed effects	yes	yes	yes	yes	yes
Country-specific time trends	yes	yes	yes	yes	yes
Root mean square error	.32	.24	.17	.24	.23
Observations	743	743	743	724	743

SOURCE.—Cols. 1 and 2: PRIO/Uppsala; col. 3: Collier and Hoeffler (2002); col. 4: Doyle and Sambanis (2000); col. 5: Fearon and Laitin (2003).
NOTE.—Huber robust standard errors are in parentheses. Regression disturbance terms are clustered at the country level. The instrumental variables for economic growth are growth in rainfall, t and growth in rainfall, $t-1$ with the GPCP measures. A country-specific year time trend is included in all specifications (coefficient estimates not reported). Regression 1 reproduces the result from regression 6 of table 4, and regression 2 reproduces the result from regression 7 of table 4.
* Significantly different from zero at 90 percent confidence.
** Significantly different from zero at 95 percent confidence.
*** Significantly different from zero at 99 percent confidence.

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