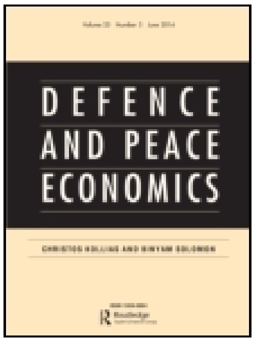

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RAIN, GROWTH, AND CIVIL WAR: THE IMPORTANCE OF LOCATION

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We re-examine the Miguel *et al.* (2004) study of the impact of growth on civil war, using growth in rainfall as an instrument. Miguel *et al.* (2004) – in our view, erroneously – include countries participating in civil wars in other states. Restricting the conflict data to states with conflict on their own territory reduces the estimated impact of economic growth on civil war. We show how spatial correlations in rainfall growth and participation in civil conflicts induce a stronger apparent relationship in the mis-classified data.

Keywords: Civil war; Conflict; Growth; Rainfall; Location; Spatial econometrics

JEL Codes: D74, O0, C2

INTRODUCTION

In an important effort to address the potential endogeneity between economic growth and conflict, Miguel *et al.* (2004) estimate the partial effect of growth on conflict in Sub-Saharan Africa using rainfall as an instrument. Many contributions to theories of domestic conflict have postulated that changing economic conditions are likely to strongly influence resort to violence. Davies (1962) argued that conflict was particularly likely to occur during periods of economic setbacks, where the gap between the aspirations of individuals based on further economic growth and the actual economic difficulties experienced would be perceived as particularly severe. More recently, Collier and Hoeffler (2004) have argued that rebel recruitment will be easier in poor societies due to lower opportunity costs in terms of foregone income from regular economic activities when participating in insurgencies, as well as lower wages for combatants. They use economic growth as one of their proxies for foregone income, anticipating that conflict episodes will be preceded by low growth. Finally, Fearon and Laitin (2003) argue that civil wars are more common in poor societies since states in low-income societies tend to have weaker capacity for deterring and defeating violent insurgencies. From this perspective, low or negative economic growth could be seen as an indicator of weakening state capacity, which may increase the risk of rebellion. Many empirical studies of civil war

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have found evidence for a negative relationship between conflict and economic growth (see, in particular, Collier and Hoeffler, 2004; Hegre and Sambanis, 2006).

However, it is clearly problematic to directly use observed data on economic growth for an estimate of the causal effect of growth on conflict, since economic growth itself may be affected by ongoing conflict or anticipated violence. Indeed, numerous empirical studies have confirmed the negative effects of conflict on economic growth (see, e.g., reviews in Collier *et al*., 2003). Murdoch and Sandler (2002, see also Murdoch and Sandler, 2004) suggest a number of ways by which conflict may influence economic growth within a Solow growth model (see Mankiw *et al*., 1992). These include the effects of conflict on labour (e.g. through substitution into soldiering and deaths from conflict), capital (e.g. destruction of capital stock, diverting savings into unproductive activities, and deterring investment), as well as possible unmeasured effects such as the destruction of social capital and trust. The many theoretical arguments and empirical studies suggesting an impact of conflict on economic growth attest to how the potential problems of endogeneity may be very severe when assessing the effects of economic growth on conflict.

Miguel *et al*. (2004) argue that weather shocks, or abrupt declines in rainfall per capita, provide plausible instruments for economic growth in economies that depend primarily on rain-fed agriculture, as is the case in most of Sub-Saharan Africa. Their empirical study suggests a robust negative relationship between economic growth instrumented by rainfall and conflict incidence in Sub-Saharan Africa. Their study has generated a great deal of interest; As of October 2008 their paper has 48 recorded citations in the Social Science Citation Index, which is quite impressive for a paper only appearing in print in 2004.

However, despite the innovative instrument for economic growth, we believe that Miguel *et al*. (2004) use a problematic coding of conflict by including states that participate in civil wars in other countries. We expand below on why we think their measure is problematic and why we believe that estimates of the effects of growth of conflict should be limited to conflict on a state's territory. Moreover, we argue that including external participation can give misleading estimates of the impact of growth shocks on conflict. Since droughts tend to jointly affect neighbouring countries and countries primarily intervene in neighbouring civil conflicts, common geographical clustering in rainfall and war participation may overstate the causal effect. Our re-analyses of Miguel *et al*.'s (2004) proposed model using conflict data restricted to location indicate a smaller impact and much less robust evidence for a negative impact of growth on the incidence of civil conflict, and we demonstrate the effects of spatial correlation on their original estimates.

THE IMPORTANCE OF LOCATION IN LINKING GROWTH AND CONFLICT

Miguel *et al*. (2004) use the Uppsala/PRIO Armed Conflict Data (ACD) to identify civil conflicts with at least 25 battle-related deaths per year as well as wars with more than 1000 battle-deaths (see Gleditsch *et al*., 2002). In brief, Miguel *et al*. (2004) find a significant effect of lagged economic growth on conflict incidence with the low conflict threshold at the 5% level, and a negative coefficient for current economic growth on major civil conflicts, significant at the 10% level.

However, upon closer inspection of their data it becomes clear that Miguel *et al*. (2004) include not just African countries that see civil wars themselves, but also countries that send troops to civil wars in other states, even when these do not experience conflict on their own territory. We believe that it is inappropriate to include participation in civil wars in other states to evaluate the effects of growth on conflict. Miguel *et al*. (2004) do not develop new theoretical propositions relating growth shocks to civil war. However, the main causal arguments invoked from previous research seem to us to apply to conflict on a country's territory and specifically highlight the effects on potential insurgents. Collier and Hoeffler (2004), for example, emphasize how poor economic performance can lower the opportunity costs for potential insurgents from participating in insurgencies rather than regular economic activities, and how low growth will make it easier to mobilize rebellion against the state. By contrast, sending troops to a civil war in another country is a choice by the government, which can rely on forced conscription irrespective of economic conditions. Moreover, it could be argued that the logic in Fearon and Laitin (2003), who stress the role of state strength in deterring insurgencies and how this declines with lower income, would suggest that lower growth should decrease the capacity of governments to commit troops to civil wars in other states and make external participation in conflict less likely. None of the previous studies of economic growth and civil conflict discussed in Miguel *et al*. (2004) examine government participation in civil wars in other states.[1]

The case of Zimbabwe illustrates the difference between considering civil war locations and participation in civil wars elsewhere. Although Zimbabwe has been affected by a series of droughts since 1991 and sent troops to the war in the Democratic Republic Congo in 1998–99, it did not experience violence on its own territory during this period. Rather than insurgents taking up arms against the government under worsening economic conditions it is actually the Zimbabwean government that forcefully drafts individuals to fight in a civil war in another state.

In a response to previous discussions, Miguel *et al*. (2007) now argue that extending conflict to troops in other countries and locations is appropriate as their 'proposed causal mechanism is that adverse economic shocks …make it easier to recruit fighters for civil conflicts'. However, we believe that government recruitment by conscription is fundamentally different from rebel recruitment and much less likely to be positively affected by economic strain. For example, it seems unreasonable to argue that the Zimbabwean government commits forces abroad to satisfy popular demands for alternative employment under adverse conditions. Indeed, involvement in Congo has been generally unpopular, widely criticized as a costly waste of resources, and the government has forcefully cracked down on protests against intervention.[2] If one actually wanted to consider the effect of economic conditions on decisions to send troops to conflicts abroad, then we would need a justification for why one should only consider civil wars elsewhere in Africa, as Miguel *et al*. (2004) do, and not cases where a state sends troops to civil conflicts outside the region, such as Nigerian peacekeepers in Bosnia, or participation in interstate wars.[3]

The extension to participation in civil war elsewhere in Africa does not seem consistent with the logic of the mechanisms through which economic growth may be linked to conflict, and we conclude that conflict should be restricted to the locations where conflict occurs, to properly evaluate the effects of growth shocks empirically. In this paper, we compare the results reported by Miguel *et al*. (2004) to new estimates when using the ACD data restricted to location.

[1] Miguel *et al*. (2004: 741–742) note that the results are less significant for alternative data on civil war from Collier and Hoeffler (2004), Doyle and Sambanis (2000), and Fearon and Laitin (2003), but do not offer an explanation for why the results differ. Our results indicate that the reported significant results with the ACD data on major conflicts primarily differ from the results using other conflict data sources due to their inclusion of external participants.

[2] See, for example, http://news.bbc.co.uk/1/hi/world/africa/205100.stmand; http://news.bbc.co.uk/1/hi/world/africa/611898.stm.

[3] Miguel *et al*. (2004: 731) state that they 'leave an empirical analysis of the causes of conflicts between countries for future research'. This would seem to suggest that they believe wars between states to have different causes than civil wars, but they do not comment on why this would not be the case for the decision to commit troops to civil wars in other states.

ESTIMATING THE EFFECTS OF GROWTH ON CONFLICT RESTRICTED TO LOCATION

In this section we first replicate the original findings of Miguel *et al*. (2004) and then consider how the estimates change when we restrict the conflict data to conflict on a state's territory. We provide a full list of African civil conflicts in the ACD data in Appendix A, highlighting cases of participation in civil wars in other states coded as conflict in Miguel *et al*. (2004) that we believe ought not to be included, as well as an error.[4] We follow the setup of Miguel *et al*. (2004) in using 2SLS estimation with growth in rainfall and its lag as instruments for economic growth and its lag, and we refer to their paper for details on the proposed model and data sources. Table I compares the estimates of the partial effect of growth and lagged growth on conflict incidence when including participation as in Miguel *et al*. (2004) and when using the corrected conflict data restricted to location.

For Model 1 with a minor civil conflict threshold, the estimate for the partial effect of lagged growth is –2.41 with the corrected conflict data, compared with the –2.55 coefficient reported by Miguel *et al*. (2004) when including the external participants. Miguel *et al*. (2004) found a point estimate of –2.25 for lagged growth for their Model 2, including a number of country-specific covariates from Fearon and Laitin (2003). This declines to –2.17 when using the corrected conflict coding. Although the consequences of misclassification from including

TABLE I Economic Growth and Civil Conflict (ACD 1.2a)

	Civil conflicts >=25 deaths				*>=1000 deaths*	
	Model 1		*Model 2*		*Model 3*	
Variable	*Miguel* et al.	*Corrected*	*Miguel* et al.	*Corrected*	*Miguel* et al.	*Corrected*
Economic Growth, *t*	–1.13	–0.894	–0.41	–0.48	–1.48*	–1.04
	(1.40)	(1.19)	(1.48)	(1.26)	(0.82)	(0.65)
Economic Growth, *t* – 1	–2.55**	–2.41**	–2.25**	–2.17**	–0.77	–0.41
	(1.10)	(1.09)	(1.07)	(1.04)	(0.70)	(0.62)
Log (GDP per capita), 1979			0.05	0.0071		
			(0.10)	(0.006)		
Democracy (Polity IV), *t* – 1			0.004	0.45		
			(0.006)	(0.37)		
Ethno-linguistic fractionalization			0.51	0.038		
			(0.39)	(0.42)		
Religious fractionalization			0.22	–0.074		
			(0.44)	(0.22)		
Oil-exporting country			–0.104	–0.074		
			(0.22)	(0.22)		
Log (national population), *t* – 1			0.159*	0.13		
			(0.093)	(0.09)		
Log (mountainous terrain)			0.060	0.08		
			(0.058)	(0.06)		
Country fixed effects	Yes	Yes	No	No	Yes	Yes
Country-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	743	743	743	743	743	743

Note: Entries are unstandardized coefficients with Huber-White robust standard error estimates clustering on countries in parenthesis.
* Significantly different from 0 in a 10% test.
** Significantly different from 0 in a 5% test.

4. Guinea is coded as at war in 1998, probably due to confusion with the war in Guinea-Bissau.

external participation may not seem dramatic in the case of the minor civil conflict threshold, for the major conflict threshold in Model 3 their original –1.48 estimate for current growth falls to –1.04 with the correct conflict coding, i.e. a drop of about 30%. This estimate also becomes non-significant, although we note that it is still close to the 10% significance level.[5]

The estimates reported in Table I are based on the version of the ACD data used by Miguel *et al*. (2004) to ensure comparability with their study. However, the ACD data have been revised since then in light of new information about specific incidents, and the new and improved data should clearly be used if we wish to assess what the best available data suggest with regards to the plausible magnitude of the effects of economic growth on conflict. In Table II we provide new estimates using the most recent version of the ACD data. When using the corrected conflict coding, the estimated effects of lagged economic growth for the minor conflict threshold become notably further reduced relative to the original estimates reported by Miguel *et al*. (2004), and the size of the coefficient estimates for lagged economic growth declines to about 75% of the original size for their Models 1 and 2. The magnitude of the coefficient estimate for current economic growth declines even further for the high conflict threshold in their Model 3 when using the corrected conflict coding and is no longer statistically significant. Hence, although one cannot fault Miguel *et al*. (2004) for not considering

TABLE II Economic Growth and Civil Conflict (updated to ACD version 4–2006b)

	Civil conflicts >=25 deaths				*>=1000 deaths*	
	Model 1		*Model 2*		*Model 3*	
Variable	*Miguel* et al.	*Corrected*	*Miguel* et al.	*Corrected*	*Miguel* et al.	*Corrected*
Economic Growth, *t*	–1.13	–0.356	–0.41	–0.034	–1.48*	–0.998
	(1.40)	(1.05)	(1.48)	(1.19)	(0.82)	(0.62)
Economic Growth, *t* – 1	–2.55**	–1.91**	–2.25**	–1.71**	–0.77	–0.371
	(1.10)	(0.85)	(1.07)	(0.84)	(0.70)	(0.62)
Log (GDP per capita), 1979			0.05	0.064		
			(0.10)	(0.082)		
Democracy (Polity IV), t–1			0.004	0.0097		
			(0.006)	(0.006)		
Ethno-linguistic fractionalization			0.51	0.45		
			(0.39)	(0.37)		
Religious fractionalization			0.22	0.045		
			(0.44)	(0.40)		
Oil-exporting country –0.104	–0.08					
			(0.22)	(0.21)		
Log (national population), *t* – 1			0.159*	0.124		
			(0.093)	(0.08)		
Log (mountainous)			0.060	0.08		
			(0.058)	(0.06)		
Country fixed effects	Yes	Yes	No	No	Yes	Yes
Country-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	743	743	743	743	743	743

Note: Entries are unstandardized coefficients with Huber-White robust standard error estimates clustering on countries in parenthesis.
* Significantly different from 0 in a 10% test.
** Significantly different from 0 in a 5% test.

[5] Since Miguel *et al*. (2004) include two terms for economic growth, it may be more appropriate to consider their joint significance rather than individual coefficients. F-tests here reveal generally low overall significance; The *p*-value is just below 0.10 for the minor threshold, and falls from the 15% level to the 29% level for the major threshold with the corrected conflict coding.

changes to the conflict data that have occurred after the publication of their article, it is clear from Table II that replacing their conflict data with the most recent and improved version of the ACD conflict data suggests a smaller impact of growth.

These results when removing countries that participate in civil wars elsewhere cast doubt on the strength and robustness of the impact of economic growth shocks on civil conflict incidence reported in Miguel *et al*. (2004). This is not to say that economic growth may not affect the prospects for conflict. However, a proper estimate of the effect would need to use appropriate data and research design.

The reported revised results and the consequences of removing external participants in turn raise a number of questions. First, one might ask why the effects of the misclassification or including external participants are larger for the high conflict threshold than the low conflict threshold. Second, why are the estimated effects of economic growth on the probability of conflict lower for the high threshold? And third, most notably, why does including conflict participation elsewhere yield a higher estimate of the effects of growth on conflict when this would seem theoretically less appropriate?

The first question is relatively easy to answer as including states participating in conflict elsewhere has much greater effects on the conflict incidence vector on the left-hand side in the case of the model with the high conflict threshold than in the case of the low conflict threshold. As can be seen from Appendix A, we have a much smaller share of remaining country-year instances of conflict incidence at the high threshold when we remove external participation in civil wars in other states than when we remove external participants without conflict on their own territory at the low conflict threshold. This is the case since several of the countries that participated in high intensity conflicts elsewhere also had conflicts on their own territory that exceeded the low conflict threshold (i.e. at least 25 deaths), but these conflicts did not reach the high conflict threshold of at least 1000 deaths in one calendar year. In this sense, the measurement error introduced by including the external participants is more severe for the high conflict threshold.

The second question is more difficult to answer conclusively. We first note that the difference in the estimated effects of growth for the two different conflict thresholds as well as the difference in the lag structure were also present in the original results presented by Miguel *et al*. (2004) , who acknowledge these differences, but do not offer an explanation for them. One possible interpretation is that if growth shocks have some causal impact on conflict, but their effect is limited, then it is unlikely that a growth shock alone would have sufficient impact on the opportunity costs of potential insurgents to allow recruiting an organization large enough to generate sufficient violence to exceed the 1000 battle-deaths threshold. Large scale escalation or major civil war is likely to require a much higher level of organization and successful collective action than small insurgencies. It seems likely that differences in organizational capacity and the ability to achieve successful collective action would be related to various pre-existing political characteristics and grievances unlikely to be captured in the right-hand side factors included in the model. As such, it is not so surprising that the marginal effect of economic growth is much weaker for the smaller set of conflicts that have escalated beyond the high threshold.

With regards to the third question, it seems at first counterintuitive that a theoretically less appropriate conflict measure should yield stronger evidence for an impact of growth on conflict. Measurement error in the dependent variable will generally introduce added noise and decrease confidence in the estimated effects of right-hand-side variables. However, the estimated size of coefficients may be inflated when the source of error is correlated with the explanatory variables. In this case, the apparently stronger results reported by Miguel *et al*. (2004) when including conflict participation may arise from spatial clustering in their data on growth in rainfall and patterns of external participation in civil wars. We investigate this issue empirically in the following section.

THE IMPACT OF SPATIAL PATTERNS IN RAINFALL AND CONFLICT PARTICIPATION

Physical phenomena such as rainfall and drought often display spatial clustering, and as such it is likely that measures of growth in rainfall will be similar among geographically close countries. Easterly and Levine (1999) demonstrate spatial clustering in economic growth among African states, although they do not evaluate whether this may be due to common geographical shocks such as droughts. Moreover, conflict participation tends to follow geographical patterns, as neighbouring states are more likely to intervene or provide assistance in an ongoing civil war than more distant countries (see Gleditsch, 2002).

In this specific case we find that many of the additional conflict observations that arise when including participants in civil wars elsewhere come from states that suffer exceptional droughts in Southern and Western Africa over the period. Growth in rainfall in these misclassified observations is often substantially below country and global averages over the time period, as can be seen in Table III for the low conflict threshold and Table IV for the high conflict thresholds. Moreover, there is also a regional trend in that the states that participate in civil war in other states tend to intervene in neighbouring countries. Hence, as neighbouring countries also have similar low growth in rainfall, including participating states may induce bias that appears to strengthen the effects of growth shocks on conflict incidence.

We assess the extent and impact of spatial correlation more formally by using common measures and techniques from spatial econometrics. Assessing spatial clustering requires us

TABLE III Growth in Rainfall for Misclassified Cases Relative to Country Average, Any Conflict

	Growth in rainfall	
Country	*Misclassified years*	*Average*
Burkina Faso (1990)	0.070	0.021
Guinea (1998–99)	–0.001	–0.003
Namibia (1998–99)	0.007	0.159
Rwanda (1996)	0.015	0.006
Zaire (1981–89)	0.035	–0.006
Zimbabwe (1998–99)	–0.044	0.034
(Global mean)		–0.080

TABLE IV Growth in Rainfall for Misclassified Cases Relative to Country Average, Major Conflict Threshold

	Growth in rainfall	
Country	*Misclassified years*	*Average*
Angola (1997)	–0.063	0.014
Burkina Faso (1990)	–0.201	0.021
Chad (1998–99)	0.146	0.023
Guinea (1998)	0.065	–0.003
Namibia (1998–99)	0.007	0.159
Rwanda (1997, 1999)	0.019	0.006
South Africa (1984–85)	0.098	0.015
Uganda (1998–99)	0.001	0.019
Zaire (1981–89)	0.035	–0.006
Zimbabwe (1998–99)	–0.044	0.034
(Global mean)		–0.080

to first specify the hypothesized connectivities between the individual observations (see Anselin, 1988). Typically, this is done through a binary connectivity matrix *C*, where each element $c_{ij} = 1$ if two units *i* and *j* are 'neighbors' or connected by some criterion and $c_{ij} = 0$ otherwise. In this case, we expect states that are 'close' to be more similar on rainfall and conflict participation. In the absence of clear expectations of the specific reach of spatial dependence and since this may well differ for rainfall or conflict participation we consider two plausible criteria based on the minimum distance between the outer boundaries of states, using data from Gleditsch and Ward (2001). The first criterion considers states connected if within 100 km of one another. This includes states that have directly contiguous borders as well as states that are near contiguous or separated by short stretches of water. Our second criterion includes connectivities up to 800 km. This provides a more encompassing measure of a state's regional context (Gleditsch, 2002), and also corresponds to the connectivity measures used by Murdoch and Sandler (2004) in their study of the spill-over effects of conflict on economic growth.

We first assess the extent of spatial correlation by considering the Moran's I statistic for rainfall per capita, conflict participation, and war participation (i.e. the high conflict threshold). The Moran's I statistic is a common measure of the similarity between the values of a variable *y* for one unit *i* and its neighbours *j*. The formula for Moran's I is

$$I = \frac{N\sum_i \sum_j c_{ij}(y_i - \bar{y})(y_j - \bar{y})}{\left(\sum_i \sum_j c_{ij}\right) \sum_i (y_i - \bar{y})^2}$$

The variance for the Moran's I can be derived based on assumptions about sampling. This in turns allows testing for the significance of spatial clustering in *y*, using the deviations from the expected value, i.e. $Z = [I - E(I)]/SE(I)$. We refer to Schaenberger and Gotway (2005:18–22) and Ward and Gleditsch (2008) for further details. Table V displays Moran's I correlation coefficients for the two distance thresholds, with significant correlations flagged based on the estimated variance assuming random sampling. As can be seen from Table V, we find significant positive spatial clustering or similarity in rainfall growth in 17 out of 19 years in our sample at the 100 km threshold. For the 800 km threshold we find significant positive spatial clustering for 14 out of the 19 years in the sample. Whereas the evidence for spatial clustering in rainfall seems to decline somewhat with the more encompassing threshold, we find that the opposite is the case with conflict participation. More specifically, we find significant spatial clustering in conflict participation in 1996, 1997, and 1998 at the 100 km threshold, as well as 1982, 1984–85, and 1988 with the 800 km threshold. For war participation or conflict at the high threshold we find significant correlations for 1998 and 1997 at the 100 km threshold, as well as 1983–89, 1991, and 1993 for the 800 km threshold. In particular, we note that we see evidence of spatial clustering in conflict in precisely those years where we see many additional cases of participation in civil war included in Miguel *et al*. (2004) where the states involved did not have conflict on their own territory.

Although these descriptive measures suggest considerable spatial clustering in these data it is possible that some of this clustering could disappear when we condition on the other independent variables in the Miguel *et al*. (2004) model that may be spatially clustered in similar ways. Moreover, Miguel *et al*. (2004) also consider a variety of robustness tests such as country fixed effects in their model specifications, and these robustness tests are sometimes suggested as helpful for addressing the potential effects of spatial correlation. To assess how spatial correlation may have affected their reported estimates more systematically we estimate a so-called spatially lagged *y* model, where we add to the right-hand side of the model a spatial lag, or a term indicating whether neighbouring states are involved in conflict based on the

TABLE V Moran's I Statistics for Spatial Clustering

	Precipitation per capita		*Conflict participation*		*War participation*	
Year	*100km*	*800 km*	*100km*	*800 km*	*100km*	*800 km*
1999	0.038	–0.111	0.109	0.033	0.089	0.002
1998	0.44**	0.306**	0.175**	0.096**	0.222**	0.092**
1997	0.027	–0.02	0.174**	0.119**	0.181**	0.087**
1996	0.302**	0.301**	0.233**	0.125**	–0.037	–0.029
1995	0.449**	0.254**	–0.095	0.02	–0.037	–0.029
1994	0.453**	0.324**	0.04	–0.03	–0.023	–0.027
1993	0.501**	0.415**	–0.193	–0.048	–0.086	0.053*
1992	0.607**	0.496**	–0.221	–0.087	–0.149	0.008
1991	0.466**	0.295**	0.008	–0.013	0.029	0.114**
1990	0.2**	0.165**	–0.203	–0.074	–0.131	–0.059
1989	0.302**	0.114**	–0.012	0.04	0.055	0.132**
1988	0.198**	0.009	0.062	0.116**	0.053	0.065*
1987	0.16**	0.04	–0.028	0.034	0.053	0.065*
1986	0.341**	0.2**	–0.005	0.046	0.053	0.065*
1985	0.214**	0.09**	0.062	0.116**	0.053	0.065*
1984	0.191**	0.037	0.036	0.132**	0.053	0.065*
1983	0.315**	0.316**	–0.014	0.042	0.053	0.065*
1982	0.528**	0.363**	0.04	0.12**	–0.097	0.031
1981	0.588**	0.301**	–0.184	–0.067	–0.097	0.031

Note: Entries are Moran's I correlation coefficients.
** denotes entries that are significantly different from 0 in a 5% test, based on an estimate of the variance assuming randomization.
* denotes entries that are significantly different from 0 in a 10% test, based on an estimate of the variance assuming randomization.

product of a row-normalized connectivity matrix **W** and *y* itself, i.e. **W***y*. If many incidents in the Miguel *et al*. (2004) conflict coding stem from participation in neighbouring states, then we should see a positive coefficient for the spatial lag of *y*. Furthermore, if we have common clustering in conflict participation and rainfall, then the estimated effect of growth instrumented by rainfall may decline when we take into account the spatial clustering in conflict.

The presence of the spatial lag of *y* on the right hand side of the model creates a problem of simultaneity, and we estimate the model using **X**, **WX**, and $\mathbf{W}^2\mathbf{X}$ as instruments for the spatial lag **W***y*. We refer to Anselin (1988) and Ward and Gleditsch (2008) for further details on the spatially lagged *y* model and estimation.

Table VI reports the findings for Miguel *et al*.'s (2004) Models 2 and 3 with spatial lags of conflict for the original and corrected data, using the 100 km and the 800 km thresholds. The positive coefficient estimates for the spatial lag in Model 2 for the conflict coding including participation indicate that conflict incidence (in the wide sense, i.e. including conflict that takes place elsewhere) is much more likely when conflict is recorded for a neighbouring state. The coefficient estimate for the spatial lag using Miguel *et al*.'s (2004) original data including participation falls just short of significance at the 0.1 level for the low conflict threshold in Model 2 at the 100 km distance threshold.[6] However, the coefficient estimate for the spatial lag is large and significant for the model when estimated with the 800 km threshold, suggesting considerable spatial clustering in the data when including participants in conflict in other states.

[6] We note, however, that it is significant at the 11% level. Moreover, the ratio of the estimated spatial lag coefficient in this model to its standard error is higher than the ratio of the coefficient for current economic growth in Model 3 in Table I to its standard error.

TABLE VI Economic Growth and Civil Conflict, Spatial Lag Estimates

	Civil conflicts >=25 deaths				*>=1000 deaths*			
	Model 2				*Model 3*			
	Miguel et al.		*Corrected*		*Miguel* et al.		*Corrected*	
	100km	*800km*	*100km*	*800km*	*100km*	*800km*	*100km*	*800km*
Economic Growth, *t*	–0.077	0.698	–0.031	0.644	–1.283	–0.233	–1.032*	–0.449
	(1.297)	(1.256)	(1.158)	(1.149)	(1.037)	(0.893)	(0.57)	(0.629)
Economic Growth, *t* – 1	–1.734*	–1.624	–1.703**	–1.797*	0.149	–0.778	–0.364	–0.691
	(0.985)	(1.167)	(0.825)	(0.955)	(1.134)	(0.796)	(0.612)	(0.462)
Log (GDP per capita), 1979	0.05	0.021	0.063	0.039				
	(0.084)	(0.09)	(0.075)	(0.085)				
Democracy (Polity IV), *t* – 1	0.005	0.007	0.01*	0.012**				
	(0.006)	(0.006)	(0.005)	(0.006)				
Ethno-linguistic fractionalization	0.606*	0.666*	0.448	0.555				
	(0.364)	(0.386)	(0.36)	(0.378)				
Religious fractionalization	–0.015	–0.033	0.041	–0.075				
	(0.412)	(0.436)	(0.385)	(0.398)				
Oil-exporting country	–0.13	–0.207	–0.075	–0.119				
	(0.21)	(0.209)	(0.185)	(0.203)				
Log (national population)	0.151*	0.152*	0.124	0.117				
	(0.085)	(0.088)	(0.079)	(0.082)				
Log (mountainous terrain)	0.025	–0.002	0.075	0.044				
	(0.065)	(0.064)	(0.064)	(0.065)				
Spatial lag of conflict	0.199	0.413**	0.004	0.217	0.108**	0.423**	–0.012	0.173**
	(0.122)	(0.18)	(0.131)	(0.174)	(0.047)	(0.111)	(0.131)	(0.066)
Country fixed effects	No	No	No	No	Yes	Yes	Yes	Yes
Country-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	743	743	743	743	743	743	743	743

Note: Entries are unstandardized coefficients with Huber-White robust standard error estimates clustering on countries in parenthesis.
* Significantly different from 0 in a 10 percent test.
** Significantly different from 0 in a 5 percent test.

Comparing the results in Table VI to the results originally reported by Miguel *et al*. (2004) in Table I indicates a further decline in the estimates for lagged economic growth at the lower conflict threshold when controlling for spatial correlation. The original estimate for the conflict coding with external participation, i.e. –2.25, is over 30% higher than the –1.73 estimate for lagged economic growth in the model including the spatial lag in the model with the 100 km distance threshold.[7] The coefficient estimate is smaller yet in the model at the 800 km distance threshold, and is no longer statistically significant at conventional significance levels. Hence, even if one actually believes that participation in conflicts in other states should be included in a reasonable measure of conflict 'incidence', it will be important to consider the possible spatial correlation in conflict when estimating the effect of growth on conflict. We thus conclude that there seems to be considerable spatial clustering in the conflict incidence

[7]The coefficient for x_k in a spatially lagged y model is not directly comparable to its counterpart in a model without the spatial lag, since this indicates the immediate impact of a change in x_k without the feedback implied by the model. With a spatially lagged y, a change in a right-hand side x_k for country i will first change y_i directly, and then indirectly, through the effect of y_i on the neighbours of i, which in turn feeds back onto i, and reverberates through the system until reaching a new equilibrium. The full 'equilibrium impact' hence also depends on the spatial multiplier $(\mathbf{I} - \rho\mathbf{W})^{-1}$, where ρ indicates the parameter for the spatially lagged y (see Ward and Gleditsch, 2008). In this case, the median equilibrium effect is –1.75, still substantially lower than the original estimate.

data including participation. However, this appears to by driven largely by including participating states. The estimate for the spatial lag is close to zero with the corrected data restricted to location at the 100 km threshold and is not significant, albeit larger, at the 800 km threshold.

The results are somewhat mixed for Model 3 with the high conflict threshold and country-specific fixed effects. We find significant evidence of positive spatial clustering for the original Miguel *et al*. (2004) conflict coding including external participation at both the 100 km and the 800 km distance thresholds. The estimate for contemporary economic growth is not significant in either of these model estimates and far from conventional levels of significance. For the corrected conflict data we find a smaller, yet statistically significant, coefficient for lagged economic growth at the 100 km threshold, whereas the coefficient is non-significant in the model at the 800 km threshold. The coefficient for the spatial lag of conflict is significant in the model at the 800 km threshold, but non-significant and even has a negative sign in the model at the 100 km threshold. We are hesitant to conclude too much from these results since this model includes country fixed effects, as in practice it is often difficult to identify both country fixed effects and spatial lags with much precision in the same model (see, for example, Elhorst, 2003). However, this in our view reconfirms the lack of stability for the results of current economic growth on conflict at the high threshold, and we believe that these results in general again demonstrate the importance of considering the possible spatial correlation in conflict when estimating the effects of economic growth on conflict.

CONCLUSION

We argue that a closer look at the suggested causal mechanisms linking growth and civil war does not make it reasonable to include external participation in civil conflicts elsewhere. We have documented how an inappropriate coding of conflict including participation influences the estimates of the effects of growth on conflict reported in Miguel *et al*. (2004), and how the estimated effects of growth are notably smaller when we use a theoretically appropriate coding restricted to location. Furthermore, spatial correlation in growth in rainfall and conflict participation – especially in the conflict in the Democratic Republic of Congo (Zaire) – leads to a higher estimate of the effects of growth on conflict incidence. These cases do not seem to be consistent with the argument that adverse economic conditions make it easier to mobilize fighters in civil wars.

We emphasize again that the effects of economic growth on conflict is a topic worthy of investigation, and that rainfall may be a very useful instrument for growth in countries where rain-fed agriculture is the dominant economic activity. However, investigating the impact of growth on conflict also requires that we carefully detail the theoretical linkages and use appropriate research designs and data. Although space precludes us from further discussion here, we believe that research on these issues can be enhanced through data disaggregated below the nation state and more comprehensive violence data, and preliminary research supports these conjectures.

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APPENDIX A: COMPARISON OF MIGUEL *ET AL*. (2004) AND CORRECTED CONFLICT CODING

TABLE AI Civil Wars >=1000 Deaths per Year

Country	*Miguel* et al. *(2004) coding*	*Corrected and updated*
Angola	1981–94, 1997–99	1975–94, 1998–2001. Sample for estimation only includes until 1999 Participated in DRC war in 1997
Benin	No civil wars	No civil wars
Burkina Faso	1990	Participated in Liberia in 1990
Burundi	1998	1998
Cameroon	No civil wars	No civil wars
Central African Republic	No civil wars	No civil wars
Chad	1965–88, 1990, 1998–99	1965–88, 1990. Participated in in the DRC war 1998–99
Congo Brazzaville	1997–99	Only >= 1000 in 1997–98. 1999 is included in older versions
Congo, DR/Zaire	1981–89, 1997–2000	Listed as participant in Angola 1981–89, 1997–2000 in old versions
Djibouti	No civil wars	No civil wars
Ethiopia	1976–91	1976–91
Gabon	No civil wars	No civil wars
Gambia	No civil wars	No civil wars
Ghana	No civil wars	No civil wars
Guinea	1998	No civil wars (but conflict in Guinea–Bissau)
Guinea–Bissau	1998	1998
Ivory Coast	No civil wars	No civil wars
Kenya	No civil wars	No civil wars
Lesotho	No civil wars	No civil wars
Liberia	1990	1990 (1992, 2003 not included because of data availability for growth)
Madagascar	No civil wars	No civil wars
Malawi	No civil wars	No civil wars
Mali	No civil wars	No civil wars
Mauritania	No civil wars	No civil wars
Mozambique	1981–92	1981–92
Namibia	1998–99	Participated in DRC war 1998–99
Niger	No civil wars	No civil wars
Nigeria	No civil wars	No civil wars
Rwanda	1991–92, 1997–99	1991–92, 1998. Participated in DRC war 1997–1999
Senegal	No civil wars	No civil wars
Sierra Leone	1998–99	1998–99
South Africa	1980–93	1980–83, 1986–88. Older versions include war in 1989–93. Participated in Angolan war 1975–89
Sudan	1983–92, 1995–2002	1983–92, 1995–2002
Swaziland	No civil wars	No civil wars
Tanzania	No civil wars	No civil wars
Togo	No civil wars	No civil wars
Uganda	1981–1988, 1989, 1991, 1998–99	1981–1988, 1989, 1991 participated in DRC war 1998–99
Zambia	No civil wars	No civil wars
Zimbabwe	1998–99	No civil wars. Participates in the DRC war 1998–99

TABLE A2 Civil Wars >= 25 Deaths per Year

Country	*Miguel* et al. *(2004) coding*	*Corrected and updated*
Angola	1981–1999	1981–1999
Benin	No conflict	No conflict
Burkina Faso	1987, 1990–91	1987. Participates in Liberia 1990–91
Burundi	1990–92, 1995–99	1991–92, 1994–99, in new ACD
Cameroon	1984	1984
Central African Republic	No conflict	No conflict
Chad	1981–94, 1997–99	1981–94, 1997–99
Congo–Brazzaville	1997–99	1993–94, 1997–99
Congo, DR/Zaire	1981–89, 1996–1999	1996–1999. Participates in Angola 1981–89 older ACD versions
Djibouti	1991	1991
Ethiopia	1981–91, 1996–99	1981–91, 1996–99
Gabon	No conflict	No conflict
Gambia	1981	1981
Ghana	1981,1983	1981,1983
Guinea	1998–99	No conflict
Guinea Bissau	1998–99	1998–99
Ivory Coast	No conflict	No conflict
Kenya	1982	1982
Lesotho	1998	1998
Liberia	1989–91	1989–91 (1992, 2003 not included because of data availability)
Madagascar	No conflict	No conflict
Malawi	No conflict	No conflict
Mali	1990, 1994	1990, 1994
Mauritania	No conflict	No conflict
Mozambique	1981–92	1981–92
Namibia	1998–99	Participates in the DRC war 1998–99
Niger	1990–92, 1994, 1996–97	1992, 1994, 1996–1997, due to ACD changes
Nigeria	No conflict	No conflict
Rwanda	1990–94, 1996–99	1990–94, 1997–99. Participates in the DRC in 1996
Senegal	1990, 1992–93, 1995, 1997–99	1990, 1992–93, 1995, 1997–99
Sierra Leone	1991–99	1991–99
South Africa	1981–93	1981–88; 1989–93 removed in updated ACD
Sudan	1983–99	1983–99
Swaziland	No conflict	No conflict
Tanzania	No conflict	No conflict
Togo	1986, 1991	1986, 1991
Uganda	1981–1991, 1994–99	1981–1991, 1994–99
Zambia	No conflict	No conflict
Zimbabwe	1998–99	Participates in DRC war in 1998–99