Transitory Economic Shocks and Civil Conflict

by

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Abstract

Miguel, Satyanath, and Sergenti (2004) argue that Sub-Saharan African civil conflicts 1981-1999 started following negative rainfall shocks. Using the same data and controls, I conclude the opposite: conflicts started after positive rainfall shocks. Our conclusions disagree because I treat rainfall as mean reverting; MSS identify the effect of rainfall shocks on conflict outbreak assuming rainfall follows a random walk. The opposing conclusions highlight the importance of correctly capturing the persistence of economic shocks, an issue the conflict literature has not dealt with so far. I also examine the link between rainfall shocks and civil conflict in recent data.

Key words: Transitory shocks, mean reversion, rainfall, conflict

JEL codes: O0, P0, Q0

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1. Introduction

Is civil conflict triggered by economic downturns? Answering this question is complicated by many difficult-to-measure economic, social, political, and institutional factors that may affect both income and the likelihood of civil conflict. In addition, economic downturns may partly be driven by the expectation of future civil conflict. Miguel, Satyanath, and Sergenti (2004) address both issues by using a panel-data setup to control for unobservables, and rainfall as an exogenous source of economic shocks in Sub-Saharan Africa. Their empirical work yields that civil conflict between 1981 and 1999 was more likely to start following years of low interannual rainfall growth. Miguel, Satyanath, and Sergenti conclude that negative rainfall shocks trigger civil conflict. Their line of argument follows Collier and Hoeffler (1998, 2002, 2004) and Fearon and Laitin (2003), who have argued that economic variables are important determinants of civil conflict.

Estimating the effect of economic shocks on civil conflict involves an interesting issue that has not been analyzed so far. Consider Miguel, Satyanath, and Sergenti's finding that civil conflict is more likely to start in year *t*, the lower rainfall growth is between year *t*-1 and year *t*-2. This result is consistent with a high probability of conflict outbreak when a drought year at *t*-1 (a negative rainfall shock) follows an average rainfall year at *t*-2. But the result is also consistent with a high conflict probability when exceptionally good rainfall conditions at *t*-2 (a positive rainfall shock) are followed by an average rainfall year at *t*-1. The general point is that because rainfall shocks are transitory, low interannual rainfall growth could be due to a negative rainfall shock or due to mean reversion following a positive shock. Miguel, Satyanath, and Sergenti's empirical finding that civil conflict is more likely when interannual rainfall growth is low is therefore consistent with conflict following negative rainfall shocks or the exact opposite conclusion that civil conflict follows positive rainfall shocks. As a result, their conclusion that civil conflict is triggered by negative rainfall shocks does not follow from their empirical findings. In fact, my empirical work, using the same data and control variables, reaches the opposite conclusion of Miguel, Satyanath, and Sergenti: civil conflict is triggered by positive rainfall shocks.

Answering the question whether civil conflict is triggered by (transitory) negative rainfall shocks requires relating the outbreak of civil conflict not to interannual rainfall growth rates but to rainfall levels. If conflict is triggered by negative rainfall shocks, civil conflict outbreak should be more likely the lower rainfall levels are in previous years. On the other hand, if conflict is most likely following positive rainfall shocks, civil conflict outbreak should be more probable following years of high rainfall. When I apply this approach to the Miguel, Satyanath, and Sergenti data, I find that civil conflicts are statistically more likely to break out following positive rainfall shocks. Miguel, Satyanath, and Sergenti reach the opposite conclusion because their empirical framework assumes that negative interannual rainfall growth rates reflect negative rainfall shocks. This would be the case if rainfall levels followed a random walk. But rainfall shocks are transitory, and their empirical findings capture instances of low interannual rainfall growth following positive rainfall shocks (mean reversion following positive rainfall shocks).

The remainder of the paper is organized as follows. Section 2 presents the empirical framework and the data. Section 3 uses the civil conflict data of Miguel, Satyanath, and

Sergenti but reaches the opposite of their conclusion. Section 4 examines the link between rainfall shocks, economic shocks, and civil conflict in recent data.

2. Estimation Framework and Data

My main estimating equation relates civil conflict in country *c* and year *t* to log rainfall levels in the country in years *t*, *t*-1, and *t*-2 and the controls of Miguel, Satyanath, and Sergenti: country-specific fixed effects (α_c) and country-specific linear time trends ($\beta_c t$). In addition, some specifications also control for time-varying unobservable risk factors common to Sub-Saharan Africa (δ_t). Formally, the estimating equation is

$$(1)\quad conflict_{c,t} = \alpha_c + \beta_c t\ [+\delta_t] + \gamma_1 \log Rain_{c,t} + \gamma_2 \log Rain_{c,t-1} + \gamma_3 \log Rain_{c,t-2} + \varepsilon_{c,t}$$

where $\alpha, \beta, \delta, \gamma$ denote parameters to be estimated and ε a disturbance term. I have put δ in square brackets to stress that common time effects are only included in some specifications. The *conflict* variable is either conflict onset (outbreak) or conflict incidence. Conflict incidence is an indicator variable that takes the value of 1 when there is a civil conflict in country *c* and year *t*, and is 0 otherwise. Conflict onset is an indicator variable that is 1 if there is a conflict in a given country-year but there was no conflict in the same country in the previous year; the onset indicator is 0 if there is no conflict in a given year and there was no conflict in the previous year. Conflict onset is the appropriate variable for examining whether rainfall shocks contribute to the outbreak of civil conflict (conflict incidence captures both onset and conflict duration). I also estimate (1) with income per capita levels on the right-hand side and rain levels as instruments.[1]

[1] The main estimation method is IV-2SLS, which is usually preferred in case with a dichotomous explanatory variable as alternative estimation approaches require strong specification assumptions (Angrist and Krueger, 2001; Wooldridge, 2002).

The specification to estimate the effect of interannual rainfall growth on conflict corresponds to that of Miguel, Satyanath, and Sergenti

(2) $$conflict_{c,t} = \alpha_c + \beta_c t + \gamma_1 RainGrowth_{c,t} + \gamma_2 RainGrowth_{c,t-1} + \varepsilon_{c,t}$$

where $RainGrowth_{c,t}$ and $RainGrowth_{c,t-1}$ are the growth rates of rainfall between years *t* and *t*-1 and years *t*-1 and *t*-2 respectively.

The conflict data of Miguel, Satyanath, and Sergenti come from the UCDP/PRIO Armed Conflicts Dataset of the International Peace Research Institute's (PRIO) Centre for the Study of Civil War and the Uppsala Conflict Data Program.[2] The latest version of this database is 2007. Rainfall data for the period 1981-2001 come from Miguel, Satyanath, and Sergenti and their data website; the original data source is the NASA GPCP (NASA Global Precipitation Climatology Project).[3] I extend the GPCP rainfall database to 2006 using the same methodology and data source (NASA Global Precipitation Climatology Project, Version 2).[4] The income per capita data are taken from the Penn World Tables (PWT) 6.2, which stop in 2003.[5]

3. Empirical Results

3.1. Results Using the Miguel, Satyanath, and Sergenti Data

Table 1 makes the main point of the paper. Panel A reproduces the Miguel, Satyanath, and Sergenti regressions linking interannual rainfall growth and income growth to civil conflict onset and incidence. Column (1) shows that civil conflict in year *t* is more likely to start following low internannual rainfall growth between years *t* and *t*-1 and low

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[2] The dataset is available at http://new.prio.no/CSCW-Datasets/Data-on-Armed-Conflict.
[3] The data are available at http://elsa.berkeley.edu/~emiguel/data.shtml.
[4] See Adler et al. (2003). The data are available at http://precip.gsfc.nasa.gov.
[5] The dataset is available at http://pwt.econ.upenn.edu.

interannual rainfall growth between years *t*-1 and *t*-2. Moreover, the effect of interannual rainfall growth between *t*-1 and *t*-2 is statistically significant at standard confidence levels. The two-stage least-squares regressions in column (2) use rainfall growth as an instrument for income growth (the first-stage regression yields an F-statistic of 4.5 on the excluded instruments, see Miguel, Satyanath, and Sergenti, p. 735). The two-stage least-squares results show that low (rainfall-driven) income growth between years *t* and *t*-1 and low income growth between years *t*-1 and *t*-2 make civil conflict outbreak in year *t* more likely. Moreover, the effect of income growth between *t* and *t*-1 is statistically significant at standard confidence levels. The conflict incidence results in columns (3) and (4) yield analogous results. Still, it should be kept in mind that civil conflict incidence partly reflects the duration of conflicts, and that conflict onset is the right variable for examining the factors that start conflicts.

The key issue here is the interpretation of the results in Panel A. Miguel, Satyanath, and Sergenti conclude that civil conflicts are started by negative rainfall shocks. As explained in the introduction, this conclusion is unwarranted because rainfall levels are mean reverting. To see whether negative rainfall shocks raise the probability of civil conflict outbreak it is necessary to examine the relationship between civil conflict outbreak and rainfall levels, not interannual growth rates. This is what I do in Panel B, column (1). The results show that the only statistically significant effect is a positive effect of year *t*-2 rainfall levels on the probability of civil conflict outbreak; a 10 percent increase in rainfall levels in *t*-2 is associated with a 1.56 percentage point increase in the probability of civil conflict outbreak in *t*. Hence, the statistical evidence indicates that civil conflict is most likely to start following positive rainfall shocks—the opposite of the conclusion of Miguel, Satyanath, and Sergenti.

Panel B, column (2) contains the two-stage least-squares result with deviations of income from trend instrumented by rainfall levels. The corresponding first-stage regression, which regresses log income per capita in year *t* on country fixed effects and country-specific linear time trends as well as log rainfall levels in year *t*, yield an exclusion F-statistic of 15.05 on the rainfall variable (results not shown). The two-stage least-squares results show that civil conflict outbreak is more likely following positive income shocks in year *t*-1 and year *t*-2, but negative income shocks in year *t*. The effects are not significant at standard confidence levels however. It is interesting to note that the positive effect of income in year *t*-2 becomes statistically significant when I exclude income at *t* or income at *t* and *t*-1 from the empirical specification. The negative effect in year *t* on the other hand, never becomes statistically significant at conventional levels whether I drop income at *t*-1, income at *t*-2, or income at *t*-1 as well as *t*-2.

Panel B, columns (3) and (4) use civil conflict incidence instead of onset as the left-hand-side variable. The reduced form regression in column (3) yields that past rainfall levels are a statistically insignificant determinant of conflict incidence. The two-stage least-squares regression in column (4) shows that conflict incidence is statistically more likely following positive income shocks in year *t*-2; a 10 percent increase in income in *t*-2 is associated with a 2.31 percentage points increase in the probability of civil conflict. This is the opposite of the conclusion that Miguel, Satyanath, and Sergenti reached based on the results in the corresponding column in Panel A. The reason is that their approach does not take into account that rainfall-driven income shocks are transitory.

3.2. Results Using the Latest Data

The conflict data used by Miguel, Satyanath, and Sergenti cover the period from 1981 to 1999. The 2007 version of the same dataset goes up to 2006, extending the sample by more than one third. The latest conflict dataset does not only cover a wider time span than the Miguel, Satyanath, and Sergenti data but also differs for the 1981-1999 period. For example, 14 percent of civil conflicts in the Miguel, Satyanath, and Sergenti sample are no longer classified as conflicts in the 2007 dataset, although the classification criteria did not change. It therefore seems likely that UCDP/PRIO corrects coding mistakes in previously covered periods when adding to their database. Such a large number of coding mistakes is a serious concern and raises doubts on the quality of the conflict data (and the conclusions drawn from it). But it still seems interesting to examine the link between rainfall shocks, income shocks, and civil conflict according to the latest data.

I therefore re-examine the link between rainfall shocks, income shocks, and civil conflict using the latest conflict data. In doing so, I also change the control variables compared to Miguel, Satyanath, and Sergenti. To see why, note that Miguel, Satyanath, and Sergenti’s specifications do not control for common factors affecting the risk of civil conflict throughout Sub-Saharan Africa. But such factors could have played a role. For example, Fearon and Laitin (2003) point to the end of the Cold War as a factor that may have led to more civil conflicts. In fact, adding a dummy for the period after the end of the Cold War to Miguel, Satyanath, and Sergenti’s conflict onset regressions renders the link between interannual rainfall growth and conflict statistically insignificant. The post Cold War dummy, on the other hand, is significantly positive (results not shown). To have the clearest possible answer regarding the effect of rainfall shocks on civil conflict, I therefore take the panel methodology one step further and include time effects to control for unobserved risk factors that are common to all Sub-Saharan African countries.

The empirical results linking civil conflict onset and incidence for 1981-2006 to rainfall shocks and rainfall-driven income shocks are in Table 2. The reduced form regressions for onset in column (1) and incidence in column (3) now both yield that civil conflict is more likely following negative rainfall shocks. The common time effects are very highly statistically significant in both columns (the p-values of the hypothesis that time effects can be excluded are 0.0000 in both cases). Still, I obtain the same results when I eliminate common time effects (results not shown). The results also remain unchanged when I consider the Miguel, Satyanath, and Sergenti 1981-1999 time period, whether I control for time effects or not (results not shown). Hence, the corrected conflict data for 1981-1999 yield a different result than the 1981-1999 conflict data employed by Miguel, Satyanath, and Sergenti. The two-stage least-squares regression in column (4) yields that civil conflict incidence is statistically more likely when there was a negative rainfall-driven income shock in t-1. The results for civil conflict onset in column (2) are weaker but point in the same direction. (The two-stage least-squares regressions are based on fewer observations than the corresponding reduced forms because the PWT income data stop in 2003.) Overall, the data indicate that negative rainfall shocks trigger civil conflict. The two-stage least-squares results are weaker but point in the same direction.

In addition to the determinants of civil conflict, the conflict literature has also analyzed the determinants of civil war, which UCDP/PRIO defines as civil conflicts with more than 1000 annual battle deaths. I do not find evidence of civil wars being triggered by rainfall shocks however, whether I control for time effects or not (results not shown).

3. Conclusion

To examine the effect of economic shocks on the outbreak of civil conflict it is necessary to deal appropriately with the persistence of economic shocks. I have illustrated this general point by re-examining the study of Miguel, Satyanath, and Sergenti (2004). They find that civil conflict is more likely following low interannual rainfall growth and conclude that negative rainfall shocks trigger civil conflict. But as rainfall shocks are transitory, interannual rainfall growth can be low because of positive or negative rainfall shocks. Hence, the empirical finding of Miguel, Satyanath, and Sergenti is consistent with conflict starting following negative rainfall shocks or the exact opposite conclusion that conflict starts following positive rainfall shocks. I re-examine their data using an empirical approach that distinguishes positive from negative rainfall shocks and find that conflict tends to break out following positive rainfall shocks—the opposite of the conclusion of Miguel, Satyanath, and Sergenti.

The conflict database employed by Miguel, Satyanath, and Sergenti has since undergone substantial corrections and has also been extended by more than one third. The latest data and new empirical approach yield that civil conflict is more likely to start following negative rainfall shocks, but no evidence of rainfall shocks affecting the probability of civil war.

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Table 1: Economic Shocks and Conflict in the Miguel, Satyanath, and Sergenti (2004) data (1981-1999)

	Civil Conflict Onset		Civil Conflict Incidence	
		Panel A: Rainfall Growth		
	(1)	(2)	(3)	(4)
	LS	2SLS	LS	2SLS
Rainfall Growth, t	-0.062 (-1.36)		-0.024 (-0.57)	
Rainfall Growth, t-1	-0.120* (-1.84)		-0.122** (-2.42)	
GDP Growth, t		-3.154* (-1.85)		-1.132 (-0.87)
GDP Growth, t-1		-1.840 (-1.36)		-2.546** (-2.48)
Country FE + Linear Trend	Yes	Yes	Yes	Yes
No Observations	555	555	743	743
		Panel B: Rainfall Levels		
	(1)	(2)	(3)	(4)
	LS	2SLS	LS	2SLS
Log Rainfall, t	-0.073 (-0.96)		-0.076 (-1.22)	
Log Rainfall, t-1	-0.026 (-0.36)		-0.115 (-1.55)	
Log Rainfall, t-2	0.156** (2.19)		0.110 (1.43)	
Log GDP, t		-1.278 (-1.01)		-0.846 (-0.92)
Log GDP, t-1		1.590 (1.04)		-0.887 (-0.73)
Log GDP, t-2		1.702 (1.37)		2.310** (2.23)
Country FE + Linear Trend	Yes	Yes	Yes	Yes
No Observations	555	555	743	743

Note: The method of estimation in columns (1) and (3) is least squares; columns (2) and (4) two-stage least squares. Huber robust standard errors are clustered at the country level; t-values in brackets. *Significantly different from zero at 90 percent confidence, ** 95 percent confidence, *** 99 percent confidence.

Table 2: Economic Shocks and the Latest Conflict Data (1981-2006)

	Civil Conflict Onset		Civil Conflict Incidence	
	(1)	(2)	(3)	(4)
	LS	2SLS	LS	2SLS
Log Rainfall, t	0.072 (1.24)		0.020 (0.42)	
Log Rainfall, t-1	-0.142*** (-2.64)		-0.174*** (-3.22)	
Log Rainfall, t-2	0.033 (0.89)		-0.037 (-0.50)	
Log GDP, t		1.942 (1.10)		0.043 (0.04)
Log GDP, t-1		-4.215 (-1.61)		-2.512** (-2.42)
Log GDP, t-2		2.513 (1.03)		0.946 (0.64)
Country FE + Linear Trend	Yes	Yes	Yes	Yes
Time Effects (p-value)	Yes (0.0000)	Yes (0.0000)	Yes (0.0000)	Yes (0.0000)
No Observations	802	668	1049	865

Note: The method of estimation in columns (1) and (3) is least squares; columns (2) and (4) two-stage least squares. Huber robust standard errors are clustered at the country level; t-values in brackets. *Significantly different from zero at 90 percent confidence, ** 95 percent confidence, *** 99 percent confidence.