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IS THE RELATIONSHIP ROBUST?

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Climate and Civil War: Is the Relationship Robust?
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ABSTRACT

A recent paper by Burke et al. (henceforth "we") finds a strong historical relationship between warmer-than-average temperatures and the incidence of civil war in Africa (Burke et al. 2009). These findings have recently been challenged by Buhaug (2010) who finds fault with how we controlled for other potential explanatory variables, how we coded civil wars, and with our choice of historical time period and climate dataset. We demonstrate that Buhaug's proposed method of controlling for confounding variables has serious econometric shortcomings and show that our original findings are robust to the use of different climate data and to alternate codings of major war. Using Buhaug's preferred climate data under sound econometric assumptions yields results that suggest an even stronger relationship between temperature and conflict for the 1981-2002 period than we originally reported. We do find that our historical relationship between temperature and conflict weakens over the last decade, a period of unprecedented African economic growth and very few large wars.

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Introduction

A recent paper by Burke et al. (henceforth "we") links historical variation in climate to the incidence of African conflict, with warmer-that-average temperatures related to large increases in civil war between 1981-2002 (Burke et al. 2009). These results have recently been challenged, mostly notably by Buhaug (2010), who argues that our findings rely on an "unorthodox" understanding of civil war, on specific "methodological fixes", and on the time period of analysis. We demonstrate that Buhaug's challenges depend on dubious econometric choices and on less relevant conceptualizations of conflict, and, that our original results are robust to alternative econometrically sound specifications, to different climate datasets, and to alternative ways of coding large wars. At the same time, we find with Buhaug that African conflict appears less sensitive to climate over the past decade, a change likely related to the unprecedented economic growth and democratization that most of the continent has recently experienced.

Methodological choices

A number of methodological choices must be made to credibly estimate the effect of past climate variability on civil war. One choice concerns how to control for the many other variables beyond climate that might affect conflict incidence. Of particular concern is accounting for variables that are correlated with the explanatory variable(s) of interest, some of which are likely to be time-invariant and others that might trend over time. For instance, hotter countries are on average much poorer, less democratic, and have lower educational attainment and worse health outcomes.[1] If (as seems likely) these differences are not explained by climate alone, then failing to control for them in some way will lead to biased estimates of the effect of climate variables on conflict (so-called "confounding" or "omitted variables bias"). Unfortunately, the analyst might not have good data on – or even be aware of – all of the different confounding variables that affect conflict and are correlated with climate. Thus eliminating omitted variables bias through explicit controls

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[1] E.g. see Dell, Jones, and Olken (2009). Similar cross-sectional regressions show that warmer temperatures are associated with less democratic institutions, lower life expectancy, and lower adult literacy (results available on request).

for all other explanatory variables is likely to be difficult, if not impossible (Imbens and Wooldridge 2009).

A related problem exists with time-trending factors that also correlate with climate. As shown in Figure 1, both temperature and conflict were generally trending up over the study period. Other conflict-relevant variables (such as per capita income and regime type) have also generally been improving since roughly 2000 in across the continent, and countless other variables (e.g. peacekeeping interventions, global export commodity prices, etc.) were also trending over time. These common time trends make it all too easy to assert a relationship between two particular trending variables, when in fact a third variable is the key causal driver.

Unfortunately, controlling explicitly for trends in variables such as income and political regime type likely introduces a second sort of estimation bias – simultaneity bias – that results from the two-way ("endogenous") causal relationship between these variables and armed conflict. For instance, slow income growth might make civil wars more likely, but civil wars are clearly also destructive to economic progress. Even using lagged income measures is unlikely to solve this endogeneity problem, because economic actors may anticipate the outbreak of war and adjust investment accordingly (Miguel, Satyanath, and Sergenti 2004).

Buhaug's solutions to these well-worn econometric issues are unsatisfactory and yield biased estimates of the coefficients in question. Buhaug either does nothing at all to control for either the time-invariant or time-varying factors correlated with conflict (as in his main results in Table 1) which leads to biased coefficient estimates due to the correlation between conflict and both time-invariant and time-trending variables.[2] Or he includes endogenous independent variables in his regression (and admits as such) in the form of lagged income in Table 3, which as any basic econometrics text explains biases *all* coefficient estimates in the regression (Wooldridge 2002). Furthermore, if the effect of climate on conflict occurs through changes in income, as past work has argued (Collier and Hoeffler 2004; Miguel, Satyanath, and Sergenti 2004), then including both income

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[2] As a "robustness check", Buhaug simply drops the fixed effects from the model. This amounts to checking whether our results are robust to increasing the extent of omitted variables bias.

and climate as regressors (as in all of Buhaug's Table 3) *should* reduce the estimated effect of climate on conflict, which appears to be what Buhaug finds and then misinterprets as evidence of a diminished role of climate. Buhaug's analytical approach thus reveals little of interest about the true causal relationship between climate and conflict.

Our solution to these issues is the standard approach in economics and econometrics (Wooldridge 2002). We use generic country indicator variables (called "fixed effects") to control for all time-invariant country characteristics, and generic linear time trends to control for other time-trending variables. Causal identification of the effect of climate on conflict is based on the fact that year-to-year variation in climate is essentially random, and thus exogenous to conflict conditions on the ground. One potential downside of the fixed effects approach is that it is difficult to estimate the direct impact of other potential explanatory variables of interest (e.g. perhaps consistently democratic countries have less conflict). The advantage – overwhelming, in our opinion – is that our coefficients of interest are unlikely to be biased by either omitted variables or endogeneity. Put another way, these "methodological fixes" are what is required to generate unbiased estimates of the effects of climate on conflict.

Results

Using this approach, our baseline model then relates the incidence of civil war to country-specific deviations from trend in temperature and precipitation. Using climate data from CRU (Mitchell and Jones 2005), Model 1 in Table 1 reproduces the original result in (Burke et al. 2009), with Model 2 updating these results with more recent Uppsala/PRIO conflict data as well as adding the few additional African countries missing from the original sample. In the updated sample, the contemporaneous effect of temperature declines slightly in magnitude but remains significant at the 90% confidence level, and the sum of the contemporaneous and lagged temperature effects is nearly identical to our earlier findings. Surprisingly, and contrary to the thrust of Buhaug's argument, running the same specifications with Buhaug's preferred climate data (discussed below) significantly strengthens the estimated relationship between temperature and conflict (Models 3 and 4): a 1 degree C increase in temperature results in a greater than 10% increase in the historical risk of conflict, which is roughly double what we estimated in our earlier work, and this effect is significant at over 95% confidence. Buhaug's climate data, when used within a sound econometric framework with fixed effects, strengthens rather than undermines our results.[3]

To address Buhaug's concern that our previous work made arbitrary assumptions about the functional form of the time trend controls, Table 2 explores robustness to alternative approaches to controlling for these trends. Our temperature coefficient is robust to whether or not we control for country-specific time trends (columns 1 and 2) or a common time trend across countries (columns 3 and 4), and to whether these trends are linear (columns 1 and 3) or quadratic (columns 2 and 4). Thus the specific functional form of the time trend controls does not affect our results.

A further concern raised by Buhaug is that our original study focused only on the incidence of large wars (those years in which a given war resulted in >1000 battle deaths), whereas many earlier benchmark studies focused on conflict onset (e.g. Fearon and Laitin (2003)) or on smaller wars with fewer casualties (e.g. Miguel, Satyanath, and Sergenti (2004)). Buhaug's preferred coding of civil war focuses on the onset of conflicts that eventually accumulate 1000 battle deaths.

We have four responses to this concern. First, there is no theoretical reason *a priori* to prefer one measure to another, in our view, and arguably our initial focus on the incidence of the largest wars captures the relevant outcome of interest to policymakers. Our focus tackles the central question in the ongoing debate on climate and conflict: can climate help explain the occurrence of the most destructive wars? Second, Buhaug's preferred coding of civil war *onset* is arguably a less attractive interpretation of the likely effects of climate variables on conflict: if climate shocks depress rural incomes and lower the opportunity cost of joining rebellions (Miguel, Satyanath, and Sergenti 2004), the years in which small wars become larger wars – i.e. the years in which negative climate shocks induce more people to rebel – are of particular interest. Smaller-scale conflict might be more consistent with "narratives of environmental marginalization", as

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[3] Buhaug does not report these specifications. He uses his own climate data only in his Table 3, which employs logit specifications and include likely endogenous regressors (e.g. GDP per capita lagged one period) and – in the absence of fixed effects – a quite narrow set of other controls.

Buhaug argues, but are almost certainly less destructive than the years of serious conflict that our coding attempts to capture. Third, interpreting our measure of civil war as "unorthodox" is to misread decades of political science research on the subject. The 1000 battle death threshold has long been the standard in the well-known Correlates of War data, as well as in earlier manifestations of the PRIO data. From a historical perspective, the more recent 25-battle-death measure would appear a more significant challenge to orthodoxy.

Finally, estimating the effects of climate on small conflicts is less a robustness test than an answer to a somewhat different question. Concluding that climate has no effect on small conflicts is interesting in its own right, but not obviously relevant to the relationship between climate and large wars.[4] As noted above, there are many reasons to suspect that these larger wars could be differentially affected by climate. Nevertheless, in Table 3 we explore how both large and small wars respond to climate. Similar to Buhaug, we find a weaker relationship between temperature and the incidence or onset of all wars with at least 25 battle deaths (Models 3-4), where onset is coded as the first year in which a particular conflict crosses the battle-death threshold of interest. However, consistent with other recent studies (e.g. Hendrix and Salehyan (2010); Witsenburg and Adano (2009)), we do find a positive relationship between contemporaneous rainfall and the incidence of all conflicts (small and large wars), and thus climate changes might still be important drivers of these conflicts.

As Table 3 shows, our climate data do successfully predict the onset of large wars as well as their incidence (Models 1 and 2). We retain similar temperature coefficient estimates whether we drop from the sample the years following onset in which the war continued (as in Miguel, Satyanath, and Sergenti (2004), and as shown in Model 2), or code all non-onset years as "0" and keep them in the sample (as in Fearon and Laitin (2003), not shown). Thus positive temperature deviations appear to explain both when

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[4] Analogously, a finding that climate has little effect on non-African civil wars (which indeed is what the country data seem to show) is interesting, but tells us little about climate and African civil wars – the purported topic of interest. As with the relationship between climate and large wars, there are reasons to expect that climate would exert disproportionate pressure on African economic and political outcomes relative to other regions.

wars become large, and whether they continue to be particularly destructive over time. This set of findings refutes Buhaug's claims about climate and large wars.

Finally, Table 4 explores the sensitivity of our results to the use of an alternative, independent climate dataset from Matsuura and Willmott (2009) – the "UDel" data. Model 1 in Table 4 re-runs our baseline specification over the 1981-2002 period using the UDel data, yielding very similar results to the CRU. Buhaug's analysis retains the CRU temperature data but (somewhat inexplicably) replaces precipitation with data from GPCP (Adler et al. 2003). As noted above, using this combination of datasets in our preferred specifications yields a much stronger relationship between climate and conflict than we had reported in our earlier work. Consistent with similar sensitivity tests reported in our original work, our results are not an artifact of the specific climate dataset.[5]

Unlike our baseline climate dataset (CRU 2.1), the UDel data allow us to extend our analysis through 2008. We do not think that changing the study time period constitutes a "robustness test" but rather an answer to a slightly different question: i.e., were African economic and political institutions as sensitive to variation in climate over the most recent decade as they were over the previous two? Africa was clearly a different continent over the last decade compared to the two decades previous (Miguel 2008). As Table 5 shows, average annual per capita GDP growth over 2003-2008 was six times higher than the 1981-2002 period (where it was near zero), and the Polity Score (a -10 to +10 measure of democratic political institutions) improved an average of more than 4 points between the two periods. If there are effects of income growth or regime type on the risk of conflict, as past studies have suggested (Miguel, Satyanath, and Sergenti 2004; Fearon and Laitin 2003; Collier and Hoeffler 2004), then these changes would reduce Africa's overall conflict propensity. As shown in Figure 1, this propensity does seem to have fallen over the last few years across the continent, at least for large wars.

Our results for the most recent decade are consistent with this view. Model 2 in Table 4 suggests that the relationship between climate and conflict over the 2003-2008

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[5] We conducted similar tests in the original work with two additional alternative datasets (GPCP and NCC), with results reported in the supplementary material. Our results over the 1981-2002 period are robust to these different data as well.

period is not significantly different than zero at standard confidence levels. The reasons for this apparent decline in sensitivity to climate remain unclear, however, and it's possible that other factors beyond strong economic performance or democratization might have also played a role in this apparent decline in sensitivity to climate, such as the recent increase in international peace-making and peace-keeping efforts across Africa (Human Security Report Project 2007). Understanding what exactly occurred to reduce the sensitivity of African conflict to hot temperatures and low rainfall, and whether these changes are likely to be fleeting or permanent, is thus of paramount importance.

Conclusion

Overall, we find little support for Buhaug's methodological criticisms of our earlier work, with our original results largely insensitive to credible controls for other explanatory variables, for alternative codings of major wars, or to different choices of climate datasets. Buhaug's dubious econometric choices in many of his specifications – including his decision not to control for country fixed effects or deal adequately with time trends in many specifications, or his willingness include endogenous regressors that bias all of his coefficients – further call into question his results. This of course does not imply that climate is solely "to blame" for African civil wars, as Buhaug's provocative title would suggest that we are arguing. Rather it implies that during a particularly violent recent period in African history, variation in climate was a significant contributor to the incidence of large, destructive civil wars. We believe that this relationship is both robust and of significant interest to policy-makers tasked with reducing the incidence or impact of future conflicts.

We do confirm that the climate-conflict relationship has weakened substantially in Africa in recent years, and are hopeful that whatever economic and political changes have occurred will persist in the coming years, thus lessening the potential adverse impacts of future warming on African conflict. However, it is equally possible that the coming decades in Africa will resemble the 1980s and 1990s more than the last decade, and thus understanding the causes of these recent changes is a critical area for future research.

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Table 1: Results with original and updated sample, 1981-2002. Model (1) uses the original sample in ref (1), and model (2) an updated sample with the most recent Uppsala/PRIO data (v4, 2009) and three additional African countries missing from the original sample (Equatorial Guinea, Eritrea, and Mauritius). Models (3) and (4) use Buhaug's climate data for the original and updated PRIO data (the sample size reflects the observations for which climate data were available in the Buhaug data). All models include country fixed effects and country-specific time trends, with Huber-White standard errors adjusted for clustering at the country level.

	(1) Civil war	(2) Civil war	(3) Civil war	(4) Civil war
temperature	0.043*	0.035*	0.063**	0.073**
	(0.022)	(0.018)	(0.028)	(0.032)
temperature lag (1 year)	0.013	0.022	0.043*	0.057
	(0.023)	(0.028)	(0.025)	(0.035)
precipitation	-0.023	0.009	0.020	0.038
	(0.052)	(0.037)	(0.075)	(0.076)
precipitation lag (1 year)	0.025	0.024	0.097	0.117*
	(0.049)	(0.035)	(0.062)	(0.069)
Constant	-1.127	-1.194	-2.423**	-3.061**
	(0.780)	(0.870)	(1.103)	(1.502)
Number of Observations	889	968	867	867
R squared	0.657	0.641	0.668	0.650

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Models with country-specific or common time trends, linear and quadratic. Data are the updated sample from Table 1, 1981-2002. All models include country fixed effects, with Huber-White standard errors adjusted for clustering at the country level.

	(1) Civil war	(2) Civil war	(3) Civil war	(4) Civil war
temperature	0.035*	0.035*	0.037*	0.036*
	(0.018)	(0.020)	(0.020)	(0.020)
temperature lag (1 year)	0.022	0.022	0.021	0.021
	(0.028)	(0.027)	(0.027)	(0.027)
precipitation	0.009	0.020	0.021	0.021
	(0.037)	(0.046)	(0.049)	(0.050)
precipitation lag (1 year)	0.024	0.041	0.009	0.008
	(0.035)	(0.043)	(0.048)	(0.048)
Constant	-1.194	-1.144	-1.231	-1.102
	(0.870)	(0.898)	(0.900)	(0.883)
country-specific time trends	Yes	Yes	No	No
country-specific time squared	No	Yes	No	No
common time trend	No	No	Yes	Yes
common time squared	No	No	No	Yes
Number of Observations	968	968	968	968
R squared	0.641	0.675	0.473	0.473

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Alternate coding of civil war. Model (1) is our standard coding of civil war incidence (years with $>$1000 battle deaths), Model (2) is large war onset; Models (3) and (4) repeat the exercise for large and small wars combined (all years with $>$25 battle deaths). Data are the updated sample from Table 1. All models include country fixed effects and country-specific time trends, with Huber-White standard errors adjusted for clustering at the country level.

	(1) Civil war	(2) War onset	(3) Any war	(4) Any onset
temperature	0.035^{*}	0.034^{*}	0.014	0.007
	(0.018)	(0.018)	(0.037)	(0.026)
temperature lag (1 year)	0.022	0.000	-0.018	-0.022
	(0.028)	(0.022)	(0.030)	(0.027)
precipitation	0.009	-0.004	0.102^{*}	0.028
	(0.037)	(0.039)	(0.055)	(0.049)
precipitation lag (1 year)	0.024	0.006	0.047	-0.008
	(0.035)	(0.020)	(0.048)	(0.042)
Constant	-1.194	-0.837	-0.022	0.173
	(0.870)	(0.692)	(1.239)	(0.752)
Number of Observations	968	889	968	783
R squared	0.641	0.226	0.648	0.236

Standard errors in parentheses

$^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Table 4: Results using UDel climate data, for periods 1980-2002 (Model 1) and 2003-2008 (Model 2). All models include country fixed effects and country-specific time trends, with Huber-White standard errors adjusted for clustering at the country level.

	(1) Civil war	(2) Civil war
UDel temperature	0.042^{**}	-0.012
	(0.020)	(0.019)
UDel temperature lag	0.004	-0.024
	(0.026)	(0.041)
UDel precipitation	0.030	0.083
	(0.028)	(0.079)
UDel precipitation lag	0.047^{*}	0.035
	(0.026)	(0.034)
Constant	-0.997	1.084
	(0.941)	(1.432)
Number of Observations	1056	288
R squared	0.645	0.592

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Early and late period GDP per capita growth and Polity score, average across Sub-Saharan Africa. GDP data are from Penn World Tables (Heston et al 2009), and Polity data from the PolityIV Project (Marshall et al 2009)

	1981-2002	*2003-2008*
GDP per capita growth	0.5	2.8
Polity Score (-10 to 10)	-2.5	1.8

Figure 1: Trend in Sub-Saharan African conflict and temperature, 1960-2008. Dark red indicates number of large wars (>1000 deaths) ongoing in a particular year, and light red the number of small wars (>25 deaths) ongoing, based on the Uppsala/PRIO data. Blue line indicates continental average temperature (right Y-axis, from UDel data).