Climate Change, Weather Shocks and Violent Conflict:

A Critical Look at the Evidence

Jeroen Klomp and Erwin Bulte

Development Economics Group

Wageningen University

Box 8130, 6700 EW Wageningen, Netherlands

Phone: +31 317 485286, Fax: +31 317 484037,

Email: Jeroen.klomp@wur.nl and erwin.bulte@wur.nl

Selected Paper prepared for presentation at the International Association of Agricultural Economists (IAAE) Triennial Conference, Foz do Iguaçu, Brazil, 18-24 August, 2012.

Climate Change, Weather Shocks and Violent Conflict:

A Critical Look at the Evidence

Jeroen Klomp and Erwin Bulte

Development Economics Group
Wageningen University
Box 8130, 6700 EW Wageningen, Netherlands
Phone: +31 317 485286, Fax: +31 317 484037,
Email: Jeroen.klomp@wur.nl and erwin.bulte@wur.nl

Abstract: We use cross-country data to explore whether temperature and rainfall shocks trigger violent conflict, or not. We include a wide range of country and time samples, and explore whether the impact of weather shocks is conditional on income or political regimes. Our overall conclusion is sobering. Notwithstanding the attention this topic has attracted from the media and policy makers, we find little robust evidence linking weather shocks to the onset of conflict.

Keywords: climate change, temperature, rainfall, violence, Africa, resource scarcity.

1. Introduction

Two widely-publicized theories consider the relation between natural resources and violent conflict. The first one focuses on the implied incentives of an *abundance* of natural resources to engage in fighting, and the second one examines how resource *scarcity* affects conflict. While at first sight it might seem odd to identify both abundant and scarce resources as determinants of conflict, both theories could be true simultaneously. The former theory is usually associated with exhaustible resources (e.g., minerals), and fits into the overarching paradigm of the so-called resource curse hypothesis. The latter theory is most often associated with renewable resources (e.g., water, land), and has gained prominence because it links up with concerns about the consequences of global climate change.

Both the abundance and scarcity theory have been scrutinised by academics. For example, a special issue of the *Journal of Conflict Resolution* (August 2005) examines the resource curse hypothesis in detail, and more recently a special issue of the *Journal of Peace Research* (January 2012) probes the relation between climate change and conflict. The general message emerging from these efforts is mixed (see next section). The evidence linking resources – be they scarce or abundant – to conflict is not strong, or even ambiguous. This sobering conclusion evades most policy makers and the popular media. The press continues to eagerly report about "blood barrels," efforts to regulate the international trade in "blood diamonds," or brutalities in the DRC apparently motivated by the desire to mine the soil for precious coltan. Similarly, high profile policy makers, prominently including Kofi Annan, Ban Ki-Moon and Barack Obama, have on various occasions, stated that climate change is a threat to peace and security.

In this paper we re-examine the link between weather shocks and the onset of conflict. Advocates of the "resource scarcity causes conflict" thesis argue that weather shocks may affect conflict patterns by lowering the productivity of rainfed agriculture or cattle herding – this would lower the opportunity cost of engaging in conflict, and might aggravate tensions or intensify competition for dwindling resources. Other have pointed out matters may be not so simple, and argue that weather shocks depress both the opportunity *cost* of fighting as well as the *value* of the contested prize (think of African rangeland, a case analysed by Butler and Gates 2012). It is easy to construct theoretical models predicting that conflicts occur when weather conditions are favorable, as abundant resources imply strong incentives to engage in fighting.

The issue of weather shocks and conflict is becoming more important because climate models project weather variability to increase. While considerable uncertainties about magnitudes and sometimes even directions of change remain,[1] it is likely that local temperatures and rainfall patterns will change in the future (e.g., IPCC 2007). Building on this literature we consider three different weather variables: local temperatures, rainfall, and global temperature (associated with the El Niňo phenomenon). We use both absolute temperature and rainfall levels, as well as measures of temporal variation ("standard deviations").

[1] Gleditsch (2012) writes that "current climate models and even data for the past few decades leave much to be desired in terms of forecasting accuracy and geographical precision."

While existing work on heterogeneous treatment effects is scanty, there is tentative evidence that weather variability interacts with other factors to trigger conflict. For example, the impact of drought or temperature shocks on conflict may be conditional on the quality of institutions (Tir and Stinnett 2012) or political regimes (Koubi et al. 2012). To examine heterogeneous treatment effects in more detail we consider multiple country and time samples, and seek to stratify countries in our sample based on institutions and income. We also consider two measures of conflict, capturing both medium-intensity conflict and outright civil war.

Overall, our results paint a nuanced picture, suggesting that variation in weather variables (at least, of the magnitude as experienced until now) is unlikely to invite conflict for the great majority of countries. However, we also detect variation over time, and find that some countries are more sensitive to the disturbing effects of climate shocks than others.

The paper is organised as follows. In section 2 we briefly discuss major contributions to the extant literature on resources and conflict, focusing on the theory that (climate change-induced) scarcity of resources is a factor explaining the onset of violence. In section 3 we summarise our data and outline our identification strategy, paying special attention to how we seek to probe heterogeneous treatment effects. Section 4 contains our main results, and in section 5 we draw some conclusions and place our findings in the larger context of resources, grievances, and conflict.

2. Literature

Across the board, climate change is expected to exacerbate the scarcity of renewable resources (even if locally they may become more abundant). According to Homer-Dixon (1999) increased scarcity could result in frustration and social unrest. Matters would be worse if people are forced to migrate due to extreme weather events (or, say, sea level rise), as this would intensify competition for resources elsewhere, possibly inviting inter-group tensions and conflict. In-depth case studies have produced mixed evidence for this line of reasoning. While Baechler et al (1996) find anecdotal evidence that links environmental degradation to conflict, other studies find no such effects (Benjaminsen et al. 2012) or even opposite patterns in the data (e.g. Witsenburg and Adano 2009, Theisen 2012, Adano et al. 2012).

As will become clear below, large N-country studies also fail to come to a consensus. Broadly speaking we may distinguish between two categories of large N-studies – those focusing on long-term trends (often based on measures of environmental degradation) and those focusing on inter-annual variation in weather conditions. The former category closely fits the scarcity narrative, but has the disadvantage that measures of environmental degradation are typically not exogenous to human management. Hence, endogeneity concerns might eventuate. To circumvent such problems, we base our analysis on inter-annual variation in weather conditions – variables that are exogenous to the determination of specific conflicts, and have the advantage they can be easily linked to projections of climate change.

Before discussing the most important cross-country evidence on the relation between climate change (or weather shocks) and conflict in more detail, it is useful to emphasise that the literature distinguishes between different types of violence. These include inter-state war, civil war, social conflict, and non-state violence (or inter-community violence). The distinction between these categories is not trivial, and results for one type of conflict do not necessarily spill over to other types (e.g. Raleigh and Kniveton 2012, Buhaug 2010). The most-often used proxy defines conflict as events causing the death of at least 25 persons, and often (but not exclusively) analysts zoom in on events involving the state as one of the warring parties. We will return to our conflict measures in section 3.

2.1 Rainfall and conflict

The relation between rainfall and conflict has become a prominent research issue because of a paper that, strictly speaking, focused on something else. Miguel et al. (2004) are not interested in the effect of rain on conflict *per se*, but rather on the causal effect of economic performance on conflict. Recognising that income should enter as an endogenous variable in models explaining the onset or incidence of conflict, they seek to instrument for income. Since income in Africa depends to a large part on agriculture, and since the returns to African farming vary with erratic patterns of rainfall (due to limited opportunities for irrigation), they use rainfall as an instrumental variable for income. Specifically, they regress income growth on rainfall growth (and lagged rainfall growth), arguing that positive or negative rainfall growth represents a favourable or unfavourable productivity shock. Their main result is that positive rainfall growth is associated with higher incomes which, in turn, translates into less conflict. This result has been dubbed the "cornerstone of the literature on the economics of civil conflict" (Ciccone 2011, p.215)

While the instrumenting strategy may be successful, at least for the period studied by Miguel et al, we should be careful when interpreting the *reduced form* evidence – the coefficients from a model directly linking the onset of conflict at time *t* to rainfall growth between *t*-1 and *t*-2. It is tempting to conclude that drought shocks cause conflict, but this conclusion may be erroneous. Ciccone (2011) emphasises that rainfall levels are strongly mean-reverting: positive or negative rainfall shocks are likely to be followed by a reversal to average values. Positive growth in year-on-year rainfall may thus reflect a positive rainfall shock, or may be a reversal to normal conditions following a negative shock. To test which story fits the data, Ciccone regresses conflict on (lagged) rainfall *levels* rather than rainfall *growth*. He argues "if conflict was triggered by lower rainfall levels or negative rainfall shocks, the negative correlation should have been due to a significantly negative correlation between conflict in *t* and rainfall levels in *t*-1" (p.216). This turns out to be not true. Instead, and consistent with the mean-reversion nature of rainfall data, conflict is associated with positive rainfall shocks at *t*-2. Ciccone concludes the correlation between conflict and negative rainfall growth is driven by a positive correlation between conflict in *t* and rainfall levels in *t*-2.

This might seem like a puzzling result, flying in the face of the resource scarcity hypothesis. Ciccone himself refers to the positive correlation between conflict and lagged rainfall levels as "counterintuitive." Miguel and Satyanath (2011), in a reply, counter they "have yet to encounter a micro study that associates better rainfall with more conflict" (p. 232). However, and as mentioned above, various micro studies studying livestock raiding in semi-arid rangelands of Western and Eastern Africa report evidence that places the scarcity hypothesis on its head. For example, Witsenburg and Adano (2009) find that livestock raiding is more violent during wet seasons when pasture and water are abundant and when the livestock is in good health (see also Theisen 2012 and Benjaminsen et al 2012).[2] The relation between rain and conflict remains ill understood.

2.2 Local temperatures and conflict

Zhang et al. (2007) explore how long-term cycles of temperature change affected conflict in the pre-industrial period (1400-1900), and find that variation in temperature is correlated with the frequency of wars in Europe and China. Specifically, war frequency increases (and population declines) in relatively cool periods. The proposed mechanism linking these factors is the impeding effect of cooling on agricultural production.[3] If we accept that the land's carrying capacity is a linking pin between temperatures and conflict, then increases in local temperatures should be correlated with the onset of war in tropical countries. After all, "hot years" in the tropics are associated with lower agricultural production (as are cold years in temperate zones).

In a much-publicised paper, Burke et al. (2009) indeed find a strong positive correlation between local temperatures and armed conflict in sub-Saharan Africa. They use a fixed effects panel regression that links the incidence (not the onset) of major civil

[2] Note that Witsenburg and Adano (2009) relate conflict to current rainfall, rather than lagged rainfall levels, as done by Ciccone (2011).

[3] This also explains why the adverse effects of cool periods weaken in the industrialised world (Tol and Wagner 2010). As societies industrialise and develop economic sectors that do not directly depend on primary production, Malthusian constraints become less binding.

wars to current and lagged temperature (and time-varying controls, in some models). Their findings were staggering. A 1% increase in temperature leads to a 4.5% increase in civil war in the same year, and a 0.9% increase in the next year — representing a 49% relative increase in the incidence of war. Combining these regression results with temperature projections for the next decades, Burke et al. (2009, p. 20670) predict a "54% increase in armed conflict incidence by 2030, or an additional 393,000 battle deaths if future wars are as deadly as recent wars." They warn the adverse effects of warming are likely to "outweigh any offsetting effects of strong economic growth and continued democratization" (p. 20673). These dismal outcomes imply support for extra international efforts to improve the capacity of African farmers to cope with heat, and justify the up-scaling of insurance schemes or emergency assistance.

Buhaug (2010) is sceptical about this analysis and its implications. In response, he (i) considers a broader variety of conflict measures (i.e., not just the incidence of major civil wars), (ii) focuses on the onset (rather than the incidence) of conflict, and (iii) uses a more extensive series of control variables to replace the fixed effects and time trends employed by Burke et al. Regardless of the "fix" that he adopts, the temperature-conflict link disappears – higher temperatures are not (robustly) linked to conflict. Moreover, he demonstrates that the results of the Burke et al. specification do not extend to a dataset that includes the most recent conflict-temperature observations. Buhaug concludes "under a host of alternative measures of drought, heat, and civil war, under various model specifications, this paper concludes that climate variability is a poor predictor of armed conflict" (p. 16477). This perspective, in turn, is rejected by

Burke et al. (2010), who argue that their model specification is superior to that of Buhaug. So the debate about temperature and conflict, like the one about rain and conflict, continues.

2.3 Global temperature and conflict

In a recent publication that attracted a lot of media attention, Hsiang et al (2011) claim that *global* temperature shocks may drive the onset of civil war. They correlate the onset of conflict to the so-called El Nino/Southern Oscillation (ENSO) phenomenon. ENSO explains quasi-periodic and large-scale changes in sea surface temperature in part of the Pacific Ocean, affects atmospheric circulation patterns, and thereby influences weather patterns in many countries. Based on historical climate data they distinguish between so-called "teleconnected countries," directly affected by ENSO effects, and so-called "weakly-affected countries," that are not directly affected. Hsiang and colleagues then proceed by correlating the annual risk of conflict during the period 1950-2004 for these two groups of countries to an annual ENSO index. A significant correlation between the ENSO index and the risk of conflict eventuates for the group of teleconnected countries, but not for the weakly-affected ones. They conclude that "ENSO may have had a role in 21% of all civil conflicts since 1950" and that "this result … is the first demonstration that the stability of modern societies relates strongly to the global climate."

Given the lack of a consensus regarding the impact of local temperatures on the onset of local conflict, it seems puzzling to try and relate such conflicts to variations in global temperature. In the absence of clear evidence that local weather matters – arguably directly affecting the opportunity cost of time of potential rebels, and possibly accentuating local grievances – why would the global climate drive civil war? Hsiang and colleagues argue that the global climatic patterns may be a driver of conflict because the global climate invites certain transboundary effects. "This is possible if non-local processes such as increasing global commodity prices or conflict contagion strongly influence local conflict risk." Maybe so, but this introduces the question why these effects are confined to the sample of "teleconnected countries"? Why are "weakly affected" countries not only insulated from El Nino, but also from its destabilizing spill-over price effects?

It seems fair to conclude that the confusion characterising the nature of the weather-conflict relation extends to the issue of global temperatures. Moreover, since Hsiang et al. use an unconventional model specification and adopt some unorthodox coding choices,[4] it seems prudent to probe the robustness of their findings in a more standard model setting.

3. Data and Identification Strategy

In this section we will summarize our data, and outline our identification strategy.

[4] Specifically, the authors ignore the state and duration dependence of conflict. Their regression models seek to explain the onset of violence—a binary variable. The start of a new conflict is coded as a 1, and peace years are coded as a 0. However, their onset dummy also receives a value of 0 in years with on-going conflict, which introduces a failure to distinguish between *on-going conflict* and *peace*. Such an assumption is not innocuous because conflicts starting in year 1 cannot start again in year 2 (at least not when involving the same parties). The analyst should control for this. The literature contains various suggestions for remedying this problem. Most dramatically, observations for on-going conflict can be dropped from the analysis (Collier and Hoeffler 2004). Another approach is to code on-going conflict by a zero and simultaneously include an extra (dummy) variable to control for the incidence of "prior conflict" (Fearon and Laitin 2003). Finally, the analyst can model on-going conflict by a zero while also including a vector of additional controls capturing the time since the last conflict ("peace years") and higher-order peace years terms (see Beck et al. 1998, and Koubi et al. (2012).

3.1. Data

Data on civil conflict and their impact are documented in the UCDP/PRIO Armed Conflict Dataset from the Peace Research Institute Oslo. This dataset has worldwide coverage and includes about 1900 conflicts for the 1946-2009 period. Civil conflict is defined as an event involving distinct parties resulting in at least 25 battle deaths annually. In our analysis, we focus on internal civil conflicts, or conflict between the government ("state") and one or more internal opposition group(s), possibly involving intervention or support from other states (secondary parties). However, and as a robustness check, we have also repeated all estimations using the 1000 battle deaths threshold, conventionally used to identify major conflict or war. Figure 1 shows the distribution of the onsets of civil conflicts between 1950 and 2009. On average, four new conflicts start each year. In the period 1989 to 1998, conflict onset peaks with on average 9 new cases. This is mainly caused by the break-up of many communistic regimes in eastern Europe and Asia. In total, we count some 220 internal conflicts between 1960 and 2009, which is our period of analysis.

[Insert Figure 1 about here]

Turning to our weather variables, we use temperature data reported by the National Centers for Environmental Prediction (NCEP) Climate Data Assimilation System 1 (CDAS1) and precipitation data from the Combined Precipitation Dataset of NASA's Global Precipitation Climatology Project. Both data sets provide worldwide monthly climate data at 0.5 x 0.5 degree resolution since 1950. These data are summarised in Figures 2-3, which shows the global mean temperature and rainfall between 1950 to

2006.[5] There is an increasing trend in temperature of about 0.02°C annually, and average global temperature is approximately 18.7°C. The variation between countries is considerable, as is evident from the relative standard deviation (which is 0.37). The highest temperature is observed in Mauritania, with an average temperature of 28.4 ºC, and the coldest country is Mongolia, with an average temperature of -1.8 ºC. Turning to the rainfall data, we observe a declining trend. Average global rainfall is 1104 mm, which goes down by 1.5 mm per year. Again there are large differences between countries – the relative standard deviation equals 0.66. Costa Rica is the wettest country in our sample, with a year average of 4874 mm. In contrast, the lowest level of precipitation is in Egypt, which is only 19 mm per year. The correlation between the temperature and the precipitation is about 0.29, so they measure different dimensions of the climate in a particular country.

[Insert Figure 2 and 3 about here]

3.2 Model

We use a standard model to examine the relationship between climate change and the onset of conflict. Our dependent variable is binary, and takes the value 1 if conflict starts in a particular country and year (and 0 otherwise). Our results are based on an unbalanced panel that includes data from about 170 countries between 1960 to 2008, and we use a conditional fixed effects logit model:[6]

---

[5] Weighted by population.

[6] Appendix A1 lists the countries used in our analysis, and provides the first year in which they appeared in our dataset. Besides, we report the number of onsets of conflict in these country between 1960 and 2008. We have also estimated a series of Probit models, and this produced similar outcomes.

$$y_{it}^{*} = x'_{it}\beta + \alpha_i + u_{it} \quad (1)$$

Outcome *y* (the onset of conflict) depends on observed variables (*x*), unobserved individual (country) characteristics (α), and a random error term (*u*). Beck et al. (1998) argue that data on political events, like civil conflicts, are duration dependent, so we employ their remedy to test and correct for duration dependence (adding cubic splines, along with a count variable for the number of years since the last conflict).[7] The probability to observe the start of conflict is given by:

$$P(y_{it} = 1) = \frac{1}{1 + e^{-(x'_{it}\beta + \kappa_{t-t0})}} \quad (2)$$

Where κ_{t-t0} are the duration dependence parameters. In essence, the conditional fixed effects logit estimator compares all observations within a given country when a conflict starts with all observations without the onset of conflict. We test for the appropriate panel data model using the Hausman test, testing the null-hypothesis that all country fixed effects equal zero by comparing the estimates of a conditional fixed effects logit model and the unrestricted (pooled) logit model. Supporting our choice for a conditional fixed effects logit specification, the null-hypothesis of no country specific effects is rejected for all models. The estimated model reads as:

$$conflict_{it} = \alpha + \gamma climate_{it-l} + \beta_j x'_{jit-l} + \eta_{iT} + \eta_i + \eta_t + u_{it} \quad (3)$$

[7] The approach is equivalent to including a series of dummy variables that "count" the number of non-crisis years. Thus, there is a dummy variable coded one for country-years without major crisis for exactly one year and 0 otherwise; another dummy coded one for country-years without crises for exactly two years and 0 otherwise, another for three years without crisis, etc. Beck et al. (1998) suggest three splines are sufficient. The empirical test for the relevance of temporal dependence is an F-test on the splines and the years since the last crisis. For our sample the temporal dependence variables are jointly significant at the 1% level in all estimations.

where *conflict* is one in country *i* at year *t* when a civil conflict starts; *climate* refers to our (lagged) temperature or precipitation variables in a particular country-year. As discussed above, there is considerable uncertainty about the sign of the associated coefficients. Vector x'_j is a set of control variables commonly used to explain conflict, and includes lagged real per capita GDP (in logarithms) and the Polity IV score as a proxy for the level of democracy. Next, η_i and η_t are, respectively, country and time fixed effects, and η_{iT} is a country-specific time trend. Finally, u_{it} is an error term.

One contribution of our study is that we explicitly address temporal variation in the relation between weather variables and the onset of conflict. Buhaug (2010) pointed to the possibility there may be structural breaks in the conflict data generation process (post-2002 conflict data appear "different" from pre-2002 data), and Ciccone (2012) makes a similar observation for the relation between rainfall and conflict. We adopt a rigorous approach to probe the temporal robustness, and use a so-called rolling regression (see, for example, Foster and Nelson 1996, and O'Reilly and Whelan 2005). The idea is to repeat the same regression model for multiple samples. Specifically, we use a 30-year "rolling window" for our panel estimations, and for every regression we add one observation at the end of the sample and drop one at the beginning.[8] Hence, we re-estimate the same model multiple times, for a slightly revised (30-year) sample period. We first document the significance of the weather variables for the sub-sample 1960-1979, then switch to the slightly more recent subsample of 1961-1980, and so on.

[8] In general, rolling regressions are more sophisticated than alternative models based on once-off breaks, considered by the Andrews-Quandt test. (O'Reilly and Whelan, 2005). A 30 year window implies we have sufficient observations per model and also a sufficiently large number of models to probe dynamic effects.

We continue this procedure until we finally solve the model for the subsample 1979-2008 so that, in total, we estimate 20 different (albeit overlapping) models. If there is no structural break in the relation between weather and the onset of conflict, the coefficient and standard errors should be relatively constant across subsamples. If, in contrast, the weather variable enters significantly for some early subsamples but not for later ones – or the other way around – then the relation between climate and conflict has changed over time. The "average" effect as measured in a single regression covering the entire period, would then be less relevant for policy makers as it under – or overestimates the true *current* effect. Our hypothesis is that the climate effect is not constant over time periods T, so that:

$$\gamma_{1960\text{-}1989} \neq \gamma_{1961\text{-}1990} \neq \cdots \neq \gamma_{1979\text{-}2008} \quad (4)$$

One might be concerned that cutting up the 1960-2008 sample into multiple 30-year subsamples implies a significant reduction in sample size, possibly reducing the power of the analysis and increasing the risk of a so-called type II error. However, while our approach reduces the sample size by some 45%, we are not concerned about low power. For example, the number of observations for all analyses reported in Figure 2 below exceeds 412, and on average it equals 912. Another issue concerns the nature of the results we generate. While our rolling regression window approach generates 19 different coefficients (and standard errors) for the climate variables, these cannot be treated as independent realizations. After all, model 1 (1960-1989) shares no less than 29 years with model 2 (1961-1990). This implies we should be careful when subjecting them to additional statistical analysis.

Finally, we are not only interested in how the relation between conflict and weather variables evolves over time. Importantly, the nature of this relation may also be conditional on local incomes and institutions. However, unlike time, which ticks away exogenously, the evolution of incomes and institutions is arguably endogenous with respect to the history of violence. To attenuate such concerns, we use both initial (1960) and average income and democracy levels over the study period. We line up countries based on these per capita income and democracy levels and again use a rolling regression approach. While initial income and democracy may affect the trajectory of violence that eventuates (and the nature of the relation between weather shocks and conflict), violence in the 1980s cannot affect income or democracy in 1960.

4. Results

4.1 Level effects

Let us now turn to the results, which are sobering. Column (1) of Table 1 summarizes the temperature results using all observations. To obtain robust standard errors we use the bootstrap estimator with 1.000 replicators.[9] As mentioned, this logit model includes country and time fixed effects (to control for time-invariant country specific factors and common shocks, respectively) as well as a country-specific time trend and time-varying controls (income and the Polity measure of political regimes). Including civil conflicts which cause at least 25 battle deaths, we find there is no statistical significant relationship between lagged temperatures and the onset of conflict. When re-estimating the

[9] In view of the unequal distribution of the availability of the data across the countries, we clustered the Huber-White standard errors. Furthermore, by weighting the observations, we make the data more representative of the world population to account for missing data. Missing data occurs more frequently for the less developed economies which are most influenced by climate conditions.

model for key region samples separately (Asia and Africa), we still do not find any significant impact.

In column (4) to (6) we replace our temperature measure by (lagged) precipitation levels. Now some (marginally) significant results emerge. For the pooled sample, when precipitation levels increase by 100 mm, the likelihood of the onset of conflict decreases by 0.5%. Not surprisingly, this aggregate effect is driven by the impact of changing precipitation levels in African countries (column 5). This latter result is consistent with the conclusion by Miguel et al. (2004), and we will probe its robustness below.[10]

In columns (7-9) we focus on global temperatures and include our ENSO variable. Across the board – lumping teleconnected and weakly connected countries – we do not find evidence of any significant effect. Focusing on Asia and Africa, continents where many of the teleconnected countries are found, we still do not find support for the claim that high global temperatures cause conflict. There is no effect for Africa, and the onset of conflict in Asian countries is lower in El Nino years.

[Insert Table 1 about here]

Until now we implicitly assumed parameters are stable over the study period. To examine this assumption in more detail we next report results for the rolling regressions, outlined above. Figure 4 summarizes how the (average) impact of temperature varies over

[10] This result is in contrast to Ciccone (2011), who finds a significant *positive* impact of (lagged) rainfall on conflict. One explanation for these diverging results is that Ciccone (2011) takes a logarithmic transformation of the rainfall variables. This is a routine procedure to deal with outliers, but in this case it made the dataset more skewed than the raw rainfall data (albeit now to the right, rather than the left side). This is confirmed by the Doornik-Hansen test for normality. If we include a twice-lagged rainfall levels, as Ciccone did, and don't take logs, then rainfall does not enter significantly.

time for the pooled sample, and also for sub-samples of Asian and African countries. More specifically: it reports t-values of the temperature variable for the various temporal subsamples, and also the threshold values indicating significance at the 10% and 5% confidence level (horizontal lines at ±1.64 and ±1.96, respectively). T-values below or above these thresholds indicate that, for that particular sub-period, there is a significant correlation between temperature and the onset of conflict. From Figure 2 is evident that there is no robust relationship between temperature and the start of conflict (even if, for Africa, there is one specific model predicting that the likelihood of the onset of conflict goes down as temperatures go up).

In Figure 5 we consider the evolving relationship between precipitation levels and the onset of a civil conflict. Interestingly, for rainfall the nature of the relation between rainfall and conflict appears to change over time. For the pooled sample, we observe a downward trend in the relevant t-values between 1960 until 2008. More specifically, rainfall enters significantly in only few of the models, concentrated towards the end of the sample. That is, following a positive rainfall shock, the likelihood of the onset of conflict goes down over time. For the African sample, too, we find that droughts are significant drivers of conflict, but only towards the beginning and end of the sample period. Additional research should identify the factors that are responsible for this evolving sensitivity for rainfall shocks (factors beyond the time trend and income and democracy controls already in the model). Note that the results in Figure 5 stand in contrast with the observation by Ciccone (2011) and Miguel and Satyanath (2011) that rainfall ceases to be significantly correlated with conflict for the latest, expanded dataset (extending to 2009, so covering the period 1979-2009). It appears as if a significant relation exists for the most recent years.

Finally, Figure 6 shows the impact of global temperatures on the probability of the start of conflict. Conflict in Africa appears unaffected by global temperatures. However, we find rather robust evidence for a negative correlation between global temperatures and the onset of violence in Asia. This finding is opposite to the results of Hsiang et al. (2010), who conclude that in El Nino years significant more conflicts break out than in La Nina years. When we further probe the robustness of the Hsiang et al. result and use their exact model specification and statistical program in a rolling regression framework (so not the standard logit model), we also do not find any robust result of the ENSO effect in Africa or Asia (detailed results are available upon request).[11]

[Insert Figures 4-6 about here]

4.2 Climate variability

One manifestation of (projected) climate change is the increased likelihood of extreme weather events. That is: in addition to changes in mean temperatures captured by trends in our key variables, the variability of the weather may increase. We now explore whether focusing on (changes in) volatility of weather patterns affects our results. We re-estimate the models but now include the standard deviation of temperature and precipitation variables as our measures of climate variability. We use a rolling window of 10-years to calculate the relevant standard deviation. The results, presented in Table 2,

[11] The results in Hsiang et al. (2010) are derived usinguse a user-written program in STATA based on a weighted OLS regression. This program can be downloaded from www.nature.com/nature.

suggest an overall significant negative impact of temperature variability on conflict. This aggregate effect is mainly driven by Asian countries. Temperature volatility has no significant effect in African countries. Turning to precipitation and ENSO variability, we find no significant impact on the onset of conflict.

[Insert Table 2 about here]

In Figures 7-9 we present new results for our rolling regression models. We find distinct patterns in the data. While temperature volatility has no significant impact on the start of conflict in Africa, we observe a declining trend for the sample of Asian countries (and the total sample). Casual interpretation of this finding would suggest that, over time, Asian societies become less sensitive to temperature volatility – the most recent time samples even indicate that higher volatility reduces the likelihood of conflict. However, this interpretation would be misleading, as becomes evident from additional analysis. The declining trend is caused by the inclusion of former Communistic countries added to the sample since the end of the eighties, and the break-up of Yugoslavia at the beginning of the nineties. Recall that we are estimating an unbalanced sample, and while this does not matter for the rest of our results,[12] we do find that the enlargement of our sample affects the results for temperature volatility. Many of the new countries in the sample have suffered from conflict in the post 1990 period, but these conflicts are usually not attributed to climate change or weather shocks. This latter result is confirmed when we include only the countries for which we have data from 1960 onwards.

[12] Specifically, neither the level effects discussed in section 4.1, nor the volatility effects for rainfall and ENSO change (in a qualitative sense) when we restrict the analysis to a balanced panel for which we have data starting in 1960.

These results show that there is no significant trend in the impact of temperature shocks (results not reported, but available upon request).

When we have a closer look at volatility in ENSO using the rolling regression approach, Figure 9, we cannot detect general patterns. Most of the samples do not display significant effects. Only when the sample includes the 90s (and therefore excludes the 60s) there is a significant positive impact of ENSO shocks in Africa. Moreover, at the end of the 90s we document a short period with negative associations for the Asian countries.

[Insert Figures 7-9 about here]

4.3 Heterogeneous treatment effects

We now test whether the relation between weather shocks and conflict differs across different types of countries. For this reason we line up the countries from the poorest to the richest, and from the least democratic to the most democratic (using average or initial income and democracy levels). We start our analysis by organising countries based on their Polity IV score. In the first sub-sample we include only those countries with a Polity score below (or equal to) -7. In the next sub-samples we then increase the Polity score by one point, and delete the observations from the bottom of the distribution. That is, while the first sub-sample include countries with scores between -10 and -7, the next subsample considers countries scoring between -9 and -6, and so on.

Results in Figure 10 show that, for the great majority of countries, none of the climate variables has a significant impact on the onset of conflict. However, and interestingly, it also appears as if autocracies are vulnerable to negative temperature shocks (the extreme left-hand side of the distribution: in autocracies conflict is more likely following exceptionally *cold* years). In contrast, in the most democratic subsamples we find evidence of a negative correlation between rainfall and conflict. None of these patterns in the data is easily aligned with existing theories on weather shocks as a determinant of conflict. Qualitatively similar results emerge when we use initial, rather than average, income.

In Figure 11 we divide countries into subsamples based on average per capita income.[13] Each subsample includes 50 countries: we start with a sample containing the 50 poorest countries and end with the 50 richest countries. Again, results indicate there is no robust relationship between weather shocks and conflict. In fact, the only conclusion we can draw from Figure 11 is that global temperatures (the ENSO variable) matters for conflict in the richest and poorest sub-samples. However, as above, when significant the estimated coefficient is negative – the onset of conflict is correlated with low global temperatures, not high ones (as argued by Hsiang et al.). Again, it seems difficult to match these patterns in the data with existing theories on climate change and conflict.

[Insert Figures 10-11 about here]

4.4 Alternative measures of conflict

Weather shocks may have different impacts on the likelihood of the onset of small scale conflict and large scale conflict. In our regression models until now we focused on civil conflicts with 25 battle deaths, or more. To assess the robustness of our findings, we

[13] We also estimated this model ordering the countries based on initial (1960) income levels. These results are similar to those reported in Figure 11.

have also used an alternative definition of conflict, and considered only conflicts with more than 1,000 battle deaths ("major civil wars"). Using this definition reduces the number of conflicts in our sample by about 75%. In addition, our dataset is dramatically reduced because more than 50% of the countries drops as they never experience any major war in our study period. Notwithstanding these caveats and qualifications, we ran the various models, and found that the earlier results hold up — we find no evidence of a robust relation between weather variables and conflict. We do not present all models in detail, but a summary of the pooled data is contained in Table 3.

5. Discussion and conclusions

According to the most recent IPCC projections, local climates across the globes are in a process of transition. While exact predictions are imprecise, most researchers believe some places will become warmer and others will become cooler. Similarly, some places will become drier and others are projected to become wetter. Neo-Malthusian reasoning implies conflicts will become more frequent as resources become more scarce – think of African countries projected to become hotter and drier. A media frenzy has followed recent studies pointing at correlations between weather variables and conflict.

We have estimated hundreds of weather-conflict models, and as expected a handful of those produce evidence consistent with the neo-Malthusian perspective. However, we also report outcomes contrary to this perspective, and the great majority of models exploring the association between temperature or rainfall and the onset of conflict do not yield any significant result. The relationship between resource scarcity and conflict appears weak, or possibly more complex than captured in simple models.

We have explored robustness by slicing and dicing our dataset into a wide range of subsamples – along temporal and continental dimensions, but also distinguishing between more and less democratic countries, and between poor and rich countries. This has not advanced our understanding much. Few robust results emerged, and those that did are not easy aligned with theory (e.g., the finding that especially democratic countries appear sensitive to drought shocks, or that undemocratic countries are sensitive to low temperatures). Moreover, such result tend to be not robust to small specification changes, such as taking logarithms of the data, or slightly changing the sample population. These sobering results echo earlier failures to robustly link the onset of conflict to resource abundance (Brunnschweiler and Bulte 2008, 2009).

It is tempting to conclude that analysts seeking to explain the determinants of conflict should direct their attention elsewhere. Based on a cross-country analysis, Buhaug (2010) concludes "African civil wars can be explained by generic structural and contextual conditions: widespread ethnopolitical exclusion, poor national economy, and the collapse of the cold war system" (p. 16477). Ethnopolitical exclusion refers to grievances, and the lack of inclusive policies. Such a view is consistent with theoretical work by Butler and Gates (2012), pointing at (biased) protection of property rights, the "politics of land," and the role of the state in explaining conflict. It is also consistent with case studies of specific conflicts, routinely attributed to the geophysical environment. Think of rebellions in the Sahel supposedly driven by desertification (Benjaminsen 2008) or of rebellions in the forest of Sierra Leone supposedly motivated by greedy rebels grabbing blood diamonds (Mokuwa et al., 2011). While such cases differ in many details, an overarching and common theme is that some social groups feel marginalized, and believe they have little or no stake in local economic development.

If marginalization plays such an important role in understanding the origins of conflict, why do many analysts focus on resource abundance or scarcity? We can think of three possible explanations. First, and obviously, the availability of data might matter — while group-specific measures of grievances cannot be pulled from the internet, various proxies for weather conditions and resource dependence are readily available. Second, geophysical factors hold the luring promise of technical fixes. Trade in blood diamonds can be regulated, and the disturbing effects of temperature and rainfall shocks can be attenuated by new crops or production technologies. In contrast, it is not obvious how to address informal institutions that are unfair, and geared towards the exploitation of one social group by another. Third, economists and political scientists are increasingly concerned with proper identification of causal effects – exogeneity rules. From this perspective, focusing on temperature or rainfall is easier than struggling with complex socioeconomic constructs, which have evolved in response to historic patterns of conflict (and perhaps the shadow of future conflict). Such constructs arguably are "endogenous" in conflict models, and teasing out causal effects via randomized controlled trials is obviously no option.[14] It would be unfortunate if the desire for statistical rigor implies ignoring less convenient, but possibly more important, factors causing conflict.

In light of all of this it is no surprise that we call for a bridge between large N studies and careful case studies. This requires econometricians to move beyond the conventional "country-year" focus, and embrace shorter time intervals and sub-national regions. It also requires proponents of case studies to look beyond the peculiarities of specific conflicts, formulating generic and testable hypotheses, and seeking to consistently 'quantify' grievances or measures of marginalization. The domain of conflict could be a fruitful area for economists and anthropologists to work together.

References

Adano, W., T. Dietz, K. Witsenburg. and F. Zaal, 2012. Climate change, violent conflict and local institutions in Kenya's drylands. *Journal of Peace Research* 49: 65-80.

Baechler, G., V. Boege, S. Kloetzli, S. Libiszewski and K. Spillman, 1996. *Environmental degradation as a cause of war: Ecological conflicts in the third world world and peaceful ways of resolving them*. Zurich: Ruegger.

Beck, N., J. Katz, and R. Tucker, 1998. Taking time seriously: Time series- cross section analysis with a binary dependent variable. *American Journal of Political Science* 42: 1260-1288.

[14] Instrumental variable approaches might work, but typically require detailed knowledge of the system under study to produce credible instruments.

Benjaminsen, T., 2008. Does supply-induced scarcity drive violent conflicts in the African Sahel? The case of the Tuareg rebellion in northern Mali. *Journal of Peace Research* 45: 819-836.

Benjaminsen, T., K. Alinon, H., Buhaug, and J.T. Buseth, 2012. Does climate change drive land-use conflicts in the Sahel? *Journal of Peace Research* 49: 97-111.

Brunnschweiler, C.N. and E.H. Bulte, 2008. Are resource-rich countries cursed? Linking natural resources to slow growth and more conflict. *Science* 320: 616-617.

Brunnschweiler, C. and E.H. Bulte, 2009. Natural resources and violent conflict: Resource abundance, dependence and the onset of civil wars. *Oxford Economic Papers* 61: 651-674.

Buhaug, H., 2010. Climate not to blame for African civil wars. *Proceedings of the National Academy of Sciences* 107: 16477-16482.

Burke, M., E. Miguel, S. Satyanath, J. Dykema, and D. Lobell, 2009. Warming increases the risk of civil war in Africa. *Proceedings of the National Academy of Sciences* 106: 20670-20674.

Burke, M., E. Miguel, S. Satyanath, J. Dykema, and D. Lobell, 2010. Climate robustly linked to African civil war. *Proceedings of the National Academy of Sciences* 107. E-185.

Butler, K. and S. Gates, 2012. African range wars: Climate, conflict, and property rights. *Journal of Peace Research* 49: 23-34.

Ciccone, A., 2011. Economic shocks and civil conflict: A comment. *American Economic Journal: Applied Economics* 3: 215-227.

Collier, P. and A. Hoeffler, 2004. Greed and grievance in civil war. *Oxford Economic Papers* 56: 563-595.

Fearon, J.D. and D. Laitin, 2003. Ethnicity, insurgency, and civil war. *American Political Science Review* 97: 75-90.

Foster, P. and B. Nelson, 1996. Continuous record asymptotics for rolling sample variance estimators. *Econometrica* 64: 139-174.

Gleditsch, N.P., 2012. Whither the weather? Climate change and conflict. *Journal of Peace Research* 49: 3-9.

Hendrix, C. and I. Salehyan, 2012. Climate change, rainfall, and social conflict in Africa. *Journal of Peace Research* 49: 35-50.

Homer-Dixon, T.F., 1999. *Environment, scarcity, violence*. Princeton NJ: Princeton University Press

Hsiang, S., K. Meng and M. Cane, 2011. Civil conflicts are associated with the global climate. *Nature* 476: 438-441.

Koubi, V., T. Bernauer, A. Kalbhenn, and G. Spilker, 2012. Climate variability, economic growth, and civil conflict. *Journal of Peace Research* 49: 113-127.

Miguel, E., S. Satyanath and E. Sergenti, 2004. Economic shocks and civil conflict: An instrumental variables approach. *Journal of Political Economy* 112: 725-753.

Miguel, E. and S. Satyanath, 2011. Re-examining economic shocks and civil conflict. *American Economic Journal: Applied Economics* 3: 228-232.

Mokuwa, E., M. Voors, E. Bulte, and P. Richards, 2011. Peasant grievance and insurgency in Sierra Leone: Judicial serfdom as a driver of conflict, *African Affairs* 110: 339-366.

Raleigh, C. and D. Kniveton, 2012. Come rain or shine: An analysis of conflict and climate variability in East Africa. *Journal of Peace Research* 43: 51-64.

O'Reilly, G. and K. Whelan, 2005. Has euro-area inflation persistence changed over time? *The Review of Economics and Statistics* 87: 709-720.

Sutton, A., J. Dohn, K. Loyd, A. Tredennick, G. Bucini, A. Solorzano, L. Prihodko and N. Hanan, 2010. Does warming increase the risk of civil war in Africa? *Proceedings of the National Academy of Sciences* 107: E102.

Theisen, O., 2012. Climate clashes? Weather variability, land pressure, and organized violence in Kenya, 1989–2004. *Journal of Peace Research* 49: 81-96.

Tir, J. and D. Stinnett, 2012. Weathering climate change: Can institutions mitigate international water conflict? *Journal of Peace Research* 49: 211-225.

Tol, R., and W. Wagner, 2010. Climate change and violent conflict in Europe over the last millennium. *Climatic Change* 1-2: 65-79.

Witsenburg, K. and W. Adano, 2009. Of rain and raids: Violent livestock raiding in Northern Kenya. *Civil Wars* 11: 514-538.

Zhang, D., P. Brecke, H. Lee, Y-Q He, and J. Zhang, 2007. Global climate change, war, and population decline in recent human history. *Proceedings of the National Academy of Sciences* 104. 19214-19219.

Tables and Graphs

Table 1: Conflict and the level of temperature and precipitation

	Dependent variable: Onset of civil conflict, >25 battle deaths								
	All	Africa	Asia	All	Africa	Asia	All	Africa	Asia
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Real GDP per capita	-0.235	2.057	-2.350	-1.113	1.065	-3.773	-0.232	2.134	-2.501
	[-0.22]	[1.04]	[-1.50]	[-0.95]	[0.64]	[-1.57]	[-0.21]	[1.09]	[-1.55]
Polity IV	-0.001	0.006	-0.007	0.013	0.011	0.023	-0.001	0.007	-0.005
	[-0.10]	[0.33]	[-0.46]	[0.88]	[0.72]	[0.82]	[-0.03]	[0.35]	[-0.35]
Temperature level	-0.050	-0.058	-0.089						
	[-0.18]	[-0.11]	[-0.25]						
Precipitation level				-0.516	-1.184	-0.317			
				[-1.84]*	[-1.87]*	[-1.12]			
ENSO level							-0.224	-0.107	-0.423
							[-1.65]	[-0.52]	[-2.36]**
Observations	2459	1264	673	1500	807	367	2449	1257	670
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country specific time trend	YES	YES	YES	YES	YES	YES	YES	YES	YES
F-test dependence variables (p-value)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

***/* Indicating significance levels of respectively 5 and 10 percent. t-values are shown between brackets.*

Table 2: Conflict and the variability of temperature and precipitation

	Dependent variable: Onset of civil conflict>25 battle deaths								
	All	Africa	Asia	All	Africa	Asia	All	Africa	Asia
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
real GDP per capita	-0.167	2.004	-2.146	-0.823	1.441	-3.467	-0.189	2.297	-2.362
	[-0.17]	[1.00]	[-1.79]	[-0.75]	[0.73]	[-1.55]	[-0.18]	[1.15]	[-1.49]
Polity IV	-0.001	0.006	-0.006	0.012	0.005	0.039	-0.001	0.006	-0.005
	[-0.07]	[0.32]	[-0.36]	[0.75]	[0.31]	[0.99]	[-0.08]	[0.35]	[-0.37]
Temperature variability	-2.455	2.278	-4.192						
	[-2.07]**	[0.92]	[-2.49]**						
Precipitation variability				3.081	5.871	1.789			
				[1.28]	[0.93]	[0.74]			
							-0.662	-2.599	-0.609
							[-0.57]	[-1.62]	[-0.32]
Observations	2459	1264	673	1422	753	367	2459	1264	673
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country specific time trend	YES	YES	YES	YES	YES	YES	YES	YES	YES
F-test intertemporal variables (p-value)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

***/* Indicating significance levels of respectively 5 and 10 percent. t-values are shown between brackets.*

Table 3: Conflict and the level of temperature and precipitation

	Dependent variable: Onset of a civil conflict, > 1000								
	All	Africa	Asia	All	Africa	Asia	All	Africa	Asia
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Real GDP per capita	1.567	2.222	0.672	1.618	2.080	1.270	1.417	1.721	0.836
	[3.09]**	[3.76]**	[0.61]	[1.85]*	[3.09]**	[0.38]	[2.87**]	[2.56]**	[1.01]
Polity IV	-0.002	-0.013	0.036	0.005	-0.012	0.065	0.006	-0.008	0.041
	[-0.17]	[0.86]	[1.57]	[0.35]	[-0.58]	[2.10]**	[0.51]	[-0.42]	[1.50]
Temperature level	-0.720	-0.639	-0.644						
	[-1.26]	[-0.75]	[-0.72]						
Precipitation level				-0.081	-0.363	0.357			
				[-0.17]	[-0.37]	[0.71]			
ENSO level							0.323	0.089	0.568
							[1.33]	[0.30]	[1.35]
Observations	650	260	308	517	215	230	593	244	274
Country fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country specific time trend	YES	YES	YES	YES	YES	YES	YES	YES	YES
F-test intertemporal variables (p-value)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

***/* Indicating significance levels of respectively 5 and 10 percent. t-values are shown between brackets.*

Figure 1: Onset of civil conflict between 1950 - 2009

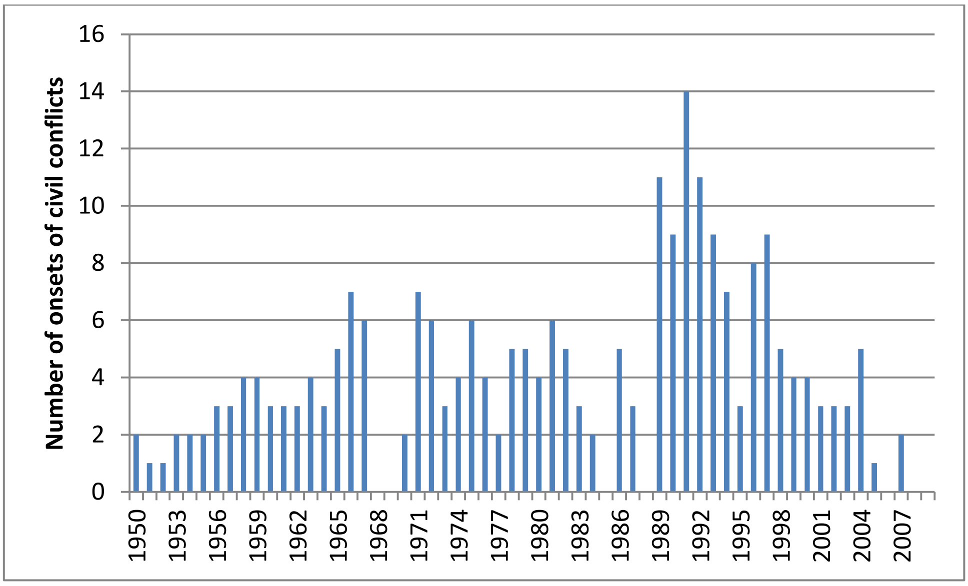


Figure 2: Average temperature 1950-2006

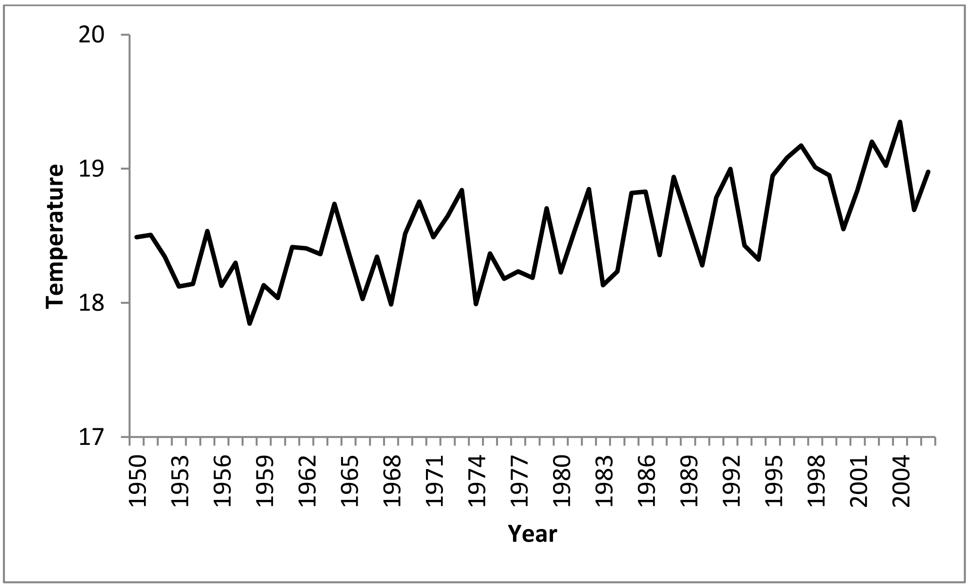


Figure 3: Average rainfall 1950-2006

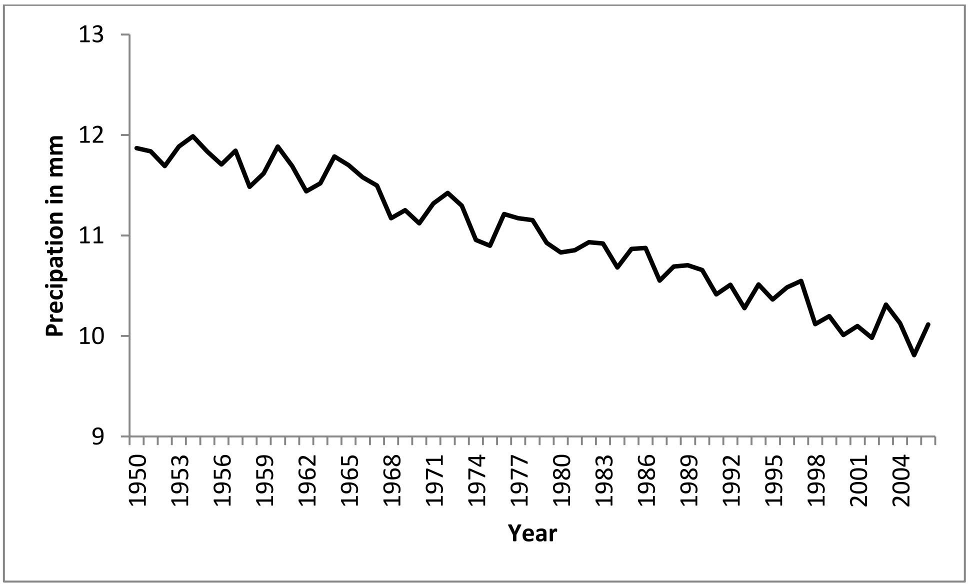
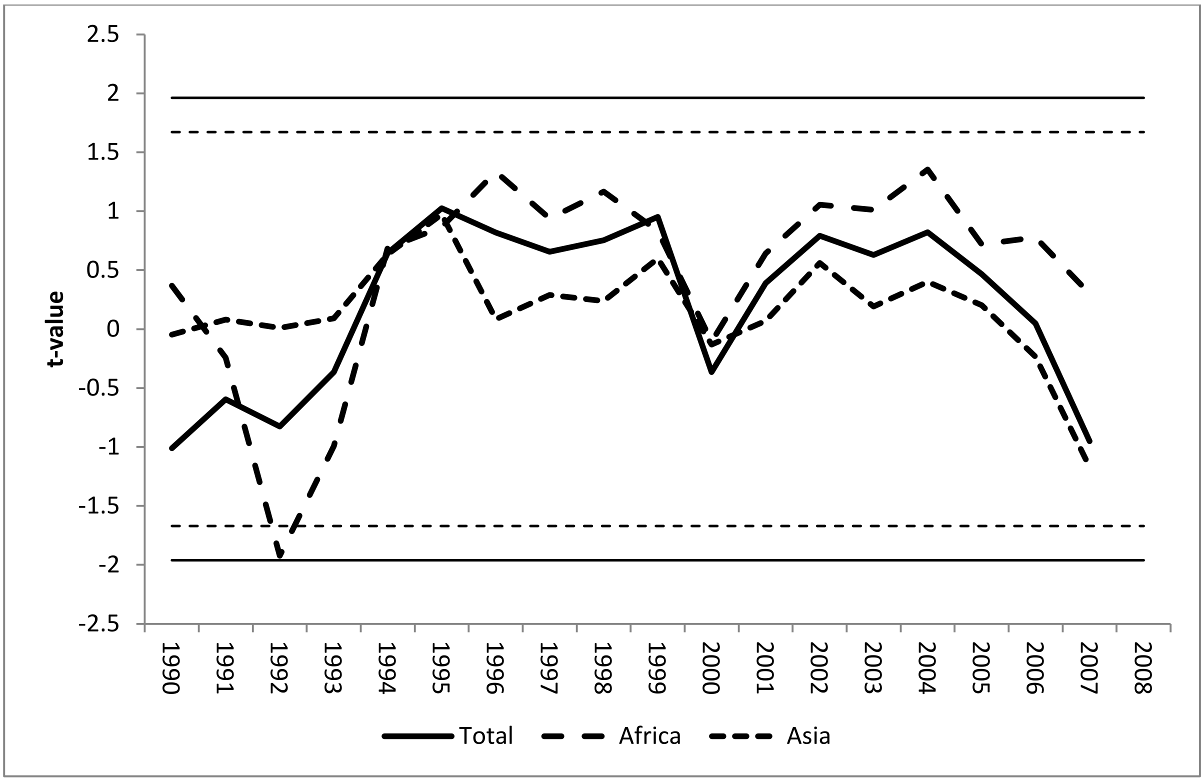


Figure 4: Rolling regression temperature level



Note: Regression results for a rolling window of 30 years. The horizontal axis displays the final year of the window (i.e. 1960-1989, 1961-1990, etc.). The vertical axis displays the t-value of the climate coefficient for the associated regression model and relevant significance thresholds.

Figure 5: Rolling regression precipitation level

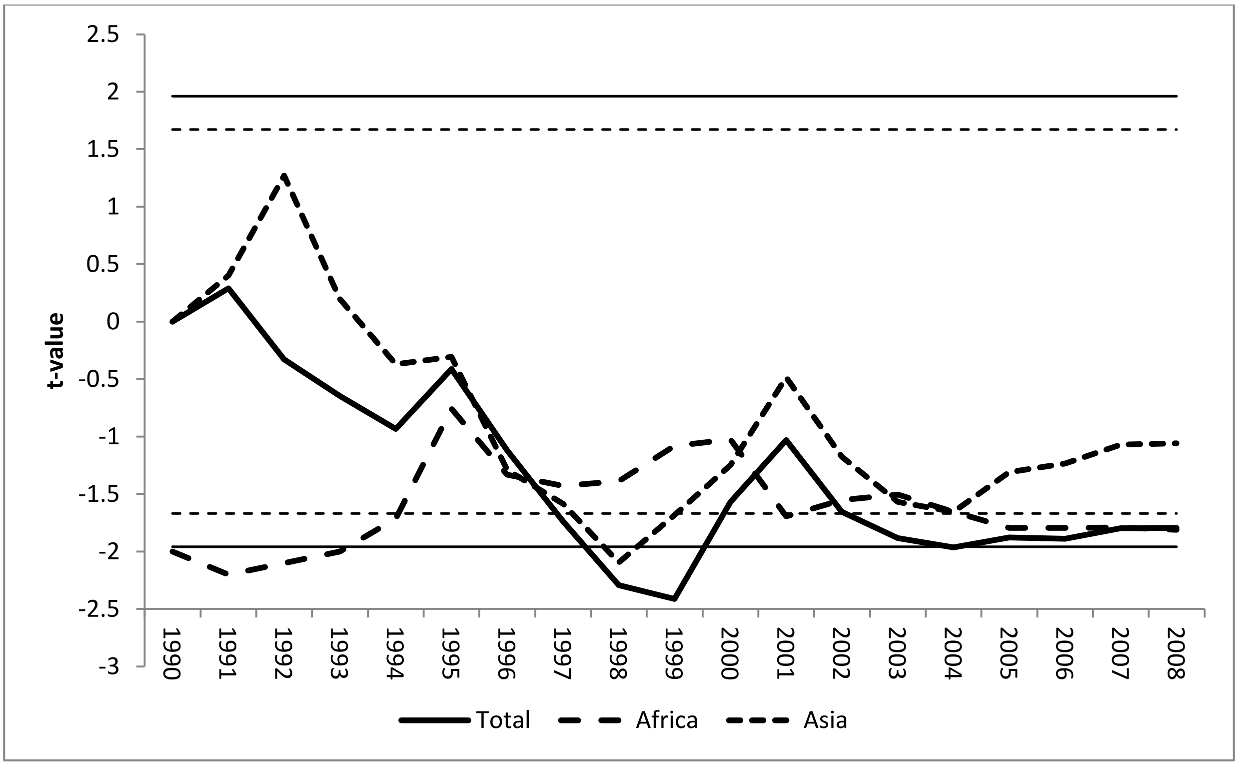


Figure 6: Rolling regression ENSO level

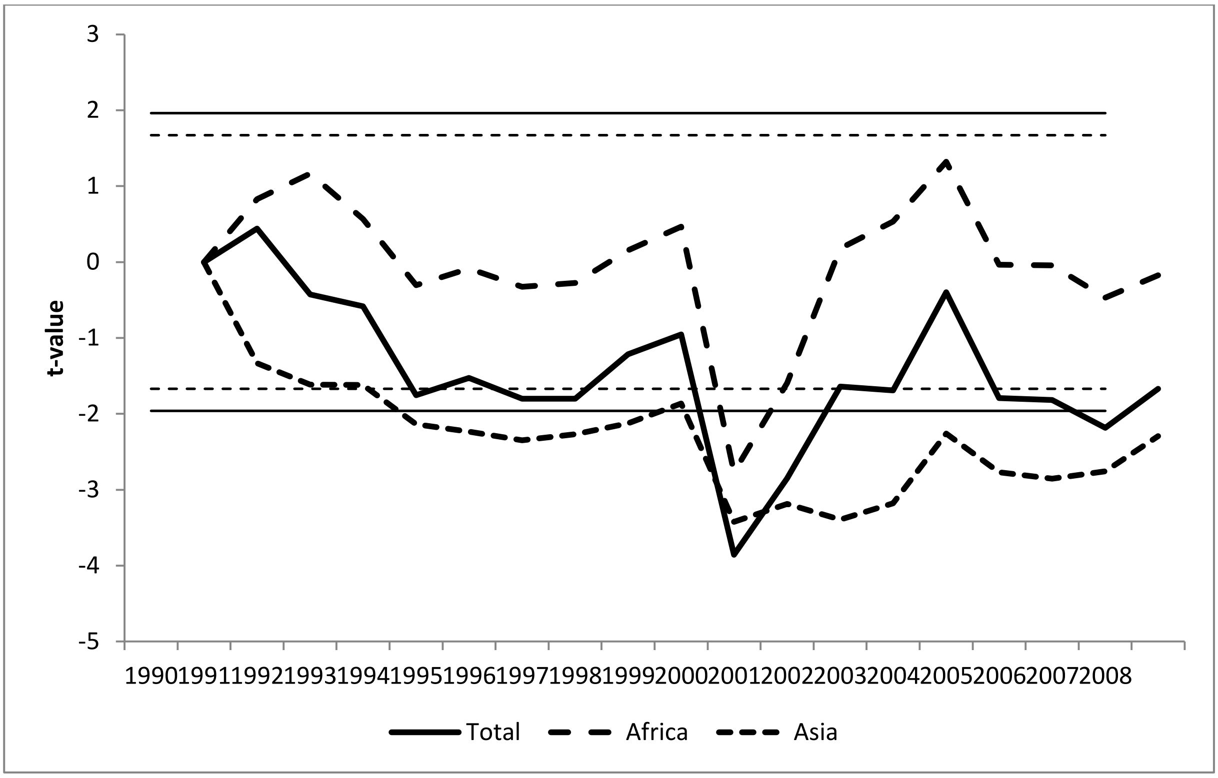


Figure 7: Rolling regression temperature variability

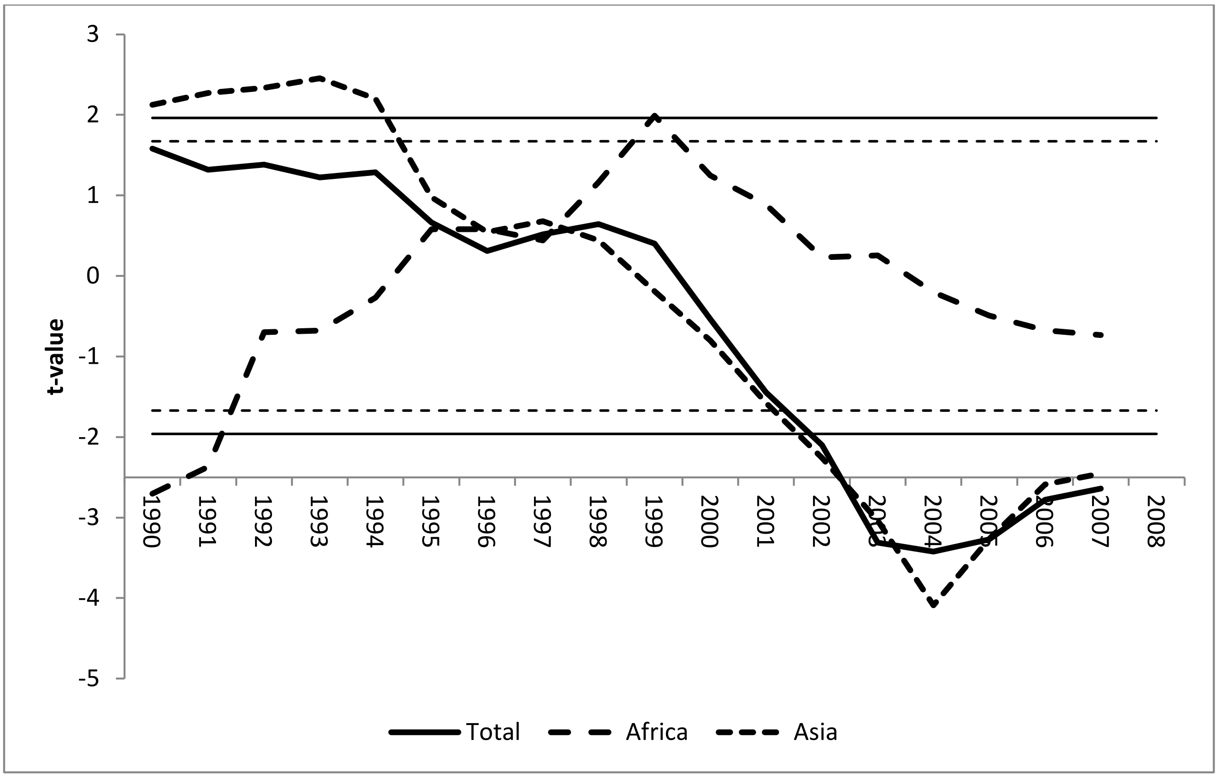


Figure 8: Rolling regression precipitation variability

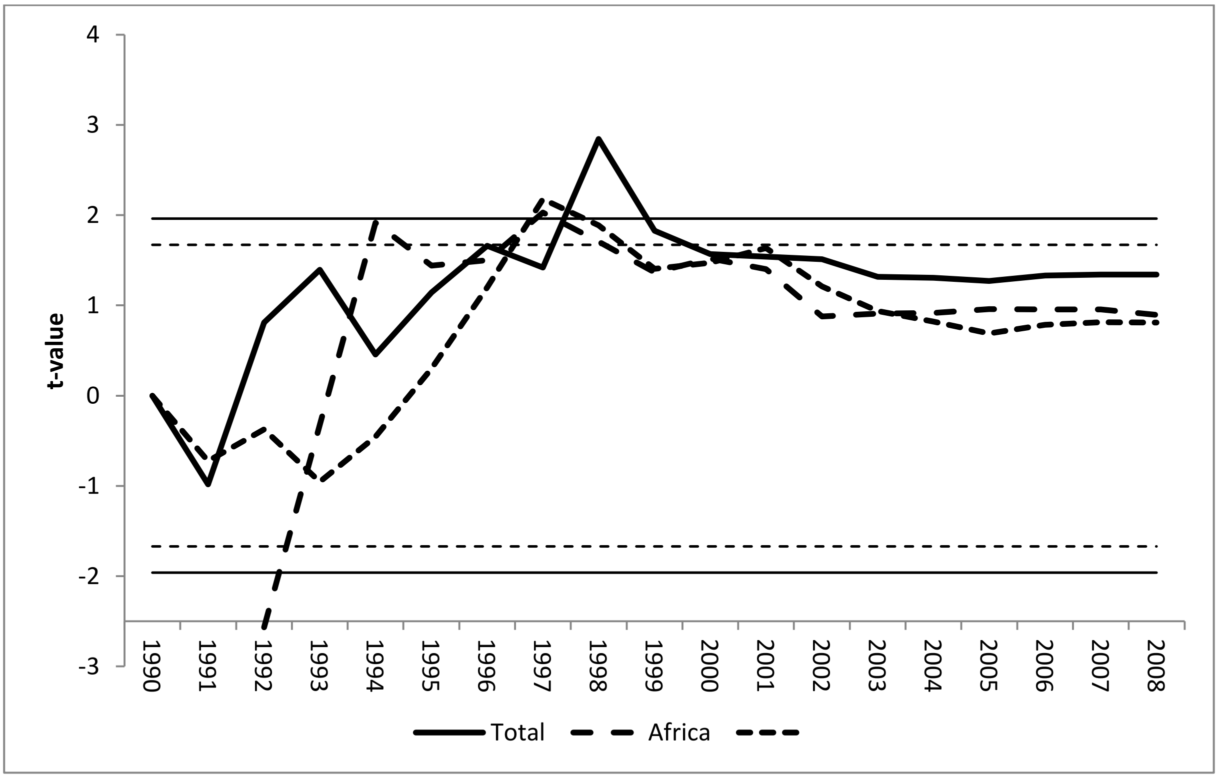


Figure 9: Rolling regression ENSO variability

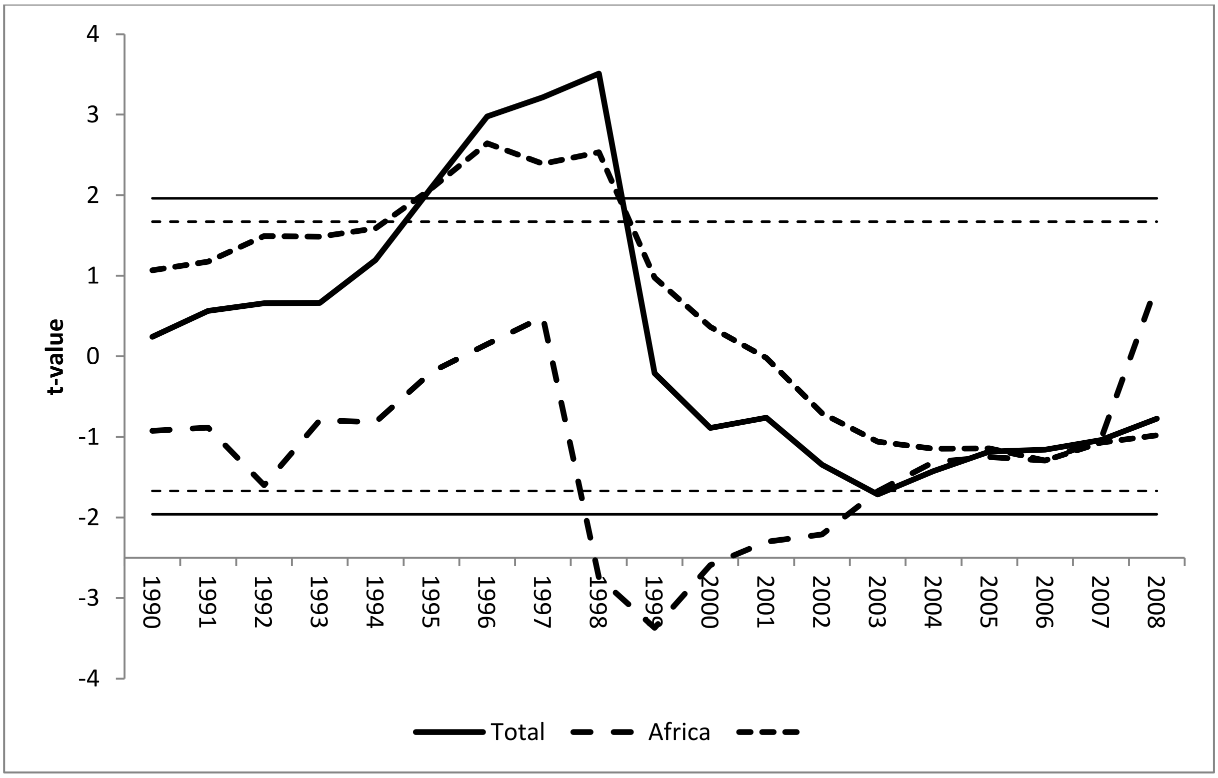


Figure 10: Rolling regression based on the level of democracy (Polity IV score)

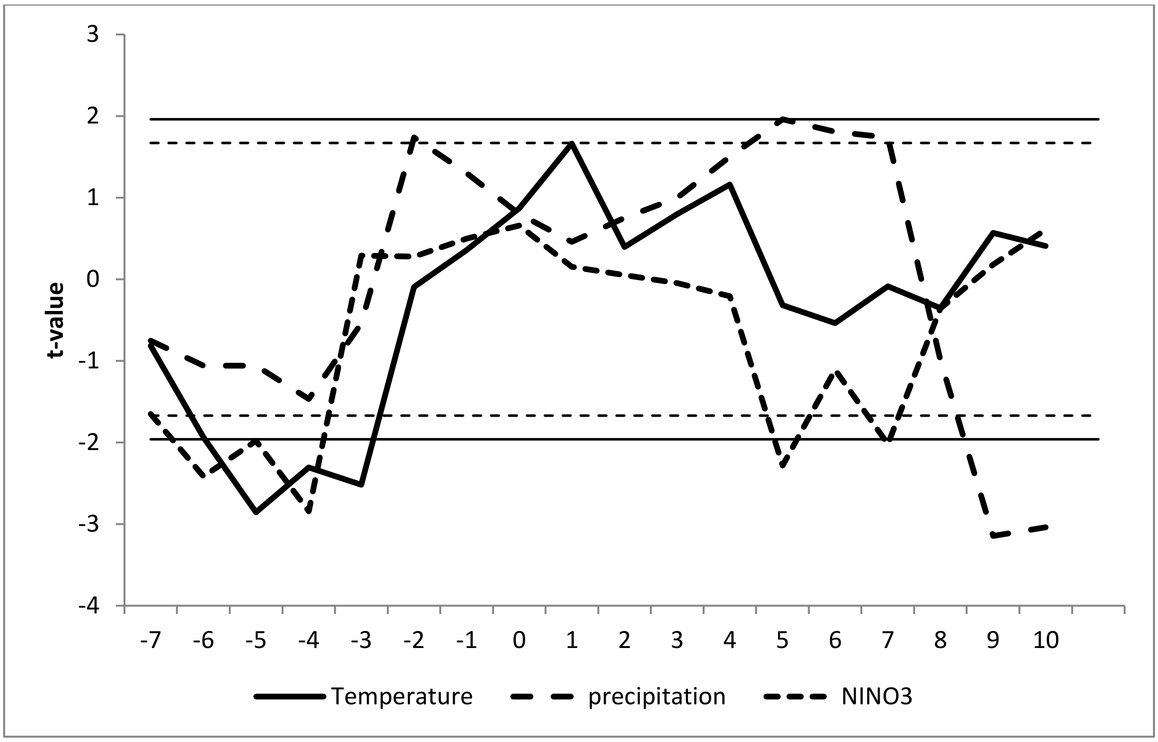
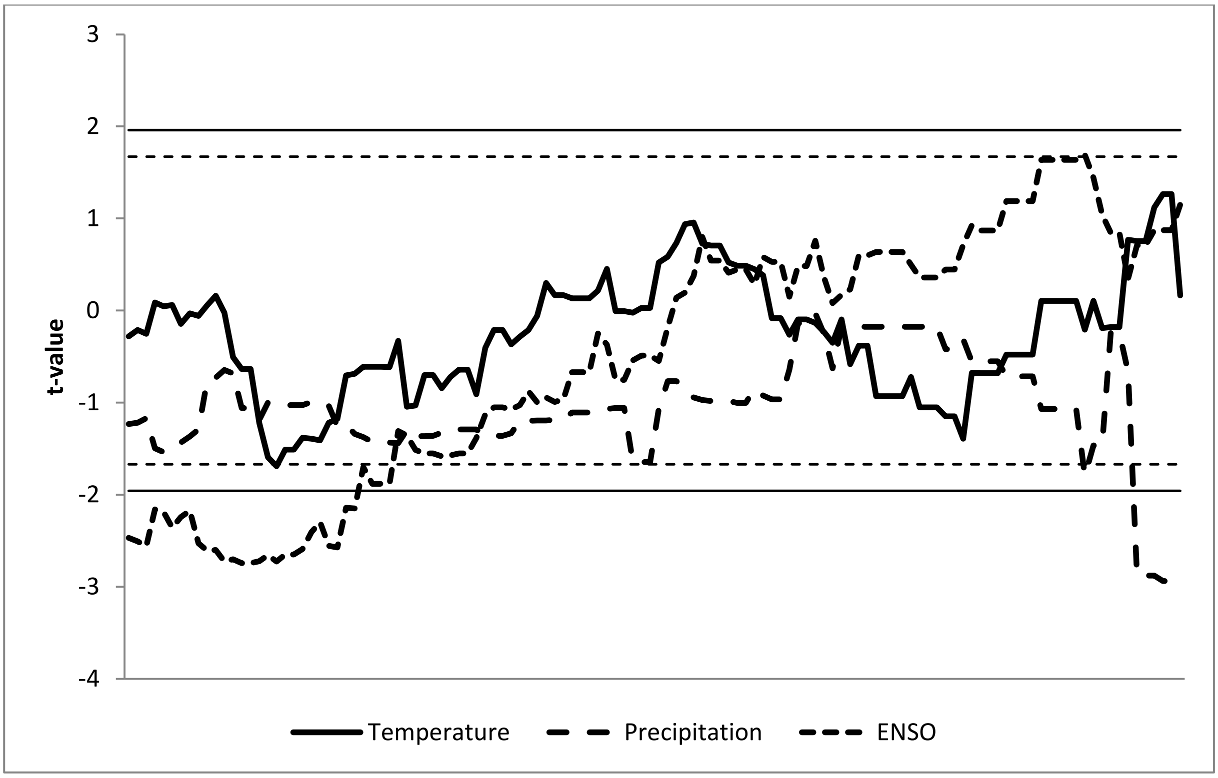


Figure 11: Rolling regression based on the level of income per capita



Appendix A1: Country sample

Country	First year	# onsets	Country	First year	Onsets	Countries	First year	# onsets
Afghanistan	1960	1	Iraq	1960	8	Tajikistan	1991	1
Algeria	1962	1	Kenya	1963	1	Thailand	1960	1
Angola	1975	5	Laos	1960	2	Togo	1960	2
Argentina	1960	2	Lebanon	1960	2	Trin- Tobago	1962	1
Azerbaijan	1991	3	Lesotho	1966	1	Tunisia	1960	1
Bangladesh	1972	1	Liberia	1960	3	Turkey	1960	2
Bolivia	1960	1	Macedonia	1991	1	Uganda	1962	4
Bosnia-Herzegovina	1992	2	Madagascar	1960	1	UK	1960	2
Burkina Faso	1960	1	Malaysia	1960	3	Uruguay	1960	1
Burundi	1962	2	Mali	1960	3	Uzbekistan	1991	2
Cambodia	1960	2	Mauritania	1960	1	Venezuela	1960	2
Cameroon	1960	1	Mexico	1960	2	Yemen	1960	4
Central Af. Rep.	1960	1	Moldova	1991	1	Yugoslavia	1960	2
Chad	1960	2	Morocco	1960	2	Zimbabwe	1965	1
Chile	1960	1	Mozambique	1964	2			
Colombia	1960	1	Myanmar	1960	9			
Comoros	1975	2	Nepal	1960	2			
Congo	1960	3	Nicaragua	1960	2			
Congo, DR	1960	5	Niger	1960	4			
Cote D'Ivoire	1960	1	Nigeria	1960	3			
Croatia	1991	1	Oman	1960	1			
Cuba	1960	1	Pakistan	1960	4			
Djibouti	1977	2	Panama	1960	1			
Dominican Rep.	1960	1	Papua NG	1975	1			
Egypt	1960	1	Paraguay	1960	1			
El Salvador	1960	2	Peru	1960	2			
Equatorial Guinea	1968	1	Philippines	1960	3			
Eritrea	1993	2	Rumania	1960	1			
Ethiopia	1960	6	Russia	1960	4			
Gabon	1960	1	Rwanda	1962	2			
Gambia	1965	1	Saudi Arabia	1960	1			
Georgia	1991	3	Senegal	1960	1			
Ghana	1960	2	Sierra Leone	1961	1			
Guatemala	1960	1	Somalia	1960	3			
Guinea	1960	2	South Africa	1960	2			
Guinea-Bissau	1974	1	Spain	1960	3			
Haiti	1960	3	Sri Lanka)	1960	4			
India	1960	12	Sudan	1960	5			
Indonesia	1960	7	Surinam	1975	1			
Iran	1960	7	Syria	1960	2			

Total number of onsets 217