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Local warming and violent armed conflict in Africa

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ABSTRACT

Research on the effect of climate change on violent armed conflict relies almost exclusively on analysing annual variation in climatic conditions. A shortcoming of this approach is that it conflates weather variation with climate change, while implicitly assuming that adverse weather shock could immediately trigger conflict. Although this relatively high-frequency data can help understand conflict seasonality, it fails to address the question of whether climate change is an important conflict determinant. This study tries to address this issue using long-term change in local climate to proxy climate change. Focusing on the African continent, shifts in average temperature and precipitation levels are used to estimate the effect on conflict risk between 2003–17. The data is analysed using Bayesian model averaging to test if the variables measuring local climatic conditions contribute consistently in explaining conflict risk. The reduced-form estimations show that temperature is robustly linked to armed conflict: a two-standard deviation increase in average temperature corresponds to about a 31 percent increase in conflict risk. Precipitation changes have no discernible effect. Changes in local climate are more strongly linked to the continuation of existing conflicts, rather than the outbreak of new ones. The association between climate and conflict found in the analysis also suggests a potential lack of adaptation. While the findings of this study are in agreement with earlier results, one remaining shortcoming is that the analysis does not provide much insight into the specific mechanisms linking climate and conflict.

1. Introduction

The 2018 global heatwave was a stark reminder that global warming is not a hypothetical future event. Instead the effects are already felt today. Serious concerns exist concerning the potentially negative consequences of climate change on human society in the absence of appropriate adaptation and mitigation strategies. One such concern is the increase in risk of violent armed conflict. Global warming might lead to reductions in the availability of key natural resources, such as water and arable land, thereby increasing tensions between groups using these resources, with the potential of escalation in the absence of functioning institutions or conflict resolution mechanisms. Over the past decade there has been a surge in research on this climate-conflict nexus which has produced some valuable insights. For example insurgency-related violence is more likely to follow after a bad harvest as a result of too much precipitation (Crost, Duquennois, Felter, & Rees, 2018); droughts can affect conflict through livestock prices (Maystadt & Ecker, 2014) and livestock raiding tends to be more violent during wet periods (Witsenburg & Adano, 2009). But research has also shown that the main correlates of conflict are often economic or socio-political in nature rather than climatological (Linke, O'Loughlin, McCabe, Tir, & Witmer, 2015; Wischnath & Buhaug, 2014).

A schism exists concerning the interpretation of the results of the literature as a whole (Mach et al., 2019). Two recent studies argued that most results tend to be in agreement with the hypothesis that deteriorating climatic conditions—such as higher temperatures and lower precipitation levels—are linked to increased conflict risk (Hsiang & Burke, 2014; Hsiang, Burke, & Miguel, 2013). On average, a standard deviation increase in temperature is associated with a 14% increase in conflict probability (Hsiang et al., 2013). This result is heavily contested (Buhaug et al., 2014) as the conclusions differ from those reached in other literature reviews (e.g. (Klomp & Bulte, 2013; Theisen, Gleditsch, & Buhaug, 2013)—see Hsiang, Burke, & Miguel (2014) for response). This lack of consensus might not be surprising given the sensitivity of results in the conflict literature (Hegre & Sambanis, 2006). However, an important point that has remained largely unaddressed is the fact that climate variability, particularly year-to-year variation in weather, is often conflated with climate change (Buhaug, 2015) and might not be a good proxy (Selby, 2014).

For illustration consider, the following standard statistical model:

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$$C_{it} = \alpha + \beta C_{i,t-1} + \delta L^k W_{it} + \theta X'_{it} + \epsilon_{it} \quad (1)$$

Outcome variable C_{it} measures conflict in region i (e.g. a country or grid-cell) at time t. The outcome variable is often coded using a binary indicator for conflict incidence. In this model the outcome variable is linked to the temporal lag, to account for state-dependence; a vector of control variables X'_{it} (e.g. population or income levels); and changes or shocks in weather variables captured by $L^k W_{it}$—α is a constant and ϵ_{it} the error term.

Note that L^k indicates that it could include lags, such as weather anomalies of previous years. An anomaly is measured as the deviation from the long-term (typically 30 years) mean (formally $(W_{it} - \overline{W}_i)$). As such it captures the difference of the observed value from the expected value, allowing to identify anomalous periods. Although this model can help estimate the effect of local weather shocks on conflict risk, arguably it provides scant information on the effect of climate change: which is a longer and more gradual process, even at its current unprecedented pace. An additional shortcoming of this model is the implicit assumption that experienced climate change immediately triggers conflict, which might seem untenable (Selby, 2014). For instance in rural Africa, the focus of most studies on climate conflict, local populations have a long history of applying adaptive measures to deal with the local climate (Berhe et al., 2017).

In contrast with existing research this study follows the example of Burke and Emerick (2016), who estimate the effect of climate change on US agricultural output using a so called long differences approach. This approach is a hybrid between identification from time-series variation, as in Eq. (1) cross-sectional analysis which ignores time-series variation and uses information exclusively from differences between units (Hsiang, 2016). In this particular case conflict incidence is linked to 15-year changes in local climatic conditions. Eq. (1) can be re-written as

$$C_i = \alpha + \beta LC_i + \delta \Delta W_i + \theta X'_i + \epsilon_i \quad (2)$$

where conflict incidence (C_i) linked to its temporal lag (LC_i); a vector of other explanatory variables (X'_i); and the change in local climatic conditions measured by the difference in average anomalies for temperature and precipitation (ΔW_i).

This study is similar to the pioneering work of Harari and La Ferrara (2018) as it also uses the grid-cell as unit-of-analysis. In contrast with theirs, this study covers a longer period (with conflict data available from 1989 onwards), but more importantly focuses on relatively long-term changes in local climate, rather than relying on within-year variation; measuring change in local climate between 2003–17 relative to a benchmark period to estimate the effect on conflict incidence at grid-cell level. This paper's main contribution is therefore providing an analysis of the effect of changes in local climate on conflict-risk; addressing some of the previously discussed shortcomings of the current literature. It thereby complements existing studies that focus on the specific timing of conflict (e.g. Breckner & Sunde (2019), Crost et al. (2018), Harari & La Ferrara (2018), Maystadt & Ecker (2014)). Focusing on these changes can help shed some light on whether households living in areas adversely affected by these shifts posses adaptive capabilities to deal with changing conditions (see also Castells-Quintana, del Pilar Lopez-Uribe, & KJ McDermott (2018)). The results suggest that this is not the case as there is a positive correlation between temperature increases—but likely not precipitation—and conflict risk. Importantly, the analysis reveals that the correlation between conflict risk and long-term changes is stronger compared to short-term weather shocks, which seem inconsequential.

A shortcoming of this study is that it is a reduced-form estimation, directly linking climatic conditions to conflict risk. As such, it provides no analysis on the specific mechanisms. This limitation is due to data constraints as there is little information on for instance local commodity prices for a long enough period or regional trade which would allow for testing more specific hypothesis as in Crost et al. (2018), Maystadt and Ecker (2014) or the theory of market collapse by Olsson (2016). This is therefore left for future research.

Within the climate-conflict literature only a handful of studies have focused on changes over longer periods of time. Zhang, Brecke, Lee, He, and Zhang (2007) link lower temperatures to the outbreak of war and subsequent population decline in pre-industrial China and Europe; Tol and Wagner (2009) find that in the past millennium conflict was more intense during cooler periods in Europe, but this effect has waned in the industrial era—see van Bavel et al. (2019) for a critique. For the contemporary period Hsiang, Meng, and Cane (2011) use the El Niño Southern Oscillation to show that conflict risk doubles during El Niño years compared to the cooler La Niña years. A shortcoming of their approach—using the changes between El Niño and La Niña years as a source of variation—is that it relies on the somewhat heroic assumption that households don't adapt to these shifts. This study makes no such assumption, although the results suggest that lack of adaptation and mitigation might indeed have increased conflict risk in certain areas. Finally, there is van Weezel (2019) who exploits a sudden and abrupt precipitation decline in the Horn of Africa as a proxy for climate change; showing that areas that experienced a more pronounced decline experience higher rates of communal conflict. It is unclear how these results generalise given the use of a very specific shock in local climate. Indeed, this study finds that precipitation is a modest explanatory factor.

2. Data and measurement

This study uses georeferenced data aggregated to a resolution of a 1 degree square grid—corresponding to 110 km at the Equator—covering the African continent. In total there are 2557 spatial units; 2525 when omitting grid-cells with missing values. The focus of this study is the link between conflict incidence and climatic conditions at the local level between 2003 and 2017. To this end the data is analysed examining cross-sectional variation in conflict, climate, and a number of other possible conflict determinants such as population density, economic activity, and ethnic division.

2.1. Temperature and precipitation

Temperature data is taken from the Berkeley Earth Surface Temperature (BEST) dataset (Rohde et al., 2013). This dataset combines temperature data from various measurement stations and produces averages using the spatial technique kriging to account for statistical outliers and create a homogeneous climatology on a 1 degree lattice. An important advantage of BEST of other comparable datasets is that it is not as vulnerable to conflict-related station loss (Schultz & Mankin, 2017), which might otherwise introduce a downward bias in the estimates. The dataset provides information on the average monthly temperature expressed as an anomaly from the 1951–80 mean which serves as baseline. The dataset itself covers the period from 1750 onwards, but most grid-cells on the African continent only contain measurements from about 1900.

The monthly data is aggregated to calculate annual average temperature. These annual averages are used to construct the variable that serves as a proxy for climate change. Specifically, the variable of interest is the change in the average temperature anomaly for 2003–17 relative to the benchmark period (1988–2002).

$$\Delta T = \overline{T_{2003-2017}} - \overline{T_{1988-2002}} \quad (3)$$

There are constraints on the period of analysis as the available georeferenced conflict data covers a relatively short time period (1989–2017). As such we are left with two 15-year periods and little room for manoeuvre. Fifteen years is a relatively brief time in climatic terms, but given the pace of anthropogenic global warming there has been a substantial increase in the average temperature, of about 0.4 (standard error = 0.15) in the anomaly, across Africa.

Precipitation data is taken from the CHIRPS (Climate Hazards Group InfraRed Precipitation with Station) dataset (Funk et al., 2015). The advantage of this dataset is that it combines estimates on so called infra-red cold cloud duration, a precipitation proxy measured by satellites, with gauge station data to provide a comprehensive dataset on monthly precipitation for the whole continent. The data is available from 1981 onwards and is projected on a 0.05 degree grid (5 km at the Equator), accounting for local variation in precipitation. A disadvantage of the data is the limited temporal availability; the result of the use of satellite measurements. This entails that all available data, from 1981 till 2018, is used to construct a baseline. The data is aggregated to an annual precipitation total for each grid-cell which is used to calculate the change in precipitation between 2003 and 17 relative to the benchmark period.

Fig. 1 shows the aggregated data for temperature (panel a) and precipitation (panel b). There has been an increase in temperature across the continent, ranging from a modest 0.08 to a maximum of 0.76. Increases have been relatively minor in Africa south of the Equator in contrast with more substantial increases in places like Egypt, the Horn of Africa, and Northern Nigeria. Important to note is that research has shown that temperature increases have not been accompanied by increases in variability (Huntingford, Jones, Livina, Lenton, & Cox, 2013). Precipitation changes display more heterogeneity, which is likely the result of differences in local geography as well as inter-decadal variation. Most notably precipitation levels have decreased in South Africa's Cape region, which has lead to severe water shortages in recent years.

2.2. Civil conflict

Conflict data is taken from the Georeferenced Event Dataset (GED, version 18.1) from the Uppsala Conflict Data Programme (Sundberg & Melander, 2013). The GED contains detailed information on the location and timing of conflict events and the type of conflict. It also includes an indication of the geographic precision of the geolocated event. It has near global coverage for the years 1989–2017. This is currently the most comprehensive conflict event dataset publicly available; superior to similar datasets in terms of precision (Eck, 2012) and accuracy of included events (Weidmann, 2013, 2015). There are some important caveats though. First, an event is included only when it is associated with a conflict that has reached a fixed fatality threshold of at least 25 battle-related deaths (Croicu & Sundberg, 2015). This means that events at the lower end of the violence spectrum are not included (e.g. protests, riots or other forms of social distress). The analysis can therefore provide no insights into these types of conflict. Second, media reports form the basis of the majority of observations. This could introduce a reporting bias in the data. The effect of this type of bias for this particular dataset is likely to be small (Croicu & Kreutz, 2016). Third, there is the issue of Known Geographically Imprecision (Croicu & Hegre, 2018). The analysis discards conflict events that cannot be accurately located within a 25 km. radius, which entails a loss of information. As such some grid-cells will appear more peaceful than they are in real life as the conflict event cannot be accurately matched with the grid-cell. As an alternative all conflict events could be used, but this would introduce a more severe bias in the estimates as conflicts are linked to grid-cell characteristics that are incorrect.

Matching the data with the grid-cells a binary conflict incidence indicator is constructed which takes value 1 if there was a conflict event in the respective grid-cell in the period 2003–17 and 0 otherwise. A rather simple measure for conflict is used, focusing on the extensive margin. As an alternative outcome variable, focusing on the intensive margin, conflict prevalence is used which is the proportion of years a grid-cells experienced conflict between 2003 and 2017. Since conflict is persistent over time and space, the estimation includes the temporal and spatial lag of conflict. The temporal lag is a binary indicator taking value 1 if there was conflict in the grid-cell between 1989–2002, and 0 otherwise. The spatial lag is constructed using the temporal lag and is simply a count of conflict incidence in the directly neighbouring cells. For simplicity let $y_{i,t-1}$ be the temporal lag of conflict incidence; the spatial lag is $Wy_{i,t-1}$ where W is the connectivity matrix. The connectivity matrix is based on queen contiguity, so directly neighbouring grid-cells on all sides, with $W_{ij} = 0$ for the diagonal elements by construction and $W_{ij} = 1$ if cell i and j are contiguous neighbours. The spatial covariate for k neighbouring cells can thus be written as

$$\sum_k W_{ik} y_{k,t-1} \tag{4}$$

As Fig. 2 illustrates conflict tends to be localised, clustering in particular places. A large number of clusters are located in and around the Sahel region. While conflict is also prevalent in the eastern part of the Democratic Republic of the Congo; a region that has been endemic to violence since the end of the Second Congo War in 2003. Striking is the reduction in conflict over time in the

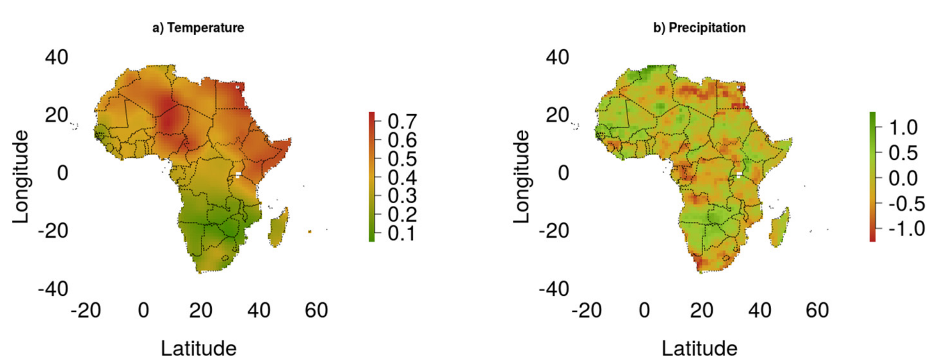

Fig. 1. Change in average anomaly between 1988–2002 and 2003–17 for (a) temperature and (b) precipitation. Note: There is no precipitation estimate for the island of Mauritius.

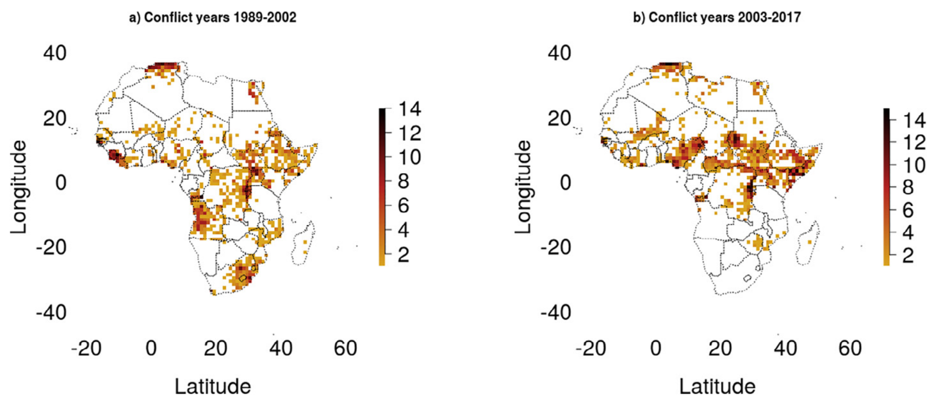

Fig. 2. Conflict years per grid cell for (*a*) 1989–2002 and (*b*) 2003–17.

southern part of the continent; after the end of the Apartheid regime in South Africa and the civil wars in Angola and Mozambique.

2.3. Other possible determinants

The analysis considers a number of other factors commonly associated with civil conflict. Larger populations put more pressure on local resources and provide a larger pool of potential insurgents. Therefore population density is included in the model using the 2000 estimate from the Gridded Population of the World (Center for International Earth Science Information Network, 2016). The discussion on conflict determinants is often framed in terms of greed versus grievance (Collier & Hoeffler, 2002), to differentiate between economic and identity based motives for rebellion or violence. Concerning economic motives to account for lower opportunity costs for conflict night-light emissions are used as a proxy for economic activity (Henderson, Storeygard, & Weil, 2012). The initial average per grid-cell in 1992–93 is included as variable along with the change in average value, as a growth proxy, between 1992–93 and 2001–02. Concerning identity-based motivations for conflict a variable for ethnic polarisation (Garcia-Montalvo & Reynal-Querol, 2005) is constructed using data from GeoEPR (Wucherpfennig, Weidmann, Girardin, Cederman, & Wimmer, 2011). Ethnic polarization is normally measured using an index based on population numbers but unfortunately such information is not available at the local level. Therefore the area in a grid-cell covered by a particular group is used. A caveat of this approach is that there can be overlap between different groups, meaning that the index exceeds the unity interval. The values are therefore normalised to restrict the values between 0 and 1. Another shortcoming of the data is that it doesn't capture the whole universe of local ethnic groups. There is however no comparable dataset available with the same coverage. Table 1 provides summary statistics.

Table 1
Summary statistics.

Variable	All cells N = 2,525	Conflict N = 639	No conflict N = 1,886
Conflict incidence	0.25 (0.43)	1 (0)	0 (0)
Conflict prevalence	0.06 (0.14)	0.23 (0.20)	0 (0)
Temperature	0.41 (0.15)	0.46 (0.13)	0.40 (0.16)
Precipitation	−0.03 (0.41)	−0.02 (0.39)	−0.04 (0.41)
Conflict incidence $_{t-1}$	0.27 (0.44)	0.52 (0.50)	0.18 (0.38)
W Conflict incidence $_{t-1}$	2.01 (2.26)	3.32 (2.29)	1.56 (2.07)
Population density	26.67 (73.05)	57.02 (127.01)	16.37 (35.61)
Night-light emissions	0.13 (0.42)	0.26 (0.62)	0.09 (0.32)
Δ Night-light emissions	0.05 (0.14)	0.07 (0.17)	0.04 (0.13)
Ethnic polarization	0.04 (0.10)	0.06 (0.14)	0.03 (0.07)

NB—Reported are means with standard deviation between parentheses.

2.4. Exploratory data analysis

A preliminary data examination does not show a strong empirical pattern linking conflict to adverse changes in local climate (Fig. 3). A *t*-test does reveal that the average change in temperature was higher in grid-cells that experienced conflict with a probability larger than 0.99; precipitation levels were also higher with a probability of 0.82. To delve deeper the conditional probability of conflict on temperature change is examined. To account for state dependence in conflict the sample is restricted to grid-cells that did not have any reported conflict events between 1989–2002. This is important as the autocorrelation in the data is high: the probability of conflict between 2003–17 conditional on conflict between 1989–2002 is 0.49 (0.16 for other cells). Limiting the sample reduces the number of observations but 73.6% of the original data is retained. The conditional probability is calculated using different threshold on the 0.1–0.7 interval using 100 equally-sized increments. In other words, I calculate the probability of conflict

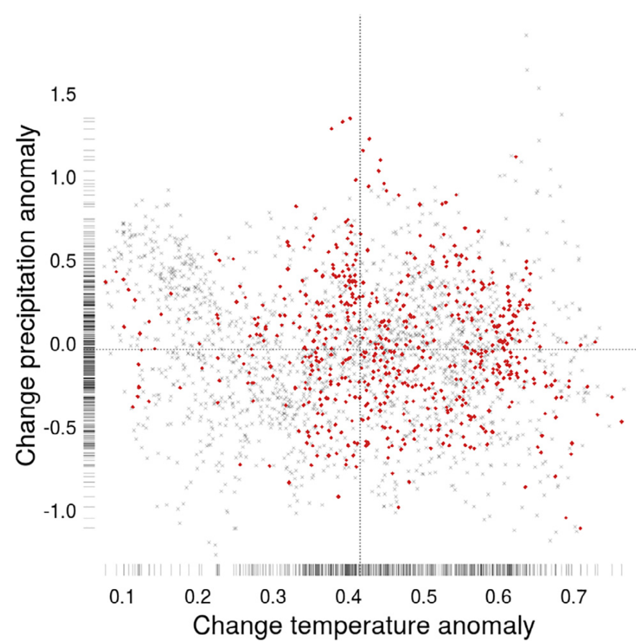

Fig. 3. Change in precipitation versus change in temperature anomaly comparing the years 2003–17 with the benchmark period (1988–2002). Dotted lines indicate sample averages for precipitation (horizontal) and temperature (vertical); red diamonds indicate grid-cells with conflict between 2003–17; grey crosses are cells without reported conflict. Tick marks on the *x* and *y* axes show the distribution for the grid-cells with conflict. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

conditional on a temperature change of at least size i, where i is located on the 0.1–0.7 interval. For example, for $i = 0.5$ this probability is 0.18. The results of this exercise show a gradual increase in conflict risk as temperature increases (Fig. 4). The probability peaks at 0.23 at a temperature anomaly increase of 0.61, after which it decreases to reach a minimum of 0.10 at a temperature anomaly increase of 0.68.

3. Empirical strategy

The variables' explanatory power is estimated using Bayesian Model Averaging (BMA) (Raftery, Madigan, & Hoeting, 1997), with R's 'BMA' package (Raftery, Hoeting, Volinsky, Painter, & Yeung, 2018). Within this Bayesian framework the identification problem is framed in terms of uncertainty with regard to the 'true' set of variables—or model uncertainty. Suppose there are K different variables, this means there are 2^K different models for the researcher to consider, assuming there is no preference for any particular specification beforehand. With BMA, to account for model uncertainty, the posterior probability is estimated for all 2^K possible permutations, calculating a weighted average over the most-likely models. Collecting all theoretically relevant variables K in matrix X and with 2^K possible models that make up model space $\mathscr{M} = \{M_1, \ldots, M_K\}$; the estimation framework can be written as

$$y_i = \alpha + \beta_k X_{ki} + \epsilon_{ki} \tag{5}$$

where y_i is conflict incidence in cell i; α is a constant term; β_k the effect of the explanatory variable on conflict incidence in model k; and ϵ_{ki} the Gaussian error term. The quantities of interest within this framework are the model-weighted posterior distributions of the coefficients:

$$Pr(\beta|y,X) = \sum_{k=1}^{2^K} Pr(\beta|M_k, y, X) Pr(M_k|y,X) \tag{6}$$

This equation provides a way of summarizing model uncertainty after having observed the data. The last term in this equation is the model weight – for model M_k – which is based on the model's posterior probability, or

$$Pr(M_k|y,X) = \frac{Pr(y|M_k,X)Pr(M_k)}{Pr(y|X)} \tag{7}$$

where $Pr(M_k)$ is the model's prior probability and $Pr(y|M_k,X)$ the marginal likelihood. Similar to Zhukov (2016) a uniform distribution is used for the model's prior since there is no justification to prefer one model specification over the other given the sensitivity of research results in the conflict literature (Hegre & Sambanis, 2006).

A major advantage of BMA is that by estimating all 2^K possible models it provides a general assessment of a variable's performance across the whole model space. Whether a particular variable contributes consistently to the models' explanatory power can be assessed by summing the posterior probabilities of the model that include that variable (Montgomery & Nyhan, 2010). Calculating the posterior inclusion probability (PIP) one has an indication of the likelihood of inclusion in the 'true' model.

Concerning statistical inference, an estimate of the sign and magnitude of the coefficient can be obtained by averaging the expected value (EV) across the model space. Mathematically the expected values for coefficient β is given by

$$E(\beta|y,X) = \sum_{i=1}^{2^K} Pr(\mathscr{M}_k|y,X)\, E(\beta_k|\mathscr{M}_k, y, X) \tag{8}$$

As discussed by Zhukov (2016) there are a number of benefits associated with BMA, which include (i) giving an overall performance assessment across different model specifications; (ii) providing information on whether a variable consistently contributes to the models' performance; and (iii) being a transparent model selection tool preventing cherry-picking based on statistical significance. In addition, it provides better predictions due to averaging across all the possible models (Raftery et al., 1997). A drawback is that the software implementation is currently limited to a select number of generalised linear models.

A final note on estimation. For this study all explanatory variables, except those for climate, are measured before the realisation of the outcome variable. This to prevent endogeneity resulting from reverse causality and avoid a bad control problem where some explanatory variables could be an outcome variable in the model. As such, conflict is predicted based on more or less structural factors measured before 2003 and the contemporaneous change in climate. All input variables are placed on a common scale—subtracting the mean and dividing by twice the standard deviation—to ease interpretation and making the coefficients directly comparable to each other (Gelman, 2008).

4. Discussion of the empirical results

Fitting a logit model to the binary outcome variable (conflict incidence) shows a strong correlation between temperature change and conflict risk but a relatively weak link between precipitation change and conflict risk (Fig. 5, panel a). For 512 possible models the posterior inclusion probability (PIP) is 0.96 for temperature and 0.28 for precipitation. The results do not suggest a strong spillover effect from climate, where conflict risk in cell i is influenced by local climate changes in the k neighbouring cells. The probabilities that the coefficients for the spatial lag of temperature and precipitation do not equal zero are 0.08 and 0.14 respectively. These estimates need to be interpreted with caution given the high correlation ($\geqslant$0.90); the result of existing spatial correlation and the manner in which the datasets were constructed, using kriging to impute missing values. Omitting these variables leads to quantitatively similar results. The analysis relies on aggregating point-based data inducing the risk of statistical bias emerging from the modifiable areal unit problem (MAUP). Aggregating the data to a resolution of 2-degree does not alter the conclusions (Table 2, rows 7–8).

The three strongest explanatory variables in the model space are the temporal and spatial lag of conflict along with population density; with a PIP of 1 each (Fig. 5). Variables accounting for

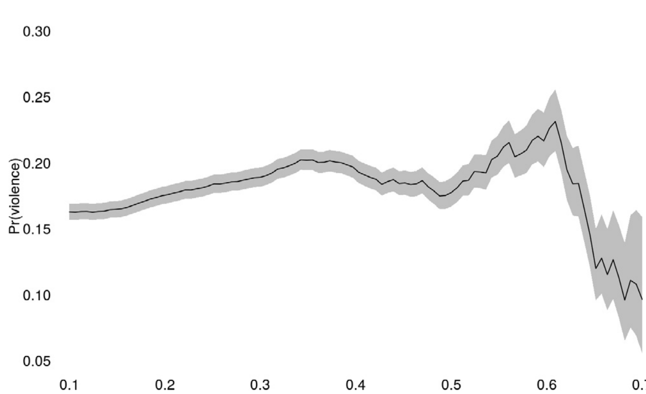

Fig. 4. Conditional probability of conflict at different thresholds of the change in the average temperature anomaly for cells without conflict between 1989–2002. Grey-shaded area represents 50% uncertainty interval.

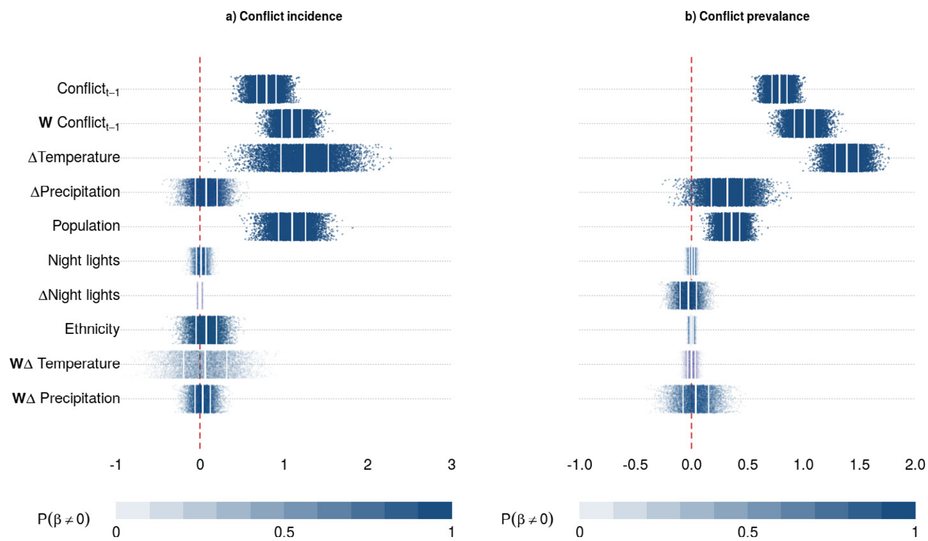

Fig. 5. Local determinants conflict incidence (*a*) and conflict prevalence (*b*). Blue dots represent draws from the posterior distribution for each respective variable; vertical lines indicate the mean and 66% uncertainty interval. The posterior inclusion probability of a variable is reflected by the opacity of the points with full transparency indicating $P(\beta \neq 0|y, X) = 0$ and full opacity indicating $P(\beta \neq 0|y, X) = 1$. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Table 2
Robustness checks.

	Specification	Temperature		Precipitation	
		PIP	EV	PIP	EV
1	Incidence 2003–17	96	1.2 (0.3)	28	0.1 (0.1)
2	Prevalence 2003–17	100	1.4 (0.1)	87	0.3 (0.2)
3	Incidence 2003–17: 1981–2002 climate benchmark	91	1.0 (0.4)	42	0.2 (0.3)
4	Prevalence 2003–17: 1981–2002 climate benchmark	100	1.4 (0.1)	87	0.3 (0.2)
5	Incidence 2007–17	90	0.9 (0.3)	38	0.2 (0.3)
6	Prevalence 2007–17	99	1.0 (0.2)	75	0.3 (0.2)
7	Incidence 2003–17: 2-degree aggregation	85	1.2 (0.6)	7	0.02 (0.08)
8	Prevalence 2003–17: 2-degree aggregation	100	1.5 (0.2)	77	0.3 (0.2)
9	Conflict onset 2003–17	63	0.5 (0.4)	2	0 (0.02)
10[a]	Conflict events 2003–17	91	1.1 (0.4)	20	0.1 (0.2)
11[b]	Change conflict prevalence 2003–17	100	0.058 (0.008)	91	0.021 (0.008)
12	Incidence 2003–17: country indicators	37	−0.4 (0.5)	4	0.01 (0.06)
13	Prevalence 2003–17: country indicators	3	−0.01 (0.06)	39	0.2 (0.2)
14[c]	Incidence 2003–17: 15-y moving average	1	0.0 (0.1)	100	0.29 (0.06)
15[c]	Incidence 2003–17: annual deviation	100	0.59(0.08)	100	0.44 (0.06)
16.1[c]	Incidence 2003–17: annual deviation	0.7	0 (0.01)	2.6	0 (0.02)
16.2[c]	Incidence 2003–17: long-term average	100	0.92 (0.07)	100	0.70 (0.06)

PIP: posterior inclusion probability; EV: expected value.
[a] Data fitted using quasi-Poisson model; outliers (3) removed from sample.
[b] Data fitted using Gaussian model.
[c] Model excludes spatial lag of climate due to collinearity. Includes year indicators to account for common shocks.

classic greed-or-grievance based motivations do not feature prominently. The proxy for economic activity (night-light emissions) possibly also captures urbanisation levels; its effect might therefore be captured by population density. Indeed, excluding population density from the model space increases the PIP for night-lights to 1. It might be germane to briefly reflect on the interpretation of the effect of population density, which is a strong explanatory variable. Given the reliance on media reports for the conflict data coding we have to be aware of the bias introduced through selective reporting. Although areas with higher population densities could face higher risk due to fiercer competition over local resources or strategic considerations by insurgents; it could also be the case that violence in rural areas goes unnoticed as they are not deemed news-worthy. This is an issue that deserves more research attention, although beyond the scope of this particular study.

In terms of estimated magnitude, the expected value (EV) of the temperature variable equals 1.2 (standard deviation (s.d.) = 0.3). At the upper bound a two-standard deviation increase is associated with a 30% increase in conflict risk; this is similar to the reported effect by the meta-analysis of Hsiang et al. (2013) (28%). There is some non-linearity—as illustrated by Fig. 4—with the turning point at a temperature increase of 0.62. Precipitation's EV is considerably lower at a 1 percent increase in conflict risk. Omitting precipitation from the model space increases the root mean squared error by just 0.0006. The estimates are sensitive to the choice of benchmark period. Using a longer benchmark period (1981–2002) increases precipitation's PIP to 0.42 although the estimates magnitude remains marginal (Table 2, row 3). For temperature the results remain largely unaltered with an EV of 1.0 (s.d. = 0.4). Shifting the period to measure changes for the period 2007–17 relative to 1988–2006 produces similar results (Table 2, rows 5–6).

Testing the extensive margin there is the risk that the correlations shown in the data are the result of erroneously matching conflict at the beginning of the period with changes in local climate that occur later. To account for this an alternative outcome variable (conflict prevalence) is used testing the intensive margin. Conflict prevalence is measured as the proportion of years a grid-cell experienced conflict between 2003 and 2017 (Fig. 2). The outcome bounded between 0 and 1, the data is fitted using a quasibinomial generalised linear model since ordinary least squares would, in all likelihood, produce biased estimates as the fitted values are not constrained to the unity interval. Using a different outcome variable produces similar results (Fig. 5, panel b). A main difference is that precipitation's PIP increases to 0.87; the EV remains small at 0.3 (s.d. = 0.2). The main results are also robust using other outcome variables such as conflict onset or the number of conflict events (Table 2, rows 9–11). Conflict onset captures the outbreak of new conflicts: it is a binary indicator that takes values 1 if there was a reported conflict between 2003–17 but not between 1989–2002, and 0 otherwise. Using this variable there is a considerable decrease in the PIP and EV for both temperature and precipitation. Importantly, this suggests that the relation between local climate and conflict is mainly driven by conflict continuation rather than the outbreak of new ones. The results are not robust to the inclusion of country indicators, to adjust for possible country-level factors such as colonial history. The probability that the expected values do not equal zero drops considerably (Table 6, rows 12–13). While the pooled estimates show that warming is associated with conflict risk, there is seemingly no strong correlation between climate and conflict once adjusting the estimates for common national factors. This could entail that the determinants of conflict are socio-political in nature (Linke et al., 2015; Wischnath & Buhaug, 2014); where local conflict is linked to national characteristics such as centre-periphery relations (see Mach et al. (2019)). The data shows that the proportion of cells reporting conflict in a country increases with temperature (Fig. A1), but there is almost no difference in average temperature change (relative to the country mean) for grid-cells with conflict incidence or onset compared to grid-cells without reported conflict. This could entail—as a reviewer suggested—that conflict is not experienced locally. Instead warming could increase conflict risk anywhere in the country. For instance in more favourable locations where there is access to resources (Detges, 2014) or in urban areas through internal migration by those affected by climatic stress (Ash & Obradovich, 2019). Research has shown that climate-related conflict is more likely when migration is not a viable mitigation strategy, e.g. due to poor road infrastructure (Detges, 2016) An important caveat concerning country fixed effects is that the estimates are based exclusively on within-country variation; treating each country as unrelated to each other, eliminating between-variation that could help improve the estimates. That is potentially problematic in this setting given that the inference is based on little variation for some countries, as the median number of data points is about 35.

Whether weather variability is correlated with conflict is something that cannot be entirely ruled out at first instance. Using cross-section time-series data produces contrasting results (Table 2, rows 13–14). A 15-year moving average shows no average effect for temperature but a strong positive correlation for precipitation. A caveat is that this approach risks introducing a bias in the data through the Slutsky effect (Kelly & Ó Gráda, 2014). Annual shocks, relative to the benchmark period, produces estimates suggesting a positive correlation between weather variability and conflict risk. Similarities between estimates based on time-series variation and long-differences have been documented elsewhere (Burke & Emerick, 2016; Burke, Hsiang, & Miguel, 2015; Dell, Jones, & Olken, 2012). This approach might capture both the short and long term effects however. To distinguish between the two the long-term change in local climate is included in the model space (Table 2, rows 16.1–16.2). The results show that there is a stronger correlation between conflict risk and long-term changes in contrast with short-term shocks. The latter seems inconsequential once accounting for the long-term trend.

Finally, the analysis has been agnostic about conflict type. Some forms of violence might exhibit stronger links with local climatic conditions (Fjelde & von Uexkull, 2012; Theisen, 2012). To accommodate this the data is fitted using three different outcome variables distinguishing between different conflict types: (i) state-based or civil conflict, (ii) non-state or communal conflict, and (iii) repression or violence against civilians. Fig. 6 summarises the results, showing an increase in uncertainty associated with the estimates as well as a reduction in the magnitude. For instance, for communal violence (panel b) the PIP of temperature drops to 0.23 and the EV to 0.3 (s.d. = 0.7). In contrast, the spatial lag of temperature has a PIP of 0.81 with an EV of 1.3 (s.d. = 0.7). This suggests that regional changes are more salient compared to local changes. An important caveat however is collinearity: Dropping the spatial lag shows high posterior inclusion probabilities for temperature for all three conflict types.

4.1. Cross-validation

An important check is to test whether the models capture an underlying relationship or in contrast just fit the data's idiosyncrasies. To this end a leave-one-out cross-validation exercise is carried out to examine the out-of-sample predictive power. As a benchmark a model space is considered including all non-climate variables that had a posterior inclusion probability of 1 (population density and the spatial and temporal lag of conflict). This model space is compared to one that includes the variables capturing changes in local climate (temperature and precipitation). Predictions are generated by leaving out one grid-cell at a time; using the remaining data to estimate the parameters and predict the outcome for the left-out cell. This process is repeated for all grid-cells in the sample. The predictive performance is compared to see whether including information on climate change improves predictive accuracy. Predictive accuracy is measured by precision-recall (PR) curve. This curve plots the relation between the true positive rate (recall) and the precision of the model (Davis & Goadrich, 2006; Saito & Rehmsmeier, 2015), or the number of true positives relative to the predicted number of positives. The PR curve is preferred over the commonly used receiver-operator characteristics (ROC) curve as the latter suffers from a bias as it can correctly predict a large number of true negatives at different threshold for a relatively rare event such as conflict without penalty which will inflate the true negative rate.

The results show (Fig. 7) that omitting information on climate leads to poorer predictions as the benchmark curve is closer to the baseline. Note that the baseline is set at 0.25 (the proportion of grid-cells with reported conflict between 2003–17). A junk classifier randomly assigning probabilities would attain this curve. The fact that changes in local climate improve the predictive performance of the model contrasts with other studies that have focused on climate variability (O'Loughlin, Linke, & Witmer, 2014;

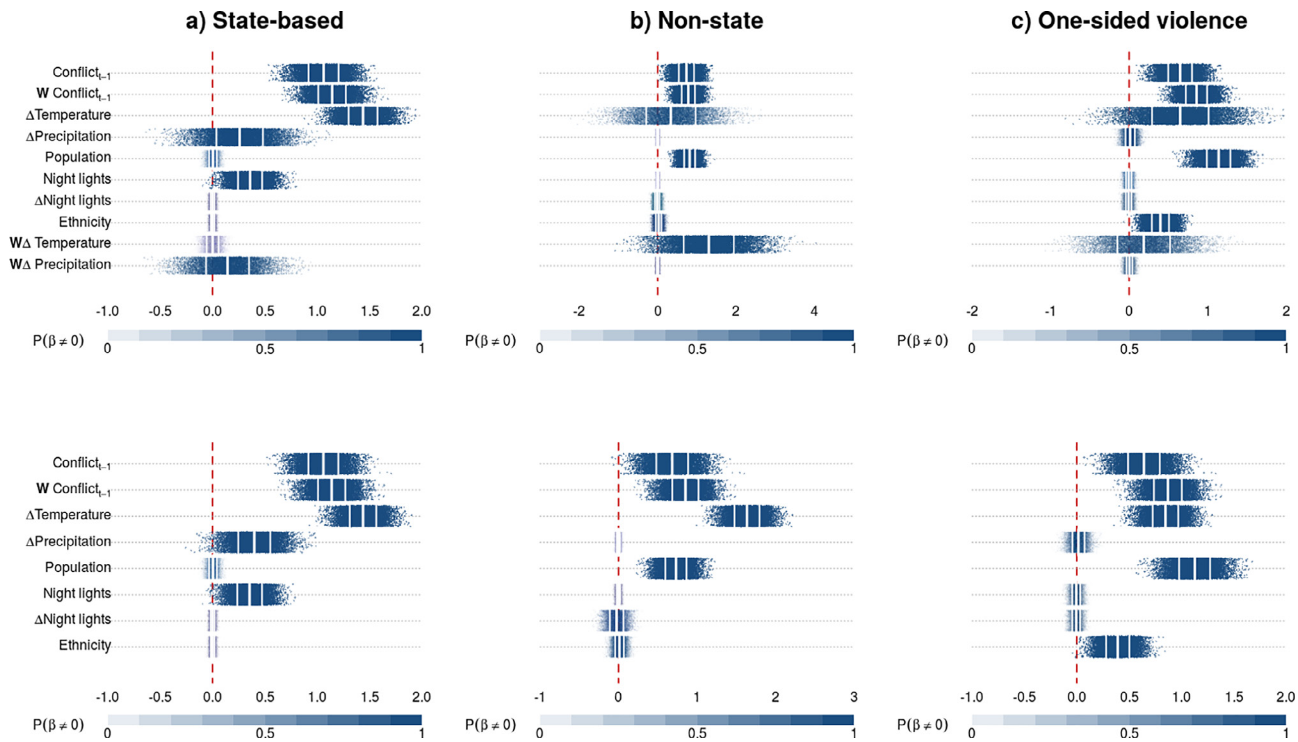

Fig. 6. Local conflict determinants for different types of conflict: state-based (a), non-state (b), and violence against civilians (c). Blue dots represent draws from the posterior distribution for each respective variable; vertical lines indicate the mean and 66% uncertainty interval. The posterior inclusion probability of a variable is reflected by the opacity of the points with full transparency indicating $P(\beta \neq 0|y, X) = 0$ and full opacity indicating $P(\beta \neq 0|y, X) = 1$. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

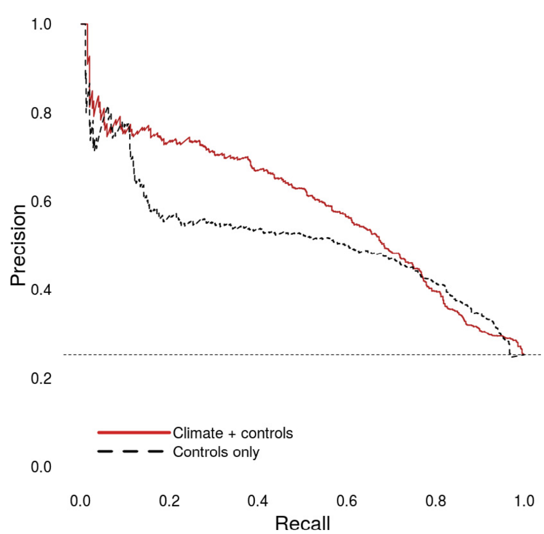

Fig. 7. Precision-recall curve in-sample prediction of conflict between 2003–17 per grid-cell for the best model of the Bayesian Model Averaging result (solid red line) and a benchmark model omitting the climate variables (dashed line); the dotted line indicates the baseline. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Wischnath & Buhaug, 2014) and climate change (van Weezel, 2019).

The differences are small though. The F-score—the harmonic mean of precision and recall (van Rijsbergen, 1979)—is 0..46 for the benchmark compared to 0.48 for the model space including the climate variables. The difference is more pronounced considering the area under the curve: 0.51 compared to 0.58. Importantly, both models perform better than random guessing. This illustrates that reasonably accurate predictions can be made on the bases of just a factors such as past conflict and population density—although it does not provide much in terms of understanding conflict dynamics. Adding information on climate change, particularly temperature, improves the performance by about 7 points.

Although the aggregate performance is decent, getting accurate point predictions is more challenging as illustrated by the prediction error in panel a of Fig. 8. There tends to be over-prediction of conflict incidence on the basis of historical patterns, as shown in Angola and South Africa. While new conflict zones—Central African Republic, Libya, Nigeria—are under-predicted. This reflects earlier results, specifically using conflict onset as outcome variable.

To identify potential climate conflict hot-spots, the predicted values can be contrasted with those generated by the benchmark (panel b Fig. 8). The results illustrate that the benchmark assigns higher conflict probabilities predominantly to areas in southern Africa. In all likelihood these are driven by past conflict patterns such as the struggle against Apartheid in South Africa and the civil wars in Angola and Mozambique. Accounting for changes in local climate, the predicted probabilities are higher in three specific regions: Egypt, the Horn of Africa, and the area around Lake Chad. This pattern reflects the temperature increases as shown in panel a of Fig. 1. Focusing on the difference in predicted values might be more fruitful to identify risk areas as is illustrated by the case of northern Nigeria.

5. Conclusions

There is an ongoing debate on the salience of climate as a determinant of violent armed conflict (Mach et al., 2019). While some have argued that there is a preponderance of evidence linking adverse climatic conditions to increased conflict risk, other studies have shown that the contribution is relatively marginal. One

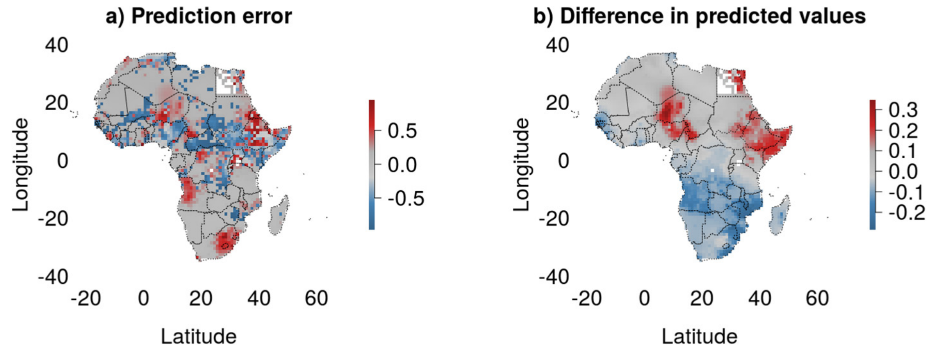

Fig. 8. Prediction error (*a*) and difference in predicted value relative to benchmark model (*b*) per grid-cell for conflict incidence between 2003–2017. For panel *a* red values indicate false positives while blue values indicate false negatives. For panel *b* red values indicate higher predicted values relative to the benchmark and blue vice versa. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

important deficiency of the literature is that conclusions on the link between climate change and conflict are often drawn on the basis of empirical analyses using climate variability as a proxy for climate change. Although examining the effects of climate variability has its merits, as it provides us with insights on the timing and duration of conflict in relation to climatic conditions or weather shocks (e.g. Breckner & Sunde, 2019; Maystadt & Ecker, 2014), it does not allow us to make any claims about the effect of climate change, which is a more gradual long-term process. This study has aimed to address this issue by focusing on relatively long-term changes in local climatic conditions across Africa and estimate whether these changes can help explain conflict risk. The empirical analysis does lend some support to the hypothesis that climate change is linked to violent armed conflict. The regression analysis showed that temperature changes contributed consistently to explaining variation in conflict across a large number of different model specifications. Thereby echoing the results of similar studies (e.g. Harari & La Ferrara (2018)). This result was robust to changing the outcome variable, using different time periods, and adjusting the estimation for a number of other possible conflict determinants; but not when adjusting the estimation for country-specific factors. Importantly, including climate also improved the predictive performance of the model considerably and could in the future potentially help identify areas that are vulnerable to climate-induced conflict. The analysis also provides some evidence suggesting that changes in local climate is more strongly linked to the continuation of existing conflicts, instead of the outbreak of new conflict. This potentially indicates that climate-induced conflict is mainly the result of a decision-making process at the individual level, where people join an ongoing insurgency or rebellion as a result of adverse changes to their livelihood or the prospect of short-term gains. This in contrast with a collective decision by a larger pool of people. We could conjecture from this that climate change affects people in already vulnerable areas, rather than that it increases conflict risks across the board. The association between adverse changes in local climate and conflict risk also suggests a potential lack of adaptation. This could be due to the fact that ongoing conflict disrupt civil society and the local economy, hampering the adaptation of suitable strategies to deal with changes in climate.

The analysis presented here does come with a number of caveats. First, the results are based on reduced-form estimation. The research design therefore provides no insights into the particular mechanisms linking climate and conflict, in contrast with other studies exploiting climate variability (e.g. Crost et al., 2018; Maystadt & Ecker, 2014). This limitation is partially due to data constraints. For instance, there is a paucity of information on local commodity prices, both in terms of spatial and temporal coverage; while there is no information at all on local trade which would allow for testing interesting hypotheses linking climate change to conflict through inter-group trade collapse Olsson (2016). One avenue that could possibly be further pursued is examining whether adverse changes to agricultural areas focused on cash crop production, as opposed to subsistence farming, affect conflict risk differently (Papaioannou & de Haas, 2017). Second, the coding rules for the conflict data entail that the included number of conflict events is likely an underestimate of the true level of conflict. This could have biased the results; making some areas look more peaceful than they are. Here one also has to consider that climate could be linked to other forms of social distress (e.g. Papaioannou & de Haas (2017)). Third, the possibility of conflict displacement is something not really addressed in this study. It could well be that adverse changes in local climate leads to violence in areas that are less affected (Detges, 2014). On the longer run, migration from adversely affected regions to urban areas could increase tensions, again possibly culminating into violence. Migration is possibly an important intermediary variable (Freeman, 2017). The interconnections between climate change, migration, and conflict is given the available data at subnational level still hard to disentangle. Fourth and finally, the estimates were not robust to adjustments made for country-level factors. Whether this is because of lack of within-country variation or due to unmeasured country-level factors influencing conflict risk is unclear. The interaction between local climate and country-level determinants is a possible area for future research.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A

See Fig. A1.

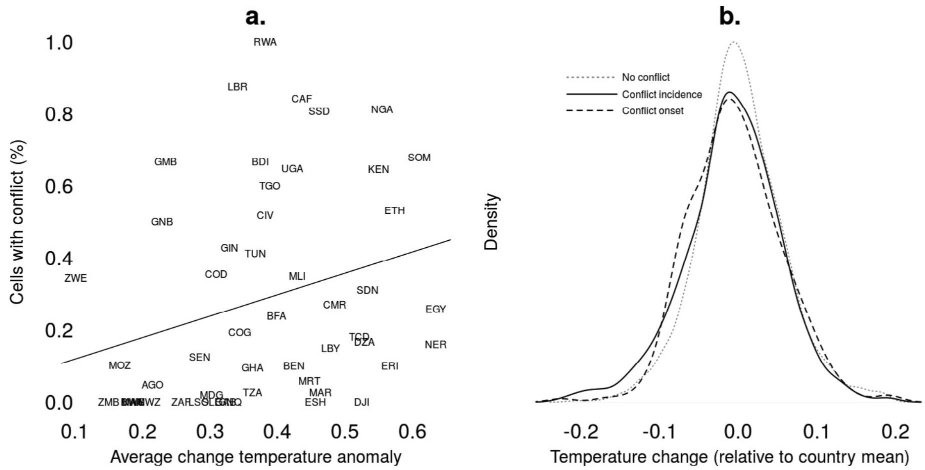

Fig. A1. Cross-country heterogeneity. (*a*) Average change in temperature anomaly versus proportion of grid-cells with conflict between 2003–17; (*b*) Distribution of change in temperature anomaly for 2003–17 relative to country mean for three states: no conflict; conflict incidence; conflict onset.

Appendix B. Supplementary data

Supplementary data associated with this article can be found, in the online version, athttps://doi.org/10.1016/j.worlddev.2019.104708.

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