Article

Heterogeneous impacts of climate conditions on conflict

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Abstract

Climate change has been linked with increased conflict risk through resource scarcity or forced migration but there remains a gap in understanding how these impacts vary across different types of conflicts. This study addresses this gap by examining the heterogeneous effects of temperature and precipitation levels and long-term deviations on various conflict categories. I employ count data panel regression and a long-difference approach with UCDP data for 2780 conflicts from 1998-2020 in 81 low- and mid-income countries. My main finding confirms that climate impacts are indeed heterogeneous across conflict categories. A one-year lagged temperature change of 1°C increases the expected mean count of non-state conflict by 8.54% while armed conflict and one-sided violence are not affected significantly. My data suggests different vulnerability levels, as warmer countries also experience higher real conflict counts. The long-difference approach supports these results. Contrary to earlier studies, I did not find robust evidence for precipitation effects.

Keywords: Climate Change, Armed Conflict, Non-state Conflict, One-sided Violence, UCDP, Climate Conflict Nexus, Temperature, Precipitation, Poisson, Negative Binomial, Panel Regression, Long-difference

1. Introduction

International policymakers have long voiced the opinion that climate change impacts violence and conflict. Ban Ki Moon, secretary general of the United Nations (UN) for nine years, wrote in 2007: *"[…]the Darfur conflict began as an ecological crisis, arising at least in part from climate change*." as he claims drought-induced water and food shortages ignited the deadly ethnical conflict. The former president of the United States of America (Obama 2015) even stated: "*Around the world, climate change increases the risk of instability and conflict*." In the UN Security Council however, resolutions connecting climate and security have been vetoed by Russia, India, and China who claim lacking scientific evidence for a climate conflict link (Buhaug et al. 2023). Although leading scholars and the Intergovernmental Panel on Climate Change (IPCC 2022) agree that climate change can act as a "threat multiplier" and "risk factor" (Mach et al. 2019) through various indirect pathways like migration or resource scarcity, a direct causal relationship between climate and conflict remains disputed (Hendrix et al. 2023).

My paper contributes to this debate by answering the question "How do temperature and precipitation influence the count of conflicts across different conflict categories?". Previous studies did not account for conflict heterogeneity, neglecting the fact that climate conditions might influence different forms of conflict through different pathways and with different magnitudes. I address the issue of conflict heterogeneity by estimating climate effects on armed conflict, non-state conflict and, one-sided violence with separate panel regressions and a long-difference approach, using combined data from the Uppsala Conflict Data Program (UCDP) and the Climate Research Unit (CRU) for 81 low- and middle-income countries from 1998 to 2020. My contribution to the literature is threefold. First, my paper systematically accounts for conflict heterogeneity by estimating regression models separated by UCDP conflict types to identify heterogeneous climate effects. Second, I add a count data approach to the existing panel data studies, which is better suited to capture violence intensity than binary violence variables from previous studies. Third, I adapt a long-difference approach to the climate conflict nexus to ensure that my results capture long-term changes caused by climate instead of short-term weather variations. By not limiting my sample to a specific area, I also tackle potential sampling bias.

I find that the effects of climate on the number of conflicts are indeed heterogeneous. Both the panel model and the long-difference model show that increases in temperature are positively correlated with non-state conflict counts but not armed conflict and one-sided violence. An increase in one-year lagged temperature of 1°C is associated with an increase in the expected mean count of non-state conflict by 8.54%. The long-difference approach confirms this finding. Precipitation results are not robust over both approaches. Descriptive analysis also indicates that countries exhibit heterogeneous climate change vulnerability because those with a higher base temperature experience higher mean conflict counts. I argue that alongside issues like sampling bias and different methods, not accounting for conflict heterogeneity is a key factor why researchers still produce mixed results in the climate conflict nexus. Figure 1 illustrates such heterogeneity across conflict types as the number of events increases at different margins. Although all three conflict types have risen in the last decade, the effect is strongest in one-sided violence. In times of rising global mean temperature calls for correlation remain easy to make but difficult to back up with data (Hendrix et al. 2023).

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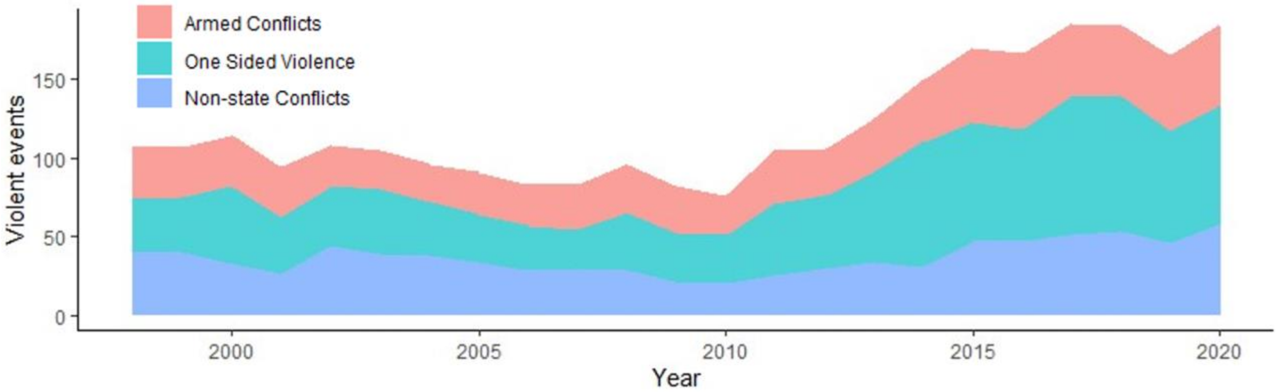

Figure 1: Total number of violent events (1998-2020)
The number of annual armed conflicts, non-state conflicts and one-sided violence adds up the number of violent events. Different conflict types show different margins of increase due to conflict heterogeneity. The sample used includes 81 selected mid and low-income countries. Own visualization. Source: UCDP/PRIO

The paper is structured as follows. Chapter 2 presents a detailed state of the literature. This includes pathways from climate conditions to violence and the conflict heterogeneity problem. Chapter 3 describes the methods, particularly the sample, different panel models, the long-difference approach, and the data used. Results of the panel models and the long-difference approach are presented in Chapter 4. Chapter 5 compares said results with previous findings, discusses new findings and empirical limitations of this study. Lastly, chapter 6 concludes.

2. State of the literature

After the end of the Cold War area, the focus of peace researchers began to shift focus towards environmental impacts and resource scarcity, linking climate conditions as both a consequence and cause of civil war, armed conflict, and violence (Gleditsch 1998; Scheffran 1999). Early on Homer-Dixon (1991) introduced an influential framework to explain how environmental dangers can lead to acute conflict. Drawing on theory and case studies, he argues that environmental dangers affect both the economic and social sector, and combined with other, non-climate related, destabilizing factors, can be pathways toward different types of conflict. Miguel et al. (2004) then estimated civil conflict likelihood by using rainfall as an instrument variable for growth shocks in 41 African countries. They find negative growth shocks to increase conflict likelihood regardless of known conflict causes like income, institutions, and ethnical diversity. Further studies point out severe consequences of global warming, especially for the African continent. Burke et al. (2009) estimate an increase of armed conflict in Sub-Saharan Africa by 54% in 2030, causing 390000 additional battle deaths for a mean temperature increase of 1°C. In addition to temperature and rainfall effects, climate-related disasters have also been linked to past conflict onset (Ide et al. 2020). Case studies discuss if the 2003 Darfur conflict was influenced by insufficient rainfall (Kevane and Gray 2008). Scholars also claimed that the uprising in Syria was partly rooted in poor governance, after a severe three-year drought lead to food and water scarcity, forcing mass migration into overcrowded urban areas, where social living conditions dropped (Kelley et al. 2015).

The relation between climate and conflict is still heavily discussed due to empirical problems like omitted variable bias and sampling bias. While omitting important explanatory factors undermines the internal validity, studying only a small number of selected countries yields low external validity of past findings. Especially the African continent, conflicts with a high number of battle deaths or regions suffering extraordinarily from climate have been the focus of previous studies (Burke et al. 2015a; Adams et al. 2018; Hendrix 2017). Some researchers have argued this is due to data availability (Ide et al. 2023) while others see evidence for a streetlight effect (Adams et al. 2018). The streetlight effect is defined by Kaplan (1964) as "…researchers tending to focus on particular places for reasons of convenience." Case study evidence like Syria also remains highly disputed as neighboring countries like Lebanon or Jordan suffered from the exact same drought but did not experience any increase in violence (Hendrix 2017). Results also depend on the temporal unit of analysis (Coulibaly and Managi 2022). Nevertheless, latest literature reviews agree that climate conditions can act as an indirect multiplier to conflict risk and dynamics

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but the effects are highly dependent on context and non-climatic factors (Mach et al. 2019). In contrast to earlier reports, the IPCC (2022) has lately acknowledged impacts of climate variability and extremes on organized crime and armed conflict through different pathways. Due to anthropogenic climate change, future effects of climate conditions on conflict and violence are estimated to be even bigger but remain subject to large uncertainty (Mach et al. 2019).

2.1. Causal pathways

Even though the last decade produced a vast amount of empirical studies, causal pathway analysis and detailed investigation of mechanisms have long been one of the weak spots of climate conflict research (Ide et al. 2020). Still, recent studies find multiple possible pathways like lowering economic conditions, state capacity, and the opportunity cost of violence or increasing income inequality (Koubi 2017). Here, I present the two main pathways from climate to conflict - Resource scarcity and climate-induced migration flows.

Resource Scarcity has already been introduced in terms of water scarcity in the introduction. Most often understood as supply scarcity of environmental resources like cropland, forests, or food and fresh water, this pathway can affect small-scale violence like individual disputes or interstate war over water access through socio-economic conditions (Homer-Dixon 1991). Not only are these (renewable) resources dependent on climate but they also play a vital role especially in less-developed countries as primary sources of income and food in agriculture (Homer-Dixon 1999).
When considering climate change, an increasingly important pathway is migration. Each year over 20 million people have been internally displaced alone since 2008 due to extreme weather events and projections predict even more migration due to higher frequencies of these events (IPCC 2022). Scholars agree that environmental migration alone does not cause conflict but contributes to increasing conflict risk when paired with other socio-economic conflict catalysts, e.g., increasing ethnic tensions after cross-border migration (Reuveny 2007; Burrows and Kinney 2016).

Figure 2 presents a simple framework of the main pathways, based on Homer-Dixon's (1991) methodology and the recent state of the literature based on the IPCC (2022). Climate variability and extremes, both subject to projected increases due to climate change, impact societies through scarcity of food and water, leading to loss of income and livelihood, potentially triggering forced migration. Exacerbated by additional phenomena like poverty or a high economic dependency on agriculture they influence conflict dynamics and outbreak. Nevertheless, non-climate related factors like political and ethnical reasons remain the main sources of conflict onset (IPCC 2022).

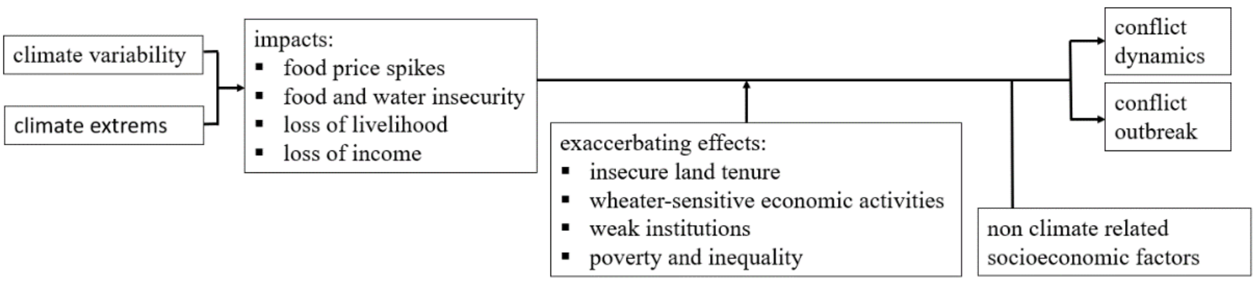

Figure 2: Pathways of climate conflict interaction
Own visualization. Methodology based on Homer-Dixon (1991). Effects are taken from the sixth assessment report of WG II of the (IPCC 2022a).

2.2. Conflict heterogeneity

Conflict definitions, violence thresholds, and structural differences make it difficult to compare violent events (Burke et al. 2015a). Thus, systematic accounting for conflict heterogeneity has gained increasing attention in empirical conflict research, e.g., regarding trade (Kamin 2022) or education (Unfried and Kis-Katos 2023). Some early climate-related studies separated different conflict types (O'Loughlin et al. 2014) but literature reviews have long aggregated all types of violent events (Burke et al. 2015a). Large empirical studies focused almost exclusively

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on state-based armed conflict and civil war, disregarding conflicts with no state involved (Fjelde and Uexkull 2012). Also, (one-sided) violence against civilians is often overlooked, even though actors increasingly weaponize scarce environmental resources like freshwater against civilians (King 2023).

Figure 1 already visualized conflict count heterogeneity from 1998 to 2020 in my sample, showing increases in armed conflict, non-state conflict, and one-sided violence at different rates. Figure 3 shows the absolute number of deaths in conflicts from 1998 to 2021 by conflict type to picture the heterogeneous intensity of violence. A long-term stabilization in higher death levels can be observed in the last decade for both state-based conflict and non-state conflict while one-sided violence deaths remain relatively stable. I argue that it is highly unlikely that climate impacts are equal over all forms of violence because pathways to different violent events differ fundamentally (Homer-Dixon 1991). My methodology does specifically account for conflict heterogeneity. Methods will be further explained in the next chapter.

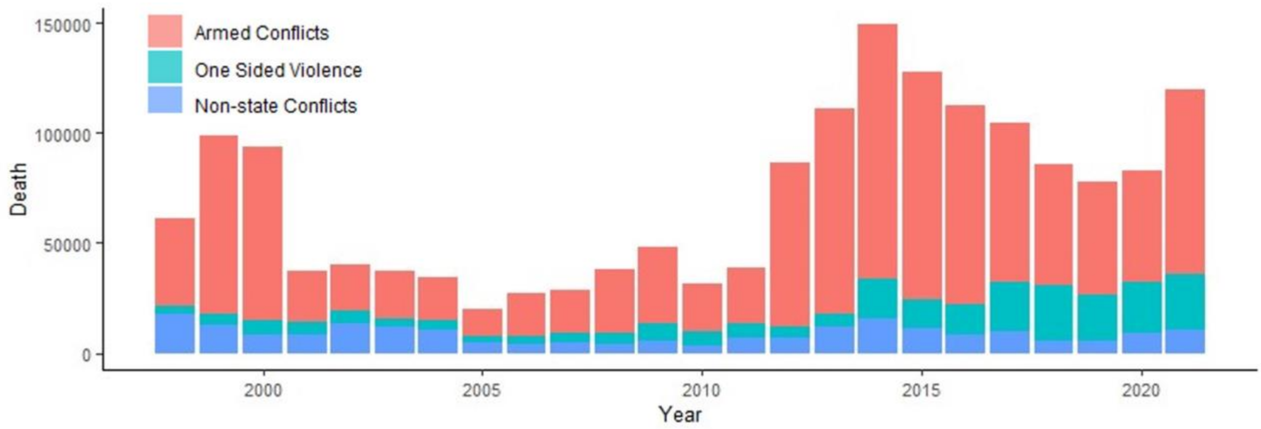

Figure 3: Global deaths in conflicts (1998-2021)
Deaths numbers are best guess estimates, including direct deaths of both military personnel and civilians. Own visualization, based on Roser et al. (2016). Source: UCDP

3. Methods

In this paper, I use two well-established approaches from climate economics to estimate the effect of climate conditions on conflicts. First, I use a panel regression but adapt the models to count data. Second, I focus on long-term effects in a long-difference approach. To account for conflict heterogeneity, I apply identical models with each of the three conflict types as dependent variables to obtain comparable results by type of conflict. To test the hypothesis that warmer countries experience higher climate change impacts I use a reduced sample of 57 countries with a baseline temperature (1901-2000) of 20°C or more to account for structural differences in climatic conditions.

3.1. Annual panel model

Equation (1) presents the general model introduced by Miguel et al. (2004):

$$\text{conflict\_variable}_{i,t} = \beta \times climate\_variable_{\ i,t} + \phi_i + \psi_t + \varepsilon_{i,t} \qquad (1)$$

where a conflict variable in a location *i*, in a time period *t* is explained by the product of the estimated coefficient β and a climate variable. ϕ_i and ψ_t represent location and time-fixed effects, holding country or time-specific factors fixed. Finally, the error term of the model is called $\varepsilon_{i,t}$. It is also common to include additional control variables and lagged climate variables to account for delayed violent event outbreaks through long-term pathways caused by climate impacts (Burke et al. 2015a). Linear regression can be used for estimating climate impacts on conflict despite non-linear long-term temperature and precipitation effects by interpreting the regression line as a local linearization of a (most likely) nonlinear function (Burke et al. 2015a, 2015b). Here the 23-year sample period is short enough to assume such a linearization (Appendix, Figure 12).

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The dependent variable can either be modeled as binary, indicating the incidence of conflict, or as count data, indicating the total number of conflict events. Cappelli et al. (2022) argue that count data approaches, with the annual number of conflicts in a location, generate more insight by reflecting the intensity of conflict. Because the sixth assessment report of the IPCC (2022) states that climate influences the dynamics of conflict more than conflict outbreak risk, I use conflict counts to better suit the climate conflict relation. Count data takes nonnegative integer values, is of a discrete nature, and is often not normally distributed, thus violating the OLS assumptions of normal distributed residuals and homoscedasticity and leading to biased OLS estimates (Beaujean and Grant 2019). A common choice when analyzing count data is Poisson regression (Cameron and Trivedi 2015), where a dependent count variable y_i given an independent variable x_i is Poisson distributed with a density function

$$f(\mathrm{y}_i|\mathrm{x}_i) = \frac{e^{-\lambda_i}\lambda_i{}^{\mathrm{y}_i}}{\mathrm{y}_i!} \tag{2}$$

where λ_i is the conditional mean of the distribution.

$$E[\mathrm{y}_i|\mathrm{x}_i] = \lambda_i = \exp(\mathrm{x}_i\beta) \tag{3}$$

Poisson regression does assume that the conditional mean of the distribution λ_i equals the variance of the distribution, which is also known as equidispersion (Palmer et al. 2007). If λ_i takes values close to zero, the distribution is skewed to the left-hand side, which is true for the data in this study (Figure 6, Appendix A).

$$E[\mathrm{y}_i|\mathrm{x}_i] = \lambda_i = Var[\mathrm{y}_i|\mathrm{x}_i] \tag{4}$$

In praxis, the equidispersion assumption is frequently violated because count data is often overdispersed, where the conditional variance is bigger than the conditional mean. Then, Poisson models underestimate the standard errors and therefore cause false inference (Cameron and Trivedi 2015). In such cases, negative binomial models are used to relax the equidispersion assumption by modeling the variance with an extra dispersion parameter θ (Beaujean and Grant 2019).

$$Var[\mathrm{y}_i|\mathrm{x}_i] = \lambda_i + \frac{\lambda_i^2}{\theta} \tag{5}$$

Table 1 shows that for the data used in this paper $E[\mathrm{y}_i|\mathrm{x}_i] \neq Var[\mathrm{y}_i|\mathrm{x}_i]$ for three out of four dependent variables which is a violation of the equidispersion assumption. Thus, I use a negative binomial model for these three variables. Further, I will introduce only the Poisson model in detail, because besides the extra dispersion parameter, the model's structure is identical.

Table 1. Check for overdispersion: Mean and variance of dependent variables

	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
$E(\mathrm{y})$	1.47	0.40	0.62	0.45
Var (y)	8.98	0.50	4.11	1.10
$\frac{\textit{residual deviance}}{\textit{degress of freedom}}$	2.12	0.94	1.09	1.04

The basic Poisson model is a log-lin model, written as:

$$\log(\lambda_i) = \alpha + \beta \times X_i \tag{6}$$

where λ_i denotes the conditional mean count for an entity i, transformed by the natural log-link function, X_i is an explanatory variable and α and β are the intercept and the slope of the function. Because λ_i denotes the predicted (conditional mean) count and not the observed count y_i, no zeros will be produced, which solves the y_i equals zero problem of regular log-linear OLS regression (Cameron and Trivedi 2015). The integration of climate conflict characteristics and panel data structure leads to

$$\log(\lambda_{i,t}) = \omega \times T_{i,t} + \beta \times T_{i,t-1} + \phi_i + \psi_t \tag{7}$$

where $\log(\lambda_{i,t})$ denotes the logged mean conditional count of violent event variables. The entity *i* denotes the location (country) and *t* the time period (year) of an event. ω is a vector of contemporaneous climate-related coefficients, $T_{i,t}$ is a vector of contemporaneous temperature and precipitation variables. β is a vector similar to ω, but based on one-year lagged temperature and precipitation variables $T_{i,t-1}$. Country-fixed effects are represented by ϕ_i and year-fixed effects by ψ_t. The remaining variation is at the country-year level only. I include no further control variables because these would block pathways through which climate could affect conflict counts (Burke et al. 2015a). This “bad control problem” will be discussed in detail after the model presentation (Cinelli et al. 2024). To preserve the original count scale, I use the inverse of the log-link function.

$$\lambda_{i,t} = \exp(\omega \times T_{i,t} + \beta \times T_{i,t-1} + \phi_i + \psi_t) \quad (8)$$

Estimation was applied through the RStudio package “fixest”. Poisson regression coefficients are interpreted as percentage change in the expected mean count,

$$percentage\ change\ in\ expected\ counts = 100\ \times [\exp(\beta \times \Delta) - 1] \quad (9)$$

where β (or equally ω) is the coefficient and Δ the amount of change (Beaujean and Grant 2019). For this paper, I will always interpret $\Delta = 1$. Burke et al. (2015a) argue that it is possible to add both the contemporaneous and the lagged effect for a cumulative interpretation of climate variables. For comparison with studies that use a binary dependent conflict variable, I also report a logit model that estimates the probability of conflict incidence with the same model structure

$$y_{i,t} = \omega \times T_{i,t} + \beta \times T_{i,t-1} + \phi_i + \psi_t \quad (10)$$

where different to the Poisson model $y_{i,t}$ denotes the binary coded incidence of one or more violent events in a country *i* in a year *t* instead of the logged mean conditional count $\lambda_{i,t}$. Probit regression coefficients give the change in the log odds of conflict incidence, associated with a one-unit increase in the independent climate variable. A common interpretation of the exponentiated coefficients is the odds ratio:

$$\exp(\beta) = Odds\ Ratio = \frac{P(Y=1)}{P(Y=0)} \quad (11)$$

If the odds ratio exceeds one, this can be interpreted as a multiplicative increase in the probability of conflict occurrence, given a one-unit change in explanatory (climate) variables. The comparison of the count and probability models is based on the Bayesian Information Criterion (BIC). BIC uses the likelihood of the model and punishes a high number of model parameters (Schwarz 1978). When comparing non-linear panel regression models typically the model with the lowest criterion is selected (Beaujean and Grant 2019). I also report R^2 and Pseudo R^2.

After the introduction of the panel models, this section discusses threats to causal identification and explains why no additional controls were added to the panel models. Normally, it is common to include additional control variables that have explanatory power for the dependent variable in regressions to avoid bias through omitted variables (Angrist and Pischke 2009). However, several controls used for explaining conflict are potentially bad controls and induce further bias due to endogeneity and pathway blocking (Cinelli et al. 2024). Many conflict controls are both affected by the dependent variable conflict and the main independent variables, making it impossible to estimate a causal effect with proper identification (Angrist and Pischke 2009; Burke et al. 2015a). For example, controlling for GDP leads to biased estimates because both conflicts and temperature can lower economic output, especially in countries that depend heavily on agriculture (Miguel et al. 2004; Burke et al. 2015a). Further, bad controls can block possible pathways through which climate effects could affect conflicts (Cinelli et al. 2024). If a panel regression would include a measure of migration, this would bias the coefficient of temperature towards zero as parts of the total temperature effects were captured by the migration coefficient, given we assume both have positive impacts on conflict. Similar concerns can be voiced for all other socio-economic covariates.

With the threat of endogeneity looming around, one would consider an IV approach to be a remedy for such problems. Although Miguel et al. (2004) used rainfall as an IV for economic growth to study the impacts on conflict risk, further studies did not apply instrumental variables for a simple reason. In the face of several (simultaneous) connections between socio-economic, conflict, and climate, the exclusion restriction, meaning that the instrument affects the independent variable only through the dependent variable, will not hold. Consequently, the identification of a causal effect will fail. Researchers have especially used weather as an IV for a variety of social or economic variables, leading per se to a violation of the exclusion restriction (Mellon 2020). Finding an IV for conflict themselves is also difficult because they affect a wide range of socio-economic factors, and the same holds for these social and economic factors themselves (Hendrix et al. 2023).

Still, it is possible to deal with the problem of bad controls in panel data. Both panel data models apply two fixed effects. County-fixed effects capture all time-invariant factors that differ between countries. This includes factors like geographic location, cultural and historical factors, institutions, and ethnic composition of the population, given the assumption that they are time-invariant over the sample period. They also control for countries that always

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experience conflicts or remain peaceful. Time-fixed effects capture time-varying macro shocks that affect all countries in a given year, e.g., international economic shocks or technological progress. Thus, many climate conflict studies do not use explicit control variables but rely on fixed effects only to control for most of the traditional factors that explain conflict at both the country and the year level (Burke et al. 2015a). Still, by putting emphasis on avoiding bias through bad controls, researchers allow omitted variable bias at the remaining country-year level. Such strategies are also used in non-conflict studies by climate economists for analyzing climate effects on growth (Burke et al. 2015b). Because this model does not identify specific channels, the estimates should be interpreted as aggregated climate effects over all potential pathways (Burke et al. 2015a). Based on Burke et al. (2009), I report an additional specification, where I use extra control variables (Tables 14-17, Appendix B).

3.2. Long difference model

Climate economics have argued panel regression can only observe regional weather variability due to the short (annual) nature of observation and is therefore not suited to measure the impacts of long-term climate trends (Kalkuhl and Wenz 2020). Others argue that a rise in the frequency of continuous climate variabilities is an economically relevant part of climate since those also trigger human responses (Burke et al. 2015a). A way to address these concerns is the use of longer time intervals in a long-difference approach. Here I separate the data into two time periods 1998-2009 and 2010-2020, calculate interval averages, and compute the changes between the intervals. OLS regression is then used to detect the effects of long-term climate changes on conflict changes. In contrast to the panel regression with count data, the residuals of the changes do follow normal distributions (Figure 7 and Figure 8, Appendix C).

$$\Delta y_i = \alpha \times \Delta T_i + \beta \times \Delta X_i \quad (12)$$

Let Δy_i denote the changes in the count of violent events in a country i, α denotes a vector of climate-related coefficients, ΔT_i denotes the changes in temperature and precipitation deviation and $\beta \times \Delta X_i$ are vectors for coefficients and respective control variables.

The long-difference approach used here is based on the before and after comparison model (Stock and Watson 2019) and has been used to estimate climate change effects on agricultural output changes (Burke and Emerick 2016) or growth rates (Kalkuhl and Wenz 2020). To the best of my knowledge, van Weezel (2019) is the only study that adopted a long-difference approach to the climate conflict nexus but only on a case study level. One downside to this approach is possible low variation in climate data causing low statistical significance for the climate-related coefficients (Kalkuhl and Wenz 2020). Here changes do not suffer from low variation. Low variation in changes is further discussed and checked in Appendix A (see Figure 4). Here additional controls do not introduce bias because long-term changes are less likely to be heavily affected by the other variables. I also include interaction terms between climate variables and migration and agricultural variables to identify the impacts on different pathways.

3.3. Data

The main goal of sample selection was obtaining data for comparable countries while avoiding sampling bias. Especially oversampling on regions with long-term violence occurrence has previously caused overestimation, thus undermining the external validity of positive climate conflict findings (Adams et al. 2018). High-income countries as well as countries that recorded zero violent events from 1998-2020 were excluded from the sample because they are not expected to experience many or even any violent events at all due to structural differences. For example, Hegre et al. (2003) find high-income countries have a low risk of civil war and violence because they offer stable political and economic conditions. Additionally, a low number of countries had to be excluded because of data unavailability. The sample includes 81 countries with a total of 2675 violent events and covers primarily Africa, Asia, and parts of South America.

Figure 1 shows descriptive maps for the main variables of interest. The number of violent event occurrences per country is mapped in part A. Countries with the most violent events recorded are Sudan, the Democratic Republic of Congo, Nigeria, Syria, and Mexico. Most conflicts were recorded on the African continent, Mexico, and the Middle East. It is important to cover these regions because they are the ones where the most violent events have occurred

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in the last 2 decades (Obermeier and Rustad 2023). The average annual temperature per country from 1998-2020 is 21.88 °C. The mean annual temperature deviation from a long-term baseline (1901-2000) is 0.76° C. Nearly all countries experienced positive mean annual temperature deviations from 1998-2020. This indicates that global warming is captured in the data. High temperature deviations are specially recorded in Africa, the Middle East and Asia (Figure 1, part B). The average precipitation per country is 1091.74 mm with mean long-term deviations of 1.93 mm. Different from temperature, precipitation deviations have a wide range of positive and negative values. In absolute terms, the mean precipitation deviation is 94.6 mm. Figure 1, part C shows that negative deviations are primarily recorded in Africa and the Middle East. Full summary statistics, additional descriptive maps sorted by conflict type, and lists of countries with core data are available in the Appendix (Table 10, Appendix A).

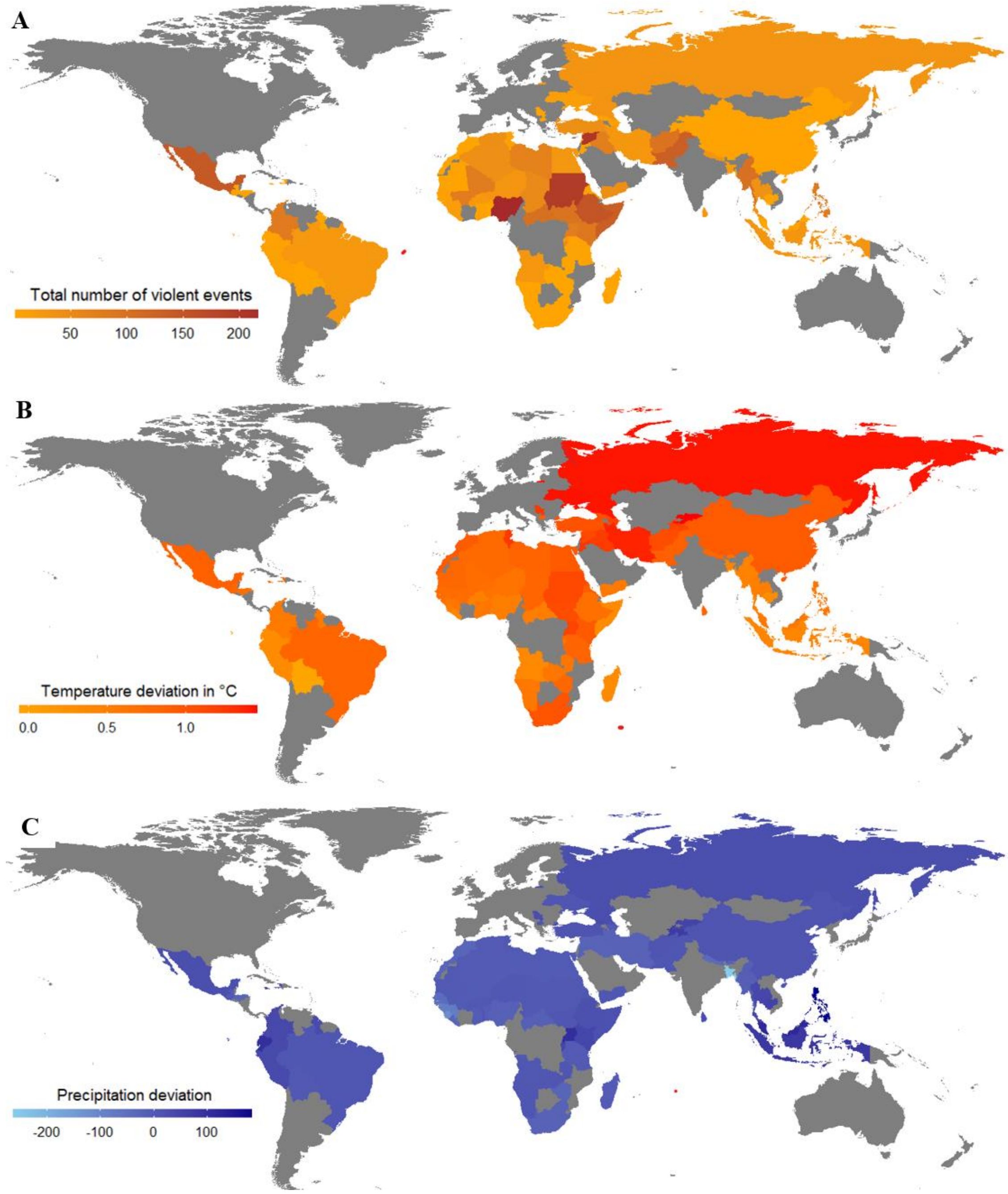

Figure 1: Descriptive maps of climate and conflict variables
(A) the total number of violent event (B) mean annual temperature deviations in °C (C) mean annual precipitation deviations in mm, all 1998-2020 for 81 selected low- and mid-income countries. Own visualization, Source: (A) UCDP/PRIO (B) and (C) CRU

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3.3.1. Conflict variables

To account for conflict heterogeneity, I use four dependent variables to estimate climatic effects on different conflict types. Data about the annual number of armed conflicts, non-state conflicts, and one-sided violence events in a country from 1998-2020 was taken from the corresponding UCDP/PRIO datasets (Davies et al. 2022). This dataset is the workhorse database for climate conflict research, used especially by studies with similar methods to this paper (see Table 10, Appendix B). As discussed previously these are the conflict events of the most interest to climate conflict studies or those that have not yet been studied well. They share a common threshold of 25 battle-related deaths and differ in actors. Definitions are:

- *Armed conflict* (794 total events):

  A state-based armed conflict is a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths in one calendar year (Gleditsch et al. 2002).
- *Non-state conflict* (1140 total events):

  The use of armed force between two organised armed groups, neither of which is the government of a state, which results in at least 25 battle-related deaths in a year (Sundberg et al. 2012).
- *One-sided violence* (846 total events):

  The deliberate use of armed force by the government of a state or by a formally organised group against civilians which results in at least 25 deaths in a year (Eck and Hultman 2007).

In addition to these types, the data was aggregated into a variable that counts the total number of annual violent events in a country (2780 total events). This variable allows only a limited interpretation because it does not capture conflict heterogeneity. I still use aggregated violent events because I intend to show that the use of this variable might be misleading as coefficients can show significant results even though they are driven by a specific form of violence.

3.3.2. Climate variables

All climate data is taken from the CRU CY dataset. CRU CY includes ten climatic variables from 1901-2021 on a country-level and is derived from the Climate Research Unit gridded Time Series (CRU TS) dataset, which covers all land areas at a 0.5° resolution (Harris et al. 2020). The data is obtained from stations and then interpolated by using a distance weighting method to impute missing values (Harris et al. 2020). To measure the effects of changing climate conditions in the panel analysis I use both ***temperature levels*** and long-term ***temperature deviations***. Because past studies have used both levels and long-term deviations, I estimate the panel models with both levels and deviations and compare the results. CY includes only total levels, so annual temperature deviation is calculated by computing the difference between a country-specific long-term temperature baseline (1901-2000) and the annual temperature levels (1998-2020) to capture long-term changes in climate conditions. The temperature baseline itself is computed by dividing the sum of annual temperature levels per country from 1901-2000 by the length of the baseline (100 years).

Precipitation deviations and levels can affect conflict risk through social and economic consequences of droughts or floods (Homer-Dixon 1991). Ever since Miguel et al. (2004) influentially used rainfall to measure the impact of growth on civil conflict, wet days, rainfall shocks, and precipitation are commonly included side by side with temperature variables to measure climate factors in economic climate conflict studies (Hsiang and Burke 2014). Annual precipitation deviations are derived from CRU CY and calculated the same way as temperature deviations.

3.3.3. Socio-economic control variables

Several control variables were coded to capture non-climate related effects on conflict in the long-difference approach. In this chapter, I explain both the direct effects on conflict and further effects of climate on conflicts running through controls to illustrate why those variables are bad controls.

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First, studies suggest a positive correlation between ***population*** growth and military conflict through urban overcrowding (Tir and Diehl 1998). But (urban) overpopulation can also be affected by climate-induced migration, stressing social and economic systems and thus fostering violence (Kelley et al. 2015). I use people per square kilometer of land area, taken from the World Bank, to measure population density to account for the overcrowding pathway. ***Natural resource availability*** has been associated with poor economic performance and increased instability since the formulation of the resource curse hypothesis (Sachs and Warner 2001). Vesco et al. (2020) show that not only scarcity but also resource abundance can increase conflict risk. As discussed earlier, climate can affect resource availability as well. Here I use total natural resource rents, which is the sum of oil, natural gas, coal, mineral, and forest rents, expressed as a share of GDP from The World Bank. Also important to conflict is ***agriculture dependency*** because in agricultural-dependent countries violence can be triggered through food insecurity (Wischnath and Buhaug 2014). Naturally, the agricultural sector is itself dependent on climate conditions (Maystadt and Ecker 2014). Agricultural dependency is included as the added value of agriculture (crop and livestock production), forestry, and fishing as a share of GDP.

Economic growth is found to affect civil and ethnic war onset significantly (Feron, 2003) and Miguel et al. (2004) find a five-percentage point decrease in GDP growth to increase civil conflict likelihood by twelve percentage points. They use rainfall as an IV for economic growth and thus also show a pathway from climate to growth. Negative economic shocks can increase the onset and length of conflict by lowering state capacity, e.g., its ability to maintain order and provide public goods (Hendrix et al. 2023). It has to be noted, that others claim there is no direct growth rate link to conflict onset (Bergholt and Lujala 2012). To measure economic effects GDP based on purchasing power parities (PPP) in constant 2017 US $ from the World Bank is used. ***Migration*** can contribute to social pressure and thus increase the risk of conflict (Kelley et al. 2015). Cappelli et al. (2022) argue migration is endogenous to climate and the IPCC (2022) predicts large migration flows in different warming scenarios. The net migration rate, which I use, measures the number of immigrants minus the number of emigrants in the previous five years, divided by the person-years lived by the destination population over that period (Ortiz-Ospina et al. 2022). Annual data is not available for many countries but the United Nations Department of Economic and Social Affairs (UN DESA) estimates net migration rates for all countries in fixed five-year intervals (Ortiz-Ospina et al. 2022). To obtain a balanced dataset, said estimates were used for imputation of the missing values. Next, ***unemployment*** can affect conflict of increased recruitment due to lower opportunity cost. Youth-focused case studies have shown a positive correlation between unemployment growth and intensity and incidence of violence (Caruso and Gavrilova 2012). The United Nations Development Programme (UNDP) finds employment to be the "single most frequently cited immediate need at the time of joining" a violent extremist group (Ozonnia et al. 2017). It is again possible that worse climate conditions are correlated with higher unemployment, especially in sectors like agriculture. Thus, unemployment estimates from the World Bank and the International Labor Organization (ILO) were used to account for potential effects of employment, including all persons without current jobs but available and looking for work as a share of the total workforce.

Institutions and governance are often closely related to the level of public goods allocation in climate conflict literature (Homer-Dixon 1991). Recently authors have claimed that unsustainable water management of the Syrian government may be linked with the start of the civil war in 2011 through forced migration into urban areas just after the country had experienced the most severe drought on record from 2007-2010 (Kelley et al. 2015). To account for the quality of institution this paper relies on data from OurWorldInData (OWID). Their data includes historical information about the typology of political systems (Herre et al. 2013) and is based on Lührmann et al. (2018) and Regimes of the World (RoW). I create a dummy that equals one if a country has been listed as an electoral or liberal democracy in a year *t*. Autocracies serve as reference group. Lastly, ***ethnic differences*** have long been brought forward as a conflict source (Yinger 1994) and empirical studies support a close connection between ethnic distribution and conflict (Esteban et al. 2012). Here ethnic distribution is represented by Drazanovas (2019) Historical Index of Ethnic Fractionalization (HIEF). HIEF covers annual ethnic fractionalization for a total of 165 countries from 1945 to2013. Ethnic fractionalization describes "the probability that two randomly drawn individuals within a country are not from the same ethnic group"(Drazanova 2020). Because data is only available until 2013 the values for the interval from 2014 to 2020 had to be estimated country-wise by using the AAA version of the ETS algorithm in Excel.

4. Results

4.1. Annual panel models

This subsection presents the results of the annual panel models with count data in Tables 2 and 3 as well as the results of the probit model in Tables 4 and 5. Tables 2 and 4 show the models that use levels of climate variables and Tables 3 and 5 display the models with long-term climate deviations. For model comparison, I report R^2, Pseudo-R,^{2} and BIC for all panel models.

Table 2. Count data models with annual climate levels

	(1)	(2)	(3)	(4)
Dependent Var.:	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
T	-0.161 (0.176)	-0.068 (0.126)	-0.359 (0.322)	-0.166 (0.176)
T *t-1*	0.027 (0.018)	0.014 (0.018)	0.082*** (0.031)	0.012 (0.019)
P	0.0004 (0.0003)	0.0004* (0.0002)	-0.0008 (0.0006)	0.001*** (0.0004)
P *t-1*	-0.0002* (0.0001)	-0.0002 (0.0001)	-4.2e-5 (0.0003)	-0.0002 (0.0001)
Family	Neg. Bin.	Poisson	Neg. Bin.	Neg. Bin.
Observations	1,862	1,379	1,149	1,494
Squared Cor.	0.56050	0.55667	0.46271	0.53065
Pseudo R2	0.25868	0.25773	0.22593	0.25366
BIC	5,099.2	2,684.3	2,680.0	2,912.2
Over-dispersion	2.6597	--	1.2345	12.381

Note: *** = p < 0.01, ** = p < 0.05, * = p < 0.1. S.E clustered at the country level. All models include country and year fixed-effects. T = temperature & P = precipitation.

Table 2 shows the results for the annual count data models with country and year-fixed effects with temperature and precipitation levels. I estimate negative binomial models for violent events, non-state conflict, and one-sided violence because of overdispersion while armed conflict is equidispersed and can be estimated with a Poisson model. Standard errors are clustered at the country level to account for correlation within countries over time. Different numbers of observations are due to the collinearity of conflict variables with country-fixed effects. First, all contemporaneous temperature effects are negative but without significance, but all coefficients of one-year lagged temperature levels are positive. Estimates indicate an 8,54% (100*(e^0.082-1)) increase in the expected mean count of non-state conflicts for an increase in annual temperature in *t-1* by 1°C. This effect is significant at the 1% level. Contemporaneous precipitation increases of 1mm show positive effects on armed conflict and one-sided violence (0.0004%/0.001%) at the 10% and the 1% significance level. Lagged precipitation levels only show a significant negative effect on aggregated violent events (-0.0002%) but no significant individual effects. R^2 are between 0.46 and 0.56. Pseudo R^2 are between 0.23 and 0.26.

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Table 3. Count data models with long-term climate deviations

	(1)	(2)	(3)	(4)
Dependent Var.:	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
Δ T	-0.167 (0.154)	-0.073 (0.109)	-0.277 (0.274)	-0.216 (0.162)
Δ T *t-1*	-0.067 (0.123)	-0.044 (0.102)	-0.159 (0.210)	-0.018 (0.169)
Δ P	-0.0007* (0.0004)	5.77e-5 (0.0002)	-0.002* (0.0009)	-0.001** (0.0006)
Δ P *t-1*	-0.001*** (0.0005)	-0.0004 (0.0003)	-0.003*** (0.0007)	-0.001** (0.0006)
Family	Neg. Bin.	Poisson	Neg. Bin.	Neg. Bin.
Observations	1,862	1,379	1,149	1,494
Squared Cor.	0.56368	0.55684	0.46996	0.52317
Pseudo R2	0.25976	0.25691	0.22929	0.25308
BIC	5,093.0	2,686.6	2,670.7	2,913.9
Over-dispersion	2.7250	--	1.3008	11.519

Note: *** = p < 0.01, ** = p < 0.05, * = p < 0.1. S.E clustered at the country level. All models include country and year fixed-effects. T = temperature & P = precipitation.

The model used for the results displayed in Table 3 is equivalent to the model used in Table 2 but instead of climate variable levels, here I use annual deviations from long-term country means. Not only do both temperature deviations show no significant effects, but also the sign of the lagged temperature deviation switched compared to Table 2, indicating negative effects of both contemporaneous and lagged temperature deviations on expected mean conflict counts. Also, in contrast to Table 2 contemporaneous precipitation deviations lower expected mean counts of aggregated violent events, non-state conflict, and one-sided violence (-0.0007%/-0.002%/0.001%). Given an absolute mean precipitation deviation of 94.6 mm, this corresponds to approximately -0.07% for violent events, 0.19% for non-state conflict, and 0.095% for one-sided violence. Similar coefficients are estimated for one-year lagged precipitation deviations. R^2 lie between 0.47 and 0.56 and Pseudo R^2 between 0.23 and 0.26 and are close to the values in Table 2.

Table 4. Logit model with annual climate levels

	(1)	(2)	(3)	(4)
Dependent Var.:	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
T	-0.354 (0.368)	-0.145 (0.403)	-0.686 (0.465)	-0.347 (0.396)
T *t-1*	0.076** (0.037)	0.061 (0.043)	0.095** (0.040)	0.015 (0.032)
P	0.001 (0.0006)	0.0007 (0.0008)	-0.0005 (0.0007)	0.002*** (0.0008)
P *t-1*	-0.0006** (0.0003)	-0.0006* (0.0003)	-3.38e-5 (0.0003)	-0.0003 (0.0002)
Observations	1,633	1,311	1,103	1,448
Squared Cor.	0.41553	0.43133	0.31864	0.35052
Pseudo R2	0.35742	0.36039	0.27531	0.30546
BIC	2,059.3	1,726.8	1,449.7	1,865.2

Note: *** = p < 0.01, ** = p < 0.05, * = p < 0.1. S.E clustered at the country level. All models include country and year fixed-effects. T = temperature & P = precipitation.

Next, I estimate a logit model on binary conflict incidence with the same model structure, country and year-fixed effects, and clustered standard errors. Table 4, again with climate variables in levels, shows a similar picture to Table 2. Contemporaneous temperature estimates are negative but without significance. One-year lagged temperature increases of 1°C are associated with an increase in the probability of incidence of violent events of 1.08% (e^0.076) and 1.01%(e^0.095). Both effects are significant at the 5% level. Contemporaneous precipitation yields only a significant effect on one-sided violence (1.002%(e^0.002) while one-year lagged precipitation affects only armed conflict at the 10% significance level. Like Tables 2 and 3, Table 5 shows that all temperature effects vanish when long-term deviations replace levels. Again, both precipitation coefficients show minor changes in significance and effect size.

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Table 5. Logit model with long-term climate deviations

	(5)	**(5)**	**(6)**	**(8)**
Dependent Var.:	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
Δ T	-0.270 (0.272)	0.071 (0.309)	-0.527 (0.337)	-0.317 (0.266)
Δ T *t-1*	0.089 (0.237)	0.148 (0.260)	-0.105 (0.257)	0.270 (0.211)
Δ P	-0.0004 (0.0009)	0.001 (0.001)	-0.001 (0.001)	-0.002* (0.0010)
Δ P *t-1*	-0.003*** (0.0008)	-0.001 (0.001)	-0.003*** (0.0007)	-0.002* (0.0009)
Observations	1,633	1,311	1,103	1,448
Squared Cor.	0.39181	0.39356	0.29978	0.32089
Pseudo R2	0.33735	0.33410	0.26354	0.27559
BIC	1,938.5	1,615.4	1,310.7	1,757.4

Note: *** = p < 0.01, ** = p < 0.05, * = p < 0.1. S.E clustered at the country level. All models include country and year fixed-effects. T = temperature & P = precipitation.

When compared to a model that uses one-year lagged log GDP and democracy controls based on Burke et al. (2009) (Tables 14-17, Appendix B), especially the non-state conflict coefficients are lower. Additionally, some climate coefficients become slightly less significant. Using only country and not time-fixed effects yields higher coefficients for the main results, but some coefficients also lose their significance (see Tables 18 and 19, Appendix B).

4.2. Long difference model

Next, Table 6 presents the results of the long-difference approach. Because the model is estimated with basic OLS, interpretation remains intuitive but one must consider that all variables are long-term changes between 1998-2009 and 2010-2020 country averages instead of levels or counts. Thus, a one-unit increase in one independent variable ΔT_i or ΔX_i increases the change of violent event counts $\Delta \gamma_i$ by β_i. A positive coefficient for changes in a climate variable would thus mean that the analyzed conflict category appears on average more frequently, given a change in climate.

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Table 6: OLS estimates for long differences approach with all variables (1-4)

	(1)	(2)	(3)	(4)
Dependent Var.	Δ Violent Events	Δ Armed Conflict	Δ Non-state Conflict	Δ One-sided Violence
Δ T	1.668 (1.520)	-0.063 (0.302)	1.838* (1.065)	-0.107 (0.470)
Δ P	0.002 (0.005)	-0.0001 (0.001)	0.001 (0.003)	0.001 (0.002)
Δ Population Density	-0.002 (0.010)	0.0001 (0.002)	-0.001 (0.007)	-0.001 (0.003)
Δ Democracy	0.186 (0.879)	-0.085 (0.175)	0.386 (0.616)	-0.115 (0.272)
Δ Migration	-0.172** (0.079)	-0.009 (0.016)	-0.118** (0.055)	-0.046* (0.024)
Δ Ethnicity	-2.698 (9.278)	0.026 (1.846)	-3.115 (6.503)	0.391 (2.866)
Δ Agriculture	0.012 (0.079)	0.014 (0.016)	-0.041 (0.055)	0.038 (0.024)
Δ log GDP	-2.415* (1.251)	-0.402 (0.249)	-1.813** (0.877)	-0.200 (0.386)
Δ Unemployment	0.238* (0.130)	0.068** (0.026)	0.089 (0.091)	0.081** (0.040)
Δ Resources	-0.030 (0.054)	-0.011 (0.011)	-0.037 (0.038)	0.018 (0.017)
Δ T x Δ Agriculture	0.392 (0.373)	-0.017 (0.074)	0.475* (0.261)	-0.066 (0.115)
Δ P x Δ Agriculture	0.001 (0.002)	-0.0001 (0.0003)	0.001 (0.001)	-0.00004 (0.0005)
Δ T x Δ Migration	-0.034 (0.191)	-0.037 (0.038)	-0.003 (0.134)	0.006 (0.059)
Δ P x Δ Migration	-0.001 (0.001)	-0.0001 (0.0002)	-0.0002 (0.001)	-0.0002 (0.0004)
Constant	1.249* (0.712)	0.327** (0.142)	0.624 (0.499)	0.299 (0.220)
Observations	81	81	81	81
R2	0.432	0.315	0.412	0.301
Adjusted R2	0.312	0.169	0.287	0.152
Residual Std. Error (df = 66)	2.056	0.409	1.441	0.635
F Statistic (df = 14; 66)	3.586***	2.164**	3.299***	2.027**

Note: * p < 0,1; ** p < 0,05; ***p < 0,01 . T = temperature & P = precipitation. Δ = changes between 1998-2009 and 2010-2020 country averages of the corresponding variables.

Table 6 illustrates that on average a 1°C increase in country average temperature change is associated with an increase of 1.838 in non-state conflict change at the 10% significance level. Other conflict types and all precipitation coefficients remain without significance. Control variables show significant negative effects for migration and log GDP and further positive effects for unemployment over different conflict types. Further controls yield no significant long-term effects. The interaction between temperature changes and changes in agricultural dependency also yields a significant effect for non-state conflict (0.475) at the 10% level. R^2 range between 0.301 and 0.432. R^2 for non-state conflict changes is the second highest at 0.412. Adjusted R^2 are lower for all conflict categories, punishing the high number of additional controls. Still, non-state conflict adjusted R^2 is 0.287.

5. Discussion

Based on the previous chapter I find evidence for heterogeneous impacts on violent events, both in the panel regression and the long-difference approach. Estimating the regressions solely for the aggregated violent events would result in largely misleading findings. In my models, the effects on conflict count are primarily driven by increases in non-state conflict rather than by one-sided violence or armed conflict. This finding remains robust when applying the long-difference approach. Panel data models also indicate some non-robust correlations between precipitation and all three conflict types, with variations in effect size and significance across the different models. The long-difference approach does not confirm any precipitation results. This section discusses how these findings relate to the existing literature. I focus on temperature-related results for non-state conflict due to their robustness across both approaches.

Most notably, mean non-state conflict counts are estimated to increase by 8,54% for a 1°C change in one-year lagged temperature (Table 2). The results are robust to the use of binary incidence in the logit model. This finding

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is also in line with the long-difference approach that estimates a long-term 1°C change in temperature is associated with a 1.838-unit increase in the change of non-state conflict counts. Thus, both non-state conflict counts and onset probability are positively correlated with temperature. Furthermore, Table 6 illustrates an additional positive effect of temperature change in agricultural-dependent countries (0.475). Because this is the only robust temperature effect, these findings are evidence for heterogeneous impacts of conflict and confirm that non-state conflict is more affected by changes in long-term climate conditions than state-based conflict (Fjelde and Uexkull 2012). I follow the interpretation of Fjelde and Uexkull (2012), who argue that fighting against the state does not mitigate resource scarcity or migration effects as effectively as violence against non-state actors, such as fighting an armed group over a local water supply because states are generally equipped with relatively high violence suppression capacities through their respective military and security forces. Still, it must be noted that other studies do not find significant temperature effects for non-state conflict (Buhaug, 2015). When compared to the model with additional controls based on Burke et al. (2009) (Tables 14-17, Appendix B), the temperature-related non-state conflict coefficients are lowered (0.054/0.077). Although robust in significance, the lowered magnitude of the estimate illustrates the problem of bad control variables. Parts of the total estimated temperature effect are now captured by the controls, which block relevant pathways (income and institutions) through which climate and conflict can affect each other, leading to bias (Cinelli et al. 2024). In a further robustness check, the temperature coefficients for non-state conflict increase to 0.087 and 0,1 from originally 0.082 and 0.95, when time-fixed effects are dropped from the estimation, allowing for additional variation (see Tables 18 and 19, Appendix B). Because time-fixed effects can partly capture global climate phenomena like El Niño, which can affect conflict risk (Hsiang et al. 2011), the estimates in Tables 2-5 might be underestimated.

Negative contemporaneous and lagged precipitation effects were significant only when long-term deviations were used in the panel approach (Tables 3 and 4). Both the long-difference and the climate level panel regression did not find precipitation impacts on non-state conflict. In contrast, the only comparable study that uses count data and a long-difference approach, a case study in Ethiopia and Kenya, finds a negative correlation between precipitation and annual conflict events per district (van Weezel 2019). There are three possible explanations for the difference in results. First, in my paper, nonsignificant findings can be caused by joint estimation of negative and positive deviations. While negative deviations are associated with droughts, positive deviations indicate floods. Thus, precipitation coefficients lack the context of the specific mechanism in cross-country regressions. E.g., precipitation shocks can be mitigated by the presence of dams or other omitted factors (Sarsons 2015). Fjelde and Uexkull (2012) and Nordkvelle et al. (2017) find precipitation deviation impacts on non-state (communal) conflict depend on the length, severity, and direction of deviations. Second, other (case) studies may suffer from sampling bias, focusing only on countries that are highly affected by both climate change and reoccurring conflict. Like my study, especially those with a broader study population find no robust precipitation effects for non-state conflict at all (Buhaug et al. 2015; O'Loughlin et al. 2014). The same reasoning applies to the effects of climate variables on armed conflict. No model finds any significant impact of temperature variables and precipitation effects from the panel models are not robust to the long-difference approach. But previous studies predict armed conflict risk to increase with rising temperature, temperature deviations, and low precipitation levels (Burke et al. 2009; Cappelli et al. 2022; Coulibaly and Managi 2022; Ge et al. 2022). Again, I argue my sample is not restricted to a certain region and avoids oversampling especially on the African continent, thus avoiding previous bias in armed conflict estimates (Adams et al. 2018). I also avoid oversampling on regions with high conflict counts and battle deaths because the UCDP threshold for including a conflict in the dataset is only 25 battle-related deaths per year instead of higher thresholds. E.g., civil wars, which Miguel et al. (2004) and Burke et al. (2009) link with highly increased civil war risk, use a higher threshold of 1000 battle-related deaths. UCDP data avoids oversampling on intense conflicts and reflects lower conflict intensity better than civil war data, enabling more nuanced findings, less bias, and potentially explaining lower results. Third, previous studies produced mixed results also due to differences in model design and the inclusion of bad control variables (Burke et al. 2015a). The heterogeneity between the estimates for different conflict types shows that it is important to carefully select control variables in panel data models and consider both pathways from climate toward the different conflict types and the implications of their introduction in the empirical model.

To further check the validity of the main non-state conflict finding, I examine recent trends of non-state conflict. In line with other violence types, the last decade has shown significantly higher levels of event counts and battle deaths (Obermeier and Rustad 2023). Ever since the introduction of the UCDP data, non-state conflict has been largely driven by conflict in Africa (Obermeier and Rustad 2023). Figure 2 shows that most non-state conflicts in the sample are located on the African continent and Mexico. Also, the ten African countries with the highest counts represent 70% of all non-state conflicts (Table 6, Appendix A). Additionally, in the estimation process, all countries without any non-state conflict were dropped. Thus, it is possible that my results also suffer from focusing on a set of violent countries. Still, this bias is of minor importance, as I also include countries with only a low number of conflicts. 82.72% of the included countries even record less than one annual non-state conflict.

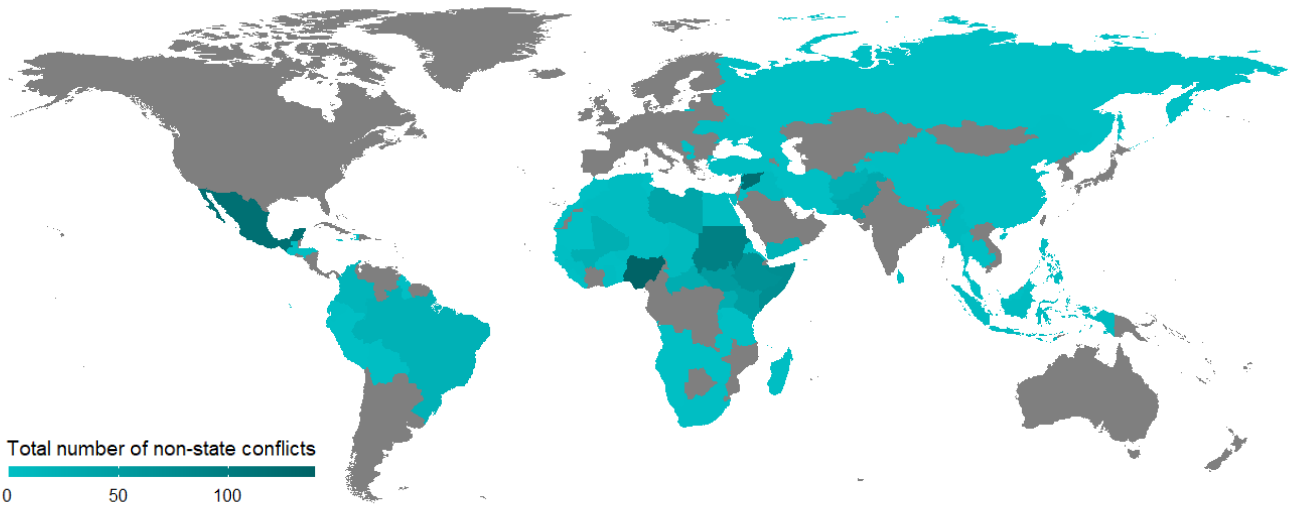

Figure 2: Descriptive map of non-state conflicts per country
Map of the total number of non-state conflicts (1998-2020). Own visualization. Source: UCDP/PRIO

Another notable trend concerns the heterogeneity of subtypes of non-state conflict. Nearly all climate related studies focused on communal conflict (Table 10, Appendix B) but the rise in non-state conflict is mostly due to the rise in clashes including formally organized groups (Obermeier and Rustad 2023). In combination with the positive findings presented here, this suggests that not (only) communal conflict, in which actors are informally organized groups that rely on a shared religious, ethnic or tribal identity, but also conflicts between formally organized groups like militias may be positively impacted by changing climate conditions. Future research could focus on this through case studies or differentiated non-state data analysis, considering differences in causal pathways when explaining this link.

In Tables 2-5, I find differing results for precipitation on one-sided violence for both contemporaneous and one-year lagged variables. I do not obtain significant results from the long-difference approach. Research on one-sided violence is to date very scarce (Table 13, Appendix B) but high temperature deviations have been associated with rising one-sided violence events in Sub-Saharan Africa (O'Loughlin et al. 2014). Most notably the Rwandan genocide, which is by far the most deadly violent event in the near past and categorized as one-sided violence, has been found to be not connected with resource scarcity even though the country heavily relied on agriculture (Percival and Homer-Dixon 1996) which supports non-significant findings partly. Although my study shows no robust evidence of a link with climate, future research should pay attention to climate impacts on this conflict type due to the increasing number of events (Figure 1).

I find significant impacts in the panel analysis especially in one-year lagged variables. This reflects the delayed response of conflict counts through causal pathways such as food insecurity or migration. Particularly, resource scarcity impacts like food insecurity that run through an agricultural pathway may be dampened or buffered by more than a year through individual savings, food substitution, crop insurance, redistribution schemes, government policy, or even international aid, which has yet to be explicitly captured in empirical studies (Wischnath and Buhaug 2014). Climate-induced migration decisions might also take time due to reasons like risk or cost of travel and will affect conflict rather through long-term effects like overcrowding of urban areas than contemporaneous ones. In line with the literature, I use one-year lagged variables to account for this in the panel approach. I also test lags up

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to three years, which yielded similar results. Some authors, such as Burke et al. (2009), use longer (5-year) lags to account for long-term impacts of climate variability in panel regression. Here, the long-term impacts of climate conditions are captured in the long-difference approach. Given the appropriate data, it would also be possible to analyze even shorter time periods (monthly or quarterly). However, one would need to consider these weather effects rather than climate (change) effects due to the short-term variability of weather events.

Still, there are some limitations in my study that affect the internal and external validity of the findings. My first concern lies with the explanatory climate variables because not only long-term climate deviations and climate levels but also extreme weather events can influence conflict. Maystadt and Ecker (2014) find substantial impacts of drought intensity and length on conflict risk in Somalia by affecting the livestock market. Floods are found to influence conflict dynamics via displacement, especially in developing countries (Ghimire et al. 2015). Hsiang et al. (2011) link the El Nino phenomenon to increased civil conflict onset probability in the tropics and Schleussner et al. (2016) estimate a coincidence rate of 9% between armed conflict outbreak and climate-related disasters. In this study, climate deviations can capture these dynamics only to a certain extent. I argue that extreme weather event counts could better reflect climate change impacts in annual regression. The aggregation of monthly deviations to annual deviation does not capture the seasonality of extreme weather events well due to the long observation period. In annual deviation data, a drought, measured by high negative precipitation deviation in the summer months, can be hidden by high deviations in the opposite direction in the same year. This is especially important as extreme weather events are projected to increase in frequency and magnitude (IPCC 2022). Coulibaly and Managi (2022) compare studies with monthly and annual observations of precipitation and find significant results only for monthly but not for annual observation, stating that the timing of precipitation deviations is key (Coulibaly and Managi 2022). Precipitation shocks are most likely to affect conflicts in the growing season of important food or export crops (Mach et al. 2019). Thus, low evidence for precipitation impacts study may also be caused by the use of annual climate data and through aggregation of high-frequency climate data. In a possible extension to this paper, one could run similar regressions with higher frequency climate data to explore if timing and aggregation bias affect results for conflict heterogeneity.

Importantly, the results from the panel model depend heavily on the choice of climate data. Besides the non-state conflict coefficient, many results change significance over the four count models from Tables 2-5. To avoid bias through the selection of a preferred specification with the most significant results (often referred to as p-hacking) I report both findings (Brodeur et al. 2020). Because previous studies used both levels and deviations (see Table 12, Appendix B), contrasting results could originate not only from sampling bias and a poor understanding of conflict heterogeneity but also from differences in the selection of climate variables. Future studies should carefully evaluate and explain, which type of climate data fits the research question and the empirical design the most. Deviations could, e.g., be better suited to explain climate extremes than levels. On the other hand, it is possible that levels better reflect factors like vulnerability or adaptation to climate change. The latter two are major factors in climate economy and received little attention in early climate conflict research. Vulnerability represents the level at which one is affected while (human) adaptation corresponds to the rate at which one can adapt to risks and chances (IPCC 2022). Studies pointed out several factors that make countries more vulnerable to increasing conflict risk like large populations, high agricultural dependency, and low human development or political exclusion of ethnic groups (Fjelde and Uexkull 2012; Ide et al. 2020). Buhaug and Uexkull (2021) introduce a framework of interactions between climatic hazards, vulnerability, exposure, and armed conflict arguing that climate impacts and armed conflict both cause socio-economic problems, increasing vulnerability and exposure to climate change impacts, creating a trap of feedback effects they call a vicious cycle. They also recognize that climate change impacts are distributed unevenly, with severe consequence for development, especially in poorer countries. Bigger adaptation capacity is associated with lower armed conflict likelihood in Afrika as societies develop methods to cope with resource (water) scarcity (Regan and Kim 2020). So far, neither vulnerability nor adaptation has been fully implemented in empirical strategies. This omits heterogeneous vulnerability levels across entities and the possibility of climate induced damages being offset by human adaptation, and can lead to biased estimates in regression frameworks (Regan and Kim 2020).

Different mean vulnerability is partly shown by Figure 3, which shows that on average in my sample warmer countries are those more vulnerable to high conflict counts. This finding is robust over all conflict types (Figure 9,

Appendix C). High mean temperature does not automatically cause violence because a lot of high temperature countries experience close to zero mean violent events. I argue that not only vulnerability but also adaptation could affect these findings. Especially the long-difference model might capture (human) adaptation towards high climate pressure in high-temperature countries (e.g., through harvesting more resilient crops or even reducing agricultural dependency). High base vulnerability and early exposure to climate change impacts could force the population of these countries into early adaptation processes that might already be captured in the data. Although I explicitly control for factors like agricultural dependency and population density in the long-difference approach and apply country and time-fixed effects in the panel approach, I cannot rule out bias due to these complex processes. Especially the panel approach cannot control for time-varying effects because climate variables vary at this level.

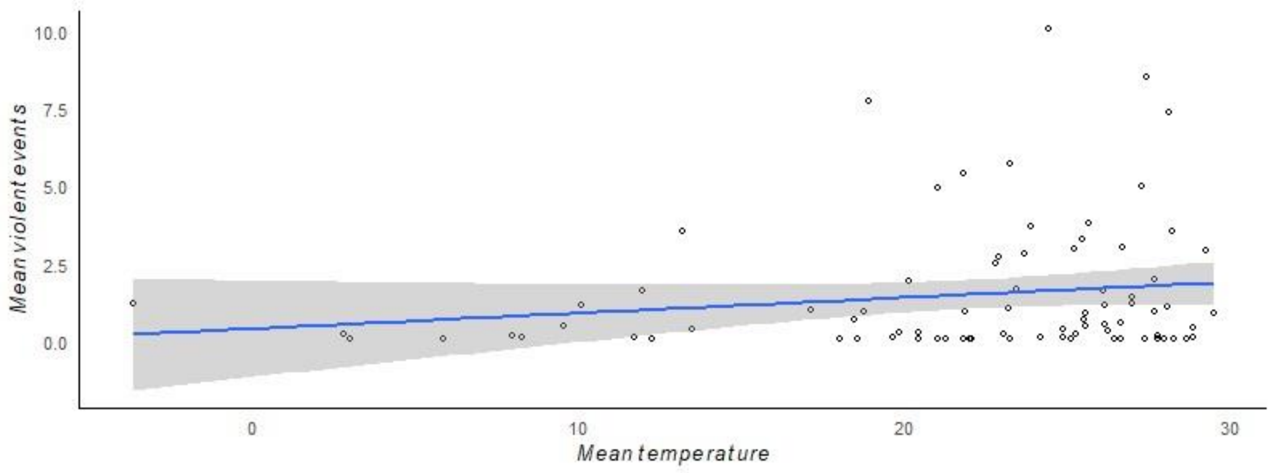

Figure 3: Scatterplot for mean annual temperature and violent events
Mean annual temperature is plotted against the mean violent event count per country from 1998-2020 for 81 selected low- and mid-income countries. A linear trendline with standard error visualization is added. Figure with separated conflict types reported in the Appendix. Own visualization. Source UCDP & CRU.

Additionally, neither climate change nor conflicts stop at national borders, which is not reflected in my country-level data. Conflicts are actually more likely to appear near borders as they give rebels advantages like refuge (Buhaug and Gates 2002). Ide (2017) stresses climate can influence conflicts in countries that are not the location of climate shocks through spillovers. While short-term climate conflict effects are strongly dependent on the local characteristics of the studied unit, long-run effects such as migration flows influence neighboring units (Cappelli et al. 2020). Spatial dynamics are therefore included both in panel and long-difference regression (van Weezel 2019). Studies increasingly rely on high-resolution, geolocated data to account for differences within smaller units of observation like regions or even villages (Ge et al. 2022). This better reflects regional differences within one country and increases the number of observations compared to cross-country studies. Country-level variables could suffer from spatial aggregation bias because it cannot detect effects at the subnational level. Also, this paper cannot implement spillovers over borders or other spatial dynamics through basic dummy variables in the estimation strategy because the data does not capture all countries, and thus spatial variables would suffer from missings and low precision.

A last limitation originates in the use of count data. Count data can suffer from an excess amount of systematical zeros, where some individuals never experience non-zero counts due to exogenous reasons, causing bias in the estimates (Coxe et al. 2009). Conflict counts are no exception to excess zeros as shown by Cappelli et al. (2022), who use a zero-inflated negative binomial model to estimate climate impacts on armed conflict in Africa. In their data, 92% of the observations record a zero. A large number of zero observations does not cause a problem, but in the context of conflicts, there are exogenous reasons why some countries remain completely peaceful all the time and others experience conflict each year. According to the conflict trap theory, one of the most important predictors of conflict is previous conflict (Collier et al. 2003). Through negative impacts on the economy and state capacity, and increased weapon availability, organizations and actors can develop an interest in continuous violence out of financial motives (Collier et al. 2003). Such effects are captured partly in country-fixed effects. Other

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studies implement conflict persistency by including dummy variables for no conflict in *t-1* (Cappelli et al. 2022) or a lagged conflict variable in their regression (van Weezel 2019). Zero-inflated models solve this problem by estimating two separate models. A Logistic regression predicts the probability of experiencing only zeros, the non-occurrence of an event, and a Poisson or negative binomial model predicts the frequency of event occurrence (Beaujean and Grant 2019). In this paper, I exclude all countries that experience no aggregated violent events at all, which deals to some extent with excess zeros. Nonetheless, non-state conflicts record an extraordinary number of zeros (Figure 6, Appendix C). Zero inflated models should be considered in future count data studies in the climate conflict nexus.

6. Conclusion

I study heterogeneous impacts of changing long-term climate conditions on conflict types by using a count data panel regression and a long-difference approach for 81 countries from 1998-2020. I find only non-state conflict counts to be positively correlated with temperature while armed conflict and one-sided violence are not significantly affected. No robust precipitation impacts were found. Different agricultural dependency levels of economies also impact conflicts. Countries with high mean temperatures are also more vulnerable to experiencing high conflict counts. I explain differences to previous studies with several empirical challenges including bad controls, sampling bias, choice of climate data, spatial spillovers, human adaptation and excess zeros.

In line with previous literature, my findings indicate a link between long-term climate conditions and violent events. Climate can act as a threat multiplier via pathways like migration and scarcity when paired with socio-economic conditions that favor the incidence of violence rather than being a direct causal conflict source. A clear understanding of how and under which exact circumstances climate conditions increase different types of violence can be achieved by focusing on conflict heterogeneity as my results differ heavily by the type of violence. This paper shows only effects for non-state conflict and none for armed conflict or one-sided violence. Because of estimated count increases and the recent trend of increased involvement of formally organized groups combined with the former focus on communal conflict, non-state conflict should be further investigated. For additional evidence case studies could focus on countries used in this panel analysis. Findings should be worked into a detailed framework of heterogeneous security impacts of climate change. Such a framework would carry big policy relevancy as it might help to end the blocking of related preventive action in intergovernmental organizations like the UN Security Council.

Policymakers themselves should work towards a common understanding of the climate conflict nexus. Even though it is not yet a tale of direct causality, in the future, this could be subject to change due to severe uncertainty involved in projections, exponential damages, and human adaptation. In a worst-case scenario climate change could potentially trap the most vulnerable people in a self-intensifying combined climate conflict trap and lead to humanitarian crisis. The international community should therefore strengthen efforts both in stopping climate change and improving the resilience of vulnerable, conflict-affected regions to avoid vicious cycles. Further implementation of climate change in national and global security policies, not only as direct threat of interstate disputes over scarce resources but also through indirect pathways like forced migration is needed to be prepared for exponential impacts of climate change.

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Appendix A. List of Variables, descriptive statistics & additional checks

Table 7: List of variables

Variable	Definition	Source
Agriculture	agriculture, forestry and fishing, value added (% of GDP)	The World Bank
Armed Conflict	number of armed conflicts in country i in timeperiod t	UCDP/PRIO Armed Conflict Dataset version 22.1
Democracy	dummy, = 1 if country i is listed as democracy in timeperiod t	OWID (V-DEM, RoW)
Ethnicity	Historical Index of Ethinc Fractionalization (HIEF)	HIEF Dataset
GDP	GDP based on PPP (constant 2017 international $)	The World Bank
Migration	net migration rate	UN DESA
Non-state Conflict	number of non-state conflicts in country i in timeperiod j	UCDP Non-State Conflict Dataset version 22.1
One-sided Violence	number of one-sided violence events in country i in timeperiod j	UCDP One-sided Violence Dataset version 22.1
Population density	people (regardless of legal status) per square kilometer of land area	The World Bank
Precipitation	annual precipitation level	CRU CY v.406
Precipitation Δ	annual deviation of precipitation from a long-term baseline (1901-2000)	CRU CY v.406
Resources	total natural resources rents are the sum of oil rents, natural gas rents, coal rents (hard and soft), mineral rents, and forest rents	The World Bank
Temperature	annual temperature level	CRU CY v.406
Temperature Δ	annual deviation of temperature from a long-term baseline (1901-2000)	CRU CY v.406
Unemployment	share of the labor force that is without work but available and seeking employment	The World Bank/ILO
Violent Events	number of violent events in country i in timeperiod j	UCDP

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Table 8. Summary statistics

Variable	Mean	Variance	Min	Max
GDP	4.362637E+11	2.46E+24	1360245573	2.29961E+13
Agriculture	1.920812E+01	183.7018601	1.383652541	79.04236246
Resources	9.639676E+00	123.9675943	0.001067507	66.05989355
Population density	1.058340E+02	23849.37796	2.108555916	1286.171553
Unemployment	8.088351E+00	38.63402968	0.116	37.32
Temperature	2.187670E+01	48.22802844	-5	29.8
Temperature Δ	7.588771E-01	0.20693942	-0.544	3.375
Precipitation Δ	1.934931E+00	23430.36047	-734.47	1390.75
Precipitation	1.091745E+03	604593.2371	13	3504.2
Migration	-1.434979E+00	34.69655667	-54.746	41.525
Non-state Conflict	6.119163E-01	3.841254274	0	32
One-sided Violence	4.541063E-01	1.151356653	0	12
Armed Conflict	4.261943E-01	0.515360751	0	4
Ethnicity	5.376877E-01	0.067896803	0.02	0.889
Democracy	3.472893E-01	0.226801187	0	1
Violent Events	1.492217E+00	8.987989873	0	39

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Table 9. Summed violent event counts per country (total of 81) from 1998-2020 (A-L)

Country	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
Afghanistan	82	30	22	30
Algeria	38	23	4	11
Angola	22	15	0	7
Azerbaijan	9	9	0	0
Bangladesh	16	4	3	9
Benin	1	0	0	1
Bhutan	1	0	0	1
Bolivia	1	0	1	0
Brazil	21	0	20	1
Burkina Faso	21	5	2	14
Burundi	44	14	4	26
Cambodia	1	1	0	0
Central African Republic	87	14	27	46
Chad	46	19	8	19
China	4	2	1	1
Colombia	68	22	15	31
Comoros	1	0	1	0
Congo	9	4	0	5
Cote d'Ivoire	28	4	11	13
Democratic Republic of Congo	232	26	76	130
Ecuador	2	0	2	0
Egypt	24	11	4	9
Eritrea	7	7	0	0
Ethiopia	132	45	69	18
Gambia	1	0	0	1
Georgia	3	2	1	0
Ghana	4	0	4	0
Guatemala	3	0	2	1
Guinea	12	2	4	6
Guinea-Bissau	2	2	0	0
Guyana	1	0	0	1
Haiti	5	1	0	4
Honduras	3	0	2	1
Indonesia	26	10	6	10
Iran	22	19	0	3
Iraq	62	18	9	35
Jamaica	1	0	1	0
Jordan	3	1	1	1
Kenya	75	6	49	20
Kyrgyzstan	1	0	1	0
Lebanon	23	3	15	5
Lesotho	1	1	0	0

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Table 10. Summed violent event counts per country (total of 79) from 1998-2020 (M-Z)

Country	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
Liberia	11	4	0	7
Libya	58	12	39	7
Madagascar	2	0	2	0
Malaysia	1	1	0	0
Mali	67	21	24	22
Mauritania	3	1	0	2
Mexico	124	0	121	3
Morocco	1	0	0	1
Myanmar	85	58	7	20
Namibia	2	0	0	2
Nepal	27	9	1	17
Niger	25	8	4	13
Nigeria	196	19	142	35
North Macedonia	2	1	0	1
Pakistan	114	45	32	37
Peru	6	6	0	0
Philippines	70	50	4	16
Russia	28	25	0	3
Rwanda	16	12	0	4
Senegal	10	5	1	4
Serbia	3	2	0	1
Sierra Leone	13	4	0	9
Somalia	115	20	73	22
South Africa	2	0	1	1
South Sudan	82	11	52	19
Sri Lanka	22	10	3	9
Sudan	170	24	95	51
Syria	178	24	125	29
Tajikistan	5	4	0	1
Tanzania	5	1	0	4
Thailand	33	19	0	14
Togo	1	0	0	1
Tunisia	6	4	0	2
Turkey	37	28	3	6
Uganda	65	21	27	17
Ukraine	11	10	1	0
Yemen	37	15	18	4
Zambia	1	0	0	1
Zimbabwe	1	0	0	1
Total	2780	794	1140	846

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Table 11. Mean climate variables per country (total of 81) from 1998-2020 (A-L)

Country	Temperature	Δ Temperature	Precipitation	Δ Precipitation	Baseline > 20°C
Afghanistan	13.22	1.07	346.81	11.28	
Algeria	23.47	0.76	90.68	-3.61	Yes
Angola	21.87	0.35	1049.57	5.27	Yes
Azerbaijan	13.50	1.17	469.64	-0.45	
Bangladesh	25.53	0.40	2442.47	-263.10	Yes
Benin	28.29	0.61	1042.09	-9.99	Yes
Bhutan	5.89	0.66	1372.74	-19.33	
Bolivia	21.29	-0.06	1153.82	1.70	Yes
Brazil	25.56	0.80	1784.52	7.08	Yes
Burkina Faso	29.49	0.46	809.47	1.04	Yes
Burundi	20.14	0.56	1213.17	46.81	
Cambodia	27.37	0.30	1870.54	55.52	Yes
Central African Republic	25.67	0.51	1351.79	-15.47	Yes
Chad	27.67	0.80	336.95	1.92	Yes
China	8.00	0.92	631.12	12.06	
Colombia	25.22	0.54	2574.62	38.90	Yes
Comoros	25.13	0.35	1743.71	-99.65	Yes
Congo	24.89	0.40	1645.84	19.88	Yes
Cote d'Ivoire	27.02	0.52	1302.67	-49.13	Yes
Democratic Republic of Congo	24.43	0.41	1505.81	-10.72	Yes
Ecuador	21.82	0.27	2150.87	98.86	Yes
Egypt	23.19	0.94	23.51	-4.85	Yes
Eritrea	26.24	0.91	379.18	6.39	Yes
Ethiopia	23.27	0.68	874.67	21.57	Yes
Gambia	28.69	0.76	877.27	-70.70	Yes
Georgia	8.29	1.05	1163.53	31.03	
Ghana	27.77	0.59	1207.80	-9.83	Yes
Guatemala	24.20	0.79	2174.51	-1.01	Yes
Guinea	26.16	0.61	1757.03	-82.93	Yes
Guinea-Bissau	27.99	0.74	1735.86	-94.32	Yes
Guyana	26.44	0.53	2464.32	31.91	Yes
Haiti	25.29	0.78	1483.71	29.51	Yes
Honduras	24.89	0.69	1895.37	-26.06	Yes
Indonesia	26.17	0.40	2706.64	78.21	Yes
Iran	18.75	1.36	210.72	-22.90	
Iraq	22.89	1.21	202.09	-22.97	Yes
Jamaica	26.46	0.68	2001.93	81.28	Yes
Jordan	19.65	1.27	102.52	-19.74	
Kenya	25.48	0.94	724.84	43.33	Yes
Kyrgyzstan	3.03	1.47	468.65	48.57	
Lebanon	17.13	1.28	685.53	-59.43	
Lesotho	12.29	1.03	802.28	-55.73	

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Table 12: Mean climate variables per country (total of 81) from 1998-2020 (L-Z)

Country	Temperature	Δ Temperature	Precipitation	Δ Precipitation	Baseline > 20°C
Liberia	25.57	0.38	2426.97	-25.86	Yes
Libya	22.80	0.83	42.10	-4.79	Yes
Madagascar	23.25	0.31	1474.39	-6.12	Yes
Malaysia	26.63	0.58	2998.08	115.13	Yes
Mali	29.28	0.73	304.97	-4.66	Yes
Mauritania	28.87	0.78	110.23	-1.17	Yes
Mexico	21.80	0.86	753.14	26.07	Yes
Morocco	18.02	0.92	331.07	-29.45	
Myanmar	23.90	0.44	2151.91	-12.87	Yes
Namibia	20.43	0.57	287.93	7.05	
Nepal	10.12	0.79	1122.91	-37.34	
Niger	28.08	0.64	172.12	4.29	Yes
Nigeria	27.42	0.50	1199.59	-17.53	Yes
North Macedonia	21.04	0.88	629.63	7.30	Yes
Pakistan	21.04	0.88	292.34	6.58	Yes
Peru	19.87	0.31	1575.82	36.48	
Philippines	26.69	0.60	2685.23	189.62	Yes
Russia	-3.66	1.42	479.27	25.22	
Rwanda	18.47	0.79	1224.64	50.48	
Senegal	28.89	0.74	728.21	-46.06	Yes
Serbia	11.74	1.25	747.26	27.14	
Sierra Leone	26.66	0.51	2716.46	-118.84	Yes
Somalia	27.27	0.33	267.56	18.39	Yes
South Africa	18.56	1.00	473.53	-22.14	
South Sudan	28.22	1.07	973.87	7.40	Yes
Sri Lanka	27.67	0.80	1705.79	27.28	Yes
Sudan	28.13	1.11	254.98	1.10	Yes
Syria	18.94	1.11	309.55	-26.04	
Tajikistan	2.80	1.17	696.96	70.05	
Tanzania	23.06	0.82	979.69	-3.50	Yes
Thailand	26.99	0.49	1683.26	42.29	Yes
Togo	27.81	0.60	1170.39	8.80	Yes
Tunisia	20.44	1.23	311.25	2.69	
Turkey	11.98	1.01	629.90	11.58	
Uganda	23.70	1.16	1250.80	101.92	Yes
Ukraine	9.57	1.43	549.99	8.91	
Yemen	26.13	0.54	171.02	4.38	Yes
Zambia	22.10	0.60	1010.50	8.19	Yes
Zimbabwe	22.02	0.79	707.19	-25.42	Yes
Mean	21.88	0.76	1091.74	1.93	

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Additional check for low variation in changes

When estimating the long-difference approach, Kalkuhl and Wenz (2020) use an additional cross-sectional model in their analysis because of low variation in their climate data. They argue that within-country changes are stronger correlated than the absolute levels of temperature and precipitation, causing low statistical significance for the climate-related coefficients (Kalkuhl and Wenz 2020). In the data used here, within-country deviation is indeed often bigger than the respective changes for the two intervals in the long-difference approach. Figure 4 shows temperature and precipitation changes per country plotted against aggregated violent events. A trendline is added to illustrate if linear regression is suitable for the data. Both Δ temperature and Δ precipitation values experience a sufficient amount of variation. Additional figures separated by violence type are also available (Figure 7 & Figure 8).

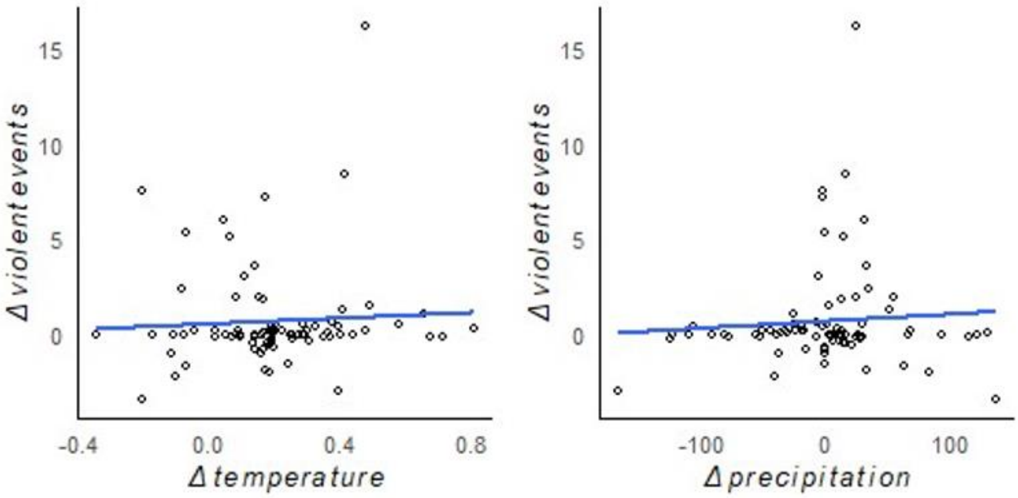

Figure 4: Variation of climate variables in the long-difference approach
Changes are calculated between the intervals 1998-2009 and 2010-2020 by using mean deviations per country. Own visualization. Source: UDCP and CRU.

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List of abbreviations

AAA	Additive error, Additive trend and Additive seasonality
Adj	adjusted
AIC	Akaike Information Criterion
AR	Assessment Report
BIC	Bayesian Information Criterion
CRU TS	Climate Research Unit gridded Time Series
CRU CY	Climate Research Unit Country
ETS	Exponential Smoothing
FE	Fixed Effects
HIEF	Historical Index of Ethnic Fractionalization
GDP	Gross Domestic Product
ILO	International Labor Organization
IPCC	Intergovernmental Panel on Climate Change
NOAA	National Oceanic and Atmospheric Administration
Obs.	Observations
OLS	Ordinary Least Squared
OWID	Our World in Data
PGLM	Panel Generalized Linear Model
PPP	Purchasing Power Parities
PRIO	Peace Research Institute Oslo
RoW	Regimes of the World
UCDP	Uppsala Conflict Data Program
UN DESA	United Nations Department of Economic and Social Affairs
UNDP	United Nations Development Programme
UNESCO	United Nations Educational, Scientific and Cultural Organization
UNICEF	United Nations International Children's Emergency Fund
US	United Staates
VDEM	Varieties of Democracy
WG	Working Group
WHO	World Health Organization

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List of symbols

α	Intercept of the basic poisson model
β	Vector of coefficients
$\lambda_{i,t}$	Mean conditional count of dependent variable for country i in year t
θ	Dispersion parameter of negative binominal model
$y_{i,t}$	Dependent variable for country i in year t
$X_{i,t}$	Vector of independent variables for country i in year t
$T_{i,t-1}$	Vector of climate deviation values for country i in year $t-1$
Δy_i	Change of dependent variable for country i
ΔT	Vector of changes of climate levels for country i
ΔX_i	Change of independent variables for country i
ψ_t	Country fixed effects
ϕ_i	Location fixed effects
$\varepsilon_{i,t}$	Error term for country i in year t
$f(y_i \vert x_i)$	Density function of y_i conditional on x_i
$E[y_i \vert x_i]$	Expected value of y_i conditional on x_i
$Var[y_i \vert x_i]$	Variance of y_i conditional on x_i

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Appendix B. Supplementary Tables

Table 13 A. Systematic comparison of selected findings in the literature (2004-2017)

Author	Study objects	Methods and data	Main findings
(Miguel et al. 2004)	precipitation deviation from average country rainfall level as IV for economic growth on **civil war** in Africa from 1981-1999	panel regression, 2,5° grid scale, 1 year lag, country FE and time trends, 1000 battle related deaths threshold, UCDP/PRIO	negative 5 percentage point growth shock increases civil war likelihood by ~ 50% in the next year
(Burke et al. 2009)	temperature and precipitation effects on **civil war** in Africa 1981-2002	panel regression, country level, 1 year lag, country FE and time trends, 1000 battle related deaths threshold, UCDP/PRIO, CRU	warmer years correlated to higher likelihood of war, 54% projected increase in civil war in Africa by 2030
(Fjelde and Uexkull 2012)	rainfall deviation effects on **non-state conflict** (communal) in Sub-Saharan Africa between 1990 and 2008	panel regression, sub-country level, entity FE, 25 battle related deaths threshold, UCDP/PRIO, GPCP	Large negative rainfall deviations increase communal conflict risk
(O'Loughlin et al. 2014)	precipitation and temperature effects on the number of **conflict** (battles) **non-state conflict** (riots and protests) and **one-sided violence** (violence against civilians) from 1980-2012 in Sub-Saharan Africa	Quasi experimental matching analysis and poisson multilevel model-random random effects model, 1 year lag, ACLED/UCDP, CRU	High temperature increases number of violent events (10% increase for 1 standard deviation), driven by violence against civilians, no precipitation effects, only in Sub-Saharan-Africa
(Buhaug et al. 2015)	precipitation and temperature deviation effects as an IV for food production on **non-state conflict** (intercommunal violence) from 1990-2009 in Sub-Saharan Africa	panel regression, 1 year lag, time varying regressors, 25 battle related deaths threshold, UCDP, NOAA	no robust link between non-state conflict and precipitation and temperature deviation
(Schleussner et al. 2016)	Climate related natural disaster effects on **armed conflict** outbreak from 1980-2010 in	Event coincidence analysis, 25 battle related deaths threshold, UCDP/PRIO, NatCatSERVICE	9% coincidence rate between armed conflict outbreak and disasters
(Nordkvelle et al. 2017)	Monthly precipitation effects on **non-state conflict** (communal) from 1989-2013 in selected subnational regions	Panel regression with randomized treatment effect, region FE, 25 battle related deaths threshold, UCDP, GADM	Short dry and long wet periods increase communal conflict likelihood

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Table 13 B. Systematic comparison of selected findings in the literature (2018-2023)

Author	Study objects	Methods and data	Main findings
(van Weezel 2019)	precipitation effects on **non-state conflict** (communal) in Ethiopia and Kenya from 1999-2014 on a district level	negative binominal long difference regression, count data, 25 battle related deaths threshold, UCDP, CenTrends	average precipitation decline associated with 1,3 conflict events more per district
(Helman et al. 2020)	temperature effects on **non-state conflict** in Africa and the Middle East from 1990-2017	structural equation modelling approach, 0,5° grid scale, 25 battle related deaths threshold, UCDP/PRIO, CHIRPS	direct effects stronger than indirect ones, increased risk in Afrika, decreased risk in the Middle east
Coulibaly and Managi (2022)	rainfall effects on **armed conflict** on a subnational level from 1998-2020	panel regression, entity and time FE, 25 battle related deaths threshold, UCDP, CHIRPS	rainfall reduces conflict risk on a monthly level, no results for annual level, results depend on context and region
(Cappelli et al. 2022)	long term temperature and precipitation deviation effects on **armed conflict** in Africa from 1990-2016	zero inflated negative binominal panel regression, count data, 1° grid scale, 1 year lag, country FE and time trends, 1 battle related deaths threshold, UCDP/PRIO	Long term climate changes can increase the number of conflicts by 4-5 times, depending on spatial interactions and nonlinearity
(Ge et al. 2022)	Temperature and precipitation deviations **on armed conflict** from 2000-2015 globally	boosted regression tree modeling framework, 0,1° grid scale, 1 battle related deaths threshold, UCDP/PRIO, CRU	Long term climate deviations have greater impact on risk (3,806%) than on incidence (2,5%)
This paper (2024)	temperature and precipitation (deviations) effects on **armed conflict**, **non-state conflict** and **one-sided violence** in 81 low- and mid-income countries from 1998-2020	poisson & negative binominal panel regression and long difference approach, count data, country level, 1 year lag, country and year FE, 25 battle related deaths threshold, UCDP/PRIO, CRU	heterogenous effects of climate on different conflict types, 1°C of 1-year lagged temperature increases non-state conflict (8.54%) counts, low evidence for precipitation effects on non-state conflict and one-sided violence, no climate effects of deviations from a long term trend, positive long term temperature effects in the long difference approach on non-state conflict, warmer countries are more likely to experience high counts.

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Table 14. Count data models with annual climate levels and controls

	(1)	(2)	(3)	(4)
Dependent Var.:	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
T	-0.167 (0.175)	-0.076 (0.128)	-0.335 (0.298)	-0.175 (0.177)
T *t-1*	0.025 (0.018)	0.013 (0.019)	0.054 (0.053)	0.008 (0.020)
P	0.0004 (0.0003)	0.0004* (0.0002)	-0.0008 (0.0006)	0.001*** (0.0004)
P *t-1*	-0.0002 (0.0001)	-0.0001 (0.0001)	-6.25e-5 (0.0002)	-0.0001 (0.0001)
log(GDP) *t-1*	-0.021 (0.086)	0.018 (0.092)	-0.458* (0.261)	-0.002 (0.099)
Democracy *t-1*	-0.314 (0.209)	-0.223 (0.186)	-0.004 (0.252)	-0.477* (0.263)
Family	Neg. Bin.	Poisson	Neg. Bin.	Neg. Bin.
Observations	1,862	1,379	1,149	1,494
Squared Cor.	0.55466	0.55553	0.44320	0.53033
Pseudo R2	0.26013	0.25864	0.23077	0.25723
BIC	5,105.9	2,696.2	2,680.7	2,916.0

Note: *** = p < 0.01, ** = p < 0.05, * = p < 0.1. S.E clustered at the country level. All models include country and year fixed-effects. T = temperature & P = precipitation.

Table 15. Logit model with annual climate levels and controls

	(1)	(2)	(3)	(4)
Dependent Var.:	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
T	-0.333 (0.376)	-0.128 (0.416)	-0.691 (0.458)	-0.379 (0.403)
T *t-1*	0.080** (0.036)	0.064 (0.042)	0.077 (0.055)	0.005 (0.033)
P	0.001 (0.0007)	0.0007 (0.0008)	-0.0005 (0.0008)	0.003*** (0.0009)
P *t-1*	-0.0005* (0.0003)	-0.0005 (0.0004)	-4.05e-5 (0.0003)	-0.0002 (0.0003)
log(GDP) *t-1*	0.203 (0.144)	0.174 (0.159)	-0.253 (0.301)	-0.084 (0.163)
Democracy *t-1*	-0.580 (0.430)	-0.660 (0.471)	-0.124 (0.491)	-0.813** (0.373)
Observations	1,633	1,311	1,103	1,448
Squared Cor.	0.41913	0.43692	0.32522	0.35934
Pseudo R2	0.36171	0.36504	0.27742	0.31211
BIC	2,065.2	1,733.0	1,461.0	1,868.1

Note: *** = p < 0.01, ** = p < 0.05, * = p < 0.1. S.E clustered at the country level. All models include country and year fixed-effects. T = temperature & P = precipitation.

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Table 16. Count data models with annual climate levels and controls

	(1)	(2)	(3)	(4)
Dependent Var.:	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
Δ T	-0.180 (0.153)	-0.085 (0.111)	-0.273 (0.260)	-0.236 (0.165)
Δ T *t-1*	-0.059 (0.122)	-0.055 (0.105)	-0.045 (0.194)	-0.033 (0.171)
Δ P	-0.0007 (0.0004)	0.0001 (0.0002)	-0.001* (0.0009)	-0.001* (0.0006)
Δ P *t-1*	-0.001** (0.0005)	-0.0003 (0.0003)	-0.003*** (0.0007)	-0.001* (0.0006)
log(GDP) *t-1*	-0.024 (0.085)	0.012 (0.091)	-0.470* (0.252)	0.004 (0.100)
Democracy *t-1*	-0.316 (0.202)	-0.237 (0.184)	-0.006 (0.230)	-0.459* (0.261)
Family	Neg. Bin.	Poisson	Neg. Bin.	Neg. Bin.
Observations	1,862	1,379	1,149	1,494
Squared Cor.	0.55726	0.55583	0.45765	0.52183
Pseudo R2	0.26128	0.25796	0.23466	0.25640
BIC	5,099.2	2,698.1	2,669.9	2,918.5

Note: *** = p < 0.01, ** = p < 0.05, * = p < 0.1. S.E clustered at the country level. All models include country and year fixed-effects. T = temperature & P = precipitation.

Table 17. Logit model with long-term climate deviations and controls

	(1)	(2)	(3)	(4)
Dependent Var.:	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
Δ T	-0.389 (0.323)	-0.126 (0.365)	-0.679 (0.444)	-0.528 (0.365)
Δ T *t-1*	-0.222 (0.288)	-0.199 (0.309)	-0.294 (0.319)	0.052 (0.330)
Δ P	-0.0006 (0.0009)	0.0009 (0.001)	-0.002 (0.001)	-0.002* (0.001)
Δ P *t-1*	-0.003*** (0.0009)	-0.002 (0.001)	-0.004*** (0.0009)	-0.002* (0.001)
log(GDP) *t-1*	0.183 (0.156)	0.155 (0.165)	-0.308 (0.280)	-0.083 (0.160)
Democracy *t-1*	-0.660 (0.413)	-0.724 (0.460)	-0.139 (0.440)	-0.785** (0.364)
Observations	1,633	1,311	1,103	1,448
Squared Cor.	0.41588	0.43165	0.33800	0.35865
Pseudo R2	0.36031	0.36335	0.28714	0.30685
BIC	2,068.1	1,736.0	1,448.5	1,877.3

Note: *** = p < 0.01, ** = p < 0.05, * = p < 0.1. S.E clustered at the country level. All models include country and year fixed-effects. T = temperature & P = precipitation.

Table 18. Count data models with annual climate levels without year fixed effects

	(1)	**(2)**	**(3)**	**(4)**
Dependent Var.:	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
T	-0.023 (0.160)	0.119 (0.112)	-0.218 (0.326)	-0.105 (0.136)
T *t-1*	0.032* (0.019)	0.021 (0.018)	0.087*** (0.030)	0.008 (0.021)
P	0.0005* (0.0003)	0.0004** (0.0002)	-0.0007 (0.0005)	0.001*** (0.0004)
P *t-1*	-0.0002* (0.0001)	-0.0002 (0.0001)	4.34e-5 (0.0003)	-0.0002 (0.0002)
Family	Neg. Bin.	Poisson	Neg. Bin.	Neg. Bin.
Observations	1,862	1,379	1,149	1,494
Squared Cor.	0.50149	0.50909	0.36791	0.49329
Pseudo R2	0.24491	0.24137	0.21082	0.23525
BIC	5,013.3	2,570.7	2,566.8	2,806.8
Over-dispersion	2.1258	--	0.97475	6.7770

Note: *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. S.E clustered at the country level. All models include only country fixed-effects. T = temperature & P = precipitation.

Table 19. Logit model with annual climate levels without year fixed effects

	(1)	**(2)**	**(3)**	**(4)**
Dependent Var.:	Violent Events	Armed Conflict	Non-state Conflict	One-sided Violence
T	-0.222 (0.310)	0.106 (0.348)	-0.564 (0.371)	-0.175 (0.274)
T *t-1*	0.082** (0.041)	0.064 (0.047)	0.100** (0.045)	0.015 (0.036)
P	0.0010 (0.0006)	0.0009 (0.0008)	-0.0005 (0.0006)	0.002*** (0.0008)
P *t-1*	-0.0006** (0.0003)	-0.00000018	2.81e-5 (0.0003)	-0.0004 (0.0002)
Observations	1,633	1,311	1,103	1,448
Squared Cor.	0.39494	0.39858	0.29381	0.32252
Pseudo R2	0.3397	0.33714	0.25865	0.27954
BIC	1,933.50	1,610.00	1,317.00	1,750.50

Note: *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. S.E clustered at the country level. All models include only country fixed-effects. T = temperature & P = precipitation.

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Appendix C. Supplementary Figures

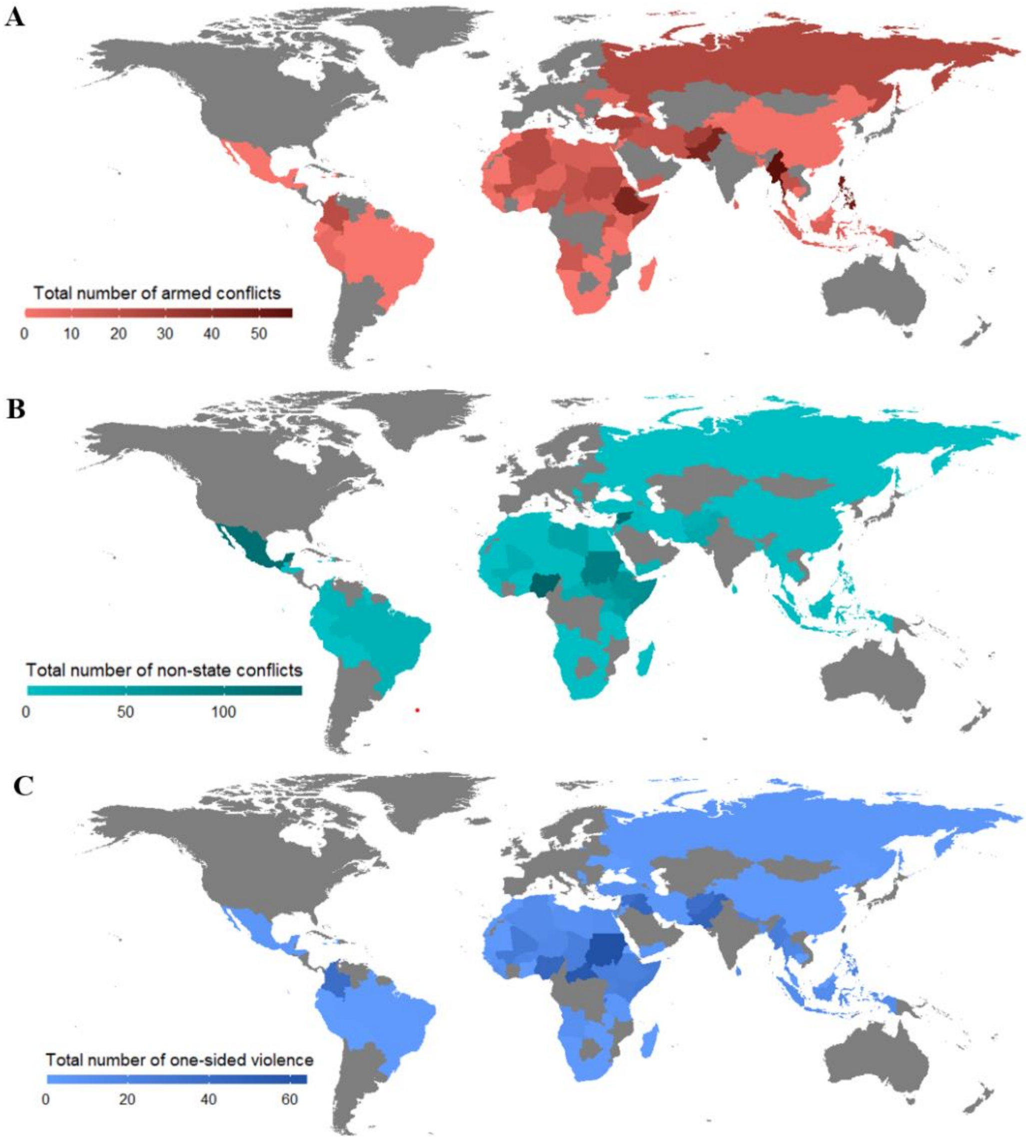

Figure 5: Descriptive map of conflict heterogeneity

Violent events are mapped separately to account for regional variability and conflict heterogeneity, (A) armed conflict, (B) non-state conflict, and (C) one-sided violence events**,** all 1998-2020 for 79 selected low- and mid-income countries. Own visualization, Source: UCDP/PRIO

10.14324/111.444.2755-0877.1841

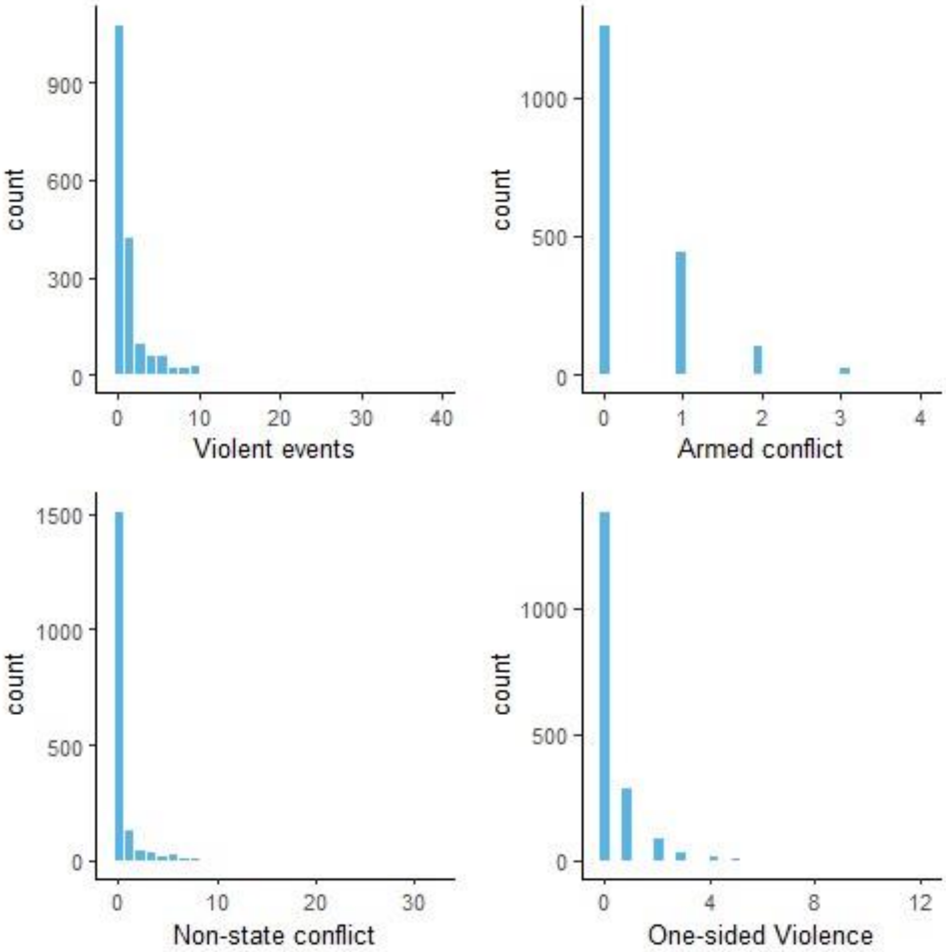

Figure 6: Histograms of the dependent variables

Histograms illustrate the absolute frequency of the dependent variables. The X axis shows the violent event count (x events in a country i in a year t) the Y axis shows how often this specific count was counted in the data. Zero counts (no event in a country i in a year t) are more often recorded (counted) than higher counts, indicating that peace is the norm. Own visualization, source: UCDP/PRIO

10.14324/111.444.2755-0877.1841

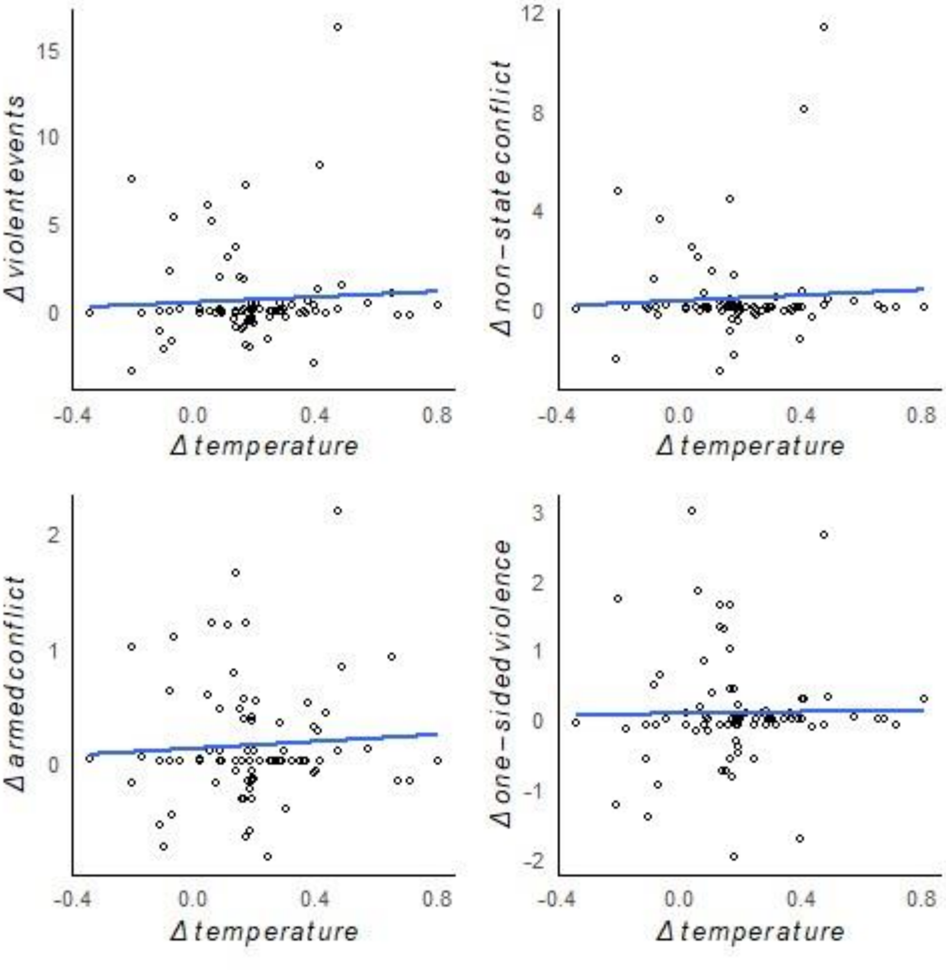

Figure 7: Scatterplots of changes in conflict counts and temperature levels used in the long-difference approach Changes are computed between the country means of each period (1998-2009 and 2010-2020). A linear trendline was added to illustrate how a simple linear model would fit a regression line.

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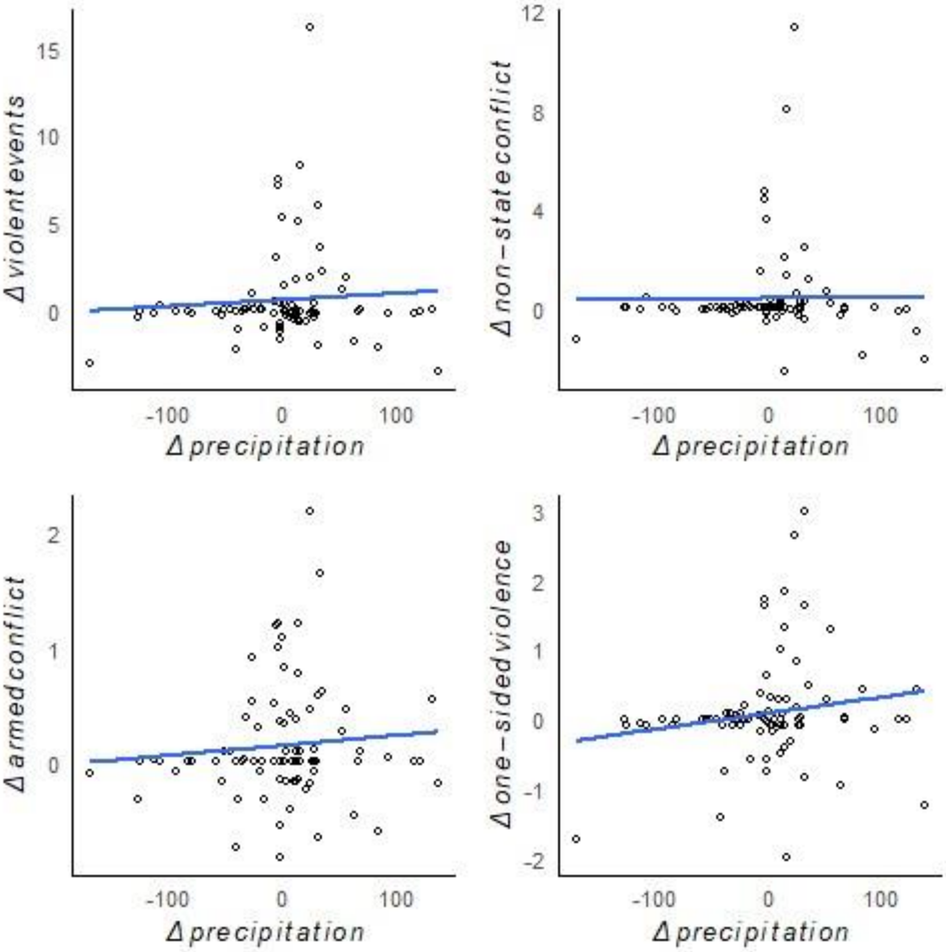

Figure 8: Scatterplots of changes in conflict counts and precipitation levels used in the long-difference approach Changes are computed between the country means of each period (1998-2009 and 2010-2020). A linear trendline was added to illustrate how a simple linear model would fit a regression line.

10.14324/111.444.2755-0877.1841

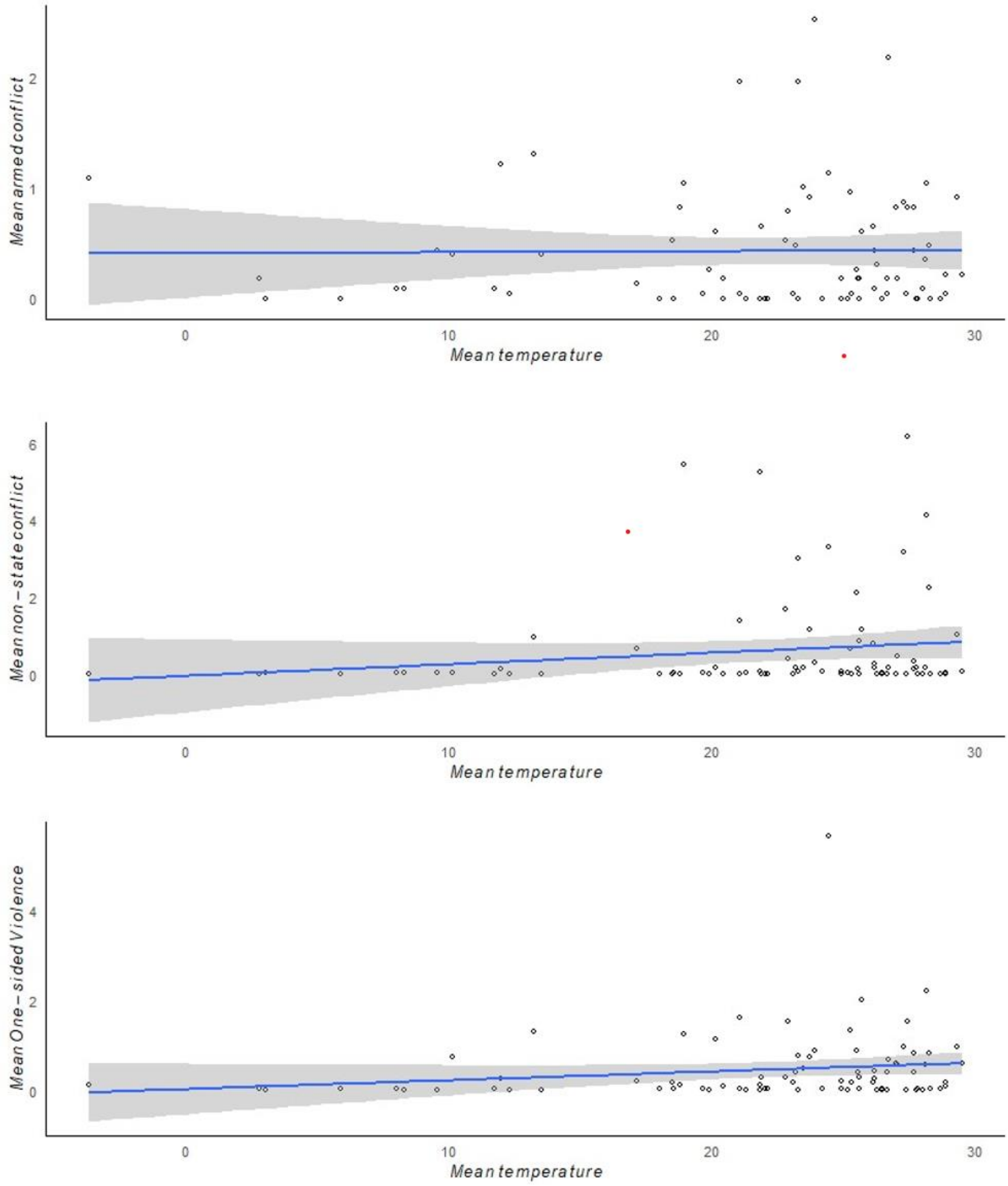

Figure 9: Scatterplots of temperature and different violent events

Mean annual temperature is plotted against the mean count of an event type per country from 1998-2020 for 79 selected low- and mid-income countries. A linear trendline with standard error visualization is added to illustrate how a simple linear model would fit a regression line. Own Visualization. Source UCDP & CRU.

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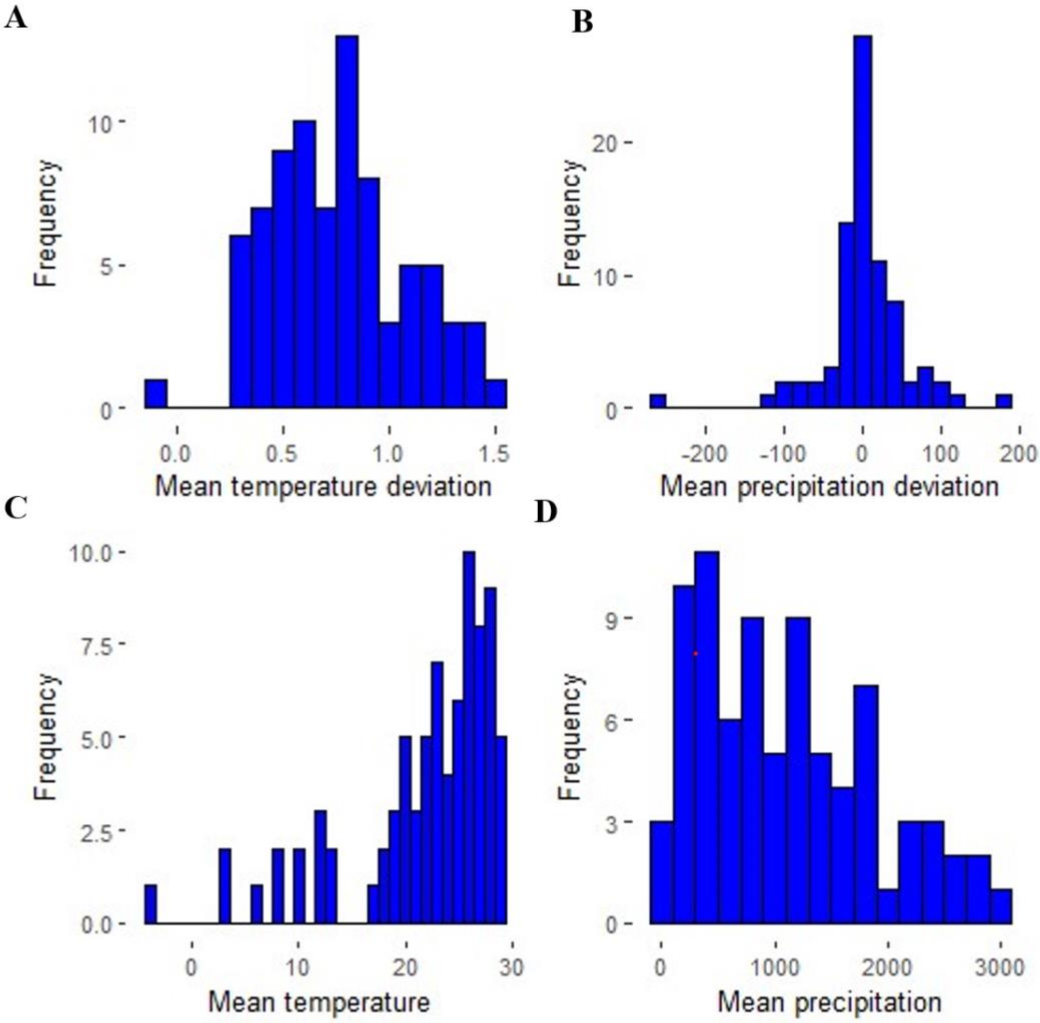

Figure 10: Distribution of country means of climate variables
Relative frequency of mean temperature in °C (A), mean precipitation in mm (B), mean temperature deviation in °C (C), and mean precipitation deviation in mm (D). Own visualization. Source: CRU

10.14324/111.444.2755-0877.1841