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Extreme weather impacts do not improve conflict predictions in Africa

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Quantitative climate and conflict research has thus far considered the role of biophysical extreme weather impacts in conflict dynamics only to a limited extent. We do not fully understand if and if so how, extreme weather impacts can improve conflict predictions. Addressing this gap, we use the Generalized Random Forest (GRF) algorithm to evaluate whether detailed information on extreme weather impacts improves conflict forecasts made with well known conflict predictors such as socio-economic, governance, and history of conflict indicators. We integrate data on biophysical extreme weather impacts such as droughts, floods, crop production shocks, and tropical cyclones from the Inter-Sectoral Impact Model Intercomparison Project 2a (ISIMIP2a) project into predictive models of conflict in mainland Africa between 1994 and 2012. While we find that while extreme weather impacts alone predict violent conflicts modestly well, socio-economic and conflict history indicators remain the strongest individual predictors of conflicts. Finally, fully specified forecast models including conflict history, governance, and socio-economic variables are not improved by adding extreme weather impacts information. Some part of this can be explained by spatial correlations between extreme weather impacts and other socioeconomic and governance conditions. We conclude that extreme weather impacts do not contain any unique information for forecasting annual conflict incidence in Africa, which calls into question its usefulness for early warning.

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Introduction

Most quantitative work on climate and conflict focuses on temperature or precipitation (see the studies considered in Burke et al. 2015 for example). Thus, we have limited knowledge about the role biophysical impacts of extreme weather (e.g., droughts, floods, crop production shocks) play in conflict dynamics. This matters because extreme weather impacts are known to be getting worse as the climate changes (Cissé et al. 2022; Coumou and Rahmstorf, 2012; Lange et al. 2020; Lehmann et al. 2015). Concentration of people in vulnerable areas and persistent poverty will likely increase the number of human tragedies resulting from these more frequent biophysical hazards associated with climate change (Cappelli et al. 2021).

What research exists on the relationships between extreme weather events and armed conflict, tends to highlight its complexity (Beaumont and Coning, 2022; Siddiqi, 2014). For instance, the same devastating 2004 tsunami that served as a "powerful catalyst in [already ongoing] diplomatic talks" in Acheh (Gaillard et al. 2008), proceeded an escalation of the civil war in Sri Lanka. As Ide (2023) reports from a study on ongoing conflicts, "changes in the strategic environment for conflict parties is by far the most relevant causal pathway connecting disasters to conflict dynamics." At the same time Ide (2023) also documents dozens of cases where serious disasters had no impact on conflict dynamics.

Improving forecasts of future conflicts does not require extreme weather impacts to play an important role in conflict dynamics in all cases. If extreme weather impacts affect conflict dynamics only under certain conditions, and these conditions are identifiable, then these events might improve future conflict predictions. Thus, extreme weather impacts may be quite useful for short-term conflict forecasting due to their potential to change social, humanitarian, political, and environmental conditions in a relatively short time frame (e.g., days for cyclones or floods, or up to a few years for droughts) (Schleussner et al. 2016).

This paper asks: *Can information about extreme weather impacts improve conflict predictions?* This question is motivated by an investigation of whether extreme weather impacts (and climate variables more generally) can help us to better understand and predict conflict patterns. We use extreme weather data from state-of-the-art climate impact models that give us high resolution drought, flood, crop production shock, and tropical cyclone data. We then explicitly test, using the non-parametric Generalized Random forest (GRF) model (Athey et al. 2019), whether adding extreme weather impacts (i.e., droughts, floods, crop production shocks, and tropical cyclones) to forecasts made using well known conflict predictors improves the forecasting accuracy. We follow up our main question by investigating what types of predictive information extreme weather impacts might contain. For example: *Do extreme weather impacts help us to predict conflict outbreaks? Or just conflict incidence?*

In contrast to causal analyses, which typically isolate the effect of one variable on the outcome of interest, predictive analyses use a number of variables to generate guesses about unobserved outcomes. When these predictions are about the future, the predictive analysis becomes forecasting. Prediction enables us to estimate a larger number of parameters than causal inference (Hegre et al. 2017). We can estimate the total amount of additional predictive information contained in a set of extreme weather impacts instead of trying to isolate the effect of a single one (e.g., just floods). Thus, we can quantify the importance of extreme weather impacts, as a group, on conflict. This is a major advantage over previous literature that has almost always looked only at specific extreme weather impacts or climatic variables in isolation (Breckner and Sunde, 2019; Buhaug et al. 2021; Burke et al. 2018; Perry, 2013; Von Uexkull et al. 2016). We can also account for the heterogeneous, non-linear ways in which climate change and extreme weather impacts are known to impact conflict, conditioned upon factors such as agricultural dependency, political marginalization, etc. (Breckner and Sunde, 2019; Buhaug et al. 2021; Goyette and Smaoui, 2022; Ide et al. 2021; Von Uexkull et al. 2016). Finally, prediction avoids focusing on variables that causally affect the outcome of interest in statistically significant ways, but are not good predictors of conflict and likely do not contain enough information about conflict to be meaningful. For example, Schutte et al. (2021) documents such a phenomenon where drought and temperature anomalies are causally linked to asylum migration, but are poor predictors of these same asylum flows.

The few scholarly attempts to incorporate climate impact modeling into understanding future conflict risks, have focused on generating projections far into the future along Representative Concentration Pathway (RCP) and Shared Socioeconomic Pathway (SSP) scenarios (de Bruin et al. 2022). Perhaps closest to our paper, Hoch et al. (2021) assessed the role of hydro-climatic indicators as drivers of armed conflict in Africa and found their importance is limited. However, this paper solely considers a hydrological model (Sutanudjaja et al. 2018), employs a small number of socioeconomic covariates, and is primarily focused on how adding hydrological indicators impacts future conflict projections along SSP-RCP scenarios. Witmer et al. (2017) conducts a similar analysis using temperature from Harris et al. (2014), and reports that the ultimate effect of rising temperature on conflicts depends on population growth and political rights. Crucially, this analysis only includes four socioeconomic and governance variables in it's future scenarios; non-violent media reports, infant mortality rates, population, and political rights. Because these studies are interested in long-term conflict projections, they can only consider a small number of climate impacts and socioeconomic and governance variables. This limits their ability to address our central question of whether extreme weather impacts as a group improve conflict predictions when added to a fully specified conflict prediction model.

A small body of scholarship attempts to combine climatic predictors more broadly into conflict prediction, without using sophisticated climate modeling. For example, Schleussner et al. (2016) rely on insurance data from Wirtz et al. (2014) and find that only the most extreme disasters (>10% of gross domestic product or GDP) increase the risk of armed conflict outbreaks globally, but that climate-related disasters increase armed-conflict risks in ethnically fractionalized countries. Perry (2013) finds drought and flood frequency increase accuracy in conflict prediction models, however this is based on static indicators of droughts and floods frequencies. Looking just at one country, Linke et al. (2022) find adding weather variability and vegetation health to a conflict prediction model based on survey data that included "29 demographic and contextual variables" actually made the predictions worse. Looking at both Indonesia and Colombia, Bazzi et al. (2022) report that "predictive accuracy improves little when we add time-varying factors, including natural disasters, ..., fluctuations in rainfall, [and] temperature."

The under-consideration of extreme weather impacts extends to the broader quantitative literature on climate and conflict in general. In a pair of influential literature reviews, Burke et al. (2015) and Hsiang and Burke (2014) show, although not intentionally, that the vast majority of scholarship on climate and conflict focuses primarily on temperature and precipitation.[1] When other extreme weather impacts are considered, it is often only a single type of impact such as droughts (Von Uexkull et al. 2016), floods (Ide et al. 2021), or crop production shocks (Vesco et al. 2021). Furthermore, the focus on causal analysis in this literature prevents adequate consideration of the total impacts of extreme weather impacts as a group, as well as the complex interactions between such impacts and socioeconomic, governance, and conflict history indicators.

By integrating extreme weather impact data into a non-parametric conflict model, our paper contributes to the existing quantitative literature on climate and conflict as well as the literature on conflict forecasting. We further the literature on conflict forecasting by explicitly testing whether extreme weather impacts add predictive information to well-known conflict forecasts, which can inform future modeling and policy making. We do this by integrating historical impact simulations forced by observational weather as generated within the second phase of the Inter-Sectoral Impact Model Comparison Project (ISIMIP2a) (Schewe et al. 2019) to capture extreme weather impacts such as weather driven crop production shocks (Arneth et al. 2017; Bondeau et al. 2007), droughts (Gosling et al. 2017; Mester et al. 2021), tropical cyclones (Geiger et al. 2018), and river flooding (Yamazaki et al. 2011) up to 5 years before conflict occurrence on a $0.5° \times 0.5°$ resolution into conflict prediction models. While a few analyses have partially integrated biophysical climate impact modeling into conflict forecasting, we are the first to explicitly test whether extreme weather impacts from climate impact models improve conflict predictions made using a full suite of socioeconomic, governance, and conflict history predictors. We find that, while extreme weather impacts can predict conflict alone, they do not improve upon predictions made using well-known conflict predictors. This finding advances the literature on conflict forecasting and reconciling this finding with previous studies that did find an effect will be important (Schleussner et al. 2016).

We also advance the broader quantitative literature on climate and conflict by showing that extreme weather impacts, even when heterogeneity is properly accounted for, contain very little information about conflict incidence in the following year. There is broad agreement in this literature that the impacts of climate change on conflict will be largely indirect and heterogeneous (sometimes this involved discussion about "scope conditions") (Mach et al. 2019; von Uexkull and Buhaug, 2021). This has led to numerous, successful attempts to find conditional relationships between climate and conflict (Buhaug et al. 2021; Schleussner et al. 2016; Vesco et al. 2021; Von Uexkull et al. 2016). Our findings from Africa between 1994 and 2012 suggest that such information has limited use for forecasting purposes.

The paper proceeds as follows. The "Data" section describes the data sources we use. The "Methods" section explains the algorithm, methodology, and evaluation metrics. The "Results" section presents the results and follow up analysis. We reflect on the meaning of our findings and point out opportunities for future work in the "Discussion" section.

Data

This analysis rests on linking well known predictors of conflict with the outputs from cutting edge climate impact models of extreme weather events, which we refer to in this paper as extreme weather impacts. Compared to previous models relying primarily on temperature or precipitation from climate models, climate impact models represent a step closer to the lived reality of someone on the ground. Previous work hypothesized that precipitation or temperature changes might impact conflict through agricultural shocks or floods (Dube and Vargas, 2013; Von Uexkull et al. 2016), but we go one step further by using actual crop production and flood modeling.

Every variable in our data set is described in detail in Section 1 titled "Data Sources" of the Supplementary Information. Furthermore, Section 5 of the Supplementary Information titled "Data visualization" contains maps of each variable at different administrative levels across time.

Outcome variables. We build a series of predictive models to test whether extreme weather impacts add predictive power to socioeconomic variables, governance indicators, and conflict history when predicting conflict incidence. Conflict incidence is a binary variable that answers the question, was there a conflict event recorded in a given place during a given year? To construct this variable we use geo-located conflict event data which comes from both the Uppsala Conflict Data Program (UCDP) and the Armed Conflict Location and Events Data Project (ACLED). To ensure our results are robust to specification and coding differences between the two data sources (which can be significant) (Eck, 2012; Rød et al. 2023), we use data from both. UCDP only records battles that are linked to a conflict in which more than 25 people die in a single year (Sundberg and Melander, 2013), while ACLED records battles, riots, protests, violence against civilians, strategic violence, and explosions regardless of how much life is lost (ACLED, 2019; Raleigh et al. 2010). We reduce these six ACLED categorizations to three to represent three theoretically separate forms of conflict by combining riots and protests into a single group as well as combining violence against civilians, strategic violence, and other events.[2]

The main challenge with our datasets is coverage. Both ACLED and UCDP now cover a large number of countries, but ACLED only goes back far enough (1997) for mainland Africa. Most of our extreme weather impacts data ends by 2012. Our conflict history predictor group primarily consists of lagged conflict data from the previous five years. So we begin our analysis for ACLED in 2002 because this is the first year with five years of conflict data lags available. Thus we analyze the years 2002–2012 for ACLED data, which gives us 11 years of data. UCDP goes back to 1989, so we analyze the years 1994–2012 for UCDP which gives us 19 years of data. We separately analyze UCDP prediction for the period 2002–2012 so that the results can be compared with those from ACLED. Visualizations of the conflict outcomes can be found in Section 5.2 of the Supplementary Information titled "Conflict Data Maps."

Administrative levels. Having defined our conflict variable, we next need to define the spatial units of analysis. For this, we draw upon the Database of Global Administrative Areas (GADM), which provides us with polygons of different administrative levels (GADM, 2018). We conduct our analyses at different administrative levels, i.e., country as well as two sub-national administrative levels, to capture different types of conflict patterns (Bazzi et al. 2022; Blair et al. 2017; van Weezel, 2018; von Uexkull and Buhaug, 2021). Nations are coded as level zero. Level one is the first sub-national level in a given country. Level two, the second subnational level, is the most granular level that we use. This gives us 6269 polygons at the most granular (second) administrative level. Using three separate, predefined spatial aggregation levels reduces the risk that our results are driven by aggregation bias. Visual inspections of the maps found in Section 5 titled "Data Visualization" of the Supplementary Information revels that a number of variables have significantly different spatial distributions and values when aggregated at different administrative levels.

Common predictor groups. For each of these observations, we measure socio-economic data, governance indicators, conflict history, and extreme weather impacts. Socio-economic data, governance indicators, and conflict history are our three common predictor groups which we use as a baseline to evaluate whether extreme weather impacts contain additional predictive information about future conflict.

Our conflict history predictor group is assembled from the same data sources, UCDP and ACLED, that we used to construct our conflict outcomes (Eck, 2012; Sundberg and Melander, 2013). Instead of using binary conflict incidences (i.e., conflict/no conflict), we use the number of recorded conflict events and total number of deaths for each of our four conflict types (i.e., UCDP battles, ACLED battles, ACLED riots and protests, and ACLED other) for each of the previous 5 years. For UCDP, we also include the number of battle deaths and the number of civilian deaths for each of the past 5 years. Furthermore, we have the estimated total number of people displaced by conflict, as well as the estimated number of people newly displaced by conflict, at the national level in the previous year (Desai et al. 2018). Visualizations of the conflict history variables can be found in Section 5.2 of the Supplementary Information titled "Conflict Data Maps," alongside the mapping of the outcome variables.

Our socio-economic data consists of crop production levels (Hurtt et al. 2020), infant mortality rates (CIESIN, 2005), land use (Hurtt et al. 2020; Ramankutty et al. 2008), population size (Goldewijk et al. 2017), area (GADM, 2018), population density (GADM, 2018; Goldewijk et al. 2017), urbanization (Goldewijk et al. 2017), GDP (Kummu et al. 2018), and GDP per capita (Kummu et al. 2018). Crop production levels are an average of the past 5 years' annual soy, maize, wheat, and rice yields (Arneth et al. 2017; Bondeau et al. 2007). Infant mortality is a static indicator of the number of deaths per 10,000 live births in 2000 (CIESIN, 2005). Land use comes from two sources. One calculates the percent of area being used for crop production in 2000 (Ramankutty et al. 2008). The second calculates the average amount of land used for pastureland, irrigated cropland, rain fed cropland, and total crop land over the past 5 years as well as the percentage change in each type of land use over the past 15 years (Hurtt et al. 2020). Similarly, population data includes the 5 year moving averages and 15 years growth patterns for both population levels and urbanization rates (Goldewijk et al. 2017). Area is the area of each administrative unit (GADM, 2018). Population density is population per square kilometer. GDP and GDP per capita are 5-year moving averages and 15-year growth rates for both total and per capita output (Kummu et al. 2018). Taken together, these give a reasonable socioeconomic profile of each of the administrative units in our data set. Visualizations of the socioeconomic variables can be found in Section 5.1 of the Supplementary Information titled "Socioeconomic Data Maps."

We have three sources of governance indicators. The World Bank's World Governance Indicators are reported at the national level. They include government effectiveness, rule of law, control of corruption, political stability, regulatory quality, and voice and accountability (Kaufmann et al. 2011). We also have a measure of liberal democracy, which is an annual, country level index (Gerring et al. 2021). Our only subnational governance indicator is a count of the number of ethnic groups with a "homeland" in a given administrative area. Because the point of this analysis is to determine if extreme weather impacts add information to other common conflict predictors, the paucity of subnational data should not limit the analysis because subnational governance data availability is limited in general, not just for our analysis. Visualizations of the governance data can be found in Section 5.3 of the Supplementary Information titled "Governance Data Maps."

Extreme weather impacts. We have several types of extreme weather impacts data: floods, droughts, tropical cyclones, crop production shocks, and disaster-induced displacement. With the exception of disaster induced displacement, we borrowed heavily from the extreme weather impacts data found in Lange et al. (2020), which is itself based on the ISIMIP project (Frieler et al. 2017; Schewe et al. 2019). One advantage of using ISIMIP2a data is that the simulations are harmonized and comparable across all the different impact categories. This means, however, that we cannot switch crop simulation from ISIMIP2a for other crop simulations. Thus, we are limited by the data availability of ISIMIP2a, and cannot extend our study beyond 2012.

To measure flooding, we use results from the global flood model CaMa-Flood (Yamazaki et al. 2011), driven by runoff simulated by the global hydrological model WaterGAP2 (Mueller Schmied et al. 2016), which was in turn driven by historical meteorological inputs from the Princeton Global Forcing dataset version 2 (PGFv2) (Sheffield et al. 2006) in the framework of the Inter-Sectoral Impact Model Intercomparison Project (Frieler et al. 2017), phase 2a (Schewe et al. 2019). Inundated areas at a 2.5 arc min horizontal resolution were derived assuming that each pixel is protected against a 100-year return level flood, or according to the regional flood protection standard indicated in the FLOPROS database (Scussolini et al. 2016).[3] Essentially, our flood models tell us where there was a flood, and where there was a flood with enough water that it would only happen every 100 years under preindustrial conditions. We then aggregate this grid cell-level data to administrative unit levels. We do this both by taking the averages by land area and by population. Using averages instead of totals allows us to compare across administrative areas with different sizes and populations. What we end up with is a measure of the average exposure of each person, and each populated location, in an administrative area to floods and major floods for each of the past 5 years.

We define droughts as the number of months in a year where soil moisture is below the 10th percentile (Gosling et al. 2017; Mester et al. 2021) in a given month in a given grid cell. The percentiles are calculated using historical data from each grid cell. This means that results can be compared across space, because what in one place would be a lot of soil moisture would be very little in another. As with floods, we aggregate from the grid cell to administrative unit polygon by averaging the drought exposure by population and populated area. So, for each of the past 5 years we know the average drought exposure of each person, and each populated location, in an administrative unit. Section 5.4 titled "Extreme Weather Events Data Maps" shows that drought exposure has a shifting spatial pattern across time. This is because the percentile metrics our drought indicator is based on compare each place to it's own long-term average.

We separate tropical storms by intensity, recording areas that experienced 34, 64, and 96 knot tropical storms (Geiger et al. 2018). Again, we aggregate from the grid cell to administrative unit polygon by averaging the exposure to each type of storm exposure by population and populated area. So, for each of the past 5 years we know what fraction of the population, and populated territory, was exposed to different intensities of tropical storms. Figures 52–57 in Section 5.4 titled "Extreme Weather Events Data Maps" shows that tropical cyclone exposure was primarily concentrated on Madagascar and Africa's southeast coast.

One key narrative about how climate could impact conflict is through its impacts on agricultural production (Dube and Vargas, 2013; Von Uexkull et al. 2016). To capture the climate related portion of this, we use a crop model to determine where and when there are climate-related shocks to crop production (Arneth et al. 2017; Bondeau et al. 2007). We define crop production shocks as the number of times in the past 5 years that crop yields deviate positively or negatively by more than 20 percent compared to long term averages. Positive and negative deviations are counted separately.

Disaster induced displacement is the number of internally displaced people due to "natural" disasters within a country (Desai et al. 2018). These are reported at a national level. Unlike with conflict related disasters, only the number of newly displaced people due to "natural" disasters are reported. While most of the disasters included in this data set are weather and climate related, it does include people displaced by extreme events, such as earthquakes, that are not extreme weather events.

One advantage of using the GRF is that colinearity within predictor groups is not an issue. If, for example, our drought lag indicators heavily correlate with each other, it should not significantly affect the model performance because we are only interested in the total amount of information contained in each predictor group. Asking about the combined information of a number of extreme weather impacts is what allows us to have so many predictors in each group. Furthermore, we do not have to assume that there is no correlation between the groups. For example, if extreme weather impacts and governance indicators are correlated, this does not challenge the validity of our results.

Methods

Our methodology operates on the logic that if predictions made with the information sets A and B combined perform better than predictions made with information set A alone, then B adds predictive information to A. As an example, if predictions made using both socioeconomic indicators and extreme weather impacts predict future conflict incidence better than predictions made with socioeconomic indicators alone, we would say that extreme weather impacts contain additional information about future conflict compared to socioeconomic indicators.

Generalized Random Forest (GRF). While there has been a large scientific debate about the role of climate, climate stress, and climate change in shifts in conflict dynamics (Mach et al. 2019), nearly everyone agrees that the relationship between climate and conflict is extremely complex (Beaumont and Coning, 2022). To address this, we use a non-parametric machine learning model to generate our forecasts because it is best suited to capture the many conditionalities in the relationship between extreme weather impacts and conflict (Perry, 2013).

We made all of our predictions using the GRF, which is a random forest model specially built to avoid inflating the predictive performance of the algorithm due to overfitting. A random forest is a supervised machine learning model constructed by aggregating the results from a large number (in our case 2000) randomized decision trees. For each decision tree, the data is continuously split into two groups based on a predictor and cutoff. The predictor and cutoff are selected to best sort the two groups based on the outcome of interest. For example, the data might be split into places with a GDP per capita (predictor) above and below $4321 (cutoff) because this is the split that best separates places in conflicts from places at peace. The process is then repeated on each of the two newly formed groups separately, and this is repeated until the resulting groups are sufficiently small. This process generates a set of rules (predictor and cutoff values) by which predictions can be made. If a place, upon following each of the rules in a tree generated, ends up at a final node where 58% of the places are in conflict, then the tree returns a prediction that there is a 58% chance that the place will be in conflict. One issue with decision trees is that they can be highly unstable (i.e., if one rule changes, the whole tree changes). To address this, many decision trees are made from randomly drawn subsets of a large data set and their predictions are averaged. The resulting algorithm is the random forest (Athey et al. 2019; Biau and Scornet, 2016).

The main advantages of the GRF for our purposes are "honesty" and cross-validation. "Honesty" means part of the training data is used to determine which variable is the most important cleavage and another part of the training data is used to determine which threshold of that variable should be used to split the data into two groups. This is designed to reduce overfitting, which improves the predictive power on the validation set. Cross validation means that each of the trees in the forest is built on one part of the data and generates predictions for the other part. Essentially this means that each data point serves both as a training and validation set, which allows us to use all of the data in training the algorithm without biasing our estimates of the out-of-sample predictive power (Athey et al. 2019).

Evaluating predictions. To evaluate our continuous predictions from the GRF models, and compare between predictors, to compare our continuous predictors to our binary outcomes (conflict/no conflict). We employ two different metrics to do this; the area under the Receiver Operator Characteristic (ROC) curve and the area under the Precision Recall (PR) curve, which are two of the most common ways to evaluate conflict prediction in early warning models (Rød et al. 2023). Both metrics require converting our continuous predictions to binary outcomes using a threshold above which we predict conflict and below which we do not. This implies a trade off between the true positives rate (predicted conflict, conflict occurred) and false positives rate (predicted conflict, peace occurred). A higher threshold will mean a lower false rate (i.e., fewer "false alarms" where predicted conflict does not occur), but also a lower true positive rate (i.e., less of the conflicts that do occur will be predicted). A lower threshold will have the opposite effect. This trade off can be graphed and that graph is called the ROC curve. The area under that curve is one of our metrics.

The PR curve works the exact same way, except that this curve plots the precision versus recall instead of the true positive versus false positive rate. The precision is defined as the fraction of projected conflicts that actually occur and the recall is the fraction of the conflicts that occur that were predicted. A higher threshold will increase the precision by reducing the number of false positives (i.e., "false alarms"), but reduce the recall because there will be more false negatives (i.e., more conflicts that do occur will go unpredicted). As with the ROC curve, this trade off can be visualized and that visualization is called a PR curve. The area under the PR curve is the second of our metrics.

While they are constructed in very similar ways, the ROC and PR curves have a few different properties. While an ROC curve treats all predictions the same, a PR curve responds much more to correctly predicting conflict (Branco et al. 2017). At the most granular administrative level, only about 5% of places have conflict which is pretty low. Combined with the relative ease of identifying locations with a very low risk of conflict, this makes PR curves more useful for these cases because they privilege getting correctly predicted conflicts over correctly predicted peace (Davis and Goadrich, 2006).

ROC curves, by contrast, are easier to compare across outcomes. If you randomly make predictions, the area under the ROC curve will, on average, be 0.5. This is true no matter what the percentage of conflict in the data set. PR curves by contrast have a "guessing rate" equal to the percentage of conflicts in the data set. Thus, it is easier to compare ROC curve scores between different outcomes (i.e., ACLED groups and UCDP) and specifications that have different fractions of place-years with conflicts (See the "Conflict Fractions" section of the Supplementary Information for the fraction of places in conflict for each conflict outcome and administrative level).

Results

Our analysis relies on comparing the performance of predictive models accounting for conventional conflict indicators (i.e., governance, socio-economic, and history of conflict) to models that in addition account for extreme weather impacts in conflict forecasts. Our dataset covers mainland Africa between 1994 and 2012 for UCDP and between 2002 and 2012 for ACLED. We conduct the same analysis separately at the national and first two subnational levels to account for different types for conflict patterns. We forecast conflict incidence one year ahead (i.e., will there be a conflict in this administrative area next year?), which enables us to see whether extreme weather impacts improve upon forecasts using the conventional conflict predictors.[4]

Key findings. We set out to determine whether extreme weather impacts improve conflict forecasts made from models with other known conflict predictors, applying GRF. Figure 1 addresses this directly by comparing the performance of forecasts made using all four variable groupings to those made made leaving out each grouping, across all three administrative levels. Both the ROC and PR curves show that neither extreme weather impacts nor socioeconomic indicators add information to forecasts made using all of the other variables, and thus extreme weather impacts do not improve upon known conflict forecasts.[5] A deeper dive into why this is, reveals that extreme weather impacts add no information to socioeconomic indicators and almost no information to governance variables or conflict history, which is driving the result. This can be seen in Section 2 of the Supplementary Information titled "Full Predictive Results," where, extreme weather impacts do not add information to socioeconomic indicators regardless of the specific conflict outcome, administrative level of analysis, or performance metric.

This result surprised us, because extreme weather impacts do predict conflict by themselves fairly well. Figure 2 compares the performances of forecasts of each of our four predictor groups by themselves. It shows that, while extreme weather impacts are not such a strong a predictor of future conflict as the other predictor groupings, they perform much better than if the model were merely guessing.

Our findings on the performances of the three commonly known predictors of conflict are in line with previous literature, which shows that places in conflict tend to stay in conflict (Hegre et al. 2016; McGuirk and Burke, 2020; Mueller and Rauh, 2022; Perry, 2013). We find previous conflict not only to be a strong predictor of future conflict on its own, but Fig. 1 shows that it contains the most unique information about future conflicts. Furthermore, governance indicators predicted future conflict well on their own, and improved upon forecasts made using the other indicators. Much of the economic literature on conflict focuses on socio-economic factors driving conflict (Dube and Vargas, 2013; McGuirk and Burke, 2020). Our results concur with this trend by showing that on their own, socioeconomic indicators are about as good as previous conflict at predicting future conflict incidence. However, we find that socioeconomic indicators add no unique information to forecasts made using other groups of predictors.

We also consider the possibility that extreme weather impacts might only add important information to predicting conflict onset or conflict ending. To check this, we split our dataset into two groups; places with a conflict in the past 5 years and places with no conflict in the past 5 years. We then evaluated whether our results were the same for each group, and found they were. While the overall model performance differs between places with a history of peace and those with a history of conflict, our central results hold. In both cases, extreme weather impacts added no predictive power to models made using conflict history, governance, and socioeconomic predictors.

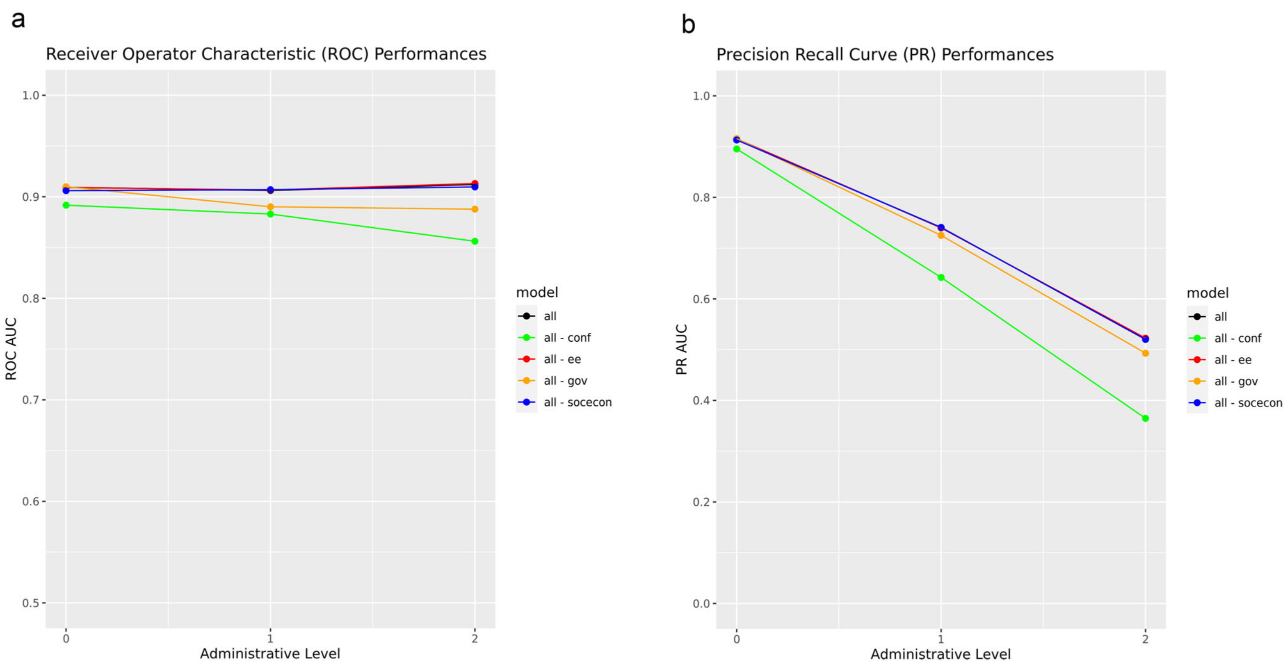

Fig. 1 Area under ROC (left) and PR (right) curves from forecasts of conflict incidence in Africa at different administrative levels: unique predictive power of indicator groups. The figures show model performances—ROC and PR curve results—of conflict predictions made using a Generalized Random Forest (GRF) algorithm with different indicator groups at different administrative levels from the most aggregate (level 0 is the national level) to the most granular (level 2). The figures highlight the effect of removing each of the four indicator groups—extreme weather events (ewi-red), conflict history (conf-green), socioeconomic indicators (socecon-blue), and governance (gov-gold)—from a predictive model with all of the indicator groups. The difference between a model with all indicator groups and a model with all except one captures the unique information found in that indicator group. Battles from the Uppsala Conflict Data Program (UCDP) are used as an outcome variable.

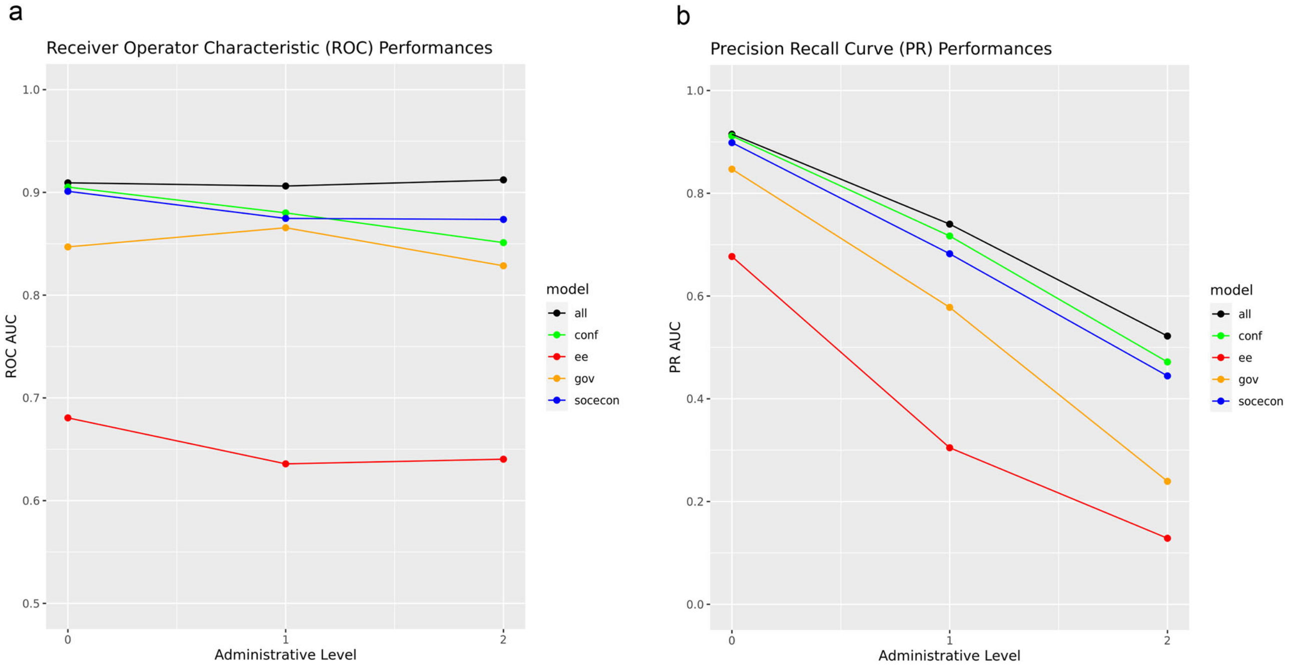

Fig. 2 Area under ROC (left) and PR (right) curves from forecasts of conflict incidence in Africa at different administrative levels: individual predictive power of indicator groups. These figures show the individual model performances—ROC and PR curve results—of conflict incidence predictions made using a Generalized Random Forest (GRF) of four indicator groups: conflict history (conf-green), governance indicators (gov-gold), extreme weather events (ewi-red), and socioeconomic indicators (socecon-blue). The predictions are conducted at different administrative levels from the most aggregate (level 0 is the national level) to the most granular (level 2). Battles from the Uppsala Conflict Data Program (UCDP) are used as an outcome variable.

Table 1 R^2 from regressing leading socioeconomic predictors of conflict on extreme weather event.

Variable	EWI $- R^2$	EWIPI $- R^2$
Area	0.02	NA
IMR	0.15	NA
GDPPC	0.15	0.03
$\text{Pop}_{\text{growth}}$	0.02	0.01
Maize	0.05	0.01
Land use	0.04	NA
Cropland	0.06	0.01
Rainfed	0.06	0.01
$\text{GDPPC}_{\text{growth}}$	0.04	0.05
$\text{Pastures}_{\text{growth}}$	0.02	0.02
Irrigated	0.07	0.01
$\text{Cropland}_{\text{growth}}$	0.05	0.04
Wheat	0.05	0.01
$\text{Rainfed}_{\text{growth}}$	0.03	0.04
$\text{Ethnicity}^{\text{count}}$	0.04	NA

The variable column shows the most important predictors of conflict at the most granular subnational level in descending order. The extreme weather impacts (EWI) column shows the R^2 value from regressing each indicator on the extreme events, which tells us the percent of the variance that can be explained by extreme events. The place-specific fixed effects (EWIPI) column shows the percent of the remaining variation can be explained by extreme events after controlling for place specific means. Ethnicity count, area, infant mortality rates, and land use have missing values because they are static indicators.

Potential correlations. Combining the results from Figs. 1 and 2, we see that extreme weather impacts do have forecasting information, but that information is somehow also contained in the other indicators groups. This raises the question: are these groupings correlated? To check this, we regressed each variable on each of the other variable groups and reported the R^2. We then repeated the exercise three times, controlling for place specific means, time trends, and both. Table 1 shows the R^2 from regressing the most important socioeconomic predictors of conflict onto extreme events, both before and after controlling for place specific means. This results shows are from the second administrative level. The full results for all predictor groups, levels, and specifications can be found in the Supplemental Information section titled "Correlations Between Predictor Groups."

Table 1 reveals two things. First, extreme weather impacts do not have a strong predictive relationship with the most important predictors of conflict, i.e., socioeconomic indicators. Second, what predictive power there is goes away after controlling for place-specific fixed effects, which indicates that extreme weather impacts partially co-occur with specific socioeconomic spatial, time-invariant patterns. Extreme events tell us very little about changes in socioeconomic indicators over time. Controlling for place specific fixed effects, extreme weather impacts explain almost none of the variation in socioeconomic indicators. This is important because many theories about how climate shocks impact conflict involve climate related changes in socioeconomic conditions (for example, Dube and Vargas, 2013).

We follow this by asking how much do our predictions vary across time and space? In the Supplementary Information Section 4.2 titled "Spatial versus Temporal Variation," we report the fraction of variation in our models' prediction of future conflict risks can be explained by year and place fixed effects alone. We find that almost none of the variation in predicted conflict risk can be explained by annual fixed effects. By contrast, in all cases more than half of the variation in predicted conflict risk can be explained entirely by it's spatial components, and in some cases as much as 90% can be explained by spatial patterns alone. This is particularly true for socioeconomic variable where, in almost all cases more than 90% of the information used by the model for prediction was spatial. Even in the case of extreme weather events, which do vary quite a bit over time, most of the important information used by the model for forecasting came from it's spatial variation. In other words, where extreme weather events tend to occur tells us more about conflict than when they occur.

We can thus explain some of how the predictive power of extreme weather impacts gets entirely subsumed by the other predictors. Extreme weather impacts tend to spatially overlap with other socioeconomic variables. In fact, all of the variation in different socioeconomic indicators explained by extreme weather impacts was spatial variation (as opposed to temporal variation). It appears that places with higher exposure to extreme weather impacts are socioeconomically different from those with lower rates. While this could be pure coincidence, we suspect it to be the result of many causes, both known and unknown. For example, places with higher exposure to extreme weather impacts in the present, might be closer or further from important bodies of water, have different colonial histories, or simply be spatially clustered in ways that overlap with socioeconomic clusters. In another case, tropical storms and tropical cyclones are clustered on the island of Madagascar and parts of southeastern mainland Africa. For many reasons, these places are socioeconomically different than, say, central Mali. This does not mean that all of the information contained in extreme weather impact patterns is actually just socioeconomic. Political ecologists have argued for decades that social and ecological phenomenon co-evolve (Benjaminsen and Svarstad, 2021; Robbins, 2019). Instead, we can say that socioeconomic predictors and extreme weather impacts contain some of the same spatial information about future conflict. We see this as a powerful confirmation of the importance of using non-parametric methods, where possible, to explicitly test the additional predictive power of proposed predictors.

Discussion

Employing cutting edge climate impact modeling, we explicitly test whether extreme weather impacts add information to conflict prediction models made with many of the underlying conditions known to be important for conflict. To conduct this test we first assemble a data set with a large number of socioeconomic, governance, and conflict history predictors commonly used for conflict prediction. We then combine this with data from state-of-the-art climate impact models that capture floods, droughts, tropical cyclones, and crop production shocks. We also include data on disaster induced displacement. We then generate our forecasts using a GRF algorithm that is specifically designed to capture non-linear, conditional relationships between the underlying variables and give unbiased estimates of their prediction power. We use such non-parametric methods in response to the body of work on climate and conflict arguing that climate-related conflict impacts must be conditional upon local circumstances (Buhaug et al. 2021). We find that the information on extreme weather impacts from our climate models do not add predictive power to our comprehensive set of known predictors.

Our results concur with some previous literature (Bazzi et al. 2022; Linke et al. 2022; Perry, 2013) that find no predictive power for climatic variables, and differ from findings by Schleussner et al. (2016), who show that "natural" disasters tend to co-occur with conflict outbreaks in ethnically fractionalized countries. There are several differences between the studies that could explain the diverging findings. Schleussner et al. (2016) and other conflict onset research, tend to work on much shorter time spans (monthly instead of annually) and by definition are explaining a smaller set of cases (new conflicts) as compared to our study. While we do test our results to ensure they are similar when only places with a history of peace or conflict are considered (see section "Key findings"), we emphasize that it is quite possible for extreme weather impacts to help predict conflict onset in a specific number of vulnerable cases without increasing overall predictive power. For example, an extreme event might simply cause a shift in when a conflict breaks out, but not if it occurs at all (e.g., a conflict that would have occurred anyways might break out a month earlier or later).

Furthermore, our analysis focuses on sub-national levels whereas Schleussner et al. (2016) focuses only on the country level. The existence and outbreaks of national conflicts might have a qualitatively different set of causes or dynamics than those occurring at the regional level. We address this by also reporting national level findings. One benefit of conducting the same analysis at different administrative levels is that it reduces the risk that the results are driven by aggregation bias. This is particularly true because our results are similar across administrative levels. Our national level results are consistent with the results from more granular administrative units with more observations. However, the GRF requires a lot of data to properly work. With only 11 years observations for 49 countries, this is not a lot of data for fitting a non-parametric machine learning model. For this reason, we primarily rely on our sub-national level analysis, where we have more observations. This is less of a problem for Schleussner et al. (2016) who use event coincidence analysis, which requires less data to work well. Finally, it is quite common in the literature to find no meaningful overall effect of climate impacts on conflict outcomes. Even Schleussner et al. (2016) and Von Uexkull et al. (2016), who focus their analyses on their positive findings, report no overall effects of climate impacts on future conflict. Our use of GRF in this paper is designed to see whether these highly conditional relationships increase our knowledge of future conflicts.

Our results are informative, but we cannot definitive say that extreme weather will never improve conflict predictions of some type. We freely acknowledge that by looking at a different region, considering a different time period (as data availability increases), using a different set of climate impact models, predicting a different type of conflict outcome, or some combination of the above might find different results (Rød et al. 2023). It is also possible that weighting the predictive model to focus more on conflict cases would improve predictive power. We leave this to future research.

Beyond our specific findings, we also set a standard for what would be required for a positive result. Extreme weather events data from a climate impact model, which we have called "extreme weather impacts," that is not related to socioeconomic data, must be shown to improve predictions made with the full suite of other known climate predictors, preferably using a non-parametric model that can account for the complexity of the relationship. This is crucial because extreme weather impacts do predict conflict on their own. Other works has also shown that under select conditions extreme weather impacts predict some future conflict outcomes (Schleussner et al. 2016; Von Uexkull et al. 2016). Some of these exercises may be useful for knowledge building. Before incorporating extreme weather impacts, or any climate indicators, into forecasting or prediction models, it should be explicitly tested (as we have here) if they add predictive information to currently know predictors.

Future research solely interested in understanding the relationship between climate and conflict might take these results as evidence that extreme weather impacts are not very important factors for understanding conflict, particularly when they do not line up with more important socioeconomic and political drivers of conflict. This is consistent with some previous works (Mach et al. 2019; Selby et al. 2017; Slettebak, 2012; von Uexkull and Buhaug, 2021). This is particularly true because socioeconomic predictors and extreme weather impacts were found to contain some of the same spatial information about conflict patterns. Our findings are also consistent with other works showing that weather and climatic events are poor predictors of migration, despite being causally linked (Schutte et al. 2021).

Future research primarily interested in forecasting conflict should interpret our results as a recommendation against integrating extreme weather impacts into forecasts on conflict incidence. Predicting where conflicts risks are high is much easier than predicting when new conflicts will break out (Mueller and Rauh, 2022). Extreme weather impacts have important spatial patterns, as do socioeconomic variables, and they capture at least some of the same spatial information about future conflict instance. Thus is true even when the variables are constructed in such a way as to ensure there is no causal relationship between them. Thus, in the case of conflict incidence forecasting, extreme weather impacts may have little to contribute.

Data availability

The Supplementary Information Section contains source links, descriptions, and maps illustrating spatial and temporal variation for each variable. No data were generated during this study and all of the analyzed data are publicly available. The code to assemble the data, the processed datasets, the code used for the analysis, and the results can all be found here. Furthermore, the corresponding author remains available to respond to reasonable requests.

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Notes

1 While the specific conclusions of Hsiang and Burke (2014), and the general conclusions of Burke et al. (2015) that followed it the next year, have been heavily criticized (Buhaug et al. 2014), the studies considered within the meta-analysis still reflect the types of variables considered in the literature.
2 “Other events” is an ACLED category.
3 For further information about the flood simulations, see Mester et al. (2021).
4 A full description of our conflict variables can be found in the “Data” section or the Supplementary Information section on “Data Sources.”
5 The fact that the shown predictive power of “all” is less than that of “all - ewi” indicates that the extreme events did not contain any additional information about future conflict and caused overfitting.

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