Rainfall Variability and Violent, State-Based Conflict

A Machine Learning Approach to Estimate Context Specificity

Mathilde Bålsrud Mjelva

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Abstract

This thesis investigates the conditions under which rainfall variability affects violent, state-based conflict. More than a decade of research on the climate-conflict nexus has produced diverse results, which could imply that the link is context specific. Yet, the literature on the nexus has focused excessively on finding one effect that applies to all contexts, and has blamed the divergent results on differences in research design, even though the effect is likely to vary. To address this research gap, this thesis studies how the effect of rainfall variability on violent, state-based conflict varies depending on different factors of vulnerability. The effect of rainfall on conflict is hypothesized to be larger the more vulnerable a community is to rainfall shocks and conflict. The thesis uses a new machine learning method for causal inference called causal forest, and applies it on global data from 1989-2018. As such, the thesis offers a new methodological approach to studying the climate-conflict nexus. Excess and scarce rainfall are operationalized through the Standardized Precipitation and Evapotranspiration Index (SPEI) and violent, state-based conflict is derived from the Uppsala Conflict Data Program (UCDP). The results show that excess and scarce rainfall have small effects on conflict in all contexts, but that some types of vulnerability, such as having large parts of a population employed within the agricultural sector, makes that effect larger. The results of the thesis emphasize the need for more research that studies the context specificity of the climate-conflict link. Estimates of an average effect of climate variability on conflict is of little interest if the true effect differs from context to context.

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Responsibility for mistakes and inaccuracies remain entirely mine.

Mathilde Bålsrud Mjelva
Oslo, June 21, 2020

R-scripts from the thesis can be found at `https://github.com/mbmjelva/MA-thesis`.

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List of Abbreviations

ATE Average treatment effect

CATE Conditional average treatment effect

CI Confidence intervals

GDP Gross domestic production per capita

GRF Generalized random forest

HTE Heterogeneous treatment effect

IPCC Intergovernmental Panel on Climate Change

ITE Individual treatment effect

ML Machine learning

OLS Ordinary Least Squares

PET Potential Evapotranspiration

UCDP Uppsala Conflict Data Program

SHDI Subnational Human Development Index

SPEI Standardized Precipitation and Evapotranspiration Index

SPI Standardized Precipitation Index

Chapter 1

Introduction

In 2015, a severe drought hit communities in the North-Eastern part of Ethiopia, leaving millions in need for emergency relief assistance (Climate & Development Knowledge Network, 2017; Reliefweb, n.d.). Few months later, massive uprisings erupted in the same region of the country, leading to violent clashes between government forces and protesters (Abbink, 2016; Busby, 2018, p. 342; Human Rights Watch, n.d.). Did the drought lead to the massive uprisings? Between 2006 and 2011, Syria experienced an acute drought that affected agricultural production and led many Syrians to migrate (Gleick, 2014, p. 332). Did the drought affect the uprisings in 2011 and consequently the Syrian war? These conflicts are sometimes projected as caused or influenced by the climatie event that preceded them (see for instance Murphy, 2017). Indeed, climate change has long been recognized as a "threat multiplier" in the policy field, thus as a force that could intensify the risk of conflict (Abrahams, 2020; Busby, 2018). But did climate change cause the aforementioned conflicts? Would droughts of the same magnitude lead to uprisings elsewhere in the world? If not, what conditions were present for such climatic events to affect conflict outcomes in these cases, if that was truly what happened?

This thesis will not answer the question of whether climate variability affected these conflicts specifically. Rather, the thesis will focus on the conditions under which climate variability could influence conflict more generally. Despite the widespread political recognition of climate change as an issue of security, results from the literature on the climate-conflict nexus are disparate, and researchers have found both positive and negative, but also nonexistent effects of climate change and variability on conflict. These disparate findings are often framed as inconclusiveness, implying that studies of the climate-conflict nexus should yield similar results even when focusing on different contexts and cases. However, the effect of climate change and variability on conflict is likely to be context specific, which could explain why the results in the literature are so divergent. Little attention has been devoted to whether and how the climate-conflict nexus could vary depending on the societal conditions of the communities hit by a climate shock. To address this research gap, this thesis investigates whether the effect of climate variability on conflict varies depending on societal factors. The thesis concentrates on identifying context specificity of the effect, because it is essential to understand *how* the effect varies before explaining *why* it differs. Due to considerations of data availability and the scope of the thesis, the focus will be on the effect of rainfall variability on violent, state-based conflict, instead of investigating the effect of all types of climatic events on all types of conflicts. Specifically, the research question of the thesis is: *Under what conditions, if any, does rainfall variability affect violent, state-based conflict?*

I argue that there are two ways that rainfall variability could indirectly influence violent, state-based conflict. First, rainfall shocks could lead to increased grievances in a population, which could cause uprisings against the government. Second, extreme rainfall events could increase rebel recruitment by shifting the opportunity costs of civilians to join rebel groups. This could, in turn, increase the probability of violent, state-based conflict. Violent conflict is, however, unlikely to be an inevitable consequence of rainfall variability. For these mechanisms to exist, some contextual factors must be in place. Since the literature has devoted little attention to studying the context specificity of the effect, evidence is scarce concerning what factors could impact the climate-conflict nexus. Yet, based on limited evidence from the literature, I present seven vulnerability factors that could affect the probability of rainfall-induced violent conflicts. I also make three hypotheses concerning the nature of the effect heterogeneity. As such, the thesis takes a semi-inductive approach to answering the research question. It tests how the effect of rainfall on conflict varies across a relatively large number of variables that are, based on scarce theory, plausible to have an impact on the effect. The results from the analysis contribute to theory building on the types of vulnerability that impact the risk of rainfall-induced conflicts.

I use global data on rainfall variability and conflict, and include a range of contextual variables, derived from nine different sources, that operationalize the vulnerability factors. The data is on grid cell-year level from 1989 to 2018. Rainfall variability is operationalized through the Standardized Precipitation and Evapotranspiration Index (SPEI), and violent, state-based conflict is derived from the Uppsala Conflict Data Program (UCDP). To study how the contextual variables might affect the rainfall-conflict link, I use a relatively new methodology called the causal forest, which is a non-parametric machine learning model for causal inference. The causal forest relies less on theoretical assumptions and can handle more complex data structures than other methods for causal inference. These features makes it ideal when investigating heterogeneous treatment effects based on scarce theoretical knowledge of the nature of the heterogeneity, which allows for an inductive approach with many variables.

The results from the analysis show that the effect of rainfall variability on violent, state-based conflict is small in all types of contexts, but that some of the vulnerability factors affect the treatment effect. The first hypothesis, which states that the effect of excess and scarce rainfall on violent, state-based conflict is dependent on the vulnerability of the community hit by the rainfall shock, is confirmed. The second and third hypotheses state that the effect of rainfall variability on violent, state-based conflict will be larger the more vulnerable a community is to rainfall shocks and conflict. These hypotheses are only partially confirmed by the analysis. The results also show that the elements that affected the effect of scarce rainfall on conflict are not the same as those that affect the link between excess rainfall and conflict. For most of the contexts studied, the effect of rainfall variability is larger the more vulnerable a community is to anomalous rainfall and conflict.

There are two main motivations behind the research question. On a general level, the thesis contributes to the knowledge of how climate change affects conflict. In order to draw useful and adequate policies to promote peacebuilding, policymakers need knowledge about the context specificity of the nexus. If climate change affects conflict outcomes, or if it does not, then that also adds to the broader understanding of the impacts of climate change on society. As the effects of climate change become more apparent, understanding these mechanisms is important in order for the international society to adequately adapt.

The second motivation has a more scientific root. The thesis contributes to build theory on the impacts of climate change, on the causes of conflict, and on how climate change could affect conflict. Such theory is needed, both to better inform policymakers, but also inherently to evolve the scientific understanding of the connection between climate and conflict. Because there is still much uncertainty over how the climate-conflict nexus works, such theory-building is needed in order to direct scientific resources (i.e. people, time and funding designated for the research topic) onto the right path.

Although not a main motivation behind the thesis, a third motivation is that by applying the causal forest, the thesis is one of the first to use a causal machine learning technique in trying to study the climate-conflict link. In fact, the causal forest is such a new development that few scientists have used it at all, in any scientific field. Thus, although the novelty of the method implies caution when using it, the application in this thesis can help statisticians in general, and the research community on the climate-conflict nexus more specifically, to understand how the methodology could be used for causal inference. The causal forest, and its application here, could open the door to a whole new approach to answering research questions about the link between climate change and conflict.

1.1 Key Concepts and Limitations of the Thesis

The terminology of the thesis is intricate and can be somewhat confusing. This section will hopefully help navigate through the terminology jungle by defining some of the key concepts that will be used throughout the thesis. Section 1.1.1 defines climate change, climate variability and rainfall variability. Section 1.1.2 defines violent, state-based conflict, while Section 1.1.3 gives a brief explanation of what heterogeneous treatment effects are. While defining these concepts, I will also describe the limitations of the thesis.

1.1.1 Climate Change and Rainfall Variability

Climate change can be defined as the long-term changes in the average weather patterns of the earth's climates (NASA, n.d.). It refers to statistically significant changes in the state of the climate that persist for a decade or longer (IPCC, 2012, p. 557).[1] These definitions encompass very diverse and intertwined climatic changes. For example, climate change is likely to lead to warmer summers some places and longer winters in other areas, it makes the ice melt at the poles, ecosystems shift and will increase the frequency and intensity of extreme weather events such as floods, cyclones and droughts.

It is difficult to study the concrete effects of climate change on society, because climate change is a long-term process. Indeed, measuring whether a rapid event such as a conflict could have been caused by the long-term, slow and multifaceted process of climate change is challenging, as it is difficult to separate the effects of climate change on society from other events or processes. Climate change might be an underlying cause of some conflicts, but it is difficult to statistically quantify the magnitude of that cause. Hence, instead of studying the impact of climate change more generally, most of the research on the link between climate and conflict have focused on climate variability. According to the IPCC, climate variability can be defined as "variations in the mean state and other statistics (such as standard deviations, the occurrence

[1] The usage of the term "climate change" can vary, as it sometimes refers to climate change caused by human activities and natural variabilities, while it in other cases only refer to the changes influenced by human activities (United Nations Framework Convention on Climate Change, 2011). As the purpose of this analysis is to investigate the *consequences* of climate change and not what causes it, the term when used here covers both climate change caused by human activities and natural variabilities.

of extremes, etc.) of the climate on all spatial and temporal scales beyond that of individual weather events" (IPCC, 2013, p. 1451).[2] Climate variability could thus refer to events such as extreme temperatures, cyclones, floods, droughts or other weather events that differ from what is considered normal (Koubi, 2017, p. 200). It is easier to study the impacts of these events than of climate change more broadly, as the events are more rapid and concrete than climate change. Since climate change is likely to lead to increased frequency of weather events such as droughts and floods, studying the effects of climate variability on society can also give insights into how one of the consequences of climate change might affect violence.

Rainfall variability is here viewed as a subcategory of climate variability. It refers to variations in precipitation that deviate from normal conditions. Although excess or scarce rainfall could often take the form of droughts and floods, what a deviation from normal looks like depends on the typical climate of the area that experience the variability. Less than normal precipitation in the Amazon might not lead to a drought, whilst a drought could erupt from an equal deviation from normal in a dryer climate. Moreover, excess rainfall could be desperately needed for crops to grow in a dry climate, but lead to floods in wetter areas. Hence, "rainfall variability", when used in this thesis, means *deviations from normal precipitation patterns*, and not necessarily droughts or floods.

Although I sometimes refer to the more general literature on or consequences of climate change and climate variability in this thesis, the focus of the analysis is on the effects of rainfall variability. The reason for that is twofold. First, the data available on other types of climate variability could not be used as unconfounded treatment variables. As will be further explained in Chapter 5, a prerequisite for using SPEI as the treatment is that it can be treated as if the variable was randomly assigned. Because the SPEI index is composed of two independent distributions, namely precipitation and temperature, SPEI can be treated as-if randomly assigned, in accordance with the Central Limit Theorem. To my knowledge, no similar indexes or other types of measurements exist for other types of climate variability.

The second reason why the focus of the thesis is limited to studying rainfall variability is that different types of climate variability could have very different effects on society. If cyclones, earthquakes, thunderstorms, floods and droughts all impact the probability of conflict, they are likely to do that through very different causal mechanisms. Even if the causal mechanisms are similar, I cannot just assume such similarities, as the literature gives few, if any, proof of the mechanisms being similar.

[2]Climate anomalies, climate shocks, climate hazards, climate extremes and climate events are sometimes used as synonyms to climate variability. Although these terms can have slightly different connotations, and although they might be used differently in different contexts, the expressions will be treated as synonyms to climate variability in this thesis, for simplicity.

The research that exist on the connection between the various climate extremes and conflict show divergent results, as will be discussed in the literature review in Chapter 2. Hence, until the literature is more certain about the similarities and differences of the impacts of different types of climate hazards, it is best to treat the events and their impacts as separate. Therefore, I will limit the analysis to study rainfall variability, and with that analyse the effects of scarce and excess precipitation separately. The results found in this analysis cannot be generalized to hold for all types of climate variability, but gives a picture of how one type of climate variability, and thus how one consequence of climate change, could affect violent, state-based conflict.

Why focus on both excess and scarce precipitation in the same thesis, if I treat them as separate types of events? One reason is that I want to take advantage of the data available. As will be clarified in Chapter 5, the same index that is widely used to measure scarce precipitation could also be used to measure excess precipitation. The same data and methodology could thus be easily transferred to investigate the effects of one type of rainfall variability to another. Moreover, as will be discussed in Chapter 3, both excess and scarce precipitation seem to affect state-based conflict through the same mechanisms, and the effects of both types of anomalous rainfall are plausibly modified by the same factors of vulnerability. Hence, both types of rainfall variability are assumed to have similar *societal* consequences, even if their climatic consequences are unequal. It therefore makes sense to study both excess and scarce precipitation in the same thesis. If the results show that the societal consequences of rainfall shocks are not the same for the two types of anomalous precipitation, then that could also be of scientific interest.

1.1.2 Violent, State-Based Conflict

This thesis uses the Uppsala Conflict Data Program (UCDP)'s definition of state-based, armed conflict as its definition of violent, state-based conflict. UCDP define state-based, armed conflict as “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 a calendar year" (Högbladh, 2019, p. 28). In this definition, opposing parties to the government must be other states or any non-governmental organized group who have an announced name and use violence to solve the incompatibility it has with the state (Högbladh, 2019, pp. 28-29). That means that lone actors that use violence for a political cause are not included in the definition of violent, state-based conflict.

Similar to how different types of climate hazards could have different consequences, different types of conflicts have different causes. Therefore, the pathway from climate variability to violent, state-based conflict would plausibly be different than the pathway to non-state conflict or non-violent conflict, if such pathways exist. Trying to estimate the causes of all these types of conflicts simultaneously (thus having merely conflict as the outcome variable) could yield nonsensical results if the causes of the conflicts are very different. Therefore, this thesis is limited to studying the effect of rainfall variability on one type of conflict, namely those that are violent and where a governmental actor is involved.

Based on the definition above, violent, state-based conflict could mean both interstate and intrastate conflicts. However, few conflicts between states have occurred since the cold war (Dupuy & Rustad, 2018). Although it might cause disputes between states, climate change is unlikely to cause interstate violent conflict. As intrastate and interstate conflicts are quite different and could have very different causes, the theoretical framework of the thesis will only consider conflicts where at least one of the parties to the conflict is a non-governmental actor.

Some researchers argue that communal conflicts, which are non-state conflicts between informally organized groups (Vestby, 2018, p. 80), are the types of conflict most likely to erupt from climate extremes (see for example Fjelde & von Uexkull, 2012; Hendrix & Salehyan, 2012). This is because attacking other societal groups, rather than the government, will be more efficient if the purpose of the violence is to secure access to livelihood essentials (Fjelde & von Uexkull, 2012, p. 446). However, as will be discussed in Chapter 2, the current literature has focused disproportionately on Sub-Saharan Africa, where communal conflicts seem to be more relevant than in other regions (Nordqvist & Krampe, 2018, p. 2). Hence, when studying the effects of climate variability on global data, it could be useful to investigate how state-based rather than communal conflict is affected, as that seem more relevant on a global scale.

Climate variability could also lead to peacebuilding and cooperation rather than conflict (Buhaug, 2015, p. 272; Raleigh, Linke, & O'Loughlin, 2014, p. 77). Asking whether rainfall variability leads to violent conflict does not exclude the possibility of results that point to peace as the outcome of climate shocks. Although the focus in the thesis is on the mechanisms that could lead to a more violent world post-climate extremes, the results could show that climate variability in some contexts decrease the probability of violent, state-based conflict. If so, future research should investigate how and why such mechanisms exist, but that will not be the primary focus here.

Unless otherwise specified, the terms "conflict", "violent conflict", and "state-based conflict" will be used interchangeably with "violent, state-based conflict" for the remainder of the thesis.

1.1.3 Heterogeneous Treatment Effects

One term that will be repeatedly used in the thesis is heterogeneous treatment effect (HTE). For clarification, the concept will be defined briefly below. A more thorough definition of how HTE can be estimated is given in the methodological framework in Chapter 4.

As mentioned, the effect of rainfall variability on violent, state-based conflict is likely to depend on contextual variables. Rainfall variability is assumed to have bigger effects some places than others, and might even lead to *less* conflict in some communities. In other words, the treatment effect of rainfall on conflict is likely to be *heterogeneous*, meaning that it is expected to vary from context to context.[3] On the contrary, had the effect of rainfall on conflict been homogeneous, then a rainfall event would have the same effect (i.e. the same impact) on violent, state-based conflict no matter which area was hit by the event. A homogeneous effect is sometimes described as an average treatment effect (ATE), but the terms are not entirely interchangeable. An ATE is the estimated effect you would get by averaging the individual or group-level effects of a population. If the treatment effect is homogeneous, then the ATE would be a good estimate of the true effect. However, if an effect is heterogeneous, it is still possible to mathematically calculate the ATE, but it would only be the mean effect across the population and might not convey a meaningful picture of reality.

Yet, the most common procedure in statistical analyses is to estimate an ATE and interpret that as a homogeneous effect that is of the same size and direction in all situations. Tests of treatment effect heterogeneity are usually not applied, which limits the usefulness of such results. By focusing exclusively on estimating ATEs rather than checking if effects vary, researchers could present results that exaggerate what the effect looks like in some contexts and underestimate it in others, which will have consequences for policymakers basing decisions on those results. Therefore, as the effect of rainfall variability on conflict is believed to vary across contexts, the thesis will focus on investigating the heterogeneity of the effect, rather than studying the ATE. If heterogeneity in the treatment effect is found, then future analyses should incorporate that knowledge in their studies of the rainfall-conflict link. If heterogeneity is not found, then the ATE might be a good estimate after all.

[3] The expression "treatment effect" is used mostly in the terminology of experiments or statistical analysis, while simply "effect" is usually used in other parts of Academia. The expressions will be used interchangeably throughout the thesis.

1.2 Structure of the Thesis

The thesis is structured as follows. Chapter 2 presents a review of the current literature on the climate-conflict nexus. Two key limitations of the literature are presented, namely that it has focused excessively on estimating average treatment effects of climate on conflict while the effect is likely heterogeneous, and that the literature has focused disproportionately on cases from Sub-Saharan Africa at the expense of cases from other parts of the world. Chapter 3 introduces the theoretical framework of the thesis, and the indirect and heterogeneous pathways in which rainfall variability could plausibly affect violent conflict are explained. Three hypotheses are made based on the theoretical framework, which together represent the expectations of what the nature of the heterogeneity of the effect will be. The hypotheses lay the premise for the focus of the analysis. Chapter 4 describes the methodological framework of the thesis, including a more thorough explanation of what heterogeneous treatment effects are and how to estimate them. The chapter also describes how the causal forest works and why it makes sense to use it to answer the research question. Chapter 5 is a review of the data and operationalizations of the variables used in the analysis. It presents the operationalizations of the outcome and the treatment variable, as well as of the vulnerability factors introduced in Chapter 3. The data consists of two million observations distributed across 15 variables derived from 9 different data sources. Chapter 5 also summarizes the theoretical expectations of how the contextual variables that operationalize the vulnerability factors should vary for the hypotheses to be confirmed. Chapter 6 presents the results from the analysis. The chapter also gives a discussion of the implications, validity and reliability of the results. The thesis ends with a conclusion that emphasizes the relevance of the results and the need for future research on the topic.

Chapter 2

Literature Review

This chapter gives an overview of the literature on the connection between climate variability and conflict. The literature review concerns the wider research field on the climate-conflict nexus, rather than on the relation between rainfall variability and state-based conflict specifically, because the results from this analysis contribute to the broader literature on the climate-conflict link. I argue that there are two research gaps in the literature. The first is that the literature has focused on estimating average treatment effects of climate on conflict, while the effect is more likely to be heterogeneous, and thus context specific. A consequence and reinforcement of this narrow view is that disparate research results are interpreted as signs of inconclusiveness caused by differences in research design, which implies that a homogeneous effect should be feasible to find. Due to the focus on estimating average treatment effects, the policy field has struggled to find targeted and fruitful policies to cope with the possible contextual effects of climate variability and change on conflict. The second research gap is that the literature has focused disproportionately on cases from Sub-Saharan Africa. Although it should not stop studying those areas, research on other areas of the world that are vulnerable to climate change should be augmented.

2.1 The Implausibility of a Homogeneous Effect

This section explains the first research gap of the literature, namely that it in large part has focused on estimating average rather than heterogeneous treatment effects of climate on conflict. Section 2.1.1 explains that framing the literature as inconclusive is an indication of how the research field treats the effect as homogeneous. Section 2.1.2 looks at the consequences the narrow research has had on the policy field, which underscore the need for research on how the effect of climate on conflict vary.

2.1.1 The Pursuit of Conclusive Results

Results from the research field on the climate-conflict nexus are divergent. So far, experts agree on two points: that climate change has affected organized armed conflict in the past, and that other conflict drivers have been much more influential to conflict risk than climate-related drivers (Mach et al., 2019, p. 194). Apart from that, results from the research on the climate-conflict link are disparate, where some conclude that the effect of climate change on conflict is direct, some that the effect is indirect, some that the effect is small or big or nonexistent, and some that climate change and variability leads to *less* conflict (for an overview see Koubi, 2017 and Theisen, Gleditsch, & Buhaug, 2013, p. 617).

Indeed, even the more narrow research field on the effect of rainfall variability on violent conflict have found both significant and insignificant results.[1] In their study from 2004, Miguel et al. (2004) found that drought increased the probability of conflict in agriculturally-depended communities in Sub-Saharan Africa. However, Ciccone (2011) rebut the finding of Miguel et al. and claimed that the results were driven by research design, and saw no relation between rainfall and conflict in more recent data. Moreover, in their study of conflicts in East Africa, Raleigh and Kniveton (2012) found that dry conditions led to higher rates of rebel conflicts, while wet conditions increased the likelihood of communal conflict. Salehyan and Hendrix (2014) identified a positive relationship between excess precipitation and political violence when looking at global data, while Couttenier and Soubeyran (2013) and Detges (2016) found weak, but positive links between drought and conflict in Sub-Saharan Africa. Others found insignificant effects between climatic variability and civil conflict in Africa (see for instance Buhaug, 2010; Theisen, Holtermann, & Buhaug, 2011).

One explanation for the incompatible results in the literature could be that the effect of climate variability on conflict actually *does* vary from context to context, so that diversity in findings are indications of effect heterogeneity. Certainly, policymakers and researchers, either implicit or explicit, describe the effect of climate variability on conflict as context specific, i.e. as heterogeneous. In the policy field, climate change is often described as a *threat multiplier*, thus as a factor that can intensify the risk of conflict (Abrahams, 2020, p. 2). Following this discourse, policymakers have implemented a range of strategies to meet the presumed threat of climate change on conflict risk (see for instance The Center for Climate and Security, 2019; Ministry of Defence (MOD), 2012; United Nations Environment Programme, 2017). If climate change is a threat multiplier, then some threats must exist in the societies hit by a climate hazard prior to the event for climate change to be able to

[1] These results are of research on both state-based and non-state conflicts.

multiply those threats. The multiplication of zero is zero, so societies that are not initially at risk for conflict should neither be at risk for conflict after experiencing a climate shock, if this discourse is correct.

Recent IPCC reports declare that climate change will affect societies differently based on the societies' vulnerability to the change (e.g. "People who are socially, economically, culturally, politically, institutionally, or otherwise marginalized are especially vulnerable to climate change and also to some adaptation and mitigation responses" (IPCC, 2014, p. 6), "Although land degradation is a common risk across the globe, poor countries remain most vulnerable to its impacts" (IPCC, 2019b, p. 89), and "People with the highest exposure and vulnerability are often those with lowest capacity to respond" (IPCC, 2019a, p. 29)). If climate change and variability have various impacts on communities depending on factors such as economic, political and institutional capacity, then conflict risk arising from climate change should also be heterogeneous depending on how vulnerable societies are to climate change and variability.

Also in Academia, the climate-conflict nexus is described as heterogeneous by some. The effect of climate change on conflict is expected to be greater in more climatically, economically and/or politically vulnerable areas (Busby, Smith, Krishnan, Wight, & Vallejo-Gutierrez, 2018; Koubi, 2019, p. 348; Nordqvist & Krampe, 2018; Theisen, 2017; von Uexkull, Croicu, Fjelde, & Buhaug, 2016; Wischnath & Buhaug, 2014). Among other factors, studies suggest that climate variability could affect conflict in areas dependent on agriculture, that have politically excluded groups and ineffective institutions (Koubi, 2017, p. 201). It should not be surprising that the literature on the climate-conflict nexus finds heterogeneous results if the effect they try to estimate is heterogeneous.

Despite the apparent apprehension that the climate-conflict nexus will vary based on contextual factors, the literature has concentrated on finding an average treatment effect (ATE) of climate on conflict. Due to the diversity in results, the literature is often described as inconclusive. Literature reviews of the research have stated that the "evidence on the effect of climate change and variability on violence is contested" (IPCC, 2014, p. 758), that "scientific evidence of this relationship remains elusive" (Adams, Ide, Barnett, & Detges, 2018, p. 200), and that "extant studies provide mostly inconclusive insights, with contradictory or weak demonstrated effects of climate variability and change on armed conflict" (Theisen et al., 2013, Abstract p. 613). Yet, inconclusiveness implies that a conclusion exists. By framing the literature as ambiguous and inconclusive, researchers insinuate that the effect of climate variability on conflict is homogeneous. That is, interpreting the diverging results as signs of elusiveness creates an expectation of the effect as being uniform, as if analyses on separate cases *should* yield comparable results.

A common response to the supposed inconclusiveness has been to declare it as caused by variation in research designs. As it happens, much of the debate in the literature have been on the consequences that differing methodological choices, datasets, and scales of analyses have on results (see Adams et al., 2018; Buhaug, Nordkvelle, et al., 2014; Buhaug, 2015; Hsiang & Burke, 2014; Hsiang & Meng, 2014; Mach et al., 2019; Salehyan, 2014). Certainly, some of the variation in the aforementioned findings must be attributed to differences in research design, as researchers have conducted analyses with divergent spatial and temporal scales, various operationalizations of climate and conflict and with research questions that differ in their theoretical underpinnings (Buhaug, 2015; Mach et al., 2019, p. 193). Just as different types of conflicts presumably have distinct causes, different types of climatic variability should have varying impacts on society, so studies with varying operationalizations of climate and conflict should lead to varying results (Buhaug, 2015, p. 271; Salehyan, 2014, p. 3). Other types of research design choices will similarly affect outcomes of analyses. Nonetheless, by presenting differences in research design as the cause of the divergent results, the perception of the effect of climate variability on conflict as homogeneous is reinforced. This solution suggests that reaching a collective conclusion should be feasible, as long as the correct research approach is used. Although researchers should be careful when comparing results from analyses with contrasting research designs, focusing on methodological choices as the problem and solution to the disparate findings is a way to continue to treat the climate-conflict relationship as homogeneous.

Although it is possible to statistically identify an average effect of rainfall on conflict, that effect is of little substantial interest if it differs greatly from context to context. The exercise of estimating an average effect "is mathematically feasible but the outcome may have no relevant meaning" (Buhaug, Nordkvelle, et al., 2014, p. 395). If the focus of the research is changed from seeking consensus on the nature of a homogeneous effect to assume that the effect is heterogeneous, new opportunities arise. By actively investigating whether and how the treatment effect is heterogeneous, the research field might find that conclusions can indeed be drawn, as long as they are context specific.

2.1.2 Disjuncture Between Policy Needs and Research Focus

Another advantage of actively investigating the heterogeneity of the treatment effect is that the results will be more relevant and useful for policymakers. The policy field has had difficulties finding targeted policies that can modify the climate-conflict link (Abrahams, 2020, pp. 1-2). As scholars debate the validity of various research designs, "[p]olicymakers do not have the luxury of waiting for an academic consensus on the nature of the relationship between climate factors and security" (Busby, 2018, p. 338). While waiting for research consensus, policymakers have had to prepare for the impacts of climate change through potentially suboptimal policies founded on uncertain scientific results (Busby, 2018, p. 338). The aforementioned recognition of climate change as a threat multiplier gives little guidance as to when, how and where policies should be implemented to reduce the risk of violent conflict erupting (Busby, 2018, p. 342). As Abrahams explains,

"[...] when a particular conflict is attributed to climate change, one must understand not only the sources, magnitude, and severity of any changes and their associated impacts, but also the pathways by which those impacts affect conflict outcomes or create opportunities for peacebuilding in a particular place. Without this understanding, responding to climate-related conflicts becomes extremely difficult." (Abrahams, 2020, p. 6).

Hence, in order to draw meaningful and useful policies, practitioners need knowledge both of how climate will impact society and of how those impacts could influence conflict. Furthermore, this knowledge needs to be context specific. To make policies that will reduce the likelihood of conflict, policymakers must understand what areas are most likely to erupt into conflict spurred by climate shocks, not what the global or regional average effect of climate variability is. Hence, focusing on the heterogeneity of the effect of climate on conflict will yield more context-specific and concrete results which will be easier to translate into fruitful policies. The disjuncture between policy needs and the focus of the literature could be at least partially solved through an investigation of the heterogeneity of the treatment effect.

2.2 Narrow Empirical Base

The second weakness of the literature is that it has had a narrow empirical focus. A quick look at the current literature on the climate-conflict nexus reveals that a majority of the research has focused on Sub-Saharan Africa. Indeed, in their literature review, Adams et al. found that 65 percent of the statistical studies have had Africa as its empirical base, and 57 percent of the statistical articles focused on Sub-Saharan Africa (Adams et al., 2018, p. 202). Although there is still much uncertainty concerning the climate-conflict nexus in Africa, meaning that more research is still needed on cases from the continent, the extensive focus on Sub-Saharan Africa pose at least two limitations on the research field. First, it seems that research attention to African communities has been given at the expense of research on other areas of the world that are just as, if not more, vulnerable to climate change. In fact, Adams et al. (2018, p. 202) found a mismatch between the countries most studied in the climate-conflict literature and the ones most exposed to and at risk from climate change. Moreover, in their literature review, Nordqvist and Krampe found that South and Southeast Asia, two regions that have been highly affected by both climate change and conflict, are understudied (Nordqvist & Krampe, 2018, p. 2). This is problematic because these areas could be among the most vulnerable to climate-induced conflicts. These regions are highly vulnerable to the impacts of climate change, where temperature and sea-level rise as well as an increased frequency and intensity of droughts, floods and cyclones are projected to have severe and altering impacts across the continent (Asian Development Bank, 2017). Furthermore, a majority of the people in South and South-East Asia live in rural areas, where many work within and are depended on agriculture and fishing (Asian Development Bank, 2017, p. 55; Nordqvist & Krampe, 2018, pp. 3-4). As dependency on agriculture is one of the factors that plausibly affects the effect of climate change on violent conflict (as will be explained in Chapter 3), this region deserves more research attention. Similar arguments could be made concerning other regions, such as Latin America, a region even less studied in the field, but where some of the countries are among the most at risk to climate change (Adams et al., 2018, pp. 201-202).

The second problem with the narrow empirical focus of the literature is that it limits the scope of knowledge that it is possible to acquire about how the effect of climate change on conflict could vary across contexts. In order to study heterogeneous treatment effects, heterogeneity in contextual variables is key. Of course, Africa is a big continent, consisting of countries with a great variety of political and economic systems, degree of history of violence, population size and densities, climatic situations and vulnerability to climate change. Heterogeneity in contextual factors is certainly found here. Nevertheless, if we want to acquire more general knowledge about how the impacts of climate change could affect conflict, we need to also research the effect in other regions than Africa. Moreover, although much contextual heterogeneity is found across the countries and communities in Africa, even more heterogeneity could be found worldwide. To understand how much the effect of rainfall variability on conflict is depended on the level of agricultural production in a community, research must focus on communities with varying degrees of dependency on farming. Furthermore, to understand how institutional capacity could mediate the effect of climate change on conflict, a broad variety of institutional capacities should be studied. A global focus on the climate-conflict nexus allows for more heterogeneity in the contextual variables, which again allows for more thorough investigations of how the effect of climate on conflict varies across contexts.

Chapter 3

Theoretical Framework

Both excess and scarce precipitation could increase the probability of violent, state-based conflict. As discussed in the literature review, the effect of rainfall on conflict is likely to be indirect and heterogeneous. This chapter explains why the effect is believed to be of such nature. The chapter has three parts. Section 3.1 discusses why anomalous rainfall is expected to have an indirect effect on violent, state-based conflict. It will be argued that rainfall variability could produce both grievances and changes in opportunity costs, and that both mechanisms could lead to violent, state-based conflict. Section 3.2 explains why the effect is likely to be heterogeneous, and why it plausibly depends on the vulnerability of the community hit by a rainfall shock. I present some factors that determine the vulnerability of a community to be at risk from rainfall-induced conflicts. These factors are hence believed to modify the effect of rainfall variability on conflict. In Section 3.3, three hypotheses are formed based on the theoretical expectations derived from this chapter. It is beyond the scope of this thesis to test whether the effect of anomalous rainfall on conflict is indirect (as opposed to direct), so no hypotheses are made based on the argumentation from Section 3.1. Instead, and in line with the research question, the hypotheses concern the heterogeneity of the treatment effect of rainfall on conflict. The hypotheses are further developed in Chapter 5, when the theoretical factors mentioned here have been operationalized.

3.1 The Indirect Effect of Rainfall Variability on Conflict

In order to understand how a climate shock could affect conflict, we need to understand what can drive people to rebel in the first place. What sorts of mechanisms could make civilians take up arms so that an armed, state-based conflict erupts, and how could rainfall shocks possibly create or affect such mechanisms. The following discussion describes how grievances and opportunity costs could affect state-based conflict. Section 3.1.3 presents some examples that link rainfall to conflict, explained in the terminology of grievances and opportunity costs.

3.1.1 Grievances

From the French revolution to the Arab Spring, many violent, state-based conflicts seem to have started based on a sense of grievances amongst the revolting population (Cederman, Gleditsch, & Buhaug, 2013, p. 1).[1] As postulated by Gurr, grievances could arise from *relative deprivation*, thus when expectations of social, economic and political goods deviate from people's ability to obtain those goods (Gurr, 2011, pp. 24-25). The relative deprivation theory implies that sources of discontent are not universal, because individuals or groups must *perceive* themselves as deprived in order for grievances to appear. Grievances hence arise when there is a mismatch between the resources and capabilities people have and what they believe they are entitled to acquire (Hillesund et al., 2018, p. 455).

There are at least two types of instances where such a mismatch exists. First, relative deprivation could appear if people experience a deterioration in their life situation, due to a mismatch between the current situation and the previous situation (and hence what they believe they are entitled to). Secondly, relative deprivation could result from perceived inequalities in a society, where individuals or groups compare their situation with that of other individuals or groups (Hillesund et al., 2018, p. 456; P. M. Kuhn & Weidmann, 2015, p. 548). Indeed, inequality is often posited as an underlying cause of civil war, because people percieve themselves as unfairly treated (Buhaug, Cederman, & Gleditsch, 2014; Regan & Norton, 2005, p. 320). Grievances could motivate people to rebel against the state. Uprisings could be non-violent and turn violent if the state use arms to stop demonstrators, it could turn violent over time as a means to get the government's attention if it has not responded to the uprisings in the desired way, or rebellion could be violent from the start. For uprisings to be directed towards the government, the protestors must percieve the government as part of the cause or solution to their grievances.

[1] Grievances can be defined as "sources of discontent" amongst a group of people (Heery & Noon, 2017), or as a "feeling of resentment over something believed to be wrong or unfair" (Lexico, n.d.).

3.1.2 Opportunity Costs

The opportunity cost theory suggests that individuals that consider participating in rebel groups will evaluate the costs and benefits of rebellion relative to licit work.[2] The opportunity costs of rebellion decrease if the benefits of licit work diminish, which again increases the chance of civilians becoming rebels (Collier & Hoeffler, 2004; Koubi, 2017, p. 201; Theisen, 2017). In contrast to the grievance theory that focus on frustration and deprivation as motivation for rebellion, the opportunity cost theory focus on how material or non-material needs could make people join in violence, which in turn could increase the prevalence of violent, state-based conflict.

To be sure, many factors contribute to an individual's estimation of the opportunity costs of rebellion. Motivation for rebellion often stem from a mix of political, economic and social factors (Florez-Morris, 2007, p. 620; Tezcür, 2016). In addition to support for the rebel group's motive for rebellion (which could be affected by the existance of grievances), factors such as physical, social and economic protection, as well as a consideration of the possible dangers of not joining, all could influence the decision to join a rebel group. If any of these factors shift so that the balance between joining and not joining is tilted in favor of rebellion, then a civilian might choose to become a rebel.

3.1.3 Linking Rainfall to Conflict

In accordance with the aforementioned definitions, this section explains how anomalous rainfall could plausibly increase grievances and tilt opportunity costs in favor of rebellion, which could, in turn, augment the risk of violent, state-based conflict. There are three overarching mechanisms that link rainfall to conflict: (i) Rainfall variability could create grievances by reducing the standard of living of individuals and groups, (ii) it could create grievances if the impacts of rainfall shocks hit groups of people differently, so that inequality is increased or accentuated or (iii) it could tilt opportunity costs of joining rebel groups by reducing resources that rebel groups could provide for and by giving people time to rebel if becoming unemployed. These mechanisms will be explained in what follows.

The first mechanism mentioned is that rainfall shocks could increase grievances by reducing people's standards of living. This could happen in several ways. First, both droughts and floods, or milder forms of rainfall variability, could destroy agricultural crops, and lead to lost incomes of people employed within the agricultural sector, or increased prices of goods for buyers of the agricultural product that was destroyed

[2] Rebel groups can be defined as groups that use violence for a political cause (Jo, Dvir, & Isidori, 2016). Opportunity costs are the "costs of any activity measured in terms of the value of the next best alternative not chosen"(Manley, Foot, & Davis, 2019).

(IPCC, 2014, pp. 60, 117, 494). This would decrease the economic capacity of people and could create grievances. Second, if droughts or floods lead to large production shortages in the agricultural sector in an area, widespread hunger or famine could arise.[3] Third, if severe, floods could also destroy homes, workplaces and lives, which would increase grievances.[4] Fourth, excess precipitation could increase grievances by increasing the presence of diseases. Insects that contain diseases such as malaria and dengue depend on water for breeding, so the prevalence of these insects is affected by the supply of standing water. These diseases have devastating health and economic impacts on society (Bomblies, 2012; Krefis et al., 2011; National Research Council (US) Committee on Climate, Ecosystems, Infectious Diseases, and Human Health, 2001; Odongo-Aginya, Ssegwanyi, Kategere, & Vuzi, 2005; Okuneye & Gumel, 2017). Lastly, increased frequency of excess or scarce rainfall events that destroy homes, reduce incomes, increase food prices and/or lead to hunger or increased prevalence of diseases could force people to migrate (IPCC, 2014, p. 20).[5] Migration could lead to increased grievances among the receiving population where the migrants go to because they now have to share resources and space with the newcomers. Grievances, in the form of frustrations against the state, could also erupt among the migrants, if for instance they feel like the government in the sending or receiving country or region should have helped them out, or amongst the stayers for the same reasons. For all of the five types of grievances mentioned above to lead to *state-based* conflict, civilians must feel themselves deprived of something that the state should have helped out with, meaning that the government is not doing enough to help those affected by the climate extremes, or should have done something to prevent the impacts of the shock.

The second mechanism that could link rainfall to conflict is that grievances could arise due to increased inequality if the effects of rainfall events mentioned above hit people disproportionately. Rainfall events could also make existing inequalities more visible. Grievances created by inequality could make people rebel against the state if they believe the state should act differently, or against other groups whom they perceive deprive them of freedom or security, which could be a conflict where the

[3]For example, the drought in the North-Eastern part of Ethiopia in 2015 to 2016 led about 5.6 million people to depend on emergency food assistance in 2017 (Reliefweb, n.d.). Moreover, when South Sudan was hit by a severe drought followed by a widespread flood in early and mid-2019, about 73,000 tons of cereal were destroyed. As in Ethiopia, the rainfall shocks led to food insecurity of those affected, as about 1 million people were in need of emergency food assistance following the events (Phiri, 2019).

[4]For instance, in early 2020, Jakarta, the capital of Indonesia, was hit by a heavy flood that killed more than 60 people, displaced about 28,000 and damaged about 1,600 homes, according to reports from January 10, which, of course, deteriorated the living situation of those affected (ACAPS, 2020).

[5]Although effects of rainfall events on migration seem to vary (see IPCC, 2014, pp. 769-770 for an overview), migration has been attributed to drought and land degradation in Guatemala (Escobar & Rabanales, 2020; López-Carr, 2012), and to flooding in Vietnam (Dun, 2011).

state might get involved.

The third mechanism in which rainfall could impact conflict is by rainfall shocks tilting opportunity costs of joining rebel groups. If expected returns from agricultural labor falls due to rainfall irregularities, or if houses and workplaces are destroyed, affected civilians might consider participating in rebel groups that could offer income opportunities and/or benefits such as shelter, food and security (Miguel et al., 2004, p. 726; Vestby, 2019). Moreover, total or partial unemployment give people more time to participate in rebellion, which could further decrease the opportunity costs of violent revolt (Berman, Callen, Felter, & Shapiro, 2011, p. 498; Grossman, 1991). Increased rebel recruitment could increase the probability of violent conflict, because the larger a rebel group is, the more confident will its ability to fight.

More examples of connections between rainfall variability and grievances/opportunity costs probably exist. Moreover, the examples mentioned are only discussed briefly, but nuances and complications of these mechanisms does of course exist. Nevertheless, the above examples show that both excess and scarce rainfall, although different hydrologic events, could create imbalances in societies adapted to certain types of weather patterns, and consequently increase the risk of violence.[6]

3.2 Vulnerability and the Effect of Rainfall on Conflict

Although anomalous rainfall could have an indirect effect on violent, state-based conflict, the effect is doubtfully homogeneous. In some communities, the effect of a rainfall shock might be nonexistent because of contextual factors that mitigate the societal consequences of the event. In other contexts, rainfall variability could be the force that pushes communities into violent conflict. Indeed, the size of an effect of rainfall variability on conflict is likely to depend on the vulnerability of the society that is hit by the event.

In this thesis, vulnerability is defined as the propensity of communities to erupt in conflict in the aftermath of an anomalous rainfall event. It complies with the definition given by the IPCC, who classify vulnerability to climate change as composed of two elements: sensitivity and adaptive capacity (IPCC, 2012, p. 33).[7] The level of

[6] A natural output from Section 3.1 would be to form some hypotheses about the indirect relationship between rainfall variability and conflict. The research question of the thesis is: *Under what conditions, if any, does rainfall variability affect violent, state-based conflict?* Although interesting in its own right, hypotheses about the indirect, causal mechanisms that are assumed to link rainfall variability to conflict does not help answer the research question, and are therefore outside the scope of this thesis. Section 3.1 was included as a basis for the discussion in Section 3.2.

[7] The IPCC defines sensitivity and adaptive capacity as parts of communities' and groups' vulner-

sensitivity in a society determines the degree of societal impact that a climate event could cause, while adaptive capacity refers to the ability to cope with such societal impacts, if they emerge (Adger, 2006, p. 270; Ide et al., 2014, p. 69).[8]

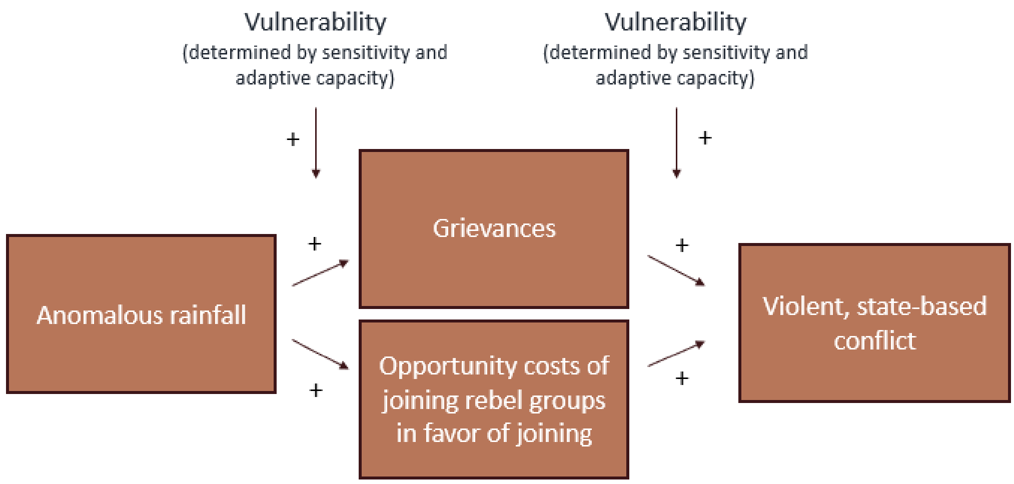


Figure 3.1: Causal Mechanisms

Figure 3.1 shows a simplified picture of the indirect, causal mechanism that connects anomalous rainfall events to violent, state-based conflict, as described in Section 3.1. It shows that sensitivity and adaptive capacity are relevant in both steps of the causal mechanisms. First, a community could be sensitive to be socially affected by a rainfall event, and it could be sensitive to erupt in conflict due to underlying causes of tension in a society. For instance, as will be elaborated below, a society could be sensitive to experience production shortages caused by a drought (which could create grievances) if its economy is highly depended on agricultural output. Moreover, it could be sensitive to conflicts erupting as a consequence of increased grievances if the society has a history of politically excluding certain ethnic groups, and if that inequality is accentuated by the increased grievances. Second, a community's adaptive capacity could reduce both the chance of grievances or changed opportunity costs appearing from a climate event, and it could reduce the probability of conflict erupting where grievances have risen and opportunity costs have been changed. For example, if a rainfall event reduces farmers' returns from production, that might not lead to

ability to climate change. Although I look at the vulnerability of communities to erupt in conflict in the aftermath of a rainfall event specifically, the distinction between sensitivity and adaptive capacity is also relevant here.

[8]The exposure of a location to climate hazards is viewed as irrelevant for its vulnerability status according to this definition.

grievances if the state is able to compensate the income loss of the farmers. Moreover, if a rainfall shock accentuates inequalities, a state might be able to lessen the risk of violent conflict if it meets the demands of those who perceive themselves as unequally treated. Sensitivity and adaptive capacity hence interplay and modify the effect of rainfall on conflict in complex and multifaceted ways. Both the type and magnitude of vulnerability in a society will affect the likelihood of rainfall-induced conflict.

Since vulnerability is complex and difficult to quantify, it cannot be reduced to a single metric (Adger, 2006, p. 274). Therefore, I have gathered seven factors that could contribute to a community's sensitivity and adaptive capacity, which are presented in the following sections. The brevity of the discussion below reflects a literature that has not indulged in studying contextual factors. Because of the lack of research on the heterogeneity of the climate-conflict link, I take a semi-inductive approach to answering the research question, and include many factors that could create effect heterogeneity, derived from scarce evidence in the existing literature. I create three hypotheses of how the effect of rainfall on conflict should vary based on the limited theoretical knowledge that exists.[9] The results from the analysis shows which of these factors modify the rainfall-conflict link, and how they do so. As such, those results can contribute to the theory-building on the subject, which will inform the research field of what factors should be further investigated. Due to the limited scope of this thesis, the list of vulnerability factors presented below is not exhaustive, but presents some plausible contributors to heterogeneity in the rainfall-conflict link. Future research should examine what other features could influence the nexus.

3.2.1 Dependency on Rain-Fed Agriculture

The level of agricultural dependency in a community affects its sensitivity to rainfall shocks. Economic systems based on agricultural incomes are the most vulnerable to rainfall anomalies, because rainfall events could destroy crops and affect economies (von Uexkull et al., 2016, p. 12391). Research on agriculturally-based communities support this claim. Miguel et al. (2004) found that drought increased the probability of intrastate conflict in Sub-Saharan Africa. von Uexkull (2014) found that drought substantially increased the risk of civil conflict for sub-national regions depended on rain-fed agriculture. Moreover, in a study that covered Asia and Africa, von Uexell et al (2016) found that for agriculturally depended groups, the likelihood of sustained conflict involvement increased by the occurrence and duration of drought, while no such effect was found for the majority of groups studied. In a global, country-level

[9] The length of the discussions of the factors also reflects, to some extent, the number of articles that have studied the implication of that factor, and how complicated the implication of the factor is.

sample, Salehyan and Hendrix observed a positive correlation between excess rainfall and political violence, a correlation that was stronger in more agriculturally depended communities (2014).

Irrigation systems make agriculture less depended on rainfall and more resilient to rainfall shocks (Hendrix & Salehyan, 2012). Fishman (2018) found that the damage of rainfall irregularity on yield output in India was substantially lowered where irrigation systems were in place. The effect of rainfall variability on violent conflict should therefore be smaller in agriculturally-depended areas with irrigation systems than in areas that depend on rain-fed agriculture, as they would have better adaptive capacity to cope with the rainfall shocks.

3.2.2 Temperature

As explained in Section 3.1.3, mosquitoes who transmit malaria and dengue depend on standing water for breeding. These mosquitoes also depend on high temperatures to breed. Hence, for grievances to erupt due to increased disease occurrence, high temperatures are needed. Moreover, some researchers believe that high temperatures produce aggression (Anderson & Delisi, 2011; Chersich et al., 2019). Although this link is contested, if correct, it could make people more susceptible to join in rebellion if a rainfall shock has produced grievances. Temperature could thus affect both the sensitivity of excess rainfall to produce grievances and the sensitivity of grievances to lead to violent, state-based conflict.

3.2.3 Population Density

For anomalous rainfall to affect society, people must have been affected by the abnormality. If no people live in an area where excess or scarce precipitation occurs, it is unlikely to have any societal consequences (Busby et al., 2018, p. 93). The greater the population affected by a rainfall hazard, the greater are the societal consequences likely to be. Population density can thus affect the sensitivity of a community to anomalous rainfall.

3.2.4 Globalization and Urbanization

Globalization could make a country more robust to agricultural shortcomings. Scarce or excess precipitation could decrease the local supply of agricultural goods, and consequently increase prices. If a country participates in a global market, the lack of local supply of a product could more easily be substituted than if the country is not globalized. Economic downturn caused by climatically reduced agricultural output could diminish if an international market for the product exists (Vestby, 2018, p. 37), and if the country is able to participate in that market. Similarly, if physically excluded from other communities, a community will have a tougher time replacing local products than if closer to an urban center. If experiencing rainfall-induced crop-fails, it will be more difficult to replace the goods than in more urban areas, even if the products can be replaced by the international market. People who live in urban centres are also less likely to work within agriculture, and are hence less likely to be sensitive to anomalous rainfall. Hence, communities that are globalized and urbanized are likely to be less sensitive to rainfall shocks and have more adaptive capacity to cope with the societal consequences of such events, if they impact the economy.

3.2.5 Regime Type

Some debate exists over the connection between regime type and civil conflict. In general, civil conflict and political violence are believed to be less likely to happen in democracies than in other types of political regimes (Hegre, Ellingsen, Gates, & Gleditsch, 2001; Stockemer, 2010). Semi-democratic regimes are both somewhat open and somewhat repressive, which creates both grievances and opportunities to organize and fight (Hegre et al., 2001, p. 33). On one hand, people in democracies can use other channels than violence (such as media attention through non-violent protests or voting in elections) to get the government's attention and put their grievances on the political agenda. Politicians in democratic regimes are depended on reelection to stay in power, and are therefore more likely to answer to grievances than politicians in non-democratic regimes. On the other hand, autocratic regimes might be less afraid of using violence to stop uprisings in a population and might be more capable of repression. High likelihood of capture and repression makes rebellion less likely (Hendrix, 2010, p. 273). Hence, where grievances due to rainfall anomalies are severe, uprisings are most likely to become violent in semi-democratic regimes. Regime type thus affects the adaptive capacity of societies to deal with grievances and opportunity cost-changes. It could also determine the adaptivity of a regime to answer to the needs of the population so that grievances and changes in opportunity costs are not produced.

3.2.6 Good Governance

Good governance affects both a state's sensitivity to erupt in conflict and its adaptive capacity to mitigate the impact of economic crisis (United Nations Development Programme, 2015, p. 270). Good governance is a complex term which could affect the climate-conflict link in several manners. According to the United Nations, good governance has 8 characteristics: participatory, consensus oriented, accountable, transparent, responsive, effective and efficient, equitable and inclusive and follows the rule of law (United Nations Economic and Social Commission for Asia and the Pacific, 2009).

Effective, efficient and equitable political institutions could have more adaptive capacity to cope with the impacts of a rainfall hazard, if the state is capable of accommodating grievances via institutionalized channels (Hendrix, 2010, p. 273). One way that a state could accommodate grievances is through the economic compensation of income loss. If a state has the capacity to compensate people who have lost their income due to climate related causes, people might have less motivation for rebellion as grievances are not permitted to flourish, and as the compensation could balance out the opportunity costs of joining rebel groups. To be able to compensate, a state must have economic means, but must also have the institutional capacity to be able to handle the compensation efficiently and fairly. If the compensation is inefficient, it is unlikely to balance out the effects of the income loss. Moreover, if the state compensates the population in a way that is perceived as unequal or unfair, that could possibly create *more* grievances than had the state not compensated at all. Furthermore, a state's accountability could be important for the sensitivity of conflict erupting. A lack of trust in governmental institutions could lead people to solve conflicts on their own. People might be less likely to join rebel groups due to income loss or to rebel due to increased grievances if they are confident that the government will be able and willing to handle the crisis. Hence, good governance is important for the governments adaptive capacity to be able to meet the needs of the population in times of crisis, but also to its sensitivity to erupt in conflict if grievances or tilted opportunity costs have happened (Busby, Smith, White, & Strange, 2013, p. 158; Busby et al., 2018, p. 93).

Another part of good governance that could contribute to a community's vulnerability is the level of political inclusion in the society. For instance, ethnicity has been a key factor in a majority of armed conflicts since the second world war (Denny & Walter, 2013). The existence of ethnic tensions in a society makes it easier to mobilize civilians for a rebellion (Denny & Walter, 2013). Ethnic divisions could form a premise for conflict that could be pushed over the line by rainfall abundance or scarcity (Schleussner, Donges, Donner, & Schellnhuber, 2016). Moreover, politically excluded ethnic groups are less likely to receive support and compensations from the government in times of crisis, as they cannot threaten the stability of the regime in the same way as can the politically included groups (Raleigh, 2010). Politically excluded groups thus have fewer coping strategies, which could make them more susceptible to violence (Fjelde & von Uexkull, 2012, p. 447). In their analysis of vulnerable groups in Africa and Asia, von Uexkull and colleagues found that politically excluded ethnic groups are more likely to take up arms in times of climatic hardships than other groups (von Uexkull et al., 2016). The level of political exclusion in a society could thus influence both the sensitivity and adaptive capacity of a society to prevent a rainfall shock to lead to violent, state-based conflict.

3.2.7 Prevalence of Non-State Conflict

Lastly, the prevalence of conflicts where the state is not involved could affect the likelihood of a rainfall shock leading to state-based conflict. Non-state conflict can often pave the way for other types of unrest (Brosché & Elfversson, 2012). Governmental actors often intervene in communal conflicts happening within their borders in order to secure land or other resources, particularly if the conflict stands along ethnic lines (Elfversson, 2015). If a rainfall shock leads to increased grievances that fall on top of an existing conflict, then the conflict might increase in size because more people get involved. In such scenarios, a state might find it necessary to intervene to secure people, land or other resources that now seem threatened. Moreover, for changes in opportunity costs to lead to increased rebel recruitment, some rebel groups must exist prior to the rainfall shock. If a non-state conflict is present when a rainfall shock hits, and that conflict involves rebel groups, then changes in opportunity costs could indeed lead to increased rebel recruitment, because an illicit alternative to licit work exists. The prevalence of non-state conflict in a community prior to an anomalous rainfall event can thus affect its sensitivity to rainfall-induced state-based conflict.

3.3 Formation of Hypotheses

Following the discussion in Section 3.2, there seems to be a multitude of reasons why the effect of both excess and scarce rainfall on violent, state-based conflict would depend on vulnerability factors. I make three hypotheses about the nature of the effect of rainfall on conflict. The first hypothesis is formulated as follows:

H1: *The effect of excess or scarce rainfall on violent, state-based conflict is dependent on the vulnerability of the community hit by the rainfall shock.*

Hypothesis two and three (H2 and H3) concern *how* the treatment effect of rainfall on conflict is expected to vary depending on vulnerability factors. I make separate hypotheses for scarce and excess rainfall events.

H2: *The effect of scarce rainfall on violent, state-based conflict is larger the more vulnerable a community is to rainfall shocks and conflict.*

H3: *The effect of excess rainfall on violent, state-based conflict is larger the more* *vulnerable a community is to rainfall shocks and conflict.*

To be able to test these hypotheses, the variables that are supposed to measure the vulnerability factors must be introduced and operationalized. That will happen in Chapter 5. Therefore, Chapter 5 will also introduce the theoretical expectations of how the treatment effect should vary across the vulnerability factors if H2 and H3 are to be confirmed. This will guide the analysis in Chapter 6.

Chapter 4

Methodological Framework

This chapter presents the methodological framework for the analysis. As discussed in the literature review, the research field has focused excessively on estimating an average treatment effect (ATE) of climate change and variability on conflict, although the effect is likely to be context specific. This has produced diverging results. To understand the rainfall-conflict link better, researchers should investigate if the effect is heterogeneous, and if so, how it varies. The causal forest presents an ideal opportunity to do just that, which will be explained in this chapter.

The chapter is structured as follows. First, the concepts of causal inference and heterogeneous treatment effects are explained. Thereafter, Section 4.2 explains why causal forest has been chosen to estimate treatment effect heterogeneity. There are two main motivations for using the causal forest. First, it is a nearest neighbor matching method, meaning that it relaxes the functional form assumption and is more efficient in the estimation of the treatment effect, compared to other non-experimental designs. Second, it is a machine learning method, which means that it handles complexity well and can divide the observations into subgroups based on patterns in the data, rather than on the basis of theoretical assumptions. Section 4.3 gives an overview of the causal forest algorithm and the assumptions that must hold for the causal forest to be used.

4.1 Causal Inference and Heterogeneous Treatment Effects

In this thesis, causation is defined by the potential outcomes model. A causal effect is defined as the contrast between the outcome when an observation has received a treatment with the outcome when the treatment is not present (Morgan & Winship, 2015, p. 4).[1] There are thus two potential outcomes: the outcome when the treatment is present and the outcome when the treatment is absent (Gerber & Green, 2012, p. 22). The fundamental problem of causal inference is that only one of these outcomes can be observed, an observation is either given the treatment or not (Gerber & Green, 2012, pp. 23-24). With this definition of causation, it is impossible to observe causal effects on the individual level (Morgan & Winship, 2015, p. 4). Instead, the focus has to be on average effects for defined groups.

In experimental designs, the fundamental problem of causal inference is solved by assigning observations randomly to treatment and control groups (where the first has received the treatment and the other not), and estimate the difference in outcomes between the two groups (Angrist & Pischke, 2009, pp. 14-15, 53-54). The randomization ensures that there is no systematic difference between the groups (no selection bias), so that the results are comparable (Angrist & Pischke, 2009, pp. 14-15; Gerber & Green, 2012, p. 31). For a binary treatment, the treatment effect is calculated based on Equation 4.1, where i is the sample, τ is the treatment effect, $y_i(1)$ is the value on the dependent variable for the treatment group and $y_i(0)$ is the value for the control group. The estimated effect is then the ATE, and is estimated to apply to the whole population. Equation 4.1 can also be used for calculation of the ATE in non-experimental designs. Section 4.2.1 gives an explanation of how one could estimate τ_i through matching methods.

$$\tau_i = E[y_i(1) - y_i(0)] \tag{4.1}$$

Just as the mean is not always a good description of a distribution, the ATE is not always very useful or actionable. People often respond different to similar situations depending on factors such as experience, life situation, education, gender or age. Hence, in some situations the treatment effect differs from subgroup to subgroup, based on some or one of these contextual factors. As mentioned in the introduction, a heterogeneous treatment effect (HTE) is an effect that varies across subgroups and/or settings (Gerber & Green, 2012, pp. 289-290). To estimate HTEs, one can identify variations in the conditional average treatment effect (CATE) of subgroups of the population. CATEs can be described as the ATEs of subgroups of a population, and are calculated through Equation 4.1, but for specified subsets. If there is treatment effect heterogeneity in the population, then the CATEs for different subgroups will vary. As discussed in Chapter 2, the ATE of rainfall variability on violent conflict is doubtfully the most useful estimate if we want to understand how climate affects

[1]The outcome is often called the dependent variable, while the treatment is sometimes referred to as the independent variable. These terms will be used interchangeably. The outcome and the treatment variables could be any type of factor or event whose relation is of theoretical interest.

society. Even if it is possible to identify an average effect of rainfall on conflict, that effect is of little interest if it differs widely from context to context. I will therefore concentrate on investigating heterogeneity rather than estimating the ATE of rainfall on conflict.

4.2 Motivations for the use of the Causal Forest

To estimate treatment effect heterogeneity, I will use the causal forest, which is a matching method and machine learning technique for causal inference.[2] In the following, I will explain the advantages of using matching analysis and machine learning in the estimation of HTEs.

4.2.1 Matching Methods

There are various ways to estimate causal effects under the potential outcomes model. Randomized experiments are often viewed as the gold-standard within causal inference, as it gives the researcher control over treatment assignment. Since conducting a randomized experiment with rainfall variability as the treatment and conflict as the outcome is impossible, a non-experimental design has to be used to answer the research question in this thesis. A common problem for non-experimental designs is that the researcher does not have control over the treatment assignment, and the treatment assignment could thus depend on some confounding variables. One way to deal with non-random assignment is through matching methods. Matching is a collective term that refers to non-parametric analyses where observations are matched based on their values on some contextual variables. Treated observations are matched with untreated observations with similar characteristics, and then the treatment effect is calculated by comparing the outcome values for the matched observations (Morgan & Winship, 2015, pp. 141-143; Quinn, 2011). As the observations have the same (or very similar) values on the variables they are matched on, variance in outcomes has to be due to variance in the treatment variable only (given that some assumptions hold) (Morgan & Winship, 2015, p. 143).

Regression analysis is another non-experimental method for estimating treatment effects, where confoundedness could be accounted for by adding control variables. An advantage of matching methods compared to regression analysis is that matching is

[2] The causal forest is part of the Generalized Random Forests package developed by Athey, Tibshirani and Wager, which also includes random forest algorithms for instrumental variable analysis and non-parametric quantile regression. Most of the characteristics of causal forests described in this section are shared by all the forests in the Generalized Random Forests package, but with somewhat different applications. For more information, see Athey et al. (2019).

non-parametric, meaning it relaxes the functional form assumptions that parametric models such as regression analysis depend on. Parametric models are constructed based on assumptions of the form of the relation between the treatment and the outcome variable (Green & Kern, 2012, p. 495). If the researcher believes there is a linear relationship, they would shape the model thereafter. If they believe there is a curve-linear relationship, the researcher would include an exponential term (if a regression model), and so on. Incorrect modeling of the functional form can lead to biased estimates (Green & Kern, 2012, p. 495). In matching analysis, the population distribution can take any shape, which is advantageous because it makes the construction of the model less depended on the researcher's knowledge of the type of relation between the treatment and the outcome. In a field of research that consists of as much uncertain theory and ambiguous results as in the literature on the climate-conflict nexus, we should use models that do not require extensive assumptions and choices made by the researcher, at least until more certain conclusions are obtained. As parametric models build on assumptions about the structure of the data and relation between the covariates, they are sensitive to misspecifications, but this uncertainty is not visible in the standard errors that are usually portrayed in the conclusions of the analyses (Green & Kern, 2012, p. 492). Hence, because they are non-parametric methods, matching methods are less prone to bias created by incorrect model specifications.

Another advantage of matching methods is that the focus of the analysis is completely on the relationship between the treatment and the outcome variable. This is especially beneficial when investigating heterogeneous treatment effects. In regression analyses, HTEs are modeled through the inclusion of interaction terms. However, when including interaction terms, the model does not only estimate how the treatment effect varies with the contextual variables, but also how the effect of the contextual variables vary with the treatment variable. Simply put, regression models with interaction terms are usually constructed through equations of the form $Y = beta_0 + beta_1 W + beta_2 X + beta_3 W * X$, where Y is the estimated value of the outcome, W is the treatment variable, X is a covariate, and $W * X$ is the interaction term (Christophersen, 2018, p. 87). The *beta*s are estimated parameter effects. In a regression analysis with interaction terms, the model simultaneously estimates the effect of the treatment W *and* the contextual variable(s) X on the outcome (Morgan & Winship, 2015, p. 142). Therefore, for each interaction term that is added, two extra parameters have to be estimated. In a matching analysis, however, one can estimate how the treatment effect varies for different subgroups of the population, conditional on the contextual variables, without having to simultaneously estimate the effect of the contextual variables on the outcome (Morgan & Winship, 2015, p.

142). That makes the models more efficient as the estimates made are all valuable for the purpose of the analysis, namely to investigate varieties in the effect of the treatment on the outcome.

4.2.2 Machine Learning

Machine learning (ML) is a form of artificial intelligence that permits a machine to learn from patterns in some data to predict or estimate the patterns of new data (Alloghani, Al-Jumeily, Mustafina, Hussain, & Aljaaf, 2020, p. 4). ML is used for a huge variety of purposes, from targeted marketing to predicting winners of elections.[3] The main advantage of using ML for estimating HTEs is that it handles complexity better than experimental and observational studies based on parametric models (Athey et al., 2019, p. 6; Green & Kern, 2012, p. 492). Models with HTEs quickly become complex if we believe that the treatment effect depends on many variables. As explained above, in regression models, treatment heterogeneity is accounted for through the inclusion of interaction terms. If the treatment effect is believed to depend on many covariates, the model grows complex very fast, as two additional parameters have to be estimated for each extra interaction term. For instance, in this analysis, which includes 13 covariates that might modify the effect of rainfall variability on violent conflict, constructing a regression analysis with a 13-way interaction would be both statistical exhaustive and difficult to interpret. In contrast, the causal forest can handle hundreds of covariates without having to compensate on statistical power.

Another advantage of machine learning when estimating HTEs is that the machine itself creates the subgroups that will be used for estimation of the CATEs, based on patterns in the data, instead of the researcher having to do that based on theoretical assumptions. In the context of causal forests, the machine learns from patterns in the data to divide observations into subgroups where the treatment effect differs the most (this will be explained in further detail below) (Wager & Athey, 2018, p. 1240). In non-machine learning models, the researcher itself would have to pick the subgroups where it believed the treatment effect to differ, based on knowledge acquired from former research and assumptions about the data. Yet, it may not be clear which covariates should be used to categorize subgroups. That is particularly the case in fields where theory is not fully developed and former research is uncertain

[3] In ML, it is distinguished between supervised and unsupervised learning. Supervised learning happens when a dataset is divided into input and output variables and the machine uses the input to predict the value of the output (Alloghani et al., 2020, p. 9; Hastie, Tibshirani, & Friedman, 2009, p. 4). The machine is constantly given feedback on its performance, and uses that feedback to adjust its model of prediction. In contrast, within unsupervised learning, all the variables are viewed as input. Thus, the machine attempts to label the data based on the inherit groupings it finds instead of predicting or classifying an output (Alloghani et al., 2020, p. 4). When in the following referring to machine learning, it is meant supervised learning.

about the relation between the treatment and the outcome variable, such as in the research field of climate variability and violent conflict. As argued by Chernozhukov and colleagues, ML models *"seem to be ideal to explore heterogeneity of treatment effects, when researchers have access to a potentially large array of baseline variables to form subgroups, and little guiding principles on which of those are likely to be relevant"* (Chernozhukov, Demirer, Duflo, & Fernández-Val, 2019, p. 2). If the researcher itself picks the subgroups, there is a risk that the researcher will overlook important heterogeneity, and focus on the wrong groups. As Green and Kern put it: *"the credibility of CATE estimates diminishes when researchers get to choose how to divide their data into subgroups"* (2012, p. 492). Machine learning avoids this problem by letting the machine decide what conditional variables are important, based on structures in the data.

4.3 The Causal Forest Algorithm

Causal forest can be viewed as an adaptive nearest neighbor matching method. Nearest neighbor matching finds matches based on some distance measure. Each observation is matched with one or several observations from the treatment group and the control group based on how close they are on a scale created from the contextual variables (Morgan & Winship, 2015, p. 160). For instance, if age and sex are the variables that we want to match on, we would pick a treated observation and find the observation from the control group that is closest in distance in age and sex to the treated observation. That observation is then the nearest neighbor to the treated. If matching the treated with more than one observation, we would find the k observations with the closest values to the treated (from both the control and treatment group). Causal effects are then estimated by comparing the value on the outcome for the treated observation and its nearest neighbors.

In a causal forest, each observation is matched with a set of neighbors that are weighted based on their closeness to that observation. When estimating treatment effects, the observation's value on the outcome variable is compared to its neighbors' outcomes. The closer the neighbor is to the observation, the bigger is its weight (i.e. its importance), in the estimation of the treatment effect. The causal forest method is adaptive because the number and type of variables that pick the nearest neighbors is determined by the machine, and can thus vary based on what is needed to make good matches (Wager & Athey, 2018, pp. 1230, 1240). To understand how the causal forest works, an explanation of the algorithm is required.

Causal forest is a type of random forest algorithm for estimation of heterogeneous treatment effects (Athey et al., 2019).[4] A causal forest consist of many decision trees, called causal trees. Most people have used some sort of a decision tree to reach a decision at some point. A decision tree consists of "one or more nested if-then statements for predictors that partition the data" (M. Kuhn & Johnson, 2013, p. 173). In a man-made decision tree, each tree usually begins with a question whose answer decides what path to follow. The path chosen leads to a new question whose answer leads to a new one until a decision is made in the final node of the tree. Rather than reaching decisions, the final node in a machine-made decision tree gives some sort of estimated value for observations that end up in that leaf. The tree begins with a variable that splits the data in two based on the value of the observations on that variable. This process continues until the observation has ended up in a leaf, where an estimate is made. The observations in a final leaf can be considered nearest neighbors, as they share values on the variables that led them to that leaf. Figure 4.1 shows an example of what a causal tree could look like.

The strength of decision trees is that they are easy to interpret. However, their estimates are often inaccurate (M. Kuhn & Johnson, 2013, p. 174). Causal forests (and random forests) usually consist of hundreds or thousands of decision trees whose estimates are averaged. While building the forest, each tree is built on a random sample of the data, and is at each split point given a random subset of the variables to pick from. These two features ensure that the trees are decorrelated, as they will all be slightly different depending on what data and variables they are presented with in the building process. The decorrelation of the trees and aggregation of the estimates make the forest's estimates more stable than that of a single decision tree (M. Kuhn & Johnson, 2013, pp. 192-194).

In order to estimate heterogeneous treatment effects, the causal forest has a special splitting criterion when creating the trees. At each split point, the algorithm is set to pick the variable and value that best separate the data into groups where the difference in treatment effects is biggest. Each group of observations that end up in a final leaf of a tree can be viewed as subgroups of the population, as they have similar values on the variables that led them to that leaf. When a new observation x is sent down the tree, the observations that are in the same leaf as x are viewed as its nearest neighbors. Each neighbor to x is then given a weight. If there was only one observation in the leaf together with x (which is seldom the case), then the weight of the neighbor

[4]Since random forest is used for prediction and causal forest for causal inference, the algorithms differ slightly, with two main differences. First, the splitting criterion in a causal forest is set to maximize treatment effect heterogeneity, while a random forest maximizes homogeneity on the outcome variable. Second, causal forests build honest trees through double sampling, which is usually not done in a random forest. This chapter will describe the algorithm that is specific to the causal forest.

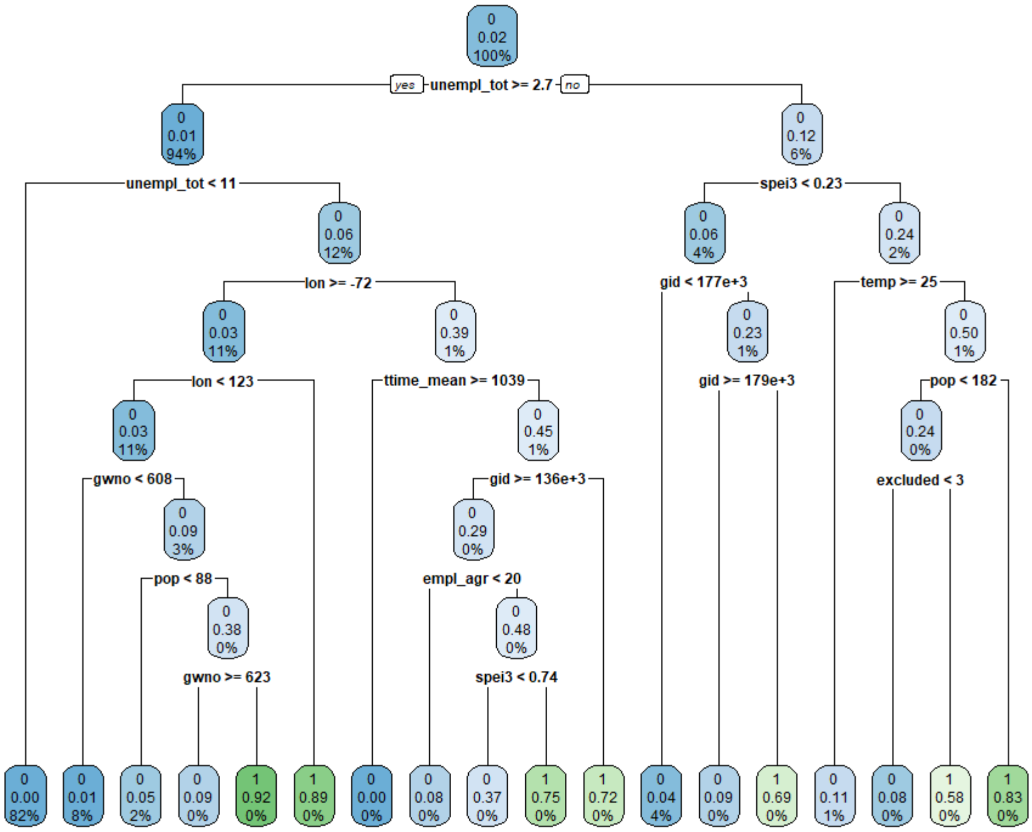


Figure 4.1: Example of Decision Tree
This figure shows an example of a decision tree. The tree begins with a variable that splits the data in two based on the value of the observations on that variable. The splitting continues until the model has divided the data into subgroups where the treatment effect differ.

calculated from that tree would be 1. If there were two observations in the leaf in addition to x, then the weight of the neighbor would be 0.5, and so forth. The weight of each neighbor thus depends on the number of observations in the neighborhood (i.e. the leaf). When each tree has generated weights for the neighbors of x, the algorithm finds the average weight across all the trees. The average weights that the forest generates as a whole is therefore a measure of how many times the observations fall into the same leaf as x. In other words, as with other nearest neighbor methods, each observation in a causal forest is matched with other observations based on how close they are on a distance measure. The distance measure here is whether or not they end up in the same leaf as x, hence, whether or not they share the same values on the contextual variables.

Specifically, the weights are calculated based on Equation 4.2, taken from Athey et al. (2019, pp. 6-7). Let i be part of a training set of n observations, where $i = 1, ..., n$.

We want to find and give weight to the nearest neighbors of a test observation with values on the contextual variables equal to x. A forest is grown consisting of B trees, indexed $b = 1, ..., B$, and $L_b(x)$ is the number of observations in the training set that has fallen into the same leaf as x in the bth tree. Then $\alpha_{bi}(x)$ is a weight given to training example i derived from the bth tree, depending on how many observations are in that leaf together with x and i, hence the size of the neighborhood. If the neighborhood is small, then observation i should be given more weight than if the neighborhood size was bigger. $\alpha_i(x)$ gives a measure of how many times training observation i falls into the same leaf as x. It is the sum of the $\alpha_{bi}(x)$ of all the trees in the forest, divided by the number of trees, hence the average of the weights of observation i. If an observation never ends in the same leaf as x, its weight would be zero, which would mean that it had very different values on the contextual variables than x. The weights of all of the neighbors to x add up to 1, and are used to calculate the CATE or the ATE through a local maximum likelihood estimation. The weights make the estimation local.

$$\alpha_{bi}(x) = \frac{1(\{Xi \in L_b(x)\})}{|L_b(x)|}, \ \alpha_i(x) = \frac{1}{B}\sum_{b=1}^{B} \alpha_{bi}(x) \tag{4.2}$$

The causal forest has one important characteristic, in addition to the special splitting criterion, that makes it different from other random forest models. The algorithm builds what the authors call *honest* trees (Athey et al., 2019, p. 11). This is to ensure consistency of the estimates. Recall that each tree in a causal forest is built based on a random sample from the data. For the trees to be honest, the random sample is split into two subsamples, where one is used to choose the split points in a tree (hereafter called the splitting subsample), while the other subsample is used to estimate the size of the effect once the tree is built (hereafter called the estimating subsample) (Wager & Athey, 2018, pp. 1231-1232). The subsampling into a splitting and an estimating subsample is randomized. Consequently, some data points will be part of the splitting subsample in some trees and of the estimating subsample in others, but will never partake in both the splitting and the estimating subsample in one single tree (Wager & Athey, 2018, p. 1232). This feature ensures that the subsampling is efficient and does not lead to any information loss, as single data points can be used both for building trees and estimating effects in the forest. Although the final subsamples may end up being quite small (as the data is split twice), any loss of precision due to small subsamples is also outbalanced by the possibility of unbiased estimates that honest forests permit (Athey & Imbens, 2016, p. 7354). To verify that there are observations from both the treatment and the control group in each leaf, the algorithm makes sure that a minimal number of observations from each group end up in the final leaf (Tibshirani, Athey, & Wager, n.d.).[5] An error term is included in the splitting criterion as a correction to prevent the model from picking split points that increase the variance of the parameter estimates (Athey et al., 2019, p. 8; Eichenberger, 2020).

When built honestly, the causal forests are asymptotically normal (Wager & Athey, 2018), meaning that the treatment effect converges to a normal distribution as the sample size approaches infinity (White II, 2018). As normal distributions have a fixed shape, the relation between the mean and standard deviation of the distribution is known. Asymptotic normality is important because it makes it possible to construct confidence intervals and thus acquire knowledge about the certainty of the estimates. Since the estimated effect builds on data from a sample of the population, the estimate that the model gives might deviate from the true effect of the treatment. Confidence intervals convey an area within which the true effect, with a certain probability, lies (Christophersen, 2018, p. 28). If the probability distribution of the treatment effect is asymptotically normal, then the estimate of a causal forest is consistent and converges towards the true effect.

4.3.1 Assumptions

There are two assumptions that must hold for the results from a causal forest analysis to be trustworthy. The first of these is the unconfoundedness assumption, which assumes that the treatment assignment is independent of the potential values on the outcome variable, and could be expressed as in Equation 4.3 (Imbens & Wooldridge, 2009, p. 21).[6] In Section 4.1 I explained how randomization of the treatment assignment ensures that there is no systematic difference between the treatment and control group. However, in non-experimental designs where the researcher does not have control over treatment assignment, the observations that end up in the treatment group might have systematically different values on some confounding variables than those in the control group. If we can observe the variables that create the difference, we can assume unconfoundedness conditional on the variables that we have matched on, and then the observations can be treated as if they were part of a randomized experiment (Athey et al., 2019, p. 20; Wager & Athey, 2018, p. 1230). If the confounding variables cannot be observed, then some of the correlation between the treatment and the outcome might stem from the confounders, and the results would be biased. The unconfoundedness assumption is impossible to test, so fulfillment of the assumption,

[5] For continuous variables, the minimal node size gives the minimal number of observations in the final leaf with a value above and below the average value of the treatment variable.

[6] The unconfoundedness assumption is also known as "selection on observables", "ignorability", or "exogeneity" (Imbens & Wooldridge, 2009, p. 2)

and thus inclusion of all relevant variables, relies on theory.

$$W_i \perp \Big(Y_i(1),\ Y_i(0)\Big) \mid X_i \tag{4.3}$$

The second assumption that must hold is the overlap assumption. For the overlap assumption to be verified, the propensity scores (i.e. the probability of being assigned the treatment given a set of observed contextual variables) should not be very close to 0 or very close to 1 (Tibshirani et al., n.d.). Specifically, the overlap assumption could be expressed as in Equation 4.4 (Imbens & Wooldridge, 2009, p. 21), where $pr(W_i = 1|X_i = x)$ is the probability of observation i receiving the treatment, conditional on its values on the contextual variables x. In a randomized experiment, the *distribution* of the observations' values on the contextual variables x should be very similar for the treatment and the control group. If randomized, each unit should have a probability of being treated and a probability of being untreated. If we are to treat observations from an observational study as if they were randomly treated, then the distribution of the observations' values on x should be similar for the two groups also here (Imbens & Rubin, 2015, p. 309). If there is considerable differences in values on x between the treatment and control group, and/or the propensity scores are very close to 0 or 1, then the overlap assumption is violated. Results from analyses without enough overlap are usually sensitive to model specifications and can thus be biased (Imbens & Rubin, 2015, p. 309)

$$0 < pr(W_i = 1|X_i = x) < 1 \tag{4.4}$$

There are various situations where the overlap assumption could be violated. It might be that some of the treated or untreated observations do not have matches among the observations with opposite values on the treatment variable, meaning that all observations with some values on the contextual variables have ended in the same group (Morgan & Winship, 2015, p. 148). For instance, maybe no people from some part of a country received the treatment. One or several of the contextual variables (for example geographical location) could then be a deterministic indicator of whether an observation received the treatment or not. In such situations, where the overlap assumption is violated, the researcher will not be able to find consistent and unbiased estimates of the ATE and the CATE (Tibshirani et al., n.d.). That is because, if all observations from one subgroup of the population have ended in the treatment group (or the control group), there must be something important that hinders them from entering the other group so that using the outcome values of observations from other subgroups to estimate the potential outcome for this group is simply not justifiable (Morgan & Winship, 2015, p. 148).

Chapter 5

Data and Operationalizations

This chapter presents an overview of the operationalizations of the variables used in the analysis. The variables are set to measure the concepts presented in Chapter 3. For good measurement validity, the operationalizations should adequately reflect the concepts that they seek to measure (Adcock & Collier, 2001, p. 529).[1] While presenting the operationalizations, I also present the datasets that the variables were derived from, together with the potential strengths or disadvantages of using these data sources. Section 5.3 elaborates on how the treatment effect of rainfall variability on violent, state-based conflict is expected to vary depending on the vulnerability factors. These expectations lay the premise for testing the hypotheses in Chapter 6. A summary of the data and data sources is found at the end of the chapter.

5.1 Structure of the Data

The thesis uses global data on grid cell-year level that covers the time period from 1989 to 2018. Chapter 2 explained that a gap in the literature on the climate-conflict nexus is that it has focused disproportionately on cases from Sub-Saharan Africa. Research on other areas that are vulnerable to climate change and to conflict is needed. Moreover, broadening the view to incorporate data from other world regions would give even more variation in the contextual variables, which could make it easier to investigate how the effect differs. Therefore, I use global data to study how the effect of rainfall variability on violent, state-based conflict varies. The use of global data should complement case studies and other forms of analyses in the strive to understand the climate-conflict nexus.

The data is derived from nine different data sources and contains in all 15 variables and more than two million observations. The original datasets vary in their

[1]A discussion of the validity and reliability of the thesis is given at the end of Chapter 6.

disaggregation both spatially and temporally. All of the data was converted to grid cell-years at a spatial resolution of 0.5 x 0.5 degree (30 arc-minute). Where the data was not originally on grid cell level, all of the cells within the aggregated level were given the same value.

The choice of resolution for the analysis involves some trade-offs. Grid cell level could make it difficult to identify connections between some conflict and climate events. Conflict might erupt in other cells than where the climatic event happens. Yet, this spatial resolution is chosen to ensure comparability. Aggregating the data to the national or sub-national level would have made the units of analysis difficult to compare, at least climatically, as the countries or regions would differ greatly in size. Moreover, as there might be considerable differences within countries for several of the variables, aggregating to a higher level could lead to a loss of information and nuances that might prove important.

The choice of year instead of month as the temporal scale also involves some trade-offs. It leads to some information loss, but makes the data less depended on seasonal dynamics. Conflict patterns could be based on seasonality, as rebel groups often structure their plans based on weather forecasts, because heavy rain or cold winters could make it more difficult to fight (Eriksen & Heier, 2009; Carter & Veale, 2014). Patterns in climate data also depend on the seasons of the year. Conversely, if dealing with data on the monthly level, we might identify correlations between rainfall and conflict that could lead to conclusions in favor of the hypotheses, but that are really caused by strategic considerations based on seasonal weather patterns. Aggregating the data to the yearly level decreases the probability of seasonality leading to wrong conclusions. Moreover, having data on the yearly level limits the amount of observations in the analysis. There are already two million observations in the dataset, and having the data on monthly level would multiply that number of units by 12. Such big datasets are difficult to work with, and although I can draw a sub-sample of the data to make it more manageable, I preserve a greater amount information when having data on a higher temporal level.

A drawback of using data on the yearly level is that it could mean that many of the conflict events happen before the climatic shock that is estimated to have caused it. Furthermore, the effect of an independent variable on the dependent variable might not be effective until some time has passed. To account for this, all of the explanatory variables are lagged one year. Where full years are missing from a variable, the missing information is imputed with non-missing values from the year closest in time.

A missing data analysis is found in Appendix A. In short, except for the missing data caused by aggregating the dependent variable to the yearly level (as will be explained in Section 5.2.2), missing data seems to be missing at random, and should thus not affect the validity of the results.

The full dataset in this analysis contains more than two million observations. Such large datasets are difficult to work with and cause computational difficulties for the machine. To fix this, I have picked a random sample from the original dataset to use in the analysis. The new dataset contains one million observations, which seems to have been the maximum amount for my machine to manage when building causal forests.

5.2 Operationalization of Variables

In the following, the operationalization of the variables will be presented. The section begins with a presentation of the outcome variable, violent state-based conflict, for then to explain how and why the Standardized Precipitation and Evapotranspiration Index (SPEI) is used as an operationalization of scarce and excess precipitation. Thereafter, the operationalization of the contextual variables that determine the level of vulnerability of a society are presented. Lastly, I will briefly explain where the geographical data is derived from.

5.2.1 Outcome Variable - Violent, State-Based Conflict

The outcome variable of the thesis is violent, state-based conflict. The data for the outcome variable is derived from the Uppsala Conflict Data Program Georeferenced Event Dataset (hereafter the UCDP GED) version 19.1, which is a geocoded, global dataset that covers conflict events in the world (excluding Syria) from 1989-01-01 to 2018-12-31 (Högbladh, 2019, p. 3; Sundberg & Melander, 2013). The UCDP GED v 19.1 contains information on conflict events with maximum resolution being the day and individual village/town (Högbladh, 2019, p. 4).

UCDP defines state-based armed conflict as: *"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 a calendar year"* (Högbladh, 2019, p. 28). The actor opposing the state might be another state or any opposition organization or alliance of organizations. The opposing party, if non-governmental, must be organized, have an announced name and use armed force to influence the outcome of the conflict (Högbladh, 2019, p. 28). In that way, spontaneous violence is not included in the dataset (Högbladh, 2019, p. 28).[2]

[2]This choice leans on some theoretical considerations. Organized violence is believed to be theoretically different from semi- or unorganized violence. If no organized actors are identified, it is impossible to distinguish between violent conflict and widespread murder (Sundberg & Melander,

For the purpose of the analysis, the event-data from UCDP GED is coded into a dichotomous variable called "conflict". A value of 1 on the conflict variable suggests that there has been at least one event within the grid cell during the year in focus. A value of 0 on the conflict variable suggests that no state-based violent event has occurred within the grid cell-year. UCDP defines an event as: *"The incidence of the use of armed force by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death in either the best, low or high estimate categories at a specific location and for a specific temporal duration."* (Sundberg & Melander, 2013, p. 524). As apparent from this definition, the UCDP GED estimates death tolls in three different ways. A low or high estimate reflects the lowest or highest number of deaths that is reported. A best estimate is the most reliable estimate that exist among the sources (Högbladh, 2019, p. 5). Hence, a value of 1 on the conflict variable indicates that at least one conflict event resulting in at least one reported death has occurred between an organized armed group and the government of a state within the grid cell-year in question.[3]

State-based violent conflicts are fortunately rare. However, as a consequence, the dependent variable has an imbalanced proportion of classes, where less than 0.01 percent of the observations have the value 1 on the conflict-variable. This could lead the model to struggle with accuracy. To fix this problem, I used a stratified sampling strategy when subsampling the data, so that 25 percent of the data in the subsample have the value 1 on conflict, and 75 percent has the value 0.

Figure 5.1 shows the geographic distribution of violent, state-based conflict among the grid cells that are included in the analysis. Table 5.1 shows the top ten countries with the highest number of violent, state-based conflict events between 1989-2018. The geographic distribution of violent, state-based conflict is quite scattered, but

2013, p. 527). The distinction between the two types of violence is important in order to know that violent events are not random. It might be that more than 25 people were killed in a city during a year, but the murders could be totally unrelated, where the perpetrators did not have any common cause that led them to violence. Still, if the UCDP dataset did not exclude such unorganized violence, such events could have been coded as an active conflict. Hence, UCDP's distinction between organized and unorganized violence ensures that the violence observed is done by groups in opposition to the state and not by random actors with separate reasons for acting violently.

[3]This categorization of the conflict-variable means that some data is lost in the coding process. The dependent variable does not convey information on the amount of violence that occurred within the given grid-year. The value 1 could signify any number of events between 1 and 361 (which is the maximum number of events within one grid-year in the dataset). Moreover, the number of deaths could vary from 1 (according to one of the estimates of the number of deaths) to more than 49 000 (which is the maximum number of deaths based on the best-estimate of deaths in the dataset). Still, there should be a substantial difference between the values of the dependent variable. A death caused by a conflict between an organized group and the state must be considered a sure sign of unrest. Even if the low or best estimate of a death poll do not report any deaths resulting from an event, the fact that some recordings report that a death found place must be considered a sure enough sign of unrest. Although the variable do not convey the variance of intensity between conflicts, it does help answer the research question.

some countries have experienced considerably more conflict than others.

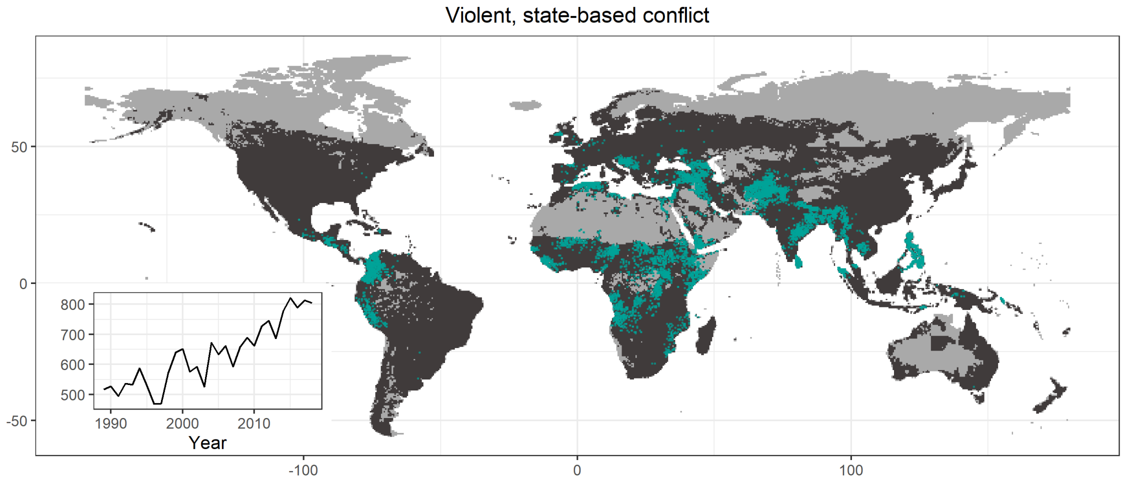


Figure 5.1: Geographic Distribution of Violent, State-Based Conflict
The blue dots show the geographic distribution of areas with at least one violent, state-based conflict between 1989 and 2018. The light grey areas are areas that are not included in the analysis due to missingness on the SPEI variable. The black areas are grid cells included in the analysis, but without any state-based conflict events across the years.

Table 5.1: Top Ten Countries with the Highest Amount of Violent, State-Based Conflicts 1989-2018

	Conflicts	Country
1	2,395	Afghanistan
2	1,938	India
3	1,558	Colombia
4	1,257	Philippines
5	1,056	Turkey
6	830	Algerie
7	716	Pakistan
8	608	Iraq
9	570	Angola
10	516	Sudan

5.2.2 Treatment Variable - Rainfall Variability

Excess and scarce rainfall are operationalized through the Standardized Precipitation and Evapotranspiration Index, hereafter SPEI. It is retrieved from the Climate Research Unit at the University of East Anglia's high resolution gridded dataset, the CRU TS version 4.03. The dataset contains data on weather patterns and anomalies from 1901 to 2018, and was released in May 2019 (University of East Anglia Climatic Research Unit;, Harris, & Jones, 2020). The data is at grid level with a 0.5 degree longitude/latitude information (Harris, Jones, Osborn, & Lister, 2014), which means that it is compatible with the UCDP GED dataset. The CRU TS v. 4.03 contains information on six different climate variables, where the variables on precipitation and evapotranspiration, are used to form SPEI.

SPEI is a standardized measure of the hydrological system that was first proposed in 2010 (Vicente-Serrano, Beguería, & López-Moreno, 2010). SPEI is calculated by subtracting potential evapotranspiration (PET) from precipitation for a specific time frame, and is then compared to the precipitation minus PET in that location of similar time frames in earlier years (Vicente-Serrano et al., 2010, p. 1699).[4] SPEI values above zero refer to anomalously wet conditions, while negative SPEI values refer to anomalously dry conditions.[5]

The use of SPEI as a Treatment Variable

SPEI is often used as a measurement of drought and/or floods. It is important to repeat that the focus of this analysis is not exclusively on such events. Although SPEI measures anomalous climatic situations, its effects on ecosystems are likely to differ depending on the season and climate zone studied, due to differences in actual water supply that are not captured by the index (Zang et al., 2020, p. 322). It is problematic to set a threshold as to what SPEI-values constitute a drought or flood, as this would vary across time and location (Slette et al., 2019, p. 3198). The focus of this thesis is therefore on anomalous rainfall rather than drought and floods exclusively, because that would require knowledge about what SPEI values would create drought and floods in all types of areas included in the analysis, which is beyond the scope of this analysis.

There are several reasons why SPEI is chosen as a measurement of rainfall varia-

[4]There are different ways to calculate PET, but as complex and simple methods yield similar results, the calculation of PET for SPEI is based on the simplest approach which only require data on monthly-mean temperatures (Mavromatis, 2007; Thornthwaite, 1948; Vicente-Serrano et al., 2010, pp. 1700-1701). This is done to ensure PET values from as many regions as possible, as more complex data would be available in fewer regions (Vicente-Serrano et al., 2010, pp. 1700-1701). For details on the calculation of PET, see Vicente-Serrano et al. (2010, pp. 1700-1702).

[5]For more details on the calculation of SPEI, see Vicente-Serrano et al. (2010)

tion. The standardization of the index is one reason, as it makes us able to compare the degree of climatic anomaly across regions with different environmental conditions (Slette et al., 2020, p. e2). However, SPEI is not the only standardized index that measures scarce and excess precipitation. SPEI is based on the Standardized Precipitation Index (SPI), but includes precipitation *and* PET, whereas SPI is only based on precipitation data. The main difference between SPEI and SPI is that the distribution of SPEI converges to a normal distribution, while that is not the case for SPI. This is important if SPEI is to be treated as-if randomly assigned. An important assumption that must hold is that there are no omitted variables that affect both the treatment and the outcome (Gerber & Green, 2012, pp. 39-40). If SPEI is as-if randomly assigned, it must be statistically independent of any observed or unobserved variables, so that any correlation between the treatment and the unobserved is random (Gerber & Green, 2012, p. 95). This is in line with the unconfoundedness assumption mentioned in Chapter 4. The treatment assignment must be independent of any potential values on the outcome. According to the central limit theorem, the sum of several independent distributions for an independent, random variable converges to normal, even if the separate distributions are not normally distributed (Routledge, 2019; Vestby, 2019, Appendix B, p. 3). As SPEI is calculated based on data from both precipitation and temperature, the sum of those distributions should converge to a normal distribution, and could then be treated as-if randomly assigned to units. Vestby (2019, Appendix B, pp. 2-3) proved that this assumption is reasonable for the distribution of SPEI, while SPI, which is only based on precipitation, is not normally distributed.

Specifications of SPEI for this Analysis

SPEI can be calculated for different time scales that capture different types of anomalous weather. For instance, SPEI3, a SPEI estimate with a three-month time scale, uses precipitation and PET information for the month in question and the two months before and compares it with historical data to calculate the SPEI-value. SPEI3 for September 2011 compares the total precipitation minus the total evapotranspiration of July, August and September of 2011 with the precipitation and evapotranspiration of July to September of the 30 years preceding it.

To pick a suitable time frame, one must consider what types of precipitation abnormality is of theoretical interest. As described in Chapter 3, I want to investigate how rainfall variability affects grievances and opportunity costs through reduced economic output and/or reduced living standards. To do that, SPEI must, among other things, capture effects on agricultural output, which can lead to income loss, increased food prices or famine. It would preferably also capture floods that can destroy buildings and excess rainfall that can increase the presence of mosquitoes that carry diseases like malaria. Whether or not it is possible to capture all of these outcomes with the same time scale is questionable. Agricultural output responds to precipitation and evapotranspiration at relatively short time periods, so a time scale between 1 and 6 months is usually suitable to capture impacts on crops (Svoboda, Hayes, & Wood, 2012, p. 6; Vicente-Serrano et al., 2010, p. 1715). Yet, the effect on vegetation of exceptionally dry or exceptionally wet conditions also depends on the type of crops we are looking at. Some plants will die after a few weeks of insufficient water supply, while other plants last longer. In this analysis, SPEI3 is used because the SPEI values at a three month time scale should reflect agricultural drought well, even though some crops will be more affected by longer-term precipitation abnormalities, and because that time frame should also reflect increased standing water, which could increase the frequency of diseases, and floods.

As the unit of analysis for this thesis is grid cell-years, the SPEI3-values had to be aggregated from month to year. This was done by taking the mean SPEI values for all the months in that year weighted by the proportion of the area of the cell that is in growing season out of the total area of the cell that is used for cropland. The weights were given based on the MICRA2000 Cropping Periods List (CPL) version 1.1, which is data giving the start and end month of the growing season for each type of crop in each grid cell (Portmann, Siebert, & Döll, 2010). This information was transformed to give the proportion of the cell that is being harvested each month, by using code inspired by the code for PRIO-GRID v3.beta. (Vestby, Bergstad Larsen, Landsverk, & Tollefsen, 2020). The greater the proportion of the cell that is harvested, the heavier the weight. Weighting SPEI3 by the growing season should be better than simply calculating the mean SPEI3 value within a year. In their study of grid-cells in Africa, Harari and Ferrara (Harari & Ferrara, 2018) found that SPEI values outside of the growing season had no effect on conflict, while there was a significant, positive effect on conflict of dry conditions within the growing season. The growing season is the time of the year where agricultural output should be affected the most by anomalous weather. More weight is therefore given to parts of the year that would affect agriculture the most, which is what is of theoretical interest for the analysis.

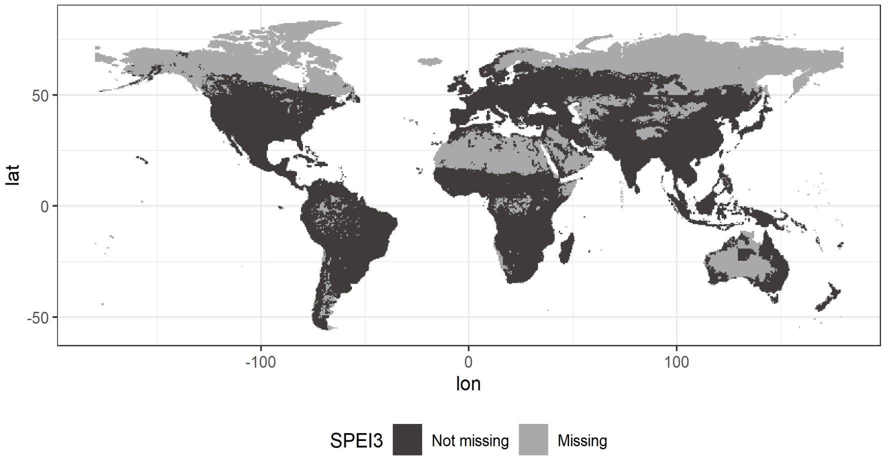
Weighting the mean by the growing season leads to about 40 percent of the SPEI3 values being missing. When aggregating by taking the normal mean, only three percent of the SPEI3 values are missing. The missing information is caused by missing information in the MIRCA2000 dataset. Figure 5.2 shows the geographic distribution of the missing data. Most of the cells with missing information are cells without arable land (such as the Sahara desert and areas with permafrost in Siberia) and that also have few residents. Although throwing the missing data out of the analysis creates some information loss, the grid cells lost are not of great importance for the analysis. Theoretically, I am most interested in areas with a reasonable amount of inhabitants and with arable land, as society in these areas are the ones most likely to be affected by rainfall shocks. As seen in Figure 5.2, few of the cells with missing SPEI3 values have experienced conflict between 1989 and 2018, so removing these cells does not lead to much information loss on the dependent variable. Therefore, I choose to keep the growing season weights, despite the information loss.

The SPEI3 variable is split in two, where one variable consists of only the positive values of SPEI, and the other only of the negative values. Positive and negative SPEI values are unlikely to have the same effect on society, so the splitting is done in order to compare the results. For the binary negative SPEI3 variable, values on the original SPEI3 axis that were between 0 and -1 are set to 0, while values below negative one are set to 1, and indicate dryness. Similarly, for the binary positive SPEI3 variable, values below 1 are set to 0 and values above 1 are set to 1, and indicate abnormally wet conditions.

Lastly, SPEI is likely to be subject to a time trend. The earth gets gradually warmer, and as temperature is part of the calculation of SPEI, this will affect the SPEI values over time. SPEI depicts abnormalities, it gives a value of deviation from normal compared to data from the previous 30 years. If the reference period becomes increasingly warmer, this will affect the SPEI values. To test whether such a trend is found in this analysis, I estimated two OLS models with SPEI3-positive and SPEI3-negative as the dependent variables and years as the independent variables. Time had a small, but significant effect on the SPEI values. I detrended the variables by subtracting the mean effect. Table 5.2 shows the effect of time on SPEI.

5.2.3 Contextual Variables

The contextual variables are derived from a variety of sources. In the following comes an overview of the operationalizations of the variables used to test the hypotheses made in Chapter 3. I also explain how the treatment effect of rainfall on violent, state-based conflict should vary depending on these factors. For simplicity, the chapter is structured following the structure of the vulnerability factors as they are presented in the theoretical framework. When describing the variables, I will also introduce the datasets they are derived from. A summary of the data, operationalizations and datasets they belong to is found in Appendix B.



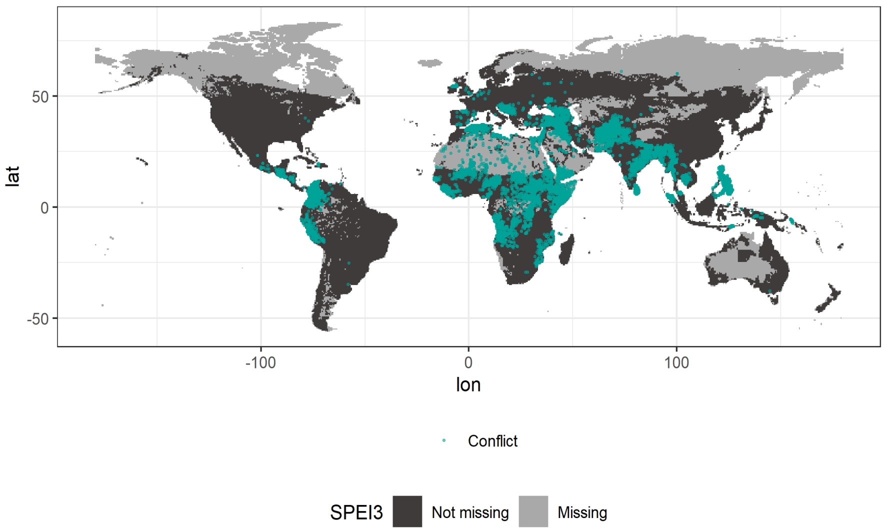


Figure 5.2: Missing SPEI3 Values and State-Based Conflict
The upper map shows the geographic distribution of the missing SPEI3 values. The bottom map shows the geographic distribution of the missing SPEI3 values as well as the geographic distribution of all the state-based conflicts that have occurred between 1989 and 2018. The geographic distribution of the missing information is equal for all years.

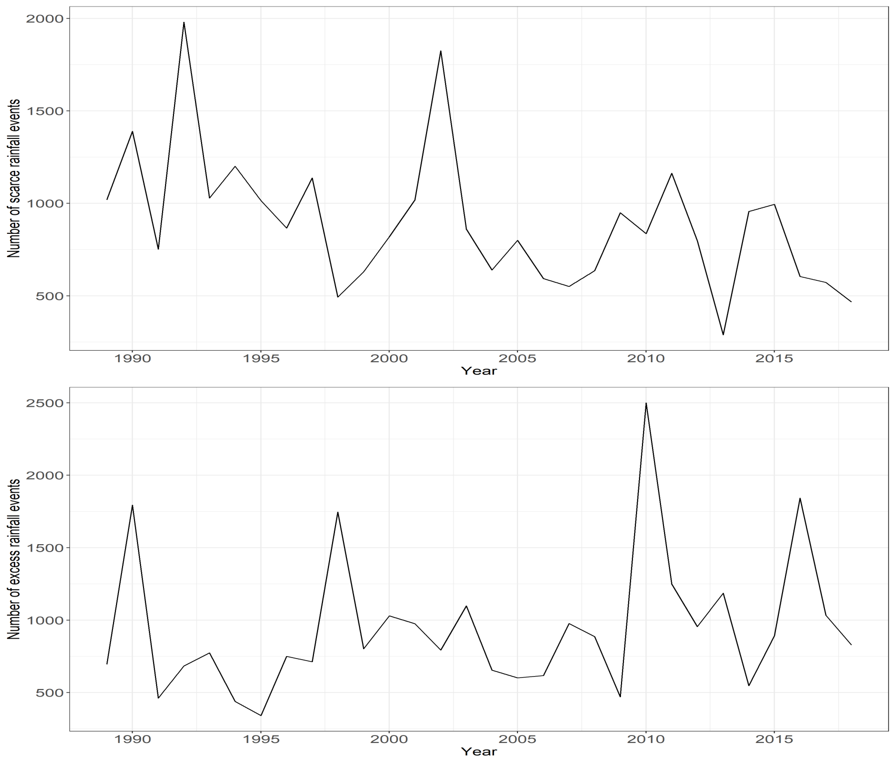
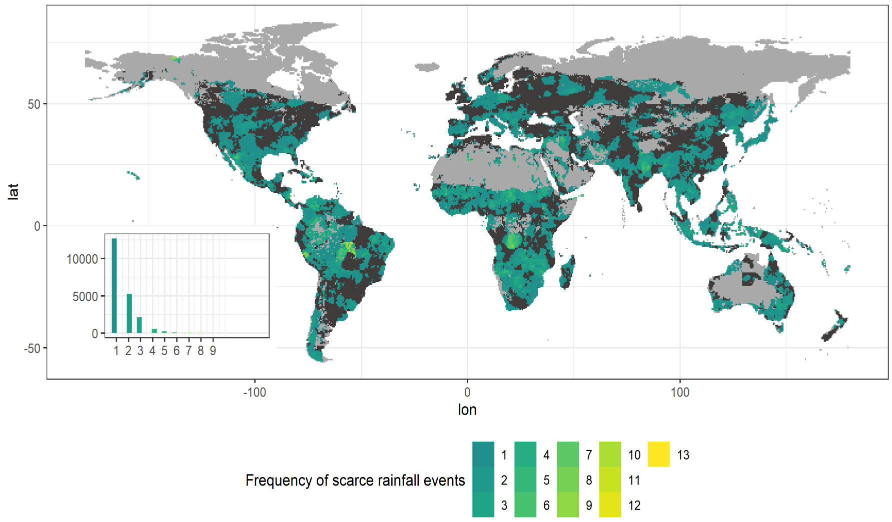


Figure 5.3: SPEI3 Over Time
Number of scarce and excess rainfall events over time. The upper figure shows the frequency of observations with negative SPEI3 value below -1 (and which are thus coded as 1 in the SPEI_neg variable), while the bottom figure shows frequency of observations with a positive SPEI3 value above 1 (and which are thus coded as 1 in the SPEI_pos variable) over the years that are included in the analysis.

Dependency on Rain-Fed Agriculture

As discussed in Chapter 3, the higher the dependency on rain-fed agriculture, the more vulnerable is a community to rainfall shocks. Dependency on rain-fed agriculture is operationalized through three variables. Two of the variables are derived from the PRIO-GRID dataset version 2.0, which covers the world in grid format at 0.5 decimal degrees from 1946 to 2014 (Tollefsen, Bahgat, Nordkvelle, & Buhaug, 2015).[6] The two variables derived from PRIO-GRID are *agri_ih* and *irrig_sum*. The last variable, *empl_agr* is derived from the World Development Indicators by the World Bank. The three variables measure slightly different aspects of dependency on agriculture, so they are all included in the analysis. According to the theoretical expectations, the variables should give the same picture of whether or not dependency on rain-fed agriculture matters in the rainfall-conflict nexus. I will begin by describing the

[6]The PRIO-GRID dataset is a collection of variables derived from a variety of datasets. Some of the variables in this analysis that are not derived from the PRIO-GRID dataset also exist in the PRIO-GRID dataset. However, where updated data exist, I have found the data directly from the original source that PRIO-GRID had used.



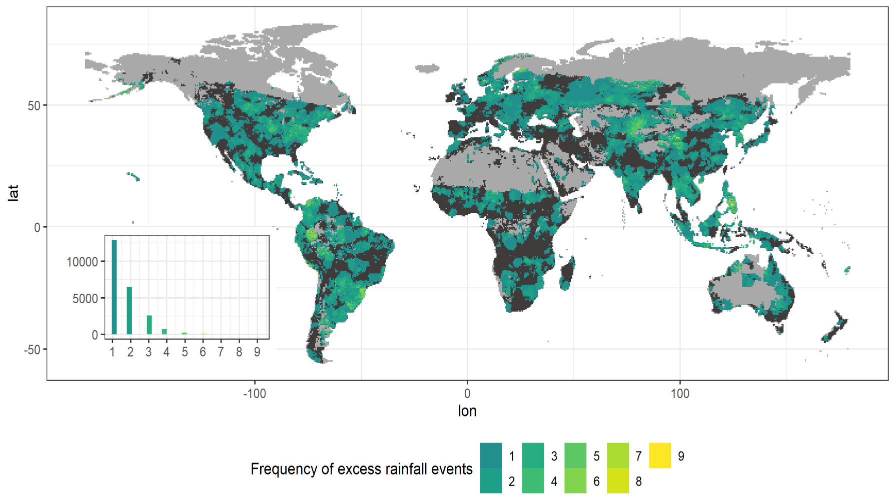


Figure 5.4: Geographical Distribution of SPEI3 Events
Geographical distribution of scarce and excess rainfall events. The upper figure shows the frequency of scarce rainfall events, while the bottom shows that of excess precipitation, according to the colors in the legends. Grey areas are grid cells not included in the analysis due to missing SPEI3 values.

Table 5.2: Effect of Time on the Value of SPEI3

	Dependent variable:	
	SPEI3 negative	SPEI3 positive
	(1)	(2)
Year	0.002***	0.002***
	(0.00003)	(0.00003)
Constant	−4.788***	−2.789***
	(0.067)	(0.068)
Observations	1,157,550	1,157,550
R^2	0.004	0.002
Adjusted R^2	0.004	0.002
Residual Std. Error (df = 1157548)	0.311	0.318
F Statistic (df = 1; 1157548)	4,709.152***	1,941.026***
Note:		*p<0.1; **p<0.05; ***p<0.01

variables from the PRIO-GRID dataset.

agri_ih. This variable gives the percentage of the cell that is covered by agricultural land, and is computed on a ten-year basis, which means that the data in this analysis comes from 1980, 1990, 2000 and 2010 (Tollefsen et al., 2015).[7] The years between the calculation points were originally counted as missing in the dataset. I have imputed the missing data with the values from the most recent calculation point that exist for the cells. That means that values for 2000-2009 are given the value of 2000, while every year after 2010 are given the value of 2010. This is done because it is likely that the values will be quite similar, although it could create some computational mistakes. A high value on this variable means that the cell is widely covered by agricultural land, and thus that it is depended on agriculture.

irrig_sum. This variable gives the total area that is covered by irrigation within the cell (Tollefsen et al., 2015).[8] It is a measurement of the dependency of rain-fed agriculture of a community, as opposed to non-rain-fed agriculture. Data only exist for the years 1990, 1995, 2000, and 2005. Missing values are filled in using the same method as for *agri_ih.* 1989 is given the value of 1990. A high value on this variable means that the grid cell has widespread irrigation systems, and makes the cell less depended on rain for crops to grow.

empl_agri. The World Development Indicators by the World Bank is a set of data with different indicators on development of countries since 1960 (The World

[7] *agri_ih* is derived from the ISAM-HYDE land use data (Meiyappan & Jain, 2012).

[8] The data comes from the Historical Irrigation dataset v.1 (Siebert et al., 2015).

Bank, n.d.). *empl_ agri* gives the percentage of the total employed population that is employed in agriculture. As information from 1989 is missing from the *empl_ agri* variable, the missing information is imputed with the values from 1990. A high value on the *empl_ agri* variable (hence a high percentage of the working population employed in the agricultural sector) imply high dependency on agriculture.

empl_ agri measures a different aspect of agricultural dependency than the *agri_ ih* variable. On the one hand, a cell could have a large area deducted to agriculture, but have few people employed in agriculture who live in that cell. The agricultural land could, for instance, be driven by big companies where the land is cultivated mostly by machines. On the other hand, many people might be employed in agriculture, but in an area that is far away from the grid cell that they live in, so that a rainfall shock that happens within the grid cell of their home would not affect them economically. Figure 5.5 shows a correlation plot between the variables. The correlations are quite small, which could mean that they capture different aspects of dependency on rain-fed agriculture. One could have created an index based on these variables (and maybe included other relevant variables as well), but that would make it impossible to reveal possible *differences* between the effect of these variables. It might be that only some of the variables affect the climate-conflict nexus, which would also be a result of theoretical interest. I thus include *agri_ ih* and *empl_ agri* as complementary variables that together with *irrig_ sum* should capture the concept of dependency on rain-fed agriculture, but where differences in heterogeneity could be found due to the low correlation.

Temperature

The temperature variable comes from the same dataset as SPEI, namely the CRU TS version 4.03. It is weighted by the growing season and aggregated to the yearly level similar to SPEI. A high value on the temperature variable means that the grid cell had a high mean temperature within the year in question. Following the discussion from Chapter 3, high temperatures could make communities more vulnerable to rainfall-induced conflicts, because certain diseases more easily spread in high temperatures, and because people are supposed to be more aggressive when temperatures are high.

Population Density

The population density variable is taken from the Gridded Population of the World (GPW) version 4, which contain gridded data on the counts and density of the human population of the world.[9] As the data is on grid-level, it is compatible with the data

[9]The GPW data is produced by the Socioeconomic Data and Applications Center (SEDAC) at NASA (Center for International Earth Science Information Network, Columbia University, 2018).

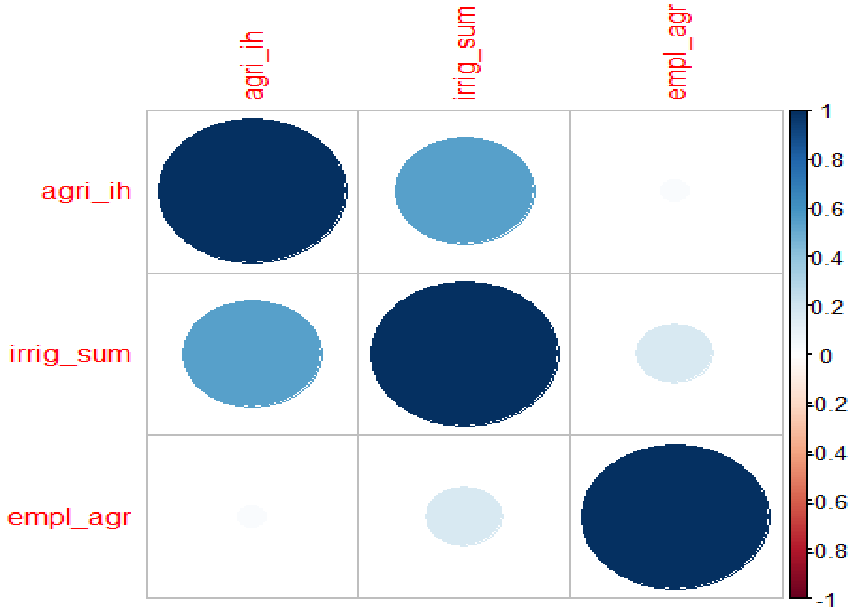


Figure 5.5: Correlation Plot of Indicators of Dependency on Rain-Fed Agriculture

from UCDP GED, PRIO-GRID and CRU. The *pop* variable in this analysis signifies the number of people per square kilometer who inhabit each grid cell each year. Rather than using the population count, the population density reveals information on the compactness of the area where people live. As all observations (grid cell-years) have approximately the same size, the variable values also give a picture of the size of the population in that cell. As the GPW v4 only contains information from 1990 to 2018, missing information for 1989 is filled in with values from 1990. Following the discussion in Chapter 3, a high population density should make people more vulnerable to rainfall-induced conflict.

Globalization and Urbanization

Globalization is operationalized by the 2019 KOF Globalization Index, which is a composite index based on 43 indicators that together measure the level of globalization. It covers 203 countries from 1970 to 2017 (Gygli, Haelg, Potrafke, & Sturm, 2019).[10] As the index does not yet have globalization measures for 2018, the missing

GPW v.4 consists of 9 datasets, collectively referred to as the Revision 11. The data in this analysis come from the Population Density dataset from the collection. Data on population density is calculated at a five-year interval (Center for International Earth Science Information Network, Columbia University, 2018).

[10]The KOF Globalization Index distinguishes between de facto and de jure globalization and includes indicators on economic, social and political globalization (Gygli et al., 2019, p. 545). The index was originally introduced in 2006 (Dreher, 2006), with a revised version finished in 2018 (Gygli et al., 2019). It builds on the following definition of globalization: *Globalization describes the process*

information is filled with values from 2017 to fit in to the analysis. High values on the globalization variable (*global_ ind)* means a high level of globalization, and thus lower vulnerability, according to the discussion of globalization in Chapter 3.

Two variables, *capdist* and *ttime_ mean*, are set to measure urbanization, both derived from the PRIO-GRID dataset. *capdist* gives the spherical distance in kilometers from the center of the grid cell to the capital of the country it belongs to (Tollefsen et al., 2015).[11] Together with *ttime_ mean* it gives a picture of how remote the grid cell is to urban centers and the capital. Since this variable measures distance to the capital, it also says something about how physically close grid cells are to the governmental power. The variable was log-transformed to adjust for skewness. High values on *capdist* means that the distance to the capital is big, and thus imply higher levels of vulnerability. *ttime_ mean* gives the average travel time in minutes of a grid cell to the nearest major city with more than 50 000 inhabitants (Tollefsen et al., 2015).[12] It was log-transformed to adjust for skewness. High levels on *ttime_ mean* means long distance to the nearest urban center and thus higher vulnerability.

Figure 5.6 shows a correlation plot of the two variables that operationalize urbanization. The correlation between the two is quite modest. As for the "dependency on rain-fed agriculture" variables, *capdist* and *ttime_ mean* are included as complementary variables to measure urbanization, but might turn out to have different impacts on the climate-conflict nexus, due to the low correlation.

Regime Type

Regime type is operationalized through the liberal democracy variable *libdem*, which was retrieved from V-Dem. V-Dem's liberal democracy index measures the extent to which individual and majority rights are protected against the tyranny of the state or the majority (Coppedge et al., 2019, p. 40). Indicators on the level and efficiency of constitutionally protected civil liberties, rule of law, independent judiciary and checks and balances, as well as an electoral democracy index (which measures the level to which rulers are responsive to their citizens) together constitute the index (Coppedge et al., 2019, p. 40). Although there are many ways to theoretically capture the concept

of creating networks of connections among actors at intra- or multi-continental distances, mediated through a variety of flows including people, information and ideas, capital, and goods. Globalization is a process that erodes national boundaries, integrates national economies, cultures, technologies and governance, and produces complex relations of mutual interdependence (Clark, 2000; Gygli et al., 2019, p. 546; Norris, 2000).

[11]The variable is based on the coordinate pairs of capital cities from the cShapes dataset (Tollefsen et al., 2015; Weidmann, Kuse, & Gleditsch, 2010).

[12]The variable is derived from a global high-resolution raster map of accessibility (Uchida & Nelson, 2009)

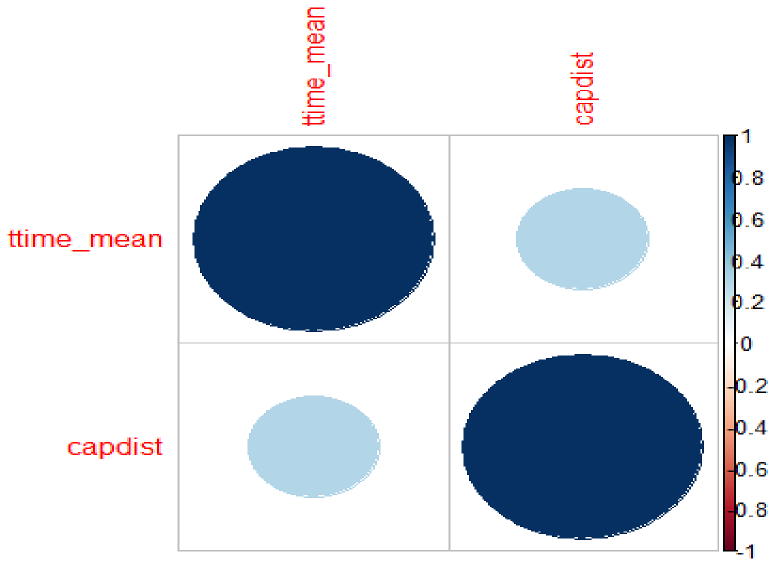


Figure 5.6: Correlation Plot of Indicators of Urbanization

of democracy, the liberal democracy index seems adequate.[13] The liberal democracy index is on the country-year level, so each grid cell within a country within a given year is given the same value. High values on the *libdem* scale means high democracy and presumable low vulnerability.

Good Governance

Good governance is a complex concept that requires more than one variable to measure. Indeed, good governance has 8 characteristics: participatory, consensus oriented, accountable, transparent, responsive, effective and efficient, equitable and inclusive and follows the rule of law (United Nations Economic and Social Commission for Asia and the Pacific, 2009). I have chosen to measure the concept through three variables which I believe capture different aspects of good governance. In the following comes an explanation of this choice.

The first variable that is part of the operationalization of good governance is the Subnational Human Development Index (SHDI) version 3.0, which contains information on human development in 1730 sub-national regions within 161 countries from

[13]Participatory, deliberative and egalitarian democracies, which are other indexes included in V-Dem, measure, respectively, the level of active participation by citizens, the level to which the common good motivates political decisions and the level of egalitarian treatment of the citizens (Coppedge et al., 2019, pp. 40-41) will not be included here. The first two of the indexes will be omitted as the liberal democracy index seem to capture a more fundamental democratic value, which is what is of interest here (it measures people's ability to influence their leaders), while the third concept will be somewhat included in the EPR

1990-2017 (*Subnational HDI History - Global Data Lab*, n.d.).[14] SHDI is constructed based on three dimensions: Education, health and standard of living. In this analysis, only the total SHDI score is included. High SHDI scores could be argued to be the output of good governance. Only accountable, transparent, responsive, effective, efficient and inclusive governments could manage to give its population an all over good education, health and standard of living. Therefore, SHDI should be a good operationalization that captures many aspects of the good governance concept. SHDI is not on grid-level, so each cell within one of the SHDI-regions is, for the purpose of this analysis, given the same SHDI score. Missing information for the years 1989 and 2018 are imputed with the SHDI values for the preceding or subsequent year.

In order to deliver services connected to good governance, the state must also have the economic capability to govern well. To continue on the example of good governance given in Chapter 3, in order to be able to compensate income loss induced by a rainfall shock, the state must not only have the institutional capacity to compensate, but also the money to do so. Therefore, I have included the gross domestic production per capita (GDP) as an aspect of good governance. The *gdp* variable is retrieved from the Varieties of Democracy database (hereafter V-Dem) v9, which was published in April 2019 (Coppedge et al., 2019, p. 326). The variable was log-transformed to adjust for skewness. V-DEM do not have information on GDP for 2017 and 2018, so these years are given the values of 2016.

The last variable that is used to operationalize good governance is the level of political exclusion of ethnic groups (hereafter called *excluded*). The Family of Ethnic Power Relations (EPR) Datasets version 2014 offers various data on ethnicity (Vogt et al., 2015). The *excluded* variable is based on information from two of the datasets in the EPR Family: the EPR Core Dataset and the GeoEPR Dataset. Both datasets have been updated in 2019 and cover the time period 1946-2017. The EPR Core Dataset contains information on the politically relevant ethnic groups and their access to power in all independent countries of the world that has a population of at least 250,000 and where ethnicity has been politicized (Vogt et al., 2019). The GeoEPR v.2 is a geocoded dataset that codes the settlement patterns of politically relevant ethnic groups in independent states (Vogt et al., 2019).

The *excluded* variable counts the number of excluded ethnic groups settled in the grid cell within a given year. If an ethnic group is coded as "powerless" or

[14]The third version of the index was released in September 2019 and could be viewed as an extension of the Human Development Index (HDI) which has been published annually since 1990 by the United Nations Development Program (UNDP)(Smits & Permanyer, 2019, p. 2). The Subnational Human Development Index (SHDI) is a yearly HDI at the sub-national level. The SHDI scores were computed based on data from statistical offices and from survey and census datasets from the Area Database of the Global Data Lab (Smits & Permanyer, 2019, p. 1).

"discriminated" in the "status"-variable in the EPR Core, it is counted as an excluded group. NA values signify either that there is no information on the inclusion of ethnic groups within that grid cell, or that ethnicity has not been politicized in that cell-year. Since equity and inclusiveness are characteristics of good governance, it makes sense to include *excluded* as part of the measurement of good governance. The *excluded* variable is constructed based on the code from the PRIO-GRID v3.beta. (Vestby et al., 2020). Cell-years for 2018 are given the value of 2017.

High levels of the SHDI and GDP variables imply low vulnerability to climate-induced conflicts. High levels of *excluded*, on the other hand, implies a high number of politically excluded ethnic groups and thus high vulnerability. Figure 5.7 shows a correlation plot of the three variables. There is very low correlation between *excluded* and the other variables, whilst *shdi* and *gdp* correlate highly. This is not so surprising, since political exclusion captures a slightly different aspect of good governance than the remaining variables, while SHDI and GDP are both to some degree measures of institutional capacity. Other variables might also be relevant for measuring good governance, but as these three variables cover most of the characteristics of the concept, they should be able to indicate whether good governance plays any part in making the treatment effect heterogeneous. Future researchers should investigate the possibility and utility of making an index of good governance that could incorporate other variables in the mix. That, however, needs careful statistical and theoretical considerations of whether the variables could be justified to take part in an index, and has not been done here.

Prevalence of Non-State Conflict

The last contextual variable that needs to be operationalized is prevalence of non-state conflict. This variable is derived from the same dataset as the outcome variable, thus UCDP GED v 19.1 dataset (Sundberg & Melander, 2013). UCDP defines a non-state conflict as “the use of armed force between two organized armed groups, neither of which is the government of a state, which results in at least 25 battle-related deaths in a year" (Högbladh, 2019, p. 29). It is coded similarly to the outcome variable, meaning that all grid cell-years with at least one conflict *event* (at least 1 direct death caused by the conflict between the opposing parties) are given the value 1 on the variable, while the rest are set to 0. In accordance with the discussion from Chapter 3, a value of 1 on the *non_state_conflict* variable indicates high vulnerability to rainfall-induced violent conflict.

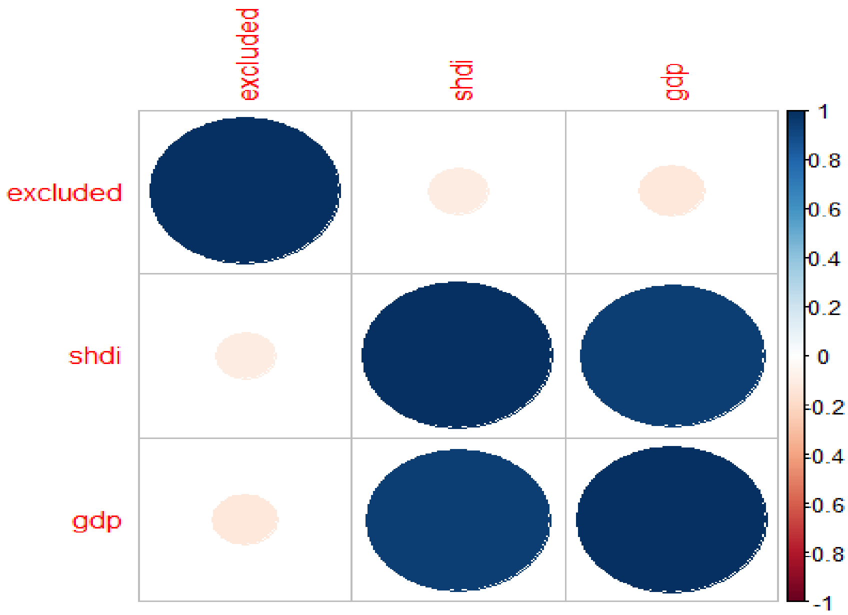


Figure 5.7: Correlation Plot of Indicators of Good Governance

5.2.4 Geographical Data

As mentioned, the data described until now have all been on either some grid cell level or on subnational or national scales. To fit all the data to the same spacial scale (0.5 x 0.5 degree grid cell resolution), I have used grid cell information and a country variable, *gwno*, derived from the PRIO-GRID v.2 dataset. The country variable builds on the Gleditsch and Ward system membership list and cShapes geometry (Gleditsch & Ward, 1999; Tollefsen et al., 2015; Weidmann et al., 2010). Grid cells that fall within two countries are assigned to the country that cover the largest proportion of that cell. Cells within the territory of non-independent states are coded as not available (NA) (Tollefsen et al., 2015). The code is assigned to cells based on the country the cell belongs to on December 31 each year.

5.3 Summary of Data and Theoretical Expectations for Analysis

Table 5.4 shows some descriptive statistics of the variables for the analysis, based on data from the full dataset. For a summary of the different sources the data was derived from, see Appendix B. Table 5.3 shows a summary of the variables used to operationalize the contextual factors from Chapter 3. The table also shows the theoretical expectations of the factors that make observations vulnerable to climate-induced conflict. The last column indicates whether a high or a low value on the variable used to measure a contextual factor indicates vulnerability to rainfall-induced conflicts. When referring to vulnerability in the figure, it is meant vulnerable to violent, state-based conflicts erupting in the aftermath of an anomalous rainfall event (i.e. either scarce or excess precipitation). As an example, consider the contextual factor "dependency on rain-fed agriculture". Three variables are used to operationalize the factor, as depicted in the second and third columns of Table 5.3. High levels of dependency on rain-fed agriculture make communities vulnerable to rainfall-induced conflict. For the *agri_ih* and *empl_agr* variables, high values indicates high vulnerability, because high values on these variables means that the observation has a big percentage of agricultural land in its cell or a high percentage of its population employed in agriculture, respectively. For the *irrig_sum* variable, however, high values indicate lower dependency on rain-fed agriculture (because the variable gives the total area of cells covered by irrigated agriculture) and therefore lower vulnerability to rainfall-induced conflict. The "indication of vulnerability" column presents the expectations of how the treatment effect should vary across the variables, if the theoretical expectations are correct. If the treatment effect depends on communities' dependency on rain-fed agriculture, then observations with high values on the *agr_ih* and *empl_agr* and with low values on the *irrig_sum* should experience bigger effects of rainfall variability on violent, state-based conflicts, than those with other values on these variables.

Table 5.3: Summary of Variables and Theoretical Expectations

Contextual factor	Theoretical expectation	Operationalization	Variable name in dataset	Indication of vulnerability
Dependency on rain-fed agriculture	High levels of dependency on rain-fed agriculture make communities vulnerable	Percentage of cell covered by agricultural land	*agri_ih*	High value
		Total area of cell covered by irrigated	*irrig_sum*	Low value
		Percentage of employed population employed in agriculture	*empl_agr*	High value
Temperature	High temperatures make communities more vulnerable	Temperature	*temp*	High value
Population density	High population density indicates higher vulnerability	Population density (number of people per square kilometer)	*pop*	High value
Globalization and urbanization	Globalization and urbanization makes communities less vulnerable	KOF globalization index	*global_ind*	Low value
		Distance to capital (in kilometers)	*capdist*	High value
		Average travel time (in minutes) to nearest urban center	*ttime_mean*	High value
Regime type	Strong democracies are less vulnerable	Liberal democracy index	*libdem*	Low value
Good governance	Good governance makes communities less vulnerable	Subnational Human Development Index (SHDI)	*shdi*	Low value
		Gross domestic production per capita (GDP)	*gdp*	Low value
		Level of political exclusion of ethnic groups	*excluded*	High value
Prevalence of non-state conflict	Prevelance of non-state conflict makes communities more vulnerable	Non-state conflict	*non_state_conflict*	High value

Table 5.4: Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Non-State Conflict	1,944,525	0.002	0.048	0	0	0	1
SPEI3	1,155,180	0.022	0.537	−3.847	−0.333	0.376	2.189
SPEI3 positive	1,155,180	0.222	0.318	−0.002	−0.002	0.374	2.187
SPEI3 negative	1,155,180	−0.204	0.311	−3.850	−0.336	−0.002	−0.002
Temperature	1,159,080	16.713	9.260	−18.972	9.307	25.274	34.886
Agricultural area in cell (percentage)	1,798,530	9.998	18.888	0.000	0.000	9.830	100.000
Total are covered by irrigation in cell	1,940,535	4,503.331	17,369.330	0.000	0.000	704.730	265,679.900
Distance to capital (km)	1,944,525	6.992	1.112	1.020	6.236	7.926	8.982
Travel time to nearest urban center (min)	1,941,495	6.349	1.226	1.812	5.411	7.303	10.310
Population	1,941,615	51.692	245.106	0.000	0.069	21.441	22,112.020
Employment in agriculture	1,932,955	21.902	22.496	0.059	3.491	36.595	92.303
Number of excluded groups	1,368,093	0.328	0.632	0.000	0.000	1.000	5.000
SHDI	1,706,820	0.706	0.168	0.166	0.622	0.844	0.975
Liberal Democracy Index	1,934,338	0.436	0.308	0.009	0.142	0.785	0.914
Globalization index	1,920,534	62.080	15.614	19.200	50.218	74.334	91.313
GDP	1,889,367	9.396	1.122	4.905	8.792	10.358	11.959

Chapter 6

Analysis

This chapter presents the results from the analysis, and test the hypotheses made in the theoretical framework in Chapter 3. There, I explained that the effect of rainfall variability on violent, state-based conflict is likely to be indirect and heterogeneous. I made 3 hypotheses about the nature of the heterogeneity of the treatment effect. Table 5.3 summarized the theoretical expectations of how that effect should vary depending on the operationalizations of the factors of vulnerability introduced in the theoretical framework. These lay the premise for the testing of the hypotheses in this chapter.

Two causal forest models were made based on the operationalizations from Chapter 5, one with negative SPEI3 values (CF-neg) and one with positive SPEI3 values (CF-pos) as the treatment variable. Both forests have a binary treatment (SPEI3) and outcome variable (violent, state-based conflict). The models contain 13 variables in addition to the treatment and outcome, where all variables that vary over time are lagged one year.[1] All tunable parameters are tuned automatically by the causal forests.[2]

The chapter begins with a test of the model assumptions for the causal forest. I find that there is weak overlap in the data, which means that I have to exclude

[1]The variables that are not lagged are the geographic variables (grid cell number, latitude, longitude and country code) and travel time to nearest urban center, which is a static variable

[2]There are several parameters that could be tuned in a causal forest. *mtry*, which is the number of variables that the machine gets to choose from at each split in the tree, is the mean of a poisson distribution, where a new number of variables is chosen before each split (Tibshirani et al., n.d.). The forests were built with an *mtry* at 21, and a minimal of 5 observations with each value of the treatment in each final leaf. The trees are built honestly, where half of the subsample is used to pick split points in the model, while the other half is used to estimate effects. The number of trees in the forest had to be set manually. To be able to estimate confidence intervals, a large amount of trees are needed (Tibshirani et al., n.d.). The reliability of the models usually improves with the number of trees, until it reaches a plateau where improvements are no longer made. The default of the *causal.forest* R-function is 2000. I tried using a greater amount of trees in the forest than the default, but as a shift from 2000 to 5000 trees considerably increased the run time, keeping the default of 2000 seemed reasonable

observations with propensity scores very close to 0 or 1. Thereafter, I begin the hypothesis testing by investigating the first hypothesis, H1, which states that there is heterogeneity in the treatment effect of excess or scarce rainfall on violent, state-based conflict. I conduct an omnibus test of treatment effect heterogeneity, which shows signs of considerable heterogeneity in the data. However, due to the weak overlap in the data, more investigation is needed before concluding that the effect is heterogeneous. The chapter thus proceeds by studying variations in the conditional average treatment effect (CATE). An estimate of the best linear projection of the CATE shows signs of heterogeneity across some of the contextual variables. Next, I calculate the CATE for specific subgroups with high and low values on the contextual variables. The results show that neither excess nor scarce precipitation have large effects on rainfall variability in any contexts, but that some vulnerability factors have impacts on the size of the effects. The factors that impact the effect of scarce precipitation are others than those that impact the effect of excess precipitation.

6.1 Validation of the Model Assumptions

Before investigating whether and how the contextual variables might modify the effect of rainfall variability on violent conflict, I must assess whether the model assumptions hold. As mentioned in the methodological framework, there are two assumptions that need to hold for the results from the analysis to be trusted. The first of them, the unconfoundedness assumption, is impossible to test, but relies on theory (we must assume that we have included all confounding variables in the models).

The second assumption, the overlap assumption, can be evaluated through an assessment of the propensity scores of the sample (Imbens & Rubin, 2015, pp. 309, 314-317). As discussed in Chapter 4, propensity scores give the probability of an observation being assigned the treatment given a set of observed contextual variables. If an observation has a propensity score of 0, that means that it has no probability of being assigned the treatment, while a value of 1 indicates the opposite. Hence, if the propensity scores are very close to 0 or 1, the overlap assumption is violated.

Figure 6.1 shows the distribution of the propensity scores for the models. There is considerable lack of overlap in both the CF-neg and the CF-pos, because the distributions are skewed towards 0. Weak overlap means that one or several of the contextual variables are able to predict the observations' values on SPEI3. In other words, since the distribution of the propensity scores for being treated with both positive and negative SPEI3 are very close to 0, the assumption that treatment assignment of SPEI3 is independent of the contextual variables is less likely. This means that estimates based on this data will be imprecise.

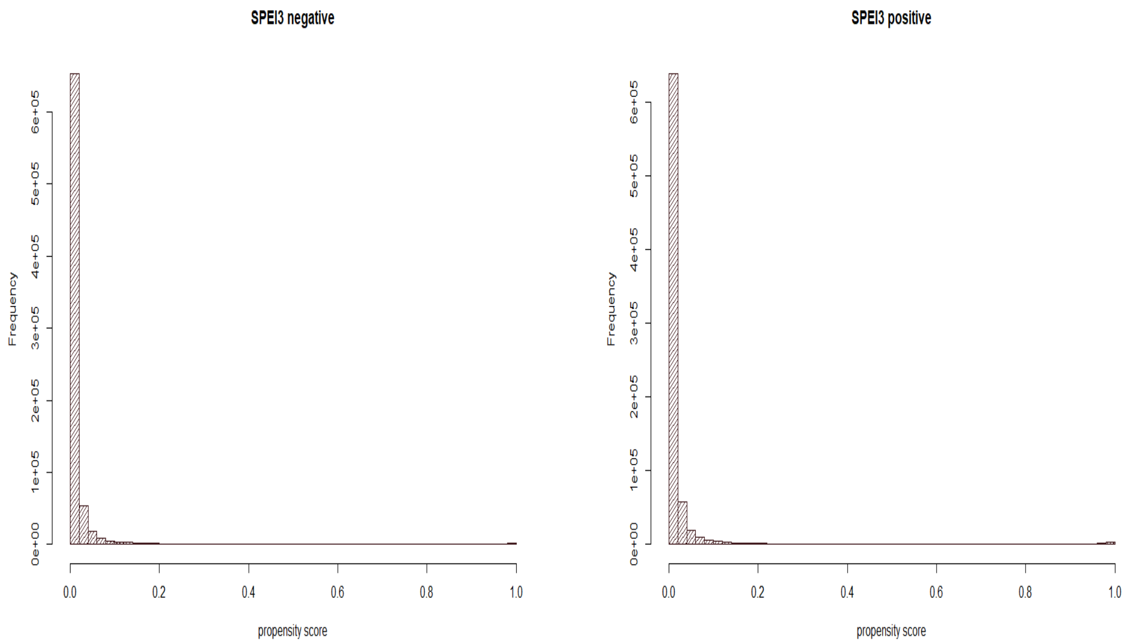


Figure 6.1: Test of Overlap Assumption

In an attempt to understand why the distributions of the propensity scores are so skewed, and hence what it is that predicts SPEI3 so accurately, I investigate whether the treatment and control groups differ in their covariate distributions. Table 6.1 and 6.2 show the mean and standard deviation values on the covariates for observations in the treatment (SPEI3 negative = 1 or SPEI3 positive = 1) and control group (SPEI3 negative = 0 or SPEI3 positive = 0) for the two models. If one of the contextual variables predict the treatment of SPEI3, there should be a considerable difference between the values of the observations in the treatment and control group on that contextual variable. However, Table 6.1 and 6.2 do not show a significant difference between the distributions of the groups. This is peculiar. The contextual variable with the biggest difference between the mean values for the treatment and control groups is *irrig_sum*. Yet, although the mean value on *irrig_sum* for the two groups differ, the standard deviations are so large that the difference is not significant. Moreover, the values on this variable have a bigger range than most of the other variables, so the fact that the difference in mean values for the treatment and control group is bigger than for other covariates need not mean much.

Table 6.3 and 6.4 show the distributions of the treatment and control group on the contextual variables for observations with propensity scores between 0.1 and 0.9. This data hence have relatively more overlap than the total data, because the propensity scores are further away from 0 and 1. Since the treatment and control groups' mean values on the contextual variables were quite balanced when looking at the total data,

Table 6.1: Mean and SD, SPEI3 negative

	label	levels	0	1	diff
1	Total N (%)		969471 (96.9)	30529 (3.1)	
2	temp	Mean (SD)	17.4 (9.0)	20.8 (7.9)	3.4
3	agri_ih	Mean (SD)	17.5 (22.3)	15.7 (21.4)	-1.8
4	irrig_sum	Mean (SD)	9395.8 (24204.5)	8825.7 (24725.4)	−570.1
5	capdist	Mean (SD)	6.4 (1.1)	6.4 (1.0)	0
6	ttime_mean	Mean (SD)	5.7 (0.9)	5.8 (0.9)	0.1
7	pop	Mean (SD)	109.6 (365.5)	108.1 (457.0)	1.5
8	empl_agr	Mean (SD)	31.0 (24.1)	35.9 (24.9)	4.9
9	excluded	Mean (SD)	0.4 (0.7)	0.5 (0.8)	0.1
10	shdi	Mean (SD)	0.6 (0.2)	0.6 (0.2)	0
11	libdem	Mean (SD)	0.4 (0.3)	0.4 (0.3)	0
12	global_ind	Mean (SD)	56.4 (15.9)	52.2 (15.8)	-4.2
13	gdp	Mean (SD)	8.9 (1.2)	8.7 (1.2)	0

Table 6.2: Mean and SD, SPEI3 Positive

	label	levels	0	1	difference
1	Total N (%)		965333 (96.5)	34667 (3.5)	
2	temp	Mean (SD)	17.5 (9.0)	17.4 (9.5)	0.1
3	agri_ih	Mean (SD)	17.4 (22.3)	17.6 (22.3)	0.2
4	irrig_sum	Mean (SD)	9415.0 (24314.6)	8357.5 (21418.9)	1057.5
5	capdist	Mean (SD)	6.4 (1.1)	6.4 (1.0)	0
6	ttime_mean	Mean (SD)	5.7 (0.9)	5.7 (0.9)	0
7	pop	Mean (SD)	109.5 (367.0)	111.7 (412.0)	2.2
8	empl_agr	Mean (SD)	31.3 (24.2)	28.5 (21.5)	2.8
9	excluded	Mean (SD)	0.4 (0.7)	0.4 (0.7)	0
10	shdi	Mean (SD)	0.6 (0.2)	0.7 (0.2)	0
11	libdem	Mean (SD)	0.4 (0.3)	0.4 (0.3)	0
12	global_ind	Mean (SD)	56.2 (16.0)	58.9 (14.5)	2.8
13	gdp	Mean (SD)	8.9 (1.2)	9.1 (1.0)	0.1

it is not surprising that the balance does not improve much when studying data with more overlap. Indeed, for some of the contextual variables, the difference in means is actually greater after removing the data with propensity scores close to 0 and 1.

Table 6.3: Mean and SD, SPEI3 Negative, With Overlap

	label	levels	0	1	difference
1	Total N (%)		7756 (42.6)	10471 (57.4)	
2	temp	Mean (SD)	20.5 (8.2)	20.0 (8.1)	-0.5
3	agri_ih	Mean (SD)	12.7 (20.4)	15.0 (21.6)	2.4
4	irrig_sum	Mean (SD)	8095.0 (23490.6)	9015.7 (24774.7)	920.6
5	capdist	Mean (SD)	6.6 (0.9)	6.6 (1.0)	-0.1
6	ttime_mean	Mean (SD)	6.0 (1.0)	5.9 (1.0)	-0.2
7	pop	Mean (SD)	59.4 (210.7)	76.0 (276.5)	16.6
8	empl_agr	Mean (SD)	29.8 (24.3)	30.6 (24.4)	0.8
9	excluded	Mean (SD)	0.6 (0.8)	0.5 (0.8)	0
10	shdi	Mean (SD)	0.6 (0.2)	0.6 (0.2)	0
11	libdem	Mean (SD)	0.4 (0.3)	0.4 (0.3)	0
12	global_ind	Mean (SD)	55.6 (14.9)	55.5 (15.5)	-0.1
13	gdp	Mean (SD)	8.9 (1.2)	8.9 (1.2)	0

Table 6.4: Mean and SD, SPEI3 Positive, With Overlap

	label	levels	0	1	difference
1	Total N (%)		9496 (42.4)	12904 (57.6)	
2	temp	Mean (SD)	14.9 (9.3)	15.4 (9.4)	0.5
3	agri_ih	Mean (SD)	18.0 (22.9)	18.2 (23.3)	0.2
4	irrig_sum	Mean (SD)	7488.7 (19197.1)	8021.0 (20653.4)	532.2
5	capdist	Mean (SD)	6.8 (1.0)	6.7 (1.0)	-0.1
6	ttime_mean	Mean (SD)	5.7 (1.0)	5.7 (1.0)	0
7	pop	Mean (SD)	65.2 (227.8)	76.4 (253.1)	11.2
8	empl_agr	Mean (SD)	21.7 (20.5)	24.2 (21.2)	2.5
9	excluded	Mean (SD)	0.4 (0.6)	0.4 (0.7)	0
10	shdi	Mean (SD)	0.7 (0.1)	0.7 (0.2)	0
11	libdem	Mean (SD)	0.5 (0.3)	0.4 (0.3)	0
12	global_ind	Mean (SD)	62.1 (13.8)	61.1 (14.2)	-1
13	gdp	Mean (SD)	9.4 (1.0)	9.3 (1.0)	-0.1

Figure 6.2 shows the geographic distribution of the data with propensity scores above 0.1 and below 0.9, thus with relative overlap. The orange dots are grid cells with propensity scores between 0.1 and 0.9, while the black areas are grid cells included in the analysis, but with propensity scores below 0.1 and above 0.9. From studying the maps in Figure 6.2, it seems that the data with propensity scores close to 0 and 1 (thus with weak overlap) is spread out across the globe, although there are particularly

many in Africa when positive SPEI3 is the treatment variable. Moreover, it seems that the data with very low and very high propensity scores depend on country affiliation, as some countries such as Chile, Angola, Nigeria, South Sudan, Norway, Germany and Papa New Guinea are partially or completely removed when removing cell-years with propensity scores below 0.1 and above 0.9. It is curious that the propensity of experiencing weather such as scarce or excess rainfall varies with state borders. Unfortunately, it is beyond the scope of this thesis to further examine why the distribution of propensity scores is so skewed. The findings here should, however, serve as a warning to anyone using SPEI3 as their treatment variable. The research field needs to understand what it is that predicts assignment of SPEI3 before basing conclusions on analysis with SPEI3 as the treatment.

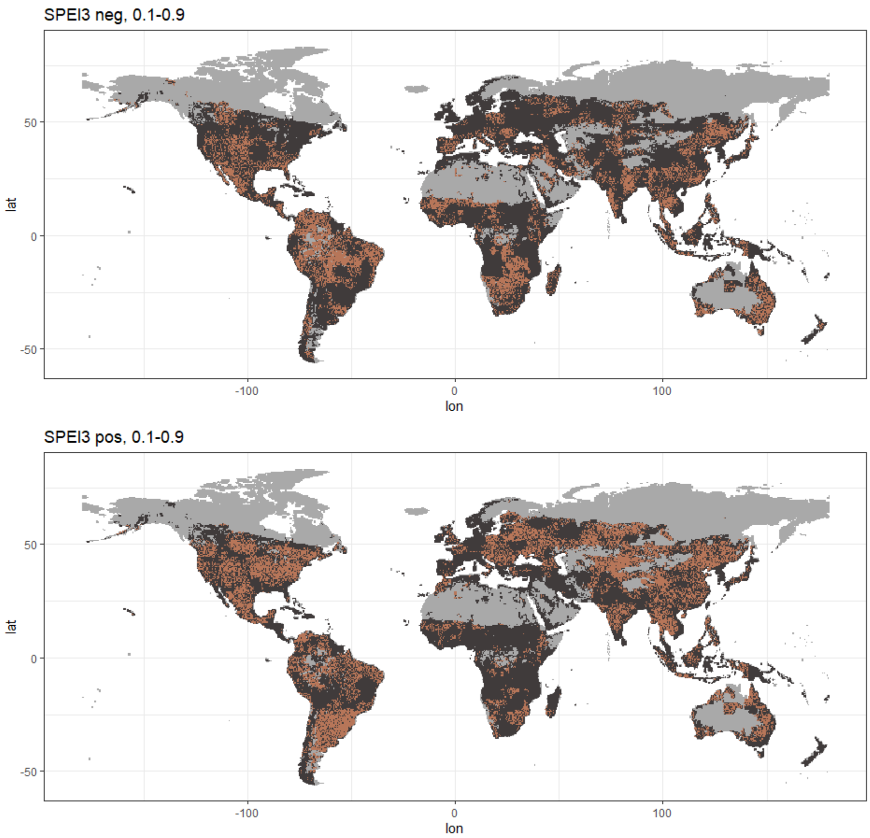


Figure 6.2: Geographic Distribution of Data With Overlap
These maps show the geographic distribution of data with propensity scores above 0.1 and below 0.9 for negative and positive values of SPEI3. The orange dots are the cells with propensity scores between 0.1 and 0.9. The light gray areas of the maps are areas that had missing SPEI3 values, and that are thus left out of the analysis. The black areas are grid cells that are included in the dataset, but that do not have propensity scores between 0.1 and 0.9. The maps do not convey differences in scores over time, so some areas with orange dots could have had scores lower than 0.1 for some of the years, but still appear here and be included in the graph.

The violation of the overlap assumption does, of course, have consequences for this analysis as well. Because of the weak overlap, it is not possible to produce unbiased estimates of the effect of SPEI3 on conflict using all of the data. Instead, I can estimate the effect across the observations that have propensity scores acceptably far away from 0 and 1, hence of observations that have a possibility to end in both the treatment and control group. Until now, I have looked at how observations with scores between 0.1 and 0.9 vary. However, choosing the right cut point could be tricky, and will have consequences for the precision of the estimates. In general, the further away from 0 and 1 the propensity scores of the observations are located, the less biased will the estimated effects be. Yet, cutting out observations with high and low propensity scores also means cutting the number of observations that the analysis will be based on. Analyses with very few observations tend to be imprecise and not generalizable. There is no definitive answer as to what the propensity score cut point should be, although cut points at 0.1 and 0.9 are common. Since the distribution of the propensity scores are very skewed in my data, a higher threshold could be advisable.

Table 6.5 shows the number of observations in the treatment and control group depending on two different cut points for the propensity scores. Column two and three in the table signify the treatment group that observations are in (1 means a value of 1 on SPEI3 neg or SPEI3 pos, while 0 means a value of 0 on those variables), while the last column shows the total number of observations (Total N). When removing observations with propensity scores below 0.1 or above 0.9, the number of observations is reduced from 1,000,000 to 18,227 when looking at the assignment of SPEI3_neg, and from 1,000,000 to 22,400 when SPEI3_pos is the treatment variable. Changing the threshold to only include observations with scores between 0.2 and 0.8 further reduces the total number of observations by about half the size. Furthermore, cutting out observations without overlap drastically changes the balance between the number of observations in the treatment and control groups. While about 97 percent of the observations were in the control group prior to the cutting, about 57 percent are now in the treatment group when using a cut point at 0.1 and 0.9. A larger cut point further increases this tendency. This is not so surprising, as most of the cell-years had propensity scores close to 0, which means that they were very likely to be (and probably had been) assigned 0 as their value on the treatment. As increasing the threshold from 0.1-0.9 to 0.2-0.8 has such big implications on the number of and balance between observations in the treatment and control groups, I choose to use a cut point at 0.1-0.9. A higher threshold throws out too many observations, while no threshold gives uncertain estimates. Even so, we should be careful in generalizing the results based on this data, because the observations that the results are based on are relatively few compared to the original dataset, and are not extracted randomly, as propensity scores seem to depend on country affiliation. For the remainder of this chapter, however, I will use observations within the threshold and examine the results as if they are unbiased.

Table 6.5: Number of Observations Depending on Propensity Score Cutpoints

	0	1	Total
SPEI3 neg, all	969,471	30,529	1,000,000
SPEI3 neg, propensity scores between 0.1 and 0.9	7,756	10,471	18,227
SPEI3 neg, propensity scores between 0.2 and 0.8	2,426	7,246	9,672
SPEI3 pos, all	965,333	34,667	1,000,000
SPEI3 pos, propensity scores between 0.1 and 0.9	9,496	12,904	22,400
SPEI3 pos, propensity scores between 0.2 and 0.8	3,170	9,107	12,277

6.2 Omnibus Test of Heterogeneity

The first of the hypotheses that needs to be examined is H1: *The effect of excess or scarce rainfall on violent, state-based conflict is dependent on the vulnerability of the community hit by the rainfall shock.* Before investigating *how* the effect of rainfall variability on violent conflict might vary, it must be established whether the effect varies at all, depending on the contextual variables. The Generalized Random Forest (GRF) package does not, per now, include an option to exclude observations with high or low overlap, so the results from this test must be interpreted cautiously. Table 6.6 shows an omnibus test of heterogeneity for the two models, based on the contextual variables. The "mean forest prediction" in Table 6.6 indicates whether the average treatment effect (ATE) can be trusted. If the coefficient of the mean forest prediction is 1, then the ATE is correctly estimated (Tibshirani et al., 2020, p. 48). The CF-neg model has a mean forest prediction of 1.349, while the CF-pos has a mean forest prediction of 4.869. This means that none of the models have estimated the ATE correctly, but that the ATE of the CF-neg gives a slightly better estimate than that of the forest with positive SPEI3 values. This might indicate treatment effect heterogeneity.

The "differential forest prediction" is an estimate of the level of treatment effect heterogeneity, depending on the contextual variables, and is calculated by subtracting the ATE from the individual treatment effects.[3] If the differential forest prediction is significantly greater than zero, we can reject the null hypothesis of no heterogeneity (Tibshirani et al., 2020, p. 48). As both of the causal forests have a differential

[3]see `https://github.com/grf-labs/grf/blob/master/r-package/grf/R/forest_summary.R` for the R-code for the differential forest prediction.

Table 6.6: Omnibus Test of Heterogeneity

	SPEI3 neg	SPEI3 pos
	(1)	(2)
Mean forest prediction	1.349***	4.865***
	(0.148)	(0.604)
Differential forest prediction	2.345***	2.006***
	(0.077)	(0.056)
Note:	*p<0.1; **p<0.05; ***p<0.01	

forest prediction that is above 2 and significant, the models have found reasonable treatment effect heterogeneity.

Figure 6.3 shows the distribution of estimated individual treatment effects (ITE) from the two causal forests. ITEs are the estimates of what the treatment effect of SPEI3 on violent, state-based conflict would be for individual grid cell-years, based on their values on the contextual variables. The plots only show the treatment effects for observations with propensity scores between 0.1 and 0.9. For most of the sample, it seems that SPEI3 has no effect on violent conflict, as the majority of the treatment effects are zero. Yet, the figure reveals some treatment effect heterogeneity, as there are both positive and negative effects of SPEI3 on conflict in both models. Although these findings signify that the effect of SPEI3 on conflict is heterogeneous, some more investigation is needed before confirming H1, since I have not been able to exclude observation with weak overlap in these models.

6.3 Variation of the Conditional Average Treatment Effect

Section 6.2 showed signs of treatment effect heterogeneity through an omnibus test. In order to verify that the effect of SPEI3 on violent, state-based conflict is truly heterogeneous, a more thorough analysis must be done. The following section presents two different estimates of variation in the conditional average treatment effect (CATE). As discussed in Chapter 4, the CATE is the ATE of specified subgroups of a population, so if the CATE varies from subgroup to subgroup, heterogeneity in the effect is detected. Section 6.3.1 presents results from a best linear projection of the CATE. It shows that variation in some of the variables affect the treatment effect linearly, but

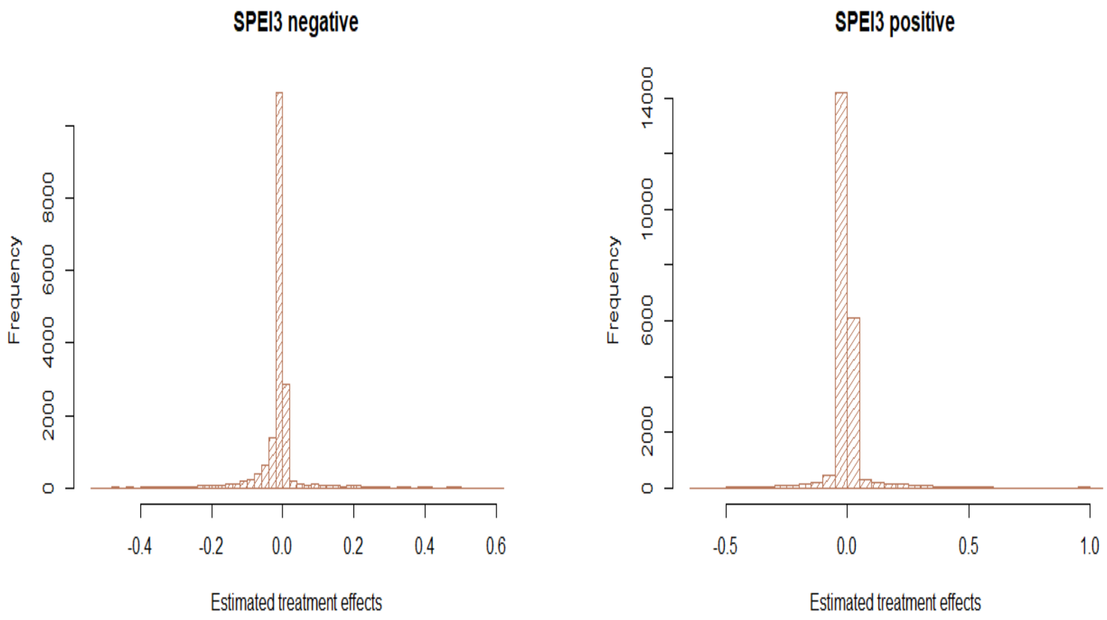


Figure 6.3: Estimated Individual Treatment Effects

that variation in other variables have an insignificant effect on the CATE. Section 6.3.2 presents the CATE of specified subgroups based on values on the contextual variables. Although some heterogeneity is detected, the variation in the CATE is small.

6.3.1 Best Linear Projection of the CATE

One way to proceed in exploring heterogeneity is to take a look at the best linear projection of the treatment effect. The best linear projection of the CATE is the solution to equation 6.1, where $\tau(X_i)$ is the estimated CATE for the ith observation, A_i is a set of features, $beta_0$ is the intercept, and $beta$ is the estimated parameter (Tibshirani et al., n.d.). In other words, the best linear projection estimates the treatment effect of SPEI3 on conflict as a linear function of a set of covariates. It is comparable to a linear regression model, except that the scalar response is the CATE rather than some outcome variable. Table 6.7 and 6.8 show the results from the best linear projection of the CATE. As the outcome variable is binary, the CATE must be interpreted as the change in the probability of a violent, state-based conflict happening, associated with a change in the treatment. Thus, the best linear projection of the CATE is an estimate of how the change in the probability of the outcome that is associated with a change in the treatment will vary depending on an observation's values on the contextual variables. The betas (i.e. the coefficients) in Table 6.7 and 6.8 are estimates of how the effect changes when confronted with a change in one of the contextual variables. For instance, holding the other covariates constant, a one unit increase in the temperature variable (temp) leads to a decrease of the CATE by 0.0002 for the CF-neg model. The warmer it is, the smaller is the effect of SPEI3 on violent conflict, according to Table 6.7.

$$\tau(X_i) = beta_0 + A_i * beta \tag{6.1}$$

It makes sense to estimate such a linear function of the CATE because the hypotheses in this analysis are linear. That is, I expect observations with a greater value on a covariate to have a larger (or smaller) CATE than an observation with a lower (or higher) value on that variable. Hence, if the best linear projection shows that the treatment effect varies significantly and linearly with one of the contextual variables, then that would be in accordance with the theoretical expectation if the linearity is in the correct direction.

Before discussing the results, one disclaimer must be made: the estimated effects on the outcome variable that are presented here cannot be interpreted causally. If SHDI is estimated to have an effect on the CATE through the best linear projection, that does not mean that changing the SHDI score of a community will increase or decrease the effect size. If the best linear projection estimates an effect of SHDI on the CATE, that only signifies that there is a correlation between high and low values of SHDI and high and low values of the CATE. For there to be a causal relation, it must be plausible that the correlation exists because SHDI affects the CATE, and not the other way around. It must, in that case, also be justifiable that there are no omitted variables that could have affected both the values of SHDI and of the CATE, and thus have caused the correlation between the two. Although it might be possible to make such claims so that one could interpret the results from the best linear projection causally, that is not done here. This thesis is only concerned with investigating *how* the effect of SPEI3 on conflict varies with the contextual variables (e.g. whether cell-years with low values on SHDI have a high CATE), and not with understanding *why* it varies in that way. The cause of the heterogeneity of the treatment effect will have to be the focus of future research. For simplicity, however, I will describe the contextual variables as having an estimated effect on the CATE, when looking at the results from the best linear projection. This only means that the causal forests have detected correlations between the contextual variables and the CATE, not that the relation is causal.

The results from a linear projection could be sensitive to the variables included, if one variable is highly correlated to others. As a robustness check, I have therefore constructed a correlation plot of the variables, displayed in Figure 6.4. It seems that particularly *empl_agr*, *shdi*, *global_ind*, and *gdp* correlate strongly with each other and with some of the other variables. Table 6.7 and 6.8 therefore include models with all contextual variables, as well as four models where one of the variables that correlates strongly with others are excluded, to see whether the estimates stay unchanged.

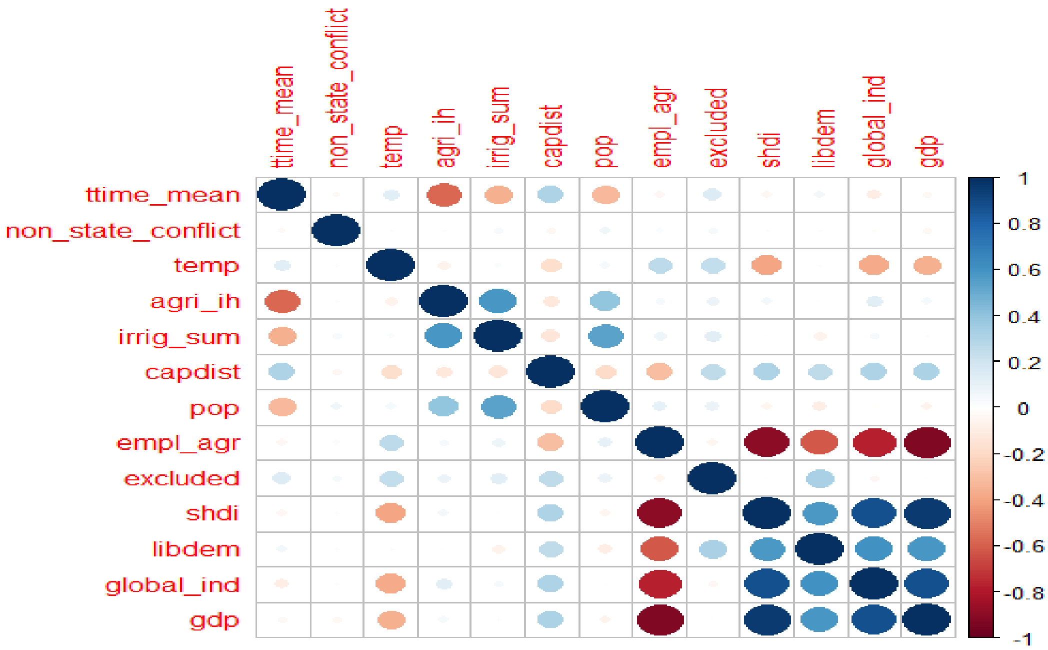


Figure 6.4: Correlation Plot of Contextual Variables

Let us begin by reevaluating H1, which stated that the effect of excess or scarce rainfall on violent, state-based conflict is depended on the vulnerability of the community hit by a rainfall shock. From the omnibus test, it seemed that heterogeneity was found across the vulnerability factors. In Table 6.7 and 6.8, we can see that heterogeneity is found across some of the contextual variables, but not all. Yet, since there is heterogeneity across some of the variables in both models, H1 can be confirmed. The effect of scarce and excess rainfall on conflict vary depending on the vulnerability of the community hit by the rainfall shock.

Assessing whether H2 and H3 hold is more complicated. The hypotheses state that the effect of scarce and excess rainfall on conflict is bigger the more vulnerable a community is to rainfall shocks and conflict. Table 5.3 summarized the theoretical expectations of how the effect of scarce and excess rainfall should vary depending on the contextual variables, if H2 and H3 are to be confirmed. It told us which values of the contextual variables were associated with the highest vulnerability, and that the effect of rainfall variability on conflict should be largest in those areas. In Table 6.7 and 6.8, the direction of the relationship between the covariate and the CATE tells us whether high or low values of the covariate are associated with high or low treatment

Table 6.7: Best Linear Projection of the CATE, CF-neg

	CF-neg				
	All	Employment Agriculture	GDP	Globalization	SHDI
	(1)	(2)	(3)	(4)	(5)
ttime_mean	0.003	0.002	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
non_state_conflict	−0.041	−0.041	−0.044	−0.042	−0.039
	(0.060)	(0.060)	(0.060)	(0.060)	(0.060)
temp	0.0003	0.0002	0.0003	0.0003	0.0001
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
agri_ih	0.0001	0.0001	0.0001	0.00004	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
irrig_sum	−0.00000**	−0.00000**	−0.00000**	−0.00000**	−0.00000**
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
capdist	0.0001	0.0003	0.001	−0.001	−0.0001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
pop	0.00000	0.00000	0.00000	0.00000	0.00000
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
empl_agr	0.001**		0.0004	0.001*	0.001*
	(0.0003)		(0.0003)	(0.0003)	(0.0003)
excluded	0.001	0.001	0.001	0.002	0.002
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
shdi	0.088	0.058	0.136***	0.059	
	(0.060)	(0.060)	(0.047)	(0.057)	
libdem	−0.003	−0.011	−0.005	−0.012	−0.005
	(0.011)	(0.010)	(0.010)	(0.009)	(0.010)
global_ind	−0.001	−0.0005	−0.0005		−0.0005
	(0.0005)	(0.0004)	(0.0004)		(0.0004)
gdp	0.017	0.005		0.012	0.024***
	(0.011)	(0.009)		(0.010)	(0.009)
Constant	−0.222***	−0.085*	−0.107**	−0.188**	−0.235***
	(0.085)	(0.046)	(0.045)	(0.084)	(0.082)
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 6.8: Best Linear Projection of the CATE, CF-pos

	CF-pos				
	All	Employment Agriculture	GDP	Globalization	SHDI
	(1)	(2)	(3)	(4)	(5)
ttime_mean	0.005**	0.004*	0.005**	0.004*	0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
non_state_conflict	0.057	0.057	0.057	0.054	0.057
	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)
temp	0.0003	0.0003	0.0003	0.0003	0.0003
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
agri_ih	−0.0001	−0.0001	−0.00005	−0.0001	−0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
irrig_sum	0.00000*	0.00000	0.00000*	0.00000*	0.00000*
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
capdist	−0.005*	−0.005*	−0.004*	−0.005**	−0.005*
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
pop	0.00003**	0.00003**	0.00003**	0.00003**	0.00003**
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00002)
empl_agr	−0.001**		−0.001***	−0.001***	−0.001**
	(0.0002)		(0.0002)	(0.0002)	(0.0002)
excluded	−0.016***	−0.016***	−0.016***	−0.014***	−0.016***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
shdi	−0.026	0.002	0.015	−0.076	
	(0.054)	(0.053)	(0.039)	(0.052)	
libdem	−0.014**	−0.009	−0.014**	−0.022***	−0.014**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
global_ind	−0.001***	−0.001***	−0.001***		−0.001***
	(0.0003)	(0.0003)	(0.0003)		(0.0003)
gdp	0.010	0.017**		0.003	0.008
	(0.008)	(0.007)		(0.007)	(0.005)
Constant	0.007	−0.079*	0.066**	0.054	0.012
	(0.048)	(0.041)	(0.030)	(0.047)	(0.046)
Note:					*p<0.1; **p<0.05; ***p<0.01

effects. If the estimated effect is positive, then that means that observations with high values on that variable have higher effects than observations with lower values. Similarly, if the estimate is negative, then observations with low values experience bigger treatment effects than observations with higher values. Hence, for the theoretical expectations to be confirmed by the models, the heterogeneity must be in the same direction as summarized in Table 5.3 at the end of Chapter 5.

I will assess the results from the CF-neg model first, which are the ones that correspond to H2 and are displayed in Table 6.7. The only covariate with significant effects on the CATE in the CF-neg model with all variables included is employment in agriculture (empl_agr). The effect of irrigation (irrig_sum) is also significant, but as the effect is zero, I will not consider it further. However, the estimated effect of *empl_agr* is not consistently significant when removing some of the other variables. For instance, in the model that excludes GDP, *empl_agr* is not estimated to be significant, while the significance level is lower in the models without globalization and without SHDI. Moreover, in the model without GDP, SHDI is significant on a 99 percent significance level, with an effect much larger in the remaining models. On the contrary, when excluding SHDI, GDP becomes highly significant. Because the significance levels on these variables are sensitive to what other variables are included, I will not confirm or dismiss heterogeneity across these variables until I have looked at the CATE for subgroups in the next section.

Yet, some comments can already be made. First, as only three of the variables have significant effects on the CATE, the CATE of negative SPEI3 on conflict does not depend much on vulnerability factors, or at least not linearly.[4] Since so few of the vulnerability factors have an effect on the CATE, H2 can only be partially confirmed, even if Section 6.3.2 finds that the CATE varies with *empl_agr*, GDP and SHDI. Moreover, the estimated effects that the contextual variables have on the CATE are not large. For instance, the estimated effect of *empl_agr* on the CATE in the model with all variables is at 0.001. That means that the effect of scarce rainfall on the probability of conflict increases by 0.001 when confronted with a one unit change in *empl_agr*. Since *empl_agr* is a measure of the percentage of the employed population that is employed within the agricultural sector, an increase of 10 percentage points on the *empl_agr* scale leads to an increase of 1 percentage point (0.01) of the CATE. If the CATE was 5 when no people were employed within agriculture, it would be 6 if 10 percent of the employed population worked within farming. In other words,

[4] Table 6.7 only projects the best estimate of a *linear* relationship between the contextual variables and the CATE. Insignificant estimates could conceal curve-linear or other types of relations between the variables and the CATE, but that would not prove significant when only estimating linear effects. Since the expectation from the theoretical framework is that higher vulnerability would yield bigger CATEs, curve-linearity is beyond the scope of the thesis.

the effect of scarce rainfall on violent conflict does not depend much on the level of employment in the agricultural sector, nor on any of the other contextual factors.

The picture is quite mixed for the CF-pos model, whose results are projected in Table 6.8. The contextual variables with significant effects on the CATE from the CF-pos model are travel time to urban center (ttime_mean), distance to capital (capdist), population density (pop), employment in agriculture (empl_agr), level of political exclusion (excluded), level of democracy (libdem) and globalization (global_ind). These variables have significant effects on the CATE across all of the models. In addition, GDP has a significant effect when *empl_agr* is excluded from the model. As for the CF-neg model, *irrig_sum* also has a significant effect on the CATE, but the effect is so small that it equals to zero. The estimated effects on the CATE are small, as was the case for the CF-neg model.

According to the theoretical expectations presented in Table 5.3, high values of the *ttime_mean, capdist, pop, empl_agr,* and *excluded* as well as low values of *libdem* and *global_ind* imply high vulnerability. For most of the variables with significant effects in Table 6.8, high vulnerability is associated with a large CATE, in accordance with Table 5.3. Nevertheless, for *capdist* and *empl_agr*, the relationship is opposite of what was expected. It thus seems as if some types of vulnerability make the effect larger, while other types reduce its size. Just as H2 can only be partially confirmed, H3 cannot be entirely rejected nor entirely confirmed based on Table 6.8. I will wait with drawing such conclusions until looking at the CATE for specified subgroups in the next section.

6.3.2 Plots of Variation in the CATE

Figure 6.5 and 6.6 show the CATE for specified subgroups of the data. The values of each variable are divided into four quantiles, where the highest quantile is the highest 25 percent values that it is possible to have on that variable. For *libdem*, that means that the highest quantile includes all observations with a value between 0.75 and 1 on the liberal democracy scale. The CATE is then calculated for observations that partake in the different quantiles. Here, I only show the CATE for quantiles of the variables with significant estimates from the best linear projection. Plots with CATE across all of the variables are found in Appendix C.

Figure 6.5 and 6.6 show the CATE of negative SPEI3 on conflict for different subgroups. The dots show the estimated CATE for the subgroups, while the lines signify confidence intervals (CI), hence the level of certainty of the estimated effect.[5] The CI

[5]The dots without lines in the *gdp* and *pop* plots in Figure 6.6 actually conceal CIs that were so large that fitting the x-scale to include them would make it difficult to interpret the size of the CI for the other dots. They are therefore left as they are.

are at a 95 percent confidence level, which means that with 95 percent certainty, the true treatment effect for a subgroup lies within the lines projected. If the CI crosses zero, then the effect is not significantly different from zero (i.e. the effect *might* be zero), and we cannot reject a null hypothesis of no effect. Significant estimates are marked in blue, and not significant estimates in dark red. All figures have the same range on the x-axis so that the size of the effects are comparable. Appendix C includes two plots where a continuous SPEI3 variable is used as the treatment, instead of using a binary treatment as was done in the analysis. The plots there show other tendencies than what was found here. Explaining these differences is outside the scope of this thesis, but should be further investigated.

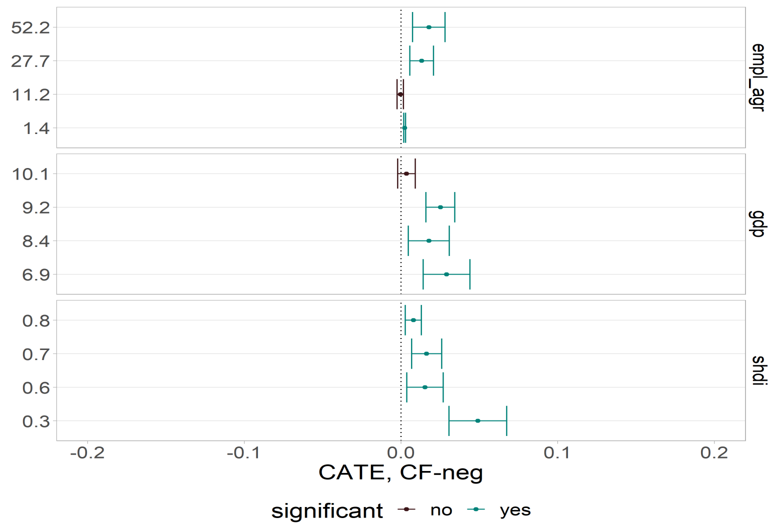


Figure 6.5: Variation in the CATE, CF-neg

There is scarce heterogeneity across the variables in Figure 6.5. The CATE seems to vary linearly across the variables, so that high values of for example *empl_ agr* have larger effects than lower values. Yet, the confidence intervals of the CATEs partially or totally overlap for all three variables, meaning that the CATE of one quantile is not significantly different from the CATE of the quantile above or below it. However, there *is* a significant difference between the CATE of the highest and lowest value for all three variables. As would be expected from the small effects estimated in the best linear projection, moving from the lowest to the highest value of the contextual factors does not change the effect of SPEI3 on conflict much. The largest change is found in the SHDI variable, where observations with SHDI scores equal to or below 0.3 have a CATE of about 0.05, while the largest quantile (SHDI score equal to or above 0.8) has a score of about 0.01. The difference in effect size between these subgroups is about 0.04. What is interesting is that these variables are all part of the operationalization of a larger vulnerability concept, namely dependency on rain-

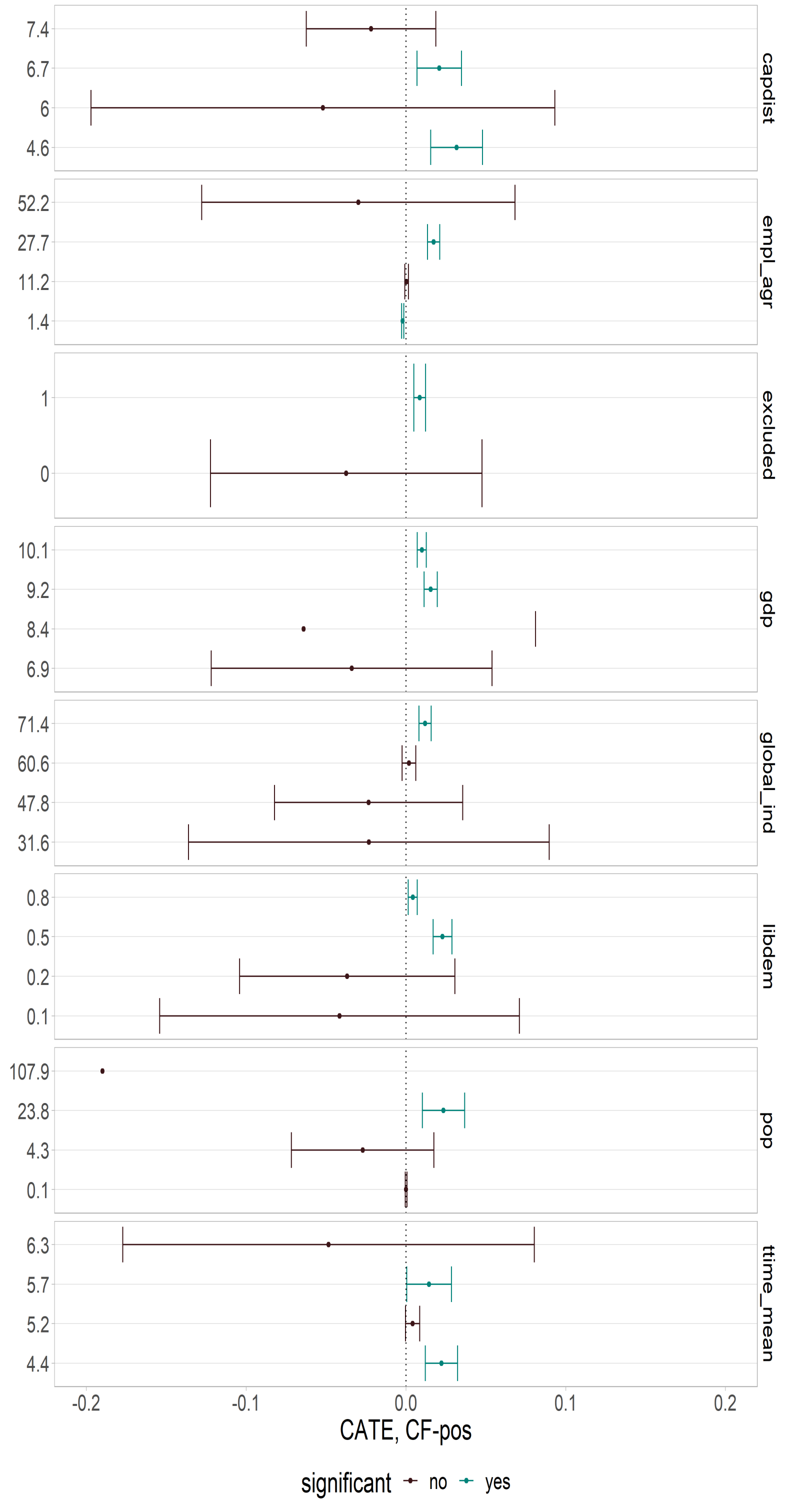


Figure 6.6: Variation in the CATE, CF-pos

fed agriculture and good governance. It seems that the treatment effect depends on some parts of good governance, and on some parts of dependency on agriculture, but that aspects of these vulnerability factors do not have implications for the effect. In total, the effect of scarce rainfall on state-based conflict only partially, and to a small degree, depend on the vulnerability of the community hit by the rainfall shock. H2 can thus be partially confirmed.

Figure 6.6 reveals much uncertainty in the estimated CATE for some of the subgroups in the CF-pos model. The uncertainty is most likely caused by a limited number of observations that have the different quantile values. From the figure, it is difficult to identify significant effect heterogeneity across any of the variables. However, as the best linear projection managed to estimate significant linear relations between these covariates and the CATE, H3 can be partially confirmed. More investigation into these variables and how they vary should be done, however.

Lastly, Figure 6.7 shows the geographical distribution of the ITEs. In accordance with Figure 6.3 in Section 6.2, most of the grid cells have small treatment effects of SPEI3 on violent, state-based conflict. It does not look like there are any specific geographic characteristics that affect the effect of SPEI3 on conflict. No areas included in the sample seem to be more vulnerable to rainfall-induced conflict than others.

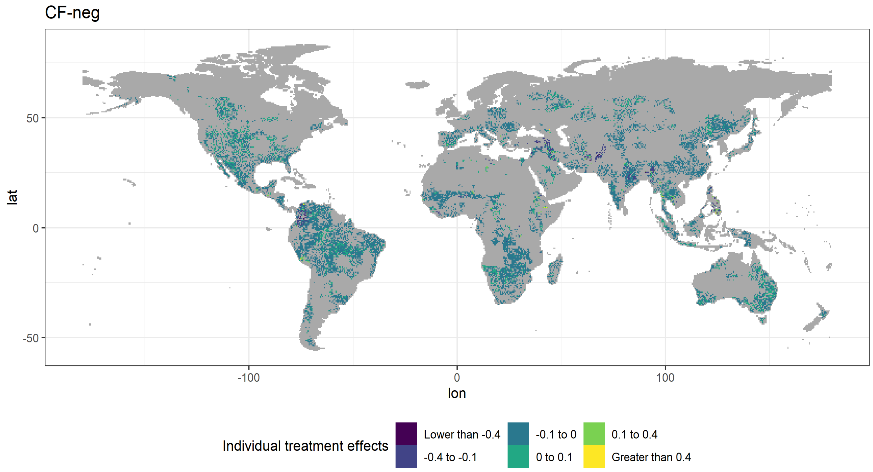
6.4 Discussion

This section discusses the implication of the results from the analysis. The reliability and validity of the results is also examined.

6.4.1 Implications of the Results

The results from the analysis have shown that there is little treatment effect heterogeneity across the vulnerability factors. This is contradictory to what was expected. Table 6.8 shows the summary of the theoretical expectations, with the results from the analysis presented in the two last columns. The picture is mixed. More heterogeneity was found in the CF-pos model than in the CF-neg model, but there is connected much uncertainty to those results. Even variables that are supposed to measure the same contextual factor show differences in their impacts on the CATE, but the differences are not consistent with the level of correlation between the variables.

Four variables did not have an impact on the treatment effect in any of the models. These are percentage of cell covered by agricultural land (*agri_ih*), area of cell covered by irrigated land (*irrig_sum*), temperature (*temp*), and non-state conflict. Other than that, the variables either have a significant effect in one of the models and insignificant in the other or the impact of the variable is significant in both models, but with heterogeneity in opposite directions. One conclusion that can be drawn based on that result is that the effect of scarce rainfall on conflict is not heterogeneous *in the same way* as the effect of excess rainfall on conflict, although both are weakly



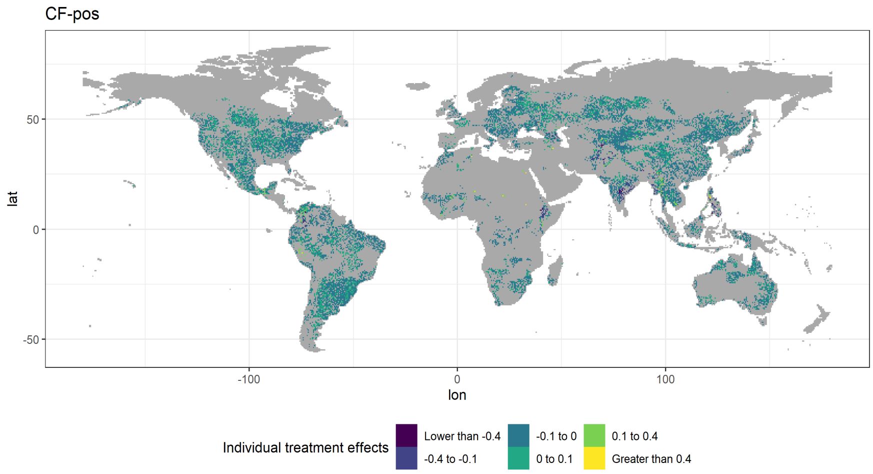


Figure 6.7: Maps of the Individual Treatment Effects
The plots shows the geographic distribution of individual treatment effects. The color codes indicates the size of the effects, according to the legends. The upper map shows the results from the CF-neg model, while the bottom map shows the results from the CF-pos model.

heterogeneous.

It is surprising that more heterogeneity was not found across the contextual variables. The modest heterogeneity must, however, be viewed in the context of the size of the rainfall-conflict effect. Indeed, the effect size of rainfall variability on conflict is

Contextual factor	Theoretical expectation	Operationalization	Variable name in dataset	Indication of vulnerability	Result CF-neg	Result CF-pos
Dependency on rain-fed agriculture	High levels of dependency on rain-fed agriculture make communities vulnerable	Percentage of cell covered by agricultural land	*agri_ih*	High value	Not significant	Not significant
		Total area of cell covered by irrigated agriculture	*irrig_sum*	Low value	Significant, but zero	Significant, but zero
		Percentage of employed population employed in agriculture	*empl_agr*	High value	Significant, but small implication	Significant, but opposite direction and small implication
Temperature	High temperatures make communities more vulnerable	Temperature	*temp*	High value	Not significant	Not significant
Population density	High population density indicates higher vulnerability	Population density (number of people per square kilometer)	*pop*	High value	Not significant	Significant, but small implication
Globalization and urbanization	Globalization and urbanization makes communities less vulnerable	KOF globalization index	*global_ind*	Low value	Not significant	Significant, but small implication
		Distance to capital (in kilometers)	*capdist*	High value	Not significant	Significant, but opposite direction and small implication
		Average travel time (in minutes) to nearest urban center	*ttime_mean*	High value	Not significant	Significant, but small implication
Regime type	Strong democracies are less vulnerable	Liberal democracy index	*libdem*	Low value	Not significant	Significant, but small implication
Good governance	Good governance makes communities less vulnerable	Subnational Human Development Index (SHDI)	*shdi*	Low value	Significant when excluding GDP, small implication	Not significant
		Gross domestic production per capita (GDP)	*gdp*	Low value	Significant when excluding SHDI, small implication	Significant when excluding *empl_agr*, small implication
		Level of political exclusion of ethnic groups	*excluded*	High value	Not significant	Significant, but opposite direction and small implication
Prevalence of non-state conflict	Prevelance of non-state conflict makes communities more vulnerable	Non-state conflict	*non_state_conflict*	High value	Not significant	Not significant

Figure 6.8: Summary of Theoretical Expectations, With Results

small, also when estimating individual treatment effects as in Figure 6.3 and 6.7. For most of the subgroups presented in Figure 6.5 and 6.6 that had significant CATEs, the effect size is somewhere between 0 and 0.1. A treatment effect of 0.1 should be interpreted as a 10 percentage point increase in the probability of conflict associated with a change from normal to abnormal rainfall conditions. If the largest effect size that rainfall events can possibly have on conflict is an increase of 10 percentage points, then a change of 1.4 percentage points (as was the best linear projection of the effect of *libdem* on the CATE in CF-pos) could be substantial after all.

The meaning of an increase in the probability of something happening could be difficult to grasp intuitively. Although an effect of rainfall on conflict of 10 percentage points signifies that scarce or excess rainfall events could make conflict more likely to erupt, it does not, however, mean that conflict *will* erupt. The consequence of an increase in the probability of something happening depends on the probability of that something happening prior to the increase. If the propensity of a conflict erupting was zero prior to a climatic event, then a 10 percentage point increase would mean that the probability of conflict was now 10 percent. In other words, in that society, peace would still be much more likely than conflict. Had the probability of conflict erupting prior to a climatic event been 80 percent, then a 10 percentage point increase might have a substantial consequence on that society. Similarly, the impact of a *change* in the effect depends on the probability of conflict in a society prior to an event. If the effect of a rainfall shock on the probability of conflict is 1.4 percentage points larger in communities with low levels of democracy than in those with high levels of democracy, then the difference in the effect could be insignificant if the probability of conflict erupting was low in these communities to begin with. However, if at the brink of war, an excess rainfall event happening in a cell-year with low levels of democracy might be what pushes a community into conflict.[6] Nevertheless, the results seem to substantiate that rainfall variability is not a major cause of war, no matter the context.

From Table 6.8 it is clear that grid cell-years with high percentages of their population employed in agriculture and with low values of SHDI and GDP are most vulnerable to rainfall-induced conflict in the aftermath of a scarce rainfall event. Furthermore, cell-years with high percentages of their population employed in agriculture, with high population density, with low globalization, short distance to the capital, but otherwise long distance from urban centers, with low levels of democracy and GDP, and with little political exclusion are the ones most vulnerable to erupt in conflict in the aftermath of an excess rainfall shock.

Even though the variation in treatment effects is moderate in this analysis, that is not proof that researchers should continue estimating the ATE of rainfall on conflict. First, since the overlap assumption is violated in the original data, future

[6] Note also that the constant in table 6.7 and 6.8 is at -0.222 for the CF-neg model and 0.007 for the CF-pos model. In other words, for some of the subgroups, the effect of a rainfall shock on conflict will be negative, meaning that rainfall variability leads to a lower probability of violent, state-based conflict.

research should investigate if the propensity score cut point that I chose affects the results. Second, heterogeneity might be stronger across other data. Since I had to cut the randomly selected global dataset from having 1,000,000 observations to only include around 20,000, and, which is discussed below, because these were not selected randomly, other data might show a different picture. Third, there might be heterogeneity across other variables than those studied here, as I have only included factors of vulnerability. Fourth, having data on other temporal or spatial scales could yield different results. As will be discussed in the section below, a weakness of this study is that climate and conflict events that are connected could happen in different grid cells. There might be larger heterogeneity in the data if we were able to capture how a climate event in one cell might affect conflict events in cells further away. Fifth, it could be that if we studied how a rainfall event affected the number of deaths in or length of conflicts, more heterogeneity would be found. Lastly, this thesis has only investigated whether the contextual factors impacted the effect *linearly*, because that was what was hypothesized. However, future research should investigate if the effect might also vary curve-linearly or along other shapes.[7]

Although the treatment effect heterogeneity is scarce, the results from this analysis show that the effect varies. Future research should investigate *why* it varies as it does, and why the effect of scarce and excess rainfall vary in dissimilar ways. Particularly, future research should investigate why the heterogeneity across distance to capital and political exclusion are opposite of what was expected theoretically, as more vulnerability of these variables yield smaller effects. The results found here are a springboard for further research that should focus on how and why the effect of rainfall variability on conflict is heterogeneous.

6.4.2 Reliability and Validity of the Results

Both the reliability and validity of an analysis affect the credibility of the results. Reliability concerns the presence of random error in the analysis (Adcock & Collier, 2001, pp. 531-532). Running an analysis several times should yield similar results, if the results are reliable. Moreover, Cook and Campbell define four types of validity for causal analyses to be of good quality. These are statistical conclusion validity, internal validity, construct validity and external validity (Cook & Campbell, 1979; recounted and summarized in Lund, 2002, p. 105). Some aspects of reliability and validity have already been discussed. Nevertheless, I will discuss whether the results here can be classified as reliable and valid in the following subsections. Statistical conclusion validity, which concerns the size and significance of the results, will not be

[7]Indeed, Figure C.1 and C.2 in Appendix C show signs of curve-linearity across some of the variables.

further discussed as it has already been elaborated upon above.

Reliability

The missing data analysis found in Appendix A shows that the results do not depend on some systematic missing information. Moreover, although the significance of the estimates varied slightly when excluding some of the variables, the best linear projection showed similar sizes and directions of the estimates across the models. Yet, when plotting the CATE for subgroups of the data with a continuous treatment variable, as seen in Figure C.3 and C.4 in Appendix C, the tendency was a bit different than in the plots of the CATEs depicted above.[8] Overall, however, the results seem robust when having a binary SPEI3 as the treatment.

However, one thing that could have improved the reliability of the results is to experiment with the tuning of parameters for the causal forest models more thoroughly. For now, I have let the machine choose the optimal fit for the tuneable parameters in the models. When it comes to the number of trees in the analysis, where the researcher itself must pick the best parameter, I chose the default value. I tried experimenting with the number of trees in the causal forests, but a combination of big datasets, many variables and a computer with limited capacity made it difficult. Running each causal forest model with 2000 trees, which is the default value, takes about two hours each. When I tried to increase the number of trees to 5000, the causal forest ran for about 8 hours before the R-program collapsed. Experiencing with the number of trees and with other tunable parameters in the causal forest models could improve the reliability of the results, but requires computers with higher capacity than the one I had access to.

The novelty of the causal forest methodology could have implications for the reliability of the results. A more common statistical methodology such as Ordinary Least Squares (OLS) regression have had so many users by now that the method has been cross-checked, discussed and revised many times. In general, we know what makes OLS reliable, what assumptions must hold, and how results should be interpreted. The causal forest, however, is so new that few scientists have used it yet. It might be that future users of the methodology find better and more efficient ways of presenting the results from a causal forest. Certainly, the novelty of the methodology has made it difficult and time consuming to find literature that has applied the causal forest, other than the article that presents the methodology found in Athey et al., 2019. For instance, none of the articles that I found who applied the methodology had

[8] I did not manage to run the best linear projection with a continuous SPEI3 variable as the treatment, because the R-program kept shutting down when I attempted to do so. It seems like it was too big of a computational task for my computer to handle.

encountered problems with weak overlap in the data. The future might bring other suggestions of how that problem should be solved, which could have consequences for both the reliability and validity of the models.

Internal Validity

Internal validity concerns the basis for conclusions about a causal relationship between the operationalizations of the treatment and the outcome variable (Lund, 2002, p. 106). In this analysis, the focus has been mostly on whether the effect of SPEI3 on conflict is heterogeneous, not on the relation between the treatment and the outcome in itself. As such, throughout the analysis, the causality of the relation between the treatment and the outcome has been assumed. Yet, the violation of the overlap assumption has consequences also for the validity of assuming that SPEI is unconfounded (i.e. independent of the other variables in the model). A prerequisite for using SPEI as the treatment variable is that it is as good as randomly assigned and that it is not depended on any confounding variables. However, since most of the observations have propensity scores of being assigned SPEI close to 0, something in the data is able to predict the values of SPEI, so the treatment is unlikely to be independent after all. Since I have removed the observations with very low and very high propensity scores, the SPEI values of the data in the subset should be independent of confounding variables. The causal relationship identified based on this data should thus be valid. Yet, the skewed propensity score distribution discussed in the beginning of this chapter raises a red flag for the validity of the results of other studies that use SPEI as the treatment and that have not excluded observations with weak overlap, or made other modifications to adjust for the weak overlap.

As discussed earlier, the variation in treatment effects found here cannot be interpreted as if the contextual variables have a *causal* effect on the CATE. This analysis can only determine *how* the treatment effect of SPEI3 on violent, state-based conflict varies, not if it varies because the contextual variables causally affect the treatment effect.

Construct Validity

Construct validity concerns how well the operationalizations measure the concepts they are supposed to measure (Lund, 2002, p. 106). The justification for operationalizations of the variables and vulnerability factors are discussed in Chapter 5. Some points can still be said now about the choice of operationalizations in this thesis. There is some discussion in the literature about the accuracy of SPEI as a measurement of excess and scarce precipitation. Although important for the analysis, determining questions such as how well SPEI measures drought, floods and other variations of precipitation lies outside of the field of political science and thus outside the scope of this thesis. The choice of SPEI as the treatment variable leaned on information and discussions found in the literature. Having a higher cut point for what observations were coded as 1 and 0 on the SPEI variables could also affect the results. A higher cut point was not chosen because it would have led to too few observations in the treatment group, as very few observations had values above 1. Making the SPEI variable binary also made it easier to interpret the results. As mentioned, as a robustness check, I included plots of variation in the CATE when SPEI3 was coded as a continuous variable instead of being binary. These plots are found in Appendix C. The results show other tendencies in treatment effect heterogeneity than what has been displayed in this chapter, as most of the CATEs in those plots seem to be either negative or not significant. This implies that the cut point chosen for making SPEI3 binary might not have been optimal. In any case, it shows that the results are sensitive to the operationalization of the treatment variable, which should be investigated further.

A limitation of this thesis is that it has studied whether climatic events affect the probability of a conflict event happening in *the same cell* as the rainfall shock. Yet, rainfall-induced conflicts could have happened in grid cells far away from where the climatic event happened. This is particularly likely when studying state-based conflicts (as opposed to non-state conflicts that are fought between organized or unorganized non-state groups). If a rainfall hazard creates grievances that make people want to fight the state, they could be more likely to do so in an area where the power is, such as in a regional or state capital. This could also explain why the effect of *capdist* on the CATE was opposite of what was hypothesized.[9] Moreover, if rainfall shocks lead to migration, and if that leads to grievances in the areas that people have migrated to, such grievances would happen in an area that is not the grid cell where the rainfall shock happened. It is difficult to connect conflicts to climate events that happen very far away from each other. One possibility could be to aggregate the grid cells to a higher level, so that more cells are incorporated in one unit of analysis. In any case, different aggregation levels might affect the results, but it has been outside the scope of this thesis to test whether that is true.

External Validity

External validity concerns the generalizability of the results across time and space (Lund, 2002, p. 106). As briefly discussed in Section 6.1, the results from this

[9]Indeed, distance to capital could be a more complex variable than what it has been presented as in this thesis. Including it as part of the operationalization of urbanization was maybe too simplified.

analysis are not representative for all grid cell-years, due to the lack of overlap that forced me to throw out large parts of the data. I was unable to determine the reason for the lack of overlap, but the fact that I have thrown data out of the analysis based on observations' propensity scores, which in turn could be depended on some confounding variable(s), means that the sample is not representative for the broader population. The sample is not randomly selected, so the results cannot be generalized to hold for data with other propensity scores.

Nevertheless, the results are representative of observations with similar propensity scores as those included in the analysis. The data in the samples are spread out across time and space, as seen in Figure 6.2. Although the sample is considerably reduced when only including observations with propensity scores between 0.1 and 0.9, there are still close to 20,000 observations that are used from both models to estimate the effects found here. These are large samples, much bigger than what is customary for conducting statistical analysis, and should therefore be representative of grid cell-years with propensity scores of being assigned SPEI3 between 0.1 and 0.9.

Another question is whether the results are generalizable over time. Climate change is a gradual and protracted process that is likely to lead to a higher frequency of events such as scarce or excess rainfall. It might be that people will react differently to events of anomalous rainfall if they happen very often, than if the events are rare. Maybe higher frequencies of extreme events lead to more grievances and thus more armed conflict, or maybe it will generate fatigue so that people do not have the time or energy to fight. It could also lead to peacebuilding if those affected perceive themselves to be affronted with a common enemy so that disagreements are postponed until after the crises. It is impossible to predict exactly what the consequences of scarce and excess rainfall will look like in the future. Even so, understanding how the effect of rainfall variability on violent, state-based conflict varies with contextual factors is vital if we are to understand what the future might look like. If we know what makes societies vulnerable to climate shocks, or to conflict erupting from such shocks, we might be better equipped to meet the future. The results found here should therefore give insights to which contextual factors mediate the effect also in future scenarios.

Chapter 7

Conclusion

This thesis has sought to answer the following research question: *Under what conditions, if any, does rainfall variability affect violent, state-based conflict?* As such, the thesis aligns itself with the greater literature on the climate-conflict nexus. The effect of rainfall variability on conflict is likely to be context specific. Yet, previous research have focused on estimating average treatment effects of climate on conflict, rather than studying how the effect varies. To fill this research gap, the thesis has studied treatment effect heterogeneity across vulnerability factors. Through the use of global data, it has contributed to the literature by offering an analysis that goes beyond studying Sub-Saharan Africa, which is the area that has received most attention from the research field. Moreover, the thesis serves as an application of a new methodological framework that can move the research field forward by approaching the climate-conflict nexus in new ways.

The theoretical framework explained that the effect of rainfall variability on state-based conflict is likely to vary depending on the vulnerability of communities to rainfall shocks and to erupt in conflict. I listed seven factors of vulnerability that might cause heterogeneity in the treatment effect. Based on these factors, three hypotheses were made about the nature of the effect of rainfall variability on conflict. These stated that the effect is dependent on the vulnerability of the community hit by the rainfall shock (H1), and that the effect would increase with the level of vulnerability (H2 and H3). Scarce and excess rainfall were operationalized through positive and negative values on the Standardized Precipitation and Evapotranspiration Index (SPEI). Violent, state-based conflict was derived from the Uppsala Conflict Data Program (UCDP). The vulnerability factors were operationalized through 13 variables derived from 9 different data sources.

The results from the analysis show that rainfall variability have a small effect on conflict in all types of settings studied here, but that some vulnerability factors could affect the size of that effect. H1 is therefore confirmed, while H2 and H3 can only be partially confirmed as treatment effect heterogeneity was only found across a few of the vulnerability variables. To answer the research question: Scarce rainfall is most likely to increase the probability of violent, state-based conflict in communities that have a high percentage of its population working within agriculture, and that does not have good governance in some of the aspects of the term (i.e. that have low values of SHDI and GDP). Excess rainfall is likely to have the largest effect on violent, state-based conflict in communities with low percentages of people working in agricultural production, with high population densities, with low levels of globalization, that are situated close to the capital, but far away from other urban areas, that had low levels of democracy and GDP and that have little political exclusion. As apparent from this summary, the effect of excess rainfall on conflict was heterogeneous across more variables than was the effect of scarce precipitation. Some of the variables, namely distance to capital and political exclusion, also modified the effect opposite of what was expected.

These results can only establish that the effect of excess and scarce precipitation on violent, state-based conflict is context specific. They cannot explain *why* the effect varies across these variables, nor if the relation is causal so that changing the degree of vulnerability in a community will lead to lower or higher effects of rainfall on conflict. Instead, the analysis showed which contexts are likely to be most vulnerable to violent, state-based conflict erupting in the aftermath of a rainfall shock. As observations with propensity scores below 0.1 and above 0.9 had to be removed from the analysis due to weak overlap, the results found here can only be generalized to hold for observations with the same propensity scores as those studied.

Although not generalizable to all types of data, the results from the thesis contribute to the theory-building on the subject. As little research has been done on how the treatment effect varies, the theoretical expectations about the context specificity of the effect were also few and underdeveloped. The results found here show a mixed picture, but reveal that, except for prevalence of non-state conflict and temperature, all of the vulnerability factors listed in the theoretical framework affect the treatment effect in some manner. Dependency on rain-fed agriculture was more complex than first anticipated, because only one of the three variables used to operationalize the factor had significant impacts on the treatment effect. More research is needed before theory is properly built, but the results show that the effect of rainfall variability on violent, state-based conflict is context specific.

7.1 Future Research

The thesis hypothesized that scarce and excess rainfall on conflict would be affected by the same vulnerability factors, which seemed plausible based on the current literature. The results from the analysis showed that the vulnerability factors modified the effects of scarce and excess precipitation differently, which signify that scarce and excess rainfall affect conflict through separate mechanisms. It might be, for instance, that excess rainfall could produce different types of grievances than those produced by scarce rainfall, and that the factors that modify these causal mechanisms are therefore different. It could also be that the vulnerability factors modified the effects in a curve-linear manner, which could have caused some of the difference in results. Future research must study the mechanisms that link rainfall to conflict more thoroughly. This would make it easier for others to understand what factors could influence the climate-conflict nexus, and why they would do so.

A surprising result from the analysis was that the propensity of experiencing a scarce or excess rainfall event was zero for about 98 percent of the observations. This affects the results of analyses that use SPEI as the treatment, if not dealt with properly. Future research must ascertain what drives these propensity scores, before basing conclusions on analyses with SPEI as the treatment. Moreover, other operationalizations of variables and/or other levels of analyses could also affect the outcome. Future research should investigate how robust the results found here are to other research designs.

The causal forest, and the Generalized Random Forest package more widely, gives many opportunities for studying the climate-conflict nexus. This thesis serves as one application of the methodology. However, causal forest can handle hundreds of interacting variables and large numbers of observations, which opens the gate for new possibilities for the climate-conflict research field. Future research could use the methodological framework from this analysis to study similar cases with other operationalizations and research questions, or it could further exploit the possibilities given by the causal forest. Although the violation of the overlap assumption limited the scope of generalizability of the results, that is a data-problem, not a methodology-problem. The advantages of using machine learning for causal inference are so many that the research field must be attentive of new developments that can help move the research forward, and thus help the research community and others understand and adapt to the consequences of climate change.

The results from this thesis emphasize the need for more research on the pathways through which climate change and variability could affect conflict. Future research should continue exploring treatment effect heterogeneity. It should seek to explain the effect variability found in my results, and it must investigate whether the link between other types of conflicts and climate variabilities are also context specific. Without such knowledge, the research field is groping in the dark when communicating conclusions based on average treatment effects. Such estimates are useless if the effect is context specific, both for researchers who want to understand the climate-conflict link and for policymakers and organizations who need to respond to the impacts of climate change.

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Appendix A

Missing Data Analysis

Any missing data is a loss of information. In order to trust the results from an analysis, we need to know that values are not systematically missing. Preferably, data should be missing completely at random (MCAR), that is, the probability of being missing should be equal for all observations (van Buuren, 2018, section 1.2). This is seldom the case. More often, data is missing at random (MAR). In such cases, the probability of being missing is equal only within groups defined by the observed data (van Buuren, 2018, section 1.2). For example, the probability of being missing might be higher for observations from Sweden than from Norway, but the probability of missingness is the same for all Swedish observations (i.e. probability of being missing depends on country affiliation). If data is neither MCAR nor MAR, the data is missing not at random (MNAR). This type of missingness is difficult to handle as it implies that there are some unknown factors that affect the pattern of the missing data.

Some adjustments have already been made concerning missing data for the analysis. Due to about 40 percent missing SPEI3 values caused by missing MIRCA values, the cells with missing SPEI3 values were deleted from the data frame (see section 5.2.2 for a full explanation of that choice). Moreover, where whole years are missing from a variable, I have imputed the missing information with that of the year closest in time. Still, even after these adjustments, some variables have a lot of missing information. In the following I will investigate whether the data is MCAR, MAR, or MNAR.

Table A.1 shows the percentage of missing values for each variable. *excluded*, SHDI and agricultural area of cell have the highest percentages of missing information. Particularly *excluded* and SHDI have levels of missing data that should be further investigated, with 16.7 and 10.4 percent missing information, respectively. Figure A.1 shows the missing data sorted by observation ID. There does not seem to be any pattern in the missingness across the variables. Figure A.2 confirms this suspicion. It shows a correlation matrix between dummy variables of the variables with the highest percentage of missing information, where missing data is set to 1 and non-missing data is set to 0. All correlations seem to be of acceptable size.

Table A.1: Missing Data Statistics

	Variable	Not missing	Number missing	Percent missing
1	excluded	962,449	192,731	16.7
2	shdi	1,034,550	120,630	10.4
3	agri_ih	1,097,550	57,630	5.0
4	gdp	1,118,999	36,181	3.1
5	global_ind	1,142,248	12,932	1.1
6	libdem	1,147,210	7,970	0.7
7	empl_agr	1,147,246	7,934	0.7
8	pop	1,154,700	480	0.04
9	irrig_sum	1,155,150	30	0.003
10	gid	1,155,180	0	0
11	year	1,155,180	0	0
12	gwno	1,155,180	0	0
13	lon	1,155,180	0	0
14	lat	1,155,180	0	0
15	non_state_conflict	1,155,180	0	0
16	spei3	1,155,180	0	0
17	spei3_pos	1,155,180	0	0
18	spei3_neg	1,155,180	0	0
19	temp	1,155,180	0	0
20	capdist	1,155,180	0	0
21	ttime_mean	1,155,180	0	0
22	conflict	1,155,180	0	0

Figure A.2 also displays the amount of observations that have missing values on different combinations of variables. Only the variables with high percentages of missing information are displayed. With more than one million total observations, there is a relatively small number of observations that have missing values on a combination of two or more of the variables. Moreover, the maps in figure A.3 do not signify that missingness depends on geographic location of the data. The missing information is not MNAR. Since there was some correlation between the missing data in figure A.2, I can neither conclude that the missing information is MCAR. The missing data on the three variables with considerable amount of missing information can be classified as MAR.

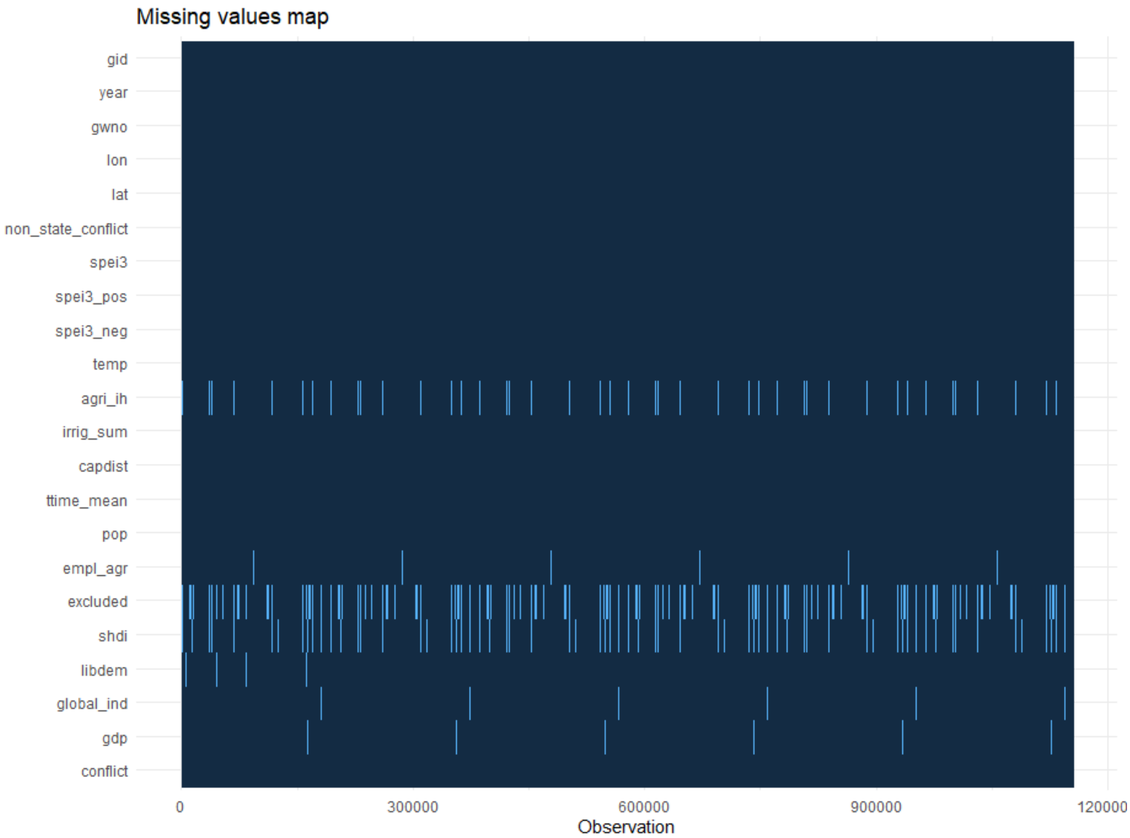
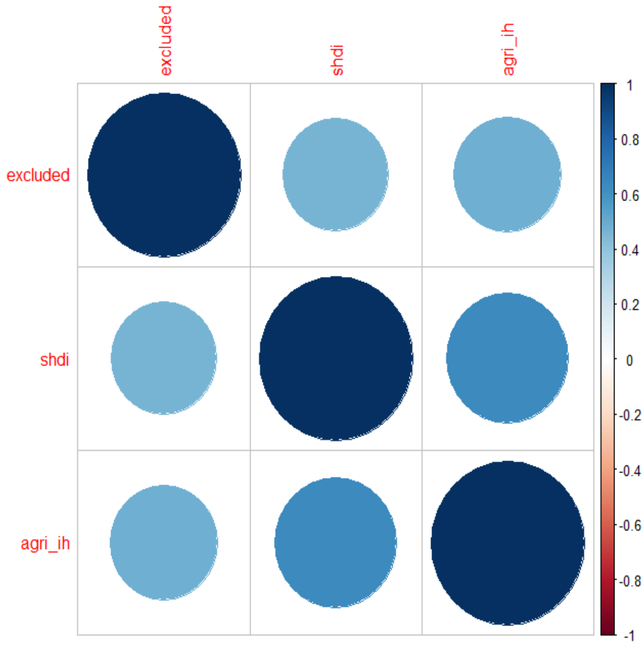


Figure A.1: Heath Plot of Missing Values
This figure shows a heat plot of missing values. The horizontal axis show the observation number, while the vertical axis show the variable name. Missing values are marked with a light blue line.



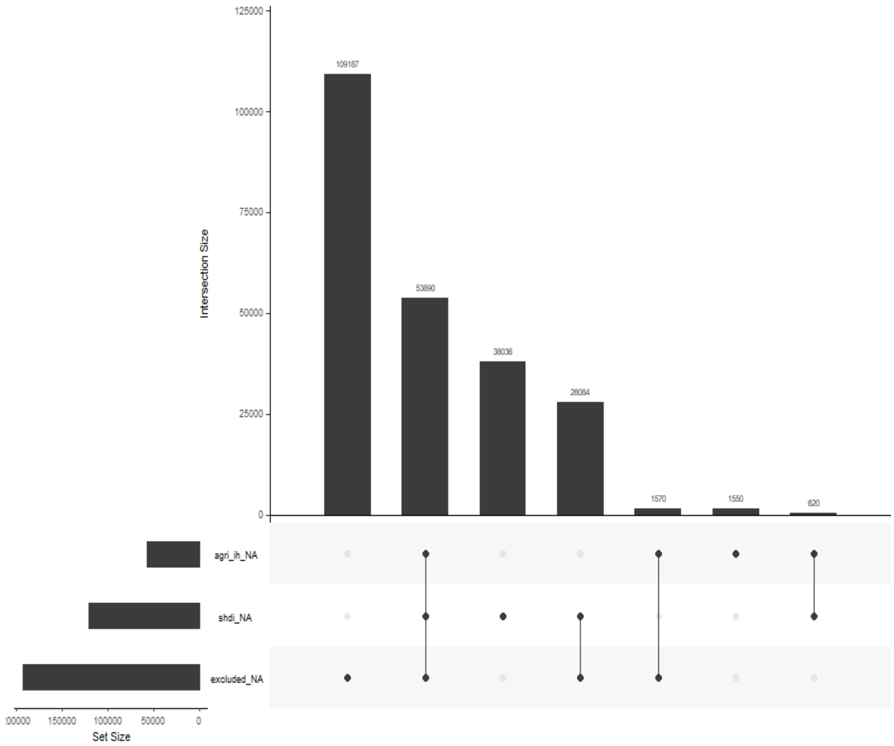
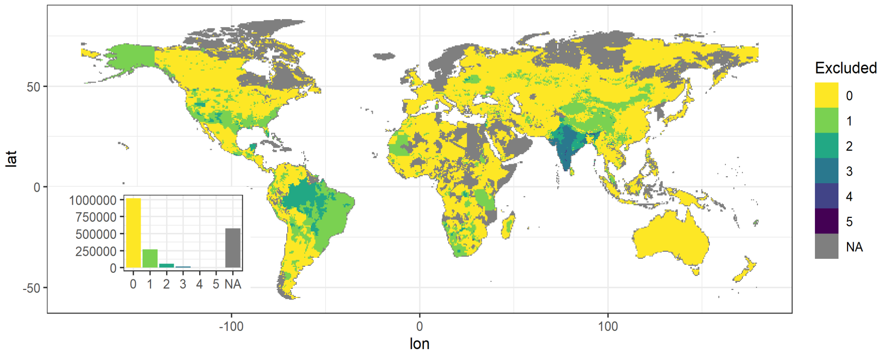


Figure A.2: Missing Values Correlations
This upper figure shows a correlation matrix between dummy variables with missing information. The bottom figure shows the amount of observations that have missing information on different combinations of the variables.



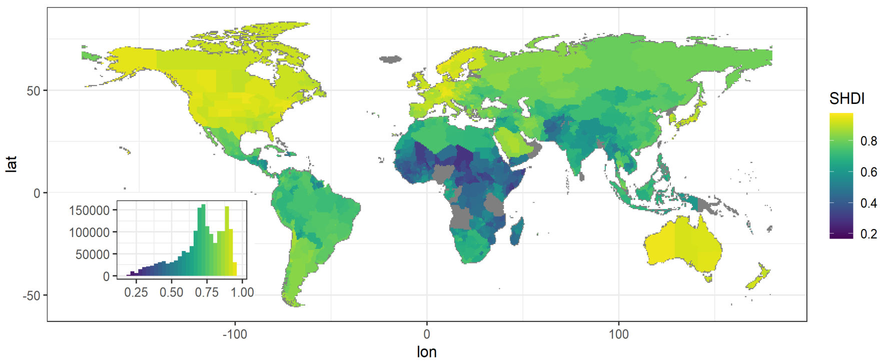


Figure A.3: Geographic Distribution of Missing Data, Excluded and SHDI
The maps show the geographic distribution of values, including missing values, on the variables "excluded" and "SHDI". The upper map shows the distribution of the "excluded" variable, while the bottom map shows the distribution of the "SHDI" variable. The geographic distribution of the missing information is equal for all years. However, the distribution of the other values on the variables is just the mean across the years.

Appendix B

Summary of Data Sources

Theoretical concept	Operationalization	Variable name in dataset	Dataset source
Violent, state-based conflict	Violent, state-based conflict	*conflict*	UDCP GED v. 19.1
Scarce rainfall event	Negative values of SPEI3	*spei3_neg*	CRUTS v. 4.03, weighted with use of MIRCA2000 CPL
Excess rainfall events	Positive values of SPEI3	*spei3_pos*	CRUTS v. 4.03, weighted with use of MIRCA2000 CPL
Country	Country number	*gwno*	PRIO-GRID v. 2.0, Gleditsch and Ward system membership list and cShapes geometry
Dependency on rain-fed agriculture	Percentage of cell covered by agricultural land	*agri_ih*	PRIO-GRID v. 2.0
	Total are of cell covered by irrigated	*irrig_sum*	PRIO-GRID v. 2.0
	Percentage of employed population	*empl_agr*	World Development Indicators
Temperature	Temperature	*temp*	CRUTS v. 4.03, weighted with use of MIRCA2000 CPL
Population density	Population density (number of people	*pop*	GPW v.4 Revision 11
Globalization and urbanization	KOF globalization index	*global_ind*	KOF Globalization Index 2019
	Distance to capital (in kilometers)	*capdist*	PRIO-GRID v. 2.0
	Avarage travel time (in minutes) to nearest urban center	*ttime_mean*	PRIO-GRID v. 2.0
Regime type	Liberal democracy index	*libdem*	V-DEM v. 9
Good governance	Subnational Human Developmen Index (SHDI)	*shdi*	SHDI
	Gross domestic production per capita (GDP)	*gdp*	V-DEM v. 9
	Level of political exclusion of ethnic groups	*excluded*	EPR Core v. 2014 and GeoEPR v. 2
Prevalence of non-state conflict	Non-state conflict	*non_state_conflict*	UDCP GED V. 19.1

Table B.1: Summary of Data Sources

Appendix C

Robustness Checks

Figure C.1 and C.2 show CATEs for different subgroups across all variables. Figure C.3 and C.4 show the CATEs with a continuous SPEI3 variable as the treatment, instead of having a dichotomous treatment variable, as was used in the analysis. Note that the x-axis of the plots vary. That is done to allow the full confidence intervals of the quantiles with the highest uncertainty to be displayed.

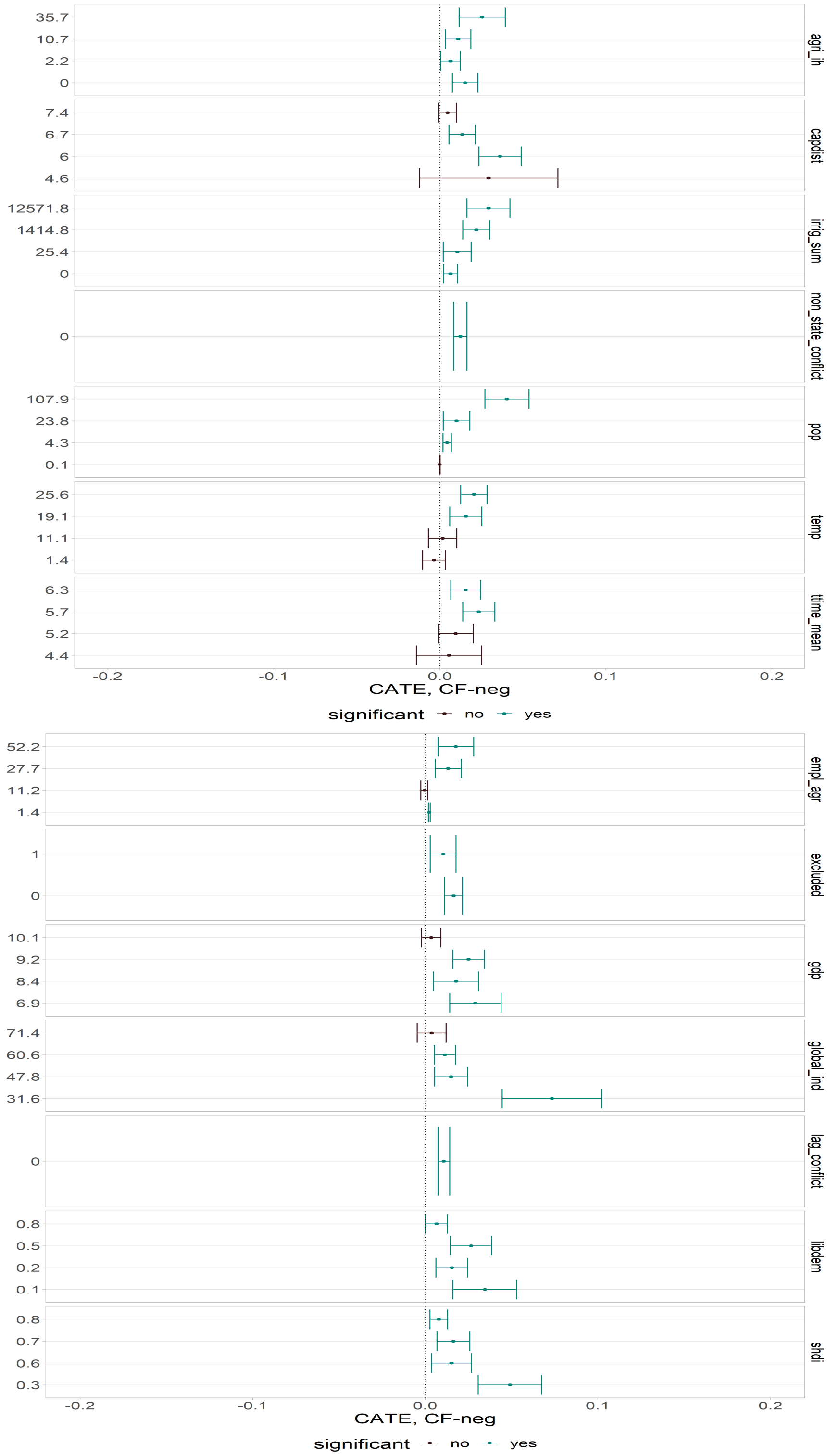


Figure C.1: CATEs CF-neg, all Variables

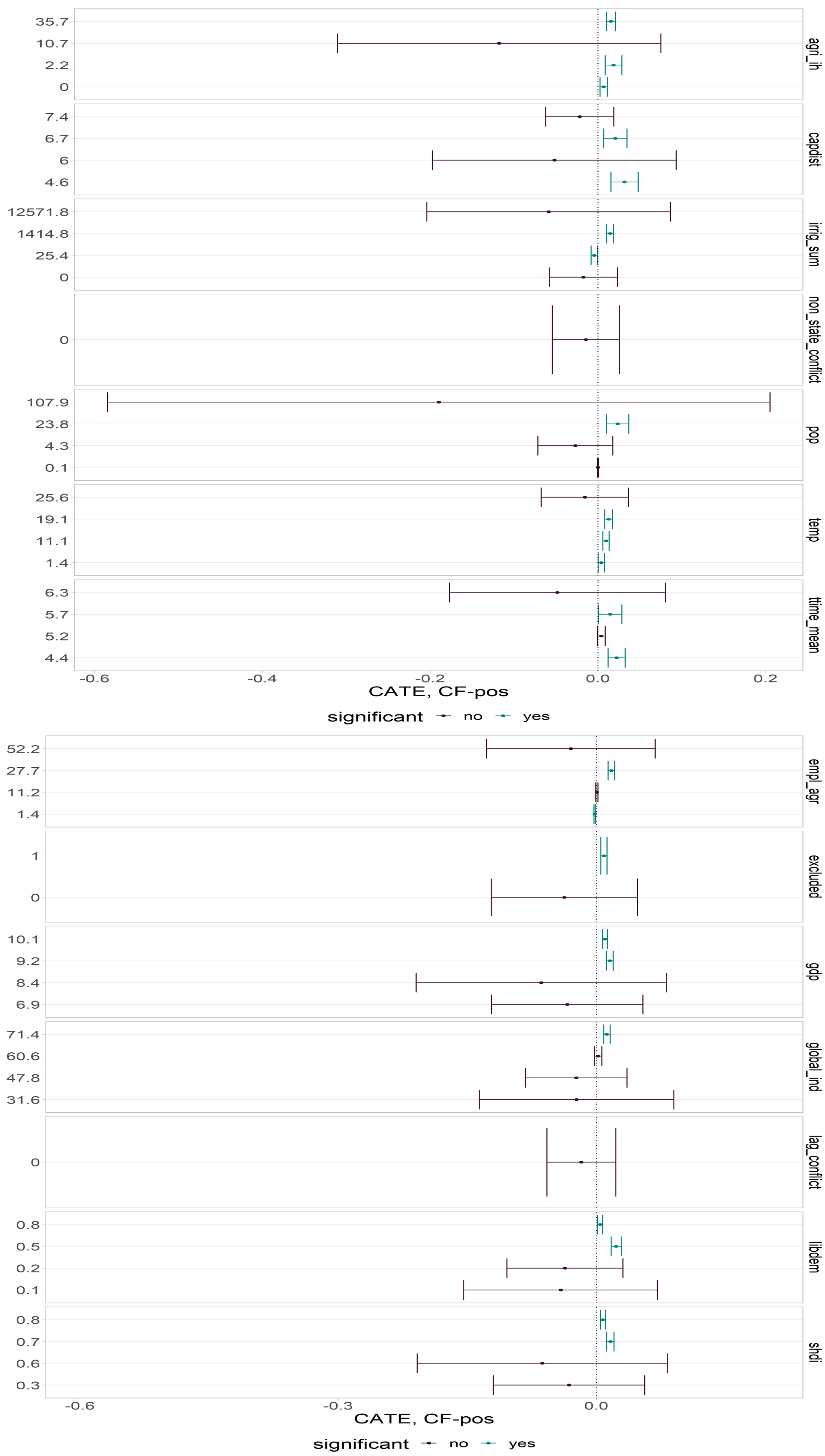


Figure C.2: CATEs CF-pos, all Variables

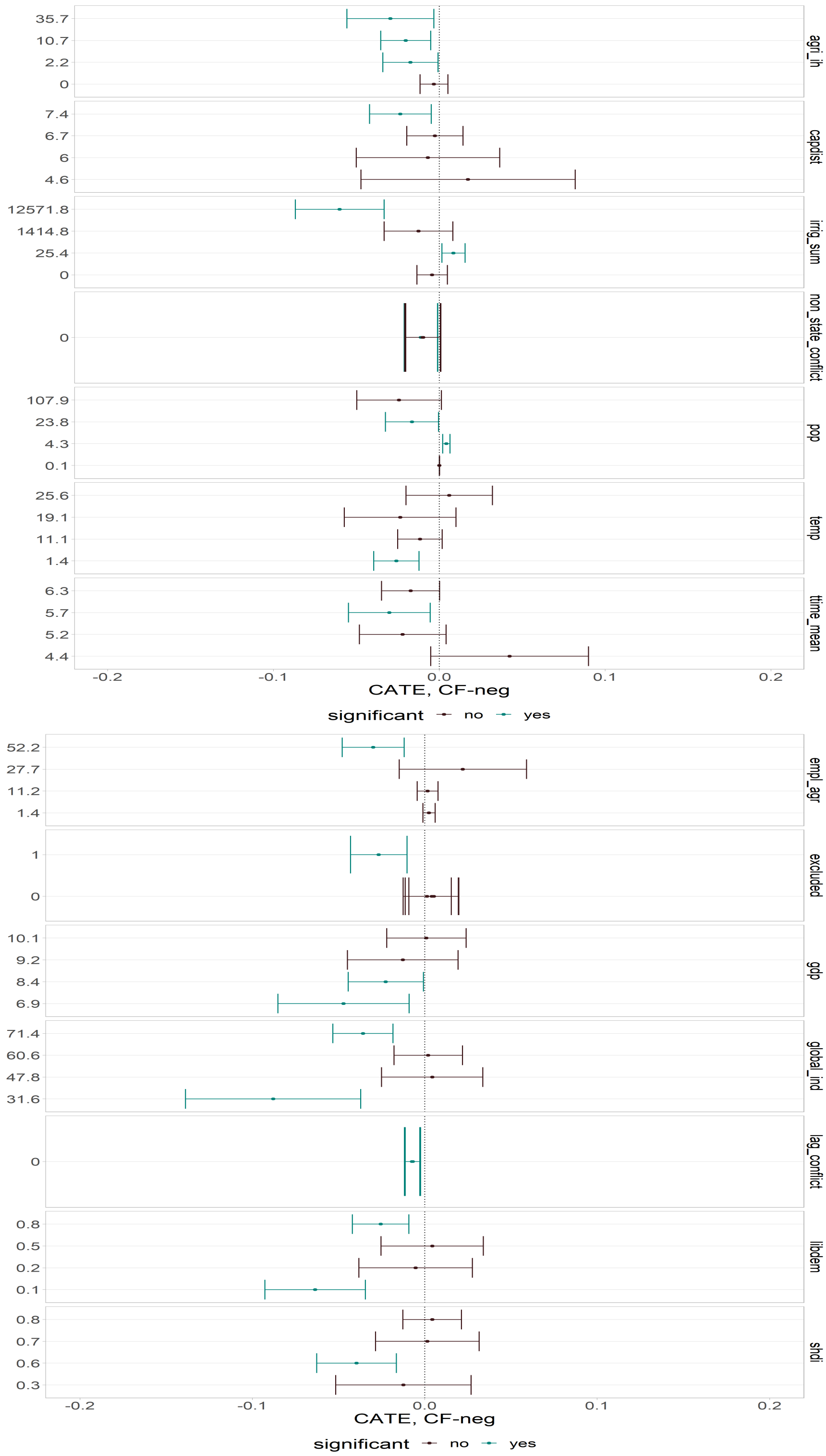


Figure C.3: CATEs CF-neg, with Continuous Treatment Variable

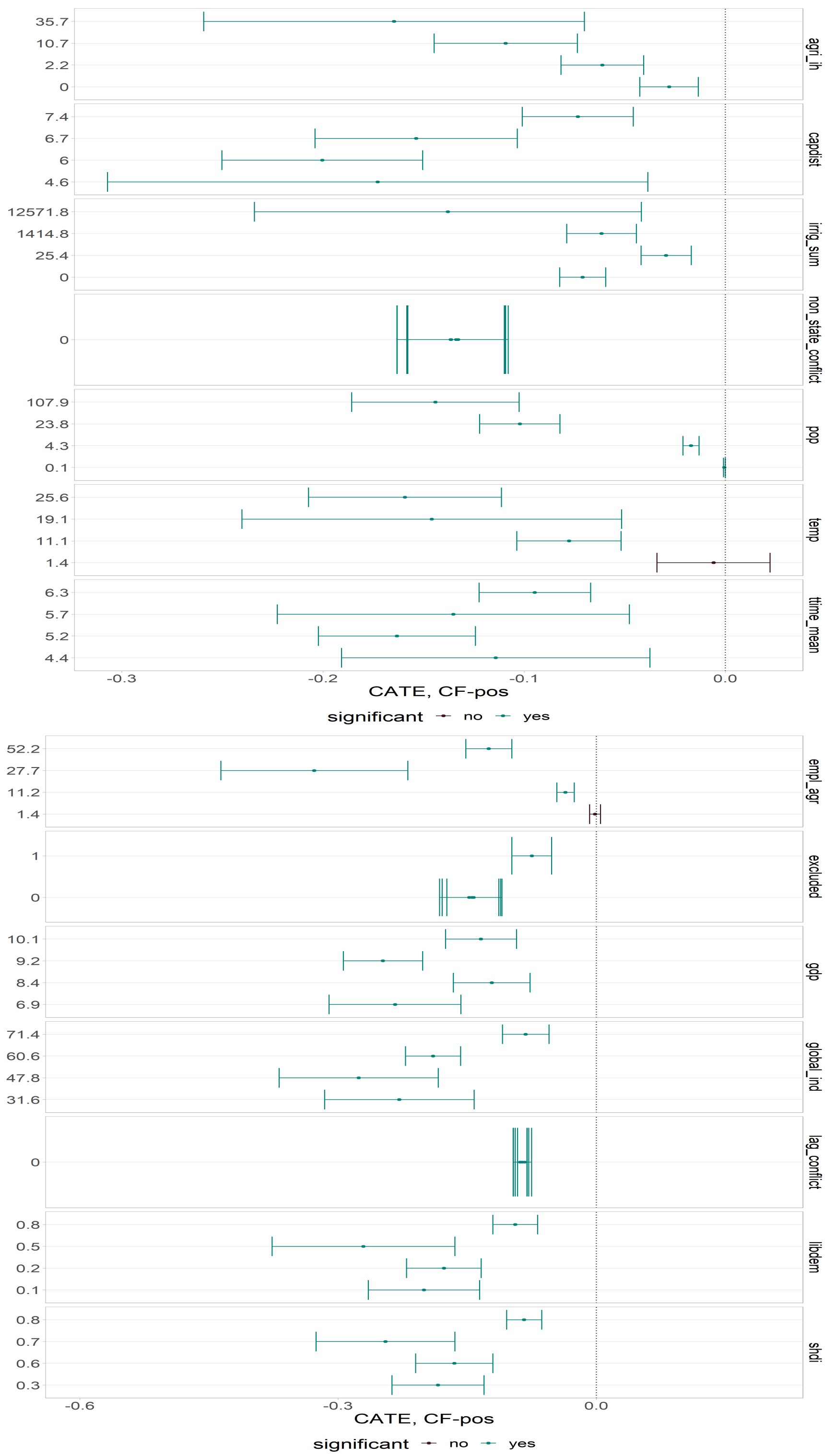


Figure C.4: CATEs CF-neg, with Continuous Treatment Variable