Climate, development, and conflict: Learning from the past and mapping uncertainties of the future

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During the course of writing this dissertation, I changed my name from Nordkvelle to Vestby. My original name appears on the two articles published before my name changed.

Part I

1 Introduction

1.1 Motivation and overview

Will climate change increase the prevalence of violent conflict in the future? And if so, why? To provide convincing explanations that can answer these questions, we need both to *learn from the past*, and to *map our uncertainties about the future*.

There is an active field attempting to learn about the relationship between climate and violent conflict through the use of historical data. In recent years, researchers have recognized the need to study climate-conflict relationships at a finer resolution and for particular conflict types in order to ask more context specific questions. This has raised new *methodological issues*, which I address. Solving these issues will result in making our explanations more convincing.

The effects of climate on the likelihood of violent outcomes are *context sensitive*. In order to generate better expectations about the relationships between climate and conflict in the *future*, we therefore need models of how the relevant contexts will change. This dissertation contributes to the work on building models of future political and socio-economic development by mapping both the likelihood of armed conflicts and the uncertainties about this likelihood, far enough into the future for climate change to be well underway. Improving our expectations about relevant future contexts is crucial for arriving at convincing explanations about the relationships between climate change and violent conflict.

The relationship between climate and conflict received international attention after Al Gore and the IPCC were awarded the Nobel Peace Price in 2007. Rajendra K. Pachauri, the former chairman of the IPCC, who gave the Nobel Peace Price Lecture in 2007, proposed a very broad definition of peace as presence of human security such as access to clean water, sufficient food, and stable health conditions (Pachauri 2007). Two years later, Barack Obama emphasized the conflict potential of climate change, arguing that "[t]here is little scientific dispute that if we do nothing, we will face more drought, more famine, more mass displacement — all of which will fuel more conflict for decades" (Obama 2009). He explicitly noted that US military leaders "call for swift and forceful action" because they "understand our common security hangs in the balance" (Ibid.). Over time, the climate-conflict debate has moved from a humanitarian debate to a security debate. And although the humanitarian issues are broadly accepted, it is not true, as Obama claimed, that there is little *scientific dispute* about the security issues (Nordås and Gleditsch 2007; Salehyan 2008; Bernauer, Böhmelt, and Koubi 2012; Gleditsch 2012; Ciccone 2011; Miguel and Satyanath 2011; Hsiang, Burke, and Miguel 2013; Buhaug et al. 2014; Salehyan and Hendrix 2014).

Despite disagreements, three conjectures can be drawn from the research as it stood around 2013-2014 when I started working on this dissertation. First, different kinds of conflicts are likely to be affected differently (Buhaug et al. 2014). The IPCC reported that the effect of climate variability on armed conflicts was weak and uncertain. Stronger results could be found for non-state (communal) conflicts in resource-dependent economies, as reported by "a growing body of research" based on newly coded sub-national event data (Adger et al. 2014, p.772). Second, in order to ask more theoretically relevant questions, researchers need to create more context specific studies with *disaggregated* data (Miguel and Satyanath 2011; Buhaug et al. 2015; Salehyan and Hendrix 2014). Third, the socio-economic and political context is and will be very important for how climate exposure affects the likelihood of conflicts (IPCC 2014, Figure SPM.1). Often missing from the debate is the fact that the context is changing quickly. Education levels are generally increasing (Barro and Lee 2013; Lutz, Butz, and KC 2014; Lee and Lee 2016), maternal, newborn and child health is improving (Alkema et al. 2016; UNICEF 2017) and growth-rates are high in many of the countries where we observe communal conflicts today (Diao, Harttgen, and McMillan 2017).

A main contribution of this dissertation, as discussed in Section 1.5.1, is its engagement with the generation of forecasts that can give us better indications of the *future* socio-economic and political context within which climate change will happen. Specifically, my co-authors and I contribute to this debate in an article that estimates baseline forecasts of the likelihood of armed conflicts at the country-year level up to 2100, using quantifications of the Shared Socio-Economic Pathways (SSPs) (Moss et al. 2010; O'Neill et al. 2014, 2017). The SSPs are developed for the climate impact community.

This article offers an important political context for these pathways, which can be relevant also for other types of climate impacts than violence.

Another significant contribution of this dissertation, as I will discuss in Section 1.5.2, is to identify methodological issues in recent disaggregated studies of the effect of climate variability on different types of violent conflict. Three of the four articles in this dissertation provide solutions to the identified issues. Providing methodologically convincing estimates in this field is important because the published estimates do not clearly point in one direction (Gleditsch 2012; Buhaug et al. 2014; Salehyan and Hendrix 2014). Some of this variation could be due to identification, rather than substantial issues. Since sound identification of causal effects seems to be within reach in climate-conflict research, and because the estimates will be used to extrapolate *future* causal effects of the relationship between climate change and violent conflict (Hsiang, Burke, and Miguel 2013), it is important to attempt to identify and solve methodological issues.

The identified methodological issues are related to our theoretical understanding of how violent conflicts arise and of different types of conflicts. One example of this is studies using data from the Armed Conflict Location and Event Data project (ACLED) (Raleigh et al. 2010) without distinguishing the different conflict types in the dataset. The ACLED data consist of both "rebel conflict events" and "communal violence" (Raleigh and Kniveton 2012). We know that actors in "rebel conflict events" (i.e., events in armed conflicts) can have been recruited from a many areas of a country, and that armed conflict campaigns can travel over large distances. Thus, matching high resolution climate data to the whereabouts of conflict events is likely to be a bad way to test motivational theories of why individuals chose to join armed groups.

An important aspect of the methodological solutions I offer is to make informed choices about the unit of analysis when testing theoretically relevant causal hypotheses at the sub-national level for particular contexts and conflict outcomes. Higher resolution data is not always a benefit, as the increased resolution will require that causal models account for ever more specific types of conflict dynamics. In the four articles in this dissertation, I use four different units of analysis: countries were studied when forecasting propensity for civil conflicts; living areas of politically excluded ethnic groups, when studying the effect of precipitation on the probability of an armed conflict onset for such groups; large areas known for being inhabited by non-state groups fighting each other, when studying the effect of droughts on the probability of observing violence between these groups; and surveys of individuals, when studying whether experiencing a deterioration in living conditions increases the probability of participation in political violence. All four articles actively engage with theories of conflict (i.e., explanations) at some specific level of analysis, context and conflict outcome.

Environmental vulnerabilities, as well as the ability and need for mobilization into violent collective action, *vary* depending on the type of conflict actors engage in and the context in which they find themselves. Since both conflict theories and empirical results suggest that we should expect different impacts of climate variability on conflict outcomes depending on the socio-economic and political context and the conflict type, convincing causal estimates of the effect of climate variability in different contexts are needed as well as good forecasts as possible of the socio-economic and political context in the future.

This dissertations two main contributions (forecasting and solving methodological issues in models aimed at testing theoretical claims on disaggregated data) combine to offer a more **convincing explanation** to answer the question *when and why will climate change increase the prevalence of violent conflict?*

My current best answer, based on the studies presented in this dissertation as well as on the research literature, is that climate change is likely to increase the probability of communal conflicts in resource-scarce areas where the government is failing to provide services, particularly in areas where conflict dynamics are directly related to resources, such as pastoralist-farmer conflicts in the Nigerian highlands, cattle thievery in Eastern-Africa, and land occupations by landless workers in Brazil (Fjelde and von Uexkull 2012; Raleigh and Kniveton 2012; Hidalgo et al. 2010; Nordkvelle, Rustad, and Salmivalli 2017).

Although we can observe an effect of climate variability on communal conflicts, policy actors should not think about these conflicts as "climate conflicts". Communal conflicts are only found in very specific parts of the world. Since extreme weather is also prevalent in areas where conflicts do not occur, it cannot be a main explanatory factor of communal conflicts; if it were, there should have been many more such conflicts. As other researchers have documented, other explanations - such as lacking state capacity and biased state policies - are more salient in explaining why these conflicts occur where they do (Benjaminsen and Ba 2009; Benjaminsen et al. 2012; Elfversson 2015).

It is not given, however, that the contextual conditions which make communal conflicts possible will endure into the future. Some countries that experience communal conflicts today, for example, Kenya and Nigeria, are also experiencing rapid socio-economic development. It is possible that the governments in these areas will become able and willing to provide services that can deal with the non-state conflicts within their borders by 2050. Supporting socio-economic development in areas where we today observe communal conflicts should be high on the agenda for reducing the conflict-propensity of climate extremes and climate change into the future.

Although there is empirical support for believing that climate can affect communal conflicts as described above, the evidence that adverse climatic conditions increase the likelihood of armed conflicts is much weaker (Buhaug 2010b; Miguel and Satyanath 2011). One possible explanation for this difference is that actors in armed conflicts - i.e states and organizations daring to oppose the states - are resourceful and well organized. Such organizations are less environmentally vulnerable than are less organized groups. A state military is less vulnerable to climate variations than is a small group of herders. In addition, it is possible that more extreme climatic exposures which affect even resourceful organizations can lead to a breakdown of organizational capacity, and, therefore, of the capacity to mount violent collective action against a state.

Another explanation for the difference in observed effects between different conflict types may be that the aims of violence in communal and armed conflicts differ. Whereas communal conflicts in many cases are aimed at direct, pecuniary gains (or prevention of pecuniary losses), armed conflicts aim for a change of government or secession of territory. Although private and local gains also permeate armed conflicts (Kalyvas 2006), and although armed groups often recruit rebels by paying wages, such opportunistic behavior is made possible by the larger conflict. In the case of communal conflict, the behavior of those involved is made possible by structural conditions in society - a lack of state capacity or willingness to police, for example (Elfversson 2015). Since climate shocks mainly modulate pecuniary losses, and not necessarily the opinion towards the government or state capacity, the lower/zero effect on armed conflicts makes theoretical sense.

The substantial conclusion in this dissertation about these matters does not deviate from the IPCC report from 2014 (Adger et al. 2014). The dissertation does offer an explanation that is better substantiated (meaning one that we should have a higher confidence in), than that of the 2014 IPCC report. Additionally, it offers information about the future context of climate change and starts a fruitful debate about model assumptions, in order to generate better expectations about whether climate change will increase the propensity for violent conflict in the future.

1.1.1 Structure of the introduction

This introduction provides the reader with context to the articles in the dissertation. The plan for this introduction is as following.

The key concepts of the dissertation - violent conflict and climate - are defined in Section 1.2.

Previous literature on the quantitative study of climate and conflict is reviewed, and two main unresolved issues are identified: the need to take into account future changes in the context and the methodological issues of causal inference in disaggregated studies. The literature review is divided into three sections (1.3, 1.4 and 1.5). Section 1.3 looks at correlational studies of climate change and conflict over the last 1000 years. Section 1.4 reviews causal studies of climate variability and conflict in the last 70 years. Section 1.5 identifies unsolved issues in the literature, and explain in detail the main contributions of this dissertation.

Section 1.6 outlines a theory of violent conflict. It analyzes theories from both an actor and structure perspective, and argues that theoretical expectations of the effect of climate variability should differ depending on the type of conflict being studied. A conclusion of this dissertation is that group-level and structural (contextual) issues are important for understanding the relationship between climate variability and conflict outcomes. The theoretical section therefore draws on a wider theoretical literature than just the individually focused opportunity-cost theory.

The overarching methodological framework is presented in Section 1.7. Here, the role of model prediction/forecasting and causal inference in explanation are explained. I argue that both approaches can help us explain *when* and *why* (if at all) climate change can affect the likelihood of violent conflict in the future. Section 1.7.1 explains the potential outcomes approach to causal inference, and why climate variability measures can be used in causal inference.

This dissertation uses methodological concepts from the field experiment literature, rather than the more standard econometric treatment of causal inference. In Section 1.7.2, I make the argument for using the more practical language from the field experiment literature in the context of causal inference of climate variability on violent conflict. I also encourage the use of the concept "randomization" outside the context of experiments.

Section 1.8 contains descriptions of the four articles in the dissertation and their main findings. I put each of these studies in context and address the questions and problems I was considering when initiating these studies.

Finally, I offer a conclusion in Section 1.9, in which I offer answers to the overarching research question posed in this introduction and suggest pathways for future research.

1.2 Key concepts

1.2.1 Violent conflict

I define violent conflict as a phenomenon that must involve at least one organization with a contested claim against another organization or group, where at least one of these groups is using violent force against the other group. I use violent conflict interchangeably throughout the dissertation with political conflict, group conflict and conflict.

An organization consists of more than one person. The criteria for exactly what makes several people an organization are deliberately kept vague. However, an organization must have some ability to coordinate and discuss strategy, and the individuals within it must act on behalf of the organization. Lone wolf terrorists, for instance, Anders Behring Breivik, are not included in this definition.

In three of the four articles, I use conflict data from the Uppsala Conflict Data Program (UCDP). This group defines a conflict event in their Georeferenced Event Dataset (GED) as "[a]n incident where armed force was used by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date" (Croicu and Sundberg 2017, p.9, bold and underline removed).

UCDP-GED distinguishes among state-based, non-state and onesided violence. State-based conflicts are conflicts in which at least one party is the government of an independent state, whereas non-state conflicts are conflicts between non-state groups (with differing levels of organization). One-sided violence is violence perpetrated by an organized actor against civilians.

State-based conflicts and armed conflicts are defined similarly. UCDP/PRIO define an 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" (Themnér 2016, p.1). Inter-state conflicts and civil conflicts are both state-based conflicts. The the former involves two or more state groups, whereas the latter is between a state group and one or more non-state groups. This dissertation looks only at civil conflicts, and uses state-based conflicts, armed conflicts, and civil conflicts as synonyms.

An important difference between the UCDP-GED state-based event data, and the UCDP/PRIO armed conflict data is that UCDP-GED includes events in years where the number of battle-related deaths dips below 25 ("non-active years"), as long as the conflict-dyad involved over 25 battle-related deaths at some point in time (Croicu and Sundberg 2017, p.15).

Non-state conflicts are parsed in the UCDP data according to the group's level of organization (Sundberg, Eck, and Kreutz 2012). They distinguish between formally organized groups (with capacity to be included in state-based conflicts), and two types of informally organized groups: supporters and affiliates of political parties, and "groups that share a common identification along ethnic, clan, religious, national or tribal lines" (Allansson and Croicu 2017, p.4). The conflicts involving the latter type are commonly called communal conflicts; it is this type of non-state conflict that I analyze in this dissertation. Since the only type of non-state conflict that I discuss is communal conflict, I will use these two terms interchangeably throughout the dissertation.

UCDP-GED removes the criterion that there must be an incompatibility, other than that state-based conflict events must be between groups also included in the UCDP/PRIO armed conflict dataset where only actors with a stated incompatibility against the state are included (Croicu and Sundberg 2017; Themnér 2016). For event data, intent can be difficult to ascertain. Violence can indeed be committed in the absence of any perceived incompatibilities, contested claims or reasons. A group can suddenly engage in violence for no apparent reason. However, at a theoretical level, the assumption will always be that violence is instrumental for some other goal (i.e., there is a contested claim), and not an end to itself. I use “contested claim”, rather than “incompatibility”, because some contested claims that result in violence need not involve an incompatibility. For instance, if I am a cattle raider, I might want “a cow”, rather than “that cow”. A solution can be found through cooperation, with both parties gaining cows. In armed conflicts, “incompatibility” is more often the case, as the contest is over a particular territory or control over a particular government.

When I use a particular UCDP dataset, I employ the definitions of that dataset. When discussing all violent conflict in general, I adhere to the definition offered at the beginning of this section.

1.2.2 Climate

Climate is the statistical description of mean and variability of weather. Weather describes “the conditions of the atmosphere at a certain place and time with reference to temperature, pressure, humidity, wind, and other key parameters (meteorological elements); the presence of clouds, precipitation; and the occurrence of special phenomena, such as thunderstorms, dust storms, tornadoes and others” (Cubasch et al. 2013, p.124ff).

In conflict literature, *climate variability* refers to shorter-term statistical descriptions of weather, e.g. weekly, monthly, seasonal and yearly. Climate variability is different from the phenomena we call *climate change*, which is classically defined as occurring over over 30-year or longer periods. The climate has changed when there is a statistically significant difference between measurements in a time-series of 30 year moving average and variance of some climatological variable. Those worried about climate change, are not concerned about differences among seasons, or about an odd cold or warm year; rather, they are worried about long-term changes in the climate norm (such as changes in the long-term mean or variance of climate measurements).

The climate, world-wide, is changing, and this is due to increased radiative forcing. The main reason for increased radiative forcing is increased concentration of CO_2 and other greenhouse gases in the atmosphere (IPCC 2013b, Figure SPM.5). Other possible future drivers of changes in radiative forcing, are the ice-albedo feedback, vegetation feedbacks and the salt-advection feedback (NOAA 2008).

Increased radiative forcing has led to temperatures going up in most parts of the world, already resulting in a global average warming of .85°C since 1880 (IPCC 2013b). For precipitation, the story is more complex. The general theoretical idea is that increased temperatures will lead to more evaporation (mainly from the sea). At the same time, however, the atmosphere will be able to hold more water. In some areas, the end result will be more precipitation, whereas other areas will observe less precipitation (IPCC 2013a, FAQ 3.2). Researchers are still not certain how monsoons will be affected by climate change (FAQ 14.1), although the climate models project that, for the global monsoon area, total precipitation and intensity will increase (Christensen et al. 2013, p.1226). Indeed, the Intergovernmental Panel on Climate Change (IPCC) has low confidence that anthropogenic forcings have increased the prevalence of droughts on a global scale, but high confidence that this is the case in the Mediterranean, West Africa, central North America and north-west Australia (Hartmann et al. 2013, p.215).

There are many different climate variability measurements in use. The ones most commonly used in conflict research are based on measurements of temperature and precipitation. Rather than using absolute measurements, researchers often use growth in these measurements, or separation of growth into positive growth and negative growth. Is is also common to standardize the measurement, which means to subtract the mean and divide by the standard deviation. A choice must be made as to which mean and standard deviation should be used. The easiest is to take the global mean and standard deviation for the whole variable in the dataset. However, it may make better sense to use a running mean and standard deviation for a period of time before the measurement. For monthly data, it can also make sense to use the mean and standard deviation for a particular month in time, rather than to use the global mean and standard deviation.

Two more specialized versions of the standardized variable are the Standardized Precipitation Index (SPI) and the Standardized Precipitation-

Evapotranspiraition Index (SPEI). At its simplest, the SPI can be viewed as a standardized precipitation where each month of the year has been standardized using that month's historical average and standard deviation. However, since precipitation is not generally normally distributed, is has been argued that we should use a gamma distribution to estimate the probability of a given level of precipitation (McKee, Doesken, and Kleist 1993). In this way, SPI is better than standardized precipitation at making a connection to unusual precipitation. SPI is also calculated according to different lengths of time. SPI-1 is calculated on 1 month periods (as described above). An SPI-6 uses the average precipitation over a given 6 month period, rather than over a given month period.

The difference between SPI and SPEI is that SPEI includes temperature through an estimate of potential evapotranspiration (PET), and it uses the log-logistic distribution (Vicente-Serrano, Beguería, and López-Moreno 2010). Since it takes into account both precipitation and evapotranspiration, SPEI arguably measures droughts better than does the SPI. Instead of standardizing precipitation, SPEI is standardizing precipitation minus PET. There are different ways to estimate PET. I will be using the Thornthwaite method, which is generally considered the simplest (Thornthwaite 1948). The benefit of this method is that PET can be estimated with just the latitude and month (to estimate sun hours) in addition to temperature. SPEI tends to be closer to a normal distribution than SPI, because it is a product of subtracting two relatively independent measurements. However, since temperature is increasing slowly over time, and SPEI uses the average over the whole time-series, using SPEI causes a potential problem that needs to be addressed when using SPEI is that high values (wet and cold) are more likely early in long time-series and low values (dry and warm) are more likely later in the time-series.

1.3 Is nature becoming less important?

The quantitative literature on climate and violent conflict can roughly be divided in two: long-term studies of the correlations between changes in climate and conflict patterns over the last 1000 years, and causal studies of climate variability and conflict since World War II when both climate and conflict measurements became better. The research suggest that social order is gradually becoming less dependent on the climate, probably due to technological advances, and that today, empirical relationships are mainly found in resource-scarce areas of poor and conflict-ridden countries where the government is unable or unwilling to solve violent conflicts between non-state groups.

Historians have discussed the possible relationships between climate change, disease, population change, social disorder, and conflict. Fraser (2011) describe the situation in Europe during the start of what is called 'The Little Ice Age': the tumultuous time from 1300 onwards[1].

Temperature increased in the Early to High Middle Ages (700–1200 BC), a period in which Europe was characterized by "population growth, a rising standard of living, a pleasant climate, and institutional changes centered on the monastery" (p.1273). However, Fraser argues that institutions gradually became corrupted, with large income inequality, inflation, over-population and a shaky food supply system (p.1276f). At the same time as the climate cooled down, Europe witnessed several terrible famines, such as in the years 1314–1316, during which "possibly 10% of Europe perished" (p.1275). The next decades would bring even more famines, peasant uprisings, the Black Death, and large-scale wars.

Goldstone (1991, p.459ff) offers a similar type of explanation for the periodic state breakdowns between 1500 to 1850. His emphasis, however, is on population growth in the context of inflexible social structures, rather than the climatic exposure. Fraser (2011, p.1270) argues that the food price volatility observed by Goldstone can be explained by climatic events.

Zhang et al. (2007) base their work on reconstructions of climate, price inflation, famine, population, and war outbreak in Asia and Europe during the Little Ice Age (which they set at 1450–1850). From the successive correlational patterns of these variables, they argue that "worldwide and synchronistic war-peace, population, and price cycles in recent centuries have been driven mainly by long-term climate change" (p.19214). Using Granger-causality tests, Zhang et al. (2011) find that none of their proposed hypotheses that relate climate change to bio-productivity, agricultural production, epidemics, famine, population changes, nutritional status, migration, social disturbance, or war can be rejected (Zhang et al. 2011).

Tol and Wagner (2010) confirm the relationships found in Zhang et al.

1. The exact start and end dates of the Little Ice Age are still debated, and estimates vary by several hundred years.

(2007) using a longer time-series (1000–1990) in Europe. However, they only find a significant relationship between temperature and conflict between 1300 and 1650. Their argument is that the many changes to human society that occurred with modernity may have reduced the dependency between nature and social order.

Historical studies are interesting for several reasons. The increased variation in all relevant parameters which can be seen by looking further into the past improves our ability to think about the relationship between nature and social order (or climate change and conflict) today and into the future. Demographic change, disease, technology, trade, monetary politics, international relations, and state institutions all affect how changes to natural endowments will play out.

Tol and Wagner (2010) are unable to find strong correlations between nature and social order in Europe after 1600, but that does not mean that there cannot still be pockets of societies, now or in the future, where nature has an important effect on social stability. As the Goldstone study reveals, although climate may have played a role in destabilizing states, it did so through important interactions with various social structures and developments.

All of these historical studies are correlational studies, meaning that care must be taken in interpreting these relations as causal. Tol and Wagner (2010) also show how precarious the data-situation is for these historical stretches of time. Therefore, although such studies are useful for exploring empirical data with more variation in climate, it is difficult to convincingly separate out and estimate the effects of individual exposures.

Observations of weather have been made for millennia. However, it was only with the development of weather observation networks — the first established by Ferdinando Il de Medici in 1654 — that systematic collection improved. An important technology that kick-started these networks was the telegraph, which made it possible to send weather data from large areas to a central hub in a short time. By 1900, there were well established national meteorological services were well established in Europe, India, Japan, Australia, and the US. Yet, global coverage was not attained until after WWII. A major development was the launch of weather satellites, the first being TIROS-1 in 1960. Today, weather is surveyed continuously by a wide range of satellites and automatic/manual weather stations. This means that conflict researchers are limited to studying the world after

WWII if they want to use systematic, high resolution and reasonably reliable weather data (Becker et al. 2013).

The first quantitative and arguably causal study of climate variability and conflict was done by Miguel, Satyanath, and Sergenti (2004). Their study was made possible by the development of consistent country-year data on armed conflicts after WWII (Gleditsch et al. 2002), the development of global high-resolution datasets on precipitation and temperature (Adler et al. 2003), and an ongoing debate over the effects of economic development on armed conflict (Fearon and Laitin 2003; Collier and Hoeffler 2004). They argue that yearly growth in rainfall within countries can instrument per capita economic growth, and show that instrumented economic growth reduc the probability of armed conflict onset.

Burke et al. (2009) and Buhaug (2010b) tested many alternative climate variability specifications and control methods in country-year studies of armed conflict incidence and onsets in Africa and globally. Burke et al. (2009) find an effect of absolute temperature in a model of civil war incidences (more than 1000 battle-related deaths) with fixed effects and a trend variable. Buhaug (2010b) shows that this effect is not significant for armed conflict incidences (Those with more than 25 battle-related deaths), or for civil war outbreaks.

Salehyan (2014, p.242) used the Palmer Drought Severity Index, which is a composite index using temperature, precipitation, and soil conditions to measure droughts. They find some evidence that water abundance increases the probability of armed conflict incidence and severity, but only in agriculturally dependent areas.

In a replication study of Miguel, Satyanath, and Sergenti (2004), Ciccone (2011) found that the results are no longer significant when using data from 1979 to 2009 (rather than to 1999). The following rejoinder by Miguel and Satyanath (2011) shows that growth in rainfall and economic growth are not correlated in the period between 1999 and 2008. They argue that it “may be related to Africa’s unprecedented recent economic growth in the past decade in nonagricultural sectors, as well as public policy changes perhaps stemming from spreading democratization” (p.231).

A theory that, therefore, gained support both from the long-term studies based on climatological data found in tree-rings and ice-samples, and from as climate shock data based on weather observations, is that modern societies and the social order within them have gradually become less sensitive to natural fluctuations in weather and climate.

1.4 The need for disaggregation

Modern state-building, technologies, and development seem likely reasons for a decreased dependency on nature by the social order at the state level. But all areas of the world are not equally developed, and all social groups are not equally able to deal with climatic variations. Both Miguel and Satyanath (2011) and Ciccone (2011) recognize this and conclude that disaggregated data (as opposed to country-level data) should be used in the future.

The first disaggregated studies were country specific, such as the studies of Hindu-Muslim riots in India (Bohlken and Sergenti 2010), and of land occupations in Brazil (Hidalgo et al. 2010). The availability of regional conflict event data has spurred more disaggregated studies, and climate-conflict researchers have been among the first to embrace this data (Raleigh et al. 2010; Salehyan et al. 2012; Sundberg and Melander 2013).

Hidalgo et al. (2010) studied events in Brazil in which social activist groups, such as the Landless Workers Movement, occupied land to settle and grow agricultural produce. These land occupations are sometimes non-violent, but they often result in violence between the landowners and those who are landless (p.507). The results of Hidalgo et al. (2010) are in line with the opportunity cost mechanism: landless workers are more willing to participate in occupations during periods when income from agriculture is relatively low. They speculate as to whether income shocks have a greater effect in areas where many are asset-poor, but not necessarily where there are many income poor, as the latter group would not be strongly affected by income shocks (p.518). They also found that the income shock effect is likely to be more pronounced in municipalities with high land inequality.

Bohlken and Sergenti (2010) studied of the effect of economic growth on Hindu-Muslim riots in 15 Indian states between 1982 to 1995, using percentage change (from one year to another) in rainfall as their instrument. They conclude that 1% increase in growth rates decreases the number of riots by 10% (p.599).

Another early attempt at disaggregation is Theisen, Holtermann, and Buhaug (2011), who analyzed civil war onset (coding the location within a country where the fighting started) using grid cells in Africa as their unit of analysis. Their causal inference relies on comparing the grid cell of the conflict onset location with a random sample of other grid cells in Africa that did not have an observed onset, using an annualized SPI-6 measurement and a variety of observed controls. Likely due to the relatively few armed conflict onsets, their estimates are highly imprecise, varying from possibly very large to very small.

Raleigh and Kniveton (2012) used ACLED data to study state-based ("rebel conflict") and communal violence in East Africa and whether rainfall variability affected the number of these different conflict events (Raleigh et al. 2010). Their study makes important distinctions among different conflict types, as well as between a scarcity hypothesis and a wealth-seeking hypothesis. The units of analysis were village-months with at least one conflict event, and they tested whether positive and negative rainfall deviations over these villages could explain the number of events in these villages (Raleigh and Kniveton 2012, p.55f). They find that both extremely wet and dry conditions increase conflict event frequencies.

Fjelde and von Uexkull (2012) did a similar study of communal conflict events. Unlike Raleigh and Kniveton (2012), they used data from UCDP-GED (Sundberg and Melander 2013), studied first-order administrative units in sub-Saharan Africa, used yearly instead of monthly temporal aggregation, employed unit fixed-effects, and were especially interested in whether the effect of standardized rainfall was exacerbated by political exclusion of ethnic groups. They did find a significant effect for negative rainfall anomaly, but not for positive rainfall anomaly. Although this finding can be seen as contradicting that of Raleigh and Kniveton (2012), Fjelde and von Uexkull argue that the abundance thesis possibly only works well in the context of cattle-raiding in East Africa, and that the effect is canceled out when looking at a larger subset.

Hendrix and Salehyan (2012) studied whether deviations in rainfall patterns could affect the propensity for demonstrations, riots, strikes, communal violence, and anti-government violence, using country-year counts of conflict events based on their new database, Social Conflict in Africa (SCAD)[2] (Salehyan et al. 2012). They find that "[e]xtreme deviations in rainfall [...] are associated positively with all types of political conflict" (Hendrix and Salehyan 2012, p.35). However, only violent events and

2. Now named Social Conflict Analysis Database, as the scope has expanded to other areas.

government-targeted events in the SCAD are statistically significant when adding fixed effects.

O'Loughlin, Linke, and Witmer (2014) observed ACLED event counts in 1x1° grid cells in sub-Saharan Africa from 1980–2012. They used 6-month interval standardized precipitation and temperature indices as their causal variables of interest. Additionally, they used coarsened exact matching on key socio-political indicators to drop outlying observations and ensure a fairer comparison between units. They find support for the link between temperature extremes and particular types of conflict but not for precipitation. They also observe that the in-sample predictive contribution of the climate variables (in an alternative random effects model specification) is very low compared to other variables, such as distance to border, infant mortality rates, and previous conflict experiences.

Maystadt and Ecker (2014) made a focused study of counts of ACLED events in administrative areas of Somalia, testing whether livestock prices, measured at monthly intervals and instrumented through temperature anomalies and drought length can explain the prevalence of conflict. Rather than interpreting the second stage directly, they calculated the mediation effect of drought through cattle prices. They report that the combined effect of a 1°C increase in temperature anomaly and 1-month increase in drought length, associated with a cattle price reduction of 6%, increases the number of conflict events by 71.6% (p.1168).[3]

Several meta-studies and commentaries of this field have been published, and the conclusions vary considerably. Gleditsch (2012, p.7) concludes, for instance, that "[o]n the whole, however, it seems fair to say that so far there is not yet much evidence for climate change as an important driver of conflict". Hsiang, Burke, and Miguel (2013, p.1235367-1), on the other hand, conclude that there is a "strong causal evidence linking climatic events to human conflict across a range of spatial and temporal scales and across all major regions of the world".

Salehyan and Hendrix (2014) argue that in order to make sense of the findings in the field, we need to take into account the geographic, temporal and social scale of the phenomena being studied. Analyses are done on different geographical scales — from whole countries to small villages —

3. That is quite a large effect. The use of 2SLS when the outcome variable is a very skewed distribution, with a mean of 1.38, minimum of 0 and maximum of 79, can be questioned (Maystadt and Ecker 2014, p.1168).

and on different temporal scales — from monthly shocks to century long changes in climate — and at different social scales — from armed conflicts requiring the mobilization of a larger population against a professional state military to communal conflicts, such as a raid on the cattle of herders in Eastern Kenya or a demonstration in Johannesburg. In Buhaug et al. (2014), my co-authors and I argue that one problem with the meta-analysis in Hsiang, Burke, and Miguel (2013) is that it is based on a comparison of measurements using very different geographical, temporal, and social scales that use a method reserved for studies of the same underlying phenomena (inverted-precision weighting) (see also Buhaug and Nordkvelle 2014; Hsiang and Burke 2014; Hsiang, Burke, and Miguel 2014). A similar critique can be mounted against the conclusion in Gleditsch (2012), although his paper does argue for disentangling causal chains and thinking about the type of violence that is studied.

Reflecting on the way forward, Buhaug (2015) argues that studies should be more connected to theoretical arguments, which specify and justify relevant climatic conditions, causal mechanisms, actors, context, social outcome and spatio-temporal domain.

1.5 Unsolved issues and the contributions of this dissertation

1.5.1 Taking into account future changes

The future is likely to differ from the present. Knowledge about the past, however, is still useful to generating better expectations about the future. Conflict researchers have built forecasts of armed conflicts (see e.g. Hegre et al. 2013), and the climate impact community has created scenario-based forecasts of socio-economic development (O'Neill et al. 2014). But previous conflict forecasts failed to take into account uncertainties in the (interdependent) inputs to the conflict models, and the socio-economic forecasts failed to take into account the development of political issues, such as regime development and the likelihood of armed conflicts. This dissertation aims to connect these two strands of research to gain better expectations about the future political and socio-economic context within which climate change is likely to happen.

The population of the world has experienced vast improvements to living-standards and educational attainment, and people are living longer than ever before (Barro and Lee 2013; Lutz, Butz, and KC 2014; Lee and Lee 2016; Alkema et al. 2016; UNICEF 2017; Diao, Harttgen, and McMillan 2017). As already argued, these changes appear to have reduced the connection between climate variations, livelihood and social disorder. Modern humans are less *vulnerable* to climate variations. However, modernity has resulted in various kinds of environmental stresses and damages, among them fossil fuel emissions, which cause climate change. And just as the benefits of modern life have not been spread equally across the world population, neither have the environmental vulnerabilities.

Adger (2006, p.268) defines environmental vulnerability as "the state of susceptibility to harm from exposure to stresses associated with environmental and social change and from the absence of capacity to adapt." Violent conflict stands in an interesting relationship to environmental vulnerability, because many of the same structural factors which reduce environmental vulnerability also reduce the likelihood of conflict, and because violent conflict is an important cause of environmental vulnerability (p.274). To properly model the future environmental vulnerability of human populations and the expected future relationship between climate and violent conflict, we therefore need to know how important drivers of environmental vulnerability, including patterns of violent conflict, are likely to develop into the future.

A "hands-off" approach to this problem is to simply say that current causal estimates will need to be adjusted with some unknown weight ω in any given future setting (Burke 2013, Point 1). Although this is trivially true, it does not help much in providing us with an understanding of the future effects of climate change on violent conflict. The goal should be to develop better expectations of what these weights are likely to be.

A cross-sectional approach would be to map climate vulnerability today, to at least get an impression of the cross-sectional variation in vulnerability. This can give us a sense of which populations, where in the world, are severely exposed to the vagaries of weather. Busby et al. (2013) and Ide et al. (2014) offer examples of this approach.

The cross-sectional approach does not take into account the future, however. How should long-term forecasting be approached? An important insight is that the future is uncertain. Accounting for uncertainties and scenarios that are possible are just as important as providing the most likely trajectory, particularly for policy development and planning. Although baseline models of armed conflict have been published already (Hegre et al. 2013), much work remains to be done to account for varying types of uncertainties.

A hard-won insight that the IPCC community came to was that it quickly becomes impractical to attempt to simulate everything simultaneously. Modeling experts are usually field-specific experts, and development has to be done in serial, rather than in parallel (Moss et al. 2010). Dividing future scenarios (by fiat) into independent Representative Concentration Pathways (RCPs) and Shared Socio-Economic Pathways (SSPs) was therefore suggested for the fifth assessment report (Moss et al. 2010; O'Neill et al. 2014, 2017).

The SSPs are quantified narratives of the future up to 2100, designed to capture challenges to climate adaptation and mitigation. To model challenges to climate adaptation is precisely to attempt to model environmental vulnerabilities, whereas modeling challenges to climate mitigation is to model drivers of greenhouse gas emissions (which are not independent of socio-economic development).

When the first article in this dissertation was written, forecasts for economic growth, population, and education levels were available (Dellink et al. 2017; KC and Lutz 2017). Three years later, two alternative economic growth projections and forecasts of urbanization have been added (van Vuuren et al. 2017; Cuaresma 2017; Leimbach et al. 2017; Jiang and O'Neill 2017). All articles about the data were published in a special issue in *Global Environmental Change* called "Quantification of the Shared Socioeconomic Pathways" in 2017.[4]

Although conflict issues were clearly written into the SSP narratives, projections of armed conflicts (or other types of violence) were not included in the quantitative projections. Since violent conflicts affect both socioeconomic growth (Collier et al. 2003; Gates et al. 2012; Costalli, Moretti, and Pischedda 2017) and environmental vulnerability (Adger 2006; Barnett 2006), formal models of violent conflicts should have a natural place in the SSP framework.

The first article in this dissertation contributes by accounting for armed conflicts in the SSPs. In this article, my co-authors and I make baseline

4. The data can be downloaded from `https://tntcat.iiasa.ac.at/SspDb/`.

estimates of armed conflicts up to 2100, based on the quantified pathways in the SSPs. As I elaborate on in Section 1.8.1, the five pathways in the SSPs made it possible to explore forecasted conflict probabilities across a range of possible future scenarios, a source of uncertainty not accounted for in already published armed conflict forecasts (Hegre et al. 2013). Forecasting conflicts also revealed problems with in the SSP models, particularly with respect to the political economy. The particular challenges developing countries are facing have not been taken properly into account in the quantified models. Since the populations in poor and conflict-ridden countries generally are more vulnerable to the environment, the failure of the SSPs to model such countries properly is a severe flaw.

In the other three articles in the dissertation, I provide causal estimates of climate variability on (1) armed conflict onsets involving politically excluded and regionally based ethnic groups, (2) on the probability of experiencing a communal conflict in six hotspots around the world, and (3) whether particular individuals who experience worsening of living conditions — instrumented through climate variability — are more likely to report having participated in political violence. The causal estimates form the base from which we use quantified forecasts to extrapolate effects into the future. I do not find an effect of climate variability on armed conflicts in the subset I study, and such effects are not found at the country-level either. It is therefore likely that climate variability does not affect the baseline estimates of armed conflict. We may then assume that environmental vulnerability and the likelihood of communal conflicts will be affected by changes in socio-economic development or political outcomes, such as the onset of armed conflicts. The forecasts (assuming constant climate exposure) reveal that socio-economic development is more likely to improve and armed-conflict risk is more likely to go down in the countries that have experienced many communal conflicts (e.g., Nigeria, Sudan, Kenya and India). Although this effect might be counteracted by climate change, this cautiously optimistic point-of-view needs to be taken into account when assessing the future social impact of climate change.

1.5.2 Causal inference on disaggregated data

Studying climate variability and violent conflict on disaggregated data requires dealing with new methodological challenges. Although the field has seen significant innovation, problems remain and these are reflected in several published articles. Since we should have confidence in the causal estimates before we extrapolate these effects into the future (and possibly weigh them for expected changes in vulnerability), this dissertation made identifying and solving methodological issues in published studies a priority. Five issues in particular came up (although not always all at the same time):

Ensuring a random treatment

First, not all studies are able to argue with confidence that they have a design that allows for a fair comparison of differently treated units. A source of bias is that the propensity for a given climate treatment, in a given area, change over time; or, that the treatment propensity differ from place to place (Angrist and Pischke 2008, p.81). These phenomena are well known. For example, it rains more in London than in Timbuktu; India has a monsoon season; and temperatures are increasing gradually over time all over the world. Failing to account for these differences in treatment propensity when estimating causal parameters can induce bias. There are many other trends which increase gradually over time and correlate with global warming.

One suggested solution to this problem is to add unit fixed-effects, temporal fixed-effects and area specific fixed trend effects (Dell, Jones, and Olken 2014). Some of the studies at least include unit-fixed effects, such as Hidalgo et al. (2010), Maystadt and Ecker (2014) and Fjelde and von Uexkull (2012), but there are still studies which rely on pooling different areas and using observed control variables, such as Theisen, Holtermann, and Buhaug (2011) and Raleigh and Kniveton (2012).

Another approach to this problem is to operationalize the climate treatment variable in smarter ways. Using standardization, calculating growth, or de-meaning can ensure a stable treatment propensity over time or across units. Changing operationalization can possibly solve issues which the fixed-effect approach cannot solve, or solve them more effectively than the fixed-effect approach. The observed variance in both operationalizations of the climate variable and uses of panel controls, and the disconnectedness between these choices in the literature (as can be seen in Table 1 in the second article), show that we need better concepts and procedures to help researchers make better choices.

Treatment interference

Second, few studies make social dynamics and treatment interferences a central challenge, even though such challenges should be expected increasingly to become an issue when increasing the resolution of the study. The most in-depth discussion is found in Harari and La Ferrara (2014). They test several different spatial lag models, such as the spatial Durbin model, and a dynamic, spatially autoregressive Durbin model, which lets "conflict in one cell depend on lagged conflict in the cell itself, on contemporaneous conflict in the neighboring cells, on covariates in the cell itself and on covariates in the neighboring cells" (p.12). They conclude that "[c]onflict spillovers, both in time and space, appear to be very significant" (p.26).

Accounting for system dynamics such as treatment interference and conflict contagion is difficult, and in some instances, articles make erroneous claims. For instance, Raleigh and Kniveton (2012) write that since many of their observations have the same treatment, "[a]reas with violence proximate to each other are likely to be contained in the same rainfall unit, so controlling for neighboring lags is unnecessary" (p.57). At best, this only makes their estimated standard error overconfident, since the model is assuming more independent observations than are actually the case (i.e. they should at least have corrected for spatial error). However, treatment interference should not be expected to go away just because, for example, your neighbor receives the same treatment as you. This could affect outcomes, and lead to contagion. A similar error is made by Theisen, Holtermann, and Buhaug (2011, p.96), who argue that "random sampling of nonconflict observations removes spatial correlation among the independent variables". However, random sampling only has the potential to reduce spatial correlation if the treatment has only been applied to that random sample. As it is, weather affects the whole world. Random sampling of already treated units cannot remove treatment interference.

Rather than choosing the design that is least likely to be affected by treatment interference and conflict contagion, most studies add control variables, such as spatial lags. There are many ways to model dynamics incorrectly, and in most cases, no one way that is the right (Plümper and Neumayer 2010). Attempts to model spatial dynamics in studies where causal inference is the goal is therefore problematic, particularly if there are alternative ways to answer the research questions posed.

Attrition

Third, disaggregated studies of climate variability and violent conflict need to think about whether they are measuring the correct outcome for the correct treatment. Since humans can move around, it may not always be the case that a spatial matching of climate variability and conflict outcomes is correct. If a unit of analysis was treated somewhere else, and decided to take actions which was then recorded as an outcome, the result would be *attrition* (missing/wrong outcome data for treated units). Attrition can severely affect estimates of causal effects (Gerber and Green 2012, p.212).

Raleigh and Kniveton (2012, p.55) write that "it is exceedingly difficult to argue that a location, in and of itself, is an appropriate unit given the networks and movements of conflict over time". When interpreting their empirical results, they adapt to this notion by arguing that the estimated effect from "rebel conflicts" is due to changes in tactical considerations, "as the locations in which rebel violence occurs are far more widespread with fewer repeated events and no correlation to poverty, attacks on primarily urban centers or population places" (p.58). They show that matching climate variability and violent conflict - particularly state-based/"rebel conflicts" - can have severe implications for theoretical interpretations. Few other studies adapt their interpretations to this problem, and none has actively attempted to solve the issue methodologically (von Uexkull et al. (2016) addressed the issue in later years). The issue is particularly pronounced in studies using ACLED data, as most of these studies do not differentiate among different conflict types.

The Modifiable Areal Unit Problem

Fourth, all studies based on aggregated data need to address the *Modifiable Areal Unit Problem* (Dark and Bram 2007). When using aggregated data, it is not possible to avoid this problem entirely, but studies should make an active effort. Studies that use climate exposure as a treatment aggregate both across space and over time. The three-dimensional flexibility of this aggregation means that there is a degree of arbitrariness when defining treatments.

Fjelde and von Uexkull (2012, p.447) is one of the few studies that implicitly discusses this problem by arguing that first-order administrative units are more suitable than other arbitrarily defined spatial units, because “they tend to play a significant role in the political process; they are often a site of electoral contest; local government authority structures are often constructed based on these divisions; and they often see administrative bodies with important roles in the collection of tax-revenue and provision of public goods”.

It is uncommon to test the sensitivity of results to MAUP in the research field, even though procedures are available to do so in a principled manner (Schutte and Donnay 2014). A particularly neglected consideration for which it would be both relatively easy to test sensitivities and possible to develop theoretical expectations depending on aggregation level, is the time-frame for which a given climate treatment should be calculated. Should it be one week, one month, six months, or one year? Different aggregations tend to measure different exposures. For example, unusually wet/dry 1-year periods are generally tied to stream flows and reservoir levels, whereas shorter periods of unusual dryness or wetness can better measure immediate soil moisture conditions (National Drought Mitigation Center 2014).

Using instruments

Fifth, an issue which is becoming increasingly important for research on *intermediate* variables such as food prices (Buhaug 2015), is the proper use of instrumental variables and 2SLS. None of the studies I have read in this field make instrumental variables (IV) assumptions central in their research, and few test these assumptions or interpret their results correctly (Sovey and Green 2011). This seriously compromises the confidence we can have in reported estimates.

The assumption that the instrument is independent of potential outcomes (i.e., random) is not directly discussed in any of the articles (Angrist and Pischke 2008, p.117), although Maystadt and Ecker (2014, p.1174) make the assumption that it holds. The exclusion restriction is discussed by Bohlken and Sergenti (2010, Supplementary Materials, p.2) and Maystadt and Ecker (2014, p.1174). Non-interference/Stable Unit Treatment Value Assumption (SUTVA) is partially discussed in Maystadt and Ecker (2014, p.1173), but not in any of the other articles, and monotonicity is never discussed.

Maystadt and Ecker (2014, p.1169) and Caruso, Petrarca, and Ricciuti (2016, p.75) test for weak instruments. Bohlken and Sergenti (2010, Supplementary Materials, p.1) and Buhaug et al. (2015, Supplementary Materials, p.19f) also test for weak instruments, although the reported numbers do not seem to be high enough to pass such a test.

Last, only Bohlken and Sergenti (2010, p.599) mention that the second stage effect of their variable of interest needs to be interpreted as a local average treatment effect/complier average causal effect (LATE/CACE) (Sovey and Green 2011, p.191). Although it is not a problem in itself that these results are LATE/CACE (when testing for theoretical causal mechanisms, this may be exactly what we want), the fact is omitted from the interpretations in these articles. This omission becomes especially problematic when the articles are discussing the severity and the societal-wide implications of their findings, because the complier share of the population is unknown. The effect could be large but only affect a few. Even Bohlken and Sergenti (2010), who mention LATE, fail to apply that important caveat to their abstract and conclusions.

Raleigh, Choi, and Kniveton (2015) argue that the devil is in the details when studying the effect of food prices and conflict outcomes. Rather than relying on country aggregates, they use mean market prices in first level administrative units where the availability of commodity prices is good. They argue that there is a reciprocal relationship between food price and conflict, and that rainfall affects conflict both through food price and other pathways (p.190). If this is true, it essentially means that IV models of food price and conflict using rainfall as instrument do not meet the exclusion restriction, and that identification of causal effects in this system is very difficult, if not impossible. Unfortunately, they do not discuss the implications of this in their study.

In summary, disaggregated studies of climate and conflict need to think more and better about methodological problems of causal inference. Whereas variations in estimated outcomes may be due to actual variations in effects on the ground (Hsiang, Burke, and Miguel 2013; Buhaug et al. 2014), it is just as likely that they are due to the unsolved methodological problems of causal inference, especially in disaggregated studies.

The last three articles in this dissertation address these issues, and are more closely elaborated on in Sections 1.8.2, 1.8.3, and 1.8.4 of this introduction. A more detailed discussion of the methodological aspects is also found in the appendix to each article. The starting point in these articles is that both the choice of conflict outcome and the theoretical hypothesis have methodological consequences. In addition to identifying methodological issues and incorrect theoretical conclusions arising from the methods used, I offer solutions to the problems. The essence of the solutions I propose is to simplify issues unrelated to the concrete theoretical question at hand by making conscious choices about the unit of analysis and the operationalization of the climate treatment. The problem that many studies fall into is to make a detailed model and then having to remove unwanted variance. Using the solutions I suggest, the statistical models and the assumptions made for causal inference to be true should be easier to follow, and the models themselves will be more closely connected to the relevant theoretical questions posed in each article.

1.6 A theoretical basis

> The central puzzle about war, and also the main reason we study it, is that wars are costly but nonetheless wars recur.
>
> (Fearon (1995))

Fearon's puzzle has many possible explanations, and in the cited article, he investigates rationalist explanations that assume unitary state actors. But it is the puzzle itself that I want to address here. A war is a very costly way of settling a dispute or changing policy (Ghobarah, Huth, and Russett 2003; Gates et al. 2012). The alternatives to violent conflict, such as nonviolent resistance, are often more effective in providing the participants with positive social change (Chenoweth and Stephan 2011). Yet, 118,000 people were killed in organized violence in 2015 (state-based, non-state, and one-sided violence) (Melander, Pettersson, and Themner 2016).

Even assuming unitary state actors, researchers have found theoretical solutions to the puzzle. Fearon (1995) argues that miscalculation due to lack of information or disagreement about relative power can make the conflicting parties overestimate the gains and underestimate the costs of conflict, even compared to other options.

Did Colonel Riad al-Asaad expect the current outcome in Syria, when he claimed command of the Free Syrian Army, and warned the Syrian government that he would “send my troops to fight against the army if they do not stop the operations in Deir ez-Zor”? (News24 2011) Or did Bashar al-Assad anticipate the results of his crack down on demonstrations the day after, in what has been called ‘the Ramadan Massacre’? It seems doubtful.

The puzzle becomes easier to solve, but more complex, by not assuming unitary state actors and taking into account the dynamics of conflict. Some individuals are actually likely to benefit immensely from war, even though the general result is devastating. The possibilities for settling scores and redistributing wealth and power are huge during conflicts (Kalyvas 2006). And once civil wars have started, peaceful settlements are difficult to reach and implement due to both information and commitment problems (Walter 2009).

The existence of non-unitary actors who expect varying risk and gains from violence, information problems between group actors, and commitment problems are factors that provide the background for explanations of why violent conflicts occur and persist. The reason that economic factors are often pointed to to explain why violent conflicts occur is that they, to a large extent, modulate these factors.

At their outset, climate-conflict studies offered conflict theories in which both structure and actor perspectives are important. Miguel, Satyanath, and Sergenti (2004, p.727f) argue that the grievances and weak repressive capabilities of African states, as well as the large share of the population working in agriculture, form the important background conditions for why climate-induced growth shocks can affect the likelihood of armed conflict. The direct mechanism they propose, however, is individualistic, arguing that “[n]egative growth shocks make it easier for armed militia groups — which are often major combatants in Africa’s civil wars — to recruit fighters from an expanding pool of underemployed youths”.

Whereas this basic model is generally retained in contemporary studies, it has been argued that some types of intergroup violence are more likely to be affected by climate variations than others (Fjelde and von Uexkull 2012). Fjelde and von Uexkull argue that non-state conflicts between informally organized groups, particularly in resource scarce areas, should be expected to be more sensitive than armed conflicts to climate variations.

To understand better why this should be the expectation, it is useful to consider conflict theories from both actor and structure perspectives. How climate variability and environmental vulnerability relate to different types of intergroup conflict also requires discussion. The broad expectations I derive are that some groups are more likely to engage in violent conflict against the state than are others due to climate variability, and that climate variability is less likely to affect state-based conflicts than non-state conflicts.

1.6.1 An actor perspective

Many theories of violent conflict start with the assumption that violent actions are intentional actions, meaning that the actor must have some kind of motivation for the action, as well as an opportunity to follow the motivation with desired action (Collier, Hoeffler, and Rohner 2009). Most such theories focus on the different types of things that can modulate motivations and opportunities.

Opportunities and motivations are seldom clearly distinguished, however, and a level of confusion exists in the conflict literature about what constitutes opportunities and what constitutes motivations. The article by Collier and Hoeffler (2004) is a particular source of the confusion, which the authors themselves acknowledge in a subsequent publication (Collier, Hoeffler, and Rohner 2009, p.2). In their 2009 paper, they attempt to reduce the confusion by introducing the idea of "the sheer financial and military feasibility of rebellion", which they distinguish from the motivation for rebellion (p.2). The distinction may have solved some issues, but it still fails to distinguish between motivation and opportunity. The confusion is particularly created by the authors' failure to specify the level of analysis. Increased individual level motivations for participation can, for instance, be a group level opportunity if the group wants to recruit more members.

A clearer intentional action theory with respect to violent action should therefore start by recognizing that there are two types of actors: individuals and organizations/groups. These actors each have motivations which encourage some actions and discourage other, and the range of possible actions (i.e., opportunities) is constrained by the capacities of the actors and the environment (e.g., other competing actors).

A central theoretical issue limiting the types of things that can modulate individual motivations and opportunities is the collective action problem. Olson (1971) argues that, if the added chance of winning and thus, resolving grievances (e.g., though the shift of government), is negligible, regardless of whether any individual joins, and if the risk of dying or getting hurt is high for those who participate, then a rational, self-interested individual would not choose to participate unless the gain of winning is very large, or there is "coercion or some other special device to make individuals act in their common interest" (p.2).

Olson argues that organizations are "special devices" that we create to overcome the collective action problem. Organizations can provide *selective incentives* to members - such as the payment of wages, conditional on participation. Following the work by Olson, researchers commonly emphasize selective incentives when explaining individual motivations. In contrast, grievances and other overarching motivations that individuals may have for their actions, but which may be constrained by the collective action problem, are analyzed at the group/organizational level.

Wages and other pecuniary rewards are commonly used selective incentives. When it comes to the pecuniary rewards that violent groups can provide, however, it is important to consider the alternative costs of participation for the individuals considering joining (Grossman 1991; Collier and Hoeffler 2004). Individuals have a finite number of hours they can work each day, and if the benefits from engaging in non-violent labor are greater than from working for violent groups, it would make little sense for them to join the violent group for the money.

It is not only the legal wage and the illicit wage which must be considered when calculating alternative costs, however.

By joining violent groups, an individual may become tied to illicit activities, be monitored by the police and counter-insurgency organizations, face incarceration, have difficulties finding future work, among other things. Indeed, many violent organizations require new members to take part in clandestine activities in order to test trustworthiness and burn bridges to other alternatives (Hegghammer 2013, p.12). Participants in violence should, therefore, to varying degrees, expect to risk punishment for participating in illicit activities (Blattman and Annan 2016).

Another consideration is that some types of violent activities are engaged in as part of ordinary economic activities, or to protect ordinary economic activities. Farmers who attack pastoralists who let livestock pasture on their fields, herders who steal other herders' cattle, or drug dealers who defend their area-monopoly are all examples of individuals who face more complex pecuniary cost-benefit analyses than simply calculating the difference between an expected legal and an illicit wage.

Whereas pecuniary rewards and punishments are common selective incentives in violent conflicts, selective incentives can also be non-pecuniary (Gates 2002; Petersen 2002; Wood 2003a; Kalyvas 2006). Wood (2003a) argues that non-participation can involve moral costs which she called *defiance*. This means that even though there are no positive benefits of participation, participants may become incentivized anyway, because non-participation is preferred less. Wood also argues that individuals who have been oppressed can experience a *pleasure of agency* by acting autonomously. As a side note, it is worth bearing in mind that both rational faculties and emotions combine to produce perceived benefits that are both pecuniary and non-pecuniary (Elster 1998).

Another cost of non-participation is the threat of being killed. Many participants in violent movements are either abducted, volunteer at gunpoint, or believe they are safer inside, rather than outside a violent group (Humphreys and Weinstein 2008; Andvig and Gates 2010; Eck 2014).

Actors are all able to plan, which means that part of their motivation is to assess how likely it is that a given action will be successful, and how risky it is to engage in a specific course of action. A variant of this argument is found in the literature on *relative deprivations*. Gurr (1970, p.24) defined RD as discrepancy between what people believe they are entitled to (*value expectations*), and what they believe they are capable of getting (*value capabilities*).

Gurr used the idea of relative deprivation to argue that we should not necessarily expect the poorest individuals (those "absolutely deprived") to be most motivated to fight, since value expectations and value capabilities may be lower for poor individuals. Value expectations and value capabilities are beliefs, and they react dynamically to each other, as well as to material endowments and social interactions. Other researchers have argued that personal material endowments are not the only factors modulating capabilities. Social networks and endowments do not necessary correlate with material endowments, but they can be important when mobilizing support for organizations capable of feats individuals cannot achieve alone (Tilly 1978; Staniland 2014).

1.6.2 A structural perspective

A common way to look at a political system is through the polity model. Political groups within a country seek representation of their interests in the state apparatus (Tilly 1978, p.52). These groups represent, or claim to represent, a broader group in the population. Within a country (i.e. the territory to which a state lays claim), there can therefore at any time be political groups with representation in the state apparatus, political groups without power in the state, political groups with only regional power, political groups actively discriminated against and excluded from participation in the state, etc. (Cederman, Gleditsch, and Buhaug 2013; Vogt et al. 2015).

A state is a set of institutions which sets out to create and enforce laws within a territory, to provide welfare and economic opportunities for groups in society, and to protect itself from outside threats. The state seeks order (North, Wallis, and Weingast 2009, p.13ff) and security from outside threats (Waltz 1959, p.159ff).[5] In this dissertation, I equate states with modern states, defined as above but with the addition that they have a clearly defined border, or at least a clearly defined border dispute with adjacent states, and are recognized by other states as a state.

According to Huntington (2006, p.1), "[t]he most important political distinction among countries concerns not their form of government but their degree of government". Quantitative studies of armed conflict at the state level have embraced this idea. Inconsistent regimes with low state capacity and reach are much more likely to experience conflict than are consistent autocracies or democracies with competent state institutions that can elicit "quasi-voluntary" cooperation from society, and credibly can secure third-party arbitration (Fearon and Laitin 2003; Hegre 2001; Fjelde and De Soysa 2009; Hendrix 2010; Buhaug 2010a; Buhaug, Gates, and Lujala 2009).

5. Since this dissertation is mainly interested in intra-state conflict, I will not discuss international political theory here. That is not to say that international relations and IP theory are not relevant to the topic, as in, for instance, the matter of third-party military interventions (Walter 2009, p.255f).

The state polity is privileged in a country. It has military and policing capacities, institutions for collecting taxes, and a judicial system, among other things. Violence against the state, with the aim of taking over government or separating a territory from the state, is therefore regarded as a special type of conflict. It is reasonable to assume that only reasonably (socially and/or economically) well endowed social groups can create a violent organization able to threaten the state.

Conflict researchers categorize conflicts into different types depending on which groups fight each other, and the organizational level of those who fight. Violent conflicts between states, or a state and groups that can oppose states, require better organized violent groups than do organized cattle thievery or a drug gang shootout. State-based conflicts and communal conflicts are also different in the sense that the goal of the violence is different. Whereas the overarching goal in state-based conflicts is control over government or secession of territory, communal conflicts can have a wide range of goals, many of which are tied to immediate pecuniary benefits such as gaining cattle or access to water. Such immediate benefits also can be pursued *during* armed conflicts (Kalyvas 2006), but the onset of armed conflicts cannot be explained by a large set of diverging, local pecuniary goals in themselves.

State capacity and reach can explain not only the absence of state-based conflicts (Fearon and Laitin 2003), but also non-state conflicts (Rudolfsen 2017). State intervention in non-state conflicts "is explained by a combination of strategic interests and state capacity" (Elfversson 2015, p.791). In some cases, non-state conflicts are state conflicts by proxy. In other cases, states are unable to stop the conflicts, or unwilling to provide the resources necessary to do so. The political relations between the state and non-state groups are likely to affect the strategic choices of states involved in non-state conflicts.

States can exacerbate non-state conflicts through poor policy-making, for instance, by providing property rights in a biased manner to different groups (Butler and Gates 2012). In any case, interpreting causes of non-state conflicts as being completely independent of state capacity, state policies, and group allegiances within a state framework, is very likely to produce poor inferences (Benjaminsen and Ba 2009; Brosché and Elfversson 2012).

It is not a given that opinions, ideas, and grievances within the population are represented by political groups. The population must mobilize (or become mobilized) around these ideas or complaints (Benford and Snow 2000), and other groups or the state can actively support or undermine such mobilization attempts (Schutte 2016; Lyall and Wilson 2009). Groups mobilize along ethnic identity, class, religion, and urban-rural divides, etc. Of course, an individual may have multiple identities that cross or align with more than one mobilized group (Lipset and Rokkan 1967).

An important strand of research in the recent years has asked whether initial endowments and social networks of the rebel organization affect the type of members the organization gets, and consequently, the motivations and actions of the rebel organization. Weinstein (2006, p.7) argues that when rebel leaders lack social endowments ("shared beliefs, expectations, and norms that may exist in [or be mobilized from within] certain ethnic, religious, cultural, or ideological groups"), they need to draw more heavily on economic endowments, with the result that the group recruits more people who are motivated to fight because of the promisted money than because of some overarching cause. Whereas Weinstein starts with the idea that rebel leadership needs to recruit members, Staniland (2014) starts with the observation that most insurgent groups grow out of existing organizations or social networks. He argues that "[t]he prewar networks in which insurgent leaders are embedded determine the nature of the organizations they can build when a war begins" (p.1f).

These structural perspectives all make it clear that a narrow focus on individual opportunities and motivations will necessarily fail to explain why violence occur. While the main direct link between climate change and conflict is suggested to be through changes to individual opportunity costs, we should not lose sight of the contextual/structural conditions necessary for collective action.

1.6.3 How is conflict affected by climate variability?

The most common argument found in climate-conflict studies is that climate variability can affect the agricultural production or income of environmentally vulnerable populations. Thus, if individuals in such populations also are considering recruitment to violent activity, their opportunity cost goes down, increasing participation in potentially violent groups and the likelihood of violence (Miguel, Satyanath, and Sergenti 2004). However, losing agricultural yields to nature could also induce relative deprivation and grievances. This could be particularly true if it is possible to point to some societal actor who made one's environmental vulnerability larger than it might have been, or who failed to deliver on promises to help after the fact. In studies of climate variability and conflict, it is therefore very difficult to separate pecuniary and non-pecuniary motivations. Most studies that do look for motivations find them to be complex (Humphreys and Weinstein 2008; Oyefusi 2008; Schilling, Opiyo, and Scheffran 2012; Blattman and Annan 2016).

Although these mechanisms are the main ways in which researchers believe that climate variability will affect violent conflict, there are reasons to believe that the mechanisms are important, to varying degrees, in different types of conflict and for different groups.

Groups and individuals are not equally vulnerable to variations in the local environment. Agricultural laborers are directly dependent on favorable climates for their production output. Agricultural wage laborers risk losing their jobs if the harvest is poor (Hidalgo et al. 2010). Wage laborers in other sectors will mainly experience losses in income due to changes in food prices (Smith 2014). However, where international markets exist, substitutes for local products may alleviate the cost of a decrease in local supplies.

The agricultural sector in developing countries is one of the least productive sectors in terms of per worker productivity (McMillan, Rodrik, and Verduzco-Gallo 2014; Restuccia, Yang, and Zhu 2008). One reason for this low productivity is underemployment (McCullough 2017). Low per-worker productivity does mean that there is less income to invest in reducing environmental vulnerability, and, therefore, the capacity to handle environmental damages is lower. Countries with a large share of agricultural workers and agricultural groups within countries should be expected to be more vulnerable to variations in the local environment (Miguel, Satyanath, and Sergenti 2004).

Fjelde and von Uexkull (2012) argue that politically excluded groups have fewer coping strategies available to them when exposed to resource scarcities. Political groups that are included in the state can expect, to a larger degree that can politically excluded groups, to get help before disasters happen and relief if they do.

Although there can be a direct link between climate variability and conflict through rebel recruitment and the opportunity cost mechanism, contemporary studies emphasize the interaction between individually experienced losses and group level dynamics. Raleigh (2010) and Fjelde and von Uexkull (2012) emphasize the interaction between political marginalization, environmentally induced hardship, and violent conflict. Politically excluded groups are likely to perceive their environmental vulnerability to be a function of their poor relationship with the state to a larger extent than are included groups.

Just as institutions and organization can mobilize for violent action, they can also mobilize for peaceful action. When a society is experiencing hardship, the optimal thing for the society to do may be to collaborate. But collaboration can fail due to the collective action problem. Linke et al. (2015) show that local communities can create norms and institutions aimed at ensuring collaboration and peace during periods of environmental distress.

Arguably, the same organizational capacities necessary to wage an armed conflict against the state may also reduce environmental vulnerabilities. For instance, national defense forces are employed to prevent damages during floods. This means that there are likely groups other than those engaging in armed conflicts that are most vulnerable to the environment. Fjelde and von Uexkull (2012) argue that we should see a stronger relationship between climate variability and communal conflicts (non-state conflicts between informally organized groups), than for armed conflicts, for that reason.

Potentially violent groups are differently situated to create and mobilize around effective violent organizations. In the second article of this dissertation, I argue that politically excluded ethnic groups are not only more likely to be environmentally vulnerable than other parts of the population, but they are also more likely to be able to mobilize such organizations (Eck 2009; Cederman, Wimmer, and Min 2010). Such groups are therefore a "most-likely" case for seeing an effect of climate variability on the onset of armed conflict. The differences in estimated effects for different conflict types also suggest that those groups that are most environmentally vulnerable are largely unable to mount large scale rebellions against modern states. Rather, we observe effects of climate variability for communal conflicts in resource-scarce areas where the state is unable or unwilling to intervene.

1.7 The role of prediction and causal inference in explanation

When answering the question *Will climate change increase the prevalence of violent conflict?*, we need a *convincing explanation* of why that is (or is not) the case.

Explanations are statements which provide information about *why* things happen (to be). As such, they can be separated from purely descriptive statements. I can say "that male bluebird has blue feathers", and it would be true, but it is not an explanation. On the other hand, saying "that bluebird has blue feathers because it is a male, and male birds of that species have blue feathers" is both true and an explanation. Explanations are useful because they can provide us with better expectations about the future. Humans evolved not only to grasp the moment, but also to expect the future and take advantage of it. In conflict research, it is important to understand why groups and individuals resort to violence in order to better anticipate it, prepare for it, and preferably find ways to avoid it.

To be considered convincing, an explanation must generally meet three basic requirements:

- It provides accurate predictions with new data that are better than previous explanations.
- It fits with other relevant theories of how the world works, that are themselves convincing explanations — particularly those that have proven especially resilient — better than those who are at odds with these theories ([partial] confirmation holism).
- It provides predictions that are just as good and fit just as well with other relevant theories, but offers a simpler explanation ([strict] Occam's Razor).

The second and third requirements can help up choose among explanations that are underdetermined, and Occam's Razor is a safeguard against unnecessarily bold theories. However, the first condition is without doubt the most important. It can be shown that "any hypothesis that is seriously wrong will almost certainly be "found out" with high probability after a small number of examples [new independent samples], because it will make an incorrect prediction. Thus, any hypothesis that is consistent with a sufficiently large set of training examples is unlikely to be seriously wrong: that is, it must be **probably approximately correct**" (Russell and Norvig 2009, p.714, bold in original).

The Probably Approximately Correct (PAC) learning model was first formulated by Valiant (1984), and follows logically from the assumption that future examples are drawn from the same fixed distribution as past examples, and that each new example is independent of other examples, or examples that follow Markov dependence (Aldous and Vazirani 1990). Thus, as long as we assume that the true explanation does not change over time (which it may, of course), explanations that only meet the first condition can, in principle, be proven to be *probably approximately correct*. This computational learning theorem is the main basis for using a prediction-based research method.

A problem that can occur when generating the expectation and testing it on the same data is what we call "over-fitting" (Hastie, Tibshirani, and Friedman 2009, p.37f). Over-fitting happens when the model is better at predicting the observations in-sample (i.e., the sample used to generate the expectation) than the new observations. Assuming that the true explanation has not changed between the generation of the expectation and the testing of it (an assumption that I always make in this dissertation, unless explicitly indicated otherwise), then the true explanation should yield predictions that are just as good in-sample as out-of-sample. Over-fitting shows that the model optimization over the data-sample optimized at least partially for the wrong things. Therefore, the over-fitted model cannot be the explanation we where hoping for.

It is only through the use of predictions that we can know whether we become better at anticipating the future. To learn whether we get better, we need to form explicit expectations and test our performance against new data. Out-of-sample prediction performance is the closest thing we get to measuring the most important reason for making explanations. As a measurement of scientific progress, out-of-sample prediction should therefore also be a core activity in all sciences.

Prediction is a necessary activity when doing empirical science, but it is not just only used to measure scientific progress. To make a prediction is to match an expected outcome (usually derived from a statistical model or a textual explanation) to an observed outcome from the world. For example, through prediction, I can put the expectation that all male bluebirds have blue feathers to test. If I then find a non-blue male bluebird, I must refine my explanation. I could do this by saying, "male bluebirds are usually blue due to a particular gene. However, a few male bluebirds carry albino genes which override the genes making male bluebirds blue. These bluebirds are white and black."

Statistical models result in a mathematical explanation (model) of an operationalized and measured outcome that minimizes the difference between the expectation of the model and the observed outcome. As such, it is a formalized way of *generating* expectations from empirical data. However, statistical models are also often used to *test* expectations. We can generate expectations for many different parameters in a model, and the only way to test these expectations is through out–of–sample prediction. In this dissertation, I will be looking at two sets of parameters when testing model expectations: the out–of–sample performance of the expected predicted probability derived from the models, and the parameters of interest in causal inference.

We use causal inference to test explanations which involve the claim that a specific variable, on average, causes a change in the outcome. The explanation usually also involves an expectation about the direction of change, but seldom (at least in political science) does it involve expectations of particular strengths. Causal inference is a method to rule out the possibility that the average observed differences in outcomes depending on the variable of interest could have been due to any other variable. However, it is important to remember that causal inference is in principle also out-of-sample prediction, as long as the expectation have been derived from different data than the test. I will define and discuss causal inference further in the next section.

Causal effects can change, just as true explanations of a phenomenon can change. This is because the underlying phenomenon we want to explain can change or be different for a different population. Explanations for violent conflicts changes over time, because, among other things, technologies and societies change. Technology has made it possible for strong states to intervene in violent conflicts anywhere in the world, or to threaten to eliminate a large portions of the population of any aggressor through the use of nuclear weapons. Technologies for production, transport, communication and health have given billions of humans safer, longer, and better lives. The effect that nature has on social life, which is the broad theme of this dissertation, should, in particular, be expected to be modulated by technological and societal progress, since much of that progress is about making life in nature easier.

A solution to the problem of change would be to generate more general theories that take change into account (i.e., that make the things that are changing parameters of the model). However, in social science, such theories tend to become too complex to be useful, and often demand data that is no available. In empirical social sciences, there is therefore a clear preference for middle-range theories (Merton 1968). If the phenomenon that the theory describes changes in fundamental ways, the change then happens outside of the scope of the theory, and therefore, cannot be explicitly captured by elements within the theory. Therefore, causal effects in, or out-of-sample predictive performance of, middle-range theories are subject to change.

Nate Silver, in his book *The Signal and the Noise*, makes a distinction between *a prediction* ("a definitive and specific statement about when and where") and *a forecast* ("a probabilistic statement, usually over a longer time scale") (Silver 2012, p.149). Forecasts are particularly important in dynamic systems that rely on a sequence of probabilistic events. In such systems, the same model can end up producing myriad different outcome trajectories. Therefore, the most meaningful description of the future prediction in such models is the probability distribution over the temporal trajectory. It is possible to have a high error-rate for point predictions despite basing these predictions on an unbiased dynamic model.

Forecasts have a wide range of uses. The most obvious is for generating expectations about the future for those who trust the model; that is, forecasts can be used to plan actions into the future. For example, I trust the national weather-forecast in the morning of the day that I am deciding how to dress, but I refrain from building strong expectations based on the long-term weather forecast.

Forecasts are also a simple summary of models. If there are large deviations between what an expert would expect and what the model expect, that is interesting, because it means that the model knows something the expert does not know, or vice versa. Such deviations are therefore a source for either improving the model, or potentially learning something new. Creating forecasts can therefore be a way to test *face validity* (Bryman 2004, p.73). A theory that may sound good on paper can provide obviously flawed results in a statistical model. Simulation is a very useful tool for theory-building, and worthwhile in itself.

Similarly, making forecasts is a way to show how good one's current best model is, and to stick one's neck (or the model's neck) out to make that claim. Particularly, if the current best model is performing worse than the most simple models available (such as persistence, stable growth, or long term averages), then that tells us something about the current state of the model/quantified knowledge.

When answering the question of whether climate change will increase the propensity for violent conflict in the future, it is important aspect to generate reasonable expectations about other relevant aspects of the future. In what kind of societies will climate change occur? Whereas I may have the expectation that climate change will increase the propensity of violent conflict in the future, *all else being equal*, this expectation is also possibly irrelevant if the future is going to be different from today. Alternatively, I may have different expectations of the independent effect of climate change depending on how other relevant aspects of the world will change in the future.

This dissertation will use predictions to test expectations about causality, to test out-of-sample predictive performance of statistical models, and to create structured models of the future producing forecasts of violent conflict outcomes. While the total effort in this dissertation is not enough to provide definite answers to the main question, I have provided additional insights which should build *confidence* in causal estimates and provide a useful point-of-departure for generating more detailed expectations about the relationship between climate change and violent conflict in the future.

1.7.1 Causality

> One difficulty that arises in talking about causation is the variety of questions that are subsumed under the heading. Some authors focus on the ultimate meaningfulness of the notion of causation. Others are concerned with deducing the causes of a given effect. Still others are interested in understanding the details of causal mechanisms. The emphasis here will be on *measuring the effects of causes* because this seems to be a place

> where statistics, which is concerned with measurement, has contributions to make. (Holland (1986, p.945))

I share Holland's main interest in measuring the effects of causes, and this is the main concern in this dissertation with respect to causality. However, before measuring such effects, we need a working definition of them. I base myself on the *potential outcomes* definition of causality (Rubin 1974; Gerber and Green 2012). The potential outcomes definition of a causal effect states that a causal effect is the difference between two potential outcomes (usually where one of these outcomes is the actual outcome). An alternative, and arguably more general, definition of a causal effect is found in Pearl (2000). While Pearl's definition opens up some possibilities for inference that are not available under the potential outcomes definition through the do-calculus, the potential outcomes definition is sufficient for my use, and (in my opinion) simpler to convey than Pearl's definition.

A potential outcome is a realization of the world, given a particular set of events. This set of events is usually called the treatment. I will refer to the movie "Back to the Future" to illustrate the potential outcome framework. In this framework, the causal effect of the character Marty McFly saving his father from being hit by the car driven by Marty's future grandfather was that his mother became infatuated with Marty, her son, rather than his father (who would have otherwise been hit by the car). As a result of this, Marty was in danger of being erased from history. (The effect also involved Marty having to get his mother to fall in love with his father at the school dance, which ended up changing aspects of the future, including, for example, his father's the self–confidence and Biff's occupation.)

In the absence of time machines, we will never be able to observe more than one potential outcome — the actualized one. This is what Holland calls "the fundamental problem of causal inference" (Holland 1986, p.947), which means that it is impossible to estimate individual causal effects. It turns out, however, that we can estimate average causal effects. The estimated average treatment/causal effect (ATE) is the expected difference in mean outcomes between two groups in which the expected outcome for each is identical before application of the treatment (Gerber and Green 2012, p.30ff). The research design process that seeks to achieve this kind of comparison is called causal inference.

One often overlooked difference between a causal effect and the average causal effect is that in the latter, we must be very precise about the kind of outcome being measured. In the McFly case, for instance, the whole future could be treated as the outcome of the treatment. Precision about the outcome of interest is essential in order to make a sensible comparison for average causal effects.

Another difference is that the average causal effect answers a different question than does the causal effect, namely how the outcome *tends* to change when the analyzed units are exposed to a particular treatment. In many cases, we are more interested in the answer to that question, than to the question of what would have happened in a particular case. For instance, when taking paracetamol for a headache, I am more interested to learn that it removes pain for most people, most of the time, rather than that some particular person did not experience any effect. In other cases, the particularities of the situation may actually be more relevant.

One reason to be interested in causal inference for climate variability, is that it is quite natural to think of changes in climate as natural interventions/treatments. Pearl (2000, p.331f) makes a sharp distinction between the statistical means to infer causal effects, and the causal definition. However, the potential outcomes definition is less outspoken about this point. The crucial difference between the strictly statistical definition of a causal effect and a causal effect, is that causal effects are about "the dynamics of events under *changing conditions*... [whereas] there is nothing in a distribution function to tell us how that distribution would differ if external conditions were to change" (p.332). We need to consider what it will mean to make the intervention that we are estimating the treatment effect of.

Climate variability and causal inference

Conflict researchers have been interested in climate measurements for two main reasons. Either they have wanted to know in whether particular climatic exposures affect conflict outcomes, or they expect that climatic exposure can be used as instruments of variables in the social system that is argued to affect conflict outcomes. In some cases, these two aims intersect.

A methodologically important distinction is therefore between climatic exposure that can arguably be used in causal inference and that which cannot. It has been argued that measurements of climatic shocks (i.e. unusual happenstances) have qualities that make them suitable as instruments (Dell, Jones, and Olken 2014). First, climatic *shocks* can be said to be exogenous to social processes, which is necessary for causal identification to work. Second, climatic shocks can be argued to be random (Dell, Jones, and Olken 2014), in the sense that it is possible to make models that control for the propensity of getting a particular treatment (Angrist and Pischke 2008, p.81). In the three articles in which I do causal inference, I discuss how best to use climate measurements to attain the goal of having a random treatment, as well other goals in causal inference, such as absence of treatment interference and excludability.

Other climatic phenomena, such as seasonal variation in temperature, can easily be predicted, and social life is planned around them. These phenomena are neither exogenous to social systems, nor particularly suited for causal inference (Gartzke and Böhmelt 2015). This can be a problem when making inferences from the studies based on causal inference of climate on conflict to the effects of climate change on conflict, since studies based on causal inference are limited to only a few of the exposures we expect from climate change. Alternative validations approaches, such as evaluating out–of–sample predictive performance of models with and without climate variables less suited for causal inference, is a way to learn if these variables are likely to be affecting conflict causally (i.e. a causal hypothesis can be shown to be probably approximately correct). Using unsuitable validation approaches when doing causal inference does not contribute to building more convincing explanations.

1.7.2 Concepts and work–flows in causal inference

This dissertation deviates from many other studies that identify causal effects in the climate-conflict literature in that it uses concepts and a work-flow taken from the field experiment literature (Gerber and Green 2012). The work-flow and concepts are not intrinsically better than other ways to study causal inference. They are mainly *different*, which can be helpful for looking at problems in a new light. That being said, the field experiment work-flow and way of thinking about causal inference are particularly helpful when dealing with arguably random, or near random, treatments, such as climatic shocks.

Field experimenters take a practical approach to causal inference, since they are able to create the conditions suitable for such designs themselves. However, they must deal with limited funds, sub-optimal implementation, and unforeseen events. Field experiments share with observational studies an inability to control all aspects of the design.

Observational data studies and experimental studies have different work-flows. Observational studies tend to start with both a dependent and an independent variable, and *then* seek to account for possible omitted variable bias (King and Verba 1994, p.94ff). Omitted variable bias itself refers to the observational data studies work-flow — finding the variable(s) that can be added to the model in order to estimate the unbiased causal effect of the variable of interest.

Experimental studies start by randomizing the treatment assignment — effectively creating a variable that can yield unbiased causal estimates (Gerber and Green 2012). In such experimental settings, bias can sneak in after or during randomization, for example, through treatment interference (when the potential outcomes in one unit are affected by the particular assignments of treatments in other units); attrition (failing to match treated units with the outcome, e.g., by failing to measure the outcome for some treated units); or through non-exclusive treatments (when changes in potential outcomes are due to being assigned to the treatment group, rather than to the treatment itself).

Similar issues are also discussed in observational studies, but given different names. Non-interference is just another name for the Stable Unit Treatment Value Assumption (SUTVA) (p.253ff). Excludability comes up in observational studies through instrumental variable (IV) designs, where it is called the exclusion restriction (p.39ff). Average treatment effects in IV models are estimated only for the unknown subgroup of the sample that reacted to the instrument — i.e., the Local Average Treatment Effect (LATE). A similar situation is found in experimental studies when faced with non-compliance. In that case, treatment assignment can be an instrument of actual treatment, and the Complier Average Treatment Effect (CACE) is estimated (p.141ff).

In the three articles in which causal identification is pursued, I have attempted to emulate the work–flow of a field experimenter, rather than the common work–flow in observational data studies. The approach makes sense when studying the effect of climate variability on social outcomes, because randomization is arguably feasible for measurements of climate variability. Although it is not possible to assign climate treatments in a random way, it is possible to select an optimal operationalization of the climate treatment, to think about when and where treatment propensities are likely to change, and to avoid comparing observations when treatment propensities are likely to be different. Thinking of climate variability as a (possibly random) *treatment*, rather as an independent variable, may enhance the ability to make sound design decisions in this field.

It could be claimed that it is not possible to assign treatments in observational studies, and that, therefore the experimental concepts of "randomization" and "treatment" cannot be used in this type of research. "Randomization" comes from "to randomize", defined in the Merriam-Webster dictionary as "to select, assign, or arrange in a random way". In the case of observational studies, researchers have not assigned treatment to the subjects at all; they have just observed what has happened in the world.

This view, although understandable, misses the point of *why* we randomize treatment. We randomize treatment in order to learn the probability of being assigned a given treatment. This is the definition of a random treatment, and the one I will be using in this dissertation. At least in principle, someone other than us could be randomizing treatment, and then telling us the probabilities afterwards. Taken a step further, it is possible to imagine that it is nature that is randomizing treatment, and that rather than telling us, it is providing clues about the probabilities. This places us in the realm of observed studies. The problem we are faced with in all non-experimental settings, is to find the treatment assignment probabilities decided upon by nature.

Indeed, even experiments can fail to properly randomize if the execution is sloppy. A random treatment is not an ontological property, defined by the fact that some humans decided which subjects should get which treatment. Rather, it is an epistemic property, defined by us *knowing* the treatment propensities and properly accounting for these propensities in our statistical models. Of course, assigning treatments oneself is generally a very good way of gaining this knowledge.

In this dissertation, there will always be doubt about the treatment propensities, and I therefore do not deal with randomized treatments. However, I do make the argument that the concepts we get from the experiment literature can be very useful in non-experimental settings. Importantly, by changing concepts, I found it easier to see the flaws in already published studies, and arrive at solutions to them. Widening the repertoire of ways to look at causal inference — such as employing different concepts to address the same underlying problems — is useful for improving causal inference.

I would like to offer one comment about the use of concepts. In the third article, I use the word "naturally occurring experiment" to describe what I am doing (Nordkvelle, Rustad, and Salmivalli 2017). I have since concluded that "experiments" should only be used to denote research situations set up by the experimenters, in which the treatment is devised and introduced by the experimenters. A "naturally occurring experiment" is used in Gerber and Green (2012, p.15) to refer to cases in which treatment has been randomized and assigned by some human agency, but not for the sake of making an experiment. What I do in article three is more akin to a "quasi–experiment". However, the term "quasi–experiment" is really an oxymoron, and therefore meaningless. Although it may have some use in putting some readers in the right frame of mind, its use has gotten a highly varied reception. I have refrained from using the word "experiment" in the same way in the other articles, but, because the third article has already been published, the use there will remain.

1.8 Overview of the articles

Table 1.1: Tabular overview of articles

Article	Climate variable	Randomization	Conflict variable	Context
1	None	None	Armed conflict incidence	Future socio-economic development
2	SPI	Across time	Armed conflict onset	Excluded groups
3	SPI	Across time	Communal conflict event	Conflict hot-spots
4	SPEI	Across space	Participation in political violence	Urban/Rural

1.8.1 First article

Hegre, Håvard, Halvard Buhaug, Katherine V Calvin, Jonas Nordkvelle, Stephanie T Waldhoff and Elisabeth Gilmore (2016). "Forecasting civil conflict along the shared socioeconomic pathways." *Environmental Research Letters*, 11(5): 054002.

The first article[6] was the first attempt at quantifying baseline conflict risks using the SSP/RCP framework that is being used by the larger climate impact community (Moss et al. 2010; O'Neill et al. 2017). Since the effects of climate change will be observed in the coming decades, and since the world is changing rapidly, it is important to take into account expected socio-economic developments when generating expectations of the likely impacts of climate change on violent conflict. The onset of armed conflicts has detrimental effects on societies, which can affect environmental vulnerability, socio-economic development, institutional quality, and the likelihood of observing other conflict types. Having good forecasts of armed conflicts is therefore important to generate good expectations about future impacts of climate change.

Specifically, this article explores how armed conflict patterns can be expected to develop between 2014-2100 under five different socio-economic scenarios ("shared socioeconomic pathways" or SSPs) that were developed to give us variation in challenges to climate change mitigation and adaptation. The goal of the study was to explore the variation in baseline risk of armed conflicts that our models produced using the different scenarios in the SSPs. Two questions were of particular interest: whether the cost of climate mitigation would involve increased risks of conflict, and whether plausible, but pessimistic, scenarios for climate adaptation would still yield an estimated reduction in the likelihood of conflict, as reported in Hegre et al. (2013).

The model we use is a dynamic multinomial model of country-years with three possible outcomes: no conflict, minor conflict (25–999 annual casualties) and major conflict (1000 or more casualties). The structural variables that vary in the scenarios are population, GDP/capita, and education (percentage of young men with higher education). The exact parameterization was chosen by using the model (which was estimated using data for 1960–2000) that had the best predictive performance for the period 2001–2013. The structural variables we use in the model are all known to be good predictors of armed conflict, and a wide range of conflict theories support the general directions of the parameters in the model.

The main finding in the model is that conflict propensities across the

6. During the course of writing this dissertation, I changed my name from Nordkvelle to Vestby. My original name appears on the two articles published before my name changed.

quantifications made by the climate community vary most in relatively poor countries with recent conflict histories. Another way to look at the same finding is to see that the cost of changing the economic system to one that is environmentally sustainable (assuming that the cost is mainly absorbed by the rich countries), will not affect the propensity for armed conflict in these rich countries. We believe the model supports the idea that rich countries can risk transforming their economies, even if doing so would involve a lower growth in the medium term than would following "business-as-usual". Although the forecasts for poor countries with recent conflict histories vary most, we also find that the scenarios lean toward optimism; that is, it seems more likely that conflict risks are going down, rather than up.

Another use of this study is to give information back to the development of the scenario quantifications. One obvious point is that conflicts directly affect the structural variables (Gates et al. 2012). Another is that long term economic growth must take into account the political economy (Acemoglu 2009, Part VIII). This is currently not done in the SSPs.[7] The education projections also have some quite dramatic, and unlikely assumptions, such as a complete freeze in education attainment in SSP 3. Conflict researchers have much to contribute when it comes to developing reasonable future scenarios, which can contribute not only to climate-conflict research, but also to the wider research on the social impact of climate change.

This study did not include forecasts of climatic factors, such as average temperature or climate variability (e.g., more extreme weather), unlike a similar study which came out this year (Witmer et al. 2017). Although this was a possible route for us to follow, we did not take it for to two reasons: First, armed conflicts at the country-year level, which was the type of conflict that we were confident we could forecast with some skill into the future, had not been proven to be robustly affected by climate variability (Buhaug 2010b; Miguel and Satyanath 2011). Second, sub-national data were needed to study the areas where the causal relationship between climate variability and conflict outcomes was expected to be stronger. How-

7. The current model is an augmented Solow growth model (Acemoglu 2009, p.26ff), in which the augments are a notion of human capital, and explicit modeling of energy demand and patterns of extraction of fossil fuel resources (Dellink et al. 2017). The assumption in the model that poor countries will converge with rich countries over time (i.e. grow faster) does not fit well empirically with the experience of many countries of the world (Acemoglu 2009, p.15ff).

ever, these studies presented methodological issues which had not yet been solved.

1.8.2 Second article

> Vestby, Jonas (Working Paper). "Climate shocks, environmental vulnerability, mobilization, and the onset of ethnic civil conflicts"

This study began as a critique of the current way we study sub-national effects of climate variability on conflict event data, and, in particular, armed conflicts. A large share of studies on climate exposure on armed conflicts test motivational theories of violent action using data that spatially matches climate exposure with armed conflict events (O'Loughlin et al. 2012; O'Loughlin, Linke, and Witmer 2014; Harari and La Ferrara 2014; Maystadt and Ecker 2014; Maystadt, Calderone, and You 2015), despite the fact that the creator of the dataset warned against this use, arguing that "it is exceedingly difficult to argue that a location, in and of itself, is an appropriate unit given the networks and movement of conflict over time" (Raleigh and Kniveton 2012, p.55). In addition to this (attrition) problem, this article identifies four areas in which published studies using disaggregated data all, to some degree, fail to provide good enough solutions: accounting for climate treatment propensities, treatment interference and conflict contagion, excludability, and the Modifiable Areal Unit Problem. The design which is suggested is an attempt to avoid or solve these issues.

Country-year studies of the relationship between climate shocks and armed conflicts do not find strong or significant effects (Buhaug 2010b; Ciccone 2011). One possible reason, it was argued, is that the relationship between climate shocks and economic outcomes were reduced in later years (Miguel and Satyanath 2011). This means, however, that there could still be sub-populations in the world where stronger effects could be observed. The attrition problem above has made testing this proposition elusive, however.

The article argues that a "most likely" sub-population that could provide evidence of a stronger relationship is politically excluded and regionally based ethnic groups. Such groups should be expected to be more environmentally vulnerable due to *ethnic favoritism* and disfavorable "initial" conditions (Kramon and Posner 2013; Fearon and Laitin 2011). Additionally, ethnically based rebel groups should be expected to be comparably better equipped than non-ethnically based rebel groups to take advantage of climate induced economic damages through *ethnic mobilization* and group-grievances (Eck 2009; Cederman, Wimmer, and Min 2010; Cederman, Gleditsch, and Buhaug 2013). However, the estimated effects for this sub-population are neither statistically significant nor substantive in a global sample from 1946 to 2013. The article concludes that we should therefore not think that climate shocks were a major driver of armed conflict onsets in this period.

The design is made at the ethnic group-month level. A Standardized Precipitation Index is calculated for each month of the year for each groups' main living areas, as defined in GeoEPR (Vogt et al. 2015). UCDP/PRIO Armed conflict onset months are then matched to the ethnic groups using a dataset on the relationship between rebel groups in the UCDP/PRIO ACD and ethnic groups in the Ethnic Power Relations dataset (Gleditsch et al. 2002; Allansson, Melander, and Themnér 2017; Vogt et al. 2015). I constrain the data to contain only politically excluded and regionally based ethnic groups in order to increase causal heterogeneity and prevent treatment interference and attrition. Twelve group fixed-effects are added, one for each month of the year, for each group, to account for differing climate treatment propensities over a year. Causal identification is based on the argument that the probability of a given precipitation level for a given group-month (e.g., Maya living areas in January months) is the same between January 1946 and December 2013.

1.8.3 Third article

Nordkvelle, Jonas, Siri Aas Rustad and Monika Salmivalli (2017). "Identifying the effect of climate variability on communal conflict through randomization." *Climatic Change*, 141 (4), 627–639.

This article was motivated in particular by observing that disaggregated studies of climate variability on communal conflicts failed to convincingly account for treatment interference, treatment propensities, and the Modifiable Areal Unit Problem. The approaches used, such as matching climate variability and conflict events at village levels (Raleigh and Kniveton 2012), and using annualized SPI-6 (Fjelde and von Uexkull 2012, p.449), were vulnerable to methodological critique. In this article, we wanted to make a more robust test of the effect of climate variability on communal conflicts.

Theoretically, there are several good reasons to believe that climate variability will have a stronger effect on communal conflict, than on state-based conflict. Communal conflicts are usually fought between groups that are militarily weaker than the state (Raleigh and Kniveton 2012, p.53). Communal conflict events are more likely to be initiated by groups in order to attain short-term goals. State-based conflict events are more often parts of military tactics in a larger campaign (Fjelde and von Uexkull 2012, p.446). This means that in non-state conflicts, it is more reasonable to explain individual events with reference to economic motivations, whereas in state-based conflicts each individual event is more likely to be contingent on the ongoing fighting.

Specifically, we studied relatively large sub-national hotspots for what UCDP defines as non-state conflict events between informally organized groups ("communal conflicts") at a monthly time scale (Petterson 2016). By studying whole areas where communal conflicts have been fought, we sought to eliminate the potential for treatment interference. What is new, in contrast to previous studies, is the unit of analysis, that we use SPI directly, use monthly unit fixed-effects, and test different SPI aggregations (SPI on 1-month aggregates, 3-month aggregates, 6-month, etc.).

Testing the effect on different month aggregates is to some extent arbitrary. This means that significant variations depending on aggregation show that MAUP is a problem. However, at the same time, different aggregations also measure different types of exposures. A low SPI-1 value usually means that the soil is drier than usual, but that this may not affect groundwater or reservoir levels much. A low SPI-12 may not measure dry soils, but, rather, reservoir levels.

Our use of different exposures could lead to the criticism that, if we test many different treatments, at some point we are going to hit a significant result at random. We therefore also interpret our results using a Bonferroni correction (dividing the target p-value by the number of tests) to avoid this problem.

We find that short-term unusually dry climate (dry soil) and long-term unusually wet climate (better stream flow, larger water reservoirs) both increase the probability of observing a communal conflict event in a month.

We explain the first finding with a resource scarcity hypothesis, and the second, with a resource abundance hypothesis.

In terms of severity, we find that the effect implies a 4% higher probability of a conflict event, on average, in about 1/10 of the months in communal conflict hotspots, *if we somehow could sever the link between climate variability and social disorder*. Thus, although this mean that lives could be spared if order in these societies were less dependent on the climate, there are probably more effective ways to reduce the probability of communal conflicts.

1.8.4 Fourth article

> Vestby, Jonas (Working Paper). "Climate variability and individual motivations to participate in political violence"

Economic outcome is the most commonly suggested link between climate variability and violent conflict. In the two preceding articles, I was not able to directly test whether economic outcome was, in fact, the intermediary variable between climate variability and conflict. One reason is that the data quality of economic performance for excluded ethnic groups from 1946 and beyond, as well as for the six areas we studied in the communal conflict article from 1989 onwards, is very poor (and I lacked monthly resolution data). In these articles, the economic pathway was implied in the subset I was studying, and by reading other research in the field, e.g., Miguel, Satyanath, and Sergenti (2004) and Schilling, Opiyo, and Scheffran (2012).

I also could not directly test individual motivational theories of conflict using aggregated data. For instance, economic losses in the excluded ethnic group territory could have been used by political entrepreneurs to mobilize against the government. Or individuals who experienced deterioration in living conditions could have become more willing to join a rebel group, due either to opportunity costs or, to personal grievances against the state. I wanted to test whether individual motivational explanations of participation could find quantitative empirical support. There are relatively few quantitative studies of motivational theories of participation in violence at the individual level (Oyefusi 2008; Humphreys and Weinstein 2008; Blattman and Annan 2016), and none have tested whether climate variability can explain such individual variations. This was the motivation for the fourth article of this dissertation.

Theoretically, I argue that previous studies have been unclear as to whether motivation and opportunity should be understood at the individual or group level. Opportunity costs, for instance, motivate individuals, but can create an opportunity effect at the group level. Properly identifying motivations and opportunities at the various levels of analysis reveals that many theoretical conclusions found in published literature do not follow directly from their empirical tests. For instance, even though climate variability can affect opportunity costs, this does not directly translate into more rebels, because the supply of rebels may already be saturated. In many conflicts, there are more willing rebels than there are rebels who are recruited, for instance because rebel groups need to make sure that new recruits can be trusted (Hegghammer 2013). *Who is most motivated* does not always explain *who fights*.

To test whether experiencing negative change in living conditions increases the motivation to participate in violence, I instrument personally reported changes in living conditions as reported in the Afrobarometer, rounds 2 and 5. I use a Standardized Precipitation-Evapotranspiration Index (SPEI), calculated for the first-order administrative area of the respondent, and test whether this instrumented treatment affects the likelihood of these same individuals reporting that they have participated in political violence.

I use SPEI, rather than SPI here, because the survey data used is only cross-sectional. SPI distributions are highly varying across space, whereas SPEI is a mix of two relatively independent processes (precipitation and temperature), making it more suitable for causal inference through randomization. One the other hand, SPEI is not ideal in time-series, because of quite strong trends in temperature over time, even locally. Although I am less confident in the assumption that SPEI is randomized in cross-sections, than in the assumption that SPI is randomized across time for the same units, overall, using SPEI is the best approach for studying this particular data.

For the unknown share of the population that responded to the instrument in the survey study (i.e. who experienced a deterioration in living conditions that would not have been experienced if the climate had been more favorable)[8], experiencing negative changes in living conditions increased the probability of participation in political violence by around 12 percentage points. Since the share is unknown, the severity of this number is also unknown. Beyond indicating the severity, the figure lends credibility to the claim that negative changes to personal living conditions motivate participation in political violence, and that such losses can be induced by the climate in contemporary African contexts.

1.9 Conclusions

Will climate change increase the prevalence of violent conflict in the future, and if so, why? In this dissertation, I argue that to answer that question, we need both to learn from the past and to map uncertainties of our future. This dissertation makes two main contributions to climate-conflict research: it indentifies a lack of methodological rigor in disaggregated studies and presents solutions to the identified problems, and it offers suggestions for building better expectations of the *future* political and socio-economic context.

A key theme which arises from the dissertation is the importance of taking into account the socio-economic and political context both when estimating causal effects of different violent conflict outcomes and when making forecasts about the future effects of climate change on violent conflict outcomes. It is not only the climate which is likely to change in the future, but the context itself is also changing rapidly. Knowing how different types of conflict in different contexts react to climate variability is therefore essential for generating good expectations about the future.

This dissertation contributes to improving our expectations about this future context by creating forecasts of baseline armed conflict risks at the country level up to the year 2100. The first article creates different forecasts for five different socio-economic scenarios, based on narratives built for the climate impact community (Hegre et al. 2016). The study shows that conflict risks vary most among scenarios in poor and conflict-ridden countries, and that conventional models of armed conflict do not lead us to expect that the cost of transforming the economy to achieve

8. Causal estimates from second stage IV models are complier average causal effects/local average treatment effects (Gerber and Green 2012).

sustainable development — if borne by rich and developed countries — will result in an increased likelihood of armed conflict.

By formalizing the models used in the forecasting, we make it easier to discuss assumptions, model dynamics and input data. The study reveals that the SSPs take political contexts insufficiently into account. Countries with a recent history of violent conflict have a higher chance of recurrence of violence, because, among other things, they destroy wealth, hinder development, and reduce the quality of government institutions (Hegre et al. 2017). The conflict trap is real (Braithwaite, Dasandi, and Hudson 2016; Gates et al. 2012; Costalli, Moretti, and Pischedda 2017). Although assuming that socio-economic drivers are exogenous can be useful for streamlining research (Moss et al. 2010), future forecasts need to do a better job taking into account the endogenous relationship between conflict exposure and economic development.

Socio-economic development is less certain in poor and conflict-ridden countries, and not simply because of recent violent conflict. Many of the changes a society needs to expand its economy are structural, and involve changing of elites and their preferences (North, Wallis, and Weingast 2009; McMillan, Rodrik, and Verduzco-Gallo 2014). The unknowns in such a situation, with respect to processes and structures are vast in contrast to the certainties of a stable an well-managed modern economic system (Acemoglu 2009; Banerjee and Duflo 2005; Temple 2005).

It is safe to say that the effect of climate variability on conflict outcomes can be assumed to be highest in poor and conflict-ridden countries. This can be argued just on the basis of a higher baseline probability of conflict in poor and conflict-ridden countries (Hegre et al. 2016).

There are, however, several other structural factors that can explain why climate variability should have stronger effects in particular areas, and for particular types of conflicts. First, the share of labor working in agriculture increases the number of people directly affected economically by adverse climatic conditions (Miguel, Satyanath, and Sergenti 2004). This share is closely correlated with GDP/capita (Restuccia, Yang, and Zhu 2008, Figure 1), and is commonly associated with poverty. The fourth article in this dissertation shows that climate variability only has an effect on reported changes in living conditions in rural areas in Africa.

Second, underemployment indicates a difficulty in finding alternative, legal work. Recent studies find that underemployment is large in many

African countries (McCullough 2017). For the subset of the population that is both willing to use violence and has an opportunity to do so, a lack of viable legal ways to gain an income can be an important reason for participation in violent work (Blattman and Annan 2016).

Third, a government's inability and/or unwillingness to police violence between non-state groups lowers the threshold for organizing violent activities aimed at immediate pecuniary rewards (Elfversson 2015; Rudolfsen 2017). Similarly, poor and/or biased government interventions can severely increase grievances among local groups (Benjaminsen and Ba 2009). In such instances, adverse climatic conditions can accentuate the lack of government intervention, the existence of unfair policies, or the opportunities for use of violence for pecuniary aims.

All of these accentuating factors are tied to economic development and institutional quality. Thus, there is an inherent uncertainty about the future effect of climate on violent conflict. The effect is likely to be much smaller if the countries most likely to be affected today manage to grow and provide better services to their population. Developing more precise expectations about growth trajectories in these countries is therefore one of the most effective ways of improving our expectations about the effect of climate on conflict in the future.

The other main contribution of this dissertation is to address methodological weaknesses in published disaggregated studies of climate variability and violent conflict and offer solutions. Such studies are necessary to provide empirical data on context-specific and theory-near relationships. The increased resolution and data on new types of conflict outcomes have generated intriguing theoretical propositions and novel methodological designs. This literature has several methodological problems, however. Although not all published articles make every possible mistake, most articles make some. To address these problems, I suggest, in essence, that the selection of the unit of analysis and the operationalization of the climate treatment should be made to give variation on the theoretical issue at hand, and to simplify and avoid as many other issues as possible (such as having to control for varying treatment propensities, treatment interference, or mismatches between treatment and outcome).

Three of the four articles in this dissertation follow this strand of inquiry. One reason is that different conflict types, and different theoretically interesting questions, require different methodological solutions. There is no one solution that can be used for all outcomes that will provide unbiased causal inference. Furthermore, studying different conflict types and different social contexts generates variance, which can be used to make theoretical advances in answering the question of whether climate change will increase the propensity of violent conflict in the future.

The social scientist Charles Tilly argued that violent collective action requires a capacity for organization and mobilization. Steeped in the writings of John Stuart Mill, Karl Marx, Émile Durkheim and Max Weber, he argued that theories that focus only on individual motivations and ignore the issue of mobiliation may lead us to believe that "the faster and more extensive the social change, the more widespread the anomic and restorative forms of collective action: concretely, we will expect rapid industrialization or urbanization to produce exceptionally high levels of conflict or protest" (Tilly 1978, p.23). It is worth thinking about Tilly's words in the context of climate-conflict studies. Even if climate change leads individuals or communities to move, lose their livelihoods, or worse, it does not necessarily follow that these individuals or communities will mobilize into violent collective action to any greater extent than they might have in the absence of climate change. The capacity for mobilization can deteriorate just as quickly as the motivation for action can increase. That being said, the possible effects of climate change are undesirable and politicians should work both to reduce environmental vulnerabilities and to improve the capacity of individuals and societies to cope with future climate change, regardless of whether or not negative economic impacts lead to armed conflicts. Climate change need not be a security threat. But this does not mean it should not be mitigated and prepared for.

The dissertation focus on climate shocks, the causal effects of which are easy to measure. Climate change will not only lead to higher amplitudes in climate variations, however. Some of the adverse impacts of climate change on societies will be gradual and predictable. Rising sea levels, higher temperatures than are comfortable for humans, animals and plants, and stronger cyclones will all result from climate change. Such developments may lead to adaptation strategies, such as migration. The scope of effects tested in this dissertation therefore does not encompass all possible climate change impacts that could have an effect on conflict outcomes.

Such impacts may need to be studied through means other than causal inference, for instance through validation of causal claims through the evaluation of out-of-sample predictive performance. But the general pattern used in this dissertation is significant for future studies of the social impact of climate change on violent conflict. Rigorous causal inference, out-of-sample prediction evaluation, and formal forecasting models need to be combined with theoretical rigor and attention to detail.

Part II

Articles

2 Forecasting civil conflict along the shared socioeconomic pathways

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LETTER

Forecasting civil conflict along the shared socioeconomic pathways

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Abstract

Climate change and armed civil conflict are both linked to socioeconomic development, although conditions that facilitate peace may not necessarily facilitate mitigation and adaptation to climate change. While economic growth lowers the risk of conflict, it is generally associated with increased greenhouse gas emissions and costs of climate mitigation policies. This study investigates the links between growth, climate change, and conflict by simulating future civil conflict using new scenario data for five alternative socioeconomic pathways with different mitigation and adaptation assumptions, known as the shared socioeconomic pathways (SSPs). We develop a statistical model of the historical effect of key socioeconomic variables on country-specific conflict incidence, 1960–2013. We then forecast the annual incidence of conflict, 2014–2100, along the five SSPs. We find that SSPs with high investments in broad societal development are associated with the largest reduction in conflict risk. This is most pronounced for the least developed countries—poverty alleviation and human capital investments in poor countries are much more effective instruments to attain global peace and stability than further improvements to wealthier economies. Moreover, the SSP that describes a sustainability pathway, which poses the lowest climate change challenges, is as conducive to global peace as the conventional development pathway.

1. Introduction

While the global incidence of armed conflict has declined markedly in recent decades [1], this trend may not continue. Poor economic performance [2] combined with sustained population growth [3], especially in developing countries, may lead to increased conflict in the future. The impacts of climate change may further hinder socioeconomic development [4, 5] and thus constitute a significant 'threat multiplier' to stability in vulnerable societies [6]. Assessing the impact of climate change on conflict, however, is complicated. Not only is the main impact likely to be indirect, but climate change itself depends on socioeconomic changes that also profoundly affect conflict propensities. Moreover, specific trajectories of socioeconomic development that facilitate peace may not necessarily facilitate mitigation and adaptation to climate change. For example, while economic growth lowers the risk of armed conflict, this growth is generally associated with increased greenhouse gas emissions and increased costs of mitigation policies. The newly developed societal scenarios [7, 8], known as the shared socioeconomic pathways (SSPs), for the first time permit a systematic investigation of how these links may play out in the future. This study represents the first attempt to simulate trajectories of future conflict along the five SSP scenarios, based on an estimation of the historical association between socioeconomic conditions and conflict involvement. We show that SSPs that imply high challenges to *adaptation* to climate change are associated with

Table 1. Global characteristics of the five shared socioeconomic pathways.

Pathway	Mitigation challenges	Adaptation challenges	Economic growth	Population growth	Education attainment
SSP1: Sustainability	Low	Low	High	Low	High
SSP2: Middle of the Road	Medium	Medium	Medium	Medium	Medium
SSP3: Fragmentation	High	High	Low	High	Low
SSP4: Inequality	Low	High	Medium	Medium	Low
SSP5: Conventional Development	High	Low	High	Low	High

Note: table adapted from Chateau, Dellink, Lanzi, and Magné [28].

increased levels of internal armed conflict in the future. Whether and in what way SSPs that imply high *mitigation* challenges will affect future conflict propensities is less clear.

The conflict research community has identified a handful of robust country-level correlates of civil conflict, the three most powerful of which are a history of prior conflicts, a large population, and a low level of socioeconomic development [9, 10]. Larger populations are associated with more conflict due to larger heterogeneity of identities and preferences, larger pools of potential rebels, and logistical challenges of controlling large territories [11]. Strong economic performance decreases conflict risk by strengthening networks of economic dependence among societal groups, reducing public grievances, increasing costs of rebel recruitment, and strengthening the state's counterinsurgency capability [12, 13]. Other aspects of socioeconomic development, including school enrollment levels, educational expenditures, and literacy rates, also have a pacifying effect [14, 15].

Previous attempts to evaluate the association between climatic conditions and conflict risk have largely focused on direct, short-term effects of variations in weather patterns, using proxies such as temperature, precipitation, and various drought indices. Taken together, this work reveals a weak and inconsistent climate effect, with some studies concluding in direct opposition to each other [16–21]. An indirect and conditional effect is more plausible, particularly because consequences of climatic shocks depend on the affected societies' resilience and adaptive capacity. Similarly, changing weather patterns and other physical processes associated with climate change can amplify common drivers of armed conflict, such as economic underperformance, food insecurity, and human displacement [22, 23], but these effects will vary with the affected societies' level of development. Current attempts to forecast future conflict trends [24–26] have not accounted for indirect effects of climate change.

To investigate security implications of alternative climate change-related scenarios, this study draws on the SSPs [8]. The SSPs were developed to evaluate the uncertainty in how impacts of climate change and the ability to mitigate adverse societal effects may evolve as a function of socioeconomic drivers. The scenarios are designed to span a range of alternative futures and are shaped by different assumptions about society, including economic development, education improvements, and population growth. Unlike earlier scenarios developed by the climate change research community—such as the Special Report on Emissions Scenarios—the SSPs are explicitly decoupled from the physical processes associated with climate change. Instead, each pathway is defined in terms of challenges to climate change mitigation and adaptation. High challenges to mitigation are here understood as involving high dependence on fossil fuel-based energy and low levels of international cooperation on global environmental issues. High adaptation challenges are characterized by low development growth rates, low investments in human capital, and increasing economic inequality [27]. Four of the scenarios (SSP1, SSP3, SSP4, SSP5) capture the four possible combinations of low and high barriers to adaptation and mitigation whereas the fifth (SSP2) represents a middle pathway. The key components of the SSPs are summarized in table 1.

Although the specified SSP scenarios comprise a variety of possible futures, one might imagine other constellations of economic and demographic development as well as greater spatiotemporal heterogeneity in growth rates that could be considered equally plausible pathways. For the sake of consistency and to provide an explicit evaluation of some implications of the modeling decisions underlying the original SSPs, we decided to remain true to these pathways. We reflect on some limitations imposed by the SSP framework in the concluding discussion.

The quantification of the five socioeconomic pathways are based on existing end-of-century projections of population, GDP per capita, and the proportion of young males with upper secondary schooling or higher. Figure 1 compares the projected trends in economic development and educational attainment across the scenarios. All SSPs display positive economic growth per capita, though aggregate and country-specific growth rates vary considerably between the scenarios. In fragmentation (SSP3) and inequality (SSP4), educational attainment rates remain very low, although they vary by socioeconomic level in SSP4. In

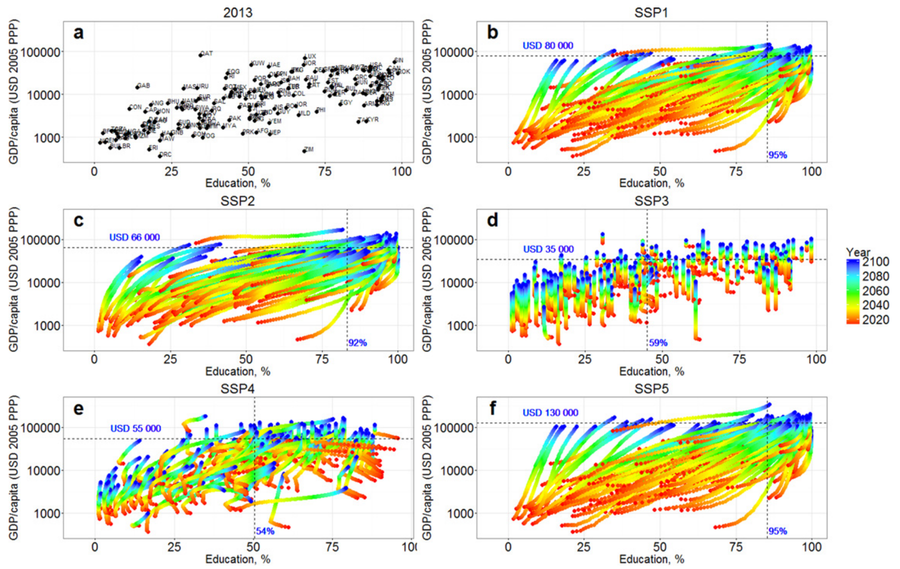

Figure 1. Projections of economic output and education levels along the shared socioeconomic pathways. Vertical axes represent $\log_{10}$ GDP per capita in constant 2005 US Dollars whereas horizontal axes represent secondary education attainment (%) among males, age 20–24. Panel (a) shows the situation in 2013 with three-letter country identifiers of individual countries (see table S12 in supplementary information for details); panels (b)–(f) visualize SSP1–SSP5 with colors denoting time scale from 2014 (red) to 2100 (blue). Dashed lines indicate projected end-of-century global means of GDP per capita and education level for each scenario.

both cases the countries' points of departure represent the situation several years prior to 2014, hence the downward adjustment in the initial years; see [29] for a complete description. The other scenarios assume continued progress in educational attainment throughout the century.

2. Methods and data

Our approach builds on the model presented by Hegre *et al* [25] and consists of three main steps. First, we assembled a joint dataset of historical and projected variables, covering all independent countries in the world for all years, 1960–2100 [30]. In addition to the socioeconomic indicators shown in figure 1, we include country-specific information on population size, conflict history, time since independence, and conflict involvement among neighboring countries, all of which have been shown to exert consistent influence on civil conflict risk [9, 10], and for which authoritative projections until 2100 are available. Historical conflict data are derived from the UCDP/PRIO Armed Conflict Dataset [1, 31]. A civil conflict is here understood as a military conflict between a state and one or more non-state actors over territorial or government control that results in at least 25 battle-related deaths in a calendar year. The analysis distinguishes between minor conflict (25–999 annual casualties) and major conflict (⩾1000 casualties). In line with the SSP framework, socioeconomic development is operationalized as GDP per capita and secondary educational attainment among men aged 20–24. The historical data as well as projections along the SSPs draw on two existing models. Population and education scenarios up to 2100 are based on the IIASA and Wittgenstein Centre for Demography and Global Human Capital model [32–34]. The original historical dataset spans 1970–2010. We extrapolate education rates back to 1960, assuming similar rates of change as for the 1970–2010 period. Historical GDP per capita statistics are derived primarily from the World Development Indicators [35]. Economic growth beyond 2013 is projected using an augmented Solow growth model with representations of human capital and fossil fuel usage, known as the OECD ENV-Growth model [28]. As the projected data come in five-year intervals, we linearly interpolate between the time steps. We also adjust the data to align the historical records with the starting point of the SSPs. Finally, a small number of countries for which we have historical data are not defined in the SSPs. To maximize the number of countries in our analysis we borrow adjusted projections from appropriate matching countries for these cases. See sections S3 and S8 in supplementary information for further details on the construction of the dataset.

Second, we developed a statistical model of civil conflict onset, duration, and termination. The unit of analysis is the country-year, 1960–2013. We use a

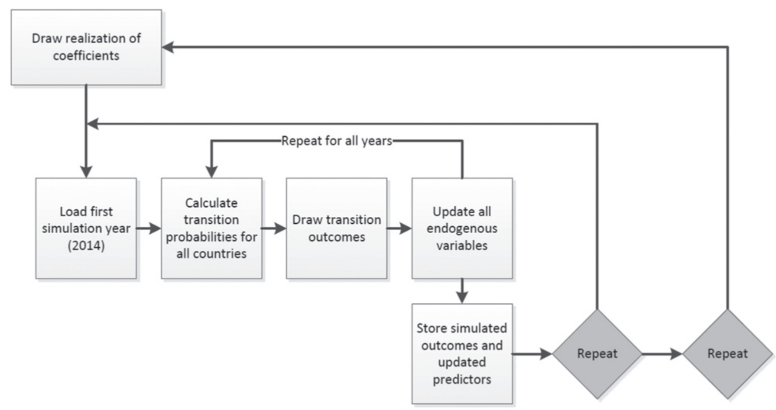

Figure 2. Flow chart of the simulation process.

random-effects multinomial logit model to estimate the transition probabilities between peace, minor conflict, and major conflict as a function of temporally and spatially lagged conflict indicators, population, GDP per capita, and educational attainment, as well as interaction terms for these socioeconomic factors, decade constants, and country-specific intercepts. The results from this model, which are used to inform the forecasting simulation, are reported in section S3.3 in the supplementary information. The preferred statistical model and inclusion of terms were determined based on an out-of-sample evaluation of model performance across various specifications; see section S5 for details.

Third, we used the statistical model and a simulation procedure to generate annual projections of armed conflict for each country over the SSPs, 2014–2100. Specifically, the procedure (i) calculates the transition probabilities for a given year for all countries based on a realization of the statistical coefficients, (ii) draws a conflict outcome based on these probabilities, (iii) updates all conflict variables, and (iv) moves on to the next year using the updated conflict history data. This is repeated for all years and for a large number of realizations of the estimated probability distributions of coefficients. In total, we run 9000 simulations for each scenario to account for the uncertainty in the parameters (40 simulations for each of 15 realizations of the country intercepts for each of 15 realizations of parameter estimates). The general setup of the simulation procedure is shown in figure 2.

3. Results

We began the simulation analysis by estimating the historical effect of socioeconomic development on civil conflict occurrence, 1960–2013. The results from this empirical analysis, documented in table S3 in supplementary information, are consistent with those reported in earlier research [10, 12, 13]. Based on the observed situation at the end of 2013 and the estimated probabilities of transition between different levels of conflict implied by our empirical model, we then simulated the annual incidence of civil conflict onset, duration, and termination for all countries across the globe along the five SSPs, 2014–2100.

Figure 3 shows the aggregate results from these projections, expressed as the annual proportion of countries in armed civil conflict by scenario and region. Because the conflict forecasting procedure is probabilistic in its approach, it generates prediction intervals around the mean projections. Each projection is shown with a one-standard deviation prediction interval—i.e. the band within which 68% of the simulated proportions lie, assuming logistic-normal distribution [36].

In line with earlier forecasting efforts [24], we find that global conflict incidence declines as socioeconomic development (GDP per capita and education) increases, while larger populations generally are associated with higher rates of conflict. In sustainability (SSP1), middle-of-the-road (SSP2), and conventional development (SSP5), the positive socioeconomic projections outweigh the impact of expected population growth and result in declining conflict rates over time. In the fragmentation (SSP3) and inequality (SSP4) pathways, however, low investments in education and technological innovation and medium to high population growth imply upward conflict trends. Overall, the most dramatic reduction in global conflict is observed in conventional development, where the mean estimate of projected end-of-century incidence is only a quarter of that of fragmentation. Although conventional development has the highest rate of economic growth and the lowest overall global conflict burden, this result is not statistically different from the sustainability scenario (SSP1), in

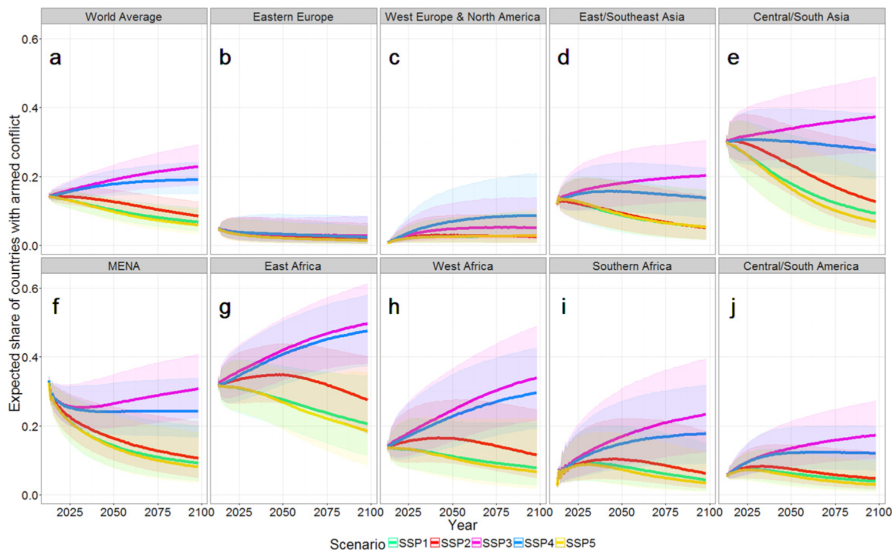

Figure 3. Projected proportion of countries in armed conflict by scenario and year, 2014–2100. Panel (a) shows the world average results; panels (b)–(j) show regionally disaggregated estimates ('MENA' refers to the Middle East and North Africa). Shaded areas represent one SD (68%) prediction intervals. Table S12 (supplementary information) provides details on the region classification.

which the world has a far better capacity to adapt to and mitigate climate change.

GDP per capita is essential to conflict reduction primarily to the extent that it is translated into broader social welfare improvements. High educational attainment rates may better proxy this type of development, and projections for education are the same in sustainability and conventional development. By contrast, fragmentation implies a marked increase in global conflict over time for most regions. For inequality, the richer developed countries do equally well as in conventional development. The poorer countries, however, have conflict risks that are more akin to fragmentation since the global average improvements in socioeconomic development are driven largely by the wealthiest countries that already enjoy very low conflict risks. Expectations of sustained population growth in Africa also increase the conflict rate, especially in the high population growth pathway of fragmentation. There is modest spatial variation across scenarios in the sense that existing conflict clusters (Central Africa, the Middle East, and South Asia) remain centers of instability throughout the century in all pathways, but the magnitude of conflict varies considerably. Overall, the lowest estimated rates of conflict are found in Eastern Europe, owing to the region's low and declining population and sustained economic growth.

To demonstrate the positive security impact of improvements in socioeconomic conditions and restrained population growth—which by design are associated with lower barriers to climate change mitigation—figure 4 visualizes the difference in estimated end-of-century conflict risk between fragmentation (SSP3) and sustainability (SSP1). In this setting, wealthier countries are minimally affected by the choice of scenario; their conflict propensities are small across all SSPs. In contrast, most countries in the developing world show dramatic reductions in conflict incidence with sustainability relative to the fragmentation pathway. More generally, the improvements in socioeconomic conditions in Africa—a present hotspot of armed conflicts—play an important role in reducing future global conflict burdens. This is highlighted by inequality (SSP4), where Africa continues to observe increasing conflict rates due to low socioeconomic development and high population growth. Additionally, the marginal impact of more modest economic growth in sustainability compared to the fossil fuel-dependent conventional development (SSP5) does not imply a significant increase in conflict risk because the burden of lower rates of economic growth are absorbed by wealthy, robust countries. Section S7.1 in the supplementary information provides further details on regional and country-specific results from these simulations.

4. Discussion

Our projections of armed conflict are a function of the quantified variables defined exogenously in the SSPs. The projections for armed conflict also have implications for these variables, notably GDP. The SSPs are

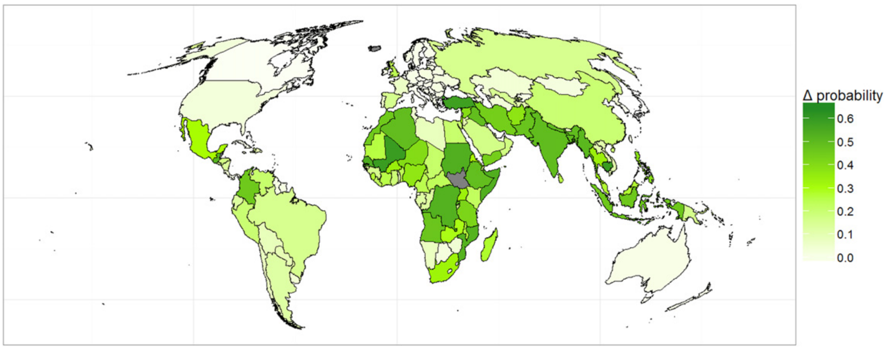

Figure 4. End-of-century differences in estimated conflict risk between SSP1 and SSP3. Darker shades indicate larger benefits in terms of reduced absolute conflict risk by shifting from the adverse regional fragmentation scenario (SSP3) to a sustainable growth scenario (SSP1). Countries in gray have insufficient historical data to be included in the forecasting model.

designed to span a range of expectations for future development trajectories, but the limited set of alternative scenarios and modeling assumptions necessarily imply that other, perhaps equally plausible, futures are not captured. For educational attainment, the more optimistic SSPs project near universal secondary education rates by the end of the century, while fragmentation (SSP3) and inequality (SSP4) assume virtually no progress at all for most countries. These bounding assumptions are reflected in the two distinct sets of projections of conflict incidence (figure 2). By contrast, GDP growth rates over the century differ in magnitude across SSPs, but all are positive. Thus, if we construct our model using GDP per capita only, the five SSPs have more similar conflict incidence forecasts (section S7.2 in supplementary information). Due to the assumptions of convergence in the OECD ENV-Growth model [28], even the most pessimistic scenario—SSP3—projects positive growth in GDP per capita for all countries. Historically, however, growth has been much more uneven across countries over the last four decades [37]. A partial reason is armed conflict, which often critically stunts GDP growth and other welfare developments [38, 39]. These types of political obstacles to growth are not included in the OECD ENV-Growth model and thus are not reflected in the SSPs. Lower economic growth in the SSPs that do not have broad societal development, such as SSP3 and SSP4, could generate further armed conflict which in turn may cause additional hindrances to mitigation and adaptation. For this reason, the end-of-century conflict rates for the worst-case scenarios should be considered conservative estimates, and the value-added of following a sustainable growth pathway may be larger than suggested by the simulation model[7].

[7] In the supplementary information (figure S17), we show results from an additional simulation, based on SSP3 but with no improvement in GDP per capita or education. This model yielded incidence rates slightly higher than those of the original SSP3.

The simulation draws on the three quantified indicators of socioeconomic development, but the SSPs also have qualitative narratives, or storylines, that describe five societal dimensions, including institutional quality and political stability [27]. Other non-quantified variables, such as oil revenues and inequality, may also affect future conflict risk. The higher reliance on fossil fuels in SSP5 would likely increase the estimated conflict propensity in countries that are highly dependent on such revenues [12, 13] or vulnerable to commodity price fluctuations [40]. Similarly, SSP4 entails high intra-country inequality that also could exacerbate conflict [41]. Importantly, the ability to manage grievances that can produce conflict depends on the legitimacy of political institutions [42]. In our model, institutional effects are captured partly by the socioeconomic variables from the SSPs and partly by country-specific intercepts that reflect unobserved differences between countries in the underlying conflict risk. Obviously, the country-specific intercepts approach implies a strong assumption about time-invariance. Although it is important to account for static risk factors (e.g., landlockedness, terrain, and historical legacy), political institutions and other malleable societal and contextual features are likely to change over the course of the century. Indeed, the evolution of national and intergovernmental institutions in the storylines for the SSPs is central to defining the challenges to mitigation and adaptation, where capable and responsible institutions are more likely to facilitate these actions. Absent quantification of these storylines, they are best accounted for by making qualified judgments about the simulated conflict trajectories.

An attractive feature of the SSPs is the opportunity to consider various combinations of societal and climate change scenarios in an integrated framework. At the same time, the explicit disconnect between the SSPs and climate change (beyond the different mitigation and adaptation challenges they impose) means that the conflict projections presented here do not directly capture effects of climate change. We show, however, that SSPs 3 and 4 that imply high challenges to adaptation to climate change are uniformly associated with increased levels of internal armed conflict in the future because societies that lack the capacity to adapt to climate change are the same as those that struggle with armed conflict. Whether and in what way SSPs that imply high mitigation challenges will affect future conflict propensities is less clear, however. If, as in SSP5, high mitigation challenges due to fossil fuel-driven economic growth are associated with low adaptive challenges across all countries in the world, we do not predict more conflict than SSP1, which implies small mitigation challenges. If the high mitigation challenges, on the other hand, unfold together with high adaptation challenges in large parts of the world (as in SSP3), armed conflicts will continue to be a serious global problem in the future. This assumes that there is no direct or indirect impact of climate change on conflict. In a final test, we investigated a possible separate effect of temperature anomalies on conflict risk. This test revealed a weak and insignificant effect in the historical sample, and accounting for temperature anomalies did not improve the predictive performance of the model (section S5 in supplementary information). We did not consider a direct effect of precipitation on civil conflict as the association between rainfall patterns and climate change is less well understood, especially at finer levels of spatial resolution [43].

Climate change, however, may indirectly affect core drivers of armed conflict. Radical mitigation policies or adaptation challenges may halt improvements to living conditions that are important to restrain conflict. Recent research provides little support for an indirect association between drought and conflict via poor agro-economic performance [44, 45], but this is an area that requires more research. This research, however, should not ignore the powerful impact on conflict propensity implied by socioeconomic development in itself.

Three other patterns in our simulations have particular policy relevance. First, the trajectory of future socioeconomic development will have a substantial impact on both the incidence of global conflict and the capacity to mitigate and adapt to climate change. Two of the five SSPs imply a reversal of the recent decline in armed conflict, with end-of-century global conflict rate for SSP3 being twice as high as today's and four times higher than that projected for the optimistic SSP5. Regional and between-country differences in estimated conflict risk across the pathways are more dramatic still (supplementary information, table S8). Second, while rapid, universal growth in GDP per capita is associated with substantial decline in the long-term risk of civil conflict, our model also shows that achieving broader socioeconomic development, as expressed by higher educational attainment rates, offsets most of the additional risk from reducing economic growth. The risk-reducing effect of education is especially pronounced among countries in the developing world. Third, our simulations reveal that investing in a sustainable future is fully consistent with an ambition of global stability and peace while simultaneously having comparatively low barriers to climate change mitigation and adaptation. Poverty alleviation and educational improvements in the global south thus stand out as key policies in achieving both objectives.

Acknowledgments

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3 Climate shocks, environmental vulnerability, mobilization, and the onset of ethnic civil conflicts

Climate shocks, environmental vulnerability, mobilization, and the onset of ethnic civil conflicts

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Abstract

Country–level studies of the effect of climate variability on the likelihood of armed conflicts have not found strong correlations. It is possible, however, that climate variability affects the incidence of armed conflict at the level of the sub–group but not an entire population, which may reflect large causal heterogeneity. This article argues that regionally based and politically excluded ethnic groups are a "most likely" sample for finding such effects, because they can be expected to be more environmentally vulnerable, and because rebel groups tied to ethnic groups can more easily mobilize the local population and motivate around group–based grievances. Disaggregated studies of armed conflicts cannot rely on spatial matching of climate exposure and conflict outcomes, however, because rebel strategic action and mobility can lead to bias through attrition and treatment interferences. To address this issue, the study matches armed conflict actors with ethnic groups and climate exposure with the main living areas of these groups. Other methodological issues, such as excludability, data quality and the Modifiable Areal Unit Problem, are also discussed in the article. The study does not find a significant or substantial effect for this global sample of ethnic groups between 1946 and 2013. This confirms country–level studies which fail to find that climate shocks have been an important driver of armed conflict onsets in the world after 1946.

1 Introduction

The last decade has seen the publication of several statistical studies estimating the causal effect(s) of climate variability on violent conflict. These studies use different climatic measurements, spatio–temporal scales, conflict outcomes, and identification methods (Miguel, Satyanath, and Sergenti 2004; Burke et al. 2009; Buhaug 2010; Hidalgo et al. 2010; Bohlken and Sergenti 2010; Theisen, Holtermann, and Buhaug 2011; Fjelde and von Uexkull 2012; Dube and Vargas 2013; Couttenier and Soubeyran 2014; O'Loughlin, Linke, and Witmer 2014). Whereas early articles were country–year studies of armed conflict incidents or onsets, more recent articles study the disaggregated effects of a wide variety of conflict outcomes, such as riots (Bohlken and Sergenti 2010), land occupations (Hidalgo et al. 2010) and communal conflicts (Fjelde and von Uexkull 2012).

The research on the effect of climate variability on *armed conflicts* at the country level did not find strong relationships (Buhaug 2010; Ciccone 2011). Furthermore, the correlation between growth in rainfall and economic growth which played an important role in the initial paper that reported a relationship between climate variability and armed conflict (Miguel, Satyanath, and Sergenti 2004), has not been shown to be very strong in the last two decades (Miguel and Satyanath 2011).[1]

One possible reason for the zero finding for armed conflicts at country–year level is large casual heterogenity. *Where should we expect that the effect of adverse climatic exposure on the likelihood of armed conflict onsets is highest?* This article tests the effect of climate variability on the likelihood of armed conflict onsets for an arguably "most likely" and less heterogenous sample: regionally based and politically excluded ethnic groups. Two aspects make this sample "most likely". First, such groups are more environmentally vulnerable due to two mechanisms: *sons of the soil* and *ethnic favoritism* (Fearon and Laitin 2011; Wucherpfennig, Hunziker, and Cederman 2016; Kramon and Posner 2013). Second, ethnic identity and group–based grievances make mobilization easier for rebel groups with ethnic connections (Eck 2009; Cederman, Wimmer, and Min 2010). This study does not find a significant or substantial effect of climate variability on the likelihood of armed conflict onsets in this sample, and conforms to the findings of country–level studies.

Disaggregation introduce new methodological problems for the study of the relationship between climate variability and armed conflicts. There are two main problems. First, spatial matching of climate treatment and violent outcome is problematic because rebels in civil wars move around and are recruited from a large area of the country (Theisen, Holtermann, and Buhaug 2011; Raleigh and Kniveton 2012; Beardsley, Gleditsch, and Lo 2015). Second, violent events can occur all over the country as strategic responses to previous battles; thus, it is difficult to control for various forms of treatment interference (Harari and La Ferrara 2014; Weidmann 2015).

Clionadh Raleigh, who is directing the coding of the ACLED dataset, asserts that "it is exceedingly difficult to argue that a location, in and of itself, is an appropriate unit given the networks and movement of conflict over time" (Raleigh and Kniveton 2012, p.55). As a consequence, Raleigh

1. Miguel and Satyanath (2011, p.231) speculated that a reason for this change could be "related to Africa's unprecedented recent economic growth in the past decade in nonagricultural sectors, as well as public policy changes perhaps stemming from spreading democratization".

and Kniveton (2012) interpret the estimated effects of climate variability on rebel conflicts (ACLED armed conflict events) as only having to do with local tactical considerations, and not with issues of rebel motivation and recruitment, as originally argued in Miguel, Satyanath, and Sergenti (2004).

Take, for instance, the case of Boko–Haram. This group initiated a full insurgency after Nigerian security forces killed 17 Boko Haram members in a confrontation, and ransacked their hideout in Bauchi State. This led to a riot, and, eventually, the government killed the former Boko Haram leader, Mohammed Yusuf. The group then retreated to Borno state, in the nearby border areas around Lake Chad. From there, they launched attacks in cities far away from Borno State, such as Abuja and Kano (Agbiboa 2013, 2015).[2] The group has thus shown an ability to move around in large territories. Using spatial matching of climate variability and conflict events, we cannot know whether rainfall in areas significant to potential Boko Haram recruits or supporters and violent attacks initiated by Boko Haram are correctly matched.

The method used in Raleigh and Kniveton (2012) does not attempt to separate a motivational mechanism from the tactical mechanism. Although highly mobile rebels make local matching dubious, not all rebels are highly mobile. Sometimes, the estimated effects arise from changes in motivation, and other times to tactical considerations. They can also reflect bias due to poor matching of treatment and outcome (*attrition* (Gerber and Green 2012, p.211ff)), or failure to account for spatio–temporal dependencies (*treatment interference* (p.253ff)).[3] Such problems are pervasive in many of the disaggregated studies of climate variability and *armed conflicts* relying on spatial matching of climate exposure and conflict events, such as studies using ACLED (O'Loughlin et al. 2012; O'Loughlin, Linke, and Witmer 2014; Harari and La Ferrara 2014; Maystadt and Ecker 2014; Maystadt, Calderone, and You 2015) or state–based conflicts (Theisen, Holtermann, and Buhaug 2011; Detges 2016).

This study addresses these issues by studying the living areas of ethnic groups over time, and matching climate exposure and rebel activity to the ethnic group instead of spatially matching climate exposure and conflict events. Climate variability is calculated for the ethnic groups' main living areas and conflicts are matched to the ethnic groups using data on the relationship between rebel groups and ethnic groups (Wucherpfennig et al. 2012; Vogt et al. 2015). A similar matching approach is used in von

2. There is a distance of 580 km between Maiduguri (capital in Borno state) and Kano, 140 km from Maiduguri to Gambaru on the border to Cameroon (and 200 km to the border to Chad), and 840 km between Maiduguri and Abuja.

3. Theisen, Holtermann, and Buhaug (2011) also discuss the issue of mobility, but argue that the problem is less acute for onset data. Their evidence for this idea is anectodal, and they do not "disregard possible insecurity effects of droughts that work through national–level mechanisms and that might have nationwide implications" (p.90).

Uexkull et al. (2016). That study, however, has a flawed approach to causal identification.

2 Literature review

Miguel, Satyanath, and Sergenti (2004) argue that climate shocks can have an adverse effect on the private economy, which, in turn, can affect taxation–based state income. Adverse changes in the private economy would affect the individual opportunity cost of participation in rebellion (Collier and Hoeffler 2004), whereas losses to state income could reduce the ability of the state to pay for military expenditures, thus weakening the state vis-à-vis potential rebels (Fearon and Laitin 2003). The effect would be particularly strong for economies with a high share of laborers working in agriculture, and for economies mainly dependent on agricultural production.

The opportunity–cost theory is central to a large share of disaggregated studies of climate variability and conflict. Hidalgo et al. (2010) argue that droughts make landless workers in Brazil jobless, resulting in a higher likelihood that they will occupy private land for their own use. Maystadt and Ecker (2014, p.1158) test "an economic perspective and assume that opportunity costs determine conflict participation". O'Loughlin, Linke, and Witmer (2014) cite both Maystadt and Ecker (2014) and Miguel, Satyanath, and Sergenti (2004) as inspiring their study, although they also note that there are "complex linkages between political marginalization, government policing, and even criminal networks that determine conflict risk - in addition to evident weather anomalies" (O'Loughlin, Linke, and Witmer 2014, p.11712).

Detges (2016, p.699) takes a different theoretical angle, arguing that "regions with poorly developed road infrastructures are not only more vulnerable to the adverse effects of droughts, and hence more likely to see drought–stricken populations joining armed groups, but also more likely to see armed groups taking advantage of inaccessible terrain and generally weak involvement of the state". This argument, which rests mainly on assumptions about tactics, is also pursued in Raleigh and Kniveton (2012). Tactical issues are also discussed in Miguel, Satyanath, and Sergenti (2004), who (correctly) regard the tactical link between climate variability and violent conflict as a potential violation of the exclusion restriction when testing the economic mechanisms of climate shocks on armed conflict outcomes.

The relationship between short–term climatic variations and tactical issues of warfare is well known for certain areas of the world. For instance, "the fighting season" in Afghanistan - a pattern of large seasonal variation in both the number of conflict incidents (Eriksen and Heier 2009, p.66) and fatalities (Carter 2011, p.101) - is clear. The explanations for the seasonal variation in conflict intensity in Afghanistan are mainly two-fold: low temperatures and snow during the winter, conditions which reduce conflict, and the spring opium harvest, which pull labor away from the front line (Eriksen and Heier 2009; Carter 2011). Although the explanation for the winter lull takes motivation into consideration, more commonly, it rests on the tactical: "[Taliban] relies mainly upon motorbikes and other lightweight vehicles... [which] provide insurgents with a high operational tempo and a large amount of freedom of movement; factors often regarded as a prerequisite for surprise, local dominance, and protection against ISAF's rather static but overwhelming and accurate fire-power. Deep snow, however, severely breaks down the insurgents' mobility concept..." (Eriksen and Heier 2009, p.67).

It is important to distinguish in climate–conflict studies between things that make the local population more vulnerable to climate exposure, and things that make the exposed population more likely to be mobilized to violent action. Detges (2016), for instance, argues that poor water infrastructure not only makes people more vulnerable to droughts, but that it also can induce grievances against the government which failed to deliver the infrastructure.

The literature has distinguished between different conflict types partly because groups engaging in different types of conflict face varying challenges, including mobilization and environmental vulnerability. Fjelde and von Uexkull (2012) argue, for instance, that communal conflicts (non–state conflicts between informally organized groups) should be more likely than armed conflicts to be affected by climate variability, for several reasons. Groups that engage in communal conflicts tend to be more vulnerable to environmental changes, communal violence is a direct strategy to secure immediate economic goals and such violence requires less mobilization and capacity since it is mounted against other groups that are poorly organized (at least compared to a state military).

The assessment of the literature on the relation between climate and violent conflict in the fifth IPCC assessment report concludes that although "the impact of changes in climate on armed conflict is negligible", "[t]here is some agreement that either increased rainfall or decreased rainfall in resource–dependent economies enhances the risk of localized [communal] violent conflict" (Adger et al. 2014, p.772). It is likely that the differences between armed conflicts and communal conflicts in vulnerability and ability to transfer individual experiences into violent collective action explain the difference in effects.

Disaggregated studies of climate and conflict differ in suggested theoretical mechanisms or conflict types, but also in their causal inference strategies. In a methodological review, Dell, Jones, and Olken (2014) ask, "What Do We Learn from the Weather?" They make a case for the use of unit, time, and trend fixed–effects to ensure equal treatment propensities (i.e., a random treatment) when estimating the causal effects of weather shocks.

Study	Scope	Unit of observation	Outcome variable	Climate variable	Spatial panel control	Temporal panel control
Bohlken and Sergenti 2010*•	India, 1982-1995	FOA-Year	Riot count	Percentage change in rainfall	FOA fixed-effects	Year fixed-effects
Caruso et.al. 2016*•	Indonesia, 1993-2003	Province-Year	UNSFIR count	December deviation of rainfall and minimum temperature	Pooled	Pooled
Detges 2016•	SSA, 1990-2010	FOA-Year	State-based/communal GED incidence	Annualized SPI-6 (3 consecutive months) aggregated to FOA	Pooled + FOA random effects	Pooled
Fjelde and von Uexkull 2012•	SSA, 1990-2008	FOA-Year	Communal conflict GED incidence	Annual cell rainfall deviation aggregated to FOA	FOA fixed-effects	Pooled
Harari and La Ferrara 2014	Africa, 1997-2011	Cell-Year	ACLED incidence	Fraction of consecutive growing season months where SPEI was below its mean	Country fixed-effects	Year fixed-effects
Hidalgo et.al 2010	Brazil, 1988-2004	Municipality-Year	Number of land occupations	Standardized precipitation (station-month + municipality-year)	Municipality fixed effects	Year fixed-effects
Maystadt and Ecker 2014*	Somalia, 1997-2009	FOA-Month	ACLED count	By month temperature and rainfall deviation, drought length	FOA-month fixed-effects	Month-year fixed-effects
Maystadt et.al. 2015	Sudan and South Sudan, 1997-2009	Cell-Quarter	ACLED count	By quarter rainfall and temperature deviation	Cell fixed-effects	Quarter-year fixed-effects and linear trend
O'Loughlin et.al 2014•	SSA, 1980-2012	Cell-Month	ACLED count	SPI-6 and TI-6	Cell random effects	Pooled
Raleigh and Kniveton 2012•	East Africa, 1997-2010	Village-Month	ACLED count	By month rainfall deviation	Country fixed-effects	Pooled
Sarsons 2015*	India, 1961-1995	District-Year	Riot count and incidence	Annual sum of by month rainfall deviation, share of months with large by month rainfall deviation	District fixed effects	Year fixed-effects
Theisen 2012	Kenya, 1989-2004	Cell-Year	Communal conflict incidence	Annual cell rainfall deviation, annualized SPI-6, absolute temperature	Pooled	Year fixed-effects and linear trend
Theisen et.al. 2011•	Africa, 1960-2004	Cell-Year	Armed conflict onset location	Annualized SPI-6 (3 consecutive months)	Pooled + Country fixed-effects	Pooled
von Uexkull et.al. 2016•	Ethnic groups in Asia and Africa, 1989-2014	Ethnic group-Year	Group involvement in armed conflict onset	Share of growing season where SPEI-1 is below -1.	Ethnic group random effects	Linear trend

FOA:First-order administrative area, *:IV study, •:Observed controls

Table 1: Overview of disaggregated studies of climate variability and violent conflict

Similar arguments can be found in Burke, Hsiang, and Miguel (2015). As can be seen from Table 1, however, not all studies include such controls.

An alternative to using fixed–effects is to transform the treatment variable so that problematic variance is removed. The studies in Table 1 employ various operationalizations of the climate variable, ranging from deviations in rainfall, to a standardized temperature index and shares of growing season months where the Standardized Precipitation-Evapotranspiration Index over one month (which takes into account precipitation, temperature and sun hours) is below -1. Although there is variance, the methodological relation between the choice of panel controls and the operationalization of the climate variable is not clear. A review of the literature suggests that the choice of climate variable is often influenced by theoretical rather than methodological considerations.

Although the fixed–effects approach can solve most issues related to ensuring equal treatment propensities, other issues must also be solved in order to attain proper causal identification. Attrition, non–interference, excludability, and the Modifiable Areal Unit Problem are four additional issues (Gerber and Green 2012; Dark and Bram 2007). These issues are all defined and discussed in the Appendix (sections 4.2-4.5) to this article.

Buhaug (2015, p.270) observes "a disturbing disconnect between underlying theoretical arguments and the manner in which empirical analyses normally are carried out". We should expect this disconnect to affect the methodological quality of studies, particularly with respect to the five issues mentioned above.

There is a difficult balancing act, however, between proper causal inference and closer engagement with theories. On the one hand, there is the problem that control variables can remove theoretically relevant variance, possibly leading to "throwing out the baby with the bath water" (Beck and Katz 2001), or to what Dell, Jones, and Olken (2014) call "over–controlling". On the other hand, there is the problem of including theoretical relevant variance in ways that make it impossible to obtain unbiased causal estimates.

The core design idea in von Uexkull et al. (2016) – matching violent outcomes and climate exposure to regionally based ethnic groups – is similar to the one proposed in this article. The study is theoretically ambitious, and aims not only to estimate the casual effect of climate variability on the likelihood of onset for regionally based ethnic groups, but also to test how the causal effect of climate variability is mediated differently in divergent contexts such as areas predominantly inhabited by agriculturally dependent/independent populations, politically included/excluded ethnic groups, or areas with high/low economic development. Additionally, they choose to use an annualized SPEI-1 measurement of drought in the growing season of the main crop for the ethnic group, arguing that it better captures the theoretically relevant exposure.

The ambitious aim led to a less convincing methodology for causal inference. Particularly, their argument that the use of annualized SPEI-1 and random effects leads to unbiased causal inference is unpersuasive. Rather than arguing that their treatment should be regarded as random, they discuss a limited set of potential omitted variables such as ethnic group size, log GDP/capita, a linear time trend, and past conflicts. They do not discuss how these variables might affect climate treatment propensities. They interpret the estimated effects of both annualized SPEI-1 in growing season, and ethnic group status and their interactions as causal mediation effects, without discussing possible omitted–variable bias in both measurements or the difficulties that mediation adds to causal inference (Green, Ha, and Bullock 2010; Imai et al. 2011). This study aims to achieve a better balance between the theoretically relevant questions and sound causal inference methods.

3 Theory

A theory explaining a causal relationship between climate variability and violent collective action needs three elements: A theory of environmental vulnerability, a theory of individual motivation from environmental vulnerability, and a theory of collective action as a way to capitalize on climate variability affecting individuals.

In the social sciences, environmental vulnerability is understood broadly as susceptibility to adverse effects of environmental exposure (Adger 2006). The effects can be on private health, economy, infrastructure, ability to adapt to future exposure, or other elements relevant to social coping capacity.

The vulnerability of populations to local droughts or other environmental hazards varies. Vulnerability is a function of sensitivity to hazards and probability of exposure. Populations living in areas where they are more likely to experience extreme hazards, such as cyclones or earthquakes, are more vulnerable than populations living in areas without such hazards. Agricultural workers, who are directly dependent on favorable climatic conditions for their production output, can be expected to be more vulnerable than other populations (Miguel, Satyanath, and Sergenti 2004). Precipitation is an important predictor of yields in some areas of the world (Ray et al. 2015). Whereas droughts are considered more consequential than floods for crop yields (Porter et al. 2014, p.497), higher than normal precipitation is associated with higher yields in sub-Saharan Africa (Lobell, Schlenker, and Costa-Roberts 2011; Porter et al. 2014; Buhaug et al. 2015). Poor and uneducated agricultural workers are more vulnerable than rich and educated agricultural workers. Rich, educated workers can take actions to become less affected by hazards (e.g., they can irrigate, buy drought resistant crops, or provide shade and water to livestock), and can better handle damages if they do occur (e.g., they may have insurance, savings, or alternative sources of income). Precipitation is, for instance, less important in areas with irrigation (Porter et al. 2014, p.497). Wealthy and well managed societies can build infrastructure, institutions and knowledge, which may reduce their sensitivity to hazards (Ide et al. 2014).

Economic losses arising from climate variability due to environmental vulnerabilities are seen as increasing individual motivations for participation in violent groups, through the opportunity–cost mechanism (Miguel, Satyanath, and Sergenti 2004). It is not the case, however, that the likelihood of violent conflict increases just because individuals have fewer material goods, become sick, or in other ways become negatively exposed to environmental hazards. Collier and Hoeffler (2004) argue that rational individuals, when faced with a choice of whether to use time and energy in armed rebellion, will take into account the opportunity cost of doing so. Individuals who have high incomes, find their current job worthwhile for other reasons, or have alternatives which provide higher utility than does joining a rebellion, are less likely to choose to join (also see Blattman and Annan 2016). Joining a violent group also entails a risk of apprehension, punishment, or death. Even if rebel groups can provide higher immediate pecuniary rewards than what a potential recruit is currently receiving, the potential recruit may find that the risk outweighs the immediate benefits. States with strong counter–insurgency capacities create higher risks and threaten costlier punishments, reducing the benefits of joining a rebellion (Fearon and Laitin 2003; Collier, Hoeffler, and Rohner 2009; Blattman and Annan 2016).

Even in cases where opportunity costs are low, and the potential rewards of rebellion are high, potential rebels must solve the collective action problem (Olson 1971). A violent *organization* must be able to provide these potential rewards to those who participate; prevent free–riders from benefiting; identify, recruit, train and retain a steady stream of rebels; gather intelligence; solve logistical challenges; secure safe shelters, among other things (Guevara 1985; Tilly 1978; Gates 2002; Parkinson 2013). In essence, a theory of violent conflict cannot ignore issues related to *mobilization*.

Charles Tilly argues that if theories of violent conflict ignored how groups organize and mobilize into collective action, they will lead us to believe that "the faster and more extensive social change, the more widespread the anomic and restorative forms of collective action" (Tilly 1978, p.23). This is particularly relevant in climate–conflict studies. While costs incurred from climatic exposure can increase the likelihood of conflict, they can also erode the ability of potentially violent organizations to mount a credible rebellion against a state.

On the basis of this theoretical background, *where should we expect the effect of adverse climatic exposure on the likelihood of armed conflict onsets to be highest?* Since politically excluded ethnic groups are more likely to be environmentally vulnerable, to perceive the current government of the state as the problem, and to be socially endowed with an organizational capacity to mount a rebellion against the state despite material short–comings, the effect of adverse climatic exposure *should* be strongest for populations who are part of politically excluded ethnic groups. This article tests the hypothesis that *climate shocks in living areas of politically excluded ethnic groups affect the likelihood of the onset of armed conflict in which the exposed group is engaged.*

What is a politically excluded ethnic group? The Ethnic Power Relations dataset (EPR), used in this study, defines an ethnicity as "any subjectively experienced sense of commonality based on the belief in common ancestry and shared culture" (Cederman, Wimmer, and Min 2010, p.98). This inter–subjectively held belief uses ascriptive markers such as "common language, similar phenotypical features, adherence to the same faith, and so on" to identify members (p.99).

To understand political exclusion, a model of politics in a state is needed. EPR bases itself on Charles Tilly's polity model (1978). In that model, political groups seek access to power over the government, which is a set of institutions used to rule the state. Included groups are those that enjoy some political access and power over the government. The *polity* is the government plus all the included political groups. The included groups can use their power to provide privileges to themselves and to keep other political groups out of the polity. The groups that are kept outside of the polity are called politically excluded groups (Cederman, Wimmer, and Min 2010, p.92f).

This model is a simplification, and can be extended in many ways. For instance, political groups can be split into elites and ordinary members. This extension of the model opens up discussions such as how ordinary members can become elites (e.g. open competition versus limited access), or to what extent elites provides privileges to themselves as opposed to the whole group (North, Wallis, and Weingast 2009; North et al. 2012). Another extension which is done in the EPR dataset, is to classify group status in more detail than just whether or not a group is part of the polity. The EPR distinguishes between seven different kinds of group status: monopoly, dominant, senior partner, junior partner, powerless, discriminated and self–exclusion. The latter three are what the EPR uses to define excluded groups (Vogt et al. 2015, p.1331).

Ethnic groups are deemed to be politically relevant by the EPR "if at least one political organization claims to represent it in national politics or if its members are subjected to state–led political discrimination" (Cederman, Wimmer, and Min 2010). Politically excluded ethnic groups are therefore ethnic groups that are politically represented, but that do not currently hold any power in the state polity.

We should be most likely to observe an effect of climate variability on the likelihood of armed conflict onsets in politically excluded ethnic groups, because they are more environmentally vulnerable than politically included groups, and because *ethnic mobilization* and group–grievances make them easier to mobilize into a violent rebellion.

There are two mechanisms that make politically excluded ethnic groups are more environmentally vulnerable than included groups: *sons of the soil* and *ethnic favoritism*. "Sons of the soil" refers to ethnic groups that have lived in environmentally and economically favorable areas for a very long time. They are more likely to be part of the polity, than groups that live in areas less naturally endowed. The latter groups are more likely to be politically excluded. Fearon and Laitin (2011) argue that this is the case in Asia. In Africa, however, colonialism is likely to have made this relationship less obvious. Wucherpfennig, Hunziker, and Cederman (2016, p.886) argue that "peripheral ethnic groups were more likely to gain access to power in newly independent [British] states than comparable groups in former

French colonies", owing to the different strategies the colonizers used to rule their colonies. Politically excluded ethnic groups are more vulnerable to the vagaries of nature because they tend to live in areas of the country where resources are scarcer.

Ethnic favoritism is the idea that the state will distribute resources favorable to ethnic groups represented in the polity. Studies of ethnic favoritism show that states do not distribute resources in disfavor of excluded ethnic groups in all outcomes, but that favoritism can be found in different outcomes in different countries (Kramon and Posner 2013; Golden and Min 2013). Kramon and Posner (2013) used DHS survey data, and the outcomes they studied were educational attainment, access to water and electricity, and infant survival. Hodler and Raschky (2014) find that night–time lights (as a measurement of local wealth) increased more in the birth region of the current political leader than in other areas. Weidmann et al. (2016) find that living areas of politically excluded ethnic groups consistently get worse Internet coverage than other areas. Although there are observed differences across countries, there is ample evidence for ethnic favoritism.

Ethnic mobilization is when rebel groups rely on ethnic ties in their mobilization efforts (Eck 2009). Rebels relying on ethnic mobilization can more easily identify potential recruits because of the ascriptive nature of ethnicities. They are able to recruit more committed rebels because they can draw on social endowments and networks to make credible promises and offer non–pecuniary rewards for participation (Gates 2002; Weinstein 2006; Staniland 2014). Ethnically mobilized rebels are more difficult to co–opt because they risk retribution from co–ethnics. Since the rebels recruit along ethnic lines, violence can easily implicate the whole ethnic group in the fight, creating a security dilemma for the ethnic population, which, as a consequence, will find it difficult to remain neutral (Eck 2009).

While the security dilemma mechanism relies on the conflict having started, the other mechanisms should make violent conflicts more likely to start if the rebel group relies on ethnic mobilization. However, although the ethnic composition within a country is unrelated to the likelihood of the onset of armed conflicts (Fearon and Laitin 2003), countries with politically excluded ethnic groups are more likely to experience armed conflicts than are other countries (Cederman, Wimmer, and Min 2010; Cederman, Gleditsch, and Buhaug 2013). This empirical pattern can be an indication that, although ethnic mobilization may be necessary for this effect, it is not sufficient (Cederman, Gleditsch, and Wucherpfennig 2017).

Cederman, Wimmer, and Min (2010, p.94ff) distinguish between three different ways that ethnic groups can gain motivation to fight against the state: underrepresentation, exclusion, and downgrading. Given ethnic favoritism, underrepresentation and/or political exclusion can be assumed to have a negative affect on the material outcomes of ethnic groups. In addition, political entrepreneurs in such groups can frame the situation as unfair, and as evidence of ethnic favoritism or dominance by "others", for example. Perceptions of underrepresentation, or actual downgrading of political status, can stir fears of ethnic domination, anger and resentment.

Negative material outcomes of political exclusion can arise from increased environmental vulnerability. For instance, if the state has allowed farmers to settle on pasture–land used during droughts, the decision can cause pastoralists to experience anger and resentment toward the state (Benjaminsen and Ba 2009). The perception of rigid taxation schemes during hard times is another source of resentment against the government, as was observed, for instance, among Vietnamese smallholders during French rule (Scott 1976, p.93). The lack of representation in the state can easily become perceived as the problem when material losses are experiences as a result of environmental hazards, and gaining representation or control can thus be perceived as the solution. However, this direct link does not need to be there (or be acknowledged) for there to be a causal effect between climate variability and armed conflict as described above.

4 Research design

This study is designed to test whether exposure to climate variability in the living areas of politically excluded ethnic groups affects likelihood of the onset of armed conflict in which the exposed group takes part. It aims to calculate the average difference in onset probabilities for same group–months (for instance, Januaries between 1946 and 2013 in Maya) between months with high, normal, and low Standardized Precipitation Index (SPI) as estimated for the main living areas of the ethnic group. The final estimate is the average difference in onset probabilities for same group–months, averaged for all months and all groups. Causal identification is based on the argument that the probability of a given precipitation level for a given group–month (i.e. Maya living areas in January months) is the same between January 1946 and December 2013 (See the Appendix, section 4.1).

The dependent variable is whether or not an ethnic group was implicated in an armed conflict onset in a particular month or not. An observation in the dataset is at the group–month level. The data on armed conflict onsets is taken from the UCDP/PRIO Armed Conflict version 4-2015 dataset (ACD) (Gleditsch et al. 2002; Allansson, Melander, and Themnér 2017). That dataset's definition of an armed conflict is "a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths in a calendar year" (Themnér 2017).

The timing of onset months is coded using the the start date of an armed conflict related to the group, as long as there was no such coded armed conflict for the previous year. There are two start dates in the UCDP/PRIO ACD. The first (`Startdate`) denotes the first battle-related death in the conflict. The second (`Startdate2`) denotes when an episode reached 25 battle-related deaths in a year. The latter is used in the main analysis because the former is only coded for the first episode in a conflict (meaning that there are fewer onsets).

The UCDP/PRIO ACD does not code ethnic groups as conflict actors. Ethnic groups do not fight in conflicts. Rebel groups do. The ACD2EPR dataset is used to connect the main living areas of ethnic groups with the rebel groups in the UCDP/PRIO ACD (Wucherpfennig et al. 2012; Vogt et al. 2015). Rebel and ethnic groups are matched in the ACD2EPR based on whether the rebel group claims to represent the ethnic group, recruits from the ethnic group, or is supported by a majority of the ethnic group.

Although it is in principle possible for an ethnic group to be implicated in more than one armed conflict against the same state, the likelihood of this happening is small. To avoid treating months of an ongoing conflict as potential onset months, I discarded all group–months for which an ethnic group already has an associated rebel group fighting the government.

The causal (independent) variable in the study is a Standardized Precipitation Index (SPI), calculated according to on the monthly average precipitation measured in the main living areas of the ethnic group. The main living areas of ethnic groups are defined in the GeoEPR dataset (Vogt et al. 2015). From this dataset, only groups which are "regionally based" are selected and matched with the EPR dataset. Monthly average precipitation in the main living areas from January 1946 to December 2013 is calculated from GPCC v.7.0 0.5° grid resolution product (Schneider et al. 2015). I use all grid observations which intersect with the group polygon as basis for the aggregation.

SPI can be calculated over different time–periods. An SPI–1 is based on the monthly average precipitation, whereas an SPI–3 is based on a three–month interval average precipitation. There are substantial differences in the interpretation of SPI intervals. Crucially, they tend to pick up different types of droughts/floods. Whereas the shorter SPI intervals tend to reflect soil moisture conditions (e.g. dry soil, flooded soil), the larger (12+) intervals are more properly tied to stream flows, and reservoir and groundwater levels (National Drought Mitigation Center 2014). Further information about SPI may be found in the Appendix (section 4.1) of this study.

Only groups with at least one armed conflict onset between 1946 and 2013 were selected, because the study relies on within–group variation. Groups without variation in the dependent variable do not add information to the estimate, in any case. In Figure 1, the main living areas of the ethnic groups in the study are plotted. For a more detailed list of the ethnic groups included, see the Appendix (section 5.1) to this study.

The cost of limiting the study in this way is that considerable data is not

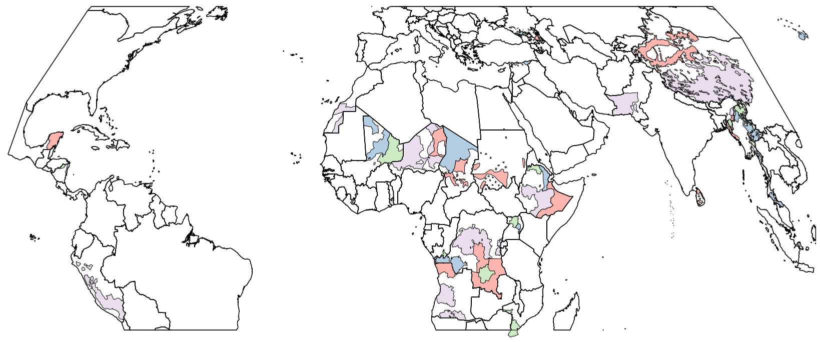


Figure 1: Study areas

used. However, a large share of the armed conflict onsets after WWII have been between a state and politically excluded ethnic groups; thus, splitting up the total sample of armed conflicts in other ways would likely result in eliminating even more data.[4]

4.1 Statistical model

Two dummy variables are defined from the SPI measurement: $drought = 1$ if $SPI <= -1.5$ and $flood = 1$ if $SPI >= 1.5$. $|SPI|$ of 1.5 or higher. "Drought" and "flood" should in this case be interpreted as unusually low or high precipitation measured in a specific area given the estimated distribution of precipitation in that specific area over the period 1946-2013.[5] Unusually wet and dry months are separated, since floods and droughts may produce different effects. Dummy variables are easily understood. I do not have to make any strong assumptions about the functional form of the relationship between the treatment and the outcome. However, the average effect of absolute SPI is also estimated. Using absolute SPI rather than the two dummies can be a more effective use of the data if the effect of floods and droughts are in the same direction.

4. Cederman, Wimmer, and Min (2010, p.101) found 124 ethnic conflicts between 1946 and 2005. The total number of internal armed conflicts recorded in the UCDP/PRIO ACD v.4-2016 is 207 (Melander, Pettersson, and Themner 2016). These conflicts are marked by 380 internal armed conflict episode onsets. I use about half of all ethnic conflicts, and 33% of all conflict onsets.

5. Since SPI is fitted to a Pearson III distribution (Vicente-Serrano, Beguería, and López-Moreno 2010, p.1702), we are more limited in how to interpret standard deviations than if it had been a normal distribution. Chebyshev's inequality shows that a minimum of $1 - 1/k^2 = 1 - 1/1.5^2 = 56\%$ of the distribution should be within ±1.5 standard deviations of the mean. It means that at least a majority of the months are considered normal months.

The model parameters are estimated using OLS. Since the assumption that OLS provides marginal effects when the outcome is binary hinges on some distributional assumptions (Chamberlain 1980; Angrist and Pischke 2008; Beck 2011), conditional logit models with clustered standard error are included in the Supplementary Materials as robustness checks. They yield similar results.

The base statistical model considered is

$$ac_{jt} = \alpha_{jm} + \beta_1 * drought_{jt-1} + \beta_2 * flood_{jt-1}, \tag{1}$$

where ac_{jt} measures whether or not the ethnic group j was involved in an armed conflict onset in month t. α_{jm} are fixed–effects for each ethnic group j for each of the 12 months of the year m. It is important to control for the different precipitation propensities in each group–month. (See the Supplementary Materials for a discussion.) Adding a fixed–effect for each group–month does this, assuming that the precipitation propensity in each group–month does not change over time.

Past armed conflicts are controlled for because temporal dependence could be a problem (See Supplementary Materials for the discussion). The control variable used is log time since last conflict incidence month. I take care to control only for past conflicts that happened before nature started to apply its treatment (i.e. t-4 for SPI-3). The three first polynomials are included to account for a non-linear relationship between the time since last conflict and onset of armed conflict. To code time since last conflict, it is assumed that it was 150 months since last conflict at the outset.

$$\begin{aligned} ac_{jt} = \alpha_{jm} + \beta_1 * drought_{jt-1} + \beta_2 * flood_{jt-1} \\ + log(timesince_{jt}) + log(timesince_{jt})^2 + log(timesince_{jt})^3 \end{aligned} \tag{2}$$

Assuming that the effects of floods and droughts go in the same direction, we can get rid of a degree of freedom by using absolute SPI (deviation from zero) instead of the dummy version. When using continuous treatments, the linearity assumption in the OLS model cannot be true for large changes in SPI (i.e. if probabilities go below 0). However, as long as we are interpreting results within small changes of SPI, it is defensible to use OLS.

$$\begin{aligned} ac_{jt} = \alpha_{jm} + \beta_3 * |SPI|_{jt-1} \\ + log(timesince_{jt}) + log(timesince_{jt})^2 + log(timesince_{jt})^3 \end{aligned} \tag{3}$$

In addition to these fixed effects models, a corresponding hierarchical model is estimated where normal distributed random intercepts rather than fixed intercepts are assumed.

$$\begin{aligned} ac_{jt} = \alpha_{jm} + \beta_3 * |SPI|_{jt-1} \\ + log(timesince_{jt}) + log(timesince_{jt})^2 + log(timesince_{jt})^3 \\ \alpha_{jm} \sim \mathcal{N}(0, \sigma^2_\alpha) \end{aligned} \tag{4}$$

The inference strategy is finally tested using a placebo test. Rather than using the causal treatment at time $t-1$, the placebo use the 24–month lead of SPI. A 24–month lead should be unrelated to the outcome variable for all SPI variables that is tested.

5 Results

Before interpreting the regressions, it is instructive to look at the cross-tabulations in Table 2. For the six–month drought case, for instance, we can see that .0079 of the months with a drought had a conflict onset, compared to the .0040 of the months without droughts. The causal effect in this case, (i.e., assuming blocks with equal probability of treatment and non-interference [*a pooled model*]), is $ITT = .0079 - .0040 = .0039$.

It is worth noting that there are few onsets, particularly in months after one–month and 24–month droughts. There are only 15 onsets after unusually dry six–month intervals. In the dataset, 1904 such intervals are observed. Fewer than 1% of unusually dry six–month intervals are followed by an armed conflict onset. King and Zeng (2001) argue that logistic regression with rare events data is likely to induce bias. However, the issue with rare events is not the share of events of the total, but rather the number of positive events. Although 128 events is not a lot, based on the discussion by Allison (2012), it is likely to be enough to avoid the issue. The issue also only arises in logit models, and here, OLS is used to estimate the marginal effects. Nevertheless, the aim of estimating the average intention–to–treat effect of exposure to such unusual weather on the probability of an armed conflict onset should be seen as ambitious. Further discussion on the intention–to–treat effect is in the the Appendix (section 4.3) to this article.

Figure 2 shows estimates for the four models defined in the last section. Best estimates are indicated by a point, 95% confidence intervals are shown with a solid line, and 99% confidence intervals are indicated by a dotted line (which should be followed to apply a Bonferroni correction)[6]. Regression tables for the four different models for the five different SPI intervals can be found in the Appendix (section 6) to this study.

It is important to notice that the placebo estimates are never significantly different from zero, but that the uncertainty of the estimates is quite large. If we take the variation of the placebo effect as an indication of what we should interpret as noise, then we need to be careful to make strong inferences about the average intention–to–treat effect using this design.

If we take the maximum highest estimate on the 99% confidence in-

6. In a Bonferroni correction, you divide the target p–value with the number of tests you have done. Since five different SPI measurements are tested (SPI1,3,6,12 and 24), it is possible to argue that the correct target p–value should be 0.05/5 = 0.01 (Gerber and Green 2012, p.300).

Conflict Onset		1 month drought 0	1	All
0	n	28056	1657	29714
	Percent	99.56	99.82	99.57
1	n	125	3	128
	Percent	0.44	0.18	0.43
All	n	28181	1660	29842
	Percent	100.00	100.00	100.00

Conflict Onset		1 month flood 0	1	All
0	n	27827	1886	29714
	Percent	99.60	99.21	99.57
1	n	113	15	128
	Percent	0.40	0.79	0.43
All	n	27940	1901	29842
	Percent	100.00	100.00	100.00

Conflict Onset		3 month drought 0	1	All
0	n	27830	1783	29714
	Percent	99.58	99.50	99.57
1	n	118	9	128
	Percent	0.42	0.50	0.43
All	n	27948	1792	29842
	Percent	100.00	100.00	100.00

Conflict Onset		3 month flood 0	1	All
0	n	27797	1816	29714
	Percent	99.59	99.29	99.57
1	n	114	13	128
	Percent	0.41	0.71	0.43
All	n	27911	1829	29842
	Percent	100.00	100.00	100.00

Conflict Onset		6 month drought 0	1	All
0	n	27574	1889	29714
	Percent	99.60	99.21	99.57
1	n	112	15	128
	Percent	0.40	0.79	0.43
All	n	27686	1904	29842
	Percent	100.00	100.00	100.00

Conflict Onset		6 month flood 0	1	All
0	n	27729	1734	29714
	Percent	99.58	99.43	99.57
1	n	117	10	128
	Percent	0.42	0.57	0.43
All	n	27846	1744	29842
	Percent	100.00	100.00	100.00

Conflict Onset		12 month drought 0	1	All
0	n	27274	1892	29714
	Percent	99.58	99.47	99.57
1	n	115	10	128
	Percent	0.42	0.53	0.43
All	n	27389	1902	29842
	Percent	100.00	100.00	100.00

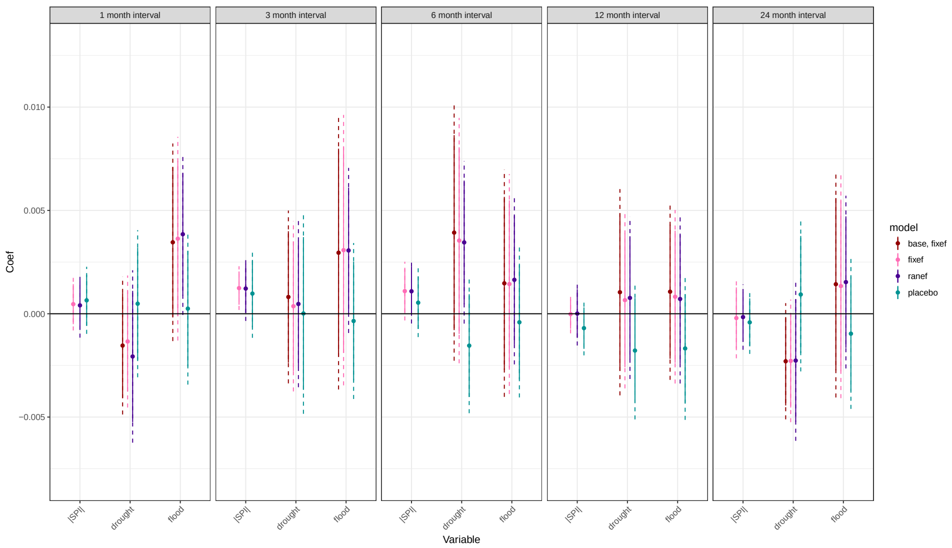
Conflict Onset		12 month flood 0	1	All
0	n	27441	1725	29714
	Percent	99.58	99.48	99.57
1	n	116	9	128
	Percent	0.42	0.52	0.43
All	n	27557	1734	29842
	Percent	100.00	100.00	100.00

Conflict Onset		24 month drought 0	1	All
0	n	26604	1972	29714
	Percent	99.55	99.80	99.57
1	n	120	4	128
	Percent	0.45	0.20	0.43
All	n	26724	1976	29842
	Percent	100.00	100.00	100.00

Conflict Onset		24 month flood 0	1	All
0	n	26902	1674	29714
	Percent	99.58	99.41	99.57
1	n	114	10	128
	Percent	0.42	0.59	0.43
All	n	27016	1684	29842
	Percent	100.00	100.00	100.00

Table 2: Cross-tabulations

Figure 2: Marginal intention–to–treat effects



terval as truth, and say that the ITT = .01, then the probability of a conflict would have been 1 percent points higher in the months that had normal climate, if the average precipitation rather had been unusual in the last 6 months. Since the average probability of an armed conflict onset is $p_{(c)onflict} = 128/29842 = 0.0043$, that implies a 230% increase in the probability. However, since approximately $p_{(u)nusual} = 1904/29842 = 6.4\%$ of the months in the dataset had unusual weather, the long–run change in probability due to this effect (assuming p_c is baseline) is 0.07 percentage points[7]. This equates to 19 more conflict onsets in the period from 1946 to 2013.[8] The highest best estimate yields seven more conflict onsets, whereas the lowest best estimate yields four fewer conflict onsets.

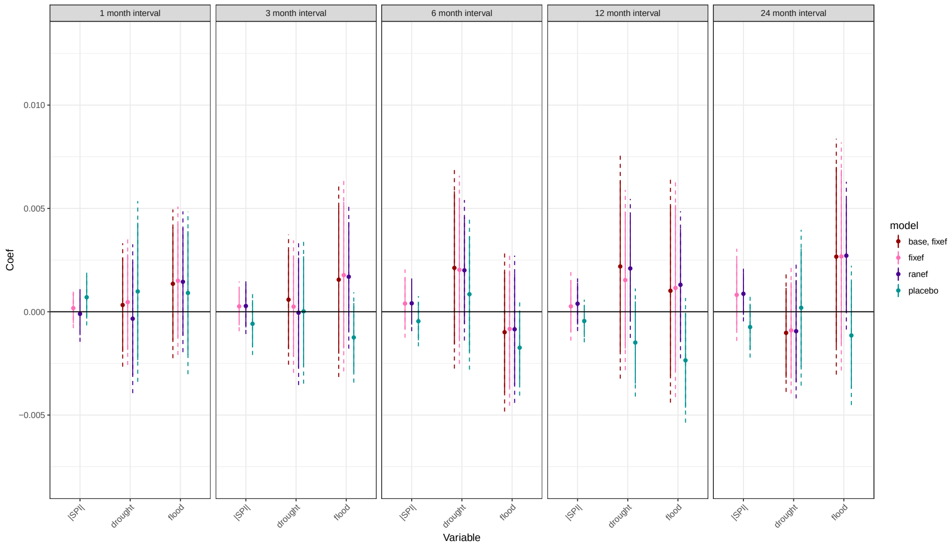
Since absolute SPI has the lowest uncertainty, and there are no obvious reasons to think that these models are more overconfident than the dummy models are, these estimates should be considered to be the most reasonable. The highest and lowest best estimates using absolute SPI are 0.0012 and -0.0002; this corresponds to 2.3 more and 0 fewer conflict onsets respectively, using the method above. The former of these estimates is the only one which is significantly different from zero.

The use of lagged conflict as a control variable appears consistently to pull the estimate down in almost all models. This may imply that controlling for past conflicts reduces bias. At the same time, this bias adjustment

7. $p_c * (1 - p_u) + (p_c + .01) * p_u - p_c = 0.00064$

8. $29842 * p_{after} - 29842 * p_c = 19.1$. One caveat here is that all group–months with an ongoing conflict are excluded. If we had seen more onsets, then there would be less opportunities to get a new one. The number should therefore be lower.

Figure 3: Marginal intention–to–treat effects, alt. startdate



is small compared to the uncertainty around the estimate.

The differences between fixed and random effects models using this design are only minor. This is mainly due to the fact that using this design, the variables included mainly have within-unit variance. The between-unit variance is very minor, as all units have at least one armed conflict onset whereas the maximum number of onsets is 6, and SPI is a standardized variable where the average is around zero. The choice between fixed and random effects would have been more consequential in models that have variables with more between-unit variance, such as in onset models where units without any onsets in the study period are included.

The results using the other definition of start date in the UCDP/PRIO Armed Conflict dataset offers some indication of how sensitive these estimates are. In Figure 3, `Startdate` is used rather than `Startdate2`. This means that rather than having the onset month be the first month that the number of battle-related deaths exceeds 25 during that year, the month the first death occurred in a conflict that eventually reached 25 BRDs during a year is used. None of these results are statistically significantly different from the estimates in the other model. There is an indication, however, that estimates are closer to zero with this specification.

6 Discussion

This article started with the hypothesis, although an average causal effect of climate shocks on armed conflict onsets in the global sample is not in evidence, the effect should be expected to be larger for a subsample of conflicts where politically excluded ethnic groups were implicated than for the global population. Rather than using data that spatially matches climate treatment with conflict events, this study attempts to correct for methodological problems by matching climate treatment with the living areas of ethnic groups, and matching ethnic groups to conflict actors.

The study finds no statistically significant effect of 1 to 24–month climate shocks on the likelihood of armed conflict. Since it is argued that the sample studied is a "most likely" case, this finding should render dubious strong claims about the effect between climate variability and armed conflict onsets. That being said, the statistical uncertainty is large enough that it is not possible to rule out completely the possibility that there can be an effect of some significance. The effects are comparable to those found in von Uexkull et al. (2016), and the study's finding does not change the conclusions found in Buhaug (2010).

The design used in this article emphasizes unbiased causal inference in preference to effective use of data. Since lack of data is a likely source of the uncertainty in the estimations, future studies should be advised to balance the design more toward effectivity and less toward reducing bias. Any such study should not validate its findings through null hypothesis significance testing, however. Rather, it should interpret precision intervals and validate causal claims using out–of–sample evaluation of predictive performance.

Climatic shocks might not have a strong effect on the likelihood of armed conflict onsets, but they do affect other types of inter–group conflicts in particular contexts (Hidalgo et al. 2010; Fjelde and von Uexkull 2012). This article argues that one possible reason for the discrepancy is that the demands for mobilization are much higher in armed conflicts, and such demands are not obviously easier to meet if a vulnerable society is being exposed to an extreme environmental hazard.

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4 Identifying the effect of climate variability on communal conflict through randomization

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Identifying the effect of climate variability on communal conflict through randomization

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Abstract In recent years, the focus of quantitative climate-conflict research has shifted from studying civil wars to studying different types of conflicts, particularly non-state and communal conflicts, based on the argument that these local-level conflicts are a more likely consequence of climate variability than civil war. However, the findings from previous research do not paint a consistent picture of the relationship between climate and communal conflict. We posit that a research design treating the climate variable as randomized is a better and more convincing strategy for estimating the relationship between climate variability and communal conflict compared to the conventional control method to account for confounders. In this paper, we ask two questions: (1) what type of research design allows us to treat climate variability as randomized and (2) what can we say about the relationship between climate variability and communal violence using this new design? To answer these questions, we analyze six large subnational areas, at a monthly time scale, and calculate the standardized precipitation index for each area for each month. We find that both short, unusually dry intervals and long, unusually wet intervals increase the likelihood of a communal conflict event.

1 Introduction

In recent years, the focus of quantitative climate-conflict research has shifted from studying civil wars to studying different types of conflicts—including non-state conflicts in general and communal conflicts in particular. It has been argued that climate variability is more likely to

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lead to communal conflicts than civil war because threshold to engage in a conflict with another group is lower than the threshold for challenging the state (Fjelde and von Uexkull 2012; Theisen 2012). Further, it is clearly very costly for any group to engage in a civil war because the government is likely to have a relatively strong military force (Hendrix and Salehyan 2012). Thus, weak groups, which are most likely to suffer from resource scarcity induced from unfavorable climate, should not be expected to start civil wars (Raleigh 2010). Rather, we should expect—if anything—that such groups fight other weak groups over scarce resources. However, based on the current status of the literature on climate variability and non-state/communal conflict, it is difficult to draw any conclusion about the relationship (Fjelde and von Uexkull 2012; Hendrix and Salehyan 2012; O'Loughlin et al. 2012; Landis 2014 and Raleigh and Kniveton 2012).

The lack of consistency in findings could be attributed to the large variety of research designs, using different units of analysis, study sites, measures of climate, and definitions of conflict. However, they all share the feature that they do not treat the climate variable as a randomized treatment variable, even though climate variables have been argued to have properties conducive to such treatment (Miguel et al. 2004). Instead, they use control variables to account for confounders to establish a causal relationship.

The innovation in this paper is that we explore how climate variability can be used as a randomized variable, and let this guide our design choices. We think this leads us to better and more principled design solutions, compared to a strategy of controlling for omitted variable, although these strategies try to solve the same problem (Angrist and Pischke 2008: 60ff, 80ff). In this paper, we ask two questions: (1) what type of research design allows us to treat climate variability as randomized and (2) what can we say about the relationship between climate variability and communal violence using this new design?

To address the first question, we create a research design were we arguably treat our climate variability variable as randomized, through analyzing six large sub-national areas, at a monthly time scale, calculating a standardized precipitation index for each area-month based on monthly average precipitation in those areas, and adding 12 fixed effects for each area (one for each month). Using such a research design answering the second question, we find that unusually dry short intervals increase the likelihood of a communal conflict event within an area, while short unusually wet intervals do not have the same effect for our sample. On the other hand, long unusually wet intervals increase the likelihood of a communal conflict event. While our research design is particularly suited for studying communal conflict, we argue that our substantial conclusion could have broader implications and be relevant for other types of violence such as civil wars.

In the following, we first we review the literature on communal conflict and climate variability; second, we discuss how climate variability can be treated as a randomized variable and present our research design and data. Finally, we discuss our findings.

2 Communal conflict and climate variability

The literature argues that climate change will to lead to conflict through scarcity of resources in interaction with social, political, and economic factors (e.g., Homer-Dixon 1994). Supply-induced scarcity may worsen people's livelihoods by affecting their access to food and water. Thus, in countries where people are dependent on income from agriculture or herding, the potential for climate-induced conflict, such as conflict over grazing land and water sources is considered high. While the bulk of the climate and conflict literature focuses on civil war, some studies argue that climate-induced conflicts are more likely to be in the form of local communal conflict.

First, communal conflicts and non-state conflicts are more frequent than civil wars and the threshold to engage in a conflict with another group is lower than the threshold for challenging the state, due to costs, the relative strength toward the government, and the relative distance to other communal groups (Fjelde and von Uexkull 2012; Theisen 2012; Hendrix and Salehyan 2012; Raleigh 2010). Further, communal conflicts are arguably also more likely to be motivated by climate variability than other types of non-state conflicts, such as conflicts between highly organized rebel groups. Having a highly organized rebel organization requires extensive resources. Poor and marginalized groups, which are more likely to suffer from the consequences of climate variability, often do not have such resources. Hence, they are unlikely to engage in fighting with highly organized rebel groups in order to ease their resource scarcity, but rather less organized communal groups. Several studies have tested this relationship, but currently there is not enough evidence to make any claims about the relationship.

Witsenburg and Adano (2009) found there to be more violent incidents related to cattle raiding in Northern Kenya during times of abundant rainfall. They noted that the environment in years with abundant rainfall makes raiding easier; for instance, there is better access to water during raiding trips. In contrast, in times of scarce rainfall and consequently scarce resources, the pastoralists do not have the resources to spend on raiding.

Studying sub-Saharan Africa, Fjelde and von Uexkull (2012) found, using communal conflict from the UCDP non-state conflict dataset, that exceptionally dry years are positively correlated with communal conflicts, thereby supporting the environmental scarcity thesis, which predicts that droughts may lead to resource scarcity, which in turn may lead to conflict. Fjelde and von Uexkull (2012) also found some evidence to support their hypothesis that areas characterized by political and economic marginalization see a higher risk for communal violence than other areas in exceptionally dry years.

Using a different conflict dataset (Social Conflict in Africa Database), Hendrix and Salehyan (2012) found that all types of social conflict are more likely to occur both in abnormally dry and wet years, thus partly supporting the findings by Fjelde and von Uexkull (2012). In a global sample study, Salehyan and Hendrix (2014) found that political violence, including non-state conflicts when measured separately, was more frequent at times when resources were abundant.

Studying East Africa, O'Loughlin et al. (2012) found that abnormally wet years are more peaceful than years with average rainfall, whereas abnormally dry years are not related to conflict. These results contrast with the continent-wide studies by Hendrix and Salehyan (2012) and Fjelde and von Uexkull (2012). Moreover, O'Loughlin et al. (2012) also used temperature as a climate variable and found that violence occurs more in warm years than in years with average temperature. The study by O'Loughlin et al. (2014) confirms these results for temperature, finding that high temperature extremes increased the risk of conflict in sub-Saharan Africa in 1980–2009. When studying seasonal changes in weather, Landis (2014) found that prolonged periods of warm weather are positively associated with both civil war and non-state conflicts.

Contrary to O'Loughlin et al. (2012), Raleigh and Kniveton (2012) found that violent events connected to civil wars and communal conflict in East Africa are more frequent both in times of drought and in times of excessive rain. Nevertheless, civil war events are most likely during droughts, whereas communal conflict events are most likely in times of abundant rainfall.

The current literature is characterized by inconsistent results, which may be related to a wide variety of research designs. In particular, we think that trying to solve the causal inference problem by controlling for confounders to ensure conditional independence is problematic in these studies and likely a source of the varying results. We suggest an alternative causal inference strategy, where our research design arguably can treat climate variability as a randomized variable.

3 Causal identification strategy

The potential outcomes definition of causality says that a causal effect is the difference between an actual outcome, and how it would have been if and only if the causal agent had been different in some specific way (Gerber and Green 2012). While this effect is fundamentally unobservable, it can be shown that the *average* causal effect between larger groups can be estimated. One identification strategy with a proven track record is randomized experiments. The randomization process ensures that the expected average outcomes between groups decided by the randomized variable are equal. Any differences in outcomes must be attributed to the difference in treatment in these groups.

Experiments are commonly designed by the researcher. However, it is also possible to find naturally occurring experiments or naturally occurring processes that can arguably be treated as experiments (see Gerber and Green 2012:15ff). Nonetheless, regardless of what kind of experiment you have, the same criteria must be fulfilled. Most important of these is a randomized treatment assignment: Assignment must be *stochastic* and with *known probabilities*. While you generally would know these two criteria to be correct when designing your own experiment, in any natural experiment, we must somehow discover these properties.

Previous literature has tried to identify a causal effect of climate variability of conflict through controlling for (un)observables through a panel structure. However, we believe that a more promising identification strategy is the randomized experiment, as it is based on a much simpler statistical procedure (in principle no need for controls), and relies more on a sound research design. Our central argument is that precipitation, treated in a particular way, can be seen as a randomized treatment. Further, we describe how we get to the naturally occurring randomization process and discuss the two additional core assumptions in experiments: non-interference and excludability.

3.1 Randomization of treatment

The definition of a randomized treatment is that treatment is assigned with a known probability. The problem with experiments where nature randomizes and assigns treatment is that we do not know the probability of treatment assignment. Thus, we need to approach natural experiments through finding processes where the treatment can be argued to be assigned with equal probabilities. With the case of precipitation, it is clear that the probability distribution of precipitation varies across space and over different times of the year. It rains more some months of the year than others and more in some areas than in others. It is only for specific area-month sets (e.g., Darfur, in January between 1948 and 2013) that it is convincing to assume equal probability of precipitation.

Following this assumption, we could do an experiment, using monthly average precipitation within one area as our causal variable. However, a problem arises when we try to combine that experiment with experiments from other area-months. To find an unbiased causal estimate in such an analysis, we have to know the probability distribution from which precipitation was assigned for each area-month and incorporate each such distribution into our analysis.

The Standardized Precipitation Index (SPI) tries to fix this exact problem (McKee et al. 1993; Guttman 1999). SPI is estimated through fitting the probability distribution of precipitation for a given area-month and then standardizes that fit (with expected mean 0 and a standard deviation of 1). The SPI should be interpreted as the normality of observing the given precipitation in that area for that month. Large negative numbers mean it was abnormally dry, and vice versa for large positive numbers.

In principle, if we select −1.5 SPI or lower to be our treatment assignment variable, then the probability of treatment should be expected to be equal for all area-months, meaning that we could treat all area-months as a large pooled experiment. However, SPI normalization is not perfect; some area-months are vastly more likely to have an SPI of −1.5 than others. For instance, in a very dry area such as Darfur, where the normal outcome during the driest months of the year is near zero precipitation, large negative SPI values for those months are not observed at all because they are not possible. Therefore, even after standardization, we need to take the blocked randomization into account when doing our analysis. We do it through adding area-month fixed effects.

Fjelde and von Uexkull (2012) also use SPI and employ an administrative unit-year design. However, rather than estimating SPI for each unit-year, they apply the annualized discrete coding used in PRIO-GRID (Tollefsen et al. 2012). This means that they have no means to correctly control for different treatment probabilities, which should be expected to lead to biased results.

Our research design needs 12 fixed effects for each areal unit; hence, we need to ensure that the probability of observing a conflict event within each unit is fairly large. Therefore, rather than using administrative units, or grid-cells, we identified six spatially segregated areas.

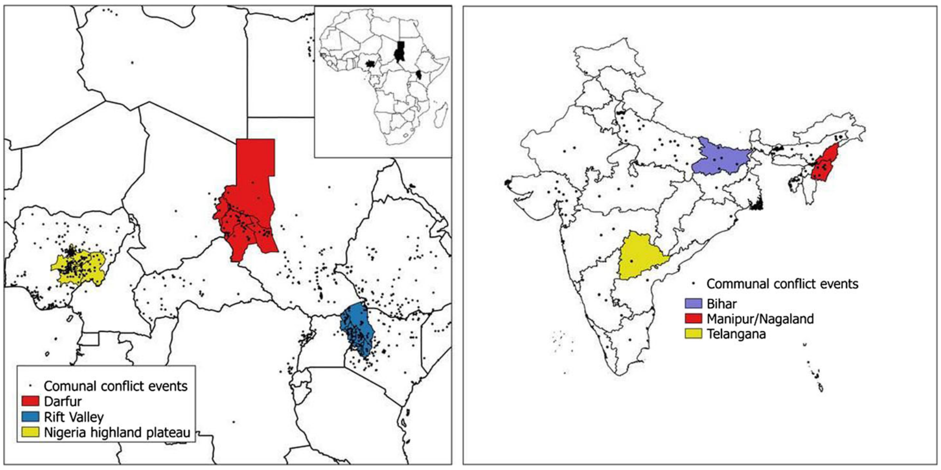
The areas we selected are by no means a random sample, rather we have selected areas were we believe we might observe the hypothesized effect. All the areas had a large number of communal conflict events between 1989 and 2013, and the livelihoods in these areas are likely to be affected by climate variability. Since we did not select randomly, we must be cautious about generalizing. Our areas are the Nigeria highland plateau, Rift Valley in Kenya including neighboring areas in Uganda, Darfur in Sudan, and three areas in India (Telangana, Bihar, and Manipur/Nagaland) (Map 1).

3.2 Non-interference

Non-interference is the assumption that the treatment or outcome of another unit is independent of the treatment of any other unit (Gerber and Green 2012: 43f).

A popular approach in the climate-conflict literature has been to use grid-cells or administrative units and add lags to control for spatial dependencies (e.g., Fjelde and von Uexkull 2012). However, this type of control does not fix the problem that treatment is being applied non-randomly over a larger area. By treating each grid-cell as independent, the estimate of the standard error will be lower than it should. The modelling solution to this would be to somehow cluster cells depending on the monthly weather patterns. Another solution is to add fixed effects on each area-month, as we already do to ensure a randomized treatment assignment.

Rather than modelling spatial dependence, we have selected areas that contain whole conflicts. Our assumption here is that communal conflicts are local phenomena, between



Map 1 The areas of analysis as well as the spread of communal violence

relatively stationary groups. By increasing the area size and selecting areas containing whole groups and conflicts, we reduce the likelihood that outside interference affects the outcomes.

Another source of interference in our setup is temporal dependence. It might be that past conflicts that partly were a result of droughts increase the likelihood of subsequent conflicts. If this is the case, we will overestimate the effect of the treatment. A problem with our setup is that the treatment is applied over a longer period of time (1, 3, 6, 12, and 24 months). If we control for conflict events that happened in the period after treatment is being applied, we might inadvertently control for outcomes that happened due to the treatment. Our solution is to control for conflict events before treatment is being applied (2 months before measuring conflict events when using 1-month SPI [since we measure outcome 1 month after the last treatment month], 4 months before when using 3-month SPI, etc.). The control we use is $\log(n+1)$ of the number of events (n) that happened in the 6- and 12-month periods before starting treatment.

3.3 Excludability

Excludability refers to the point that random assignment should only affect the outcome through the assigned treatment, and not something else in addition (Gerber and Green 2012: 39ff). To discuss excludability, we must be clear about what the treatment is. While the treatment assignment variable (Z) is the SPI, our treatment of interest is not SPI, rather it is scarcity of resources, lost revenues, and lost livelihoods that may result from the extreme weather (X). One problem arising from this is "non-compliance," simply the point that some individuals who are assigned to unusually dry weather "fail" to lose their livelihoods or vice versa. "Non-compliance" should be expected to bias the estimate of the average treatment effect of X on communal conflict toward zero.

Further, we cannot exclude the possibility that the government or NGOs provide assistance whose intensity is correlated with the treatment assignment variable (Z). We can imagine that the effect of SPI on conflict approaches zero if the government has sound insurance policies for all farmers who lose their income and investments under floods and droughts, if food prices are subsidized when prices surge due to droughts, and if markets are well connected so that the needed resources are available to the population. This scenario would bias our estimate toward zero as assistance is increased.

Another scenario is that both droughts and floods change the terrain, which could make fighting more or less difficult (Miguel et al. 2004). If roads are impossible to travel due to floods, then the likelihood of conflicts might go down. However, floods could make it more difficult to pursue attackers with heavy weaponry such as government forces, possibly making floods a good cover to initiate a raid.

We have no way of controlling for these influences in our current setup. Thus, what we estimate is the so-called intention-to-treat effect (ITT) (Gerber and Green 2012: 138f). The ITT effect is the causal effect of experiencing unusual precipitation on the likelihood of a communal conflict. Not being able to estimate the average treatment effect of resource scarcity, however, means that we must be careful in our interpretation of the ITT. In the following section, we describe the data and the statistical interference method that we will use to test the relationship between communal conflict and climate variability.

4 Data

Our outcome variable is geo-coded communal conflict events between 1989 and 2013 by combining the UCDP GED and the UCDP non-state conflict datasets (Sundberg and Melander 2013; Sundberg et al. 2012). The definition of a communal conflict is "conflict [stands] along lines of communal identity," where communal identity is defined as "[g]roups that share a common identification along ethnic, clan, religious, national or tribal lines. These are not groups that are permanently organized for combat, but who at times organize themselves along said lines to engage in fighting" (Pettersson 2014).

These events are then aggregated into the six chosen sets of administrative areas geographically defined in the GADM v.2.5 database of global administrative areas. Conflict events that span more than 1 month are coded in all those months. Our outcome variable is coded whether (1) or not (0) at least one communal conflict event in a given month intersects with the area of interest.

We get our treatment assignment variable by calculating the SPI from the average monthly precipitation in the areas we study. The precipitation measurement we use is the GPCC v.7.0 0.5° grid resolution product (Schneider et al. 2015). This data is based on station gauges and interpolated between stations. To calculate SPI, we use the SPEI R package with standard options (Beguería and Vicente-Serrano 2013). We include all months from 1948 to 2014 when estimating the SPI.

From the SPI, we define three different treatment assignment categories: "drought" (SPI ≤ -1.5), normal climate (SPI between -1.5 and 1.5), and "flood" (SPI ≥ 1.5). A drought should here be understood as unusually dry climate compared to the precipitation in that area for that month or interval of months (e.g., an unusually dry January compared to normal precipitation in January).

SPI can be defined for different temporal intervals. Rather than fitting a distribution for the set of January months, we could fit a distribution for the sum of precipitation for January, December, and November, for all years. Called SPI-3, this 3-month interval is calculated for each month and measures whether the precipitation the last 3 months is unusual compared to that same interval of months in other years.

Different SPI intervals are relevant because they measure different types of "floods" and "droughts." Below normal rainfall in the short term can lead to the soil drying up and crop stress. Below normal rainfall over a whole year might not materialize as immediate crop stress, but could reduce stream flows and reservoir levels. It is possible that any given area is both exposed to a short-term drought and a long-term flood at the same time. We test a variety of SPI time scales (1, 3, 6, 12, and 24 months) to capture these related-but-different phenomena.

To some degree, it is also possible to interpret the different SPI intervals as capturing the sustained versus the short-term effect of unusually dry or wet weather. Floods might have a more immediate effect than droughts and thus affect conflict in the short term (Raleigh and Kniveton 2012; von Uexkull 2014). However, longer SPI intervals do not necessarily capture short-term floods or droughts. Sustained large floods might better be operationalized through measuring consecutive months of high SPI-1, rather than, e.g., SPI-12. Within this research design, however, that kind of effect must be regarded as a possible interference problem, rather than something we can estimate the causal effect of. The reason is that while we can have confidence that the probability of some level of precipitation in a specific place in January is equal across time, we do not know whether the probability of having consecutive droughts in January to April is comparable across time and space.

5 Statistical inference

Since we cannot exclude other treatments from happening given our treatment assignment, and since the population will to a varying extent take the treatment if assigned (e.g., lose their livelihoods in a flood), we are estimating the average intention-to-treat effect, not the average treatment effect. We run OLS with fixed effects for each area-month, dummies for the two treatment conditions (holding normal weather as the base) as independent causal variables, and communal conflict as outcome.

$$cc_{it} \sim \beta_1 * \text{drought}_{it} + \beta_2 * \text{flood}_{it} + a_i * \text{area-month}_i$$

There is an ongoing discussion whether OLS give just as good estimates of marginal effects when the outcome is binary, as nonlinear models such as logit or probit (Beck 2011). The benefit of using OLS is that we get the marginal effects that we are interested in directly, rather than having to calculate these from the output of the nonlinear model (Angrist and Pischke 2008, 107). However, since Beck (2011) shows that the Angrist and Pischke's "folk theorem" only is true under specific circumstances, we also estimate conditional logit models to check for the robustness of this assumption, and the results hold (see Supplementary Materials).

Because the core assumptions could be wrong (especially problematic is temporal dependence of past treatment), we control for the log sum of number of events in the area before treatment started in the last 6 and 12 months in separate regressions.

$$\begin{gathered}
\text{ttime} = 1,\ 3,\ 6,\ 12,\ 24 \\
\text{eventcount}_{it} = \text{sum}\left(\text{ccevents}_{(it-\text{ttime}-6,\, it-\text{ttime}-1)}\right) \\
cc_{it} \sim \beta_1 * \text{drought}_{it-1} + \beta_2 * \text{flood}_{it-1} + a_i * \text{area-month}_i + c_1 * \log\left(\text{eventcount}_{it} + 1\right) \\
\text{ttime} = 1,\ 3,\ 6,\ 12,\ 24 \\
\text{eventcount}_{it} = \text{sum}\left(\text{ccevents}_{(it-\text{ttime}-12,\, it-\text{ttime}-1)}\right) \\
cc_{it} \sim \beta_1 * \text{drought}_{it-1} + \beta_2 * \text{flood}_{\text{it}-1} + a_i * \text{area-month}_i + c_2 * \log\left(\text{eventcount}_{it} + 1\right)
\end{gathered}$$

In addition to the dummy treatment, we also consider the more parsimonious model using absolute SPI. The assumption here is that the direction of the average effect of unusual weather is the same whether it is unusually wet or unusually dry.

$$\begin{gathered}\text{ttime} = 1,\ 3,\ 6,\ 12,\ 24\\ \text{eventcount}_{it} = \text{sum}\left(\text{ccevents}_{(it-\text{ttime}-6,it-\text{ttime}-1)}\right)\\ cc_{it} \sim \beta_3 * |\,\text{SPI}\,|_{it-1} + \ a_i * \text{area-month}_i + c_1 * \log(\text{eventcount}_{it} + 1)\end{gathered}$$

We test the null hypothesis that the intention-to-treat effect is zero through normal approximation using the *t* test. We consider the two-tailed test appropriate because we consider both negative and positive deviations from zero theoretically interesting. We reject the null hypothesis at $p < 0.05$ level of significance.[1]

Table 1 shows the results for the four equations using the five different SPI intervals. In the uppermost section, we show the model without controlling for previous conflicts. In the next two, we control for log of the number of past events during the last 6 and 12 months respectively. In the lowermost section, we have swapped the dichotomous measures of drought and flood with absolute SPI, and control for log of the past 6 month events. All regressions have fixed effects for each specific area-month. There are 20 different regressions in the table. For all drought intervals except the 24-month interval, the best estimate is above zero. Unusually dry climate the last three months is estimated to increase the likelihood of conflict with 10 percentage points in the study areas in the model without controls. Only the estimate for 3- and 6-month droughts is statistically significant. Unusually wet climate over short periods does not increase the likelihood of communal conflicts, but unusually wet years and two-year periods are estimated to increase the likelihood of communal conflicts with 6 and 7 percentage points respectively.

When we control for previous conflict events, the estimates for droughts approach zero—more so when using the last 6 months (before treatment starts) control than when using the last 12 months control. However, the best estimate is still positive for the drought intervals 1 and 12 and significantly so for 3- and 6-month intervals.

The estimates for unusually wet yearly and 2-year periods stay similar to the base model, and the 2-year interval is statistically significant. One reason for this can be that the previous conflict control is less effective, as we cannot control for conflicts happening in the last year and 2 years, respectively, due to the long exposure time. From R^2, it is evident that the model where we are controlling for communal conflicts closer in time to the outcome is a better fit to the data, than when controlling for communal conflicts longer back in time.

Since the direction of effects for both flood and drought are theorized to be the same, and we also see it in our estimates, we calculate a more parsimonious model with absolute SPI as treatment. One increase in absolute SPI-6 or SPI-12 increases the probability of a communal conflict event in a month with 4%. This latter has a *p* value of 0.0065, meaning that even after Bonferroni correction for testing five different SPI treatments, it is still significant.

Since we argue that SPI is a randomized variable, *future* SPI should be independent of past conflict events. A placebo test can be a way to test the validity of the causal model. We calculated the same models as in Table 1, only that all SPI values were shifted 24 months into the future. The full results are shown in the Supplementary Materials. We find that the effects

[1] Since we are testing five different but related treatments, there is an argument that we must adjust our target *p* value accordingly. A conservative approach is to do a Bonferroni correction, which gives us a new target *p* value of $0.05/5 = 0.01$.

Table 1 Regressions (fixed effects not shown, but included in all models)

	1 month	3 months	6 months	12 months	24 months
(Intercept)	0.54***	0.54***	0.53***	0.54***	0.54***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Drought	0.07	0.10*	0.11**	0.07	−0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Flood	−0.00	−0.00	0.03	0.06	0.07*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)
R^2	0.19	0.19	0.19	0.19	0.19
Adj. R^2	0.15	0.15	0.15	0.15	0.15
Num. obs.	1728	1728	1728	1728	1728
RMSE	0.36	0.36	0.36	0.36	0.36
(Intercept)	0.23**	0.30***	0.36***	0.44***	0.46***
	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
Drought	0.04	0.07*	0.07*	0.05	−0.03
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)
Flood	−0.02	−0.01	0.02	0.07*	0.09**
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
Log(# past 6-month events)	0.20***	0.17***	0.12***	0.07***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
R^2	0.39	0.33	0.26	0.22	0.21
Adj. R^2	0.37	0.30	0.23	0.18	0.17
Num. obs.	1698	1698	1698	1698	1698
RMSE	0.32	0.33	0.35	0.36	0.36
(Intercept)	0.27***	0.29***	0.32***	0.43***	0.47***
	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
Drought	0.06	0.08*	0.09*	0.05	−0.03
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Flood	−0.01	−0.00	0.02	0.07	0.10**
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Log(# past 12-month events)	0.15***	0.12***	0.09***	0.05***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
R^2	0.35	0.30	0.26	0.22	0.22
Adj. R^2	0.32	0.26	0.22	0.19	0.18
Num. obs.	1662	1662	1662	1662	1662
RMSE	0.33	0.34	0.35	0.36	0.36
(Intercept)	0.28***	0.29***	0.32***	0.43***	0.48***
	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
|SPI|	−0.01	0.02	0.04*	0.04**	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log(# past 6-month events)	0.20***	0.16***	0.12***	0.07***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
R^2	0.39	0.33	0.26	0.22	0.21
Adj. R^2	0.37	0.30	0.23	0.18	0.17
Num. obs.	1698	1698	1698	1698	1698
RMSE	0.32	0.33	0.35	0.36	0.36

$^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

are estimated to be not different from zero for all operationalizations, except for the 12-month drought specifications. There are two reasonable points to make. First, barely significant results are expected to happen once in a while even while the average effect is zero. Thus, we do not think that randomization has failed, based on the placebo test. Second, while our results show that the estimated effect is significantly different from zero, the results are for the most part not significantly different from a world in which the effects of drought, flood, and |SPI| were not significantly different from zero.

6 Discussion: causal inference

The core problem in quantitative causal inference is to find sets of units that work as each other's counter-factual, suitable for comparison. In statistics, there are in principle many ways to get such a setup. However, the gold standard is randomized experiment. In this paper, we argue that we can compare conflict outcomes in months within area-month sets, randomized into groups through the SPI. We can combine several such experiments as long as we take into account differing probabilities of being assigned treatment in the different area-month sets.

The setup has several advantages compared to the traditional control setup. There is a simple rule for when identification is successful, compared to control setup, where there is always room for another potential confounder. We can compare our results with results from a placebo test. And perhaps most important, all the parts that go into the identification process are transparently laid out in the research design, rather than being sealed within a multivariate regression scheme.

There are some obvious short-comings of naturally occurring experiments. Most important is that not all research questions come with a convenient and relevant naturally occurring randomized treatment, and climate variability is a case in point. The causal effect of absolute rainfall or temperature cannot be determined through naturally occurring experiments because they are not randomized. We suggest that one of the few such randomized variables is SPI compared across months within area-month sets. Thus, there is a limit to the types of causal effects we can identify from this approach.

The other shortcoming is that it is still doubtful whether we manage to properly identify the stochastic process that determines average monthly precipitation. This is the core argument our results are resting on, and proof that this identification is flawed would undermine our results. However, this problem is true for all naturally occurring experiments. We think the placebo test supports the core argument and that we actually are making fair comparisons.

Results seem to be biased due to temporal dependence. Past conflicts that partly were a result of droughts increase the likelihood of subsequent conflicts. Controlling for the number of past events in the last 6 months moves the effect closer to zero. There is a possibility that an even better control variable can bring the average effect lower. We think the gain of doing so, however, would be minimal as there would still need to be *some* effect in order for treatment interference through temporal dependence to be a problem.

Another possible interference problem comes from being affected by neighboring conflicts affected by droughts. In this study, we have taken a pragmatic design choice, by selecting quite large areas which contain whole communal conflicts and groups living within these areas. We think this choice has removed the need to control for spatial interference problems.

Further, communal conflicts are more local and static than state-based conflicts, which often move across the country. We would therefore not recommend using this research design when studying state-based conflicts. An alternative for sub-country studies of state-based conflicts is to match climate variability with the living areas of conflict actors, rather than spatially matching with the conflict event itself (Nordkvelle 2016; von Uexkull et al. 2016).

Before interpreting the effect as substantially being due to scarcity of resources, lost revenues and/or lost livelihoods, we would need to exclude several other theories, such as varying levels of humanitarian assistance and tactical considerations. The long-term "floods," where we get a significant effect, are not especially likely to affect infrastructure, as this is not a situation where water is necessarily coming quickly in large amounts. It might be, however, that the effects of shorter term floods would have been higher, if it had not also been the case that such floods made it more difficult to travel. Humanitarian assistance is also expected to reduce the effect of droughts and floods toward zero. Therefore, we believe that the estimates we find are there *despite* any humanitarian efforts in the areas we studied.

7 Discussion: substantial results

The results show that both droughts and floods increase the likelihood of communal conflict events in the areas we investigated. There seem to be qualitative differences between the two, however. Unusually wet 1- to 6-month periods are estimated to have zero effect on communal conflicts. Unusually wet 12- to 24-month periods, on the other hand, do seem to have an effect. The converse seems to be true for droughts, where long-term droughts have no effect, but short-term droughts do—which partly supports Fjelde and von Uexkull (2012).

This result could give support to both resource abundance and resource scarcity theories. Possibly, there needs to be some resource abundance for communal conflicts to "pay off" (long, unusually wet periods), while droughts that destroy harvests (short, unusually dry periods) pull resource-scarce groups into violence.

It is important to notice that the prevalence of droughts as we have defined them will not increase in the future. Therefore, these results cannot be used to argue that we will observe more or less communal conflicts in the future. If the future will see more extreme weather, it means that extreme weather will become more usual, making such weather having a lower SPI. However, from a theoretical perspective, we can argue that to the extent climate change puts strains on the ability to adapt to unusual weather, or creates more unusual weather before we manage to adapt, we should expect more communal conflict events in the study areas due to climate change.

There is not a clear link between the result for communal conflicts and that for state-based conflicts (civil wars). While the results indicate that changes to the resource base do affect the willingness of marginalized communal groups to use violence against other marginalized groups, it is not obvious that they would increasingly choose to target the government. The government is a much stronger adversary, the risk is higher, and the end goal of the violence is usually quite different.

At the same time, some of the most convincing empirical tests of the economic causal mechanisms of why people use violence, such as the opportunity cost mechanism, can be found in studies of non-state conflicts. Since communal conflicts are more locally situated, methodological issues such as treatment interference should be less of a problem than in state-based conflicts. Studies of communal conflict can therefore be used to motivate theoretical explanations of violence even in the civil war setting, perhaps particularly when studying local dynamics of civil war (Kalyvas 2006). However, we should be careful to directly extrapolate causal effects of climate variability from the communal conflict setting to the civil war setting.

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5 Climate variability and individual motivations for participating in political violence

Climate variability and individual motivations for participating in political violence

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Abstract

Climate shocks are argued to affect the likelihood of conflict through individual economic outcomes which changes individual opportunity costs of participation in violent activities. Studies testing this proposition, however, either fail to answer convincingly because they use aggregated data, because they rely on observed controls and strong assumptions about variable independence, or because their study sample is restricted to particular conflicts. This article uses two rounds in Afrobarometer where respondents were asked about participation in politically motivated violence as well as how their living conditions had changed in the last year. To get around endogeneity problems, perceived changes in living conditions are instrumented using a standardized precipitation-evapotranspiration index (SPEI). The study finds that participation in violence would, on average, have been more likely if an individual experienced deterioration of living conditions than if it had not, in the subpopulation in Africa whose living conditions are affected by droughts.

Individual opportunity costs are the most cited causal link between climate shocks and violent intergroup conflict outcomes (Miguel, Satyanath, and Sergenti 2004; Hidalgo et al. 2010; Fjelde and von Uexkull 2012; Maystadt and Ecker 2014; O'Loughlin, Linke, and Witmer 2014; Harari and La Ferrara 2014; Salehyan and Hendrix 2014; Burke, Hsiang, and Miguel 2015). However, to date, none have made empirical estimates of the effect size at the individual level. *Are the same individuals who experience economic losses due to climate shocks more likely to participate in political violence than if they had not experienced such losses?*

Miguel, Satyanath, and Sergenti (2004) instrumented growth in GDP/capita using growth in annual rainfall, and theorized that the interpretation of this variable could affect both opportunity cost (individual motivation) and state capacity (group opportunity) mechanisms (Fearon and Laitin 2003; Collier and Hoeffler 2004). The first theory suggests that the individuals who experience economic losses will be more motivated to join violent groups than if they had not experienced such losses. The second theory argues that aggregate losses will make states less able to prevent violent groups from mobilizing and take action. The individuals who experience direct losses from climate exposure in this theory are not necessarily implicated in the violence. Since there are alternative theories which do not expect an individual level effect, it is reasonable to ask whether individually endured economic losses from climate shocks translate into increased motivation to participate in politically motivated violence.

Although many other studies have investigated individual level motivations for participation in conflict (see e.g. Verwimp 2005; Oyefusi 2008; Humphreys and Weinstein 2008; McDoom 2013; Claassen 2014; Blair et al. 2013), very few have tested whether marginal changes at the individual level affect the likelihood of participation (Blattman and Annan 2016), and none have done this in the context of climate shocks or outside of country–specific contexts. This article offers an instrumental variables approach, based on survey data and local measurements of drought, to obtain variation in and isolate motivational aspects of participation in violent conflict. It finds that some drought specifications had both a strong effect on reported changes in living conditions in Africa in two Afrobarometer survey rounds, and a substantial and statistical significant complier average causal effect of reported changes in living conditions on the likelihood of reported participation in political violence.

Individual motivational theories are not necessarily the most obvious explanation for why a given conflict happened, but they do matter for thinking about whether policy interventions aimed at affecting individual motivation structures, such as demobilization, disarmament, and reintegration programs (Blattman and Annan 2016), are useful. This study directly addresses whether individually experienced losses due to environmental damages can be expected to affect individuals' willingness to participate in violence. The result indicates that programs which directly help those exposed to environmental damages (such as governmental compensation schemes) can reduce the ability of violent organizations to recruit new members. Policymakers should not lose sight of the structural basis that gives rise to such conflicts in the first place, however.

1 Individual and collective theories of conflict

Collier and Hoeffler (2004) compare an explanation of rebellion with the investigation of a murder: both require an exploration of motive and opportunity. Both earlier and more recent research have pointed out how information problems (Fearon 1995), commitment problems (Walter 2009), coercion (Andvig and Gates 2010; Eck 2014) and the dynamics of conflict (Kalyvas 2006; Wood 2008a; Staniland 2014) constrain and challenge the

Table 1: Motivation and opportunity at different levels of analysis

	Individual level	Group level
Motivation	Opportunity costs; Non-pecuniary preferences for participation in violence (relative deprivation, defiance, pleasure of agency, etc.)	Grievances; leadership; identity; etc.
Opportunity	Obstacles for being recruited; social networks; skills; trust-ability; etc.	Obstacles for mobilization/recruitment; counter-insurgency; state capacity/reach; rebel group finances; recruiter social capital; safe-havens; etc.

notion of conflicts and participation in violence as the result of calculated plans. Yet, the intentional model of conflict is central in our understanding of violent conflicts.

The distinction that Collier and Hoeffler (2004) makes between "greed" (or feasibility) and "grievance" is not a distinction between motive and opportunity, however. At the individual level, both "greed" and "grievances" describe motivations. At the group level, such motivations can become opportunities. Without specifying the level of analysis, motives and opportunities can be difficult to differentiate. Collier and Hoeffler (2004) mainly think of opportunity costs (the "greed" mechanism) as something modulating the rebel group opportunities for recruitment, as in the case of sudden inflows of cash or the opportunity to do a heist. For instance, they write that "the incidence of rebellion is not explained by motive, but by the atypical circumstances that generate profitable opportunities" (p.564).

Although opportunity costs can be viewed as a group–level opportunity factor, it is more proper to think of opportunity costs as primarily modulating the individual level motivation for participation. The reason for this is that there are individual level opportunity factors that can affect individual participation; such individual opportunity factors also affect rebel group opportunities, possibly negating the effect of increased individual motivations on recruitment. Rebel groups can, for instance, be under pressure from counter–insurgency measures, which force rebel groups to recruit only those they can trust. It can be costly and time–consuming to recruit under this trust dilemma (Hegghammer 2013). When a violent organization can operate without significant opposition, membership can be a privilege (Le Billon 2003; North, Wallis, and Weingast 2009; Wood 2010). In neither of these instances, i.e., when rebel supply is abundant, is it necessarily the case that the opportunities for rebel organizations improve when opportunity costs fall. In Table 1, I have made an non–exhaustive list that indicates how aspects discussed in conflict theories might fit into a two–by–two table of motivation and opportunity at the individual and group level.

When Collier and Hoeffler (2004) connected opportunity costs with "greed", they also tied the opportunity–cost theory to material gains. However, opportunity costs can also be non–pecuniary. Eriksen and Heier (2009, p.67) argue, for instance, that a reason for the reduced intensity of fighting in Afghanistan during winter is that "[locals] are sitting around their stoves and they try to live as comfortable as possible". The alternative cost to fighting in the cold of winter is, among other things, to be inside where it is warm and comfortable. The opportunity–cost theory is a smaller part of a more general utility framework for thinking about individual motivations for actions. Anything that increases the utility of continuing to do something at the expense - pecuniary or otherwise - of other activities increases opportunity costs.

Not all individual wishes or grievances motivate individual action. For instance, although someone may want a change in government, that person's individual participation might only change marginally the likelihood that this wish will be realized, at the expense of a risk of apprehension given your participation (Olson 1971). Rational actors can, therefore, find themselves in situations where they would like a change that require some people to participate, but they do not see the benefit of participating themselves. This is what Olson (1971) calls *the collective action problem*; his solution was somehow to provide *selective incentives* - that is, incentives conditional on participation. Subsequent studies have shown how a wide range of pecuniary and non-pecuniary benefits can be given, and are given, by violent *organizations* (Grossman 1991; Lichbach 1995; Gates 2002; Weinstein 2006) or arise from the social dynamics of political strife and conflict (Petersen 2002; Wood 2003a). This article relies on the theory that individual preferences and motivations are determined by a broader set of deliberations than pecuniary opportunity costs, but offers, at the same time, a simple and stylized model of human agency (Blattman and Annan 2016).

As Miguel, Satyanath, and Sergenti (2004) argue, climate shocks can be seen to affect the likelihood of conflict by affecting individual opportunity costs, as well as by affecting the relative capacity and opportunities of violent groups. Group–level explanations are therefore an alternative or supplement to understanding the results between climate variability and violent conflict outcomes at the aggregate level.

Indeed, the contemporary literature on resource scarcity and violent conflict emphasizes structural conditions. Fjelde and von Uexkull (2012), for instance, find that the effect of climate variability on the likelihood of communal conflict events is higher for politically excluded groups than for included groups, whereas the effect is similar in impoverished areas and richer areas (see also von Uexkull et al. 2016). They argue that politically excluded groups have fewer strategies available to them when exposed to increased resource scarcities, and therefore end up taking violent action more often.

Lack of state capacity and income are said not only to affect coercive capacities, but also capacities to provide the population with important services (Fjelde and De Soysa 2009). Low state capacity and corruption also play important roles in non–state (communal) conflicts, which is the conflict type in which climate variability effects are found to be the strongest (Fjelde and von Uexkull 2012; Elfversson 2015; Rudolfsen 2017). Benjaminsen and Ba (2009) and Benjaminsen et al. (2012) document how political vacuums, poorly executed or biased government policies, and rent–seeking/corruption among government officials form the core of farmer–herder conflicts in the Sahel, conflicts which are commonly portrayed as due to droughts and changes in opportunity costs. Similarly, Sneyd, Legwegoh, and Fraser (2013) show that, whereas international media portrayed poverty and hunger as the factors which linked the 2008 food price rises and riots, local media discussed a more complex set of factors linked, in particular, to citizen dissatisfaction and lack of access to power.

Studies which survey *who fights* can give some indication of opportunity and motivation patterns at both individual and group level. Verwimp (2005) traced households that were part of a survey before the Rwandan genocide to analyze the background characteristics of perpetrators in comparison with other characterizations such as innocent, victim, thief, and protector. He found that men who earned a high percentage of income from off–farm activities and who rented land for cultivation were more likely to be perpetrators (p.317). These men were not working the poorest soils and they did not have lower than average marginal labor productivity, however. Rather, Verwimp (2005, p.319) argues that the group of quasi–landless could expect to gain from participation — "perpetrators were very interested in the property of the murdered Tutsi."

Social networks also played an important role in deciding who would be perpetrators and who would not, according to another survey of Rwanda, this one by McDoom (2013). Perpetrators and non–perpetrators lived in clusters, McDoom argues. "Participation then may be as much the product of social interaction as of individual agency" (p.453).

Christopher Claassen's survey of residents in Alexandra, South Africa also seems to show that social interactions and group–level dynamics play a role in who fights (Claassen 2014). He found that participants in communal riots in the township were 34 years old on average, "more likely to support an opposition party, attend community policing meetings and have a high–school education" (p.1).

Blair et al. (2013) did not survey participants, but rather the support for militants among the urban population in Pakistan. They found that the poor, urban Pakistanis disliked the militants *more* than did the middle–class. A likely explanation for this, they argue, is that the urban poor are more exposed to the externalities of militant violence than other groups.

Oyefusi (2008) did a non–representative survey of 1337 individuals in the Niger Delta in Nigeria to explain participation in rebel violence there. He found large, significant partial correlations for questions that, he argues, tapped into both "greed" and "grievance" motivations, as well as feasibility. However, interpretation becomes difficult, as the variables in the regression are not independent of each other, and the same variable can measure different types of motivations. Oyefusi's conclusion is that group–level grievances cannot, by themselves, explain participation, and that both individual–level factors such as income and educational attainment, and communal level factors that create opportunities for illicit profit, also need to be addressed.

Humphreys and Weinstein (2008) surveyed post–combatants in the civil war in Sierra Leone, dividing their rationales for participation into three categories: grievances, expectation of selective incentives for participation, and social cohesion. They found that all these factors were salient in explaining participation. The study is unique in that it managed to separate abductees from volunteers. However, the control approach that was applied to test individual causal effects leaves much to be desired, as many of the variables of interest can hardly be said to be independent of each other.

All of the above studies show that a simple expectation that those who have less are more likely to fight does not find support in empirical data. Social dynamics, networks and group–level phenomena structure the behavior of individuals, who, themselves, are capable of individual deliberation about their own situation now and in the future, as well as about whether violence is to their gain or detriment. These studies do not tell us, however, whether a marginal change in incomes, brought about by a climate shock, can increase the likelihood of participation in political violence.

A study with an arguably more convincing causal design is Blattman and Annan (2016). Using a randomized field experiment, they evaluated a program aimed at drawing Liberian ex–fighters out of illicit work and into agriculture. They found that giving these high risk individuals both training in agriculture and initial capital inputs reduced the number of hours they were involved in illicit activities and increased the hours they engaged in farm labor. The study is a direct test of whether pecuniary incentives affect individual motivations for participating in illicit activities. They note, however, that "[a]rmed social networks, ethnic solidarity, and grievances undoubtedly influenced men's interest in mercenary work. And these drivers may have played an even larger role in a less opportunistic conflict than the Ivorian one" (p.16). For climate–conflict studies, in which mechanisms going through changes to individual motivations are commonly posited, it is particularly relevant to test the size of this effect in a sample larger than that used by Blattman and Annan.

Qualitative studies of motivational aspects in climate–related conflicts create the expectation that individual–level motivations should have a noticeable effect on participation. One example is the studies of the Turkana–Pokot conflict in north–western Kenya, where violent livestock raiding is an integrated part of the economy, and where a *tit-for-tat* strategy gone wrong has been employed for as long participants can remember (McCabe 2004; Eaton 2008; Schilling, Opiyo, and Scheffran 2012; Linke et al. 2015). From focus group interviews in north–western Kenya, Schilling, Opiyo, and Scheffran (2012) found that accumulation of wealth, poverty, hunger, and payments of dowry were mentioned as important motivations.

2 Theoretical expectations

This article closely follows the formal model laid out in Blattman and Annan (2016, p.7), which they describe as a "classic occupational choice of crime model, except with home production and the potential for (dis)utility over illicit labor". The only difference is that this article assumes different models for an urban worker and a rural worker. The formal model is used because it provides an elegant and concise description of an individual–level intentional–actor model that is sufficient for creating the expectations that are tested in this article.

The model for rural workers assume that they can allocate their time between legal agricultural production L^a, illicit wage labor L^v and leisure l. The gain from agriculture follows a classic production function and a price p_t for those outputs, where output is a function of productivity θ, labor L_t^a and capital inputs K_{t-1}, $p_tF(\theta, L_t^a, K_{t-1})$. Agricultural labor is assumed to give diminishing returns to output.

The expected gain from illicit wage labor is a function of the wage w_t^v, labor input L_t^v, and a risk of future punishment. Blattman and Annan assume here that the punishment f and probability of arrest ρ is linear to the illicit labor input in the last period. The total expected earning in the rural case is therefore $y_t^u \equiv p_tF(\theta_t, L_t^a, K_{t-1}) + w_tL_t^v - \rho f L_{t-1}^v$.[1]

In the urban case, the worker has the choice among leisure, legal wage labor L^l and illicit wage labor. Because legal wage labor can be constrained in supply, it is assumed that there can be diminishing returns for such labor. The total expected earning is $y_t^u \equiv F(w_t^l L_t^l) + w_t^v L_t^v - \rho f L_{t-1}^v$.

1. An important omission in this model is that the cost of apprehension will also be related to the income from legal labor. The model does not punish the ability to provide labor input into labor in the next period, regardless of punishment. This omission is only relevant for the broader discussion about "who fights", and not about "are they more likely to fight given a shock to the system", however.

Individuals are assumed to have the utility function $U(c, l, \sigma L^v)$. A person has some preferences for consumption c and leisure l, but also for doing illicit work. When σ is negative, the person gains negative utility from working illicitly. This negative utility must be balanced by the positive utility from monetary gains that can be used for consumption. The model also allows for positive σ, meaning that the person can get pleasure from participation in violent illicit activity in itself, apart from the monetary gains. σ is a way to capture non-wage motivations for participation in violence, such as feelings of "fighting the good fight", revenge, or defiance (Gates 2002; Wood 2003a).

A person's labor preferences can be thought to be affected due to the instrument through three different parameters in the rural case: that period's productivity θ_t, the last period's capital inputs K_{t-1}, or a change for the preference of illicit labor σ_t. In the urban case, labor preferences can be affected through consumption preferences c and preferences toward illicit work σ.

The climate can affect living conditions through several pathways. The most important are arguably changes in crop yields, availability of (clean) drinking water, sufficient pasture for livestock, conditions for vector-borne diseases and flood damages (Porter et al. 2014; FAO 2016; Delpla et al. 2009; Smith et al. 2014; Thornton, Boone, and Ramirez-Villegas 2015; Vicente-Serrano, Beguería, and López-Moreno 2010). A simple narrative for how rural worker's preference for illicit work could increase is that agricultural productivity θ_t goes down due to unfavorable climatic conditions. Assuming that market prices are not perfectly elastic to local supply, the expected gain from agricultural labor goes down. For those in the population whose gain follow the above model, it means that more time will be allocated to illicit wage labor such as participation in violent action, all else being equal.

In a given area, when the average rural worker's experience is lower agricultural productivity, there could be some who are not affected. Assuming that market prices are at least partially elastic to local supply, the expected outcome for such deviant farmers will be an increase in the expected gain from agricultural labor and less time used for illicit wage labor. However, this outcome can only be true for a minority of farmers, and should therefore not drive our expectations. This scenario is also not a given, since farmers seldom are self-sufficient when it comes to food. Depending on the level of consumption at market prices, and how different food stuffs are affected, their gain could be mitigated by increased costs. Indeed, most poor producers in sub-Saharan Africa experience a net loss of income from higher food prices (Wodon and Zaman 2010).

Another narrative is that rural producers lose capital inputs, or income to buy capital inputs in the previous period K_{t-1}. For instance, a failed crop can lead a farmer to not getting the required seeds for the next season, or a goat that was going to produce more goats next season had to be slaughtered for food or money. As in the case of reduced productivity, assuming imperfect local supply elasticity, more time will be allocated to illicit wage labor.

When economic losses affect vital consumption patterns, we should expect that the will to work to increase at the expense of leisure time. Drawing on studies of the psychology of loss and loss aversion, it could be argued that individuals may even overcompensate for the experienced loss by working more and taking less leisure time (Novemsky and Kahneman 2005). In the cases in which individuals compensate for losses by engaging in less leisure, it is not obvious that they would report worsened living conditions (although having less leisure time could be considered a decline in living conditions). Such adaptive behavior can therefore attenuate estimates toward zero.

In the case of urban workers, changes to their income as a direct result of changes in productivity or capital inputs due to local climate variations are not generally observed. Such variation is only observed if market prices are elastic to local supply, which could affect consumption c. A potential alternative for consumers is to switch their diet to other, possibly unaffected food–stuffs. Additionally, while farming is always suffering from diminishing returns of labor, urban workers can, in principle, simply seek more lawful work L^l at the expense of leisure. Only when there are diminishing returns to legal labor (e.g., when the only available extra work pays less) will there be a clear economic reason for switching labor from legal to illicit work.

There are many ways in which σ can be affected. Some such examples are discussed below.

Experiments have shown that humans emphasize direct experience over observational data (Simonsohn et al. 2008). Although we can know that a policy is wrong, biased, or nonexistent, we may not necessarily act on that knowledge before we ourselves are exposed to the consequences. Experiencing economic loss from environmental damages which a person knew could have been prevented through some previous intervention could, therefore, lead to increased anger or resentment, thereby increasing σ.

Technologies available today can mitigate effects of temporary extreme climatic conditions. Irrigation, drought resistant seeds, access and knowledge about fertilizer usage and meteorological forecasts are all examples of such technologies. In many cases, governments take partial responsibility for the spread of such technologies, and the lack of access to these technologies can be perceived as a form of deprivation over which grievances can arise. To the extent that violence is perceived to address these grievances, it can make individuals more favorable towards its use, thereby increasing σ.

Pastoralists and farmers can come increasingly into conflict during droughts because areas suitable for pasture shrink. In some cases, this means that pastoralists have to move their livestock to areas occupied by farmers. In such situations, both farmers and pastoralists can have reasons to be more favorable towards the use of violence. Farmers may wish for justice or revenge. Pastoralists may think that farmers have unjustly encroached on territories they need for pasture, and/or that government policies about land–use are unfair.

It is also possible to reduce σ. For instance, in bad times, people may feel the need to stick together and cooperate rather than fight over material resources. Linke et al. (2015) and Linke et al. (2017) find that respondents in communities with formal and informal institutions that provide norms of behavior during droughts are less accepting of the use of violence and endorsed ethnic community militias to a less extent than communities that did not have such institutions.

Based on this theory, we should expect rural workers to become more willing to join illicit violent groups after experiencing economic losses from environmental damages unless the potential pecuniary utility to be gained is offset by a similar reduction in the non-pecuniary utility of participating in violent acts. We should not in general expect urban workers to change their working preferences because they are experiencing local droughts, however. First, it is not obvious that they would endure economic losses, and second, even if they do, it is not obvious that they would be more inclined to take up illicit work.

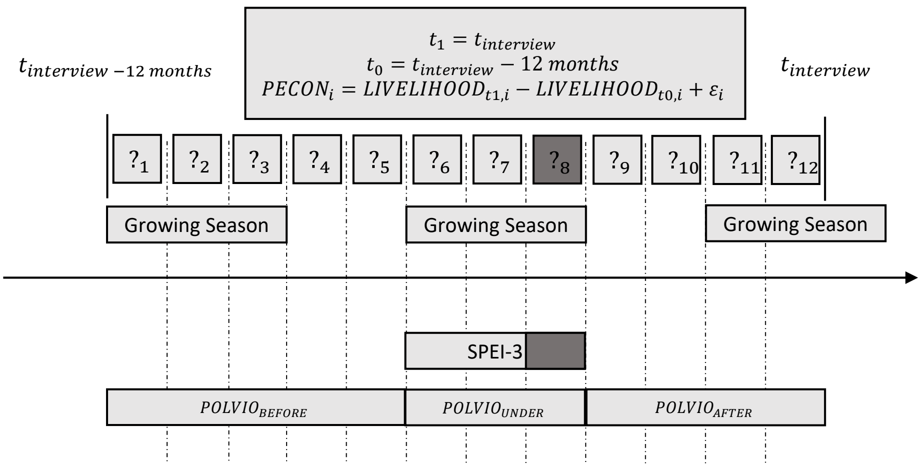
The model for this article is made for a very particular context, and it abstracts away important details. For instance, it assumes that all individuals see information about the possibility of work for violent organizations and that they are approached by these organizations. In the real world, not all will have knowledge about the possibility for such work, and many individuals have such a low σ that they do not regard such incomes as serious alternatives to lawful work. As with Blattman and Annan (2016), this model is mainly relevant to persons at high risk of joining illicit violent groups to begin with, or who are already working for violent groups.

3 Research Design

To test the theory, the article uses two survey items in Afrobarometer that tap into changes in personal living conditions (PECON) and participation in political violence (POLVIO) (Afrobarometer Data 2016)[2]. Since respondents are asked to compare their current living conditions at the time of the survey with what they were one year previously, the core idea of the paper is to instrument reported changes in personal living conditions through exogenous events in the 12 month period before the interview, and then estimate the casual effect of the instrumented PECON variable on the likeli-

2. PECON is called q3b in round 2, and Q5B in round 5, whereas POLVIO is called q25e in round 2, and Q26E in round 5.

Figure 1: Conceptual design



The figure depicts the conceptual design in this article. The main question is which of the months – $?_{1-12}$ in the 12 month period before the interview – should be chosen to instrument perceived changes of livelihood between t_1 and t_0 using SPEI. The SPEI interval length and timing can be varied, and it can be matched with growing seasons. A concern is that the outcome can be measured before the treatment ($POLVIO_{BEFORE}$).

hood of reported participation in political violence. Specifically, the article argues that a standardized precipitation-evapotranspiration index (SPEI) can act as such an instrument.

Figure 1 shows the conceptual research design of this paper. It is constructed around the time–line ending with the date of Afrobarometer interviews, and going back in time 12 months. It shows some of the choices that can be made when constructing the instrument. They will be discussed below.

The dependent variable in the study is the response to a survey question about participation in political violence (POLVIO). The exact wording of the question is: "Here is a list of actions that people sometimes take as citizens. For each of these, please tell me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance: Used force or violence for a political cause." The respondent can choose between seven different answers: "No, would never do this", "No, but would do if had the chance", "Yes, once or twice", "Yes, several times", "Yes, often", "Don't know" and "Refused to answer".

The question can capture a wide range of activities. "Using force *or violence*" means that respondents could report non–violent uses of force, for instance non–violent protest (e.g., a Greenpeace activist chaining herself). A reasonable interpretation, however, is that the question implies illicit acts, outside of participation in a legal protest or strike. Another aspect of the question which can be interpreted in different ways is "for a political cause". A broad interpretation assumes that actions were directed against other groups, and not (only) specific individuals. But there could be a tendency to think of the government. Some respondents may interpret the question to exclude conflicts in which the state is not directly involved (e.g., conflicts between criminal gangs).

There is disagreement over whether participation in violence can be measured simply by asking people whether they have engaged in violence. Wood (2003a, p.32) argues that survey data provide little insight into these questions. She is especially skeptical as to whether respondents would be willing to respond (truthfully) to a survey, and wonders whether an admission of violence could in fact just be a statement of desire to commit violence. On the other hand, since participating in political violence generally is illegal, admitting to such actions is to admit to a crime. If the respondent believes that there could be repercussions from admitting participation, it seems likely that he/she would lie or refuse to answer.

Social desirability bias — which occurs when respondents anticipate the views of the enumerator and provide answers aimed at pleasing her — is a well known issue in the survey literature (Blair et al. 2013, p.33). This bias is likely to be even greater when respondents are asked about sensitive issues, such as participation in illicit activities. Creatively designed surveys, that employ, for example, secret ballots or endorsement experiments when asking about sensitive issues, have been used (Lyall, Blair, and Imai 2013; Claassen 2014). The Afrobarometer is not such a survey. Although social desirability bias can be an obvious problem, particularly when the goal is to produce representative statistics from a population, it is less problematic when estimating causal effects, because this bias will mainly have the effect of attenuating the estimated effect towards zero. This bias therefore only increase the risk of a Type 1 error. Any positive effects should still be interpreted as a rejection of the null hypothesis.

The failure to respond to a question (item nonresponse) can increase when questions are asked about sensitive issues (Blair et al. 2013, p.34). Approximately half as many as those who reported participation answered "Don't know" or refused to answer (See Table 2). It can be asked whether this group should be treated as missing at random, or if participants in violence are more likely than non–participants to refuse to answer. The appendix (section 3) to this article includes a sensitivity test whereby nonresponse items are varyingly assigned to the other two possible outcomes. Based on those results, it is likely that most refusals to answer are non-participants. Since the share of participants also is very low, such an interpretation seems warranted.

The independent variable is the respondent's evaluation of his/her living conditions now compared to twelve months earlier (PECON).[3] The respondent can answer "Much worse", "Worse", "Same", "Better", "Much Better", "Don't know" or "Refused to answer".

The PECON item is open to interpretation. Individuals will focus on different things when thinking about living conditions, such as personal income, housing conditions, personal and family health, relations with neighbors, their commute, and changes in personal relations. Changes of any of these can be related to the theory outlined above. However, the climate variability instrument is unlikely to affect all types of changes in living conditions. The instrumented effect must therefore be interpreted as a Local Average Treatment Effect/Complier Average Causal Effect (LATE/CACE) (Gerber and Green 2012, p.141ff).

In order to successfully identify a causal effect, IV models must involve a strong instrument which is exogenous and random, and must also meet the exclusion restriction and the stable unit value assumption (non–interference). Additionally, when heterogeneous treatment effects are assumed, monotonicity must also be assumed (Gerber and Green 2012; Angrist and Pischke 2008; Sovey and Green 2011). The following paragraphs will discuss some of these criteria. A more elaborate discussion is found in the appendix (section 1) to this article.

An instrument must be exogenous and only affect the outcome through the mediating factor (Sovey and Green 2011, p.189f). One way of making sure that the instrument meets the latter criterion (assuming excludability and non–interference) is if the propensity for being exposed to a particular instrumental treatment is known and controlled for (that is, when the instrument is random) (Angrist and Pischke 2008, p.81).

SPEI is an estimate of the probability distribution of the precipitation–evapotranspiration system in a given area–month (or interval) (Vicente-Serrano, Beguería, and López-Moreno 2010). The assumption in this article is that this distribution is equal for all areas. This assumption is quite reasonable when using SPEI (See Appendix, Figure 1). A benefit to using SPEI is that it is a function of two relatively independent variables (precipitation minus evapotranspiration). The central limit theorem says that any sum of several independent distributions tends toward a normal distribution. This is exactly what happens in SPEI. Since the underlying SPEI distributions are similar (unlike SPI or growth in rainfall), SPEI can be used as a random instrument in cross–sectional analysis.

A strong instrument is needed to prevent bias (Bound, Jaeger, and Baker 1995). In this design, a strong instrument arises if and when variation in SPEI can explain changes in personal living conditions. Precipitation and evapotranspiration can affect living conditions through several pathways.

3. The exact wording is: "Looking back, how do you rate the following compared to twelve months ago: Your living conditions?"

The most important are arguably changes in crop yields, availability of (clean) drinking water, sufficient pasture for livestock, conditions for vector-borne diseases and flood damages (Porter et al. 2014; FAO 2016; Delpla et al. 2009; Smith et al. 2014; Thornton, Boone, and Ramirez-Villegas 2015; Vicente-Serrano, Beguería, and López-Moreno 2010).

There are two parameters that can be manipulated in SPEI to get the strongest possible instrument. One is the timing of the end–month of SPEI in the last year before the interview. It may be thought, for instance, that droughts during the growing season will have a more substantial impact on living conditions than will droughts outside of the growing season. The other is the interval over which SPEI is calculated. If precipitation and temperature are averaged over large intervals, the average tends to pick up different underlying phenomena, such as soil dryness/flooding, reservoir levels and stream–flows (National Drought Mitigation Center 2014). Generally, shorter intervals capture immediate soil conditions, whereas longer intervals capture stream-flow and reservoir levels.

The bottom of Figure 1 represents the dependent variable. It points to a problem that the outcome can potentially be measured before the treatment, something which can severely threaten unbiased inference[4]. The closer to the time of the interview (t_1) that we instrument changes in living conditions, the longer the time that has elapsed during which a respondent could have participated in political violence. Also, a consequence of longer SPEI intervals is that longer time has gone where we do not know whether the respondent participated before or after treatment. Ideally, SPEI-1 at $t_1 - 11$ months should be a strong instrument, as it minimizes the time during which participation in violence could have happened before the treatment. On the other hand, there may be other interval lengths and timings that produce a stronger instrument of $PECON_i$.

To put the research design to the test, this article estimated the same models, using only SPEI values from another time–period that could not have affected the outcome, akin to a placebo–control test. The values used are SPEI data from 20 years before Afrobarometer interviews. The expectation for such a test is that the relationship between SPEI and living conditions is near zero, as SPEI 20 years before the interview cannot affect changes in living conditions between one year before the interview and the time of the interview.

The main model specification is a recursive bivariate probit model, which allows for non-linear splines in both the first and the second stage (Marra and Radice 2011). It means that SPEI can be modeled as a non-linear spline effect, rather than assuming linearity or making an arbitrary binary cut. Since POLVIO and PECON has more than two response alternatives, they were collapsed into binary variables called BPOLVIO and BPECON.

4. There is a simulation of the problem in the Appendix (section 3).

For BPOLVIO, those answering "Yes" (3 different answers) are coded 1, and those answering "No, would never do this" and "No, but would do if had the chance" are coded 0. Since the latter response could arguably capture motivation for violence, the models were also run using a variable whereby that response is coded as 1. For BPECON, those answering "Same", "Better" and "Much Better" are coded as 1 and "Worse" and "Much Worse" are coded as 0. Those who did not know, refused to answer, or were otherwise missing are coded as missing. See the Appendix (section 2) for details.

Since SPEI is only argued to be random in cross–sections, the models control for which Afrobarometer round the respondent participated in. While other control variables should not be needed here, the precision of estimates can benefit from including variables which predict the two outcome variables. Three predictors are included: the respondents gender and age, as well as a random country–effect. None of these predictors are control variables. Age and SPEI are modeled as thin–plate splines, which are denoted by *ts*() (Wood 2003b). Country specific effects are modeled using an i.i.d. normal random effects smoother, which are denoted *rs*() (Wood 2008b).

$$\begin{aligned} \Phi(BPOLVIO_i) &\sim BPECON_i + GNDR_i + ts(AGE_i) + RND_t + rs(CNTRY_c) \\ \Phi(BPECON_i) &\sim ts(SPEI_d) + GNDR_i + ts(AGE_i) + RND_t + rs(CNTRY_c) \end{aligned} \tag{1}$$

When estimating standard errors, or when testing for weak instruments, since randomization happens at group–level rather than at the individual level, it is important to take this clustering into account. The assumption is that the instrument is randomized at the first level administrative unit, and the clustering is done accordingly.

Since F-statistics are not meaningful for bivariate probit models, an alternative 2SLS model is used when calculating the weak instrument tests. The 2SLS models are also not capable of estimating random smoothers and splines.[5] The model used is a best 2SLS approximation of the recursive bivariate probit model. Specifically, I estimate

$$\begin{aligned} BPOLVIO_i &\sim BPECON_i + GNDR_i + AGE_i + AGE_i^2 + RND_t + CNTRY_c \\ BPECON_i &\sim SPEI_d + GNDR_i + AGE_i + AGE_i^2 + RND_t + CNTRY_c \end{aligned} \tag{2}$$

4 Data

Afrobarometer rounds 2 and 5 were carried out in 2002–2004 and 2011–2013 in 16 and 35 countries in Africa, respectively (Afrobarometer Data

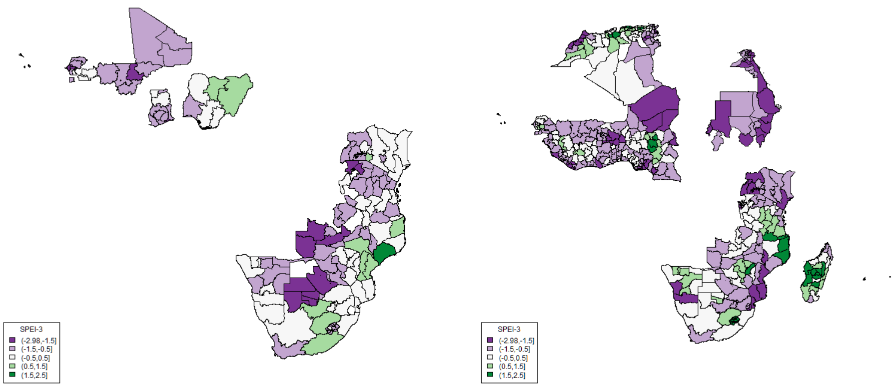
5. The reason for this is that the splines must be estimated in penalized likelihood models.

2004, 2015). In most countries, the survey was done within a 3–4 week period. Afrobarometer aims at a representative study at the national level, but does stratified sampling at the district level, as well as a random sampling of primary sampling units (PSU) within each district. Within each PSU, commonly eight individuals are randomly sampled by finding a random starting point, randomly selecting households, and then randomly selecting a member of the household.

This article assume that Afrobarometer is representative in each first administrative level in all the surveyed countries. Although their sampling strategy can ensure local representativeness, the low N in each PSU and the relatively few PSUs in each administrative unit mean that the uncertainty about the true local average is quite high. Afrobarometer only promise representativeness at the country level. In the sample used, after removing observations with missing data, the minimum number of respondents in each administrative area is 8 (i.e. only one PSU), the median is 90, and the maximum number of respondents is 1473 (the Central region in Malawi). The full distribution can be seen in the Appendix (figure 3).

Monthly precipitation data is taken from the GPCC v.7.0 0.5° grid resolution product (Schneider et al. 2015); temperature from GHCN/CAMS, also at 0.5° grid resolution (Fan and Dool 2008)[6]. After manually matching all districts in Afrobarometer with the geo–referenced polygon representation of them in GADM v.2.8 (Hijmans 2015), the mean precipitation, temperature and centroid latitude was calculated for each district using all grid observations that intersected with a polygon. From the time-series of mean precipitation and temperature from 1948 to 2014, SPEI was calculated for each district using the SPEI package in R (Beguería and Vicente-Serrano 2013). The centroid latitude of the district was used to measure sun–hours in each month, in order to estimate Potential Evapotranspiration (PET) using the Thorntwaithe method (Thornthwaite 1948). The result can be seen in Figure 2. Since only parts of Africa were surveyed in round 2 and 5, only those parts are included in the study.

To decide the final month of the growing season, the mode of all grid-cells intersecting each district of the growend variable from PRIO-GRID was used. growend is defined as the growing season for the main crop with the highest harvested area within a PRIO-GRID cell (Tollefsen, Strand, and Buhaug 2012; Tollefsen, Bahgat, and Nordkvelle 2016). The original crop data is from the MIRCA 2000 dataset (Portmann, Siebert, and Döll 2010).



(a) Round 2 (b) Round 5

Figure 2: SPEI used in main models (10 months before interview)

		BPOLVIO		
BPECON		Did not participate	Did participate	Missing
Worse	Count	24052	948	500
	Share	0.943	0.037	0.020
Same or better	Count	47837	1515	832
	Share	0.953	0.030	0.017
Missing	Count	658	30	57
	Share	0.883	0.040	0.077

Table 2: Bivariate relations in Afrobarometer survey items

5 Results

The bivariate relations (Table 2) show that the vast majority of the respondents did not report participating in political violence and that more people reported having same or better living conditions than they had 12 months earlier. An naive estimate of the causal effect we are interested in would be to subtract the share who participated in violence and reported having same or better living conditions, P(Participated in [v]iolence|Same or [b]etter), from the share who participated in violence and reported a deterioration in living conditions, P(V|[W]orse):

$$\hat{ATE} = P(V|B) - P(V|W) = .030 - .037 = -.007 \approx 0 \tag{3}$$

6. The GHCN Gridded V2 data was provided by the NOAA/OAR/ESRL PSD, Boulder, Colorado, USA, on their web site, which can be found at http://www.esrl.noaa.gov/psd/.

From this naive estimate, we would assume that there is no relationship between these two variables. However, there may be confounding factors which bias the estimate toward zero, suggesting the need for an IV method.

Table 3: F-statistics

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
	Rural, BPECON, SPEI, Treatment												
SPEI-1	0.00	21.27	0.87	1.38	12.21	14.14	0.22	14.70	8.43	11.35	21.22	12.87	2.01
SPEI-3	3.16	0.01	12.97	7.39	10.18	7.76	4.71	6.74	15.16	17.23	30.89	16.48	6.64
SPEI-6	2.34	4.37	11.56	7.07	13.20	8.64	5.41	7.07	12.61	15.16	30.33	14.96	11.52
SPEI-12	2.20	4.46	5.23	5.16	8.88	9.35	15.49	19.82	11.81	6.44	7.38	7.96	5.44
	Urban, BPECON, SPEI, Treatment												
SPEI-1	0.81	3.98	0.00	0.31	3.63	4.66	0.18	5.43	3.86	1.96	5.53	3.09	0.34
SPEI-3	1.37	0.30	3.89	1.89	2.90	3.11	3.08	5.99	11.00	2.26	4.85	3.67	3.50
SPEI-6	0.01	0.44	5.00	2.80	3.66	3.05	4.27	6.24	7.61	1.33	3.24	2.99	4.28
SPEI-12	0.03	0.83	2.09	1.07	2.37	1.71	5.03	7.45	3.23	0.01	0.01	0.30	1.53
	Rural, BPECON, SPEI, Placebo												
SPEI-1	3.09	6.16	6.22	7.53	1.59	2.59	0.22	0.93	0.40	1.96	6.11	0.79	4.01
SPEI-3	2.71	7.55	7.57	8.12	3.50	0.76	0.15	2.22	4.43	2.11	4.84	0.83	5.35
SPEI-6	3.46	10.46	6.66	8.00	4.09	3.09	1.47	5.30	3.03	3.87	1.82	1.33	4.39
SPEI-12	3.97	6.66	7.68	6.44	4.88	4.13	5.41	8.05	2.63	1.55	0.44	0.72	5.14
	Urban, BPECON, SPEI, Placebo												
SPEI-1	3.40	3.75	1.81	0.43	1.60	0.06	0.53	0.02	3.36	0.17	7.49	0.33	1.64
SPEI-3	2.20	1.96	2.26	0.40	0.60	0.57	0.00	1.92	5.13	1.33	2.66	0.42	0.00
SPEI-6	0.02	3.35	2.72	0.56	0.27	0.25	0.38	3.69	3.78	3.16	0.90	0.00	0.01
SPEI-12	0.03	0.22	1.89	1.72	2.93	2.15	2.50	4.91	1.23	0.30	0.46	0.02	1.13

The table shows the F-statistics comparing a base model of BPECON and a model where SPEI is included. The greyed cells are the only specifications which result in a F-statistic larger than 16.38, which is the threshold for SPEI to be regarded as a strong enough instrument to yield consistent estimates in the second stage.

Table 3 shows the F-statistics for the models defined in Equation 2, both for the main treatment models, and for the placebo test. The grayed cells are the only specifications which pass the weak instrument test ($F > 16.38$).

The first thing to notice is that we find specifications which pass the weak instrument test only in rural areas. Since this finding is in line with the theoretical model, it should increase our confidence that SPEI is picking up the signal we want it to pick up.

The second thing to notice is that in the specifications using the end–month of the last growing season before the interview (the column furthest to the right), SPEI does not seem to be strongly related to reported changes in living conditions. This finding should decrease our confidence that SPEI is picking up the signal we want it to. One possible explanation is that the measurement of growing season is poor. Growing seasons can vary between years, and the producers of the mode of the main crop may not be representative.

The third thing to notice is that, although the placebo test does have specifications where the F–statistic is around 7-10, and although the placebo test generally has higher F–statistics in the rural than in the urban subsample, the placebo never reaches the levels of association that we find for the actual treatment.

The strongest instrument from all the specifications came from using

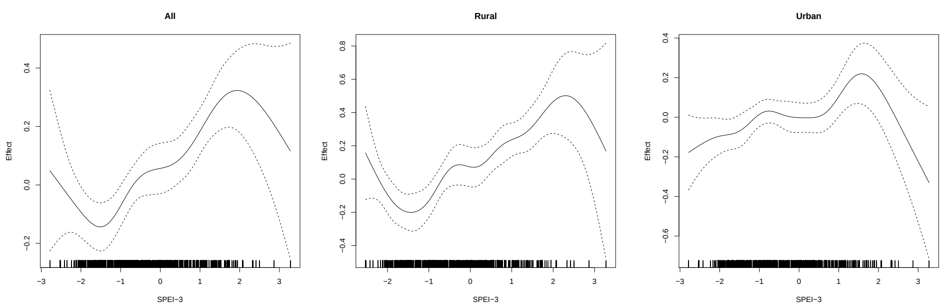


Figure 3: Non-linear effect of SPEI-3 on living conditions

SPEI–3 as a linear instrument, measured at ten months before the interview. Here, the F–statistic is 30.89 which passes the weak instrument test suggested by Stock and Yogo (2005). Since this selection comes quite early in the 12–month interval, this candidate could be a good way to circumvent the problem that the outcome could have been decided before the treatment. That specification is used in the following tests.

The estimated effect of SPEI–3 on BPECON measured at ten months before the interview, for all observations and for the urban and rural subsample, is shown in Figure 3. In the rural subsample, respondents more often report same or better living conditions as the climate becomes wetter and colder. In the urban subsample, however, the differences are negligible and not statistically significant[7].

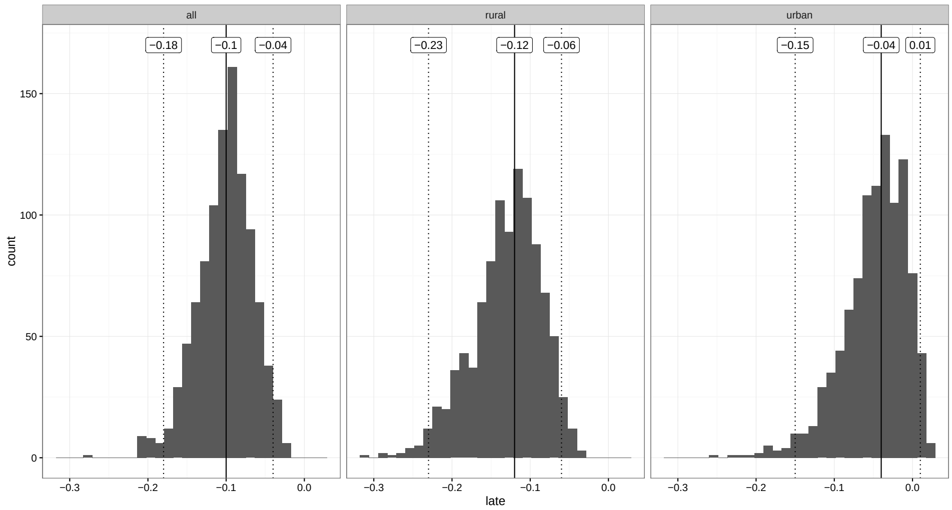
Figure 4 shows our quantity of interest, the complier average causal effect (CACE) of the instrumented BPECON on BPOLVIO.[8] Since the instrument is weak for the urban subsample, that estimate is only shown for completeness. In the rural subsample, the CACE is estimated to be -.12, meaning that a person in the complier sub-population is 12% less likely to participate in politically motivated violence if his living conditions stayed the same or became better, than if he had experienced a deterioration of his living conditions. The 95% precision interval of this simulation is [-.23, -.06].

The complier sub-population includes those whose living conditions respond to the instrument. Since we do not know the proportion of compliers in the sample, there is no simple way to get back to the ATE in the population, and thus to the societal effect of a deterioration in living conditions on participation. It does show, however, that the unknown number who are

7. The first and second stage regression tables, as well as plots of the other variable splines, can be seen in the Appendix (section 4).

8. In the recursive bivariate probit model, the CACE is obtained through posterior simulation using the `AT` function in the `SemiParBIVProbit` package in R. This is comparable to the 2SLS estimate.

Figure 4: Complier average treatment effects of same of better living conditions on participation in political violence



compliers are on average more likely to participate when living conditions are getting worse.

In the above tests, missing observations are treated as missing at random. It is quite likely, however, that they are not. To investigate this, the main model for the rural sample is run twice, one where all missing values are coded as having both experienced a deterioration in living conditions and participated in conflict, and one where all missing values are coded as having experienced improvements in living conditions and not participated in conflict. The first model results in a best estimate of -.18 [-.27, -.1], while the second model estimate is -.12 [-.22, -.05] (as shown in Figure 6 in the Appendix to this article). It shows that the lower bound estimate is not that much lower as a result of missing values, but that the higher bound estimate could be higher.

Recoding "No, but would do if had the chance" from BPOLVIO = 0, to BPOLVIO = 1, dramatically affects the result. Now, the estimated effect becomes -.06 [-.25, .07] (See the Appendix, figure 5). One possibility is that there is an important difference between those who actually participate, and those who only say that they would have liked to do so.

6 Discussion

This paper is the first to quantify the causal effect of a person's *experiencing* a deterioration in living conditions due to droughts on *that person's* likelihood of participation in political violence. The results show that participation in political violence, on average, would increase for those among the population that comply with the instrument when living conditions goes down. This is empirical support for the notion that individual motivations affect conflict participation given the right context, and that climate variations can increase such motivations.

The estimated complier average causal effect is large. That 1 out of 10 (of those whose living conditions are affected by droughts) would have participated in political violence that they would otherwise have engaged in if not for having experienced a deterioration of living conditions is potentially a massive effect. The unknown in this equation, of course, is the size of the population that are compliers.

Four points make the identification credible. First, empirical SPEI distributions are similar. This is a core assumption when arguing that SPEI can be treated as a randomized variable in cross-sectional studies. Second, the instrument is only effective in rural areas, which makes sense theoretically. Third, the instrument is not strong in the placebo–test. Fourth, the first stage relationship in the strongest specification is quite good. The F-statistic is double the size of what is needed to pass the weak instrument test.

Although these results cannot be used to say that there will be more conflicts due to droughts (since structural conditions may also change), they can be used to argue that policy interventions which directly target those who have lost income or capital because of droughts can reduce the ability of violent organizations to recruit new members.

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Part III

Appendix

6 Supplementary information, Forecasting civil conflict along the shared socioeconomic pathways

Supplementary Information,
Forecasting Civil Conflict along the Shared Socioeconomic Pathways

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Supplementary Information

Contents

S1 Overview

This Supplementary Information provides a summary of the storylines embedded in the Shared Socioeconomic Pathways or SSPs (Section S2), the statistical model underlying the simulations (S3), the simulation procedure generating the forecasts (S4), documentation of the out-of-sample evaluation (S5), a review of the core predictors in the model as operationalized in each SSP (S6), additional simulation results (S7), and a set of adjustments done to the historical and projected data used (S8).

Replication files, instructions to reproduce all results, data, and further documentation are available at http://havardhegre.net/forecasting/ and https://www.prio.org/data/.

S2 The Shared Socioeconomic Pathways

The Shared Socioeconomic Pathways (SSPs) are intended to represent five potential future pathways of development, as points of departure for assessing possible futures with various implications for climate change [1,2]. The SSPs replace the previous scenarios developed by the climate change research community, known as SRES, from the Special Report on Emissions Scenarios [3]. The SSPs are developed as narratives and require formulation as projections for specific variables in order to be operable. The operationalization of the SSPs are documented in Section S4.2 and presented visually in S6.

Unlike the SRES, these reference SSP pathways do not include explicit projections of emissions. Rather, modeling teams will employ the socioeconomic and demographic information contained in the SSPs to estimate emissions and end-of-century implications for global and regional climatic changes using a range of integrated assessment models (IAMs). , Climate policies can be then be modeled for each of these pathways with mitigation and adaption costs expected to differ across the SSPs.

Capturing alternative plausible but divergent pathways, the SSPs comprise the following scenarios: “Sustainability” (SSP1), “Middle of the Road” (SSP2), “Fragmentation” (SSP3), “Inequality” (SSP4), and “Conventional Development” (SSP5). These five scenarios are classified according to the challenges for the mitigation of the corresponding greenhouse gas (GHG) emissions and challenges for adaptation to the impacts of climate change. For example, both SSP1 and SSP5 have stabilizing populations and economic convergence across countries, but differ in the structure of the economy and fossil-fuel dependency. With a greater reliance on fossil fuel, the SSP5 pathway was designed to constitute larger challenges to GHG emissions mitigation than SSP1, which has lower energy-service demand and more use of renewable energy technologies. Both SSP1 and SSP5 pathways, however, are likely to allow for relatively easy adaptation to the impacts of climate change. By contrast, a SSP3 pathway with significant growth in population but lower economic growth would render both mitigation and adaptation more challenging. Complementing the assumptions of climate change adaptation and mitigation, each SSP has specific quantitative drivers, notably population, GDP, and education. These storylines also have qualitative descriptions of other drivers, such as technological development and agricultural yield growth rates. Assumptions about institutional characteristics (e.g., land use change regulations) are incorporated in these storylines, although political institutions are not explicitly modeled.

S2.1 Sustainability (SSP1)

In SSP1, the world is making good progress towards sustainable development with strong international governance and local institutions. Importantly, this pathway assumes that the Millennium Development Goals (MDG) are achieved early (i.e., within the next one to two decades) for all countries [4]. This implies rapid development of low-income countries, high levels of environmentalism, and planned urbanization. The economy is assumed to be open and globalized. Economic inequality both between and within countries will decrease. Consumption is oriented towards low material growth and energy intensity. Traditional fossil fuels experience a quick phase-out, driven by policies and technological development. Large investments are made in education. Due to economic growth, higher education, and family planning, the world population peaks mid-century and declines to 7.2 billion in 2100 [5].

S2.2 Middle of the Road (SSP 2)

SSP2 is designed to be a middle-of-the-road pathway. There is some progress to achieving the MDGs. Reductions in resource and energy intensity happens at historic rates, with a slowly diminishing fossil-fuel dependency.

S2.3 Fragmentation (SSP3)

In SSP3, the world experiences rapid population growth coupled with slow economic growth. Specifically, it fails to achieve the MDGs. The international system is characterized by weak international governance and local institutions. Countries organize into regional blocks with little coordination between them. International trade is severely restricted. There is high resource intensity in the economy, high levels of fossil fuel dependency, low levels of investments in technology development, and unplanned settlements. There is also little investment in education. As a result of both low education and low economic development, the global population reaches 14.1 billion in 2100, although growth varies by country, with the majority of the population growth occurring in Africa.

S2.4 Inequality (SSP4)

This pathway envisions a world with persistent and increasing levels of inequality within and across countries. Governance is centralized and controlled by a small number of rich global elites. This leads to a world where only a small elite is responsible for GHG emissions, as the poor majority contributes little in that regard. Investments in new energy technologies are made by global energy corporations. Most societies are shaped by limited access to higher education and basic services. The population reaches 11.8 billion in 2100, with most of the growth occurring in poor regions (i.e., roughly half of the global population in 2100 is in Africa).

S2.5 Conventional Development (SSP5)

In SSP5, economic growth is seen as the solution to socioeconomic concerns; thus, the world pursues rapid economic development, however, using more conventional measures. Specifically, energy systems continue to be dominated by fossil fuels with a focus on consumerism. The MDGs are mostly achieved with the eradication of extreme poverty and universal access to education and basic services. Large investments in technology development lead to highly engineered infrastructure and ecosystems. Education and technology development, coupled with an open global economic system, lead to a rapid convergence and a global population that peaks mid-century before declining to 7.7 billion in 2100.

S3 The statistical model underlying the simulations

The unit of analysis in the study is the independent country, observed once every year for all years, 1960–2013 (historical period) and 2014–2100 (simulation period). The sample is consistent with the Gleditsch and Ward historical list of independent states [6] and assumes no changes in the existence or delineation of states throughout the simulation period.

We apply a mixed-effects multinomial logistic regression model that estimates the transition probabilities between peace and two intensity levels of armed conflict, 1960–2013. The specification of this model reflects the state-of-the-art of quantitative civil war research[7]. The 'no conflict' outcome is set as the reference outcome such that we estimate one equation for the minor conflict and one for the major conflict outcome, including the same independent variables in each equation. Sections 3.1 and 3.2 detail the data used to estimate the model. Tables S1 and S2 report descriptive statistics for the data used in the estimation. In section S4.2, we account for the projections up to 2100 for the predictors. We made a number of adjustments to several of these datasets in order to obtain maximum coverage of cases and to link the historical series to the projections. This is detailed in Section S8.

S3.1 Dependent variable

Our conflict data are from the 2014 update of the UCDP/PRIO Armed Conflict Dataset [8,9], the industry standard for quantitative conflict research. This dataset records conflicts at two severity levels with annual statistics, 1946–2013. Minor conflicts are those that pass the 25 annual battle-related deaths threshold, but have less than 1,000 deaths in a calendar year. Major conflicts are those conflicts that generate at least 1,000 annual deaths. We only look at civil conflicts, i.e., those that involve military battles between a state government and one or more organized non-state actors. This is by far the dominant form of armed conflict today. Following convention, we only consider the countries whose governments are included in the primary conflict dyad as hosting a civil conflict (i.e., countries that intervene in an ongoing civil conflict in another state are not coded as part of the conflict). In our historical sample, around 16% of the observations hosted a civil conflict (Table S1).

Table S1. Descriptive statistics for the conflict data, 1960–2013

Indicator	Outcome	N
Conflict incidence	No conflict	6,528
	Minor conflict	886
	Major conflict	395
Neighboring conflict	No conflict	4,273
	Conflict	3,536

S3.2 Independent variables

Countries with previous conflicts are more likely to see renewed conflict [10,11]. We include information on conflict status (no conflict, minor, or major conflict) at t–1, the year before the year of observation. This is coded as two dummy variables, **c1** and **c2,** for minor and major conflicts, respectively. In addition, to account for the legacy of a longer conflict history, we include a variable capturing the number of years in peace in a country up to (but not including) the year before the year of the observation. The variable is log-transformed to reflect that an additional year of peace changes the risk of conflict more in the first year after the conflict than a couple of decades later. In the tables below (as well as in the replication dataset), this variable is called **ltsc0**.

Newly established states are also more fragile than states that have been around for some time [7,12,13]. To capture this, we code for each country the time since the state was established in its current form or since the year 1700 if the state became independent before then [6]. The count is log-transformed since it is reasonable to assume that the uncertainty surrounding a new statehood decays over time. The variable is named **ltimeindep**.

Adverse impacts of armed conflict often extend beyond the boundaries of the host state [7,14]. We capture the conflict situation in neighboring countries with two variables. The first is a dummy for whether any land-contiguous neighboring country had a minor or a major conflict during the year prior to the year of observation. This variable is called **nc**. 45% of all observations had at least one conflict in a neighboring country (Table S1). The other variable captures the long-term conflict history in the neighborhood, given as the (log) number of years since the most recent neighboring conflict, up to (but not including) the year before the year of observation. The variable is referred to as **ltsnc**.

Unobserved global time-specific shifts in armed conflict propensity are captured through decade dummies for the **1960s**, **1970s**, **1980s**, and **1990s**, with 2000–13 as the reference category.

The three main predictors representing the SSPs are **Population**, **GDP/capita**, and **YMHEP** (share of males age 20–24 that have attained at least upper secondary education). All variables are lagged one year. Population, derived from the UN World Population Prospects 2012 Revision [15], are given in (log) thousands. GDP/capita is given in (log) purchasing power parity (PPP) adjusted 2005 US dollars. The primary source for the empirical GDP data is the World Development Indicators [16], using the variable NY.GDP.PCAP.PP.KD. When this series had missing observations, we resorted to either NY.GDP.PCAP.KD from the same source, Penn World Table [17] or Maddison [18]. YMHEP is based on age group-specific education data from IIASA [19] for the period 1970–2010 and backdated to 1960 using linear interpolation. Table S2 presents the descriptives for these indicators.

Table S2. Descriptive statistics for independent variables, 1960–2013

Indicator	25th percentile	Median	75th percentile
Population	3,114,014	8,117,742	22,671,134
GDP/capita	1,337	3,983	11,408
YMHEP (education)	0.16	0.33	0.58
ltsc0 (log time since conflict)	1.10	2.71	3.71
ltsnc (log time since conflict, neighbors)	1.39	2.49	3.22
ltimeindep (log time since independence)	3.09	3.83	4.93
N (country years)			7,809
N (listwise deletion)			97
N (valid sample)			7,712
N (countries)			166

Note: Population and GDP statistics reflect values before transformation.

S3.3 The multinomial logistic regression model

Since the dependent variable contains three outcomes (no conflict, minor conflict, major conflict), the multinomial logit model is appropriate. In order to model unobserved differences in armed conflict propensities between countries, we estimate time-invariant country-specific intercepts. We include separate sets of intercepts in each of the minor and major conflict equations, estimated as random effects in two multilevel mixed-effects logistic regressions (`melogit` in Stata) with the same control variables as in the main model. To account for the uncertainty in the magnitude of the country-specific intercepts, the simulation alters between 15 different draws from the probability distributions of these country-specific effects.

We estimate two alternative models in parallel. The first (and main) model includes all variables presented above whereas the second model excludes the YMHEP education indicator. The results from these estimations are reported in Table S3. Due to space constraints we only report the results for the first model in the article.

When we include YMHEP in the model, much of the effect of log(GDP/capita) is absorbed by the education variable. In effect, the model with YMHEP imposes an upper limit to the effect of socioeconomic development since the education indicator by definition is bounded at 100% attainment. Moreover, the education level is assumed to remain virtually constant at present levels in two of the SSP scenarios (see Fig. S4 below) whereas GDP per capita continues to grow across all scenarios. Thus, the socioeconomic development envisaged for YMHEP is much more pessimistic than what we see for GDP/capita. Since we are unable to separate the effect of education and productivity on armed conflict, we interpret both YMHEP and GDP/capita as proxies for different dimensions of socioeconomic development.

A comparison of the two models reveals that broad socioeconomic development, as represented by universal education, is more important for conflict risk reduction than the narrower conceptualization of development usually represented by average income statistics. Overall, Model 1 has a lower AIC score than the simpler Model 2 and also performs slightly worse in terms of out-of-sample prediction (see Table S7).

The simulation procedure takes the joint effect of all these parameters into account, but a brief discussion of the substantial impact of the various terms is useful. The discussion below refers to the minor and major conflict equations in Model 1, but the effects are roughly similar in Model 2. The discussion is in terms of the effect of odds of conflict. Since the baseline probability of armed conflict is relatively low, the effect on odds of conflict is roughly similar to the effect on the probability of conflict.

The estimates for log population indicate that the odds of both minor and major conflict increase by 39% when population is increased by a factor of $e \approx 2.7$ – more populous countries have more frequent conflicts, but considerably less conflict per capita than smaller ones.

Table S3. Estimation results of civil conflict incidence, 1960–2013

	Group	(Model 1)				(Model 2)			
		Minor		Major		Minor		Major	
		β	*z*-stat.	β	*z*-stat.	β	*z*-stat.	β	*z*-stat.
(Intercept)		**-6.058**	(-8.33)	**-6.841**	(-5.42)	**-4.143**	(-6.52)	**-6.427**	(-5.38)
Log(Population t-1)	1	**0.327**	-7.11	**0.327**	-4.46	**0.277**	-6.13	**0.366**	-4.94
Log(GDP/capita t-1)	2	0.053	-0.66	-0.232	(-1.85)	**-0.27**	(-4.17)	**-0.403**	(-3.74)
Log(GDP/capita t-1)*c1	2	**0.12**	-4.76	0.057	-1.17	**0.122**	-4.84	0.06	-1.24
Log(GDP/capita t-1)*c2	2	**0.126**	-3.25	**0.123**	-2.22	**0.129**	-3.39	**0.125**	-2.27
Log(GDP/capita t-1)*ltsc0	2, 4	-0.018	(-1.63)	-0.04	(-1.89)	-0.019	(-1.76)	-0.041	(-1.91)
YMHEP(education) t-1	3	**-2.141**	(-5.66)	-0.802	(-1.41)				
c1 (minor conflict at t-1)		**2.885**	-9.57	**3.519**	-4.69	**2.866**	-9.62	**3.72**	-4.83
c2 (major conflict at t-1)		**2.277**	-4.88	**5.379**	-6.73	**2.374**	-5.09	**5.676**	-6.97
ltsc0 (log time since conflict)	4	-0.178	(-1.69)	-0.022	(-0.09)	-0.179	(-1.71)	-0.055	(-0.22)
Ltimeindep (log time since independence)	5	0.072	-1.1	0.04	-0.39	**0.19**	-3.02	0.073	-0.74
nc (neighbor in conflict)	6	0.335	-1.15	0.398	-0.48	0.315	-1.08	0.509	-0.6
ltsnc (log time since conflict, neighbors)	6	0.009	-0.18	0.015	-0.18	-0.067	(-0.58)	0.183	-0.56
nc*c1	6	-0.293	(-0.83)	-0.115	(-0.13)	-0.233	(-0.66)	-0.302	(-0.34)
nc*c2	6	0.28	-0.5	0.519	-0.55	0.147	-0.26	0.209	-0.22
ncts0 (neighbor in conflict*ltsc0)	4, 6	-0.04	(-0.34)	0.13	-0.41	-0.044	(-0.81)	0.025	-0.3
1960s	7	-0.173	(-0.82)	0.505	-1.5	0.093	-0.46	**0.822**	-2.52
1970s	7	0.023	-0.12	**0.655**	-2.17	0.224	-1.23	**0.789**	-2.66
1980s	7	0.197	-1.11	**0.91**	-3.43	**0.336**	-1.9	**1.052**	-3.98
1990s	7	0.128	-0.8	0.32	-1.25	0.156	-0.97	0.37	-1.43
Country-effect minor conflict	8	**1.065**	-9.83	**0.684**	-3.78	**1.005**	-8.83	0.263	-1.48
Country-effect major conflict	8	0.176	-1.87	**1.135**	-6.84	**0.242**	-2.36	**1.078**	-6.58
N		7,553				7,553			
AIC		3,447.20				3,473.60			
Log-likelihood		-1,679.60				-1,694.80			

Note: Mixed-effects multinomial logit coefficients with z-scores in parenthesis. Coefficients significant at the 0.05 level are set in boldface. Model 1 is the complete historical model; Model 2 is without the education indicator. To assess the joint significance of parameters across equations and interaction terms, we assigned the variables in Model 1 to eight variable groups (as indicated with group numbers in Column 2 of the table). We then reran Model 1 omitting these variables in turn and calculated likelihood ratio tests. All groups of variables were significant at the 0.05 level except for group 6 (neighborhood variables).

Because of the multiple interaction terms involving ln GDP per capita, the direct effect of income is less straightforward to interpret from the estimates (the simulation procedure takes the joint effect of all the terms into account, though). Among countries currently at peace, increasing GDP per capita from e.g. USD 1,000 to USD 2,700 decreases the odds of major conflict by 20%. The interaction term with ltsc0 (log number of years since conflict up to t-2) indicates that this effect is even stronger for countries that have been peaceful for some time – after 20 years of peace, this increase in GDP per capita reduces risk of major conflict by 30%. In countries that already have a conflict, GDP per capita has a much weaker effect (this is driven by the long conflicts in relatively rich countries such as the UK and Israel/Palestine).

Controlling for the effect of GDP per capita, increasing YMHEP education by 0.1 (e.g., changing the proportion of the male population between 20 and 24 years from 30 to 40%) further reduces the odds of conflict by 20%.

The estimates for lagged conflict (c1, c2) must be interpreted taking the interaction with GDP per capita into account. A low-income country with GDP per capita at USD 1,100 (i.e., 7 in log form) has an estimated odds of minor conflict more than 20 times higher than a similar country at peace, whereas the odds of major conflict is 500 times higher than that of a peaceful country. This 'conflict trap' effect is a powerful contributor to the simulation results shown in Fig. 3.

The term ltsc0 reflecting the log of consecutive years in peace up to t-2 adds to the conflict trap effect. A country that has been at peace for 20 years up to t-1 has an estimated odds of minor conflict 40% lower than for a country where peace broke out at t-1.

A number of terms for conflict in the neighborhood complement the model of the conflict trap. Most importantly, the nc term implies that a country with a neighboring country in conflict is 40–50% more likely to be in conflict than one that is located in a peaceful neighborhood.

Table S4 shows the correlation between the dependent variable and predictors in Model 1. Correlations are always substantial between multiplicative interaction terms as well as the terms constituting categorical variables. In addition, the correlation between log GDP per capita and our education measure is considerable, at r=0.69.

The direct interpretation of the parameter estimates is interesting in itself, but the coefficients' main function is to provide a basis for calculating the probabilities of no conflict, minor conflict, and major conflict as a function of the values for the predictors as described below. The procedure handles multicollinearity problems by construction, since the simulation draws realizations of the coefficients reported in Table S3 while simultaneously taking into account both the standard errors of the estimates and the correlation between them as estimated in the variance-covariance matrix. The variance-covariance matrix for Model 1 is reported (in correlation form) in Table S5.

Table S4. Matrix of correlation between predictors

	conflict	c1	c2	ltsc0	nc	ncc1	ncc2	ltsnc	ncts0	lpop	lGDPcap	lGDPcap_c1	l_GDPcap_c2	lGDPcap_ltsc0	YMHEP	ltimeindep	1960s	1970s	1980s	1990s	random_1	ramdom_2
conflict	1.00																					
c1	0.47	1.00																				
c2	0.64	-0.08	1.00																			
ltsc0	-0.57	-0.55	-0.35	1.00																		
nc	0.18	0.13	0.12	-0.26	1.00																	
ncc1	0.37	0.78	-0.06	-0.43	0.31	1.00																
ncc2	0.55	-0.07	0.84	-0.30	0.21	-0.05	1.00															
ltsnc	-0.01	-0.02	0.00	0.22	-0.07	0.02	0.01	1.00														
ncts0	-0.33	-0.30	-0.20	0.65	-0.77	-0.24	-0.16	0.22	1.00													
lpop	0.24	0.21	0.13	-0.12	0.19	0.18	0.11	0.16	-0.15	1.00												
lGDPcap	-0.23	-0.13	-0.18	0.47	-0.28	-0.14	-0.17	0.22	0.44	-0.03	1.00											
lGDPcap_c1	0.46	0.99	-0.08	-0.54	0.12	0.75	-0.07	-0.02	-0.30	0.21	-0.09	1.00										
lGDPcap_c2	0.63	-0.08	0.99	-0.35	0.11	-0.06	0.81	0.00	-0.19	0.13	-0.15	-0.08	1.00									
lGDPcap_lt~0	-0.52	-0.49	-0.32	0.97	-0.28	-0.38	-0.27	0.25	0.69	-0.10	0.62	-0.49	-0.32	1.00								
YMHEP	-0.16	-0.12	-0.10	0.36	-0.19	-0.11	-0.09	0.21	0.36	0.17	0.69	-0.09	-0.08	0.48	1.00							
ltimeindep	0.01	0.06	-0.03	0.36	-0.07	0.04	-0.03	0.36	0.27	0.40	0.38	0.08	-0.02	0.41	0.24	1.00						
1960s	-0.04	-0.05	-0.02	-0.02	-0.08	-0.05	-0.02	-0.20	0.04	-0.03	-0.12	-0.05	-0.02	-0.04	-0.16	-0.14	1.00					
1970s	-0.02	-0.02	-0.02	0.00	-0.06	-0.06	-0.02	-0.06	0.03	-0.06	-0.05	-0.02	-0.02	-0.02	-0.12	-0.08	-0.18	1.00				
1980s	0.06	0.02	0.06	0.01	0.03	0.01	0.05	0.06	-0.02	-0.02	-0.02	0.02	0.06	0.00	-0.06	0.03	-0.19	-0.22	1.00			
1990s	0.04	0.05	0.02	-0.06	0.07	0.06	0.02	-0.03	-0.07	0.01	0.00	0.04	0.02	-0.05	0.07	0.00	-0.20	-0.23	-0.24	1.00		
random_1	0.19	0.30	0.01	-0.22	0.03	0.19	0.00	-0.07	-0.12	-0.01	0.04	0.31	0.02	-0.19	0.05	0.00	0.00	-0.01	-0.01	0.00	1.00	
random_2	0.18	0.02	0.20	-0.08	0.03	0.03	0.16	-0.02	-0.05	0.00	-0.12	0.01	0.20	-0.08	-0.04	-0.05	0.00	-0.01	-0.01	0.00	-0.03	1.00

Table S5. Matrix of correlation between estimates, model 1

	1 (minor conf)	c1	c2	ltsc0	nc	ncc1	ncc2	ltsnc	ncts0	llpop	llGDPcap	llGDPc~1	llGDPc~2	llGDPc~0	lYMHEP	ltimei~p	1960s	1970s	1980s	1990s	random_1	random_2	_cons
	1																						
c1		1.00																					
c2		0.46	1.00																				
ltsc0		0.19	0.11	1.00																			
nc		0.63	0.41	-0.31	1.00																		
ncc1		-0.77	-0.34	0.26	-0.82	1.00																	
ncc2		-0.34	-0.74	0.15	-0.52	0.43	1.00																
ltsnc		0.02	0.02	0.03	-0.03	-0.02	-0.02	1.00															
ncts0		0.53	0.34	-0.39	0.82	-0.68	-0.43	-0.04	1.00														
llpop		-0.02	-0.03	-0.05	-0.05	0.01	0.03	-0.03	0.05	1.00													
llGDPcap		0.09	0.09	0.10	0.08	-0.02	-0.02	0.03	0.04	0.21	1.00												
llGDPcap_c1		-0.19	-0.12	-0.05	-0.04	0.02	0.01	-0.01	-0.06	-0.01	-0.23	1.00											
llGDPcap_c2		-0.12	-0.31	-0.05	-0.01	0.01	0.03	-0.03	-0.03	-0.01	-0.13	0.48	1.00										
llGDPcap_l~0		-0.31	-0.20	-0.62	-0.06	0.03	0.03	-0.04	-0.08	0.09	-0.28	0.52	0.35	1.00									
lYMHEP		-0.06	-0.07	-0.02	-0.05	0.05	0.04	-0.07	-0.07	-0.31	-0.57	-0.02	-0.04	-0.02	1.00								
ltimeindep		-0.02	-0.01	-0.02	0.03	0.01	0.01	-0.20	0.01	-0.39	-0.34	-0.15	-0.07	-0.12	0.20	1.00							
1960s		0.01	0.01	0.02	0.03	0.02	0.00	0.16	0.00	-0.07	-0.05	-0.01	-0.03	-0.07	0.20	0.13	1.00						
1970s		-0.03	-0.02	-0.01	0.02	0.05	0.01	0.10	0.01	-0.03	-0.09	-0.04	-0.02	-0.05	0.19	0.13	0.40	1.00					
1980s		-0.05	-0.04	-0.06	0.00	0.04	0.02	0.03	0.03	0.04	-0.08	-0.05	-0.07	-0.03	0.14	0.09	0.36	0.41	1.00				
1990s		-0.01	0.00	0.01	0.01	0.01	0.00	0.06	0.02	0.07	0.01	-0.02	-0.04	-0.03	0.01	0.04	0.37	0.41	0.42	1.00			
random_1		-0.05	0.01	-0.03	0.02	0.01	-0.01	0.10	-0.01	0.15	-0.09	-0.06	0.03	0.12	-0.14	0.03	-0.04	-0.01	0.03	0.01	1.00		
random_2		-0.01	-0.05	-0.03	0.01	-0.02	0.01	0.04	0.00	0.10	0.07	-0.02	-0.10	0.02	-0.03	0.03	0.04	0.05	0.10	0.06	0.07	1.00	
_cons		-0.28	-0.19	-0.07	-0.30	0.21	0.12	-0.10	-0.26	-0.60	-0.76	0.15	0.07	0.20	0.49	0.19	-0.11	-0.09	-0.09	-0.18	-0.07	-0.14	1.00
	2																						
c1		0.14	0.03	0.04	0.03	-0.10	-0.02	0.00	0.02	0.00	0.01	-0.05	-0.01	-0.07	0.00	-0.02	0.01	0.00	-0.01	-0.01	-0.03	0.02	-0.01
c2		0.05	0.32	0.04	0.03	-0.02	-0.24	0.00	0.02	-0.01	0.03	-0.04	-0.11	-0.06	-0.02	-0.02	0.00	0.00	-0.01	-0.01	-0.01	0.00	-0.01
ltsc0		0.04	0.05	0.12	-0.02	0.02	-0.01	0.01	-0.02	-0.01	0.03	-0.01	-0.01	-0.12	0.00	-0.01	0.01	0.00	-0.02	0.00	-0.01	0.00	-0.02
nc		0.02	0.02	-0.01	0.05	-0.04	-0.03	-0.01	0.03	-0.01	0.02	-0.01	0.00	-0.01	-0.01	0.00	0.01	0.00	-0.01	0.00	0.00	0.01	-0.01
ncc1		-0.10	-0.01	0.01	-0.04	0.13	0.02	0.00	-0.03	0.00	0.00	0.00	-0.01	0.00	0.01	0.01	0.01	0.01	0.02	0.01	0.01	-0.01	0.00
ncc2		-0.03	-0.23	0.01	-0.04	0.03	0.34	0.00	-0.03	0.01	0.00	0.00	0.01	0.01	0.01	0.01	0.00	0.00	0.02	0.01	0.00	0.00	-0.01
ltsnc		0.01	-0.01	0.01	-0.02	-0.01	0.02	0.44	-0.02	0.00	0.00	0.00	-0.01	-0.01	-0.01	-0.10	0.06	0.04	0.01	0.04	0.03	0.02	-0.04
ncts0		0.02	0.02	-0.01	0.04	-0.03	-0.02	-0.01	0.04	0.01	0.01	-0.01	0.00	-0.01	-0.02	0.00	-0.01	0.00	0.00	0.00	0.00	-0.01	-0.01
llpop		0.00	0.02	-0.01	-0.02	0.00	0.02	0.01	0.02	0.40	0.08	0.00	0.01	0.03	-0.12	-0.18	-0.03	-0.01	0.02	0.03	0.05	0.01	-0.23
llGDPcap		0.03	0.03	0.04	0.03	-0.01	0.00	0.01	0.01	0.09	0.41	-0.06	-0.02	-0.10	-0.23	-0.12	-0.01	-0.03	-0.02	0.01	-0.03	0.04	-0.32
llGDPcap_c1		-0.03	-0.01	0.02	-0.01	0.00	0.01	0.00	-0.01	0.00	-0.05	0.28	0.08	0.10	0.00	-0.06	0.00	-0.01	-0.01	-0.01	-0.03	0.01	0.03
llGDPcap_c2		-0.02	-0.13	0.00	0.00	0.00	0.02	-0.01	-0.01	0.00	-0.03	0.14	0.45	0.09	-0.02	-0.04	-0.01	-0.01	-0.03	-0.02	0.01	-0.04	0.02
llGDPcap_l~0		-0.07	-0.08	-0.13	-0.01	0.00	0.03	-0.01	-0.01	0.02	-0.08	0.11	0.08	0.21	-0.01	-0.05	-0.02	-0.01	-0.01	-0.01	0.04	-0.01	0.06
lYMHEP		-0.03	-0.05	-0.01	-0.02	0.01	0.00	-0.02	-0.03	-0.13	-0.23	-0.01	-0.01	-0.01	0.38	0.09	0.07	0.07	0.05	-0.01	-0.05	-0.02	0.19
ltimeindep		-0.01	-0.02	-0.01	0.02	0.01	0.03	-0.11	0.00	-0.18	-0.13	-0.07	-0.03	-0.06	0.09	0.41	0.05	0.05	0.03	0.02	0.01	0.03	0.08
1960s		0.00	-0.03	0.01	0.01	0.01	0.01	0.06	0.00	-0.03	0.00	0.00	-0.01	-0.03	0.07	0.04	0.36	0.17	0.16	0.16	-0.02	0.01	-0.05
1970s		-0.01	-0.01	-0.01	0.01	0.02	0.00	0.03	0.00	-0.01	-0.02	-0.01	-0.01	-0.02	0.06	0.04	0.16	0.39	0.17	0.17	-0.01	0.01	-0.04
1980s		-0.02	-0.02	-0.03	0.00	0.02	0.01	0.01	0.01	0.02	-0.02	-0.02	-0.03	-0.01	0.05	0.03	0.16	0.18	0.46	0.20	0.02	0.04	-0.05
1990s		0.00	-0.01	0.01	0.00	0.01	0.01	0.03	0.01	0.03	0.01	-0.01	-0.01	-0.01	0.00	0.01	0.16	0.18	0.18	0.44	0.01	0.03	-0.08
random_1		-0.01	0.05	-0.01	0.01	0.01	-0.01	0.03	0.00	0.06	-0.04	-0.02	0.02	0.04	-0.05	0.01	-0.02	0.00	0.02	0.01	0.36	0.01	-0.02
random_2		0.00	0.00	-0.01	0.01	-0.01	0.00	0.02	0.00	0.00	0.04	-0.01	-0.04	0.00	-0.02	0.02	0.01	0.01	0.03	0.03	0.00	0.37	-0.04
_cons		-0.03	-0.03	-0.03	-0.03	0.01	-0.01	-0.04	-0.03	-0.22	-0.28	0.05	0.00	0.08	0.16	0.09	-0.05	-0.04	-0.04	-0.07	-0.01	-0.06	0.33

2 (major conf)	c1	c2	ltsc0	nc	ncc1	ncc2	ltsnc	ncts0	llpop	llGDPcap	llGDPc~1	llGDPc~2	llGDPc~0	lYMHEP	ltimei~p	1960s	1970s	1980s	1990s	random_1	random_2	_cons
2|																						
c1	1.00																					
c2	0.84	1.00																				
ltsc0	0.13	0.14	1.00																			
nc	0.73	0.68	-0.39	1.00																		
ncc1	-0.78	-0.64	0.37	-0.94	1.00																	
ncc2	-0.65	-0.76	0.33	-0.88	0.83	1.00																
ltsnc	0.00	0.00	0.01	-0.02	0.00	0.00	1.00															
ncts0	0.60	0.57	-0.51	0.85	-0.80	-0.76	-0.03	1.00														
llpop	-0.03	-0.02	-0.02	-0.04	0.02	0.04	-0.07	0.03	1.00													
llGDPcap	0.09	0.10	0.09	0.04	-0.02	-0.01	0.07	0.03	0.17	1.00												
llGDPcap_c1	-0.23	-0.23	-0.16	-0.05	0.03	0.04	-0.04	-0.07	-0.04	-0.33	1.00											
llGDPcap_c2	-0.18	-0.30	-0.16	-0.03	0.02	0.03	-0.05	-0.05	-0.04	-0.27	0.71	1.00										
llGDPcap_l~0	-0.27	-0.29	-0.50	-0.05	0.03	0.06	-0.02	-0.09	0.04	-0.30	0.62	0.56	1.00									
lYMHEP	-0.02	-0.04	0.01	-0.01	0.03	0.02	-0.10	-0.05	-0.27	-0.50	-0.01	-0.01	-0.04	1.00								
ltimeindep	-0.02	-0.03	-0.02	0.00	0.01	0.02	-0.17	0.01	-0.33	-0.28	-0.15	-0.10	-0.13	0.16	1.00							
1960s	0.02	0.01	0.03	0.01	0.01	0.01	0.14	-0.02	0.00	-0.03	-0.03	-0.04	-0.08	0.19	0.18	1.00						
1970s	0.00	0.01	0.00	0.01	0.03	0.01	0.09	-0.01	0.00	-0.06	-0.08	-0.04	-0.08	0.22	0.17	0.44	1.00					
1980s	-0.03	-0.03	-0.04	-0.02	0.04	0.04	0.03	0.00	0.08	-0.07	-0.08	-0.09	-0.06	0.18	0.10	0.44	0.48	1.00				
1990s	-0.02	-0.02	-0.01	-0.02	0.02	0.02	0.07	0.00	0.09	0.03	-0.04	-0.05	-0.03	-0.01	0.10	0.41	0.44	0.49	1.00			
random_1	-0.06	-0.01	-0.01	0.00	0.02	0.00	0.16	-0.01	0.10	-0.10	-0.06	0.02	0.09	-0.12	-0.03	0.02	0.00	-0.01	-0.02	1.00		
random_2	-0.01	-0.01	0.00	0.01	0.01	0.00	0.00	-0.01	0.20	-0.02	0.00	-0.07	0.02	-0.01	-0.04	0.06	0.06	0.12	0.04	0.20	1.00	
_cons	-0.49	-0.47	-0.07	-0.42	0.38	0.34	-0.08	-0.36	-0.55	-0.66	0.23	0.19	0.23	0.35	0.12	-0.17	-0.14	-0.13	-0.20	0.00	-0.13	1.00

S4 Simulation procedure and data projections

S4.1 Simulation procedure

The general setup of the simulation procedure is summarized below. We use the methodology developed in earlier work [13]. The model is dynamic, allowing us to capture how a simulated conflict in one country at any point in time affects the future conflict risk of the same country as well as of its neighbors. At the core is the matrix of transition probabilities. The transition probabilities (i.e., relative frequencies) observed for the 1960–2013 period are given in Table S6. Among the 6,385 country years that had no conflict at t–1, 6,176 (96.7%) remained at peace at t, 184 (2.9%) transitioned into minor conflict, and 25 (0.4%) transitioned into major conflict. The statistical model described above allows formulating these transition probabilities as functions of the predictors for use in the simulation.

Table S6. Transition probability matrix, 1960–2013

	No conflict at t	Minor conflict at t	Major conflict at t	Total
No conflict at t-1	6,176 (0.967)	184 (0.029)	25 (0.004)	6,385 (1.000)
Minor conflict at t-1	172 (0.198)	613 (0.705)	84 (0.097)	869 (1.000)
Major conflict at t-1	27 (0.069)	80 (0.206)	282 (0.725)	389 (1.000)
Total	6,375 (0.834)	877 (0.115)	391 (0.051)	7,643 (1.000)

Note: Observed number of transitions from state at t–1 (rows) to state at t (columns), with relative transition frequencies expressed as proportions in parentheses.

The procedure consists of a series of steps outlined below and depicted in Fig. S1 (an extended version of Fig. 4 in the article):

1. Specify and estimate the underlying statistical model.
2. Make assumptions about the distribution of values for all exogenous predictor variables for the first year of simulation and about future changes. In this paper, we base the simulations for the predictor variables on the SSPs, described in Section S2.
3. Draw a realization of the country-fixed effects based on the estimate from the multilevel mixed-effects logistic model.
4. Draw a realization of the coefficients of the multinomial logit model based on the estimated coefficients and the variance-covariance matrix for the estimates.
5. Start simulation in first year. The first simulated year is 2014, using starting values from 2013.
6. Calculate the probabilities of transition between conflict levels (as illustrated in Table S6) for all countries for the first year, based on the realized coefficients and the projected values for the predictor variables.
7. Randomly draw whether a country experiences minor or major conflict, based on the estimated transition probabilities.
8. Update the values for the explanatory variables. A number of these variables, most notably those measuring historical experience of conflict and the neighborhood conflict variables, are contingent upon the outcome of step 7.
9. Repeat (5) – (7) for each year in the forecast period, e.g., for 2014–2100, based on predictor values updated based on the outcome of (7), and record the simulated

outcomes. Repeat forty times to even out the impact of individual realizations of the transition probabilities.

10. Repeat (4) – (9) fifteen times to even out the impact of individual realizations of the multinomial logit coefficients.
11. Repeat (3) – (10) fifteen times to even out the impact of individual realizations of the country-specific intercepts.

In total, we get 40 × 15 × 15 = 9,000 simulated outcomes for each year for each country.

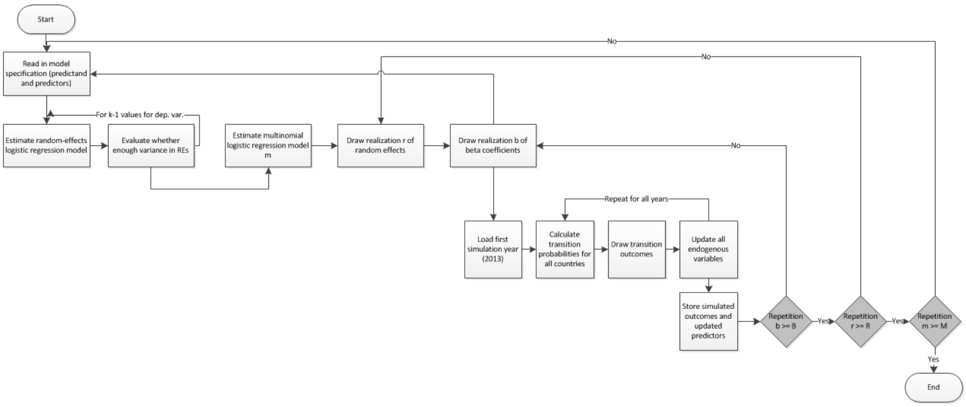


Figure S1. Flow chart of the simulation procedure.

Fig. S1 shows the structure of the general simulation procedure we use. The program reads in the model specification (the multinomial regression model shown in Table S3), estimates separate random-effects models for the *k*-1=2 conflict levels, and estimates the multinomial regression model. The procedure draws *R* realizations of the random effects and *B* realizations of beta coefficients. For each *r*b* combination of realizations, it runs a number of simulations from the first year (2013) to the last (2100), as described above. The procedure allows for running and averaging over *M* different models, but model averaging is not applied in this project.

The simulation procedure has many methodological advantages. Most importantly, it allows modeling the dynamic nature of armed conflicts: If a new conflict is simulated to break out in country *i* in year *t*, the procedure accounts for the fact that this increases the risk of future conflict in that country and its neighbors for decades afterwards. The procedure draws multiple realizations of the model parameters as given by the vector of coefficients (reported in Table S3) and the variance-covariance matrix (Table S5). This allows representing the uncertainty in the statistical estimates underlying the simulations. This uncertainty and the uncertainty originating in drawing outcomes from the estimated transition probability matrix (as illustrated in Table S6) are reflected in the gradually widening prediction intervals shown in Figs. 3 and S10.

Another major advantage is that the procedure allows interpreting the estimated model parameters jointly taking problems with multicollinearity into account. Step 4 in the list above is very close to the procedure in the Clarify software (Tomz et al. 2003) [20], which was designed to handle similar problems.

S4.2 Projections used in the simulation stage

All conflict variables are treated as endogenous variables, and their values are updated in the course of each simulation. This applies to the conflict history dummies (c1, c2), time since the previous conflict (ltsc0), and the neighborhood conflict indicators (nc, ltsnc). Time since independence (ltimeindep) is updated by running a new calculation every year before log transforming. Unlike the historical regression models (Table S3), the simulations do not include any decade dummies but instead assume that the intercept in the future will be similar to the 2000–13 reference category.

The GDP per capita projections were developed by a team at the OECD [21]. The OECD ENV-Growth model is an augmented Solow growth model that takes into account human capital and income from fossil fuels. The model ignores institutional factors that may affect growth performance (beyond time-invariant country effects). We make a few adjustments to the projections as documented in Section S8.

We use version 1.0 of the population projections from IIASA [22]. A newer version 1.1 has been published [23], but the authors recommend using version 1.0 together with the OECD model because the version 1.0 data also is used as input to the OECD-ENV model. We make a few adjustments to the projections as documented in Section S8.

For the education (YMHEP) variable, we use data from the Wittgenstein Centre for Demography and Global Human Capital [23]. We use version 1.1 of this dataset to facilitate matching to the historical data. Details regarding the matching procedure as well as documentation of a few adjustments are given in Section S8.

S5 Out-of-sample evaluation

Table S7 shows the results from an out-of-sample evaluation of the predictive performance of the model as compared to a set of alternative models. We estimated the model corresponding to Model 1 reported in Table S3 for all countries over the 1960–2000 period and ran the simulations as described above for the 2001–13 period (again, for all countries). We then compared the proportion of simulated conflicts (minor or major) for every country for every year 2010, 2011, 2012, and 2013 with the actual occurrence of conflict in these countries. These results are reported as `model 1' in Table S4. The evaluation of Model 2, Table S3 is reported in row 2, and those of nine other models in the remaining rows.

For the evaluation, we construct three dichotomous outcome variables: No conflict vs. conflict, minor conflict vs. not minor conflict, and major conflict vs. minor or no conflict. For each of these outcomes, we report Brier and AUC scores. Models that predict well out of sample obtain low Brier scores and high AUC scores (see [24, 25] for an introduction to these measures). Brier scores cannot be less than 0 and AUC scores cannot be larger than 1. It is, however, somewhat misleading to claim that the AUC scores of 0.90 we obtain constitute very good predictions since they to some extent reflect that any model could do well by simply predicting 'no conflict'. For similar reasons, the scores are comparable across models within the same predicted outcome (no conflict, minor conflict major conflict), but not across different outcomes, since the metrics depend on the distribution of the outcome variables. We report how the 11 models are ranked for each of the six metrics. In order to provide a rough summary of them, we also calculate the sum of ranks in the right-most column of Table S7.

Models 3–11 deviate from Model 1 in various ways. In Model 3, we removed GDP per capita (but left education/YMHEP in). In Model 4, we removed both GDP per capita and and YMHEP. In Model 5, we retained GDP per capita as a main term but removed the interactions and the YMHEP variable. In Model 6, we retained GDP per capita and education as main terms but removed the interactions. Model 7 is Model 1 without log population. Model 8 is Model 1 without decade dummies, and Model 9 is without Time since independence. In Model 10 we removed several terms. Lastly, in Model 11 we added to Model 1 a variable that records the annual deviation from each country's average temperature for the 1970–2000 period, based on data generated through PRIO-GRID [26]. The temperature term was not statistically significant in either equation in the multinomial model, and the out-of-sample predictive performance of Model 11 is uniformly worse than Model 1 across all outcomes and metrics.

Table S7. Out-of-sample evaluation of predictive performance, 2001–2013

Model	Description	No conflict		Minor conflict		Major conflict		Sum of ranks
		Brier	AUC	Brier	AUC	Brier	AUC	
1	Final Model (FM) (Model 1)	.08445 (3)	.90116 (2)	.07911 (4)	.8799 (2)	.03386 (6)	.8516 (3)	20
2	FM without education (Model 2)	.08190 (1)	.9014 (1)	.07711 (1)	.8821 (1)	.03540 (10)	.8502 (4)	18
3	FM without GDP per capita	.08473 (6)	.8936 (7)	.08080 (8)	.8662 (6)	.03297 (3)	.8490 (5)	35
4	FM without GDP per capita and education	.08399 (2)	.8974 (4)	.07956 (5)	.8666 (4)	.03353 (5)	.8343 (10)	30
5	FM without education and GDP per capita interactions	.08449 (5)	.8937 (6)	.07888 (3)	.8665 (5)	.03195 (1)	.8425 (9)	29
6	FM without GDP per capita interactions	.08577 (8)	.8964 (5)	.08216 (11)	.8621 (9)	.03248 (2)	.8482 (7)	42
7	FM without population	.08943 (9)	.8836 (11)	.07960 (6)	.8667 (3)	.03333 (4)	.8336 (11)	44
8	FM without decade dummies	.09143 (11)	.8912 (9)	.08191 (10)	.8596 (10)	.03604 (11)	.8524 (2)	52
9	FM without time since independence	.08446 (4)	.8922 (8)	.07862 (2)	.8634 (8)	.03449 (8)	.8460 (8)	38
10	FM without interactions, time since independence, decade dummies	.09000 (10)	.8884 (10)	.08121 (9)	.8544 (11)	.03495 (9)	.8580 (1)	50
11	FM with temperature deviation	.08500 (7)	.8977 (3)	.08052 (7)	.8634 (7)	.03429 (7)	.8483 (6)	37

Note: Area under the ROC curve (AUC) and Brier scores for the model used in simulations compared with a set of alternative models. Ranks are given in parentheses. Models were trained for 1960–2000 period and evaluated against 2001–2013.

Overall, our two preferred models do better than the others across the various metrics. Model 2 produces the best predictions for no conflict and minor conflict according to both the Brier and AUC scores. Model 1's Brier score for major conflict is among the worst, however. The fact that Model 1 performs poorly for major conflict should be seen in light of the low number of major conflicts in the evaluation period, so the score is quite uncertain. Model 1 never obtains the best score, but does well across all outcomes.

Removing terms from the model clearly hurts predictive performance. The models without decade dummies perform the worst, and removing the population term also hurts prediction.

In [13] we explore a larger set of terms that were not included here. Most important among these are variables denoting oil dependence and the ethnic composition of the country. Since we only have time-invariant data for these predictors, the country-specific random effects included here account for such variation between countries.

S6 Review of the predictors under each of the SSPs

Fig. S2 shows observed population, 1970–2013, and projected population, 2014–2100, for each SSP and for each of nine regions. The definitions of the regions are given in Table S12 below. Global population growth is highest in the Fragmentation (SSP3) and Inequality (SSP4) pathways, and lowest in the Sustainability (SSP1) and Conventional Development (SSP5) pathways. The difference is particularly marked in Central and South Asia and in Africa. SSP 2 is an intermediate scenario.

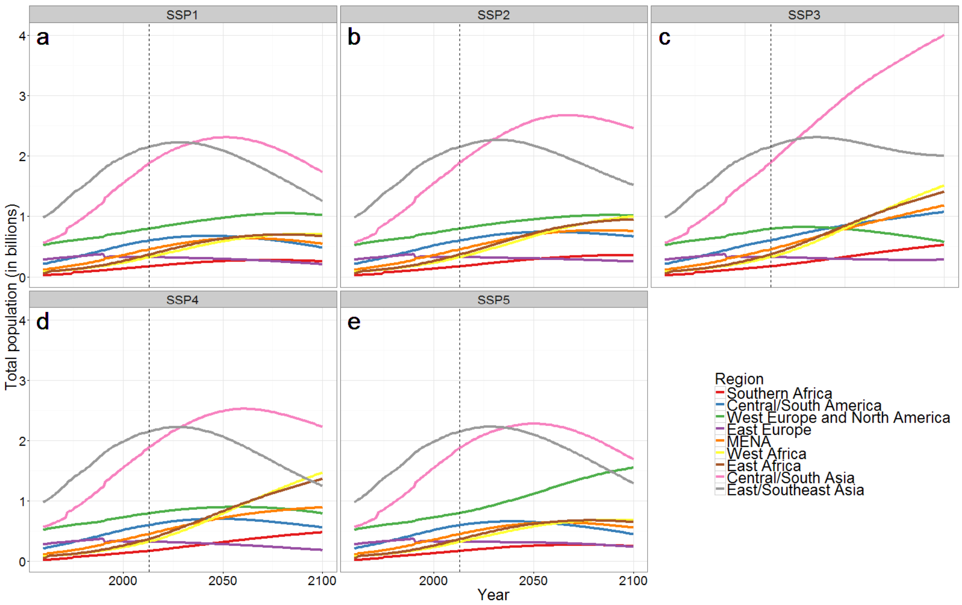


Figure S2. Total population by region and SSP.

Fig. S3 shows observed and projected GDP per capita broken down by region and SSP. GDP per capita growth is highest in SSP5; in 2100, the OECD-ENV model projects all regions to have considerably higher average GDP per capita than current levels in Western Europe and North America. This is also the case for SSP1, although growth is markedly lower. SSP3 has the slowest growth in global average GDP per capita. In SSP4, some regions grow as fast as in SSP1, but regions that are currently poor grow at rates similar to those in SSP3. The Middle-of-the-Road scenario (SSP2) has growth rates only slightly lower than SSP1 and similar to historical trajectories.

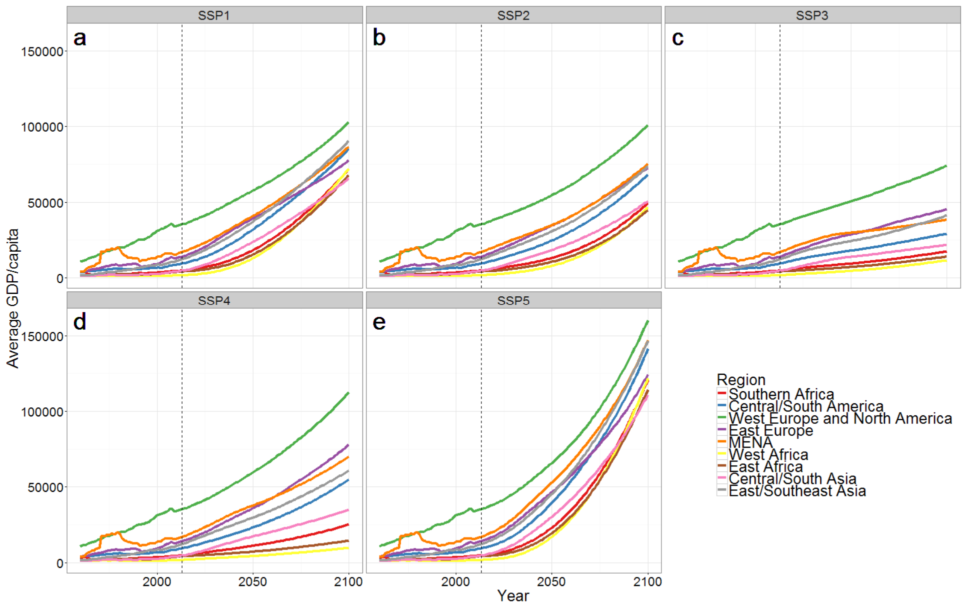


Figure S3. Country average GDP per capita (2005 USD PPP) by region and SSP.

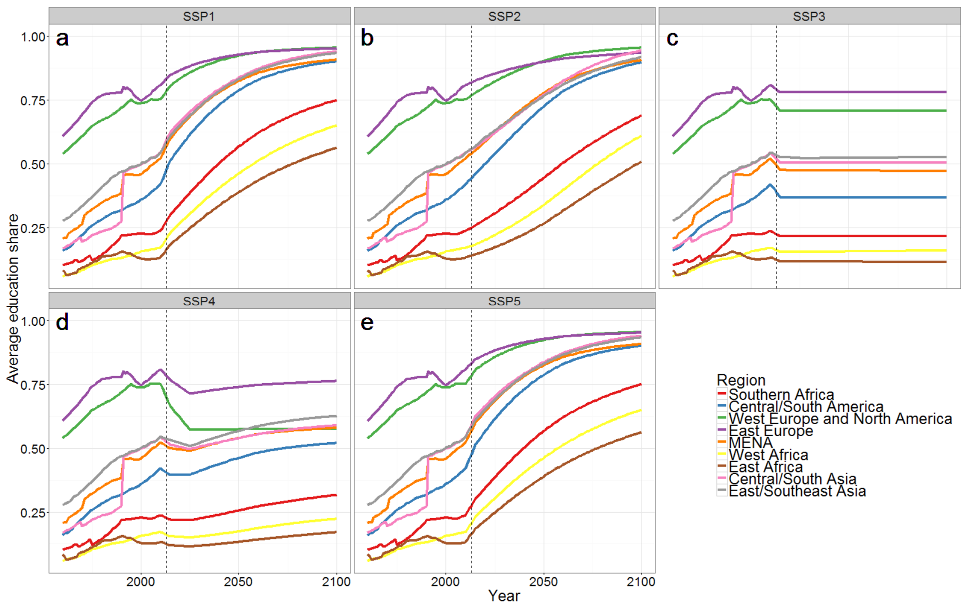


Figure S4. Share of males, age 20-24, with secondary education or higher by region and SSP.

Fig. S4 shows the corresponding observed and projected data for secondary education attainment. In SSP1, SSP2, and SSP5, all regions make substantial progress toward universal secondary education. In SSP3 and SSP4, however, education attainment rates are held virtually constant at levels observed several years period to the beginning of the simulation, hence the notable drop in education attainment in the initial simulation years for these scenarios. Moreover, in SSP4 the extent of this backdating depends on prior levels of education attainment, where more developed countries experience a relatively larger drop. See [27] for a complete description.

S7 Additional simulation results

In this section, we present simulation results broken down at the country level. Figures S5–S9 and S12–S16 display projected risk in the form of maps for each SSP for Models 1 and 2, respectively. All maps were generated using R and ggplot2, while the maps come from the cshapes-package [28,29]. Tables S8 and S9 report the mean point estimates for the probabilities underlying these maps.

S7.1 Model 1 (including education)

This section reports projected conflict risks based on the model that includes both GDP per capita and education attainment (Model 1 in Table S3, the same model as reported in the article).

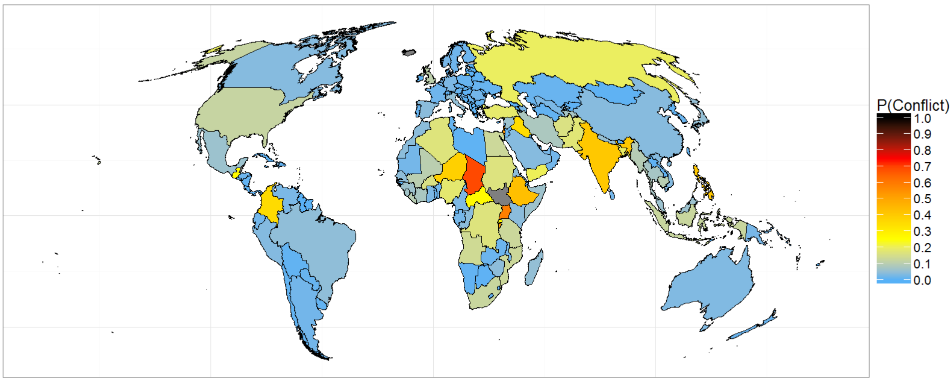


Figure S5. Projected probability of conflict in 2100, SSP1, Model 1.

Figure S5 shows country-level projected risk of minor or major armed conflict at the end of the century given the SSP1 pathway. Most countries have moderate projected conflict risks, including several African countries such as Somalia, Senegal, DR Congo, and Madagascar. The exceptions are a few landlocked or high-population countries, all of them with very violent conflict histories up to 2013, such as Niger, Chad, Ethiopia, India, and Israel, all of which have conflict in 2100 in more than a third of the simulations.

Fig. S6 shows that the corresponding projected probabilities of conflict for SSP2 are slightly higher than for SSP1, but with about the same global distribution.

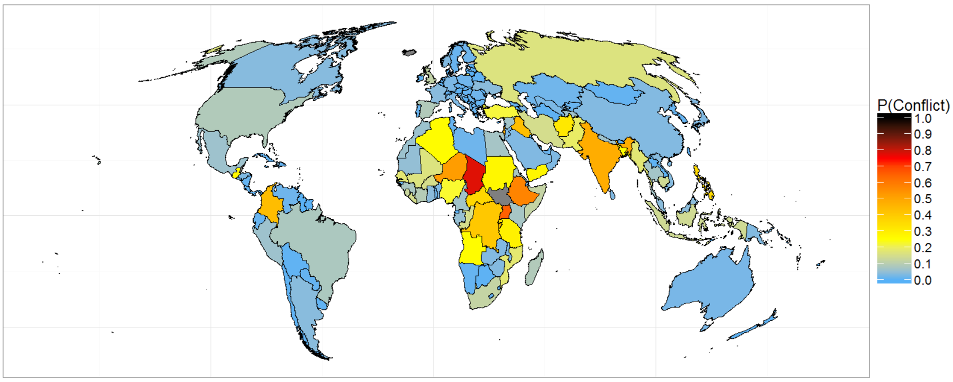


Figure S 6. Projected probability of conflict in 2100, SSP2, Model 1.

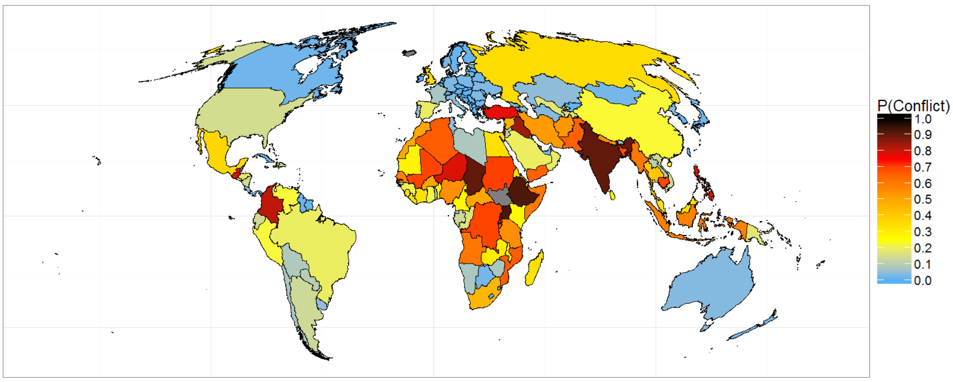


Figure S7. Projected probability of conflict in 2100, SSP3, Model 1.

Fig. S7 reports the projected probabilities given the Fragmentation pathway (SSP3). Here, the projected incidence of conflict is much higher in the developing world than in the previous two SSPs. Conflict propensities are extremely high (i.e., conflict in more than two-thirds of the simulations) in several countries, e.g., Chad, Sudan, Uganda, Ethiopia, and India. Conflict is also likely in many other countries, including Brazil, Russia, Iran, and China. We project a low risk of conflict in North Korea for this SSP as well as all the others. North Korea is a small country in a stable neighborhood with no recent armed conflict. In addition, the data and projections for the country are highly uncertain and possibly overestimate its level of socioeconomic development.

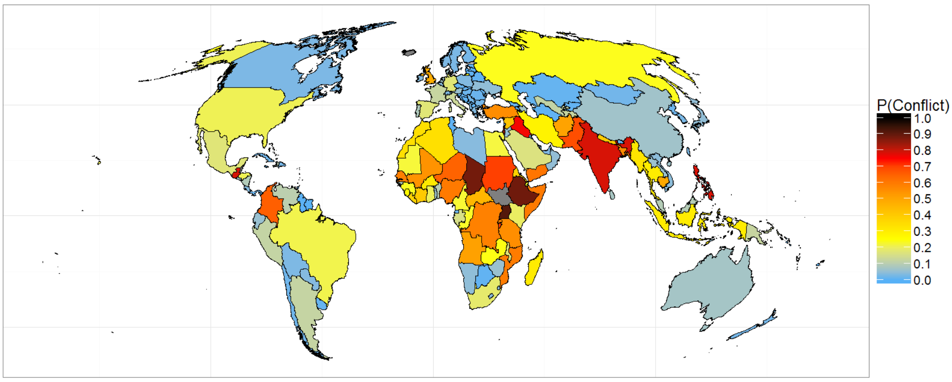


Figure S8. Projected probability of conflict in 2100, SSP4, Model 1.

Fig. S8 shows end-of-century conflict risk for the Inequality pathway (SSP4). In this scenario, projected risks are extremely high for many of the countries that are considered at some risk in other scenarios, such as Chad, Sudan, Uganda, Ethiopia, and India. Because of the more favorable growth trajectories projected for countries that are currently becoming firmly embedded in the global economy, other countries have lower risks of conflict. The lower conflict risk compared to SSP3 is particularly marked for Mexico, South Africa, Vietnam, and China.

Fig. S9 shows projected conflict risks for the Conventional Development pathway. Conflict risks in 2100 are very low in most countries. Main exceptions are countries that have had atypically high levels of conflict up to 2013. Since we include country-specific intercepts in the model, countries such as Chad, Ethiopia, Iraq, Israel, and the Philippines continue to have a relatively high likelihood of armed conflict.

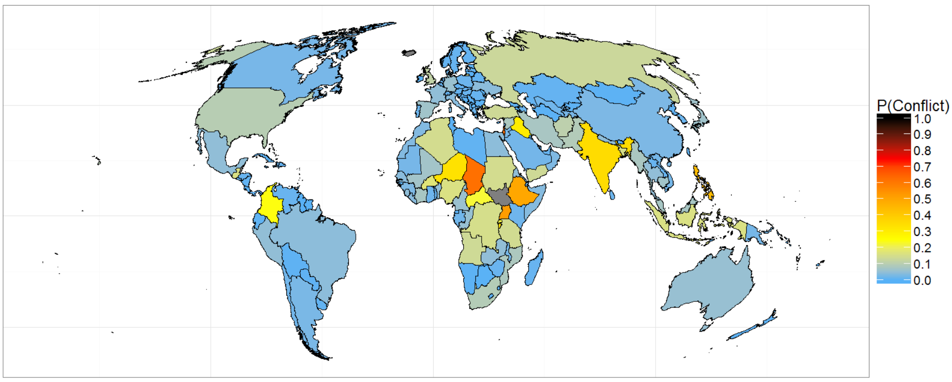


Figure S9. Projected probability of conflict in 2100, SSP5, Model 1.

Table S8 gives a complete list of simulated conflict risk in 2100 for all sample countries, sorted alphabetically. For several countries, the gap in estimated probability between the best (typically SSP5) and the worst (most often SSP3) scenario exceeds an order of magnitude (e.g., Nepal and Senegal).

Table S8. Projected probability of armed conflict in 2100 by country and SSP, Model 1

Country	SSP1	SSP2	SSP3	SSP4	SSP5
Afghanistan	11.8	24.8	46.3	43.1	7.2
Albania	0.6	1.2	2.9	2.8	0.6
Algeria	14.3	24.9	65.2	33.8	12.2
Angola	14.8	28.1	65.0	54.3	14.6
Argentina	1.8	3.9	13.6	11.5	2.4
Armenia	0.3	0.5	0.4	0.7	0.2
Australia	2.7	1.9	3.2	6.1	4.9
Austria	0.9	0.4	0.4	5.4	0.8
Azerbaijan	2.6	2.3	3.7	6.7	1.4
Bahrain	1.1	0.6	5.0	0.8	0.5
Bangladesh	17.0	23.4	68.5	55.5	14.0
Belarus	0.6	0.5	1.6	0.6	0.9
Belgium	1.6	0.3	1.3	3.5	0.7
Benin	1.6	2.8	24.6	10.2	4.5
Bhutan	0.5	0.4	5.8	1.0	0.4
Bolivia	0.6	0.6	7.7	3.1	0.5
Bosnia and Herz.	0.4	0.3	0.6	0.5	0.4
Botswana	0.4	0.5	2.0	0.6	0.2
Brazil	4.0	7.4	19.5	21.5	3.6
Bulgaria	0.7	0.8	1.7	1.9	0.4
Burkina Faso	16.4	15.5	49.2	30.9	17.4
Burundi	51.2	42.6	64.9	70.3	33.2
Cambodia	8.8	12.6	68.0	48.3	6.3
Cameroon	2.5	6.3	26.8	22.9	5.4
Canada	3.1	2.8	1.8	2.3	2.1
Cape Verde	0.4	0.3	1.9	0.8	0.2
Central Afr.Rep.	23.9	33.5	44.7	42.1	21.1
Chad	65.2	75.3	88.9	86.9	58.7
Chile	0.6	0.5	6.8	1.5	1.5
China	3.4	3.3	22.7	6.3	2.4
Colombia	30.1	39.6	79.2	61.8	21.3
Comoros	2.1	3.7	12.5	9.5	2.1
Congo	3.8	15.0	15.7	23.9	5.1
Congo, DRC	13.6	35.0	64.9	51.9	12.4
Costa Rica	0.3	0.7	4.1	2.9	0.4
Cote d'Ivoire	7.9	9.4	33.6	40.0	7.3
Croatia	1.5	2.1	1.8	0.9	0.7
Cuba	0.5	0.8	1.6	2.0	0.4
Cyprus	0.3	0.4	0.8	0.3	0.4
Czech Republic	0.8	0.9	0.7	0.9	0.6
Denmark	0.8	0.7	1.7	6.9	1.4
Djibouti	2.1	2.0	19.9	22.6	1.9
Dominican Republic	0.7	0.9	8.9	2.2	0.8
Ecuador	1.5	1.0	13.7	5.3	0.6
Egypt	12.4	6.5	32.8	22.5	3.9
El Salvador	0.9	1.3	15.0	6.2	0.8
Equatorial Guinea	0.4	0.4	4.1	1.5	0.3
Eritrea	3.9	7.2	35.6	38.2	4.3
Estonia	0.4	0.4	0.8	0.9	0.5
Ethiopia	35.7	51.6	90.5	87.0	41.7
Fiji	0.2	0.3	0.9	0.4	0.2
Finland	1.3	1.4	0.8	1.9	0.8
France	1.6	3.7	7.0	11.8	4.3
Gabon	2.1	4.9	13.7	14.9	1.8
Georgia	0.4	1.0	2.5	1.0	0.7
Germany	2.5	2.4	2.4	16.2	5.3
Ghana	2.6	4.1	22.8	22.3	1.6
Greece	0.5	0.9	1.4	3.2	1.2
Guatemala	23.8	25.8	77.6	78.5	14.3
Guinea	6.5	7.4	21.4	25.3	2.7
Guinea-Bissau	0.4	1.1	12.9	8.7	0.4
Guyana	0.1	0.2	1.0	0.4	0.1
Haiti	3.7	4.4	21.3	10.1	1.5
Honduras	1.5	2.8	18.6	21.0	1.1
Hungary	0.5	0.8	0.5	1.3	1.2
India	35.4	44.4	88.8	76.3	30.1
Indonesia	13.8	15.4	62.6	32.9	16.4
Iran	6.7	13.1	50.7	26.8	5.9
Iraq	30.9	40.2	83.2	73.4	26.3
Ireland	0.6	0.6	0.6	3.6	0.3
Israel	53.8	47.9	86.8	86.3	61.9
Italy	4.0	2.6	4.0	11.5	3.2
Jamaica	0.3	0.4	3.8	0.6	0.3
Japan	4.2	2.3	1.5	4.1	5.5
Jordan	1.1	2.5	10.1	4.8	2.2
Kazakhstan	1.1	1.4	4.5	1.1	0.9
Kenya	3.4	6.7	25.6	19.5	1.6
Kosovo	0.4	0.3	0.5	0.4	0.3
Kuwait	0.7	0.7	2.2	1.5	0.4
Kyrgyzstan	0.5	1.0	0.8	0.8	0.5
Laos	0.5	0.6	9.5	5.6	0.9
Latvia	0.3	0.6	0.6	0.3	0.8
Lebanon	1.0	0.8	7.3	3.3	1.0
Lesotho	0.7	0.7	2.4	3.7	0.4
Liberia	9.0	13.0	29.4	33.6	3.5
Libya	0.5	1.2	6.6	3.2	0.6
Lithuania	0.3	0.3	1.6	0.7	0.3
Luxembourg	0.4	0.2	0.6	1.4	0.2
Macedonia	0.3	0.8	1.1	0.6	0.6
Madagascar	4.6	8.4	31.6	29.0	1.1
Malawi	2.1	2.6	28.7	16.4	2.0
Malaysia	3.7	2.9	27.4	6.6	4.9
Mali	8.1	13.5	66.1	51.0	5.4
Mauritania	1.7	4.3	25.8	21.5	2.0
Mauritius	0.3	0.2	2.3	1.3	0.3
Mexico	5.3	6.1	36.1	17.3	4.1
Moldova	0.4	0.3	1.2	0.6	0.3
Mongolia	0.5	0.8	2.0	1.8	0.5
Montenegro	0.2	0.3	0.4	0.5	0.5
Morocco	4.3	6.2	51.1	35.6	3.3
Mozambique	10.5	15.7	62.0	55.2	6.3
Myanmar	6.9	13.4	54.4	27.8	5.4

Country	SSP1	SSP2	SSP3	SSP4	SSP5
Namibia	0.6	0.9	8.0	4.5	0.4
Nepal	5.0	5.7	52.4	37.0	4.0
Netherlands	1.9	0.6	2.3	6.2	1.3
New Zealand	0.9	0.3	2.1	1.8	0.4
Nicaragua	0.8	1.1	14.9	10.0	0.7
Niger	36.6	49.9	77.2	62.4	32.5
Nigeria	16.1	20.4	52.5	56.0	13.9
North Korea	0.6	0.9	3.7	1.7	0.5
Norway	2.2	3.1	2.7	2.7	0.8
Oman	1.8	2.8	17.8	4.2	2.8
Pakistan	15.9	17.5	63.1	66.9	7.5
Panama	0.8	0.8	2.3	1.5	0.8
Papua New Guinea	2.1	3.2	17.9	12.3	2.0
Paraguay	1.0	0.8	9.3	3.3	0.6
Peru	2.7	5.2	17.8	9.0	4.0
Philippines	44.9	41.0	81.6	80.5	48.0
Poland	0.9	1.7	2.3	3.0	1.6
Portugal	0.9	0.8	5.1	7.5	1.4
Qatar	0.4	0.4	9.9	3.2	0.4
Romania	1.8	1.3	2.9	1.2	1.0
Russia	19.4	15.9	31.6	22.4	12.0
Rwanda	24.2	30.3	74.9	56.8	20.3
Saudi Arabia	3.0	2.8	19.6	15.5	1.7
Senegal	4.1	17.2	58.9	59.6	3.7
Serbia	1.1	1.1	1.2	1.9	0.6
Sierra Leone	7.2	12.8	35.1	28.9	4.3
Singapore	0.4	0.4	0.6	0.4	0.4
Slovakia	0.6	0.9	0.5	0.8	1.1
Slovenia	0.2	1.6	0.4	0.4	0.4
Solomon Is.	0.2	0.3	1.8	1.3	0.3
Somalia	4.3	8.3	53.0	53.8	2.4
South Africa	9.6	9.8	43.3	17.2	7.4
South Korea	0.9	0.8	0.6	2.2	1.1
Spain	4.1	9.3	20.8	18.7	6.8

Country	SSP1	SSP2	SSP3	SSP4	SSP5
Sri Lanka	2.6	4.3	31.8	8.4	2.1
Sudan	15.2	26.2	68.7	68.2	13.4
Suriname	0.3	0.3	1.3	0.3	0.3
Swaziland	0.6	0.4	2.3	4.0	0.4
Sweden	1.4	1.0	0.9	2.6	1.0
Switzerland	0.5	0.7	1.1	2.6	1.0
Syria	5.1	5.3	40.3	37.0	6.4
Taiwan	0.6	0.6	2.0	2.2	0.7
Tajikistan	7.0	5.5	14.7	10.9	2.3
Tanzania	12.9	29.1	54.7	54.4	13.9
Thailand	5.0	4.8	35.3	24.6	3.5
The Gambia	0.4	0.6	9.3	6.4	0.5
Timor Leste	0.4	0.4	4.0	5.2	0.3
Togo	0.9	1.4	9.4	6.5	0.9
Trinidad and Tobago	0.2	0.2	1.3	0.4	0.2
Tunisia	0.9	0.9	5.7	0.7	0.4
Turkey	14.7	18.6	73.2	49.9	10.1
Turkmenistan	0.7	0.5	3.0	0.7	0.4
Uganda	53.7	61.2	91.2	90.2	46.9
Ukraine	0.9	3.8	2.9	4.1	4.0
United Arab Emirates	2.2	1.1	1.8	2.6	1.0
United Kingdom	12.1	12.9	39.2	50.8	12.4
United States	11.0	8.4	14.0	20.5	9.4
Uruguay	0.7	0.5	4.3	1.0	0.7
Uzbekistan	2.3	2.5	16.0	10.9	1.5
Venezuela	2.3	1.2	22.0	9.8	1.1
Vietnam	3.3	3.3	13.0	4.0	1.7
Yemen	20.5	26.1	62.4	58.3	13.9
Zambia	1.3	3.1	30.3	24.6	4.5
Zimbabwe	4.4	2.9	7.2	5.3	1.1

S7.2 Model 2 (excluding education)

Fig. S10 visualizes the aggregate global results from the simulation without education (Model 2) and is analogous to Fig. 2 in the article that is based on Model 1. Overall, the projected risks are lower since GDP per capita, which is assumed to grow monotonically across all scenarios, has a much larger estimated effect in the absence of education. This is especially clear for SSP3 and SSP4, which have very pessimistic expectations for education levels.

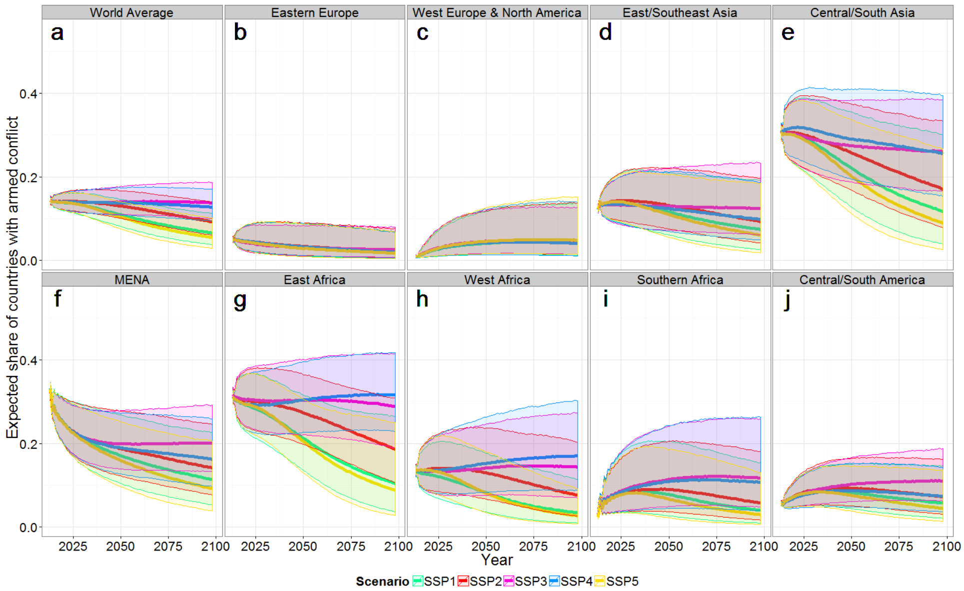


Figure S10. Projected proportion of countries in armed conflict by scenario and year, 2014–2100, Model 2. Panel **a** shows the world average results; panels **b–j** show regionally disaggregated estimates. Shaded areas represent the 50% prediction intervals.

Fig. S11 shows the difference in estimated end-of-century conflict risk between SSP3 and SSP1. The figure is comparable to Fig. 3 in the main article, but is based on Model 2 without education. Since the average incidence of conflict is lower, Fig. S11 indicates a more modest difference between the two pathways. Given this model, the countries that benefit the most from the more optimistic Sustainability pathway are those that currently have very low levels of GDP per capita and recent conflict histories (e.g., Chad, Ethiopia, and Iraq).

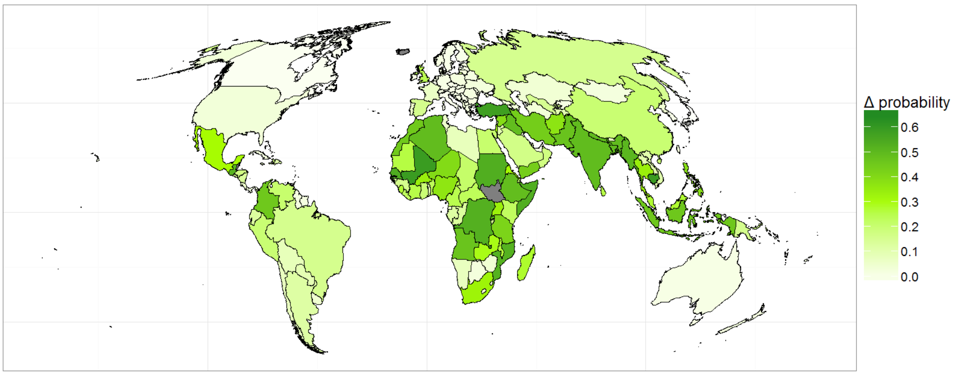


Figure S11. Map of country-specific differences in estimated conflict risk between SSP1 and SSP3 in 2100, Model 2. Darker shades indicate larger benefits in terms of reduced conflict risk by shifting from the adverse regional Fragmentation scenario (SSP3) to a sustainable growth scenario (SSP1). Countries in grey have insufficient historical data to be included in the forecasting model.

Figs. S12–S16 show end-of-century projected conflict risks for each country for each of the SSPs, based on Model 2. Table S9 reports the same information in tabular form.

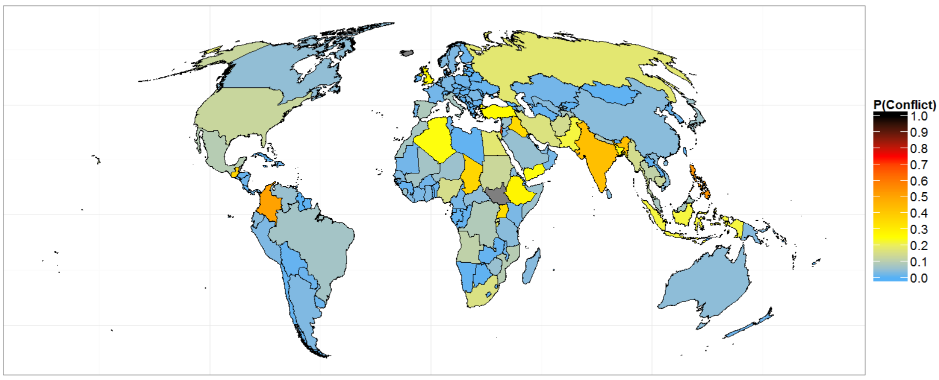


Figure S12. Projected probability of conflict in 2100, SSP1, Model 2.

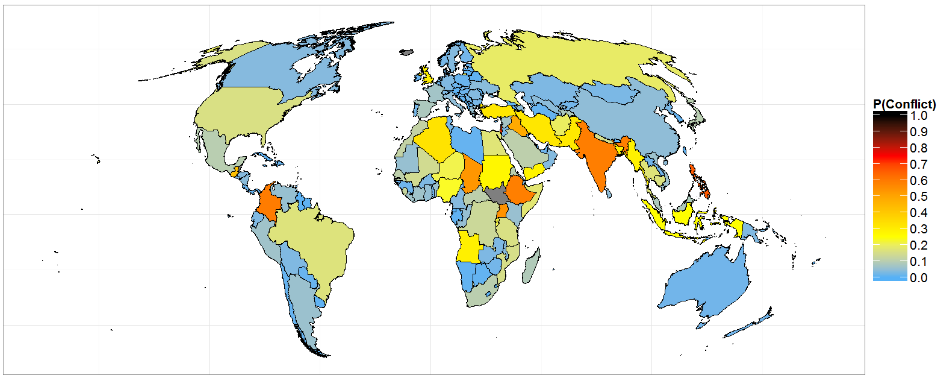


Figure S13. Projected probability of conflict in 2100, SSP2, Model 2.

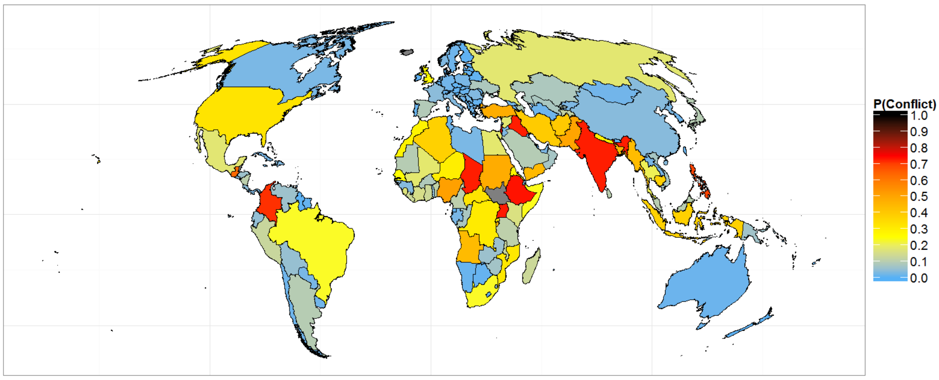


Figure S14. Projected probability of conflict in 2100, SSP3, Model 2.

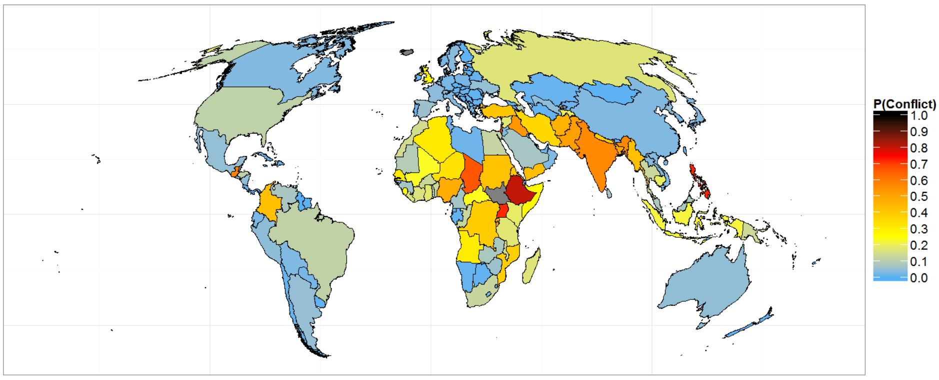


Figure S15. Projected probability of conflict in 2100, SSP4, Model 2.

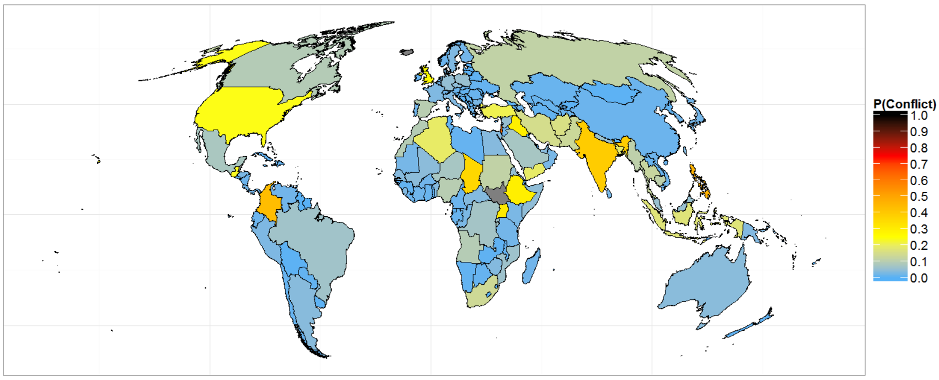


Figure S16. Projected probability of conflict in 2100, SSP5, Model 2.

Table S9. Projected probability of armed conflict in 2100 by country and SSP, Model 2.

Country	SSP1	SSP2	SSP3	SSP4	SSP5
Afghanistan	14.3	19.9	40.0	46.1	15.8
Albania	0.9	0.6	0.7	2.6	0.9
Algeria	24.8	32.8	37.8	30.8	19.2
Angola	10.9	29.0	43.9	30.1	9.4
Argentina	3.0	5.8	9.5	5.1	3.7
Armenia	0.7	0.6	1.6	0.4	0.3
Australia	4.3	1.8	1.5	4.9	4.0
Austria	1.3	0.6	1.0	1.4	1.5
Azerbaijan	4.8	4.5	8.8	8.7	1.9
Bahrain	2.0	0.5	0.8	0.5	0.4
Bangladesh	24.7	29.7	45.9	49.1	18.6
Belarus	0.8	0.5	0.8	0.9	0.6
Belgium	3.7	1.8	1.2	4.5	2.5
Benin	1.5	5.7	11.3	13.8	3.3
Bhutan	0.6	0.4	0.6	0.4	0.3
Bolivia	1.0	3.1	5.4	3.0	0.5
Bosnia and Herz.	0.8	0.8	1.7	0.5	0.3
Botswana	0.5	0.7	1.0	0.8	1.0
Brazil	7.0	16.7	24.0	11.2	6.7
Bulgaria	1.4	2.5	1.1	1.2	2.3
Burkina Faso	1.7	2.8	8.5	12.9	1.8
Burundi	19.4	22.1	45.0	49.2	7.1
Cambodia	12.9	16.4	38.2	19.7	10.9
Cameroon	2.2	3.6	10.7	6.9	1.4
Canada	4.8	3.7	2.5	3.3	9.3
Cape Verde	0.4	0.5	0.5	0.3	0.2
Central Afr. Rep.	4.6	10.7	32.5	24.5	3.3
Chad	36.4	53.7	73.7	67.0	36.2
Chile	0.9	0.9	1.6	1.0	1.1
China	4.0	4.6	3.8	3.3	1.5
Colombia	50.5	59.6	72.2	42.7	43.4
Comoros	0.5	0.8	6.1	5.9	0.4
Congo	1.0	5.2	7.7	12.9	2.5
Congo, DRC	8.8	13.2	30.4	39.2	7.0
Costa Rica	0.5	1.3	4.4	1.0	2.0
Cote d'Ivoire	2.5	6.4	15.2	16.3	1.6
Croatia	4.0	1.8	4.9	1.7	1.5
Cuba	0.6	0.7	2.5	1.9	0.9
Cyprus	0.5	0.5	0.6	0.6	0.3
Czech Rep.	1.2	1.5	0.8	0.4	4.4
Denmark	1.2	1.9	1.2	1.2	2.9
Djibouti	2.1	3.6	3.4	4.7	1.4
Dominican Rep.	1.3	1.2	3.2	0.8	0.9
Ecuador	3.7	3.0	5.0	2.6	1.5
Egypt	18.0	17.5	18.5	12.0	3.7
El Salvador	1.6	2.9	4.0	3.6	0.6
Equatorial Guinea	0.5	0.3	0.9	0.4	0.3
Eritrea	4.2	10.5	27.1	39.5	5.1
Estonia	0.5	1.2	0.4	0.6	0.2
Ethiopia	27.6	58.8	74.5	80.6	29.4
Fiji	0.2	0.3	1.3	0.6	0.2
Finland	1.6	2.0	0.6	0.7	2.0
France	2.0	6.9	4.3	4.2	3.1
Gabon	0.8	0.7	2.4	0.9	1.3
Georgia	0.5	0.8	2.3	0.8	0.7
Germany	2.6	3.0	1.8	2.4	5.0
Ghana	2.9	5.1	13.4	18.4	1.2
Greece	0.8	5.3	1.8	2.8	1.6
Guatemala	36.6	42.6	63.3	58.3	24.1
Guinea	1.1	1.6	3.6	7.0	0.8
Guinea-Bissau	0.5	1.6	4.8	4.9	0.6
Guyana	0.2	0.4	0.5	0.3	0.1
Haiti	3.8	1.9	4.0	7.4	1.6
Honduras	1.1	3.6	6.1	11.3	2.5
Hungary	0.8	1.5	1.3	1.0	0.8
India	42.7	59.1	73.8	56.6	39.2
Indonesia	22.6	25.2	37.8	22.5	17.2
Iran	16.5	31.6	39.3	35.8	14.8
Iraq	35.2	48.6	73.8	53.4	29.0
Ireland	1.0	1.2	0.6	1.4	1.0
Israel	67.5	71.2	79.3	67.6	61.3
Italy	7.5	4.7	3.3	3.4	3.2
Jamaica	0.5	0.4	2.3	0.6	0.5
Japan	6.5	10.0	9.7	3.3	2.0
Jordan	2.1	2.6	2.8	3.2	1.9
Kazakhstan	1.9	2.9	8.3	1.2	1.3
Kenya	2.6	5.6	16.2	19.3	2.7
Kosovo	1.3	1.1	1.4	0.8	1.8
Kuwait	1.2	2.4	2.4	0.9	0.6
Kyrgyzstan	0.6	2.1	2.8	1.4	0.5
Laos	0.6	1.7	2.4	3.6	1.2
Latvia	0.4	0.6	0.5	0.6	0.4
Lebanon	2.2	4.7	3.8	4.8	1.6
Lesotho	0.8	0.3	0.9	2.3	0.3
Liberia	3.2	6.6	13.2	16.8	1.4
Libya	0.8	0.9	2.6	1.7	0.9
Lithuania	0.4	0.9	1.0	0.5	0.4
Luxembourg	0.7	0.3	0.6	0.4	0.3
Macedonia	0.5	1.5	1.9	0.8	0.5
Madagascar	3.6	8.8	13.1	17.0	1.8
Malawi	1.7	3.9	12.8	15.9	1.0
Malaysia	7.8	8.3	9.9	8.3	5.6
Mali	6.9	14.2	18.4	24.1	4.2
Mauritania	2.0	5.4	9.7	9.3	2.2
Mauritius	0.3	0.3	0.7	0.4	0.2
Mexico	9.7	10.0	17.9	5.0	7.8
Moldova	0.6	0.5	1.2	0.7	0.4
Mongolia	0.6	2.6	1.8	0.5	0.4
Montenegro	0.3	0.3	0.8	0.3	0.3
Morocco	7.4	11.7	28.1	14.5	9.4
Mozambique	9.6	15.2	31.4	38.4	6.2
Myanmar	14.6	28.8	43.7	42.5	10.8
Namibia	0.8	0.7	1.2	0.9	0.7
Nepal	9.1	14.8	29.0	39.0	5.7
Netherlands	3.7	2.5	2.0	2.2	1.8
New Zealand	1.7	1.9	0.5	0.7	0.7
Nicaragua	1.6	4.0	10.7	4.5	1.4
Niger	6.0	21.5	29.6	33.4	7.4
Nigeria	15.1	23.9	51.3	47.8	9.8
North Korea	1.1	2.4	2.9	4.7	0.8

Country	SSP1	SSP2	SSP3	SSP4	SSP5
Norway	3.3	2.1	3.0	2.4	1.4
Oman	2.1	3.3	7.1	3.0	2.4
Pakistan	22.5	31.3	49.8	51.5	13.2
Panama	2.2	0.7	2.7	0.5	1.7
Papua New Guinea	3.7	3.1	6.5	13.6	3.1
Paraguay	1.3	1.2	2.4	2.2	0.7
Peru	2.8	6.4	12.6	4.4	2.7
Philippines	54.2	66.4	69.8	73.4	46.3
Poland	1.5	1.1	2.5	2.3	5.6
Portugal	2.0	2.2	1.1	1.3	1.7
Qatar	0.5	0.3	0.5	0.3	0.5
Romania	2.7	3.3	3.2	0.8	1.3
Russia	18.0	19.1	18.1	17.0	11.4
Rwanda	13.1	22.7	29.6	42.9	6.4
Saudi Arabia	5.3	10.7	9.3	7.4	7.4
Senegal	3.3	10.8	25.0	30.6	3.7
Serbia	2.1	2.1	2.1	1.3	0.7
Sierra Leone	6.3	12.5	16.8	24.6	3.3
Singapore	0.6	0.8	0.4	0.4	0.4
Slovakia	1.1	2.1	1.1	0.6	1.2
Slovenia	0.4	0.5	0.4	1.3	0.5
Solomon Is.	0.4	0.4	2.0	0.9	0.3
Somalia	5.9	17.8	24.4	26.5	4.5
South Africa	16.0	10.3	24.0	12.4	13.5
South Korea	1.5	0.9	1.5	2.8	1.8
Spain	8.5	8.1	9.0	6.1	9.9
Sri Lanka	4.1	4.7	10.1	7.0	2.3
Sudan	13.0	27.2	47.9	41.9	11.4
Suriname	0.6	0.6	1.0	0.6	0.3
Swaziland	0.7	0.4	5.0	1.3	0.2

Country	SSP1	SSP2	SSP3	SSP4	SSP5
Sweden	2.7	3.3	1.6	3.7	3.0
Switzerland	0.8	3.1	1.1	4.5	1.9
Syria	5.8	7.2	18.0	17.5	5.4
Taiwan	0.8	0.6	1.1	0.8	3.4
Tajikistan	7.8	8.5	11.6	20.8	1.5
Tanzania	4.0	15.8	10.7	17.7	2.1
Thailand	11.2	17.3	20.7	12.7	12.5
The Gambia	0.6	1.5	3.8	2.1	0.5
Timor Leste	0.5	0.7	5.5	1.9	0.2
Togo	0.8	1.7	4.9	9.6	0.6
Trinidad and Tobago	0.3	0.6	1.5	0.4	0.3
Tunisia	1.9	2.4	1.9	2.0	1.0
Turkey	26.1	31.5	47.8	40.8	21.2
Turkmenistan	1.0	1.0	1.6	0.9	0.9
Uganda	34.4	54.5	73.9	73.0	30.9
Ukraine	1.1	4.1	8.3	5.0	1.1
United Arab Emirates	3.6	1.7	2.8	1.6	4.0
United Kingdom	26.0	29.1	26.1	26.0	25.5
United States	12.7	15.8	32.6	11.1	24.5
Uruguay	1.2	0.5	1.8	0.6	0.5
Uzbekistan	3.8	4.2	7.8	4.5	1.2
Venezuela	5.7	5.4	6.2	7.6	1.7
Vietnam	4.5	6.1	7.5	3.7	1.7
Yemen	24.7	28.0	44.3	43.2	19.2
Zambia	0.9	2.8	6.0	7.8	0.6
Zimbabwe	5.6	2.0	6.7	6.2	3.5

S7.3 Some simulations with alternative scenarios

The operationalization of the five SSPs spans only a limited set of possible future scenarios. In Fig. S17, we show the projections for three variations of the SSPs where we have adjusted some underlying assumptions. The green and pink lines represent the original Sustainability (SSP1) and Fragmentation (SSP3) pathways. These projections are the same as in Fig. 3 in the main article and are included here for comparison.

The first additional scenario is labeled 'SSP3Nogrowth'. Here, we assume that no country has any growth in GDP per capita between 2013 and 2100, whereas population and education behaves similarly to the original. The forecasted incidence of armed conflict is somewhat higher than in SSP3, but not dramatically so – the strong population increase and weak economic growth already makes this is a high-conflict scenario. A scenario with systematic negative growth in GDP per capita (i.e., a scenario where economic growth is slower than population growth) would have yielded a higher proportion of countries in conflict.

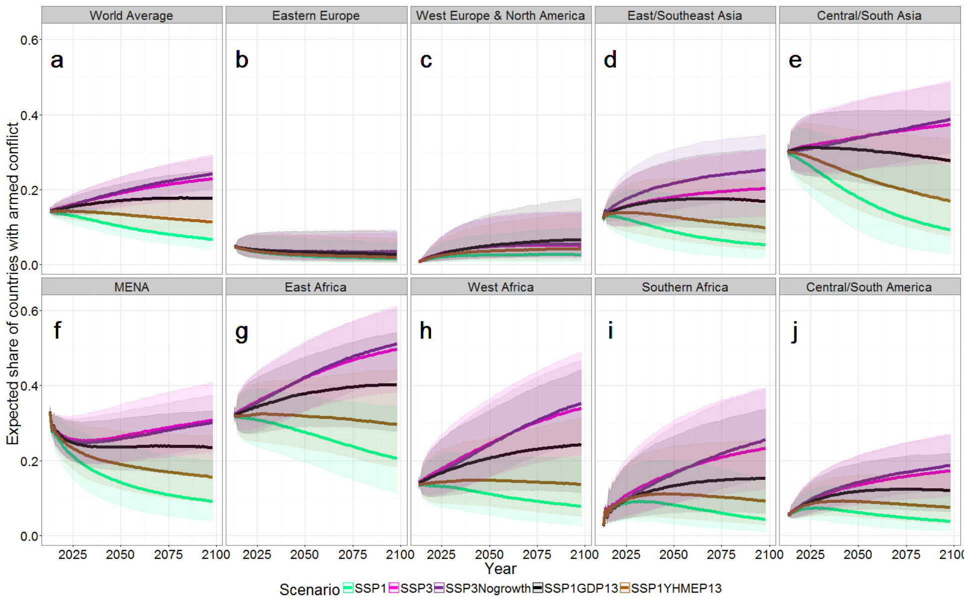


Figure S17. Projected proportion of countries in armed conflict by scenario and year, 2014–2100, Model 2, alternative scenarios. Panel **a** shows the world average results; panels **b–j** show regionally disaggregated estimates. Shaded areas represent the 50% prediction intervals.

The second additional scenario is labeled 'SSP1GDP13'. Here, GDP per capita is the average of GDP per capita in SSP1 and SSP3, but population and education/YMHEP are as in SSP1. This scenario yields a predicted proportion of countries in conflict somewhat higher than the midpoints between SSP1 and SSP3.

The third additional scenario is labeled 'SSP1YMHEP13'. Here, population and GDP per capita are as in SSP1, but YMHEP is the average of those projected in SSP1 and SSP3. The predicted incidence of armed conflict here is between those of SSP1 and SSP3. Comparing the two additional intermediate scenarios, they demonstrate that the variation in GDP per capita between scenarios covers a much wider range than the variation in education assumptions: Compared to the conflict predictions for SSP1, moving to the midpoint between SSP1 and SSP3 in terms of GDP per capita increases conflict incidence much more than moving to the midpoint in terms of education. This does not necessarily imply that GDP per capita is more important than education, however, only that there is more variation spanned in terms of the economic indicator.

S8 Adjustments to historical data and projections

S8.1 Conflict data

We made one slight modification to the conflict data. In the original UCDP/PRIO dataset, the attacks of 9/11 2001 are coded as a major civil armed conflict in the US, since the event satisfies the criteria for being classified as such: An organized non-state actor (Al-Qaeda) targeted the government of an independent state (the United States of America), and these strikes resulted in more than 1,000 casualties. In the UCDP/PRIO database, the subsequent US military operations in Afghanistan and elsewhere against Al-Qaeda troops are coded as a continuation of this conflict, meaning that the US is coded with a civil armed conflict in every year since 2001. We consider this a very special case (i.e., the fighting post-9/11 occurs exclusively outside the territory of the government involved in the conflict, unlike every other civil conflict in the database) and we thus decided to code the US as hosting a civil conflict in 2001 only.

S8.2 Education

The historical education data (1970–2010) are from the IIASA [22] whereas the projections (2010–2100) are obtained from Wittgenstein Centre for Demography and Global Human Capital [23]. We use version 1.1 of the projected education data. The earlier version (1.0) was used as input in the OECD ENV-Growth model. We would have preferred to use the same version to maximize consistency, but reconciling the historical data with version 1.1 projections was much less problematic since the end-point for historical data and start point for projections were the same (2010).

From these datasets, we used the measure 'number of males between 20–24 who have completed upper secondary school or higher'. We calculate the share of the relevant population with completed secondary by dividing by the number of educated males by the total size of the age cohort from the same source. Since the historical education data only cover years since 1970, we extrapolated country-specific values back to 1960 assuming similar country-level change rates as for 1970–2010.

These sources report data for current political entities also for periods before they were created. To make the data consistent with the Gleditsch and Ward historical list of independent states [6], used for this project, we reconstructed observations for states that do not longer exist. Hence, Montenegro, FYR Macedonia, Croatia, Bosnia and Herzegovina, Slovenia, and Serbia were merged into Yugoslavia for the period 1970–1990, using population-weighted averaging of the individual estimates. Similarly, we merged Kazakhstan, Kyrgyzstan, Tajikistan, Turkmenistan, Azerbaijan, Georgia, Armenia, Belarus, Ukraine, Lithuania, Latvia, Estonia, Republic of Moldova, and Russia into the Soviet Union for the period 1970–1990; and the Czech Republic and Slovakia into Czechoslovakia 1970–1990. In addition, we merged Israel and the Occupied Palestinian Territories into a single entity for the entire period 1970–2100 (corresponding to how UCDP/PRIO Armed Conflict Dataset codes the Israel/Palestine conflict).

Kosovo, Taiwan, and South Sudan are missing from both the historical and the projected education datasets. For Kosovo, we assume the education levels to be similar to Bosnia-Herzegovina's. We assume the series for Taiwan are similar to those of Malaysia.

In addition, 19 countries that are included in the projected time-series are missing from the historical dataset. In order to estimate values for these cases, we consulted the Barro-Lee education dataset (version 1.3, 2013) [30] and UNESCO's December 2013 release of their Educational Attainment data [31]. Based on these sources, we filled in values by matching missing countries with countries with similar profiles that have education data. Table S10 lists the country matches chosen.

Table S10. Matching cases to replace missing information in the historical datasets

Country with missing observations in the IIASA or WDI datasets	Country selected as match for education data	Countries selected as match for conversion rates
Afghanistan	Pakistan	
Angola	Mozambique	
Barbados	Jamaica	
Botswana	Zimbabwe	
Brunei	Philippines	
Cuba		Dominican Republic
Czechoslovakia		Czech Republic
Djibouti	Ethiopia	
East Germany		West Germany
Eritrea	Ethiopia	
Fiji	Malaysia	
Kosovo	Bosnia-Herzegovina	
Libya	Egypt	
Mauritania	Morocco	
North Korea	Cambodia	Nepal (North Korea completely lacked data)
Oman	Bahrain	
Palestine		Jordan
Papua New Guinea	Laos	
Solomon Islands	Laos	
Somalia		Ethiopia
South Sudan	Sudan	Sudan (South Sudan completely lacked data)
Sri Lanka	Philippines	
Taiwan	Malaysia	South Korea
Taiwan/Malaysia	Malaysia	
Togo	Nigeria	
Uzbekistan	Tajikistan	
Yemen	Ethiopia	
Yugoslavia		FYR Macedonia
Zimbabwe		Zambia

S8.3 GDP per capita

To construct a complete historical dataset for GDP per capita, we combined data from World Development Indicators (WDI) [16], Maddison [18], and Penn World Tables v.8.0 [17]. The OECD ENV-Growth projections [21] we use for 2011 and beyond are in PPP-adjusted 2005 US Dollars (USD) based on WDI. Where WDI were missing data, we supplemented with data from the two other sources and rescaled values to have the same metric. We applied a simple conversion rate, calculated as the average of the ratio between the time series for overlapping years.

For eight countries, we lacked PPP data from WDI and could not calculate this ratio directly. Here, we used conversion rates from similar countries to translate these data to 2005 USD

PPP values, see Table S5. We interpolated values for years where needed and possible. GDP per capita values are usually generated by making a comprehensive survey for base years, while only deriving changes in sectors between base years [32]. Thus, for most years, even the main raw data are growth rates, making our procedure more suitable than if GDP per capita where derived each year from absolute numbers.

While GDP per capita observations for most countries are generally consistent across the three data sources, we observe large differences for Kuwait, Qatar, and the United Arab Emirates (UAE). For instance, in 1980, the UAE have USD 123,433 (2005 PPP) in WDI, while Maddison reports USD 27,709 (1990 PPP). Similar figures for 2010 are USD 37,688 (2005 PPP) and USD 13,746 (1990 PPP), respectively. This suggests that it would be inappropriate to apply a time-constant conversion rate. For these countries, we use our best judgment to develop reasonable compromises between the estimates in the different sources, but ultimately constraining the values to match the WDI.

For the UAE, we used WDI Constant USD before 1985 and PPP values after 1985. That way, GDP per capita does not reach extreme values, and we obtain a data series that goes back to independence. For Qatar, we use PWT 8.0 converted to USD 2005 PPP using the conversion rate method before 2003 and WDI PPP for more recent years. Maddison operates with very high estimates in the early years while PWT have high estimates for later years, compared to the other sources. Finally, for Kuwait, PWT converted to USD 2005 PPP is used for the period 1970–1979 and 1990–1994, Maddison without conversion is used before 1970, while WDI PPP is used after 1994.

For the GDP per capita projections, we use the information given in the OECD ENV-Growth source with the following modifications. The projections are calculated in 5-year intervals. We create yearly observations by applying log-linear interpolation for each country. For some countries, there is poor agreement between the historical record and the starting point of the SSP forecasts. This mismatch is particularly notable for the eight countries listed in Table S6. We attribute the inconsistencies partly to our approach to reconstruct observations in the PPP metric, partly to a recent revision in WDI (Barbados), and partly to what may be an error in the OECD ENV-Growth projections (Somalia). We opted to adjust the projections to match the historical dataset. We summarize these changes in Table S11. In most cases, GDP per capita values have been adjusted upwards relative to the original projections. This implies that the projections probably overestimate future GDP per capita in these countries, as the OECD ENV-Growth model would predict lower growth rates if GDP per capita had been higher at the outset.

Additionally, the OECD ENV-Growth forecasts exclude a small number of countries, namely Kosovo, South Sudan, and North Korea. To `impute' forecasts for North Korea, we identified a reasonably similar `model' country, Nepal, and rescaled the forecast trajectories to fit the observed 2010 levels for the imputation country. Further, we assume that Sudan and South Sudan will follow a similar trend. For Kosovo, we assume the same trajectory as Serbia, only starting at a lower level of GDP per capita. This procedure is likely to underestimate future GDP per capita in Kosovo relative to what the OECD-ENV model would have yielded.

Table S11. Modifications to projected GDP per capita estimates

Country	Problem	Change
Barbados	WDI use updated national accounts from November 2013. SSPs use old data. USD 17,576 in old data becomes USD 23,261 in new data for 2009.	Multiply SSP projection by 1.32.
Cuba	SSPs seem to be using current USD. PPP data are not available so we use Dominican Republic PPP conversion rates for historical data. Thus, a higher GDP per capita (USD 8,831 versus USD 5,747).	Multiply SSP projection by 1.54.
Cyprus	SSPs have USD 18,907 GDP per capita in 2010, WDI have USD 25,198 GDP per capita. Here, the WDI data appear to be incorrect (dimilar POP and GDP values, only it does not sum up in WDI).	None
Myanmar	We use Maddison without any conversion from 1990 to 2005 USD PPP, which is much higher than the number supplied by the SSPs (USD 3,709 versus 1,442). It is unclear which source the SSP data uses, however it is similar to the figure in CIA World Factbook. Maddison uses data from the Asian Development Bank. Maddison data looks reasonable, and is backed by more evidence than the value supplied by CIA. It is not entirely clear, however, and Myanmar's accounting is not very good, as can be seen from all the sources. Maddison suggests that Myanmar GDP per capita is 40% that of Thailand, 70% of that in Sri Lanka and triple that of Nepal. CIA data suggests that Myanmar is similar to Nepal. It might be that the CIA number is outdated some, as Myanmar have had high growth the last decade.	None
Nicaragua	SSPs have USD 2,499 GDP per capita in 2010, WDI have USD 3,256 GDP per capita. Population estimates are similar, GDP is much lower in SSPs (18.9 billion versus 14.467).	Multiplied SSP projection by 1.30
Sierra Leone	SSPs have USD 742 GDP per capita in 2010, WDI have USD 997 GDP per capita in the same year. Population is higher in SSPs, while GDP is lower.	Multiplied SSP projection by 1.34
Somalia	SSPs work with a very low GDP (USD 330 million). More accurate estimate is probably between 4 to 6 billion.	Use Ethiopia projection as template, adjust to Somalia at outset.
Zimbabwe	SSPs seem to use GDP per capita in constant 2005 USD instead of PPP data.	None

S8.4 Population

Empirical population data are based mainly on the 2012 revision of the United Nations World Population Prospects [15], which has data from 1950 to 2012 for most countries. For a few countries with missing values (Taiwan, Yemen, and East/West Germany), we use data from Maddison [33]. For the east/west share of the population in Germany, we used statistics from the German statistical office [34]. For Kosovo and Serbia after 2006, and South Sudan from 2011, we use WDI population figures [16].

As noted for the education data, the UN source reports population estimates for current political entities also for years before major secessions or state mergers. To reconstruct figures for the former USSR, Yugoslavia, or East and West Germany, we sum the values for the constituting territories to estimate total population for each of the historical countries. Hence, population numbers for Yugoslavia in 2005 are the sums of the populations reported for Serbia, Montenegro, and Kosovo, while the population figure for Serbia in 2006 are made up from Serbia and Kosovo estimates. Only in 2008, Kosovo is included as an independent state. For Israel, we use population figures that cover both Israel and the West Bank and Gaza.

The SSP population projections are from IIASA [22], and line up well with the empirical population data. The IIASA data lack information for Taiwan, Kosovo, and South Sudan. To generate projections for Taiwan, we identified South Korea as a reasonably similar model country, and assumed population growth rates, 2010–2100, to be similar in both. For Kosovo and South Sudan, we generated projections from disaggregated population growth estimates for Serbia and Kosovo, and Sudan and South Sudan, respectively.

S8.5 Regions

Each country in the sample is categorized into one of nine geographical regions in order to visualize and detect notable differences between regions in trends and projections (e.g., Figs. S2–S4 and S10 as well as Fig. 2 in the article). These regions are consistent with the regional projections climate change of the Global Change Assessment Model (GCAM) and other integrated assessment models. Table S12 provides a list of this categorization, including the three-letter country code from the Gleditsch and Ward system membership data [6] that are used to identify individual countries in Fig. 2 in the article.

Table S12. Region definitions

Region	Country	Code	Region	Country	Code
Southern Africa	Angola	ANG	East/Southeast Asia	Brunei Darussalam	BRU
	Botswana	BOT		Cambodia	CAM
	Lesotho	LES		China	CHN
	Malawi	MAW		Fiji	FJI
	Mozambique	MZM		Indonesia	INS
	Namibia	NAM		Japan	JPN
	South Africa	SAF		Korea, Democr. Peoples' Rep. of	PRK
	Swaziland	SWA		Korea, Rep. of	ROK
	Tanzania, United Rep. of	TAZ		Lao Peoples Democr. Rep.	LAO
	Zambia	ZAM		Malaysia	MAL
	Zimbabwe	ZIM		Myanmar	MYA
Central/South America	Argentina	ARG		Papua New Guinea	PNG
	Bahamas	BHM		Philippines	PHI
	Barbados	BAR		Singapore	SIN
	Belize	BLZ		Solomon Islands	SOL
	Bolivia	BOL		Taiwan	TAW
	Brazil	BRA		Thailand	THI
	Chile	CHL		Timor Leste	ETM
	Colombia	COL		Viet Nam	DRV
	Costa Rica	COS	West Africa	Benin	BEN
	Cuba	CUB		Burkina Faso	BFO
	Dominican Rep.	DOM		Cameroon	CAO
	Ecuador	ECU		Cape Verde	CAP
	El Salvador	SAL		Central African Rep.	CEN
	Guatemala	GUA		Chad	CHA
	Guyana	GUY		Congo	CON
	Haiti	HAI		Congo, the Democr. Rep. of the	DRC
	Honduras	HON		Cote dIvoire	CDI
	Jamaica	JAM		Equatorial Guinea	EQG
	Mexico	MEX		Gabon	GAB

Region	Country	Code
	Nicaragua	NIC
	Panama	PAN
	Paraguay	PAR
	Peru	PER
	Suriname	SUR
	Trinidad and Tobago	TRI
	Uruguay	URU
	Venezuela	VEN
Eastern Europe	Albania	ALB
	Belarus	BLR
	Bosnia and Herzegovina	BOS
	Bulgaria	BUL
	Croatia	CRO
	Cyprus	CYP
	Czech Rep.	CZR
	Estonia	EST
	Hungary	HUN
	Latvia	LAT
	Lithuania	LIT
	Macedonia	MAC
	Malta	MLT
	Moldova, Rep. of	MLD
	Montenegro	MNG
	Poland	POL
	Romania	RUM
	Russian Federation	RUS
	Serbia	SER
	Slovakia	SLO
	Slovenia	SLV
	Turkey	TUR
	Ukraine	UKR
	Yugoslavia, Fed. Rep. of	YUG
Middle East and North Africa (MENA)	Algeria	ALG
	Bahrain	BAH
	Egypt	EGY
	Iran, Islamic Rep. of	IRN
	Iraq	IRQ
	Israel	ISR
	Jordan	JOR
	Kuwait	KUW
	Lebanon	LEB
	Libyan Arab Jamahiriya	LIB
	Morocco	MOR
	Oman	OMA
	Qatar	QAT
	Saudi Arabia	SAU
	Syrian Arab Rep.	SYR
	Tunisia	TUN
	United Arab Emirates	UAE
	Yemen	YEM
Central/South Asia	Afghanistan	AFG
	Armenia	ARM
	Azerbaijan	AZE
	Bangladesh	BNG

Region	Country	Code
	Gambia	GAM
	Ghana	GHA
	Guinea	GUI
	Guinea-Bissau	GNB
	Liberia	LBR
	Mali	MLI
	Mauritania	MAA
	Niger	NIR
	Nigeria	NIG
	Senegal	SEN
	Sierra Leone	SIE
	Togo	TOG
East Africa	Burundi	BUI
	Comoros	COM
	Djibouti	DJI
	Eritrea	ERI
	Ethiopia	ETH
	Kenya	KEN
	Madagascar	MAG
	Mauritius	MAS
	Rwanda	RWA
	Somalia	SOM
	Sudan	SUD
	South Sudan	SSD
	Uganda	UGA
Western Europe and North America	Australia	AUL
	Austria	AUS
	Belgium	BEL
	Canada	CAN
	Denmark	DEN
	Finland	FIN
	France	FRN
	Germany	GFR
	Greece	GRC
	Iceland	ICE
	Ireland	IRE
	Italy	ITA
	Luxembourg	LUX
	Netherlands	NTH
	New Zealand	NEW
	Norway	NOR
	Portugal	POR
	Spain	SPN
	Sweden	SWD
	Switzerland	SWZ
	United Kingdom	UKG
	United States of America	USA

Supplementary Information

Region	Country	Code	Region	Country	Code
	Bhutan	BHU			
	Georgia	GRG			
	India	IND			
	Kazakhstan	KZK			
	Kyrgyzstan	KYR			
	Maldives	MAD			
	Mongolia	MON			
	Nepal	NEP			
	Pakistan	PAK			
	Sri Lanka	SRI			
	Tajikistan	TAJ			
	Turkmenistan	TKM			
	Uzbekistan	UZB			

7 Supplementary information, Climate shocks, environmental vulnerability, mobilization, and the onset of ethnic civil conflicts

Appendix to Chapter 3

1 Ethnic groups and main living areas

An ethnic group is defined in the Ethnic Power Relations (EPR) dataset as "a subjectively experienced sense of commonality based on a belief in common ancestry and shared culture" (Vogt 2014, p.2). The EPR is based on an expert survey that asked regional and country experts to identify politically salient ethnic groups. The EPR is a time-series dataset; groups may come and go and their political status may change. Ethnic groups are not natural units of observations, in a primordial sense. Rather, ethnic groups are social constructs which are inter-subjectively observed at a given time. However, this also makes them entities that people understand, can delimitate, and organize around. Similarly, the rebels connected to these groups do not necessarily represent the group in any essential way, only (or at least) by claiming to represent the ethnic group, recruiting from the ethnic group, or by being supported by a majority of the ethnic group.

The EPR dataset codes politically relevant ethnic groups. A group is politically relevant if "at least one political organization has claimed to represent its interests at the national level or if its members are subjected to state-led political discrimination" (Vogt et al. 2015, p.1329). This ensures that the units of observation are groups that people not only identify (with), but that also have organized.

A basic premise for the research design is that it can code the main settlement patterns for those people that the rebel groups grew out of, recruited or gained support from. An important choice was, therefore, to study only groups that had specific sub–national settlement areas.

The GeoEPR 2014 dataset defines "regionally based" ethnic groups as those groups that are "located in a particular region/in particular regions that are easily distinguishable on a map" (Bormann 2014, p.1). They are distinguished from groups that are urban (meaning that more than 60% of the population lives in cities), migrant, dispersed, or statewide (spread throughout the country). Thus, regionally based groups are mainly rural groups with a clear regional settlement pattern.

2 Armed conflict onsets

An armed conflict is defined by UCDP/PRIO 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" (Themnér 2016, p.1). The conflict data is dyadic. The government is coded as side A, whereas those opposing the government – i.e. rebel group(s) – are coded on side B.

It is evident from the criteria that a conflict must have escalated to the use of violence, and that more than 25 persons from the government or rebel forces must have lost their lives before a conflict is coded for any given year. The start date of the conflict, which is used to determine the onset month, is set at the date when the conflict reaches the threshold of 25 battle-related deaths during the year (Startdate2). Thus, unless the first event lead to 25 battle-related deaths, the conflict could have started a bit earlier. UCDP/PRIO ACD also provides the date for the first battle-related death for the conflict (Startdate), but only for the first episode in any conflict.

The study only includes armed conflicts where UCDP/PRIO were able to pinpoint the starting month (*startprec* < 4). This could be a source of bias, as those conflicts where the starting date could not be pinpointed could systematically be related to extreme weather.

3 Matching EPR and ACD

The ACD2EPR dataset matches ethnic groups in the ETH EPR with side B groups in the UCDP ACD (Wucherpfennig et al. 2012, p.94ff). Naively matching EPR and ACD leads to a duplication of onsets, because a rebel group might claim to represent more than one ethnic group. Most notably, the onset of the armed conflict between the Forces of the Caucasus Emirate and the government of Russia (id 1-257) in October 2007 is tied to 13 different ethnic groups in the EPR (Avars, Dargins, Kabardins, Ingush, Kumyks, Lazgins, Adyghe, Balkars, Circassians, Karachai, Laks, Nogai and Tabasarans). It is difficult to claim that these should be treated as independent onsets. There are other similar examples in the data, but then with only 2–3 groups being affiliated with the same onset. To deal with this, ethnic groups that are always connected to the same rebel group are collapsed into one combined group. The Luba Shaba and Lunda-Yeke (FLNC) in DR Congo and the Miskitos and Sumus (Contras/FDN) in Nicaragua, however, are both in onsets together and separately. They are treated as separate groups and separate onsets.

The onsets between FLNC and the government in DRC and Contras/FDN and the government in Nicaragua are the only two onsets that happen in the same country-year-month. There are a few onsets which happen within the same country-year. This is true for the government of India in 1992 (NSCN - IM (Naga) in August, ATTF (Indigenous Tripuri) in October and PLA, UNLF (Manipuri) in December), the government of Ethiopia in 1996 (AIAI, ONLF (Somali (Ogaden)) in August and ARDUF (Afar) in December), the government of Myanmar in 1996 (RCSS (Shan) in January and BMA (Mons) in December), and the government of DRC in 2007 (CNDP (Tutsi-Banyamulenge) in January and BDK (Bakongo) in February). As these are different conflicts with different claims, they are not collapsed. However, it is possible that these conflicts are interdependent. To prevent treating them as independent, standard errors are clustered on country. This was not done for the random effects models, but uncertainties were almost the same when nesting the group-month random effects into country random effects.

The study ends up with 28595-29842 group-months (depending on which SPI measurement is used). There are only 128 onsets in all of this data. These group-months are divided up among 67 different groups (3 of which are a collapsed set of groups), leading to an average of ca. 445 months per group (37 years). Most groups only observe one onset. However, the maximum number of onsets in one group is six (Shan in Myanmar). Most of these onsets are recurrences of the same conflicts (called *episodes* in UCDP/PRIO ACD). The definition for a new episode is that it must be preceded by at least one year with fewer than 25 battle–related deaths.

4 Causal identification

From the propensity score theorem, we find that we can attain unbiased inference if we are able to control for the probability of treatment (Angrist and Pischke 2008, p.81). In addition, we must assume non–interference after treatment assignment and excludability, as these issues relate to possible complications that happen after treatment (in this case rainfall) (Gerber and Green 2012, p.39ff). Since we observe aggregated data across both space and time, the Modifiable Areal Unit Problem (MAUP) must be addressed. Data quality issues are discussed mainly through the notion of excludability. However, failing to properly match treatment and outcome data result in a related problem called *attrition*. These issues (treatment propensity, attrition, excludability, non–interference and MAUP) and how they are dealt with are discussed in the following sections.

4.1 Treatment propensity

The core assumption for causal identification is this article is that the data–generating process creating precipitation in the areas studied was equal for the same area–months for the years 1946–2013. For instance, that all June months in the Maya living area (as defined by GeoEPR) from 1946 to 2013 had the same probability of a given level of precipitation. June months can, for instance, be divided into groups using precipitation levels (e.g., less than 50 mm/month), and the average causal effect can be calculated as the difference in the probability of an armed conflict onset between rebel groups associated with the Maya (e.g. EZLN) and the Mexican government depending on the different precipitation levels.

While the average treatment effect within an area for a month set can be calculated because the probability of treatment is equal for all those area-months, the data across different month sets or groups cannot be pooled without knowing the probability distribution of precipitation for each area–month set. One way to calculate the average treatment effect across all of the area-month sets is to first calculate the average in each, and then average all of the averages. This number can be calculated by adding fixed effects for each area-month in an OLS regression.

A problem with this approach, however, is that the treatment we apply to each area–month set is actually quite different. Precipitation of 50 mm/month is a very different phenomenon depending on where you are. When calculating average effects, it is useful if the treatment heterogeneity is low. It is possible to reduce heterogeneity by converting the precipitation measure into a variable that takes on reasonably similar values for similar phenomena across the study areas.

The Standardized Precipitation Index (SPI) is one way to do this while at the same time preserving the qualities needed to use it as a randomized variable (McKee, Doesken, Kleist, et al. 1993; Guttman 1999). The SPI assumes — as do I — that there is a stable data–generating process across time for a particular area for a particular month; it is estimated by fitting a probability distribution to the observed levels of precipitation. SPI is fitted to a gamma distribution (Pearson III) for each area–month set from monthly average precipitation within each main living area with data from 1946-2013, using the SPEI package in R (Beguería and Vicente-Serrano 2013).

SPI has an expected value of 0, and SPI values are standard deviations from 0. SPI measures how unusual a given monthly precipitation level is, assuming that monthly precipitation levels were driven by the estimated underlying data generating process.

The estimated SPI distributions show the importance of not comparing climate variability measures between months or across space. In some areas, such as around the Sahel, there are many months which usually have little to no rainfall. The probability distribution for rainfall in such area months is heavily skewed towards zero, whereas the distribution looks more Normal for area–months where the average rainfall is higher. If we directly compare areas with a different distribution, we can no longer know whether we have a balanced comparison, and we cannot argue that we have a causal effect.

SPI can be calculated for different intervals. In this article, SPI is calculated for 1, 3, 6, 12, and 24 month intervals. This means that the distribution of $SPI3_{im}$ for location j in m=January is calculated based on $PRECIP_{j,NOV-JAN,y} = PRECIP_{j,m,y} + PRECIP_{j,m-1,y} + PRECIP_{j,m-2,y}$, where $m-1$ is December, and $m-2$ is November. The specific $SPI3_{jmy}$, the value of SPI3 for a given location-month-year, is a descriptiption of where on the distribution $SPI3_{jm}$ we find the particular $PRECIP_{j,(m-2)-m,y}$. For instance, $SPI3_{j,JAN,2001}$ is the placement of a particular $PRECIP_{j,NOV-JAN,2001}$ on the estimated distribution $SPI3_{j,JAN}$.

While I assume that the underlying propensity for precipitation is stable over time, that assumption could be problematic. Large, multi-year oscillating weather systems, such as the ENSO and the North Atlantic Oscillation can systematically affect precipitation patterns across the globe. The fact that most excluded ethnic groups are situated along the teleconnection for the ENSO should evoke some worry (Hsiang, Meng, and Cane 2011).

4.2 Attrition

In causal inference, we commonly assume that we observe outcomes for all units allocated to either treatment. In both field experiments and for observed data studies, however, it is not uncommon that data is missing, poorly measured or mismatched. All of these data–related issues are examples of *attrition* (Gerber and Green 2012, p.211ff). Attrition is a problem for causal inference if it is systematically related to potential outcomes of treated units.

The main attrition problem addressed in this study is the mismatch between treatment and outcome due to the high mobility of potential rebel recruits and rebel groups. It is largely untenable to spatially match climate shocks, argued to affect motivational mechanisms for rebel recruitment, and armed conflict events, when estimating the causal effect of such theoretical mechanisms. By using the matching procedure described above, the match between the climatic treatment of a particular population and recruitment from that population is arguably much better.

The alternative matching procedure is likely to introduce its own sources of attrition, however. GeoEPR uses information about the main living areas of ethnic groups, drawing mainly from the GREG (Geographic Representation of Ethnic Groups) dataset (Weidmann, Rød, and Cederman 2010); itself is a GIS version of *Atlas Narodov Mira*. The original atlas was made by Soviet ethnographers and published in 1964 (Wucherpfennig et al. 2011). Drawing ethnic groups on a map does not come without its own problems, however. Living areas are likely to change over time, the density of ethnic population is likely to vary within the denoted areas, and the salience of the ethnic identity is likely to vary. While the EPR dataset family (Vogt et al. 2015) attempts to address these issues, data limitations are likely to result in some attrition.

Poor and varying measurement quality of both climate and conflict variables result in attrition, and can be a source for bias in estimates. Gohdes and Price (2013) and Krüger et al. (2013) document severe issues with data quality in conflict research. Gohdes and Price (2013) study the background data for the PRIO Battle Deaths Dataset, and find that most recorded observations before the 1980s lack data that can attribute deaths to combat. Rather, the data is based on estimates of total deaths during entire conflict periods. Krüger et al. (2013) argue that even in recent years, event data reflect *reporting patterns* more than they reflect patterns of violence.

In the case of climate–conflict research, these reporting patterns can be particularly relevant. Humanitarian aid can affect the measurement of outcomes. Since NGOs are an important source for conflict databases (Krüger et al. 2013), one possible reason for correlations between extreme climatic exposure and violent conflict could be that violent events are more often reported by NGOs because they travel to such sites during droughts or floods. Alternatively, extreme weather can create difficult terrain or otherwise prevent journalists and other reporters of violence to be in an area where violence happens. In that case, bias would go in the opposite direction. Varying reporting patterns can therefore affect estimates in both directions.

In a working paper, Schultz and Mankin (2017) argue that civil conflicts in sub–Saharan Africa have affected the quality of temperature measurements. They find that this has led to a small attenuation bias in causal estimates of temperature on civil conflict data. Although they do not test precipitation measurements, it is worth having in mind this possible effect.

The number of meteorological stations in the world vary over time, peaking around 1986 in all regions (Becker et al. 2013; Schneider et al. 2014). In Latin America and Africa, there is also a clear drop in the density of stations in the inland areas (Becker et al. 2013). The impact of varying meteorological data quality over time on causal estimates in social sciences should be investigated more closely, particularly since the ability to measure weather can be assumed to be affected by the onset of violent conflict or natural disasters (Schultz and Mankin 2017).

The GPCC data used in this paper rely only on gauge data. In their grid products, the GPCC adjusts for systematic gauge–measuring error and interpolate between measurement stations (Schneider et al. 2014, p.19f). The GPCC has tested different ways to interpolate, and it argues that their approach is effective at reducing sampling error (Becker et al. 2013; Schneider et al. 2014).

Since the bias can go in different directions, and probably does so in different ways in different areas, this study is unable to create clear expectations about how this lack of data quality may affect the results. Obviously, more research should be done in this field.

While there is reason to think these data quality issues are more problematic when studying high resolution event data than when studying armed conflict onsets, they may definitely also affect this study. One particular issue is that the conflict onset month, rather than the onset year, is used. While the UCDP/PRIO ACD data measure temporal precision, the onset month is based on when the battle deaths threshold reached 25. Given the issues discussed in Gohdes and Price (2013), poor measurement of the onset date can lead to attenuation bias in the results. The sensitivity of the results to the change of startdate (as shown in Figure 2 and 3 in the article) is an indication that data quality is an important source of variance in published results.

4.3 Excludability

Excludability means that changes in potential outcomes can only be due to the treatment given, not due to being assigned to the treatment group (Gerber and Green 2012, p.45). If third-party interventions happen contingent on treatment assignment, outcomes may be affeced, affecting the results in other ways than through the proposed treatment.

One possible example of this for the case of climate shocks as treatment, is humanitarian aid. Humanitarian aid could be invoked in the case of a drought. In areas where that is the case, every time the area is treated with a drought, it gets another treatment designed to countermeasure adverse effects (like economic costs or even social conflict and unrest). Assuming that these interventions have a positive effect, they would reduce a possible negative effect of the treatment. This effectively introduces dependency after treatment assignment, as we could imagine that where and who in the world that are more likely to get post-disaster help is not random . Thus, we cannot estimate the average treatment effect of a drought, even after having randomized it.

Since this article has no way of separating out this effect, it must rather estimate the intention–to–treat effect. The ITT is the average effect of being “assigned” a drought, as opposed to being “assigned” a non-drought. It is therefore the average effect given that people are vulnerable in varying ways and that society can actively counteract negative effects after they have occurred. Studying only politically excluded ethnic groups is a way to reduce the variance of the ITT effect.

Lack of excludability is a more benign problem than attrition, because it it still possible to estimate the ITT effect if that is the only problem. Attrition, on the other hand, can lead to biased estimates, also of the ITT, if it is systematically related to potential outcomes.

4.4 Non-interference

Non-interference is the assumption that the assignment of treatment to one unit does not affect the treatment or potential outcomes of other units. There are three kinds of interferences that are especially problematic in climate-conflict studies:

i The treatment is applied non-randomly to large areas at a time (e.g. a rain-cloud, a high-pressure system, or a climatic zone);

ii The treatment of some units affects the outcome of another unit. It is useful to separate these spatial dependencies into two categories:

   (a) Spatial dynamics, e.g., a violent conflict diffuses to nearby towns, or moves toward the capital;
   (b) System dependencies, e.g., lost harvests due to droughts in a central producing area can affect both treatment and outcomes in other areas through global market mechanisms;

iii A previous treatment affects a later outcome.

(**i**) particularly refers to the problem that nearby units are handled in the estimation routine as being exposed to independent treatments, when the treatment actually is very similar. The study address this issue by observing larger, non–contiguous units of observations, and by clustering the standard errors at the country level.

Since precipitation is not randomly distributed locally, local spatial dynamics (**iia**) could affect outcomes. This problem, however, is much larger for studies using conflict events as their outcome, rather than armed conflict onsets, as the local spatial dynamics during a conflict could be argued to be much stronger than local spatial dynamics potentially affecting the onset of a conflict.

System dependencies (**iib**) are less problematic in this study than in studies relying on between–unit variation. As long as the system dependencies are not affecting precipitation propensities, they can only attenuate the within–unit estimate. For instance, if the whole world would be affected equally bad by every local drought, we would not observe a local effect of drought anywhere. If some areas are more vulnerable to system dependencies than others, within–unit comparisons will likewise just observe an attenuation of the causal estimate, while between–unit comparisons could pick up the non–random global average vulnerability as the independent effect of local climate shocks on local outcomes. Although such global variance can be interesting to investigate, it is a nuisance factor if the intent is to estimate the local causal effect of climate shocks on violent outcomes.

The units are observed over a long time, and previous treatments may affect subsequent onsets through a previous onset. Previous onsets should therefore be controlled for. Because the treatment is measured over a varying amount of time depending on SPI interval, only onsets that occurred before the first month of the SPI interval are controlled for.

4.5 Modifiable Areal Unit Problem

MAUP is a set of issues that can arise whenever data is aggregated into arbitrary sized and zoned spatial units (Dark and Bram 2007, p.472). These issues consist of changes in data variability and in averages. When MAUP is present, "anything could happen to the correlation at the aggregated level" (Cressie and Wikle 2011, p.198).

A pragmatic approach to MAUP is to vary zoning and spacing to test the sensitivity of the results (Schutte and Donnay 2014). Sensitivity tests, however, do not avoid MAUP; they merely ensure that our statistics take MAUP into account.

The best way to avoid MAUP is to not use data that can be arbitrarily sized and zoned, for instance, by using individual–level data. Using surveys to ask about climate exposure and conflict is a direct solution to MAUP, for example as is done in Linke et al. (2015). When arbitrary sized and zoned data cannot be avoided, the main advice is to find the scale and zone for a given phenomenon that all can agree captures the phenomenon best (Dark and Bram 2007, p.477). The argument that we should use political boundaries such as the state, or administrative boundaries such as municipalities, are rooted in this sentiment (e.g. Fjelde and von Uexkull 2012, p.447). The debate lacks nuance, however.

Some conflicts should be expected to have a stronger relation to administrative boundaries than do others. State–based conflicts, for instance, are likely less to be constrained by such boundaries. Communal conflicts may be fought between groups residing within different administrative boundaries. Or administrative boundaries may be superficially imposed on perhaps more relevant traditional boundaries (Wig 2016). Also, some administrative boundaries are actually more important than others. Federations and confederations, such as Brazil, Belgium, India, the USA, and Switzerland, give broad powers to their states, whereas other countries' administrative areas have very few powers. Lastly, the size of administrative areas varies widely, which can be seen from the map in Fjelde and von Uexkull (2012, p.448). It is doubtful that all of these areas are equally likely to contain/embody the phenomena that are of interest.

MAUP can also arise from temporal aggregation. Five different SPI time intervals are tested, in part to address MAUP sensitivity. However, as argued in the text, there are also substantial reasons to believe that different aggregation levels can have different effects on the likelihood of conflict. The problem, however, is that it is difficult to generate clear expectations for this sample.

Since more than one hypothesis test of the same underlying theoretical question is made, there is an increased probability of observing an unlikely sample, which would lead us to falsely reject the null hypothesis. The most conservative approach is to apply the Bonferroni correction. This means that for $h = 5$ number of tests we need to aim for a p-value of $p/h = 0.05/5 = 0.01$ in order to claim statistically significant results (Gerber and Green 2012, p.300). But because the different intervals are not independent, the Bonferroni correction is overly conservative. In the regression tables, estimates that meet $p > 0.05$ are denoted with one star and estimates that meet $p > 0.01$, with two stars.

5 Descriptive statistics

5.1 List of ethnic groups in study

Country	Group	Startyear	Endyear
Angola	Bakongo	1975	2005
Angola	Cabindan Mayombe	1975	2013
Angola	Ovimbundu-Ovambo	1975	2005
Azerbaijan	Armenians	1991	2013
Bangladesh	Tribal-Buddhists	1972	2013
Chad	Arabs	1960	2009
Chad	Muslim Sahel groups	1960	1975
Chad	Toubou	1960	2013
China	Koreans	1946	2013
China	Tibetans	1946	2013
China	Uyghur	1946	2013
Congo	Lari/Bakongo	1969	2013
Congo, DRC	Bakongo	1966	2013
Congo, DRC	Luba Kasai	1960	2013
Congo, DRC	Luba Shaba	1961	1997
Congo, DRC	Lunda-Yeke	1960	1997
Congo, DRC	Tetela-Kusu	1961	2013
Congo, DRC	Tutsi-Banyamulenge	1960	2013
Ethiopia	Afar	1946	2013
Ethiopia	Christian Eritreans	1952	1993
Ethiopia	Oroma	1946	1991
Ethiopia	Somali (Ogaden)	1946	2013
Georgia	Abkhazians	1991	2013
Georgia	Ossetians (South)	1991	2013
Greece	Macedonians	1946	2013
India	Bodo	1963	2013
India	Indigenous Tripuri	1947	2013
India	Manipuri	1947	2013
India	Mizo	1947	2013
India	Naga	1947	2013
Indonesia	East Timorese	1976	2001
Indonesia	Minahasa	1949	1966
Liberia	Gio	1981	1989
Liberia	Mano	1981	1989
Mali	Arabs/Moors	1960	1995
Mali	Tuareg	1960	1995
Mexico	Maya	1946	2013
Morocco	Sahrawis	1977	2013
Mozambique	Shona-Ndau	1975	2013
Myanmar	Kachins	1959	2013
Myanmar	Karenni (Red Karens)	1948	2013
Myanmar	Kayin (Karens)	1948	2013
Myanmar	Mons	1948	2013
Myanmar	Muslim Arakanese	1948	2013
Myanmar	Shan	1959	2013
Myanmar	Wa	1948	2013
Nicaragua	Miskitos	1946	2013
Nicaragua	Sumus	1946	2013
Niger	Toubou	1960	2011
Niger	Tuareg	1960	2011
Pakistan	Baluchis	1947	2013
Peru	Indigenous peoples of the Andes	1946	2013
Philippines	Indigenous	1986	2013
Russia	Avars	1946	2013
Sri Lanka	Sri Lankan Tamils	1948	2013
Sudan	Azande	1956	2011
Sudan	Bari	1956	2011
Sudan	Dinka	1956	2005
Sudan	Latoka	1956	2011
Sudan	Nuer	1956	2011
Sudan	Other Arab groups	1956	2013
Syria	Kurds	1958	2013
Tajikistan	Kyrgyz	1991	2013
Thailand	Malay Muslims	1946	2013
Togo	Ewe (and related groups)	1967	2005
Uganda	Langi/Acholi	1972	2013
Uganda	Teso	1966	2013

Table 1: List of ethnic groups in analysis

5.2 Onsets with unusual climate

Table 2 shows the onsets in the data where at least one of the SPI measurements were larger than or equal to ±1.5.

statename	group	status	year	month	sideb	spi1l1	spi3l1	spi6l1	spi12l1	spi24l1
Nicaragua	Miskitos	POWERLESS	1982	4	Contras/FDN	0.29	1.45	1.23	2.22	2.69
Nicaragua	Sumus	POWERLESS	1982	4	Contras/FDN	0.19	1.24	1.07	1.53	1.93
Peru	Indigenous peoples of the Andes	POWERLESS	1982	8	Sendero Luminoso	0.40	0.01	0.45	1.60	1.39
Peru	Indigenous peoples of the Andes	POWERLESS	2007	11	Sendero Luminoso	-1.28	-1.76	-1.77	-1.54	-1.29
Russia	Avars	POWERLESS	2007	11	Forces of the Caucasus Emirate	0.32	-0.76	-1.73	-0.69	-0.91
Mali	Arabs/Moors	DISCRIMINATED	1994	10	FIAA	1.94	1.41	1.10	0.84	-0.27
Niger	Toubou	POWERLESS	1995	7	FDR	-0.06	1.32	1.43	1.43	1.55
Congo	Lari/Bakongo	POWERLESS	1998	1	Cocoyes, Ninjas, Ntsiloulous	1.37	1.90	1.94	0.64	0.29
Congo	Lari/Bakongo	POWERLESS	2002	4	Ntsiloulous	-1.10	-0.50	-1.85	-1.58	-0.56
Congo, DRC	Bakongo	POWERLESS	2007	2	BDK	-1.98	0.80	0.18	0.49	-0.05
Congo, DRC	Lunda-Yeke	SELF-EXCLUSION	1961	11	State of Katanga	2.52	2.54	2.20	1.25	
Congo, DRC	Lunda-Yeke	IRRELEVANT	1967	7	Military Faction (Forces of Jean Schramme)	1.99	0.56	0.03	-0.21	0.06
Congo, DRC	Tutsi-Banyamulenge	DISCRIMINATED	1999	1	RCD	1.90	1.96	1.12	0.49	-0.53
Uganda	Langi/Acholi	DISCRIMINATED	1972	1	Kikosi Maalum	-0.91	-1.95	-1.73	-1.78	-0.77
Uganda	Langi/Acholi	POWERLESS	2013	6	ADF, LRA	-0.14	-0.56	1.54	0.68	-0.41
Uganda	Teso	DISCRIMINATED	1987	1	UPA	0.49	-0.91	-2.07	-1.51	-1.88
Ethiopia	Oroma	DISCRIMINATED	1977	12	OLF	1.41	2.62	1.89	1.29	0.89
Ethiopia	Oroma	DISCRIMINATED	1983	7	OLF	-0.14	1.78	0.98	1.01	1.77
Ethiopia	Somali (Ogaden)	DISCRIMINATED	1976	10	WSLF	1.87	1.57	2.67	0.84	0.13
Ethiopia	Christian Eritreans	DISCRIMINATED	1973	1	EPLF	1.40	1.51	-0.65	-0.78	-1.42
Angola	Bakongo	POWERLESS	1994	12	FLEC-FAC, FLEC-R	1.87	1.89	1.86	0.41	0.09
Angola	Cabindan Mayombe	POWERLESS	1994	12	FLEC-FAC, FLEC-R	1.82	2.09	2.07	0.16	-0.15
Angola	Cabindan Mayombe	POWERLESS	2007	12	FLEC-FAC	2.24	2.51	2.50	3.12	2.67
Sudan	Azande	POWERLESS	1963	12	SSLM	1.88	0.10	1.00	1.88	1.88
Sudan	Nuer	POWERLESS	2011	5	Republic of South Sudan	-1.21	-1.51	-1.39	-1.29	-2.07
China	Tibetans	SELF-EXCLUSION	1950	10	Tibet	-0.09	-1.01	-1.58	-0.69	4.17
China	Tibetans	POWERLESS	1956	5	Tibet	-0.21	0.05	0.36	0.75	1.78
China	Tibetans	POWERLESS	1959	3	Tibet	2.22	2.71	0.64	0.01	-0.46
India	Bodo	POWERLESS	1989	3	ABSU	0.77	0.93	1.26	2.33	2.23
India	Bodo	POWERLESS	2001	8	NDFB	-1.75	-1.06	-0.75	-0.23	0.06
India	Bodo	POWERLESS	2009	6	NDFB - RD	-1.59	-1.81	-1.96	-0.56	-0.15
India	Bodo	POWERLESS	2013	12	NDFB-S	-1.26	-1.46	-2.85	-2.33	-1.21
India	Indigenous Tripuri	POWERLESS	1992	10	ATTF	0.33	-1.08	-1.61	-0.76	1.10
India	Manipuri	POWERLESS	1982	7	PLA	1.64	0.62	0.47	-1.16	-0.60
India	Manipuri	POWERLESS	1998	12	PLA, UNLF	0.45	-1.62	-1.29	-0.38	-0.63
India	Naga	POWERLESS	1992	8	NSCN-IM	0.68	-0.65	-0.47	1.01	1.94
Pakistan	Baluchis	DISCRIMINATED	1974	12	BLF	-0.35	-0.36	-1.61	-1.38	-1.53
Pakistan	Baluchis	DISCRIMINATED	2004	8	BLA	-1.26	-1.40	-2.85	-2.22	-0.85
Pakistan	Baluchis	DISCRIMINATED	2006	3	BLA, BRA	-1.01	-2.03	-1.83	-0.43	-0.39
Myanmar	Kachins	DISCRIMINATED	1961	2	KIO	0.27	-0.09	-0.84	-1.64	-0.53
Myanmar	Kayin (Karens)	SELF-EXCLUSION	1966	1	KNU	1.53	1.60	0.24	0.72	0.43
Myanmar	Kayin (Karens)	SELF-EXCLUSION	1994	12	KNU	-0.89	-1.66	1.61	1.54	0.16
Myanmar	Kayin (Karens)	SELF-EXCLUSION	2000	3	DKBA 5, KNU	1.23	0.68	0.84	1.64	-1.12
Myanmar	Mons	POWERLESS	1959	1	NMSP	-0.82	-1.62	-0.72	-0.81	-1.43
Myanmar	Mons	SELF-EXCLUSION	1990	2	NMSP	-0.16	-1.66	-0.43	-1.06	-2.03
Myanmar	Muslim Arakanese	SELF-EXCLUSION	1973	1	RPF	0.16	-1.23	-1.74	-2.40	-0.99
Myanmar	Muslim Arakanese	SELF-EXCLUSION	1994	5	RSO	0.75	1.53	0.30	0.49	-0.11
Myanmar	Shan	SELF-EXCLUSION	1972	12	SSA	1.51	-1.05	-1.67	-2.08	-0.06
Myanmar	Karenni (Red Karens)	DISCRIMINATED	1957	12	KNPP	-0.29	-1.46	-1.37	-1.81	-0.91
Myanmar	Karenni (Red Karens)	DISCRIMINATED	1992	9	KNPP	0.00	-0.80	-1.71	-0.89	-1.08
Myanmar	Karenni (Red Karens)	DISCRIMINATED	1996	3	KNPP	2.73	1.40	0.24	0.29	0.52
Sri Lanka	Sri Lankan Tamils	SELF-EXCLUSION	1984	9	LTTE	-0.28	1.15	2.05	2.25	0.72
Sri Lanka	Sri Lankan Tamils	POWERLESS	2005	12	LTTE	1.89	1.27	0.82	0.74	0.26

Table 2: Onsets in UCDP/PRIO ACD with $|SPI| >= 1.5$

6 Regression output

6.1 Using Startdate2

	Model 1	Model 2	Model 3	Model 4	Model 5
drought1	−0.0015				
	(0.0013)				
flood1	0.0035				
	(0.0019)				
drought3		0.0008			
		(0.0016)			
flood3		0.0030			
		(0.0026)			
drought6			0.0039		
			(0.0024)		
flood6			0.0015		
			(0.0021)		
drought12				0.0010	
				(0.0019)	
flood12				0.0011	
				(0.0017)	
drought24					−0.0023*
					(0.0011)
flood24					0.0014
					(0.0021)
R^2	0.0334	0.0334	0.0338	0.0332	0.0342
Adj. R^2	0.0111	0.0110	0.0113	0.0104	0.0110
Num. obs.	29841	29740	29590	29291	28700
RMSE	0.0651	0.0650	0.0651	0.0650	0.0654

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 3: Fixed effects model without controls, dummy SPI

	Model 1	Model 2	Model 3	Model 4	Model 5
drought1	-0.0013				
	(0.0012)				
flood1	0.0036				
	(0.0019)				
logtsconfl2	0.0234^{***}				
	(0.0036)				
logtsconfl2sq	-0.0069^{***}				
	(0.0011)				
logtsconfl2cu	0.0006^{***}				
	(0.0001)				
drought3		0.0004			
		(0.0016)			
flood3		0.0031			
		(0.0025)			
logtsconfl4		0.0201^{***}			
		(0.0029)			
logtsconfl4sq		-0.0063^{***}			
		(0.0010)			
logtsconfl4cu		0.0006^{***}			
		(0.0001)			
drought6			0.0035		
			(0.0023)		
flood6			0.0014		
			(0.0021)		
logtsconfl7			0.0199^{***}		
			(0.0029)		
logtsconfl7sq			-0.0066^{***}		
			(0.0010)		
logtsconfl7cu			0.0006^{***}		
			(0.0001)		
drought12				0.0007	
				(0.0017)	
flood12				0.0008	
				(0.0016)	
logtsconfl13				0.0211^{***}	
				(0.0036)	
logtsconfl13sq				-0.0077^{***}	
				(0.0013)	
logtsconfl13cu				0.0007^{***}	
				(0.0001)	
drought24					-0.0023^{*}
					(0.0012)
flood24					0.0013
					(0.0021)
logtsconfl25					0.0072
					(0.0056)
logtsconfl25sq					-0.0037^{*}
					(0.0019)
logtsconfl25cu					0.0004^{*}
					(0.0002)
R^2	0.0351	0.0351	0.0358	0.0364	0.0358
Adj. R^2	0.0126	0.0125	0.0131	0.0135	0.0125
Num. obs.	29735	29635	29485	29187	28595
RMSE	0.0636	0.0638	0.0639	0.0637	0.0641

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 4: Fixed effects model with controls, dummy SPI

	Model 1	Model 2	Model 3	Model 4	Model 5
abs(spi1l1)	0.0005				
	(0.0005)				
logtsconfl2	0.0235***				
	(0.0036)				
logtsconfl2sq	−0.0069***				
	(0.0011)				
logtsconfl2cu	0.0006***				
	(0.0001)				
abs(spi3l1)		0.0012**			
		(0.0004)			
logtsconfl4		0.0201***			
		(0.0029)			
logtsconfl4sq		−0.0063***			
		(0.0010)			
logtsconfl4cu		0.0006***			
		(0.0001)			
abs(spi6l1)			0.0011*		
			(0.0006)		
logtsconfl7			0.0199***		
			(0.0030)		
logtsconfl7sq			−0.0066***		
			(0.0010)		
logtsconfl7cu			0.0006***		
			(0.0001)		
abs(spi12l1)				−0.0000	
				(0.0004)	
logtsconfl13				0.0211***	
				(0.0036)	
logtsconfl13sq				−0.0077***	
				(0.0013)	
logtsconfl13cu				0.0007***	
				(0.0001)	
abs(spi24l1)					−0.0002
					(0.0008)
logtsconfl25					0.0073
					(0.0056)
logtsconfl25sq					−0.0037*
					(0.0019)
logtsconfl25cu					0.0004*
					(0.0002)
R^2	0.0349	0.0351	0.0357	0.0364	0.0357
Adj. R^2	0.0124	0.0125	0.0131	0.0135	0.0124
Num. obs.	29735	29635	29485	29187	28595
RMSE	0.0637	0.0638	0.0639	0.0637	0.0641

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table 5: Fixed effects model with controls, absolute SPI

	Model 1	Model 2	Model 3	Model 4	Model 5
abs(spi1l1)	0.0881				
	(0.1495)				
logtsconfl2	29.0025***				
	(4.3565)				
logtsconfl2sq	−7.1919***				
	(1.0142)				
logtsconfl2cu	0.5637***				
	(0.0764)				
abs(spi3l1)		0.2536			
		(0.1408)			
logtsconfl4		24.3781***			
		(3.7461)			
logtsconfl4sq		−6.2094***			
		(0.8904)			
logtsconfl4cu		0.4963***			
		(0.0682)			
abs(spi6l1)			0.2567		
			(0.1346)		
logtsconfl7			17.6943***		
			(2.8342)		
logtsconfl7sq			−4.7630***		
			(0.7009)		
logtsconfl7cu			0.3956***		
			(0.0555)		
abs(spi12l1)				0.0275	
				(0.1463)	
logtsconfl13				4.9066***	
				(0.9167)	
logtsconfl13sq				−1.7781***	
				(0.2752)	
logtsconfl13cu				0.1747***	
				(0.0255)	
abs(spi24l1)					−0.0641
					(0.1525)
logtsconfl25					1.7967***
					(0.4204)
logtsconfl25sq					−0.9393***
					(0.1733)
logtsconfl25cu					0.1072***
					(0.0187)
AIC	797.2409	795.8907	797.3908	801.0049	819.2789
R^2	0.0031	0.0031	0.0031	0.0025	0.0014
Max. R^2	0.0292	0.0293	0.0294	0.0292	0.0293
Num. events	122	122	122	120	119
Num. obs.	29735	29635	29485	29187	28595
Missings	107	207	357	655	1247

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 6: Conditional logit model with controls, absolute SPI

	Model 1	Model 2	Model 3	Model 4	Model 5
(Intercept)	−0.0100**	−0.0062*	−0.0037	0.0011	0.0109***
	(0.0037)	(0.0030)	(0.0024)	(0.0019)	(0.0016)
drought1	−0.0021				
	(0.0016)				
flood1	0.0038*				
	(0.0015)				
logtsconfl2	0.0209***				
	(0.0034)				
logtsconfl2sq	−0.0063***				
	(0.0010)				
logtsconfl2cu	0.0005***				
	(0.0001)				
drought3		0.0005			
		(0.0016)			
flood3		0.0031*			
		(0.0016)			
logtsconfl4		0.0191***			
		(0.0029)			
logtsconfl4sq		−0.0060***			
		(0.0009)			
logtsconfl4cu		0.0005***			
		(0.0001)			
drought6			0.0035*		
			(0.0015)		
flood6			0.0016		
			(0.0016)		
logtsconfl7			0.0191***		
			(0.0026)		
logtsconfl7sq			−0.0063***		
			(0.0008)		
logtsconfl7cu			0.0006***		
			(0.0001)		
drought12				0.0008	
				(0.0015)	
flood12				0.0007	
				(0.0016)	
logtsconfl13				0.0193***	
				(0.0023)	
logtsconfl13sq				−0.0071***	
				(0.0008)	
logtsconfl13cu				0.0007***	
				(0.0001)	
drought24					−0.0023
					(0.0015)
flood24					0.0015
					(0.0016)
logtsconfl25					0.0057*
					(0.0022)
logtsconfl25sq					−0.0031***
					(0.0008)
logtsconfl25cu					0.0003***
					(0.0001)
AIC	−79131.7045	−78767.1601	−78234.8148	−77648.6598	−75687.6818
BIC	−79065.3039	−78700.7864	−78168.4817	−77582.4080	−75621.5939
Log Likelihood	39573.8523	39391.5800	39125.4074	38832.3299	37851.8409
Num. obs.	29735	29635	29485	29187	28595
Num. groups: grpmonth	672	672	672	672	672
Var: grpmonth (Intercept)	0.0000	0.0000	0.0000	0.0000	0.0000
Var: Residual	0.0041	0.0041	0.0041	0.0041	0.0041

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 7: Random intercept model with controls, dummy SPI

	Model 1	Model 2	Model 3	Model 4	Model 5
(Intercept)	-0.0102^{**}	-0.0070^{*}	-0.0043	0.0012	0.0109^{***}
	(0.0038)	(0.0030)	(0.0025)	(0.0020)	(0.0016)
abs(spi1l1)	0.0004				
	(0.0006)				
logtsconfl2	0.0209^{***}				
	(0.0034)				
logtsconfl2sq	-0.0063^{***}				
	(0.0010)				
logtsconfl2cu	0.0005^{***}				
	(0.0001)				
abs(spi3l1)		0.0012^{*}			
		(0.0006)			
logtsconfl4		0.0191^{***}			
		(0.0029)			
logtsconfl4sq		-0.0060^{***}			
		(0.0009)			
logtsconfl4cu		0.0005^{***}			
		(0.0001)			
abs(spi6l1)			0.0011		
			(0.0006)		
logtsconfl7			0.0191^{***}		
			(0.0026)		
logtsconfl7sq			-0.0063^{***}		
			(0.0008)		
logtsconfl7cu			0.0006^{***}		
			(0.0001)		
abs(spi12l1)				0.0000	
				(0.0006)	
logtsconfl13				0.0193^{***}	
				(0.0023)	
logtsconfl13sq				-0.0071^{***}	
				(0.0008)	
logtsconfl13cu				0.0007^{***}	
				(0.0001)	
abs(spi24l1)					-0.0002
					(0.0006)
logtsconfl25					0.0059^{**}
					(0.0022)
logtsconfl25sq					-0.0032^{***}
					(0.0008)
logtsconfl25cu					0.0003^{***}
					(0.0001)
AIC	-79134.8391	-78778.4602	-78243.3484	-77659.4239	-75695.6134
BIC	-79076.7385	-78720.3832	-78185.3069	-77601.4536	-75637.7865
Log Likelihood	39574.4195	39396.2301	39128.6742	38836.7120	37854.8067
Num. obs.	29735	29635	29485	29187	28595
Num. groups: grpmonth	672	672	672	672	672
Var: grpmonth (Intercept)	0.0000	0.0000	0.0000	0.0000	0.0000
Var: Residual	0.0041	0.0041	0.0041	0.0041	0.0041

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 8: Random intercept model with controls, absolute SPI

6.2 Using Startdate

	Model 1	Model 2	Model 3	Model 4	Model 5
drought1	0.0003				
	(0.0012)				
flood1	0.0014				
	(0.0014)				
drought3		0.0006			
		(0.0012)			
flood3		0.0016			
		(0.0018)			
drought6			0.0021		
			(0.0019)		
flood6			−0.0010		
			(0.0015)		
drought12				0.0022	
				(0.0021)	
flood12				0.0010	
				(0.0021)	
drought24					−0.0010
					(0.0011)
flood24					0.0027
					(0.0022)
R^2	0.0569	0.0572	0.0573	0.0572	0.0641
Adj. R^2	0.0327	0.0328	0.0329	0.0324	0.0394
Num. obs.	26914	26813	26663	26364	25774
RMSE	0.0536	0.0534	0.0535	0.0532	0.0532

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 9: Fixed effects model with controls, dummy SPI, Startdate

	Model 1	Model 2	Model 3	Model 4	Model 5
drought1	0.0005				
	(0.0012)				
flood1	0.0015				
	(0.0014)				
logtsconfl2	0.0123***				
	(0.0037)				
logtsconfl2sq	−0.0038**				
	(0.0012)				
logtsconfl2cu	0.0004**				
	(0.0001)				
drought3		0.0002			
		(0.0012)			
flood3		0.0018			
		(0.0018)			
logtsconfl4		0.0099**			
		(0.0033)			
logtsconfl4sq		−0.0033**			
		(0.0012)			
logtsconfl4cu		0.0003**			
		(0.0001)			
drought6			0.0020		
			(0.0018)		
flood6			−0.0008		
			(0.0014)		
logtsconfl7			0.0086**		
			(0.0030)		
logtsconfl7sq			−0.0031**		
			(0.0011)		
logtsconfl7cu			0.0003**		
			(0.0001)		
drought12				0.0015	
				(0.0017)	
flood12				0.0012	
				(0.0021)	
logtsconfl13				0.0082**	
				(0.0028)	
logtsconfl13sq				−0.0032**	
				(0.0011)	
logtsconfl13cu				0.0003**	
				(0.0001)	
drought24					−0.0009
					(0.0012)
flood24					0.0027
					(0.0021)
logtsconfl25					0.0048*
					(0.0023)
logtsconfl25sq					−0.0022*
					(0.0010)
logtsconfl25cu					0.0002*
					(0.0001)
R^2	0.0572	0.0572	0.0574	0.0657	0.0523
Adj. R^2	0.0328	0.0327	0.0327	0.0409	0.0271
Num. obs.	26808	26708	26558	26260	25669
RMSE	0.0517	0.0518	0.0519	0.0513	0.0511

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 10: Fixed effects model with controls, dummy SPI, Startdate

	Model 1	Model 2	Model 3	Model 4	Model 5
abs(spi1l1)	0.0002				
	(0.0004)				
logtsconfl2	0.0123***				
	(0.0037)				
logtsconfl2sq	−0.0038**				
	(0.0012)				
logtsconfl2cu	0.0004**				
	(0.0001)				
abs(spi3l1)		0.0003			
		(0.0005)			
logtsconfl4		0.0099**			
		(0.0033)			
logtsconfl4sq		−0.0033**			
		(0.0012)			
logtsconfl4cu		0.0003**			
		(0.0001)			
abs(spi6l1)			0.0004		
			(0.0006)		
logtsconfl7			0.0086**		
			(0.0030)		
logtsconfl7sq			−0.0031**		
			(0.0011)		
logtsconfl7cu			0.0003**		
			(0.0001)		
abs(spi12l1)				0.0003	
				(0.0006)	
logtsconfl13				0.0082**	
				(0.0028)	
logtsconfl13sq				−0.0032**	
				(0.0011)	
logtsconfl13cu				0.0003**	
				(0.0001)	
abs(spi24l1)					0.0008
					(0.0009)
logtsconfl25					0.0050*
					(0.0023)
logtsconfl25sq					−0.0022*
					(0.0010)
logtsconfl25cu					0.0003**
					(0.0001)
R^2	0.0572	0.0572	0.0573	0.0656	0.0522
Adj. R^2	0.0328	0.0327	0.0327	0.0409	0.0271
Num. obs.	26808	26708	26558	26260	25669
RMSE	0.0517	0.0518	0.0519	0.0513	0.0511

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 11: Fixed effects model with controls, absolute SPI, Startdate

	Model 1	Model 2	Model 3	Model 4	Model 5
abs(spi1l1)	0.0228				
	(0.2078)				
logtsconfl2	24.4250***				
	(7.1505)				
logtsconfl2sq	−6.1768***				
	(1.6086)				
logtsconfl2cu	0.5006***				
	(0.1174)				
abs(spi3l1)		0.0657			
		(0.2027)			
logtsconfl4		20.5728***			
		(6.1368)			
logtsconfl4sq		−5.3652***			
		(1.4075)			
logtsconfl4cu		0.4453***			
		(0.1045)			
abs(spi6l1)			0.1041		
			(0.1841)		
logtsconfl7			14.9915**		
			(4.6688)		
logtsconfl7sq			−4.1607***		
			(1.1115)		
logtsconfl7cu			0.3617***		
			(0.0851)		
abs(spi12l1)				0.0531	
				(0.1924)	
logtsconfl13				4.8223***	
				(1.4445)	
logtsconfl13sq				−1.8814***	
				(0.4233)	
logtsconfl13cu				0.1998***	
				(0.0384)	
abs(spi24l1)					0.2395
					(0.1827)
logtsconfl25					2.5996**
					(0.8398)
logtsconfl25sq					−1.2256***
					(0.3059)
logtsconfl25cu					0.1423***
					(0.0308)
AIC	447.6058	448.1090	448.6379	434.9021	432.9912
R^2	0.0023	0.0022	0.0022	0.0021	0.0017
Max. R^2	0.0185	0.0185	0.0186	0.0182	0.0181
Num. events	74	74	74	72	69
Num. obs.	26808	26708	26558	26260	25669
Missings	107	207	357	655	1246

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 12: Conditional logit model with controls, absolute SPI, Startdate

	Model 1	Model 2	Model 3	Model 4	Model 5
(Intercept)	−0.0055	−0.0033	−0.0018	−0.0002	0.0020
	(0.0039)	(0.0030)	(0.0024)	(0.0019)	(0.0015)
drought1	−0.0003				
	(0.0014)				
flood1	0.0015				
	(0.0013)				
logtsconfl2	0.0095**				
	(0.0034)				
logtsconfl2sq	−0.0030**				
	(0.0010)				
logtsconfl2cu	0.0003***				
	(0.0001)				
drought3		−0.0000			
		(0.0014)			
flood3		0.0017			
		(0.0014)			
logtsconfl4		0.0082**			
		(0.0029)			
logtsconfl4sq		−0.0027**			
		(0.0009)			
logtsconfl4cu		0.0003***			
		(0.0001)			
drought6			0.0020		
			(0.0013)		
flood6			−0.0008		
			(0.0014)		
logtsconfl7			0.0076**		
			(0.0025)		
logtsconfl7sq			−0.0027***		
			(0.0008)		
logtsconfl7cu			0.0003***		
			(0.0001)		
drought12				0.0021	
				(0.0013)	
flood12				0.0013	
				(0.0014)	
logtsconfl13				0.0073**	
				(0.0022)	
logtsconfl13sq				−0.0028***	
				(0.0008)	
logtsconfl13cu				0.0003***	
				(0.0001)	
drought24					−0.0009
					(0.0013)
flood24					0.0027
					(0.0014)
logtsconfl25					0.0039
					(0.0021)
logtsconfl25sq					−0.0018*
					(0.0007)
logtsconfl25cu					0.0002**
					(0.0001)
AIC	−81938.2555	−81532.4332	−80926.2943	−80449.2181	−79131.7078
BIC	−81872.6838	−81466.8914	−80860.7976	−80383.8117	−79066.4835
Log Likelihood	40977.1277	40774.2166	40471.1472	40232.6090	39573.8539
Num. obs.	26808	26708	26558	26260	25669
Num. groups: grpmonth	672	672	672	672	659
Var: grpmonth (Intercept)	0.0000	0.0000	0.0000	0.0001	0.0000
Var: Residual	0.0027	0.0027	0.0027	0.0027	0.0026

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 13: Random intercept model with controls, dummy SPI, Startdate

	Model 1	Model 2	Model 3	Model 4	Model 5
(Intercept)	−0.0053	−0.0034	−0.0021	−0.0002	0.0013
	(0.0039)	(0.0030)	(0.0024)	(0.0019)	(0.0015)
abs(spi1l1)	−0.0001				
	(0.0005)				
logtsconfl2	0.0095**				
	(0.0034)				
logtsconfl2sq	−0.0030**				
	(0.0010)				
logtsconfl2cu	0.0003***				
	(0.0001)				
abs(spi3l1)		0.0003			
		(0.0005)			
logtsconfl4		0.0082**			
		(0.0029)			
logtsconfl4sq		−0.0027**			
		(0.0009)			
logtsconfl4cu		0.0003***			
		(0.0001)			
abs(spi6l1)			0.0004		
			(0.0005)		
logtsconfl7			0.0076**		
			(0.0025)		
logtsconfl7sq			−0.0027***		
			(0.0008)		
logtsconfl7cu			0.0003***		
			(0.0001)		
abs(spi12l1)				0.0004	
				(0.0005)	
logtsconfl13				0.0073***	
				(0.0022)	
logtsconfl13sq				−0.0028***	
				(0.0008)	
logtsconfl13cu				0.0003***	
				(0.0001)	
abs(spi24l1)					0.0009
					(0.0005)
logtsconfl25					0.0042*
					(0.0021)
logtsconfl25sq					−0.0018*
					(0.0007)
logtsconfl25cu					0.0002**
					(0.0001)
AIC	−81948.4561	−81542.6132	−80935.5692	−80457.9748	−79141.4760
BIC	−81891.0809	−81485.2641	−80878.2596	−80400.7441	−79084.4047
Log Likelihood	40981.2281	40778.3066	40474.7846	40235.9874	39577.7380
Num. obs.	26808	26708	26558	26260	25669
Num. groups: grpmonth	672	672	672	672	659
Var: grpmonth (Intercept)	0.0000	0.0000	0.0000	0.0001	0.0000
Var: Residual	0.0027	0.0027	0.0027	0.0027	0.0026

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 14: Random intercept model with controls, absolute SPI, Startdate

6.3 Placebo, shifted SPI 24 months into the future

	Model 1	Model 2	Model 3	Model 4	Model 5
lead_drought1	0.0003				
	(0.0014)				
lead_flood1	0.0000				
	(0.0014)				
lead_drought3		−0.0003			
		(0.0018)			
lead_flood3		−0.0004			
		(0.0015)			
lead_drought6			−0.0016		
			(0.0012)		
lead_flood6			−0.0003		
			(0.0014)		
lead_drought12				−0.0014	
				(0.0013)	
lead_flood12				−0.0016	
				(0.0014)	
lead_drought24					0.0012
					(0.0016)
lead_flood24					−0.0001
					(0.0015)
R^2	0.0332	0.0333	0.0334	0.0336	0.0334
Adj. R^2	0.0109	0.0109	0.0109	0.0109	0.0103
Num. obs.	29842	29756	29627	29367	28861
RMSE	0.0651	0.0652	0.0654	0.0651	0.0647

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 15: Placebo fixed effects model with controls, dummy SPI

	Model 1	Model 2	Model 3	Model 4	Model 5
lead_drought1	0.0005				
	(0.0014)				
lead_flood1	0.0002				
	(0.0014)				
logtsconfl2	0.0235***				
	(0.0036)				
logtsconfl2sq	−0.0069***				
	(0.0011)				
logtsconfl2cu	0.0006***				
	(0.0001)				
lead_drought3		0.0000			
		(0.0019)			
lead_flood3		−0.0004			
		(0.0015)			
logtsconfl4		0.0202***			
		(0.0029)			
logtsconfl4sq		−0.0064***			
		(0.0010)			
logtsconfl4cu		0.0006***			
		(0.0001)			
lead_drought6			−0.0015		
			(0.0013)		
lead_flood6			−0.0004		
			(0.0014)		
logtsconfl7			0.0202***		
			(0.0030)		
logtsconfl7sq			−0.0067***		
			(0.0011)		
logtsconfl7cu			0.0006***		
			(0.0001)		
lead_drought12				−0.0018	
				(0.0013)	
lead_flood12				−0.0017	
				(0.0013)	
logtsconfl13				0.0211***	
				(0.0040)	
logtsconfl13sq				−0.0077***	
				(0.0015)	
logtsconfl13cu				0.0007***	
				(0.0001)	
lead_drought24					0.0009
					(0.0014)
lead_flood24					−0.0010
					(0.0014)
logtsconfl25					0.0062
					(0.0056)
logtsconfl25sq					−0.0033
					(0.0019)
logtsconfl25cu					0.0004*
					(0.0002)
R^2	0.0348	0.0350	0.0358	0.0367	0.0357
Adj. R^2	0.0124	0.0124	0.0129	0.0134	0.0115
Num. obs.	29735	29549	29270	28712	27614
RMSE	0.0637	0.0639	0.0641	0.0637	0.0636

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 16: Placebo fixed effects model with controls, dummy SPI

	Model 1	Model 2	Model 3	Model 4	Model 5
abs(spi1lead)	0.0006				
	(0.0006)				
logtsconfl2	0.0234***				
	(0.0036)				
logtsconfl2sq	−0.0069***				
	(0.0011)				
logtsconfl2cu	0.0006***				
	(0.0001)				
abs(spi3lead)		0.0010			
		(0.0008)			
logtsconfl4		0.0202***			
		(0.0029)			
logtsconfl4sq		−0.0063***			
		(0.0010)			
logtsconfl4cu		0.0006***			
		(0.0001)			
abs(spi6lead)			0.0005		
			(0.0006)		
logtsconfl7			0.0201***		
			(0.0030)		
logtsconfl7sq			−0.0067***		
			(0.0011)		
logtsconfl7cu			0.0006***		
			(0.0001)		
abs(spi12lead)				−0.0007	
				(0.0005)	
logtsconfl13				0.0211***	
				(0.0040)	
logtsconfl13sq				−0.0077***	
				(0.0015)	
logtsconfl13cu				0.0007***	
				(0.0001)	
abs(spi24lead)					−0.0004
					(0.0006)
logtsconfl25					0.0062
					(0.0056)
logtsconfl25sq					−0.0034
					(0.0019)
logtsconfl25cu					0.0004*
					(0.0002)
R^2	0.0349	0.0351	0.0358	0.0366	0.0357
Adj. R^2	0.0124	0.0125	0.0130	0.0134	0.0115
Num. obs.	29735	29549	29270	28712	27614
RMSE	0.0637	0.0639	0.0641	0.0637	0.0636

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table 17: Placebo fixed effects model with controls, absolute SPI

	Model 1	Model 2	Model 3	Model 4	Model 5
abs(spi1lead)	0.1842				
	(0.1439)				
logtsconfl2	29.1978***				
	(4.3640)				
logtsconfl2sq	−7.2374***				
	(1.0157)				
logtsconfl2cu	0.5670***				
	(0.0765)				
abs(spi3lead)		0.2216			
		(0.1419)			
logtsconfl4		24.4713***			
		(3.7319)			
logtsconfl4sq		−6.2365***			
		(0.8872)			
logtsconfl4cu		0.4987***			
		(0.0680)			
abs(spi6lead)			0.1475		
			(0.1395)		
logtsconfl7			17.9054***		
			(2.8486)		
logtsconfl7sq			−4.8165***		
			(0.7041)		
logtsconfl7cu			0.3999***		
			(0.0558)		
abs(spi12lead)				−0.1176	
				(0.1592)	
logtsconfl13				4.8587***	
				(0.9136)	
logtsconfl13sq				−1.7654***	
				(0.2752)	
logtsconfl13cu				0.1738***	
				(0.0256)	
abs(spi24lead)					−0.1019
					(0.1616)
logtsconfl25					1.6731***
					(0.4330)
logtsconfl25sq					−0.8949***
					(0.1792)
logtsconfl25cu					0.1031***
					(0.0194)
AIC	796.0189	795.7937	797.6069	785.1948	774.6599
R^2	0.0032	0.0031	0.0030	0.0024	0.0013
Max. R^2	0.0292	0.0294	0.0296	0.0291	0.0287
Num. events	122	122	122	118	113
Num. obs.	29735	29549	29270	28712	27614
Missings	107	293	572	1130	2228

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 18: Placebo conditional logit model with controls, absolute SPI

8 Supplementary information, Identifying the effect of climate variability on communal conflict through randomization

Appendix to Chapter 4

Here we have added supplementary materials that are referred to in the main text.

Table 1A is the placebo test of the main results. This is referred to on page 13 and 14 in the main manuscript

Table 2A shows the analysis using a logit model instead of an OLS model. This is commented on page 11 in the main manuscript

Table 1A: Regressions (24 month lead placebo) (fixed effects not shown)

	1 month	**3 month**	**6 month**	**12 month**	**24 month**
(Intercept)	0.62***	0.62***	0.62***	0.62***	0.62***
	(-0.08)	(-0.08)	(-0.08)	(-0.08)	(-0.08)
Drought	-0.04	-0.04	0.01	0.08*	-0.01
	(-0.04)	(-0.04)	(-0.04)	(-0.04)	(-0.04)
Flood	0.04	0.04	0.01	-0.01	-0.05
	(-0.04)	(-0.04)	(-0.04)	(-0.04)	(-0.04)
R^2	0.23	0.23	0.23	0.23	0.23
Adj. R^2	0.19	0.19	0.19	0.19	0.19
Num. obs.	1518	1518	1518	1518	1518
RMSE	0.35	0.35	0.35	0.35	0.35
(Intercept)	0.32***	0.38***	0.44***	0.54***	0.58***
	(-0.07)	(-0.07)	(-0.08)	(-0.08)	(-0.08)
Drought	-0.03	-0.03	0.01	0.08*	-0.01
	(-0.04)	(-0.04)	(-0.04)	(-0.04)	(-0.04)
Flood	0.05	0.06	0.04	0	-0.05
	(-0.03)	(-0.04)	(-0.04)	(-0.04)	(-0.04)
Log(#Past 6 month Events)	0.19***	0.15***	0.10***	0.05***	0.03**
	(-0.01)	(-0.01)	(-0.01)	(-0.01)	(-0.01)
R^2	0.39	0.33	0.27	0.24	0.23
Adj. R^2	0.36	0.29	0.23	0.2	0.19
Num. obs.	1518	1518	1518	1518	1518
RMSE	0.31	0.33	0.34	0.35	0.35
(Intercept)	0.30***	0.38***	0.45***	0.53***	0.57***
	(-0.07)	(-0.08)	(-0.08)	(-0.08)	(-0.08)
Drought	-0.03	-0.02	0.02	0.09*	-0.01
	(-0.04)	(-0.04)	(-0.04)	(-0.04)	(-0.04)
Flood	0.06	0.06	0.04	0	-0.05
	(-0.03)	(-0.04)	(-0.04)	(-0.04)	(-0.04)
Log(#Past 12 month Events)	0.14***	0.10***	0.07***	0.04***	0.02*
	(-0.01)	(-0.01)	(-0.01)	(-0.01)	(-0.01)
R^2	0.34	0.29	0.26	0.24	0.23
Adj. R^2	0.31	0.26	0.22	0.2	0.19
Num. obs.	1518	1518	1518	1518	1518
RMSE	0.33	0.34	0.35	0.35	0.35
(Intercept)	0.31***	0.38***	0.42***	0.53***	0.59***
	(-0.07)	(-0.07)	(-0.08)	(-0.08)	(-0.08)
|SPI|	0.02	0.01	0.02	0.02	-0.02
	(-0.01)	(-0.01)	(-0.01)	(-0.02)	(-0.02)

Log(#Past 6 month Events)	0.19***	0.15***	0.10***	0.05***	0.03**
	(-0.01)	(-0.01)	(-0.01)	(-0.01)	(-0.01)
R^2	0.39	0.33	0.27	0.24	0.23
Adj. R^2	0.36	0.29	0.23	0.2	0.19
Num. obs.	1518	1518	1518	1518	1518
RMSE	0.31	0.33	0.34	0.35	0.35

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 2A: Conditional logit, absolute SPI + count of last 6 months events before treatment start

	Model 1	Model 2	Model 3	Model 4	Model 5
log((cc1msum6 + 1))	1.18^{***}				
	(0.08)				
abs(spi1l1)	-0.09				
	(0.13)				
log((cc3msum6 + 1))		0.92^{***}			
		(0.07)			
abs(spi3l1)		0.15			
		(0.12)			
log((cc6msum6 + 1))			0.62^{***}		
			(0.07)		
abs(spi6l1)			0.23^{*}		
			(0.11)		
log((cc12msum6 + 1))				0.35^{***}	
				(0.07)	
abs(spi12l1)				0.25^{*}	
				(0.11)	
log((cc24msum6 + 1))					0.28^{***}
					(0.07)
abs(spi24l1)					0.07
					(0.11)
AIC	850.27	946.80	1042.90	1107.33	1127.11
R^2	0.16	0.11	0.06	0.02	0.01
Max. R^2	0.49	0.49	0.49	0.49	0.49
Num. events	331	331	331	331	331
Num. obs.	1698	1698	1698	1698	1698
Missings	30	30	30	30	30

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

9 Supplementary information, Climate variability and individual motivations for participating in political violence

Appendix to Chapter 4

1 Climate variability as instrument

1.1 Assumptions required for an instrument

In order to successfully identify a causal effect, IV models must involve a strong instrument, as well as meet the exclusion restriction and the stable unit value assumption (non–interference). Additionally, when we assume heterogeneous treatment effects, we must assume monotonicity (Gerber and Green 2012; Angrist and Pischke 2008). The following paragraphs will define and discuss these criteria.

As Bound, Jaeger, and Baker (1995) has noted (see especially Equations 4-7, p.444), a weak correlation between the instrument Z and the endogenous treatment X can lead to inconsistencies when there are even small correlations between Z and the error μ. We should assume that there are such small correlations in all finite samples. This means that the instrument needs to be strong enough to make this problem reasonable small. Stock and Yogo (2005) made recommendations as to how large specific statistics measuring instrument strength should be. Specifically, in 2SLS with one instrument and one endogenous regressor, the Cragg-Donald statistic (the first-stage F-statistic) should be above 16.38 for us to assume that the worst-case relative bias (relative to the squared bias of the OLS estimate) is 10% or less (with a 5% significance level), or above 8.96 for us to assume that the worst-case relative bias is 15% or less (Table 5.2 p.101).

The exclusion restriction states that the instrument Z must be unrelated to the final outcome Y except through the treatment X. The central statistical property is that the covariance between Z and the error μ must converge to 0. When it is true, the IV model is consistent[1]. Since we are left with a finite sample, the discussion about whether or not the exclusion restriction has been met is largely a theoretical one.

There are two ways that the exclusion restriction may break: either when Z affects Y through some other causal process, or when Z is dependent on potential outcomes, conditional on covariates (Angrist and Pischke

1. A consistent estimator converges on the true parameter as the number of observations increase. An unbiased estimator is an estimator for which the expected value is the same as the true parameter.

2008, p.117). One way to meet this criterion is by letting the instrument be an external stochastic variable with a known probability distribution, i.e., random.

The non–interference assumption (SUTVA) states that we do not want treated units to affect the potential outcomes of other units. If this happens, then the units that the study treats as untreated are actually treated, or somewhat treated, or vice versa. Such interference can lead to biased estimates, even if assumptions on randomization and excludability are met (Gerber and Green 2012, p.253ff).

Monotonicity means that all who are affected by the instrument are affected in the same direction (p.179ff). We only need to assume monotonicity when we are assuming heterogeneous treatment effects. We have to assume heterogeneous treatment effects when we cannot assume that everyone complies with our instrument. A complier responds to the treatment if and only if she/he/it was assigned to respond through the instrument[2] (Sovey and Green 2011, p.191).

When not everyone is complying with the instrument, we cannot any longer estimate the average treatment effect, since we can only know the average effect for those who comply. The complier average causal effect (CACE), which is what we estimate, is less useful if we want to make general statements about a population, but still useful if we want to find out whether a proposed relation actually does exist, at least for some sub–population.

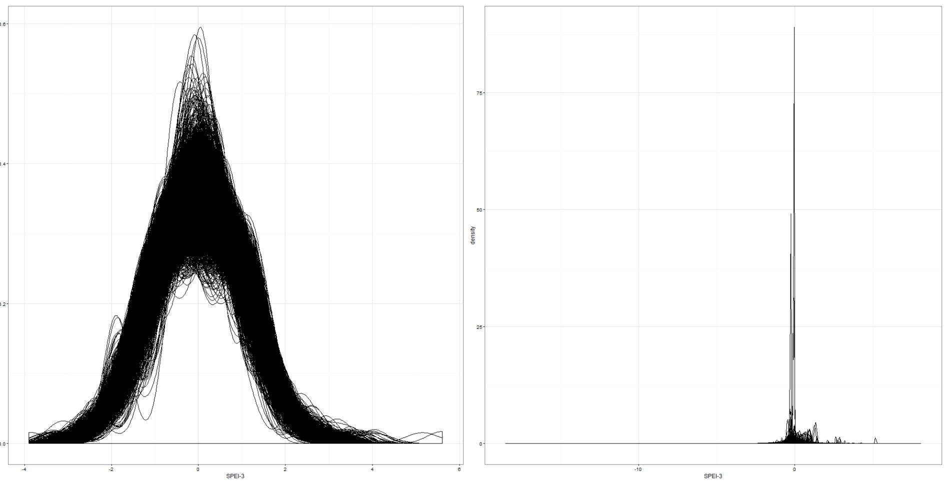
1.2 Random instrument

Rainfall has been used in econometric research to instrument economic outcomes, e.g. Miguel, Satyanath, and Sergenti (2004). However, as Sovey and Green (2011, p.196f) noted, rainfall and rainfall growth can predict factors such as population and mountainous terrain. Although their tests was not jointly significant at the country–level, we should be worried that such correlations can create bias in disaggregated studies.

An alternative that, arguably, addresses some of these problems is the Standardized Precipitation-Evapotranspiration Index (SPEI) (Vicente-Serrano, Beguería, and López-Moreno 2010). SPEI is an estimate of how unusual the precipitation-evapotranspiration system has been for a given interval of time, in comparison to that same interval for a specific area across time. SPEI is derived by estimating a probability distribution of interval (e.g., monthly) average precipitation minus potential evapotranspiration (PET) in a specific area. PET is estimated based on the average temperature in a given interval, and the number of sun hours (determined by the latitude

2. This concept is easier to understand when the instrument and treatment are both binary variables rather than continuous ones.

Figure 1: Empirical distributions for all area-months



(a) Estimated SPEI-3 (b) Estimated SPI-3

and month of the year), using the Thornthwaite equation (Thornthwaite 1948).

The SPEI is an estimate of the underlying probability distribution of the precipitation–evapotranspiration system. The assumption in this article is that these distributions are equal for all areas. It turns out that this assumption is quite reasonable when using SPEI, but not at all so when using SPI (at least for the areas studied here).[3]

In Figure 1, I have plotted the empirical distributions for all area–months in the study, using data from 1948 to 2014. Whereas the distributions for SPEI (Figure 1a) all produce a similarly shaped distribution, those for SPI (Figure 1b) vary more. The central limit theorem says that any sum of several independent distributions tends toward a normal distribution. This is exactly what happens in SPEI, as SPEI is the combination of precipitation minus evapotranspiration.

The instrument is not randomly assigned to each individual. Rather, many individuals are affected by the same SPEI at the same time. This is not a problem for an instrument. In randomized experiments, researchers are often forced to assign treatment to more than one individual at the same time. Clustered random assignment does tend to affect the standard errors of the estimate, however (Gerber and Green 2012, p.80ff). When estimating

3. SPI (Standardized Precipitation Index) is calculated similarly to SPEI. The main difference is that SPI only use precipitation, rather than precipitation minus estimated PET. SPEI and SPI calculations also differ over which general distributional family is being assumed. SPEI originally assumed a log-logistic distribution, whereas SPI assumed a gamma distribution (Vicente-Serrano, Beguería, and López-Moreno 2010, p.1715). I use the same distributional assumptions here.

standard errors under clustered random assignment, we must also cluster the standard errors to account for the fact that the actual number of independent treatments is lower than the number of individual observations.

I argue that SPEI can be treated as random in cross–sections at the first–level administrative unit. The main problem with this argument is that climate systems can be quite large and are in many cases larger than an administrative unit. This can not only make standard errors smaller than they should be, but it can also induce selection bias. To counteract this problem, I take advantage of the fact that the interviews were being done at different times in each country. Thus, it is not the same climate system that is being observed in each. Additionally, for the models where I select SPEI end–month using the timing of the growing season, there is some within–country variation between administrative units.

1.3 Strong instrument

Precipitation and evapotranspiration can affect living conditions through several pathways. The most important are arguably changes in crop yields, availability of (clean) drinking water, sufficient pasture for livestock, conditions for vector-borne diseases and flood damages (Porter et al. 2014; FAO 2016; Delpla et al. 2009; Smith et al. 2014; Thornton, Boone, and Ramirez-Villegas 2015; Vicente-Serrano, Beguería, and López-Moreno 2010). The first three of these are arguably more adverse in Africa during droughts (although as with droughts, floods can destroy crops and clean water sources). Vector-borne diseases, in contrast, are more common in humid environments, and flood damages obviously only occur during floods. This article's working hypothesis, however, is that that crop yields, and availability of clean water and pasture for livestock are the most salient assets — at least in Africa, the area being studied.

The processes which determine crop yields are complex. Heavy rain followed by dry spells, can for instance trigger nitrogen leaching from nutrient poor soils, thus negating the positive contribution the rain would otherwise have had (Tingem and Colls 2008; Ray et al. 2015). Despite huge variation between crops and across space, Ray et al. (2015) conclude that precipitation variability is an important predictor of crop yields in sub-Saharan Africa, due to the prevalence of rain-fed agricultural systems. Following a similar logic, increased use of irrigation reduces the effect of precipitation (Porter et al. 2014). Similarly, Buhaug et al. (2015, p.1) find in sub–Saharan Africa, "a robust link between weather patterns and food production where more rainfall generally is associated with higher yields". Tingem and Colls (2008) find that wetter conditions give higher maize yields with lower variability in tests sites in Cameroon. However, they also found that "all extreme dry and wet years are not equal in terms of their regional manifestation" (p.357). Precipitation is less important as a predictor of crop yields when aggregated to larger areas, than it is when used for highly disaggregated data, due to the large spatial variation in precipitation (Li et al. 2010; Porter et al. 2014).

Seasonal climatic variations are built into local cultures. For instance, transhumance (moving livestock to seasonal pastures) is an adaptation to the expected land productivity as it varies throughout the year. If climatic variations during periods of the year when expected land productivity is high are more important for personal living conditions than when the expected productivity is low, we would be advised to take the seasonal timing of climate events into account (Uexkull et al. 2016). With this understanding in mind, I test whether the instrument is stronger during the growing season of the main crop in the area.

As I argue in the main text, there is reason to believe that the instrument is stronger for rural producers than for urban consumers. I test whether the instrument is stronger in primary sampling units which were classified by the interviewer as "rural" versus PSUs classified as "urban" or "semi–urban".

Since most administrative units have a mix of both urban and rural PSUs, and statistical power when clustering is mainly based on the number of administrative units, I lose little in terms of statistical power by splitting the sample into rural and urban PSUs. The F–statistics in the weak instrument test for each subset will be used to decide whether or not there are differences between these subsets.[4]

1.4 Exclusion restriction

Miguel, Satyanath, and Sergenti (2004, p.745) suggested that excessive rainfall could violate the exclusion restriction because floods could "destroy the road network and thus make it more costly for government troops to contain rebel groups", or could "make it difficult for both government and rebel forces to engage each other in combat". Thus, rainfall could affect the probability of violent conflict onset by other means than creating economic losses. I assume here that such influences are only minor.

1.5 Non–interference

The assumption of non–interference can be difficult to make in field studies of conflict and climate. Individuals in conflict situations observe and are influenced by what others are doing (Smith 1976; Parkinson 2013; Wood 2008). Part of the estimated effect in this study could be explained by the

4. An alternative way to operationalize this theoretical notion would have been to have information about the occupation of the respondent. While there is some information about this in Afrobarometer, unfortunately, the rate of missing responses is very large, making it useless for my purpose.

fact that several people are responding to the treatment and *perceive that others are responding to it*, as opposed to a strictly individual treatment effect. If many would like to participate in violence (and perceive that many others would like to as well) because of a deterioration of living conditions, then each person could think that the risk of participation is lower than if only a few participated (Epstein 2002).

Similarly, for climate, if only a few are affected, then it could be cheap for the government to intervene. If many are affected, then international organizations might take some of the burden. There could therefore be a non–linear function between the proportion treated with extreme weather, and the level of humanitarian intervention designed to prevent negative changes to living conditions. These treatment interferences can introduce bias even if SPEI could be treated as a randomized variable in this setting.

1.6 Heterogeneous treatment effects and monotonicity

SPEI is a better description of extreme/unusual weather than is absolute rainfall, and has a more consistent interpretation over space (McKee, Doesken, and Kleist 1993; Vicente-Serrano, Beguería, and López-Moreno 2010). For instance, 25 mm of precipitation in Oslo, Norway, during January, which is quite dry for the season, usually come in the form of snow and are hastily removed from streets or formed into cross-country ski tracks. The same level of precipitation in the Atacama Desert in Chile would likely lead to mud slides, destroyed roads, and possibly the death of several people (as happened during a flood in the area in the spring of 2015) (The Weather Channel 2015). 25 mm precipitation in Oslo during January yield a different SPEI score, than the same level of precipitation in the Atacama Desert.

We should, however, not assume that unusually dry or wet weather has a homogenous treatment effect on changes in living conditions, or that changes in living conditions have a homogenous treatment effect on participation in violence. This is trivially true since the outcome is not continuous. If there is an effect, it is probably much larger for those uncertain as to whether or not to use violence, than for those who have more or less decided what they will do. That is, the chance that changes in living conditions will affect potential outcomes is larger for those with a relatively high likelihood of both outcomes, than for those who are strongly biased toward one potential outcome. It is also non–trivially true, as we would not expect that the effect on the log–odds of participating would be equal for all. People get varyingly motivated by the same exposure, even if the base probabilities of their potential outcomes are similar.

Even though we do not need to assume treatment homogeneity, we still need to assume monotonicity. The monotonicity assumption would be broken if improvements to living conditions caused some people to participate in political violence (Gerber and Green 2012, p.179ff). While I will be assuming that monotonicity is true in this study, there may be some reasons to think that it is not. What could especially threaten this assumption is that relative deprivation is defined as the discrepancy between what people believe they are entitled to and what they believe they are capable of getting (Gurr 1970, p.24). When people improve their living conditions, they may also change their beliefs about what they are entitled to. If they change their beliefs about what they are entitled to more quickly than their living conditions improve, then their relative deprivation will increase, rather than decrease. In this case, if increased relative deprivation increases the propensity for participation, then the monotonicity assumption will not hold.

2 Model description

POLVIO and PECON are arguably nominal or ordinal scale variables. While ordinal IV models have been proposed, they are still at the experimental stage (Donat and Marra 2015). The options we have left are either to make binary variables, or to treat the variables as continuous. This provides us with four options for two stage models: 1) a model with linear first and second stages, 2) a model with a linear first stage and binary second stage, 3) a model with a binary first stage and a linear second stage and 4) a model with a binary first and second stage. I believe the last choice is the best.

The choice is between making wrong assumptions (that the effect is linear across alternatives) in alternatives 1-3, or maybe making the right assumption while throwing out some information (alternative 4) (Angrist and Pischke 2008, p.197ff). The worst of these alternatives is 3), which is referred to as *forbidden regression* (Wooldridge 2002, p.265ff).

I have little confidence that POLVIO can meaningfully be used as a continuous measurement. The theoretical pathways suggested should not obviously lead those who have already participated in violence to participate more. It is also problematic to treat the “No, but would do if had the chance” statement as being on a linear continuum between “No, would never do this” and “Yes, once or twice”. I use the following definition as the outcome variable.

$$BPOLVIO_i = \begin{cases} 1 & : POLVIO_i \left\{ \begin{array}{l} \text{“Yes, once or twice”} \\ \text{“Yes, several times”} \\ \text{“Yes, often”} \end{array} \right\} \\ 0 & : POLVIO_i \left\{ \begin{array}{l} \text{“No, would never do this”} \\ \text{“No, but would do if had the chance”} \end{array} \right\} \\ NA & : POLVIO_i \left\{ \begin{array}{l} \text{“Don't know”} \\ \text{“Refused to answer”} \\ \text{“Missing”} \end{array} \right\} \end{cases}$$

PECON is more likely to behave well in continuous models. While I will report the alternative 4 in the main results, alternative 2 type models are shown in this appendix. When I use PECON as a continuous variable, I denote this variable as CPECON. When I use PECON as a binary outcome, I denote it as BPECON, which is defined as

$$BPECON_i = \begin{cases} 1 & : PECON_i \, \{\text{“Same”, “Better”, “Much Better”}\} \\ 0 & : PECON_i \, \{\text{“Worse”, “Much Worse”}\} \\ NA & : PECON_i \left\{ \begin{array}{l} \text{“Don't know”} \\ \text{“Refused to answer”} \\ \text{“Missing”} \end{array} \right\} \end{cases}$$

When modeling binary outcomes, I have two alternatives: either using a linear approach such as 2SLS or LIML, or using the recursive bivariate probit model (Heckman 1978). Whereas Angrist and Pischke (2008, p.103) argued that the former alternative should provide consistent marginal effects with binary outcomes, others have argued that their conclusion rests on distributional assumptions that are not always true (Beck 2011, p.4).

I use a semi–parametric implementation of the recursive bivariate probit model, which allows for non-linear splines in both the first and the second stage (Marra and Radice 2011). This means that I can model SPEI as a non–linear spline effect, rather than assuming linearity or making an arbitrary binary cut.

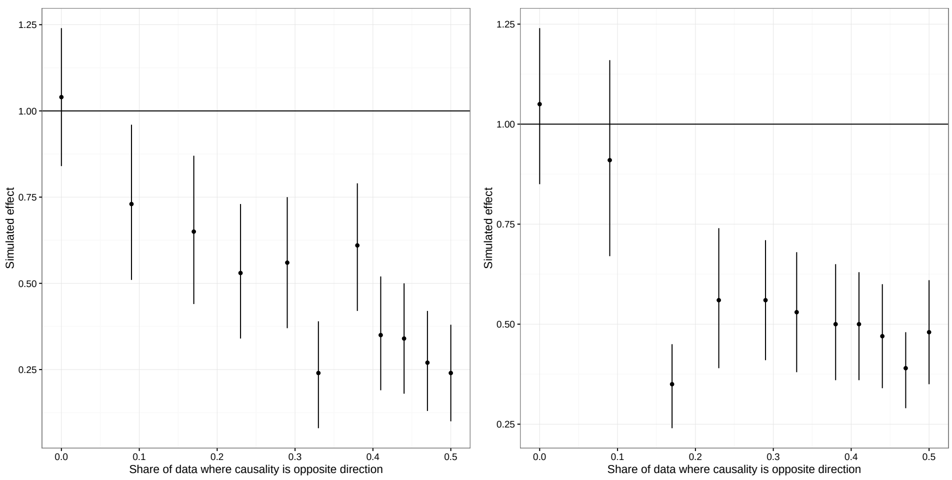
3 Simulation study of the outcome-before-treatment problem

$$Y_1 = \begin{cases} 1 & \text{if } X_1 + \mu_1 > 0, \\ 0 & \text{otherwise.} \end{cases} \tag{1}$$

$$X_1 = \begin{cases} 1 & \text{if } Z_1 + \mu_1 > 0, \\ 0 & \text{otherwise.} \end{cases} \tag{2}$$

$$Z_1 = \mathcal{N}(0,1), \mu_1 = \mathcal{N}(0,1) \tag{3}$$

To illustrate this problem, I created two data-generating processes (DGPs). One is in accordance with the assumptions in the bivariate probit model (and yields consistent estimates), and one treats the outcome in the model (Y_2), as affecting the treatment (X_2), rather than the other way around.



(a) No control

(b) Control

Figure 2: Simulation results

$$Y_2 = \begin{cases} 1 & \text{if } \mu_2 > 0, \\ 0 & \text{otherwise.} \end{cases} \tag{4}$$

$$X_2 = \begin{cases} 1 & \text{if } Y_2 + Z_2 > 0, \\ 0 & \text{otherwise.} \end{cases} \tag{5}$$

$$Z_2 = \mathcal{N}(0,1), \mu_2 = \mathcal{N}(0,1) \tag{6}$$

Then I gradually contaminate observations from the first DGP with observations from the second DGP. The simulation shows what happens when the share (θ) of observations coming from DGP_2 increases.

$$\theta = \frac{N(DGP_2)}{N(DGP_1) + N(DPG_2)} \tag{7}$$

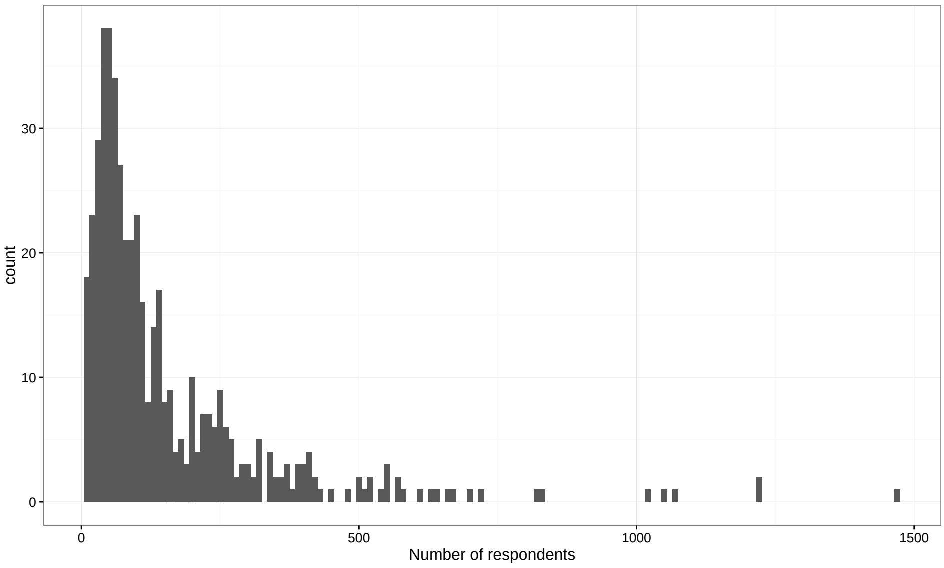
The effect of X_1 on Y_1 is 1. The simulation in Figure 2 shows that the bivariate probit correctly estimates the effect when θ is 0. When adding a control variable that denotes which of the DGPs the observations come from, results are not much better than if not controlling for this, as can seen by the difference in estimates in Figure 2a and 2b.

4 Descriptive statistics and regression tables

	Round 2		Round 5	
Country	From	To	From	To
Algeria			27.01.2013	19.02.2013
Benin			16.11.2011	06.12.2011
Botswana	21.06.2003	23.07.2003	30.06.2012	11.07.2012
Burkina Faso			03.12.2012	17.12.2012
Burundi			26.11.2012	12.12.2012
Cameroon			07.03.2013	02.04.2013
Cape Verde	03.06.2002	14.06.2002	03.12.2011	12.12.2011
Cote d'Ivoire			11.03.2013	25.03.2013
Egypt			08.03.2013	19.03.2013
Ethiopia			01.08.2013	22.09.2013
Ghana	29.08.2002	11.09.2002	08.05.2012	27.05.2012
Guinea			25.03.2013	12.04.2013
Kenya	17.08.2003	23.09.2003	04.11.2011	29.11.2011
Lesotho	24.02.2003	07.04.2003	26.11.2012	21.12.2012
Liberia			24.06.2012	08.07.2012
Madagascar			11.03.2013	07.04.2013
Malawi	29.04.2003	18.05.2003	04.06.2012	01.07.2012
Mali	25.10.2002	23.10.2002	16.12.2012	10.01.2013
Mauritius			12.01.2012	04.03.2012
Morocco			27.04.2013	30.05.2013
Mozambique	11.08.2002	21.08.2002	17.11.2012	09.12.2012
Namibia	15.08.2003	28.09.2003	19.11.2012	18.12.2012
Niger			31.03.2013	15.04.2013
Nigeria	13.10.2003	29.10.2003	29.10.2012	30.11.2012
Senegal	29.11.2002	18.12.2002	17.02.2013	20.03.2013
Sierra Leone			23.06.2012	18.07.2012
South Africa	13.09.2002	13.10.2002	20.10.2011	30.11.2011
Sudan			13.02.2013	23.02.2013
Swaziland			22.05.2013	04.06.2013
Tanzania	05.07.2003	06.08.2003	28.05.2012	30.06.2012
Togo			17.12.2012	29.12.2012
Tunisia			10.01.2013	01.02.2013
Uganda	13.08.2002	05.10.2002	02.12.2011	27.02.2012
Zambia	08.05.2003	05.06.2003	21.01.2013	08.02.2013
Zimbabwe	26.04.2004	17.05.2004	16.07.2012	30.07.2012

Table 1: Survey dates

Figure 3: Number of respondents in each administrative area

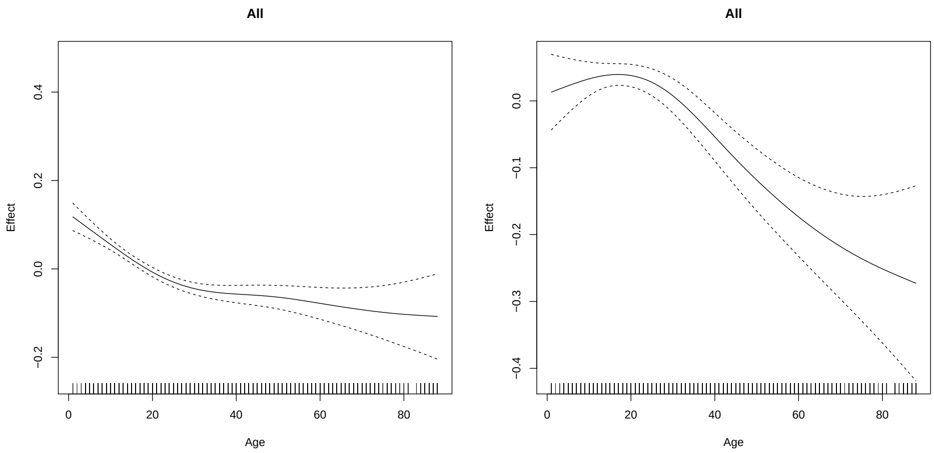


Variable	All	Rural	Urban
(Intercept)	0.491***	0.494***	0.471***
	(0.042)	(0.051)	(0.041)
Male	−0.002	−0.002	−0.002
	(0.01)	(0.012)	(0.017)
Round 5	−0.069	−0.119*	0.035
	(0.048)	(0.058)	(0.048)
s(Age)	edf(3.368)***	edf(4.688)***	edf(3.235)***
s(Country)	edf(32.546)***	edf(32.492)***	edf(31.49)***
s(SPEI-3)	edf(6.917)***	edf(8.368)***	edf(7.046)**
AIC	110422.956	69332.151	40796.843
N	73968	44991	28977

Table 2: Recursive biprobit model, first stage

Variable	All	Rural	Urban
(Intercept)	-1.249^{***}	-1.143^{***}	-1.52^{***}
	(0.173)	(0.196)	(0.276)
BPECON	-0.867^{***}	-0.986^{***}	-0.49
	(0.194)	(0.205)	(0.347)
Male	0.15^{***}	0.136^{***}	0.18^{***}
	(0.019)	(0.023)	(0.031)
Round 5	-0.148^{**}	-0.109	-0.253^{***}
	(0.052)	(0.061)	(0.054)
s(Age)	$edf(3.064)^{***}$	$edf(2.735)^{***}$	$edf(3.284)^{*}$
s(Country)	$edf(31.368)^{***}$	$edf(30.637)^{***}$	$edf(27.546)^{***}$
AIC	110422.956	69332.151	40796.843
N	73968	44991	28977

Table 3: Recursive biprobit model, second stage



(a) Age estimate, first stage model (b) Age estimate, second stage model

Figure 4: Non-linear effect of age

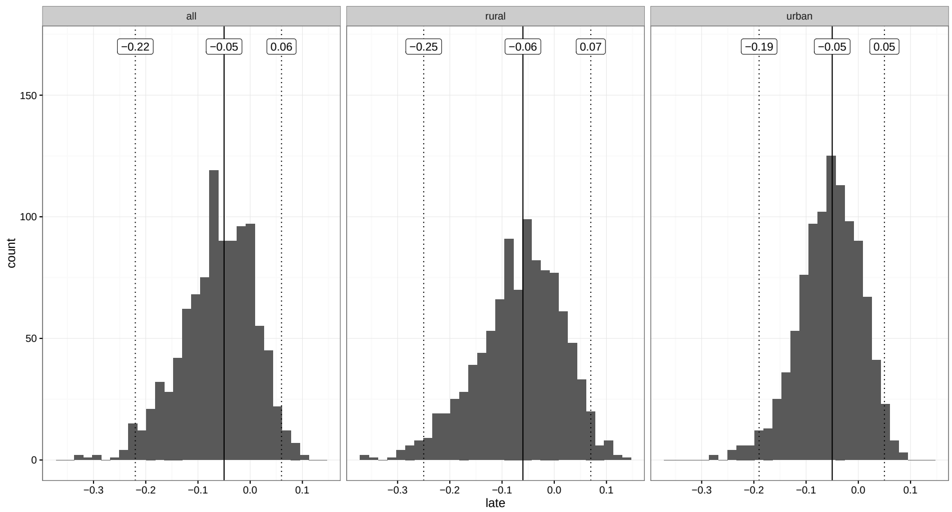
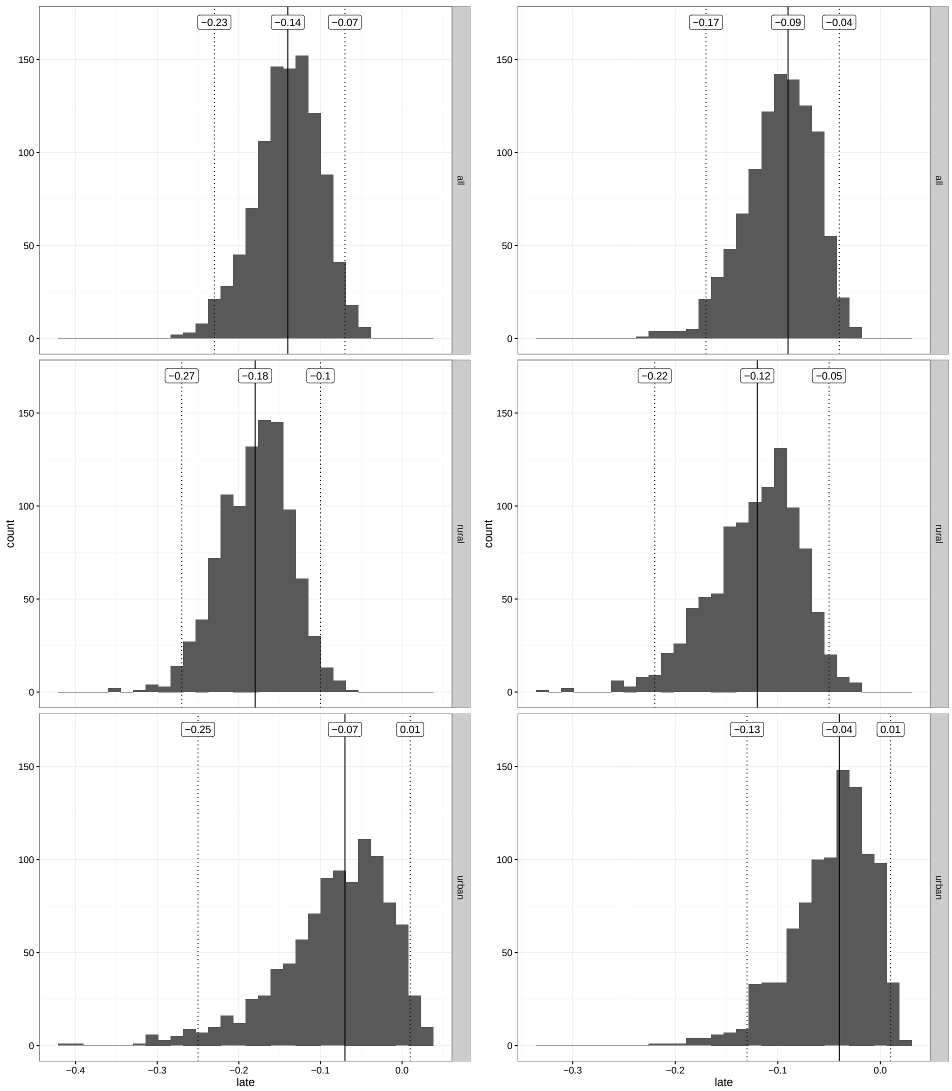


Figure 5: Alternative cut-off for BPOLVIO



(a) Missing observations lost livelihood and participated

(b) Missing observations did not lose livelihood nor participated

Figure 6: Alternative missing assumptions

Table 4: F-statistics, full sample, bpecon, drought

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	1.62	4.45	8.79	0.42	5.95	15.95	2.91	12.07	1.92	1.13	12.56	1.06	1.04
SPEI-3	8.55	3.36	6.81	3.88	7.25	3.34	6.71	4.27	7.80	3.61	6.38	4.66	4.34
SPEI-6	1.62	0.08	9.05	7.01	6.60	2.39	4.48	7.89	3.11	0.11	2.43	6.16	11.19
SPEI-12	1.02	0.23	0.35	0.93	4.52	0.23	0.15	7.29	8.17	2.56	1.22	4.71	3.07

Table 5: F-statistics, full sample, cpecon, drought

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	2.35	4.76	11.81	0.60	4.01	17.27	2.90	7.84	1.45	1.64	10.10	0.51	0.89
SPEI-3	4.26	2.18	5.68	7.35	8.41	5.53	7.83	4.84	6.73	4.98	6.09	6.73	3.29
SPEI-6	2.41	0.28	10.48	10.55	6.36	3.77	5.68	8.29	2.08	0.11	3.36	6.66	9.87
SPEI-12	2.62	0.33	2.29	3.85	6.96	0.00	0.32	6.76	6.69	2.72	1.16	4.12	3.62

Table 6: F-statistics, full sample, bpecon, spei

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.04	17.88	0.55	1.13	11.59	12.92	0.44	12.98	8.01	9.10	18.86	11.01	1.80
SPEI-3	3.90	0.10	11.17	6.65	9.78	8.36	5.57	8.26	17.87	12.53	25.71	14.24	7.39
SPEI-6	0.76	3.22	11.43	7.28	12.75	9.09	6.54	8.21	13.91	9.95	24.30	13.23	11.90
SPEI-12	1.06	4.00	5.06	4.48	8.39	7.92	14.45	18.76	10.24	2.86	4.28	5.48	4.97

Table 7: F-statistics, full sample, cpecon, spei

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.02	13.76	1.21	1.85	10.43	15.08	2.05	8.76	8.25	10.57	15.38	13.69	1.87
SPEI-3	3.58	0.01	13.42	7.91	11.21	8.92	8.19	8.76	16.90	14.18	21.12	13.30	7.60
SPEI-6	1.26	4.20	12.77	9.81	12.27	10.58	7.83	8.37	13.28	11.32	21.39	11.57	13.21
SPEI-12	2.67	5.02	6.52	6.25	8.83	9.38	14.49	17.07	11.48	5.59	4.86	4.39	5.55

Table 8: F-statistics, Rural sample, bpecon, drought

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	2.32	7.04	10.06	0.94	9.45	18.71	2.73	13.86	0.88	1.73	12.89	0.75	1.57
SPEI-3	9.79	3.05	4.76	4.63	8.67	3.52	4.99	2.79	7.04	4.98	5.22	3.58	3.26
SPEI-6	1.49	0.60	10.43	8.73	6.16	2.06	3.63	5.18	2.28	0.38	1.98	5.17	10.63
SPEI-12	1.31	0.08	0.67	1.41	5.13	0.14	0.18	7.04	6.01	3.10	1.05	5.71	3.75

Table 9: F-statistics, Rural sample, cpecon, drought

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	2.79	7.46	12.00	1.04	6.52	20.78	2.42	9.01	0.83	2.27	9.20	0.20	0.75
SPEI-3	4.41	2.35	3.96	6.95	9.42	4.56	5.87	3.83	5.93	5.73	5.08	5.59	2.47
SPEI-6	2.39	0.08	11.22	11.66	5.81	2.94	5.11	6.14	2.04	0.25	2.58	5.10	9.27
SPEI-12	2.34	0.39	2.47	4.44	7.72	0.00	0.55	7.03	5.58	2.74	0.87	5.04	3.58

Table 10: F-statistics, Rural sample, cpecon, spei

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.03	18.02	1.36	1.76	11.06	15.90	1.21	10.49	7.38	9.73	17.13	15.62	1.22
SPEI-3	3.06	0.04	15.23	8.26	11.13	8.01	6.91	7.45	13.66	16.07	22.82	14.06	5.93
SPEI-6	2.78	5.01	13.16	9.02	12.12	9.48	6.80	7.37	11.78	14.05	24.21	12.83	11.97
SPEI-12	3.31	5.14	6.53	6.47	8.89	10.20	15.36	17.53	11.84	7.80	6.29	6.38	5.93

Table 11: F-statistics, Urban sample, bpecon, drought

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.00	0.08	1.59	0.02	0.04	6.13	1.45	5.32	4.38	0.17	3.64	1.38	0.09
SPEI-3	2.39	1.59	6.70	0.80	1.48	0.95	6.34	7.85	5.13	0.52	4.03	5.74	2.57
SPEI-6	0.93	0.19	2.40	1.33	2.95	1.26	3.87	9.86	2.39	0.53	1.49	5.40	5.11
SPEI-12	0.38	0.18	0.00	0.01	1.24	0.48	0.17	4.11	8.79	0.96	0.92	1.02	0.50

Table 12: F-statistics, Urban sample, cpecon, drought

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.01	0.16	3.65	0.01	0.05	6.47	2.24	2.56	2.68	0.48	4.28	1.05	0.55
SPEI-3	1.64	0.64	4.68	3.02	2.43	4.77	7.19	5.78	4.81	1.82	4.99	6.07	1.59
SPEI-6	1.35	0.32	2.64	3.69	3.96	3.94	4.35	8.90	0.95	0.39	3.35	7.07	4.74
SPEI-12	2.01	0.20	0.81	1.16	2.80	0.00	0.08	3.25	6.12	2.32	1.58	1.25	1.41

Table 13: F-statistics, Urban sample, cpecon, spei

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.23	2.42	0.12	1.05	3.59	5.63	1.95	2.70	5.61	4.31	4.38	4.37	1.63
SPEI-3	1.05	0.00	4.65	2.71	4.32	3.94	4.68	5.27	10.84	5.16	6.57	5.42	4.87
SPEI-6	0.17	1.23	5.00	4.59	4.65	4.90	4.43	5.61	7.89	4.27	5.33	3.69	6.10
SPEI-12	0.77	1.81	3.12	2.62	3.56	3.25	5.53	7.96	5.53	1.56	0.99	0.41	2.42

Table 14: F-statistics, full sample, bpecon, drought, placebo

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.01	0.15	2.12	0.43	0.77	0.40	0.52	1.12	0.50	8.66	0.02	0.05	2.24
SPEI-3	1.15	0.95	1.15	1.33	0.18	6.36	1.68	6.59	17.32	10.00	3.81	0.45	11.37
SPEI-6	0.90	2.23	2.75	0.24	2.27	4.87	10.50	15.61	6.83	7.71	0.56	0.05	7.41
SPEI-12	0.75	0.86	7.60	2.76	3.18	9.98	20.78	17.51	7.36	11.19	0.32	11.45	7.67

Table 15: F-statistics, full sample, cpecon, drought, placebo

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.01	0.13	1.38	0.47	0.51	0.45	1.64	0.43	0.31	12.41	0.40	0.03	1.95
SPEI-3	1.29	1.54	2.18	1.42	0.44	6.02	2.31	12.99	16.33	12.17	5.17	0.91	11.43
SPEI-6	1.41	1.93	3.08	0.06	2.44	10.37	13.63	15.00	6.12	6.04	0.01	0.10	8.14
SPEI-12	0.75	0.65	6.79	2.85	3.13	14.21	19.87	16.38	4.97	7.99	0.70	7.37	5.23

Table 16: F-statistics, full sample, bpecon, spei, placebo

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	4.62	6.19	5.50	5.28	1.91	1.35	0.04	0.35	1.31	1.61	10.42	0.12	2.55
SPEI-3	3.81	5.89	6.35	5.70	2.48	0.01	0.02	2.59	8.10	2.52	5.93	0.12	1.60
SPEI-6	1.77	8.96	6.98	5.77	2.38	1.70	1.05	6.20	4.84	5.48	1.98	0.59	1.46
SPEI-12	2.36	3.97	5.87	4.90	4.79	3.86	5.19	8.43	3.12	1.36	0.01	0.12	3.56

Table 17: F-statistics, full sample, cpecon, spei, placebo

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	4.39	7.96	3.65	6.81	2.08	2.26	0.98	2.02	0.21	1.75	14.89	0.29	3.66
SPEI-3	4.18	3.62	7.44	10.19	4.29	1.30	1.08	4.52	8.92	5.58	9.78	0.82	4.42
SPEI-6	1.42	9.86	8.64	10.61	5.04	5.62	4.66	9.26	7.86	7.93	5.05	2.64	4.08
SPEI-12	1.63	3.95	10.09	7.71	6.88	6.37	8.22	10.41	4.21	3.86	1.52	1.65	4.71

Table 18: F-statistics, Rural sample, bpecon, drought, placebo

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.33	1.04	2.53	6.75	1.86	1.82	0.25	0.45	0.20	4.75	0.00	0.06	3.12
SPEI-3	1.96	2.23	3.33	1.95	1.44	5.05	0.70	5.69	13.27	4.53	5.70	0.31	9.60
SPEI-6	2.63	1.91	3.92	0.57	0.89	2.95	6.68	11.52	3.97	3.81	0.00	0.15	6.39
SPEI-12	2.00	2.23	10.27	3.47	1.28	4.86	12.68	11.84	1.92	4.11	4.02	14.60	7.13

Table 19: F-statistics, Rural sample, cpecon, drought, placebo

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.26	5.41	2.27	4.25	1.71	1.39	0.62	0.04	0.21	4.88	0.41	0.18	2.31
SPEI-3	2.18	2.96	5.09	2.28	2.11	3.83	0.72	8.30	10.65	5.58	9.77	0.71	8.40
SPEI-6	4.04	1.94	4.26	1.05	0.82	3.88	9.54	11.57	3.16	3.20	0.06	0.06	7.60
SPEI-12	2.04	1.78	8.16	2.94	1.30	7.50	15.88	12.53	1.47	3.54	1.83	9.23	5.00

Table 20: F-statistics, Rural sample, cpecon, spei, placebo

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	2.96	5.92	4.20	5.96	3.15	3.84	1.89	2.79	0.02	1.20	8.25	0.84	3.17
SPEI-3	3.71	2.83	7.25	11.21	6.56	2.83	1.43	3.25	3.90	3.51	5.93	1.47	7.05
SPEI-6	2.65	9.30	7.18	11.72	7.32	6.57	3.82	6.05	3.94	3.75	3.26	2.79	5.84
SPEI-12	2.46	5.36	9.82	8.93	7.44	5.74	5.82	6.62	2.03	2.63	2.23	2.54	4.46

Table 21: F-statistics, Urban sample, bpecon, drought, placebo

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.85	5.26	1.07	4.22	0.01	0.36	0.32	1.90	1.69	9.17	0.65	0.70	0.59
SPEI-3	0.00	0.08	0.11	0.02	1.38	3.58	3.57	2.40	6.71	8.56	0.18	0.41	5.37
SPEI-6	0.34	1.06	0.35	5.27	2.64	3.77	4.54	7.36	5.75	4.86	2.53	0.00	2.31
SPEI-12	0.13	0.13	1.01	0.38	2.52	7.03	9.51	7.38	7.63	10.13	0.34	1.93	2.24

Table 22: F-statistics, Urban sample, cpecon, drought, placebo

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	0.55	4.81	0.11	1.61	0.27	0.01	1.53	1.94	0.88	17.73	0.11	0.00	0.53
SPEI-3	0.01	0.06	0.02	0.01	0.78	3.22	4.60	4.92	9.97	7.78	0.04	2.98	5.92
SPEI-6	0.81	0.35	0.19	4.35	1.75	7.53	3.68	5.18	7.31	4.06	0.00	0.39	2.05
SPEI-12	0.36	0.38	0.81	0.35	1.52	6.01	4.79	4.31	3.16	6.21	0.44	12.12	0.51

Table 23: F-statistics, Urban sample, cpecon, spei, placebo

	t1-0	t1-1	t1-2	t1-3	t1-4	t1-5	t1-6	t1-7	t1-8	t1-9	t1-10	t1-11	Grow
SPEI-1	3.55	5.44	0.68	1.72	0.52	0.27	0.21	0.66	1.42	1.00	11.02	0.09	4.25
SPEI-3	1.98	1.97	1.88	1.36	0.83	0.00	0.43	4.14	8.36	4.65	7.20	0.01	1.40
SPEI-6	0.07	4.19	3.03	1.60	0.72	1.80	3.21	7.62	8.17	8.52	4.16	0.81	1.63
SPEI-12	0.05	0.25	4.06	2.68	3.23	4.32	6.49	9.83	4.00	2.80	0.22	0.78	3.20

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