Climate and Conflict

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Abstract

We review the emerging literature on climate and conflict. We consider multiple types of human conflict, including both interpersonal conflict, such as assault and murder, and intergroup conflict, including riots and civil war. We discuss key methodological issues in estimating causal relationships and largely focus on natural experiments that exploit variation in climate over time. Using a hierarchical meta-analysis that allows us to both estimate the mean effect and quantify the degree of variability across 55 studies, we find that deviations from moderate temperatures and precipitation patterns systematically increase conflict risk. Contemporaneous temperature has the largest average impact, with each 1σ increase in temperature increasing interpersonal conflict by 2.4% and intergroup conflict by 11.3%. We conclude by highlighting research priorities, including a better understanding of the mechanisms linking climate to conflict, societies' ability to adapt to climatic changes, and the likely impacts of future global warming.

1. INTRODUCTION

The goal of this article is to survey and synthesize the rapidly expanding econometric literature that studies the links between climate and conflict. Until the past decade, neither climate nor conflict had been a core area of inquiry within the field of economics as a whole, and the same holds even for the subfield of development economics, in which their study is arguably most natural. As recently as 2007, when one of the authors carried out a survey of 63 development economics course syllabi (at both the undergraduate and graduate levels) at leading US universities, only a handful of courses mentioned either conflict or climate, and leading development economics textbooks did not contain these words in their subject index (see Blattman & Miguel 2010 for details on the survey). However, both topics have moved to center stage over the past decade and are now widely taught and researched within economics and throughout the broader social science research community. This shift is at least partly a result of greater awareness of the role that climate might play in driving economic outcomes and, in particular, rising public concern about climate change. Similarly, the violent aftermath of the Arab Spring revolutions and the broader fact that conflict remains widespread in most low- and middle-income regions have made it clear to many observers that economic development, political change, and violent conflict are inextricably linked and that armed conflict is not going away any time soon.

In this article, we focus on over 50 quantitative studies that examine the link between climate and conflict using modern econometric methods that make credible attempts to draw causal inferences from data. Illustrating just how new research interest on these topics is within economics, the median year of publication among the studies we consider is 2012. Although the quantitative literature on this topic is very recent, researchers working in other disciplines—including archeology, criminology, geography, history, political science, and psychology—have long debated the extent to which climatic changes are responsible for conflict, violence, or political instability (see, e.g., Huntington 1917, Levy 1995, Homer-Dixon 1999, Anderson et al. 2000, Davis 2001, DeMenocal 2001, Kuper & Kröpelin 2006, Grove 2007, Gleditsch 2012, Scheffran et al. 2012), and historians have connected prolonged periods of extreme climate with the collapse of major human civilizations (see Cullen et al. 2000, Fagan 2000, Haug et al. 2003, Diamond 2005, Yancheva et al. 2007, Buckley et al. 2010). Numerous pathways linking the climate to these outcomes have been proposed. For example, climatic changes may alter the supply of natural resources and lead to disagreement over their allocation or may shape the relative appeal of using violence to achieve an objective. Presently, improvements in data availability, computing, and statistical methods have prompted an explosion of quantitative analyses seeking to test these theories and quantify the strength of these previously proposed linkages. A central goal of this article is to make sense of this diverse and growing body of literature and to chart a productive path forward for future research.

In this article, we use the terms climate and conflict to describe broad classes of variables. Thus, it is worth clarifying our use of these key terms up front.

1.1. Climate

We use the term climate to refer to observations of climatic variables: temperature, rainfall, and water availability, as well as climate indices that proxy for these measures, such as the El Niño Southern Oscillation Index or the Palmer Drought Severity Index. These variables may be averaged over longer or shorter observational periods. Some authors argue that short averaging periods (e.g., annual) describe only the weather or climate variability and thus have little to say about the impact of climate. We do not agree with this view. Societies experience climatic variables in continuous time and respond to both short-lived and long-lived changes, making the frequency of short-lived events an economically relevant feature of the climate. For example, if hot temperatures increase the likelihood of riots in a city—even if extreme temperatures are experienced only for a few hours—then this is important for understanding climate impacts because the frequency of these momentary events may change if the distribution of daily temperatures changes.

1.2. Conflict

We use the term conflict to describe events for which normal patterns of dispute resolution fail. These events are usually violent in nature (although they need not be in all cases); they may involve individuals or groups; they may be organized or disorganized; and they may be personally, politically, or otherwise motivated. Whereas most existing empirical studies examine only a single type of conflict at a time, in this review we examine this comprehensive set of outcomes because different types of conflict are potentially related, and their responses to climate might exhibit some commonalities. Our hope is that jointly evaluating these phenomena might help us better understand each individually.

The rest of the article is organized as follows. Section 2 presents the existing evidence linking climate to conflict and is the core of the article. We begin the section by discussing the key methodological issues in estimating causal relationships in this area and then survey the existing evidence across different types of conflict with particular attention to those studies capable of making credible causal claims. After collecting and standardizing estimated effect sizes across papers, we carry out a hierarchical Bayesian meta-analysis that both allows us to estimate the mean effect of climate variation on conflict outcomes and quantifies the degree of variability in this effect size across studies. Section 3 lays out the leading theoretical mechanisms linking extreme climate to conflict (including both economic theories and noneconomic explanations, such as those from psychology), evaluates the limited body of empirical evidence regarding these channels, and recommends methods for identifying pathways in future work. Section 4 discusses remaining challenges, including data limitations, the need to better understand societies' ability to effectively adapt to climatic changes, and the likely future impacts of global warming. Section 5 concludes.

2. EVIDENCE LINKING CLIMATE TO CONFLICT

Climatic conditions never cause conflict alone, but changes in climate can alter the conditions under which certain social interactions occur and thus have the potential to change the likelihood that conflict results. The situation is similar to the rise in car accident rates during rainy days. Car accidents themselves almost always result from some form of driver or mechanical error; however, heavy rainfall may increase the probability of a critical error or the risk that a small error has cascading effects that in turn generate a crash (perhaps the car begins to fishtail, setting off a multicar accident). Without the possibility of driver or mechanical errors, rainfall would have no effect on car accident rates, but without rainfall, there would still be some accidents. Similarly, climatic conditions are neither necessary nor sufficient for conflicts to occur, but changes in climatic conditions could have measurable impact on the probability and intensity of conflict, holding other conflict-related factors fixed. The central empirical challenge addressed by the literature to date has been to quantify this effect.

2.1. The Empirical Problem

In an ideal experiment, we would observe two identical populations or societies, change the climate of one, and observe whether this treatment leads to more or less conflict relative to the control condition. Because the climate cannot (yet) be experimentally manipulated, research has relied on natural experiments in which plausibly exogenous variation in climatic variables generates changes in conflict risk that can be measured by an econometrician. The central challenge in this context is to identify plausibly homogeneous populations, only some of which are naturally treated with a climatic event, that one can reasonably believe would have behaved similarly had none been subject to a climate treatment (Holland 1986, Freedman 1991).

2.1.1. Cross-sectional approaches. One approach to the above problem would be to assume that populations or societies inhabiting different locations are identical to one another in all respects except their climate, usually after regression adjustment for observable economic, social, and political correlates of conflict. For example, Buhaug (2010a) compares the rate of civil war across different countries in Africa. Yet it seems implausible that the conditions needed for causal inference are met in this setting: There are many ways in which populations and societies differ from one another (e.g., culture, history), many of them unobserved or hard to measure, so we cannot infer whether a climatic treatment has a causal effect (Wooldridge 2002, Angrist & Pischke 2008). In the above example, the cross-sectional analysis by Buhaug (2010a) compares average rates of civil conflict in South Africa and Nigeria (among many comparisons), attributing observed differences to the different climates of these countries—despite the many other potentially important ways in which these countries differ. Hsiang & Meng (2014) revisit this example and explicitly test the assumption that no important variables are missing from the analysis. Perhaps unsurprisingly, they strongly reject the assumption that baseline conflict rates in these countries are comparable, suggesting that they are unlikely to be valid counterfactuals for one another. We take the critique by Hsiang & Meng (2014) seriously and argue that in general, the handful of covariates such as national per capita income or political indices that are commonly used in cross-sectional regression analyses are insufficient to credibly account for the numerous ways in which populations and societies differ from one another. Because the full suite of determinants of conflict is unknown, and many are unmeasured, it is implausible to us that a cross-sectional analysis can explicitly account for all important differences. For this reason, we do not draw causal inferences on the relationship between climate and conflict from cross-sectional analyses in this article and instead rely on panel data approaches.

2.1.2. Identification in time series. Rather than presuming that all confounders are accounted for in a cross-sectional regression, the bulk of recent studies estimate the effect of climate on conflict by using time-series variation for identification, usually in a panel data context. In this research design, a single population serves as both the control population (e.g., just before a change in climatic conditions) and the treatment population (e.g., just after a change in climatic conditions). Inferences are thus based on how a fixed population responds to different climatic conditions that vary over time. Here the assumptions necessary for causal inference are more likely to be met, as the structure, history, and geography of comparison populations are nearly identical. Therefore, we follow Hsiang et al. (2013a) and restrict our attention in this review to studies that use variation over time in a given location to study the climate/conflict relationship.

As pointed out by Hsiang & Burke (2014), the central shortcoming of this approach is the frequency-identification trade-off that emerges because populations and societies evolve at a much faster rate than do many low-frequency climatic changes of interest. For example, if we are interested in the effect of a climate change that takes 100 years to manifest, then the control and treatment populations in our sample must necessarily be roughly 100 years apart on average. However, human populations may change dramatically over 100 years, violating the assumption that the control and treatment populations are largely comparable. This generates a direct tension between our ability to credibly identify causal effects of climate and our ability to examine slow-moving climatic changes. Stated generally, for an outcome Y_t observed at time t, conditional on contemporaneous climatic conditions C_t, the estimate for the effect of a change after a time interval Δt is

$$\hat{\beta} = E\left[Y_{t+\Delta t}|C_{t+\Delta t}\right] - E\left[Y_t|C_t\right]. \tag{1}$$

This estimate approaches the true parameter of interest

$$\beta = E\left[Y_t|C_{t+\Delta t}\right] - E\left[Y_t|C_t\right] \tag{2}$$

so long as Y_t is comparable to $Y_{t+\Delta t}$ conditional on C (and possibly other covariates). This is the identifying assumption of this research design. However, as the frequency $1/\Delta t$ of the climatic variation of interest becomes lower (climatic changes become more gradual), Δt becomes larger, and the assumption that Y_t and $Y_{t+\Delta t}$ are comparable becomes increasingly difficult to justify. This trade-off between the temporal frequencies over which climate effects can be identified and those that we may wish to understand is arguably the central drawback of this approach.

2.2. Econometric Specification

The time-series or panel analyses that we focus on use versions of the general model:

$$\text{conflict\_variable}_{it} = \beta \times \text{climate\_variable}_{it} + \phi_i + \psi_t + \epsilon_{it}, \tag{3}$$

where locations are indexed by i, observational periods are indexed by t, β is the parameter of interest, and ϵ is the error. This modern panel data approach was first introduced (to the best of our knowledge) to the conflict literature by Miguel et al. (2004). If different locations in a sample exhibit different average levels of conflict—because of any number of cultural, historical, political, economic, geographic, or institutional differences between the locations—this will be accounted for by the location-specific fixed effects ϕ_i. Time fixed effects ψ_t flexibly account for other time-trending variables such as economic growth or gradual demographic changes that could be correlated with both climate and conflict. In some cases in the existing literature, the ψ_t parameters may be replaced by a generic trend (e.g., $\overline{\psi} \times t$), which is possibly nonlinear and either is common to all locations or may be a vector of location-specific trends (e.g., $\overline{\psi}_i \times t$). In many cases, Equation 3 also includes in the covariates the climate variable of interest lagged and possibly controls for nuisance climate variables (e.g., rainfall, if temperature is the variable of interest) that are treated as controls because they may be correlated with the climate variable of interest (Auffhammer et al. 2013).

We first summarize conclusions from the literature as initially presented by the authors in their preferred regression specification. These models may have different structure imposed on their trends, different climate controls, and different lags of climate variables. However, all include location-specific fixed effects as they are central to the credibility of the result, and this is the methodological selection criterion we employ for inclusion in this review.

These author-preferred results are useful to highlight because individual authors are likely to have detailed knowledge about the contexts they study, and thus it is reasonable to believe that they would be more likely to choose an appropriate econometric model based on this insight (i.e., recognizing which climate variables are most influential or which time controls are most important). However, differences in authors' econometric modeling approaches present some difficulties when comparing results across studies. It is possible that authors selectively focus on their strongest results in terms of coefficient magnitude or statistical significance, introducing bias. For these reasons, we also present a second set of empirical results in which we use a single standardized specification including both contemporaneous and lagged terms for all available climate variables. In cases in which the authors did not present such a specification in their paper, we obtain the original data wherever possible and reanalyze the results using this approach, or we contacted authors and received updated analyses from them in line with our specification. In total, 24 studies were reanalyzed out of the 56 total studies included in this review.[1]

2.2.1. Lag structure, displacement, and persistence. In cases in which authors present a distributed-lag model or when we reanalyze a data set, the general form of the estimation equation is

$$\text{conflict\_variable}_{it} = \beta_0 \times \text{climate\_variable}_{it} + \beta_1 \times \text{climate\_variable}_{i,t-1} + \phi_i + \psi_t + \epsilon_{it}, \qquad (4)$$

where β_1 is the effect of the prior period's climate $(t-1)$ on conflict in the present period (t), and β_0 is the contemporaneous effect. β_1 might be nonzero for three reasons. First, climatic events might induce conflicts to be displaced in time, for instance, delaying a conflict that will eventually occur anyway or accelerating the emergence of a conflict that would have otherwise occurred in the future. In either case, β_0 and β_1 would have opposite signs but be equal in magnitude, and the net effect of the climatic event—the sum of β_0 and β_1—would be zero. If there is an increase in the number of contemporaneous conflicts in addition to a displacement of conflicts forward in time (i.e., partial displacement), then the lagged effect will be negative, but the cumulative effect will remain positive (Hsiang et al. 2014). In the presence of either full or partial displacement, estimating Equation 3 instead of Equation 4 will overstate the effect of climate on conflict.

Second, it is also possible that climate events could have persistent or delayed effects on conflict. Suppose that conflicts in rural regions are more likely when agricultural productivity is low (an oft-cited mechanism, discussed in detail below). Because agricultural growing seasons are long and often span calendar years, a climatic event early in the growing season might affect the harvest in the next calendar year, which could result in a zero coefficient on β_0 and a nonzero β_1. Third, bad shocks can persist: A bad agricultural harvest in one year could lower the resources available to invest in the next year's crop, lowering productivity in that year as well. In this setting, β_0 and β_1 would have the same sign.

If any of these dynamics are at play, then estimating Equation 4 and summing contemporaneous and lagged effects will likely provide a more complete picture of the climate/conflict relationship than estimating Equation 3 alone. Therefore, when presenting our second set of results using the standardized statistical model, we compute the cumulative effect of each climate variable by summing the effect of current and lagged climate conditions. We focus on this cumulative effect because our primary interest is whether climatic effects cause a net change in conflict rates over time, but inspection of individual lags also allows us to study whether climate events merely displace conflict into the future or whether climate events have delayed or persistent effects.

2.2.2. Nonlinear responses. Some studies assume a linear relationship between climatic factors and conflict risk, whereas others assume a nonlinear relationship. Taken as a whole, the evidence suggests that over a sufficiently large range of temperatures and rainfall levels, both temperature and precipitation are likely to have a nonlinear relationship with conflict. However, this curvature is not apparent in every study, probably because the range of temperatures or rainfall levels observed within a given sample is limited. Thus, most studies report only linear relationships that should be interpreted as local linearizations of a possibly curved global response function.

[1]We obtained data and performed the reanalysis ourselves on 24 studies, and for 5 other studies, the authors were kind enough to perform the reanalysis for us and share the results.

Some historical studies that examine temperate locations during cold epochs find that abrupt cooling from an already cold baseline temperature may lead to conflict. Yet, as we detail below, modern studies occurring during the current relatively warm epoch (1950–present) overwhelmingly obtain the result that warmer temperatures lead to more conflict. Taken together, this collection of locally linear relationships suggests a global U-shaped relationship between temperature and conflict—with most modern societies now on the warm, upward-sloping portion of the response curve.

In studies of rainfall effects, not all authors use the same parameterization of rainfall as an explanatory variable, making the determination between linear and nonlinear responses more difficult. In a few cases in which a wide range of rainfall levels is observed, such as the study by Hidalgo et al. (2010) examining the effect of rainfall on land invasions in Brazil, there is a clear nonlinear effect of rainfall on conflict. In an effort to make findings from these particular studies comparable to studies with linearized effects (the majority of studies), we follow the approach of Hidalgo et al. (2010) and use the absolute value of rainfall deviations from the mean as the independent variable, effectively linearizing the response function to rainfall on both sides of a U-shaped relationship.

2.2.3. Bad control. Some studies expand Equations 3 and 4 to explicitly control for potential confounding factors, such as average national income. For example, Buhaug (2010a) alters the analysis of a temperature–war association studied by Burke et al. (2009) to include indices for political exclusion and average income. Although well intentioned, this approach may introduce bias in the coefficients describing the effect of climate on conflict because these controls are endogenously determined and may themselves be affected by climate variation. This can cause the signal in the climate variable of interest to be inappropriately absorbed by the control variable or the estimate to be biased because societies differ in unobserved ways that become artificially correlated with climate when the control variable is included. This approach is commonly termed bad control (Angrist & Pischke 2008) and is a particular difficulty in this setting because climatic variables may affect so many of the socioeconomic factors commonly included as control variables, such as crop production, infant mortality, population (via migration or mortality), and even political regime type. To the extent that these outcome variables are used as covariates, studies might draw mistaken conclusions about the relationship between climate and conflict. A detailed discussion of this issue specific to the climate–conflict context is provided by Hsiang et al. (2013a). In what follows, we modify estimates that rely on this method by excluding bad controls in our reanalysis.

2.2.4. Reduced-form estimates versus instrumental variables approaches. We focus on the reduced-form relationship between climatic variables and conflict variables. Many studies follow the influential early analysis by Miguel et al. (2004) who use rainfall as an instrumental variable for economic growth when examining the effect of growth on civil conflict. However, with an increasing number of studies showing that climatic events can affect a variety of socioeconomic outcomes (see Dell et al. 2014 for a review), the key identifying assumption in the instrumental variables approach—in that case, that climate only affects conflict through a particular intermediary variable—becomes increasingly implausible. Climatic events affect many factors that may in turn affect conflict, such as agricultural income (Schlenker & Lobell 2010), human health (Burke et al. 2015b), and residential mobility (Bohra-Mishra et al. 2014). Given these difficulties, we instead focus in this review on the total effect of climatic events on conflict as described by the reduced form. We interpret the reduced form as the net effect of climate on conflict operating through numerous potential channels. Formally, we conceptualize this total effect as

$$\frac{d\,\text{conflict\_variable}}{d\,\text{climate\_variable}} = \sum_i \frac{\partial\,\text{conflict\_variable}}{\partial\,\text{pathway}_i} \cdot \frac{\partial\,\text{pathway}_i}{\partial\,\text{climate\_variable}}, \tag{5}$$

where pathway_i is some variable characterizing an intermediary mechanism, such as income or human aggression. We focus on this total effect as a natural starting point because it is a quantity that can be reliably measured, but we turn to a discussion of potential mechanisms and the extent of evidence supporting specific channels in Section 3. In our view, identifying these channels is a central task for future research.

2.3. What We Know So Far

Econometric studies have examined different conflict outcomes that span the full spectrum of scales of human organization. For example, **Figure 1** displays results from four different studies that analyze nested spatial scales: Tanzanian villages (Miguel 2005), 1° grid squares of East Africa (O'Loughlin et al. 2012), countries in Sub-Saharan Africa (Burke et al. 2009), and the global tropics (Hsiang et al. 2011). The top panels display the nested structure of the spatial units analyzed, and the corresponding bottom panels display the conditional probability of conflict at that unit of analysis. These watercolor regression plots use the darkness of color saturation to display the probability that the conditional expectation function passes through a given point, out to the 95% confidence interval, using a bootstrapped distribution of estimates (Hsiang 2012).

At all scales, although there is some commonality in how conflict rates respond to climatic events, we observe enough systematic differences that we categorize conflicts into two classes that we subsequently evaluate separately: (*a*) interpersonal conflict and (*b*) intergroup conflict. Interpersonal conflicts are conflicts between individuals, which include various acts commonly described as crime, such as assault, rape, and robbery, as well as other types of conflict that may not necessarily be criminal, such as violence in sporting events, road rage, and violent acts by police. Intergroup conflicts are conflicts between collections of individuals, such as organized political violence, civil conflicts, wars, riots, and land invasions.

Hsiang et al. (2013a) describe a third class of conflict termed institutional breakdown and population collapse. These events, such as the disintegration of Chinese dynasties and the collapse of Icelandic populations, are certainly of interest to economists; however, most of the quantitative analyses we draw on to understand these events is undertaken by archeologists, paleoclimatologists, and historians who do not analyze these data in the econometric framework described above. For this reason, we have omitted these studies from this review, although we do think there are important economic insights to be gleaned from these studies, and we encourage economists to analyze these events in future research, especially as new panel data become available.

In new work, Baysan et al. (2014) describe a fourth class of violent conflict that may also respond to climatic events: intrapersonal conflict. Using the econometric framework above, Baysan et al. demonstrate that suicides in Mexico are positively correlated with local temperature. However, because this is the only study of the subject to employ this econometric approach (to our knowledge), we do not review this fourth class here either, although we do discuss this study in the context of other classes of conflicts that it analyzes (including both interpersonal and intergroup violence). We hope that future work will establish whether an effect of climate on suicide and other forms of intrapersonal violence is present in other contexts.

2.3.1. Interpersonal conflict. Experimental studies in psychology have long observed that individuals are more likely to behave violently toward one another if ambient temperatures are higher

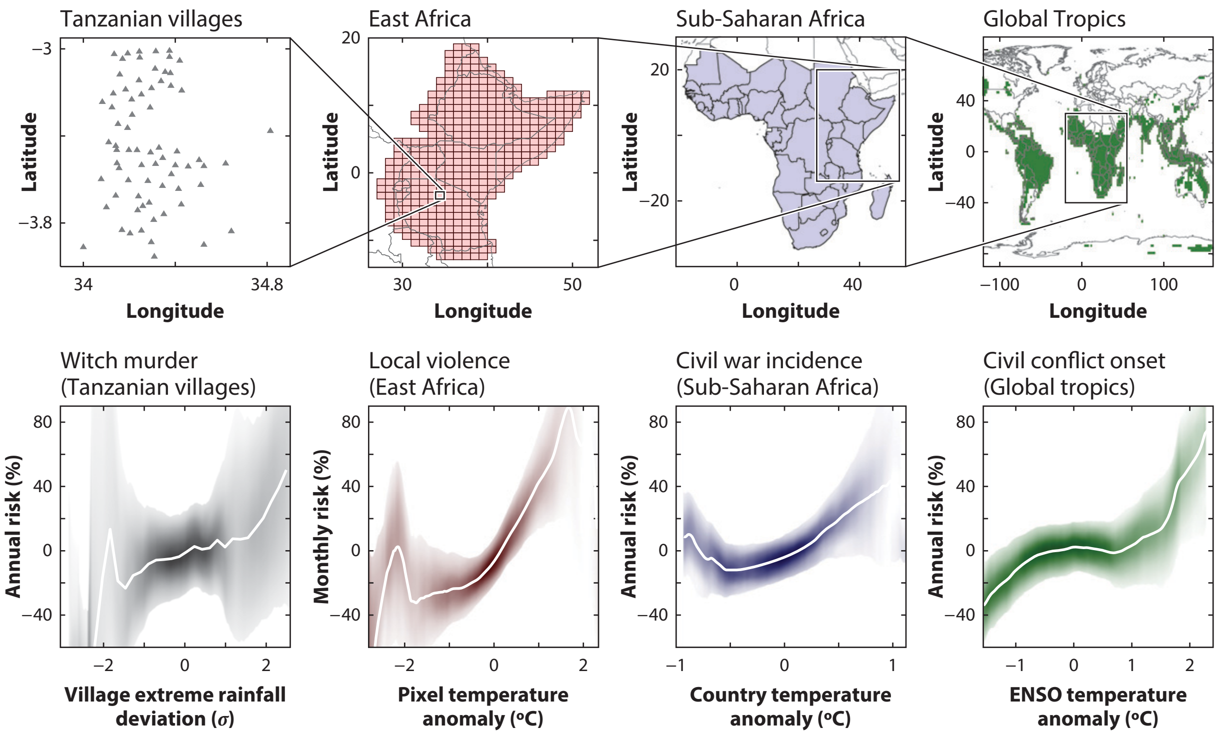

Figure 1

Climate and conflict across spatial scales, showing evidence that climate influences the risk of modern human conflict. Murder in Tanzanian villages increases with more extreme rainfall (data from Miguel 2005), local violence in 1° grid cells increases with temperature (data from O'Loughlin et al. 2012), civil war in African countries increases with temperature (data from Burke et al. 2009), and civil conflict risk in the tropics increases with sea-surface temperature (data from Hsiang et al. 2011). The top row illustrates nested spatial scales of analysis, and the bottom row displays estimated effects. Both dependent and independent variables have had location effects and trends removed, so all samples have a mean of zero. We reanalyze relationships between climate and conflict outcomes and display them as nonparametric watercolor regressions, in which the color intensity of 95% confidence intervals depicts the likelihood that the true conditional expectation function passes through a given value; darker is more likely (Hsiang 2012). The white line in each panel denotes the conditional mean. Abbreviation: ENSO, El Niño Southern Oscillation.

(Rohles 1967, Kenrick & MacFarlane 1986, Vrij et al. 1994). This behavior might contribute to results of natural experiments that use versions of Equation 3, in which it is generally found that crimes between individuals—particularly violent crimes such as assault, murder, rape, and domestic violence—tend to increase at higher temperatures in Australia (Auliciems & DiBartolo 1995), India (Blakeslee & Fishman 2014, Iyer & Topalova 2014), Mexico (Baysan et al. 2014), the Philippines (Wetherley 2014), Tanzania (Burke et al. 2014), and the United States (Anderson et al. 1997, 2000; Cohn & Rotton 1997; Rotton & Cohn 2000; Bushman et al. 2005; Jacob et al. 2007; Card & Dahl 2011; Mares 2013; Ranson 2014). **Table 1** presents a detailed listing of these studies.

Ranson (2014) is among the most detailed of these analyses. Using US data, Ranson separately documents a nonlinear effect of temperature on property crime, with property crimes increasing up until about 70°F, and documents a remarkably linear effect of temperature on violent crimes, such as murder. Remarkably, the effect of temperature on crime in the United States has remained virtually unchanged since 1960 and is relatively uniform across the country. Jacob

Table 1 Unique quantitative studies testing for a relationship between climate and conflict, violence, or political instability

Study	Sample period	Sample region	Time unit	Spatial unit[a]	Independent variable	Dependent variable	Reject $\beta = 0$[b]
Interpersonal conflict (N = 18)							
Anderson et al. (2000)[c]	1950–1997	United States	Year	Country	Temperature	Violent crime	Yes
Auliciems & DiBartolo (1995)[d]	1992	Australia	Week	Municipality	Temperature	Domestic violence	Yes
Baysan et al. (2014)[d,e,f]	1990–2007	Mexico	Month	Province	Temperature	Murder and suicide	Yes (Yes)
Blakeslee & Fishman (2014)[d]	1971–2000	India	Year	District	Rain	Violent and property crime	No[g]
Card & Dahl (2011)[d,f]	1995–2006	United States	Day	Municipality	Temperature	Domestic violence	Yes (Yes)
Cohn & Rotton (1997)[h]	1987–1988	United States	Hours	Municipality	Temperature	Violent crime	Yes
Iyer & Topalova (2014)[d,h]	1971–2010	India	Year	District	Rain	Violent and property crime	Yes
Jacob et al. (2007)[d,e,f]	1995–2001	United States	Week	Municipality	Temperature	Violent and property crime	Yes (Yes)
Kenrick & MacFarlane (1986)[i]	1985	United States	Day	Site	Temperature	Road rage	Yes
Larrick et al. (2011)[d,f]	1952–2009	United States	Day	Site	Temperature	Violent retaliation	Yes (Yes)
Mares 2013	1990–2009	United States	Month	Municipality	Temperature	Violent crime	Yes
Mehlum et al. (2006)	1835–1861	Germany	Year	Province	Rain	Violent and property crime	Yes
Miguel (2005)[d,e,f]	1992–2002	Tanzania	Year	Municipality	Rain	Murder	Yes (No)
Ranson (2014)[d,e,f]	1960–2009	United States	Month	County	Temperature	Violent crime	Yes (Yes)
Rotton & Cohn (2000)[h]	1994–1995	United States	Hours	Municipality	Temperature	Violent crime	Yes

(*Continued*)

Table 1 *(Continued)*

Study	Sample period	Sample region	Time unit	Spatial unit[a]	Independent variable	Dependent variable	Reject $\beta = 0$[b]
Sekhri & Storeygard (2013)[d]	2002–2007	India	Year	Municipality	Rain	Murder and domestic violence	Yes
Vrij et al. (1994)[i]	1993	Netherlands	Hours	Site	Temperature	Police use of force	Yes
Wetherley (2014)[d]	1990–2008	Philippines	Year	Province	Rain	Violent and property crime	Yes
Intergroup conflict and institutional change (N = 38)							
Anderson et al. (2013)	1100–1800	Europe	Decade	Municipality	Temperature	Minority expulsion	Yes
Bai & Kung (2011)	220–1839	China	Decade	Country	Rain	Nomadic invasion	Yes
Baysan et al. (2014) [f,j,k]	1990–2007	Mexico	Month	State	Temperature	Drug cartel murder	Yes (Yes)
Bellemare (2015)	1990–2011	Global	Month	Country	Natural disaster	Social unrest	Yes
Bergholt & Lujala (2012)[f,j,k]	1980–2007	Global	Year	Country	Flood/storm	Civil conflict	No (No)
Bohlken & Sergenti (2010)[f,j,k]	1982–1995	India	Year	Province	Rain	Ethnic riot	(No)
Brückner & Ciccone (2011)[f,j,k]	1980–2004	Sub-Saharan Africa	Year	Country	Rain	Institutional change	No (No[g])
Buhaug (2010a)[f,j,k]	1979–2002	Sub-Saharan Africa	Year	Country	Temperature	Civil conflict	No (No)
Burke (2012)[f,j,k]	1963–2001	Global	Year	Country	Rain/temperature	Leadership exit	No (No[g])
Burke & Leigh (2010)[f,j,k]	1963–2007	Global	Year	Country	Rain/temperature	Institutional change	No (Yes)
Burke et al. (2009)[f,j,k,l]	1981–2002	Sub-Saharan Africa	Year	Country	Temperature	Civil conflict	Yes (Yes)
Caruso et al. (2014)[j]	1990–2003	Indonesia	Month	Province	Temperature	Civil conflict	No
Cervellati et al. (2011)	1960–2005	Global	Year	Country	Drought	Civil conflict	Yes

(*Continued*)

Table 1 (*Continued*)

Study	Sample period	Sample region	Time unit	Spatial unit[a]	Independent variable	Dependent variable	Reject $\beta = 0$[b]
Chaney (2013)	641–1438	Egypt	Year	Country	Nile floods	Political instability	Yes
Couttenier & Soubeyran (2014)[f,j,k]	1957–2005	Sub-Saharan Africa	Year	Country	Palmer Drought Severity Index	Civil conflict	No (No)
Dell et al. (2012)[f,j,k]	1950–2003	Global	Year	Country	Temperature	Political instability and civil conflict	Yes (Yes)
Fetzer (2014)[j]	2000–2006	India	Quarter	District	Rain	Civil conflict	Yes
Fjelde & von Uexkull (2012)[f,j,k]	1990–2008	Sub-Saharan Africa	Year	Province	Rain	Communal conflict	Yes (No[g])
Harari & La Ferrara (2013)[j]	1960–2010	Africa	Year	Pixel (1°)	Drought	Civil conflict	Yes
Hendrix & Salehyan (2012)[f,j,k]	1991–2007	Sub-Saharan Africa	Year	Country	Rain	Social conflict	Yes (Yes)
Hidalgo et al. (2010)[f,j,k]	1988–2004	Brazil	Year	Municipality	Rain	Land invasions	Yes (Yes)
Hsiang et al. (2011)[f,j,k]	1950–2004	Global	Year	World	El Niño Southern Oscillation	Civil conflict	Yes (Yes)
Hsiang et al. (2013b)[f,j,k]	1989–2004	Kenya	Year	Pixel (0.25°)	Temperature	Intergroup and political conflict	Yes (Yes)
Jia (2014)	1470–1900	China	Year	Province	Drought/flood	Peasant rebellion	Yes
Kim (2015)[j]	1960–2005	Global	Year	Country	Rain	Coup	Yes
Kung & Ma (2014)	1651–1910	China	Year	District	Rain	Peasant rebellion	Yes
Lee et al. (2013)	1400–1999	Europe	Decade	Region	North Atlantic Oscillation	Armed conflict	Yes
Levy et al. (2005)[f,j,k]	1975–2002	Global	Year	Pixel (2.5°)	Rain	Civil conflict	Yes (No[g])
Maystadt & Ecker (2014)[j]	1997–2009	Somalia	Month	Province	Temperature	Civil conflict	Yes
Maystadt et al. (2015)[j]	1997–2009	Sudan	Quarter	Pixel (0.5°)	Temperature	Civil conflict	Yes

(*Continued*)

Table 1 *(Continued)*

Study	Sample period	Sample region	Time unit	Spatial unit[a]	Independent variable	Dependent variable	Reject $\beta = 0$[b]
Miguel et al. (2004)[f,j,k,m]	1979–1999	Sub-Saharan Africa	Year	Country	Rain	Civil war	Yes (Yes)
O'Loughlin et al. (2012)[f,j,k]	1990–2009	East Africa	Month	Pixel (1°)	Temperature	Civil conflict	Yes
Salehyan & Hendrix (2012)	1979–2006	Global	Year	Country	Rain	Civil conflict	Yes
Sarsons (2011)	1970–1995	India	Year	Municipality	Rain	Ethnic riots	Yes
Theisen et al. (2011)[f,j,k,n]	1960–2004	Africa	Year	Pixel (0.5°)	Rain	Civil conflict	No (No)
Tol & Wagner (2010)	1500–1900	Europe	Decade	Region	Temperature	Transboundary conflict	Yes
Zhang et al. (2006)	1000–1911	China	Decade	Country	Temperature	Civil conflict	Yes
Zhang et al. (2007)[o]	1400–1900	Northern Hemisphere	Century	Region	Temperature	Transboundary and civil conflict	Yes
56 total studies (24 reanalyzed)							**46 (44)**[p]

[a]Province refers to the first subnational administrative unit and district to the second subnational administrative unit for all nations.
[b]"Reject $\beta = 0$" is Yes if the authors reject an effect size of 0 at the 95% confidence level; our corresponding reanalysis results are in parentheses.
[c]See also Anderson et al. (1997).
[d]Shown in **Figure 2**.
[e]Shown in **Figure 4**.
[f]Reanalyzed using the common statistical model containing location and time fixed effects.
[g]The effect size in the study is statistically significant at the 10% level but not at the 5% level.
[h]See also discussion in Bushman et al. (2005).
[i]Actual experiment.
[j]Shown in **Figure 3**.
[k]Shown in **Figure 5**.
[l]See also discussion in Buhaug (2010a,b), Buhaug et al. (2010), Burke et al. (2010a,b,c), and Sutton et al. (2010).
[m]See also discussion in Ciccone (2011) and Miguel & Satyanath (2011).
[n]See also Hsiang et al. (2013c).
[o]See also Zhang et al. (2011).
[p]This is the number of studies rejecting a zero effect, with the number in parentheses incorporating our reanalyzed results.

et al. (2007) document that there is some temporal displacement of crimes in the United States, with high temperatures elevating crimes in the contemporaneous week but then lowering crimes in later weeks, although the combined effect is significantly positive. Larrick et al. (2011) document that the probability of violent retaliation in sporting events increases on hot days, and Vrij et al. (1994) find that police officers are more likely to draw and fire their weapons at an assailant during a training simulation conducted at hot temperature.

In low-income settings, extreme rainfall events that adversely affect agricultural income (too much or too little rain) are also associated with higher rates of personal violence and property crime (Miguel 2005, Mehlum et al. 2006, Sekhri & Storeygard 2013, Blakeslee & Fishman 2014, Iyer & Topalova 2014). Analysis of US data suggests that rainfall does not have substantial effects on crime in this primarily nonagricultural economic setting (Ranson 2014).

Wetherley (2014) provides the only study, to our knowledge, to estimate the effect of climatic disasters on crime. Wetherley finds that a year after Filipino communities are exposed to strong typhoon winds, property crime rises. This lagged structure mirrors the lagged effect of typhoons on household income and consumption in the Philippines (Anttila-Hughes & Hsiang 2012).

2.3.2. Intergroup conflict. Historical analyses of temperate or cold locations have found that anomalously cold events during cold epochs are associated with episodes of political instability in dynastic China and feudal Europe (Zhang et al. 2006, 2007, 2011; Tol & Wagner 2010) as well as interethnic violence in Europe (Anderson et al. 2013). Drying events, periods of low rainfall, have also triggered political instability in historical Egypt (Chaney 2013), Europe (Lee et al. 2013), and China (Jia 2014, Kung & Ma 2014) and have fueled transboundary invasions of nomadic populations in historical China (Bai & Kung 2011).

In analyses of the modern period (1950–present), a relatively warm epoch by historical standards, high temperatures elevate the risk of many forms of intergroup conflict, both political violence and other forms of collective violence (Burke et al. 2009; Hsiang et al. 2011, 2013b; Dell et al. 2012; O'Loughlin et al. 2012; Baysan et al. 2014; Caruso et al. 2014; Maystadt & Ecker 2014; Maystadt et al. 2015). In all cases, these effects are observed primarily in low- and middle-income settings in which populations are exposed to warm or hot temperatures on average. This linear effect is conspicuously similar in sign and structure to the response of interpersonal conflict to temperature, and it can be observed at scales ranging from gang violence (Baysan et al. 2014) to the risk of civil conflicts throughout the entire global tropics (Hsiang et al. 2011).

Also in the modern period, studies find that low or declining rainfall increases the risk of communal conflict, such as Hindu-Muslim riots in India (Bohlken & Sergenti 2010, Sarsons 2011) or land invasions in Brazil (Hidalgo et al. 2010), as well as organized political conflict (Miguel et al. 2004, Levy et al. 2005, Cervellati et al. 2011, Hsiang et al. 2011, Fjelde & von Uexkull 2012, Hendrix & Salehyan 2012, O'Loughlin et al. 2012, Harari & La Ferrara 2013, Couttenier & Soubeyran 2014, Fetzer 2014), leadership changes (Burke 2012, Dell et al. 2012), and coups (Kim 2015). In some settings in which a large range of rainfall levels is observed, it is also found that exceptionally high levels of rainfall increase rates of communal conflict relative to average rainfall conditions (Hidalgo et al. 2010, Hendrix & Salehyan 2012, Ralston 2013), suggesting a global response function that is probably U-shaped. Again, these effects are observable primarily in low- and middle-income settings in which the economic impacts of climatic variation are likely to be substantial owing to their importance to local agricultural production. **Table 1** contains a detailed listing of these studies.

2.4. Comparing and Combining Results from Modern Samples

Broadly speaking, the econometric literature surveyed above suggests that different classes of conflict, in different contexts and at different scales of analysis, share the general feature that their likelihood of occurring is influenced by climatic events. This qualitative statement, although conceptually intriguing, is alone unsatisfying because there remain many differences among studies, making it difficult to determine what, if anything, can be said about a more general quantitative relationship between climate and conflict. Furthermore, there is substantially more uncertainty in the results of some studies owing to the nature of their data, making it imperative that point estimates from individual studies are not overinterpreted and that statistical uncertainty is accounted for when assessing the extent of agreement between studies (Hsiang et al. 2013c, Hsiang & Meng 2014). Finally, the strength of statistical conclusions that we can draw from the literature as a whole may be substantially stronger than the strength of conclusions from individual studies because the larger body of literature draws on far more data than do individual studies—so long as there is a reasonable way to combine these varied sources of data. For these three reasons, we standardize effect sizes (and in some cases statistical models) as described below so that results from different studies may be compared to one another more systematically. We then implement a meta-analysis to synthesize existing results into more general findings. Importantly, the meta-analytic technique we employ does not assume that all effect sizes are identical, as they certainly are not, but instead tries to identify whether there is a common component in the results while simultaneously characterizing the degree of heterogeneity in treatment effects across studies.

2.4.1. Standardizing and comparing effect sizes.

To compare quantitative results across studies of modern data, we computed standardized effect sizes for those studies for which it was possible to do so. Standardization is essential for an apples-to-apples comparison of results across studies for two reasons.

First, most studies report changes in the probability of conflict, but different types of conflict (within both classes of conflict, interpersonal and intergroup) exhibit different baseline frequencies. For example, a 0.1 change in probability for a conflict that occurs with a baseline probability of 0.3 has a very different interpretation (a 33% increase in risk) than the same change in probability for a type of conflict that occurs with a baseline probability of 0.01 (a 1,000% increase in risk). Hsiang & Meng (2014) point out that in prior research on Africa, comparing probability changes for civil conflict incidence and civil war outbreak is an apples-to-oranges comparison because they have baseline probabilities of 0.25 and 0.012, respectively. To adjust for baseline probabilities, we convert marginal changes in probabilities to marginal changes in relative risk, normalizing probability by the average risk of conflict in the observed sample.

Second, most studies report changes in climate variables in physical units, such as degrees of temperature or millimeters of rainfall, but different locations around the world exhibit different within-location baseline variances in these measures, which is further exacerbated by differences in the areal extent that is averaged over to compute exposure levels. For example, a 1°C temperature change is a relatively small change for average weekly temperature in a US county; however, it is an enormous change for annual average temperature in an African country. To adjust for these large differences in baseline climate variance, we convert all physical measures of climate into standardized measures based on the within-location standard deviation in climate.

Thus, for a coefficient reporting the marginal change in probability of conflict caused by a one physical unit change in climate, we convert to standardized units:

$$\beta_{\text{standardized}} = \beta_{\text{reported}} \cdot \frac{\sigma(\text{climate})}{\text{avg}(\Pr(\text{conflict}))}, \tag{6}$$

which is the change in the relative risk of conflict for each standard deviation change in climate variables and where σ(climate) is the within-location standard deviation in the climate variable. We focus on computing this effect for studies using modern data (1950–present).

We plot these standardized β's for each author's preferred model and variables in **Figures 2** and **3**, displaying interpersonal and intergroup conflict, respectively. The utility in distinguishing between these two classes of conflict is immediately apparent, as the overall magnitude and precision of estimates for these two classes appear quite different. The reported effect of climate on interpersonal conflict (**Figure 2**) has generally smaller effects, with point estimates in the single digits, and most effects are precisely estimated and statistically significant individually. The reported effect of climate on intergroup conflict (**Figure 3**) tends to be larger, with standardized

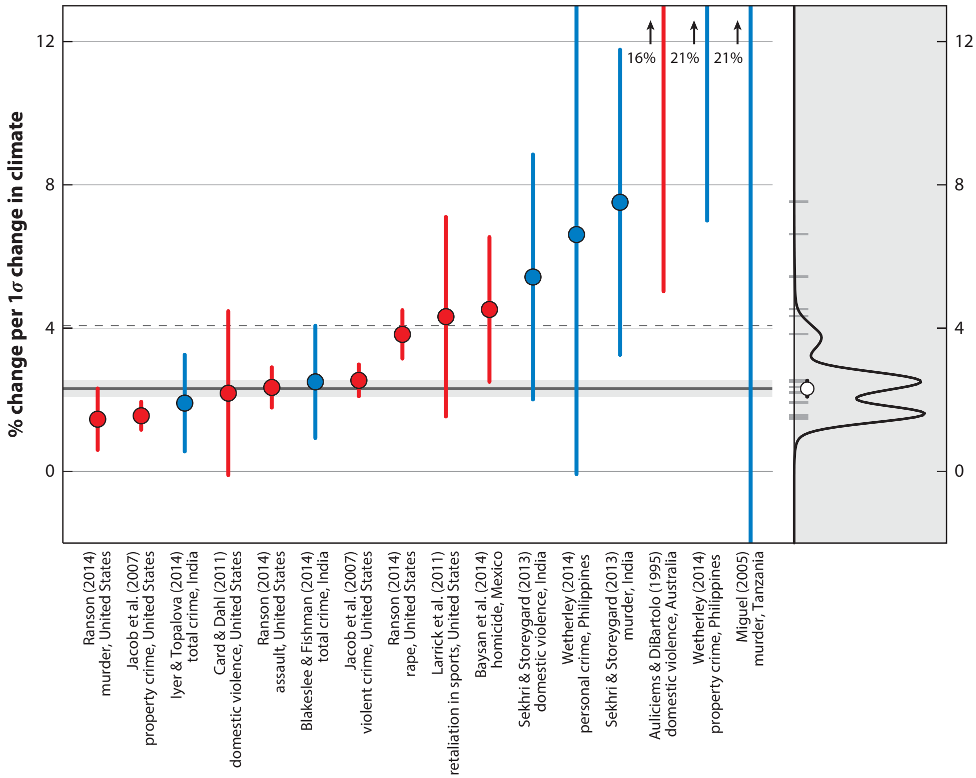

Figure 2

Estimates for the effect of climatic events on the risk of interpersonal violence using authors' preferred specifications. Each marker represents the estimated effect of a 1σ increase in a climate variable, expressed as a percentage change in the conflict variable relative to its mean, and whiskers represent the 95% confidence interval. Colors indicate temperature (*red*) or rainfall loss (*blue*). The dashed line is the median estimate, and the dark gray line is the precision-weighted mean with its 95% confidence interval shown in gray. The panel on the right shows the precision-weighted mean effect (*circle*) and the distribution of study results (*gray ticks*); probability distributions are the posterior for the expected distribution of an additional study (*solid black line*).

values spanning 0–40% per σ, and with each individual estimate exhibiting larger confidence intervals. The greater precision of interpersonal estimates in part results from the very large number of interpersonal conflicts observed in data (i.e., there are many more murders than civil wars), and the large and high-resolution data sets (in both spatial and temporal dimensions) used to study these effects, such as the FBI Uniform Crime Reports (Jacob et al. 2007, Ranson 2014). Overall, we observe visually that once effect sizes are standardized and conflicts are separated by class, there appears to be considerable consistency across studies on the reported relative influence of climate on conflict.

One concern with these results, raised by Hsiang et al. (2013a), is that there may be some selectivity by authors in which results they present and how their results are framed in their analyses. For example, not all studies examine the effects of both temperature and rainfall (or they use some transformation of these variables to describe drought or water availability), and not all studies account for the potential effect of climate variables in prior periods. These modeling decisions are sometimes based on data availability and in many cases represented best practices at the time the study was conducted. However, there is growing awareness that climate variables such as temperature and rainfall may be highly correlated with one another contemporaneously and autocorrelated over time (Auffhammer et al. 2013), indicating that estimates constructed without multiple climate variables and lags may be biased owing to omitted variables. Furthermore, some authors may focus (consciously or not) on the effect of climate variables that are most statistically significant, so summarizing authors' preferred results may introduce some selection bias into parameter estimates.

To address this issue, we compute standardized estimates for the cumulative (contemporaneous plus lagged) temperature and rainfall effects for all studies for which it was possible to do so and report both the temperature and rainfall coefficients instead of focusing only on the authors' preferred choice. This approach substantially reduces the number of studies we can include in the analysis—we do not have access to the data needed to compute cumulative effects and their standard errors for many studies shown in **Figures 2** and **3**—but it also substantially reduces the degrees of freedom authors have over which particular results are displayed.

Results from this more refined analysis are shown in **Supplemental Figures 1** and **2** (follow the Supplemental Material link from the Annual Reviews home page at **http://www.annualreviews.org**). In the case of interpersonal conflict (**Supplemental Figure 1**), results suggest that the effects of cumulative temperature and rainfall are highly consistent, with temperature having effects clustered tightly around the 2% per σ median estimate and rainfall having close to a zero effect. Results for intergroup conflict (**Supplemental Figure 2**) become similarly clarified with this refinement, with temperature having a strong cumulative effect (median = 18% per σ) and slightly smaller estimates appearing for rainfall (median = 11% per σ), although as before these estimates are less precise than those for interpersonal conflict.

Supplemental Material

2.4.2. Identifying mean effects and heterogeneity through meta-analysis. Ideally we would like to systematically characterize the extent to which there is a link between climate and conflict that is general across societies. However, in light of the discussion above, it is clear that different types of conflict in distinct contexts respond to climate events heterogeneously, even within a class of conflict. Thus, any attempt at summarizing results from the literature must take into account these differences, even if the goal is to identify common features of the response. To achieve this, we adopt the method proposed by Gelman et al. (2004) and employ a hierarchical Bayesian approach to meta-analysis of this literature. This approach recognizes that the true effects underlying different parameter estimates may indeed be distinct from one another and is designed to characterize the extent of this cross-study heterogeneity as well as any common component across studies. The magnitude and precision of this common component represent the generalizable conclusions we might draw from this growing literature.

In each study j, we compute the standardized effect $\hat{\beta}_j$ and standard error for that estimate $\hat{\sigma}_j$, using the regression models described above. Following Hsiang et al. (2013a), we assume that there is some common component to these results μ but that there might also be heterogeneity in how different populations are affected by the climate. That is, we allow for the fact that the cross-study differences we observe might not be driven only by sampling variability (i.e., that even if each study were executed with infinite data, we would not expect them to obtain the exact same parameter estimate). This situation is well suited for a random effects framework in which we assume that the true β_j that generates the data in each study can be thought of as being drawn from a distribution of all possible samples and studies where

$$\beta_j \sim N\left(\mu, \tau^2\right) \tag{7}$$

and where μ and τ are unobserved hyperparameters that determine the central tendency and dispersion of findings in the literature. μ is the generalizable component describing the mean response to changes in climate. τ describes the extent of heterogeneity across contexts in these responses. If τ is much larger than μ, then differences between results are much larger than their commonalities. We point out here that if μ is large, this may be of theoretical or policy interest regardless of whether τ is large, and if τ is large, it suggests there are substantial differences between studies that may be worth modeling explicitly. For instance, rainfall may have distinct effects on conflict across settings with differential economic dependence on agriculture. Our objective is to estimate both μ and τ. Specifically, we estimate these values separately for the effects of both current and lagged temperature and rainfall, separately for both interpersonal and intergroup conflict.

Under a uniform prior, the conditional posterior for μ is

$$\mu|\tau, y \sim N\left(\hat{\mu}, V_\mu\right), \tag{8}$$

where

$$\hat{\mu} = \frac{\sum_j \frac{1}{\hat{\sigma}_j^2 + \tau^2} \hat{\beta}_j}{\sum_j \frac{1}{\hat{\sigma}_j^2 + \tau^2}}, \quad V_\mu^{-1} = \sum_j \frac{1}{\hat{\sigma}_j^2 + \tau^2}, \tag{9}$$

where τ is computed to have larger values if the between-study differences in $\hat{\beta}_j$ are large relative to the within-study standard error estimates $\hat{\sigma}_j$ (the standard errors normally reported for individual regression results). When differences in estimates across studies are large relative to uncertainty in parameter estimates within studies, then studies are treated nearly uniformly when estimating an average effect. Meanwhile, if τ is close to zero, then it is more likely that the true β_j are drawn from a narrow distribution, and sampling variability drives most of the variation in the estimates $\hat{\beta}_j$. In this case, the weight on each study approaches its precision $1/\hat{\sigma}_j^2$, and estimates with large uncertainty are downweighted. In the limit that $\tau \to 0$, this estimate approaches the optimal composite parameter estimate for the case in which each study examines different subsamples of the same population, which is only exactly true when $\tau = 0$.

Intuitively, if estimated treatment effects in all studies are near one another and have relatively wide and overlapping confidence intervals, then most variation likely results from sampling variation, and the value of τ (the degree of treatment effect heterogeneity across studies) is likely to

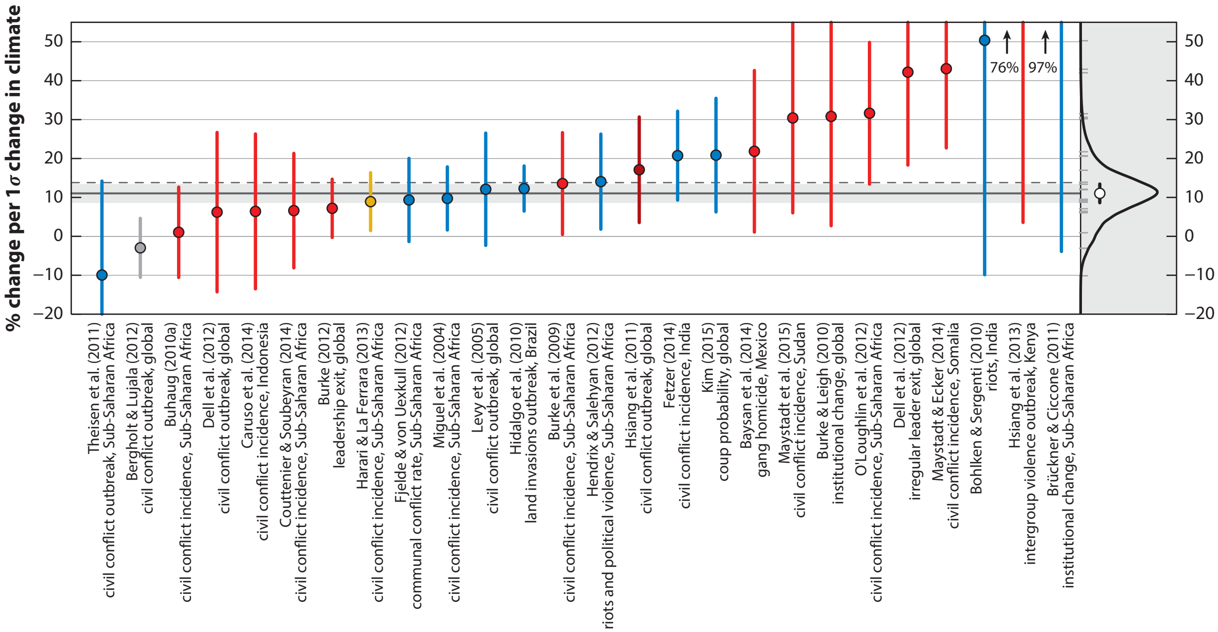

Figure 3

Estimates for the effect of climatic events on the risk of intergroup conflict using authors' preferred specifications. Each marker represents the estimated effect of a 1σ increase in a climate variable, expressed as a percentage change in the conflict variable relative to its mean, and whiskers represent the 95% confidence interval. Colors indicate temperature (*red*), rainfall loss (*blue*), drought (*yellow*), the El Niño Southern Oscillation (*dark red*), and storms (*gray*). The dashed line is the median estimate, and the solid dark gray line is the precision-weighted mean with its 95% confidence interval shown in gray. The panel on the right shows the precision-weighted mean effect (*circle*) and the distribution of study results (*gray ticks*); probability distributions are the posterior for the expected distribution of an additional study (*solid black line*).

be close to zero. Alternatively, if there is extensive variation in the estimated average treatment effects, but each effect is estimated quite precisely, then τ will be relatively large, and there is likely to be considerable heterogeneity in treatment effects across studies, as sampling variation alone is unlikely to be able to explain this pattern of results. Casual observation of **Figure 2** and **Supplemental Figure 2** suggests a climate-conflict literature somewhere in between: There is substantial overlap in confidence intervals, but also substantial variation in the estimated effects with some confidence intervals that do not overlap.

Supplemental Material

Using a uniform prior, we apply Bayes' rule to update our estimates of μ, τ, and the β_i's for estimates of interpersonal violence and intergroup conflict separately. We then use 10,000 simulations to characterize the posterior distributions of each of these variables.[2]

Meta-analysis of authors' preferred results. We first present the posterior mean and confidence interval for the common component μ using each author's preferred climate measure and model. These values are shown for interpersonal conflict and intergroup conflict in the far-right panels of **Figures 2** and **3**, respectively. The probability distribution in these panels displays the posterior distribution for the β_i's (i.e., the distribution of effects we would expect to obtain if we were to implement a new

[2]Details of our approach to hierarchical Bayesian meta-analysis closely follow Gelman et al. (2004), to which we refer readers for further technical details.

study). Pooling authors' preferred results, we estimate that for each 1σ change in climate toward hotter, drier, or more extreme rainfall conditions, interpersonal conflict rises 2% and intergroup conflict rises 11%. Both these estimates are highly statistically significant (P value < 0.01) (see **Table 2**).

Cumulative effects for all climate variables. We then consider our reanalyzed results that account for both potential lagged effects of climate on conflicts, including possible forward displacement in time. The far-right panels in **Supplemental Figures 1** and **2** display these results for interpersonal and intergroup conflict, respectively, pooling all climate variables regardless of whether an author emphasized a particular climate variable in his or her analysis. Computing cumulative effects, we find that on average, interpersonal conflict rises 1.2% and intergroup conflict increases 4.5% for each 1σ change in climate toward more adverse conditions (see the panel showing all estimates in **Supplemental Figures 1** and **2**), with both effects highly significant (P value < 0.01). This suggests that on average, climatic shifts have a net effect on conflict rates and do not only displace conflicts forward or backward in time.

Supplemental Material

Results broken down by climate variable. Because our reanalysis treats climate variables identically, we look separately at the cumulative effects of temperature and precipitation. [For this exercise, we classify variables that are not simply direct temperature or rainfall exposure as whichever atmospheric variable is actually used to define exposure—i.e., exposure to drought in Brückner & Ciccone (2011) is coded as rainfall, and exposure to the El Niño Southern Oscillation in Hsiang et al. (2011) is coded as temperature.] The rightmost panels in **Supplemental Figures 1** and **2** display results for temperature and rainfall separately for both classes of conflict, with results also presented in **Table 2**. For interpersonal conflict, temperature and rainfall have clearly distinguishable cumulative effects, with the effect of temperature (2.1% per σ) roughly seven times larger than the effect of rainfall (0.3% per σ), although this small effect of rainfall is still statistically different from zero (P value < 0.05).

For intergroup conflict, higher temperature has a much larger average effect and also greater dispersion across study estimates, with each 1σ increase in temperature increasing intergroup conflict 11.3% on average (P value < 0.01). Notably, the posterior distribution of β_i is negative for one-tenth of values, suggesting that cross-study heterogeneity is large enough to generate negative estimates in individual studies even when the common component μ is positive and well separated from zero. The common components of rainfall effects on intergroup conflict are also positive but smaller, at 3.5% per σ, and are also statistically significant (P value < 0.05). The greater cross-study dispersion in temperature effects relative to rainfall effects is described by the hyperparameter τ, which is 14.4% per σ for the cumulative temperature effect and 3.1% per σ for rainfall (**Table 2**). Uncertainty over τ is also larger for temperature, suggesting that the extent of cross-study differences is perhaps more difficult to characterize. This appears to largely result from the combination of dispersed point estimates, each of which has relatively large uncertainty (**Supplemental Figure 2**), making it challenging to determine if cross-study differences represent meaningful differences in context or if they mostly result from sampling variability.

Thus, although there is strong evidence of important differences between studies, there is simultaneously strong evidence of some underlying commonality between studies, namely a link between more extreme climate and conflict. There remains considerable heterogeneity in the response to climate across studies that should be recognized, and understanding the sources of this variation is an important area for future inquiry.

Results broken down by timing of effects. The above results suggest that temperature and rainfall have a discernible cumulative effect on conflict. However, it is also useful to decompose the

Table 2 Meta-analysis results

Effect type	Median	Mean (μ)	Standard error [SE(μ)]	Cross-study standard deviation (τ)	N
Intergroup conflict					
Authors' preferred	13.81	11.04	1.22	8.46	26
Temperature, contemporaneous	14.75	11.33	1.90	6.29	14
Temperature, lagged	7.85	2.84	2.34	10.64	14
Temperature, combined	17.74	11.33	2.96	14.44	11
Precipitation, contemporaneous	1.90	1.90	1.08	3.84	19
Precipitation, lagged	4.24	2.28	1.00	2.09	19
Precipitation, combined	10.95	3.54	1.20	3.05	17
Interpersonal conflict					
Authors' preferred	4.07	2.31	0.11	1.35	16
Temperature, contemporaneous	3.37	2.38	0.12	1.83	8
Temperature, lagged	−0.20	−0.23	0.07	0.52	8
Temperature, combined	2.14	2.08	0.14	1.26	6
Precipitation, contemporaneous	1.16	0.59	0.08	0.78	9
Precipitation, lagged	0.01	−0.21	0.09	0.31	9
Precipitation, combined	0.04	0.28	0.13	0.91	7

Authors' preferred estimates correspond to those shown in **Figures 2** and **3**. All other estimates correspond to estimates from a model with both contemporaneous and lagged effects, with the combined estimates equaling the sum of the two effects and corresponding to the effects displayed in **Supplemental Figures 1** and **2**; we include all studies for which it was possible to compute these estimates. The first two columns give the precision-weighted mean and its standard error for the sample of studies. The third column gives the estimated between-study standard deviation, as derived from a Bayesian hierarchical model. The last column gives the number of studies used in each estimate.

common component of cumulative effects into a common component for each time period (current and lagged) because understanding the timing of these average effects provides insight into mechanisms that might be driving the cumulative result. Thus, we compute a separate value for μ for both classes of conflict, for both climate variables, and for both lags—effectively characterizing multidimensional impulse-response functions that are the generalizable component for the effect of climate on conflict.[3] These results are shown in **Figure 4** and presented in **Table 2**.

We find substantial and statistically significant effects of contemporaneous temperature on both interpersonal and intergroup conflict, with lags that are smaller and not significantly different than zero. The effect of contemporaneous temperature on intergroup conflict (11.3% per σ) is

[3]Here we can add back in a few studies that were omitted in the cumulative effects calculations—studies that report both contemporaneous and lagged effects but for which we do not have access to the data to calculate the cumulative effect and its standard error. The number of studies contributing to each estimate is shown in the last column in **Table 2**.

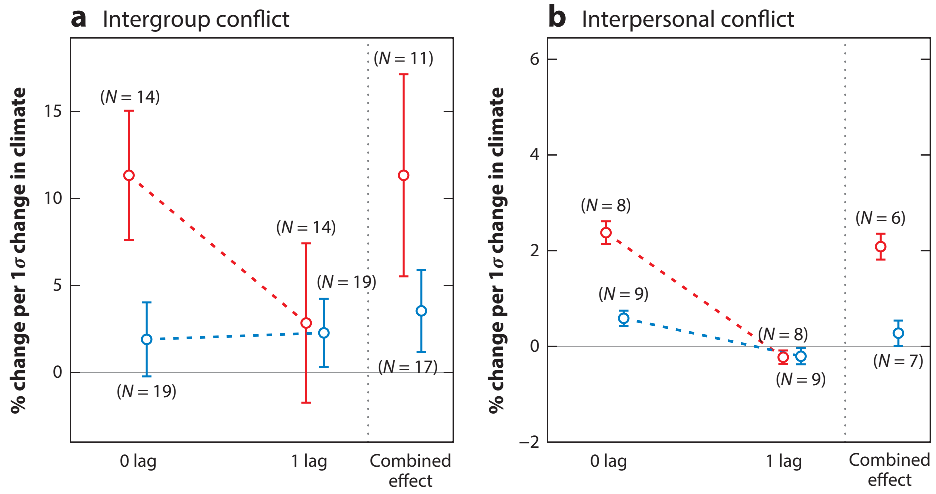

Figure 4

Summary of meta-analysis for studies reanalyzed with distributed-lag structure, showing estimated precision-weighted mean effects and 95% confidence intervals for (*a*) intergroup and (*b*) interpersonal conflict, for both contemporaneous (zero lag) and one-period lagged temperature (*red*, left offset) and precipitation (*blue*, right offset). Combined effects equal the sum of the contemporaneous and one-period lagged effects for studies for which the calculation was possible. The number of studies contributing to each estimate is given in parentheses.

roughly four times larger than the lagged temperature effect (2.8% per σ); however, both are positive, suggesting that on average, the temperature effect of climate on conflict does not result from displacement alone. Similarly, the effect of contemporaneous temperature on interpersonal conflict (2.4% per σ) is much larger than the lagged effect (-0.23% per σ), which suggests that roughly one-tenth of the observed effect is attributable to temporal displacement.

The effect of rainfall on intergroup conflict is smaller than the effect of temperature, but it remains statistically significant, although interestingly there is roughly the same size effect for both contemporaneous and lagged rainfall, which may not be surprising in agrarian settings in which lagged rainfall is a key input into current harvests and thus local economic conditions. The effects of both current and lagged rainfall on interpersonal conflict are small, although the contemporaneous effect of rainfall (0.6% per σ) is highly statistically significant, and the lagged effect (-0.2% per σ) suggests that roughly one-third of this effect is attributable to temporal displacement.

2.4.3. Publication bias. Evidence increasingly suggests that many empirical social science literatures exhibit some form of publication bias (Gerber & Malhotra 2008a,b; Brodeur et al. 2013), with a common form of bias arising from the research community's reluctance to investigate or publish null results. This is generally thought to manifest in two ways in a literature. First, authors may never release their data or findings if they obtain a null result early on in their investigation. This is known as the file-drawer problem because these researchers return their findings to a proverbial file drawer in which no other member of the research community observes them (Rosenthal 1979). This source of publication bias is difficult to combat because it is generally difficult or impossible to observe what researchers investigate but do not report (see Franco et al. 2014 for a discussion of how this is sometimes possible in practice). However, we think this issue is generally less likely to be problematic in the climate-conflict literature because many papers have been published reporting associations that are not statistically significant (Buhaug 2010a, Theisen et al. 2011, Couttenier & Soubeyran 2014). In particular, these analyses are often framed as interesting precisely because they present an alternative finding on a controversial topic and can accrue substantial scholarly and media attention as a result. Thus, we believe that the high profile and large number of previous positive results in this literature create strong incentives to not withhold null or negative results.

A second potential source of publication bias is that authors may conduct multiple statistical tests in their analysis, for example, varying climate variables, conflict variables, or subsamples, and report only their strongest findings. These findings may not be representative of the data; furthermore, they may exhibit spurious correlations because statistical uncertainty is generally not corrected for the multiple testing. Implicit norms in the research community sometimes can address this issue, in the form of journal referees often requiring that authors demonstrate a single result multiple ways, for example, by using different sources of variation, placebo tests, and cross-partial effects (e.g., Burke et al. 2009, Hsiang et al. 2011, Dell et al. 2012). The logic is that a single test may prove spuriously significant if repeated enough times on different variables, but it is dramatically less likely for multiple, orthogonal tests of a single result to be spuriously significant. Nonetheless, many findings are published with a single hypothesis test, exposing this literature to this second potential source of bias. To the extent that this form of bias is driven by authors selecting a preferred specification, temporal lag, or climate variable, our reanalysis of results should in principle correct for this bias—implying that the results shown **Supplemental Figures 1** and **2** are likely to be less biased than the authors' preferred specifications.

Supplemental Material

Nevertheless, we look for evidence of publication bias across our full sample of studies (i.e., whether or not we have their replication data) by examining whether the statistical strength of individual studies reflects their sample size. Following Card & Krueger (1995) and Disdier & Head (2008), standard sampling theory suggests that the t-statistic on a coefficient estimate should be proportional to the degrees of freedom in the study. In particular, with the null hypothesis $H_0 = 0$, vector of independent variable observations $\mathbf{X}$, vector of residuals $\boldsymbol{\varepsilon}$, degrees of freedom $n - k$, and again indexing studies by j, we obtain

$$t\left(\hat{\boldsymbol{\beta}}_j\right) = \frac{\hat{\boldsymbol{\beta}}_j}{\hat{\sigma}_j} = \frac{\hat{\boldsymbol{\beta}}_j}{\left(\frac{\sqrt{\boldsymbol{\varepsilon}_j'\boldsymbol{\varepsilon}_j}}{\sqrt{n_j - k_j}\sqrt{\mathbf{X}_j'\mathbf{X}_j}}\right)} = \sqrt{n_j - k_j} \times \left(\frac{\hat{\boldsymbol{\beta}}_j\sqrt{\mathbf{X}_j'\mathbf{X}_j}}{\sqrt{\boldsymbol{\varepsilon}_j'\boldsymbol{\varepsilon}_j}}\right). \tag{10}$$

Taking logs, we see that there should be unit elasticity between the log of the t-statistic and the log of the square root of the degrees of freedom. We use this insight to look for evidence of publication bias in the literature we analyze. If there is a true relationship between climate and human conflict, then we expect the statistical power of studies to increase with their sample size (and thus with their degrees of freedom). However, if there is no true relationship, and instead authors are just searching through data until they find data that allow them to reject a null hypothesis at the 95% confidence interval, then large sample sizes should provide no benefit in terms of statistical power. Thus, if publication bias is a major problem in this literature, we predict that $\log\left(t\left(\hat{\beta}_j\right)\right)$ should not increase with $\log\left(\sqrt{n-k}\right)$. For example, Card & Krueger (1995) find a negative relationship between the t-statistics and degrees of freedom, which they interpret as strong evidence of publication bias.

Figure 5 shows the plotted relationship between the log of the t-statistic and the log of the square root of the degrees of freedom, for the 40 estimates for which we are able to calculate standardized effects (we use author-reported statistics here because those are the values that authors, editors,

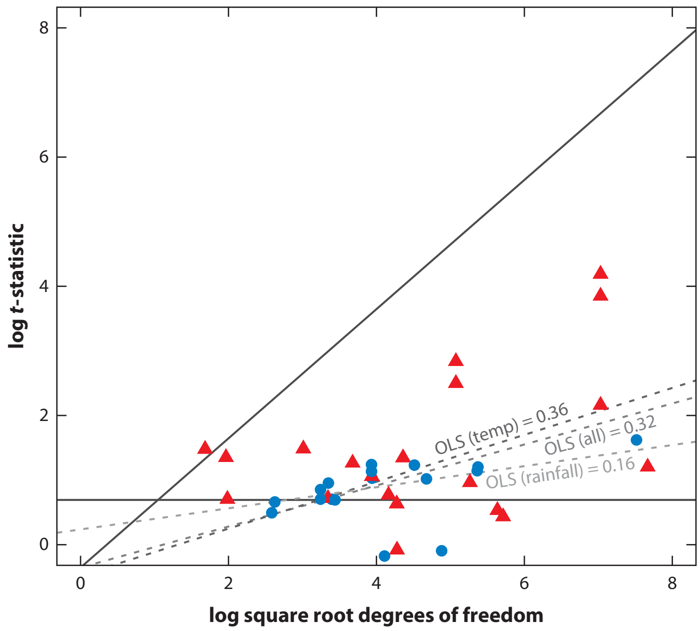

Figure 5

Relationship between the log of the *t*-statistic and the log of the square root of the degrees of freedom, using author-reported *t*-statistics. Circles represent studies focusing on rainfall, and triangles represent studies focusing on temperature. Ordinary least squares (OLS) estimates of the slope of the relationships for all estimates, temperature-only estimates, and rainfall-only estimates are given by the dashed lines. The solid 45° line (the unit elasticity) indicates the slope of 1 that theory predicts would occur in the absence of any publication bias.

and reviewers would consider at the time of release/publication). We strongly reject a slope of zero for both the full sample (P value < 0.01) and the temperature subsample (P value < 0.05) and marginally reject a zero slope for the precipitation estimates (P value < 0.10). And although we can also reject a one-to-one relationship for each sample, studies with larger sample sizes on average do have larger *t*-statistics in the climate and conflict literature we survey, suggesting that authors with large samples are not simply searching through specifications or data mining to find significant effects at exactly the 95% confidence interval. We note that for both samples, the upward relationship stands in sharp contrast to the results of Card & Krueger (1995), with the negative slope they estimate. Our estimates are more similar to that of Disdier & Head (2008), who interpret their results as ruling out any large role for publication bias in the trade literature they survey.

3. UNDERSTANDING THE MECHANISM

Reduced-form evidence in the literature indicates a causal effect of climatic events on multiple forms of conflict (**Figure 4**). As described by Equation 5, this reduced-form effect is the sum effect of all potential pathways. Thus, taken alone, these reduced-form effects provide little information about what mechanisms play a role in generating this response. Understanding what mechanisms drive observed patterns is not essential for all applications of these results. For example, under some assumptions, the social impact of climate change can be estimated using just these reduced-form results (as discussed below), and law enforcement or humanitarian organizations can use climate forecasts for planning purposes without understanding all the underlying mechanisms.

However, many scientific questions and policy interventions do require an understanding of the mechanisms linking conflict to climate. For instance, a more detailed understanding of the processes involved is necessary for implementing policies to reduce conflict risk in hot periods. Here we present a simple framework to describe several mechanisms that researchers have highlighted, describe the evidence to date on hypothesized pathways, and conclude that studying mechanisms should be a central task for future research.

3.1. Framework

We employ the theoretical model developed by Chassang & Padro-i-Miquel (2009) to illustrate potential channels. In this model, two agents decide whether to engage in costly conflict and redistribution when bargaining fails. We opt not to present the full solution to the model here (we refer readers to the original paper for the details) but instead focus on using the framework to provide an illustration of key ideas. We follow Baysan et al. (2014) by enriching the basic model with additional mechanisms that have been proposed but were not in the original analysis.

Consider two agents who cannot commit to not attacking one another in an infinite number of periods, indexed by t. Each agent has assets with productivity θ_t that produces $\theta_t l$ output when combined with l units of labor [Chassang & Padro-i-Miquel (2009) set $l = 1$]. We enrich the model to account for a population n_t (not all of which must be laborers) that consume this output for a per capita, per period consumption of $(\theta_t l)/n_t$ under nonconflict conditions. If one of the agents attacks the other first, then he or she gains a first-strike advantage and captures all of the opponent's output and assets with probability $P_t > 0.5$. Such an attack costs both the aggressor and defender a fraction $c > 0$ of output. If both agents choose to attack simultaneously, they each win with probability 0.5. Following Baysan et al. (2014), an attacker experiences a nonrival psychological consumption value of violence γ_t; if the attacker dislikes being violent, then $\gamma_t < 0$, and if the attacker derives positive utility from violence, then $\gamma_t > 0$.

Following the original formulation, if an agent loses the conflict, then he or she is removed from the game. If there is no attack in the current period, then each agent expects a peaceful continuation value V^P, which is the discounted per capita utility of expected future consumption from the agent's initial assets and which captures expectations about future values of all parameters. Similarly, if an attacker wins, then he or she has a continuation value of victory V^V, which is the per capita expected utility from consumption of both the attacker's initial assets and the assets the attacker captures from his or her opponent. δ is the per period discount rate.

Chassang & Padro-i-Miquel (2009) show that the condition for no conflict is (modified for the new terms n_t and γ_t)

$$\underbrace{\frac{\theta_t l}{n_t} + \delta V^P}_{\text{value of peace}} > \underbrace{P_t\left(2\frac{\theta_t l}{n_t}(1-c) + \delta V^V\right) + \gamma_t}_{\text{value of attacking}}. \tag{11}$$

In other words, an agent finds it privately beneficial not to attack if the per capita value of consuming all output with initial assets plus discounted expected utility under peace δV^P exceeds the expected utility of consumption from both the agent's original assets and captured assets, less expenditures on the conflict, plus the expected continuation value $P_t \delta V^V$ and the consumption value of violence γ_t, which is experienced with certainty.

Chassang & Padro-i-Miquel (2009) apply this result only to intergroup conflicts, particularly focusing on organized political violence. But interpreted heuristically, Equation 11 can be generalized to describe both interpersonal and intergroup conflicts. In interpersonal conflicts,

$l = n_t = 1$, and V^V contains information on an agent's expectation that he or she will be apprehended and punished for the attack. In purely violent crimes that involve no intention to transfer wealth between individuals, such as some murders, assaults, and rapes, then we consider $\theta_t = 0$, and γ_t provides the primary motivation for the attacker.

Following Chassang & Padro-i-Miquel (2009), we can rearrange Equation 11 so that the condition for no conflict becomes

$$\frac{\theta_t l}{n_t}\left(1 - 2P_t(1-c)\right) - \gamma_t > \delta\left[P_t V^V - V^P\right], \tag{12}$$

where the left-hand side of the inequality is the marginal value of peace in the current period weighed against the discounted marginal expected utility from attacking on the right-hand side. For expositional purposes, we assume that initially this inequality is satisfied, and thus there is no conflict. From that baseline, we can then consider how marginal changes in parameter values driven by climate might cause this inequality to be violated, and we do this in the next subsection.

3.2. Indirect Evidence

Among economists, the majority of attention has focused on the potential for climatic conditions to adversely affect economic productivity θ_t and associated living standards. In the framework above, a temporary reduction in productivity reduces the current opportunity cost of conflict more than it alters the value of victory because the productivity loss is temporary. Miguel et al. (2004) hypothesize that adverse rainfall conditions could increase the risk of African countries because these changes temporarily lower agricultural productivity, and many studies since have argued for or alluded to a similar mechanism (Miguel 2005, Burke et al. 2009, Burke & Leigh 2010, Brückner & Ciccone 2011, Burke 2012, Dell 2012, Jia 2014, Maystadt & Ecker 2014, Kim 2015). It is now increasingly well documented that climatic events similar in structure to those that increase conflict risk (hot and dry, or very wet) also reduce productivity in agriculture (Schlenker & Roberts 2009, Hidalgo et al. 2010, Schlenker & Lobell 2010, Welch et al. 2010, Hsiang et al. 2011, Lobell et al. 2011), as well as nonagricultural incomes (Barrios et al. 2010, Hsiang 2010, Jones & Olken 2010, Dell et al. 2012, Graff Zivin & Neidell 2014), making this a plausible assertion. Furthermore, there is some evidence that positive income shocks reduce the likelihood of certain types of conflict (Berman et al. 2011b, Iyengar et al. 2011, Dube & Vargas 2013), although there are also findings that suggest a limited or opposite effect of income on conflict risk (Berman et al. 2011a, Dube & Vargas 2013, Arezki & Brueckner 2014, Crost et al. 2014). However, even if it were true that income affects conflict risk, and climate affects income, these two facts on their own are insufficient to prove that climate affects conflict mainly through income, following our discussion in Section 2.2.4 on the appropriate use of instrumental variables approaches.

An alternative approach to looking for an income channel is to demonstrate that the pattern in which climate affects income is similar to the pattern for how the climate affects conflict. For example, Hidalgo et al. (2010) match functional forms by demonstrating that the nonlinear inverted-U-shaped relationship between agricultural income and rainfall in Brazilian municipalities is almost a mirror reflection of the U-shaped relationship between land-invasion risk and rainfall in the same municipalities. In a different example, Hsiang et al. (2011) match patterns of heterogeneous responses to climate for both income and conflict, showing that both are correlated with the timing of the El Niño Southern Oscillation only in the tropics and not at higher latitudes. In other examples, studies match the timing of climatic events that affect conflict with the timing of climatic events that are thought to be economically important. For example, both Fetzer (2014) and Blakeslee & Fishman (2014) show that monsoon rainfall affects conflict in India more than nonmonsoon rainfall does, and Caruso et al. (2014) find that high minimum nighttime temperatures known to adversely affect rice yields (e.g., Welch et al. 2010) predict the risk of conflict in Indonesia. Taken together, these exercises provide suggestive evidence that an economic productivity channel is likely to play a meaningful role. However, they are not conclusive as many other economic and social factors may simultaneously be affected by climatic conditions and thus exhibit similar patterns (Dell et al. 2014), and it could still be the case that climatic conditions affected productivity because they affected conflict.

Less focus has been paid to the potential role of population density changes, although some existing evidence suggests that this pathway might play a role. In Equation 12, if climatic conditions cause the total population n_t to rise without generating a similar rise in the productive labor force l, this increases the chance that conflict will result. This could occur, for example, if climatic events induced the migration of rural populations into urban centers where labor markets are unable to fully absorb this influx, at least in the short to medium run, causing per capita income $(\theta_t l)/n_t$ to decline. At present, there is some evidence that implicates climatic events as inducing the migration of rural populations to urban centers (Barrios et al. 2006; Feng et al. 2010, 2012; Marchiori et al. 2012; Bohra-Mishra et al. 2014), although there is as yet limited work linking these patterns to the onset of conflict.

Some authors have suggested that climatic conditions could alter the risk of conflict because they alter the probability that an aggressor is successful. In the context of Equation 12, a climate-induced increase in the probability of a successful attack P_t raises the likelihood of conflict because the expected value of attacking is higher. This idea is presented in the literature in two different ways. The first interpretation is strictly logistical; that is, climatic conditions might alter the physical environment in such a way that attacking is easier or has a greater chance of success. For example, Miguel et al. (2004) discuss whether rainfall and floods adversely affect roads used to transport troops in large-scale political violence, and Meier et al. (2007) and Ralston (2013) argue that high seasonal rainfall causes greater growth of vegetation that is used as cover for cattle raids in East Africa, giving aggressors a strategic advantage. The second interpretation is that climatic conditions might compromise an opponent's strength, increasing the likelihood that an attack is successful. This argument is usually framed as a change in state capacity if an incumbent government becomes militarily weaker because it has fewer economic resources to draw on, via taxation, when climatic events cause the economy to contract. In support of this notion, several analyses find that incumbent leaders face a greater risk of being removed from power following adverse climatic events (Burke & Leigh 2010, Brückner & Ciccone 2011, Burke 2012, Dell et al. 2012, Chaney 2013, Kim 2015), and Shapiro et al. (2014) demonstrate that climatic events may increase voter turnout and incite aggressive demands of government services. These findings are interpreted by some authors as evidence that climatic changes weakened these leaders' power, causing P_t (for challengers) and the risk of conflict to rise. We interpret these results as consistent with this hypothesis, although these empirical observations are not generally in disagreement with other explanations, and there is no clear way to rule out alternative mechanisms in these cases; in fact, several of these authors also argue that changes in θ_t contribute to their findings.

Finally, climate might affect the risk of conflict because it alters the psychological rewards (or costs) of employing violence, described as γ_t in Equation 12. If individuals normally have a distaste for violence, perhaps because of cultural norms or innate preferences, then $\gamma_t < 0$. In contrast, if $\gamma_t > 0$, then an agent derives some positive utility from the act of violence itself. If climatic conditions influence γ_t by increasing the utility (or decreasing the psychological cost) of acting violently, then these changes may increase the likelihood that Equation 12 is violated and conflict occurs. Studies of interpersonal violence provide the strongest suggestive evidence that a psychological mechanism is playing a role, especially in high-frequency settings in which there is no plausible economic or state capacity explanation linking climate to violence over very short time periods. For example, Auliciems & DiBartolo (1995) and Card & Dahl (2011) find that reports of domestic violence increase on hot days in Australia and the United States, respectively; Jacob et al. (2007) and Ranson (2014) find that assaults, rapes, and murders increase during hot weeks and months (respectively) in the United States; and Kenrick & MacFarlane (1986) find that individuals exhibit road rage with greater likelihood on hot days in a field experiment. Although detailed statistics were not reported, Rohles (1967) notes that when laboratory subjects were placed in a hot room, they exhibited higher rates of arguing and fighting (and even an attempted knifing in one instance) relative to a cooler treatment condition.

Baysan et al. (2014) make the case that changes in γ_t may contribute to multiple classes of conflict in the same setting by demonstrating that elevated rates of drug cartel–related killing (intergroup violence), regular homicide (interpersonal violence), and suicide (intrapersonal violence) in Mexico all respond to higher temperatures with similar patterns that appear unrelated to economic conditions. In support of this notion, some results suggest mechanisms through which changes in personal tastes for violence might escalate interpersonal conflicts into intergroup conflicts. For example, Larrick et al. (2011) demonstrate that during sporting events, individuals' willingness to retaliate violently against earlier violent acts committed on the field increases on hot days, and Vrij et al. (1994) find that police are more willing to use threatening behavior and violence for self-defense during training exercises in a laboratory when they are subject to high temperatures. If high temperatures reduce individuals' normal inhibitions against violence in response to a threat (or in retaliation to violence), then relatively small disputes between individuals might more readily escalate into costly confrontations, for instance, in the case of the Mexican drug cartel violence studied by Baysan et al. (2014).

The above examples identify the effects of relatively short-lived changes in climatic conditions on conflict. However, findings by Prediger et al. (2014) suggest that sustained exposure to adverse climates might also affect the likelihood of conflict through a psychological mechanism. They find that pastoralists in Namibia subject to multiyear real-world environmental scarcity are more willing to inflict harm on others in a joy-of-destruction laboratory game relative to comparable neighboring pastoralists who had not been subject to similar sustained scarcity.

Although these patterns are strongly suggestive of the importance of psychological mechanisms, to our knowledge there is still no consensus within the psychology literature on which fundamental neurophysiological channels underpin the purported link between extreme temperature and violence. Neural structures react to ambient temperature changes in order to regulate internal body temperature (Benzinger 1970), but precisely which neural structures are involved in this process is poorly understood (Morrison et al. 2008, Ray et al. 2011). The most compelling evidence lies in the role of serotonin, a neurotransmitter that facilitates body temperature regulation and is associated with aggressive behavior (Pietrini et al. 2000, Moore et al. 2002). Specifically, evidence suggests that as ambient temperature rises, serotonin levels are depressed in order to regulate body temperature, and this decreased neurotransmission then contributes to aggressive behavior. However, similar neurotransmitters, neuromodulators, and hormones, such as testosterone, norepinephrine, corticotropin releasing hormone and cholesterol, may also link temperature to violent behavior, but these pathways are understudied (Davidson et al. 2000, Seo et al. 2008).

3.3. Path Forward: Testing by Eliminating Pathways

The results above provide varying degrees of support for different pathways that may play a role in linking climatic events to conflicts. However, any single result alone is generally unable to definitively rule out alternative hypotheses. The central issue is that climatic conditions often affect so many aspects of an economy and society; thus, simply matching patterns in the climate response of both conflict and intermediary variables cannot logically reject all possible alternative explanations. In our view, some of the studies above that argue that psychological pathways (via γ_t) play some role in contributing to interpersonal conflict are among the most convincing because economic conditions and most other plausible alternative mechanisms usually do not respond to the climatic conditions on the timescale analyzed. For instance, abnormal violence committed by wealthy athletes during a professional sporting event on an exceptionally hot day (Larrick et al. 2011) is difficult to attribute to the economic or other impacts of that hot day. Similarly, the effect of climate on weekly crime rates (e.g., Jacob et al. 2007) seems unlikely to be explained by economic factors, although there is some new evidence that the economic impacts of hot days may sometimes manifest quickly (e.g., Deryugina & Hsiang 2014).

We believe a fruitful path forward is to conduct direct tests of individual hypotheses (e.g., income effects) by eliminating other potential pathways and observing whether the linkage between climate and conflict persists. For example, to test the role of income effects in driving conflict, it would be useful to exploit an exogenous source of variation that decoupled income from climate and assess whether climatic events continue to contribute to conflict risk. It is important that the potential pathway being tested is eliminated by exogenous events that are not affected by the climate nor correlated with unobserved heterogeneity between populations.

A small number of studies have begun to employ this strategy. Sarsons (2011) engages this conceptual approach, demonstrating that wage rates in Indian localities with dams are less correlated with rainfall than in districts without dams, but localities with dams continue to exhibit correlations between rainfall and Hindu-Muslim riots at similar rates, suggesting that income alone is unlikely to be the key pathway linking climate to conflict in this context. We think this study is an early step in the right direction, but it is ultimately inconclusive because districts with dams may not be comparable to districts without dams in important dimensions, and because real local income and living standards may also be affected by local prices, hours worked, and the return to capital investments, all of which might be affected by rainfall and are not captured in wage rates.

Adopting a related approach, also in India, both Fetzer (2014) and Iyer & Topalova (2014) examine whether the rollout of the large-scale Indian National Rural Employment Guarantee (NREGA) is associated with a change in the link between rainfall and conflict. The intuition is that NREGA creates a floor under household income for the rural poor, so the rollout of NREGA should in principle weaken the link between climate and conflict by dampening income changes. Iyer & Topalova (2014) estimate that NREGA had little effect on rainfall's impact on interpersonal conflict, whereas Fetzer (2014) estimates that there is a substantial pacifying effect on rainfall-induced insurgencies. The results of these two studies contrast perhaps because they focus on different outcome variables and measure climate variables differently. There also remain some unresolved issues regarding the nonrandom placement of NREGA, especially the possibility that its rollout correlates with other time-varying local economic, political, or social trends. Nonetheless, we believe these two studies represent important examples of the type of empirical approach that can more conclusively identify—or rule out—the underlying mechanisms linking climate to conflict.

4. DISCUSSION

In this section, we briefly discuss three further questions that could usefully motivate future research. First, what econometric and data challenges are potentially biasing current empirical estimates of the link between climate and conflict? Second, how relevant are existing results for the long run, when societies have additional time to adapt to a changing climate? Third, and closely related, how do these results inform estimates of the future economic and social costs of climate change?

4.1. Some Remaining Empirical Challenges

Various technical, economic, and data issues remain debated in the literature. One issue is that there may be serial correlation in both climate variables and conflict variables that is not fully accounted for by detrending, and which has the potential to lead to incorrect inference if *P* values are not appropriately adjusted (Bertrand et al. 2004). For example, Tol & Wagner (2010), Zhang et al. (2006, 2007, 2011), O'Loughlin et al. (2012), and Lee et al. (2013) apply low-pass filters to their data prior to running regressions. The objective of this filtering is to allow the authors to examine gradual, low-frequency climatic changes, but this procedure induces strong serial correlation in residuals that needs to be accounted for.

A recent development in the literature is to use increasingly high-resolution spatial data for large parts of the world, often with a Cartesian grid to define units of observation (e.g., Theisen et al. 2011, O'Loughlin et al. 2012, Harari & La Ferrara 2013, Maystadt et al. 2015). This approach is designed to allow researchers to observe spatially fine-grained structures in the response of conflict to climate. For example, Harari & La Ferrara (2013) are able to obtain estimates for the average spatiotemporal lag structure in the response across African pixels. However, an important unresolved concern with this approach in general is that the underlying climate data used by these studies may not actually be as finely resolved as the grid density might suggest, often relying on the interpolation of observations or lower-resolution weather models (see Auffhammer et al. 2013). This issue is particularly pronounced in remote regions where the presence of government data collection may be minimal, the same types of regions where conflict may be more likely to occur.

Supplemental Material

Supplemental Figure 3 illustrates this point, overlaying the spatial distribution and completeness of rainfall observations with a map of civil war onset locations studied in Theisen et al. (2011) and displaying the completeness of these records at the conflict locations of interest. We note that most conflict locations of interest have zero rainfall observations during the period of study, and no location of interest has a complete record, implying that most of the statistical analysis relies on interpolated data. Thus, we caution that research using high-resolution gridded data may overstate the quality of information actually contained in the data set. To our knowledge, the extent of this error has not been formally characterized, and its impact on regression estimates is not known. To the extent that these errors are classical in most high-resolution data—or at least mean zero in expectation—it is likely that average measurement error is smaller for more aggregated variables (e.g., average temperature in an entire country) because positive and negative errors will partially cancel each other out. Future work could usefully attempt to characterize the extent and importance of this issue.

Finally, climatic events may have different spatial extents, and it remains unknown whether spatial structure in climatic events matters. Different types of conflict have been shown to respond to climatic events with different spatial and temporal scales (Hsiang et al. 2013a) (e.g., **Figure 1**). Yet it is unknown if the spatial extent of a climatic anomaly has a causal effect on the likelihood that the event triggers conflict. In cases in which societies sometimes cope with climatic disturbances through trade or migration, the spatial scale of an event could plausibly determine whether these strategies are viable options for mitigating this risk.

4.2. Adaptation to Climate

The extent to which populations might adapt to climatic conditions, thereby dampening the effect of climate on conflict, remains widely debated. As discussed above, econometric concerns regarding unobservable heterogeneity across space motivate panel data research designs that exploit local time-series variation in climatic conditions. This approach allows insight into climate's effect on when conflict occurs in a location, but it does not allow us to infer whether climatic conditions influence where conflict will occur on average. If societies successfully and fully adapt to their average conditions in the long run, then no society would exhibit conflicts that are attributable to their average climate, but perturbations from that average may still cause conflicts. However, if societies do not adapt effectively even in the long run, the same mechanisms that generate conflicts from temporary climatic changes may also persistently alter conflict rates if a society is exposed to a permanently different climate.

The ideal thought experiment in this context is to relocate an entire population from a cold climate, such as Norway, to a hot climate, such as Nigeria, and observe whether rates of conflict adjust to be nearer to the rates originally observed in Nigeria. The frequency-identification trade-off discussed above, as well as geographical constraints, makes empirical study of this problem fundamentally difficult. Nonetheless, determining if populations can and do adapt to climate is central to assessing whether the effects identified through the panel data approach described in Section 2 above can be interpreted as having explanatory power both in the cross section and into the distant future.

Two distinct approaches have been used to understand if populations adapt to climatic events after being exposed to particular climatic conditions for substantial lengths of time, and although both are useful, neither provides entirely definitive answers. The first approach exploits high-frequency climatic events and identifies heterogeneous effects for populations in different climates. The underlying idea is that if populations have adapted to a particular climate (i.e., a hot climate), then they might become less vulnerable to short-term changes in climate (i.e., hotter than average temperatures). For example, Ranson (2014) and Rotton & Cohn (2000) test whether hot days in hotter US locations, such as Dallas, produce similar impacts on interpersonal conflict as hot days in cooler locations, such as Minneapolis, and find that impacts are in fact similar. This result cannot prove that communities do not adapt to long-term climate conditions, as the identifying variation is all short term; however, it does suggest that long-term exposure need not always lead to a more effective short-term response.

The second approach to understanding adaptation examines long-term climatic variation directly. For example, Bai & Kung (2011) and Anderson et al. (2013) look at the effect of decade-long average climate on conflict in China and Europe, respectively. Changes in decadal averages require that climatic events are sustained over time, and thus these analyses describe how societies cope with long-term shifts in climate. Data are a major challenge with this approach: Large samples with multiple decade-long observational periods are required, so the data must by necessity stretch far backward in time. For instance, Bai & Kung (2011) are able to examine decades between 220 and 1839 AD, but similar data are obviously lacking for most settings. Nevertheless, these studies generally find that sustained shifts in climate are associated with changes in various forms of conflict, indicating that populations do not fully adapt to decade-long climatic changes. Yet because these analyses stretch so far back in time, it is not clear whether the societies studied provide valid comparisons for modern societies. Hsiang & Burke (2014) point out that these historical societies might be reasonable proxies for some populations today because their average per capita income is similar to the average income in many low-income countries today, but the technological frontier and possible trading partners available to modern low-income economies are clearly very different from those faced centuries ago.

A related approach, which has not yet been widely implemented in the literature but which we believe holds promise, is to study how gradual trends in conflict and gradual trends in climate are correlated over time across locations. This long-difference empirical approach is employed by Burke & Emerick (2013) to demonstrate that the effects of climatic changes on agriculture in the United States over the course of multiple decades mirror estimates derived from the high-frequency annual time-series approach in Equation 3.

To demonstrate how this approach can be applied to the study of conflict, we implement a new analysis here using pixel-level data on a comprehensive measure of local conflict in East Africa (1991–2009) from O'Loughlin et al. (2012). We first compute gradual trends in conflict for each pixel and gradual trends in temperature for each pixel by differencing average values between the period 1991–1995 and the period 2005–2009. Trends in local temperature are shown in **Figure 6*a*** and range from pixels with no warming to pixels with more than 2°C warming. We then regress the change in local conflict on the change in local climate at the pixel level. In **Figure 6*b***, we compare the point estimate from this long-difference regression with the point estimate obtained using the high-frequency panel data approach in Equation 3. Similar to the findings of Burke & Emerick (2013), this long-difference estimate is nearly identical to the annual panel estimate, suggesting that these communities do not exhibit effective adaptation to climate change over this 20-year period. We also display the point estimate when we use even longer difference periods of 1991–1999 and 2000–2009 and find that the estimated effect actually becomes somewhat larger in magnitude (rather than smaller, which would be the case if there were partial adaptation over time), although we note that the effect is less precisely estimated and not statistically different from the other two estimates.

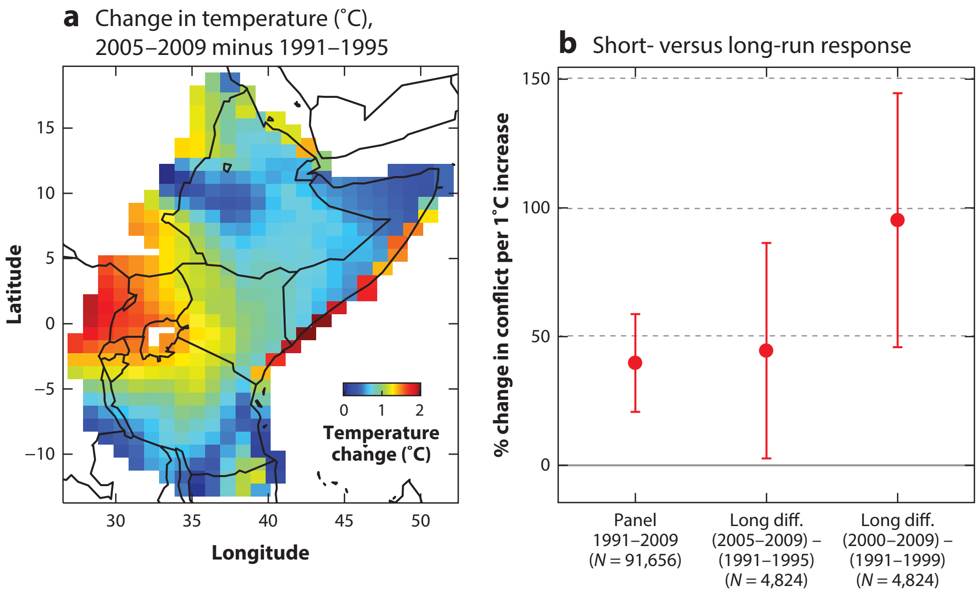

Figure 6

New calculations using data from O'Loughlin et al. (2012) demonstrating how adaptation to longer-run changes in temperature can be studied. (*a*) Multidecadal change in average temperature (in Celsius) across East Africa. (*b*) Comparison of panel estimates of how conflict responds to temperature using an annual panel data approach (as in Equation 3) and using a long-differences estimate that compares trends in conflict at each location with trends in temperature (from panel *a*).

The pattern in **Figure 6** indicates that, at least in the context of local conflict in East Africa, short-run changes in climate are closely related to the effect of longer-term and more gradual shifts in climate. To our knowledge, no study has characterized the degree of adaptation to climate in terms of conflict outcomes in this way. Further analyses using a similar empirical approach will help provide important insight into whether, when, and how societies can adapt to gradual climatic changes in other contexts, and such studies are a top priority for future research. Given the quantitatively large effect of current climate on conflict, it appears that future adaptation will need to be dramatic if it is to offset the potentially large adverse effect of future climate on conflict.

4.3. Implications for Global Climate Change

The above evidence makes a prima facie case that future anthropogenic climate change could worsen conflict outcomes across the globe in comparison to a future with no climatic changes, given the large expected increase in global surface temperatures and the likely increase in the variability of precipitation across many regions over coming decades (Field et al. 2012, Stocker et al. 2013). Recalling our finding that a 1σ rise in a location's temperature is associated with an average 2.1% net increase in the rate of interpersonal conflict and a 11.3% increase in the rate of intergroup conflict, and assuming that future societies will respond to climatic shifts in ways similar to current societies, one can calibrate the potential effect of anthropogenic global warming by rescaling expected temperature changes according to each location's historical variability. Hsiang et al. (2013a) rescale expected warming by 2050, computed as the ensemble mean for 21 climate models running the A1B emissions scenario (which is now thought to be conservative regarding future carbon emissions), in terms of location-specific standard deviations (Meehl et al. 2007). Almost all inhabited locations are projected to warm by at least 2σ, with the largest increases exceeding 4σ in tropical regions that are already warm and also currently experience relatively low interannual temperature variability. These large predicted climatological changes, combined with the quantitatively large effect of climate on conflict—particularly intergroup conflict—suggest that amplified rates of human conflict could represent a large and critical impact of anthropogenic climate change.

The magnitude of these back-of-the-envelope estimates provides some intuition for the potential change in conflict one might expect, but policy design requires more careful projections of potential impacts. Burke et al. (2015a) point out that projections of potential impacts should account for uncertainty in the structure and magnitude of warming that will be observed under a given policy, in addition to statistical uncertainty in the parameter estimates relating historical climate to conflict. This approach is adopted in two studies that make probabilistic projections for changes in the rate of future conflict attributable to climate change. Burke et al. (2009) use their estimates (which are slightly larger than the mean estimated conflict impact μ generated in the meta-analysis above) to project the incidence of civil wars and associated battle-related deaths in Africa under a business-as-usual scenario.[4] Accounting for uncertainty in the global climate response, Burke et al. (2009) estimate that warming will increase armed conflict incidence by 54% in the coming decades (95% confidence interval of 6–119% increase). If future civil conflicts remain as deadly as those that took place recently, then they project that this increase in conflict would result in 393,000 additional battle deaths by 2030.

[4]Burke et al. (2009) use the A1B scenario for models in CMIP3, a multimodel data set compiled from climate model output contributed by major modeling centers between 2005 and 2006.

Houser et al. (2015) conduct a meta-analysis of the nonlinear responses for violent crime and property crime to the distribution of daily temperatures in the United States. Then using the posterior response function, they project future changes in crime attributable to three warming scenarios using the distribution of 400 model-derived climate projections for each county in each scenario.[5] **Figure 7** demonstrates the spatial distribution and probability distribution of nationally aggregated changes to violent crime rates. Ceteris paribus, Houser et al. (2015) estimate that nationally averaged crime rates will rise 1.7–5.4% (with a median of 3%) by the end of this century under a business-as-usual warming scenario. Applying valuations to specific types of crime from a meta-analysis by Heaton (2010), Houser et al. (2015) estimate that these additional interpersonal conflicts would have a social cost of roughly $5–20 billion if they occurred in today's economy, a loss near but slightly less than their median projection for economic costs incurred through agricultural impacts. The bottom line is that the adverse impact of global climate change both on conflict outcomes themselves and on resulting economic outcomes may be substantial.

These projections for changes in conflict due to climate change capture only the partial effect of climate, and the overall trend in conflict will also certainly be affected by nonclimate factors. For example, if crime rates are expected to decline in the future because of (say) improved enforcement technologies and rising incomes, then the projected positive impact of future global warming on crime in the United States must be considered together with these other effects. It is clearly possible that overall conflict rates will decline in the future, even if warming causes an upward trend in conflict relative to a counterfactual with no warming. Any projected impact of climate change must be considered along with these other trends.

5. CONCLUSION

In this article, we survey and summarize the new and rapidly expanding economic literature on the links between climate and conflict. The question of the linkage between climate and conflict has been widely and inconclusively debated in other disciplines for many years, mainly using qualitative or case-study methods. Findings from the growing body of rigorous research in economics, as well as from political science and other disciplines that use modern econometric analytical approaches, indicate that adverse climatic events increase the risk of violence and conflict, at both the interpersonal level and the intergroup level, in societies around the world and throughout history.

The median effect of a 1σ change in climate variables is a 14% change in the risk of intergroup conflict and a 4% change in interpersonal violence, across the studies that we review in which it is possible to calculate standardized effects. In our separate meta-analyses of the contemporaneous and lagged effects of temperature and rainfall, we find that contemporaneous temperature has the largest average effect by far (2.4% per σ for intergroup conflict, 11.3% per σ interpersonal conflict) but that the cumulative effect of rainfall on intergroup conflict is also substantial (3.5% per σ). If future responses to climate are similar to these past responses, then anthropogenic climate change has the potential to substantially increase global violent crime, civil conflict, and political instability, relative to a world without climate change. This finding of course does not imply, nor do we conclude, that climate is the sole or even the primary driving force behind human conflict. However, we do conclude that large variations in climate can have large impacts on the incidence of conflict and violence across a variety of contexts.

[5]Houser et al. (2015) use the Representative Concentration Pathways (RCPs) from CMIP5, so their business-as-usual scenario is RCP8.5.

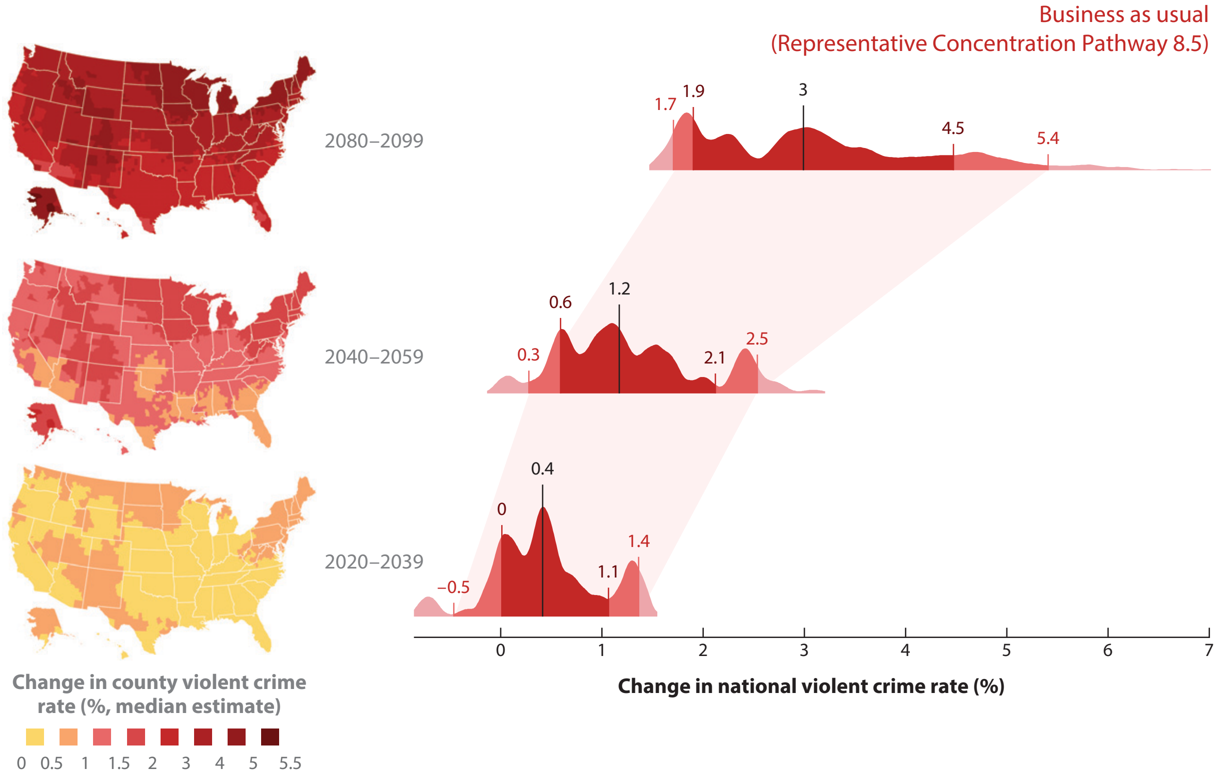

Figure 7

Projections of violent crime rates attributable to anthropogenic climate change in the United States under the business-as-usual Representative Concentration Pathway 8.5. Projections are compiled by resampling 40 climate models (weighted to reconstruct the probability distribution of global climate sensitivities; see Houser et al. 2015), 10 artificial weather realizations, and nonparametric response-function estimates from a US-specific meta-analysis. Maps on the left depict county-specific median projections. Probability distributions on the right depict the evolution of the national violent crime rate for the same periods (multiple modes result from climate model differences). National rates are constructed by weighting county changes by the current distribution of violent crimes across counties. Labeled critical values are 5th to 95th centiles (outer), 16.7th to 83.3th centiles (inner two-thirds of the probability mass), and the median (*black*). Figure modified from Houser et al. (2015).

We identify many open research questions and productive avenues for future investigation in this literature. Achieving a better understanding of the mechanisms—both economic and noneconomic, including possible psychological channels—underlying the link between extreme climate and conflict is arguably the most important and fruitful direction for future work. In this emphasis on research regarding mechanisms, we echo Dell et al.'s (2014) conclusion in their recent review of existing research on the links between climate and economic performance, in which conflict may play an important role.

It seems likely that climatic changes influence conflict through multiple pathways that may differ between contexts. There is considerable suggestive evidence that economic factors are important mechanisms, especially in low-income settings in which extreme climate often quite directly affects economic conditions through agriculture. The strong link between temporary high temperatures and short-run increases in crime in wealthier societies provides evidence that noneconomic factors, perhaps working through individual psychology, are also important. In other settings, the combination and interaction of economic, social, political, and psychological factors might be critical.

To place the state of the existing body of research on climate and conflict into perspective, it is worth recalling that statistical analyses identified the smoking of tobacco as a proximate cause of lung cancer by the 1930s (Witschi 2001), although the research community was unable to provide a detailed account of the mechanisms explaining the linkage until decades later. So although future research will be critical in pinpointing precisely why and how climate affects human conflict across different settings, disregarding the potential effect of anthropogenic climate change on human conflict in the interim is, in our view, a dangerously misguided interpretation of the available evidence.

The lack of a single, simple causal pathway linking climate and conflict does not imply that there is a lack of evidence that a causal relationship exists, but it does urge caution when applying these econometric estimates to either policy prescriptions or future global warming scenarios. Nonetheless, identifying key causal pathways is a research objective that holds great promise, as the policies and institutions necessary to reduce conflict can only be designed if we understand why conflicts arise in particular contexts. The success of such institutions will be increasingly important in the coming decades as changes in climatic conditions amplify the risk of human conflicts, and as the need for effective adaptation to climate becomes increasingly pressing.

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