A APPENDIX

This supplementary online appendix contains

- Supplementary Tables (2)
- Supplementary Figures (2)
- Supplementary Text: Relation to earlier reviews
- Supplementary Text: Methodological issues and criteria for inclusion in the review
- Supplementary Text: Leading explanations for the linkages between climate and conflict

Appendix Table A.1: **Unique quantitative studies testing for a relationship between climate and conflict, violence or political instability during the pre-1900 period**

Study	Sample Period	Sample Region	Time Unit	Spatial Unit	Independent Variable	Dependent Variable	Ref.
Africa and the Middle East							
Chaney 2011	641–1438	Egypt	annual	country	Nile floods	political instability	[33]
Cullen et al. 2000	4000 BCE–0	Syria	century	country	drought	collapse	[38]
Kuper et al. 2006	8000–2000 BCE	N. Africa	millenia	region	rain	collapse	[69]
The Americas							
DeMenocal 2001	500–2000	Peru	century	country	drought	collapse	[41]
Haug et al. 2003	0–1900	Mexico	century	country	drought	collapse	[52]
Kennett et al. 2012	40 BCE–2006	Belize	decade	country	rain	collapse	[65]
Stahle et al. 1998	1200–2000	USA	multiyear	municipality	PDSI$^{\odot}$	collapse	[93]
Asia							
Bai et al. 2010	220–1839	China	decade	country	rain	transboundary	[9]
Buckley et al. 2010	1030–2008	Cambodia	decade	country	drought	collapse	[18]
Kung et al. 2012	1651–1910	China	annual	county	rain	peasant rebellion	[68]
Yancheva et al. 2007	2100 BCE–1700	China	century	country	rain/temp	collapse	[100]
Zhang et al. 2006	1000–1911	China	decade	country	temp	conflict/collapse	[101]
Europe							
Büntgen et al. 2011	400 BCE–2000	Europe	decade	region	rain/temp	instability	[22]
Mehlum et al. 2006	1835–1861	Germany	annual	province	rain	violent	[77]
Patterson et al. 2010	200 BCE–1700	Iceland	decade	country	temp	collapse	[83]
Tol et al. 2009	1500–1900	Europe	decade	region	rain/temp	transboundary	[97]
Zhang et al. 2007*	1400–1900	N. Hem.	century	region	temp	instability	[102]

$^{\odot}$ PDSI is the Palmer Drought Severity Index. *Also see ref [103].

Appendix Table A.2: **Unique quantitative studies testing for a relationship between climate and conflict, violence or political instability during the post-1900 period**

Study	Sample Period	Sample Region	Time Unit	Spatial Unit	Independent Variable	Dependent Variable	Ref.
Africa							
Almer et al. 2012	1985–2008	SSA	annual	country	rain/temp	civil conflict	[2]
Brückner et al. 2011	1980–2004	SSA	annual	country	rain	political instability	[17]
Buhaug 2010**†[a]	1979–2002	SSA	annual	country	temp	civil conflict	[19]
Burke et al. 2009**†[a]	1981–2002	SSA	annual	country	rain/temp	civil conflict	[23]
Couttenier et al. 2011	1957–2005	SSA	annual	country	PDSI$^{\odot}$	civil conflict	[37]
Fjelde et al. 2012†	1990–2008	SSA	annual	province	rain	inter-group	[45]
Harari et al. 2011	1960–2010	SSA	annual	pixel (1°)	drought	civil conflict	[51]
Hendrix et al. 2012	1991–2007	SSA	annual	country	rain	inter-group	[54]
Miguel et al. 2004**[b]	1979–1999	SSA	annual	country	rain	civil conflict	[79]
Miguel 2005†	1992–2002	Tanzania	annual	municipality	rain	violent crime	[78]
O'Loughlin et al. 2012†	1990–2009	E. Africa	month	pixel (1°)	rain/temp	civil/inter-group	[81]
Pasquale et al. 2010	1979–2007	SSA	annual	country	rain	insurgency	[82]
Theisen et al. 2011†	1960–2004	Africa	annual	pixel (0.5°)	rain/drought	civil conflict	[96]
Theisen 2012†	1989–2004	Kenya	annual	pixel (0.25°)	rain/temp	civil/inter-group	[95]
The Americas, Europe, and Asia							
Bohlken et al. 2011	1982–1995	India	annual	province	rain	inter-group	[16]
Anderson et al. 2000[c]	1950–1997	USA	annual	country	temp	violent crime	[3]
Cohn et al. 1997**[d]	1987–1988	USA	hours	municipality	temp	violent crime	[36]
Hidalgo et al. 2010†	1988–2004	Brazil	annual	municipality	rain	inter-group	[56]
Jacob et al. 2007	1995–2001	USA	week	municipality	temp	crime	[62]
Kenrick et al. 1986 ‡	1985–1985	USA	day	site	temp	hostility	[66]
Larrick et al. 2011†	1952–2009	USA	day	site	temp	violent retaliation	[71]
Ranson 2012	1960–2009	USA	month	county	temp	crime	[84]
Rotton et al. 2000[d]	1994–1995	USA	hours	municipality	temp	violent crime	[86]
Sarsons 2011	1970–1995	India	annual	municipality	rain	inter-group	[88]
Vrij et al. 1994‡	1993–1993	Holland	hours	site	temp	police use of force	[98]
Global							
Bergholt et al. 2012	1980–2007	Global	annual	country	flood/storm	civil conflict	[13]
Burke et al. 2010†	1963–2007	Global	annual	country	rain/temp	political instability	[28]
Burke 2012†	1963–2001	Global	annual	country	rain/temp	political instability	[27]
Cervellati et al. 2011	1960–2005	Global	annual	country	drought	civil conflict	[32]
Dell et al. 2012	1950–2003	Global	annual	country	temp	pol. inst./conflict	[40]
Levy et al. 2005†	1975–2002	Global	annual	pixel (2.5°)	rain	civil conflict	[73]
Hsiang et al. 2011	1950–2004	Global	annual	world	ENSO*	civil conflict	[58]
Salehyan et al. 2012	1979–2006	Global	annual	country	PDSI$^{\odot}$	terrorism	[87]

†Reanalyzed using the common statistical model containing location fixed effects and trends (see ref. [61]). **Follow-up work challenges conclusions. ‡Actual experiment. *The El Niño-Southern Oscillation. $^{\odot}$The Palmer Drought Severity Index. [a]Also see discussion in refs [94, 21, 20, 19, 26, 24, 25]. [b]Also see discussion in ref [35, 80]. [c]Also see ref [4]. [d]Also see discussion in ref [29].

B Relation to earlier reviews

In prominent recent articles, [15], [46] and [89] discuss some of the recent progress in this literature and, to varying degrees, conclude that evidence of a causal association between climate and human conflict is mixed. They attribute this indeterminacy to inter-study differences in methods and a lack of sufficient data. The conclusions of this study diverge from these previous studies for five reasons:

1. More data is available in the literature than what these previous authors examine. Of the 37 distinct conflict data sets considered here, [15] examines 4 of them (11%), [46] examines 5 (14%) and [89] examines 3 (8%). Combined, these three articles discuss only 12 of the 50 primary studies that we review here.

2. These previous authors do not adopt a clear standard for scientific rigor, treating all articles as if the evidence they provide is of equal strength, even when some studies use substantially weaker research designs. For example, [15] and [46] cite [67] as providing strong causal evidence, even though the statistical theory the analysis it relies on had been previously shown to contain an error [48], and the first-stage relationship in their two-stage least-squares analysis is not significant, undermining the second stage of their analysis [49]. For these reasons [67] is not used for inference in the present study.

3. These previous authors sometimes misinterpret statistical findings. For example, [15] and [89] misinterpret [20] as overturning [23], even though the results of [20] are not statistically different from those of [23].

4. The authors sometimes make subjective judgements regarding the validity of findings without providing evidence to support those judgements. For example, [46], summarizes a recent finding that disagrees with his final conclusions by stating "Hsiang, Meng & Cane (2011) argue that the El Niño/Southern Oscillation (ENSO) has a significant influence on the onset of civil conflict. The link to global warming is tenuous and questions have been raised about the robustness of this finding" -p. 7, when no such questions have ever been raised in the published literature and no citation is provided by [46].

5. They review both qualitative and quantitative studies, with some of their conclusions based on a qualitative literature that is not evaluated here.

C Methodological issues and criteria for inclusion in the review

The basic goal of empirical research on climate and conflict is to identify causal relationships between climate variables and conflict outcomes of interest. Accomplishing this goal requires specifying a falsifiable hypothesis (e.g. "hot temperatures cause an increase in conflict") and developing a research design that can credibly evaluate the hypothesis. Specifically, it requires data on both climate variables and conflict and the specification of an empirical framework that will allow the researcher to test the hypothesis in the data and convincingly rule out alternate hypotheses.

While many potential empirical frameworks are available, not all are equal in their ability to make credible causal claims. Here we briefly review the advantages and pitfalls of various research designs, and highlight the criteria by which modern quantitative work judges a design capable of making credible causal statements about the relationship between two variables.

The quasi-experimental interpretation of historical data

A useful thought exercise is to identify the "ideal experiment" we might wish to run to be able to most cleanly identify the causal impact of climate variables on conflict outcomes. Suppose we are interested in the impact of increased global mean temperatures on the incidence of civil conflict - e.g., if the earth warms 2°C over the next 50 years, how will conflict incidence change? The ideal experiment to answer this question might then be the canonical "parallel worlds" setup, in which one of two identical worlds is exposed to 2°C warming over 50 years, and civil conflict outcomes are tracked in each. Because the two worlds are otherwise perfectly identical, comparing conflict outcomes in the heated world to the unheated world provides the causal estimate of the effect of a 2°C warming trend on civil conflict.

Such an experiment is unavailable and empirical research seeking to answer this or similar questions must somehow approximate this idealized experiment. There are two basic sources of variation in existing data that researchers can exploit to do so. The first is *cross sectional* variation, in which the researcher observes climate variables and conflict outcomes across many different units (individuals, units) at a single point in time. Exploiting this sort of variation leads to research questions such as: "Are countries that are hotter on average also more conflict prone?" The second sort of variation is *time series* variation, in which the researcher observes population over many different time periods. A researcher using time series variation might ask: "Over the past half century, has Country A experienced more conflict in hot years relative to cool years?". These two sources of variation can be combined in so-called *panel* or *longitudinal* studies, in which multiple units are observed over multiple time periods.

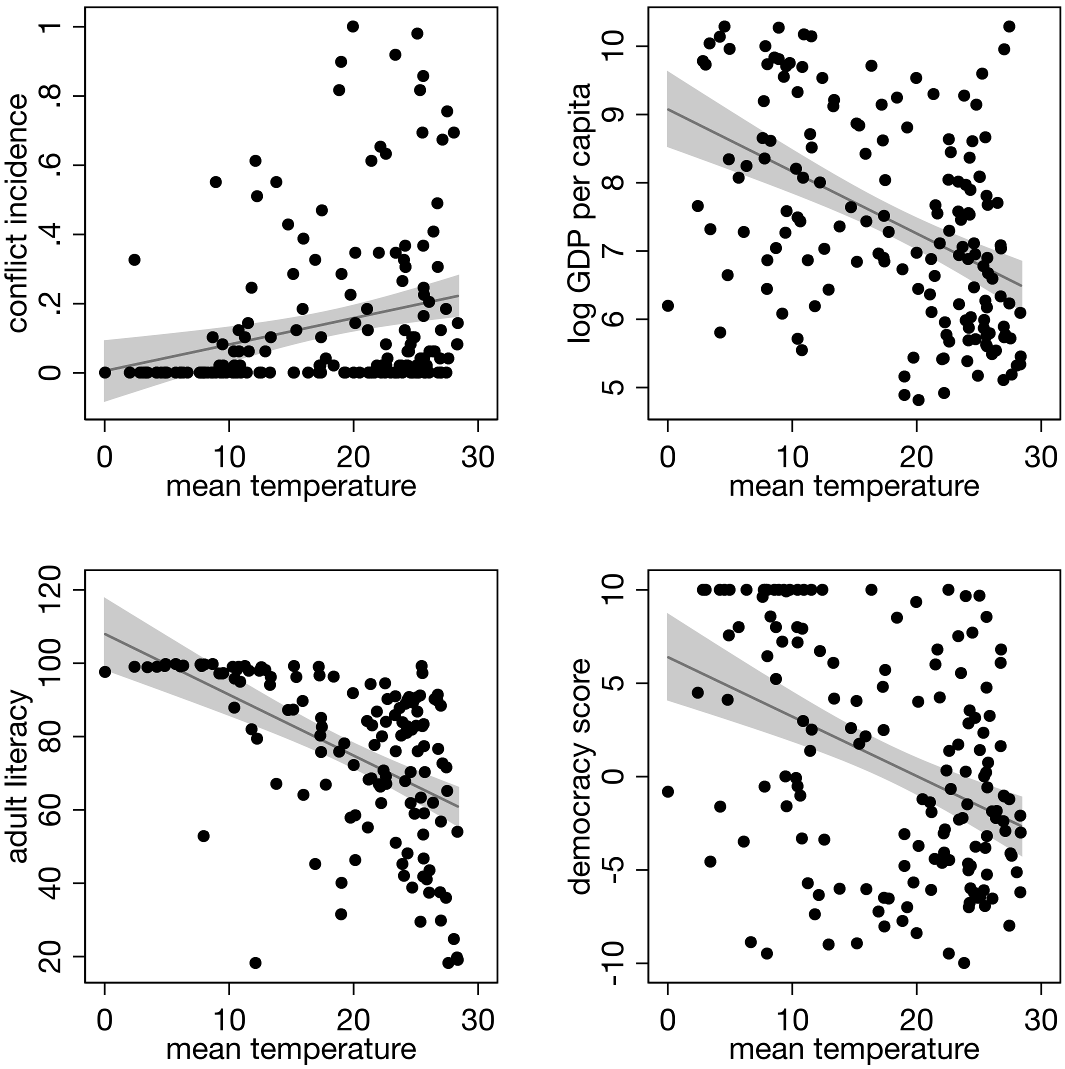
Unlike in the ideal experiment, however, the researcher using either of these sources of variation must confront the fact that her empirical setting does not have perfect experimental control. That is, because variation in climate could affect conflict in myriad ways, because in the real world climate variables could be correlated with many other variables that affect conflict, and because it is generally impossible to collect data on *all* the variables that might be correlated with both climate and conflict, the researcher must make assumptions about how the variables on which she does have data relate to variables on which she does not. Modern empirical work generally regards some of these assumptions as a plausible basis with which to make causal claims, and others as much less plausible.

Difficulties in the cross section

Consider the researcher using cross-sectional variation to explore the question of whether countries that are hotter on average are also more conflict prone. Using country-level data from the last half century, a simple least squares regression of average conflict risk (defined as the percent of years the country experiences a conflict that results in at least 25 battle deaths) on average temperature suggests a strong positive relationship between the two: a 1°C increase in average temperature is associated with a more than 5% increase in the likelihood of a conflict event. To interpret this relationship causally, the critical assumption is that average temperature is not correlated with other factors that also affect conflict risk.

Figure C.1 shows why this particular assumption is probably false. While temperature is positively related to conflict risk, it is negatively related to per capita incomes, adult literacy, and democratic institutions (among many other variables), and all three of these variables could plausibly affect conflict risk. Simply controlling for these or similar variables in the regression is unlikely to fix the problem, because there could be countless other variables that we do not observe that are also correlated with temperature and conflict (this is known as the problem of "omitted variables bias"). Moreover, controlling for variables that themselves could be affected by the climate variables of interest could actually *worsen* the problem by introducing new sources of bias[1] (this is known as "bad control").

[1] For instance, consider a regression of average conflict risk on average temperature at the country level. Including average income as a control in this regression is intuitively problematic if income is also affected by temperature: the regression is being forced to answer the question, "what is the effect of hot temperatures on conflict in poor countries" while being told to ignore the fact that the poverty could be a result of the same variation in temperature. The poor



Appendix Figure C.1: **The challenge of omitted variables in cross-sectional analysis.** Cross sectional relationship between country-level average temperature and four different outcomes over 1960-2008: civil conflict (top left), GDP/capita (top right), adult literacy (bottom left) and democracy (bottom right). The dark grey lines represent the predicted values from a regression of the outcome variable on a quadratic in temperature, and the grey shaded area is the 95% confidence interval. We include all countries for which data exist. Civil conflict data are from Uppsala/PRIO and indicate the percentage of years over the period in which there was a conflict resulting in at least 25 battle deaths. Data on GDP/capita come from the Penn World Tables, data on literacy are from the World Development Indicators. Democracy score comes from the PolityIV project [76], with -10 being perfect autocracy and 10 being perfect democracy.

In general, because it is impossible to know whether all potential confounders have been controlled for, studies that rely on cross-sectional variation to make causal claims about the relationship between climate and conflict must rely on assumptions that we know to be generally implausible.

Exploiting variation over time

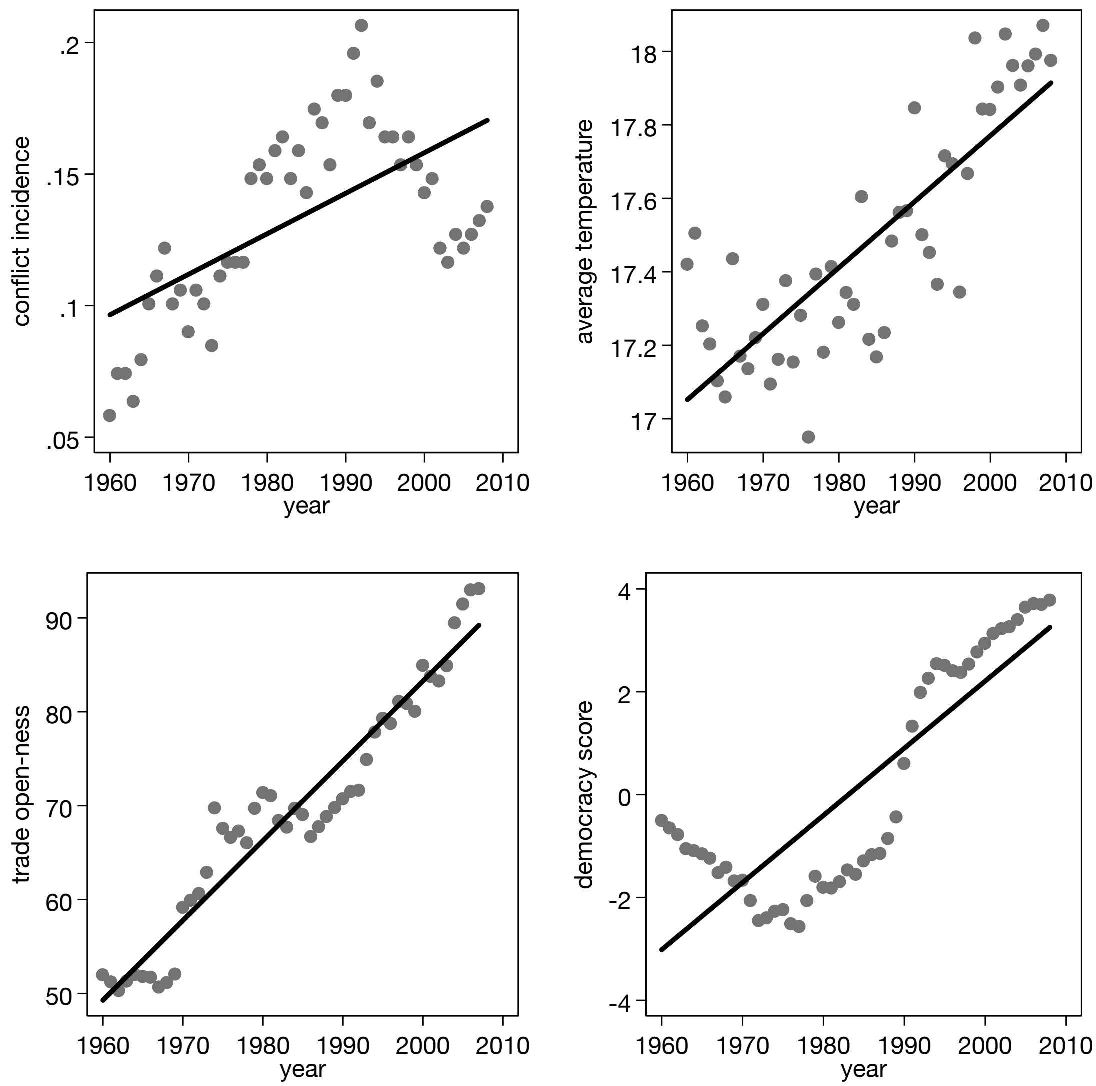
Time series variation offers some potential solutions to this identification problem, but comes with its own hurdles. The top two panels of Figure C.2 demonstrates that at the global level, conflict and temperature have both generally been trending up since 1960, and a simple time series regression of global average conflict on global average temperature would suggest that a 1°C increase in temperature increases the incidence of civil conflict by an impressive 25%. There is a clear omitted variable problem here too, however. As shown in the bottom panels of the figure, factors such as political regime type and trade openness have also been trending over the same period (as have numerous other variables such as population, economic growth, peacekeeping interventions, commodity prices, etc), and these common time trends mean that the observed relationship between conflict and temperature could again be driven by a third variable.

A potentially more promising time-series approach exploits *non-monotonic and high-frequency* variation in the climate and conflict variables of interest. This approach assumes that while trends themselves are not valid sources of variation on which to base causal claims, *variation around these trends* is plausibly random, particularly for the climate variables of interest. That is, temperature might be trending over time due to climate change, but whether a particular year is hotter or colder than the trend is likely to be somewhat random and thus less likely to be spuriously correlated with other factors that also affect conflict.[2] Causal claims using this sort of variation, then, rely on generally more plausible assumptions than in the previous two cases.

Finally, *panel* models utilize observations on multiple units (individuals, countries) over multiple time periods. Although both cross-sectional and time-series variation are available in this setting, in most applications it is again assumed that any observed time-invariant differences across units might be correlated with unobserved differences, and thus that these differences are an untrustworthy source of variation. Instead, panel models typically use unit-specific constants (known as "fixed effects") to eliminate these time-invariant cross sectional differences, and identification of causal effects again

countries that are not hot are likely fundamentally different than the countries that are hot (something made the former poor other than temperature), making the comparison in conflict outcomes as a function of temperature between these two types of countries clearly problematic. See [6] (p64-68) for a discussion of this issue.

[2]Operationally, these trends are typically removed via the inclusion of linear or quadratic time trend terms in the regression or year fixed effects.



Appendix Figure C.2: **The challenge of omitted variables in time-series analysis of monotonic trends.** Time series of globally-averaged civil conflict, average temperature, trade openness, and democracy score for the period 1960-2008. Civil conflict data are from Uppsala/PRIO and indicate the percentage of experiencing a conflict resulting in at least 25 battle deaths in a given year. Trade openness is total trade (imports + exports) as a percent of GDP, and comes from the Penn World Tables 7.0. Democracy score comes from the PolityIV project [76], with -10 being perfect autocracy and 10 being perfect democracy.

comes from within-unit variation over time.[3] The key assumption required for causal interpretation of these results is thus similar to the time-series approach discussed above: within-unit variation over time in the explanatory variables of interest is uncorrelated with other things that might affect conflict outcomes. If climate variables such as temperature or rainfall are the explanatory variables of interest, then this again appears fairly plausible in most settings. We note again, however, that there could be important trends in these variables over time, and failing to account for these trends in the research design could easily lead to spurious correlations between variables of interest.

Hypothesis testing

Once the researcher has arrived at a research design whose results have a plausible causal interpretation, the second important step is in determining the magnitude of the relationship between the variables of interest and whether any signal is statistically distinguishable from noise. Traditionally this involves specifying a falsifiable hypothesis (e.g. the null hypothesis that the effect of hot temperatures on conflict = 0) which the researcher then tries to reject. In statistical studies, this is accomplished by constructing a confidence interval for the effect size and then examining whether values consistent with the falsifiable hypothesis fall outside of that interval[4]. This approach is designed to minimize the possibility of mistakenly rejecting the baseline falsifiable hypothesis (eg. the null hypothesis) and thus concluding that the initial hypothesis was wrong when it was actually correct (a "Type I" error or "false positive"). From an inference point of view this is important, and researchers are typically hesitant to reject the baseline hypothesis unless there is less than a 10% chance that the observed effect size would have occurred by chance. When an estimated effect size passes this test, it is deemed "statistically significant".

Critically, however, the absence of a statistically significant change in the effect size, relative to the baseline hypothesis, does not automatically imply that there was no effect. Failing to falsify the initial hypothesis when it is wrong is known as a "Type II" error (or "false negative") and the probability of this type of error goes up (all else equal) when the signal to noise ratio falls, which may occur if the

[3]As in the time series application, spurious time trends are also a concern. These can be eliminated by including year fixed effects (i.e. a dummy for each year in the regression), or by including common or country-specific time trends, or perhaps both. A full treatment of panel models is not possible here, but the reader is referred to [90] for an accessible treatment and to [99] for a more complete treatment.

[4]The likelihood that an effect size is outside of a confidence interval is often measured with at test statistic. However, it is important to remember that test statistics are only a method for summarizing the relationship between an estimated effect size and a given confidence interval. A test statistic that indicates estimates are statistically significant at the X% level contains no additional information, it simply summarizes whether the observed relationship between the effect size, confidence interval and falsifiable hypothesis is likely to occur due to sampling error 100-X% of the time.

sample sizes shrink or when the variance in the outcome increases. In many situations, for example, the estimated effect size might be large but not significantly different from zero. In these cases it is probably incautious to conclude that there is no effect. Rather, the proper approach is return to the estimated confidence interval for the effect size, and to use this interval to characterize the range of effect sizes that are consistent with the data[5]. If the researcher is unable to rule out meaningful effect sizes, then this should be made clear. The converse is also true: a statistically significant finding does not automatically imply that the effect size is economically meaningful (the effect size could be precisely estimated but very small). In both cases, researchers should be clear about the range of effect sizes that are consistent with the data, and consider whether any effects within that range are meaningful.

We discuss the issues above because statistical research into the climatological triggers of conflict or social instability relies heavily on hypothesis tests and is particularly vulnerable to Type II error, which is often misinterpreted in the literature. Because these violent events are infrequent, and because it is likely that climate variables are only associated with a fraction of conflict events, studies with relatively few conflict observations as a percentage of total observations may have a low ratio of signal to noise. This has lead some recent studies to estimate associations between climate and conflict that have wide confidence intervals that are simultaneously consistent with the null hypothesis of no association as well as alternative hypotheses positing large associations. Results with so much uncertainty are relatively uninformative, but in some cases these findings[6] have been misinterpreted as proving a null hypothesis. The explanation above is intended to clarify why this is an incorrect interpretation of the hypothesis testing procedure.

External validity

The discussion above deals mainly with issues of "internal validity": under what conditions can a researcher credibly identify a causal relationship between two variables? Internal validity is clearly a necessary condition for sound empirical work, but is not the only concern. "External validity", or the generalizability of results from a particular study to other settings, is also important, particularly if

[5]It is exceedingly difficult to *prove* the baseline hypothesis (eg. the null). In fact, it is logically impossible. Hypothesis testing can only reject hypotheses that are inconsistent with the data, but it can never prove exactly which hypothesis is correct. The best we can do is consistently narrow the range of uncertainty in our estimates until our confidence intervals become very small. Thus, any statistical study which claims to prove that an effect size is a precise value, such as zero, is making a substantive error in interpreting its own analysis.

[6]Most of these studies are excluded from this review on separate methodological grounds, however some are described briefly in later sections.

(as in this review) we are interested in drawing broader conclusions about the relationship between climate and social stability.

We highlight two concerns with respect to external validity. The first is that a given relationship between climate and conflict at one spatial/temporal scale does not necessarily imply a relationship at another spatial/temporal scale. For instance, the mechanics of village-level conflict might differ significantly from that of national-level civil war, such that a strong relationship between climate and conflict estimated from a village-level study might not show up in a study using country-level data (or vice versa). Similarly, estimates derived from a particular historical time period might not be relevant for another time period if (again) the underlying mechanics that shape conflict outcomes have changed between the two periods. Understanding *why* climate variables affect civil conflict in a particular setting could provide additional insight into whether a particular finding extrapolates to another setting, but with available data it is often difficult to pin down the mechanism linking climate to conflict, even when studies show a clear and robust association between the two. However, as we review below, the fact that a large number of internally valid studies show similar results *and* span a range of spatial and temporal scales make the overall question of scale-dependence less worrisome. We cannot know for certain if the studies reviewed here translate to other settings of interest, however our belief in the external validity of these studies is bolstered by the observation that among the studies we examine, the findings of one study could, to a first approximation, predict the findings of other studies with different samples and at a different scale temporal and/or spatial scales.

The second concern is an idea we call the "frequency-identification tradeoff". As discussed above, many threats to internal validity can be overcome by using high resolution temporal data – for instance, having many observations on many units over time – but these gains come at a potential cost. A researcher using year-to-year variation in climate to understand the relationship between temperature and conflict is no longer estimating what happens to conflict as temperatures slowly warm over time (as in our ideal experiment). Instead she is asking how conflict responds to a year of hot weather relative to a year of normal weather. If agents respond to longer-run trends differently than to these inter-annual deviations, then estimates of the effect of climate on conflict from this time series approach might not represent actual impacts of a trending climate on conflict outcomes. Time series approaches that use lower-frequency variation in climate and conflict – e.g. decade, century or millennium-scale variation, as in the pre-1900 studies discussed below – more closely approximate the ideal experiment. Unfortunately, these gradual climatic changes could be accompanied by unrelated social changes that also affect conflict, an unknown omitted variable. Thus, at high frequencies, the observed climatological changes are a poor proxy for future warming but a causal relationship can be well identified (no omitted variables), whereas at low frequencies, the observed climatological changes are a good proxy for future warming but our identification of causal relationships weaken (it is more likely there are omitted variables). This frequency-identification tradeoff is fundamental to the problem under investigation as it arises from the mismatch between the time-scale at which human societies evolve and the time-scale at which anthropogenic warming will occur. Because this tradeoff is so fundamental, it is unlikely that there is an easy or clever resolution to this problem and, as a result, we instead simply try to examine the best-identified studies across the full spectrum of frequencies. By exploring this entire frontier of best studies and discovering that both low and high frequency studies arrive at similar conclusions, we gain confidence that our inferences are not driven by inappropriate climate measures or omitted variables. Nonetheless, it is important to be cognizant of this tradeoff throughout the review.

D Leading explanations

Why are there such strong associations between climatic changes and conflict around the world, throughout history, and across all spatial and temporal scales? The literature does not yet provide a definitive answer. Several non-exclusive pathways have been proposed and we briefly describe them here, noting that it is possible that multiple mechanisms are simultaneously involved, and that some mechanisms could be relatively more important than others in certain contexts.

Government capacity

Peace and stability within a society is enforced, in part, by the branches of government that maintain law and order. In situations where government capacity is weakened, laws may be difficult to enforce and opportunistic opposition groups may attempt to expand their control. Climatic changes may affect the strength of an incumbent government or the balance of power within a government since government finances are supported by taxing an economy that depends on current climatic conditions [92, 59, 91, 11, 47, 40, 64]. Several studies above suggest that this mechanism may destabilize the position of individuals within the government [27, 33] or existing government institutions [18, 100, 52, 103, 28, 17]. However, the role of this mechanism for influencing violence or conflict outside of the incumbent government (eg. personal or inter-group violence) remains unknown.

Labor markets

If individuals within a population willingly choose to engage in violent conflict, it must be worth their time to do so. If the rewards from using violence increase relative to the value of alternative non-violent activities, then it should become more likely that rational individuals will engage in violence [34, 39]. In contexts with stable climates, it has been shown that this mechanism can contribute to unstable situations if the value of non-violent activities falls [42, 14] or if the value of violence rises [42, 5, 72], but it is unknown whether this mechanism plays an important role in situations where the climate is changing. It has been argued that a climate-induced slowdown of economic growth should reduce the value of participating in the non-violent economy [79, 16], however studies making this case have not explicitly tested and rejected alternative explanations.

Inequality

Violence and instability may arise from the desire of individuals to redistribute wealth based on notions of fairness, holding labor market opportunities constant. If climatic changes differentially affect the income or wealth of individuals within a given population [7], this may increase inequality and could plausibly trigger conflict. Alexis de Tocqueville argued that climate-induced crop failures in France were a contributing factor to determining the timing of the French Revolution [50] and Hidalgo et al. (2010) found that highly unequal distributions of land rights in Brazilian municipalities were an important precursor to predicting rainfall-induced land invasions. However, in neither of these accounts is the initial distribution of wealth determined exogenously, so it is possible that unequal wealth distributions and conflict susceptibility are not directly related but instead are both outcomes of some unobserved social characteristic.

Food prices

Climatological changes influence the productivity of food production systems [92, 74, 91, 60] and these changes in food supply alter food prices around the world [85]. Rapid increases in food prices have large impacts on the budgets of low income households who spend large fractions of their income on staple foods, possibly altering the incentives of these households to willingly engage in conflict or to demand political action or changes [70, 103, 8]. However, the strength of a systematic link between food prices and conflict has recently been called into question [12, 31] and it remains to be shown that climate-induced price changes are directly associated with violence or instability.

Logistics

Organized violence and political conflict require the execution of various organizational and logistical feats [43], and it is possible that climatic conditions alter various logistical constraints that would otherwise hinder violent contests. For example, it has been suggested that heavy rains might prevent armed groups from traveling quickly along dirt roads [51], that the presence of rain-fed vegetation makes inter-group conflicts, such as cattle raiding, more cost effective [30], and that terrorism attacks require financing that is hampered by drought [87]. However, to date, clear evidence of these mechanisms is absent and studies that propose them do not exclude alternative hypotheses.

Misattribution

Popular support for pre-existing institutions and incumbent leaders is often based on their recent performance [55, 75]. However, it has been demonstrated that seemingly rational individuals often misattribute random events to the performance of their governments [1, 53] and sometimes punish them, through voting or otherwise, for events that are beyond their control. If climatic events generate economic hardship [59, 11, 40], then populations may misattribute the deterioration of economic conditions to the actions of their government, causing them to support political acts which may lead to conflict or instability. It is also possible that patterns of personal violence arise from a misattribution of personal responsibility for poor economic conditions among non-leaders, such as naming (and killing) witches who have "cursed" a village with low rainfall [78].

Psychology

Participants in organized political conflict often exhibit careful and rational calculation of the costs and benefits of specific actions. However, large-scale violence also can emerge organically from disorganized emotional interactions among many individuals, as is the case in riots, or because a calculating leader inflames the passions of their followers, as is the case in many contested elections. Psychological studies above repeatedly document that holding an initial stimulus fixed, violent responses by subjects become more likely under hotter environmental conditions [66, 98, 36, 3, 86, 71, 62]. While these results have clear implications for personal level violence, it has not yet been shown whether psychological responses to climatic changes contribute to large-scale conflicts or instability.

Migration and urbanization

It has been hypothesized that patterns of spatial contagion in climate-associated conflict [63] could be driven by population displacements, but this notion remains unverified. In addition, some types of conflict or instability emerge from densely populated urban centers and it has been demonstrated that climatic events that damage agricultural incomes can lead to the migration of populations out of rural locations [10, 44, 57] and into urban centers. If rapid climate-induced urbanization places additional stress on urban infrastructure and resources then this may substantially influence urban markets (eg. for food, labor or land), generate popular frustration with overextended local governments, or directly contribute to new conflict through other pathways.

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