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Supplementary Materials for

Quantifying the Influence of Climate on Human Conflict

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This Supplementary Online Materials appendix contains four sections, with additional treatment of the following topics:

A Study selection, reanalysis and evaluation

Our conclusions from the literature are based only on those studies that implement Eq. 1 from the main text or one of the mentioned alternatives. In select cases, studies did not meet this criteria but the data from these analyses were publicly available or supplied by the authors. In these cases, we reanalyzed the data from the original analysis using this common method. Table 1 lists the 60 studies that met this criteria initially or were reanalyzed, and upon which we base our conclusions.

Following the norms of social science where publication lags are long – often on the order of several years – we include archived working papers that have not yet been published in journals. This standard is important because journals may exhibit a publication bias by underreporting null results and because research on this topic has expanded rapidly in the last few years, with half the studies released since 2011. After their careful review, we decided to include 16 unpublished working papers as primary studies to ensure our analysis is as up-to-date as possible. The inclusion of these studies does not drive our findings and our conclusions would continue to hold even if they were omitted. For example, of the 27 estimates for the effect of temperature on modern human conflict, 21 come from published studies and all 21 indicate that higher temperatures are associated with higher rates of conflict. If there were no causal relationship between warming and conflict, obtaining 27 estimates agreeing in a positive association would be extremely unlikely to occur from sampling variability alone. If we were to omit the four unpublished estimates then we would obtain agreement in sign across 21 published studies, still a very unlikely event to occur by chance.

Similarly, we note that while the authors of this analysis are active researchers in this field, our own contributions are modest in comparison to the collective output of the global research community. By our count, at least 190 authors from around the world conducted the 60 primary studies that we analyze here, of which only five (*40, 43, 48, 51, 64*) are products of our own efforts in collaboration with coauthors that did not participate in this analysis. In this review, we critically revaluate our own work, in light of more recent methodological advances and published criticisms (see Table 1 and below) and in three out of five cases returned to our original data (*40, 48, 64*) to reanalyze it. In one case (*40*) we found it appropriate to focus on conclusions weaker than the results emphasized in the original study to ensure that our inferences from the study were consistent with the rigorous methodological standards we adopt here (although, note that the result presented here was also presented in the original study). We also point out that if our five studies were omitted from this analysis then our overall conclusions would remain unchanged.

Below we provide additional details on the papers appearing in Fig. 4-5 (a subset of which appear in Fig. 2), including the econometric specification used for each, and the method and reason for reanalysis in cases where it was done.

Our strict methodological standards led us to exclude several studies, some of which reported an association between climate and conflict and some of which reported no association (see Section A.2 below). Because reanalysis of previous studies is often difficult and time-intensive, it is neither cost-effective nor reasonable to reanalyze all studies that failed to meet our standards. In determining which studies merited reanalysis, we selected studies that presented unique data sets that were not analyzed according to our standards elsewhere, that were structured such that Eq. 1 could be estimated, and for which data and replication code was available. For the sake of transparency, we describe below why several published studies were not reanalyzed here. We also note that while reanalysis caused the body of literature as a whole to appear more consistent than previously thought, it did not uniformly cause all studies to exhibit a stronger association than was originally reported – the results of some studies that reported a climate-conflict relationship were weakened by reanalysis (for examples, refs. (*49, 55*)).

A.1 Papers appearing in Fig. 4-5

- Auliciems and DiBartolo 1995 (*29*). We compute the average response across days in each week by dividing day-of-week-specific coefficients reported in their Table 3 by day-of-week-specific average $CALLS$ reported in their Table 1 and then averaging across days.

- Bergholt and Lujala 2012 (*75*). We use replication data to estimate Model 12 in their Table V, dropping the outcome variable covariate (per capita GDP). Standard errors are clustered at the country level. The coefficient on population affected by natural disasters is essentially unchanged from what is reported in their table.

- Bohlken and Sergenti 2011 (*44*). We use replication data to estimate the reduced form version of Model 2 in their Table 4, and focus on the contemporaneous effect of rainfall on the number of riots. We estimate the model with state and year fixed effects but without the additional controls, and cluster the errors at the state level. The p-value on the contemporaneous rainfall coefficient we report is p = 0.124.

- Brückner and Ciccone 2011 (*78*). We report results from Model 1 in their Table 3.

- Buhaug 2010 (*22*). This paper and a companion paper (*134*) challenge earlier results in Burke et al. 2009 (*64*). Many of the results presented in these papers are based on research designs that did not meet our common standard: they do not control for location fixed effects[1] or time trends, they include outcome variables as covariates, and they misinterpret statistical uncertainty. The new result in these papers that meets our methodological criteria is to show that the statistical significance of

[1]Buhaug (*134*) explicitly argues that fixed effects should not be included in the analysis, however in our reanalysis we conduct an F-test to jointly test the significance of the fixed effects – if the fixed effects were similar, it is possible that their omission could be justified in this special case. However, this F-test rejects the hypothesis that country fixed effects are the same across countries ($p<0.001$) indicating that they should not be omitted from the model.

the temperature result in Burke et al. depends on the mortality threshold used for defining conflict.

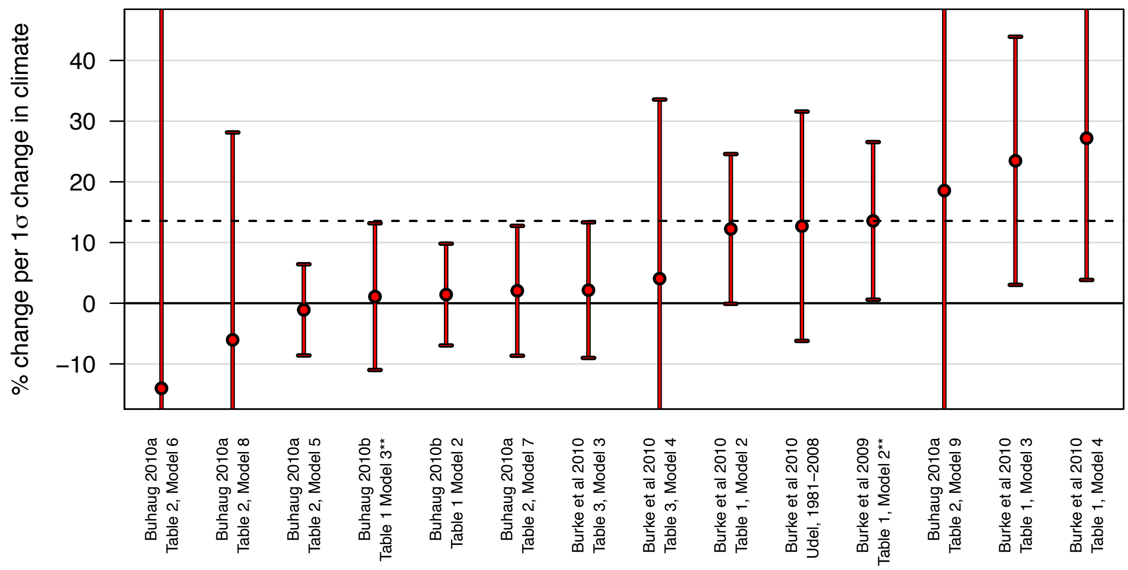
In Fig. 4, we report the finding in Model 3 in Table 1 in ref. (*22*), focusing on the contemporaneous effect of temperature on conflict incidence. This is the median standardized effect size across seven different estimates given in Buhaug (*22, 134*) that meet our criteria. We plot these estimates in Fig. S1, along with the range of comparable estimates from Burke et al (*64, 135*). Confidence intervals on most estimates are wide, and only 2 out of 14 estimates (14.3%) can reject a standardized effect size of 10% per 1σ. Models in Supplementary Fig. S1 with stars (**) are those that are shown in Fig. 5. Following critiques in ref. (*22*), we also include a model using data from the full 1981-2008 period that was not reported in ref. (*135*); the standardized effect in this estimate is nearly identical to the main result in Burke et al 2009, albeit with a somewhat wider confidence interval.

- Burke et al 2009 (*64*). We report results from Model 2 in their Table 1, focusing on the contemporaneous effect of temperature. This is shown in Fig. S1, labeled with "**". Standard errors are clustered at the country level.

- Burke 2012 (*71*) (who we note is not related to the author of this article) originally used temperature and rainfall as instrumental variables for the effect of economic growth on leadership exit. Burke reported the reduced form effect of current and lagged temperature and rainfall on leadership exit in a model that contained outcome variables as covariates (their Table 3). Following the author, we use data from the author to estimate a linear probability model for leadership exit:

$$leader\_exit_{it} = \beta_1 temp_{it} + \beta_2 temp_{i,t-1} + \beta_3 prec_{it} + \beta_4 prec_{i,t-1} + \mu_i + \bar{\theta}_i \times t + \epsilon_{it} \quad (2)$$

where i=country, t=year, μ_i is a country fixed effect, and $\bar{\theta}_i$ is a country-specific trend. $leader\_exit$ is a dummy variable for any leader exit and $temp$ and $prec$ are temperature and precipitation variables, all are described in their paper. Errors are clustered at the country level. We focus on the contemporaneous effect of temperature. The p-value on the temperature coefficient we report is p = 0.06.



Supplementary Figure S1: **Standardized effect sizes in Buhaug (*22,134*) (2010a, 2010b) and Burke et al. (*64,135*) (2009, 2010).** Each marker represents the estimated effect of a 1σ increase in a climate variable, with the magnitude of the response expressed as a percentage change in the outcome variable, relative to the mean conflict rate. Whiskers represent the 95% confidence interval on this point estimate. Colors and marker shapes are as in Fig. 5, and dashed line is the median estimate from Fig. 5. Estimates marked with "**" are shown in Fig. 5. The selected estimate from Buhaug is his median estimate while the selected estimate from Burke et al. is their primary specification, which is below their median estimate.

- Burke and Leigh 2010 (*77*) (we note that the former is not related to the author of this article) originally used temperature and rainfall as instrumental variables for the effect of economic growth on democratic-change events. The authors do not report the reduced form effect of these instruments. Following the authors instrumental variables specification and notation (Table 4, Model 2), we use the replication data to estimate

$$\begin{aligned} demchangeevent_{it} = &\beta_1 tempdevnew1interact_{i,t-1} + \beta_2 tempdevnew1interact_{i,t-2} \\ &+ \beta_3 precipitationinteract_{i,t-1} + \beta_4 precipitationinteract_{i,t-2} \\ &+ \mu_i + \theta_t + \epsilon_{it} \end{aligned}$$

  where i=country, t=year, μ_i is a country fixed effect, and θ_t is a country-specific trend. $demchangeevent$ is a dummy variable for democratization events and temperature and precipitation are represented by the variables $tempdevnew1interact$ and $precipitationinteract$, as described in their paper. Errors are clustered at the country level. We focus on the one-year lag effect of temperature and precipitation, which are implicitly the focus of the authors who use these variables to instrument for one-year lagged income growth. We find that the effect of temperature on democratization events (β_1) is positive, large and statistically significant (p= 0.03) for the full sample (shown in Fig. 5). When we restrict the sample to countries that are initially autocratic, which is the sample that the authors focus on, the effect becomes 15% larger but also slightly less precisely estimated (p=0.06). The effect of precipitation (β_3) is negative, consistent with other analyses, but it is not statistically significantly different than zero (p=0.50).

- Card and Dahl 2011 (*37*). We report results from a version of Model 3 in their Table 4, where we obtained replication data from the authors and re-estimated the regression with only the “hot” and “cold” weather variables as covariates. “Hot” is a dummy variable indicating a maximum daily temperature above 80F. The coefficient on the “cold” dummy variable (a day below 32F), which we do not display, is negative and also significant: cold temperatures reduce domestic violence in their data by about the same amount. Following the authors, standard errors are clustered by the team-season. Estimating the model with OLS instead of poisson regression delivers coefficients that are slightly larger and more statistically significant, and coefficient estimates including their full set of football score covariates delivers similar results. We report the smaller and marginally significant poisson coefficients (p = 0.06).

- Couttenier and Soubeyran 2012 (*53*). We report results from Model 3 in their Table 2.

- Dell, Jones, and Olken 2012 (*21*). We report results from Models 4 and 5 in their Table 6, for civil war onset and irregular leader transition, respectively. Following the authors, standard errors are two-way clustered at the country level and at the year level.

- Fjelde and von Uexkull 2012 (*55*). We use data from the authors to estimate Model 5 in their Table 1 with OLS and include location fixed effects and year fixed effects, dropping their outcome variable covariates. Standard errors are clustered at the location level (first administrative level). Marginal effects are somewhat smaller than what they report and standard errors are larger, causing their results to be marginally statistically significant after reanalysis. The p-value on the coefficient we report is p = 0.09.

- Harari and La Ferrara 2013 (*52*). The updated (2013) version of their working paper does not include a model with cell fixed effects, so we retain the estimate from the earlier (2011) version of the paper with cell fixed effects. After discussions with the authors, we report results from Model 1 in their Table 4 in their 2011 paper, focusing on the contemporaneous effect of SPEI (the "Standardized Precipitation-Evapotranspiration Index"). We note that in this version of the paper, a higher SPEI indicates less drought-like conditions, such that the relationship between SPEI and conflict is negative.

- Hendrix and Salehyan 2012 (*46*). We re-estimate their results in Model 7 in Table 3 with OLS and country fixed effects and year fixed effects, dropping the additional control variables. Standard errors are clustered at the country level. To make results comparable with other studies, we follow Hidalgo et al and use the absolute value of rainfall deviations from the mean as the independent variable.

- Hidalgo et al 2010 (*25*). We report results from Model 7 in their Table 3. Errors are clustered at the municipal level.

- Hsiang, Meng, and Cane 2011 (*51*). We report results from Model 3 in the their Table 1, focusing on the effect for teleconnected regions.

- Jacob, Lefgren, and Moretti 2007 (*35*). We report results from the first columns of Panel A and B in their Table 1, which we re-estimate using data from the authors. Following the authors, regressions are weighted by county average crime levels. To account for spatial and temporal correlation, we cluster the standard errors by both jurisdiction and state-year.

- Larrick et al 2011 (*36*). Using data from the authors to estimate the linear probability model
$$hit\_batter_{ijdt} = \beta temp_{jdt} + \mu_j + \theta_t + \epsilon_{ijdt} \tag{3}$$
where i=is an at-bat, j=ball park, d=day of game, t=year, μ_j is a park fixed effect, and θ_t is a year fixed effect. hit_batter is a dummy variable that is one if a batter is hit by the pitcher. Errors are clustered at the game level. Because Larrick et al focus on retaliation against batters and interact temperature variables with factor variables describing the number of previously struck batters, we restrict the sample to those at-bats when retaliation is possible, i.e. plays when the batter's pitcher had previously hit a batter on the opposing team.

- Levy et al 2005 (*49*). We use data from the authors to reestimate the linear probability model
$$any\_conflict_{ijt} = \beta WASP_{ijt} + \mu_i + \theta_t + \epsilon_{ijt} \tag{4}$$
where i=grid cell, j=country, t=year, μ_i is a grid-cell fixed effect, and θ_t is a year fixed effect. $any\_conflict$ is a dummy variable that is one if any conflict is estimated

to begin in the cell and $WASP$ is the Weighted Anomaly Standardized Precipitation index, where both variables are described in their paper. Errors are two-way clustered, at the country-by-year level to account for spatial correlation and at the grid-level to account for serial correlation. The p-value on the coefficient we report is p=0.10.

- Maystadt, Ecker, and Mabiso 2013 (*66*). We report results from Column 1 in their Table 1. Errors are clustered at the administrative region level.
- Miguel, Satyanath, and Sergenti 2004 (*48*). We report results from column 1 in their Table 3, focusing on the role of lagged precipitation, with errors clustered at the country level. We report this result in addition to Burke et al 2009 because Miguel et al focus on a different outcome (civil conflict rather than civil war) and a different climate variable (rainfall growth rather than temperature). Using a different climate dataset, Burke et al 2009 find that the effect of precipitation is no longer significantly different from zero once temperature is included in the regression; however, using the original precipitation data in Miguel et al 2004 and including the temperature data from Burke et al 2009 yields a precipitation coefficient statistically indistinguishable from what we report here. Thus we retain the Miguel et al result as a unique result.
- Miguel 2005 (*40*). We report results from column 5 in his Table 4. Standard errors are clustered at the village level. The p-value on the coefficient we report is p = 0.14.
- O'Laughlin et al 2012 (*23*). In their main specifications reported in the paper, the authors do not control for location fixed effects, and they include outcome variables as covariates (although they show in an appendix that their temperature results are robust to including the location fixed effects and to dropping outcome variables from their controls). Because the authors use monthly data, an additional concern is that the authors have not accounted for seasonality: certain months in the year could be both warmer and have higher conflict for some unobserved reason (e.g. perhaps conflict always happens in the non-harvest season). To account for this, we follow Ranson (*38*) and include grid cell-by-month fixed effects, which accounts for any

seasonality in each grid cell. Preserving the variable names in their replication files, we estimate the following specification:

$$events_{imt} = \beta_1 spi6_{imt} + \beta_2 ti6_{imt} + \mu_{im} + \theta_t + \varepsilon_{imt} \tag{5}$$

where i=grid cell, m=month, t=year, μ_{im} is a grid-by-month fixed effect, and θ_t is a year fixed effect. $events$, $spi6$, and $ti6$ are the conflict, precipitation, and temperature variables as defined in their paper and replication files. Errors are two-way clustered at the country-by-year level to account for spatial correlation and at the grid-level to account for serial correlation. The β's are thus estimated from within grid cell variation over time, accounting for any seasonality in climate or conflict within each grid, and accounting for any trends in climate or conflict over the study period across the region as a whole. We find that the authors' results for temperature are robust, but that their precipitation results are no longer statistically significant. Estimating the model with country-by-year fixed effects, which would allow for non-parametric country-level trends in climate and conflict, yields quantitatively similar results for temperature.

- Ranson 2012 (*38*). To make results comparable to other studies, we use data from the author to replicate Columns 1, 2, and 4 in his Table 2 but replace the binned temperature and precipitation variables with simpler linear specifications; we include maximum temperature and precipitation (and their lags) as regressors, in addition to his county-by-month and county-by-year fixed effects. Following the author, regressions are weighted by county population. Errors are clustered two-ways, at the state-by-year level to account for spatial correlation and at the county-level to account for serial correlation.

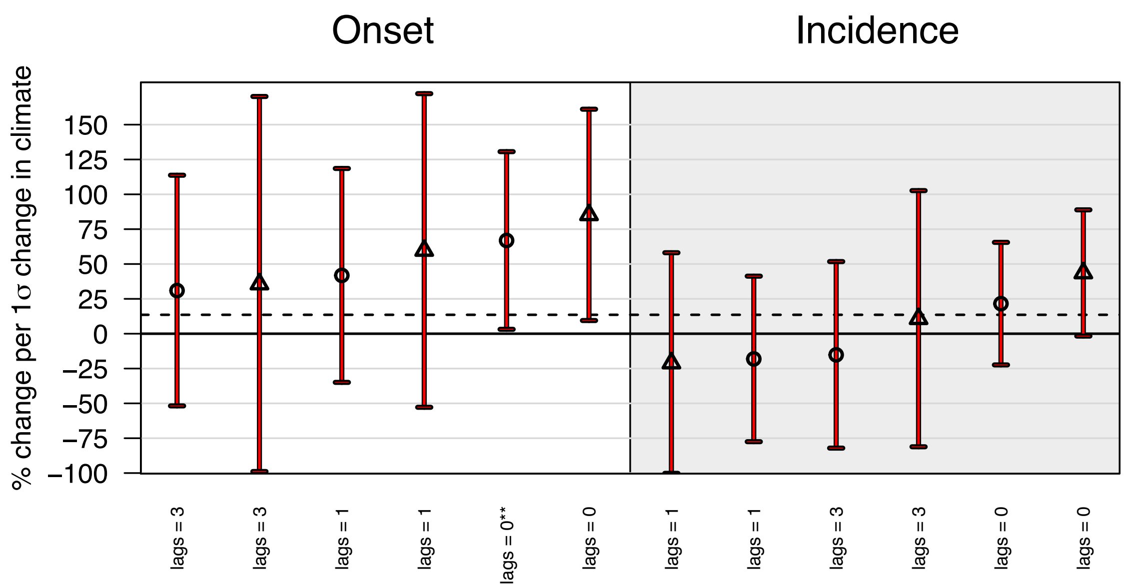
- Sekhri and Storeygard 2012 (*41*). We report results from Column 1 in Table 2 (dowry deaths) and Column 1 in Table 3 (domestic violence).

- Theisen 2012 (*14*). The original result did not include both temperature and precipitation in the same regression and did not report the marginal effect of temperature in their Table 1 or Fig. 2, where marginal effects of other variables are reported. Using replication data, we estimate:

$$conflict_{it} = \beta_1 temp_{it} + \beta_2 prec_{it} + \mu_i + \theta_t + \varepsilon_{it} \tag{6}$$

where i=grid cell, t=year, μ_i is a grid fixed effect, and θ_t is a year fixed effect. $conflict$, $temp$ and $prec$ are the conflict, precipitation, and temperature variables, respectively, as defined in their paper. Errors account for spatial correlation across contemporaneous grid cells within 100 km of one another and grid-specific serial correlation (equivalent to grid-level clustering) following the approach of ref. (*20*). The coefficient on temperature is large and statistically significant at the 95% confidence level (p=0.027), which is at odds with the paper's conclusions. The original article does not test for the marginal effect of temperature, and instead only presents models that are quadratic in temperature and then separately tests whether the coefficients on $temp$ and $temp^2$ are significant, whereas the correct test for the effect of temperature requires that both coefficients are tested jointly. As our Fig. 2F and the author's Table 1 Model 5 shows, there is no evidence that the relationship between conflict and temperature is quadratic in this sample, so there is no reason to use a quadratic model.

To demonstrate that the marginal effect of temperature that we report from the paper is representative of the many different models estimated in the original article, we re-estimate multiple versions of Equation 6 for both of his outcome variables ("conflict" is a dummy for the "first event of a conflict that generated at least 25 deaths in the same year"; "event" is a dummy for whether or not there was a conflict in that cell-year. We label these "onset" and "incidence" in the Figure). Following Theisen, we estimate models with climate variables in levels or in anomalies (i.e. subtracting the cell mean and dividing by the cell standard deviation). Finally, because Theisen notes that temperature variables appear to switch signs depending on how they are lagged, we estimate models with either 0, 1, or 3 lags (e.g. the model with 3 lags includes $Temp_t$, $Temp_{t-1}$, $Temp_{t-2}$, and $Temp_{t-3}$). For each model we calculate the standardized effect of temperature, which for models with lags is the summed effect of temperature across all lags including the zero lag. Results are shown in Fig. S2. None of the 12 models estimate can reject our median estimate of 14% per 1σ, and 7 out of 12 models cannot reject a 100% per 1σ effect.



Supplementary Figure S2: **Standardized effect sizes in Theisen (*14*).** Each marker represents the estimated effect of a 1σ increase in a climate variable, with the magnitude of the response expressed as a percentage change in the outcome variable, relative to the mean conflict rate. Whiskers represent the 95% confidence interval on this point estimate, with standard errors corrected for spatial correlation. Colors are as in Fig. 5; circular markers indicate models using climate variables in levels, and triangular markers indicate models using climate variables in anomalies. Models include the number of climate lags as indicated on the x-axis, and the estimate marked with "**" – the model closest in functional form to the other studies we analyze – is shown in Fig. 5. Outcomes are given at the top of the figure and are as described in the text. The dashed line is the median estimate from Fig. 5.

- Theisen, Holterman and Buhaug 2012 (*24*). The published version of this paper does not control for location fixed effects and includes outcome variables as covariates. Preserving the variable names in their data files, we use data from the authors to estimate the following specification:

$$onsetx_{it} = \beta spi6dum_{it} + \mu_i + \theta_t + \varepsilon_{it} \tag{7}$$

where i=grid cell, t=year, and $onsetx$ and $spi6dum$ are the conflict onset and precipitation variables as defined in their paper. We run the specification on the full dataset instead of downsampling the data as was done in the original analysis. As with the other gridded datasets, errors are two-way clustered at the grid level and at the country-year level

The estimated effect of precipitation is positive, but as indicated in the figure the confidence interval is large, meaning that large negative or positive effects cannot be ruled out. We note that this high level of uncertainty is "built in" to the statistically underpowered design of the study, which attempts to predict 59 pixel-by-year conflict events across 363,811 pixel-by-year observations. The primary innovation of the study is to assign large scale wars to very specific locations ($\sim$55km $\times$ 55km) that are assigned pixel-specific levels of water availability. However, an important impact of increasing the resolution of the civil war data is that conflicts become extraordinarily rare events in the data, with the unconditional probability that any location exhibits conflict being $\frac{59}{363{,}811} = 0.00016 = 0.016\%$. Because at most only a fractional component of these conflicts could be attributed to climatic forcing, detecting this influence is difficult or impossible under reasonable assumptions.

A.2 Details on select excluded papers

Some quantitative papers in this literature did not meet our criteria. A subset of these present the same data and same fundamental analysis as other included papers, as indicated below.

- Adano et al (J. Peace Res., 2012). We did not include this study because it presented largely qualitative analysis and summary statistics. Nonetheless, we note that the authors appear to show a positive correlation between high precipitation years and cattle raids in Kenya.

- Benjaminsen, Tor, Alinon, Buhaug and Buseth (J. Peace Res., 2012). This article presents quantitative data but states that it is a qualitative study and not a quantitative study, so it did not fall within the scope of our review.
- Dell 2012 ("Insurgency and long-run development: Lessons from the Mexican Revolution", working paper). We did not include this paper because it does not meet our standards for including location-specific fixed effects. Dell shows that locations in Mexico that experienced severe drought just prior to the Mexican Revolution were substantially more likely to participate in the ensuing Revolution. While her drought measure is constructed from local deviations from location-specific means, she cannot similarly demean the dependent variable because there is only one observation per locality (demeaning both the independent and dependent variables is equivalent to including locality-specific fixed effects).
- Gartzke (J. Peace Res., 2012). This study was not included because the area-weighted global average temperature variables are dominated by uninhabited regions, such the oceans and polar regions, and thus do not reflect the conditions of countries in the sample.
- Koubi, Bernauer, Kalbhenn, and Spilker (J. Peace Res., 2012). The authors do not estimate the reduced form relationship between climate and conflict, but the reduced form that they would have estimated is redundant with Dell, Jones, and Olken (*21*) for the global sample, and Burke et al (*64*) for the African sample. For this reason the study is not included.
- Nel and Righarts (International Studies Quarterly, 2008). This paper finds that natural disasters significantly increase the risk of armed conflict, but the paper does not use location-specific fixed effects and appears to use disaster and conflict data identical to that of Bergholt and Lujala (which we include). For that reason, we do not include this paper.
- Raleigh and Kniveton (J. Peace Res. 2012). This paper was not included because it did not meet our standards for including location-specific controls. It is not reanalyzed because it presents the same data as O'Loughlin et al. (*23*) and Harari and

La Ferrara 2013 (*52*), which were analyzed according to our methodological criteria and are included in the analysis.

- Sletteback (J. Peace Res., 2012). This paper was not included because it did not meet our standards for including location-specific controls. It was not reanalyzed because it presents the same data as Bergholt and Lujala (2012), which was analyzed according to our methodological criteria.

B Evaluating and combining effect sizes

The goal of our analysis is to identify studies that estimate the causal relationship of climate on some conflict outcome, and to compare the direction and magnitude of the estimated effects across studies. Where possible, this latter goal is accomplished by calculating standardized effects for each study. Given an overarching assumption that all of the studies are comparable in some general sense, we can summarize the distribution of effect sizes in a number of ways.

A simple but transparent first approach, shown in Fig. 4-5, computes the median across studies and shows that most of the confidence intervals of each individual study overlaps this value. A benefit of this approach is that it is robust to outliers. A downside to this approach is that it ignores much of the available information provided by estimates other than the median estimate. Being cognizant of both this benefit and cost, we tabulate median values for all estimates and for temperature-only estimates in Table S1.

A second approach, also shown in Fig. 4-5, assumes that while not all the studies are measuring the same specific outcome, the standardized effects we calculate could describe a generic phenomenon that is common across samples. Under this assumption, a common approach to summarizing the phenomenon is to compute the weighted average across estimates, using inverse-variance weights (*91*). For a set of M estimates indexed by j, each with estimated treatment effect $\hat{\beta}_j$ and associated standard error $\hat{\sigma}_j$, a weighted estimate of the mean effect across studies is

$$\tilde{\beta} = \sum_{j=1}^{M} \omega_j \hat{\beta}_j \tag{8}$$

where ω_j is the weight for study j, and $\sum \omega_j = 1$. The estimated variance of $\tilde{\beta}$ is

$$Var(\tilde{\beta}) = \sum_{i=1}^{M} \sum_{j=1}^{M} \left[\omega_i \omega_j Cov(\hat{\beta}_i, \hat{\beta}_j) \right] \tag{9}$$

If the studies are independent, then $Cov(\hat{\beta}_i, \hat{\beta}_j) = 0$ for all $i \neq j$ and

$$Var(\tilde{\beta}) = \sum_{j=1}^{M} \omega_j^2 \hat{\sigma}_j{}^2 \tag{10}$$

Many of our estimates are likely independent – eg. violence in United States baseball games is unlikely to be correlated with Hindu-Muslim riots in India. However, other studies use common datasets for either climate or conflict variables and/or overlap in their sample, suggesting their estimated effects might indeed be correlated. Because we have no practical way to measure the cross-study correlation in parameter estimates, we begin by simply assuming it is zero (allowing us to use Eq. 10) and later relax this assumption.

Because of the Central Limit Theorem, it is natural to assume that our $\hat{\beta}_j$'s are each normally distributed. Under this assumption, the optimal[2] weighting scheme is

$$\omega_j = \frac{\frac{1}{\hat{\sigma}_j^2}}{\sum_{j=1}^{M} \frac{1}{\hat{\sigma}_j^2}} \tag{11}$$

where the weight assigned to each estimate is proportional to the inverse of its estimated variance. Because the inverse of variance is termed "precision", this approach is called "precision-weighting" both in the literature and our main text. The term in the denominator is simply the sum of all inverse variances, which is necessary to ensure that the sum of weights equals one. We use the weights from Equation 11 applied to Equations 8 and 10 to estimate the precision-weighted mean and confidence interval shown in Fig. 4-5 of the main text. These values are also tabulated in Table S1.

In a third approach, we expand on the precision-weighting approach to more fully characterize the distribution of effects a study might obtain, rather than simply focusing

[2]This approach is considered optimal because it minimizes $Var(\tilde{\beta})$.

Supplementary Table S1: **Summary statistics for the distribution of effects across studies**

	Median	Mean*	SE**	Percentiles of $\tilde{B}_\beta$						
				2.5%	5%	25%	50%	75%	95%	97.5%
Intergroup	13.56	11.12	1.34	-7.80	-4.60	5.80	10.20	14.30	32.00	40.10
Intergroup (Temp.)	23.96	13.21	1.95	-4.40	-1.30	5.60	9.70	16.60	40.10	46.00
Interpersonal	3.89	2.29	0.12	1.10	1.20	1.50	2.20	2.60	4.00	4.20
Interpersonal (Temp)	2.48	2.26	0.12	1.10	1.20	1.50	2.20	2.60	3.90	4.20

$*\tilde{\beta}$, $**Var(\tilde{\beta})$

on only the precision-weighted mean estimate $\tilde{\beta}$. Defining $N_\beta(m, s)$ to be a normally distributed probability density function over values of β centered on m with standard deviation s, we construct an estimate for the probability distribution $\tilde{B}_\beta$ that describes the probability of obtaining an estimate $\hat{\beta}$ unconditional on the study sample:

$$\tilde{B}_\beta = \sum_{j=1}^{M} \omega_j N_\beta(\hat{\beta}_j, \hat{\sigma}_j) \tag{12}$$

where we continue to use the weights from Equation 10 so that $\mathrm{E}[\tilde{B}_\beta] = \tilde{\beta}$ for consistency. Our estimates of $\tilde{B}_\beta$ for all studies and temperature-only studies are shown in Fig. 4-5 of the main text (solid lines). Percentiles from these four distributions are also tabulated in Table S1.

As discussed above, the estimates of $\hat{\beta}$ are unlikely to be independent across all studies ($Cov(\hat{\beta}_i, \hat{\beta}_j) \neq 0$ for all $i \neq j$). We cannot estimate all of these covariances, but we think it is both reasonable and conservative to assume that they are likely to be weakly positive in our setting – as they are probably an increasing function of the spatial and temporal overlap in a given set of studies. To develop a sense of whether our assumption of "no cross-study correlation" is generating a false sense of statistical significance in our precision-weighted average effects, we assume that *all* cross-study covariances take on arbitrary positive values and ask how this alters our estimates of $Var(\tilde{\beta})$ using Equation 9. Observing that the population of cross-study estimates have correlation $\rho_{ij} = Cov(\hat{\beta}_i, \hat{\beta}_j)/(\sigma_i \sigma_j)$, we assume $\rho_{ij} \in \{0.1, 0.3, 0.5, 0.7\}$ and then estimate

$$Cov(\hat{\beta}_i, \hat{\beta}_j) = \rho_{ij} \hat{\sigma}_i \hat{\sigma}_j \tag{13}$$

which are then used in Equation 9. Using these four values for ρ_{ij}, we obtain estimates for $\sqrt{Var(\tilde{\beta})}$ of 2.1, 3.1, 3.8, and 4.5, respectively, for the studies of intergroup conflict (our least-precise result). Since the estimated mean effect is 11.1 for this set of studies, we infer that this result would still be statistically significant even if $\rho_{ij} = 0.7$ for all pairs of studies. Since it is very likely that ρ_{ij} is much lower for all pairs of studies, this shows that our central conclusion about the general relationship between climate an conflict is not dependent on our assumption of cross-study correlations.

B.1 Estimating the distribution of effect sizes with a Bayesian hierarchical model

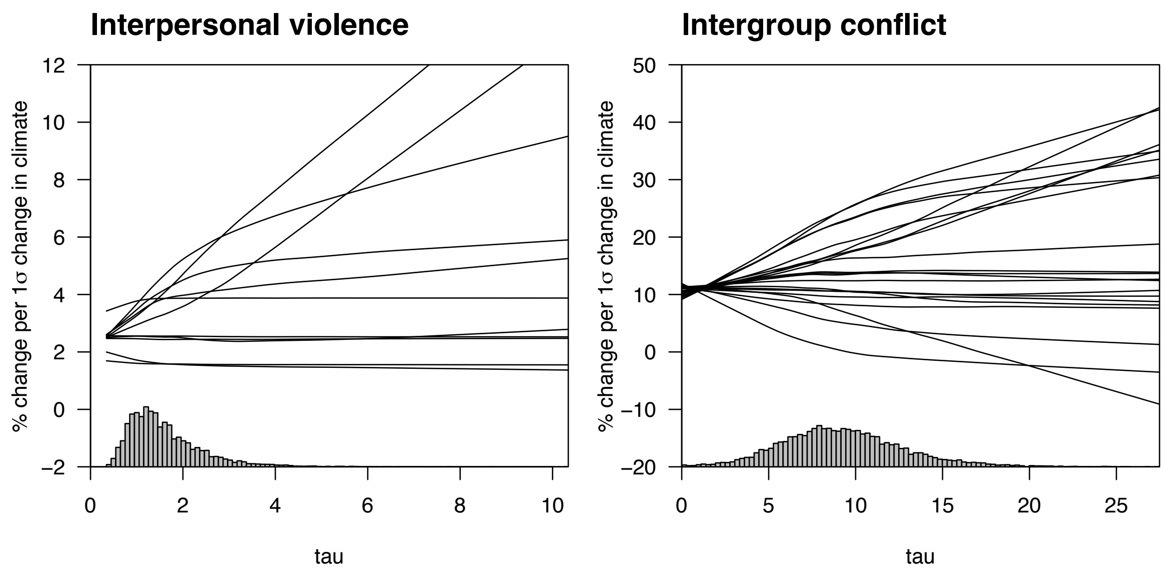
It is likely the case that differences among estimated effect sizes are not due to sampling variability alone. That is, studies looking at the effect of climate on different outcomes might be expected to share some similarities (different outcomes might be related), but also some important differences (some outcomes or samples might exhibit different responses to climate). The precision-weighting averages above are no longer optimal under these conditions since the sampling variability is not strictly normal (although these results remain useful), and the precision-weighted distribution $\tilde{B}_\beta$ – while it does not make any assumptions about cross-study sampling variability – does not use all the available information.

We use a fourth approach, the Bayesian hierarchical normal model, which provides a way to model the distribution of effect sizes while simultaneously allowing for similarities and differences across studies. This approach utilizes the available information more efficiently than our estimate of $\tilde{B}_\beta$ and provides more insight into the structure of between-study variation, although it is noteworthy that the unconditional posterior distribution for β ends up being similar in this setting. Our implementation closely follows Gelman et al (*92*), and readers are referred to that volume (particularly Chapter 5) for a more comprehensive treatment.

As before, consider a set of M independent studies, each estimating a treatment effect β_j. Denote each study's estimate of that treatment effect $\hat{\beta}_j$, with standard error $\hat{\sigma}_j$. Further assume that these treatment effects β_j are drawn from a normal distribution with unknown mean μ and standard deviation τ. In this setting, it could be the case that β_j is the same across studies and observed variation in the $\hat{\beta}_j$'s results from sampling error alone (implying $\tau = 0$ and $\beta_j = \mu$ for all j), or it could be the case that the "true" effect in each study is different (meaning $\tau > 0$). Because τ is unknown, the goal of this approach is to compute a distribution of τ's that are consistent with the observed $\hat{\beta}_j$'s and $\hat{\sigma}_j$'s, and then use these estimates to simulate the distribution of each β_j.

Intuitively, if estimated treatment effects in all studies are very near one another and have relatively wide and overlapping confidence intervals, then most simulated values of

τ are likely to be close to zero. Alternatively, if there is large variation in the estimated effects but each effect is estimated precisely, then τ will $\gg 0$ and "true" treatment effects likely differ across studies. Casual observation of Fig. 5 perhaps suggests a world somewhere in between: substantially overlapping confidence intervals, but also substantial variation in the estimated effects with some confidence intervals that do not overlap.



Supplementary Figure S3: Conditional posterior means of treatment effects for the 11 interpersonal violence studies (left panel) and the 21 intergroup conflict studies (right panel), as a function of τ, the estimated between-study standard deviation. The histograms at bottom shows the distributions of the estimated τ.

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 seperately (*92*). We then use 10,000 simulations to characterize the posterior distributions of these variables and present the results in Fig. S3 and Tables S2-S3. In Fig. S3, each line in the two panels represents the estimated effect size in a single study, conditional on the between-study standard deviation τ. The estimated distribution of τ across the 10,000 simulations is shown at the bottom of each panel. For both the intergroup conflict studies and individual conflict studies, simulated values of τ are distributed away from zero, meaning that

Supplementary Table S2: Posterior quantiles of treatment effects for the 10 studies on interpersonal violence, based on 10,000 simulation draws from a Bayesian hierarchical model. The $\hat{\beta}_j$ and $\hat{\sigma}_j$ columns represent our original estimated effect and standard error from each study. The last three rows give the posterior distributions for the population parameters μ and τ, as well as the "predicted effect" $\hat{\beta}_j^*$ (the predicted outcome of a new study).

					Posterior distribution				
#	Study	year	$\hat{\beta}_j$	$\hat{\sigma}_j$	2.5%	25%	median	75%	97.5%
1	Ranson	2012	1.47	0.45	0.77	1.33	1.64	1.93	2.50
2	Jacob Lefgren Moretti	2007	1.55	0.20	1.20	1.46	1.59	1.72	1.98
3	Card and Dahl	2011	2.18	1.17	0.66	1.90	2.52	3.11	4.34
4	Ranson	2012	2.42	0.30	1.87	2.25	2.45	2.64	3.00
5	Jacob Lefgren Moretti	2007	2.54	0.23	2.12	2.40	2.55	2.70	2.99
6	Ranson	2012	3.89	0.35	3.13	3.59	3.82	4.06	4.52
7	Larrick et al	2011	4.32	1.42	1.69	2.93	3.63	4.40	5.99
8	Sekhri and Storeygard	2012	5.43	1.74	1.74	3.09	3.90	4.87	6.93
9	Sekhri and Storeygard	2012	7.51	2.17	1.90	3.36	4.33	5.53	8.19
10	Auliciems and DiBartolo	1995	16.28	5.74	1.01	2.77	3.79	5.16	9.47
11	Miguel	2005	21.45	14.45	0.01	2.20	3.16	4.34	8.23
	Mean, μ				1.94	2.64	3.02	3.49	4.84
	Standard deviation, τ				0.62	1.05	1.43	1.98	3.77
	Predicted effect, $\hat{\beta}_j^*$				1.05	2.08	2.78	3.91	6.84

the estimated treatment effects are very unlikely to describe a single underlying value and suggesting that there are likely important differences across studies. Given the range of outcomes, geographies, and time periods that these studies cover, this is not surprising. Nonetheless, the component of the effects β that is common across studies (μ) tends to be substantial (median=3.0%/σ for interpersonal violence, median=13.8%/σ for intergroup conflict) and larger than the cross-study standard deviation τ (median=1.4%/σ for interpersonal violence, median=9.0%/σ for intergroup conflict). Thus, while there is strong evidence of important differences between studies, there is simultaneously strong evidence that there is also something in common between these studies. There remains considerable heterogeneity in the response to climate across studies that should be recognized, and understanding the sources of this variation could be a fruitful subject for future inquiry.

An additional insight provided by this approach is that it allows us to formally consider the plausibility of point estimates reported by individual studies, given what we learn from all the other studies. Our simulations estimate the distribution of individual β_j's, given the individual estimates of $\hat{\beta}_j$, $\hat{\sigma}_j$ and the distributions of μ and τ, which encapsulate information about the full collection of results. In cases where the uncertainty in estimates is large, our simulations rely more heavily on information about the group of studies (μ), since the study-specific information ($\hat{\beta}_j$) is less likely to be reliable[3]. We describe these posterior distributions for each study in Tables S2-S3. In situations where the estimated effect $\hat{\beta}_j$ is far out in the tail of the posterior distribution for that parameter, the immediate implication is that either those point estimates are unlikely to be accurate or that there is something unique about that study that sets it apart from the rest of the literature for some substantive, albeit not yet well understood, reason.

C Publication bias

Following Card and Krueger (*93*) and Disdier and Head (*140*), standard sampling theory suggests that the t-stat on a coefficient estimate should be proportional to the degrees of freedom in the study. In particular, with the null hypothesis $H_0 = 0$, independent variable

[3]This effect results in "shrinkage" in the posterior distribution of β_j's towards the group mean (*92*).

Supplementary Table S3: Posterior quantiles of treatment effects for the 21 studies on intergroup conflict, based on 10,000 simulation draws from a Bayesian hierarchical model. The $\hat{\beta}_j$ and $\hat{\sigma}_j$ columns represent our original estimated effect and standard error from each study. The last three rows give the posterior distributions for the population parameters μ and τ, as well as the "predicted effect" $\hat{\beta}_j^*$ (the predicted outcome of a new study).

					Posterior distribution				
#	Study	year	$\hat{\beta}_j$	$\hat{\sigma}_j$	2.5%	25%	median	75%	97.5%
1	Theisen Holtermann Buhaug	2011	-12.54	15.53	-11.92	1.61	7.72	12.65	22.69
2	Bergholt and Lujala	2012	-3.68	4.83	-8.62	-2.35	0.96	4.30	10.36
3	Buhaug	2010	1.09	6.17	-5.59	1.97	5.68	9.26	15.47
4	Dell Jones Olken	2012	6.21	10.44	-3.72	6.19	10.76	15.17	24.68
5	Burke	2012	7.20	3.82	1.44	6.06	8.45	10.86	15.25
6	Harari and La Ferrara	2011	8.91	3.80	2.95	7.49	9.83	12.06	16.34
7	Fjelde and von Uexkull	2012	9.41	5.49	1.34	7.54	10.63	13.66	19.94
8	Miguel Satyanath Sergenti	2004	9.74	4.14	3.33	8.07	10.60	13.03	17.75
9	Hidalgo et al	2010	12.29	2.96	7.02	10.49	12.36	14.19	17.83
10	Levy et al	2005	13.33	8.11	1.68	9.46	13.27	17.28	25.37
11	Burke et al	2009	13.56	6.68	3.03	9.98	13.34	17.02	24.31
12	Hendrix and Salehyan	2012	13.94	6.12	4.10	10.36	13.61	17.06	23.90
13	Hsiang Meng Cane	2011	17.11	6.92	4.94	11.82	15.56	19.41	27.22
14	Burke and Leigh	2010	30.81	14.33	3.80	12.76	18.04	23.83	37.51
15	Couttenier and Soubeyran	2012	31.34	9.31	8.77	16.53	21.59	27.04	38.16
16	O'Laughlin et al	2012	31.63	9.29	9.21	16.68	21.80	27.07	38.06
17	Maystadt Ecker Mabiso	2013	35.84	9.43	10.03	18.18	23.59	29.00	40.41
18	Dell Jones Olken	2012	42.19	12.18	8.74	17.16	23.17	29.89	43.15
19	Bohlken and Sergenti	2011	50.37	30.74	-0.27	10.67	16.30	23.08	39.87
20	Theisen	2012	75.91	36.90	-0.36	11.08	16.85	23.79	42.49
21	Bruckner and Ciccone	2010	95.38	50.62	-1.75	10.39	15.83	22.56	40.54
	Mean, μ				8.74	11.97	13.82	16.01	20.93
	Standard deviation, τ				2.96	6.83	9.00	11.32	17.17
	Predicted effect, $\hat{\beta}_j^*$				-2.70	8.38	12.95	19.04	34.98

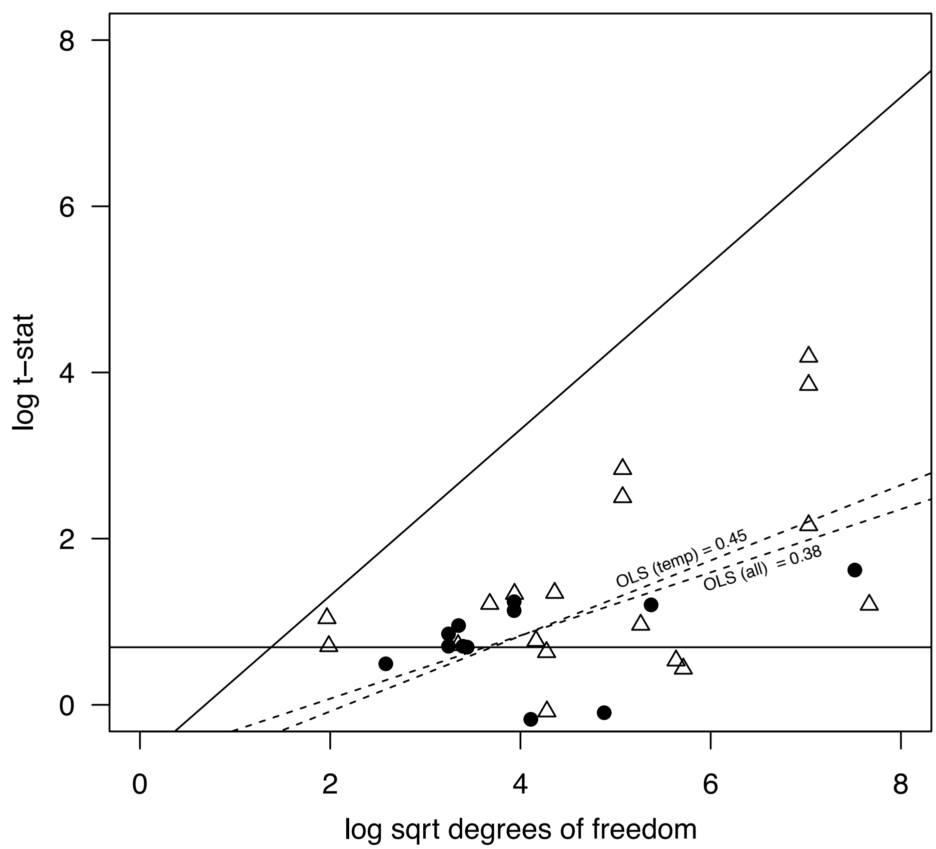
X, errors e, $n-k$ degrees of freedom, and again indexing studies by j:

$$t(\hat{\beta}_j) = \frac{\hat{\beta}_j}{\hat{\sigma}_j} = \frac{\hat{\beta}_j}{\left(\frac{\sqrt{e_j' e_j}}{\sqrt{n_j - k_j}\sqrt{X_j' X_j}}\right)} = \sqrt{n_j - k_j} \times \left(\frac{\hat{\beta}_j \sqrt{X_j' X_j}}{\sqrt{e_j' e_j}}\right) \tag{14}$$

Taking logs, we see that there should be unit elasticity between the log of the t-stat 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 (*93, 140*). If there is a true relationship between climate and human conflict, than 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 allows them to reject H_0 using standard tests, 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(t-stat) should not rise with log($\sqrt{n-k}$). For example, in Card and Krueger, the authors found a *negative* relationship between t-stats and degrees of freedom, which they interpreted as strong evidence of publication bias.

Fig. S4 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 32 studies for which we are able to calculate standardized effects (we use author-reported statistics here because those are the values that authors, editors and reviewers would consider at the time of release/publication). The unit elasticity is given by the 45-degree line, and OLS estimates of the relationship for the full sample and for the temperature-focused studies are given by the dashed lines. We strongly reject a slope of zero (see Columns 1-2 in Table S4). And although we can reject a 1-to-1 relationship, studies with larger sample sizes 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 marginally significant effects. We note that for both samples, the upward relationship stands in sharp contrast to the results in Card and Krueger, with the negative slope they estimate. Our estimates are more similar to that of Disdier and Head (2009), who interpret their results as ruling out any large role for publication bias in the trade literature that they survey.

Card et al (*141*) argue that there is an important reason why the slope coefficient that we focus on could be less than one even in the absence of publication bias, namely, if stud-



Supplementary Figure S4: Relationship between log of t-stat and log of the square root of the degrees-of-freedom, using author reported t-statistics. Circles represent studies focusing on rainfall, triangles studies focusing on temperature.

Supplementary Table S4: The relationship between log t-stat and log square root of the degrees-of-freedom, using author-reported t-statistics. Column 1 is the full sample, Column 2 the studies that focus on temperature. The bottom row is the p-value on the test that the d.o.f. coefficient is unity.

	(1) Full	(2) Temp
log sqrt deg. of. freedom	0.380***	0.454**
	(0.123)	(0.180)
Constant	-0.686	-0.987
	(0.585)	(0.907)
Observations	32	20
R squared	0.242	0.260
P-val elasticity = 1	0.000	0.007

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

ies with larger samples also employ more complex research designs, altering the study's "design effect". To the extent that this is true, we would expect a slope coefficient less than one, but it ultimately becomes difficult to quantify the precise extent of publication bias in this case. Card et al (*141*) cannot reject a zero slope for the vast labor economics literature they survey on the impacts of active labor market policies, but they conclude that there is unlikely to be considerable publication bias in that setting due to their point on research design complexity.

Nevertheless, to help prevent publication biases from developing in this field, we briefly outline the evidence needed to establish a compelling null result in a specific context – so that researchers, editors and reviewers may be confident in a strong null finding should they encounter one. To demonstrate that climatic variables have no effect on human conflict, studies must (i) account for unobservable differences between populations (*9, 12*) and trends in conflict (*26*); (ii) document that both linear models and non-linear models exhibit no association; (iii) control for all relevant climatological covariates and their lags simultaneously (*126*); (iv) stratify their sample using baseline climate conditions; (v) avoid controlling for socio-economic covariates that are also influenced by climatic variables (*12*); (vi) demonstrate that their confidence interval excludes substantial effect sizes (*26*) reported elsewhere, such as our median estimates presented here; (vii) use simple simulations to demonstrate that their statistical tests are adequately powered.

D Projected changes in temperature

In Fig. 6 we plot expected warming by 2050, calculated as the mean projected warming at a given location divided by the historical standard deviation of annual temperature at that location. The projected warming at each location is the mean at that location computed across the 21 available global climate models running the A1B emissions scenario. The historical standard deviation of temperature at each location is calculated using half-degree gridded weather data from the University of Delaware dataset. Almost all inhabited locations warm by $> 2\sigma$, with the largest increases exceeding 4σ in tropical regions that are already warm and currently experience very low inter-annual temperature variability.

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