Climatic Change
DOI 10.1007/s10584-014-1276-z

RESPONSE

Reconciling climate-conflict meta-analyses: reply to Buhaug et al.

Solomon M. Hsiang · Marshall Burke · Edward Miguel

Received: 17 July 2014 / Accepted: 28 September 2014

Abstract A comment by Buhaug et al. attributes disagreement between our recent analyses and their review articles to biased decisions in our meta-analysis and a difference of opinion regarding statistical approaches. The claim is false. Buhaug et al.'s alteration of our meta-analysis misrepresents findings in the literature, makes statistical errors, misclassifies multiple studies, makes coding errors, and suppresses the display of results that are consistent with our original analysis. We correct these mistakes and obtain findings in line with our original results, even when we use the study selection criteria proposed by Buhaug et al. We conclude that there is no evidence in the data supporting the claims raised in Buhaug et al.

Buhaug et al. (2014) argue that conclusions presented in our review article (Hsiang and Burke, *Climatic Change*, 2014) and our reanalysis/meta-analysis of existing studies (Hsiang et al., *Science*, 2013a; hereafter HBM) depart from conclusions in other recent reviews because of errors in our meta-analysis. As detailed in both articles, the difference between HBM's conclusions and earlier findings arises because other reviews omitted many existing studies and did not systematically interpret statistical uncertainty in the included studies, and HBM's analysis had more stringent criteria for the research design and methodological rigor of included studies.

Buhaug et al. suggest three reasons why HBM's analysis is flawed and offer suggestive support for each claim. Here we examine the criticisms in turn and demonstrate that each concern arises from Buhaug et al.'s misrepresentation of HBM's analysis, misrepresentation of prior studies in the literature, and statistical or coding errors in their alteration of the original meta-analysis.

Buhaug et al.'s first criticism is that some studies in the analysis rely on similar data sets, causing the results of these related studies to potentially be correlated. If true, this would cause

This reply refers to the comment available at doi:10.1007/s10584-014-1266-1.

S. M. Hsiang (✉) · E. Miguel
University of California, Berkeley, Berkeley, CA 94720, USA
e-mail: shsiang@berkeley.edu

S. M. Hsiang · E. Miguel
National Bureau of Economic Research, Cambridge, MA 02138, USA

M. Burke
Stanford University, Stanford, CA 94305-6055, USA

Published online: 28 October 2014

the statistical uncertainty in HBM's result to be understated, which could theoretically cause HBM's highly statistically significant finding to be rendered insignificant. The more highly correlated data are across more studies, the larger this concern. Buhaug et al. offer one example to support their concern, a single case where data in two studies is correlated with $r=0.6$. But in direct contrast to Buhaug et al.'s claim that HBM ignored this issue, HBM explain it in detail and systematically test its influence on their result in Section B of HBM's supplement, stating explicitly that "[T]he estimates of β are unlikely to be independent across all studies…" HBM find that even if they assume $r=0.7$ for *all* pairs of studies, a much more hazardous environment for HBM's analysis than Buhaug et al.'s example of a single pair of correlated studies, HBM's meta-analysis result remains statistically significant with 95 % confidence. Buhaug et al.'s first criticism was addressed in HBM's original article.

Buhaug et al.'s second criticism is to assert that HBM assume "causal homogeneity", i.e., that all studies recover the same causal effect, and that this assumption "is essential for [HBM's] meta-analysis to be meaningful." Both assertions are false. The meta-analytic technique used in HBM explicitly assumes that effects across studies are not the same even within a given class of conflict. The Bayesian random effects approach in HBM, based on Gelman et al. (2004), allows different types of intergroup conflict in different regions to respond differently to climate variables. A central strength of HBM's meta-analysis is to model these potential differences while simultaneously examining whether estimates across multiple studies share any common component. HBM assume neither causal homogeneity nor complete causal heterogeneity, instead allowing for any arbitrary mixture of the two and "letting the data speak". HBM explain this approach in detail and extensively quantify and discuss the extent of heterogeneity across studies in Section B of HBM's supplement and report in the main text, "we recover estimates for the between-study s.d. (a measure of the underlying dispersion of true effect sizes across studies) that are… two-thirds of the precision-weighted mean for intergroup conflict…. By comparison, if variation in effect sizes across studies was driven by sampling variation alone [i.e., the assumption of causal homogeneity were true], then this s.d. in the underlying distribution of effect sizes would be zero. This finding suggests that true effects probably differ across settings, and understanding this heterogeneity should be a primary goal of future research." In direct contradiction to Buhaug et al.'s second claim, HBM do not assume causal homogeneity and instead discuss cross-study differences and formally characterize them.

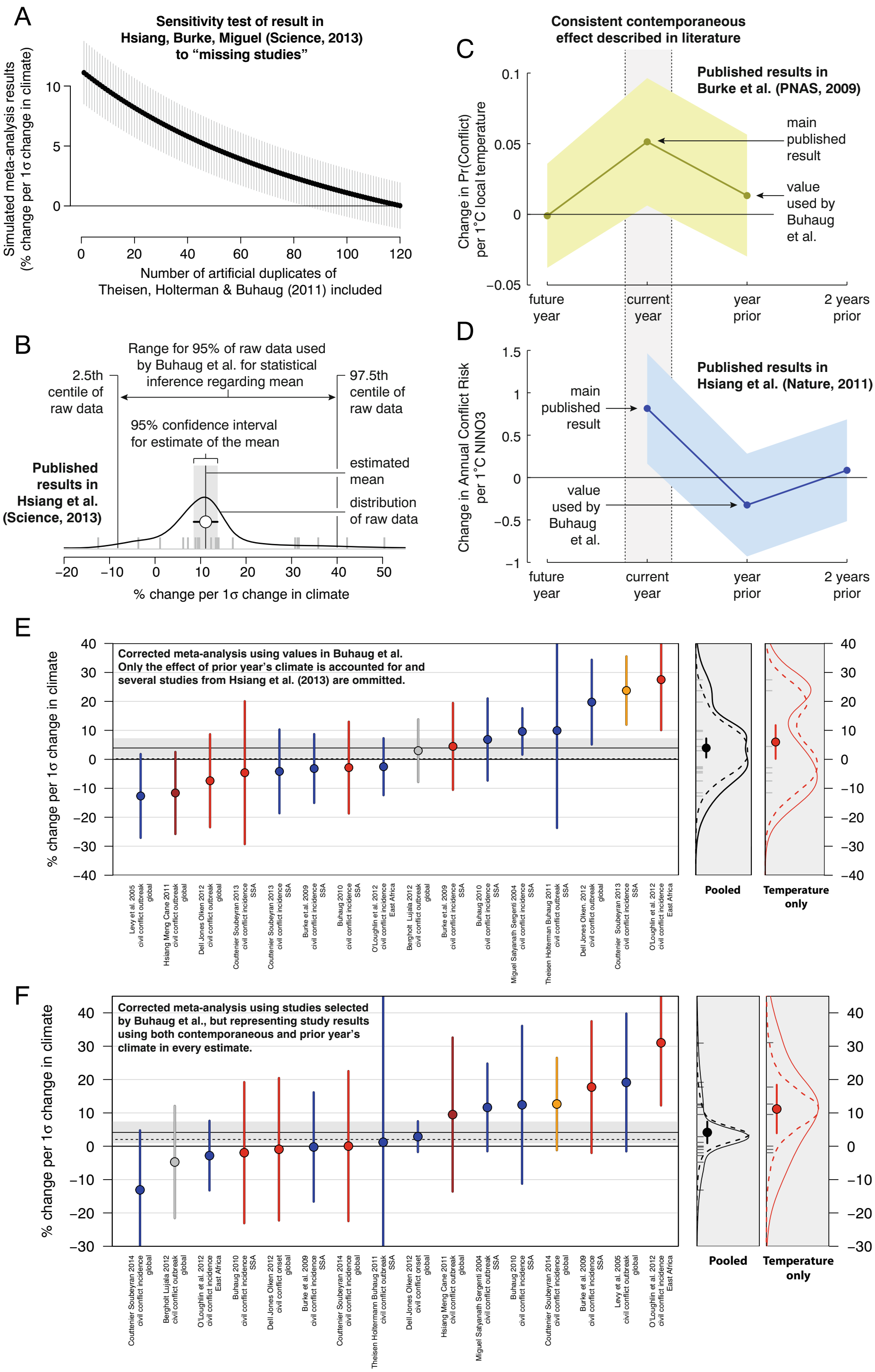
Buhaug et al.'s third criticism is that HBM's results are not "balanced" because HBM use "selection criteria that explicitly disregard studies that revisit previously investigated climate-conflict associations." This is an erroneous representation of HBM's selection method and does not accurately describe how revisited data sets were handled. Exact replications were omitted to avoid double counting but studies that revisited prior relationships were included in the review and were used to interpret findings in the prior study (see footnotes of Table 1 and Section A of the supplement in HBM). For example, follow-up analysis of Bushman et al. (2005) was used to adjust how HBM interpreted the original study by Cohn and Rotton (1997), and Buhaug (2010) which followed-up Burke et al. (2009) is fully included in the meta-analysis and discussed extensively. Buhaug et al. also criticize HBM for analyzing climate variables that are the main result of each original study, an odd criticism given that the role of meta-analysis is to summarize the main results of a collection of studies. Buhaug et al. take a different approach when altering HBM's meta-analysis by focusing exclusively on results that researchers do not claim are important, discussed in detail below.

To rigorously assess Buhaug et al.'s central claim that HBM's result is driven by systematically biased study selection, we implement a "stress test" where we suppose that there actually are numerous "missing" results that HBM inappropriately omitted, each of which obtained results identical to the most "negative" result presented in HBM, Theisen et al. (2011) (henceforth THB)—negative in the sense that the estimated effect differs most in magnitude from the meta-analytic mean reported in HBM. We replicate HBM's analysis many times, each time adding in an additional artificial duplicate of the THB result as a surrogate for an unknown missing study and examining how this alters the result of HBM (Fig. 1a). If HBM had missed five studies that replicated results in THB, the "corrected" findings that added in these "missing" studies to the analysis would remain essentially unchanged from what is reported in HBM (10.4±1.3 %/σ, P<0.001). If instead we assume that HBM had omitted a staggering 20 such studies (i.e., excluding half of the literature, since the original result had N= 21 studies) then the result of HBM would change slightly (8.2±1.3 %/σ) but would remain large, positive, and highly statistically significant (P<0.001). Repeating this exercise for even higher omission rates, we find that HBM would need to have omitted over 80 studies similar to THB in order to conclude that the literature is "inconclusive"—i.e. to no longer be able to reject the hypothesis of no relationship between climate and conflict. Put another way, in order for the results of HBM to be driven by study selection bias alone, HBM would have to be so biased that they selectively omitted roughly four out of five studies in the literature, if the typical omitted study had findings similar to THB, the most negative estimate of all existing studies. Buhaug et al.'s claim that study selection bias drives HBM's result thus seems implausible.

Buhaug et al. conclude by altering the meta-analysis for intergroup conflict presented in HBM, using selection criteria that Buhaug et al. claim simultaneously "streamline the sample" and "obtain a more representative sample." Buhaug et al. argue that their adjustments render the results of HBM not statistically significant, thus demonstrating the erroneous nature of HBM's overall approach. The analysis by Buhaug et al. builds directly on HBM's publicly available data and analysis replication code, and we have carefully examined Buhaug et al.'s analysis to understand how it differs from the original. Buhaug et al. make statistical errors in their alteration, and they misrepresent results from the literature they employ so that their results are incomparable to results in HBM. Buhaug et al. also incorrectly classify multiple studies, make computer coding errors, and modify HBM's original coding in such a way that it prevents their Fig. 1 from displaying results that are consistent with HBM's conclusions. We detail these errors and misrepresentations below, and provide a corrected version of Buhaug et al.'s analysis, which notably does not contradict the findings in HBM. We then redo Buhaug et al.'s analysis in a way that more accurately addresses concerns about the lag structure in the existing literature and find that it remains consistent with HBM.

Key errors in Buhaug et al.'s altered meta-analysis:

1) Buhaug et al. incorrectly use the *range* of raw data as a measure of uncertainty for the estimated mean of the data (Fig. 1b). The range of values between the 2.5th centile and the 97.5th centile of raw data are population-level statistics that do *not* change in expected value when more (or less) data is used and they have no mathematical relation to the 95 % confidence interval for the estimated mean of the distribution (used by HBM for hypothesis testing), which will shrink substantially when more data is collected. Buhaug et al. suggest that this centile range, which they coin the "95 % highest density interval," should be used to determine whether an estimate of the mean is statistically different from zero. This approach is objectively wrong and runs counter to basic statistical theory (Neyman 1937). Even if the true mean effect were large and different from zero and an analyst had infinite data, Buhaug et al.'s approach would lead them to conclude that there was no relationship. For clarity, consider a simpler example: imagine a sample of 100 individuals with ages uniformly distributed between 1 and 100 years old. The estimated mean age of


A
Sensitivity test of result in
Hsiang, Burke, Miguel (Science, 2013)
to "missing studies"
Simulated meta-analysis results
(% change per 1σ change in climate)
Number of artificial duplicates of
Theisen, Holterman & Buhaug (2011) included
B
Range for 95% of raw data used
by Buhaug et al. for statistical
inference regarding mean
2.5th
centile of
raw data
97.5th
centile of
raw data
95% confidence interval
for estimate of the mean
estimated
mean
distribution
of raw data
Published
results in
Hsiang et al.
(Science, 2013)
% change per 1σ change in climate
C
Consistent contemporaneous
effect described in literature
Published results in
Burke et al. (PNAS, 2009)
main
published
result
value
used by
Buhaug
et al.
Change in Pr(Conflict)
per 1°C local temperature
future
year
current
year
year
prior
2 years
prior
D
Published results in
Hsiang et al. (Nature, 2011)
main
published
result
value
used by
Buhaug
et al.
Change in Annual Conflict Risk
per 1°C NINO3
future
year
current
year
year
prior
2 years
prior
E
Corrected meta-analysis using values in Buhaug et al.
Only the effect of prior year's climate is accounted for and
several studies from Hsiang et al. (2013) are ommitted.
% change per 1σ change in climate
Pooled
Temperature
only
F
Corrected meta-analysis using studies selected
by Buhaug et al., but representing study results
using both contemporaneous and prior year's
climate in every estimate.
% change per 1σ change in climate
Pooled
Temperature
only

◀ **Fig. 1** **a** Stress test of HBM's result where artificial duplicates of Theisen et al. (2011) are included in meta-analysis. Point estimate and confidence interval are for the average effect of climate on intergroup conflict. **b** The distribution of raw data in HBM is different from the confidence interval for the estimated mean of the data. **c** The estimated effect of future, current, and lagged temperature on conflict from Burke et al. (2009), point estimates and 95 % confidence intervals. **d** The estimated effect of current and two lag years of NINO3 on annual conflict risk from Hsiang et al. (2011), point estimates and 95 % confidence intervals. **e** Corrected version of Fig. 1 from Buhaug et al. (2014), see text for details. **f** Updating HBM's results using study and variable selection from Buhaug et al. (2014), but using the cumulative effect of current and lagged climate variables in every study to be consistent with literature findings

the sample would be 50.5 years old (95 % confidence interval: 44.7–56.3 years old). Buhaug et al.'s approach would suggest that the mean age could be 4 or 96 years old, since the 2.5–97.5 centiles of the data span these values. The grey stripe shown in Buhaug et al.'s Fig. 1 visually mimics HBM's confidence interval, but the values shown in Buhaug et al. are not a confidence interval and cannot be used for inference as Buhaug et al. use them. The actual confidence interval is far narrower (see below).

2) The results shown in Fig. 1 of Buhaug et al. are not comparable to results presented in HBM because Buhaug et al. alter the code of HBM to analyze only the *lagged* effect of climate on conflict (i.e., the effect of the prior year's climate on conflict in most cases) rather than on the contemporaneous effect of climate on conflict, as is the focus of both most of the existing literature and HBM's analysis. For example, Fig. 1c–d display two published studies in the literature that found large, statistically significant effects of current climate on conflict. Buhaug et al. omit these contemporaneous effects and instead present results using only effects of the prior year, which neither publication claimed was important. Buhaug et al. state that they ignore the main findings from the literature to "ensure analytical consistency," but then directly compare their results for the lagged effect of climate with HBM's results for the current effect of climate, which is analytically inconsistent. Buhaug et al.'s presentation of lagged climate effects as if they are the focus of the literature, or comparable to HBM's result, is a misrepresentation. Buhaug et al. also inexplicably drop two recent high-resolution studies from HBM's reanalysis, Harari and La Ferrara (2013) and Maystadt and Ecker (2014), both of which focus on civil conflict (per Buhaug et al.'s criteria), and both of which reported large positive contemporaneous and lagged effects of temperature on conflict (consistent with HBM).

   Buhaug et al. appear aware their decision to focus on lagged effects produces weaker results (they write: "with all contemporaneous effects, the aggregate point estimate increases"), but they do not present this finding in their main text. Furthermore, their claim that the contemporaneous effect is "statistically indistinguishable from zero" is false. When we replicate their Supplementary Figure A3 and compute standard errors for their estimate we recover a result almost identical to HBM's original result for contemporaneous conflict and which is highly statistically significant (10.0 ± 1.8 %/σ, $P<0.001$). Buhaug et al. do not explain why they chose not to display this result.

3) Without documenting this alteration, Buhaug et al. change HBM's original code such that studies focusing on the effects of drought or El Niño-Southern Oscillation (ENSO)—variables that explicitly include information about temperature—are no longer included in the temperature meta-analysis. For example, the large positive effect reported by Couttenier and Soubeyran (2013) for the Palmer Drought Severity Index is no longer included. These changes cause the average effect of temperature to appear smaller. Also without documenting this change, Buhaug et al. reclassify two results that use a standardized precipitation index (SPI) as drought studies (Theisen et al. 2011; O'Loughlin et al. 2012); in neither case does the employed SPI measure include any information about

temperature, and so was coded as "rainfall" in HBM. This conflation of variables introduced by Buhaug et al. generates inconsistency into HBM's otherwise internally consistent approach.

4) A coding error introduced into HBM's code by Buhaug et al. causes them to systematically drop the large temperature effect reported in O'Loughlin et al. (2012) in the temperature meta-analysis. Specifically, Buhaug et al. attempt a case-sensitive string match (in the statistical package R) for the temperature variable names "TI" and "ti," which returns *false* and drops the large positive estimated effect of temperature for the O'Laughlin et al. study. This error is visible in Buhaug et al.'s Fig. 1 because there is no grey tick-mark for O'Loughlin et al. (2012) in the right-most panel. This error reduces the estimated average effect of temperature on conflict.
5) Buhaug et al. altered the original meta-analysis code of HBM in a way that prevents display of the mean effect and its confidence interval in the grey panels showing the estimated distribution of raw data on the right hand side of their Fig. 1. Estimating these values is the central scientific contribution of HBM, so it is puzzling that Buhaug et al. actively chose to suppress their display when replicating HBM's analysis.

All five errors or display choices independently cause the meta-analysis results depicted in Fig. 1 of Buhaug et al. to appear nearer to zero and/or less statistically significant, in opposition to the results in HBM.

We fix Buhaug et al.'s errors 1, 3, and 4 while reversing their suppression of the main result (issue 5), using the exact same studies and parameters that Buhaug et al. use. These results remain incomparable to HBM's result because they focus only on the lagged effect of climate (issue 2)—which runs counter to most of the existing literature—but this exercise demonstrates how coding errors and display choices alone affected the appearance of Buhaug et al.'s altered meta-analysis. The results are shown in Fig. 1e. Notably, when using Buhaug et al.'s selection of studies and their focus only on lagged effects, we obtain a statistically significant effect of climate on conflict (3.92 ± 1.69 %/σ, $P<0.01$) and a larger, significant effect when focus is restricted to temperature (5.97 ± 2.97 %/σ, $P<0.01$). This result is broadly consistent with findings in HBM and directly contradicts Buhaug et al.'s reported results and conclusions.

We provide an updated version of Buhaug et al.'s analysis that retains the literature's focus on contemporaneous effects, but takes the potential for offsetting lagged effects seriously. In particular, we sum both the current and lagged effects of climate to obtain the cumulative effect of the climate across both periods, which is the technique for estimating total effects using a distributed lag model when there may be temporal displacement of conflicts between periods (e.g. p. 561 in Greene 2003). We use the same selection of studies used by Buhaug et al. and apply HBM's meta-analytic approach. These results are shown in Fig. 1f. When considering the cumulative effect of climatic events on civil conflicts in both the year they occur (the focus of HBM and the literature) and the following year (the focus of Buhaug et al.), using the studies and climate variables that Buhaug et al. select, we recover a statistically significant effect of climate on civil conflict (4.14 ± 1.65 %/σ, $P<0.01$), in agreement with HBM's original finding. Also consistent with HBM, when we examine the effects of temperature we obtain a larger estimated effect (11.17 ± 3.71 %/σ, $P<0.01$) that is very close to the 13.2 %/σ average temperature effect originally reported in HBM.

We conclude that the issues raised in Buhaug et al. do not alter the conclusions of the meta-analysis in HBM and the disagreement in findings are not explained by a difference of opinion regarding statistical methods. Rather, the appearance of a disagreement arises from Buhaug et al.'s inaccurate portrayal of the analysis conducted and reported in HBM as well as statistical and coding errors in their alteration of HBM's meta-analysis.

Hsiang et al. (2013a) and Hsiang and Burke (2014) attempted to summarize and clarify many recent findings in the literature on climate and conflict; in particular, they attempted to establish what findings to date were robust so that future research could build on these findings, further advancing and deepening our understanding of these critical scientific questions. The utility of this contribution has already been demonstrated by the Intergovernmental Panel on Climate Change's recent adoption of HBM's analytical framework (Field et al. (2014), section 18.4.5) and results (Field et al. (2014), sections 12.5.1, 19.4.2.2, and Summary for Policy Makers), as well as by many new studies that follow HBM's analytical approach and confirm the impact of climate on human conflict reported by HBM (e.g., Calderone et al. (2013), Hsiang et al. (2013b), Ralston (2013), Caruso et al. (2014), Fetzer (2014), Iyer and Topalova (2014), Kim (2014) and Wetherley (2014)).

References

Buhaug H (2010) Climate not to blame for African civil wars. *Proceedings of the National Academy of Sciences*, 107(38):16477–16482

Buhaug H et al (2014) One effect to rule them all? A comment on climate and conflict. Clim Chang. doi:10.1007/s10584-014-1266-1

Burke M, Miguel E, Satyanath S, Dykema J, Lobell D (2009) Warming increases risk of civil war in Africa. Proc Natl Acad Sci U S A 106:20670–20674. doi:10.1073/pnas.0907998106

Bushman BJ, Wang MC, Anderson CA (2005) Is the curve relating temperature to aggression linear or curvilinear? A response to Bell (2005) and to Cohn and Rotton (2005). J Pers Soc Psychol 89:74–77

Calderone M, Maystadt JF, You L (2013) Local Warming and Violent Conflict in North and South Sudan. IFPRI discussion paper

Caruso R, Petrarca I, Ricciuti R (2014) Climate Change, Rice Crops and Violence. Evidence from Indonesia. Working paper

Cohn EG, Rotton J (1997) Assault as a function of time and temperature: a moderator-variable time-series analysis. J Pers Soc Psychol 72(6):1322

Couttenier M, Soubeyran R (2013) Drought and civil war in Sub-Saharan Africa. Econ J. doi:10.1111/ecoj.12042

Fetzer T (2014) Can workfare programs moderate violence? Evidence from India. Working paper

Field CB et al (2014) Climate Change 2014: Impacts, Adaptation, and Vulnerability. Contribution of Working Group II to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change

Gelman A, Carlin JB, Stern HS, Rubin DB (2004) Bayesian data analysis. Chapman& Hall/CRC, Boca Raton

Greene W (2003) Econometric analysis. Upper Saddle River, Prentice Hall

Harari M, La Ferrara E (2013) Conflict, climate and cells: a disaggregated analysis. Centre for Economic Policy Research, London

Hsiang SM, Burke M (2014) Climate, conflict, and social stability: what does the evidence say? Clim Chang 123:39–55. doi:10.1007/s10584-013-0868-3

Hsiang SM, Meng KC, Cane MA (2011) Civil conflicts are associated with the global climate. Nature 476(7361):438–441

Hsiang SM, Burke M, Miguel E (2013a) Quantifying the influence of climate on human conflict. Science 341. doi:10.1126/science.1235367

Hsiang SM, Burke M, Miguel E (2013b) Reconciling Temperature-conflict Results in Kenya. CEGA working paper

Iyer L, Topalova PB (2014) Poverty and Crime: Evidence from Rainfall and Trade Shocks in India. Harvard Business School BGIE Unit Working Paper 14-067

Kim NK (2014) Revisiting economic shocks and coups. J Confl Resolut. doi:10.1177/0022002713520531

Maystadt J-F, Ecker O (2014) Extreme weather and civil war: does drought fuel conflict in Somalia through livestock price shocks? Am J Agric Econ

Neyman J (1937) Outline of a theory of statistical estimation based on the classical theory of probability. Philos Trans R Soc Lond Ser A Math Phys Eng Sci 236(767):333–380

O'Loughlin J, Witmer FD, Linke AM, Laing A, Gettelman A, Dudhia J (2012) Climate variability and conflict risk in East Africa, 1990–2009. Proc Natl Acad Sci U S A 109:18344–18349. doi:10.1073/pnas.1205130109

Ralston LR (2013) Essays on conflict, cooperation and economic development. Diss. Massachusetts Institute of Technology

Theisen OM, Holtermann H, Buhaug H (2011) Climate wars?: Assessing the claim that drought breeds conflict. Int Secur 36(3):79–106

Wetherley E (2014) Typhoons and Temperature Impact Crime Rates: Evidence from the Philippines