One effect to rule them all? A comment on quantifying the influence of climate on human conflict

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Abstract: A recent scientific review finds a remarkable convergence of evidence linking climatic events to violent conflict. Unfortunately, the meta-analysis suffers from important shortcomings with respect to sample selection and analytical coherence, and their sweeping conclusion is at odds with available scientific evidence. Contrary to the article's claim, scientific research on climate and intergroup conflict to date has produced mixed and inconclusive results.

Main text: Hsiang, Burke, and Miguel (HBM) find a "remarkable convergence of results" and "strong causal evidence" that climatic events are linked to social conflict at all scales and across all major regions of the world (1). The average effect from their meta-analysis indicates that a 1 standard deviation (σ) increase in temperature or rainfall anomaly is associated with an 11.1% change in the risk of "intergroup conflict". Assuming that future societies respond similarly to climate variability as past populations, they warn that increased rates of human conflict might represent a "large and critical impact" of climate change.

What HBM set out to do – to synthesize scientific evidence on climate and conflict – is a timely and important task, although their study is only one more contribution to a long list of scientific reviews published in the past two years (2, 3, 4, 5, 6, 7). However, HBM depart from these other reviews by attempting to quantify the average climate effect on conflict through a meta-analysis of selected statistical articles and unpublished papers on climate and human conflict. This comment identifies three central problems with HBM's analysis and shows that reasonable modifications to the original setup can result in a different conclusion. We conclude by briefly discussing why we believe the meta-analysis is fundamentally flawed.

The problems that we consider here relate to three underlying assumptions of the meta-analysis: (i) cross-study independence, (ii) causal homogeneity, and (iii) sample representativeness. HBM's main analysis rests on the assumption that sample studies are fully independent of each other. The problem is, there is considerable overlap between them. 19 of the 22 studies of modern climate-intergroup conflict links include African countries and half of these are limited to post-1980 Sub-Saharan Africa or a subset of country years. In one case, the cross-study correlation is estimated at r=0.6 (see supplementary material). Accordingly, the precision-weighted calculation of climate effects conducted by HBM returns unrealistically precise estimates and the true uncertainty around the average climate effect is much larger than reported.

Second, HBM's sample of candidate studies covers a wide range of phenomena from horn honking to imperial war, involves temporal scales between hours and millennia, concerns actors that range from individuals to ancient civilizations, and assumes climate effects that sometimes are linear, at other times parabolic; sometimes instant and at other times materialize after a distinct temporal lag. Claiming the same underlying climate effect across these heterogeneous studies is certainly a bold exercise, but this assumption is essential for the meta-analysis to be meaningful. A careful read of the literature, or inspection of Figure 1, reveals a variation in findings that is inconsistent with the assumption of causal homogeneity.

Third, aggregating and generalizing results from selected studies serves no purpose unless the sample constitutes a representative subset of all relevant scientific research. Yet, HBM's sample inclusion strategy favors form over function by using strictly methodological selection criteria. The result is a meta-analysis that disregards modern studies that revisit previously investigated climate-conflict associations, regardless of whether they complement, contrast or correct earlier findings. For example, the country-level relationship between rainfall and civil conflict is represented by a single peer-reviewed article (82), ignoring several more recent investigations that reach different conclusions (9, 10, 11, 12). Moreover, HBM pick just one climate indicator from each of the replicated studies, in many cases the one that indicated the strongest effect, despite most of the original studies exploring multiple alternative climate measures that sometimes produce contrasting results.

These are not trivial concerns. To demonstrate, we replicated HBM after implementing three changes that go some way towards addressing the issues raised above. More specifically, we first limited the sample to studies of contemporary civil conflict. These are the forms of conflict with the gravest social and political implications, and narrowing the scope of the dependent variable increases the plausibility of the assumed common causal effect. Next, to obtain a more representative sample and avoid the temptation to selectively pick indicators that produce a desired result, we included all main climate variables from the relevant studies. For the same reason, and to ensure analytical consistency and the correct sequence of events, we also applied a consistent lag of one time period (t–1) to all climate parameters across all models.

The updated meta-analysis with all three modifications implemented is visualized in Figure 1 (see supplementary material for further details). The result is striking. In contrast to HBM, we find no evidence of a convergence of findings on climate variability and civil conflict. Recent studies disagree not only on the magnitude of the impact of climate variability but also on the direction of the effect. The aggregate median effect from these studies suggests that a one-standard deviation increase in temperature or loss of rainfall is associated with a trivial 3.5% increase in conflict risk, although the 95% precision interval cannot exclude the possibility of large negative or positive effects.

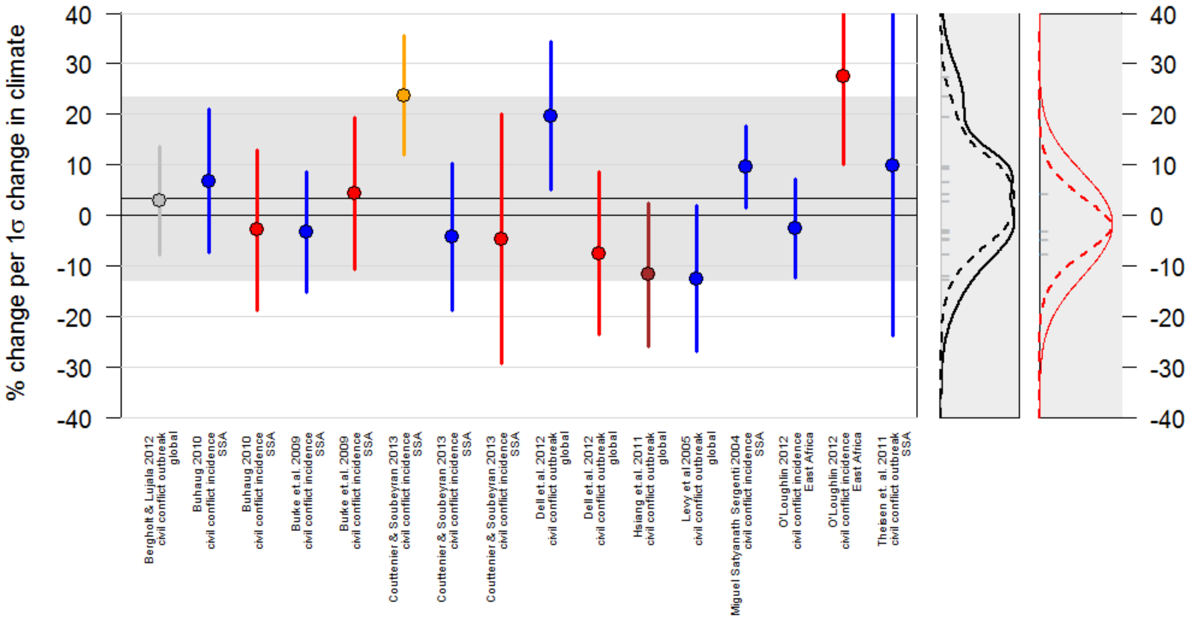

Fig. 1. Modern empirical estimates for the effect of climate variability on civil conflict. The markers illustrate the estimated percentage change in conflict with a 1σ increase in temperature (red), loss of rainfall (blue), deviation from normal rainfall (green), increase in drought (orange), El Niño-like conditions (brown) or increase in severity of climatic natural disasters (blue). Whiskers denote the 95% confidence interval. The solid horizontal line indicates the median climate effect with 95% precision interval in grey, based on a Bayesian hierarchical model. The panels at the right show the distribution of results from all candidate studies (black) or those focusing squarely on temperature effects (red). Studies listed alphabetically.

Does our updated meta-analysis provide the true quantification of the average effect of climate on violent conflict? No. While the modifications we have implemented increase analytical consistency and representativeness and offer meaningful alternative specifications, the most fundamental problem of this analysis remains largely unaddressed. Unlike meta-analyses of medical treatment studies, which are based on similar individual-level investigations from independent sample populations, this assessment bundles together partly overlapping observations at different spatial and temporal scales in an inconsistent and atheoretical fashion. Quantifying the "average" effect across this heterogeneous sample, weighting results by statistical precision but ignoring variations in substantive relevance and potential for generalization, makes no sense. It is a bit like averaging the price of apples and oranges, sampled in different locations and time periods, at different scales, using different metrics. The exercise is mathematically feasible but the outcome has no relevant meaning. Remedying this problem can only be done by taking into account the substantive content of candidate studies, ensuring similar units of analysis across studies based on unique samples, and parameterizing empirical indicators in a theoretically consistent manner.

Recent reviews of the scientific literature conclude that research to date has failed to converge on a specific association between climate and violent conflict (2, 3, 4, 5, 6, 7). We agree. This does not mean that climate cannot influence political violence in more subtle and complex ways. Hence, future research should continue the recent trend toward spatial and temporal disaggregation (13, 14, 15) and investigate possible mechanisms and intervening effects in order to identify the conditions under which climatic events plausibly may have a measurable impact on conflict dynamics.

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Supplementary Material

One effect to rule them all? A comment on "Quantifying the influence on climate on human conflict"

I INTRODUCTION

This document offers more details and additional documentation of the issues raised in the commentary article (REF). We discuss three limitations with Hsiang et al.'s (2013; henceforth HBM) meta-analysis and propose modifications to make the underlying assumptions somewhat more plausible. Each of these changes constitutes a modest improvement of the approach chosen by HBM, and each modification suggests that the average climate effect no longer can be considered significantly different from zero. Taken together, the updated meta-analysis indicates that the "average" effect of climate on violent conflict is small and sensitive to sample and model specification. Yet, this quantification hardly makes more sense than HBM's original analysis and fails to provide meaningful insight on the general climate-conflict relationship. We conclude by briefly discussing why we believe the meta-analysis is fundamentally flawed.

II CROSS-STUDY DEPENDENCE

HBM's main meta-analysis rests on the assumption that sample studies are independent of each other. This is assumed despite the fact that 19 of the 22 studies of modern climate-intergroup conflict links include African countries and half of these are limited to post-1980 Sub-Saharan Africa or a subset of country years (HBM Table 1). In so doing HBM calculate the average climate effect with a variance-weighted standard error by setting all covariances in the variance-covariance matrix to 0. Under this assumption, it is not possible to end up with a higher aggregate uncertainty than the lowest uncertainty among the candidate studies.[1] Yet, it is obvious that there is considerable cross-study correlation, meaning that the procedure to calculate confidence levels is inappropriate and the average effect is substantially less precise than indicated in HBM's analysis. To illustrate, Figure A1 compares the residuals from two of the candidate studies, Burke et al. (2009) and Buhaug (2010). If these studies were unrelated, we should see a random covariance of plots. Instead we find that most observations cluster along the diagonal and the data points correlate at a significant r=0.61.

[1] The most certain point estimate in HBM's analysis of "intergroup conflict" is derived from a municipality-level study of rainfall deviations and land invasions in Brazil (Hidalgo et.al. 2010). The variance-weighted standard error method is essentially saying that our uncertainty about how climate (broadly defined) affects conflict (broadly defined) cannot be any larger than the uncertainty reported in that study.

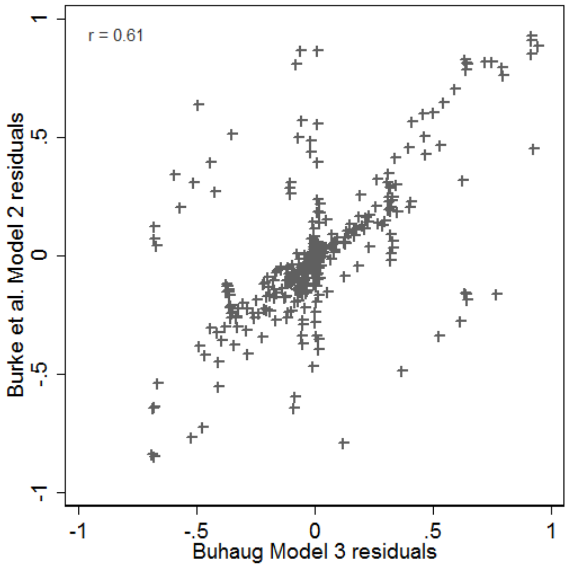

Fig. A1. Evaluation of cross-study dependence. The figure plots residuals from Model 2 in Burke et al. (2009) against the residuals from Model 3 in Buhaug (2010).

Acknowledging that the underlying assumptions of the meta-analysis might not hold, HBM then refer to estimations based on a Bayesian hierarchical model. Deriving the 95% precision interval from this estimation suggests a much larger uncertainty around the average point estimate, where the reported climate effect fractionally insignificant at a conventional level of confidence. As shown in Figure A2, the more plausible error band around HBM's original result suggests an overall effect somewhere between -2.7% and 27.3%. That is, under the added assumptions that the sample studies are measuring the same underlying effect and that the replicated effects are representative of the larger empirical literature on climate and violent conflict.

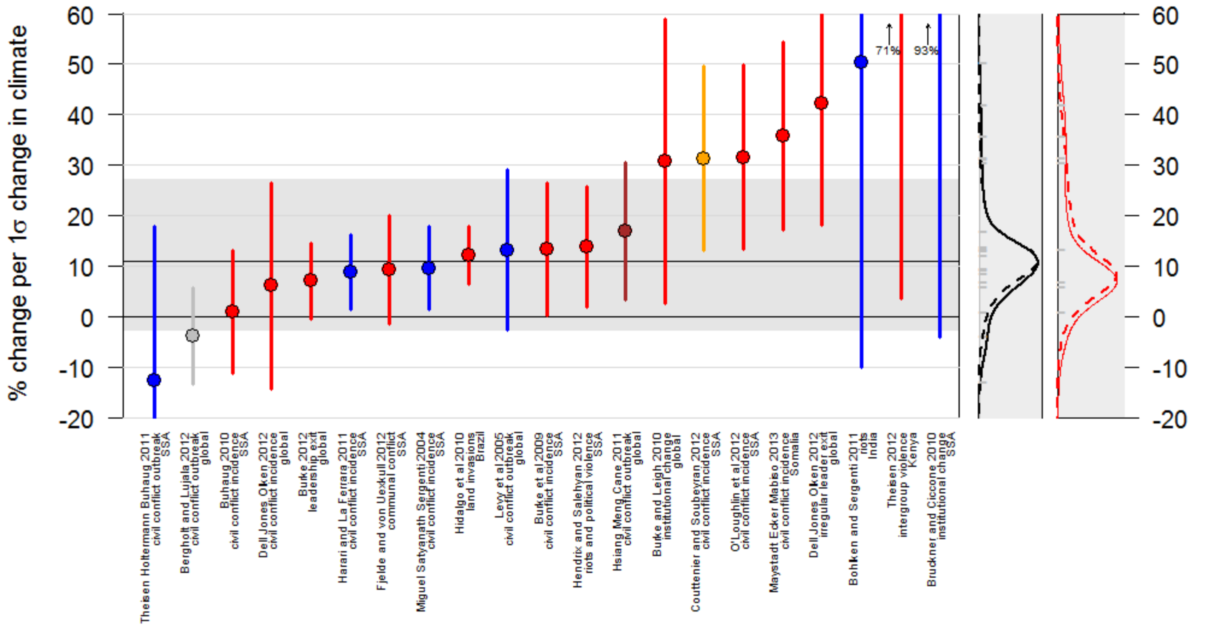

Fig. A2. Replicated HBM Fig. 5 with precision interval derived from a Bayesian hierarchical model. The markers illustrate the estimated percentage change in conflict with a 1σ increase in temperature (red), loss of rainfall (blue), deviation from normal rainfall (green), increase in drought (orange), El Niño-like conditions (brown) or increase in severity of climatic natural disasters (blue). Whiskers denote the 95% confidence interval. The solid horizontal line indicates the median climate effect with 95% precision interval in grey, based on a Bayesian hierarchical model. The panels at the right show the distribution of results from all candidate studies (black) or those focusing squarely on temperature effects (red). Studies listed in the order shown in Figure 5 in HBM.

III CAUSAL HETEROGENEITY

A second assumption of HBM's meta-analysis is that the sample studies are measuring the same underlying effect. Even so, the selection criteria for candidate studies are strictly methodological, with no explicit consideration of the content being analyzed, beyond relevance to a loosely operationalized concept "human conflict." The result is a sample that covers almost anything from horn honking to imperial war, involves temporal scales between hours and millennia, concerns actors that range from individuals to ancient civilizations, and assumes climate effects that sometimes are linear, at other times parabolic; sometimes instant and at other times materialize after a distinct temporal lag.

Claiming the same underlying climate effect across these heterogeneous studies is certainly a bold exercise, but this assumption is critical for the meta-analysis to be meaningful. In other words, the causal effect of drought-affected changes in cattle prices on local conflict events in contemporary Somalia (Maystadt et al. 2013) is assumed to be similar to the impact of a multi-decade cold period on war in medieval Europe (Tol and Wagner 2010), where any variation in estimated effects between studies is due to random error. Yet, in HBM's Figure 5, three of the 21 studies have confidence intervals that fall completely outside the estimated "true" effect, with results suggesting both larger positive and negative effects. Comparing the estimated climate effect from Bergholt and Lujala (2012) with the opposite effect from Dell et al. (2012) suggests that they must have been extremely unlucky in their sampling. The alternative explanation, of course, is that these studies are estimating different things. A *t*-test reveals that the probability that the reported effects are similar is virtually zero ($p<2.2e-16$).

The diversity of candidate studies is a fundamental problem with HBM's analysis. One way to make the assumption of causal homogeneity somewhat more plausible is to limit focus to the effect of climate on contemporary civil conflict, pitting state military forces against non-state rebel armies. Figure A3 shows the result after dropping studies of social behavior beyond civil conflict. The estimated median effect, following HBM's procedure, is still clearly positive but the uncertainty around this estimate now ranges from -5.9 to 30.5, meaning that we can no longer confidently conclude in favor of a robust positive association.

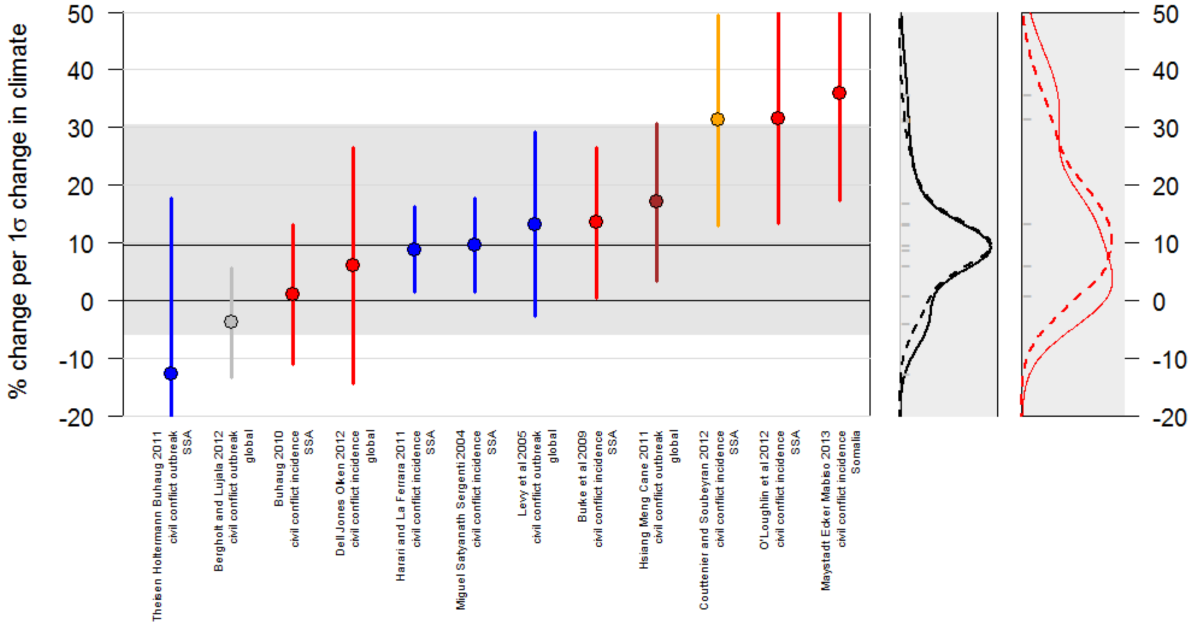

Fig. A3. Replicated HBM Fig. 5 limited to studies of contemporary armed conflict. See Figure A1 for details on coloring.

A complementary means to streamline the meta-analysis is to ensure a consistent treatment of the time dimension. HBM blended contemporaneous and lagged climate effects with no seeming consideration of the underlying theoretical argument or the precision and temporal resolution of the data.[2] We replicate HBM's model with a consistent one-unit time lag (*t*–1) to the climate effect.[3] The specification of a short time lag makes sense since it ensures the correct sequence of events. Almost all studies in the sample use annualized data. For conflict onsets and violent events occurring early in the year, using contemporaneous climate variables means adding right-hand side variables that largely reflect environmental conditions after the violence took place. Also, the mobilization of resources and personnel necessary for triggering the types of collective violence considered here (typically causing 25+ battle deaths) usually takes some time. The result after updating the analysis, visualized in Figure A4, is striking. Half of the point estimates now indicate a negative association with conflict and the aggregate climate effect is estimated at a negligible -0.1.

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[2] For example, Levy et al. (2005) is specified with contemporaneous rainfall effect (despite the original study reporting lagged effects only) whereas Miguel et al. (2005) is represented with a lagged rainfall effect (the original study also reported contemporaneous effects).

[3] Because we do not have access to the replication data for the working papers by Harari and La Ferrara (2013) and Maystadt et al. (2013), these drop out when we specify lagged climate effects.

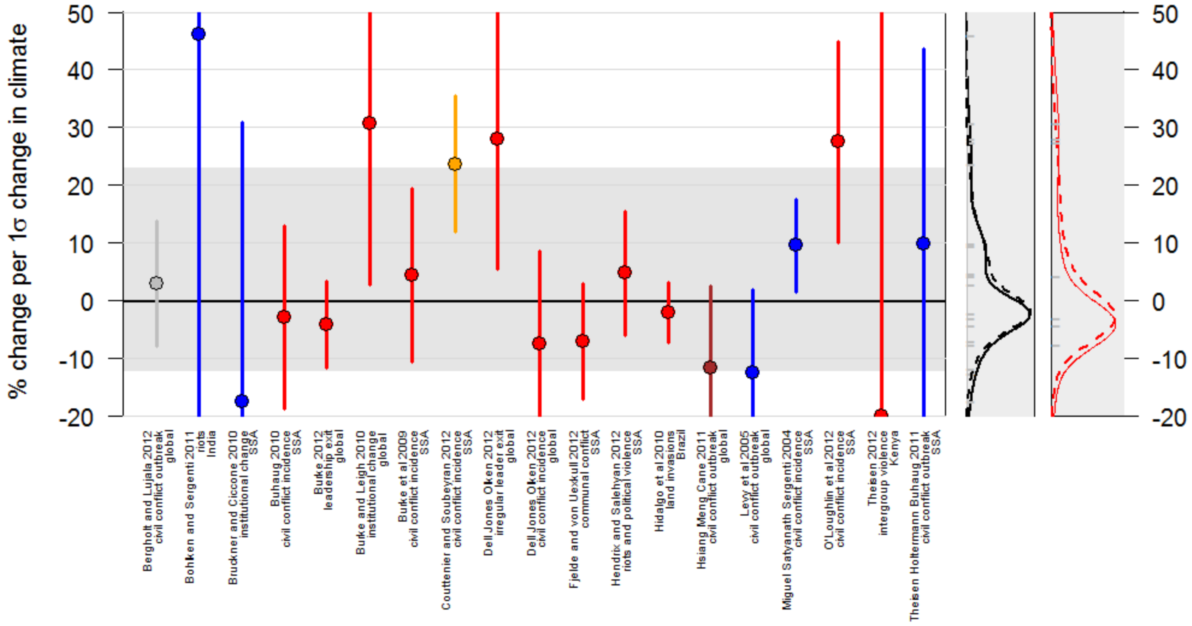

Fig. A4. Replicated HBM Fig. 5 with consistent treatment of time (time lag *t*-1). See Figure A1 for details on coloring.

There are other aspects of HBM's analysis that are inconsistent with the causal homogeneity assumption, too. For example, some candidate studies (e.g., Bohlken and Sergenti 2010) use climate indicators that give absolute levels or growth of rainfall, where the causal effect is assumed to be near linear with conflict risk increasing as precipitation drops. Others studies (e.g., Hendrix and Salehyan 2012) parameterize climate variability by measuring deviations from the long-term mean, whereby both dry and anomalously wet conditions are considered more hazardous. As a result, very different empirical patterns may produce seemingly consistent results. For example, Bohlken and Sergenti report a negative association between rainfall growth and riots whereas Hendrix and Salehyan find a parabolic inverted U relationship, with somewhat higher risk of violent events occurring in periods with abundant rainfall. In the meta-analysis (HBM Figure 2), these findings are portrayed to signal the same causal effect although clearly they are not.

IV UNREPRESENTATIVE SAMPLE

HBM identified the sample for their meta-analysis based on two methodological criteria: Candidate studies must provide quantitative analysis with location-specific fixed effects and corrections for time trends, and they must be based on new data. The resulting sample contains studies on a wide range of social outcomes and includes many unpublished papers, but it excludes a large number of peer-reviewed publications that investigate similar climate-conflict constellations on grounds of failing to comply with the "new data" criterion. It is not clear what constitutes new data, however, and the decision to disregard revisiting studies, regardless of whether they complement, contrast or correct earlier findings, is clearly problematic. Newer studies often benefit from improved data and longer time-series that permit more precise and representative empirical assessments.

To illustrate, consider the country-level relationship between rainfall and civil conflict. In HBM's meta-analysis, this relationship is represented by a single peer-reviewed article (Miguel et al. 2004), thus ignoring several more recent investigations, including two by the same authors (Burke et al. 2009; Miguel and Satyanath 2011) that come to different conclusions. Miguel et al. (2004) report a robust negative association between rainfall growth and civil war. Burke et al. (2009) find no significant effect and conclude that "the role of precipitation remains empirically ambiguous" (p. 20672) whereas Miguel and Satyanath (2011) concede a point made by Ciccone (2011) that the rainfall effect disappears when expanding the time period into the 2000s. Neither of these newer findings is taken into account in HBM's meta-analysis.[4]

In order to make the meta-analysis more representative of the universe of available results, it should consider all climate indicators from the candidate studies instead of picking just the one that happens to produce the strongest effect or (in a limited number of cases) the median effect.[5] Theories of climate-conflict linkages rarely are sufficiently detailed and precisely formulated to dictate (i) which of a set of conceptually overlapping climate parameters is preferable, (ii) whether and how the climate effect should be modeled contingent on certain social and/or structural factors, and (iii) what would be the appropriate time lag. Accordingly, studies that give preference to one among several tested climate indicators tend to do so based on the indicators' empirical behavior rather than theoretical appropriateness. Moreover, many studies include indicators of several distinct phenomena, such as temperature and rainfall. Picking just one estimate from such studies disregards the breadth of the original analysis.

Figure A5 replicates HBM's Figure 5 but expands the sample by adding 12 results from the candidate studies for additional climate indicators that were ignored by HBM.[6] Remarkably, all 12 point estimates fall below the average effect reported by HBM, none of them is statistically significant, and most of them indicate effects very close to zero.[7]

[4] Although Burke et al.'s (2009) insignificant rainfall result is not part of HBM's meta-analysis, the significant temperature effect is considered in the evaluation of the temperature-conflict link, see HBM Table 1. Other candidate studies that are subject to a similar selective treatment include Couttenier and Soubeyran (2013), O'Loughlin et al. (2012), and Theisen (2012); see Figure A5 for details.

[5] Adding related studies may increase cross-study dependence. However, it is clearly better to apply a weighting scheme to accounts for overlap between studies than dropping follow-up analyses and assume that original studies are representative of the universe of scientific research.

[6] Some candidate studies also investigate interaction terms and non-linear effects. For the sake of parsimony, we follow HBM and only consider direct effects here. Also, we decided not to include the over 20 alternative operationalizations of drought (all insignificant) reported in Theisen et al. (2011/12).

[7] Several of the candidate studies also reported effects using alternative indicators of conflict, such as outbreak versus incidence of conflict or occurrence of specific conflict events. Adding these results to this assessment is beyond the scope of this comment though we note that these data points very likely would add further heterogeneity to the sample.

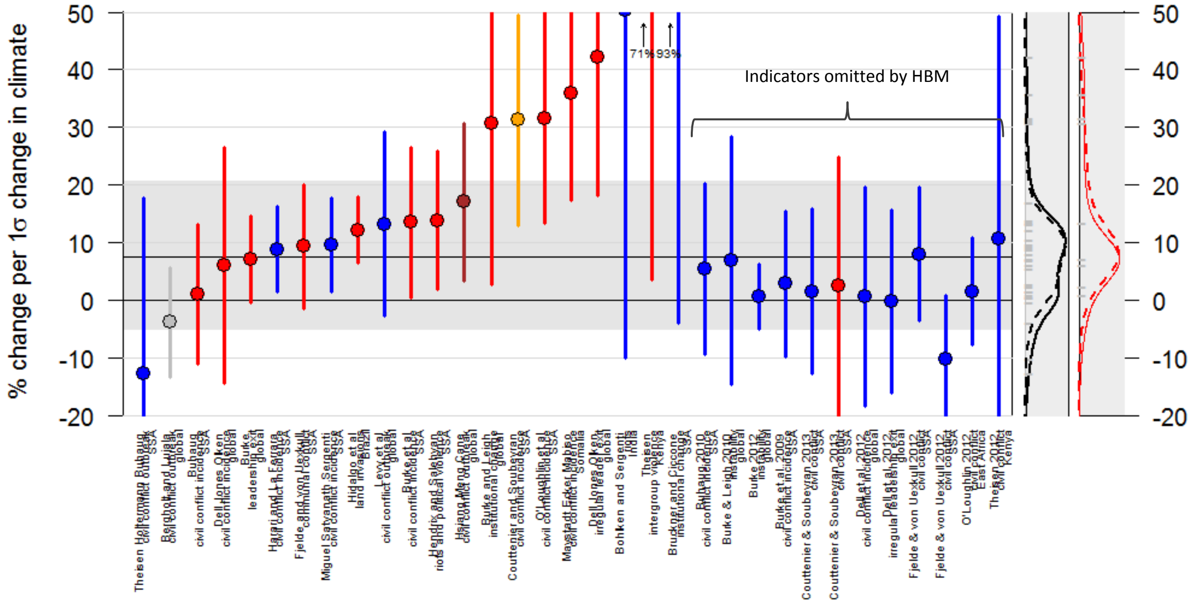

Fig. A5. Replicated HBM Fig. 5 with 12 additional climate indicators from candidate studies. See Figure A1 for details on coloring.

V CONCLUDING REMARKS

We have identified three important limitations with HBM's study and proposed three simple modifications to mitigate some of these problems. The outcome of these modifications is summarized in Table A1. Several aspects are worth noting. First, we see that two of the three proposed improvements, when implemented individually, have a modest impact on the median effect reported by HBM. The third modification, applying a consistent time lag, in contrast completely eradicates the original result. Second, each of the three modified analyses returns a confidence interval that includes both positive and negative effect, meaning that we cannot reject the null of no association. Third, when all three modifications are implemented, the meta-analysis returns a trivial median effect of 3.5%. The final model is shown in Figure A6.

Table A1. Median effect and 95% precision interval for alternative specifications.

	2.5%	50%	97.5%
HBM original	-2.7	11.0	27.3
1. Only civil conflict studies	-5.9	9.7	30.5
2. Add consistent time lag (*t*-1)	-12.3	-0.1	22.9
3. Add omitted indicators	-5.2	7.6	20.8
All three modifications	-13.1	3.5	23.4

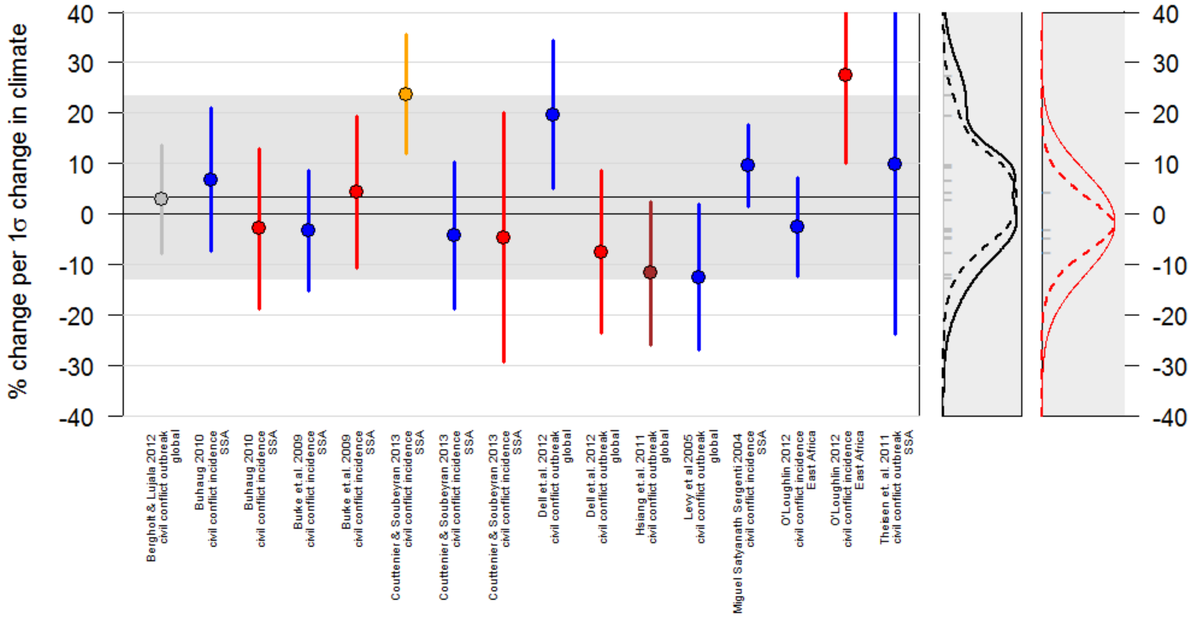

Fig. A6. Replicated HBM Fig. 5 with all three modifications implemented. The sample includes all main climate indicators from candidate studies of civil conflict, with consistent imposed time lag of one time period (*t*–1). See Figure A1 for details on coloring. Candidate studies listed alphabetically.

Political violence and civil conflict are social processes and there is not one right way to model such behavior. Different theoretical perspectives and various methodological concerns may give preference to different model specifications – or imply that several alternative specifications are equally reasonable. One potentially contentious issue is whether to model lagged or contemporaneous climate effects. That depends not only on theoretical approach and which causal mechanisms one believes are more important, but also on the quality of information and data resolution. With data structured in the calendar year format, the notion of extreme weather events acting as a conflict trigger may be incompatible with a one-period time lag (Fjelde and von Uexkull 2012); here, monthly or weekly structured data would better suited to model a short temporal lag.[8] In other instances, the claimed causal chain from climate extremes to violent conflict spans decades (Faris 2007).

Does this imply that our modified analysis provides a reasonable approximation of the true general climate effect on violent conflict? No. While the modifications we have implemented reduce some problems with HBM's study and offer meaningful alternative specifications, the most fundamental problem of this analysis remains largely unaddressed. Unlike meta-

[8] Replicating the analysis in Figure A6 with contemporaneous climatic (*t*) conditions gives a median effect of 7.4% with a 95% precision interval ranging from -6.1 to 24.0.

analyses of medical treatment studies, which follow the same general setup and are based on similar individual-level data from independent sample populations, this assessment bundles together partly overlapping observations at different spatial and temporal scales in an inconsistent and atheoretical fashion. Quantifying the "average" effect of widely different climate phenomena on widely different social outcomes, while ignoring variations in relevance and potential for generalization between studies, makes no sense. It is a bit like averaging the price of apples and oranges, sampled in different locations and time periods, at different scales, using different metrics. The exercise is mathematically feasible but the outcome has no substantive meaning.[9] Remedying this problem can only be done by taking into account the substantive content of candidate studies, ensuring similar units of analysis across studies based on unique samples, and parameterizing empirical indicators in a theoretically consistent manner.

Unlike HBM, we see no remarkable convergence of findings, and a representative collection of relevant published results (e.g., Figure A6) leaves the same impression. We conclude along the lines of Bernauer et al. (2012), Gleditsch (2012), Meierding (2013), Scheffran et al. (2012a,b), and Theisen et al. (2013): There is no scientific consensus on a simple and general impact of climatic changes on violent conflict. This does not mean that climate cannot influence political violence in more subtle and complex ways. Hence, future research should investigate possible mechanisms and intervening effects and identify the conditions under which climatic events plausibly may have a measurable impact on conflict dynamics.

[9] The most influential data point in HBM's meta-analysis, a municipality-level study of mostly non-violent "land invasions" in Brazil (Hidalgo et al. 2010), is considered *ten times* more important and representative of the general climate-conflict relationship than a cross-national analysis of civil war in Sub-Saharan Africa (Couttenier and Soubeyran 2013) and *one hundred times* more important than Bohlken and Sergenti's (2011) study of climatic shocks and riots in India, simply because the dataset in the former study allows for a more precise estimation of the rainfall effect (assigned weights for mentioned studies are 0.204, 0.021, and 0.002, respectively). Implementing more reasonable weights that simultaneously account for cross-study correlation and generalizability of results is beyond the scope of this commentary article.

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